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This special briefing has been written to accompany the CPCS event *Monitoring Parents: Science, evidence, experts and the new parenting culture*.

We are very grateful indeed to John T. Bruer, author of the book *The Myth of the First Three Years* for taking the time to do this.

Revisiting "The Myth of the First Three Years"

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Introduction

Since 1986, I have served as president of the James S. McDonnell Foundation, a charitable trust. The foundation has supported research in cognitive psychology, including its applications to education and rehabilitation, helped establish cognitive neuroscience to link psychology with systems neuroscience, and funded international programs in child health, such as the Task Force on Hepatitis-B Immunization and field studies of how chronic parasitic diseases might affect children's cognitive development. In 1998, the foundation also collaborated with the MacArthur Foundation in funding the Research Network on Early Experience and Brain Development. The core of scientists in this network are now associated with the Harvard Center on the Developing Child.

Given this background in funding psychology, neuroscience, and education, I was surprised when in the mid-1990s, I began to hear and read about breakthroughs in brain science which were about to revolutionize early child care and parenting. In reviewing thousands of research proposals, I had seen nothing that might fit this description. When I began reading the 'new' early childhood literature, mostly policy reports, popular books, and news articles, I found that the purported new breakthroughs were in fact 'old' neuroscience. These old results had been carefully selected, over-simplified, and over-generalized and then woven into an argument to support U.S. legislation to fund Early Head Start, a birth to three program for the disadvantaged. The rationale was that Head Start had not been as effective as its early advocates had hoped, because the intervention did not start early enough. Too much brain development had occurred by age three to be permanently changed by a pre-school program.

Neuroscience and the brain have a strong hold on the popular imagination. Once claims that the first three years of life were *the* critical period for brain development appeared on the covers of *Newsweek* and *Time* magazines, upper middle-class parents world-wide became students of the new brain science and consumers of brain based products like *Baby Einstein, My Baby Can Read,* and Mozart CDs. Policy reports appeared and conferences occurred in Canada, New Zealand, Australia, Japan, and the United Kingdom, to name but a few.

The Myth of the First Three Years (Bruer, 1999; hereafter, *The Myth*) presented a review of the relevant neuroscience and a critique of how this research was being used by early childhood advocates. I am not at all opposed to early childhood programs, particularly if there are scientific data to support interventions and to assess their efficacy. However, I do believe that politicians and advocates should be held accountable for the arguments they promulgate in support of their preferred policies. I also believe that scientists should not be complicit in misrepresenting research and should be held to the same standard in speaking to the media or in policy forums as they are when speaking to their scientific colleagues.

In the following section, I summarize the myth of the first three years, discuss its purported scientific basis, and present my criticisms of it. Next I review the scientific community's reaction to my book, a reaction that supports a less enthusiastic and more cautious approach to applying neuroscience to early childhood. The purpose of this review is not to say "I was right and the advocates were wrong", but rather to establish some ideas and principles for ongoing discussion and criticism of the purported neuro-scientific basis of early intervention programs, ideas and principles that are relevant to current U.K. policy papers and to future discussions of these issues.

The Myth, Its Basis and Its Misconceptions

As one tries to assess the merits and demerits of arguments about the impact of early childhood experience on brain development, it is useful to keep two things in mind. First, the original intent of the myth was to argue for programs to help disadvantaged children, not to encourage products and programs to help middle and upper class children gain admission to elite schools. To their credit, the current UK policy papers remain true to the original purpose. Second, the myth had its origin in policy and advocacy circles, not in the scientific community. Certainly, the myth gained credibility through statements some scientists made in the media and in policy reports, but it was not the case that scientists had compelling, new results that they felt obliged to share with the wider public.

If one examines the science cited in policy papers from the mid-1990s through today, one finds that more behavioral science – psychology, psychiatry and sociology – is cited than is neuroscience. Neuroscience was chosen as the scientific vehicle for the public relations campaign to promote early childhood programs more for rhetorical, than scientific reasons. In 1994, the Carnegie Corporation, a U.S. private foundation, had commissioned *Starting Points* (Carnegie Corporation, 1994), a document intended to support the Early Head Start legislation. *Starting Points* alluded to some neuroscientific results which the authors deemed pertinent to early childhood interventions. These allusions were amplified into a neuroscientific basis for an early childhood campaign in *Rethinking the Brain: New Insights into Early Development.* (Shore, 1996) Also in 1996, Sharon Begley, then a journalist at *Newsweek,* wrote an article "Your Child's Brain", which appeared in the Spring/Summer edition of *Newsweek.* The magazine received more reprint requests for this article than any article it had previously published. In summer of 1996, the Carnegie Corporation sponsored a meeting of early childhood advocates with public relations experts experienced in designing national campaigns to reduce drug abuse, child abuse, and smoking. A media campaign emerged that built on the apparent public interest in early brain development. The media experts and advocates concurred that talking about the brain's 'hard-wiring' and synaptic 'circuits' presented a mechanistic image of the mind-brain. This image appealed to both men and women and could be used to frame issues in early childhood development. Talking about the brain and its circuits is much more concrete than talking about the mind and its functions. (The entire background to this campaign is presented in Chapter 2 of *The Myth.*)

The value of a mechanistic basis for, or image of, childhood development had not been lost on researchers and advocates. At a meeting on early childhood and the brain sponsored by the U.S. Department of Education, one researcher told us about work with Latino families. In this researcher's view Latino homes were chaotic and possibly detrimental to children's social and emotional development. However, it was exceedingly difficult to get Latino mothers to change their behavior, until researchers told them that the home environment was raising cortisol levels in their infants' brains, possibly damaging them for life. A brain-based explanation brought about instant compliance. Appreciating the rhetorical value of brain science persists. On the website of the Frameworks Institute (www.frameworksinstitute.org), one of their recommendations to the Harvard Center on the Developing Child is "the importance of moving from a 'mentalist' communications perspective to a 'materialist' perspective. The former focuses on subjective, abstract mental experiences (thoughts, feelings, emotionality, willfulness) while the latter emphasizes the physical changes that take place in a child's brain (pruning, circuits, hormones, chemicals)." As they continue, "The mentalist perspective does not include the important notion of a "damaged system." The value of this mechanistic image is illustrated, again with cortisol as an example in (Shonkoff & Bales, 2011):

> For example, one focus group participant who was exposed to the model and then asked to explain the physiological consequences of severe stress associated with deep poverty on early brain development (i.e., elevated cortisol and its potential harm to developing neural circuits) stated, ''I think what really gets me from the study is that it could actually have a chemical or biological or some sort of impact on the child's brain. . . . Behavior is one thing, and attitude and personality is one thing, but if it can really negatively impact . . . the chemistry and the makeup of the brain—you can damage that that early—that's really serious. That's more than just having a bad personality, that's really screwing up a kid'' … These kinds of findings underscore the need for science communicators to help nonscientists envision the material (i.e., concrete) factors that support development, rather than focusing primarily on mental (i.e., abstract) processes.

Of course, rhetoric is not necessarily misleading. In this case it depends upon the extent to which advocates' appeals to pruning, circuits, hormones, and chemicals accurately represent the current scientific understanding of those phenomena, their implications for early brain development, and the life-long effects of early experience. Such claims bring us to the heart of the myth and *The Myth.*

The Myth of the First Three Years can be stated as follows: The age from birth to three is the period of peak synaptic density (i.e. peak number of synapses per volume of brain tissue) in the human brain, during which more synapses form than are eliminated. Synaptic densities peak during early childhood at levels exceeding adult densities. By puberty synapse elimination reduces these densities to adult levels. The myth uses these neuroscientific findings to claim that the period of high synaptic density is *the* critical or sensitive period for brain development, during which children learn most easily and efficiently and during which experience results in largely irreversible, neural changes. These irreversible changes determine life-long behavior. During this period, enriched environments cause more synapses to be formed and thus more synapses to be retained after synaptic pruning occurs at puberty. Conversely, deprivation, neglect, or abuse during this period results in fewer synapses being formed and thus fewer being retained into adulthood.

The myth is based on three well-established, and relatively old, findings in developmental neurobiology. First, in early childhood there are periods of rapid developmental synaptogenesis, followed by synaptic pruning at puberty. Second, there are critical (or sensitive) periods in development, where normal experience is required for normal development. Third, rodent studies have shown that rearing animals in complex environments has demonstrable effects on brain structure. Let's briefly review the neurobiology.

Developmental synaptogenesis. The phenomenon of developmental synaptogenesis was first documented in visual areas of cat and monkey brain in the mid-1970s (Cragg et al., 1975; Lund et al. 1977). Rakic et al. (1986, 1994) examined four different areas of the monkey brain and found that at birth synaptic densities were approximately the same as densities found in adult monkeys, but the densities increased rapidly, peaking at two to three months of age, at which time densities were twice those found in adult monkeys. Densities remained at this high level until around three years of age, the age of sexual maturity in monkeys. Synaptic densities then rapidly decreased to adult levels by age four to five years.

Peter Huttenlocher and colleagues in a series of papers (Huttenlocher, 1979, 1990; Huttenlocher & Dabholkar, 1997: Huttenlocher et al. 1982; Huttenlocher & de Courten, 1987) found that peak synaptic densities and synapse elimination in humans varied by brain area. For example, synaptic density peaked in the auditory cortex at around three months of age, but not in frontal cortex until around three-and-one-half years. Synaptic elimination was complete in the auditory cortex by age 12 years, but continued in frontal cortex until mid-adolescence. Using positron emission tomography, Chugani et al. (1987) found a similar pattern in the rise and fall of glucose uptake in various areas of the human brain. This is no surprise because synapses require energy derived from glucose metabolism for their maintenance. Glucose uptake remained above adult levels until children were around 9 years old.

The problems that the science raises for the myth are obvious. Developmental synaptogenesis is not confined to the first three years of life. If the period of peak synaptic density is the critical period in brain development, then the critical period is not birth to three. Although brain areas differ, the period of high synaptic density continues well beyond age three, lasting until children are near puberty or beyond for some brain areas. Furthermore, the idea that early childhood stimulation plays a fundamental role in developmental synaptogenesis is not warranted. Developmental synaptogenesis appears to be under genetic, not environmental control. As Patricia Goldman-Rakic (1997) concluded in her review of the topic, "the developmental accumulation of synapses [i.e., the phase of early rapid increases in synaptic density] is altered much less by environmental stimulation than has been appreciated or would be expected by conventional wisdom."

Finally, all of the studies on synaptic densities, including the PET study, look at brain anatomy or physiology. None of these studies consider or measure neural function; that is, how the anatomy or physiology relates to learning or behavior. Psychologists use a variety of tasks to test for the development of memory functions, functions associated with frontal lobe activity. Figure 1 shows the relationship between performance on one of these tasks and glucose uptake in the frontal cortex of children taken from Chugani's study. The maximum rate of glucose uptake is at around five to six years of age (red line). The blue line shows change by age in the number of trials required to reach criterion (i.e., the learning rate) for young children on the one part oddity task. (Overman et al. 1996) The more trials required, the more difficult and less efficient learning is. The Y-axis for the blue learning curve is inverted. So, as the learning curve heads downward learning worsens; as it heads upward learning improves. For this task, learning and frontal lobe glucose uptake are almost perfectly negatively correlated. As brain metabolism or synaptic density increases, learning deteriorates. As synaptic density falls, however, learning improves and continues to improve until at least age 20 years. For memory tasks and spatial learning tasks, at least, the period of peak synaptic density is not the period of easy, efficient learning. Although one can talk about synapses and circuits, we know very little about how biological phenomena at this level contribute to either development or learning. There is a tendency in the myth literature to imply that we know much more about how synapses and circuits are related to specific behaviors than we actually do.

Critical periods. The concept of a critical period had its origins in embryology and ethology, most notably in Konrad Lorenz's work on imprinting in graylag geese. The fundamental idea involved in a critical period is that a structure or behavior requires normal input during a specific period, if normal development is to occur. (For an introduction to this surprisingly problematic concept see Bruer, 2001.) The concept became prominent in neuroscience through the Nobel Prize winning research of David Hubel and Torsten Wiesel on the critical period for the development of ocular dominance columns in kittens, worked that started in the 1960s. (The relevant papers have been collected in Hubel & Wiesel, 2004). Hubel and Wiesel sutured shut one eye of kittens at various ages and for various durations. These experimental manipulations affected how nerve cells from the kittens' eyes were connected to target cells in their brains. They found that cortical structures were susceptible to disruption during a "critical vulnerable period" beginning shortly after birth and lasting until two months of age. During this two month period, there was a heightened period, during the fourth and fifth months of life, when suture caused the greatest disruption to the formation of normal neural connections. They called this the *sensitive period.* Disruption during this sensitive period rendered the kittens functionally and permanently blind in the affected eye. In the popular literature, this period is likened to "a window of opportunity that slams shut."

Subsequent usage of the terms *critical* and *sensitive* did not follow Hubel and Wiesel's. *Critical period* was used to denote the period in development when lack of normal experience would cause permanent, irreversible damage to the developing organ, structure, or behavior. In recent years, developmental scientists and early childhood advocates have started to eschew *critical* altogether and to talk instead only about *sensitive periods.* (See for example the website for Harvard's Center on the Developing Child, http://developingchild.harvard.edu) This is particularly true in studying social and personality development, where it is impossible to meaningfully and ethically vary the onset and duration of experiences thought fundamental to human socio-emotional development. *Sensitive period* is intended to carry the implication of longer, ill-defined periods, when specific kinds of experiences have particularly pronounced effects on development, still allowing that these effects might be modifiable or reversible by subsequent experience. Note, however, that there is no standard usage of the terms *critical* and *sensitive* in the scientific literature. It is best to look for definitions of these terms within each article where they appear.

The myth relies on a narrow, rigid notion of critical period, where the "windows of opportunity slam shut" never to be opened again. However, it is not the case that birth to three is *the* critical period for brain development. First of all, even for a single system like vision, each function – acuity, color vision, motion vision, depth perception, binocular vision – has its own critical (or sensitive) period, some stretching into the teenage years. Furthermore, the critical periods we do know about do not all occur during the first three years of life.

Most importantly, however, critical, or even sensitive, periods constrain only specific and limited kinds of learning and development. Most learning is not subject to critical (or sensitive) period constraints. Developmental neuroscientists have come to distinguish between two kinds of brain plasticity, *experience-expectant* brain plasticity and *experience-dependent* brain plasticity. (Greenough, Black & Wallace, 1987) Evolution has resulted in there being neural systems that *expect* to encounter certain kinds of stimuli in the environment in order to fine-tune their performance. The visual system is an example. When these stimuli do not occur, typically because of defects in the eye, not because of environmental deprivation, the systems cannot be properly fine-tuned. Depriving an organism of these expected experiences results in abnormal development. Critical period constraints on development are the result of experience-expectant brain plasticity. Fortunately, the kinds of stimuli these systems expect are widely available to any individual organism and a wide range of stimuli are adequate. For example, children learn their native language under a wide variety of cultural conditions. Western mothers feel compelled to speak to their infants in motherese. In other cultures, adults do not speak to infants at all: Infants do not have anything to say. Unless an infant has damaged sensory systems or is locked in a dark room, the odds are overwhelming that the infant will have appropriately fine-tuned visual, auditory, motor and linguistic systems. Experience expectant plasticity and critical period constraints apply only to species-wide and species typical traits – vision or language in children, imprinting in geese.

Of course, one of the key assumptions of the myth is that socio-emotional development is subject to critical period constraints. The myth takes this assumption from the tradition of research in attachment theory. One reads in policy reports that the age of birth to 18 months is the critical or sensitive period for the development of the attachment relationship between infant and caregivers and that this relationship, the reports suggest, predict or determine future well-being and mental health. (See for example Allen, 2011.) It is not clear that John Bowlby, the originator of attachment theory, espoused the existence of a critical period for the formation of an attachment relationship or that he believed in the predictive or deterministic strength of the attachment relationship. (For a review of this contentious topic see Thompson 2001.) The implications of attachment theory for child development and future well-being are probably more important to understand in policy discussions than is the brain science.

Traits and behaviors unique to individuals, social groups, and cultures are acquired through the mechanism of *experience-dependent* brain plasticity. This type of plasticity accounts for most of our learning – what we learn in school and in daily life – and this plasticity is retained throughout our lifetimes. The myth literature tends to over-emphasize experience-expectant plasticity and critical periods, extending it to all areas of learning and thus highlighting the purported importance of early experience for all learning. This leads to statements such as the journalist Ron Kotlulak's about the period of high synaptic density, "Learning a foreign language, math, a musical instrument is much easier during this time." (Washington Post, August 31, 1993, Casting New Light on the Brain: The Surprising Power of Early Stimulation, p. z10) or Harry Chugani's, "Thus, it is now believed by many (including this author) that the biological "window of opportunity" when learning is efficient and easily retained is perhaps not fully exploited by our educational system." (Chugani, 1998) Education and learning are not constrained by critical or sensitive periods.

As Rutter (2002) observes, the distinction between experience-expectant and experience-dependent brain plasticity – between developmentally constrained critical periods, versus life-long learning – is probably the most important distinction to keep in mind when reading about the brain and early childhood experience.

Enriched environments. The third and final strand of the myth relates to the effects of enriched environments on brain development. This work has been done exclusively on rodents, starting in the late 1940s. (For a summary and review of this literature see Bruer and Greenough, 2001) There are two reasons why this research does not support the myth. First, enriched environments in animal experiments consist of rodents living together in the presence of objects and obstacles. This is to be contrasted with typical laboratory rearing conditions: rats living alone in shoe-box sized cages. The environment is enriched only in comparison to the usual laboratory living condition for rodents – deprivation versus no deprivation. It is not clear how this contrast applies to child rearing. Secondly, any effects of such environmental enrichment were found to influence brain development throughout the life-time of the animals. It says nothing about the heightened effects of early experience. These experimental results speak to life-long learning – experience dependent plasticity – not to critical period effects. Mark Rosenzweig, a scientist who engaged in research on the effects of enriched environments cautioned, "It is difficult to extrapolate from an experiment with rats under one set of conditions to the behavior of rats under another set of conditions, and it is much riskier to extrapolate from a rat to a mouse to a monkey to a human." (Rosenzweig et. al. 1972)

In summary, the developmental neurobiology cited by early childcare advocates *circa* 1999, does not provide scientific support for the central claims of the myth of the first three years.

Reaction from the Scientific Community

The Myth elicited considerable reaction among the general public, the media, politicians and policy makers. In May 2011, a Google search yielded 78K hits for *The Myth.* Comparable searches for other books on early childhood resulted in 18K hits for *Scientist in the Crib* (Gopnik, et al., 1999)*,* 31K hits for *Einstein Never Used Flashcards* (Hirsh-Pasek et al., 2004)*,* and 361K hits for *The Nurture Assumption* (Harris, 1998)*. The Myth* was reviewed in major American literary publications. (Tavris 1999, Bruner 2000) Generally, the response was favorable among neuroscientists and cognitive psychologists. The reaction among child psychiatrists, attachment theorists, and policy advocates was less favorable. Here I concentrate on reactions to *The Myth* from the scientific community as they appeared in the refereed, scientific literature. Since 1999, the book has been cited approximately 103 times (according to the Web of Science), primarily in developmental psychology, education, special education, and psychiatry journals.

A substantial portion of the citations might be characterized as neutral citations. These authors cite *The Myth* as a general review of brain science and early experience. For example, a National Institute of Child Health and Development study stated: "Some believe that the first 2 or 3 years of children's lives are particularly important for their cognitive development (see Bruer, 1999 for a review; Carnegie Corporation, 1994; Shore, 1997)." This citation contrasts *The Myth* with two policy reports on brain science prepared by early childhood advocates. Rutter (2004), in discussing the history of strong claims about the permanent effects of early adverse experiences and the importance of critical periods in development wrote, "During the 1990s, there was a reemergence of these earlier claims in a slightly different form, supposedly supported by neuroscience findings on early brain development and on the ways in which such development is sculpted and shaped by early experience (see Bruer, 1999, for a critique)." It is fair to say that citations of this type place *The Myth* in the category of significant scholarly writing, a book that could contribute to the ongoing scholarly discussion.

The Myth was reviewed in the two major scientific weeklies, *Science* and *Nature.* In *Science*, Mark Johnson (1999) wrote "Bruer points out that closer inspection of the scientific evidence does not support the myth" and that "most developmental neurobiologists and psychologists would agree with Bruer's conclusion that the current state of our scientific knowledge of early development does not yet provide the basis for social policy recommendations." He also rightly cautions that we must not "throw out the baby with the bath water," by ignoring what we currently do know about child development and by failing to pursue further research in this area. This is a conclusion with which I concur.

In the *Nature* review, Elizabeth Spelke (2000) wrote that the book contained "a clear and balanced review of three strains of research on brain development" and an argument that in no area "do research findings show that the potential to learn declines after the first years of human life." She states that the book "should put parents' and educators' minds at rest, but it raises troubling questions for neuroscientists, cognitive scientists, and policy-makers." Spelke raises two important issues. She notes that "a crucial gap must be bridged before research in neuroscience can inform efforts to foster children's learning: the child's developing cognitive systems must be understood at a functional level, and cognitive development must be related systematically both to developing brain function and to educational practice." Here Spelke calls attention to the lack of cognitive psychological input into the myth's discussion of early childhood development and the need for cognitive models, based on behavioral research, to guide neuro-scientific research. We need functional models of learning and development, if we are to understand brain structure and how neural structures support mental functions. Thus, Spelke articulates the central assumption of cognitive neuroscience, a discipline which attempts to link models of mental function with neural structures. The myth of the first three years, with its emphasis on attachment theory, derives more from psychiatry and a tradition within cellular and molecular neurobiology which attempts to ground psychiatry in neuroscience. As we have seen, it is difficult to link explanations, or better descriptions, at the synaptic level with development, learning, and behavior. Too often in the myth literature developmental neurobiologists make this leap by relying on their intuitions, for example, making claims about ease and efficiency of learning when their published work contains no measures of learning.

Second, Spelke asks, "Why were the implications of research on brain development in infancy so badly misunderstood by so many spokespeople?" Early childhood advocates certainly recognized the rhetorical value of developmental neurobiology, but what role did public relations experts, advocates, journalists, and even scientists take in generating and disseminating these misconceptions? This is certainly a question that psychologists and sociologists interested in how science, culture, and parenting interact should address.

The largest single class of citations were positive; that is, citations which explicitly endorsed the argument and conclusions of *The Myth*. Cichetti and Toth (2002) write: "For example, the recent embracement of "research" documenting the critical nature of the first 3 years of life for brain development although well intentioned greatly exaggerates the actual scientific data on which policy recommendations have been based and actually may undermine policies for vulnerable populations. (cf. Bruer, 1999; Nelson 1999; Shonkoff, 2000)" Charles Nelson (1999), a member of the MacArthur-McDonnell research network on Early Experience and Brain Development, wrote that examples of the success of preschool programs and critical periods for first language learning "have been used – and misused – to suggest that early experience *in general* is critical to brain and behavioral development (for discussion, see Bruer 1999)." Jack Shonkoff (2000), another member of the network and currently director of the Harvard Center on the Developing Child stated, "One example was the over generalization of research on critical periods that fueled the erroneous conclusion that human brain development is effectively solidified by the age of 3 years, despite the fact that critical (vs. sensitive) periods in the maturation of the human brain are the exception rather than the rule. (Bruer, 1999; Knudsen, 2004)" Here Shonkoff makes the distinction between critical versus sensitive periods, a distinction which has been embraced by developmental scientists and early childhood advocates.

Rutter (2002) offers an historical perspective on the science of early childhood development. His article, "*Nature, Nurture, and Development: From Evangelism through Science toward Policy and Practice"* should be required reading for anyone engaged in early childhood research or policy advocacy. Rutter discusses the difficulties involved in doing genetic, neuro-scientific, and behavioral research on early childhood. He summarizes not only the successes that the science has had, but also how the science has in some instances been misinterpreted. Rutter characterizes claims about the extent to which early experiences determine brain development as a type of evangelism. He cites Ron Kotulak's (1996) book *Inside the Brain* as one source for this evangelistic fervor. Kotulak's book, like several of the policy documents now current in the UK, attempted to argue that brain science and our understanding of early brain development were the keys to eliminating poverty, crime, and mental illness in American society. Rutter continues: "There has been a misleading extrapolation of the findings on experience-expectant development to the entirely different notion that higher quality psychosocial experiences in the first 2 or 3 years of life will have a much greater effect than similar experiences later, because the early experiences bring about a lasting change in brain structure. As several commentators have pointed out, the claims (which come from people outside the field of neuroscience research) are misleading and fallacious for several different reasons (Bruer, 1999)." Rutter underscores the root of the myth's over-generalization. It over-states the extent of experience-expectant brain plasticity and critical (or sensitive) period constraints at the expense of experience-dependent brain plasticity and life- long learning. In this statement Rutter also implies that psychosocial development should not, at least not entirely, be viewed as an experience-dependent phenomena governed by temporal constraints in development.

Of course, there were also negative reactions to *Myth* in the scientific literature. In my view, the most significant of these was the criticism of A.N. Schore, a psychiatrist and attachment theorist whose work is often cited in the UK early intervention literature. Schore states: "The neuropyschobiological literature underscores a central finding of developmental science – that the maturation of the infant's brain is experience dependent, and that these experiences are embedded in the attachment relationship." (Schore 2001) It is not clear whether his use of 'experience dependent' here should be contrasted with 'experience expectant.' Certainly, he means to convey that brain maturation depends on experience. As an attachment theorist, Schore does contend that there is a sensitive period in infant development, ending as early as 18 months, when infant and care-giver establish an attachment relationship. His concern is that when the infant brain is subjected to trauma, insecurity, and neglect, the effects of these adverse circumstances can have permanent, life-long effects. His criticism is that books on early childhood development like *The Myth* and *Scientist in the Crib* (Gopnik et al., 1999) take no account of the effects of early trauma, abuse, and neglect on developing brain anatomy. *Scientist in the Crib* is about cognitive development. Schore's interest is in the roots of psychopathology. He contends that attachment theory is fundamental to understanding not only individual development, but also social pathology as evidenced by poverty, violence, crime, and mental illness.

Schore is certainly correct about the considerable gulf between developmental cognitive psychology and attachment theory. Alison Gopnik, a developmental psychologist and co-author of *Scientist in the Crib,* and I appeared together on a U.S. television show to discuss our books in fall 1999. When the topic of attachment theory arose, Gopnik had not heard of it before. Thus, there is need for discussion and collaboration between these perspectives. Initially, however, this discussion might best occur at the level of behavioral science, rather than neuroscience. In most of the myth literature there are more citations to psychiatry than to neuroscience. For example in the current U.K. report, *Early Intervention: The Next Steps* (Allen 2011), brain science is presented in Chapter 2. That chapter contains 32 unique citations, 20 are to the psychiatry/attachment literature; only seven are to neuroscience articles. None are to the developmental cognitive psychology literature. Furthermore, there are disagreements even among the attachment theorists about the extent of the life-long effects of early experience on social and emotional development and whether attachment is best viewed as an experience-expectant or experience-dependent phenomenon. (Thompson, 2001) Social and emotional development is a place where, in the spirit of Spelke, we would benefit from a more precise functional theory of the phenomenon and the underlying mental representations, before proceeding to identify the possible neural structures that implement those representations.

Of the 103 citations of *Myth* in the scientific literature, only one, Dawson et al. (2000), presented an extensive discussion of the scientific basis of my argument. This article provides an excellent review of the science and should also be on our required reading list.

With respect to developmental synaptogenesis, Dawson et al. observe that although rapid synapse growth occurs early in development, there is only evidence for a correlation between synaptogenesis and behavioral change. What the causal mechanism might be linking these two phenomena is unknown. They point out, as does *The Myth*, that Fragile X syndrome results in higher than normal levels of synaptic density at maturity and this higher density is associated with mental retardation. There is no simple relationship between synaptic density and intelligence. They also note that early stimulation does not necessarily lead to higher mature synaptic densities. Enriched environments, better described as normal in contrast to deprived environments according to Dawson et al., does effect synapse formation in rats, but this is true throughout the rat life span, not just in the first few months. Their conclusion is, "… it is unknown to what extent the early years of life, because of the tremendous change in the sheer number of synapses that takes place during this time, represent a particularly sensitive period for selecting and establishing preferentially active patterns of neural networks that are less susceptible to change later in life."

Dawson et al. note that there are well established sensitive periods for acquiring language and vision. They also state that early interventions for phenylketonuria and autism appear to be successful. The primary focus of their discussion of sensitive periods, however, relate to the effects of early exposure to stress and social deprivation on brain and behavior and on long-term outcomes. Most of this research, as they note, has been conducted on rats and non-human primates and "less is known about the effects of prenatal stress in humans." Most behavioral studies done on humans have been retrospective studies that are susceptible to confounding and selection bias. Their conclusion about this work is rather weak: "The mechanisms responsible for these behavioral changes *may lie* [my italics] in brain development changes that arise as a result of prenatal stress." The prospective studies conducted at the time of their writing the review provided "little direct evidence" about "outcomes in children exposed to stress in prenatal or early life" and the "relation between stress response and prenatal stress in the human is hypothetical at best." The human research shows that the mother-infant relationship, that is the attachment relationship, is associated with the infant's cortisol levels; however, they conclude, it is not clear if this is a strong causal link or only correlational evidence. The most extensive discussion in the review concerns the effects of maternal depression on children's long-term emotional development, Dawson's own area of research expertise: "There is some evidence that the first years of life may represent a time of increased vulnerability for enduring effects of maternal depression," but the biological and social mechanisms of this influence are not yet understood.

None of Dawson et al.'s conclusions contradict the claims made in *The Myth*. Although the authors do venture some general policy recommendations, their review of the relevant science literature might cause one to be cautious about basing national polices on what is known about early brain development. Their review certainly should make us cautious of any strong or sweeping recommendations.

Conclusion

The Myth was well received and reviewed by the scientific community. It serves as a review of the main themes in developmental neuroscience relevant to early childhood. Its conclusions about the abuse of brain science in early childhood policy discussions have been endorsed by leading figures in developmental neuroscience, child psychiatry, and cognitive science. The arguments presented in *The Myth* are as applicable to current brain-based policy and advocacy as they were to *Rethinking the Brain* in the late 1990s.

I am often asked, "But haven't your views changed based on progress made in the neurosciences over the past 12 years?" My answer is, "No." It is true that developmental neurobiology is a vast and growing field, as are cognitive and developmental psychology, cognitive neuroscience, and psychiatry. Attachment theory has a history of over 60 years, with hundreds, if not thousands of investigators and clinical practitioners involved. When assessing the scientific basis of claims about the implications of early experience for brain development, one should keep in mind the size of the relevant scientific literatures compared to the relatively few scientific papers and authors cited in the myth literature. Note also that for the most part, the authors and papers cited in the mid- to the late 1990s are the same authors and papers being cited in current policy reports. The same over-generalizations and over-simplifications that appeared then are appearing now. The evidentiary base for claims about early brain development does not seem to be expanding, the interpretations are not improving, and the same examples, phrases, and images constantly recur. The criticisms made in *The Myth* twelve years ago still apply.

One positive contribution of *The Myth* is that it helped make some scientists realize that not all ink is good ink, contrary to what a public relations expert might say. Starting in the mid-1990s many neuroscientists uncritically stepped onto the myth bandwagon, believing that publicizing socially relevant implications of their work might not only help children, but also might increase public interest in and support of neuro-scientific research. However, the critique provided in *The Myth* motivated some scientists to take a more critical look at how neuroscience was being used and interpreted in support of early childhood interventions. Rutter (2002) provides an excellent example of what a serious and critical analysis of the science can contribute to our deliberations.

Another result has been more cautious, guarded, and/or hedged claims within the advocacy literature. Critical windows of opportunity that slammed shut, gave way to multiple sensitive periods. Irreversible brain damage gave way to claims that later remediation was not impossible, but rather not cost-effective. Advocacy for early childhood interventions became more self-consciously sophisticated. (See for example Shonkoff & Bales, 2011.) The best example of this is the material that is now appearing on the website of Harvard's Center on the Developing Child, which serves as the primary source for the recent *Parenting Matters: Early Years and Social Mobility.* (Paterson, 2011) Of course, more guarded claims are not necessarily more accurate claims. As mentioned above the rhetorical value of brain science still plays a prominent role in this new advocacy literature.

Spelke and Schore raised important scientific issues. Better functional models of early childhood development are needed, if we are to understand how neural structures support development. These models would allow us to bring the methods of cognitive neuroscience to bear on development. Schore points to the gulf in methods and interests between cognitive psychology and cognitive neuroscience versus psychiatry and its brand of neuroscience. Some progress is being made on spanning this divide through work in the relatively new field of social cognitive neuroscience. All parties involved in the early childhood debate would also benefit from a clearer understanding of attachment theory and its implications for child development.

From the wider perspective of parenting culture, the myth raises numerous issues that would benefit from study by social scientists. First of all, why is it that parents and policy makers find brain science so compelling? Why do they find behavioral and psychological data so unconvincing? There is probably more to it than merely providing a mechanistic image of child development. Kagan (1998) reviewed the long history of infant determinism in Western culture. Why is this idea so appealing? Why does it persist? Again following Spelke's lead, what is the source of the widespread misperceptions about brain science? Does it come solely from advocacy groups, non-profits, and lobbyists? How are scientists involved, if at all? When is it appropriate for scientists and scholars based in universities to engage in advocacy? What norms should apply to them? Scientific and scholarly norms or the norms of real-world politics? Appeals to science to support policy ends are only going to increase. How might we develop norms and institutions to govern and moderate this phenomenon?

There is nothing wrong with attempts to improve parenting, child care, and social policy through appropriate use of the natural and social sciences. We should look for every opportunity to do so. What we should avoid, however, is selective appeals to science to rationalize what may be only our own pre-conceived policy ends. This is politics disguised as science.

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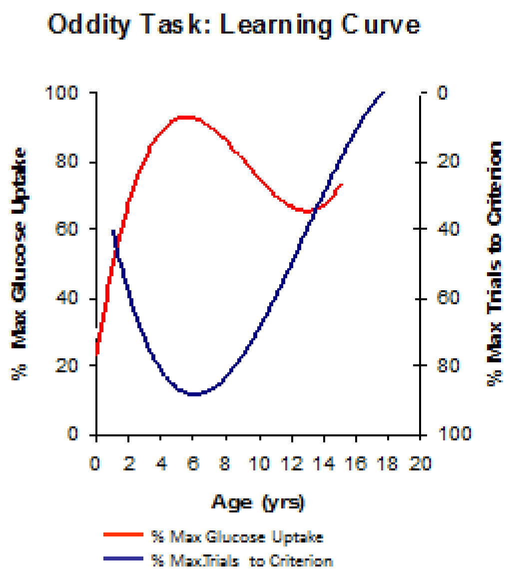
Figure 1



Oddity Task: Learning Curve
% Max Glucose Uptake
% Max Trials to Criterion
Age (yrs)
100
80
60
40
20
0
0
20
40
60
80
100
0 2 4 6 8 10 12 14 16 18 20
% Max Glucose Uptake
% Max.Trials to Criterion

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