REPORT

Evaluation of the Independence at Home Demonstration: An Examination of the First Four Years - Appendices

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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) directed home-based primary care teams. These teams aimed "to reduce expenditures and improve health outcomes" of Medicare beneficiaries with multiple chronic conditions and substantial functional limitations (Appendix A). As we discussed in Chapter I, the IAH demonstration introduced both an incentive to reduce Medicare expenditures (incentive payments) and a service delivery model (home-based primary care led by physicians or NPs). As we describe in Chapters II and III, the Mathematica Policy Research study team estimated a difference-in-differences model to determine whether the demonstration affected Medicare expenditures, service utilization, and quality of care (measured as hospital use for ambulatory care–sensitive conditions [ACSCs]). We also examined how the IAH practices provided care and identified changes they made during the demonstration. In addition, we assessed how IAH beneficiaries and their caregivers viewed their care from IAH practices. In this appendix, we present the sample, data, and methods we used for the analyses in Chapters II and III.

The quantitative evaluation design of the demonstration was a difference-in-differences analysis using repeated cross sections of eligible beneficiaries within demonstration practices, with a propensity score–matched comparison group. We had two years of pre-demonstration data and four years of post-demonstration data (the first four years of the demonstration). We used three key pieces of information to determine the effect of the demonstration on expenditures, service utilization, and quality of care in a given year. To determine the effect of the demonstration on expenditures (and other outcomes) in a given year, such as Year 4, 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 4 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 beneficiary characteristics such as time since most recent hospitalization, 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, and we controlled 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 and that affected both IAH and comparison beneficiaries. This before-and-after design was 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. However, the difference-in-differences model was not without limitations; we address limitations of our evaluation at the end of this appendix.

Our total sample consisted of 14 practices, because we treated the consortium in Richmond as one practice (Table I.2). 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 this appendix, we begin by describing how we identified the IAH and comparison groups to evaluate the effect of the demonstration. The demonstration is designed to have IAH sites assess and enroll participants; however, we could not use data from the IAH sites to identify our sample 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. As we describe in the next section, 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 identify beneficiary and practice subgroups. We then present the sources of data and measures for our quantitative analyses. Then, we describe the estimation of demonstration effects using frequentist and Bayesian models. Next, we present the methods and sources of data for our qualitative and survey analyses. We conclude with a discussion of the limitations of our study and supplemental tables for Chapters II and III.

II. 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 the enrollees met health status and other clinical and programmatic requirements, such as providing consent. The implementation contractor used both 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 Section 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 group 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. Also, 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 information that the sites provided to the implementation contractor. We describe our process for defining the IAH group in this section. We describe our process for identifying the comparison group in Section 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 4. After explaining these approaches in Sections II.A and II.B, we provide details about reasons for differences in the counts of IAH practices' eligible beneficiaries in Section II.C.

A. Process the IAH implementation contractor used to determine 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 conducting home visits to explain the demonstration, the IAH sites enrolled willing participants in the demonstration and uploaded a list of potential enrollees to a reporting system created for the demonstration using a process established by the demonstration's implementation contractor. The implementation contractor then used administrative data to verify that each enrolled beneficiary had both a qualifying hospitalization and 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 hospitalization and rehabilitation services, the implementation contractor also assisted IAH sites with identifying potential beneficiaries for enrollment into the demonstration based on the eligibility criteria. The implementation contractor identified beneficiaries who had received at least one home visit by the demonstration practice and had had qualifying hospitalization and

rehabilitation service events but whom the sites had not yet enrolled in the reporting systems; these beneficiaries were called potential enrollees. The implementation 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. 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 had a qualifying hospitalization and post-acute rehabilitation services and a home visit by the IAH practice within the previous 12 months. The home visit by the practice may have occurred before or after the qualifying hospitalization 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 follow-up information on the potential enrollee, the implementation contractor assumed 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. CMS allowed beneficiaries who enrolled in the demonstration in a given year to continue in the demonstration, whether or not they requalified in the subsequent years.

B. Process Mathematica used to identify the sample of eligible and attributed beneficiaries for the evaluation

To identify beneficiaries who were 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 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 group 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 who were eligible for the demonstration in that year according to our analysis of Medicare enrollment, claims, and assessment data.

We applied the following criteria to identifying beneficiaries for the IAH group:

- Enrollment in fee-for-service (FFS) Medicare
- Two or more ADLs that required human assistance
- Two or more chronic conditions

- Inpatient hospitalization or observation stay in the previous 12 months[1]
- Use of acute or subacute rehabilitation services in the previous 12 months[2]
- 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, Year 4 began on October 1, 2016. If a beneficiary had a hospitalization in July 2016 and home health care in October 2016, then the beneficiary would be eligible for the demonstration as of November 1, 2016.

In the following section, we explain how we used assessment data to measure limitations in ADLs. After that, we explain how we used Medicare claims to attribute eligible beneficiaries to the IAH group.

1. Eligibility and assessment data

We measured ADL limitations in accordance with the guidelines that the IAH implementation contractor gave to IAH practices. Those guidelines stated that a beneficiary qualified as having an ADL limitation if he or she needed 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), which is collected when a beneficiary receives home health care; (2) the Minimum Data Set (MDS), which is collected when a beneficiary receives skilled nursing facility (SNF) care; and (3) the Inpatient Rehabilitation Facility Patient Assessment Instrument (IRF-PAI), which is collected when a beneficiary receives inpatient rehabilitation facility care. All three data sets provided information about the extent to which a beneficiary 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 indicates the extent of limitations, if any, for each of the six ADLs. If a beneficiary did not have any assessment data in a given year, that person was ineligible for the demonstration in that year and we did not include him or her 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 datasets. Second, different providers collect ADL data at

[1] Includes acute care, critical access, and psychiatric hospitals.

[2] Includes discharge from inpatient rehabilitation hospitals and rehabilitation units or skilled nursing facilities, and use of home health (but not necessarily discharge). We did not include long-term care hospitals.

different points in time. Third, a beneficiary 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 dataset

Each ADL limitation is coded differently in each data set, and the codes do not always clearly define the person'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 a particular beneficiary indicated that the person needed human assistance to do the activity safely, then we classified him or her as needing human assistance with that ADL. Therefore, 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 the beneficiary 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 a beneficiary had an OASIS assessment with that value for the transferring data element, we did not consider that beneficiary 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. However, it might also have excluded some people who required human assistance and therefore could be IAH-eligible.

Although we usually did not score a beneficiary as having a limitation if he or she needed 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, a beneficiary 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 to measure ADL limitations to determine IAH eligibility. We used discharge assessments from all three data sets, and we also used interim assessments from the OASIS data set. We did not use admission or interim assessments from the MDS and IRF-PAI, because a beneficiary must be discharged from an SNF or inpatient rehabilitation facility before becoming eligible for IAH. Unlike with skilled nursing and inpatient rehabilitation services, a beneficiary can receive Medicare-funded home health care on the date he or she becomes eligible for IAH. Therefore, we included interim OASIS assessments[3] in addition to discharge assessments to ensure we had the latest information in the study year.

[3] 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.

c. A beneficiary can have multiple assessments in a given year

A beneficiary could have had more than one assessment in a given year. For example, in one demonstration year, a beneficiary 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 a beneficiary had at least two ADL limitations. We selected the most recent ADL assessment in which a beneficiary had at least two ADL limitations because we sought to identify beneficiaries who were least likely to recover from the ADL limitation. If a beneficiary had assessment data during a given year but did not have at least two ADL limitations in any of those assessments, that person was ineligible for the demonstration in that year and we did not include him or her in our sample. Also, if a beneficiary did not have any assessment data in a given year, that person was ineligible for the demonstration in that year and we did not include him or her in our sample.

2. 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):

1. Residence in the same state as the demonstration practice
2. At least one evaluation and management[4] (E&M) or non-E&M home visit from the demonstration practice; home included private homes, assisted living facilities, group homes, and custodial care facilities
3. 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 who were eligible for the demonstration be enrolled in the demonstration. Therefore, we required only one home visit for attribution to the IAH practice for beneficiaries who were eligible for the demonstration for three months or less. Some beneficiaries who were 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 IAH practice for beneficiaries who were eligible for the demonstration for more than three months.

In each of the six 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

[4] Evaluation and management visit refers to a patient-provider encounter during which the provider assesses the patient's medical history, conducts an evaluation, and engages in medical decision-making.

site as Mathematica-eligible IAH beneficiaries (or simply, IAH beneficiaries). IAH beneficiaries were the treatment group for the evaluation. The IAH group included two types of beneficiaries:

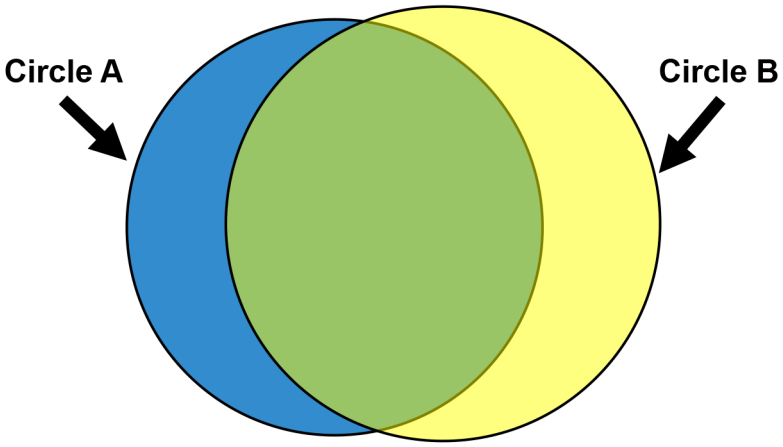
1) Those who met the eligibility and attribution criteria outlined above and were enrolled in the demonstration (in Figure B.1, the intersection of Circle A and Circle B, or the green oval)
2) Those who met the eligibility and attribution criteria outlined above but were not enrolled in the demonstration (the part of Circle B excluding Circle A, or the yellow crescent)

Thus, in order for a beneficiary to be in the IAH group for the evaluation, he or she 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 nonenrollment) in the demonstration did not affect whether that person was in the IAH group for the evaluation. As we described above, 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 (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 in 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.

In the rest of this appendix, we use the term *green oval* to refer to beneficiaries who were enrolled in the demonstration and met the eligibility and attribution criteria for the demonstration in administrative data according to Mathematica's analysis of administrative 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*) are those who were enrolled but not confirmed eligible for the demonstration or attributed to the IAH site according to administrative data.

Figure B.1. Groups of IAH beneficiaries based on different identification processes



Key:

Circle A (blue circle) = beneficiaries who were on the implementation contractor's list of IAH enrollees (IAH enrollees)

Circle B (yellow circle) = beneficiaries who Mathematica identified as eligible and attributed to an IAH practice (IAH beneficiaries used for the evaluation)

Intersection of Circle A and Circle B (green oval) = IAH enrollees Mathematica identified as eligible and attributed to an IAH practice

Circle A without Circle B overlap (blue crescent) = IAH enrollees Mathematica did not identify as eligible and attributed to an IAH practice

Circle B without Circle A overlap (yellow crescent) = beneficiaries Mathematica identified as eligible and attributed to an IAH practice but who were not enrolled in IAH

Note: Figure not drawn to scale.

After we identified an IAH beneficiary, the beneficiary remained in the sample for the rest of the demonstration or pre-demonstration year unless that person died or left Medicare FFS. For example, if an IAH beneficiary became eligible for the demonstration in November 2015 (month 2 of Year 4) and moved out of the IAH practice's geographic area or entered long-term care in April 2016, we continued to follow that beneficiary through the end of the measurement year (September 30, 2016, for all practices in Year 4).[5]

Demonstration Year 1. Mathematica identified 8,216 beneficiaries who met the demonstration eligibility criteria and were attributed to the demonstration practice during the first year (Table B.1). This group represented the IAH group in the first year of the demonstration. This group included 4,530 beneficiaries who were enrolled in the IAH demonstration according to the implementation contractor (the intersection of Circles A and B, the green oval in Figure B.1) and 3,686 beneficiaries who were not enrolled in the IAH demonstration in Year 1 (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 who we did not find to be eligible for the demonstration using administrative data (Circle A excluding Circle B, the blue crescent).

[5] For pre-demonstration years and Years 1–3, 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 Year 4 in October 2015.

Table B.1. Groups of beneficiaries based on different identification processes

Demonstration year	Mathematica-eligible IAH beneficiaries[a]			IAH-enrolled only (blue crescent)
	Mathematica-eligible and IAH-enrolled (green oval)	Mathematica-eligible only (yellow crescent)	Total IAH group (all Mathematica-eligible regardless of enrollment, yellow circle)	
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

Source: Mathematica's analysis of data from the implementation contractor and Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for IAH and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4. The data exclude three practices (Atlanta, Chicago, and Stuart) that withdrew from the demonstration before Year 4 and one practice (Louisville) terminated for cause.

[a]Corresponds to the yellow circle in Figure B.1, which is all Mathematica-eligible IAH beneficiaries (that is, those who met the demonstration eligibility criteria and were attributed to the demonstration practice).

IAH = Independence at Home.

Demonstration Year 2. 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 (the green oval in Figure B.1 and Table B.1), 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 who we did not find eligible for the demonstration using administrative data (the blue crescent).

Beneficiaries who were enrolled but not eligible and/or attributed according to Mathematica in Year 2 (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 who were included in the yellow circle in Year 1) and those who were not eligible according to administrative data in Year 1 (the blue crescent in Year 1). The enrollment process did not require an individual 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 (the yellow circle in Figure B.1 and Table B.1). As we explained previously, our method for measuring the effect of the demonstration required us to use the same data sources and approach to identifying 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 requirements, and it included people who were 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. This is 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. 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 beneficiaries were enrolled in the IAH demonstration in Year 3 (the intersection of Circle A and Circle B, or green oval, in Figure B.1 and Table B.1), and 3,066 beneficiaries were not enrolled (Circle B, excluding Circle A, or the yellow crescent). These 7,564 beneficiaries included people who were in the analysis sample in Years 1 or 2 and requalified in Year 3 by meeting eligibility and attribution requirements, and people who were 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 does not include the 4,718 beneficiaries who were on the implementation contractor's enrollment list in Year 3 but who we did not find eligible for the demonstration using administrative data (Circle A excluding Circle B, the blue crescent). Beneficiaries who were enrolled but were not eligible according to Mathematica in Year 3 (the blue crescent) include those who enrolled for the first time in Year 3. Beneficiaries who were enrolled but were not eligible according to Mathematica in Year 3 also include 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. 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 beneficiaries were enrolled in the demonstration (the intersection of Circles A and B, the green oval, in Figure B.1 and Table B.1), and 3,485 beneficiaries were not (Circle B excluding Circle A, 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, and people who were not in the analysis sample in any of the first three years.

As in Years 1 through 3, the analysis sample for the evaluation did not include the 5,663 beneficiaries who were enrolled in the IAH demonstration in Year 4 but who we found ineligible for and/or attributed to the demonstration using administrative data (Circle A excluding Circle B, or the blue crescent). The Year 4 IAH group also did not include beneficiaries who initially enrolled in Year 1, Year 2, or Year 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, the IAH 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 may 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 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 Year 1. The increase from Year 3 to Year 4 could have also been caused in part by some IAH practices participating in accountable care organizations (ACOs) in Year 4. Several IAH practices participated in ACOs 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 section of this appendix, Section IX.

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

The approaches of Mathematica and the implementation contractor to identifying eligible beneficiaries yielded different counts of IAH practices' beneficiaries. These counts differ because of two overarching reasons: (1) differences in the data sources and (2) differences in how the data were analyzed. Mathematica used only administrative data to identify eligible beneficiaries, whereas the implementation contractor used administrative data and data from the IAH practices.

We begin by examining reasons why beneficiaries who were enrolled in the demonstration in Year 4 did not meet Mathematica's eligibility and/or attribution criteria in Year 4 (the blue crescent in Figure B.1 and Table B.1). Next, we examine reasons why beneficiaries found eligible and attributed by Mathematica in Year 4 were not enrolled in the demonstration in Year 4 (the yellow crescent).

1. Reasons why some IAH enrollees did not meet Mathematica's eligibility and/or attribution criteria

In all four years, two of the three most common reasons that the Mathematica sample-construction process excluded an enrollee from the IAH group were that the relevant administrative data did not have information on the beneficiary's ADLs and that the beneficiary needed human assistance with zero or one ADL (Table B.2). The implementation contractor obtained ADL information from the IAH sites and used the IAH clinicians' interpretation of when a patient needed human assistance with an ADL. By contrast, we used assessment data that were collected as part of acute or subacute rehabilitation services. As we described previously in this appendix, if the beneficiary did not have any assessment instruments during the demonstration year, then we classified him or her as not having ADL data in that year, and we excluded him or her from the IAH group. In addition, if a beneficiary's assessment data indicated that he or she needed human assistance with zero or one ADL, we excluded him or her from the IAH group.

Besides differences in the source of ADL data, there were two other main reasons why beneficiaries who were enrolled were not found eligible or attributed by Mathematica. First, because enrollees who did not meet the eligibility criteria in a later demonstration year could remain enrolled in the demonstration, the number of enrollees who did not have ADL

information in administrative data or did not have a qualifying hospitalization increased over time. Specifically, the number of enrollees who did not have ADL information in administrative data was almost three times as high in Year 4 as in Year 1. Similarly, several hundred enrollees did not have qualifying hospitalizations in Years 2, 3, and 4, whereas that was the case for only 38 enrollees in Year 1.

Table B.2. Reasons beneficiaries who were enrolled in the demonstration were not found eligible or attributed by Mathematica

Reasons for differences	Number of beneficiaries			
	Year 1[a]	Year 2[a]	Year 3[a]	Year 4[a]
No ADL information in administrative data in the given demonstration year	1,157	1,658	2,590	3,068[b]
No ADLs or one ADL needing human assistance in the given demonstration year	575	598	532	704
Did not reside in an IAH state for the plurality of months during the demonstration year, based on Mathematica's assignment rules and according to CMS's enrollment database	138	192	164	290
Fewer than two chronic conditions	17	32	26	30
No qualifying hospitalization	38	594[b]	554[b]	600[b]
No qualifying rehabilitation services stay	6	29	11	22
In hospice[c]	66	273	136	147
Failed to meet FFS criteria after becoming eligible for the demonstration	40	132	68	69
Identified in administrative data as a long-term nursing home resident for the entire demonstration year after becoming eligible for the demonstration	187	248	263	360
Eligible for the demonstration but did not meet attribution rules	180	303	374	373
Other	1	0	0	0
Total	**2,405**	**4,059**	**4,718**	**5,663**

Source: Mathematica's analysis of data from the implementation contractor and Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for IAH and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4.

[a]Corresponds to the blue crescent in Figure B.1 and Table B.1.

[b]Most of these beneficiaries enrolled in the demonstration in a previous year and were not required to have a second qualifying hospitalization or rehabilitation services. See Section II.A of this appendix for details on the enrollment process.

[c]Beneficiary met all other eligibility criteria but was in hospice for all months in the demonstration year or on the date of eligibility for the demonstration.

ADLs = activities of daily living; CMS = Centers for Medicare & Medicaid Services; FFS = fee for service; IAH = Independence at Home.

The final main reason why beneficiaries who were enrolled were not found eligible or attributed by Mathematica is that Mathematica used claims-based algorithms to identify otherwise eligible beneficiaries who were in long-term care and to measure whether an eligible beneficiary met the attribution criteria based on visits from the IAH practice after the eligibility date for the demonstration. Unlike Mathematica, the implementation contractor used a combination of administrative data and information from the IAH practices to measure long-term nursing home residence and visits to the IAH practice. Beneficiaries who were in long-term care were not eligible for the demonstration. Using claims data, we identified 360 enrollees as long-term nursing home residents in Year 4.[6] We identified another 373 enrollees in Year 4 as being eligible for the demonstration in that year, but they did not have the visit(s) required for attribution to the IAH group. Of those 373 beneficiaries, we did not attribute one-quarter (96) to the IAH practice, because they had only one visit from the IAH practice and were eligible for the demonstration for more than three months. (As we described in this appendix, Section II.B, we required at least two visits from the IAH practice for beneficiaries who were eligible for more than three months.) Some of the other beneficiaries had at least one visit from the IAH practice, but none of those visits occurred after their claims-based eligibility date in Year 4. (As explained previously, Mathematica used visits that occurred between the claims-based eligibility date and the end of the demonstration year or date of death to attribute beneficiaries to IAH practices. The claims-based eligibility date reflected when the beneficiary met the utilization requirements for demonstration eligibility.)

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

Next, we explored data for the other group of beneficiaries who Mathematica and the implementation contractor identified differently in Year 4—3,485 beneficiaries who were in the IAH group for the evaluation but were not IAH enrollees (the yellow crescent in Figure B.1 and Table B.1). We split the nonenrollee IAH beneficiaries into three groups: those who the implementation contractor did not find eligible based on their examination of administrative data, those who were eligible according to the implementation contractor's examination of administrative data but were not eligible according to data from the IAH practice, and those who were eligible according to the implementation contractor and IAH practice but disenrolled from the demonstration within six months (Table B.3). We discuss each of those three groups in this section.

a. IAH beneficiaries whom the implementation contractor did not find eligible based on administrative data

In each demonstration year, several hundred IAH beneficiaries in the evaluation sample were not enrolled in the demonstration because they did not meet all eligibility criteria according

[6] Measuring the use of institutional long-term care is challenging for a number of reasons. The main challenge is that often it is unclear whether entry into a facility for care is a temporary or permanent move. Patients and their families often believe that the move is temporary and that the patient will return home. It is only with hindsight that participants, or researchers, can determine whether the care qualified as long-term care. The second challenge is that institutional long-term care is paid for by a number of sources—out-of-pocket spending by individuals and their families, private insurance, publicly reimbursed managed care programs (such as the Program of All-Inclusive Care for the Elderly), and Medicaid. As a result, no single claims-based dataset captures entry into institutional long-term care.

to the implementation contractor. In Year 4, this was the case for 1,000 IAH beneficiaries. The main reason for this was differences in how Mathematica and the implementation contractor measured whether the beneficiary had a qualifying hospitalization and rehabilitation services stay within a 12-month period. As part of determining eligibility for enrolling in the demonstration, the implementation contractor considered the dates that the beneficiary had a hospitalization, a rehabilitation services stay, and a home visit from the IAH practice. Mathematica considered only the dates of the qualifying hospitalization and rehabilitation services stay. Unlike the implementation contractor, we did not require a beneficiary to have had a home visit from the IAH practice before becoming eligible for the demonstration, and we did not rely on the date of a home visit when setting the demonstration eligibility date. The reason we did not use the date of the home visit as part of determining the IAH eligibility date was that we could not replicate that requirement for the comparison group, who did not receive home-based primary care.

The number of beneficiaries in the evaluation sample who were not enrolled because they did not meet all eligibility criteria according to the implementation contractor increased from Year 3 (588 beneficiaries) to Year 4 (1,000 beneficiaries). According to our analysis of administrative data, all of those beneficiaries met the eligibility and attribution criteria we described previously. However, because of the large increase from Year 3 to Year 4, we examined how much care those 1,000 beneficiaries received from the IAH practice in Year 4. To do this, we counted the number of actual (not annualized) E&M and non-E&M home visits by IAH clinicians between a beneficiary's date of eligibility for the demonstration and the end of Year 4. More than 70 percent of these 1,000 beneficiaries had either three or more home visits from the IAH practice (576 beneficiaries) or fewer than three home visits but were observed in claims data three months or fewer (137 beneficiaries). In other words, most of the beneficiaries in our IAH group who were not enrolled by the sites had several visits from the IAH practice or had one or two visits but were observed in claims data only for a short time.

b. IAH beneficiaries whom the implementation contractor found eligible for enrollment but were excluded from enrollment on the basis of information from IAH practice

The second group of IAH beneficiaries who were in the evaluation sample but were not enrolled in Year 4 comprised 2,107 beneficiaries whom the implementation contractor originally identified as eligible based on claims but later excluded from the Year 4 enrollment sample in response to information supplied by the sites. The most frequently offered reason for excluding them from enrollment was that they did not meet the ADL criterion. Of the 792 beneficiaries who did not meet the chronic condition and/or ADL criterion, 666 failed to meet the ADL criterion only. According to our analysis of the assessment data, however, all of these beneficiaries met the ADL requirement. The conflicting ADL measures might suggest that these 666 beneficiaries improved between the assessment and the clinical evaluation by site, meaning they did not require human assistance for at least two ADLs at the time the IAH clinician measured their ability to perform ADLs.

Among beneficiaries whom the implementation contractor originally identified as potentially eligible based on claims but later excluded from the enrollment sample, the second most commonly offered reason for exclusion was that the beneficiary was not currently a patient of the IAH practice. The Mathematica process required each beneficiary who was eligible for

more than three months during a given demonstration year to have received at least two visits from an IAH clinician; however, IAH practices reported that some Medicare beneficiaries were no longer part of the in-home practice. Examples of why a beneficiary would have received some home-based primary care but the IAH practice reported him or her as not being a patient of their practice include the following: (1) The patient received transitional care following acute care use or the practice made a few in-home visits but was not their primary care clinician, (2) the patient moved out of the practice's service area, or (3) the patient changed to another clinician (who could provide home-based care). The number of IAH beneficiaries whom Mathematica attributed to the IAH practice but were reported as not being a patient of the IAH practice increased from 320 in Year 3 to 643 in Year 4. Of those 643 beneficiaries, 263 were from the Brooklyn practice. It is possible that some of those beneficiaries chose to change clinicians when Brooklyn merged with another home-based primary care practice.

We assessed the extent to which IAH clinicians treated these 643 beneficiaries by examining data on actual (not annualized) E&M and non-E&M home visits by IAH clinicians in Year 4. Across all sites, 61 percent of the 643 beneficiaries had three or more home visits between their date of eligibility for the demonstration and the end of Year 4. The remaining 39 percent of the 643 beneficiaries had one or two home visits. Among the group who had one or two home visits, 95 beneficiaries were observed for six months or fewer in Year 4, and 157 were observed for more than six months. In other words, for 157 of these 643 people, there were more than six months between the time the beneficiary became eligible for IAH in Year 4 and the end of the demonstration year (or date of death), but the beneficiary had only one or two home visits in that time. Durham was a notable outlier, as nearly three-quarters (73.7 percent) of the IAH beneficiaries in its IAH group whom Durham reported as not being a patient of its practice had three or more home visits.

c. IAH beneficiaries who were found eligible by the implementation contractor and IAH practice but disenrolled from the demonstration

The third category of beneficiaries who were in the IAH group for the evaluation but were not on the list of enrollees was beneficiaries who disenrolled from the demonstration. The RTI process allows for enrollees to voluntarily disenroll from the demonstration when a beneficiary changes clinicians within the practice service area, is discharged by the practice, declines home care, or elects hospice and changes clinician. 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. The number of beneficiaries in our IAH group who voluntarily disenrolled increased from 121 in Year 3 to 352 in Year 4. Because of this increase, we assessed the extent to which IAH clinicians treated these 352 beneficiaries by examining data on actual (not annualized) E&M and non-E&M home visits by IAH clinicians in Year 4. Almost two-thirds (64.2 percent) of the beneficiaries who voluntarily disenrolled within six months were treated by one of four Visiting Physicians Association (VPA) practices: Dallas, Flint, Jacksonville, or Lansing. Among the beneficiaries who voluntarily disenrolled from one of those four VPA practices, 37.2 percent had six visits or more and 44.2 percent had three to five visits.

Table B.3. Reasons beneficiaries found eligible and attributed by Mathematica were not enrolled in the demonstration in the same year

Reasons for differences	Number of beneficiaries			
	Year 1[a]	Year 2[a]	Year 3[a]	Year 4[a]
Patient not found eligible by the implementation contractor				
Patient did not meet all eligibility criteria in the given demonstration year[b]	351	447	588	1,000
Implementation contractor reported no home visit by an IAH clinician within one year of the given demonstration year start date	703[c]	1	2	0
Subtotal	**1,054**	**448**	**590**	**1,000**
Patient found eligible by the implementation contractor but not reported eligible by the IAH practice				
Patient did not have two chronic conditions and/or two ADLs requiring human assistance	893	1,249	1,267	792[e]
Patient died before receiving IAH notification letter or before enrollment date	722	488	582	414
Patient was not currently part of an IAH in-home practice	776	223	320	643
Patient did not agree to participate[d]	46	74	122	176
Practice reported hospice care or moved to long-term care before receiving IAH notification letter	56	28	44	82
Subtotal	**2,493**	**2,062**	**2,335**	**2,107**
Patient found eligible by the implementation contractor and IAH practice but disenrolled[f]				
Patient voluntarily disenrolled within six months of enrollment	139	151	121	352
Patient was enrolled in a previous demonstration year and disenrolled before the current year	n.a.	41	20	26
Subtotal	**139**	**192**	**141**	**378**
Total	**3,686**	**2,702**	**3,066**	**3,485**

Source: Mathematica's analysis of data from the implementation contractor and Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for IAH and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4.

Notes: The implementation contractor used administrative data to determine whether the beneficiary met Medicare enrollment and service utilization criteria required for demonstration eligibility. Among those whom the implementation contractor found to be eligible using administrative data, IAH practices identified patients who failed to meet the remaining IAH eligibility criteria, such as ability to perform ADLs and not residing in a long-term nursing home.

[a]Corresponds to the yellow crescent in Figure B.1 and Table B.1, which is beneficiaries who were Mathematica-eligible only, not enrolled in IAH.

[b]Differences in the methods the implementation contractor and Mathematica used to assess administrative data led to Mathematica identifying some beneficiaries as meeting the service use criteria for IAH eligibility in a given year, though the implementation contractor did not.

[c]In Year 1, 703 beneficiaries we considered eligible for the demonstration were not found eligible because the implementation contractor reported no home visit. The primary reason this number decreased sharply after Year 1 is that we used the National Provider Identifiers collected by the implementation contractor from the IAH practices, whereas in Year 1, Mathematica collected National Provider Identifiers from the IAH practices in a separate process, which took place at a different time. Sites did not identify the same clinicians as being IAH clinicians in the two different time periods.

[d]We excluded patients who refused to participate in the demonstration from the IAH group we used to estimate practice-level expenditure regressions for shared savings calculations, but we did not exclude them from the IAH group for the evaluation.

[e]Of these 792 beneficiaries, 85 failed to meet only the chronic condition requirement, 666 failed to meet only the ADL requirement, and 41 failed to meet both requirements.

[f]Most beneficiaries who disenrolled from the demonstration left the IAH practice.

ADLs = activities of daily living; IAH = Independence at Home.

n.a. = not applicable.

In Year 4, our eligibility analysis, based on administrative data, did not identify slightly less than half of the 11,682 beneficiaries enrolled in the demonstration as eligible for the IAH group. The proportion of such enrollees varied across sites, ranging from 36.6 percent in Dallas to 64.4 percent in Washington, DC (Table B.4). The share of enrollees we did not identify as eligible in Year 4 (51.5 percent) was the same as in Year 3 (51.2 percent) and somewhat higher than in Year 2 (47.1 percent), all of which were substantially higher than in Year 1 (34.7 percent). This increase occurred because beneficiaries who enrolled in the first and second years of the demonstration were not required to qualify for the demonstration in the third or fourth year.

Table B.4. Number of IAH enrolled beneficiaries and Mathematica-eligible beneficiaries, by practice, demonstration Year 4

Site	Number of enrollees[a] (A)	Number of Mathematica-eligible IAH beneficiaries[b] (B)	Percentage of enrollees who are not Mathematica-eligible[c] (C)	Percentage of Mathematica-eligible IAH beneficiaries who are not enrolled[d] (D)
Austin, TX	491	686	40.1	57.1
Boston, MA	214	149	47.7	24.8
Brooklyn, NY	866	1,055	48.3	57.5
Cleveland, OH	350	331	46.0	42.9
Dallas, TX	1,539	1,344	36.6	27.4
Durham, NC	2,052	1,705	51.2	41.3
Flint, Michigan	2,044	1,607	43.6	28.3
Jacksonville, FL	737	504	52.4	30.4
Lansing, MI	1,016	652	49.6	21.5
Long Island, NY	352	235	63.4	45.1
Milwaukee, WI	911	575	58.0	33.4
Philadelphia, PA	87	58	47.1	20.7
Portland, OR	255	171	62.4	43.9
Richmond, VA	100	85	47.0	37.6
Washington, DC	295	134	64.4	21.6
Wilmington, DE	373	213	54.2	19.7
Total	**11,682**	**9,504**	**48.5**	**36.7**

Source: Mathematica's analysis of data from the implementation contractor and Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for IAH and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4.

Note: Beneficiaries who were enrolled in Year 1, 2, or 3 remained enrolled for the duration of the demonstration unless they disenrolled, died, or left FFS Medicare.

[a]Corresponds to blue circle in Figure B.1, which is all beneficiaries who were IAH enrolled, whether or not they were Mathematica-eligible.

[b]Corresponds to yellow circle in Figure B.1, which is beneficiaries who were Mathematica-eligible, whether or not they were IAH-enrolled.

[c]Percentage of Column A who were not Mathematica-eligible. Corresponds to the blue crescent in Figure B.1 and Table B.1, which is beneficiaries who were IAH enrolled only, not Mathematica-eligible.

[d]Percentage of Column B who were not enrolled. Corresponds to yellow crescent in Figure B.1 and Table B.1, which is beneficiaries who were Mathematica-eligible only, not enrolled in IAH. Denominator is the yellow circle, which is all Mathematica-eligible IAH beneficiaries.

IAH = Independence at Home.

III. IDENTIFYING THE COMPARISON GROUP

In this section, 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 four 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 Section 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. 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. For example, if a site had at least one IAH beneficiary who lived in each of 57 zip codes during demonstration Year 1, then 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, New York; Austin and Dallas, Texas; and Flint and Lansing, Michigan), some zip codes contained IAH beneficiaries for two practices. We could not simply restrict potential comparison beneficiaries to only those living in the zip codes represented by beneficiaries served by the IAH practice in a given year, because that 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, as described later) to split the comparison sample into two potential comparison groups. For each pair of sites located in the same state, we included in the preliminary model all the IAH beneficiaries in those two sites as well as all beneficiaries who were 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, and this determined the site potential comparison pool to which the beneficiary was assigned. Using this partition, we matched IAH beneficiaries to the potential comparison for each practice using the same approach as for other sites.

Since many potential comparison beneficiaries lived in a zip code served by both demonstration practices in a given state, we could not simply restrict potential comparison beneficiaries to only those living in the zip codes represented by beneficiaries served by the IAH practice in a given year. In this situation, we used a basic, first-stage propensity score matching to split the overlapping comparison sample into two potential comparison groups, each designated to treatment beneficiaries in one of the two IAH practices.[7] To conduct the first-stage matching, we combined the IAH beneficiaries for both sites and the overlapping comparisons (already limited by the zip code restriction) and ran the matching algorithm using the same covariates that we used for the final matching (described later). In a few cases, we used exact matching variables during first-stage matching to divide the matching problem into smaller, computationally feasible pieces, because of the large number of treatment and potential comparison beneficiaries. After using first-stage matching to split the overlapping comparison sample into two potential comparison groups (one group per site), we then matched comparison beneficiaries within each designated pool to treatment beneficiaries for each IAH practice, by the same approach described above.

As was the case for IAH beneficiaries, beneficiaries in the matched comparison group in demonstration Years 1, 2, or 3 were again identified as potential comparison beneficiaries if they met all IAH eligibility requirements in Year 4.

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 individuals 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 (receipt of home-based primary care from an IAH practice) and outcomes. Then, subject to that constraint, we 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, as well as measures of health status, health trajectory, and other personal characteristics observable in administrative data that are predictive of healthcare expenditures. Limiting the comparison group to Medicare beneficiaries who closely matched observed characteristics of the IAH group might also have reduced differences between the two groups on *unobserved* characteristics if those characteristics were correlated with matching variables. However, as discussed later in this appendix in the section on limitations, there could still be differences in unobserved characteristics between the IAH and matched comparison groups that affect the results.

[7] Conducting a first-stage 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.

We conducted matching for the entire IAH group, which consisted of beneficiaries who met the eligibility and attribution criteria based on administrative data (the yellow circle in Figure B.1 and Table B.1). For Year 4, for example, we matched 9,504 IAH beneficiaries on observable characteristics with beneficiaries who were similar and lived in the same geographic area but who 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 demographics and health-related variables for matching 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 inpatient 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 utilization tend to be substantially higher in the months following a hospitalization, and these were our key outcomes of interest. 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 an inpatient admission, we used whether the beneficiary had an observation stay but not an inpatient admission in the prior year (Table II.2).[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, many of which were indicators for the CMS HCCs that CMS defined for its managed care risk-adjustment model. 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. To increase the likelihood that the comparison group was as similar as possible to the IAH beneficiaries in terms of 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) identified any of the diagnosis codes in that HCC as predictive of mortality among elderly low-income Medicare beneficiaries.

[8] For pre-demonstration years and Years 1–3, 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 Year 4 in October 2015.

We collapsed several of the individual HCCs based on the type of condition, frequency in the IAH group, and relative factor, the last of which represents the contribution of that HCC to the overall HCC risk score.[9,10,11] We also used the risk score itself as a matching variable. Additional details about how we calculated HCC score and indicators are available in Section V of this appendix.

Table B.5. Variables used in propensity–score matching equation

Variable
Eligibility and utilization
Number of months since most recent inpatient admission (1, 2 or 3, 4 or more)
Month of the demonstration year that beneficiary met eligibility criteria (1, 2–6, 7–12)[a]
Whether beneficiary had an observation stay and no inpatient admission in prior 12 months
Demographic characteristics
Age: younger than 65, 65–79, 80 or older
Gender
Race: white, black or African American, 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: two, three to four, five to six
Whether information about the feeding ADL was missing[b]
Health status
HCC risk score
Specific HCCs
HCC8: Metastatic cancer and acute leukemia[c]
HCC9–10: Lung and other severe cancers; lymphoma and other cancers
HCC11–12: Colorectal, bladder, and other cancers; breast, prostate, and other cancers and tumors
HCC18: Diabetes with chronic complications
HCC21: Protein-calorie malnutrition[c]
HCC27: End-stage liver disease
HCC28–29: Cirrhosis of liver; chronic hepatitis
HCC46: Severe hematological disorders
HCC48: Coagulation defects and other specified hematological disorders
HCC51: Dementia with complications[c]
HCC52: Dementia without complications[c]

[9] For example, we combined cirrhosis of the liver (HCC28) and chronic hepatitis (HCC29) into a single indicator for matching but did not combine them with end-stage liver disease (HCC27). Less than 2 percent of the treatment group had cirrhosis of the liver or chronic hepatitis, and the relative factor for those conditions was less than half the relative factor for end-stage liver disease.

[10] 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/MedicareAdvtgSpecRateStats/Downloads/Announcement2012.pdf.

[11] We used software version V2116 to calculate HCC scores for beneficiaries in Year 4, which incorporated versions 9 and 10 of the *International Classification of Diseases* (ICD-9 and ICD-10). To address the concern that the transition from the ICD-9 system to the ICD-10 system in October 2015 might have affected the HCC score calculation, we reran the effect regression on total expenditures, including full interactions of HCC score and indicators with the Year 4 indicator, in addition to other covariates. The results did not change, suggesting that the ICD-10 transition did not affect our estimated effects.

Table B.5 (continued)

Variable
HCC54–55: Drug/alcohol psychosis; drug/alcohol dependence
HCC57–58: Schizophrenia; major depressive, bipolar, and paranoid disorders
HCC70–71: Quadriplegia; paraplegia
HCC72: Spinal cord disorders/injuries
HCC85: Congestive heart failure[c]
HCC96: Specified heart arrhythmias
HCC103–104: Hemiplegia/hemiparesis; monoplegia, other paralytic syndromes
HCC106: Atherosclerosis of the extremities with ulceration or gangrene
HCC107–108: Vascular disease with complications; vascular disease
HCC111: Chronic obstructive pulmonary disease
HCC134: Dialysis status[c]
HCC136–138: Chronic kidney disease, stages 3–5[c]
HCC139–140: Chronic kidney disease, stages 1–2 or unspecified; unspecified renal failure
HCC157–159: Pressure ulcer of skin with necrosis through to muscle, tendon, or bone; or with full or partial thickness skin loss
Depression[d]
Anemia
Fluid and electrolyte disorders
Number of chronic conditions (2–5, 6–9, 10 or more)[d]
Whether beneficiary had a complicating condition or major complicating condition during the most recent inpatient 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 that is used as an independent variable in the matching regression equation.

[a]For pre-demonstration years and Years 1–3, 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 Year 4 in October 2015.

[b]Feeding assessments were not available on 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, we used the feeding values from that assessment; however, sometimes there was no previous discharge assessment.

[c]Identified as a key predictor of mortality by Gagne et al. (2011); they are the measures of health status that 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]Chronic condition categories measured by the Chronic Conditions Warehouse.

ADL = activity of daily living; ESRD = end-stage renal disease; HCC = hierarchical condition category; IAH = Independence at Home.

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. 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 inpatient 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

A standard statistic used to assess similarities between the treatment group and final matched comparison group is the standardized difference in means (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 for our matching of 0.01. We examined the matching results for both the variables that we used in the matching algorithm and the variables that might be important to control for but could not be included, such as individual HCCs aggregated with other HCCs in the matching equation (such as cirrhosis of the liver and chronic hepatitis), and individual chronic conditions measured by the Chronic Conditions Warehouse.

Across all 14 sites together (treating the three Mid-Atlantic Consortium sites as one site), in the fourth demonstration year, the absolute value of the standardized difference was less than 0.10 on all matching variables and less than 0.10 on all but three nonmatching variables (Table B.6).[12] All 14 sites individually had standardized differences of less than 0.25 on all the matching variables, and for 12 of those sites the standardized differences were also less than 0.10 on all the matching variables (data not shown). Furthermore, 7 of the sites had standardized differences of less than 0.25 on all the nonmatching variables.

Table B.6. Characteristics of potential comparison beneficiaries, matched comparison beneficiaries, and IAH beneficiaries, Year 4

Variable	Potential comparison group	Matched comparison group	IAH beneficiaries	Standardized difference
Eligibility for the demonstration				
Proportion with number of months since most recent inpatient admission[a]				
One	0.570	0.387	0.387	0.000
Two or three	0.156	0.171	0.171	0.000
Four or more	0.274	0.443	0.443	0.000
Proportion with month of the demonstration year that beneficiary met eligibility criteria[b]				
Month 1	0.425	0.643	0.635	−0.016
Months 2-6	0.286	0.208	0.212	0.010

[12] Data for the matched comparison group included 19 beneficiaries who we later removed from our comparison group because we learned that they were enrolled in IAH in Year 4. The reason a comparison beneficiary could have been enrolled in IAH is as follows: For us to identify a beneficiary for the potential comparison group, that person must have had no visits from IAH practices during the current demonstration year. However, because we use a different eligibility date for measuring visits to IAH practices than the date used by the implementation contractor, there is a possibility that potential comparison beneficiaries could receive a visit from an IAH practice in the current year and be enrolled in IAH. For our analysis of Year 4, we conducted propensity-score matching before receiving IAH enrollment data. Subsequently, we removed from the matched comparison group any beneficiaries who were enrolled in IAH. In Year 4, 19 matched comparison beneficiaries were enrolled in IAH. Those 19 beneficiaries were enrolled in Cleveland, Durham, Jacksonville, the Mid-Atlantic Consortium, Milwaukee, and Wilmington. We excluded the 19 matched comparison beneficiaries who were enrolled in IAH from the final sample used for effect analyses. We reweighted those 19 matched sets so that the number of weighted matched comparison beneficiaries equaled the number of treatment beneficiaries.

Table B.6 (continued)

Variable	Potential comparison group	Matched comparison group	IAH beneficiaries	Standardized difference
Months 7-12	0.288	0.149	0.153	0.010
Proportion with observation stay and no inpatient admission in prior 12 months	0.055	0.100	0.095	−0.016
Demographic characteristics				
Female	0.618	0.666	0.670	0.007
Age				
Proportion younger than 65	0.133	0.161	0.165	0.010
Proportion 65–79	0.421	0.325	0.328	0.006
Proportion 80 or older	0.445	0.514	0.507	−0.013
Race and ethnicity				
Proportion white	0.755	0.697	0.691	−0.013
Proportion black	0.185	0.249	0.256	0.017
Proportion other	0.06	0.054	0.053	−0.005
Proportion dually eligible for Medicare and Medicaid	0.253	0.382	0.384	0.005
Original reason for Medicare entitlement				
Proportion whose original eligibility was due to age	0.737	0.675	0.67	−0.011
Proportion whose original eligibility was due to disability	0.241	0.314	0.319	0.011
Proportion whose original eligibility was due to ESRD or ESRD plus disability	0.022	0.012	0.012	−0.001
ADLs				
Proportion with two ADLs	0.131	0.085	0.083	−0.007
Proportion with three or four ADLs	0.310	0.328	0.324	−0.010
Proportion with five or six ADLs	0.559	0.586	0.593	0.014
Proportion missing information about feeding ADL	0.100	0.170	0.178	0.023
Health status				
HCC risk score	3.496	3.888	3.931	0.024
Proportion with HCCs				
HCC 8: Metastatic cancer	0.044	0.017	0.017	−0.003
HCC 9–10: Lung, lymphoma and other cancers	0.06	0.038	0.037	−0.005
HCC 11–12: Colorectal, bladder, breast, prostate, and other cancers	0.109	0.084	0.081	−0.01
HCC 18: Diabetes with chronic complications	0.309	0.333	0.331	−0.004
HCC 21: Protein-calorie malnutrition	0.132	0.184	0.201	0.044
HCC 27: End-stage liver disease	0.018	0.01	0.01	0
HCC 28–29: Cirrhosis of liver and chronic hepatitis	0.027	0.023	0.023	−0.002
HCC 46: Severe hematological disorders	0.018	0.013	0.012	−0.01
HCC 48: Coagulation defects and other specified hematological disorders	0.166	0.132	0.13	−0.006
HCC 51: Dementia with complications	0.069	0.169	0.179	0.029
HCC 52: Dementia without complications	0.199	0.326	0.317	−0.021

Table B.6 (continued)

Variable	Potential comparison group	Matched comparison group	IAH beneficiaries	Standardized difference
HCC 54–55: Drug/alcohol psychosis and drug/alcohol dependence	0.069	0.072	0.07	−0.009
HCC 57–58: Schizophrenia, major depressive, bipolar, and paranoid disorders	0.158	0.234	0.237	0.006
HCC 70–71: Quadriplegia, paraplegia	0.025	0.06	0.067	0.032
HCC 72: Spinal cord disorders/injuries	0.025	0.021	0.02	−0.006
HCC 85: Congestive heart failure	0.447	0.509	0.503	−0.011
HCC 96: Specified heart arrhythmias	0.386	0.36	0.351	−0.018
HCC 103–104: Hemiplegia/hemiparesis, monoplegia, other paralytic syndromes	0.093	0.131	0.131	0.002
HCC 106: Atherosclerosis of the extremities with ulceration or gangrene	0.043	0.041	0.042	0.007
HCC 107–108: Vascular disease with or without complications	0.424	0.488	0.481	−0.014
HCC 111: Chronic obstructive pulmonary disease	0.333	0.372	0.374	0.004
HCC 134: Dialysis status	0.055	0.038	0.039	0.003
HCC 136–138: Chronic kidney disease, stage 3-5	0.084	0.11	0.111	0.002
HCC 139–140: Chronic kidney disease stage 1-2, unspecified renal failure	0.051	0.067	0.067	0.001
HCC 157–159: Pressure ulcer of skin with necrosis or skin loss	0.066	0.136	0.148	0.036
Number of chronic conditions measured by Chronic Conditions Warehouse				
Fewer than six	0.17	0.097	0.101	0.012
Six to nine	0.468	0.438	0.431	−0.013
More than nine	0.362	0.465	0.468	0.006
Proportion with anemia[c]	0.382	0.23	0.222	−0.018
Proportion with depression	0.418	0.542	0.546	0.008
Proportion with fluid and electrolyte disorders[c]	0.398	0.425	0.419	−0.011
Proportion with chronically critically ill or medically complex diagnosis[d]	0.321	0.329	0.331	0.006
Proportion with complicating condition or major complicating condition during the most recent inpatient admission	0.556	0.55	0.551	0.001

Source: Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for IAH and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4.

Notes: Data for the matched comparison group include 19 beneficiaries who we later removed from our comparison group because we learned that they were enrolled in IAH in Year 4. Data for the matched comparison group are weighted to reflect multiple comparison beneficiaries matched to individual IAH beneficiaries. The final sample sizes in Year 4 were 9,504 IAH beneficiaries and 38,365 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 one or more months before the demonstration, the Month 1 group includes that person. For all sites in Year 4, Month 1 was October.

[c]Measured using claims from the most recent inpatient stay and observation stay in the year before the demonstration eligibility date. Diagnosis codes for these conditions were drawn from Gagne et al. (2011).

Table B.6 (continued)

[d]Measured using diagnoses from the most recent inpatient stay in the year before the demonstration eligibility date. Diagnoses were drawn from Kandilov et al. (2014).

ADLs = activities of daily living; ESRD = end-stage renal disease; HCC = hierarchical condition category; IAH = Independence at Home.

As in Year 4, the IAH and matched comparison groups were very similar in each of the first three demonstration years. Across all sites together in each of the first three 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 level, all 14 sites in Years 1 to 3 had standardized differences of less than 0.25 on all the matching variables, and most of the 14 sites had standardized differences of less than 0.10 on all the matching variables.

D. Number of beneficiaries and eligible months

Over the four years of the demonstration, the number of IAH beneficiaries varied, and for each IAH beneficiary, we matched four comparison beneficiaries, on average. Across the demonstration years, the average number of eligible months for the comparison beneficiaries was slightly smaller than among the IAH beneficiaries (Table B.7). This difference arose because the comparison beneficiaries were more likely to die within one year of the eligibility date than the IAH 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 possible concerns that this difference might cause, we incorporated an eligibility fraction in the weighting design for regressions, where the eligibility weight reflected the number of months eligible for the demonstration in a given year. For example, a beneficiary who was eligible for the demonstration for six months in Year 4 had half the weight of a beneficiary who was eligible for the demonstration for twelve months in Year 4. 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 inpatient admission, and month of the demonstration year that the beneficiary met the eligibility criteria. We did this to control for differences in the time between when beneficiaries met the service utilization criteria required for demonstration eligibility and their eligibility date. Those who qualified in the first month may have met both of the service utilization criteria up to one year before the demonstration year began, whereas those who qualified in later months met at least one of the two service utilization criteria in the month immediately before the eligibility date. Section VI of this appendix provides additional details about weights and control variables.

Table B.7. Analysis sample, by years

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

Source: Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for IAH and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4.

IAH = Independence at Home.

IV. IDENTIFYING BENEFICIARY AND PRACTICE SUBGROUPS

In addition to estimating the effects of the demonstration across all IAH sites, we estimated effects for subgroups to assess whether the demonstration payment incentive worked better for certain groups than for others.

A. Beneficiaries with dementia

We hypothesized that there might be more value in the 48-hour follow-up visit and medication reconciliation required by the IAH demonstration for dementia patients than for other types of patients. To test this hypothesis, we separately estimated the demonstration's effects on key outcomes (Medicare expenditures and use of acute care services) for the subgroups of beneficiaries with dementia versus those without dementia, and assessed whether results differed between the two groups.

We identified IAH and comparison beneficiaries with dementia as those for whom either of the two HCC flags for dementia (HCC51 and HCC52) equaled one. In Year 4, half of the IAH and matched comparison beneficiaries had dementia. Because we did not use dementia as an exact matching variable, we assessed the similarity of each of the two subgroups by reviewing standardized differences on the variables we used for propensity-score matching (Table B.5). Using data for all sites pooled, IAH and comparison beneficiaries were very similar in both subgroups—beneficiaries with and without dementia. Neither group had any matching variables whose standardized difference exceeded 0.10 (data not shown).

B. Type of practice

To test whether the financial and operational differences between practice types affected the estimated effects, we examined the key outcomes separately for each type of practice in Years 4 and 5. For this subgroup analysis, we organized the IAH practices with structurally similar features into the following three groups:

- VPAs (VPA Dallas, VPA Flint, VPA Jacksonville, VPA Lansing, and VPA Milwaukee). The five practices (all sole entities) that fall into the VPA category were part of the same corporate structure, the Visiting Physician Association.[13] Although each practice varied by geographic location and patient population, the VPA corporate leadership attempted to standardize operations and delivery of care methods among all VPA practices.
- Independent practices (Austin, Brooklyn, Durham, and Portland). The four practices that we classified as independent practices were all privately owned and were not attached to an overarching health care system.
- Academic health centers/health systems (Boston, Cleveland, North Shore, Richmond, and Wilmington). The five sites (four sole entities and one consortium) in this category were integrated into health care systems affiliated with a university or medical school. These practices typically received significant financial and operational support from their health systems.

[13] VPA is a health care company based in multiple states and is part of the corporate umbrella organization U.S. Medical Management.

V. MEDICARE DATA AND OUTCOMES

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. Data for determining demonstration eligibility and measuring outcomes in the analytic files were drawn from several sources (Table B.8). We accessed all data through the Virtual Research Data Center Data Enclave.

Table B.8. Data sources

Data	Demographic characteristics	Chronic conditions	Activities of daily living	Service use: Demon-stration eligibility	Service use: Outcome measures
Medicare enrollment database	X				
Master beneficiary summary file		X			
Inpatient claims				X	X
Outpatient claims				X	X
Physician or supplier claims				X	X
Home health agency claims				X	X
Skilled nursing facility claims				X	X
Hospice claims				X	X
Durable medical equipment claims					X
Inpatient rehabilitation facility–patient assessment instrument[a]			X		
Minimum data set			X		
Outcome and assessment information set			X		

[a]Includes inpatient rehabilitation hospitals and rehabilitation units. Excludes long-term care hospitals.

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 (Table B.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 group, we estimated the HCC score by using the publicly available HCC software (CMS 2017) 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 terms of being sicker and frailer than the average Medicare beneficiary.

Table B.9. Measures of Medicare expenditures, service utilization, and health outcomes used in regressions

Measure
Medicare expenditures per beneficiary per month
Total
Inpatient
Home health service[a]
Outpatient
Skilled nursing facility
Physician or supplier
Hospice
Durable medical equipment
Medicare service utilization
Acute inpatient care
Number of hospital admissions per beneficiary per year[b]
Number of hospital admissions for an ACSC per beneficiary per year (AHRQ PQI)[b]
Number of ED visits per beneficiary per year[c]
Number of outpatient ED visits for an ACSC per beneficiary per year (AHRQ PQI)[c]
Percentage of beneficiaries with a qualifying index discharge and an unplanned readmission within 30 days of discharge in the year[d]
Other types of utilization
Number of E&M visits in nonacute settings by primary care clinicians per year[e]
Number of E&M visits in nonacute settings by specialists per year[e]
Percentage of beneficiaries with home health use in the year
Number of home health days per beneficiary per year
Number of home health visits per beneficiary per year
Percentage of beneficiaries who used hospice in the year
Percentage of beneficiaries who used skilled nursing facility services in the year
Health outcomes
Death within 12 months of eligibility date

Notes: Measures are constructed 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, expenditure measures are not truncated, but are risk-adjusted, annualized, and weighted 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]Includes inpatient admissions and observation stays.

[c]Measured as specified in the CMMI Priority Measures for Monitoring and Evaluation.

[d]Eligible index discharges for the numerator of the readmission measure include index 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 might affect whether a beneficiary has an eligible index discharge in a particular year. Such an effect could lead to estimating biased rates of readmission for the IAH and comparison groups if readmission is defined only for beneficiaries who had an eligible index discharge as recommended by the CMMI Priority Measures for Monitoring and Evaluation. Therefore, we defined the readmission measure using all beneficiaries in the denominator, rather than limiting it to beneficiaries with an eligible discharge. For example, if home-based primary care or the demonstration reduces the likelihood of having an eligible index discharge, then IAH beneficiaries who have such a discharge might be sicker on average than comparison beneficiaries who have such a discharge. Being sicker could lead to an increased risk of readmission.

[e]Nonacute settings are defined as home (including assisted-living facilities, group homes, and custodial care facilities), offices, and outpatient clinics. Primary care clinicians are defined as primary care physicians, nurse practitioners, and physician assistants. Specialists include all physicians who are not primary care physicians.

ACSC = ambulatory care-sensitive condition; AHRQ= Agency for Healthcare Research and Quality; CMMI = Center for Medicare & Medicaid Innovation; ED = emergency department; E&M = evaluation and management; FFS = fee-for-service; IAH = Independence at Home; PQI = Prevention Quality Indicator.

CMS has separate HCC models for beneficiaries who reside in the community and beneficiaries who reside 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. Nor did we use the demographics-only model for new enrollees. Given the service use requirements for the demonstration, all IAH-eligible beneficiaries had some claims history during the previous 12 months. Using any available diagnoses information in the HCC model should have provided a score that captures 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. However, we included indicators for the number of ADLs with which the beneficiary needed 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. We did this because Medicaid enrollment data were not available promptly enough for us to define dual eligibility using Medicaid enrollment 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 three 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) Medicare service utilization, and (3) health outcomes (Table B.9). Medicare expenditures PBPM, service utilization, and health outcomes were measured 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. All claims-based outcomes were measured at the beneficiary level in that particular study year, starting with the first day of the first month after the beneficiary met all eligibility criteria. 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 get expenditures PBPM. Claims-based outcomes other than expenditures and binary measures (such as likelihood of hospice use or death) were annualized. For example, if a beneficiary had four hospital admissions and an eligibility weight of 0.5 (because he or she was eligible for the demonstration for 6 of 12 months in the demonstration year), then the annualized number of hospital admissions would be eight.

Because mortality affected the period of observation (that is, the number of eligible months during which we measured expenditures and other outcomes), we measured the probability of death during the 12 months following the eligibility date rather than during the months between eligibility and the end of the demonstration year. For example, if a beneficiary became eligible for the demonstration in Year 4 on February 1, 2016, the mortality indicator measured whether the beneficiary died between February 1, 2016 and January 31, 2017.

Hospitalizations and outpatient emergency department (ED) visits for ACSCs (potentially avoidable hospitalizations and ED visits). Hospital use for an ACSC occurs when ambulatory care might have prevented or reduced the need for a hospital admission or ED visit. We measured a beneficiary as having a hospitalization or ED visit for an ACSC if the principal diagnosis for the hospitalization or ED visit was an ACSC. We based our definition of ACSCs on the Agency for Healthcare Research and Quality 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 ACSCs excluded ED visits that led to an inpatient admission, because there was no diagnosis from the ED visit in a claim record when an ED visit led to an inpatient admission.

ED visits. Our primary measure of emergency care was total number of ED visits. However, to better understand the results of the effect of the demonstration on total ED visits, we also used two other measures of ED visits: (1) those that led to an inpatient admission and (2) outpatient ED visits (including ED 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. An ED visit that led to an admission might suggest that the beneficiary was more seriously ill than when an ED 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 might include some cases in which a hospital bills 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 an eligible index discharge. Eligible index discharges for the readmission measure included index 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 eligible index 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.

The regression for readmission included all beneficiaries; those who had an eligible index 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 a patient had an eligible index discharge and, if so, whether the patient had an unplanned readmission within 30 days.

We excluded planned readmissions from this measure. To identify planned readmissions, 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 (2015). Unlike the Yale measure, our list of procedure codes to identify planned readmissions did not include procedural codes that apply only to all-payer populations.

Physician and other clinician visits. We used physician or supplier claims to construct measures of primary care and specialist visits, and we used those measures in regressions of the effect of the demonstration on outcomes. Both measures reflected E&M visits in nonacute settings. We defined nonacute settings as home (including assisted living facilities, group homes, and custodial care facilities), office, and outpatient clinic. The two measures reflected visits billed in the physician or supplier file only, because visits in the outpatient claims file do not indicate the clinician's specialty. Therefore, the two measures excluded visits that are in the outpatient claims file, such as some therapy visits and visits in federally qualified health centers or rural health centers.[14]

- **Primary care visits: E&M visits in nonacute settings by primary care physicians, NPs, and physician assistants (PAs).** Primary care physicians include those engaged in general practice, family practice, internal medicine, geriatric medicine, and preventive medicine. Medicare claims cannot determine the specialties of NPs and PAs, so our measure of visits by primary care clinicians likely includes some visits to NPs and PAs working in fields other than primary care.
- **Specialist visits: E&M visits in nonacute settings by specialist physicians.** Specialists include all physicians who are not primary care physicians.

For our analyses in Chapter III (results reported in Table III.2 and Table B.49), we used physician or supplier claims to create the following measures of visits by IAH clinicians:

- **Mean percentage of E&M visits per beneficiary in all locations that were made in a specific type of location.** We constructed this measure in two steps. First, using E&M visits for each beneficiary in a given practice, we calculated the number of visits in each type of location divided by the total visits that the beneficiary received in all types of locations. This measure reflects visits billed by IAH primary care physicians, NPs, PAs, and other clinicians in the physician or supplier claims. Second, using the beneficiary-level percentages, we calculated the mean for each location across all beneficiaries in each practice.
- **Mean percentage of E&M visits per beneficiary by all IAH clinicians that were made by a given type of clinician.** We constructed this measure in two steps. First, using E&M visits for each beneficiary in a given practice, we calculated the number of visits by each

[14] The outpatient claims contain FFS claims submitted by institutional outpatient providers such as hospital outpatient departments, rural health clinics, outpatient rehabilitation facilities, comprehensive outpatient rehabilitation facilities, community mental health centers, and home health agencies providing therapy visits to patients not using the Medicare home health benefit. With the exception of rural health clinics and federally qualified health centers, these claims generally do not cover the professional services of physicians and others billing physician services, such as NPs; such services are billed separately as professional claims. We examined whether the IAH beneficiaries or the comparison groups received visits from rural health clinics or federally qualified health centers, and we found that they used these services rarely.

clinician type divided by the total number of visits that the beneficiary received from all IAH clinicians. We used E&M visits by IAH practices in any setting that we received from primary care physicians, NPs, PAs, and other clinicians. Second, using the beneficiary-level percentages, we calculated the mean for each clinician type across all beneficiaries in each practice. As explained earlier, this measure reflects visits billed in the physician or supplier file only, because visits in the outpatient claims file do not indicate the clinician's specialty.

VI. ESTIMATION OF DEMONSTRATION EFFECTS

We used a difference-in-differences model to estimate the effect of IAH in each demonstration year and the average annual effect for the four-year demonstration period. Our difference-in-differences estimated effect measured the difference in a given outcome between the year before the demonstration and any demonstration year for IAH beneficiaries, relative to the difference during the same period for comparison beneficiaries. We implemented the difference-in-differences model using two approaches—a frequentist model and a Bayesian model. In this section, we describe the specification and assumptions of each model, the methods we used to account for clustering, and robustness checks for testing whether our results were sensitive to different ways of estimating the effects.

A. Frequentist difference-in-differences model

1. Model specification

We estimated effects of the demonstration by comparing the regression-adjusted differences in outcomes between IAH treatment and comparison groups in the pre- and post-demonstration periods. We used a difference-in-differences estimation strategy to test for significant differential changes in all claims-based outcomes between the IAH and comparison groups during the two pre-demonstration years and the first four years of the demonstration. Equation (1) shows the model we estimated for each outcome—pooled across all practices and separately for beneficiary and practice subgroups:

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

where Y_{it} is the claims-based outcome measured for a beneficiary *i* in year *t*; α is a constant term; X_{it} is a set of beneficiary characteristics measured in the index year; PD_1 is a binary variable or 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); $DY_1 - DY_4$ are a set of indicators for each post-demonstration year; $treatment_{it}$ is an indicator variable for being in a IAH practice; and ε_{it} is a random error term. As we describe below, the set of beneficiary characteristics included in X_{it} were largely the same as the variables used for matching, and they controlled for any remaining differences between the IAH and matched comparison groups in these characteristics.

The key parameters were $\theta_1 - \theta_4$, which constituted the difference-in-differences coefficients; these were the change of outcomes from the year before the demonstration to each year after the intervention for the IAH treatment group, net of the change in outcomes 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 (that is, for the coefficient in one demonstration to differ from the coefficient in another demonstration year).

In cases in which we estimated a linear model, such as total Medicare expenditures, the difference-in-difference coefficients ($\theta_1 - \theta_4$) equaled the difference-in-differences estimated effects. In cases where we used nonlinear models, such as a logistic regression for the likelihood

of unplanned readmission, we transformed $\theta_1 - \theta_4$ into difference-in-differences estimated effects using the following steps, with the estimated effect in Year 4 as an example:

- 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 4). For example, we used the mean covariate values of the Year 4 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 estimate assumed that beneficiaries did not receive home-based primary care in that year (the comparison group estimate).
- We calculated the difference of the regression-adjusted outcome for the IAH group and matched comparison group in Year 4.
- We calculated the change in the difference between the IAH and matched comparison group in Year 4 relative to the difference in the year before the demonstration. We referred to this estimate as the difference-in-differences estimated effect.

Our difference-in-differences estimated effects 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 effect 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 both IAH and comparison beneficiaries over time.

In addition to estimating the yearly effect, we estimated a separate difference-in-differences 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 difference-in-differences estimate over the four post-demonstration years. As equation (2) shows, DY_{it} is an indicator for the demonstration period where $t = 1$ in demonstration Years 1 through 4 (and 0 otherwise). This model provided a measure of the effect of the demonstration, if any, during the entire demonstration period considered as a whole, by averaging across all the yearly observations for the demonstration years as shown in equation (2), where the average difference-in-differences estimated effect over all demonstration years is given by θ_1.

$$(2) \quad Y_{it} = \alpha + X_{it}\beta + \tau \cdot treatment_{it} + \gamma_{-1}\, PD_1 + \gamma_1\, DY_{it} + \theta_{-1} treatment_{it} \cdot PD_1 + \theta_1 treatment_{it} \cdot DY_{it} + \varepsilon_{it}$$

In addition to reporting all difference-in-differences estimates in absolute terms, we also calculated the effects in percentage terms by dividing the estimated effect for an outcome by the unadjusted IAH group mean for that same outcome in the year before the demonstration. The percentage effect helped us to interpret whether the absolute effect in a given year was likely to be meaningful.

We used linear regressions for expenditures, and we used logistic regressions for binary outcomes such as death and health service utilization. To account for over-dispersion in utilization counts, we used negative binomial regressions for the following outcomes: admissions, ED visits, home health visits, home health length of stay, visits in nonacute settings by primary care clinicians, and visits in nonacute settings by specialists. To account for both over-dispersion and the large percentage of beneficiaries with no utilization during the period, we used zero-inflated negative binomial regressions for admissions and ED visits for ACSCs.

2. Adjustment to standard errors for clustering

To obtain accurate estimates of standard errors for the estimated effects, 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. However, we could not model practice-level clustering of the comparison group, because we selected those beneficiaries without knowledge of the practice from which they receive 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 must take this time dependence of repeated observations into account.

To account for asymmetric practice-level clustering and multiple observations for some beneficiaries, we used what we referred to as a hybrid clustering approach. This approach accounted for clustering at the practice level for the IAH group only, not the comparison group, and took into account the time dependence of repeated observations for both IAH and comparison beneficiaries.[15] Implementing this approach meant that all IAH beneficiaries in a given site were from one single cluster. To correctly identify the clustering effect in the IAH group, we excluded the site fixed effects from the regression equation.[16]

[15] Accounting for clustering at the practice level for the treatment group captures the correlation among observations in each IAH practice, whether for the same individual across time periods or for different individuals 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 practice ID for the treatment group and the value of the beneficiary ID for the comparison group.

[16] Ideally, including site fixed effects would improve estimation by controlling for factors that varied across geographic locations and affected outcomes for IAH and comparison beneficiaries within a given area. However, because all IAH beneficiaries in a given site (stratum) were from a single practice (cluster), controlling for both stratification and clustering at the same level would lead to underidentification. 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 in 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 effect of the

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 served by those practices. Instead, we assumed that the IAH beneficiaries in a given practice are a random sample of all eligible beneficiaries of that practice, and thus our statistical tests accounted for the random variation among eligible beneficiaries who received care from the demonstration sites.

3. Weighting

We estimated regressions with observations at the beneficiary level, and we 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 effect regression was not disproportionally weighted toward the comparison beneficiaries.

The final regression weights varied depending on the outcome of interest. For all outcomes except mortality, we constructed the final regression weights in three steps. First, we constructed the eligibility weight. For nonbinary outcomes, the eligibility weights captured the share of months that the beneficiary was 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. That beneficiary remained in our analysis sample for the entire year unless he or she left Medicare FFS or died. For example, if a beneficiary entered the Year 4 sample on January 1, 2016, and died on June 20, 2016, that beneficiary was eligible for the demonstration for six months and thus had an eligibility weight of 0.5.

For binary outcomes other than mortality, we used a different eligibility weight than we used for nonbinary outcomes. The eligibility weight equaled 1 if the outcome occurred (for example, if the beneficiary used hospice care in the demonstration year), and it equaled the eligibility weight described in the preceding paragraph if the outcome did not occur. In other words, when the beneficiary did not experience the outcome and the binary dependent variable was truncated—that is, the beneficiary was observed for fewer than 12 months in the demonstration year—those observations received less weight in the analysis than observations observed for the full time period. The reason for giving less weight to beneficiaries whom we observed for fewer than 12 months was that they had less time to experience the outcome than did those who were observed for 12 months. In contrast, a beneficiary who was observed for fewer than 12 months but experienced the outcome was not considered to be truncated. This was because the length of

demonstration. To avoid that problem, we could not take advantage of the gains that we would have achieved by accounting for the stratified approach.

time observed did not matter in cases in which the beneficiary experienced a particular outcome. For mortality, we did not use any eligibility weight, because the eligibility weight was affected by the time of a beneficiary's death.

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 four comparison beneficiaries 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 last step, we rescaled the composite weight to ensure equality in the weighted number of IAH and comparison beneficiaries for each site and year. As described earlier, we implemented hybrid clustering adjustments but cannot use site fixed effects (an indicator for each site). Because beneficiaries had different eligibility weights, the total number of weighted IAH and comparison beneficiaries in a given site and year might differ if we used the composite weight without rescaling it. For this reason, we rescaled the weights so that, for each year, the weighted number of IAH beneficiaries equaled the weighted number of comparison group beneficiaries for each site. This ensured that the estimated treatment–comparison differences and the difference-in-differences estimates for each year account for any differential weighting of the IAH and comparison groups.[17] We specifically rescaled the composite weight for comparison group beneficiaries by a factor equal to the following ratio: the sum of IAH group weights in that site and year divided by the sum of comparison group weights in that site and year before rescaling. The rescaling factor varied by site and year and was applied only to the comparison beneficiaries' eligibility weights (Table B.10). After rescaling, the weighted number of comparison observations (or beneficiaries) in each site and year equaled the weighted number of IAH observations in that same site and year. However, the sum and the proportion of weighted observations from the IAH (or comparison) group coming from a site could vary across years. Because the eligibility weights differed for each binary outcome, for reasons mentioned previously, we calculated and applied one set of rescaling factors for all continuous outcomes and separate rescaling factors for each binary outcome (except mortality). In addition, for the dementia subgroup analysis and the sensitivity analysis with fixed geographic areas, we calculated and applied separate rescaling factors. This was because the sample sizes and the sum of weights for the IAH and comparison groups differed in that analysis for each site and year.

[17] Rescaling the weights would not have been necessary had we used site fixed effects, because site-specific effects would be differenced out by the fixed effects.

Table B.10. Rescaling factors applied to weights for comparison beneficiaries, by site and year

Site	2 years before	1 year before	Year 1	Year 2	Year 3	Year 4
Boston	1.05	1.10	1.04	1.05	1.03	1.06
Delaware	1.02	1.06	1.13	1.05	1.05	1.09
Cleveland	1.06	1.09	1.10	1.07	1.08	1.08
Durham	1.03	1.04	1.06	1.04	1.04	1.04
Brooklyn	1.10	1.08	1.08	1.08	1.11	1.11
Austin	1.04	1.05	1.05	1.04	1.06	1.08
Portland	1.13	1.03	1.00	1.04	1.03	1.05
Long Island	1.09	1.09	1.09	1.05	1.12	1.07
Dallas	1.05	1.05	1.04	1.04	1.06	1.06
Flint	1.05	1.06	1.06	1.06	1.04	1.08
Jacksonville	1.10	1.05	1.07	1.04	1.06	1.09
Lansing	1.07	1.04	1.06	1.04	1.02	1.06
Milwaukee	1.06	1.03	1.04	1.06	1.03	1.06
Washington, DC	1.06	1.06	1.07	1.06	1.02	1.06
Philadelphia	1.11	1.14	1.08	1.05	1.05	1.15
Richmond	1.14	1.07	1.11	1.03	1.13	1.12

Source: Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for IAH and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4.

Note: Rescaling factors have been rounded to two decimal places in this table.

IAH = Independence at Home.

For mortality, we used the matching weight only. We did not include an eligibility weight in the mortality regression because we assessed mortality at the 12-month follow-up for all beneficiaries. In other words, unlike all other outcomes we measured through the end of the study year, we measured mortality over a 12-month period beginning with the date of eligibility for the demonstration. The weights used for the mortality 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.

For subgroup analysis by practice types, we used a different rescaling factor than described earlier. As we describe in further detail in Section B.VI.A.6, because larger practices weighed more in the pooled regression than smaller practices, the estimated effects could have reflected changes in the relative contribution of individual sites as their number of demonstration beneficiaries grew or shrank over time. This was particularly problematic when we grouped sites by their type of ownership and estimated the demonstration effect among these three groups of sites: A single site could drive the results. Because our primary interest was the average effect in sites within each type, we had to remove the effect of changing practice sizes from our estimates. To do so, we rescaled the composite weights for all beneficiaries so that the sum of weights was equal across all sites in a year. Specifically, the rescaling ratio equaled one-sixteenth[18] of the summed weights across all sites in a year divided by the summed weights for that site in that

[18] There were 16 sites, including the three consortium practices.

year, where the summed weights included both IAH and comparison beneficiaries. After rescaling, the weighted number of beneficiaries in each site was equal to one-sixteenth of the total weighted number of beneficiaries in a given year, regardless of the actual number of beneficiaries in that site.

4. Control variables

Although our matching process ensured that the comparison groups were very similar to the IAH groups along many characteristics, there might still be important differences in some of these characteristics that 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 (Table B.11). We included all specific HCC indicators and categories of HCCs used for matching (Table B.5). Some of these control variables were on a more detailed level than the variables we used in matching; for example, we used three age categories in propensity score matching, whereas we used five age categories in the outcome regressions.

As noted earlier, we included a dummy variable for each year and an indicator of whether the beneficiary was in the IAH or comparison group. Given 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 4, we used the Year 4 values of all control variables, whether or not a beneficiary appeared in the sample in an earlier demonstration year.

Table B.11. Control variables used in regressions

Variable
Eligibility for the demonstration
Number of months since most recent inpatient admission: one, two to three, four or more
Month of the demonstration year that beneficiary met eligibility criteria (1, 2–6, 7–12)[a]
Demographic characteristics
Age: younger than 65, 65–74, 75–79, 80–84, 85 or older
Gender
Race and ethnicity: white, black or African American, Hispanic, Asian, American Indian/Alaskan 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: two, three or four, five or six
Whether information about the feeding ADL was missing[b]
Health status
HCC risk score
Specific HCCs
HCC8: Metastatic cancer and acute leukemia
HCC9–10: Lung and other severe cancers; lymphoma and other cancers
HCC11–12: Colorectal, bladder, and other cancers; breast, prostate, and other cancers and tumors
HCC18: Diabetes with chronic complications
HCC21: Protein-calorie malnutrition
HCC27: End-stage liver disease
HCC28–29: Cirrhosis of liver; chronic hepatitis

Table B.11 (continued)

Variable
HCC46: Severe hematological disorders
HCC48: Coagulation defects and other specified hematological disorders
HCC51: Dementia with complications
HCC52: Dementia without complications
HCC54–55: Drug/alcohol psychosis; drug/alcohol dependence
HCC57–58: Schizophrenia; major depressive, bipolar, and paranoid disorders
HCC70–71: Quadriplegia; paraplegia
HCC72: Spinal cord disorders/injuries
HCC85: Congestive heart failure
HCC96: Specified heart arrhythmias
HCC103–104: Hemiplegia/hemiparesis; monoplegia, other paralytic syndromes
HCC106: Atherosclerosis of the extremities with ulceration or gangrene
HCC107–108: Vascular disease with complications; vascular disease
HCC111: Chronic obstructive pulmonary disease
HCC134: Dialysis status
HCC136–138: Chronic kidney disease, stages 3–5
HCC139–140: Chronic kidney disease, stages 1–2 or unspecified; unspecified renal failure
HCC157–159: Pressure ulcer of skin with necrosis through to muscle, tendon, or bone; or with full or partial thickness skin loss
Chronic conditions measured by Chronic Conditions Data Warehouse
Alzheimer's or dementia
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[c]
Fluid and electrolyte disorders[c]
Chronically critically ill or medically complex diagnosis

Notes: This table lists HCCs used in all regressions.

[a]For all sites in Year 4, Month 1 is October. In Years 1–3, 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 is September.

[b]Feeding assessments were not available on 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, we used the feeding values from that assessment; however, sometimes there was no previous discharge assessment.

[c]Measured using claims from the most recent inpatient stay and observation stay in the year before the demonstration eligibility date. Diagnosis codes for these conditions were drawn from Gagne et al. (2011).

ADLs = activities of daily living; ESRD = end-stage renal disease; HCC = hierarchical condition category.

5. Testing for the validity of the difference-in-differences estimates

The validity of the difference-in-differences estimates for the demonstration years relied on the classic difference-in-differences assumption that there was no significant differential trend between the IAH and matched comparison groups during the pre-demonstration period. Therefore, the difference-in-differences estimate for two years before the demonstration, θ_{-1}, served two purposes: (1) It ruled out or identified significant 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} indicated 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. This meant that 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 estimated effects 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 effect of the demonstration payment incentive. On the other hand, it would also be possible that random fluctuation in the yearly difference between the two groups caused a significant difference in the pre-demonstration period, not a pre-existing trend.

We examined the difference-in-differences estimate for two years before the demonstration for all outcomes reported. The estimate was not statistically significant for most outcomes, including expenditures and hospital care use, suggesting that the parallel-trend assumption held for those outcomes. For the few outcomes whose difference-in-differences estimate for two years before the demonstration was statistically significant (and hence violating the parallel-trend assumption), we discuss implications for the results in Chapter II. Furthermore, as described next, we implemented a sensitivity analysis for all outcomes, using both pre-demonstration years as the baseline period, to examine whether the estimated effects were sensitive to the choice of baseline year. Nonetheless, because it was impossible to rule out the possibility of truly nonparallel pre-existing trends for outcomes where the difference-in-differences estimate for two years before the demonstration was significant, we were cautious when we interpreted the estimated effects for those outcomes.[19]

6. Sensitivity analyses

In addition to estimating our main regressions using the modeling approach described above, we implemented four sensitivity analyses to test whether our results were sensitive to the choice of baseline year, outliers, practices' expansion into new service areas, and changes in practice sizes. We present full results from these checks in Section X of this appendix. Chapter II includes a discussion of these results when they had affected our interpretation of the demonstration effects. When the results of a particular test had not affected our interpretation, they are not part of our discussion in Chapter II.

Sensitivity test regarding choice of baseline year. First, because random variation in outcomes could affect the difference between the IAH and comparison groups in the baseline year, we re-estimated the model for all Medicare claims–based outcomes by using both pre-demonstration years combined as the baseline period rather than using only the year before the demonstration. Under this model, the difference-in-differences estimate for any demonstration year represented the difference between the IAH and comparison groups in that demonstration year relative to the difference between the two groups during the two years before the demonstration.

[19] One could control for pre-existing trends by including linear time trends in the regression. However, this would impose an overly restrictive assumption on our model—that the one-year pre-demonstration trends continue throughout the demonstration.

Sensitivity test regarding outliers. Second, even though we recognize that the demonstration intended to save costs across the spectrum of eligible beneficiaries, including those who incur especially high Medicare expenditures, we tested whether outliers influenced our results. Specifically, in each year, we reset the outcome measures for total expenditures and acute care use of IAH beneficiaries above the 99th percentile to the 99th percentile. We did the same thing for comparison group members using the 99th percentile of the comparison group's distribution in a given year. This enabled us to understand whether the overall results were sensitive to including these highest-use patients.

Sensitivity test regarding changes in service area. Third, during the demonstration period, some IAH practices might have strategically expanded the geographic areas in which they offered home-based primary care, which could affect the effect estimation through local area variation in practice patterns or changes in patient mix.[20] To examine the influence of geographic changes of services on the estimated effects, we conducted a sensitivity analysis by restricting the IAH beneficiaries to those who lived in the set of counties we observed in the year before the demonstration, which we referred to as the baseline counties. This sensitivity analysis enabled us to remove any effect of changes in the geographic composition of beneficiaries from the estimated effects. Specifically, for each site, we identified IAH beneficiaries in any year who were not from one of the baseline counties. We did this for each of the four demonstration years and two years before the demonstration (that is, the year before the baseline year). We excluded these IAH beneficiaries and their corresponding comparisons regardless of whether the comparison beneficiaries were in the baseline counties. Next, we used the geographically consistent subsample of IAH beneficiaries to estimate the effect of the demonstration on key outcomes.

Removing IAH beneficiaries who did not live in the baseline counties and their matched comparisons from the sample could lead to a sample in which the IAH and comparison beneficiaries have notable differences in some variables, such as HCC score or race. Therefore, before estimating the effect of the demonstration using the subgroup of IAH beneficiaries who lived in the baseline counties, we needed to determine whether the IAH and comparison beneficiaries in a particular site in a particular year were sufficiently similar, or balanced. We assessed the balance of the subgroup for all sites pooled, and we assessed balance for a given site and a given year if we had excluded 5 percent or more of the IAH beneficiaries from that site in that year. To assess balance, we calculated the standardized difference for each matching variable. When pooling all sites, the subgroup balance in each year was as good as the balance on the full sample. Similar to the full sample of all sites pooled, there were no matching variables for which the standardized difference exceeded 0.10 when comparing the pooled subgroup of IAH beneficiaries in the baseline counties and their matched comparisons.

We also checked the following sites and years, because the proportion of patients from new geographic areas exceeded these sites by 5 percent or more: Durham in all four demonstration years; Jacksonville, Long Island, Portland, and Washington, DC, in Year 4; Philadelphia in Year

[20] For example, a practice might have expanded services in new, more profitable geographic areas, where the new IAH patients were healthier or sicker than the existing patient population or had different attitudes toward behavior changes and treatment recommendations. If these differences were also correlated with outcomes and could not be captured, they might have biased our estimated effects.

3; and Richmond in the two years before the demonstration. In many cases, the subgroup balance was about as good as it was for the full sample. For example, across the four years we checked balance for the Durham subgroup in baseline counties, the standardized difference exceeded 0.10 for only one matching variable in one year, and the standardized difference for that variable was less than 0.15. This was similar to the results for the full Durham sample in the four demonstration years, in which there were no matching variables for which the standardized difference in the full sample exceeded 0.10.

In the sites with fewer IAH beneficiaries, balance in the subgroup sample was not quite as good as in the full sample. For example, in Philadelphia the full sample in Year 3 had zero variables whose standardized difference exceeded 0.15 and four whose standardized difference exceeded 0.10. In the subgroup defined by Year 3 IAH beneficiaries living in the baseline counties, two variables had standardized differences that exceeded 0.15 (but were less than 0.25), and three other variables had standardized difference that exceeded 0.10 (but were less than 0.15). We expected to find slightly poorer balance at the site level in the baseline county subgroup for small sites, because the calculation of standardized difference is sensitive to the number of beneficiaries in the sample. However, because we used the pooled sample for our main effect analyses, our primary goal was achieving good balance for the pooled sample of IAH beneficiaries in baseline counties. As explained previously, we achieved that goal.

Sensitivity test regarding practice size. In each year, some IAH practices had many more IAH-eligible beneficiaries than other practices. In addition, over the four demonstration years, some practices grew substantially while others shrank. For example, the number of IAH beneficiaries in Durham increased from 713 in the year before the demonstration to 1,705 in Year 4, becoming the largest among all sites. On the other hand, Austin was the largest site in the baseline, but its number of beneficiaries declined by half since the year before the demonstration (from 1,349 to 686 beneficiaries). Under a repeated cross-sectional design, our difference-in-differences estimates ascribed changes in outcomes due to changes within practices and due to changes in relative practice sizes to the effect of the demonstration. If a change in the relative size of the practice was related to the demonstration, our estimated effects correctly captured that change. (For example, a practice that earned shared savings in the first two years could have invested the shared savings into hiring new clinicians and therefore grew substantially in the next two years.) However, a limitation of this approach was that the estimated effects could also reflect any differences in practice sizes or changes in practice sizes unrelated to the demonstration.

To address this limitation, we removed the effect of differences in practice sizes and changes in practice sizes on the estimated effect by imposing equal weights across all sites through all years. We estimated a so-called equal-weight regression model, in which the summed weights of beneficiaries were equal across all 16 sites in each year (treating the three members of the Mid-Atlantic Consortium as separate sites). To implement this model, we rescaled the regression weights for every beneficiary by a ratio that varied by site and year. Details on the weight construction are in Section B.VI.A.3. We then estimated the effect regressions on key outcomes using the rescaled weights.

7. Beneficiary and practice subgroups

As we discussed in Chapter IV of this appendix, we examined subgroups of beneficiaries and IAH practices to understand whether the demonstration payment incentive had greater effects on certain groups than on others. We separately estimated the demonstration's effects on key outcomes (Medicare expenditures and use of hospital care) for beneficiaries with dementia versus those without dementia (see identification of subgroups in section IV.A). We then compared the estimation results between the two groups, by conducting tests for statistically significant difference in the estimated effect in each year between the two subgroups.

We also performed separate analyses for each of the three types of practices (VPA, academic medical centers, and independent practices) to assess the extent to which the demonstration payment incentive had different effects on beneficiaries at different types of practices. Again, we estimated the demonstration's effects on Medicare expenditures and use of acute care services by practice type. For each estimation, we imposed equal weights across all practices through all pre-demonstration and demonstration years. For example, we rescaled the regression weights so that the summed weights of beneficiaries were equal across all five VPA sites (Dallas, Flint, Jacksonville, Lansing, and Milwaukee) in each year. This method was different from the pooled analysis and was analogous to the practice size sensitivity test we described in the last section. We used the equal-weight model for the practice type subgroups because the focus of this analysis was comparing the demonstration's effects by practice type, and therefore we wanted to remove the effect of differences and changes in practices' sizes from the estimated effect of the demonstration payment incentive.

B. Bayesian difference-in-differences models

1. Overview

In addition to the frequentist (traditional) analyses we describe in Section VI.A, 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 while providing a more flexible interpretation of the program's effects. The frequentist approach classifies the demonstration's effect as either statistically significant or not statistically significant; in contrast, a Bayesian analysis allows more granular inference. For example, one could conclude that "there was an 84 percent chance that the IAH demonstration payment incentive produced savings of at least $50 PBPM in demonstration Year 4." Such conclusions offer the opportunity to tailor inference to substantive questions of interest and to apply subject-matter expertise in deeming effects meaningful.

Overall, the Bayesian and frequentist analyses were analogous. 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 same control variables. However, the Bayesian analysis diverged from the frequentist analysis in three ways, as we describe here. In this section, 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 thus allowed for probabilistic inference. We placed a standard normal prior distribution—denoted $N(0,1)$—on the overall effect of IAH. By doing so, we incorporated a prior expectation that very large positive or negative effects of IAH on expenditures were substantially less likely than small and moderate effects. We based our prior expectation on the general result that other interventions of the effect of home-based primary care and other interventions for chronically ill, frail beneficiaries 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 using random effects, whereas the frequentist analysis used cluster-robust standard errors (as described earlier, in Section VI of this appendix). Specifically, the two-stage full Bayesian model accounted for clustering using beneficiary- and site-specific random effects for both the IAH and comparison groups, where 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 by 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.[21] This methodological difference in accounting for clustering could lead to differences in both point estimate and standard error of the estimate. We report results from the full Bayesian model in the Executive Summary and Chapter II, where we refer to it simply as the Bayesian model.

We estimated an empirical Bayesian model to help explain any differences in results between the frequentist and full Bayesian models. As in the full Bayesian model, the empirical Bayesian model used random effects to account for clustering. However, unlike the full Bayesian model, the empirical Bayesian did not include priors. Thus, the empirical Bayesian model enabled us to isolate the source of any differences between the frequentist and full Bayesian estimates as the use of priors, the random effects approach to clustering, or both. Section VI.B.2 discusses the details of the empirical Bayesian model. We report results from the empirical Bayesian model in this appendix only.

c. Two-stage model

We further modified the frequentist model to make Bayesian computationally feasible. We adopted these modifications purely as a computational convenience and they are not inherently Bayesian; a traditional effect 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 (see equation [3] below), but such a model would have taken more than a month to converge on the GovCloud platform offered by Amazon Web Services. Given time constraints,

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

we used a *two-stage approximation* of this ideal beneficiary-level model. In the first stage, we aggregated the beneficiary-level dataset to the site level. Using output from Stage 1, we estimated the effect of the IAH demonstration using a Bayesian difference-in-differences framework in Stage 2. We tested the validity of the approximation by comparing estimates from a beneficiary-level empirical Bayesian model to results from our two-stage Bayesian approach. Results from the two models closely resembled each other, suggesting that our two-stage approach was a reliable approximation of the full Bayesian model. Section VI.B.2 discusses the details of the two-stage full Bayesian model.

2. Full Bayesian model

To understand the full Bayesian model, we begin by presenting a single, unified model at the beneficiary level. As we show in equation (3), this procedure accomplishes effect estimation and risk adjustment simultaneously through a model of the following form:

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

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

- We use i to index beneficiaries; $j = 1, \ldots, 16$ to index geographic areas (or loosely speaking, sites that both IAH and comparison beneficiaries reside in); and $t = -1, \ldots, 4$ to index years.
- Y_{ijt} is the 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; and 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 θs are the difference-in-differences effects of interest. As with the frequentist model, we estimated γ_{-1} and θ_{-1} for two years before the demonstration and $\gamma_1 - \gamma_4$ and $\theta_1 - \theta_4$ for each of the four demonstration years. Note that $t = 0$ corresponds to the baseline year (the year before the demonstration), so γ_0 and θ_0 are both omitted from the model.
- Random effects are denoted by roman letters: The a's and b's are random intercepts at the beneficiary and site level, 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, whereas 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 final regression weights that we used in the frequentist analysis, by multiplying the eligibility weight by the matching weight and ensuring equality in the weighted number of IAH and comparison beneficiaries for each site and year, as discussed in Section VI.A.3.

We estimated the adjusted total Medicare expenditures for the IAH and matched comparison groups in each year, the difference-in-differences estimates (θ_{-1}, θ_1-θ_4), and percentage effect relative to unadjusted IAH group mean expenditures in the year before the demonstration. In addition, we estimated the probability of reducing expenditures by at least $50, at least $100, and at least $200 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 (fourth) demonstration year, which is the same approach we used in the frequentist analysis.

Because of the number of observations in the dataset, fitting equation (3) as a single, unified model at the beneficiary level was computationally prohibitive. For that reason, we fit the full Bayesian model using a two-stage approximation to decrease computational run times. We fit the first-stage model at the beneficiary-year–level using hierarchical linear regression. The goals of the first-stage analysis were the following: to aggregate beneficiaries to the site level and to risk-adjust outcomes to enable comparisons across sites and years whose case mix differed (equation [4]). In the first-stage model, we adjusted for the same beneficiary-level covariates as in the frequentist model (see Table B.11). The risk-adjusted site-year–level output from Stage 1 was used as data in Stage 2, which estimated the effect of the IAH demonstration in a Bayesian difference-in-differences framework (equation [5]).

(4) Stage 1: $Y_{ijt} = A_{jz_{it}t} + X_{it}\beta + a_i + \varepsilon_{it}$

As just 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 192 such fixed effects from two groups (IAH and comparison) from each of the 16 sites in each year. The parameters β describe the effects of beneficiary-level control variables X_{it}, while 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_{jzt}) from the Stage 1 model when we estimated the Stage 2 full Bayesian difference-in-differences regression (equation [5]).

(5) 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_j, as described earlier. The parameters of interest, θ_t, represent the overall difference-in-differences terms. To estimate the overall estimated effect of all four post-demonstration years, we re-estimated the Stage 2 model with one post-demonstration dummy instead of separate dummies for each demonstration year.

We assigned a standard normal distribution—Normal(0, 1)—as the prior for each model parameter: $\alpha \sim N(0,1), \tau \sim N(0,1), \gamma \sim N(0,1), \theta \sim N(0,1)$, $\left(b_j, c_j\right) \sim MVN\left(0, \Sigma\right)$, and $d \sim N(0, \sigma^2)$ 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[22] correlation prior (Lewandowski, Kurowicka, and Joe 2009), and the latter took on a standard normal distribution. The multiplication of these two parts constituted the prior on $\Sigma : \Sigma = \begin{pmatrix} \sigma_c & 0 \\ 0 & \sigma_d \end{pmatrix} \Omega \begin{pmatrix} \sigma_c & 0 \\ 0 & \sigma_d \end{pmatrix}$ where $\sigma_c, \sigma_d \sim N(0,1)$ and $\Omega \sim \text{LKJ}(2)$. Last, our prior on the error term is given by $\varepsilon_{jzt} \sim Normal\left(0, s^2_{jzt}\right)$. Therefore, both σ^2 and s^2_{jzt} 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.

3. Empirical Bayesian model

We estimated an empirical Bayesian model—also known as the error components model, hierarchical linear model, or random effects model—to help explain any differences in results between the frequentist and full Bayesian models. The empirical Bayesian model was identical to the full Bayesian model except that it assumed flat, uniform priors for the model's main parameters, including the demonstration effect, the covariate effects, and the variance components (Gelman et al. 2004). We used the "lme4" package in R to fit our empirical Bayesian model.

The estimated effects from the frequentist, empirical Bayesian, and full Bayesian models were largely consistent in directions and statistical significance, but the sizes of empirical Bayesian estimated effects were closer to full Bayesian estimates than to the frequentist estimates (Appendix B, Tables B.16–19). This suggested that the differences between the frequentist and Bayesian estimates were mainly due to the different way in which each model accounted for correlations of beneficiaries within practices and over time.[23] As described earlier, we used hybrid cluster-robust standard errors for the frequentist model and random effects for the full and empirical Bayesian model. (For more information on the frequentist model, see Section VI.A.) Nevertheless, our interpretation of the effect of the demonstration on expenditures was the same regardless of the estimation model—empirical Bayesian, full Bayesian, or frequentist.

[22] The LKJ distribution is a distribution on correlation matrices (usually called Ω). The distribution has one parameter, ν, so $\Omega \sim \text{LKJ}(\nu)$. When $\nu = 1$, the distribution is uniform over all possible correlation matrices. As ν increases, the distribution is more concentrated on the identity matrix, which corresponds to zero correlations. So, for $\nu = 2$, the distribution slightly favors less correlation, shrinking the correlations somewhat toward zero. This is a weakly informative prior to help stabilize the estimation.

[23] The site-level random intercepts included in the Bayesian model could drive the differences in point estimates between the frequentist and Bayesian models. By controlling for these random effects, the Bayesian model estimated the effect as an average of site-specific effects. On the other hand, the frequentist model estimated the pooled effect by lumping together data from all sites in all years. The frequentist estimate thus reflected both changes within practices and changes in relative practice sizes.

VII. QUALITATIVE METHODS AND DATA

To understand why and how the incentive payments might (or might not) have affected outcomes, we needed to understand how IAH practices' provision of home-based primary care changed after the IAH demonstration began, when the incentives were in place. Identifying the potential effect of changes by the IAH practices 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 without harming patients. Here, we present information about two sources of qualitative data that we used in this report: data gathered from demonstration sites and data gathered from partners that work with demonstration sites.

A. Data gathered from demonstration sites

In Chapter III, we report qualitative data gathered from demonstration sites during demonstration Years 1 through 3 and Year 5.

- We conducted the most recent interviews in April 2017. During these interviews, we interviewed 25 clinical and administrative staff at 15 IAH practices and the VPA corporate office in Troy, Michigan. In these interviews, 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 reaction 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 rounds of 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 also collected information on barriers to and facilitators of meeting the requirements of the demonstration and 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, 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 individuals involved in implementing the model; and (5) processes used to implement the model. We used ATLAS.ti software 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.

Our analysis of qualitative data entailed a description of 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 conduct site visits until after the demonstration began, data on practices' operations and structure before the demonstration was limited to information that interviewees told us was different in Year 1 relative to before the demonstration.

B. Data gathered from demonstration sites' partners

In Chapter III, we examined how care partner organizations perceive IAH practices on three key dimensions of home-based primary care: care coordination, accessibility, and continuity, as defined by the World Health Organization and the Agency for Healthcare Research and Quality. Care partners are organizations external to the IAH practice's care team with which the practice has an established working relationship to coordinate care for patients. We also examined care partners' perceptions of their collaboration with IAH home-based primary care practices compared with their collaboration with office-based primary care practices and the changes that occurred during the IAH demonstration. To recruit interview respondents for this evaluation, we requested that each IAH practice identify and provide contact information for up to seven care partners that played a role in their home-based primary care delivery model. Care partners could include home health agencies, hospices, specialists, durable medical equipment suppliers, pharmacists, social workers, and social service organizations. At CMS's request, we asked practices to include at least one home health agency in their list of care partners. We interviewed a maximum of five care partners for each practice, selecting a range of partner types from the lists provided by practices. We then contacted the designated person for each care partner to invite them to participate in a 30-minute interview to discuss their work with the IAH practice. Topics for the interview included experiences with communicating and sharing information, coordinating care, accessibility, and continuity. We also asked respondents to compare these experiences to their work with office-based practices and to identify any changes in their experiences with IAH practices since the demonstration began in 2012. We collected data from care partners identified by the IAH practices as important partners in providing care to patients, and the views expressed by these care partners might not represent the views of all care partners working with IAH practices.

We interviewed 48 care partners across all participating IAH practices except Brooklyn (Table B.12); we did not receive a care partner list from Brooklyn. Regarding VPA practices, we also interviewed the VPA corporate vice president of case management and leaders of Grace Hospice and Pinnacle Senior Care—two organizations that work closely with local VPA practices and, like VPA, are owned by U.S. Medical Management, which has headquarters in Troy, Michigan. We included these interviews in the counts of care partner type and listed them as VPA Corporate in practice counts in Table B.12. Following standard qualitative methods (Miles et al. 2013; Bradley et al. 2007), all 48 interviews conducted in 2016 and 2017 were recorded and professionally transcribed, then reviewed by research staff for accuracy and quality. We identified the main research themes of interest to develop a coding scheme, including code names and definitions; we used ATLAS.ti, a software tool used to manage and analyze qualitative information, to apply these codes to the transcripts. After coding, we used an inductive analysis approach, identifying themes and reviewing outliers for relevance and alternative explanations of findings, and then reported our results.

Table B.12. Number of respondents, by care partner type and IAH practice

	Respondents
Total	**48**
Care partner type	
Home health	21
Hospice	7
Specialist	5
Assisted-living facility/adult foster care	3
Pharmacist	2
DME/oxygen	6
Other[a]	4
Practice	
Austin	1
Boston	3
Brooklyn[b]	0
Cleveland	3
Dallas	3
Durham	3
Flint[c]	3
Jacksonville	2
Lansing	1
Long Island	4
Milwaukee	2
Philadelphia	2
Portland	4
Richmond	5
VPA Corporate (Troy)	3
Washington, DC	5
Wilmington	4

Source: Care partner interviews, November 2016–January 2017.

[a]Includes staff from an imaging center, emergency medical services program, Area Agency on Aging program, and a Medicaid waiver program.

[b]Brooklyn did not submit a list of care partners, despite multiple attempts to contact the practice lead.

[c]All three of the Flint care partners reported information relevant to both the Flint and Lansing practices, as well as differences between the two practices.

DME = durable medical equipment; IAH = Independence at Home.

VIII. IAH SURVEY DATA AND METHODS

To answer research questions about beneficiaries' and caregivers' experiences, we conducted two interconnected, self-administered surveys distributed by mail: (1) a survey of beneficiaries currently or previously enrolled in the IAH demonstration (or their proxies), excluding beneficiaries who were deceased or who had moved from the area, and (2) a survey of the beneficiaries' caregivers.

Together, the surveys measured the following domains: experiences of care, satisfaction, clinician attributes, self-reported health outcomes, beneficiary characteristics, and caregiver attributes. After identifying the key domains and subdomains, we reviewed existing questionnaires to identify survey items measuring the domains of interest. We prioritized survey items from the Medicare Current Beneficiary Survey, because the questions were designed for Medicare beneficiaries. If that survey lacked adequate coverage for a particular domain or subdomain, we adapted items from the questionnaires used in the Consumer Assessment of Healthcare Providers and Systems and the Medicare Care Management Performance Demonstration evaluation (Dale et al. 2012), which Mathematica designed and collected for CMS.

For questions specific to the caregiver survey, we adopted or adapted items from the questionnaires used in (1) Mathematica's evaluation of the Cash and Counseling demonstration (Brown et al. 2007) and (2) a study of unmet needs of adults with disabilities conducted by the Massachusetts Department of Public Health and the Center for Survey Research at the University of Massachusetts–Boston (2008). We overlapped the content covered by the beneficiary and caregiver questions, because they provided different perspectives on experience in the IAH program. In some cases, we developed our own questions specific to the evaluation, such as whether beneficiaries would contact their clinicians if they did not feel well and were unsure whether they should go to the ED. In this report, we present data on a number of measures of beneficiary and caregiver experience.

We created a 15-minute questionnaire for beneficiaries and a 10-minute questionnaire for caregivers. After developing these surveys, we conducted a small, iterative, two-phase pretest to evaluate whether respondents had difficulty completing the questionnaires. We translated all survey materials into Spanish and made the translated materials available to beneficiaries and their caregivers upon request.

The original size of the beneficiary sample was 7,293 beneficiaries whose claims-based enrollment date, as reported by the design and implementation contractor, occurred from June 1, 2012, to June 30, 2014. The design and implementation contractor determined the claims-based enrollment date as the date on which the beneficiary first met the Medicare enrollment, hospitalization, and rehabilitation services criteria for the demonstration. All of these beneficiaries were eligible and enrolled according to the IAH practice. Beneficiaries who died before the survey was fielded did not receive a survey, though they were used to estimate the number of dead people among the unlocated or nonrespondent sample members.

We conducted the survey in seven cohorts. Cohort 1 included beneficiaries whose claims-based enrollment date was June 1, 2012, to December 31, 2012. Cohort 2 included beneficiaries whose claims-based enrollment date was January 1, 2013, to March 31, 2013. Similarly, all other cohorts consisted of beneficiaries enrolled in a three-month period ending with Cohort 7, which included beneficiaries whose claims-based enrollment date was April 1, 2014, to June 30, 2014.

The initial mailing included a cover letter, the beneficiary and caregiver surveys, a survey fact sheet, and two postage-paid return envelopes. If the respondent did not return one or both surveys, we attempted a series of subsequent contacts, including three postcards and two additional survey packets. All beneficiaries who did not respond to the mailings were selected for telephone follow-up. Interviewers attempted to complete the survey on the telephone with the beneficiary. We mailed an additional paper survey to the caregiver of each beneficiary who completed the survey by telephone.

The survey data presented in this report reflect responses from 3,870 beneficiaries (a response rate of 63.3 percent) and 2,519 caregivers (a response rate of 55.7 percent), and we present responses weighted for nonresponse.[24] We calculated the response rate after excluding beneficiaries who could not respond, such as those who had a language barrier or who passed away before or during data collection. We calculated separate nonresponse weights for beneficiaries and caregivers. For each group, we calculated the weights in three stages: (1) eligibility determination, (2) location adjustment, and (3) cooperation adjustment. We used the eligibility model to estimate the probability of survival among unlocated cases. We calculated the location and cooperation adjustments using weighting classes based upon propensity scores from logistic models. The location adjustment accounts for people who were sampled but could not be located during data collection. The weights of these people were reallocated to similar sample members who were located (both respondents and nonrespondents). Their eligibility was assumed to be unknown.

[24] The response rate was calculated using the fourth response rate formula from the standard definitions of the American Association for Public Opinion Research (2016). In this formula, the numerator is the number of completed interviews and the denominator is the number of completed interviews plus the estimated number of eligible nonrespondents, assuming all nonrespondents have unknown eligibility. The latter figure is calculated as the product of the number of nonrespondents and the estimated eligibility rate. The eligibility rate is estimated by taking the number of eligible sample members (in this case, completed interviews) divided by the sum of eligible and ineligible sample members. Ineligible sample members included beneficiaries or caregivers who had a language barrier or who passed away before or during data collection.

IX. LIMITATIONS

As with all analyses, our study of changes in how IAH practices provided care, patient and caregiver survey data, and the effect of the demonstration on outcomes such as Medicare expenditures and utilization has some limitations.

Generalizability of the practices. This examination was not designed to draw conclusions about how the IAH demonstration might affect outcomes for Medicare FFS beneficiaries who receive home-based primary care from practices other than those in the demonstration. 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. 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, 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.

In addition to the small number of demonstration sites, only a portion of any IAH site's patients qualified for the demonstration and, in many cases, the number who qualified was very small. These small numbers of observations made it difficult to compare demonstration sites and obtain robust information about what works for an individual site or across groups of sites. We did not have the ability (that is, statistical power) to identify small effects of the demonstration payment incentive across all demonstration sites. This limitation was more pronounced when we examined issues that affect the subgroups of the population, such as beneficiaries with dementia.

Generalizability of the patient population to the target population. Congress identified a target population for the demonstration by establishing eligibility criteria in the IAH legislation. However, the criteria could be interpreted in different ways, and the IAH practices varied in how they interpreted and implemented the eligibility criteria. Also, the approaches of Mathematica and the implementation contractor to identifying eligible beneficiaries yielded different counts of IAH practices' beneficiaries.

For the sake of scientific validity, our sample differed from the population of IAH enrollees in two ways. First, we excluded beneficiaries who were enrolled but not confirmed eligible in the administrative data we used for the evaluation. If a beneficiary was eligible for and enrolled in the demonstration in one year and continued to be enrolled in the demonstration the next year, that beneficiary was in our sample in the next year only if he or she met all of the demonstration eligibility criteria again. This meant that we excluded beneficiaries who avoided recent hospital stays or use of rehabilitation services (two of the demonstration eligibility criteria). The value of the demonstration for beneficiaries with chronic conditions who avoided recent hospital stays or use of rehabilitation services is not known and might differ from what we measured in the study. Second, we included in the evaluation beneficiaries who received care from the demonstration clinicians and were eligible for the demonstration *based on administrative data* but who were not enrolled in the demonstration. The fact that sites did not enroll all the eligible beneficiaries who we identified in administrative data underscores the difficulties the demonstration faced in applying the eligibility criteria consistently.

Limitations regarding changes over time. There may have been unmeasured differences in how IAH and comparison beneficiaries changed over time. For example, in setting the beneficiary requirements for the IAH demonstration, Congress used four key health status and health care utilization factors to define eligibility: (1) two or more chronic conditions, (2) needing human assistance with two or more ADLs, (3) recent hospitalization, and (4) recent use of rehabilitation services. The last three of these measures can identify a patient who was temporarily acutely ill and disabled or a patient who was chronically ill and permanently disabled. Because we could not distinguish between these two underlying situations, we did not know whether the proportion of these types of beneficiaries had changed differently over time, which could have affected the measurement of the demonstration effect. More broadly, if the patient mix in the IAH and comparison groups changed over time in ways that we could not observe, and the change was not due to the demonstration payment incentive, the results could be inaccurate.

Another factor that may have caused unmeasured changes in the IAH and comparison groups over time was the participation of several IAH practices in ACOs in Year 4. ACO patients treated by the IAH sites might have been healthier (or sicker) on average than other non-ACO IAH beneficiaries. CMS has rules for attributing beneficiaries to IAH or to an ACO for the purpose of calculating demonstration incentive payments. However, given concerns about accurately separating ACO and non-ACO IAH beneficiaries, we did not attempt to control for ACO status in the regression. If ACO and non-ACO IAH beneficiaries had differences in health status that affected Medicare expenditures but which we could not measure in administrative data, and if the comparison group did not experience a similar change in health status, then participation in ACOs would cause bias in our effect results in Year 4 and the average annual effect of the demonstration. However, we have no strong evidence about whether such participation may have led to higher or lower expenditure reductions in Year 4 than would have occurred without participation in ACOs.

Possibility of differential outcome trends from the baseline. The validity of our estimated effects assumes that the outcomes of IAH and matched comparison groups followed the same trend before the demonstration. That is, we assumed that outcomes changed 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 the two-year pre-demonstration period. We examined this assumption by testing whether the outcomes changed differentially in the pre-demonstration period. Most outcomes, including expenditures and hospital care use, did not have pre-demonstration differences in trends, but a few outcomes did.[25] In other words, for a few outcomes, the change for the IAH beneficiaries from two years before the demonstration to one year before the demonstration was statistically significantly different from the change for comparison beneficiaries over those two pre-demonstration years. It was possible that the differential trends might have contributed to the post-demonstration differences in these outcomes, masking the true effect of the demonstration (if any). For example, if the use of hospice increased at a faster rate for IAH beneficiaries than for comparison beneficiaries during

[25] Outcomes that had differences in pre-demonstration trends included mortality, probability of hospice use, probability of home health use, and number of visits to primary care providers.

the two years before the demonstration, then our estimated effects could capture a widening gap between the two groups even though that gap may not have been a result of the demonstration.

Data constraints. Our evaluation of the effect of the demonstration had the following limitations related to data:

- Our patient and caregiver survey did not collect information from a comparison group, nor did it collect information from IAH patients and caregivers before the demonstration began. Therefore, we could not assess whether the demonstration payment incentive improved patient or caregiver satisfaction or increased patients' and caregivers' willingness to contact the IAH practice when the patient is unsure if he or she needs emergency care. As result, we interpreted the survey data in a descriptive fashion only.
- Our measures of primary and specialty care visits came from physician or supplier claims, which enabled us to identify the type of clinician. We excluded care from institutional settings, which includes care from rural health clinics and other institutional providers, because we could not identify, from the claims, the type of clinician who provided care. Fewer than 3 percent of the IAH beneficiaries and 7 percent of the comparison beneficiaries used rural health clinics or federally qualified health centers. Further, many of the comparison beneficiaries who used rural health clinics or federally qualified health centers also had claims for visits from primary care clinicians in the physician or supplier claims, which we included in our measure of primary care visits. Thus, it is unlikely that including visits to rural and federally qualified health centers would have had a substantive effect on the measure of the demonstration's effect on visits by primary care clinicians.
- We did not report the effect of the demonstration on entry into institutional long-term care. Because beneficiaries who meet the IAH eligibility requirements have multiple chronic conditions and require assistance with ADLs, they are at higher risk of entry into institutional long-term care than Medicare beneficiaries who do not meet eligibility requirements. Entry into long-term care is an outcome that matters to patients and their caregivers, because most people prefer to live at home as long as possible. It is also important because Medicaid pays for institutional long-term care for dually eligible beneficiaries. Measuring long-term care is challenging for a number of reasons. From the data available, it is often unclear whether entry into a facility for care is a temporary or permanent move. Also, because institutional long-term care is paid for in a number of ways, no single claims-based data set captures entry into institutional long-term care. We continue to explore the best way to measure long-term care use.

X. SUPPLEMENTARY TABLES

The tables in this section present results for analyses we describe in Chapters II and III.

Table B.13. Baseline unadjusted means of outcomes among all IAH beneficiaries

Outcome name	Baseline mean for IAH beneficiaries: One pre-demonstration year	Alternative baseline mean for IAH beneficiaries: Two pre-demonstration years pooled[a]
PBPM Medicare expenditures		
Total Medicare expenditures	$4,397	$4,400
Inpatient hospital services	$1,741	-
SNFs	$605	-
Home health services (Parts A and B)	$781	-
Hospice services	$153	-
Outpatient services	$253	-
Physician/supplier	$715	-
Durable medical equipment	$150	-
Service utilization outcomes		
Number of hospital admissions per beneficiary per year[b]	1.78	1.77
Number of hospital admissions for an ACSC per beneficiary per year[c]	0.46	0.46
Probability of having a qualifying hospital discharge and an unplanned readmission within 30 days of discharge (percentage)[d]	19.55	19.99
Number of ED visits per beneficiary per year	2.90	2.85
Number of outpatient ED visits per beneficiary per year[e]	1.46	-
Number of ED visits resulting in inpatient admission per beneficiary per year[f]	1.44	-
Number of outpatient ED visits for an ACSC per beneficiary per year	0.19	0.19
Number of visits in nonacute settings by primary care clinicians[g] per beneficiary per year	11.24	10.96
Number of visits in nonacute settings by specialists per beneficiary per year	5.66	5.52
Probability of home health use (percentage)	91.26	91.47
Home health days per beneficiary per year	165.98	169.12
Number of home health visits	62.32	65.65
Probability of hospice use (percentage)	17.86	16.98
Probability of SNF use (percentage)	41.01	40.91

Table B.13 (continued)

Outcome name	Baseline mean for IAH beneficiaries: One pre-demonstration year	Alternative baseline mean for IAH beneficiaries: Two pre-demonstration years pooled[a]
Health outcomes		
12-month mortality (percentage)	18.13	17.57

Source: Mathematica Policy Research's analysis of data from the IAH implementation contractor and Medicare claims and enrollment data for 2009–2012 obtained from the Virtual Research Data Center for treatment and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4. The data exclude three practices (Atlanta, Chicago, and Stuart) that withdrew from the demonstration before Year 4 and one practice (Louisville) terminated for cause.

[a]Unadjusted baseline means for the pooled pre-demonstration period are reported for outcomes on which we ran the pre-period model sensitivity test.

[b]The number of hospital admissions includes observation stays.

[c]The number of hospital admissions for an ACSC includes observation stays. An admission for an ACSC is one in which appropriate primary and specialty care might prevent or reduce the need for a hospital admission.

[d]The probability of an unplanned readmission for a beneficiary is measured over the IAH-eligible months during each demonstration year. The probability equals zero for beneficiaries who did not have a qualifying hospital discharge or an unplanned readmission within 30 days of a qualifying hospital discharge during the measurement period.

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

[f]The number of outpatient ED visits for an ACSC measures ED visits not resulting in a hospital admission, including those resulting in an observation stay. An ED visit for an ACSC is one in which appropriate primary and specialty care might prevent or reduce the need for an ED visit.

[g]Primary care clinicians are defined as primary care physicians, nurse practitioners, and physician assistants. Nonacute settings are defined as home, office, outpatient clinic, federally qualified health center, or rural health clinic.

ACSC = ambulatory care–sensitive conditions; ED = emergency department; IAH = Independence at Home; PBPM = per beneficiary per month; SNF = skilled nursing facility.

Table B.14. Baseline unadjusted means of outcomes among IAH beneficiaries one year before the demonstration, by practice type

Outcome name	VPA practices	Independent practices	Academic/health system practices
PBPM Medicare expenditures	$4,281	$4,088	$4,577
Number of hospital admissions per beneficiary per year[a]	1.81	1.46	2.08
Probability of having a qualifying hospital discharge and an unplanned readmission within 30 days of discharge[b] (percentage)	17.88	15.41	22.59
Number of ED visits per beneficiary per year[c]	2.97	2.52	2.75

Source: Mathematica's analysis of data from the IAH implementation contractor and Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for treatment and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4. The data exclude three practices (Atlanta, Chicago, and Stuart) that withdrew from the demonstration before Year 4 and one practice (Louisville) terminated for cause.

Note: To calculate treatment group baseline means, we weighted beneficiaries using the same weights implemented in the equal-weight regressions for the practice type subgroup analysis.

[a]The number of hospital admissions includes observation stays.

[b]The probability of an unplanned readmission for a beneficiary is measured over the IAH-eligible months during each demonstration year. The probability equals zero for beneficiaries who did not have a qualifying hospital discharge or an unplanned readmission within 30 days of a qualifying hospital discharge during the measurement period.

[c]The number of ED visits includes those resulting in an observation stay.

ED = emergency department; IAH = Independence at Home; PBPM = per beneficiary per month; VPA = Visiting Physicians Association.

Table B.15. Baseline unadjusted means of outcomes among IAH beneficiaries one year before the demonstration, by dementia subgroups

Outcome name	Beneficiaries with dementia	Beneficiaries without dementia
PBPM Medicare expenditures	$3,949	$4,808
Number of hospital admissions per beneficiary per year[a]	1.62	1.92
Probability of having a qualifying hospital discharge and an unplanned readmission within 30 days of discharge[b] (percentage)	18.08	20.90
Number of ED visits per beneficiary per year[c]	2.71	3.08

Source: Mathematica's analysis of data from the IAH implementation contractor and Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for treatment and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4. The data exclude three practices (Atlanta, Chicago, and Stuart) that withdrew from the demonstration before Year 4 and one practice (Louisville) terminated for cause.

[a]The number of hospital admissions includes observation stays.

[b]The probability of an unplanned readmission for a beneficiary is measured over the IAH-eligible months during each demonstration year. The probability equals zero for beneficiaries who did not have a qualifying hospital discharge or an unplanned readmission within 30 days of a qualifying hospital discharge during the measurement period.

[c]The number of ED visits includes those resulting in observation stay.

ED = emergency department; IAH = Independence at Home; PBPM = per beneficiary per month.

Table B.16. Estimated effect of IAH on total Medicare expenditures PBPM for IAH beneficiaries under frequentist, empirical Bayesian, and full Bayesian models, pooled across all sites, demonstration Years 1 through 4

	Frequentist	Empirical Bayesian	Full Bayesian
Four-year average annual effect[a]	−$161 ($142)	−$37 ($101)	−$47 ($98)
Year 4	−$282 ($205)	−$164 ($126)	−$169 ($118)
Year 3	−$178 ($158)	−$57 ($126)	−$63 ($118)
Year 2	−$32 ($139)	$137 ($125)	$126 ($116)
Year 1	−$120 ($97)	−$67 ($124)	−$72 ($119)
One year pre-IAH[b]	-	-	-
Two years pre-IAH	−$33 ($57)	−$46 ($127)	−$48 ($122)

Total unweighted number of observations across all years: 243,947

Source: Mathematica's analysis of data from the IAH implementation contractor and Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for treatment and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4. The data exclude three practices (Atlanta, Chicago, and Stuart) that withdrew from the demonstration before Year 4 and one practice (Louisville) terminated for cause.

Notes: Standard errors are given in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers both 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. 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. Because of rounding, a difference-in-differences estimate that is displayed as zero might be shown alongside a percentage effect that exceeds zero. We report results from the full Bayesian model in the Executive Summary and Chapter II, where we refer to it simply as the Bayesian model.

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

[b]The difference-in-differences estimate for the period before the demonstration is zero (with no standard error) in all regressions, because the difference-in-differences estimate for each year was calculated as the difference in means between IAH and comparison beneficiaries in the two years before the demonstration minus the difference in the year before the demonstration.

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

IAH = Independence at Home; PBPM = per beneficiary per month.

Table B.17. Estimated effect of IAH on total Medicare expenditures PBPM for IAH beneficiaries, frequentist model, pooled across all sites, demonstration Years 1 through 4

	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect[a]
Four-year average annual effect[b]	$4,312	$4,573	-$261	−$161 ($142)	−3.7
Year 4	$4,394	$4,778	−$384	−$282 ($205)	−6.4
Year 3	$4,577	$4,857	−$280	−$178 ($158)	−4.1
Year 2	$4,737	$4,871	−$133	−$32 ($139)	−0.7
Year 1	$4,756	$4,977	−$221	−$120 ($97)	−2.7
One year pre-IAH[c]	$4,894	$4,995	−$101	−	−
Two years pre-IAH	$5,071	$5,206	−$135	−$33 ($57)	−0.8
Total unweighted number of observations across all years: 243,947					

Source: Mathematica's analysis of data from the IAH implementation contractor and Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for treatment and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4. The data exclude three practices (Atlanta, Chicago, and Stuart) that withdrew from the demonstration before Year 4 and one practice (Louisville) terminated for cause.

Notes: Standard errors are given in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers both 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. 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. Because of rounding, a difference-in-differences estimate that is displayed as zero might be shown alongside a percentage effect that exceeds zero.

[a]We used the unadjusted treatment group mean in the year before the demonstration to calculate the percentage effect for each demonstration year. Table B.13 reports the baseline unadjusted treatment 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 treatment status to obtain an average annual estimated effect across four demonstration years.

[c]The difference-in-differences estimate for the period before the demonstration is zero (with no standard error) in all regressions, because the difference-in-differences estimate for each year was calculated as the difference in means between IAH and comparison beneficiaries in the two years before the demonstration minus the difference in the year before the demonstration.

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

IAH = Independence at Home; PBPM = per beneficiary per month.

Table B.18. Estimated effect of IAH on total Medicare expenditures PBPM for IAH beneficiaries, empirical Bayesian model, pooled across all sites, demonstration Years 1 through 4

	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect[a]
Four-year average annual effect[b]	$4,455	$4,717	−$262	−$37 ($101)	−0.9
Year 4	$4,394	$4,772	−$378	−$164 ($126)	−3.7
Year 3	$4,588	$4,858	−$270	−$57 ($126)	−1.3
Year 2	$4,737	$4,814	−$76	$137 ($125)	3.1
Year 1	$4,761	$5,041	−$280	−$67 ($124)	−1.5
One year before IAH[c]	$4,788	$5,001	−$213	-	-
Two years before IAH	$4,945	$5,204	−$259	−$46 ($127)	−1.0
Total unweighted number of observations across all years: 243,947					

Source: Mathematica's analysis of data from the IAH implementation contractor and Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for treatment and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4. The data exclude three practices (Atlanta, Chicago, and Stuart) that withdrew from the demonstration before Year 4 and one practice (Louisville) terminated for cause.

Notes: Standard errors are given in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers both 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. 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. Because of rounding, a difference-in-differences estimate that is displayed as zero might be shown alongside a percentage effect that exceeds zero.

[a]We used the unadjusted treatment group mean in the year before the demonstration to calculate the percentage effect for each demonstration year. Table B.13 reports the baseline unadjusted treatment 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 treatment status to obtain an average annual estimated effect across four demonstration years.

[c]The difference-in-differences estimate for the period before the demonstration is zero (with no standard error) in all regressions, because the difference-in-differences estimate for each year was calculated as the difference in means between IAH and comparison beneficiaries in the two years before the demonstration minus the difference in the year before the demonstration.

IAH = Independence at Home; PBPM = per beneficiary per month.

Table B.19. Estimated effect of IAH on total Medicare expenditures PBPM for IAH beneficiaries, Bayesian model, pooled across all sites, demonstration Years 1 through 4

	Difference (IAH-comparison)	Difference-in-differences estimated effect	Percentage effect[a]	Probability of savings ≥ $50	Probability of savings ≥ $100
Four-year average annual effect[b]	−$250	−$47 (−$209, $114)	−1.1	48.8%	29.0%
Year 4	−$371	−$169 (−$364, $26)	−3.8	84.3%	72.6%
Year 3	−$265	−$63 (−$256, $134)	−1.4	54.9%	37.8%
Year 2	−$77	$126 (−$61, $317)	2.9	6.0%	2.5%
Year 1	−$275	−$72 (−$268, $121)	−1.6	57.0%	40.6%
One year pre-IAH[c]	−$202	-	-	-	-
Two years pre-IAH	−$250	−$48 (−$247, $153)	−1.1	33.1%	10.4%
Total unweighted number of observations across all years: 243,947					

Source: Mathematica's analysis of data from the IAH implementation contractor and Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for treatment and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4. The data exclude three practices (Atlanta, Chicago, and Stuart) that withdrew from the demonstration before Year 4 and one practice (Louisville) terminated for cause.

Notes: Parentheses report 90 percent credible intervals. We computed coefficients and standard errors by using the weighted sample size, which considers both 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. 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. Because of rounding, a difference-in-differences estimate that is displayed as zero might be shown alongside a percentage effect that exceeds zero. We report results from the full Bayesian model in the Executive Summary and Chapter II, where we refer to it simply as the Bayesian model.

[a]We used the unadjusted treatment group mean in the year before the demonstration to calculate the percentage effect for each demonstration year. Table B.13 reports the baseline unadjusted treatment 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 treatment status to obtain an average annual estimated effect across four demonstration years.

[c]The difference-in-differences estimate for the period before the demonstration is zero (with no credible interval) in all regressions, because the difference-in-differences estimate for each year was calculated as the difference in means between IAH and comparison beneficiaries in the two years before the demonstration minus the difference in the year before the demonstration.

IAH = Independence at Home; PBPM = per beneficiary per month.

Table B.20. Estimated effect of IAH on Medicare expenditures PBPM, by service category for IAH beneficiaries, pooled across all sites, demonstration Years 1 through 4

Service type/period	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect[a]
Inpatient hospital services					
Four-year average annual effect[b]	$1,748	$2,023	−$276	−$87 ($88)	−5.0
Year 4	$1,803	$2,148	−$346	−$156 $134)	−9.0
Year 3	$1,924	$2,193	−$269	−$79 ($95)	-4.5
Year 2	$1,988	$2,219	−$231	−$41 ($77)	−2.4
Year 1	$2,011	$2,256	−$245	−$56 ($61)	−3.2
One year pre-IAH[c]	$2,033	$2,222	−$189	–	–
Two years pre-IAH	$2,122	$2,323	−$201	−$12 ($50)	−0.7
SNF					
Four-year average annual effect[b]	$641	$851	−$210	−$6 ($27)	−1.0
Year 4	$666	$888	−$223	−$18 ($50)	−3.0
Year 3	$693	$897	−$204	$0 ($27)	0.0
Year 2	$666	$857	−$192	$13 ($25)	2.1
Year 1	$661	$879	−$218	−$14 ($19)	−2.3
One year pre-IAH[c]	$675	$880	−$205	-	-
Two years pre-IAH	$720	$934	−$214	−$9 ($18)	−1.5
Home health services (Parts A and B)					
Four-year average annual effect[b]	$696	$499	$196	−$8 ($26)	−1.0
Year 4	$665	$479	$186	−$17 ($36)	−2.2
Year 3	$666	$494	$171	−$33 ($38)	−4.2
Year 2	$795	$562	$234	$30 ($24)	3.8
Year 1	$755	$559	$196	−$8 ($17)	−1.0
One year pre-IAH[c]	$797	$593	$204	-	-
Two years pre-IAH	$856	$644	$212	$8 ($10)	1.0
Hospice services					
Four-year average annual effect[b]	$158	$98	$60	$3 ($7)	1.9
Year 4	$164	$109	$56	−$1 ($10)	−0.6
Year 3	$157	$96	$61	$4 ($12)	2.9
Year 2	$145	$78	$67	$10 ($11)	6.5
Year 1	$157	$100	$56	$0 ($8)	−0.2
One year pre-IAH[c]	$159	$102	$57	-	-
Two years pre-IAH	$138	$94	$44	−$12 ($8)	−8.1
Outpatient services					
Four-year average annual effect[b]	$276	$348	−$72	−$11 ($10)	−4.4
Year 4	$302	$381	−$79	−$18 ($14)	−7.3
Year 3	$299	$369	−$70	−$10 ($11)	−3.8
Year 2	$289	$351	−$61	−$1 ($9)	−0.3

Table B.20 (continued)

Service type/period	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect[a]
Year 1	$268	$342	−$74	−$14* ($8)	−5.4
One year pre-IAH[c]	$278	$339	−$61	-	-
Two years pre-IAH	$273	$326	−$53	$8 ($8)	3.1
Physician/supplier services					
Four-year average annual effect[b]	$692	$663	$29	−$26 ($31)	−3.7
Year 4	$708	$691	$17	−$38 ($39)	−5.3
Year 3	$739	$711	$28	−$27 ($33)	−3.8
Year 2	$743	$709	$35	−$20 ($32)	−2.8
Year 1	$760	$723	$37	−$18 ($24)	−2.5
One year pre-IAH[c]	$789	$734	$55	-	-
Two years pre-IAH	$792	$749	$42	−$13 ($11)	−1.8
Durable medical equipment					
Four-year average annual effect[b]	$102	$89	$13	−$25*** ($7)	−16.8
Year 4	$87	$83	$4	−$33*** ($11)	−22.3
Year 3	$100	$97	$3	−$35*** ($9)	−23.1
Year 2	$110	$94	$15	−$22*** ($9)	−14.8
Year 1	$145	$118	$28	−$10** ($5)	−6.7
One year pre-IAH[c]	$164	$126	$38	-	-
Two years pre-IAH	$171	$136	$34	−$3 ($5)	−2.1
Total unweighted number of observations across all years: 243,947					

Source: Mathematica's analysis of Medicare claims and enrollment data from 2010–2015 obtained from the Virtual Research Data Center for treatment and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 3. The data exclude three practices (Atlanta, Chicago, and Stuart) that withdrew from the demonstration before Year 3 and one practice (Louisville) terminated for cause.

Notes: Standard errors are given in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers both 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. Percentage effects are calculated using the unadjusted IAH group mean in the year before the demonstration. 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. Because of rounding, a difference-in-differences estimate that is displayed as zero might be shown alongside a percentage effect that exceeds zero.

[a]We used the unadjusted treatment group mean in the year before the demonstration to calculate the percentage effect for each demonstration year. Table B.13 reports the baseline unadjusted treatment 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 treatment 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 the difference-in-differences estimate for each year was calculated 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.

IAH = Independence at Home; PBPM = per beneficiary per month; SNF = skilled nursing facility.

Table B.21. Estimated effect of IAH on hospital care use for IAH beneficiaries, pooled across all sites, demonstration Years 1 through 4

Service type/period	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect[a]
Number of hospital admissions per beneficiary per year[b]					
Four-year average annual effect[c]	1.71	1.97	−0.27	−0.07 (0.05)	−3.8
Year 4	1.73	2.05	−0.32	−0.11 (0.08)	−6.0
Year 3	1.87	2.17	−0.31	−0.09 (0.07)	−5.0
Year 2	1.90	2.15	−0.25	−0.03 (0.06)	−1.9
Year 1	1.94	2.21	−0.27	−0.05 (0.04)	−3.0
One year pre-IAH[d]	2.02	2.23	−0.22	-	-
Two years pre-IAH	2.10	2.36	−0.25	−0.04 (0.03)	−2.0
Probability of having a qualifying hospital discharge and an unplanned readmission within 30 days of discharge (percentage)[e]					
Four-year average annual effect[c]	18.60	21.53	−2.93	−1.22 (0.89)	−6.3
Year 4	17.63	21.57	−3.94	−2.12* (1.20)	−10.8
Year 3	20.64	24.32	−3.68	−1.85** (0.92)	−9.5
Year 2	21.23	24.08	−2.85	−1.02 (1.17)	−5.2
Year 1	22.99	24.77	−1.78	0.04 (0.80)	0.2
One year pre-IAH[d]	22.82	24.65	−1.82	-	-
Two years pre-IAH	25.58	27.54	−1.96	−0.14 (0.79)	−0.7
Number of ED visits resulting in inpatient admission per beneficiary per year					
Four-year average annual effect[c]	1.36	1.49	−0.13	−0.10** (0.04)	−7.0
Year 4	1.37	1.56	−0.19	−0.15** (0.07)	−10.4
Year 3	1.49	1.67	−0.17	−0.14** (0.05)	−9.4
Year 2	1.51	1.62	−0.11	−0.07 (0.05)	−4.8
Year 1	1.59	1.70	−0.11	−0.07** (0.03)	−5.2
One year pre-IAH[d]	1.67	1.71	−0.04	-	-
Two years pre-IAH	1.77	1.82	−0.05	−0.02 (0.03)	−1.0
Number of outpatient ED visits per beneficiary per year[f]					
Four-year average annual effect[c]	1.58	1.59	−0.01	−0.02 (0.05)	−1.1
Year 4	1.70	1.75	−0.05	−0.06 (0.08)	−4.1
Year 3	1.78	1.80	−0.02	−0.03 (0.07)	−2.1
Year 2	1.72	1.64	0.08	0.07 (0.06)	4.9
Year 1	1.54	1.57	−0.03	−0.04 (0.06)	−2.4
One year pre-IAH[d]	1.58	1.58	0.01	-	-
Two years pre-IAH	1.55	1.55	0.00	0.00 (0.04)	−0.3

Table B.21 (continued)

Service type/period	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect[a]
Total number of ED visits per beneficiary per year					
Four-year average annual effect[c]	2.94	3.09	−0.14	−0.12** (0.05)	−4.1
Year 4	3.08	3.31	−0.24	−0.21** (0.08)	−7.1
Year 3	3.29	3.47	−0.19	−0.16* (0.09)	−5.4
Year 2	3.22	3.25	−0.03	0.00 (0.07)	−0.1
Year 1	3.13	3.27	−0.14	−0.11 (0.08)	−3.8
One year pre-IAH[d]	3.25	3.28	−0.03	-	-
Two years pre-IAH	3.28	3.34	−0.06	−0.03 (0.05)	−1.0

Source: Mathematica's analysis of data the IAH implementation contractor and Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for treatment and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4. The data exclude three practices (Atlanta, Chicago, and Stuart) that withdrew from the demonstration before Year 4 and one practice (Louisville) terminated for cause.

Notes: Standard errors are given in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers both 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. 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. Because of rounding, a difference-in-differences estimate that is displayed as zero might be shown alongside a percentage effect that exceeds zero.

[a]We used the unadjusted treatment group mean in the year before the demonstration to calculate the percentage effect for each demonstration year. Table B.13 reports the baseline unadjusted treatment 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 treatment status to obtain an average annual estimated effect across four demonstration years.

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

[e]The probability of unplanned readmission for a beneficiary is measured over the IAH-eligible months during each demonstration year. The probability equals zero for beneficiaries who did not have a qualifying hospital discharge or an unplanned readmission within 30 days of a qualifying hospital discharge during the measurement period.

[f]The number of ED visits measure includes visits resulting in a hospital admission or an observation stay.

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

ED = emergency department; IAH = Independence at Home.

Table B.22. Estimated effect of IAH on use of hospice and SNF services for IAH beneficiaries, pooled across all sites, demonstration Years 1 through 4

Service type/period	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect[a]
Probability of hospice use (percentage)					
Four-year average annual effect[b]	17.69	14.67	3.02	−0.83 (0.68)	−4.6
Year 4	17.18	15.37	1.81	−2.04** (0.90)	−11.4
Year 3	17.96	14.57	3.39	−0.46 (0.88)	−2.6
Year 2	17.56	13.67	3.88	0.03 (0.81)	0.2
Year 1	18.35	15.06	3.29	−0.56 (0.73)	−3.1
One year pre-IAH[c]	18.83	14.98	3.85	-	-
Two years pre-IAH	17.32	15.22	2.10	−1.75** (0.76)	−9.8
Probability of SNF use (percentage)					
Four-year average annual effect[b]	42.91	54.76	−11.85	0.19 (0.82)	0.5
Year 4	43.60	55.83	−12.23	−0.12 (1.22)	−0.3
Year 3	44.72	56.58	−11.85	0.26 (0.89)	0.6
Year 2	43.05	53.87	−10.82	1.29 (1.06)	3.1
Year 1	43.28	55.87	−12.59	−0.48 (0.75)	−1.2
One year pre-IAH[c]	43.23	55.34	−12.11	-	-
Two years pre-IAH	44.52	55.55	−11.04	1.07 (0.91)	2.6

Total unweighted number of observations across all years: 243,947

Source: Mathematica's analysis of data from the IAH implementation contractor and Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for treatment and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4. The data exclude three practices (Atlanta, Chicago, and Stuart) that withdrew from the demonstration before Year 4 and one practice (Louisville) terminated for cause.

Notes: Standard errors are given in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers both 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. 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. Because of rounding, a difference-in-differences estimate that is displayed as zero might be shown alongside a percentage effect that exceeds zero.

[a]We used the unadjusted treatment group mean in the year before the demonstration to calculate the percentage effect for each demonstration year. Table B.13 reports the baseline unadjusted treatment 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 treatment 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 the difference-in-differences estimate for each year was calculated 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.

IAH = Independence at Home; SNF = skilled nursing facility.

Table B.23. Estimated effect of IAH on use of home health for IAH beneficiaries, pooled across all sites, demonstration Years 1 through 4

Service type/period	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect[a]
Probability of home health use (percentage)					
Four-year average annual effect[b]	90.52	77.83	12.69	−0.45 (0.70)	−0.5
Year 4	89.92	77.42	12.51	−0.17 (0.84)	−0.2
Year 3	90.98	79.14	11.84	−0.84 (0.83)	−0.9
Year 2	91.66	79.24	12.42	−0.26 (0.76)	−0.3
Year 1	91.13	78.89	12.24	−0.43 (0.55)	−0.5
One year pre-IAH[c]	91.45	78.78	12.67	-	-
Two years pre-IAH	91.64	80.22	11.42	−1.26*** (0.47)	−1.4
Home health days per beneficiary per year					
Four-year average annual effect[b]	160.56	106.62	53.94	−0.58 (5.67)	−0.4
Year 4	157.36	103.65	53.72	−3.31 (8.69)	−2.0
Year 3	162.35	110.35	52.00	−5.02 (7.74)	−3.0
Year 2	182.13	117.86	64.27	7.25 (6.99)	4.4
Year 1	171.30	115.25	56.05	−0.98 (3.39)	−0.6
One year pre-IAH[c]	178.14	121.12	57.02	-	-
Two years pre-IAH	181.87	125.62	56.24	−0.78 (4.29)	−0.5
Number of home health visits					
Four-year average annual effect[b]	60.71	41.60	19.11	0.14 (2.43)	0.2
Year 4	60.03	41.08	18.96	−1.02 (4.20)	−1.6
Year 3	62.20	43.58	18.62	−1.36 (3.47)	−2.2
Year 2	71.45	46.27	25.18	5.20** (2.32)	8.4
Year 1	62.99	44.75	18.24	−1.74 (1.83)	−2.8
One year pre-IAH[c]	67.05	47.07	19.98	-	-
Two years pre-IAH	72.05	50.91	21.15	1.17 (2.07)	1.9

Total unweighted number of observations across all years: 243,947

Source: Mathematica's analysis of data from the IAH implementation contractor and Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for treatment and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4. The data exclude three practices (Atlanta, Chicago, and Stuart) that withdrew from the demonstration before Year 4 and one practice (Louisville) terminated for cause.

Notes: Standard errors are given in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers both 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. 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. Because of rounding, a difference-in-differences estimate that is displayed as zero might be shown alongside a percentage effect that exceeds zero.

[a]We used the unadjusted treatment group mean in the year before the demonstration to calculate the percentage effect for each demonstration year. Table B.13 reports the baseline unadjusted treatment group mean for all outcomes.

Table B.23 (continued)

[b]We estimated a separate model using a single demonstration indicator (instead of separate indicators for each demonstration year) and used its interaction with treatment 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 the difference-in-differences estimate for each year was calculated 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.

IAH = Independence at Home.

Table B.24. Estimated effect of IAH on visits from clinicians for IAH beneficiaries, pooled across all sites, demonstration Years 1 through 4

Service type/period	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect[a]
Visits in nonacute settings by primary care clinicians[b] per beneficiary per year					
Four-year average annual effect[c]	11.99	6.27	5.71	0.59 (0.57)	5.3
Year 4	12.87	6.23	6.64	1.40 (1.02)	12.4
Year 3	12.39	6.56	5.83	0.58 (0.63)	5.2
Year 2	12.24	6.55	5.69	0.45 (0.55)	4.0
Year 1	11.54	6.41	5.13	−0.12 (0.31)	−1.1
One year pre-IAH[d]	11.67	6.43	5.25	-	-
Two years pre-IAH	11.15	6.41	4.75	−0.50*** (0.17)	−4.4
Visits in nonacute settings by specialists per beneficiary per year					
Four-year average annual effect[c]	5.27	7.51	−2.24	−0.39 (0.32)	−6.9
Year 4	5.29	7.53	−2.24	−0.30 (0.41)	−5.2
Year 3	5.44	7.90	−2.46	−0.52 (0.35)	−9.2
Year 2	5.61	8.10	−2.49	−0.55* (0.32)	−9.8
Year 1	5.66	7.89	−2.23	−0.29 (0.32)	−5.0
One year pre-IAH[d]	5.98	7.92	−1.94	-	-
Two years pre-IAH	5.71	7.72	−2.01	−0.07 (0.19)	−1.2
Total unweighted number of observations across all years: 243,947					

Source: Mathematica's analysis of data from the IAH implementation contractor and Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for treatment and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4. The data exclude three practices (Atlanta, Chicago, and Stuart) that withdrew from the demonstration before Year 4 and one practice (Louisville) terminated for cause.

Notes: Standard errors are given in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers both 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. 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. Because of rounding, a difference-in-differences estimate that is displayed as zero might be shown alongside a percentage effect that exceeds zero.

[a]We used the unadjusted treatment group mean in the year before the demonstration to calculate the percentage effect for each demonstration year. Table B.13 reports the baseline unadjusted treatment group mean for all outcomes.

[b]Primary care clinicians are defined as primary care physicians, nurse practitioners, or physician assistants. Nonacute settings are defined as home, office, outpatient clinic, federally qualified health center, or rural health clinic.

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

[d]The difference-in-differences estimate for the year before the demonstration is zero (with no standard error) in all regressions, because the difference-in-differences estimate for each year was calculated 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.

IAH = Independence at Home.

Table B.25. Estimated effect of IAH on 12-month mortality for IAH beneficiaries, pooled across all sites, demonstration Years 1 through 4

Period	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect[a]
Four-year average annual effect[b]	18.3	20.6	−2.4	−0.2 (0.5)	−1.0
Year 4	17.62	21.26	−3.64	−1.43*** (0.54)	−7.9
Year 3	19.27	20.60	−1.33	0.87 (0.60)	4.8
Year 2	18.63	20.52	−1.89	0.32 (0.65)	1.7
Year 1	19.06	21.40	−2.34	−0.13 (0.55)	−0.7
One year pre-IAH[c]	19.46	21.66	−2.20	-	-
Two years pre-IAH	18.58	22.71	−4.13	−1.93*** (0.68)	−10.7
Total unweighted number of observations across all years: 243,947					

Source: Mathematica's analysis of data from the IAH implementation contractor and Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for treatment and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4. The data exclude three practices (Atlanta, Chicago, and Stuart) that withdrew from the demonstration before Year 4 and one practice (Louisville) terminated for cause.

Notes: Standard errors are given in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers both 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. 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. Because of rounding, a difference-in-differences estimate that is displayed as zero might be shown alongside a percentage effect that exceeds zero.

[a]We used the unadjusted treatment group mean in the year before the demonstration to calculate the percentage effect for each demonstration year. Table B.13 reports the baseline unadjusted treatment 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 treatment 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 the difference-in-differences estimate for each year was calculated 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.

IAH = Independence at Home.

Table B.26. Estimated effect of IAH on number of total Medicare expenditures PBPM for IAH beneficiaries: Results by dementia subgroups, demonstration Years 1 through 4

Subgroup/period	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect[a]
Dementia					
Year 4	$3,824	$4,258	−$434	−$222 ($143)	−5.6
Year 3	$4,042	$4,372	−$330	−$118 ($136)	−3.0
Year 2	$4,170	$4,380	−$211	$1 ($117)	0.0
Year 1	$4,175	$4,493	−$318	−$106 ($74)	−2.7
One year pre-IAH[b]	$4,279	$4,491	−$212	-	-
Two years pre-IAH	$4,481	$4,700	−$219	−$7 ($84)	−0.2
Total unweighted number of observations across all years: 113,971					
Nondementia					
Year 4	$4,939	$5,275	−$336	−$347 ($279)	−7.2
Year 3	$5,075	$5,303	−$228	−$238 ($214)	−5.0
Year 2	$5,281	$5,342	−$61	−$71 ($184)	−1.5
Year 1	$5,315	$5,433	−$118	−$128 ($160)	−2.7
One year pre-IAH[b]	$5,481	$5,470	$10	-	-
Two years pre-IAH	$5,631	$5,680	−$49	−$59 ($76)	−1.2
Total unweighted number of observations across all years: 129,976					
Chow test for significant differences across separate regressions for subgroups[c]				X^2 = 2548.98 p = 0.0000	
Test for significant difference in the estimated effect in each year across subgroups, jointly for all years[d]				X^2 = 6.52 p = 0.6871	

Source: Mathematica's analysis of data from the IAH implementation contractor and Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for treatment and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4. The data exclude three practices (Atlanta, Chicago, and Stuart) that withdrew from the demonstration before Year 4 and one practice (Louisville) terminated for cause.

Notes: Standard errors are given in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers both 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 that is displayed as zero might be shown alongside a percentage effect that exceeds zero.

[a]We used the unadjusted treatment group mean in the year before the demonstration to calculate the percentage effect for each demonstration year. Table B.15 reports the baseline unadjusted treatment 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 the difference-in-differences estimate for each year was calculated as the difference in means between IAH and comparison beneficiaries in that year minus the difference in the year before the demonstration.

[c]We used the Chow test to check whether the estimated regression coefficients considered jointly for each subgroup differed significantly across the subgroups, thus warranting a subgroup analysis by practice types.

[d]We used a Wald test to determine whether the difference-in-differences estimates are jointly significantly different for the subgroups.

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

Table B.26 (continued)

IAH = Independence at Home; PBPM = per beneficiary per month.

Table B.27. Estimated effect of IAH on number of hospital admissions per beneficiary per year for IAH beneficiaries: Results by dementia subgroups, demonstration Years 1 through 4

Subgroup/period	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect[a]
Dementia					
Year 4	1.52	1.88	−0.36	−0.11** (0.05)	−6.6
Year 3	1.66	1.98	−0.32	−0.07 (0.07)	−4.1
Year 2	1.70	1.96	−0.26	−0.01 (0.07)	−0.4
Year 1	1.70	2.03	−0.32	−0.07* (0.04)	−4.4
One year pre-IAH[b]	1.78	2.03	−0.25	-	-
Two years pre-IAH	1.85	2.13	−0.28	−0.03 (0.05)	−1.9
Total unweighted number of observations across all years: 113,971					
Nondementia					
Year 4	1.93	2.21	−0.28	−0.11 (0.11)	−5.6
Year 3	2.06	2.35	−0.29	−0.11 (0.10)	−5.9
Year 2	2.08	2.31	−0.23	−0.06 (0.07)	−3.2
Year 1	2.16	2.37	−0.21	−0.04 (0.06)	−2.0
One year pre-IAH[b]	2.24	2.42	−0.17	-	-
Two years pre-IAH	2.34	2.56	−0.22	−0.05 (0.05)	−2.5
Total unweighted number of observations across all years: 129,976					
Chow test for significant differences across separate regressions for subgroups[c]			$X^2 = 1592.22$ $p = 0.0000$		
Test for significant difference in the estimated effect in each year across subgroups, jointly for all years[d]			$X^2 = 25.23$ $p = 0.0027$		

Source: Mathematica's analysis of data from the IAH implementation contractor and Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for treatment and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4. The data exclude three practices (Atlanta, Chicago, and Stuart) that withdrew from the demonstration before Year 4 and one practice (Louisville) terminated for cause.

Notes: Standard errors are given in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers both 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 that is displayed as zero might be shown alongside a percentage effect that exceeds zero.

[a]We used the unadjusted treatment group mean in the year before the demonstration to calculate the percentage effect for each demonstration year. Table B.15 reports the baseline unadjusted treatment 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 the difference-in-differences estimate for each year was calculated as the difference in means between IAH and comparison beneficiaries in that year minus the difference in the year before the demonstration.

[c]We used the Chow test to check whether the estimated regression coefficients considered jointly for each subgroup differed significantly across the subgroups, thus warranting a subgroup analysis by dementia/non-dementia patients.

[d]We used a Wald test to determine whether the difference-in-differences estimates are jointly significantly different for the subgroups.

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

Table B.27 (continued)

IAH = Independence at Home.

Table B.28. Estimated effect of IAH on total number of ED visits per beneficiary per year, by IAH beneficiaries: Results by dementia subgroups, demonstration Years 1 through 4

Subgroup/period	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect[a]
Dementia					
Year 4	2.91	3.09	−0.18	−0.12 (0.09)	−4.6
Year 3	2.96	3.24	−0.28	−0.22*** (0.08)	−8.3
Year 2	2.96	3.01	−0.05	0.00 (0.09)	0.2
Year 1	2.82	2.99	−0.18	−0.12* (0.07)	−4.5
One year pre-IAH[b]	2.93	2.99	−0.06	-	-
Two years pre-IAH	2.92	3.03	−0.11	−0.05 (0.08)	−1.8
Total unweighted number of observations across all years: 113,971					
Nondementia					
Year 4	3.24	3.55	−0.31	−0.33** (0.12)	−10.6
Year 3	3.60	3.71	−0.11	−0.13 (0.14)	−4.3
Year 2	3.50	3.50	0.00	−0.02 (0.09)	−0.6
Year 1	3.41	3.52	−0.11	−0.13 (0.10)	−4.3
One year pre-IAH[b]	3.57	3.55	0.02	-	-
Two years pre-IAH	3.65	3.65	0.00	−0.02 (0.07)	−0.6
Total unweighted number of observations across all years: 129,976					

Chow test for significant differences across separate regressions for subgroups[c]	X^2 = 1475.76 p = 0.0000
Test for significant difference in the estimated effect in each year across subgroups, jointly for all years[d]	X^2 = 15.34 p = 0.0819

Source: Mathematica's analysis of data from the IAH implementation contractor and Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for treatment and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4. The data exclude three practices (Atlanta, Chicago, and Stuart) that withdrew from the demonstration before Year 4 and one practice (Louisville) terminated for cause.

Notes: Standard errors are given in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers both 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 that is displayed as zero might be shown alongside a percentage effect that exceeds zero.

[a]We used the unadjusted treatment group mean in the year before the demonstration to calculate the percentage effect for each demonstration year. Appendix Table B.15 reports the baseline unadjusted treatment 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 the difference-in-differences estimate for each year was calculated as the difference in means between IAH and comparison beneficiaries in that year minus the difference in the year before the demonstration.

[c]We used the Chow test to check whether the estimated regression coefficients considered jointly for each subgroup differed significantly across the subgroups, thus warranting a subgroup analysis by dementia/non-dementia patients.

[d]We used a Wald test to determine whether the difference-in-differences estimates are jointly significantly different for the subgroups.

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

Table B.28 (continued)

ED = emergency department; IAH = Independence at Home.

Table B.29. Estimated effect of IAH on probability of a qualifying hospital discharge and an unplanned readmission within 30 days of discharge for IAH beneficiaries: Results by dementia subgroups, demonstration Years 1 through 4

Subgroup/period	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect[a]
Dementia					
Year 4	14.90	19.38	−4.48	−2.17** (1.08)	−12.0
Year 3	18.47	21.45	−2.99	−0.67 (1.54)	−3.7
Year 2	18.02	21.67	−3.65	−1.34 (1.31)	−7.4
Year 1	20.24	22.31	−2.07	0.24 (1.24)	1.3
One year pre-IAH[b]	20.37	22.69	−2.31	-	-
Two years pre-IAH	23.64	24.85	−1.21	1.10 (1.10)	6.1
Total unweighted number of observations across all years: 113,971					
Nondementia					
Year 4	20.23	23.57	−3.34	−2.10 (1.50)	−10.0
Year 3	22.62	26.85	−4.23	−2.99*** (0.94)	−14.3
Year 2	24.25	26.30	−2.05	−0.81 (1.36)	−3.9
Year 1	25.49	26.94	−1.45	−0.21 (1.01)	−1.0
One year pre-IAH[b]	25.16	26.40	−1.24	-	-
Two years pre-IAH	27.44	29.89	−2.45	−1.21 (1.20)	−5.8
Total unweighted number of observations across all years: 129,976					

Chow test for significant differences across separate regressions for subgroups[c]	$X^2 = 1013.28$ $p = 0.0000$
Test for significant difference in the estimated effect in each year across subgroups, jointly for all years[d]	$X^2 = 42.56$ $p = 0.0000$

Source: Mathematica analysis of data from the IAH implementation contractor and Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for treatment and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4. The data exclude three practices (Atlanta, Chicago, and Stuart) that withdrew from the demonstration before Year 4 and one practice (Louisville) terminated for cause.

Notes: Standard errors are given in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers both 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. 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. Because of rounding, a difference-in-differences estimate that is displayed as zero might be shown alongside a percentage effect that exceeds zero.

[a]We used the unadjusted treatment group mean in the year before the demonstration to calculate the percentage effect for each demonstration year. Table B.15 reports the baseline unadjusted treatment 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 the difference-in-differences estimate for each year was calculated as the difference in means between IAH and comparison beneficiaries in that year minus the difference in the year before the demonstration.

[c]We used the Chow test to check whether the estimated regression coefficients considered jointly for each subgroup differed significantly across the subgroups, thus warranting a subgroup analysis by dementia/non-dementia patients.

Table B.29 (continued)

[d]We used a Wald test to determine whether the difference-in-differences estimates are jointly significantly different for the subgroups.

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

IAH = Independence at Home.

Table B.30. Estimated effect of IAH on total Medicare expenditures PBPM for IAH beneficiaries: Results by practice type, demonstration Years 1 through 4

Practice type/period	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect[a]
All VPA practices (Dallas, Flint, Jacksonville, Lansing, and Milwaukee)					
Year 4	$4311	$4679	−$368	−$62 ($112)	−1.5
Year 3	$4590	$4877	−$287	$18 ($67)	0.4
Year 2	$4720	$4944	−$224	$81 ($123)	1.9
Year 1	$4653	$5003	−$350	−$45 ($72)	−1.0
One year pre-IAH[b]	$4791	$5096	−$305	-	-
Two years pre-IAH	$4927	$5341	−$414	−$109 ($86)	−2.5
Total unweighted number of observations across all years: 126,881					
All independent practices (Austin, Brooklyn, Durham, and Portland)					
Year 4	$4303	$4807	−$504	−$354 ($279)	−8.7
Year 3	$4494	$4788	−$294	−$144 ($261)	−3.5
Year 2	$4481	$4736	$−254	−$105 ($202)	−2.6
Year 1	$4823	$4942	−$119	$31 ($214)	0.8
One year pre-IAH[b]	$4678	$4828	−$150	-	-
Two years pre-IAH	$4947	$4928	$19	$168 ($146)	4.1
Total unweighted number of observations across all years: 82,130					
All academic/health system practices (Boston, Cleveland, Mid-Atlantic consortium [Philadelphia, Richmond, and Washington, DC], Long Island, and Wilmington)					
Year 4	$4291	$4672	−$381	−$145 ($231)	−3.2
Year 3	$4371	$4667	−$296	−$60 ($171)	−1.3
Year 2	$4576	$4574	$2	$238 ($191)	5.2
Year 1	$4526	$4949	−$423	−$186 ($241)	−4.1
One year pre-IAH[b]	$4663	$4900	−$237	-	-
Two years pre-IAH	$4835	$5109	−$274	−$37* ($220)	−0.8
Total unweighted number of observations across all years: 34,936					

Chow test for significant differences across separate regressions for practice subgroups[c]: $X^2 = 5178.44$, $p = 0.0000$

Test for significant difference in the estimated effect in each year across subgroups, jointly for all years[d]: $X^2 = 22.13$, $p = 0.0085$

Source: Mathematica analysis of data from the IAH implementation contractor and Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for treatment and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4. The data exclude three practices (Atlanta, Chicago, and Stuart) that withdrew from the demonstration before Year 4 and one practice (Louisville) terminated for cause.

Notes: Standard errors are given in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers both 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

Table B.30 (continued)

difference-in-differences estimate that is displayed as zero might be shown alongside a percentage effect that exceeds zero.

[a]We used the unadjusted treatment group mean in the year before the demonstration to calculate the percentage effect for each demonstration year. Table B.14 reports the baseline unadjusted treatment 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 the difference-in-differences estimate for each year was calculated as the difference in means between IAH and comparison beneficiaries in that year minus the difference in the year before the demonstration.

[c]We used the Chow test to check whether the estimated regression coefficients considered jointly for each subgroup differed significantly across the subgroups, thus warranting a subgroup analysis by practice types.

[d]We used a Wald test to determine whether the difference-in-differences estimates are jointly significantly different for the subgroups.

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

IAH = Independence at Home; PBPM = per beneficiary per month; VPA = Visiting Physicians Association.

Table B.31. Estimated effect of IAH on number of hospital admissions per beneficiary per year for IAH beneficiaries: Results by practice type, demonstration Years 1 through 4

Practice type/period	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect[a]
All VPA practices (Dallas, Flint, Jacksonville, Lansing, and Milwaukee)					
Year 4	1.83	2.20	−0.36	0.05 (0.07)	2.8
Year 3	1.96	2.34	−0.37	0.04 (0.08)	2.3
Year 2	1.99	2.35	−0.36	0.05 (0.07)	2.9
Year 1	2.03	2.39	−0.36	0.06 (0.04)	3.3
One year pre-IAH[b]	2.07	2.49	−0.42	-	-
Two years pre-IAH	2.17	2.58	−0.41	0.01 (0.04)	0.4
Total unweighted number of observations across all years: 126,881					
All independent practices (Austin, Brooklyn, Durham, and Portland)					
Year 4	1.42	1.84	−0.42	−0.19 (0.13)	−12.8
Year 3	1.61	1.92	−0.31	−0.08 (0.15)	−5.1
Year 2	1.57	1.84	0.00	−0.04 (0.10)	−2.5
Year 1	1.65	1.99	−0.34	−0.11 (0.13)	−7.4
One year pre-IAH[b]	1.69	1.92	−0.23	-	-
Two years pre−IAH	1.87	2.06	−0.19	0.04 (0.07)	2.8
Total unweighted number of observations across all years: 82,130					
All academic/health system practices (Boston, Cleveland, Mid-Atlantic consortium [Philadelphia, Richmond, and Washington, DC], Long Island, and Wilmington)					
Year 4	1.80	1.91	−0.11	−0.06 (0.08)	−2.7
Year 3	1.83	1.91	−0.09	−0.04 (0.12)	−1.8
Year 2	1.92	1.94	−0.02	0.03 (0.08)	1.6
Year 1	1.82	2.09	−0.28	−0.23 (0.15)	−10.9
One year pre-IAH[b]	2.10	2.14	−0.05	-	-
Two years pre-IAH	1.98	2.20	−0.23	−0.18* (0.10)	−8.5
Total unweighted number of observations across all years: 34,936					

Chow test for significant differences across separate regressions for practice subgroups[c]	X^2 = 1510.91 p = 0.0000
Test for significant difference in the estimated effect in each year across subgroups, jointly for all years[d]	X^2 = 14.70 p = 0.0996

Source: Mathematica's analysis of data from the IAH implementation contractor and Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for treatment and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4. The data exclude three practices (Atlanta, Chicago, and Stuart) that withdrew from the demonstration before Year 4 and one practice (Louisville) terminated for cause.

Notes: Standard errors are given in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers both 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

Table B.31 (continued)

regression coefficients to the covariates of IAH beneficiaries in the latest demonstration year. Because of rounding, a difference-in-differences estimate that is displayed as zero might be shown alongside a percentage effect that exceeds zero.

[a]We used the unadjusted treatment group mean in the year before the demonstration to calculate the percentage effect for each demonstration year. Table B.14 reports the baseline unadjusted treatment 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 the difference-in-differences estimate for each year was calculated as the difference in means between IAH and comparison beneficiaries in that year minus the difference in the year before the demonstration.

[c]We used the Chow test to check whether the estimated regression coefficients considered jointly for each subgroup differed significantly across the subgroups, thus warranting a subgroup analysis by practice types.

[d]We used a Wald test to determine whether the difference-in-differences estimates are jointly significantly different for the subgroups.

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

IAH = Independence at Home; VPA = Visiting Physicians Association.

Table B.32. Estimated effect of IAH on total number of ED visits per beneficiary per year, by IAH beneficiaries: Results by practice type, demonstration Years 1 through 4

Practice type/period	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect[a]
All VPA practices (Dallas, Flint, Jacksonville, Lansing, and Milwaukee)					
Year 4	3.24	3.51	−0.26	−0.08 (0.09)	−2.7
Year 3	3.51	3.73	−0.22	−0.04 (0.10)	−1.3
Year 2	3.49	3.55	−0.05	0.13 (0.11)	4.3
Year 1	3.41	3.59	−0.18	0.00 (0.09)	0.1
One year pre-IAH[b]	3.39	3.57	−0.18	-	-
Two years pre-IAH	3.41	3.65	−0.24	−0.05 (0.07)	−1.8
Total unweighted number of observations across all years: 126,881					
All independent practices (Austin, Brooklyn, Durham, and Portland)					
Year 4	2.62	3.15	−0.53	−0.52*** (0.20)	−20.8
Year 3	2.73	3.13	−0.40	−0.40** (0.18)	−15.7
Year 2	2.70	2.87	0.00	−0.16 (0.10)	−6.3
Year 1	2.77	2.99	−0.23	−0.22 (0.16)	−8.7
One year pre-IAH[b]	2.88	2.89	−0.01	-	-
Two years pre-IAH	3.02	2.98	0.03	0.04 (0.11)	1.6
Total unweighted number of observations across all years: 82,130					
All academic/health system practices (Boston, Cleveland, Mid-Atlantic consortium [Philadelphia, Richmond, and Washington, DC], Long Island, and Wilmington)					
Year 4	2.67	2.98	−0.31	−0.05 (0.12)	−1.8
Year 3	2.84	3.04	−0.20	0.06 (0.19)	2.1
Year 2	2.58	2.87	−0.29	−0.04 (0.12)	−1.3
Year 1	2.15	2.37	−0.22	0.04 (0.34)	1.4
One year pre-IAH[b]	2.78	3.04	−0.26	-	-
Two years pre-IAH	2.72	2.99	−0.27	−0.01* (0.13)	−0.5
Total unweighted number of observations across all years: 34,936					

Chow test for *significant differences across separate regressions for practice subgroups*[c]	$X^2 = 2236.59$
	$p = 0.0000$
Test for significant difference in the estimated effect in each year across subgroups, jointly for all years[d]	$X^2 = 11.71$
	$p = 0.2300$

Source: Mathematica's analysis of data from the IAH implementation contractor and Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for treatment and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4. The data exclude three practices (Atlanta, Chicago, and Stuart) that withdrew from the demonstration before Year 4 and one practice (Louisville) terminated for cause.

Table B.32 (continued)

Notes: Standard errors are given in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers both 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 that is displayed as zero might be shown alongside a percentage effect that exceeds zero.

[a]We used the unadjusted treatment group mean in the year before the demonstration to calculate the percentage effect for each demonstration year. Table B.14 reports the baseline unadjusted treatment 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 the difference-in-differences estimate for each year was calculated as the difference in means between IAH and comparison beneficiaries in that year minus the difference in the year before the demonstration.

[c]We used the Chow test to check whether the estimated regression coefficients considered jointly for each subgroup differed significantly across the subgroups, thus warranting a subgroup analysis by practice types.

[d]We used a Wald test to determine whether the difference-in-differences estimates are jointly significantly different for the subgroups.

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

ED = emergency department; IAH = Independence at Home; VPA = Visiting Physicians Association.

Table B.33. Estimated effect of IAH on probability of having a qualifying hospital discharge and an unplanned readmission within 30 days of discharge for IAH beneficiaries: Results by practice type, demonstration Years 1 through 4

Practice type/period	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect[a]
All VPA practices (Dallas, Flint, Jacksonville, Lansing, and Milwaukee)					
Year 4	0.16	0.20	−0.03	−0.01 (0.02)	−4.6
Year 3	0.20	0.23	−0.03	−0.01 (0.01)	−3.5
Year 2	0.20	0.23	−0.03	0.00 (0.02)	−1.8
Year 1	0.21	0.23	−0.02	0.01 (0.01)	4.3
One year pre-IAH[b]	0.21	0.24	−0.03	-	-
Two years pre-IAH	0.25	0.26	−0.01	0.01 (0.01)	6.3
Total unweighted number of observations across all years: 126,881					
All independent practices (Austin, Brooklyn, Durham, and Portland)					
Year 4	0.13	0.15	−0.03	−0.04 (0.02)	−23.2
Year 3	0.15	0.18	−0.03	−0.04* (0.02)	−22.8
Year 2	0.15	0.17	0.00	−0.03 (0.03)	−17.5
Year 1	0.18	0.19	−0.01	−0.02 (0.02)	−11.3
One year pre-IAH[b]	0.19	0.18	0.01	-	-
Two years pre-IAH	0.18	0.21	−0.02	−0.03 (0.03)	−20.6
Total unweighted number of observations across all years: 82,130					
All academic/health system practices (Boston, Cleveland, Mid-Atlantic consortium [Philadelphia, Richmond, and Washington, DC], Long Island, and Wilmington)					
Year 4	0.17	0.17	−0.01	−0.03 (0.02)	−11.9
Year 3	0.18	0.17	0.01	−0.01 (0.02)	−4.9
Year 2	0.21	0.19	0.02	0.00 (0.02)	−1.4
Year 1	0.21	0.21	−0.01	−0.03 (0.03)	−12.1
One year pre-IAH[b]	0.23	0.21	0.02	-	-
Two years pre-IAH	0.23	0.24	−0.01	−0.03* (0.02)	−14.9
Total unweighted number of observations across all years: 34,936					

Chow test for significant differences across separate regressions for practice subgroups[c]	$X^2 = 5178.44$ $p = 0.0000$
Test for significant difference in the estimated effect in each year across subgroups, jointly for all years[d]	$X^2 = 22.13$ $p = 0.0085$

Source: Mathematica's analysis of data from the IAH implementation contractor and Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for treatment and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4. The data exclude three practices (Atlanta, Chicago, and Stuart) that withdrew from the demonstration before Year 4 and one practice (Louisville) terminated for cause.

Table B.33 (continued)

Notes: Standard errors are given in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers both 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 that is displayed as zero might be shown alongside a percentage effect that exceeds zero.

[a]We used the unadjusted treatment group mean in the year before the demonstration to calculate the percentage effect for each demonstration year. Table B.14 reports the baseline unadjusted treatment 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 the difference-in-differences estimate for each year was calculated as the difference in means between IAH and comparison beneficiaries in that year minus the difference in the year before the demonstration.

[c]We used the Chow test to check whether the estimated regression coefficients considered jointly for each subgroup differed significantly across the subgroups, thus warranting a subgroup analysis by practice types.

[d]We used a Wald test to determine whether the difference-in-differences estimates are jointly significantly different for the subgroups.

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

IAH = Independence at Home; VPA = Visiting Physicians Association.

Table B.34. Estimated effect of IAH on total Medicare expenditures PBPM with two pre-demonstration years as the baseline, demonstration Years 1 through 4

Period	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect[a]
Year 4	$4394	$4778	−$384	−$266 ($209)	−6.0
Year 3	$4577	$4857	−$280	−$162 ($162)	−3.7
Year 2	$4737	$4870	−$133	−$16 ($139)	−0.4
Year 1	$4755	$4976	−$221	−$103 ($101)	−2.4
Pre-demonstration years[b]	$4978	$5095	−$117	-	-

Total unweighted number of observations across all years: 243,947

Source: Mathematica's analysis of data from the IAH implementation contractor and Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for treatment and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4. The data exclude three practices (Atlanta, Chicago, and Stuart) that withdrew from the demonstration before Year 4 and one practice (Louisville) terminated for cause.

Notes: Standard errors are given in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers both 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 that is displayed as zero might be shown alongside a percentage effect that exceeds zero.

[a]We used the unadjusted treatment group mean during two years before the demonstration to calculate the percentage effect for each demonstration year. Table B.13 reports the baseline unadjusted treatment 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 the difference-in-differences estimate for each year was calculated as the difference in means between IAH and comparison beneficiaries in the two years before the demonstration minus the difference in the year before the demonstration.

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

IAH = Independence at Home; PBPM = per beneficiary per month.

Table B.35. Estimated effect of IAH on use of ED and acute hospital care with two pre-demonstration years as the baseline, demonstration Years 1 through 4

Service type/period	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect[a]
Number of hospital admissions per beneficiary per year[b]					
Year 4	1.73	2.05	−0.32	−0.09 (0.08)	−5.1
Year 3	1.87	2.17	−0.31	−0.07 (0.07)	−4.1
Year 2	1.90	2.15	−0.25	−0.02 (0.06)	−0.9
Year 1	1.94	2.21	−0.27	−0.04 (0.04)	−2.0
Pre-demonstration years[c]	2.06	2.29	−0.23	-	-
Probability of having a qualifying hospital discharge and an unplanned readmission within 30 days of discharge[d]					
Year 4	17.63	21.57	−3.94	−2.04* (1.06)	−10.2
Year 3	20.64	24.31	−3.68	−1.78** (0.88)	−8.9
Year 2	21.23	24.07	−2.85	−0.95 (0.99)	−4.8
Year 1	22.98	24.76	−1.78	0.11 (0.72)	0.6
Pre-demonstration years[c]	24.10	26.00	−1.90	-	-
Total ED visits per beneficiary per year					
Year 4	3.08	3.32	−0.24	−0.20** (0.08)	−7.1
Year 3	3.29	3.48	−0.18	−0.15* (0.09)	−5.2
Year 2	3.23	3.26	−0.03	0.01 (0.07)	0.2
Year 1	3.12	3.26	−0.14	−0.11 (0.08)	−3.9
Pre-demonstration years[c]	3.27	3.30	−0.03	-	-

Source: Mathematica's analysis of data from the IAH implementation contractor and Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for treatment and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4. The data exclude three practices (Atlanta, Chicago, and Stuart) that withdrew from the demonstration before Year 4 and one practice (Louisville) terminated for cause.

Notes: Standard errors are given in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers both 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 that is displayed as zero might be shown alongside a percentage effect that exceeds zero.

[a]We used the unadjusted treatment group mean during two years before the demonstration to calculate the percentage effect for each demonstration year. Table B.13 reports the baseline unadjusted treatment 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 the difference-in-differences estimate for each year was calculated as the difference in means between IAH and comparison beneficiaries in the two years before the demonstration minus the difference in the year before the demonstration.

[d]The probability of unplanned readmission for a beneficiary is measured over the IAH-eligible months during each demonstration year. The probability equals zero for beneficiaries who did not have a qualifying hospital discharge or an unplanned readmission within 30 days of a qualifying hospital discharge during the measurement period.

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

ED = emergency department; IAH = Independence at Home.

Table B.36. Estimated effect of IAH on visits to clinicians with two pre-demonstration years as the baseline, demonstration Years 1 through 4

Service type/period	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect[a]
Visits in nonacute settings by primary care clinicians[b] per beneficiary per year					
Year 4	12.87	6.23	6.64	1.64 (1.00)	15.0
Year 3	12.39	6.56	5.83	0.82 (0.63)	7.5
Year 2	12.24	6.55	5.69	0.69 (0.54)	6.3
Year 1	11.54	6.41	5.13	0.12 (0.34)	1.1
Pre-demonstration years[c]	11.42	6.42	5.01	-	-
Visits in nonacute settings by specialists per beneficiary per year					
Year 4	5.29	7.53	−2.24	−0.26 (0.38)	−4.7
Year 3	5.44	7.90	−2.46	−0.49 (0.31)	−8.8
Year 2	5.61	8.10	−2.49	−0.52* (0.29)	−9.4
Year 1	5.66	7.89	−2.23	−0.25 (0.29)	−4.6
Pre-demonstration years[c]	5.86	7.83	−1.97	-	-
Total unweighted number of observations across all years: 243,947					

Source: Mathematica analysis of data from the IAH implementation contractor and Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for treatment and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4. The data exclude three practices (Atlanta, Chicago, and Stuart) that withdrew from the demonstration before Year 4 and one practice (Louisville) terminated for cause.

Notes: Standard errors are given in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers both 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 that is displayed as zero might be shown alongside a percentage effect that exceeds zero.

[a]We used the unadjusted treatment group mean during two years before the demonstration to calculate the percentage effect for each demonstration year. Table B.13 reports the baseline unadjusted treatment group mean for all outcomes.

[b]Primary care clinicians are defined as primary care physicians, nurse practitioners, and physician assistants. Nonacute settings are defined as home, office, outpatient clinic, federally qualified health center, or rural health clinic.

[c]The difference-in-differences estimate for the period before the demonstration is zero (with no standard error) in all regressions, because the difference-in-differences estimate for each year was calculated as the difference in means between IAH and comparison beneficiaries in the two years before the demonstration minus the difference in the year before the demonstration.

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

IAH = Independence at Home.

Table B.37. Estimated effect of IAH on use of home health with two pre-demonstration years as the baseline, demonstration Years 1 through 4

Service type/period	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect[a]
Probability of home health use (percentage)					
Year 4	89.92	77.42	12.51	0.43 (0.80)	0.5
Year 3	90.98	79.14	11.84	−0.24 (0.77)	−0.3
Year 2	91.66	79.24	12.42	0.34 (0.70)	0.4
Year 1	91.13	78.89	12.24	0.16 (0.52)	0.2
Pre−demonstration years[b]	91.54	79.46	12.08	-	-
Home health days per beneficiary per year					
Year 4	157.37	103.64	53.72	−2.92 (7.36)	−1.7
Year 3	162.35	110.34	52.01	−4.64 (6.26)	−2.7
Year 2	182.13	117.86	64.27	7.63 (6.52)	4.5
Year 1	171.29	115.24	56.05	−0.60 (2.29)	−0.4
Pre-demonstration years[b]	179.91	123.27	56.64	-	-
Number of home health visits					
Year 4	60.04	41.08	18.97	−1.58 (4.43)	−2.4
Year 3	62.20	43.58	18.62	−1.92 (3.75)	−2.9
Year 2	71.45	46.27	25.19	4.65** (2.22)	7.1
Year 1	62.98	44.74	18.24	−2.30 (1.96)	−3.5
Pre-demonstration years[b]	69.44	48.90	20.54	-	-

Total unweighted number of observations across all years: 243,947

Source: Mathematica analysis of data from the IAH implementation contractor and Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for treatment and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4. The data exclude three practices (Atlanta, Chicago, and Stuart) that withdrew from the demonstration before Year 4 and one practice (Louisville) terminated for cause.

Notes: Standard errors are given in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers both 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 that is displayed as zero might be shown alongside a percentage effect that exceeds zero.

[a]We used the unadjusted treatment group mean during two years before the demonstration to calculate the percentage effect for each demonstration year. Table B.13 reports the baseline unadjusted treatment 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 the difference-in-differences estimate for each year was calculated as the difference in means between IAH and comparison beneficiaries in the two years before the demonstration minus the difference in the year before the demonstration.

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

IAH = Independence at Home.

Table B.38. Estimated effect of IAH on use of hospice and SNF services with two pre-demonstration years as the baseline, demonstration Years 1 through 4

Service type/period	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect[a]
Probability of hospice use (percentage)					
Year 4	17.18	15.37	1.81	−1.12 (0.77)	−7.1
Year 3	17.96	14.57	3.39	0.37 (0.82)	2.2
Year 2	17.56	13.68	3.88	0.86 (0.64)	5.1
Year 1	18.35	15.06	3.29	0.27 (0.71)	1.6
Pre-demonstration years[b]	18.12	15.10	3.02	-	-
Probability of SNF use (percentage)					
Year 4	43.60	55.83	−12.23	−0.64 (1.39)	−1.6
Year 3	44.72	56.57	−11.85	−0.26 (0.98)	−0.6
Year 2	43.04	53.87	−10.82	0.77 (1.22)	1.9
Year 1	43.28	55.87	−12.59	−0.99 (0.78)	−2.4
Pre-demonstration years[b]	43.84	55.44	−11.59	-	-
Total unweighted number of observations across all years: 243,947					

Source: Mathematica analysis of data from the IAH implementation contractor and Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for treatment and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4. The data exclude three practices (Atlanta, Chicago, and Stuart) that withdrew from the demonstration before Year 4 and one practice (Louisville) terminated for cause.

Notes: Standard errors are given in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers both 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 that is displayed as zero might be shown alongside a percentage effect that exceeds zero.

[a]We used the unadjusted treatment group mean during two years before the demonstration to calculate the percentage effect for each demonstration year. Table B.13 reports the baseline unadjusted treatment 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 the difference-in-differences estimate for each year was calculated as the difference in means between IAH and comparison beneficiaries in the two years before the demonstration minus the difference in the year before the demonstration.

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

IAH = Independence at Home; SNF = skilled nursing facility.

Table B.39. Estimated effect of IAH on 12-month mortality with two pre-demonstration years as the baseline, demonstration Years 1 through 4

Period	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect[a]
Year 4	17.62	21.26	−3.64	−0.51 (0.53)	−2.9
Year 3	19.27	20.60	−1.33	1.79*** (0.58)	10.2
Year 2	18.63	20.52	−1.89	1.24* (0.65)	7.1
Year 1	19.06	21.40	−2.34	0.79 (0.51)	4.5
Pre-demonstration years[b]	19.04	22.16	−3.13	-	-
Total unweighted number of observations across all years: 243,947					

Source: Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for treatment and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4, excluding Atlanta, Chicago, Louisville, and Stuart.

Notes: Standard errors are given in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers both 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 that is displayed as zero might be shown alongside a percentage effect that exceeds zero.

[a]We used the unadjusted treatment group mean during two years before the demonstration to calculate the percentage effect for each demonstration year. Table B.13 reports the baseline unadjusted treatment 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 the difference-in-differences estimate for each year was calculated as the difference in means between IAH and comparison beneficiaries in the two years before the demonstration minus the difference in the year before the demonstration.

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

IAH = Independence at Home.

Table B.40. Estimated effect of IAH on trimmed Medicare expenditures PBPM and utilization, demonstration Years 1 through 4

Period	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect[a]
Medicare expenditures PBPM					
Year 4	$4,394	$4,778	−$384	−$282 ($205)	−6.4
Year 3	$4,577	$4,857	−$280	−$178 ($158)	−4.1
Year 2	$4,737	$4,871	−$133	−$32 ($139)	−0.7
Year 1	$4,756	$4,977	−$221	−$120 ($97)	−2.7
One year pre-IAH[b]	$4,894	$4,995	−$101	-	-
Two years pre-IAH	$5,071	$5,206	−$135	−$33 ($57)	−0.8
Number of hospital admissions per beneficiary per year,[c] trimmed					
Year 4	1.73	2.05	−0.32	−0.11 (0.08)	−6.0
Year 3	1.87	2.17	−0.31	−0.09 (0.07)	−5.0
Year 2	1.90	2.15	−0.25	−0.03 (0.06)	−1.9
Year 1	1.94	2.21	−0.27	−0.05 (0.04)	−3.0
One year pre-IAH[b]	2.02	2.23	−0.22	-	-
Two years pre-IAH	2.10	2.36	−0.25	−0.04 (0.03)	−2.0
Number of total ED visits per beneficiary per year, trimmed					
Year 4	3.08	3.32	−0.24	−0.22** (0.09)	−7.5
Year 3	3.29	3.48	−0.18	−0.16* (0.09)	−5.6
Year 2	3.23	3.26	−0.03	−0.010 (0.07)	−0.3
Year 1	3.12	3.26	−0.14	−0.12 (0.08)	−4.2
One year pre-IAH[b]	3.25	3.27	−0.02	-	-
Two years pre-IAH	3.29	3.34	−0.05	−0.03 (0.05)	−0.9
Total unweighted number of observations across all years: 243,947					

Source: Mathematica's analysis of data from the IAH implementation contractor and Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for treatment and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4. The data exclude three practices (Atlanta, Chicago, and Stuart) that withdrew from the demonstration before Year 4 and one practice (Louisville) terminated for cause.

Notes: Standard errors are given in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers both 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 that is displayed as zero might be shown alongside a percentage effect that exceeds zero.

[a]We used the unadjusted treatment group mean in the year before the demonstration to calculate the percentage effect for each demonstration year. Table B.13 reports the baseline unadjusted treatment 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 the difference-in-differences estimate for each year was calculated as the difference in means between IAH and comparison beneficiaries in the two years before the demonstration minus the difference in the year before the demonstration.

[c]The number of hospital admissions includes observation stays.

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

ED = emergency department; IAH = Independence at Home; PBPM = per beneficiary per month.

Table B.41. Estimated effect of IAH on total Medicare expenditures PBPM and utilization for IAH beneficiaries from the baseline counties, demonstration Years 1 through 4

Service type/period	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect[a]
Total Medicare expenditures					
Year 4	$4,416	$4,802	−$385	−$283 ($204)	−6.4
Year 3	$4,598	$4,887	−$289	−$186 ($156)	−4.2
Year 2	$4,766	$4,893	−$128	−$26 ($136)	−0.6
Year 1	$4,784	$5,001	−$217	−$115 ($95)	−2.6
One year pre-IAH[b]	$4,918	$5,020	−$102	-	-
Two years pre-IAH	$5,095	$5,234	−$139	−$37 ($57)	−0.8
Number of hospital admissions per beneficiary per year[c]					
Year 4	1.75	2.06	−0.32	−0.10 0.08)	−5.5
Year 3	1.88	2.19	−0.31	−0.09 (0.07)	−5.1
Year 2	1.91	2.16	−0.25	−0.03 (0.06)	−1.8
Year 1	1.96	2.22	−0.27	−0.05 (0.04)	−2.7
One year pre-IAH[b]	2.03	2.25	−0.22	-	-
Two years pre-IAH	2.12	2.37	−0.25	−0.03 (0.03)	−1.8
Probability of having a qualifying hospital discharge and an unplanned readmission within 30 days of discharge (percentage)[d]					
Year 4	17.82	21.74	−3.92	−2.09* (1.22)	−10.7
Year 3	20.74	24.49	−3.75	−1.92** (0.90)	−9.8
Year 2	21.30	24.28	−2.98	−1.15 (1.18)	−5.9
Year 1	23.29	24.93	−1.64	0.19 (0.78)	0.9
One year pre-IAH[b]	22.97	24.79	−1.83	-	-
Two years pre-IAH	25.81	27.70	−1.90	−0.07 (0.79)	−0.3
Total number of ED visits per beneficiary per year[e]					
Year 4	3.10	3.33	−0.23	−0.21** (0.09)	−7.2
Year 3	3.29	3.49	−0.20	−0.18* (0.09)	−6.1
Year 2	3.23	3.27	−0.04	−0.020 (0.07)	−0.6
Year 1	3.14	3.28	−0.14	−0.12 (0.08)	−4.2
One year pre-IAH[b]	3.26	3.28	−0.02	-	-
Two years pre-IAH	3.32	3.36	−0.04	−0.02 (0.05)	−0.8
Total unweighted number of observations across all years: 237,985					

Source: Mathematica analysis of data from the IAH implementation contractor and Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for treatment and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4. The data exclude three practices (Atlanta, Chicago, and Stuart) that withdrew from the demonstration before Year 4 and one practice (Louisville) terminated for cause.

Notes: Standard errors are given in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers both 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 that is displayed as zero might be shown alongside a percentage effect that exceeds zero.

Table B.41 (continued)

[a]We used the unadjusted treatment group mean in the year before the demonstration to calculate the percentage effect for each demonstration year. Table B.13 reports the baseline unadjusted treatment 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 the difference-in-differences estimate for each year was calculated as the difference in means between IAH and comparison beneficiaries in that year minus the difference in the year before the demonstration.

[c]The number of hospital admissions includes observation stays.

[d]The probability of unplanned readmission for a beneficiary is measured over the IAH-eligible months during each demonstration year. The probability equals zero for beneficiaries who did not have a qualifying hospital discharge or an unplanned readmission within 30 days of a qualifying hospital discharge during the measurement period.

[e]The number of ED visits measure includes visits resulting in a hospital admission or an observation stay.

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

ED = emergency department; IAH = Independence at Home; PBPM = per beneficiary per month.

Table B.42. Estimated effect of IAH on total Medicare expenditures PBPM and utilization: Results using equal weights for all sites and years, demonstration Years 1 through 4

Service type/period	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect	Percentage effect[a]
Total Medicare expenditures					
Year 4	$4,300	$4,719	−$418	−$180 ($130)	−4.1
Year 3	$4,480	$4,789	−$309	−$71 ($102)	−1.6
Year 2	$4,614	$4,757	−$143	$95 ($110)	2.2
Year 1	$4,646	$4,988	−$342	−$104 ($124)	−2.4
One year pre-IAH[b]	$4,722	$4,960	−$238	-	-
Two years pre-IAH	$4,906	$5,162	−$256	−$17 ($110)	−0.4
Number of hospital admissions per beneficiary per year[c]					
Year 4	1.72	1.99	−0.27	−0.07 (0.06)	−3.5
Year 3	1.81	2.05	−0.23	−0.03 (0.07)	−1.8
Year 2	1.86	2.05	−0.18	0.02 (0.05)	1.0
Year 1	1.84	2.17	−0.32	−0.12 (0.09)	−6.7
One year pre-IAH[b]	2.00	2.20	−0.20	-	-
Two years pre-IAH	2.02	2.30	−0.28	−0.08 (0.06)	−4.2
Probability of having a qualifying hospital discharge and an unplanned readmission within 30 days of discharge (percentage)[d]					
Year 4	0.15	0.18	−0.02	−0.02* (0.01)	−12.4
Year 3	0.18	0.19	−0.01	−0.02 (0.01)	−8.4
Year 2	0.19	0.20	−0.01	−0.010 (0.01)	−4.6
Year 1	0.20	0.21	−0.01	−0.01 (0.02)	−7.5
One year pre-IAH[b]	0.22	0.21	0.00	-	-
Two years pre-IAH	0.22	0.24	−0.02	−0.02 (0.01)	−10.4
Total number of ED visits per beneficiary per year[e]					
Year 4	2.84	3.19	−0.35	−0.18** (0.08)	−6.4
Year 3	3.04	3.28	−0.24	−0.07 (0.13)	−2.5
Year 2	2.88	3.08	−0.20	−0.020 (0.08)	−0.8
Year 1	2.67	2.89	−0.22	−0.04 (0.19)	−1.4
One year pre-IAH[d]	2.99	3.17	−0.18	-	-
Two years pre-IAH	3.00	3.19	−0.19	−0.01 (0.07)	−0.5
Total unweighted number of observations across all years: 243,947					

Source: Mathematica's analysis of data from the IAH implementation contractor and Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for treatment and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4. The data exclude three practices (Atlanta, Chicago, and Stuart) that withdrew from the demonstration before Year 4 and one practice (Louisville) terminated for cause.

Notes: Standard errors are given in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers both 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 that is displayed as zero might be shown alongside a percentage effect that exceeds zero.

[a]We used the unadjusted treatment group mean in the year before the demonstration to calculate the percentage effect for each demonstration year. To calculate treatment group baseline means, we weighted beneficiaries using the same weights implemented

Table B.42 (continued)

in the equal-weight regressions, instead of the weights used in other regressions. For more information on the equal-weight regression and weights used in other regressions, see Section VI.A of this appendix.

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

[c]The number of hospital admissions includes observation stays.

[d]The probability of unplanned readmission for a beneficiary is measured over the IAH-eligible months during each demonstration year. The probability equals zero for beneficiaries who did not have a qualifying hospital discharge or an unplanned readmission within 30 days of a qualifying hospital discharge during the measurement period.

[e]The number of ED visits measure includes visits resulting in a hospital admission or an observation stay.

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

ED = emergency department; IAH = Independence at Home; PBPM = per beneficiary per month.

Table B.43. IAH practices' structural characteristics, as of 2017

Site	Year founded or began current management	Affiliation	Ownership	Full-time clinicians making house calls	Part-time clinicians making house calls	Other staff involved in care team
VPA practices						
Dallas, TX	1993	US Medical Management	For profit	17 clinicians[a]	None	18 MAs, 2 clinical educators on site, 1 scheduler, 1 patient care coordinator, 1 practice manager[b]
Flint, MI	1993	US Medical Management	For profit	23 clinicians[a]	None	24 MAs, 5 clinical educators on site, 1 scheduler, 1 patient care coordinator, 1 practice manager[b]
Jacksonville, FL	1993	US Medical Management	For profit	14 clinicians[a]	2 clinicians	10 MAs, 1 clinical educator on site, 1 scheduler, 1 patient care coordinator, 1 practice manager[b]
Lansing, MI	1993	US Medical Management	For profit	10 clinicians[a]	None	11 MAs, 2 clinical educators on site, 1 scheduler, 1 patient care coordinator, 1 practice manager[b]
Milwaukee, WI	1993	US Medical Management	For profit	12 clinicians[a]	None	11 MAs, 1 clinical educator on site, 1 scheduler, 1 patient care coordinator, 1 practice manager[b]
Academic medical centers						
Boston, MA	1875	Boston Medical Center	Nonprofit	None	6 physicians	5 nurses, 1 office manager, 3 ambulatory service representatives, 1 project coordinator
Cleveland, OH	2008	Cleveland Clinic	Nonprofit	7 physicians, 3 NPs	1 PA	3 RNs, 4 MAs, 1 nurse manager, 1 social worker, 3 schedulers, 1 pharmacist
Long Island, NY	2012	Northwell Health	Nonprofit	4 physicians, 2 NPs	2 physicians	6 nurses, 6 medical coordinators, 5 social workers, 1 clinical data analyst, 1 DME coordinator
Philadelphia, PA[c]	1994	University of Pennsylvania	Nonprofit	1 NP	3 physicians 1 NP	1 social worker
Richmond, VA[c]	1984	Virginia Commonwealth University	Nonprofit	2 physicians, 6 NPs	2 physicians 1 NP	2 RNs, 1 consulting pharmacist, 3 social workers, 1 office manager, 3 patient access representatives
Washington, DC[c]	1999	MedStar Health	Nonprofit	6 physicians, 5 NPs	1 NP	1 RN, 1 LPN, 5 MAs, 1 social worker, 1 outcomes analyst
Wilmington, DE	2007	Christiana Care Health Systems	Nonprofit	1 physician, 3 NPs	4 physicians 1 PA, 1 NP	1 phlebotomist, 4 RNs, 4 MAs, 3 social workers, 1 office manager

Table B.43 (continued)

Site	Year founded or began current management	Affiliation	Ownership	Full-time clinicians making house calls	Part-time clinicians making house calls	Other staff involved in care team
Independent practices						
Austin, TX	Late 1990s	Kindred Health Care	For profit	4 physicians 9 NPs, 4 PAs	2 physicians	5 LPNs, 2 MAs serving as patient service coordinators, 2 intake coordinators, 1 office manager, 1 medical records personnel
Brooklyn, NY	1968	None	For profit	10 physicians, 15 PAs, 9 NPs[d]	None[d]	Quality assurance nurse, patient liaison[d]
Durham, NC	2002	None	For profit	33 physicians, 35 PAs, 7 NPs	None	6 podiatrists; 2 psychologists; 1 social worker; 130 additional office support staff, 40 of whom are MAs serving in clinical service, management, and scheduling capacities
Portland, OR	1995	None	Nonprofit	4 physicians, 3 NPs, 1 PA	1 physician 1 PA, 3 NPs	17 RNs, 4 LPNs, 7 social workers, care coordinators, care coordinator supervisor, DME specialist

Source: Information from interviews with practice staff conducted in 2015 and 2017.

[a]VPAs did not provide a breakdown of physicians, NPs, and PAs.

[b]Additional care team staff are located at the corporate office in Troy, Michigan, and provide support to local sites: 1 social worker, 1 DME intake, 1 care manager.

[c]These three sites (Philadelphia, Richmond, and Washington, DC) are considered one practice for purposes of the demonstration.

[d]The Brooklyn, New York, site did not provide information in 2017 on the number of full- and part-time clinicians making house calls, or other staff involved in the care team.

DME = durable medical equipment; IAH = Independence at Home; LPN = licensed practical nurse; MA = medical assistant; NP = nurse practitioner; PA = physician assistant; RN = registered nurse; VPA = Visiting Physicians Association.

Table B.44. IAH practice care delivery: Operational characteristics, as of 2017

Practice site	Visits per clinician per day	Clinicians' panel size	Nonbillable visits	Trip fee	Weekend visits	After-hours visits	Practice clinicians' visits in settings other than home[a]
VPA practices							
Dallas, TX	8 or 9	175	Yes: clinical educator visits	No	Yes: for both regular and urgent or post-discharge visits	No	No
Flint, MI	8 or 9	175	Yes: clinical educator or social worker visits	No	Yes: for both regular and urgent or post-discharge visits	No	No
Jacksonville, FL	8 or 9	175	Yes: clinical educator visits	No	Yes: for both regular and urgent or post-discharge visits	No	No
Lansing, MI	8 or 9	175	Yes: clinical educator or social worker visits	No	Yes: for both regular and urgent or post-discharge visits	No	No
Milwaukee, WI	8 or 9	175	Yes: clinical educator visits	No	Yes: for both regular and urgent or post-discharge visits	No	No
Academic medical centers							
Boston, MA	4	90	Yes: visits from nurse care manager	No	No	Yes: for urgent visits only, uncommon	Yes
Cleveland, OH	6 or 7	150 to 200	No	No	Yes: for urgent or post-discharge only	Yes: for urgent and regular visits, uncommon	No
Long Island, NY	6	170	Yes: community paramedic visit for urgent issues only	No	No	No	No
Philadelphia, PA	6	140	No	No	Yes: for urgent or post-discharge only	Yes: for urgent visits only, uncommon	Yes
Richmond, VA	3 to 6	40	Yes: nurse visit, but uncommon	No	No	No	Yes
Washington, DC	6	150 to 170	Yes: social worker visit, as needed	No	Yes: for urgent or post-discharge only	Yes: for regular visits, uncommon	Yes
Wilmington, DE	6	90 to 120	Yes: RN or social worker visit as needed or requested by patients and caregivers	No	Yes: for urgent or post-discharge only	No	Yes
Independent practices							
Austin, TX	10	200	No	No	No	Yes: for urgent visits only, uncommon	No

Table B.44 (continued)

Practice site	Visits per clinician per day	Clinicians' panel size	Nonbillable visits	Trip fee	Weekend visits	After-hours visits	Practice clinicians' visits in settings other than home[a]
Brooklyn, NY	8 to 10	120 to 130	Yes: visits to uninsured patient; uncommon	No	Yes: for both regular and urgent or post-discharge visits	Yes: for urgent and regular visits, common	No
Durham, NC	10 to 15	150 to 200	No	Yes: for private residences only; majority of visits occur in ALFs	Yes: for urgent or post-discharge only	No	No
Portland, OR	4 or 5	80 to 120	Yes: RN, social worker, or chaplain visit as needed	No	Yes: for urgent or post-discharge only	No	No

ALF = assisted living facility; IAH = Independence at Home; RN = registered nurse; VPA = Visiting Physicians Association.

Table B.44 (continued)

Practice site	Formal risk-stratification classification	Remote access to patient's record, remote data collection, and remote submission of orders	Notification of hospitalization or ED visit	Participate in ACO (years)	Proactive outreach to patients or caregivers
VPA practices					
Dallas, TX	Yes: 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 (2)	Yes: call as needed based on acuity of patient
Flint, MI	Yes: 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	Yes (5)	Yes: call as needed based on acuity of patient
Jacksonville, FL	Yes: 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	Yes (2)	Yes: call as needed based on acuity of patient
Lansing, MI	Yes: 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	Yes (2)	Yes: call as needed based on acuity of patient
Milwaukee, WI	Yes: 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 (2)	Yes: call as needed based on acuity of patient
Academic medical centers					
Boston, MA	No: clinical judgment only	Yes	Automated notice from some sites	Yes[b]	Yes: call as needed based on care plan
Cleveland, OH	No: clinical judgment only	NA	Automated notice from some sites	Yes (1)	Yes: call twice weekly
Long Island, NY	Yes: determines level of proactive outreach and care team involvement	Yes	Automated notice from some sites	No	Yes: call as needed based on acuity of patient
Philadelphia, PA	No: clinical judgment only	No	Automated notice from some sites	No	Yes: call as needed based on clinician's judgment
Richmond, VA	No: clinical judgment only	Yes	Automated notice from some sites	No	No
Washington, DC	No: clinical judgment only	Yes	Automated notice from some sites	No	Yes: call monthly

Table B.44 (continued)

Practice site	Formal risk-stratification classification	Remote access to patient's record, remote data collection, and remote submission of orders	Notification of hospitalization or ED visit	Participate in ACO (years)	Proactive outreach to patients or caregivers
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	Yes (1)	Yes: call as needed for high-need patients; for those recently hospitalized, weekly in first month post-discharge and biweekly for second month post-discharge
Independent practices					
Austin, TX	Yes: assessment scores, hospitalization history, and clinical judgment used to assign level-of-risk score, which determines level of proactive outreach and care team involvement	Yes	Rely on patient or caregivers to notify practice	No	Yes: call weekly or biweekly based on acuity of patient
Brooklyn, NY	No: clinical judgment only	Yes	Automated notice from some sites	Yes (2)	No
Durham, NC	No: clinical judgment only	Yes	Notification from ALFs where majority of patients live	No	Yes: call as needed
Portland, OR	No: clinical judgment only	Yes	Automated notice from HIE	No	Yes: call as needed based on acuity of patient and if patient was recently hospitalized

Source: Information from interviews with practice staff conducted in 2015 and 2017. The Brooklyn, New York, site did not provide information in 2017.

[a]Refers to visits in nonhome locations such as office, outpatient clinic, inpatient hospital, or skilled nursing facility.

[b]Length of ACO involvement not available.

ACO = accountable care organization; ALF = assisted living facility; ED = emergency department; HIE = health information exchange; NA = not available; VPA = Visiting Physicians Association.

Table B.45. Estimated effect of IAH on hospital use for ACSCs, demonstration Years 1 through 4

Service type/period	IAH	Comparison	Difference (IAH - comparison)	Difference-in-differences estimated effect (standard error)	Percentage effect[a]
Number of hospital admissions for ACSCs per beneficiary per year[b]					
Four-year average annual effect[c]	0.42	0.48	−0.06	−0.03** (0.02)	−6.7
Year 4	0.41	0.51	−0.10	−0.07** (0.03)	−14.3
Year 3	0.47	0.55	−0.08	−0.04* (0.02)	−8.8
Year 2	0.49	0.54	−0.04	−0.01 (0.02)	−1.4
Year 1	0.53	0.58	−0.05	−0.01 (0.01)	−2.8
One year pre-IAH[d]	0.55	0.59	−0.04	-	-
Two years pre-IAH	0.58	0.65	−0.07	−0.03* (0.02)	−6.8
Number of outpatient ED visits for ACSCs per beneficiary per year[e]					
Four-year average annual effect[c]	0.21	0.22	−0.01	0.00 (0.01)	0.4
Year 4	0.23	0.27	−0.03	−0.02 (0.01)	−9.5
Year 3	0.24	0.25	−0.01	0.01 (0.02)	3.5
Year 2	0.24	0.24	0.01	0.02 (0.01)	10.8
Year 1	0.22	0.23	−0.01	0.01 (0.01)	3.0
One year pre-IAH[d]	0.22	0.23	−0.01	-	-
Two years pre-IAH	0.22	0.23	−0.01	0.01 (0.01)	4.6

Total number of observations across the five years: 243,947

Source: Mathematica analysis of data from the IAH implementation contractor and Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for treatment and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4. The data exclude three practices (Atlanta, Chicago, and Stuart) that withdrew from the demonstration before Year 4 and one practice (Louisville) terminated for cause.

Notes: An admission (or ED visit) for an ACSC is one in which appropriate primary and specialty care may prevent or reduce the need for a hospital admission (or ED visit). Standard errors are given in parentheses. We computed coefficients and standard errors by using the weighted sample size, which considers both 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. 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. Because of rounding, a difference-in-differences estimate that is displayed as zero might be shown alongside a percentage effect that exceeds zero.

[a]We used the unadjusted treatment group mean in the year before the demonstration to calculate the percentage effect for each demonstration year. Table B.13 reports the baseline unadjusted treatment 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 treatment status to obtain an average annual estimated effect across four demonstration years.

[d]The difference-in-differences estimate for the period before the demonstration is zero (with no standard error) in all regressions, because the difference-in-differences estimate for each year was calculated as the difference in means between IAH and comparison beneficiaries in the two years before the demonstration minus the difference in the year before the demonstration.

[e]The number of outpatient ED visits measures ED visits not resulting in hospital admission, including those resulting in observation stay. The measure excluded ED visits that led to an inpatient admission, because there was no diagnosis from the ED visit in a claim record when an ED visit led to an inpatient admission.

Table B.46 (continued)

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

ACSC = ambulatory care sensitive condition; ED = emergency department; IAH = Independence at Home.

Table B.46. IAH beneficiaries' demographic characteristics in Year 4 and one year pre-demonstration, by site

IAH practice	Proportion of IAH beneficiaries age 80 or older			Proportion of IAH beneficiaries dually eligible		
	One year pre-demonstration	Year 4	Percentage change	One year pre-demonstration	Year 4	Percentage change
VPA practices						
Dallas, TX	0.323	0.289	−10.5	0.570	0.578	1.4
Flint, MI	0.434	0.424	−2.3	0.402	0.368	−8.5
Jacksonville, FL	0.513	0.536	4.5	0.480	0.440	−8.3
Lansing, MI	0.527	0.422	−19.9	0.311	0.322	3.5
Milwaukee, WI	0.621	0.551	−11.3	0.261	0.303	16.1
Academic medical centers						
Boston, MA	0.686	0.765	11.5	0.420	0.530	26.2
Cleveland, OH	0.547	0.634	15.9	0.375	0.269	−28.3
Long Island, NY	0.772	0.762	−1.3	0.162	0.166	2.5
Philadelphia, PA	0.677	0.638	−5.8	0.246	0.345	40.2
Richmond, VA	0.552	0.459	−16.8	0.604	0.529	−12.4
Washington, DC	0.769	0.694	−9.8	0.594	0.567	−4.5
Wilmington, DE	0.515	0.465	−9.7	0.417	0.451	8.2
Independent practices						
Austin, TX	0.480	0.404	−15.8	0.430	0.373	−13.3
Brooklyn, NY	0.631	0.594	−5.9	0.345	0.403	16.8
Durham, NC	0.675	0.648	−4.0	0.254	0.304	19.7
Portland, OR	0.609	0.649	6.6	0.225	0.199	−11.6
Total	**0.517**	**0.507**	**−1.9**	**0.401**	**0.384**	**−4.2**

Source: Mathematica analysis of data from the IAH implementation contractor and Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for treatment and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4. The data exclude three practices (Atlanta, Chicago, and Stuart) that withdrew from the demonstration before Year 4 and one practice (Louisville) terminated for cause.

Notes: Percentage change refers to the difference between Year 4 and one year pre-demonstration. These figures apply to beneficiaries who met eligibility criteria for IAH in administrative data and were attributed to a demonstration site as described in Section II.B of this appendix.

IAH = Independence at Home; VPA = Visiting Physicians Association.

Table B.47. IAH beneficiaries' health status and functional status in Year 4 and one year pre-demonstration, by site

IAH practice	HCC score			Proportion of IAH beneficiaries requiring human assistance with at least 5 ADLs		
	One year pre-demonstration	Year 4	Percentage change	One year pre-demonstration	Year 4	Percentage change
VPA practices						
Dallas, TX	3.21	4.04	25.7	0.344	0.455	32.3
Flint, MI	3.82	4.43	16.0	0.549	0.604	10.0
Jacksonville, FL	3.51	4.01	14.3	0.561	0.585	4.3
Lansing, MI	3.72	4.26	14.5	0.572	0.617	7.9
Milwaukee, WI	3.37	3.65	8.2	0.675	0.638	−5.5
Academic medical centers						
Boston, MA	2.92	3.61	23.5	0.479	0.557	16.3
Cleveland, OH	3.89	3.84	−1.5	0.646	0.650	0.6
Long Island, NY	3.63	3.97	9.3	0.669	0.711	6.3
Philadelphia, PA	3.20	3.52	10.0	0.569	0.707	24.3
Richmond, VA	3.88	4.25	9.5	0.698	0.718	2.9
Washington, DC	3.91	3.94	0.8	0.650	0.687	5.7
Wilmington, DE	3.99	4.13	3.6	0.621	0.681	9.7
Independent practices						
Austin, TX	3.65	4.14	13.4	0.574	0.643	12.0
Brooklyn, NY	3.71	4.05	9.3	0.641	0.606	−5.5
Durham, NC	3.14	3.23	2.8	0.553	0.577	4.3
Portland, OR	3.21	3.36	4.6	0.675	0.731	8.3
Total	**3.52**	**3.93**	**11.6**	**0.550**	**0.593**	**7.8**

Source: Mathematica analysis of data from the IAH implementation contractor and Medicare claims and enrollment data for 2009–2016 obtained from the Virtual Research Data Center for treatment and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4. The data exclude three practices (Atlanta, Chicago, and Stuart) that withdrew from the demonstration before Year 4 and one practice (Louisville) terminated for cause.

Notes: Percentage change refers to the difference between Year 4 and one year pre-demonstration. These figures apply to beneficiaries who met eligibility criteria for IAH in administrative data and were attributed to a demonstration site as described in Section II.B of this appendix. To calculate HCC scores, we used the HCC model that CMS has used since 2012 for the Program of All-Inclusive Care for the Elderly (PACE). Changes in coding and population characteristics may increase the average risk score over time. Therefore, despite the fact that the average HCC score for IAH beneficiaries increased by 11.6 percent from the year before the demonstration to Year 4, it is likely that the average IAH beneficiary was not substantially sicker in Year 4.

ADLs = activities of daily living; HCC = Hierarchical Condition Category; IAH = Independence at Home; VPA = Visiting Physicians Association.

Table B.48. Proportion of Year 4 IAH beneficiaries living in a county that had zero IAH beneficiaries in the year before the demonstration

IAH practice	Proportion of Year 4 IAH beneficiaries
VPA practices	
Dallas, TX	0.028
Flint, MI	0.007
Jacksonville, FL	0.054
Lansing, MI	0.035
Milwaukee, WI	0.037
Academic medical centers	
Boston, MA	0.000
Cleveland, OH	0.003
Long Island, NY	0.204
Philadelphia, PA	0.017
Richmond, VA	0.035
Washington, DC	0.082
Wilmington, DE	0.009
Independent practices	
Austin, TX	0.050
Brooklyn, NY	0.027
Durham, NC	0.080
Portland, OR	0.058

Source: Mathematica analysis of data from the IAH implementation contractor and Medicare claims and enrollment data for 2011–2016 obtained from the Virtual Research Data Center for treatment and matched comparison group beneficiaries in all IAH practices that participated in demonstration Year 4. The data exclude three practices (Atlanta, Chicago, and Stuart) that withdrew from the demonstration before Year 4 and one practice (Louisville) terminated for cause.

Note: These figures apply to beneficiaries who met eligibility criteria for IAH in administrative data and were attributed to a demonstration site as described in Section II.B of this appendix.

IAH = Independence at Home; VPA = Visiting Physicians Association.

Table B.49. Mean percentage of visits per IAH beneficiary by site of care, demonstration Years 2 and 4

	Year 2				Year 4			
	Home	ALF[a]	Inpatient hospital	Office/ out-patient clinic	Home	ALF[a]	Inpatient hospital	Office/ out-patient clinic
VPA practices								
Dallas, TX	88.3	11.7	0.0	0.0	88.3	11.1	0.0	0.6
Flint, MI	62.5	37.3	0.0	0.1	77.6	22.4	0.0	0.1
Jacksonville, FL	69.0	30.9	0.0	0.0	68.0	32.0	0.0	0.0
Lansing, MI	62.3	37.7	0.0	0.0	65.5	34.3	0.0	0.1
Milwaukee, WI	28.5	71.5	0.0	0.0	27.9	70.6	0.0	1.5
Academic medical centers								
Boston, MA	77.7	4.1	14.3	3.3	79.5	4.4	8.4	5.2
Cleveland, OH	84.1	13.7	0.0	2.3	82.7	15.2	0.0	1.9
Long Island, NY	100.0	0.0	0.0	0.0	100.0	0.0	0.0	0.0
Philadelphia, PA	92.8	0.0	1.5	0.4	94.8	0.0	0.8	3.2
Richmond, VA	93.6	0.0	0.3	1.9	96.1	0.4	0.9	1.4
Washington, DC	87.9	0.5	10.8	0.7	86.6	0.0	12.0	1.4
Wilmington, DE	93.3	1.6	2.4	1.9	93.5	5.0	0.9	0.6
Independent practices								
Austin, TX	84.4	15.6	0.0	0.0	86.9	13.1	0.0	0.0
Brooklyn, NY	100.0	0.0	0.0	0.0	99.8	0.0	0.0	0.1
Durham, NC	10.7	84.4	0.0	5.0	9.4	86.9	0.0	3.2
Portland, OR	16.6	83.4	0.0	0.0	16.0	83.9	0.0	0.1
Mean per practice	**72.6**	**24.0**	**1.7**	**0.9**	**73.3**	**23.7**	**1.4**	**1.2**

Source: Medicare claims and enrollment data for 2013–2016 obtained from the Virtual Research Data Center.

Notes: All figures are the mean percentage of evaluation and management visits by IAH clinicians in all places per eligible IAH beneficiary enrolled in the demonstration in Years 2 and 4. We focused on those years because the reduction in expenditures due to the demonstration might have increased in that period. Because we do not present data on visits in other settings, such as SNFs, rows might not sum to 100 within a given year. Most sites had less than 0.5 percent of visits in other settings.

[a]ALF includes group homes and custodial care facilities.

ALF = assisted-living facility; IAH = Independence at Home; SNF = skilled nursing facility; VPA = Visiting Physicians Association.

APPENDIX C

BENEFICIARY AND CAREGIVER PERSPECTIVES ON THE IAH PROGRAM

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I. OVERVIEW

Understanding the impacts of the IAH demonstration on health care spending, provision of care, health outcomes, and service use is essential for measuring the demonstration's success and determining whether and how to expand the model of care. However, given that the IAH demonstration provides an incentive for the practices to reduce spending to qualify for shared savings, it is also important to review beneficiaries' and caregivers' experiences during the demonstration, including whether they were satisfied with the care they received. We discussed some data from the survey of IAH beneficiaries and caregivers in Chapter III. In this Appendix, we examine additional survey data on how beneficiaries and their caregivers viewed key features of the IAH program.

First, we examine characteristics of the IAH beneficiaries and their caregivers, including the caregiver's interactions with the beneficiaries. Next, we present information on beneficiaries' and caregivers' willingness to contact the IAH practice when they face uncertainty about whether the beneficiary needs care in the ED. Then, we look at access to care, patient engagement, and caregiver education. Finally, we examine the satisfaction of beneficiaries and caregivers with care from the IAH practice. As discussed previously, since we do not have survey data prior to the demonstration, we could not know whether beneficiaries' opinions of how clinicians give care changed relative to their opinions prior to the demonstration.

The sample frame for the survey was beneficiaries whose claims-based enrollment date for the demonstration, as reported by RTI/ARC, occurred from June 1, 2012, through June 30, 2014.

II. WHO WERE THE IAH BENEFICIARIES?

As expected, most beneficiaries who answered the survey needed substantial care. Almost half (47.7 percent) of the respondents reported needing human assistance with four or more ADLs (not shown). Beneficiaries were particularly likely to need help with bathing or showering (77.0 percent) and/or dressing (64.3 percent); a substantial minority (33.5 percent) needed human assistance with eating (Table C.1). Further, 63.4 percent received one or more types of assistance completing the survey, such as answering questions, reading questions, or writing answers (not shown). Roughly one of five of respondents lived alone in a private residence (19.8 percent), and the majority lived in assisted living (26.2 percent) or with one or more family members (44.6 percent). A small share (9.8 percent) of respondents indicated that they no longer receive in-home care from the IAH practice.

Two-thirds (67.3 percent) of respondents have received in-home care from the IAH practice for at least one year, which, for some respondents, predates their enrollment in the demonstration. Respondents typically indicated receiving care quite recently. More than 60 percent had an in-home visit from the IAH practice within the month before taking the survey, which corresponds with practices' reports of using regular visit schedules.

Table C.1. Use of in-home care and beneficiaries' characteristics

	Percentage of all nonmissing, valid responses[a]
Beneficiaries who need human assistance with ADLs[b,c]	
Bathing or showering	77.0
Dressing	64.3
Eating	33.5
Getting in or out of bed or chairs	52.9
Walking	54.1
Using the toilet	46.4
Beneficiary's current living situation[c]	
Lives alone in a private residence	19.8
Lives with family member	44.6
Lives with spouse or partner	19.6
Lives with child	19.1
Lives with parent	3.7
Lives with other family member	11.7
Assisted living	26.2
Other setting with non-family members	11.0
(Response missing or invalid)	2.8
Length of time beneficiary has received in-home care from the IAH practice	
Fewer than 6 months	6.2
6 months to less than 1 year	16.6
1 year or more	67.3
No longer gets in-home care from this practice	9.8
(Response missing or invalid)	3.9

Table C.1 (continued)

	Percentage of all nonmissing, valid responses[a]
Last time beneficiary received in-home care from the IAH practice	
Fewer than 2 weeks	39.3
2 weeks to less than 1 month	23.7
1 month to fewer than 2 months	16.8
2 months to fewer than 3 months	5.6
3 months or more	14.5
(Response missing or invalid)	4.9

Sources: IAH beneficiary surveys conducted from 2013 to 2015.

Note: Data are weighted for overall nonresponse. Mathematica sent the survey to all enrollees who were still alive at the time of the survey and whom Mathematica could locate, regardless of whether they were eligible for the demonstration according to Mathematica's criteria.

[a]The reported percentages are shown as a share of nonmissing values for each variable. The percentage of invalid responses for each variable is included in row labeled "Response missing or invalid" and includes cases in which the response was missing, the beneficiary provided multiple responses, or the beneficiary wrote "not applicable" when there was no such answer choice. Missing or invalid responses are shown as percentage of all surveys received (n = 3,870).

[b]Percentage of all respondents (n = 3,870) with missing or invalid responses for each activity of daily living ranged from 2.4 to 9.6 percent. The percentage of responses missing differs because we asked respondents separately about each item.

[c]Because we instructed respondents to mark all that apply, the figures for this variable sum to more than 100 percent.

ADL = activity of daily living; IAH = Independence at Home.

III. WHO WERE THE CAREGIVERS FOR THE IAH BENEFICIARIES?

Most caregivers who responded to the survey are members of the beneficiary's family and have helped the beneficiary for many years. Caregivers are typically unpaid and are present at most or all of the beneficiary's in-home visits. Only 10.3 percent of caregivers were not present at any of the beneficiary's home visits from the IAH practice (Table C.2). The caregiver is most often the beneficiary's child (40.4 percent) or spouse (16.7 percent). Nearly three-quarters (73.8 percent) of caregivers have helped the beneficiary for more than two years, including 48.2 percent who have helped for at least five years. Although only 12.3 percent of respondents are formal caregivers, more than one-quarter (28.4 percent) of caregivers said they are paid to help the beneficiary, which suggests that some family members are receiving pay for caring for their relative. Home health agencies are the most common payer, although one in five paid caregivers (22.6 percent) is compensated by the beneficiary or the beneficiary's family.

Most caregivers either live with the beneficiary or live nearby and are in contact with the beneficiary multiple times a week. About half of caregivers (48.7 percent) live with the beneficiary and, among that group, almost two-thirds (62.7 percent) have lived together for five years or more (Table C.3). Only a small number of caregivers have lived with the beneficiary for fewer than six months (4.5 percent of those who live together). Among the half of caregivers who do not live with the beneficiary, nearly all live less than an hour away: 65.4 percent live less than 20 minutes away and 26.5 percent live at least 20 minutes but less than an hour away. Caregivers who do not live with the beneficiary tend to be in contact with the beneficiary frequently: about 8 in 10 (79.1 percent) see the beneficiary a few times a week or more, and about 7 in 10 (67.9 percent) talk to the beneficiary on the phone a few times a week or more.

Table C.2. Caregiver's relationship to the beneficiary

	Percentage of all nonmissing, valid responses[a]	Percentage of nonmissing, valid responses from respondents receiving payment for care[b]
Caregiver's relationship to the beneficiary		
Child	40.4	-
Spouse	16.7	-
Parent	8.4	-
Other family member	12.1	-
Friend or neighbor	3.8	-
Formal caregiver	12.3	-
Other nonfamily member	6.4	-
(Response missing or invalid)	5.0	-
Length of time caregiver has been helping the beneficiary because of health or physical problems		
6 months or fewer	4.7	-
More than 6 months but less than 1 year	5.4	-
1 to 2 years	15.9	-
More than 2 years but fewer than 5 years	25.6	-
5 years or more	48.2	-
Whether caregiver is paid for helping the beneficiary		
Paid	28.4	-
Unpaid	71.6	-
(Response missing or invalid)	5.1	-
Source of payment to caregiver		
Home health agency	13.4	50.7
Beneficiary or beneficiary's family	6.0	22.6
Other	7.0	26.6
(Response missing or invalid)	7.7	9.5
Caregiver's presence at beneficiary's in-home visits from the IAH practice		
All visits	40.7	-
Most visits	25.6	-
Some visits	23.4	-
No visits	10.3	-
(Response missing or invalid)	4.9	-

Sources: IAH caregiver surveys conducted from 2013 to 2015.

Note: Data are weighted for overall nonresponse. Mathematica sent the IAH caregiver survey to caregivers of all enrollees who were still alive at the time of the survey and whom Mathematica could locate, regardless of whether the enrollee was eligible for the demonstration according to Mathematica's criteria.

[a]The reported percentages are shown as a share of nonmissing values. The percentage of invalid responses for each variable is included in the row labeled "Response missing or invalid" and includes cases in which the response was missing, the beneficiary provided multiple responses, or the beneficiary wrote "not applicable" when there was no such answer choice. Missing or invalid responses are shown as percentage of all surveys received (n = 2,519).

[b]The subset of caregivers to whom this question applied is the 28.4 percent who indicated they are paid for caregiving.

Table C.3. Caregiver's interactions with the beneficiary

	Percentage of all nonmissing, valid responses[a]	Percentage of nonmissing, valid responses from respondents to whom the question applied
Whether caregiver lives with beneficiary		
Yes	48.7	-
No	51.3	-
(Response missing or invalid)	13.6	-
Length of time caregiver and beneficiary have lived together		
6 months or less	2.1	4.5
More than 6 months but less than 1 year	2.3	4.8
1 to 2 years	5.8	12.2
More than 2 years but fewer than 5 years	7.5	15.8
5 years or more	29.7	62.7
(Response missing or invalid)	15.8	5.3
Travel time from caregiver's home to beneficiary's home		
Less than 20 minutes	33.5	65.4
At least 20 minutes but less than 1 hour	13.5	26.5
1 to 2 hours	2.4	4.8
More than 2 hours	1.8	3.3
(Response missing or invalid)	14.7	2.0
Frequency of caregiver's talks with beneficiary by phone		
Every day or almost every day	22.6	46.2
A few times a week	10.3	21.7
Once a week	3.2	6.7
Less than once a week but at least once a month	3.2	6.9
Less than once a month	8.8	18.5
(Response missing or invalid)	19.9	13.9
Frequency of caregiver's in-person visits with beneficiary		
Every day or almost every day	24.0	45.9
A few times a week	17.0	33.2
Once a week	6.0	11.7
Less than once a week but at least once a month	4.0	7.9
Less than once a month	0.6	1.3
(Response missing or invalid)	14.3	1.4

Source: IAH caregiver surveys conducted from 2013 to 2015.

Notes: Data are weighted for overall nonresponse. Mathematica sent the IAH caregiver survey to caregivers of all enrollees who were still alive at the time of the survey and whom Mathematica could locate, regardless of whether the enrollee was eligible for the demonstration according to Mathematica's criteria.

[a]The presented percentages are shown as a share of nonmissing values. The percentage of invalid responses for each variable is included in the row labeled "Response missing or invalid" and includes cases in which the response was missing, the beneficiary provided multiple responses, or the beneficiary wrote "not applicable" when there was no such answer choice. Missing or invalid responses are shown as percentage of all surveys received (n = 2,519).

[b]The subset of caregivers to whom this question applied is the 48.7 percent who indicated they live with the beneficiary.

[c]The subset of caregivers to whom this question applied is the 51.3 percent who indicated that they do not live with the beneficiary.

IAH = Independence at Home.

IV. WOULD BENEFICIARIES AND CAREGIVERS CONTACT THE PRIMARY CARE TEAM IF THEY WERE UNSURE WHETHER THE PROBLEM WAS SERIOUS ENOUGH TO GO TO THE ED?

The demonstration requires that demonstration practices offer their IAH beneficiaries 24-hour accessibility To understand whether beneficiaries know about this requirement and would use it to avoid potentially unnecessary visits to the ED, we asked IAH beneficiaries and caregivers whether they would contact the primary care team if they were unsure whether the problem was serious enough to go to the ED. We asked them separately whether they would contact the primary care team during a weekday and separately whether they would do so at night or during the weekend.

Most IAH beneficiaries (77.9 percent) know how to contact their primary care team at night or during the weekend (Table C.4). In addition, most beneficiaries said that if they were not feeling well and were unsure whether they needed to go to the ED, they would contact the IAH practice: 80.0 percent would do this during a weekday and 70.5 percent would do this during a weeknight or weekend. However, a sizeable minority indicated a preference for visiting the ED in this situation, rather than contacting the IAH practice. Beneficiaries commonly cited two reasons why they would not contact the IAH practice during a weeknight or weekend: (1) the beneficiary's caregiver or someone else prefers the beneficiary goes to the ED; and (2) the beneficiary feels that the ED is the best place, providing better and more convenient care when it is unclear whether the problem is serious. The two most common reasons beneficiaries cited for not contacting the IAH practice during a weekday were the same, but beneficiaries were less likely during a weekday to prefer the ED when it is unclear whether the problem is serious. Almost 9.0 percent and 14.5 percent of beneficiaries said the ED was the best place for them during a weekday or weeknight/weekend, respectively, if they were unsure whether their problem was serious. When asked why beneficiaries would not call the IAH practice, they cited the inability to talk to someone familiar on the primary care team and too long of a wait to receive help from the primary care team as reasons for not calling at night or during a weekend rather than during a weekday.

Table C.4. Beneficiaries' willingness to contact the IAH practice if they are not feeling well and are unsure whether they need to go to the ED

	Percentage of all nonmissing, valid responses[a]	Percentage of nonmissing, valid responses from respondents who would not contact primary care team
Whether beneficiary knows how to contact primary care team during a *weeknight or weekend*		
Yes	77.9	-
No	22.0	-
(Response missing or invalid)	4.3	-
Whether beneficiary would contact primary care team during a *weeknight or weekend*		
Yes	70.5	-
No	29.5	-
(Response missing or invalid)	7.8	-
Reasons why beneficiary would not contact primary care team during a *weeknight or weekend*[b,c]		
I don't want to bother primary care team	6.1	22.4
It takes too long to get help	11.9	44.5
When I call, I cannot talk to someone I know	10.9	41.2
It is hard to remember to call primary care team when I am not feeling well	6.4	23.7
The ED is the best place for me when I am unsure whether my problem is serious	14.5	53.7
Caregiver, family member, or friend prefers that I go to the ED	15.3	56.3
I get better care in the ED	12.1	44.6
The ED is more convenient	12.5	46.0
Whether beneficiary would contact primary care team during a *weekday*		
Yes	80.0	-
No	20.0	-
(Response missing or invalid)	6.8	-
Reasons why beneficiary would not contact primary care team during a *weekday*[d,e]		
I don't want to bother primary care team	3.0	15.2
It takes too long to get help	7.6	40.1
When I call, I cannot talk to someone I know	6.4	33.9
It is hard to remember to call primary care team when I am not feeling well	5.4	28.5
The ED is the best place for me when I am unsure whether my problem is serious	8.7	45.8
Caregiver, family member, or friend prefers that I go to the ED	11.1	57.3
I get better care in the ED	8.4	44.4
The ED is more convenient	8.4	44.2

Source: IAH beneficiary surveys conducted from 2013 to 2015.

Notes: Data are weighted for overall nonresponse. Mathematica sent the survey to all enrollees who were still alive at the time of the survey and whom Mathematica could locate, regardless of whether they were eligible for the demonstration according to Mathematica's criteria.

[a]The presented percentages are shown as a share of nonmissing values for each variable. The percentage of invalid responses for each variable is included in row labeled "Response missing or invalid" and includes cases in which the response was missing, the

Table C.4 (continued)

beneficiary provided multiple responses, or the beneficiary wrote "not applicable" when there was no such answer choice. Missing or invalid responses are shown as percentage of all surveys received (n = 3,870).

[b]The subset of respondents to whom this question applied is the 29.5 percent who answered "No" when asked whether the beneficiary would contact his or her primary care team during a weeknight or weekend if a beneficiary is not feeling well and is unsure whether he or she needs to go to the ED.

[c]Percentage of all respondents (n = 3,870) with missing or invalid responses for each reason ranged from 10.7 to 11.5 percent. Among respondents who reported that they would not contact primary care team during a weeknight or weekend if a beneficiary is not feeling well and is unsure whether he or she needs to go to the ED, the percentage with missing or invalid responses for each reason ranged from 10.6 to 13.5 percent. There are differences in the percentage of responses missing because we asked respondents separately about each item.

[d]The subset of respondents to whom this question applied is the 20.0 percent who answered "No" when asked whether the beneficiary would contact his or her primary care team during a weekday if a beneficiary is not feeling well and is unsure whether he or she needs to go to the ED.

[e]Percentage of all respondents (n = 3,870) with missing or invalid responses for each reason ranged from 7.5 to 8.0 percent. Among respondents who reported that they would not contact primary care team during a weeknight or weekend if a beneficiary is not feeling well and is unsure whether he or she needs to go to the ED, the percentage with missing or invalid responses for each reason ranged from 11.3 and 13.3 percent. There are differences in the percentage of responses missing because we asked respondents separately about each item.

ED = emergency department; IAH = Independence at Home.

Awareness about how to contact the primary care team at night or during the weekend is high among IAH caregivers. Most caregivers said that if the beneficiary was not feeling well and the caregiver was unsure whether an ED visit was necessary, the caregiver would contact the IAH practice (Table C.5). Although most caregivers said they would contact the IAH practice if the beneficiary was not feeling well during a weeknight or weekend, the percentage who said they would not call was substantially larger if the situation arose during a weekday (27.1 versus 15.0 percent). More than half of the caregivers who said they would not contact the IAH practice said that the ED is the best place for the beneficiary when the caregiver is unsure whether the problem is serious (almost 60 percent during any time of the day or week). The second most commonly cited reason among caregivers who said they would not contact the IAH practice is related to the availability of the primary care team: about 4 in 10 said that when they call, it takes too long to get help. These answers are very similar to the beneficiary survey, in which about 57 percent of beneficiaries who said they would not contact the IAH practice reported that their caregiver, family member, or friend prefers that they go to the ED, and about 40 percent said that it takes too long to get help during any time of the day or week (Table C.4).

Table C.5. Caregivers' willingness to contact the IAH practice if a beneficiary is not feeling well and caregivers are unsure whether the beneficiary needs to go to the ED

	Percentage of all nonmissing, valid responses[a]	Percentage of nonmissing, valid responses from respondents who would not contact team
Whether caregiver knows how to contact primary care team during a *weeknight or weekend*		
Yes	85.0	-
No	15.0	-
(Response missing or invalid)	4.9	-
Whether caregiver would contact primary care team during a *weeknight or weekend*		
Yes	72.8	-
No	27.1	-
(Response missing or invalid)	7.0	-
Reasons why caregiver would not contact primary care team during a *weeknight or weekend*[b,c]		
I don't want to bother primary care team	2.8	11.5
It takes too long to get help	10.3	41.6
When I call, I cannot talk to someone I know	8.6	34.9
It is hard to remember to call primary care team when the beneficiary is not feeling well	3.1	12.8
The beneficiary prefers to go to the ED	7.9	32.2
The ED is the best place for the beneficiary when I am unsure whether the problem is serious	15.0	59.8
The beneficiary gets better care in the ED	8.4	33.9
The ED is more convenient	8.5	34.8
Whether caregiver would contact primary care team during a *weekday*		
Yes	84.1	-
No	15.9	-
(Response missing or invalid)	5.6	-
Reasons why caregiver would not contact primary care team during a *weekday*[d,e]		
I don't want to bother primary care team	1.0	5.7
It takes too long to get help	5.7	37.8
When I call, I cannot talk to someone I know	4.4	27.4

Table C.5 (continued)

	Percentage of all nonmissing, valid responses[a]	Percentage of nonmissing, valid responses from respondents who would not contact team
It is hard to remember to call primary care team when the beneficiary is not feeling well	1.8	11.2
The beneficiary prefers to go to the ED	5.0	30.5
The ED is the best place for the beneficiary when I am unsure whether the problem is serious	9.1	56.8
The beneficiary gets better care in the ED	5.8	36.3
The ED is more convenient	5.5	34.4

Sources: IAH caregiver surveys conducted from 2013 to 2015.

Note: Data are weighted for overall nonresponse. Mathematica sent the IAH caregiver survey to caregivers of all enrollees who were still alive at the time of the survey and whom Mathematica could locate, regardless of whether the enrollee was eligible for the demonstration according to Mathematica's criteria.

[a]The presented percentages are shown as a share of nonmissing values for each variable. The percentage of invalid responses for each variable is included in row labeled "Response missing or invalid" and includes cases in which the response was missing, the caregiver provided multiple responses, or the caregiver wrote "not applicable" when there was no such answer choice. Missing or invalid responses are shown as percentage of all surveys received (n = 2,519).

[b]The subset of caregivers to whom this question applied is 27.1 percent who answered "No" when asked whether the caregiver would contact the primary care team during a weeknight or weekend if a beneficiary is not feeling well and the caregiver is unsure whether the beneficiary needs to go to the ED.

[c]Percentage of all respondents (n = 2,519) with missing or invalid responses for each reason ranged from 9.8 to 10.2 percent. Among respondents who reported that they would not contact the primary care team during a weeknight or weekend if a beneficiary is not feeling well and the caregiver is unsure whether the beneficiary needs to go to the ED, the percentage with missing or invalid responses for each reason ranged from 10.0 to 12.9 percent. There are differences in the percentage of responses missing because we asked caregivers separately about each item.

[d]The subset of caregivers to whom this question applied is the 15.9 percent who answered "No" when asked whether the caregiver would contact the primary care team during a weekday if a beneficiary is not feeling well and the caregiver is unsure whether the beneficiary needs to go to the ED.

[e]Percentage of all respondents (n = 2,519) with missing or invalid responses for each reason ranged from 4.3 to 4.6 percent. Among respondents who reported that they would not contact primary care team during a weeknight or weekend if a beneficiary is not feeling well and the caregiver is unsure whether the beneficiary needs to go to the ED, the percentage with missing or invalid responses for each reason ranged from 8.0 and 9.2 percent. There are differences in the percentage of responses missing because we asked respondents separately about each item.

ED = emergency department; IAH = Independence at Home.

The proportions of beneficiaries and caregivers who were knowledgeable about and willing to contact the IAH practice in a potentially emergent situation were similarly high, and reasons cited for not contacting the primary care team were similar. Among beneficiaries who would not contact the IAH practice, the most common reason cited was the caregiver's belief that the ED was the most appropriate place to get care in this situation. Similarly, among caregivers who would not contact the IAH practice, the most common reason cited was their belief that the ED was the most appropriate place. About 40 percent of beneficiaries and caregivers said they would not contact the primary care team because it takes too long to get help. A preference among some beneficiaries and caregivers to go to the ED when they are unsure whether the problem is serious and a concern that it takes too long to get help—particularly on nights and weekends—might contribute to the demonstration's lack of an impact on outpatient ED visits (see Chapter II for more details).

V. HOW DID BENEFICIARIES AND CAREGIVERS VIEW ACCESS TO CARE UNDER THE DEMONSTRATION?

One of the core requirements of the demonstration is that practices improve beneficiaries' access to care via direct service provision in the beneficiary's home and coordination of services. Therefore, we asked beneficiaries and caregivers to rate the ease of obtaining in-home care from the IAH practice.

The vast majority of beneficiaries indicated they do not have trouble obtaining the in-home care they need from the IAH practice. Only a small share of beneficiaries (12.6 percent) said they have had trouble obtaining the in-home care they needed from the IAH practice in the past six months (Table C.6) Among the respondents who have had trouble getting needed in-home care, the most common reason was an issue of availability of the primary care team—specifically, they thought the wait was too long or the team was too busy.

In addition to providing in-home care, another way IAH practices can improve access to care is to help beneficiaries obtain care from other providers. More than half of the beneficiaries reported the primary care team provided a significant amount of help (36.1 percent) or some help (19.0 percent) arranging to obtain medical care they needed outside the home. However, 13.4 percent said their IAH practice provided no help arranging to obtain medical care they needed outside the home. Nearly one-quarter (23.4 percent) of respondents said they have not needed care outside the home.

Table C.6. Beneficiaries' perceptions of access to in-home care and coordination of care outside the home

	Percentage of all nonmissing, valid responses[a]	Percentage of nonmissing, valid responses from respondents having trouble getting in-home care[b]
Whether beneficiary had trouble getting in-home care they needed from the IAH practice in the past six months		
Yes	12.6	-
No	87.4	-
(Response missing or invalid)	6.3	-
Reasons why beneficiary had trouble getting in-home care they needed from the IAH practice in the past six months[c]		
Not enough money	2.2	20.0
Cost too high	1.9	17.0
Needed services or supplies that were not covered	2.8	25.5
Trouble contacting office to make appointments	3.7	33.0
Difficult to find appointment times	2.5	22.7
No treatment available	1.7	15.3
Wait too long or primary care team too busy	4.3	38.6
Other	5.8	52.0
(Response missing or invalid)	7.8	12.6
Amount of help from primary care team when making plans to get medical care outside the home		
A lot	36.1	-

Table C.6 (continued)

	Percentage of all nonmissing, valid responses[a]	Percentage of nonmissing, valid responses from respondents having trouble getting in-home care[b]
Some	19.0	-
A little	8.0	-
Not at all	13.4	-
Have not needed care outside the home	23.4	-
(Response missing or invalid)	5.1	-

Sources: IAH beneficiary surveys conducted from 2013 to 2015.

Note: Data are weighted for overall nonresponse. Mathematica sent the survey to all enrollees who were still alive at the time of the survey and whom Mathematica could locate, regardless of whether they were eligible for the demonstration according to Mathematica's criteria.

[a]The presented percentages are shown as a share of nonmissing values for each variable. The percentage of invalid responses for each variable is included in row labeled "Response missing or invalid" and includes cases in which the response was missing, the beneficiary provided multiple responses, or the beneficiary wrote "not applicable" when there was no such answer choice. Missing or invalid responses are shown as percentage of all surveys received (n = 3,870).

[b]The subset of respondents to whom this question applied is the 12.6 percent who answered "Yes" when asked whether the beneficiary had trouble getting in-home care they needed from the IAH practice in the past six months.

[c]Because we instructed respondents to mark all that apply, the figures for this variable might sum to more than 100 percent.

IAH = Independence at Home.

Few caregivers reported that beneficiaries had trouble obtaining the in-home care they needed from the IAH practice, and many said that when beneficiaries need care outside the home, the practice helps arrange that care. Of the caregivers who said the beneficiary has had trouble obtaining care, the most common reasons were that the wait was too long or the primary care team was too busy (32.5 percent; Table C.7); of note, 38.6 percent of beneficiaries who reported they had trouble obtaining care also cited these reasons (Table C.6).

Table C.7. Caregivers' perceptions of access to in-home care and coordination of care outside the home

	Percentage of all nonmissing, valid responses[a]	Percentage of nonmissing, valid responses from respondents having trouble getting in-home care[b]
Whether beneficiary had trouble getting in-home care they needed fl'mrom the IAH practice in the past six months		
Yes	11.0	-
No	89.0	-
(Response missing or invalid)	4.2	-
Reasons why beneficiary had trouble getting in-home care they needed from the IAH practice in the past six months[c]		
Not enough money	1.6	15.6
Cost too high	1.1	10.8
Needed services or supplies that were not covered	2.1	21.3
Trouble contacting office to make appointments	2.1	20.4

Table C.7 (continued)

	Percentage of all nonmissing, valid responses[a]	Percentage of nonmissing, valid responses from respondents having trouble getting in-home care[b]
Difficult to find appointment times	1.3	12.8
No treatment available	0.7	6.8
Wait too long or primary care team too busy	3.3	32.5
Other	4.4	43.6
(Response missing or invalid)	5.1	9.1
Amount of help from primary care team when making plans to get medical care outside the home		
A lot	44.1	-
Some	21.4	-
A little	9.0	-
Not at all	11.5	-
Beneficiary has not needed care outside the home	13.9	-
(Response missing or invalid)	11.7	-

Sources: IAH caregiver surveys conducted from 2013 to 2015.

Note: Data are weighted for overall nonresponse. Mathematica sent the IAH caregiver survey to caregivers of all enrollees who were still alive at the time of the survey and whom Mathematica could locate, regardless of whether the enrollee was eligible for the demonstration according to Mathematica's criteria.

[a]The presented percentages are shown as a share of nonmissing values for each variable. The percentage of invalid responses for each variable is included in row labeled "Response missing or invalid" and includes cases in which the response was missing, the beneficiary provided multiple responses, or the beneficiary wrote "not applicable" when there was no such answer choice. Missing or invalid responses are shown as percentage of all surveys received (n = 2,519).

[b]The subset of caregivers to whom this question applied is the 11.0 percent who answered "Yes" when asked whether the beneficiary had trouble getting in-home care they needed from the IAH practice in the past six months.

[c]Because we instructed caregivers to mark all that apply, the figures for this variable might sum to more than 100 percent.

IAH = Independence at Home.

Beneficiaries and caregivers agreed that in-home care was generally not difficult to attain, but for those who experienced problems, the most common barrier to in-home care was waiting too long for services. A somewhat higher share of beneficiaries than caregivers reported having trouble contacting the office to make appointments (33.0 versus 20.4 percent, respectively). Although most beneficiaries and caregivers reported that the practice provided at least some help when it was necessary to obtain medical care outside the home, about 20 percent of both groups reported that the practice provided a little or no help.

VI. TO WHAT EXTENT WERE THE IAH PRACTICES TAKING INTO ACCOUNT PATIENTS' PREFERENCES, PROMOTING PATIENT ENGAGEMENT, AND EDUCATING CAREGIVERS?

IAH practices are expected to provide patient-centered care (that is, care tailored to an individual beneficiary's chronic conditions and preferences), which could include coaching beneficiaries and caregivers on how to handle symptoms that emerge and manage medications. Because the demonstration provides an incentive for IAH practices to reduce their patients' Medicare spending, it is important to know whether IAH beneficiaries and caregivers believe the primary care team takes into account their opinions and preferences and engages them in making health care decisions.

A large majority of IAH beneficiaries believe the primary care team takes into account their opinions and preferences and engages them in making health care decisions. However, some beneficiaries expressed a preference for having a better understanding of how to take their medications. More than 85 percent of respondents agreed that in the past six months, the practice has taken into account their ideas and opinions; helped them learn how to avoid accidents or illnesses and maintain their health; and explained what to do if problems or symptoms continue, get worse, or return (Table C.8). Similarly, more than 90 percent of beneficiaries who talked with their primary care team about starting or stopping a prescription medication said the team took into account their opinion. A sizeable minority (30 percent) of respondents reported that they did not need help from the IAH practice to understand how to take their medications. Regarding the 70 percent of respondents who felt they needed help, the vast majority reported that the primary care team has provided some or significant help (57.8 percent of all respondents, which accounted for 82.6 percent of the respondents who felt they needed help).

Table C.8. Patients' preferences and engagement based on interactions in the past six months

	Percentage of all nonmissing, valid responses[a]	Percentage of nonmissing, valid responses from respondents deciding whether to change medication[b]
Primary care team has taken beneficiary's opinions into account when planning care		
Yes	89.0	-
No	11.0	-
(Response missing or invalid)	5.8	-
Primary care team has helped beneficiary plan how to avoid accidents/illnesses and maintain health		
Yes	86.0	-
No	14.0	-
(Response missing or invalid)	5.5	-
Primary care team has explained to beneficiary what to do if problems or symptoms continue, get worse, or return		
Yes	86.2	-
No	13.7	-
(Response missing or invalid)	6.0	-

Table C.8 (continued)

	Percentage of all nonmissing, valid responses[a]	Percentage of nonmissing, valid responses from respondents deciding whether to change medication[b]
Primary care team has talked with beneficiary about starting or stopping a prescription medication		
Yes	56.8	-
No	43.2	-
(Response missing or invalid)	6.9	-
If starting or stopping a medication was discussed with the beneficiary, the primary care team has taken the beneficiary's opinion into account		
Yes	49.1	92.6
No	3.9	7.4
(Response missing or invalid)	14.4	14.2
Amount of help from primary care team regarding understanding how to take medications		
A lot	40.9	-
Some	16.9	-
A little	7.3	-
Not at all	4.9	-
Have not needed help understanding how to take medication	30.0	-
(Response missing or invalid)	5.6	-
Frequency with which the beneficiary would like the primary care team to visit him or her		
More often than they have been visiting	18.6	-
About as often as they have been visiting	76.4	-
Less often than they have been visiting	4.9	-
(Response missing or invalid)	6.3	-

Sources: IAH beneficiary surveys conducted from 2013 to 2015.

Note: Data are weighted for overall nonresponse. Mathematica sent the survey to all enrollees who were still alive at the time of the survey and whom Mathematica could locate, regardless of whether they were eligible for the demonstration according to Mathematica's criteria.

[a]The presented percentages are shown as a share of nonmissing values for each variable. The percentage of invalid responses for each variable is included in row labeled "Response missing or invalid" and includes cases in which the response was missing, the beneficiary provided multiple responses, or the beneficiary wrote "not applicable" when there was no such answer choice. Missing or invalid responses are shown as percentage of all surveys received (n = 3,870).

[b]The subset of respondents to whom this question applied is the 56.8 percent who answered "Yes" when asked whether the primary care team has talked with the beneficiary about starting or stopping a prescription medication in the past six months.

IAH = Independence at Home.

Although most caregivers reported that the IAH practice has incorporated the beneficiary's preferences and educated the caregiver about how to address the beneficiary's health problems or symptoms, fewer reported that their understanding of the beneficiary's treatment or care plan has changed because of interactions with the primary care team. The majority (76.6 percent) of caregivers said that the primary care team has taken into account the beneficiary's opinion when planning care (Table C.9). If we remove cases in which the beneficiaries are unable to express preferences about their care plan because of their health, the figure rises to 93.2 percent. More than 8 in 10 (82.8 percent) caregivers reported that the primary care team has explained what the caregivers should do if the beneficiary's problems or symptoms continue, get worse, or return.

Table C.9. Incorporating patients' preferences and caregivers' activation based on interactions in the past six months

	Percentage of all nonmissing, valid responses[a]
Primary care team has taken beneficiary's opinion into account when planning care	
Yes	76.6
No	5.6
Beneficiary is unable to plan his or her care because of his or her health	17.8
(Response missing or invalid)	8.9
Primary care team has explained to caregiver what to do if problems or symptoms continue, get worse, or return	
Yes	82.8
No	17.2
(Response missing or invalid)	2.5

Sources: IAH caregiver surveys conducted from 2013 to 2015.

Note: Data are weighted for overall nonresponse. Mathematica sent the IAH caregiver survey to caregivers of all enrollees who were still alive at the time of the survey and whom Mathematica could locate, regardless of whether the enrollee was eligible for the demonstration according to Mathematica's criteria.

[a]The presented percentages are shown as a share of nonmissing values for each variable. The percentage of invalid responses for each variable is included in row labeled "Response missing or invalid" and includes cases in which the response was missing, the beneficiary provided multiple responses, or the beneficiary wrote "not applicable" when there was no such answer choice. Missing or invalid responses are shown as percentage of all surveys received (n = 2,519).

IAH = Independence at Home.

Beneficiaries and caregivers had similar positive beliefs about the extent to which the primary care team takes into account their opinions and preferences; educates them about how to prevent accidents, handle symptoms, and manage medications; and engages them in making health care decisions.

VII. WERE BENEFICIARIES AND CAREGIVERS SATISFIED WITH THE CARE IAH PRACTICES PROVIDED?

Understanding whether beneficiaries and caregivers are satisfied with the in-home care the IAH practices provided and whether they prefer the service to office-based care is essential to assessing the value added by home-based care. It is also helpful in evaluating whether the demonstration payment incentive had unintended negative consequences for beneficiaries.

Beneficiaries and caregivers might prefer home-based care provided under the demonstration to traditional primary care delivered in an office or clinic for several reasons. For example, home-based care could be more convenient because it reduces the need for transportation to appointments and eliminates waiting room time. In addition, the demonstration requires beneficiaries to have round-the-clock access to an on-call clinician, a service that other primary care practices might not offer. Nonetheless, beneficiaries might appreciate that medical appointments with an office-based practice provide a reason to leave their home, which can be an important quality-of-life issue for disabled beneficiaries. In addition, a beneficiary or caregiver might be concerned that receiving home-based care limits the beneficiary's access to diagnostic and other services.

The vast majority of IAH beneficiaries were satisfied with the quality of care they received from the IAH practices and with in-home care in general. Half of beneficiaries (50.2 percent) were very satisfied and another 42.7 percent were satisfied with the overall quality of the care they had received from the IAH practice in the past six months (Table C.10). Beneficiaries were even more positive about in-home care in general; nearly three-quarters (72.5 percent) liked receiving primary care in their home "a lot more" than in an office or clinic.

Table C.10. Beneficiaries' satisfaction with in-home care

	Percentage of all nonmissing, valid responses[a]
Satisfaction with overall quality of in-home care from IAH practice in the past 6 months	
Very satisfied	50.2
Satisfied	42.7
Dissatisfied	4.9
Very dissatisfied	2.1
(Response missing or invalid)	4.7
How much beneficiary likes receiving in-home care compared to primary care in an office or clinic	
A lot more	72.5
Somewhat more	10.5
About the same	12.4
Somewhat less	2.6
A lot less	1.9
(Response missing or invalid)	5.1

Sources: IAH beneficiary surveys conducted from 2013 to 2015.

Note: Data are weighted for overall nonresponse. Mathematica sent the survey to all enrollees who were still alive at the time of the survey and whom Mathematica could locate, regardless of whether they were eligible for the demonstration according to Mathematica's criteria.

Table C.10 (continued)

[a]The presented percentages are shown as a share of nonmissing values for each variable. The percentage of invalid responses for each variable is included in row labeled "Response missing or invalid" and includes cases in which the response was missing, the beneficiary provided multiple responses, or the beneficiary wrote "not applicable" when there was no such answer choice. Missing or invalid responses are shown as percentage of all surveys received (n = 3,870).

IAH = Independence at Home.

Among caregivers, overall satisfaction with the quality of in-home care the IAH practice delivered in the past six months was high; more than half (51.7 percent) of caregivers were very satisfied and another 41.4 percent were satisfied (Table C.11). The large majority (93.5 percent) of caregivers said the primary care team efficiently answered all their questions not only in face-to-face interactions but also via telephone or email. In-home primary care was highly popular among the caregivers of IAH beneficiaries; almost three-quarters (71.7 percent) of caregivers expressed that they prefer the beneficiary receives care at home "a lot more" than they prefer the care to occur in an office.

Table C.11. Caregivers' satisfaction with in-home care

	Percentage of all nonmissing, valid responses[a]
Satisfaction with overall quality of in-home care from IAH practice in the past six months	
Very satisfied	51.7
Satisfied	41.4
Dissatisfied	4.8
Very dissatisfied	2.1
(Response missing or invalid)	4.0
How much the caregiver prefers the beneficiary's receipt of primary care at home versus in an office or clinic	
A lot more	71.7
Somewhat more	12.6
About the same	11.1
Somewhat less	2.8
A lot less	1.8
(Response missing or invalid)	4.3
Agreement with statements about in-home care from IAH practice[b,c]	
Answers all of the caregiver's questions	93.5
Caregiver has great confidence in the primary care team	91.4

Sources: IAH caregiver surveys conducted from 2013 to 2015.

Note: Data are weighted for overall nonresponse. Mathematica sent the IAH caregiver survey to caregivers of all enrollees who were still alive at the time of the survey and whom Mathematica could locate, regardless of whether the enrollee was eligible for the demonstration according to Mathematica's criteria.

[a] The presented percentages are shown as a share of nonmissing values for each variable. The percentage of invalid responses for each variable is included in row labeled "Response missing or invalid" and includes cases in which the response was missing, the beneficiary provided multiple responses, or the beneficiary wrote "not applicable" when there was no such answer choice. Missing or invalid responses are shown as percentage of all surveys received (n = 2,519).

[b] The reported percentages are the respondents who strongly agreed or agreed.

[c] Percentage of all respondents (2,519) with missing or invalid responses for each statement ranged from 8.8 to 10.8 percent. There are differences in the percentage of responses missing because we asked respondents separately about each item.

IAH = Independence at Home.

Caregivers and beneficiaries had similar levels of satisfaction with the IAH practice and in-home primary care; about 93 percent of both groups were either very satisfied or satisfied with in-home care from the IAH practice and about 83 percent of both groups preferred receiving primary care in the home rather than in the office.

VIII. SUMMARY

Among beneficiaries who enrolled in the demonstration from June 1, 2012, through June 30, 2014, they and their caregivers reported high levels of satisfaction with home-based primary care. About 93 percent of beneficiaries and caregivers reported that they were very satisfied or satisfied with the overall quality of care they had received from the IAH practice in the past six months. The large majority of beneficiaries prefer receiving primary care in their home "a lot more" than in an office or clinic, and a similarly large share of caregivers expressed a preference for the beneficiary to receive primary care at home. Accordingly, the large majority believed the IAH practice provides the health care beneficiaries need without much trouble, involves beneficiaries in making decisions about their health care, and considers beneficiaries' opinions. Although most beneficiaries and their caregivers said they would contact the IAH practice if they were unsure whether symptoms required emergency care, a sizable minority of beneficiaries and caregivers would prefer to visit the ED—instead of contacting the IAH practice—in this situation. Despite adequate access to patient-centered primary care services, some beneficiaries' preference for the ED in uncertain situations might contribute to the demonstration's lack of an impact on outpatient ED visits.

APPENDIX D

TECHNICAL APPENDIX FOR CHAPTER IV

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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 direct home-based primary care teams who aim "to reduce expenditures and improve health outcomes" of Medicare beneficiaries with multiple chronic conditions and substantial functional limitations (Appendix A). As we discussed in Chapter I, the IAH demonstration introduced both an incentive to reduce Medicare expenditures (incentive payments) and a service delivery model (physician or nurse practitioner led home-based primary care). Chapter IV of this report presents a description and selected results from analyses to determine whether the service delivery model affected Medicare expenditures and service utilization.

This appendix provides additional details about the data, sample, and methods we used for those analyses. We used Medicare claims data to identify our sample members and to construct measures of Medicare expenditures and service utilization. Our sample consisted of beneficiaries that started home-based primary care in 2010 through 2014 and a comparison set of beneficiaries that we identified using propensity score matching. The quantitative evaluation of the service delivery model was a difference-in-differences analysis using repeated observations of our home-based primary care recipients and the matched comparison. Additionally, Chapter IV contains results from an analysis designed to look for evidence of selection bias in the difference-in-difference impact estimates. This analysis used beneficiary survey responses. Information on the development of the survey instrument and the data collection process are presented here. The final section of this appendix contains additional results that were not presented in Chapter IV in the interest in brevity and focus. These results include difference-in-differences estimates for subsets of the full sample (for example the subset of sample members attributed to an IAH practice) and separately by panel as well as selection survey responses.

II. DATA

We used several sources of data to construct the home-based primary care impact analysis analytic files, which are summarized in Table D.1. We used these data to determine IAH eligibility (sample construction), create control variables, and measure outcomes. We accessed all data through the Virtual Research Data Center data enclave.

Table D.1. Data sources for covariates and outcome measures

Data	Demographic characteristics	Chronic conditions	Activities of daily living[a]	Service use: eligibility	Outcome measures
Medicare Enrollment Database	X				X
Master Beneficiary Summary File		X			
Inpatient hospital claims			X	X	X
Outpatient claims			X	X	X
Physician or supplier claims			X	X	X
Home health agency claims			X	X	X
SNF claims			X	X	X
Hospice claims			X	X	X
DME claims			X		X

[a] We used Medicare claims data to predict the probability of each person having two or more ADLs needing human assistance. The technical appendix contains a description of our model.

ADL = activity of daily living; DME = durable medical equipment; SNF = skilled nursing facility.

As Table D.2 shows, the outcome measures can be broadly grouped into two categories—Medicare expenditures and Medicare service use. We measured all claim-based outcomes at the beneficiary level; for the post-period, we measured the outcomes starting on the first of the month in the month containing the index date (the index month). Below are descriptions of how we defined ambulatory care-sensitive conditions (ACSC) and how we constructed outpatient emergency department (ED) visits and unplanned readmission measures.

ACSC. We based our definition of ACSCs on the Agency for Healthcare Research and Quality Prevention's (AHRQ) Quality Indicator 90, which includes:

- Diabetes short-term complications
- Diabetes long-term complications, uncontrolled diabetes
- Lower-extremity amputation among diabetics
- Chronic obstructive pulmonary disease (COPD) or asthma in older adults
- Hypertension
- Heart failure
- Angina without procedure
- Dehydration
- Bacterial pneumonia
- Urinary tract infection

Table D.2. Measures of Medicare expenditures and service utilization

Outcome measure
Average monthly Medicare expenditures
Total
Inpatient
Home health service[a]
Outpatient
SNF
Physician or supplier services
Hospice
DME
Medicare service utilization (acute inpatient care)
Number of hospital admissions per beneficiary per year[b]
Number of hospital admissions per beneficiary per year for ACSC (AHRQ PQI)[b]
Number of ED visits that did not result in admission per beneficiary per year[c]
Number of ED visits that did not result in admission per beneficiary per year for ACSC (AHRQ PQI)[c]
Percentage of beneficiaries with both a qualifying index discharge and an unplanned readmission within 30 days of discharge

Note: Admissions and ED visits are annualized for beneficiaries who were not observed for the full year.

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

[b] Included inpatient hospital admissions and observation stays.

[c] Measured as specified in the CMMI Priority Measures for Monitoring and Evaluation.

ACSC = ambulatory care-sensitive condition; AHRQ = Agency for Healthcare Research and Quality; CMMI = Center for Medicare & Medicaid Innovation; DME = durable medical equipment; ED = emergency department; SNF = skilled nursing facility; PQI = Prevention Quality Indicator.

The measure of ED visits for ACSCs excludes ED visits that lead to an inpatient hospital admission because there is no diagnosis from the ED visit in a claim record when the 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, and 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 equaled 70000 to 79999 or 80000 to 89999, which identify lab or imaging services. The measure of outpatient ED visits included cases in which a beneficiary was transferred to a different hospital for admission, and might 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 index discharge. Eligible index discharges for the readmission measure include index discharges from nonfederal acute care hospitals for patients who were enrolled in Medicare fee-for-service (FFS), alive at the time of discharge, and not transferred to another acute care facility. The eligible index discharges include patients discharged to nonacute care settings. The measure excludes index admissions for patients:

- Admitted to Prospective Payment System (PPS)-exempt cancer hospitals
- Without at least 30 days of post-discharge enrollment in Medicare FFS
- Discharged against medical advice
- Admitted for primary psychiatric diagnoses

- Admitted for rehabilitation
- Admitted for medical treatment of cancer

The regression for readmission included all beneficiaries; it is not conditional on having a qualifying index 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 index discharge and, if so, whether the patient had an unplanned readmission within 30 days.

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

III. HOME-BASED PRIMARY CARE IMPACT ANALYSIS SAMPLE CONSTRUCTION

As described in Chapter IV, we constructed five sample panels, one for each calendar year from 2010 to 2014. In this section, we provide supplementary information regarding the construction of the samples. To identify our home-based primary care recipients and comparison beneficiary samples for each panel, we started with the population of Medicare FFS beneficiaries living in a state with an IAH practice. We retained beneficiaries who met the eligibility criteria required for inclusion in the IAH demonstration (we describe these criteria in Section A below). We checked for use of home-based primary care and assigned beneficiaries to the home-based primary care and potential comparison groups based on utilization criteria, explained below. We then conducted propensity-score matching (described in Section C) to create the final sample of comparison beneficiaries.

Specification of dates. Home-based primary care recipients had their first home-based primary care visit during the panel year. We refer to the date of this visit as the "index date," which defines the start of the post-home-based primary care period. Many of the sample inclusion/exclusion criteria and matching variables are defined based on beneficiary characteristics as of the index date or relative to a 12-month look-back period ending one day before the index date. Because comparison beneficiaries did not start home-based primary care, their index date was specified in a different manner than for home-based care recipients. To allow matching of home-based care recipients to comparison subjects who were at the same place in their health status trajectory (and to maximize the chances of finding a look-back period during which a potential comparison subject matches a home-based primary care recipient), we considered 12 versions of each potential comparison beneficiary. The first version had an index date of January 1 and a look-back period from January through December in the year before the panel year, the second version had an index date of February 1 and a look-back period from February of the prior year through January of the panel year, and so on. In this manner, we created up to 12 versions of each potential comparison beneficiary, each with a unique index date, capturing his or her status as of each month in the panel year (details of the matching process are described below).

Home-based primary care utilization. Beneficiaries who used home-based primary care during the panel year but not in the two years prior were retained as potential home-based care beneficiaries. To be retained in the potential comparison group, beneficiaries must not have had any evaluation and management (E&M) visits from a primary care clinician in their home in the two years prior to the index date through the first six months following the index date.

To be retained in the home-based care group, beneficiaries must have had at least two E&M visits from a primary care clinician (a physician engaged in general practice, family practice, internal medicine, geriatric medicine, or preventive medicine; an NP; or a PA) in the home during the six-month period starting with the first home visit. In addition, the majority of E&M visits from a primary care clinician during that same period must have taken place in the home. These restrictions ensured that the dominant mode of primary care for home-based care recipients was home based.

We identified the subset of home-based care beneficiaries who received the plurality of their care from an IAH practice; we considered these beneficiaries IAH-attributed. The zip codes in which IAH-attributed beneficiaries lived defined the IAH catchment areas. Finally, we retained all home-based care recipients, including those not attributed to an IAH practice, who lived in an IAH catchment area.

A. Eligibility criteria

As noted above, our first step in creating the five panel samples was to screen for Medicare FFS beneficiaries who met the IAH eligibility criteria—those who had two or more chronic conditions, two or more ADLs needing human assistance, and a hospitalization and rehabilitative services in the prior year. While most of these screens were straightforward, we created a model to predict whether each beneficiary met the ADL requirement, which is described in detail in Section 3.b. below.

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

Inpatient hospitalization or observation stay and utilization of rehabilitation services within 12 months of the index date. We used inpatient, outpatient,[26] home health, and skilled nursing facility (SNF) claims data to identify Medicare FFS beneficiaries who met both of these eligibility criteria.

Eliminate beneficiaries currently in hospice care. The analysis did not include beneficiaries who used hospice within 30 days before the index date.

Two or more ADLs requiring human assistance. To be included in the sample, beneficiaries must have required human assistance with at least two ADLs. In the next section, we describe how we enforced this criterion.

B. ADL prediction model

Information on ADL dependence from the most recent post-acute care assessment can be used (for example, from the Minimum Data Set [MDS]), Outcome and Assessment Information Set [OASIS], or Inpatient Rehabilitation Facility Patient Assessment Instrument [IRF-PAI]). However, the challenges using these assessment data (see Appendix B, Section B.1.) can result in measures for some beneficiaries that do not accurately capture current levels of dependency. During the analysis of the IAH demonstration, we compared the set of patients attributed to IAH practices who were identified in the assessment data as having two or more ADLs needing human assistance to the list of IAH enrollees with two or more ADLs provided by the IAH demonstration sites. There were discrepancies between those two lists; for example, there were

[26] Outpatient claims were used to identify observation stays. Outpatient claims are not used to satisfy the rehabilitation requirement.

2,405 beneficiaries enrolled by the sites in Year 1 who we did not identify as eligible based on claims data (Table B.2, Appendix B, Chapter II), and approximately 1 in 10 of the 8,216 FFS beneficiaries we found to be eligible based on claims data were not considered eligible by the sites because, according to their evaluation, these beneficiaries did not have two or more ADLs needing human assistance.

To determine more accurately which Medicare FFS beneficiaries with two or more chronic conditions met the ADL eligibility criteria for the panel sample, we estimated the likelihood of having two or more ADLs needing human assistance. We followed the process tested in Faurot et al. (2015). Using Medicare claims data, Faurot et al. developed a predictive model for dependence, in which ADL dependence was defined either by the respondents' self-reported need for human assistance or by their inability to complete at least one ADL in the Medicare Current Beneficiary Survey (MCBS). The researchers found that the following diagnosis and procedure codes predicted ADL dependence: use of a home hospital bed, wheelchair, home oxygen therapy, ambulance transport or cardiopulmonary resuscitation, a stroke or brain injury, heart failure, diabetes complications, decubitus ulcer, paralysis, weakness, difficulty walking, sepsis, and podiatric care (Faurot et al. 2015). They also found cancer screening and lipid abnormalities to be negative predictors of ADL dependence. We used Faurot and his team's model, along with claims data to predict dependence, which served as the criteria for sample inclusion as described below.

Data. Our sample for the predictive model consisted of two groups: (1) IAH enrollees in Years 1 and 2 of the demonstration, all of whom were evaluated by IAH clinicians and met the requirement of two ADLs needing human assistance at the time of enrollment; and (2) all people in Years 1 and 2 of the demonstration who were identified in the assessment data as being eligible for the demonstration but were reported by sites as not having two or more ADLs needing human assistance. The first group is equivalent to using the MCBS respondents who reported dependence as a group known to be frail, and the second group is equivalent to using MCBS respondents who did not report dependence at the time of the survey as a group known not to be frail.

There are two key differences between Faurot and his team's study and our study. First, we did not use MCBS data and therefore we did not have a self-reported of level of dependence; we used assessment data and IAH site evaluations instead (as described above). Second, our sample had less variation in level of dependence given that both groups in our predictive model were identified as having two or more ADLs requiring human assistance in *assessment data*. The implication is that with less variation in our training sample, it was more difficult for the model to distinguish between those identified *by the sites* as having two or more ADLs requiring human assistance and those with fewer ADLs requiring human assistance.

Our final data set contained 14,525 beneficiaries; 12,331 (85 percent) were reported as having two or more ADLs needing human assistance at the time of enrollment and 2,194 (15 percent) as not having this level of ADL dependency. We randomly split our sample into sets of 11,620 beneficiaries for training (80 percent) and 2,905 for validation (20 percent). We used the training set for model building and the validation set to determine the optimal cutoff point for our ADL prediction model.

Model. We estimated the following equation on our full sample of beneficiaries:

(1) $$Pr(2^{+}ADLs) = f(\alpha' + \beta'.X_i + \gamma'.M_i)$$

where $Pr(2^{+}ADLs)$ is the probability that the beneficiary had two or more ADLs needing human assistance as identified by the IAH clinician ($2^{+}ADLs$ is a binary indicator variable), X_i is a vector of beneficiary demographic characteristics, and M_i is a vector of claims-based beneficiary measures that are possible predictors of having two or more ADLs. We estimated Equation (1) using a logit model to obtain the estimated coefficients.

In Table D.3, we list all demographic covariates (X_i) and claims-based covariates (M_i) included in our estimation and indicate whether these variables were included in the Faurot et al. model. Claims-based covariates (M_i) were constructed by reviewing all Medicare FFS claims in the period beginning eight months before the index date. Following the Faurot et al. model, we looked back eight months rather than 12 months, as we had done in our propensity score matching. Using this time period increased the likelihood of capturing the beneficiary's disease state and health condition at the time of index date. We examined prevalence for all covariates. One covariate, neutropenia, had less than 1 percent prevalence and was therefore dropped from the list of potential predictors. There were no significant missing values for any covariates.

In addition to the list of covariates in Faurot et al., we included home hospital bed accessories, wheelchair accessories, home oxygen accessories, home health indicator, number of chronic conditions, number of chronic conditions squared, and interaction of the number of chronic conditions with age. We added these conditions to better capture the beneficiary's health status. Some durable medical equipment (DME) purchases, such as hospital beds and wheelchairs, could be one-time purchases and occur outside the eight-month window; including accessories codes captured use of recurring DME accessories and thus established DME use for these individuals.

We fit Equation (1) using our training set of beneficiaries. Specifically, we fit a multivariable logistic regression with stepwise backward elimination to identify statistically significant predictors of ADL dependency, controlling for all other variables in the model. We then used bootstrapping (1,000 samples with replacement) to assess the internal validation of our model. Variables that were statistically significant in 50 percent or more of our bootstrapped data sets were selected for the final model. We performed the Hosmer–Lemeshow goodness-of-fit test on our final model and checked for multicollinearity and influential points.

As the final step, we applied our prediction model to the validation set (a 2,905-member random sample of the 14,525 IAH beneficiaries). To choose an optimal cutoff point, we assumed a list of potential cutoff points from 0.10 to 0.90 and calculated sensitivity, specificity, positive predictive value (PPV), and negative predictive value (NPV) for all cutoff points. We then plotted the Receiver Operating Characteristic curve. The optimal cutoff point was chosen to jointly maximize sensitivity and specificity. Finally, we applied our model to 2010 panel data to assess the percentage of beneficiaries above the chosen optimal cutoff point.

Table D.3. Logistic results from ADL prediction model

List of potential predictors	Parameter estimate of significant[a] predictors	*p*-value of significant predictors	Potential predictor in Faurot et al. (2015) (Y/N)	Parameter estimate of significant predictors, Faurot et al.[b]
Demographic variables				
Female	−0.070	0.244[c]	Yes	0.324
Age (centered at 65)	0.021	<0.0001	Yes	−0.001
Age (centered at 65) square	0.000	0.010	Yes	0.002
Race (reference group = White)			Yes	
Black or African-American	0.379	<0.0001		0.276
Hispanic[d]				−0.507
Other	0.299	0.064		0.862
Dual eligibility	0.119	0.064	No	
Original reason for Medicare entitlement			No	
High-risk disease states				
Stroke/brain injury			Yes	0.467
Heart failure	0.182	0.002	Yes	0.412
Cancer screening			Yes	−0.508
Cancer			Yes	
Psychiatric diagnoses			Yes	0.530
Bladder dysfunction	0.212	0.0005	Yes	0.341
Coagulopathy			Yes	−0.727
Paralysis	0.157	0.051	Yes	1.513
Dementia	0.256	<0.0001	Yes	0.689
Lipid abnormality	−0.109	0.081	Yes	−0.426
Vertigo	−0.186	0.007	Yes	−0.523
Parkinson's disease	0.274	0.039	Yes	1.104
Podiatric care	0.160	0.044	Yes	0.433
Arthritis	−0.122	0.073	Yes	0.257
Skin ulcer (decubitus)	0.352	<0.0001	Yes	0.417
Sepsis			Yes	0.460
Weakness			Yes	0.359
Difficulty walking			Yes	0.400
COPD	−0.110	0.067	Yes	
Respiratory distress/failure			Yes	
Pneumonia			Yes	
Liver disease	−0.130	0.073	Yes	
Coronary disease			Yes	
Peripheral vascular disease			Yes	
Hypotension/shock			Yes	
Herpes zoster			Yes	
Dysphagia	0.302	0.0001	Yes	
Renal failure			Yes	
Back problems	−0.254	<0.0001	Yes	
Electrolyte abnormalities	−0.161	0.004	Yes	
Inflammatory arthritis			Yes	
Abnormal X-ray	−0.139	0.028	Yes	
Valve disease			Yes	
Pulmonary embolus			Yes	
Neutropenia			Yes	
Malnutrition	0.200	0.008	Yes	
DM complication			Yes	0.389
Aphasia, dysphasia			Yes	
Bowel dysfunction			Yes	
Geriatric syndromes				
Falls			Yes	
Hip pelvic fracture			Yes	
Vertebral fracture	0.275	0.046	Yes	

Table D.3 (continued)

List of potential predictors	Parameter estimate of significant[a] predictors	*p*-value of significant predictors	Potential predictor in Faurot et al. (2015) (Y/N)	Parameter estimate of significant predictors, Faurot et al.[b]
Other fracture			Yes	
Weight loss			Yes	
Pneumonia			Yes	
Dehydration			Yes	
Delirium			Yes	
Durable medical equipment				
Home hospital bed	0.683	<.0001	Yes	1.694
Home hospital bed accessories	0.504	0.054	No	
Wheelchair	0.396	<.0001	Yes	1.364
Wheelchair accessories			No	
Home oxygen			Yes	0.783
Home oxygen accessories	0.166	0.042	No	
Walker			Yes	
Ambulance/life support			Yes	0.407
Rehabilitation services	0.158	0.097	Yes	−0.511
Other				
Home health indicator			No	
Number of chronic conditions	−0.132	0.009	No	
Number of chronic conditions squared	0.008	0.009	No	
Number of chronic conditions/age interaction			No	

Note: All predictors are measured in the eight month period before the index date.

[a]Only those estimated parameters with *p*-value < 0.10 are shown.

[b]Faurot et al. (2015) did not provide the associated *p*-value.

[c]Sex, age, and race/ethnicity were kept both by us and by Faurot et al., even when not statistically significant predictors.

[d]We did not include a Hispanic category due to the small number of Hispanic beneficiaries in our sample.

ADL = activities of daily living; COPD = chronic obstructive pulmonary disease; DM = diabetes mellitus.

Results. Our final prediction model consisted of six demographic variables (Table D.3) and 25 claims-based predictors. The c-statistic based on our final model was 0.674, on the cusp of the 0.7 threshold for a good prediction model. Furthermore, our model showed no evidence of lack of fit and no multicollinearity; in addition, we found no influential points.

To jointly maximize sensitivity and specificity, we chose the optimal cutoff point to be 0.83 (in other words, there was an 83 percent probability that the person had two or more ADLs needing human assistance). Our model yielded sensitivity of 0.69 and specificity of 0.5. Under the optimal cutoff point, our model correctly identified 66.5 percent of subjects in the validation set: 1,684 subjects identified as having two or more ADLs needing assistance by our prediction model did have two or more ADLs needing assistance, as specified by their clinicians at time of enrollment, and 249 subjects identified as not having two or more ADLs needing assistance by our prediction model did not have two or more ADLs needing assistance (Table D.4). Correctly predicting 89 percent of those specified as having two or more ADLs needing assistance (1,684 of 1,893) is a high PPV. However, correctly identifying only 25 percent of subjects (249 of 1,012) as not having two or more ADLs needing assistance is a relatively low NPV. The low NPV was due to the lack of sample variation—that is, all our subjects had two or more chronic conditions, an inpatient stay, and utilization of rehabilitation services in the previous 12 months

and either had, or were expected to have, two or more ADLs needing human assistance in the previous year. In short, our prediction model was conservative in identifying subjects with two or more ADLs. Because we wanted to include only frail Medicare FFS beneficiaries in both the home-based care and comparison groups, we concluded it was appropriate to use this conservative cutoff point.

Table D.4. ADL prediction model performance based on the validation set, n = 2,905

		Predicted ADL status		Total
		Two or more	Fewer than two	
ADL status as provided by site	Two or more	1,684	763	2,447
	Fewer than two	209	249	458
	Total	1,893	1,012	2,905

ADL = activities of daily living.

We also tested whether the model was able to distinguish between frail and extremely frail beneficiaries; for example, we hoped to differentiate between those with two ADLs and those with five or six ADLs when we constructed the matched comparison group. Using assessment data and our validation set, we tested the discrimination property of our model. Beneficiaries with five or six ADLs needing human assistance—according to assessment data—did have, on average, higher predicted probabilities than did beneficiaries with three or four ADLs. In addition, the *p*-value from a Cochran-Armitage Trend Test was less than 0.001, indicating a statistically significant trend in the number of ADLs needing human assistance from assessment data and predicted probabilities.

Finally, we applied our model to the sample of beneficiaries who met the other criteria (chronic conditions and inpatient and use of rehabilitation services in the previous 12 months) for the 2010 panel data. The predicted probability of having two or more ADLs was above the chosen optimal cutoff point for 75 percent of potential home-based primary care beneficiaries and 72 percent of potential comparison beneficiaries.

Strengths and limitations. Our ADL prediction model had several strengths and limitations. The primary strengths were that the predicted probability could be calculated for all beneficiaries, regardless of the availability of assessment data, and that the prediction was based on recent data for all beneficiaries. The main limitation was that the model did not perform as well as expected based on the results in Faurot et al. (2015). The lower c-statistics (and low NPV) reflected the use of a sample that, by necessity, excluded Medicare FFS beneficiaries who were not frail. We tested the model on the 2010 panel sample used in our regression analyses. Home-based care recipients had higher predicted ADL probabilities than the potential comparison group, and predicted ADL probabilities at baseline were higher than scores 12 months prior, as expected. Overall, the strengths of using a model-based approach to determine whether beneficiaries have two or more ADLs that require human assistance outweighed its limitations, particularly given the alternative of relying on missing or outdated assessment data.

C. Propensity score matching methods

Medicare FFS beneficiaries who met the IAH eligibility criteria and chose to start home-based primary care may differ systematically in terms of attributes that are correlated with health care use and expenditures compared to those who did not choose to start home-based primary care. Through propensity score matching, we aimed to minimize bias in the estimation of home-based care effect that would result from this nonrandom self-selection process by constructing a matched comparison group similar to the home-based care recipients on key observable covariates. As noted in Chapter IV, we had up to 12 versions of each potential comparison beneficiary—that is, a version for each month of the panel year that the potential comparison beneficiary met all eligibility criteria. We used a step-by-step matching procedure to ensure optimal matching in our setting with different versions of each beneficiary.

Data. We identified 34,887 eligible (that is, meeting the eligibility criteria discussed above) home-based primary care beneficiaries and 3,973,676 eligible potential comparison beneficiary/month versions (including all versions of each unique potential comparison) across the five panels (2010 through 2014). We defined home-based care recipients as being new to home-based primary care, identifying people who had no E&M visits in the home in the previous 24 months; there were no overlapping home-based primary care beneficiaries across the five panels. Different monthly versions of a potential comparison beneficiary could appear in more than one panel. For example, a potential comparison beneficiary could have an inpatient and post-acute care stay in May 2009 and meet all other eligibility criteria in March 2010 and then have a new inpatient and post-acute care stay in August 2011 and meet all other eligibility criteria in October 2011. Therefore, two versions of this beneficiary would appear in the 2010 and 2011 potential comparison beneficiary panels. Thus, this potential comparison beneficiary's March 2010 index date version could be matched to a home-based primary care beneficiary in the 2010 panel, and his or her other version with an October 2011 index date could be matched to a different home-based primary care beneficiary in the 2011 panel.

Methods. We divided all home-based primary care beneficiaries into two groups: (1) those with one E&M visit from a primary care clinician in the home in the six-month period starting with the initial home visit, and (2) those with two or more such visits in the same period. We were concerned that those with only one home visit might not have received enough treatment (that is, having primary care visits in the home) to provide an estimate of the impact of home-based primary care. We discovered that more than half of these people died during that six-month period. The remainder stopped having observed home visits for a variety of reasons, including leaving FFS, moving into a long-term-care facility or to another location, or deciding that receiving primary care in their home was not what they wanted. As a result, we used those with two or more E&M visits in the six-month window as our main analytic sample. We matched home-based primary care beneficiaries in the main sample to the entire pool of potential comparison beneficiaries to obtain the strongest matches possible.

We used logistic regression models to estimate propensity scores by panel years. To assess common support, we examined distributions of propensity scores by home-based care recipients and potential comparison groups for all panel years.

We applied optimal matching methods (Rosenbaum 1991), separately for each panel, to select those comparison beneficiaries from the potential comparison pool who, in the aggregate, were most similar to home-based care recipients on the matching covariates. Specifically, we matched without replacement, whereby each potential comparison beneficiary version could be matched to only one home-based care recipient. Optimal matching minimizes a global distance criterion, instead of many local criteria as used by nearest neighbor matching. In our approach, optimal matching minimized the total sum of differences on the estimated propensity scores between the home-based primary care and matched comparison beneficiaries. To implement optimal matching, we used the *optmatch* package available on the Comprehensive R Archive Network. The *fullmatch* function in this package creates optimal full matches for the specified home-based primary care group (Hansen and Klopfer 2006).

By assigning up to 12 index dates for each potential comparison beneficiary, each with a different look-back period, we could better match beneficiaries based on IAH eligibility criteria and use, as well as other time-varying covariates. For example, if the first visit by a home-based care recipient (T) was in September 2010 and her most recent inpatient stay was seven months earlier (February 2010), she might get matched to a comparison beneficiary (C) whose index date was in December 2010 and whose most recent inpatient stay was seven months earlier (May 2010) as well. The May 2010 version of C was likely to be very similar to the April 2010 version of C, because they shared the same time-invariant demographic characteristics, as well as comparable time-varying health status and health care expenditure covariates.

In a traditional optimal matching approach, it was probable that C's April 2010 version also would get matched to T. To avoid this situation, we used a modified optimal matching approach. First, we ran optimal matching, fixing the number of comparisons matched to each home-based care recipient to be one. Next, we recycled the pool of unmatched comparison beneficiaries but removed all other versions of comparison beneficiaries who had at least one version matched in the previous one-to-one matching step. This step was to ensure that all other versions of matched comparison beneficiaries could not get matched again in subsequent steps. We then did another round of one-to-one optimal matching using the recycled pool of unmatched comparison beneficiaries. We repeated this-step by-step process, eliminating all other versions of previous matching comparison beneficiaries each time until each home-based care recipient was matched to up to five unique comparison beneficiaries.

Matching variables. The list of matching covariates was similar to that in the IAH demonstration impact analysis. As in that analysis, we included many beneficiary characteristics as predictors in the propensity score model, including demographic characteristics, original reason for Medicare entitlement, functional status, and health status (Table D.5).

We used several measures of health status, many of which we created using the CMS Hierarchical Condition Category (HCC) risk adjustment model. We used the community score calculated by version 21 of the HCC model, which was developed and calibrated for the Programs of All-Inclusive Care for the Elderly population. We included the HCC score measured at baseline, as well as the one measured 12 months prior. As we did for the IAH impact analysis sample, we included an individual HCC in the matching equation if any of the International Classification of Diseases (ICD-9) diagnosis codes in that HCC were identified by Gagne et al. (2011) as being predictive of mortality among elderly low-income Medicare beneficiaries. We

included three other conditions not measured by HCCs: anemia, electrolyte disorders, and depression. Finally, we included a categorical measure of the number of chronic conditions identified by the CCW: 2 to 5, 6 to 9, or 10 or more. In addition to measures related to HCCs and individual conditions, we included another measure of health: an indicator of whether the Medicare Severity Diagnosis Related Group (MS-DRG) included a major complicating condition or a complicating condition from the most recent inpatient stay.

Table D.5. Characteristics of home-based care recipients and matched comparison beneficiaries, 2010 panel

Variable	Potential comparison group	Matched comparison group	Home-based care recipients	Standardized difference
Beneficiary characteristics used for exact matching				
Number of months since last hospital admission				
1	0.148	0.234	0.234	0.000
2 or 3	0.226	0.351	0.351	0.000
4 or more	0.626	0.415	0.415	0.000
Observation stay only in prior year (no hospital admission)	0.036	0.044	0.044	0.000
IAH catchment area				
Austin, Texas	0.193	0.177	0.177	0.000
Boston, Massachusetts	0.020	0.011	0.011	0.000
Cleveland, Ohio	0.050	0.047	0.047	0.000
Dallas, Texas	0.099	0.126	0.126	0.000
Durham, North Carolina	0.097	0.074	0.074	0.000
Flint, Michigan	0.088	0.128	0.128	0.000
Jacksonville, Florida	0.069	0.054	0.054	0.000
Lansing, Michigan	0.094	0.125	0.125	0.000
Milwaukee, Wisconsin	0.073	0.045	0.045	0.000
New York (combining the areas for the Brooklyn IAH practice and the Long Island Jewish IAH practice)	0.140	0.154	0.154	0.000
Philadelphia, Pennsylvania	0.008	0.006	0.006	0.000
Portland, Oregon	0.013	0.010	0.010	0.000
Richmond, Virginia	0.022	0.013	0.013	0.000
Washington, DC	0.017	0.016	0.016	0.000
Wilmington, Delaware	0.019	0.013	0.013	0.000
Demographic characteristics				
Age				
Younger than 65	0.048	0.040	0.041	0.007
65 to 79	0.268	0.213	0.215	0.004
80 or older	0.684	0.747	0.744	−0.007
Race				
White	0.740	0.808	0.805	−0.006
Black or African-American	0.198	0.149	0.151	0.007
Other	0.062	0.044	0.043	−0.002
Dual eligibility status	0.266	0.204	0.205	0.001
Female	0.629	0.678	0.679	0.003
Original reason for Medicare entitlement				
Age	0.838	0.868	0.867	−0.003
Disability	0.153	0.125	0.127	0.004
ESRD or ESRD and disability	0.010	0.007	0.007	−0.002
ADL				
Predicted probability of having two or more ADLs as of index date	0.883	0.900	0.902	0.046
Predicted probability of having two or more ADLs 12 months before the index date	0.851	0.855	0.856	0.029

Table D.5 (continued)

Variable	Potential comparison group	Matched comparison group	Home-based care recipients	Standardized difference
Health status				
HCC score	3.560	3.688	3.677	−0.006
HCC score, 12 months prior	2.278	2.114	2.079	−0.023
HCC 8, metastatic cancer	0.030	0.024	0.024	0.005
HCC 9–10, lung, lymphoma, and other cancers	0.052	0.043	0.041	−0.012
HCC 11–12, colorectal, bladder, breast, prostate, and other cancers	0.134	0.131	0.130	−0.003
HCC 18, diabetes with chronic complications	0.242	0.212	0.21	−0.004
HCC 21, protein-calorie malnutrition	0.153	0.186	0.184	−0.006
HCC 27, end-stage liver disease	0.016	0.014	0.014	0.001
HCC 28–29, cirrhosis of liver and chronic hepatitis	0.011	0.011	0.010	−0.005
HCC 46, severe hematological disorders	0.023	0.017	0.017	0.000
HCC 48, coagulation defects and other specified hematological disorders	0.138	0.137	0.131	−0.017
HCC 51, dementia with complications	0.139	0.197	0.20	0.009
HCC 52, dementia without complications	0.293	0.408	0.405	−0.005
HCC 54–55, drug/alcohol psychosis and drug/alcohol dependence	0.033	0.043	0.045	0.010
HCC 57–58, schizophrenia, major depressive, bipolar, and paranoid disorders	0.127	0.157	0.158	0.004
HCC 70–71, quadriplegia, paraplegia	0.024	0.027	0.028	0.008
HCC 72, spinal cord disorders/injuries	0.025	0.025	0.026	0.002
HCC 85, congestive heart failure	0.530	0.525	0.510	−0.029
HCC 96, specified heart arrhythmias	0.424	0.433	0.422	−0.023
HCC 103–104, hemiplegia/hemiparesis, monoplegia, other paralytic syndromes	0.109	0.129	0.131	0.007
HCC 106, atherosclerosis of the extremities with ulceration or gangrene	0.054	0.041	0.043	0.011
HCC 107–108, vascular disease with or without complications	0.455	0.487	0.481	−0.011
HCC 111, chronic obstructive pulmonary disease	0.318	0.303	0.294	−0.018
HCC 134, dialysis status	0.037	0.024	0.022	−0.012
HCC 136–138, chronic kidney disease, stage 3–5	0.059	0.054	0.051	−0.014
HCC 139–140, chronic kidney disease stage 1–2, unspecified renal failure	0.075	0.066	0.066	−0.001
HCC 157–159, pressure ulcer of skin with necrosis or skin loss	0.064	0.073	0.076	0.012
Anemia[a]	0.191	0.181	0.180	−0.002
Depression	0.301	0.317	0.327	0.023
Fluid and electrolyte disordersa	0.332	0.358	0.361	0.005
Chronically critically ill/medically complex	0.275	0.291	0.293	0.004
MS-DRG with major complicating condition or complicating condition	0.538	0.530	0.530	0.000
Health care expenditures and utilization in 12 months prior to start date				
Average monthly expenditures (on the log scale)	8.042	8.173	8.173	0.001
Average monthly expenditures, 12 months prior (on the log scale)	6.370	6.230	6.174	−0.029
Hospice utilization in the past 12 months	0.009	0.019	0.020	0.008
Number of specialist visits (non-inpatient setting)	24.377	21.599	21.049	−0.034
Number of ED visits	0.937	1.064	1.098	0.017
Number of hospitalizations	1.856	2.027	1.982	−0.029

Table D.5 (continued)

Variable	Potential comparison group	Matched comparison group	Home-based care recipients	Standardized difference
Number of observation stays	0.154	0.177	0.174	−0.006
Top MDRGs[d] from the most recent inpatient stay				
Kidney and urinary tract infections	0.042	0.061	0.065	0.013
Heart failure and shock	0.054	0.052	0.050	−0.011
Intracranial hemorrhage or cerebral infarction	0.035	0.043	0.045	0.008
Septicemia without MV 96+ hours	0.038	0.041	0.044	0.011
Hip and femur procedures except major joint	0.030	0.039	0.037	−0.011
Simple pneumonia and pleurisy	0.038	0.031	0.030	−0.005
Renal failure	0.022	0.020	0.020	0.002
Major joint replacement or reattachment of lower extremity	0.045	0.023	0.024	0.011
Recency variables				
Months since last wheelchair code as of index date				
Less than 3 months	0.135	0.251	0.277	0.060
3–12 months	0.055	0.057	0.057	0.001
Not observed in 12 months	0.810	0.692	0.666	−0.057
Months since last wheelchair code 12 months prior				
Less than 3 months	0.056	0.070	0.070	−0.002
3–12 months	0.034	0.044	0.041	−0.015
Not observed in 12 months	0.910	0.886	0.889	0.011
Months since last hospital bed code as of index date				
Less than 3 months	0.082	0.231	0.252	0.050
3–12 months	0.037	0.038	0.042	0.020
Not observed in 12 months	0.881	0.731	0.706	−0.056
Months since last hospital bed code 12 months prior				
Less than 3 months	0.034	0.052	0.053	0.005
3–12 months	0.020	0.021	0.021	0.001
Not observed in 12 months	0.947	0.928	0.926	−0.004
Months since last home oxygen code as of index date				
Less than 3 months	0.068	0.083	0.080	−0.010
3–12 months	0.022	0.017	0.018	0.006
Not observed in 12 months	0.910	0.900	0.902	0.007
Months since last home oxygen code 12 months prior				
Less than 3 months	0.046	0.044	0.042	−0.008
3–12 months	0.018	0.021	0.019	−0.015
Not observed in 12 months	0.936	0.935	0.939	0.016
Number of CCW chronic condition categories[b]				
Fewer than 6	0.273	0.341	0.332	−0.019
6–9	0.511	0.466	0.475	0.018
10 or more	0.216	0.193	0.193	0.000
Time since first diagnosed with Alzheimer's disease or senile dementia				
Less than 3 months	0.027	0.085	0.060	−0.091
3–12 months	0.106	0.121	0.140	0.058
More than 12 months	0.364	0.422	0.433	0.022
Never	0.504	0.372	0.367	−0.011
Time since first diagnosed with COPD				
Less than 3 months	0.012	0.025	0.020	−0.035
3–12 months	0.052	0.042	0.048	0.033
More than 12 months	0.403	0.384	0.368	−0.032
Never	0.533	0.550	0.564	0.029
Time since first diagnosed with heart failure				
Less than 3 months	0.019	0.048	0.035	−0.061
3–12 months	0.083	0.071	0.080	0.037
More than 12 months	0.575	0.553	0.538	−0.028

Table D.5 (continued)

Variable	Potential comparison group	Matched comparison group	Home-based care recipients	Standardized difference
Never	0.323	0.329	0.346	0.037
Time since first diagnosed with hip pelvic fracture				
Less than 3 months	0.012	0.038	0.024	−0.073
3–12 months	0.049	0.041	0.051	0.051
More than 12 months	0.097	0.102	0.103	0.005
Never	0.842	0.820	0.821	0.005
Time since first diagnosed with osteoporosis				
Less than 3 months	0.008	0.017	0.012	−0.037
3–12 months	0.036	0.028	0.032	0.027
More than 12 months	0.310	0.348	0.345	−0.008
Never	0.646	0.607	0.611	0.008
HCC 18, diabetes with chronic complications, 12 months prior	0.200	0.162	0.160	−0.006
HCC 27, end-stage liver disease, 12 months prior	0.008	0.007	0.006	−0.009
HCC 28–29, cirrhosis of the liver, chronic hepatitis, 12 months prior	0.009	0.008	0.008	−0.003
HCC 57–58, schizophrenia, major depressive, bipolar, and paranoid disorders, 12 months prior	0.087	0.079	0.078	−0.003
HCC 70–71, quadriplegia, paraplegia, 12 months prior	0.012	0.011	0.012	0.008
HCC 103–104, hemiplegia/hemiparesis, monoplegia, other paralytic syndromes, 12 months prior	0.050	0.047	0.046	−0.003
HCC 134, dialysis status, 12 months prior	0.024	0.014	0.013	−0.014
HCC 157–159, pressure ulcer of skin with necrosis or skin loss, 12 months prior	0.017	0.014	0.013	−0.001

[a]Measured using claims from the most recent inpatient stay and observation stay in the year before the index date. Diagnosis codes for these conditions were drawn from Gagne et al. (2011).

[b]Chronic condition categories measured by the CCW.

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

There were several differences between the covariates in the main evaluation analysis and those in the home-based primary care impact analysis. These differences, which reflect both the home-based care recipient sample construction and the intervention being studied in this analysis, include the following:

- We used three measures for exact matching: (1) the number of months since the beneficiary's last inpatient admission (1, 2 to 3, or 4 or more); (2) whether the beneficiary had an observation stay and no inpatient admission in the previous 12 months; and (3) catchment area (the collection of zip codes for home-based care recipients attributed to an IAH practice).
- We applied calipers (the maximum tolerated difference between matched beneficiaries) equal to 0.5 times the standard deviation of the home-based care group on HCC risk scores and average expenditure.

- We used the predicted probabilities of having two or more ADLs from our prediction model at index time and at 12 months before index month. Matching on both predicted probabilities should increase the likelihood that the matched comparison beneficiaries would have a level of frailty similar to that of corresponding home-based care recipients in terms of ADLs needing human assistance.
- We included two measures of previous Medicare FFS expenditures: (1) average monthly expenditures within the 12 months before the index date, and (2) average monthly expenditures over the period 13 to 24 months before the index date). We also included the following measures of health care utilization in the 12 months before the index date: number of hospitalizations, specialist visits, ED visits, observation stays in the past 12 months, and hospice utilization. In the IAH impact analysis, these variables would likely reflect any effect of receiving primary care in the home for those IAH enrollees who were long-term patients of the IAH practice; therefore, we excluded these measures. For the home-based primary care impact analysis, however, these expenditure and utilization measures occurred before the home-based care recipient started home-based primary care.
- We included several recency variables—that is, measures that increased our ability to match beneficiaries at a similar level of frailty and disease progression. These variables included the number of months since first diagnosis for a set of chronic conditions—such as Alzheimer's disease—that deteriorate over time, as well as markers for whether specific conditions (such as pressure ulcers) were present both currently and 12 months before the index date.
- We included the most frequent Modified Diagnostic Related Groups (MDRG) from the most recent inpatient stay. We examined the discharge MDRG from the most recent inpatient stay of the home-based care recipients across all five panels and identified the most frequent MDRGs. On average across the panels, eight MDRGs accounted for 33.1 percent of the home-based care recipients (Table D.5).

Results. A standard statistic to assess the balance of a sample before and after matching is the standardized difference. We calculated the standardized differences on all matching variables. The standardized differences for all matching variables and the propensity score were less than 0.10 for all five panels in both the main sample and the sample of home-based care recipients with only one home visit in the six-month period starting with that initial home visit. (See Table D.5 for the standardized differences for the 2010 panel.) We obtained similar results for the 2011–2014 panels.

Overall, the home-based care recipient and matched comparison groups were well balanced for all five panels. In addition to assessing standardized differences and the closeness of matched comparison beneficiaries to each home-based care recipient's estimated propensity score, we calculated the *p*-value from testing significant differences in means (z-test for binary variables and chi-squared test for categorical variables). For discrete or categorical covariates, we reported the sample proportion.

In addition to reviewing standardized differences, our matching diagnostics included the sample sizes and the distribution of matching ratios. These ratios indicate the degree to which multiple comparison beneficiaries were available as suitable matches for a given home-based care recipient. (See Table D.6 for the matching ratios for the total sample.) All home-based care

recipients in all five panels were matched to at least one comparison beneficiary. Of 30,324 total home-based care recipients, 29,956 were matched to five comparison beneficiaries. Our final matched sample consisted of 30,324 home-based care recipients and 150,677 matched comparison beneficiaries, which included up to six versions of 129,703 unique comparison beneficiaries.

Table D.6. Matching ratios

Panel	1T:1C	1T:2C	1T:3C	1T:4C	1T:5C	Total
2010 panel	8	3	10	2	5,259	5,282
2011 panel	7	8	6	169	5,477	5,667
2012 panel	3	4	4	3	6,392	6,406
2013 panel	7	5	5	2	6,337	6,356
2014 panel	8	4	3	7	6,491	6,513
Total	**33**	**24**	**28**	**183**	**29,956**	**30,324**

Note: All home-based care recipients were matched to at least one comparison beneficiary.
C = comparison beneficiary; T = home-based care recipient.

We also looked at how many versions of each unique comparison beneficiary were matched to a home-based care recipient. In our step-by-step optimal matching scheme, different versions of a particular comparison beneficiary (there are up to 12 index months for each beneficiary) could not be matched to the same home-based care recipient. However, different versions of a particular comparison beneficiary could be matched to another home-based care recipient in the same round. In Table D.7, we summarize the number of times each unique comparison beneficiary was matched. Ninety-five percent of all unique matched comparison beneficiaries had only one version matched. Because nearly all unique comparison beneficiaries were matched only once, we were not concerned with the potential correlation between different versions of the same comparison beneficiary. In our main difference-in-differences analysis, we clustered standard errors at the unique beneficiary level to account for potential correlation between different versions of the same comparison beneficiary.

Table D.7. Number of versions matched for unique comparison beneficiaries

Panel	1 version	2 versions	3 versions	4 versions	5 versions	6 versions	Total
2010 panel	23,900	1,064	76	19	3	0	25,062
2011 panel	25,284	1,232	107	7	1	0	26,631
2012 panel	28,998	1,327	98	11	1	0	30,435
2013 panel	28,439	1,438	112	12	4	1	30,006
2014 panel	29,363	1,433	81	9	0	0	30,886

Finally, because a different version of each potential comparison beneficiary could appear in more than one panel, we also checked the number of comparison beneficiaries whose versions were matched in different panels. As Table D.8 shows, 117,541 (90.2 percent) matched comparison beneficiaries appeared in only one panel.

Table D.8. Number of panels in which each unique matched comparison beneficiary appears

	1 panel	2 panels	3 panels	4 panels	5 panels	Total
Number of matched C beneficiaries	117,541	10,097	979	82	4	129,703

C = comparison beneficiary.

Characteristics of the home-based care recipient samples. The number of home-based care recipients identified ranged from 5,282 in the 2010 panel to 6,513 in the 2014 panel (Table D.9). Table D.5 provides characteristics of the home-based care recipients in the 2010 panel. In all five panels, more than three-fourths of the home-based care recipients were at least 80 years old, two-thirds were female, 80 percent were white, and 20 percent were dually eligible for Medicare and Medicaid. On average, the group experienced two inpatient hospital admissions in the year before beginning home-based primary care: about one-fourth of them had a hospital admission in the month before their first E&M visit in the home, and another one-third had an admission two to three months before that first home visit. The average HCC score at baseline ranged from 3.7 to 3.9, and the average HCC score during the year before was 2.1 to 2.2. Nearly one-half had six to nine chronic conditions at baseline. About 40 percent had dementia without complications, and 20 percent had dementia with complications. Half of the home-based care recipients had congestive heart failure and/or vascular disease.

Table D.9. Number of home-based care recipients identified in each IAH catchment area, by panel

	Number of home-based care recipients (percentage attributed to IAH practice)				
IAH catchment area	2010 panel	2011 panel	2012 panel	2013 panel	2014 panel
Austin, Texas	937 (26)	948 (24)	867 (20)	684 (18)	547 (18)
Boston, Massachusetts	57 (44)	76 (51)	86 (49)	62 (40)	46 (67)
Brooklyn/North Shore, New York[a]	816 (13)	1,025 (13)	1,238 (14)	1,290 (16)	1,633 (19)
Cleveland, Ohio	247 (14)	304 (13)	416 (19)	482 (18)	475 (18)
Dallas, Texas	663 (18)	594 (15)	543 (16)	587 (14)	541 (13)
Durham, North Carolina	393 (51)	420 (53)	401 (54)	667 (45)	847 (41)
Flint, Michigan	677 (29)	644 (33)	1,217 (24)	864 (24)	631 (26)
Jacksonville, Florida	285 (45)	414 (39)	407 (42)	360 (25)	350 (18)
Lansing, Michigan	660 (22)	564 (22)	470 (24)	490 (23)	571 (21)
Milwaukee, Wisconsin	240 (54)	277 (63)	299 (62)	303 (55)	375 (56)
Philadelphia, Pennsylvania	34 (21)	59 (17)	78 (22)	74 (15)	56 (27)
Portland, Oregon	51 (71)	97 (68)	87 (78)	88 (72)	78 (64)
Richmond, Virginia	67 (30)	77 (25)	76 (28)	142 (19)	110 (14)
Washington, DC	84 (57)	92 (49)	107 (51)	101 (48)	93 (71)
Wilmington, Delaware	71 (41)	76 (32)	114 (50)	162 (62)	160 (47)
Total	**5,282 (28)**	**5,667(28)**	**6,406 (27)**	**6,356 (26)**	**6,513 (26)**

Source: Medicare claims and enrollment data for 2010–2016 obtained from the Virtual Research Data Center for home-based care recipients and matched comparison group beneficiaries.

[a]The residential zip codes for a large proportion of the home-based care recipients attributed to the two IAH practice sites in the New York area overlapped. Therefore, we combined the home-based care recipients attributed to these two practices to create one catchment area.

D. Subgroup analyses

In addition to examining the full sample of beneficiaries, we also estimated the effect of home-based primary care on two subsamples: (1) those home-based primary care recipients attributed to IAH practices and their comparison beneficiaries and (2) those beneficiaries with dementia. Those samples are described below.

Beneficiaries attributed to an IAH practice. The full group of home-based primary care recipients consisted of new entrants into home-based primary care who lived in one of the IAH practice catchment areas. We also analyzed the subset of home-based care recipients who were attributed to IAH practices—approximately one-fourth of the home-based care recipient sample (Table D.9).

Home-based care recipients attributed to IAH practices accounted for 26 to 28 percent of the home-based care recipient sample, on average (this varied by panel). The proportion of the home-based care recipient sample attributed to an IAH practice differed substantially across the different catchment areas. In some panels, the proportion was as low as 13 percent (in Brooklyn/North Shore, Cleveland, and Dallas); it was as high as 78 percent in Portland in the 2012 panel. The characteristics of the home-based care recipients receiving home-based primary care from IAH practices were similar to those of home-based care recipients receiving care from non-IAH clinicians, with a few differences. Those receiving care from an IAH practice were more likely—by one or two percentage points—to be younger than 65, to be black or African-American, to have dual-eligibility status, and to have disability as the original reason for Medicare eligibility. The two groups did not differ, however, in overall health status (as measured by HCC scores), specific chronic conditions, or health care use (such as number of hospitalizations in the previous year) and expenditures in the baseline period. The results of our analysis are presented in Chapter IV.

Beneficiaries with dementia. To focus on the effect of home-based primary care among a specific subgroup—beneficiaries with dementia—we identified home-based care and comparison beneficiaries with dementia in each panel using HCC flags and estimated the effect of home-based primary care for this subgroup.[27] On average, across the five panels, nearly two-thirds of home-based care recipients and matched comparison beneficiaries had dementia.

Not all matched comparisons of home-based care recipients with dementia also had dementia; similarly, not all comparison beneficiaries with dementia were matched to a home-based care recipient with dementia. Therefore, we checked whether the dementia subgroup was sufficiently balanced on baseline matching variables. We calculated the standardized difference for the pool of home-based care recipients and comparisons with dementia.

The absolute standardized differences were less than 0.25 for all matching variables (results not shown), indicating that the home-based care recipients and matched comparison groups were well matched. In addition, 76 of 79 matching variables had absolute standardized differences less

[27]Dementia was defined as either HCC51: Dementia with complications or HCC52: Dementia without complications.

than the relatively strict 0.10 cutoff.[28] Matching variables with absolute standardized differences greater than 0.10 in any panel included months since first diagnosed with Alzheimer's disease or senile dementia, months since last wheelchair code as of index date, and months since last hospital bed code as of index date. Overall, the home-based care recipients had fewer recent diagnoses (within three months) of Alzheimer's disease or senile dementia than the matched comparison beneficiaries. Because the largest absolute standardized difference was only slightly over 0.10, we thought the dementia subgroup was well balanced and chose not to rematch.

We adjusted for the small differences remaining between the home-based care recipients and matched comparison groups in the subsequent difference-in-differences regression models by adding covariates for time since first diagnosis. Specifically, we added as a covariate the number of years since first diagnosed with Alzheimer's disease or senile dementia, using the first-ever occurrence date in the CCW database. Fewer than 10 percent of the subgroup did not have a valid first-ever occurrence date, so we included an indicator for these beneficiaries with missing time since first diagnosis information. Finally, we included interaction terms of HCC51 (dementia with complications) with the home-based care recipient and period variables to obtain separate home-based primary care effects for beneficiaries who had dementia with complications and those who had dementia without complications.

[28] As noted in Chapter IV, the literature suggests that a standardized difference of less than 0.25 is an appropriate threshold for determining that the home-based care and comparison groups are well matched on a particular characteristic (Rubin 2001).

IV. ESTIMATION METHODS

To answer the key question—What was the effect of providing home-based primary care to Medicare FFS beneficiaries eligible for the demonstration?—we used a difference-in-differences approach similar to that described in Chapter II. We examined the effect of home-based primary care on several key outcomes of interest that can be broadly grouped into two categories: (1) Medicare FFS expenditures and (2) Medicare service use (Table D.10). Our difference-in-differences framework estimates the pre-post changes in outcomes for beneficiaries in the home-based care recipient group and for the matched comparison group during the same analysis period (annual, monthly, or quarterly, depending on the analysis). The effect of home-based primary care was estimated as the difference between the degree of change for the home-based care recipients and the matched comparison beneficiaries.

We observed people in four 12-month periods: two periods before the index date and two after the index date. We defined the baseline period as the second period (the 12-month period immediately before the index date). Therefore, for a home-based care recipient who began receiving primary care in the home in January 2010 and was alive until the end of 2011, January to December 2008 is the first period, January to December 2009 is the second (baseline) period, and so on through the final period: January to December 2011.

In this subsection, we describe the details of the three estimation approaches: (1) frequentist, (2) Bayesian, and (3) a modified version of the frequentist approach that incorporates survival probabilities.

Table D.10. Measures of Medicare expenditures and service utilization and associated regression model

Variable	Regression model
Medicare expenditures (PBPM)	
Total	Ordinary least squares
Inpatient	Ordinary least squares
Home health service[a]	Ordinary least squares
Outpatient	Ordinary least squares
Skilled nursing facility	Ordinary least squares
Physician or supplier	Ordinary least squares
Hospice	Ordinary least squares
Durable medical equipment	Ordinary least squares
Medicare service utilization (acute inpatient care)	
Number of hospital admissions per beneficiary per year[b]	Negative binomial
Number of hospital admissions per beneficiary per year for ACSC (AHRQ PQI)[b]	Negative binomial
Number of ED visits that did not result in admission per beneficiary per year[c]	Negative binomial
Number of ED visits that did not result in admission per beneficiary per year for ACSC (AHRQ PQI)[c]	Negative binomial
Percentage of beneficiaries with a qualifying index discharge and an unplanned readmission within 30 days of discharge	Logistic

Notes: Measures of admissions and ED visits are annualized and weighted to reflect part-year observations. Expenditures were per beneficiary per month and were not annualized but weighted to reflect part-year observations. We do not price-standardize the expenditure measures.

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

[b]Included inpatient admissions and observation stays.

[c]Measured as specified in the CMMI Priority Measures for Monitoring and Evaluation.

ACSC = ambulatory care sensitive condition; AHRQ = Agency for Healthcare Research & Quality; CMMI = Center for Medicare & Medicaid Innovation; ED = emergency department; E&M = evaluation and management; PBPM = per beneficiary per month; PQI = prevention quality indicator.

A. Frequentist estimation

Equation (2) specifies the frequentist regression equation we used to estimate the effect of home-based primary care for the five panels combined:

$$(2) \qquad Y_{it} = \alpha + \beta'.X_i + \sigma'.C_i + \tau.treatment_i + \gamma_1 P_1 + \gamma_3 P_3 + \gamma_4 P_4 + \delta_1.PAN2011 + \delta_2.PAN2012 + \delta_3.PAN2013 + \delta_4.PAN2014 + \theta_1.treatment_i.P_1 + \theta_3.treatment_i.P_3 + \theta_4.treatment_i.P_4 + \omega_{it}$$

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; C_i are the additional set of beneficiary demographic characteristics indicating the IAH catchment area; $treatment_i$ is a binary indicator of whether individual i is in the home-based care recipient group (receives primary care in the home); P_t's are the four period indicators, where $P_t = 1$ in period t and 0 otherwise. We also included a set of binary indicators variables (PAN2011, PAN2012, PAN2013, PAN2014) for each panel, with the 2010 panel as the base category. ω_{it} is a random error term.

The Greek letters are parameters to be estimated. The parameter τ estimates the home-based care recipient–comparison difference in an outcome during the reference period—that is, $P_2 = 1$, which is the year before the first home visit; γ_t measures changes in the outcome for the comparison group over time; and the θ_t's 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 home-based care recipients and the matched comparison beneficiaries in the first year after starting home-based primary care minus the difference between these two sets of beneficiaries in the year before the home-based care recipients had their first home visit. This estimate is interpreted as the effect of home-based primary care. The impact estimate accounts for differences between the home-based care recipient 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.

In addition to the pooled analysis described above, we estimated Equation (2) separately for each of the five panels, dropping the set of binary indicators variables (PAN2011, PAN2012, PAN2013, PAN2014) for each panel.

Covariates. Although our propensity score matching resulted in comparison groups similar to the home-based primary care recipients along many dimensions, there could still have been important differences that affected the outcomes under study. Therefore, we included four types of control variables in the vector X_i: (1) baseline utilization variables, (2) demographic characteristics, (3) measures of health status, and (4) chronic condition indicators. In Table D.11, we provide the full list of covariates used in our analyses (not shown are the panel fixed effects and the period fixed effects). The covariates in the regression model are a subset of matching variables listed in Table D.5.

Table D.11. Beneficiary characteristics included as control variables for impact estimation

Variable	Home-based care recipient group mean[a]	Matched comparison group mean
Utilization characteristics		
Number of months since most recent inpatient hospital admission or observation stay before first home-based primary care visit		
1	23.1	23.1
2 or 3	35.6	35.6
4 or more	41.3	41.3
Demographic characteristics		
Age		
Younger than 65	3.9	3.8
65 to 74	9.3	9.4
75 to 79	10.3	10.4
80 to 84	19.8	22.0
85 or older	56.7	54.5
Female	66.3	66.1
Race/ethnicity		
White	80.1	80.0
Black or African-American	15.2	15.1
Hispanic	2.0	2.1
Asian	1.3	1.5
Other/unknown	1.4	1.2
Dually eligible for Medicare and Medicaid	20.3	20.4

Table D.11 (continued)

Variable	Home-based care recipient group mean[a]	Matched comparison group mean
Original reason for Medicare entitlement		
Old age	87.1	87.0
ESRD or ESRD and disability	12.4	12.5
Disability only	0.5	0.5
IAH catchment area		
Austin, Texas	13.2	13.2
Boston, Massachusetts	1.1	1.1
Brooklyn/North Shore, New York	19.9	19.9
Cleveland, Ohio	6.4	6.4
Dallas, Texas	9.7	9.7
Durham, North Carolina	9.0	9.0
Flint, Michigan	13.3	13.3
Jacksonville, Florida	6.0	6.0
Lansing, Michigan	9.1	9.1
Milwaukee, Wisconsin	4.9	4.9
Philadelphia, Pennsylvania	1.0	1.0
Portland, Oregon	1.3	1.3
Richmond, Virginia	1.6	1.6
Washington, DC	1.6	1.6
Wilmington, Delaware	1.9	1.9
Health status characteristics		
Probability of having two or more ADLs needing human assistance	90.0	89.7
HCC risk score	3.9	3.9
Individual HCCs		
HCC8: Metastatic cancer and acute leukemia	2.4	2.4
HCC9: Lung and other severe cancers	2.3	2.5
HCC11: Colorectal, bladder, and other cancers	3.4	3.8
HCC18: Diabetes with chronic complications	21.2	21.4
HCC21: Protein-calorie malnutrition	19.2	19.0
HCC27: End-stage liver disease	1.2	1.2
HCC29: Chronic hepatitis	0.5	0.5
HCC46: Severe hematological disorders	2.0	1.9
HCC52: Dementia without complications	41.0	41.6
HCC54: Drug/alcohol psychosis	2.9	2.8
HCC55: Drug/alcohol dependence	1.5	1.5
HCC57: Schizophrenia	2.2	2.4
HCC70: Quadriplegia	2.1	1.9
HCC71: Paraplegia	1.1	1.2
HCC103: Hemiplegia/hemiparesis	11.7	11.4
HCC106: Atherosclerosis of the extremities with ulceration or gangrene	4.4	4.4
HCC107: Vascular disease with complications	7.7	8.3
HCC157: Pressure ulcer of skin with necrosis through to muscle, tendon, or bone	2.2	2.0
HCC158: Pressure ulcer of skin with full thickness skin loss	4.1	4.0
HCC159: Pressure ulcer of skin with partial thickness skin loss	4.7	4.4
Chronically critically ill or medically complex diagnosis[a]	31.5	31.6
Anemia[a]	24.6	24.8
Fluid and electrolyte disorders[b]	42.7	42.3
Chronic conditions measured by Chronic Conditions Warehouse		
Number of chronic conditions	7.3	7.3
Number of chronic conditions squared	61.7	62.5

Table D.11 (continued)

Variable	Home-based care recipient group mean[a]	Matched comparison group mean
Alzheimer's or dementia	55.5	55.2
Acute myocardial infarction or ischemic heart disease	3.3	3.4
Asthma	8.9	8.9
Hip or pelvic fracture	7.6	7.4
Stroke or transient ischemic attack	18.8	18.9

[a]The mean can be interpreted as the sample percentage for characteristics that are binary (for example: female).

[b]Measured 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. Diagnosis codes for these conditions were drawn from Gagne et al. (2011).

ADL = activities of daily living; ESRD = end-stage renal disease; HCC = Hierarchical Condition Category; IAH = Independence at Home.

Regression model. The regression model we used varied by the outcome (Table D.10). We used ordinary least squares (OLS) with identity link function for all types of expenditures, negative binomial distribution with log link for the number of hospital and ED admissions, and the logit with log link for readmissions.

Adjustment to standard errors for clustering. There were multiple observations over time on each beneficiary in the sample. Because the observations on a given beneficiary in one period were clearly not independent of the observations on the same beneficiary in other periods, our estimator of the variance accounted for this time dependence of repeated observations.

Weighting. Our outcome variables were monthly averages (for expenditures), annualized counts (inpatient hospital admissions and emergency room visits), or binary indicators (30-day readmission). We weighted observations in each post-home-based primary care year by fractional eligibility weights that captured the share of months alive and in FFS during each post-period. We also used matching weights in all of our analyses. We assigned each home-based care recipient 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. For example, if a home-based care recipient was matched to five comparison beneficiaries (the typical set in our sample), each of the five comparison beneficiaries received a weight of 0.20. Comparison beneficiaries' matching weights ranged from 0.20 (if there were five matched comparisons for a particular home-based care recipient) to 1 (one matched comparison).

Parallel trends assumption. The difference-in-differences design rests on the assumption that the home-based primary care recipients in our sample would have followed a similar health status trajectory as the comparison group had they not started home-based primary care. It is a limitation of this design that the assumption of "parallel trends" between the baseline and the periods following the index date was not testable. However, since we had two outcomes periods prior to the index date we were able to consider trends in the pretreatment period. We could control for pre-existing trends by including linear time trends in the regression that were allowed to differ between home-based care and comparison groups, where impact estimates were effectively deviations from this trend. While doing so would relax the parallel trends assumption, it would impose a different assumption: that the one-year pretreatment trends continue throughout the two-year post period. We believe that trends from the baseline year backwards are not reliable proxies for trends from the baseline forward. The reason is that all sample

subjects experienced a major health event (resulting in an inpatient stay and post-acute care) during the year prior to the index date (the baseline year). This caused a sharp increase in Medicare expenditures and utilization relative to the year prior. On average, for both the home-based primary care recipients and comparison beneficiaries, we observed that Medicare expenditures returned to lower levels in the two years following the baseline year.

Even though we could not test the parallel trends assumption from the baseline into the post-treatment periods, we could evaluate whether the home-based primary care recipients and their matched comparisons had parallel trends over the two pretreatment periods. The regression specification shown in Equation (2) includes a parameter, $\emptyset_1$, which is an estimate of the difference in the pretreatment period trends between the home-based primary care recipients and the comparison group. If $\emptyset_1$ is statistically significant, it indicates that the difference between the two groups changed significantly from two years before the index date to the year immediately prior to the index date. Table D.12 shows the parameter estimates for the pretreatment trends. Most outcomes show statistically significant differences in the pretreatment trend between the home-based primary care recipients and their matched comparisons. However, as noted above, the health status of our sample members changed markedly over the course of the baseline year. Variation in the nature of the baseline health shock means that parallel trends in the pre-treatment periods do not guarantee parallel trends into the post-treatment period. Ultimately, we relied on our matching process to identify a comparison group whose utilization and Medicare expenditures provided an accurate account of how expenditures and utilization would have evolved for home-based primary care recipients had they not started home-based primary care. Our matching variables did include a subset of variables measured in the year prior to the baseline year but the majority of our matching variables were baseline year measures.

Table D.12. Estimated difference in pretreatment trend

	Difference-in-differences estimate	Standard error
Medicare expenditures PBPM		
Total expenditures	$6	$26
Inpatient hospital services	$108***	$19
Skilled nursing facilities	−$139***	$8
Home health services (Parts A and B)	$6**	$3
Hospice services	−$11***	$2
Outpatient services	$21***	$2
Physician or supplier services	$10***	$4
Durable medical equipment	$10***	$1
Service utilization		
Number of hospital admissions per beneficiary per year	0.07***	0.01
Number of hospital admissions for ACSCs per beneficiary per year	0.01***	0.01
Number of ED visits per beneficiary per year	−0.02*	0.01
Number of ED visits for ACSCs per beneficiary per year	< 0.01	<0.01
Probability of having a qualifying hospital discharge and an unplanned readmission within 30 days of discharge	0.9%***	0.3%
Total number of observations across all years: 671,257		

Source: Medicare claims and enrollment data for 2010–2016 obtained from the Virtual Research Data Center for home-based care and matched comparison group beneficiaries.

Note: Expenditures were measured per beneficiary per month.

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

ACSC = ambulatory care sensitive condition; ED = emergency department; PBPM = per beneficiary per month.

Intent to treat. We monitored home-based primary care recipients and comparison beneficiaries for utilization in the first six months after the index date, as described previously. 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. We knew that some in the home-based care recipient group would be short-term users—that is, they would stop having the majority (or any) of their E&M visits in their place of residence shortly after the initial six-month period. There are various explanations for this switch, some of which could be endogenous to the treatment (that is, affected by receiving primary care visits in the home) itself:

- Changes in health status that could lead to exit from home-based primary care include:
  - The original need for home-based primary care might have reflected a temporary need due to a surgical procedure or short-term medical treatment.
  - The home-based primary care patient's condition could worsen and lead to entry into a hospice or long-term care facility.
  - Timelier and coordinated primary care from a home-based primary care clinician could result in improved health status, reducing the need for home-based primary care.
- Changes in the patient's preferences or need for primary care in the home that are independent of home-based care or health status, such as:
  - A home-based primary care patient (or that patient's caregiver) may want more emphasis on specialist care, or what they perceive to be more sophisticated care, than what they are receiving from the home-based primary care clinician.
  - A comparison beneficiary might lose access to transportation from a formal or informal caregiver, which opens the door to home-based primary care.

Among all home-based care recipients, 14 to 16 percent stopped having the majority of their E&M visits by a primary care clinician in the home during months 7 through 12 after starting home-based primary care, with the percentage of beneficiaries leaving home-based primary care declining to 8 to 9 percent in months 13 to 18 and then increasing to 11 to 13 percent in months 19 to 24 (Table D.13). In each six-month period, about 4 to 6 percent of home-based care recipients died, 2 to 3 percent stopped home-based primary care and entered hospice, and less than 1 percent left FFS. Among those who stopped having visits in the home, 3 to 5 percent returned to home-based primary care in the following six-month period. Although there were few differences between those home-based care recipients attributed to an IAH practice and those receiving home care from a different clinician in baseline characteristics and previous health care utilization behavior, there were differences in disposition over time. In general, IAH-attributed beneficiaries were less likely to leave home-based primary care; however, those IAH-attributed beneficiaries who did leave home-based primary care were less likely to return in the following six-month period than home-based care recipients receiving care from a non-IAH clinician.

Similarly, factors affecting preferences, as well as access to, and need for, home-based primary care, can change for beneficiaries in the comparison group. In most panels, fewer than 3 percent of comparison beneficiaries had their first E&M visit by a primary care clinician in the

home during months 7 through 12. The same was true for months 13 through 18 and months 19 through 24.

Many of the circumstances that affect exit and entry into home-based primary care are unobservable, which means we could neither quantify nor control for differences in switching behavior in our models. In addition, it is likely that some of the effects of home-based primary care will affect future health outcomes and health care use and expenditures for some of those who left home-based primary care. If a large proportion switched during those time periods, our analysis would yield a diluted measure of the effect of home-based primary care in those months.

Table D.13. Changes in status of home-based care recipients over time, by six-month period, by panel

Group	Months 7 to 12	Months 13 to 18	Months 19 to 24
2010 panel			
Percentage who started six-month period and			
Remained in home-based primary care	72.5	79.6	81.1
Left home-based primary care	15.8	8.9	11.2
Died	5.6	5.1	4.8
Left FFS	0.2	0.3	0.2
Entered hospice	1.7	2.4	2.6
Left home-based primary care but returned to home-based primary care in following six-month period	4.3	3.5	n.a.
2011 panel			
Percentage who started six-month period and			
Remained in home-based primary care	71.7	80.8	80.1
Left home-based primary care	16.6	8.4	11.4
Died	5.7	4.9	5.8
Left FFS	0.3	0.5	0.1
Entered hospice	1.4	1.9	2.6
Left home-based primary care but returned to home-based primary care in following six-month period	4.3	3.5	n.a.
2012 panel			
Percentage who started six-month period and			
Remained in home-based primary care	71.9	81.5	82.5
Left home-based primary care	15.7	8.1	10.7
Died	5.3	4.5	4.9
Left FFS	0.5	0.4	0.4
Entered hospice	1.7	2.1	2.2
Left home-based primary care but returned to home-based primary care in following six-month period	4.9	3.5	n.a.
2013 panel			
Percentage who started six-month period and			
Remained in home-based primary care	73.3	79.8	78.1
Left home-based primary care	14.0	8.8	13.1
Died	6.3	4.8	5.1
Left FFS	0.6	0.5	0.2
Entered hospice	1.5	2.2	3.5
Left home-based primary care but returned in following six-month period	4.3	3.7	n.a.
2014 panel			
Percentage who started six-month period and			
Remained in home-based primary care	72.8	80.0	81.1
Left home-based primary care	14.8	8.2	12.3
Died	5.3	5.0	4.7
Left FFS	0.4	0.0	0.0
Entered hospice	1.1	1.6	0.4
Left home-based primary care but returned in following six-month period	3.5	2.7	n.a.

TABLE D.13 (continued)

FFS = fee-for-service; n.a. = not applicable.

B. Bayesian estimation

We estimated a full Bayesian model using both yearly and quarterly data. We discuss the details of the Bayesian model in this section.

Yearly full Bayesian model. We examined whether starting home-based primary care reduced expenditures using a Bayesian difference-in-differences analysis (Equation 3), which complements the frequentist analysis and permits intuitive, flexible inferential statements.

$$Y_{ijtp} = \alpha + X_i\beta + \tau z_{it} + \gamma_t + \mu_j + \delta_p + \theta_t z_{it} + a_i + b_{jtz_{it}} + \epsilon_{ijtp} \quad (3)$$

This model uses slightly different notation than its frequentist counterpart (Equation 2), for clarity of presentation of the random effects. We use i to index beneficiaries, $j = 1, \ldots, 15$ to index the IAH catchment areas that both home-based care recipients and comparison beneficiaries reside in, and $t = -1, \ldots, 2$ to index the four 12-month periods. Y_{ijtp} is the PBPM total Medicare expenditure measured for a beneficiary i from catchment area j in period t and in panel p; X_i is a set of beneficiary characteristics measured in the baseline (Table D.11); and z_{it} indicates whether beneficiary i in period t is a home-based primary care recipient.

Greek letters denote parameters to be estimated: α is a constant term; β contains the effects of the beneficiary characteristics; τ captures any baseline differences between home-based care recipients and comparison beneficiaries that persist despite matching; γ_t describes the secular effect of time t that applies to both home-based care recipients and comparison beneficiaries; μ_j describes the effect of being in catchment area j; and δ_p describes the effect of being a member of panel p. The θs are the difference-in-differences effects of interest. $t = 0$ corresponds to the baseline period, so γ_0 and θ_0 are both omitted from the model. Roman letters denote random effects: the a's are beneficiary-level random intercepts, which account for the correlation across repeated observations on a given beneficiary, and the b's are site-treatment (receiving home-based primary care)-period random intercepts. We assumed that the a's and b's each follow a univariate normal distribution. Finally, we weighted the regression using the same scaled composite weights as are used in the frequentist analysis, by multiplying the eligibility weight by the matching weight.

Within the Bayesian framework, our model estimation resulted in a posterior distribution of estimates for each coefficient. This framework has the benefit of facilitating intuitive, flexible inferential statements. The posterior distribution is composed of two component parts: the prior and the likelihood. We do not include domain-based research priors in this analysis. Instead, we assigned a standard normal prior distribution to each model parameter. We selected these priors to remain agnostic about whether each model parameter was positive or negative, and to protect against improbably large or improbably small estimates.

Using the posterior distribution of the coefficients from this regression, we estimated the difference-in-difference effect of home-based care on total Medicare expenditures in each period ($\theta_{-1}, \theta_1 - \theta_2$). In addition, we estimated the probability of reducing expenditures by at least $50

and of any decrease in expenditures. We also estimated the probability of increasing expenditures by $100, $200, and $300, and of any increase in expenditures.

Two-stage approximation. Because fitting Equation (3) as a single, unified model at the beneficiary level was computationally prohibitive, we fit the full Bayesian model using a two-stage approximation to decrease computational run times. In the first stage, we risk adjusted and aggregated—specifically, we used a mixed-effects model to calculate area-period-treatment group outcomes, adjusting for beneficiary-level covariates and accounting for correlations across repeated observations on the same beneficiary (Equation 4). We aggregated from a beneficiary-level data set to one in which each row represented a unique area-period-treatment group. For example, outcomes from all home-based care recipient beneficiaries in catchment area 1 in period 2 were aggregated. Using output from Stage 1, we then estimated the effect of home-based primary care using a Bayesian difference-in-differences framework in Stage 2 (Equation 5).

(4) $$\text{Stage 1: } Y_{ijtp} = A_{jz_{it}t} + X_{it}\beta + \delta_p + a_i + \varepsilon_{it}$$

The area-treatment-period effect A_{jzt} represents the estimated fixed effect for catchment area j and home-based care recipient group z in period t. The parameters β describe the effects of beneficiary-level control variables X_i, while beneficiary-level random effects a_i account for correlations across repeated observations on beneficiary i. The parameters δ describe the association between panel p and the outcome, with the 2010 panel serving as the reference group. The beneficiary-level random effects a_i and the overall error term ε_{it} were each assumed to come from a normal distribution with mean zero and its own variance. Similar to the frequentist model, we weighted the first-stage regression by the scaled composite weight in Stage 1. The aggregated area-period-treatment group estimates ($\hat{A}_{jzt}$) and associated standard errors (s_{jzt}) from the Stage 1 model were used in the Stage 2 full Bayesian difference-in-differences formulation regression (Equation 5).

(5) $$\text{Stage 2: } \hat{A}_{jzt} = \alpha + \tau z + \gamma_t + \mu_j + \theta_t z + b_{jzt} + \varepsilon_{jzt}$$

In the Stage-2 model, we included an overall intercept α, and controls for the secular time trend γ, catchment area μ, and treatment status τ. The parameters of interest, θ_t, represent the overall difference-in-differences effect at time t.

We assigned a standard normal distribution – Normal(0, 1) – as the prior for each fixed parameter: $\alpha \sim N(0,1), \tau \sim N(0,1), \gamma \sim N(0,1), \mu \sim N(0,1), \theta \sim N(0,1)$. The random effects b are the site-period-treatment group random intercepts from Equation (3). They have a standard normal prior with variance σ_b^2 estimated from the data. Our prior on the error term is given by $\varepsilon_{jzt} \sim Normal\left(0, s_{jzt}^2\right)$, where s_{jzt} indicates the standard error of the aggregated area-period-treatment group estimate $\hat{A}_{jzt}$.

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.

Quarterly full Bayesian model. Our quarterly full Bayesian model was identical to the yearly full Bayesian model, with two exceptions: (1) the outcome variable was measured at the quarterly, rather than yearly, level; and (2) the secular time trend and impact parameters (γ_t and θ_t) were given autoregressive rather than standard normal priors, to induce smoothing of the estimated time trends.

(6) Stage 1: $Y_{ijqp} = A_{jz_{it}q} + X_{iq}\beta + \delta_p + a_i + \varepsilon_{iq}$

(7) Stage 2: $\hat{A}_{jzq} = \alpha + \tau z + \gamma_t + \mu + \theta_t z + b_{jzq} + \varepsilon_{jzq}$

The equation for the Stage 2 model for the quarterly analysis (Equation 7) differs from the Stage 2 model for the pooled analysis (Equation 5) in the specification of priors for the parameters γ and θ. We assigned standard normal priors for the four quarters in the pre-baseline period: $\theta_q \sim N(0,1)$ for $q = 1, \ldots, 4$. During the baseline period $q = 5, \ldots, 8$, no priors were assigned, because we did not estimate an effect during these quarters ($\theta_5 = \cdots = \theta_8 = 0$). We assigned autoregressive priors to smooth the time trend of the quarterly effects in the post-home-based care recipient period: $\theta_q \sim N\left(\theta_{q-1}, \sigma_\theta\right)$ for $q = 9, \ldots, 16$. We also assigned autoregressive priors to the secular time trend γ. The first quarter is omitted as the reference category ($\gamma_1 = 0$), and for all other quarters we smooth: $\gamma_q \sim N\left(\gamma_{q-1}, \sigma_\gamma\right)$ for $q = 2, \ldots, 16$.

Bayesian model assumptions. There are two sets of assumptions made by the Bayesian analyses:

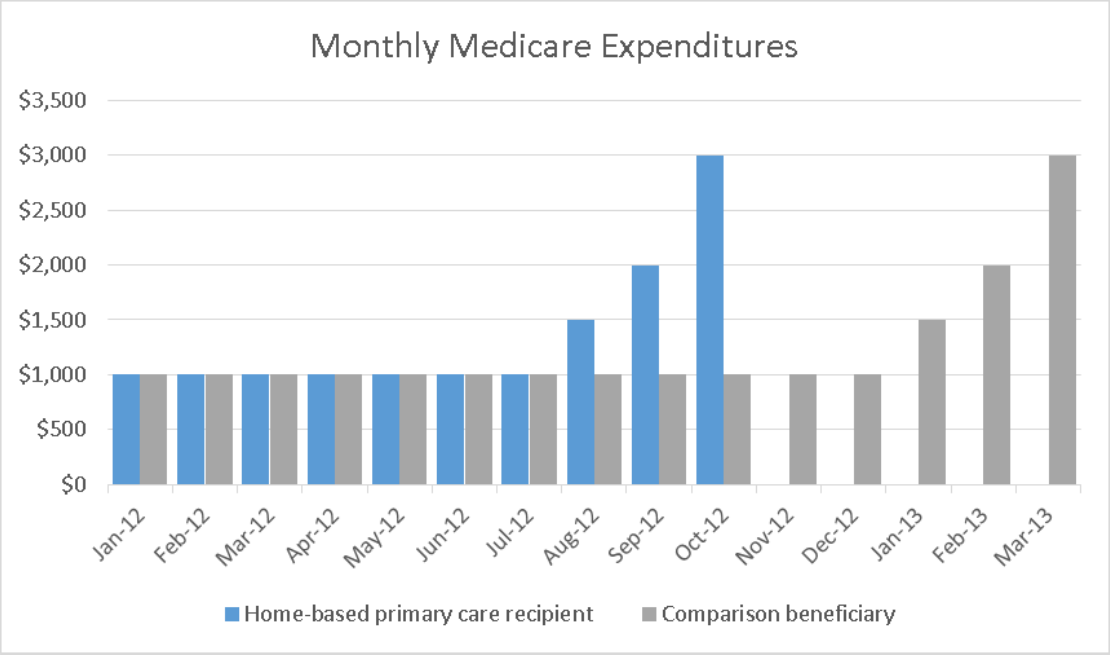
1. We assumed that random effects account for clustering. This assumption has implications for the standard errors around the Bayesian estimates.
2. We made prior assumptions about effects.
   a. We placed a standard normal prior distribution—denoted N(0,1)—on the overall effect of home-based primary care. By doing so, we incorporated a prior expectation that very large positive or negative effects of home-based primary care on expenditures were substantially less likely than small and moderate effects. We based our prior expectation on the general result that other evaluations of the effect of home-based primary care and other interventions for chronically ill Medicare beneficiaries rarely show effect sizes larger than two standard deviations. We centered the normal distribution at a mean of zero to remain agnostic about whether home-based primary care would reduce total expenditures. This prior does not increase precision of the impact estimates.
   b. For the quarterly home-based primary care impact analysis, we used a prior that induces borrowing of strength across quarters. This prior does increase precision of the impact estimates, relative to what we would have seen if we had conducted a frequentist analysis of quarterly effects.

C. Frequentist estimation with differential mortality

Medicare expenditures are often high in the final months and weeks of life. If there is a systematic difference in expected survival as of the index date (unrelated to the receipt of home-

based primary care) between the home-based primary care and comparison group[29] then death will occur more frequently within the observation period for the group with higher mortality. Even if the trajectory of expenditures in the months leading up to death is the same for both groups, if death occurs more frequently in the observation period for one group, then the observed average expenditures for that group will be higher in the period of analysis. Figure D.1 contains a hypothetical example showing monthly total Medicare expenditures for a home-based primary care recipient who dies in October of 2012 and a comparison beneficiary who dies in March of 2013. In this example, each beneficiary has the same trajectory of expenditures in the months leading up to death. However, if the outcomes period is calendar year 2012 then the PBPM expenditures for the home-based primary care recipient will be $1,350 versus $1,000 for the comparison beneficiary.

Figure D.1. Hypothetical example of the effect of differential mortality on average expenditures



We found differences in the unadjusted mortality rate during the first six months of the post-period between home-based care recipients in the panel sample and their matched comparison beneficiaries that varied across panels. In the 2010 and 2012 panels, the rate for the home-based care recipient group was lower than that of the comparison group; in the 2014 panel, the reverse was true; and in the 2011 and 2013 panels, the rates were the same (Table D.14). However, in the other time periods—months 7 through 12 and 13 through 24 after the index date—the mortality rate for home-based care recipients was higher, in every cohort, with the differences increasing with the length of follow-up. We cannot say whether these differences would have been observed in the absence of home-based primary care, but the observed differences suggest the

[29] Ideally, the matching process would result in a comparison group with the same expected survival (as of the index date) as the home-based primary care recipients. However, it is possible that there are factors that are not observable in claims data that cause differential expected survival between the two groups.

possibility of unobserved factors (that is, not controlled for by the matching variables) that affect expected survival as of the index date.

Table D.14. Unadjusted mortality rate for home-based care recipients and matched comparison beneficiaries, by panel (percentages)

Group	Died within first six months	Died within months 7 to 12 of index date	Died during second year after index date
2010 panel			
Home-based care recipients	15.3	11.7	18.1
Matched comparison beneficiaries	17.3	9.8	16.0
2011 panel			
Home-based care recipients	16.9	11.0	19.1
Matched comparison beneficiaries	16.8	10.2	16.3
2012 panel			
Home-based care recipients	16.4	11.6	18.0
Matched comparison beneficiaries	17.8	10.3	15.7
2013 panel			
Home-based care recipients	16.3	12.5	18.8
Matched comparison beneficiaries	16.6	10.0	16.5
2014 panel			
Home-based care recipients	17.1	12.6	18.6
Matched comparison beneficiaries	16.9	10.2	15.6

Source: Medicare claims and enrollment data for 2010–2016 obtained from the Virtual Research Data Center for home-based care recipients and matched comparison group beneficiaries.

Note: Data for the comparison group were weighted to reflect multiple comparison beneficiaries matched to individual home-based care recipients.

The difference-in-differences model described above relied on the matching process to produce a comparison group with the same expected survival (as of the index date) as the home-based primary care recipients and did not directly adjust for potential differences in baseline expected survival between the groups. We used a modeling approach developed by Deb (2016) to account for differences in baseline expected mortality, or survival probability, between home-based care recipients and their matched comparison beneficiaries in estimating Medicare expenditure differences. The model builds on the work of Lin et al. (1997) and is a simplified version of the model in Basu and Manning (2010).

The basic idea behind Deb's model is to first estimate a survival model to derive the predicted probability of death in each period (we used monthly periods). The predictors in the survival model include treatment status as well as a number of other baseline covariates (described in more detail below). A key assumption in this model is that home-based primary care itself does not affect mortality, but the model accounts for pre-existing differences in survival probability between home-based care recipients and the matched comparisons. Specifically, the inclusion of the treatment indicator in the survival model is intended to control for underlying mortality differences between the two sets of beneficiaries that would have existed even in the absence of the intervention. In the second step, we used a modified version of the difference-in-differences model presented in Equation (2) to estimate the effect of home-based primary care on monthly expenditures.[30] We included additional regressors from the

[30] The survival model results discussed in Chapter IV show the average monthly effect across the 12 months in an outcome period in order to facilitate comparison with our other impact estimates that represent average expenditures over the 12 month period.

survival model that account for mortality differences across beneficiaries. The survival model also provides weights for observations that represent the beneficiary's survival probability as of the end of the preceding month. Weighting in this manner is done to model expected expenditures and therefore, monthly observations with a low survival probability receive a lower weight in the expenditure analysis than observations with a higher survival probability.

The survival model. We first used a survival model to estimate the predicted probability of a beneficiary dying in each month. Survival estimates were based on home-based care recipients and comparison beneficiaries in all panel years pooled. We modeled survival starting from the beneficiary's index date since by construction all sample members were alive as of the index date.

The survival function $S_{i,k-1}(\cdot)$ measures the probability of survival until the end of month k-1:

$$P(T_i \geq k-1) = S_{i,k-1}(\beta_z' Z_i + \sigma'.C_i + \gamma.IAH_i + \beta_R treatment_i + \beta_{2011}.PAN2011 + \beta_{2012}.PAN2012 + \beta_{2013}.PAN2013 + \beta_{2014}.PAN2014 + + \beta_{RM}'.treatment.M_i + \beta_{R2011}.treatment.PAN2011 + \beta_{R2012}.treatment.PAN2012 + \beta_{R2013}.treatment.PAN2013 + \beta_{R2014}.treatment.PAN2014) \quad (8)$$

where T_i is the survival time of individual i; Z_i is a set of beneficiary characteristics measured in the baseline period (see Table D.15); C_i is a set of indicators for the IAH catchment areas; IAH_i is an indicator for whether the beneficiary was IAH attributed; and $treatment_i$ is a binary indicator of whether individual i is in the home-based care recipient group. We also included a set of binary indicators variables (PAN2011, PAN2012, PAN2013, PAN2014) for each panel, with the 2010 panel as the baseline. We interacted the home-based care recipient indicator with the panel indicators as well as a subset of variables in our model (M_i) to allow their effect on survival to vary by home-based care recipient status (see the last panel of Table D.15 for a list of these interactions).

We estimated one common survival function for all panels combined according to Equation (8). We used weights to account for the number of matched comparisons per home-based care recipient so that the two groups were the same size. Our model used a generalized gamma distribution for the survival function S(·) and the set of covariates listed in Table D.15.[31] For each beneficiary, we had up to 48 months of data, beginning two years before the index month. The index month is the 25th month, that is, k = 25. We measured survival starting from the beneficiary's index date since beneficiaries were alive at least until this date. Beneficiaries who moved out of FFS were considered censored in the period they lost FFS status and were not included in those periods.

[31] To inform our choice of the survival function, we compared the goodness-of-fit of models using different distributions and sets of controls. We considered five types of parametric survival distributions: (1) Weibull, (2) log logistic, (3) log normal, (4) generalized gamma, and (5) Gompertz. We also considered four sets of controls, which differed in the extent to which the home-based care status indicator was interacted with baseline covariates. In choosing the final model, we analyzed the log likelihood, the Akaike information criterion, and the Bayesian information criterion across these different models.

Table D.15. Beneficiary characteristics used in survival estimation

Variable
Home-based primary care status
Home-based primary care recipient
Utilization characteristics
Number of months since last inpatient admission before index date: 1; 2–3; 4-12
Demographic characteristics
Age: younger than 65, 65–74, 75–79, 80–84, 85 or older
Gender
Race/ethnicity: white, black or African American, Hispanic, Asian, American Indian/Alaskan Native, other, or unknown
Dually eligible for Medicare and Medicaid
Original reason for Medicare entitlement: old age, ESRD or ESRD and disability, disability only
IAH attributed
Panel indicators
IAH catchment areas
ADLs
Predicted probability of having two or more ADLs as of index date
Predicted probability of having two or more ADLs 12 months before the index date
Health status characteristics
HCC risk score, as of index date (quintile)
HCC risk score, 12 month prior (quintile)
Individual HCCs
HCC8: Metastatic cancer and acute leukemia
HCC9: Lung and other severe cancers
HCC10: Lymphoma and other cancers
HCC11: Colorectal, bladder, and other cancers
HCC18: Diabetes with chronic complications
HCC21: Protein-calorie malnutrition
HCC27: End-stage liver disease
HCC28: Cirrhosis of liver
HCC29: Chronic hepatitis
HCC46: Severe hematological disorders
HCC51: Dementia with complications
HCC52: Dementia without complications
HCC54: Drug/alcohol psychosis
HCC55: Drug/alcohol dependence
HCC57: Schizophrenia
HCC58: Major depressive, bipolar, and paranoid disorders
HCC70: Quadriplegia
HCC71: Paraplegia
HCC85: Congestive heart failure
HCC103: Hemiplegia/hemiparesis
HCC106: Atherosclerosis of the extremities with ulceration or gangrene
HCC107: Vascular disease with complications
HCC111: Chronic obstructive pulmonary disease
HCC134: Dialysis status
HCC157–159: Pressure ulcer of skin
Chronic conditions measured by Chronic Conditions Warehouse
Number of chronic conditions, and number of chronic conditions squared
Individual chronic conditions
Alzheimer's disease or senile dementia
Acute myocardial infarction or ischemic heart disease
Asthma
Hip or pelvic fracture
Stroke or transient ischemic attack
Anemia[a]
Fluid and electrolyte disorders[a]
Health care expenditures
Average monthly expenditures, in year before index date (quintile)
Average monthly expenditures, 13–24 months before index date (quintile)

Table D.15 (continued)

Variable
Variables interacted with home-based primary care status[b]
Dually eligible for Medicare and Medicaid
Original reason for Medicare entitlement: old age, ESRD or ESRD and disability, disability only
Probability of having 2+ ADLs, as of index date
Number of chronic conditions, and number of chronic conditions squared
HCC risk score, as of index date (quintile)
Individual HCCs
HCC8: Metastatic cancer and acute leukemia
HCC9: Lung and other severe cancers
HCC27: End-stage liver disease
HCC134: Dialysis status

[a]Measured with claims from the most recent inpatient stay and observation stay in the 12 months before the index date. Diagnosis codes for these conditions were drawn from Gagne et al. (2011).

[b]Interaction variables were added to allow differences in how covariates predict mortality across the home-based care recipients versus comparison groups. We chose a subset of existing covariates that could affect the probability of survival differentially for home-based care recipients and comparison beneficiaries. We then tested four models that varied the set of interactions and chose the one with the best fit.

ADL = activities of daily living; ESRD = end stage renal disease; HCC = Hierarchical Condition Category; IAH = Independence at Home.

After obtaining the survival estimates, we calculated the probability of dying (h) within a given month by taking the difference in the probability of survival at the end of the month S_k from the probability of survival at the end of the previous month, S_{k-1}. Specifically, h_{ik} is the probability of dying over period k for individual i and is derived as $h_{ik} = S_{i,k-1}$-$S_{i,k}$. To obtain the estimates of S_{k-1} and S_k, we applied the predicted probability values from the estimated survival function for each individual i over months k = 1, 2,…,48. For k = 1,2,…,24, the period before the index date, the probability of survival S_{ik} =1 and $h_{ik} = 0$ for all i because all subjects were alive for the two years before the index date.

Difference-in-differences cost regression specification. We next used the difference-in-differences framework to estimate the effect of home-based primary care on month-specific costs, taking into account the predicted probability of dying (h_{ik}) and the interaction between home-based care status and the probability of dying. To determine the effect of home-based primary care, conditional on survival, we used regression-adjusted differences between the home-based care recipients and the comparison beneficiaries to derive impact estimates that weight beneficiaries by their probability of survival.

We built on the main analysis specified in Equation (2) and estimated the survival adjusted model in Equation (9):

$$(9) \quad Y_{ik} = \hat{S}_{i,k-1}(\cdot) * (\alpha + \beta'.X_i + \sigma'.C_i + \tau.treatment_i + \beta_h h_{i,k} + \beta_{Rh} treatment_i h_{i,k} + \gamma_1 P1 + \gamma_3 P3 + \gamma_4 P4 + \delta_1.PAN2011 + \delta_2.PAN2012 + \delta_3.PAN2013 + \delta_4.PAN2014 + \theta_1.treatment_i.P_1 + \theta_3.treatment_i.P_3 + \theta_4.treatment_i.P_4 + \omega_{it})$$

where Y_{ik} is total monthly Medicare expenditures for beneficiary i in month k; $S_{i,k-1}(\cdot)$ is the predicted probability of surviving until the end of the previous month (month k-1), as defined in Equation (8); $treatment_i$ is a binary indicator of whether individual i is in the home-based care group; and $h_{i,k}$ is the predicted probability of death. The remaining covariates are the same as in the main analysis: X_i is the set of beneficiary characteristics shown in Table D.11); C_i is a set of

IAH catchment area indicators; P_t's are the period year indicators; and PAN2011 through PAN2014 are panel indicators. As before, ω_{it} is a random error term.

In Equation (9), we added supplementary controls to account for differences in survival across beneficiaries. Specifically, the probability of dying in the month (h_{ik}) was included to allow for the possibility that cost accumulation changes as one approaches death. The term h_{ik} measures the *predicted* probability of dying in each month for all beneficiaries, regardless of their actual survival or censoring status. Therefore, because we were accounting for differences in expected mortality across beneficiaries in our expenditure analysis, this model allowed us to overcome the censoring of expenditures issue. The interaction of home-based primary care and the probability of dying was included to allow for the possibility that cost accumulation changed as an individual approached death differentially for those in the home-based care group versus the comparison group.

We calculated the effect of home-based primary care on expenditures in a given month *k* using the estimates from Equation (9) to compare two counterfactuals as shown in Equation (10). This equation compared expenditures for the home-based care group with expenditures for the comparison group, each weighted by the survival function applicable to the home-based care group. In other words, it held predicted survival constant while calculating the difference in expenditures due to home-based primary care. Using this approach, we explicitly accounted for underlying mortality differences between the two sets of beneficiaries in estimating our difference-in-differences effects, but we did not explicitly allow home-based primary care itself to affect survival.[32]

$$
\begin{aligned}
\tau_k = \sum_{i=1}^{N} \hat{S}_{i,k-1} & \left(\begin{array}{l} \beta_x X_i + \delta_1 \cdot PAN2011 + \delta_2 \cdot PAN2012 + \delta_3 \cdot PAN2013 + \\ \delta_4 \cdot PAN2014 + \beta_m M_i + \beta_t treatment_i + \beta_{t,m} treatment_i M_i \end{array} \right) \times \\
& \left[\begin{array}{l} \alpha + \beta' X_i + \sigma' C_i + \tau \cdot treatment_i + \beta_h h_{i,k} + \beta_{th} treatment_i h_{i,k} + \gamma_1 P_1 + \gamma_3 P_3 + \\ \gamma_4 P_4 + \delta_1 \cdot PAN2011 + \delta_2 \cdot PAN2012 + \delta_3 \cdot PAN2013 + \delta_4 \cdot PAN2014 + \\ \theta_1 \cdot treatment_i \cdot P_1 + \theta_3 \cdot treatment_i \cdot P_3 + \theta_4 \cdot treatment_i \cdot P_4 \end{array} \right] - \\
& \left[\begin{array}{l} \alpha + \beta' X_i + \sigma' C_i + \beta_h h_{i,k} + \gamma_1 P_1 + \gamma_3 P_3 + \gamma_4 P_4 + \delta_1 \cdot PAN2011 + \\ \delta_2 \cdot PAN2012 + \delta_3 \cdot PAN2013 + \delta_4 \cdot PAN2014 \end{array} \right]
\end{aligned} \tag{10}
$$

Estimation and bootstrapping. We estimated Equation (9) using OLS, weighted by the prior month's probability of survival, $S_{i,k-1}$ in order to give observations with a low likelihood of survival (a high probability of censoring) a lower weight in the expenditure analysis and those with a high probability of survival (a low probability of censoring) a higher weight. We also used matching weights as in the main analysis. Again, in all estimates, we adjusted the standard errors for clustering at the unique beneficiary level to allow for serial correlation of the outcomes of individual beneficiaries over time in our longitudinal data set.

[32] This is seen in Equation (9) where the survival weights are being held constant. Deb (2016) also presents another estimate that measures the effect of home-based primary care on costs, thereby taking into account the effect of home-based primary care on survival.

Because estimation of Equation (9) involves generated regressors—$S_{i,k-1}$ and $h_{i,k}$—we bootstrapped our estimates and standard errors. We took a multiple-imputation approach to bootstrapping (Deb 2016). Specifically, we drew 50 random samples with replacement of individual beneficiaries from the original sample and estimated Equation (8) to derive survival estimates. For each iteration, we used those estimates to obtain the predicted probability of dying (h) for each month k and the probability of survival S_{k-1}until the end of the previous month on the full, original sample. We saved these estimates as vectors $S_1, h_1,$ through S_{50}, h_{50}.

Next, we ran Equation (9) 50 times using the values of $S_1, h_1,$ through S_{50}, h_{50} for the $S_{i,k-1}$ and the $h_{i,k}$. We combined the estimates and standard errors across the 50 samples. We then calculated the estimated effect of home-based primary care on expenditures in a given period P_t (the θ_t's) using the average of the 50 estimated effects and its standard error as the square root of average of variances of these effects. Finally, we compared our results to those derived from Equation (2), which does not directly account for differences in mortality between home-based care recipients and comparison beneficiaries.

D. Robustness checks

In addition to estimating our main regressions using the modeling approach described earlier, we implemented a commonly used robustness check to test the sensitivity of our results to outliers. Specifically, we reset the expenditures of people above the 99th percentile to the 99th percentile, and similarly reset the expenditures of matched comparison group members with expenditures or acute care use above the 99th percentile of that distribution.

We also tested whether any differences in impact estimates between the standard and the survival-adjusted models could be induced because we used different measures of monthly expenditures for the two models. Our standard regression models used average monthly expenditures in each 12-month period as the outcome variables, whereas the model that accounted for potential effects of mortality on expenditures used monthly data. Our sensitivity test required fitting a modified version of our standard model using monthly data on expenditures as outcomes. We also carried out outlier sensitivity analyses for these models by trimming both average and actual monthly expenditures at the 99th percentile.

In summary, we compared results from four models as robustness checks: (1) regressions with average monthly expenditure, (2) regressions with actual monthly expenditures, (3) regressions with trimmed average monthly expenditures, and (4) regressions with trimmed average monthly expenditures.

V. METHODS FOR SURVEY TO INVESTIGATE SELF-SELECTION BIAS

A. Development of the survey

To answer research questions about the potential for selection bias in the home-based primary care impact analysis, we conducted a survey of Medicare beneficiaries who received care from IAH practices and a matched set of comparison beneficiaries who did not receive home-based primary care. We designed the survey instrument to provide information on how Medicare FFS beneficiaries who chose to start home-based primary care differed from matched comparison beneficiaries who did not receive primary care in their homes. The survey focused on access barriers and attitudes toward, and preferences for, health care delivery, as well as health status. Where possible, we used or modified questions from existing surveys, such as the MCBS or our 2013-2015 survey of IAH enrollees (described in Chapter IV). A search of the literature on home-based primary care, home health care, hospice, and palliative care yielded a list of possible reasons for choosing or not choosing home-based primary care. The content of the resulting survey instrument was designed to reflect the goals of the survey; specifically, to aid in detecting whether there was nonrandom self-selection bias into home-based primary care and determining whether that bias was likely to affect the effect of home-based primary care on health care expenditures and use.

Before fielding the survey, we conducted pre-test interviews to learn whether respondents had difficulty completing the questionnaire and whether the instrument adequately captured respondents' reasons for starting home-based primary care. We formulated recommendations for issues (such as wording or response formats that resulted in confusion) that arose during the pre-test interviews and revised the survey instrument as necessary.

B. Sample construction

The process for constructing the survey sample was the same as for the panel, with the following exceptions:

- **Timing of index date.** The survey sample members had an index date—for home-based care recipients, this was the date of the first home visit—between January 1, 2015, and June 30, 2016. This was later than the latest panel in the home-based primary care impact analysis, which consisted of beneficiaries with an index date during 2014. The intent was to administer the survey with the shortest possible elapsed time since the index date, increasing the likelihood that respondents would recall the reasons for starting (or not starting) home-based primary care, as well as limiting loss of sample size due to death before contact.
- **Attribution period.** To allow sufficient claims run-out, the attribution period (the period of time after the index date in which we monitored for home-based primary care utilization) was shortened to four months for the survey sample (compared to six months for the home-based primary care impact analysis). For example, for a home-based care recipient whose first home visit was in June 2016, we looked for additional E&M visits in the home through October 2016. Sample extraction occurred in December 2016, allowing two months of claims run-out for the latest possible attribution period.
- **ADL eligibility criterion.** We determined whether the beneficiary had two or more ADLs requiring human assistance using the most recently available assessment data (as of the

index date), rather than the prediction model used for the home-based primary care impact analysis. The prediction model requires claims diagnoses codes as inputs, and the survey time frame straddled the transition from ICD-9 to ICD-10, which the prediction model was not equipped to handle.

- **IAH-attributed beneficiaries.** The home-based primary care group in the survey sample was limited to Medicare beneficiaries who were attributed to practices participating in the IAH demonstration. For the home-based primary care impact analysis, this restriction was relaxed (to augment sample size) so that home-based care recipients need not have been attributed to IAH practices.

C. Matching

The matching process to construct the survey sample comparison group was similar to the process used to construct the comparison group for the home-based primary care impact analysis, but with the following differences:

- **Nonresponse bias.** We did not know beforehand who would respond to the survey. If home-based care recipients from a specific subset of matching characteristics (for example, those 85 and older or those with an above average number of ED visits in the 12 months before the index date) were less likely to respond to our survey than their matched comparisons, balance on those characteristics in the final sample would be affected. To protect against this possibility, we stratified the sample into quintiles of propensity score and matched separately within each of the five strata. This approach provided additional protection against imbalance caused by nonresponse by ensuring that the pool of home-based care recipients and the pool of comparison beneficiaries were similar in each stratum.
- **Unique versions of each comparison beneficiary.** We aimed to match two comparison beneficiaries to one home-based care recipient. As with the home-based primary care analysis, we had more than one version of each potential comparison beneficiary in the comparison pool (associated with different index dates). In the earlier analysis, it was possible to match multiple versions of the same comparison beneficiary to different home-based care recipients in the same round of matching—for example, in round 1, February's version of C1 could be matched to T1, and July's version of C1 could be matched to T2. However, for the survey analysis, each matched comparison beneficiary was asked to participate in the survey. Therefore, we could include, and therefore match, only one version of each comparison beneficiary. To avoid having more than one version of a comparison beneficiary get matched, we used the following modified step-by-step optimal matching approach:

  1. Run optimal matching, using the "optmatch" package in R, fixing the number of comparisons matched to each home-based care recipient to be one. At this step, we allowed more than one version of a potential comparison beneficiary to be an optimal match for separate home-based care recipients.
  2. Recycle the pool of unmatched comparison beneficiaries, after removing all remaining versions for those comparison beneficiaries with one or more versions matched in step 1. This ensured that all other versions of matched comparison beneficiaries could not be matched in subsequent steps.

3. First, examine all matched pairs, identifying those comparison beneficiaries with more than one version matched to different home-based care recipients—for example, C1Feb matched to T1 and C1July matched to T2. Then calculate the difference in propensity scores, which we will refer to as the "distance," for the pairs C1Feb–T1 and C1July–T2 and keep the matched pair with the smallest distance—for example, if C1Feb–T1 had the smaller distance, we kept this matched pair, removed C1July from the potential comparison pool, and rematched T2 from the remaining pool of potential comparison beneficiaries.
4. If again multiple versions of the same unique comparison beneficiary were matched, repeat steps 2 and 3. Otherwise move to step 5. Steps 2 and 3 needed to be repeated until each home-based care recipient was matched to a unique comparison beneficiary.
5. Repeat steps 1 through 4 one more time so that each home-based care recipient gets matched to another unique comparison beneficiary. This resulted in each home-based care recipient having two matched comparison beneficiaries. No comparison beneficiary was matched to more than one home-based primary care beneficiary.

- **Variables used for exact match.** For the survey, we used five measures for exact matching: (1) the number of months since the beneficiary's last inpatient admission (1, 2 to 3, or 4 or more months); (2) whether the beneficiary had an observation stay and no inpatient admission in the previous 12 months; (3) catchment area (the collection of zip codes for home-based care recipients attributed to an IAH practice); (4) propensity score quintile; and (5) a calendar year indicator. The calendar year indicator specified whether a beneficiary was eligible in calendar year 2015 or 2016. For home-based care recipients, the calendar year indicator showed whether a beneficiary received primary care visits in the home in 2015 or 2016. For comparison beneficiaries who had no home-based primary care use between January 2013 and December 2015, their calendar year indicator was set to 2015. For comparison beneficiaries who had no primary care visits in their home in 2016, their calendar year indicator was 2016.

A usual statistic to assess the balance of the sample before and after matching is the standardized difference, which is akin to an effect size (Stuart 2010). The effect size literature suggests that the difference in covariate means between matched groups be less than 0.25 pooled standard deviations for the groups to qualify as balanced, a threshold suggested by Rubin (2001). The absolute value of the standardized difference was less than 0.10 on all matching variables for the survey sample. Table D.16 shows detailed information on standardized difference on specific matching variables. In addition to reviewing the standardized differences on matching variables, we calculated the weighted means and standardized difference for the propensity scores, which was less than 0.10. Overall, the home-based care recipients and matched comparison groups were well balanced.

Our final sample of target respondents consisted of 1,820 home-based care recipients and 3,640 matched comparison beneficiaries.

Table D.16. Covariate balance for survey sample

	Potential comparison group mean	Matched comparison group mean	Home-based primary care group mean	Standardized difference between home-based care and matched comparison
Beneficiary characteristics used for exact matching				
Number of months since last hospital admission				
1	0.092	0.174	0.174	0.000
2–3	0.199	0.331	0.331	0.000
≥4	0.708	0.495	0.495	0.000
Observation stay only in prior year (no hospital admission)	0.035	0.041	0.041	0.000
IAH catchment area				
Austin, Texas	0.065	0.064	0.064	0.000
Boston, Massachusetts	0.024	0.020	0.020	0.000
Cleveland, Ohio	0.056	0.049	0.049	0.000
Dallas, Texas	0.072	0.069	0.069	0.000
Durham, North Carolina	0.140	0.194	0.194	0.000
Flint, Michigan	0.149	0.144	0.144	0.000
Jacksonville, Florida	0.061	0.047	0.047	0.000
Lansing, Michigan	0.084	0.073	0.073	0.000
Milwaukee, Wisconsin	0.063	0.084	0.084	0.000
New York City combined (Brooklyn and Long Island)	0.222	0.178	0.178	0.000
Philadelphia, Pennsylvania	0.010	0.007	0.007	0.000
Portland, Oregon	0.009	0.019	0.019	0.000
Richmond, Virginia	0.005	0.004	0.004	0.000
Washington, DC	0.013	0.018	0.018	0.000
Wilmington, Delaware	0.029	0.030	0.030	0.000
Carrier claims year				
2015	0.771	0.702	0.702	0.000
2016	0.229	0.298	0.298	0.000
Propensity score stratum				
1 (0, 0.00156]	0.567	0.200	0.200	0.000
2 (0.00156, 0.00257]	0.206	0.200	0.200	0.000
3 (0.00257, 0.00395]	0.116	0.200	0.200	0.000
4 (0.00395, 0.00748]	0.078	0.200	0.200	0.000
5 (0.00748, 0.119]	0.034	0.200	0.200	0.000
Demographic characteristics				
Number of specialist visits (non-inpatient setting)	27.004	22.875	21.968	−0.056
Number of ED visits	1.090	1.538	1.531	−0.002
Age				
Younger than 65	0.109	0.110	0.103	−0.021
65–79	0.415	0.315	0.311	−0.008

Table D.16 (continued)

	Potential comparison group mean	Matched comparison group mean	Home-based primary care group mean	Standardized difference between home-based care and matched comparison
79 or older	0.476	0.575	0.586	0.021
Race				
White	0.773	0.777	0.773	−0.011
Black or African-American	0.173	0.177	0.179	0.004
Other	0.054	0.045	0.048	0.014
Dual eligibility status	0.241	0.274	0.269	−0.010
Female	0.657	0.682	0.681	−0.002
Original reason for Medicare entitlement				
Age	0.750	0.743	0.752	0.021
Disability	0.232	0.249	0.240	−0.022
ESRD or ESRD and disability	0.017	0.008	0.008	0.003
ADL				
Number of ADLs				
2	0.144	0.066	0.071	0.021
3–4	0.297	0.257	0.250	−0.016
5–6	0.559	0.677	0.679	0.004
Missing data on feeding ADL	0.069	0.095	0.097	0.007
Recency variables				
Months since last wheelchair code as of index date				
Less than 3 months	0.073	0.170	0.199	0.074
3–12 months	0.024	0.033	0.034	0.003
Not observed in 12 months	0.902	0.797	0.768	−0.071
Months since last wheelchair code 12 months prior				
Less than 3 months	0.026	0.045	0.045	0.003
3–12 months	0.017	0.029	0.025	−0.022
Not observed in 12 months	0.957	0.927	0.930	0.012
Months since last hospital bed code as of index date				
Less than 3 months	0.030	0.123	0.141	0.053
3–12 months	0.013	0.026	0.025	−0.003
Not observed in 12 months	0.957	0.851	0.834	−0.048
Months since last hospital bed code 12 months prior				
Less than 3 months	0.013	0.031	0.031	0.000
3–12 months	0.009	0.025	0.023	−0.016
Not observed in 12 months	0.978	0.944	0.947	0.011
Months since last home oxygen code as of index date				
Less than 3 months	0.030	0.042	0.037	−0.028
3–12 months	0.011	0.010	0.011	0.005
Not observed in 12 months	0.959	0.947	0.952	0.022

Table D.16 (continued)

	Potential comparison group mean	Matched comparison group mean	Home-based primary care group mean	Standardized difference between home-based care and matched comparison
Months since last home oxygen code 12 months prior				
Less than 3 months	0.020	0.025	0.021	−0.022
3–12 months	0.008	0.007	0.005	−0.014
Not observed in 12 months	0.972	0.969	0.973	0.026
Time since first diagnosed with Alzheimer's disease or senile dementia				
Less than 3 months	0.016	0.077	0.058	−0.076
3–12 months	0.081	0.132	0.135	0.006
More than 12 months	0.268	0.404	0.397	−0.014
Never	0.634	0.386	0.410	0.049
Time since first diagnosed with COPD				
Less than 3 months	0.009	0.025	0.017	−0.052
3–12 months	0.053	0.040	0.043	0.015
More than 12 months	0.409	0.411	0.404	−0.015
Never	0.529	0.524	0.536	0.024
Time since first diagnosed with heart failure				
Less than 3 months	0.011	0.036	0.027	−0.052
3–12 months	0.073	0.056	0.061	0.020
More than 12 months	0.499	0.531	0.525	−0.012
Never	0.416	0.376	0.387	0.022
Time since first diagnosed with hip pelvic fracture				
Less than 3 months	0.009	0.030	0.018	−0.076
3–12 months	0.058	0.041	0.052	0.053
More than 12 months	0.076	0.110	0.112	0.006
Never	0.857	0.820	0.819	−0.002
Time since first diagnosed with osteoporosis				
Less than 3 months	0.006	0.012	0.009	−0.029
3–12 months	0.035	0.029	0.030	0.008
More than 12 months	0.343	0.365	0.354	−0.022
Never	0.616	0.594	0.607	0.025
HCC 18, diabetes with chronic complications, 12 months prior	0.224	0.234	0.215	−0.043
HCC 27, end-stage liver disease, 12 months prior	0.007	0.004	0.003	−0.019
HCC 28–29, cirrhosis of the liver, chronic hepatitis, 12 months prior	0.016	0.017	0.016	−0.006
HCC 57–58, schizophrenia, major depressive, bipolar, and paranoid disorders, 12 months prior	0.121	0.156	0.143	−0.035
HCC 70–71, quadriplegia, paraplegia, 12 months prior	0.016	0.022	0.020	−0.011
HCC 103–104, hemiplegia/hemiparesis, monoplegia, other paralytic syndromes, 12 months prior	0.042	0.070	0.064	−0.023

Table D.16 (continued)

	Potential comparison group mean	Matched comparison group mean	Home-based primary care group mean	Standardized difference between home-based care and matched comparison
HCC 134, dialysis status, 12 months prior	0.033	0.025	0.022	−0.018
HCC 157–159, pressure ulcer of skin with necrosis or skin loss, 12 months prior	0.027	0.041	0.042	0.008
Health status				
Number of CCW chronic condition categories				
<6	0.217	0.234	0.238	0.009
6–9	0.517	0.495	0.502	0.014
>9	0.266	0.271	0.260	−0.024
HCC score	3.393	3.896	3.897	0.000
HCC 8, metastatic cancer	0.022	0.021	0.023	0.017
HCC 9-10, lung, lymphoma, and other cancers	0.052	0.036	0.036	0.001
HCC 11-12, colorectal, bladder, breast, prostate, and other cancers	0.116	0.099	0.100	0.003
HCC 18, diabetes with chronic complications	0.275	0.280	0.269	−0.026
HCC 21, protein-calorie malnutrition	0.117	0.168	0.173	0.013
HCC 27, end-stage liver disease	0.011	0.009	0.008	−0.006
HCC 28−29, cirrhosis of liver and chronic hepatitis	0.021	0.022	0.019	−0.021
HCC 46, severe hematological disorders	0.015	0.014	0.014	0.000
HCC 48, coagulation defects and other specified hematological disorders	0.169	0.142	0.134	−0.024
HCC 51, dementia with complications	0.086	0.177	0.182	0.012
HCC 52, dementia without complications	0.202	0.359	0.332	−0.056
HCC 54-55, drug/alcohol psychosis and drug/alcohol dependence	0.062	0.078	0.074	−0.017
HCC 57-58, schizophrenia, major depressive, bipolar, and paranoid disorders	0.169	0.246	0.232	−0.031
HCC 70-71, quadriplegia, paraplegia	0.028	0.048	0.050	0.010
HCC 72, spinal cord disorders/injuries	0.028	0.028	0.029	0.008
HCC 85, congestive heart failure	0.427	0.453	0.456	0.007
HCC 96, specified heart arrhythmias	0.379	0.380	0.387	0.016
HCC 103-104, hemiplegia/hemiparesis, monoplegia, other paralytic syndromes	0.092	0.128	0.138	0.030
HCC 106, atherosclerosis of the extremities with ulceration or gangrene	0.041	0.041	0.048	0.034
HCC 107-108, vascular disease with or without complications	0.458	0.465	0.457	−0.017
HCC 111, chronic obstructive pulmonary disease	0.319	0.325	0.317	−0.018
HCC 134, dialysis status	0.045	0.036	0.032	−0.024
HCC 136-138, chronic kidney disease, stage 3–5	0.076	0.075	0.076	0.006
HCC 139-140, chronic kidney disease stage 1–2, unspecified renal failure	0.055	0.048	0.049	0.003

Table D.16 (continued)

	Potential comparison group mean	Matched comparison group mean	Home-based primary care group mean	Standardized difference between home-based care and matched comparison
HCC 157-159, pressure ulcer of skin with necrosis or skin loss	0.065	0.115	0.135	0.059
Anemia	0.202	0.143	0.173	0.084
Depression	0.402	0.485	0.473	−0.025
Fluid and electrolyte disorders	0.329	0.310	0.330	0.044
MS-DRG with major complicating condition or complicating condition	0.510	0.566	0.575	0.018
Health care expenditures and utilization in 12 months before index date				
Average monthly expenditures	4354.080	4718.889	4796.790	0.020
Hospice utilization in the past 12 months	0.004	0.016	0.022	0.046
Number of hospitalizations	1.770	2.177	2.036	−0.082
Number of observation stays	0.203	0.305	0.275	−0.043
Propensity score	0.002	0.006	0.006	0.053
Sample sizes				
Number of potential comparison beneficiaries[a]	846,121			
Number of matched comparison beneficiaries [unweighted]	3,640			
Number of matched comparison beneficiaries [weighted]	1,820			
Number of home-based care recipients	1,820			

[a]Included up to 12 versions of each unique potential comparison beneficiary.

ADL = activities of daily living; COPD = chronic obstructive pulmonary disease; ED = emergency department; ESRD = end stage renal disease; HCC = Hierarchical Condition Category.

D. Data collection

Mode of collection. Our approach to the data collection design was built upon lessons learned from the 2013–2015 IAH beneficiary survey (described in Appendix B) about response preferences, challenges, and locating a hard-to-reach population. Given the large number of telephone completes in that survey, we felt that a telephone survey would be well suited to our target population (many of whom might have difficulty reading and filling out a self-administered hard-copy survey because of physical or cognitive impairments). In addition, because of the expected high level of impairments among this population, we anticipated significant participation of proxy respondents. To facilitate proxy help in responding, we provided a pencil-and-paper survey to telephone nonrespondents approximately two months after the start of telephone dialing.

Waves of data collection. To minimize the lag time between the respondent index date and the administration of the survey, as well as attrition due to death, we separated the sample into three groups: those with an index date in January through June 2015 were assigned to wave 1,

July to December 2015 were assigned to wave 2, and January through June 2016 were assigned to wave 3.

Locating efforts. We also knew, from our experience with the IAH Patient Survey, that inaccurate or missing contact information would pose a challenge to survey response. The inability to locate respondents in the IAH Patient Survey accounted for about half of the noncompleted cases. We expected to deal with a similar issue because (1) an address in the Medicare Enrollment Database could be the address of the "guardian/executor" to whom the explanation of benefits and reimbursements is sent and might not correspond to the residence of the sample member, and (2) data from the Medicare Enrollment Database do not include telephone numbers. In addition, the contact information for the respondent could have been from 9 to 22 months old by the time of the survey and respondents might have moved or passed away after they were selected for the survey sample.

Before each wave of data collection, our locating specialists worked with a national telephone and address look-up company and other online databases to conduct multiple rounds of searches of each sample member for accurate and complete contact information. After confirming addresses, we sent a letter on CMS letterhead describing the study and encouraging sample members to participate; we also encouraged sample members to call in and complete the survey. Our notification letters supported the locating process, as the returned letters were routed to our locating specialists who worked to find contact information for these hard-to-reach cases. We also sent a reminder postcard encouraging nonrespondents to participate.

We anticipated that the most significant source of survey nonresponse would come from sample members we could not locate because of poor contact information (such as incorrect or outdated addresses or telephone numbers). To address this issue, we assigned field locators the remaining cases we could not locate (for example, cases with bad telephone numbers or cases in which we were unable to talk to a person or confirm the identity of the sampled respondent). The field locators attempted to locate these hard-to-reach respondents in each of the 16 geographic locations near an IAH practice. When the field locators found the sample members, they asked them if they wanted to complete the survey with a telephone interviewer by calling the Survey Operations Center. Field locators also offered a $20 incentive to sample members for completing the survey.

Table D.17 shows the timeline for the steps in the data collection process for each wave.

Table D.17. Timeline for data collection

	Advance letter sent	Telephone calling started	Reminder postcard sent	Paper survey sent	Paper survey resent	Field locating started
Wave 1	2/14/2017	2/21/2017	3/20/2017	4/21/2017	5/15/2017	5/22/2017
Wave 2	2/27/2017	3/6/2017	4/4/2017	5/2/2017	5/26/2017	6/8/2017
Wave 3	3/13/2017	3/20/2017	4/18/2017	5/15/2017	6/12/2017	6/27/2017

Finally, we sent two rounds of postcards (July 5, 2017, and July 17, 2017) to all noncompleted cases in all waves, offering a $20 incentive for completing the survey.

E. Respondent sample

Response rate and nonresponse weights. The survey data presented in this report reflect responses from 651 home-based care recipients (a response rate of 41.9 percent) and 1,316 comparison beneficiaries (a response rate of 40.9 percent), and the results presented in the report are weighted for nonresponse. We calculated the response rate after excluding beneficiaries who could not respond, such as those who had a language barrier or had passed away before or during data collection. We calculated nonresponse weights in three stages: (1) eligibility determination, (2) location adjustment, and (3) cooperation adjustment. We used the eligibility model to estimate the probability of survival among unlocated cases. The location and cooperation adjustments were calculated using weighting classes based upon propensity scores from logistic models. The location adjustment accounted for people who were sampled but could not be located during data collection. We used the Chi-squared automatic interaction detector to help identify the pool of main effects and interactions in the stepwise regressions. All models accounted for both the home-based primary care status (home-based care recipient versus comparison) and the 15 IAH catchment areas. We calculated the final adjustments at each stage by taking the inverse of the response rate in weighting classes determined from the propensity scores for each model.

Time lapse between index date and survey response. As described above, the survey was rolled out in waves to minimize the time lapse between sample members' index date and the survey responses. Index dates spanned from January 2015 through June 2016; survey responses were collected between February and August 2017. The average elapsed time between the index date and the survey response was 19 months—the minimum was 9 months, the maximum was 31 months.

Final sample characteristics. Balance on the subset of survey respondents was good—there were no standardized differences larger than 0.15. Table D.18 shows the sample characteristics and standardized differences for the final analysis sample.

Table D.18. Covariate balance for survey respondents

	Matched comparison group mean	Home-based primary care group mean	Standardized difference between home-based care recipient and matched comparison
Beneficiary characteristics used for exact matching			
Number of months since last hospital admission			
1	0.172	0.166	−0.016
2–3	0.335	0.339	0.009
≥4	0.493	0.495	0.003
Observation stay only in prior year (no hospital admission)	0.032	0.035	0.019
IAH catchment area			
Boston, Massachusetts	0.017	0.020	0.025
Wilmington, Delaware	0.029	0.028	−0.007
Cleveland, Ohio	0.051	0.057	0.026
Durham, North Carolina	0.215	0.164	−0.127

Table D.18 (continued)

	Matched comparison group mean	Home-based primary care group mean	Standardized difference between home-based care recipient and matched comparison
New York City combined (Brooklyn and Long Island Jewish)	0.166	0.160	−0.016
Austin, Texas	0.063	0.066	0.012
Portland, Oregon	0.016	0.018	0.019
Dallas, Texas	0.078	0.089	0.039
Flint, Michigan	0.137	0.180	0.120
Jacksonville, Florida	0.046	0.041	−0.024
Lansing, Michigan	0.083	0.077	−0.022
Milwaukee, Wisconsin	0.078	0.065	−0.050
Washington, DC	0.015	0.022	0.048
Philadelphia, Pennsylvania	0.005	0.008	0.042
Richmond, Virginia	0.002	0.006	0.065
Carrier claims year			
2015	0.700	0.716	0.035
2016	0.300	0.284	−0.035
Propensity score stratum			
1 (0 , 0.00156]	0.245	0.235	−0.024
2 (0.00156, 0.00257]	0.205	0.195	−0.025
3 (0.00257, 0.00395]	0.186	0.201	0.038
4 (0.00395, 0.00748]	0.190	0.181	−0.022
5 (0.00748, 0.119]	0.173	0.187	0.037
Demographic characteristics			
Age			
Less than 65	0.114	0.124	0.032
65–79	0.329	0.332	0.006
79 or older	0.557	0.544	−0.027
Race			
White	0.784	0.754	−0.072
Black or African-America	0.184	0.204	0.052
Other	0.032	0.041	0.052
Dual eligibility status	0.221	0.258	0.087
Female	0.676	0.684	0.017
Original reason for Medicare entitlement			
Age	0.752	0.724	−0.066
Disability	0.237	0.267	0.070
ESRD or ESRD and disability	0.011	0.009	−0.014
Activities of Daily Living			
Number of ADLs			
2	0.063	0.065	0.002
3–4	0.256	0.258	0.002

Table D.18 (continued)

	Matched comparison group mean	Home-based primary care group mean	Standardized difference between home-based care recipient and matched comparison
5–6	0.681	0.677	−0.004
Missing data on feeding ADL	0.107	0.098	−0.029
Recency variables			
Months since last wheelchair code as of index date			
Less than 3 months	0.175	0.212	0.095
3–12 months	0.030	0.038	0.045
Not observed in 12 months	0.795	0.750	−0.109
Months since last wheelchair code 12 months prior			
Less than 3 months	0.046	0.057	0.048
3–12 months	0.028	0.023	−0.032
Not observed in 12 months	0.926	0.920	−0.020
Months since last hospital bed code as of index date			
Less than 3 months	0.122	0.147	0.075
3–12 months	0.022	0.028	0.037
Not observed in 12 months	0.856	0.825	−0.085
Months since last hospital bed code 12 months prior			
Less than 3 months	0.029	0.037	0.046
3–12 months	0.024	0.028	0.026
Not observed in 12 months	0.948	0.935	−0.052
Months since last home oxygen code as of index date			
Less than 3 months			
3–12 months	0.011	0.008	−0.030
Not observed in 12 months	0.951	0.959	0.037
Months since last home oxygen code 12 months prior			
Less than 3 months	0.023	0.017	−0.041
3–12 months	0.006	0.008	0.020
Not observed in 12 months	0.971	0.975	0.026
Time since first diagnosed with Alzheimer's disease or senile dementia			
Less than 3 months	0.072	0.060	−0.049
3–12 months	0.131	0.137	0.018
More than 12 months	0.335	0.339	0.009
Never	0.462	0.464	1.022
Time since first diagnosed with COPD			
Less than 3 months	0.033	0.014	−0.118
3–12 months	0.039	0.043	0.022
More than 12 months	0.388	0.396	0.016
Never	0.540	0.547	1.080
Time since first diagnosed with heart failure			
Less than 3 months	0.040	0.029	−0.055
3–12 months	0.070	0.069	−0.003
More than 12 months	0.491	0.521	0.060

Table D.18 (continued)

	Matched comparison group mean	Home-based primary care group mean	Standardized difference between home-based care recipient and matched comparison
Never	0.399	0.381	0.998
Time since first diagnosed with hip pelvic fracture			
Less than 3 months	0.028	0.014	−0.095
3–12 months	0.045	0.060	0.069
More than 12 months	0.103	0.103	0.001
Never	0.824	0.823	1.025
Time since first diagnosed with osteoporosis			
Less than 3 months	0.011	0.009	−0.014
3–12 months	0.031	0.037	0.032
More than 12 months	0.348	0.333	−0.031
Never	0.610	0.621	1.013
HCC 18, diabetes with chronic complications, 12 months prior	0.228	0.210	−0.042
HCC 27, end-stage liver disease, 12 months prior	0.002	0.003	0.016
HCC 28–29, cirrhosis of the liver, chronic hepatitis, 12 months prior	0.016	0.018	0.019
HCC 57–58, schizophrenia, major depressive, bipolar, and paranoid disorders, 12 months prior	0.156	0.134	−0.062
HCC 70–71, quadriplegia, paraplegia, 12 months prior	0.025	0.015	−0.066
HCC 103–104, hemiplegia/hemiparesis, monoplegia, other paralytic syndromes, 12 months prior	0.066	0.058	−0.032
HCC 134, dialysis status, 12 months prior	0.024	0.023	−0.008
HCC 157–159, pressure ulcer of skin with necrosis or skin loss, 12 months prior	0.037	0.037	−0.002
Health Status			
Number of CCW chronic condition categories			
<6	0.246	0.227	−0.044
6–9	0.494	0.502	0.017
>9	0.260	0.270	0.024
HCC score	3.824	4.061	0.115
HCC 8, metastatic cancer	0.017	0.029	0.081
HCC 9-10, lung, lymphoma, and other cancers	0.030	0.040	0.053
HCC 11-12, colorectal, bladder, breast, prostate, and other cancers	0.106	0.091	−0.050
HCC 18, diabetes with chronic complications	0.272	0.263	−0.021
HCC 21, protein-calorie malnutrition	0.154	0.207	0.141
HCC 27, end-stage liver disease	0.005	0.006	0.022
HCC 28-29, cirrhosis of liver and chronic hepatitis	0.024	0.018	−0.035
HCC 46, severe hematological disorders	0.008	0.012	0.049
HCC 48, coagulation defects and other specified hematological disorders	0.147	0.163	0.043
HCC 51, dementia with complications	0.144	0.135	−0.024
HCC 52, dementia without complications	0.320	0.313	−0.014

Table D.18 (continued)

	Matched comparison group mean	Home-based primary care group mean	Standardized difference between home-based care recipient and matched comparison
HCC 54-55, drug/alcohol psychosis and drug/alcohol dependence	0.088	0.083	−0.018
HCC 57-58, schizophrenia, major depressive, bipolar, and paranoid disorders	0.228	0.243	0.035
HCC 70-71, quadriplegia, paraplegia	0.050	0.052	0.009
HCC 72, spinal cord disorders/injuries	0.032	0.037	0.027
HCC 85, congestive heart failure	0.460	0.467	0.015
HCC 96, specified heart arrhythmias	0.386	0.378	−0.017
HCC 103-104, hemiplegia/hemiparesis, monoplegia, other paralytic syndromes	0.128	0.155	0.080
HCC 106, atherosclerosis of the extremities with ulceration or gangrene	0.043	0.054	0.049
HCC 107-108, vascular disease with or without complications	0.457	0.458	0.002
HCC 111, chronic obstructive pulmonary disease	0.317	0.347	0.065
HCC 134, dialysis status	0.037	0.034	−0.018
HCC 136−138, chronic kidney disease, stage 3–5	0.068	0.072	0.015
HCC 139-140, chronic kidney disease stage 1–2, unspecified renal failure	0.049	0.046	−0.012
HCC 157-159, pressure ulcer of skin with necrosis or skin loss	0.106	0.152	0.140
Anemia	0.142	0.187	0.125
Depression	0.474	0.452	−0.045
Fluid and electrolyte disorders	0.311	0.341	0.065
MS-DRG with major complicating condition or complicating condition	0.584	0.590	0.013
Health care expenditures and utilization in 12 months before index date			
Average monthly expenditures	4772.060	5145.820	0.093
Number of specialist visits (non-inpatient setting)	24.372	22.986	−0.083
Number of ED visits	1.397	1.613	0.082
Hospice utilization in the past 12 months	0.018	0.017	−0.010
Number of hospitalizations	2.197	2.098	−0.055
Number of observation stays	0.279	0.261	−0.027

Note: Mean values reported are unweighted.

ADL = activities of daily living; COPD = chronic obstructive pulmonary disease; ED = emergency department; ESRD = end stage renal disease; HCC = Hierarchical Condition Category.

F. Determining survey variables to include in difference-in-differences impact analysis

To check for evidence of selection bias in the home-based primary care impact analysis estimates, we identified the set of survey responses that differed systematically between the home-based primary care recipients and the comparison group. We first estimated a logistic

regression in which the dependent variable was an indicator for home-based primary care use (yes/no) and, in addition to the survey response variables, then included the same control variables used in the earlier analysis. We estimated univariate versions that included one survey variable, then a multivariate version that included all survey variables. Table D.19 shows the *p*-values for the multivariate model. Every survey variable that was statistically significant in the multivariate model was also significant in its respective univariate model.

The 16 survey variables that were statistically significant (at the 5 percent level) predictors of home-based primary care status in the multivariate model comprise the "predictive subset" of survey variables that we used as controls when testing for evidence of selection bias in the impact estimates.

Table D.19. Survey questions as predictors of home-based primary care status (multivariate)

Question	Description	p-value	Include in predictive subset?
D1	Low back pain, what do you do?	<0.01	Yes
D2	Dizzy, what do you do?	0.32	
D3A	Flu: home-based primary care easier on family?	0.06	
D3B	Flu: home-based primary care more comfortable?	<0.01	Yes
D3C	Flu: home-based primary care as good?	<0.01	Yes
D3D	Flu: home-based primary care safe?	0.03	Yes
D3E	Flu: office easier on family?	<0.01	Yes
D3F	Flu: don't like people in home?	0.14	
D3G	Flu: safer at office?	<0.01	Yes
D4	Do you avoid doctor?	0.02	Yes
D5	Did you have trouble getting care?	0.97	
D6	Satisfied with primary care provider?	0.35	
E1	Self-report of health	0.01	Yes
E2	Health compared to last year	0.02	Yes
E3A	Help: bathe?	0.09	
E3B	Help: dress?	0.05	
E3C	Help: eat?	0.27	
E3D	Help: bed?	0.96	
E3E	Help: walk?	0.01	Yes
E3F	Help: toilet?	0.27	
E3G	Help: errands?	0.75	
E3H	Help: meds?	0.34	
E3I	Help: medical equipment?	<0.01	Yes
E4A	Difficulty: errands?	0.64	
E4B	Difficulty: feeding yourself?	0.36	
E4C	Difficulty: using toilet?	0.01	Yes
F1	Have a primary caregiver?	0.22	
G1	Education	0.01	Yes
G2_1	Living: alone?	0.25	

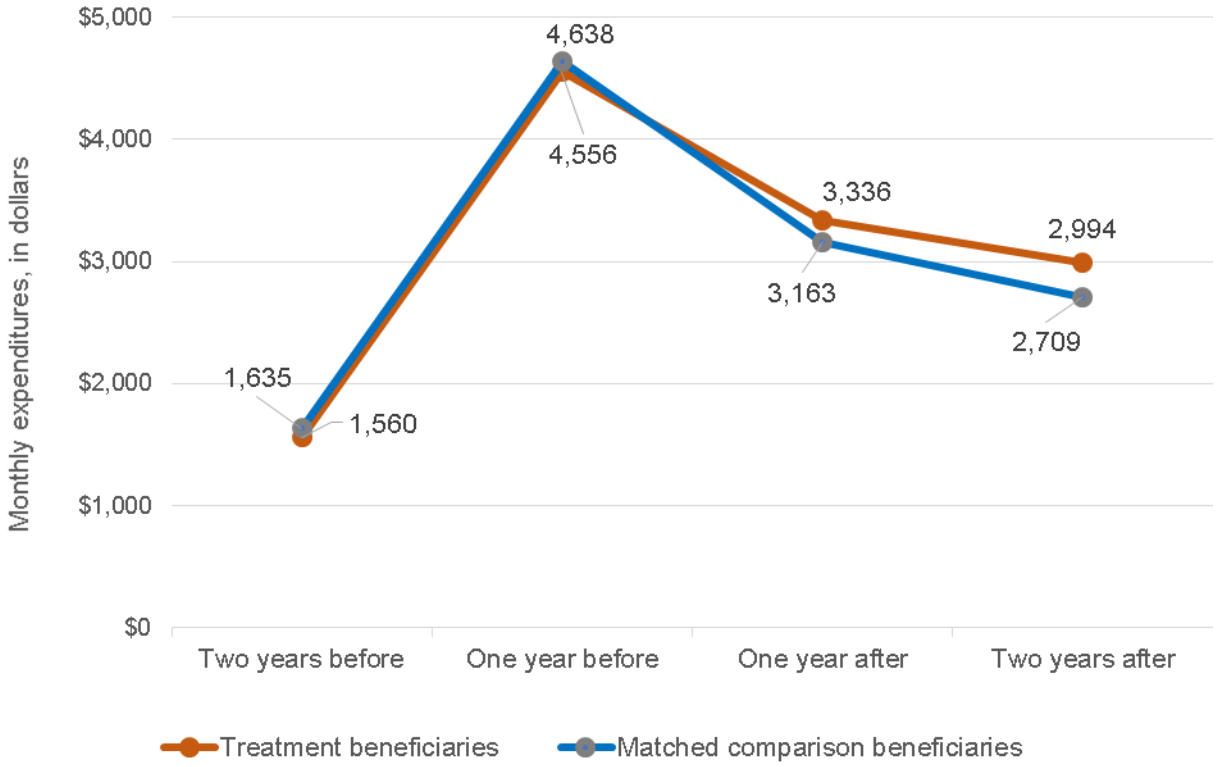
Question	Description	p-value	Include in predictive subset?
G2_2	Living: with spouse?	0.02	Yes
G2_3	Living: with children?	0.02	Yes
G2_4	Living: with parent?	0.59	
G2_5	Living: with other family?	0.57	
G2_6	Living: in assisted living facility?	<0.01	Yes
G2_7	Living: other?	0.45	

VI. SUPPLEMENTAL RESULTS

The tables that follow present additional results that were not presented in Chapter IV, in the interest of brevity, but provide a fuller picture of our findings. These results include regression-adjusted means and difference-in-differences estimates for total Medicare expenditures, expenditures by service category, and several kinds of hospital use. We also include tables giving results from two subgroup analyses: (1) estimating the effect of home-based primary care for each panel and (2) estimating the effect for only those home-based primary care recipients receiving care from an IAH clinician. Finally, we give detailed descriptions of the responses to the survey capturing attitudes and preferences of home-based primary care recipients and matched comparison beneficiaries.

Total Medicare expenditures. On average, both the home-based care recipients and comparison beneficiaries experienced a sharp increase in average PBPM Medicare FFS expenditures in the baseline period—that is, the year before the index date (Figure D.2).

Figure D.2. Estimate effect of home-based primary care on average PBPM total Medicare expenditures in the baseline year



Source: Medicare claims and enrollment data for 2010–2016 obtained from the Virtual Research Data Center for home-based care recipients and matched comparison group beneficiaries.

Note: Expenditures were measured per beneficiary per month. Regression-adjusted means are obtained by applying the estimated regression coefficients to the covariates of home-based care recipients in the baseline year.

PBPM = per beneficiary per month.

The spike in spending in the year before home-based care recipients started receiving home-based primary care is explained in part by the IAH-eligibility criteria, which required that both the home-based care recipients and their matched comparisons have an inpatient stay and utilization of rehabilitation services at some point in the 12 months before the index date. The spike also reflects the appearance of some health-related event for those who started home-based primary care—for example, the onset of a new condition or reaching a threshold in a disabling preexisting condition—that apparently led to a change in their need, or preference, for receiving primary care in their home. We matched the comparison beneficiaries using claims-based measures that may serve as a proxy for some of these events, such as the MDRG from the most recent inpatient hospital stay before the index date. As a result, their average PBPM total Medicare expenditures exhibited the same pre-period trend. For both groups, there was a substantial decline in PBPM total expenditures in the first 12 months after the baseline year, with the comparison group experiencing, on average, a larger decline than the home-based care group. Expenditures continued to decline in the second follow-up year, but at a substantially lower rate for both groups.

Table D.20 reports the regression-adjusted means of PBPM Medicare total expenditures in the two years before and the two years after the index date for both groups, as well as whether the differences between those means were statistically significant. Whether or not the model incorporated an adjustment for expected survival, the profile of expenditures across the four years followed the same pattern for both the home-based care recipients and their matched comparisons. In each period, and for both variants of the difference-in-differences model, there were statistically significant differences in the regression-adjusted mean of total PBPM Medicare expenditures between home-based care recipients and their matched comparisons. In the two years before starting home-based primary care, beneficiaries had total PBPM expenditures slightly lower than those of the matched comparison beneficiaries. For example, with the standard model in the period 12 months before the index date, home-based care recipients had expenditures 1.8 percent lower than the set of matched comparison beneficiaries. In the years following the start of home-based primary care, the pattern was reversed: home-based care recipients had higher total PBPM Medicare expenditures than the set of matched comparison beneficiaries. After adjusting for the difference in survival trajectories between home-based care recipients and their matched comparisons, we observed similar expenditure patterns in all time periods. Controlling for survival trajectories also did not alter the estimated effect of home-based primary care on total expenditures (Table D.21).

Table D.20. Regression-adjusted means of total PBPM Medicare expenditures for home-based care recipients and matched comparison group beneficiaries, standard model and with survival, pooled across panels

Period	Regression-adjusted mean for home-based care recipients	Regression-adjusted mean for matched comparison group beneficiaries	Difference between home-based care and comparison group beneficiaries (standard error)	Difference between home-based care and comparison group beneficiaries as percentage of the home-based care group mean
Second year after starting home-based primary care				
Standard model	$2,994	$2,709	$285*** ($27)	9.5
Survival model	$3,075	$2,811	$264*** ($29)	8.6
First year after starting home-based primary care				
Standard model	$3,336	$3,163	$174*** ($22)	5.2
Survival model	$3,374	$3,211	$164*** ($22)	4.8
One year before starting home-based primary care				
Standard model	$4,556	$4,638	−$82*** ($19)	−1.8
Survival model	$4,527	$4,600	−$72*** ($19)	−1.6
Two years before starting home-based primary care				
Standard model	$1,560	$1,635	−$76*** ($16)	−4.8
Survival model	$1,531	$1,596	−$66*** ($16)	−4.2
Total number of observations across the four years: 671,257				

Source: Medicare claims and enrollment data for 2010–2016 obtained from the Virtual Research Data Center for home-based care recipients and matched comparison group beneficiaries.

Note: Expenditures were measured per beneficiary per month. Regression-adjusted means are obtained by applying the estimated regression coefficients to the covariates of home-based care recipients in the baseline year. Any discrepancies in the column showing the difference between the two groups' means versus the difference calculated using the displayed home-based care and comparison group means are due to rounding.

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

PBPM = per beneficiary per month.

Table D.21. Estimated effect of home-based primary care on total PBPM Medicare expenditures, standard model and with survival, pooled across panels

Period	Difference-in-differences estimate (standard error)	90 CI LL	90 CI UL	80 CI LL	80 CI UL	Percentage effect (relative to home-based primary care group mean in the year before starting home-based primary care)
Second year after start						
Standard model	$367*** ($33)	312.7	421.3	324.8	409.2	8.1
Survival model	$336*** ($36)	276.8	395.2	289.9	382.1	7.3
First year after start						
Standard model	$256*** ($28)	209.9	302.1	220.2	291.8	5.6
Survival model	$236*** ($29)	188.3	283.7	198.9	273.1	5.1
Two years before start						
Standard model	$6 ($26)	−37.7	49.9	−28.0	40.3	0.1
Survival model	$6 ($26)	−36.6	49.2	−27.2	39.7	0.1
Total number of observations across the four years: 671,257						

Source: Medicare claims and enrollment data for 2010–2016 obtained from the Virtual Research Data Center for home-based care recipients and matched comparison group beneficiaries.

Note: Expenditures were measured per beneficiary per month. The difference-in-differences estimate for each year was calculated as the difference in means between home-based primary care and comparison beneficiaries in that year minus the difference in the means in the year before the index date.

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

CI = confidence interval; LL = lower limit; PBPM = per beneficiary per month; UL = upper limit.

Categories of expenditures. There were statistically significant differences in PBPM expenditures between home-based primary care recipients and the set of matched comparisons across all service categories and in most of the time periods (Table D.22). In most years, average PBPM expenditures on inpatient, SNF, and outpatient services were significantly lower for home-based care recipients than for their matched comparison beneficiaries. The reverse was true for home health and hospice services. The spending profile for inpatient services across years mimicked the profile for total Medicare expenditures, peaking in the base year. Because all beneficiaries had an inpatient stay during the base year, this was not unexpected. Expenditures on SNF services followed the same pattern across the four years. Because of the requirement that all beneficiaries had to use rehabilitation care in the 12 months before the index date, this was also expected.

Table D.22. Regression-adjusted means of PBPM Medicare expenditures for home-based care recipients and matched comparison group beneficiaries, by service category, pooled across panels

Service type and period	Regression-adjusted mean for home-based care recipients	Regression-adjusted mean for matched comparison group beneficiaries	Difference between home-based care and comparison group beneficiaries (standard error)	Difference between home-based care and comparison group beneficiaries as percentage of the home-based care group mean
Inpatient services				
Two years after starting home-based primary care	$1,099	$1,111	−$11 ($17)	−1.1
One year after starting home-based primary care	$1,256	$1,312	−$55*** ($15)	−4.5
One year before home-based primary care	$2,091	$2,246	−$155*** ($14)	−7.4
Two years before home-based primary care	$612	$660	−$47*** ($10)	−7.8
Skilled nursing facilities				
Two years after starting home-based primary care	$401	$434	−$33*** ($7)	−8.2
One year after starting home-based primary care	$336	$598	−$263*** ($6)	−78.0
One year before home-based primary care	$1,136	$1,034	$102*** ($7)	9.0
Two years before home-based primary care	$212	$249	−$37*** ($4)	−17.5
Home health services (Parts A and B)				
Two years after starting home-based primary care	$372	$224	$147*** ($4)	39.8
One year after starting home-based primary care	$610	$272	$337*** ($4)	55.4
One year before home-based primary care	$423	$396	$27*** ($3)	6.4
Two years before home-based primary care	$220	$187	$33*** ($3)	15.0
Hospice services				
Two years after starting home-based primary care	$393	$220	$174*** ($8)	44.0
One year after starting home-based primary care	$261	$165	$96*** ($5)	36.8
One year before home-based primary care	$30	$25	$5*** ($2)	16.7
Two years before home-based primary care	$7	$13	−$6*** ($1)	−85.7

Table D.22 (continued)

Service type and period	Regression-adjusted mean for home-based care recipients	Regression-adjusted mean for matched comparison group beneficiaries	Difference between home-based care and comparison group beneficiaries (standard error)	Difference between home-based care and comparison group beneficiaries as percentage of the home-based care group mean
Outpatient services				
Two years after starting home-based primary care	$185	$226	−$41*** ($3)	−22.2
One year after starting home-based primary care	$201	$244	−$43*** ($3)	−21.4
One year before home-based primary care	$199	$234	−$36*** ($3)	−17.6
Two years before home-based primary care	$142	$156	−$14*** ($2)	−9.9
Physician or supplier services				
Two years after starting home-based primary care	$485	$443	$41*** ($4)	8.7
One year after starting home-based primary care	$568	$498	$70*** ($4)	12.3
One year before home-based primary care	$616	$632	−$16*** ($3)	−2.6
Two years before home-based primary care	$321	$327	−$6** ($3)	−1.9
Durable medical equipment				
Two years after starting home-based primary care	$60	$52	$8*** ($2)	13.3
One year after starting home-based primary care	$105	$73	$32*** ($2)	30.5
One year before home-based primary care	$61	$69	−$9*** ($1)	−13.1
Two years before home-based primary care	$44	$43	$1 ($1)	2.3
Total unweighted number of observations across all years: 671,257				

Source: Medicare claims and enrollment data for 2010–2016 obtained from the Virtual Research Data Center for home−based care recipients and matched comparison group beneficiaries.

Note: Expenditures were measured per beneficiary per month. Regression-adjusted means are obtained by applying the estimated regression coefficients to the covariates of home-based care recipients in the baseline year. Any discrepancies in the column showing the difference between the two groups' means versus the difference calculated using the displayed home-based care recipient and comparison group means are due to rounding.

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

PBPM = per beneficiary per month.

As noted above, we also estimated the effect of home-based primary care among beneficiaries with dementia. Because of their limited capacity, access to health care for these beneficiaries can differ from those without dementia. The presence of complications may reflect different positions in the trajectory of the disease, suggesting different health care needs. As was the case with the full sample, home-based primary care recipients who had dementia, with or without complications, also experienced smaller decreases in total and inpatient expenditures and larger increases in home health and hospice expenditures than their matched comparison beneficiaries after beginning home-based primary care (Table D.23).

Table D.23. Estimated effect of home-based primary care on PBPM Medicare expenditures, by service category, beneficiaries with dementia, pooled across panels

Period	Dementia with complications		Dementia without complication	
	Difference-in-differences estimate (standard error)	Percentage effect (relative to home-based care group mean in the year before starting home-based primary care)	Difference-in-differences estimate (standard error)	Percentage effect (relative to home-based care group mean in the year before starting home-based primary care)
Two years after starting home-based primary care	$502*** ($71)	10.4	$444*** ($48)	10.6
One year after starting home-based primary care	$261*** ($62)	5.4	$278*** ($41)	6.7
Two years before home-based primary care	$192*** ($56)	4.0	$96** ($37)	2.3
Inpatient services				
Two years after starting home-based primary care	$157*** ($48)	7.2	$137*** ($33)	7.4
One year after starting home-based primary care	$67 ($44)	3.1	$77*** ($29)	4.2
Two years before home-based primary care	$159*** ($39)	7.3	$131*** ($27)	7.1
Skilled nursing facilities				
Two years after starting home-based primary care	$31 ($23)	2.3	−$85*** ($16)	−7.9
One year after starting home-based primary care	−$274*** ($21)	−19.9	−317*** ($14)	−29.5
Two years before home-based primary care	$34* ($19)	2.5	−$65*** ($13)	−6.1
Two years after starting home-based primary care	$99*** ($9)	26.5	$114*** ($7)	26.5
One year after starting home-based primary care	$312*** ($9)	83.3	$290*** ($7)	67.2
Two years before home-based primary care	−$36*** ($6)	−9.7	−$3 ($5)	−0.6
Hospice services				
Two years after starting home-based primary care	$191*** ($20)	617.8	$202*** ($14)	532.5
One year after starting home-based primary care	$93*** ($12)	300.0	$100*** ($8)	262.4
Two years before home-based primary care	−$4 ($4)	−13.3	−$15*** ($3)	−40.8

Table D.23 (continued)

Period	Dementia with complications		Dementia without complication	
	Difference-in-differences estimate (standard error)	Percentage effect (relative to home-based care group mean in the year before starting home-based primary care)	Difference-in-differences estimate (standard error)	Percentage effect (relative to home-based care group mean in the year before starting home-based primary care)
Outpatient services				
Two years after starting home-based primary care	−$27*** ($6)	−15.4	−$6 ($5)	−3.6
One year after starting home-based primary care	−$25*** ($5)	−20.2	−$3 ($4)	−1.6
Two years before home-based primary care	$16*** ($4)	9.2	$20*** ($3)	11.9
Two years after starting home-based primary care	$41*** ($10)	6.4	$63*** ($7)	11.5
One year after starting home-based primary care	$62*** ($9)	9.7	$93*** ($6)	16.8
Two years before home-based primary care	$15** ($8)	2.3	$19*** ($5)	3.5
Durable medical equipment				
Two years after starting home-based primary care	$9*** ($3)	22.7	$18*** ($2)	36.9
One year after starting home-based primary care	$36*** ($3)	86.1	$38*** ($2)	79.9
Two years before home-based primary care	$8*** ($2)	19.6	$8*** ($1)	17.9
Total number of observations across the four years with and without complications: 420,066				

Source: Medicare claims and enrollment data for 2010–2016 obtained from the Virtual Research Data Center for home-based care recipients and matched comparison group beneficiaries.

Note: Expenditures were measured per beneficiary per month. The difference-in-differences estimate for each year was calculated as the difference in means between home-based care recipients and comparison beneficiaries in that year minus the difference in the means in the year before the index date.

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

PBPM = per beneficiary per month .

Hospital use. For both home-based care recipients and comparison beneficiaries, the regression-adjusted means for the five utilization measures showed relatively lower levels of use in the period two years before starting home-based primary care and a jump in all types of use during the year before the index date (Table D.24). In the two years prior to starting home-based primary care, home-based care recipients had statistically significantly fewer inpatient admissions and admissions for ACSCs, and fewer qualifying hospital discharges with an unplanned readmission within 30 days of discharge. The only statistically significant difference between home-based care recipients and the matched comparison group n ED use was that home-based care recipients had slightly fewer ED visits (0.65 versus 0.67 visits per year) in the two years prior to starting home-based primary care. This very small difference suggests that ED use was very similar for the home-based care recipients and comparison beneficiaries before the intervention.

In the first post-intervention year, home-based care recipients had statistically significantly more total hospital admissions (1.35 versus 1.29) and more admissions for ACSCs (0.36 versus 0.32) than the matched comparison beneficiaries. In the second post-intervention year, there

were no statistically significant differences in either measure of hospital admissions. There was no statistically significant difference in the probability of having a qualified hospital discharge and an unplanned readmission within 30 days in either of the post-intervention years. For both groups, the average number of hospital admissions in the post-baseline period was three to four times the average for all Medicare beneficiaries nationally.

Home-based care recipients had more of both measures of ED visits in the post-intervention years. In the first post-intervention year, home-based care recipients had 1.10 ED visits on average (0.14 for ACSCs), whereas matched comparison beneficiaries had 0.89 (0.12 for ACSCs). In the second post-intervention year, the difference between home-based care recipients and the matched comparisons was smaller but remained statistically significant for total ED visits (0.87 versus 0.82 total ED visits), but there was not a statistically significant difference in ED visits for ACSCs.

Table D.24. Regression-adjusted means of ED and hospital inpatient care for home-based care recipients and matched comparison group beneficiaries, pooled across panels

Service type and period	Regression-adjusted mean for home-based care recipients	Regression-adjusted mean for matched comparison group beneficiaries	Difference between home-based care and comparison group beneficiaries (standard error)	Difference between home-based care and comparison group beneficiaries as percentage of the home-based care group mean
Number of hospital admissions per beneficiary per year				
Second year after starting home-based primary care	1.11	1.11	−0.01 (0.01)	0.00
First year after starting home-based primary care	1.35	1.29	0.06*** (0.01)	4.40
One year before starting home-based primary care	1.94	2.06	−0.11*** (0.01)	−6.20
Two years before starting home-based primary care	0.72	0.76	−0.04*** (0.01)	−5.60
Number of hospital admissions for ACSCs per beneficiary per year				
Second year after starting home-based primary care	0.28	0.28	0.00 (0.01)	0.00
First year after starting home-based primary care	0.36	0.32	0.03*** (0.01)	11.10
One year before starting home-based primary care	0.43	0.46	−0.02*** (<0.01)	−7.00
Two years before starting home-based primary care	0.17	0.18	−0.01*** (<0.01)	−5.90
Number of ED visits per beneficiary per year				
Second year after starting home-based primary care	0.87	0.82	0.05*** (0.01)	5.70
First year after starting home-based primary care	1.10	0.89	0.21*** (0.01)	19.10
One year before starting home-based primary care	1.09	1.10	0.00 (0.01)	−0.90
Two years before starting home-based primary care	0.65	0.67	−0.02*** (0.01)	−3.10
Number of ED visits for ACSCs per beneficiary per year				
Second year after starting home-based primary care	0.11	0.11	0.00 (<0.01)	0.00

Table D.24 (continued)

Service type and period	Regression-adjusted mean for home-based care recipients	Regression-adjusted mean for matched comparison group beneficiaries	Difference between home-based care and comparison group beneficiaries (standard error)	Difference between home-based care and comparison group beneficiaries as percentage of the home-based care group mean
First year after starting home-based primary care	0.14	0.12	0.02*** (<0.01)	14.30
One year before starting home-based primary care	0.13	0.14	−0.01* (<0.01)	−7.70
Two years before starting home-based primary care	0.08	0.08	0.00 (<0.01)	0.00
Probability of having a qualifying hospital discharge and an unplanned readmission within 30 days of discharge				
Second year after starting home-based primary care	12.10	12.50	−0.40 (0.30)	−3.30
First year after starting home-based primary care	14.80	14.80	0.00 (0.20)	0.00
One year before starting home-based primary care	21.30	23.10	−1.80*** (0.20)	−8.50
Two years before starting home-based primary care	6.20	7.10	−0.90*** (0.20)	−14.50
Total number of observations across all years: 671,257				

Source: Medicare claims and enrollment data for 2010–2016 obtained from the Virtual Research Data Center for home-based care recipients and matched comparison group beneficiaries.

Note: Regression-adjusted means are obtained by applying the estimated regression coefficients to the covariates of home-based care recipients in the baseline year. Any discrepancies in the column showing the difference between the two groups' means versus the difference calculated using the displayed home-based care recipient and comparison group means are due to rounding.

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

ACSC = ambulatory care sensitive condition; ED = emergency department.

Impact estimates across panels. There was a statistically significant positive effect of home-based primary care on total expenditures across all panels; however, the effect declined for later panels, from $355 for the 2010 panel to $154 for the 2014 panel (Table D.25). The decrease in total Medicare expenditures over time was due to smaller increases in expenditures on home health services, physician or supplier services, and DME for home-based primary care recipients along with larger decreases in expenditures on SNF and outpatient services (Figure D.3). The effect of home-based primary care on inpatient admissions—relative increases— steadily declined across the five panels (Table D.26).

Table D.25. Estimated effect of home-based primary care on PBPM Medicare expenditures, by service category and panel

Service type	2010 panel	2011 panel	2012 panel	2013 panel	2014 panel	Pooled (all panels)
Estimated effect one year after starting home-based primary care						
Total Medicare expenditures	$355*** ($69)	$344*** ($64)	$267*** ($63)	$184*** ($62)	$154*** ($59)	$256*** ($28)
Inpatient hospital services	$117** ($52)	$145*** ($47)	$77* ($46)	$67 ($45)	$100** ($42)	$100*** ($21)

Table D.25 (continued)

Service type	2010 panel	2011 panel	2012 panel	2013 panel	2014 panel	Pooled (all panels)
Skilled nursing facilities	−$320***	−$356***	−$347***	−$381***	−$412***	−$365***
	($20)	($22)	($19)	($20)	($20)	($9)
Home health services (Parts A and B)	$319***	$331***	$321***	$300***	$284***	$310***
	($11)	($10)	($9)	($9)	($9)	($4)
Hospice services	$81***	$76***	$80***	$105***	$111***	$91***
	($11)	($12)	($11)	($11)	($11)	($5)
Outpatient services	$3	$1	−$5	−$9	−$25***	−$8***
	($7)	($6)	($6)	($7)	($7)	($3)
Physician or supplier services	$101***	$99***	$88***	$71***	$76***	$86***
	($10)	($10)	($9)	($9)	($9)	($4)
Durable medical equipment	$54***	$48***	$52***	$32***	$20***	$41***
	($4)	($4)	($3)	($3)	($3)	($2)
Estimated effect two years after starting home-based primary care						
Total Medicare expenditures	$417***	$451***	$353***	$307***	$327***	$367***
	($78)	($74)	($71)	($74)	($72)	($33)
Inpatient hospital services	$157***	$191***	$123**	$103***	$155***	$144***
	($56)	($51)	($49)	($51)	($50)	($23)
Skilled nursing facilities	−$114***	−$132***	−$114***	−$156***	−$157***	−$135***
	($23)	($24)	($23)	($23)	($23)	($10)
Home health services (Parts A and B)	$119***	$138***	$119***	$125***	$103***	$120***
	($11)	($10)	($9)	($10)	($9)	($4)
Hospice services	$160***	$161***	$153***	$181***	$189***	$169***
	($18)	($18)	($16)	($17)	($17)	($8)
Outpatient services	$9	$1	−$2	−$9	−$27***	−$6***
	($8)	($8)	($7)	($8)	($8)	($4)
Physician or supplier services	$63***	$69***	$88***	$52***	$53***	$58***
	($13)	($12)	($9)	($11)	($10)	($5)
Durable medical equipment	$22***	$24***	$52***	$11***	$11***	$17***
	($4)	($5)	($3)	($3)	($3)	($2)
Total number of observations across all years	**117,721**	**125,515**	**142,290**	**141,294**	**144,437**	**671,257**

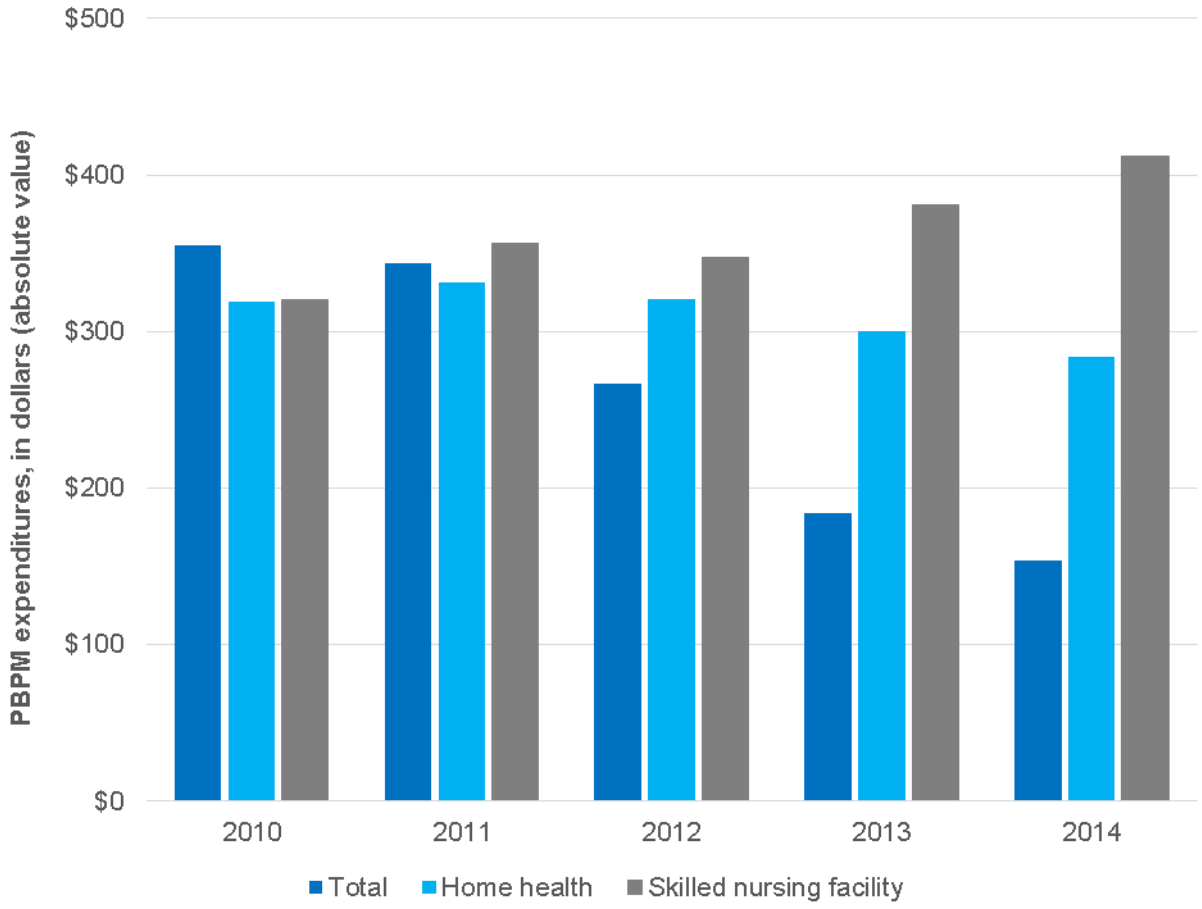
Source: Medicare claims and enrollment data for 2010–2016 obtained from the Virtual Research Data Center for home-based care recipients and matched comparison group beneficiaries.

Note: Expenditures were measured per beneficiary per month. The difference-in-differences estimate for each year was calculated as the difference in means between home-based primary care and comparison beneficiaries in that year minus the difference in the means in the year before the index date.

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

PBPM = per beneficiary per month.

Figure D.3. Estimated effects of home-based primary care recipients on total, home health, and skilled nursing facility Medicare expenditures (absolute value)



Source: Medicare claims and enrollment data for 2010–2016 obtained from the Virtual Research Data Center for home-based care recipients and matched comparison group beneficiaries.

Note: Expenditures were measured per beneficiary per month. The difference-in-differences estimate for each year was calculated as the difference in means between home-based primary care and comparison beneficiaries in that year minus the difference in the means in the year before the index date.

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

Table D.26. Estimated effect of home-based primary care on use of ED and hospital inpatient care, by panel and pooled

Service type and period	Difference-in-differences estimate (standard error)					
	2010 panel	2011 panel	2012 panel	2013 panel	2014 panel	Pooled (all panels)
Number of hospital admissions per beneficiary per year						
Second year after starting home-based primary care	0.15*** (0.03)	0.16*** (0.03)	0.11*** (0.03)	0.07** (0.03)	0.05* (0.03)	0.11*** (0.01)
First year after starting home-based primary care	0.20*** (0.03)	0.19*** (0.03)	0.18*** (0.03)	0.15*** (0.03)	0.14*** (0.03)	0.17*** (0.01)
Number of ED visits per beneficiary per year						
Second year after starting home-based primary care	0.11*** (0.03)	0.12*** (0.03)	0.02 (0.03)	0.00 (0.03)	0.00 (0.03)	0.05*** (0.01)
First year after starting home-based primary care	0.21*** (0.03)	0.24*** (0.03)	0.25*** (0.03)	0.17*** (0.03)	0.18*** (0.03)	0.21*** (0.01)
Total number of observations across all years	**117,721**	**125,515**	**142,290**	**141,294**	**144,437**	**671,257**

Source: Medicare claims and enrollment data for 2010–2016 obtained from the Virtual Research Data Center for home-based care recipients and matched comparison group beneficiaries.

Notes: The difference-in-differences estimate for each year was calculated as the difference in means between home-based primary care and comparison beneficiaries in that year minus the difference in the means in the year before the index date. Because of rounding, the percentage impact might exceed zero when the difference-in-differences estimate is zero.

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

ED = emergency department.

Patients of IAH practices and their comparison beneficiaries. We found that the results were qualitatively the same as those estimated using the full sample. Tables D.27, D.28, and D.29 show results for total Medicare expenditures, categories of expenditures, and hospital utilization respectively. These results suggest that receiving home-based primary care from the IAH practices did not have a different effect on expenditures and utilization.

Table D.27. Estimated effect of home-based primary care on total PBPM Medicare expenditures, standard model, and with survival, IAH-attributed and full sample, pooled across panels

Period	Difference-in-differences estimate (standard error)	90 CI LL	90 CI UL	80 CI LL	80 CI UL	Percentage effect (relative to home-based care group mean in the year before starting home-based primary care)
Beneficiaries receiving care from IAH practice						
Second year after start						
Standard model	$451*** ($66)	342.4	559.6	366.5	535.5	9.8
Survival model	$390*** ($70)	274.9	505.2	300.4	479.6	8.5
First year after start						
Standard model	$293*** ($54)	204.2	381.8	223.9	362.1	6.4
Survival model	$260*** ($55)	169.5	350.5	189.6	330.4	5.7
Two years before start						
Standard model	$30 ($53)	−57.2	117.2	−37.8	97.8	0.7
Survival model	$31 ($53)	−56.2	118.2	−36.8	98.8	0.7
Total number of observations across the four years: 181,246						
Full sample						
Second year after start						
Standard model	$367*** ($33)	312.7	421.3	324.8	409.2	8.1
Survival model	$336*** ($36)	276.8	395.2	289.9	382.1	7.3
First year after start						
Standard model	$256*** ($28)	209.9	302.1	220.2	291.8	5.6
Survival model	$236*** ($29)	188.3	283.7	198.9	273.1	5.1
Two years before start						
Standard model	$6 ($26)	−37.7	49.9	−28.0	40.3	0.1
Survival model	$6 ($26)	−36.6	49.2	−27.2	39.7	0.1
Total number of observations across the four years: 181,246						

Source: Medicare claims and enrollment data for 2010–2016 obtained from the Virtual Research Data Center for home-based care recipients and matched comparison group beneficiaries.

Note: Expenditures were measured per beneficiary per month. The difference-in-differences estimate for each year was calculated as the difference in means between home-based primary care and comparison beneficiaries in that year minus the difference in the means in the year before the index date.

CI = confidence interval; LL = lower limit; PBPM = per beneficiary per month; UL = upper limit.

Table D.28. Estimated effect of home-based primary care on PBPM Medicare expenditures, by service category, IAH-attributed, pooled across panels

Service type and period	Difference-in-differences estimate (standard error)	90 CI LL	90 CI UL	80 CI LL	80 CI UL	Percentage effect (relative to home-based care group mean in the year before starting home-based primary care)
Inpatient hospital services						
Two years after starting home-based primary care	$125*** ($47)	47.2	202.8	64.5	185.6	5.7
One year after starting home-based primary care	$44 ($41)	−24.0	111.4	−8.9	96.4	2.0
Two years before home-based primary care	$74* ($40)	8.0	139.1	22.6	124.6	3.3
Skilled nursing facilities						
Two years after starting home-based primary care	−$83*** ($19)	−113.9	−51.2	−107.0	−58.2	−7.8
One year after starting home-based primary care	−$327*** ($17)	−353.9	−299.1	−347.8	−305.2	−31.0
Two years before home-based primary care	−$56*** ($16)	−81.8	−29.7	−76.0	−35.4	−5.3
Home health services (Parts A and B)						
Two years after starting home-based primary care	$98*** ($8)	85.0	111.3	87.9	108.4	21.9
One year after starting home-based primary care	$295*** ($8)	282.5	307.9	285.3	305.1	65.9
Two years before home-based primary care	−$18*** ($5)	−26.6	−8.8	−24.6	−10.7	−3.9
Hospice services						
Two years after starting home-based primary care	$200*** ($16)	174.2	226.4	180.0	220.6	476.9
One year after starting home-based primary care	$114*** ($10)	97.3	131.0	101.0	127.3	271.7
Two years before home-based primary care	−$18*** ($4)	−24.7	−11.9	−23.3	−13.3	−43.6
Outpatient services						
Two years after starting home-based primary care	−$4 ($7)	−14.8	7.0	−12.4	4.6	−1.9
One year after starting home-based primary care	−$12** ($5)	−20.5	−2.8	−18.5	−4.7	−5.7
Two years before home-based primary care	$24*** ($4)	16.6	30.9	18.2	29.3	11.6
Physician or supplier services						
Two years after starting home-based primary care	$92*** ($9)	76.3	107.2	79.7	103.8	15.4
One year after starting home-based primary care	$127*** ($8)	114.2	139.1	116.9	136.3	21.2
Two years before home-based primary care	$16** ($7)	4.8	27.6	7.3	25.0	2.7
Durable medical equipment						
Two years after starting home-based primary care	$22*** ($3)	16.9	27.9	18.2	26.7	33.4
One year after starting home-based primary care	$51*** ($3)	45.8	56.3	46.9	55.1	76.2
Two years before home-based primary care	$9*** ($2)	5.2	12.0	6.0	11.2	12.8

Table D.28 (continued)

Service type and period	Difference-in-differences estimate (standard error)	90 CI LL	90 CI UL	80 CI LL	80 CI UL	Percentage effect (relative to home-based care group mean in the year before starting home-based primary care)
Total number of observations across all years: 181,246						

Source: Medicare claims and enrollment data for 2010–2016 obtained from the Virtual Research Data Center for home-based care recipients and matched comparison group beneficiaries.

Note: Expenditures were measured per beneficiary per month. The difference-in-differences estimate for each year was calculated as the difference in means between home-based care recipients and comparison beneficiaries in that year minus the difference in the means in the year before the index date.

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

CI = confidence interval; LL = lower limit; PBPM = per beneficiary per month; UL = upper limit.

Table D.29. Estimated effect of home-based primary care on use of ED and hospital inpatient care, IAH-attributed, pooled across panels

Service type and period	Difference-in-differences estimate (standard error)	90 CI LL	90CI UL	80 CI LL	80 CI UL	Percentage effect (relative to home-based care group mean in the year before starting home-based primary care)
Number of hospital admissions per beneficiary per year						
Second year after starting home-based primary care	0.05* (0.03)	0.01	0.09	0.01	0.08	2.40
First year after starting home-based primary care	0.12*** (0.02)	0.08	0.15	0.09	0.15	5.60
Two years before starting home-based primary care	0.06*** (0.02)	0.03	0.09	0.03	0.08	2.80
Number of hospital admissions for ACSCs per beneficiary per year						
Second year after starting home-based primary care	0.00 (0.01)	−0.02	0.02	−0.02	0.02	−0.40
First year after starting home-based primary care	0.03** (0.01)	0.01	0.05	0.02	0.05	6.60
Two years before starting home-based primary care	0.01 (0.01)	−0.01	0.02	−0.01	0.02	1.50
Number of ED visits per beneficiary per year						
Second year after starting home-based primary care	0.00 (0.03)	−0.04	0.05	−0.03	0.04	0.20
First year after starting home-based primary care	0.20*** (0.03)	0.16	0.24	0.17	0.23	16.60
Two years before starting home-based primary care	−0.01 (0.02)	−0.04	0.03	−0.03	0.02	−0.60
Number of ED visits for ACSCs per beneficiary per year						
Second year after starting home-based primary care	0.00 (0.01)	−0.02	0.01	−0.02	0.01	−2.90
First year after starting home-based primary care	0.03*** (0.01)	0.01	0.04	0.02	0.04	15.90
Two years before starting home-based primary care	0.00 (0.01)	−0.01	0.01	−0.01	0.00	−2.70
Probability of having a qualifying hospital discharge and an unplanned readmission within 30 days of discharge						
Second year after starting home-based primary care	0.60 (0.70)	−0.50	1.70	−0.20	1.50	2.70
First year after starting home-based primary care	0.80 (0.60)	−0.20	1.80	0.00	1.60	3.60
Two years before starting home-based primary care	0.50 (0.50)	−0.40	1.40	−0.20	1.20	2.10
Total number of observations across all years: 181,246						

Source: Medicare claims and enrollment data for 2010–2016 obtained from the Virtual Research Data Center for home-based care recipients and matched comparison group beneficiaries.

Notes: The difference-in-differences estimate for each year was calculated as the difference in means between home-based primary care and comparison beneficiaries in that year minus the difference in the means in the year before the index date. Because of rounding, the percentage effect might exceed zero when the difference-in-differences estimate is zero.

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

ACSC = ambulatory care sensitive condition; CI = confidence interval; ED = emergency department; IAH = Independence at Home; LL = lower limit; UL = upper limit.

Responses to survey questions. Table D.30 contains an overview of the information collected in the survey described in Chapter IV and in this appendix. The tables that follow (D.31 through D.37) summarize the results of the survey responses. The survey questions were designed to investigate the possibility of unobserved differences between home-based primary

care recipients and comparison beneficiaries that influence both the decision to start home-based primary care and health care use and expenditure patterns. Certain survey questions were only asked of the home-based primary care recipients (Table D.31), others were only asked of the comparison respondents (Table D.32), and some were asked of both groups (Tables D.33 through D.37).

Table D.30. Summary of survey questions

Information collected from all respondents
Receipt of home-based primary care in the past year
Location of routine medical care
Current living situation
If help was received while completing survey
Preferences for how to handle various health issue scenarios (e.g. back pain, dizziness, flu)
Preference for doctor avoidance
Whether respondent had trouble accessing care
Level of satisfaction with primary care provider
Self-report of general health
Self-report of change in health status over the past year
Whether respondent gets help with certain everyday activities
Whether respondent has difficulty doing certain activities
Whether the respondent has a primary caregiver
If the respondent has a primary caregiver, what is the relationship and do they live in the same household
Information collected from home-based primary care recipients but not comparison beneficiaries
Who recommended home-based primary care
Reason for starting home-based primary care
Level of satisfaction with home-based primary care compared to doctor's office or clinic
Current use of home-based primary care
If no longer using home-based primary care, reason for discontinuing
Information collected from comparison beneficiaries but not home-based primary care recipients
Whether respondent had interest in home-based primary care
If respondent was interested in home-based primary care, the reason for interest
If respondent was not interested in home-based primary care, the reason for no interest

Table D.31. Questions for home-based primary care respondents

Question	Percentage of home-care recipients responding
Who recommended starting home-based primary care? (multiple responses allowed)	
My doctor	47.3
My home health agency	30.1
My social worker	30.5
One or more of my family members	43.2
Someone else	20.4
No one	4.9
Why did you switch to home-based primary care? (multiple responses allowed)	
You did not have transportation to the doctor's office	35.1
It became too difficult for you to travel to the doctor's office	75.0
You thought your out-of-pocket costs would be less with home-based primary care	18.3
It was more convenient to receive care in your home than to go to the doctor's office	79.1
You were not satisfied with the care at the doctor's office	12.2
You thought you could get better health care in your home	35.7
You felt it was your best option for getting the health care you needed	75.1
Your family wanted you to have it	63.2
It is easier on the people who look after you for you to get health care in your home	75.8
Other reason	18.4
Primary care in the home compared to primary care in a doctor's office or clinic	
I like home care a lot more	50.0
I like home care a little more	11.7
I like home care and doctor's office or clinic care about the same	21.8
I like home care a little less	4.8
I like home care a lot less	3.5
Don't know, missing, refused, multiple response	8.2
Most recent use of home-based primary care	
Less than 3 months ago	75.2
3 months ago or more	8.4
No longer receiving home-based primary care	9.5
Don't know, missing, multiple response	6.8
If no longer receiving primary care in the home, reasons for stopping (multiple responses allowed)	
The out-of-pocket costs were too high for me	18.2
I required more specialized care for my condition	49.1
I didn't like my home-based primary care provider	10.4
I thought I could get better health care going to the doctor's office or clinic	24.8
I got transportation to go to the doctor's office or clinic	31.4
My health improved so I no longer needed medical care at home	27.5
Home-based primary care was not as convenient as going to the doctor's office or clinic	15.1
My primary care provider stopped offering home-based primary care	25.5
Someone else made the decision that I would stop receiving home-based primary care	37.5
Other (specify)	46.3

Source: Mathematica's analysis of 2013–2016 Medicare claims and enrollment data on the Virtual Research Data Center and survey data.

Notes: The percentages reported are weighted using nonresponse weights.

Table D.32. Questions for comparison respondents

Question	Percentage of comparison beneficiaries responding
How interested are you in receiving home-based primary care?	
Extremely interested	17.4
Somewhat interested	24.8
Not at all interested	53.9
Don't know, missing, refused, multiple response	3.9
If extremely or somewhat interested, why? (multiple responses allowed)	
It's difficult to get transportation to the doctor's office or clinic	51.6
It's difficult for me to travel to the doctor's office or clinic	62.1
I think the out-of-pocket costs will be less than going to the doctor's office or clinic	25.6
I think it would be more convenient than to go to the doctor's office or clinic	68.8
I am not satisfied with the care I get at my doctor's office or clinic	17.5
I think I could get better health care with home-based primary care	28.1
My family wants me to get it	31.5
It would be easier on the people who look after me for me to get health care in my home	63.9
Other	24.5
If not interested, why not? (multiple responses allowed)	
My primary care provider doesn't offer it and I do not want to switch to a provider who does	26.3
I like going to my doctor's office or clinic	53.3
I am satisfied with the care I get at my doctor's office or clinic	73.2
There aren't any health care providers in my area that provide home-based primary care	10.9
I think it is more convenient to go to the doctor's office or clinic	43.9
I require specialized care for my condition that I don't think I could get in my home	26.5
I think the out-of-pocket costs will be too high for me	25.9
I don't want someone coming to my home	23.2
I don't think getting health care in my home would be as good as getting health care in doctor's office or clinic	33.3
Other (specify)	18.2

Source: Mathematica's analysis of 2013–2016 Medicare claims and enrollment data on the Virtual Research Data Center and survey data.

Notes: The percentages reported are weighted using nonresponse weights.

Table D.33. Help completing the survey and current living situation

Question	Percentage of home-based care respondents	Percentage of comparison respondents	Difference between home-based care and comparison respondents, percentage points
Did someone help you complete the survey?			
Yes	35.3	25.4	9.9*** (1.8)
No	52.5	63.6	−11.1*** (1.9)
Don't know, refused, or missing	12.2	11.1	1.1 (1.3)
If someone helped, type of help received by pencil-and-paper respondents (multiple responses allowed)			
Read the questions to me	27.0	35.7	−8.7 (5.8)
Wrote down the answers I gave	25.2	27.4	−2.2 (5.5)
Answered the questions for me	50.1	36.8	13.3** (6.2)
Translated the questions into my language	0.0	4.3	−4.3** (1.8)
Helped me in some other way	6.5	6.8	−0.3 (3.1)
If someone helped, type of help received by telephone respondents answering via proxy			
Answered questions without talking to the respondent	90.0	85.9	4.1 (2.7)
Answered questions together with the respondent	6.2	9.0	−2.8 (2.2)
Provided help in some other way	3.1	5.1	−2 (1.7)
Don't know	0.7	0.0	0.7 (0.5)
Current living situation (multiple responses allowed)			
Living alone	25.3	28.5	−3.2** (1.6)
Living with spouse/partner	19.3	27.7	−8.4*** (1.5)
Living with children at home	16.5	20.0	−3.5** (1.4)
Living with parent/guardian	3.8	5.1	−1.3 (0.7)
Living with other family members	15.3	16.5	−1.2 (1.3)
Living with others in an assisted-living facility	28.7	14.0	14.7*** (1.5)
Living with others in some other setting	13.2	11.6	1.6 (1.2)

Source: Mathematica's analysis of 2013–2016 Medicare claims and enrollment data on the Virtual Research Data Center and survey data.

Notes: The percentages reported are weighted using nonresponse and matching weights.

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

Table D.34. Comparison of preferences, doctor avoidance, trouble accessing care, and satisfaction with primary care provider

Question	Percentage of home-based care respondents who chose this response	Percentage of comparison respondents who chose this response	Difference between home-based care and comparison respondents, percentage points
How would you handle an episode of back pain?			
Continue to deal with the symptoms by yourself or ask for advice from friends or family	17.6	15.6	2.0 (1.3)
Call your primary care provider's office to get advice over the telephone	25.0	15.9	9.1*** (1.4)
Make an appointment to see your primary care provider	31.9	41.1	−9.2*** (1.7)
Make an appointment to see a specialist	4.1	6.7	−2.6*** (0.8)
Go to an urgent care center or an emergency room	13.7	14.1	−0.4 (1.2)
Don't know, missing, refused, multiple response	7.7	6.5	1.2 (0.9)
How would you handle an episode of dizziness?			
Continue to deal with the symptoms by yourself or ask for advice from friends or family	23.0	21.0	2.0 (1.5)
Call your primary care provider's office to get advice over the telephone	24.8	20.2	4.6*** (1.5)
Make an appointment to see your primary care provider	18.9	22.6	−3.7** (1.4)
Make an appointment to see a specialist	1.7	1.7	0.0 (0.5)
Go to an urgent care center or an emergency room	24.2	28.1	−3.9** (1.6)
Don't know, missing, refused, multiple response	7.5	6.4	1.1 (0.9)
You will do just about anything to avoid seeing a doctor.			
Agree	19.6	19.2	0.4 (1.4)
Disagree	76.2	78.3	−2.1 (1.5)
Don't know, missing, refused, multiple response	4.2	2.5	1.7*** (0.6)
Have you had trouble getting care that you wanted or needed?			
Yes	13.6	10.3	3.3*** (1.2)
No	83.9	87.9	−4.0*** (1.2)
Don't know, missing, refused, multiple response	2.5	1.8	0.7 (0.5)
Level of satisfaction with primary care provider			
Very satisfied	48.9	54.3	−5.4*** (1.8)
Satisfied	43.1	38.7	4.4** (1.8)
Dissatisfied	4.5	3.8	0.7 (0.7)
Very dissatisfied	1.3	0.9	0.4 (0.4)
Don't know, missing, refused, multiple response	2.1	2.4	−0.3 (0.5)

Source: Mathematica's analysis of 2013–2016 Medicare claims and enrollment data on the Virtual Research Data Center and survey data.

Notes: The percentages reported are weighted using nonresponse and matching weights.

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

Table D.35. How would you handle an episode of flu?

Statement	How much do you agree or disagree?		
		Percent disagree (strongly or somewhat)	Percent agree (strongly or somewhat)
Receiving health care in my home would be easier on my friends and family.	Home-based care recipient	13.4	79.6
	Comparison	29.5	63.5
Receiving health care in my home would be more comfortable than in a doctor's office or clinic.	Home-based care recipient	10.7	83.1
	Comparison	30.6	62.3
Receiving health care in my home can be as good as in a doctor's office or clinic.	Home-based care recipient	18.6	75.1
	Comparison	38.3	53.0
I would feel safe receiving health care in my home.	Home-based care recipient	11.0	84.0
	Comparison	26.4	66.1
Receiving health care in a doctor's office or clinic would be easier on my friends and family.	Home-based care recipient	56.4	36.5
	Comparison	36.4	56.2
It would bother me to have people come to my home to take care of me.	Home-based care recipient	67.4	25.7
	Comparison	50.5	42.3
I would feel safer receiving health care at a doctor's office or clinic.	Home-based care recipient	59.8	31.9
	Comparison	26.5	67.8

Source: Mathematica's analysis of 2013–2016 Medicare claims and enrollment data on the Virtual Research Data Center and survey data.

Notes: The percentages reported are weighted using nonresponse and matching weights. Not shown: the percentage who responded, "don't know," the percentage who refused to answer, and the percentage who did not respond. The *p*-value for the Wilcoxon rank sum test comparing the distribution of responses between claims-based home-based care recipients and comparison respondents is <0.01 for each statement.

Table D.36. Self-assessment of health status, assistance, and difficulty with daily activities

Question	Percentage of home-based care respondents who chose this response	Percentage of comparison respondents who chose this response	Difference between home-based care and comparison respondents, percentage points
Compared to other people your age, how would you rate your health?			
Excellent	4.3	4.5	−0.2 (0.7)
Very good	10.9	15.1	−4.2*** (1.2)
Good	22.9	27.6	−4.7*** (1.6)
Fair	30.6	31.0	−0.4 (1.6)
Poor	29.7	19.9	9.8*** (1.5)
Don't know, missing, refused, multiple response	1.6	1.9	−0.3 (0.5)
Compared to one year ago, how would you rate your health in general now?			
Much better now than one year ago	9.5	10.9	−1.4 (1.1)
Somewhat better now than one year ago	15.5	12.4	3.1** (1.2)
About the same	36.6	41.7	−5.1*** (1.7)
Somewhat worse now than one year ago	25.5	24.5	1.0 (1.5)
Much worse now than one year ago	11.9	8.6	3.3*** (1.1)
Don't know, missing, refused, multiple response	1.0	2.0	−1.0** (0.4)
Due to a health or physical problem, do you receive help from another person with the following everyday activities?			
Bathing or showering	73.2	56.0	17.2*** (1.7)
Dressing	63.2	45.2	18.0*** (1.8)
Eating	32.6	23.1	9.5*** (1.6)
Getting in or out of bed or chairs	53.5	37.5	16.0*** (1.8)
Walking	52.7	42.3	10.4*** (1.8)
Using the toilet	46.7	31.8	14.9*** (1.7)
Doing errands such as shopping or visiting a doctor's office or clinic	81.7	70.8	10.9*** (1.5)
Taking your prescribed medications in your home	67.8	53.0	14.8*** (1.7)
Using medical equipment (e.g., dialysis equipment, wheelchair, respirator, inhaler)	60.6	43.0	17.6*** (1.8)
Due to a health or physical problem, do you have difficulty doing any of the following activities on your own?			
Doing errands such as shopping or visiting a doctor's office	83.5	71.4	12.1*** (1.5)
Feeding yourself	29.7	19.3	10.4*** (1.5)
Using the toilet	49.1	31.0	18.1*** (1.7)

Source: Mathematica's analysis of 2013–2016 Medicare claims and enrollment data on the Virtual Research Data Center and survey data.

Notes: The percentages reported are weighted using nonresponse and matching weights.

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

Table D.37. Relationship with a primary caregiver

Question	Percentage of home-based care respondents who chose this response	Percentage of comparison respondents who chose this response	Difference between home-based care and comparison respondents, percentage points
Do you have a primary caregiver?			
Yes	81.3	71.4	9.9*** (1.5)
No	15.0	25.7	−10.7*** (1.4)
Don't know, missing, refused, multiple response	3.7	2.9	0.8 (0.6)
If you have a primary caregiver, what is that person's relationship to you?			
Family member	46.8	59.2	−12.4*** (2.0)
Friend	2.9	3.7	−0.8 (0.7)
Paid caregiver	40.0	30.5	9.5*** (2.0)
Other	8.0	5.0	2.8*** (1.0)
Don't know, missing, refused, multiple response	2.6	1.6	1.0* (0.6)
If you have a primary caregiver, do you live in the same household most of the time?			
Yes	53.4	54.0	−0.6 (2.0)
No	44.5	44.7	−0.2 (2.0)
Don't know, missing, refused, multiple response	2.1	1.3	0.8 (0.5)

Source: Mathematica's analysis of 2013–2016 Medicare claims and enrollment data on the Virtual Research Data Center and survey data.

Notes: The percentages reported are weighted using nonresponse and matching weights.

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

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