Development and Psychopathology, **15** (2003), 671–690
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Printed in the United States of America
DOI: 10.1017.S0954579403000336

Early experiences and psychological development: Conceptual questions, empirical illustrations, and implications for intervention

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Abstract
This paper reviews alternative conceptual and descriptive models of how early exposure to risk may predict later development. Particular attention is paid to the methodological and data analytic strategies for differentiating among the models. Example empirical illustrations of human studies are then provided that highlight the potential significance of early experiences for behavioral/emotional development. Finally, implications of these models of early experiences for intervention and prevention are examined and identified as an important methodological tool for further research.

Alternative theories have been proposed to account for if and how early experiences shape long-term psychological development. Contemporary writings on the topic indicate that we remain some distance from converging on an empirically based model (see Bruer, 1999; Cacioppo et al., 2002; Clarke & Clarke, 2000; Nelson & Luciana, 2001). Moreover, despite the obvious conceptual and clinical significance of the topic, and in contrast to the very substantial research base on animals, very few studies of humans are designed to be able to address questions concerning the role of early experience per se.

The aim of this paper is to examine key conceptual and methodological issues in the study of early experience. Toward this end, this article first reviews alternative conceptual and descriptive models of how early experience may shape normal and atypical development. Second, conceptual and methodological challenges for differentiating and testing these approaches are considered. Third, example illustrations from the study of early experience in humans are given. Fourth, the implications of alternative models of early experience for treatment and preventive interventions are assessed.

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Conceptual Models of Early Experience

Three distinct approaches are considered in this paper: sensitive periods, experience-adaptive or developmental programming, and cumulative models. Detailed treatments of these approaches are available elsewhere (Bateson & Martin, 1999; Bornstein, 1989; Clarke & Clarke, 2000; Greenough, Black, & Wallace, 1987). This review contrasts these perspectives on early experience according to four dimensions: (a) individual differences versus normative, species-typical development; (b) the nature of the environmental input and the magnitude of environmental effect; (c) the pu-

https://doi.org/10.1017/S0954579403000336 Published online by Cambridge University Press

tative role of psychological mechanisms (and the prominence of human versus animal studies); and (d) resilience and the likelihood of recovery following intervention.

Sensitive periods

"Sensitive period" is a term that describes the finding that, for particular experiences and particular outcomes, the effects vary according to age or developmental phase. Of special interest for this review is the possibility that there are sensitive periods early in development that shape long-term psychological outcomes.

The notion of a sensitive period is somewhat hard to define, does not constitute a clear theory or model, and has been defined differently by different investigators. This likely explains part of the current debate and confusion, especially concerning human development. One example of a sensitive period has been discussed by Greenough as an "experience-expectant" model of development (Greenough et al., 1987), in which the organism requires particular kind of environmental input for normal development to proceed. If the expectant environmental input is not provided during the sensitive period window, then normal development does not occur. This is the version of the sensitive period that is adopted in this paper. There are, however, other ways in which a sensitive period has been operationalized. For example, sensitive periods are incorporated (in a somewhat different form) in other models of early experience, such as developmental programming (see below). There are further complications in defining what a sensitive period model is. For instance, although sensitive periods are typically associated with early development, they may also occur in later life, as in the case of parenting or bonding to a newborn offspring (Fleming, O'Day, & Kraemer, 1999). In addition, different examples of sensitive periods imply different mechanisms, as in the case of parent–offspring attachment or visual acuity. In summary, in general terms, the sensitive period notion is descriptive rather than explanatory and makes no statement about the underlying mechanisms. Nonetheless, because the concept of sensitive period has been extensively examined and there is a core set of expectations that derive from it, or at least one version of it (Bornstein, 1989; Greenough et al., 1987), it is a starting point for considering the effects of early experience on psychological development.

Individual differences versus normative development

A central feature of the sensitive period model is a concern with species-typical development; the significance of individual differences among individuals within the normal (or even modestly abnormal) range is minimal. This is apparent from research on bird song (e.g., Thorpe, 1963), vision (Hubel & Wiesel, 1970), imprinting, and social attachments (Harlow & Suomi, 1970; Insel, 1997), among others. In these studies the outcomes of interest, which are invariably critical for species survival and adaptation, are defined in terms of the presence or absence of grossly pathological development. One reason for this emphasis on species-typical development is that this research is concerned with elucidating core principles about the developing organism.

Nature of the environmental input

Research into sensitive periods or the experience-expectant model examines what kind of environmental input ("experience") is necessary for normal development to proceed; there is much less interest in assessing gradients of environmental input spanning deprived to optimal conditions. This is a corollary of the focus on species-typical behavior, as noted above. Thus, for example, most of the better known examples of sensitive periods exposed animals to obviously pathogenic environments rather than to gradients across the spectrum of environmental exposure. That approach to studying the nature of environmental input may be partly explained by the early interest in assessing the extent to which putatively "instinctual behavior" required *any* environmental input. According to this approach, the function relating environmental condition to

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outcome is a threshold function rather than a linear, dose–response pattern.

Despite the little interest in variation in the environment, the sensitive period model predicts a very substantial environmental "effect." In fact, according to the experience-expectant model, normal development *does not proceed* without certain environmental input. Thus, most research on sensitive periods places strong emphasis on the innateness of biobehavioral development, but similarly emphasizes that particular forms of exposure/experiences play a causal role in normal development.

The role of psychological mechanisms and the prominence of human versus animal studies

Studies testing sensitive period hypotheses of behavioral development are based almost exclusively in animal experimentation (Bornstein, 1989). To the extent that sensitive periods are invoked in human development, the assessed outcomes are very restricted, typically to language (e.g., development of accent in late language use and second language, e.g., Johnson & Newport, 1989) or vision (e.g., the need for early correction of cataracts; see Maurer, Lewis, Brent, & Levin, 1999). An untested implication of the prominence of animal research and the very limited human data is that the more commonly assessed psychological mechanisms (e.g., cognitive style) may not mediate the long-term impact of early experience on long-term development. This is not to say that complex psychological mechanisms are not observable in very young children. Instead, the notion is that these early psychological processes may not be important for explaining sensitive period effects on social and cognitive outcomes, if they exist.

An important addendum to this conclusion is that exposure to risks very early in life may predict psychological/behavioral outcomes in adulthood. One such example is the finding that rubella infection during the first trimester predicts an increased incidence of autism spectrum disorders (Chess, 1977). Risk exposure in the fetal period may predict other psychological constructs, including schizophrenia and depression (e.g., Brown, van Os, Driessens, Hoek, & Susser, 2000; Cicchetti & Walker, 2003).

Likelihood of recovery or resilience

If a sensitive period for a particular outcome did exist, then interventions would need to be delivered during the "window of sensitivity." Interventions delivered outside this period would be expected to have much less, if any, effect. Thus, the timing of the intervention is the key factor. Neither the duration nor the intensity of the intervention is a particularly relevant factor insofar as the organism expects "only" adequate rather than "enriched" input for normal development to proceed, and there is no additional benefit of an intervention that persists beyond the sensitive period.

Furthermore, if there were a failure to receive the necessary expectant environmental input (either naturally or from an intervention), then the strong prediction would be that there would be minimal likelihood of recovery. An alternative expectation is that a particular intervention would have a *different* effect on the long-term development of the organism depending on when it was delivered. Examples of each of these predictions in the area of behavioral development is available, but only in animals. One of the better known examples of the "treatment-resistant" nature of disturbed behavior involving sensitive periods is found in Lorenz's experiments on imprinting. Those animals that imprinted to a different species (notably, Lorenz himself) did not show any signs of imprinting to a member of the same species when introduced after the sensitive period effect, nor did they show any change in their imprinting to Lorenz. In that case, "treatment" to undo disturbed behavior was a failure.

Research on primates has also shown that treatment to remedy the effects of early deprivation on social behavior (see Ramey & Sackett, 2000) are modestly successful, at best, and less so if they are delivered later, rather than earlier, in development (e.g., Martin, Spicer, Lewis, Gluck, & Cork, 1991; see also Ramey & Sackett, 2000). Other studies have

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shown that the effects of early maternal deprivation persist into adulthood to impair mothering behavior, and thus constitutes a risk that is transmitted intergenerationally (although, interestingly, improvements in mothering abilities were detected toward subsequent born offspring without any intervention provided, other than the experience of mothering the firstborn; Ruppenthal, Arling, Harlow, Sackett, & Suomi, 1976).

We do not yet know if there are parallel examples of treatment timing effects in humans, although it is unlikely that we would find the kind of "treatment resistance" that has been found in animal studies (especially involving lower animals). Nonetheless, this possibility is attracting increasing attention in response to the apparent difficulties in successfully treating attachment and behavioral disturbances in a minority of children who experienced gross, severe early deprivation, in the context of either institutional rearing or maltreatment/multiple care placements.

Experience-adaptive or developmental programming

The experience-adaptive or programming model (variously referred to as biological or developmental programming or fetal programming if the period in question is the fetal period) proposes that biological systems adapt to input from the environment during especially sensitive periods of development; furthermore, the malleability of biological systems to adjust to environmental input is developmentally constrained, so that systems that adapt to environments early in life do not readapt to subsequent changes in the environment; as a consequence, the individual is maladapted if there is a substantial change in environmental demands.

One widely replicated example of a programming effect concerns antenatal stress. Several investigators have shown that exposing pregnant animals to uncontrollable stress produces direct and long-term effect on the offspring, which are observable in behavior and physiology and evident into adulthood (Henry, Kabbaj, Simon, LeMoal, & Maccari, 1994; Schneider & Moore, 2000; reviewed by Weinstock, 2001). A central mechanism thought to account for this phenomenon is that the offspring's hypothalamic–pituitary–adrenal (HPA) axis is programmed according to the early exposure to stress hormones in utero. A second paradigm used in research on animals is early postnatal deprivation. Postnatal maternal deprivation also predicts life-long behavioral disturbances and altered stress response. The HPA axis is also thought to be one mechanism mediating disturbances, although in this case the risk derives not from exposure to maternal stress hormones in utero but instead from the offspring's response to environmental stress.

A programming hypothesis in humans has been suggested by epidemiological investigations linking such factors as low birthweight and growth patterns in the early months of life (e.g., rapid catch-up of weight gain) to health outcomes in adulthood, notably coronary heart disease and diabetes (Barker, 1998; Erikkson et al., 1999; Hales & Barker, 1992; Ravelli et al., 1998). In humans, one of the better examples concerns the rates of diabetes/glucose tolerance of individuals reared in nutritionally deprived circumstances in early life. Those individuals who experience a dramatic improvement in the nutritional environment from infancy to adulthood (e.g., because of migration to a richer country) are more likely to develop diabetes later in life. Specifically, the body's capacity to metabolise glucose was "set" according to the nutritional environment in early life; the body is therefore unable to adjust to the dramatic improvement in the nutritional environment (e.g., increase in glucose), and so disease results.

The programming model differs from the sensitive period model outlined above. Whereas both hypothesize that the organism is sensitive to environmental input during a particular period, the experience-expectant sensitive period model assumes that the input (or lack of input) determines whether normal development proceeds; in contrast, the programming model hypothesizes that the individual adapts to the environmental input; whether pathology results depends on how well the individual remains adapted to the environment. In the example above, increased risk for diabetes is thought to result because the individual, after

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migration, is maladapted to the new environmental demands. Had the individual not been exposed to altered environmental demands, pathology would not have resulted.

Individual differences versus normative development

Much of the work on developmental programming seeks to explain normative development. Examples include the programming of the stress response system from in utero exposure to stress hormones (Weinstock, 2001) or the development of the body's "thrifty" manner of metabolism following nutritional deprivation (Hales & Barker, 1992). However, the programming hypothesis does not necessarily imply that differences along a continuum would not have programming effects. This is evident in recent studies examining naturally occurring variation or variation induced from experimental manipulation (Francis & Meaney, 2002). Studies of individual differences within a programming model has been partly limited by the small sample sizes used in animal studies, so they may be more evident in human studies assessing large samples.

Nature of the environmental input

The programming model assumes a substantial role for the early environment on long-term development and a limited set of specified environmental inputs. The degree to which gradients in environmental input predict variation in normal and abnormal functioning is not specified in most studies or reviews, although, as noted, there is growing interest in examining variation within the normal range; hence, this is one feature of the model that is yet to be developed.

The role of psychological mechanisms and the prominence of human versus animal studies

Whereas research on sensitive periods is dominated by animal investigations, research on humans has played a key role in the popularization of the developmental programming hypothesis. However, to a large extent, existing human studies leave open questions of mechanisms, and the mediating role of psychological processes in particular. One of the few studies to explicitly consider psychological processes for possible programming effects found support for a programming pattern on attachment disturbance, but no clear evidence of underlying psychological mediating factors (specifically, cognitive development; Rutter & O'Connor, in press). Furthermore, few, if any, psychological processes are thought to follow a programming pattern. A case in point is the attachment theory hypothesis concerning internal working models of attachment relationships, one of the earliest developed psychological theories to explain continuity in social and emotional development. Far from being resistant to subsequent experience, working models are open to change and responsive to experiences with caregivers, as predicted from theory (Bowlby, 1988) and as demonstrated by empirical research (e.g., Toth, Maughan, Manly, Spagnola, & Cicchetti, 2002). Further research is needed to establish how and if psychological outcomes may be "programmed" and what psychological processes might be implicated in this effect.

Likelihood of recovery or resilience

In contrast to studies on sensitive periods, several studies of programming mechanisms show that the adverse effects of early exposure to risk are reversible with intervention. Thus, the adverse effects of antenatal stress exposure are eliminated by postnatal handling or adoption by a supercompetent parent (Francis, Diorio, Liu, & Meaney, 1999; Maccari et al., 1995) and may also be eliminated by pharmacological intervention (Ladd et al., 2000). That is, in the absence of intervention, the early effects of risk exposure predict adjustment problems into adulthood, but this does not close off opportunities for change. However, because most of these studies delivered an intervention in the early postnatal period, it is not clear if there are periods after which interventions are ineffective.

The implications of the programming hypothesis for interventions with humans are not known. To date, human investigations have

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simply demonstrated that such processes may exist. Whether response to intervention (e.g., for cardiovascular disease) differs according to the presence of a programming origin of the disease requires further research.

Cumulative effects model

A cumulative effects model proposes that early experiences may have lasting effects on the individual only insofar as these effects are reinforced or maintained by subsequent events. According to this approach, early experiences are not, by themselves and in a direct manner, causally connected to long-term outcomes. Instead, the *carrying forward of effects* of early experience on long-term outcomes would be mediated by the risk trajectories or pathways subsequently followed by the individual. The metaphor of developmental pathways (Bowlby, 1988) makes clear that there is not a deterministic, but a probabilistic, connection between early experience and long-term development. Although this is an influential model in developmental research, few studies are well positioned to rigorously test the cumulative effects model because of the strong continuity of risk exposure in most naturalistic or observational studies (i.e., the same difficulty that undermines research into sensitive periods or developmental programming). As a result, a key tenet of the model—that early risk does not predict long-term disturbance in the absence of concurrent risk—often goes untested. A further consideration is that most studies based on the cumulative risk model presume additive effects, that is, the effects of later risks add to the effects of earlier risks: individuals who experience more adversity over time are predicted to show worse outcomes than those who experience fewer adversities or risks (when in development these risks are experienced is not given much emphasis). An alternative version of the additive model is a cumulative effects model proposing interactive effects, that is, the effects of later experiences depend on the history of early risks. This is an equally plausible model with clearly different expectations from the additive model, but interactive models are much less researched and developed.

Individual differences versus normative development

A core feature of the cumulative effects models seek to explain why there is variation among individuals in the developmental pathways followed and how these different pathways account for individual differences in adjustment. Of course, normative development (e.g., transition to early adolescence) is also of interest, but the focus tends to be on explaining why some children have particular difficulty adjusting to normative transitions.

Nature of the environmental input

The role of environmental factors in studies adopting a cumulative effects model is essential, and considerable effort has been dedicated to identifying the kinds of risk experiences that may be associated with individual differences in adjustment across the life span. On the other hand, the magnitude of effect of any particular experience/exposure is thought to be modest. Thus, whereas the sensitive period and programming perspectives hypothesize that certain *specified* environmental inputs are necessary and perhaps even sufficient for shaping long-term development, no such assumption is made in the cumulative effects model. That is, rather than assume a deterministic approach associated with the sensitive period and programming models, the cumulative effect model assumes a probabilistic connection between environmental input and outcome.

The role of psychological mechanisms and the prominence of human versus animal studies

Whereas animal studies are cental to research into sensitive period and developmental programming hypotheses, animal studies are rarely designed to test hypotheses based on a cumulative effects model. This distinction is noteworthy, given that the cumulative effects model is the favored model in many reviews of the literature on early experience and long-term development (e.g., Schaffer, 2000). The tendency to favor a cumulative effects model in psychological research is not to say that there are not several different kinds of com-

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plex psychological processes occurring in the young infant and child (see Fonagy, Gergely, Jurist, & Target, 2002; Jaffee, Beebe, Feldstein, Crown, & Jasnow, 2001) or that they are necessarily unimportant for long-term development. Instead, it is simply the case that studies of this sort have not yet demonstrated that early emerging psychological processes mediate long-term outcomes (e.g., most do not conduct long-term follow-up). It may be that individual differences in early emerging psychological processes lay the groundwork for later psychological processes that are thought to mediate longer term development (e.g., social attributions), but this is not known. In summary, cumulative effects models tend to be based almost exclusively on human studies and, although a role of psychological processes is central to the model, the focus is on psychological processes contemporaneous with adjustment and not those found in infancy and early childhood.

Likelihood of recovery or resilience

The cumulative effects models is distinguished from the other models in emphasizing the role of recovery following early experiences or exposure to adverse early environments. That is, the expectation is that the individual or organism is malleable, and there are comparatively few (if any) limits. Indeed, what the limits of resilience are remains to be specified. So, for example, major life events that occur later in development may be as important—and perhaps more important—as those in early development in shaping life-course patterns and adjustment. Examples of this include enrollment in the military (Elder, 1986) and marriage (Quinton & Rutter, 1988). In the case of intervention, the cumulative effects model proposes that the timing of the intervention is not nearly as important as its duration and intensity, just the opposite to what the sensitive period and programming approaches presume. This theme is expanded on in the final section.

Summary

The above models of the effects of early experience on later development differ widely from one another in several respects and propose strikingly different expectations for the connections between early exposure to risk and long-term development. Nevertheless, this does not necessarily mean that they would be readily distinguishable. Conceptual and methodological considerations for research of this kind are examined in the next section.

Conceptual and Methodological Considerations

As the preceding section illustrates, a range of alternative views have been put forward to explain how, and if, early experiences or exposure to risk causes long-term disturbance. The preceding discussion also indicates that we are some way from differentiating these models in human research. Some of the main reasons for this are addressed in this section.

Methodological and analytic considerations

Slow progress in researching the effects of early experience in humans is attributable to several kinds of methodological limitations. One of the more significant is that few studies assess high-risk children in which the exposure to risk was restricted to the first months or years of life. That is, most of the high-risk environments of interest to developmentalists and clinicians, such as maltreatment or severe poverty, are, in the majority of cases, continuous. As a result it has been difficult to disentangle the effects of early experiences per se from the cumulative effects of continuous adversity. Further complications arise because few studies examine the impact of specific experiences (e.g., deprivation) at different points in development, thus preempting the study of a core feature of the sensitive period hypothesis. That is, whereas studies of children rescued following early deprivation are able to assess the long-term effects of early deprivation per se, they are unable to show that the effects of early deprivation are *different* from the effects resulting from deprivation experienced only later in development. Compounding this situation is the finding that early risk increase the likelihood of exposure to a variety of different kinds of risks (Caspi & Mof-

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fitt, 1995; O'Connor, Thorpe, Dunn, Golding, & the ALSPAC Study Team, 1999; Quinton, Pickles, Maughan, & Rutter, 1993; Rutter, 2000). So, for example, poor parenting in infancy and early childhood is not only likely to be stable from childhood to adulthood, but also increases (directly or indirectly) the risk for poor peer relationships and exposure to deviant peers, academic failure, and behavioral/emotional problems in adolescence; in turn, these risks increase the likelihood that individuals will not adeptly manage the developmental tasks of young adulthood. In short, naturalistic studies reveal how misleading it would be to make direct causal connections between early risk and later outcomes without identifying the mediating risk processes.

The continuity of risk experiences/exposures that is found in most circumstances does not necessarily mean that a life-course, cumulative risk model is "best" for understanding early experiences on long-term developmental outcomes. Instead, the relative lack of studies in which there is marked discontinuity in risk exposure simply means that we are unable to differentiate the alternative models outlined above. So, for example, even if there were a "real" effect of early maltreatment per se on stress response in the young child, it would be difficult to document because the continuity of exposure to maltreatment would diminish the likelihood of finding that early maltreatment made a unique significant prediction to child adjustment independent of concurrent measures of maltreatment (i.e., collinearity).

A related conceptual/methodological consideration is that most of the early risk experiences that might be thought to cause long-term disturbance covary with other risk factors. In the case of experiencing parental divorce in early childhood, for example, other risks such as family conflict, low socioeconomic status, compromised parental sensitivity, and parental mental illness are also evident and confound attempts to isolate divorce as the causal factor explaining long-term problems in child adjustment (Hetherington, Bridges, & Insabella, 1998; Rutter, 1981). Experimental studies in animals are able to reduce the degree to which risks naturally co-occur, although even in these studies it is sometimes difficult to isolate what the particular risk mechanism is (e.g., the effects of separation per se may be compounded by changes in maternal behavior toward that animal induced by the separation).

A third methodological consideration for differentiating the above models concerns the timing and frequency of measurement. That is, if a sensitive period or programming hypothesis is to be tested, then it is necessary to assess the outcome of interest throughout the hypothesized period of sensitivity or programming. However, for most behavioral outcomes of interest we do not have enough information to determine what the window of sensitivity is (if it exists), and there are practical limits in the frequency with which assessments can be conducted without fatiguing subjects or creating method bias due to practice effects. One example that might be open to this approach concerns the links between brain development and cognitive processes. Specifically, sharp periods of discontinuity in brain development (Herschkowitz, Kagan, & Zilles, 1997; Huttenlocher, 1994) may signal sensitive periods to environmental input in the emergence of cognitive capacities. If that were true (and direct evidence remains elusive), then it would be useful to examine if experiences during these periods have particularly strong or different effects on cognitive development than the same experiences at other phases of development.

Another possible area of study in this regard is child–parent attachment. In this case, Bowlby's (1982) ontogenetic model of attachment provides the basis for studying the differential effects of the same risk (e.g., caregiving deprivation, separation) at different stages of attachment. In this regard, Wolkind (1974) found that disinhibition toward strangers, a core attachment behavioral disturbance linked with institutionalization, was associated with *early* institutional care but not institutional care that occurred postinfancy.

There are parallel timing and measurement implications for testing a cumulative model of early risk on later development. Specifically, each junction hypothesized along the risk tra-

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jectory (e.g., Quinton et al., 1993) requires assessment. For example, developmental models of antisocial behavior point to a cascade of events from early childhood that increase the likelihood of eventual antisocial behavior in adolescence (e.g., Dishion, Patterson, Stoolmiller, & Skinner, 1991). In addition to timing, the ordering of major life events (e.g., educational attainment, marriage, childbearing) may also be significant for healthy long-term development (Rindfuss, Swicegood, & Rosenfeld, 1987). The complex patterns of interconnected risks in the course of development means that it is inadequate to assess only early exposure to risk and ignore subsequent exposure to risk.

A fourth concern is that for some features of development (such as vision) there is an empirically based model of early experience but for other domains of development we do not have comparable data on the onset, offset, or biological bases of sensitive periods. This is true even within the same species. The lesson here is that models concerning early experience are likely to be domain specific. Thus, for example, processes governing language acquisition or accent may be irrelevant for socialization outcomes.

A fifth conceptual–methodological consideration is that the mechanisms implied by the alternative models noted above may operate simultaneously and may not be mutually exclusive. This possibility is suggested by the findings reported by Heim and colleagues (Heim et al., 2000). They reported that abuse in early childhood was associated with heightened response to an experimentally induced stress in adulthood. On one hand, this finding is consistent with the programming hypothesis that early exposure to marked adversity initiated a physiological change to the stress response system that persisted into adulthood. On the other hand, an implication of this finding is that this vulnerability to increased stress response would predict psychiatric disturbance such as affective disorder when combined with stressful life events, thereby emphasizing the role of adverse experiences across the life course. (Whether, for the purposes of [preventive] intervention, the stress response per se is the phenotype of interest rather than affective disorder is an interesting possibility suggested by the authors.) The point here is that developmental programming may be observed alongside, and complement rather than contradict, a cumulative effects model connecting risk and adjustment.

Sixth, a further conceptual and methodological complication for studies of early experience is the existence of individual differences in response to adverse environments, even gross deprivation. Individual differences in response to severe deprivation are found in animal studies (see Ramey & Sackett, 2000), but often go unexplained. The significance of individual differences in the subset of severe deprivation in humans is particularly marked. Thus, even among those children who experienced a minimum of 2 years and as long as 3.5 years of global deprivation, cognitive ability at age 6 years ranged from severely delayed to well above average (O'Connor et al., 2000). It is not yet clear how these substantial individual differences can be accounted for within the developmental programming and sensitive period models discussed in this review. The study of individual differences in further research into these models is significant not only for clarifying underlying concepts and identifying plausible mechanisms but also for providing an entry point for considering how genetic factors might regulate individual variation in sensitivity to environmental risk.

Related to the concept of individual differences is the consistent finding that different risks may lead to the same outcome and that the same risk may lead to diverse negative outcomes (Cicchetti & Cohen, 1995). A striking illustration of the latter comes from the study of children adopted into the United Kingdom following institutional deprivation (Rutter, Kreppner et al., 2001). In that study, duration of institutional deprivation was strongly associated with cognitive impairment, severe attachment disturbance, quasiautistic behavior, and inattention/overactivity. Although there was some overlap among these outcomes, some children exhibited specific disturbances of one kind but not another. Why

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institutional deprivation led some children to exhibit quasiautistic behavior but others to exhibit disturbances in inattention/overactivity or attachment instead could not be determined.

A final obstacle in the formation of models of early experience concerns generalizability between, and even within, species. For example, Ramey and Sackett (2000) pointed out that demonstrations of sensitive periods are not as clear in primates as they are in birds and other species; furthermore, this difference between nonprimates and primates is most clearly evident when examining socialization outcomes. Additional caution about the generalizability from animals to humans comes from research showing differential distribution of, and likely differential functional significance of, receptors involved in regulating stress in rodents and primates (see Sanchez, Ladd, & Plotsky, 2001). There are also well-documented behavioral differences among subspecies, again particularly for socialization outcomes (e.g., attachment in Old World vs. New World monkeys; Suomi, 1999). It is interesting that follow-up physiological investigations have identified some of the bases for these behavioral differences. Thus, for example, the remarkably different patterns of pair bonding observed in two subspecies of voles is associated with neuroendocrine and hormonal differences, notably oxytocin (e.g., Insel, 1997).

Empirical Illustrations

The two most widely used paradigms for studying the effects of early experience/exposure in animals are deprivation in the early postnatal period and antenatal stress. These paradigms are much less common in humans; evidence from two of the few existing studies is presented below. These examples are presented because the results challenge existing assumptions and raise important questions for further research into early experiences in humans.

Early institutional deprivation and attachment and social relationships

Early observations on the effects of parental deprivation in humans (Bowlby, 1951) and subsequent experimental studies of deprivation in animals (Ramey & Sackett, 2000; Suomi, 1999) provide a rich source of information concerning the effects of early experience on long-term development. The biological bases of behavioral abnormalities associated with early deprivation has, in turn, influenced how research in humans is currently carried out (Cacioppo et al., 2002; Gunnar, Morison, Chisholm, & Schuder, 2001). As anticipated from the previous discussion, there remain significant differences in how some of this work is conceptualized in humans and animals, but a dialogue between human and animal investigations into the effects of early deprivation is emerging.

The range of institutional settings studied in developmental research varies widely, and this is apparent in reports of Bowlby (1951), Spitz (1946), Provence and Lipton (1962), Anna Freud (Freud & Burlingham, 1973), Tizard and Rees (1975), and the contemporary examples of children in institutions in Romania, China, and Russia. In other words, institutional deprivation covers a heterogeneous mixture of severity and patterns of deprivation, and so it is potentially misleading to refer to a single kind of institutional rearing (Rutter, 1981). One implication is that it may be difficult to generalize from one set of findings to another. On the other hand, despite these difficulties, studies of institutional rearing in humans provide a compelling and provocative—and to some degree even consistent—picture of how early experiences may predict longer term adjustment. Key findings from one such study are reviewed below, which is an ongoing prospective, longitudinal investigation of children adopted into the United Kingdom following institutional deprivation in Romania.

The English and Romanian Adoptee (ERA) Study has followed up a sample of 165 children adopted into the United Kingdom after experiencing institutional deprivation that ranged from the first few months of life to 3.5 years; these children are studied alongside 52 early placed, within-UK adoptees who did not experience institutional deprivation (O'Connor et al., 2000; Rutter et al., 2001). What distinguishes the study is that it provides consider-

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able leverage in assessing the effects of *early* adversity because the children's experience of deprivation was limited to the first months or years of life and they were subsequently adopted into low to normal risk families. The unusual degree of and (relatively) precisely timed discontinuity in severe early exposure to adversity offers a "natural experiment" for studying the longer term effects of early risk that is not confounded with cumulative risk.

Several findings from the study are relevant to questions concerning the effects of early adversity on long-term development (Rutter & O'Connor, in press). Findings on attachment behavior are considered here because this is an outcome that has a long history in research into the effects of early experience.

The ERA study included multiple indices of attachment behavior, including reports of severe disturbances (similar to attachment disorders in *DSM-IV*), observer reports of attachment behavior in a modified separation–reunion paradigm, and observer reports of the child's behavior toward strangers in a play setting (see O'Connor et al., 2003). Findings on maternal reports of severe disturbances (disinhibited behavior according to *DSM-IV* terminology) are most illustrative and are consistent with other investigations into institutional deprivation on attachment disturbance (e.g., Zeanah, Smyke, & Dumitrescu, 2002). A summary of those findings is given here.

First, institutional deprivation predicted *persisting* deficits between age 4 and 6 years. So, for example, longitudinal analyses of data collected at ages 4 and 6 years found considerable stability of individual differences (r = .59, $p < .001$) and no mean decrease in disturbances over time. This pattern of persisting problems is in contrast to other outcomes, for which we observed considerable improvement in the same time period, notably quasi-autistic behavior (Rutter et al., 1999). Second, there was substantial individual variation in attachment outcomes. Thus, whereas some children showed severe impairment according to multiple indicators of attachment disturbance, other children showed no such disturbances and were classified as exhibiting a secure attachment according to the modified separation–reunion procedures (O'Connor et al., 2003). A comparable degree of variation was even observed among the sample of later placed children who experienced at least 2 years and up to 3.5 years of institutional deprivation. Third, individual differences in attachment outcomes and the degree of persistence were not predicted from characteristics in the adoptive home environment (e.g., the parent–child interaction quality observed in the home). Fourth, the effect of duration of institutionalization was strongly associated with attachment disorder related behavior in a dose–response manner. The absence of a threshold effect is an important observation insofar as it contradicts the pattern expected from the animal data linking deprivation and attachment or socialization outcomes, including non-human primates.

What do these findings tell us about the effect of early institutional deprivation on attachment disturbances? In the first instance, the findings are incompatible with the cumulative effects hypothesis. That is, even in the absence of concurrent adversity, the effects of institutionalization on child–parent attachment disturbances persisted in a sizable minority of children (whether that continues to be the case into adolescence is something we are currently investigating). There is, however, an additional consideration in this regard. The ERA study assesses family factors associated with child outcomes in an adopted sample. Given that associations between parenting/home environment characteristics and child outcomes are weaker in adoptive than biological families, finding a comparatively weak association between characteristics of the home environment and child outcomes in further follow-up assessments in the ERA study might simply point to the partial genetic mediation of parenting–child outcome associations.

In summary, the data on severe attachment disturbances through age 6 years in the ERA study challenge existing models of early experience on long-term development, as none is able to handle to combination of wide individual differences, a dose–response pattern of association, and persisting disturbance in the absence of contemporaneous risk. Further follow-up of the sample may provide clues to the

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usefulness of these models or, more likely, the need for alternative models.

Antenatal anxiety and behavioral/emotional development

A second source of data to challenge the cumulative model of early experience on later development comes from research into the effects of antenatal anxiety on children's biobehavioral development. Human studies in this area follow on from a wealth of animal data suggesting strong causal links between antenatal stress and a range of behavioral and neuromotor impairments in the offspring that persist into adulthood (e.g., Henry et al., 1994; Insel, 1990; Thompson, 1957; see reviews by Schneider & Moore, 2000; and Weinstock, 2001). What these reports indicate is that exposing pregnant animals to uncontrollable stress produces direct and long-term effects on behavioral and neuromotor development and physiology in the offspring. Much of this work implicates changes in the HPA axis as one of the likely several potential mechanisms mediating this link. So, for example, mimicking the physiological effects of stress by injection of ACTH has the same effect as exposing pregnant mothers to uncontrollable environmental stress and, moreover, this effect is prevented by adrenalectomy. Although the effects of antenatal stress may be eliminated with sensitive postnatal handling or maternal care (e.g., Maccari et al., 1995), without this form of intervention, the effects persist into adulthood. These data strongly support a programming mechanism whereby fetal exposure to (elevated) stress hormone in utero programs the organism's stress response that then persists into adulthood (in the absence of postnatal "intervention," as noted). Given the substantial animal database, it is natural to ask whether a similar process exists in humans.

One line of investigation suggests that maternal emotional state correlates with fetal behavior, heart rate, and heart rate variability (Di Pietro, Hodgson, Costigan, Hilton, & Johnson, 1996). For example, Di Pietro and colleagues (1996) reported that greater perceived stress in pregnant women was associated with reduced fetal heart rate variability, which may increase the likelihood (or be a marker) of temperamental behavioral inhibition in early childhood. Perhaps the most often reported finding in this area links antenatal stress/anxiety with obstetric outcome, notably preterm delivery (e.g., Hedegaard, Henriksen, Sabroe, & Secher, 1993). In a large (n = 8,719) Danish study reported by Hedegaard et al., self-reported general distress at 30 weeks gestation predicted an increased risk of preterm delivery, defined as less than 37 weeks. Whether the increased risk for prematurity that is associated with antenatal anxiety/stress will exert longer term effects on the child is not clear from these studies.

There is also human evidence that stress/anxiety in pregnancy may alter neurological development. For instance, mothers of children with congenital abnormalities report more stress during pregnancy than controls (e.g., Nimby, Lundberg, Sveger, & McNeil, 1999), although this might be explained by parental suspicions about the child's health from antenatal visits. One of the strongest associations of this kind was recently reported by Hansen and colleagues (Hansen, Lou, & Olsen, 2000), who found that severe life events experienced in the first trimester of pregnancy increased the risk of congenital abnormalities in neural crest derived organs. The finding that the effect was for the first trimester is significant, given that this is when gross organ formation would be most vulnerable.

Several recent reports link antenatal stress/anxiety to cognitive and motor development and early emerging temperament characteristics in infants and young children (Brouwers, van Baar, & Pop, 2001; Huizink, de Medina, Mulder, Visser, & Buitelaar, 2002). Findings from the Avon Longitudinal Study of Parents and Children (ALSPAC), a prospective longitudinal study of women followed since early/middle pregnancy, provide some of the strongest evidence to date that antenatal stress/anxiety predicts behavioral/emotional problems in the child. Specifically, mothers who reported high levels of anxiety in late pregnancy had an approximately twofold increased risk of children with significantly elevated behavioral/emotional problems at 47 months after controlling for the effects of antenatal, obstetric, and psychosocial risks and postnatal anxi-

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ety and depression (O'Connor, Heron, Golding, Beveridge, & Glover, 2002). Follow-up of the children through to 81 months indicated that the effect of antenatal anxiety remained and, moreover, was of comparable magnitude as the earlier outcome assessed nearly 3 years previous (O'Connor, Heron, Golding, Glover, & the ALSPAC Study Team, in press). The persistence of the antenatal effect, despite controlling for multiple postnatal measures of anxiety (which contributed independently to child behavioral/emotional problems), is noteworthy and is consistent with the pattern found in animal investigations. Furthermore, the suggestion that anxiety in late pregnancy (32 weeks' gestation) was particularly predictive of behavioral/emotional problems may be important because it is consistent with the evidence showing that in the human fetus the HPA axis does not become functional until midgestation (Gitau, Fisk, Teixeira, Cameron, & Glover, 2001).

Findings from the ALSPAC study of antenatal stress are consistent with data from animal studies, but nevertheless fall short of confirming a developmental programming model. In particular, research is needed that more directly shows a programming effect on, for example, the stress response system. Intervention designs might also be useful to test the programming hypothesis, although it is not yet clear how the effectiveness (or failure) of interventions would support or reject the programming notion. Nonetheless, the significance of the ALSPAC findings is to raise the possibility of a programming effect in utero, an hypothesis that has been largely neglected in human research.

Summary

Findings reviewed above illustrate how two of the most widely used paradigms to study the long-term effects of early experience in the animal literature raise significant questions for human models of early experience. It is significant that limited available data from both paradigms reject the simple versions of a cumulative effects or sensitive period hypothesis; these data are a better fit to a developmental programming model, although here, too, the fit is far from adequate. Most importantly, in showing a connection between *early* experiences and later development, these findings highlight the need for more concentrated efforts to refine and differentiate existing models of how early experiences might shape long-term development and point to the kinds of research that is needed to advance this field. One such example, studies using an intervention/prevention design, is considered next.

Implications for Prevention and Intervention

This third section examines the implications of research into the effects of early experience on long-term development for prevention and intervention. Two themes stand out. The first is that prevention and intervention designs may be especially useful for differentiating among the alternative views described above. The second is that there is controversy concerning what existing prevention and intervention studies can tell us about the significance of early experience.

Conceptual models and practical implications

Theoretical frameworks for preventive interventions (Coie et al., 1993; Reiss & Price, 1996) emphasize the need for sensitivity to the child's development and the developmental challenges facing the child. Examples of programs developed from these models, such as for conduct disorder (e.g., Conduct Problems Prevention Research Group, 1992, 2002), administer a comprehensive array of specific developmentally based preventive interventions to reduce the risks for conduct disorder identified from a robust volume of studies from childhood and early adolescence. Thus, developmental theory is embedded within these models, but these interventions are not concerned with testing hypotheses about the role of early experience. Similarly, early intervention programs are also based on developmental principles, but these, too, are typically not designed to contrast alternative models of early development. Reviews of this

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literature (e.g., Ramey & Ramey, 1998) emphasize intensity and duration as predictors of effectiveness; whether the timing of the intervention is a predictor of program success is less certain because there are very few (preventive) interventions that compare the effectiveness of interventions delivered early versus later in development. In summary, the extensive database of prevention/intervention studies may be adequate for guiding clinical and policy decisions, but it is inadequate to resolve debates concerning the long-term effects of these early intervention experiences. This important limitation of existing research has kindled controversy (e.g., Bruer, 1999).

To a considerable extent, differentiating the above models of early experience in the context of preventive interventions is a question of timing: does a particular intervention have a stronger effect on a desired outcome than the same or comparable intervention delivered later in development or are there particular windows of development during which an intervention may be most effective?

An essential methodological requirement underlying the timing issue is that the interventions delivered at different periods in development are "comparable." At least in some areas, this criterion may be difficult to achieve. For example, in the area of parent–child relationship quality, effective interventions do exist for infants and toddlers, preschool and school-age children, and adolescents. However, the kinds of interventions developed for these different age groups are based on different theoretical models, and the nature of the interventions differs. For infants and toddlers, parent–child interventions tend to adopt an attachment theory focus; however, for middle childhood and early adolescence, social learning theory is dominant and few attachment-based interventions exist. Whether this is a developmentally sensitive approach to treating the parent–child dyad is debatable, but the consequence is that studies seeking to test a timing hypothesis for parent–child interventions would confound timing with the kind of intervention being delivered.

If timing played a substantial role in determining the effectiveness or efficacy of a prevention or intervention, then it would follow that interventions/preventions delivered after a certain point in development would be unsuccessful. No clear examples of this sort of timing effect are available because few studies are designed to explicitly test the timing hypothesis. Nevertheless, studies including a timing element exist, primarily in the area of early interventions for academic success/school readiness (Campbell & Ramey, 1994; Reynolds, 1994; see also Barnett, 1995). One of the better known examples is the Abecedarian project, a longitudinal study of the effectiveness of an intervention with high-risk children who were randomly assigned to treatment or control conditions (Campbell, Pungello, Johnson, Burchinal, & Ramey, 2002; Campbell & Ramey, 1994). Three treatment groups were compared with the control group. One group of children was given the intervention from infancy through 3 years of schooling; a second group received the preschool treatment only; a third group received only the school-based treatment; these three groups were then compared to a control group. Each of these four groups consisted of approximately 25 children. Findings at age 12 years (Campbell & Ramey, 1994) indicated that the children who received the continuous intervention from infancy scored highest on intellectual and achievement tests; those children who received the preschool intervention only scored somewhat lower, and those children who received the intervention only from school age were still somewhat lower. Each of these intervention groups performed better than the control children, but all groups performed worse than a nonrisk comparison sample.

The finding relevant to the timing issue is that the school-age intervention did not improve cognitive or academic outcomes above the effects of the preschool intervention. That is, what was important for predicting age 12 outcomes was whether the child received the intervention in the preschool period. The further follow-up at age 21 years indicated that the early intervention continued to have long-term effects on cognitive and academic outcomes. However, because those children who received the early intervention received more years than those who received the intervention starting in school, the role of early interven-

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tion per se could not be established, and so the role of early experience in this study remains controversial (Bruer, 1999; Campbell et al., 2001).

Intervention timing is thought to be important in a variety of other contexts (whether or not this is supported by available data). One such example concerns the effectiveness of parenting/family interventions for conduct disorder. Although there is some suggestion that parenting interventions are more effective for children than adolescents, the data are mixed (see Scott, 2002). In the area of cognitive–behavioral therapy for children, there is some evidence that older children respond more positively and that this may be accounted for by the greater sophistication to access cognitive processes that mediate treatment (Durlak, Fuhrman, & Lampman, 1991). A third example concerns intervention for substance use, for which there is a suggestion that programs are most effective if delivered prior to the initiation or experimentation with substances; interventions delivered after that period may be ineffective (e.g., see Dishion et al., 2002).

A separate line of research on mood disorders also suggests timing may be important for intervention and recovery. Evidence from several studies suggests that although first episodes of depressive illness may be linked with psychosocial factors, subsequent episodes are more autonomous from psychosocial factors, and this may be increasingly so with repeated episodes (Post, 1992). The implication is that the initial episode may make the individual more vulnerable to subsequent episodes because of neurobiological changes brought on by the illness (Post, Weiss, & Leverich, 1994). The treatment implication is that the earlier treatment is administered, the less likely the individual is to be neurobiologically sensitized and vulnerable to recurrent episodes. In this case timing is not tied to development but instead to the presumed progression of the disease.

In addition to the above intervention examples, there is also evidence that the effect of a particular risk may be moderated by the age of the child (which age indexes are not clear in most cases). Perhaps the most demonstrated example is the age differences in recovery from brain injury and, in particular, the greater plasticity of transferring function across hemispheres in the less mature brain (e.g., Rutter, Chadwick, & Shaffer, 1983).

One set of findings that may be especially noteworthy for deriving a model of early experience are interventions administered to children who experienced gross early deprivation. One project focuses on the quality of attachment between children and carers in institutions. Zeanah and colleagues (2002; C. H. Zeanah, personal communication, 2002) found that manipulating the care structure by improving the child/staff ratio and limiting the number of staff involved was associated with significant decreases in severely disturbed attachment behavior and increased rates of likely normal patterns of attachment to a primary caregiver. A second experimental intervention study that provided a more stimulating environment (although more akin to a normal than "enriched" environment) within the institution on an intensive basis (6 hr/day, 5 days/week for approximately 1 year) found that children in the treated group showed substantial gains over children who did not receive the intervention (see Ramey & Sackett, 2000). The above studies, which took place in institutions in Romania, demonstrate that altering the institutional environment does have a direct impact on children's social and cognitive outcomes. In this way, these studies are consistent with early studies of institutionalized mentally retarded individuals (e.g., see Clarke, Clarke, & Reiman, 1958) and more recent evidence showing that adoption of cognitive-delayed children into high socioeconomic homes even as "late" as early childhood is associated with clinical and statistically significant increases in IQ (Duyme, Dumaret, & Tomkiewicz, 1999).

Importantly, there was little evidence of an effect of the child's age on the intervention in either Romanian institution study (although neither was sufficiently able to test this hypothesis rigorously). The finding of a recovery following intervention, but one that is incomplete—and particularly so for the children who were deprived the longest/were oldest at the time of the intervention—is consistent with what is known about the development of chil-

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dren adopted from the institutions (O'Connor et al., 2000). Adoption studies, which assess the most intensive sort of intervention, also find considerable catch-up but no differential catch-up. That is, the "amount" of improvement is constant across children who varied in age at which the intervention was delivered; older (or late-adopted) children were no more or less responsive to the change in circumstances than younger (or early-adopted) children. The failure to find an age or timing effect in studies of children who experienced gross institutional deprivation is inconsistent with the strong age effect on somewhat parallel manipulations found in primate studies (see Ramey & Sackett, 2000).

To the extent that the timing of the intervention predicts effectiveness, we need to ask what the mediating mechanisms might be (see Hinshaw, 2002). Treatment and, to a considerable degree, prevention models inherently adopt a cumulative effects model. That is, the assumption is that if an individual exhibits psychopathology or is at high risk for psychopathology, then an intervention would be able to return that individual to a normal developmental "trajectory." Within this perspective, whether the individual returns to normal development may be attributable to the likelihood of receiving adequate intervention for the range of problems exhibited, and not because the intervention was provided "too late" in development. In this regard, it might be that interventions applied earlier in development are more effective because it is easier to bring back to a normal developmental trajectory an individual who has only recently strayed onto a deviant developmental course. A corollary of this approach is that later interventions can be effective, but they would need to be far more extensive than earlier interventions and, at the extreme, might involve inpatient care. In other words, the key association is that between the age of the child and the intensity of the intervention rather than intervention effectiveness. Early interventions might also be preferred because they may inoculate the child against later adversities and could be easier to deliver (e.g., because younger children would be less actively exposed to risks in the peer, school, and community settings; Cowen & Durlak, 2000; Olds, 2002).

Summary

A thesis of this paper is that (preventive) intervention studies will likely play an important role in differentiating alternative hypotheses concerning the role or early experience on long-term adjustment. This view derives, in large part, from the difficulty of naturalistic studies to disentangle early effects from cumulative or concurrent experiences. However, to date, such studies are rare and the small amount of data that do exist are insufficient to test alternative predictions. "Natural experiments" in humans and experimental research on animals are, of course of value, but both have potential limits of generalization that can only be satisfied when supported by experimental interventions.

Conclusions

Given its central significance of understanding the effects of early experience on the development of psychopathology, it is surprising that comparatively few studies are designed to assess this issue directly. The difficulty in addressing this issue is not simply a matter of creating and maintaining long-term longitudinal studies (many such studies exist). Instead, the limited data base currently available to assess the role of early experiences on long-term development in humans is attributable to the methodological problems of distinguishing the early effects per se and the lack of conceptual focus in the role of early experiences. Consequently, we currently have no empirically based theory of how, or to what extent, early experiences shape long-term development and what the implications are for intervention/prevention. In its place is considerable debate and alternative interpretations to the same set of findings.

This paper examined core assumptions underlying alternative conceptual and descriptive models of how early experience/exposure to risk may influence long-term development. A second aim of the article was to draw paral-

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lels and raise questions about the applicability and generalizability of animal findings to humans and the need for greater integration among levels of analysis and research paradigms. In addition, by highlighting the debates concerning the role of early (preventive) interventions, the aim was to identify the usefulness of these designs for differentiating alternative models of how early experiences shape development.

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