

Estimated Effect of Asthma Case Management Using Propensity Score Methods

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The National Asthma Control Program of the Centers for Disease Control and Prevention was created in 1999 with the goal of reducing the impact of asthma. The program has spent almost \$208 million¹ on 2 intervention models: the Inner-City Asthma Intervention and the Controlling Asthma in American Cities Project. The Inner-City Asthma Intervention was located at 23 sites. The programs all had asthma educators who were based in health organizations that treated low-income inner-city children. The Controlling Asthma in American Cities Project also was based in low-income urban areas, but each intervention was locally developed and varied in its design and components. It was implemented in 7 cities. However, there has been no rigorous evaluation of these investments to determine whether they represent the best use of available funds to reduce asthma morbidity.

Whether asthma interventions can shift behavior from treatment of asthma attacks to prevention of attacks through risk-reducing investments remains unclear.² Few interventions are designed using a methodology to identify the change in health outcomes attributable to the intervention. An extensive review of program assessments found that of 223 asthma programs, 1% used a matched comparison, 35% used a randomized controlled trial, and the rest did not use a methodology to estimate their treatment effect.³ Furthermore, assessments that did not use a statistical methodology were more likely to find improved outcomes than those that did, suggesting potential positive bias.

Conducting randomized controlled trials for asthma interventions may be unrealistic.^{4,5} However, we addressed this issue by matching participants in an asthma intervention to comparable nonparticipants and measuring changes in behaviors and outcomes attributable to participation. We used this method to evaluate the case management program Oakland Kicks Asthma (OKA), a large-scale, multifaceted intervention that is part of the Controlling Asthma in American Cities Project.

Oakland Kicks Asthma was developed by the American Lung Association of the East Bay and researchers at the UC Berkeley School of Public Health. Oakland, California, is a highly diverse city, whose population is 31% white, 36% black, 15% Asian, and 22% Hispanic⁶; 29% of children live in poverty.⁷ The annual rate of asthma-related hospitalizations for children under age 15 years is twice that for the state (36.63 vs 18.73 per 10,000 residents).

Objective: To estimate the treatment effect of participation in an asthma intervention that was part of the National Asthma Control Program.

Study Design: Cross-sectional; difference in outcomes between participants and comparable nonparticipants matched by using propensity scores.

Methods: Data on children who participated in asthma case management (n = 270) and eligible children who did not participate in case management (n = 2742) were extracted from a Medicaid claims database. We constructed measures of healthcare utilization, sociodemographics, and neighborhood characteristics. After creating a comparison group similar to the participants in terms of all characteristics before participation, we estimated the effect of the program on asthma outcomes.

Results: Participants were more likely to have vaccinations for pulmonary illness (95% confidence interval [CI] = 1.82, 4.81), to fill a prescription for controller medications (95% CI = 1.07, 2.19), and to have a refill for rescue medication (95% CI = 1.07, 2.07) after the program than comparable nonparticipants. There was no statistically significant difference in the number of nebulizer treatments or emergency department visits between the 2 groups.

Conclusions: The program did increase the use of preventive healthcare by participants. Over the time period we studied, these behaviors did not decrease healthcare utilization for asthma exacerbations. We were unable to discern whether the lack of effect was because of the nature of the program, heterogeneity of the effects, or barriers outside the program's control.

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Take-Away Points

The treatment effects of Oakland Kicks Asthma, an asthma case management program, were evaluated.

- Oakland Kicks Asthma improved preventive behavior in the short run, but a longer run study may be needed to observe decreases in adverse events.
- Administrative claims data can be used for a methodologically rigorous evaluation of program benefits.
- The expected benefits of interventions based on the National Cooperative Inner-City Asthma Study may not be realized in other urban populations.

Asthmatic children aged 6 to 18 years were referred to OKA by physicians or the local Medicaid provider, or were identified by school district surveillance surveys. Their families were invited to participate in the program, and interested families were assigned an asthma care manager trained by American Lung Association staff. Over a 3- to 5-month period, each asthma care manager completed 3 or 4 home visits per student. At the first visit the asthma care manager collected measures of symptom frequency, exposure to indoor air triggers, current asthma management and behaviors, and attitudes. The asthma care manager then developed a personalized, one-on-one education plan to develop the family's skills in reducing and avoiding asthma triggers, using Asthma Action Plans and peak flow meters to respond promptly to changes in symptom severity, and obtaining related health and social services. The program was free to all participants. The program, data collection tools, and all protocols for protection of personal data were approved by the Committee for the Protection of Human Subjects, UC Berkeley. Informed consent was obtained for all OKA participants.

METHODS**Data**

To estimate the changes in outcomes attributable to OKA, we compared outcomes for participants and for a similar group of nonparticipants using a matching methodology.⁸⁻¹⁰ Our data come from the claims database of Alameda Alliance for Health (AAH), a Medicaid umbrella organization in Alameda County. To construct our sample of participants and nonparticipants, we examined all AAH plan members who in 2003-2007 had a health claim with a primary diagnosis code for asthma (*International Classification of Diseases, Ninth Revision [ICD-9]* code 493.x) and were age 6 to 20 years at the time of service. We divided this group into OKA participants (n = 282) and nonparticipants (n = 8307). Of the nonparticipants, we retained records for those who had an Oakland residence, were born between 1985 and 2000, had not participated in any other asthma program, did not have a diagnosis of a confounding medical condition, and had complete

records for primary language, race/ethnicity, and residence, for a total of 2742 nonparticipants. Our exclusion process is outlined in **eAppendix A**, available at www.ajmc.com.

We created an inventory of health-care utilization measures based on the billing codes for asthma-relevant health services provided the year before and the year after OKA (summarized in **eAppendix B**). In addition to healthcare encounters, we identified asthma-related prescription drug claims, distinguishing between rescue medications (to treat asthma exacerbations) and controller medications (preventive medication). Because glucocorticoids are not limited to asthma treatment, we included a prescription for glucocorticoids only if it occurred in conjunction with an asthma diagnosis. We did not include pulmonary testing because claims data cannot be used to determine whether the testing was used to treat an exacerbation or as a preventive measure. For each class of medication, we created both binary and count variables, as well as indicators for excessive use (**eAppendix C**). These indicators were based on recommended doses and prescription volumes. Information on doses per canister was compiled using a MEDLINE search on each prescription inhaler. (Documentation is available from the authors upon request.)

To assess continuity of care, we counted the number of different primary care providers for all asthma claims within 12 months before the program start date, as well as an indicator variable for no primary care provider assigned. We also counted encounters with pulmonologists, allergists, or any respiratory specialists. Lastly, we created an indicator for whether the child had ever had a claim with an allergy diagnosis.

The eligibility files for AAH members provided 11 socio-demographic variables, including race/ethnicity, language, age, type of health plan and group, and number of people and number of adults on the health plan. (A complete list of the procedure and billing codes used to create each variable is available from the authors upon request.) Finally, we geocoded our subjects to the 2000 US Census tract level to describe each child's neighborhood composition. We considered 20 measures including age distribution, country of birth, family structure, employment status, and income characteristics.

Methodology for Estimating Treatment Effects

An advantage of a randomized controlled trial is that it facilitates estimation of the treatment effect. However, randomization into treatment and control groups is generally not feasible for community-level interventions that are focused on individual behaviors; therefore, researchers must

use methodologies that can discern whether changes in outcomes are attributable to the intervention.¹¹ We compared changes in outcomes for a participant group and a comparison (nonparticipant) group that was similar to the participant group at the start of the program.¹⁰

Constructing the comparison group was challenging, because there were many attributes that might have differed between participants and nonparticipants. Furthermore, it was statistically inefficient to match each participant to one nonparticipant such that the pair was equivalent on each attribute.^{12,13} Instead, we created a comparison group of nonparticipants that resembled the participant group across all pretreatment characteristics and utilization variables. To create this comparison group, we constructed subsets of participants and nonparticipants such that, within each subset, the average profile of nonparticipants was equivalent to that of the matched subset of participants.^{14,15}

Estimation of Probability of Participation

First we estimated each individual's likelihood of participating in the program by regressing actual participation against variables measuring pretreatment healthcare utilization, continuity of care, and socioeconomic status. The fitted values from this regression gave us each individual's propensity score, which collapsed all of the attributes that were predictive of program participation into 1 measure.¹⁶ Identifying the model that best predicts participation was challenging, because there were more than 80 potential explanatory variables (see eAppendix B). Furthermore, all else being equal, a parsimonious model was preferable, because the robustness and precision of our models increased with the number of participants per explanatory variable.¹⁷ Therefore, we used the cross-validation–based Deletion/Substitution/Addition algorithm to identify the combination of independent variables that best predicted the empirical distribution of participation.¹⁸ (In the model selected by the Deletion/Substitution/Addition algorithm, we had 27 positive outcomes for OKA participation per explanatory variable. The Deletion/Substitution/Addition procedure is publicly available as an R package at <http://www.stat.berkeley.edu/~laan/Software/>. We allowed for up to 15 terms with a maximum of 3 interactions and a maximum power of 3 for each model.) The resulting model had the following as independent variables: number of control medications in the past 6 months, a binary indicator for any control medication in the past year, the square and the cube of the number of emergency department (ED) or urgent care visits for asthma in the past 6 months, and the square of the number of outpatient visits for asthma in the past year. The logistic regression results are reported in eAppendix D (available at www.ajmc.com).

Initial Characteristics Without Matching

Table 1 illustrates how demographics and preintervention healthcare utilization differed between participants and nonparticipants. The participant group had slightly more Hispanic children (and more Spanish speakers) and slightly fewer black and Asian children than the nonparticipant group, perhaps because of recruiting efforts in a local clinic targeting Spanish-speaking families. There was a substantially higher rate of allergy diagnosis among participants. Participants appear to have been heavier users of healthcare; they were more likely to have had an ED visit, inpatient hospitalization, outpatient visits, a prescription for systemic steroids, high medication intensity for both rescue and controller medications, and preventive care such as flu or pneumonia vaccinations or pulmonary testing.

Panel A of the Figure shows the distribution of the fitted values of the logistic regression for participation—the propensity score. There was little overlap between participants and nonparticipants, confirming that the 2 groups differed before the program.

Ideally, the intervention would be designed so that there was a group of nonparticipants who were similar to participants before the intervention, thus facilitating matching and decreasing the possibility of introducing bias.¹⁹ In our case, there were no barriers to entering the program and the intent was to reach all eligible children. Strategies to deal with this limitation include allowing for variable numbers of matches,²⁰ optimal matching that minimizes distances between groups,²¹ using network flows for full matching,²² or integrating all of these approaches into optimal full matching.¹⁴ We applied this last approach.

Matching on Propensity Score

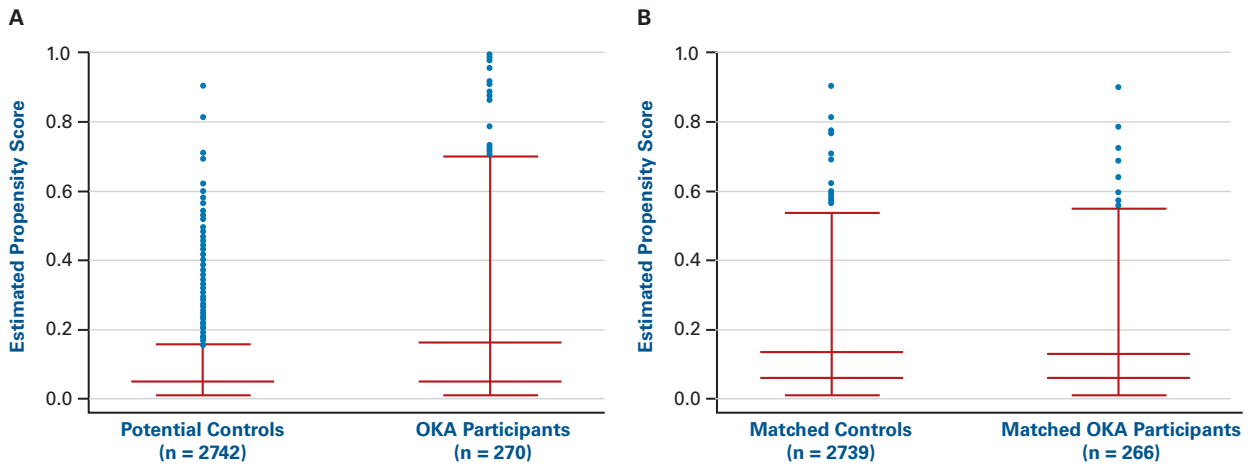
We calculated the difference in propensity scores between each participant–nonparticipant pair (the Mahalanobis distances). Subsets of participants were matched to subsets of nonparticipants to minimize these differences. Our strategy was denoted an M to K optimal full match, because it used the full sample of participants and nonparticipants. This method can produce significantly better matching between the 2 groups than methods that match 1 participant to 1 nonparticipant.¹⁴ (This matching program is publicly available in the “optmatch” package in R [<http://www.r-project.org/>]. We tried several optimal matching combinations to verify that our results were not dependent on the matching technique. Because optimal full matching is preferred on statistical grounds, we limited our presentation to only those results, but the results were qualitatively equivalent for all matching techniques. Details on the methodology are available in a working paper by the authors.) The optimal full matching produced 247 strata of matched participants and nonparticipants. For each stratum we calculated the weighted average of each covariate and

■ **Table 1.** Characteristics of Health Plan Enrollees With Asthma (N = 3012)

Characteristic	No. (%)	
	Participant Group	Nonparticipant Group
Demographics		
Race/ethnicity		
Black	139 (51)	1515 (55)
Hispanic	96 (36)	736 (27)
Asian	35 (13)	491 (18)
Language		
English	155 (57)	1778 (65)
Asian group	25 (9)	371 (13)
Spanish	88 (33)	593 (22)
Missing/unknown	2 (1)	0 (0)
Sex		
Male	147 (54)	1446 (53)
Female	123 (46)	1296 (47)
Age group at study start, y		
2-11	103 (38)	1234 (45)
12-16	148 (55)	1024 (37)
17-22	19 (7)	484 (18)
Allergy diagnosis		
No	103 (38)	1664 (61)
Yes	167 (62)	1078 (39)
Utilization before OKA intervention		
Emergency department or urgent care visits		
0	210 (78)	2568 (94)
1 or more	60 (22)	174 (6)
Inpatient hospitalization		
0	255 (94)	2709 (99)
1 or more	15 (6)	33 (1)
Outpatient or physician office visits		
0	108 (40)	1831 (67)
1 or more	162 (60)	911 (33)
Flu or pneumonia vaccine		
0	226 (84)	2509 (92)
1 or 2	43 (16)	233 (8)
Prednisone prescriptions		
0	218 (81)	2545 (93)
1 or more	52 (19)	197 (7)
Pulmonary tests		
0	196 (73)	2464 (90)
1 or more	74 (27)	278 (10)
Rescue prescriptions		
0	61 (23)	1468 (54)
1-8	175 (65)	1208 (44)
9 or more	34 (12)	66 (2)
Control prescriptions		
0	104 (39)	1997 (73)
1-2	65 (24)	403 (15)
More than 2	101 (37)	342 (12)
Number in group	270	2742

OKA indicates Oakland Kicks Asthma.

■ **Figure.** Box Plot of Propensity Score Before (A) and After (B) Weighting



OKA indicates Oakland Kicks Asthma.

■ **Table 2.** *t* Test Results of Covariate Balance Based on Weighted Distribution Among Matched Sets

Variable	Mean Difference	<i>t</i> Statistic
No. of outpatient visits for asthma in the previous year ^a (squared)	-0.45	-1.01
No. of ED or urgent care visits for asthma in the previous 6 months (squared)	0.00	-0.02
No. of control medications in the previous 6 months ^a	-0.13	-2.18
No. of different primary care physicians listed for a child in the previous 6 months	0.02	1.17
No. of adult family members with AAH health insurance	-0.09	-1.83
Allergy diagnosis (proportion)	0.02	0.77
No. of different primary care physicians listed for a child in 6 months (squared)	0.02	0.89
Percentage of households in a study participant's census tract with income between \$25,000 and \$50,000 (cubed)	331.22	1.08
Any control medication in the previous year ^a	-0.04	-2.51
No. of ED or urgent care visits for asthma in the previous 6 months (cubed)	0.00	0.03

AAH indicates Alameda Alliance for Health; ED, emergency department.
^aHealthcare Effectiveness Data and Information Set–defined variable.

outcome, where the weight was the inverse of the number of matches within that group.

After the strata were created, we recalculated the descriptive statistics for participants and nonparticipants. Panel B of the Figure shows that, after full matching, the nonparticipant group had the same distribution of propensity scores as the participant group. (Three children were dropped from the equivalent nonparticipant group because they differed substantially from all participants. Four children were dropped from the participant group for the same reason.) In other words, full matching created a group of nonparticipants that were similar to participants in terms of the characteristics that best predict participation.

Furthermore, we conducted *t* tests for each of the pre-treatment variables selected by the Deletion/Substitution/Addition algorithm. Because the relationship between participants and nonparticipants in each matched group was not one-to-one, we constructed the sum of differences between each pair of individuals within each group and weighted this by the inverse of the number of matches within that group.

The test statistics are reported in **Table 2**. With the exception of the number of control medications in the previous 6 months and the number of adult family members with AAH insurance, the participant and comparison groups do not differ significantly at the mean. (We adjusted for these slight differences in the conditional regression model.) To-

Table 3. Conditional Logistic Regression After OKA Case Management Treatment

Outcome	No. of Observations and Matched Sets	OR	P	Likelihood Ratio	90% CI	95% CI	SE
Preventive care							
No. of control medication prescriptions 6 months postintervention	2359 172 sets	1.53	.019	-761.28	1.13, 2.07	1.07, 2.19	0.28
Any flu or pneumonia vaccine 1 year postintervention	1507 110 sets	2.95	.000	-316.73	1.97, 4.45	1.82, 4.81	0.73
Treatment of exacerbation							
No. of rescue medication prescriptions 6 months postintervention	2511 183 sets	1.49	.017	-1154.90	1.13, 1.96	1.07, 2.07	0.25
Any nebulizer administration 6 months postintervention	1462 66 sets	1.93	.058	-230.84	1.09, 3.42	0.98, 3.81	0.67
Any ED or urgent care center visit for asthma 6 months postintervention	1501 70 sets	1.73	.099	-259.98	1.00, 3.00	0.90, 3.33	0.58

CI indicates confidence interval; ED, emergency department; OKA, Oakland Kicks Asthma; OR, odds ratio.

gether, the overlap in the propensity scores of the 2 groups and the results of the *t* tests suggest that optimal full matching substantially reduced the bias in comparing outcomes across the 2 groups.

After creating the 247 strata of matched participants and nonparticipants, we calculated for each stratum the weighted average of each asthma outcome measured at 6 and 12 months before the intervention and 6 and 12 months after the intervention. Then we ran conditional logistic regressions for 5 outcomes: any flu or pneumonia vaccine within 12 months after the intervention and 4 outcomes within 6 months after the intervention (any ED or urgent care center visit, any nebulizer administration, the number of rescue medication prescriptions, and the number of control medication prescriptions). In each regression, the independent variable was a binary indicator for participation in OKA and the logit was conditioned on the strata from optimal matching. (For completeness, we tested whether including the 2 covariates that were not balanced after matching affected our estimates of the treatment effect. It did not; in fact, the difference was <10%.) The conditional logistic regression controlled for heterogeneity in baseline risk of that outcome across the matched strata.²³

RESULTS

Table 3 shows the odds ratios for 5 outcomes. Participants were more likely than nonparticipants to have engaged in 2 risk-reducing behaviors: vaccinations for pulmonary infections and filling prescriptions for controller medications. The odds ratios were 2.95 for vaccinations (95% confidence interval [CI] = 1.82, 4.81) and 1.53 for controller medications (95% CI = 1.07, 2.19). This increase in risk-reducing behaviors should

translate into a reduction in asthma exacerbations. However, of 3 indicators of treatment for asthma exacerbations—the number of prescriptions for rescue medications, any nebulizer treatment, and any ED visit—only 1 was statistically different between the 2 groups. The odds of a prescription being filled for a rescue inhaler were 1.49 times greater for participants than for nonparticipants (95% CI = 1.07, 2.07).

DISCUSSION

Participants in health interventions typically differ systematically from nonparticipants; thus, it is difficult to extrapolate program results across the population at large. Our evaluation of OKA, which addressed this problem by matching participants to a similar group of nonparticipants, demonstrates the challenge of quantitatively validating the case management approach to asthma intervention. Program participants were more likely than nonparticipants to use 2 types of recommended preventive healthcare. However, we found no reduction in treatments for asthma exacerbations and an increase in filling prescriptions for rescue medications.

It is unclear how these results should be interpreted. Most children with asthma should have a rescue inhaler on hand in case of an emergency. The OKA program may have made participants aware of this fact and encouraged them to have an adequate number of inhalers, or participants may have suffered more exacerbations, forcing them to refill their prescriptions more often. Program managers indicated that they recommended that families update expired prescriptions or obtain new prescriptions to have rescue medication available at school. Therefore, filling a prescription might have been a worthwhile risk-reducing behavior, although this interpreta-

tion cannot be verified using the claims data available. The most plausible implication for evaluation purposes is that, in this context, filling the first 1 or 2 rescue medication prescriptions was a type of risk-reducing behavior and not a marker of asthma exacerbations.

Reductions in nebulizer treatments and ED visits, the other markers of asthma exacerbations, would be difficult to find in any case, because these are uncommon events and because any benefits of improved preventive care would only appear over a longer time frame.²⁴⁻²⁶ For these reasons, even a successful program could lead to an increase in short-term utilization before any long-term decrease in utilization is measurable.

The program may have improved some outcomes that did not show up in our administrative data. Our research design enabled comparison of participants and nonparticipants who were similar in all other relevant dimensions by using data from administrative or insurance records. Measuring other changes (eg, quality of life) would require collecting primary data for both participants and nonparticipants.

Alternatively, a program could be effective for some segments of the target population but ineffective for others, making its impact statistically insignificant for participants as a whole. Whether that was the case cannot be determined without additional research to identify which program components were most effective and which participants were more responsive to health interventions.

Finally, the program may have been hampered by structural challenges such as failure of physicians to follow best practices. Despite efforts to encourage physicians to implement existing asthma management guidelines, there is significant empirical evidence that physician compliance is low. Future research on these issues is needed.

CONCLUSION

There has been substantial public investment in asthma intervention programs that use case management to affect household behaviors. Because health organizations have scarce resources and a range of investment possibilities, they must evaluate the impact of intervention programs on program participants. We used a rigorous methodology to evaluate one of these programs and found that the program did not significantly decrease utilization of healthcare to treat asthma exacerbations, although preventive care did increase.

This conclusion raises a number of difficult questions because there are competing explanations for the failure to find significant improvement in outcomes: (1) the postintervention time period was too short; (2) utilization data missed relevant clinical or behavioral changes; (3) even if the program had no significant impact on average, it had a positive

impact on some participants; (4) structural barriers limited the program's impact; or (5) the program was not effective. Distinguishing between these explanations requires a broad range of data about participants and nonparticipants over an adequately long period of time. These data would improve the accuracy of the evaluation and enable us to construct a richer model of the relationships between program attributes, participant behaviors, and outcomes, making it possible to design more effective asthma intervention programs.

We recommend three changes to how these national programs are currently implemented. First, resources must be invested in collecting long-run, detailed data on both participants and nonparticipants to improve outcome measurement. Second, more research is needed to identify individual or household characteristics that increase the probability of a positive outcome. Third, programs should be targeted to those who are likely to derive benefits from participation. For example, a 2-stage program could provide a low-cost group education program for all eligible households, followed by a second, more costly, individualized intervention for the more committed families. This design would reduce the total number of participants, but could increase the probability of improved outcomes and therefore the return on investment. Such a shift in the collection and use of program data would enhance the benefits of asthma programs and make it possible to shift resources toward those intervention models that are proven to succeed.

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