Child Development, September/October 2004, Volume 75, Number 5, Pages 1435–1456

Is Home Visiting an Effective Strategy? A Meta-Analytic Review of Home Visiting Programs for Families With Young Children

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Home visiting programs for families with young children have been in effect for many years; however, this is the first comprehensive meta-analytic effort to quantify the usefulness of home visits as a strategy for helping families across a range of outcomes. Sixty home visiting programs contributed data to analysis within 5 child and 5 parent outcome groups. Standardized effect sizes were computed for each end-of-treatment outcome measure, for each treatment versus control contrast. Weighted mean standardized effect sizes ranged from −.043 to .318; 6 of the 10 significantly differed from 0. No one program characteristic consistently affected effect sizes across outcome groups. The extent to which these findings have practical use for the field is discussed.

Home visiting programs have been providing services to families with young children in the United States for many years; the first published documentation dates back to the 1880s (Charity Organization Society, 1883). As of 1999, Gomby, Culross, and Behrman estimated that as many as 550,000 families were enrolled in the six programs they reviewed, and they estimated that thousands of home visiting programs exist in the United States alone. These programs are supported by millions of dollars from both public and private sources. The enormous number of families and financial resources involved justify a comprehensive quantitative evaluation, but only two meta-analyses concentrating solely on child abuse outcomes have been published (Guterman, 1999; Roberts, Kramer, & Suissa, 1996). This meta-analysis addresses a broader question of whether home visiting programs actually help families across a variety of outcomes.

Home visiting programs are linked by their method of service delivery, their goal of helping children by helping the parents of those children, and their focus on younger children. The method of delivering the service or intervention to families in their own homes offers advantages in that parents do not have to arrange transportation, child care, or time off from work. Bringing the intervention into the home also provides opportunity for more whole-family involvement, personalized service, individual attention, and rapport building. These factors may aid families in and of themselves but may also increase program retention rates.

Home visiting programs operate under the belief that parents mediate changes for their children. Most home visiting programs have trained practitioners not to interact directly with children but to encourage and train parents to help their children. Direct help might include coaching parents to help their children with homework, and indirect help might include providing parents with emotional support and job training. Current programs are more likely to involve both parents, although traditionally such services have worked with mothers more than fathers.

Home visiting programs also share a focus on prevention, be it prevention of low-birth-weight babies, child abuse, reliance on public assistance, learning delays, and so on. Problematic behaviors that begin in a child's younger years are difficult, perhaps even impossible, to change or ameliorate later on. Home visiting practitioners believe that it is best to influence the family when the child is young so that good behaviors, and their associated positive outcomes, are evidenced early on, and progress throughout the child's and family's life span. Beyond these similarities, however, there is much variation across programs. *Home visiting* is an umbrella term that implies a strategy for delivering a service, rather

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This meta-analytic review was supported by the David and Lucile Packard Foundation. Coded data were provided by Abt Associates, who completed this work as part of a larger meta-analytic review of family support programs. Their work, funded under Contract 105-94-1925, was conducted for the Administration on Children, Youth, and Families in the U.S. Department of Health and Human Services. A portion of this work was presented at the Home Visiting Workshop (National Academy of Sciences Board on Children, Youth, and Families) in Washington, D.C., March 1999.

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than a type of intervention, per se. Programs differ along many dimensions, including the types of families served (e.g., single, teenage mothers; families of particular ethnicities; socioeconomic backgrounds; or social risk factors), targeted behaviors or outcomes (e.g., child abuse, school readiness, or mothers' employment), type of service delivery staff (e.g., nurses, or mothers from the community), ages of children targeted (e.g., enrolling pregnant mothers, or families with preschool children), length and intensity of services, types of services provided, methods of recruitment, and methods of assigning families to treatment groups. The services provided in the home vary from program to program, and even within program (e.g., teaching home safety and health, training parents how to teach their children to read, and providing empathetic practitioners to lessen parental stress and improve parents' state of mind).

Such variation in home visiting programs precluded the possibility of a single meta-analysis. Instead, separate analyses were conducted for groups of similar outcomes. Given that home visitation programs work with parents to improve children's lives, both parent and child outcomes were analyzed. Child outcomes were separated into cognitive, socioemotional, and child-abuse-related groups, and parent outcomes were separated into parenting behavior, parenting attitudes, and enhancement of life course groups. Program efficacy was measured by weighted mean standardized effect sizes calculated for each outcome group, and the relationship between program characteristics and program success was explored within each outcome group.

Method

Literature Search

The MEDLINE, ERIC, PSYCINFO, Psychological Abstracts, and Social Work Research and Abstracts databases were searched for the literature relating to home visiting programs for young children, and their reference lists and bibliographies were searched for additional relevant work. To reduce the possibility of publication bias and the file drawer problem, published authors and home visiting programs were also contacted directly and asked to contribute relevant unpublished work.

Coding of Research

Abt Associates coded research articles and reports, as part of a larger meta-analysis of family support programs (Layzer, Goodson, Bernstein, & Price, 2001). Articles were coded by two independent coders; coding discrepancies were resolved with a coding director. Standardized effect sizes, adjusted for small sample bias, were computed from statistical information reported in program evaluations (Shadish, Robinson, & Lu, 1997). In some cases this meant means and standard deviations, F tests, t tests, or correlations, while in some cases it meant computing effect sizes from p values and sample sizes.

Inclusion and Exclusion Criteria

Only home visiting programs conducted and reported after 1965 were considered; around 1965, such programs shifted from primarily health- and safety-related endeavors to the more multifaceted, comprehensive programs that remain in existence today. Inclusion was restricted to programs conducted in the United States to allow for more accurate generalizations to U.S. programs. Programs designed solely for developmentally delayed, physically challenged, or chronically ill children were excluded. Programs targeting such special needs children are likely to differ systematically from programs targeting normally developing children and should be evaluated separately. In addition, only programs whose primary service delivery strategy was home visits were included. Programs including home visits as a supplement to another primary type of service mechanism and programs in which home visiting interventions were inextricably combined with interventions delivered through other service strategies were excluded. Only end-of-treatment measures and whole-group comparisons were included. Measures taken during treatment, follow-up data, or data resulting from any type of subgroup analysis within a study were excluded.

Outcome Groups

Outcomes were initially divided into child outcomes and parent or maternal outcomes. Child outcomes were further separated into cognitive, socioemotional, and prevention of child abuse outcomes. Prevention of child abuse was operationalized into three categories for analysis: actual abuse, potential abuse, and parent stress. Actual abuse may have been reported or suspected. Examples of potential abuse included number of emergency room visits, number of injuries or ingestions treated, and number of accidents requiring medical attention. Parent stress was included as an abuse category in that higher levels of stress related to parenting may

result in child abuse. Parent outcome groups spanned two broad categories: enhanced childrearing and enhancement of maternal life course. Enhanced childrearing outcomes included parenting behaviors and parenting attitudes categories. Enhancement of maternal life-course outcomes included mothers' education since the child was born or program inception, mothers' employment, and mothers' reliance on public assistance categories. The set of outcome groups chosen for analysis is not comprehensive. Although it may be possible to analyze additional outcome constructs, the groups chosen for this meta-analytic review are representative of outcomes that home visitation researchers hope to effect most.

Data Hierarchy and Level of Analysis

Sixty of the programs reviewed met the inclusion and exclusion criteria, and provided data on outcomes chosen for analysis. Each program could be subdivided into studies, contrasts, and outcomes. All studies within a program were included in analyses, as long as the data were independent of one another. Within a study, multiple contrasts of treatment and control groups were possible. Consider, for example, one contrast defined as home visitation versus control, and another defined as home visitation plus case management versus case management. All data-independent contrasts that isolated the effects of home visiting were included in analysis. Within a contrast, multiple outcomes were often measured both within and across outcome groups; such contrasts contributed multiple outcomes to analysis. A standardized effect size was computed for every pertinent outcome. When a contrast provided multiple measures within an outcome group, a median effect size was computed for the particular contrast. Medians were used to reduce influence of extreme effect sizes.

The result of these data manipulations was a set of 10 outcome groups, each with its own set of contrasts. Within each outcome group, each contrast was associated with a single, standardized effect size. Each contrast, however, often contributed effect sizes to more than one outcome group.

Results

Program Characteristics

The 60 programs that contributed effect sizes can be summarized in terms of a set of program characteristics. These defining features include primary program goals, populations targeted, program services, child age during intervention, length of program, and home visitor staff type.

Primary Goals

Primary goals were the stated objectives or overall mission of each program. Up to four primary goals were coded for each program. The two most frequently reported primary goals were parent education (96.7%) and child development (85%). Parent education goals included improvement of parenting skills, behavior, and attitudes, and parent–child interaction skills. Child development goals included attempts to improve children's development or well-being in any way. Programs also listed primary goals of: (a) direct provision of health care (30%); (b) parent social support: ways of making parents feel more at ease and providing social resources (28%); (c) preventing child abuse (18.3%); (d) parent self-help: raising parents' self-esteem, sense of competence, empowerment, or leadership, or generally helping parents gain strength in all aspects of their lives (10%); and (e) parent self-sufficiency: job training, education, or literacy training (8.3%).

Populations Targeted

A small percentage of programs (6.7%) universally enrolled families. The majority of programs targeted families at some type of environmental risk (75%). This measure of risk was a more generic measure of potential negative consequences for the child that may be attributable to the environment; factors contributing to environmental risk might include low family income, welfare dependency, abuse or neglect, teenage parent, and maternal depression. Some programs targeted single, specific populations, such as low-income families (55%), families with a low-birth-weight child (15%), families at risk for child abuse or neglect (13.6%), teenage mothers (10.2%), depressed mothers (5.1%), and families dependent on public assistance (3.4%).

Program Services

Programs offered the following services directed toward parents: parenting education (98.3%), parent social support (58.3%), parent counseling (41.7%), parent leadership and advocacy training (15%), and adult basic education (1.7%). Programs also provided information on child development (91.7%), fostered parent–child together activities (58.3%), supplied material goods to families (28.3%), provided

home-based early childhood education (20%), and provided center-based early childhood education (15%). In addition, 38.3% of programs reported providing case management services, and 33.3% provided child health or developmental screening of some sort. Programs provided both referrals to social and health services (68.3% for parent, 50% for child) and direct provision of health care (23.3% to parent, 31.7% to child).

Child Age During Intervention

Only 3.3% of programs in this review were not targeted to a certain child age or age range. Almost 75% of programs began and ended sometime between birth and 3 years of age. Almost one fourth of programs began when children were still in utero (20% prenatal to 3 years, 1.7% prenatal to 5 years), 21.7% of programs targeted a child's first year of life, 30% targeted families with children in their first 3 years of life, 3.3% targeted families with toddlers (18 months to 4 years), 6.7% targeted families with preschoolers (3 to 5 years), and 1.7% targeted families with children in elementary school. Few programs were designed to accommodate a range of starting ages; only 10% enrolled children anytime between birth and 5 years of age, and 1.7% enrolled children anytime between birth and 8 years of age.

Intended Length of Program

Most programs were intended to last for 9 to 12 months (18.3%), 12 to 24 months (30%), or 24 to 36 months (23.3%). Some programs were intended to last for shorter periods (8.3% 0 to 3 months, 8.3% 3 to 6 months). Few programs were intended to last for 3 to 5 years (6.7%), and fewer were unbounded (5%). The intended program length reported here is not the same as the actual average length of home visits. In many cases, it was difficult to extract actual average length of home visits from reported program information.

Home Visiting Staff

Programs listed up to three staff types that worked directly with families in their homes: professionals, paraprofessionals, and nonprofessionals. Most programs (75%) employed professionals, those with formal training and education before their home visiting work. Paraprofessionals, who often came from the same community as those being visited and were often helped by home visiting programs themselves, were employed by 45% of programs. A small number of programs employed nonprofessionals (8.3%) who had formal education but no home visiting training before employment.

Combining Standardized Effect Sizes

Standardized effect sizes, pooled within-study variance estimates (v_i), control and treatment group sample sizes, and weights ($1/v_i$) were calculated for each contrast for each of the 10 outcome groups. Where one contrast provided multiple outcome measures within a particular outcome group, median standardized effect size and median number of participants in control and treatment groups were the unit of analysis. These contrast-level data do not appear in text but are contained in an appendix available by contacting the authors.

Random Effects Model

Support for selection of random effects model. In a distribution of effect size estimates, there are two potential sources of variation. The first, v_i, measures within-study variance, or differences between observed effect size estimates and a population effect size parameter (single δ). The second component, σ^2_δ, measures between-studies variance, or random effects variance. This component measures the degree to which there is variability in population effect size parameters (multiple δ_i). If there is indeed a distribution of effect size parameters (δ_i) with a true population mean (μ_δ), it is expected that the random effects variance component would be greater than zero (Hedges, 1994; Hedges & Olkin, 1985; Raudenbush, 1994). Random effects variance components were estimated using a weighted model; σ^2_δ estimates for each of the 10 outcome groups are reported in Table 1, along with their corresponding statistical significance tests, Q. Random effects variance component estimates ranged from 0.0 to .501; 7 of the 10 were significantly greater than zero. These results supported the use of a random effects model to estimate mean standardized effect sizes for all outcome groups.

Weighted mean standardized effect sizes. Weighted mean standardized effect sizes ($M_{\delta*}$) were computed for each outcome group. Note that in this step of the analysis, weights (w_{i*}) were defined as the inverse of the variance of the estimated effects (v_{i*}). Specific values of v_{i*} were calculated by summing v_i (fixed-effects variance, or within-study variance) and the estimate of $\sigma^2{}_\delta$ (random effects variance, or between-studies variance). Weighted mean standardized effect sizes, the number of programs contributing

Table 1
Estimates of Random Effects Variance and Corresponding Significance Tests

Outcome group	Estimated σ^2_δ	Q
Child development		
Cognitive	.072	257.79***
Socioemotional	.008	62.11
Prevention of child abuse		
Abuse	.501	70.63***
Potential abuse	.049	49.54***
Parenting stress	.086	10.94*
Childrearing		
Parenting behavior	.054	198.76***
Parenting attitudes	.025	76.82***
Maternal life course		
Education	.029	59.23***
Employment/wages	.000	16.45
Public assistance	.010	30.76

*$p \leq .05$. ***$p < .001$.

contrast-level effect sizes, the number of contrasts contributing to mean standardized effect sizes (k), the standard error associated with mean standardized effect sizes ($SE_{M\delta*}$), and the Z tests and p values used to determine whether standardized effect sizes differed from zero are reported in Table 2. Weighted mean standardized effect sizes ranged in size from −.043 to .318 and were significantly greater than zero for cognitive, socioemotional, potential child abuse, parenting behavior, parenting attitudes, and maternal education outcome groups. Families from home visiting programs fared better than did control group families, on average.

Table 2
Random Effects Model: Weighted Mean Standardized Effect Sizes for Child and Parent Outcomes

Outcome group	No. of programs	k	$M_{\delta*}$	$SE_{M\delta*}$	Z
Child development					
Cognitive	41	82	.184	.038	4.79***
Socioemotional	24	49	.096	.028	3.38***
Prevention of child abuse					
Abuse	7	7	.318	.282	1.13
Potential abuse	13	16	.239	.072	3.34***
Parenting stress	4	5	.210	.168	1.25
Childrearing					
Parenting behavior	37	73	.139	.036	3.81***
Parenting attitudes	15	40	.110	.037	2.98**
Maternal life course					
Education	5	27	.134	.044	3.03**
Employment/wages	7	28	.017	.018	0.99
Public assistance	3	23	−.043	.038	1.12

**$p < .01$. ***$p < .001$.

Meta-Analysis Diagnostics

Basic diagnostics were performed to assess potential limitations to generalizations made from this meta-analytic review. These include analysis of the potential for publication bias, solution sensitivity, and solution consistency (Begg, 1994; Greenhouse & Iyengar, 1994). Should the reader wish to conduct more in-depth or further diagnostic analysis, information necessary to do so is presented in Tables 1 and 2 and in the appendix available on request.

The possibility of publication bias was explored by examining funnel plot graphs for each outcome group. Such funnel graphs plot sample size against effect size and, if funnel shaped, provide evidence against publication bias (Begg, 1994). Positively skewed graphs may indicate publication bias; small effect sizes would likely be missing from such plots because they tend to be associated with nonsignificant findings and nonpublication. Several of the plots in this study did show evidence of positive skew, but perhaps not because of publication bias. Nonpublished studies with large sample sizes were included in this analysis; most contributed small, nonsignificant effect sizes. The lack of a lower tail may be due to few reports of significant benefit to control groups over home-visited groups, which would not necessarily result from publication bias, as such results would be significant and of interest.

Solution sensitivity analysis allowed for exploration of how much any one contrast influenced the weighted mean standardized effect size. Solution sensitivity can be addressed by determining differences between (a) the mean effect size computed for the entire set of contrasts and (b) mean effect sizes computed when, one by one, single contrasts are removed from analysis (Greenhouse & Iyengar, 1994). Such comparisons are difficult to interpret when generated from a random effects model, however, because the weights change each time a contrast is removed. Examination of funnel plots, which highlight potential outliers or influential contrasts in terms of effect size, sample size, or a combination of the two, served as a reasonable alternative. Several plots had one or two potential outlier effect sizes, which all had relatively small sample sizes. Several plots also highlighted one or two effect sizes with very large sample sizes; in all cases, the effect sizes associated with such groups were close to zero. Plots evidenced variability in both

sample size and effect size, but there were no obvious combination outliers.

Solution consistency is demonstrated if significance tests for mean effect size estimates are consistent under changing analytic assumptions. Model effect type (fixed vs. random) and type of mean computation (unweighted or weighted) were crossed, resulting in four models. For 8 of the 10 outcome groups, all four models yielded consistent results; for 7 of the outcome groups, all estimates were significantly greater than zero. None of the estimates significantly differed from zero for the parenting stress group. For the child abuse group, both of the fixed-effects mean estimates were significantly greater than zero, whereas the random effects estimates were not. This was due at least partly to the small number of contrasts in this group ($k = 7$). For the maternal employment group, both of the unweighted mean effect size estimates were significantly greater than zero, whereas neither of the weighted mean estimates differed significantly from zero.

Factors Associated With Variability in Effect Sizes

Homogeneity Analysis

Effect size estimates for each outcome group were tested for homogeneity before analyses of the effects of program characteristics on effect size. A significant homogeneity test indicates that contrasts do not share a common population effect size; that is, there are one or more factors systematically varying along with effect size. Homogeneity tests were computed using Q, and tests of whether Q differed from zero, as defined by Shadish and Haddock (1994). Four of the child outcome groups (cognition, abuse, potential abuse, and parenting stress) and three of the parent outcome groups (parenting behavior, parenting attitudes, and maternal education) varied significantly in effect size estimates. Computed Q values and the p values associated with each outcome group can be found in Table 1. The same Q statistic was used to test for both (a) significance of the random effects variance and (b) homogeneity of effect size.

Determination of Adequate Sample Size

Only outcome groups with at least 10 effect size estimates and a significant amount of variability among effect size estimates were analyzed further. Child cognition, potential child abuse, parenting behavior, parenting attitudes, and maternal education groups met these criteria.

Weighted Analyses of Variance (ANOVAs) and Regressions

A series of univariate tests were conducted within each outcome group to determine whether variability in effect sizes could be explained by program characteristics. When program characteristics were continuous in nature, weighted regressions were used, and when program characteristics were categorical in nature, weighted ANOVAs were used. When appropriate, post hoc comparisons were made using Tukey's honestly significant difference (HSD) tests ($\alpha_{fw} = .05$). Program features used in these analyses were not analogous to those reported previously. Characteristics coded at the contrast level were used to explain variability in effect size estimates in the upcoming sections.

Program characteristics were separated into three sets for analysis: program design features, populations targeted, and primary goals. Tables 3 through 6 provide summary results for child cognition outcomes, potential child abuse outcomes, parenting behavior outcomes, and maternal education outcomes, respectively. Tables include the number of contrasts contributing to each analysis; results of statistical significance tests (F); the amount of variance in outcomes accounted for by each univariate model (r^2); and, where F tests were significant, beta weight estimates or results Tukey's HSD tests. Because only one of the univariate analyses for parenting attitudes outcomes was significant, results for this group are described only in text. Targeted populations or primary goals may have been excluded from analysis for one of three reasons: all contrasts targeted the particular population, no contrasts targeted the particular population, or the number of contrasts in each group was too small for analysis.

Program design features. Univariate ANOVAs were performed for: (a) type of intervention, (b) location of intervention, (c) form of family assignment to conditions, and (d) staff type. Weighted univariate regression analyses were performed for: (a) targeted child age, (b) intended length of intervention, (c) actual length of intervention, (d) average number of home visits, and (e) average number of hours of home visits.

Intervention types included single, one-time bounded studies (usually some sort of research demonstration); ongoing single-site interventions; and ongoing multisite interventions. Type of intervention accounted for a significant amount of variability in outcomes for three of the five groups: child cognition, potential child abuse, and parenting

Table 3
Program Characteristics as Potential Influences on Effect Size: Child Cognition Outcomes

Characteristic	No. of contrasts	F	r^2	Significant differences or betas
Design feature				
Intervention type	82	4.05*	0.09	Single site > multisite
Location	78	*ns*		
Form of assignment	82	25.14***	0.39	Quasi > random
Staff type	64	3.04*	0.13	Professional > nonprofessional
Child age	82	*ns*		
Intended length	82	*ns*		
Actual length	47	*ns*		
No. of home visits	55	4.89*	0.08	$b = .004$
No. of hrs. of home visits	51	6.40*	0.12	$b = .006$
Population targeted				
Universal	82	8.01**	0.09	Targeted > universal
Environmental risk	82	*ns*		
Low birth weight	82	8.85**	0.10	Targeted > nontargeted
Teenage mothers	81	*ns*		
Low income	82	*ns*		
Primary goal				
Child development	82	*ns*		
Prevent child abuse	82	*ns*		
Health care	82	4.49*	0.05	Primary > not primary
Maternal self-sufficiency	82	4.16*	0.05	Not primary > primary
Maternal social support	82	*ns*		
Maternal self-help	82	4.06*	0.05	Not primary > primary

*$p \leq .05$. **$p < .01$. ***$p < .001$.

behavior. For child cognition outcomes, ongoing home visiting programs were more successful as single-site ventures ($M = .483$, $SD = 1.83$) than as multisite ventures ($M = .008$, $SD = 2.39$). This was also the case for potential child abuse outcomes (single-site: $M = .547$, $SD = 1.69$; multisite: $M = -.058$, $SD = 1.10$). For parenting behavior outcomes, however, ongoing multisite interventions ($M = .267$, $SD = 1.84$) were more successful than one-time, bounded research demonstrations ($M = .058$, $SD = 1.35$).

Location of intervention was split into four categories: primarily urban, primarily suburban, primarily rural, and a combination of location types. Only the parenting behavior outcome group evidenced a significant association with location of intervention. More specifically, suburban programs ($M = 3.35$, $SD = 2.31$) were more successful than were rural programs ($M = -.069$, $SD = 1.40$).

Form of assignment to conditions included random assignment to groups, quasi-experimental assignments, and contrasts with no comparison group. No-comparison-group contrasts included pre–post designs as well as designs in which treatment groups were compared with norms. Three of the five outcomes were related to form of assignment: child cognition, parenting behavior, and maternal education. For all three of these outcome groups, quasi-experimental studies, on average, yielded significantly higher effect sizes than did studies in which families were randomly assigned. Means and standard deviations for quasi-experimental studies and for randomized studies, respectively, are as follows: child cognition, $M = .365$, $SD = 1.46$ and $M = .126$, $SD = 1.39$; parenting behavior, $M = .308$, $SD = 2.27$ and $M = .056$, $SD = 1.21$; maternal education, $M = .640$, $SD = 1.75$ and $M = .100$, $SD = 1.41$.

Home visiting staff type groups included professionals, nonprofessionals, paraprofessionals, and a mix of staff types. Only ANOVAs for child cognition and potential child abuse outcome groups were significant. For the child cognition group, families visited by professionals ($M = .250$, $SD = 1.47$) fared better than did families visited by nonprofessionals ($M = -.070$, $SD = 2.67$) relative to control group families. For the potential child abuse group, paraprofessional home visitors were associated with higher effect sizes ($M = .577$, $SD = 1.30$) than were either professionals ($M = .132$, $SD = 1.32$) or nonprofessionals ($M = -.085$, $SD = 1.14$).

Table 4
Program Characteristics as Potential Influences on Effect Size: Potential Child Abuse Outcomes

Characteristic	No. of contrasts	F	r^2	Significant differences or betas
Design feature				
Intervention type	16	5.09*	0.44	Single site > multisite
Location	14	*ns*		
Form of assignment	16	*ns*		
Staff type	16	6.34**	0.61	Paraprofessional > professional, nonprofessional
Child age	16	*ns*		
Intended length	16	4.89*	0.26	$b = -.015$
Actual length	4			
No. of home visits	14	*ns*		
No. of hrs. of home visits	12	*ns*		
Population targeted				
Universal	16	4.05†	0.22	Targeted > universal
Environmental risk	16	16.86**	0.55	Targeted > not targeted
Low birth weight	16	*ns*		
Teenage mothers	16	*ns*		
Low income	16	10.32**	0.42	Targeted > not targeted
Primary goal				
Child development	16	*ns*		
Prevent child abuse	16	4.35‡	0.24	Primary > not primary
Health care	16	*ns*		
Maternal self-sufficiency	16	*ns*		
Maternal social support	16	8.24*	0.37	Not primary > primary
Maternal self-help	16	*ns*		

*$p \leq .05$. **$p < .01$. †$p = .063$. ‡$p = .056$.

Targeted child age was operationalized as average child age at the end of treatment because of the overlap and redundancy of the targeted child age categories. Regression analyses failed to achieve significance for all five outcome groups, indicating that outcomes did not vary significantly according to child age.

Both intended and actual lengths of intervention were measured in months. Fewer programs reported contrast-level information about the actual average length of home visiting programs than contrast-level information about the intended length of the intervention. Only potential child abuse outcomes were significantly related to intended length of program. The observed beta weight of −.015 indicated that as programs tended to get longer, effect sizes tended to get smaller. In other words, home-visited families achieved less benefit from programs, relative to control group families, as program length increased. None of the regressions analyses involving actual length of programs was significant; there was no observed relationship between actual program length and program efficacy.

Some research reports included average number of home visits received and average total number of hours of home visits received; note, however, that analyses using these variables have a considerably smaller sample size because this information was not reported as often as other program information. The child cognition group was the only outcome group significantly related to either of these measures. As the number of home visits increased, benefit to treatment group families (relative to control group families) tended to increase ($b = .004$). In addition, as the number of hours of home visits increased, effect sizes also tended to increase ($b = .006$).

Populations targeted. Because population targeted groups were not mutually exclusive, univariate weighted ANOVAs were conducted separately for each population targeted category subgroup. For each subgroup, effect sizes from contrasts involving that particular targeted population were compared with those resulting from contrasts not involving the particular targeted population. For instance, one particular targeted population was families at risk for child abuse. Effect size estimates resulting from contrasts involving families labeled as at risk for child abuse were compared with effect sizes from contrasts in which families at risk for child abuse were not targeted. Analyzed targeted populations

Table 5
Program Characteristics as Potential Influences on Effect Size: Parenting Behavior Outcomes

Characteristic	No. of contrasts	F	r^2	Significant differences
Design feature				
Intervention type	73	3.59*		Multisite > research demonstration
Location	66	3.66*		Suburban > rural
Form of assignment	73	3.47*		Quasi > random
Staff type	54	*ns*		
Child age	71	*ns*		
Intended length	72	*ns*		
Actual length	45	*ns*		
No. of home visits	50	*ns*		
No. of hrs. of home visits	44	*ns*		
Population targeted				
Universal	73	4.91*		
Environmental risk	73	7.79**		Not targeted > targeted
Low birth weight	73	3.84*		Targeted > not targeted
Teenage mothers	72	*ns*		
Low income	73	3.97*		Not targeted > targeted
Primary goal				
Child development	73	*ns*		
Prevent child abuse	73	*ns*		
Health care	73	*ns*		
Maternal self-sufficiency	73	*ns*		
Maternal social support	73	3.54‡		Primary > not primary
Maternal self-help	73	7.02**		Primary > not primary

*$p \leq .05$. **$p < .01$. ‡$p = .064$.

included families with a low-birth-weight child, families with low income, and teenage mothers.

Contrasts targeting families with generic, environmental risk factors did not differ significantly from other contrasts in terms of child cognition, parenting attitudes, and maternal education effect sizes. These studies did reduce incidences of potential child abuse ($M = .355$, $SD = 1.51$) more than did studies not targeting families at environmental risk ($M = -.011$, $SD = .839$). Conversely, parenting behavior effect sizes were significantly lower for studies targeting families at environmental risk ($M = .054$, $SD = 1.51$) than for other studies ($M = .300$, $SD = 1.89$).

For contrasts targeting families with low-birth-weight children, only child cognition and parenting behavior effect sizes differed from those of other contrasts. Contrasts targeting low-birth-weight children had significantly higher child cognition effect sizes ($M = .411$, $SD = 1.43$) than did contrasts not targeting these children ($M = .089$, $SD = 1.73$); these contrasts also had significantly higher parenting behavior effect sizes ($M = .482$, $SD = 1.38$) than did other contrasts ($M = .083$, $SD = 1.64$).

Contrasts targeting teenage mothers did not differ significantly from other contrasts for any outcome group except maternal education. More specifically, studies that targeted teenage mothers had significantly higher maternal education effect sizes ($M = 1.15$, $SD = .847$) than did other studies ($M = .086$, $SD = .978$).

Studies targeting low-income parents were more successful ($M = .354$, $SD = 1.69$) than other studies ($M = .086$, $SD = .978$) in terms of preventing potential child abuse. They were, however, less successful ($M = .055$, $SD = 1.59$) than other studies ($M = .206$, $SD = 1.70$) in terms of enhancing parenting behavior of treatment group families relative to control group families. They did not differ significantly from other studies for child cognition, parenting attitudes, and maternal education outcomes.

Some contrasts involved universally enrolled families; that is, no specific population(s) was targeted, and any family could participate. When possible (when sample sizes were large enough) universal contrasts were compared with contrasts in which at least one particular group was targeted. For child cognitive outcomes, effect sizes were significantly higher for contrasts in which families were targeted ($M = .165$, $SD = 1.50$) than for contrasts in which families were universally enrolled ($M = -.104$, $SD = 3.18$). This was also the case for potential child

Table 6
Program Characteristics as Potential Influences on Effect Size: Maternal Education Outcomes

Characteristic	No. of contrasts	*F*	r^2	Significant differences
Design feature				
Intervention type	27	*ns*		
Location	25	*ns*		
Form of assignment	27	4.30*	0.15	Quasi > random
Staff type	9	*ns*		Small sample size
Child age	27	*ns*		
Intended length	27	*ns*		
Actual length	20	*ns*		
No. of home visits	6	*ns*		Small sample size
No. of hrs. of home visits	6	*ns*		Small sample size
Population targeted				
Universal	27	*ns*		
Environmental risk	27	*ns*		
Low birth weight	27			No targeted contrasts
Teenage mothers	27	37.60***	0.60	Targeted > not targeted
Low income	27	*ns*		
Primary goal				
Child development	27			Only 1 not primary contrast
Prevent child abuse	27	*ns*		
Health care	27	*ns*		
Maternal self-sufficiency	27	*ns*		
Maternal social support	27	*ns*		
Maternal self-help	27	*ns*		

*$p \leq .05$. ***$p < .001$.

abuse outcomes (targeted: $M = .229$, $SD = 1.86$; universal: $M = -.049$, $SD = .964$). However, for parenting behavior outcomes, the effect was reversed: Effect sizes were significantly higher when families were universally enrolled ($M = .292$, $SD = 2.44$) than when families were targeted in some way ($M = .067$, $SD = 1.50$). Universal versus targeted comparisons failed to reach significance for parenting attitudes and maternal education outcomes.

Primary goals. Because primary goals were not mutually exclusive, each goal was analyzed using a separate univariate weighted ANOVA, in which effect sizes from studies that defined the specified goal as primary were compared with effect sizes from studies that did not define the specified goal as primary. Consider the case where the specified primary goal is child development. Effect sizes from studies listing child development as a primary goal were compared with effect sizes from studies where child development was not specified as a primary goal. Analyzed primary goals included child development, child abuse, health care, parent self-sufficiency, parent social support, and parent self-help.

Almost all contrasts defined child development as a primary goal, lending little power to comparisons. Contrasts in which child development was listed as a primary goal did not differ from other contrasts for any of the five outcome groups tested.

Contrasts in which prevention of child abuse was listed as a primary goal did not differ from other contrasts in terms of child cognition, parenting behaviors, parenting attitudes, or maternal education outcomes. These contrasts were associated with significantly higher effect sizes ($M = .516$, $SD = .695$) than were other contrasts ($M = .123$, $SD = 1.75$) when the outcome measure was potential child abuse.

Comparisons between contrasts in which health care was listed as a primary goal and other contrasts were significantly different only for child cognitive outcomes and parenting attitudes outcomes. Child cognitive outcomes were significantly higher ($M = .263$, $SD = 1.83$) for primary goal contrasts than for other contrasts ($M = .085$, $SD = 1.74$), as were parenting attitudes outcomes (primary: $M = .444$, $SD = 2.39$; not primary: $M = .088$, $SD = 1.21$; $p < .05$).

Contrasts in which maternal life enhancement was a primary goal were compared with contrasts in which maternal life enhancement was not listed as a primary goal. Maternal life enhancement was separated into three categories: maternal self-sufficiency, maternal social support, and maternal self-help. For maternal self-sufficiency, primary-goal contrasts had

significantly lower child cognitive effect sizes ($M = .053$, $SD = 1.16$) than did other contrasts ($M = .196$, $SD = 1.94$). For maternal social support, primary-goal contrasts had significantly lower potential child abuse effect sizes ($M = .084$, $SD = 1.43$) than did other contrasts ($M = .445$, $SD = 1.60$) but had significantly higher parenting behavior effect sizes ($M = .199$, $SD = 1.80$) than did other contrasts ($M = .057$, $SD = 1.58$). For maternal self-help, primary goal contrasts also had higher parenting behavior effect sizes ($M = .294$, $SD = 2.00$) than did other contrasts ($M = .057$, $SD = 1.51$) but had lower child cognition effect sizes ($M = -.027$, $SD = 2.98$) than did contrasts not listing this goal as primary ($M = .157$, $SD = 1.50$).

Discussion

Is Home Visiting an Effective Strategy?

To be considered effective, home visiting programs must help both parents, the mediators of child enhancement, and children, the group thought to benefit ultimately from home visits. In general, children in families who were enrolled in home visiting programs fared better than did control group children. Within the set of child outcomes, three of the five average effect sizes were significantly greater than zero. Only child abuse and parent stress as an indicator of potential for child abuse did not yield an average effect size significantly greater than zero. The number of contrasts contributing to each group was more than adequate, and sample sizes for each of the contrasts were of good size. Within the set of parent outcomes, three of the five average effect sizes were significantly greater than zero. Two of these included the more direct measures of parent mediation of child enhancement: parenting behavior and parenting attitudes. The more indirect measures of parent mediation of child improvement, the enhanced maternal life-course outcomes, were not as influenced by home visitation. Mothers in home-visited groups did go back to school or seek out some form of education more than did control group mothers, but did not differ from control group mothers in terms of employment and self-sufficiency, or reliance on public assistance. Note, however, that a small number of programs contributed information to these outcome groups (5, 7, and 3, respectively).

As a first pass, then, this set of findings indicates that home visiting programs actually did help families. Parents received benefit from home visits in terms of their parenting attitudes and behavior—two things that should benefit their children. There is some evidence that home visiting programs encouraged mothers to return to school or to seek out some form of education. This may also benefit children, although the benefit may not be realized until some point in the future. Children also seemed to benefit from home visits by the end of treatment. Cognitive and socioemotional outcomes were higher for home-visited children than for control group children. The actuality and possibility of abuse was lower for home-visited children than for control group children. In terms of statistical significance, then, home visiting programs as a whole did provide a benefit to both parents and children.

Statistical significance, however, does not necessarily indicate practical significance. Whether or not the magnitude of observed effect sizes is meaningful or important remains to be determined. Consider the average effect size for child cognitive outcomes: An average standardized effect size of .184 translates into a difference of only a few points on a standardized intelligence scale, which typically has a standard deviation of at least 10 points. The question remains as to whether an increase of this magnitude is worth the effort, time, and cost required to generate it. Cohen (1988) provided guidelines from which to interpret practical use for size of standardized effect sizes; a small effect size was defined as .20 or lower, a medium effect size was defined as .50, and a large effect size was defined as .80 or higher. Average standardized effect sizes for two of the four significant child outcome groups were less than .20, and all three were lower than .25. Average effect sizes for parent outcomes were even lower—all three of the average effect sizes achieving statistical significance were smaller than .14. By Cohen's standards, all of these effect sizes would fall under the small category. The nature and severity of the outcome deserves consideration as well; an effect size indicating even a fractional reduction in child abuse may have more practical significance than a small effect size relating to an IQ measure.

It is also possible that home visiting programs do have real, practical use for some families, and that these families and their program experiences differ in some systematic way(s) from families who did not benefit from such programs. What if, for instance, 20% of families in a certain home visiting program showed significant and practical improvement? Once averaged in with the rest of the group, this improvement would likely go unnoticed, especially by the time the results of such a study contributed to a meta-analysis. There are pros and cons of aggregating information, and a meta-ana-

lytic review is an aggregate of already aggregated information.

Which Types of Home Visiting Programs Work Best for Which Outcomes?

This question was addressed with a series of univariate analyses relating program characteristics to effect size. Only child cognition, potential abuse, parenting behavior, parenting attitudes, and maternal education outcome groups were analyzed; thus, no conclusions can be drawn about which program characteristics are associated with variation in effect sizes for child socioemotional outcomes, child abuse outcomes, parent stress outcomes, maternal employment outcomes, or maternal reliance on public assistance outcomes.

A caveat before the set of program characteristic analyses are interpreted: Each analysis was performed independently of all others. It is likely, though, that program design features, populations targeted, and primary goals were related, even possibly confounded, with one another to some extent. This muddies interpretation of univariate findings and may even have resulted in alpha levels more liberal than intended. However, given that cell sizes were too small when all possible interactions and relationships were taken into account, the analyses reported here were the most precise possible. The set of analyses cannot be clearly and incisively interpreted. They may, however, suggest some themes to be taken up by future research.

Program Design Features

No clear and consistent pattern emerged across outcome groups. For three of the outcome groups, quasi-experimental designs were associated with larger effect sizes than were random assignment designs, providing some support for the idea that more rigorous programs yield smaller effect sizes than do less methodologically rigorous programs. This support was tempered by the lack of significance for the other two outcome groups tested and by the possibility that programs that differ in terms of how they assign families to conditions may also differ systematically across other dimensions.

For two child outcome groups (child cognition and potential child abuse), effect sizes from ongoing single-site programs were larger than effect sizes from ongoing multisite programs, indicating a potential dilution effect. This finding was not consistent; for the parenting behavior outcomes, multisite contrasts were associated with higher effect sizes than were research demonstrations, which are usually thought to be the most stringent, standardized types of home visiting treatments.

Staff type was inconsistently related to effect sizes across outcome groups. For child cognitive outcomes, professional home visitors were associated with higher effect sizes than were nonprofessional home visitors. No differences were found between performance of professionals and paraprofessionals, even though home visiting programs designed to enhance children's cognitive abilities tend to espouse the paraprofessional as most capable of changing parents' behaviors. In the potential child abuse outcome group, paraprofessionals were associated with higher effect sizes than were professional and nonprofessional home visitors, providing some support for the notion that individuals who were once themselves helped by home visiting programs are better able to help parents in home visiting programs. This support is weakened, however, by the lack of significant findings across the child cognitive, parenting behavior, parenting attitudes, and maternal education outcome groups.

The location of the study was not related to effect size. Location of study was significantly related to effect size only for parenting behaviors outcomes, where higher effect sizes were associated with suburban sites compared with rural sites. Neither child age nor actual length of program was related to effect size for any of the outcome groups. In the one instance where intended program length was a significant predictor of effect size, the negative slope estimate ($b = -.015$) indicated that as the intended length of program tended to increase, effect sizes tended to decrease—just the opposite of what one might expect. The number of home visits and the amount of home visits predicted effect size only for the child cognition outcome group; more specifically, more visits and more hours of visits tended to increase effect sizes. The magnitudes of the slope estimates, however, were very small ($b = .004$ and $b = .006$). This, in conjunction with nonsignificant results in the other four outcome groups tested, indicates that the effect of home visit dosage is weak at best.

Taken together, the results of program design features analyses were inconclusive. No one program feature emerged as a significant influence on effect size across outcomes. Often, when a design feature was related to effect size, the nature of the relationship changed across outcome groups. In most cases, the practical significance of mean differences and slope estimates was negligible. More often than not, design features were not related to effect sizes at all.

Populations Targeted

As a whole, targeted population analyses generated often-contradictory and hard-to-interpret results. As with the group of design features analyses, this group of analyses is inconclusive at best. Studies targeting one or more populations yielded higher effect sizes than did studies in which families were universally enrolled for child cognition and potential child abuse outcomes, but they yielded lower effect sizes for parenting behavior outcomes. Programs targeting families at environmental risk generated higher effect sizes than did those not targeting such families for potential child abuse outcomes, but the opposite pattern was observed in the parenting behaviors outcome group. Conversely, programs targeting low-income families had higher average parenting behavior effect sizes than did those not targeting low-income families, but the opposite was observed for potential child abuse outcomes.

There were a few instances when targeting a certain population of families did result in greater benefit to them. Programs targeting families with low-birth-weight children were more effective than were other programs for both child cognitive and parenting behavior outcomes. The meaning of this is unclear; there appears to be no logical reason why programs targeting families with low-birth-weight children should positively influence child cognitive and parenting behavior outcomes without a corresponding enhancement of other outcomes as well. Maternal education effect sizes were higher for targeted teenage mothers than for other families. Perhaps teenage mothers are more easily persuaded to return to school than are older mothers, or perhaps it takes less effort to return to school when younger. Perhaps younger mothers return to school to earn a high school diploma, and older mothers return to school to earn a college degree, and the former requires less effort than the latter. Although these are plausible explanations, there are many others.

Primary Program Goals

If a program listed a goal as primary, it stands to reason that outcomes directly relating to this goal should have been improved on program completion. This was not always the case. Programs listing prevention of child abuse as a primary goal were associated with less potential for child abuse than were programs not listing this as a primary goal ($p = .056$). Programs listing child development as a primary goal were expected to enhance child cognition and reduce potential child abuse, compared with other programs, but they did not. It is possible that low cell counts and reduced power played a role in the lack of findings. Programs listing maternal self-sufficiency, maternal social support, and maternal self-help as primary goals were expected to enhance maternal education outcomes and possibly even parenting behavior and parenting attitudes outcomes compared with programs not listing these goals as primary. They did not enhance maternal education and parenting attitudes outcomes. For parenting behavior outcomes, maternal social support ($p = .064$) and maternal self-help primary goal contrasts did yield higher effect sizes than did contrasts where these goals were not primary.

In some cases, studies with certain primary goals actually worsened related outcomes. For child outcome groups, maternal self-sufficiency, maternal social support, and maternal self-help primary goal contrasts were often associated with lower effect sizes than were contrasts not listing these goals as primary. It would seem that the opposite should be the case—when mothers' lives become more settled, less stressful, and more in control, children should benefit. At the very least, these children should not fare more poorly than children in programs not listing these goals as primary. It is possible, however, that mothers who concentrate more on themselves concentrate less on their children.

Additional Sources of Variability in Effect Size

At this point the question of which types of programs work best for which types of outcomes has not been definitively answered. Some additional factors should be considered, however, before concluding that program outcomes are independent of program design. Homogeneity tests indicated that there was a significant amount of variability in effect sizes for certain outcome groups. The source of this variability has yet to be explained; it does not appear to stem consistently from any of the program design features, populations targeted, or primary program goals tested in this meta-analytic review. There are, however, some other factors that may have contributed to variability in outcome groups—factors that are not easily measured or accounted for.

Each program likely has a lot of internal noise. This noise, though not easily explained and perhaps even more difficult to measure, may be related to effect size. Consider that two programs employing the same type of home visitor—professional nurses—may have nurses pursue the same goal in a different way once they enter the home. Both prescribe that the home be made a safer environment for

children, but in one, nurses are asked to interact in a much more personable, friendly, and peer-like way than in the other, which prescribes that nurses be more professional and teacher-like. This difference is not easily quantifiable, yet it may result in a difference in observed effect size. Any number of such factors may contribute to variability in effect sizes within a particular outcome group. Consider also that just because a program reports certain goals, features, and services as delivered does not mean that this is actually the case when it comes to individual homes and families. It is likely that adherence to the program-level model is variable at the level of the home visitor. At a nationwide workshop on home visiting (Margie & Phillips, 1999), in fact, program practitioners were clearly aware that this may represent a considerable problem for evaluation. Home visitors themselves often had very different views of the goals of their visits as compared with program mission statements, and home visitors' own views of the program goals strongly influenced their behaviors and actions with families in the home. This type of factor could easily be a source of variation in effect size, though it too cannot be easily measured or accounted for.

The extent to which programs strictly follow a defined model is another potential influence on program efficacy. This type of adherence is at a higher level than home visitors' adherence as described earlier. Programs may be represented by a broad statement of purpose in which many goals are stated, but certain goals are pushed more than others in actual practice. Programs may measure a variety of outcomes although a smaller set is the true focus. This unaccounted-for differential weighting may seriously influence findings. Factors such as program quality, fidelity to treatment model, and equal weights of program goals and measures are not easily measured, or often reported, yet they are likely to be related to effect size.

Implications and Conclusions

This meta-analytic review of home visiting programs does not completely span the field of outcomes available for analysis. Additional outcome groups have yet to be analyzed, as do intermediate and follow-up data and subgroup data. From the work completed in this report, however, some generalizations to the field can be made.

Home visiting does seem to help families with young children, but the extent to which this help is worth the cost of creating and implementing programs has yet to be determined. What exactly makes a home visiting program successful is unclear at this time. It is clear that home visiting programs vary greatly along several dimensions, some of which may not be easily measured or even explained in program reports and evaluations. Some of these potential sources of variation are reasons for home visitation researchers to consider program standardization, both within individual programs and across the field. Standardization would likely enable future meta-analytic efforts to make more definitive statements about what types of programs work best for which types of outcomes. More definitive statements, in turn, might enable home visiting researchers to tailor their programs to meet better the needs of families enrolled.

At this point, the utility of home visiting programs as a whole cannot be clearly stated. This may be due, at least in part, to difficulties in assessing the utility of individual home visiting programs. It is often difficult to both qualify and quantify development and implementation of individual interventions, and this difficulty becomes further confounded when results are collapsed across such studies. The data presented here show that home visiting programs tend to be multifaceted and complex; practitioners attempt to affect positively multiple domains, be it child socioemotional development and safety in the house as well as maternal life enhancement or some other set. In addition, benefits to the family are often thought to arise indirectly from home visiting services, making it even harder to quantify program effects. And finally, home visiting is a strategy for delivering a service and is not a service in and of itself. What happens while a home visitor is in the home is difficult to quantify; there are many intangible factors, such as the personality and attitude of the home visitor, that may influence success but often go unmentioned and unmeasured.

More precise and detailed conceptualization and measurement of both program intervention implementation and service delivery implementation may allow for a more clear understanding of the utility of home visiting programs. This may mean designing programs more specifically with evaluation in mind. From very early on in a program's inception, issues of who is to be most affected, how such families will be affected, and how this effect is to be measured should be addressed, resolved, and clearly reported, allowing for a more precise evaluation of the field as well as the potential for more success at the individual program level. At the same time, it may be possible to start thinking about the efficacy of home visiting programs in a relational sense. Cost–benefit analyses may help outline more clearly the practical

benefit of interventions delivered through home visits, and comparisons between home visiting outcomes and those derived through other service delivery strategies may further help define the usefulness and effectiveness of home visiting as a strategy. In summation, what this meta-analytic review provides is a starting place for practitioners, program developers, evaluators, and funding agencies to begin thinking about the utility of home visiting as a strategy to deliver interventions to families.

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