

Evaluation of the Independence at Home Demonstration

An Examination of the First Six Years - Appendices

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Appendix A

Effects of IAH payment incentive: technical appendix

I. Overview

Congress mandated the Independence at Home (IAH) demonstration to test a payment incentive and service delivery model for home-based primary care. Under the IAH demonstration, physicians and nurse practitioners (NPs) direct home-based primary care teams. These teams aim to reduce expenditures and improve the health outcomes of Medicare beneficiaries with multiple chronic conditions and substantial functional limitations. As we discussed in Chapter I, the IAH demonstration introduced an incentive to reduce Medicare expenditures (incentive payments) and a service delivery model (home-based primary care led by physicians or NPs). As we described in Chapter II, the Mathematica study team estimated a difference-in-differences model to determine whether the demonstration payment incentive affected Medicare expenditures, hospital use, and quality of care (measured as potentially avoidable hospital use and readmission). We also examined whether the demonstration payment incentive affected health outcomes (that is, mortality or entry into institutional long-term care). In this appendix, we present the sample, data, and methods we used for the analyses in Chapter II.

The quantitative evaluation design of the demonstration was a difference-in-differences analysis using repeated cross-sections of eligible beneficiaries within demonstration practices (which we also refer to as sites) with a propensity score-matched comparison group that did not receive home-based primary care. We had two years of pre-demonstration data and six years of post-demonstration data (that is, the first six years of the demonstration). We observed beneficiaries analyzed by the evaluation for the number of months they were eligible for IAH for each demonstration year. We used three key pieces of information to determine the effect of the demonstration on expenditures, hospital use, quality of care, and health outcomes in a given year. To determine the effect of the demonstration on expenditures (and other outcomes) in a given year, such as Year 6, we did the following:

- Estimated the difference in Medicare expenditures per beneficiary per month (PBPM) between the year before the demonstration (the baseline year) and Year 6 for IAH beneficiaries. We restricted claims to those occurring between the date of eligibility for the demonstration in a given year and the end of that year (and date of death). We controlled for beneficiaries' characteristics, such as time since most recent hospital admission; demographic characteristics; activities of daily living (ADLs); and several measures of health status, including the Centers for Medicare & Medicaid Services (CMS) Hierarchical Condition Categories (HCC) risk score. We provide a complete list of control variables later in this appendix.
- Estimated the difference in Medicare expenditures during the same period for comparison beneficiaries. As with the IAH group, we restricted claims to those that occurred between the date of eligibility and the end of the year, controlling for beneficiary characteristics.
- Obtained the estimated effect of the demonstration by calculating the difference between the change in expenditures for IAH beneficiaries and the change in expenditures for comparison beneficiaries.

We refer to this model as a difference-in-differences model because it measured the change between two differences (the pre- and post-demonstration differences). This method isolated the effect of the demonstration by accounting for two factors. First, it accounted for the difference in expenditures between IAH and comparison beneficiaries before the demonstration. Second, it accounted for changes in expenditures during the demonstration caused by factors unrelated to the demonstration that affected IAH and comparison beneficiaries. The difference-in-differences design provides a strong assessment of the demonstration's effect, assuming that the difference in expenditures between IAH and comparison beneficiaries was stable before the demonstration. As we describe later, we tested this assumption. The

difference-in-differences model, however, was not without limitations; we address our evaluation's limitations at the end of this appendix.

Our total sample for estimating the impact of the demonstration in Years 1 to 5 consisted of 14 practices, and our total sample for estimating the impact of the demonstration in Year 6 consisted of 12 practices; in both cases, we treated the consortium in Richmond as one practice (Exhibit A.1). In all years, our quantitative analyses excluded three practices (Atlanta, Chicago, and Stuart) that withdrew from the demonstration before Year 3 and one practice (Louisville) that CMS terminated for cause after completing the first three years. In Year 6, we excluded the two practices that left the demonstration after Year 5 (Austin and Cleveland).

In this appendix, we begin by describing how the IAH practices operate and the characteristics of their patients. Next, we describe how we identified the IAH and comparison groups to evaluate the effect of the demonstration, which is designed to have IAH sites assess and enroll participants. We could not use data from the IAH sites to identify our sample, however, because we needed to use the same source of data to identify the IAH and comparison groups. As a result, the sample of beneficiaries enrolled by the practices in the demonstration differed from the beneficiaries in the IAH group we used for the evaluation. For example, about 34 percent of the IAH group we used for the evaluation in Year 6 was enrolled in the demonstration in Year 6. As we describe in the next chapter, the IAH group for the evaluation consisted of beneficiaries who Mathematica identified as eligible for the demonstration and attributed to an IAH site. Next, we describe how we selected the comparison group. We then present the sources of data and measures for our quantitative analyses. We then describe the estimation of demonstration effects. Next, we present the methods and sources of data for our qualitative analysis. Finally, we discuss differences between incentive payments and evaluation results.

Exhibit A.1. IAH demonstration practices and number of beneficiaries by year

Demonstration practice location	IAH Year 1	IAH Year 2	IAH Year 3	IAH Year 4	IAH Year 5	IAH Year 6
	Jun 2012– May 2013	Jun 2013– May 2014	Jun 2014– May 2015	Oct 2015– Sept 2016	Oct 2016– Sept 2017	Jan 2019– Dec 2019
Practices that participated in Years 1 to 6						
Boston, Massachusetts ^a	183	166	157	149	136	107
Brooklyn, New York	371	410	505	1,055	991	491
Dallas, Texas ^b	1,373	993	994	1,344	1,264	1,290
Durham, North Carolina	828	1,066	1,267	1,705	1,974	1,979
Flint, Michigan ^b	1,542	969	991	1,607	1,641	1,415
Jacksonville, Florida ^b	780	654	497	504	874	621
Lansing, Michigan ^b	524	526	702	652	611	608
Long Island, New York ^a	246	220	220	235	288	331
Milwaukee, Wisconsin ^b	514	553	634	575	489	450
Portland, Oregon	161	144	138	171	180	159
Richmond, Virginia (3 practices) ^{a,c,d}	290	311	280	277	323	310
Wilmington, Delaware ^a	225	254	241	213	235	232
Total IAH beneficiaries in Year 6 analyses in this report	7,037	6,266	6,626	8,487	9,006	7,993
Practices that left the demonstration after Year 5^e						
Austin, Texas ^e	911	684	601	686	574	n.a.
Cleveland, Ohio ^{a,e}	268	316	337	331	378	n.a.
Total IAH beneficiaries in Years 1 to 5 analyses in this report	8,216	7,266	7,564	9,504	9,958	n.a.
Practices that left the demonstration before Year 5^f						
Atlanta, Georgia (2 practices) ^c	60	n.a.	n.a.	n.a.	n.a.	n.a.
Chicago, Illinois (7 practices) ^{c,d}	202	n.a.	n.a.	n.a.	n.a.	n.a.
Louisville, Kentucky	1,698	2,264	2,647	n.a.	n.a.	n.a.
Stuart, Florida (2 practices) ^{c,d}	356	n.a.	n.a.	n.a.	n.a.	n.a.

Source: Mathematica’s analysis of data from the IAH implementation contractor and 2011–2019 Medicare claims and enrollment data from the Chronic Conditions Warehouse.

^a These practices participated in health systems affiliated with a university or medical school.

^b These practices participated as part of the Visiting Physicians Association.

^c Practices located in Richmond, Philadelphia and Washington DC participated as consortia.

^d Richmond, Chicago, and Stuart started Years 1 to 3 on September 1.

^e We exclude practices that left the demonstration after Year 5 because they could no longer meet the demonstration requirements from analyses of Year 6 in this report.

^f We exclude practices that left the demonstration before Year 5 because they could no longer meet the demonstration requirements from all analyses in this report. Atlanta, Chicago, and Stuart left the demonstration during Year 2. Louisville left the demonstration after Year 3.

n.a. = not applicable.

II. Description of IAH practices and beneficiaries

To understand the features of the IAH practices and identify the changes they made to improve care, we collected and analyzed interview data from the practices and analyzed their claims data. Every IAH site had substantial experience providing home-based primary care before the demonstration. The sites differed substantially, however, in their approaches to care, such as who was included on the care team; whether they were notified automatically of patients' hospital admissions or emergency department (ED) visits; whether they focused on serving in private homes or assisted living facilities; and whether they used a formal risk-stratification system, which groups the beneficiaries into high- and low-risk groups to aid in care planning. In this chapter, we summarize care delivery patterns according to each of the three types of practices: (1) Visiting Physicians Association (VPA) practices, (2) academic medical center practices, and (3) independent practices. We obtained information about the settings from which the practices provided care from claims data in Years 2 and 4 of the demonstration. We obtained other information from the IAH practices.¹ Exhibits A.2, A.3, and A.4 provide site-by-site information on practices' structural and operational characteristics and care delivery for practices that participated in Year 6 of the demonstration.²

A. VPA practices

The five VPA practices (Dallas, Flint, Jacksonville, Lansing, and Milwaukee) had similar structural and operational characteristics. VPA is a for-profit corporation with multiple home-based primary care practices operating in several states; five of those practices participated in the demonstration. In each of these practices, most clinicians were physicians, and three of the practices employed NPs. None employed a social worker.

Historically, each practice had a patient care coordinator who was the main point of contact for patients and had access to the VPA corporate infrastructure for finance, human resources, data analytics, and data support. In Year 6, the VPA practices renamed the patient care coordinator position to nurse navigator. The nurse navigator role encompassed all patient care coordinator responsibilities and included managing care for patients with the highest hospital and ED use. On rare occasions when a clinician perceived a patient as needing extra support after discharge from the hospital or ED, a nurse navigator made a home visit.

Patients (both IAH beneficiaries and others) were assigned to a mobile care team consisting of one physician and one medical assistant.³ VPA nurse navigators often visited the homes of their patients, although those visits were not billable. In four of the VPA sites, about two-thirds of visits occurred in private homes. In Milwaukee, about two-thirds of visits occurred in assisted living or other group living facilities.

In Year 6, the VPA practices reported continuing to foster relationships with skilled nursing facilities (SNFs) and nursing homes to help coordinate care. In addition, to continue to strengthen existing

¹ Information in this chapter comes from site visits we conducted from February to May 2013 and February to July 2014. In January and February 2017 and in November 2019 to February 2020, we interviewed practices by telephone to confirm and update information.

² Information about the two practices that left the demonstration after Year 5, Austin and Cleveland, is available in the [evaluation report](#) which covered Years 1 to 4 of the IAH demonstration and the [evaluation report](#) which covered Years 1 to 5 of the IAH demonstration.

³ The term patients in this chapter refers to all patients treated by the practice regardless of IAH enrollment status or whether the beneficiary was in Medicare fee for service.

relationships, clinicians frequently reached out to these care partners to remind them to coordinate with the practice when caring for their IAH patients.

Each VPA practice risk-stratified patients on the basis of their history of hospital admission and ED visits to determine the needed level of care and the frequency of proactive phone calls to patients and caregivers. Two practices developed relationships with hospitals and their staff; those staff notified the practice directly when one of its patients was hospitalized or visited the ED, whereas the remaining three received automated notices from hospitals.

B. Academic medical center practices

In Year 6, six IAH practices (Boston, Long Island, Philadelphia, Richmond, Washington, and Wilmington) were part of nonprofit academic medical centers or health systems with academic missions.⁴ This status gave them access to institutional resources and information technology systems and support. Clinicians in these settings were typically responsible for training and education in addition to clinical care, so many saw patients only part time.

In Boston and Long Island, physicians conducted most visits; in Philadelphia, Richmond, and Washington, NPs conducted most of the visits. In Wilmington, NPs and physicians conducted most of the visits. Social workers were key members of the care team for many academic medical center practices because they coordinated home health services and referred patients to social services and supports. All but one academic medical center provided nonbillable visits, such as those conducted by social workers or nurses not acting under a physician's direction or as part of a home health episode. All academic medical center practices conducted most visits in private home settings; three (Long Island, Philadelphia, and Washington) conducted no visits in assisted living facilities. In Year 6, Wilmington reported working to strengthen relationships with local assisted living facilities via proactive outreach to ensure more coordinated care for their beneficiaries.

In Year 6, the Long Island and Washington practices reported testing e-consults with specialists to help manage patients' conditions and reduce Medicare expenditures. Clinicians sent questions and received advice by email from a specialist (for example, cardiologists or pulmonologists) to help manage homebound patients who could not travel to a specialist for an office visit. These two practices also used psychiatry consult services for clinicians to ask questions about managing patients' psychiatric conditions.

Academic medical centers varied in their use of technologies to facilitate care delivery and planning. Most relied on clinical judgment to determine the level of care rather than using a formal risk-stratification system. Nearly all were notified automatically of patients' hospital admissions or ED visits from at least some hospitals with which they built relationships.

C. Independent practices

In Year 6, the demonstration included three practices that began the demonstration as independent practices; these practices differed in size, structure, and operations. The practice in Durham dropped out of the demonstration late in the year (fall 2019). In Year 6, the Brooklyn and Portland practices experienced the following changes related to ownership arrangements and partnerships, respectively:

⁴ Three practices (Philadelphia, Richmond, and Washington) participated as one consortium, which the demonstration considers as one site for the purpose of calculating incentive payments.

- The administrator of the Durham practice reported that the practice did not change care delivery after leaving the demonstration. Yet as a result of no longer needing to meet performance requirements for demonstration quality measures, the practice stopped calling assisted and independent senior living facilities daily to check whether any residents were sent to the hospital or ED in the past day, and it no longer visited homes within 48 hours of hospital discharge or ED visit when not medically necessary.
- During Year 6, the independent practice in Brooklyn was acquired by Heal, a home-visiting service company that newly entered the Brooklyn market. In Year 6, the practice continued to participate in IAH with minimal modifications to the way it delivered care before being acquired by Heal; respondents from Heal reported treating all patients, regardless of IAH status, the same.
- Finally, HouseCall Providers in Portland signed a partnership agreement with CareOregon, a nonprofit health plan that provides care to Medicaid and Medicare beneficiaries, in May 2017 (during Year 5). Through this partnership, the practice aimed to increase access to home-based medicine across the Portland area and increase the share of their IAH-eligible patients who were dually eligible. Indeed, the percentage of IAH beneficiaries who were dually eligible increased from 19 percent in Year 5 to 27 percent in Year 6.

Although two of the three independent practices experienced organizational changes since we last collected qualitative data in April 2017, all three practices reported during Year 6 that their care delivery for IAH beneficiaries remained stable. These practices had staff dedicated to coordinating care for patients, but the type of staff used to coordinate care varied across the sites. For example, some used nurse care managers, and others trained medical assistants or similar staff as patient care coordinators. In Brooklyn and Durham, physicians continued to provide most of the visits, whereas in Portland, NPs provided most of those visits. The sites of care for independent practices varied; Brooklyn conducted most visits in private home settings, and Durham and Portland conducted most visits in assisted living or other group living facilities. Some of the independent practices provided nonbillable visits by social workers and nurse care managers. One independent practice reported risk-stratifying patients as a way to determine the intensity of care the practice would provide, and the remaining three reported relying on clinicians' judgment for these determinations. These practices reported using different methods for learning about patient hospital admissions and ED visits; one relied on patients and caregivers to notify the practice, and others received notice through health information exchanges.⁵

⁵ Health information exchanges allow the electronic sharing of health care information. They can be implemented at different levels, including a region (such as the greater Washington, DC, area), community, or hospital system.

Exhibit A.2. IAH practices' structural characteristics, as of 2019

Site	Affiliation	Ownership	Participate in ACO (years)	Accept Medicare Advantage plans	Clinicians making home visits			Other staff involved in care team				
					Physicians	PAs	NPs	Care coordinators ^a	RNs	MAs	SWs	Others
VPA practices												
Dallas, TX	U.S. Medical Management	For profit	Yes (3)	No	13 FT	-	7 FT	1	2	31	-	
Flint, MI	U.S. Medical Management	For profit	Yes (3)	Yes	23 FT ^b	-	-	1	-	24	-	
Jacksonville, FL	U.S. Medical Management	For profit	Yes (3)	No	3 FT 2 PT	-	1 FT 3 PT	4	1	9	-	
Lansing/Ann Arbor, MI	U.S. Medical Management	For profit	Yes (3)	Yes	9 FT	1 FT	-	1	-	11	-	
Milwaukee, WI	U.S. Medical Management	For profit	Yes (3)	Yes	8 FT	-	2 FT	4	1	11	-	
Academic medical centers												
Boston, MA	Boston Medical Center	Nonprofit	Yes (5)	Yes	6 PT	-	2 FT	-	5	-	-	
Long Island, NY	Northwell Health	Nonprofit	No	Yes	4 FT 4 PT	-	3 FT	-	5	-	5	
Philadelphia, PA	University of Pennsylvania	Nonprofit	No	Yes	2 PT	-	1 FT 1 PT	1	-	-	1	-
Richmond, VA	Virginia Commonwealth University	Nonprofit	No	Yes	2 FT	-	3 FT 6 PT	-	2	-	3	1 consulting pharmacist
Washington, DC	MedStar Health	Nonprofit	Yes (3)	Yes	6 FT	-	5 FT	5	1	-	5	1 LPN
Wilmington, DE	Christiana Care Health Systems	Nonprofit	Yes (5)	Yes	2 FT 4 PT	3 FT 1 PT	4 FT 3 PT	-	5	3	4	
Independent practices												
Brooklyn, NY	Heal ^c	For profit	Yes (5)	Yes	3 FT	7 FT	21 FT	-	-	-	-	1 quality assurance nurse
Durham, NC	None	For profit	No	Yes	35 FT	55 FT ^d		-	-	-	12	6 podiatrists, 2 psychologists

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Exhibit A.2 (continued)

Site	Affiliation	Ownership	Participate in ACO (years)	Accept Medicare Advantage plans	Clinicians making home visits			Other staff involved in care team				
					Physicians	PAs	NPs	Care coordinators ^a	RNs	MAs	SWs	Others
Portland, OR	None	Nonprofit	No	Yes	3 PT	2 PT	5 FT 11 PT	6	5	-	4	1 LPN, 1 pharmacist

Source: Information gathered from structured data collection before interviews with practice staff conducted from November 2019 to February 2020 (late in Year 6 of the demonstration to the beginning of Year 7).

Notes: For information on the two practices that left the demonstration after Year 5 (Austin and Cleveland), refer to the [evaluation report](#) which covered Years 1 to 5 of the IAH demonstration.

^a Care coordinators are health professionals that help to manage a patient's care by monitoring and coordinating patients' care plans, connecting them with health care providers, and making telephone check-in calls. IAH sites use differing titles for this category of care, including nurse navigators, patient care coordinators, and care managers. For IAH practices, these staff generally are MAs, RNs, or LPNs.

^b The Flint site did not provide a breakdown of physicians, PAs, or NPs.

^c The Brooklyn site began the demonstration as an independent practice and was acquired by Heal during Year 6 (2019).

^d The Durham site did not provide a breakdown on the number of PAs and NPs; instead, it classified them together under the count of 55.

ACO = accountable care organization; DME = durable medical equipment; FT = full-time; LPN = licensed practical nurse; MA = medical assistant; NP = nurse practitioner; PA = physician assistant; PT = part-time; RN = registered nurse; SW = social worker; VPA = Visiting Physicians Association.

Exhibit A.3. IAH practice care delivery: Operational characteristics, as of 2019

Site	Visits per clinician per day	Clinicians' panel size	Nonbillable visits	Weekend visits	After-hours visits ^a	Visits outside of home ^b
VPA practices						
Dallas, TX	9 to 10	175	Yes: nurse navigator visits	Occasionally ^c	No	No
Flint, MI	8 to 9	175	Yes: nurse navigator	Yes	No	No
Jacksonville, FL	8 to 9	175	Yes: nurse navigator visits	Occasionally ^c	No	No
Lansing/ Ann Arbor, MI	8 to 9	175	Yes: nurse navigator and home health company visits	Occasionally ^c	No	No
Milwaukee, WI	8 to 9	175	Yes: nurse navigator visits	Yes	No	No
Academic medical centers						
Boston, MA	4	90	Yes: nurse care manager visits	No	Yes: for urgent visits only; uncommon	Yes
Long Island, NY	6	200	Yes: community paramedicine, RN, and SW visits	No	No	No
Philadelphia, PA	6	140	No	Yes	Yes: for urgent visits only; uncommon	Yes
Richmond, VA	3 to 4	40	Yes: nurse visits	No	No	Yes
Washington, DC	6	150	Yes: nurse visits	Yes	Yes: for regular visits; uncommon	Yes
Wilmington, DE	6	90 to 120	Yes: RN or SW visits	No	No	Yes
Independent medical practices						
Brooklyn, NY	8 to 10	120 to 130	Yes: visits to uninsured patients	Yes	Yes: for urgent and regular visits; common	No
Durham, NC	10 to 15	150 to 200	No	Yes	No	No
Portland, OR	4 ^d	122	Yes: RN, social worker, or chaplain visits	Yes	No	No

Source: Information gathered from structured data collection before interviews with practice staff conducted from November 2019 to February 2020 (late in Year 6 of the demonstration to the beginning of Year 7).

Notes: For information on the two practices that left the demonstration after Year 5 (Austin and Cleveland), refer to the [evaluation report](#) which covered Years 1 to 5 of the IAH demonstration.

^a After-hour visits are those done outside of the practice's normal business hours. This can vary from practice to practice.

^b Visits outside of the home refers to those in nonhome settings such as an office, outpatient clinic, inpatient hospital, or skilled nursing facility.

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Exhibit A.3 (continued)

^c The term occasionally regarding weekend visits varies from practice to practice. The Dallas site defines it as “Saturdays occasionally.” The Jacksonville site defines it as “a case-by-case basis up to 6/7 times a year.” The Lansing/Ann Arbor site defines it as “up to the providers’ discretion.”

^d The Portland site provided an average of visits per clinician per day rather than a range.

RN = registered nurse; SW = social worker; VPA = Visiting Physicians Association.

Exhibit A.4. IAH practices’ care delivery processes, as of 2019

Site	Formal risk-stratification classification	Remote access to patient’s record, remote data collection, remote submission of orders	Notification of hospital admission or ED visit	Proactive outreach to patients or caregivers
VPA practices				
Dallas, TX	Yes. It is based on hospital or ED admissions: if patient has two or more visits in 60-day period, the patient is enrolled in an intensive care management program	Yes	Rely on hospital staff to notify practice	Yes. Call as needed based on acuity of patient
Flint, MI	Yes. It is based on hospital or ED admissions: if patient has two or more visits in 60-day period, the patient is enrolled in an intensive care management program	Yes	Automated notice from all sites within the state through HIE	Yes. Call as needed based on acuity of patient
Jacksonville, FL	Yes. It is based on hospital or ED admissions: if patient has two or more visits in 60-day period, the patient is enrolled in an intensive care management program	Yes	Automated notice from all sites within the state through HIE	Yes. Call as needed based on acuity of patient
Lansing/Ann Arbor, MI	Yes. It is based on hospital or ED admissions: if patient has two or more visits in 60-day period, the patient is enrolled in an intensive care management program	Yes	Automated notice from all sites within the state through HIE	Yes. Call as needed based on acuity of patient
Milwaukee, WI	Yes. It is based on hospital or ED admissions: if patient has two or more visits in 60-day period, the patient is enrolled in an intensive care management program	Yes	Rely on hospital staff to notify practice	Yes. Call as needed based on acuity of patient
Academic medical centers				
Boston, MA	No. Clinical judgment only	Yes	Automated notices from some sites through EHR	Yes. Call as needed based on care plan
Long Island, NY	Yes. Determines level of proactive outreach and care team involved	No	Automated notices from some sites through EHR	Yes. Call as needed based on acuity of patient

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Exhibit A.4 (continued)

Site	Formal risk-stratification classification	Remote access to patient's record, remote data collection, remote submission of orders	Notification of hospital admission or ED visit	Proactive outreach to patients or caregivers
Philadelphia, PA ^c	No. Clinical judgment	Yes	From within the health system, but not from other systems	Yes. Call as needed based on clinician's judgment
Richmond, VA ^c	No. Clinical judgment only	Yes	Automated notice from practice's own hospital	No
Washington, DC ^c	No. Clinical judgment only	Yes	Automated notices from some sites through EHR	Yes. Monthly call
Wilmington, DE	Yes. Software assesses patients and assigns level of acuity score, which determines level of proactive outreach and care team involvement	Yes	Automated notice from all sites within the state through HIE	Yes. Call as needed based on the acuity of the patient
Independent practices				
Brooklyn, NY	No. Clinical judgment only	Yes	Automated notice from some sites through EHR	Yes. Proactive outreach regarding vaccines in partnership with Merck
Durham, NC	No. Clinical judgment only	Yes	Automated notice from some sites through EHR	Yes. No formal schedule, call as needed
Portland, OR	Yes. Not fully rolled out, but it covered about three-quarters of patients in late 2019	Yes	Automated notice from all sites within the state through HIE	Yes. Call as needed based on acuity of patient and if patient was recently hospitalized

Source: Information gathered from structured data collection before interviews with practice staff conducted from November 2019 to February 2020 (late in Year 6 of the demonstration to the beginning of Year 7).

Notes: For information on the two practices that left the demonstration after Year 5 (Austin and Cleveland), refer to the [evaluation report](#) which covered Years 1 to 5 of the IAH demonstration.

ED = emergency department; EHR = electronic health record; HIE = health information exchange; VPA = Visiting Physicians Association.

III. Identifying the IAH beneficiaries

To comply with the legislation that established the IAH demonstration, the demonstration used a site-based enrollment process. Sites were responsible for ensuring that enrollees met health status and other clinical and programmatic requirements such as providing consent. The implementation contractor used administrative data and information provided by the sites to construct the list of enrolled beneficiaries as part of its work to calculate spending by IAH beneficiaries in each practice.

Although the implementation contractor used Medicare claims data, other administrative data, and information provided by the sites to construct the list of enrollees, Mathematica used only Medicare claims and other administrative data to identify the IAH group for the evaluation. (See Chapter V of this appendix for more information about the data sources we used to determine eligibility.) To measure the effect of the demonstration, we had to use the same data sources and approach to identify the IAH and comparison groups across all pre-demonstration and demonstration years. Information provided by the sites to construct the list of IAH enrollees was available for the demonstration years only, not the pre-demonstration years. In addition, no information other than administrative data was available for the comparison group. As a result, we used only administrative data to define the IAH group in each pre-demonstration and demonstration year rather than using the information the sites provided to the implementation contractor. We describe our process for defining the IAH group in this chapter. We describe our process for identifying the comparison group in Chapter III of this appendix.

The approaches of Mathematica and the implementation contractor to identifying eligible beneficiaries yielded different counts of IAH practices' beneficiaries in Years 1 to 6. After explaining these approaches in Chapters III.A and III.B, we provide details about reasons for differences in the counts of IAH practices' eligible beneficiaries in Chapter III.C.

A. IAH implementation contractor's process for determining the sample of enrolled beneficiaries

The IAH sites identified beneficiaries they thought were eligible to participate in the demonstration; we list the eligibility requirements in Chapter I. After providing these beneficiaries with information about the demonstration and visiting homes to explain it, the IAH sites enrolled willing participants and uploaded a list of potential enrollees to a reporting system created for the demonstration using a process established by the implementation contractor. The contractor then used administrative data to verify that each enrolled beneficiary had a qualifying hospital admission and used rehabilitation services in the previous 12 months, was covered by Medicare Parts A and B, and was not enrolled in a Medicare Advantage plan as of the date of IAH enrollment.

In addition to verifying whether the beneficiaries enrolled by the practices had a qualifying hospital admission and used rehabilitation services, the implementation contractor also helped IAH sites identify potential beneficiaries for enrollment into the demonstration based on the eligibility criteria. The contractor identified beneficiaries who received at least one home visit by the demonstration practice and had qualifying hospital admission and rehabilitation service events but whom the sites had not yet enrolled in the reporting systems; these beneficiaries were called potential enrollees. The contractor provided the sites with information on the potential enrollees, and the sites then reviewed their records and assessed additional information about the beneficiaries' eligibility (such as whether they met the ADL

and chronic condition criteria). Clinicians followed up with potential enrollees who met all demonstration criteria and enrolled them in the demonstration.

The implementation contractor set the enrollment date as the first day of the month after the beneficiary had a qualifying hospital admission, used rehabilitation services, and received a home visit by the IAH practice within the previous 12 months. The home visit by the practice might have occurred before or after the qualifying hospital admission and rehabilitation services as long as all three occurred within 12 months before the enrollment date.

If the beneficiary did not meet the demonstration eligibility criteria, the sites provided the implementation contractor with the reason for the beneficiary's ineligibility. Reasons sites reported for not enrolling beneficiaries whom the contractor identified as potential enrollees included the following: (1) the beneficiary did not meet the ADL or chronic condition criteria; (2) the beneficiary received primary care from another practice and the IAH practice was not considered the beneficiary's primary practice; (3) the beneficiary began receiving hospice care, moved into a nursing home, or died before receiving notification of his or her eligibility for the demonstration; and (4) the beneficiary refused to participate in the demonstration. If the IAH practice did not provide any reasons for ineligibility for a potential enrollee, the implementation contractor assumed that the beneficiary was eligible and added that person to the official demonstration enrollment records.

We refer to all beneficiaries confirmed as IAH participants in the implementation contractor's records as enrolled beneficiaries. Unless an IAH practice disenrolled a beneficiary—or a beneficiary died or was no longer enrolled in Medicare fee for service (FFS)—CMS allowed beneficiaries who enrolled in the demonstration in a given year to continue in the demonstration whether or not they requalified in subsequent years as IAH eligible or had a home visit from the IAH practice in subsequent years.

B. Mathematica's process for identifying the sample of eligible and attributed beneficiaries for the evaluation

To identify beneficiaries eligible for the demonstration and attributed to a demonstration practice, Mathematica used different processes and data sources than those used by the implementation contractor and the IAH sites. As we explained earlier, our method for measuring the effect of the demonstration required us to use the same data sources and approach to identify the IAH and comparison groups across all pre-demonstration and demonstration years. We could not use enrollment in the demonstration as part of determining who would be in our sample because enrollment was based in part on information from the IAH practices. Therefore, the IAH group consisted of all beneficiaries eligible for the demonstration in that year according to our analysis of Medicare enrollment, claims, and assessment data.

We used the following criteria to identify beneficiaries for the IAH group:

- Enrollment in FFS Medicare
- Two or more ADLs that required human assistance
- Two or more chronic conditions
- Inpatient hospital admission or observation stay in the previous 12 months⁶

⁶ This includes acute care, critical access, and psychiatric hospitals.

- Use of acute or subacute rehabilitation services in the previous 12 months⁷
- Not in hospice or long-term care for the entire time they were eligible for the demonstration in a given year and not on hospice on the first day of demonstration eligibility

For beneficiaries enrolled in the demonstration, the eligibility date determined by Mathematica based on administrative data sometimes differed from the enrollment date determined by the implementation contractor. Mathematica set the eligibility date as the first day of the month following the last service use required to qualify for the demonstration. For example, if a beneficiary had a hospital admission in July 2019 and home health care in October 2019, that person would be eligible for demonstration Year 6 as of November 1, 2019.

In the following section, we explain why we identified the IAH group separately for each study year rather than retaining IAH beneficiaries in the sample until the demonstration ended. Then, we explain why we removed from the sample IAH practices that did not participate in the demonstration in a given year. Next, we describe how we used assessment data to measure limitations in ADLs. Finally, we explain how we used Medicare claims to attribute eligible beneficiaries to the IAH group.

1. Rationale for identifying the IAH group separately for each study year

An alternative to identifying the IAH group separately for each study year, which is called a repeated cross-sectional design, would be to retain beneficiaries in the IAH (and comparison) group until the demonstration ended as long as the beneficiary remained enrolled in Medicare FFS. This approach is sometimes referred to as intent to treat, and it requires following all beneficiaries ever attributed to the IAH group during the demonstration, including those who did not requalify as eligible for IAH or stopped receiving care from the IAH practice.⁸ This type of study design would have enabled us to evaluate outcomes for beneficiaries over a longer period, including for beneficiaries who did not requalify as eligible for IAH because they did not have another hospital admission or observation stay.

We did not use this type of study design for two primary reasons: (1) this approach can attenuate (that is, underestimate) true effects because it requires retaining beneficiaries in the intervention group who are no longer served by IAH practices, and (2) this approach would have required changing the length of the pre-demonstration period each time the demonstration was extended. First, unless one expected the intervention to affect outcomes for several years after receipt of the intervention ended, the risk of underestimating true effects is high if a substantial minority of intervention group beneficiaries stopped receiving care from the IAH practice. This is the case with the IAH demonstration; among the beneficiaries from the Year 1 IAH group who remained alive and enrolled in FFS in Year 2, 38 percent were not attributed to the IAH practice in Year 2. In other words, 38 percent of beneficiaries from Year 1 did not receive care from an IAH practice in Year 2. We discuss attribution criteria in detail later in this section, but in general, attribution would have required the following: (1) at least one home visit from the IAH practice in Year 2, and (2) for a beneficiary who was alive more than 3 months in Year 2, a second

⁷ This includes discharge from inpatient rehabilitation hospitals and rehabilitation units or SNFs and use of home health (but not necessarily discharge). We did not include long-term care hospitals.

⁸ The reason that an intent to treat design retains beneficiaries in the analysis even if they stop receiving care from the IAH practice is to avoid introducing bias related to attrition. That is, the intent to treat design considers attrition as part of the estimated population-level effect that should be included in the estimate. For example, if beneficiaries who continue to receive care from an IAH practice have lower expenditures than those who stop receiving care from an IAH practice, failing to include beneficiaries who no longer receive care from IAH practices could overstate the reduction in total expenditures for the full population compared to what might be expected, were the demonstration to be rolled out in another setting.

visit from the IAH practice. The percentage of the intervention group who were no longer served by IAH practices would have increased over time as we retained beneficiaries in the sample for the entire demonstration (or at least for several years), diluting any actual effect of the IAH payment incentive on expenditures and other outcomes. Second, a study design that retains beneficiaries in the sample for several years after they no longer meet IAH eligibility criteria carries a substantial risk of bias due to changes in health status (and resulting expenditures) that could have been associated with unobservable factors that led to IAH beneficiaries receiving care from IAH practices. For example, it is possible that declining cognitive status is what led some beneficiaries to start receiving home-based primary care from an IAH practice, and that (1) similar declines did not happen for the comparison group and (2) declining cognitive status tends to lead to higher (or lower) Medicare expenditures. If these unmeasured changes in cognitive status occurred over several years during the demonstration period—and not in the shorter pre-demonstration period—then retaining beneficiaries in the IAH sample for the entire demonstration could result in biased estimates of the effects of the payment incentive. The way to reduce this risk would be to have a pre-demonstration period equal to the length of the demonstration period, since both periods would offer the same amount of time for beneficiaries to experience unmeasured changes. However, since the demonstration was extended four times by Congress, this would have required updating the pre-demonstration period multiple times—including generating new estimated effects for earlier demonstration years.

2. Rationale for removing from the sample IAH practices that did not participate in the demonstration in a given year

In all, 18 practices began the demonstration in 2020. Our estimates for Years 1 to 5 are based on data for the 14 practices that completed Year 5 of the demonstration, and our estimates for Year 6 are based on data for the 12 practices that completed Year 6. An alternative to removing IAH practices from the sample if they did not participate in the demonstration in a given year would be to retain practices until the demonstration ended. This type of study design may have enhanced external generalizability of the impact estimates because the practices that left the demonstration may have been different from—and possibly less successful at reducing expenditures than—the practices that remained in the demonstration. We did not use this approach for the evaluation of the IAH demonstration for two reasons.

First, for a variety of reasons, we could not have included data from subsequent demonstration years for several of the practices that left the demonstration. For example, one practice that left the demonstration in Year 2 did not have enough home-based primary care patients in the pre-demonstration period to be included in our sample in any year. In other words, we could not include this practice in our analyses of the effect of the demonstration regardless of whether it continued participating. Another practice was terminated from the demonstration by CMS for violating the Federal False Claims Act and subsequently closed. Including this practice in our analysis when it was in operation would have caused bias in our results when comparing expenditures for the IAH beneficiaries with those of a comparison group. A third practice—the practice that had a substantial influence on the estimated results for Year 5—stopped providing home-based primary care after it left the demonstration.⁹ Even if we had wanted to include this practice until the demonstration ended, we could not have done so because we could not identify IAH beneficiaries for this practice after it stopped providing home-based primary care. We examined the relative influence of individual practices (see Chapter VI.B), however, and we considered this information when interpreting results.

⁹ For more information, refer to the [evaluation report](#) which covered Years 1 to 5 of the IAH demonstration.

Second, this demonstration was not designed to draw conclusions about the broad Medicare FFS population. The IAH practices were not selected to represent the national population of practices providing home-based primary care to Medicare beneficiaries with multiple chronic conditions and substantial functional limitations. Rather, among the pool of home-based primary care practices that volunteered for the demonstration, CMS selected 18 sites to represent different types of practices and geographic areas. Thus, even if we were to retain all IAH practices in the sample until the demonstration ended, we could not generalize the results of this study to Medicare FFS beneficiaries who received home-based primary care from practices other than those in the demonstration.

3. Eligibility and assessment data

We measured ADL limitations in accordance with the guidelines the IAH implementation contractor gave to IAH practices. Those guidelines stated that beneficiaries qualify as having an ADL limitation if they require any type of human assistance with the activity. The exception to this general guideline was for wheelchair use: use of a wheelchair as the primary mode of mobility with or without human assistance qualified as an ADL limitation for enrollment in the IAH demonstration.

To measure limitations in ADLs for the evaluation sample, we used assessment data from the given pre-demonstration or demonstration year. We used three sources of assessment data: (1) the Outcome and Assessment Information Set (OASIS), collected when beneficiaries receive home health care; (2) the Minimum Data Set (MDS), collected when beneficiaries receive SNF care; and (3) the Inpatient Rehabilitation Facility Patient Assessment Instrument (IRF-PAI), collected when beneficiaries receive inpatient rehabilitation facility care. All three data sets provided information about the extent to which beneficiaries could complete the six standard ADLs: dressing, bathing, toileting, transferring, ambulating, and feeding. Transferring includes transfer between bed and chair and excludes transferring to or from the bath or toilet. Each assessment instrument has one or more data elements that indicate the extent of limitations, if any, for each of the six ADLs. If beneficiaries did not have any assessment data in a given year, they were ineligible for the demonstration in that year, and we did not include them in our sample.

We faced three challenges when measuring limitations with the six ADLs. First, each ADL is coded differently in each of the three data sets. Second, different providers collect ADL data at different points in time. Third, beneficiaries can have multiple assessments in a given year. Next, we discuss how we handled each of those three challenges.

a. Each ADL is coded differently in each data set

Each ADL limitation is coded differently in each data set, and the codes do not always clearly define someone's need for human assistance to do the activity. We reviewed all of the values of each variable that measured ADL functioning. If the value for beneficiaries indicated that they required human assistance to do the activity safely, we classified them as requiring human assistance with that ADL; we had to measure the need for human assistance as best we could.

In cases in which the level of functioning did not make clear that beneficiaries required human assistance to complete the activity, we erred on the side of not including patients. For example, one of the possible values for the transferring data element in an OASIS assessment was "able to transfer with minimal human assistance or with use of an assistive device," such as a walker. If beneficiaries had an OASIS assessment with that value for the transferring data element, we did not consider them to have a limitation that required human assistance for transferring based on that particular assessment. This conservative approach excluded from our sample beneficiaries who required a device but not human assistance, such as

beneficiaries who could get out of bed alone when using a walker. It may also have excluded some people, however, who required human assistance and therefore could be IAH eligible.

Although we usually did not score beneficiaries as having a limitation if they required human assistance or an assistive device, we applied one exception to that rule. In accordance with the guidelines given to IAH practices by the implementation contractor, use of a wheelchair as the primary mode of mobility with or without human assistance qualified as an ADL limitation.

b. Different providers collect ADL data at different points in time

CMS requires that health care providers conduct OASIS, MDS, and IRF-PAI assessments at specific points in time. For example, beneficiaries who received skilled nursing services for a 60-day period may have had MDS data from assessments at admission, at discharge, and at the time of any significant changes in status. Because providers conduct each of these assessments at multiple points in time, we had to determine which assessments we would use in measuring ADL limitations to determine IAH eligibility. We used discharge assessments from all three data sets as well as interim assessments from the OASIS data set. We did not use admission or interim assessments from the MDS and IRF-PAI because beneficiaries must be discharged from a SNF or inpatient rehabilitation facility before becoming eligible for IAH. Unlike with skilled nursing and inpatient rehabilitation services, beneficiaries can receive Medicare-funded home health care on the date they become eligible for IAH. Therefore, we included interim OASIS assessments in addition to discharge assessments to ensure we had the latest information in the study year.¹⁰

c. Beneficiaries can have multiple assessments in a given year

Beneficiaries could have had more than one assessment in a given year. For example, in one demonstration year, beneficiaries could have had three sets of assessment data: an interim OASIS assessment from home health care, a discharge OASIS assessment from home health care, and a discharge MDS assessment from skilled nursing care. When beneficiaries had more than one assessment in a given year, we kept the most recent assessment in which beneficiaries had at least two ADL limitations. We selected the most recent ADL assessment in which beneficiaries had at least two ADL limitations because we sought to identify beneficiaries who were least likely to recover from the ADL limitation. If beneficiaries had assessment data during a given year but not at least two ADL limitations in any of those assessments, they were ineligible for the demonstration in that year, and we did not include them in our sample. In addition, if beneficiaries did not have any assessment data in a given year, they were ineligible in that year, and we did not include them in our sample.

4. Attribution and enrollment data

In addition to determining eligibility for the demonstration, in each year we applied the following criteria for attributing a patient to a demonstration site (we used Medicare claims data for visits to the IAH practice that occurred between the date of eligibility for the demonstration and the end of the demonstration year):

- Residence in the same state as the demonstration practice

¹⁰ Interim home health (OASIS) assessments do not include scoring on one activity: feeding. Because this item's effect on overall eligibility determination is small, we did not apply any adjustments to interim assessments.

- At least one evaluation and management or non–evaluation and management home visit from the demonstration practice; home included private homes, assisted living facilities, group homes, and custodial care facilities¹¹
- For beneficiaries eligible for the demonstration for more than three months, at least one additional visit from the demonstration practice in the home, an assisted living facility, or an office

The demonstration rules required that all patients of the IAH practice eligible for the demonstration be enrolled in the demonstration. Therefore, we required only one home visit for attribution to the IAH practice for beneficiaries eligible for the demonstration for three months or less. Some beneficiaries eligible for the demonstration for many months in a given year may have had only one visit with the IAH practice before returning to office-based primary care. To reduce the chance that the analysis sample would include beneficiaries who received only a single visit from the IAH practice, we required at least one additional visit from the practice for beneficiaries eligible for the demonstration for more than three months.

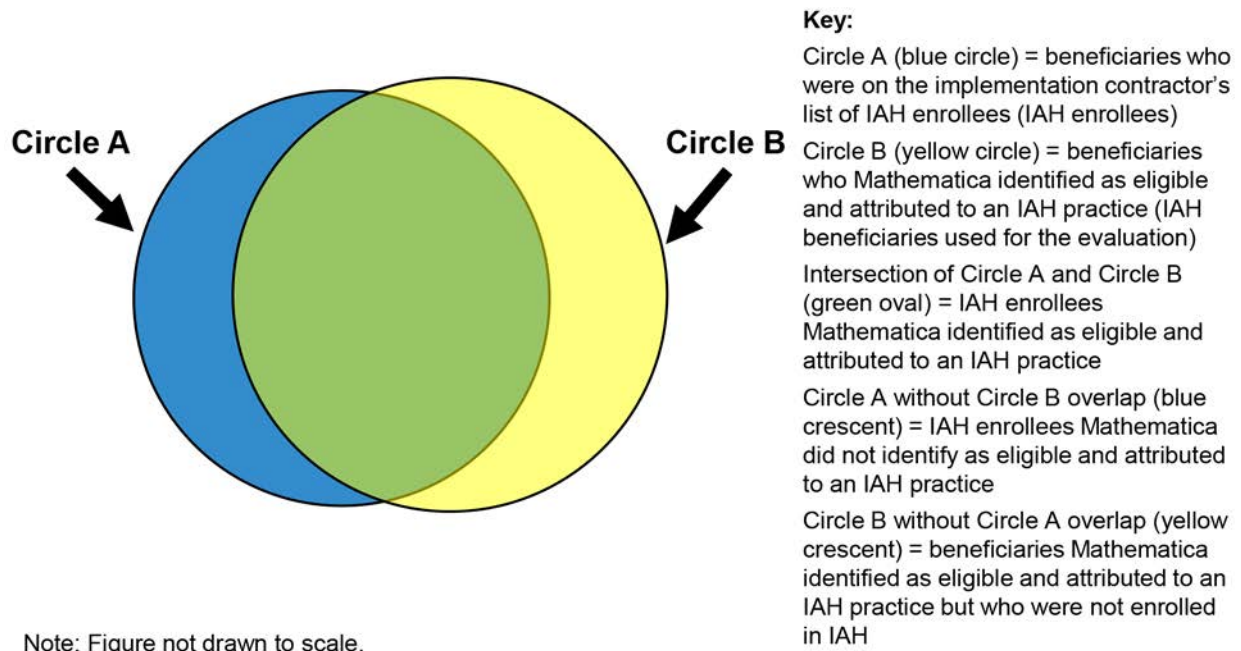
In each of the eight pre-demonstration and demonstration years, we refer to the beneficiaries who met eligibility criteria for IAH in administrative data and were attributed to a demonstration site as Mathematica-eligible IAH beneficiaries (or simply IAH beneficiaries). IAH beneficiaries were the treatment group for the evaluation. For beneficiaries to be in the IAH group for the evaluation, they had to meet the eligibility and attribution criteria outlined above according to Mathematica’s analysis of Medicare enrollment, claims, and assessment data.

A beneficiary’s enrollment (or non-enrollment) in the demonstration did not affect whether that person was in the IAH group for the evaluation. As we described, demonstration enrollment was based in part on data from the IAH practices, such as ADL limitations, chronic conditions, and residence in a long-term nursing home. In contrast, we excluded beneficiaries from the evaluation IAH group who were not eligible for the IAH demonstration and attributed to the IAH site according to administrative data (in Appendix A.6, this is the part of Circle A excluding Circle B, or the blue crescent). We excluded those beneficiaries from the IAH group for two reasons: (1) we needed to identify the IAH group consistently in all study years, but demonstration enrollment data existed for the demonstration years only, not the pre-demonstration years; and (2) we could not replicate the enrollment process for comparison group members. In other words, we had no practice-reported data for identifying IAH beneficiaries in the pre-demonstration years, nor did we have such data for comparison group members in any year. Because our study design required that we use the same data sources to identify IAH and comparison beneficiaries in all years, we could not use practice-reported data to identify IAH beneficiaries in the demonstration years.

As shown in Exhibit A.5 and in the rest of this appendix, we use the term green oval to refer to beneficiaries who were enrolled in the demonstration and met its eligibility and attribution criteria in administrative data according to Mathematica’s analysis of those data. We use yellow circle to refer to beneficiaries who met the eligibility and attribution criteria for the demonstration regardless of whether they were enrolled in the demonstration. The yellow circle is the group we refer to as IAH beneficiaries (the treatment group for the evaluation). Enrollees who were not in the evaluation IAH group (the blue crescent) were those who were enrolled but not confirmed as eligible for the demonstration or attributed to the IAH site according to administrative data.

¹¹ Evaluation and management visit refers to a patient–provider encounter during which the provider assesses the patient’s medical history, evaluates the patient, and engages in medical decision making.

Exhibit A.5. Groups of IAH beneficiaries based on different identification processes



After we identified an IAH beneficiary, that beneficiary remained in the sample for the rest of the demonstration or pre-demonstration year unless the person died or left Medicare FFS. For example, if an IAH beneficiary became eligible for the demonstration in February 2019 (Month 2 of Year 6) and moved out of the IAH practice's geographic area or entered long-term care in April 2019, we continued to follow that beneficiary through the end of the study year (December 31, 2019, for all practices in Year 6).

Demonstration Year 1 (June 2012 to May 2013).¹² Mathematica identified 8,216 beneficiaries who met the demonstration eligibility criteria and were attributed to the demonstration practice during the first year (Exhibit A.6). This group represented the IAH group in the first year of the demonstration. It included 4,530 beneficiaries who were enrolled in the IAH demonstration according to the implementation contractor (in Exhibit A.5, the intersection of Circles A and B, the green oval) and 3,686 beneficiaries not enrolled in the IAH demonstration in Year 1 (in Exhibit A.5, Circle B excluding Circle A, the yellow crescent). The analysis sample did not include the 2,405 beneficiaries whom the implementation contractor identified as enrollees but whom we did not find eligible for the demonstration using administrative data (in Exhibit A.5, Circle A excluding Circle B, the blue crescent).

¹² For sites that began the demonstration in June 2012, Month 1 was June. For sites that began the demonstration in September 2012, Month 1 was September.

Exhibit A.6. Numbers of beneficiaries based on different identification processes

Demonstration year	Mathematica-eligible IAH beneficiaries ^a			
	Mathematica-eligible and IAH-enrolled (green oval)	Mathematica-eligible only (yellow crescent)	Total IAH group (all Mathematica-eligible regardless of enrollment, yellow circle)	IAH-enrolled only (blue crescent)
1	4,530	3,686	8,216	2,405
2	4,564	2,702	7,266	4,059
3	4,498	3,066	7,564	4,718
4	6,019	3,485	9,504	5,663
5	5,950	4,008	9,958	6,407
6	2,692 ^b	5,301	7,993	3,696

Source: Mathematica’s analysis of data from the IAH implementation contractor and 2009–2019 Medicare claims and enrollment data from the Chronic Conditions Warehouse for IAH and matched comparison group beneficiaries. Data for Years 1 to 5 reflect the 14 IAH practices that participated in Year 5. Data for Year 6 reflect the 12 IAH practices that participated in Year 6.

^a This corresponds to the yellow circle in Exhibit A.5, which encompasses all Mathematica-eligible IAH beneficiaries (that is, those who met the demonstration eligibility criteria and were attributed to the demonstration practice).

^b The withdrawal of Durham in Month 10 of Year 6 was the largest contributor to the decrease in the number of Mathematica-eligible and IAH-enrolled beneficiaries from Year 5 to Year 6. As a result of this early withdrawal, the implementation contractor did not identify any Durham patients as IAH enrollees. We retained Durham in the evaluation for Year 6 because it participated in the demonstration for most of the year.

Demonstration Year 2 (June 2013 to May 2014).¹³ In Year 2, Mathematica identified 7,266 beneficiaries who met the demonstration eligibility criteria and were attributed to the demonstration practice. This group represented the IAH group in the second year of the demonstration. Of these 7,266 IAH beneficiaries, 4,564 were enrolled in the IAH demonstration in Year 2 (in Exhibit A.5, the green oval), and 2,702 beneficiaries were not enrolled (the yellow crescent). As in Year 1, the analysis sample for the evaluation did not include the 4,059 beneficiaries who were enrolled in the demonstration in Year 2 but whom we did not find eligible for the demonstration using administrative data (the blue crescent).

Beneficiaries enrolled but not eligible or attributed according to Mathematica in Year 2 (in Exhibit A.5, the blue crescent in Year 2) included people who enrolled for the first time in Year 2. They also included two groups of beneficiaries who initially enrolled in Year 1 and continued to be enrolled in Year 2: those who were eligible and attributed according to administrative data in Year 1 (that is, those included in the yellow circle in Year 1) and those not eligible according to administrative data in Year 1 (the blue crescent in Year 1). The enrollment process did not require someone who was enrolled in Year 1 to meet the qualifications for enrollment in Year 2.

The IAH group for the Year 2 analysis sample consisted of the 7,266 beneficiaries identified as eligible and attributed by Mathematica (in Exhibit A.5, the yellow circle). As we explained, our method for measuring the effect of the demonstration required us to use the same data sources and approach to identify the IAH and comparison groups across all pre-demonstration and demonstration years. When we identified the Year 2 IAH beneficiaries, we did not consider whether a beneficiary was in the IAH group, comparison group, or neither group in Year 1. Therefore, the Year 2 IAH group included beneficiaries who were in the analysis sample in Year 1 and requalified in Year 2 by meeting eligibility and attribution

¹³ For sites that began the demonstration in June 2012, Month 1 was June. For sites that began the demonstration in September 2012, Month 1 was September.

requirements as well as people not in the analysis sample in Year 1. It did not include beneficiaries who were in the IAH group in Year 1 but did not requalify for the IAH group in Year 2 because they failed to meet eligibility or attribution requirements. Including beneficiaries who qualified for the IAH group in Year 1 but did not requalify in Year 2 would potentially bias our estimates of the effect of the demonstration in Year 2 because non-requalifying beneficiaries in Year 2 could differ from the IAH beneficiaries in Year 1 and the pre-demonstration years, all of whom were selected without regard to which beneficiaries were in the IAH group in the prior year.

Demonstration Year 3 (June 2014 to May 2015).¹⁴ In Year 3, Mathematica identified 7,564 beneficiaries who met the demonstration eligibility criteria and were attributed to the demonstration practices. This group represented the IAH group in the third year of the demonstration. Of these 7,564 IAH beneficiaries, 4,498 were enrolled in the IAH demonstration in Year 3 (in Exhibit A.5, the intersection of Circle A and Circle B, or green oval), and 3,066 were not enrolled (the yellow crescent). These 7,564 beneficiaries included people in the analysis sample in Years 1 or 2 and who requalified in Year 3 by meeting eligibility and attribution requirements as well as people not in the analysis sample in either of the first two years. These beneficiaries could be new patients who met the eligibility criteria or patients who previously received care from the IAH practice and did not meet the eligibility criteria for the demonstration until Year 3.

As in demonstration Year 1, the analysis sample for the evaluation did not include the 4,718 beneficiaries who were on the implementation contractor's enrollment list in Year 3 but whom we did not find eligible for the demonstration using administrative data (in Exhibit A.5, the blue crescent). Beneficiaries enrolled but not eligible according to Mathematica in Year 3 included those who enrolled for the first time in Year 3. Beneficiaries enrolled but not eligible according to Mathematica in Year 3 also included beneficiaries who initially enrolled in Years 1 or 2, continued to be enrolled in Year 3, but did not requalify for the demonstration in Year 3 because they failed to meet eligibility or attribution requirements.

Demonstration Year 4 (October 2015 to September 2016). In Year 4, Mathematica identified 9,504 beneficiaries who met the demonstration eligibility criteria and were attributed to the demonstration practices. This group represented the IAH group in the fourth year of the demonstration. Of these 9,504 IAH beneficiaries, 6,019 were enrolled in the demonstration (in Exhibit A.5, the green oval), and 3,485 were not (the yellow crescent). These 9,504 beneficiaries included people who were in the analysis sample in Years 1, 2, or 3 and requalified in Year 4 by meeting eligibility and attribution requirements as well as people not in the analysis sample in any of the first three years.

As in Years 1 to 3, the analysis sample for the evaluation did not include the 5,663 beneficiaries enrolled in the IAH demonstration in Year 4 but who we found ineligible for or attributed to the demonstration using administrative data (the blue crescent). The Year 4 IAH group also did not include beneficiaries who initially enrolled in Years 1, 2, or 3, continued to be enrolled in Year 4, but did not requalify for the demonstration in Year 4 because they failed to meet eligibility or attribution requirements.

The IAH group was substantially larger in Year 4 than in previous demonstration years. For all sites combined, this group increased 26 percent from Year 3 to Year 4. Five sites had increases of more than 20 percent from Year 3 to Year 4: Brooklyn, Durham, Dallas, Flint, and Portland. This increase could reflect the expansion of existing IAH practices. Brooklyn merged with another home-based primary care practice, and the Durham practice has expanded throughout North Carolina since the demonstration

¹⁴ For sites that began the demonstration in June 2012, Month 1 was June. For sites that began the demonstration in September 2012, Month 1 was September.

began. In Year 4, Dallas expanded into a new geographic area, and Flint added clinicians in its existing geographic area. Finally, Portland's sample size in Year 4 was larger than in Year 3 but was about the same size as in Year 1. The increase from Year 3 to Year 4 also could have been caused in part by some IAH practices participating in accountable care organizations (ACOs) in Year 4. Several practices did so in Year 4, including three of the five practices with the largest increases in sample sizes: Brooklyn, Dallas, and Flint. Other providers in the ACO may have referred some patients to the IAH practice. We discuss the implications of ACO participation in the limitations chapter of this appendix, Chapter VII.

Demonstration Year 5 (October 2016 to September 2017). In Year 5, Mathematica identified 9,958 beneficiaries who met the demonstration eligibility criteria and were attributed to the demonstration practices. Of these 9,958 IAH beneficiaries, 5,950 were enrolled in the demonstration (in Exhibit A.5, the green oval), and 4,008 were not (the yellow crescent). These 9,958 beneficiaries included people in the analysis sample in Years 1 to 4 who requalified in Year 5 by meeting eligibility and attribution requirements as well as those not in the analysis sample in any of the first four years.

As in Years 1 to 4, the analysis sample for the evaluation did not include the 6,407 beneficiaries who were enrolled in the IAH demonstration in Year 5 but whom we found ineligible for or attributed to the demonstration using administrative data (in Exhibit A.5, the blue crescent). The Year 5 IAH group also did not include beneficiaries who initially enrolled in Years 1 to 4, continued to be enrolled in Year 5, but did not requalify for the demonstration in Year 5 because they failed to meet eligibility or attribution requirements.

The noticeable increase in size from Year 3 to Year 4, in which the IAH group increased by 26 percent, did not repeat in Year 5. Rather, the sample size increased by only 5 percent, consistent with the observed increase from Years 2 to 3 (4 percent). This stability suggests that the observed increase in the overall size of the IAH sample in Year 4 was likely the result of events that may not reoccur in subsequent years, such as Brooklyn merging with another home-based primary care practice. This finding would be consistent with the proposed reasons for sample size increases outlined in the Year 4 summary above.

Demonstration Year 6 (January to December 2019). In Year 6, Mathematica identified 7,993 beneficiaries who met the demonstration eligibility criteria and were attributed to the demonstration practices. Of these 7,993 IAH beneficiaries, 2,692 were enrolled in the demonstration (In Exhibit A.5, the green oval), and 5,301 were not (the yellow crescent). These 7,993 beneficiaries included people in the analysis sample in Years 1 to 5 who requalified in Year 6 by meeting eligibility and attribution requirements as well as those not in the analysis sample in any of the first five years.

As in Years 1 to 5, the analysis sample for the evaluation did not include the 3,696 beneficiaries who were enrolled in the IAH demonstration in Year 6 but whom we found ineligible for or attributed to the demonstration using administrative data (in Exhibit A.5, the blue crescent).¹⁵ The Year 6 IAH group also did not include beneficiaries who initially enrolled in Years 1 to 5, continued to be enrolled in Year 6, but

¹⁵ When reviewing claims data for IAH enrollees at Brooklyn in Year 6, we found a large increase in the number of beneficiaries who were IAH-eligible and received care from a Brooklyn clinician but who were not included in the evaluation sample because their claims did not have the necessary procedure code(s) for attribution to the IAH practice. There were 55 such beneficiaries in Year 5 and 280 such beneficiaries in Year 6. Most of the reviewed claims for these 280 beneficiaries were for home health recertification with patient not present, suggesting that the observed changes may have been the result of changes in coding processes coinciding with a change in ownership from Years 5 to 6.

did not requalify for the demonstration in Year 6 because they failed to meet eligibility or attribution requirements.

The number of beneficiaries in the IAH group according to our analysis of administrative data (in Exhibit A.5, the yellow circle) decreased by 20 percent from Year 5 to Year 6. This decrease was a departure from the increases seen in previous years. Nearly all of this decrease can be attributed to three factors: (1) Cleveland and Austin leaving the demonstration after Year 5, (2) a substantial decrease in the number of IAH beneficiaries identified for Brooklyn, and (3) a drop in clinicians treating patients at the Jacksonville practice. If we were to exclude those four practices in both years, the number of IAH beneficiaries was nearly as large in Year 6 as in Year 5.

The number of beneficiaries who were in the IAH group according to our analysis of administrative data but not enrolled in the demonstration (in Exhibit A.5, the yellow crescent) increased by 32 percent from Year 5 to Year 6, a departure from the trend in previous years (13 to 15 percent year-over-year increases from Year 3 to Year 5). Some of this change was a result of the withdrawal of Durham toward the end of Year 6. The implementation contractor finalizes its list of enrollees after each demonstration year, reflecting practices that completed participation in that year. Beneficiaries from Durham were not identified as enrollees because of the midyear withdrawal, which means that no beneficiaries from Durham were included in the green oval.

In addition to site-level variation from Year 5 to Year 6, there were two data artifacts that may have contributed to the year-over-year sample variability. First, at the time we identified our Year 6 sample, some of the assessment data we use to measure assistance with ADLs were unavailable. OASIS data were available for assessments administered from January 2019 to November 2019 but not for those administered in December 2019. IRF-PAI data were available for assessments administered from January 2019 to September 2019 but not for those administered from October 2019 to December 2019.

Second, the IRF-PAI underwent a major revision between Year 5 and Year 6, and most IRF-PAI variables we used for ADL identification in previous study years were not available in Year 6. The two versions of the IRF-PAI administered in 2019 contained other items that evaluated ADLs. Using these other variables, we constructed new measures of ADLs requiring human assistance that are as similar as possible to the original measures. Changes in the IRF-PAI assessment, however, may have led to slight differences in ADL measures in Year 6 compared with earlier years.

Neither of these issues—missing data for OASIS and IRF-PAI and the change in the ADL measures in the IRF-PAI—poses a substantial risk of bias to the estimated impacts of the demonstration in Year 6, primarily because these issues affect both the IAH and comparison groups. A secondary factor that limits concern is that only a small share of the sample in each year is identified as IAH-eligible based on IRF-PAI data.

C. Reasons for the differences between demonstration enrollment and evaluation analysis cohorts

The evaluation analysis group identified by Mathematica and the enrolled group identified by the implementation contractor differed for two overarching reasons: (1) the use of different data sources and (2) the use of different analytic techniques. The primary reason Mathematica used different data sources and analytic techniques was that the implementation contractor had to identify only an IAH group, whereas Mathematica had to identify both an IAH group and a comparison group. Because Mathematica had to use the same procedures to identify both groups, and we could not obtain clinical data from the

comparison group’s primary care providers, we relied on administrative data alone when identifying the IAH group for the evaluation. This approach was in contrast to that of the implementation contractor, which used administrative data and data from IAH practices to identify IAH enrollees. This difference resulted in Mathematica excluding some beneficiaries identified as enrollees by the implementation contractor and including some beneficiaries in the IAH group for the evaluation who were excluded by the implementation contractor. We presented a detailed discussion regarding the differences in the samples for the evaluation versus enrollment in a prior report; we highlight key findings in this chapter.¹⁶

1. Reasons some IAH enrollees did not meet Mathematica’s eligibility or attribution criteria

The use of different data sources was the primary reason Mathematica excluded some beneficiaries from the IAH group that the implementation contractor identified as enrollees. In each demonstration year, most IAH enrollees not identified by Mathematica did not meet the ADL criterion because they had missing or insufficient ADL information in the assessment data. In contrast, the contractor used information provided by the IAH practices to determine whether a beneficiary required human assistance with at least two ADLs. In addition, the number of enrollees that Mathematica did not find eligible for the demonstration increased over time because beneficiaries remained on the IAH enrollment list from one year to the next regardless of whether they met IAH eligibility criteria again.

2. Reasons some beneficiaries found eligible and attributed by Mathematica were not enrolled

Among those who were in the IAH group for the evaluation but were not IAH enrollees, Mathematica identified three groups of beneficiaries:

- **Beneficiaries not found to be eligible by the implementation contractor based on administrative data.** As part of determining eligibility for enrolling in the demonstration, the contractor considered the dates that the beneficiary had a hospital admission, used rehabilitation services, and had a home visit from the IAH practice. Mathematica, however, considered only the dates of the qualifying hospital admission and rehabilitation services stay. Mathematica did not rely on the date of a home visit when measuring the 12-month period and setting the demonstration eligibility date because we could not replicate that requirement for the comparison group, who did not receive home-based primary care and therefore received no home visit.
- **Beneficiaries excluded from enrollment based on information from IAH practices.** The reason sites offered most frequently for excluding a beneficiary from enrollment was that the beneficiary did not meet the ADL criterion. The implementation contractor used information provided by the IAH practices to determine whether a given ADL required human assistance, which provided a more nuanced picture of ADL severity. Mathematica used only administrative data when identifying ADLs that required human assistance because information from clinicians was not available for the comparison group.
- **Beneficiaries who disenrolled from the demonstration.** Enrollees may voluntarily disenroll from the demonstration when they change clinicians within the practice service area, are discharged by the practice, decline home care, or elect hospice and change clinicians. If the beneficiary voluntarily disenrolled within six months of enrollment in the demonstration, the implementation contractor did not identify that beneficiary as an enrollee in the final enrollment list for a given year. Mathematica

¹⁶ For more information, refer to the [evaluation report](#) which covered Years 1 to 4 of the IAH demonstration.

did not exclude a beneficiary who voluntarily disenrolled within six months because we could apply no such restriction to the comparison group.

D. Characteristics of IAH beneficiaries

To understand how characteristics of IAH-eligible beneficiaries differed from the average Medicare beneficiary at the start of the IAH demonstration, we used Medicare administrative data to identify beneficiaries who met IAH eligibility criteria and were treated by IAH practices in the year before the demonstration. In the year before the demonstration, more than half of beneficiaries who met the IAH eligibility criteria data were age 80 or older, and 40 percent were dually eligible for Medicare and Medicaid (Exhibit A.7). The demonstration eligibility criteria focused on Medicare beneficiaries who were chronically ill and disabled. As a result, about 43 percent of IAH eligible beneficiaries had 10 or more chronic conditions, and 55 percent required human assistance with at least five ADLs. On average, IAH eligible beneficiaries incurred nearly \$4,400 in Medicare expenditures PBPM in the year before the demonstration. They had an average of 1.8 hospital admissions and 2.9 ED visits per year.

IAH eligible beneficiaries were more likely to be dually eligible, to be older, to have more chronic conditions, and to have a higher mortality rate than the average Medicare beneficiary. Among the IAH states, the average percentage of Medicare beneficiaries who were dually eligible in 2013 was 19 percent (Kaiser Family Foundation). In 2012, 36 percent of beneficiaries who resided in the community (not a facility) were older than age 75, 26 percent had five or more chronic conditions, and 3 percent died over the course of the survey year (CMS 2012). IAH eligible beneficiaries also struggled with daily activities at a higher rate than the average Medicare beneficiary. Only 12 percent of Medicare beneficiaries who resided in the community reported difficulties in performing three or more ADLs without human assistance or special equipment, such as a walker or grab bar (CMS 2012).

Exhibit A.7. Demographic characteristics and health status, Medicare expenditures, and service use of beneficiaries who were eligible for IAH and treated by IAH practices in the year before the demonstration

Variable name	Value for beneficiaries eligible for IAH in the year before the demonstration
Demographic characteristics and health status	
Percentage age 80 or older	51.7
Percentage dually eligible for Medicare and Medicaid	40.1
Average HCC score	3.5
Percentage with 10 or more chronic conditions	42.7
Percentage requiring human assistance with at least five activities of daily living	55.0
12-month mortality (percentage)	18.1
Average Medicare expenditures per beneficiary per month	
Total	\$4,397
Inpatient hospital services	\$1,741
Skilled nursing facility services	\$605
Home health (Parts A and B)	\$781
Hospice services	\$153
Outpatient services	\$253
Clinician/supplier services	\$715
Durable medical equipment	\$150

Variable name	Value for beneficiaries eligible for IAH in the year before the demonstration
Average numbers of key health service use events per beneficiary per year	
Number of hospital admissions ^a	1.8
Number of potentially avoidable hospital admissions ^b	0.5
Number of ED visits	2.9
Probability (as a percentage) of having a qualifying hospital discharge and unplanned readmission within 30 days of discharge	19.6

Source: Mathematica’s analysis of data from the IAH implementation contractor and 2010–2012 Medicare claims and enrollment data from the Chronic Conditions Warehouse for IAH and matched comparison group beneficiaries in all IAH practices that participated in Year 6.

^a The number of hospital admissions includes observation stays.

^b The number of potentially avoidable hospital admissions includes observation stays. A potentially avoidable hospital admission is one in which appropriate primary and specialty care may prevent or reduce the need for a hospital admission.

ED = emergency department; HCC = Hierarchical Condition Category.

IV. Identifying the comparison group

In this chapter, we begin by describing how we used Medicare administrative data to identify a potential comparison group of beneficiaries who were eligible for the demonstration, lived in the same area as the IAH beneficiaries, and did not receive home-based primary care. Next, we present the methods and results of propensity score matching. Finally, we present the number of IAH and comparison beneficiaries and eligible months in the evaluation sample.

A. Identifying the potential comparison group

To identify the potential comparison group beneficiaries, we relied on administrative data. We identified a set of potential comparison beneficiaries from each state in each year. We based our analyses on data for two pre-demonstration years and six demonstration years. Beneficiaries who had no visits to any of the demonstration practices in the study year and met all demonstration eligibility criteria were eligible to be in the potential comparison group for all sites in that state in that year. For example, a beneficiary who lived in Michigan, had no visits from any IAH practice, and met all demonstration eligibility criteria in Year 2 was in the potential comparison group for Flint and Lansing. We refer to these comparison groups as potential because we identified the final comparison groups using propensity score matching (described later in Chapter III.B). Because we sought to compare beneficiaries who primarily received in-home physician care with those who did not receive such care, we excluded from the potential comparison group all beneficiaries who had two or more home visits from any clinician during or after their first month of eligibility through the end of the study year. In addition, we excluded all beneficiaries who had any visit from an IAH practice in the study year. As with the IAH beneficiaries, we did not assess whether potential comparison beneficiaries had home visits before the first month of eligibility.

In addition, to control for possible geographic variation in practice styles, access to services, and costs, we restricted our comparison groups to beneficiaries who lived in the ZIP codes served by the demonstration practices. The list of ZIP codes served by a demonstration practice in a given year reflected all ZIP codes in which the practice’s IAH beneficiaries lived in that year according to beneficiary address information in Medicare administrative data. For example, if a site operated in one state and had at least one IAH beneficiary who lived in each of 57 ZIP codes in that state during demonstration Year 1, the potential

comparison group for that site in Year 1 included all beneficiaries who met demonstration eligibility requirements, had no visits to any demonstration practice in that year, had no more than one home-based primary care visit in that year, and lived in one of those ZIP codes. We used this ZIP code–based restriction for all practices in all years.

For the six practices located in states that had two demonstration practices (Brooklyn and Long Island in New York as well as Flint and Lansing in Michigan in Years 1 to 6; Austin and Dallas in Texas in Years 1 to 5), some ZIP codes contained IAH beneficiaries for two practices. We could not simply restrict potential comparison beneficiaries only to those living in the ZIP codes represented by beneficiaries served by the IAH practice in a given year because it would have allowed a single potential comparison beneficiary to be selected as a matched comparison for two IAH beneficiaries in different practices. In those cases, we identified the potential comparison group by conducting a preliminary propensity score matching (using the same model to predict treatment status we describe below) to split the comparison sample into two potential comparison groups.¹⁷ For each pair of sites located in the same state, we included in the preliminary model all of the IAH beneficiaries in those two sites as well as all beneficiaries in the comparison pool for both sites after applying the ZIP code restriction. Each comparison beneficiary was matched to an IAH beneficiary in one of the two sites; this process determined the site potential comparison pool to which the beneficiary was assigned. After using preliminary matching to split the overlapping comparison sample into two potential comparison groups (one group per practice), we matched IAH beneficiaries to the potential comparison group for each practice using the same approach as for other sites.

As with IAH beneficiaries, we again identified beneficiaries in the matched comparison group in demonstration Years 1, 2, 3, 4, or 5 as potential comparison beneficiaries if they met all IAH eligibility requirements in Year 6.

B. Propensity score matching methods

For each analysis year before and after the demonstration began, we used propensity score matching to create a comparison group of nonparticipants similar in observable characteristics to IAH beneficiaries but who did not receive home-based primary care. The goals of matching were twofold. First, we sought to minimize nonrandom selection of people in the IAH group by constructing a matched comparison group that appeared similar to the treatment group on key observable characteristics that affect treatment status (such as receipt of home-based primary care from an IAH practice) and outcomes. Subject to that constraint, we then sought to maximize the size of the comparison group to increase statistical efficiency. For the IAH demonstration, key characteristics for matching included those that determined eligibility for the demonstration and measures of health status, health trajectory, and other personal characteristics observable in administrative data that predict health care expenditures. Limiting the comparison group to Medicare beneficiaries who closely matched the observed characteristics of the IAH group may also have reduced differences between the two groups on unobserved characteristics if those characteristics were correlated with matching variables.

We conducted matching for the entire IAH group, which consisted of beneficiaries who met the eligibility and attribution criteria based on administrative data (in Exhibit A.5, the yellow circle). For Year 6, for example, we matched 7,993 IAH beneficiaries on observable characteristics with beneficiaries who were

¹⁷ Conducting a preliminary match typically provides a better match in both sites than using a random split because it ensures that the covariate distribution for the pool of eligible comparison beneficiaries is closely aligned with the covariate distribution for the treatment beneficiaries at each of the two practices.

similar and lived in the same geographic area but did not receive home-based primary care. We matched each site separately, including each member of the Mid-Atlantic Consortium. We created a comparison group for each practice by estimating a propensity score equation using data for the IAH group and the potential comparison group, and then using the results to find the best matches for each IAH beneficiary.

We used demographic and health-related variables to match beneficiaries in the IAH group with comparison beneficiaries. We used only one measure for exact matching: the number of months since the beneficiary's last hospital admission (one, two or three, or four or more months). Exact matching means that an IAH beneficiary could be matched only to potential comparison beneficiaries who had the same value of that variable. We chose this measure for exact matching because expenditures and use—our key outcomes of interest—tend to be substantially higher in the months after a hospital admission. Preliminary data analyses indicated that adding other exact matching variables would likely result in dissimilarities on other key characteristics, such as disability. Therefore, we chose not to add other exact matching variables. We used two other measures related to eligibility for the demonstration as ordinary matching variables: (1) because a beneficiary can enter the sample at any time in a given year, we used a categorical measure of the month the beneficiary met eligibility criteria (Months 1, 2 to 6, or 7 to 12); and (2) because beneficiaries who had an observation stay may have been less acutely ill than those with a hospital admission, we used whether the beneficiary had an observation stay but not a hospital admission in the prior year (Exhibit A.8). We included the following demographic variables in the matching model but did not seek exact matches for them: age (younger than 65, 65 to 79, or 80 or older), gender, race, whether the beneficiary was dually eligible for Medicare and Medicaid, original reason for Medicare eligibility, and number of ADLs (two, three or four, or five or six). We used an indicator variable to identify beneficiaries with missing information for feeding assistance.

We used various measures of health status. We measured individual HCCs using each beneficiary's claims history for the 12 months before the date of eligibility for the demonstration in a given year. Beneficiaries who meet IAH eligibility criteria are at much higher risk of mortality in a given year than the average Medicare FFS beneficiary, and mortality can substantially affect expenditures in the year before death. To increase the likelihood that the comparison group was as similar as possible to the IAH beneficiaries in health status measures that predict mortality, we matched the IAH and comparison beneficiaries on risk factors for mortality. After reviewing the literature on mortality among Medicare beneficiaries, we selected chronic conditions or diagnoses that were significant predictors of mortality for use in matching. We included an HCC in the matching equation if Gagne et al. (2011) had identified a diagnosis code as predicting mortality among elderly Medicare beneficiaries with low income. We collapsed several of the individual HCCs based on the type of condition, frequency in the IAH group, and a relative factor, the last of which represents the contribution of that HCC to the overall HCC risk score.^{18,19,20} We also used the risk score itself as a matching variable. Additional details about how we calculated the HCC score and HCC indicators are available in Chapter V of this appendix.

¹⁸ For example, we combined cirrhosis of the liver (HCC 28) and chronic hepatitis (HCC 29) into a single indicator for matching but did not combine them with end-stage liver disease (HCC 27). Less than 2 percent of the treatment group had cirrhosis of the liver or chronic hepatitis; the relative factor for those conditions was less than half of the relative factor for end-stage liver disease.

¹⁹ Table 9 of the Announcement of Calendar Year (CY) 2012 Medicare Advantage Capitation Rates and Medicare Advantage and Part D Payment Policies and Final Call Letter lists the relative factor for each HCC. Available at <https://www.cms.gov/Medicare/Health-Plans/MedicareAdvgtgSpecRateStats/Downloads/Announcement2012.pdf>.

²⁰ We used software version V2119 to calculate HCC scores for beneficiaries in Year 6, which incorporated version 10 of the *International Classification of Diseases*.

Exhibit A.8. Variables used in propensity score matching equation

Variable
Eligibility and use
Number of months since most recent hospital admission (1, 2 or 3, 4 or more)
Month of the demonstration year beneficiary met eligibility criteria (1, 2 to 6, 7 to 12) ^a
Whether beneficiary had an observation stay and no hospital admission in prior 12 months
Demographic characteristics
Age: younger than 65, 65 to 79, 80 or older
Gender
Race: White, Black, other, or unknown
Dually eligible for Medicare and Medicaid
Original reason for Medicare entitlement: old age, ESRD or ESRD and disability, disability only
ADLs
Number of ADLs for which beneficiary requires human assistance (2, 3 to 4, 5 to 6)
Whether information about the feeding ADL was missing ^b
Health status
HCC risk score
Specific HCCs
HCC 8, metastatic cancer and acute leukemia ^c
HCC 9–10, lung and other severe cancers; lymphoma and other cancers
HCC 11–12, colorectal, bladder, and other cancers; breast, prostate, and other cancers and tumors
HCC 18, diabetes with chronic complications
HCC 21, protein-calorie malnutrition
HCC 27, end-stage liver disease
HCC 28–29, cirrhosis of liver; chronic hepatitis
HCC 46, severe hematological disorders
HCC 48, coagulation defects and other specified hematological disorders
HCC 51, dementia with complications ^c
HCC 52, dementia without complications ^c
HCC 54–55, drug/alcohol psychosis; drug/alcohol dependence
HCC 57–58, schizophrenia; major depressive, bipolar, and paranoid disorders
HCC 70–71, quadriplegia; paraplegia
HCC 72, spinal cord disorders/injuries
HCC 85, congestive heart failure ^c
HCC 96, specified heart arrhythmias
HCC 103–104, hemiplegia/hemiparesis, monoplegia, other paralytic syndromes
HCC 106, atherosclerosis of the extremities with ulceration or gangrene
HCC 107–108, vascular disease with complications; vascular disease
HCC 111, chronic obstructive pulmonary disease
HCC 134, dialysis status ^c
HCC 136–138, chronic kidney disease, stages 3–5 ^c
HCC 139–140, chronic kidney disease, stages 1–2 or unspecified; unspecified renal failure
HCC 157–159, pressure ulcer of skin with necrosis through to muscle, tendon, or bone; or with full or partial thickness skin loss

Variable
Depression ^d
Anemia
Fluid and electrolyte disorders
Number of chronic conditions (2 to 5, 6 to 9, 10 or more) ^d
Whether beneficiary had a complicating condition or major complicating condition during the most recent hospital admission
Chronically critically ill or medically complex diagnosis

Note: Exact matching means that an IAH beneficiary can be matched only to a potential comparison beneficiary with the same characteristic. An ordinary matching variable is one used as an independent variable in the matching regression equation.

^a For pre-demonstration years and Years 1 to 3, Month 1 was June or September. For sites that began the demonstration in June 2012, Month 1 was June. For sites that began the demonstration in September 2012, Month 1 was September. All sites began Years 4 and 5 in October 2015 and October 2016, respectively. Month 1 was January 2019 for Year 6.

^b Feeding assessments were not available with home health assessment data at the time of recertification. If the beneficiary had a previous assessment during the study year that was recorded at the time of discharge from home health care, we used the feeding values from that assessment, but sometimes there was no previous discharge assessment.

^c Gagne et al. (2011) identified these measures as key predictors of mortality; they are the measures of health status we prioritized most highly when determining which of several alternative matched comparison groups was most appropriate for a particular site in a particular year.

^d These are chronic condition categories measured by the Chronic Conditions Warehouse.

ADL = activity of daily living; ESRD = end-stage renal disease; HCC = Hierarchical Condition Category.

In addition to the HCCs included in the matching equation based on Gagne et al. (2011), we included an HCC indicator of pressure ulcers because a large share of the IAH population has poor functional status and could be at higher-than-average risk for a pressure ulcer. We included three other conditions not measured by HCCs: anemia, depression, and electrolyte disorders. Gagne et al. (2011) identified anemia and electrolyte disorders as predictive of mortality.

We included two other measures of health status using diagnosis codes from the beneficiary’s most recent hospital admission in the past year. The first measure indicated whether the Medicare Severity Diagnosis Related Group included a complicating condition or major complicating condition. The second measure indicated whether, according to the diagnosis in the claim, the beneficiary was chronically critically ill or medically complex (Kandilov et al. 2014).

C. Results of propensity score matching

The standardized difference in means is a standard statistic used to assess similarities between the treatment group and the final matched comparison group (Stuart 2010). The literature suggests that a standardized difference of less than 0.25 is an appropriate threshold for determining that the treatment and comparison groups are well matched on a particular variable (Rubin 2001). We applied a more stringent standard of 0.10 for our matching. We examined the matching results for both of the variables used in the matching algorithm and the variables that could be important to control for but could not be included, such as individual HCCs aggregated with other HCCs in the matching equation (for example, cirrhosis of the liver and chronic hepatitis), and individual chronic conditions measured by the Chronic Conditions Warehouse.

Across all 12 sites together (treating the three Mid-Atlantic Consortium sites as one), the absolute value of the standardized difference in the sixth demonstration year was less than 0.10 on all matching variables and less than 0.10 on all but one nonmatching variable (Exhibit A.9). All 12 sites individually had standardized differences of less than 0.25 on all of the matching variables; for 11 of those sites, the standardized differences were also less than 0.10 on all of the matching variables (data not shown). Furthermore, 8 of the sites had standardized differences of less than 0.25 on all of the nonmatching variables.

Exhibit A.9. Characteristics of potential comparison beneficiaries, matched comparison beneficiaries, and IAH beneficiaries, Year 6

Variable	Potential comparison group	Matched comparison group	IAH beneficiaries	Standardized difference
Eligibility for the demonstration				
Number of months since most recent hospital admission, percentage^a				
One	58.0	38.4	38.4	0.000
Two or three	15.5	16.8	16.8	0.000
Four or more	26.5	44.8	44.8	0.000
Month of the demonstration year that beneficiary met eligibility criteria, percentage^b				
Month 1	43.2	67.0	65.5	-0.032
Months 2 to 6	31.7	21.5	22.6	0.026
Months 7 to 12	25.0	11.5	11.9	0.012
Observation stay and no hospital admission in previous 12 months	5.4	10.4	9.6	-0.026
Demographic characteristics				
Female	60.6	65.4	65.7	0.007
Dually eligible for Medicare and Medicaid	25.8	38.5	39.2	0.014
Age, percentage				
Younger than 65	10.8	13.9	14.3	0.013
65 to 79	41.7	33.3	33.5	0.005
80 or older	47.5	52.8	52.2	-0.014
Race and ethnicity, percentage				
White	75.3	72.3	72.3	0.000
Black	18.2	22.3	22.3	0.000
Other	6.5	5.4	05.4	-0.001
Original reason for Medicare entitlement, percentage				
Age	73.4	66.6	66.0	-0.012
Disability	24.5	32.1	32.7	0.012
ESRD or ESRD and disability	2.2	1.3	1.3	0.001
Number of ADLs requiring human assistance, percentage				
Two	11.0	6.6	6.6	0.000
Three or four	28.8	30.3	30.0	-0.006
Five or six	60.2	63.1	63.4	0.005
Missing information about feeding ADL	7.8	15.9	16.5	0.019
Health status				
HCC risk score	3.82	4.22	4.26	0.021
HCC 8, metastatic cancer	5.1	1.7	1.7	-0.002
HCC 9–10, lung, lymphoma, and other cancers	6.1	3.5	3.4	-0.005
HCC 11–12, colorectal, bladder, breast, prostate, and other cancers	11.2	8.2	7.9	-0.011
HCC 18, diabetes with chronic complications	37.9	39.9	39.2	-0.015

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Exhibit A.9 (continued)

Variable	Potential comparison group	Matched comparison group	IAH beneficiaries	Standardized difference
HCC 21, protein-calorie malnutrition	15.0	21.7	22.9	0.029
HCC 27, end-stage liver disease	1.9	1.0	1.0	-0.003
HCC 28–29, cirrhosis of liver and chronic hepatitis	3.0	2.4	2.3	-0.002
HCC 46, severe hematological disorders	1.8	1.0	1.1	0.008
HCC 48, coagulation defects and other specified hematological disorders	18.2	14.5	14.5	0.001
HCC 51, dementia with complications	7.2	16.2	17.5	0.037
HCC 52, dementia without complications	18.7	30.5	29.5	-0.023
HCC 54–55, drug/alcohol psychosis and drug/alcohol dependence	7.7	8.8	8.7	-0.002
HCC 57–58, schizophrenia, major depressive, bipolar, and paranoid disorders	22.9	35.0	35.7	0.016
HCC 70–71, quadriplegia, paraplegia	2.9	6.9	7.7	0.033
HCC 72, spinal cord disorders/injuries	2.7	2.1	2.1	0.002
HCC 85, congestive heart failure	46.7	52.7	52.0	-0.014
HCC 96, specified heart arrhythmias	40.4	37.2	36.4	-0.016
HCC 103–104, hemiplegia/hemiparesis, monoplegia, other paralytic syndromes	10.6	15.1	15.5	0.012
HCC 106, atherosclerosis of the extremities with ulceration or gangrene	49.0	4.6	4.4	-0.007
HCC 107–108, vascular disease with or without complications	44.4	52.0	51.8	-0.004
HCC 111, chronic obstructive pulmonary disease	32.4	37.0	36.3	-0.015
HCC 134, dialysis status	6.3	4.1	4.0	-0.006
HCC 136–138, chronic kidney disease, stages 3–5	9.7	13.0	12.7	-0.009
HCC 139–140, chronic kidney disease stages 1–2, unspecified renal failure	4.5	5.8	6.0	0.007
HCC 157–159, pressure ulcer of skin with necrosis or skin loss	8.8	17.4	18.5	0.031
Number of chronic conditions measured by Chronic Conditions Warehouse				
Fewer than six	13.8	9.7	10.5	0.026
Six to nine	48.4	45.4	45.7	0.005
More than nine	37.8	44.8	43.8	-0.021
Anemia ^c	17.8	18.0	17.9	-0.003
Depression	48.0	58.5	58.6	0.003
Fluid and electrolyte disorders ^c	40.7	43.6	43.4	-0.004
Diagnosis of chronically critically ill or medically complex ^d	37.2	38.0	38.6	0.013
Complicating condition or major complicating condition during the most recent hospital admission	60.5	59.2	59.3	0.001

Evaluation of Independence at Home: Years 1 to 6 - Appendices

Exhibit A.9 (continued)

Source: Medicare claims and enrollment data for 2018–2019 obtained from the Chronic Conditions Warehouse for IAH and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 6.

Notes: The final sample sizes in Year 6 were 7,993 IAH beneficiaries and 32,496 matched comparison beneficiaries. The number of weighted matched comparison beneficiaries equaled the number of IAH beneficiaries.

^a Variable used for exact matching.

^b Month refers to the first month in the demonstration year after the beneficiary met eligibility criteria. For example, if a beneficiary had a qualifying admission and rehabilitation services in one or more months before the demonstration, the Month 1 group included that person. For all sites in Year 6, Month 1 was January.

^c Measured using claims from the most recent hospital admission or observation stay in the year before the demonstration eligibility date. We drew diagnosis codes for these conditions from Gagne et al. (2011).

^d Measured using diagnoses from the most recent hospital admission in the year before the demonstration eligibility date. We drew diagnoses from Kandilov et al. (2014).

ADLs = activities of daily living; ESRD = end-stage renal disease; HCC = Hierarchical Condition Category.

As in Year 6, the IAH and matched comparison groups were very similar in each of the first five demonstration years. Across all sites together in each of the first five years, the absolute value of the standardized difference was less than 0.10 on all matching variables and less than 0.25 on all variables not included in matching. At an individual site level, all 12 sites in Years 1 to 5 had standardized differences of less than 0.25 on all of the matching variables; most of the 12 sites had standardized differences of less than 0.10 on all of the matching variables.

D. Number of beneficiaries and eligible months

Beneficiaries in both the IAH group and comparison group were analyzed from the month they became eligible for the demonstration and observed for the remaining months in a given demonstration year. Over the six years of the demonstration, the number of IAH beneficiaries varied; for each IAH beneficiary, we matched up to five comparison beneficiaries. On average, each IAH beneficiary matched to four comparison beneficiaries. Across the demonstration years, the average number of eligible months for the comparison beneficiaries was slightly smaller than among the IAH beneficiaries (Exhibits A.10 and A.11). This difference arose because the comparison beneficiaries were more likely to die during the demonstration year than the IAH beneficiaries (as shown in Exhibit B.11a, regression-adjusted mortality in Year 6 was 15.2 percent for IAH beneficiaries and 21.1 percent for comparison beneficiaries), and the IAH beneficiaries were more likely to qualify for the demonstration earlier in the 12-month period than the comparison beneficiaries. To address any possible concerns that this difference might cause, we incorporated an eligibility fraction into the weighting design for regressions, in which the eligibility weight reflected the number of months eligible for the demonstration in a given year. For example, a beneficiary eligible for the demonstration for 6 months in Year 6 had half the weight of a beneficiary eligible for the demonstration for 12 months in Year 6. Using an eligibility fraction in the weight ensured that each beneficiary's contribution to the estimation was proportionate to how long we observed that person during a given year. In addition, we added two control variables: number of months since most recent hospital admission and month of the demonstration year that the beneficiary met the eligibility criteria. In this way, we controlled for differences in the time between when beneficiaries met the service use criteria required for demonstration eligibility and their eligibility date. Those who qualified in the first month may have met both the service use criteria up to one year before the demonstration year began, and those who qualified in later months met at least one of the two service use criteria in the month immediately before the eligibility date. Chapter VI of this appendix provides additional details about weights and control variables.

Exhibit A.10. Analysis sample, by years, practices that participated in Years 1 to 5

	Two years before the demonstration	One year before the demonstration	Year 1	Year 2	Year 3	Year 4	Year 5
Number of IAH beneficiaries	6,837	7,367	8,216	7,266	7,564	9,504	9,958
Total number of eligible months for IAH beneficiaries	65,781	70,591	79,396	69,768	72,215	90,223	95,003
Average number of eligible months per IAH beneficiary	9.6	9.6	9.7	9.6	9.5	9.5	9.5
Number of comparison beneficiaries	29,517	31,888	33,916	32,248	31,259	38,365	41,387
Total number of eligible months for comparison beneficiaries	264,558	286,314	303,770	293,081	278,015	335,250	363,251
Average number of eligible months per comparison beneficiary	9.0	9.0	9.0	9.1	8.9	8.7	8.8

Source: Medicare claims and enrollment data for 2009–2017 obtained from the Chronic Conditions Warehouse for IAH and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 5.

Note: This table reflects the sample we used to estimate the effects of the IAH payment incentive in Years 1 to 5.

Exhibit A.11. Analysis sample, by years, practices that participated in Year 6

	Two years before the demonstration	One year before the demonstration	Year 1	Year 2	Year 3	Year 4	Year 5	Year 6
Number of IAH beneficiaries	5,524	5,826	7,037	6,266	6,626	8,487	9,006	7,993
Total number of eligible months for IAH beneficiaries	53,071	55,802	67,596	59,993	63,068	80,595	85,611	77,224
Average number of eligible months per IAH beneficiary	9.6	9.6	9.6	9.6	9.5	9.5	9.5	9.7
Number of comparison beneficiaries	23,839	25,089	28,325	27,315	26,630	33,348	36,707	32,496
Total number of eligible months for comparison beneficiaries	212,510	225,531	251,398	247,050	235,762	291,711	320,428	291,352
Average number of eligible months per comparison beneficiary	8.9	9.0	8.9	9.0	8.9	8.7	8.7	9.0

Source: Medicare claims and enrollment data for 2009–2019 obtained from the Chronic Conditions Warehouse for IAH and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 6.

Note: This table reflects the sample we used to estimate the effects of the IAH payment incentive in Year 6.

V. Medicare data and measures

In this chapter, we describe the data sources and measures we used in our analyses of the effect of the demonstration.

We constructed our yearly analytic files with observations at the beneficiary year level. We drew data for determining demonstration eligibility and measuring outcomes in the analytic files from several sources (Exhibit A.12). We accessed all data through the Chronic Conditions Warehouse Data Enclave.

Exhibit A.12. Data sources

Data	Demographic characteristics	Chronic conditions	Activities of daily living	Service use: Demonstration eligibility	Service use: Outcome measures	Health outcomes
Medicare enrollment database	✓					✓
Master beneficiary summary file		✓				
Inpatient claims				✓	✓	
Outpatient claims				✓	✓	
Clinician/supplier claims				✓	✓	
Home health agency claims				✓	✓	
Skilled nursing facility claims				✓	✓	
Hospice claims				✓	✓	
Durable medical equipment claims					✓	
Inpatient rehabilitation facility–patient assessment instrument ^a			✓			
Minimum data set			✓		✓	
Outcome and assessment information set			✓			
Timeline file ^b						✓

Source: Medicare claims and enrollment data for 2009–2019

^a This instrument includes inpatient rehabilitation hospitals and rehabilitation units and excludes long-term care hospitals.

^b This file is used to measure whether a beneficiary entered institutional long-term care.

A. HCC score and indicators

To account for differences in health status and the differential risks of incurring high Medicare expenditures, we used the CMS-HCC risk-adjustment model to create HCC scores and indicators (Exhibit A.9). To estimate the HCC scores, we used a 12-month look-back period for Medicare claims to obtain diagnosis information. Because the claims-based eligibility dates for IAH and comparison beneficiaries can vary for a specific pre-demonstration or demonstration year, the 12-month look-back period also varied depending on the beneficiaries' eligibility dates. For each beneficiary in the IAH and comparison groups, we estimated the HCC score by using the publicly available HCC software (CMS 2019c) and information on demographics, Medicare eligibility, and dual eligibility status as well as Medicare claims for the 12 months before the person's claims-based eligibility date. We used fewer than 12 months of Medicare claims if a beneficiary was not enrolled in Medicare for all 12 months. We used Version 21 of the HCC model, which was developed and calibrated for the Program of All-Inclusive Care for the Elderly population, because that population resembles the IAH-eligible population in being sicker and frailer than the average Medicare beneficiary.

CMS has separate HCC models for beneficiaries residing in the community and those residing in an institution. We used the HCC score estimated by the community model for all beneficiaries in our sample. Beneficiaries cannot reside in an institution when they become eligible for the demonstration, so we did not use scores predicted by the institutional model for any beneficiary. We also did not use the demographics-only model for new enrollees. Because of the service use requirements for the demonstration, all IAH-eligible beneficiaries had some claims history during the previous 12 months. Using any available diagnosis information in the HCC model should have provided a score that captured health status better than a demographics-only model. The specific scale of the HCC score should not have affected propensity score matching if the score was estimated similarly for both IAH and potential comparison beneficiaries; thus, we did not normalize or rescale HCC scores. We did not apply any frailty factors to the HCC scores because (1) we did not have survey-based ADL measures that calculate plan-level frailty factors for the Program of All-Inclusive Care for the Elderly population and (2) we could not apply plan- or practice-specific frailty factors to the comparison group in this case. We did include, however, indicators for the number of ADLs with which the beneficiary required human assistance as control variables in all regressions.

B. Dual eligibility

When we did propensity score matching for the full sample in all demonstration and pre-demonstration years, we measured dual eligibility using the monthly Part A and Part B state buy-in variables on Medicare enrollment data because Medicaid enrollment data were not available promptly enough for us to define dual eligibility using those data. If a beneficiary had state buy-in for Part A, Part B, or both in any month in a pre-demonstration or demonstration year, we identified that person as being dually eligible in that year. We used the same measure of dual eligibility as a control variable in the regression models for Medicare expenditures and other Medicare claims-based outcomes.

C. Outcome variables

We used four groups of measures for the regression analysis of outcomes in the demonstration based on Medicare Part A and Part B claims as well as the Medicare enrollment database: (1) Medicare expenditures, (2) hospital care use, (3) quality of care, and (4) health outcomes (Exhibit A.13). We measured these outcomes for the number of months a beneficiary was observed in a study year starting with the first day of

the first month after the beneficiary met all eligibility criteria in each year based on our analysis of Medicare enrollment and administrative data.

We measured all claims-based outcomes at the beneficiary level in that particular study year. For expenditures, we measured each outcome PBPM. For example, if a beneficiary was alive and in Medicare FFS for four months from the demonstration eligibility date through the end of the year, we divided expenditures during those four months by four to measure expenditures PBPM. We annualized claims-based outcomes other than expenditures and binary measures (such as the likelihood of unplanned readmission or mortality). For example, if beneficiaries had four hospital admissions and an eligibility weight of 0.5 (because they were eligible for the demonstration for 6 of 12 months in the demonstration year), the annualized number of hospital admissions would be eight.

Exhibit A.13. Measures of Medicare expenditures, hospital use, quality of care, and health outcomes used in regressions

Measure
Medicare expenditures per beneficiary per month
Total
Inpatient
Home health service ^a
Outpatient
Skilled nursing facility
Clinician/supplier
Hospice
Durable medical equipment
Hospital use
Number of hospital admissions per beneficiary per year ^b
Number of ED visits per beneficiary per year ^c
Quality of care
Number of potentially avoidable hospital admissions per beneficiary per year (AHRQ PQI) ^b
Number of potentially avoidable outpatient ED visits per beneficiary per year (AHRQ PQI) ^c
Probability (as a percentage) of having a qualifying hospital discharge and unplanned readmission within 30 days of discharge ^d
Health outcomes
Death within the study year
Entry into institutional long-term care within the study year

Notes: We constructed measures using data from the date the beneficiary became eligible in the demonstration year through the end of that demonstration year. Following the CMMI Priority Measures for Monitoring and Evaluation, we did not truncate expenditure measures. Instead, we risk-adjusted, annualized, and weighted them to reflect partial year observations. We did not price standardize the expenditure measures.

^a Total home health expenditures include all care provided under the home health benefit. Claims for therapy appear only in the outpatient file.

^b This includes hospital admissions and observation stays.

^c We measured this as specified in the CMMI Priority Measures for Monitoring and Evaluation.

^d Qualifying hospital discharges include discharges for patients who were enrolled in Medicare FFS, discharged from nonfederal acute care hospitals, alive at the time of discharge, and not transferred to another acute care facility. Home-based primary care and the demonstration may affect whether a beneficiary has a qualifying hospital discharge in a particular year. Such an effect could lead to estimating biased rates of readmission for the IAH and comparison groups if

Exhibit A.13 (continued)

readmission is defined only for beneficiaries who had a qualifying hospital discharge, as recommended by the CMMI Priority Measures for Monitoring and Evaluation. Thus, we defined the readmission measure using all beneficiaries in the denominator rather than limiting it to beneficiaries with a qualifying discharge. For example, if home-based primary care or the demonstration reduces the likelihood of having a qualifying hospital discharge, IAH beneficiaries who have such a discharge may be sicker on average than comparison beneficiaries who have such a discharge. Being sicker could lead to an increased risk of readmission.

AHRQ = Agency for Healthcare Research and Quality; CMMI = Center for Medicare & Medicaid Innovation; ED = emergency department; FFS = fee for service; PQI = Prevention Quality Indicator.

Potentially avoidable hospital admissions and outpatient ED visits. Potentially avoidable hospital use occurs when ambulatory care may have prevented or reduced the need for a hospital admission or ED visit. We measured a beneficiary as having a potentially avoidable hospital admission or ED visit if the principal diagnosis for the hospital admission or ED visit was an ambulatory care-sensitive condition. We based our definition of ambulatory care-sensitive conditions on the Agency for Healthcare Research and Quality's Prevention Quality Indicator 90, which includes the following conditions for 2019: diabetes short-term complications, diabetes long-term complications, uncontrolled diabetes, lower-extremity amputation among diabetics, chronic obstructive pulmonary disease or asthma in older adults, hypertension, heart failure, angina without procedure, bacterial pneumonia, and urinary tract infection. The measure of potentially avoidable ED visits included only outpatient ED visits (that is, ED visits not accompanied by an admission). We excluded ED visits that led to a hospital admission because the principal diagnosis on the inpatient claim would not necessarily be the ambulatory care-sensitive condition leading to the ED visit.²¹

ED visits. Our primary measure of emergency care was total number of ED visits. To better understand the results of the effect of the demonstration on total ED visits, however, we used two other measures of such visits: (1) those that led to a hospital admission and (2) outpatient ED visits (including those visits that led to an observation stay). We used these measures because the demonstration could have different effects on the two types of ED visits. One that led to an admission may suggest that the beneficiary was more seriously ill than when such a visit did not lead to an admission. The measure of outpatient ED visits included cases in which a beneficiary was transferred to a different hospital for admission and may have included some cases in which a hospital billed ED and inpatient services separately.

Unplanned readmission within 30 days of discharge. The unplanned readmission measure indicated whether the beneficiary had at least one unplanned readmission within 30 days of a qualifying hospital discharge. Qualifying hospital discharges for the readmission measure included discharges from nonfederal acute care hospitals for patients who were enrolled in Medicare FFS, alive at the time of discharge, and not transferred to another acute care facility. The qualifying discharges included patients discharged to nonacute care settings. Index discharges did not include admissions to Prospective Payment System–exempt cancer hospitals or admissions for patients without at least 30 days of post-discharge enrollment in FFS Medicare Parts A and B (unless a patient was enrolled in FFS but died within 30 days), patients discharged against medical advice, primary psychiatric diagnoses, rehabilitation, and medical treatment of cancer.

²¹ ED visits appear in Medicare inpatient and outpatient claims. A beneficiary whose ED visit led to a hospital admission would not have a separate claim in the outpatient file; the ED claim would be part of the hospital claim in the inpatient file, which would have diagnoses that reflect the hospital stay. Using inpatient claims to measure potentially avoidable ED visits that led to hospital admission poses two problems. First, the diagnosis that led someone to the ED may be different from the diagnoses on the inpatient claim (for example, a beneficiary visits the ED because of shortness of breath but is later admitted because of another underlying factor). Second, hospital admissions with potentially avoidable diagnoses are counted in the potentially avoidable hospital admission measure. If we also counted them as potentially avoidable ED visits, we would double count the utilization.

We excluded planned readmissions from this measure. To identify them, we followed the approach used by CMS’s hospital-level 30-day risk-standardized readmission measure developed by the Yale New Haven Health Services Corporation/Center for Outcomes Research & Evaluation (2020). Unlike the Yale measure, our list of procedure codes to identify planned readmissions did not include codes that apply only to all-payer populations.

All beneficiaries who had a qualifying hospital discharge and an unplanned readmission within 30 days were identified as having an unplanned readmission. Therefore, the measure provided an estimate of the combined effect of the demonstration on whether patients had a qualifying hospital discharge and, if so, whether they had an unplanned readmission within 30 days.

Entry into institutional long-term care. The institutional long-term care measure identified beneficiaries who had at least one episode of long-term care spanning 90 or more days during a given study year. An episode of long-term care began when a beneficiary entered a skilled or unskilled nursing facility and ended when the beneficiary spent more than 14 consecutive days in the community or the study year ended. We evaluated each study year (demonstration or pre-demonstration year) separately. In other words, we required a beneficiary to have a 90-day episode of institutional long-term care during a single study year to identify that person as entering long-term care in that study year. Beneficiaries in long-term care for the entire time they were otherwise eligible for the demonstration in a given year could not be in the IAH group for the evaluation in that year.

We created this measure using the Timeline file, which combines data from claims (inpatient, SNF, and home health) and assessment data (MDS and OASIS) to flag a beneficiary’s residency status for each day of a calendar year. The daily residency flag can contain one of the following values: I (inpatient), S (SNF), M (MDS, which includes nursing home days not paid by Medicare), C (community, which includes days identified by home health claims or OASIS assessment data and days with no claims or assessment data), D (dead), or blank (not Medicare eligible). We considered all days flagged with a C, H, O or that were blank to be community days. At the time we conducted the analyses, timeline data were available only through 2018, so we calculated the long-term care measure through Year 5.

Potential episodes of long-term care began when the beneficiary entered a long-term care institution (a daily status of S or M) in a given study year. Inpatient days that occurred during a potential episode of long-term care were considered part of the long-term care episode. An episode of long-term care, however, could not begin with an inpatient stay. For example, an inpatient day that immediately preceded the beneficiary’s first SNF or MDS day did not count toward an episode of long-term care, but an inpatient day that occurred the day after a SNF day did. We counted days in the community that occurred during an episode of long-term care toward the 90-day requirement as long as there were no more than 14 consecutive community days and the beneficiary reentered an institution—a daily status of S, M, or I—on or before a 15th community day.

VI. Estimation of demonstration impacts

We used a difference-in-differences model to estimate the impact of IAH in each demonstration year. Our difference-in-differences impact estimates measured the difference in a given outcome between the year before the demonstration started and any demonstration year for beneficiaries comprising the IAH group relative to the difference during the same period for beneficiaries comprising the comparison group.

We implemented the difference-in-differences model using two approaches: a frequentist approach and a Bayesian approach. In this chapter, we describe changes in the sample starting in Year 6 and how those differences affect our estimation. Then, separately for the frequentist and Bayesian analyses, we describe the

specifications and assumptions, the model specifications we employed for different outcomes, and the methods we used to account for clustering.

A. Samples used to estimate effects in each demonstration year

Our primary estimates of the effect of the IAH payment incentive on outcomes are based on two samples of IAH practices from separate regressions, depending on the years being estimated. For demonstration Years 1 to 5, we estimated effects using a sample containing the 14 sites that participated through Year 5 of the demonstration; for demonstration Year 6, we estimated effects using a separate regression from a sample containing the 12 sites that participated in Year 6. (See Chapter III of this appendix for additional information about why we did not retain IAH practices after they stopped participating in the demonstration.) Estimates pertaining to Year 6 use a different sample in Year 6 and its corresponding baseline years than estimates pertaining to Years 1 to 5. In addition to the primary impact estimate in Year 6, we also use results from the regression containing only the 12 sites that participated in Year 6 to examine whether the estimated effect of the demonstration changed from Years 5 to 6 for sites that participated in both years.

B. Frequentist difference-in-differences model

1. Model specification for continuous and count outcomes

We estimated the impacts of the demonstration by comparing the regression-adjusted differences in outcomes between the IAH treatment and comparison groups in the pre- and post-demonstration periods. We used a difference-in-differences estimation strategy to test for differential changes in all claims-based outcomes between the IAH and comparison groups during the two pre-demonstration years and the first six years of the demonstration. Equation (1) shows the model we estimated for each outcome in Year 6:

$$(1) Y_{it} = \alpha + X_{it}\beta + \tau \cdot treatment_{it} + \gamma_{-1} PD_1 + \gamma_1 DY_1 + \gamma_2 DY_2 + \gamma_3 DY_3 + \gamma_4 DY_4 + \gamma_5 DY_5 + \gamma_6 DY_6 + \theta_{-1} treatment_{it} \cdot PD_1 + \theta_1 treatment_{it} \cdot DY_1 + \theta_2 treatment_{it} \cdot DY_2 + \theta_3 treatment_{it} \cdot DY_3 + \theta_4 treatment_{it} \cdot DY_4 + \theta_5 treatment_{it} \cdot DY_5 + \theta_6 treatment_{it} \cdot DY_6 + \varepsilon_{it}$$

where Y_{it} is the claims-based outcome measured for a beneficiary i in demonstration year t ; α is a constant term; X_{it} is a set of beneficiary characteristics measured in the index year; PD_1 is an indicator for pre-demonstration Year 1 (that is, two years before the start of the demonstration, with the year immediately preceding the demonstration serving as the reference or omitted category); $DY_1 - DY_6$ are a set of indicators for each post-demonstration year; $treatment_{it}$ is an indicator variable for being in an IAH practice; and ε_{it} is a random error term. As we describe later in this chapter, the set of beneficiary characteristics included in X_{it} were largely the same as the variables used for matching; they controlled for any remaining differences between the IAH and matched comparison groups in these characteristics.

The key parameters are $\theta_1 - \theta_6$, which constitute the difference-in-differences coefficients; they are the change in an outcome from the year before the demonstration to each year after the demonstration for the IAH group, net of the change in outcome for the comparison group during the same period. Separate estimates for each year (that is, one θ per year) allowed for nonlinearities in such trends. Last, the parameter

θ_{-1} captures the differential change in outcome between the IAH and matched comparison groups during the two pre-intervention years. We use θ_{-1} to examine whether the two groups were on the same outcome trajectories before the demonstration; we discuss this test later in the chapter.

In cases in which we estimated a linear model, such as total Medicare expenditures, the difference-in-differences coefficients $\theta_1 - \theta_6$ equaled the difference-in-differences impact estimates. In cases in which we used non-linear models, such as a negative binomial regression for the number of hospital admissions, we transformed $\theta_1 - \theta_6$ into difference-in-differences impact estimates using the following steps using the estimated impact in Year 6 as an example:

1. Using the coefficients obtained from Equation (1), we calculated the average outcomes for IAH treatment and comparison groups in each year. We adjusted the yearly average outcomes for both groups to reflect the covariate distribution of the IAH group in the latest demonstration year (Year 6). For example, we used the mean covariate values of the Year 6 IAH group to generate two estimates of predicted total Medicare expenditures in the year before the demonstration: one estimate assumed that beneficiaries received home-based primary care in that year (the IAH treatment group estimate) and one assumed that beneficiaries did not receive home-based primary care in that year (the comparison group estimate).
2. We calculated the difference of the regression-adjusted outcome for the IAH group and matched comparison group in Year 6.
3. We calculated the change in the difference between the IAH and matched comparison groups in Year 6 relative to the difference in the year before the demonstration. We refer to this estimate as the difference-in-differences impact estimate.

Our difference-in-differences impact estimates measured the change between two differences: the pre- and post-demonstration difference for IAH beneficiaries, and the pre- and post-demonstration difference for comparison beneficiaries. This method isolated the impact of the demonstration by accounting for two factors that affected outcomes. First, it accounted for the difference in outcomes between IAH and comparison beneficiaries before the demonstration, controlling for differences in observed beneficiary characteristics. Second, it accounted for changes in outcomes during the demonstration caused by factors unrelated to the demonstration that affected IAH and comparison beneficiaries over time.

In addition to reporting all difference-in-differences estimates in absolute terms, we calculated the impacts in percentage terms by dividing the impact estimate for an outcome by the unadjusted IAH group mean for that same outcome in the year before the demonstration. The percentage impact helped us to interpret whether the magnitude of an impact in a given year is meaningful in practical terms.

We used linear regressions for expenditures. We used negative binomial regressions for the number of hospital admissions and ED visits to account for over-dispersion of counts, and zero-inflated negative binomial regressions for the number of potentially avoidable hospital admissions and outpatient ED visits to account for both over-dispersion and the large percentage of beneficiaries with no admissions or outpatient ED visits during the time period.

For all outcomes, we adjusted standard errors for clustering at the practice level for the IAH group and at the beneficiary level for the comparison group (which we refer to below as hybrid clustering). We estimated the effect of the demonstration on all outcomes using two weighting schemes, which we refer to as beneficiary

weighting and practice weighting. Sections VI.4 and VI.5 of this appendix describe clustering and weighting in detail.

In a previous report, we described the methodology we used to obtain an average effect of the demonstration over Years 1 to 5—a separate difference-in-differences model using a single demonstration indicator (instead of separate indicators for each demonstration year).²² In that regression, we included data in each of the seven pre- and post-demonstration years for all practices that participated in Year 5. We report results from this regression in Appendix B using the sample of 14 practices that participated in Years 1 to 5. Estimating an average annual effect over Years 1 to 6, however, would have required combining estimates across models with different samples and interpretations because we estimated the effect in Year 6 using a model that included data in all years from only the 12 practices that participated in Year 6. We could have estimated the average annual effect over Years 1 to 6 using only these 12 practices, but it would not have represented the correct estimated impact from Years 1 to 5 when there were 14 practices in the demonstration.

2. Model specification for mortality

We used survival modeling techniques to estimate whether the demonstration had an effect on the probability of a beneficiary dying within the demonstration year. The advantage of this approach relative to a logistic regression model is that it allowed us to use a flexible functional form to account for some beneficiaries becoming eligible after the beginning of the demonstration year and thus having shorter periods of observation relative to other beneficiaries. We used the accelerated failure time hazard specification to estimate a survival-time model in Equation (2) as follows:

$$(2) \log(T_{it}) = X_{it}\beta + \tau \cdot treatment_{it} + \gamma_{-1} PD_1 + \gamma_1 DY_1 + \gamma_2 DY_2 + \gamma_3 DY_3 + \gamma_4 DY_4 + \gamma_5 DY_5 + \gamma_6 DY_6 + \theta_{-1} treatment_{it} \cdot PD_1 + \theta_1 treatment_{it} \cdot DY_1 + \theta_2 treatment_{it} \cdot DY_2 + \theta_3 treatment_{it} \cdot DY_3 + \theta_4 treatment_{it} \cdot DY_4 + \theta_5 treatment_{it} \cdot DY_5 + \theta_6 treatment_{it} \cdot DY_6 + z_{it}$$

where T_{it} denotes the number of days that beneficiary i survived in demonstration year t subsequent to that individual's eligibility date in that year; X_{it} includes the same set of beneficiary characteristics measured in the index year as in Equation (1); PD_1 is an indicator for two years before the demonstration; $DY_1 - DY_6$ are a set of indicators for each post-demonstration year; and $treatment_{it}$ is an indicator variable for being in an IAH practice. The term z_{it} is an error term with a distribution $f(\cdot)$.

The model in Equation (2) accounts for the exact survival time not being observed for beneficiaries who did not die at the end of a given demonstration year (that is, right censoring) and the survival time not being measured from the beginning of the demonstration year for beneficiaries who entered the study sample late (that is, left truncation). We estimated the model using the maximum likelihood method with a generalized gamma distribution for $f(\cdot)$ to allow for the possibility of non-monotonic hazard functions.²³ We used

²² For more information, refer to the [evaluation report](#) which covered Years 1 to 5 of the IAH demonstration.

²³ To inform our choice of the survival function, we compared the goodness of fit of models using different distributions. We considered five types of parametric survival distributions: (1) Weibull, (2) log logistic, (3) log normal, (4) generalized gamma, and (5) Gompertz. In choosing the final model, we analyzed the log likelihood, the Akaike information criterion, and the Bayesian information criterion across these different models, selecting the distribution that consistently produced the smallest value on these metrics.

matching weights to account for the number of matched comparisons per IAH beneficiary so that the two groups were the same size. We adjusted standard errors using the hybrid clustering approach, which we describe in detail below.

After estimating the survival regression, we transformed $\theta_1 - \theta_6$ into difference-in-differences effect estimates, following steps similar to those we used for estimating impacts for other outcomes. Specifically, we obtained the regression-adjusted average mortality (that is, one minus the probability of survival by the end of the demonstration year) for IAH and comparison groups in each year. We adjusted the yearly average outcomes for both groups to reflect the covariate distribution of the IAH group in Year 6. Finally, we estimated the difference-in-differences impact by calculating the difference of the regression-adjusted mortality for the IAH group and matched comparison groups in that year relative to the difference between the two groups in the year before the demonstration.

As part of the outputs from the survival regression, we obtained the predicted mortality for each beneficiary during a given demonstration year based on the individual's treatment status and baseline characteristics.

This predicted mortality, denoted as h_{it} , then fed into the estimation model for other binary outcomes, which we describe next.

3. Model specification for other binary outcomes

In addition to mortality, we estimated the impact of the demonstration on two other binary outcomes: the probability of having an unplanned readmission and the probability of entering institutional long-term care within the demonstration year. Our model specification for these outcomes was similar to that for continuous and count outcomes, but we used additional controls to account for differences between IAH and comparison group beneficiaries in the length of time they were exposed to the risk of the outcome (as a proportion of days eligible for the demonstration in a given demonstration year).

We measured outcomes for the period that beneficiaries were eligible during a given demonstration year, which started from the date of eligibility through the end of the demonstration year or date of death. Therefore, the eligibility period differed across beneficiaries, depending on their eligibility start dates and death dates. In particular, death occurred less frequently in each demonstration year for the IAH group than for the matched comparison group (for example, 16.2 percent of IAH beneficiaries died during Year 6 compared with 19.1 percent of matched comparison beneficiaries).²⁴ Such a difference implied the importance of controlling for observation length because, all else being equal, IAH beneficiaries spent more time during the demonstration year at risk for the outcome than did the matched comparison beneficiaries. Further, mortality might directly affect the probability of readmission (or entry into long-term care) if the probability changes as people approach death. Thus, not controlling for death could bias the estimated effect of the demonstration.

For continuous and count outcomes, we accounted for differential observation lengths by annualizing the outcome and using eligibility weights in regressions. Because we could not annualize binary outcomes, however, we employed a modeling approach similar to the one used in Deb (2016). The basic idea behind Deb's model is to first estimate a survival model to derive the predicted probability of dying for each

²⁴ Ideally, the matching process would result in a comparison group with the same expected survival (as of the eligibility date) as the IAH beneficiaries. But it is possible that factors not observable in claims data caused a differential expected survival between the two groups. To the extent that these differences are changing over time in a way that we cannot control for in claims data, the differential trend could lead to bias in our impact estimates.

individual in each time period and then include the predicted probability of dying in the second stage to account for the differences in outcomes because of the differences in mortality rate across individuals.

Following Deb's approach, we estimated a survival-adjusted difference-in-differences model, controlling for the predicted probability of dying within the demonstration year, (h_{it}), the interaction between treatment status and the probability of dying, and the proportion of time during the demonstration year that the beneficiary was eligible and alive ($survdays_{it}$).

Equation (3) shows our model specification:

$$(3) \quad P(Y_{it} = 1) = \alpha + X_{it}\beta + \tau.treatment_{it} + \beta_h h_{it} + \beta_{Rh} treatment_{it} h_{it} + \beta_s survdays_{it} + \gamma_{-1} PD_1 + \gamma_1 DY_1 + \gamma_2 DY_2 + \gamma_3 DY_3 + \gamma_4 DY_4 + \gamma_5 DY_5 + \gamma_6 DY_6 + \theta_{-1} treatment_{it} \cdot PD_1 + \theta_1 treatment_{it} \cdot DY_1 + \theta_2 treatment_{it} \cdot DY_2 + \theta_3 treatment_{it} \cdot DY_3 + \theta_4 treatment_{it} \cdot DY_4 + \theta_5 treatment_{it} \cdot DY_5 + \theta_6 treatment_{it} \cdot DY_6 + \omega_{it}$$

where Y_{it} is a binary variable for whether the beneficiary had an unplanned readmission (or long-term care entry); h_{it} is the predicted probability of dying in the demonstration year, derived from the estimated survival model in Equation (2); $survdays_{it}$ is the number of days from the beneficiary's eligibility date through the end of demonstration year or date of death, divided by 365 (or 366 for a leap year); and ω_{it} is a random error term. The remaining covariates are the same as those in Equation (1).

In Equation (3), the term h_{it} measures the predicted probability of beneficiaries' dying in the year regardless of their actual survival or censoring status. Because h_{it} was derived from the difference-in-differences survival model, it accounted for any mortality difference between the IAH and comparison groups that was not captured in matching as well as any mortality difference resulting from the demonstration. Coefficient β_h captured changes in the probability of readmission (long-term care entry) as the mortality rate increases, and coefficient β_{Rh} captured differential changes in this probability for those in the IAH group versus the comparison group. Last, coefficient β_s captured the effect of the length ω_{it} of time at risk of readmission (long-term care entry) conditional on predicted mortality.

We estimated Equation (3) using a logistic regression model.²⁵ As with mortality, we adjusted standard errors for hybrid clustering and used matching weights to ensure equal sizes of IAH and comparison groups. Because estimation of Equation (3) involves a generated regressor h_{it} , we bootstrapped our estimates and standard errors, employing a multiple-imputation approach (Deb 2016). After bootstrapping, we transformed $\theta_1 - \theta_6$ into difference-in-differences effect estimates, following steps similar to those we used in estimating impacts for other outcomes. For each outcome, we also estimated a separate difference-in-differences model

²⁵ As we noted, we calculated the long-term care measure through Year 5 because data were unavailable for Year 6.

that used a post-demonstration indicator and its interaction with the IAH status to obtain the five-year annual effect estimate.

4. Adjustment to standard errors for clustering

To obtain accurate estimates of standard errors for the impact estimates, it was important to account for possible clustering of observations within geographic areas. CMS selected certain practices to implement IAH, each of which serves beneficiaries in a specific area. We selected patients from the same geographic catchment area for the matched comparison group. The IAH group sample was clustered by practice in that geographic area—all beneficiaries who met the eligibility criteria and received home-based primary care from the same demonstration practice. We could not model practice-level clustering of the comparison group, however, because we selected those beneficiaries without knowledge of the practice from which they received their primary care. We accounted for this asymmetric clustering structure of the two groups in our regression to avoid overstating the precision of the estimates.

In addition to the practice-level clustering, we had multiple observations for some beneficiaries in the sample. Because the observations on a given beneficiary in one period clearly were not independent of the observations on the same beneficiary in other periods, our estimator of the variance had to account for this time dependence of repeated observations.

To account for asymmetric practice-level clustering and multiple observations for some beneficiaries, we used what we refer to as a hybrid clustering approach. This approach accounted for clustering at the practice level for the IAH group only and took into account the time dependence of repeated observations for IAH and comparison beneficiaries.²⁶ Implementing this approach meant that all IAH beneficiaries in a given site were from a single cluster. Because the entire practice was selected to provide IAH in the given area, we have to account for this clustering effect to avoid overstating the precision of the estimates (that is, to avoid standard errors that are too small, giving a false sense of confidence about the effect of the demonstration). To correctly identify the clustering effect in the IAH group, we did not include site fixed effects in the regression equation.²⁷

Our approach to adjusting standard errors was consistent with the goal of evaluating only the practices that participated in the demonstration in this report. We could not generalize beyond the demonstration practices to home-based primary care provided across the nation as a whole because demonstration practices were not a random sample of all practices, and we did not know the extent to which IAH sites were similar to other practices and the types of patients they serve. Instead, we assumed that the IAH beneficiaries in a given practice were a random sample of all eligible beneficiaries of that practice. For this reason, our statistical

²⁶ Accounting for clustering at the practice level for the treatment group captures the correlation among observations in each IAH practice, whether for the same person across time periods or different people in the same time period. We implemented the hybrid clustering approach in the statistical software used for the analysis (Stata) by defining a cluster variable that takes the value of the practice ID for the treatment group and the value of the beneficiary ID for the comparison group.

²⁷ Ideally, including site fixed effects would improve estimation by controlling for factors that varied across geographic areas and affected outcomes for IAH and comparison beneficiaries within a given area. But because all IAH beneficiaries in a given site (stratum) were from a single practice (cluster), controlling for stratification and clustering at the same level would lead to under-identification. That is, we could not identify the clustering effect with only one IAH group practice per site in a stratified design (Schochet 2008). Relative to the site fixed effects, clustering was by far the more important factor to account for when estimating the variance of the estimate. If we failed to account for clustering when estimating variance, the standard errors and statistical significance of the estimates would be misleading and could lead to incorrect conclusions about the impact of the demonstration. To avoid that problem, we could not take advantage of the gains we would have achieved by accounting for the stratified approach.

tests accounted for the random variation among eligible beneficiaries who received care from the demonstration sites.

5. Weighting

For continuous and count outcomes, we estimated regressions with observations at the beneficiary level and weighted the observations to capture two factors: (1) the share of months a given beneficiary was eligible for the demonstration during each pre-demonstration or demonstration year and (2) the number of comparison beneficiaries matched to each treatment beneficiary. We referred to the former as the eligibility weight; it controlled for differences in the length of time that beneficiaries were observed during a given study year. We referred to the latter as the matching weight. Because we matched each treatment beneficiary to up to five comparison beneficiaries, applying matching weights ensured that the impact regression was not disproportionately weighted toward IAH beneficiaries who had more matched comparison beneficiaries (such as five versus two).

The construction of final beneficiary weights for continuous and count outcomes required three steps. First, we constructed the eligibility weight as the share of months eligible for the demonstration during each pre-demonstration or demonstration year. After we determined a beneficiary's eligibility for the demonstration in a given pre-demonstration or demonstration year, we included the beneficiary in the analysis sample beginning on the first day of the following month. The beneficiary remained in our analysis sample for the entire year unless that beneficiary left Medicare FFS or died. For example, if a beneficiary entered the Year 6 sample on January 1, 2019, and died on June 20, 2019, that person was eligible for the demonstration for six months and thus had an eligibility weight of 0.5.

Second, we constructed matching weights to account for the size of the matched set. Each IAH beneficiary received a weight of 1, and each matched comparison beneficiary received a weight that was the inverse of the number of comparison beneficiaries within the matched set. For example, if an IAH beneficiary was matched to four comparison beneficiaries, each of the latter received a weight of 0.25. Comparison beneficiaries' matching weights ranged from 0.2 (if there were five matched comparisons for a particular IAH beneficiary) to 1 (one matched comparison). For all outcomes other than mortality, we obtained a composite weight by multiplying the eligibility weight by the matching weight.

In the third step, we created the final analytic weight for each beneficiary by rescaling the composite weight to ensure equality in the weighted number of IAH and comparison beneficiaries for each site and year.²⁸ As we described, we implemented hybrid clustering adjustments but could not use site fixed effects (an indicator for each site). Because beneficiaries had different eligibility weights, the number of weighted IAH beneficiaries in a given site and year might differ from the number of weighted comparison beneficiaries in the same site and year if we used the composite weight without rescaling it. For this reason, we rescaled the weights for comparison beneficiaries by site and year so that for each year, the weighted number of IAH beneficiaries equaled the weighted number of comparison group beneficiaries for each site. This approach ensured that the estimated treatment–comparison differences and the difference-in-differences estimates for each year accounted for any differential weighting of the IAH and comparison groups.

For binary outcomes, we used matching weights only. We did not include an eligibility weight in the mortality regression because the survival model we employed accounts for differential observation lengths

²⁸ For more information, refer to the [evaluation report](#) which covered Years 1 to 4 of the IAH demonstration.

for the outcome via a hazard function.²⁹ For other binary outcomes (probability of unplanned readmission and entering institutional long-term care), we used a survival-adjusted model for binary outcomes, which explicitly accounts for the effects of mortality and time survived since eligibility. We describe the model specifications for these binary outcomes earlier in this section.

We refer to the above weighting scheme as beneficiary weighting. Under beneficiary weighting, large practices that served more beneficiaries had more influence on the estimated effect and smaller practices had less influence. We also report estimation results based on an alternative weighting scheme that allows all practices to have equal influence on the estimated effect regardless of the size of their patient population. This method, which we refer to as the practice-weight method, yields an estimate that reflects the average effect of changes that practices made in response to the payment incentive. Under this approach, each practice has equal influence on the results.

An example may help explain the difference between the two methods. Let us assume that the demonstration had 4 large practices of 1,000 patients each that did not change care delivery in response to the demonstration and 12 practices of 250 patients each that changed care delivery. The practice-weight method would give equal influence to all practices, and the estimate would reflect that most practices (12 out of 16) changed care delivery. The beneficiary-weight method, on the other hand, would give more influence to the 4 practices that served most of the beneficiaries (4,000 out of 7,000), and the estimate would reflect that most of the beneficiaries were treated by practices that did not change care delivery.

As the example illustrates, when the effect of the demonstration differs across IAH practices, the beneficiary-weight method would lead to an estimated effect that tends to resemble those of the largest practices, thus masking the effect on smaller practices. The beneficiary-weight estimate reported in Chapter II can be informative for policymaking if the largest IAH practices are a representative sample of the largest home-based primary care providers in the nation. But although the IAH practices were selected to include diverse approaches to providing home-based primary care, they do not represent all practices in the nation that provide home-based primary care. Therefore, the practice-weight estimate provides an important understanding of the average effect of the demonstration across a variety of delivery models for home-based primary care. Considering beneficiary- and practice-weight methods together is important to our understanding of the effect of the payment incentive on outcomes.

To implement the practice-weight method, we reweighted the final analytic weights for all IAH and matched comparison beneficiaries using a ratio that varied by site and year so that the summed weights among all beneficiaries in each practice were equal across all practices in each year (treating the three members of the Mid-Atlantic Consortium as separate sites). We then reestimated the effect on total Medicare expenditures as specified in Equation (1) using these new weights.

6. Control variables

Although our matching process ensured that the comparison groups were very similar to the IAH groups along many characteristics, there may still be important differences in some of these characteristics that could affect the outcomes. Therefore, we included four types of control variables: (1) variables describing eligibility for the demonstration; (2) demographic characteristics; (3) ADL indicators; and (4) measures of health status, including HCC risk score, HCC indicators, and chronic condition indicators (Exhibit A.14). We

²⁹ The weights used for the survival regression did not have to be rescaled because, without any eligibility weights, the matching weights ensured that the weighted number of IAH and comparison beneficiaries for each site and year were equal to each other.

included all HCC indicators and categories of HCCs used for matching (Exhibit A.8), but some of the other control variables were included in our regressions at a more detailed level than the variables we used in matching (to help ensure balance during matching). For example, we used three age categories in propensity score matching, whereas we used five age categories in the outcome regressions.

As we noted, we included a dummy variable for each year and an indicator of whether the beneficiary was in the IAH or comparison group. Because of the repeated cross-sections in our multiyear data set, we used contemporaneous control variables for all years of the demonstration; for example, in demonstration Year 6, we used the Year 6 values of all control variables whether or not a beneficiary appeared in the sample in an earlier demonstration year.

Exhibit A.14. Control variables used in regressions

Variable
Eligibility for the demonstration
Number of months since most recent hospital admission: 1, 2 or 3, 4 or more
Month of the demonstration year that beneficiary met eligibility criteria (1, 2 to 6, 7 to 12) ^a
Demographic characteristics
Age: younger than 65, 65 to 74, 75 to 79, 80 to 84, 85 or older
Gender
Race and ethnicity: White, Black, Hispanic, Asian, American Indian or Alaska Native, other, or unknown
Dually eligible for Medicare and Medicaid
Original reason for Medicare entitlement: old age, ESRD or ESRD and disability, disability only
ADLs
Number of ADLs for which beneficiary requires human assistance: 2, 3 or 4, 5 or 6
Whether information about the feeding ADL was missing ^b
Health status^c
HCC risk score
Specific HCCs
HCC 8, metastatic cancer and acute leukemia
HCC 9–10, lung and other severe cancers; lymphoma and other cancers
HCC 11–12, colorectal, bladder, and other cancers; breast, prostate, and other cancers and tumors
HCC 18, diabetes with chronic complications
HCC 21, protein-calorie malnutrition
HCC 27, end-stage liver disease
HCC 28–29, cirrhosis of liver; chronic hepatitis
HCC 46, severe hematological disorders
HCC 48, coagulation defects and other specified hematological disorders
HCC 51, dementia with complications
HCC 52, dementia without complications
HCC 54–55, drug/alcohol psychosis; drug/alcohol dependence
HCC 57–58, schizophrenia; major depressive, bipolar, and paranoid disorders
HCC 70–71, quadriplegia; paraplegia
HCC 72, spinal cord disorders/injuries
HCC 85, congestive heart failure
HCC 96, specified heart arrhythmias
HCC 103–104, hemiplegia/hemiparesis; monoplegia, other paralytic syndromes
HCC 106, atherosclerosis of the extremities with ulceration or gangrene
HCC 107–108, vascular disease with complications; vascular disease
HCC 111, chronic obstructive pulmonary disease
HCC 134, dialysis status
HCC 136–138, chronic kidney disease, stages 3–5
HCC 139–140, chronic kidney disease, stages 1–2 or unspecified; unspecified renal failure
HCC 157–159, pressure ulcer of skin with necrosis through to muscle, tendon, or bone; or with full or partial thickness skin loss

Variable

Chronic conditions measured by Chronic Conditions Data Warehouse

- Alzheimer’s disease or related disorders
- Acute myocardial infarction or ischemic heart disease
- Asthma
- Hip or pelvic fracture
- Stroke or transient ischemic attack
- Number of chronic conditions and the square of the number of conditions

Other measures of health status

- Anemia^d
- Fluid and electrolyte disorders^d
- Chronically critically ill or medically complex diagnosis

^a For all sites in Year 6, Month 1 was January. For all sites in Years 4 and 5, Month 1 was October. In Years 1 to 3, sites began the demonstration in June or September each year. For sites that began in June, Month 1 is June. For sites that began in September, Month 1 was September.

^b Feeding assessments were not available with home health assessment data at the time of recertification. If the beneficiary had a previous assessment during the study year that was recorded at the time of discharge from home health care, we used the feeding values from that assessment. Sometimes, however, there was no previous discharge assessment.

^c For binary outcomes (mortality, entry into institutional long-term care and readmission), we estimated a survival-adjusted difference-in-differences model which controlled for additional variables: the predicted probability of dying within the demonstration year, the interaction between treatment status and the probability of dying, and the proportion of time during the demonstration year that the beneficiary was eligible and alive.

^d Measured using claims from the most recent inpatient stay and observation stay in the year before the demonstration eligibility date. We drew diagnosis codes for these conditions from Gagne et al. (2011).

ADL = activity of daily living; ESRD = end-stage renal disease; HCC = Hierarchical Condition Category.

7. Testing the parallel trends assumption

The validity of the difference-in-differences estimates as impact estimates of the demonstration rely on the classic difference-in-differences parallel trends assumption that there was no significant differential trend between the IAH and matched comparison groups during the pre-demonstration period. That is, outcomes should change at the same rate for both groups in the two-year pre-demonstration period, so any difference in outcomes between the two groups would remain the same during that period. Therefore, the difference-in-differences estimate for two years before the demonstration, θ_{-1} , served two purposes: (1) it ruled out or identified treatment-comparison differences in trends during the pre-demonstration period and (2) in so doing, it helped inform the more important difference-in-differences analysis for the demonstration period. Specifically, a statistically significant θ_{-1} would indicate that the difference in a given outcome between the IAH and comparison groups changed significantly from two years before the demonstration to the year before the demonstration. In this case, the IAH and comparison groups could have been on nonparallel outcome trajectories during the pre-demonstration period. We referred to nonparallel outcome trajectories during the pre-demonstration period as a pre-existing difference in trend.

The possible presence of nonparallel pre-demonstration trends would have limited our confidence in the demonstration impact estimates for a given outcome. This was because the difference-in-differences estimates for the demonstration years could have reflected the continuation of a pattern (for example,

narrowing or widening differences between the two groups) that began during the pre-demonstration period rather than reflecting an impact of the demonstration payment incentive.

We examined the difference-in-differences estimate for two years before the demonstration for all outcomes reported. The estimate was not statistically significant and small as a percentage for most outcomes, including total expenditures and hospital care use, suggesting that the parallel-trend assumption held for those outcomes between those two years. We found statistically significant difference-in-differences estimates for two years before the demonstration for hospice expenditures in the Year 6 sample and mortality in the Years 1 to 5 and Year 6 samples, which violated the parallel-trend assumption. In both cases, the estimated trend in the pre-demonstration period was relatively large compared with the pre-demonstration IAH group mean outcome. Because it was impossible to rule out the possibility of truly nonparallel pre-existing trends for these outcomes in which the difference-in-differences estimate for two years before the demonstration was different from zero, we were cautious in interpreting the impact estimates for hospice category expenditures in Year 6 and mortality.³⁰

8. Assessing the relative influence of individual practices

As we noted, under beneficiary weighting, practices have different amounts of influence on the estimated effect depending on their sizes. To understand which practices drove the estimated effects of the demonstration, we reestimated the beneficiary-weight regression, leaving out one practice at a time.³¹ Specifically, we estimated 14 regressions (treating each member of the Richmond-based consortium separately), with each regression excluding the IAH beneficiaries from one practice and their matched comparisons in all years. If all 14 regressions showed similar estimates for Year 6 as the main regression, we would conclude that all practices equally influenced the full sample estimate. On the other hand, if excluding a given practice substantially changed the estimated effect, we would conclude that the site strongly influenced the full sample estimate. In Appendix B, we report the Year 6 estimated effects from each of the 14 regressions for selected outcomes.

C. Bayesian difference-in-differences model

1. Overview

In addition to the frequentist (traditional) analyses we described in Chapter VI.B, we conducted a set of analyses using the Bayesian statistical paradigm. Assessing the effects of IAH probabilistically, as Bayesian techniques permit, maintains a rigorous statistical standard and provides a more flexible interpretation of the program's effects. The frequentist approach classifies the demonstration's impact as statistically significant or not statistically significant; in contrast, a Bayesian analysis allows probabilistic estimates about whether the demonstration achieved a certain outcome. For example, one could conclude that "there was a 45 percent chance that the IAH demonstration incentive reduced expenditures by at least \$41 PBPM in demonstration Year 6." Such conclusions offer the opportunity to tailor inferences to substantive questions of interest and apply subject matter expertise in deeming meaningful effects.

³⁰ It is possible to control for pre-existing trends by including linear time trends in the regression, but this approach would impose an overly restrictive assumption on our model: that the one-year pre-demonstration trends would continue throughout the demonstration.

³¹ We could have estimated regressions separately for each site to obtain site-specific estimated effects, but the statistical power for some of these regressions was too low because of the small sample sizes at the site level. Estimating the regressions and excluding one site at a time enabled us to assess the influence of each site by comparing those estimates with the estimate from the full sample.

Overall, the Bayesian and frequentist analyses were similar, but they had some differences. As with the frequentist approach, the Bayesian analysis used a comparison group difference-in-differences design to identify effects attributable to the IAH demonstration. The outcome of interest was total Medicare expenditures PBPM. We used the same data sets for the frequentist and Bayesian analyses. Moreover, we used the same eligibility and matching weights and the same control variables. The Bayesian analysis diverged from the frequentist analysis, however, in three ways, as we describe here. In this chapter, we describe the three factors that differentiated the Bayesian analyses from their frequentist counterparts: the prior distributions, the method used to account for clustering, and the computational approach used to fit the models.

a. Prior distribution

Assigning a prior distribution to each model parameter translated the model into the Bayesian framework and allowed for probabilistic inference. We placed a standard normal prior distribution—denoted $N(0,1)$ —on the overall impact of IAH. By doing so, we incorporated a prior expectation that very large positive or negative impacts of IAH on expenditures were substantially less likely than small and moderate impacts. We based our prior expectation on the general result that other interventions of the impact of home-based primary care and other interventions for chronically ill, frail beneficiaries very rarely show effect sizes larger than two standard deviations. We centered the normal distribution at a mean of zero to remain agnostic about whether the IAH demonstration would be successful.

b. Method used to account for clustering

The full Bayesian model accounted for clustering by using random effects, and the frequentist analysis used cluster-robust standard errors (as we described earlier in this appendix). Specifically, the two-stage full Bayesian model accounted for clustering using beneficiary- and site-specific random effects for the IAH and comparison groups, in which each site included IAH beneficiaries from a demonstration practice and their matched comparison beneficiaries. In contrast, the frequentist analysis estimated cluster-robust standard errors, which assumed that IAH beneficiaries were clustered by practices and comparison beneficiaries were clustered by individual beneficiaries rather than practices (a hybrid clustering approach). The Bayesian model could not apply the same approach because it accounted for clustering using random effects instead of cluster-robust standard errors.³² This methodological difference in accounting for clustering could lead to differences in point estimate and standard error of the estimate.

c. Two-stage model

We further modified the frequentist model to make Bayesian computationally feasible. We adopted these modifications purely as a computational convenience; they are not inherently Bayesian, and a traditional impact estimation framework could also adopt this approach. Ideally, we would have liked to fit a single, unified model at the beneficiary level, as in the frequentist analysis, but such a model would have taken a prohibitively long time to converge on our analysis platform. Because of time constraints, we used a two-stage approximation of this ideal beneficiary-level model. In the first stage, we aggregated the beneficiary-level data set to the site level. Using output from Stage 1, we estimated the impact of the IAH demonstration using a Bayesian difference-in-differences framework in Stage 2.

³² A Bayesian model requires a fully model-based approach to account for clustering, and cluster-robust standard errors are an adjustment performed after the modeling process.

2. Full Bayesian model, pooled

To understand the full Bayesian model, we begin by presenting a single unified model at the beneficiary level. As we show in Equation (4), this procedure accomplishes impact estimation and risk adjustment simultaneously through a model of the following form:

$$(4) Y_{ijt} = \alpha + X_{it}\beta + \tau z_{it} + \gamma_t + \theta_i z_{it} + a_i + b_j + c_j z_{it} + d_{jzt} + \varepsilon_{it}$$

This model uses a slightly different notation than its frequentist counterpart, Equation (1), for clarity of presentation of the random effects.

- We use i to index beneficiaries; $j = 1, \dots, 14$ to index geographic areas (or, loosely speaking, sites at which both IAH and comparison beneficiaries resided in Year 6); and $t = -1, \dots, 6$ to index years.
- Y_{ijt} is total Medicare expenditures PBPM measured for beneficiary i from site j in year t ; X_{it} is a set of beneficiary characteristics measured in year t ; z_{it} is the treatment status of beneficiary i in year t .
- Greek letters denote parameters to be estimated: α is a constant term; β contains the effects of the beneficiary characteristics; τ captures any differences between IAH and comparison beneficiaries in the year before the demonstration that persist despite matching; γ describes the secular time trend that applies to both IAH and comparison beneficiaries; and the θ s are the difference-in-differences impacts of interest. As with the frequentist model, we estimated γ_{-1} and θ_{-1} for two years before the demonstration, and $\gamma_1 - \gamma_6$ and $\theta_1 - \theta_6$ for each of the six demonstration years. Note that $t = 0$ corresponds to the baseline year (the year before the demonstration), so we omitted γ_0 and θ_0 from the model.
- Random effects are denoted by Roman letters: the a 's and b 's are beneficiary- and site-level random intercepts, respectively, which account for the correlation across repeated observations on a given beneficiary or site; the c 's are site-specific baseline IAH/comparison differences; and the d 's are site-treat-year random intercepts. We assume that the a 's and d 's each follow a univariate normal distribution, and the b 's and c 's jointly follow a bivariate normal distribution. The latter assumption allowed for correlation between a site's intercept and the IAH/comparison difference in that site.

Last, we weighted the regression using the same weighting scheme (beneficiary weighting) that we used in the frequentist analysis, as we discussed earlier.

We calculated the adjusted total Medicare expenditures for the IAH and matched comparison groups in each year, the difference-in-differences estimates (θ_{-1} , $\theta_1 - \theta_6$), and percentage impact relative to unadjusted IAH gap mean expenditures in the year before the demonstration. In addition, we estimated the probability of reducing expenditures by at least \$41 (which corresponds to the estimated effect of the payment incentive in Year 6 from the frequentist model) and \$100 PBPM. In all calculations, we adjusted the yearly average outcomes for both groups to reflect the covariate distribution of the IAH group in the latest (sixth) demonstration year—the same approach we used in the frequentist analysis.

Because of the number of observations in the data set, fitting Equation (4) as a single, unified model at the beneficiary level was computationally prohibitive. For this reason, we fitted the full Bayesian model using a

two-stage approximation to decrease computational run times. The first-stage model was a beneficiary-year-level risk adjustment fit using hierarchical linear regression. The goals of the first-stage analysis were to aggregate beneficiaries to the site level and risk-adjust outcomes to enable comparisons across sites and years whose case mix differed (Equation [5]). In the first-stage model, we adjusted for the same beneficiary-level covariates as the frequentist model (see Exhibit A.14). We used the risk-adjusted site-year-level output from Stage 1 as data in Stage 2, which estimated the impact of IAH demonstration in a Bayesian difference-in-differences framework (Equation [6]).

$$(5) \text{ Stage 1: } Y_{ijt} = A_{jzt} + X_{it}\beta + a_i + \varepsilon_{it}$$

As we described, the site-treatment-year effect A_{jzt} represents the estimated fixed effect for site j and treatment group Z in year t . There were 224 such fixed effects from two groups (IAH and comparison) from each of the 14 sites in each year (counting each member of the consortium as a separate site). The parameters β describe the effects of beneficiary-level control variables X_{it} , whereas beneficiary-level random effects a_i account for correlations across repeated observations on beneficiary i . We assumed that the beneficiary-level random effects a_i and the overall error term ε_{it} came from a normal distribution with mean zero and its own variance. Similar to the frequentist model, we used the rescaled composite weights for the Stage 1 model. Then, we used the aggregated site-treatment-year estimates (\hat{A}_{jzt}) and associated standard errors (S_{zjt}) from the Stage 1 model when we estimated the Stage 2 full Bayesian difference-in-differences regression (Equation [6]).

$$(6) \text{ Stage 2: } \hat{A}_{jzt} = \alpha + \tau z + \gamma_t + \theta_t z + b_j + c_j z + d_{jzt} + \varepsilon_{jzt}$$

In the Stage 2 model, we included an overall intercept α and controls for the secular time trend γ_t and treatment τ . We accounted for clustering through random effects b_j , c_j , and d_{jzt} , as we described earlier. The parameters of interest, θ_t , represent the overall difference-in-differences terms.

We assigned a standard normal distribution—Normal(0, 1)—as the prior for each model parameter:

$\alpha \sim N(0, 1)$, $\tau \sim N(0, 1)$, $\gamma \sim N(0, 1)$, $\theta \sim N(0, 1)$, $(b_j, c_j) \sim MVN(0, \Sigma)$, $d \sim N(0, \sigma^2)$ where σ^2 is the overall noise variance. The prior for Σ included two parts: one part to address correlations between b_j and c_j , and one to address the standard deviation of b_j and c_j . The former part took on an LKJ correlation prior (Lewandowski et al. 2009); the latter took on a standard normal distribution. The multiplication of these two parts constituted the prior on $\Sigma : \Sigma = \begin{pmatrix} \sigma_b & 0 \\ 0 & \sigma_c \end{pmatrix} \Omega \begin{pmatrix} \sigma_b & 0 \\ 0 & \sigma_c \end{pmatrix}$ where $\sigma_b, \sigma_c \sim N(0, 1)$ and $\Omega \sim \text{LKJ}(2)$.³³ Last, our prior on the error term is given by

³³ LKJ is a distribution on correlation matrices (usually called Ω). The distribution has one parameter, ν , so

$\varepsilon_{jzt} \sim \text{Normal}(0, s_{jzt}^2)$. Therefore, both σ^2 and s_{jzt}^2 act as weights in Stage 2. We used the “lme4” package in R to fit the Stage 1 model. For Stage 2, we used a novel probabilistic programming language called Stan, which provides fast, full Bayesian inference, even for complex models.

VII. Accounting for other programs and payments

Other programs administrated by CMS that take place concurrently with the IAH demonstration could influence our estimates of the effect of the IAH payment incentive on total Medicare expenditures. We addressed the potential influence of two CMS programs that providers for IAH or comparison beneficiaries may have participated in during 2019: the Quality Payment Program and ACOs.

A. Excluding Quality Payment Program payment adjustments in estimating the impact of the IAH payment incentive

1. Background

As required by law, in 2017, CMS implemented the Quality Payment Program, which aims to incentivize clinicians to provide high-value care. Clinicians can participate in two tracks in the model: the merit-based incentive payment system (MIPS) and alternative payment models (APMs). CMS pays clinicians for performance, participation, or both in these programs. Though performance measurement began in 2017, the first year it is relevant to estimating impacts for IAH is 2019 because that is the first year CMS made payments under the Quality Payment Program. This coincides with Year 6 for IAH, which began January 1, 2019.

The MIPS program evaluates qualifying clinicians’ performance across four domains and, based on performance, adjusts payments for professional services covered under the Medicare Physician Fee Schedule. The program is not voluntary; all MIPS-eligible clinician types who meet a threshold for volume of services are subject to a MIPS payment adjustment. The overwhelming majority of adjustments made in 2019 were positive but modest; 93 percent of adjustments were positive (that is, CMS increased reimbursement), and the maximum positive adjustment was 1.9 percent (CMS 2019b). CMS applies MIPS adjustments to payments for clinicians’ professional services claims, and claims-based measures of expenditures include these adjustments by default.³⁴

The APM track of the Quality Payment Program allows clinicians to participate in customized payment models that often seek to engage specific populations or care delivery approaches. Examples include Comprehensive Primary Care Plus, IAH, and Bundled Payments for Care Improvement models. Participation in an APM can offer additional bonus payments and changes the MIPS reporting requirements. CMS distinguishes between two types of APMs: Advanced APMs and MIPS APMs.

- Most Advanced APMs require participants to bear significant financial risk that is determined on an individual basis for each Advanced APM (for example, by setting an expenditure target above which the

$\Omega \sim \text{LKJ}(\mathbf{V})$. When $\mathbf{V} = 1$, the distribution is uniform over all possible correlation matrices. As \mathbf{V} increases, the distribution is more concentrated on the identity matrix, which corresponds to zero correlations. Thus, for $\mathbf{V} = 2$, the distribution slightly favors less correlation, shrinking the correlations somewhat toward zero. This is a weakly informative prior that helps stabilize the estimation.

³⁴ Though adjustments in 2019 were made based on performance in 2017, we account for Medicare expenditures in the year they were made, so we began to count MIPS adjustments in 2019.

Advanced APM is responsible for costs). Clinicians who achieve threshold levels of patients or payments through the Advanced APM are eligible to receive a 5 percent APM incentive payment on their professional services claims and are excluded from MIPS and its reporting requirements. Unlike MIPS adjustments, the 5 percent incentive payments paid to Advanced APM participants are *not* included as part of claims and are paid separately to qualifying clinicians.

- MIPS APMs refer to APMs not designated as Advanced and include clinicians eligible for MIPS. Unlike Advanced APMS, Clinicians participating in a MIPS APM are not eligible for the 5 percent incentive payment on professional services claims and are required to participate in MIPS and its reporting requirements. Clinicians participating in a MIPS APM are still subject to MIPS adjustments but with modified performance category weighting and reporting requirements. For example, in the 2020 performance year, the cost performance domain received 0 weight for MIPS APM participants (compared with 15 percent for non-MIPS APM participants). IAH is a MIPS APM.

2. Payments included when estimating impacts of the IAH demonstration

We included MIPS adjustments on payments made to MIPS-eligible clinicians in 2019 in our primary analysis of total Medicare expenditures. By default, the professional services claims include MIPS adjustments. Including these payments in our measure of total Medicare expenditures more accurately reflected the total amount CMS paid for services in 2019 compared with not including them. We also conducted an additional analysis excluding these payments to see whether they changed the Medicare expenditure outcomes in our sample in a way that influenced our estimate of the effect of the IAH payment incentive.

We did not include Advanced APM payments made to clinicians in 2019 in any analysis of total Medicare expenditures. Because these payments are calculated at the clinician level and are not reported in Medicare claims data, we would have to assign a portion of a clinician's payments to beneficiaries based on assumptions and data analysis. We do not believe this approach would be appropriate for our sample. First, because we do not attribute comparison beneficiaries to specific clinicians (or practices), we do not know which clinicians that received an Advanced APM payment provided care for a given comparison beneficiary. Second, because a clinician who had at least one patient in the comparison group likely had many patients who were *not* included in the comparison group, we do not know the share of that clinician's payment that we ought to assign to a single comparison beneficiary per month.

In general, if Advanced APM payments were relatively large or if the share of IAH beneficiaries seen by clinicians in an Advanced APM was relatively different from the share of comparison beneficiaries seen by such clinicians, then we may have misrepresented total Medicare expenditures and therefore the impact of IAH. These payments, however, probably would not have counted for a substantial component of total Medicare expenditures for beneficiaries in our sample. Advanced APM payments are most likely to make a difference to a measure of total expenditures (1) if the sample includes all FFS beneficiaries treated by a particular practice or clinician and (2) if a fairly large share of total expenditures for those beneficiaries were for services under the Physician Fee Schedule. Neither of these is the case for the IAH evaluation:

- Participation in Advanced APMs is substantially lower than MIPS; fewer than 100,000 clinicians received an Advanced APM payment in 2019. Therefore, any effects of additional payments will likely (although we cannot be certain) be limited to a small share of comparison or IAH beneficiaries, who themselves represent only a small share of an individual practice's total FFS patient population.

- Total expenditures for IAH-eligible beneficiaries—which includes the IAH and comparison groups—are less likely to be substantially affected by Advanced APM bonus payments made to clinicians compared with the general FFS Medicare population. Only relatively high-cost beneficiaries meet IAH eligibility requirements, and among this group, only a small share of total expenditures is for services that appear in clinician/supplier claims. Specifically, clinician/supplier claims accounted for only 17 percent of total expenditures in Year 6 for IAH beneficiaries and 15 percent for comparison beneficiaries, and only a minority of these would have been claims for services provided by clinicians who received an Advanced APM payment). These services represent a larger share of total expenditures for the general FFS Medicare population because the general population tends to have lower expenditures for inpatient, skilled nursing, and other services than the IAH-eligible population.

3. Excluding MIPS payment adjustments

To assess whether MIPS payments affected Medicare expenditures for beneficiaries in our sample in a way that affected the estimated impact of the IAH payment incentive, we repeated our total expenditure regression models after removing MIPS payment adjustments from the expenditures measure. MIPS adjustment amounts appear directly in the clinician/supplier and outpatient claim files. We removed the sequestration amount before subtracting the MIPS adjustment and then reapplied sequestration.³⁵

B. Controlling for ACO participation in estimating the impact of the IAH payment incentive

1. Background

During the IAH demonstration, participation in ACOs increased. ACOs are groups of clinicians, hospitals, and other health care providers held accountable for the quality and cost of their patients' care. If an ACO achieves savings for CMS relative to a target spending level, then the ACO can share in those savings if it meets certain quality measures.

Simultaneous participation in IAH and an ACO could affect estimates of the impact of the IAH payment incentive. For instance, if IAH practices that were in ACOs changed their care delivery because of their ACO involvement, those changes could have affected their patients' Medicare expenditures (perhaps by improvements in care coordination that reduce total Medicare expenditures). ACO participation may also affect other outcomes of interest such as use and quality of care. If there were a significantly different effect of ACO participation on IAH practices relative to the comparison group, the estimated impact of the demonstration would reflect a combined impact of the IAH payment incentive and participation in the ACO.

To assess the influence of ACO participation on the estimated impact of the IAH payment incentive, we categorized beneficiaries as participating or not participating (or more specifically, being attributed to a provider which is or is not participating) in an ACO in each pre-demonstration and demonstration year and controlled for this participation in regression analyses. We measured participation in the ACO using attribution for payment calculations by CMS (which we refer to simply as attribution in this chapter) or additional information gathered directly from the IAH practices, as we describe next.³⁶

³⁵ Because CMS applies sequestration to payments as the last step in generating the total paid amount, to remove MIPS payments, we must first undo sequestration by multiplying claims by 1.02, then remove the MIPS adjustment, and then reapply sequestration to the final amount.

³⁶ Throughout this analysis, we specify two distinct terms related to beneficiary involvement with ACOs. Participation denotes a beneficiary's relationship with an ACO for the purpose of our analysis. Designation of participation is based

2. Assigning ACO participation to IAH and comparison beneficiaries

A key challenge to assessing the influence of ACO participation on the estimated impact of the IAH payment incentive is to identify beneficiaries in our sample reached by ACOs. Attribution to an ACO initiative depends on having qualified claims from ACO providers as measured by Medicare claims that have the National Provider Identifier and Tax Identifier Number (TIN) of a clinician at an organization that participates in the ACO initiative. CMS stores beneficiary-level data on attribution to an ACO initiative as well as participation in other initiatives such as IAH in the CMS Master Data Management (MDM) system, which is a longitudinal database system.

By design, beneficiaries are generally attributed to a single initiative in the MDM at a time (to avoid CMS making multiple payments for the same beneficiary at the same time). Beneficiaries enrolled in IAH are identified as such in the MDM during their period of enrollment. Enrollment in IAH in the MDM takes precedence over attribution in the MDM to any other initiative, including ACO initiatives.³⁷ Beneficiaries in the IAH evaluation sample but not enrolled in the IAH demonstration (discussed in Chapter III of this appendix) are not identified as participating in IAH in the MDM; therefore, these beneficiaries are eligible for attribution to an ACO in the MDM. Because enrollment in IAH in the MDM takes precedence, we supplemented MDM attribution data with qualitative information gathered from the IAH sites on ACO participation. After identifying which IAH practices were part of an ACO, we assigned IAH beneficiaries to an ACO based on the ACO participation of the practice from where they received care in a given demonstration year (Exhibit A.15).

We identified IAH beneficiaries who participated in an ACO in each study year (demonstration Years 1 to 6 and two pre-demonstration years) based on the following logic:³⁸

- We assigned IAH beneficiaries to an ACO if their IAH practice TIN was participating in an ACO at the beginning of the study year.³⁹
- We assigned IAH beneficiaries whose practice TIN did not participate in an ACO at the beginning of the demonstration year to an ACO if they were attributed to an ACO in the beneficiary-level data in the MDM at any point during that study year.
- We assigned comparison beneficiaries to an ACO if they were attributed to an ACO in the beneficiary-level data in the MDM at any point during the study year.

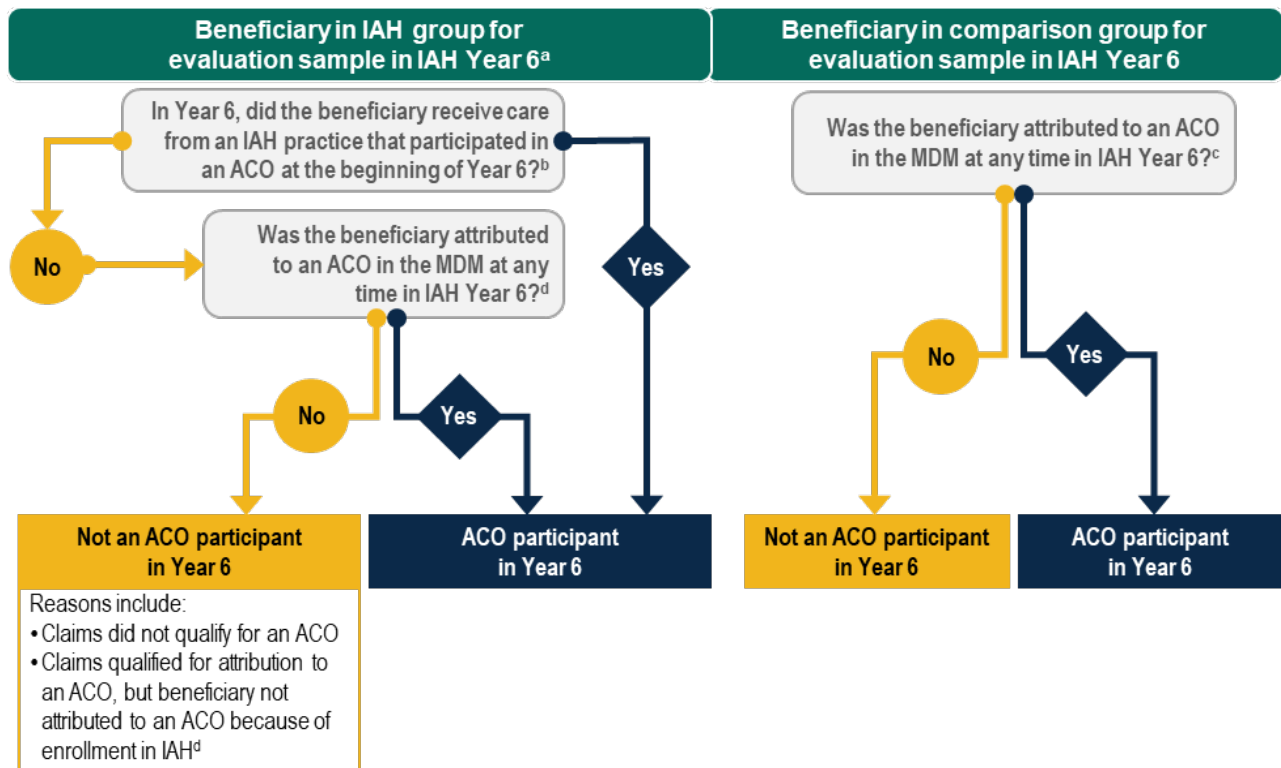
on information from CMS as well as qualitative information collected from sites. Attribution refers to CMS's formal designation related to identifying the population of beneficiaries relevant when calculating payments for ACOs. Beneficiaries are considered attributed to an ACO based entirely on their status in the CMS Master Data Management (MDM) system.

³⁷ It is possible for a beneficiary to be simultaneously enrolled in IAH and attributed to an ACO in the MDM. Our analysis accounts for this possibility and identifies those beneficiaries as participating in an ACO. This situation is, however, rare; the vast majority of beneficiaries enrolled in IAH according to the MDM would not be simultaneously attributed to an ACO because of the rule that IAH takes precedence. Although IAH practices could be part of ACOs and IAH beneficiaries could be treated by providers in these or other ACOs, CMS did not allow IAH beneficiaries to be attributed to an ACO for purposes of calculating ACO savings and payments.

³⁸ IAH enrollment in the MDM does not align perfectly with inclusion in the IAH group we identified for the evaluation. See Chapter III of this appendix for a detailed explanation of the difference between the IAH group used for the evaluation and the list of IAH enrollees identified by the implementation contractor.

³⁹ IAH demonstration years are not the same as calendar years (see Exhibit A.1). For most practices, demonstration Year 1 began in June 2012, Year 2 began in June 2013, and Year 3 began in June 2014. For the practices in Washington, DC; Philadelphia; and Richmond, Years 1 to 3 began in September rather than June. For all IAH practices, Year 4 began in October 2015, Year 5 began in October 2016, and Year 6 began in January 2019.

Exhibit A.15. ACO assignment for IAH and comparison group beneficiaries



Notes: IAH Year 6 was January to December 2019.

^a We identified the IAH evaluation group using Medicare claims and other administrative data, and it included beneficiaries who were enrolled in the demonstration and some who were not enrolled. See Chapter III of this appendix for details on the differences between the IAH enrolled and evaluation samples.

^b We based measurement of ACO participation for IAH practices largely on qualitative information provided by the IAH practices to Mathematica.

^c CMS stores beneficiary-level data on IAH enrollment, attribution to an ACO initiative, and participation in other initiatives in the MDM system.

^d Beneficiaries enrolled in IAH are identified as such in the MDM during their period of enrollment. Enrollment in IAH in the MDM takes precedence over attribution in the MDM to any other initiative, including ACO initiatives. Beneficiaries who are in the IAH evaluation sample but not enrolled in the IAH demonstration are not identified as participating in IAH in the MDM; therefore, these beneficiaries are eligible for attribution to an ACO in the MDM.

ACO = accountable care organization; MDM = Master Data Management system.

3. ACO participation by IAH practices during the demonstration

Although IAH practices were not excluded from participating in ACOs before Year 3, CMS explicitly told practices that they could participate starting in Year 3 (which began in June 2014 for most practices). As a result, several IAH practices joined ACOs in Years 3, 4, and 5 of the IAH demonstration. Exhibit A.16 shows ACO participation for each IAH site at the beginning of each demonstration year according to information provided by IAH practices. No sites took part in an ACO before 2012. Five of the sites never participated in an ACO, and one (Flint) was in an ACO for Years 1 to 5 of the demonstration. Of the remaining 11 sites, most joined an ACO in IAH Year 4, which began in 2015.

The percentage of IAH beneficiaries assigned to an ACO for the purpose of our analysis increased substantially from Year 3 (19.8 percent) to Year 4 (73.7 percent) (Exhibit A.16). Among comparison beneficiaries, participation in an ACO increased steadily over time, up to 48.8 percent in Year 6.

Exhibit A.16. IAH sites' ACO participation at the start of each IAH demonstration year

Demonstration practice location	IAH Year 1 Jun 2012– May 2013	IAH Year 2 Jun 2013– May 2014	IAH Year 3 Jun 2014– May 2015	IAH Year 4 Oct 2015– Sept 2016	IAH Year 5 Oct 2016– Sept 2017	IAH Year 6 Jan 2019–Dec 2019
Practices that participated in Years 1 to 6						
Boston, Massachusetts ^a				✓	✓	✓
Brooklyn, New York				✓	✓	✓
Dallas, Texas ^b				✓	✓	✓
Durham, North Carolina						
Flint, Michigan ^b	✓	✓	✓	✓	✓	✓
Jacksonville, Florida ^b				✓	✓	✓
Lansing, Michigan ^b				✓	✓	✓
Long Island, New York ^{a,c}					✓	✓
Milwaukee, Wisconsin ^b				✓	✓	✓
Philadelphia, Pennsylvania ^{a,d}						
Portland, Oregon						
Richmond, Virginia ^{a,d}						
Washington, DC ^{a,d}				✓	✓	✓
Wilmington, Delaware ^a				✓	✓	✓
Practices that left the demonstration after Year 5^e						
Austin, Texas ^e						n.a.
Cleveland, Ohio ^{a,e}				✓	✓	n.a.

Sources: Information provided by IAH sites to Mathematica, supplemented with records from meetings with CMS, changes in practices' TINs, and web searches.

Note: We do not report data for the first or second pre-demonstration years because none of the IAH practices participated in an ACO in those years (June 2010 to May 2011 and June 2011 to May 2012, respectively). As shown in Exhibit A.17, however, some IAH beneficiaries were assigned to an ACO in the second pre-demonstration year through attribution in the MDM.

^a These practices participated in health systems affiliated with a university or medical school.

^b These practices participated as part of the Visiting Physicians Association.

^c Long Island did not respond to email inquiries in March 2020, but its website indicates that it joined an ACO in 2016. Therefore, we identified Long Island as being in an ACO in Year 5, which began October 1, 2016.

^d These practices participated as consortia and started Years 1 to 3 on September 1.

^e We excluded practices that left the demonstration after Year 5 because they could no longer meet the demonstration requirements from analyses of Year 6 in this report.

ACO = accountable care organization; MDM = Master Data Management system; n.a. = not applicable.

Exhibit A.17. Number and percentage of IAH and comparison beneficiaries assigned to an ACO, by evaluation year

	Second pre-demonstration year: Jun 2011– May 2012 ^a	Year 1: Jun 2012– May 2013 ^a	Year 2: Jun 2013– May 2014 ^a	Year 3: Jun 2014– May 2015 ^a	Year 4: Oct 2015– Sept 2016	Year 5: Oct 2016– Sept 2017	Year 6: Jan 2019– Dec 2019 ^b
Number of IAH beneficiaries	107	1,773	1,392	1,498	7,001	7,445	5,964
Percentage of IAH beneficiaries	1.5	21.6	19.2	19.8	73.7	74.8	74.6
Number of comparison beneficiaries	1,246	5,412	8,431	10,685	15,846	19,670	15,872
Percentage of comparison beneficiaries	3.9	16.0	26.1	34.2	41.3	47.5	48.8

Source: CMS’s MDM database and information provided by IAH sites to Mathematica, supplemented with records from conversations with CMS and its implementation contractor and web searches.

Note: We do not report data for the first pre-demonstration year (June 2010 to May 2011) because no IAH or comparison beneficiaries were assigned to an ACO that year.

^a For the Richmond-based consortium, Years 1 to 3 and the two pre-demonstration years began in September rather than June.

^b Year 6 includes only the 12 practices that participated in that year; all other years include the 14 practices that participated in Years 1 to 5.

ACO = accountable care organization; MDM = Master Data Management system.

4. Accounting for ACO participation in regression analysis

To assess whether ACO participation affected the impact of the IAH payment incentive on Medicare expenditures, we repeated our difference-in-differences regression controlling for a binary measure of ACO participation in each year, as we defined above. This allowed us to see whether the estimated effect of the IAH payment incentive differed depending on whether we controlled for ACO participation.

There are limitations to this analysis, largely related to potential measurement error associated with the measure of ACO participation we constructed. First, because our measure of participation in an ACO did not look at claims for individual beneficiaries, some IAH enrollees in the evaluation sample could have been misclassified as non-participants in an ACO. We assigned ACO participation to IAH beneficiaries based on attribution in the MDM as well as participation in an ACO at the practice level. IAH beneficiaries who received care from an IAH practice that was not affiliated with an ACO and who were enrolled in IAH in the MDM would not be considered ACO participants for our analyses (except in rare cases in which an IAH enrollee was attributed to an ACO program in the MDM).

Second, we assigned IAH beneficiaries to an ACO based on their practice’s ACO participation status at the start of the demonstration year. This approach could classify some beneficiaries as not in an ACO even though their IAH practice took part in an ACO for multiple months that year. For example, four IAH practices joined an ACO on January 1, 2015, partway through Year 3 of the IAH demonstration (June 2014 to May 2015). We classified beneficiaries associated with these practices as not being in an ACO for Year 3 because these practices were not affiliated with the ACO at the start of Year 3. If we measured these four practices as being part of an ACO in Year 3, we would have misclassified June to December 2014 as ACO months. Regardless of this distinction, if joining an ACO led IAH practices to experience substantial

unmeasured changes in the patient population or care delivery that affected Medicare expenditures, such changes probably did not occur immediately after joining an ACO.

VIII. Qualitative methods and data

To understand why and how the incentive payments might (or might not) have affected outcomes, we had to understand how IAH practices' provision of home-based primary care changed after the IAH demonstration began and throughout the demonstration. Identifying the potential effect of IAH practices' changes also required understanding how the IAH participating practices provided home-based primary care before the IAH demonstration. Understanding the care delivery model enabled us to assess whether changes made by the participating practices appeared to be designed to reduce Medicare expenditures and improve health outcomes. When reporting information about the IAH practices in Chapter I and interpreting the results in Chapters II and V, we relied on qualitative data gathered from demonstration sites during demonstration Years 1 to 3, Year 5, and Year 6.

- We conducted the most recent interviews from November 2019 to February 2020 with 30 respondents. For all practices except the one in Durham, North Carolina, we interviewed one clinician and one practice administrator from each participating practice. In addition, we interviewed one corporate leader from the parent company of the VPA to collect additional insight on centralized supports for VPA practices. The Durham practice withdrew from the demonstration in fall 2019, so we conducted an exit interview with the practice administrator in December 2019. As part of this data collection effort, we used a semistructured interview protocol to collect information on changes practices made between our last round of interviews in April 2017 (during Year 5) and fall 2019 (during Year 6), including their efforts associated with meeting performance requirements for IAH quality measures. We also asked about motivations for any changes and factors that affected implementation of those changes.
- During the previous round of telephone interviews (conducted in April 2017), we interviewed 25 clinical and administrative staff at 15 IAH practices and the VPA corporate office in Troy, Michigan. We asked respondents about changes their practices had made during the demonstration to reduce hospital admissions and readmissions, reduce avoidable ED use, coordinate care, ensure round-the-clock access to care, follow up with patients and reconcile medications within 48 hours after discharge from the hospital or ED, and document patients' preferences. We also asked about motivation for making changes, clinician and staff reactions to changes, and factors that affected implementation of those changes.
- During telephone interviews conducted in January and February 2017, we collected information about IAH practices' structural characteristics and how they deliver care.
- During visits to demonstration sites from April 2015 to October 2015, we interviewed the sites' IAH team members and administrative staff involved in implementing the IAH demonstration. During this round of site visits, we focused on documenting changes in how the practices delivered care, the barriers to and facilitators of meeting the requirements of the demonstration, and how sites planned to sustain the home-based primary care model.
- Finally, we provide information gathered during earlier rounds of site visits: February to May 2013 (visits during Year 1) and February to July 2014 (visits during Year 2). During these earlier site visits, we focused on documenting how the practices delivered care, including changes from the year before the demonstration to Year 1 and changes from Year 1 to Year 2. During this period, we collected information on barriers to and facilitators of meeting the requirements of the demonstration and on how

sites used information technologies such as electronic health records and health information exchange to support their work.

For all interviews, we coded the data using a template that reflected the various requirements of the IAH demonstration (for example, providing patients with 24-hour access to the care team and working to reduce ED visits). The coding template also captured aspects of the five domains identified by the Consolidated Framework for Implementation Research (Damschroder et al. 2009) as playing an important role in implementation success: (1) the inner setting (internal attributes) of the practice sites, including structural and cultural characteristics affecting capacity for change; (2) the external environment (such as the availability of clinicians in the IAH practice's local market); (3) characteristics of the IAH demonstration itself; (4) characteristics of the people involved in implementing the model; and (5) processes used to implement the model. We used qualitative analysis software (NVivo) to sort data using this coding template. We analyzed the sorted data to identify key barriers to and facilitators of implementation of the IAH demonstration in each participating site and identified common themes across sites.

In our analysis of qualitative data, we described what happened during the demonstration. We did not have a comparison group of primary care practices, so we could not be certain whether changes in practices' operations or structure occurred because of the demonstration. In addition, because we did not visit sites until after the demonstration began, data on practices' operations and structure before the demonstration was limited to what interviewees told us was different in Year 1 relative to before the demonstration.

IX. Understanding differences between evaluation results and incentive payments

A. Approach for calculating incentive payments

Practices participating in the IAH demonstration are eligible to receive annual incentive payments. Specifically, practices can earn incentive payments if their patients' Medicare expenditures are below the practice's target expenditures and the practice meets certain standards for a set of quality measures. Calculation of incentive payments is based on (1) comparison of costs incurred (that is, actual expenditures of IAH-applicable beneficiaries during the performance year) with the target expenditures and (2) performance on payment-related quality measures. Actual expenditures of IAH-applicable beneficiaries are determined from Medicare FFS claims data. Target expenditures represent the expected expenditures based on Medicare FFS beneficiaries not exposed to the demonstration. Performance on payment-related quality measures is determined from information IAH practices report (for three of the six measures) and Medicare FFS claims data (for the remaining three measures).⁴⁰ IAH-applicable beneficiaries are those who are enrolled in the demonstration; for more information about IAH enrollees, see Chapter III of this appendix.

Although procedures for measuring performance on payment-related quality measures have remained unchanged throughout the IAH demonstration, calculations of actual and target expenditures have changed multiple times. Before beginning the demonstration, "CMS developed a risk-based actuarial methodology (the 'original actuarial methodology') for calculating incentive payments. In response to questions raised by participating IAH practices in early performance years regarding the risk scores used in the demonstration, CMS explored a different approach to the original actuarial method and developed a second methodology (the 'regression-based methodology'), which was later revised (the 'revised regression-based methodology')"

⁴⁰ See Exhibit II.4 for a list of practice-reported and claims-based measures.

(CMS 2021b). For Year 5, calculations for 10 practices used the revised regression-based methodology, and calculations for 4 practices used the original actuarial methodology.⁴¹

For Year 6, calculations for all practices used the “revised actuarial methodology,” which generated “practice-specific PBPM target expenditures based on historical Medicare FFS per capita expenditures for the Medicare FFS population in the same counties as IAH-applicable beneficiaries. The per capita expenditures are adjusted to reflect the average CMS-HCC risk score, the average frailty score (used in the Program of All-inclusive Care for the Elderly), and a utilization factor of the IAH-applicable population in each practice” (CMS 2021b). The utilization factor is an add-on to the base risk score that “reflects the level of risk that was not captured by the CMS-HCC model for beneficiaries with a hospital admission and rehabilitation services use in the 12 months prior to their enrollment date in the performance year” (CMS 2021b). New enrollees of IAH providers “receive a prospective CMS-HCC risk score, frailty factor, and utilization factor. The risk score and frailty factor for continuing enrollees are updated in future performance years only for changes in demographics (age and Medicaid status). The utilization factor is applied in future performance years only if a continuing enrollee had a hospitalization and post-acute care in the 12 months prior to the performance year” (CMS 2021c). The utilization factor led to an increase in target expenditures; all else equal, higher target expenditures result in a larger incentive payment. This implicitly assumes that IAH enrollees who were hospitalized and had post-acute care in one year are at equal risk for these outcomes in the following year. However, if some IAH enrollees tended to have a lower risk of hospitalization and post-acute care in the year after they had such utilization—in other words, if their expenditures tended to regress to the lower level of expenditures they incurred prior to the year that included a hospitalization and post-acute care—then the utilization factor may overestimate target expenditures. Finally, the adjusted per capita expenditures are trended to the performance year by the increase in total per capita Medicare FFS expenditures, as estimated by CMS’ Office of the Actuary” (CMS 2021b). Trending expenditures forward to the performance year is necessary because the county-level data used to set target expenditures are lagged by about 15 months. For example, for Year 6, CMS used calendar year 2018 expenditures, which were published in April 2020. Trend factors come from the most recently available Medicare Trustees Report and are updated annually (CMS 2021c).

Equation (7) shows a simplified version of how CMS calculated the spending target for each beneficiary enrolled in the demonstration in Year 6. Equation (8) shows how CMS calculated the savings or loss PBPM for each practice.

$$(7) \text{ target expenditures} = \text{average monthly FFS county cost} * \text{cost trend} *$$

$$(\text{beneficiary risk score} + \text{frailty factor} + \text{utilization factor})$$

$$(8) \text{ savings / loss PBPM} = \text{average target expenditures PBPM} - \text{average actual expenditures PBPM}$$

The maximum incentive payment for each practice in Year 6 depended on the following factors (CMS 2021c):

- Savings or loss PBPM.
- Whether the estimated savings was statistically significant. “A one-sided confidence interval is constructed around each practice’s actual expenditures, for use in determining whether savings are

⁴¹ More information on these methodologies is in the 2018 IAH Report to Congress, available at <https://innovation.cms.gov/Files/reports/iah-rtc.pdf>.

statistically significant. [Target expenditures] are compared to the upper bound of the confidence intervals (80th and 85th) to determine if any observed savings are likely to be actual, rather than due to random variation” (CMS 2021c).

- The number of months of enrollment in the demonstration by IAH-applicable beneficiaries.
- Whether the maximum incentive payment exceeded the 5 percent savings kept by CMS, which was calculated as 5 percent multiplied by target expenditures and total beneficiary months.

If a practice had statistically significant savings that exceeded the 5 percent of savings that CMS retained, then the final incentive payment depended on the following:

1. Whether the target expenditures were greater than the upper bound of the 85th confidence interval (rather than the 80th). If its target expenditures were greater than the upper bound of the 85th confidence interval, the practice earned 90 percent of the maximum payment. Otherwise, its target expenditures were greater than the upper bound of the 80th confidence interval, and the practice earned 50 percent of its target expenditures.
2. The practice’s performance on the six payment-related quality measures. If a practice met the standards for all six quality measures tied to payment, then it earned 100 percent of the maximum incentive payment. If a practice achieved the threshold for five, four, or three quality measures, it earned, respectively, 83 percent, 67 percent, or 50 percent of the maximum payment. The only requirement is that a practice must meet the threshold for the quality measures—payment does not vary by how much the practice exceeded the threshold.

As an example, consider a practice that had a \$1 million maximum incentive payment after subtracting the 5 percent of savings that CMS retains and both of the following: (1) its target expenditures were greater than the upper bound of the 85th confidence interval; and (2) it achieved the threshold for three quality measures. For this practice, the actual incentive payment would be \$1 million multiplied by 80 percent (for the 85th confidence interval) multiplied by 50 percent (for achieving the threshold for three quality measures), for a final payment of \$400,000.

B. Differences between evaluation results and incentive payments

1. Purposes of the evaluation and incentive payment calculation

The evaluation and the incentive payment calculation serve different purposes. The evaluation needs to estimate the effect of the demonstration after accounting for how factors other than the demonstration that changed during the performance period affected expenditures. That is, the evaluation measures expenditure reductions (if any) that would not have occurred without the payment incentive implemented during the IAH demonstration. Conducting a rigorous evaluation requires using the same data and approach to identify IAH and comparison groups and advanced statistical methods to reduce the risk of bias in the study results. Specifically, it requires using a comparison group of beneficiaries that are similar to IAH beneficiaries but did not receive home-based primary care and—as we describe in the following section—examining changes in expenditures for the comparison group relative to changes for patients of IAH practices over the same time period. By contrast, the incentive payment calculation needs to offer a timely way to measure a target spending level to reward participation, subject to other requirements such as quality measure performance. As described in the previous section of this chapter, this is done by trending historical expenditures for Medicare FFS beneficiaries and adjusting those expenditures to reflect IAH beneficiaries’ health status and past utilization.

2. Reasons why the evaluation and incentive payment calculation results differed

In all years of the demonstration, large differences have existed between the total incentive payments paid to IAH practices and the estimated aggregate effect of the IAH payment incentive calculated by the evaluation (see Exhibit II.2 for more information). In Year 6, the estimated aggregated effect was a reduction of \$3.2 million, and total incentive payments were \$11.1 million. The evaluation and incentive payment calculation results differed because of (1) differences in the sample of beneficiaries included and (2) differences in the methods used. As we explain in this section, one particular methodological difference accounts for the vast majority of the discrepancy between evaluation findings and incentive payment calculation results.

To evaluate the effect of the IAH payment incentive, Mathematica used a quasi-experimental difference-in-differences design. Under this design, we estimated effects as the change in outcomes for beneficiaries receiving care from IAH practices before and after the start of the demonstration relative to the change during the same period for a matched comparison group. Equation (9) shows a simplified version of how the evaluation calculated the effect of the IAH payment incentive on expenditures in Year 6. The terms IAH_{y6} and C_{y6} reflect regression-adjusted PBPM expenditures in Year 6 for IAH and comparison beneficiaries, respectively. The terms IAH_{pd} and C_{pd} reflect regression-adjusted PBPM expenditures in the pre-demonstration year 2011–2012 for IAH and comparison beneficiaries, respectively; this is the year we used as the baseline for calculating the difference-in-differences estimate.

$$(9) \text{ effect on expenditures} = (IAH_{y6} - C_{y6}) - (IAH_{pd} - C_{pd})$$

In Year 6, regression-adjusted PBPM expenditures were \$4,725 for IAH beneficiaries and \$5,137 for the comparison group, for a difference of -\$412 PBPM (Exhibit B.2a). This difference of -\$412 PBPM is conceptually similar to the difference between actual and target expenditures used in the incentive payment calculation, where the difference between actual expenditures for IAH-applicable beneficiaries and target expenditures was -\$529 PBPM for the average beneficiary in Year 6 (data not shown). In other words, $IAH_{y6} - C_{y6}$ in Equation (9) is conceptually similar to Equation (8). The difference between the IAH-comparison group difference in Year 6 calculated by the evaluation (-\$412 PBPM) and that calculated for incentive payments (-\$529 PBPM) was -\$117 PBPM, or 28 percent. It is likely that each of the differences in sample (identification of IAH beneficiaries and a comparison or target group)⁴² and methods (calculation of expenditures) contributed at least somewhat to the -\$117 PBPM difference.

While the figure of -\$529 PBPM generated by the incentive payment calculation was 28 percent larger than that calculated by the evaluation (-\$412 PBPM), the calculation of \$11.1 million in incentive payments was about 250 percent larger than the estimated reduction in expenditures calculated by the evaluation (\$3.2 million). The stark difference between \$11.1 million and \$3.2 million is largely due to one methodological difference: unlike the incentive payment calculation, the evaluation seeks to estimate the *change* in the difference between IAH and comparison beneficiaries relative to before the demonstration began. Before the demonstration began, beneficiaries who met IAH eligibility criteria and were treated by IAH practices in the year before the demonstration had lower expenditures than a matched comparison group of beneficiaries who had similar observable characteristics and health status but did not receive home-based primary care. One year before the demonstration, regression-adjusted PBPM expenditures were \$4,626 for beneficiaries eligible

⁴² For more information about differences in the IAH samples used for the evaluation and the incentive payment calculation, see Chapter III of this appendix.

for IAH and \$4,996 for the comparison group, for a difference of $-\$371$,⁴³ the difference between $-\$412$ (from Year 6) and $-\$371$ is $-\$41$, which is the evaluation's estimated effect of the IAH payment incentive in Year 6.

Why did the evaluation use the change in the difference between IAH and comparison beneficiaries rather than the IAH-comparison difference in Year 6 only? First, interpreting the difference in mean expenditures for IAH and comparison beneficiaries in a single demonstration year as the effect of the demonstration poses a substantial risk of bias by not accounting for any potential unobserved factors that affected expenditures unrelated to the payment incentive and delivery of home-based primary care. For example, it is possible that unobserved differences between IAH beneficiaries and comparison beneficiaries influenced both the decision to start home-based primary care at an IAH practice and health care expenditure patterns. These could include differences in health status and environment that were not captured in Medicare claims or administrative data, as well as differences in attitudes and preferences about health care. Using a difference-in-differences approach avoids this type of bias so long as unobserved differences between IAH beneficiaries and comparison beneficiaries are consistent over time. Also, by using data from both the baseline and Year 6 for both IAH and comparison beneficiaries, this approach accounts for underlying trends that affect Medicare expenditures the same way for both groups. However, using the change in the difference between IAH and comparison beneficiaries does not eliminate the risk of bias from changes in unobserved factors; for more information about this and other limitations of the evaluation, see Chapter II of this report.

Second, while the demonstration has two components—a payment incentive and the delivery of home-based primary care—only the payment incentive has the potential to generate expenditure reductions for Medicare that *would not have been achieved without the payment incentive*. Using the change in the difference between IAH and comparison beneficiaries allows us to measure the expenditure reduction (if any) for Medicare that was achieved solely because of the demonstration. This approach does not attempt to measure any spending reductions (or increases) caused by the provision of home-based primary care from providers that met the demonstration requirements relative to the receipt of office-based care, because payment for home-based primary care was in effect before the demonstration. (We examined the effects of home-based primary care in Chapter III of this report and in an earlier evaluation report.)

3. Reasons for changes in evaluation results and incentive payments from Year 5 to Year 6

The estimated aggregate effect the evaluation calculated decreased sharply from a reduction of \$31.4 million in Year 5 to a reduction of \$3.2 million in Year 6. By contrast, total incentive payments increased from \$6.8 million in Year 5 to \$11.1 million in Year 6. Why did the evaluation results and incentive payment calculations change in opposite directions from Year 5 to Year 6?

The primary reason for the large decrease from Year 5 to Year 6 in the estimated aggregate effect calculated by the evaluation is that the evaluation had to use different samples for these years because two practices left the demonstration before Year 6 began. As discussed in Chapter II, the Year 6 estimate comes from a sample that includes just the 12 practices that participated in Year 6, whereas the Year 5 estimate includes the 14 practices that participated in Year 5.⁴⁴ About two-thirds of the estimated effect in Year 5 was driven by a

⁴³ Figures are rounded, leading to a difference in the pre-demonstration year of $-\$371$ (rather than $-\$370$). The estimated effect of $-\$41$ was calculated before rounding.

⁴⁴ Cleveland and Austin left the demonstration after Year 5. Durham left the demonstration in the last quarter of Year 6 and was excluded from Year 6 incentive payment calculations but was included in the Year 6 evaluation.

single influential practice that discontinued participation in the demonstration after Year 5 and no longer provides home-based primary care.

Unlike for the evaluation, the changing composition of the samples the implementation contractor used to calculate total incentive payments—from 14 practices in Year 5 to 11 practices in Year 6—contributed little to the increase in incentive payments from Year 5 to Year 6. The two practices that left the demonstration after Year 5 did not earn a payment in Year 5. A third practice was not included in incentive payment calculations for Year 6 because it withdrew from the demonstration during the year; since this practice received a payment in Year 5, the increase in total incentive payments from Year 5 to Year 6 may have been larger had this practice completed Year 6. The main reason for the increase in the total incentive payments was the introduction of the revised actuarial methodology in Year 6.

To assess how the change in methodology contributed to an increase in total incentive payments between Year 5 and Year 6, we focused on the nine practices that earned a payment in Year 6 (seven of which also earned a payment in Year 5). For each of these nine practices, we calculated the within-year difference between (1) actual expenditures for IAH-applicable beneficiaries and (2) target expenditures, and we compared the difference between these numbers in Year 5 to the difference in Year 6.⁴⁵ We found that, for the average practice, the difference between actual and target expenditures was $-\$464$ PBPM in Year 5 and increased to $-\$600$ PBPM in Year 6.⁴⁶ Larger differences between actual and target expenditures led to more practices earning incentive payments or larger incentive payments. Also, two of the three largest practices—those that contributed the most IAH beneficiary-months to the incentive payment calculation—experienced the largest relative changes between actual and target expenditures from Year 5 to Year 6.

⁴⁵ Data on actual and target expenditures for each practice for [Year 5](#) and [Year 6](#) are available on CMS's website. Between actual and target expenditures tended to be smaller for practices with a large number of IAH-applicable beneficiaries relative to practices with a small number of IAH-applicable beneficiaries.

⁴⁶ The figure of $-\$600$ PBPM for Year 6 differs from the figure of $-\$529$ PBPM cited earlier in this chapter because $-\$600$ was the difference for the average practice, whereas $-\$529$ was the difference for the average beneficiary. The fact that the difference for the average beneficiary was smaller than the difference for the average practice indicates that the gap.

Appendix B

Effects of IAH payment incentive: supplementary exhibits

The exhibits in this section present results for the analyses we describe in Chapter II.

Exhibit B.1. Baseline unadjusted means of outcomes among all IAH beneficiaries

Outcome name	Baseline unadjusted IAH mean, practices that participated in Year 5	Baseline unadjusted IAH mean, practices that participated in Year 6
PBPM Medicare expenditures		
Total	\$4,397	\$4,186
Inpatient	\$1,741	\$1,627
Skilled nursing facility	\$605	\$621
Home health (Parts A and B)	\$781	\$717
Hospice	\$153	\$162
Outpatient	\$253	\$247
Clinician/supplier	\$715	\$675
Durable medical equipment	\$150	\$136
Service use outcomes		
Number of hospital admissions per beneficiary per year ^a	1.78	1.73
Number of potentially avoidable hospital admissions per beneficiary per year ^b	0.46	0.44
Probability (as a percentage) of having a qualifying hospital discharge and unplanned readmission within 30 days of discharge	16.04	15.91
Number of ED visits per beneficiary per year	2.90	2.86
Number of outpatient ED visits per beneficiary per year ^c	1.46	1.44
Number of ED visits resulting in hospital admission per beneficiary per year	1.44	1.42
Number of potentially avoidable outpatient ED visits per beneficiary per year ^d	0.19	0.18
Health outcomes		
Probability (as a percentage) of dying in the demonstration year	14.85	14.90
Probability (as a percentage) of entering institutional long-term care in the demonstration year	7.00	n.a. ^e

Source: Mathematica’s analysis of data from the IAH implementation contractor and 2009–2019 Medicare claims and enrollment data from the Chronic Conditions Warehouse for IAH and matched comparison beneficiaries.

Notes: We calculated the baseline means of outcomes using the sample of IAH-eligible beneficiaries in the year before IAH started. We report the baseline means for two samples: the 14 practices that participated in Year 5 and the 12 practices that participated in Year 6.

^a The number of hospital admissions includes observation stays.

^b The number of potentially avoidable hospital admissions includes observation stays. A potentially avoidable hospital admission is one in which appropriate primary and specialty care may prevent or reduce the need for a hospital admission.

^c The number of outpatient ED visits measures all those not resulting in a hospital admission, including those resulting in an observation stay.

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Exhibit B.1.(continued)

^d A potentially avoidable outpatient ED visit is one in which appropriate primary and specialty care may prevent or reduce the need for such visits. The measure excluded ED visits that led to a hospital admission because there was no diagnosis from such a visit in a claim record when the ED visit led to an admission.

^e Data on entry into institutional long-term care were available through Year 5 only, so we do not report baseline data on this measure for the practices that participated in Year 6.

ED = emergency department; n.a. = not applicable; PBPM = per beneficiary per month.

Exhibit B.2a. Estimated effect of the IAH payment incentive on total Medicare expenditures PBPM in Years 1 to 6, practices that participated in Year 6

	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect ^a
Year 6	\$4,725	\$5,137	-\$412 (\$108)	-\$41 (\$126)	-1.0
Year 5	\$4,530	\$5,014	-\$483 (\$169)	-\$113 (\$157)	-2.7
Year 4	\$4,326	\$4,715	-\$389 (\$194)	-\$19 (\$179)	-0.4
Year 3	\$4,473	\$4,791	-\$318 (\$139)	\$52 (\$117)	1.3
Year 2	\$4,610	\$4,830	-\$220 (\$119)	\$150 (\$122)	3.6
Year 1	\$4,587	\$4,857	-\$270 (\$125)	\$101 (\$92)	2.4
One year pre-IAH ^b	\$4,626	\$4,996	-\$371 (\$122)	-	-
Two years pre-IAH	\$4,811	\$5,209	-\$398 (\$121)	-\$27 (\$64)	-0.6
Total unweighted number of observations across all years: 290,514					

Source: Mathematica's analysis of data from the IAH implementation contractor and 2009–2019 Medicare claims and enrollment data from the Chronic Conditions Warehouse for IAH beneficiaries in all IAH practices that participated in Year 6 and matched comparison beneficiaries.

Notes: We give standard errors in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers the matching and eligibility weights. The table reports the regression-adjusted means of the IAH and matched comparison groups in each year. We obtained regression-adjusted means by applying the estimated regression coefficients to the covariates of IAH beneficiaries in the latest demonstration year. Because of rounding, a difference-in-differences estimate displayed as zero may be shown alongside a percentage effect that exceeds zero.

^a We used the unadjusted IAH group mean in the year before the demonstration to calculate the percentage effect for each demonstration year. Exhibit B.1 reports the baseline unadjusted IAH group mean for all outcomes.

^b The difference-in-differences estimate for the period before the demonstration is zero (with no standard error) in all regressions because we calculated that estimate for each year as the difference in means between IAH and comparison beneficiaries in that year minus the difference in the year before the demonstration.

*/**/** The difference is statistically significant at the 0.10/0.05/0.01 level.

PBPM = per beneficiary per month.

Exhibit B.2b. Estimated effect of the IAH payment incentive on total Medicare expenditures PBPM in Years 1 to 5, practices that participated in Year 5

	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect ^a
Five-year average annual effect ^b	\$4,362	\$4,664	-\$302 (\$139)	-\$200 (\$151)	-4.6
Year 5	\$4,526	\$4,958	-\$432 (\$158)	-\$330* (\$182)	-7.5
Year 4	\$4,301	\$4,685	-\$384 (\$177)	-\$282 (\$205)	-6.4
Year 3	\$4,481	\$4,762	-\$280 (\$129)	-\$178 (\$158)	-4.1
Year 2	\$4,639	\$4,771	-\$133 (\$124)	-\$31 (\$139)	-0.7
Year 1	\$4,658	\$4,879	-\$221 (\$146)	-\$119 (\$97)	-2.7
One year pre-IAH ^c	\$4,794	\$4,896	-\$102 (\$186)	-	-
Two years pre-IAH	\$4,972	\$5,107	-\$135 (\$190)	-\$33 (\$57)	-0.8

Total unweighted number of observations across all years: 295,292

Source: Mathematica’s analysis of data from the IAH implementation contractor and 2009–2017 Medicare claims and enrollment data from the Chronic Conditions Warehouse for IAH beneficiaries in all IAH practices that participated in Year 5 and matched comparison beneficiaries.

Notes: We give standard errors in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers the matching and eligibility weights. The table reports the regression-adjusted means of the IAH and matched comparison groups in each year. We obtained regression-adjusted means by applying the estimated regression coefficients to the covariates of IAH beneficiaries in the latest demonstration year. Because of rounding, a difference-in-differences estimate displayed as zero may be shown alongside a percentage effect that exceeds zero.

^a We used the unadjusted IAH group mean in the year before the demonstration to calculate the percentage effect for each demonstration year. Exhibit B.1 reports the baseline unadjusted IAH group mean for all outcomes.

^b We estimated a separate model using a single demonstration indicator (instead of separate indicators for each demonstration year) and used its interaction with IAH status to obtain an average annual estimated effect across five demonstration years.

^c The difference-in-differences estimate for the period before the demonstration is zero (with no standard error) in all regressions because we calculated that estimate for each year as the difference in means between IAH and comparison beneficiaries in that year minus the difference in the year before the demonstration.

*/**/** The difference is statistically significant at the 0.10/0.05/0.01 level.

PBPM = per beneficiary per month.

Exhibit B.3. Estimated effect of the IAH payment incentive on total Medicare expenditures PBPM under Bayesian model in Years 1 to 6, practices that participated in Year 6

	Difference-in-differences estimated effect	Percentage effect ^a	Probability of reducing expenditures ≥ \$100 PBPM ^b	Probability of reducing expenditures by \$41 PBPM ^b
Year 6	-\$24 (-\$239, \$193)	-0.6	28.0%	44.6%
Year 5	-\$116 (-\$330, \$91)	-2.8	54.2%	72.0%
Year 4	-\$26 (-\$238, \$190)	-0.6	28.4%	45.4%
Year 3	\$18 (-\$197, \$233)	0.4	18.4%	32.3%
Year 2	\$179 (-\$39, \$393)	4.3	1.7%	4.8%
Year 1	-\$7 (-\$224, \$208)	-0.2	24.0%	38.8%
One year pre-IAH ^c	-	-	-	-
Two years pre-IAH	-\$44 (-\$262, \$177)	-1.0	33.5%	50.7%

Source: Mathematica's analysis of data from the IAH implementation contractor and 2009–2019 Medicare claims and enrollment data from the Chronic Conditions Warehouse for IAH beneficiaries in all IAH practices that participated in Year 6 and matched comparison beneficiaries.

Notes: We report the 90 percent credible intervals in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers the matching and eligibility weights. The table reports the regression-adjusted means of the IAH and matched comparison groups in each year. We obtained regression-adjusted means by applying the estimated regression coefficients to the covariates of IAH beneficiaries in the latest demonstration year. Because of rounding, a difference-in-differences estimate displayed as zero may be shown alongside a percentage effect that exceeds zero.

^a We used the unadjusted IAH group mean in the year before the demonstration to calculate the percentage effect for each demonstration year. Exhibit B.1 reports the baseline unadjusted IAH group mean for all outcomes.

^b We used \$100 PBPM because this amount represents an expenditure decrease of about 2 percent from IAH beneficiaries' spending in the year before the demonstration. We used \$41 PBPM because that was the estimated effect of the IAH payment incentive on total expenditures according to the frequentist model.

^c The difference-in-differences estimate for the period before the demonstration is zero (with no credible interval) in all regressions because we calculated that estimate for each year as the difference in means between IAH and comparison beneficiaries in that year minus the difference in the year before the demonstration.

PBPM = per beneficiary per month.

Exhibit B.4a. Estimated effect of the IAH payment incentive on Medicare expenditures PBPM in Years 1 to 6, by service category, practices that participated in Year 6

Service type	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect ^a
Inpatient					
Year 6	\$1,876	\$2,313	-\$436	-\$66 (\$90)	-4.1
Year 5	\$1,815	\$2,263	-\$448	-\$77 (\$116)	-4.8
Year 4	\$1,718	\$2,067	-\$349	\$21 (\$130)	1.3
Year 3	\$1,805	\$2,109	-\$304	\$67 (\$88)	4.1
Year 2	\$1,854	\$2,157	-\$303	\$67 (\$79)	4.1
Year 1	\$1,864	\$2,135	-\$271	\$99 (\$65)	6.1
One year pre-IAH ^b	\$1,839	\$2,209	-\$370	-	
Two years pre-IAH	\$1,927	\$2,303	-\$376	-\$6 (\$54)	-0.3
SNF					
Year 6	\$687	\$882	-\$196	\$36 (\$35)	5.8
Year 5	\$713	\$929	-\$217	\$15 (\$52)	2.4
Year 4	\$694	\$927	-\$233	-\$1 (\$56)	-0.2
Year 3	\$725	\$938	-\$213	\$19 (\$32)	3.1
Year 2	\$711	\$902	-\$191	\$40 (\$28)	6.5
Year 1	\$691	\$919	-\$228	\$4 (\$22)	0.6
One year pre-IAH ^b	\$706	\$938	-\$232	-	-
Two years pre-IAH	\$755	\$1,008	-\$253	-\$21 (\$22)	-3.4
Home health (Parts A and B)					
Year 6	\$662	\$475	\$187	-\$9 (\$38)	-1.2
Year 5	\$689	\$485	\$204	\$8 (\$35)	1.2
Year 4	\$647	\$460	\$187	-\$8 (\$36)	-1.2
Year 3	\$649	\$473	\$176	-\$20 (\$34)	-2.7
Year 2	\$762	\$535	\$227	\$31 (\$23)	4.4
Year 1	\$717	\$531	\$186	-\$10 (\$18)	-1.3
One year pre-IAH ^b	\$739	\$544	\$196	-	
Two years pre-IAH	\$798	\$595	\$204	\$8 (\$12)	1.1
Hospice					
Year 6	\$224	\$130	\$94	\$30** (\$14)	18.2
Year 5	\$174	\$117	\$57	-\$8 (\$11)	-4.7
Year 4	\$177	\$119	\$58	-\$6 (\$12)	-3.8
Year 3	\$171	\$107	\$64	\$0 (\$14)	-0.1
Year 2	\$156	\$89	\$67	\$3 (\$12)	1.6
Year 1	\$171	\$111	\$60	-\$4 (\$9)	-2.4
One year pre-IAH ^b	\$176	\$112	\$64	-	-
Two years pre-IAH	\$150	\$105	\$45	-\$19** (\$9)	-11.7
Outpatient					
Year 6	\$361	\$457	-\$96	-\$31 (\$26)	-12.6
Year 5	\$324	\$401	-\$77	-\$12 (\$15)	-5.0
Year 4	\$301	\$380	-\$79	-\$15 (\$15)	-6.0
Year 3	\$300	\$368	-\$68	-\$4 (\$10)	-1.4

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Exhibit B.4a.(continued)

Service type	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect ^a
Year 2	\$290	\$352	-\$62	\$3 (\$9)	1.1
Year 1	\$268	\$340	-\$71	-\$7 (\$8)	-2.7
One year pre-IAH ^b	\$274	\$339	-\$65	-	
Two years pre-IAH	\$271	\$323	-\$51	\$13 (\$8)	5.4
Clinician/supplier					
Year 6	\$803	\$779	\$24	\$17 (\$38)	2.6
Year 5	\$727	\$729	-\$2	-\$9 (\$21)	-1.3
Year 4	\$699	\$679	\$21	\$14 (\$30)	2.1
Year 3	\$722	\$699	\$23	\$16 (\$23)	2.4
Year 2	\$727	\$702	\$25	\$19 (\$21)	2.7
Year 1	\$736	\$705	\$31	\$24* (\$14)	3.6
One year pre-IAH ^b	\$737	\$731	\$6	-	
Two years pre-IAH	\$745	\$739	\$6	\$0 (\$11)	0.0
Durable medical equipment					
Year 6	\$112	\$101	\$11	-\$19** (\$8)	-13.6
Year 5	\$89	\$90	\$0	-\$30*** (\$7)	-22.3
Year 4	\$89	\$83	\$6	-\$24*** (\$8)	-17.4
Year 3	\$101	\$97	\$4	-\$26*** (\$7)	-19.2
Year 2	\$110	\$93	\$17	-\$13** (\$6)	-9.5
Year 1	\$139	\$116	\$23	-\$7 (\$5)	-5.1
One year pre-IAH ^b	\$154	\$124	\$30	-	-
Two years pre-IAH	\$164	\$136	\$28	-\$2 (\$6)	-1.6
Total unweighted number of observations across all years: 290,514					

Source: Mathematica's analysis of data from the IAH implementation contractor and 2009–2019 Medicare claims and enrollment data from the Chronic Conditions Warehouse for IAH beneficiaries in all IAH practices that participated in Year 6 and matched comparison beneficiaries.

Notes: We give standard errors in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers the matching and eligibility weights. The table reports the regression-adjusted means of the IAH and matched comparison groups in each year. We obtained regression-adjusted means by applying the estimated regression coefficients to the covariates of IAH beneficiaries in the latest demonstration year. We calculated percentage effects using the unadjusted IAH group mean in the year before the demonstration. Because of rounding, a difference-in-differences estimate displayed as zero may be shown alongside a percentage effect that exceeds zero.

^a We used the unadjusted IAH group mean in the year before the demonstration to calculate the percentage effect for each demonstration year. Exhibit B.1 reports the baseline unadjusted IAH group mean for all outcomes.

^b The difference-in-differences estimate for the year before the demonstration is zero (with no standard error) in all regressions because we calculated that estimate for each year as the difference in means between IAH and comparison beneficiaries in that year minus the difference in the year before the demonstration.

*/**/*** The difference is statistically significant at the 0.10/0.05/0.01 level.

PBPM = per beneficiary per month; SNF = skilled nursing facility.

Exhibit B.4b. Estimated effect of the IAH payment incentive on Medicare expenditures PBPM in Years 1 to 5, by service category, practices that participated in Year 5

Service type	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect ^a
Inpatient					
Five-year average annual effect ^b	\$1,768	\$2,076	-\$308	-\$118 (\$95)	-6.8
Year 5	\$1,834	\$2,245	-\$410	-\$221* (\$120)	-12.7
Year 4	\$1,720	\$2,065	-\$346	-\$156 (\$134)	-9.0
Year 3	\$1,838	\$2,106	-\$269	-\$79 (\$95)	-4.5
Year 2	\$1,900	\$2,130	-\$231	-\$41 (\$77)	-2.4
Year 1	\$1,923	\$2,168	-\$245	-\$55 (\$61)	-3.2
One year pre-IAH ^c	\$1,943	\$2,133	-\$190	-	-
Two years pre-IAH	\$2,032	\$2,233	-\$201	-\$12 (\$49)	-0.7
SNF					
Five-year average annual effect ^b	\$653	\$863	-\$210	-\$5 (\$31)	-0.8
Year 5	\$693	\$900	-\$207	-\$2 (\$46)	-0.3
Year 4	\$676	\$900	-\$224	-\$18 (\$50)	-3.0
Year 3	\$705	\$911	-\$205	\$0 (\$27)	0.0
Year 2	\$678	\$871	-\$192	\$13 (\$25)	2.2
Year 1	\$673	\$892	-\$219	-\$14 (\$19)	-2.2
One year pre-IAH ^c	\$687	\$893	-\$206	-	-
Two years pre-IAH	\$734	\$949	-\$215	-\$9 (\$18)	-1.5
Home health (Parts A and B)					
Five-year average annual effect ^b	\$697	\$498	\$200	-\$5 (\$28)	-0.7
Year 5	\$703	\$494	\$208	\$4 (\$36)	0.5
Year 4	\$660	\$473	\$187	-\$17 (\$36)	-2.2
Year 3	\$659	\$487	\$172	-\$33 (\$38)	-4.2
Year 2	\$788	\$554	\$234	\$30 (\$24)	3.8
Year 1	\$748	\$552	\$196	-\$8 (\$17)	-1.1
One year pre-IAH ^c	\$791	\$586	\$204	-	-
Two years pre-IAH	\$849	\$637	\$212	\$8 (\$10)	1.0
Hospice					
Five-year average annual effect ^b	\$160	\$102	\$58	\$2 (\$8)	1.3
Year 5	\$167	\$112	\$55	-\$1 (\$11)	-0.8
Year 4	\$169	\$113	\$55	-\$1 (\$10)	-0.8
Year 3	\$162	\$101	\$61	\$5 (\$12)	3.0
Year 2	\$151	\$84	\$67	\$10 (\$11)	6.7
Year 1	\$162	\$106	\$56	\$0 (\$8)	-0.2

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Exhibit B.4b.(continued)

Service type	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect ^a
One year pre-IAH ^c	\$164	\$108	\$56	-	-
Two years pre-IAH	\$144	\$100	\$44	-\$12 (\$8)	-8.0
Outpatient					
Five-year average annual effect ^b	\$286	\$360	-\$73	-\$12 (\$10)	-4.9
Year 5	\$319	\$395	-\$76	-\$15 (\$15)	-6.1
Year 4	\$296	\$375	-\$79	-\$19 (\$14)	-7.3
Year 3	\$294	\$365	-\$70	-\$10 (\$11)	-3.8
Year 2	\$285	\$346	-\$61	-\$1 (\$9)	-0.3
Year 1	\$263	\$337	-\$74	-\$14* (\$8)	-5.4
One year pre-IAH ^c	\$273	\$334	-\$61	-	-
Two years pre-IAH	\$268	\$231	-\$53	\$8 (\$8)	3.1
Clinician/supplier					
Five-year average annual effect ^b	\$699	\$677	\$22	-\$33 (\$32)	-4.7
Year 5	\$721	\$724	-\$2	-\$57 (\$35)	-8.0
Year 4	\$693	\$675	\$17	-\$38 (\$39)	-5.3
Year 3	\$723	\$695	\$28	-\$27 (\$32)	-3.8
Year 2	\$727	\$692	\$35	-\$20 (\$32)	-2.8
Year 1	\$744	\$706	\$38	-\$18 (\$24)	-2.5
One year pre-IAH ^c	\$772	\$717	\$55	-	-
Two years pre-IAH	\$775	\$732	\$42	-\$13 (\$10)	-1.8
Durable medical equipment					
Five-year average annual effect ^b	\$99	\$89	\$10	-\$28*** (\$7)	-18.8
Year 5	\$89	\$89	\$0	-\$38*** (\$9)	-25.2
Year 4	\$87	\$82	\$5	-\$33*** (\$11)	-22.2
Year 3	\$100	\$96	\$4	-\$35*** (\$9)	-23.1
Year 2	\$110	\$94	\$16	-\$22*** (\$8)	-14.9
Year 1	\$145	\$117	\$28	-\$10** (\$5)	-6.7
One year pre-IAH ^c	\$163	\$125	\$38	-	-
Two years pre-IAH	\$170	\$135	\$35	-\$3 (\$5)	-2.1

Total unweighted number of observations across all years: 295,292

Source: Mathematica's analysis of data from the IAH implementation contractor and 2009–2017 Medicare claims and enrollment data from the Chronic Conditions Warehouse for IAH beneficiaries in all IAH practices that participated in Year 5 and matched comparison beneficiaries.

Notes: We give standard errors in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers the matching and eligibility weights. The table reports the regression-adjusted means of the IAH and matched comparison groups in each year. We obtained regression-adjusted means by applying the estimated regression coefficients to the covariates of IAH beneficiaries in the latest demonstration year. We calculated percentage effects using the unadjusted IAH

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Exhibit B.4b. (continued)

group mean in the year before the demonstration. Because of rounding, a difference-in-differences estimate displayed as zero may be shown alongside a percentage effect that exceeds zero.

^a We used the unadjusted IAH group mean in the year before the demonstration to calculate the percentage effect for each demonstration year. Exhibit B.1 reports the baseline unadjusted IAH group mean for all outcomes.

^b We estimated a separate model using a single demonstration indicator (instead of separate indicators for each demonstration year) and used its interaction with IAH status to obtain an average annual estimated effect across five demonstration years.

^c The difference-in-differences estimate for the year before the demonstration is zero (with no standard error) in all regressions because we calculated that estimate for each year as the difference in means between IAH and comparison beneficiaries in that year minus the difference in the year before the demonstration.

*/**/** The difference is statistically significant at the 0.10/0.05/0.01 level.

PBPM = per beneficiary per month; SNF = skilled nursing facility.

Exhibit B.5a. Estimated effects of the IAH payment incentive on outcomes in Year 6 excluding one site at a time

Excluding site	Medicare expenditures per beneficiary per month		Hospice expenditures per beneficiary per month		Number of hospital admissions per beneficiary per year		Number of ED visits per beneficiary per year	
	Estimated effect	Percentage effect ^a	Estimated effect	Percentage effect ^a	Estimated effect	Percentage effect ^a	Estimated effect	Percentage effect ^a
A	-\$158 (\$148)	-3.7	\$27 (\$13)**	16.9	46 (73)	2.6	-123 (113)	-4.3
B	-\$85 (\$147)	-2.1	\$33 (\$16)**	20.5	31 (79)	1.9	-7 (76)	-0.3
C	-\$58 (\$128)	-1.4	\$31 (\$14)**	19.7	12 (67)	0.7	-93 (107)	-3.2
D	-\$55 (\$127)	-1.3	\$32 (\$14)**	20.1	23 (66)	1.3	-72 (107)	-2.5
E	-\$53 (\$130)	-1.3	\$29 (\$15)**	18.1	18 (67)	1.0	-77 (109)	-2.7
F	-\$45 (\$127)	-1.1	\$30 (\$14)**	18.4	21 (66)	1.2	-79 (107)	-2.7
G	-\$42 (\$124)	-1.0	\$26 (\$14)*	15.7	21 (67)	1.2	-83 (106)	-2.9
H	-\$40 (\$129)	-0.9	\$31 (\$14)**	18.9	23 (67)	1.4	-83 (108)	-2.9
I	-\$34 (\$126)	-0.8	\$29 (\$14)**	17.4	28 (66)	1.7	-77 (107)	-2.7
J	-\$31 (\$136)	-0.7	\$26 (\$17)	14.7	16 (55)	0.9	-64 (113)	-2.2
K	-\$25 (\$130)	-0.6	\$25 (\$15)*	15.8	10 (71)	0.6	-77 (112)	-2.7
L	-\$3 (\$132)	-0.1	\$29 (\$14)**	17.6	32 (67)	1.8	-60 (109)	-2.1
M	\$6 (\$127)	0.2	\$38 (\$14)***	24.9	17 (72)	1.0	-75 (112)	-2.6
N	\$25 (\$111)	0.6	\$26 (\$16)	16.0	59 (53)	3.5	-69 (113)	-2.4
Full sample	-\$41 (\$126)	-1.0	\$30** (\$14)	18.2	25 (66)	1.4	-73 (106)	-2.6

Source: Mathematica’s analysis of data from the IAH implementation contractor and 2009–2019 Medicare claims and enrollment data from the Chronic Conditions Warehouse for IAH beneficiaries in all IAH practices that participated in Year 6 and matched comparison beneficiaries.

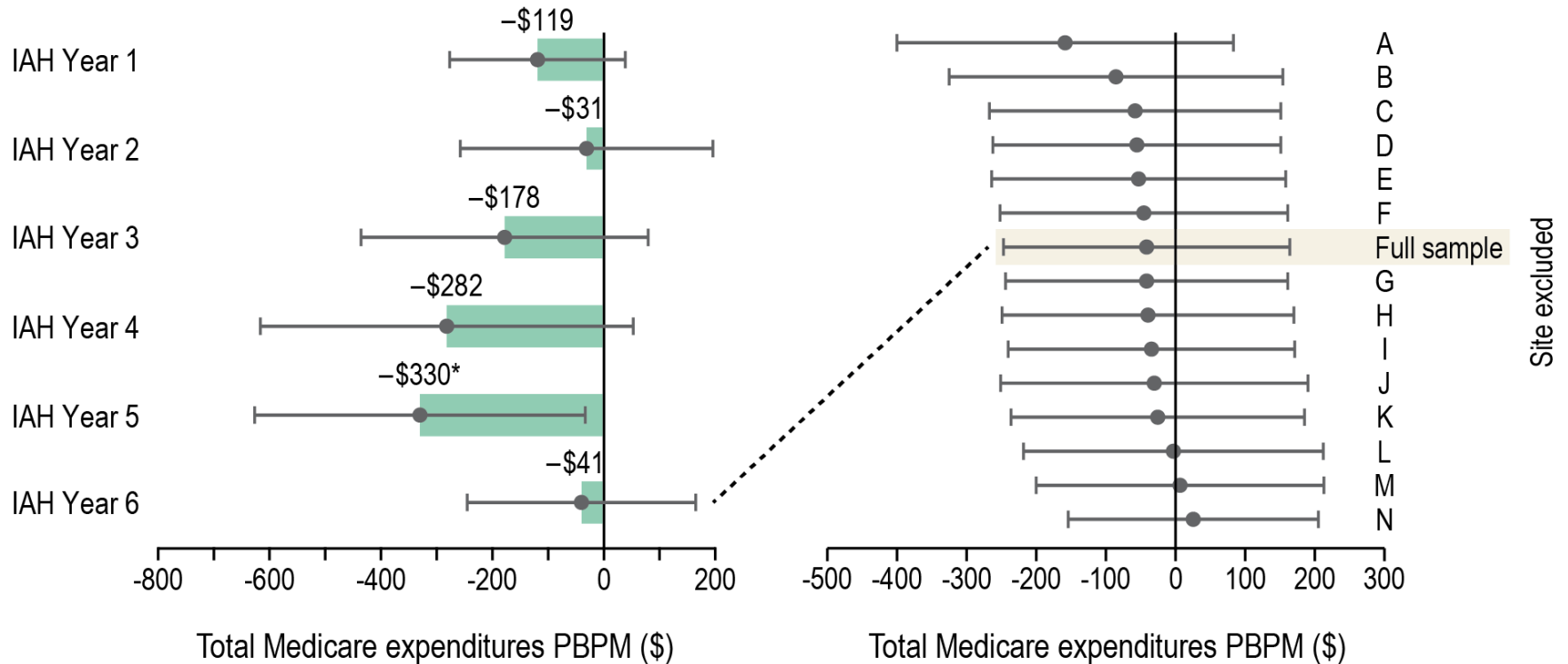
Notes: For each outcome in the table, we estimated 14 regressions (treating each of the three organizations in the Richmond-based consortium separately), with each regression excluding the IAH beneficiaries from one practice and their matched comparisons in all years. We give standard errors in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers the matching and eligibility weights.

^a We used the unadjusted IAH group mean in the year before the demonstration excluding the respective site to calculate the percentage effect for each demonstration year.

*/**/** The difference is statistically significant at the 0.10/0.05/0.01 level.

ED = emergency department.

Exhibit B.5b. Estimated effects of the IAH payment incentive on total Medicare expenditures in Year 6 excluding one site at a time



Source: Mathematica’s analysis of data from the IAH implementation contractor and 2009–2019 Medicare claims and enrollment data from the Chronic Conditions Warehouse for IAH beneficiaries in all IAH practices that participated in Year 6 and matched comparison beneficiaries.

Notes: The exhibit shows the estimated effect in each demonstration year in the left panel. The right panel shows estimates of the Year 6 effect after excluding one of the 14 IAH sites (treating each of the three organizations in the Richmond-based consortium separately) at a time. The horizontal lines represent 90 percent confidence intervals.

^a We used the unadjusted IAH group mean in the year before the demonstration excluding the respective site to calculate the percentage effect for each demonstration year.

*/**/** The difference is statistically significant at the 0.10/0.05/0.01 level.

ED = emergency department; PBPM = per beneficiary per month.

Exhibit B.6a. Estimated effect of the IAH payment incentive on total Medicare expenditures PBPM for IAH beneficiaries at practices that participated in Year 6, using beneficiary and practice weighting

	Beneficiary weighting		Practice weighting	
	Estimated effect	Percentage effect ^a	Estimated effect	Percentage effect ^a
Year 6	-\$41 (\$126)	-1.0	\$76 (\$166)	1.8
Year 5	-\$113 (\$157)	-2.7	-\$59 (\$143)	-1.4
Year 4	-\$19 (\$179)	-0.4	-\$59 (\$136)	-1.4
Year 3	\$52 (\$117)	1.3	\$21 (\$109)	0.5
Year 2	\$150 (\$122)	3.6	\$176 (\$116)	4.1
Year 1	\$101 (\$92)	2.4	-\$15 (\$138)	-0.4
One year pre-IAH ^b	.	-	-	-
Two years pre-IAH	-\$27 (\$64)	-0.6	\$22 (\$113)	0.5

Total unweighted number of observations across all years: 290,514

Source: Mathematica’s analysis of data from the IAH implementation contractor and 2009–2019 Medicare claims and enrollment data from the Chronic Conditions Warehouse for IAH beneficiaries in all IAH practices that participated in Year 6 and matched comparison beneficiaries.

Notes: We give standard errors in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers the matching and eligibility weights. The table reports the regression-adjusted means of the IAH and matched comparison groups in each year. We obtained regression-adjusted means by applying the estimated regression coefficients to the covariates of IAH beneficiaries in the latest demonstration year. Because of rounding, a difference-in-differences estimate displayed as zero may be shown alongside a percentage effect that exceeds zero. For information about the difference between beneficiary and practice weighting, see Appendix A, Section VI.

^a We used the unadjusted IAH group mean in the year before the demonstration to calculate the percentage effect for each demonstration year. Exhibit B.1 reports the baseline unadjusted IAH group mean for all outcomes.

^b The difference-in-differences estimate for the period before the demonstration is zero (with no standard error) in all regressions because we calculated that estimate for each year as the difference in means between IAH and comparison beneficiaries in that year minus the difference in the year before the demonstration.

*/**/** The difference is statistically significant at the 0.10/0.05/0.01 level.

PBPM = per beneficiary per month.

Exhibit B.6b. Estimated effect of the IAH payment incentive on total Medicare expenditures PBPM for IAH beneficiaries at practices that participated in Year 5, using beneficiary and practice weighting

	Beneficiary weighting		Practice weighting	
	Estimated effect	Percentage effect ^a	Estimated effect	Percentage effect ^a
Five-year average annual effect ^b	-\$200 (\$151)	-4.6	-\$86 (\$99)	-2.0
Year 5	-\$330* (\$182)	-7.5	-\$131 (\$131)	-3.0
Year 4	-\$282 (\$205)	-6.4	-\$179 (\$130)	-4.1
Year 3	-\$178 (\$158)	-4.1	-\$69 (\$102)	-1.6
Year 2	-\$31 (\$139)	-0.7	\$98 (\$110)	2.2
Year 1	-\$119 (\$97)	-2.7	-\$102 (\$125)	-2.3
One year pre-IAH ^c	-	-	-	-
Two years pre-IAH	-\$33 (\$57)	-0.8	-\$16 (\$109)	-0.4

Total unweighted number of observations across all years: 295,292

Source: Mathematica’s analysis of data from the IAH implementation contractor and 2009–2017 Medicare claims and enrollment data from the Chronic Conditions Warehouse for IAH beneficiaries in all IAH practices that participated in Year 5 and matched comparison beneficiaries.

Notes: We give standard errors in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers the matching and eligibility weights. The table reports the regression-adjusted means of the IAH and matched comparison groups in each year. We obtained regression-adjusted means by applying the estimated regression coefficients to the covariates of IAH beneficiaries in the latest demonstration year. Because of rounding, a difference-in-differences estimate displayed as zero may be shown alongside a percentage effect that exceeds zero.

^a We used the unadjusted IAH group mean in the year before the demonstration to calculate the percentage effect for each demonstration year. Exhibit B.1 reports the baseline unadjusted IAH group mean for all outcomes.

^b We estimated a separate model using a single demonstration indicator (instead of separate indicators for each demonstration year) and used its interaction with IAH status to obtain an average annual estimated effect across five demonstration years.

^c The difference-in-differences estimate for the period before the demonstration is zero (with no standard error) in all regressions because we calculated that estimate for each year as the difference in means between IAH and comparison beneficiaries in that year minus the difference in the year before the demonstration.

*/**/*** The difference is statistically significant at the 0.10/0.05/0.01 level.

PBPM = per beneficiary per month.

Exhibit B.7. Estimated effect of the IAH payment incentive on total Medicare expenditures PBPM in Year 6, with and without MIPS adjustments, practices that participated in Year 6

	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect ^a
Year 6 (including MIPS adjustments) ^b	\$4,725	\$5,137	-\$412 (\$108)	-\$41 (\$126)	-1.0
Year 6 (excluding MIPS adjustments)	\$4,721	\$5,133	-\$412 (\$108)	-\$41 (\$126)	-1.0

Source: Mathematica’s analysis of data from the IAH implementation contractor and 2009–2019 Medicare claims and enrollment data from the Chronic Conditions Warehouse for IAH beneficiaries in all IAH practices that participated in Year 6 and matched comparison beneficiaries.

Note: In 2019, Medicare began using MIPS. Under MIPS, CMS makes payment adjustments to individual providers nationally on the basis of quality and efficiency metrics.

^a We used the unadjusted IAH group mean in the year before the demonstration to calculate the percentage effect for each demonstration year. Exhibit B.1 reports the baseline unadjusted IAH group mean for all outcomes.

^b This result is the same as we reported in Exhibit B.2a.

CMS = Centers for Medicare & Medicaid Services; MIPS = Merit-based Incentive Payment System; PBPM = per beneficiary per month.

Exhibit B.8. Estimated effect of the IAH payment incentive on total Medicare expenditures PBPM in Year 6, with and without adjusting for ACO participation, practices that participated in Year 6

	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect ^a
Year 6, without controlling for ACO participation ^b	\$4,725	\$5,137	-\$412 (\$108)	-\$41 (\$126)	-1.0
Year 6, controlling for ACO participation	\$4,725	\$5,126	-\$401 (\$109)	-\$29 (\$124)	-0.7

Source: Mathematica’s analysis of data from the IAH implementation contractor and 2009–2019 Medicare claims and enrollment data from the Chronic Conditions Warehouse for IAH beneficiaries in all IAH practices that participated in Year 6 and matched comparison beneficiaries.

Note: ACOs are groups of providers that coordinate the care of an assigned Medicare population and are held financially accountable for the quality, cost, and experience of care they provide. In 2019, there were 487 ACO organizations with 10.4 million assigned Medicare beneficiaries.

^a We used the unadjusted IAH group mean in the year before the demonstration to calculate the percentage effect for each demonstration year. Exhibit B.1 reports the baseline unadjusted IAH group mean for all outcomes.

^b This result is the same as we reported in Exhibit B.2a.

ACO = accountable care organization; PBPM = per beneficiary per month.

Exhibit B.9a. Estimated effect of the IAH payment incentive on hospital care use in Years 1 to 6, practices that participated in Year 6

Outcome	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect ^a
Number of hospital admissions per beneficiary per year^b					
Year 6	1,761	2,076	-315	25 (66)	1.4
Year 5	1,765	2,141	-376	-36 (60)	-2.1
Year 4	1,631	1,950	-319	21 (74)	1.2
Year 3	1,738	2,066	-328	12 (66)	0.7
Year 2	1,761	2,064	-303	37 (57)	2.2
Year 1	1,793	2,085	-292	48 (39)	2.8
One year pre-IAH ^c	1,850	2,190	-340	-	-
Two years pre-IAH	1,945	2,288	-344	-4 (31)	-0.2
Total number of ED visits per beneficiary per year^d					
Year 6	3,176	3,395	-218	-73 (106)	-2.6
Year 5	3,134	3,387	-252	-107 (69)	-3.7
Year 4	2,945	3,164	-219	-74 (74)	-2.6
Year 3	3,129	3,321	-193	-48 (78)	-1.7
Year 2	3,041	3,123	-82	63 (65)	2.2
Year 1	2,922	3,090	-168	-23 (78)	-0.8
One year pre-IAH ^c	3,031	3,176	-145	-	-
Two years pre-IAH	3,074	3,238	-164	-19 (57)	-0.7
Number of ED visits resulting in hospital admission per beneficiary per year					
Year 6	1,421	1,598	-178	-33 (61)	-2.3
Year 5	1,454	1,679	-225	-81 (73)	-5.7
Year 4	1,318	1,513	-195	-51 (72)	-3.6
Year 3	1,421	1,615	-194	-49 (60)	-3.5
Year 2	1,437	1,589	-153	-8 (57)	-0.6
Year 1	1,489	1,639	-150	-6 (39)	-0.4
One year pre-IAH ^c	1,567	1,712	-144	-	-
Two years pre-IAH	1,668	1,805	-137	8 (29)	0.6
Number of outpatient ED visits per beneficiary per year^e					
Year 6	1,755	1,797	-42	-25 (102)	-1.8
Year 5	1,671	1,717	-46	-29 (100)	-2.0
Year 4	1,613	1,642	-30	-13 (78)	-0.9
Year 3	1,695	1,698	-3	13 (71)	0.9
Year 2	1,612	1,542	70	86 (56)	6.0
Year 1	1,440	1,459	-19	-2 (57)	-0.2
One year pre-IAH ^c	1,467	1,484	-16	-	-
Two years pre-IAH	1,436	1,464	-28	-12 (44)	-0.8
Total unweighted number of observations across all years: 290,514					

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Exhibit B.9a. (continued)

Source: Mathematica's analysis of data from the IAH implementation contractor and 2009–2019 Medicare claims and enrollment data from the Chronic Conditions Warehouse for IAH beneficiaries in all IAH practices that participated in Year 6 and matched comparison beneficiaries.

Notes: We give standard errors in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers the matching and eligibility weights. The table reports the regression-adjusted means of the IAH and matched comparison groups in each year. We obtained regression-adjusted means by applying the estimated regression coefficients to the covariates of IAH beneficiaries in the latest demonstration year. Because of rounding, a difference-in-differences estimate displayed as zero may be shown alongside a percentage effect that exceeds zero.

^a We used the unadjusted IAH group mean in the year before the demonstration to calculate the percentage effect for each demonstration year. Exhibit B.1 reports the baseline unadjusted IAH group mean for all outcomes.

^b The number of hospital admissions includes observation stays.

^c The difference-in-differences estimate for the year before the demonstration is zero (with no standard error) in all regressions because we calculated that estimate for each year as the difference in means between IAH and comparison beneficiaries in that year minus the difference in the year before the demonstration.

^d The measure includes outpatient ED visits and ED visits resulting in hospital admission.

^e The number of outpatient ED visits reflects all those not resulting in a hospital admission, including those resulting in an observation stay.

*/**/** The difference is statistically significant at the 0.10/0.05/0.01 level.

ED = emergency department.

Exhibit B.9b. Estimated effect of the IAH payment incentive on hospital care use in Years 1 to 5, practices that participated in Year 5

Outcome	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect ^a
Number of hospital admissions per beneficiary per year^b					
Five-year average annual effect ^c	1,722	2,003	-281	-81 (50)	-4.6
Year 5	1,785	2,118	-332	-129** (62)	-7.2
Year 4	1,630	1,935	-304	-101 (71)	-5.7
Year 3	1,761	2,049	-287	-84 (64)	-4.7
Year 2	1,786	2,020	-234	-30 (52)	-1.7
Year 1	1,828	2,081	-252	-49 (36)	-2.8
One year pre-IAH ^d	1,898	2,102	-203	-	-
Two years pre-IAH	1,979	2,216	-237	-33 (29)	-1.9
Total number of ED visits per beneficiary per year^e					
Five-year average annual effect ^c	2,982	3,142	-159	-142*** (55)	-4.9
Year 5	3,143	3,361	-218	-201** (79)	-6.9
Year 4	2,922	3,143	-221	-205** (80)	-7.1
Year 3	3,122	3,292	-170	-153* (85)	-5.3
Year 2	3,059	3,083	-24	-7 (64)	-0.3
Year 1	2,956	3,089	-133	-116 (71)	-4.0
One year pre-IAH ^d	3,078	3,095	-16	-	-
Two years pre-IAH	3,116	3,159	-43	-26 (49)	-0.9
Number of ED visits resulting in hospital admission per beneficiary per year					
Five-year average annual effect ^c	1,379	1,525	-145	-111** (45)	-7.7
Year 5	1,452	1,636	-184	-149** (62)	-10.4
Year 4	1,298	1,473	-175	-141** (61)	-9.8
Year 3	1,410	1,572	-162	-127** (49)	-8.8
Year 2	1,427	1,527	-99	-65 (45)	-4.5
Year 1	1,499	1,603	-104	-69** (31)	-4.8
One year pre-IAH ^d	1,573	1,608	-34	-	-
Two years pre-IAH	1,661	1,711	-49	-14 (26)	-1.0
Number of outpatient ED visits per beneficiary per year^f					
Five-year average annual effect ^c	1,599	1,616	-17	-23 (59)	-1.6
Year 5	1,683	1,732	-48	-55 (99)	-3.8
Year 4	1,613	1,662	-49	-56 (80)	-3.9
Year 3	1,701	1,711	-9	-17 (72)	-1.2
Year 2	1,639	1,561	78	70 (54)	4.9
Year 1	1,461	1,488	-27	-34 (53)	-2.4

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Exhibit B.9b. (continued)

Outcome	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect ^a
One year pre-IAH ^d	1,503	1,496	7	-	-
Two years pre-IAH	1,475	1,470	4	-2 (37)	-0.2
Total unweighted number of observations across all years: 295,292					

Source: Mathematica's analysis of data from the IAH implementation contractor and 2009–2017 Medicare claims and enrollment data from the Chronic Conditions Warehouse for IAH beneficiaries in all IAH practices that participated in Year 5 and matched comparison beneficiaries.

Notes: We give standard errors in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers the matching and eligibility weights. The table reports the regression-adjusted means of the IAH and matched comparison groups in each year. We obtained regression-adjusted means by applying the estimated regression coefficients to the covariates of IAH beneficiaries in the latest demonstration year. Because of rounding, a difference-in-differences estimate displayed as zero may be shown alongside a percentage effect that exceeds zero.

^a We used the unadjusted IAH group mean in the year before the demonstration to calculate the percentage effect for each demonstration year. Exhibit B.1 reports the baseline unadjusted IAH group mean for all outcomes.

^b The number of hospital admissions includes observation stays.

^c We estimated a separate model using a single demonstration indicator (instead of separate indicators for each demonstration year) and used its interaction with IAH status to obtain an average annual estimated effect across five demonstration years.

^d The difference-in-differences estimate for the year before the demonstration is zero (with no standard error) in all regressions because we calculated that estimate for each year as the difference in means between IAH and comparison beneficiaries in that year minus the difference in the year before the demonstration.

^e The measure includes outpatient ED visits and visits resulting in hospital admission.

^f The number of outpatient ED visits reflects all those not resulting in a hospital admission, including those resulting in an observation stay.

*/**/** The difference is statistically significant at the 0.10/0.05/0.01 level.

ED = emergency department.

Exhibit B.10a. Estimated effect of the IAH payment incentive on potentially avoidable hospital admissions and outpatient ED visits, and probability of unplanned readmission in Years 1 to 6, practices that participated in Year 6

Outcome	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect ^a
Number of potentially avoidable hospital admissions per beneficiary per year^b					
Year 6	385	454	-69	-8 (27)	-1.9
Year 5	454	551	-97	-36 (24)	-8.3
Year 4	366	463	-98	-37 (25)	-8.4
Year 3	397	488	-92	-31 (21)	-7.0
Year 2	430	479	-49	12 (22)	2.6
Year 1	450	509	-58	3 (14)	0.6
One year pre-IAH ^c	472	533	-61	-	-
Two years pre-IAH	500	580	-80	-19 (15)	-4.3
Number of potentially avoidable outpatient ED visits per beneficiary per year^d					
Year 6	254	254	0	17 (17)	9.0
Year 5	244	260	-16	1 (12)	0.5
Year 4	215	246	-31	-14 (13)	-7.5
Year 3	220	230	-10	6 (17)	3.5
Year 2	211	215	-3	13 (13)	7.1
Year 1	193	200	-8	9 (13)	5.0
One year pre-IAH ^c	194	211	-17	-	-
Two years pre-IAH	200	202	-2	15 (14)	8.0
Probability (as a percentage) of having a qualifying hospital discharge and unplanned readmission within 30 days of discharge					
Year 6	15.91	18.17	-2.26	-0.02 (1.05)	-0.1
Year 5	15.94	18.56	-2.62	-0.38 (1.20)	-2.4
Year 4	13.71	16.23	-2.52	-0.28 (1.15)	-1.8
Year 3	16.06	19.06	-3.00	-0.76 (0.94)	-4.9
Year 2	16.67	18.71	-2.04	0.20 (1.09)	1.3
Year 1	17.64	18.74	-1.10	1.13 (0.81)	7.2
One year pre-IAH ^c	17.47	19.70	-2.24	-	-
Two years pre-IAH	20.00	21.90	-1.90	0.34 (0.68)	2.2

Total number of observations across the five years: 290,514

Source: Mathematica’s analysis of data from the IAH implementation contractor and 2009–2019 Medicare claims and enrollment data from the Chronic Conditions Warehouse for IAH beneficiaries in all IAH practices that participated in Year 6 and matched comparison beneficiaries.

Notes: We define a potentially avoidable hospital admission (or outpatient ED visit) as one in which appropriate primary and specialty care may prevent or reduce the need for a hospital admission (or ED visit). We give standard errors in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers the matching and eligibility weights. The table reports the regression-adjusted means of the IAH and matched comparison groups in each year. We obtained regression-adjusted means by applying the estimated regression coefficients to the covariates of IAH beneficiaries in the latest

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Exhibit B.10a (*continued*)

demonstration year. Because of rounding, a difference-in-differences estimate displayed as zero may be shown alongside a percentage effect that exceeds zero.

^a We used the unadjusted IAH group mean in the year before the demonstration to calculate the percentage effect for each demonstration year. Exhibit B.1 reports the baseline unadjusted IAH group mean for all outcomes.

^b The number of hospital admissions includes observation stays.

^c The difference-in-differences estimate for the period before the demonstration is zero (with no standard error) in all regressions because we calculated that estimate for each year as the difference in means between IAH and comparison beneficiaries in that year minus the difference in the year before the demonstration.

^d The number of outpatient ED visits reflects those not resulting in hospital admission, including those resulting in an observation stay. The measure excluded ED visits that led to a hospital admission because there was no diagnosis from such a visit in a claim record when it led to a hospital admission.

*/**/** The difference is statistically significant at the 0.10/0.05/0.01 level.

ED = emergency department.

Exhibit B.10b. Estimated effect of the IAH payment incentive on potentially avoidable hospital admissions and outpatient ED visits, and probability of unplanned readmission in Years 1 to 5, practices that participated in Year 5

Outcome	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect ^a
Number of potentially avoidable hospital admissions per beneficiary per year^b					
Five-year average annual effect ^c	425	494	-68	-36** (16)	-8.1
Year 5	457	544	-87	-55** (24)	-12.1
Year 4	361	452	-90	-58** (23)	-12.8
Year 3	412	481	-68	-36* (18)	-7.9
Year 2	433	470	-37	-5 (19)	-1.1
Year 1	463	508	-45	-13 (12)	-2.8
One year pre-IAH ^d	487	519	-32	-	-
Two years pre-IAH	510	569	-58	-26* (14)	-5.8
Number of potentially avoidable outpatient ED visits per beneficiary per year^e					
Five-year average annual effect ^c	220	232	-12	-1 (9)	-0.7
Year 5	245	263	-18	-7 (13)	-3.6
Year 4	214	243	-29	-18 (13)	-9.3
Year 3	223	229	-6	5 (14)	2.6
Year 2	223	216	6	18 (11)	9.5
Year 1	198	206	-7	4 (11)	1.9
One year pre-IAH ^d	202	213	-11	-	-
Two years pre-IAH	203	208	-4	6 (13)	3.5
Probability (as a percentage) of having a qualifying hospital discharge and unplanned readmission within 30 days of discharge					
Five-year average annual effect ^c	15.39	17.32	-1.92	-1.13 (0.78)	-7.0
Year 5	15.86	18.14	-2.29	-1.47 (1.05)	-9.2
Year 4	13.65	15.97	-2.32	-1.50 (0.98)	-9.4
Year 3	16.18	18.70	-2.52	-1.70** (0.77)	-10.6
Year 2	16.32	18.14	-1.82	-1.00 (0.98)	-6.3
Year 1	17.93	18.63	-0.70	0.12 (0.66)	0.7
One year pre-IAH ^d	17.78	18.60	-0.82	-	-
Two years pre-IAH	19.88	20.89	-1.02	-0.20 (0.66)	-1.3
Total number of observations across the five years: 295,292					

Source: Mathematica’s analysis of data from the IAH implementation contractor and 2009–2017 Medicare claims and enrollment data from the Chronic Conditions Warehouse for IAH beneficiaries in all IAH practices that participated in Year 5 and matched comparison beneficiaries.

Notes: We define a potentially avoidable hospital admission (or outpatient ED visit) as one in which appropriate primary and specialty care may prevent or reduce the need for a hospital admission (or ED visit). We give standard errors in parentheses. We computed coefficients and standard errors by using the weighted

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Exhibit B.10b (*continued*)

sample size, which considers the matching and eligibility weights. The table reports the regression-adjusted means of the IAH and matched comparison groups in each year. We obtained regression-adjusted means by applying the estimated regression coefficients to the covariates of IAH beneficiaries in the latest demonstration year. Because of rounding, a difference-in-differences estimate displayed as zero may be shown alongside a percentage effect that exceeds zero.

^a We used the unadjusted IAH group mean in the year before the demonstration to calculate the percentage effect for each demonstration year. Exhibit B.1 reports the baseline unadjusted IAH group mean for all outcomes.

^b The number of hospital admissions includes observation stays.

^c We estimated a separate model using a single demonstration indicator (instead of separate indicators for each demonstration year) and used its interaction with IAH status to obtain an average annual estimated effect across five demonstration years.

^d The difference-in-differences estimate for the period before the demonstration is zero (with no standard error) in all regressions because we calculated that estimate for each year as the difference in means between IAH and comparison beneficiaries in that year minus the difference in the year before the demonstration.

^e The number of outpatient ED visits reflects those not resulting in hospital admission, including those resulting in an observation stay. The measure excluded ED visits that led to a hospital admission because there was no diagnosis from such a visit in a claim record when it led to a hospital admission.

//*** The difference is statistically significant at the 0.10/0.05/0.01 level.

ED = emergency department.

Exhibit B.11a. Estimated effect of the IAH payment incentive on probability (as a percentage) of dying within the demonstration year in Years 1 to 6, practices that participated in Year 6

	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect ^a
Year 6	15.17	21.11	-5.94	0.56 (0.86)	3.8
Year 5	14.97	21.92	-6.95	-0.45 (0.69)	-3.1
Year 4	14.82	21.94	-7.11	-0.61 (0.70)	-4.2
Year 3	16.51	21.24	-4.73	1.77** (0.71)	12.1
Year 2	15.74	21.31	-5.57	0.93 (0.75)	6.4
Year 1	16.23	22.01	-5.78	0.72 (0.62)	4.9
One year pre-IAH ^b	16.32	22.82	-6.50	-	-
Two years pre-IAH	15.61	23.75	-8.14	-1.64** (0.79)	-11.2

Total unweighted number of observations across all years: 290,514

Source: Mathematica’s analysis of data from the IAH implementation contractor and 2009–2019 Medicare claims and enrollment data from the Chronic Conditions Warehouse for IAH beneficiaries in all IAH practices that participated in Year 6 and matched comparison beneficiaries.

Notes: We give standard errors in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers the matching and eligibility weights. The table reports the regression-adjusted means of the IAH and matched comparison groups in each year. Regression-adjusted means are obtained by applying the estimated regression coefficients to the covariates of IAH beneficiaries in the latest demonstration year. Because of rounding, a difference-in-differences estimate displayed as zero may be shown alongside a percentage effect that exceeds zero.

^a We used the unadjusted IAH group mean in the year before the demonstration to calculate the percentage effect for each demonstration year. Exhibit B.1 reports the baseline unadjusted IAH group mean for all outcomes.

^b The difference-in-differences estimate for the year before the demonstration is zero (with no standard error) in all regressions because we calculated that estimate for each year as the difference in means between IAH and comparison beneficiaries in that year minus the difference in the year before the demonstration.

*/**/** The difference is statistically significant at the 0.10/0.05/0.01 level.

Exhibit B.11b. Estimated effect of the IAH payment incentive on probability (as a percentage) of dying within the demonstration year in Years 1 to 5, practices that participated in Year 5

	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect ^a
Five-year average annual effect ^b	13.90	19.53	-5.63	-0.32 (0.55)	-2.2
Year 5	13.90	20.58	-6.68	-1.20* (0.70)	-8.1
Year 4	13.74	20.52	-6.77	-1.29** (0.63)	-8.7
Year 3	15.43	19.88	-4.44	1.04 (0.68)	7.0
Year 2	14.71	19.86	-5.15	0.33 (0.67)	2.2
Year 1	15.36	20.83	-5.47	0.02 (0.56)	0.1
One year pre-IAH ^c	15.54	21.03	-5.48	-	-
Two years pre-IAH	14.94	22.05	-7.11	-1.62*** (0.64)	-10.9

Total unweighted number of observations across all years: 295,292

Source: Mathematica’s analysis of data from the IAH implementation contractor and 2009–2017 Medicare claims and enrollment data from the Chronic Conditions Warehouse for IAH beneficiaries in all IAH practices that participated in Year 5 and matched comparison beneficiaries.

Notes: We give standard errors in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers the matching and eligibility weights. The table reports the regression-adjusted means of the IAH and matched comparison groups in each year. Regression-adjusted means are obtained by applying the estimated regression coefficients to the covariates of IAH beneficiaries in the latest demonstration year. Because of rounding, a difference-in-differences estimate displayed as zero may be shown alongside a percentage effect that exceeds zero.

^a We used the unadjusted IAH group mean in the year before the demonstration to calculate the percentage effect for each demonstration year. Exhibit B.1 reports the baseline unadjusted IAH group mean for all outcomes.

^b We estimated a separate model using a single demonstration indicator (instead of separate indicators for each demonstration year) and used its interaction with IAH status to obtain an average annual estimated effect across five demonstration years.

^c The difference-in-differences estimate for the year before the demonstration is zero (with no standard error) in all regressions because we calculated that estimate for each year as the difference in means between IAH and comparison beneficiaries in that year minus the difference in the year before the demonstration.

*/**/** The difference is statistically significant at the 0.10/0.05/0.01 level.

Exhibit B.12. Estimated effect of the IAH payment incentive on probability (as a percentage) of entering institutional long-term care within the demonstration year in Years 1 to 5, practices that participated in Year 5

	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect ^a
Five-year average annual effect ^b	6.75	12.78	-6.03	0.49 (0.40)	7.0
Year 5	6.70	12.89	-6.20	0.87 (0.62)	12.4
Year 4	6.51	12.78	-6.26	0.15 (0.51)	2.1
Year 3	7.44	13.26	-5.82	0.59 (0.49)	8.4
Year 2	7.10	12.76	-5.66	0.75 (0.47)	10.7
Year 1	7.46	13.49	-6.03	0.38 (0.47)	5.5
One year pre-IAH ^c	7.88	14.29	-6.41	-	-
Two years pre-IAH	8.03	15.14	-7.11	-0.70 (0.49)	-10.1

Total unweighted number of observations across all years: 295,292

Source: Mathematica’s analysis of data from the IAH implementation contractor and 2009–2017 Medicare claims and enrollment data from the Chronic Conditions Warehouse for IAH beneficiaries in all IAH practices that participated in Year 5 and matched comparison beneficiaries.

Notes: Data on entry into institutional long-term care were available through Year 5 only, so we do not report results for Year 6. We give standard errors in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers the matching and eligibility weights. The table reports the regression-adjusted means of the IAH and matched comparison groups in each year. We obtained regression-adjusted means by applying the estimated regression coefficients to the covariates of IAH beneficiaries in the latest demonstration year. Because of rounding, a difference-in-differences estimate displayed as zero may be shown alongside a percentage effect that exceeds zero.

^a We used the unadjusted IAH group mean in the year before the demonstration to calculate the percentage effect for each demonstration year. Exhibit B.1 reports the baseline unadjusted IAH group mean for all outcomes.

^b We estimated a separate model using a single demonstration indicator (instead of separate indicators for each demonstration year) and used its interaction with IAH status to obtain an average annual estimated effect across four demonstration years.

^c The difference-in-differences estimate for the year before the demonstration is zero (with no standard error) in all regressions because we calculated that estimate for each year as the difference in means between IAH and comparison beneficiaries in that year minus the difference in the year before the demonstration.

*/**/** The difference is statistically significant at the 0.10/0.05/0.01 level.

Appendix C

**Effects of home-based primary care for dually eligible beneficiaries:
technical appendix and supplemental exhibits**

I. Overview

This appendix provides details on the data, methods, and results for our analysis of the effect of home-based primary care among dually eligible beneficiaries. Section II presents our approach to identifying our sample, Section III describes the data and outcomes, Section IV includes our estimation methods, and Section V includes supplementary exhibits.

II. Identifying the sample

A. Medicare sample criteria

For the prior Medicare analysis, we constructed panels of new entrants into home-based primary care, with each panel corresponding to calendar years 2010 to 2014. For example, Panel 1 included home-based primary care recipients who met the Independence at Home (IAH) eligibility criteria and entered home-based primary care in 2010 as well as their matched comparisons. We matched comparison beneficiaries included in a given panel with one or more home-based primary care recipients based on their characteristics and health status as of some point during the calendar year. Although the Medicare panels we constructed cover the years 2010 to 2014, we had to limit our analyses of dually eligible beneficiaries to the 2010 and 2011 panels because of the availability of Medicaid data (described further in Section II.B of this appendix). Here, we describe the Medicare panel sample criteria.

1. Comparison beneficiary versions

For beneficiaries new to home-based primary care, the date of their index home-based primary care visit is an unambiguous anchor date from which to look back to the pre-intervention period and forward to the post-intervention period. Comparison beneficiaries, because they have no home-based primary care visit, do not have an analogous anchor date. We had to specify a date to construct matching variables, which are based on a period of a specific length looking backward, and outcomes variables, which are based on a period of specific length looking forward. Because the population of interest consists of beneficiaries who recently suffered a health shock, a given beneficiary's health status could deteriorate quickly from one month to the next. Our goal was to match treated beneficiaries with comparison beneficiaries who were at the same point in their health status trajectory. To do so, we constructed up to 12 versions of each potential comparison beneficiary using the first day of each month in the calendar year as the anchor date for the period looking back. We use the term start date to refer to this anchor date (for beneficiaries in the home-based primary care group, the start date is the date of the first home-based primary care visit). Each version of a given comparison beneficiary served as a distinct potential match in our matching process, and we used propensity score matching to determine the final matched comparison group (described in Section II.C of this appendix).

2. Sample selection criteria

We applied several Medicare sample criteria to identify the home-based primary care group and potential comparison group for the Medicare analysis of home-based primary care (Exhibit C.1). We applied these same Medicare sample criteria for the dually eligible subgroup.

Fee for service (FFS). Beneficiaries had to satisfy the FFS criteria for each of the 24 months before the start date. Their status in the Medicare Enrollment Database had to indicate that they were alive, were not

in Medicare Advantage or the Program of All-Inclusive Care for the Elderly, had Part A and Part B coverage, and had Medicare as their primary payer.

Two or more chronic conditions. We counted the chronic condition flags from the Chronic Conditions Warehouse to determine whether a beneficiary had two or more chronic conditions. For beneficiaries who had a start date in the first half of the panel year (for example, 2010), we used the Chronic Conditions Warehouse end-of-year flag from the year before the panel year (in this case, 2009). For beneficiaries who had a start date in the second half of the panel year, we used the midyear flag from the panel year.

Inpatient hospital admission or observation stay and use of acute or subacute rehabilitation services within 12 months of the start date. We used inpatient, outpatient, home health, and skilled nursing facility (SNF) claims data to identify Medicare FFS beneficiaries who met these eligibility criteria.

Exclude beneficiaries currently in hospice care. We did not include beneficiaries who used hospice within 30 days before the start date.

Determine whether beneficiaries have two or more activities of daily living (ADLs) for which they need human assistance. Following the approach developed for the Medicare panel analyses—modified from the process used in Faurot et al. (2015)—beneficiaries were identified as having two or more ADLs for which they needed human assistance based on a claims-based model for ADL prediction that relied on Medicare claims. The evaluation report for IAH demonstration Years 1 to 4 describes further details about the approach and model performance.⁴⁷

Home-based primary care. Home-based primary care was defined as a primary care provider visit in the home or assisted living facility with a billing code for an evaluation and management visit. A list of codes and the approach used to identify home-based primary care are available in the evaluation report for IAH demonstration Years 1 to 4.⁴⁸ Primary care providers include certain physicians, nurse practitioners, and physician assistants. Primary care physicians are those from the following specialties: general practice, family practice, internal medicine, geriatric medicine, and preventive medicine. Beneficiaries in the home-based primary care and potential comparison groups must not have had any home-based primary care visits in the two years before the start date.

Additional criteria for beneficiaries in the home-based primary care group. To be retained in the home-based primary care group, beneficiaries must have had at least two home-based primary care visits in the six-month period after the first home visit, including the first visit. In addition, most evaluation and management visits from a primary care provider during that period must have taken place in the home or assisted living facility. These restrictions ensure that the dominant mode of primary care for beneficiaries in the home-based primary care group was home based. We identified the subset of beneficiaries in the home-based primary care group who received the plurality of their care from an IAH practice; these beneficiaries are considered IAH attributed. The sets of ZIP codes in which IAH-attributed beneficiaries reside defined the IAH catchment areas. Finally, we retained all beneficiaries in the home-based primary care group who resided in an IAH catchment area, including those not attributed to an IAH practice.

Additional criteria for potential comparison beneficiaries. To be retained in the potential comparison group, beneficiaries must not have had any home-based primary care visit in the six-month period after

⁴⁷ For more information, refer to the [evaluation report](#) which covered Years 1 to 4 of the IAH demonstration.

⁴⁸ For more information, refer to the [evaluation report](#) which covered Years 1 to 4 of the IAH demonstration.

the start date (defined as the first day of each month in the calendar year). In addition, potential comparison beneficiaries must have resided in an IAH catchment area as of the start date.

Intent to treat. We monitored home-based primary care recipients and comparison beneficiaries for use in the first six months after the start date, as we described. The intent-to-treat design retained beneficiaries in the home-based care recipient group even if they eventually stopped using home-based primary care after the first six months. Similarly, those in the comparison group remained in the comparison group even if they began to receive primary care in the home after the first six months.

Exhibit C.1. Overview of Medicare sample inclusion criteria

All beneficiaries in the final sample:

- Had continuous Medicare fee-for-service status that began two years before the start date
- Lived in an Independence at Home catchment area as of the beginning of the panel year
- Had two or more chronic conditions as of the start date
- Had two or more activities of daily living requiring human assistance as of the start date
- Had an inpatient hospital admission or observation stay within 12 months of the start date
- Used acute or subacute rehabilitation services within 12 months of the start date
- Were not in hospice within 30 days of the start date
- Had no home-based primary care visits within two years before the start date

Home-based primary care beneficiaries in the final sample:

- Had two or more home-based primary care visits during the six-month period starting with (and including) the start home visit
- Received most evaluation and management visits in the home or and assisted living facility during the six-month period starting on the start date (and including the start visit)

Comparison beneficiaries in the final sample

- Had no home visits during the six-month period starting on the start date

B. Medicaid sample criteria

We used Medicaid data to verify sample inclusion for our subgroup analysis of dually eligible beneficiaries. Because of the availability of Medicaid data during our analysis and our requirement that states could only be included in the sample if there was a minimum of one extra quarter of data beyond the end of the calendar year (to capture lags in claim submissions), we were able to examine outcomes through 2012 for all the sample states. We were, however, only able to examine outcomes through 2013 for eight of the sample states (Massachusetts, Michigan, New York, Ohio, Oregon, Pennsylvania, Texas, and Virginia). These years covered the two-year post-intervention period for the 2010 and 2011 panels, so we could include only these two panels in our analysis of dually eligible beneficiaries.

To be included in our analysis of dually eligible beneficiaries, a beneficiary must have met all Medicare sample criteria and met the dual eligibility criteria based on Medicaid data (listed below). Although we applied the Medicare criteria for the 24 months before the start date (described in Section II.A of this

appendix), we applied the Medicaid criteria for just the 12 months before the start date to increase the sample size. By relaxing the Medicaid criteria from 24 to 12 months before the start date, we increased sample sizes for this subgroup analysis by 12 to 14 percent. Using this requirement, we had a smaller baseline period window to identify the matched comparison group using Medicaid data, but we still used two years of Medicare data to identify the matched comparison group. After applying the Medicare sample criteria, all beneficiaries in the final sample met the following Medicaid sample criteria:

- They had the same unique Medicaid identifier across all years.⁴⁹
- They lived in a sample state for all years of the analysis.⁵⁰
- They had matching state codes in the Medicare and Medicaid data.
- They were eligible for full Medicaid benefits for all 12 months before starting home-based primary care and during the first month of starting home-based primary care (or the start month).⁵¹
- They resided in the community as of the start date. We dropped from the analysis any beneficiaries who were in a SNF, a nursing facility, or an inpatient setting on the start date or the day after to focus on beneficiaries eligible to receive home-based primary care.⁵² This criterion was important for the dually eligible sample to ensure the home-based primary care recipients and potential comparison beneficiaries did not differentially use institutions.

Exhibit C.2 displays the pre-intervention and post-intervention years for each group in relation to the Medicare and Medicaid sample criteria and identification of home-based primary care.

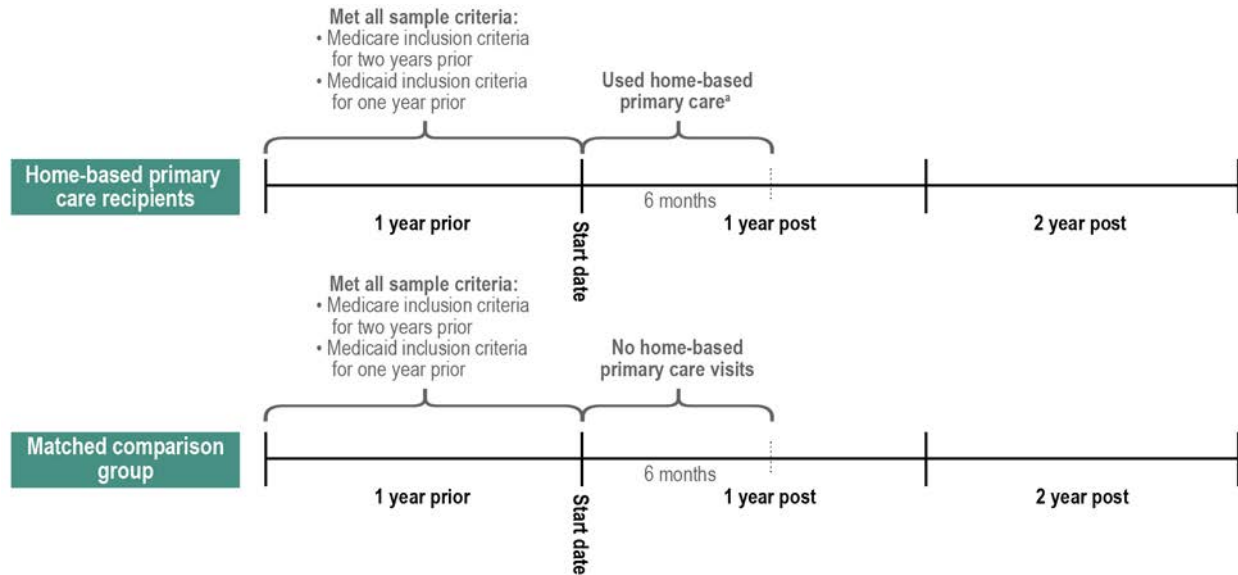
⁴⁹ Medicaid identifiers can change over time, so we required the same unique identifier across all years to ensure that we could follow the same beneficiary over time.

⁵⁰ The relevant sample states differed for the 2010 and 2011 panels, so we applied this criterion differently for each panel. This exclusion was critical to apply at a panel level because of the Medicaid data availability limitations across states. For the 2010 panel, the sample states included all IAH states, but for the 2011 panel, the sample states included only eight (Massachusetts, Michigan, New York, Ohio, Oregon, Pennsylvania, Texas, and Virginia).

⁵¹ For beneficiaries who lost full Medicaid benefits during the post-intervention period, we constructed outcome measures only through the month in which the beneficiary was eligible for full Medicaid benefits. After beneficiaries lost full Medicaid benefits, they did not rejoin the sample, even if they regained benefits in a later month of the analysis period. We annualized all outcomes.

⁵² To identify community residence, we used the Timeline file. This file combines data from claims (inpatient, SNF, and home health) and assessment data from the Outcome and Assessment Information Set and the Minimum Data Set to flag a beneficiary's residency status for each day of a calendar year. The daily residency flag can contain one of the following values: I (inpatient), S (SNF), M (Minimum Data Set, which includes nursing home days not paid by Medicare), C (community, which includes days identified by home health claims or Outcome and Assessment Information Set assessment data and days with no claims or assessment data), D (dead), or blank (not Medicare eligible). We considered all days flagged with a C, H, O, or that were blank to be community days.

Exhibit C.2. Pre- and post-intervention years for home-based primary care recipients and comparison beneficiaries



Note: Medicare and Medicaid sample criteria are described in Section II.A and II.B of this appendix.

^a During the first six months of the post period, beneficiaries had at least two home-based primary care visits and most of their evaluation and management visits from a primary care clinician occurred in the home.

Exhibit C.3 shows the number of beneficiaries from the 2010 and 2011 Medicare panels who met all Medicare criteria described previously and the number of beneficiaries dropped for each additional Medicaid sample exclusion. Our resulting samples included 708 beneficiaries in the home-based primary care group and 98,582 potential comparison beneficiaries from the 2010 panel. It also included 619 beneficiaries in the home-based primary care group and 88,806 potential comparison beneficiaries from the 2011 panel.

Exhibit C.3. Medicaid sample exclusions for 2010 and 2011 dually eligible beneficiary subgroups

Exclusions	2010 home-based primary care group	2010 potential comparison group	2011 home-based primary care group	2011 potential comparison group
Number of records from Medicare panel file before matching^a	6,116	899,400	6,552	919,533
Not in Medicaid Person Summary (enrollment) file	4,058	520,098	4,467	527,332
Not in included state	38	7,072	501	106,762
Multiple Medicaid IDs	52	7,164	36	5,347
Did not have full Medicaid benefits for all 12 months before start date	1,070	170,326	780	118,693
Medicaid state not the same as Medicare state in enrollment files	1	631	2	574
Did not reside in community on start date	55	95,039	37	71,658
Did not have full Medicaid benefits in start month	11	488	14	361
Had only one home-based primary care visit (home-based primary care group only)	123	n.a.	96	n.a.
Final sample before matching	708	98,582	619	88,806

Source: Mathematica’s analysis of Medicare and Medicaid claims, assessment, and enrollment data for 2009 to 2013 obtained from the Chronic Conditions Warehouse for home-based primary care recipients and matched comparison group beneficiaries.

^a We describe the number of beneficiaries in the home-based primary care group and potential comparison beneficiary versions who met the Medicare sample inclusion criteria in Section II.A of this appendix. For the analysis of dually eligible beneficiaries, we used this set of records to apply Medicaid sample inclusion criteria to define our samples for matching.

n.a. = not applicable.

C. Propensity score matching

1. Overview

To determine the final sample of dually eligible beneficiaries for our analysis, we used a propensity score matching approach similar to the one we used for the Medicare panel analysis, with one major exception: we did not use Medicare or Medicaid baseline outcome measures in our matching. To the extent that health care use and expenditures before the intervention give us additional information about health status at time of entry into home-based primary care that we cannot observe otherwise in administrative data, including these measures would improve balance on observed and possibly unobserved measures of health status, which would affect selection into both home-based primary care and expenditures during the post period. Recent literature, however, has suggested that using outcome measures in propensity score matching can introduce regression to the mean bias in difference-in-differences models, leading to biased impact estimates (Daw and Hatfield 2018). We examined descriptive output to explore this issue and concluded that using baseline outcome measures in our matching for this analysis could lead to regression to the mean bias, so we did not include baseline outcome measures in our propensity score model.

Aside from the baseline outcome measures, the variables we used to create the dually eligible matched comparison group included the same variables we used for the full Medicare panel analysis and additional variables constructed from Medicaid data. The Medicare and Medicaid variables included those that determine eligibility for the demonstration and other measures of frailty and demographic characteristics at the time the beneficiary began home-based primary care; we could observe these variables in claims and other administrative data. Because of our long list of matching variables, we prioritized covariates and applied calipers to ensure balance.

As we noted, we constructed up to 12 comparison beneficiary versions for each beneficiary in the potential comparison pool. We then matched each beneficiary in the home-based primary care group with five potential beneficiary versions. The traditional optimal matching strategy can match different versions of the same comparison subject (that is, copies of the same beneficiary that may vary only slightly across versions) to the same home-based primary care beneficiary, which could place outsized weight on comparison beneficiaries that are matched more than once. To avoid matching different versions of the same comparison beneficiary, we employed a matching strategy in R called GroupMatch designed for matching with rolling enrollment (Pimentel et al. 2019). With this approach, we could match each beneficiary in the home-based primary care group to exactly five comparison beneficiaries and still ensure that only one version of any unique comparison beneficiary appeared in the selected comparison group.

2. Variables used for exact matching

We exact matched on several variables. First, we exact matched on each IAH practice's catchment area (that is, the collection of ZIP codes for the beneficiaries in the home-based primary care group attributed to an IAH practice). In this way, the eligible home-based primary care patients seen by providers other than the IAH providers live in the same area, with access to, and outcomes influenced by, the same health care market. Second, we exact matched on the number of months since the most recent hospital admission and according to whether the beneficiary did not have a hospital admission in the 12 months before the start date but still met eligibility criteria through an observation stay.

3. Variables used for propensity score matching

We constructed two variables from the Medicaid administrative data to use in matching for our dually eligible subgroup: an indicator for Medicaid managed care status in the 12-month period before the start date and an indicator for whether the beneficiary was newly dually eligible in the 12-month period before the start date. The latter indicator was flagged based on whether the beneficiary had any Medicaid benefits (full or partial) observed in the 24-month period before the start date.⁵³

Other measures we used for matching we constructed from Medicare administrative data. To increase the probability that the matched comparison subjects will be at a similar level of frailty (in terms of ADLs needing human assistance) as their corresponding beneficiary in the home-based primary care group, we used the predicted ADL score as of the start date and as of 12 months before the start date from our ADL prediction model.

⁵³ Although we required full Medicaid benefits in the 12 months before the start date for a beneficiary to be included in the dually eligible sample, we used Medicaid data from the 24 months before the start date to look back and determine whether beneficiaries had any Medicaid benefits before the baseline year or whether they were new dually eligible beneficiaries in the baseline year. We constructed the indicator to use in matching because new dually eligible beneficiaries may differ from beneficiaries who have received Medicaid benefits for many years.

We used individual Hierarchical Condition Categories (HCCs) and the HCC score to reflect health status. We identified the beneficiary's individual HCCs by looking back 12 months and running the publicly available Centers for Medicare & Medicaid Services software of the HCC model. We used Version 21 of the HCC model, which was developed and calibrated for the Program of All-Inclusive Care for the Elderly population, because that population resembles the IAH-eligible population in being sicker and frailer than the average Medicare beneficiary. Because there are many condition categories that are available from the HCC model output, we selected HCCs to include in the propensity score matching model based on whether any of the diagnosis codes included in the condition category were among those identified by Gagne et al. (2011) as predictive of mortality among elderly Medicare beneficiaries with low income. The number of chronic conditions came from the Chronic Conditions Warehouse.

We included several recency variables, which were measures that helped us match beneficiaries at a similar level of frailty and disease progression. These included the number of months in the previous 12 months since the first claim on which a specific chronic condition was identified. We included chronic conditions such as Alzheimer's disease (which progresses over time) and markers for whether specific conditions (such as pressure ulcers) were present currently and present up to 12 months before the start month. We also included the discharge modified–diagnosis related group of the most recent inpatient stay (before the start month). The most frequent modified diagnosis-related groups include heart failure and shock, intracranial hemorrhage or cerebral infarction, and hip and femur procedures. In addition, we included measures of specialist visits, observation stays, and hospice use in the past 12 months and during the 12-month period two years before the start date. These measures served as proxies for health status and health care use behavior. Specifically, the number of specialist visits reflected acuity and care-seeking behavior, observation stays reflected health care use, and hospice use reflected health status and end-of-life health care behavior.

Before matching, we dropped potential comparison beneficiaries in exact match strata without any home-based primary care recipients. This resulted in 97,274 potential comparison beneficiaries for the 2010 panel (from an original pool of 98,582 comparisons) and 88,247 potential comparison beneficiaries for the 2011 panel (from an original pool of 88,806 comparisons).

4. Results

After propensity score matching, we had 3,540 matched comparison beneficiaries for our 2010 panel and 3,095 for our 2011 panel. Overall, our samples were well matched on all characteristics for both panels (Exhibits C.4 and C.5). Although we did not use baseline outcome measures in our matching, we present balance for these outcome measures to show how the home-based primary care group compared with the matched comparison group. For the 2010 panel, there were no characteristics with a standardized difference greater than 0.10. For the 2011 panel, there were no characteristics with a standardized difference greater than 0.25, but hospital admissions (0.125) and Medicaid managed care enrollment status in the year before the start date (at least one month of comprehensive managed care enrollment in the year before the start date [0.106]) had a standardized difference greater than 0.10. Although small, the differences for many characteristics suggest that the treatment group was slightly healthier at baseline.

Exhibit C.4. Balance for 2010 dually eligible panel sample

Variable	Potential comparison group mean (N = 98,582)	Matched comparison group mean (N = 3,540)	Home-based primary care group mean (N = 708)	Standardized difference
Beneficiaries' characteristics used for exact matching				
Number of months since last hospital admission				
1	0.118	0.268	0.268	0.000
2 or 3	0.220	0.298	0.298	0.000
4 or more	0.662	0.434	0.434	0.000
Observation stay only in baseline year (no hospital admission)	0.038	0.048	0.048	0.000
IAH catchment area				
Boston, Massachusetts	0.044	0.013	0.013	0.000
Wilmington, Delaware	0.004	0.007	0.007	0.000
Cleveland, Ohio	0.042	0.037	0.037	0.000
Durham, North Carolina	0.125	0.083	0.083	0.000
New York (combining the areas for the Brooklyn IAH practice and the Long Island Jewish IAH practice)	0.247	0.290	0.290	0.000
Austin, Texas	0.183	0.129	0.129	0.000
Portland, Oregon	0.007	0.003	0.003	0.000
Dallas, Texas	0.074	0.092	0.092	0.000
Flint, Michigan	0.062	0.102	0.102	0.000
Jacksonville, Florida	0.043	0.042	0.042	0.000
Lansing, Michigan	0.065	0.103	0.103	0.000
Milwaukee, Wisconsin	0.053	0.054	0.054	0.000
Washington, DC	0.025	0.024	0.024	0.000
Philadelphia, Pennsylvania	0.008	0.006	0.006	0.000
Richmond, Virginia	0.019	0.017	0.017	0.000
Beneficiaries' characteristics used in propensity score matching				
Demographic characteristics				
Age				
Younger than 65	0.127	0.123	0.129	0.016
65 to 79	0.347	0.316	0.305	-0.023
80 or older	0.526	0.561	0.566	0.011
Race				
White	0.430	0.556	0.540	-0.034
Black	0.340	0.303	0.311	0.017
Other	0.230	0.141	0.150	0.025
Female	0.701	0.753	0.754	0.004

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Exhibit C.4 (continued)

Variable	Potential comparison group mean (N = 98,582)	Matched comparison group mean (N = 3,540)	Home-based primary care group mean (N = 708)	Standardized difference
Original reason for Medicare entitlement				
Age	0.709	0.712	0.710	-0.002
Disability	0.269	0.268	0.270	0.005
ESRD or ESRD and disability	0.022	0.021	0.020	-0.008
Medicaid managed care enrollment				
FFS enrollment for the entire baseline year	0.537	0.484	0.473	-0.021
At least one month of comprehensive managed care enrollment in the baseline year	0.159	0.122	0.138	0.050
Any other type of managed care enrollment pattern in the baseline year	0.305	0.394	0.388	-0.012
Medicaid enrollment status in 12 months before the baseline year				
Full Medicaid benefits for all 12 months in 12 months before the baseline year	0.930	0.899	0.907	0.027
Full Medicaid benefits for less than 12 months in 12 months before the baseline year	0.064	0.093	0.086	-0.024
No months of full Medicaid benefits but partial Medicaid benefits for any number of months in 12 months before the baseline year	0.001	0.001	0.001	0.008
No months of full or partial Medicaid benefits in 12 months before the baseline year (new dual enrollees)	0.005	0.007	0.006	-0.017
ADL				
Predicted probability of having two or more ADLs as of the start date	0.887	0.907	0.909	0.042
Predicted probability of having two or more ADLs 12 months before the start date	0.860	0.866	0.867	0.016
Health status				
HCC score	3.527	4.176	4.034	-0.068
HCC score, 12 months before the baseline year	2.559	2.760	2.635	-0.072
HCC 8, metastatic cancer	0.027	0.012	0.014	0.021
HCC 9–10, lung, lymphoma, and other cancers	0.050	0.036	0.032	-0.020
HCC 11–12, colorectal, bladder, breast, prostate, and other cancers	0.097	0.108	0.102	-0.019
HCC 18, diabetes with chronic complications	0.322	0.312	0.288	-0.052
HCC 21, protein-calorie malnutrition	0.117	0.190	0.195	0.014
HCC 27, end-stage liver disease	0.017	0.011	0.008	-0.022
HCC 28–29, cirrhosis of liver and chronic hepatitis	0.019	0.016	0.014	-0.014

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Exhibit C.4 (continued)

Variable	Potential comparison group mean (N = 98,582)	Matched comparison group mean (N = 3,540)	Home-based primary care group mean (N = 708)	Standardized difference
HCC 46, severe hematological disorders	0.020	0.019	0.018	-0.006
HCC 48, coagulation defects and other specified hematological disorders	0.108	0.118	0.113	-0.016
HCC 51, dementia with complications	0.081	0.162	0.155	-0.017
HCC 52, dementia without complications	0.243	0.355	0.360	0.011
HCC 54–55, drug/alcohol psychosis and drug/alcohol dependence	0.036	0.048	0.048	0.000
HCC 57–58, schizophrenia, major depressive, bipolar, and paranoid disorders	0.131	0.175	0.175	0.001
HCC 70–71, quadriplegia, paraplegia	0.036	0.040	0.042	0.010
HCC 72, spinal cord disorders/injuries	0.021	0.027	0.028	0.005
HCC 85, congestive heart failure	0.535	0.618	0.586	-0.066
HCC 96, specified heart arrhythmias	0.337	0.386	0.366	-0.041
HCC 103–104, hemiplegia/hemiparesis, monoplegia, other paralytic syndromes	0.104	0.157	0.161	0.010
HCC 106, atherosclerosis of the extremities with ulceration or gangrene	0.053	0.066	0.064	-0.009
HCC 107–108, vascular disease with or without complications	0.432	0.499	0.492	-0.015
HCC 111, chronic obstructive pulmonary disease	0.339	0.347	0.318	-0.061
HCC 134, dialysis status	0.059	0.060	0.054	-0.026
HCC 136–138, chronic kidney disease, stage 3–5	0.062	0.058	0.054	-0.018
HCC 139–140, chronic kidney disease stage 1–2, unspecified renal failure	0.076	0.076	0.079	0.013
HCC 157–159, pressure ulcer of skin with necrosis or skin loss	0.052	0.108	0.102	-0.019
Anemia ^a	0.195	0.178	0.185	0.018
Depression	0.290	0.339	0.340	0.004
Fluid and electrolyte disorders ^a	0.311	0.329	0.343	0.031
Chronically critically ill/medically complex	0.264	0.298	0.306	0.018
MS-DRG with major complicating condition or complicating condition	0.519	0.519	0.524	0.011
Health care use				
Hospice use	0.005	0.014	0.020	0.044
Number of specialist visits (non-inpatient setting)	23.718	24.860	23.010	-0.089
Number of observation stays	0.159	0.174	0.155	-0.039

Evaluation of Independence at Home: Years 1 to 6 - Appendices

Exhibit C.4 (continued)

Variable	Potential comparison group mean (N = 98,582)	Matched comparison group mean (N = 3,540)	Home-based primary care group mean (N = 708)	Standardized difference
Top MDRGs from the most recent hospital stay				
Kidney and urinary tract infections	0.036	0.053	0.052	-0.001
Heart failure and shock	0.060	0.062	0.056	-0.024
Intracranial hemorrhage or cerebral infarction	0.025	0.023	0.028	0.037
Septicemia without MV 96+ hours	0.030	0.051	0.056	0.027
Hip and femur procedures except major joint	0.019	0.018	0.023	0.036
Simple pneumonia and pleurisy	0.037	0.031	0.031	0.003
Renal failure	0.023	0.028	0.025	-0.014
Major joint replacement or reattachment of lower extremity	0.034	0.015	0.016	0.005
Recency variables				
Months since last wheelchair code as of the start date				
Less than 3 months	0.167	0.244	0.251	0.017
3 to 12 months	0.069	0.091	0.086	-0.018
Not observed in 12 months	0.764	0.665	0.662	-0.005
Months since last wheelchair code 12 months before the baseline year				
Less than 3 months	0.083	0.117	0.110	-0.020
3 to 12 months	0.052	0.077	0.075	-0.008
Not observed in 12 months	0.865	0.806	0.815	0.022
Months since last hospital bed code as of the start date				
Less than 3 months	0.141	0.301	0.312	0.025
3 to 12 months	0.063	0.071	0.071	0.000
Not observed in 12 months	0.796	0.629	0.617	-0.024
Months since last hospital bed code 12 months before the baseline year				
Less than 3 months	0.070	0.111	0.107	-0.013
3 to 12 months	0.045	0.047	0.041	-0.028
Not observed in 12 months	0.885	0.842	0.852	0.027
Months since last home oxygen code as of the start date				
Less than 3 months	0.085	0.121	0.113	-0.026
3 to 12 months	0.023	0.017	0.016	-0.013
Not observed in 12 months	0.892	0.861	0.871	0.030
Months since last home oxygen code 12 months before the baseline year				
Less than 3 months	0.063	0.072	0.065	-0.029
3 to 12 months	0.025	0.029	0.030	0.005
Not observed in 12 months	0.912	0.899	0.905	0.022

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Exhibit C.4 (continued)

Variable	Potential comparison group mean (N = 98,582)	Matched comparison group mean (N = 3,540)	Home-based primary care group mean (N = 708)	Standardized difference
Number of CCW chronic condition categories ^b				
Fewer than 6	0.263	0.267	0.290	0.052
6 to 9	0.513	0.486	0.476	-0.021
10 or more	0.224	0.247	0.234	-0.029
Time since first diagnosed with Alzheimer's disease or senile dementia				
Less than 3 months	0.016	0.040	0.032	-0.041
3 to 12 months	0.077	0.085	0.086	0.005
More than 12 months	0.346	0.465	0.475	0.019
Never	0.561	0.410	0.407	-0.006
Time since first diagnosed with COPD				
Less than 3 months	0.009	0.018	0.014	-0.026
3 to 12 months	0.047	0.039	0.040	0.003
More than 12 months	0.452	0.459	0.434	-0.052
Never	0.492	0.484	0.513	0.057
Time since first diagnosed with heart failure				
Less than 3 months	0.012	0.033	0.030	-0.019
3 to 12 months	0.065	0.048	0.059	0.052
More than 12 months	0.637	0.674	0.651	-0.049
Never	0.285	0.245	0.260	0.035
Time since first diagnosed with hip pelvic fracture				
Less than 3 months	0.005	0.015	0.014	-0.009
3 to 12 months	0.033	0.031	0.035	0.024
More than 12 months	0.066	0.077	0.088	0.040
Never	0.896	0.877	0.863	-0.042
Time since first diagnosed with osteoporosis				
Less than 3 months	0.007	0.005	0.003	-0.026
3 to 12 months	0.033	0.025	0.027	0.011
More than 12 months	0.299	0.313	0.312	-0.001
Never	0.661	0.658	0.658	0.001
HCC 18, diabetes with chronic complications, 12 months before baseline year	0.281	0.240	0.222	-0.043
HCC 27, end-stage liver disease, 12 months before baseline year	0.010	0.005	0.007	0.036
HCC 28–29, cirrhosis of the liver, chronic hepatitis, 12 months before baseline year	0.017	0.012	0.010	-0.019
HCC 57–58, schizophrenia, major depressive, bipolar, and paranoid disorders, 12 months before baseline year	0.106	0.124	0.120	-0.011
HCC 70–71, quadriplegia, paraplegia, 12 months before baseline year	0.025	0.033	0.034	0.005

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Exhibit C.4 (continued)

Variable	Potential comparison group mean (N = 98,582)	Matched comparison group mean (N = 3,540)	Home-based primary care group mean (N = 708)	Standardized difference
HCC 103–104, hemiplegia/hemiparesis, monoplegia, other paralytic syndromes, 12 months before baseline year	0.057	0.073	0.078	0.017
HCC 134, dialysis status, 12 months before baseline year	0.044	0.044	0.038	-0.031
HCC 157–159, pressure ulcer of skin with necrosis or skin loss, 12 months before baseline year	0.022	0.027	0.025	-0.007
Beneficiaries' characteristics not used in propensity score matching^c				
Average monthly Medicare expenditures (on the log scale)	7.969	8.136	8.198	0.073
Average monthly Medicare expenditures, 12 months before baseline year (on the log scale)	6.684	6.836	6.681	-0.089
Average monthly Medicaid expenditures (on the log scale)	6.724	7.037	7.038	0.001
Number of ED visits	1.192	1.328	1.294	-0.016
Number of hospital admissions	1.896	2.319	2.240	-0.039

Source: Mathematica's analysis of Medicare and Medicaid claims, assessment, and enrollment data for 2009 to 2013 obtained from the CCW for home-based primary care recipients and comparison group beneficiaries.

Note: Unless otherwise noted in the variable description, we constructed variables based on the year-long period directly before the start date (baseline year).

^a This was measured using claims from the most recent inpatient stay and observation stay in the year before the start date. We drew diagnosis codes for these conditions from Gagne et al. (2011).

^b These are chronic condition categories measured by the CCW.

^c We did not use baseline outcome measures for propensity score matching, but we examined balance for these measures after matching was complete.

ADL = activities of daily living; CCW = Chronic Conditions Warehouse; COPD = chronic obstructive pulmonary disease; ED = emergency department; ESRD = end-stage renal disease; FFS = fee for service; HCC = Hierarchical Condition Category; IP = inpatient; MDRG = modified diagnostic-related groups; MS-DRG = Medicare Severity Diagnosis Related Group; MV = mechanical ventilation.

Exhibit C.5. Balance for 2011 dually eligible panel sample

Variable	Potential comparison group mean (N = 88,806)	Matched comparison group mean (N = 3,095)	Home-based primary care group mean (N = 619)	Standardized difference
Beneficiaries' characteristics used for exact matching				
Number of months since last hospital admission				
1	0.117	0.296	0.296	0.000
2 or 3	0.218	0.299	0.299	0.000
4 or more	0.665	0.405	0.405	0.000
Observation stay only in baseline year (no hospital admission)	0.038	0.029	0.029	0.000
IAH catchment area				
Boston, Massachusetts	0.050	0.023	0.023	0.000
Cleveland, Ohio	0.057	0.069	0.069	0.000
New York (combining the areas for the Brooklyn IAH practice and the Long Island Jewish IAH practice)	0.403	0.477	0.477	0.000
Austin, Texas	0.259	0.129	0.129	0.000
Portland, Oregon	0.014	0.011	0.011	0.000
Dallas, Texas	0.062	0.094	0.094	0.000
Flint, Michigan	0.057	0.081	0.081	0.000
Lansing, Michigan	0.068	0.079	0.079	0.000
Philadelphia, Pennsylvania	0.013	0.016	0.016	0.000
Richmond, Virginia	0.018	0.021	0.021	0.000
Beneficiaries' characteristics used in propensity score matching				
Demographic characteristics				
Age				
Younger than 65	0.109	0.118	0.121	0.010
65 to 79	0.331	0.286	0.284	-0.004
80 or older	0.559	0.596	0.595	-0.003
Race				
White	0.413	0.503	0.519	0.032
Black or African American	0.264	0.295	0.270	-0.055
Other	0.323	0.203	0.212	0.022
Female	0.685	0.726	0.729	0.006
Original reason for Medicare entitlement				
Age	0.750	0.743	0.737	-0.014
Disability	0.230	0.243	0.254	0.026
ESRD or ESRD and disability	0.020	0.015	0.010	-0.042

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Exhibit C.5 (continued)

Variable	Potential comparison group mean (N = 88,806)	Matched comparison group mean (N = 3,095)	Home-based primary care group mean (N = 619)	Standardized difference
Medicaid managed care enrollment				
FFS enrollment for the entire baseline year	0.565	0.567	0.540	-0.055
At least one month of comprehensive managed care enrollment in the baseline year	0.197	0.134	0.171	0.106
Any other type of managed care enrollment pattern in the baseline year	0.238	0.299	0.289	-0.021
Medicaid enrollment status in 12 months before baseline year				
Full Medicaid benefits for all 12 months in 12 months before the baseline year	0.930	0.906	0.908	0.008
Full Medicaid benefits for less than 12 months in 12 months before the baseline year	0.064	0.087	0.086	-0.005
No months of full Medicaid benefits but partial Medicaid benefits for any number of months in 12 months before the baseline year	0.001	0.005	0.005	-0.005
No months of full or partial Medicaid benefits in 12 months before the baseline year (new dual enrollees)	0.004	0.002	0.002	-0.014
ADL				
Predicted probability of having two or more ADLs as of start date	0.886	0.908	0.911	0.064
Predicted probability of having two or more ADLs 12 months before the start date	0.859	0.860	0.864	0.049
Health status				
HCC score	3.595	4.500	4.366	-0.060
HCC score, 12 months before the baseline year	2.657	2.756	2.680	-0.045
HCC 8, metastatic cancer	0.027	0.038	0.034	-0.022
HCC 9–10, lung, lymphoma, and other cancers	0.050	0.061	0.057	-0.019
HCC 11–12, colorectal, bladder, breast, prostate, and other cancers	0.090	0.087	0.079	-0.028
HCC 18, diabetes with chronic complications	0.357	0.345	0.328	-0.035
HCC 21, protein-calorie malnutrition	0.130	0.203	0.199	-0.010
HCC 27, end-stage liver disease	0.021	0.020	0.024	0.027
HCC 28–29, cirrhosis of liver and chronic hepatitis	0.024	0.017	0.013	-0.035
HCC 46, severe hematological disorders	0.020	0.012	0.013	0.006
HCC 48, coagulation defects and other specified hematological disorders	0.122	0.153	0.129	-0.066
HCC 51, dementia with complications	0.088	0.217	0.202	-0.036
HCC 52, dementia without complications	0.261	0.335	0.339	0.008

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Exhibit C.5 (continued)

Variable	Potential comparison group mean (N = 88,806)	Matched comparison group mean (N = 3,095)	Home-based primary care group mean (N = 619)	Standardized difference
HCC 54–55, drug/alcohol psychosis and drug/alcohol dependence	0.030	0.048	0.048	0.000
HCC 57–58, schizophrenia, major depressive, bipolar, and paranoid disorders	0.135	0.199	0.202	0.006
HCC 70–71, quadriplegia, paraplegia	0.041	0.067	0.063	-0.017
HCC 72, spinal cord disorders/injuries	0.019	0.028	0.026	-0.014
HCC 85, congestive heart failure	0.547	0.607	0.575	-0.065
HCC 96, specified heart arrhythmias	0.347	0.429	0.397	-0.063
HCC 103–104, hemiplegia/hemiparesis, monoplegia, other paralytic syndromes	0.106	0.163	0.170	0.019
HCC 106, atherosclerosis of the extremities with ulceration or gangrene	0.056	0.067	0.073	0.024
HCC 107–108, vascular disease with or without complications	0.465	0.505	0.486	-0.037
HCC 111, chronic obstructive pulmonary disease	0.336	0.402	0.394	-0.015
HCC 134, dialysis status	0.063	0.048	0.037	-0.052
HCC 136–138, chronic kidney disease, stage 3–5	0.072	0.066	0.066	0.003
HCC 139–140, chronic kidney disease stage 1–2, unspecified renal failure	0.077	0.074	0.076	0.007
HCC 157–159, pressure ulcer of skin with necrosis or skin loss	0.060	0.139	0.131	-0.023
Anemia ^a	0.212	0.265	0.249	-0.037
Depression	0.315	0.360	0.370	0.022
Fluid and electrolyte disorders ^a	0.331	0.406	0.394	-0.024
Chronically critically ill/medically complex	0.263	0.329	0.346	0.036
MS-DRG with major complicating condition or complicating condition	0.491	0.514	0.522	0.015
Health care use				
Hospice use	0.005	0.016	0.023	0.047
Number of specialist visits (non-inpatient setting)	24.716	25.321	23.772	-0.077
Number of observation stays	0.151	0.130	0.134	0.010
Top MDRGs from the most recent hospital stay				
Kidney and urinary tract infections	0.037	0.049	0.052	0.010
Heart failure and shock	0.054	0.068	0.063	-0.019
Intracranial hemorrhage or cerebral infarction	0.028	0.034	0.048	0.080
Septicemia without MV 96+ hours	0.030	0.043	0.050	0.033
Hip and femur procedures except major joint	0.019	0.010	0.016	0.055

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Exhibit C.5 (continued)

Variable	Potential comparison group mean (N = 88,806)	Matched comparison group mean (N = 3,095)	Home-based primary care group mean (N = 619)	Standardized difference
Simple pneumonia and pleurisy	0.040	0.030	0.032	0.015
Renal failure	0.028	0.038	0.034	-0.024
Major joint replacement or reattachment of lower extremity	0.032	0.007	0.008	0.007
Recency variables				
Months since last wheelchair code as of the start date				
Less than 3 months	0.162	0.263	0.283	0.044
3 to 12 months	0.069	0.082	0.082	0.000
Not observed in 12 months	0.769	0.654	0.635	-0.041
Months since last wheelchair code 12 months before the baseline year				
Less than 3 months	0.087	0.093	0.095	0.009
3 to 12 months	0.055	0.064	0.061	-0.012
Not observed in 12 months	0.858	0.843	0.843	0.001
Months since last hospital bed code as of the start date				
Less than 3 months	0.132	0.296	0.318	0.048
3 to 12 months	0.055	0.054	0.060	0.024
Not observed in 12 months	0.812	0.649	0.622	-0.057
Months since last hospital bed code 12 months before the baseline year				
Less than 3 months	0.068	0.081	0.084	0.011
3 to 12 months	0.041	0.053	0.052	-0.006
Not observed in 12 months	0.891	0.866	0.864	-0.005
Months since last home oxygen code as of the start date				
Less than 3 months	0.070	0.128	0.128	-0.001
3 to 12 months	0.017	0.022	0.023	0.007
Not observed in 12 months	0.913	0.850	0.850	-0.002
Months since last home oxygen code 12 months before the baseline year				
Less than 3 months	0.048	0.081	0.079	-0.007
3 to 12 months	0.014	0.006	0.005	-0.013
Not observed in 12 months	0.938	0.913	0.916	0.010
Number of CCW chronic condition categories ^b				
Fewer than 6	0.219	0.233	0.246	0.030
6 to 9	0.504	0.446	0.439	-0.012
10 or more	0.278	0.322	0.315	-0.015
Time since first diagnosed with Alzheimer's disease or senile dementia				
Less than 3 months	0.016	0.053	0.045	-0.036
3 to 12 months	0.079	0.074	0.078	0.015
More than 12 months	0.369	0.478	0.478	0.001
Never	0.536	0.395	0.399	0.007

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Exhibit C.5 (continued)

Variable	Potential comparison group mean (N = 88,806)	Matched comparison group mean (N = 3,095)	Home-based primary care group mean (N = 619)	Standardized difference
Time since first diagnosed with COPD				
Less than 3 months	0.009	0.023	0.016	-0.048
3 to 12 months	0.048	0.047	0.047	-0.002
More than 12 months	0.447	0.497	0.486	-0.021
Never	0.495	0.433	0.451	0.036
Time since first diagnosed with heart failure				
Less than 3 months	0.010	0.025	0.019	-0.034
3 to 12 months	0.062	0.056	0.055	-0.004
More than 12 months	0.670	0.697	0.677	-0.044
Never	0.258	0.222	0.249	0.063
Time since first diagnosed with hip pelvic fracture				
Less than 3 months	0.005	0.011	0.010	-0.010
3 to 12 months	0.030	0.021	0.026	0.031
More than 12 months	0.065	0.067	0.066	-0.001
Never	0.901	0.901	0.898	-0.011
Time since first diagnosed with osteoporosis				
Less than 3 months	0.008	0.007	0.006	-0.011
3 to 12 months	0.038	0.032	0.034	0.009
More than 12 months	0.352	0.357	0.354	-0.007
Never	0.602	0.603	0.606	0.006
HCC 18, diabetes with chronic complications, 12 months before the baseline year	0.319	0.290	0.281	-0.020
HCC 27, end-stage liver disease, 12 months before the baseline year	0.014	0.010	0.010	-0.003
HCC 28–29, cirrhosis of the liver, chronic hepatitis, 12 months before the baseline year	0.019	0.016	0.013	-0.024
HCC 57–58, schizophrenia, major depressive, bipolar, and paranoid disorders, 12 months before the baseline year	0.110	0.139	0.142	0.010
HCC 70–71, quadriplegia, paraplegia, 12 months before the baseline year	0.026	0.028	0.027	-0.002
HCC 103–104, hemiplegia/hemiparesis, monoplegia, other paralytic syndromes, 12 months before the baseline year	0.057	0.068	0.069	0.005
HCC 134, dialysis status, 12 months before the baseline year	0.046	0.038	0.027	-0.058
HCC 157–159, pressure ulcer of skin with necrosis or skin loss, 12 months before the baseline year	0.029	0.035	0.034	-0.007

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Exhibit C.5 (continued)

Variable	Potential comparison group mean (N = 88,806)	Matched comparison group mean (N = 3,095)	Home-based primary care group mean (N = 619)	Standardized difference
Beneficiaries' characteristics not used in propensity score matching^c				
Average monthly Medicare expenditures (on the log scale)	8.005	8.241	8.237	-0.004
Average monthly Medicare expenditures, 12 months before the baseline year (on the log scale)	6.815	6.839	6.682	-0.095
Average monthly Medicaid expenditures (on the log scale)	7.008	7.295	7.406	0.079
Number of ED visits	1.020	1.112	1.065	-0.029
Number of hospital admissions	1.867	2.412	2.183	-0.125

Source: Mathematica's analysis of Medicare and Medicaid claims, assessment, and enrollment data for 2009 to 2013 obtained from the CCW for home-based primary care recipients and comparison group beneficiaries.

Note: Unless otherwise noted in the variable description, we constructed variables based on the year-long period directly before the start date (baseline year).

^a We measured using claims from the most recent inpatient stay and observation stay in the year before the start date. We drew diagnosis codes for these conditions from Gagne et al. (2011).

^b These are chronic condition categories measured by the CCW.

^c We did not use baseline outcome measures for propensity score matching, but we examined balance for these measures after matching was complete.

ADL = activities of daily living; CCW = Chronic Conditions Warehouse; COPD = chronic obstructive pulmonary disease; ED = emergency department; ESRD = end-stage renal disease; FFS = fee for service; HCC = Hierarchical Condition Category; IP = inpatient; MDRG = modified diagnostic related groups; MS-DRG = Medicare Severity Diagnosis Related Group; MV = mechanical ventilation.

III. Data and outcomes

We drew upon Medicare and Medicaid administrative data to determine sample eligibility and measure outcomes for our dually eligible subgroup (Exhibit C.6).

Exhibit C.6. Medicare and Medicaid data sources

Data	Demographic characteristics ^a	Chronic conditions and HCC score	Activities of daily living	Service use: Sample eligibility	Service use and expenditures: Outcome measures
Medicare					
Medicare enrollment database	✓				
Master beneficiary summary file	✓	✓			
Inpatient claims		✓		✓	✓
Outpatient claims		✓		✓	✓
Clinician/supplier claims		✓		✓	✓
Home health agency claims				✓	✓
Skilled nursing facility claims				✓	✓
Hospice claims				✓	✓
Durable medical equipment claims					✓
Inpatient rehabilitation facility–patient assessment instrument ^b			✓		
Minimum Data Set			✓		
Outcome and assessment information set			✓		
Timeline file				✓	
Medicaid					
Medicaid PS file	✓				
Medicaid IP file claims					✓
Medicaid OT file claims					✓
Medicaid LT file claims					✓
Medicaid RX file claims					✓

^a We used demographic characteristics to define sample eligibility, matching variables, and control variables for the regression analyses.

^b This includes inpatient rehabilitation hospitals and rehabilitation units. It excludes long-term care hospitals.

HCC = hierarchical condition category; IP = inpatient; LT = long-term care; OT = other services; PS = person summary; RX = prescription drug.

The outcomes of interest for our analysis included expenditures, service use, and health outcomes. We measured these outcomes at the beneficiary level in 12-month periods. For a given beneficiary, the first (of two) year-long post-intervention periods began with the month that contains the beneficiary’s start date. We also measured all outcomes in the one pre-intervention year before the start date. Exhibit C.2 displays how we identified the pre-intervention and post-intervention years for each group.

A. Medicare outcomes

As Exhibit C.7 shows, we examined the impact of home-based primary care on Medicare FFS expenditures, Medicare service use, and health outcomes.

Exhibit C.7. Measures of Medicare expenditures, service use, and health outcomes

Outcome measure
Average per beneficiary per month Medicare FFS expenditures
Total
Inpatient
Home health ^a
Outpatient
SNF
Clinician/supplier
Hospice
DME
Medicare service use and health outcomes
Number of hospital admissions per 1,000 beneficiaries per year ^b
Number of potentially avoidable hospital admissions per 1,000 beneficiaries per year ^b
Number of ED visits that did not result in admission per 1,000 beneficiaries per year ^c
Number of potentially avoidable ED visits that did not result in admission per 1,000 beneficiaries per year ^c
Percentage of beneficiaries with a qualifying discharge and an unplanned readmission within 30 days of discharge

Note: We annualized measures of admissions and ED visits for beneficiaries that we did not observe for the full year.

^a Total home health expenditures include Part A, Part B, and other home health expenditures.

^b This includes inpatient hospital admissions and observation stays.

^c We measured this as specified in the Center for Medicare & Medicaid Innovation’s Priority Measures for Monitoring and Evaluation.

DME = durable medical equipment; ED = emergency department; FFS = fee for service; SNF = skilled nursing facility.

Potentially avoidable hospital admissions and outpatient ED visits. Potentially avoidable hospital or ED use occurs when ambulatory care may have prevented or reduced the need for a hospital admission or ED visit. We measured a beneficiary as having a potentially avoidable hospital admission or ED visit if the principal diagnosis for the hospital admission or ED visit was an ambulatory care-sensitive condition. We based our definition of ambulatory care-sensitive condition on the Agency for Healthcare Research and Quality's Prevention Quality Indicator 90, which includes the following conditions: diabetes short-term complications, diabetes long-term complications, uncontrolled diabetes, lower-extremity amputation among diabetics, chronic obstructive pulmonary disease or asthma in older adults, hypertension, heart failure, angina without procedure, dehydration, bacterial pneumonia, and urinary tract infection. The measure of ED visits for ambulatory care-sensitive conditions excluded ED visits that led to an inpatient hospital admission because there is no diagnosis from the ED visit in a claim record when an ED visit leads to an inpatient hospital admission.

Outpatient ED visits not leading to an inpatient stay. We used revenue center codes 0450, 0451, 0452, 0456, 0459, or 0981 to measure outpatient ED visits using claims in the outpatient file. Line items with an ED revenue center code did not contribute to the count of ED visits if the procedure code on that line item equals 70000 to 79999 or 80000 to 89999, which identify lab or imaging services. The measure of outpatient ED visits could include cases in which a beneficiary was transferred to a different hospital for admission and may include some cases in which a hospital billed the ED visit and the inpatient stay that resulted separately.

Unplanned readmission within 30 days of discharge. The unplanned readmission measure indicates whether the beneficiary had at least one unplanned readmission within 30 days of an eligible discharge. Eligible discharges for the readmission measure include discharges from nonfederal acute care hospitals for patients enrolled in Medicare FFS, alive at the time of discharge, and not transferred to another acute care facility. Eligible discharges also include patients discharged to nonacute care settings. Eligible discharges do not include admissions to Prospective Payment System–exempt cancer hospitals or admissions for patients without at least 30 days of post-discharge enrollment in FFS Medicare Parts A and B (unless a patient was enrolled in FFS but died within 30 days), patients discharged against medical advice, primary psychiatric diagnoses, rehabilitation, and medical treatment of cancer.

We excluded planned readmissions from this measure. To identify planned readmissions, we followed the approach used by CMS for the hospital-level 30-day risk-standardized readmission measure developed by the Yale New Haven Health Services Corporation/Center for Outcomes Research & Evaluation (2015). Unlike the Yale measure, however, we limited our list of codes to identify planned readmissions to the set of procedure codes that apply to Medicare beneficiaries, excluding codes that apply only to all-payer populations.

The regression for unplanned readmission included all beneficiaries; it was not conditional on having an eligible discharge. Impact estimates of home-based primary care on this outcome provide an estimate of the combined effect of home-based primary care on whether a patient had an eligible discharge and, if so, whether the patient had an unplanned readmission within 30 days.

B. Medicaid outcomes

In addition to the Medicare outcomes, we examined total Medicaid expenditures, capitated expenditures, and five categories of Medicaid FFS expenditures (Exhibit C.8). Finally, we analyzed combined total Medicare and Medicaid expenditures. In all cases, we measured expenditures as per beneficiary per month (PBPM).

Exhibit C.8. Measures of Medicaid expenditures and total Medicare and Medicaid expenditures

Outcome measure	Definition
Average per beneficiary per month Medicaid expenditures	
Total	Total FFS and capitated expenditures.
FFS institutional	FFS expenditures for nursing facilities, intermediate care facilities for people with intellectual disabilities, psychiatric hospitals, and independent (freestanding) psychiatric wings of acute care hospitals from MAX LT file.
FFS prescription drug	FFS expenditures for prescription drugs from MAX RX file.
FFS inpatient hospital	FFS expenditures for inpatient hospital services from MAX IP file.
FFS HCBS	FFS expenditures for HCBS from MAX OT file. HCBS were defined as case management; round-the-clock services; supported employment; day services; nursing; home-delivered meals; rent and food expenses for live-in caregiver; home-based services; caregiver support; other mental health and behavioral services; other health and therapeutic services; services supporting participant direction; participant training; equipment, technology, and modifications; non-medical transportation; community transition services; and other services.
FFS physician, clinic, and outpatient service expenditures other than for HCBS	FFS expenditures from the MAX OT file, less expenditures for FFS HCBS. Includes physician, clinic, and outpatient services.
Capitated	Capitated payments from the MAX OT file.
Average per beneficiary per month Medicare and Medicaid expenditures	
Total Medicare and Medicaid	Total Medicare FFS and Medicaid FFS and capitated expenditures.

Note: Medicaid FFS expenditures include all Medicaid expenditures, including those for Medicare deductibles and co-payments.

FFS = fee for service; HCBS = home and community-based services; IP = inpatient; LT = long-term care; MAX = Medicaid Analytic eXtract; OT = other services; RX = prescription drug.

Home and community-based services (HCBS) expenditures. To construct Medicaid FFS HCBS expenditures, we used claims from the Medicaid Analytic eXtract (MAX) other services (OT) file. We applied the HCBS taxonomy crosswalks for each year to all FFS claims in the OT files.⁵⁴ We then

⁵⁴ The HCBS taxonomy crosswalk was originally developed for section 1915(c) waiver claims. We applied the taxonomy crosswalk in two ways: restricting the taxonomy crosswalk to section 1915(c) waiver claims and not restricting the taxonomy crosswalk to section 1915(c) waiver claims so that we could identify other state plan services that were likely HCBS. We reviewed output from both versions of the coding, and we examined section 1915(c) waiver enrollment among our sample together with the two versions of the HCBS expenditure coding. Based on this analysis, we determined that we should use the version of the taxonomy coding that was applied to all claims to comprehensively identify HCBS for this sample of dually eligible beneficiaries. Otherwise, we would misclassify relevant HCBS expenditures by using the approach restricted to section 1915(c) waiver claims.

summed expenditures across all FFS claims that contained a HCBS taxonomy code to calculate FFS HCBS expenditures.⁵⁵

Physician, clinic, and outpatient service expenditures other than for HCBS. To calculate physician, clinic, and outpatient service expenditures other than for HCBS, we summed total FFS expenditures from the MAX OT file and subtracted all expenditures for FFS HCBS.

Capitated expenditures. For capitated expenditures, we included all capitated payments from the MAX OT file (which contain the information on capitated payments). Beneficiaries can have many different types of arrangements for their Medicaid benefits, which means that even if beneficiaries are enrolled in a specific type of managed care plan (such as a behavioral health managed care plan), they can still have FFS expenditures for other Medicaid expenditure categories; therefore, we captured all relevant FFS and capitated expenditures. Most beneficiaries with capitated expenditures also had FFS expenditures, but not all beneficiaries with FFS expenditures had capitated expenditures.

IV. Estimation methods

We used a difference-in-differences analysis to examine the effects of home-based primary care for IAH-eligible beneficiaries dually enrolled in Medicare and Medicaid. We measured the effect as the change in the home-based primary care group after netting out any change from other trends in the health care system underway before and during the same time period as observed in the matched comparison group. We followed each beneficiary over three years: one year before the intervention (which we refer to as the baseline year) and two years after the intervention.

A. Model specification

Our estimation approach involved pooling the panels and estimating impacts jointly. Equation (1) specifies the regression model we used to estimate the impact of home-based primary care for the two panels combined:

$$(1) \quad Y_{it} = \alpha + \beta' X_i + \sigma' C_i + \tau \cdot treatment_i + \gamma_3 P_3 + \gamma_4 P_4 + \delta_1 \cdot PAN2011 + \theta_3 \cdot treatment_i \cdot P_3 + \theta_4 \cdot treatment_i \cdot P_4 + \omega_{it}$$

where Y_{it} is the outcome variable for beneficiary i in period t ; α is a constant term; X_i is a set of beneficiary characteristics measured in the baseline period (Exhibit C.9); C_i are indicators for the IAH catchment area (area-level fixed effects); $treatment_i$ is a binary indicator of whether individual i is in the home-based primary care group; P_t 's are the period indicators, where $P_t = 1$ in period t and 0 otherwise. P_3 is the first year in the post-intervention period (corresponding to 2011 for the 2010 panel and to 2012 for the 2011 panel). We also included a binary indicator variable for the panel ($PAN2011$) coded as 1 if

⁵⁵ The categories in the HCBS taxonomy coding include case management; round-the-clock services; supported employment; day services; nursing; home-delivered meals; rent and food expenses for live-in caregiver; home-based services; caregiver support; other mental health and behavioral services; other health and therapeutic services; services supporting participant direction; participant training; equipment, technology, and modifications; non-medical transportation; community transition services; and other services.

the beneficiary was in the 2011 panel and coded as 0 if the beneficiary was in the 2010 panel. Finally, ω_{it} is a random error term.

The Greek letters are all parameters that we estimated. The parameter τ estimated the home-based primary care–comparison difference in an outcome during the reference period (the year before home-based primary care entry), γ_t measured changes in the outcome for the comparison group over time, and the θ_t parameters are the difference-in-differences estimates. The difference-in-differences estimates for the first and second year after starting home-based primary care (θ_3 and θ_4 , respectively) are the key parameters of interest. Specifically, θ_3 represents the difference in the regression-adjusted mean of the outcome between the year before starting home-based primary care and the first year after starting for home-based primary care recipients relative to the difference during the same period for matched comparison beneficiaries. We interpret this estimate as the impact of home-based primary care. The impact estimate accounts for differences between the home-based primary care and comparison groups that are constant over time as well as any changes over time (for example, in the local health care environment) that affect outcomes for both groups equally.

The regression model we used varied by the outcome. We used ordinary least squares for the expenditure variables, logistic regression for unplanned readmission, and negative binomial for the measures of hospital admissions and ED visits (both total and potentially avoidable).

B. Adjustment to standard errors for clustering

We observed each beneficiary in the sample multiple times (baseline year and up to two post-period years). Because the observations for a given beneficiary in one period were not independent of the observations for the same beneficiary in other periods, our estimator of the variance accounted for this time dependence of repeated observations.

C. Weighting

Our outcomes variables were either monthly averages (for expenditures), annualized counts (total and potentially avoidable hospital admissions and ED visits), or a binary indicator (unplanned readmission). We weighted observations in each post-intervention year by fractional eligibility weights that captured the share of months alive, in Medicare FFS, and receiving full Medicaid benefits during each post period. We also used matching weights in our analyses. Beneficiaries in the home-based primary care group received a matching weight of 1, and each matched comparison beneficiary received a weight that was the inverse of the number of comparison beneficiaries within the matched set. Because all home-based primary care beneficiaries matched with five comparison beneficiaries, each of the five comparison beneficiaries received a weight of 0.20.

We obtained a composite weight by multiplying the eligibility weight by the matching weight, and we used the composite weight in the regressions.

D. Control variables

Although our propensity score matching resulted in comparison groups that were similar in terms of demographics and health status to the home-based primary care groups, there could still be important differences that affect the outcomes we examined. Therefore, we included a range of control variables (shown in Exhibit C.9), including most of the variables used in matching: demographic characteristics, such as age and original reason for Medicare entitlement, and measures of health status, such as the probability of having two or more ADLs needing human assistance, HCC risk score, and the presence of specific chronic conditions.

Exhibit C.9. Beneficiaries’ characteristics included as control variables for impact estimation

Variable	Home-based primary care group mean	Matched comparison group mean
Number of months since most recent inpatient hospital admission or observation stay before first home-based primary care visit, percentage		
1	27.1	27.0
2 or 3	29.9	29.8
4 or more	43.0	43.2
Demographic characteristics		
Age, percentage		
Younger than 65	12.9	12.5
65 to 74	16.6	16.7
75 to 79	13.4	14.0
80 to 84	21.7	19.7
85 or older	35.4	37.0
Female, percentage	74.4	74.3
Race/ethnicity, percentage		
White	51.7	52.6
Black	29.9	29.9
Hispanic	8.8	8.4
Asian	5.5	6.1
Other/unknown	4.1	3.0
Original reason for Medicare entitlement, percentage		
Old age	71.6	72.0
Disability only	26.9	26.2
ESRD or ESRD and disability	1.5	1.7
IAH catchment area, percentage		
Boston, Massachusetts	1.7	1.7
Wilmington, Delaware	0.4	0.4
Cleveland, Ohio	5.2	5.2
Durham, North Carolina	4.7	4.4
New York (combining the areas for the Brooklyn IAH practice and the Long Island IAH practice)	37.1	37.4
Austin, Texas	12.5	12.9

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Exhibit C.9.(continued)

Variable	Home-based primary care group mean	Matched comparison group mean
Portland, Oregon	0.7	0.7
Dallas, Texas	9.3	9.5
Flint, Michigan	9.3	9.2
Jacksonville, Florida	2.4	2.2
Lansing, Michigan	9.4	9.1
Milwaukee, Wisconsin	2.9	3.0
Washington, DC	1.3	1.3
Philadelphia, Pennsylvania	1.0	1.1
Richmond, Virginia	2.0	1.9
Medicaid managed care enrollment, percentage		
FFS enrollment for the entire baseline year	50.0	51.6
At least one month of comprehensive managed care enrollment in the baseline year	15.1	13.2
Any other enrollment pattern in the baseline year	34.8	35.2
Medicaid enrollment status in prior period, percentage		
Full Medicaid benefits for all 12 months in 12 months before the baseline year	91.2	90.6
Full Medicaid benefits for less than 12 months in 12 months before the baseline year	8.2	8.6
No months of full Medicaid benefits but partial Medicaid benefits for any number of months in 12 months before the baseline year	0.4	0.3
No months of full or partial Medicaid benefits in 12 months before the baseline year (new dual enrollees)	0.3	0.5
Health status characteristics		
Probability of having two or more ADLs requiring human assistance, percentage	90.9	90.6
HCC risk score	4.10	4.21
Number of chronic conditions	7.41	7.73
Number of chronic conditions squared	63.69	68.53
Individual HCCs and conditions, percentage		
HCC 8, metastatic cancer and acute leukemia	1.9	1.9
HCC 9, lung and other severe cancers	2.2	2.3
HCC 11, colorectal, bladder, and other cancers	3.6	4.1
HCC 18, diabetes with chronic complications	30.6	32.8
HCC 21, protein-calorie malnutrition	18.4	18.6
HCC 27, end-stage liver disease	1.6	1.4
HCC 29, chronic hepatitis	0.9	0.8
HCC 46, severe hematological disorders	1.3	1.6
HCC 51, dementia with complications	17.9	18.3
HCC 52, dementia without complications	34.6	33.8
HCC 54, drug/alcohol psychosis	2.8	2.9

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Exhibit C.9.(continued)

Variable	Home-based primary care group mean	Matched comparison group mean
HCC 55, drug/alcohol dependence	1.9	1.8
HCC 57, schizophrenia	5.5	4.7
HCC 70, quadriplegia	3.2	3.3
HCC 71, paraplegia	2.1	2.1
HCC 85, congestive heart failure	56.4	60.1
HCC 103, hemiplegia/hemiparesis	15.6	15.0
HCC 106, atherosclerosis of the extremities with ulceration or gangrene	6.6	6.2
HCC 107, vascular disease with complications	8.2	9.7
HCC 134, dialysis status	4.3	5.0
HCC 136, chronic kidney disease, stage 5	1.4	1.4
HCC 137, chronic kidney disease, stage 4	1.1	1.2
HCC 138, chronic kidney disease, stage 3	3.4	3.4
HCC 157, pressure ulcer of skin with necrosis through to muscle, tendon, or bone	4.4	3.3
HCC 158, pressure ulcer of skin with full thickness skin loss	2.7	3.6
HCC 159, pressure ulcer of skin with partial thickness skin loss	3.7	4.5
Chronically critically ill or medically complex diagnosis ^a	31.3	29.9
Anemia ^a	20.4	20.9
Fluid and electrolyte disorders ^a	35.8	35.3
Alzheimer's or dementia	50.3	49.9
Acute myocardial infarction or ischemic heart disease	66.3	69.2
Asthma	14.7	15.6
Hip or pelvic fracture	4.3	4.3
Stroke or transient ischemic attack	19.4	21.0

Source: Mathematica's analysis of Medicare and Medicaid claims, assessment, and enrollment data for 2009 to 2013 obtained from the Chronic Conditions Warehouse for home-based primary care recipients and matched comparison group beneficiaries.

Note: We report characteristics using the composite weight used in the regression analyses.

^a We measured this with claims from the most recent inpatient hospital stay and observation stay in the 12 months before the date on which the beneficiary began home-based primary care. We drew diagnosis codes for these conditions from Gagne et al. (2011).

ADL = activities of daily living; ESRD = end-stage renal disease; FFS = fee for service; HBPC = home-based primary care; HCC = Hierarchical Condition Category.

V. Supplementary exhibits and findings

A. Difference-in-differences estimates

Exhibit C.10 shows regression-adjusted means of the home-based primary care and matched comparison groups and difference-in-differences estimates for Medicare and Medicaid expenditures.

Exhibit C.10. Regression-adjusted means and estimated impact of home-based primary care on Medicare and Medicaid expenditures

Outcome and period	Regression-adjusted mean for home-based primary care group	Regression-adjusted mean for matched comparison group	Difference-in-differences estimate (standard error)	Percentage impact ^a
Average per beneficiary per month Medicare FFS expenditures				
Total				
One year prior	\$4,959	\$4,760	–	–
One year post	\$4,135	\$3,730	\$205 (\$176)	4.1
Two years post	\$3,563	\$3,341	\$22 (\$190)	0.5
Inpatient				
One year prior	\$2,663	\$2,581	–	–
One year post	\$2,037	\$1,904	\$50 (\$140)	1.9
Two years post	\$1,685	\$1,649	-\$46 (\$144)	-1.7
Home health^b				
One year prior	\$526	\$549	–	–
One year post	\$647	\$383	\$287*** (\$20)	54.7
Two years post	\$434	\$304	\$153*** (\$22)	29.2
Outpatient				
One year prior	\$246	\$291	–	–
One year post	\$209	\$294	-\$40*** (\$14)	-16.1
Two years post	\$221	\$278	-\$12 (\$19)	-4.7
SNF				
One year prior	\$674	\$483	–	–
One year post	\$233	\$332	-\$290*** (\$39)	-43.0
Two years post	\$289	\$335	-\$237*** (\$43)	-35.2

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Exhibit C.10.(continued)

Outcome and period	Regression-adjusted mean for home-based primary care group	Regression-adjusted mean for matched comparison group	Difference-in-differences estimate (standard error)	Percentage impact ^a
Clinician/supplier				
One year prior	\$708	\$704	–	–
One year post	\$699	\$604	\$91*** (\$23)	12.9
Two years post	\$597	\$554	\$38 (\$27)	5.4
Hospice				
One year prior	\$26	\$18	–	–
One year post	\$109	\$68	\$32** (\$16)	125.0
Two years post	\$191	\$99	\$84*** (\$27)	326.2
DME				
One year prior	\$116	\$134	–	–
One year post	\$200	\$145	\$73*** (\$12)	63.4
Two years post	\$146	\$123	\$41** (\$16)	35.8
Average per beneficiary per month Medicaid expenditures				
Total FFS and capitated				
One year prior	\$2,665	\$2,616	–	–
One year post	\$3,038	\$2,943	\$46 (\$70)	1.7
Two years post	\$3,086	\$3,039	-\$2 (\$90)	-0.1
FFS institutional^c				
One year prior	\$279	\$270	–	–
One year post	\$90	\$362	-\$281*** (\$39)	-100.7
Two years post	\$313	\$586	-\$282*** (\$62)	-101.1
FFS prescription drug^d				
One year prior	\$6	\$7	–	–
One year post	\$7	\$7	\$0 (\$1)	7.4
Two years post	\$8	\$7	\$1 (\$2)	14.4

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Exhibit C.10.(continued)

Outcome and period	Regression-adjusted mean for home-based primary care group	Regression-adjusted mean for matched comparison group	Difference-in-differences estimate (standard error)	Percentage impact ^a
FFS inpatient hospital^e				
One year prior	\$79	\$97	–	–
One year post	\$73	\$97	-\$6 (\$19)	-7.7
Two years post	\$52	\$84	-\$14 (\$20)	-18.3
FFS HCBS^f				
One year prior	\$1,731	\$1,628	–	–
One year post	\$2,155	\$1,766	\$286*** (\$67)	16.5
Two years post	\$1,701	\$1,384	\$214** (\$93)	12.4
FFS physician, clinic, and outpatient other than HCBS^g				
One year prior	\$117	\$145	–	–
One year post	\$100	\$123	\$6 (\$7)	5.1
Two years post	\$87	\$101	\$15* (\$9)	12.6
Medicaid capitated^h				
One year prior	\$453	\$470	–	–
One year post	\$612	\$587	\$41* (\$24)	9.0
Two years post	\$926	\$878	\$64 (\$43)	14.2
Average per beneficiary per month Medicare and Medicaid expenditures				
Total Medicare FFS and Medicaid FFS and capitated				
One year prior	\$7,624	\$7,376	–	–
One year post	\$7,172	\$6,673	\$252 (\$184)	3.3
Two years post	\$6,649	\$6,381	\$20 (\$209)	0.3

Source: Mathematica's analysis of Medicare and Medicaid claims, assessment, and enrollment data for 2009 to 2013 obtained from the Chronic Conditions Warehouse for home-based primary care recipients and matched comparison group beneficiaries.

Note: Medicaid FFS expenditures include all Medicaid expenditures, including those for Medicare deductibles and co-payments.

^a Percentage impact is relative to home-based primary care group mean in the year before starting home-based primary care.

^b Total home health expenditures include Part A, Part B, and other home health expenditures.

^c FFS institutional expenditures include expenditures from the MAX LT file for nursing facilities, intermediate care facilities for people with intellectual disabilities, psychiatric hospitals, and independent (freestanding) psychiatric wings of acute care hospitals.

^d FFS prescription drug expenditures include expenditures from the MAX prescription drug RX file.

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Exhibit C.10.(continued)

^e FFS inpatient hospital expenditures include expenditures from the MAX inpatient IP file.

^f FFS HCBS expenditures include expenditures from the MAX other services OT file for HCBS.

^g FFS physician, clinic, and outpatient services expenditures include expenditures from the MAX OT file, less expenditures for FFS HCBS.

^h Capitated expenditures include all capitated payments from the MAX OT file.

//*** The difference is statically significant at the 0.10/0.05/0.01 level.

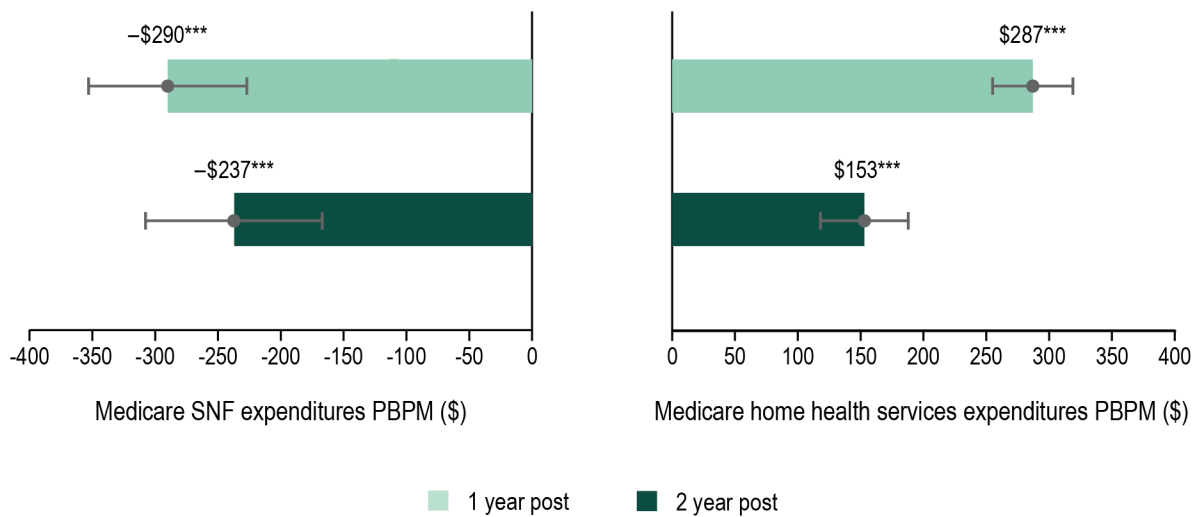
DME = durable medical equipment; FFS = fee for service; HCBS = home and community-based services; IP = inpatient; LT = long-term care; MAX = Medicaid Analytic eXtract; OT = other services; RX = prescription drug; SNF = skilled nursing facility.

We described key findings for total Medicare expenditures and inpatient expenditures in Chapter III. Additional findings for other components of Medicare expenditures include the following:

- The estimated effect on SNF expenditures was a reduction of \$290 (43.0 percent) PBPM in the first year and a reduction of \$237 (35.2 percent) PBPM in the second year. These changes were statistically significant at the 0.01 level (Exhibit C.10).
- The estimated effect on outpatient expenditures was a statistically significant reduction of \$40 (16.1 percent) PBPM in the first year but a non-statistically significant reduction of \$12 (4.7 percent) PBPM in the second year. The effect in the first year was statistically significant at the 0.01 level, but the effect in the second year was not statistically significant at the 0.10 level (Exhibit C.10).
- The estimated effect on home health service expenditures was an increase of \$287 (54.7 percent) PBPM in the first year and \$153 (29.2 percent) PBPM in the second year. These changes were statistically significant at the 0.01 level (Exhibit C.10).
- There were also statistically significant increases in clinician/supplier expenditures in the first year (\$91 PBPM or 12.9 percent), hospice expenditures in the first (\$32 PBPM or 125.0 percent) and second years (\$84 PBPM or 326.2 percent), and DME expenditures in the first (\$73 PBPM or 63.4 percent) and second years (\$41 PBPM or 35.8 percent; Exhibit C.10).

Notably, there were substantial reductions in SNF expenditures and corresponding substantial increases in home health expenditures, changes that are consistent with home-based primary care recipients receiving more care provided in the home and less in institutional outpatient settings (Exhibit C.11).

Exhibit C.11. Estimated effect of home-based primary care on Medicare SNF and home health services expenditures by year



Source: Mathematica’s analysis of Medicare and Medicaid claims, assessment, and enrollment data for 2009 to 2013 obtained from the Chronic Conditions Warehouse for home-based primary care recipients and matched comparison group beneficiaries.

Notes: The home-based primary care recipients included 1,327 beneficiaries, and the matched comparison group included 6,635 beneficiaries. The horizontal lines represent 90 percent confidence intervals. If zero was within the confidence interval, the estimated effect (denoted by dots) was not statistically significantly different from zero at the 90 percent confidence level. The 90 percent confidence intervals for estimates of Medicare SNF expenditures were as follows: one year after intervention, the lower limit was -\$354 and upper limit -\$226, and two years after, the lower limit was -\$308 and upper limit -\$166. The 90 percent confidence intervals for estimates of Medicare home health services expenditures were as follows: one year after intervention, the lower limit was \$254 and upper limit \$320, and two years after, the lower limit was \$117 and upper limit was \$189.

*** The difference is statically significant at the 0.01 level.

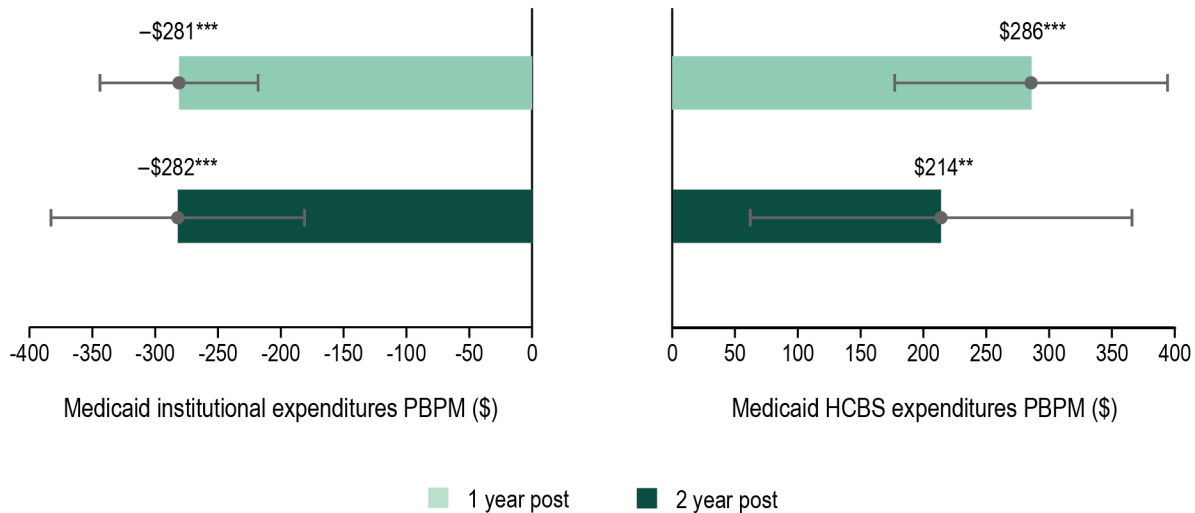
PBPM = per beneficiary per month; SNF = skilled nursing facility.

We described findings for total Medicaid expenditures in Chapter III. Additional findings for key components of Medicaid expenditures include the following:

- The estimated effect on Medicaid FFS institutional expenditures was a reduction of \$281 (100.7 percent) PBPM in the first year and a reduction of \$282 (101.1 percent) PBPM in the second year. These changes were statistically significant at the 0.01 level (Exhibit C.10).
- The estimated effect on FFS Medicaid HCBS expenditures was an increase of \$286 (16.5 percent) in the first year and an increase of \$214 (12.4 percent) in the second year. These changes were statistically significant at the 0.01 and 0.05 levels, respectively (Exhibit C.10).
- We also found increases in capitated expenditures in the first post-intervention year (\$41 PBPM or 9.0 percent) and in expenditures for physician, clinic, and outpatient services in the second post-intervention year (\$15 PBPM or 12.6 percent). The changes were statistically significant at the 0.10 level (Exhibit C.10).

Medicaid FFS institutional expenditures decreased by about the same amount Medicaid FFS HCBS expenditures increased, resulting in no effect on overall Medicaid long-term services and supports expenditures but indicating the home-based primary care recipients received more care in the home and less in institutional settings (Exhibit C.12).

Exhibit C.12. Estimated effect of home-based primary care on Medicaid institutional and HCBS expenditures by year



Source: Mathematica’s analysis of Medicare and Medicaid claims, assessment, and enrollment data for 2009 to 2013 obtained from the Chronic Conditions Warehouse for home-based primary care recipients and matched comparison group beneficiaries.

Notes: The home-based primary care recipients included 1,327 beneficiaries, and the matched comparison group included 6,635 beneficiaries. The horizontal lines represent 90 percent confidence intervals. If zero was within the confidence interval, the estimated effect (denoted by dots) was not statistically significantly different from zero at the 90 percent confidence level. The 90 percent confidence intervals for estimates of Medicaid institutional expenditures were as follows: one year after intervention, the lower limit was -\$345 and upper limit -\$217, and two years after, the lower limit was -\$384 and upper limit -\$180. The 90 percent confidence intervals for estimates of Medicaid HCBS expenditures were as follows: one year after intervention, the lower limit was \$176 and upper limit \$396, and two years after, the lower limit was \$61 and upper limit was \$367.

*** The difference is statically significant at the 0.01 level.

HCBS = home and community-based services; PBPM = per beneficiary per month.

Exhibit C.13 shows regression-adjusted means of the home-based primary care and matched comparison groups and difference-in-differences estimates for Medicare service use and health outcomes. Detailed findings on hospital use and quality of care include the following:

- The home-based primary care group had 87 (4.1 percent) more hospital admissions per 1,000 beneficiaries in the first year than matched comparison beneficiaries did and 49 (2.3 percent) more in the second year, but these estimates were not statistically significant (Exhibit C.13).

- The home-based primary care group had 21 (3.9 percent) more potentially avoidable hospital admissions per 1,000 beneficiaries in the first year and 8 (1.5 percent) fewer potentially avoidable hospital admissions in the second year. Neither of these estimates was statistically significant (Exhibit C.13).
- The home-based primary care group had 33 (2.8 percent) more ED visits per 1,000 beneficiaries in the first year than they would have otherwise and 4 (0.4 percent) more in the second year, but these estimates were not statistically significant (Exhibit C.13).
- The home-based primary care group had 9 (6.3 percent) more potentially avoidable ED visits per 1,000 beneficiaries in the first year and 5 (3.3 percent) more in the second year than the comparison group did. These estimates were not statistically significant (Exhibit C.13).
- In both years, home-based primary care increased the probability of having a qualifying hospital discharge and an unplanned readmission within 30 days of discharge by 2.0 percentage points (7.5 percent) in the first year and 0.9 percentage points (3.5 percent) in the second year. These estimates were not statistically significant (Exhibit C.13)

Exhibit C.13. Regression-adjusted means and estimated impact of home-based primary care on Medicare service use and health outcomes

Outcome and period	Regression-adjusted mean for home-based primary care group	Regression-adjusted mean for matched comparison group	Difference-in-differences estimate (standard error)	Percentage impact ^a
Number of hospital admissions per 1,000 beneficiaries per year^b				
One year prior	2,111	2,247	–	–
One year post	1,430	1,479	87 (63)	4.1
Two years post	1,205	1,292	49 (68)	2.3
Number of potentially avoidable hospital admissions per 1,000 beneficiaries per year^b				
One year prior	532	562	–	–
One year post	400	409	21 (33)	3.9
Two years post	310	348	-8 (34)	-1.5
Number of ED visits that did not result in admission per 1,000 beneficiaries per year^c				
One year prior	1,170	1,243	–	–
One year post	993	1,034	33 (59)	2.8
Two years post	896	965	4 (70)	0.4
Number of potentially avoidable ED visits that did not result in admission per 1,000 beneficiaries per year^c				
One year prior	149	175	–	–
One year post	126	142	9 (18)	6.3
Two years post	113	134	5 (20)	3.3
Percentage of beneficiaries with a qualifying discharge and an unplanned readmission within 30 days of discharge				
One year prior	26.2%	28.3%	–	–
One year post	20.4%	20.5%	2.0% (1.7%)	7.5
Two years post	16.1%	17.2%	0.9% (1.8%)	3.5

Source: Mathematica’s analysis of Medicare and Medicaid claims, assessment, and enrollment data for 2009 to 2013 obtained from the Chronic Conditions Warehouse for home-based primary care recipients and matched comparison group beneficiaries.

Note: We annualized measures of admissions and ED visits for beneficiaries that we did not observe for the full year.

^a Percentage impact is relative to home-based primary care group mean in the year before starting home-based primary care.

^b This includes inpatient hospital admissions and observation stays.

^c We measured this as specified in the Center for Medicare & Medicaid Innovation’s Priority Measures for Monitoring and Evaluation.

*/**/** The difference is statically significant at the 0.10/0.05/0.01 level.

ED = emergency department.

B. Comparison to findings from previous analysis of home-based primary care

Although some of our findings for this dually eligible beneficiary subgroup are consistent with the findings for the full group of Medicare beneficiaries—dually eligible and non-dually eligible—we previously examined, there are some important differences. In the previous analysis of all Medicare beneficiaries,⁵⁶ we found that home-based primary care led to statistically significant reductions in Medicare SNF and outpatient expenditures as well as increases in home health service, hospice, durable medical equipment, and clinician/supplier expenditures, which is consistent with the findings for the dually eligible subgroup. But our analysis of all Medicare beneficiaries also found that home-based primary care recipients had statistically significant increases in total and inpatient expenditures and hospital use, which differs from our findings of no impact of home-based primary care on inpatient expenditures or use for dually eligible beneficiaries. These differences in findings could be attributable in part to the smaller sample size for our dually eligible subgroup analysis or the different matching approach we used for this subgroup analysis, in which we did not use baseline outcome measures in our propensity score model as we did for matching the full group of Medicare beneficiaries. The differences in findings could also relate to the different service coverage for the samples. Many dually eligible beneficiaries (both home-based primary care recipients and matched comparison beneficiaries) received Medicaid HCBS that provided formal supports in the home, such as personal care, to which non-dually eligible Medicare beneficiaries may not have had access, which could have affected inpatient expenditures and hospital use.

⁵⁶ For more information, refer to the [evaluation report](#) which covered Years 1 to 4 of the IAH demonstration.

Appendix D

Association between home-based primary care and Medicare expenditures at the end of life: technical appendix and supplemental exhibits

I. Overview

This appendix provides details on the data, methods, and results for our analysis of the association between home-based primary care and end-of-life expenditures. Section II presents our approach to identifying the sample, Section III describes the data and outcomes, Section IV includes our estimation methods, and Section V includes supplementary exhibits.

II. Identifying the sample

The analysis sample consisted of Medicare beneficiaries who met the eligibility criteria for the Independence at Home (IAH) demonstration as of January 1, 2017; lived in an area where Medicare fee-for-service (FFS) beneficiaries received home-based primary care; and died in 2017 or 2018. In this section, we describe how we constructed the analysis sample.

A. Identifying beneficiaries who met the IAH eligibility criteria

The IAH eligibility criteria require that sample members had enrolled in FFS Medicare, had two or more activities of daily living requiring human assistance, had two or more chronic conditions, had an inpatient hospital admission or observation stay in the previous 12 months, and used acute or subacute rehabilitation services in the previous 12 months.

We required enrollment in Parts A and B, Medicare as primary payer, and no enrollment in Medicare Advantage or Programs of All-Inclusive Care for the Elderly plan. We retained beneficiaries who satisfied these Medicare enrollment criteria from January 2015 to their death in 2017 or 2018. These criteria enabled us to observe service use and expenditures in Medicare claims during the periods in which we measured home-based primary care use (2015 to the date of death), observed control variables, measured IAH eligibility (2016), and examined home-based primary care use and expenditures in the period preceding death (2017 and 2018, depending on the date of death).

To determine whether beneficiaries had two or more activities of daily living that required human assistance, we used Medicare assessment data in the same manner we used those data to identify IAH-eligible beneficiaries for the demonstration impact analysis. Appendix A of this report describes this process in detail.

We used the chronic condition flags from the Chronic Conditions Warehouse to count the number of chronic conditions as of January 1, 2017. We retained beneficiaries who had two or more chronic conditions using the end-of-year flag from 2016.

We used inpatient, outpatient, home health, and skilled nursing facility (SNF) claims data to identify Medicare FFS beneficiaries who had a hospital admission or observation stay and used rehabilitation services during 2016.

B. Identifying home-based primary care use and excluding beneficiaries with prior home-based primary care use

We defined home-based primary care as a primary care provider visit in a private home or assisted living facility with a billing code for evaluation and management. Primary care providers included nurse practitioners, physician assistants, and physicians who specialized in general practice, family practice, internal medicine, geriatric medicine, or preventive medicine. We did not consider specialist visits to be

home-based primary care even if the specialist billed for an evaluation and management visit. This approach is consistent with our prior analysis of home-based primary care use and other prior work that examined the provision and effects of home-based primary care (De Jonge et al. 2014; Yao et al. 2016).⁵⁷ With this definition, it is possible that some sample members who we classified as nonusers could have received primary care in the home (or assisted living facility) from a specialist physician, but we believe that it would be rare. In a separate analysis (described in Appendix E of this report) we examined episodes of home-based primary care use that occurred, at least in part, in 2016 (not limited to IAH-eligible beneficiaries). Of these episodes, 5 percent were specialist-only episodes. Of those episodes, 64 percent were a single visit (by comparison, 39 percent of episodes that were not specialist only had just one visit). The IAH-eligible population would be much less likely than a more general population to have only specialty care.

We wanted to ensure that receipt of home-based primary care did not affect our determination of IAH eligibility or the control variables. Therefore, we retained beneficiaries who had no home-based primary care use in 2015 or 2016 during the IAH eligibility period. This restriction also ensured that earlier use of home-based primary care did not affect end-of-life expenditures for beneficiaries who we considered nonusers (that is, those who did not use home-based primary care in 2017 or 2018).

We included beneficiaries in the home-based primary care group if they had any home-based primary care visits in 2017 or 2018. Not all users, however, had home-based primary care during the end-of-life period; in all, 35 percent of users stopped home-based primary care before the beginning of the three-month end-of-life period. We do not know a priori whether any possible effects of home-based primary care on end-of-life expenditures would have persisted after a beneficiary stopped receiving primary care in the home. If home-based primary care has a lasting effect on expenditures, then it is reasonable to keep users in the sample even if they stop receiving primary care in the home. But if home-based primary care is most effective when used during the end-of-life period, including those who stopped before then would tend to bias the estimated association between home-based primary care and expenditures to zero. The issue of the relative timing of home-based primary care visits and the end-of-life period is complex. For example, there could be cases in which home-based primary care use leads a beneficiary to choose to enter hospice care and stop receiving home-based primary care, as evidenced by our results showing hospice expenditures that are substantially higher for low-intensity users than high-intensity users and nonusers. Restricting the measure of home-based primary care use to beneficiaries who received visits during the end-of-life period would fail to account for these beneficiaries.

C. Excluding beneficiaries who reside in areas in which home-based primary care is not available

Because expenditure patterns may differ in areas in which home-based primary care is available compared with areas in which it is not, we excluded beneficiaries who did not reside in an area in which home-based primary care was available as of January 1, 2017. We classified a ZIP code as one in which home-based primary care was available if in both 2016 and 2017 two or more beneficiaries residing in the ZIP code used home-based primary care.

⁵⁷ For more information, refer to the [evaluation report](#) which covered Years 1 to 4 of the IAH demonstration.

D. Excluding beneficiaries who did not reside in the community

We used the daily residency flags in the Timeline file to identify beneficiaries who resided in the community as of January 1, 2017. The Timeline file combines data from claims (inpatient, SNF, and home health) and assessment data (Minimum Data Set and Outcome and Assessment Information) to flag a beneficiary's residency status for each day of a calendar year. The daily residency flag can contain one of the following values: H (home health agency); O (Outcome and Assessment Information); I (inpatient); S (SNF); M (Minimum Data Set, which includes nursing home days not paid by Medicare); C (community, which includes days identified by home health claims or Outcome and Assessment Information assessment data and days with no claims or assessment data); D (dead); or blank (not Medicare eligible). We considered all days flagged with a C, H, O or that were blank to be community days. We restricted our sample to beneficiaries residing in the community as of that date. Medicare beneficiaries who did not reside in the community—which includes those in a hospital, in a SNF, or in a nursing facility—could not receive primary care in the home. Furthermore, beneficiaries who did not reside in the community may have differed from those who do in ways for which we could not control. We retained beneficiaries in the sample, however, if they transitioned from the community into an institution after January 1, 2017, because that transition may have been related to the receipt of home-based primary care.

E. Identifying date of death

We used Medicare enrollment data to determine date of death, retaining beneficiaries who died in 2017 or 2018.

F. Identifying end-of-life periods

Our study examined beneficiaries who died in 2017 or 2018. In our analyses, we measured end-of-life expenditures in three durations preceding death: 3, 6, and 12 months. Therefore, we retained beneficiaries who died in or after March 2017 in the sample for the 3-month end-of-life duration and excluded beneficiaries who died in January or February 2017. By the same logic, beneficiaries in the 6-month sample died in or after June 2017 and beneficiaries in the 12-month sample died in or after December 2017.

G. Sample sizes

We applied the criteria described above in an order that allowed for efficient processing of the data sets that were of considerable size. Exhibit D.1 shows the order of the sample construction process and the number of beneficiaries remaining in contention for sample inclusion at each step. In Exhibit D.2, we show the resulting sample sizes for the 3-, 6-, and 12-month analyses, including the number of home-based primary care users and nonusers comprising the comparison group in each sample.

Exhibit D.1. Sample inclusion criteria for home-based primary care users and non-users

Step	Inclusion criteria	Number of beneficiaries remaining in contention	Number of beneficiaries excluded	Percentage excluded of those remaining after prior step
1	As of January 1, 2017, lived in an area in which home-based primary care was available in 2016	55,836,709	n.a.	n.a.
2	Two or more ADLs requiring human assistance	4,005,317	51,831,392	92.8
3	Died in 2017 or 2018	1,056,697	2,948,620	73.6
4	Two or more chronic conditions as of January 1, 2017	829,052	227,645	21.5
5	Continuous Medicare FFS coverage, 2015 death	636,260	192,792	23.3
6	Inpatient or observation stay in 2016	500,003	136,257	21.4
7	Acute or subacute rehabilitation services in 2016	474,430	25,573	5.1
8	In Timeline file	474,425	5	< 1.0
9	Resided in the community as of January 1, 2017	333,768	140,657	29.6
10	No home-based primary care in 2015 or 2016	264,466	69,302	20.8
11	As of January 1, 2017, lived in an area in which home-based primary care was available in 2017	192,959	71,507	27.0
12	Died in March 2017 or later ^a	172,770	20,189	10.5
13	Died in June 2017 or later ^b	143,304	29,466	17.1
14	Died in December 2017 or later ^c	94,578	48,726	34.0

Source: Mathematica’s analysis of Medicare claims, assessment, and enrollment data for 2015–2018 from the Chronic Conditions Warehouse.

^a Beneficiaries who survived at least through March 2017 make up the sample we used to examine expenditures in the last 3 months of life.

^b Beneficiaries who survived at least through June 2017 make up the sample we used to examine expenditures in the last 6 months of life.

^c Beneficiaries who survived at least through December 2017 make up the sample we used to examine expenditures in the last 12 months of life.

ADL = activity of daily living; FFS = fee-for-service; n.a. = not applicable.

Exhibit D.2. Sample size, by end-of-life duration

	Home-based primary care users	Nonusers (comparison group)	Total sample size
3 months	22,371	150,399	172,770
6 months	20,602	122,702	143,304
12 months	15,423	79,155	94,578

Source: Mathematica’s analysis of Medicare claims, assessment, and enrollment data for 2015–2018 from the Chronic Conditions Warehouse for home-based primary care users and nonusers who met IAH eligibility criteria.

III. Data and outcomes

We used Medicare administrative data to determine sample eligibility and measure outcomes (Exhibit D.3).

Exhibit D.3. Medicare data sources

Data	Demographic characteristics ^a	Chronic conditions and HCC score	Activities of daily living	Service use: sample eligibility	Expenditure outcome measures
Medicare enrollment database	✓				
Master Beneficiary Summary file	✓	✓			
Inpatient claims		✓		✓	✓
Outpatient claims		✓		✓	✓
Clinician/supplier claims		✓		✓	✓
Home health agency claims				✓	✓
Skilled nursing facility claims				✓	✓
Hospice claims					✓
Durable medical equipment claims					✓
Inpatient rehabilitation facility–patient assessment instrument ^b			✓		
Minimum Data Set			✓		
Outcome and assessment information set			✓		
Timeline file				✓	

^a We used demographic characteristics to define sample eligibility and as control variables for the regression analyses.

^b This includes inpatient rehabilitation hospitals and rehabilitation units. It excludes long-term care hospitals. HCC = hierarchical condition category.

A. Hierarchical condition category score and indicators

To account for differences in health status and the differential risks of incurring high Medicare expenditures, we used the CMS hierarchical condition category (HCC) risk-adjustment model to create HCC scores and indicators. For each beneficiary in the analysis sample, we estimated the HCC scores and indicators by using the publicly available HCC software and information on demographics, Medicare eligibility, and dual eligibility status as well as Medicare claims. We used Version 21 of the HCC model, developed and calibrated for the Programs of All-Inclusive Care for the Elderly population, because that population resembles the IAHH-eligible population in being sicker and frailer than the average Medicare beneficiary.

We used the HCC score estimated by the community model rather than the institutional or new enrollee models because all beneficiaries in our sample resided in the community as of January 1, 2017 and had been in Medicare FFS since 2015.

Most HCC indicators that we used as controls were based on diagnoses observed during 2016. We used a second observation based on 2015 diagnoses for a subset of HCC indicators. We identified the subset of HCCs observed in both time periods in consultation with a primary care clinician as conditions that typically progress over time. Including observations from 2015 and 2016 enabled us to control for some dynamics in beneficiaries' health conditions in the years preceding death.

B. Dual eligibility

We measured Medicaid dual eligibility using the monthly dual status code variables from the Medicare Master Beneficiary Summary file. If a beneficiary had full Medicaid benefits in any month in 2016, we identified that person as being dually eligible in that year.

C. Measuring intensity of home-based primary care use

We categorized beneficiaries who used home-based primary care into three levels of intensity: low, medium, and high. We counted the number of home-based primary care visits between the month of the first home-based primary care visit during or after January 2017 and the month of death. We divided this count by the number of months between the first home-based primary care visit and death to create a measure of monthly visits. On average, home-based primary care users had 0.7 visits per month (Exhibit D.4 shows the distribution of this measure), which is about one visit every six weeks. We used tercile cutoffs to assign users to each category; the 33rd percentile was 0.4 visits per month (about every 11 weeks), and the 66th percentile was 0.9 visits per month (about every five weeks). Exhibit D.5 shows the number of users in each category for the 3-, 6-, and 12-month duration samples.

Exhibit D.4. Distribution of home-based primary care visits per month (percentile)

5th	10th	25th	33rd	50th	66th	75th	90th	95th
0.1	0.1	0.3	0.4	0.6	0.9	1.0	1.5	2.0

Source: Mathematica's analysis of Medicare claims, assessment, and enrollment data for 2015–2018 from the Chronic Conditions Warehouse for home-based primary care users and nonusers who met IAHH eligibility criteria.

Exhibit D.5. Sample size including intensity categories by end-of-life duration

	Home-based primary care users					Total sample size
	Nonusers	All users	Low-intensity users	Medium-intensity users	High-intensity users	
3 months	150,399	22,371	7,758	7,395	7,218	172,770
6 months	122,702	20,602	7,390	6,780	6,432	143,304
12 months	79,155	15,423	5,848	4,980	4,595	94,578

Source: Mathematica’s analysis of Medicare claims, assessment, and enrollment data for 2015–2018 from the Chronic Conditions Warehouse for home-based primary care users and nonusers who met IAH eligibility criteria.

Note: We categorized home-based primary care users as high-, medium-, and low-intensity users based the number of monthly visits between the month of the first home-based primary care visit in 2017 or 2018 and the month of death.

D. Outcome variables

The outcomes of interest for our analysis were expenditures measured at the beneficiary level in three durations preceding death: 3, 6, and 12 months. We calculated expenditures as per beneficiary per month over the duration of the end-of-life period. We constructed expenditure measures for total Medicare expenditures and separately for inpatient, home health (including Part A, Part B, and other home health expenditures), outpatient, SNF, clinician/supplier, hospice, and durable medical equipment.

IV. Estimation methods

We used an ordinary least squares regression model to examine the association between home-based primary care and end-of-life expenditures. For each of the three end-of-life durations, we estimated a model for each expenditure category and for total expenditures. Equation (1) specifies the regression model we used:

$$(1) Y_i = \alpha + \beta'X_i + \gamma.HBPC_i + \omega_i$$

where Y_i is the expenditure outcome variable for beneficiary i ; α is a constant term; X_i is a set of beneficiary characteristics measured in 2016 or 2015. In the analyses comparing home-based primary care users with nonusers, $HBPC_i$ is a binary indicator for use of home-based primary care. In the analyses in which we measured intensity of use, $HBPC_i$ is a categorical variable for no, low-, medium-, and high-intensity use of home-based primary care. Finally, ω_i is a random error term.

Using the coefficients obtained from Equation (1), we estimated the average outcomes for home-based primary care users and nonusers adjusted to reflect the covariate distribution of the full sample of IAH-eligible beneficiaries who lived in an area in which home-based primary care was used and who died in 2017 or 2018.

We included controls for demographic characteristics and health status (Exhibit D.6). We observed the health status variables in the period before when home-based primary care users began receiving primary care at home. We did not include prior expenditures in our set of controls to avoid potential bias, but home-based primary care users had lower total Medicare expenditures in 2015 and 2016 compared with nonusers (\$2,064 versus \$2,380 in 2015 and \$4,297 versus \$4,785 in 2016). The characteristics of users do vary across intensity level in generally the same way that home-based primary care users vary with non-users. In other words, the characteristics of low-intensity home-based primary care users tend to look the most like those of nonusers, and high-intensity users look the least like nonusers.

Exhibit D.6. Mean values of beneficiaries' characteristics included as control variables for impact estimation

Variable	Home-based primary care users	Nonusers
Demographic characteristics		
Age, percentage		
Younger than 65	3.3	6.5
65 to 74	11.1	17.2
75 to 79	11.6	14.1
80 to 84	17.8	18.0
85 or older	56.3	44.2
Sex		
Female	60.5	55.9
Race and ethnicity, percentage		
White	89.0	85.7
Black	6.9	9.3
Hispanic	1.5	1.8
Asian	1.2	1.5
Other or unknown	1.4	1.8
Entitlement status, percentage		
Dually eligible for Medicare and Medicaid	12.9	16.0
Original reason for Medicare entitlement, percentage		
Old age	86.9	80.5
Disability only	12.2	17.4
ESRD or ESRD and disability	0.9	2.1
Health status characteristics		
Number of ADLs requiring human assistance, percentage		
2	10.8	12.4
3 or 4	33.7	34.4
5 or 6	55.6	53.2
HCC risk score		
As of December 2015	2.6	2.8
As of December 2016	3.9	4.2

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Exhibit D.6 (continued)

Variable	Home-based primary care users	Nonusers
Individual HCCs, percentage		
HCC 8, metastatic cancer and acute leukemia	4.4	7.4
HCC 9–10, lung and other severe cancers, lymphoma and other cancers	6.2	7.9
HCC 11–12, colorectal, bladder, breast, prostate, and other cancers and tumors	11.6	11.5
HCC 18, diabetes with chronic complications, 2015	23.8	27.6
HCC 18, diabetes with chronic complications, 2016	30.4	35.2
HCC 21, protein-calorie malnutrition	14.9	16.8
HCC 27, end-stage liver disease, 2015	0.9	1.3
HCC 27, end-stage liver disease, 2016	1.4	2.3
HCC 28–29, cirrhosis of liver, chronic hepatitis, 2015	1.4	1.9
HCC 28–29, cirrhosis of liver, chronic hepatitis, 2016	1.8	2.6
HCC 46, severe hematological disorders	2.4	2.9
HCC 48, coagulation defects and other specified hematological disorders	17.9	20.1
HCC 51, dementia with complications	13.5	8.8
HCC 52, dementia without complication	33.1	25.8
HCC 54–55, drug/alcohol psychosis, drug/alcohol dependence	5.3	6.7
HCC 57–58, schizophrenia; major depressive, bipolar, and paranoid disorders, 2015	10.0	9.4
HCC 57–58, schizophrenia; major depressive, bipolar, and paranoid disorders, 2016	17.5	15.6
HCC 70–71, quadriplegia, paraplegia, 2015	1.0	1.1
HCC 70–71, quadriplegia, paraplegia, 2016	2.2	1.9
HCC 72, spinal cord disorders/injuries	1.9	1.9
HCC 85, congestive heart failure	53.3	57.4
HCC 96, specified heart arrhythmias	50.7	51.6
HCC 103–104, hemiplegia/hemiparesis, monoplegia, other paralytic syndromes, 2015	4.4	4.5
HCC 103–104, hemiplegia/hemiparesis, monoplegia, other paralytic syndromes, 2016	9.7	9.7
HCC 106, atherosclerosis of the extremities with ulceration or gangrene	3.9	5.2
HCC 107-108, vascular disease with complications, vascular disease	49.4	48.4
HCC 111, chronic obstructive pulmonary disease	35.2	41.3
HCC 134, dialysis status, 2015	3.0	5.5
HCC 134, dialysis status, 2016	3.9	7.4
HCC 136–138, chronic kidney disease, stages 3–5	11.9	11.1
HCC 139–140, chronic kidney disease, stages 1–2 or unspecified renal failure	5.5	5.1

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Exhibit D.6 (continued)

Variable	Home-based primary care	
	users	Nonusers
HCC 157–159, pressure ulcer of skin with necrosis through to muscle, tendon, or bone, 2015	3.7	4.0
HCC 157–159, pressure ulcer of skin with necrosis through to muscle, tendon, or bone, 2016	10.6	10.5
Number of chronic conditions measured by Chronic Conditions Warehouse		
2 to 5	11.6	11.6
6 to 9	49.6	49.2
10 or more	38.7	39.2
Number squared	82.7	83.0
Specific chronic conditions, percentage		
Alzheimer's or related disorders	56.8	45.0
Acute myocardial infarction or ischemic heart disease	64.3	67.2
Asthma	11.4	12.7
Depression	44.6	41.6
Hip or pelvic fracture	8.4	7.9
Stroke or transient ischemic attack	18.3	17.2
Other measures of health status		
Anemia ^a	20.0	21.0
Fluid and electrolyte disorder ^a	40.2	41.2
Chronically critically ill or medically complex diagnosis ^b	33.1	38.0
DRG with complicating condition or major complicating condition	59.3	63.7

Source: Mathematica's analysis of Medicare claims, assessment, and enrollment data for 2015–2018 from the Chronic Conditions Warehouse for home-based primary care users and nonusers who met IAH eligibility criteria.

^a We measured this with claims from the most recent hospital admission or observation stay before January 1, 2017. We drew diagnosis codes for these conditions from Gagne et al. (2011).

^b We measured this using diagnoses from the most recent hospital admission in the year before January 1, 2017. We drew diagnoses from Kandilov et al. (2014).

ADL = activity of daily living; DRG = diagnosis-related group; ESRD = end-stage renal disease; HCC = hierarchical condition category.

V. Supplementary exhibits and findings

A. Regression estimates of mean end-of-life expenditures

Exhibit D.7 shows regression-adjusted mean end-of-life expenditures of home-based primary care users and nonusers. Exhibit D.8 shows regression-adjusted mean end-of-life expenditures by intensity of use and for nonusers. Exhibit D.9 shows whether regression-adjusted mean expenditures were statistically significantly different between levels of intensity.

Exhibit D.7. Comparison of regression-adjusted mean Medicare expenditures per beneficiary per month

	Nonusers	Home-based primary care users	Difference in mean expenditures (standard error)	Percentage difference ^a
Total				
3 months	\$9,360	\$8,969	-\$391*** (\$65)	-4.2
6 months	\$7,421	\$7,707	\$285*** (\$50)	3.8
12 months	\$5,575	\$6,193	\$618*** (\$40)	11.1
Inpatient				
3 months	\$5,251	\$4,627	-\$624*** (\$56)	-11.9
6 months	\$3,818	\$3,675	-\$142*** (\$41)	-3.7
12 months	\$2,630	\$2,739	\$109*** (\$30)	4.1
Skilled nursing facility				
3 months	\$999	\$914	-\$85*** (\$15)	-8.5
6 months	\$894	\$951	\$56*** (\$13)	6.3
12 months	\$705	\$832	\$127*** (\$10)	18.0
Home health (Parts A and B)				
3 months	\$195	\$313	\$118*** (\$3)	60.2
6 months	\$270	\$435	\$165*** (\$3)	61.2
12 months	\$294	\$469	\$175*** (\$4)	59.4
Hospice				
3 months	\$1,162	\$1,415	\$253*** (\$11)	21.7
6 months	\$847	\$1,016	\$169*** (\$10)	20.0
12 months	\$559	\$690	\$130*** (\$9)	23.3
Outpatient				
3 months	\$457	\$417	-\$40*** (\$7)	-8.7
6 months	\$508	\$475	-\$33*** (\$7)	-6.6
12 months	\$503	\$479	-\$24*** (\$7)	-4.8
Clinician/supplier				
3 months	\$1,247	\$1,222	-\$25** (\$10)	-2.0
6 months	\$1,027	\$1,085	\$58*** (\$9)	5.6
12 months	\$824	\$914	\$90*** (\$8)	10.9
Durable medical equipment				
3 months	\$49	\$61	\$12*** (\$2)	25.4
6 months	\$58	\$70	\$12*** (\$2)	21.0
12 months	\$60	\$71	\$11*** (\$2)	18.7

Source: Mathematica’s analysis of Medicare claims, assessment, and enrollment data for 2015–2018 from the Chronic Conditions Warehouse for home-based primary care users and nonusers who met IAH eligibility criteria.

^a Percentage is relative to the adjusted mean of nonusers.

*/**/** The difference is statistically significant at the 0.10/0.05/0.01 levels, respectively.

Exhibit D.8. Comparison of regression-adjusted mean Medicare expenditures per beneficiary per month, by intensity of home-based primary care use

	Regression-adjusted mean end-of-life expenditures			
	Non-users	Low-intensity users	Medium-intensity users	High-intensity users
Total				
3 months	\$9,360	\$9,234	\$8,939	\$8,713
6 months	\$7,422	\$7,990	\$7,606	\$7,484
12 months	\$5,576	\$6,469	\$6,086	\$5,955
Inpatient				
3 months	\$5,251	\$4,738	\$4,632	\$4,502
6 months	\$3,818	\$3,813	\$3,627	\$3,567
12 months	\$2,630	\$2,851	\$2,677	\$2,662
Skilled nursing facility				
3 months	\$999	\$1,033	\$910	\$788
6 months	\$894	\$1,037	\$934	\$869
12 months	\$705	\$917	\$821	\$735
Home health (Parts A and B)				
3 months	\$195	\$201	\$320	\$427
6 months	\$270	\$334	\$447	\$539
12 months	\$294	\$400	\$490	\$535
Hospice				
3 months	\$1,162	\$1,626	\$1,417	\$1,183
6 months	\$847	\$1,223	\$1,004	\$789
12 months	\$559	\$850	\$667	\$509
Outpatient				
3 months	\$457	\$415	\$396	\$442
6 months	\$508	\$475	\$461	\$489
12 months	\$503	\$496	\$462	\$474
Clinician/supplier				
3 months	\$1,247	\$1,169	\$1,203	\$1,298
6 months	\$1,027	\$1,046	\$1,061	\$1,156
12 months	\$824	\$889	\$896	\$966
Durable medical equipment				
3 months	\$49	\$51	\$62	\$72
6 months	\$57	\$63	\$72	\$75
12 months	\$60	\$67	\$73	\$74

Source: Mathematica’s analysis of Medicare claims, assessment, and enrollment data for 2015–2018 from the Chronic Conditions Warehouse for home-based primary care users and nonusers who met IAH eligibility criteria.

Note: We categorized home-based primary care users as high-, medium-, and low-intensity users based the number of monthly visits between the month of the first home-based primary care visit in 2017 or 2018 and the month of death.

Exhibit D.9. Tests for difference across intensity categories in regression-adjusted mean Medicare expenditures per beneficiary per month, *p*-value

	Low versus medium	Low versus high	Medium versus high
Total			
3 months	0.04	<0.01	0.13
6 months	<0.01	<0.01	0.29
12 months	<0.01	<0.01	0.16
Inpatient			
3 months	0.40	0.06	0.31
6 months	0.04	0.01	0.51
12 months	0.01	<0.01	0.82
Skilled nursing facility			
3 months	<0.01	<0.01	<0.01
6 months	<0.01	<0.01	0.02
12 months	<0.01	<0.01	<0.01
Home health (Parts A and B)			
3 months	<0.01	<0.01	<0.01
6 months	<0.01	<0.01	<0.01
12 months	<0.01	<0.01	<0.01
Hospice			
3 months	<0.01	<0.01	<0.01
6 months	<0.01	<0.01	<0.01
12 months	<0.01	<0.01	<0.01
Outpatient			
3 months	0.21	0.07	<0.01
6 months	0.35	0.39	0.08
12 months	0.04	0.19	0.48
Clinician/supplier			
3 months	0.13	<0.01	<0.01
6 months	0.41	<0.01	<0.01
12 months	0.68	<0.01	<0.01
Durable medical equipment			
3 months	0.03	<0.01	0.05
6 months	0.06	0.01	0.50
12 months	0.30	0.21	0.83

Source: Mathematica’s analysis of Medicare claims, assessment, and enrollment data for 2015–2018 from the Chronic Conditions Warehouse for home-based primary care users and nonusers who met IAH eligibility criteria.

Note: We categorized home-based primary care users as high-, medium-, and low-intensity users based on the number of monthly visits between the month of the first home-based primary care visit in 2017 or 2018 and the month of death. Cells shaded in gray show differences that were significant at the 0.10 level.

B. Comparison to findings from previous analysis of home-based primary care

The analysis described in this memo is similar in certain respects to a prior analysis with some important improvements.⁵⁸ The prior analysis compared end-of-life expenditures between IAH-eligible home-based primary care users who died from 2010 to 2016 with a set of IAH-eligible nonusers who died in the same time period. The comparison was not regression adjusted and examined expenditures only in the period three months before death. It did not consider intensity of exposure to home-based primary care. By contrast, in the current analysis, we used more recent data (deaths in 2017 and 2018), longer time periods, and adjusted the comparison of average expenditures to control for observable differences between users and nonusers.

Despite the differences in methods, sample, and analysis period, the results of the prior analysis were qualitatively similar to the results presented in this memo for the three-month end-of-life period. We found that home-based primary care users had lower total expenditures as well as inpatient, SNF, clinician/supplier, and outpatient expenditures and higher hospice, home health, and durable medical equipment expenditures in the three months preceding death.

⁵⁸ For more information, refer to the [evaluation report](#) which covered Years 1 to 4 of the IAH demonstration.

Appendix E

Home visit episodes by Medicare beneficiaries

I. Overview

The Independence at Home (IAH) demonstration was designed to test a payment incentive and service delivery model for providing home-based primary care to chronically ill and functionally limited Medicare beneficiaries. Understanding how beneficiaries are using home-based primary care—and the extent to which some of them receive it during short episodes rather than indefinitely—can inform future analyses of and models or policies related to home-based primary care. We included all beneficiaries enrolled in Medicare fee for service (FFS) with a home visit episode because the number of home-based primary care recipients nationwide who are eligible for the demonstration is limited since the demonstration requires a beneficiary to have recent hospital use and functional limitations, among other criteria. We did, however, describe differences between home visit episodes of all FFS beneficiaries with the home visit episodes of IAH beneficiaries.

In this appendix, we begin by describing our approach to measuring home visit episodes by Medicare FFS beneficiaries. Next, we provide results from our descriptive analysis of home visit episodes.

II. Measuring home visit episodes

Study population and measurement of home visits. We studied the national population of Medicare FFS beneficiaries who had a home visit in 2016. Beneficiaries had to be continuously enrolled in Medicare FFS (Parts A and B, with Medicare the primary payer) from 2015 to 2017 for all months they were alive. Specifically, we used clinician/supplier and outpatient claims to identify all beneficiaries who had a home visit provided by a physician (including specialists), nurse practitioner (NP), physician assistant (PA), Rural Health Clinic, or Federally Qualified Health Center in 2016.⁵⁹ We included specialists because we wanted to understand patterns of episodes that included home visits by geriatric psychiatrists and other physicians. Home visits include visits for evaluation and management services, annual wellness, transitional care management (TCM), and advance care planning in a private home or other home setting.

Data. We constructed an episode-level file for this analysis using multiple data sources: Medicare claims and enrollment data from January 2014 to April 2018 and the Timeline file. For more information about the Timeline file, see Appendix A.

Identifying an episode of care. All episodes involved a home visit in 2016. To determine the beginning of an episode, we started with the first home visit observed in 2016 and looked at prior claims until we identified one of the following: (1) an office-based visit from a primary care physician, NP, or PA or (2) a gap of 64 days or more with no home visits.⁶⁰ We truncated the search process as of January 1, 2015. The first home visit we observed following an office-based primary care visit or 64-day gap, whether it took place in 2016 or 2015, defined the beginning of the episode. To determine the end of an episode, we looked at claims after the first home visit until we identified one of the following: (1) an office-based visit from a primary care physician, NP, or PA;⁶¹ (2) a gap of 64 days or more with no home visits; or (3)

⁵⁹ We used outpatient claims with a revenue center code indicating a home visit by a clinician from a Rural Health Clinic or Federally Qualified Health Center.

⁶⁰ We selected a gap of 64 days after examining the distribution of home-based visits and observing a noticeable drop in home visits after nine weeks (63 days).

⁶¹ The reason that we did not use an office-based visit from a specialist physician to identify the end of a home visit episode is that we know from our analyses of patients treated by IAH practices that many beneficiaries who receive

death. The last home visit before the office-based primary care visit or the 64-day gap defined the end of the episode or death. This process enabled us to combine multiple home visits into a single episode of care. For example, if a beneficiary had three home visits within 45 days, and the next home visit was 100 days later, then the episode began on the date of the first home visit and ended on the date of the last (third) home visit before the 100-day gap. We truncated the search process after December 2017.

If the first episode ended before December 2016, we searched from the end of the first episode to identify the next home visit. That next home visit started the second episode, and the same rules applied; we looked at subsequent claims through December 2017 until we identified an office-based primary care visit or a gap of 64 days or more with no home visits, which ended the second episode. We repeated this process for additional episodes until all home visits in 2016 were contained within an episode. For episodes in which the 64-day window extended past December 2016, we looked at claims in 2017 and continued to combine home visits into an episode until the episode ended because of an office-based primary care visit or we saw a 64-day gap in home visits. We did not extend episodes past December 31, 2017. Using this approach, all episodes included at least one home visit in 2016.

If at least one home visit during the episode was in an assisted living facility, group home, or custodial care facility, we identified the episode as occurring in an assisted living facility.⁶² This means that we classified episodes that had at least one visit in a private residence and at least one visit in a facility as occurring in an assisted living facility; we included these with episodes that had only assisted living facility visits because people who enter an assisted living facility tend to remain there for quite some time (AHCA/NCAL).⁶³

Using the first day of the episode, we used Medicare FFS claims to measure service use and expenditures that occurred in the 90 days before the episode and used the Timeline file to determine days in an inpatient, a skilled nursing, a nursing home, or a community setting; we also measured expenditures in the 12 months before the episode. Using the last day of the episode, we identified the following in the 90 days after the episode: service use and expenditures; days in an inpatient, a skilled nursing, a nursing home, or a community setting; and date of death. If the 90-day period following a first episode overlapped the start of a second episode, the overlapping service use and expenditures were counted in both the 90-day period and the second episode. We used claims from January 1, 2014, to April 1, 2018, and the Timeline file to create these measures. Finally, we used the Centers for Medicare & Medicaid Services (CMS) Hierarchical Condition Categories (HCC) risk-adjustment model to create HCC scores and indicators using claims from the 12 months before the episode. For more information about measuring HCC score, see Appendix A.

Comparison to IAH beneficiaries. To understand how home visit episodes differed for IAH beneficiaries compared with all Medicare FFS beneficiaries, we compared data for all episodes provided to Medicare FFS beneficiaries that included at least one home visit in 2016 (including IAH beneficiaries) and the subgroup of these episodes that were provided to IAH beneficiaries. We used the IAH beneficiaries who were in the IAH group for the evaluation during Year 4 (9,504 beneficiaries identified

home-based primary care continue to receive office-based specialty care. Using office-based specialist visits to end home visit episodes likely would have prematurely ended many home visit episodes.

⁶² CMS has established that if an individual in an assisted living facility comes to a central office in the facility for the visit, that claim should be coded as an office visit. If the visit takes place in the individual's room, then the visit should be coded as an assisted living facility visit.

⁶³ According to the National Center for Assisted Living, the median stay in an assisted living facility is around 22 months.

in Exhibit A.6) or Year 5 (9,958 beneficiaries identified in Exhibit A.6). We chose these years because they occurred from October 2015 to September 2017, which overlapped with the time period during which we identified episodes (2016). For more information about how we identified the IAH group for the evaluation, see Chapter III of Appendix A.

Episodes in assisted living facilities. We examined the data separately for episodes that took place in an assisted living facility and those that took place in a private residence because increases in the use of home-based primary care have been concentrated largely among beneficiaries in assisted living facilities (Reckrey et al. 2020, Schuchman et al. 2018, Yao et al. 2018).

III. Analysis

To understand how Medicare beneficiaries use home visits and the services they receive before, during, and after an episode, we created frequencies of the number of episodes, the length of episodes, and the characteristics of the beneficiaries at the time the episode began. We also calculated the intensity of services by the length of the episode. In this appendix, we present an array of data and focus our comments on those we found to be most notable.

We identified nearly 1.3 million home visit episodes in 2016 (Exhibit E.1). Just over half (52 percent) of the home visit episodes occurred in an assisted living facility. IAH beneficiaries comprised just 1.6 percent of the full population of 734,569 beneficiaries who had at least one episode.

Exhibit E.1. Number of episodes and beneficiaries used for analyses of home visit episodes

	Number of episodes	Number of beneficiaries
Total episodes	1,269,406	734,569
Episodes that included a visit in an ALF (as a percentage of total)	664,539 (52.4%)	389,581 (53.0%)
Episodes for IAH beneficiaries ^a (as a percentage of total)	20,410 (1.6%)	11,541 (1.6%)

Source: Mathematica’s analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

^a For information about how we identified IAH beneficiaries for the evaluation, see Appendix A.

ALF = assisted living facility.

IV. Results for the full sample of home visit episodes

A. How many home visits occurred per episode, how long did episodes last, and how did episodes end?

The distribution of the number of home visits per episode was skewed. Although the average was 6.3 visits per episode, more than 60 percent of episodes had three or fewer visits (Exhibit E.2). A large minority (40 percent) of episodes had one home visit. About half of all episodes lasted five weeks or fewer. More than 20 percent lasted more than seven months.

More than half (54 percent) of episodes ended with a gap in care of 64 days or more, and a little less than one-third (32 percent) ended with an office-based visit with a primary care physician, NP, or PA. About 10 percent of episodes ended because the beneficiary died.

Of the episodes that ended because the beneficiary received an office-based primary care visit, a substantial minority of cases (23 percent) were followed by another home visit episode within 30 days (Exhibit E.3). In most cases, beneficiaries who had an episode that ended because the beneficiary received an office-based primary care visit had only one interrupted episode; only 35 percent had 2 or more episodes interrupted by an office-based primary care visit (Exhibit E.4).

The 42 percent of episodes that were not followed by a subsequent episode during the study period reflected a variety of outcomes: about one-quarter of these episodes were followed by death, and many of the others were followed by hospice use, nursing home entry, or a new home visit episode that began in 2018 (the study period ended December 31, 2017). Later in this appendix, we discuss service use in the 90 days after the end of an episode.

Exhibit E.2. Percentage of home visit episodes by length and home visit count

Episode length	1 visit	2 to 3 visits	4 to 5 visits	6 to 10 visits	11 to 30 visits	More than 30 visits	All episodes
All episodes	40.0	22.7	9.2	10.5	13.8	3.8	-
1 day	40.0	0.1	0.0	0.0	0.0	0.0	40.1
2 to 7 days	0.0	1.0	0.0	0.0	0.0	0.0	1.0
2 to 5 weeks	0.0	9.1	0.6	0.1	0.0	0.0	9.9
6 weeks to 3 months	0.0	11.1	3.3	1.1	0.1	0.0	15.7
4 to 6 months	0.0	1.3	4.9	4.2	0.8	0.0	11.3
7 months to 1 year	0.0	0.0	0.3	4.8	4.2	0.1	9.5
More than 1 year	0.0	0.0	0.0	0.2	8.6	3.7	12.5

Source: Mathematica’s analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

Exhibit E.3. Percentage of home visit episodes by days from the end of an episode until the beginning of the next episode

Days until next episode began	Percentage of episodes
Any episode followed by another home visit episode during the study period	58.0
Next episode began 7 or fewer days after previous episode ended	2.6
Next episode began 8 to 30 days after previous episode ended	19.9
Next episode began 31 to 63 days after previous episode ended	21.9
Next episode began 64 days or more after previous episode ended	13.5

Source: Mathematica’s analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

Exhibit E.4. Percentage of beneficiaries by number of interrupted episodes during the study period

Beneficiaries with ended episodes	Percentage of beneficiaries
Any episode ended by an office-based primary care visit	31.9 ^a
1 episode ended	65.0
2 to 3 episodes ended	26.0
4 or more episodes ended	9.0

Source: Mathematica’s analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

^a Beneficiaries with any number of episodes interrupted by an office-based primary care visit were 31.9 percent (234,128 beneficiaries) of the full sample of 734,569 beneficiaries.

B. How many episodes did each beneficiary have, and what were the characteristics of these beneficiaries?

Of beneficiaries who had at least one home visit episode in 2016, more than half (57 percent) had only one episode (Exhibit E.5). One-quarter had two, and about 18 percent had three or more episodes.

We examined beneficiaries’ characteristics in two ways: at the episode level and at the beneficiary level (regardless of the number of episodes). Data at the beneficiary level tell us about the characteristics of the entire population of FFS beneficiaries who had a home visit episode in 2016, and comparing these with episode-level data enable us to understand which characteristics were associated with more (or fewer) episodes per person. In general, characteristics did not differ substantively between the episode and beneficiary levels (Exhibit E.6). The slightly higher prevalence of a few characteristics at the episode level—such as being younger than age 65, entitled to Medicare because of a disability, or dually eligible for Medicare and Medicaid—suggests that beneficiaries in these groups tended to have more episodes per person than beneficiaries who were not in these groups. The average HCC score was considerably higher at the episode level (2.6) than at the beneficiary level (1.5), suggesting that sicker beneficiaries had more episodes than those who were less sick.

Compared with all Medicare beneficiaries in 2016, beneficiaries who had home visits were more likely to be women, older, and dually eligible. Two-thirds of those with a home visit episode were women compared with 55 percent of all Medicare beneficiaries; 46 percent were age 85 or older compared with 11 percent of all Medicare beneficiaries; and 31 percent were dually eligible compared with 20 percent of all Medicare beneficiaries (CMS 2019a). More than half of home visit episodes took place in an assisted living facility or other group residence. Beneficiaries who received home visits had poorer health status than the average Medicare beneficiary. The average HCC score was 1.5 for beneficiaries with a home visit episode compared with an average of 1.0 for all Medicare beneficiaries. More than 44 percent of beneficiaries with any home visit had dementia, either without complications (31 percent) or with complications (15 percent), compared with 11 percent of all Medicare beneficiaries (Chronic Conditions Data Warehouse 2020).⁶⁴

Exhibit E.5. Percentage of beneficiaries by number of episodes during the study period

Number of episodes per beneficiary during the study period	Percentage of beneficiaries
1 episode	56.6
2 episodes	25.0
3 episodes	11.9
4 or more episodes	6.5

⁶⁴ There are some differences between the HCC-based measure of dementia (used in Exhibit E.6) and the Chronic Conditions Warehouse measure of dementia. The biggest difference is that the HCC-based measure is based on claims from the previous year, whereas the Chronic Conditions Warehouse–based measure is based on claims from the previous three years. If we had data on the prevalence of the HCC-based measure of dementia for the entire Medicare FFS population, the difference in prevalence between beneficiaries who had a home visit episode and that of all FFS beneficiaries would have been even larger than the difference we report here.

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Source: Mathematica's analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

Exhibit E.6. Demographic characteristics of beneficiaries who received home visits

Characteristic	Percentage of episodes	Percentage of beneficiaries ^a
Sex		
Women	66.5	66.2
Age		
Younger than 65	14.3	12.5
65 to 74	15.2	15.1
75 to 84	25.9	26.6
85 and older	44.5	45.7
Medicare entitlement reason		
Aged	74.0	76.3
Disability only	25.4	23.1
ESRD only	0.2	0.2
ESRD and disability	0.4	0.4
Dually eligible		
Yes	33.6	31.0
ALF		
Yes	52.6	52.4
Race		
Unknown	0.4	0.4
White	81.7	83.0
Black	12.8	11.7
Other	1.1	1.0
Asian	1.6	1.5
Hispanic	2.2	2.2
North American Native	0.3	0.3
HCC score and most common conditions^b		
HCC score, average	2.6	1.5
HCC 108, vascular disease	37.8	36.8
HCC 85, congestive heart failure	30.6	30.7
HCC 52, dementia without complications	29.6	30.1
HCC 96, specified heart arrhythmias	27.2	28.0
HCC 111, chronic obstructive pulmonary disease	25.8	25.2
HCC 18, diabetes without chronic complications	21.6	20.7
HCC 75, polyneuropathy	20.0	18.9
HCC 58, major depressive, bipolar, and paranoid disorders	16.5	15.7
HCC 135, acute renal failure	13.8	14.7
HCC 51, dementia with complications	14.0	14.5

Exhibit E.6. (continued)

Source: Mathematica's analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

^a Characteristics were measured for each episode during 2014 to 2018. The same beneficiary may have had different characteristics appearing in multiple episodes over time. Time-varying beneficiary characteristics in this table were estimated as the proportion of characteristics among episodes weighted by the inverse of the beneficiary's total episodes.

^b HCC health conditions were measured for each episode during 2014 to 2018. The same beneficiary may have had different conditions appearing in multiple episodes over time. Percentages of beneficiaries in this table were estimated as the proportions of conditions among episodes weighted by the inverse of the beneficiary's total episodes. For more information about how we measured the HCC score, see Appendix A.

ALF = assisted living facility; ESRD = end-stage renal disease; HCC = Hierarchical Condition Categories.

C. What was the average rate of home visits per episode, and which types of clinicians provided home visits?

Episodes that lasted two to seven days tended to include an average of two visits (Exhibit E.7). Episodes that were six weeks or longer tended to average about one visit every three to four weeks, even for episodes longer than one year.

Primary care physicians and NPs provided most home visits—primary care physicians provided 47 percent of visits, and NPs provided about 37 percent (Exhibit E.8). The longer an episode, the smaller the share of home visits provided by a specialist physician or psychiatrist, but the larger the share provided by a PA. Psychiatrists proportionately provided about as many home visits as all other physician specialists.

Exhibit E.7. Average (and median) number of home visits in episode lengths greater than one day

Episode length	Percentage of episodes	Number of visits overall	Number of visits per week
2 to 7 days	1.0	2.1 (2.0)	2.1 (2.0)
2 to 5 weeks	9.9	2.4 (2.0)	0.7 (0.7)
6 weeks to 3 months	15.7	3.3 (3.0)	0.4 (0.3)
4 to 6 months	11.3	6.0 (5.0)	0.3 (0.3)
7 months to 1 year	9.5	11.6 (10.0)	0.3 (0.3)
More than 1 year	12.5	27.2 (23.0)	0.3 (0.3)

Source: Mathematica’s analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

Exhibit E.8. Percentage of visits that occurred during episodes by type of clinician and episode length

Episode length	Percentage of episodes	Primary care physician	Nurse practitioner	Physician assistant	Specialist physician	Psychiatrist	FQHC & RHC ^a
Any length (all episodes)	100.0	47.3	36.7	6.2	3.7	3.6	2.5
1 day	40.1	49.0	34.1	5.2	4.4	4.2	3.0
2 to 7 days	1.0	42.6	38.9	5.6	4.5	3.2	5.2
2 to 5 weeks	9.9	43.5	40.4	6.2	3.6	3.5	2.7
6 weeks to 3 months	15.7	47.8	37.1	6.2	3.4	3.2	2.3
4 to 6 months	11.3	47.5	38.0	6.5	2.9	2.8	2.1
7 months to 1 year	9.5	46.3	39.0	7.2	2.7	2.8	2.0
More than 1 year	12.5	45.4	38.4	7.9	3.0	3.6	1.8

Source: Mathematica’s analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

^a Measured using outpatient claims with a revenue center code indicating a home visit by a clinician from a Rural Health Clinic or Federally Qualified Health Center.

FQHC = Federally Qualified Health Center; RHC = Rural Health Clinic.

D. Which types of service use other than home visits occurred during home visit episodes?

More than half of home visit episodes did not include office-based specialist visits, which is not surprising given how short many of the episodes were. Also, some beneficiaries who receive home visits may have substantial difficulty attending an office-based specialist visit. Among episodes that included office-based specialist visits, the average number of office-based specialist visits increased with the length of the episode (Exhibit E.9).

Home health agency service use was common among the longest episodes—more than half of episodes that lasted more than six months included home health use (Exhibit E.10). For all episode lengths, a

slightly higher proportion of episodes included at least one home health skilled nursing visit than included a visit for physical, occupational, or speech therapy.

Half of episodes that lasted longer than one year had at least one emergency department (ED) visit, and 40 percent had at least one hospital admission (Exhibit E.11). Even among shorter episodes such as four to six months, a substantial minority had an ED visit (27 percent) and a hospital admission (19 percent). As with ED and hospital use, skilled nursing facility (SNF) and hospice use increased in prevalence as episodes grew longer, suggesting that beneficiaries with longer episodes tended to be more ill and closer to death than those with short episodes.

Use rates for any durable medical equipment were relatively uniform for episodes from two days to three months and were more common for longer episodes, suggesting that beneficiaries who had longer episodes may have been more functionally impaired than those with shorter episodes.

Across episodes of any length, community residence predominated at more than 90 percent of days (Exhibit E.12). One exception was episodes that lasted two to seven days; these episodes had a somewhat higher rate of SNF days, which could reflect beneficiaries who were discharged home from the hospital before entering a SNF.

Exhibit E.9. Average (and median) number of office-based specialist visits per episode by episode length

Episode length	Overall	Conditional on having at least one office-based specialist visit
Any length (all episodes)	0.6 (0.0)	3.2 (2.0)
1 day	0.0 (0.0)	1.0 (1.0)
2 to 7 days	0.0 (0.0)	1.1 (1.0)
2 to 5 weeks	0.2 (0.0)	1.3 (1.0)
6 weeks to 3 months	0.4 (0.0)	1.7 (1.0)
4 to 6 months	0.8 (0.0)	2.4 (2.0)
7 months to 1 year	1.5 (0.0)	3.4 (2.0)
More than 1 year	2.5 (0.0)	5.3 (3.0)

Source: Mathematica’s analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

Exhibit E.10. Percentage of episodes having home health visits on a claim during the episode

Episode length	Any home health visit	Home health skilled nursing	Home health therapy ^a	Home health aide
Any length (all episodes)	24.8	21.6	18.8	3.2
1 day	1.0	0.9	0.7	0.1
2 to 7 days	6.5	5.9	5.0	0.8
2 to 5 weeks	15.2	13.1	10.6	1.9
6 weeks to 3 months	29.2	25.4	18.7	3.8
4 to 6 months	42.7	36.8	30.6	5.5
7 months to 1 year	54.8	47.5	43.2	7.0
More than 1 year	66.1	58.3	55.4	8.7

Source: Mathematica’s analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

^a Includes physical, occupational, and speech therapy.

Exhibit E.11. Percentage of episodes by episode length with the following service uses during the episode

Episode length	Percentage of episodes	Hospital use	ED visits	SNF	Any DME ^a	DME hospital bed ^a	DME wheelchair ^a	Hospice
Any length (all episodes)	100.0	12.3	17.1	4.3	38.8	6.6	9.7	5.7
1 day	40.1	0.4	0.4	1.2	31.7	4.0	6.0	0.4
2 to 7 days	1.0	2.2	5.2	4.8	36.2	4.8	7.9	2.5
2 to 5 weeks	9.9	6.4	9.6	2.5	37.3	5.6	8.4	4.3
6 weeks to 3 months	15.7	11.2	15.7	3.3	37.5	6.3	9.2	6.3
4 to 6 months	11.3	18.7	26.8	5.4	41.7	7.7	11.5	9.0
7 months to 1 year	9.5	28.2	39.8	8.7	47.7	9.7	14.3	12.7
More than 1 year	12.5	39.5	53.4	12.4	55.3	12.5	18.4	15.4

Source: Mathematica’s analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

^a These measures reflect DME use during the episode or in the previous 90 days because DME is often used for weeks or months beyond the from date for the DME claim. For example, if a beneficiary had a DME claim for a wheelchair 45 days before the episode, then we identified the episode as having wheelchair use.

ED = emergency department; DME = durable medical equipment.

Exhibit E.12. Average percentage of days during the episode spent in different settings by episode length

Episode length	Hospital ^a	SNF	Nursing home	Community
Any length (all episodes)	1.0	1.3	1.9	95.8
1 day ^b	0.6	1.2	1.8	96.3
2 to 7 days	1.2	4.3	1.4	92.9
2 to 5 weeks	1.6	1.9	1.5	95.0

Episode length	Hospital ^a	SNF	Nursing home	Community
6 weeks to 3 months	1.4	1.5	1.7	95.4
4 to 6 months	1.2	1.2	2.0	95.5
7 months to 1 year	1.1	1.1	2.3	95.5
More than 1 year	0.8	0.9	2.4	95.9

Source: Mathematica’s analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

^a Reflects days covered by an inpatient claim, which includes days in facilities such as general acute care hospitals, psychiatric and other specialty hospitals, long-term care hospitals, and inpatient rehabilitation facilities.

^b A one-day home visit episode could have occurred on the same day that a beneficiary was admitted to a hospital, a SNF, or a nursing home.

SNF = skilled nursing facility.

E. To what extent did clinicians providing home visits offer other services?

Because beneficiaries who receive home visits are older and in poorer health than the average Medicare beneficiary, we examined the extent to which clinicians providing home visits offered chronic care management (CCM) services, TCM services, home health or hospice oversight, and advance care plan services. We expected that rates of these services would be fairly low because some beneficiaries did not qualify for them during the episode (particularly during short episodes), and CMS only introduced some of the services not long before the study period.⁶⁵ For example, a beneficiary is eligible for TCM only after discharge from a facility such as a hospital or a SNF, and, as we discuss in the following section, only a minority of episodes were preceded by a hospital stay in the previous 90 days (19 percent of episodes) or a SNF stay (12 percent of episodes).

Overall, about 6 percent of episodes had CCM, 3 percent had TCM, 5 percent had home health or hospice oversight and close to 3 percent had advance care planning services (Exhibit E.13). As expected, provision of these services was higher among long episodes. Among episodes lasting longer than a year, 22 percent included CCM, 6 percent included TCM, 15 percent included oversight of home health or hospice, and 7 percent included advance care planning services.

Exhibit E.14 limits data to episodes in which a home visit clinician provided a certain type of service (CCM, TCM, home health or hospice oversight, or advance care planning) and examines the length of the episode. Almost none of the episodes that included CCM or home health or hospice oversight services were single-day episodes. Of the episodes with TCM given by a home visit clinician, however, 31 percent lasted only one day. These data may suggest that a fairly substantial share of beneficiaries who had a home visit for TCM returned to office-based primary care after a single home visit. Also, of the episodes with advance care planning provided by a home visit clinician, 15 percent lasted only one day.

⁶⁵ Our study sample included all episodes provided to Medicare FFS beneficiaries that included at least one home visit in 2016. CMS introduced CCM in January 2015 and increased reimbursement in 2017. CMS introduced TCM in 2013 and advance care plan services in 2016. Until 2021, a clinician could not bill CCM and TCM in the same month. The study period predated the introduction by CMS of reimbursement for cognitive assessment and care plan services in January 2017.

Exhibit E.13. Percentage of episodes by episode length in which a home visit clinician provided a particular service during the episode

Episode length	Chronic care management	Transitional care management	Home health oversight or hospice oversight	Advance care planning
Any length (all episodes)	6.0	2.9	4.8	2.6
1 day	0.1	2.3	0.2	1.0
2 to 7 days	0.6	3.8	0.6	1.8
2 to 5 weeks	2.7	2.3	2.8	1.8
6 weeks to 3 months	4.9	2.0	5.3	2.0
4 to 6 months	8.2	2.8	7.4	2.9
7 months to 1 year	13.2	4.2	10.2	4.5
More than 1 year	22.0	5.8	14.6	7.3

Source: Mathematica’s analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

Exhibit E.14. Distribution of episode length among episodes in which a home visit clinician provided a particular service

Episode length	Chronic care management	Transitional care management	Home health oversight or hospice oversight	Advance care planning
Total episodes during which a home visit clinician provided the service (percentage)	76,503 (100.0)	37,174 (100.0)	60,994 (100.0)	32,487 (100.0)
1 day	0.9	30.9	1.6	15.0
2 to 7 days	0.1	1.3	0.1	0.7
2 to 5 weeks	4.4	7.9	5.8	7.0
6 weeks to 3 months	12.8	10.5	17.2	12.2
4 to 6 months	15.4	11.0	17.3	12.9
7 months to 1 year	20.7	13.6	20.0	16.5
More than 1 year	45.6	24.8	38.0	35.7

Source: Mathematica’s analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

F. Which types of health care did beneficiaries receive in the 90 days before the home visit episode and in the 90 days after the home visit episode?

About 19 percent of home visit episodes were preceded by a hospital admission in the previous 90 days, compared with 12 percent that were preceded by a SNF stay (Exhibit E.15). Almost 38 percent of episodes were preceded by home health care use in the previous 90 days, compared with only 4 percent preceded by hospice use.

The largest difference between use in the 90 days before and after the episode was for hospice—although only 4 percent of episodes were preceded by hospice use, 12 percent were followed by hospice use (Exhibit E.16). About 12 percent of episodes were followed by death within 90 days. Of beneficiaries who died on the episode end date or in the following 90 days, the average time from the end of the episode to date of death was 30 days (Exhibit E.17).

We identified some differences in the use of services in the 90 days after the episode for beneficiaries that died versus those that survived 90 days. Episodes in which the beneficiary died in the following 90 days were much more likely to be followed by hospice services or a hospital admission than episodes in which the beneficiary survived 90 days.

Most people who entered a nursing home tended to do so fairly quickly. The median time from the end of the episode to nursing home entry was 14 days, which was longer than the median time from the end of the episode to admission to a hospital or other inpatient facility (23 days) or a SNF (25 days).

Exhibit E.15. Percentage of episodes with various types of health care use in the preceding 90 days

Type of health care use	Percentage of episodes
Hospital	19.2
Home health care	37.5
SNF	11.9
Nursing home	4.5
Hospice	4.0

Source: Mathematica’s analysis of Medicare claims, Medicare enrollment, and Timeline file data for 2014–2018 from the Chronic Conditions Warehouse.

SNF = skilled nursing facility.

Exhibit E.16. Percentage of episodes followed within 90 days by various types of health care use or death

Type of health care use or death	Overall percentage of episodes	Episodes in which the beneficiary survived 90 days after episode ended	Episodes in which the beneficiary died within 90 days after episode ended
Hospital	18.4	15.7	38.2
Home health	33.9	35.0	27.0
SNF	8.9	8.5	11.9
Nursing home	5.5	5.0	9.0
Hospice	12.0	6.3	52.4
Death	12.4	n.a.	100.0

Source: Mathematica’s analysis of Medicare claims, Medicare enrollment, and Timeline file data for 2014–2018 from the Chronic Conditions Warehouse.

n.a. = not applicable; SNF = skilled nursing facility.

Exhibit E.17. Average (and median) number of days after a home visit episode until a particular outcome, among only episodes having that outcome on or within 90 days of the day the episode ended

Type of health care use or death	All episodes	Episodes in which the beneficiary survived 90 days after episode ended	Episodes in which the beneficiary died within 90 days after episode ended
Hospital ^a	30.4 (23.0)	34.8 (29.0)	17.6 (11.0)
SNF	30.7 (25.0)	32.8 (28.0)	20.5 (16.0)
Nursing home	25.2 (14.0)	26.5 (14.0)	20.1 (12.0)
Death	n.a.	n.a.	30.5 (23.0)

Source: Mathematica’s analysis of Medicare claims, Medicare enrollment, and Timeline file data for 2014–2018 from the Chronic Conditions Warehouse.

Note: We report days from end of episode to a given outcome using settings identified by the Timeline file: inpatient, SNF, or nursing home.

^a Reflects days covered by an inpatient claim, which includes days in facilities such as general acute care hospitals, psychiatric and other specialty hospitals, long-term care hospitals, and inpatient rehabilitation facilities.

n.a. = not applicable; SNF = skilled nursing facility.

G. What was the average daily Medicare expenditures before, during, and after the home visit episode, and did the average vary by length of the episode?

To compare expenditures for different episode lengths, we examined average daily expenditures. Average daily expenditures in the 365-day period before the episode were lower than expenditures in the previous 90 days in nearly all categories, with the largest difference for inpatient and SNF expenditures (Exhibit E.18). Inpatient and SNF expenditures averaged \$26 and \$12 a day in the year before the episode, respectively, compared with \$35 and \$18 a day in the 90 days prior, suggesting that the costly onset of an acute health condition or exacerbation of a chronic condition may have prompted some home visit episodes.

Higher average daily clinician/supplier expenditures during the episode (\$56) relative to the 90 days before (\$16) and 90 days after (\$15) was because of the cost of the home visit or visits during short episodes (Exhibits E.20 through E.22). For example, except for the few episodes that included a home visit provided by a Federally Qualified Health Center or Rural Health Clinic (visits in which the expenditures appeared in outpatient claims), all one-day episodes included clinician/supplier expenditures for a home visit, leading to average clinician/supplier expenditures of \$112 per day for one-day episodes. Also, on average, about 21 percent of expenditures during one-day episodes were for inpatient expenditures, which generally reflected episodes in which a beneficiary was admitted to the hospital on the same day he or she received a home visit.

Exhibit E.18. Average per-day Medicare expenditures before, during, and after a home visit episode by types of service

Time period	Total	Inpatient	Outpatient	SNF	Clinician/supplier	Home health	Hospice	DME
365 days before episode	\$73	\$26	\$7	\$12	\$14	\$10	\$4	\$1
90 days before episode	\$92	\$35	\$7	\$18	\$16	\$11	\$5	\$1
During episode	\$129	\$31	\$9	\$7	\$56	\$16	\$7	\$3
90 days after episode ^a	\$90	\$31	\$7	\$16	\$15	\$12	\$8	\$1

Source: Mathematica’s analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

^a Average expenditures for the 90 days after the episode are based on beneficiaries who survived the full 90-day period.

DME = durable medical equipment; SNF = skilled nursing facility.

Exhibit E.19. Average per-day Medicare expenditures during the 365 days before a home visit episode by types of service and episode lengths

Episode length	Total	Inpatient	Outpatient	SNF	Clinician/ supplier	Home health	Hospice	DME
1 day	\$71	\$26	\$7	\$10	\$14	\$9	\$4	\$1
2 to 7 days	\$91	\$36	\$9	\$15	\$18	\$9	\$3	\$2
2 to 5 weeks	\$85	\$33	\$8	\$14	\$16	\$10	\$3	\$1
6 weeks to 3 months	\$74	\$26	\$7	\$12	\$14	\$10	\$4	\$1
4 to 6 months	\$74	\$26	\$6	\$13	\$13	\$10	\$4	\$1
7 months to 1 year	\$75	\$26	\$6	\$14	\$13	\$10	\$4	\$1
More than 1 year	\$69	\$23	\$5	\$13	\$13	\$11	\$3	\$1

Source: Mathematica’s analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

DME = durable medical equipment; SNF = skilled nursing facility.

Exhibit E.20. Average per-day Medicare expenditures during the 90 days before a home visit episode by types of service and episode lengths

Episode length	Total	Inpatient	Outpatient	SNF	Clinician/ supplier	Home health	Hospice	DME
1 day	\$89	\$35	\$8	\$14	\$16	\$10	\$5	\$2
2 to 7 days	\$137	\$60	\$10	\$26	\$24	\$11	\$4	\$2
2 to 5 weeks	\$115	\$47	\$9	\$22	\$19	\$11	\$4	\$2
6 weeks to 3 months	\$91	\$33	\$7	\$18	\$15	\$11	\$5	\$1
4 to 6 months	\$91	\$32	\$7	\$20	\$15	\$11	\$5	\$1
7 months to 1 year	\$95	\$33	\$6	\$23	\$15	\$11	\$5	\$1
More than 1 year	\$84	\$28	\$5	\$20	\$14	\$12	\$4	\$1

Source: Mathematica’s analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

DME = durable medical equipment; SNF = skilled nursing facility.

Exhibit E.21. Average per-day Medicare expenditures during a home visit episode by types of service and episode lengths

Episode length	Total	Inpatient	Outpatient	SNF	Clinician/ supplier	Home health	Hospice	DME
1 day	\$201	\$42	\$11	\$10	\$112	\$15	\$7	\$4
2 to 7 days	\$217	\$71	\$20	\$16	\$76	\$22	\$9	\$3
2 to 5 weeks	\$112	\$34	\$11	\$6	\$29	\$22	\$7	\$3
6 weeks to 3 months	\$82	\$25	\$8	\$6	\$18	\$17	\$7	\$2
4 to 6 months	\$74	\$21	\$7	\$5	\$16	\$16	\$8	\$2
7 months to 1 year	\$68	\$18	\$6	\$4	\$15	\$15	\$9	\$1
More than 1 year	\$58	\$14	\$5	\$3	\$13	\$14	\$8	\$1

Source: Mathematica’s analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

DME = durable medical equipment; SNF = skilled nursing facility.

Exhibit E.22. Average per-day Medicare expenditures during the 90 days after a home visit episode by types of service and episode lengths, among episodes where the beneficiary survived 90 days after the episode

Episode length	Total	Inpatient	Outpatient	SNF	Clinician/ supplier	Home health	Hospice	DME
1 day	\$78	\$27	\$8	\$10	\$14	\$12	\$6	\$2
2 to 7 days	\$115	\$42	\$10	\$20	\$20	\$15	\$7	\$2
2 to 5 weeks	\$101	\$36	\$9	\$17	\$17	\$14	\$7	\$2
6 weeks to 3 months	\$88	\$30	\$7	\$16	\$14	\$12	\$7	\$1
4 to 6 months	\$95	\$33	\$7	\$20	\$14	\$11	\$9	\$1
7 months to 1 year	\$109	\$38	\$6	\$25	\$15	\$11	\$11	\$1
More than 1 year	\$103	\$35	\$6	\$22	\$15	\$11	\$13	\$1

Source: Mathematica’s analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

DME = durable medical equipment; SNF = skilled nursing facility.

H. What was the relationship between the number of visits per episode and different characteristics of the episode?

Based on an exploratory analysis using a prediction model that included a host of variables, we identified several characteristics linked to receiving more home visits, and we descriptively examined the extent to which the number of visits per episode varied for different values of the characteristics. Specifically, we considered the share of all episodes with a certain characteristic that included only one home visit, which was the case for 40 percent of all episodes. Among the characteristics we identified as being linked to having more home visits, the following had somewhat less than 40 percent of episodes with only one home visit (meaning that these characteristics were associated with more visits per episode on average): residing in an assisted living facility; having dementia; having at least 21 days of acute inpatient or SNF care in the prior 90 days; having institutional nursing care in the prior 14 days; having total Medicare expenditures at or above the 25th percentile in the prior year; and having home health use in the prior 90 days (Exhibit E.23). For example, 33 percent of the episodes that occurred in an assisted living facility had one visit compared with 48 percent of those that took place in private residences. Similarly, about 35 percent of episodes for beneficiaries with dementia had one visit relative to 44 percent of episodes for beneficiaries without dementia.

The relationships between the number of visits per episode and assisted living facility residence and dementia may be seen more clearly in Exhibit E.24. As the number of visits per episode increases, the percentage of episodes in an assisted living facility and percentage of episodes in which the beneficiary had dementia are higher. Most episodes that had more than six visits were for beneficiaries living in an assisted living facility or who had dementia. More than 75 percent of the episodes that had at least than 31 visits occurred in an assisted living facility, and 55 percent of these episodes were for beneficiaries with dementia. But even though residing in an assisted living facility and having dementia were common among these episodes with many visits, episodes with many visits were still uncommon. For example, only 23 percent of episodes in an assisted living facility had more than 10 visits.

Exhibit E.23. Percentage of episode with a given number of visits, by episode characteristic

	1 visit	2 to 3 visits	4 to 5 visits	6 to 10 visits	11 to 30 visits	31+ visits	Total number of episodes
Episode occurred in an ALF							
Yes	33.1	22.4	10.0	12.0	16.9	5.6	664,539
No (private residence)	47.7	23.0	8.3	8.8	10.3	1.9	604,867
Dementia with or without complications							
Yes	34.7	22.4	9.9	11.8	16.3	4.8	553,909
No	44.2	22.8	8.7	9.4	11.7	3.1	715,497
Number of days of acute inpatient care or SNF services during the 90 days before the episode							
0 days	40.6	22.7	9.1	10.3	13.5	3.8	968,682
1 to 20 days	43.5	22.9	8.8	9.6	11.9	3.3	161,863
21+ days	32.4	22.2	10.3	13.2	17.4	4.5	138,861
Institutional nursing care (from SNF or nursing home) during the 14 days before the episode							
Yes	32.6	21.5	10.2	13.3	18.1	4.5	109,408
No	40.8	22.8	9.1	10.2	13.3	3.8	1,159,998
Total Medicare expenditures during the 365 days before episode							
<25th percentile	43.8	22.2	8.7	9.5	12.6	3.2	317,351
25th to <75th percentile	39.1	22.7	9.3	10.6	14.1	4.2	634,703
75th+ percentile	38.1	23.1	9.6	11.2	14.1	3.8	317,352
Home health use during the 90 days before the episode							
Yes	37.5	23.0	9.5	11.1	14.8	4.2	476,131
No	41.6	22.5	9.0	10.1	13.1	3.6	793,275

Source: Mathematica’s analysis of Medicare claims, Medicare enrollment and Timeline file data for 2014–2018 from the Chronic Conditions Warehouse.

ALF = assisted living facility; SNF = skilled nursing facility.

Exhibit E.24. Distribution of episode characteristic by number of visits per episode: assisted living, dementia, and prior Medicare expenditures

Number of visits	ALF		Dementia		Total number of episodes
	Yes	No	Yes	No	
1	43.3	56.7	37.8	62.2	508,372
2 to 3	51.7	48.3	43.2	56.8	287,701
4 to 5	57.0	43.0	46.8	53.2	117,034
6 to 10	60.1	39.9	49.3	50.7	133,105
11 to 30	64.3	35.7	51.9	48.1	174,552
31+	76.1	23.9	54.9	45.1	48,642

Source: Mathematica’s analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

ALF = assisted living facility.

V. Results for beneficiaries in the IAH evaluation sample

Before examining data on home visit episodes for IAH beneficiaries compared with all Medicare FFS beneficiaries, we assessed the two groups for similarities and differences. Using data from beneficiaries in

the IAH group for the evaluation of Year 5 of the demonstration, we saw that the average health status for IAH beneficiaries was substantially worse than for the full group of beneficiaries who had a home visit episode. We expected this finding because beneficiaries in the IAH group met demonstration eligibility criteria, including at least two chronic conditions, recent hospital admission, and requiring human assistance with at least two activities of daily living. The average HCC score was 4.1 for beneficiaries in the IAH group for the evaluation of Year 5 of the demonstration compared with 1.5 for all beneficiaries with a home visit episode. Beneficiaries in the IAH group had a substantially higher prevalence of conditions such as congestive heart failure, chronic obstructive pulmonary disease, and dementia.

A. How many home visits occurred per episode, how long did episodes last, and why did episodes end?

When we examined the distribution of the number of home visits per episode for IAH beneficiaries compared with episodes for all Medicare FFS beneficiaries, we found that IAH beneficiaries generally had longer episodes and more visits per episode. About 20 percent of the IAH beneficiaries’ episodes were just one day long compared with 40 percent of all Medicare FFS episodes (Exhibit E.25). At the longer end of the distribution, about 39 percent of the IAH episodes lasted longer than seven months, but just 22 percent of all episodes lasted more than seven months. Similarly, nearly half of IAH beneficiaries’ episodes had six or more visits, compared with 28 percent of all episodes. The average number of visits per episode was 50 percent higher for IAH beneficiaries (9.8 visits) than for all episodes (6.3 visits). Having longer episodes and more frequent visits is consistent with the goals and eligibility criteria of the IAH demonstration, which aimed to reduce expenditures and improve health outcomes among beneficiaries with multiple chronic conditions, functional limitations, and recent hospital admission.

Reasons for why the episode ended were similar for IAH beneficiaries and all episodes, but IAH beneficiaries whose episodes ended with an office-based primary care visit were more likely to have another home visit episode begin within 30 days of the previous home visit episode. Among episodes for IAH beneficiaries ended by an office-based primary care visit, 32 percent were followed by another home visit episode within 30 days, compared with 22 percent for all episodes. The percentage of episodes that ended because the beneficiary died were similar for IAH beneficiaries’ episodes (11 percent) and all episodes (10 percent).

Exhibit E.25. Percentage of home visit episodes by length and home visit count, IAH beneficiaries

Episode length	1 visit	2 to 3 visits	4 to 5 visits	6 to 10 visits	11 to 30 visits	More than 30 visits	All episodes
All episodes	20.3	19.8	10.8	16.6	26.3	6.2	-
1 day	20.3	0.1	0.0	0.0	0.0	0.0	20.3
2 to 7 days	0.0	0.7	0.0	0.0	0.0	0.0	0.7
2 to 5 weeks	0.0	7.9	0.8	0.1	0.0	0.0	8.8
6 weeks to 3 months	0.0	10.0	3.9	1.7	0.2	0.0	15.8
4 to 6 months	0.0	1.2	5.8	6.3	1.7	0.0	15.0
7 months to 1 year	0.0	0.0	0.3	8.1	7.7	0.5	16.6
More than 1 year	0.0	0.0	0.0	0.3	16.7	5.7	22.7

Source: Mathematica’s analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

B. What was the average daily Medicare expenditures before, during, and after the home visit episode, and did the average vary by length of the episode?

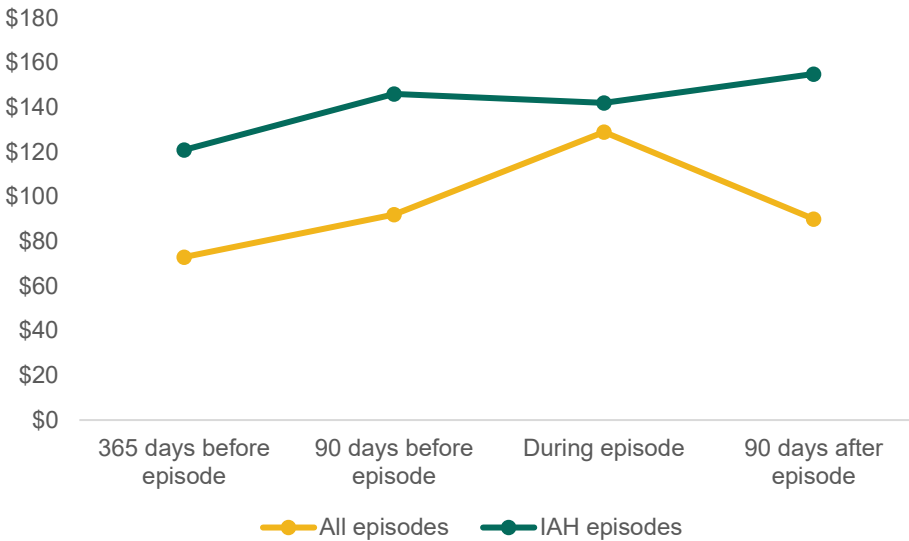
To qualify for the IAH demonstration, beneficiaries had to have a hospital admission and used rehabilitation services in the past year, so we expected that episodes for these beneficiaries would have been preceded by higher average Medicare expenditures compared to expenditures that preceded all episodes of Medicare FFS beneficiaries. And it was true: expenditures in the year before the episode were nearly two-thirds higher for IAH beneficiaries' episodes than for all episodes of Medicare FFS beneficiaries (\$121 PBPM and \$73 PBPM, respectively) (Exhibit E.26). The higher spending was largely because of inpatient and SNF expenditures; in the year before the episode, IAH beneficiaries had substantially higher average inpatient expenditures (96 percent higher) and SNF expenditures (75 percent higher) than all episodes of Medicare FFS beneficiaries.

We also expected that episodes for these beneficiaries would have been followed by higher average Medicare expenditures compared to expenditures that followed all episodes of Medicare FFS beneficiaries, since IAH beneficiaries had worse health. And it was also true: after excluding episodes where the beneficiary died within 90 days of the episode end date, we found that expenditures in the 90 days after the episode were more than two-thirds higher for IAH beneficiaries' episodes than for all episodes of Medicare FFS beneficiaries (\$155 PBPM and \$90 PBPM, respectively).

Within each group of beneficiaries (IAH and the full sample), average daily expenditures were similar in the 90 days before and after the episode. The major difference between the two groups was that the full population of beneficiaries had a sharp increase in total expenditures during the episode, but the IAH beneficiaries did not. For episodes for the full population of beneficiaries, total expenditures during the episode averaged \$129 a day compared with \$92 in the previous 90 days and \$90 in the following 90 days. Expenditures were somewhat more constant for IAH beneficiaries; \$142 a day during the episode compared with \$146 in the previous 90 days and \$155 in the following 90 days. Since both groups experienced large increases in clinician/supplier expenditures during the episode relative to the 90 days before and after the episode because of the cost of the home visit(s) during the episode, these different patterns were largely due to different changes in inpatient and SNF expenditures (Exhibits E.27 and E.28). For IAH beneficiaries, the increase in clinician/supplier expenditures during the episode was offset by decreases in inpatient and SNF expenditures. This was not true for the full population of beneficiaries with home visit episodes; the decreases in inpatient and SNF expenditures relative to before and after the episode were much smaller for the full population than for IAH beneficiaries.

Variation in expenditures during and after the episode by length for IAH beneficiaries' episodes was generally similar to variation by length for all beneficiaries' episodes (Exhibits E.32 and E.33).

Exhibit E.26. Average per-day total expenditures before, during, and after a home visit episode by types of service, all beneficiaries' episodes and IAH beneficiaries' episodes

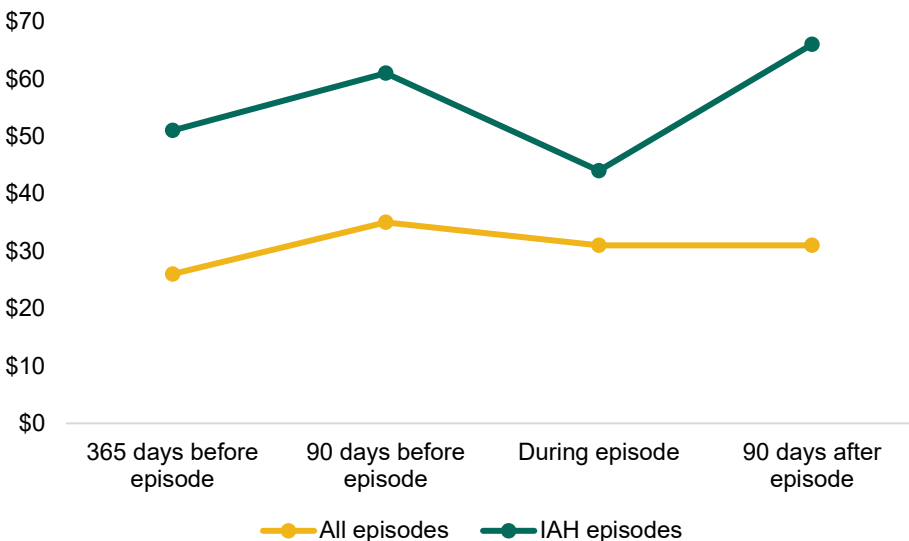


Source: Mathematica's analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

Notes: Average expenditures for the 90 days after the episode are based on beneficiaries who survived the full 90-day period.

SNF = skilled nursing facility.

Exhibit E.27. Average per-day inpatient expenditures before, during, and after a home visit episode by types of service, all beneficiaries' episodes and IAH beneficiaries' episodes

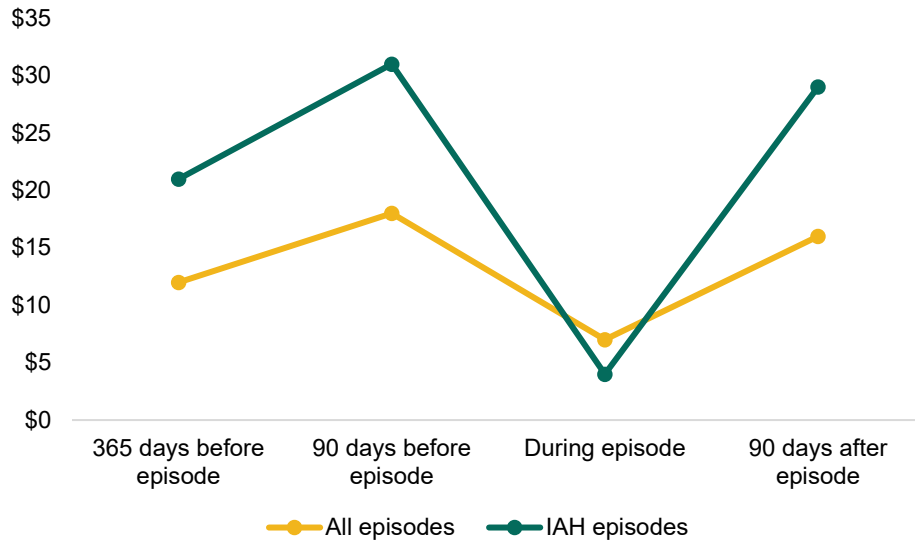


Source: Mathematica's analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

Note: Average expenditures for the 90 days after the episode are based on beneficiaries who survived the full 90-day period.

SNF = skilled nursing facility.

Exhibit E.28. Average per-day SNF expenditures before, during, and after a home visit episode by types of service, all beneficiaries' episodes and IAH beneficiaries' episodes



Source: Mathematica's analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

Note: Average expenditures for the 90 days after the episode are based on beneficiaries who survived the full 90-day period.

SNF = skilled nursing facility.

Exhibit E.29. Average per-day Medicare expenditures before, during, and after a home visit episode by types of service, all beneficiaries' episodes and IAH beneficiaries' episodes

Time period and population	Total	Inpatient	Outpatient	SNF	Clinician/ Home supplier health	Hospice	DME	
365 days before episode, IAH beneficiaries	\$121	\$51	\$9	\$21	\$20	\$16	\$1	\$2
365 days before episode, all beneficiaries	\$73	\$26	\$7	\$12	\$14	\$10	\$4	\$1
90 days before episode, IAH beneficiaries	\$146	\$61	\$10	\$31	\$22	\$18	\$2	\$2
90 days before episode, all beneficiaries	\$92	\$35	\$7	\$18	\$16	\$11	\$5	\$1
During episode, IAH beneficiaries	\$142	\$44	\$10	\$4	\$52	\$25	\$4	\$3
During episode, all beneficiaries	\$129	\$31	\$9	\$7	\$56	\$16	\$7	\$3
90 days after episode, IAH beneficiaries ^a	\$155	\$66	\$10	\$29	\$23	\$18	\$6	\$2
90 days after episode, all beneficiaries ^a	\$90	\$31	\$7	\$16	\$15	\$12	\$8	\$1

Source: Mathematica's analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

^a Average expenditures for the 90 days after the episode are based on beneficiaries who survived the full 90-day period.

DME = durable medical equipment; SNF = skilled nursing facility.

Exhibit E.30. Average per-day Medicare expenditures during the 365 days before a home visit episode by types of service and episode lengths, all beneficiaries' episodes and IAH beneficiaries' episodes

Episode length and population	Total	Inpatient	Outpatient	SNF	Clinician/ supplier	Home health	Hospice	DME
1 day, IAH beneficiaries	\$124	\$53	\$11	\$17	\$22	\$17	\$2	\$2
1 day, all beneficiaries	\$71	\$26	\$7	\$10	\$14	\$9	\$4	\$1
2 to 7 days, IAH beneficiaries	\$136	\$55	\$13	\$21	\$26	\$17	\$1	\$2
2 to 7 days, all beneficiaries	\$91	\$36	\$9	\$15	\$18	\$9	\$3	\$2
2 to 5 weeks, IAH beneficiaries	\$135	\$59	\$10	\$23	\$23	\$16	\$1	\$2
2 to 5 weeks, all beneficiaries	\$85	\$33	\$8	\$14	\$16	\$10	\$3	\$1
6 weeks to 3 months, IAH beneficiaries	\$125	\$53	\$10	\$23	\$21	\$16	\$1	\$2
6 weeks to 3 months, all beneficiaries	\$125	\$26	\$7	\$12	\$14	\$10	\$4	\$1
4 to 6 months, IAH beneficiaries	\$123	\$51	\$10	\$23	\$20	\$15	\$2	\$2
4 to 6 months, all beneficiaries	\$74	\$26	\$6	\$13	\$13	\$10	\$4	\$1
7 months to 1 year, IAH beneficiaries	\$121	\$51	\$9	\$23	\$19	\$15	\$1	\$2
7 months to 1 year, all beneficiaries	\$75	\$26	\$6	\$14	\$13	\$10	\$4	\$1
More than 1 year, IAH beneficiaries	\$107	\$43	\$7	\$18	\$18	\$16	\$1	\$2
More than 1 year, all beneficiaries	\$69	\$23	\$5	\$13	\$13	\$11	\$3	\$1

Source: Mathematica's analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

DME = durable medical equipment; SNF = skilled nursing facility.

Exhibit E.31. Average per-day Medicare expenditures during the 90 days before a home visit episode by types of service and episode lengths, all beneficiaries' episodes and IAH beneficiaries' episodes

Episode length	Total	Inpatient	Outpatient	SNF	Clinician/ supplier	Home health	Hospice	DME
1 day, IAH beneficiaries	\$135	\$57	\$12	\$19	\$22	\$19	\$3	\$2
1 day, all beneficiaries	\$89	\$35	\$8	\$14	\$16	\$10	\$5	\$2
2 to 7 days, IAH beneficiaries	\$170	\$72	\$14	\$31	\$30	\$19	\$1	\$3
2 to 7 days, all beneficiaries	\$137	\$60	\$10	\$26	\$24	\$11	\$4	\$2
2 to 5 weeks, IAH beneficiaries	\$179	\$79	\$11	\$38	\$27	\$20	\$1	\$2
2 to 5 weeks, all beneficiaries	\$115	\$47	\$9	\$22	\$19	\$11	\$4	\$2
6 weeks to 3 months, IAH beneficiaries	\$152	\$62	\$11	\$33	\$23	\$18	\$2	\$2
6 weeks to 3 months	\$91	\$33	\$7	\$18	\$15	\$11	\$5	\$1
4 to 6 months, IAH beneficiaries	\$157	\$64	\$11	\$38	\$23	\$17	\$2	\$2
4 to 6 months, all beneficiaries	\$91	\$32	\$7	\$20	\$15	\$11	\$5	\$1
7 months to 1 year, IAH beneficiaries	\$153	\$64	\$9	\$36	\$22	\$17	\$2	\$2
7 months to 1 year, all beneficiaries	\$95	\$33	\$6	\$23	\$15	\$11	\$5	\$1
More than 1 year, IAH beneficiaries	\$128	\$51	\$7	\$29	\$20	\$17	\$2	\$2
More than 1 year, all beneficiaries	\$84	\$28	\$5	\$20	\$14	\$12	\$4	\$1

Source: Mathematica's analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

DME = durable medical equipment; SNF = skilled nursing facility.

Exhibit E.32. Average per-day Medicare expenditures during a home visit episode by types of service and episode lengths, all beneficiaries' episodes and IAH beneficiaries' episodes

Episode length	Total	Inpatient	Outpatient	SNF	Clinician/ supplier	Home health	Hospice	DME
1 day, IAH beneficiaries	\$259	\$57	\$9	\$1	\$161	\$25	\$3	\$2
1 day, all beneficiaries	\$201	\$42	\$11	\$10	\$112	\$15	\$7	\$4
2 to 7 days, IAH beneficiaries	\$346	\$189	\$23	\$0	\$90	\$29	\$8	\$8
2 to 7 days, all beneficiaries	\$217	\$71	\$20	\$16	\$76	\$22	\$9	\$3
2 to 5 weeks, IAH beneficiaries	\$149	\$55	\$13	\$1	\$39	\$36	\$2	\$3
2 to 5 weeks, all beneficiaries	\$112	\$34	\$11	\$6	\$29	\$22	\$7	\$3
6 weeks to 3 months, IAH beneficiaries	\$119	\$44	\$11	\$5	\$25	\$27	\$4	\$2
6 weeks to 3 months, all beneficiaries	\$82	\$25	\$8	\$6	\$18	\$17	\$7	\$2
4 to 6 months, IAH beneficiaries	\$113	\$40	\$11	\$6	\$23	\$25	\$5	\$2
4 to 6 months, all beneficiaries	\$74	\$21	\$7	\$5	\$16	\$16	\$8	\$2
7 months to 1 year, IAH beneficiaries	\$103	\$36	\$9	\$6	\$21	\$23	\$5	\$3
7 months to 1 year, all beneficiaries	\$68	\$18	\$6	\$4	\$15	\$15	\$9	\$1
More than 1 year, IAH beneficiaries	\$90	\$31	\$7	\$5	\$19	\$21	\$5	\$3
More than 1 year, all beneficiaries	\$58	\$14	\$5	\$3	\$13	\$14	\$8	\$1

Source: Mathematica's analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

DME = durable medical equipment; SNF = skilled nursing facility.

Exhibit E.33. Average per-day Medicare expenditures during the 90 days after a home visit episode by types of service and episode lengths, among episodes where the beneficiary survived 90 days after the episode, IAH beneficiaries

Episode length	Total	Inpatient	Outpatient	SNF	Clinician/ supplier	Home health	Hospice	DME
1 day, IAH beneficiaries	\$139	\$59	\$12	\$19	\$22	\$20	\$4	\$2
1 day, all beneficiaries	\$78	\$27	\$8	\$10	\$14	\$12	\$6	\$2
2 to 7 days, IAH beneficiaries	\$185	\$79	\$16	\$30	\$32	\$19	\$4	\$6
2 to 7 days, all beneficiaries	\$115	\$42	\$10	\$20	\$20	\$15	\$7	\$2
2 to 5 weeks, IAH beneficiaries	\$165	\$71	\$12	\$28	\$26	\$23	\$3	\$2
2 to 5 weeks, all beneficiaries	\$101	\$36	\$9	\$17	\$17	\$14	\$7	\$2
6 weeks to 3 months, IAH beneficiaries	\$154	\$67	\$10	\$29	\$22	\$18	\$5	\$2
6 weeks to 3 months, all beneficiaries	\$88	\$30	\$7	\$16	\$14	\$12	\$7	\$1
4 to 6 months, IAH beneficiaries	\$168	\$74	\$11	\$33	\$23	\$18	\$8	\$2
4 to 6 months, all beneficiaries	\$95	\$33	\$7	\$20	\$14	\$11	\$9	\$1
7 months to 1 year, IAH beneficiaries	\$169	\$72	\$9	\$38	\$23	\$16	\$8	\$2
7 months to 1 year, all beneficiaries	\$109	\$38	\$6	\$25	\$15	\$11	\$11	\$1
More than 1 year, IAH beneficiaries	\$149	\$62	\$8	\$30	\$21	\$16	\$9	\$2
More than 1 year, all beneficiaries	\$103	\$35	\$6	\$22	\$15	\$11	\$13	\$1

Source: Mathematica’s analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

DME = durable medical equipment; SNF = skilled nursing facility.

VI. Comparing episodes that occurred in assisted living facilities and episodes that occurred in private residences

A. How many home visits occurred per episode, how long did episodes last, and how did episodes end?

As we discussed in Section IV.H of this appendix when we examined episodes for the entire population of beneficiaries, episodes that took place in an assisted living facility tended to have more visits and last longer than episodes in private residences. The average number of visits per episode was 7.9 for assisted living facility episodes compared with 4.7 for private residence episodes. About 39 percent of the episodes in an assisted living facility lasted at least four months compared with 27 percent in private residences (Exhibit E.34).

One of the contributing factors to the longer episodes in assisted living facilities was that a smaller proportion of these episodes ended with the beneficiary visiting an office-based primary care clinician. Although 26 percent of the episodes in assisted living facilities ended with an office-based primary care visit, the same was true for 38 percent of the episodes in private residences (Exhibit E.35). Furthermore, a larger share of those episodes in assisted living facilities that ended with an office-based primary care visit were followed by a new home visit episode within 30 days of the previous home visit episode (25 percent for assisted living facility episodes compared with 20 percent for private residence episodes) (Exhibit E.36). This difference suggests that beneficiaries who had home visit episodes in assisted living facilities were more likely to have had a continuing need and desire for home visits than beneficiaries who had home visit episodes in private residences.

Exhibit E.34. Percentage of home visit episodes by length and home visit count, home visit episodes in assisted living facilities and private residences

Episode length	1 visit		2 to 3 visits		4 to 5 visits		6 to 10 visits		11 to 30 visits		More than 30 visits		All episodes	
	ALF	Private	ALF	Private	ALF	Private	ALF	Private	ALF	Private	ALF	Private	ALF	Private
All episodes	33.1	47.7	22.4	23.0	10.0	8.3	12.0	8.8	16.9	10.3	5.6	1.9	-	-
1 day	33.1	47.7	0.1	0.1	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	33.2	47.8
2 to 7 days	0.0	0.0	1.0	1.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	1.0	1.0
2 to 5 weeks	0.0	0.0	9.3	8.9	0.8	0.5	0.1	0.1	0.0	0.0	0.0	0.0	10.2	9.6
6 weeks to 3 months	0.0	0.0	10.7	11.5	4.1	2.5	1.6	0.7	0.1	0.1	0.0	0.0	16.5	14.8
4 to 6 months	0.0	0.0	1.3	1.4	4.9	4.9	5.4	2.9	1.2	0.3	0.0	0.0	12.8	9.6
7 months to 1 year	0.0	0.0	0.0	0.0	0.3	0.4	4.7	4.8	5.9	2.4	0.2	0.1	11.1	7.6
More than 1 year	0.0	0.0	0.0	0.0	0.0	0.0	0.2	0.3	9.6	7.5	5.4	1.8	15.1	9.6

Source: Mathematica’s analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

ALF = assisted living facility.

Exhibit E.35. Percentage of beneficiaries by number of episodes ended because of office-based primary care visit, home visit episodes in assisted living facilities and private residences

Beneficiaries with:ended episodes	Percentage of beneficiaries (ALF)	Percentage of beneficiaries (private)
Any episode ended by an office-based primary care visit	26.4 ^a	37.4 ^b
1 episode ended	65.5	66.2
2 to 3 episodes ended	26.5	24.6
4 or more episodes ended	7.9	9.1

Source: Mathematica’s analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

^a Beneficiaries with any number of episodes interrupted by an office-based primary care visit were 26.4 percent (102,948 beneficiaries) of the sample of 389,581 beneficiaries having episodes in assisted living facilities.

^b Beneficiaries with any number of episodes interrupted by an office-based primary care visit were 37.4 percent (134,180) of the sample of 358,544 beneficiaries having only episodes in private residences.

ALF = assisted living facility.

Exhibit E.36. Percentage of home visit episodes by days from the end of an episode until the beginning of the next episode, home visit episodes in assisted living facilities and private residences

Days until next episode began	Percentage of episodes (ALF)	Percentage of episodes (private)
Any episode followed by another home visit episode during the study period	60.2	54.1
Next episode began 7 or fewer days after previous episode ended	3.1	2.1
Next episode began 8 to 30 days after previous episode ended	21.7	17.9
Next episode began 31 to 63 days after previous episode ended	21.4	21.6
Next episode began 64 days or more after previous episode ended	14.0	12.5

Source: Mathematica’s analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

ALF = assisted living facility.

B. How many episodes did each beneficiary have, and what were the characteristics of these beneficiaries?

Beneficiaries who had at least one home visit episode in an assisted living facility were slightly more likely to have more than one such episode (46 percent) than were beneficiaries who had at least one home visit episode in a private residence (42 percent) (Exhibit E.37).

Beneficiaries who had a home visit episode in assisted living facilities had many differences in demographic characteristics from those who had an episode in a private residence. The beneficiaries in assisted living facilities were older than those with episodes in private residences (54 percent were older than age 85 versus 37 percent) and more likely to be White (92 percent versus 74 percent). Also, beneficiaries in assisted living facilities were less likely to have qualified for Medicare because of a disability (19 percent versus 28 percent) and less likely to be dually eligible for Medicare and Medicaid (25 percent versus 37 percent) (Exhibit E.38).

Beneficiaries who had episodes in assisted living facilities had slightly lower HCC scores than those who had episodes in private residences (1.4 versus 1.7), but they had very different health profiles. Beneficiaries in assisted living facilities were nearly twice as likely to have cognitive or mental health conditions such as dementia (58 percent versus 30 percent) and major depressive, bipolar, and paranoid disorders (19 percent versus 13 percent). On the other hand, those living in private residences were more likely to have congestive heart failure, chronic obstructive pulmonary disease, diabetes, and polyneuropathy. These differences in chronic conditions suggest that the types of primary care and other health services needed by the two groups may be very different.

Exhibit E.37. Percentage of beneficiaries by number of episodes during the study period, beneficiaries with episodes in assisted living facilities and private residences

Number of episodes	Percentage of beneficiaries (ALF)	Percentage of beneficiaries (private)
1 episode	53.7	57.7
2 episodes	26.9	23.9
3 episodes	13.1	11.3
4 or more episodes	6.2	7.1

Source: Mathematica’s analysis of Medicare claims, Medicare enrollment and Timeline file data for 2014–2018 from the Chronic Conditions Warehouse.

ALF = assisted living facility.

Exhibit E.38. Demographic characteristics of beneficiaries who received home visits, episodes in assisted living facilities and private residences

Characteristic	Episodes in ALFs		Episodes in private residences	
	Percentage of episodes	Percentage of beneficiaries ^a	Percentage of episodes	Percentage of beneficiaries ^a
Sex				
Women	67.9	67.9	65.1	64.3
Age				
Younger than 65	12.0	10.1	16.8	15.1
65 to 74	11.1	10.8	19.8	19.8
75 to 84	24.6	25.4	27.4	28.0
85 and older	52.3	53.6	35.9	37.1
Medicare entitlement reason				
Aged	78.7	81.1	68.8	71.1
Disability only	21.1	18.7	30.0	27.8
ESRD only	0.1	0.1	0.4	0.4
ESRD and disability	0.1	0.1	0.7	0.7
Dually eligible				
Yes	27.2	25.2	40.6	37.3
Race				
Unknown	0.2	0.2	0.5	0.5
White	91.4	91.6	71.1	73.5
Black	5.4	5.1	20.9	18.9
Other	0.7	0.7	1.5	1.4
Asian	0.8	0.8	2.4	2.2
Hispanic	1.3	1.3	3.3	3.2
North American Native	0.2	0.2	0.3	0.3
HCC score and most common conditions^b				
HCC score, average	2.4	1.4	2.9	1.7
HCC 108, vascular disease	40.6	39.9	34.8	33.5
HCC 85, congestive heart failure	25.8	26.2	35.9	35.5
HCC 52, dementia without complications	36.6	37.4	21.9	22.2
HCC 96, specified heart arrhythmias	26.7	27.4	27.7	28.6
HCC 111, chronic obstructive pulmonary disease	21.0	20.6	31.1	30.1
HCC 18, diabetes without chronic complications	16.3	16.0	27.4	25.9
HCC 75, polyneuropathy	15.3	15.0	25.0	23.2
HCC 58, major depressive, bipolar, and paranoid disorders	19.3	18.5	13.6	12.7
HCC 135, acute renal failure	12.2	13.0	15.6	16.5

Exhibit E.34 (continued)

Characteristic	Episodes in ALFs		Episodes in private residences	
	Percentage of episodes	Percentage of beneficiaries ^a	Percentage of episodes	Percentage of beneficiaries ^a
HCC 51, dementia with complications	19.9	20.6	7.6	7.8

Source: Mathematica’s analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

^a We measured characteristics for each episode during 2014 to 2018. The same beneficiary may have had different characteristics appearing in multiple episodes over time. Time-varying beneficiary characteristics in this table were estimated as the proportion of characteristics among episodes weighted by the inverse of the beneficiary’s total episodes.

^b We measured HCC health conditions for each episode during 2014-2018. The same beneficiary may have had different conditions appearing in multiple episodes over time. Percentages of beneficiaries in this table were estimated as the proportions of conditions among episodes weighted by the inverse of the beneficiary’s total episodes. For more information about how we measured the HCC score, see Appendix A.

ALF = assisted living facility; ESRD = end-stage renal disease; HCC = Hierarchical Condition Categories.

C. What was the average rate of home visits per episode, and what types of clinicians provided home visits?

The frequency of home visits for episodes in assisted living facilities and those in private residences was essentially the same. Although episodes in facilities lasted longer, they averaged about the same number of weekly visits as those in private residences (Exhibit E.39).

For both groups, primary care physicians and NPs provided the vast majority of home visits for both groups (Exhibit E.40). Reflecting the differences in health status; however, psychiatrists provided a higher proportion of visits (6 percent for assisted living facility episodes versus less than 1 percent in private residences), and other specialists provided more visits in private residences (5 versus 3 percent). These patterns correspond to higher prevalence of dementia and mental illness among beneficiaries with facility episodes and higher prevalence of conditions such as congestive heart failure and chronic obstructive pulmonary disease among beneficiaries with private residence episodes. Nearly all visits provided at home by clinicians from Federally Qualified Health Centers and Rural Health Clinics took place in private residences.

Exhibit E.39. Average (and median) number of home visits in episode lengths greater than one day, home visit episodes in ALF versus private residences

Episode length	Percentage of episodes		Number of visits overall		Number of visits per week	
	ALF	Private	ALF	Private	ALF	Private
2 to 7 days	1.0	1.0	2.1 (2.0)	2.1 (2.0)	2.1 (2.0)	2.1 (2.0)
2 to 5 weeks	10.2	9.6	2.4 (2.0)	2.4 (2.0)	0.7 (0.7)	0.7 (0.6)
6 weeks to 3 months	16.5	14.8	3.5 (3.0)	3.1 (3.0)	0.4 (0.3)	0.4 (0.3)
4 to 6 months	12.8	9.6	6.5 (6.0)	5.4 (5.0)	0.3 (0.3)	0.3 (0.3)
7 months to 1 year	11.1	7.6	12.6 (11.0)	9.9 (9.0)	0.3 (0.3)	0.3 (0.2)
More than 1 year	15.1	9.6	29.5 (25.0)	23.1 (19.0)	0.3 (0.3)	0.3 (0.2)

Source: Mathematica’s analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

ALF = assisted living facility.

Exhibit E.40. Percentage of visits that occurred during episodes by type of clinician and episode length, home visit episodes in ALF versus private residences

Episode length	Percentage of episodes		Primary care physician		Nurse practitioner		Physician assistant		Specialist physician		Psychiatrist		FQHC & RHC ^a	
	ALF	Private	ALF	Private	ALF	Private	ALF	Private	ALF	Private	ALF	Private	ALF	Private
Any length (all episodes)	100.0	100.0	48.0	46.6	36.9	36.5a	6.3	6.0	2.6	4.8	6.1	0.8	0.1	5.1
1 day	33.2	47.8	49.8	48.4	33.6	34.5	4.7	5.6	3.4	5.2	8.4	1.1	0.2	5.1
2 to 7 days	1.0	1.0	42.7	42.5	41.9	35.8	6.4	4.7	3.0	6.0	5.8	0.4	0.1	10.6
2 to 5 weeks	10.2	9.6	45.8	40.9	39.8	41.2	6.3	6.2	2.4	5.1	5.7	0.9	0.1	5.7
6 weeks to 3 months	16.5	14.8	48.8	46.5	37.4	36.8	6.2	6.2	2.3	4.8	5.1	0.8	0.1	4.8
4 to 6 months	12.8	9.6	48.5	46.1	38.2	37.8	6.7	6.3	2.1	4.2	4.3	0.7	0.2	5.0
7 months to 1 year	11.1	7.6	47.1	45.2	39.0	38.9	7.5	6.6	2.0	3.9	4.2	0.4	0.1	4.9
More than 1 year	15.1	9.6	45.2	45.6	38.4	38.3	8.4	7.1	2.4	3.9	5.4	0.3	0.1	4.8

Source: Mathematica’s analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

^a Measured using outpatient claims with a revenue center code indicating a home visit by a clinician from a Rural Health Clinic or Federally Qualified Health Center.

ALF = assisted living facility; FQHC = Federally Qualified Health Center; RHC = Rural Health Clinic.

D. What types of service use other than home visits occurred during home visit episodes?

There were no substantive differences in use of office-based specialist visits during the home visit episode according to whether the episode was in an assisted living facility or private residence (Exhibit E.41).

Episodes that occurred in assisted living facilities and private residences were about equally likely to include the use of home health agency services, but the types of home health services differed by setting of the home visit episode. Compared with episodes in private residences, episodes in assisted living facilities were more likely to include therapy services and less likely to include aide services (Exhibit E.42). The difference in aide use was most stark among long episodes: 4 percent of episodes that lasted more than one year in an assisted living facility included home health aide use compared with 17 percent of episodes that lasted more than one year in a private residence. This difference is likely because of the fact that residing in an assisted living facility often provides a beneficiary with access to assistance with activities of daily living similar to what a home health aide can provide, such as bathing. It may also suggest that the ability for a beneficiary to remain in a private residence is to some degree helped by receiving home health aide services (and possibly also home visits).

Because of the longer episode lengths in assisted living facilities, we may expect to see that beneficiaries use more services during these episodes than during home visit episodes. When we examined the use of other services accounting for length of the episodes, we found two substantial differences. First, episodes that occurred in assisted living facilities were much more likely to include an ED visit, regardless of episode length, yet rates of hospital admissions were fairly similar for the two groups. Among episodes in assisted living facilities, 22 percent included an ED visit compared with 12 percent of episodes in private residences; the difference in hospital use was much smaller (14 percent for episodes in facilities versus 11 percent for those in private residences) (Exhibit E.43). Second, episodes in assisted living facilities were more likely to include the use of hospice services than those in private residences. Although the rates of hospice use among shorter episodes were similar for the two settings, episodes lasting at least six weeks in assisted living facilities were about twice as likely to include hospice services than those lasting the same duration in private residences. Private residence episodes, however, were more likely to include durable medical equipment use.

On average across episodes of any length, the two groups did not differ much in average percentage of days spent in hospital, SNF, or nursing home settings (Exhibit E.44). When we examine data by episode length, however, two differences appear. First, episodes in assisted living facilities that lasted two to seven days were less likely to include time in a SNF than episodes in private residences (2 percent versus 7 percent). As we noted, this difference could reflect beneficiaries who were discharged home from the hospital before entering a SNF. Second, among the longest episodes, episodes in assisted living facilities were less likely to include time in a nursing home than episodes in private residences (2 percent versus 4 percent).

Exhibit E.41. Average (and median) number of office-based specialist visits per episode by episode length, home visit episodes in assisted living facilities and private residences

Episode length	Overall		Conditional on having at least one office-based specialist visit	
	ALF	Private	ALF	Private
Any length (all episodes)	0.7 (0.0)	0.6 (0.0)	3.2 (2.0)	3.3 (2.0)
1 day	0.0 (0.0)	0.0 (0.0)	1.0 (1.0)	1.0 (1.0)
2 to 7 days	0.0 (0.0)	0.1 (0.0)	1.1 (1.0)	1.1 (1.0)
2 to 5 weeks	0.2 (0.0)	0.3 (0.0)	1.2 (1.0)	1.4 (1.0)
6 weeks to 3 months	0.4 (0.0)	0.5 (0.0)	1.6 (1.0)	1.8 (1.0)
4 to 6 months	0.8 (0.0)	0.9 (0.0)	2.2 (2.0)	2.6 (2.0)
7 months to 1 year	1.4 (0.0)	1.6 (0.0)	3.2 (2.0)	3.8 (2.0)
More than 1 year	2.4 (0.0)	2.8 (0.0)	4.9 (3.0)	5.9 (3.0)

Source: Mathematica’s analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

ALF = assisted living facility.

Exhibit E.42. Percentage of episodes having home health visits on a claim during the episode, home visit episodes in assisted living facilities and private residences

Episode length	Any home health visit		Home health skilled nursing		Home health therapy ^a		Home health aide	
	ALF	Private	ALF	Private	ALF	Private	ALF	Private
Any length (all episodes)	25.3	24.2	20.6	22.8	21.0	16.3	1.4	5.2
1 day	0.6	1.2	0.5	1.2	0.5	0.9	0.0	0.2
2 to 7 days	5.9	7.2	5.0	6.9	4.7	5.3	0.3	1.2
2 to 5 weeks	12.3	18.6	9.6	17.2	9.6	11.6	0.6	3.5
6 weeks to 3 months	23.3	36.4	18.4	34.0	17.4	20.3	1.2	6.9
4 to 6 months	36.8	51.4	29.1	48.0	29.5	32.3	1.8	10.8
7 months to 1 year	50.5	61.7	41.0	57.9	42.9	43.7	2.7	13.9
More than 1 year	63.5	70.5	53.2	67.0	55.7	55.0	3.7	17.3

Source: Mathematica’s analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

^a Includes physical, occupational, and speech therapy.

ALF = assisted living facility.

Exhibit E.43. Percentage of episodes by episode length with the following service uses during the episode, home visit episodes in assisted living facilities and private residences

Episode length	Hospital use		ED visits		SNF		Any DME ^a		DME hospital bed ^a		DME wheelchair ^a		Hospice	
	ALF	Private	ALF	Private	ALF	Private	ALF	Private	ALF	Private	ALF	Private	ALF	Private
Any length (all episodes)	13.8	10.6	21.6	12.2	5.1	3.3	31.7	46.5	5.0	8.2	10.0	9.4	8.2	3.0
1 day	0.3	0.4	0.4	0.5	1.4	1.0	22.0	39.0	2.6	5.1	5.7	6.2	0.4	0.4
2 to 7 days	1.9	2.5	6.0	4.4	2.5	7.2	28.3	44.6	3.8	5.8	8.7	7.1	2.8	2.3
2 to 5 weeks	5.5	7.5	10.4	8.7	2.0	3.1	28.9	47.2	4.1	7.4	8.4	8.5	5.3	3.2
6 weeks to 3 months	10.5	12.0	17.4	13.5	3.3	3.2	29.8	46.9	4.5	8.5	9.2	9.3	7.9	4.4
4 to 6 months	18.3	19.4	29.8	22.5	5.9	4.8	34.8	51.8	5.8	10.6	11.3	11.7	11.2	5.7
7 months to 1 year	28.0	28.4	43.7	33.6	9.7	7.2	41.1	58.4	7.3	13.5	14.1	14.5	15.9	7.5
More than 1 year	39.2	40.0	57.6	46.0	13.5	10.6	47.9	68.2	9.3	17.9	17.5	19.8	19.6	8.0

Source: Mathematica’s analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

^a These measures reflect DME use during the episode or in the previous 90 days because DME is often used for weeks or months beyond the from date for the DME claim. For example, if a beneficiary had a DME claim for a wheelchair 45 days before the episode, then we identified the episode as having wheelchair use.

ALF = assisted living facility; DME = durable medical equipment; ED = emergency department; SNF = skilled nursing facility.

Exhibit E.44. Average percentage of days during the episode spent in different settings by episode length, home visit episodes in assisted living facilities and private residences

Episode length	Hospital ^a		SNF		Nursing home		Community	
	ALF	Private	ALF	Private	ALF	Private	ALF	Private
Any length (all episodes)	0.9	1.0	1.3	1.3	2.0	1.8	95.8	95.8
1 day ^b	0.5	0.7	1.4	1.0	2.7	1.2	95.4	97.1
2 to 7 days	1.1	1.3	2.2	6.5	1.6	1.2	95.0	90.7
2 to 5 weeks	1.3	1.9	1.4	2.4	1.5	1.6	95.8	94.1
6 weeks to 3 months	1.2	1.6	1.4	1.5	1.7	1.7	95.7	95.1
4 to 6 months	1.1	1.4	1.2	1.3	1.8	2.5	95.9	94.9
7 months to 1 year	1.0	1.2	1.2	1.1	1.8	3.0	96.0	94.8
More than 1 year	0.7	0.9	0.8	1.0	1.7	3.5	96.7	94.7

Source: Mathematica’s analysis of Medicare claims, Medicare enrollment and Timeline file data for 2014–2018 from the Chronic Conditions Warehouse.

^a Reflects days covered by an inpatient claim, which includes days in facilities such as general acute care hospitals, psychiatric and other specialty hospitals, long-term care hospitals, and inpatient rehabilitation facilities.

^b A one-day home visit episode could have occurred on the same day that a beneficiary was admitted to a hospital, a SNF, or a nursing home.

ALF = assisted living facility; SNF = skilled nursing facility.

E. To what extent did clinicians providing home visits offer other services?

Home visit clinicians were less likely to provide all four of the following services during episodes in assisted living facilities than episodes in private residences: CCM, TCM, home health or hospice oversight, and advance care planning (Exhibit E.45). We did not expect less home health and hospice oversight among assisted living facilities because these episodes tended to last longer than episodes in private residences, were equally likely to include home health use, and were more likely to include hospice use. It is interesting to note the lower provision of advance care planning during episodes in assisted living facilities than in private residences (6 versus 11 percent), despite the fact that beneficiaries receiving episodes in such a facility tended to be older, more likely to have dementia, and more likely to use hospice during the episode.

We noted earlier that many of the episodes in which the home visit clinician provided TCM or advance care planning were one-visit episodes. When examining the data by setting of episode, we see that these results were driven by episodes in private residences. For example, of the private residence episodes with TCM given by a home visit clinician, 48 percent lasted only one day (Exhibit E.46). Of course, TCM services were rare overall—they were provided by the home visit clinician during just 3.7 percent of all episodes in private residences. Still, when they occurred, they were often for a one-day episode. We did not see this pattern for assisted living facility episodes. Of the assisted living facility episodes with TCM given by a home visit provider, 6 percent lasted only one day.

Exhibit E.45. Percentage of episodes by episode length in which a home visit clinician provided a particular service during the episode, home visit episodes in assisted living facilities and private residences

Episode length	Chronic care management		Transitional care management		Home health oversight or hospice oversight		Advance care planning	
	ALF	Private	ALF	Private	ALF	Private	ALF	Private
Any length (all episodes)	5.0	7.1	2.3	3.7	2.9	6.9	2.0	3.2
1 day ^b	0.1	0.2	0.4	3.7	0.0	0.3	0.4	1.4
2 to 7 days	0.5	0.7	1.7	6.1	0.2	1.0	1.3	2.3
2 to 5 weeks	1.6	3.9	1.2	3.7	0.8	5.1	1.3	2.4
6 weeks to 3 months	3.0	7.4	1.5	2.5	1.6	9.7	1.5	2.7
4 to 6 months	5.1	12.9	2.6	3.2	3.2	13.5	2.1	4.1
7 months to 1 year	8.9	20.0	4.1	4.4	5.8	17.2	3.5	6.1
More than 1 year	17.7	29.4	6.1	5.3	9.9	22.8	5.5	10.5

Source: Mathematica’s analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

ALF = assisted living facility.

Exhibit E.46. Distribution of episode length among episodes in which a home visit clinician provided a particular service, home visit episodes in assisted living facilities and private residences

Episode length	Chronic care management		Transitional care management		Home health oversight or hospice oversight		Advance care planning	
	ALF	Private	ALF	Private	ALF	Private	ALF	Private
Total episodes in which a home visit clinician provided the service (percentage)	33,374 (100.0)	43,129 (100.0)	14,966 (100.0)	22,208 (100.0)	19,392 (100.0)	41,602 (100.0)	13,431 (100.0)	19,056 (100.0)
1 day	0.7	1.0	6.1	47.7	0.3	2.2	7.0	20.6
2 to 7 days	0.1	0.1	0.7	1.7	0.1	0.2	0.7	0.7
2 to 5 weeks	3.3	5.3	5.3	9.6	2.7	7.2	6.5	7.3
6 weeks to 3 months	9.7	15.3	11.3	9.9	9.3	20.8	11.9	12.5
4 to 6 months	13.0	17.3	14.8	8.3	14.0	18.9	13.6	12.4
7 months to 1 year	19.7	21.4	20.4	9.1	22.1	19.1	19.1	14.7
More than 1 year	53.5	39.5	41.3	13.7	51.5	31.7	41.2	31.9

Source: Mathematica’s analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

ALF = assisted living facility.

F. Which types of health care did beneficiaries receive in the 90 days before the home visit episode and 90 days after the home visit episode?

Episodes that occurred in assisted living facilities were less likely to have been preceded by a hospital admission (16 percent in assisted living versus 23 percent in private residences) and home health care (29 percent in assisted living versus 47 percent in private residences) (Exhibit E.47). But they were more likely to have been preceded by receipt of hospice services.

In the 90 days after the home visit episode ended, we found similar patterns to those that preceded the episode. Episodes in assisted living facilities were less likely to be followed by a hospital admission or home health care and more likely to be followed by hospice (Exhibit E.48). Even though episodes in assisted living facilities were more likely to be followed by hospice, episodes in assisted living facilities were more likely to be followed by death (14 percent versus 11 percent in private residences).

Differences between home visit episode settings in the average number of days from the end of the episode until a beneficiary entered a hospital, a SNF, or a nursing home or died were small (just two to three days' difference in the averages) (Exhibit E.49).

Exhibit E.47. Percentage of episodes with various types of health care use in the preceding 90 days, home visit episodes in assisted living facilities and private residences

Type of health care use	Percentage of episodes (ALF)	Percentages of episodes (private)
Hospital	15.9	22.9
Home health care	28.7	47.2
SNF	12.3	11.3
Nursing home	5.3	3.5
Hospice	4.8	0.8

Source: Mathematica's analysis of Medicare claims, Medicare enrollment and Timeline file data for 2014–2018 from the Chronic Conditions Warehouse.

ALF = assisted living facility; SNF = skilled nursing facility.

Exhibit E.48. Percentage of episodes followed within 90 days by various types of health care use or death, home visit episodes in assisted living facilities and private residences

Type of health care use or death	Overall percentage of episodes		Episodes in which the beneficiary survived 90 days after episode ended		Episodes in which the beneficiary died within 90 days after episode ended	
	ALF	Private	ALF	Private	ALF	Private
Hospital	16.9	20.1	14.1	17.4	34.8	42.8
Home health	24.7	44.0	25.5	45.0	19.9	36.7
SNF	9.2	8.5	8.9	8.1	11.8	12.1
Nursing home	6.5	4.3	6.0	3.9	9.8	8.0
Hospice	14.4	9.4	8.0	4.5	54.7	49.2
Death	13.6	11.0	n.a.	n.a.	100.0	100.0

Source: Mathematica's analysis of Medicare claims, Medicare enrollment and Timeline file data for 2014–2018 from the Chronic Conditions Warehouse.

ALF = assisted living facility; n.a. = not applicable; SNF = skilled nursing facility.

Exhibit E.49. Average (and median) number of days after a home visit episode until a particular outcome, among only episodes having that outcome on or within 90 days of the day the episode ended, home visit episodes in assisted living facilities and private residences

Type of health care use or death	All episodes		Episodes in which the beneficiary survived 90 days after episode ended		Episodes in which the beneficiary died within 90 days after episode ended	
	ALF	Private	ALF	Private	ALF	Private
Hospital ^a	29.2 (21.0)	31.5 (25.0)	33.9 (27.0)	35.5 (30.0)	16.8 (10.0)	18.5 (12.0)
SNF	29.5 (23.0)	32.1 (28.0)	31.7 (26.0)	34.0 (30.0)	19.4 (15.0)	21.9 (18.0)
Nursing home	25.5 (15.0)	24.6 (11.0)	26.8 (15.0)	25.9 (12.0)	20.4 (13.0)	19.6 (11.0)
Death	n.a.	n.a.	n.a.	n.a.	29.0 (21.0)	32.6 (26.0)

Source: Mathematica’s analysis of Medicare claims, Medicare enrollment and Timeline file data for 2014–2018 from the Chronic Conditions Warehouse.

^a Reflects days covered by an inpatient claim, which includes days in facilities such as general acute care hospitals, psychiatric and other specialty hospitals, long-term care hospitals, and inpatient rehabilitation facilities.

ALF = assisted living facility; n.a. = not applicable; SNF = skilled nursing facility.

G. What was the average daily Medicare expenditures before, during, and after the home visit episode, and did the average vary by length of the episode?

Total Medicare expenditures were lower for episodes in assisted living facilities than for those in private residences before, during and after home visit episodes. Average daily expenditures were 26 percent lower in the year before the episode (\$63 per day versus \$85 per day), 28 percent lower in the prior 90 days (\$78 per day versus \$108), and 36 percent lower during the episode (\$102 versus \$159) (Exhibit E.50). Among episodes where the beneficiary survived the following 90 days, average daily expenditures were 22 percent lower (\$79 versus \$101).

Expenditures on almost all services were lower before, during and after episodes in assisted living facilities, with the exceptions of SNF and hospice. The primary factor that contributed to episodes in assisted living facilities having lower expenditures than episodes in private residences was lower inpatient expenditures. These results likely reflect the differences in health conditions prevalent in the two groups—for example, beneficiaries with chronic conditions such as congestive heart failure, chronic obstructive pulmonary disease, and diabetes (more common among private residence episodes) may have had a higher risk of hospital admission than those with dementia or certain mental illnesses (more common among assisted living facility episodes).

Examining expenditures before, during and after the episode by length of the home visit episode, we found that the difference in average daily total expenditures by setting was larger for shorter episodes than for longer episodes (Exhibits E.51 through E.54). In other words, the longer the episode, the more similar average daily total expenditures were for episodes in assisted living facilities and episodes in private residences. For example, among the episodes that lasted two to five weeks, average daily total expenditures were 28 percent lower for episodes in assisted living facilities than those in private residences; by contrast, for the episodes that lasted more than one year, average daily total expenditures were 8 percent lower for episodes in assisted living facilities.

Exhibit E.50. Average per-day Medicare expenditures before, during, and after an episode by types of service, home visit episodes in assisted living facilities and private residences

Time period	Total		Inpatient		Outpatient		SNF		Clinician/supplier		Home health		Hospice		DME	
	ALF	Private	ALF	Private	ALF	Private	ALF	Private	ALF	Private	ALF	Private	ALF	Private	ALF	Private
365 days before episode	\$63	\$85	\$20	\$33	\$6	\$8	\$13	\$11	\$12	\$16	\$8	\$12	\$4	\$3	\$1	\$2
90 days before episode	\$78	\$108	\$25	\$45	\$6	\$9	\$19	\$17	\$13	\$18	\$8	\$13	\$6	\$3	\$1	\$2
During episode	\$102	\$159	\$23	\$39	\$7	\$11	\$5	\$9	\$44	\$70	\$13	\$20	\$9	\$5	\$1	\$5
90 days after episode ^a	\$79	\$101	\$24	\$38	\$6	\$9	\$17	\$15	\$13	\$17	\$9	\$15	\$10	\$5	\$1	\$2

Source: Mathematica’s analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

^a Average expenditures for the 90 days after the episode are based on beneficiaries who survived the full 90-day period.

ALF = assisted living facility; DME = durable medical equipment; SNF = skilled nursing facility.

Exhibit E.51. Average per-day Medicare expenditures during the 365 days before an episode by types of service and episode lengths, home visit episodes in assisted living facilities and private residences

Episode length	Total		Inpatient		Outpatient		SNF		Clinician/supplier		Home health		Hospice		DME	
	ALF	Private	ALF	Private	ALF	Private	ALF	Private	ALF	Private	ALF	Private	ALF	Private	ALF	Private
1 day	\$56	\$82	\$18	\$32	\$6	\$9	\$10	\$10	\$11	\$16	\$6	\$11	\$5	\$3	\$1	\$2
2 to 7 days	\$76	\$107	\$27	\$46	\$7	\$11	\$15	\$15	\$15	\$21	\$7	\$10	\$4	\$2	\$1	\$2
2 to 5 weeks	\$70	\$102	\$24	\$43	\$6	\$10	\$14	\$14	\$14	\$19	\$8	\$12	\$4	\$2	\$1	\$2
6 weeks to 3 months	\$64	\$86	\$21	\$33	\$6	\$8	\$13	\$12	\$12	\$16	\$8	\$13	\$4	\$3	\$1	\$2
4 to 6 months	\$66	\$85	\$21	\$32	\$6	\$8	\$14	\$12	\$12	\$15	\$8	\$13	\$4	\$3	\$1	\$2
7 months to 1 year	\$68	\$85	\$22	\$33	\$5	\$7	\$15	\$13	\$13	\$14	\$9	\$13	\$4	\$3	\$1	\$2
More than 1 year	\$64	\$76	\$20	\$28	\$5	\$5	\$14	\$11	\$12	\$14	\$10	\$14	\$4	\$2	\$1	\$2

Source: Mathematica's analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

ALF = assisted living facility; DME = durable medical equipment; SNF = skilled nursing facility.

Exhibit E.52. Average per-day Medicare expenditures during the 90 days before an episode by types of service and episode lengths, home visit episodes in assisted living facilities and private residences

Episode length	Total		Inpatient		Outpatient		SNF		Clinician/supplier		Home health		Hospice		DME	
	ALF	Private	ALF	Private	ALF	Private	ALF	Private	ALF	Private	ALF	Private	ALF	Private	ALF	Private
1 day	\$65	\$107	\$21	\$46	\$6	\$10	\$13	\$15	\$11	\$19	\$7	\$12	\$7	\$3	\$1	\$2
2 to 7 days	\$108	\$167	\$41	\$81	\$8	\$13	\$25	\$28	\$19	\$29	\$9	\$12	\$5	\$3	\$1	\$2
2 to 5 weeks	\$93	\$142	\$33	\$65	\$7	\$11	\$22	\$23	\$16	\$24	\$9	\$14	\$5	\$3	\$1	\$2
6 weeks to 3 months	\$79	\$105	\$26	\$42	\$6	\$9	\$19	\$17	\$13	\$18	\$8	\$14	\$6	\$3	\$1	\$2
4 to 6 months	\$83	\$103	\$27	\$40	\$6	\$8	\$22	\$18	\$13	\$16	\$9	\$14	\$6	\$4	\$1	\$2
7 months to 1 year	\$88	\$104	\$29	\$41	\$6	\$7	\$25	\$20	\$14	\$16	\$9	\$14	\$5	\$4	\$1	\$2
More than 1 year	\$81	\$89	\$26	\$33	\$5	\$6	\$22	\$16	\$14	\$15	\$10	\$14	\$5	\$3	\$1	\$2

Source: Mathematica's analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

ALF = assisted living facility; DME = durable medical equipment; SNF = skilled nursing facility.

Exhibit E.53. Average per-day Medicare expenditures during an episode by types of service and episode lengths, home visit episodes in assisted living facilities and private residences

Episode length	Total		Inpatient		Outpatient		SNF		Clinician/supplier		Home health		Hospice		DME	
	ALF	Private	ALF	Private	ALF	Private	ALF	Private	ALF	Private	ALF	Private	ALF	Private	ALF	Private
1 day	\$161	\$232	\$31	\$50	\$8	\$14	\$6	\$12	\$97	\$124	\$10	\$20	\$9	\$5	\$1	\$6
2 to 7 days	\$185	\$250	\$59	\$84	\$16	\$23	\$8	\$23	\$71	\$81	\$18	\$27	\$11	\$7	\$1	\$6
2 to 5 weeks	\$95	\$132	\$27	\$42	\$10	\$13	\$4	\$9	\$27	\$31	\$18	\$27	\$8	\$5	\$1	\$5
6 weeks to 3 months	\$74	\$92	\$21	\$30	\$7	\$9	\$5	\$6	\$17	\$19	\$15	\$21	\$8	\$5	\$1	\$2
4 to 6 months	\$69	\$80	\$18	\$25	\$7	\$8	\$5	\$4	\$15	\$16	\$14	\$19	\$10	\$5	\$1	\$3
7 months to 1 year	\$66	\$73	\$16	\$22	\$6	\$7	\$5	\$3	\$14	\$15	\$14	\$18	\$11	\$5	\$1	\$3
More than 1 year	\$56	\$61	\$12	\$17	\$4	\$5	\$3	\$3	\$13	\$14	\$13	\$16	\$11	\$4	\$1	\$3

Source: Mathematica’s analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

ALF = assisted living facility; DME = durable medical equipment; SNF = skilled nursing facility.

Exhibit E.54. Average per-day Medicare expenditures during the 90 days after a home visit episode by types of service and episode lengths, among episodes where the beneficiary survived 90 days after the episode, home visit episodes in assisted living facilities and private residences

Episode length	Total		Inpatient		Outpatient		SNF		Clinician/supplier		Home health		Hospice		DME	
	ALF	Private	ALF	Private	ALF	Private	ALF	Private	ALF	Private	ALF	Private	ALF	Private	ALF	Private
1 day	\$61	\$92	\$19	\$33	\$6	\$9	\$10	\$11	\$11	\$16	\$8	\$16	\$7	\$5	\$1	\$2
2 to 7 days	\$96	\$135	\$31	\$53	\$8	\$12	\$20	\$20	\$17	\$23	\$12	\$18	\$8	\$6	\$1	\$3
2 to 5 weeks	\$85	\$119	\$27	\$47	\$7	\$10	\$17	\$17	\$15	\$21	\$11	\$17	\$8	\$5	\$1	\$2
6 weeks to 3 months	\$78	\$100	\$24	\$38	\$6	\$9	\$16	\$15	\$13	\$16	\$9	\$15	\$9	\$6	\$1	\$2
4 to 6 months	\$86	\$107	\$26	\$42	\$6	\$8	\$20	\$19	\$13	\$16	\$9	\$13	\$11	\$6	\$1	\$2
7 months to 1 year	\$101	\$120	\$31	\$49	\$6	\$8	\$26	\$25	\$14	\$17	\$9	\$13	\$14	\$7	\$1	\$2
More than 1 year	\$99	\$110	\$30	\$45	\$5	\$6	\$22	\$21	\$14	\$16	\$10	\$13	\$17	\$6	\$1	\$2

Source: Mathematica’s analysis of Medicare claims and enrollment data for 2014–2018 from the Chronic Conditions Warehouse.

ALF = assisted living facility; DME = durable medical equipment; SNF = skilled nursing facility.

VII. Conclusion

This analysis provided a detailed look at home visit episodes by Medicare FFS beneficiaries. The following are key takeaways from this analysis:

- Although the average was 6.3 visits per episode, a majority of episodes had only one visit (40 percent of episodes) or two to three visits (23 percent). However, 22 percent of episodes lasted more than six months. Episodes of six weeks or longer tended to average about one visit every three to four weeks, even for episodes longer than one year. More than half of episodes ended because of a gap in home visits longer than seven weeks (rather than ending because of death, an office-based primary care visit, or the end of the study period). Several characteristics were associated with more home visits in an episode: being in an assisted living facility or other group residence, having dementia, and being in a SNF or nursing facility before the episode.
- Compared with all Medicare beneficiaries in 2016, beneficiaries who had home visits were sicker and were more likely to be women, older, and dually eligible. Beneficiaries who had home visit episodes in assisted living facilities were older, more likely to be White, less likely to have qualified for Medicare because of a disability, and less likely to be dually eligible for Medicare and Medicaid than those who had home visit episodes in private residences. Also, beneficiaries who had episodes in assisted living facilities had very different health profiles than those with episodes in private residences: they were much more likely to have dementia or severe mental health conditions and much less likely to have congestive heart failure, chronic obstructive pulmonary disease, and diabetes.
- About 19 percent of home visit episodes were preceded by a hospital admission in the previous 90 days. Compared with episodes in private residences, episodes in assisted living facilities were less likely to have been preceded by a hospital admission but more likely to have been preceded by receipt of hospice services.
- Home health agency service use, ED visits, and hospital admissions were common among the longest episodes. Episodes that occurred in assisted living facilities were much more likely to include an ED visit than those in private residences regardless of episode length, yet rates of hospital admissions were fairly similar for the two settings. Episodes in assisted living facilities were more likely to include the use of hospice services than those in private residences.
- Home visit clinicians were less likely to provide all four of the following services during episodes in assisted living facilities than during episodes in private residences: CCM, TCM, home health or hospice oversight, and advance care planning. When a home visit clinician provided TCM or advance care planning services, clinicians often provided them during a one-day episode in a private residence.
- Average daily Medicare expenditures in the 365 days before the episode were lower than expenditures in the previous 90 days in nearly all categories, with the largest difference for inpatient and SNF expenditures. Average daily Medicare expenditures were lower for episodes in assisted living facilities than for those in private residences before and during home visit episodes; much of these differences were because of lower inpatient expenditures among beneficiaries in assisted living facilities.

- Compared with episodes for all Medicare FFS beneficiaries, episodes for IAH beneficiaries generally were longer and included more visits per episode. Having longer episodes and more frequent visits is consistent with the goals and eligibility criteria of the IAH demonstration, which aimed to reduce expenditures and improve health outcomes among beneficiaries with multiple chronic conditions, functional limitations, and recent hospital admission. Consistent with IAH beneficiaries' poorer health status, their episodes were preceded and followed by higher expenditures compared to all Medicare FFS beneficiaries' episodes.

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