

CHAPTER

1



Meeting the Challenge of
Translational Research in
Child Development
Opportunities and Roadblocks

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In this 35th volume of the Minnesota Symposia series we focus on the rewards and challenges of conducting translational research in developmental science. The Institute of Child Development at the University of Minnesota was founded in 1925 along with other child welfare stations on the belief that the scientific study of the child was essential to the promotion of child welfare (Hartup, Johnson, & Weinberg, 2001; Sears, 1975; Senn, 1975). The translation of basic research into practice was at the heart of this “child development movement” in the United States (Senn, 1975). Stokes (1997) labeled this *use-inspired basic research* and asserted the criticality of grounding research in practice in order to meet the needs of society. Further, he believed that the relationship between research and practice should be bidirectional, with each contributing to the knowledge base of the other.

As the six editions of the *Handbook of Child Psychology* (see Carmichael, 1946, 1954; Damon, 1998, 2006; and Mussen, 1970, 1983) and the growing number of journals in the field of developmental psychology attest, during the past century child development researchers have produced tremendous amounts of basic scientific information on the typical progression of perceptual, cognitive, linguistic, social, and emotional development and on the patterns of family and extrafamilial experiences associated with variations in developmental outcomes (Bornstein, 2006; Lamb & Ahnert, 2006). The burgeoning knowledge about developmental processes contributed to the adoption of social policies such as Head Start that were implemented to improve the development and eventual circumstances of children residing in disadvantaged poverty environments (McLoyd, Aikens, & Burton, 2006; for a history of Head Start, see Zigler & Valentine, 1997). Likewise, increased attention was devoted to children who were developing with handicapping conditions, such as those who were suffering from organic or cultural-familial forms of Mental Retardation (Zigler & Balla, 1982) and children whose parents were afflicted with mental disorders (Garnezy & Streitman, 1974). The discovery that even adults with the most serious forms of psychopathology (e.g., Bipolar Disorder, Schizophrenia) (Zigler & Glick, 1986), as well as their offspring, could escape the most dire predictions of the extant developmental theories not only led to reformulations of the existing models (e.g., from *main effect* and *interactional* to *ecological transactional*), but it also led to important research aimed at discovering the processes contributing to resilience (Cicchetti & Garnezy, 1993; Luthar, Cicchetti, & Becker, 2000; Masten, 2001).

Each of the expansions in focus of the field of developmental psychology has conspired to help forge the disciplines of developmental psychopathology and prevention science (Cicchetti, 1984, 1990; Ialongo et al., 2006), whose missions were even more explicitly to develop the scientific basis for improving children's mental health and their academic and social success. In addition, the knowledge gains in child psychology made it possible for developmentalists to embark upon large-scale collaborative studies—such as the National Institute of Child Health and Human Development Early Child Care Research Network—to address a variety of issues of social significance.

In the past several decades, technological advances in genomics and neuroimaging have ushered in opportunities for developmental geneticists, neuroscientists, psychologists, and psychopathologists to contribute to clinical research and to reduce the burden of mental illness (Gottesman & Hanson, 2005). Specifically, the International Hap Map Project, an organization whose goal is to develop a haplotype map of the human genome, has provided the tools for mapping the common patterns of human genetic variation (Crawford & Nickerson, 2005; Insel & Quirion, 2005). We now have an unprecedented opportunity to investigate how genomic variation, in interaction with well-defined environmental pathogens, confers risk for, or resilience to, the development of mental disorders (Moffitt, Caspi, & Rutter, 2006; Rutter, 2006). Likewise, brain imaging research and studies of brain event-related potentials (ERPs) and the resultant increases in spatial and temporal resolution in brain research are making possible functional studies of the neural circuits underlying psychopathology and resilience (Charney, 2004; Cicchetti & Curtis, 2007; Thomas & Cicchetti, 2008).

Although the field of developmental psychology has made a number of important contributions to improving the lives of children and families, we now are immersed in a new era in which heightened emphasis is being paid to the translation of basic research into practical application and treatments. The translational *push* is designed to speed up the translation of basic scientific findings into practical applications. Indeed, over 4 decades ago Martin and Lois Hoffman (1964) advocated greater communication between researchers and social service providers toward the goal of having behavioral science contribute to the social good.

Consistent with the prevailing viewpoint that it is essential to apply findings from basic research to the context of practice, the past two editions of the *Handbook of Child Psychology* have included volumes devoted to “Child Psychology in Practice” (Sigel & Renninger, 1998; Sigel, 2006). Unlike chapters in prior editions that “served as a standard of dispassionate, scientific rigor,” contributors were asked to write reviews that bridged research and practice (Sigel & Renninger, 1998, p. xxii).

In the closing paragraphs to the “Child Psychology in Practice” volume of the *Handbook of Child Psychology*, Sigel (2006) posed the following question: “Should not developmental research offer useful

and meaningful explanations for the course of development and where needed provide approaches for the prevention and amelioration of conditions that may hinder the optimization of the developmental trajectory?" (p. 1022). We echo this question and strongly agree with Sigel that the application of research to practice "requires stretching and/or adapting the root metaphors in which we have been trained so that collaborations between researchers and practitioners are the basis of research and of any application of research to practice" (Sigel, 2006 p. 1022).

The new era of translational research is not only exerting impacts on developmental psychology, it is also affecting all fields of research in the medical, physical, social, and clinical sciences. Furthermore, the push for translational research is closely tied to funding priorities at the major federal agencies that provide financial support for university research. The impetus to conduct translational research in the behavioral sciences has emanated largely from the National Institute of Mental Health (Insel, 2005; Insel & Scolnick, 2006) and was spurred by the recognition of the tremendous individual, social, and economic burden associated with mental illness (National Advisory Mental Health Council, 2000). Hence, the emphasis on translational research at the funding agencies is quickly translating itself into priorities within the academy.

There is an old saying, "If I open the window, then it is a breeze; if someone else opens it, then it's a draft." Some basic researchers who have conducted their work for years with the expectation that someone else would take their findings and translate them into practical applications are experiencing this new era as an unwelcome, and even threatening, *draft*. And there are potential pitfalls and threats to basic research. However, the increased emphasis on translating basic research into application also carries tremendous opportunities for advancing not only the welfare of children and families, but also the health of our field and the next generation of child development researchers. If we meet these challenges well, then advances in our understanding of the basic processes of development should not suffer. Indeed, as many of the chapters in this volume attest, our knowledge of basic processes should thrive in this translational era.

In the following chapter, we will discuss some of what is needed to support a healthy climate for translational research in developmental science. This discussion will include what we can expect to be roadblocks in the process of enhancing a translational climate for developmental research. One of the critical issues that face us is how to organize graduate and post-graduate training to permit students to develop enhanced capacities for participating in multidisciplinary, translational research, without at the same time delivering such diluted training that our students become dilettantes and not experts. Other issues involve questions of when our basic science is solid enough to attempt translational work; the types of partnerships that we need and how these can be encouraged; the role of curiosity-driven basic research in an era of translational effort; and so on. We are aided in this discussion by the authors of the chapters in this volume. Each was asked not only to discuss their research, but also to lift the curtain to reveal some of the underlying issues and quandaries facing the researcher who moves his or her research into a more translational realm.

WHAT IS TRANSLATIONAL RESEARCH?

Despite the current emphasis on translational research, there is little consensus on what constitutes a translational research program. National meetings organized around discussion of the importance of translational research (National Institute of Mental Health, 2005) have involved long discussions of the definition without reaching a clear consensus. Program announcements from various funding agencies tend to emphasize the translation of basic ideas, insights, and discoveries into the treatment or prevention of human disease (National Institute of Neurological Disorders and Stroke, 2005). Likewise, statements from the National Institute of Mental Health emphasize the translation of basic research into improvements in diagnosis, prevention, treatment, and delivery of services for mental illness (Pellmar & Eisenberg, 2000). This would seem to limit translational research to a small subset of the research conducted by developmental researchers. However, we argue that these definitions actually describe the goals of translational research rather than define the universe of what constitutes translational research.

Clearly, the goal of translational research is to move basic findings more rapidly through the pipeline into novel treatments and preventive efforts to reduce or alleviate physical, emotional, and behavioral health problems. It is widely recognized that information must flow in both directions through the pipeline. That is, it is not enough for the basic researchers to hand their findings off to the clinicians and prevention scientists who then develop novel treatments and approaches. The information gleaned from testing those treatments and intervention programs needs to feed back to inform basic research. When basic and applied scientists attend different meetings, read different journals, and use different metrics for evaluating findings, the pipeline remains clogged and information flows poorly, if at all. Information exchange requires integration across different disciplines and subdisciplines so that clinicians, prevention scientists, and basic researchers are all at the actual or metaphorical table (Cicchetti & Toth, 1998, 2006a, b; Shonkoff, 2000; Sigel, 2006; Toth, Manly, & Nilsen, 2008). This, in turn, requires a melding and integration of concepts, metrics, and goals. Developing a common language is often discussed as one of the critical components of successful translational efforts; however, the need for a common language is based on the need to establish multidisciplinary approaches to identifying and solving inherently translational problems. Hence, the concept of multidisciplinary work is often conflated with that of translational research. The true relationship between translational and multidisciplinary research is better described as a Venn diagram. Not all translational efforts require multidisciplinary teams of researchers and not all multidisciplinary research has translational goals. But the two are closely linked. This is especially true for developmental research where effective intervention and treatment often require an understanding of multiple facets of development and the multiple contexts in which children develop. This is why in this volume we have included chapters from researchers who have adopted multidisciplinary programs of research that allow them to move comfortably between basic information to application and back again with greater fluidity.

To foster the translation of basic findings to application along the metaphorical pipeline often requires creating links among multiple points of discovery. Moving from one point to the other can be

inherently translational, even if the work does not involve a novel treatment or intervention or improvement in diagnostic tools. That is, it can be translational if the *end goal* is to get the findings into a form in which their applicability to a human developmental problem can be more readily discerned, and the motivation to achieve that end goal is not simply to satisfy the need to write a few lines in the *Significance* section of a grant. For example, researchers studying the neural basis of fear and anxiety have identified a distributed neural network of which the amygdala often plays a critical role (Davis, Walker, & Lee, 1997). This work has often been conducted first on rats. Identifying a behavior common to humans and rats and dependent, in rats, on an intact neural fear system (Davis, Falls, Campeau, & Kim, 1993); then testing the behavior for its relevance in human fear disorders (Davis et al., 1997), establishing its neural basis in primate models whose development and neural functioning is more similar to our own (Antoniadis, Winslow, Davis, & Amaral, 2007); and then documenting that theoretically identified aspects of experimentally induced early experience may alter the behavior (Sanchez et al., 2005) are all components of translational research, even though they do not involve testing a novel treatment or examining the efficacy of a novel preventive intervention—or even the study of children. Nonetheless, movement through the pipeline is not complete until this basic research makes contact with studies of, in our case, human development and human developmental treatment and intervention research. Once technological advances permit the conduct of such research with humans and if similar results are obtained, then the inclusion of neurobiological and behavioral measures in the design and evaluation of randomized preventive interventions aimed at reducing fearful and anxious symptoms will provide an important opportunity for developmental psychologists and psychopathologists to enhance their understanding of the processes underlying neural and behavioral plasticity (Cicchetti & Gunnar, 2008).

As this example demonstrates, critical aspects of the research needed to enhance our understanding of the development of children's mental and physical health and disorders may often lie in the work of individuals who use animal models. Moving that work into the human domain and closer to treatments and preventive intervention demands

that researchers are not only able to talk across disciplines, but also to talk across species. As there is no animal that evolved to serve as a model for human development, talking effectively across species requires training ourselves and our students to effectively and appropriately apply animal data to problems of human development. The chapter by Pollak (Chapter 6) demonstrates effective use of animal neuroscience data to address critical questions in human development by using those data as a guide to potential neural systems of interest, while not ignoring species differences in the ecology of development.

Likewise, translational research can involve assessing similar behaviors and, as critically, developmental processes in both typically and atypically developing children. The belief that the study of normative developmental processes informs our understanding of pathological development and, conversely, that the study of pathological development informs our understanding of normative development is one of the central tenets of *developmental psychopathology*—an interdisciplinary science that strives to reduce the schisms that so often separate scientific research from the application of knowledge to clinical populations (Cicchetti, 1993; Cicchetti & Toth, 1998). As one of us has previously stated, “. . . all pathology is, strictly speaking, a process. As a process, it is extended through time, and so must be understood in its temporal aspect. Since all pathology may also be conceived of as a disturbance, distortion, or degeneration of normal functioning, it thus follows that, if one would better understand pathology, then one must better understand normal functioning against which pathology is defined” (Cicchetti, 1984, p. 2). Moreover, “. . . the deviations from and distortions of normal development that are seen in pathological processes indicate . . . how normal development may be better studied and understood” (Cicchetti, 1984, p. 4).

Developmental psychopathology has emerged as a discipline through the integration of multiple single disciplines from embryology to genetics, neurophysiology, psychiatry, psychoanalysis, clinical psychology, sociology, and developmental psychology (Cicchetti, 1990). As such, developmental psychopathology provides an example of the synergistic contributions of previously disparate fields that result in the emergence of a new discipline (Cicchetti, 1984). Despite the fact that the bidirectional flow of

information provided by integrating the study of typical and atypical developmental processes is widely acknowledged among developmental psychopathologists, to date the majority of research on the normal–abnormal equation has emphasized the contributions that information on basic normative processes can make in the service of understanding pathology.

One of the chapters in this volume (Saffran, Chapter 5) provides evidence that the joint study of typically and atypically developing children can enhance our understanding of normative processes. In applying principles of statistical learning to understanding processes in language development, Saffran describes elegant studies with typically developing children. More recently, however, she and her colleagues have begun to probe processes of atypical language development using concepts and methods from her basic research. The results reveal processes that, because they are more evident among children with specific language impairments, make more salient issues that apply to typical language learners. Hence, our understanding of atypical development is not only informed by studying typically and atypically developing children in concert, but it also results from atypical learners informing our understanding of normative processes in language learning. Although likely, it remains to be determined whether this knowledge can be used to develop or evaluate novel treatments or to improve diagnosis. Saffran's work, consistent with all work that examines processes using both typically and atypically developing children, makes the point that research that attempts to *translate* our understanding of developmental *processes* between children developing along typical and atypical trajectories is inherently translational.

Such research also underscores the fact that basic research on typically developing children is essential if we are to sustain a healthy translational research climate in our field. This point is also made very clearly in the chapter by Mundy (Chapter 3). Mundy's chapter describes research that is much further along the translational pipeline, as it now includes evidence for novel treatments and approaches. However, as Mundy makes clear, the translational research program he describes would not have been possible were it not for the years of very basic research on the development of social interaction processes in infancy that preceded it. The basic infant work involved seemingly esoteric questions such as when do infants first follow a point and when and why do they spontaneously

attempt to share their emotional experiences with others? This work gave rise to efforts first to apply the methods and findings to the study of children with autism. The findings from this translational work changed the field of autism research and our understanding of core deficits in this disorder. This in turn has led to the development of novel preventive interventions focused on training joint attention behavior and evidence that focusing on joint attention skills improves other aspects of the social behavior of children with autism in ways that focusing on other social deficits (e.g., social play) does not. Mundy's chapter provides a road map for how translational research can proceed, beginning with very basic developmental studies, through translating those findings to atypically developing children, and finally through translating insights from that work into the development and testing of novel treatments.

In sum, it is easier to describe the goals than the precise definition of *translational research* because the process of moving basic information along the path from discovery to the testing of novel treatments and interventions is a bit like the game of telephone. There are multiple points of information exchange, translation is involved at each point so that the information remains accurate, and movement from any point to the next can be *translational research*. But, if the information stops at any point, then we have a problem. Thus, a critical issue for the field to address is how to keep the information flowing. These issues or roadblocks come in two forms: scientific and structural. The former involves issues of conceptualization and how fields go about organizing their modes of information exchange. The latter involves problems inherent within the academy that have to do with disciplinary boundaries, financial constraints, and the rules of judging individuals for promotion, tenure, and compensation. As the latter issues go beyond the bounds of the work addressed in this volume, we will constrain our remarks to the scientific roadblocks that slow translational research.

SCIENTIFIC ROADBLOCKS

The need to develop a common language is frequently discussed as a roadblock to multidisciplinary research. As translational research in developmental science often requires working across disciplines or

subdisciplines, this roadblock also frequently applies to translational research in our science. Terms and concepts develop rich webs of meaning as they emerge and become established within single disciplines or schools of thought. Weaving teams together that include individuals from two or more disciplines in order to approach common translational problems requires educating one another sufficiently so communication and joint problem solving is possible. Depending on the number of disciplines at the table and the number of critical constructs required to address the translational question, this process of mutual education can take considerable time.

However, if the problem of *language* were merely one of explaining constructs, then this roadblock would be relatively easy to surmount. The more intransigent problem that also falls under this heading occurs when modes of judging the validity of theories and hypotheses also must be melded in order for the research team to make progress. The following three differences in philosophies represent common roadblocks in many multidisciplinary translational endeavors: correlation versus experimentation, clinical insight versus objective measures, and elegance versus complexity. Much child development and developmental psychopathology research involves causal interpretations of inherently correlational data. If the translational problem requires bringing researchers trained to disparage findings based *merely* on correlations together with researchers trained to wrest causality from correlational designs, then the viability of the translational effort will require coming to some degree of consensus on judging scientific validity and designing valid research.

A similar chasm exists between those whose research data are weighted toward clinical insights and qualitative data and those who were trained to be skeptical of conclusions based upon such insights and data. This chasm can make it difficult to bring practicing clinicians and researchers together. Clinicians place high value on the richness of clinical insights derived from years of treating patients, while researchers are often trained to discredit conclusions that are not based on quantifiable data. Failure to recognize and overcome these biases can seriously impede translational research efforts. Despite these challenges, we feel confident that successful community clinical, funder, and research partnerships can occur. For example, two theory-driven randomized clinical

trials for maltreated infants and preschoolers indicate that it is possible to create successful evidence-based interventions, even within the complex world of the child welfare system (Cicchetti, Rogosch, & Toth, 2006; Toth et al., 2008).

Finally, different disciplines place varied emphases on conducting elegant, simple studies versus studies that embrace more complexity. There is little doubt now that the fields of genetics and neuroscience are advancing so rapidly that our ability to translate basic developmental research into improvements in diagnosis, treatment, and prevention of mental disorders will require that we work and integrate information across levels of analysis from genes to social context. Researchers conducting investigations at the cellular and molecular level of analysis and those working in the neurosciences, however, have been trained to think in terms of reducing complex problems to their simplest levels in order to design and implement elegant, clean experiments. In contrast, researchers studying social and emotional development have been trained to recognize that reducing problems of human socioemotional functioning into more simple elements often risks developing knowledge that when translated back into the reality of human life may have reduced external validity. When placed on the same research team, those trained to work from the bottom up tend to work to simplify designs and to focus measures, whereas those trained at more molar levels tend to develop research paradigms that add measures and concomitant complexity. Although it is possible to achieve designs that reflect acceptable compromises, it is also possible that these biases will defeat the research endeavor and two designs will emerge that do not have sufficient bridging points to permit a coherent translational outcome.

Overcoming the biases we have outlined, and others that we have not addressed, is more difficult for some researchers than for others. To some extent, whether a researcher is able to look past his or her biases in order to work effectively across disciplinary boundaries is a matter of differences in temperament and personality. However, to a larger extent it is also a matter of training and experience. Researchers who have previous experience successfully working across disciplinary boundaries come to new integrative endeavors with insights that allow them to more rapidly

develop the common language and perspectives needed to achieve results. To the extent that experience and training matter, questions of when this training should begin arise. In the section on training, we will take up the question of whether we need to build teaching the skills and attitudes needed to work across disciplinary boundaries into our graduate curricula.

Another significant roadblock or clog in the translational pipeline arises from highly valued facets of our scientific training (Shonkoff, 2000). Scientists are trained to be cautious. We are trained to identify errors in theory, hypotheses, experimental designs, data analyses, and conclusions. In sum, we are trained to focus on what we do not know as much as or more so than what we do know. We take it as evidence of our scientific merit to recognize the mantra that “more research is needed” is precisely the correct inference to draw from the results of any research endeavor. Furthermore, the more we burrow into any research domain, the more we recognize the fragility of our knowledge. It does not help that in developmental science we have become increasingly aware that patterns of development and our known correlates of these patterns have a nasty habit of changing when we broaden our focus to include both boys and girls, for example, or individuals from different cultural or sub-cultural groups, or individuals growing up in different settings, urban or rural, or in different economic strata of our society. Cautious scientists are quite aware that in many ways they do not yet know enough to pass what they know down the pipeline with surety, let alone to use that information to develop novel treatments or intervention programs.

Scientific caution is necessary for good science, but it slows the translation of basic research into practical applications. None of us want to burden children and families who already may be struggling with the child’s physical, emotional, or behavioral disorder with the demands of a research program based on questionable basic research or theory. Nor do we want to subject these children and families to unwarranted treatments or intervention programs. Discussions of the ethics of both attempting translational research and failing to do so should become a more common part of our discourse. Establishing forums to conduct such discussions that include both basic researchers and those who are

involved in providing treatment and interventions may prove valuable in helping basic researchers overcome their hesitancy to take the next translational steps with their work. The more the basic researcher understands about the treatments and interventions that are in use and their evidence base, the more the basic researcher may be able to discern when what he or she knows is of sufficient certainty to attempt to move it into translation. Fortunately, there are indeed settings, such as centers on developmental psychopathology (Cicchetti, Toth, Nilsen, & Manly, in press) and academic departments in clinical psychology and psychiatry, where research and practice coexist and mutual respect is accorded to members of each discipline. An examination of the structure and philosophy of such settings should prove to be instructive in facilitating similar successful research and practice collaborations.

In his chapter, Shaw (Chapter 8) provides an excellent discussion of the process he and his colleagues went through in deciding that they knew enough about the antecedents of conduct problems to risk translating that knowledge into a preventive intervention. Although Shaw notes that he is a clinician, and thus might seem to be in an excellent position to straddle the translational divide, he found that he needed to form close professional relations with individuals who had extensive histories of working in the prevention field. Their discussions led first to a limited intervention focused on children (primarily African American boys) on whom he had conducted his basic longitudinal research and then later to the melding of research sites and a reasonably cautious *leap of faith* in applying their intervention to both boys and girls and to multiple ethnic groups.

The need for basic researchers and clinicians to share ideas and insights points to a second critical clog or roadblock in the translational pipeline. Although the field of developmental psychopathology emphasizes the need for integration of research on typically and atypically developing children, it is often the case that those who conduct research have little contact with those who treat children with physical, emotional, or behavioral disorders. Nor do researchers investigating basic normative processes typically have training in clinical child psychology, psychiatry, pediatrics, neurology, or hearing, speech, and language disorders. Thus, basic researchers may have only a vague idea that they

are studying a phenomenon that has clinical applicability. Even when they do, they may have little ready access to the patient populations who might benefit from their research. Conversely, pediatricians, child neurologists, child psychiatrists, and other clinicians often lack training in basic research in developmental science and, once in practice, even those that have had this training often have little further contact with basic developmental science researchers. Funding agencies are well aware that this is one of the roadblocks to translational research. Prominent in announcements (RFAs) for translational research networks is the requirement that researchers bring clinicians onto their research teams. The hope of this requirement is that in listening to one another discuss research and cases, the divide between clinical practice and basic research will be overcome. It is unlikely, however, that such minimal efforts will achieve their translational goal.

Instead, as a field it would seem prudent to create more opportunities for researchers studying basic normative processes to learn about clinical problems and issues. We should enhance opportunities for basic normative researchers to discuss their work with colleagues in clinical fields as well. In part to achieve this goal, we need to break down barriers between developmental and clinical programs and establish more programs in developmental psychopathology. But we also need to lower barriers between developmental psychology and programs in speech, hearing, and language disorders, neuroscience, molecular genetics, pediatrics and neonatology, child neurology, and the many other disciplines where basic research on human development may find application. As Saffran (Chapter 5) notes, this is increasingly happening for researchers studying typical and atypical language development. These domains are integrating, common meetings are being held, and researchers are increasingly aware of each other's work. In our own department, barriers between pediatrics and developmental psychology have been lowered through the cross-appointment of a neonatologist who studies the impact of micronutrients on brain and behavioral development. Similarly, the formation of centers that bring together researchers from multiple departments, including those where patients are treated, facilitates the lowering of these barriers.

This last problem points to another roadblock to translational research. Clinical disorders are described as syndromes of symptoms, while basic

research typically focuses on more circumscribed phenomena. *Caseness* is an issue in clinical research; variations along the normal distribution are the focus of basic normative research. Developmental psychopathologists are interested in both categorical and dimensional approaches to disorder. Disorders share common features, such as problems with attention, emotion regulation, and so on. Basic normative researchers and developmental psychopathologists tend to study processes that contribute to these common features. It is often difficult to meld these very different approaches and perspectives. As Grigorenko (Chapter 7) has articulated, it seems possible, however, that the rising interest in endophenotypes as targets of behavior genetics and molecular genetics research on disorders will have a broader, facilitative effect in building bridges between basic developmental research and research on disorders of development.

Endophenotypes and intermediate *phenotypes* are discrete, measurable traits associated with clinical disorders that may be linked to specific genes. These traits or characteristics are typically closer to the behavioral processes studied by basic child development researchers. The chapter by Rothbart and Posner (Chapter 4) provides a prime example. Their work reflects the merging of research on infant temperament with basic research on adult cognition. The point of contact arose when Rothbart began to note stable individual differences in orienting and attentional control in her infant studies and approached Posner with the question, “What is attention?” Their joint research has examined various aspects of attention, including orienting, inhibition of return, and facets of executive attention. These components of attention and attention regulation, in turn, are known to be disturbed in various mental disorders and are increasingly intermediate phenotypes and endophenotypes of genetic studies (Rothbart & Posner, 2006). This has permitted Rothbart and Posner to bring the study of molecular genetics into their research and to develop focused intervention strategies for improving aspects of attentional control in young children (Posner, Rothbart, & Sheese, 2007). While their collaborative work has not yet progressed to the study of individuals with specific disorders, the solid framework they have constructed provides numerous opportunities for such translational efforts. Furthermore, their work has immediate implications for education—another translational domain for the developmental sciences.

A final roadblock to translational research involves a shift in thinking about the role of research participants that may be difficult for some of us. Scientific objectivity distances researchers from the objects of their study. Researchers design, implement, interpret, and disseminate research, while research participants do what researchers request. Both the distance and the sharp differentiation of the roles of *researcher* and *researched* were not only true of our field over most of its history, they were true of all medical and behavioral sciences. However, beginning with the advocacy organization of the AIDS community, demands that the public participate more actively in setting priorities for research and sit at the table when studies are conceived and fielded have grown stronger. Indeed, to a very real extent, the increased push for more rapid translation of basic findings into practice is an outgrowth of patient organization and advocacy. While most basic developmental researchers are unlikely to experience this sea change as more than simply the need to talk about research *participants* rather than *subjects*, for those conducting translational research at points in the pipeline that involve working with target populations this shift in thinking is necessary and large.

In his chapter, Shaw (Chapter 8) provides an excellent discussion of the need to bring participants into the research process not only so that the work makes sense to their implicit theories of child development, but also so that it is tailored to fit their particular concerns. He notes that researchers can gain a good deal from following procedures that therapists have learned in order to motivate individuals to seek and use available therapies. These procedures involve establishing collaborative relationships in which the targets of intervention efforts become partners in the process. Based on these insights and work in the area of motivating alcoholics to seek treatment (e.g., Miller & Rollnick, 2002), Dishion and Kavanagh (2003) designed a motivational interview procedure for parenting intervention work called the Family CheckUp, which invites parents to choose which areas of their family life or child's behavior they would like help in changing. Shaw and his colleagues adapted the Family CheckUp for their preventive intervention work and he describes how it facilitated parents' motivation to take part in their work.

However, bringing participants into the research process can mean more than using techniques to motivate their participation. It can

also mean working closely with representatives of target groups to tailor research questions, dissemination of results, and follow-up work so that the research more directly addresses their needs. For example, the National Institutes of Health has an initiative called Community-Based Participatory Research that partners scientists with agencies and requires research participants (e.g., women who have been incarcerated) to be included. While the Family CheckUp allows parents to choose from a menu of researcher-determined problems, when representatives of target groups sit at the research table, the researchers need to be open to adding or omitting research questions to their work. Conducting translational research does not require that the researcher move to such an extreme participatory-research model; nonetheless, those who have implemented procedures to bring greater representation of their target populations to the research table have found that doing so can improve rather than impede good research.

One of us (Gunnar) has been operating with a board of parent advisors for almost a decade in her work on internationally adopted children and she has hired individuals from the community's adoption agencies in order to establish community links on the front end of her research. While the primary goals of Gunnar's research address basic science questions (e.g., the neurobiological consequences of early deprivation and the capacity of impacted neural and behavioral systems to recover following adoption), working closely with a board of parent advisors and adoption professionals has helped to ensure that the way she addresses her basic science questions contributes to core questions of concern to the families and children and establishes the professional relationships that support dissemination of findings back to adoption professionals.

Translational research conducted at Mt. Hope Family Center highlights the potential for developmental and clinical scientists to export theory and research to the broader community, especially those families involved with child welfare (Cicchetti et al., in press; Toth et al., 2008). This work demonstrates that rigorous science and intervention research can take place within the community, if the time and effort are made in developing mutually supportive partnerships between researchers and community agencies. Mutuality is perhaps the most important ingredient in giving away our developmental knowledge, in that the

implementation and conduct of randomized clinical trials and data collection in the child welfare community cannot occur without a process that benefits all parties. By developing relationships between policy makers and researchers, social policy initiatives also can build upon empirical evidence. Furthermore, because the basic research conducted with maltreating families at Mt. Hope Family Center was designed with policy questions at the forefront, rather than as a post hoc afterthought, a true research-informed policy agenda could be achieved that could benefit the welfare of maltreated children and their families (Cicchetti et al., in press; Toth et al., 2008).

We have undoubtedly not covered all of the conceptual roadblocks in our field that slow the translation of basic research into improvements in diagnostic tools, treatments, or preventive interventions. What we hope we have done, however, is to show how the roadblocks are not only surmountable, but also ways in which surmounting these obstacles may create opportunities to enhance our science. While removing these roadblocks requires that we think differently and go about doing our work differently, the changes needed do not threaten the viability of our basic research endeavors. Rather, making these changes can energize and enrich both our questions about basic processes in human development and our ability to address these basic questions. We hope that future generations of researchers will find this discussion of roadblocks somewhat quaint instead of a discussion of problems with which they are still grappling. Whether or not they do, however, will depend on how we train and equip our current students to meet the translational challenges of their generation.

TRAINING RESEARCHERS IN THE TRANSLATIONAL ERA

The goal of graduate and postdoctoral training is both to develop the next generation of scientists in aggregate and, individually, to protect and enhance each student's ability to find employment and launch his or her own research career. Graduate programs grapple with the twin problems of ensuring that students gain a sufficient working knowledge of the broad domain of the field, while protecting sufficient time for them to be trained to conduct research and become experts in a particular research

area. Every general requirement added that demands coursework or reading outside of their specific area of expertise takes away time they can devote to becoming expert researchers in their own domain. As knowledge in a field accrues, tension between adding requirements to ensure breadth while protecting time to become experts grows. Ultimately, we know that students will not secure the best postdoctoral positions or tenure-track jobs based on their general knowledge of the field. Instead, they will obtain those positions based on their demonstrated expertise and research acumen in a focused set of research questions. This is why generalist training is concentrated in the first year or two of graduate work, with the latter years focusing increasingly on the development of a particular, narrow domain of expertise. Postdoctoral training may be used to broaden students' area of expertise, although it often is used to permit the students to add technical skills that were not available or that they had no time to learn during their graduate training. The net result, often lamented in discussions of how we are going to *push* translational efforts, is that we gear our graduate and postgraduate training precisely to create the narrow expertise that results in clogs in the translational pipeline.

From the student's perspective, the explosion of knowledge in the developmental sciences over the last decades, the increasing integration of genetic and neuroscience research in domains previously dominated by more purely psychological and behavioral approaches, and the rapid advance of complex technologies means that students are torn by decisions about whether to spread themselves broadly and thinly across the many areas they need to know something about, or to meet only the generalist requirements of their program and otherwise burrow deeply into a narrow area of expertise. And even when they choose to narrow their range in areas critical to translational research such as developmental behavioral neuroscience, the sheer number of techniques they feel they need to master is daunting.

Questions about how to imbue graduate and postdoctoral training to increase the number and quality of researchers who can contribute to the flow of information through the translational pipeline needs to be considered in the context of the tensions in graduate training previously outlined. However, it also may be helpful to consider that funding agencies

and universities are encouraging graduate faculty to develop training programs that are implicitly more interdisciplinary, multidisciplinary, and/or translational. Two decades ago, Cicchetti and Toth (1991) urged clinical psychology programs to adopt a developmental psychopathology framework and called for the importance of interdisciplinary training within graduate curricula in developmental and clinical psychology. Recently, the University of Minnesota has established a series of initiatives designed to both foster more interdisciplinary research and enhance interdisciplinary training of graduate and postdoctoral students. The National Institute of Health as part of its Roadmap initiative first offered in 2003 and renewed in 2005 training grant funds under its program, "Training for a New Interdisciplinary Research Workforce." Plans to explicitly train graduate and postdoctoral students in interdisciplinary research and skills are required in these training programs, including the development of interdisciplinary courses and research experiences.

This leads to the question of what skills are needed, how best to encourage the development of those skills, and when in a student's training is it the right time to do so? Is it sufficient, or even a good idea, to require that the student train in the research laboratories of individuals in two related but different disciplines? Will completing coursework for a minor degree in a related discipline be sufficient or wise in all cases? If we want to encourage students to recognize the clinical relevance of their basic research, then should we be encouraging more of them to obtain clinical degrees? Or will the rigors of clinical training detract from the time they need to develop expertise in the multiple disciplines and research skills necessary to contribute to the research required to improve the flow of information in the translational pipeline? If not clinical degrees, would it simply be sufficient to expose them to some clinical coursework or have them participate in clinical rounds, follow-up clinics, and case discussions? If we increase our emphasis on developing facility in multiple domains, then do we risk producing a generation of researchers who have a little knowledge in a lot of areas, but who are not fully experts in any? And is there a way to train students in the attitudes and flexible approaches to problem solving they will need to work effectively with researchers from different research traditions?

There are no ready answers to these questions. However, it seems likely that some of the answers may lie in the attitudes and research activities of the faculty who are involved in students' training. As faculty become more involved in work that moves information along the translational pipeline, students will become involved in this work. Attitudes about the value of basic research and translational efforts will shift. Lectures and reading groups will become imbued with more integrated approaches to the issues and problems in the field. As faculty become involved in integrated research groups organized in centers that reflect multiple disciplines, training opportunities will arise out of these centers. Students will have the opportunity to experience how their disciplinary training narrows their ability to understand the perspectives of researchers trained in other disciplines, and this will provide a platform for establishing attitudes that permit crossdisciplinary discourse before their disciplinary attitudes solidify. Observing their mentors shift between disciplinary-speak and patterns of communication that rise above narrow disciplinary boundaries will provide templates for becoming scientific multilinguists. Thus, while we should worry about how to equip our students to be successful in this translational era, it may be that if we focus our attention on how we as faculty approach this era some of our concerns about training may abate.

CHAPTERS IN THIS VOLUME

We hope that the chapters in this volume will encourage faculty and their students to evaluate how their work can enhance the flow of information in the translational pipeline. For this symposium volume, we chose researchers whose work represented different points in the translational process and we asked them to provide a bit of history of how their research program evolved. Several of the contributors began their careers addressing very basic questions in developmental behavioral science (see Meltzoff, Chapter 2; Rothbart and Posner, Chapter 4; and Saffran, Chapter 5). However, in each case the work has become increasingly translational over time. These authors provide evidence that moving basic research onto a more translational path can enhance not only our ability to use basic science to address issues of diagnosis, treatment, and

prevention of disorders, but it can also enhance our understanding of basic developmental processes. Several authors began their work focused on providing improved understanding of children with clinical disorders (see Mundy, Chapter 3) or at-risk children (see Shaw, Chapter 8); their work has progressed to the point of being used in interventions. Two of our authors (Pollak, Chapter 6, and Grigorenko, Chapter 7) focus their work on integrating across levels of organization including genetic and neurobiology data to address behavioral issues. Their work reveals not only the challenges of understanding development through integrating biological and behavioral information, but it also points to the value of these approaches for understanding the basis of behavioral disorders and potential targets for diagnosis and intervention. Together, the chapters in this volume provide a glimpse of the field of developmental science in transition from one in which research traditions contribute to clogging the translational pipeline to one in which research traditions increasingly encourage a reciprocal flow of information from discovery to improvements in patient care and preventive interventions.

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