the Ecology of Human Development

EXPERIMENTS BY NATURE AND DESIGN

Urie Bronfenbrenner

The Ecology of Human Development

The Ecology of Human Development

EXPERIMENTS BY NATURE AND DESIGN

Urie Bronfenbrenner

HARVARD UNIVERSITY PRESS

Cambridge, Massachusetts, and London, England

Printed in the United States of America
10 9 8 7

Library of Congress Cataloging in Publication Data

Bronfenbrenner, Urie, 1917-
The ecology of human development.

Bibliography: p.
Includes index.
1. Child psychology – Research. I. Title.
[DNLM: 1. Human development. BF713 B869e]
BF722.B76 155.4 78-27232
ISBN 0-674-22456-6 (cloth)
ISBN 0-674-22457-4 (paper)

To those who taught me most—
my parents,
my wife,
my children,
and my grandchildren

Foreword

Goethe, who commented wisely on so many aspects of human experience, said of our attempts to understand the world

> Everything has been thought of before,
> The difficulty is to think of it again.

To this I would add (supposing that Goethe also said something to this effect, but not having discovered his discovery) that ideas are only as important as what you can do with them. Democrites supposed that the world was made up of atomic particles. Aside from his error in overlooking the implications of assuming that all atoms move in the same direction at the same rate, his astute guess about the atomic structure of matter did not have the same impact as Rutherford's rediscovery (with cloud chamber in hand) in 1900. In short, an idea is as powerful as what you can do with it.

Approximately one hundred years ago a number of scholars began to think that it would be possible to understand human psychological processes by conducting experiments, modeled on the precision and explicit, quantitative, data-analytic techniques that had propelled the physical sciences to such prominence in human affairs. Wilhelm Wundt is usually given the credit for this idea, although the *science* of psychology was born almost simultaneously in universities located in Germany, London, Cambridge (Massachusetts), and Kazan (U.S.S.R.).

What has been lost in our textbook accounts of the history of psychology is the fact that a great many other scholars who were around when psychology embraced the laboratory were not especially moved by the new enterprise. We tend to forget that Wundt himself believed that many psychological mysteries were beyond the reach of experimental methods, a belief not always shared by his

more zealous followers. Even before dissension began to appear in the ranks of those who followed in Wundt's path, more serious reservations were voiced about the utility of laboratory techniques for explaining our inner workings. Wilhelm Dilthey was an early and eloquent critic of Wundt's "new" psychology. Dilthey, after long deliberation, concluded that psychology should give up its quest for general laws of human psychological processes. Instead, he advocated that we strive for a *descriptive* psychology that would capture the unique complexity of the individual with all of its idiosyncrasies. Dilthey believed that by reducing the complexity of human nature to carefully measured reaction times or minutely detailed introspective reports, Wundt and his followers accomplished little more than the interment of human psychological processes in a crypt fashioned of brass instruments.

Dilthey's position has not prevailed in academic psychology, and for good reason. His very enticing view of adequate psychological description has never satisfied us as a model for complete psychological analysis. The infinite tangles of past experience and present circumstances that make us what we are smother us in particulars, defying explanation or generalization; faced with such complexity, any plausible simplifying procedure can appear to be a lifeline.

Recognizing psychology's limitations, we joke that Henry James was the great psychologist, his brother William the novelist. Lamenting psychology's limitations, we nonetheless expect a proper scientific discipline to provide us with more systematic information about ourselves than a novel can. Lacking such a rigorous discipline, we have followed Wundt's narrower path in our methods, but the limitations of theory imposed by that choice do not rest easy. We are faced with the paradox of a successful science that tells us precious little about the concerns that beckon us to it. Those who engage in psychology as professionals either come to terms with its limitations or become bored with neat experiments, the significance of which remains too often obscure. Finding no promising alternatives, many choose inaction.

Although there have been many changes in the particulars of psychological theory since the time of Wundt and Dilthey, the two extreme approaches that generated the schism between descriptive and explanatory psychology in the first place have prevailed, as have their differences in sophistication of methods and acceptance as disciplines. Wundt's structuralism gave way to new schools of scientific psychology, each complete with its own structured, systematic, and constrained models and methods: Gestalt psychology,

functionalism, behaviorism, and (most recently) experimental, cognitive psychology. Dilthey's criticism of this continuing effort to build a "nomothetic" psychological science has been rediscovered repeatedly, most recently in the humanistic psychologies of the late 1960s and 1970s, but each time without the crucial analytic tools for descriptive analyses or the power to explain what it describes.

Some few among psychology's practitioners, even in the earliest days, sought ways to link the descriptive and explanatory approaches, recognizing in this schism the seeds of psychology's undoing as a discipline. For example, in the early decades of the twentieth century it was common, especially in Germany, which gave birth to both movements, to encounter discussions of the "crisis" in psychology, for which various authors proposed various solutions. Coming on the heels of a decade of social and scientific activism in the 1960s (in which he took an active part) Urie Bronfenbrenner's work represents the continuation of efforts by this small, heterogeneous, but significant group of psychologists to overcome the "crisis" in psychology by constructing a discipline that is *both* experimental *and* descriptive of our lives as we know them.

His themes are those which concern everyone who hopes that psychology will shed light on our experience. The promise he offers us is very enticing. Psychology need not choose between rigor and relevance. It can do more than explain "strange behaviors in strange places." If properly pursued, it can tell us how those strange places and strange behaviors relate to the mundane contexts we refer to as our "everyday lives."

Professor Bronfenbrenner urges upon us his concern with specifying what people do in a way that will generalize beyond the contexts of our observations. He emphasizes the crucial importance of studying the environments within which we behave if we are ever to break away from particularistic descriptions and contentless processes. In both these concerns, he follows in the footsteps of very able predecessors.

But what should lead us to believe that Bronfenbrenner's prescriptions will succeed when the work of men whose ideas he has built on (Kurt Lewin, for example) seems to have disappeared—sunk into the sands of time or so absorbed into our collective folk wisdom that it is no longer extractable for purposes of analysis? The answer lies in his specification of procedures that are enough like what we already do to make them comprehensible, yet different enough to provide a better approximation to real-life phenomena.

Almost everyone who has read about psychological experiments

has had occasion to puzzle over their meaning. Are Stanford students sadists or craven cowards as their behavior in Zimbardo's prison experiments suggests? Are people slaves to authority who would willingly inflict harm on helpless fellows as the Milgram studies of compliance tell us? Are people really indifferent to strangers in distress? Can IQ tests possibly tell us about the value of day care?

To each of these and many other questions Bronfenbrenner gives us the only honest answer imaginable—the same answer his grandmother would have offered had he been able to discuss these questions with her—"*it all depends.*" In technical language, "it all depends" translates into the idea that the explanations for what we do (assuming we achieve serviceable descriptions) are to be found in interactions between characteristics of people and their environments, past and present. As Bronfenbrenner says, "the main effects are in the interaction." He would also follow Kurt Lewin in suggesting that if we want to change behavior, we have to change environments.

All of these commonsense suggestions entail a reorientation of the way we think about psychological processes, which must come to be treated as properties of systems, systems in which the individual is but one element. These ideas will succeed if Bronfenbrenner has (to paraphrase him) irked and goaded enough able scholars by his audacious assertions into trying to prove him wrong. Systematic challenges, even if they should disable his specific assertions, would constitute success. These are ideas worth having again and again until we are ready to exploit their power. When that day arrives, psychology will become a unified science of human behavior.

Michael Cole

University of California, San Diego

Preface

In writing a volume of this kind, one becomes keenly aware that science is indeed a community of scholars. We stand on the shoulders of giants, and mistake the broadened vision for our own. In this instance the giants are Kurt Lewin, George Herbert Mead, Sigmund Freud, William I. and Dorothy S. Thomas, Edward C. Tolman, Lev Vygotsky, Kurt Goldstein, Otto Rank, Jean Piaget, and Ronald A. Fisher. From these I learned mainly by reading. There are others who struggled to teach me, often against resistance. Chief among them were my first teachers in psychology, Frank S. Freeman, Robert M. Ogden, and Walter Fenno Dearborn. Lauriston Sharp introduced me to cultural anthropology, Robert Ulich to philosophy, and Harry C. Carver to mathematical statistics and experimental design.

But the seeds of the ecological conceptions developed here had been planted long before I entered college. It was my good fortune to have been brought up on the premises of a state institution for those who were then called "the feebleminded," where my father was a neuropathologist. Along with his medical degree, he had a Ph.D. in zoology, and he was a field naturalist at heart. The institution grounds offered a rich biological and social terrain for his observant eye. There were over three thousand acres of farmland, wooded hills, moss-covered forest, and fetid swamp—all teeming with plant and animal life. In those days the institution was a functioning community; the patients spent most of their time out of the wards, not just in school classrooms but working on the farm and in the shops. There were cow, horse, pig, sheep, and chicken barns, a smithy, carpenter shops, a bakery, and a store house from which food and goods were delivered around the village in horse-drawn farm wagons driven by the inmates. All these activities are

gone now—struck down by the courts as involuntary servitude.

That was the world of my childhood. My father took me on innumerable walks, from his laboratory through the wards, shops, and farmland—where he preferred to see and talk with his patients—and even more often beyond the barbed wire fence into the woods and hills that began at our doorstep. Wherever we were he would alert my unobservant eyes to the workings of nature by pointing to the functional interdependence between living organisms and their surroundings.

I remember especially vividly his anguish when the New York City courts would commit to our institution, out of error or—more probably—sheer desperation, perfectly normal children. Before he could unwind the necessary red tape to have them released, it would be too late. After a few weeks as one of eighty inmates in a cottage with two matrons, their scores on the intelligence tests administered as a compulsory part of the discharge process proved them mentally deficient: that meant remaining in the institution for the rest of their lives. There was a way out for these children, but the opportunity did not arise until they were much older. One of the places to which adult female inmates would be assigned to work was in the homes of staff, where they helped with housework, cooking, and child care. In this way, Hilda, Anna, and others after them became de facto members of our family and significant figures in my upbringing. But they seldom stayed for long. Just at the point when as a result of my mother's training in homemaking and their own everyday initiative they had become indispensable, my father would arrange for their discharge, for they could now pass the critical minimum on the all-determining Stanford-Binet.

It was a long time, however, before these concrete experiences were reflected in conscious ideas about an ecology of human development. These first began to emerge in an informal but intensive year-long weekly faculty seminar conducted thirty years ago. Ambitiously, my colleagues and I had sought to chart new horizons for theory and research in human development. The group included, among others, Robert B. MacLeod, Alexander Leighton, and Robin Williams. It was they who shook the intellectual foundations of a young investigator wedded to belief in the rigor of the laboratory and of psychometric methods. They opened my eyes to the power both of phenomenology and of social context. My knowledge of the latter was broadened in the course of three decades of collaborative research with my colleague Edward C. Devereux. To the two Charles R. Hendersons, father and son, I owe a continuing debt for lessons in the elegance and ecological adaptability of Fisherian designs.

Two sets of experiences gave form and substance to the new perspectives I had acquired in the faculty seminar. The first involved conducting field research in a cultural context. At first it had little impact on me, for with unconscious self-protectiveness I had chosen to work in familiar social terrain—a small rural community in upstate New York. Then my fellow seminar member Alexander Leighton persuaded me to join him for a summer as he began his now classic studies of community factors affecting mental health. It was under his tutelage, on the French coast of Nova Scotia, that I began a career of cross-cultural research in Western and Eastern Europe, the U.S.S.R., Israel, and elsewhere, including a mind-shattering glimpse of the People's Republic of China.

Experience in these societies had two profound effects on me that are reflected in the present volume. First, it radically expanded my awareness of the resilience, versatility, and promise of the species Homo sapiens as evidenced by its capacity to adapt to, tolerate, and especially create the ecologies in which it lives and grows. Seen in different contexts, human nature, which I had previously thought of as a singular noun, became plural and pluralistic; for the different environments were producing discernible differences, not only across but also within societies, in talent, temperament, human relations, and particularly in the ways in which the culture, or subculture, brought up its next generation. The process and product of making human beings human clearly varied by place and time. Viewed in historical as well as cross-cultural perspective, this diversity suggested the possibility of ecologies as yet untried that held a potential for human natures yet unseen, perhaps possessed of a wiser blend of power and compassion than has thus far been manifested.

Although this last prospect may appear a product of airy idealism, it is rooted in the harder ground of cross-cultural reality.

The second lesson I learned from work in other societies is that public policy has the power to affect the well-being and development of human beings by determining the conditions of their lives. This realization led to my heavy involvement during the past fifteen years in efforts to change, develop, and implement policies in my own country that could influence the lives of children and families. Participating in the Head Start Planning Committee, two Presidential Task Forces, and other scientific advisory groups at the national, state, and local levels, as well as testifying for and collaborating with politicians and government officials on legislation, brought me to an unexpected conclusion that is a recurrent theme in the pages that follow: concern with public policy on the part of researchers is es-

sential for progress in the scientific study of human development.

These evolving ideas, whatever their merit, are no more the product of my own endeavors than of the patient and persistent efforts of my colleagues to open my eyes to the realities of the world in which they lived and worked. In the area of cross-cultural investigation, the following were among the most patient and persistent: Gerold Becker, Lydia Bozhovich, Zvi El-Peleg and family, Hsieh Ch'i-kang, Sophie Kav-Venaki, Kurt Lüscher and family, Richard and Gertrude Meili, Janusz Reykowski, Ruth Sharabany, Ron Shouval and family, Sandor Komlosi and family, Igor Kon, Aleksei Leontiev, Hartmut von Hentig, and Aleksander V. Zaporozhets.

In the interface between developmental research and public policy, my principal associates and mentors have been Birch Bayh, Orville G. Brim, John Brademas, Robert Cooke, David Goslin, Nicolas Hobbs, Sidney Johnson, Alfred Kahn, Mary Keyserling, Walter F. Mondale, Evelyn Moore, Albert Quie, Julius Richmond, John Scales, Sargent and Eunice Shriver, Jule Sugarman, Harold Watts, Sheldon White, and Edward Zigler.

This volume developed as part of a scholarly enterprise that I initiated several years ago with the counsel of a number of like-minded colleagues and with the material support of the Foundation of Child Development. Known as the Program on the Ecology of Human Development, the effort was undertaken with the aim of furthering theory, advanced training, and research in the actual environments in which human beings live and grow. Work on the book began while I was a Belding Scholar of the foundation.

In particular I express deep appreciation to Orville G. Brim, president of the Foundation of Child Development, and to Heidi Sigal, program associate, for their encouragement, wise advice, and active help in all aspects of the EHD Program, including the conception and preparation of this volume. In addition, a great debt, both intellectual and personal, is owed to the consultants to the program—Sarane Boocock, Michael Cole, Glen Elder, William Kessen, Melvin Kohn, Eleanor Maccoby, and Sheldon White. In countless letters, conversations, and phone calls, they communicated reactions and ideas that I have gradually assimilated as my own. I apologize to the extent that I have unwittingly failed to give them credit or—worse yet—to do justice to their thoughts.

I have also been fortunate in the generosity of numerous colleagues and students at Cornell and elsewhere who have been willing to read and criticize drafts of portions of the manuscript. They include Henry Alker, Irwin Altman, Jay Belsky, John Clausen, Mon-

crieff Cochran, Michael Cole, William Cross, Glen Elder, James Garbarino, Herbert Ginsburg, Stephen Hamilton, Melvin Kohn, Barbara Koslowski, Michael Lamb, Tom Lucas, Barbara Lust and her students, Kurt Lüscher, Eleanor Maccoby, Maureen Mahoney, Rudolf Moos, David Olds, Henry Ricciuti, Morris Stambler, Eric Wanner, John Weisz, Sheldon White, and one of the most astute critics, Liese Bronfenbrenner.

Two of these, Michael Cole and Eric Wanner, have also served as special and general editors of this volume. Their initiative, encouragement, and advice have improved the product and eased the perennial pains of an author's labor. Special appreciation is also expressed to the anonymous reviewers of separate chapters and total text, as well as to Harriet Moss for her thoughtful editing of the final manuscript. I also owe a scholar's debt to my friend and neighbor Geoffrey Bruun, who never forgets the source, or substance, of a quotation.

My sense of obligation and gratitude extends beyond individuals. It is a major thesis of this book that human abilities and their realization depend in significant degree on the larger social and institutional context of individual activity. This principle is especially applicable in the present instance. Since its founding, Cornell University, as a Land Grant Institution supported half by endowment and half by the state, has nurtured a tradition of freedom and responsibility and encouraged its faculty in moving beyond traditional disciplines to recognize that social science, if it is to achieve its own objectives, must be responsive to human needs and aspirations. This dual theme has found even fuller expression in the work of the New York State College of Human Ecology at Cornell under the creative leadership of three successive deans—David C. Knapp, Jean Failing, and Jerome Ziegler.

Finally, the greatest debt of all is to Joyce Brainard, who with the dedicated assistance of Mary Alexander, Stephen Kaufman, Mary Miller, and Kay Riddell supervised and carried out endless revisions of the manuscript with care, craftsmanship, and devotion.

The summary of Ogbu's research, included in chapter 10, was prepared by Stephen Hamilton for a jointly authored conference paper. Passages appearing in several chapters represent revisions of material previously published in *Child Development*, the *American Psychologist*, the *Journal of Social Issues*, and the *Zeitschrift für Soziologie*.

Urie Bronfenbrenner

Ithaca, New York

Contents

PART ONE

An Ecological Orientation

1.

Purpose and Perspective

In this volume, I offer a new theoretical perspective for research in human development. The perspective is new in its conception of the developing person, of the environment, and especially of the evolving interaction between the two. Thus development is defined in this work as a lasting change in the way in which a person perceives and deals with his environment. For this reason, it is necessary at the outset to give an indication of the somewhat unorthodox concept of the environment presented in this volume. Rather than begin with a formal exposition, I shall first introduce this concept by some concrete examples.

The ecological environment is conceived as a set of nested structures, each inside the next, like a set of Russian dolls. At the innermost level is the immediate setting containing the developing person. This can be the home, the classroom, or as often happens for research purposes—the laboratory or the testing room. So far we appear to be on familiar ground (although there is more to see than has thus far met the investigator's eye). The next step, however, already leads us off the beaten track for it requires looking beyond single settings to the relations between them. I shall argue that such interconnections can be as decisive for development as events taking place within a given setting. A child's ability to learn to read in the primary grades may depend no less on how he is taught than on the existence and nature of ties between the school and the home.

The third level of the ecological environment takes us yet farther afield and evokes a hypothesis that the person's development is profoundly affected by events occurring in settings in which the person is not even present. I shall examine data suggesting that among the most powerful influences affecting the development of

young children in modern industrialized societies are the conditions of parental employment.

Finally, there is a striking phenomenon pertaining to settings at all three levels of the ecological environment outlined above: within any culture or subculture, settings of a given kind—such as homes, streets, or offices—tend to be very much alike, whereas between cultures they are distinctly different. It is as if within each society or subculture there existed a blueprint for the organization of every type of setting. Furthermore, the blueprint can be changed, with the result that the structure of the settings in a society can become markedly altered and produce corresponding changes in behavior and development. For example, research results suggest that a change in maternity ward practices affecting the relation between mother and newborn can produce effects still detectable five years later. In another case, a severe economic crisis occurring in a society is seen to have positive or negative impact on the subsequent development of children throughout the life span, depending on the age of the child at the time that the family suffered financial duress.

The detection of such wide-ranging developmental influences becomes possible only if one employs a theoretical model that permits them to be observed. Moreover, because such findings can have important implications both for science and for public policy, it is especially important that the theoretical model be methodologically rigorous, providing checks for validity and permitting the emergence of results contrary to the investigator's original hypotheses. The present volume represents an attempt to define the basic parameters of a theoretical model that meets these substantive and methodological requirements. The work also seeks to demonstrate the scientific utility of the ecological model for illuminating the findings of previous studies and for formulating new research problems and designs.

The environment as conceived in the proposed schema differs from earlier formulations not only in scope but also in content and structure. On the first count, the ecological orientation takes seriously and translates into operational terms a theoretical position often lauded in the literature of social science but seldom put into practice in research. This is the thesis, expounded by psychologists and sociologists alike, that what matters for behavior and development is the environment as it is *perceived* rather than as it may exist in "objective" reality. In the pages that follow, this principle is applied to expose both the weaknesses and the strengths of the

laboratory and the testing room as contexts for assessing developmental processes. Evidence exists of consistent differences in the behavior of children and adults observed in the laboratory and in the actual settings of life. These differences in turn illuminate the various meanings of these types of settings to the participants, as partly a function of their social background and experience.

Different kinds of settings are also analyzed in terms of their structure. Here the approach departs in yet another respect from that of conventional research models: environments are not distinguished by reference to linear variables but are analyzed in systems terms. Beginning at the innermost level of the ecological schema, one of the basic units of analysis is the *dyad*, or two-person system. Although the literature of developmental psychology makes frequent reference to dyads as structures characterized by reciprocal relations, we shall see that, in practice, this principle is often disregarded. In keeping with the traditional focus of the laboratory procedure on a single experimental subject, data are typically collected about only one person at a time, for instance, about either the mother or the child but rarely for both simultaneously. In the few instances in which the latter does occur, the emerging picture reveals new and more dynamic possibilities for both parties. For instance, from dyadic data it appears that if one member of the pair undergoes a process of development, the other does also. Recognition of this relationship provides a key to understanding developmental changes not only in children but also in adults who serve as primary caregivers—mothers, fathers, grandparents, teachers, and so on. The same consideration applies to dyads involving husband and wife, brother and sister, boss and employee, friends, or fellow workers.

In addition, a systems model of the immediate situation extends beyond the dyad and accords equal developmental importance to what are called *N + 2 systems*—triads, tetrads, and larger interpersonal structures. Several findings indicate that the capacity of a dyad to serve as an effective context for human development is crucially dependent on the presence and participation of third parties, such as spouses, relatives, friends, and neighbors. If such third parties are absent, or if they play a disruptive rather than a supportive role, the developmental process, considered as a system, breaks down; like a three-legged stool, it is more easily upset if one leg is broken, or shorter than the others.

The same triadic principle applies to relations between settings. Thus the capacity of a setting—such as the home, school, or work-

place—to function effectively as a context for development is seen to depend on the existence and nature of social interconnections between settings, including joint participation, communication, and the existence of information in each setting about the other. This principle accords importance to questions like the following: does a young person enter a new situation such as school, camp, or college alone, or in the company of familiar peers or adults? Are the person and her family provided with any information about or experience in the new setting before actual entry is made? How does such prior knowledge affect the subsequent course of behavior and development in the new setting?

Questions like these highlight the developmental significance and untapped research potential of what are called *ecological transitions*—shifts in role or setting, which occur throughout the life span. Examples of ecological transitions include the arrival of a younger sibling, entry into preschool or school, being promoted, graduating, finding a job, marrying, having a child, changing jobs, moving, and retiring.

The developmental importance of ecological transitions derives from the fact that they almost invariably involve a change in *role*, that is, in the expectations for behavior associated with particular positions in society. Roles have a magiclike power to alter how a person is treated, how she acts, what she does, and thereby even what she thinks and feels. The principle applies not only to the developing person but to the others in her world.

The environmental events that are the most immediate and potent in affecting a person's development are activities that are engaged in by others with that person or in her presence. Active engagement in, or even mere exposure to, what others are doing often inspires the person to undertake similar activities on her own. A three-year-old is more likely to learn to talk if others around her are talking and especially if they speak to her directly. Once the child herself begins to talk, it constitutes evidence that development has actually taken place in the form of a newly acquired *molar activity* (as opposed to molecular behavior, which is momentary and typically devoid of meaning or intent). Finally, the molar activities engaged in by a person constitute both the internal mechanisms and the external manifestations of psychological growth.

The sequence of nested ecological structures and their developmental significance can be illustrated with reference to the same example. We can hypothesize that a child is more likely to learn to talk in a setting containing roles that obligate adults to talk to

children or that encourage or enable other persons to do so (such as when one parent does the chores so that the other can read the child a story).

But whether parents can perform effectively in their child-rearing roles within the family depends on role demands, stresses, and supports emanating from other settings. As we shall see, parents' evaluations of their own capacity to function, as well as their view of their child, are related to such external factors as flexibility of job schedules, adequacy of child care arrangements, the presence of friends and neighbors who can help out in large and small emergencies, the quality of health and social services, and neighborhood safety. The availability of supportive settings is, in turn, a function of their existence and frequency in a given culture or subculture. This frequency can be enhanced by the adoption of public policies and practices that create additional settings and societal roles conducive to family life.

A theoretical conception of the environment extending beyond the behavior of individuals to encompass functional systems both within and between settings, systems that can also be modified and expanded, contrasts sharply with prevailing research models. These established models typically employ a scientific lens that restricts, darkens, and even blinds the researcher's vision of environmental obstacles and opportunities and of the remarkable potential of human beings to respond constructively to an ecologically compatible milieu once it is made available. As a result, human capacities and strengths tend to be underestimated.

The structure of the ecological environment may also be defined in more abstract terms. As we have seen, the ecological environment is conceived as extending far beyond the immediate situation directly affecting the developing person—the objects to which he responds or the people with whom he interacts on a face-to-face basis. Regarded as of equal importance are connections between other persons present in the setting, the nature of these links, and their indirect influence on the developing person through their effect on those who deal with him at first hand. This complex of interrelations within the immediate setting is referred to as the *microsystem*.

The principle of interconnectedness is seen as applying not only within settings but with equal force and consequence to linkages between settings, both those in which the developing person actually participates and those that he may never enter but in which events occur that affect what happens in the person's immediate

environment. The former constitute what I shall call *mesosystems*, and the latter *exosystems*.

Finally, the complex of nested, interconnected systems is viewed as a manifestation of overarching patterns of ideology and organization of the social institutions common to a particular culture or subculture. Such generalized patterns are referred to as *macrosystems*. Thus within a given society or social group, the structure and substance of micro-, meso-, and exosystems tend to be similar, as if they were constructed from the same master model, and the systems function in similar ways. Conversely, between different social groups, the constituent systems may vary markedly. Hence by analyzing and comparing the micro-, meso-, and exosystems characterizing different social classes, ethnic and religious groups, or entire societies, it becomes possible to describe systematically and to distinguish the ecological properties of these larger social contexts as environments for human development.

Most of the building blocks in the environmental aspect of the theory are familiar concepts in the behavioral and social sciences: molar activity, dyad, role, setting, social network, institution, subculture, culture. What is new is the way in which these entities are related to each other and to the course of development. In short, as far as the external world is concerned, what is presented here is a theory of environmental interconnections and their impact on the forces directly affecting psychological growth.

Furthermore, an ecological approach to the study of human development requires a reorientation of the conventional view of the proper relation between science and public policy. The traditional position, at least among social scientists, is that whenever possible social policy should be based on scientific knowledge. The line of thought I develop in this volume leads to a contrary thesis: in the interests of advancing fundamental research on human development, *basic science needs public policy even more than public policy needs basic science.* Moreover, what is required is not merely a complementary relation between these two domains but their functional integration. Knowledge and analysis of social policy are essential for progress in developmental research because they alert the investigator to those aspects of the environment, both immediate and more remote, that are most critical for the cognitive, emotional, and social development of the person. Such knowledge and analysis can also lay bare ideological assumptions underlying, and sometimes profoundly limiting, the formulation of research problems and designs and thus the range of possible findings. A func-

tional integration between science and social policy of course does not mean that the two should be confused. In examining the impact of public policy issues for basic research in human development, it is all the more essential to distinguish between interpretations founded on empirical evidence and those rooted in ideological preference.

It is clear that the desirability of a reciprocal relation between science and social policy follows from the inclusion, in the theoretical model of the environment, of a macrosystem level involving generalized patterns of ideology and institutional structure characteristic of a particular culture or subculture. Public policy is a part of the macrosystem determining the specific properties of exo-, meso-, and microsystems that occur at the level of everyday life and steer the course of behavior and development.

Especially in its formal aspects, the conception of the environment as a set of regions each contained within the next draws heavily on the theories of Kurt Lewin (1917, 1931, 1935, 1938). Indeed this work may be viewed as an attempt to provide psychological and sociological substance to Lewin's brilliantly conceived topological territories.

Perhaps the most unorthodox feature of the proposed theory is its conception of development. Here the emphasis is not on the traditional psychological processes of perception, motivation, thinking, and learning, but on their *content—what* is perceived, desired, feared, thought about, or acquired as knowledge, and how the nature of this psychological material changes as a function of a person's exposure to and interaction with the environment. Development is defined as the person's evolving conception of the ecological environment, and his relation to it, as well as the person's growing capacity to discover, sustain, or alter its properties. Once again, this formulation shows the influence of Lewin, especially of his emphasis on a close interconnection and isomorphism between the structure of the person and of the situation (1935). The proposed conception also leans heavily on the ideas of Piaget, particularly as set forth in *The construction of reality in the child* (1954). The present thesis, however, goes considerably further. By contrast with Piaget's essentially "decontextualized" organism, it emphasizes the evolving nature and scope of perceived reality as it emerges and expands in the child's awareness and in his active involvement with the physical and social environment. Thus the infant at first be-

comes conscious only of events in his immediate surroundings, in what I have called the microsystem. Within this proximal domain, the focus of attention and of developing activity tends initially to be limited even more narrowly to events, persons, and objects that directly impinge on the infant. Only later does the young child become aware of relations between events and persons in the setting that do not from the outset involve his active participation. In the beginning the infant is also conscious of only one setting at a time, the one that he occupies at the moment. My own treatment of development not only includes the infant's awareness of the continuity of persons across settings, as implied by Piaget's concept of perceptual constancy, but also encompasses his dawning realization of the relations between events in different settings. In this way the developing child begins to recognize the existence and to develop an emerging sense of the mesosystem. The recognition of the possibility of relations between settings, coupled with the capacity to understand spoken and written language, enables him to comprehend the occurrence and nature of events in settings that he has not yet entered himself, like school, or those that he may never enter at all, such as the parents' workplace, a location in a foreign land, or the world of someone else's fantasy as expressed in a story, play, or film.

As Piaget emphasized, the child also becomes capable of creating and imagining a world of his own that likewise reflects his psychological growth. Again, an ecological perspective accords to this fantasy world both a structure and a developmental trajectory, for the realm of the child's imagination also expands along a continuum from the micro- to the meso-, exo-, and even macro- level.

The development of the child's fantasy world underscores the fact that his emerging perceptions and activities are not merely a reflection of what he sees but have an active, creative aspect. To use Piaget's apt term, the child's evolving phenomenological world is truly a "construction of reality" rather than a mere representation of it. As both Lewin and Piaget point out, the young child at first confuses the subjective and objective features of the environment and as a result can experience frustration, or even bodily harm, as he attempts the physically impossible. But gradually he becomes capable of adapting his imagination to the constraints of objective reality and even of refashioning the environment so that it is more compatible with his abilities, needs, and desires. It is this growing capacity to remold reality in accordance with human requirements and aspirations that, from an ecological perspective, represents the highest expression of development.

In terms of research method, the child's evolving construction of reality cannot be observed directly; it can only be inferred from patterns of activity as these are expressed in both verbal and nonverbal behavior, particularly in the activities, roles, and relations in which the person engages. These three factors also constitute what are designated as the *elements* of the microsystem.

In sum, this volume represents an attempt at theoretical integration. It seeks to provide a unified but highly differentiated conceptual scheme for describing and interrelating structures and processes in both the immediate and more remote environment as it shapes the course of human development throughout the life span. This integrative effort is regarded as the necessary first step in the systematic study of human development in its human context.

Throughout the volume, theoretical ideas are presented in the form of definitions of basic concepts, propositions which, in effect, constitute the axioms of the theory, and hypotheses that posit processes and relationships subject to empirical investigation.

Although some of the hypotheses to be proposed are purely deductive, following logically from defined concepts and stated propositions, the great majority derive from the application of the proposed theoretical framework to concrete empirical investigations. Thus I have by no means limited myself to theoretical exposition. I have made an effort throughout to translate ideas into operational terms. First, I have tried to find studies that illustrate the issues in question either by demonstration, or failing that, by default—by pointing out what the investigators might have done. Second, I have used investigations already published or reported to show in what way the results can be illuminated by applying concepts and propositions from the proposed theoretical framework. Third, where no appropriate researches could be found, I have concocted hypothetical studies that, to my knowledge, have never been carried out but are capable of execution. The investigations cited have been drawn from diverse disciplines and reflect a range of theoretical orientations. In addition, I have tried to select researches conducted in or concerned with varied settings (such as homes, hospitals, day care centers, preschools, schools, camps, institutions, offices and factories), contrasting broader social contexts (social classes, ethnic and religious groups, and total societies), and different age levels from early infancy through the life span. Unhappily, these attempts at achieving some representativeness across the spectra of ecology and age met with only partial success. To the extent that they exist, ecologically oriented investigations of development in real-life settings have most often been conducted with infants

and preschoolers studied in home or center. Acceptable research designs involving school-age children, adolescents, or adults observed in extrafamilial settings are few.

Having these goals, the volume is admittedly broad in scope. But it is not all-inclusive. No attempt is made to treat the standard subject matter of developmental psychology, that is, to describe the evolution of cognitive, emotional, and social processes over the life course. Nor is particular attention given to a second major preoccupation of contemporary developmental research—the mechanisms of socialization, such as reinforcement and modeling. The omissions do not reflect any lack of interest in these topics. On the contrary, the present work is motivated by my conviction that further advance in the scientific understanding of the basic intrapsychic and interpersonal processes of human development requires their investigation in the actual environments, both immediate and remote, in which human beings live. This task demands the construction of a theoretical schema that will permit the systematic description and analysis of these contexts, their interconnections, and the processes through which these structures and linkages can affect the course of development, both directly and indirectly.

I have thus eschewed the conventional organization of developmental topics in terms either of successive age levels (such as infancy, childhood, and adolescence) or of the classical psychological processes (perception, motivation, learning, and so on). Instead the sections and chapters of this volume reflect the proposed theoretical framework for an ecology of human development. Following a definition of basic concepts, successive chapters deal with elements of the microsystem (chapters 3 through 5), the joint effect of these elements as they function in specific settings (chapters 6 through 8), and the structures and operations of higher order systems at the meso-, exo-, and macro- levels (chapters 9 through 11).

One may well ask how an ecology of human development differs from social psychology on the one hand and sociology or anthropology on the other. In general the answer lies in the focus of the present undertaking on the phenomenon of *development-in-context*. Not only are the above three social science disciplines considerably broader, but none has the phenomenon of development as its primary concern. To describe the ecology of human development as the social psychology, sociology, or anthropology of human development is to overlook the crucial part played in psychological growth by biological factors, such as physical characteristics and in particu-

lar the impact of genetic propensities. Indeed the present work does not give such biological influences their due, once again because this cannot be done satisfactorily until an adequate framework for analyzing the environmental side of the equation has been developed, so that the interaction of biological and social forces can be specified.

Finally, lying at the very core of an ecological orientation and distinguishing it most sharply from prevailing approaches to the study of human development is the concern with the progressive accommodation between a growing human organism and its immediate environment, *and* the way in which this relation is mediated by forces emanating from more remote regions in the larger physical and social milieu. The ecology of human development lies at a point of convergence among the disciplines of the biological, psychological, and social sciences as they bear on the evolution of the individual in society.

The primary purpose of detailed discussions of empirical investigations is not to provide an exhaustive analysis of a particular study in terms of either content or method, nor to reach a definitive evaluation of the validity of the findings and their interpretation. To the extent that such assessments are made, they serve as a means to illustrate the practical feasibility, scientific utility, and possible substantive outcomes of an ecological model for the study of human development. Many of the works cited will have conventional virtues or faults that would deserve comment in a more comprehensive treatment but do not bear on the ecological issues under consideration.

Even more disconcerting to the reader may be the fact that many of the studies cited fall short of, or even violate, the principles set forth in this volume, including the very proposition that a given investigation is supposed to illustrate. Such is the present state of the field. I have tried to pick the best examples I could find, but most of them are only partially satisfactory. Rigorous research on human development using ecologically valid measures on both the independent and dependent side of the developmental equation and at the same time paying attention to the influence of larger social contexts is still the exception rather than the rule. At best, one or two important criteria are met, but other features remain at odds with ecological requirements of equal importance. The most typical pattern is one in which the critical conditions are satisfied on one side of the hypothesis but not on the other. For example, an in-

vestigation conducted in a real-life setting with systematic description and analysis of relevant physical and social conditions may employ outcome measures, such as an IQ test, a projective technique, or a laboratory procedure, that are of unknown applicability to the environments of scientific interest. Conversely, in another study, the dependent variables may be solidly based in experiences and contexts of everyday life but the independent factors limited to diffuse, dichotomous, and often value-laden labels (middle class and working class, black and white, single-parent family and intact family), with no other contextual evidence provided. The one-sided pattern is so common that to call attention to every instance of its occurrence would be cumbersome. Accordingly, the identification of departures from the requirements of an ecological model is usually limited to violations of principles directly under discussion.

It is important to emphasize in this connection that it is neither necessary nor possible to meet all the criteria for ecological research within a single investigation. Provided the researcher recognizes which qualifications are and are not met, useful scientific information can be gained.

Another shortcoming in the studies cited also reflects the present state of developmental research. I have taken the position that development implies enduring changes that carry over to other places at other times. In the absence of evidence for such carry-over, the observed alteration in behavior may reflect only a short-lived adaptation to the immediate situation. For many of the ideas presented in this volume, it has been impossible to find an example in the research literature that met this important criterion. The great majority of studies in the field of human development do not in fact investigate changes in a person over any considerable time, for they are typically based on brief assessments in a laboratory or testing room that are seldom repeated at a more distant time. One is left to assume that the processes occurring during the original short session will have lasting effect.

Two final disclaimers relate not to the cited researches themselves but to the hypotheses to which they are said to give rise. First, my reasoning may, on occasion, appear somewhat far-fetched. Once again, I merely used the best examples I could find, in the belief that an illustration bearing some relation, however remote, to empirical reality would be preferable to a hypothetical instance.

Second, the justification for this practice is the purpose that hypotheses are intended to serve in the present volume, for they are not offered as definitive propositions. The likelihood that they will

be validated in the form in which they are stated is, in my judgment, rather slim. The function of the proposed hypotheses is essentially heuristic—to identify questions, domains, and possibilities believed worthy of exploration.

It is with the aim of contributing to theoretical and empirical discovery that I have written this book. It will have achieved its objective not if the ideas presented prove to be precisely correct, which is improbable, but if their investigation offers new, revealing vistas for the scientific understanding of the forces shaping the development of human beings in the environments in which they live.

2.

Basic Concepts

To assert that human development is a product of interaction between the growing human organism and its environment is to state what is almost a commonplace in behavioral science. It is a proposition that all students of behavior would find familiar, with which none would take issue, and that few would regard as in any way remarkable, let alone revolutionary, in its scientific implications. I am one of those few. I regard the statement as remarkable because of the striking contrast between the universally approved twofold emphasis that it mandates and the conspicuously one-sided implementation the principle has received in the development of scientific theory and empirical work.

To be specific, the principle asserts that behavior evolves as a function of the interplay between person and environment, expressed symbolically in Kurt Lewin's classic equation: $B = f(PE)$ (Lewin, 1935, p. 73). One would therefore expect psychology, defined as the science of behavior, to give substantial if not equal emphasis to both elements on the independent side of the equation, to investigate the person *and* the environment, with special attention to the interaction between the two. What we find in practice, however, is a marked asymmetry, a hypertrophy of theory and research focusing on the properties of the person and only the most rudimentary conception and characterization of the environment in which the person is found.

To appreciate the contrast, one has only to examine the basic texts, books of readings, handbooks, and research journals in psychology in general and developmental psychology in particular. Upon perusing such materials, one will quickly discover concepts and data without end dealing with the qualities of the person. The researcher has available a rich array of personality typologies, de-

velopmental stages, and dispositional constructs, each with their matching measurement techniques, that provide highly differentiated profiles of the abilities, temperament, and predominant behavior tendencies of the individual. On the environmental side, however, the prospect is bland by comparison, both in theory and data. The existing concepts are limited to a few crude and undifferentiated categories that do little more than locate people in terms of their social address—the setting from which they come. Thus an examination of studies of environmental influences appearing in a representative sample of texts, books of readings, and journal issues in child psychology and related fields reveals the following modal typologies for describing contexts of behavior and development: family size, ordinal position, single- versus two-parent households, home care versus day care, parents versus peers, and —perhaps the most frequent—variation by social class or ethnic background. Moreover, the data in these studies consist to an overwhelming degree of information not about the settings from which the persons come but about the characteristics of the persons themselves, that is, how people from diverse contexts differ from one another.

As a result, interpretations of environmental effects are often couched in what Lewin called class-theoretical terms; thus observed differences in children from one or another setting (for example, lower class versus middle class, French versus American, day care versus home care) are "explained" simply as attributes of the setting in question. Even when the environment is described, it is in terms of a static structure that makes no allowance for the evolving processes of interaction through which the behavior of participants in the system is instigated, sustained, and developed.

Finally, and perhaps ironically, the data in these studies are typically obtained by removing the research subjects from the particular settings under investigation and placing them in a laboratory or a psychological testing room.[1] The possible impact of these rather special settings on the behavior being elicited, however, is rarely taken into account.

To be sure, there are two spheres of investigation in which some degree of specificity in the analysis of environments is achieved, but the result falls far short of the requirements of an ecological research model. One of these areas, lying primarily in the domain of social psychology, is the study of interpersonal relations and small groups. Given that the people with whom one interacts in a face-to-face situation constitute a part of one's environment, there is a sig-

nificant body of theory and research dealing with the impact of the environment, in the form of interpersonal influences, on the evolution of behavior. Indeed, to the extent that we have theories about *how* environmental influences affect behavior and development, they are theories about interpersonal processes—reinforcement, modeling, identification, and social learning. From an ecological perspective, such formulations have two shortcomings. First, they tend to overlook the impact of the nonsocial aspects of the environment, including the substantive nature of the activities engaged in by the participants. Second, and more crucial, they delimit the concept of environment to a single immediate setting containing the subject, what in this book is referred to as the *microsystem.* Seldom is attention paid to the person's behavior in more than one setting or to the way in which relations between settings can affect what happens within them. Rarest of all is the recognition that environmental events and conditions outside any immediate setting containing the person can have a profound influence on behavior and development within that setting. Such external influences can, for example, play a critical role in defining the meaning of the immediate situation to the person. Unless this possibility is taken into account in the theoretical model guiding the interpretation of results, the findings can lead to misleading conclusions that both narrow and distort our scientific understanding of the determinants, processes, and potential of human development.

There exists a second body of scholarly work in which external environmental contexts are described in considerable detail and their impact on the course of development graphically traced. Such investigations are carried out primarily in the field of anthropology and to some extent in social work, social psychiatry, clinical psychology, and sociology. But the descriptive material in these studies is heavily anecdotal and the interpretation of causal influences highly subjective and inferential. Here we encounter what I view as an unfortunate and unnecessary schism in contemporary studies of human development. Especially in recent years, research in this sphere has pursued a divided course, each tangential to genuine scientific progress. To corrupt a modern metaphor, we risk being caught between a rock and a *soft* place. The rock is rigor, and the soft place relevance. The emphasis on rigor has led to experiments that are elegantly designed but often limited in scope. This limitation derives from the fact that many of these experiments involve situations that are unfamiliar, artificial, and short-lived, and call for unusual behaviors that are difficult to generalize to other settings. From this perspective, it can be said that much of

developmental psychology, as it now exists, is *the science of the strange behavior of children in strange situations with strange adults for the briefest possible periods of time.*[2]

Partially in reaction to such shortcomings, other workers have stressed the need for social relevance in research, but often with indifference to or open rejection of rigor. In its more extreme manifestations, this trend has taken the form of excluding the scientists themselves from the research process. One major foundation has a policy that grants for research will be awarded only to persons who are themselves the victims of social injustice. Less radical expressions of this trend involve reliance on existential approaches in which "experience" takes the place of observation and analysis is forgone in favor of a more personalized and direct "understanding" gained through intimate involvement in the field situation. More common, and more scientifically defensible, is an emphasis on naturalistic observation, but with the stipulation that it be "theoretically neutral" (Barker and Wright, 1954, p. 14), hence unguided by any explicit hypotheses formulated in advance and uncontaminated by highly structured experimental designs imposed prior to data collection.

The most sophisticated argument advocating the superiority of naturalistic over experimental methods in the study of human development emphasizes the practical and ethical impossibility of manipulating and controlling variables of primary significance for psychological growth. For example, in a searching critique of contemporary research in developmental psychology, McCall (1977) starts from a position identical to mine: "It is suggested that, at present, we essentially lack a science of natural developmental processes because few studies are concerned with development as it transpires in naturalistic environments and because we rarely actually collect or analyze truly developmental data. This problem is believed to derive from the veneration of manipulative experimental methods, which have come to dictate rather than serve research questions" (p. 333).

McCall then proceeds to argue that experimental methods, while ideally suited for research in laboratory settings, are ill adapted to the study of "behavior as it typically develops in natural life circumstances" (p. 334), since, for practical and ethical reasons, it is impossible to manipulate and control all the revelant factors. In McCall's words,

> There is nothing inherently wrong with manipulative experimental studies in developmental psychology, but this methodology . . . is often impossible to execute . . . For example, exposure to visual pattern is re-

quired for the development of a variety of visual functions, but every child receives adequate patterned light. Certain sensorimotor activities may be propaedeutic to the acquisition of agent-action-object language constructions, but almost all children obtain adequate amounts of these experiences . . . To determine the necessary causes of development, one must deprive the organism of the hypothetical circumstance. However, when children are the focus of study, ethical considerations obviate experimental deprivation in most cases.

We must simply accept the fact from logical and practical standpoints that we will probably never prove the sufficient or necessary cause for the naturalistic development of a host of major behaviors, some of which represent the essence of our discipline. (Pp. 335–336)

McCall's persuasive argument assumes that the only function of the experiment in science is to establish necessary and sufficient conditions. As I argue later, to make this assumption is seriously to underestimate the scientific power of the experimental method: the experimental method is not only invaluable for the verification of hypotheses; it is equally and perhaps even more applicable to their discovery. In short, for science in general and especially for rigorous research on development-in-context, the experiment is a powerful and essential heuristic tool.

For these reasons, the orientation proposed here rejects both the implied dichotomy between rigor and relevance and the assumed incompatibility between the requirements of research in natural situations and the applicability of structured experiments at an early stage in the scientific process. It rejects as spurious the argument that, because naturalistic observation preceded experimentation in both the physical and the biological sciences, this progression is necessarily the strategy of choice in the study of human behavior and development. Such an interpretation mistakes a historical sequence for a causal one and represents yet another instance of the logical pitfalls inherent in the ever seductive *post hoc*, ergo *propter hoc* inference. In my view, twentieth-century science possesses research strategies that, had they been available to the nineteenth-century naturalists, would have enabled them to leapfrog years of painstaking, exhaustive description in arriving at a formulation of biological principles and laws. This is not to imply that taxonomy is not an essential scientific task but only to assert that a phase of purely descriptive observation, recording, and classification may not be a necessary condition for making progress in the understanding of process and that the early application of experimental paradigms may in fact lead to more appropriate taxonomies for achieving the

requisite work of the systematic ordering of natural phenomena.

Yet another restriction is unnecessarily imposed on the strategy of naturalistic observation, particularly as applied to the human case by its principal advocates—the ethnologists (Jones, 1972; McGrew, 1972) and the psychological ecologists of the Kansas school (Barker and Schoggen, 1973; Barker and Wright, 1954). Both groups have adapted to the study of human behavior a model originally developed for the observation of subhuman species. Implicit in this model is a concept of the environment that may be quite adequate for the study of behavior in animals but that is hardly sufficient for the human case: it is limited to the immediate, concrete setting containing the living creature and focuses on the observation of the behavior of one or, at most, two beings at a time in only one setting. As I shall argue below, the understanding of *human* development demands more than the direct observation of behavior on the part of one or two persons in the same place; it requires examination of multiperson systems of interaction not limited to a single setting and must take into account aspects of the environment beyond the immediate situation containing the subject. In the absence of such a broadened perspective, much of contemporary research can be characterized as the study of *development-out-of-context.*

The present work offers a foundation for building context into the research model at the levels of both theory and empirical work. I propose first an expansion and then a convergence of the naturalistic and the experimental approaches—more precisely, an expansion and convergence of the theoretical conceptions of the environment that underlie each of them. I refer to this evolving scientific perspective as the *ecology of human development.*

I begin with some definitions of substantive focus.

DEFINITION 1

The ecology of human development involves the scientific study of the progressive, mutual accommodation between an active, growing human being and the changing properties of the immediate settings in which the developing person lives, as this process is affected by relations between these settings, and by the larger contexts in which the settings are embedded.

Three features of this definition are especially worthy of note. First, the developing person is viewed not merely as a tabula rasa on which the environment makes its impact, but as a growing, dynamic entity that progressively moves into and restructures the milieu in which it resides. Second, since the environment also exerts

its influence, requiring a process of mutual accommodation, the interaction between person and environment is viewed as two-directional, that is, characterized by *reciprocity*. Third, the environment defined as relevant to developmental processes is not limited to a single, immediate setting but is extended to incorporate interconnections between such settings, as well as to external influences emanating from the larger surroundings. This extended conception of the environment is considerably broader and more differentiated than that found in psychology in general and in developmental psychology in particular. The *ecological environment* is conceived topologically as a nested arrangement of concentric structures, each contained within the next. These structures are referred to as the *micro-*, *meso-*, *exo-*, and *macrosystems*, defined as follows.

DEFINITION 2

A microsystem is a pattern of activities, roles, and interpersonal relations experienced by the developing person in a given setting with particular physical and material characteristics.

A *setting* is a place where people can readily engage in face-to-face interaction—home, day care center, playground, and so on. The factors of *activity*, *role*, and *interpersonal relation* constitute the *elements*, or building blocks, of the microsystem.

A critical term in the definition of the microsystem is *experienced*. The term is used to indicate that the scientifically relevant features of any environment include not only its objective properties but also the way in which these properties are perceived by the persons in that environment. This emphasis on a phenomenological view springs neither from any antipathy to behavioristic concepts nor from a predilection for existential philosophical foundations. It is dictated simply by a hard fact. Very few of the external influences significantly affecting human behavior and development can be described solely in terms of objective physical conditions and events; the aspects of the environment that are most powerful in shaping the course of psychological growth are overwhelmingly those that have meaning to the person in a given situation.

There is, of course, nothing original in this formulation. It draws heavily on the work of theorists from a variety of disciplines. From philosophy and psychology, it builds on the phenomenological concepts of Husserl (1950), Kohler (1938), and Katz (1930). In sociology, an analogous formulation has its roots in the role theory of George Herbert Mead (1934), and is epitomized in the Thomases'

concept of the "definition of the situation" (Thomas and Thomas, 1928). In psychiatry, the view was brilliantly applied to the study of interpersonal relations and psychopathology by Sullivan (1947). In education, the orientation is found in Dewey's emphasis on designing curricula that reflect the everyday experience of the child (1913, 1916, 1931). In anthropology, the approach has been extended to the analysis of larger social systems, most notably by Linton (1936) and Benedict (1934). Its significance for the general study of human behavior is summed up in what is perhaps the only proposition in social science that approaches the status of an immutable law—the Thomases' inexorable dictum "If men define situations as real, they are real in their consequences" (Thomas and Thomas, 1928, p. 572).

In the main, however, the phenomenological conception of the environment that lies at the foundation of the theory derives its structure and rationale from the ideas of Kurt Lewin, especially his construct of the "life space" or "psychological field" (1931, 1935, 1951). Lewin takes the position that the environment of greatest relevance for the scientific understanding of behavior and development is reality not as it exists in the so-called objective world but as it appears in the mind of the person; in other words, he focuses on the way in which the environment is perceived by the human beings who interact within and with it. An especially significant aspect of this perceived environment is the world of imagination, fantasy, and unreality. Yet despite such seeming richness, Lewin's theoretical map of the psychological field is curiously lacking in content. To use his own term, his is a "topological psychology," a systematic description of a space without substance, replete with empty regions and nested structures, separated by boundaries, joined by interconnections and pathways, and beset by barriers and detours on the way to unspecified goals. The most unorthodox aspect of Lewin's schema is his treatment of motivational forces as emanating not from within the person but from the environment itself. Objects, activities, and especially other people send out lines of force, valances, and vectors that attract and repel, thereby steering behavior and development.

What could all this mean in concrete terms? What sense, let alone application, could one make of a theory in which the perceived is viewed as more important than the actual, the unreal more valid than the real; where the motivation that steers behavior inheres in external objects, activities, and other people; and where the content of all these complicated structures remains unspecified? More point-

edly, how could anyone apply such airy abstractions to settings in everyday life, or for that matter, why should anyone wish to do so?

A basis for a plausible answer to these questions is suggested by consideration of the very first paper Lewin wrote, "Kriegslandschaft" ("War Landscape"), published at the end of the First World War after he had spent several years in the army, most of it in the front lines where he had been wounded in combat. The article, which appeared in the *Zeitschrift für Angewandte Psychologie* (1917), represents a marvelous prefiguring of all his basic theoretical concepts. In this extraordinary paper, Lewin describes how the perceived reality of the landscape changes as one moves nearer to the front. What first appears as a lovely bucolic scene of farmhouses, fields, and wooded areas is gradually transformed. The forested hilltop becomes an observation post, its sheltered side the location for a gun emplacement. An unexposed hollow is seen as a probable battalion aid station. Aspects of the natural landscape that were a delight only a few kilometers back are now perceived as ominous: the frightening defile, the camouflage of trees, the hill that hides the unseen enemy, the invisible objective to be taken, the place and moment of security after the fray—features of the environment that threaten, beckon, reassure, and steer one's course across a terrain objectively undistinguishable from scenes just a short distance behind the front.

Here are the basic premises of what later became Lewin's explicit, systematic theory: the primacy of the phenomenological over the real environment in steering behavior; the impossibility of understanding that behavior solely from the objective properties of an environment without reference to its meaning for the people in the setting; the palpable motivational character of environmental objects and events; and, especially, the importance of the unreal, the imagined—the enemy not seen, the promise of a warm meal, and the prospect of surviving to sleep, or to lie awake another night. What could be more down to earth than this?

Herein also lies the explanation for Lewin's unwillingness to specify in advance the content of the psychological field: it is a terrain that has yet to be explored. Such exploration, therefore, constitutes a major task of psychological science. One needs to discover empirically how situations are perceived by the people who participate in them. Again, without specifying content, Lewin distinguishes two aspects of every situation that are likely to capture the person's attention. The first is Tätigkeit, perhaps best translated as "ongoing activity"; it refers to the tasks or operations in which a

person sees himself or others as engaging. The second salient feature involves the perceived interconnections between the people in the setting, in terms not so much of interpersonal feelings as of the relations of the various parties with each other as members of a group engaged in common, complementary, or relatively independent undertakings.

In addition to these two aspects of the situation highlighted by Lewin, the concept of microsystem involves a third feature emphasized in the sociological theories of Mead and the Thomases, namely, the notion of role. For the present, we can make use of the standard definition of role in the social sciences: a set of behaviors and expectations associated with a position in society, such as that of mother, baby, teacher, friend, and so on.

The phenomenological perspective is also relevant at the next and succeeding levels of ecological structure.

DEFINITION 3

A mesosystem comprises the interrelations among two or more settings in which the developing person actively participates (such as, for a child, the relations among home, school, and neighborhood peer group; for an adult, among family, work, and social life).

A mesosystem is thus a system of microsystems. It is formed or extended whenever the developing person moves into a new setting. Besides this primary link, interconnections may take a number of additional forms: other persons who participate actively in both settings, intermediate links in a social network, formal and informal communications among settings, and, again clearly in the phenomenological domain, the extent and nature of knowledge and attitudes existing in one setting about the other.

DEFINITION 4

An exosystem refers to one or more settings that do not involve the developing person as an active participant, but in which events occur that affect, or are affected by, what happens in the setting containing the developing person.

Examples of an exosystem in the case of a young child might include the parent's place of work, a school class attended by an older sibling, the parents' network of friends, the activities of the local school board, and so on.

DEFINITION 5

The *macrosystem* refers to consistencies, in the form and content of lower-order systems (micro-, meso-, and exo-) that exist, or could exist, at the level of the subculture or the culture as a whole, along with any belief systems or ideology underlying such consistencies.

For example, within a given society—say France—one crèche, school classroom, park playground, café, or post office looks and functions much like another, but they all differ from their counterparts in the United States. It is as if in each country the various settings had been constructed from the same set of blueprints. An analogous difference in form appears at levels beyond the microsystem. Thus the relations between home and school are rather different in France than in our own country. But there are also consistent patterns of differentiation within each of these societies. In both worlds, homes, day care centers, neighborhoods, work settings, and the relations between them are not the same for well-to-do families as for the poor. Such intrasocietal contrasts also represent macrosystem phenomena. The systems blueprints differ for various socioeconomic, ethnic, religious, and other subcultural groups, reflecting contrasting belief systems and lifestyles, which in turn help to perpetuate the ecological environments specific to each group.

I deliberately mention in the definition of macrosystem patterns that "could exist" so as to expand the concept of macrosystem beyond limitation to the status quo to encompass possible blueprints for the future as reflected in the vision of a society's political leaders, social planners, philosophers, and social scientists engaging in critical analysis and experimental alteration of prevailing social systems.

Having been introduced to the structure of the ecological environment, we are now in a position to identify a general phenomenon of movement through ecological space—one that is both a product and a producer of developmental change.

DEFINITION 6

An ecological transition occurs whenever a person's position in the ecological environment is altered as the result of a change in role, setting, or both.

Instances of ecological transition as defined here occur throughout the life span. To name but a few: a mother is presented with her newborn infant for the first time; mother and baby return home

from the hospital; there is a succession of baby sitters; the child enters day care; a younger sibling arrives; Johnny or Mary goes to school, is promoted, graduates, or perhaps drops out. Then there is finding a job, changing jobs, losing jobs; marrying, deciding to have a child; having relatives or friends move in (and out again); buying one's first family car, television set, or home; vacationing, traveling; moving; getting divorced, remarrying; changing careers; emigrating; or, to turn to even more universal themes: becoming sick, going to the hospital, getting well again; returning to work, retiring; and the final transition to which there are no exceptions—dying.

I shall argue that every ecological transition is both a consequence and an instigator of developmental processes. As the examples indicate, the transitions are a joint function of biological changes and altered environmental circumstances; thus they represent examples par excellence of the process of mutual accommodation between the organism and its surroundings that is the primary focus of what I have called the ecology of human development. Furthermore, the alterations in the milieu can occur at any of the four levels of the ecological environment. The appearance of a younger sibling is a microsystem phenomenon, entry into school changes exo- into mesosystem, and emigrating to another country (or perhaps just visiting the home of a friend from a different socioeconomic or cultural background) involves crossing macrosystem borders. Finally, from the viewpoint of research, every ecological transition constitutes, in effect, a ready-made experiment of nature with a built-in, before-after design in which each subject can serve as his own control. In sum, an ecological transition sets the stage both for the occurrence and the systematic study of developmental phenomena.

We are brought back to the fundamental question of how development is to be conceived in the framework of an ecological theory. The formulation presented here starts from the proposition that development never takes place in a vacuum; it is always embedded and expressed through behavior in a particular environmental context.

DEFINITION 7

Human development is the process through which the growing person acquires a more extended differentiated, and valid conception of the ecological environment, and becomes motivated and able to engage in activities that reveal the properties of, sustain, or restructure that environment at levels of similar or greater complexity in form and content.

Three features of this definition are particularly worthy of note. First, development involves a change in the characteristics of the person that is neither ephemeral nor situation-bound; it implies a reorganization that has some continuity over both time and space. Second, developmental change takes place concurrently in two domains, those of perception and action. Third, from a theoretical viewpoint, each of these domains has a structure that is isomorphic with the four levels of the ecological environment. Thus in the perceptual sphere the question becomes to what extent the developing person's view of the world extends beyond the immediate situation to include a picture of other settings in which he has actively participated, the relations among these settings, the nature and influence of external contexts with which he has had no face-to-face contact, and, finally, the consistent patterns of social organization, belief systems, and lifestyle specific to his own and other cultures and subcultures. Analogously, at the level of action, at issue is the person's capacity to employ strategies that are effective, first, in providing accurate feedback about the nature of the systems existing at successively more remote levels, second, enabling these systems to continue to function and, third, reorganizing existing systems or creating new ones of comparable or higher order that are more in accord with his desires. Later I shall endeavor to show how this two-sided ecological conception of development can be fruitfully applied both to obtain a richer scientific yield from existing research findings and to design new investigations that will further illuminate the nature, course, and conditions of human development.

An ecological conception of *development-in-context* also has implications for research method and design. To begin, it accords key importance to and provides the theoretical basis for a systematic definition of a construct often alluded to in recent discussions of developmental research—*ecological validity*. Although the term as yet has no accepted definition, one can infer from these discussions a common underlying conception: an investigation is regarded as ecologically valid if it is carried out in a natural setting and involves objects and activities from everyday life. Although originally attracted to this notion, I have upon reflection come to view it not only as too simplistic but as scientifically unsound on several counts. First, while I agree wholeheartedly with the desirability of extending research activities beyond the laboratory, I question the seemingly automatic granting of scientific legitimacy to a research effort

merely because it is conducted in a real-life setting. Even more arbitrary is the converse implication that any investigation carried out in a nonnatural setting is necessarily ecologically invalid and thereby scientifically suspect on purely a priori grounds. Surely this is to prejudge the issue. Moreover, the term *ecological validity,* as it is currently used, has no logical relation to the classical definition of validity—namely, the extent to which a research procedure measures what it is supposed to measure. Indeed, there is a basic conflict between the theoretical assumptions underlying the two conceptions. In the classical definition, validity is ultimately determined by the nature of the problem under investigation. By contrast, ecological validity as heretofore defined appears to be determined once and for all by the setting in which the study is conducted, without regard to the question under investigation. In any research endeavor this last consideration must be the most decisive in assessing validity of whatever kind.

At the same time, there is implicit in current concerns with ecological validity another principle that can no longer be disregarded in the light of available evidence. This is the proposition that the properties of the environmental contexts in which an investigation is conducted or from which the experimental subjects come can influence the processes that take place within the research setting and thereby affect the interpretation and generalizability of the findings.

I have therefore sought to formulate a definition of ecological validity that takes both these principles into account. Once the task was undertaken, it was not difficult to achieve. All that was required was a logical extension of the traditional definition of validity. This definition is limited in focus, applying only to the measurement procedures employed in research operations. The definition of ecological validity proposed here expands the scope of the original concept to include the environmental context in which the research is conducted.

DEFINITION 8

Ecological validity refers to the extent to which the environment experienced by the subjects in a scientific investigation has the properties it is supposed or assumed to have by the investigator.

Again, the use of the term *experienced* in the definition highlights the importance of the phenomenological field in ecological research. The ecological validity of any scientific effort is called into ques-

tion whenever there is a discrepancy between the subject's perception of the research situation and the environmental conditions intended or assumed by the investigator. This means that it becomes not only desirable but essential to take into account in every scientific inquiry about human behavior and development how the research situation was perceived and interpreted by the subjects of the study. The importance of this injunction will become apparent when, later in this volume, we examine specific investigations from the perspective of ecological validity and find ourselves arriving at plausible alternative interpretations that cannot be resolved without our having *at least some* knowledge of the subject's definition of the situation.

In one of the few systematic analyses of the concept of ecological validity, Michael Cole and his colleagues (1978) point out that the task of determining how the subject perceives the situation is an extremely difficult one that the psychological researcher does not yet know how to accomplish. They go on to argue that Lewin's emphasis on this requirement as central to ecological validity (1943) is difficult to reconcile with the scientific demands of an alternative formulation of the concept proposed by Lewin's contemporary, Egon Brunswik (1943, 1956, 1957). Brunswik used the term in a far narrower sense to apply to a more traditional problem in the psychology of perception—the relation between a proximal cue and the distal object in the environment to which it was related. The ecological element in this conception derived from Brunswik's insistence on "representative design." In his view, to establish the existence of a given psychological process it was necessary to demonstrate its occurrence across a sample not only of subjects but also of situations. The purpose of such environmental sampling was to show that the phenomenon "possesses generality with regard to normal life conditions" (1943, p. 265).

While applauding Brunswik's emphasis on the importance of conditions of everyday life as proper referents for basic research, I shall later (chapter 6) take issue with the fundamental assumption underlying Brunswik's argument, and much of contemporary psychological science as well, that the only processes meriting scientific status in the study of human behavior are those that are invariant across contexts. For the moment, however, our concern is with the contention of Cole and company that, in practice if not in theory, the ecological requirements of Lewin and of Brunswik are incompatible with each other. They claim that to insist that research be carried out in a variety of situations and, at the same time, demand

that each situation be examined in terms of its psychological meaning to the participants imposes "an enormous burden" on the investigator, one that "is perhaps more than psychology can, or psychologists would care to take on" (Cole, Hodd, and McDermott, 1978, p. 36).

The charge is a serious one and deserves a serious answer. A first response does not resolve the dilemma, but only reaffirms that it is unavoidable. To disregard the meaning of the situation to the research subject is to risk invalid conclusions both for research and, particularly in the study of human development, for public policy. To close one's eyes to this possibility is, therefore, to be scientifically and socially irresponsible. But how is one to deal with the dilemma posed by Cole and his colleagues? Ironically, one approach to resolution is found in the work of Cole himself. In two important volumes (Cole and Scribner, 1974; Cole et al., 1971) he and his associates develop the position that the significance of much of the behavior taking place in a given social setting *can* be understood, *provided* the observer has participated in the given setting in roles similar to those taken by the participants and is a member of or has had extensive experience in the subculture in which the setting occurs and from which the actors come. This proviso still leaves much room for misconception, but it considerably reduces the likelihood of gross errors of misinterpretation. The situation is analogous to that faced by a person doing simultaneous translation at an international meeting. To accomplish the task, it is helpful—but not absolutely essential—to be a native speaker; it is a sine qua non, however, to be experienced in the ways of international conferences, have good knowledge of the subject matter, and possess full command of both languages.

The nature and necessity of these requirements is obvious enough in the case of simultaneous translation. Moreover, they are scrupulously adhered to, primarily because the participants in the proceedings have access to the record, and possess the power to press for its correction. The situation is somewhat different for the researcher of human behavior. In that case, the requirements are more one-sided: the emphasis is on mastering the knowledge, technology, and language of science rather than of the settings or persons under study. Indeed, the latter are seldom informed about the content of the scientific record and have no power to alter it. In the absence of persons able to recognize unwarranted interpretations based on misperceptions of fact, the unwitting investigator can, in all good faith, arrive at false conclusions. Once such persons are involved in

the scientific enterprise, the risk of errors is appreciably reduced.

The involvement of people from the subject's world in the research process implies a significant reorientation in the traditional relation between the researcher and the researched in the behavioral sciences. As reflected in the classical experimenter-subject paradigm of the laboratory, the former is typically thought of by both parties as possessing greater knowledge and control, whereas the latter is asked, and expected, to accept the situation as structured and to cooperate in acting as requested. An ecological orientation emphasizing the subject's definition of the situation accords far more importance to the knowledge and initiative of the persons under study. Experimental instructions and manipulations are by no means ruled out but are directed toward clarifying or determining the objective features of the environment (for example, selecting the setting, allocating roles, assigning tasks) rather than specifying the particular ways in which the subject is to behave. For by allowing activities to emerge spontaneously within the given environmental context, the investigator can obtain evidence bearing on the psychological meaning of the context to the participants.

There are of course other strategies for probing the content of the psychological field. They include interviewing participants after the fact to discover whether their retrospective view of the situation is consistent with the intention of the investigator, as well as introducing the same activities into different settings (for example the home and the laboratory) to identify any systematic effects of context.

But even if all these measures are taken, even if observers are fully familiar with the setting and the subculture, the research situation structured so as to give relatively free rein to activities initiated by the participants, the latter given opporunity to examine and comment on the scientific results and their interpretation, and investigations conducted in different contexts to highlight the distinctive features of particular settings—even if all this is achieved, serious problems still remain in ascertaining how the research situation was perceived by the persons under study. Particularly in developmental research, there exists the intriguing and often insoluble problem of understanding the phenomenological world of the infant and the young child before they can provide glimpses of their psychological experience through language. Even with adults, there is the inevitable phenomenon of idiosyncratic perception based on past experience and internal states hidden from the observer.

It was undoubtedly considerations such as these that led Cole and

his associates to come to a determined but carefully qualified stance regarding the importance and feasibility of establishing phenomenologically based ecological validity in their own sphere of special interest—cognitive development. In the final paragraph of their analysis, they offer this sobering conclusion:

> We need to know as much as possible about the subject's responses to the task-as-posed, because this is crucial information for both Brunswik's and Bronfenbrenner's notions of ecological validity. There are no currently agreed-upon methods for accomplishing these goals. While several investigators, including ourselves, are engaged in the required methods, claims for the ecological validity of cognitive tasks should be treated as programmatic hopes for the future. We have made little progress on this issue since Brunswik's and Lewin's discussion a generation ago. (1978, p. 37)

Along with the work of Cole and his associates, the present volume represents an attempt to move the field a step beyond Brunswik's and Lewin's pioneering ideas by offering a conceptual framework for analyzing the psychological life space in terms of the three microsystem elements of activity, role, and relation. The effort may not take us very far, but any added information about the nature of the perceived environment is a scientific gain in the study of development-in-context. Herein lies the basis for a somewhat more optimistic interpretation of the operational dilemma correctly posed by Cole and his associates, for it is neither necessary nor even possible to obtain a *complete* picture of the research situation as perceived by the participants. Like frictionless motion, ecological validity is a goal to be pursued, approached, but never achieved. The more closely it is approximated, however, the clearer will be the scientific understanding of the complex interplay between the developing human organism and the functionally relevant aspects of its physical and social environment.

The scope of this interplay serves as a reminder that correspondence between the subject's and the investigator's view of the research situation, or what might appropriately be called *phenomenological validity*, is only one aspect of ecological validity. Errors of interpretation may also arise because of the investigator's failure to take into account the full range of environmental forces that are operative in a given situation, including those emanating from contexts beyond the immediate setting containing the research subjects—influences at the level of meso-, exo-, and macrosystems.

The notion of ecological validity that I have set forth can be

regarded as implicit in the classical definition of scientific validity, since the failure to recognize discrepancies between the subject's and researcher's definition of the situation or the operation of influences from outside the research setting ultimately calls into question whether a given scientific procedure is measuring what it is supposed to measure. The argument follows logically enough. The question is whether its exacting implications will in fact be recognized and heeded in the absence of an explicit requirement to take into account environmental influences, real or perceived, that can affect the validity of research operations. It is this consideration that dictates the necessity of specifying a criterion of ecological validity.

Finally, this definition does not designate any particular kind of research locale as valid or invalid on a priori grounds. Thus depending on the problem, the laboratory may be an altogether appropriate setting for an investigation, and certain real-life environments may be highly inappropriate. Suppose one is interested in studying the interaction between mother and child when the child is placed in a strange and unfamiliar situation. It is clear that the laboratory approximates this condition far better than the home. Conversely, if the focus of inquiry is the modal pattern of parent-child activity prevailing in the family, observations confined to the laboratory can be misleading. As I indicate in chapter 6, findings from a number of studies demonstrate that patterns of parent-child interaction in the home can be substantially and systematically different from those observed in the laboratory. Once again, however, the fact that research results obtained in the laboratory differ from those observed in the home cannot be interpreted as evidence for the superiority of one setting over the other, except in relation to a specific research question. At the very least, such differences serve to illuminate the special properties of the laboratory as an ecological context. More important, they illustrate the as yet unexploited power of the laboratory as an ecological contrast that can highlight the distinctive features of other types of settings as they affect behavior and development. From this point of view, an ecological orientation increases rather than reduces opportunities for laboratory research by pointing to new knowledge that can be achieved through close and continuing interaction between laboratory and field research.

At a more general level, the comparison of results obtained in laboratory and real-life settings provides an illustration of the basic strategy through which ecological validity can be demonstrated

or found wanting. As was true for the definition of that concept, the method represents an extension of the procedures employed for investigating validity in its classical form. The process is essentially one of establishing construct validity (Cronbach and Meehl, 1955), in this instance by testing the ecological theory underlying the research operations—the assumptions being made about the nature and generality of the environment in which the research is being conducted. When a laboratory study is regarded as representative of behavior elsewhere, evidence must be provided of an empirical relation to similar activities in the other setting; in other words, validation must take place against an external ecological criterion, with the possibility of systematic divergence explicitly taken into account. It should be recognized, moreover, that such divergence may take the form of differences not merely in average response but in the total pattern of relationships, and in the underlying processes that they are presumed to reflect.

In research on the ecology of human development, the ability to generalize across settings is important for yet another reason. Even after ecological validity has been established, still another criterion must often be met: whenever the hypothesis under investigation implies, as it frequently does, that development has actually occurred, it is necessary to provide evidence of such an outcome before the hypothesis can be regarded as receiving empirical support. As I emphasized earlier, development implies a change that is not merely momentary or situation-specific. It is therefore not sufficient to show only that a certain variation in the environment has produced an alteration in behavior; it is also necessary to demonstrate that this change exhibits some invariance across time, place, or both. We refer to such a demonstration as the establishment of *developmental validity*, defined as follows.

DEFINITION 9

To demonstrate that human development has occurred, it is necessary to establish that a change produced in the person's conceptions and/or activities carries over to other settings and other times. Such demonstration is referred to as developmental validity.

Even the most cursory examination of published research in human development reveals that this principle is honored more in the breach than in the observance. Particularly in laboratory studies, investigations purporting to demonstrate a developmental effect

frequently offer in evidence only data that are confined to a single setting and a relatively brief period of time.

As should be true of any scientific endeavor, decisions regarding research design are dictated by theoretical considerations. Given a complex conception of person-environment interaction in the context of interdependent, nested systems, the question arises how these interdependencies can be investigated empirically. I shall argue that a strategy especially well suited for this purpose, from the earliest stages of research forward, is an *ecological experiment*, defined as follows.

> **DEFINITION 10**
> An ecological experiment is an effort to investigate the progressive accommodation between the growing human organism and its environment through a systematic contrast between two or more environmental systems or their structural components, with a careful attempt to control other sources of influence either by random assignment (planned experiment) or by matching (natural experiment).

I deliberately eschew the term *quasi-experiment*, typically employed in the research literature, because it suggests a lower level of methodological rigor, an implication I regard as unwarranted on strictly scientific grounds. There are instances in which a design exploiting an experiment of nature provides a more critical contrast, insures greater objectivity, and permits more precise and theoretically significant inferences—in short, is more elegant and constitutes "harder" science—than the best possible contrived experiment addressed to the same research question.

In other respects the definition has a familiar ring. In keeping with the commitment to rigor affirmed at the outset, the main body of the definition is a restatement of the basic logic of the experimental method. What is novel, and perhaps debatable, in this formulation is not the procedure advocated but the timing and the target of its application. I am proposing that experiments be employed in the very first phases of scientific inquiry not for the usual objective of testing hypotheses (although this device is used as a means to an end) but for *heuristic purposes*—namely, to analyze systematically the nature of the existing accommodation between the person and the milieu.

The need for early experimentation derives from the nature of the problem under investigation. The "accommodation" or "fit" between

person and environment is not an easy phenomenon to recognize. Here, looking is usually not enough. As Goethe wrote with his poet's prescience, "Was ist das Schwerste von allem? Was dir das Leichste dünket, mit den Augen zu sehen, was vor den Augen dir liegt." (What is the most difficult of all? That which seems to you the easiest, to see with one's eyes what is lying before them.) (*Xenien aus dem Nachlass* #45.)

If looking is not enough, what is one to do? How can the observer quicken his sensitivity to the critical features of the observed? The answer to the question was given me forty years ago, long before I was ready to appreciate it, by my first mentor in graduate school, Walter Fenno Dearborn. In his quiet, crisp New England accent, he once remarked, "Bronfenbrenner, if you want to understand something, try to change it." And whether one studies change by deliberately altering conditions in a contrived experiment or by systematically exploiting an "experiment of nature," the scientific purpose and effect are the same; to maximize one's sensitivity to phenomena through the juxtaposition of the similar but different constitutes the core of the experimental method and creates its magnifying power.

The case presented here for early and continuing application of experimental paradigms should not be misinterpreted as an argument against the use of other methods, such as ethnographic description, naturalistic observation, case studies, field surveys, and so on. These strategies can provide invaluable scientific information and insights. The point being made is a positive one—that the experiment plays a critical role in ecological investigation not only for the purpose of testing hypotheses but, at prior stages, for detecting and analyzing systems properties within the immediate setting and beyond. The special suitability of the experiment for this purpose is highlighted by an adaptation of Dearborn's dictum to the ecological realm: If you wish to understand the relation between the developing person and some aspect of his environment, try to budge the one, and see what happens to the other. Implicit in this injunction is the recognition that the relation between person and environment has the properties of a system with a momentum of its own; the only way to discover the nature of this inertia is to try to disturb the existing equilibrium.

It is from this perspective that the primary purpose of the ecological experiment becomes not hypothesis testing but *discovery*—the identification of those systems properties and processes that affect and are affected by the behavior and development of the

human being. Moreover, if the objective is the identification of systems properties, then it is essential that such systems properties not be excluded from research design before the fact by restricting observation to only one setting, one variable, or one subject at a time. Human environments and, even more so, the capacities of human beings to adapt and restructure these environments, are so complex in their basic organization that they are not likely to be captured through simplistic, unidimensional research models that make no provision for assessing ecological structure and variation. Unlike the classical laboratory experiment in which one focuses on a single variable at a time and attempts to "control out" all others, in ecological research the investigator seeks to "control in" as many theoretically relevant ecological contrasts as possible within the constraints of practical feasibility and rigorous experimental design. Only in this way can one assess the generality of a phenomenon beyond a specific ecological situation and, equally significant from a developmental perspective, identify the processes of mutual accommodation between a growing organism and its changing surroundings. For instance, in studying socialization strategies one might do well to stratify the sample not only, as is commonly done, by social class, but also by family structure and/or child-care setting (home versus center care). Such stratification in terms of two or more ecological dimensions provides a systematically differentiated and thereby potentially sensitive grid that makes possible the detection and description of patterns of organism-environment interaction across a range of ecological contexts. Moreover, given the extraordinary capacity of the species homo sapiens to adapt to its milieu, these patterns are more likely to be complex than simple. To corrupt, somewhat, the classical terminology of experimental design, *in ecological research, the principal main effects are likely to be interactions.*

A line of argument that urges the execution of research in more than one setting, as well as multiple classification by ecological categories both within and across settings, invites the counterargument that it is impractical in terms of the magnitude of the undertaking and the number of subjects required. Thus a critic might contend that, under such circumstances, research on the ecology of human development could be conducted only in large-scale projects far beyond the human and material resources ordinarily available to most established scientists, let alone younger investigators and graduate students. While some large-scale studies are indeed de-

sirable, they have no necessary relation to the research model advocated here. It is not the size but the structure of the design that is critical. For instance, research on ecological transitions—such as the effect on the child of the arrival of a sibling, changes in behavior at home as a function of the child's entry into and progress in school, the adaptation of an adolescent to a new father, or the impact on the family of parental unemployment—by no means requires a large number of subjects and could readily be carried out by graduate students or even undergraduate majors, especially if they worked in collaboration. Furthermore, stratification does not necessarily demand the addition of more subjects but only a systematic recognition of the different ecological contexts from which research subjects come and a deliberate selection to insure that at least the most critical and unavoidable contrasts are represented systematically rather than left to chance. Allowing the latter to occur unheeded not only inflates experimental error but also may deprive the investigator of information bearing on the interaction of different ecological conditions in shaping the course of development. The loss in degrees of freedom associated with stratification is, I suggest, more than compensated for by the gain in knowledge about combinatorial contextual effects. The occurrence of such interactions and their significance for science and social policy are illustrated by the results of specific studies reviewed in the chapters that follow. A number of these are small-scale investigations conducted by a single researcher.

I have emphasized the scientific importance of conducting ecological experiments on environmental influences beyond the immediate setting containing the developing person. Especially powerful in this regard are investigations that address properties of the macrosystem. There are two major strategies for investigating the consistent patterns of development-in-context that characterize particular cultures and subcultures. The first is the comparison of existing groups, as exemplified by the large number of studies of socioeconomic and ethnic differences in child-rearing practices and behavior. But since most of these researches focus on the characteristics of individuals almost to the exclusion of the properties of the social contexts in which the individuals are found, they can shed little light on the process of accommodation between person and environment which constitutes the core of an ecology of human development. There are some notable exceptions to this restricted perspective, but even these more broadly conceived investigations

share with all strictly naturalistic studies the disadvantage of being limited to variations in macrosystems that presently exist or have occurred in the past. Future possibilities remain uncharted, except by hazardous extrapolation.

This restriction of interest to the status quo represents a distinctive characteristic of much American research on human development. This foreshortened theoretical perspective was first brought to my attention by Professor A. N. Leontiev of the University of Moscow. At the time, more than a decade ago, I was an exchange scientist at the Institute of Psychology there. We had been discussing differences in the assumptions underlying research on human development in the Soviet Union and in the United States. In summing up his views, Professor Leontiev offered the following judgment: "It seems to me that American researchers are constantly seeking to explain how the child came to be what he is; we in the U.S.S.R. are striving to discover not how the child came to be what he is, but how he can become what he not yet is."

Leontiev's statement is of course reminiscent of Dearborn's injunction ("If you want to understand something, try to change it."), but it goes much further; indeed, in Leontiev's view, it is revolutionary in its implications. Soviet psychologists often speak of what they call the "transforming experiment." By this they mean an experiment that radically restructures the environment, producing a new configuration that activates previously unrealized behavioral potentials of the subject. Russian developmental psychologists have indeed been ingenious in devising clever experiments that evoke new patterns of response, primarily in the sphere of psychomotor and perceptual development (Cole and Maltzman, 1969). But once Soviet research moves out of the laboratory, the control group disappears, systematic data yield to anecdotal accounts, and the transforming experiment all to often degenerates into dutiful demonstration of ideologically prescribed processes and outcomes.

For rather different reasons, transforming experiments in the real world are equally rare in American research on human development. As Leontiev implied, most of our scientific ventures into social reality perpetuate the status quo; to the extent that we include ecological contexts in our research, we select and treat them as sociological givens rather than as evolving social systems susceptible to significant transformation. Thus we study social class differences in development, ethnic differences, rural-urban differences—or, at the next level down, children from one- versus two-parent homes,

large versus small families—as if the nature of these structures, and their developmental consequences, were eternally fixed and unalterable except, perhaps, by violent revolution. We are loath to experiment with new social forms as contexts for realizing human potential. "After all," we say, "you can't change human nature." This precept underlies our national stance on social policy and much of our science of human development as well.

Research on macrosystem change requires a shift in the nature of the contrasts to be employed in experiments. It is one thing to compare the effects on development of systems or system elements already present within the culture; it is quite another to introduce experimental modifications that represent a restructuring of established institutional forms and values.

The last, and most demanding, of the basic definitions outlining the nature and scope of research on the ecology of human development identifies a strategy of choice for scientific work in this sphere.

DEFINITION 11

A transforming experiment involves the systematic alteration and restructuring of existing ecological systems in ways that challenge the forms of social organization, belief systems, and lifestyles prevailing in a particular culture or subculture.

A transforming experiment systematically alters some aspect of a macrosystem. The alteration may be effected at any level of the ecological environment from the micro- to the exosystem by eliminating, modifying, or adding elements and interconnections.

A general principle pervades all the basic concepts for an experimental ecology of human development. The principle is stated as the first of a series of propositions describing the distinctive characteristics of research models appropriate for investigating development-in-context.

PROPOSITION A

In ecological research, the properties of the person and of the environment, the structure of environmental settings, and the processes taking place within and between them must be viewed as interdependent and analyzed in systems terms.

The specification of these interdependencies constitutes a major task of the proposed approach. The rest of this volume represents a beginning effort in this direction. In the chapters that follow, I

outline in greater detail the distinctive properties of an ecological model, in terms of both theory and research design, that are appropriate for analyzing developmental contexts and processes at each of the four environmental levels. At each level, I have provided one or more concrete examples of investigations—actual when available, hypothetical when not—to illustrate these distinctive properties, by either demonstration or default.

For reasons already indicated, well-designed ecological experiments are, as yet, not easy to find. I have therefore had to invent some examples where they did not exist. Moreover, in many instances there was a dearth not only of relevant research but also of relevant research ideas. Accordingly, the chapters that follow contain even more proposed hypotheses than proposed investigations.

Since the proposed hypotheses have never been tested, at least in the form and context in which they are presented, there is typically no empirical evidence bearing directly on their validity. Nevertheless, in selecting research examples for presentation, I have endeavored to pick those that illustrate at least the promise of the posited relationships. Such evidence, however, will be mostly circumstantial and never compelling or complete. For the present, therefore, the hypotheses can be judged and justified only on theoretical grounds. The ultimate test of empirical investigation still lies ahead.

When and if the test comes, the hypotheses may prove invalid, but that is an outcome that, in science, is neither uncommon nor unrespectable. The proposed investigations, however, may suffer a less honorable fate. Since they are research ideas that have never been tried out, what German psychologists have called Gedanken experiments, the effort to implement them may well reveal fatal flaws in conception, design, or feasibility. But I hope that at the very least they will point the way to fruitful scientific discoveries by future investigators.

PART TWO

Elements of the Setting

3.

The Nature and Function of Molar Activities

I begin my consideration of the elements of the microsystem with a discussion of molar activities because these constitute the principal and most immediate manifestation both of the development of the individual and of the most powerful environmental forces that instigate and influence that development—the actions of other people. To be more explicit: molar activities as exhibited by the developing person serve as indicators of the degree and nature of psychological growth; as exhibited by others present in the situation they constitute the principal vehicle for the *direct* influence of the environment on the developing person.

All molar activities are forms of behavior, but not all behaviors are forms of molar activity. The reason for making the distinction lies in the belief that not all behaviors are equally significant as manifestations of or influences on development. Many are so short-lived as to have minimal import; these are referred to as *molecular* behaviors. Others are more long-lasting but, because they lack meaning to the participants in the setting, have only negligible impact. The definition of molar activity thus emphasizes both some persistence through time and some salience in the phenomenological field of the developing person and of others present in the setting.

DEFINITION 12

A molar activity is an ongoing behavior possessing a momentum of its own and perceived as having meaning or intent by the participants in the setting.

The terms *molar* and *ongoing* are used to emphasize that an activity is more than a momentary event, such as a movement or an utterance; rather, it is a *continuing process* that entails more

than a beginning or an end. A molar activity is distinguished from an *act*, which is perceived as instantaneous and hence molecular in character. Examples of acts are a smile, a knock on the door, a single question, or an answer. The following are molar activities: building a tower of blocks, digging a ditch, reading a book, or carrying on a telephone conversation.

A second, even more distinctive property of molar activities and one that was particularly emphasized by Lewin and his students, is the fact that they are characterized by a *momentum* of their own, a tension system (Lewin, 1935) that makes for persistence through time and resistance to interruption until the activity is completed (Ovsiankina, 1928). For the most part, this momentum is produced by the existence of intent (Birenbaum, 1930)—the desire to do what one is doing either for its own sake or as a means to an end. The presence of intent creates a motive for closure, which in turn leads to perseverance and resistance to interruption. Some molar activities are not characterized by intent, at least in the form of a conscious goal (for example, sleeping, daydreaming, or running aimlessly around the room), but in these cases intention is conspicuous by its absence. The question of perceived aim is thus always relevant for defining an activity, if only by default.

Putting the issue another way, activities vary in the degree and complexity of the purposes that animate them. This variation is reflected along two additional dimensions that are completely phenomenological in character, meaning that they are defined according to how they are perceived by the actor. The first of these subjective domains is *time perspective*, determined by whether the actor perceives the activity as taking place only in the immediate present as she engages in it or as part of a larger temporal trajectory, transcending the bounds of ongoing action, reaching back into the past or forward into the future. This last component, of anticipation, often intersects with the second phenomenological domain: the extent to which the activity is consciously perceived as having an explicit *goal structure*, whether the path to the goal is perceived as direct, involving a single course of action (such as climbing to reach a desired object) or as involving a sequence of steps or subgoals, consisting of a series of preplanned stages (exemplified by organizing a beach hike with younger siblings to look for shells from which to make mother a necklace for next Christmas).

Activities with a complex goal structure typically involve an extended time perspective as well, but the converse is not necessarily true. The goal structure may be quite simple, consisting of

only a single course of action, but entail a long delay of gratification, as in saving up money in a piggy bank to buy a toy.

Another dimension along which molar activities can vary in complexity extends well beyond the parameters of time perspective and goal structure. Activities differ in the *extent to which they invoke objects, people, and events not actually present in the immediate setting.* Such invocation may be accomplished through conversation, story telling, fantasy, pictorial representation, or a variety of other media. To the extent that activities refer to events occurring in other places at other times, they reflect an expansion of the actor's phenomenological world beyond the immediate situation. Thus it is possible to speak of an "ecology of mental life" with a potential structure isomorphic with that of the ecological environment. If a person in a given setting speaks about her own activities in some other setting, either in the past or in the future, she is exhibiting the ability to create a "mental mesosystem." Television brings into the daily experience of children violent events in other places that then find violent expression in the youngster's everyday activities, thus adding an exo- and, perhaps, even more tragically, an entire macrosystem to the child's phenomenological world.

Even when a person's activities are restricted to experiences in and of the immediate setting, they can take on a high order of complexity through the introduction of another element of the microsystem, *relations with other people.* Although many molar activities can be carried out in solitude, some necessarily involve interactions with other persons. Children in particular spend much time in joint activities with adults or age-mates. In the beginning these tend to be dyadic, involving only one other person at a time. But soon the child is able to be aware of and to deal with two or more persons simultaneously, thus maintaining and eventually even creating what are later defined in the ecological schema as $N + 2$ systems.

The fact that the child becomes able to establish complex interpersonal relationships on her own reflects an important principle in the ecology of human development: as the child's phenomenological field expands to include ever wider and more differentiated aspects of the ecological environment, she becomes capable not only of participating actively in that environment but also of modifying and adding to its existing structure and content.

Finally, as the child develops, she becomes capable of *carrying on more than one molar activity at a time.* Although there is no research bearing on the question, it is possible that children acquire

and perfect this skill through contact with parents, especially mothers, who, usually by necessity, become proficient in dealing with their children while continuing to engage in one or more other essential activities.

The emerging molar activities of the child reflect the evolving scope and complexity of the perceived ecological environment, both within and beyond the immediate setting, as well as the child's growing capacity to deal with and alter this environment in accord with his needs and desires. Molar activities are important in yet another respect: when exhibited by others present in the setting, they constitute the main source for *direct* effects of the immediate environment on psychological growth. It follows from the preceding exposition that the development of the child is a function of the scope and complexity of the molar activities engaged in by others that become part of the child's psychological field either by involving her in joint participation or by attracting her attention.

In keeping with the Lewinian precedent, no mention has been made thus far of the substantive nature of molar activities, as distinguished from their structural properties. The reason for not specifying subject matter in advance has already been stated: the question is an empirical one that can be answered only after relevant data have been obtained. It is here that we encounter a major obstacle to more detailed specification of a theoretical framework for an ecology of human development. Researchers as yet know very little about the molar activities of children and their caretakers in the actual settings in which people live out their lives. Laboratory studies have yielded voluminous data about molecular acts, but information about larger behavioral units in natural environments is far more sparse. A notable exception is the painstaking research of Barker, Wright, and their colleagues (Barker and Wright, 1954; Barker and Gump, 1964; Wright, 1967; Barker and Schoggen, 1973) on the "psychological ecology" of childhood. My own theory builds on their work, but departs from it in a number of important ways. First, although similarly emphasizing the importance of studying behavior at a molar level, Barker and his associates tend to concentrate on the *process* of interaction rather than its *content*. Thus most of the analyses involve such variables as dominance, nurturance, compliance, and avoidance rather than categories dealing with the substance of the activity in the course of which these patterns of

relationships were displayed. Second, the focus of attention is on the behavior of individuals taken one at a time; for example, the researcher analyzes the behavior of children, or of caretakers, but not of child and caretaker as a dyadic unit. To state the same point in another way: the behavior of the individual is classified without regard to its relation to the behavior of other persons present in the situation. In short, activities are not viewed in their interpersonal context. Third, consistent with this orientation, the setting is conceived in purely behavioral terms without reference to social structure either in the immediate or the more remote environment. Finally, there is no attempt to examine molar activity from a developmental perspective, to view its complexity and content as reflecting the level of the person's psychological growth. In sum, neither the properties of the person nor of the environment are conceptualized in systems terms.

In the absence of concepts, methods, and data bearing on the content and interpersonal structure of molar activities exhibited in settings of everyday life by persons at varying stages of development, my colleagues and I have undertaken to make a beginning in this threefold task (Nerlove et al., 1978). As a point of departure, we chose to investigate the ongoing behaviors of three- to five-year-old children and their caretakers both at home and in preschool settings, including nursery schools and day care centers. We defined as our initial objective the development of a taxonomy of molar activities in terms of their content, complexity, and interpersonal structure. Two general methodological approaches were employed. In the first an observer was requested to focus on the activities of a particular child and the people around him, and to describe in his own words what the child was doing and what the people around him were doing. The field workers engaged in this task were familiar with both kinds of settings under investigation and came from cultural backgrounds similar to those of the persons being studied. The observers were instructed to describe activities from the perspective of the participants in the setting. The second strategy involved asking the child's principal caretaker, usually the mother, to provide a similar description for one segment of a day—a morning, afternoon, or evening, each including a meal.

Both sets of protocols were subjected to content analysis to identify the categories spontaneously employed for describing those behaviors, of both children and their caretakers, that were judged subsequently by independent coders to meet our criteria for molar

activity. Each activity was classified in four general spheres: content; "psychological momentum," as indicated by initiative, level of concentration, resistance to distraction, resumption after interruption, and so on; complexity of activity structure, as manifested in the number of molar activities carried on simultaneously, extended time perspective, and the presence of sequential subgoals; and the complexity of the perceived ecological field, as reflected in the person's participation in interpersonal systems (dyad, triad, and so on), reference to events in other settings, and the modification or expansion of the life space through fantasy or actual reconstruction of the objective environment.

Satisfactory interjudge reliability ($r = .70$ to $.80$) has been obtained in the coding of parallel running records prepared by pairs of observers independently describing the same events over fifteen-minute periods. The taxonomy will be cross-validated in an ongoing comparative study of the ecology of children and families in five modern industrialized societies (Bronfenbrenner and Cochran, 1976).

The statistical analysis of pilot study data is still under way and, beyond reliability figures, no systematic findings are as yet available. It is instructive, however, to examine the content of the molar activities that have been reported for the American sample. Categories derived from the content analysis of observations in about twenty-five families, and interviews with more than one hundred mothers, fall into the following general domains. At the more passive extreme the first domain, entitled "nonengagement," consists of such pursuits as sleeping, resting, drifting (wandering aimlessly around); the most focused behavior in this domain is waiting. A second sphere contains activities that involve paying attention to people or ongoing events without active participation. Other areas are characterized by enduring emotional states, nonfantasy and fantasy play, games, musical activity, responsibilities and work, educational processes, and activities with a predominantly social purpose.

Each activity is also analyzed for complexity as reflected in simultaneity with other ongoing behaviors, time perspective, goal structure, extent of involvement in an interpersonal system (dyad, triad, and so on), and reference in conversation, fantasy play, or symbolic representation to events, objects, or people not present in the immediate situation.

It may also be instructive to consider the potential significance of the several types of molar activities exhibited by the children in the

American pilot study. Given a theoretical perspective emphasizing the importance of motivational momentum and complexity in the structure of goals and interpersonal systems, activities of nonengagement (for instance, sleeping, resting, daydreaming, wandering aimlessly about, being restlessly hyperactive) are presumed to constitute the lower bound of the developmental continuum. Children observed as spending much of their time in such activities are viewed as less advanced in their psychological growth. At the same time, in keeping with a dynamic concept of the human organism, their preoccupation with these pursuits is seen as an effort to establish or find conditions in which they could function more effectively. The same interpretation applies with even greater force to emotional activities, both negative (such as protracted crying, expressions of anger, or fighting) and positive (joyful states, continuing expressions of affection or approval). Again, these are regarded as attempts either to alter circumstances that impair the capacity to function or to perpetuate and enhance siuations that facilitate developmental processes. The validity of these assumptions must be determined empirically by investigation of the behavioral correlates and consequences of activities of nonengagement and their longer-range sequelae in subsequent patterns of molar activity in other settings.

The domain of attending—paying heed to other people and events—is developmentally significant in constituting the necessary condition for observational learning. Whether such learning in fact occurs can, again, be determined by investigating whether the child subsequently tries to carry out activities he has seen others conduct.

The relevance of educational and musical activities for learning and development is self-evident. But the remaining domains of nonfantasy and fantasy play, games, responsibilities and work, and social activities merit discussion, particularly since they are not accorded high priority in American research on socialization processes and outcomes, or—for that matter—in actual socialization activities taking place in American society.

This neglect is particularly marked for play, fantasy, and games. Although the importance of such activities to developmental processes has been stressed in the theoretical writings and clinical observations of Piaget (1962), the translation of these ideas into research and practice has been minimal, at least in the United States. In a number of other societies, however, play, fantasy, and games are topics of extended scientific study, and the results serve as the basis of recommended practice in homes, preschools, and school curricula. The Soviet Union is a case in point. The research em-

phasis stems from the theories of Vygotsky and his disciples (Elkonin, 1978; Leontiev, 1964; Vygotsky, 1962, 1978; Zaporozhets and Elkonin, 1971), who view play, fantasy, and games as important activities for cognitive, motivational, and social development. Proceeding from this theoretical base, Soviet pedagogues have incorporated many play activities, both imaginary and real, into the preschool and elementary curriculum (Venger, 1973; Zaporozhets and Elkonin, 1971; Zaporozhets and Markova, 1976; Zhukovskaya, 1976). As the children grow older, increased importance is accorded to the educational benefits of what the Russians call *rolevaya igra*, role-playing games, in which children take roles that are common in adult society, for instance, store clerk, customer, nurse, patient, and so on. A fuller description of such activities appears elsewhere (Bronfenbrenner, 1970a).

Soviet educators use play, fantasy, and games primarily to develop what they refer to as "communist morality." From an American perspective, the Russian outcome would be viewed as representing a remarkably high level of social conformity and submission to authority. These effects are documented in a series of experiments on reactions to social pressure on the part of Soviet school children compared with age-mates from the United States and other Western societies (Bronfenbrenner, 1967, 1970b; Garbarino and Bronfenbrenner, 1976; Kav-Venaki et al., 1976; Shouval et al., 1975). There is reason to believe that play, fantasy, and games can be just as effectively utilized to develop initiative, independence, and equalitarianism. Indeed such activities probably function precisely in this fashion in contemporary American settings both within and outside school. The relevant research has yet to be carried out, and will require an appropriate taxonomy of activities that extends to children of elementary school age and beyond. One can anticipate, however, that various aspects of play, fantasy, and games will relate not only to the development of conformity versus autonomy but also to the evolution of particular forms of cognitive function. It is noteworthy in this regard that, in the course of pilot-testing our activities code, we observed the most complex cognitive operations in the realm of fantasy play.

The relevance of social and work-related activities to human development can be expressed in two statements for which no research documentation as yet exists (a fact that, paradoxically, can be interpreted as reflecting the validity of the statements themselves). First, in the United States it is now possible for a person eighteen years of age to graduate from high school without ever

having had to do a piece of work on which somebody else truly depended. If the young person goes on to college, the experience is postponed for another four years. If he goes on to graduate school, some might say the experience is postponed forever.

The second statement points to what may be an even more destructive outcome in the long run. In the United States, it is now possible for a person eighteen years of age, female as well as male, to graduate from high school, college, or university without ever having cared for, or even held, a baby; without ever having looked after someone who was old, ill, or lonely; or without ever having comforted or assisted another human being who really needed help. Again, the psychological consequences of such a deprivation of human experience are as yet unknown. But the possible social implications are obvious, for—sooner or later, and usually sooner—all of us suffer illness, loneliness, and the need for help, comfort, or companionship. No society can long sustain itself unless its members have learned the sensitivities, motivations, and skills involved in assisting and caring for other human beings.

Yet the school, which is the setting carrying primary responsibility for preparing young people for effective participation in adult life, does not, at least in American society,[1] give high priority to providing opportunities in which such learning could take place. This would not be impossible to achieve. For some years I have been advocating the introduction in our schools, from the earliest grades onward, of what I have called a *curriculum for caring* (Bronfenbrenner, 1974b, 1974c, 1978b). The purpose of such a curriculum would be not to learn *about* caring, but to engage *in* it: children would be asked to take responsibility for spending time with and caring for others—old people, younger children, the sick, and the lonely. It would be essential that such activities be carried out under firm supervision, and this supervision could not be provided by already overburdened teachers. Instead, the supervisors should be drawn from persons in the community who have experience in caring—parents, senior citizens, volunteer workers, and others who understand the needs of those requiring attention and the demands on those who would give it. Obviously such caring activities cannot be restricted to the school—they will have to be carried on in the outside community. It would be desirable to locate caring institutions, such as day care centers, adjacent to or even within the school. But it would be even more important for the young caregivers to come to know the circumstances in which their charges live and the people in their lives. For example, an

older child taking responsibility for a younger one might come to know the latter's family and become acquainted with his neighborhood by escorting him home from school. In this way, the older children, as well as the adults involved in the program, would learn to know at first hand the living conditions of the people in their community.

My purpose in describing the proposed program here is not to advocate its adoption but to illustrate ecological concepts, their concrete implications for developmental research, and the essential interplay between issues of public policy and basic science in the study of development-in-context. Viewing the suggested project first from a social policy perspective, it is clear that before any such curriculum for caring is introduced on a broad scale, it should be tested experimentally and its putative effects, along with possible unintended consequences, evaluated. But once it becomes a research enterprise, an effort of this kind also constitutes an excellent example of what I have called a transforming experiment, since it calls into question and alters in a substantial way a prevailing pattern in the American macrosystem, the current "blueprint" for what a school curriculum should and should not contain. Indeed as a scientific undertaking, the proposed program entails changes at all four levels of the ecological environment. Thus it reaches beyond the microsystem of the classroom to invoke new interconnections among home, school, and neighborhood at the level of the mesosystem. To the extent that adults from the community who become directly involved with the program are influenced to introduce changes in other settings in which they participate (for instance, committees, offices, and organizations), the curriculum may also have exosystem effects. Within the most immediate environmental sphere, the suggested project involves alteration not only in molar activities but also in the microsystem elements of role and interpersonal structure. It is in fact by introducing changes in the traditional role expectations for pupils and children that new activities involving new patterns of social interactions are set in motion. As we shall discover (chapter 5), the creation and allocation of roles is an especially powerful strategy for influencing the course of human development.

Even if the proposed curriculum did not bring about significant change in the prevailing conception of what schools are or should be trying to accomplish, it would still be important both for science and public policy to document the kind of molar activities that are now occurring in our classrooms. For the availability of

such information, whether for schools or other human habitats (homes, day care centers, playgrounds, peer group hangouts, places of work, retirement homes, and so on), would permit assessment of both the developmental status of the person and the power of the "activity milieu" to stimulate or stifle psychological growth.

Molar activities thus have manifold functions in regard to human development, since they can serve equally, and sometimes simultaneously, as cause, context, and consequence of psychological growth. But for purposes of research, it is necessary to keep these functions separate; in particular, neither cause nor context should be confounded with outcome. In accord with this methodological principle, the conclusion concerning the significance of molar activities for development is stated in two parts—the first, a proposition dealing with molar activities as developmental outcomes, and the second, a hypothesis setting forth the function of the activity milieu as a context and potential influence on developmental processes.

PROPOSITION B

The developmental status of the individual is reflected in the substantive variety and structural complexity of the molar activities which she initiates and maintains in the absence of instigation or direction by others.

Substantive variety refers to the range in content of these activities. Structural complexity is manifested in the evolving scope and differentiation of the developing person's perceived ecological environment, both within and beyond the immediate setting, as well as in her growing capacity to deal with and alter that environment in accord with her own needs and desires.

HYPOTHESIS 1

The development of the person is a function of the substantive variety and structural complexity of the molar activities engaged in by others who become part of the person's psychological field either by involving her in joint participation or by attracting her attention.

As the two foregoing statements imply, the person's perceptions of and interaction with others, in both the immediate and the more remote environment, are especially salient both as influences on and manifestations of development. The emerging structure and content of these relations, and their developmental implications, are thus of particular interest to us.

4.

Interpersonal Structures as Contexts of Human Development

We begin with a definition.

DEFINITION 13
A relation obtains whenever one person in a setting pays attention to or participates in the activities of another.

The presence of a relation in both directions establishes the minimal and defining condition for the existence of a *dyad*: a dyad is formed whenever two persons pay attention to or participate in one another's activities.

The dyad is important for development in two respects. First, it constitutes a critical context for development in its own right. Second, it serves as the basic building block of the microsystem, making possible the formation of larger interpersonal structures—triads, tetrads, and so on. In terms of its potential for furthering psychological growth, there are three different functional forms that a dyad may take.

1. An *observational dyad* occurs when one member is paying close and sustained attention to the activity of the other, who, in turn, at least acknowledges the interest being shown. For example, a child watches closely as a parent prepares a meal and makes occasional comments to the child. This type of dyad obviously meets the minimal condition necessary for observational learning, but stipulates an additional interpersonal requirement: not only must the activity of the other person actually be a focus of attention, but also that person must make some overt response to the attention being shown. Once an observational dyad is in existence, it readily evolves into the next, more active dyadic form.

2. A *joint activity dyad* is one in which the two participants per-

ceive themselves as doing something together. This does not mean that they are doing the same thing. On the contrary, the activities each engages in usually tend to be somewhat different, but complementary—part of an integrated pattern. For example, parent and child may be looking at a picture book; the mother tells the story, while the child names objects in response to her questions. A joint activity dyad presents especially favorable conditions not only for learning in the course of the common activity but also for increasing motivation to pursue and perfect the activity when the participants are no longer together.

The developmental power of a joint activity dyad derives from the fact that it enhances, and thereby exhibits in more marked degree, certain properties that are characteristic of all dyads.

Reciprocity. In any dyadic relation, and especially in the course of joint activity, what A does influences B and vice versa. As a result, one member has to coordinate his activities with those of the other. For a young child, the necessity of such coordination not only fosters the acquisition of interactive skills, but also stimulates the evolution of a concept of interdependence, an important step in cognitive development.

Furthermore reciprocity, with its concomitant mutual feedback, generates a momentum of its own that motivates the participants not only to persevere but to engage in progressively more complex patterns of interaction, as in a ping-pong game in which the exchanges tend to become more rapid and intricate as the game proceeds. The result is often an acceleration in pace and an increase in complexity of learning processes. The momentum developed in the course of reciprocal interaction also tends to carry over to other times and places: the person is likely to resume his or the other person's "side" of the joint activity in other settings in the future, either with others or alone. It is in this way that dyadic interaction, especially in the course of joint activity, produces its most powerful developmental effects.

Balance of power. Even though dyadic processes are reciprocal, one participant may be more influential than the other. For example, in a tennis game, one player, during a long volley, drives the other into a corner. The extent to which, in a dyadic relation, A dominates B is referred to as balance of power. This dyadic dimension is important for development in several respects. For a young child, participation in dyadic interaction provides the opportunity for learning both to conceptualize and to cope with differential power relations. Such learning contributes simultaneously to cog-

nitive and social development, since power relations characterize physical as well as social phenomena encountered by the growing person in a variety of ecological settings throughout the life span.

Balance of power is significant in yet another, more dynamic respect, since there is evidence to suggest that the optimal situation for learning and development is one in which the balance of power gradually shifts in favor of the developing person, in other words, when the latter is given increasing opportunity to exercise control over the situation.

Joint activity dyads are especially well suited to this developmental process. They stimulate the child to conceptualize and cope with power relations. At the same time, they provide an ideal opportunity for effecting a gradual transfer of power. Indeed this transfer often takes place "spontaneously" as a function of the active character of the developing person in relation to the environment.

Affective relation. As participants engage in dyadic interaction, they are likely to develop more pronounced feelings toward one another. These feelings may be mutually positive, negative, ambivalent, or asymmetrical (as when A likes B, but B dislikes A). Such affective relations tend to become more differentiated and pronounced in the course of joint activity. To the extent that they are positive and reciprocal to begin with and become more so as interaction proceeds, they are likely to enhance the pace and the probability of occurrence of developmental processes. They also facilitate the formation of the third type of two-person system, a primary dyad.

3. A *primary dyad* is one that continues to exist phenomenologically for both participants even when they are not together. The two members appear in each other's thoughts, are the objects of strong emotional feelings, and continue to influence one another's behavior even when apart. For example, a parent and child, or two friends, miss each other when they are not together, imagine what they might be doing, what the other one might say, and so on. Such dyads are viewed as exerting a powerful force in motivating learning and steering the course of development, both in the presence and absence of the other person. Thus a child is more likely to acquire skills, knowledge, and values from a person with whom a primary dyad has been established than from one who exists for that child only when both are actually present in the same setting.

Although each has its distinctive properties, the three dyadic forms are not mutually exclusive; that is, they can occur simultaneously as well as separately. A mother and her preschool child's

reading a book together is obviously a joint activity taking place in the context of a primary dyad. But if the child's part is mainly one of listening attentively as the mother reads aloud, the dyad is clearly also an observational one. As might be expected, such combined structures have a more powerful developmental impact than dyads confined to a single type. This point will be taken into account in the consideration of specific dyadic hypotheses and methods for their investigation.

The dyadic properties and principles I have outlined may be summarized in the form of a series of hypotheses describing the presumed impact of various types of dyadic structure on developmental processes. I begin by calling attention to an evolutionary process at the level of the dyad itself. The first two hypotheses postulate that dyads can undergo a course of development just as individuals do.

HYPOTHESIS 2
Once two persons begin to pay attention to one another's activities, they are more likely to become jointly engaged in those activities. Hence observational dyads tend to become transformed into joint activity dyads.

HYPOTHESIS 3
Once two persons participate in a joint activity, they are likely to develop more differentiated and enduring feelings toward one another. Hence joint activity dyads tend to become transformed into primary dyads.

The next hypothesis specifies the dyadic properties conducive to development.

HYPOTHESIS 4
The developmental impact of a dyad increases as a direct function of the level of reciprocity, mutuality of positive feeling, and a gradual shift of balance of power in favor of the developing person.

The hypotheses that follow deal with the joint effects produced when different kinds of dyads occur simultaneously.

HYPOTHESIS 5
Observational learning is facilitated when the observer and the person being observed regard themselves as doing something together. Thus the developmental impact of an observational dyad

tends to be greater when it takes place in the context of a joint activity dyad (a child is more likely to learn from watching a parent cook a meal when the activity is structured so that the two are acting together).

HYPOTHESIS 6
The developmental impact of both observational learning and joint activity will be enhanced if either takes place in the context of a primary dyad characterized by mutuality of positive feeling (one learns more from a teacher with whom one has a close relationship). Conversely, mutual antagonism occurring in the context of a primary dyad is especially disruptive of joint activity and interferes with observational learning.

Finally, if all these considerations are taken into account, one can stipulate the optimal conditions for learning and development in a dyadic relationship.

HYPOTHESIS 7
Learning and development are facilitated by the participation of the developing person in progressively more complex patterns of reciprocal activity with someone with whom that person has developed a strong and enduring emotional attachment and when the balance of power gradually shifts in favor of the developing person.

The question may be raised whether a positive relationship between the members of a dyad is essential as long as the participants continue to engage in progressively more complex patterns of reciprocal activity. The question assumes that the second condition is independent of the first. I shall cite evidence from research for the debilitating impact of antagonism between the participants on the functioning of the dyad as a developmental system.

Because of their focal role in the ecology of human development, it is convenient to have a single term for dyads that meet the optimal conditions, stipulated in hypotheses 4 through 7, of reciprocity, progressively increasing complexity, mutuality of positive feeling, and gradual shift in balance of power. Accordingly, I will refer to two-person systems exhibiting these properties as *developmental dyads.*

Although studies dealing with dyads are fairly common in the literature of both social and developmental psychology, few of these investigations bear directly on issues of development, once again

for the reason that they are limited to a single setting at a single point in time and hence do not meet the criterion of developmental validity. Even rarer are researches that provide evidence for or against the dyadic hypotheses. One series of experiments and follow-up studies, however, despite a glaring omission in the data, dramatically documents the motivating power and long-range developmental effect of the dyad as a context for development.

The thesis that behavior in dyads is generally reciprocal is widely accepted in theory, but it is often disregarded in research practice. The failure to take two-way processes into account reflects the inertia of the traditional laboratory model with its classical participants —an experimenter, identified cryptically as E, and another person equally informatively described as S, the subject. The term *subject* is apt, for with few exceptions the process operating between E and S is viewed as unidirectional; the experimenter presents the stimulus, and the subject gives the response. Of course in theory the influence can occur in both directions, but once the researcher puts on the white coat of scientific invisibility, she tends to focus solely on the behavior of the experimental subject, even when someone besides the experimenter is an active participant in the setting.

A case in point is the work of Klaus, Kennell, and their colleagues at the Case Western Reserve School of Medicine (Hales, 1977; Hales, Kennell, and Susa, 1976; Kennell, Traus, and Klaus, 1975; Kennell et al., 1974; Klaus and Kennell, 1976; Klaus et al., 1970, 1972; Ringler, 1977; Ringler et al., 1975). The investigators took as their point of departure observations on animals revealing complex, species-specific patterns of mother-neonate interaction immediately after delivery (Rheingold, 1963). Their aim was to explore this phenomenon in humans. Noting that still-prevailing hospital practices resulted in minimal opportunities for contact between mother and newborn, the researchers modified the established procedures to permit mothers to have their naked infants with them for about an hour shortly after delivery and for several hours daily thereafter. To avoid chilling, a heat panel was provided over the mothers' beds. Randomly assigned control groups experienced the type of "contact with their babies that is routine in American hospitals (a glance at their baby shortly after birth, a short visit six to twelve hours after birth for identification purposes, and then 20- to 30-minute visits for feeding every four hours during the day)" (Kennell et al., 1974, p. 173). To insure comparability a heat panel was installed over the control mothers' beds as well. Neither group knew that the other was being treated differently.

The reported results of these experiments strain the credulity of the reader. In the initial experiment (Klaus et al., 1970), all mothers of full-term infants in the extended exposure group exhibited "an orderly progression of behavior": "The mothers started with fingertip touch on the infants' extremities and proceeded in 4 to 8 minutes to massaging, encompassing palm contact on the trunk . . . Mothers of normal premature infants permitted to touch them in the first 3 to 5 days of life followed a similar sequence, but at a much slower rate" (p. 187). The mothers of full-term babies in the experimental treatment also "showed a remarkable increase in the time spent in the 'en face' position in only 4 to 5 minutes" (p. 190).

In a second study (Klaus et al., 1972) with a new sample, fourteen "extended-contact" mother-infant pairs and an equal number of randomly assigned controls, well matched on developmental and family background factors, were compared when their children were one month old. All the mothers were primiparous, with healthy, full-term infants. In this and other follow-up studies, none of the observers knew to which group the subjects belonged. During a hospital examination one month after birth, the mothers in the extended-contact group significantly more often stood and watched beside the examination table and soothed their babies when they cried. They also showed greater fondling and eye-to-eye contact while feeding their babies and, in an interview, expressed greater willingness to pick up their infants when they fussed and more reluctance and anxiety about leaving the baby in someone else's care. Moreover, these differences were still in evidence when the infants were reexamined at one year of age (Kennell et al., 1974). The mothers in the extended-contact group reported missing the baby more when separated from it; during the physical examination, they were again more likely to stand by the tableside and assist the physician, to soothe the infant when it cried, and to kiss their babies.

In a subsequent follow-up study (Ringler et al., 1975), when the infants were two years old, the mother's conversation with the child was observed and recorded during a free play period in a setting containing toys and books. "Speech patterns of the mothers revealed that those who had been given extra contact with their infants during the neonatal period used significantly more questions, adjectives, words per proposition, and fewer commands and content words than did the control mothers" (p. 141).

The most recent experiment in the series (Hales, Kennell, and Susa, 1976) not only provides a much-needed replication of the

initial studies with a larger sample ($N = 60$) but does so in a different cultural context and with a more rigorous experimental design that permits resolving the issue of whether there exists a critical period of susceptibility to extended contact between mother and infant. Although the original investigators spoke of "a special attachment period for an adult woman" (Klaus et al., 1972, p. 463), they acknowledged that their data left open the question of timing: was it a matter of the first few hours after birth, or extended contact over the next several days? In the latest experiment carried out at Roosevelt Hospital in Guatemala, Hales and her associates clarified this issue by introducing two early-contact groups, one limited to forty-five minutes immediately after delivery and the second for an equal interval but beginning twelve hours after the infant's birth. The results were unequivocal. Only the mothers in the immediate contact group were affected:

> Mothers who had contact with their neonates immediately after birth showed significantly more affectionate behavior ("en face," looking at the baby, talking to the baby, fondling, kissing, smiling at the infant) when compared to the mothers in the delayed and control groups . . . No significant differences were noted between the delayed and control groups. This study indicates that the maternal sensitive period is less than twelve hours in length, suggests the importance of skin to skin contact and compels reconsideration of hospital practices that even briefly separate mother and infant. (Hales, Kennell, and Susa, 1976, p. 1)

From an ecological perspective, even more remarkable than the dramatic results reported in this series of experiments are the data they omit. In none of the papers cited is there a single word about the behavior of the infant in the mother-infant dyad, and all the experimental effects are attributed entirely to the mother. Thus the investigators refer repeatedly to a "maternal sensitive period" (Klaus et al., 1972, p. 463) or a special attachment or sensitivity period existing "in the human mother" (Kennell et al., 1974, p. 173; Kennell, Trause, and Klaus, 1975, p. 87). Given the dyadic property of reciprocity, the question naturally arises whether the distinctive behavior of the mothers in the experimental group during the initial early contact, subsequent extended exposure, and later follow-up might not have occurred, at least in part, as a response to a sequence of activities *initiated by the developing infant* and reciprocated by the mother in a progressively evolving pattern of social interaction. The possibility remains unexplored. In keeping with the classic experimental model, the focus of scientific attention in these studies

was limited to the subjects of the research, who in this instance were not the children but the mothers. The omission is all the more striking given the fact that not only were the infants always present in the research situation, but all the mother's behavior being observed was directed toward them.

To be sure, in the most recent reports (Kennell, Trause, and Klaus, 1975; Ringler, 1977) follow-up data are reported on the children's developmental status at age five as related to the mother's behavior toward the child at younger ages. Although the reports still do not provide any information about the behavior of the infant toward the mother at the earlier period, the results nevertheless merit serious consideration. Ringler found that, in comparison with controls, "the five-year-olds of the early contact mothers had significantly higher IQ's, understood language as measured by a receptive language test significantly better and comprehended significantly more phrases with two critical elements" (p. 5). The IQ difference was approximately seven points. Furthermore, there were significant correlations between measures of the complexity of speech patterns employed by mothers toward their infants when the latter were two years of age and indexes of the child's level of language comprehension and performance at age five. The Pearson product-moment coefficients ranged from .72 to .75. A significant correlation of .71 was found between the child's IQ at age five and "the amount of time women spent looking at their babies during the filmed feeding at one month of age" (Kennell, Trause, and Klaus, 1975, p. 93).

These latest findings are urgently in need of replication, especially in view of the small number of subjects. Nevertheless, despite the lamentable absence of data on the infant's side of the dyadic equation, this series of experiments presents persuasive evidence for the scientific utility and promise of the concepts and hypotheses presented above. Thus the studies provide a glimpse—albeit tantalizingly one-sided—of the process through which the joint activity of mother and newborn leads to the formation of a primary dyad, which in turn sets the pace and steers the course of future development. Because one sees only the mother's part of the interaction, it is impossible to assess the level of reciprocity, the degree of mutual positive feeling, or the shift in balance of power from mother to infant (or perhaps from infant to mother?). As in listening to one side of an animated telephone conversation, one may sense the back and forth movement, the response in kind, and the rise and fall of pressure coming from the other end of the line. But scientists have yet to record the two ends of the "conversation" simultaneously and

especially to trace the resultant trajectory of development for *both* parties.

An important theoretical insight was thus ironically provided by the one-sided focus of the Western Reserve studies. By documenting the evolution of the mother's behavioral and emotional involvement with the infant, rather than the reverse, the investigators showed that in the course of dyadic interaction the mother is living through a developmental experience no less profound or consequential than that experienced by her offspring. In keeping with our conception of and criteria for developmental change, the mother does indeed manifest a progressively more extended and differentiated view of a newly prominent aspect of her environment (that is, the arrival of her child) and becomes motivated and able to undertake new activities in dealing with the environment that are of a high order of complexity in form and content. And, what is most critical for establishing that development has in fact taken place, these newly developing perceptions and activities clearly have their sequelae in other places and at other times, in this instance as much as five years later.

Since it is safe to assume that the child, too, has experienced psychological growth during this period, we arrive at a key proposition regarding the developmental properties of a dyad.

PROPOSITION C
If one member of a dyad undergoes developmental change, the other is also likely to do so.

The basic principle underlying this proposition is, of course, in no way new. With respect to mother-infant interaction, it received its definitive statement a decade ago in Harriet Rheingold's classic paper entitled "The social and socializing infant" (1969a). As far as research practice is concerned, however, the principle has been mainly ignored. Indeed the Western Reserve experiments, even though they do not view the dyad as a reciprocal system, represent a step beyond typical experimental studies, which, being limited to a single setting and point in time, cannot provide evidence for the occurrence of enduring developmental effects as distinguished from temporary reactions to the immediate situation that are of no lasting significance.

This qualification highlights a distinctive feature of proposition C, which identifies the dyad as a context not merely of reciprocal interaction but of *reciprocal development*. It is from this point of

view that the dyad, especially as it evolves into a primary relationship, constitutes a "developmental system"; it becomes a vehicle with a momentum of its own that stimulates and sustains developmental processes for its passengers as long as they remain interconnected in a two-person bond. Other, high-order interpersonal systems also exhibit this dynamic property but have additional features that introduce further complexity into the developmental equation.

Before turning to a consideration of these higher-order subsystems, I would like to emphasize the significance of the Western Reserve studies at a broader ecological level. Taken as a whole, this series of experiments on the effects of early, extended mother-infant contact provides excellent examples of several defining properties of an ecological research model, by both demonstration and default. On the positive side, the work constitutes a clear instance of ecologically valid experimentation focused directly on developmental processes. Moreover, it presents a fine illustration of how experimental intervention can bring to light critical features of an ecological process hardly likely to be identified through straightforward naturalistic observation in the unaltered existing setting. Last, but hardly least, the work provides an actually executed example of a transforming experiment. The investigators have deliberately and dramatically altered the established routine in American hospitals, clearly a macrosystem phenomenon. And they have done so "in ways that challenge the prevailing forms of social organization, belief systems, and lifestyles" (definition 11), in this instance at the level of the society as a whole. It is ironic that, at the same time, this series of studies exemplifies a striking ecological omission, a failure to take into account the actual system operating in the given environment.

This dramatic lacuna in an otherwise impressive body of research gives rise to the next proposition.

PROPOSITION D

An analysis of the microsystem must take into account the full interpersonal system operating in a given setting. This system will typically include all the participants present (not excluding the investigator) and involve reciprocal relations between them.

Once this proposition is formulated, it immediately suggests that perhaps the infant was not the only forgotten participant in the Western Reserve experiments: what about nurses, visitors, not to

mention attending physicians, two of whom were apparently the principal investigators of the project? When these parties were present, as they surely were, did they act in similar fashion toward the mothers and infants in the experimental as compared with the control groups? Or did the lengthier and more intensive interaction between mother and newborn under the condition of extended exposure invite more approving comments to the mother about the infant and her way of handling him? Did the striking departure from the usual hospital routine lead the mothers to ask questions, and if so, how did the staff members respond?

As for family members or other visitors, it is noteworthy that, both in the United States and Guatemala, the experiments were conducted in hospitals serving primarily nonwhite populations from poor socioeconomic backgrounds. In the American controlled experiment (Kennell et al., 1974), the only one for which such background data are provided, of the fourteen mothers in each group all but one was black, two-thirds were unmarried, and all the children were first-born. Is it conceivable that the mothers' subordinate social status, cultural background, and (for most of them) their special position as a single mother of a first child, predisposed them to forming the strong kind of attachment they exhibited to their offspring? To put the same question in operational terms, would similar results have been obtained with a sample of white, middle class, two-parent families having their second or third child? The original investigators have acknowledged the importance of this issue.

> The group of lower-class staff mothers has both advantages and disadvantages for a study of maternal attachment. The mothers had not been to childbirth classes, so they did not know what to expect in the hospital. They had done little reading, so they were rather "pure" for the purposes of this study. Almost all were black and their incomes and circumstances were similar in both groups. One difficulty with studies of maternal behaviour is that when people in the community begin to hear about it, their behaviour changes. Educated mothers may then behave in a special way because of what they have heard or read. (Kennell, Trause, and Klaus, 1975, p. 96)

In confining themselves to a two-person model, the Western Reserve investigators reflect yet another influence of the traditional laboratory paradigm. As previously noted, the classical psychological experiment allows for only two participants: E and S. Even in those researches that take into account the activities of more than two persons, the behavior of each is usually analyzed sepa-

rately and interpreted as an independent effect. An example is provided by research on father-infant interaction.[1] Much of this work treats the behavior of the father, and any reaction it may evoke in the child, in exclusively class-theoretical terms (Lewin, 1935) as attributable entirely to the father, without regard to the possibility that both the father's action and the child's responses may be influenced by the mother—her presence, absence, and the possible effect of her behavior on the interaction of the father with the child. I refer to this kind of indirect influence as a *second-order effect.* To state the issue in propositional form

PROPOSITION E
In a research setting containing more than two persons, the analytic model must take into account the indirect influence of third parties on the interaction between members of a dyad. This phenomenon is called a *second-order effect.*

This proposition represents an extension and further specification of proposition D as applied to a system involving more than two persons, referred to henceforth as an $N + 2$ *system.* Three recent studies of parent-child interaction that, explicitly or implicitly, employed a three-person model illustrate the application of the principle. Parke (1978) and his coworkers observed both parents with their newborns in a hospital setting to determine what effect each parent had on the other's interactions with the infant. In each case

> the presence of the spouse significantly altered the behavior of the other parent, specifically, both father and mother expressed more positive affect (smiling) toward their infant and showed a higher level of exploration when the other parent was also present . . . These results indicate that parent-infant interaction patterns are modified by the presence of another adult; in turn, the implication is that we have assumed prematurely that parent-infant interaction can be understood by our sole focus on the parent-infant dyad alone. (Pp. 86–87)

Support for Parke's conclusion comes from a study by Pederson (1976), in which the second-order effect is somewhat more remote but equally consequential. This investigator examined the influence of the husband-wife relationship (assessed through interview) on mother-infant interaction in a feeding context (as observed in the home). His results are summarized as follows: "The husband-wife relationship was linked to the mother-infant unit. When the father was supportive of the mother . . . she was more effective in feeding the baby . . . High tension and conflict in the marriage was asso-

ciated with more inept feeding on the part of the mother" (p. 6). Pederson also found that the developmental status of the infant, as measured on the Brazelton scale, was inversely related to the degree of tension and conflict in the marriage. Consistent with the principle of reciprocity, he notes that causality could occur in either direction.

Pederson's results indicate that the second-order effect can have inhibitory as well as facilitative impact. Indeed, Lamb interprets the results of three pioneering experiments explicitly designed to investigate second-order effects (Lamb, 1976b, 1977, 1978) as demonstrating that, beginning with the second year of life, the presence of the second parent reduces rather than increases parent-child interaction. His data do indeed show higher levels of interaction for a two-person parent-child system, but the interpretation is complicated by two problems of ecological validity. First, all the experiments were carried out in the laboratory. As I shall document below, a number of comparative studies (including one by Lamb) have shown that both parents and children behave rather differently in laboratory than in home settings. Second and more critical, the design employed in all three experiments involved a confounding of size of system with differing instructions given to the adult subjects about how they should behave toward each other as compared with the infant. Although they were asked to respond to the child's initiatives, the adults were enjoined from initiating interaction with the child but told to "chat to one another normally" (Lamb, 1977, p. 640). This directive meant that when an adult and a child were alone in the room, there was nothing to distract the former from reacting to the child's behavior. Once two adults were present, however, they were supposed to talk to each other. Thus their attention was focused on each other and drawn away from the infant. Under these circumstances, it is hardly surprising that adult-child interaction was lower in the three- than the two- person situation.

Lamb's interpretation of the observed difference as a second-order effect due solely to the presence of a second adult provides another example of failure to take cognizance of the actual interpersonal system operative in the setting (proposition D). It also illustrates the danger of artificially restricting the habitual behavior of research subjects, as is frequently done in laboratory experiments. While the results may be statistically reliable, they can also be experimental artifacts and hence ecologically invalid.

Again, this criticism does not mean that laboratory studies are

necessarily suspect. When employed in proper ecological perspective, they often constitute the scientific strategy of choice. For example, if the laboratory is viewed as what it almost invariably is for a young child—namely, a "strange situation" (Ainsworth and Bell, 1970)—it reveals clearly the role of the parent as a source of security for the child and, in terms of a three-person model, as a catalyst for the child's interaction with the environment, including other, unfamiliar persons. Thus in all the strange-situation experiments, the mother's presence in the laboratory reduces the child's anxiety and resistance to the "stranger." The effect is even more pronounced in the home. For example, Lamb (1975, 1976c, 1977) finds that infants in the company of their parents look and smile at the stranger more often than at their mothers.

$N + 2$ systems and second-order effects of course occur in other settings. An instructive example from the school classroom is provided by Seaver (1973) who ingeniously exploited an "experiment of nature" to investigate the controversial phenomenon of induced teacher expectancies first reported by Rosenthal and Jacobsen (1968) and referred to by them as "Pygmalion in the classroom." Seaver's research was motivated by some reservations regarding the ecological validity of methods previously employed for the study of this phenomenon. In his words, "Most previous attempts to demonstrate the teacher expectancy effect have used experimental manipulations of teacher expectancies that were artificial and surely unusual in the experience of the teacher. Quite possibly these manipulations were also implausible to the teacher and induced psychological states other than the desired expectancies" (p. 334).

To achieve ecological validity, Seaver examined differences in the academic achievement of elementary school pupils with older siblings who had had the same teacher and performed either exceptionally well or exceptionally poorly. Children taught by teachers who had not instructed the older siblings served as controls. In contrast to earlier studies, which had produced inconsistent, weak, or questionable effects, the results of Seaver's natural experiment gave substantial support to the teacher-expectancy hypothesis. As Seaver himself acknowledged, however, it was not clear who was the mediator of the observed effect. Were the teacher's expectations changed because of her prior experience with the older sibling, or did the younger sibling evoke a different response from the teacher because of the younger child's expectations created by the older sibling or by the parents (based on their previous acquaintance with the teacher), or both? The remaining ambiguity in interpreta-

tion testifies to the importance of identifying and analyzing existing interpersonal systems and higher-order effects as stipulated in proposition E.

The involvement of one or both parents as intermediaries in a process already involving two siblings and a teacher would escalate the system from a triad to a quartet or quintet, or, more generally, an $N + 3$ system. To my knowledge, no empirical studies using such a model have been carried out, despite the fact that the so-called typical American family consisting of two parents and two children constitutes a readily available example.[2]

The family is at once the richest and most underused source of natural experiments on the developmental impact of $N + 2$ systems and second-order effects. In homes and families, one does not even have to introduce contrived variations in system size, for nature provides them on a daily basis. Parents and siblings—as well as relatives, neighbors, and friends—frequently come and go, providing ready-made experiments of nature with built-in ecological validity and a before-after design in which each subject can serve as her own control. The comings and goings are of two kinds. There are the temporary and recurring arrivals and departures, as adults and children go in and out of the room, friends and neighbors drop by, or—on a more predictable basis—family members leave for and return from work, school, and recreation, relatives come for a weekend or a week, or a parent gets a vacation from work. Then there are more lasting changes: a second child is born, grandma moves in to help with the children, mother goes off to work when the children are old enough (and "old enough" now comes sooner every year), grandmother dies and the family gets a regular babysitter, there is a separation or divorce and father leaves, after a few years the mother remarries, and so on.

Both temporary and lasting changes in system size can produce second-order effects. One can observe whether and how mother-child interaction changes when the father enters or leaves the room or how the total pattern of family activity is restricted when a second child arrives, mother takes a job, or father moves out. Given the frequency of such events, particularly in certain segments of contemporary society, one would expect that these experiments of nature would not have escaped scientific attention. But in an effort to find examples in the research literature, I have been able to discover only two studies employing the suggested strategy. The first is also one of the few investigations documenting the effect of an ecological transition *within* the family—the role change involved

when a woman becomes the mother of a second child. The work was done over thirty years ago by a prescient leader in the field (Baldwin, 1947) and involved observations of maternal behavior toward the first child before, during, and after the mother's pregnancy with another child. Baldwin summarizes his results as follows: "All of these changes are linear in form. They suggest that the addition of another child in the family tends to reduce the warmth and contact between the parent and other children and to result in a more restrictive but less effective home" (p. 38).

Unfortunately, in keeping with a traditional research model Baldwin's research, like the Western Reserve studies, focused exclusively on one member of the dyad; data are provided only on the behavior of the parent and do not include that of the child. The rich scientific benefits to be gained by adopting a two-sided perspective are illustrated in the work of Hetherington and her associates.

Even though nearly half the children being born today will spend some time in a one-parent family, mostly as the result of separation or divorce (Glick, 1978), it would still require an extremely large sample to provide enough cases for a statistically adequate longitudinal study of the changes taking place in a family as it shifts from a three- to a two-person structure. Much can be learned, however, by observing the course of family life once the divorce has occurred, particularly if concurrent data are obtained for a matched sample of intact families. This was the strategy employed by Hetherington and her colleagues (Hetherington, Cox, and Cox, 1976, 1978) in a follow-up study of forty-eight recently divorced middle class parents in cases where custody had been granted to the mother. Divorced parents were identified and contacted through court records and lawyers. A comparison group of two-parent families was selected from a similar socioeconomic background and on the basis of having a child of the same sex, age, and birth order in the same nursery school as a child from a divorced family. In addition, an attempt was made to match parents with regard to age, education, and length of marriage. Only first- and second-born children were used in the study. The fact that both groups of families came from middle class backgrounds allowed the investigators to avoid a frequent source of confounding in studies of single-parent families, almost half of which (44 percent) have incomes below the poverty line (U.S. Bureau of the Census, 1977). The research procedures employed involved a wide variety of methods including parent interviews, observations of the parents and child interacting in the laboratory and home and of children's behavior

in the nursery school, as well as checklists and ratings of child behavior provided by both parents and teachers. Measures were administered at two months, one year, and two years following the divorce.

In keeping with proposition C, developmental changes were found not only in the children but in the parents as well. Initially it was the fathers who were the hardest hit by the experience of separation. Feeling anxious, insecure, and inadequate, they engaged in a desperate search for a new identity in a variety of activities. But within a year the crisis had abated, primarily because they had established a new heterosexual relationship. The problems experienced by the mothers and the children had a longer course and were not so readily resolved. The following composite picture emerges from the rich and diversified data reported in the study.

Placed in the unaccustomed position of the family head, the mother often finds it necessary, because of her reduced financial situation, to look for work or a more remunerative job than her present one. At the same time, she must care for the house and children, not to mention create a new personal life for herself. The result is a vicious circle. The children, in the absence of a father, demand more attention, but the mother has other tasks that must be attended to. In response the children become more demanding. The data reveal that, in comparison with youngsters from intact families, the children of divorce are less likely to respond to the mother's requests. Nor does it make it any easier for her that similar requests are complied with when made by the divorced father. Even when the child is responsive to her, the divorced mother is less apt to acknowledge or reward the action. In the words of the authors,

> Divorced parents made fewer maturity demands, communicated less well, tended to be less affectionate, and showed marked inconsistency in discipline and control of their children in comparison to married parents. Poor parenting was most apparent when divorced parents, particularly divorced mothers, interacted with their sons. Divorced parents communicated less, were less consistent, and used more negative sanctions with sons than with daughters. (1978, p. 163)

In keeping with the principle of reciprocity, the same pattern is mirrored in the behavior of the children toward their parents.

> After reviewing the . . . findings one might be prone to state that disruptions in children's behavior following divorce are attributable to emo-

tional disturbance in the divorced parents and poor parenting especially by mothers of boys. However, before we point a condemning finger at these parents, especially the divorced mothers who face the day to day problems of childrearing, let us look at the children . . . children of divorced parents exhibited more negative behavior than do children of intact families . . . These behaviors were most marked in boys and had largely disappeared in girls two years after divorce. Such behaviors were also significantly declining in the boys. Children exhibited more negative behavior with their mothers than with their fathers; this was especially true with sons of divorced parents.

The divorced mother was harassed by her children, especially her sons. In comparison with fathers and with mothers in intact families, the children of the divorced mother did not obey, affiliate, or attend to her in the first year after divorce. They nagged and whined, made more dependency demands, and were more likely to ignore her. (Pp. 169–170)

The disruptive effects of separation on parents, children, and their relations with each other reached their peak one year after the divorce and declined through the second year although the divorced mothers never gained as much control as their married counterparts. But given our criterion of developmental validity, the critical question is that of long-range effects. Is there any evidence that separation and divorce leave their mark on the behavior of the child in other settings and at other times? In a recent review of their own and other research on the development of children in mother-headed families, Hetherington and her colleagues concluded that "children living in mother-headed single-parent homes appear to be at higher risk for disruption in cognitive, emotional, and social development than are children in nuclear families" (Hetherington, Cox, and Cox, 1977, p. 31). The studies reviewed involved samples ranging in age from the early preschool years through adolescence and adulthood and varying widely in socioeconomic background.

Corroborative evidence from a broader and more systematic statistical base appears in findings from the National Survey of Children (Zill, 1978). Using a stratified probability sample of households in the United States containing at least one child in the age range of seven to eleven years, the investigators interviewed the eligible child and the parent who would be most capable of providing information about that child. Data on children of divorce exhibited a consistent pattern that prevailed after statistical control for socioeconomic status as measured by parental education and income. The general findings were summarized as follows:

Divorce significantly increases a child's risk of developing emotional and behavior problems. Children whose parents have been divorced by the

time the child is of grammar school age are twice as likely to need or have gotten psychiatric help as children in intact families. Such children are more likely to have had a seriously disturbing experience, either due to the divorce itself, or to other life circumstances preceding or following the divorce. The minority of children who exhibit aggressive and antisocial behavior at home, in school, or at play, is larger among children of divorce than among children of intact families. Children of divorce are also more likely to feel neglected and rejected by their parents. (P. 53)

It is important to examine in greater detail the particular ways in which developmental disturbance is manifested among children from single-parent families. Relevant information is provided in the review by Hetherington and her colleagues (1977). In the social-emotional area, children from such families were likely to experience difficulties in sex role identification, show lack of self-control, and exhibit antisocial behavior. For boys, disruptions in sex role typing (as manifested by greater dependency, reduced levels of aggressiveness, and lower preference for masculine activities) tended to occur if separation from the father took place before the age of five. Differences were apparent from the preschool years onward, with some evidence of enduring effects through adolescence and young adulthood. For girls, differences did not emerge until adolescence and were concentrated in the area of heterosexual relationships. Women from homes in which the father had been absent had difficulty in establishing satisfactory relations with men. In general both men and women who had grown up in a single-parent family were more likely to experience marital instability than their counterparts from intact families, particularly if the single parent was female and the separation had been caused by divorce rather than death.

A similar pattern was found with respect to problems of self-control and antisocial behavior, with the additional feature that difficulties were considerably more pronounced if the children were male. It is the boy from a divorced home who is more likely to be impulsive, unable to delay immediate gratifications, inconsiderate, aggressive, or delinquent. Hetherington and her colleagues (1976, 1978) see this syndrome as a product of the especially antagonistic mother-son relationship observed in their divorced families.

The same line of interpretation is offered in explanation of the consistently poorer intellectual and academic performance of children, adolescents, and adults—especially males—brought up in homes broken by separation and divorce. Hetherington and her associates view the cognitive impairment as a product of disrupted

socialization processes in the parent-child dyad. Drawing on the findings of their own study, they point out:

> It was found that in divorced families there was a marked breakdown of appropriate and consistent parental control over children, fewer demands for mature independent behavior, and less communication, explanation and reasoning with children. These poor parenting practices were associated with high distractibility, impulsivity, short attention spans and lack of persistence on tasks by the children, which in turn were associated with drops in scores on performance and quantitative tasks and on certain types of problem solving tasks. Problem solving and academic success requires the ability to concentrate and persist. This ability to focus and sustain attention seems more critical in tasks that involve reasoning such as mathematical problem solving than on such things as vocabulary. Hence, the frequently reported quantitative-verbal discrepancy found in children in mother headed families . . . What is being proposed is that poor parental control leads to high distractibility and lack of persistence in children which causes poor problem solving performance. It would seem that the quality as well as the quantity of maternal interaction in single parent families should be considered. (1977, p. 13)

This conclusion is of course nicely in accord with the hypotheses I have offered regarding the conditions most conducive to human development. More precisely, it is the breakdown of these conditions that characterizes the mother-child dyad in divorced families, particularly during the first year after separation. Consistent with hypotheses 4 through 7, one sees the especially powerful disruptive impact on development of a mutually antagonistic primary dyad as the level of reciprocity diminishes, the intensity of negative interpersonal feelings increases, and the balance of power, instead of shifting gradually toward the child, becomes in the words of the mothers themselves a "declared war," a "struggle for survival," or "like getting bitten to death by ducks" (1976, p. 425).

It is regrettable that Hetherington and her colleagues do not provide an equally full account and analysis of the mother-child relation in two-parent families. The report contains no verbal descriptions or concrete examples for this group. One can only infer from the text and tables that mother-child dyads embedded in a three-person family system were characterized by a more effective socialization pattern. There was better communication between parent and child, and the mothers engaged in more explanation and reasoning, made more frequent demands for mature, independent behavior, showed greater consistency in discipline, and were

more affectionate with their children. The youngsters themselves correspondingly exhibited more self-control, less antisocial behavior, a clearer sexual identity, more consideration for others, a greater capacity to defer gratification, and higher levels of intellectual and academic performance.

If these inferences are correct, they indicate second-order effects of impressive scope and consequence. It would appear that the presence of an adult with whom the mother has a positive relationship enables her to function more effectively in interactions with her child. Conversely, mutual antagonism in the husband-wife dyad, culminating in separation, disrupts the functioning of the mother-child dyad and impairs its capacity to serve as a context of effective socialization.

The impact of a third party on the functioning of an embedded dyad can be generalized in the form of a hypothesis that defines a key process and distinctive property of $N + 2$ systems.

HYPOTHESIS 8

The capacity of a dyad to function effectively as a context of development depends on the existence and nature of other dyadic relationships with third parties. The developmental potential of the original dyad is enhanced to the extent that each of these external dyads involves mutually positive feelings and the third parties are supportive of the developmental activities carried on in the original dyad. Conversely, the developmental potential of the dyad is impaired to the extent that each of the external dyads involves mutual antagonism or the third parties discourage or interfere with the developmental activities carried on in the original dyad.

The investigation of this hypothesis clearly requires the application of what is minimally a three-person model. Although Hetherington and her colleagues did not use such a model for analyzing the group in their sample to whom the triadic paradigm is most easily applied—two-parent families—, in their discussion of interpersonal relations in divorced families, they provide some elegant examples of how one dyad in a three-person system can be affected by the other two. Moreover, these examples are nicely in accord with directional processes stipulated in hypothesis 8.

The illustrations appear in the context of an examination by the investigators of exceptions to the general finding that there is disturbed psychological functioning among children from divorced families. The absence of such disturbance in the behavior of the

child was associated with certain positive features in the mother-child dyad. For example, "children of divorced mothers who were available, who maintained firm but sensitive discipline, and encouraged independent mature behavior showed no cognitive deficits" (1977, p. 13). The investigators then raised the question of what factors might account for the capacity of these mothers to function effectively in dealing with their children. The most critical influence in this regard turned out to be the behavior of the divorced father and the relationship between the divorced parents:

> Effectiveness in dealing with the child is related to support in child rearing from the spouse and agreement with the spouse in disciplining the child . . . When there was agreement in child rearing, a positive attitude toward the spouse, low conflict between the divorced parents, and when the father was emotionally mature . . . frequency of father's contact with the child was associated with more positive mother-child interactions and with more positive adjustment of the child. When there was disagreement and inconsistency in attitudes toward the child, and conflict and ill will between the divorced parents or when the father was poorly adjusted, frequent visitation was associated with poor mother-child functioning and disruptions in the children's behavior. (1976, pp. 425–426)

A similar influence on the effectiveness of the mother-child dyad was exerted by other third parties, but none of these was as potent as the primary relationship involving the father.

> Other support systems such as that of grandparents, brothers and sisters, close friends, especially other divorced friends or male friends with whom there was an intimate relationship, or a competent housekeeper also were related to the mother's effectiveness in interacting with the child in divorced but not in intact families. However, none of these support systems were as salient as a continued, positive, mutually supportive relationship of the divorced couple and continued involvement of the father with the child. (P. 426)

This line of analysis leads the authors to a provocative conclusion. Having reviewed the research evidence on the problems experienced by divorced families, the processes involved, and their disruptive effects on the children growing up in these families, Hetherington and her associates address the crucial issue of causal factors, and arrive at what is essentially an ecological interpretation.

> These developmental disruptions do not seem to be attributable mainly to father absence but to stresses and a lack of support systems that result in changed family functioning for the single mother and her children

> . . . An increasing number of children are going to grow up in single-parent mother-headed families.
>
> It is critical to develop social policies and intervention procedures that will reduce stresses and develop new support systems for single-parent families in order to offer these families [a] more constructive and fulfilling life style. (1977, pp. 31–32)

As the work of Hetherington and her coworkers demonstrates, looking beyond the mother-child dyad and applying an $N + 2$ model to the analysis of the family as a system inevitably directs the investigator's attention beyond relations in the immediate setting containing the child, or what we have called the microsystem, to influences emanating from successively more remote levels of the external environment—in our terms, the meso-, exo-, and macrosystems. Thus the capacity of the mother-child dyad to perform its developmental functions is seen to depend on the behavior not only of other members of the household but also of persons from the outside world. Some of these persons (such as a day care worker) interact with the child in other settings (mesosystem); others (such as a friend at work) may associate with the mother and never have contact with the child (exosystem); finally, as the investigators emphasize in their conclusion, the existence and nature of such external stresses and supports are in significant measure determined by the prevailing institutions and belief systems of the larger society (macrosystem). To effect any substantial change in the lives and thereby in the presently impaired psychological development of children from divorced families, it will be necessary to alter these existing institutional and ideological patterns.

I wish here to call attention to yet another signal aspect of Hetherington and her colleagues' outstanding investigation. It has to do neither with theory, substance, nor method but rather with an equally important requirement for effective scientific work—the initiative and resourcefulness of the investigators. This two-year longitudinal study of ninety-six divorced families and matched controls was conducted without any grant support while the principal investigator was carrying a full teaching load and editing a major research journal. Much of the planning was done in a graduate seminar and all the interviews and observations were conducted by student volunteers (E. M. Hetherington, personal communication). This is not to imply that substantial funds are not required for studying development-in-context, but it does demonstrate that original research on the ecology of human development can be carried out by workers who do not have massive financial resources

and a paid staff to assist them. Two of the other studies on $N + 2$ systems discussed above were accomplished by young, individual investigators with only modest financial support (Lamb and Seaver).

In regard to the analysis of $N + 2$ structures within the microsystem, one issue remains to be considered, namely, the particular ways in which a third party can enhance or impair the capacity of a dyad to perform its developmental functions. We have already noted that a mother can serve as a source of security for an infant in relating to a stranger (Ainsworth and Bell, 1970) and as a reinforcer (and possibly a model) for the father in interacting with his newborn child (Parke, 1978). Conversely, the father's positive relation to the mother, especially in her child-rearing role, increases her effectiveness in the care and feeding of the infant (Pederson, 1976), enhances the quality of mother-child interaction (Hetherington, Cox, and Cox, 1976, 1977, 1978) and thereby fosters the child's psychological development (Hetherington, Cox, and Cox; Pederson). Similar positive effects are achieved by encouragement from relatives, neighbors, and friends (Hetherington, Cox, and Cox). Although the systematic evidence is still lacking, it appears likely that such persons can function constructively in a number of ways: serving as confidantes, aides, substitutes, or scapegoats, providing needed information, advice, or material resources, reinforcing initiatives, facilitating the formation of new social relationships, strengthening the power of a second person as a behavior model for the first (as when a mother praises her son when he acts like his father), or, as demonstrated in Seaver's research, creating expectations for how others should behave toward the child.

On the negative side, third parties can become sources of distraction (Lamb, 1976b, 1977, 1978), be perceived as rivals (Baldwin, 1947), or, as so graphically documented in the studies of Hetherington and her colleagues, impair the quality of primary relationships with the child through their own involvement in dyads with the other parent that are characterized by mutual hostility and frustration. A finding from the National Survey on Children (Zill, 1978) is significant in this regard. On indexes of psychological disturbance, there was one group of children that consistently obtained scores almost as high as those found for children of divorce. They were children of parents from intact families who on a three-point scale of marital happiness described their marriages as least happy. These families constituted 3 percent of all two-parent households. Their children were second only to those of divorced couples

in the percentage reported by parents as experiencing psychological problems requiring professional help. Both parents and teachers also described these youngsters as among the more aggressive.

Even though the National Survey data are only cross-sectional and not longitudinal, the fact that children from divorced families typically showed an equal if not slightly higher level of psychological disturbance suggests that the legal separation of the parents did not bring about an improved situation for the child. This sobering result points to what is perhaps the most destructive effect of third parties on the course of human development—the damage produced by their absence. Such absence means the unavailability of someone to function in the constructive roles I have described, as in the case of a teen-age mother with a newborn having no one to whom she can turn for advice, assistance, encouragement, or mere companionship (Furstenberg, 1976).

The issue of the number of persons available as third parties to a given dyad calls attention to another distinctive property of an $N + 2$ system. Whereas the formation of a dyad, as I have defined it, requires that both participants be present in the same place at the same time, patterns of interaction in an $N + 2$ structure can be sequential. Many of the second-order effects described above are operative even though all the parties involved are not interacting simultaneously. The ex-husband, relative, or friend who offers support to the divorced mother in her child rearing role may do so when the child is not actually present. Such a sequential interaction system constitutes what I shall call a *social network*.

Since a minimum of three persons is required for a sequential interaction to take place, social networks are peculiar to $N + 2$ systems. A sequential interpersonal structure in which every member at some point interacts with every other member constitutes a *closed* social network.[3] A structure in which some theoretically possible dyads do not in fact occur is called an *incomplete* social network. Social networks can occur within a single setting, for example, in an office where certain employees are never present at the same time and have to communicate by leaving messages or through third parties. The most common and extensive social networks, however, are those that extend across settings and hence constitute elements of a meso- or exosystem. For this reason, I defer discussion of the properties of social networks, and their significance for development, to later chapters.

Important differences exist, documented by Hetherington and others (Felner et al., 1975; Hetherington, 1972; Hetherington, Cox,

and Cox, 1977; Santrock, 1975; Tuckman and Regan, 1966), between the development of children growing up in families in which the mother was widowed and in which she was divorced. The degree of disturbance, whether in the cognitive, emotional, or social realm, was consistently greater for the latter group than for the former. In assessing the factors contributing to this developmental difference, Hetherington and her associates (1977) point not only to the often continuing acrimony between divorced parents and the mother's anger at being abandoned but also to "the greater social stigma associated with divorce" and to the fact that "widows seem to have more extended support systems . . . than are available to divorcees" (p. 28). Consistent with this finding, results from the National Survey of Children (Zill, 1978) indicate a striking difference in assessments of mental health for divorced as compared with widowed mothers. Whereas feelings of tension and depression are often reported by the former group, "widowed mothers, who are not much better off in terms of either education or income, are surprisingly free from psychological distress" (p. 24).

It would appear that, at least in American society, a single mother left with the care of a young child is treated differently depending on whether the marriage was ended by death or by divorce. As a result, women finding themselves in these positions are subject to different sets of pressures and react accordingly. This phenomenon shows the operation of another critical element of the microsystem —*social role*, which interests us as it functions to stimulate, maintain, and, on occasion, dramatically redirect the course of human development.

5.

Roles as Contexts of Human Development

The interpretations offered in the preceding chapter of the differences found in the behavior of children and mothers from single- as compared with two-parent families were based on the unstated assumption that the observed effects were attributable to the different social positions (married versus divorced) occupied by the mothers and the role expectations associated with these positions. An alternative interpretation exists, one that views the divorced status of the mother as an outcome rather than a cause, the product of personality maladjustment that existed prior to the marriage and led to the intrafamilial conflicts that culminated in legal separation. According to this point of view, behavior problems in the mother-child relationship of the kind documented by Hetherington and her colleagues would have been present before the divorce and hence could not be explained as reflecting the differential impact of a two- as opposed to a three-person system. Moreover, the finding that children whose mothers were widowed rather than divorced showed less psychological disturbance, rather than being viewed as consistent with a role hypothesis could be regarded as being in accord with a personality-oriented interpretation: mothers who divorced were, and had been, maladjusted; those whose husbands died, were not—they were simply victims of fate, unselected in other respects.

The last assertion would be difficult to maintain given the fact that widowed mothers tend to be older and more well-to-do than those who are divorced. Yet recognition of this difference would not demolish the personality-oriented explanation. To do so would require random assignment of future parents to one or another marital status, a prospect that constitutes a perfect example of an experimental manipulation that can never be carried out in modern

civilized societies, for ethical as well as practical reasons. At least one hopes so.

Ethical considerations notwithstanding, it has been possible to make role assignments at random in other kinds of real-life situations, and the results constitute dramatic evidence that placing people in different roles, even in the same setting, can radically influence the kinds of activities and relations in which they engage and thereby presumably alter the course of their development. I say "presumably" because, in keeping with a conventional research model, virtually all the experiments conducted to date are confined to a single setting and a limited period of administration; hence there is no evidence as to the continuity of the experimentally induced changes over place and time, so that the criterion of developmental validity remains unfulfilled. It would be quite unwise to assume, however, that experiences of the nature and intensity occurring in these experiments would not, if continued over a longer interval, have some lasting effect that carries over beyond the research situation.

One other caveat is in order. While not going so far as to determine on a chance basis whether and when a person should marry or divorce, some experiments do create a situation in which persons selected at random are subjected to profoundly disturbing emotional and social experiences of an intensity not anticipated by the scientists who conducted the experiments. As a result, serious questions have been raised about the justifiability of such experiments from the viewpoint of the ethics of science. I not only share some reservations on this score but take the position that the failure to recognize the potential of psychological damage from experiments of this kind derives in part from the limitations of the conventional research model, which fails to look beyond consequences to the individual subject while she is in the research setting. As a result, even the most conscientious investigator can overlook the possibility of effects on the same person in other settings, or on her relation to other people in her life (children, spouse, parents, friends, and so on) and thus on these significant others themselves, even though the research subject may remain unaffected and unaware of this effect. It is therefore conceivable and to be hoped for that, had the original researchers been exposed to and employed an ecological model in the design of their experiments, the potential dangers would have been recognized and avoided.

It is necessary here to clarify what is meant by the concept of role as employed in the present theoretical framework. An ecolog-

ical approach requires some modification of the generally accepted definition of role as "the behavior expected of the occupant of a given position or status" (Sarbin, 1968, p. 546). Whereas this definition does imply a phenomenological frame of reference, it fails to take into account the element of reciprocity central to the systems orientation being developed here and indeed included in the classical formulations of the construct of role by G. H. Mead (1934) and Cottrell (1942). These original conceptions encompass not only expectations about how a person in a given social position is to act toward others but also how others are to act toward that person (thus when a teacher explains, the pupil is expected to pay attention). In terms of microsystem elements, these can be characterized as expectations about reciprocal activities and relations. Accordingly, our definition of role incorporates all these features.

DEFINITION 14

A *role* is a set of activities and relations expected of a person occupying a particular position in society, and of others in relation to that person.

Roles are usually identified by the labels used to designate various social positions in a culture. These are typically differentiated by age, sex, kinship relation, occupation, or social status, although other parameters (such as ethnicity and religion) may also come into play. Operationally, a person's social position and hence her role label can be defined as a reply to the question, "Who is that person?" from the perspective of someone acquainted with both the person and the social context in which the person is located.

Associated with every position in society are *role expectations* about how the holder of the position is to act and how others are to act toward her. These expectations pertain not only to the *content of activities* but also to the *relations* between the two parties, in terms of the dyadic parameters previously outlined: degree of reciprocity, balance of power, and affective relation. The contrasting roles of parent and teacher are examples. Both are expected to provide guidance to the young, who in turn are expected to accept such guidance in a relation characterized by high levels of reciprocity, mutual affection, and a balance of power in favor of the adult. But with parents the degree of reciprocity and mutual affection is presumed to be higher, and parental authority is thought to extend over a broader segment of the child's life than the teacher's, at least in modern Western societies.