

**THE LIMITS OF FAMILY INFLUENCE:
GENES, EXPERIENCE, AND BEHAVIOR**

**THE LIMITS
OF FAMILY INFLUENCE**

Genes, Experience, and Behavior

DAVID C. ROWE

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INTRODUCTION

Most people believe that different rearing experiences have something to do with differences in the way children turn out. Parents who want bright children are told to read to them, and encouraged to take them to a library rather than to a roller-skating rink. Parents are warned to be affectionate, lest a child become worried and anxious; in the parlance of socialization science, the child may develop feelings of “low self-esteem.” Parents are held to blame, or hold themselves to blame, when a child develops a psychological disorder such as schizophrenia. In our cultural beliefs, the idea that family experiences mold a child’s life course is strongly endorsed—that is, “As the twig is bent, the tree grows.” A social scientist opposing this cultural belief would be dismissed as uninformed and possibly dangerous. In response, many people would recount stories from their own lives. Social scientists would mention the massive research literature showing influences of rearing on behavioral development. Nonetheless, many societies once accepted a flat earth; both experts and cultural beliefs, on some occasions, may be wrong.

This book is about socialization science, which is the empirical effort to understand how children acquire traits from their families and cultures. It proposes the radical theme that one part of this process—broad differences in family environments, except for those that are neglectful, abusive, or without opportunity—may exert little influence on personality development over the life course. The book holds that the environmental variables most often named in socialization science (e.g., social class, parental warmth, and one- vs. two-parent households) may be devoid of causal influence on such child outcomes as intelligence, personality, and psychopathology. The major evidence supporting this thesis originates in behavior genetic studies, some old, others completed since the end of World War II. This book seeks to inform socialization

science with what behavior genetic studies can teach us about environmental influence.

The Nature–Nurture Debate

In science, good experimental design is paramount. A well-designed experimental study would vary important variables systematically. Suppose a scientist were to study the effects of both a drug and psychological therapy on depression. He or she would design a study with at least four groups: (1) drug and therapy together, (2) therapy alone, (3) drug alone, and (4) no treatment. Other groups might be added, such as a placebo drug group, but the four groups listed would be essential. If the scientist were to design a study with just two groups, drug and psychological therapy together compared with no treatment, and then to conclude, “Look, psychological therapy works!” because the treated group improved, the research would be dismissed. Even if the scientist completed 10 similar demonstrations, skepticism would remain. After all, might not the treatment effect be attributable to the drug rather than the therapy, if the scientist looked *only* at their combined effects?

Yet just this blatant error exists throughout many studies of socialization. The workhorse of socialization studies is the family study research design, in which associations are sought between family environmental variation and developmental outcomes in children. According to this research, families that provide more intellectual stimulation have (on average) brighter children; families that are affectionate and provide supervision have (on average) less delinquent children.

In all these family studies, however, the research design is flawed. As in the hypothetical scientist’s experimental study described above, in the family research design something has been left unaccounted for: namely, variation in genes that parents and children share. In psychological traits, genes may be one source of parent–child resemblance; they also may be one source of variation in family environments. Which is it that influences a child, heredity or family environment? To discover an answer, social scientists must separate the relative influence of genetic and environmental variation, using appropriate behavior genetic research designs. Asking for this effort is not being extremist or advocating genetic determinism; it is just asking for well-designed studies of environmental influence.

To some social scientists, though, the influences of “nature” and “nurture” are deemed inseparable. No organism could develop, this argument goes, without the workings of both nature and nurture, so that they appear as intertwined as threads in a cloth. Or, in Harvard psychologist Jerome Kagan’s (1984) beguiling metaphor, they are like water transforming from liquid to ice: The formation of ice crystals cannot be apportioned partly to the inherent properties of water and partly to the external change of temperature. In *The Nature of the Child*, Kagan disparaged attempts to separate nature and nurture as profoundly misguided: “Some social scientists have accepted the unprofitable and misleading dichotomy between biology and experience, in part, because they inherited statistical models that assume that influences due to endogenous forces can be separated from those due to exogenous ones” (p. 10).¹

Kagan’s freezing metaphor may apply to water, but it is untrue of nature and nurture, because understanding the growth and development of a single individual has been confused with understanding the origin of different traits in a population (Plomin, 1986, p. 7). Mary Lou Retton did not grow into an Olympic champion gymnast either without eating her Wheaties or without her DNA. Nonetheless, among the millions of well-fed Americans, we know that *variation* in most physical traits (such as height and weight) is attributable to DNA variation. We regularly observe parent–child resemblance in physical traits, and as verified in twin and adoption studies, this family similarity results from family members’ genes (Grilo & Pogue-Geile, 1991). Height and weight are not behaviors, of course. But the principal question remains the same for both physical and behavioral traits: “What influences variation in a population more strongly, genetic or environmental variation?”

Problems with Socialization Science

Although this book emphasizes the separation of genetic and environmental influences, it is also concerned with the separation of the environmental influences themselves. This latter separation can say something about “how” environmental influences operate. Some environmental influences are tied to the family unit; they can be described as “shared” by different family members, operating to make them alike in different behavioral traits. Other environmental influences touch each individual in some unique way; we call them “nonshared” environmental influences.

As defined above, shared environmental influences operate to make the members of a family (i.e., siblings, parents and children) alike. On the basis of this definition, the operation of shared influences becomes a *hypothesis* for personality resemblance in families. If we find that schizophrenic parents are more likely to have schizophrenic children than other parents, or shy parents are more likely to have shy children, or intellectually bright parents are more likely to have bright children, this hypothesis of shared family environmental influence is likely to be invoked. This book's thesis is that shared environmental influences do not create the many observed behavioral similarities in families. Thus they cannot explain why children have such different developmental outcomes (i.e., variation in normal or pathological traits among families). In this way, the influence of family environments on children's behavior has been overestimated, and is actually more limited than many people would believe.

Shared influences correspond to many specific environmental variables mentioned as developmental explanations for children's traits. Any rearing behavior that is relatively uniform for all siblings in a family, but different for children in other families, qualifies as a possible shared influence. Such family characteristics as the level of intellectual stimulation in a home, parents' emotional expressiveness, their disciplining styles (to the extent that these are alike for siblings), parental beliefs about politics and religion, family structure (e.g., whether parents are divorced or married), parental use of legal and illegal drugs, and many other rearing variables all generally operate on children as shared influences. That is, siblings are more equally exposed to them than would be children raised in different families, who thereby are exposed to different family environments. These preferred variables of socialization science are themselves called into question by the book's thesis that shared family environments have little effect on developmental outcomes.

Nonetheless, not all family environmental influences are "shared" by family members. For example, parents may favor one child over a brother or sister—a kind of parental favoritism that may spark rivalry among siblings. Therefore, not all parental treatments can be equated theoretically with a notion of "shared" family environmental influences as defined here. The book's thesis (i.e., limited shared influences) is not, then, a disproof of *all* effects of parental behaviors on children's development—because nonshared treatment effects may exist even if shared rearing effects do not.

In Chapter 5, possible nonshared effects of child rearing are considered in greater detail. Although research on nonshared parental treatments is fairly new, I do not believe that it will upset this book's basic thesis that family rearing effects (and other family-tied environmental influences) are, on the whole, limited in their effects on children's developmental outcomes. Nonshared environmental influences constitute an entire class of environmental effects; as such, they include many influences that are not tied to particular parental behaviors—all the way from accidents of embryological development that make nervous systems different (even for identical twins) to vicissitudes of experience that are somehow unique, depending on who is affected, when, and by how much. Along with siblings' genetic differences, these other forms of nonshared environment, rather than specific parental treatments, may explain why siblings are so different from one another in behavior.

The nature–nurture debate, then, may end on a discordant note. Social scientists can accept both environmental and genetic influences as important in behavioral development; no one today advocates either complete biological or environmental determinism. Still, socialization science may miss entirely which experiences are influential for personality development, and in many cases these may be experiences we cannot grasp to change our children's lives.

This book gives evidence for this conclusion and explores its many implications. The weaknesses of socialization science constitute the topic of Chapter 1. Chapter 2 provides the technical "know-how" for interpreting behavior genetic studies. The absence of family-tied environmental influences on personality and psychopathology is discussed in Chapter 3. Chapter 4 looks critically at human intelligence. Chapter 5, "Uniting Nature and Nurture," argues that the measures we label as environmental (including such central ones as social class) may hide genetic variation. Chapter 5 also covers the subtle issue of nonshared parental treatments. The next chapter deals with one kind of group difference rather than with individual differences, covering the lack of rearing influence on behavioral sex differences. In Chapter 7, the book moves beyond empirical evidence to speculation, considering why variation in family environment has so little effect on personality development.

The main conclusion is that socialization science has placed too heavy an emphasis on the family as a bearer of culture. Human learning capacities and biological reproduction enable two modes of trait transmission—one through cognition and social learning, and another through genetic heredity. But the former mode, learning via cognition

and imitation, is an extremely general and robust one because people can acquire information from many sources. One source, experiences in the family, may diminish in importance as exposure to other sources increases. Nothing said here reduces the importance of the cultural (but nonfamilial) transmission of beliefs, technologies, social practices, and those behavioral elements that blend into traits. But before we arrive at this more differentiated view of cultural and genetic influence, we need to examine how simple assumptions about family influences may fail. This is the task of Chapter 1.

Note

¹At the same time, in his recent writings, Kagan has been deeply appreciative of inherited behavioral tendencies: "Temperament has become the preferred name for the variety of initial, inherited profiles that develop into different envelopes of psychological outcomes. An infant's temperament renders some outcomes very likely, some moderately likely, and some unlikely—although not impossible—depending on experience" (Kagan & Snidman, 1991, p. 856).

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THE PRIMACY OF CHILD REARING IN SOCIALIZATION THEORY

Not long ago, an investigative TV team brought its hidden cameras into a day care center. Inside, they found all manner of neglect, even abuse, of the children placed there; incredibly, one young boy had been left to play alone with matches near a gas stove. Although only a small minority of day care centers have hurt children, a third party's concern for a child is almost never as deep as a parent's—whose love forms the bedrock of the family, the world's most enduring social institution. We know that parents will sacrifice more for their children than anyone else will. I accept it as virtually axiomatic that a parent's presence and concern is essential for a child's physical and emotional security.

The focal issue in this book is, instead, whether different rearing experiences shape differences in children's traits. The term "trait" refers broadly to enduring characteristics—intellectual characteristics, such as reading achievement and IQ; personality characteristics, such as sociability and anxiety; serious mental illnesses, such as schizophrenia; and finally, social attitudes and beliefs. I share with other behavior geneticists (Scarr, 1992) the position that parents in most working- to professional-class families may have little influence on what traits their children will eventually develop as adults. Moreover, I seriously doubt that good child-rearing practices can greatly reduce an undesirable trait's prevalence, whether it be low IQ, criminality, or any other trait of social concern.

To see the very real limits of parental influence, one need only be reminded of how remarkably different siblings are, even though they grow up together. In her book *Life Lines*, the late movie actress Jill Ireland wrote about her heartbreak over her adopted son, Jason, who was

a drug addict as a young adult. He was different from her in character even as an infant. Ireland wrote:

From the very beginning there was always a different energy emanating from him [Jason], one as unlike mine as chalk to cheese. His energy blew with an almost Eastern heat and sensuality, an energy that demanded instant gratification and satisfaction. He needed all my attention, something I gave with pleasure and love. (1989, p. 13)

As an adolescent, when her son Valentine was caught sneaking out of the house at night, he accepted responsibility and stopped breaking house rules. Jason, in contrast, left home for days at a time without telling his parents his whereabouts. Perhaps a socialization theorist would try to find some way in which Ireland was inept in her treatment of Jason, or think that Jason was somehow set apart as the family scapegoat. Yet I see in her book a deep concern for Jason, and Ireland made strenuous efforts on his behalf. So why was Jason so different?

Contrary to what Ireland had been disingenuously told at the time of Jason's adoption, he was not the biological child of an architect (a married man leading a conservative life) and an unwed mother. Instead, Jason's biological father had been a heroin abuser, and his mother had been hospitalized several times for psychiatric reasons. Ireland herself became convinced that the "lifeline" explaining how different Jason was from her biological children lay in the DNA of his natural parents.¹ This anecdote, or any other, raises but does not settle the issue of relative rearing and genetic effects; hence our need to turn to the science of child care. This chapter is the story of the weaknesses of socialization science. We know less about child-rearing effects than we think we do.

Parental Treatment Effects in Socialization Theories

The scientific justification for child-rearing influence spans three major theoretical perspectives: Freudian theory, early behaviorism, and social learning theory. All three perspectives emphasized the family context and parental treatments in their explanations for children's developmental outcomes. Freudian theory and Watsonian behaviorism, in particular, focused on early treatment experiences in infancy and childhood. Social learning theory has examined more the possible influence of parental treatments on older children and adolescents. Despite the widespread

influence of these theories, both in the past and at present, they each contain empirical and theoretical shortcomings in the insistence on parental treatment as the major shaper of personality and intellectual development.

Freudian Theory

In its most influential period from the 1920s to the 1950s, Freudian theory inspired both practical advice to parents and many research efforts by social scientists. Freudian ideas were widely interpreted to imply a primacy of early (infantile) experience for later personality development (Lomax, Kagan, & Rosenkrantz, 1978). Freud had described several psychosexual stages of development in which experience (combined with constitutional dispositions) molded personality development. The earliest stages involved breast feeding (oral stage) and toilet training (anal stage). Insensitive parenting in the oral period could lead to later anxiety disorders; similarly, inappropriate toilet training—mainly, forcing a child to control bowel movements at too early an age—could lead later to anxiety problems, or even to obsessional-compulsive personality traits. To avoid "traumatic" treatments that could result, in their view, in later psychiatric illnesses, the popularizers of Freud's theory advocated a relatively permissive regimen of child rearing.

Although Freudian theory was a major cultural influence, many social scientists never accepted it even during its heyday. Its internal contradictions, complex logic, and lack of readily observable variables kept strict Freudianism outside the realm of most empirical investigations. Freud's use of inaccurate 19th-century biological concepts, such as Haeckel's dictum that ontogeny (the life of a single individual) repeats phylogeny (the history of a species), also discouraged its use by 20th-century social scientists. Moreover, many social scientists distrusted Freud's empirical evidence because his conclusions about children were based mainly on retrospective inferences drawn from what adult patients had revealed in psychoanalytic therapy. Clinical data are more private and singular than behavioral data collected according to the scientific canons of replicability and observability. Freudian theory thus made inroads into the science of child development without ever dominating it.

Empirical research after World War II also brought central claims of Freud (or at least his interpreters) into question (Lomax et al., 1978). The primacy of early experience lost credence against findings such as

the observations of Guatemalan infants by Kagan and his colleagues (Kagan & Klein, 1973; Kagan, Kearsley, & Zelazo, 1978). As infants, these children were raised without much social or physical stimulation; their parents did not play with them, and their movements were restricted. Yet, despite a lack of parental attention to them as infants, by age 10 years the Guatemalan village children exhibited normal behavioral traits, and in most ways they were indistinguishable from children of similar age in Boston suburbs. Case histories of recovery from early emotional and physical traumas also weakened arguments for the primacy of early experience (Clarke & Clarke, 1976). Similarly, the strong claim about the importance of the mother as the sole figure for a child's attachment was also weakened by empirical observations. For example, the children of the *kibbutzim* in Israel made adequate social and intellectual progress, despite their multiple attachments and a lack of exclusive contact with their biological mothers (Lomax et al., 1978).

There is no need to belittle Freudian theory to see that *as a basis for a science of child development*, it is limited. At present, socialization science is not turning to literal forms of Freudian theory for explanation of psychiatric disorders or children's personality outcomes. Social science has moved in other directions.

Early Behaviorism

Tremendous enthusiasm for environmental effects arose in early behaviorism. In place of psychoanalytic clinical insight, behaviorism offered objective procedures—classical and, later, operant conditioning—to change observable behaviors. John Watson was at the forefront of the behaviorist movement that reacted against the subjective and mentalistic psychology prominent in his day. Watson wrote both for the general public and for the scientific audience. His advice that parents not hug or kiss a child may seem harsh by modern standards, but it reflected his concern that the levers of reward and punishment be applied systematically, and that care be taken not to spoil a child.

Watson proclaimed the environmentalist credo when he wrote in 1924:

Give me a dozen healthy infants, well-formed, and my own specified world to bring them up in and I'll guarantee to take any one at random and train him to become any type of specialist I might select—doctor, lawyer, artist,

merchant-chief, and, yes, even beggar-man and thief, regardless of his talents, penchants, tendencies, abilities, vocations, and race of his ancestors. I am going beyond my facts and I admit it, but so have the advocates of the contrary and they have been doing it for many thousands of years. Please note that when this experiment is made I am to be allowed to specify the way the children are to be brought up and the type of world they have to live in. (Watson, 1924/1970, p. 104)

In fact, the evidentiary basis for Watson's extraordinary statement was slim. His primary, and subsequently most frequently cited, demonstration of the power of conditioning to mold traits was a case study of a single infant, Albert (Watson & Rayner, 1920). The experiment, carried out some time in the winter of 1919–1920 (Samelson, 1980), involved classically conditioning Albert to fear a laboratory rat (hardly equivalent to producing a scholar or a criminal). Presentation of the rat was paired repeatedly with a noise stimulus from the striking together of two metal bars. According to Watson's report, after several conditioning trials Albert came to fear the rat when it was presented alone.

This demonstration—which even today is discussed in many social science textbooks—became hugely popular in spite of its evident weakness. The experimental procedures were sloppy; the baby's thumb had to be removed from his mouth during conditioning trials (Samelson, 1980). Watson himself never replicated the experiment. And, according to Hilgard and Marquis's (1940) authoritative work on conditioning, Watson's simple chaining concept of conditioned responses failed when applied to the prediction of complex habits.

A persisting weakness in general conditioning approaches to socialization is the avoidance of biological concepts. Watson advocated the use of animals in research, but paid no heed to the *continuity* of humans with other animals; he thus took no account of the possibility that biological instincts present in kin species might be present in humans as well (Degler, 1991). Animals were to be used because their behavior was overt (and thus countable and measurable), and also because animal studies avoided the nasty concept of consciousness, which the behaviorists sought to excise from social science. An arbitrary program of conditioning, they believed, should be able to produce any trait in any organism.

Since Watson's early behaviorism, research on conditioning has belied the independence of learning from instinct. Two students of the famous psychologist B. F. Skinner, the Brelands (Breland & Breland, 1961), provided a humorous demonstration of the limits of condition-

ing. They trained animals for circus acts using rigorous principles of operant conditioning, but despite their best efforts, trained animals often reverted to instinctive behavior patterns. For example, a pig, trained to carry a wooden coin to a "piggy bank" in return for a food reward, started rooting the ground in pig-typical food-searching movements; the rooting became so exhaustive and time-consuming as to ruin the act. In another classic study, the University of Wisconsin psychologist Harry Harlow (1971) demonstrated that monkeys preferred an artificial mother made out of cloth over one made out of wire with a bottle on it. They would go to the food-bearing "mother" when hungry, but they returned to the cloth "mother" for as much time as possible, and also retreated to it in response to fear. In the wild, monkeys cling to their mothers' soft fur in a species-typical protective response.

Sometimes biological dispositions aid classical conditioning. Garcia and Koelling (1966) discovered that rats would learn to associate a taste with a poison-induced sickness, but would not learn to associate the same taste with electric shock. As they anthropomorphized, "The hypothesis of the sick rat, as for many of us under similar circumstances, would be, 'It must have been something I ate'" (p. 124). The notion of biology-independent learning processes thus appears flawed.

Social Learning Theory

Social learning theory is a much more flexible and catholic approach to socialization than Watsonian behaviorism. Without either abandoning basic learning concepts or some elements of Freudian theory, social learning theory broadened the concept of socialization to emphasize humans' strong cognitive capacities.

Albert Bandura (1971) invigorated the theory of socialization with the powerful concept of "modeling," also called "imitation learning." His undeniable observation was that children can acquire many behaviors simply by watching others perform them. This learning process is inherently cognitive: Attention to a model and subsequent encoding of the model's behavior in memory are the crucial elements determining the extent of learning. For a child, attention to a parental model is dependent on whether the relationship of the parent and child is emotionally close and satisfying. It also depends on the power and control available to a parent, as more powerful models are preferred to ones with less power.

Bandura noted the independence of learning and performance. Behaviors were thought to be learned through observation, without requiring a pairing of the behavior with a reward. In one of Bandura's (1965) imitation of aggression experiments, children who watched an adult model kick and hit an inflated, adult-size Bobo the Clown toy were later able to reproduce the model's actions when playing with a similar toy. Children who did not spontaneously reproduce the acts, though, could be prompted to do so with a bribe of fruit juices. They also had learned the behavior, but had not displayed it for lack of any incentive to do so. Thus Bandura reasoned that observational learning should generally occur independently of reward, but that what is learned should be repeated only when incentive conditions are right. A concept of reward was believed to be essential to understanding socialization, because rewards and punishments should decide which of the many behaviors that a child has acquired will be performed. Bandura also noted that performance can depend on rewards to others (called "vicarious rewards"), as well as on rewards directly received.

Some social learning theorists also borrowed from Freudian theory. Early behaviorism had been very molecular in its approach, with each behavior having a separate history of shaping reward and punishment. In contrast, character traits seemed broader and more enduring than such a molecular approach would imply. The Freudian concept of "internalization" was used to resolve this dilemma. In this view, particular behaviors, after sufficient observational learning and reward, become ingrained as a behavior repertoire that continues despite temporary changes in reward conditions. More recent statements of social learning theory refer to cognitions such as "expectancies," which are lasting beliefs about the relation of behavior and reward outcomes. Because these cognitions do not change readily, they are "internalized" aspects of personality that loosen the control over behavior of daily reward and punishment.

Family Primacy?

Many elements of social learning theory seem to place primacy on socialization in the family. Parents seem clearly to be behavioral models. The aggressive parent who yells and hits a disobedient child models the use of violence as a solution to family conflicts. A parent who smokes or who drinks hard liquor models substance use. The social learning conditions

for strong influence effects are also thought to exist in the family context because parents exhibit the kinds of power and control that make models attractive, and, moreover, because children who love their parents should be receptive to their teachings. For these reasons, social learning theorists regard broad child-rearing styles, such as parental emotional acceptance versus rejection, as crucial for understanding the socialization process. In general, parents should be using their leverage over the child to shape socially desirable behaviors. The process of imitation nonetheless means that children may acquire parents' negative behaviors, despite the parents' wishes that they do not. Thus social learning processes should generally make children similar to their parents in behavior—clearly in the case when parents model socially desirable behaviors that they wish to see also in their children, but also in the case when parents display undesirable behaviors that are nevertheless effective within the family or in the broader society.

Although the family would appear to be a primary agent of personality development, the very universality of the social learning process raises a warning flag. If children readily learn by observation, then any person—another child, a teacher, a friend of the family—is also a potential source of influence. If vicarious rewards are used to evaluate a behavior's potential consequences, why should a child restrict his or her attention to those rewards received by siblings and parents? Later in this book, I argue that an understanding of cultural transmission processes reveals that parents may *not* be a child's primary behavioral models.

For now, let us consider the principal evidence supporting this conclusion: studies of biological children in two-parent families. These studies seem to provide a strong reason for social learning theorists to adopt a family-centered theory of socialization—namely, the ubiquitous parent-child similarity observed for behavioral traits, and the consistent correlation of rearing experiences with behavioral outcomes in children. There are literally thousands of studies verifying these kinds of "socialization" processes, so that on the surface, the evidence for the social learning model of parental effects would seem unassailable.

Research on Rearing and Child Outcomes in Biological Two-Parent Families

My purpose is not to review the whole literature on socialization. I have instead picked a few studies that serve to illustrate the data in which scientists place some faith.

Although extremes of child rearing have been examined in the empirical literature (Widom, 1989), in most studies of normal personality traits the families were comfortably middle-class and not criminally neglectful toward their children, and the children's traits fell well within the normal range. The child-rearing practices examined marked nuances within a range of normal concern and care for children.

Even in this slight range of child outcome and variation in child-rearing style, significant associations have been found between the two. One illustration comes from a frequently cited study by Baumrind (1967). In this study, the 32 children were enrolled in a university child study center. The parents were middle-class and well educated. None of the children were reported to have serious behavioral or psychological problems, and their mean IQ of 123 made them brighter on the average than about 94% of American children.

On the basis of behavior observations by child center staff members and psychologists, the children were divided into groups. In group I, the children were zestful, self-reliant, and explorative, the kind of well-adjusted kids any teacher would like; in group II, they were more unhappy and socially isolated; and in group III, they were more impulsive and immature. Parents of children in each group were then compared on a variety of measures involving observations of each parent with his or her child in a laboratory playroom and in the home.

For each group of children, different rearing experiences predominated. Group I's parents were both controlling and nurturant—a child-rearing style later labeled "authoritative." The authoritative parents mixed high maturity demands reasonably imposed on children of a particular age with love and affection. The parents of groups II and III were more like one another than they were like group I parents; in particular, they were lower in communication and affection. Group II parents used more control and less nurturance than group III parents (an authoritarian pattern, whereas group III parents used more nurturance and less control than group II parents (a permissive pattern). Remember, though, that none of these parents were likely to be child abusers, and none of the children were likely to be grossly pathological.

Studies of extreme populations show similar findings—imperfect statistical associations between a pattern of child care and children's outcomes. Widom's (1989) investigation of child abuse provides an example. In a matched case-control design, childhood victims of physical abuse and neglect were matched for age, sex, race, and class with children born at the same hospital at the same time, some 20 years ago. Widom wondered whether children who were themselves victims of

abuse and neglect would be more prone to later violent and criminal behavior—a cycle in which “violence breeds violence.” Widom vigorously searched state, local, and federal records for these children’s adolescent and adult outcomes. And indeed, abused and neglected children had a higher prevalence of behavior problems: 26% of the abused versus 17% of the controls had juvenile delinquency records, and 29% of the abused, versus 21% of the controls had adult criminal records. Thus a child who had been neglected or abused had about 1.4–1.5 times the risk of a control child for antisocial outcomes. The outcomes were more dramatic, and potentially more important for society, than the personality differences manifested by Baumrind’s (1967) children. Note, though, that about 70% of abused and neglected children failed to repeat the “cycle of violence” in their own lives.

Literally hundreds of studies have been conducted in the search for associations between intellectual experiences in the home and children’s IQ and academic achievement. Like Baumrind’s (1967) study, the great majority have examined data on biological families. These studies have given rise to “social class” as the nearly universal environmental explanation for behavioral differences among people who differ in income or occupational status. Although its historical roots can be traced, this explanation is so pervasive and influential in socialization science today that it is not commonly tied to a particular theorist or a single notable investigation. “Social class,” as an explanation of behavior, is as much a part of the *Zeitgeist* as heredity was at the turn of the century. In a later chapter of this book, a classic study by Christopher Jencks (1972) provides a starting point for investigating the role of genetic inheritance in accounting for social class differences.

In the field of criminology, a classic investigation, begun in the 1920s, was the Cambridge–Somerville Youth Study of adolescent boys (McCord, 1991). Counselors made home observations, which were used later to evaluate the child-rearing styles of both mothers and fathers. Criminal records were collected on the men when they were 45–53 years old. Child rearing was associated with adult criminal behavior. At the extremes, fairly large differences emerged between boys who had experienced good and poor child rearing. In the best families, just 11% of the retraced youths had adult criminal records; in the worst, three times as many (34%). As McCord concluded, “Competent mothers seem to insulate a child against criminogenic influences even in deteriorated neighborhoods (p. 411).” And she wrote of incompetent fathers “who undermine their wives, who fight with the family, and who are aggressive and provide models of antisocial behavior” (p. 412). Fathers’ behav-

ior had an apparently stronger association with adult outcomes than did mothers’ behavior.

Many forms of serious psychopathology are known to run in families. The risk of schizophrenia is about 1% in the general population, but 13% to the child of a schizophrenic parent; this we know on the basis of numerous Western European studies from 1920 to 1987 (Gottesman, 1991, p. 96). The risk of manic–depressive psychosis is about 0.5% in the general population (Tsuang & Faraone, 1990), but about 10 times greater to the child of a manic–depressive parent.

In an example of how psychoanalysts have historically have envisioned the etiology of psychopathology, the burden of responsibility for schizophrenia was placed on mothers: The schizophrenia-causing mother was thought to be overly restrictive and to engender dependency. In response, it was believed, a child would withdraw by becoming isolated from the family and friendless. Another influential theory of schizophrenia focused on communication problems between parents and children. To present an overly simple version of this “double-bind” theory, “Catch-22” messages from the parent, in which verbal messages contradicted nonverbal communications, were believed to produce disordered thought and emotion in a child. A double-binding parent might nonverbally elicit a loving hug from a child and then say, “Don’t touch me,” contradicting her own request for affection. In this double-binding explanation of schizophrenia, parental behavior reproduced schizophrenic-like behaviors in a milder form—so an influence of parental treatments would seem a natural inference.

Research on Authoritarianism

In the 1950s, Else Frenkel-Brunswik, a refugee from the violence of Nazi Germany, teamed up with three colleagues to investigate the determinants of anti-Semitism (Adorno, Frenkel-Brunswik, Levinson, & Sanford, 1950). Their study took place in California and included over 2,000 respondents, including, in addition to the “white rats” of personality studies (i.e., college students), public school teachers, nurses, labor union members, and prison inmates. The authors administered questionnaires on anti-Semitism, ethnocentrism, and political conservatism, and found that they all correlated positively with authoritarian personality (as assessed via the F or Fascism scale). Thus they felt confident in concluding that at the root of social prejudice is the personality disposition they called “authoritarianism,” a trait tendency to adhere rigidly to

authority and to reject democratic values—a core of personality that finds expression in disparate social prejudices. Authoritarian individuals expressed these attitudes in personal interviews (Brown, 1965):

Concerning Negroes: "They're very closely linked with the jungle. They're built for it." Concerning Jews: "Most all of them Jews talk about sex mostly, or beatin' a guy out of his money . . ." Concerning parents: "[They] always tried to teach me the right thing; being in prison is not my folks' fault." Concerning the determinants of human behavior: "If I ever did anything wrong, it was the Latin in me." (p. 495)

The authors of *The Authoritarian Personality* (Adorno et al., 1950) concluded that this trait and its (sometimes abhorrent) correlates arise because of child rearing. They thought that parents who are unsure of their own social status will adopt strict and punitive discipline styles. In their interviews with authoritarian and nonauthoritarian individuals, they confirmed the existence of stricter discipline and greater demands for conformity in the former's generally lower-class households. A psychoanalytic process was invoked to explain the transformation of punitive discipline into prejudice: Child victims of harsh and unreasonable discipline dare not direct their anger and frustration back at the disciplining parents, who have the power to hurt them further. The children's anger and frustration are instead displaced onto safer social targets (any minority group or foreigners will do), thus relieving the unpleasant emotions resulting from the psychological conflict of normal desires with harsh discipline. And the social approval of prejudiced parents may be earned at the same time.

In an explanation more consistent with current socialization theory than one based on Freudian psychoanalysis, social class differences can be invoked to explain the development of authoritarian beliefs. According to Brown (1965), instead of these beliefs' being products of intrapsychic conflict, lower-class status can lead to anxiety and insecurity because of actual competition among ethnic minorities, poor whites, and immigrants for jobs and social prestige. Authoritarian beliefs would then be ways of dealing with real threats to status and income. Certainly, the degree of authoritarianism increased greatly among Adorno et al.'s (1950) subjects as years of schooling declined: The least authoritarian persons averaged 14 years of education; the most, only 11 years. The authoritarians' admired heroes were Douglas MacArthur, Charles Lindbergh, and Henry Ford; the unprejudiced named Pushkin, Beethoven, Voltaire, and Freud. The second list would be more likely to be learned

through exposure to a college education. Yet, for the reasons discussed next, neither explanation of authoritarianism may be correct. In a later chapter, I discuss adoption data on the authoritarian trait.

Limitations of Socialization Studies

Not all studies of socialization share in the weaknesses of the examples just mentioned. Intervention studies employing control groups (ideally, randomized ones) can demonstrate child-rearing effects. Although such studies are few in number, they suggest that giving parents advice on child rearing can help some parents reduce the severity of problem behavior in children (Patterson, 1974; Patterson, Chamberlain, & Reid, 1982). Studies of socialization also excel at describing in detail the psychological processes underlying behavior. Consider Dodge and Coie's (1987) elegant demonstration that aggressive children tend to assume that others want to hurt them, when there is little real threat to justify their attributions, or Coie and Kupersmidt's (1983) demonstration that rejected children recreate their poor social statuses in new groups. Longitudinal studies can be methodologically stronger than cross-sectional ones, but even studies of children at two or more time points fail to avoid all weaknesses of single-time-point studies: Genetic influences can be dynamic and change over time, just as environmental ones do.

What is wrong with the collection of socialization studies described above? It is that they do not provide a shred of evidence about rearing experiences, because they have failed to eliminate the influence of genes.

Imagine that the National Institutes of Health were to hire someone to investigate the effects of two drugs with different active ingredients on childhood hyperactivity. Imagine further that our intrepid scientist were to find some hyperactive children and divide them into two groups—one group to receive *both* pills (drug A and drug B), and the other group to receive two placebo pills that tasted and looked like those containing real medication. Imagine that, lo and behold, the experiment worked: The hyperactive children calmed down and performed better in school. Would our intrepid scientist then approach the executives of a pharmaceutical firm and ask them to market drug A? Would the National Institutes of Health renew the grant?

The pharmaceutical firm's executives would turn the offer down. How could they be sure that the true active ingredient was the one in drug A, not drug B? Moreover, the National Institutes of Health would

drop the grant like a hot potato at the next funding cycle. How could this scientist have irresponsibly forgotten to administer different amounts of pill A at fixed levels of pill B, and different amounts of pill B at fixed levels of pill A, as well as the matched amounts? Such an abuse of science would be punished in an experimental study.

Somehow, this exact flaw is constantly excused in studies of socialization. Studies that show some degree of behavioral resemblance *in biological families*, or some degree of relationship between a child-rearing style and a child outcome *in biological families*, are consistently interpreted as though they automatically say something about socialization. They do not. Ten studies with a poor research design (i.e., one that confounds genes and environmental effects) do not tell us more than one. Like the experimental example, biological family studies confound one influence, family environment (drug A), with another, the genes shared by parent and child (drug B). If social scientists conclude some effect from such a research design, they must implicitly assume that heredity lacks any agency.

Parent-child resemblances for matching behaviors can be explained most easily through genetics. Genes possessed by the parent determine the parent's behavior via their organizing effects on the central nervous system. Some of the same genes—or, rather, copies of them—are possessed by the children and may produce the same traits in them.

But genetic effects also cloud the interpretation of child-rearing styles and child outcomes. The problem here is one of "spurious causality." Consider first that a child-rearing style is merely a behavior on the part of a parent. Even physical aspects of the home relate back to parental choices (e.g., of neighborhood, or of whether to buy an encyclopedia or use the disposable income for something else). If child rearing is a trait construct—more akin to a personality trait than to environmental influences independent of human origin—then genes can influence variation in child-rearing styles. And then copies of these same genes may produce a trait in a child that co-occurs with the particular rearing style, just because the gene effect in the parent is what social scientists call a child-rearing style and the gene effect in the child is what they call a personality trait. In the terms a statistician would use, the association of child-rearing style and children's traits can be "spurious" (noncausal). We know, for example, that the rooster's crowing does not cause the sun to rise; similarly, the conjunction of rearing style and behavior does not prove that one caused the other. A parent may hold the belief that reading to a child makes the child smarter, but this is

merely evidence of association. In McCord's (1991) and Baumrind's (1967) studies of rearing styles *in biological families*, and in many studies of intergenerational mobility *in biological families*, just this kind of confounding has occurred. Socialization theories may be on stronger grounds when they predict differences among family members—but even here genetics may be confounded with family environment, because (except for identical twins) biological family members also differ genetically.

Another problem is "self-selection" or "niche picking" (Scarr & McCartney, 1983). That is, people self-selected into groups may differ genetically as well as environmentally. In Widom's (1989) work, a comparison was made across two groups of biological families. In one set, there was an abusive or neglectful parent; in the other, there was no legal record of abuse (some control parents may have abused or neglected their children, but this effect would have worked against finding the differences that Widom reported). The groups were equated for age, sex, race, and parental social class, but they were intentionally different in child abuse histories. This comparison assumed no relevant genetic differences between groups. Yet such differences may exist, and they may contribute to the "cycle of violence." Unless we can be assured that the parents did not differ in heritable traits relevant to crime and delinquency, we cannot be assured that the family environmental interpretation of Widom's findings is the correct one.

Any time people select themselves into social categories, genetic and environmental variation may be confounded. Self-selection processes permeate societies as people move through them to find geographic locations and social roles most suited to their genetic dispositions. A clear example is the high prevalence of men with a homosexual orientation in San Francisco. No one thinks that something in the Bay Area water transforms heterosexual men into homosexual men. We recognize that the life of a homosexual man in Every Small Town, USA, is probably more oppressive and intolerant than one in San Francisco; we are not surprised that many homosexuals move from America's small towns into larger, more accepting communities like San Francisco's. If genes dispose individuals toward homosexuality (Bailey & Pillard, 1991; Bailey, Pillard, Neale, & Agyei, 1993), then they will be more prevalent in San Francisco than in Every Small Town, USA, because people migrate from small towns to the big city. These genes, and not the social environment of San Francisco, will partly account for its population's characteristics.

A third pitfall is that of child effects (Lytton, 1990). It is easy to see that parental treatments may respond to, as well as cause, a child's characteristics. The parent of a highly active son would be unwise to keep him cooped up indoors all day; the parent of a very shy daughter would not want to push her into a role in a school play; an active child's parent may have to be more assertive in discipline than the parent of a lethargic child. Child effects do not automatically correspond directly to genetic effects, just as they are not automatically linked to socialization processes. The two explanations are certainly compatible, because parents may be reacting to inborn traits. On the other hand, any environmental source of a child's trait could have the same net effect: The child's characteristics drive the parental response.

In *The Broken Cord*, Dorris (1989) describes adopting a Native American child with fetal alcohol syndrome, a child whose nervous system was damaged in his mother's womb. Before the syndrome was recognized, Dorris spent years of quiet frustration trying to understand why his adopted son, Adam, performed so poorly in school, and trying to find compensatory academic help. Like many people, he was quick to blame social biases in IQ testing for his son's failure:

I all but dismissed the [IQ test] results. . . . I noted that the WISC [Wechsler Intelligence Scale for Children] was "in significant part culture biased" in favor of "mainstream America"—as if Adam, the son of a Dartmouth professor, living in Cornish, New Hampshire, came from some exotic society. While allowing that, at age ten, such terms as "alike/different," "older/younger" were confusing to Adam, I brought all my anthropological mumbo jumbo into play in denying the accuracy of his scores. (1989, p. 112)

Reluctantly, Dorris later came to admit that the WISC test scores had been right. Although Adam continued to pass from grade level to grade level, "the further on paper Adam got ahead, the further he fell behind" (1989, p. 113).

Where Does Environmental Influence Start and Stop?

On long sailing voyages, fruits or fruit juices were carried and eaten or drunk regularly to prevent scurvy, a dietary deficiency of ascorbic acid that results first in loosening of teeth and bleeding gums, and ultimately

in death. The disease was checked in 1795, when the British government began issuing a regular dose of lime juice to sailors in the Royal Navy. Beyond dietary minimums, however, there is no solid proof that megavitamin doses will either delay aging or improve fitness (Nobel Prize winner Linus Pauling thinks that they can cure the common cold, but the scientific community is skeptical). Over a wide range of ascorbic acid levels, then, the health effects are essentially equivalent: Eating one orange a day is as good at preventing scurvy as eating five oranges a day.

Family environments also exist over a wide range, both physically (in terms of housing, food quality, and community safety) and emotionally (in terms of emotional closeness and supervision of the child, as opposed to the most hideous acts of parental abuse and neglect). One could clearly imagine that some family environments are similar to normal nutrient levels—people vary greatly in their environmental exposures, but all exposures within a wide range are functionally equivalent in their effects on psychological functioning (Scarr & Weinberg, 1978)—and that more extreme environments may produce consequences as harmful to the psyche as scurvy is to the body.

Our conclusions about environmental effects, then, may depend on the range of environments considered. To some scholars, the very existence of this problem shows the futility of attempting to compare genetic and environmental effects quantitatively. The psychologist Lytton (1990) concluded that child effects predominate in the causation of conduct disorder (fighting and serious opposition to parental discipline). In a critical reply to Lytton published in the same journal issue, Dodge (1990) answered skeptically: "This conclusion [that child effects predominate] is unfortunate because it follows from a question that need no longer be asked in this field" (p. 698). Dodge continued:

The relative strength of effects can vary greatly depending on the sample, variance of measures, and the pattern of distribution of scores. In a population in which environmental variation is great (such as an urban area in which poverty and wealth coexist), environmental effects might overwhelm child effects. In cases in which environment variation is relatively small (such as when a society has provided at least adequately for all of its members), child effects might appear larger than the environmental effects. (1990, p. 698)

Dodge's reply to Lytton is, of course, correct, but his proposed solution of abandoning quantitative estimates is practically and theoretically bankrupt. A politician may want to know whether changing the

environment in a working-class home earning \$31,000 1990 dollars per year to be more like that in an upper-middle-class home will make a difference in child outcome. Some 40% of the U.S. population is working-class, so if changes from working-class to middle-class family environments make a big difference, then many children could potentially be saved from social failures. If only children in deep poverty are affected by environmental change, this is important too. In this case, though, far fewer children could potentially benefit from the intervention, because children in severe poverty constitute only a small fraction of all children. Moreover, once these gains were realized, no further gains would be forthcoming. The point is that to know the family environmental effect for any environmental context, one must make numerical estimates that control for the influence of genes. Admittedly, behavior geneticists have been remiss in not exploring more fully how estimates of heritability and environmentality vary with social context; however, analytic methods exist for this purpose, and they can be applied to address how genetic influences on conduct disorder vary with social class (see Rowe & Waldman, 1993). The views (1) that heritabilities are *necessarily* nongeneralizable, and (2) that population-specific estimates have no policy relevance, are both mistaken.

The social learning processes of modeling and imitation should work as well in a restricted environmental range as in a broad one. There is nothing in social learning theory to say that *only* children in poverty learn by imitating their parents, but that rich ones somehow learn in some other way. I have chosen Baumrind's (1967) study earlier to illustrate precisely this point: that the families in many socialization studies have been middle-class—that this research has focused on bright and eager children free of the pains of poverty. If social learning does not work for middle-class children, this is important to know—and socialization science must answer why. Socialization science should also explain where along the threshold of family environments socialization effects begin and end. And if these effects are rare over a broad range, then our current theories must be abandoned and replaced with better ones.

Socialization science should ideally produce information about the whole range of environmental contexts. Yet information about environments to which reasonable numbers of children are exposed would seem more important than information on tiny numbers at great extremes. Furthermore, information from many animal and human case studies does not seem particularly relevant, because these examples represent very rare events. In a best-selling sociology textbook (Bassis, Gelles, Levine, & Calhoun, 1991), the necessity of socialization was demon-

strated with two case histories of extreme child abuse reported by Davis (1940, 1947)—children who had been raised in nearly total isolation during their first 6 years of life. Both children were placed in foster care after they were discovered. Of the two cases, one girl never recovered fully and died young; the other child eventually made a complete recovery. That nearly “feral” children have at least initial social and emotional problems is not a surprise. The *National Enquirer* notwithstanding, the number of such severely abused children produced in Western industrialized societies is very small. We should not turn to their case histories for answers about socialization in more typical contexts.

Although animal studies produce remarkable effects, their relevance to human socialization science is also unclear. Cats raised in complete darkness have trouble seeing—but what child is so raised? The Wisconsin psychologist Harry Harlow and his colleagues conducted a classic study of rhesus monkeys raised in their cages in an isolation as complete and sterile as solitary confinement, with no contact with their own kind (the separation was initially effected to reduce the chance of disease transmission in the monkey colony, and was only later used as a scientific method). The isolation-reared monkeys were extremely fearful of other monkeys and had confused sexual responses later (Harlow & Novak, 1973). Total isolation rearing, without sight or sound of any living thing, belongs in league with the most severe child abuse; in modern industrialized societies, anything like it happens very, very rarely.

In any case, the exact lesson to be drawn from Harlow's study is unclear, because Harlow later discovered that the severe effects of isolation rearing could be reversed. The monkeys were placed with younger monkeys who had been reared normally. Although fearful, the isolation-reared monkeys allowed the younger ones to approach and play with them. Sometimes an isolation-reared monkey was paired with just one normal monkey; at other times, a form of “group therapy” was tried, in which two isolation-reared monkeys were placed together with two normal ones. In isolation-reared monkeys so treated, normal social play and sexual behavior were restored to the degree that “experienced primatologists seldom differentiate between the normal and isolate monkeys during sessions involving social contacts and interchanges including vigorous play” (Harlow & Novak, 1973, p. 469). That species-typical behaviors return even when disrupted by severe experiences is a chink in the armor of socialization theory.

Socialization science needs to know what the limits of family influence are. The existence of flaws in past socialization research does not imply that socialization processes cannot be properly researched. Some

studies of socialization have been attuned to the necessity to control for inheritance of trait-determining genes. A study of schizophrenia has sought family environmental influences on adoptees instead of biological children (Tienari et al., 1991). A study of nonshared environmental processes combines the expertise of behavior geneticists and environmentally oriented researchers: It involves sampling twins, full siblings, half-siblings, and unrelated siblings (stepsiblings), with the latter two groups coming from families formed after previous divorce (Reiss et al., in press). There are many similar opportunities for behavior geneticists and environmentally oriented researchers to combine their areas of expertise. These efforts should lead to changes in theories of socialization that reflects the extent and limits of family influence; they should also lead to future theories of behavioral development that integrate knowledge about genetic and environmental influences of all kinds.

In later chapters, attention is paid to who was sampled in various studies as a way toward establishing the environmental context for a given conclusion. In behavior genetic studies, the range is often broader than Baumrind's (1967) sample of middle-class professional families. Many twin and adoption studies include good numbers of children in working-class families—the sons and daughters of electricians, factory workers, truck drivers, and the like. The range represented in Widom's (1989) study is broader still. Most behavior genetic studies probably underrepresent victims of child physical or sexual abuse, or children in rural or urban poverty; unfortunately, these cases are not so rare that socialization science can afford to ignore them. The claims advanced in this book are mainly limited to working-class to professional-class contexts.

In the next chapter, I discuss identifying the influences of rearing while avoiding the pitfalls of genetic confounds. The importance of this endeavor cannot be overemphasized. Much socialization science is based on studies that make no attempt to separate nature and nurture; much socialization science may thus be misleading in direction and emphasis, if not just plain wrong. Chapter 2 introduces basic biological and genetic concepts, so that the reader may learn these methods and be able to make independent judgments of the evidence.

Note

¹Sadly, Jason later committed suicide. This example is not meant to imply that most adoptions turn out poorly; most infant adoptions turn out well for both the infant adoptees and the families.

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SEPARATING NATURE AND NURTURE

In a *New Yorker* cartoon, a pair of identical twins is shown reunited, sitting in the waiting room of a patent office, with identical Rube Goldberg inventions upon their laps. In actual cases, reunited twins are known for striking coincidences in their lives. Reunited twins Jim Lewis and Jim Springer were separated at 4 weeks of age and met for the first time when they were 39 years old (Chen, 1979). They had first wives named Linda and second wives named Betty; named their sons James Alan and James Allan, respectively; and named their dogs Toy. They worked as part-time deputy sheriffs in two different towns and pursued woodworking as a major hobby. As children, they had both liked math and disliked spelling; as adults, they had similar smoking and drinking habits. Coincidences? Perhaps so, as many boys are poor at spelling and many fathers want their names carried on. On the other hand, in the Minnesota study of twins reared apart, reunited fraternal twin pairs produced few such stories, whereas many examples of amazing similarities came from biographies of reunited identical pairs (Lykken, McGue, Tellegen, & Bouchard, 1992). This chapter reviews research designs for separating the effects of nature and nurture.

Variability

Social science seeks the causes of behavioral variability. Some children learn to read before first grade, others later. Some men and women are homosexual, others heterosexual. Shyness, impulsivity, honesty, and many other character traits vary enormously among individuals. In an early study of famous men, Francis Galton (1869), a pioneer of behavior genetics, sought to understand variability in social accomplishment—that