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Spitz's Reply to Ramey's Response to Spitz's First Reply to Ramey's First Response to Spitz's Critique of the Abecedarian Project

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Responses are given to Ramey's 10 "substantive amplifications." The ability test difference between the intervention and control groups at 12 years of age is approximately the same as the difference had been at 6 months of age. This finding remains unexplained. Some of the data are still not forthcoming. I remain unconvinced that the Abecedarian Project provides evidence that quality educational day-care services can prevent mild mental retardation in children who are said to be at risk because they come from economically and socially impoverished homes.

The Lewis Carroll-like title of this article illustrates one reason why scholarly debates in professional journals are proscribed by editors. Typically, the target of a critique is given the opportunity to respond, then the author of the critique replies, and there's an end to it. Not so here. As I understand it, Dr. Ramey threatened to withdraw his first response unless he was permitted to respond to my reply. This could go on ad infinitum. I'll reply to each of the 10 "substantive amplifications" (Ramey, 1993, p. 25).

1. Over the last few years I have received neither reprints nor responses to my inquiries from Dr. Ramey's office. I do not have a fax machine and even if I did I would neither fax nor phone requests to my colleagues. In March 1991, I requested data on the Abecedarian children of mentally retarded mothers, but received no reply.

2. Instead of putting forth his theory of child development in his first response, Ramey could have responded in a point-by-point fashion to my critique, even if it made his response disjointed. That, and not the opportunity to present a new theory, was why he was asked to respond. I continue to believe that the *complete* cohort results are important, and Ramey's refusal to supply them is his own "selective scholarship." It suggests that even a request by fax or phone

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would have been unavailing. When a project suddenly stops presenting cohort analysis on new data it raises the suspicion, justifiable or not, that new results are embarrassing to the original hypothesis.

3. There were two articles Ramey (1992) had originally cited as "in press" at the time I wrote my reply (Spitz, 1993): Ramey and Campbell's (1992) chapter for *Children in Poverty,* and Ramey and Ramey's (1992) article for *Applied and Preventive Psychology.* Dr. Ramey says he sent me copies of them. If he did, I never received them. When I received his reply to my critique from Dr. Detterman's office (*not* from him), there were no articles accompanying it.

4. The term "yoked" is unnecessary; reading the sentences without it changes nothing (Spitz, 1993, pp. 20, 21).

5. The only difference between the 54-month data and the 60-month data is the control group's rise in IQ of 3 points (Spitz, 1992, Table 2, p. 230), so the only "confounding" of the data by including the 60-month scores is a small rise of 3 points in the mean score of the control group. In intervention research, it is crucial to monitor the groups continuously, because we want to know how permanent any effects might be.

Additionally, subdividing the 60-month data of the four cohort groups into the four new treatment groups would indeed greatly reduce the sample size, but why not simply look at the four cohorts as if there were no new treatment? I assume the members of the different cohorts were distributed equally, or nearly equally, to the various new conditions, or simply partial out the new treatment effects, if any. Was the new treatment so powerful that it had an immediate effect on IQ?

6. Ramey, of course, performed an analysis of variance, not a factor analysis. My subsequent statement concerning the significant interaction suggests that I was thinking analysis of variance despite my unfortunate mental typo.

7. Here is the paragraph from Ramey's reply, which reminded me of the Festinger, Riecken, and Schachter (1956) book, *When Prophecy Fails:*

> What is already clear is that substantial transactional enrichment during the first 5 years of life can produce dramatic intellectual differences (of greater than 20 IQ points or more) in experimental group children relative to controls, who come from particularly developmentally impoverished families characterized by economic poverty and maternal mental retardation (e.g., Garber, 1988; Martin et al., 1990). (Ramey, 1992, p. 252)

If Ramey had meant that to apply exclusively to the 6 experimental and 7 control children of mentally retarded mothers he should have said so. When he wrote that it was "already clear" I just assumed he was discussing the entire sample, because such finality could not be given to such a small subsample.

Although the results for the children of mentally retarded mothers—given by Ramey (1993) in his Figure 1—are based on very small groups, they are quite impressive. They greatly pleased Howard Garber (Garber, Hodge, Rynders, Dever, & Velu, 1991), who contends that "sociocultural" mental retardation results from the experience of being raised by an intellectually limited caregiver (usually the mother) and not simply from growing up in a poor social and economic environment (Garber, 1988). According to Garber and Hodge (1991), "The Abecedarian Project, which was begun after the Milwaukee Project intervention was over, chose to ignore the finding that risk for declines in IQ across age was *familial* rather than social" (p. 321). Indeed, Ramey's results suggest that this subgroup of the Abecedarian Project contributed disproportionately to the differences found for the entire group. What do the results look like with these subjects deleted?

8. Why did I ignore Ramey's other projects? Because my article was a reaction to the claim in Martin, Ramey, and Ramey (1990) that the Abecedarian Project prevents mental retardation caused by impoverishment. Writing a critique of *one* project, the Abecedarian Project, required a substantial investment of my time. To have as thoroughly reviewed Project Care and the Infant Health and Development Program would have required another year or more. I did *not* say that the other projects are irrelevant as independent samples, and it is disingenuous for Ramey to imply that I did. Here is what I wrote: "The remainder of Dr. Ramey's (1992) article is irrelevant to the critique of the Abecedarian Project" (Spitz, 1993, p. 22).

9. I'm acquainted with the literature on IQ constancy (see, e.g., Spitz, 1986). Ramey's (1993) statement that "the same score at a later age reflects more sophisticated performance" (p. 28) is a truism, but how it could have led to the curve in Figure 1 (Spitz, 1993) escapes me. In the curve, the positive effects of intervention are reflected in the extent that the curve rises from an assumed baseline of zero difference between the intervention and control group. If the groups had differed prior to intervention, that is, at 3 months, the figure would be redrawn with some indication (perhaps a dotted horizontal line) that the 3-month data point is the new baseline. The curve shows that, very early in the project, the intervention resulted in a 6-point difference and that the difference was much larger for the 2nd and 3rd years before decreasing substantially as the children entered kindergarten. Smoothing curves is so common a procedure that it doesn't deserve comment.

Ramey (1993) now informs us that the difference at 12 years of age is 5.1 points (p. 28). Consequently, we may conclude that the difference at 12 years of age is very similar to the difference at 6 months of age, when there had been about 3 months of intervention (Spitz, 1992, Table 2).

I certainly can be called a nativist, but I am surprised to learn that I have a "nondevelopmental view of life" (Ramey, 1993, p. 28). What does that mean?

Ramey takes issue with my statement that the Abecedarian Project "is no different than innumerable other such projects in which large early differences shrank in later years" (Spitz, 1993, p. 18). This statement was meant to encompass all early intervention projects, not just those that started in infancy. Ramey cites the Milwaukee Project as evidence against my assertion. The Milwaukee Project reported differences at 3 years of age of 32.5 IQ points, which shrank to a difference of almost 10 points at 12 years of age (Garber, 1988, Tables 4-1 and 10-10), a difference that, according to Garber, "only approached significance" (p. 307) based on the Bonferroni-Dunn confidence intervals that Garber used.

Reviews of the many early intervention projects in which large early differences between intervention and control groups shrank in later years have been published by Clarke and Clarke (1989) and Spitz (1986), among others. The Final Report of the Head Start Evaluation, Synthesis and Utilization Project (McKey et al., 1985) includes a meta-analysis of the results of many of the Head Start programs. Based on the results of studies in which intervention and control groups were compared on the Global Cognitive measures immediately after attending a Head Start program (21 studies), a year later (15 studies), 2 years later (12 studies), and 3 or more years later (8 studies), McKey et al. (1985) concluded that there is

> a strong immediate effect of Head Start . . . [but] the initial advantage Head Start children enjoy over their control group counterparts quickly diminishes. (p. III-10)
>
> Once the children enter school there is little difference between Head Start and control children. . . . Findings for individual cognitive measures—intelligence, readiness and achievement—reflect the same trends as the global measure. (p. III-11)

Ramey believes I am an unreliable reviewer and even suggests that perhaps I used the term "a selected history" in the subtitle of my book (Spitz, 1986) because I selected only studies that supported my view of life. However, I used the term "a selected history" because I could not include most foreign language material.

10. My response to Number 10 is contained in my other responses. Concerning the math and reading scores, they were not the subject of my critique.

To summarize, my original critique (Spitz, 1992) questioned the claim in Martin et al. (1990) that the Abecedarian Project provided evidence that "educational day care services to impoverished families may be a feasible strategy aimed at the prevention of mild mental retardation" (p. 847). I did not believe that the Abecedarian Project provided the necessary evidence to make such a claim, and nothing in Ramey's replies has altered my disbelief.

After two responses by Dr. Ramey, we still do not know how many members of the control group were mentally retarded (IQ $<$ 71) at 12 years of age. However, the mean IQ of the entire control group was approaching the average range. It appears that any control group children who were in the mentally retarded range very likely came from the small subgroup that had mentally retarded mothers. We need to know a lot more about them, including their distribution in the cohorts, their means, standard deviations and ranges, and their scores at 3 months and 5 years of age and beyond.

Most importantly, we need to understand why an additional 4.5 years of intensive intervention had so little effect that, at 6 years of age (and older), the difference between the intervention and control groups was not appreciably different than it had been at 6 months of age.

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