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THE POLITICIZED SCIENCE OF DAY CARE:
A PERSONAL AND PROFESSIONAL ODYSSEY

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In the Beginning

Once upon a time there was a little American boy who wanted above all to attend the United States Military Academy at West Point. It is very difficult to gain admission to this elite institution, which is free of charge to those few who are admitted.

When it came time, in 1970, the now young man was invited to enrol in the United States Military Academy at West Point. But then something happened which was to characterize so much of this man's professional development—he changed course. The fact of the matter was that this young man did not so much aspire to be a soldier as to be of service to his country. The School of Foreign Service at Georgetown University seemed a perfect alternative to West Point. And so it was at Georgetown, in Washington, D.C., that he began his university education.

That the little boy and young man was this author. Neither the little boy nor the young man Jay Belsky back in the 1950s and 1960s could even have imagined writing this essay today. Even though I always knew I would go to college, it never crossed my mind that one day I would regard myself as a teacher and scientist. But, as so many of us know, life is full of surprises. And around these I will structure this narrative.

After one year at Georgetown, I came to realize that the study of history and diplomacy was not for me. I found myself mired in a classic identity crisis. Who was I? What did I want? And where was I headed? These questions hung heavy on my youthful mind. But then things changed—in a seemingly accidental manner.

Discovering Children and Child Development

One fine autumn afternoon while I was sitting under a tree outside the university library, a former teammate of mine from the soccer team walked by with a whole bunch of little kids, probably

around four years of age. When I asked him how come, he responded that he had volunteered at the Georgetown University Hospital day care center, which was always looking for volunteers.

So, I picked myself up and volunteered. And boy, did I enjoy it. Apparently, too, I was pretty good at it. Within a few months the head teacher informed me that there was an after-school program for impoverished children that needed someone to run it. I signed up to do that, too.

My identity crisis soon resolved itself; I knew what I wanted to do. I still wanted to serve my country, but in a very different manner; I wanted to work with children. Indeed, what I wanted was to become a nursery school teacher—not the typical path for a Jewish boy from New York. And so I transferred from Georgetown University to Vassar College, which had a nursery school on campus and no course requirements. I could study psychology and child development—virtually exclusively—and spend time with young children. After graduating from Vassar I headed off to Cornell University, where wonderful teachers exposed me to research on the effects of day care on child development.

The Effects of Day Care: A Critical Review

At the end of my second year in graduate school I joined the eminent child developmentalist, Urie Bronfenbrenner, in reviewing what was then known about the effects of day care, under contract from the U.S. government. A fellow graduate student and I prepared a report that went out under Bronfenbrenner's name to the then U.S. cabinet department of Health, Education and Welfare.¹

Our paper, entitled "The effects of day care: A critical review," was published as the lead article in a prestigious child development journal in 1978 and was very well received. Most regarded it as giving day care "the green light." For years there had been many claims, some of them hysterical, about the dire effects of nonmaternal childrearing, but we basically concluded that such assertions could not be scientifically substantiated. Fundamentally, there was little credible scientific evidence

¹ Bronfenbrenner, Belsky, & Steinberg, 1976.

that day care disrupted the emotional bonds between mothers and children or adversely affected their psychological and behavioural adjustment.

But we further pointed out that our capacity to draw strong conclusions about the effects of day care on children was severely constrained by the nature and quality of the available scientific evidence, qualifications that went largely unheeded by citizens, academics, and policymakers alike. In short, the degree to which our essentially “green light” conclusions could be generalized was highly questionable. Nor were we in a position in 1978 to say much about child care initiated in the first year of life or that which was experienced on a full vs. part-time basis.

Infant Day Care: A Cause for Concern

Over the next eight years I was repeatedly invited to write and talk about the effects of early child care and returned again and again to the research I had reviewed initially. This led me in 1984 to publish one of the first reviews pertaining to how variation in quality of child care affected child development.² And again I pleased many, in that I concluded that when caregivers were attentive, stimulating, and affectionate, children’s cognitive and social development was enhanced; and that such high-quality care came when caregivers were reasonably well trained, the size of groups not too large, and the number of children per caregiver on the low side. Once more, without really appreciating it, I was saying politically correct things and being applauded for it. Indeed, this, along with other research, led to an early career award in 1983 by the developmental psychologists in the American Psychological Association.

By the time 1986 came around, I found myself preparing another child-care talk, this time to be delivered to an annual convention of pediatricians. As I worked my way through the newly emerging evidence, I realized I could no longer tell the same story that I had been telling—and that so many still are. Instead, I described “a slow steady trickle of disconcerting evidence” linking nonmaternal care in

² Belsky, 1984.

the first year of life with greater insecure infant-parent attachment and increased levels of aggression and disobedience when children were 3–8 years of age. Drawing the totally unacceptable conclusion that nonmaternal care beginning in the first year of life should be regarded as a “risk factor,” I pointed out that others “would, should, and could” disagree with me, because the evidentiary base was by no means clear-cut and that the “inferences” I drew were based upon “circumstantial evidence.” I noted further a possible conflict of interest, as I was the father of two young sons who were cared for during their first years of life by a stay-at-home mother and thus, unwittingly, bias could have affected my reading and interpretation of the scientific literature.

When my article entitled “Infant day care: A cause for concern?”³ appeared in print, to my amazement it generated a firestorm of controversy. Not only did stories about me and my thinking appear in the *New York Times*, *Washington Post*, *Wall Street Journal* and *Time* magazine, but I was accused of being against day care, against women working, and—more or less—a misogynist. It was rather amazing how quickly supporters and admirers turned me into the devil incarnate almost overnight. What I only came to realize in retrospect was that I had run smack into the wall of political correctness before that term was even coined. Moreover, I had violated what I have come to regard as the 11th commandment of the field of child development: “Thou shalt not speak ill of day care—in any manner, shape or form.”

I suspect that what really drove my critics to distraction was that I simply refused to back down. I had not previously written about day care in order to curry favor with my colleagues or anyone else. So it made no sense to me to change my views simply because others were displeased with them, especially as they arose using the same kind of analytic skills and reasoning that had previously led me to views that had pleased so many. Some argued that I said what I said in order to

³ Belsky, 1986.

capture attention, and even that I had published my thoughts in a professional newsletter that I knew would end up on every congressman's desk.

The facts could not have been more different. To me, the professional newsletter called *Zero to Three* (having to do with children three years of age and under) in which my infamous essay appeared in December 1986, was an obscure outlet which I thought few would read and would not gain me any credibility with those academics who would eventually evaluate me for promotion and tenure. In fact, this publication was so insignificant that I first declined to write for it. But the editor pestered me until I finally gave in.

Once it was in her hands, she passed it on—without informing me—to other child care researchers hoping for a response. And the commentary prepared by four child-care researchers was scathing. It questioned my motives, my understanding, and my intelligence. But it was also so ill-informed, despite the outstanding credentials of its authors, that it was easy to rebut. But then something even more remarkable happened. After my rebuttal was submitted so that all three pieces—my original essay, my critics' fierce commentary, and my reply— could be published in the same issue, my critics replaced their injudicious commentary with a sanitized version. I was livid. So I phoned the editor and we negotiated a settlement: either the original commentary and my rebuttal would appear along with my essay, or the sanitized commentary and a new rejoinder by me would appear in a later issue. My critics chose the latter option, no doubt recognizing that they stood to look unprofessional and obviously ideological should the first commentary appear in print.

The NICHD Study of Early Child Care

One of the most important consequences of what became known, rather appropriately, as “the day care wars”⁴ was that the U.S. government decided there was a need to study the controversial issue. One of the arms of the National Institutes of Health, the National Institute of Child Health and

⁴ Karen, 1994.

Human Development (NICHD), issued a call for proposals to conduct research on the effects of early day care. I applied with a team of collaborators, as did many of my intellectual adversaries, and what would become the NICHD Study of Early Child Care was born. Not only would a huge amount of research money be available to examine the effects of early child care, but I would end up working closely with some of the very individuals who regarded me almost as a pariah, as well as with a variety of other developmental psychologists, most of whom held views much closer to my critics than to me.

Together we launched a 10-site investigation of more than 1,300 newborns and their mothers, with plans to follow them until three years of age, in order to examine the effects of child care on socioemotional and cognitive-linguistic development. That study has been going on for more than a decade now, with children currently being studied as 11-year-olds, though here I will only be able to address findings through the first 6–8 years of life. Needless to say, the enterprise proved challenging. Differences of view in planning the research often became heated; within the NICHD, we became known as “the study from hell.” But we succeeded in something that I doubt few others have: our capacity to work together has, for the most part, increased.

Initial Findings: Early Child Care and the Mother-Child Relationship

It took some time before the NICHD Study, launched in 1989, began to yield empirical fruit that could even address, much less resolve, the infant-day-care controversy; and when it did, old views still served as the filter through which new data would be interpreted. This was clearly the case when we first addressed the contentious issue of infant day care and the security of infant-mother attachment, a developmental outcome measured at 15 months of age in our study. This developmental outcome had been a source of great contention in the field of child development and especially among those pondering the effects of infant day care. And this was because one of the controversial

conclusions I arrived at in 1986 and buttressed two years later in 1988 was that more than 20 hours per week in nonmaternal care in the first year of life increased the risk of insecure attachment as measured in a procedure called the Strange Situation⁵, which involved separating mother from baby repeatedly, while exposing the baby to a strange adult in an unfamiliar room in order to gauge the infant's reaction to this purposefully stressful situation. To many, this still highly regarded scientific method was too artificial to prove informative, though I always suspected that this critique had more to do with the findings this procedure yielded than with the procedure itself. Had the same scientific procedure shown the opposite—that high-quality care promoted secure attachment—I doubt that many critical of this approach would have raised objections.

In any event, results of the NICHD Study showed several things with respect to infant-mother attachment security.⁶ First, as anticipated, infants were more likely to develop secure attachments by the time they were 15 months of age when they had experienced more sensitive, responsive care from their mothers, irrespective of child-care experience. But it was not the case that being in care for more than 20 hours per week—by itself—increased the probability of insecure attachment. Instead, the evidence revealed that nonmaternal care in the first year of life increased the likelihood of insecure attachment only among infants whose mothers scored low in sensitivity *and* who experienced any one of the following three child care conditions: more than 10 hours per week of care, on average, across the first 15 months of life; lower quality of care as observed via extensive visits to any nonmaternal care arrangement that the child was in at six and 15 months of life; and more than one arrangement across their first 15 months of life.

Many of my earlier critics were keen to conclude that lots of time in nonmaternal care in the first year of life did not of itself predict insecure attachment. What they failed to appreciate—or

⁵ Ainsworth & Wittig, 1969.

⁶ NICHD Early Child Care Research Network, 1997.

conveniently overlooked—was that my original conclusion was that infant day care was a “risk factor,” and risk factors tend to operate in concert with other risk factors. For example, although smoking increases the risk of heart disease, this risk is far more likely to be realized when a smoker is overweight, or does not exercise, or has a family history of heart disease. Our findings were just like this: More than just 10 hours of care per week, on average, not the 20 that I had written about in follow-ups to my 1986 essay, proved to increase the chances of attachment insecurity when mothers scored low in sensitivity—a known risk factor for the development of insecure attachment. I was thus rather astonished by how cavalierly many of my collaborators drew conclusions.

One of the major struggles that the collaborative group had involved the use of the Strange Situation in order to study the quality of the mother-infant relationship. Many critics of this procedure—or at least of the findings it generated when used in studies of day care—argued, not unreasonably, that if you want to understand the mother-infant relationship, you should just observe interactions between mothers and infants (rather than expose them to separations in an unfamiliar situation, i.e., the Strange Situation). So that is also what we did when infants were 6, 15, 24, and 36 months of age. Indeed, it was videotaped observations and home-based appraisals of the quality of mothering that enabled us to classify some mothers as more vs. less sensitive. When we examined these data on mother-infant interaction with regard to effects of child care, they were fascinating, but perhaps less so than many of my collaborators’ reactions to them.

Detailed analyses revealed that when infants spent more time in nonmaternal care over their first 6 months, their first 15 months, their first 24 months and even their first 36 months of life, mothers were less sensitive to their infants when observed interacting with them at 6, 15, 24, and 36 months of age.⁷ That is, we detected this consistently negative effect of time spent in early child care using the very scientific methodology that critics of the Strange Situation argued we should use to

⁷ NICHD Early Child Care Research Network, 1999.

understand the developing mother-infant relationship and to study effects that nonmaternal care might have on it. But advocates of this methodology failed to embrace these disconcerting findings.

Instead of embracing these new data and arguing that yes, there was some kind of linkage between lots of time in nonmaternal care and less positive mother-infant and mother-toddler relationships, many of my collaborators acted as if they had never vociferously advocated observing mothers and their children while dismissing the Strange Situation. Indeed, at a press conference arranged by the NICHD at a major meeting of child developmentalists, one of my collaborators argued that the mother-infant interaction findings at age 3 were not important because similar findings did not emerge when attachment security was measured at age 1 in the Strange Situation!

Needless to say, I found this illogic ridiculous, though trying to explain it to journalists was often futile. Journalists who had themselves embraced the argument that the Strange Situation was an artificial procedure were now ready to dismiss mother-child interaction findings because they were not entirely consistent with Strange Situation results.

Moreover, when it came time to prepare a press release for another child development conference, what I have come to refer to as the “bad news” about the effects of early day care was underplayed while the so-called “good news” was heralded. Indeed, when the NICHD-Study researchers worked collaboratively on a press release, we generated one that highlighted—in its title, its opening paragraph, and throughout its first page—both sets of findings. The good news finding (not so labelled in the press release) was that good quality child care predicted enhanced cognitive and language development when children were two and three years of age; the bad news finding was, as already indicated, that more time in care predicted less sensitive mothering and less harmonious patterns of mother-child interaction across the first three years of life.

But when the press release was issued by the NICHD, the final version of which the investigators never got to see until it was distributed to the press, it read quite differently. It heralded only the good news in the title, first paragraph, and (double-spaced) first page, burying the bad news on the (single-spaced) third page! The fact that very few of my collaborators objected to this misrepresentation would prove illuminating as time went on.

Later Findings: Early Child Care and Problem Behavior

This reporting bias was demonstrated again at yet another meeting at which findings from our study were announced, this time dealing with children's social behavior, especially their so-called problem behaviour—aggression and disobedience. To fully appreciate this feature of the story, it helps to know that the results of the NICHD Study showed, consistent with my risk-factor conclusion, that the more time children had spent in nonmaternal child care across their first two years of life, the more aggression and disobedience they manifest, according to reports by caregivers. But this result was not found in children one year later, at age 36 months. This seeming discrepancy resulted in differences of opinion among collaborating investigators, differences that were reflected in the discussion of these results in the scientific paper reporting these findings.⁸ We noted that whereas, on the one hand, these results suggested that whatever risks might be related to early child care with respect to aggressive and disobedient behavior at age two were not of great concern because they were no longer evident when children were older, on the other hand, it could not be assumed that the effects detected at age two but not at three would not materialize yet again when children were older.

My own take on the inconsistency was that it was premature to draw any conclusions; that more follow-up was needed. Indeed, when I was repeatedly asked about these findings by journalists, I used the following analogy: Imagine that you step out of your home to go to work in the morning and it is raining lightly. Do you go back inside to get an umbrella because it might continue to rain for the

⁸ NICHD Early Child Care Research Network, 1998.

rest of the day? Or do you disregard the umbrella, presuming that the rain is just a passing event? And so, relatedly, one could not know on the basis of data collected and analysed through 36 months of age what would develop in terms of the relation between extensive child care experience and eventual problem behavior.

When I shared this analogy with one of my NICHD-Study colleagues, the retort was, “But it’s not even raining,” meaning that the effects of time in child care on aggression so far detected should not be considered very meaningful at all. What, of course, my colleague refused to acknowledge was the *pattern* emerging in the data: more than just 10 hours of care predicting increased probability of insecure attachment at age 15 months when mothers provided relatively insensitive care themselves; more time in nonmaternal care predicting less sensitive mothering and less harmonious patterns of mother-child interaction when children were 6, 15, 24, and 36 months of age; and more time in care predicting more externalizing problems when children were 24 months of age, even if not at 36 months of age.

The studious avoidance of the *pattern* of disconcerting evidence became even more apparent when our analyses of data on 54-month-olds once again revealed a relation between more time in nonmaternal care across the first 54 months of life and elevated levels of externalizing problem behavior at 4.5 years of age. Although most of my collaborators were happy to report findings linking better quality of care and enhanced cognitive-linguistic development at 54 months of age, the group decided that more scrutiny of bad-news findings was called for before they could be embraced.

When we did look at the kindergarten data, we again detected the relation between more time in nonmaternal care across the first 4.5 years of life and problems involving aggression and disobedience. So now we had evidence that more time in nonmaternal care across the first 4.5 years of life predicted increased problem behavior during the first year of schooling as well. Moreover, this

seemingly adverse effect was evident whether we gathered information on child behavior from mothers, caregivers, or kindergarten teachers.

But even this was not deemed sufficient by the NICHD-Study investigators to permit dissemination of these findings. We now needed to answer still another question having to do with whether more time in day care predicted high levels of problem behavior. And so we did. But never had study investigators endeavoured to determine whether lower quality of care predicted especially low levels of cognitive functioning or whether high quality care predicted especially high levels. It was enough just to find out that a statistically significant positive association existed between quality and cognitive functioning to report findings and even propose social policies promoting better quality child care.

When I pointed out this “uneven-playing-field” problem to my collaborators, the different standards demanded of good-news and bad-news data did not seem to bother them. So we proceeded to determine whether more time in child care was associated with scores in the so-called “at-risk” range. Even at this higher standard for measuring problem behavior, the data again were consistent with the conclusions I had reached back in 1986 linking lots of time in child care, especially beginning early in life, and elevated levels of aggression and disobedience. Having said this, it needs to be pointed out that the overwhelming majority of children, irrespective of time in child care, did not score in the at-risk range. By the same token, most people who smoke do not get lung cancer, even though smokers are clearly at higher risk than are nonsmokers.

But even these data were not sufficiently convincing for some of the NICHD-Study investigators. So the bar which the data had to jump over was raised once more, with the next question being whether time in child care predicted aggression and disobedience or just assertiveness, i.e., independence. Moreover, some had long argued that when it came to the effects of child care, assertive

independence might be confused with aggressive and noncompliant behavior. In response to this demand, the data on problem behavior were reconfigured to create three separate scores. The aggression score included, among others, the following behaviors: cruelty to others, destroys own things, gets in many fights, threatens others, and hits others. The noncompliance/disobedience score included, among others: defiant, uncooperative, fails to carry out assigned tasks, temper tantrums, and disrupts class discipline. And, finally, the assertiveness score included the following items: bragging/boasting, talks too much, demands/wants attention, and argues a lot. When time in child care was tested against each of these more refined problem-behavior outcomes results indicated that children who spent more time in nonmaternal care across their first 4.5 years of life were not simply more assertive, but more aggressive and disobedient.

So, as it turned out, each and every time the collaborating investigators demanded more convincing data, the data confirmed what was evident initially at 4.5 years. Only now it was not just the case that more time in care predicted more problem behavior on average at 54 months, but also more problem behavior in kindergarten, more problem behavior in the at-risk range, and more indisputably aggressive behavior. One might think that having continued to raise the bar that the bad-news' data had to jump over, open-minded, data-driven scientists would say, "There is a relation here between time in child care and problem behavior; it is as strong as that linking quality care to cognitive-linguistic functioning; it is persistent, and so it should be deemed as significant as other (good-news) study findings." This was not to be, however.

But first, several things need to be noted. Whether dealing with good-news or bad-news findings, effects of child care were never particularly strong in the NICHD Study; and usually, though not always, family factors like maternal sensitivity, maternal depression, or family income, were stronger predictors of children's functioning than child care experience. But before modest effects,

whether positive (in the case of quality) or negative (in the case of quantity), are dismissed, note that in the U.S., more and more children are spending more and more time in nonmaternal care, at younger and younger ages than ever before. It may be important, therefore, to contrast a phenomenon that has a large effect on a few children with another—perhaps time spent in nonmaternal care—that has a small effect on lots of children. Consider the consequences of being a teacher in a kindergarten classroom in which lots of children have lots of early, extensive, and continuous child-care experience vs. being a teacher in a classroom in which many fewer children have extensive child-care experience. Quite conceivably, the former teacher may spend a lot less teaching and a lot more managing of her students than the latter, given that the collective consequences of lots of children who are aggressive and disobedient.

Fratricide: The Day Care Wars Revisited

In April 2001, the NICHD Early Child Care Research Network was scheduled to present its findings pertaining to quantity of child care and children's socioemotional adjustment at a national child development conference, along with other findings derived from the collaborative study, including those pertaining to the effects of quality and type of care on children's development at age 4.5 years. Although we had not prepared a press release, an audio press conference was scheduled by the conference organizers and included two investigators from the NICHD Study, one of whom was myself, and investigators from other research projects. Not surprisingly, most of the journalists proved especially interested in the bad-news child care findings, principally, I suspect, because there was little new about the other findings.

As I had been assigned the task of reporting the findings linking lots of time in child care with problem behavior, when front page reports of our findings appeared in newspapers around the country, I was the source cited. As a result, these findings became known as emanating from a study I had

done, instead of from a multi-investigator, collaborative study in which none of these findings could be disseminated without the approval of the investigative team. In consequence, it became easy to dismiss the findings, because, as multiple stories reported, “Belsky is against child care,” implying that somehow I had created the results out of thin air, or close to it. One thing that continually amazes me is the inference that a communicator or discover of results is an advocate or in favor of them. But as I often tell reporters and colleagues alike who fall into this trap, just because the weatherman says that it is going to rain tomorrow does not mean that he is against sunshine! The truth remains, as it always has, that I am neither for nor against child care; I am, rather, a developmental scientist studying the effects of child care. When the evidence indicated to me that child care had few anticipated negative effects, I said so;⁹ and when the data indicated that higher quality of care predicted enhanced child functioning, I said so, too.¹⁰ And when the data first indicated to me that there were risks associated with care initiated in the first year of life,¹¹ especially when it was for more than 20 hours per week¹² and particularly when care continued at this high level until school entry,¹³ I said so as well. Many who accuse me of having an axe to grind either conveniently ignore this history of following the data where it leads, or simply are not aware of it.

But what was even more troubling than the reporters was how my collaborators, who were as much authors of the results reported as I, responded to the stories in the press. When finally asked to comment on them, many of them too began to act as if these were not their data and that the findings were not really what I said they were—despite the fact that what I said in the press conference was exactly what was written and approved by all in the written paper we would present at the conference.

⁹ Belsky & Steinberg, 1978.

¹⁰ Belsky, 1984.

¹¹ Belsky, 1986.

¹² Belsky, 1988, 1990.

¹³ Belsky, 1994, 2001.

Collectively, the effect was to marginalize me, to represent me as someone who distorted the data in order to pursue some kind of personal or political agenda.

Some colleagues accused me of exaggerating the size of the effects, even though I made clear to reporters that we were talking about modest effects. Some colleagues accused me of stealing the limelight, even though I was not the one who decided that I would report the pertinent child-care findings at the press conference. One very well-known policy-minded child developmentalist who was not a study investigator accused me, as did others, of making the most basic scientific error of drawing causal inferences—perhaps children would benefit if they experienced less child care—from correlational, that is, nonexperimental, data. What I found so amazing about this critique was that it was never wielded against those who argue that because (correlational) evidence shows that good quality care is related to enhanced child functioning, the government should invest more money in improving the quality of child care. In other words, it is not so much sinful to draw causal inferences from correlational data, as it is to draw *certain types* of causal inferences.

One of the concerns raised by several collaborators that bothered me the most was that reporting disconcerting day care findings would make mothers using child care feel guilty. This concern bothered me for two reasons. First, as scientists we should not play the role of clinicians, trying to take care of people. Second, if being concerned about how our results would affect the feelings of working mothers is so important, why was no one concerned when, four years earlier, the NICHD Study had reported that children who experienced high-quality child care evinced greater cognitive-linguistic functioning at ages two and three than children cared for at home exclusively by their mothers? How might this affect stay-at-home mothers?

In point of fact the critical comments made about me by my own collaborators got so bad that I finally had to threaten to take them to court for slander if their aspersions did not stop. Fortunately,

that not only stopped things, but the entire experience so embarrassed all of us that we agreed not to engage in any more name calling.

But this is not to say that the core problem of politically incorrect findings and “an uneven playing” field did not pose further problems. The next step was to proceed to get our results written up for scientific publication and public dissemination. Preparing the scientific manuscript, recently published in perhaps the most prestigious journal dealing with the science of child development¹⁴ proved a steep hill to climb. The issues were invariably the same as those negotiated all along the way. And, in consequence, the core issue became whether the scientific glass with empirical findings in it pertaining to time spent in child care and problem behavior was “half empty” or “half full,” with many intent, to my mind, on minimizing the significance of the findings. What always unnerved me was how professionals who had for years asserted that the only risks associated with lots of time spent in child care or with the early initiation of child care were the result of low-quality child care could confront our data which showed that this was not so and never, ever acknowledge that somehow they had it wrong. And what I never understood was what the shame was in defending a logically and scientifically defensible position but then abandoning it in response to the evidence. After all, why were we spending tens of millions of taxpayer dollars to study the effects of early child care if certain findings were going to be minimized, if not denied and dismissed, while others, no more or less strong were going to be embraced and perhaps even exaggerated? If we were not open-minded enough to embrace with equal fervor all of our findings, how could we claim to be developmental scientists rather than developmental missionaries?

The same old arguments had to be dealt with and even after we had finished the paper, we were not really done with it. Because months after it had been accepted for publication but before it had actually appeared in print, we needed to produce a “public summary” which the journal would release

¹⁴ NICHD Early Child Care Research Network, 2003

to the press, as well as a NICHD “press release.” In my opinion, these did not represent the multiple—and balanced—voices that the scientific text had striven to, and to an amazing extent, did achieve in the journal article. Instead, these public disseminations gave further evidence of an uneven playing field, one embraced widely by many who study child development and even those responsible for preparing certain government press releases.

For me the bottom line remains that open-minded scientists and citizens can debate the meaning of the findings we generated. But it must be a principled discussion rather than an opportunistic one. If one wants to argue that the results pertaining to aggression are not that important because they are small in magnitude, fine, but then the same analysis has to be applied when the good news findings about quality of care is under discussion, because those effects are also of modest magnitude. If one wants to argue that most children who experience early, extensive and continuous child care are not highly aggressive, only a little bit more so, then also acknowledge that most children who experience low-quality care are not seriously compromised by it, and those who experience high-quality care are not highly superior to the others. And, as a final example, if one wants to argue that it is risky to draw causal and policy-relevant inferences from correlational child-care data, then that must apply to all child-care findings, not only the politically-incorrect ones. In sum, from a scientific standpoint, to say nothing of a logical one, one cannot simply pick and choose those findings one dislikes and critique them, while refusing to deploy the same critique against more favored findings. That, after all, is not science, but politics.

Conclusion

What is rather amazing in retrospect is that more than 15 years ago I almost had my head taken off by sharing some emerging concerns about potentially negative effects of early child care. Because of legitimate concerns about the quality of research on which I based my conclusions, the NICHD

Study was launched at a cost of tens of millions of dollars. But why expend so much tax payer money if the investigators are not prepared to embrace the findings they generated, but stick to positions they held when the child care wars first broke out?

Back then the argument was that the available data on child care and attachment security, mother-child interaction, and problem behavior were compromised by three important factors. First, studies did not take into account sufficiently the fact that families that relied upon lots or little child care, or better and worse quality child care were different in the first place and such differences could be responsible for any apparent effects of child care—an argument rarely raised when good-news findings were under consideration. The second compromising factor was that studies implicating early entry into child care or lots of time in child care in the development of insecure infant-mother attachment or elevated levels of aggression and disobedience did not take into consideration the quality of child care, so effects of poor quality care might be masquerading as effects of lots of time in care beginning early in life. Interestingly, this argument was never reversed, such that data on quality care was judged to be compromised because no attention was paid to amount of care or to timing of entry into care. And the third critique was that independence and assertiveness might be confused with aggression and disobedience when it came to evaluating effects of child care, though this concern was never raised when low-quality child care was linked to poorer social functioning, only to more time in care and/or early entry to care.

Many years ago my mentor, Urie Bronfenbrenner, argued that the field of child development, and the social sciences more generally, needed social policy to highlight issues in need of scientific investigation. Sadly, I have come to regard what I once perceived as his wisdom to have been misguided. It has become my view that all too often social policy—politics—corrupts the process of science and the scientific imagination. And it does this by making certain findings “wrong” and others

“right.” As a result, the scientist who is willing to report unpopular results is all too frequently blamed for generating them and accused of wanting to find them and designing his or her research to reveal them. These blame throwers thus routinely commit the very sins they accuse others of—in the service of what they presumptuously regard as good causes. In point of fact, what I find even more scientifically scandalous than so many of my critics are the legions of my fellow social scientists who collude with them by saying nothing at all and thereby afford inconsistent, illogical, and even ridiculous arguments a credibility they simply do not deserve.

[USE AS SOURCES FOR FOOTNOTES]

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