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Estimates of ACO savings in the presence of provider and beneficiary selection

Mariétou H. Ouayogodé, PhD,
Department of Population Health Sciences, University of Wisconsin School of Medicine and Public Health, 610 Walnut Street, Madison, Wisconsin, 53726

Ellen Meara, PhD,
Harvard University, T.H. Chan School of Public Health, 677 Huntington Avenue Kresge, 4th Floor, Boston, Massachussetts, 02115.

Kate Ho, PhD,
Princeton University, Department of Economics, 285 Julis Romo Rabinowitz Building, Princeton, New Jersey 08544

Christopher M. Snyder, PhD,
Dartmouth College, Department of Economics, 301 Rockefeller Hall, Hanover, New Hampshire, 03755

Carrie H. Colla, PhD
The Dartmouth Institute for Health Policy & Clinical Practice, Geisel School of Medicine at Dartmouth, Williamson Translational Research Building, Level 5, 1 Medical Center Drive, Lebanon, New Hampshire, 03756

Abstract

Background: Medicare's accountable care organizations (ACOs)—designed to improve quality and lower spending—were associated with growing savings in previous studies. However, savings estimates may be biased by beneficiary sorting among providers based on healthcare needs and by providers opting into the program based on anticipated gains.

Methods: Using Medicare administrative claims (2009–2014), we compared annual spending changes after provider organizations joined ACOs to changes in non-ACOs (controls). To address provider selection, using novel data to identify non-ACO organizations, we restricted controls to comparably large provider organizations. To address beneficiary selection, we (a) estimated within-organization (including non-ACO comparison organizations) spending changes, (b) estimated within-beneficiary spending changes, (c) incorporated beneficiaries without qualifying healthcare expenses, and (d) used a fixed beneficiary ACO assignment using the pre-ACO period.

marietou.ouayogode@wisc.edu.

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Results: Each year, 19% of Medicare beneficiaries switched provider organizations. Spending was higher for switchers than stayers ($3,163, p<.001) and grew more the next year ($2,004; p<.001). Starting from a baseline regression modeled on previous ACO evaluations, estimated savings varied widely as we sequentially introduced methods to address selection. Combining methods, however, generated more stable estimated ACO savings of $46 ($p$=.022), averaged across cohorts.

Conclusions: When implementing a comprehensive suite of methods to adjust for provider and beneficiary selection, we estimated ACO savings that grew over time. Our estimates are in line with, but smaller than, previous estimates in the literature. Implementing piecemeal adjustments produced misleading results.

Implications: Our results confirm the importance of selection for savings estimates and for provider organizations managing costs and quality. Attribution rules that consider multiple years may help mitigate the impact of beneficiary churn for providers and payers. Implementing payment reform by randomizing early participants, or implementing fully across selected markets, may better serve efforts to evaluate and improve payment models.

Keywords

Accountable Care Organization (ACO); Medicare; selection; Shared Savings Program; alternative payment models; evaluation methodology

1. Introduction

In 2012, the Centers for Medicare and Medicaid Services (CMS) launched accountable care organizations (ACOs), aiming to lower spending and improve quality. The programs hold provider organizations accountable for cost and quality for a beneficiary population in return for a share of realized savings (or losses). Presently, there are over 1,000 ACOs, covering over 40 million lives [1].

Previous studies estimated that early ACO entrants experienced initial savings of 1–5% [2–6], savings which grew with ACO contract experience [6]. These studies used a difference-in-differences methodology, comparing changes in spending before and after ACO formation to changes over the same period in control provider groups. This paper builds on prior research comparing trends in spending between ACOs and non-ACOs by exploring the effects of nonrandom sorting of beneficiaries across providers (beneficiary selection) and nonrandom assignment of providers into ACO programs (provider selection).

Starting with standard data and methods, and using novel data to identify non-ACO provider organizations, we introduce a sequence of methods to address various forms of selection bias, evaluating the use of various econometric models and comparing ACO performance to counterfactuals that take into account different methodological limitations. The sequence of methods attempts to hone in on an answer to the question: “How did ACOs change the level and trajectory of spending compared to what would have happened absent the policy?”

2. Methods

2.1. Conceptual framework

When measuring the effect of ACOs on patient care and spending, two sources of selection can cloud inference: that of beneficiaries seeking care from a particular provider (at times in conjunction with a physician through a referral) and that due to differences between provider organizations selecting into ACO programs and those that do not. We use the term “selection” to describe nonrandom assignment of patients and provider organizations into ACOs whether or not such assignment happens by deliberate choice.

Attribution of a beneficiary to an ACO provider organization, which is then accountable for that beneficiary’s spending and quality, occurs retrospectively if that organization provided a plurality of the beneficiary’s annual primary-care expenditures. Such retrospective attribution can be unstable, leading to frequent switching of beneficiaries in and out of ACOs [7, 8]. Further, ACO entry and exit may be correlated with both the level and trend in a beneficiary’s medical spending. If beneficiaries disproportionately exit ACOs in advance of high-spending periods, apparent savings will be estimated even if the ACO did nothing to improve efficiency. Provider organizations participating in ACOs could encourage such switches, for example, by excluding specialties like oncology that draw high-service patients [9, 10], though we have no direct evidence to support or refute this possibility. The opposite dynamic could also operate: beneficiaries may be attributed to ACOs when care needs are high [11, 9] but lose attribution in years with little or no qualified spending. Such nonrandom selection into ACOs is plausible because ACOs are large organizations that often include academic medical centers or networks of physicians and resources likely to be used by beneficiaries with the onset of a new diagnosis or the flare-up of a chronic condition. When spending reverts to lower levels after an episode of intense utilization, beneficiaries could easily move back to non-ACO providers. If use reverts to zero, individuals may be unattributed to an ACO and thus, the ACO will not get “credit” for lower spending. The attribution rules thus entail that the beneficiary sample is selected on positive spending. In an analysis that, like ours, is concerned with beneficiary selection, Markowitz and colleagues [17] explain how patients following individual clinicians exiting an ACO could lead to an overstatement of ACO savings.

Provider organizations are not randomly selected for ACO participation. Eligible participants must apply and meet CMS criteria including a minimum of 5,000 attributed Medicare fee-for-service beneficiaries [12] and demonstrated experience coordinating patient care [13, 14]. Organizations anticipating the greatest gains based on prior care management capabilities and experience with risk-bearing contracts may be the most likely to apply and obtain CMS approval [15, 14, 16]. Providers that do not meet size and care capability requirements to become ACOs may be inadequate controls for ACOs because the two may have different spending trends.

2.2. Study data and population

Medicare fee-for-service claims data and summary files over 2009–2014 provided demographic information, enrollment status, death date, clinical conditions, healthcare utilization, and spending. Because CMS redacted claims indicating substance use in research files [17, 18], for consistency, we suppressed substance-abuse claims in the data prior to 2013. We used beneficiary ZIP codes to determine Hospital Referral Region (HRR, designating regional healthcare markets) and identified whether beneficiaries lived in high-poverty census tracts (at least 20% of residents below the poverty line [19]).

ACOs were identified using Pioneer and MSSP provider files. We identified 352 ACOs across four cohorts (2012 Pioneer and 2012–2014 MSSPs). Provider organizations that switched programs during the sample—say from Pioneer to MSSP—were identified with their initial programs throughout. Years after exit were censored for ACOs leaving the program.

Non-ACO providers were identified from two sources delineating ownership and management relationships among individual clinicians (identified by National Provider Identifier, or NPI), medical practices, hospitals, and health systems: the IQVIA OneKey database and an Academic Medical Centers provider file from the Office of Health and Human Services [20]. The IQVIA database uses information from several sources including proprietary data collection, the American Medical Association's Physician Masterfile, and publicly available sources; these data contribute to research on health care organization size, structure, capabilities, and performance [21–25].

By design, Medicare ACO contracts exclude small organizations. Since CMS rules stipulate that "eligible ACO participants must apply for participation in an ACO contract and meet CMS criteria requiring a minimum of 5,000 attributed Medicare fee-for-service beneficiaries", we aimed to evaluate ACOs against comparably large organizations susceptible to become future ACOs (based on size and organization structure). Following CMS size requirements [26, 27], we excluded organizations with fewer than 5,000 beneficiaries attributed to them in 2009–2011. We identified 372 non-ACO controls from the IQVIA OneKey database and 102 from the Academic Medical Centers database.

We attributed beneficiaries to provider organizations in each year following CMS MSSP rules [28–31], permitting the organization to which a beneficiary was attributed to change from year to year. Our final sample included 27,609,638 beneficiaries and 88,965,381 beneficiary-year observations. Details on attribution and the sample are in Appendix Figures A1 and A2 and related text.

2.3. Study variables

Our main outcome variable was total annual Part A and B Medicare spending, including MedPAR (inpatient, skilled nursing, long term care hospital), carrier (physician/supplier), outpatient facility, hospice, durable medical equipment, and home-health spending. The main exposure variable was an indicator for beneficiary attribution to an ACO. The model controlled for beneficiary charactistics including indicators for five-year age categories, gender, race, high-poverty census tract, dual eligibility for Medicaid, disability status, and comorbidities. We adjusted for comorbidities by including indicators for zero, one, two, three, or four or more hierarchical condition categories (HCCs) [32], or an unknown number due to an absence of claims that year.

2.4. Statistical analysis

Our main analysis consisted of six regressions, starting with a baseline regression patterned after previous research [4–6], with subsequent regressions adding methods to address beneficiary or provider selection issues, leading to a final set of regressions incorporating a full suite of statistical methods to account for selection on levels and trends in spending for beneficiaries and providers. Table 1 describes the particular selection issue addressed by the methods introduced in each regression. All regressions were estimated using ordinary least squares applying two-way clustering [33] to standard errors to account for correlation in outcomes over time and across regions (HRRs).

2.5. Ordinary least squares regressions

2.5.1. Baseline model (1): addressing selection on provider size—The baseline model (1) regressed Medicare spending for individual beneficiaries on our main exposure variable: beneficiary attribution to an ACO, included in each regression as a set of indicators for the Pioneer, MSSP 2012, MSSP 2013, or MSSP 2014 cohort in each participation year (one, two, or three) after entry. Following previous research, the regression included an indicator for each of the 352 ACO provider organizations. Such ACO indicators, or fixed effects, help to adjust for unobservable ACO characteristics that may influence changes in spending. Also consistent with previous research, we included controls for beneficiary characteristics listed above and an indicator for each HRR-year interaction (see appendix for regression equation). The only notable intended methodological difference from previous research was that, to avoid having to construct two samples of control organizations, we restricted non-ACOs to be comparable in size to ACOs (serving at least 5,000 beneficiaries) in all regressions including the baseline.

2.5.2. Model (2): addressing nonrandom beneficiary switching into non-ACOs—Model (1) included fixed effects for each ACO organization but, following previous literature, omitted analogous fixed effects for non-ACO organizations, combining them all in a single reference category. The previous literature was unable to include non-ACO fixed effects because it lacked the necessary information on non-ACOs. Novel data identifying non-ACO organizations allowed us to add fixed effects for individual non-ACOs to model (2), addressing the asymmetric treatment of ACOs and non-ACOs in the baseline (1).

With one pooled control group, as in the baseline and previous studies, the composition of providers is allowed to change over time in a way not allowed among the group of ACO provider organizations. Addressing this issue allowed us to study variation in within-provider spending. If beneficiaries switching out of ACOs tend to move to the higher-spending organizations among non-ACOs (as might happen if beneficiaries follow specialists leaving with an ACO's reorganization [34]), using a single pooled control group could bias the results toward finding ACO efficiencies. Even if there were no relative change in provider efficiency, the compositional effect of an increase in the number of beneficiaries selecting the higher-spending organizations among non-ACOs could be measured as a relative decline in the efficiency of non-ACOs. We added provider-group fixed effects to the control group to address this asymmetry and to assess whether the changing composition of provider organizations might bias ACO savings estimates (upward or downward).

2.5.3. Model (3): addressing beneficiary selection into ACOs on spending level—The previous model addressed beneficiary switching to selected *non-ACOs* but left problems with beneficiary selection into *ACOs* unaddressed. Beneficiaries change provider organizations over time, and beneficiaries may select (or be selected) into an ACO on the basis of spending levels, as discussed above. To address this issue, we introduced beneficiary fixed effects in regression (3). Adding beneficiary fixed effects narrowed the source of identifying variation down to spending changes (compared to controls) for a given beneficiary who remains with a provider organization before and after that organization enters an ACO and spending changes for a given beneficiary who switches providers from a non-ACO to an organization in the ACO program. The method intentionally leaves aside changes in a provider organization's spending due to changes in its beneficiary mix.

2.5.4. Model (4): addressing selective beneficiary attribution based on positive spending—Regression (4) controlled for beneficiary selection on positive spending. CMS rules specify that beneficiaries receiving no qualifying healthcare services are ineligible for attribution [35, 29]. Excluding non-attributed beneficiaries from longitudinal analyses could overstate average spending by excluding beneficiaries with zero spending in the post-ACO period. We reversed this exclusion by attributing patients enrolled in continuous fee-for-service Medicare to the last provider to which they could be attributed following CMS rules. Appendix Figure A1 and the accompanying text details which subset of beneficiaries were assigned zero spending in a year.

2.5.5. Models (5)–(6): addressing beneficiary switching based on spending changes—A further beneficiary-selection problem arises if beneficiaries change providers in anticipation of changing healthcare needs. Suggestive evidence for this possibility comes from recent work on MSSP beneficiary attribution [11, 9] and our own analysis of beneficiary transitions reported in the results. Beneficiary fixed effects account for differences in mean spending *levels* across beneficiaries, not within-beneficiary spending *changes*, and may even exacerbate the bias from selection on the latter [6].

To address beneficiary selection on spending changes, we implemented a fixed attribution approach in regression (5), fixing attribution to an ACO using the provider organization to which the beneficiary was attributed in the pre-ACO period, 2011. This intent-to-treat (ITT) study design only tracks spending changes resulting from an organization's entry into an ACO contract (compared to control changes over the same period). We confirmed that 2011 attribution predicted subsequent ACO attribution (F=3.38, p<.001 in test of joint significance).

Our estimates using fixed 2011 attribution could incorrectly estimate ACO savings if beneficiary spending exhibits mean reversion. Mean reversion could arise if beneficiaries requiring relatively expensive healthcare one year revert to more typical needs the following year, biasing our ITT estimates. To test for the possibility of mean reversion, regression (6) fixed beneficiary ACO attribution in 2010, one year earlier than that used in (5). If the results were substantially different, this would suggest that mean reversion biased the ITT estimates.

Appendix Table A1 and related text present instrumental variables (IV) analyses which use prior attribution (in anchor year 2011 in one specification and 2010 in another) to predict whether a beneficiary is attributed to an ACO during the post-period. The IV analyses yield a treatment-on-treated estimate of the effect of being in an ACO on beneficiary spending.

2.6. Other Sensitivity Analyses

The sample of beneficiaries necessarily change with different statistical approaches. Appendix Table A2 illustrates which beneficiary-year observations were used in regressions (1) through (6). To test whether any changes in results across regressions were driven by the sample restriction rather than the statistical method implemented, we ran the specification used in the baseline (1) on the different restricted samples used for the other regressions, reporting results in Appendix Table A5.

We conducted another sensitivity analysis to determine whether the changes associated with the implementation of a method depended on the order of implementation, adding each method alone to the baseline rather than cumulating the methods on top of each other as done in regressions (1)–(6), reporting results in Appendix Table A6.

In other sensitivity analyses, we tested whether our baseline results continued to match those in prior research [6] that restricted the sample to MSSP participants, differentiated hospital-integrated ACOs from physician-led ACOs, and limited specifications to beneficiaries attributed exclusively through primary care, reporting those results in Appendix Table A7 and Appendix Figures A6, A7 and A8. We also repeated the test for mean reversion involved in comparing regressions (5) and (6) but in an analysis stratified as in [6] to gauge the robustness of the test to such alternative stratification.

3. Results

3.1. Provider and beneficiary characteristics (unadjusted)

After limiting the control sample to large provider organizations, those entering an ACO contract differed from those that did not during the pre-ACO period (Table 2). ACOs were significantly larger than non-ACOs, with more attributed beneficiaries per organization (23,699 versus 13,620, p<.001) and more physicians (7,000 versus 5,879, p=.052). They had fewer physicians per 100 beneficiaries (37.3 versus 49.9, p<.001) and a lower share of primary care physicians (20.7% versus 22.0%, p<.001). Differences also appear within ACOs across cohorts: later ACO cohorts were markedly smaller (measured either by beneficiaries or physicians) and had more physicians per beneficiary.

Beneficiary characteristics were similar both between ACOs and non-ACOs and across ACO cohorts. Notable differences include lower pre-ACO period per-beneficiary spending among beneficiaries attributed to organizations that became ACOs ($10,301 versus $11,039 in non-ACOs, p=.001). ACOs had fewer white beneficiaries (82.2% versus 85.2%, p<.001), fewer beneficiaries living in high-poverty areas (19.1% versus 21.5%, p=.002), but more beneficiaries eligible for Medicaid (21.5% versus 19.4%, p=.002). ACO beneficiaries were healthier, having fewer average HCCs (1.4 versus 1.5, p<.001).

Table 3 shows beneficiaries observed in two years are more than twice as likely to switch from ACOs to non-ACOs (6.6%) than the reverse (3.2%). Focusing on the subsample of beneficiaries observed in consecutive years, 19% (4.1+3.2+6.6+5.0) of these beneficiaries switched across provider organization types (from an ACO to a non-ACO or the reverse). Beneficiaries switching organizations had higher spending levels than those remaining in the same provider organization type ($11,697-$8,543=$3,163 in year t, p<.001) as well as higher spending growth (($14,713-$9,546)–($11,697-$8,534))=$2,004; p<.001).

Figure 1 gauges the appropriateness of non-ACO controls as a comparison for ACO provider organizations. The dotted lines show unadjusted spending for organizations that do not become an ACO during the study period and for the subset of those organizations with at least 5000 attributed beneficiaries. Spending levels and trends more closely resemble ACOs' when the control group is limited to larger organizations. Appendix Table A3 reports our test of the pre-period differences for beneficiaries attributed to ACOs and non-ACO organizations, showing no significant difference for Pioneer, 2012 and 2013 MSSP cohorts, but a significant difference for the 2014 cohort. Therefore, results reported in the text focus on the earlier cohorts.

3.2. Regression analyses (covariate adjusted)

3.2.1. Baseline model (1): addressing selection on provider size—Table 4 reports regression results. In the baseline (1), estimated savings from ACO participation relative to controls grew over time for each ACO cohort. For example, the effect of ACO participation for the Pioneer cohort goes from an annual, per-beneficiary spending increase of $74.1 ($p$=.262) in the first year, to savings of $449.7 ($p$<.001) in the third year.

3.2.2. Model (2): addressing beneficiary switching into selected non-ACOs—Adding fixed effects for 474 individual non-ACOs in regression (2) substantially changes the results. Savings estimates from (1) do not just disappear but, in most cases, are reversed, becoming estimated spending increases in (2). Three of the estimates are moderate in size and statistically significant; for example, spending was positive $244.4 ($p$=.001) for Pioneers' first participation year. The change in results from (1) to (2) is consistent with beneficiary selection into relatively high-spending non-ACOs when they switch out of ACOs.

3.2.3. Model (3): addressing beneficiary selection into ACOs on spending level—Regression (3) adds beneficiary fixed effects to (2). While not as dramatic as the change from (1) to (2), the change from (2) to (3) is still noticeable and fairly consistent down the column of estimates. The estimated spending increases found in (2) are moderated in (3), with only one statistically significant estimate of $161.7 ($p$=.004) for the Pioneer cohort's first participation year. The change in estimates is consistent with selection of lower spending beneficiaries into ACOs, as seen in Figure 1, since the positive estimates were smaller (i.e. less excess spending) and some estimates (MSSP 2012) became negative relative to (2).

3.2.4. Model (4): addressing selective beneficiary attribution based on positive spending—Regression (4) accounts for the omission of zero-spending beneficiary years. Moving from regression (3) to (4), the spending estimates did not change, suggesting zero spending beneficiary years had little effect on spending estimates. Only the estimated ACO effect for 2014 MSSP was statistically different from zero in regression (4).

3.2.5. Models (5)–(6): addressing beneficiary switching based on spending changes—Column (5) reports estimates from the ITT regressions designed to address selection on beneficiary spending changes. This regression fixes the attribution of a beneficiary to an ACO based on the provider organization to which the beneficiary was attributed prior to the rollout of the ACO program, using 2011 as the anchor year.

Moving from regression (4) to (5), we see a large change to the results. The estimates move back in the negative direction of the baseline (1), but savings estimates in (5) remain smaller than in (1).

The ITT approach in (5) may itself be biased if beneficiary spending exhibits dynamic patterns such as mean reversion. We tested for this in regression (6) by fixing attribution in 2010 rather than 2011. The requirement that beneficiaries continue from an earlier anchor year is more stringent (requiring them to have positive spending in 2009 rather than either 2009 or 2010), leading the sample in (6) to be smaller than for (5). In regressions (5) and (6), each cohort exhibited increasing savings the longer it participated in an ACO contract. Contrary to a situation in which mean reversion of beneficiary spending incorrectly generates estimated savings, estimated savings in (6) were as large or larger than those in (5) for eight out of the nine estimates. The similarity between estimated savings in regressions (5) and (6) demonstrates stability when a suite of methods to address provider and beneficiary selection are combined.

3.2.6. Specifications using instrumental variables—Regressions (5') and (6') (Appendix Table A1) report an instrumental variables (or treatment-on-treated) estimate of the effect of being attributed to an organization participating in an ACO contract on beneficiary spending. Effectively, these estimates in (5') and (6') rescale the ITT estimates using the probability of attribution to an ACO based either on 2011 (5') or 2010 (6') to obtain estimates of treatment effects on treated beneficiaries. As expected, within-cohort trends are still preserved, in the sense that savings become larger in later participation years. Also as expected, estimated savings are higher when going from ITT estimates to treatment-on-treated.

3.2.7. Aggregate results—For ease of comparison, Figure 2 and Appendix Table A4 show results from regressions identical to those in Table 4 except that an aggregate ACO effect is estimated rather than separately by cohort and participation year. Although the figure glosses over nuanced differences across cohort and time trends, it accurately captures broader swings in the estimates as methods are introduced; it also displays the stability of estimates once the suite of methods are implemented together, including using fixed beneficiary attribution in regressions (5) and (6). The estimated average ACO savings per beneficiary decreased from \$142.1 (p<.001) in the baseline (1) to \$45.6 (p=.022) in regression (5) fixing assignment year to 2011. If mean reversion explained savings estimated when we used a fixed beneficiary assignment in 2011 (5), one would expect savings estimates to become smaller in magnitude moving from regression (5) to (6), but we found just the opposite.

3.3. Sensitivity Analyses

We tested sensitivity of our baseline specification to estimating a model with an aggregate ACO effect rather than separately by cohort and participation year and replacing the 352 ACO provider organization fixed effects with a single aggregate ACO fixed effect in one specification and replacing them with an indicator for the four ACO cohorts (Appendix Figure A3). Results are stable across specifications.

We also tested whether results in the baseline (1) were robust to the different restricted samples used for the other regressions (Appendix Table A5 and Figure A4). Results do not vary appreciably across samples, confirming that the sample restrictions are not driving the changes in results. Moreover, we found that each method has a qualitatively similar effect on the estimates whether each was added to the baseline by itself or layered on each other in sequence (Appendix Table A6 and Figure A5). Adding each method to the baseline in isolation rather than in cumulative fashion generates even larger swings in estimates than those seen in Table 4.

We next compared our results to what we believe is the most recent (data through 2015), detailed (separate effects by cohort and whether ACO includes a hospital), and widely cited estimate of ACO savings to date [6] shown in Appendix Table A7 and Figures A6–A8. As in McWilliams et al. [6], we attributed beneficiaries based on primary-care physician spending and stratified our results by ACOs integrated with hospitals and non-integrated. Despite marked differences in samples based on end date (2014 versus 2015), organization size (we limit to 5000 or more attributed beneficiaries), and redaction of claims for substance use disorders, this new baseline specification shows substantial overlap in confidence intervals with the earlier estimates, as seen in Figures A6 through A8. Just as we saw in Table 4, in Table A7, with results stratified by whether an ACO was hospital-integrated, sequentially adding statistical methods addressing provider and beneficiary selection to the baseline lead to dramatic changes in results, stabilizing when the comprehensive suite of methods is applied in the final columns.

4. Discussion

Stakeholders continue to have high hopes for the ability of ACOs to improve healthcare quality and lower spending. These hopes were bolstered by estimates of moderate savings from ACOs [4–6] and promising trends toward increased savings over time [6], leading to widespread acceptance in the policy community that ACO savings increase with experience [36]. Research on changes in care patterns associated with ACOs have also found challenges with the assignment of patients to provider groups, and noted that the set of beneficiaries attributed to ACOs changes dramatically over time, rendering it difficult to manage ACO patients to appropriately capture investments in either care processes or preventive care of beneficiaries. While ACO achievements in quality and patient experience remain important benefits of this delivery reform [37, 38], our findings suggest estimated savings are sensitive to how evaluations handle provider and beneficiary selection into ACOs. To explore the sensitivity of savings estimates to approach, we implemented a comprehensive suite of methods to adjust for provider and beneficiary selection. Using novel data to identify non-ACO provider organizations and our comprehensive suite of methods, we estimated ACO savings that grew over time. These estimates are in line with, but smaller than, previous estimates in the literature.

Our baseline regression, modeled on previous research, generated results similar to this previous research: moderate savings, with markedly increased savings over time within each ACO cohort. Starting from this baseline regression, the results varied widely as we sequentially introduced specification changes to address various provider and beneficiary selection problems. For example, replacing an overall category for non-ACO controls by individual non-ACO fixed effects profoundly changed the results. Estimated savings were wiped out, in some cases replaced by significant spending increases. However, our estimates were stable when we implemented our suite of methods to address these selection problems in a comprehensive way. Our methods, including fixed attribution of beneficiaries based on the pre-period, were robust to tests of mean reversion in beneficiary spending. Further, the combined suite of methods passed rigorous tests of parallel trends, and behaved as expected comparing estimates of intention-to-treat effects with treatment-on-treated effects.

Piecemeal corrections for some aspects of selection without accounting for others could be misleading, as our results show. One reasonable approach to these piecemeal biases is to hold back from using common approaches, like including beneficiary fixed effects, to address selection. However, our findings suggest that robust evaluation can be achieved not by doing less, but by doing more, and implementing a suite of methods at once. The full set of methods, partly enabled by unique data on provider organizations that have not entered ACO contracts, produced stable estimates, robust to a number of identification threats.

Our paper is related to contemporaneous work by Markovitz et al. [39], which employs methods to address beneficiary selection, although their methods focus more on individual clinicians rather than organizations. They conclude that the moderate savings estimated by the previous literature are illusory. While we corroborate their doubts about the reliability of previous savings estimates, we are not convinced that the approach predicting ACO attribution based on ACO participation of nearby clinicians solves the challenge of estimating ACO savings in the presence of substantial selection of beneficiaries and providers into ACOs. Instead we would emphasize the instability of the results when selection problems are not fully addressed—an instability that would have been hidden had we reported a single ACO effect not broken into separate year and cohort effects, applied statistical corrections together rather than sequentially, or omitted formal tests of the suitability of our strategy. We believe our findings have implications for how attribution occurs in ACOs—organizations cannot be expected to overhaul population health when the population is rarely “captive” to the ACO, as in retrospective attribution with frequent churn —and for payers assessing the success or failure of alternative payment models. Attribution methods that are prospective, that consider several years of spending information, or attestation, offering the option for beneficiaries to choose their provider, may be beneficial in accountability for preventive services, especially for those with limited healthcare use [35]. Additionally, prospective attribution may facilitate patient engagement by removing uncertainty about care management associated with greater churn in retrospective attribution. Implementing payment reforms through area randomization with compulsory participation for qualifying provider groups would aid in evaluation of reforms. Recent examples like Medicare's randomization of areas to a bundled payment for joint replacement have contributed valuable insights into what savings can and cannot be achieved, and subsequent conversion of the program to be voluntary further adds to our knowledge of bias [40, 41]. Statewide rollout of Medicaid ACOs, which yield an opportunity to compare the experience of otherwise similar states, offers another example in which selection issues may be mitigated.

Our study has several limitations. Perhaps the most notable limitation is the short follow-up period for some of the ACO cohorts (2014 MSSPs). Two other limitations are more minor. First, our estimates for the 2012 MSSP cohort's first participation year are best interpreted as partial-year estimates as they did not begin participation until April or July. Second, implementing some of our methods (in particular, addressing selection on positive spending) reduced sample sizes, but qualitatively similar results were obtained whether baseline methods were run on full or restricted samples.

5. Conclusion

Evaluation of the ACO model is challenging. Estimated savings are sensitive to the estimation strategy employed. Our results confirm the importance of selection for savings estimates and for provider organizations managing costs and quality. Attribution rules that mitigate the impact of beneficiary churn for providers and payers could help. Although our comprehensive data and methods can be used to address beneficiary and provider selection, such data and methods may not always be feasible for evaluators seeking real-time evidence for new payment models. Implementing payment reform by randomizing early participants, or implementing fully across a selection of markets, may better serve efforts to evaluate and improve payment models. Finally, regardless of savings estimates, ACO programs should also take into account potential effects on quality and patient satisfaction with care, which have both been shown to be positively associated with ACO implementation. Given the difficulty of achieving sustainable medical savings over time and an interest in moving providers (and particularly primary-care providers) away from fee-for-service models, considerations of other outcomes besides spending may need to be weighed more heavily in considering spread of ACO or other alternative payment models.

Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

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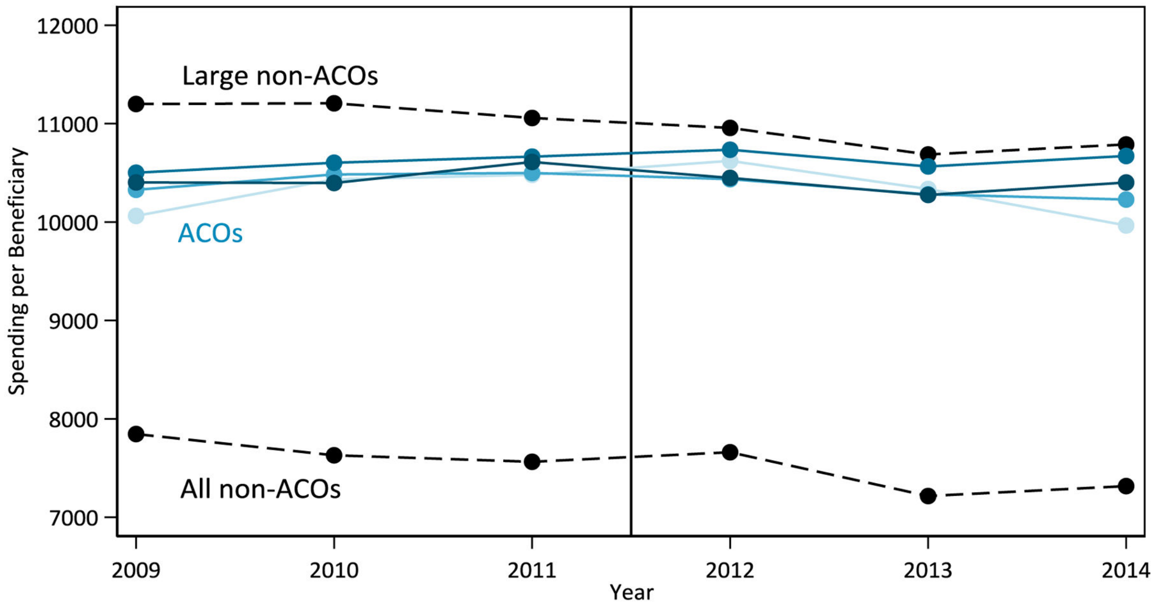

Figure 1. Trends in annual spending per beneficiary

Notes: Dashed black lines represent non-ACOs. Solid colored lines represent organizations that join ACOs in post period: Pioneer, 2012 MSSP, 2013 MSSP, and 2014 MSSP in order from lightest to darkest. Vertical line divides pre and post periods.

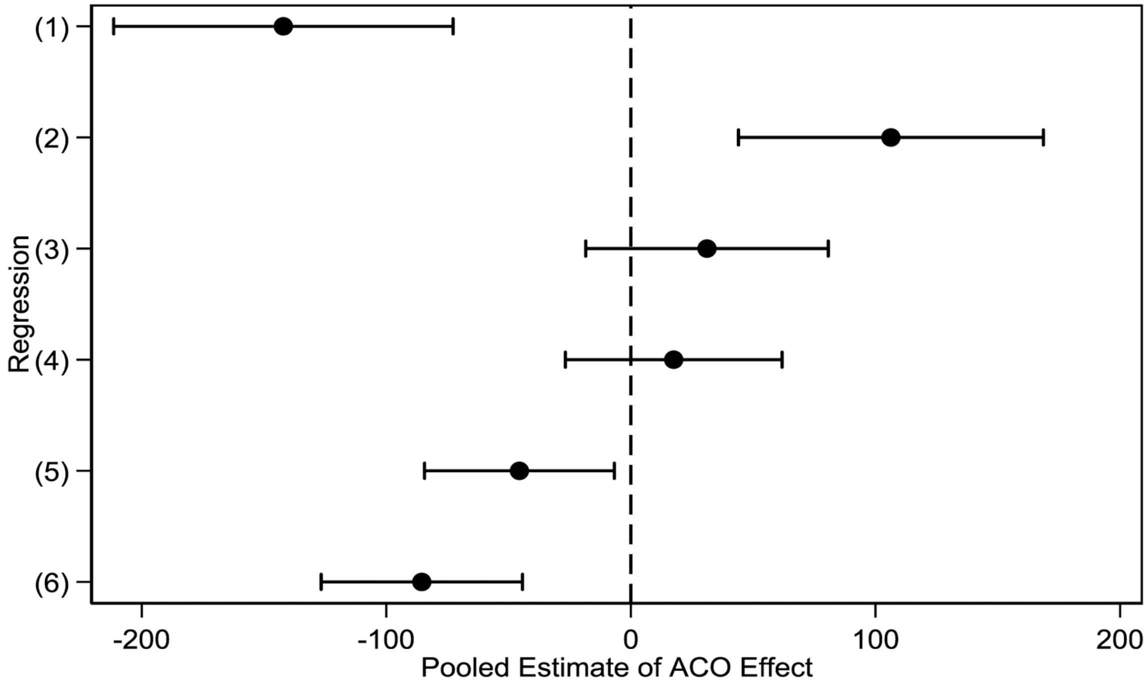

Figure 2. Comparing pooled estimates of ACO effect across regressions

Notes: Plot of results from regressions identical to those in Table 4 except that a pooled ACO effect is estimated rather than a separate effect by cohort and participation year. Estimates shown as dots and 95% confidence intervals as bars.

Table 1

Statistical methods addressing selection problems

Method	Selection Issue Addressed	Example of Problem	First Regression Incorporating	Limitations
Restrict controls to large non-ACOs	Selection on organization size	If spending trends vary by size, small non-ACOs may be poor controls for ACOs, which are large	(1)	Some ACOs formed by partnering of small provider groups
Add individual non-ACO fixed effects	Beneficiaries switch to selected non-ACOs	Beneficiary switching from ACO to high-spending non-ACO mistaken for ACO efficiency	(2)	Difficult to implement in studies having large sample of non-ACO controls
Add beneficiary fixed effects	Beneficiaries selected for ACOs on spending level	Selection of high-cost beneficiaries mistaken for ACO inefficiency	(3)	May exacerbate bias from selection on beneficiary spending changes
Incorporate zero-spending beneficiaries	Attribution rules select beneficiaries on positive spending	Efficiencies that eliminate spending for some beneficiaries may be missed	(4)	Attributing zero-spending beneficiaries to previous provider organization is guess
Fix attribution in pre-ACO anchor year	Beneficiaries selected for ACOs select on spending change	Selection of beneficiaries with rising costs mistaken for ACO inefficiency	(5)	May introduce bias if beneficiary spending exhibits regression to mean

Table 2

Descriptive statistics for study variables in pre-ACO period by organization cohort

	ACO Cohort						
Variable	**2012 Pioneer**	**2012 MSSP**	**2013 MSSP**	**2014 MSSP**	**ACOs Combined**	**Non ACOs**	**P-value for Difference**
A. Organization characteristics							
Beneficiaries, count, mean (SD)	76,494	22,010	20,905	12,214	23,699	13,620	< 0.001
	(90,940)	(25,292)	(22,604)	(7,846)	(37,363)	(19,021)	
Physicians, count, mean (SD)	18,631	6,705	6,533	4,246	7,000	5,879	0.052
	(20,793)	(7,379)	(5,538)	(2,469)	(8,995)	(7,528)	
Physicians, count per 100 beneficiaries, mean (SD)	27.4	36.0	38.9	40.1	37.3	49.9	< 0.001
	(8.9)	(17.8)	(19.0)	(19.7)	(18.4)	(34.6)	
Share of physicians in primary care, % (SD)	22.9	20.6	20.4	20.5	20.7	22.0	< 0.001
	(2.5)	(3.2)	(3.0)	(2.8)	(3.0)	(4.0)	
B. Beneficiary demographics							
Age, years, mean (SD)	71.6	72	71.8	71.6	71.7	71.7	0.929
	(0.8)	(1.5)	(1.6)	(2.4)	(1.6)	(2.1)	
Female, % (SD)	58.1	58.4	58.1	58.6	58.3	57.6	< 0.001
	(1.1)	(1.6)	(1.8)	(2.2)	(1.6)	(2.4)	
White, % (SD)	81.0	82.5	83.9	81.1	82.2	85.2	< 0.001
	(10.1)	(13.9)	(14.1)	(14.1)	(13.0)	(11.4)	
Black, % (SD)	8.2	9.3	8.2	10.9	8.9	8.7	0.635
	(5.3)	(7.6)	(7.9)	(9.7)	(7.5)	(8.3)	
High-poverty area, % (SD)	19.4	19.2	17.9	20.6	19.1	21.5	0.002
	(5.8)	(11.1)	(10.4)	(15.1)	(10.4)	(12.2)	
Dual Medicaid eligibility, % (SD)	22.7	21.3	20.9	20.7	21.5	19.4	0.002
	(8.2)	(11.9)	(10.8)	(12.9)	(10.8)	(7.9)	
Disability, % (SD)	22.6	21.8	22.4	22.6	22.3	23.0	0.167
	(3.7)	(5.9)	(6.1)	(9.0)	(6.0)	(7.4)	
HCCs, count, mean (SD)	1.4	1.5	1.4	1.4	1.4	1.5	< 0.001
	(0.2)	(0.2)	(0.2)	(0.3)	(0.2)	(0.4)	
C. Beneficiary outcomes							
Medicare spending, $, mean (SD)	10,745	10,349	9,977	9,885	10,301	11,039	0.001
	(1,744)	(2,284)	(2,281)	(2,542)	(2,542)	(2,197)	(4,109)
Number of observations							
Organization level	32	114	100	106	352	474	826

Notes: Mean of variables across organizations in the pre-ACO period, 2009–2011. Standard deviations reported below in parentheses. Organization cohort is that to which it belongs if and when it enters an ACO contract. In computing statistics, each organization contributes one observation, given by that organization's mean of the variable over the pre-ACO period. Statistics in panel A are unweighted and in B and C are weighted by the average number of Medicare fee-for-service beneficiaries attributed to each organization in the pre-ACO period. Race/ethnicity categories included in regressions but not reported in this table are Hispanic, Asian and Pacific Islander, and the residual Other category. HCC denotes hierarchical condition category. Last column reports *p* -value from two-tailed test of difference between means of preceding two columns. Sample of non ACOs restricted to those serving 5,000 or more beneficiaries. The sample, which is the same one used in the baseline regression (1), includes 826 total organizations.

Table 3

Frequency of transitions between provider groups and associated spending growth

	ACO in *t*+1		Non-ACO in *t*+1	
	Stayers Same as *t*	**Switchers Different from *t***	**Stayers Same as *t***	**Switchers Different from *t***
A. Percentage of beneficiary transitions (entries in panel add to 100%)				
ACO in *t*	41.6	4.1	—	6.6
Non-ACO in *t*	—	3.2	39.4	5.0
B. Mean spending$_t$ →spending $_{t+1}$ according to transition type across ACOs and Non-ACOs				
ACO in *t*	8,363 → 9,483	11,414 → 14,328	—	10,617 → 13,625
Non-ACO in *t*	—	12,947 → 15,258	8,714 → 9,613	12,578 → 16,138

C. Mean spending$_t$ →spending $_{t+1}$ for stayers and switchers

	Any Provider Organization (ACO or Non-ACO) in *t* + 1	
	Stayers Same as *t*	**Switchers Different from *t***
Any Provider Organization in *t*	8,534 → 9,546	11,697 → 14,713

Notes: The unit observation is a beneficiary's transition between two consecutive time-series observations. Analysis of $N = 61{,}355{,}743$ transitions for the 20,373,198 beneficiaries (constituting 81,728,941 beneficiary-year observations) observed at least twice over the sample period. These beneficiaries represent 74% of the 27,609,638 beneficiaries (constituting 88,965,381 beneficiary-year observations) in our baseline sample. During pre-priod, ACO indicates beneficiary attribution to provider that will enter an ACO contract in the post period and non-ACO to provider that will not. Percentages in top panel sum to 99.9% due to rounding. Arrows in bottom panels indicate the change from mean spending from observation to the next (observation $t \rightarrow$ observation t+1). Test for equality of mean spending between stayers and switchers in t in panel C rejects the null, p<.001. Test for equality of mean spending growth between stayers and switchers in t+1 in panel C rejects the null, p<.001.

Table 4

Regression analysis of effect of ACO participation on Medicare spending

	Regression					
ACO Cohort	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
2012 Pioneer						
Participation year 1	74.1	244.4 **	161.7 **	36.7	−42.0	−15.8
	(65.9)	(74.6)	(55.3)	(43.9)	(33.4)	(34.6)
Participation year 2	−155.7 *	176.9 *	100.1	41.5	−9.1	−21.7
	(75.4)	(82.6)	(58.6)	(56.0)	(43.6)	(46.6)
Participation year 3	−449.7 **	−30.7	−24.5	−78.0	−113.6 *	−118.8 *
	(84.8)	(85.1)	(57.6)	(60.5)	(54.1)	(48.2)
2012 MSSP						
Participation year 1	−136.9 *	40.7	−16.8	−21.0	−0.8	−38.8
	(54.9)	(48.1)	(35.5)	(31.7)	(35.5)	(32.0)
Participation year 2	−202.1 **	83.3	−25.5	−12.9	−11.6	−127.1 **
	(71.9)	(60.7)	(50.5)	(45.5)	(38.7)	(40.6)
Participation year 3	−342.8 **	21.0	−81.6	−68.3	−123.3 *	−197.0 **
	(64.8)	(61.5)	(56.5)	(54.1)	(47.5)	(46.8)
2013 MSSP						
Participation year 1	−130.3 *	91.6	20.7	22.3	−43.0	−97.8 **
	(50.3)	(49.1)	(37.9)	(36.6)	(32.6)	(31.8)
Participation year 2	−199.2 **	111.8	7.5	1.9	−150.4 **	−218.6 **
	(61.2)	(58.3)	(48.5)	(47.9)	(43.1)	(42.3)
2014 MSSP						
Participation year 1	−27.1	152.1 **	81.2 *	106.3 **	−21.8	−56.7
	(47.7)	(44.0)	(35.1)	(36.0)	(34.1)	(33.8)
Specification						
Beneficiary-level covariates	Yes	Yes	Yes	Yes	Yes	Yes
HRR-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
ACO fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Non-ACO fixed effects	No	Yes	Yes	Yes	Yes	Yes
Beneficiary fixed effects	No	No	Yes	Yes	Yes	Yes
Address zero-spending truncation	No	No	No	Yes	Yes	Yes
Fixed attribution to anchor year	No	No	No	No	Yes(2011)	Yes (2010)
Number of Observations						
Beneficiary level (# of clusters)	21,776,132	21,776,132	17,297,018	13,265,011	11,656,202	10,094,293
Beneficiary level	27,609,638	27,609,638	23,130,524	16,484,142	13,581,898	11,999,258
Beneficiary-year level	88,965,381	88,965,381	84,486,267	60,215,727	54,039,875	47,846,537

Notes: Each column reports results from a single ordinary least squares regression jointly estimating all displayed coefficients. Regressions use beneficiary data from the 100% Medicare fee-for-service beneficiaries attributed to one of the organizations in our sample. Non ACOs restricted to those serving 5,000 or more beneficiaries. Sample in regression (3) is smaller than in (1) and (2) because singleton observations in any fixed-effect category are deleted. Sample in (4) is further reduced by the measures addressing zero-spending truncation. Those measures require restricting the beneficiary sample to those having some qualifying evaluation and management visits for attribution and Medicare spending in 2009–2010 and dropping the first observation for each of them to avoid sample selection on positive spending. Regressions (5) and (6), which fix attribution to

ACO cohort in 2011 and 2010 anchor years, respectively, have smaller sample sizes than (4) because beneficiaries need to have been attributed in anchor year. Regressions control for beneficiary-level demographics listed in Table 2: age; female indicator; set of indicators for race/ethnicity (white, black, Hispanic, Asian and Pacific Islander, other); and indicators for living in high-poverty area, dual Medicaid eligibility, and disability status. Regressions control for age flexibly by including indicators for the age intervals 0–64, 65–69, 70–74, 75–79, 80–84, 85–89, 90–94, 95+. Regressions control for beneficiary-level medical conditions including indicators for number of HCCs belonging to one of the following categories: 0, 1, 2, 3, 4 or more HCCs, or no reported conditions because of no medical visits. Standard errors reported in parentheses adjust for two-way clustering on beneficiaries and HRRs. Significantly different from 0 in a two-sided test at the *5%, **1% level.