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A Brief History of the Oregon Model

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There is an old saying attributed to many writers: "If you want to understand something, try to change it." In a very real sense, that could be the core message contained in the present volume. Researchers began in the early 1960s by trying to change a variety of problem child behaviors including aggression. We and our colleagues at Tennessee found that we could train parents in such a way that problem behaviors were reduced. The initial success encouraged us to move to the more complex question concerning how it was that children developed problems in the first place. The twin focus on intervention and theory fit with very nicely with a society that was becoming increasing concerned with rising crime rates and juvenile violence. This, in turn, led to four decades of relatively continuous support from the National Institute of Mental Health and from private foundations. The present volume summarizes the outcomes of the four decades of research that ensued for both intervention and theory.

This first chapter is a brief account of the journey. It will become apparent from the overview that the studies did not move in the linear and inexorable fashion portrayed in some histories of science. In our own case, the many setbacks, twists and turns, and many cul de sacs define a journey unlikely to appear in any journal article. In the discussion that follows, the focus moves alternately from intervention to measurement to theory more or less in the sequence that we encountered these problems.

Beginnings

Intervention Problems and the Academy

In the early 1960s a small group of investigators (including Patterson, Willliam Bricker, and Jim Straughan) at the University of Oregon Psychology Department decided that existing treatments for aggressive children were not effective. Several of us shared the task of running a small outpatient child-guidance clinic, a core component in the newly created clinical training program. The psychodynamic play-therapy treatment techniques that Patterson had been trained to use at one of the nation's best child-guidance clinics (Wilder Clinic, St. Paul, Minnesota) were considered state of the art. As a young assistant professor, it was his task to train graduate students, such as John Reid, to apply these procedures to children referred for treatment.

During treatment, we routinely asked the parents (almost always mothers) how the treated child was doing. About two thirds of the time, we received reports that were mildly supportive of our efforts to treat. We learned later that mothers' reports showed little correlation with objective measures of treatment outcomes (Patterson & Narret, 1990; Patterson, Chamberlain, & Dishion, 1993). We seldom asked teachers or used objective measures to assess outcomes. However, the review of traditional therapies by Levitt (1957, 1971) showed that the traditional treatments were not effective for children with hyperactivity, oppositional behavior, and aggression. The fact that these problems constituted about two thirds of the children referred for treatment posed a major problem. As young professors, we felt challenged to design interventions that would be successful and to accompany them with more objective measures of treatment outcome.

We began by applying some operant procedures to very simple problem. However, the only cases made available to us consisted of hyperactive and autistic children. It turned out, of course, that neither of these types of problems have simple solutions. With autistic children, our applications of simple contingency procedures were dramatically unsuccessful. At that time, we had no idea of the density of reinforcing contingencies required to bring about the stunning changes that has been achieved with these cases by I. Lovaas and his colleagues (Lovaas, 1978, 1987).

In contrast, the work with hyperactive children in the classroom setting was immediately successful. Using simple reinforcement contingencies to strengthen behaviors that would compete with hyperactivity (e.g., sit still, attend), we produced very rapid and seemingly dramatic improvements in classroom behavior (Patterson, 1965). In the studies that followed in rapid succession, the effects were replicated; and the procedures developed further (e.g., Nixon, 1966; Patterson, Jones, Whittier, & Wright, 1965; Anderson, 1964). These findings led to the more practical approach of training the teacher to use positive and negative contingencies with children in the classroom setting (Hops et al., 1978). The Oregon studies were part of a network of researchers across the country that has resulted in carefully research technology for effective classroom management. Walker (1995) summarized the findings from this line of productive work. Our own group moved on to focus upon families and aggressive children.

Measurement: The Centrality of Observation Data

In the 1960s, young child clinical psychologists participated in a dubious charade. As graduate students, we were wedded to the idea of objective measures. In keeping with our belief, we recited daily canticles from the book of statistics. However, when it came to the real business of assessing and treating children in the clinical setting, we abruptly ceased to be scientists. We turned instead to projective tests and to untested treatments based on psychoanalytic ideas. At no time did the science or the facts of psychology influence our efforts to assess and treat aggressive children and their families. This created several problems. One Achilles heel was that the majority of mothers seemed to report improvement regardless of what treatment was provided (Patterson & Narret, 1990; Patterson, Dishion & Chamberlain, 1993). Using this criterion, everything worked, and nothing worked any better than anything else. We needed to find some more objective means for assessing treatment outcomes.

One of the strengths of the operant behavioral position lies in its insistence that claims for behavior change be based upon observation data. Thus, as we began to experiment with operant procedures, we also began to develop observation codes. We drew upon our early training as psychologists and addressed the usual psychometric issues concerning reliability, validity, event sampling, and convergence. We spent over 3 years completing the pilot methodological studies for the Family Interaction Code system (Reid, 1978). Our colleague Phil Schoggin introduced us to R. Barker's programmatic studies of children in natural settings (Barker, 1963; Barker & Wright, 1951). Their narrative accounts provided sequences of events occurring in real time, a notion that was quite appealing.

Observation presented us with a host of new concerns. One of our first was with the best means of training observers to be reliable (Reid, 1967; 1970). There was also a long and complex series of studies trying to define what the impact of observer presence had on classroom or family interactions (reviewed in Jones, 1973; Jones, Reid, & Patterson, 1975; Patterson, 1982). For example, in one series of studies we found that observers provided similar estimates of parent and child negativity to that obtained via tape recorders placed in the home and turned on at random intervals. Studies were also carried out to examine the possibility that families could fake good or fake bad during home observation sessions (Johnson & Lobitz, 1974). The findings showed that although both normal and distressed families could fake bad, distressed families had difficulty faking good. Studies reviewed in Patterson (1983) showed similar findings for observations of married couples and for teachers in classroom settings.

The accurate feedback that observation data provided enabled us to identify the intervention components that worked and those that did not work. Observation procedures were used to evaluate the operant procedures applied to hyperactive children in the classroom. The contingencies were designed to strengthen behaviors that would compete with the presenting problem behaviors. The initial results were encouraging. It was a beginning. We now understood a tiny bit about changing behavior, and more importantly we were beginning to understand how to measure behavior changes occurring in natural settings.

Our First Theory and Our First Cul de Sac

Our efforts to build a theory about problem children also began in the early 1960s. In this formulation, responsiveness or lack of responsiveness to social stimuli was thought of as a trait that could lead to a wide spectrum of problem behaviors (Franks, 1965; Patterson, 1960; Patterson & Anderson, 1964). There were a number of studies that suggested this was a promising approach. For example, Lykken (1957) had found that "psychopaths" were significantly less responsive in avoiding aversive contingencies (punishments) than were "non-psychopaths." In a similar vein, Levin and Simmons (1962) found that clinical samples of boys were relatively nonresponsive to positive reinforcers from adults.

To test some of these hypotheses, a series of laboratory studies were carried out using a Gewirtz (1967) type instrumental conditioning procedure. The results, summarized in Patterson (1965), showed that boys rated out of control by teachers were less responsive to parental disapproval and were more responsive to positive reinforcers delivered by peers. The peer findings were replicated in the study by

Patterson and Fagot (1967): Boys most responsive to peer reinforcement tended to be more out of control in the classroom.

As in the observational work in the laboratory studies, considerable time was spent on examining such methodological concerns as reliability, stability, and response preference biases (Patterson, 1965; Patterson & Fagot, 1967; Patterson & Hinsey, 1964; Patterson, Jones, Whittier, & Wright, 1965). To test for generalizability, we constructed alternative procedures for measuring responsiveness (Fagot, 1966). A mobile laboratory was built to facilitate collecting data when parents and peers served as reinforcing agents. Several contemporaries, such as Harold Stevenson and Robert Cairns, also succumbed to the allure of these procedures as a means for the scientific study of responsiveness to social reinforcers.

However, after a decade of work, the procedures and findings stood as empty as the Mayan ruins at Chitzen Itza. We decided to abandon the paradigm, as did our contemporaries. What produced the mass exodus was a set of findings showing that most of the variance in the change scores was attributable to method. Preference change measures were primarily accounted for by patterns of alternation identifiable at baseline (Patterson & Hinsey, 1964). The coup de grace came with Parton and Ross's (1965) report demonstrating that measures of changes in rate were produced by a variety of variables other than reinforcement per se. By the late 1960s, we had shifted our emphasis almost entirely away from laboratory studies of aggression reinforcement responsiveness to observation in natural settings.

Now, 30 years later, we are again interested in measures of responsiveness to social contingencies. In a recent proposal, we hypothesized that responsiveness might be a mechanism that varies as a function of genetic contributions. In retrospect, the question was a good one; it was just that our technology was not up to the task. For example, Snyder and Patterson (1986) described an alternative procedure for measuring responsiveness based on interactional sequences occurring in natural settings. We fully expect that in some future study the variable of responsiveness will arise phoenix-like from the ashes.

Finding a Focus: Parent Training Therapy

As we worked in the homes of families with aggressive children, we were surprised to find how little connection there was between mother-reported improvements and what we actually saw in the home and the classroom. The lack of correspondence between parent and observer data had been noted by others, but at the time we were unaware of these systematic studies (Fontana, 1966). Now we know that a poor correspondence between parent and observer reports of behavior change is ubiquitous (Atkeson & Forehand, 1978; Patterson et al., 1993).

Observation data also quickly identified a basic flaw in Skinner's early position on the ineffectiveness of punishment. Based on his position, we instructed parents of problem children to focus entirely on the of use positive reinforcement for competing prosocial child behaviors. In spite of parent reports of progress, observation data for the first few cases showed no significant reduction in child aversive behavior. It was only when we added punishment-type procedures, such as time out or point loss, for deviant behavior that the observation data showed clear changes. This phenomenon is now well understood. For example, Wells (1995) recently reviewed a set of six carefully controlled laboratory studies that demonstrate the same conclusion.

Ideas about the centrality of parental contingencies and the need for observation data to assess the efficacy of intervention with aggressive children were part of the Zeitgeist at Oregon and at other centers around the country. During this period there were frequent informal contacts with Connie Hanf at the Oregon Medical School and Bob Wahler at Tennessee. In retrospect, it is difficult to say where each specific idea came from that eventually became parent-training therapy. However, at each center the ubiquitous single-subject graphs were posted on the walls for all to see. The observation data showed that family changes actually occurred and that they persisted.

It looked like science, but it also felt like good clinical process. We were (and are) convinced that we were really helping people. The clinical and observation procedures were published in reports by Patterson, Cobb, and Ray (1972 & 1973); Patterson, Reid, Jones, & Conger (1975); and Reid (1978). By 1968, we had standardized the assessment procedures and, to some extent, the treatment procedures as well. Each family was observed during baseline, treatment, and follow-up. The findings for the cases treated from 1968 through 1977 were summarized in Patterson (1979b).

In the early 1970s we began a series of randomized studies based on group rather than single subject designs (Patterson, Chamberlain, & Reid, 1982; Walter & Gilmore, 1973; Wiltz & Patterson, 1974). Do the procedures produce reliable changes in a significant proportion of the cases treated? Do the procedures produce results that persist (Patterson & Fleischman, 1979)? The data showed the answer to both questions was an emphatic "Yes!" Finally, do the parent-training procedures work when applied to chronic offending adolescent delinquents? Reports by Bank, Marlowe, Reid, Patterson, and Weinrott (1991) and Reid, Eddy, Fetrow, and Stoolmiller (1999) showed a significant reduction in police arrests and long-term reductions in costs due to institutionalization for the experimental group.

The Winds of Change

However gratifying these early successes, they carried with them strong winds of change. It led us eventually to find a new setting for our work and to design our own work environment. It also led to modification of how outside groups perceived what we were doing. For example, emphasizing the key role of punishment in weakening aggressive or antisocial behavior, the use of group rather than single subject designs, and extensive use of statistical analyses offended some of our radical behaviorist colleagues. As a result, we were gently dropped from their list of so-called good scientists. We were often cited at national conferences sponsored by behaviorists to highlight parent-training research, but we were not asked to be among the presenters.

At the same time, the flurry of findings from our approach to treatment and theory caused a storm of protest from some members of the psychology department at the University of Oregon. The psychoanalytic component in the department was offended that we had ignored all of their variables. There were angry confrontations with Gestaltists and attacks on the behavioral position in brilliant lectures by the new revolutionaries from the cognitive sciences. The mission for the psychology department was to build the new cognitive science. Similar paradigm confrontations were going on across the country. In our own battles we lost half of our collaborating colleagues. For psychology, it marked the advent of a new paradigm, probably a long overdue vitalizing event.

For our developing social learning group, the long-range effect of the palace revolt was very positive. A small group of us retreated to the sanctuary of a nonprofit research corporation, the Oregon Research Institute (ORI). ORI had begun several years earlier on the crest of a new wave of federal funding for psychological research. The ORI administration gave us space and encouraged us to apply for research funds at National Institute of Mental Health (NIMH) and the National Science Foundation (NSF). The intellectual climate at the institute was both benign and intense. An exciting and multi-disciplinary group of investigators worked on problems of personality, measurement, cognition, and prediction (including G. Bechtel, L. Goldberg, P. Hoffman, L. Rorer). Summer meant marvelous congeries of visitors and consultants with world-class skills in a variety of areas (e.g., W. Edwards, W. Norman, D. Peabody, A. Tversky). As a group, we began to develop a working environment that we felt maximized our scientific productivity.

Performance Theory

In the early 1970s, based upon our analyses of observation data, we were being encouraged by others to develop a theoretical statement about children's aggression. For example, there were invitations to present at the Minnesota Fifth Symposium on Child Psychology (Patterson & Cobb, 1971) and at the University of Iowa conference on the control of aggression (Patterson & Cobb, 1973).

We decided that, eventually, a theory of aggression would have to account for individual differences in aggression. From this perspective, it would be necessary to demonstrate that variables thought to produce aggression must be shown to account for significant variance in whatever variables were used to assess the criterion. Based upon our understanding of Paul Meehl's position, the restricting assumption was that the same agent-method measures used for the model could not also be used to assess outcome (e.g., we would not use mother ratings to measure both family variables and child adjustment).

Our acceptance of the idea of a performance model led to an immediate reminder of our limitations. It had been known for some time that reinforcement variables could not account for individual differences in aggression or in any other response. As demonstrated in the laboratory studies by Herrnstein (1961) and many others, there simply was not a linear relation between response strength and reinforcement density. For each individual, the slope tended to become asymptotic at the upper levels of reinforcement. In keeping with this idea, our observation data from nursery schools showed no correlation between density of reinforcement and frequency of child aggressive behavior (Patterson, Littman, & Bricker, 1967). It is probably no coincidence that Skinnerians had long ago decreed that the study of individual differences was an exemplar of bad science.

We focused instead upon questions about reinforcement that could be answered with available technology. For example, why does one kind of coercive behavior, such as arguing, occur at much higher rates than does hitting? Additionally, why is that coercive behaviors occur at higher rates on some days or in some settings as compared to others? These questions led us to what we call the stimulus control studies.

The Stimulus Control Studies

Our first entree into the homes of clinical cases suggested that much of the conflict behavior was reflexive and not under direct cognitive processing. The conflict bouts between family members had all of the overlearned qualities one finds in observing someone drive their car. As we examined the sequential family interactions we could begin to see why this might be so. It became apparent that there was a surprising similarity across problem children in the networks of stimuli that controlled their coercive (e.g., noncompliance, temper tantrums, hitting) behavior (Patterson & Cobb, 1971; 1973). These overlearned patterns of action and reaction were run off repeatedly. We determined empirically what the controlling stimuli were by carrying out endless lag one analyses that described the immediate impact of one person's behavior on that of another (Patterson, 1977a; Patterson & Cobb, 1971; 1973).

When observing in the home, it was apparent that the moment-by-moment changes in rates of deviant behavior were determined directly by the controlling stimuli and only indirectly by changes in reinforcement. Is the sibling there and is the sibling teasing? Presumably, intraindividual fluctuations in rates of deviant behavior over time were determined by variations in the density of controlling stimuli (Hops, 1971; Patterson, 1977). Calculating fluctuations in density of controlling stimuli and the concomitant fluctuations in density of deviant behaviors was an onerous task. But the findings provided immediate support for the hypothesis. For example, Hops (1971) showed that across days the density of controlling stimuli correlated significantly with the density of social behavior. The correlation was .50 for one boy and .59 for another. Comparable data for a single problem child with over 50 observation sessions produced a multiple correlation of .61 (Patterson, 1973).

We also examined the question of why some coercive behaviors occurred at higher rates than others. The hypothesis was that behaviors with the highest relative rates of negative reinforcement would also have the highest rates of occurrence. For example, Patterson (1982) showed that the likelihood of negative reinforcement (summing across subjects and time) correlated .59 with likelihood of occurrence for seven coercive responses in one sample and .93 in another sample. The greater the relative payoffs (in negative reinforcement) were, the greater was the relative rate of occurrence.

The findings were greeted with indifference. It seemed clear that it is a tactical error to provide answers to questions that no one has bothered to raise. Nevertheless, as authors of this forgotten prophecy, we still find it intrinsically interesting to know that the more extreme forms of coercion occur less often than the less extreme forms and that frequency covaries with density of negative reinforcement.

The stimulus control studies assisted us in making sense of our parent-training efforts. For example, knowing that many of the coercive conflict bouts were on "automatic pilot" suggested that one an important function of parent-training therapy is to get the various steps in family conflict under direct cognitive control.

Negative Reinforcement

We knew by the early 1970s that coercion was the key mechanism by which family members train each other to be aversive and aggressive. We also knew that negative reinforcement defined this process. In coercive, dyadic process, one or both members use aversive reactions to exert short-term control over the other. The theory details the means by which these short-term effects produce long-term increases in pathology. We could see that being coercive was often functional in terminating conflicts among family members. What was frustrating was that knowing was not the same as being able to prove that what we saw was actually a causal variable. We reacted by designing a series of experiments to test the relevance of parental negative reinforcement in altering prosocial and deviant behaviors (Devine, 1971; Patterson, 1982; Woo, 1978). The experiments offered strong support for the idea. However, the question remained as to whether that was the way the process actually operated in homes. How much of individual difference variance could negative reinforcement account for remained unanswered for another decade.

Building Macro and Micro Models

The theory building and interventions might have rested here forever but for pressure exerted by S. Shah in the NIMH section of crime and delinquency. In the late 1970s, it was strongly suggested that our future depended upon our ability to both intervene and explain delinquent behavior. We were encouraged to address directly the problem of treating delinquent behavior. It can safely be said that sociologists were less than delighted by our sudden appearance in their territory. However, we applied for and eventually were funded to design a treatment study appropriate for chronic offending adolescents. At the same time, we were funded to design a passive longitudinal study that would begin with fourth grade boys and their families living in high-risk sections of our small metropolitan center.

We knew that we did not have the intellectual capital required to build a microsocial performance theory of delinquency. As noted earlier, we could not measure the negative reinforcement occurring in families (Patterson, 1982). Also, we could not measure the reinforcement for aggression supplied by peers (Patterson et al., 1967). In lieu of our inability to solve these critical problems, we decided to move to the next level of variable. This level consisted of parenting variables, which we assumed would control the reinforcing contingencies supplied for prosocial and for deviant behavior. We decided that the key to this effort would rest on our ability to adequately measure parenting variables and child aggressive outcomes. We also decided to invent more powerful measures of such parenting skills as discipline, monitoring, family problem solving, involvement, and support. Prior efforts to measure parenting skills using monoagent and monomethod approaches had not been successful. The findings simply did not replicate (Schuck, 1974).

The NIMH under the leadership of Shah was extremely supportive and eventually funded a 2-year pilot study so that we could solve the problem of how to measure the complex parenting skills, child adjustment, and contextual variables. For each of the 13 key concepts in the coercion model, we planned to use indicators based on reports from multiple agents and methods. This strategy would make it possible to test models of delinquency based on modern structural equation modeling. The measures from the planning study were revised and tailored for use in the longitudinal Oregon Youth Study. The first wave of data was collected at the fourth grade level and used to test the parenting models as summarized in *Antisocial Boys* (Patterson, Reid, & Dishion, 1992). Multimethod and multiagent measures of discipline and monitoring accounted for from 30% to 50% of the variance in latent constructs measuring antisocial behavior. The outcomes of applying this measurement strategy to three different longitudinal samples provided one of the data base for much of the present volume (e.g., chapters 3, 4, 5, 6, and 7).

Context

In the early 1980s and through the 1990s, we worked on the impact of context, such as divorce, social disadvantage, parental stress and depression, and antisocial behavior, on child adjustment. How did context impact family processes? Was the contribution of context to deviancy direct or indirect? We assumed that context influenced child outcomes only to the extent that parent and child interactions were altered (Patterson, 1983). The studies strongly emphasized the mediational role of parenting practices. For example, it was assumed that boys in divorced families evidenced problems only if good parenting practices were disrupted (Forgatch, Patterson, & Ray, 1996). The mediational role for parenting practices seemed to work for both intact and for transitional families (intact to single parent, etc.; Bank, Forgatch, Patterson, & Fetrow, 1993). Many of the well-known contextual variables seemed to load on a single factor as shown by Capaldi and Patterson (1994). The studies of context are reviewed in chapter 6.

A Developmental Model of Delinquency

We discovered that an understanding of delinquency required that we study two very different trajectories (Patterson, DeBaryske, & Ramsey et al., 1989; Patterson, Capaldi, & Bank, 1991). One path would be characterized by preschool antisocial behavior, followed by early arrest, and then chronic and violent juvenile offending with the eventual outcome as a career adult offender (Patterson, Forgatch, Yoerger, & Stoolmiller, 1998). The longitudinal data showed that 71% of all the chronic juvenile offenders had moved through all the prior points in the trajectory (childhood antisocial, early arrest, and chronic offending). This implied a single path to adult career offending. Furthermore, the data also showed that each of the points in the juvenile trajectory was maintained by the same mechanisms. The shared mechanisms were disrupted parenting, socioeconomic status and transition frequency. The extent of movement in the progression was determined by the level of involvement with deviant peers. This implies a single theory will explain all of the points on the juvenile trajectory.

The second path began in late adolescence and had a set of determinants significantly different from those that held for early-onset arrest (Patterson & Yoerger, 1997a). The eventual outcome was transient juvenile offending and no greater risk for adult offending than one would find for juvenile nonoffenders. The details of the early- and late-onset models are presented in chapter 7.

Individual Differences in Reinforcement by Family and Peers

It was the mid-1990s before we were able solve the problem of applying reinforcing contingency variables to the individual differences problem. After working at OSLC as a postdoctoral fellow, Jim Snyder was a regular consultant at OYS during the early

1980s. Our discussions often had a tendency to drift back to the unsolved problem of individual differences and reinforcement theory. As a result, we did develop a better procedure for using sequential observation data to determine whether a consequence functioned as a reinforcer in a natural setting (Snyder & Patterson, 1986). We eventually struggled through some of the ideas in the matching law (Davison & McCarthy, 1988). The laboratory procedures were obviously much too constrained to serve directly as a metaphor for our problem. Rather than just two response levers, we were studying up to 27 different behaviors in our code system. Unlike the laboratory procedures, there was no fixed supply of reinforcers in the natural environment (e.g., if response A is reinforced, that does not reduce the supply available for response B). Nor are family interactions governed by a fixed variable interval schedule.

What did apply from the matching law studies was the idea that reinforcement must be examined at the intra-individual level. This would require that observation data be collected, not just for the coercive event and the reinforcement provided, but also on the payoffs accruing to the whole range of social behaviors that occur in that setting.

We believe now that the central reinforcement provided by families occurs during family-conflict episodes (Patterson, 1982). "How well does coercion work during family conflict?" is the *wrong* question. Reframing it from the intra-individual perspective, the question becomes "How well does coercion work during family conflict compared to everything else the child does during family conflict bouts?" What is the *relative* rate of reinforcement for child coercion during family conflict bouts?

This formulation led to the pivotal publication by Snyder and Patterson (1995). We showed that knowing the relative payoff for child coercion in terminating the conflict was correlated with the relative rates of occurrence for coercive behaviors associated with these bouts. The rates of coersion also predicted the child's rates of deviancy observed a week later. If we then added how frequent conflicts or training trials occurred, we could account for over 60% of the variance in individual differences in deviancy.

Snyder and his colleagues went on to replicate this effect by using an OSLC treatment sample of boys and girls (Snyder, Schrepferman, & St. Peter, 1997). The analyses of relative payoffs for coercion during family conflicts plus density of conflict accounted for significant variance in predicting police arrest 2 years later. The Snyder studies are summarized in chapters 4 and 5 of this volume, including their application to peer reinforcement for deviant behavior. The work has also been extended in the recent analyses of negative reinforcement to long-term outcomes in the randomized trial for the divorce study (see chapter 11).

Snyder and his colleagues showed how the process of selecting friends is related to the individual's disposition to maximize immediate payoffs. Deviant children select deviant peers. Deviant peers reinforce each other for deviancy (see also Dishion, Andrews, & Crosby et al., 1995; Dishion, Spracklen, Andrews, & Patterson, 1996b). Peers who maximize the child's immediate payoffs get selected as friends. The message is that the child is not just a passive recipient of what the environment offers. Rather, the child actively selects an environment and in the process actually shapes much of it to maximize the payoffs (e.g., the child is the center of a very dynamic system that he or she, in part, creates). The selection of deviant peers insures the maintenance of deviant behaviors as well as the development of new forms of deviancy.

Our studies show that the extremely antisocial 10-year-old is likely to be one of the first to be out on the streets, unsupervised by adults (Stoolmiller, 1994). The analyses of videotaped interaction for antisocial and nonantisocial dyads by Dishion and his colleagues generate interaction data that again fit a matching law analyses (Dishion, Andrews, & Crosby, 1995). The findings show that antisocial boys are mutually reinforcing for rule-breaking talk, and that this talk predicts both later delinquency and later substance use. The deviant peer metamorphosis takes place in a microsocial matrix. These findings are reviewed in chapters 5 and 7.

H. Hops and his colleagues have carried on a fascinating application of coercion theory to the study of depression (Hops, 1992). It can be seen from these studies that one function served by depressive symptoms is to have a powerful impact in actually altering the context in which the depressed individual exists. One implication of the studies reviewed by Hops in chapter 8 is that the relative rates of reinforcement for depression may account for significant variance in depressive outcomes.

Interventions: Change in Center in Clinical Policy

To this point, our research in understanding, measuring, and intervening with conduct problems was centered in middle childhood—a developmental period with which we had a good deal of experience. Our primary focus was to understand the processes that occurred within troubled families and how to change them. During the 1970s, the therapy components had been steadily changing to accommodate the omissions that characterized these problem families. It was true that the parents tended to be noncontingent, but they were also not involved; and they were very inadequate at tracking or monitoring the whereabouts of their child. Each new problem became a crisis; their family problem-solving skills were practically nonexistent. We also added a school-achievement component to the intervention (e.g., school card, homework site, and time in home).

During this time, we also tried to push our interventions as far as we could and began working with older and with more severe cases. This strategy worked fairly well until we basically hit the wall in two studies that we began in the late 1970s. In one, we conducted a randomized trial with a sample of chronic and serious adolescent offenders (averaging over six offenses at intake) referred by the juvenile courts (Bank et al., 1991). We compared our parent-training model to an individually focused therapy intervention conducted by juvenile department probation officers. The second project was an attempt to train child welfare caseworkers to use the parent training approach with families who had been referred for abuse and neglect Fleischman (1982). It was at this point, as we were trying to export our interventions into the community, when our theory-based interventions started to falter. It took us awhile to understand that we needed to expand our underlying models developmentally into adolescence and into settings outside the family.

In the adaptation of the parent-training model for working with adolescent delinquents and their families, we used the same sets of direct parent-training techniques, as before but included a stronger emphasis on parental monitoring. The intervention and follow-up presented many obstacles and took nearly a decade to complete. We found that the parent-training condition (PT) produced better outcomes in terms of rates of subsequent arrests than the individual therapy provided in the control intervention (Bank, Marlowe, et al., 1991). We felt that, clinically speaking, we felt the effects on the parent-adolescent relationships were extremely weak. Most of the youngsters in the PT group continued delinquent activity, though at lower cumulative rate than the controls. We were convinced that, by itself, parent training was not sufficient as a treatment for chronic delinquents. it remained for Chamberlain (2000) to add one of the missing pieces. Moreover, the intervention was extremely demoralizing to the therapists, and we concluded that though it produced superior results in terms of subsequent arrest rates, parents were presented with problems that were much more complex than parents of younger children; they were more apathetic and demoralized and were resistant to intervention. These families were less cohesive, and the parents had significant mental health and substance use problems of their own.

About this same time, we began another initiative where we trained caseworkers in three protective service branch offices to use and test our parent-training procedures (Fleischman, 1982). Only a two-week training was provided. No means were provided for close supervision within the staff. The intervention format fit with neither the administrative structure nor the professional styles of the social workers at the agency. We had simply failed to find a niche for parent training within the existing structure. These experiences convinced us that we needed to better understand community contexts before we could integrate our programs into existing community services, and more fundamentally, that we needed to rethink our overall research paradigm of doing basic research to identify intervention targets, and then hatch carefully controlled efficacy trials in our institute environment.

During these first 15 years, we had made substantial headway and encountered grave difficulties. We had developed a theory-based intervention that looked promising for working with latency-aged aggressive children and their families. We had developed an innovative measurement system and had begun to develop a research staff that was focusing on increasingly complex methodological issues. We were attracting talented young researchers who wanted to collaborate with us and be mentored at our center. During the next 10 years, we refocused on basic research on theory and methodological development. During that period, we made substantial progress on several problems that turned out to be central to our overall aim of developing effective interventions across the young life course.

Developing Interventions Across the Developmental Continuum (1980–1990)

Theoretical and Methodological Progress

During this period, we devoted most of our efforts to expanding our knowledge and models of the variables and processes involved in the development of conduct problems, serious delinquency, and drug use during childhood to adolescence. After some pilot work and careful reviews of the existing developmental studies of conduct problems and antisocial behavior (Loeber, 1982; Loeber & Dishion, 1983), we initiated two longitudinal studies in 1983: Fagot began with toddlers of both genders; Patterson began the OYS with fourth grade boys. Although these two studies were begun independently (Patterson's at OSLC, and Fagot's at the University of Oregon), Fagot moved her research to OSLC in the mid-1980s, thereby adding early developmental expertise and a valuable data set that would bear fruit for us in the 1990s when the subjects in her data sets approached the ages of Patterson's sample when he began. Fortunately, these two research groups had collaborated on the development of multimethod assessment batteries, a number of direct observational coding systems for home, school, and lab settings. During this period, our developmental models expanded dramatically in terms of age spans covered and types of variables and contexts in which they were studied.

Our central focus remained the further understanding of the relationship of problem child behavior to the moment-to-moment social interactions in which the developing child was involved, but other factors received attention as well. The social interactional processes of children with persons other than parents, such as teachers (Fagot, 1981; 1984), siblings (Patterson, 1984c), and peers (Dishion, 1990; Patterson & Dishion, 1985) were studied and incorporated into developmental models of conduct problems and depression (Patterson, 1990). We also studied parent characteristics as they related to parent-child interaction and antisocial outcomes (e.g., Forgatch, 1987; Patterson, 1980; 1982; 1986a), as well as social and economic disadvantage (Larzelere & Patterson, 1990), family stress (Patterson, 1983), divorce and separation (Forgatch, Patterson, & Skinner, 1988). We studied processes other than microsocial exchanges within the family, such as problem solving and negative emotion (Forgatch, 1989); social perception, attribution, and negative parental biases (Holleran, Littman, Freund, & Schmaling, 1982; Reid, Kavanagh, & Baldwin, 1987); attachment classification (Fagot & Kavanagh, 1990); and parental supervision (Stoolmiller, 1990). We also conducted studies of the consistency of child aggression across social settings (Dishion, 1990; Harris & Reid, 1981; Loeber & Dishion, 1984).

In addition to working on research of developmental processes in multiple contexts, we conducted studies on the relationship of key family processes and the development of conduct problems to skill deficits (Dishion, Loeber, Stouthamer-Loeber, & Patterson, 1984), drug use, (Dishion, Patterson, & Reid, 1988), school problems, (Ramsey, Patterson, & Walker, 1990), and depression (Patterson & Capaldi, 1990).

The study of these processes and context during the 1980s led to the testing of a new generation of more comprehensive and complex developmental models that began to provide insights on how our interventions needed to be changed and refocused if we were to be able to develop interventions across development and settings (Baldwin & Skinner, 1989; Patterson, 1982; 1986; Patterson & Bank, 1986; Patterson, Dishion, & Bank, 1984).

As the modeling studies in the late 1980s indicate, we were able to move from a total emphasis on naturalistic observation data in family settings to a more balanced multiagent, -method, and -setting assessment system. This involved increasing our reliance on standard report measures used in this area. We developed our own parent daily-report instruments (Chamberlain & Reid, 1987), global rating scales for use by independent observers and interviewers (Weinrott, Reid, Bauske, & Brummett, 1981), latent constructs that defined a large number of variables (Capaldi & Patterson, 1989), and more sophisticated observational systems for use in multiple natural and laboratory settings (Chamberlain, 1988; Dishion, Crosby, Rusby, Shone, Patterson & Baker, 1989; Reid, 1982; Reid, Baldwin, Patterson, & Dishion, 1988). We were able to move to more complex biostatistical models during this period with the additions of Bank and Stoolmiller to our research teams.

Developing Intervention Capacity

We continued to work with families who were experiencing severe child behavior problems, family stress, and multiple personal and legal problems. In addition to the families of chronic delinquents described above, we worked with families of young children referred for serious physical abuse (Reid, Taplin, & Lorber, 1981) and families who were screened on the basis of extreme levels of child problems, parental resistance, and family stress (Patterson, 1985). Families in these groups were enmeshed with other agencies (and often in multiple service or treatment programs; or involved in litigation over termination of parental rights), and as a group, these families were extremely challenging. At the clinical level, we learned a great deal about making our parenting interventions relevant in a variety of contexts and constellations of problems, how to get parents to focus on improving their interactions, and how to increase their motivation to work on their parenting (Patterson, 1985; Reid, 1985). We used home-observation data to compare the patterns of microsocial interactions between the abusive parents and their children to those in our previous studies with less distressed families. We found more intense, but very similar, basic parenting processes in these more difficult families (Reid & Kavanagh, 1985). Observational methodology was developed to study the process of family resistance to parent training and to identify therapist and intervention characteristics that were associated with high and low resistance. To this end, we developed a coding system for quantifying key interactions between parents and interventionists, for examining the relationship between resistance and child outcomes, and for improving systems for measuring treatment fidelity during sessions (e.g., Chamberlain, 1988; Chamberlain & Baldwin, 1988; Chamberlain & Ray, 1988; Patterson & Forgatch, 1985).

Although we improved our intervention techniques during this period and, in retrospect, our interventions with these extremely difficult families were somewhat effective (Patterson & Forgatch, 1995), in the early and mid-1980s we were still operating without a comprehensive developmental model to guide us. Importantly, as was the case in our work with families of older, chronic delinquents, we were still not well integrated into the community service networks in which these families were embedded. We were still trying to work with other agencies without a base of mutual self-interest, understanding, respect, and cooperation.

Developing Interventions in the Community

At the same time that we were conducting our longitudinal research and our controlled interventions, we still pursued our interests in family-based approaches to delinquent teenagers. An opportunity arose in 1983–1984, because the state of Oregon decided to downsize the state training schools. The plan was to release all but the most dangerous delinquents into community-based programs. Chamberlain wrote a proposal to the state to offer specialized foster care as an alternative to incarceration. The proposal was developed not around the table at our weekly seminar, but with the relevant state agencies. The theoretical proposition was that skillful parenting might be effective in helping delinquents if the parenting was done by fresh families who were not demoralized by years of failure, not angry at the youngster, not socially stressed or economically disadvantaged, and who had a good family support system. The notion was to distinguish the acts of parenting from the person doing it. The plan was to recruit strong families and train the parents in noncoercive parenting, intensive supervision, and good problem-solving skills. The plan was also to develop a case management approach for each youngster in which parole officers, social and mental health workers, and educators collaborated with the foster parents to separate the youngster from the delinquent peer group and activities, and for the youngsters to begin succeeding in school and normative extracurricular activities. At the same time, the youngsters' own family was helped to deal with whatever situational or mental health problems they were having, and they were encouraged to use the time away from their youngster to learn better skills and strategies in preparation for his or her return home. Aside from the novel attempt to use the same family process model for a very different context and with different adults as providers, the new piece was the development of the intervention in collaboration with the community agencies that were intimately involved in providing services to the youngster. Data on reduced rates of aggressive behavior or better school attendance were not high on the priority lists of the community partners. Fundamental questions were: Could service be offered for less cost than institutionalization, could the community be protected, and would the intervention prevent reincarceration to the training schools? The intervention program was developed and revised continuously over the subsequent 10 years until it became an accepted and respected program in the state. It got consistently good marks on annual state audits of costs and recidivism. In addition to leading rather quickly to the development of a very promising alternative to incarceration (e.g., Chamberlain, 1990), it gave us an additional and compatible perspective and strategy for developing interventions.

Our previous work had been organized around a rather traditional model. First, do basic epidemiological and longitudinal research to build a developmental model of the disorder or problem. Second, use that model to design an experimental intervention that precisely targets the most powerful and malleable antecedents and mediators. Third, carry out a highly controlled randomized trial. Fourth, replicate it if possible. Fifth, disseminate the intervention via a community randomized trial. This fifth step is often the stumbling block. We have come to label this as an inside-out approach. That is, we develop an intervention inside the controlled environment of a research or university facility, and then take it out to the community for dissemination.

The approach used to develop the Treatment Foster Care (TFC) intervention might be termed an outside-in approach. That is, although it is informed by a continuing basic-research base, its actual development was conducted in the community context. In that way, it should be possible to deal with most of the obstacles that block the transfer of technology from research to applied settings. Rather than later having to decide continuously which parts of an experimental intervention can be adapted to the needs of a community and which cannot, one can begin to deal with those issues from the beginning. Even if the specific parameters of service structures vary from community to community, the variance within the class of communities is probably less than that between research and community settings. In addition, there is the issue of credibility. It has been useful to us to have prospective community collaborators check us out with parole officers, teachers, or protective services workers with whom we have worked in the past.

After the TFC program was well integrated into the community, and after it appeared to be a useful intervention in the overall context of community services, Chamberlain brought the intervention back into the research environment to develop an assessment strategy and a randomized intervention design. She then prepared a research grant to fund the research aspects of the project. This strategy of community collaboration and development was to become a continuing feature of much of our work in the next 10 years. Not only was it an effective strategy for developing a series of treatment foster care interventions, but the community-based building and partnerships developed would make it easier to take our center-based (inside out) interventions into the community.

Expansion of Scientific Methodology, Developmental Models, and Interventions (1990–1999)

By the late 1980s, we had developed a reasonably coherent and plausible model of the development of conduct problems from middle elementary school through mid high school (Patterson & Bank, 1989). We had made substantial progress addressing methodological problems that allowed us to define constructs with multiple indicators across agents, methods, and settings (Capaldi & Patterson, 1989; Patterson, 1986; Patterson & Bank, 1986, 1987) and use structural equation modeling techniques to frame tests of our theoretical and intervention models (Bank, Dishion, Skinner, & Patterson, 1990; Bank & Patterson, 1992; Patterson, Bank, & Stoolmiller, 1990). We were also beginning to support the assumption that the same family and contextual mediators involved in the development of conduct problems were involved in the development of substance use, school failure, and depressed mood. Beverly Fagot's research program was tracing the development of social behavior from toddlerhood into school entry, but the links between early social interaction within the family and child conduct problems were not yet established. Evidence was accumulating that the same sorts of parenting interventions that we were using with older youngsters were equally or more effective with preschool children (Webster-Stratton, 1985; 1990). This was not surprising to us because there were enough longitudinal data for us to make some guesses about the early development of coercive parent-child interaction, about its sequelae when the child entered school, and about the additional challenges of relating to the social and behavioral demands of the classroom and peer group. Indeed, on the intervention side, Dishion and Patterson (1992) found age effects in our early clinical data from the 1960s and 1970s that clearly indicated that parent training worked better for younger than older children.

In broad brush strokes, the 1990s at OSLC might be described as an explosion of intervention and theoretical work on a variety of levels, and this activity was fueled by the conception and funding of the Oregon Prevention Center in 1990.

Throughout the 1990s, we had a number of interventions being tested in the field, all driven by the same basic theoretical models, using similar multimeasure assessment strategies, assessing overlapping mediators and outcomes. We had refined our parent-training techniques and adapted them for children at different developmental levels and for families living in different situations and contexts. Parent training continued to be the centerpiece of our interventions, and we used our longitudinal work to target different aspects of parenting for youngsters of different ages.

By 1990, when we were writing our first Oregon Prevention Center proposal, we understood the importance of intervening early in the developmental cycle, though most of our intervention experience had been with children from middle childhood to middle adolescence. In fact, during the 1990s there has been an increasing portion of our intervention work focused on the preschool and school entry years (Fisher, Ellis, & Chamberlain, 1999; Forgatch & DeGarmo, 1999; Reid, 1993; Reid, Eddy, Fetrow, & Stoolmiller, 1999), while continuing to address intervention issues with older children and adolescents (Chamberlain & Moore, 1998; Chamberlain & Reid, 1998; Dishion & Andrews, 1995; Dishion, Andrews, Kavanagh, & Soberman, 1996; Dishion, Kavanagh, & Kiesner, in press). Our intervention strategies now address children's behavior problems from about age 3 to age 18, and across family contexts such as single mother, stepfather, and foster homes, as well as homes with antisocial siblings and antisocial girls.

As part of the variety of interventions we have been funded to develop, we have conceptualized intervention trials as experimental longitudinal studies and have carefully collected assessment data across all of these trials using our multiple-method and multiple-agent technology. Most of the assessment batteries for these interventions include observational data collected in one or more settings of home, classroom, playground, and laboratory. Furthermore, all interventions are manualized, which enhances fidelity; fidelity checks are also built into each intervention strategy.

Findings from our intervention and longitudinal studies were beginning to indicate that there is an orderly progression of potentially malleable and developmentally linked antecedents and mediators across accessible social domains. This suggested that there are many powerful and potentially malleable antecedents at many developmental points and in many domains over the early life course. This led us to seriously question our singular emphasis on clinical strategies to deal with full-blown conduct disorder and to consider the early life-course prevention strategy. That is, it might be feasible to use our emerging model to target antecedents and mediators of conduct problems as they became potent over the course of development. Rather than seeing early oppositional and later conduct and substance use disorders as clinical entities in the 1990 Prevention Center proposal, we conceptualized these clinical phenomena as parts of a developmental trajectory, in which poor outcomes at earlier points were antecedents for poor outcomes at later points in development.

For example, difficult infant temperament or maternal depression and family stress are antecedents for subsequent poor outcomes such as coercive parent-child interactions. These coercive interactions are, in turn, malleable antecedents for subsequent outcomes at school entry, such as poor peer and teacher relations in the first grade; these school factors become malleable antecedents for academic failure and truancy, which are, in turn, antecedents for association with delinquent peers, poor supervision, and so on. Both the developmental continuity of antisocial behavior and its antecedents and mediators across time and contexts and the fact that contextual factors (e.g., divorce, job loss) could introduce new and powerful risks for poor child adjustment at any time in development forced us to reconsider our clinical, relatively nondevelopmental approach and conceptualization of intervention. In the context of such long-term developmental trajectories, the traditional 1- or 2-year follow-up studies of interventions were appearing more and more inadequate. Thus, we have sought and been successful in securing funding to continue to collect long-term follow-up assessments for much of our intervention work, and we are continuing with that strategy. In addition to providing critical data on the malleability of specific behaviors and long-term sequalae of these interventions, these follow-up data will be incredibly helpful in understanding the cost effectiveness of particular interventions undertaken at various points along the developmental trajectory.

We have dealt with a number of very difficult issues during the 1990s, including both intervention and theoretical challenges. For example, Dishion's Adolescent Transitions Program (ATP) randomly assigned families to four groups: parent training, adolescent skills, both parenting and adolescent skills, and a biblio-video control. Much to our surprise, the teenagers in the adolescent skills groups enjoyed their weekly sessions but also showed significant increases in drug and alcohol use (Dishion, McCord, & Poulin, 1999; Poulin, Dishion, & Burraston, in press). Dishion and his colleagues concluded that putting youngsters with conduct and substance problems together in group-treatment settings provided regular weekly opportunities to establish relationships with other troubled youth, and the intervention did, in fact, produce an iatrogenic effect. Chamberlain and Reid (1998) found similar iatrogenic effects for delinquent adolescents randomly assigned to a group-home setting as compared to foster homes in the community. Boys subjected to the group-home intervention as compared to TFC were supervised less well, associated regularly with deviant peers (with no adult supervision), and were arrested more often. These results taught us that interventions with problem and delinquent teenagers should not be developed for group settings, including group sessions, camps, and other recreational, academic, or peer related activities. This result has now been replicated by Joan McCord using the Cambridge-Summerville data (Dishion, McCord, & Poulin, 1999).

Also on the theoretical level, we responded to work conducted by behavior geneticists suggesting that parenting interventions are unlikely to account for outcome variance in children's and adolescents' development (e.g., Plomin & Daniels, 1987; Rowe, 1994; Scarr, 1992). These reports and claims are serious and have motivated OSLC investigators to establish a twin sample in Oregon, to investigate assessments of observed versus reported twin behaviors, and to examine closely the methodology of the published twin and adoption studies. Results from observation data indicate that, there is far more variance accounted for by environmental variables in the prediction of children's maladaptive behaviors than previously suggested in the literature. It also seems that the estimates of variance accounted for by genetic factors are probably inflated (Leve, Winebarger, Fagot, Reid, & Goldsmith, 1998; Stoolmiller, in press).

At a practical level, we needed to develop the data management and biostatistical capability, expertise, and methodology to deal with large longitudinal data sets and to develop and test complex developmental models. During the 1990s, we have developed our capabilities with growth modeling, using visualization and latent growth modeling techniques in particular. Our associations with Hendricks Brown and Bengt Muthen and the Prevention Science Methodology Group have been pivotal and extraordinarily helpful in these endeavors. Also emerging from those associations has been our ability to adapt missing data technologies for use with our large longitudinal data sets (Duncan, Duncan, & Stoolmiller, 1994; Patterson, 1993; Stoolmiller, 1995; Stoolmiller, Duncan, Bank, & Patterson, 1993).

As our theoretical structures and biostatistical capabilities have matured, we have needed to continually upgrade and improve our data management and direct observation technologies. Currently and for the last several years, we have been in the process of moving to a completely digitalized video and video coding system. Our programming department has already created software to take advantage of the dramatically reduced processing times for locating specific images and codes. For example, finding each episode of a child's negative behavior followed by a parent's negative response over a series of home observations or laboratory tasks can be accomplished in minutes as compared to hours for locating the same segments on videotapes.

Our analysis strategies with observational data have also improved sharply with Stoolmiller's use of individual observation sessions or lab task segments as indicators for an observation latent construct (e.g., negative behavior chain). This technique has substantially increased reliability and validity of observation-based assessment (Stoolmiller, Eddy, & Reid, 2000).

Implications

Modern dynamic theories of child development say that to understand aggressive children, we must look for answers within the child (i.e., their attributions, their internal representations). The micro- and macrotheories plus the intervention strategies outlined in this volume all say otherwise. If we are to change aggressive childhood behavior, we must change the environment in which the child lives. If we are to understand and predict future aggression, our primary measures will be of the social environment that is teaching and maintaining these deviant behaviors. The problem lies in the social environment. If you wish to change the child, you must systematically alter the environment in which he or she lives.

This not the end of the journey; it is more like an early draft. But what is described in this volume is at the very least a workable theory about where child aggression comes from and how to change it. The fact that the same variables found in the theory also drive the intervention and prevention procedures should make the theory even more interesting. It is a theory of aggression that works.

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