APPENDIX A

TECHNICAL APPENDIX ON EVALUATION METHODS

SECTION I

METHODS FOR EVALUATING THE ALAMEDA ALLIANCE FOR HEALTH AND CHILDREN'S HOSPITAL AND RESEARCH CENTER AT OAKLAND ATTACK CLINIC INTERVENTION

Random Assignment

Mathematica randomly assigned each day from July 11, 2008, to June 30, 2011, in a 4-to-3 treatment-to-control ratio (with the exception of 21 nonexperimental days requested by the Children's Hospital and Research Center at Oakland [CHRCO]).[1] Because emergency department (ED) volume rises on weekends, we randomly assigned weekend days (Saturday and Sunday) separately from weekdays (Monday through Friday) so that weekend days and weekdays each contribute the same proportion of patients to the treatment and control groups.

Identifying the Study Population

Using ED and hospital data provided by CHRCO, we identified all Medicaid beneficiaries who met program eligibility criteria for the study period defined above. Table 1 provides the number of children we identified at each step of the research sample identification process. Children that meet all eligibility criteria form the study population.

Through this process, we identified 3,648 children who met the eligibility criteria with an ED visit for asthma during the study period. Of these, 2,115 were in the treatment group and 1,533 were in the control group. We identified the first date a child met all eligibility criteria including an ED visit for asthma, if ever, and refer to this first ED visit as the child's index date. Next, we identified the dates corresponding to each child's unique baseline period (the 12 months before the index date) and study period (the months following the child's index date). Since CHRCO provided data from July 1, 2007, through September 30, 2011, we were able to observe return ED visits and inpatient admissions for at least 12 months before and at least 90 days after the index date of all children in the study population.

[1] Treatment-to-control ratio was originally 3 to 2 and was changed in March 2009 at the request of Alameda-CHRCO in an effort to garner more support from CHRCO ED staff.

Table 1. Study Population Meeting Eligibility Criteria for the Alameda- CHRCO Intervention

Identified Children	N
With a CHRCO emergency department (ED) visit with a primary or secondary diagnosis of asthma (ICD9: 493.xx) from July 11, 2008, to June 30, 2011, who was not subsequently admitted to the hospital on the day of the ED visit	5,829
Who was between the ages of 1 and 19 on the day of the ED visit	5,396
Who had Medicaid as their primary or secondary insurer on the day of the ED visit (a Medicaid managed care organization or Medi-Cal), and the insurance policy number on the claim was nonmissing	3,935
Who resided in the Bay Area on the day of that ED visit[a]	3,716
Who did not have a diagnosis of cancer, cystic fibrosis, or cerebral palsy at any time in the study period[b]	3,689
Whose ED visit occurred on a randomly assigned treatment or control day[c]	3,654
Whose insurance policy number was nonmissing	3,648
Children in the research sample	
Number in the treatment group	2,115
Number in the control group	1,533
Total	3,648

Source: CHRCO ED and hospital admissions data.

[a] Eligible cities for the Alameda-CHRCO intervention include Alameda, Albany, Antioch, Benicia, Berkeley, Brentwood, Castro Valley, Concord, Danville, Dublin, El Cerrito, El Sobrante, Emeryville, Fairfield, Fremont, Hayward, Hercules, Hillsboro, Lafayette, Livermore, Manteca, Martinez, Menlo Park, Milpitas, Modesto, Moraga, Napa, Newark, Novato, Oakland, Oakley, Orinda, Petaluma, Pinole, Pittsburg, Pleasant Hill, Pleasanton, Richmond, Rodeo, San Francisco, San Jose, San Leandro, San Lorenzo, San Pablo, San Ramon, Stockton, Tracy, Union City, Vallejo, and Walnut Creek.

[b] Diagnosis codes beginning with 140 through 239 for cancer, 343 for cystic fibrosis, and 277 for cerebral palsy.

[c] Some weeks of the intervention period were assigned to nonexperimental days at the request of CHRCO.

For the children in the study population, 2,115 (58 percent) of index dates fell on a treatment day, and 1,533 fell on a control day. As expected, the ratio of the number of children in the treatment and control groups corresponded closely to the experimental design (Table 2).

Table 2. Alameda- CHRCO Experimental Design, July 11, 2008, to June 30, 2011

	Experimental Design: Number of Treatment or Control Days			Number of Children in Sample Population Who Have Their Index ED Visit on Treatment or Control Day	
	Number of Days	Percentage of Days	Percentage of Experimental Days	Number of Children	Percentage of Children
Treatment	613	56.5	57.6	2,115	58.0
Control	451	41.6	42.4	1,533	42.0
Nonexperimental[a]	21	1.9	--	0[b]	0
Total	1,085	100	100	3,648	100

Source: Mathematica random assignment and CHRCO claims data.

[a] Some weeks of the intervention period were assigned to nonexperimental days at the request of CHRCO.

[b] A child who has an ED visit for asthma on a nonexperimental day and is otherwise eligible is not assigned an index date. The child was assigned an index date if he or she returned to the ED for asthma on an experimental day.

Matching Study Population with Alameda Alliance Data

After identifying the study population for Alameda-CHRCO, we transmitted Medicaid identification numbers to the Alameda Alliance for Health (the Alliance), and they provided insurance claims data as well as enrollment and capitation information for all enrollees in the study population for July 1, 2007, through June 30, 2011. Table 3 describes the number of children transmitted in the initial file to the Alliance and the subsequent steps we took after reviewing data from the Alliance to arrive at the final group of the Alliance-insured children eligible for the study population. Among all the children eligible for the Alameda-CHRCO study population, the Alliance identified 2,059 as insured in its Medicaid managed care plan during the study period. Of this group, 1,864 (51.1 percent of the full sample) had at least one day of enrollment with the Alliance before and after their index ED visit dates; among these children, 1,079 were in the treatment group and 785 in the control group.[2] The most common reason that eligible children were not matched to the Alliance data is that they received their Medicaid insurance through the other Medicaid Managed Care plan in Alameda County (Anthem Blue Cross).

Table 3. Alameda Alliance for Health- Insured Children in the Study Population

Identified Children	N
Included in the Alameda-CHRCO study population	2,059
With at least one day of enrollment with the Alliance before and after their index date	1,864
Children with asthma in the research sample insured by the Alliance	
Number in the treatment group	1,079
Number in the control group	785
Total	1,864

Source: CHRCO ED and hospital admissions data, and Alameda Alliance enrollment data.

Identifying ATTACK Clinic Visits

Using administrative data from the ATTACK clinic, Mathematica identified children referred to the clinic, identified children who attended the clinic, and verified whether the attendance patterns aligned with the experimental design. According to the clinic's records, 1,364 children were referred to the ATTACK clinic, of whom 546 (40 percent) attended the clinic one or more times.[3]

Not all these children referred to the clinic, however, were eligible for inclusion in the research sample.[4] A total of 345 children in the research sample attended the clinic one or more times. (Fourteen attended more than once, for a total of 361 clinic visits.) Of these children, 287 were in the treatment group on their index date (N=252) or were reassigned to the treatment group because they revisited ED on treatment day prior to visiting the ATTACK clinic (N=35). The remaining children either were assigned to the control group on the date of their clinic visit (N=28) or visited

[2] One Medicaid beneficiary in the Alliance data was dropped because he or she could not be matched to the observations in the CHRCO data.

[3] All ATTACK clinic referral and attendance figures exclude children who attended the clinic but could not be matched to the CHRCO data with their identification number, or who did not meet the inclusion criteria for age, excluded illnesses, or a same-day inpatient admission. Twenty-three of the excluded children attended the ATTACK clinic. There were 4,176 children who visited the CHRCO ED for asthma or a number of other diagnoses over this period (who would not be excluded from these less-stringent criteria), so about one-third of "candidate" children were referred, and over 10 percent attended the ATTACK clinic.

[4] For example, 76 children who attended the clinic did not have Medicaid insurance, and 98 did not have an ED visit for asthma (ICD=493.x).

the ATTACK clinic before having a qualifying ED visits (N=30). Of the 2,115 children in the research sample and originally assigned to the treatment group, 631 (30 percent) were referred to the ATTACK clinic, and 267 (13 percent) attended.

In Table 4, we explore the records of the children who visited the ATTACK clinic to understand why children might have attended the clinic even though they should not have according to the experimental design assignment protocol. As seen in the table, most of the cases can be explained by children who visited the ED for other reasons (that is, visits without a primary or secondary diagnosis of asthma) or who visited the hospital when they were ineligible for inclusion in the study population (for example, they might have visited the ED for asthma when less than one year old). A number of children, however, simply had their index visit on a control day and visited the ATTACK clinic anyway.

Table 4. Treatment and Control Group Children Who Attended the ATTACK Clinic

	T/C Status on Date of First ATTACK Clinic Visit		
	Treatment	Control	No Prior Eligible ED Visit
1. Actual T/C assignment protocol	287[a]	28	30
2. Also consider ED visits for asthma with missing insurance policy number on claim	288	28	29
3. Also consider ED visits with third diagnosis of asthma (493.xx)	297	26	22
4. Also consider ED visits with third diagnosis of asthma (493.xx) or other respiratory illness (465.xx or 786.xx)	309	22	14
5. Also consider ED visits with other diagnoses	321	15	9
6. Also consider hospital admissions (any diagnosis)	322	14	9
7. Also consider ED visits and hospital admissions when child was ineligible (for example ED visit when not Medicaid beneficiary or not above minimum age requirement)	335	10	0

Source: CHRCO claims data and ATTACK clinic administrative data.

Note: Table presents the number of children who are in the treatment or control group as of their first visit to the ATTACK clinic in row 1. Children who do not visit the ATTACK clinic are excluded from the table. In rows 2 through 7, we incrementally relax the treatment/control group assignment protocol by also considering cases where a child visited the ED, or was admitted but did not meet the conditions of the actual protocol.

[a] 287 children were in the treatment group on their index date (N=252) or were reassigned to the treatment group because they revisited ED on treatment day prior to visiting the ATTACK clinic (N=35).

In our primary analysis, children were assigned to the treatment and control groups based on whether their index visit occurred on a treatment or control day, regardless of the possibility that a child in the control group might return to the ED on a treatment day (after their original index visit), be "reassigned" to the treatment group, and then be invited to the ATTACK clinic. Subsequent comparisons of health utilization between the treatment and control groups could be affected by treatment after subsequent ED visits. As discussed above, children in the control group also appear to have attended the ATTACK clinic by mistake, and some children attended before they became eligible for the intervention according to protocol. The prevalence of these issues can be observed by comparing the rates at which children attended the ATTACK clinic in the treatment and control group (Table 5):

Table 5. ATTACK Clinic Attendance and Treatment/Control Group Assignment on the Index Date

Assignment at Index Date	Ever Attended the ATTACK Clinic				Total
	Yes		No		
Treatment Day	268	(12.7%)	1,849	(87.3%)	2,117
Control Day	77	(5.0%)	1,460	(95.0%)	1,537
Total	345	(9.5%)	3,309	(94.6%)	3,654

Source: CHRCO claims data and ATTACK clinic administrative data. Because of rounding, row percentages may not sum to 100 percent.

Despite these issues, the experimental design is not compromised, as our design plan called for all impact analysis to be conducted from an intent-to-treat, or population-based, perspective. That is, we rely on the actual treatment or control assignment in the design protocol as the source of experimental variation. A child's actual attendance at the ATTACK clinic is subject to selection (by CHRCO staff or the child's family), and therefore analysis based on the (endogenous) choice to attend the clinic would be biased. Selection issues are of particular concern for the children who attended the clinic even though they should not have been referred to the ATTACK clinic according to the design protocol (e.g, they did not visit the CHRCO ED on a treatment day, or were not eligible for the study population at the time of their ED visit). In practice, this small number of children did not have a significant effect on our findings (that is, the results would remain largely unchanged even if we ignored selection issues, assigned these children to the treatment group, and repeated the analysis). We examined the primary outcome measure of interest (the return ED visit rate for asthma) by including all control group children who visited the clinic in the treatment group, and we found evidence that the ATTACK clinic had an effect.

Outcome Indicators

We used data provided by CHRCO and the Alliance to identify demographic characteristics, construct measures of health care use for each child's baseline year, and examine outcomes for children who became eligible for the study. Outcome variables and the respective data source are listed in Table 6.

Table 6. Alameda- CHRCO Outcome Variables

Outcome	Source of Claims Data: CHRCO (N=3,648)	Source of Claims Data: Alameda Alliance (N=1,864)	No Reassign-ment	Regression Model: OLS	Regression Model: Logit	Regression Model: Multi-nomial Logit
Emergency Department (ED) Visits at CHRCO (within 30, 60, 90, 120, and 180 days after index visit)						
Proportion of patients with one or more ED visits for asthma	✓		✓		✓	
Proportion of patients with zero, one, or more than one ED visit for asthma	✓					✓
Proportion of patients one or more ED visits for any reason	✓		✓		✓	
Proportion of patients with zero, one, or more than one ED visit for any reason	✓					✓
Hospitalization Admissions at CHRCO (within 30, 60, 90, 120, and 180 days after index visit)						
Proportion of patients with one or more hospital admissions for asthma	✓				✓	
Average annualized number of hospital admissions for asthma	✓			✓		
Proportion of patients with one or more hospital admissions for any reason	✓				✓	
Average annualized number of hospital admissions for any reason	✓			✓		
Outpatient Visits (within 30, 60, 90, and 180 days of index visit)						
Proportion of patients with one or more office visits for asthma		✓			✓	
Proportion of patients with zero, one, two, three, or four or more office visits for asthma						✓
Average annualized number of office visits for asthma		✓		✓		
Proportion of patients with one or more office visits for any reason		✓			✓	
Proportion of patients with zero, one, two, three, or more than four office visits for any reason						✓
Average annualized number of office visits for any reason		✓		✓		
Prescription Drug Use (within 180 and 365 days of index visit)						
Controller Medications						
Average number of fills for controller medications per year		✓		✓		
Proportion of patients with zero, one to three, four to six, seven to nine, or more than nine controller medication fills		✓				✓
Average days of medication available (DMA) for controller medications per year (the sum of the “quantity” field in the claims data)		✓		✓		
Proportion of patients with zero, more than zero and up to 90, or more than 90 DMA for controller medications		✓				✓

Outcome	Source of Claims Data		No Reassign-ment	Regression Model		
	CHRCO (N=3,648)	Alameda Alliance (N=1,864)		OLS	Logit	Multi-nomial Logit
Average percentage of days covered (PDC) for controller medications (the number of days of controller medication is available divided by the number of days in the period, adjusted for truncated observation periods)		✓		✓		
Proportion of patients with PDC over 80 percent					✓	
Proportion of patients with PDC over 90 percent					✓	
Average of the ratio of controller medications DMA to controller plus rescue medications DMA, among those with at least one fill of an asthma medication of any kind		✓		✓		
Rescue Medications						
Average number of fills for rescue medications per year		✓		✓		
Proportion of patients with zero, one to three, four to six, seven to nine, or more than nine rescue medication fills		✓				✓
Average DMA for rescue medications per year		✓		✓		
Proportion of patients with zero, more than zero and up to 90, or more than 90 DMA for rescue medications		✓				✓

Note: ED visits, inpatient admissions and readmissions, and outpatient visits for asthma include any utilization where the primary or secondary diagnosis is for asthma (ICD-9 493.xx).

Our empirical model is consistent with an intent-to-treat evaluation design to assess the effect of the intervention on the population of eligible children. Reassignment to the treatment group does not affect one key outcome—the percentage of children who return to the ED (within 30, 60, 90, 120, and 180 days)—because the outcome is observed before the reassignment occurs. All other outcome variables are potentially affected by reassignment, though the effect is minor in practice because a small fraction of children revisit the ED and then attend the ATTACK clinic.

Outcome variables constructed from data provided by CHRCO (that is, ED visits and inpatient admissions) are fully observed for all children in the study population.[5] On the other hand, outcome variables constructed from the Alameda Alliance claims data are affected by the number of months each child was enrolled in the Alameda Alliance Medicaid Managed Care plan in the baseline and study periods. For these variables, we constructed annualized outcome measures for each child's baseline period and study period and weighted all outcome analyses. For the continuous outcome measures listed in Table 6, children were weighted by the percentage of months enrolled in the

[5] For the outcome variables based on time frames over 90 days, we cannot observe outcomes for children who enter the sample on the last day of the intervention. For example, for a child who enters the sample on the last randomization day (June 30, 2011), we could observe outcomes if the child returned to the ED within 30, 60, and 90 days (because we have data through September 30, 2011), but we would drop the child from the regressions that examine whether he or she returned to the ED within 120 or 180 days.

respective time period. For binary indicator outcomes, we use the same weight we created for continuous outcomes, except that a child always receives the full weight if the outcome was observed (for example, if he or she was hospitalized). Weights were then normalized to have a mean of 1.00.[6]

Empirical Methods

To evaluate intervention impacts, we compared the means (and percentages) for intervention-period outcomes across the treatment and control groups. We used linear, logit, and multinomial logit regression models for continuous, binary, and categorical intervention period outcomes, respectively (Table 6).[7] The regressions included child-level explanatory control variables—demographic information and baseline outcomes—to improve the predictive power of our model and reduce the unexplained variation in intervention period outcomes. Explanatory variables in the regressions are listed in Table 7; these variables were all calculated with data from the index visit or from the baseline period (up to 12 months prior to the index visit). Models were estimated with Stata/MP 11 for Windows, and baseline comparisons were conducted with SAS 9.1.

We present regression-adjusted means for the treatment and comparison groups and the regression-adjusted difference between the two groups. For categorical variables, statistical significance was determined from a chi-squared test, with null hypothesis of zero treatment effect for all categories. All *p*-values are two-tailed. We interpret regression-adjusted difference as estimate of the causal effect of being assigned to the treatment group on the index date.

[6] For example, a child who was enrolled in 8 of the 12 months in the intervention period would receive a weight of two-thirds for continuous outcomes. For a binary outcome, the child would receive a weight of one if the outcome occurred and two-thirds if it did not. We would then divide by the average weight in the sample so that the sum of the weights equals the number of sample members.

[7] We used Student's t-tests and χ^2 tests to compare the treatment and comparison groups at baseline.

Table 7. Explanatory Variables Included in Alameda- CHRCO Regression Models

Variable	Description
Treatment	Indicator that equals one if the child is the treatment group, and zero otherwise
Age	An array of indicators for the child's age on the child's index date
Gender	Indicator that equals one if child is female, and zero otherwise
Medicaid Eligibility[a]	Two indicator variables for Anthem Blue Cross or MediCal FFS eligibility at the child's index date (both equal zero if Alameda Alliance eligibility)
Race/Ethnicity	An array of indicators for the child's race/ethnicity
Index Date	Indicator variables for the child's index date (one dummy for each quarter of each year a child has an index date, to control for trends over time and seasonal effects)
Prior Evidence of Asthma[c]	Three indicators that equal one if the child had a primary or secondary asthma diagnosis in the baseline period for 1 ED visit, 2 or 3 ED visits, or 1 hospital admission, or more than 3 ED visits or more than 1 hospital admission, respectively (all three are zero if no ED visits or admissions in baseline)
Common Comorbid Conditions[d]	Indicator variables for acute respiratory infection, ear infection (otitis media), and pneumonia in the baseline period
Baseline Period Outcomes	Four outcomes from the baseline period: the number of ED visits for any reason, the number of ED visits for asthma, the number of hospital admissions for any reason, and the number of hospital admissions for asthma In the analysis with outcome variables from the Alliance claims data, we also included the annualized number of outpatient visits for any reason, the annualized number of outpatient visits for asthma, indicators for any controller or rescue medication fill (two dummies), and the percentage of days covered with controller medications
Baseline Clinic Visit	Indicator variable that equals one if the child attended the ATTACK clinic prior to the child's index date (up to 30 children)

Note: The omitted categories in the regression are index date in 2008q3, 2 to younger than 5 years old on index date, male, Alameda Alliance on index date, Caucasian, only prior use is index ED visit, and no common comorbid conditions, who is in the control group ($Treat_i = 0$).

[a] Patients were classified as Medicaid enrollees if they had Medicaid fee-for-service or a Medicaid managed care organization listed as either a primary or secondary insurer on the index date. If both primary and secondary insurer types were Medicaid, we used the primary insurance type to classify them for this table.

[b] "Other" includes children who have a value of NAM, OTH, or U for the race variable or are missing a value for the ethnicity variable, as reported by CHRCO.

[c] We used CHRCO data from the 12 months before each child's index date. We classified an ED visit or hospital admission as being for asthma if its primary or secondary diagnosis was for asthma.

[d] We identified common comorbid conditions from ED and hospital claims with any diagnosis of acute respiratory infection (460.xx to 466.xx and 786.xx), ear infection (382.xx), and pneumonia (486.xx).

Alameda-CHRCO = Alameda Alliance for Health-Children's Hospital and Research Center at Oakland; ED = emergency department.

Two-Stage Least Squares

As a secondary analysis, we also estimated two-stage least squares (2SLS) models for the continuous and binary outcomes (also known as instrumental variables analysis). In the first stage equation, where the child's assignment to the treatment group at their index date is used to predict—along with the other explanatory variables—the probability the child visits the ATTACK clinic. In the second stage, the outcome was regressed on the instrumented ATTACK clinic indicator and the other explanatory variables. These regressions also include the control variables listed in Table 7. We interpreted the coefficient on the second stage ATTACK clinic variable in the

2SLS regressions as the local average treatment effect (see Imbens and Angrist 1994). Intuitively, this might be thought of as the hypothetical difference in the outcome variable that would occur if the "average" beneficiary did or did not attend the ATTACK clinic. Our main impact analysis (described above) is analogous to a reduced form equation in the 2SLS model.

For brevity, we do not present tables with the 2SLS results but briefly describe the results here. As expected, assignment to the treatment group at the index date was associated with an increase (of roughly 7 percentage points) in the odds of actually attending the ATTACK clinic in the first stage. The 2SLS models are "just identified." The second stage estimates are in the same direction as the estimates in the main (reduced form) impact analysis, and roughly (100/7=) 14.3 times as large.[8] For most outcomes, our models did not find that the intervention was associated with any that were significantly different from those observed in the control group, which is consistent with the main findings.

Supplemental Analysis

As discussed above, a number of children who were referred to the ATTACK clinic (and therefore may have attended it) were excluded from the experimental research sample because they did not meet the inclusion criteria. Our evaluation was not designed to measure the effect of the intervention on these children, because we do not believe that the children outside the research sample whom CHRCO deemed "candidates" for referral to the clinic were randomly assigned into treatment and control groups based on the date of their index visit.

The difficulty with the nonexperimental evaluation is estimating the counterfactual: what would have occurred if the referred children had not, in fact, been referred? We can use nonexperimental evaluation methods to estimate the effect of *referral* to the ATTACK clinic (analogous to the intent-to-treat effect) by comparing referred children to an appropriate comparison group. This method of evaluation is inferior to a randomized evaluation, because children who were referred may differ systematically from children who were not referred for factors that are unobserved or incompletely measured (whether the patient is a high- or low-risk asthma patient, more or less able to manage their asthma, and so on). Nonetheless, we performed this supplementary analysis to determine whether the estimated intervention effect from the primary analysis was externally valid for *all* children who were referred to the ATTACK clinic (including those not in the research sample).

We performed the supplementary analysis using the CHRCO administrative data for all children who were referred to the clinic, plus children in the research sample who were not referred.[9] The comparison group, therefore, consisted of children who were in the research sample and not

[8] The first-stage results differed slightly from one outcome variable to another because the ATTACK clinic dummy variable corresponds to the length of time corresponding to the outcome variable. For example, the dummy variable equals one if the child attended the ATTACK clinic within 90 days of the index ED visit when we are using hospitalizations within 90 days an outcome. For binary outcomes, the 2SLS models also differ, because a linear probability model is used instead of a logit specification.

[9] We excluded referred children who attended the clinic but could not be matched to the CHRCO data with their identification number, or who did not meet the inclusion criteria for age, excluded illnesses (for example, cancer), or a same-day inpatient admission. These exclusion criteria are less restrictive than the criteria used to identify the research sample for the main analysis.

referred. Many of these children may have been nonreferred because they visited the ED on a randomized "control day."

We used three methods to measure the effect of ATTACK clinic referral on the probability a child returned to the ED (for asthma and for any reason) within 30, 60, 90, 120, and 180 days after their referral date (or, if they were not referred, if they returned to the ED after their index visit). We controlled for (or matched on) the patient's age group, race, number of ED visits for asthma within 12 months, and number of ED visits not for asthma within 12 months.[10] This is not a full list of available control variables, but it should capture the most important characteristics from the patient's history with the CHRCO ED. The procedure we used to conduct this analysis comprised three steps:

1. We estimated logit regression models including data for all children who were referred to the clinic, plus children in the research sample who were not referred. These regression model estimates the effect of ATTACK clinic referral, controlling for observed patient characteristics.
2. We performed coarsened exact matching (CEM) (Iacus et al. 2011; Blackwell et al. 2009), which stratifies the sample based on age, race, and the four categories of ED visits for asthma and not for asthma. Referred children are matched to one or more comparison children who *exactly* match them on these four characteristics. CEM drops some referred children because there were no matching comparison children (that is, we restrict the data to areas of common empirical support). Once children are matched, we compare ED rates for the referred children to those of their matched counterparts in the comparison group.
3. We estimated a logit model that predicted the probability of ATTACK clinic referral and created a propensity score for each patient. We then used normalized propensity score reweighting (Busso et al. 2009) to compare ED return rates for the referred children and their matched comparisons. This technique uses weighting to equalize the means of the matching variables in the referred and comparison groups. Intuitively, the approach "forces" a "match" for referred children who lie outside the area of common empirical support in the CEM matching.

The results were qualitatively similar across the three nonexperimental methods. Other methods considered, such as nearest-neighbor matching, did not perform well, because there are relatively few children in the comparison group with a high likelihood of referral (that is, with high propensity scores). Furthermore, the results were similar if we excluded children who did not have an ED visit (for asthma or for any reason) fewer than 30 days before ATTACK clinic referral.

Power Calculations

To assess the statistical power of our evaluation, we performed power calculations to estimate the minimum detectable effects (MDEs) for our regressions. In Table 8, we present MDEs with varying incident rates for a binary outcome variable. The chosen levels of incidence (p=1 to 20 percent) roughly correspond to the percentage of children with a return ED visit in 30 to 180 days.

[10] We used four categories for the number of previous visits ED visits (0, 1-3, 4-6, and more than 6). In the regressions and propensity score model, we also included a continuous variable for the number of visits.

As seen in the second row of the table, we can expect 80 percent power to detect differences of 2.0 percentage points (at the 95 percent confidence level) for an outcome variable with a rate of 5 percent in the control group. Statistical power is stronger if the control group's incidence rate is closer to 50 percent and lower if closer to 0 percent.

Table 8. Minimum Detectable Effects for Alameda Analysis

	Minimum Detectable Effect	
Mean of Binary Outcome Variable	Estimate (percentage points)	As a Percentage of Mean
1 percent	0.91	91.1
5 percent	2.00	39.9
10 percent	2.75	27.5
15 percent	3.27	21.8
20 percent	3.66	18.3

Note: The MDE calculations assume a 95 percent confidence level for a two-tailed test, 80 percent power level, R-squared equal to 0.05, and a sample with 2,115 patients in the treatment group and 1,533 in the control group.

SECTION II

METHODS FOR EVALUATING THE CINCINNATI CHILDREN'S ASTHMA IMPROVEMENT COLLABORATIVE

Identifying the Study Population

Given Cincinnati Children's population-based, system-wide approach to improving pediatric asthma care, the target population is broadly defined as Medicaid children with asthma in Hamilton County who have some interaction with the health care system. To study this multipronged intervention, we considered all Medicaid children in Hamilton County who met patient eligibility criteria as the treatment group; Medicaid children who met these same criteria in three comparison counties—Cuyahoga, Franklin, and Montgomery—formed the comparison group.[11]

To identify children who met eligibility criteria, we used Medicaid administrative data provided by Cincinnati Children's through a data use agreement, which included enrollment and claims data for Medicaid beneficiaries from July 1, 2004, through June 30, 2011. Outpatient outcome variables are truncated at December 30, 2010, because we did not receive outpatient claims data for all children through June 30, 2011. We used a rolling sample identification process to identify when, if ever, each child was eligible for inclusion in the study population. For each month from July 2008 to June 2010 we examined whether the child met the following criteria[12]:

- The child is at least 2 years old and younger than 18[13]
- In the previous 12 months, the child had a claim for (1) one or more hospital admissions, (2) two or more ED visits, (3) two or more outpatient visits, or (4) one ED visit and one outpatient visit on which (a) the primary diagnosis is asthma (ICD-9: 493.xx), or (b) the secondary diagnosis is asthma (ICD-9: 493.xx) and the primary diagnosis is pulmonary-related (ICD-9: 460.xx-466.xx, 472.xx-492.xx, 495.xx-496.xx, 510.xx-513.xx,786.x, 034.xx)
- The child never showed evidence of any of the following conditions (on any medical claim) during the study period: cystic fibrosis (ICD-9: 343.xx), heart transplant (ICD-9: 37.51), cancer (ICD-9: 140.xx through 239.xx), cerebral palsy (ICD-9: 277.xx), heart disease (ICD-9: 390.xx-459.xx), congenital heart disease (ICD-9: 746.xx), Down's syndrome (ICD-9: 758.xx), diagnoses associated with prematurity (ICD-9: 362.xx, 774.2, 765.0, 765.1)

[11] The major cities in Hamilton, Cuyahoga, Franklin, and Montgomery Counties are Cincinnati, Cleveland, Columbus, and Dayton, respectively.

[12] June 2010 was chosen as the last date of entry into the research sample to allow for at least 12 months of follow-up observation.

[13] Nine children were dropped because date of birth was missing and age could not be calculated. We also dropped beneficiaries who were just under 18 in the baseline period and had their 18th birthday before the intervention began in July 2008. One child was dropped because in the data, a date of death was found that did not align with enrollment data.

- The child resides in either the study county (Hamilton) or one of the three comparison counties (Cuyahoga, Franklin, or Montgomery)
- The child is enrolled in the Medicaid program and was also enrolled for at least 6 of the previous 12 months and has at least one month of enrollment in the study year 1

We identified the first month a child met all these criteria, if ever, and refer to this as the child's index date. We then identified the dates corresponding to each child's (unique) baseline period (the 12 months before the index date) and study period (up to 30 months following the child's index date); the study period was further divided into study year 1 (the first 12 months after the index date), study year 2 (months 13 through 24), and study year 3 (months 25 through 30). Many of the children in the study population, but not all, had an index date of July 2008, but some entered the sample later (and therefore have shorter study periods).[14] We truncate a child's study period on their 18th birthday or when they move from Hamilton County to a comparison county (or vice versa), but otherwise use all data through December 31, 2010, so long as the child is enrolled as an Ohio Medicaid beneficiary.

The Ohio Medicaid data included 3,903,788 unique persons, and we identified a total of 6,904 children eligible for inclusion in the study population (Table 9). As seen in Table 10, 17.6 percent of the children were residents of the Hamilton County, the treatment group, and the rest lived in the comparison counties. We observe, on average, 10.6 months of enrollment in the baseline period (out of 12 months) and 25.8 months in the intervention period.

Table 9. Number of Unique Children Identified for the Cincinnati Children's Evaluation Cohort

Identified Children	Number
Number of Persons Included in Ohio Medicaid Data	3,903,788
Who met age requirements	1,729,902
Who met county of residence requirements	614,776
Who met enrollment criteria	574,854
Who met diagnostic exclusion criteria	11,136
Who met asthma diagnosis criteria	9,604
Final Sample	9,604
Study group	1,691
Comparison group	7,913

Source: Ohio Medicaid eligibility and claims data.

[14] For children who enter the sample in July 2008, the baseline period was July 2007 through June 2008, and the study period was the 30 months between July 2008 and December 2010. If, for example, a child entered the sample in October 2009 (perhaps because their second birthday occurred in that month), then his or her baseline period would cover October 2008 through September 2009, and their intervention period would be the 15 months between October 2009 and December 2010. All children's study periods end in December 2010, the last month included in the data.

Table 10. Number of Unique Children Identified for the Cincinnati Children's Evaluation Cohort Across by Study and Comparison Counties

	Number of Children	Number of Baseline-Period Child-Months	Number of Intervention-Period Child-Months
Study Group	1,691	19,254	43,306
Comparison Group	7,913	91,155	204,246
Cuyahoga	3,092	35,617	79,164
Franklin	3,751	43,302	98,183
Montgomery	1,070	12,236	26,899
Total	9,604	110,409	247,552

Source: Ohio Medicaid eligibility and claims data.

Note: The counts in each comparison county are unique counts. No child is included in more than one comparison group sample during any baseline or evaluation year. We will count the first county as county of residence for purposes of this table.

To verify that the treatment and comparison counties had similar trends during the baseline period, it was necessary to identify a study population during the baseline period (that is, before the intervention started in July 2008). In this case, we used an identical rolling sample identification process, but instead identified the first month in the baseline period that the child would have been eligible for the intervention if, in fact, the intervention had hypothetically started at an earlier date. As seen in Table 11 and Table 12, the sample selection process and the balance between the treatment group was similar in the baseline period.

Table 11. Number of Unique Children Identified for the Cincinnati Children's Evaluation Cohort

Identified Children	Baseline 1: July 1, 2004 – June 30, 2006	Baseline 2: July 1, 2005 – June 30, 2007	Baseline 3: July 1, 2006 – June 30, 2008
Number of Persons Included in Ohio Medicaid Data	3,044,214	3,044,214	3,044,214
Who met age requirements	1,303,378	1,303,378	1,303,378
Who met county of residence requirements	466,993	466,993	466,993
Who met enrollment criteria (in time period)	436,907	436,907	436,907
Who met diagnostic exclusion criteria (in time period)	3,746	3,899	3,825
Who met asthma diagnosis criteria	3,523	3,604	3,521
Final Sample	3,523	3,604	3,521
Study group	746	605	581
Comparison group	2,777	2,999	2,940

Source: Ohio Medicaid eligibility and claims data.

Table 12. Number of Unique Children Identified for the Cincinnati Children's Evaluation Cohort Across by Study and Comparison Counties, Baseline Period: July 1, 2005

	Number of Children	Number of Baseline-Period Child-Months	Number of Intervention-Period Child-Months
Baseline 1: July 1, 2004 – June 30, 2006			
Study Group	746	8,736	8,179
Comparison Group	2,777	32,805	31,186
Cuyahoga	1,204	14,240	13,592
Franklin	1,183	13,964	13,242
Montgomery	390	4,601	4,352
Total	3,523	41,541	39,365
Baseline 2: July 1, 2004 – June 30, 2006			
Study Group	605	7,068	6,654
Comparison Group	2,999	35,439	33,773
Cuyahoga	1,291	15,274	14,608
Franklin	1,281	15,157	14,354
Montgomery	427	5,008	4,811
Total	3,604	42,507	40,427
Baseline 3: July 1, 2004 – June 30, 2006			
Study Group	581	6,746	6,409
Comparison Group	2,940	34,728	32,918
Cuyahoga	1,277	15,101	14,230
Franklin	1,248	14,740	14,013
Montgomery	415	4,887	4,675
Total	3,521	41,474	39,327

Source: Ohio Medicaid eligibility and claims data.

Note: The counts in each comparison county are unique counts. No child is included in more than one comparison group sample during any baseline or evaluation year. We will count the first county as county of residence for purposes of this table.

Outcome Indicators

To evaluate the intervention's effectiveness at improving patients' quality of care, for each child in the study population, we used Medicaid claims and enrollment data to construct a number of outcome measures. Table 13 identifies the key measures for the Cincinnati Children's outcomes analysis:

Table 13. Cincinnati Children's Outcome Variables

Domain	Any Utilization (binary outcomes)	Utilization Rates (continuous outcomes)
Emergency Department (ED) Visits for Asthma	Percentage of children with any ED visit[a]	Average Annual ED visit rate[a]
Hospitalization Admissions for Asthma	Percentage of children with any hospital admission[a] Percentage of children with hospital readmission[b]	Average annual hospital admission rate for asthma[a] Average annual hospital readmission rate for asthma[b]
Outpatient Visits	Percentage of children with any outpatient visit Percentage of children with any outpatient visit for asthma[a]	Average number of outpatient visits per year Average number of outpatient visits for asthma per year[a]
Prescription Drug Use	Any fills for controller medications Any fills for rescue medications	Average number of fills for controller medications per year Average number of fills for rescue medications per year

Note: ED visits, inpatient admissions and readmissions, and outpatient visits for asthma include any utilization where the primary or secondary diagnosis is for asthma (ICD-9 493.xx).

[a] Includes ED visits, admissions, and outpatient visits where the primary or secondary diagnosis is for asthma (ICD-9: 493.xx).

[b] Includes readmissions where the primary or secondary diagnosis is for asthma (ICD-9: 493.xx) for both the initial admission and a readmission. This is measured for readmissions within 30, 60, 90, and 120 days. Children without any admission or without a readmission after an initial discharge have zero readmissions.

We constructed annualized outcome measures for each child's baseline period and study period, and for each child's study years 1, 2, and 3. We weighted all outcome analyses to account for the number of months each child was enrolled in Medicaid. For the continuous outcome measures listed in Table 13, children were weighted by the percentage of months they were observed in the respective time period. For binary indicator outcomes, we use the same weight we created for continuous outcomes, with the exception that a child always receives the full weight if the outcome was observed (for example, if the child was hospitalized). Weights were then normalized to have a mean of 1.00.[15]

Empirical Methods

The primary impact analyses for the Cincinnati Children's intervention were conducted using a difference-in-differences approach. We used this approach because a number of other factors—such as quality improvement efforts, changes in statewide Medicaid policies, and other trends in health outcomes—may have produced changes in the outcomes of interest during this period. Attributing simple differences in children's outcomes between the periods before and after the intervention would risk biased estimates of program impacts.

[15] For example, a child who was enrolled for 8 of the 12 months in the intervention period would receive a weight of two-thirds for continuous outcomes. For a binary outcome, the child would receive a weight of one if the outcome occurred and two-thirds if it did not. We would then divide by the average weight in the sample so that the sum of the weights equals the number of sample members.

We adjusted for the changes in outcomes that would have occurred even if the BCQII intervention had not been implemented, by comparing changes observed for Medicaid children in Hamilton County who meet patient eligibility criteria—the treatment group—to changes in observed outcomes for a comparison group (children who meet the same criteria in Cuyahoga, Franklin, and Montgomery Counties). The difference between the change in outcomes that occurs in the treatment group and the change in the comparison group is attributed to the program. We compare changes in outcomes between the treatment and comparison groups (1) from baseline to the study period, and separately (2) from baseline to study years 1, 2, and 3.

The difference-in-differences estimates were obtained from a regression framework in order to remove biases in intervention-period comparisons that could result from permanent differences between the treatment and comparison groups, and to control for a number of child characteristics.[16] Control variables are listed in Table 14. We estimated logit models for the binary outcomes, with two observations for each child in the regression. The continuous outcome variables were irregular (were strictly non-negative, had a masses at zero, and/or had skewed distributions); therefore we estimated the difference-in-differences model with two-part models, with a generalized linear model (GLM) in each stage. These models estimate the probability of a positive outcome in the first stage, and then model the outcome level—conditional on positive expenditures—in the second stage. The first-stage GLM was estimated with a logit link function and binomial distribution (that is, a logit model). We chose to use linear models for the second stage (identify link function and normal distribution) because the hospitalization and readmission outcomes were rare. We used an exchangeable within-individual correlation structure for the two GLM models because weights for some children varied across observations. We then combined the two stages to calculate the effect of the intervention on the outcome variable. We calculated the average marginal effect of the treatment (in the intervention period) using the double difference formulation proposed by Ai and Norton (2003), and determined *p*-values by calculating bootstrapped standard errors. This method was used for our primary impacts analysis (separate models for the study period and study years 1, 2, and 3). Models were estimated with SAS 9 for Linux.

[16] We used Student's t-tests and χ^2 tests to compare the treatment and comparison groups at baseline.

Table 14. Explanatory Variables Included in Cincinnati Children's Difference- in- Differences Regression Models

Variable	Description
$Post_t$	Indicator that equals one for observations from the study period, and zero for observations from the baseline period
$Treat_i$	Indicator that equals one if the child is the treatment group, and zero otherwise
$Treat_i * Post_t$	$Post_t$ multiplied by $Treat_i$ (the difference-in-differences term)
Age	An array of indicators for the child's age on the first day of the respective time period, in years (these change between time periods)
Gender	Indicator that equals one if child is female, and zero otherwise
Race	An array of indicators for the child's race
Hispanic	Indicator that equals one if the child is Hispanic, and zero otherwise
Index Date	An array of indicators for the month a child is first eligible (to control for trends over time and seasonal effects)
Prior Evidence of Asthma	An indicator that equals one if the child had a primary asthma diagnosis in the baseline period, and zero if the child's baseline claims included asthma only as a secondary diagnosis
Medicaid Eligibility	Two indicator variables for Health Start or "other" Medicaid eligibility at the child's index date (both equal zero if Healthy Families eligibility)

Note: The omitted categories in the regression are 2 years old; male; white; non-Hispanic; entered sample in July 2008; secondary asthma diagnosis in baseline period; Healthy Families Medicaid eligibility; living in comparison county ($Treat_i = 0$); baseline time period ($Post_t = 0$).

The critical assumption in this nonexperimental design is that changes in outcomes that occurred in counties without the intervention are representative of the changes that would have occurred in the treatment county in the absence of the intervention. We used baseline data to explore whether trends in outcomes in the treatment and comparison counties were similar prior to the intervention, using an identical model. For example, we used July 2004 through June 2005 as a first period and ran a difference-in-differences model with July 2005 through June 2006 as the second time period; failure to find a statistically significant coefficient for the main interaction term $(Treat_i * Post_t)$ was interpreted as a failure to reject the null hypotheses of no differential trends in the baseline period.

Subgroup Analyses

For most subgroup analyses, we estimated a separate model for each subgroup, using the same model that was used in the primary analysis. A different procedure was used for the high-touch subgroup analysis, because high-touch children are likely different from the average child in the comparison group, and may have had different trends in the absence of the intervention. To form the comparison group, we used propensity score reweighting (Busso et al. 2009), which gives higher weights to children in the comparison counties who are as similar as possible to the children in the high-touch treatment group on measured characteristics. To do this, we first used a logit model to estimate the probability a child is in the high-touch group in the intervention period, using the control variables from the main analysis and the child's outcomes from the baseline period.[17] This is

[17] We used the following baseline outcomes: number of ED visits and hospital admissions for asthma, a linear spline with the annualized number of outpatient visits for any reason, binary variables that equaled one if the child filled

known as a propensity score. We then re-estimated the difference-in-differences models, weighting the observations with the original weight (based on the length of time enrolled in Medicaid) multiplied by a reweighting score derived from the child's propensity score.

Power Calculations

To assess the statistical power of our evaluation, we performed power calculations to estimate the MDEs for our difference-in-differences regressions. Table 15 presents MDEs with varying incident rates for a binary outcome variable. For a point of comparison, the percentage of children in the control group who had an ED visit for asthma was 48 percent, and 11 percent had a hospital admission for asthma. We use the sample sizes of our research sample. As seen in the first row of the table, we can expect 80 percent power to detect differences of 2.7 percentage points (at the 95 percent confidence level) for an outcome variable with a rate of 5 percent in the control group. Statistical power is stronger if the incidence rate is closer to 50 percent and lower if it is closer to 0 percent.

Table 15. Minimum Detectable Effects for Cincinnati Children's Analysis

	Minimum Detectable Effect	
Mean of Binary Outcome Variable	Estimate (percentage points)	As a Percentage of Mean
1 or 99%	1.03	1.0
10 or 90%	3.10	3.4
20 or 80%	4.14	5.2
30 or 70%	4.74	6.8
40 or 60%	5.07	8.4
50%	5.17	10.3

Note: The MDE calculations assume a difference-in-differences regression model with a 95 percent confidence level for a two-tailed test, 80 percent power level, R-squared equal to 0.05, and a sample with 1,691 patients in the treatment group and 7,913 in the control group.

(continued)

a prescription for any controller medication or rescue medication, and the percentage of days covered with controller medications.

SECTION III

METHODS FOR EVALUATING MONROE PLAN'S PEDIATRIC ASTHMA CARE ENHANCEMENT INTERVENTION

Random Assignment of Practices

We randomized eligible practices identified by Monroe in a one-to-one treatment-to-control group ratio, and stratified the randomized assignment to guarantee balanced characteristics of treatment and comparison practices. First, we stratified the random assignment of all practices by the number of eligible Monroe enrollees at each practice (in the baseline period) using three strata: large (more than 200 eligible patients), mid-sized (from 100 to 200), and small (fewer than 100). Among the 25 practices that met the eligibility criteria described above, 4 fell into the large stratum, 8 were mid-sized, and 13 were small. This stratification ensured a roughly even split between treatment and control practices within each category of practice size. Among mid-sized and small practices, we also stratified on whether or not the practice was a federally qualified health center (FQHC). There were four FQHCs among the mid-sized practices and two among the small practices. Last, we also stratified small practices by whether the practice has a single physician (6 practices) or multiple physicians (7 practices). This stratification applies only to small practices, because all practices in the large and mid-sized strata have multiple physicians. The final randomization assignment is presented in Table 16.

At the same time that Monroe conducted its intervention, a research team at the University of Rochester (UR) conducted a similar practice-based intervention in Rochester. Seven practices in Monroe's final sample are also in the UR sample. Both groups agreed to let us simultaneously randomly assign practices for both studies with the goal of balancing crossover practices across the two studies. To the extent that it was possible, we randomly assigned practices in such a way that half of the crossover practices that were treatment practices in Monroe's study were also treatment practices in the UR study and vice versa.

Table 16. Monroe Experimental Group Assignment

Practice	FQHC	Physician(s)	BCQ-II Experimental Group	UR Experimental Group	Breath of Hope Initiative
Large Practices (>200):					
RGPA	Yes	Multiple	Treatment	Control	Treatment
Strong Pediatrics	No	Multiple	Control	Treatment	Treatment
GHS Peds	Yes	Multiple	Control	--	--
Finger Lakes Medical Associates	No	Multiple	Treatment	--	--
Mid- sized Practices (100-200):					
Wayne Medical Group	Yes	Multiple	Treatment	--	--
Unity West Main Peds	Yes	Multiple	Control	Control	Treatment
Lourdes	No	Multiple	Control	--	--
UMA	No	Multiple	Control	--	--
UHS	No	Multiple	Treatment	--	--
Anthony Jordan Health Center	Yes	Multiple	Treatment	Treatment	Treatment
Westside Health Center	Yes	Multiple	Control	Control	--
Highland - Family Med	No	Multiple	Treatment	Treatment	--
Small Practices (<100):					
Stony Brook Pediatrics (Red Jacket)	No	Multiple	Treatment	--	--
Southern Tier Peds	No	Multiple	Control	--	--
David Breen	No	Single	Control	--	--
Panorama Pediatric Group	No	Multiple	Treatment	--	--
Oak Orchard Community Health Center	Yes	Multiple	Control	--	--
Clinton Family Health	Yes	Multiple	Treatment	Treatment	--
William Bayer	No	Single	Treatment	--	--
John Maerz	No	Single	Treatment	--	--
Azmat Saeed	No	Single	Control	--	--
Eunice Nayo	No	Single	Treatment	--	--
Unity Associates in Family Practice	No	Multiple	Control	--	--
Endwell	No	Multiple	Treatment	--	--
Abdul Qadir	No	Single	Control	--	--

Source: Practice information collected prior to randomization. Practices are sorted by size in descending order.

Identifying the Study Population

To identify the study population, we used claims and enrollment data submitted by Monroe to identify Monroe enrollees who met the eligibility criteria for the study. In total, Monroe provided data for 18,269 children enrolled in the Monroe Medicaid managed care plan between January 1, 2008, and June 30, 2011. Table 17 shows the number of children we identified at each step of the research sample identification process, which was based on the study eligibility criteria in the Monroe design protocol. Children had to meet all study eligibility criteria to be included in the study population, including the requirement that they have at least one medical claim with a diagnosis of asthma while aged between 2 and 19. In total, 7,731 children met all the sample criteria. The primary reason children with asthma were excluded from the research sample was the failure to meet the criteria of being identified as a patient of a treatment or control group practice.

Table 17. Study Population Meeting Eligibility Criteria for Monroe Intervention

Identified Children	N
Number of Monroe Enrollees in Enrollment Data	18,269
Who had an insurance claim	18,048
Who had at least one insurance claim for asthma[a]	17,277
Who had claim for asthma while between ages 2 and 19	16,493
Who was affiliated with a treatment or control practice	9,233
Who had index date before January 1, 2011	8,441
Who had index date before January 1, 2009, and were (still) enrolled on January 1, 2009, or had index date during intervention period	8,105
Who were not identified as outliers by UNC team during ROI analysis	8,099
Who had any enrollment data after the index date	8,065
Who did not have more than 7 continuous months not enrolled in Monroe plan (enrollment gaps)	7,753
Who had at least one month of baseline data	7,731
Final Sample	7,731

Source: Monroe eligibility and claims data. A diagnosis code beginning with 493.xx was used to identify asthma.

[a]Primary or Secondary Diagnosis Code of 493.xx

Of the 7,731 children in the sample, 3,721 (48.1 percent) were associated with a treatment practice and 4,010 (51.9 percent) with a control practice. We identified each child's first date of eligibility and refer to this as the child's index date. About 45 percent of the children were identified using calendar year 2008 data and received an index date of January 1, 2009. The remaining children were identified between January 2, 2009, and January 1, 2011, and their first date of eligibility is the date of their first claim for asthma (ICD9 = 493.xx).[18] We then identified the dates corresponding to each child's "baseline period" (the period before the index date) and "study period" (the months following the child's index date). The study period was also subdivided into the first, second, and third years after the child's index date (study years 1, 2, and 3). Note that the baseline period does not refer to the same calendar dates for all children; likewise, neither does the study period (except subgroup analysis where the sample is limited to children with an index date of January 1, 2009).

As shown in Table 18, we observed data through the end of the intervention period for 70 percent of the children (N=5,434). The period of observation for the remaining children was truncated because they became ineligible prior to June 30, 2011 (N=1,954), switched from their original practice to a practice in a different experimental group (N=264), or both (N=79).

[18] We ended sample identification in January 1, 2011, so that we could observe outcomes for all children for 6 months or longer. We received data for claims through June 30, 2011.

Table 18. Data Completeness in Monroe Sample

	Monroe Plan Enrollment		
	Complete Through June 30, 2011	Ends Before June 30, 2011	Total
Without Switch from Treatment or Control Practice			
Treatment practice	2,643	920	3,563
Control practice	2,791	1,034	3,825
Subtotal	5,434	1,954	7,388
Switch from Treatment or Control Practice			
Switch from treatment to control practice	28	10	38
Switch from control to treatment practice	54	8	62
Switch from treatment to nonexperimental practice	79	41	120
Switch from control to nonexperimental practice	103	20	123
Subtotal	264	79	343
Total	5,698	2,033	7,731
Treatment practice	2,750	971	3,721
Control practice	2,948	1,062	4,010

Source: Monroe eligibility and claims data. An ICD-9 diagnosis code beginning with 493.xx was used to identify asthma. Attribution to treatment and control practices was performed by Monroe prior to data transmission.

Outcome Indicators

Mathematica used claims data and enrollment data provided by Monroe to identify demographic characteristics, construct measures of health care use for each child's baseline year, and examine outcomes for children who became eligible for the study. Outcome variables are listed in Table 19.

Table 19. Monroe Outcome Variables

Outcome	Regression Model			2nd-Stage GLM Family/Link Functions
	Logit	Two-Stage GLM	Multi-nomial Logit	
Emergency Department (ED) Visits				
Proportion of patients with one or more ED visits for asthma	✓			--
Average annualized number of ED visits for asthma		✓		OLS[a]
Proportion of patients with zero, one, or more than one ED visit for asthma			✓	--
Proportion of patients with one or more ED visits for any reason	✓			--
Average annualized number of ED visits for any reason		✓		Gamma/Log
Proportion of patients with zero, one, or more than one ED visit for any reason			✓	--
Hospitalization Admissions				
Proportion of patients with one or more hospital admissions for asthma	✓			--
Average annualized number of hospital admissions for asthma		✓		OLS[a]
Proportion of patients with one or more hospital admissions for any reason	✓			--
Average annualized number of hospital admissions for any reason		✓		OLS[a]
Outpatient Visits				
Proportion of patients with one or more outpatient visits for asthma	✓			--
Average annualized number of outpatient visits for asthma		✓		OLS[a]
Proportion of patients with one or more outpatient visits for any reason	✓			--
Average annualized number of outpatient visits for any reason		✓		Poisson/Power(0.9)
Prescription Drug Use				
Controller Medications				
Average number of fills for controller medications per year		✓		Poisson/Power(0.7)
Proportion of patients with zero, one to three, four to six, seven to nine, or more than nine controller medication fills per year			✓	--
Average days of medication available (DMA) for controller medications per year (the sum of the "quantity" field in the claims data)		✓		Poisson/Log
Proportion of patients with zero, more than zero and up to 90, more than 90 and up to 180, more than 180 and up to 270, and more than 270 DMA for controller medications			✓	--
Average percentage of days covered (PDC) for controller medications (the number of days of controller medication is available divided by the number of days in the period, adjusted for truncated observation periods)		✓		Poisson/Log
Proportion of patients with PDC over 80 percent or 90 percent (two outcomes)	✓			--
Average of the ratio of controller medications DMA to controller plus rescue medications DMA, among those with at least one fill of an asthma medication of any kind		✓		Gaussian NLLS/Linear
Rescue Medications				--
Average number of fills for rescue medications per year		✓		Gamma/Log
Proportion of patients with zero, one to three, four to six, seven to nine, or more than nine rescue medication fills per year			✓	--
Average DMA per year for rescue medications		✓		Poisson/Power(0.4)

Outcome	Regression Model			2nd-Stage GLM Family/Link Functions
	Logit	Two-Stage GLM	Multi-nomial Logit	
Proportion of patients with zero, more than zero and up to 90, more than 90 and up to 180, more than 180 and up to 270, and more than 270 DMA per year for rescue medications			✓	--
Appropriate medications: Dummy variable that equals one if (1) the child had at least 6 office visits, 1 ED visit, or 1 hospitalization for asthma in the baseline period ("persistent asthma") AND filled at least 1 controller and at least 1 rescue medication in the respective year, or (2) the child had at least 1 office visit for asthma in the baseline period ("non-persistent asthma") AND filled at least 1 rescue medication in the respective year	✓			

Note: ED visits, inpatient admissions, and outpatient visits for asthma include any utilization where the primary or secondary diagnosis is for asthma (ICD-9 493.xx).

[a] An ordinary least squares (OLS) regression was used in the second stage.

We constructed annualized outcome measures for each child's baseline period and study period, and for each child's study years 1, 2, and 3. We weighted all outcomes analyses to account for the number of months each child was enrolled in Monroe's Medicaid plan. For the continuous outcome measures, children were weighted by the percentage of months they were observed in the respective time period. For binary indicator outcomes, we use the same weight we created for continuous outcomes, except that a child always receives the full weight if the outcome was observed (for example, if the child was hospitalized). Weights were then normalized to have a mean of 1.00.[19] We truncated the period of observation for children whose Monroe enrollment ended prior to the end of the study and for children who switched away from a treatment or control practice, and weighted the children accordingly.[20]

Empirical Methods

To evaluate the PACE intervention's impacts on quality of care, we compared the means (and percentages) for child-level outcomes across the treatment and control groups.[21] As mentioned previously, children were assigned into the treatment and control groups based on whether they were affiliated with a treatment or comparison group practice. We used regression models with child-level explanatory control variables—demographic information and baseline outcomes—to improve the predictive power of our model and reduce the unexplained variation in intervention period outcomes. Explanatory variables in the regressions are listed in Table 20; these variables were all calculated with data from the index visit or from the baseline period (up to 12 months prior to the index visit). As described above, observations were weighted to account for the number of months each child was enrolled Monroe's Medicaid plan.

We estimated logit models for the binary outcomes, with one observation for each child in the regression. For categorical outcomes, we used multinomial logit regression models. For binary and categorical outcomes, we calculated the average marginal effect and calculated standard errors with the delta method. For categorical variables, statistical significance was determined from a chi-squared test, with null hypothesis of zero treatment effect for all categories.

The continuous outcome variables were irregular (were strictly non-negative, had a masses at zero, had skewed distributions, etc.); therefore, we estimated the difference-in-differences model with two-part models, with a GLM in each stage. These models estimate the probability of a positive outcome in the first stage, and then model the outcome level—conditional on positive expenditures—in the second. The first-stage GLM was estimated with a logit link function and binomial distribution. We used statistical tests to choose the GLM link function and distributional family that best fit the control group's data (Table 19, final column).[22] We then combined the two

[19] For example, a child who was enrolled in 8 of the 12 months in the intervention period would receive a weight of two-thirds for continuous outcomes. For a binary outcome, the child would receive a weight of one if the outcome occurred and two-thirds if it did not. We would then divide by the average weight in the sample so that the sum of the weights equals the number of sample members.

[20] A child who switched from a treatment practice to a different treatment practice, or from a control practice to different control practice, remained in the sample.

[21] We used Student's t-tests and χ^2 tests to compare the treatment and comparison groups at baseline.

[22] For some rare outcomes, small sample sizes (particularly in study years 2 and 3) prevented the maximum likelihood function from converging in one or more periods. For these outcomes, we used an OLS regression in the second stage for all regressions.

stages to calculate the average marginal effect of the intervention on the outcome, and bootstrapped standard errors that accounted for stratification of practices by size and intra-practice correlations. This method was used for our main impacts analysis (separate models for the study period and study years 1, 2, and 3). Models were estimated with Stata/MP 11 for Windows, and baseline comparisons were conducted with SAS 9.2.

We present regression-adjusted means for the treatment and comparison groups and the regression-adjusted difference between the groups. For all regression models, we computed *p*-values using standard errors that allow for intra-practice correlations (that is, standard errors clustered by practice) because random assignment occurred at the practice level. All *p*-values are two-tailed. We interpret regression-adjusted difference as estimate of the causal effect of being assigned to the treatment group on the index date.

Table 20. Explanatory Variables Included in Monroe Regression Models

Variable	Description
Treatment Practice	Indicator that equals one if the child is a treatment group practice, and zero otherwise
Age	An array of indicators for the child's age on their index date
Gender	Indicator that equals one if child is male, and zero otherwise
Race/Ethnicity	An array of indicators for the child's race/ethnicity
Prior Evidence of Asthma[a]	Two indicators that equal one if the child had a primary or secondary asthma diagnosis in the baseline period for (1) 6 to 9 office visits, 1 to 4 ED visits, or 1 hospitalization for asthma; or (2) more than 9 office visits, more than 1 hospitalization, or more than 4 ED visits, respectively
Common Comorbid Conditions[b]	Indicator variables for acute respiratory infection, ear infection (otitis media), attention deficit disorder, pneumonia, allergies, and/or obesity in the baseline period
Practice Size	Two indicators for affiliation with a small or mid-sized practice (both equal zero if affiliated with a small practice)
Other Initiatives	Indicator that equals one if affiliated with a University of Rochester treatment or control practice, an indicator that equals one if affiliated with a University of Rochester treatment practice, and an indicator that equals one if affiliated with a Breath of Hope treatment practice (see Table 16)
Index Date	Indicator variables for the child's index date (one dummy for each year a child has an index date, to control for trends over time)
Baseline Period Outcomes	The number of ED visits for any reason, the number of ED visits for asthma, the number of hospital admissions for any reason, the number of hospital admissions for asthma, the annualized number of outpatient visits for any reason, the annualized number of outpatient visits for asthma, indicators for any controller or rescue medication fill (two dummies), and the percentage of days covered with controller medications

Note: The omitted categories in the regression are 2 to younger than 5 years old on index date; female; white non-Hispanic; less than 6 office visits and no ED visits or hospitalizations for asthma in baseline; with no common comorbid conditions; with no baseline utilization; index date of January 1, 2009; affiliated with a large practice in the BCQ-II control group ($Treat_i = 0$), the University of Rochester control group, and not in the Breath of Hope initiative.

[a] We used claims data from the 12 months before each child's index date. We classified an ED visit or hospital admission as being for asthma if its primary or secondary diagnosis was for asthma.

[b] We identified common comorbid conditions from ED and hospital claims with any diagnosis of acute respiratory infection (ICD-9: 460.xx to 466.xx and 786.xx), ear infection (382.xx), pneumonia (486.xx), attention deficit disorder (314.xx), pneumonia (486.xx), allergies (477.xx), and/or obesity (278.xx).

ED = emergency department.

Subgroup Analyses

To evaluate whether intervention effects occurred among distinct subgroups of the study population, we performed subgroup analyses for the 11 subgroups listed in Table 21. We began by comparing means between treatment and control groups for the explanatory (control) variables and key outcomes (baseline period and study period, and study years 1, 2, and 3).

Table 21. Monroe Subgroup Analyses

	Subgroups
Child's First Date of Eligibility	• Children first eligible on January 1, 2009 • Children first eligible after January 1, 2009
Practice Location	• Southern tier practices (6 treatment, 6 control) • Rochester area practices (7 treatment, 6 control)
University of Rochester (UR) Experiment	• UR treatment or control practices (4 treatment, 3 control) • Other practices not in UR experiment (9 treatment, 9 control)
Breath of Hope (BoH)	• BoH practices (2 treatment, 2 control) • Other practices not in BoH initiative (11 treatment, 10 control)
Practice Size	• Large practices (2 treatment, 2 control) • Medium practices (4 treatment, 4 control) • Small practices (7 treatment, 6 control)

The experimental design for Monroe was intended to balance observable (and unobservable) characteristics between the treatment and control practices as a whole. We may also expect balance within (practice size) stratum, though its likelihood decreases with the number of clusters. Observable characteristics or baseline outcomes would not necessarily be balanced for the remaining subgroups. Such differences may or may not be statistically significant, even when standard errors are clustered by practice.

As may have been expected, we found that observable characteristics and baseline outcomes were not balanced for many of the subgroups. Therefore, we calculated difference-in-differences for the subgroup analyses instead of comparing intervention-period outcomes alone. difference-in-differences analysis compares the change in outcomes that occurred for children in the treatment group (between the baseline and follow-up periods) to the change that occurred in the control group. The motivation for this approach is that it "differences out" any biases that would result from permanent differences between the two groups (for the patients/practices in a particular subgroup). Initial difference-in-differences analyses were not adjusted for covariates using regressions. The difference-in-differences estimates were calculated separately for each subgroup listed in Table 21 for eight outcomes: average annualized number of ED visits for asthma, average annualized number of ED visits for any reason, average annualized number of outpatient visits for asthma, average annualized number of office visits for asthma, average annualized number of office visits for any reason, average number of fills for controller medications per year, average number of fills for rescue medications per year, and appropriate medications.

Where the unadjusted difference-in-differences estimates merited additional investigation—the small, medium, and large subgroups and the Rochester experiment subgroup—we then continued our analysis by computing regression-adjusted difference-in-differences estimates. First, we estimated a regression for the entire intervention period (two observations per child for annualized baseline and intervention period outcomes, respectively) for each outcome, plus a second regression with interaction terms to obtain difference-in-differences estimates for each year separately (with at least two and up to four observations per child). Because the control variables used in the main analysis (described above) were not available for the baseline period observations, we estimated a

linear regression model with patient fixed effects,[23] which control for any patient characteristics that do not vary over time.

Power Calculations

To assess the statistical power of our evaluation, and the effect of the clustered design, we performed power calculations to estimate the MDEs for our regressions. These calculations were performed *ex post,* and thus baseline data were available to estimate outcome means, standard errors, inter-class clustering (ICC; the proportion of total variation across all patients that is due to variation between practices), and R-squared (the percentage reduction in the group- and patient-level variance of the estimates by using regression models). In Table 22, we present MDEs for eight selected outcomes (power calculations for other variables are available upon request). As seen in the first row of the table, we can expect 80 percent power to detect differences of 0.03 basis points (at the 95 percent confidence level). This strong predictive power is due to two reasons. First, we found ICCs to be relatively low for some of the outcome variables, which indicates that the clustered experimental design does not cause large reductions in statistical power (relatively to the hypothetical experiment where one randomizes patients instead of practices). Second, our group- and patient-level R-squared estimates were high, which indicates that our control variables explain a large fraction of the variation in outcomes and thereby increases our ability to detect outcomes.

[23] Patient age, the only child characteristic not fixed over time, was included as a control variable.

Table 22. Minimum Detectable Effects for Monroe analysis

	Assumptions			Power Calculations: Minimum Detectable Effect		
Outcome	Mean	Standard Error	ICC	Estimate	As a Percentage of Mean	Equivalent N[a]
Emergency Department (ED) Visits						
Percentage with ED visit for any reason	42.70	0.0056	0.0175	0.08	0.18	1,211
Percentage with ED visit for asthma	13.79	0.0039	0.0209	0.07	0.49	1,040
Average annualized number of ED visits for any reason	1.2544	0.0293	0.0084	0.0028	0.22	2,156
Average annualized number of ED visits for asthma	0.3280	0.0146	0.0086	0.0016	0.50	2,121
Office Visits						
Percentage with office visit for any reason	95.64	0.0023	0.0062	0.03	0.03	2,648
Percentage with office visit for asthma	53.77	0.0057	0.0335	0.18	0.34	682
Average annualized number of office visits for any reason	7.4093	0.0846	0.0925	0.0186	0.25	262
Average annualized number of office visits for asthma	1.5426	0.0356	0.0102	0.0021	0.14	1,867

Note: The MDE calculations assume (1) a 95 percent confidence level for a two-tailed test, and (2) an 80 percent power level for 13 practices with 3,721 patients in the treatment group and 12 practices with 4,010 patients in the control group. The estimates for mean, standard error, and inter-class clustering (ICC) of the outcome variable are estimated from baseline period data for all practices and patients in the research sample. R-squared (not shown) was estimated using control group data (only) from the intervention period because some control variables are not available in the baseline period.

[a] The first row indicates that our evaluation (25 clusters and 7,731 patients) is equivalent to an unclustered experimental design with 1,211 patients because the ICC equals 0.0175. This calculation does not account for the effect of control variables.

REFERENCES

Ai, C., and E.C. Norton. "Interaction Terms in Logit and Probit Models." *Economics Letters,* vol. 80, no. 1, 2003, pp. 123-129.

Blackwell, M., S. Iacus, G. King, and G. Porro. "cem: Coarsened Exact Matching in Stata." *The Stata Journal,* vol. 9, no. 4, 2009, pp. 524-546.

Busso, M., J. DiNardo, and J. McCrary. "New Evidence on the Finite Sample Properties of Propensity Score Matching and Reweighting Estimators." Discussion Paper no. 3998. IZA Discussion Paper Series. Bonn, Germany: IZA, February 2009.

Cameron, A.C., and P.K. Trivedi. *Microeconometrics: Methods and Applications.* Cambridge: Cambridge University Press, 2005.

Duan, N., W.G. Manning, C.N. Morris, and J.P. Newhouse. "A Comparison of Alternative Models for the Demand for Medical Care." *Journal of Business & Economic Statistics,* vol. 1, no. 2, 1983, pp. 115-126.

Iacus, S.M., G. King, and G. Porro. "Multivariate Matching Methods That Are Monotonic Imbalance Bounding." *Journal of the American Statistical Association,* vol. 106, no. 493, 2011, pp. 345-361.

Imbens, G.W., and J.D. Angrist. "Identification and Estimation of Local Average Treatment Effects." *Econometrica,* vol. 62, no. 2, 1994, pp. 467-475.

Jones, A.M. "Health Econometrics." In *Handbook of Health Economics,* vol. 1, edited by J.C. Anthony and J.P. Newhouse. Amsterdam: North-Holland, 2000, pp. 265-344.

Wooldridge, J.M. Econometric Analysis of Cross Section and Panel Data. Cambridge & London: MIT Press, 2002.

APPENDIX B

ROI TECHNICAL METHODS

In this appendix, we describe how we constructed the primary return on investment (ROI) analysis for the Monroe Plan for Medical Care and the Alameda Alliance for Health. We computed the per-member-per-month (PMPM) savings or loss cash flow (CF) for each primary stakeholder using the following formula for each year of implementation (denotedby *i*):

(1) $CF_i = [(PMPM_{T,i} - PMPM_{T,0}) - (PMPM_{C,i} - PMPM_{C,0})]$

where:

$PMPM_{T,i}$ = average PMPM treatment group costs in the *ith* implementation year

$PMPM_{T,0}$ = average PMPM treatment group costs in the baseline year

$PMPM_{C,i}$ = average PMPM control group costs in the *ith* implementation year

$PMPM_{C,0}$ = average PMPM control group costs in the baseline year

We multiplied estimated CF_i by the total number of treatment member months during the *ith* implementation year to arrive at annual cash flow (ACF) and subtracted annual operating costs (OC) from ACF to arrive at annual net cash flows (NCFs) for each implementation year.

(2) $NCF_i = ACF_i - OC_i$

We calculated net present value (NPV) using investment costs (I_0), NCF_i, and a discount rate (*r*) with the following formula:

(3) $NPV = I_0 + \Sigma[NCF_i / (1+r)i]$

In addition to NPV, we calculated a discounted benefit-cost ratio (BCR):

(4) $BCR = [\Sigma ACF_i / (1+r)_i] \div (I_0 + \Sigma OC_i / (1+r)_i)$

where the numerator is the sum of ACFs resulting from the intervention discounted at the discount rate (*r*) and the denominator is the sum of the investment and discounted OC.

Based on reported costs and revenues, we conducted additional sensitivity analyses for ROI. For the main ROI analysis for all grantees, we calculated ROI using investment and OC, excluding those related to the evaluation, because a hospital or health plan that implements an intervention external from a research study might not incur these costs. For the Monroe plan, we included a sensitivity analysis that includes costs related to the BCQII evaluation as well.

We also calculated ROI with and without considering BCQII grant revenue that grantees have received from CHCS, because such funds might not be available to other organizations external to an initiative such as BCQII.

APPENDIX C

KEY INFORMANT INTERVIEWS

Throughout the intervention period, but focused heavily during the intervention's final year, Mathematica conducted key informant interviews with individuals working most closely with each grantee's intervention. The purpose of the interviews was to gain a ground-level understanding of each intervention, to learn about challenges encountered and successes achieved, and to gather insight as to what extent the intervention was achieving the intermediate steps in the grantee's logic model. Additional information on persons interviewed by grantee is provided in Table D.1.

Table 1: Key Informant Interviews: Persons Interviewed by Grantee

Alameda- CHRCO ATTACK Clinic Intervention		
Person Interviewed	**Organization**	**Role**
ATTACK Clinic Provider (Nurse Practitioner, MPH)	CHRCO	Provides asthma education to children at the ATTACK clinic
Practice administrator for CHRCO ambulatory clinic (MBA)	CHRCO	Financial management and strategic planning for CHRCO primary care clinc
Asthma Educator	Americorps/ CHRCO	Provided asthma education to patients at the ATTACK clinic
ED Physician (MD)	CHRCO	Works in CHRCO ED
Asthma Coordinator	Americorps	Coordinates patient follow-up care surrounding the ATTACK clinic visit
Asthma Program Manager	CHRCO	ATTACK lead for CHRCO
Pediatrician (MD)	CHRCO	ATTACK lead for CHRCO
Grants Coordinator	CHRCO	Program development and grant writing
Asthma Coordinator (RN)	CHRCO	Provided asthma education to patients at the ATTACK clinic and conducted research studies
Director of Care Coordination (MPH, CHES)	Alameda Alliance	ATTACK lead for the Alliance
CCHMC's AIC Intervention		
Person Interviewed	**Organization**	**Role**
Mona Mansour (MD), Director, Primary Care and School Health Services, Division of General and Community Pediatrics	CCHMC	CCHMC BCQII lead and co-principal investigator
Jeffrey Simmons (MD), Co-Director, Hospital Medicine	CCHMC	BCQII co-principal investigator, and strong involvement in inpatient intervention activities
Keith Mandel (MD), Vice President of Medical Affairs, Physician-Hospital Organization	CCHMC	Senior leadership perspective on BCQII from clinical vantage; involvement with Beacon Community grant
Karen Tucker (RN), Clinical Director, A6South and LA1W	CCHMC	RN lead for inpatient intervention activities for BCQII work
Michael Lake, Senior Decision Support Analyst, James M. Center for Health Systems Excellence	CCHMC	Analyst working on financial results associated with BCQII intervention activities
Alma Helpling, Vice President, Budget department	CCHMC	Senior leadership perspective on BCQII from a financial vantage
Susan Wade-Murphy, Home Care Director, Division of Home Care Services	CCHMC	Lead for home health intervention activities for BCQII work
Brandy Weiner (MSW)	CCHMC	AIC Care Coordinator
Kristin Line, James M. Anderson Center for Health Systems Excellence	CCHMC	Project Manager assisting Mona Mansour in BCQII work
Tracey Huentelman, James M. Anderson Center for Health	CCHMC	Project Manager assisting Mona Mansour in BCQII work

Systems Excellence		
Monroe Plan's PACE Intervention		
Person Interviewed	**Organization**	**Role**
Medical Director (MD, Pediatrician)	Wayne (Part of Rochester General Health System)	PACE lead; completed all chart audits
Associate Medical Director (DO)	Highland Family Medicine	PACE lead
Nurse Manager (RN)	Panorama Pediatric Group	Completed some of the chart audits
Nurse Manager (RN)	Anthony Jordan Health Center	Completed all chart audits
Medical Director (MD)	Rochester General Pediatric Associates	PACE lead; completed some chart audits
Chief Medical Officer	Monroe Plan	Conceptualized and led PACE intervention; recruited treatment and control group practices to participate in PACE; met with each treatment group practice to review chart audit results.
Director of Informatics	Monroe Plan	Prepared and submitted Monroe chart audit, survey, and claims data for the BCQII evaluation; advised on certain technical aspects of the Monroe evaluation.
Grants Coordinator	Monroe Plan	Led training of practice staff on electronic chart audit tool; led communication with PACE practices and PACE data collection.