Final Report

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David Olds, Ph.D. Principal Investigator

This report consists of three separate studies:

1. An economic analysis of the Nurse-Family Partnership (NFP) through child age 9 in the Denver trial of the NFP.
2. A study of children's school functioning employing teacher report of third grade children's grade point averages and behavior in the Denver trial of the NFP.
3. A study of the NFP in neighborhood contexts in the Elmira, Memphis, and Denver trials.

Each of these studies is reported in order below.

Return on Investment:

Nurse and Paraprofessional Home Visitation, Denver

Ted R Miller, David Olds, Michael Knudtson, Dennis Luckey, Jessica Bondy, Amanda Stevenson

The Denver trial of Nurse-Family Partnership home visitation to low-income mothers bearing their first child produced significant positive effects, albeit more modest ones than those achieved in Elmira and Memphis. Fully testing a program's worth, however, requires showing not only that it is effective but that its positive effects are produced at a reasonable price. This chapter describes an economic analysis of the Denver trial through year 9. It considers the societal return on investment in the program and the cost per unit of health and well-being the program produced. It considers the net impact on costs from both societal and government perspectives.

The chapter first describes the program costs. It then describes the program benefit estimation and the return on investment calculation using effectiveness estimates for the nurse-visited mothers. Next it presents the results for both nurse-visited and paraprofessional-visited mothers and explores who pays. A closing section discusses the strengths and limitations of the economic analysis and the implications of the findings.

All dollar values reported here are 2005 dollars. The inflator series used were the Consumer Price Index (CPI) – Medical Care for medical costs; the Employment Cost Index, Total Compensation, Total Private for wage losses; and the CPI-All Items for all other costs. All of these indices are published in the Economic Report of the President.[13] All costs and benefits beyond the year of enrollment have been converted to present values at the time the pregnant mother was enrolled. Present values were computed at the 3% discount rate recommended for base case analysis by the Panel on Cost-Effectiveness in Health and Medicine .[4]

Program Cost

Mean program costs of $10,503 per nurse-visitation client and $7,087 per paraprofessional-visitation client came from the cost data underlying Glazner et al. (2005).[3] Costs for nurse versus paraprofessional home visitation differed both because of the substantially lower salaries of paraprofessionals, differences in maternal retention and visit completion patterns between individual home visitors, and differences in employee turnover between groups.

The costs were computed from detailed accounting records that documented the actual expenditures to implement the Denver home visitation program. In many cases, it was clear that all expenditures in a line item were program-related or research-related. For those line items that could have fallen into either category or that could have been divided between the two categories, the program's accountant and director were interviewed. Based on those interviews, the research-related share of these expenditures was identified and removed from the program cost estimates. The costs include salaries, fringe benefits, supplies, travel, rent, equipment, and training expenses. All costs were converted to 2005 dollars (the time period when the year 9 interviews were conducted). For each participant, the cost estimate represents the present value at the time the pregnant woman was enrolled of program costs for up to 30 months of program participation. Costs more than one year after enrollment were converted to present value using a 3% discount rate.

Program Benefits

As Table 1 shows, the nurse-visitation program had significant effects on eight categories of outcomes. This section describes the methods used to quantify and value the benefits. Table 1 summarizes key components of the estimates. We were conscious that the Denver trial evaluated in three broad domains (pregnancy outcomes, child health and development, and maternal life-course); for each of these domains, we measured a set of hypothesized mediators of program effects on these outcome domains. At the 95% confidence level, one in 20 significant outcomes is likely to be a spurious result of random chance. Therefore, we chose not to value every outcome observed that

was significant or near significant. Instead, we restricted the analysis to outcomes where other indicators buttressed the credibility of the findings or valued the outcomes only in sensitivity analysis. Table 2 summarizes our reasons for choosing program effects to include in the analysis. The benefit-specific subsections that follow provide an inclusion rationale and detail the valuation process.

Pregnancy Outcome Changes Associated with Reduced Maternal Smoking During Pregnancy

Like in Elmira and Memphis, the Denver trial reduced maternal smoking significantly during pregnancy (p=0.05) but not after delivery. In the Denver trial, the mean cotinine reduction among the 18.7% of nurse-visited mothers who smoked at least five cigarettes per day at enrollment was 247 ng/ml, a reduction of 31.3%. Adams and Melvin (1998) estimate the fractions of selected pregnancy outcomes attributable to maternal smoking and the associated costs. Smoking increases five categories of pregnancy risks and reduces a sixth (pre-eclampsia risk). Table 3 summarizes their estimates (inflated to 2005 dollars) and develops the average cost per smoker. We assumed this cost would be reduced proportionately to the reduction in mean cotinine level. Although smoking also is well-known to reduce birth weight and nurse-visited mothers who smoked at intake were less likely to deliver a low birth-weight baby under 2500 grams than control mothers who smoked at intake (p=0.081), mean birthweight was not higher for nurse-visited mothers. Therefore we did not value birthweight impacts of

Table 1. Outcome levels in the control group, outcome changes due to treatment, benefits and costs of nurse home visitation, Denver, through age 9, including sources of the benefit values.

	Control % of Cases	Change in Outcome/Person @ Risk Served	Cost/ Case Prevented	Savings/ Family Served *	Source of Cost per Case
Smoking During Pregnancy					
Complications/Miscarriages	18.7%	31% less cotinine	$217	$13	Adams
Maternal Earnings & Employer-Paid Supplements	N/A	21.6% earnings rise ($2,068 per year, $2,514	N/A	$17,796	SSA, earnings

		with supplements)			Obama, supplements
Domestic Violence, Years 3-9	8.4%/year	37.1% prevented			
Medical		(0.217 fewer	$1140	$214	Miller
Other Resource		cases)	$316	$59	Miller
QALYs			$24,997	$4,701	Miller
Maternal Depression, Years 5-9	9.8%/year	66.3% prevented			
Medical		(0.325 fewer	$1,096/yr	$298	Arnow
Presenteeism			$1,734/yr	$283	Stewart
QALYs			$32,139/yr	$8,755	Mann
Remedial Services, Age 6	.581 services per child	34.9% reduction (0.203 fewer services)	$441	$133	Snell
ADHD, Diagnosed Age 9 (costs through age 18)	5.3%	66.0% prevented (0.035 fewer			
Medical		cases)	$1,400	$340	Swenson
QALYs			$20,152/yr	$4,892	Secnik
Held Back One Grade	6.1%	98.4% increase	$7,315	-$258	School District
TOTAL BENEFITS				$31,944	
PROGRAM COST				$10,503	
Benefit/Cost Ratio				3.05	
Net Cost Savings (excluding QALYs)				($8,036)	

* Future savings and costs were discounted to the year of intervention using a 3% discount rate; ADHD benefits only are included in sensitivity analysis; SSA =Social Security Administration reduced smoking. We also did not estimate quality-adjusted life years (QALYs) saved by reducing medical complications or the enjoyment lost while temporarily curtailing smoking.[1]

Table 2. Reasons to trust these outcomes

[1] A QALY is a measure of health-related quality of life. It is calibrated so perfect health has a value of 1.0, death has a value of 0.0, and values less than 0.0 (fates worse than death) are allowed. QALYs are routinely evaluated in analyzing the outcomes of clinical trials of medical care.

Outcome	Persists	Confidence Level	Other
Smoking	Across trials	greater than 95%	Based on cotinine levels; reductions consistent across trials
Increased Earnings	Over time, across trials	97.0%	Earnings are markedly greater each year except at enrollment
Reduced Domestic Violence, Past 6 Months	Over time, ages 2, 6, 9	95.0%, 69.0%, 87.4% 86.9% at any time between ages 2 & 6	Lack of significant 6 month effect at age 6 is tempered by reduction between ages 2 & 6
Reduced Maternal Depression	Over time	98.9%	Domestic violence victims account for almost half of the depression; preventing one prevents the other
Fewer Remedial Services	Over services	95.3% including evaluation, 97.6% excluding evaluation, 96.9%, 91.3%, 95.2% for individual services	Achenbach total problems score also reduced, 90.7% significance
Less Child ADHD, Age 9		90.7%	Benefits only counted in sensitivity analysis.
More Children Held Back		94.9%	Negative finding, so inclusion is conservative

Table 3. Selected pregnancy outcomes and complications attributable to smoking, their medical cost per case, and the average cost per birth to a smoker (in 2005 dollars)

Condition	Cost/Case	Cases Attributable to Smoking	Cost per Birth to a Smoker
Placenta previa	$16,158	2,200	$46
Abruptio previa	$12,206	5,940	$94
Premature rupture of membranes, preterm	$15,120	202	$4
Pre-eclampsia	$13,604	(9,315)	($165)
Spontaneous Abortion, Med Treated	$ 2,751	43,000	$154
Ectopic Pregnancy	$ 5,070	12,641	$84
Total			$217

Source: Adapted from Adams and Melvin (1998). The attributable case count is based on 767,410 live births to smokers in 1993. If a larger percentage of births involved smokers, the number of attributable cases would rise but the cost per smoker would not change.

Earnings

We obtained Social Security Administration earnings data and converted them to 2005 dollars.[2] After regression adjustment to account for differences among the mothers at intake, the nurse-visited mothers earned an average of $2,068 per year (21.6%) more than the control mothers (p=0.030) in the first eight years after enrollment. As Table 4 and Figure 1 show, earnings were substantially higher with nurse visitation in every year after the child's first birthday. Program years 7 and 8 saw earnings declines because of the recession in 2001-2002.

Table 4. Earnings gain by year, home-visited mothers versus controls by type of home visitor, Denver (in 2005 dollars)

Year	Gain with Nurse Visitation	Gain with Paraprofessional Visitation
1	$481	$921
2	$1,676	$1,026
3	$1,822	$1,568
4	$2,303	$955
5	$2,075	$726
6	$2,850	$838
7	$2,533	$1,348
8	$2,802	$1,763
Total	$16,542	$9,145
Present Value	$14,635	$8,199

Source: Social Security Administration earnings data from Colorado, adjusted for demographic differences and with earnings while residing outside the State inferred. Restricted to families that provided active consent for access to earnings data. Present value computed at a 3% discount rate. Employer-paid supplements to earnings increase the gain by 21.6%.

We augmented the earnings with employer-paid supplements (e.g., insurance premiums and contributions to pension plans, Social Security, unemployment insurance, and Workers' Compensation) at the US average rate from 1994 to 2005 of 21.6% of earnings (Obama 2009, Table

[2] Because the data available for this report had not yet captured earnings for some mothers in years 7-8 post-enrollment, average earnings for those years were computed assuming the percentage of mothers with earnings would match the percentage in year 6. The interview data confirm that employment rates were stable over this time period.

B-28). Conservatively, we did not adjust earnings upward to account for undeclared income from day labor, tips, etc. Except in sensitivity analysis, we also did not estimate the earnings differential beyond eight years even though the annual wage differential was not declining.

The earnings and fringe benefit gains are program benefits from society's viewpoint. Undoubtedly they are associated with an increase in quality of life, but that increase is largely not health-related so we did not try to compute the associated gain in QALYs.

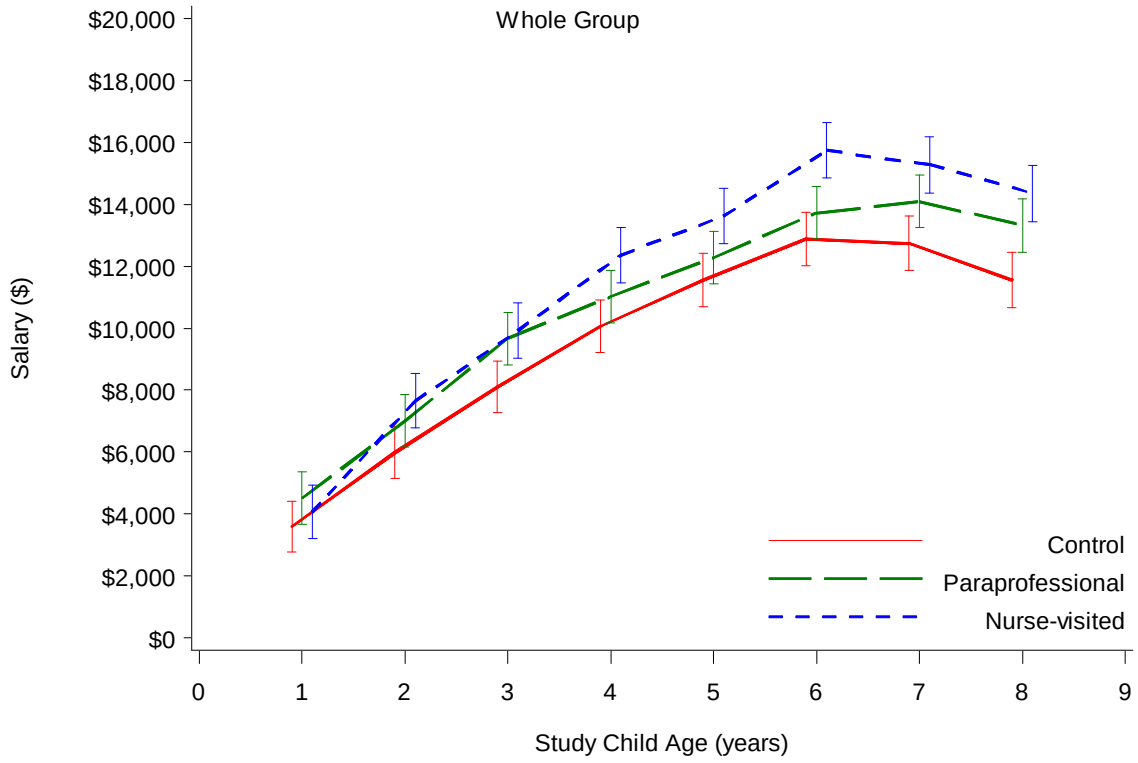

Figure 1. Earnings over time for the nurse-visited, paraprofessional-visited, and control-group mothers

Domestic Violence and Maternal Depression

Findings in the Memphis trial led to addition of a question series about domestic violence in the past six months to the maternal interviews in Denver starting with the age-4 interview. The questions focus on the experience of physical violence. In each six-month recall period, nurse-visited mothers consistently reported less domestic violence than control mothers (p=0.050 for the age-4 interview, p= 0.310 for age 6, p=0.126 for age 9). Across the three time periods, the percentage of nurse-visited mothers reporting domestic violence was 37.1% below the 8.4% rate reported by control mothers.

Over the period from age 3 to age 9, we estimate an average of 0.031 episodes per mother were prevented annually. This estimate represents the decreased number of women experiencing domestic violence during the six month recall period. Conservatively, the costing assumes only one violent incident rather than a series of incidents was avoided. Unit costs for nonfatal domestic violence by cost category were inflated from Miller et al.[11-12] Reduced work loss resulting from domestic violence is accounted for in the overall estimate of differential earnings. Like the Denver data, Miller et al. define domestic violence to include threatened violence and verbal violence. Because the domestic violence reduction was not statistically significant in some time periods, we excluded it in sensitivity analysis.

In the interview at age 6, nurse-visited mothers had strikingly better mental health scores than controls (p=0.002). At age 9, therefore, we included instruments to determine the nature of the mental health improvement. Nurse-visited mothers proved to have far fewer cases of moderate to severe depression (3.3% versus 9.8%, p=0.011). Moreover, as Table 5 shows, almost half of the depressed mothers are in the group of 44 women enmeshed in domestic violence. Thus, it seems likely that the differential depression rate was closely related to the reduction in domestic violence. Since significant mental health differences were detected at the age 6 interview but not the age 4 interview, we assumed the reduction in depression started at age 5. Over the period from age 5 to age 9, maternal depression was reduced by 66.3% from its 9.8% baseline, resulting in an average of 0.325 less depression years per nurse-visited mother.

Table 5. Relationship between domestic violence in the past six months and maternal depression, all mothers in the Denver trial, age 9 interview

	Domestic Violence		
	Yes	No	Total
Depressed	6	7	13
Not Depressed	38	478	516
Total	44	485	529

Medical costs of major adult depression without physical co-morbidities were inflated from Arnow et al.[2] Hours of productivity lost per depressed worker per week came from Stewart et al[18] and were

converted to percentage losses assuming a 40-hour work week. These losses included both absenteeism and presenteeism (meaning the employee is at work but not working productively). Reduced absenteeism and any impact on workforce participation is accounted for in the overall estimate of differential earnings. The percentage of hours affected by presenteeism (11.5%) was multiplied times the average annual earnings plus employer-paid supplements in years 6-8 after enrollment of the nurse-visited mothers (with those who were not employed included in the average).[2] QALY losses were computed from Mann et al[7] as the difference between the mean QALY level with moderate to severe depression and the mean post-treatment QALY level for people who were mildly depressed when diagnosed.[3] The dollar value of a QALY ($86,862) came from Miller and Levy[9] and has been widely used in the peer-reviewed literature.[6,10,14,20]

Attention Deficit/Hyperactivity Disorder

According to the CPT clinical confidence index, which only was administered at age 9, nurse-visited children had significantly lower risks of attention deficit/hyperactivity disorder (ADHD, 5.3% versus 1.8%, p=0.093). Because this finding was marginally significant, we only included the ADHD savings in sensitivity analysis. (For lack of benefit values, we also excluded the potentially overlapping reduction in internalizing problems observed at age 9, p=0.100, a reduction that also occurred in Memphis.) We assumed ADHD costs would extend from age 9 until age 18. Annual medical costs of child ADHD came from Swensen et al.[19] and are based on cost differentials relative to a matched control group in a privately insured population. QALY losses came from Secnik et al.[16] and are based on children in the United Kingdom. (No comparable data were available for the United States.) The dollar value of a QALY again came from Miller and Levy.[9]

Remedial Service Utilization

At age 6, nurse-visited children used significantly less evaluation, medication, and treatment services. They used significantly less services in total (0.378 versus 0.581 services, p= ???) and especially less medication and treatment services (0.183 versus 0.344, p=???). Evaluated by service, they made

[3] The article does not provide QALY levels for people without depression.

significantly less use of cognitive-life skills services, muscle motor skills services, and speech-language services (p = 0.031, 0.047, p=0.083) and insignificantly less use of ADHD and emotional-behavioral services. The consistently lower usage across services increases our confidence in these service usage findings. Furthermore, like in the Memphis trial, the Achenbach total problems score at age 6 showed that the nurse-visited children were having less psychological problems which could warrant service usage (p=0.093). The Denver public school system budgets $256 per low income child for these services.[17] We assumed the cost savings would be proportional to the change in service usage including evaluation services. We did not estimate any associated QALY savings.

Grade Retention

By age 9, 11.8% of nurse-visited children compared to 6.1% of control children had been held back a grade (p=0.051). Following Karoly and Bigelow,[5] we valued this year as the average cost of a pupil year in the Denver school system, $7,315 (School District No. One: City and County of Denver, CO 2005). The extra year of school attendance is assumed to occur at age 19, the year after grade 12 normally would be completed. We were unable to estimate the QALYs lost due to retention in grades 1-4.

Para-Professional Visited Mothers

We applied the same benefits valuation model to the outcomes for the paraprofessional-visited mothers, ignoring the significance of the individual effects since all are hypothesized to result from this program. Table 6 summarizes the effectiveness estimates, their significance, and the associated cost impacts for paraprofessional-visited mothers. Although often not significant, all the effects are correctly signed and might have proven significant if the treatment and control groups were larger.

Sensitivity Analysis

Sensitivity analysis examined how return on investment varied when selected assumptions were changed.

RESULTS: Return on Investment

Nurse-Visited Mothers. As Table 1 shows, the present value of the estimated total savings per mother from nurse visiting averaged $31,994. With program cost averaging $10,503 per participant, the estimated benefit-cost ratio was 3.05. Thus, the program returned $3.05 per dollar invested. Each participating mother-child dyad gained an average of 0.15 quality-adjusted life years (QALYs). The program offered net cost savings. Its benefits net of the value of the QALYs saved and of associated health-related earnings gains exceeded its costs by an estimated $8,036 per mother assisted.

In sensitivity analysis, we varied a few key parameters. We added the savings from reduced ADHD. The benefit-cost ratio rose to 3.54 and the QALY savings per dyad rose to 0.21 . Dropping the savings from domestic violence reduced the benefit-cost ratio to 2.16. The benefit-cost ratio was 3.40 with no discounting, 3.16 at a 2% discount rate, and 2.94 at a 4% discount rate. Assuming annual earnings differentials in years 9-13 equal the average from years 6-8 would raise the benefit-cost ratio to 4.01. The program offered net cost savings in all cases.

Paraprofessional-Visited Mothers. Table 6 shows the effectiveness estimates, their significance, and the benefit and return on investment estimates for the paraprofessional-visited cohort. the present value of the estimated total savings per mother from paraprofessional visiting averaged $16,514. With program cost averaging $7,087 per participant, the estimated benefit-cost ratio was 2.33. Each participating mother-child dyad gained an average of 0.07 quality-adjusted life years (QALYs). The program offered net cost savings.

In sensitivity analysis, adding the savings from reduced ADHD raises the benefit-cost ratio to 2.86. Dropping the savings from domestic violence reduces the ratio to 1.90. The benefit-cost ratio was 2.57 with no discounting, 2.41 at a 2% discount rate, and 2.26 at a 4% discount rate. Assuming annual earnings differentials in years 9-13 equal the average from years 6-8 would raise the benefit-cost ratio to 3.02. Because the earnings gain alone exceeded program costs, the program offered net cost savings in all cases.

Who Pays

Tables 7 summarizes the benefits for the nurse-visited and paraprofessional-visited mothers by cost category and breaks them down among payers. We assumed a marginal income tax rate of 10% for these families, some of them below the earned income tax credit threshold. Roughly 92% of the benefits went to the families served in the form of higher incomes and improved health. Government savings offset 16.7% of programs costs in the nurse-visited cohort and 15.0% in the paraprofessional-visited cohort. With adult Medicaid eligibility constricted, only 13.6% of the control mothers were on Medicaid by the age-4 interview. Thus many of their medical costs were covered by private insurance, charity care, or self-pay rather than by government, which lessened government gains from earnings increases.

Table 6. Outcome levels in the control group, outcome changes due to treatment and their significance, benefits and costs of paraprofessional home visitation, Denver, through age 9.

	Control % of Cases	Change in Outcome/Person @ Risk Served	Probability Occurred by Chance	Savings/ Family Served *
Smoking During Pregnancy			less than 0.05	
Complications/Miscarriages	18.7%	9.6% less cotinine	N/A	$4
Maternal Earnings & Employer-Paid Supplements	N/A	12.0% earnings rise ($1,143 per year, $1,390 with supplements)	0.223	$9,970
Domestic Violence, Years 3-9	8.4%/year	26.2% prevented	0.88,0.072,0.181	
Medical		(0.154 fewer		$151
Other Resource		cases)		$42
QALYs				$3,317
Maternal Depression, Years 5-9	9.8%/year	21.4% prevented	0.463	
Medical		(0.105 fewer		$96
Presenteeism		cases)		$92
QALYs				$2,829

Remedial Services, Age 6	.581 services per child	35.1% reduction (0.204 fewer services)	0.081, 0.063 with & without evaluation	$133
ADHD, Diagnosed Age 9 (costs through age 18) *	5.3%	47.2% prevented (0.025 fewer	0.234	
Medical		cases)		$243
QALYs				$3.494
Held Back One Grade	6.1%	45.9% increase	0.290	-$120
TOTAL BENEFITS				$16,514
PROGRAM COST				$7,087
Benefit/Cost Ratio				2.33
Cost per QALY Saved				less than $0

* Future savings and costs were discounted to the year of intervention using a 3% discount rate;

ADHD benefits only are included in sensitivity analysis; N/A = not applicable.

Table 7. Who Benefits by Category of Benefits and Treatment Group

	Government	Family	Other	Total
Nurse-Visited				
Medical	$83		$442 *	$525
School	($125)			($125)
Other Direct	$21	$38		$59
Employer Productivity			$283	$283
Wages & Fringes	$1,780	$16,016		$17,796
Quality of Life		$13,456		$13,456
TOTAL	$1,759	$29,510	$725	$31,994
Paraprofessional-Visited				
Medical	$38		$213 *	$251
School	$13			$13
Other Direct	$15	$27		$42
Employer Productivity			$92	$92
Wages & Fringes	$997	$8,963		$9,970
Quality of Life		$6,146		$6,146
TOTAL	$1,063	$15,146	$305	$16,514

* Mix of employer savings, family savings, and reduced public or private charity care.

Strengths and Limitations

This study relied on benefit values from the literature. Implicitly, therefore, our estimates incorporate all the limitations of the valuation studies we used. Above we noted limitations associated with small

mismatches between the problem descriptions in the Denver NFP data and in the benefit valuation literature. Larger samples in the Denver trial also might have increased the significance and refined the accuracy of observed differences.

None of these limitations is of great significance. For the nurse-visited mothers, sensitivity analysis shows that the program almost certainly yielded a positive return on investment. Indeed, the increase in maternal earnings alone exceeded the cost of the program, meaning it was an effective anti-poverty program. Similarly, the maternal and child health benefits of the program alone exceeded the program's costs, meaning it was an effective health intervention. The paraprofessional program delivered fewer benefits and its outcomes generally did not differ significantly from outcomes in the control group. If one adopts the common rule-of thumb that benefits would decline by 25% in replication,[8] our best estimate of the benefit-cost ratio for the paraprofessional program would be around 1.75. Moreover, a larger trial would be required to assure the observed benefits were not chance results. Therefore, we recommend against adopting the paraprofessional program at this time.

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TEACHER DATA FOR THE DENVER YEAR-9 FOLLOW-UP

John Holmberg, Dennis Luckey, and David Olds

With support of this grant, we augmented the data collection procedures of the Denver trial of the NFP. We obtained third-grade teachers' ratings of children's behavior and teachers reports of children's grade point averages in reading and math in third grade. The Denver trial (N=735) consisted of a three-arm study, with families randomly assigned to one of three conditions: 1) control (regular screening and referral of children for health and developmental problems); 2) paraprofessional home visitation in which paraprofessionals followed the essential elements of the NFP program; and 3) NFP nurse visitation.

Hypotheses

This work was guided by the following hypothesis:

The programs of nurse and paraprofessional home-visiting will produce enduring effects on:

a) Children's early-onset behavior problems (both externalizing and internalizing)

b) Children's executive functioning and school achievement

The effects of the program on child outcomes will be greater for children born to mothers with low psychological resources and for males with respect to externalizing problems.

Data Collection

We conducted follow-up evaluations with interviews and direct tests of the children on 575 families. The current grant enabled us to obtain data from teachers on their ratings of children's behavior and their abstraction of children's grade point averages. School data collection packets were sent to teachers on 498 of those children (87%). The remaining 13% of children could not be followed up due to one of the following conditions: parent refusal, school refusal, rapid subsequent change of school for the children without a forwarding school available, or home schooling. Of the 498

packets sent out, 473 (95% of packets sent out; 82% of families seen; 64% of families randomized into the study) were returned. About 466 of these were usable.

Teachers completed the following: 1) the Teacher Rating Form from Achenbach's Child Behavior Checklist (CBCL-TRF, 2001 version), 2) the Antisocial Process Screening Devise (ASPD) a checklist by Frick & Hare (2001), and 3) reports of the children's grade point averages in reading and math.

The low rate of teacher report data makes these data subject to attrition bias to a greater degree than the interview or child testing data. We show here the findings based upon the analysis of both teacher-report and parent-report data, but are not likely to publish data from either source as we have concerns about their validity. We are especially concerned about the large portion of missing data from teachers. Fortunately, we have direct testing data on the children that provide more valid measurements of the achievement, behavior, and executive functioning outcome domains addressed by this study. We show the data from teacher and parent reports here for the sake of study completion.

Analyses

We analyzed parent and teacher-report data in the primary statistical models used test the program in the Denver trial. We examined a three-level treatment factor, and examined outcomes separately for children whose mothers had high and low psychological resources at baseline during pregnancy. The covariates used in examining continuous child outcomes at age 9 were consistent with those used at child age 4 and 6: maternal psychological resources, housing density, age of mother at intake, randomization into the study after the 28th week of pregnancy (yes/no), frequency of conflict between the mother and her mother at enrollment, and child gender. For dichotomous outcomes, we employed logistic regression and only included psychological resources and child gender as covariates.

Results

In general, as seen in Table 3, there were no statistically significant program effects (for either nurses or paraprofessionals) on teacher report of child behavior, parent report of child behavior, or grade point averages.

Following recommendations by Kramer (2003) and others, we analyzed the child behavior checklist data by cross classifying the extent to which raters (mothers/guardians and teachers) agreed that the children fell into the clinical range on three super-ordinate scales: internalizing, externalizing, and total problems.[3] When there is agreement between raters regarding children being in the borderline or clinical range, there is stronger evidence of psychopathology, due in part to the persistence of maladaptive behavior across contexts. There was a reduction, as a trend, in the extent to which nurse-visited 9-year-old children, compared to controls, were rated as have internalizing disorders (3.6 % vs. 8.2%, p=.10, IR =0.41). This is consistent with recent results from the Memphis trial of the Nurse Family Partnership.

It is important to note that although there are no statistically significant program effects on children's grade point averages in reading and math, the results based upon parent report are approximately equal in magnitude to those found in the Memphis trial of the NFP over the same time period based upon school records. This is not corroborated by the school records data. The school record data have many more missing cases. The difference in the pattern of results may be due to differences in accuracy of parental report or missing data.

Table 3. Children's School Outcomes

		Treatment Group						Treatment Comparisons			
		Control		Paraprofessional		Nurse		Control vs. Paraprofessional		Control vs. Nurse	
		LS Mean	(SE)	LS Mean	(SE)	LS Mean	(SE)	p-value	Effect Size	p-value	Effect Size
Current Grade Placement (n=461)	Whole	4.33	0.60	4.45	0.68	4.35	0.70	0.13	0.16	0.57	0.06
	Low	4.24	0.61	4.37	0.76	4.37	0.61	0.29	0.29	0.40	0.19
Parent Report: Reading Grades (n=566)	Whole	3.18	0.05	3.12	0.05	3.14	0.06	0.47	-0.07	0.62	-0.05
	Low	2.93	0.09	3.10	0.08	3.10	0.09	0.17	0.23	0.19	0.23
School Report: Reading Grades (n=445)	Whole	2.49	0.96	2.32	0.99	2.47	1.09	0.14	-0.16	0.80	-0.03
	Low	2.44	0.94	2.15	0.98	2.30	1.14	0.14	-0.26	0.51	-0.12
Parent Report: Math Grades (n=565)	Whole	3.21	0.05	3.11	0.05	3.14	0.05	0.16	-0.15	0.32	-0.11
	Low	2.96	0.09	3.09	0.07	3.07	0.09	0.27	0.21	0.38	0.18
School Report: Math Grades (n=442)	Whole	2.68	0.93	2.51	0.93	2.54	1.13	0.15	-0.15	0.24	-0.12
	Low	2.49	0.86	2.42	0.94	2.35	1.24	0.73	-0.05	0.52	-0.11
Hours per week learning support services (n=446)	Whole	1.64	4.23	1.22	4.10	1.53	4.86	0.40	-0.09	0.83	-0.02
	Low	1.99	3.96	2.08	5.47	2.25	6.58	0.92	0.02	0.81	0.04
Times Sent to Principal's Office (n=468)	Whole	1.30	0.83	1.44	0.99	1.32	0.85	0.18	0.15	0.85	0.02
	Low	1.30	1.07	1.45	0.99	1.42	1.05	0.40	0.15	0.53	0.12
		%		%		%		p-value	Odds Ratio	p-value	Odds Ratio
Dual Rater CBCL Internalizing (Clinical) (n=455)	Whole	8.2		7.8		3.6		0.90	0.95	0.10	0.41
	Low	13.5		9.8		6.6		0.54	0.70	0.27	0.45
Dual Rater CBCL Externalizing (Clinical) (n=458)	Whole	10.2		13.7		6.6		0.33	1.40	0.26	0.62
	Low	14.1		14.4		6.4		0.96	1.03	0.23	0.42
Dual Rater CBCL Total (Clinical) (n=455)	Whole	9.6		9.4		7.3		0.94	0.97	0.47	0.74
	Low	10.2		12.4		8.6		0.71	1.25	0.80	0.84
ASPD Total Tscore (borderline/clinical) (n=469)	Whole	2.0		3.6		1.3		0.37	1.78	0.59	0.62
	Low	0.3		1.9		2.5		0.17	6.42	0.17	7.33
Special Education school report (n=445)	Whole	15.6		15.7		12.3		0.98	1.01	0.42	0.76
	Low	21.7		17.7		20.0		0.59	0.78	0.80	0.88
	Low	2.6		7.2		11.5		0.36	2.88	0.16	4.88

Effect Size = Least Square mean difference divided by the pooled standard deviation of the outcome.
§Variable standardized to mean of 100 and SD of 10.

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Neighborhood Context and the Nurse-Family Partnership

Carole Hanks, Dennis Luckey, Michael Knudtson, Harriet Kitzman, Elizabeth Anson, Kimberly Arcoleo, and David Olds, PhD

Overview

Neighborhood disadvantage is consistently related to poor maternal and child health and violence.[1] Some research has shown that the impact of neighborhood disadvantage on youth violent victimization is attenuated among youth living in two-parent households.[2] This observation led to the examination of three inter-related questions regarding the impact of a program of prenatal and infancy home visiting by nurses known as the Nurse-Family Partnership (NFP) on maternal and child health. The NFP has been tested in a series of randomized controlled trials over the past 30 years. The questions are:

1. **Did nurse-visited families move to less disadvantaged neighborhoods over time compared to their control-group counterparts?**

2. **If nurse-visited women moved to better neighborhoods, to what extent did their living in better neighborhoods account for their improvements in maternal and child health compared to control-group women and children?**

3. **To what extent did the NFP program attenuate the risk for poor maternal and child health associated with concentrated neighborhood social disadvantage?**

We geo-coded (obtained US Bureau of Census tract and block group numbers) the addresses of participants in the NFP trials in Elmira, NY (N=400), Memphis, TN (N=743), and Denver, CO (N=735). We created an index of neighborhood disadvantage composed of the average z-scores of the following block group (or Census tract) characteristics: 1) Percentage of households

below the federal poverty level; 2) Percentage of persons on public assistance; 3) Percentage of female-headed households with children; 4) Percentage of persons unemployed; and 5) Percentage of the population that is black. This index represents the degree to which there is concentrated social disadvantage within block groups and

Census tracts. At registration and at every subsequent assessment period conducted thereafter, we assigned a single z-score value to each address. Overall, families in the Memphis trial lived in neighborhoods characterized by high levels of disadvantaged and segregation.

We found that there were no coherent nurse-control differences in changes in neighborhood disadvantage over time. This meant that changes in neighborhood disadvantage could not play a role in explaining the positive effects of the program found in earlier reports on the trials.

We found some indication that previously reported NFP program effects were more pronounced among families who lived in more disadvantaged neighborhoods at registration; these effects tended to be moderated further by family risk and maternal psychological resources. We found, for example, that the previously reported program effect on state-verified cases of child abuse and neglect among high-risk families (unmarried mothers from low-SES households) were more pronounced among those who lived in more disadvantaged neighborhoods at registration. We also found that previously reported program effects on government expenditures for welfare-related expenses (food stamps, Medicaid, and AFDF/TANF) among women with higher psychological resources in the Memphis trial were more pronounced among those who lived in the most disadvantaged neighborhoods at registration. And finally, we found that more program effects in the Denver trial on children's language and intellectual functioning for those born to mothers with low psychological resources were most pronounced among those families living in the most disadvantaged neighborhoods at registration.

There were many outcomes for which there was no indication that the NFP moderated neighborhood disadvantage, yet some findings reported here support the policy contained in both the House and Senate versions of healthcare reform currently in reconciliation, which call for focusing evidence-based home visiting programs on communities with the highest concentrations of poverty and disadvantage.

Introduction

The physical and social aspects of neighborhoods are consistently associated with a wide range of adverse health outcomes, including low birth weight, childhood injury, child abuse and neglect, teen parenthood, educational failure, mental disorders, delinquency, crime, and mortality.[1] These relationships hold even after statistical controls are introduced to account for the fact that individuals at greater personal risk tend to live in places where there is concentrated social disadvantage. While the mechanisms that account for these relationships are not well understood, there is increasing acceptance that high concentrations of neighborhood social disadvantage set in motion processes that compromise the health and development of children and increase the rates of crime, disease, and death.

A recent national study of youth victimization and neighborhood adversity found that the relationship between neighborhood adversity and youth victimization is greater for children living in single-parent households than for those living with two parents. Having two parents buffers children from becoming victims of crime in disadvantaged neighborhoods.[2]

The current study built upon this observation to determine whether a program of prenatal and infancy home-visiting by nurses might buffer mothers and their first-born children from the adverse effects of concentrated neighborhood disadvantage. This program, known as the Nurse-Family Partnership (NFP), has been tested in a series of randomized controlled trials over the past 30 years, with different populations of low-income mothers and their families, living in urban and rural settings, and has been identified by the Coalition for Evidence Based Policy as the only early childhood program that meets its "Top Tier" of evidence.[3] Both the Senate and House versions of healthcare reform currently in reconciliation in Congress include provisions for expanding federal support for evidence-based home visiting programs for low-income pregnant women and parents of young children. The current study is designed to help determine whether scarce public resources might be targeted more effectively by focusing this program on our most disadvantaged neighborhoods.

In trials conducted to date, the NFP has produced consistent effects on prenatal health, child health and development, and maternal life-course (pregnancy planning and economic self-

sufficiency). In the first trial conducted in Elmira, New York, the program produced enduring effects on state-verified rates of child abuse and neglect and youth arrests through child age 15.[4,5] Across all trials, the beneficial effects of the program have endured for years after the program ended at child age two. Many of the benefits of the program were greater where there was greater family risk (e.g., where mothers were unmarried and from low-income families at registration). Program benefits for children tend to be greater for those born to mothers who had limited psychological resources (intellectual functioning, mental health, and sense of control over their lives) to manage the care of themselves and their children while living in poverty.[6-11]

Questions Addressed by the Current Study

The current study consisted of a secondary analysis of data from the NFP trials to examine three questions:

1. **Did nurse-visited families live in less disadvantaged neighborhoods over time compared to their control-group counterparts?** Given that nurse-visited mothers had improved economic self-sufficiency, there is reason to believe that their improved economic circumstances would enable them to move to better neighborhoods over time.

2. **If nurse-visited women moved to better neighborhoods, to what extent did their living in better neighborhoods account for their improvements in maternal and child health compared to control-group women and children?** Moving to neighborhoods with fewer negative influences on maternal and child functioning may play a role in accounting for the superior functioning observed for nurse-visited mothers and children compared to controls.

3. **To what extent did the NFP program attenuate the risk for poor maternal and child health associated with concentrated neighborhood social disadvantage?** Given that the program is designed to activate parents' inherent drives to protect themselves and their children, and it improved child health and development, it is possible that nurse-visited mothers and children were buffered from the adverse effects of neighborhood disadvantage.

Background

Since 1977, we have conducted three randomized controlled trials of the Nurse-Family Partnership (NFP), a program of prenatal and infancy home visiting by nurses for low-income mothers bearing first children: first, in Elmira, New York with a sample (N=400) of primarily white families living in a semi-rural community, in which families were followed through the first child's 15th birthday; second, in Memphis, Tennessee with a sample (n=743) of primarily African-American families followed through child age 12; and third, in Denver, Colorado with a sample (N=735) that includes a large portion of Hispanics (46%) through child age 9. The Denver trial included a treatment arm in which the NFP program was delivered by paraprofessional visitors rather than nurses. We have found that the program delivered by nurses improved the outcomes of pregnancy, children's health and development, and parents' life-course and economic self-sufficiency. Across these trials, program effects on child health and development were greater for children born into households at greater socio-economic risk and for those born to mothers who had fewer psychological resources to manage the challenges of living in poverty and caring well for their children.

The study reported here employs a well established index of concentrated disadvantage measured at the level of neighborhoods that is consistently associated with violent crime.[1,2] The variation in crime associated with concentrated disadvantage is explained at least in part by variation in residents' "collective efficacy," that is their ability to exert informal social control over deviance in their neighborhoods.[1] The negative impact of concentrated disadvantage on youth violent victimization is greater for children residing with two parents than for those residing with one.[2] The current study examines the extent to which the NFP can reduce the adverse impacts of concentrated neighborhood disadvantage on maternal and child health, and whether previously reported program effects on maternal and child outcomes is explained by nurse-visited mothers' moving over time to safer neighborhoods.

Given the large number of places families in the NFP trials resided, we used block group numbers derived from participants' addresses and US Census Bureau data to designate participants' neighborhoods. This allowed us to employ a consistent index of neighborhood concentrated disadvantage across hundreds of neighborhoods (block groups) over time. We assigned block-group numbers to each address and used Census Bureau data from those blocks to characterize the degree of concentrated disadvantage within those blocks, by standardizing elements of the index to national means.

In order to create a common measure of neighborhood disadvantage that could be used across trials, contexts, and time, we relied on a single, well established index of neighborhood adversity that has been used to predict youth crime and victimization.[2] As noted below, we geo-coded the census tract and block groups of the addresses that women provided for their residence at registration, at child age 2 and at child age 4 for all studies, at child ages 6 and 9 for the Memphis and Denver samples, at child age 12 for the Memphis sample, and at child age 15 for Elmira. (There were no follow-ups for the Elmira sample between child age 4 and 15.) These codes were used to derive from the census data an index of social disadvantage-tract data to create social and economic profiles of the neighborhoods (aggregated into an index of concentrated social disadvantage) in which the families lived over time.

We derived the summary index to characterize the social and economic characteristics of these neighborhoods and used it as a dependent, mediator, and moderator variable in re-analyses of maternal and child outcomes in the three trials. Specifically, we used this summary index 1) to determine the extent to which the neighborhood characteristics of nurse-visited women, in contrast to their counterparts in the control group, improved over time; 2) to estimate the extent to which the effects of the program are greatest in families who at registration lived in the most distressed neighborhoods, in contrast to control-group families who lived in neighborhoods at comparable levels of distress; and 3) to examine the extent to which these improvements in neighborhood contexts played a role in accounting for long-term beneficial effects of the program on children's behavioral and academic adaptation.

One of the more salient patterns of results found in our trials to date is that the effect of the program on child-related outcomes (such as abuse and neglect, injuries, mental health, cognitive and language functioning) was concentrated on children born to mothers at higher socio-demographic risk and with few psychological resources.[6,8-10] We believe that the moderating influence of maternal risk can be explained primarily by mothers' improved skills in coping with the demands of care-giving and of poverty.

Mothers at highest risk from the standpoint of socio-demographic characteristics (e.g., poor, unmarried, teens) and those with the fewest psychological resources (limited sense of mastery, mental health, and intellectual functioning), however, are likely to live in the most disadvantaged neighborhoods at registration.[1] We hypothesized that part of the conditional effect of maternal risk observed in the trials to date was likely to be explained by differences in the nurse-visited and control-group mothers' ability to cope with the heightened challenges of parenting and living in such high-risk social environments.

Moreover, there was anecdotal information that some nurse-visited women moved to safer housing and neighborhoods as a result of their participation in the program. Both of these processes were hypothesized to lead to greater program impact among families who lived in the most disadvantaged neighborhoods at registration. The influence of neighborhoods is likely to become especially salient as children grow older and academic and behavioral functioning become

more appropriate indications of children's functioning. As children mature and spend more time outside the home, they are exposed to a larger number of neighborhood influences, such as peers. School environments, which may be associated with neighborhood characteristics, also are likely to affect academic and behavioral adaptation. Nevertheless, competent parenting and better family functioning ought to buffer the effects of negative neighborhood and school influences even if families are unable to move to safer and less disadvantaged neighborhoods. The impact of the program on child outcomes is thus likely to be greatest in those families who lived in the most disadvantaged neighborhoods at registration and in which the mothers were at greatest personal risk by virtue of their having the lowest levels of psychological resources. In the absence of help from the nurse, the combined effect of adverse neighborhood influences and limited maternal coping ought to further increase the likelihood that children will suffer academically and behaviorally. The impact of the program on child outcomes thus ought to be greatest in those families at highest risk by virtue of limited psychological resources on the part of mothers and their living in the most disadvantaged neighborhoods.

It should be noted that there is evidence from the Elmira and Memphis trials that the immediate impact of the program on maternal life-course outcomes is greater for mothers with higher psychological resources.[6,9] In the Elmira study, *older* (> 18 years at registration) low-income, unmarried mothers were more likely to return to school more rapidly after delivery, probably because they were able to obtain work by virtue of their older age.[5] In the Memphis study, the impact of the program on maternal life-course outcomes (delay in subsequent pregnancy, increase in employment, reducing use of welfare) was greater for mothers with higher psychological resources, compared to their counterparts in the control group.[10-11] The impact of the program on these types of outcomes in the Memphis trial was not statistically significant for mothers with low psychological resources. We have interpreted this as a reflection of the high-resource mothers being able to realistically imagine their obtaining employment compared to low-resource mothers, who are likely to have a more difficult time believing that they could effectively manage being both good parents and employees at the same time. Gaining insight into how these conditional effects are moderated by neighborhood risk will be a focus of this study.

Study Hypotheses

The current research was designed to test three hypotheses within each of the three trials of the NFP:

1. **Nurse-visited women, in contrast to their counterparts in the control group, will have lived in less disadvantaged neighborhoods over time.**

2. **The impact of the nurse-visitor program on childrearing, child health and development, and maternal life-course will be greatest among those families who, compared to their counterparts in the control group, lived in the most economically and socially disadvantaged neighborhoods when they registered in the program at registration during pregnancy.**

3. **The improvements in neighborhood context among nurse-visited families will help account for the long-term beneficial effect of the program on child functioning.**

DESIGN AND METHODS

Overview

We examined the neighborhood disadvantage hypotheses for participants enrolled in each of the three trials of the Nurse Family Partnership conducted in Elmira, NY (N=400); Memphis TN (N=1139 for prenatal phase of trial and N=743 for postnatal phase), and Denver, CO (N=735). In each of these trials, participants had been randomly assigned to intervention (nurse-visitation) or control services (typically free transportation for prenatal and well-child care and/or sensory and developmental screening and referral services). We linked the addresses of participants to their Census tract and block group numbers at registration during pregnancy and at each of the follow-up assessment periods.

From the census tract and block group numbers, we extracted the following data elements from the Bureau of Census database:

1. Percentage of households below the federal poverty level
2. Percentage of persons on public assistance
3. Percentage of female-headed households with children
4. Percentage of persons unemployed
5. Percentage of population that is black

These data elements have been used previously to create an index of concentrated neighborhood disadvantage. [1,2] We created our version of this index based after factor analyzing the Census data with a commonly employed 6-item index (the 5 listed above plus percentage of persons less than age 18). We eliminated the sixth element because as it had the lowest factor weighting when all US block groups were considered. We based the index on the standardized scores of the values of each of the five elements, with the mean set a zero, and Standard Deviation set to1.0, for the 1980, 1990, and 2000 Censuses. The neighborhood disadvantage index consisted of the average of the standardized values for the 5 elements, weighted, as described below, by the intervals between the Censuses when participant assessments were conducted. The weighted scores represent the degree of concentrated disadvantage in the neighborhood in terms of national averages, weighted by the intervals to the Census. We used the neighborhood disadvantage scores to examine the hypotheses specified above. We describe below the details of the construction of the Neighborhood variables and the selection of dependent variables to use in the examination of Hypothesis three.

Geo-Coding and Construction of Neighborhood Variables from Census Data

Table 1 shows the numbers of maternal interviews conducted at registration and each phase of follow-up in each of the trials along with the number of addresses collected. 8,290 participants' addresses were sent to GeoLytics, a demographic research firm that specializes in producing reports from US Census data. They were unable to match 455 addresses. Of the 7,835 addresses they did match, 1,271 had "accuracy codes" indicating that the match was questionable. Further investigation on our part (detailed below), led us to send a second wave of data to GeoLytics but this

time the data were the coordinates (longitude and latitude) associated with a subset of addresses (N=476). Table 2 shows that 3% of the addresses were never linked to a census tract and block group and another 6% were determined from the coordinates of the address. Lastly, since intake neighborhoods were especially important, missing intake data was imputed (details below) based on other general information we had about their location.

Table 1. Number of interviews done in each trial

Follow-up Phase	Elmira Begun in 1978	Memphis Begun in 1988	Denver Begun in 1994
Registration	400	743	735
Age 2	346	679	635
Age 4	335	646	639
Age 6		641	604
Age 9		627	553
Age 12		594	
Age 15	324		
Total	1405	3930	3166

Table 2. Percent of addresses geo-coded

Final Disposition	Number	Percent
Census Information by Address	7,534	91%
Census Information by Coordinates	476	6%
Missing	280	3%
Total	8,290	

Checking the Reliability of Wave 1 GeoLytics Data

For a random sample of 5% of the census data returned from Geolytics from the first wave of addresses, we reviewed the addresses using American Fact Finder (http://factfinder.census.gov). From that sample, we concluded that GeoLytics nearly always returned valid census block group data unless they also returned an "Accuracy Code" which indicated that their program had to modify the submitted address in order to find a match in their database. In cases where GeoLytics returned an "Accuracy Code", the census data were often incorrect and we decided to treat census data with an "Accuracy Code" as suspicious.

Use of Internet Sources to Investigate Missing Census Data

The N=455 addresses that Geolytics could not find were submitted in batches to Yahoo Maps (http://www.batchgeocode.com) which was able to locate 293 of those addresses. The addresses Yahoo Maps found were then reviewed to determine the quality of the match. The valid matches were batch submitted to Google Maps (http://gpsvisualizer.com/geocoder) which returned coordinates (i.e. longitude and latitude) of the addresses. In this process, 18 addresses were determined to be invalid and 275 addresses were converted to coordinates.

Use of Internet Sources to Investigate Suspicious Census Data

The 1,271 address that GeoLytics returned with "Accuracy Codes" were submitted in batches to Yahoo Maps and the distance between the coordinates from Geolytics and the coordinates from Yahoo Maps were calculated. If that distance were less than two tenths of a mile (~1,000 feet), we assumed they were in the same neighborhood. 928 addresses were considered valid by that criterion. The 343 addresses that did not meet the 0.2 mile criterion were further investigated using Google maps. 42 were determined to agree with Geolytics; 201 were assigned coordinates to be resubmitted to Geolytics; and, 100 were declared missing.

Submission of Coordinates to Geolytics

This process resulted in 476 addresses that we were able to find addresses in which we were reasonably confident and for which we could determine coordinates. We submitted the coordinates to Geolytics along with 500 addresses from which we had already knew the GeoLytics census data as a check on their system. Their initial response did not agree well with the 500 sample. Follow-up discussions with them resulted in their rerunning the data. There still was not perfect agreement since they could not determine whether the address came from the even numbered side of the street or the odd numbered side of the street. We decided to accept the data on the assumption that the block group across the street would be "close enough".

Figure 1 shows the result for each of the 8,290 participants' addresses. The circles show the final determination for each address. "Use" means "use the census tract and block group Geolytics returned from the initial address.

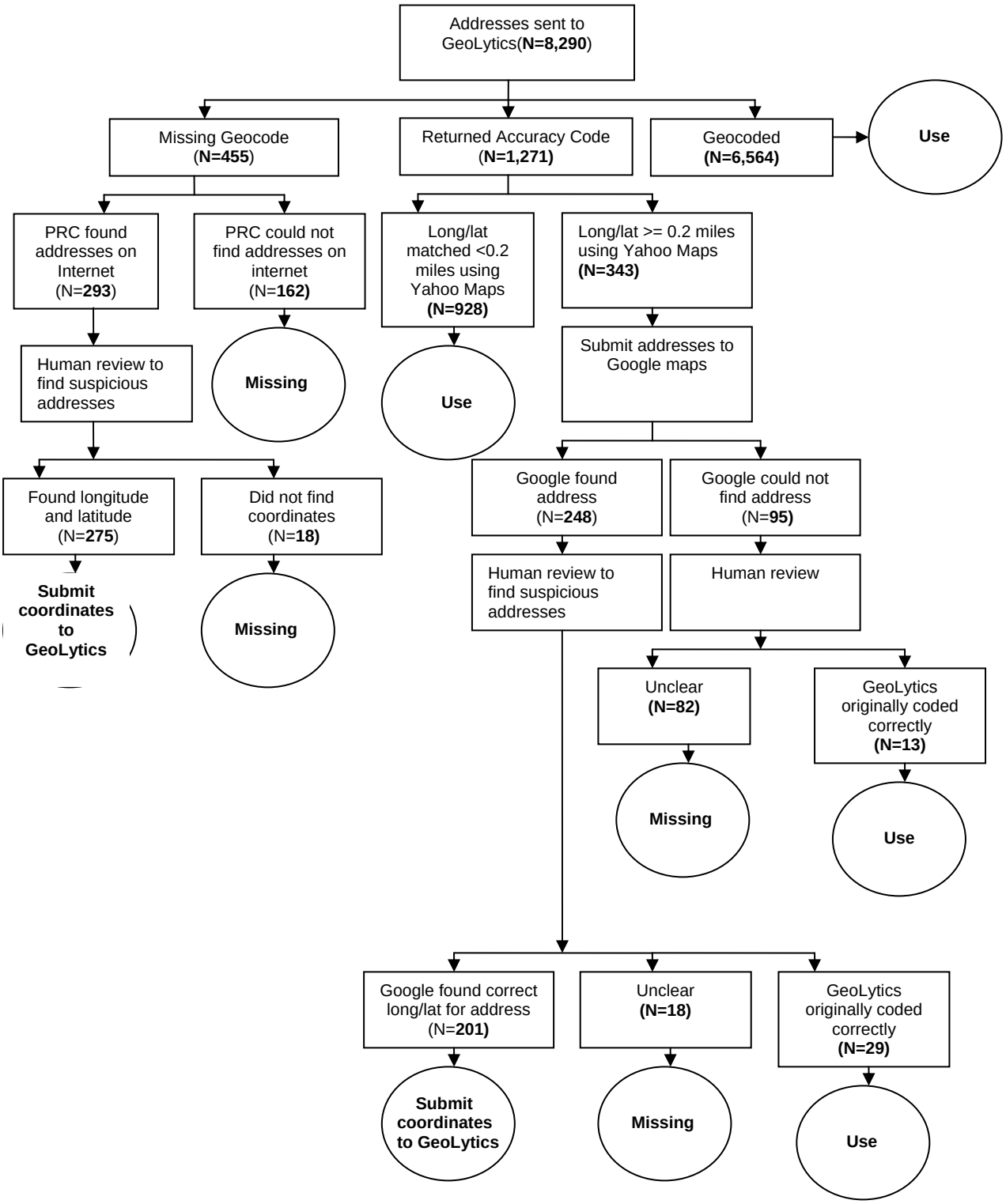

Figure 1. Procedure for processing addresses

Since we intended to use intake neighborhood disorganization as a covariate in our moderation analyses, we did not want to have missing census data at intake. We decided to aggressively investigate missing intake addresses and, if that were unsuccessful, impute data from any partial information we have on the subjects. We have a flag indicating which data were imputed.

Imputed Addresses for Elmira Intake

Intake addresses from Elmira, collected in 1978, had a high proportion (21%) of missing data due to missing address data or imprecise addresses that were sufficient in a smaller city at that time. We investigated incomplete addresses using internet resources and maps from the county showing census tract and block group boundaries from the census periods (1980-2000) and contacted the data collection team in Elmira to aid in our investigations. These investigations resolved 46 addresses leaving 38 still unknown. Fortunately, at intake, subjects were randomized based on the region of the county in which they lived with the county divided into N=7 regions. Using the known intake census data, we determined which census tracts belonged to which regions. From that, we imputed neighborhood disorganization based on a weighted average of the census tracts in the regions –weighted by the proportion of subjects from that region in a particular census tract. This weighting was based on the idea that at-risk women were likely to live in similar neighborhoods.

Addresses for Memphis and Denver Intake

Intake addresses for Memphis were missing for 20 addresses and for whom we had follow-up data (3% of those randomized). Further investigation resolved all 20 cases. Intake addresses for Denver were missing for 5 addresses (<1% of those randomized). Further investigation resolved all 5 cases.

Table 3. Number of Addresses found or imputed by trial and assessment period

Site	Period	Interviewed	Census Data N	Census Data Pct of Addresses
Elmira	Intake	400	400	100%
	24 Months	346	270	78%
	4 Years	335	284	85%
	15 Years	324	247	76%
Memphis	Intake	743	677	91%
	24 Months	679	657	97%
	54 Months	646	625	97%
	6 Years	641	627	98%
	9 Years	627	617	98%
	12 Years	594	567	95%
Denver	Intake	735	730	99%
	24 Months	635	612	96%
	4 Years	639	616	96%
	6 Years	604	591	98%
	9 Years	553	535	97%

Linear Interpolation of Neighborhood Disadvantage

Given that changes over time may be affected by changing demographics within census tracts and block groups, we obtained census data for each of the addresses for all three censuses (1980, 1980, and 2000). In order to remove a step effect when going from one census time to the next, we decided to use linear interpolation between census periods. For example, a data collection done in 1994 would be a weighted average of the 1990 census period (weighted 0.6) and the 2000 census period (weighted 0.4). This approach assumes that the change in a neighborhood occurs linearly over the 10 year period. While that is not exactly true, it seems a better estimate than to pick one of the two census periods. Data collection for each follow-up was occurred over multiple year periods. We decided to use the median year of the follow-up. Each participant in the study was assigned two scores: one for their census tract and one for their block group. If date were missing at the block group level, data from the census tract was used in its place. These variables were used to test the study hypotheses using analytic procedures described below.

Other Variables Used in Study

Baseline Assessments

Baseline assessments for each of the trials have been described in our published reports. In each of the trials, we have assessed standard sociodemographic variables (e.g., maternal age, marital status, race, ethnicity) that are used to characterize the sample and employ as classification factors and covariates in the analysis of program impact. In the Elmira trial, we also assessed women's locus of control (their belief that events are under their control versus the control of others or chance) that we employed in analyses of program impact on child maltreatment and injuries. In the Memphis and Denver trials, we created a variable to index women's psychological resources measured at registration.

Outcome Assessments

We have examined a common set of maternal and child outcomes across each of the three trials of the NFP. The outcome domains and times at which they were assessed in each of the trials are shown in Table 4. In order to reduce the statistical challenges of analyzing so many outcomes in this study, we reduced the outcome domains and selected a small number of representative outcomes within those domains that met three criteria: First, the outcomes had to be primary in the sense that their public health meaning was unequivocal. Second, if outcomes were examined at multiple points over time, we generally selected the latest point in time, and examined the outcome as a repeated measure or average across time interval. Third, we gave particular attention to outcomes for which there had been program impacts in earlier reports, given that we wanted to know whether previously reported program effects were more pronounced for individuals living in neighborhoods characterized by concentrated social disadvantage. Some low-frequency outcomes on which there were program effects in previous reports could not be examined in this analysis, as the number of events was too low to allow examination of interactions. Every outcome listed below had been examined in previously reported papers.

Table 4. Outcome Domains Assessed at Each Phase of Follow-up in Each Trial

	Elmira			Memphis					Denver			
	Child Age			Child Age					Child Age			
Outcome Domains	2	4	15	2	5	6	9	12	2	4	6	9

Child Health and Development
Language/Mental Development
Injuries
Child Abuse & Neglect (state records)
Behavioral Problems
Academic Achievement
Arrests
Substance Use
Maternal Life Course
Subsequent pregnancies
Employment
Use of Welfare
Arrests
Substance Use

Statistical Models and Methods of Analysis

The primary analyses made use of general linear model methods and their extensions. The focus was on (1) full model specification, to account for all sources of variation, and a full examination of interactions among model factors, including examination of homogeneity of regressions to understand interactions between classification effects; and (2) generalized models to analyze dichotomous outcomes with binomial error distributions and count data assumed to have negative binomial distributions.

For analysis of the Elmira data, the core model consisted of Treatments (three levels – control, nurse-visited during pregnancy, and nurse-visited during pregnancy and infancy) x Risk (unmarried women from low SES households at registration vs. all other marital status by SES classifications). We show the contrast of the control group vs. the group that received nurse-visitation during pregnancy and infancy.

For analysis of the Memphis data, the core model consisted of Treatments (control vs. nurse-visited during pregnancy and infancy) x maternal psychological resources (low vs. high, dichotomized at the median).

For the analysis of the Denver data, the core model consisted of Treatments (control vs. paraprofessional visitation vs. nurse visitation) x maternal psychological resources (low vs. high, dichotomized at values of the components of the psychological resource variable that would have created the median split in Memphis, but created a 40-60, low-high split in Denver). We show the contrast of the control vs. nurse-visited groups in the tables, but include plots of regressions for the

paraprofessional-visited group when displaying separately fitted regressions of outcomes on neighborhood disadvantage by treatment condition.

Analysis of Research Questions

Examination of Program Influence on Women's Moving to Less Distressed Neighborhoods: Hypothesis 1. The initial step in this study consisted of an examination of the effect of the program on changes in levels of neighborhood disadvantage over time. In the analyses examining Hypothesis 1, the neighborhood disadvantage index was treated as the dependent variable in the core models described above. The multiple observations on families provides for the inclusion of a repeated-measures factor for time and a random factor for families. The interaction of treatment and time (and single degree of freedoms partitioned from the overall interaction) became the primary test of whether the index of neighborhoods disadvantage improved to a greater extent in women assigned to the nurse-visited group in contrast to those assigned to the control group. We also examined 3-way interactions of treatment, time, and family risk (for Elmira) and maternal psychological resources (for Memphis and Denver) to see whether changes in neighborhood disadvantage was further conditioned by initial family risk or maternal resources to manage the challenges of living in poverty.

Examination of Process (Direct and Indirect Effects of the Intervention): Hypothesis 2. We had planned a series of mediation analyses to determine the extent to which program effects on maternal and child health outcomes were explained by reductions in neighborhood disadvantage over time. Given that there were no coherent changes in levels of neighborhood disadvantage attributable to the program, we did not conduct mediation analyses.

Examination of Whether Program is Especially Effective for Families Who Lived in the Most Disadvantaged Neighborhoods and with Mothers Who Had the Fewest Resources (Treatment by Exogenous Variable Interactions): Hypothesis 2. To determine whether the effect of the intervention is stronger for families living in the most disadvantaged neighborhoods, we treated neighborhood risk as a quantitative variable and conducted homogeneity of regressions analysis, examining the equivalence of regression slopes across levels of treatment and other

classification factors. Where regressions differ, we can infer that the experimental effects varied as a function of neighborhood disadvantage.

We display the effects for the sample as a whole and for higher risk mothers and children in Elmira, given that program effects were more pronounced for those characterized by the mothers' being unmarried and from low-SES households at registration. In Memphis, we table maternal outcome regressions for the sample as a whole and for mothers with initially higher psychological resources, as the maternal life-course effects found in Memphis tended to be greater for mothers with initially higher psychological resources. For Memphis children, we show the results for the sample as a whole and for children born to mothers with lower levels of psychological resources, as program effects in Memphis were more pronounced for those born to mothers with few psychological resources to manage living in poverty and concentrated social disadvantage. Finally, in Denver, we show effects for mothers for the sample as a whole, and for children separately for the whole sample and those born to low-resource mothers.

To aid in the interpretation of the differences in slopes, we show in the tables the individual regression coefficients, the p-value for the difference in slopes, and the estimates of outcomes separately for three points on the neighborhood covariate index: the 10th percentile, the median, and the 90th percentile. Full interpretation was facilitated by plotting the regressions of the dependent variables on the covariate separately for the nurse-visited and comparison samples. We show the plots for selected outcomes.

Results

Neighborhood Disadvantage at Baseline in the Three Trials

Figure 2 represents the distributions of baseline neighborhood disadvantage in the samples enrolled in the three trials using box and whisper plots. It shows that the middle 50% of the distribution in neighborhood disadvantage in Memphis (the box) does not overlap with the middle 50% of the distributions of neighborhood disadvantage in Elmira and Denver. (The whiskers show the essential range of the data.) The neighborhoods in which the Memphis sample lived were considerably more disadvantaged than those in which the Elmira and Denver samples lived. At

registration, families in Memphis lived in neighborhoods that were about 2.4 SD above the national mean, although the range of neighborhood disadvantage was considerable. Families in Elmira lived in neighborhoods that on average were near the national mean in neighborhood disadvantage, and those in Denver started out living in neighborhoods that were about 0.30 SD above the national mean. Families in the Memphis trial thus had to contend with levels of concentrated disadvantage far more challenging than their counterparts in the other two trials.

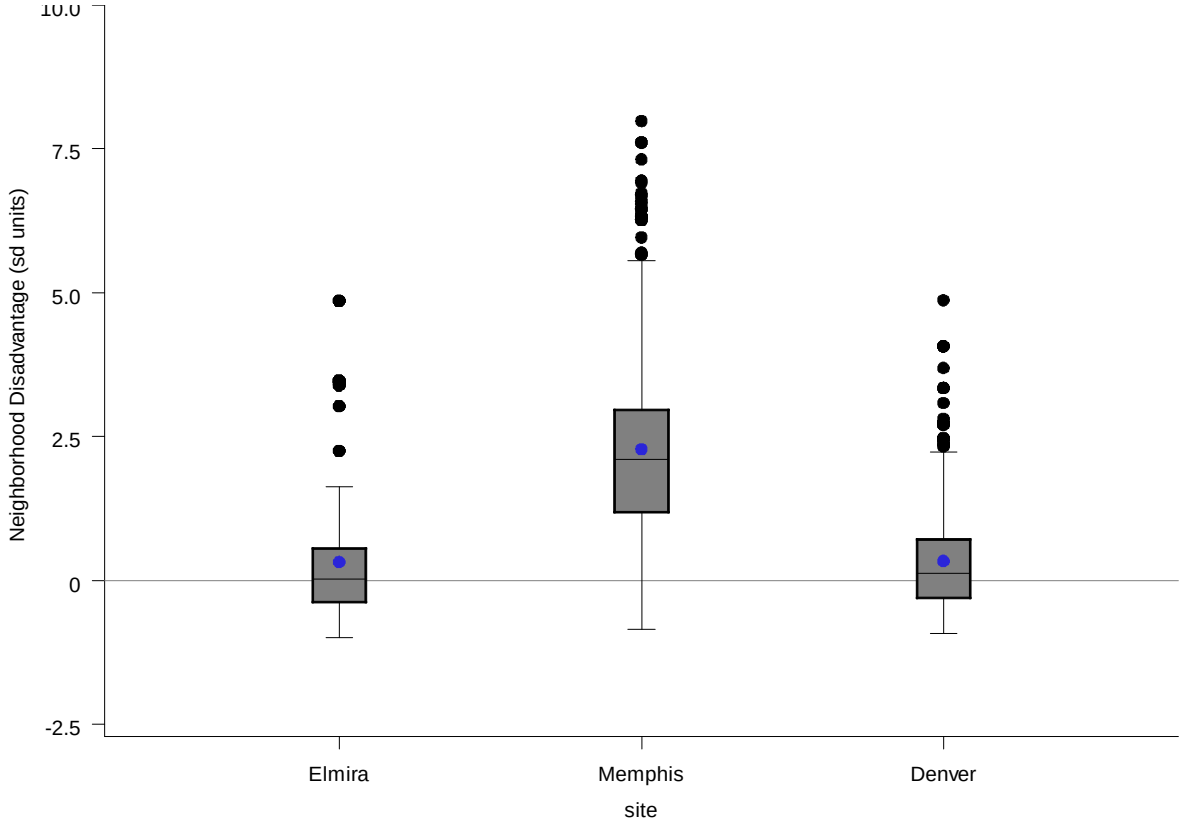

Figure 2. Box and whiskers plots of the distributions of neighborhood disadvantage scores at registration for participants in the Elmira, Memphis, and Denver trials of the NFP.

Hypothesis One:

Changes in Neighborhood Disadvantage Scores over Time

Figures 3-5 show the neighborhood disadvantage scores (and standard errors around the estimates) for all assessment periods for the whole samples, and for those at higher and lower risk for the Elmira, Memphis, and Denver trials, respectively. Families characterized by greater risk and

mothers with lower psychological resources lived in more disadvantaged neighborhoods. There is no indication in any of these plots that nurse-visited families moved to better neighborhoods over time than did families in the control group. In fact, there are indications in the Elmira and Memphis trials that some segments of the nurse-visited samples lived in worse neighborhoods than their counterparts in the control groups in later years.

Elmira

Figure 3 indicates that among the lower risk families (mothers were not unmarried and living in low SES families at registration) in the Elmira trial, nurse-visited families as a trend lived in worse neighborhoods at child age 15 than did their counterparts in the control group ($p=.093$).

Memphis

Figure 4 reveals a similar pattern among families headed by mothers with low psychological resources in Memphis: those visited by nurses either remained in or moved to more distressed neighborhoods over time than did their counterparts in the control group. While there were no differences among low-resource families through child age 5, beginning at age 6 and continuing through age 12, nurse-visited families headed by low-resource mothers lived in consistently more disadvantaged neighborhoods ($p<.05$). While nurse-visited high-resource mothers lived in more distressed neighborhoods at baseline than did their counterparts in the control group, there were no differences in neighborhood disadvantage scores thereafter. While all families, on average, moved to better neighborhoods over time, families in Memphis lived in far more disadvantaged neighborhoods than their counterparts in Elmira and Denver at later periods of follow-up.

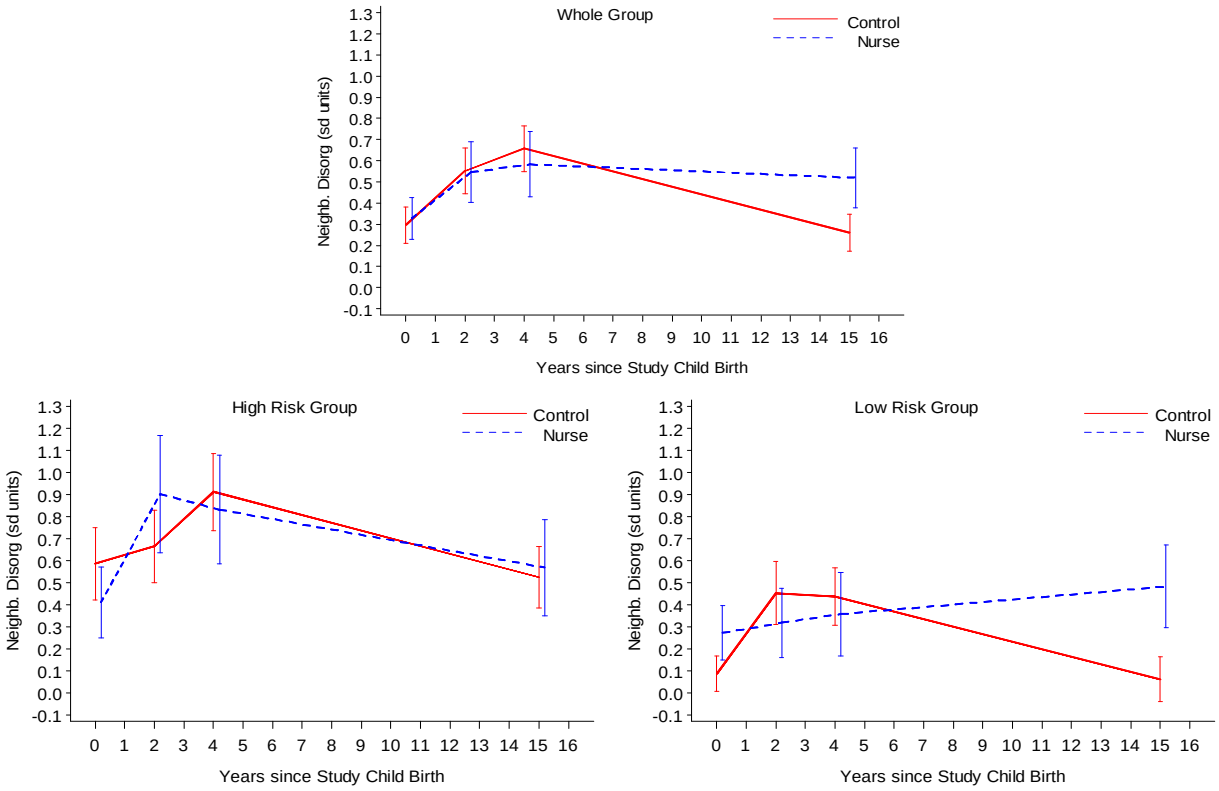

Figure 3. Changes in neighborhood disadvantage by treatment – for whole sample and those with high and low family risk – Elmira.

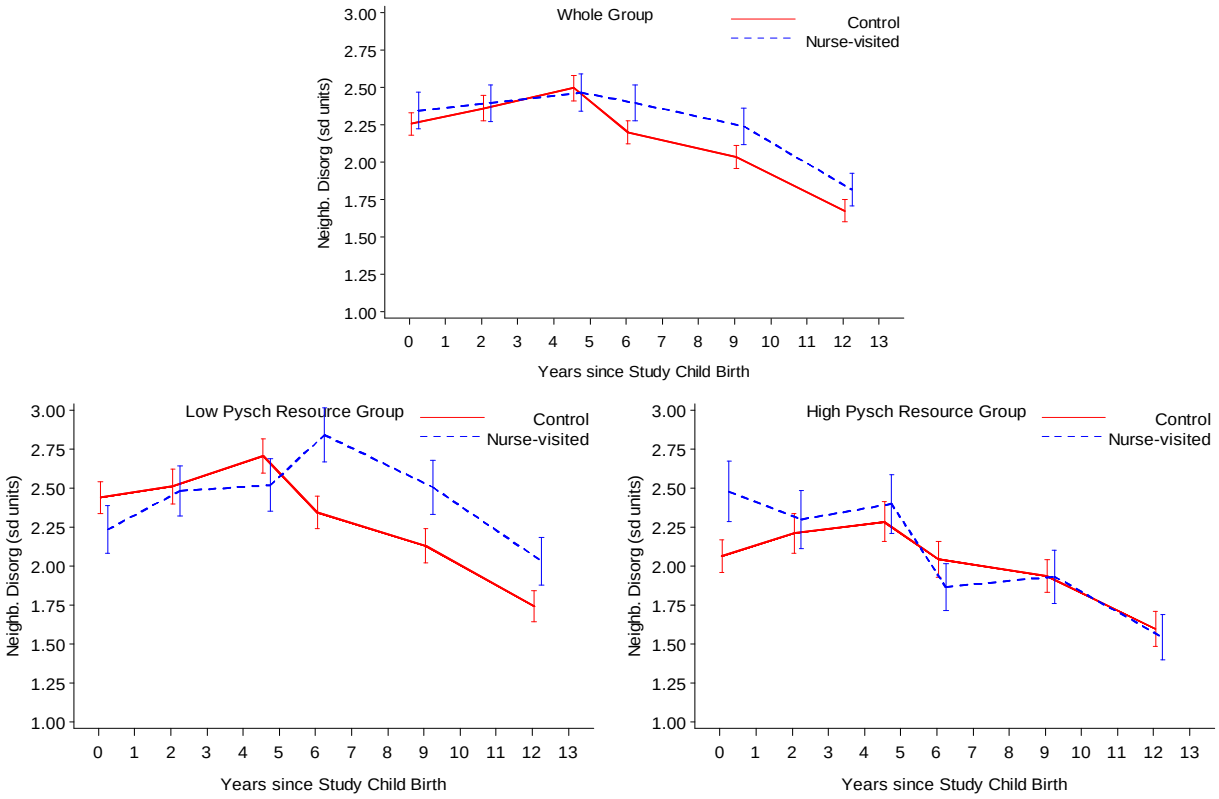

Figure 4. Changes in neighborhood risk by treatment – for whole sample and those with low and high maternal psychological resources – Memphis

Denver

As shown in Figure 5, families in Denver also moved to better neighborhoods over time. There were no differences between the nurse-visited and control group in the degree to which these neighborhoods were characterized by concentrated social disadvantage. At the 9-year follow-up, families in Denver lived in neighborhoods that were slightly better than the average neighborhood in the United States.

Hypothesis Two

Improvements in Neighborhood Characteristics as Mediators of NFP Effects

Given that there were no consistent program effects on neighborhood disadvantage in the expected direction in any of the trials, differences in neighborhood adversity cannot help explain NFP program effects on maternal and child health. It is possible, however, that the program may have buffered mothers and children from the adverse effects of concentrated social disadvantage in neighborhoods. We take up this issue in the following section.

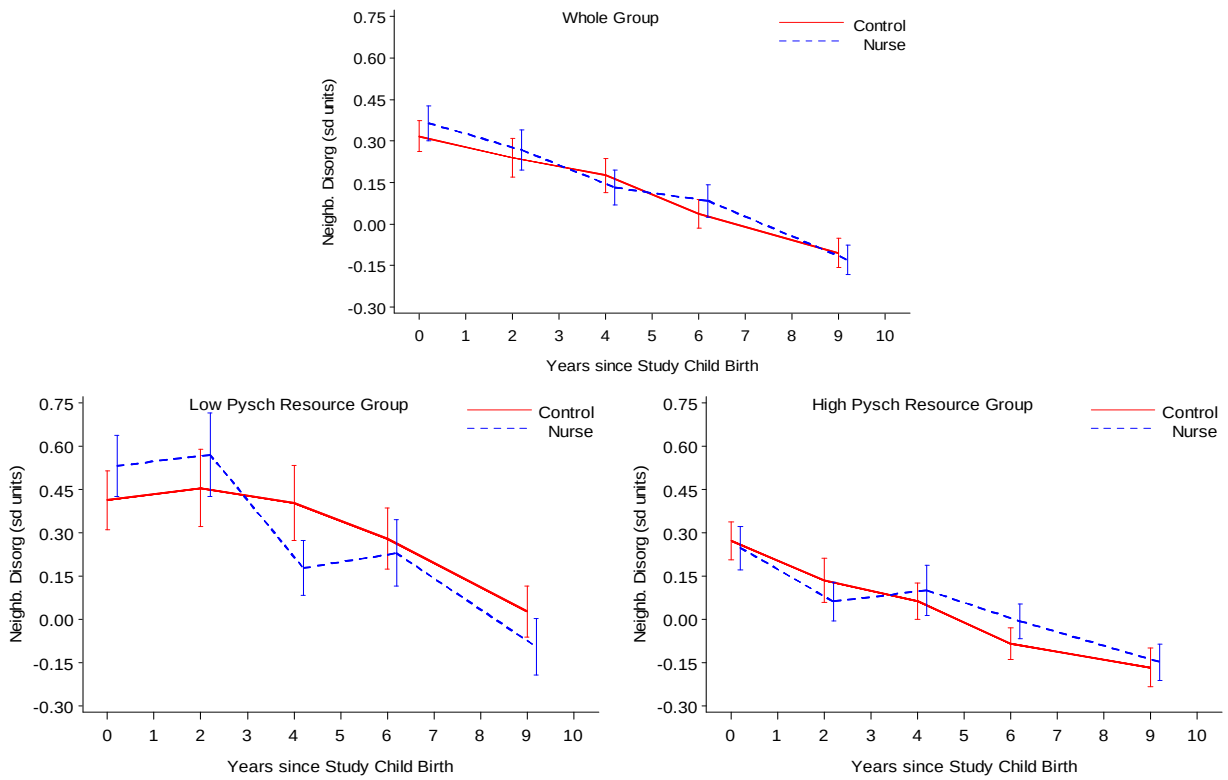

Figure 5. Changes in neighborhood risk by treatment – for whole sample and those with low and high maternal psychological resources – Denver.

Hypothesis Three:

Nurse-Visitation as a Moderator of Concentrated Neighborhood Disadvantage

To examine this hypothesis, we analyzed major maternal and child health outcomes (and welfare costs in Memphis) in statistical models that included neighborhood disadvantage covariates specified separately by treatment condition, focusing on tests of differences in the slopes of the neighborhood covariate values, and estimates of treatment differences in outcomes at the median, 10th percentile and 90th percentile values of the neighborhood disadvantage covariate to help with interpretation of the differences in slopes. We focused this analysis on major maternal and child outcomes reported in papers at each of the follow-up phases and grouped the outcomes. For each trial, we grouped the outcomes for mothers and children in separate tables and grouped outcomes in similar domains. We present the findings in order by trial, and within trial by maternal and then child outcomes. In the tables, we have shaded those outcomes that were significantly different as treatment effects in previous reports.

Elmira Trial

Maternal Outcomes. Table 3 shows the estimates of regression coefficients of maternal outcomes on neighborhood disadvantage scores and tests of the differences in those regressions by treatment (homogeneity of regression) for the whole sample. To help with interpretation of the difference in regression coefficients, we show estimates of outcomes at the 10th and 90th percentiles and the median of neighborhood disadvantage scores. There were no significant treatment differences in regressions for life-course outcomes, although there was a trend for the regressions of numbers of months women worked from birth through 46 months post partum on neighborhoods to be different for nurse-visited and control group women (p=.069). Contrary to expectation, however, the estimates of treatment-control difference in months worked indicated that nurse-visited women living in neighborhoods with lower levels of disadvantage were employed more than their control-

group counterparts, while the difference in employment was attenuated among women who lived in more disadvantaged neighborhoods at registration. The pattern of regression coefficients and treatment-control differences for rates of maternal arrests over the 15-year period following birth of the first child followed the expected pattern (i.e., the treatment-control difference in arrests was greater among women who lived in more disadvantaged neighborhoods at registration), but was non-significant (p=.135).

Table 3. Regressions of maternal life course outcomes on neighborhood disadvantage scores for mothers - specified separately for nurse-visited and control group - Elmira.

Whole Group				**Estimated Y at**			
	Tx	**Slope**	**SE**	**10%[4]**	**50%[5]**	**90%[6]**	**P-value**
Rapid Subsequent Births							
interval between birth of first and second child 0-15 years	Control	5.12	2.62	39.18	42.42	49.58	
	Visited	7.57	3.57	48.59	53.38	63.96	0.581
Employment							
• 15 Years Months employed	Control	-4.53	3.77	88.73	85.87	79.54	
	Visited	-4.92	5.05	96.76	93.65	86.78	0.951
Use of Welfare							
• 0-22 months postpartum No. days on public assistance	Control	0.85	0.70	6.99	7.53	8.72	
	Visited	2.36	0.87	5.58	7.08	10.38	0.177
• 15 Years Months receiving AFDC	Control	8.29	4.04	51.25	56.50	68.10	
	Visited	8.45	5.40	33.71	39.06	50.88	0.981
• 15 Years Months receiving food stamps	Control	7.32	4.23	56.08	60.72	70.96	
	Visited	9.72	5.66	38.27	44.42	58.01	0.735
• 15 Years Months receiving Medicaid	Control	8.48	4.46	59.71	65.08	76.95	
	Visited	15.23	5.97	43.87	53.51	74.82	0.366
Arrests							
• 15 Years Arrests	Control	0.50	0.15	0.17	0.23	0.47	
	Visited	0.01	0.30	0.16	0.16	0.17	0.135
Substance Use							
• 15 Years Substance use impairments	Control	0.20	0.18	0.45	0.51	0.68	
	Visited	-0.23	0.35	0.52	0.45	0.33	0.254

[4] Value of dependent variable at neighborhood disadvantage at lowest 10% of all neighborhood disadvantage for the entire sample
[5] Value of dependent variable at neighborhood disadvantage at median of all neighborhood disadvantage for the entire sample
[6] Value of dependent variable at neighborhood disadvantage at upper 10% of all neighborhood disadvantage for the entire sample

Table 4 shows that among women who lived in higher risk families at registration (i.e., they were unmarried and living in lower-SES households), the regressions of subsequent pregnancies through 22 months postpartum on neighborhood disadvantage were significantly different for nurse-visited and control-group women (p=.042). The treatment-control difference in subsequent pregnancies was greater among women who lived in more disadvantaged neighborhoods at registration (i.e., at the 90th percentile) than in less disadvantaged neighborhoods (the 10th percentile). The regression of subsequent pregnancy on neighborhood disadvantage was negative among nurse-visited women, and non-significant among those in the control group. The patterns of treatment differences in regression coefficients among higher risk women followed the expected pattern for maternal arrests and for behavioral impairments due to substance use, but were non-significant (p=.223 and p=.150, respectively). For both outcomes, the rate of adverse outcomes increased in the control group as neighborhood disadvantage increased and decreased in the nurse-visited condition, producing larger treatment-control differences for women living in neighborhoods with more concentrated disadvantage at registration.

Table 4. Regressions of maternal life course outcomes on neighborhood disadvantage score for mothers living in high-risk families at registration - specified separately for nurse-visited and control group - Elmira.

High Risk				**Estimated Y at**			
	Tx	**Slope**	**SE**	**10%[7]**	**50%[8]**	**90%[9]**	**P-value**
Rapid Subsequent Births							
Interval between birth of first and second child	Control	3.55	3.10	38.80	41.05	46.01	
	Visited	13.49	5.40	56.79	65.33	84.20	0.111
Employment							
• 15 Years Months employed	Control	-6.62	4.44	80.73	76.54	67.29	
	Visited	-2.72	7.67	89.55	87.82	84.02	0.661
Use of Welfare							
• 15 Years Months receiving AFDC	Control	7.74	4.76	78.62	83.52	94.35	
	Visited	7.90	8.21	47.02	52.02	63.07	0.987
• 15 Years Months receiving food stamps	Control	6.15	4.98	85.09	88.99	97.60	
	Visited	3.25	8.60	50.98	53.03	57.58	0.770
• 15 Years Months receiving Medicaid	Control	10.88	5.26	85.59	92.48	107.70	
	Visited	8.06	9.08	64.44	69.54	80.81	0.788

[7] Value of dependent variable at neighborhood disadvantage at lowest 10% of all neighborhood disadvantage for the entire sample
[8] Value of dependent variable at neighborhood disadvantage at median of all neighborhood disadvantage for the entire sample
[9] Value of dependent variable at neighborhood disadvantage at upper 10% of all neighborhood disadvantage for the entire sample

Arrests							
• 15 Years Arrests	Control	0.37	0.15	0.41	0.52	0.89	
	Visited	-0.15	0.44	0.30	0.27	0.22	0.223
Substance Use							
• 15 Years Substance use impairments	Control	0.17	0.20	0.70	0.78	1.00	
	Visited	-0.67	0.62	0.84	0.55	0.21	0.150

Child Outcomes. Table 5 shows the results of the homogeneity of regression analyses of child outcomes on neighborhood disadvantage for the whole sample. While the treatment differences in regression coefficients were significant ($p<.05$) for two outcomes (Cattell mental development scores at child age 2, and rates of child abuse and neglect through age 2), the results did not conform to the hypothesized pattern. For the Cattell scores at child age 2, nurse-visited children had higher scores than their control-group counterparts among children living in less disadvantaged neighborhoods, but for those who lived in the most disadvantaged neighborhoods, control-group children performed better than nurse-visited children. We find a similar pattern of results when we examine rates of child abuse and neglect in the first two years of life: nurse-visited children were less likely to be abused or neglected than control children when their mothers lived in neighborhoods with lower levels of disadvantage, but were more likely to be abused or neglected when living in more disadvantaged neighborhoods.

Table 5. Regressions of child outcomes on neighborhood disadvantage scores - specified separately for nurse-visited and control group - Elmira.

Whole Group				**Estimated Y at**			
	Tx	**Slope**	**SE**	**10%[10]**	**50%[11]**	**90%[12]**	**P-value**
Language/Mental Development							
• Year 1 Bayley Mental Development Index	Control	-1.84	1.34	109.77	108.61	106.04	
	Visited	-2.38	1.67	113.19	111.68	108.35	0.799
• Year 2 Cattell Developmental Score	Control	-0.75	1.15	105.61	105.13	104.09	
	Visited	-4.28	1.44	111.16	108.45	102.47	0.055
• Year 3 Stanford Binet I.Q. Test	Control	-1.20	1.19	102.16	101.40	99.73	
	Visited	-2.44	1.52	105.52	103.97	100.56	0.518
• Year 4 Stanford Binet I.Q. Test	Control	-0.98	1.09	109.03	108.41	107.03	
	Visited	-2.60	1.41	112.66	111.01	107.38	0.365
Injuries							

[10] Value of dependent variable at neighborhood disadvantage at lowest 10% of all neighborhood disadvantage for the entire sample
[11] Value of dependent variable at neighborhood disadvantage at median of all neighborhood disadvantage for the entire sample
[12] Value of dependent variable at neighborhood disadvantage at upper 10% of all neighborhood disadvantage for the entire sample

• Year 2 No. of emergency visits for accidents and poisonings	Control	-0.01	0.12	0.33	0.33	0.33	
	Visited	0.05	0.15	0.29	0.29	0.31	1.000
• Years 2 -4 No. injuries/ingestions in physician record	Control	-0.09	0.16	0.57	0.54	0.48	
	Visited	-0.14	0.24	0.36	0.33	0.27	0.856
Child Abuse & Neglect (state records)							
• Year 0-15 Substantiated reports of child abuse and neglect	Control	-0.64	0.33	0.80	0.54	0.22	
	Visited	-0.33	0.52	0.36	0.30	0.19	0.625
Behavioral Problems							
• Years 15 No. of externalizing problems, mother report	Control	1.41	0.74	10.93	11.82	13.79	
	Visited	-0.26	1.01	10.56	10.40	10.04	0.183
• Years 15 No. of externalizing problems, child report	Control	-0.21	0.72	16.08	15.95	15.66	
	Visited	0.55	0.95	15.63	15.98	16.74	0.526
Arrests							
• Years 15 Incidence of arrests	Control	0.36	0.20	0.27	0.34	0.57	
	Visited	0.14	0.29	0.18	0.20	0.24	0.534
Substance Use							
• Years 15 Incidence of cigarettes smoked per day	Control	0.05	0.21	1.55	1.60	1.72	
	Visited	-0.36	0.34	1.99	1.58	0.95	0.307
• Years 15 Incidence of days drank alcohol	Control	0.25	0.24	2.03	2.37	3.35	
	Visited	-0.55	0.54	3.18	2.25	1.05	0.218
• Years 15 Incidence of days used drugs	Control	0.47	0.41	1.81	2.44	4.69	
	Visited	0.17	0.80	2.71	3.01	3.80	0.749

As can be seen in Table 6, for children born to higher risk mothers (those who were unmarried and from lower SES families at registration), the pattern of results for substantiated cases of child abuse and neglect from birth through child age 15 conformed to the expected pattern, although the treatment difference in regression coefficients did not reach conventional levels of statistical significance (p=.060). (It should be noted, however, that the pattern of regressions in the low-risk families, reflected a complete interaction, with nurse-visited families having lower rates of child abuse and neglect than their control-group counterparts for those living in neighborhoods with lower levels of disadvantage, and higher rates than their control-group counterparts in more disadvantaged neighborhoods, p=.001.)

Table 6. Regressions of child outcomes on neighborhood disadvantage scores for children born to high-risk mothers (unmarried and low SES) - specified separately for nurse-visited and control group - Elmira.

High Risk Group (unmarried mothers from low SES families)				Estimated Y at			
	Tx	**Slope**	**SE**	**10%[13]**	**50%[14]**	**90%[15]**	**P-value**
Language/Mental Development							
• Year 1 Bayley Mental Development Index	Control	-3.73	1.55	109.74	107.38	102.16	
	Visited	0.85	2.65	112.01	112.55	113.75	0.135
• Year 2 Cattell Developmental Score	Control	-1.70	1.30	103.86	102.78	100.40	
	Visited	-1.61	2.27	108.74	107.72	105.47	0.973
• Year 3 Stanford Binet I.Q. Test	Control	-1.34	1.31	101.31	100.46	98.59	
	Visited	-0.91	2.30	103.85	103.27	102.00	0.872
• Year 4 Stanford Binet I.Q. Test	Control	-0.66	1.21	107.31	106.90	105.97	
	Visited	-1.97	2.15	110.56	109.31	106.55	0.594
Injuries							
• Year 2 No. of emergency visits for accidents and poisonings	Control	-0.02	0.13	0.37	0.36	0.35	
	Visited	0.02	0.23	0.30	0.31	0.31	1.000
• Years 2 -4 No. injuries/ingestions in physician record	Control	-0.01	0.16	0.51	0.51	0.50	
	Visited	0.21	0.28	0.29	0.33	0.45	0.504
Child Abuse & Neglect (state records)							
• Year 0-15 Substantiated reports of child abuse and neglect	Control	0.47	0.19	0.50	0.67	1.31	
	Visited	-0.98	0.99	0.42	0.22	0.06	0.060
Behavioral Problems							
• Years 15 No. of externalizing problems, child self report	Control	0.83	0.88	11.86	12.39	13.55	
	Visited	0.52	1.53	7.60	7.93	8.66	0.860
• Years 15 No. of externalizing problems, mom report	Control	0.66	0.79	16.45	16.87	17.80	
	Visited	0.81	1.38	15.75	16.26	17.40	0.925
Arrests							
• Years 15 Incidence of arrests	Control	0.34	0.20	0.31	0.38	0.61	
	Visited	0.23	0.44	0.21	0.24	0.34	0.827
Substance Use							
• Years 15 Incidence of cigarettes smoked per day	Control	0.05	0.23	2.89	2.98	3.19	
	Visited	-0.30	0.39	2.41	2.00	1.32	0.474
• Years 15 Incidence of days drank alcohol	Control	-0.11	0.26	4.77	4.46	3.82	
	Visited	-0.09	0.95	2.32	2.19	1.92	0.987
• Years 15 Incidence of days used drugs	Control	0.72	0.60	1.76	2.77	7.52	
	Visited	-0.13	1.05	4.55	4.20	3.51	0.592

Overall, results from the Elmira trial provide some support for the hypothesized pattern for maternal outcomes and for rates of child abuse and neglect in high-risk families, but lack consistency to support the hypothesis for other child outcomes.

[13] Value of dependent variable at neighborhood disadvantage at lowest 10% of all neighborhood disadvantage for the entire sample

Memphis Trial

Maternal Outcomes. Table 7 presents the results for maternal outcomes for the whole sample. There were no significant treatment differences in regression coefficients consistent with the hypothesis for the sample as a whole.

Table 7. Regressions of maternal life course outcomes on neighborhood disadvantage score for mothers - specified separately for nurse-visited and control group - Memphis.

Whole Group				Estimated Y at			
	Tx	Slope	SE	10%[16]	50%[17]	90%[18]	P-value
Rapid Subsequent Births							
• 0-12 Years Interval between first and second birth	Control	-1.41	0.86	39.17	36.73	33.54	.
	Visited	-2.00	1.18	47.26	43.79	39.26	0.685
Employment							
• Year 12 No. Months Employed per yr	Control	-0.08	0.40	23.64	23.50	23.32	.
	Visited	0.15	0.54	21.11	21.36	21.69	0.739
Use and Cost of Welfare							
• Years 0-12 No. of months receiving Food Stamps	Control	6.36	0.94	73.20	84.23	98.60	.
	Visited	5.25	1.29	69.20	78.31	90.17	0.486
• Years 0-12 No. of months receiving Medicaid	Control	5.55	0.97	94.63	104.26	116.80	.
	Visited	5.35	1.32	91.85	101.13	113.22	0.903
• Years 0-12 No. of months receiving AFDC/TANF	Control	6.78	1.06	53.84	65.61	80.94	.
	Visited	7.36	1.44	48.01	60.78	77.41	0.746
• Years 0-12 Cost (Thousands) of Food Stamps	Control	3.25	0.53	34.12	39.76	47.12	.
	Visited	2.92	0.73	30.75	35.83	42.43	0.715
• Years 0-12 Cost (Thousands) of Medicaid	Control	3.15	0.51	41.33	46.79	53.91	.
	Visited	2.60	0.69	38.86	43.37	49.24	0.520
• Years 0-12 Cost (Thousands) of AFDC/TANF	Control	4.51	0.90	26.00	33.82	44.01	.
	Visited	4.45	1.22	22.11	29.83	39.90	0.972
• Year 0-12 Total Governmental Costs (Thousands)	Control	10.90	1.83	101.95	120.86	145.49	.
	Visited	9.76	2.50	93.23	110.17	132.23	0.714
Arrests							
• 12 Years No. Maternal Arrests (0-12)	Control	-0.08	0.08	0.42	0.36	0.30	.
	Visited	-0.06	0.10	0.62	0.56	0.48	0.897
Substance Use							
• 6 Years Behavioral problems due to substance use	Control	-0.33	0.22	0.06	0.03	0.02	.
	Visited	-0.43	0.25	0.08	0.04	0.02	0.781

[14] Value of dependent variable at neighborhood disadvantage at median of all neighborhood disadvantage for the entire sample
[15] Value of dependent variable at neighborhood disadvantage at upper 10% of all neighborhood disadvantage for the entire sample
[16] Value of dependent variable at neighborhood disadvantage at lowest 10% of all neighborhood disadvantage for the entire sample
[17] Value of dependent variable at neighborhood disadvantage at median of all neighborhood disadvantage for the entire sample
[18] Value of dependent variable at neighborhood disadvantage at upper 10% of all neighborhood disadvantage for the entire sample

Table 8 presents the results of the homogeneity of regression analyses of maternal outcomes on neighborhood disadvantage scores for mothers with higher psychological resources at registration. The regressions of welfare costs through child age 12 on neighborhood disadvantage scores followed a consistent pattern: Costs rose consistently in the control group as neighborhood disadvantage increased; while they also rose in the nurse-visited group, the slope of increase was moderated. The treatment-control difference in cost was greater for families living in the most disadvantaged neighborhoods at registration. This finding is consistent with the hypothesis and the pattern of program effects reported in the previously published articles, that is the program effect on maternal life-course outcomes was greater for mothers with initially higher psychological resources.

The treatment main effect on government cost is significant, but is greater for the group of mothers with higher psychological resources at registration. Figure 6 shows that the treatment-control difference in government costs is greater for women who lived in more disadvantaged neighborhoods at registration.

Table 8. Regressions of maternal life course outcomes on neighborhood disadvantage for mothers with high psychological resources - specified separately for nurse-visited and control group - Memphis.

High Resource Group				Estimated Y at			
	Tx	Slope	SE	10%[19]	50%[20]	90%[21]	P-value
Rapid Subsequent Births							
• 0-12 Years Interval between first and second birth	Control	-2.75	1.23	41.71	36.93	30.71	.
	Visited	-2.63	1.74	50.35	45.78	39.82	0.956
Employment							
• Year 12 No. Months Employed per yr (0-12 years)	Control	-0.00	0.58	25.70	25.70	25.69	.
	Visited	0.79	0.76	20.91	22.29	24.09	0.402
Use and Cost Welfare							
• Year 0-12 No. of months receiving Food Stamps	Control	9.14	1.32	60.52	76.39	97.05	.
	Visited	6.00	1.77	58.74	69.15	82.72	0.155
• Year 0-12 No. of months receiving Medicaid	Control	7.77	1.36	85.48	98.96	116.51	.
	Visited	5.44	1.82	85.96	95.40	107.69	0.306
• Year 0-12 No. of months receiving AFDC/TANF	Control	8.87	1.51	42.68	58.08	78.13	.
	Visited	7.07	1.99	37.96	50.23	66.22	0.470
• Year 0-12 Cost (Thousands) of Food Stamps	Control	5.23	0.75	27.23	36.30	48.11	.
	Visited	2.79	1.00	25.82	30.66	36.96	0.051
• Year 0-12	Control	4.76	0.71	36.22	44.48	55.24	.

[19] Value of dependent variable at neighborhood disadvantage at lowest 10% of all neighborhood disadvantage for the entire sample
[20] Value of dependent variable at neighborhood disadvantage at median of all neighborhood disadvantage for the entire sample
[21] Value of dependent variable at neighborhood disadvantage at upper 10% of all neighborhood disadvantage for the entire sample

Cost (Thousands) of Medicaid	Visited	2.46	0.95	35.39	39.66	45.21	0.053
• Year 0-12 Cost (Thousands) of AFDC/TANF	Control	6.41	1.28	18.96	30.08	44.56	.
	Visited	3.68	1.68	15.12	21.50	29.81	0.196
• Year 0-12 Total Governmental Costs (Thousands)	Control	16.43	2.57	82.44	110.95	148.08	.
	Visited	8.77	3.45	77.27	92.49	112.31	0.075
Arrests							
• 9 Years No. Maternal Arrests (6-9 yrs)	Control	0.09	0.13	0.09	0.10	0.12	.
	Visited	0.26	0.19	0.07	0.12	0.21	0.444
• 12 Years No. Maternal Arrests	Control	0.17	0.10	0.20	0.27	0.40	.
	Visited	-0.12	0.12	0.70	0.57	0.43	0.073
Substance Use							
• 6 Years Behavioral problems attributable to substance use	Control	-0.64	0.41	0.05	0.02	0.00	.
	Visited	-0.30	0.38	0.05	0.03	0.02	0.551

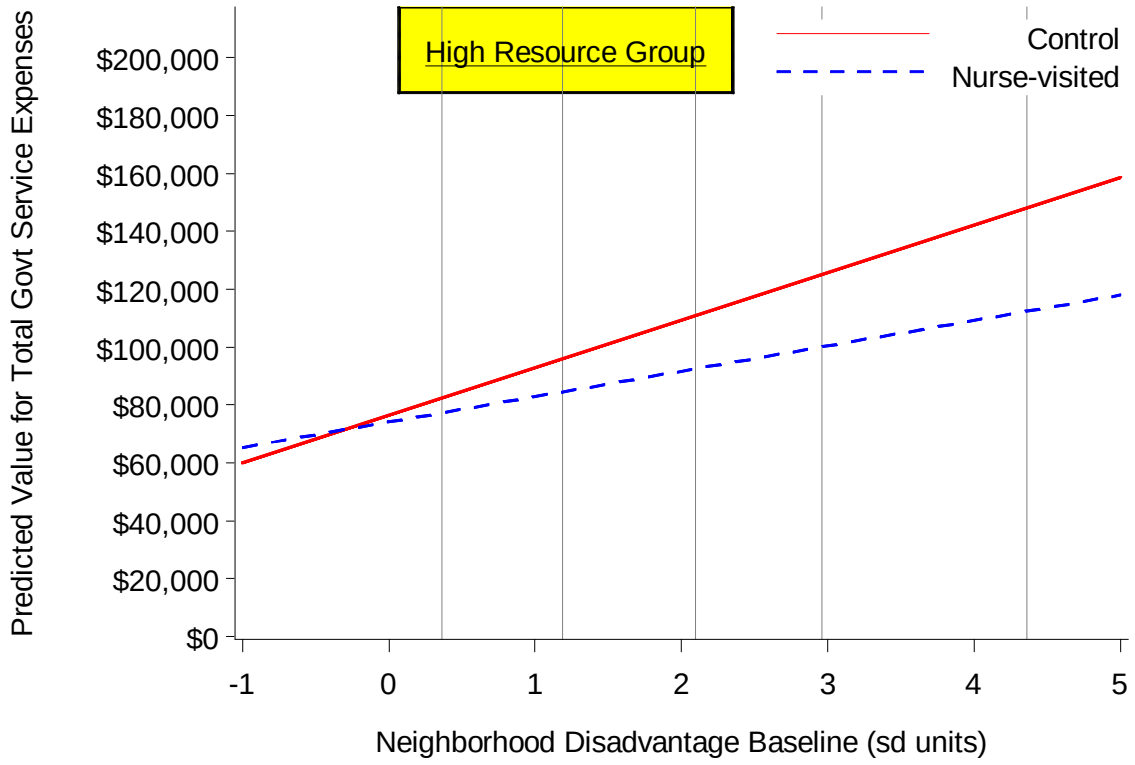

Figure 6. Regressions of total government costs (for food stamps, AFDC/TANF, and Medicaid, birth to child age 12) on neighborhood disadvantage for nurse-visited and control-group families in which the mother had higher psychological resources at baseline – Memphis.

Child outcomes. Table 9 shows the results of the homogeneity of regression analyses of child outcomes on neighborhood disadvantage for the sample as a whole in the Memphis trial. The

regression coefficients for nurse-visited and control group children were significantly different with Internalizing Problems at child age 6 as the dependent variable, but almost entirely because the rates of internalizing problems declined in the nurse-visited group as neighborhood disadvantage increased. There was a similar, but non-significant difference for externalizing problems at child age 6.

As shown in Table 10, there were no meaningful interactions between treatment assignment and neighborhood disadvantage for any of the child outcomes among children born to mothers with low psychological resources.

Table 9. Regressions of child outcomes on neighborhood disadvantage scores - specified separately for nurse-visited and control group – Memphis.

Whole Group				**Estimated Y at**			
	Tx	**Slope**	**SE**	**10%[22]**	**50%[23]**	**90%[24]**	**P-value**
Language/Mental Development							
• Year 2 Bayley Mental Development Index	Control	-1.42	0.37	97.10	94.64	91.43	
	Visited	-0.41	0.50	95.13	94.43	93.51	0.105
• Year 6 KABC Mental Processing Composite	Control	-0.60	0.34	91.81	90.77	89.40	
	Visited	-0.35	0.45	92.73	92.12	91.32	0.653
• Year 6 PPVT-III Receptive Vocabulary	Control	-1.47	0.37	85.50	82.96	79.64	
	Visited	-1.11	0.49	86.10	84.18	81.68	0.556
Injuries							
• Year 0-2 Total No. of health care encounters – injuries/ingestions	Control	0.00	0.05	0.56	0.56	0.57	
	Visited	0.01	0.08	0.44	0.44	0.46	0.950
Behavioral Problems							
• Year 2 Behavior problems total score	Control	0.86	0.67	47.67	49.16	51.11	
	Visited	-0.13	0.91	46.35	46.13	45.83	0.379
• Year 6 – Internalizing problems (borderline/clinical)	Control	0.04	0.08	0.13	0.14	0.15	
	Visited	-1.22	0.54	0.14	0.02	0.00	<.001
• Year 6 Externalizing problems (borderline/clinical)	Control	-0.04	0.08	0.21	0.20	0.18	
	Visited	-0.26	0.13	0.24	0.17	0.10	0.139
• Year 9 Count Depressive & Anxiety Disorders	Control	-0.15	0.10	0.22	0.17	0.13	
	Visited	0.09	0.14	0.09	0.11	0.13	0.180
• Year 9 Count of Disruptive Behavior Disorders (with Impairment)	Control	-0.06	0.06	0.35	0.32	0.28	
	Visited	-0.07	0.08	0.42	0.37	0.31	0.921

[22] Value of dependent variable at neighborhood disadvantage at lowest 10% of all neighborhood disadvantage for the entire sample
[23] Value of dependent variable at neighborhood disadvantage at median of all neighborhood disadvantage for the entire sample
[24] Value of dependent variable at neighborhood disadvantage at upper 10% of all neighborhood disadvantage for the entire sample

• Years 12 Internalizing disorders – self report	Control	0.01	0.07	0.32	0.32	0.32	
	Visited	0.05	0.10	0.22	0.23	0.25	0.721
• Years 12 Externalizing disorders (teacher-, parent-, self-report)	Control	-0.16	0.10	0.22	0.18	0.13	
	Visited	-0.02	0.10	0.21	0.20	0.19	0.351
• Years 12 Total Problems (teacher-, parent-, self-report)	Control	-0.08	0.09	0.22	0.20	0.17	
	Visited	-0.02	0.10	0.25	0.24	0.23	0.647
Academic Achievement							
• Year 6 Arithmetic achievement (KABC)	Control	-1.14	0.37	91.23	89.24	86.66	
	Visited	-1.01	0.50	91.40	89.64	87.35	0.836
• Year 6 Reading achievement (KABC)	Control	-0.96	0.38	95.79	94.12	91.95	
	Visited	-0.61	0.51	94.82	93.75	92.37	0.578
Year 9 – **Average Math and Reading GPA (grades 1-3)**	Control	-0.06	0.03	2.75	2.64	2.50	.
	Visited	-0.06	0.04	2.79	2.69	2.56	0.951
Year 9 **- Average Math and Reading TCAP percentile (grades 1-3)**	Control	-0.24	1.03	42.99	42.58	42.05	.
	Visited	-0.55	1.37	43.83	42.88	41.65	0.856
• Year 12 PIAT Achievement Test Scores (reading and math)	Control	-0.39	0.31	89.22	88.53	87.64	
	Visited	-0.33	0.41	89.58	89.01	88.26	0.902

Table 10. Regressions of child outcomes on neighborhood disadvantage scores for children born to mothers with low psychological resources - specified separately for nurse-visited and control groups - Memphis.

Low Resource Group				**Estimated Y at**			
	Tx	**Slope**	**SE**	**10%[25]**	**50%[26]**	**90%[27]**	**P-value**
Language/Mental Development							
• Year 2 Bayley Mental Development Index	Control	-0.99	0.53	93.51	91.80	89.57	
	Visited	-0.75	0.73	93.95	92.64	90.93	0.797
• Year 6 KABC Mental Processing Composite	Control	-0.75	0.47	89.56	88.27	86.58	
	Visited	-0.28	0.66	90.67	90.19	89.56	0.563
• Year 6 PPVT-III Receptive Vocabulary	Control	-1.06	0.52	81.82	79.98	77.57	
	Visited	-0.92	0.73	82.96	81.37	79.30	0.870
Injuries							
• Year 0-2 Total No. of health care encounters – injuries/ingestions	Control	0.08	0.07	0.58	0.67	0.81	
	Visited	0.02	0.11	0.40	0.42	0.44	0.649
Behavioral Problems							
• Year 2 Behavior problems total score	Control	0.68	0.95	53.06	54.25	55.79	
	Visited	-0.18	1.33	53.24	52.93	52.52	0.596
• Year 6 – Internalizing problems (borderline/clinical)	Control	0.10	0.11	0.14	0.16	0.19	
	Visited	-0.16	0.16	0.27	0.22	0.16	0.162
• Year 6 Externalizing problems (borderline/clinical)	Control	-0.01	0.10	0.25	0.25	0.24	
	Visited	-0.21	0.16	0.30	0.23	0.16	0.279
• Year 9 Count Depressive & Anxiety Disorders	Control	0.10	0.11	0.18	0.21	0.27	
	Visited	-0.14	0.21	0.19	0.15	0.11	0.289

[25] Value of dependent variable at neighborhood disadvantage at lowest 10% of all neighborhood disadvantage for the entire sample
[26] Value of dependent variable at neighborhood disadvantage at median of all neighborhood disadvantage for the entire sample
[27] Value of dependent variable at neighborhood disadvantage at upper 10% of all neighborhood disadvantage for the entire sample

• Year 9 Count of Disruptive Behavior Disorders (with Impairment)	Control	-0.05	0.09	0.37	0.34	0.30	
	Visited	-0.13	0.12	0.45	0.37	0.28	0.621
• Years 12 Internalizing disorders – self report	Control	-0.07	0.09	0.40	0.37	0.33	
	Visited	0.04	0.13	0.30	0.31	0.33	0.485
• Years 12 Externalizing disorders (teacher-, parent-, self-report)	Control	-0.06	0.11	0.24	0.22	0.19	
	Visited	-0.08	0.14	0.27	0.24	0.21	0.911
• Years 12 Total Problems (teacher-, parent-, self-report)	Control	-0.03	0.10	0.28	0.26	0.25	
	Visited	0.04	0.13	0.28	0.29	0.31	0.660
Academic Achievement							
• Year 6 Arithmetic achievement (KABC)	Control	-1.04	0.53	88.06	86.25	83.89	
	Visited	-0.93	0.74	90.05	88.44	86.33	0.902
• Year 6 Reading achievement (KABC)	Control	-0.91	0.53	93.15	91.57	89.51	
	Visited	-0.61	0.75	93.08	92.03	90.66	0.738
Year 9 – **Average Math and Reading GPA (grades 1-3)**	Control	-0.09	0.04	2.63	2.48	2.28	.
	Visited	-0.09	0.05	2.74	2.58	2.38	0.942
Year 9 **- Average Math and Reading TCAP percentile (grades 1-3)**	Control	0.82	1.33	35.81	37.23	39.08	.
	Visited	-0.74	1.77	43.71	42.42	40.74	0.480
• Year 12 PIAT Achievement Test Scores (reading and math)	Control	-0.47	0.43	86.51	85.69	84.62	
	Visited	-0.43	0.58	88.81	88.07	87.10	0.949

Overall, results from the Memphis trial provide some support for the hypothesized pattern of moderated program impact depending upon neighborhood disadvantage at baseline for maternal outcomes, especially those related to government costs related to welfare, but lack consistency for child outcomes.

Denver Trial

Maternal Outcomes. Table 11 presents the results of the homogeneity of regression analyses for maternal outcomes for the whole sample in the Denver trial. There were no treatment differences in the regressions of maternal outcomes on neighborhood disadvantage that were consistent with the hypothesis. There were differences in regressions for months that women used TANF and Medicaid, but the regression lines did not conform to the predicted pattern.

Child Outcomes. Table 12 shows the results of the homogeneity of regression analyses of child outcomes on neighborhood disadvantage for the whole sample in Denver. Again, the pattern of results was not consistent with the hypothesis. Table 13 shows the corresponding set of homogeneity of regression analyses for children born to women with low psychological resources.

For this set of outcomes, the pattern is consistent for language, mental development, and most academic achievement outcomes. For children born to low-resource mothers, consistent with the hypothesis, the treatment-control difference increased as the level of neighborhood disadvantage increased. The regressions are significantly different (or nearly so) for language development at child age 21 months, the 2-year Bayley mental development index, the year 6, KABC mental processing composite, and the Year 6 PPVT (receptive language score). While the regression coefficients are not significantly different for nurse-visited and control group children in academic achievement, it is clear that the treatment-control differences in achievement test scores in reading and math at age 6 and reading at age 9 are clinically significant at higher levels of initial neighborhood disadvantage. These differences are due primarily to declines in the performance of children born to control-group mothers with low psychological resources as levels of neighborhood disadvantage increase. Figures 7 and 8 illustrate this pattern of results for children's language development measured at 21 months and their receptive vocabulary scores (Peabody Picture Vocabulary – PPVT) scores at age 6. We show the fitted regressions for the paraprofessional visited group to illustrate the pattern for all three treatment groups.

The analyses of the program as a moderator of neighborhood disadvantage in the Denver trial did not find effects that were consistent with the hypothesis for maternal life-course outcomes, but did find effects that were consistent with the hypothesis for children born to low-resource mothers. The program appears to have buffered children born to mothers with few psychological resources from the damaging effects of concentrated disadvantage in their neighborhoods.

Table 11. Regressions of maternal life course outcomes on neighborhood disadvantage scores - specified separately for nurse-visited and control group - Denver.

Whole Group				Estimated Y at			
	Tx	Slope	SE	10%[28]	50%[29]	90%[30]	P-value
Rapid Subsequent Births							
• Years 0-9 Interval between first and second birth 0-9 years (months)	Control	2.07	2.13	40.33	41.75	44.49	.
	Visited	-2.34	2.09	49.58	47.98	44.90	0.139
EMPLOYMENT – USE Y09 TOTAL DATA? TOTAL EARNINGS FROM ECO ANALYSIS?							
• Year 2 Employed (13-24 months)	Control	-0.65	0.38	6.88	6.44	5.58	
	Visited	-1.29	0.36	8.36	7.48	5.77	0.225
• Years 2-3 No. Months Mother Employed (25-48 Months)	Control	-0.95	0.68	14.67	14.02	12.76	
	Visited	-0.92	0.63	15.13	14.50	13.29	0.970
• Years 4-5 No. months/yr Mother employed (49-72 Months)	Control	-1.49	0.72	16.50	15.48	13.52	
	Visited	0.03	0.67	16.29	16.31	16.35	0.122
• Years 6-9 No. months/yr Mother employed (73-108 Months	Control	-0.61	1.15	23.07	22.65	21.84	
	Visited	-0.63	1.10	22.65	22.21	21.38	0.990
Use of Welfare – condense over all years using data from Y09 report							
• Year 1 On AFDC (1-12 months)	Control	0.76	0.33	1.40	1.92	2.92	
	Visited	0.91	0.31	1.32	1.94	3.14	0.744
• Years 2-3 No. Months on AFDC (25-48 Months)	Control	0.22	0.44	1.76	1.91	2.20	
	Visited	0.45	0.41	1.40	1.71	2.30	0.699
• Years 4-5 No. Months on AFDC (49-72 Months)	Control	0.22	0.29	0.58	0.74	1.03	
	Visited	0.18	0.27	1.05	1.17	1.40	0.915
• Years 6-9 No. Months on AFDC (73-108 Months)	Control	1.21	0.55	0.23	1.06	2.65	
	Visited	-0.73	0.53	2.20	1.70	0.74	0.011
• Years 2-3 No. Months on Food Stamps (25-48 Months)	Control	0.98	0.62	2.92	3.59	4.89	
	Visited	1.19	0.57	3.13	3.95	5.51	0.807
• Years 4-5 No. Months on Food Stamps (49-72 Months)	Control	0.29	0.60	2.88	3.07	3.45	
	Visited	0.71	0.55	2.55	3.04	3.97	0.604
• Years 6-9 No. Months on Food Stamps (73-108 Months)	Control	2.73	1.05	4.80	6.67	10.27	
	Visited	3.65	1.00	4.29	6.79	11.61	0.525
• Years 2-3 No. Months on Medicaid (25-48 Months)	Control	0.14	0.77	6.86	6.96	7.14	
	Visited	2.81	0.71	4.50	6.43	10.14	0.011
• Years 4-5 No. Months on Medicaid (49-72 Months)	Control	1.98	0.82	5.57	6.92	9.53	
	Visited	1.26	0.75	5.86	6.72	8.38	0.518
• Years 6-9 No. Months on Medicaid (73-108 Months)	Control	3.12	1.25	9.34	11.48	15.60	
	Visited	4.16	1.20	7.33	10.18	15.67	0.549
Substance Use							
• Year 4 Any Behavioral Problems Due to Substance Use	Control	0.18	0.23	0.11	0.13	0.16	
	Visited	-0.18	0.22	0.19	0.17	0.14	0.260
• Year 6 Any Behavioral Problems Due to Substance Use	Control	-0.18	0.29	0.12	0.11	0.09	
	Visited	-0.13	0.29	0.10	0.09	0.08	0.902
• Year 9 Any Behavioral Problems Due to Substance Use	Control	-0.16	0.25	0.17	0.15	0.13	
	Visited	-0.26	0.30	0.13	0.11	0.08	0.803

[28] Value of dependent variable at neighborhood disadvantage at lowest 10% of all neighborhood disadvantage for the entire sample
[29] Value of dependent variable at neighborhood disadvantage at median of all neighborhood disadvantage for the entire sample
[30] Value of dependent variable at neighborhood disadvantage at upper 10% of all neighborhood disadvantage for the entire sample

Table 12. Regressions of child outcomes on neighborhood disadvantage scores - specified separately for nurse-visited and control groups – Denver.

Whole Group				Estimated Y at			
	Tx	Slope	SE	10%[31]	50%[32]	90%[33]	P-value
Language/Mental Development							
• Month 21 Language Development	Control	-1.74	0.92	102.07	100.88	98.59	
	Visited	-0.32	0.87	101.63	101.41	100.98	0.265
• Year 2 Mental Development Index	Control	-2.42	0.97	92.60	90.93	87.73	
	Visited	-0.27	0.95	90.52	90.34	89.98	0.112
• Year 4 Total Language Score	Control	-3.67	1.15	95.68	93.17	88.33	
	Visited	-1.19	1.08	94.15	93.34	91.77	0.116
• Year 4 Executive Function Composite	Control	-1.48	0.78	101.20	100.18	98.22	
	Visited	-0.83	0.73	101.58	101.02	99.92	0.541
• Year 6 KABC Mental Processing Composite	Control	-2.99	0.86	100.84	98.79	94.84	
	Visited	-3.14	0.84	101.65	99.50	95.35	0.896
• Year 6 Receptive Language PPVT	Control	-2.99	0.95	98.45	96.40	92.46	
	Visited	-2.44	0.93	98.32	96.65	93.43	0.677
Behavioral Problems							
• Month 6 Irritable Temperament	Control	-0.01	0.07	2.82	2.82	2.81	
	Visited	0.12	0.06	2.65	2.73	2.89	0.169
• Year 2 Behavior Problems Score	Control	1.60	1.56	42.51	43.61	45.71	
	Visited	1.92	1.40	40.98	42.30	44.82	0.880
• Year 4 Behavioral Regulation in Testing	Control	-1.13	0.79	100.87	100.10	98.62	
	Visited	0.63	0.73	99.04	99.47	100.31	0.103
• Year 4 Emotional Regulation in Testing	Control	-0.98	0.79	100.54	99.86	98.57	
	Visited	0.21	0.73	99.47	99.61	99.89	0.270
• Year 4 Externalizing Behavior Problems	Control	-0.19	0.56	12.37	12.24	11.99	
	Visited	0.28	0.53	11.96	12.15	12.52	0.540
• Year 6 Achenbach Internalizing	Control	0.08	0.18	0.28	0.30	0.32	
	Visited	-0.08	0.19	0.30	0.29	0.27	0.538
• Year 9 Achenbach Internalizing	Control	0.05	0.18	0.37	0.38	0.39	
	Visited	0.03	0.18	0.36	0.36	0.37	0.961
Academic Achievement							
• Year 6 KABC Reading, Decoding	Control	-1.94	1.02	99.07	97.74	95.18	
	Visited	-2.99	0.99	101.14	99.09	95.15	0.463
• Year 6 KABC Arithmetic	Control	-2.45	0.95	97.42	95.74	92.51	
	Visited	-2.68	0.92	98.08	96.24	92.71	0.863
• Year 9 PIAT Reading, Decoding	Control	-2.93	1.02	102.71	100.69	96.82	
	Visited	-2.73	0.98	101.75	99.88	96.28	0.885
• Year 9	Control	-1.53	0.94	100.88	99.83	97.82	

[31] Value of dependent variable at neighborhood disadvantage at lowest 10% of all neighborhood disadvantage for the entire sample
[32] Value of dependent variable at neighborhood disadvantage at median of all neighborhood disadvantage for the entire sample
[33] Value of dependent variable at neighborhood disadvantage at upper 10% of all neighborhood disadvantage for the entire sample

PIAT Arithmetic							
	Visited	-2.21	0.90	101.00	99.48	96.56	0.598

Table 13. Regressions of child outcomes on neighborhood disadvantage scores for children born to mothers with low psychological resources - specified separately for nurse-visited and control groups - Denver.

Low Resource Group				Estimated Y at			
	Tx	Slope	SE	10%[34]	50%[35]	90%[36]	P-value
Language/Mental Development							
• Month 21 Language Development	Control	-2.44	1.57	99.54	97.87	94.66	
	Visited	1.41	1.20	99.61	100.58	102.43	0.053
• Year 2 Mental Development Index	Control	-1.85	1.73	87.87	86.60	84.16	
	Visited	1.58	1.37	87.83	88.91	91.00	0.120
• Year 4 Total Language Score	Control	-1.31	1.94	88.46	87.56	85.83	
	Visited	2.21	1.46	88.55	90.07	92.99	0.148
• Year 4 Executive Function Composite	Control	0.42	1.32	95.19	95.48	96.03	
	Visited	0.98	1.00	98.81	99.48	100.78	0.737
• Year 6 KABC Mental Processing Composite	Control	-4.20	1.51	98.54	95.66	90.13	
	Visited	-1.12	1.16	97.42	96.65	95.17	0.106
• Year 6 Receptive Language PPVT	Control	-4.38	1.70	95.24	92.24	86.46	
	Visited	-0.20	1.30	93.80	93.67	93.41	0.052
Behavioral Problems							
• Month 6 Irritable Temperament	Control	0.02	0.14	2.94	2.95	2.98	
	Visited	0.12	0.09	2.75	2.83	2.99	0.534
• Year 2 Behavior Problems Score	Control	3.76	2.80	46.31	48.89	53.84	
	Visited	2.91	2.12	45.18	47.17	51.00	0.808
• Year 4 Behavioral Regulation in Testing	Control	-1.85	1.50	98.36	97.09	94.65	
	Visited	1.37	1.12	98.53	99.47	101.29	0.086
• Year 4 Emotional Regulation in Testing	Control	-1.34	1.41	99.63	98.71	96.93	
	Visited	0.64	1.05	98.72	99.15	100.00	0.261
• Year 4 Externalizing Behavior Problems	Control	-1.43	1.14	14.72	13.74	11.85	
	Visited	0.40	0.88	13.05	13.33	13.86	0.203
• Year 6 Achenbach Internalizing	Control	-0.01	0.33	0.33	0.33	0.33	
	Visited	-0.06	0.25	0.35	0.35	0.33	0.907
• Year 9 Achenbach Internalizing	Control	0.42	0.40	0.33	0.40	0.53	
	Visited	0.09	0.26	0.39	0.40	0.43	0.481
Academic Achievement							
• Year 6 KABC Reading, Decoding	Control	-2.30	1.78	95.29	93.71	90.68	
	Visited	-0.09	1.37	95.52	95.46	95.35	0.325
• Year 6 KABC Arithmetic	Control	-2.54	1.73	95.03	93.29	89.94	
	Visited	-0.25	1.33	94.53	94.36	94.03	0.295
• Year 9 PIAT Reading, Decoding	Control	-2.14	1.98	96.43	94.96	92.14	
	Visited	0.47	1.45	96.38	96.71	97.33	0.289
• Year 9 PIAT Arithmetic	Control	-0.94	2.01	96.90	96.25	95.01	
	Visited	-0.18	1.47	96.54	96.41	96.17	0.761

[34] Value of dependent variable at neighborhood disadvantage at lowest 10% of all neighborhood disadvantage for the entire sample
[35] Value of dependent variable at neighborhood disadvantage at median of all neighborhood disadvantage for the entire sample
[36] Value of dependent variable at neighborhood disadvantage at upper 10% of all neighborhood disadvantage for the entire sample

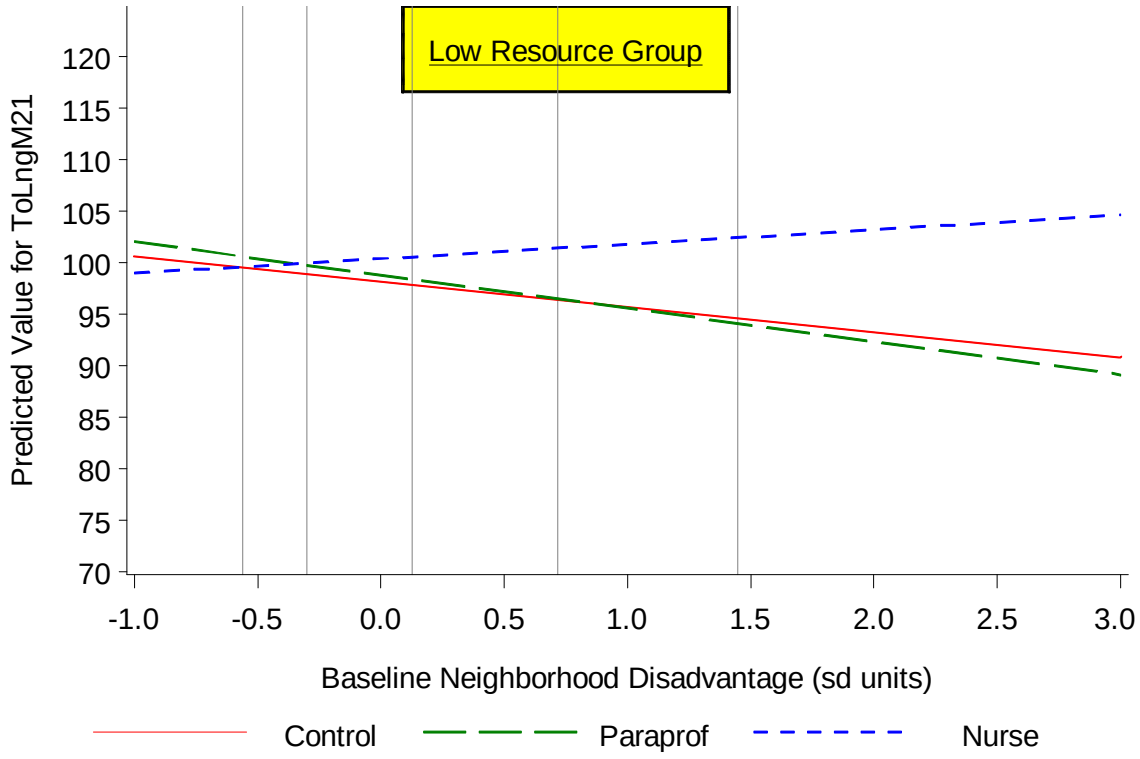

Figure 7. Regressions of 21-month Preschool Language scores on neighborhood disadvantage index by Treatment for children born to mothers with low psychological resources

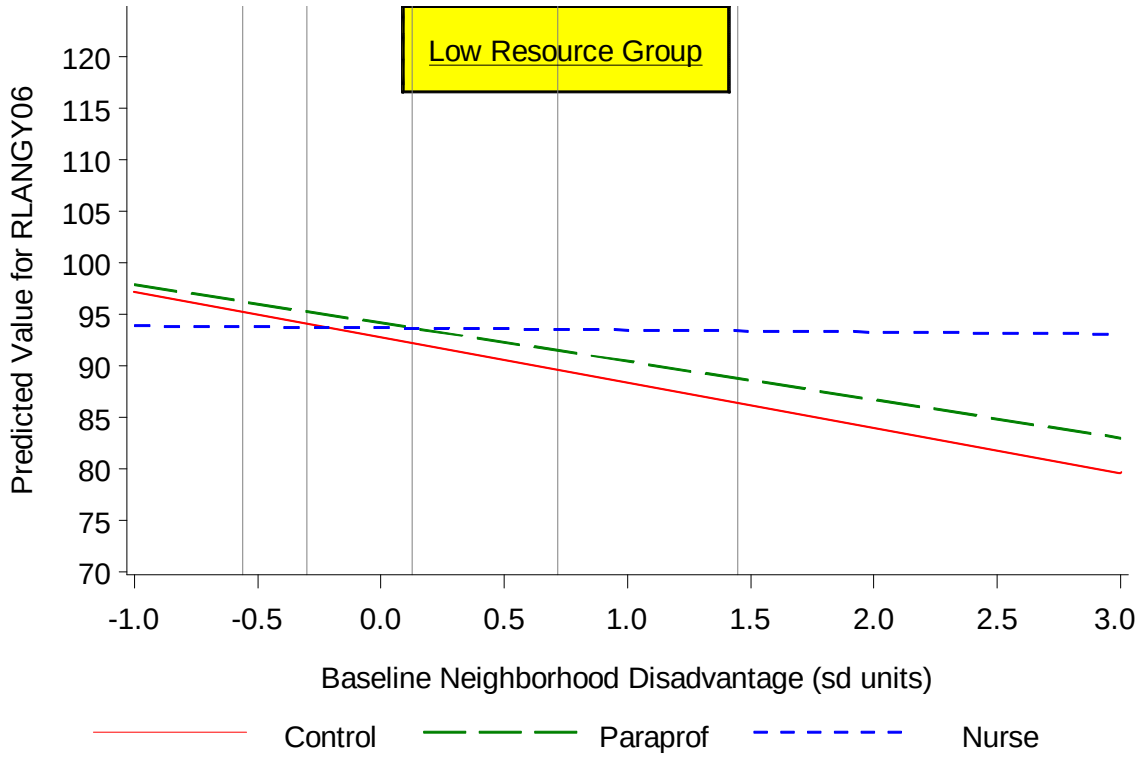

Figure 8. Regressions of 6-year Peabody Picture Vocabulary scores on neighborhood disadvantage index by Treatment for children born to mothers with low psychological resources

Discussion

Our analyses of neighborhood disadvantage as a mediator and moderator of program effects found in the three scientifically controlled trials of the Nurse-Family Partnership need to be understood in the context of widely differing levels of neighborhood disadvantage in the these trials. Neighborhood social disadvantage was substantially more concentrated in the Memphis trial than in Elmira and Denver. Elmira is a small city in Chemung County, a semirural community in the Appalachian Region of New York State. The Denver trial was conducted in the Denver metropolitan area, which includes the city of Denver and surrounding metropolitan counties. Participants in the Memphis trial lived almost exclusively in the city of Memphis. The characteristics of participants' neighborhoods in Elmira and Denver were quite similar to US national averages. The neighborhoods of participants in the Memphis trial tended to be more racially segregated and to have high rates of concentrated poverty and social disadvantage. Our examination of NFP effects on changes in participants' neighborhoods over time and of NFP moderation of neighborhood impacts on maternal and child health need to be understood in the context of these widely differing settings.

No Program Impact on Living in Better Neighborhoods

We were surprised to find that nurse-visited women did not move to better neighborhoods over time, given nurse-induced improvements in maternal life-course and economic self-sufficiency in each of the trials. Perhaps the gains in self-sufficiency were simply insufficient to support relocation to better neighborhoods. It also is possible that some of the apparent changes in neighborhood disadvantage over time may reflect changes in the composition of neighborhoods, not families' moving.

It is possible that the higher rates of neighborhood disadvantage found for nurse-visited low-resource mothers in the Memphis trial reflect nurse-visited families moving into public housing more frequently than their counterparts in the control group. Nurses have reported anecdotally that one of the most fundamental issues with which they help families is finding stable housing, including gaining access to public housing, where concentrations of poverty, disadvantage, and racial

segregation are high. Finding stable housing may have taken precedence over neighborhood safety. It also is possible that nurse-visited low-resource mothers were encouraged to live with or closer to their families of origin.

Both nurse-visited and control low-resource mothers in Memphis lived in more disadvantaged neighborhoods by child age 5 than they did at registration; nurse-visited low-resource mothers' neighborhoods were even more disadvantaged by child age 6, although their control counterparts did not. Thereafter, both groups began to decline in neighborhood disadvantage through child age 12. Nurse-visited mothers with higher psychological resources lived in neighborhoods with more concentrated social disadvantage at intake than did their control-group counterparts, but thereafter lived in neighborhoods with levels of disadvantage similar to those in the control group.

We have no explanation at this time for the nurse-visited lower risk families in Elmira to have moved to more disadvantaged neighborhoods by the child's 15th birthday.

Finally, both nurse-visited and control group families in Denver moved to neighborhoods with lower levels of disadvantage through the child's 9th birthday. There were no nurse-control differences in levels of neighborhood disadvantage at any point in time in the Denver trial. By child age 9, families in the Denver trial lived in neighborhoods with slightly lower concentrations of disadvantage than the US national average.

Neighborhood Conditions Do Not Mediate NFP Program Effects

As noted above, given that there were no consistent intervention-control differences in neighborhood disadvantage scores over time, there is almost no way that differences in neighborhood disadvantage may have mediated the beneficial effects of the NFP on maternal and child outcomes. The one possible exception to this may be traced to the observation that lower-risk families in the Elmira trial lived in worse neighborhoods at child age 15 than their control group counterparts and that nurse-visited families headed by mothers with low psychological resources lived in worse neighborhoods once the first-born child entered elementary school than did their control-group counterparts. Evidence from the Moving to Opportunity (MTO) trial indicated that adolescent boys whose families moved to better neighborhoods had higher rates of property crime

than their control group counterparts once they reached mid-adolescence, an effect attributable to higher rewards for stealing. It is possible that some of the beneficial effects of the Memphis program for children born to low-resource mothers may be attributable to their boys staying in worse neighborhoods where the opportunities for more rewarding returns on their criminal activities may be reduced. This is unlikely, however, as boys in the New York City site of the MTO trial who moved to better neighborhoods experienced fewer mental health problems at younger ages, and there were beneficial effects of the NFP on internalizing problems overall and on language and academic achievement for children born to low-resource mothers at earlier ages. We have not yet examined the rates of crime among adolescents enrolled in the Memphis trials, but there is no strong or coherent additional benefit of the Memphis program for boys compared to girls through age 12.

NFP Moderates Some of the Adverse Effects of Neighborhood Disadvantage on Maternal and Child Functioning

As noted above, there are two outcome domains within specific trials where there is coherent evidence that the program moderated the adverse effects of concentrated neighborhood disadvantage: in maternal use of welfare (and associated costs) in the Memphis trial, especially for mothers with initially higher levels of psychological resources; and in outcomes for children born to mothers with low psychological resources in the Denver trial. In both of these outcome domains, the program was found to produce consistent effects in earlier analyses. Adding neighborhood disadvantage to the analysis helped us understand more completely for whom the intervention was most effective.

In the Memphis trial, increased neighborhood disadvantage was associated with increased use of welfare and government costs in the control group. The moderating impact of the intervention was particularly pronounced among mothers with higher psychological resources at intake (higher intellectual functioning, fewer symptoms of depression and anxiety, and better sense of control over one's life). Given that most of the neighborhoods in the Memphis trial were at the extreme end of disadvantage, that is, there is limited range; it is remarkable that such moderation is detectable.

In the Denver trial, children born to mothers with low psychological resources, irrespective of neighborhood disadvantage, exhibited better behavior and development than their control-group

counterparts through child age 4.[8,9] Some of the benefits of the intervention on the functioning of children born to low-resource mothers were attenuated, however, once they entered school,[12] a pattern of results that differs from corresponding effects in the Memphis trial, where benefits endured for children born to low-resource mothers through child age 12. Some of the limited detection of corresponding effects in Denver is likely due to limited statistical power, but adding neighborhood disadvantage to the analysis helps clarify the discrepancy between the two trials. The analysis of the Denver data indicates that when children born to low-resource mothers lived in neighborhoods with levels of disadvantage comparable to those found in Memphis, the program effects in Denver were at least as large as those in Memphis. The difference, of course, is that far fewer families in the Denver trial lived in deeply impoverished neighborhoods, which undermines the stability of treatment effects in highly impoverished neighborhoods.

There were many outcomes for which there was no indication that the NFP moderated neighborhood disadvantage, yet some findings reported here are consistent with the hypothesis that program effects are more pronounced in neighborhoods characterized by dense concentrations of disadvantage. This moderation of program effects supports the policy contained in both the House and Senate versions of healthcare reform currently in reconciliation, which call for focusing evidence-based home visiting programs on communities with the highest concentrations of poverty and disadvantage.

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