

Criteria for Evaluating the Significance of Developmental Research in the 21st Century: Force and Counterforce

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Since its birth approximately 100 years ago, the field of child development has undergone fluctuations in the criteria used to determine which research topics are more or less worthy of study. The purpose of this paper is to identify the forces that influence how developmental research is prioritized and evaluated and how these influences are changing as we enter the new millennium. We do so by considering the developmental researcher in context and suggest that there will be increasing pressure to use new criteria when assessing the significance of 21st century developmental science. We review the three most commonly used forms of research validity – internal, external, and ecological – and then identify new research validities that we believe also are likely to play increasingly important roles in the next millennium. We also argue that many developmental scientists will increasingly be pressured by forces that are external to the traditional research environment and that these forces will shape the ways in which the significance of developmental research is evaluated.

How do developmental researchers choose the topics on which they focus their research? How are the research and science produced by developmental scientists prioritized? The answers to these questions have become more complicated as the subject matter for developmental scientists has broadened and the diversity of populations, contexts, and methods has been extended. The purpose of the present paper is to identify the forces and counterforces that influence how developmental research is evaluated and how these influences are changing as we enter the new millennium.

Since its birth approximately 100 years ago, the field of child development has undergone substantial fluctuations in the criteria used to evaluate which research topics are more or less worthy of study. Indeed, many different forces and counterforces interact—in complementary and conflicting ways—to influence appraisals of the value and importance of developmental research. Ongoing shifts in this assessment process have resulted in variations over time in the research topics that developmentalists choose to study and that garner support and prominence in the field (Cairns, 1998). As we embark on a new millennium, we are likely to see further changes in the criteria used to evaluate the significance of developmental research.

In the new millennium, we will see tremendous discoveries that will unlock some of the mysteries of genetics, disease, and human behavior. It also is likely that public demand and other external forces will pressure developmental scientists to remain focused on issues that have occupied much of our time during the past century—such as the development of antisocial behavior, how to parent and rear competent children, and the factors that contribute to abusive and neglectful relationships. The pursuit of new advances in developmental science may come into conflict with public demand for answers to the questions that they believe are most appropriate for developmental scientists to investigate and are most important for their everyday lives. In response to these tensions, we suggest that, in addition to existing criteria, there will be increasing

pressure to use new criteria for prioritizing 21st century developmental research. We also argue that developmental science will increasingly be pressured by forces that are external to the traditional research environment and that these external forces will shape new ways in which the significance of developmental research is evaluated. In this paper, we review the changes in these evaluation processes that have occurred and speculate about what changes are likely to continue as we move into the next millennium. In doing so, we examine the people who create the science, considering how they develop as scientists, and how they and others evaluate the significance of research agendas and make decisions about the significance of particular research topics.

The Developmental Researcher-in-Context Model

Many people believe that science is driven by its own logic and that it develops according to its own laws in isolation from the world around it (Toffler, 1984). Scientists, however, are embedded within a larger social context, and this context provides the setting within which research is conceived and conducted (Fabes, Martin, & Smith, 1994). Just as late 20th century developmental science moved toward a greater emphasis on understanding people in real life, we must recognize that developmental scientists themselves live and work in real environments and that these contexts influence the scientific choices they make.

The context for developmental scientists is nested in many levels of organization. The development of the scientist and his or her ideas is a function of the forces inherent in these multiple levels. Some of these forces are internal to the scientist and his or her immediate research environment, whereas others are more external. Consequently, multiple forces work at multiple levels over time to influence scientists' choice of research projects. This multi-level organization can be conceptualized using Bronfenbrenner's ecological model (Bronfenbrenner, 1979;

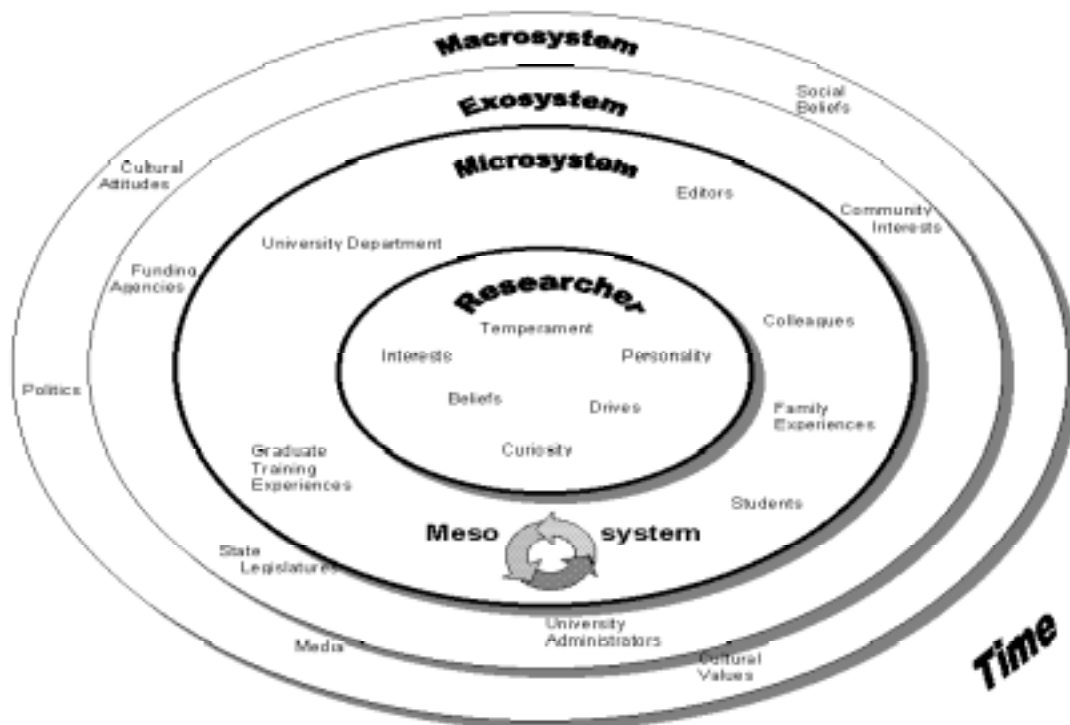


Figure 1: The developmental researcher-in-context model: Late 1900's.

Bronfenbrenner & Morris, 1998). Like Bronfenbrenner's original theory, influences at each of the different contextual levels have the potential to impact scientists' choice of research topics, although they may not all operate at all times for all researchers.

At the *person* level, researchers bring with them personalities, habits, and interests that are formed through biological, social, and environmental pressures (Figure 1). These forces guide their choices and appraisals of research topics. Other relevant person factors for scientists include developmentally generative characteristics such as responsiveness and readiness to defer gratification to pursue long-term goals. In addition, scientific curiosity is an important force in determining and evaluating research interests and topics--a researcher is unlikely to investigate a topic if he or she is uninterested in it.

The most immediate level of context is the *microsystem*. This inner-most sphere of influence involves the interactions of the personal interests and proclivities of the scientist with his or her most immediate environments. Microsystem influences begin early in life and continue throughout the developmental scientist's career. Early family, school, and neighborhood influences play a role in the initial decision to become a scientist and influence the types of questions that a scientist investigates. For instance, a child who grows up in a single-parent family headed by the mother may become interested in studying the influence of family structures on child development. Later family experiences, such as giving birth to a child with a genetic disease, may provide additional research interests. Or, researchers may begin to study the impact of divorce on children after they have

experienced a divorce and have become concerned about the potential consequences it has for the children involved.

Graduate school experiences also color a scientist's research interests. One especially important aspect of graduate training that influences the selection of research topics is training in scientific methods and statistics. Because certain methods often lend themselves to certain questions, the methods and statistics one adopts can influence the choice of topics that one pursues (Carlson, 1986). Moreover, one's academic socialization may provide a certain world view that is difficult to change, although some researchers have grown away from, or grown larger than, their original training. For instance, Bronfenbrenner (1993) discussed his own development from a scientist with a strong psychological bent to one with a more interdisciplinary focus and considered the difficulties that psychologists have in applying his ecological model by saying, "...applications of the model continue to reflect the particular biases and blind spots of each researchers' root discipline (including my own). In other words, no less in developmental science than elsewhere, every investigator inherits the strengths and shortcomings of his or her own scientific socialization" (p. 622).

A researcher's academic department or work setting can influence particular research agendas in a number of ways. One method is direct encouragement and support to the researcher for a line of research. University departments may encourage basic or applied research programs, quantitative or qualitative methodology, individual or collaborative work, or investigation of specific topics. Department values concerning research also become known

to faculty through the hiring, merit, promotion, and tenure process.

The kinds of questions that scientists find intriguing also are influenced by professional relationships. A colleague may encourage a researcher to change or modify an approach or research topic. Teaching experiences and the interests of graduate students can affect the direction a scientist's research takes. Less immediate professional relationships also are important influences, such as those with reviewers and journal editors. Each encounter, relationship, and setting holds the potential to modify, redirect, or change a scientist's thinking and decision-making about developmental science.

At the *mesosystem level*, microsystem level forces interact. A colleague's work may stimulate ideas for students' theses or dissertations, which can result in advances in the researcher's own work. Research agendas also are influenced by changes in professional organizations. A department may alter its research support based on changing views of the field, which may then influence the kinds of research questions an investigator pursues. SRCD's decision to open its pages to applied research has bolstered the status of this type of research (Zigler, 1998). A particularly influential mesosystem force involves the selection of journal editors and reviewers and grant officers and reviewers. This selection process directly influences which manuscripts and grants are published or funded. In turn, these decisions influence the nature and direction of subsequent research.

The third level of context is the *exosystem*, which involves the linkages and processes taking place between two or more broad settings. The events that occur in these interactions indirectly influence processes within the microsystem, thereby influencing the immediate environment of the person (Bronfenbrenner, 1993). In the US, the scientist's exosystem includes the government, community interests, university administrators, and federal funding agencies. An example of an exosystem influence is the history of research on AIDS. Due to the short-sighted views of the US government and public in the 1970s, little funding was initially available for AIDS research (Shilts, 1988). As attitudes about AIDS changed, funding for AIDS research increased.

The fourth level, the *macrosystem* (e.g., cultural attitudes, values, and the media), represents a potentially powerful force on scientists' developmental research agendas. In the US, the most recent notable example of this type of influence and its broad-ranging effects was the shooting of 13 students by two adolescent boys in Littleton, Colorado. Under pressure from the public and the media, the government responded within a month of the shooting by initiating a bill calling for stronger gun control measures (which was defeated by interest group counterforces but was revived after other shootings took place) and increased emphasis on the study of antisocial behavior. Although the study of youth violence has been a topic of interest for some time (see Hawkins, Farrington, & Catalano, 1998), because of macrosystem forces, developmental scientists have received increased pressure to understand this phenomenon.

The Developing Science of Developmental Science

The researcher-in-context model is a heuristic for understanding the changes in the forces that influence the development of scientists. This same model can be used to consider and understand changes in the field of developmental science. For example, in the late 19th century, developmental researchers involved in the Child Study movement focused on the real-world problems of children and studied them in their natural contexts (White, 1996). During this time, macrosystem influences were powerful. The influences on the field of developmental science then shifted away from macrosystem forces as researchers became more convinced of the importance of basic research and experimental methodologies. The main source of influence during these times was the individual researcher and his or her immediate research environment (Webb, 1960), and the boundaries between these levels and the exosystem and microsystem became relatively more impermeable (as depicted in Figure 1; Webb, 1960). For example, Zigler (1998) noted that this was a time when "developmental scientists typically worked in isolation in their laboratories, further insulated from the world by the aura of their academic settings" (p. 532).

In the 1960s and 1970s, with increased community attention paid to civil rights and poverty, developmental researchers engaged in more applied research that addressed practical issues in development, social problems, and public policy (Horowitz & O'Brien, 1989). As a result, community and government funding sources became influential forces, and universities and their faculties were increasingly sensitized to the need to be responsive to the communities in which they live. In the 1980s, as many university administrators moved toward business models of higher education, researchers were increasingly encouraged to pursue socially-relevant research topics that were perceived to be fundable.

Macrosystem forces have increased even more in the past few years. Hillary Rodham Clinton urged the membership of the SRCD to "make the connection between research, public policy, and people's ordinary lives" (SRCD, 1997, p. 1). Developmental researchers have renewed their interests in applying their expertise to improving the lives of children (Denner, Cooper, Lopez, & Dunbar, 1999) and have expanded university-community partnerships (Fabes, Martin, Melmed, & Schneider, 1999). The emergence of the field of applied developmental science, in which the emphasis is on enhancing the human condition by promoting optimal developmental outcomes (Fisher et al., 1993), also has strengthened the connection between policy, research, and the improvement of children's lives.

As the field of child development has progressively become more of a "public science" (Zigler, 1998), the relative importance and impact of person and microsystem level forces have changed. The more distal system forces

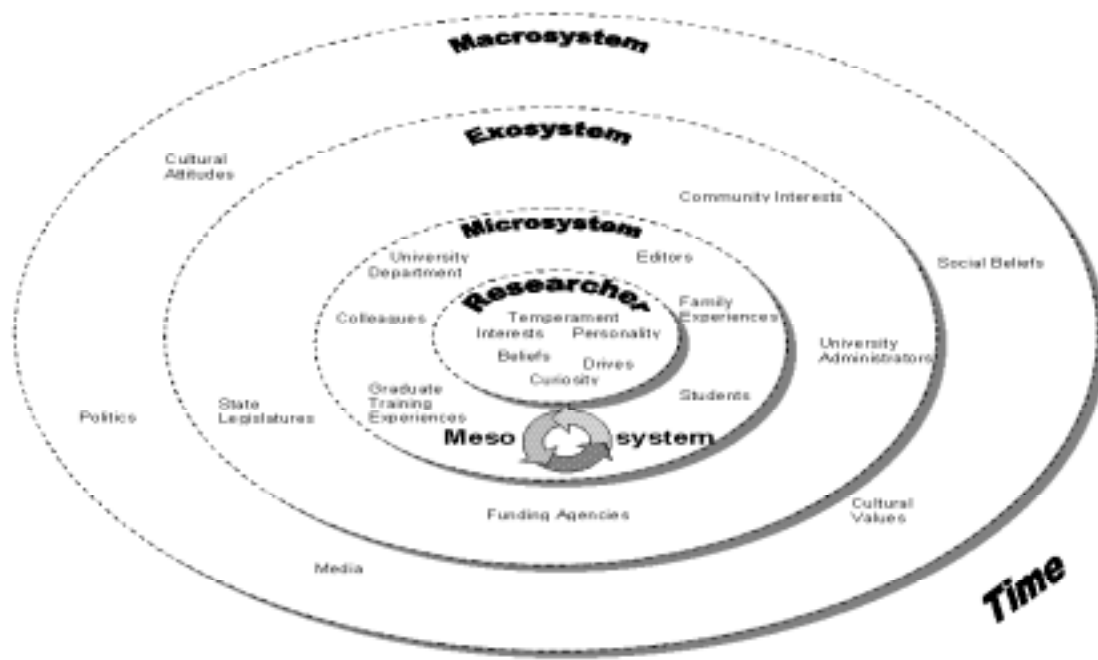


Figure 2: The developmental researcher-in-context model: 21st century.

again have assumed greater prominence and the boundaries between the microsystem and external forces are now more permeable than they once were (as depicted in Figure 2). Thus, developmental science is likely to become an even more applied specialization in the future (Reese, 1993) and “we are likely to be influenced by the increasing demands of the society to apply what we know to human needs.” (Haith, 1993, p. 368). The result is likely to be that we will be increasingly evaluated by the degree to which developmental research is perceived to meet these demands.

Contemporary Criteria for the Evaluation of Developmental Research

The increasing emphasis on contextual influences (Bronfenbrenner, 1979) and greater focus on public interest and awareness (Zigler, 1998) has had implications for the criteria used to evaluate the significance of research topics. This is not surprising given that the sociocultural belief systems of a society have an impact on every aspect of the research process—from the choice of a problem and the research design to the evaluation criteria used to determine the significance of a study (Prilleltensky, 1989). To understand changes in the criteria for evaluating research, we review the three most commonly used forms of research validity—internal, external, and ecological—and then identify new research validities that we believe also are likely to play increasingly important roles in the next millennium.

In the early 1900's, religious beliefs dominated political and social agendas and conversion to Christianity was viewed as an essential step to improving children's development and alleviating societal problems (Youniss, 1990). In subsequent decades, there was a dramatic shift in

the focus of the field of child development toward a more scientific approach and a close alignment with experimental psychology (Morrison, Lord, & Keating, 1984; Super, 1982). This change occurred partly because later researchers viewed the early work as too subjective and too intricately linked to society's religious and political beliefs (Youniss, 1990). Ultimately, this emphasis on a more rigorous scientific approach to studying children's development set the stage for widespread adoption of concepts of validity, particularly internal validity, as a foundation for judging the quality of developmental research.

Campbell and his colleagues (Campbell, 1957; Campbell & Stanley, 1963; Cook & Campbell, 1979) set the criteria for establishing internal validity (i.e., the ability to draw definitive conclusions about the relationship between the independent and dependent variables) and external validity (i.e., the generalizability of the findings to other populations, situations, and settings). Although Campbell and Stanley (1963) argued that “the selection of designs strong in both types of validity was obviously our ideal” (p. 5), they favored experimental designs that were high in internal validity and suggested that young scientists should be trained in the belief that experimentation is the “only available route to cumulative progress” (p. 3). This early emphasis on internal validity was compatible with the overarching goals of developmental researchers to develop the field into one with high scientific standards. In the years that followed, experimental, laboratory-based research dominated the field (McCall, 1977).

In the 1970's, a number of scholars (e.g., Bronfenbrenner, 1977; McCall, 1977) questioned the biases favoring experimental, laboratory-based investigations. McCall (1977), in particular, argued that “the experimental

method now dictates rather than serves the research questions we value, fund, and pursue; as a result the process of development as it naturally transpires in children growing up in actual life circumstances is largely ignored" (p. 334). Similarly, Bronfenbrenner (1977) deemed the current state of developmental sciences as the study of "the strange behavior of children in strange situations with strange adults for the briefest possible moments" (p. 513).

Bronfenbrenner deserves substantial credit for raising awareness about the research questions that would not have been addressed if the field remained rigidly committed to the belief that the experimental approach was the only acceptable method of inquiry (Bronfenbrenner, 1977; 1979). Most pertinent to our discussion is Bronfenbrenner's proposal that the context in which children's experiences occur has important implications for understanding children's reactions and behaviors. During the same period, a series of studies revealed that children's developmental outcomes differed according to the setting in which the study was conducted as well as children's past experiences, skills, and cultural background (e.g., Morrison et al., 1984), further calling into question the validity of a science solely based on laboratory studies. As a result of these trends, the field witnessed a heightened interest in external validity and a new type of validity, ecological validity, appeared (Bronfenbrenner, 1977). Ecological validity was defined as "the extent to which the environment experienced by the subjects in a scientific investigation has the properties it is supposed to or assumed to have by the investigator" (Bronfenbrenner, 1977, p. 516). Similar to the tension between internal and external validity (Mook, 1983), debates have ensued about the conflicts posed by the sometimes competing demands of internal and ecological validity (Banaji & Crowder, 1989).

Not only are researchers faced with weighing the costs and benefits of achieving internal, external, and ecological validity, but there are new validities that will further complicate the decisions faced by 21st century developmental scientists. We predict that the forces that have moved the field in this more applied and contextual direction will be even stronger in the new millennium. We also believe that as the field of developmental science moves toward more applied topics and an emphasis on diversity, its research and scholarship is increasingly evaluated by the factors that have moved the field in this direction.

This change in orientation has both costs and benefits. On the one hand, through its research and science, the field of developmental science has the potential to serve the communities, families, and people of the world. As Lerner (1998) noted, "there is nothing of greater value to society than a science devoted to using its scholarship to improve the life chances of its people" (p. 19). For some, anything less may not constitute adequate science. On the other hand, some would argue that these motivations have little to do with high scientific standards (Reese, 1991, 1993). Thus, some scholars fear that the emphasis on the application of developmental science will compromise the science itself. To date, however, this feared dilution has not occurred (Cairns, 1998). To the contrary, carefully evaluated social applications have created a more robust and relevant science.

This has the potential to change, however, as the forces of influence change in the 21st century.

New Criteria for Evaluating Developmental Research in the 21st Century

In addition to the criteria of internal, external, and ecological validity, we argue that as the field of developmental sciences moves into the 21st century, the forces of change in the new millennium will promote at least four new types of validity that will be used to evaluate the topics and issues developmental researchers address. These four types of validity are different from internal, external and ecological validity in that these new validities focus primarily on evaluation of the *topic* of a piece of developmental research rather than on *how the study is designed and conducted*. These four validities do not necessarily co-occur nor are they equally relevant to different sub-disciplines of developmental science. For example, work on the neural substrates of cognition in infancy and childhood has been dominated by laboratory-based experimentation and advances in technology and methodology (see Mehler & Franck, 1995). Thus, there are unique sources of influence on the topics selected by researchers in the different sub-disciplines of developmental science and these unique influences have their own strengths and weaknesses.

We suggest that the pressures and forces that are reflected in these new types of validity result primarily from influences that exist outside of developmental researchers themselves. These criteria are not meant to be all inclusive—other forms of validity may develop as the field progresses. Additionally, these new criteria may evoke strong reactions from some developmental scientists who believe that these qualities should have little to do with how science should proceed (i.e., counterforces).

Incidence validity. Incidence validity refers to the degree to which a particular piece of research addresses a topic that significantly affects large numbers of people. We argue that developmental researchers will be increasingly pressured to study issues that affect large groups of individuals. When all else is equal, research that lacks incidence validity is likely to be perceived to be of lesser value than are those studies that are believed to have greater incidence validity. Thus, we believe that the utilitarian processes that will exist in the 21st century will direct researchers' attention to issues that affect large numbers of children and families and minimize attention to those that affect smaller, but still significant, numbers of individuals.

Impact validity. Impact validity is defined as the degree to which a research topic is perceived to have serious and possibly enduring consequences for children and families. Social problems typically are defined by the extent of individual or social damage that occurs because of the condition (Parrillo, Stinson, & Stinson, 1985). Those social problems that cause great damage are considered to be the most urgent and serious. We suggest that 21st century developmental researchers will increasingly be pressured to justify their research agendas in terms of impact validity. Those topics that have greater impact validity will be

perceived to be more important than those perceived to have less dire consequences. A consequence of an emphasis on impact validity is that greater value may be placed on applied than basic research because the “impact” of applied research is considered to be greater and more immediate. Although basic research can have considerable impact validity, it is more difficult for people outside of the research context to recognize this. As a result, it will be increasingly important for a developmental researcher who conducts basic research to articulate and communicate the degree to which his or her research program has impact validity.

Sympathetic validity. Sympathetic validity reflects the tendency to judge the significance of developmental research based on the degree to which it generates feelings of sympathy for those individuals who are afflicted by the problem under study. This new type of validity highlights the importance of affect as a factor in influencing judgments about developmental research. We believe that there will be increasing pressure on developmental researchers to study topics that generate feelings of sympathy and concern for those who suffer certain conditions. Generally speaking, greater sympathy is generated for issues that affect a large number of individuals and/or have great impact on their lives. Moreover, groups that likely garner sympathy by having their plight publicized are likely to encourage more research efforts than groups that have not. Similarly, researchers who educate the public about the importance of their topic will garner more sympathy for their research than those who do not. This awareness issue leads to the last of the new validities for assessing the quality of 21st century developmental research.

Salience validity. Because of the larger role of the media and popular press in highlighting important social issues, the pressure to study certain topics has been influenced by heightened public awareness. Thus, we label the final new criterion for judging the significance of research topics in the 21st century salience validity – reflecting the degree to which people are aware of the problem or topic. With greater access to new information, and the greater speed with which the public can be educated about a problem, it is likely that this type of validity will increasingly be a force in influencing the direction and evaluation of new developmental research.

New Validities at Work

The new topic-oriented validities are not necessarily at odds with the traditional design-oriented validities (i.e., internal, external, and ecological validity) and, in fact, they can and do co-exist. In the future, good science will most likely be the type that is published and funded (as it is now). And a lot of good science that is interesting and necessary does not (and will not) meet the four new criteria to the same degree. For example, research on pediatric AIDS (Armistead, Forehand, Steele, & Kotchik, 1998) has high impact validity but low incidence validity, nonetheless developmental scientists consider this type of research important. In contrast, research on otitis media (Peters, Grievink, van Bon, van den Berken, & Schilder, 1997) has a different pattern of validities; it has high incidence validity

and relatively low impact validity but also is considered valuable and important. Furthermore, these new topic-oriented validities do not guarantee that research will be internally or externally valid, and there are times when rigid adherence to these new validities may undermine good science.

Already there is evidence that these new types of validities are influencing the ways in which developmental research agendas are likely to be shaped. For example, in May of 1999, the US Senate approved \$25 million in new money to conduct research on youth violence. The impetus for this funding was the tragedy in Littleton, which shocked Congress and the country about the severe consequences of youth violence (impact validity). Moreover, the fact that the Littleton shootings came on the heels of several other similar events increased the public’s awareness regarding this issue (salience validity). The media coverage of the suffering of the families and students in these communities also added a sense of concern for the protection of youth in our country (sympathetic validity).

There is no doubt that as a result of the public’s responses to incidents like the one in Littleton, there will be more attention devoted to the study and prevention of youth violence. As people around the country have tried to understand how this tragedy could occur and whether it could have been prevented, the national media inundated the public with “experts” who often offered pop psychological explanations, solutions, and finger-pointing. Although the public’s appetite for this kind of information was great, the information that came out after this incident was a stark reminder of just how little we know about the causes of violence and its prevention (APS, 1999). The message that needs to be communicated to the lay public is that more good science is the key to answering the questions about youth violence, but the public’s urgency for a solution to this issue may create the need for quick answers and interventions.

Government funding agencies also are showing evidence of responding to (and sometimes using) these new validities in making decisions about appropriations. The public and its representatives in government have high expectations that science should be directed towards ameliorating or eliminating critical health and social problems. These expectations influence how funding is appropriated and which research topics are considered to be more “significant” than others. For example, in a recent analysis of NIH funding (Gross, Anderson, & Powe, 1999), a significant relation between NIH research funding and measures of burden of disease was found. Disability-adjusted life-years (i.e., the number of years of healthy life lost to a disease) was strongly associated with amount of funding—the greater the number of years lost to a disease the greater the funding. In the past, NIH was criticized for making arbitrary funding decisions that were unrelated to the burden of the disease (Istook, 1997). In response to this, NIH now appears to consider and use impact and incidence validity to a greater extent when making appropriations decisions. We suggest that this will increase in the 21st century for most public funding agencies and that developmental scientists increasingly will be pressured to move their research agendas in line with these new criteria.

The new forces that will be used to assess the significance of developmental research have the potential of creating counterforces. For example, in the natural sciences, many scientists have dedicated their research to issues related to global warming and increased extinction rates. At the same time, some of the scientists who pursued these topics were accused of promoting environmental agendas. Furthermore, these same scientists were accused of causing a backlash about environmental concerns because they interpreted their findings in the most negative light (Budiansky, 1993). In the developmental sciences, as a result of the recent surge in emphasis regarding early brain development (i.e., the President and First Lady hosted The White House Conference on Early Childhood Development and Learning: What New Research on the Brain Tells Us about our Youngest Children, April 17, 1997), developmental scientists are increasingly likely to be pressured by external forces to address and explain complicated issues such as this. There is the potential for the same kind of backlash noted about global warming research in that developmental scientists could be accused of promoting their own "biologically-based" agendas and create a backlash from those who may favor a more "environmentally-friendly" view of development.

Additionally, incidence validity (focusing on topics that affect large numbers of people) will need to be balanced by the need to study minority populations. Thus, in the 21st century, developmental scientists will increasingly walk a fine line between the demands of the complexity and diversity of the science and the public's demand for quick, usable, and understandable information regarding development.

Conclusions

We believe that developmental researchers in the 21st century will be pressured by an array of forces and counterforces that are more extensive than those that existed in most of the 20th century. The pressures that will exist in the 21st century are predicted to move the field to study issues and topics that are more applied, utilitarian, and common. We also argue that these forces and counterforces are increasingly likely to come from sources that are outside the traditional research environment.

The appeal of this applied emphasis is clear: improving the lives of children and families is a strong motivation (i.e., sympathetic validity). Moreover, there are few topics that generate more sympathy and concern than those involving children, youth, and families. Although this type of motivation is laudable, it also is possible that it can lead to programs and research that are superficial, ineffective, and counterproductive. Thus, the desire to make things better does not itself guarantee that one's efforts will be helpful.

There are numerous examples of problems associated with ill-advised and premature interventions and social policies that are not based on solid research. Sound research is complex, costly, and time-consuming, but the external demands for improvement and intervention are unrelenting. For example, the idea that academic performance of students can be improved if schools establish standards and insist that

children meet these standards before moving on to the next grade has generated a great deal of interest and appeal from politicians, administrators, teachers, and parents who are frustrated with student underachievement (Rothstein, 1998). As a result, many federal and state legislators (including President Clinton; Sack, 1999) have taken a hard-line stance to hold students to stricter standards before graduating. Although overall test scores may improve in schools who adopt these strict standards (Grant & Johnson, 1997), students who are held back continue to struggle, are more likely to drop out, and do not fare well once they are promoted (e.g., Meisels & Law, 1993; Nartiello, 1998). Twenty-five years ago, Jackson (1975) concluded that "educators who retain pupils in a grade do so without valid research evidence that such treatment will provide greater benefits to students with academic or adjustment difficulties than will promotion to the next grade" (p. 627). This conclusion has changed little since then, but the enthusiasm for grade retention has been rejuvenated by external forces and seems to be based on an element of incidence validity—namely that the threat of flunking and being held back may motivate the larger general student population. Retention does not, however, have the desired impact on those who are held back.

These external forces (and their counterforces) place a great deal of pressure on developmental scientists to study the issues that the public deems important. Threats to internal validity may occur as we increasingly are forced to quickly turn our research attention to these issues. Developmental scientists will need to juggle rigorous scientific standards and the external demand for application of the science (e.g., Braver & Smith, 1996). As a result, some researchers may be pressured or tempted to forgo traditional aspects of design validity in service of these new validities. We maintain that these new forms of validity cannot and should not preclude good science and the reliance on traditional forms of validity to judge the overall quality of the research. The challenge for 21st century developmental scientists is that we will increasingly need to find ways to balance the changes in how the significance of developmental research is likely to be evaluated with the need for rigorous and internally valid scientific standards.

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