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MEDICAID 1115 DEMONSTRATIONS

Medicaid Section 1115 Demonstrations Summative Evaluation Report

Premium Assistance, Monthly Payments, and Beneficiary Engagement

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DISCLAIMER

The findings presented represent independent analysis conducted by the authors and do not represent the opinions of the Centers for Medicare & Medicaid Services or any state that contributed data to the report. The findings likewise do not represent the opinions of the Arkansas Insurance Department or the Arkansas Healthcare Transparency Initiative. Approval to use data from the Arkansas All-Payer Claims Database for this analysis is not an endorsement of the results presented herein.

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EXECUTIVE SUMMARY

Between 2014 and 2017, six states—Arkansas, Indiana, Iowa, Michigan, Montana, and New Hampshire—expanded Medicaid coverage to people with incomes up to 133 percent of the federal poverty level (FPL) through their state plans for medical assistance as permitted under the Affordable Care Act, and tested new approaches to administering Medicaid for this population using section 1115 authority.[1] These states implemented policy approaches that we grouped into three research domains: (1) premium assistance programs that enroll Medicaid beneficiaries in qualified health plans (QHPs) available in Marketplaces established by the Affordable Care Act, (2) monthly payment requirements similar to those in commercial health insurance, and/or (3) programs that encourage specific health behaviors to engage beneficiaries in managing their own health (Table ES.1).

The six demonstrations are large-scale policy experiments collectively affecting about 1.7 million people who were enrolled in 2017. Demonstration enrollees represented, on average, 31 percent of the total Medicaid-covered population in these states.[2]

Table ES.1. Demonstrations with premium assistance, monthly payments, and/or beneficiary engagement programs from 2014 through 2017

State	Demonstration implementation date	Domain 1: Mandatory Medicaid-supported QHP enrollment (premium assistance)	Domain 2: Premiums or other monthly contributions (monthly payments)	Domain 3: Beneficiary engagement programs to encourage health behaviors
Arkansas	Jan. 2014	X	X (started Jan. 2015, paused Apr. 2016, resumed Jan. 2017)	
Indiana	Feb. 2015		X	X
Iowa	Jan. 2014	Ended Dec. 2015	X	X
Michigan[a]	Apr. 2014		X	X
Montana	Jan. 2016		X	
New Hampshire[b]	Jan. 2016	X		

[a] Michigan received federal approval for Domain 1 premium assistance but did not implement this policy.
[b] New Hampshire implemented premium assistance in 2016 after expanding Medicaid coverage in 2014.

Aspects of all three types of policies may be of widespread interest as more states consider how to shape new versions of their Medicaid programs to best serve qualifying adults. Centers for Medicare & Medicaid Services (CMS) Administrator Seema Verma has affirmed the

[1] The Affordable Care Act established a 5 percent income disregard that increased the effective income limit from 133 to 138 percent of the federal poverty level.

[2] Estimated enrollment is based on the average of monthly state enrollment numbers in the Medicaid Budget and Expenditure system for the first three quarters of 2017. See https://www.medicaid.gov/medicaid/program-information/medicaid-and-chip-enrollment-data/enrollment-mbes/index.html.

administration's support for Medicaid reforms that include features of these policy types (Price and Verma 2017). Likewise, initial findings on the effects of these policies may be of interest to federal and state policymakers as they consider whether and how to incorporate features common in commercial health coverage within the Medicaid program.

A. Research questions

We explored the outcomes of these demonstrations from 2014 through 2017. For each of the three domains, we explored several primary research questions (Table ES.2), most of which comprised several subsidiary questions. These research questions were initially approved by CMS in 2015 and updated in two design reports focused on Medicaid expansion demonstrations. They are different from the research questions used in state-based evaluations, which often focus on state-specific policy goals or a state's implementation of section 1115 demonstration policies relative to the state's status quo of no expansion.[3] The research questions in this report also preceded CMS's 2019 evaluation design guidance for states with eligibility and coverage policies,[4] although CMS's recommended research questions about the effect of premiums or account payments on Medicaid enrollment and coverage continuity are similar to several research questions addressed in this report. (The evaluation design guidance does not address research questions about premium assistance or healthy behavior incentives to states.)

Table ES.2. Primary research questions by domain

Domain 1: Medicaid-supported enrollment in qualified health plans
1. How do states supporting QHP enrollment for newly eligible beneficiaries compare with other Medicaid expansion states in terms of access and health outcomes?
2. How do states supporting QHP enrollment compare with other Medicaid expansion states in terms of total spending?
3. How do states supporting QHP enrollment compare with other Medicaid expansion states in terms of take-up rates?
Domain 2: Premiums or other monthly contributions (monthly payments)
1. To what extent do requirements for monthly payments affect enrollment patterns?
2. What effects do monthly payments appear to have on continuity of coverage?
Domain 3: Beneficiary engagement programs to encourage health behaviors
1. What strategies are states using to educate beneficiaries about preferred health behaviors?
2. To what extent are Medicaid enrollees responsive to explicit behavior incentives?
3. Do behavior incentives affect overall access to and use of care?
4. Are population-level effects observed from Medicaid demonstration policies?

[3] This cross-state evaluation examines some outcomes also considered in state-based evaluations, such as the cost of coverage and use of the emergency department. Domain 2 outcomes—such as take-up among eligible people in states with monthly payments—were generally not considered in state evaluations, which rarely used out-of-state comparison groups, but are important for examining policy effects in a cross-state context.

[4] See the master narrative and policy-specific appendices to the eligibility and coverage evaluation design guidance: https://www.medicaid.gov/medicaid/section-1115-demo/evaluation-reports/evaluation-designs-and-reports/index.html

Tables A.1 through A.3 in Appendix A summarize the evaluation design by domain, including all research questions and the analytical approach, outcome measures, and demonstration and comparison states we used to answer each question. Demonstration states were included in analyses according to the type of policies they implemented and the availability of data; comparison states were included in analyses whenever data were available that allowed us to include them.

B. Evaluation methods

We drew on several data sources, including administrative enrollment and claims data, two national household surveys (the American Community Survey [ACS] and the Behavioral Risk Factor Surveillance System [BRFSS] survey), state-generated evaluation and monitoring data, and publicly available data on QHP issuers' participation in the Federally Facilitated Marketplace. Depending on the analysis, the available data span the period from January 2012 to December 2017, which includes some years before and after the demonstrations were implemented.[5] Analyses include up to nine comparison states that resembled the demonstration states in two important respects: each expanded Medicaid to include adults with incomes up to 133 percent FPL in 2014 or 2015, and each had historically low income eligibility thresholds for adults before the expansions (Table ES.3).

We used a variety of descriptive and regression-based analytic approaches, each of which has strengths and limitations. For example, we used national household survey data to understand coverage take-up rates among adults who were likely to be eligible for Medicaid, and to analyze changes in health behaviors and unmet health care needs among adults with low incomes. These data allowed for the largest set of demonstration and comparison states, and yielded information on the total pool of individuals who were likely to be eligible for coverage.

Survey data are also subject to different types of nonresponse bias, however, and surveys are known to undercount the number of adults enrolled in Medicaid. To balance these limitations, we also used Medicaid administrative data to examine the health care use patterns and enrollment histories of people who successfully enrolled in the demonstrations. By capitalizing on the benefits of different data sets and using different analytic strategies, we could explore whether different analyses told a consistent story about the main effects of the key policies.

[5] This evaluation focuses on demonstrations that were active in the years 2014 through 2017, although some states implemented later than 2014, Depending on the state and analysis, we include pre-implementation data from 2012 through 2015. For some analyses that include Iowa, which ended its premium assistance demonstration at the end of 2015, we include data after the demonstration was over.

Table ES.3. Demonstration and comparison states included in major analyses

State	Medicaid expansion date	Included in enrollment analyses based on administrative data	Included in utilization analyses based on administrative data	Included in analyses based on national survey data
Demonstration states				
Arkansas	January 2014	X	X	X
Indiana	February 2015	X	X	X
Iowa	January 2014	X	X	X
Michigan	April 2014	X	X	X
Montana	January 2016	X		X
New Hampshire	August 2014[a]	X	X	X
Comparison states				
Kentucky	January 2014	X	X	X
Nevada	January 2014			X
New Mexico	January 2014	X	X	X
North Dakota	January 2014			X
Ohio	January 2014	X	X	X
Oregon	January 2014			X
Pennsylvania	January 2015	X	X	X
Washington	January 2014			X
West Virginia	January 2014	X	X	X

[a] New Hampshire implemented its premium assistance demonstration in 2016 after expanding Medicaid coverage in 2014.

C. Evaluation results

Table E.4 summarizes findings for each research question, by domain. The table is followed by a brief discussion of the findings in each domain, with a focus on analyses that yielded statistically significant or otherwise meaningful results. It is important to note, however, that when comparing demonstrations to traditional Medicaid expansions, a lack of significantly different results may be an acceptable outcome. In several cases, no differences were observed because in both direct and alternative Medicaid expansion models, use of recommended services was high among the newly covered, and reductions in unmet need for care were consistently realized. When program outcomes are statistically indistinguishable from each other, states and CMS might want to consider other factors in deciding whether to pursue a demonstration or a traditional Medicaid expansion, such as the administrative costs of demonstration operations or the value of allowing state policies to vary in response to the political views and preferences of different state constituencies.

Table ES.4. Summary of findings by domain and research question

Research question	Main analytical approach	Principal findings
Domain 1: Medicaid-supported enrollment in qualified health plans		
1. How do states supporting QHP enrollment for newly eligible beneficiaries compare with other Medicaid expansion states in terms of access and health outcomes?		
1a. Can beneficiaries enrolled in QHPs access care at similar or better rates compared with beneficiaries enrolled in traditional Medicaid expansions?	Regression analysis with comparison groups	• Premium assistance was associated with higher rates of physician office visits in Iowa and New Hampshire relative to comparison states. Rates of physician office visits were lower in Arkansas, but we recommend caution in interpreting this finding because of data limitations. • There was no consistent association between premium assistance and use of prescription drug benefits or wraparound services (vision, dental, family planning, and non-emergency medical transportation).
1c. What is the unmet need for medical care?	Regression analysis with comparison groups	• In Arkansas and Iowa, premium assistance was associated with a higher probability of having had any check-up within the last year, relative to comparison states, and in New Hampshire, it was associated with a higher probability of having a personal provider.
1d. Is there continuity of coverage when switching between Medicaid and Marketplace coverage?	Descriptive analysis	• Arkansas and New Hampshire had complete overlap between Medicaid premium assistance and Marketplace issuers. Iowa and comparison states had less overlap.
2. How do states supporting QHP enrollment compare with other Medicaid expansion states in terms of total spending?		
2a. How do premium assistance states compare with other Medicaid expansion states in terms of per beneficiary spending on direct medical services and capitation payments?	Regression analysis with comparison groups	• Premium assistance was associated with higher per-member per-month Medicaid spending in New Hampshire relative to comparison states. Spending was lower in Iowa, although we recommend caution in interpreting this finding because of data limitations.
3. How do states supporting QHP enrollment compare with other Medicaid expansion states in terms of take-up rates?		
3a. How does the take-up rate among likely eligible individuals in premium assistance states compare with states with traditional Medicaid expansions?	Descriptive analysis	• Take-up in premium assistance states was lower than it was in most comparison states in most years.
3b. Are there patterns in the timing of Medicaid beneficiary enrollment in premium assistance states that could be related to the Marketplace open enrollment period, even though Medicaid beneficiaries are not subject to open enrollment periods?	Descriptive analysis	• There was no discernable relationship between enrollment in premium assistance and periods of Marketplace open enrollment.

Table ES.4 (*continued*)

Research question	Main analytical approach	Principal findings
Domain 2: Premiums and other monthly contributions (monthly payments)		
1. To what extent do requirements for monthly payments affect enrollment patterns?		
1a. Do eligible adults in states with required monthly payments enroll in Medicaid (or premium assistance programs) at the same rate as eligible adults in other states?	Regression analysis with comparison groups Descriptive analysis	• Living in a state that requires monthly payments was associated with a lower probability of enrolling in Medicaid. • Take-up in states with monthly payments was lower than it was in most comparison states in most years.
1b. Do eligible adults in key demographic groups who live in states with required monthly payments enroll in Medicaid (or premium assistance programs) at the same rate that eligible adults in other states do?	Regression analysis with comparison groups Descriptive analysis	• Having monthly payments of any amount was associated with a lower probability of Medicaid enrollment for all demographic subgroups. • Descriptive analysis of take-up rates by demographic subgroup revealed no clear differentiation in the relationship between monthly payments and enrollment by subgroups.
1d. How do monthly payment amounts affect take-up of coverage?	Regression analysis with comparison groups	• Owing a monthly payment was negatively associated with the probability of enrolling in Medicaid. The largest payment ($31+) was associated with the largest decrease in take-up.
2. What effects do monthly payments appear to have on continuity of coverage?		
2a. Is there a relationship between midyear disenrollments and the timing of monthly payment policies?	Descriptive analysis, including regressions	• There was no clear relationship between the onset of monthly payment policies and midyear disenrollment (departure prior to the renewal date).
2b. Is there a relationship between monthly payment requirements and renewals?	Descriptive analysis, including regressions	• There was a relatively low probability of renewal in three of the five states with monthly payment policies (Iowa, Indiana, Michigan); the probability of renewal in the other two (Arkansas and Montana) was higher than it was in comparison states.
2c. What is the effect of payment enforcement rules such as non-eligibility periods before re-enrollment?	Descriptive analysis	• Those disenrolled for nonpayment and subject to a non-eligibility period in Indiana were more likely to be lost to follow-up (that is, not to re-enroll within an 11-month observation period) than those who disenrolled for other reasons (except moving out of state).
2d. Is there a relationship between monthly payment requirements and long-term enrollment continuity?	Descriptive analysis, including regressions	• There was wide variation in continuous enrollment among both demonstration and comparison states. Iowa and Michigan had relatively low long-term continuous enrollment rates; Montana had higher rates than other states with monthly payments.
2e. Is there a relationship between monthly payment requirements and enrollment duration?	Regression analysis with comparison groups	• People who were estimated to have a monthly payment had shorter enrollment lengths than those without monthly payments.

Table ES.4 (*continued*)

Research question	Main analytical approach	Principal findings
Domain 3: Beneficiary engagement programs to encourage health behaviors		
1. What strategies are states using to educate beneficiaries about preferred health behaviors?		
1a. What strategies are states using to explain incentives and disincentives? Which strategies are perceived to be effective?	Document review	• States required contracted plans to use a variety of communication methods to explain demonstration incentives to beneficiaries, including mailed written materials, periodic telephone outreach, and staffed call centers. Plans often conducted additional outreach to their enrolled members. • Beneficiaries in all three states exhibited a generally limited understanding of the financial incentives and rewards available to them.
2. To what extent are Medicaid enrollees responsive to explicit behavior incentives?	Document review Descriptive analysis Regression analysis with comparison groups	• Financial incentives to have a wellness visit are associated with a higher probability of having a wellness visit in all three states.
3. Do behavior incentives affect overall access to and use of care?		
3a. Do behavior incentives yield gains in preventive care and chronic condition management?	Regression analysis with comparison groups	• Financial incentives to have a wellness visit were associated with increased use of preventive care; results for chronic condition management were mixed.
3b. Do behavior incentives yield reductions in disincentivized care (that is, non-emergent ED visits)?	Regression analysis with comparison groups	• Iowa's and Michigan's demonstrations were associated with lower non-emergency use of the emergency department. Indiana's demonstration, which was the only one with explicit financial disincentives for emergency department use, was associated with higher use.
3c. How do program incentives affect volume of and access to care?	Regression analysis with comparison groups	• Michigan's demonstration was associated with more use of primary care than in comparison states; Indiana's and Iowa's with less. • All three demonstrations were associated with more use of specialty care.
4. Are population-level effects observed from Medicaid demonstration policies?	Regression analysis with comparison groups	• There was no statistically significant influence on care-seeking or health outcomes at the population level.

Notes: There is no question 1B in Domain 1, and no question 1C in Domain 2, because those research questions, initially planned in 2015, were dropped for insufficient data.
Tables A.1 through A.3 in Appendix A summarize the evaluation design by domain, including research questions, analytical approach, outcome measures, and the demonstration and comparison states we used to answer each question.

1. Domain 1: Medicaid-supported QHP enrollment

Premium assistance seems to have expanded access to physician office visits. We assessed the use and promptness of physician office visits, prescriptions, vision, dental, family planning, and non-emergency medical transportation services. Using a difference-in-differences model, we found that during the premium assistance demonstrations in Iowa and New Hampshire, beneficiaries had more physician office visits than their counterparts in comparison states, a statistically significant result that suggests the demonstration increased access to such care. In contrast, in Arkansas (where data limitations precluded a difference-in-differences approach), a cross-sectional analysis suggested that beneficiaries had fewer physician office visits than those in comparison states; but this result could have been confounded by other factors. Beneficiaries in Iowa during its demonstration had higher prescription drug fill rates than beneficiaries in comparison states did, but demonstration beneficiaries in Arkansas and New Hampshire had lower use than those in comparison states. Beneficiaries' use patterns for vision, dental, family planning and non-emergency medical transportation services were mixed—some services were used at higher rates in demonstration states and some at lower rates.

Premium assistance also seems to have assuaged unmet needs for care. We used BRFSS data to assess unmet needs for medical care. We found that beneficiaries living in demonstration states during premium assistance demonstrations were more likely to have had a checkup in the last year (Arkansas and Iowa) or have a personal provider (New Hampshire) than those in comparison states. Unmet need for care specifically due to cost declined in both demonstration and comparison states after expansions.

State intervention may be necessary to enable continuity between Medicaid and Marketplace coverage. We found complete overlap in the sets of issuers who participated in premium assistance and in the Marketplace in Arkansas and New Hampshire, per state regulations, but there was much less overlap in Iowa, where Marketplace issuers' participation in premium assistance was optional. Complete issuer overlap in Arkansas and New Hampshire increases the potential for beneficiaries to stay enrolled with the same issuer as their eligibility for Medicaid expansion or Marketplace subsidies fluctuates. This degree of overlap seems unlikely to exist in the absence of state intervention—either through regulation or incentives—given Iowa's experience and the limited overlap between Marketplace and Medicaid Managed Care plans in most states.

Our findings suggest the demonstrations increased total Medicaid spending, but were inconclusive. We assessed how the total cost to Medicaid for coverage during premium assistance demonstrations in New Hampshire and Iowa compared to other states that enrolled beneficiaries in traditional Medicaid coverage. We were unable to include Arkansas in this analysis due to limitations of the Arkansas data. We found that expenditures were higher in New Hampshire than in comparison states, but lower in Iowa. We used a difference-in-differences regression design in New Hampshire, but not in Iowa because there were issues with Iowa's data quality outside of the demonstration period (2014–2015). Without controlling for baseline

differences, cross-sectional regression estimates for Iowa were possibly biased by unobserved differences across states, such as variation in health care prices or utilization.

Premium assistance demonstrations did not appear to spur enrollment. We found that states with premium assistance demonstrations had lower take-up rates than most comparison states in most years. The timing of Marketplace open enrollment did not appear to be associated with the timing of Medicaid enrollment in premium assistance demonstrations or in states with regular Medicaid expansions.

Policy takeaways. Overall, although our results were not the same for each state and analysis, they suggest that Medicaid beneficiaries who are enrolled in QHPs are more likely to see a doctor than those enrolled in traditional Medicaid coverage. Using difference-in-differences regression models, an analytically strong approach, we found statistically significantly higher rates of physician office visits in both Iowa and New Hampshire during their premium assistance demonstrations. We also found that premium assistance reduces unmet need for care, although there were some differences across measures. These findings align with our expectation that higher levels of physician reimbursement under premium assistance would give beneficiaries better access to care than traditional Medicaid coverage would. We did not expect the same for wraparound services, because they were provided as a fee-for-service Medicaid benefit in both demonstration and comparison states. Likewise, results of a difference-in-difference model of expenditures were consistent with our expectation that premium assistance would be more costly than traditional Medicaid coverage, because Medicaid agencies must make premium payments to QHPs that in part reflect higher physician reimbursement by plans. Finally, states with premium assistance demonstrations have lower take-up than most comparison states, but this may be due to confounding state factors that we cannot disentangle from premium assistance policies.

2. Domain 2: Premiums and other monthly financial contributions

Requiring monthly payments dampens enrollment in Medicaid. Five states—Arkansas, Indiana, Iowa, Michigan, and Montana—implemented demonstrations that collected monthly payments from beneficiaries with incomes up to 133 percent FPL. Regression models based on ACS data through 2017 reveal that people who live in states that require monthly payments are less likely to enroll in Medicaid, regardless of whether a given person is expected to owe any payments at all. There was also a negative association between owing a monthly payment and the probability of enrolling in Medicaid. The reduction in the probability of enrolling for those in the highest estimated payment category ($31 or more) was notably higher than for those with other payment amounts. The relationship between facing a monthly payment and the probability of enrollment was statistically significant and negative for all demographic subgroups. Although there are limitations to these models—most notably that they do not account for variation in the payment incentives and nonpayment consequences of different demonstration states—our findings were robust to multiple sensitivity tests and are consistent with published research on the effects of premiums on enrollment for both adults and children.

Estimated take-up rates, which combine ACS and administrative data to estimate enrollment in Medicaid among the likely eligible population, increased in all states in the year after states expanded Medicaid. Take-up in demonstration states in 2015, a year when premiums were in effect for all demonstration states but Montana (which had not yet expanded coverage), was lower than take-up in most comparison states in the same year. In 2016, the pattern was less consistent. However, take-up in states with monthly payments was lower than in most comparison states in most years.

The results of our analyses of the relationship between monthly payment policies and enrollment continuity within the first, second, and third coverage year were inconclusive. Regression estimates of renewals after the first enrollment year show a relatively low probability of renewal in three of the five states with monthly payment policies (Iowa, Indiana, Michigan), but the probability of renewal in the other two (Arkansas and Montana) was higher than for comparison states. Differences between demonstration and comparison states disappeared by the third enrollment year, suggesting that those who remain enrolled value coverage highly, are accustomed to making monthly payments, or both. Available administrative data did not permit us to disaggregate beneficiaries by income level to analyze differences in continuity between those who did and did not owe monthly payments.

Relatively few people subject to a non-eligibility period for nonpayment re-enrolled, but those who did re-enroll did so right after the non-eligibility period ended. A focused analysis of payment enforcement rules in Indiana revealed that beneficiaries who were disenrolled for nonpayment and subject to a six-month non-eligibility period tended to have one of two distinct outcomes. Such beneficiaries were more likely to be lost to follow-up (that is, to not re-enroll within an 11-month observation period) than those who disenrolled for any other reason, except for moving out of state. They were also more likely to be lost to follow-up than to re-enroll. However, beneficiaries who were disenrolled for nonpayment and who did subsequently re-enroll had an average gap between spans of 7.0 months. This gap was longer than the average gap among those who disenrolled for other reasons, but it indicates that only a short time elapsed between the end of the non-eligibility period and re-enrollment for most beneficiaries who re-enrolled within 11 months. Thus, beneficiaries who were disenrolled for non-payment either failed to re-enroll for 12 or more months or they re-enrolled shortly after regaining eligibility.

Monthly payments appear to reduce enrollment lengths. Finally, we examined long-term enrollment continuity by estimating the probability of remaining continuously enrolled for periods longer than a full year and conducting a survival analysis of the time to disenrollment throughout the study period. Results of the survival model showed that people who might have owed a monthly payment on the basis of the timing of payment obligations had enrollment lengths that were 84 percent as long as those not estimated to owe a monthly payment. This estimate was statistically significant, but should be interpreted with caution; we did not have data on people's income and were therefore unable to distinguish between those who did and did not owe premiums within each state. Descriptive analyses of the probability of continuous enrollment at 18, 24, 36, and 48 months did not reveal consistent patterns; demonstration states

with monthly payments had continuous enrollment rates at both the high and low ends of the rates observed across demonstration and comparison states.

Policy takeaways. Taken together, our results point to a negative relationship between monthly payments and enrollment. Regression models based on national survey data revealed a negative association between living in states with monthly payments and the probability of Medicaid enrollment, regardless of whether a given person is expected to owe any payments at all. There was also a negative association between owing a monthly payment and the probability of Medicaid enrollment; the largest payment was associated with the largest decrease in take-up. Analyses of enrollment continuity, drawing on administrative data, were inconsistent, and limited by the lack of an income variable that would allow us to segment analyses by those who did and did not owe payments in demonstration states. However, a survival analysis revealed a statistically significant negative relationship between enrollment duration, or length of continuous enrollment spans, and being estimated to owe a monthly payment (based on the estimated onset of payment obligations within each individual's enrollment span). A separate analysis of non-eligibility periods as a consequence of nonpayment suggests that this form of enforcement could extend the period prior to reenrollment and reduce the number of people who return to Medicaid coverage.

3. Domain 3: Beneficiary engagement programs to encourage health behaviors

Explicit financial incentives encourage healthy behavior. Three states—Indiana, Iowa, and Michigan—implemented demonstrations that included financial incentives for specific health behaviors. Our quantitative analyses suggest that more people might practice healthy behaviors if they were given specific financial incentives to do so. Regression models based on administrative data for 2014–2017 revealed that living in a state with a financial incentive to have a wellness visit was associated with a higher likelihood of making such a visit. This is consistent with our hypothesis and with previous research showing that financial incentives for one-time actions can be effective.

Our findings were more mixed on the question of whether financial incentives to have a wellness visit and a health risk assessment (HRA) were associated with favorable changes in utilization of other health care services that were not directly encouraged or discouraged. In all three states, utilization of preventive services was consistently higher than in the comparison states. But we found mixed results on management of chronic conditions, use of primary and specialty care, and use of emergency services.

In Iowa, beneficiaries with diabetes had lower rates of HbA1C testing and higher rates of diabetes-related hospitalization than comparison state beneficiaries, whereas the opposite was true for beneficiaries in Indiana and Michigan. Rates of follow-up after hospitalization varied widely between demonstration states. Michigan's demonstration was associated with increased use of primary care, but Indiana's and Iowa's were associated with decreased use of primary care and substantially higher use of specialty care. Similarly, although Iowa's and Michigan's demonstrations were associated with slightly less use of the emergency department, Indiana's

was associated with higher use. In neither of the demonstration states where we measured urgent care use was it more common than in comparison states.

Our analyses of beneficiaries who completed HRAs in Iowa revealed that they were more likely to also receive preventive services, manage their diabetes, and avoid the emergency department. However, because HRA completion is voluntary, we cannot distinguish between two competing explanations for this association. Completing an HRA might have given beneficiaries information that encouraged them to keep pursuing health care, but it is also possible that the observed association was driven by beneficiaries who were generally more motivated to manage their health and were therefore more likely to receive preventive services and manage their chronic conditions independent of any external incentives to do so.

Population-level effects are not apparent. Estimates from difference-in-differences models using BRFSS data did not indicate the presence of population-level impacts of state demonstration policies on a variety of self-reported utilization and health outcomes. In general, we found no statistically significant differences in such outcomes between demonstration and comparison states.

Policy takeaways. Combining our analytic approaches, we found evidence that financial incentives for specific healthy behaviors can prompt beneficiaries to engage in those behaviors, particularly if the incentives are easy to understand and beneficiaries have control over completing the necessary actions. We found some evidence that the demonstrations' implicit incentives to form a relationship with a physician promote receipt of preventive care, but more mixed evidence on whether such incentives encourage people to get primary care or manage their chronic conditions. We also found mixed evidence that the demonstrations implemented in Indiana, Iowa, and Michigan discouraged non-emergent use of the emergency department or shifted care to more appropriate venues, such as urgent care clinics. We also did not find evidence suggesting that there were any spillover effects of demonstration policies from Medicaid beneficiaries to their peers and family.

In sum, although incentives for specific healthy behaviors can increase use of services most directly related to the healthy behavior, use of downstream services was not systematically affected positively or negatively relative to comparison states. It is important to note that our analyses faced several data quality limitations that prevented us from controlling for important state- and beneficiary-level factors that influence service utilization. However, based on our findings, states that want to influence utilization of particular services might choose to directly focus their incentive programs on those services.

D. Conclusions

We report cross-state research results on six states that used section 1115 authority to incorporate features common in commercial health coverage within state Medicaid programs. We focus on different subsets of these states to examine each of three domains, or policy types. Our findings on the effects of these policies in our 2014–2017 study period could be of interest to federal and state policymakers as they consider whether and how to incorporate similar policy elements in the future.

Our analyses have several limitations. In particular, most of our results must be understood as correlational rather than causal because of limitations in data availability and quality. However, our use of multiple data sources and approaches to answer each primary research question increases our confidence in the findings and provides a more comprehensive examination of these policies.

High-level findings

- Our analyses of premium assistance demonstrations reveal that supporting enrollment in QHPs can give people greater access to physicians, but those services probably cost more than they do under direct expansion.
- Findings from states that implemented premiums or other monthly payments suggest that requiring people with limited incomes to make such payments probably keeps many of them from enrolling in Medicaid and causes others to drop out, shortening the average duration of enrollment. We also find that imposing a period of non-eligibility on people who fail to make payments potentially creates gaps in insurance coverage, because after the non-eligibility period ends, many people do not promptly re-enroll.
- Our analyses of states that implemented beneficiary engagement demonstrations show that more people might engage in healthy behaviors if they were given financial incentives for specific actions. However, we found little evidence of substantial downstream utilization or health changes stemming from the incentivized behaviors.

Collectively, this body of work, together with the national evaluation team's rapid-cycle reports focused on demonstration implementation, will help both CMS and state officials understand the potential for section 1115 eligibility and coverage policies to drive outcomes in state health care systems. Although few findings in this report are conclusive, they provide valuable evidence on both demonstration design and implementation considerations that CMS and states can use to meet their policy goals.

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I. INTRODUCTION

Between 2014 and 2017, six states—Arkansas, Indiana, Iowa, Michigan, Montana, and New Hampshire—expanded Medicaid coverage to people with incomes up to 133 percent of the federal poverty level (FPL) through their state plans for medical assistance as permitted under the Affordable Care Act, and tested new approaches to administering Medicaid for this population using section 1115 authority.[6,7] These states implemented: (1) premium assistance programs that enroll Medicaid beneficiaries in qualified health plans (QHPs) available in the Marketplaces established by the Affordable Care Act, (2) monthly payment requirements similar to those in commercial health insurance, and/or (3) programs that encourage specific health behaviors to engage beneficiaries in managing their own health.

The six demonstrations are large-scale policy experiments that collectively enrolled about 1.7 million people in 2017. Demonstration enrollees represented, on average, 31 percent of the total Medicaid-covered population in these states.[8]

This report presents research findings from a cross-state evaluation of policies in these six demonstrations. Findings may be of interest to federal and state policymakers as they consider whether and how to incorporate common features of commercial health coverage into state Medicaid programs. Centers for Medicare & Medicaid Services (CMS) Administrator Seema Verma has affirmed the administration's support for Medicaid reforms that include these features (Price and Verma 2017).

Analyses in this report focus on up to four years of implementation for the demonstration states. Depending on the analysis, the available data span the period from January 2012 to December 2017, which includes the years immediately before the states implemented their demonstrations in some cases. We include different combinations of states in different analyses, depending on the type of demonstration and the limitations of the data.

The research questions addressed in this report were initially approved by CMS in 2015 and updated in two design reports focused on Medicaid expansion demonstrations. They are different from the research questions used in state-based evaluations, which often focus on state-specific policy goals or a state's implementation of section 1115 demonstration policies relative to the

[6] The Affordable Care Act established a 5 percent income disregard that increases the effective income limit from 133 to 138 percent of the federal poverty level.

[7] Section 1115 of the Social Security Act authorizes experimentation by state Medicaid programs. Under its provisions, states may apply for federal permission to implement and test new approaches to administering Medicaid programs that depart from federal rules yet are consistent with the program's overall goals and budget neutral to the federal government.

[8] Estimated enrollment is based on the average of monthly state enrollment numbers in the Medicaid Budget and Expenditure system for the first three quarters of 2017. See https://www.medicaid.gov/medicaid/program-information/medicaid-and-chip-enrollment-data/enrollment-mbes/index.html.

state's status quo of no expansion.[9] The research questions in this report also preceded CMS's 2019 evaluation design guidance for states with eligibility and coverage policies,[10] although CMS's recommended research questions about the effect of premiums or account payments on Medicaid enrollment and coverage continuity are similar to several research questions addressed in this report. (The evaluation design guidance does not address research questions about premium assistance or healthy behavior incentives to states.)

This report is organized by research domains corresponding to the three policy types. In Domain 1, we explore the advantages and challenges of expanding Medicaid by supporting enrollment in QHPs offered via the Marketplaces established via the Affordable Care Act. Known as premium assistance demonstrations, these programs allow states to cover the insurance premiums for eligible adults as an alternative to providing traditional Medicaid coverage. In Domain 2, we assess the effect of premiums and monthly contributions to beneficiary accounts on take-up and continuity of coverage for states that are authorized to require such contributions for beneficiaries with incomes below 133 percent FPL. In Domain 3, we explore the mechanisms and effectiveness of beneficiary engagement programs that incentivize health behaviors such as completing health risk assessments and making wellness visits. Several states implemented more than one of these policies within the same demonstration and are thus included in more than one research domain (Table I.1). Tables A.1 through A.3 in Appendix A summarize the evaluation design by domain, including research questions, analytical approach, outcome measures, and the demonstration and comparison states we used to answer each question. Appendix B summarizes demonstration policies by state.

[9] This cross-state evaluation examines some outcomes also considered in state-based evaluations, such as the cost of coverage and use of the emergency department. Domain 2 outcomes—such as take-up among eligible people in states with monthly payments—were generally not considered in state evaluations, which rarely used out-of-state comparison groups, but are important for examining policy effects in a cross-state context.

[10] See the master narrative and policy-specific appendices to the eligibility and coverage evaluation design guidance: https://www.medicaid.gov/medicaid/section-1115-demo/evaluation-reports/evaluation-designs-and-reports/index.html

Table I.1. Demonstrations with premium assistance, monthly payments, and/or beneficiary engagement programs from 2014 through 2017

State	Demonstration start date	Domain 1: Mandatory Medicaid-supported QHP enrollment (premium assistance)	Domain 2: Premiums or other monthly contributions (monthly payments)	Domain 3: Beneficiary engagement programs to encourage health behaviors
Arkansas	Jan. 2014	X	X (started Jan. 2015, paused Apr. 2016, resumed Jan. 2017)	
Indiana	Feb. 2015		X	X
Iowa	Jan. 2014	Ended Dec. 2015	X	X
Michigan[a]	Apr. 2014		X	X
Montana	Jan. 2016		X	
New Hampshire[b]	Jan. 2016	X		

[a] Michigan received federal approval for Domain 1 premium assistance but did not implement this policy.
[b] New Hampshire implemented premium assistance in 2016 after expanding Medicaid coverage in 2014.

In the following chapters, we describe our analytical approach, including data sources and research methods (Chapter II), and present our findings organized by domain (Chapters III, IV, and V). We close with a discussion of our results (Chapter VII).

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II. DATA SOURCES AND METHODS

This evaluation draws on many data sources, including administrative enrollment and claims data, two national household surveys, state-generated evaluation and monitoring data, and publicly available data on QHP issuers' participation in the Federally Facilitated Marketplace. We employed a variety of descriptive and regression-based techniques to analyze these data. Depending on the source of data and the type of analysis, the analyses are based on six demonstration states and up to seven comparison states. In this chapter, we provide an overview of the data sources, comparison strategy, and major analytical approaches used for all three research domains. Detailed technical descriptions are available in the appendices. Appendix C contains details on administrative data sources, availability, and sample characteristics; Appendices D and E contain modeling specifications and descriptive statistics for analyses based on national household survey data.

A. Data sources

Medicaid administrative data. We obtained administrative data on Medicaid enrollment and claims from a number of sources. For most states (including all comparison states), we obtained data from the Medicaid Analytic eXtract (MAX)—including the early version of MAX data known as Alpha-MAX—for the first few years of the study period. For later years, we obtained data from the Transformed Medicaid Statistical Information System (T-MSIS) Analytic File (TAF). The number of available months of MAX, Alpha-MAX, and TAF varies by state.

For some demonstration states, we obtained administrative data from other sources. We obtained QHP encounter records for Arkansas (2014–2017) and New Hampshire (2016–2017) through each state's All-Payer Claims Database (APCD) because encounter records were not in the Medicaid administrative data these states reported to CMS. For Arkansas, we also used APCD data to get enrollment information for all demonstration beneficiaries and claims data for those enrolled in traditional Medicaid coverage. We also obtained data directly from Iowa's Medicaid agency for the years 2014 and 2015 because those data contain variables we needed to analyze the state's premium assistance demonstration. The sources of Medicaid administrative data, by state and year, are listed in Table C.1 in Appendix C.

Most of our analyses of administrative data focus on adults newly eligible for Medicaid in states that expanded eligibility to adults with incomes up to 133 percent of the FPL. There were no standardized eligibility codes for these adults in MAX and Alpha-MAX. For states where (1) MAX and Alpha-MAX were the source of administrative data for post-expansion years and (2) expansion eligibility codes were unknown to the research team, we identified the expansion population by identifying state-specific eligibility codes with large enrollment increases after Medicaid expansions were implemented (Appendix Table C.3). To identify these codes, we first restricted the data to non-disabled adults ages 19–64 who were not eligible for Medicare (that is, non-dual–eligible). We also excluded adults in limited benefit programs, such as family planning services and programs designed for pregnancy only. Of the remaining state-specific eligibility

codes, we assumed that codes with large post-expansion increases as a proportion of all enrollment in each state included the adult expansion beneficiaries.

TAF data, which we used for the later years of the study period, are relatively new, and state reporting and data processing routines are still being refined. Consequently, a number of limitations affected our analyses. Enrollment data for several states did not meet our quality standards,[11] leading us to drop four potential comparison states from analyses based on TAF and to exclude TAF for Indiana in 2017. Reducing the number of comparison states reduces our statistical power and increases the risk of idiosyncratic findings, but we were still able to include five comparison states in analyses of administrative data. Arkansas's TAF enrollment data also did not meet our standards for inclusion, but we were still able to include Arkansas because we had access to the state's APCD data.

We dropped some states from specific analyses because of poor data quality, although we included those states in other analyses. In expenditures analyses, for example, we excluded Iowa in 2016 and 2017 and Pennsylvania in every year because these two states' reported capitation payments were unreliable. We dropped New Mexico from analyses of prescription drug fill rates due to unreliable data. Still other data quality issues were not serious enough to warrant dropping states from analyses but are considered limitations. For example, Pennsylvania had high rates of missing procedure codes, and we observed indications of incomplete claim adjudication in Ohio.

We were also unable to include some important control variables. The quality of the income variable in TAF was unreliable, as was information on race and ethnicity for many states. We were unable to include a race/ethnicity variable for Iowa, Kentucky, New Mexico, or West Virginia; race/ethnicity is defined as "white, non-Hispanic" and "other/unknown" for Montana because there were not enough people in the "black, non-Hispanic" and "Hispanic/Latino" categories to create separate groups for analysis. For these reasons, models that pool several states' administrative data do not include a race/ethnicity control. For Arkansas, our data contained only three-digit zip codes for beneficiaries, and therefore we used the fraction of beneficiaries within the three-digit zip code who lived in rural areas instead of using a binary indicator for urban/rural as we did in other states.

Integrated Public Use Microdata Sample (IPUMS) from the American Community Survey (ACS). To estimate the expansion population eligible for Medicaid in each state and to model changes (from 2012 through 2017) in the probability that the likely eligible population reports having Medicaid coverage, we used IPUMS data prepared by the Minnesota Population Center at the University of Minnesota (Ruggles et al. 2019). IPUMS uses U.S. Census Bureau data collected through the ACS. The ACS provides annual data on health insurance coverage status and demographic characteristics, including income, citizenship, gender, disability status, race, and ethnicity. ACS data are collected throughout the year using 12 independent monthly

[11] We required less than a 10 percent difference in total enrollment (averaged over 12 months of the calendar year) from what is reported in the Medicaid Budget and Expenditure System.

samples. State-level estimates of health coverage derived from IPUMS-ACS are thought of as averages for the year in each state.

The ACS has a large sample, and its approach to creating annual average estimates of health insurance coverage is considered more accurate than the approach used for the Current Population Survey, which produces annual point-in-time estimates for each state. The U.S. Census Bureau constructs survey weights for the ACS to account for seasonal fluctuations in population and other sources of potential bias (Spielman et al. 2014). Our analyses incorporate these survey weights.

As with other national household surveys, health insurance coverage is self-reported in the ACS, and there is a known undercount of Medicaid enrollment. The undercount is more pronounced in Medicaid expansion states: linked 2016 ACS and Medicaid administrative data (both Medicaid and CHIP Performance Indicator data and MSIS data) suggest the undercount is 10.64 percent in expansion states versus 0.02 percent in non-expansion states (Boudreaux 2019). It is not possible to remedy this by using a different federal survey, because the Current Population Survey, National Health Interview Survey, and Medical Expenditure Panel Survey all undercount the Medicaid population to a comparable or greater degree.

Behavioral Risk Factor Surveillance System (BRFSS). BRFSS is a national household survey established by the Centers for Disease Control and Prevention to collect data on health conditions and health care utilization. We used BRFSS data for the period 2012 to 2017 to examine population-level changes in self-reported unmet need for care and in preventive and chronic care behaviors among people with low incomes. The BRFSS does not include insurance coverage status, so we constructed a sample that would resemble the Medicaid population by including respondents ages 18 to 64 with annual household incomes under $35,000. The resulting BRFSS sample included both current and former Medicaid enrollees along with some people who have never been enrolled in Medicaid even though they have low incomes. Population-level effects could take place if impacts for Medicaid beneficiaries were large enough or diffused throughout the broader population.

Information on participation of Marketplace and Medicaid issuers. We collected information on Marketplace and Medicaid issuers for demonstration and comparison states from 2014 to 2018 to update analyses that Mathematica first presented in a rapid-cycle report on issuer participation (Natzke and Chao 2018). To compile lists of Marketplace issuers for states that used the Federally Facilitated Marketplace, we used the CMS Center for Consumer Information & Insurance Oversight Service Area Public Use Files for the years 2014 to 2018 (CMS 2014a; CMS 2015; CMS 2016a; CMS 2017a; CMS 2018a), and linked the issuer identification numbers with the third-quarter 2014, second-quarter 2016, and second-quarter 2017 CMS Plan Finder data (CMS 2014b; CMS 2016b; CMS 2017b) to obtain the full issuer names. For states with a State-Based Marketplace, we used state websites, press releases, and reports as of November 5, 2018. Information on issuers participating in Iowa's premium assistance demonstration came

from quarterly monitoring reports.[12] Information on Oregon's issuers came from the 2014–2018 Oregon Health Plan Data and Reports.

We compiled lists of Medicaid issuers by using combinations of information from the database on 2014–2017 CMS Medicaid Managed Care Enrollment and Program Characteristics (CMS 2016c; CMS 2016d; CMS 2018b; CMS 2019), 2018 National Committee for Quality Assurance (NCQA) lists of health insurance plan ratings for Medicaid (NCQA n.d.), and Kaiser Family Foundation's list of 2018 Medicaid managed care organizations and their parent firms (Kaiser Family Foundation n.d. [b]). We considered the 2014–2017 CMS report to be the most accurate list of participating plans because NCQA excluded plans that had fewer than 8,000 members and either did not publicly report their data or did not report any accreditation information or data from the Healthcare Effectiveness Data and Information Set (HEDIS) or the Consumer Assessment of Healthcare Providers and Systems (CAHPS) to NCQA. CMS data were not available for the last year of the analysis, so we relied on NCQA information for 2018.

Healthy Indiana Plan (HIP) 2.0 data. We obtained data on Indiana's HIP 2.0 demonstration directly from the state because beneficiary account activity and reasons for disenrollment were not included in the Medicaid administrative data that Indiana reported to CMS. We received monthly enrollment and disenrollment data for HIP 2.0 beneficiaries from February 2015 through September 2018, and span-level data from February 2015 through December 2017. The span-level data included beneficiary account activity such as account balance "rollovers" that rewarded use of recommended services. At the end of December 2017, Indiana began to operate its beneficiary accounts on a calendar-year basis regardless of a beneficiary's coverage renewal date within the calendar year. The data we obtained from the state enabled us to observe enrollment for part of 2018, but not account activity, because beneficiary accounts were reconciled at the end of each calendar year.

State evaluation and monitoring reports. We reviewed state evaluation and monitoring reports submitted to CMS by demonstration states through December 2018 to help answer research questions that address (1) states' efforts to educate beneficiaries about the demonstrations, (2) findings from state-led surveys that could shed light on beneficiaries' understanding of incentives in the demonstrations, and (3) state findings related to provider participation and unmet need for medical care. Reviewing these evaluation and monitoring reports also provides a context for interpreting the findings of quantitative analyses.

Rapid-cycle reports. The evaluation team also produced a series of rapid-cycle reports on demonstration implementation that generated information about which outcomes were likely to change and why outcomes might have varied across states that implemented similar policies.

[12] The Arkansas and New Hampshire state departments of insurance require issuers that offer QHPs in the Marketplace to offer plans that qualify for the premium assistance program. Therefore, by definition, all of Arkansas and New Hampshire's Marketplace issuers also offer plans to Medicaid demonstration beneficiaries. Because Iowa did not have this requirement, we reviewed Iowa's quarterly monitoring reports to obtain a list of Marketplace issuers offering plans that qualified under the premium assistance program.

Throughout this report, we interpret results in light of the information generated by rapid-cycle reporting. We present key findings from rapid-cycle reports on beneficiary incentives, beneficiary education strategies, and beneficiaries' understanding of demonstration policies.[13]

B. Comparison strategy

Our strategy in each domain was to compare, where feasible, the demonstration states (Arkansas, Indiana, Iowa, Michigan, Montana, and/or New Hampshire) to non-demonstration states that were like the demonstration states in two important respects: each state expanded Medicaid to include adults with incomes up to 133 percent FPL in 2014 or 2015, and each had historically low income eligibility thresholds for adults before the expansions. This strategy helped to isolate the effects of demonstration policies from the effects of coverage expansions. The mix of demonstration and comparison states that were included in particular analyses depended on data availability. Table II.1 summarizes the comparison strategy by analysis type.

Table II.1. Demonstration and comparison states included in major analyses

State	Medicaid expansion date	Included in enrollment analyses based on administrative data	Included in utilization analyses based on administrative data	Included in analyses based on national survey data
Demonstration states				
Arkansas	January 2014	X	X	X
Indiana	February 2015	X	X	X
Iowa	January 2014	X	X	X
Michigan	April 2014	X	X	X
Montana	January 2016	X		X
New Hampshire[a]	August 2014	X	X	X
Comparison states				
Kentucky	January 2014	X	X	X
Nevada	January 2014			X
New Mexico	January 2014	X	X	X
North Dakota	January 2014			X
Ohio	January 2014	X	X	X
Oregon	January 2014			X
Pennsylvania	January 2015	X	X	X
Washington	January 2014			X
West Virginia	January 2014	X	X	X

[a] New Hampshire implemented its premium assistance demonstration in 2016 after expanding Medicaid coverage in 2014.

[13] All rapid-cycle reports on demonstration implementation are available at https://www.medicaid.gov/medicaid/section-1115-demo/evaluation-reports/federal-evaluation-and-meta-analysis/index.html.

For most analyses, we compared outcomes in the demonstration states with those in five non-demonstration comparison states: Kentucky, New Mexico, Ohio, Pennsylvania, and West Virginia. For analyses that relied only on national survey data, we included another four comparison states that met the same inclusion criteria: Nevada, North Dakota, Oregon, and Washington. These four states could not be used for analyses that relied on Medicaid administrative data due to data quality issues.

Before 2014, several of the comparison states had already implemented limited expansions of adult coverage through section 1115 authority. These early programs limited the number of enrollees (New Mexico, Oregon, Washington), the benefit plan (New Mexico), and/or the targeted geographic area (Ohio). These states experienced large increases in the number of enrolled non-disabled adults at the same time as the states that implemented alternative Medicaid expansions did. For example, there was a 65 percent increase in adult Medicaid enrollment in Oregon from 2013 to 2014, net of transfers from state-funded programs. Similarly, there was a 46 percent increase in Medicaid enrollment for adults in Washington from 2013 to 2014, net of transfers from state-funded programs. These percentage changes in enrollment were among the largest enrollment increases for states that expanded Medicaid in 2014. (Figures are based on Mathematica analysis of Medicaid and CHIP Performance Indicator data and on state reports of enrollment in state-funded programs that predated the 2014 Medicaid expansions.) Two demonstration states, Michigan and Indiana, also used section 1115 authority to operate limited programs for adults before they implemented their current demonstrations.

C. Analytic approaches

We used both descriptive and regression-based approaches to answer research questions in each domain. For reference, Table II.2 summarizes research questions by domain. (Tables A.1 through A.3 in Appendix A list research questions and summarize the analytical approaches for each.)

Table II.2. Research questions by domain

Domain 1: Medicaid-supported enrollment in qualified health plans
1. How do states supporting QHP enrollment for newly eligible beneficiaries compare with other Medicaid expansion states in terms of access and health outcomes? 1a. Can beneficiaries enrolled in QHPs access care at similar or better rates compared with beneficiaries enrolled in traditional Medicaid expansions? 1c. What is the unmet need for medical care? 1d. Is there continuity of coverage when switching between Medicaid and Marketplace coverage?
2. How do states supporting QHP enrollment compare with other Medicaid expansion states in terms of total spending? 2a. How do premium assistance states compare with other Medicaid expansion states in terms of per beneficiary spending on direct medical services and capitation payments?

Table II.2 (*continued*)

Domain 1: Medicaid-supported enrollment in qualified health plans
3. How do states supporting QHP enrollment compare with other Medicaid expansion states in terms of take-up rates? 3a. How does the take-up rate among likely eligible individuals in premium assistance states compare with states with traditional Medicaid expansions? 3b. Are there patterns in the timing of Medicaid beneficiary enrollment in premium assistance states that could be related to the Marketplace open enrollment period, even though Medicaid beneficiaries are not subject to open enrollment periods?
Domain 2: Premiums and other monthly contributions (monthly payments)
1. To what extent do requirements for monthly payments affect enrollment patterns? 1a. Do eligible adults in states with required monthly payments enroll in Medicaid (or premium assistance programs) at the same rate as eligible adults in other states? 1b. Do eligible adults in key demographic groups who live in states with required monthly payments enroll in Medicaid (or premium assistance programs) at the same rate that eligible adults in other states do? 1d. How do monthly payment amounts affect take-up of coverage?
2. What effects do monthly payments appear to have on continuity of coverage? 2a. Is there a relationship between midyear disenrollments and the timing of monthly payment policies? 2b. Is there a relationship between monthly payment requirements and renewals? 2c. What is the effect of payment enforcement rules such as non-eligibility periods before re-enrollment? 2d. Is there a relationship between monthly payment requirements and long-term enrollment continuity? 2e. Is there a relationship between monthly payment requirements and enrollment duration?
Domain 3: Beneficiary engagement programs to encourage health behaviors
1. What strategies are states using to educate beneficiaries about preferred health behaviors? 1a. What strategies are states using to explain incentives and disincentives? Which strategies are perceived to be effective?
2. To what extent are Medicaid enrollees responsive to explicit behavior incentives?
3. Do behavior incentives affect overall access to and use of care? 3a. Do behavior incentives yield gains in preventive care and chronic condition management? 3b. Do behavior incentives yield reductions in disincentivized care (that is, non-emergent ED visits)? 3c. How do behavior incentives affect volume of and access to care?
4. Are population-level effects observed from Medicaid demonstration policies?

Note: There is no question 1B in Domain 1, and no question 1C in Domain 2, because those research questions, initially planned in 2015, were dropped for insufficient data.

1. Non-regression approaches

We conducted descriptive analyses of administrative, survey, and qualitative data to answer questions in all three domains. Most of these analyses focus on adults who were enrolled in Medicaid after the expansion or demonstration was implemented. For example, we used administrative data to assess whether open enrollment periods appeared to influence Medicaid take-up among beneficiaries in premium assistance states (Domain 1). We also used administrative data to assess the proportion of adult expansion beneficiaries who disenrolled midyear in their first enrollment year (Domain 2) and who received credit for completing incentivized healthy behaviors (Domain 3). We used three types of data sources for our descriptive analyses.

Analysis of take-up rates. We combined administrative data with IPUMS-ACS data to construct Medicaid take-up rates, which we used to address research questions in Domains 1 and 2. Take-up rates are estimates of the proportion of people who were eligible under the expansion and enrolled in Medicaid in each state.

- We used Medicaid administrative data to create numerators, which are the number of non-disabled, non-dual–eligible adults ages 19 to 64 who were enrolled in Medicaid during each month in a year, averaged across the number of months for which administrative data were available for that year. People who enrolled in limited benefits programs such as family planning benefit plans or in non-relevant waiver programs like 1915(c) were excluded. The first three post-expansion months were excluded to better reflect steady-state enrollment. For example, 2014 estimates for states that expanded in January 2014 (Arkansas, Iowa, Ohio, Oregon, Washington, and West Virginia) exclude enrollment data from January through March 2014, to allow for a ramp-up period.
- We used post-expansion eligibility guidelines and IPUMS-ACS data from 2012 to 2017, the most recent year available at the time of analysis, to estimate denominators—the population eligible for Medicaid in each state and year. The likely eligible population includes adults ages 19 through 64 who are citizens or likely eligible non-citizens, have an estimated modified adjusted gross income (MAGI) below 138 percent of the FPL,[14] and do not receive Supplemental Security Income (SSI), because SSI would indicate a disability that would qualify them for Medicaid under a different eligibility category. To identify non-citizens who are likely eligible for Medicaid, we applied an algorithm developed by George Borjas to impute the immigration status of non-citizens at the individual level in microdata series such as the ACS (Borjas 2017a; 2017b). We estimated individuals' FPL by constructing health insurance units because family income underestimates the number of adults likely eligible for Medicaid (SHADAC 2012). The health insurance unit is different from the Census Bureau's definitions of household or family. A household includes all individuals who are living together, some of whom may not be related, whereas a family includes all related members of a household.

  People who reported employer-sponsored coverage or other forms of insurance coverage are included in the likely eligible population because the proportion of low-income workers who are offered health insurance coverage by their employers has declined over time (Long et al. 2016)—possibly as employers have responded to the availability of Medicaid for their employees in states that expanded coverage—or for reasons such as general management of overhead costs as insurance premiums have continued to rise. We used these estimates of the population eligible for Medicaid as denominators for analyses of take-up rates and for the IPUMS-ACS regressions described below.

[14] The threshold 138 percent of the FPL reflects eligibility expansion up to 133 percent of the FPL, plus a 5 percent income disregard under modified adjusted gross income (MAGI) calculation rules.

Descriptive analyses of HIP 2.0 data. We used HIP 2.0 data provided by Indiana to address research questions in Domains 2 and 3. For Domain 2, we used HIP 2.0 data to analyze midyear disenrollments among adults in their first, second, and third enrollment spans (defined as 2 to 12 months of consecutive enrollment, measured only where it was possible to observe 12 potential months of enrollment). This analysis compares midyear disenrollments caused by nonpayment of beneficiary account contributions to midyear disenrollments for other reasons, within each observed span. We also examined gaps in enrollment, including gaps for people who were disenrolled for nonpayment. For Domain 3, we assessed the number of 12-month spans with records for rewards for completing incentivized behaviors, which took the form of rollovers of beneficiary account balances to the next plan year. We included information on beneficiary account balances and use of preventive care because only enrollees who (1) had a positive beneficiary account balance at the end of the span and (2) received qualifying preventive care were eligible for these rollover rewards.

Qualitative analyses. Third, we used qualitative data to conduct several descriptive analyses for Domains 1 and 3. For Domain 1, we synthesized state-reported metrics to assess provider participation and the unmet need for medical care, and we analyzed Marketplace and Medicaid data on plan participation to assess the potential for continuity of coverage between Medicaid and Marketplace coverage in states with premium assistance demonstrations. For Domain 3, we analyzed state evaluation reports and findings from survey data to assess states' approaches to beneficiary education and beneficiaries' use of health accounts.

2. Regression models

We also estimated regression models to answer research questions in all three domains. In most cases, these regression models provide descriptive information on the associations between demonstration policies and key outcomes. Because we often lacked data on outcomes before states implemented demonstrations, we were unable to conduct analyses that would allow us to draw definitive conclusions about the effects of demonstration policies. In some cases, however, such as analyses based on national survey data, we could compare differences in outcomes between demonstration and comparison states both before and after demonstration implementation (difference-in-differences analysis), which yielded stronger evidence on the effects of demonstration policies.

Throughout, we present results in terms of average marginal effects, which are easier to interpret than estimated regression parameters. For logistic models, which we used to analyze binary outcome variables, the average marginal effect is the difference in the estimated probability of the outcome (in percentage points) between the demonstration and comparison groups. For negative binomial models, which we used to analyze outcome variables that were counts of services, the average marginal effect is the average difference in the estimated count between the demonstration and comparison groups. For ordinary least squares, which we used to analyze continuous outcome variables, the average marginal effect and the regression parameter are equivalent and are interpreted as the average difference in the outcome variable between the

demonstration and comparison groups. For cross-sectional analyses, we computed the average marginal effect by calculating the estimated outcome for each member of the demonstration group, first holding the demonstration variable equal to 0 (that is, estimating the outcome for each demonstration group beneficiary as if he or she were in the comparison group), then holding the demonstration variable equal to 1 and subtracting the difference between the estimated outcomes. The average of these differences across individuals is the average marginal effect. For difference-in-differences analyses, the calculation also includes the difference between pre- and post-expansion observations.

Domain 1. Descriptive models based on administrative data allowed us to examine relative use of services by beneficiaries enrolled in states with active QHP demonstrations compared to use by those enrolled in traditional Medicaid expansions. We also conducted descriptive analyses of Medicaid expenditures,[15] but included a difference-in-differences analysis estimating the effect of premium assistance in New Hampshire, which is the only demonstration state where we have reliable expenditure data both before and after it implemented its demonstration. Regression analyses using administrative data in this domain were restricted to adult expansion beneficiaries in each included state and controlled for sex, age, Chronic Illness and Disability Payment System (CDPS) score (a proxy for overall health status), and rural residence. More information on the specific models we used, as well as descriptive statistics, are in Appendix C.

Using national survey data (BRFSS) from 2012 through 2017, we conducted difference-in-differences regression models estimating the effect of being in a state with an active premium assistance demonstration on the self-reported unmet need for medical care. Key outcomes included whether respondents reported they had had a checkup in the past year, had a personal doctor or health care provider, or had unmet need because of medical costs. We limited the sample to adults ages 18 to 64 who reported an annual household income of less than $35,000 per year. Models controlled for age, sex, race/ethnicity, education, marital status, employment status, income, and indicators for disability and the presence of a child in the household.[16] Appendix D contains model specifications and descriptive statistics.

Domain 2. We employed a descriptive regression framework to analyze the effect of monthly payment policies on the probability of continued enrollment among adult expansion beneficiaries in their first, second, or third enrollment span (again defined as 2 to 12 months of consecutive enrollment, measured only where it was possible to observe 12 consecutive potential months of enrollment). We conducted a similar analysis of the probability of renewals after 12-month enrollment spans among adult expansion beneficiaries in states with and without monthly payment requirements. We also conducted a survival analysis using an accelerated failure time model. This model estimates the effect of monthly payments on the duration of enrollment.

[15] Medicaid expenditures included spending on direct medical services and capitation (or premium) payments.

[16] We created a proxy measure of disability, in which respondents were considered disabled if they indicated they were limited in their activities, used special equipment, were blind or had cognitive limitations, or had difficulty walking or climbing stairs, dressing or bathing, or doing errands alone.

Appendix C contains model specifications and descriptive statistics for Domain 2 analyses based on administrative data.

We also used IPUMS-ACS data to estimate pooled cross-sectional time series models with fixed effects for states and years to assess the relationship between monthly payment requirements and self-reported Medicaid enrollment among the likely eligible population. These models control for age, sex, race, Hispanic/Latino ethnicity, education level, employment status, presence of children in the household, and disability status. We estimated similar models that were segmented by demographic characteristics to assess whether effects persist for different groups (Domain 2). All models of survey data include data for the years 2012 to 2017. Appendix D contains model specifications, information on sensitivity tests, and descriptive statistics for analyses using IPUMS-ACS data.

Domain 3. We used descriptive regression models based on administrative data to estimate whether financial incentives encouraged Medicaid beneficiaries to receive preventive care, manage chronic conditions, and reduce use of inefficient care such as the emergency department for non-emergencies. As with Domain 1, regression analyses using administrative data in this domain were restricted to adult expansion beneficiaries in each included state, and we controlled for sex, age, CDPS score (a proxy for overall health status), and rural residence. More information on the specific models that were used, as well as descriptive statistics, are included in Appendix C.

We used BRFSS data from 2012 through 2017 to estimate difference-in-differences regression models designed to assess the population-level effects of behavior incentives on smoking cessation, physical activity, receipt of flu shots, having a checkup in the past year, measures of chronic condition care (such as blood pressure medication, cholesterol checks, and diabetes care), and receipt of cancer screenings. Diabetes care measures are from a diabetes module that is an option for states to include, cancer screening measures are from a mix of core set and optional cancer screening modules, and all others are from the BRFSS core set. We applied the same sample limitations and used the same control variables as in Domain 1.[17] Appendix D contains model specifications and descriptive statistics.

[17] Models for breast cancer screening, Pap tests, and prostate cancer screening omit sex as a control variable because only respondents of the relevant sex were asked about the applicable screenings.

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III. DOMAIN 1: MANDATORY MEDICAID-SUPPORTED QHP ENROLLMENT (PREMIUM ASSISTANCE)

Highlights of Domain 1 findings

- Three demonstration states—Arkansas, Iowa, and New Hampshire—expanded Medicaid coverage for adults and used a premium assistance model. After controlling for demographic and health characteristics, analyses of administrative data revealed that beneficiaries in Iowa and New Hampshire had more physician office visits than beneficiaries in comparison states did. Beneficiaries in Arkansas had fewer visits, but because of data limitations, we suggest caution in interpreting this finding.
- The use of prescription drugs and wraparound services was not systematically higher or lower in states with active demonstrations than in comparison states.
- Analyses of survey data revealed that during the demonstration periods, the percentage of people with low incomes who reported receiving a check-up in the previous year (Arkansas and Iowa) and the percentage with a personal provider (New Hampshire) were higher than they were in states that implemented traditional Medicaid expansions. Unmet need for care specifically due to cost declined in both demonstration and comparison states after expansions.
- Arkansas's and New Hampshire's Marketplace issuers were required to offer QHPs to premium assistance beneficiaries, which made it more likely that beneficiaries could stay enrolled with the same issuer if they lost Medicaid eligibility and transitioned to the Marketplace. Iowa did not have this requirement, and fewer Marketplace issuers participated in the premium assistance model. As context, overlap between Marketplace and Medicaid Managed Care issuers in comparison states ranged from 0 to 67 percent during the period from 2014 through 2017.
- Analyses of total Medicaid per-member per-month (PMPM) expenditures suggest that QHP coverage was more expensive than Medicaid coverage in New Hampshire. QHP coverage was less expensive in Iowa, but because of data limitations, we recommend caution in interpreting this finding.
- Data from 2012 through 2017 revealed that enrollment patterns during open enrollment periods in Arkansas, Iowa, and New Hampshire were similar to those in other states expanding Medicaid during.

Three states—Arkansas, Iowa, and New Hampshire—supported adult expansion beneficiaries' enrollment in qualified health plans (QHPs) available in the Marketplace established by the Affordable Care Act. States chose to implement these demonstrations because they believed enrolling adults in QHPs offered advantages over traditional Medicaid coverage provided on a fee-for-service basis or through Medicaid managed care. For example, premium assistance might give people better access to health services than traditional Medicaid coverage does and could eliminate some disruptions in coverage if adults transition from Medicaid to Marketplace coverage and are able to keep the same insurer and provider network.

To assess the experiences of beneficiaries and the performance of the demonstrations, we sought to answer the following research questions:

1. How do states supporting QHP enrollment for newly eligible beneficiaries compare with other Medicaid expansion states in terms of access and health outcomes?

2. How do states supporting QHP enrollment compare with other Medicaid expansion states in terms of total spending?
3. How do states supporting QHP enrollment compare with other Medicaid expansion states in terms of take-up rates?

We drew on multiple data sources to address these research questions: (1) administrative data reported by states to the Centers for Medicare & Medicaid Services; (2) administrative data from the Arkansas and New Hampshire All-Payer Claims Databases (APCDs) (which include service encounters for beneficiaries enrolled in QHPs); (3) Iowa administrative data provided by the state; (4) information on plan participation in the Marketplace and in Medicaid; (5) state evaluation reports; (6) Behavioral Risk Factor Surveillance System (BRFSS) data; and (7) Integrated Public Use Microdata Sample (IPUMS) data from the American Community Survey (ACS).

We also used several analytic strategies, with different analysis periods and different comparison states.[18] We conducted regression analyses using administrative data and BRFSS data to examine access and health outcomes and total Medicaid spending. We constructed take-up rates using administrative data and IPUMS-ACS data to assess enrollment in premium assistance demonstrations, and we used administrative enrollment data for descriptive analyses of the timing of enrollment in relation to Marketplace open enrollment periods. We synthesized information from state evaluation reports and data on plan participation. Appendix Table A.1 summarizes the analytic approach and data source for each primary and subsidiary research question.

In the following sections of this chapter, we describe key design features in premium assistance demonstrations (Section A), summarize findings, limitations, and relevant literature for each of the three primary research questions (Sections B–D), and discuss the results and synthesize findings on the research questions (Section E).

A. Key design features in premium assistance demonstrations

Arkansas, Iowa, and New Hampshire used section 1115 authority to support beneficiaries' purchase of coverage from QHPs available in the Marketplace.[19] States with premium assistance

[18] Demonstration periods were 2014–2017 for Arkansas, 2014–2015 for Iowa, and 2016–2017 for New Hampshire. Utilization and expenditures analyses cover all post-expansion months from January 2014–December 2017. The enrollment analysis covers the period from 2012–2017. Analyses of administrative data include up to five comparison states. Analyses of survey data include nine comparison states and are focused on the period from 2012 to 2017.

[19] Throughout this report, we use the terms "qualified health plan" and "QHP" to denote the plans that Medicaid beneficiaries can enroll in under premium assistance demonstrations. These premium assistance QHPs are technically off-Marketplace products that are exact duplicates of Marketplace QHPs except for their higher actuarial value (94 or 100 percent). Medicaid beneficiaries cannot buy regular QHPs in the Marketplace, and consumers who are not Medicaid beneficiaries may not apply tax credits to obtain the QHP lookalikes that are available through the Medicaid premium assistance programs.

demonstrations covered the premium payments to QHP issuers and other cost-sharing for eligible adults. Beneficiaries accessed most of their care through provider networks maintained by the QHPs and could also access mandatory "wraparound" benefits through Medicaid providers who were reimbursed on a fee-for-service basis. (See Bradley and Colby [2017] for a discussion of wraparound benefits in Marketplace-focused premium assistance programs.) All premium assistance states offered family planning services from out-of-network providers and Early and Periodic Screening, Diagnostic, and Treatment services for 19- and 20-year-olds on a wraparound basis. States also covered non-emergency medical transportation, dental, and vision services on a wraparound basis, although states varied in their coverage of these benefits, as described below. Appendix B summarizes policy designs in demonstration states.

Arkansas. Arkansas implemented its premium assistance program in January 2014 under the Health Care Independence Program (Private Option) demonstration. The state extended its demonstration for the period 2017 through 2021, including the premium assistance program, under the name Arkansas Works. In both demonstration periods, the state mandated participation in premium assistance for those insurers who sell plans in the Marketplace. The analyses in this summative report focus on enrollment during the period when the Private Option was in effect (2014–2016) and on the first year of Arkansas Works (2017).

Enrollment in the Private Option and Arkansas Works was mandatory for adult expansion beneficiaries with incomes up to and including 133 percent of the FPL, except for those who were medically frail, pregnant, or American Indians or Alaska Natives. Arkansas provided non-emergency medical transportation on a wraparound basis to Private Option beneficiaries enrolled in QHPs, as well as family planning and Early and Periodic Screening, Diagnostic, and Treatment services. However, the state received federal permission to implement a prior authorization policy for non-emergency medical transportation services.[20] Dental and vision services were not covered as a wraparound benefit, and only a few of the Arkansas QHPs available in 2014 provided such coverage.

Iowa. Iowa's premium assistance demonstration, Marketplace Choice, operated between January 2014 and December 2015. The Iowa Insurance Division allowed issuers to make their own decisions about whether to take on the risk of participating in premium assistance, because regulators did not want the state to be liable for issuer insolvency (see Bradley and Wagnerman 2017). Only two Marketplace issuers participated in premium assistance in the first year of the demonstration. One of these became insolvent in late 2014, and the other stopped accepting new Medicaid beneficiaries in 2015, effectively ending the premium assistance demonstration. The state received approval in January 2016 to modify eligibility for the other component of its 1115 demonstration, the Iowa Wellness Plan, to include the population formerly enrolled in premium assistance.

[20] After eight trip legs (transportation between two stops), beneficiaries are required to call the state's utilization review vendor to be authorized for another set of NEMT trips to obtain health care services.

While Marketplace Choice was operational, enrollment was mandatory for adults with incomes above 100 percent and up to 133 percent of the FPL—except those who were medically frail or those who identified as American Indians or Alaska Natives. Pregnant women could opt into traditional Medicaid instead of QHPs. Those with access to cost-effective employer-sponsored insurance were not eligible. Iowa received a waiver of the requirement to offer non-emergency medical transportation on a wraparound basis, except for medically frail beneficiaries, but did provide dental services to QHP enrollees through a separate plan called the Dental Wellness Plan. All beneficiaries received a set of core dental benefits. Those who completed an initial dental exam and a follow-up visit within 6 to 12 months could receive enhanced benefits, such as restorations and root canals. If beneficiaries completed a second follow-up dental visit, they could receive additional benefits, such as crowns and tooth replacements. Like other premium assistance states, Iowa also provided family planning and Early and Periodic Screening, Diagnostic, and Treatment services on a wraparound basis.

New Hampshire. New Hampshire implemented its Premium Assistance Program as part of the New Hampshire Health Protection Program demonstration, with coverage in QHPs beginning in January 2016. The state mandated participation in premium assistance for those insurers who sold plans on the health insurance exchange. Enrollment in the Premium Assistance Program was mandatory for expansion adults with incomes through 133 percent of the FPL, except for those who were medically frail. American Indians, Alaska Natives, and pregnant women could opt out of premium assistance and into traditional Medicaid coverage. The state planned to exclude individuals with access to cost-effective employer-sponsored insurance from the demonstration but did not implement this exclusion because few people had offers of such insurance. New Hampshire provided limited adult dental and vision services on a wraparound basis, in addition to family planning, non-emergency medical transport, and Early and Periodic Screening, Diagnostic, and Treatment benefits.

B. Access to care and health outcomes for beneficiaries in premium assistance demonstrations

To compare the states supporting QHP enrollment for newly eligible adults to other Medicaid expansion states in terms of access to care and health outcomes, we considered the following three research questions (listed as Questions 1a, 1c, and 1d in Appendix Table A.1).

- Are beneficiaries enrolled in QHPs able to access care at similar or better rates compared to beneficiaries enrolled in traditional Medicaid expansions?
- What is the unmet need for medical care?
- Is there continuity of coverage when switching between Medicaid and Marketplace coverage?

We analyzed Medicaid administrative data (Medicaid Analytic eXtract [MAX] files, Alpha-MAX, and TMSIS Analytic File [TAF]), APCD data for Arkansas and New Hampshire, and

state data obtained directly from Iowa Medicaid Enterprise to answer the first question, considering outcomes in 2014 through 2017. To answer the second question, we synthesized state evaluation reports and analyzed BRFSS data from 2012 through 2017. Finally, we analyzed lists of Marketplace and Medicaid issuers from 2014 through 2017 to answer the third question.

1. Access to care: Utilization and timeliness of physician office visits, prescriptions, vision, family planning, and non-emergency medical transportation services

One reason demonstration states chose to implement premium assistance is because they believed higher levels of physician reimbursement under QHPs would give beneficiaries better access to care than traditional Medicaid coverage would. We therefore expected that beneficiaries in states with premium assistance demonstrations would have physician office visits and prescriptions faster than beneficiaries in other expansion states. We did not have the same expectation for wraparound services, however, because they were provided as a fee-for-service Medicaid benefit to beneficiaries in both demonstration and comparison states. We expected to find less use of non-emergency medical transportation services by QHP beneficiaries in Arkansas and Iowa relative to expansion beneficiaries in comparison states: Arkansas had a prior authorization requirement for non-emergency medical transport, and Iowa had a waiver of the mandatory benefit for non-emergency medical transportation, although Iowa's waiver also applied to non-medically frail expansion beneficiaries who were not enrolled in QHPs. Because New Hampshire provided non-emergency medical transportation services as a wraparound benefit paid directly by the state's Medicaid program and had no prior authorization requirement, we expected use patterns for non-emergency medical transportation in New Hampshire to be similar to those in comparison states.

a. Regression analysis of service utilization

To assess whether there was differential access to services covered by QHPs in premium assistance demonstrations—or to wraparound services—we conducted regression analyses of service use outcomes, controlling for beneficiary characteristics. Specifically, we assessed the percentage of beneficiaries receiving any of the following services within the first 2, 6, or 12 months (depending on the service) after initial enrollment: physician office visits, prescription fills, vision services, dental services, family planning, and non-emergency medical transportation. The five comparison states for our analysis were Kentucky, New Mexico, Ohio, Pennsylvania, and West Virginia. New Mexico was excluded from regression analyses with prescription drug outcomes because due to concerns with data quality. (See Appendix C for details on how the measures were calculated, and Appendix G for per-member per-month [PMPM] service use.)

We used two regression models to estimate the effect of premium assistance demonstrations on beneficiaries' use of services: (1) a difference-in-differences (DD) model and (2) a cross-sectional model. The DD model exploits variation in the timing of the premium assistance programs in Iowa and New Hampshire. The disadvantage of the DD model is that we could not include Arkansas as a demonstration state. Arkansas enrolled expansion beneficiaries in QHPs

throughout the time period we studied (2014–2017), and therefore there was no period during which all Arkansas expansion beneficiaries were enrolled in traditional Medicaid. We also conducted a cross-sectional regression analysis of utilization in all three demonstration states, controlling for observable beneficiary characteristics available in the data. The estimated difference in the utilization rate for each service in Arkansas, Iowa, and New Hampshire is reflected in the average marginal effect in both the DD and cross-sectional regressions (Tables III.1 and III.2). We defined the demonstration group as all beneficiaries in demonstration states during active premium assistance demonstrations, including those in QHPs and those in traditional Medicaid coverage, because we wanted to avoid potential selection bias. Beneficiaries deemed "medically frail" and those who are pregnant are either exempt from QHP enrollment or have the option to enroll in traditional Medicaid coverage. The comparison group included (1) all beneficiaries in comparison states and (2) beneficiaries in Iowa and New Hampshire when their demonstrations were not active (Iowa 2016–2017 and New Hampshire 2014–2015). Appendix C has the full model specifications and sample characteristics.

We begin with results on access to physician office visits and prescription drug fill rates, and then turn to wraparound benefits covered on a fee-for-service basis by Medicaid.

Physician office visits. In our DD analysis, we found that the probability of having any physician office visit—captured by the estimated average marginal effect of the demonstration (Table III.1)—was higher for beneficiaries in a state with a premium assistance demonstration than for beneficiaries in a state without one. As early as two months into their enrollment spans, beneficiaries in Iowa had a higher probability of a physician office visit (by 17.7 percentage points over those in comparison states) and maintained a higher probability of having such a visit at the other two milestones we studied (6 months and 12 months). By 12 months, comparison state beneficiaries had caught up somewhat, and the difference was down to 8.8 percentage points, which is still considerable.[21] Beneficiaries in New Hampshire likewise had higher rates of physician office visits than beneficiaries in comparison states, although the difference appeared only after 6 months of enrollment (3.1 percentage points). Beneficiaries in both Iowa and New Hampshire also had higher average counts of physician office visits by 6 and 12 months (Table III.2).

In cross-sectional analyses, we found similar results for Iowa and New Hampshire—both states' beneficiaries had higher probabilities of physician office visits than beneficiaries in other states, although beneficiaries in New Hampshire had lower rates of use early on (at 2 months). In Arkansas, on the other hand, beneficiaries were 11.0 percentage points less likely to have any physician office visit within 2, 6, or 12 months. Despite these lower probabilities of having any physician office visit in all three observation windows, Arkansas beneficiaries had, on average, 0.7 more physician office visits within 12 months than beneficiaries in comparison states did. It

[21] The higher probability of physician office visits in Iowa could be partly attributable to the beneficiary engagement component of Iowa's demonstration, which used a financial incentive to encourage beneficiaries to see a physician for a wellness visit. See Chapter V for more information on the beneficiary engagement component of Iowa's demonstration.

is possible that the Arkansas result reflects improved access for the sickest patients (those with the highest number of physician office visits), despite fewer beneficiaries visiting a physician overall. In addition, because Arkansas could not be included in the DD model, some of the estimated association could be the result of underlying variation in health care utilization patterns across states that is not accounted for by control variables in the regression.

Prescription drug fill rates. We found mixed results for prescription drug fill rates in demonstration states relative to comparison states. Using our DD model, we found that beneficiaries in Iowa were 6.2 percentage points more likely than those in comparison states to fill any prescriptions within 2 months, but 1 percentage point less likely to fill any prescriptions within 12 months. The relatively high rate of prescription drug fills within 2 months in the state is likely related to the relatively high rate of physician office visits early on. New Hampshire beneficiaries, on the other hand, were 7.5 percentage points less likely than comparison state beneficiaries to have any prescriptions filled within 2 months, and at no point in the span were they more likely than comparison state beneficiaries to fill a prescription. Despite lower rates of any prescription drug fills at 12 months, Iowa had higher average counts of prescriptions filled (3.85 more within 12 months). New Hampshire beneficiaries had, on average, 3.75 fewer prescriptions within 12 months (Table III.1).

Using our cross-sectional analysis, we found that Iowa beneficiaries had a consistently higher likelihood of filling a prescription than comparison state beneficiaries at all span lengths, and New Hampshire beneficiaries had a consistently lower likelihood. In Arkansas, we found that beneficiaries were 6 percentage points less likely to have any prescription filled within a 12-month span and found similar results for shorter spans. Beneficiaries in Arkansas had an average of 4.5 fewer prescriptions over 12-month spans than beneficiaries in comparison states (Table III.2).

Table III.1. Difference-in-differences regression results for physician office visits and prescription drug fills

Outcome variable	Comparison group mean	Average marginal effect of demo in Iowa	Standard error	*p*-value	Percent change	Average marginal effect of demo in New Hampshire	Standard error	*p*-value	Percent change	N
Any use of services (percent)										
Physician office visit										
Within 2 months	42.59	17.69***	0.16	.000	41.5	0.24	0.34	.482	0.6	5,909,476
Within 6 months	58.90	15.47***	0.15	.000	26.3	3.14***	0.32	.000	5.3	5,914,067
Within 12 months	73.08	8.77***	0.12	.000	12.0	4.10***	0.25	.000	5.6	5,914,067
Any prescription										
Within 2 months	47.03	6.16***	0.15	.000	13.1	-7.54***	0.32	.000	-16.0	5,288,664
Within 6 months	61.24	2.61***	0.14	.000	4.3	-7.07***	0.32	.000	-11.5	5,288,664
Within 12 months	73.93	-0.78***	0.13	.000	-1.1	-1.91***	0.27	.000	-2.6	5,288,664
Number of services or encounters										
Physician office visits										
6 months	2.67	2.76***	2.13	.000	103.3	1.09***	4.19	.000	40.6	5,914,067
12 months	5.44	4.98***	3.83	.000	91.6	3.40***	8.93	.000	62.4	5,914,067
Prescriptions										
6 months	8.93	2.62***	5.56	.000	29.4	-2.60***	9.87	.000	-29.1	5,288,664
12 months	19.64	3.85***	10.89	.000	19.6	-3.75***	21.41	.000	-19.1	5,288,664

Source: Mathematica analysis of administrative data from 2014–2017 for Iowa and New Hampshire (demonstration states); and Kentucky, New Mexico, Ohio, Pennsylvania, and West Virginia (comparison states). Years included for each state depend on the date of demonstration implementation or coverage expansion and on data availability.

Notes: Arkansas was excluded from the DD model because the premium assistance demonstration was in effect for all four years of the study, 2014–2017. New Mexico was excluded from all regressions involving prescription drug use because of data issues with the prescription drug file. We controlled for beneficiaries' individual characteristics (sex, age, living in a rural location, and, where possible, CDPS score). CDPS score was only created for, and included in, the 12-month models.

We report results for regression analyses where the demonstration group includes all beneficiaries in demonstration states during active premium assistance demonstrations. We report analyses that are restricted to QHP beneficiaries in Appendix Table G.4.

Marginal effects were estimated using logistic and negative binomial regressions. We calculated the average of the estimated difference in outcomes, using the covariate distribution of the demonstration group.

The comparison group mean is the unadjusted comparison sample mean, presented for reference. The average marginal effect should not be added to the comparison group mean to calculate use by enrollees in demonstration states during active demonstrations.

**Significantly different from zero at the .05 level, two-tailed test.

***Significantly different from zero at the .01 level, two-tailed test.

CDPS = Chronic Illness and Disability Payment System: DD = difference-in-differences; NEMT = non-emergency medical transportation.

Table III.2. Cross-sectional regression results for physician office visits and prescription drug use

Outcome variable	Comparison group mean	Average marginal effect of demo in Arkansas	Standard error	*p*-value	Percent change	Average marginal effect of demo in Iowa	Standard error	*p*-value	Percent change	Average marginal effect of demo in New Hampshire	Standard error	*p*-value	Percent change	N
Any use of services (percent)														
Physician office visit														
Within 2 months	42.58	-11.01***	0.06	.000	-25.8	16.17***	0.00	.000	38.0	-1.23***	0.17	.000	-2.9	6,849,039
Within 6 months	58.91	-12.43***	0.07	.000	-21.1	16.58***	0.00	.000	28.1	2.36***	0.17	.000	4.0	6,854,400
Within 12 months	73.08	-11.36***	0.07	.000	-15.5	11.69***	0.00	.000	16.0	2.28***	0.15	.000	3.1	6,854,400
Any prescription														
Within 2 months	47.03	-4.32***	0.06	.000	-9.2	4.54***	0.00	.000	9.7	-6.42***	0.18	.000	-13.7	6,228,997
Within 6 months	61.24	-5.37***	0.06	.000	-8.8	4.38***	0.00	.000	7.2	-3.83***	0.18	.000	-6.2	6,228,997
Within 12 months	73.93	-6.03***	0.06	.000	-8.2	1.52***	0.00	.000	2.1	-3.24***	0.16	.000	-4.4	6,228,997
Number of services or encounters														
Physician office visits														
6 months	2.67	-0.11***	0.01	.000	-4.2	2.78***	0.02	.000	103.9	0.75***	0.03	.000	28.2	6,854,400
12 months	5.44	0.67***	0.02	.000	12.3	5.07***	0.03	.000	93.2	2.34***	0.06	.000	43.0	6,854,400
Prescriptions														
6 months	8.93	-2.01***	0.03	.000	-22.5	1.56***	0.05	.000	17.5	-1.66***	0.08	.000	-18.6	6,228,997
12 months	19.64	-4.47***	0.06	.000	-22.7	1.89***	0.11	.000	9.6	-2.63***	0.17	.000	-13.4	6,228,997

Source: Mathematica analysis of administrative data from 2014–2017 for Arkansas, Iowa, and New Hampshire (demonstration states); and Kentucky, New Mexico, Ohio, Pennsylvania, and West Virginia (comparison states). Years included for each state depend on the date of demonstration implementation or coverage expansion and on data availability.

Notes: New Mexico was excluded from all regressions involving prescription drug use because of data issues with the prescription drug file. We controlled for beneficiaries' individual characteristics (sex, age, living in a rural location, and, where possible, CDPS score). CDPS score was only created for, and included in, the 12-month models.

We report results for regression analyses where the demonstration group includes all beneficiaries in demonstration states during active premium assistance demonstrations. We report analyses that are restricted to QHP beneficiaries in Appendix Table G.5.

Marginal effects were estimated using logistic and negative binomial regressions. We calculated the average of the estimated difference in outcomes, using the covariate distribution of the demonstration group.

The comparison group mean is the unadjusted comparison sample mean, presented for reference. The average marginal effect should not be added to the comparison group mean to calculate use by enrollees in demonstration states during active demonstrations.

**Significantly different from zero at the .05 level, two-tailed test.

***Significantly different from zero at the .01 level, two-tailed test.

CDPS = Chronic Illness and Disability Payment System: NEMT = non-emergency medical transportation.

We next examined use of wraparound services—that is, services covered by Medicaid on a fee-for-service basis to beneficiaries enrolled in QHPs. We found mixed results; beneficiaries in states with an active premium assistance demonstration were not systematically more or less likely to use wraparound services than beneficiaries in comparison states were. We did not expect to find systematically different use of wraparound services, because these services were financed by fee-for-service Medicaid in all states.

Using our DD approach, we found that beneficiaries in Iowa during its premium assistance demonstration were more likely to use vision and family planning services and less likely to use dental services and non-emergency medical transportation than beneficiaries in comparison states were (Table III.3). In contrast, beneficiaries in New Hampshire during its demonstration were more likely to use non-emergency medical transportation services and less likely to use all other services categories we examined. Results for counts of services followed the same pattern.

Our cross-sectional approach yielded slightly different results. In Iowa, we found use of dental services increased instead of decreasing, and in New Hampshire we found results that went in opposite directions from the DD analysis for use of family planning and non-emergency medical transportation. In Arkansas, we found that beneficiaries had lower rates of utilization for all wraparound services than comparison state beneficiaries did (Table III.4).

The disparity between our DD and cross-sectional regression results is likely attributable to state-level variation that we could not control for in the regressions. For example, the demonstration states had substantial differences in dental coverage. During the evaluation period, New Hampshire Medicaid only covered dental services in cases of extreme pain or infection. In Iowa, however, all expansion beneficiaries were covered by the state's Dental Wellness Plan, which provided basic coverage for all beneficiaries and enhanced coverage for those who received regular dental care. The Dental Wellness Plan continued in 2016 and 2017, after premium assistance was discontinued in the state. It is likely, therefore, that the differences between rates of dental use in these states and the comparison states, as measured in cross-sectional analyses, are unrelated to premium assistance. Similarly, Iowa's waiver for non-emergency medical transportation was in effect both during and after the state's premium assistance demonstration, which can be seen in large cross-sectional differences in NEMT utilization between Iowa and the comparison group which do not appear in the DD analysis. When each state serves as its own baseline (in DD analysis), we isolate the effect of premium assistance, which is small relative to the influence of these other policies.

Table III.3. Difference-in-differences regression results for utilization of wraparound benefits

Outcome variable	Comparison group mean	Average marginal effect of demo in Iowa	Standard error	*p*-value	Percent change	Average marginal effect of demo in New Hampshire	Standard error	*p*-value	Percent change	N
Any use of services (percent)										
Any vision service										
Within 6 months	11.79	0.54***	0.11	.000	4.6	-0.78***	0.25	.001	-6.6	5,914,067
Within 12 months	20.93	1.14***	0.13	.000	5.4	-0.57	0.31	.064	-2.7	5,914,067
Any dental service										
Within 6 months	17.91	-1.86***	0.12	.000	-10.4	-2.45***	0.38	.000	-13.7	5,914,067
Within 12 months	28.51	-3.54***	0.14	.000	-12.4	-2.31***	0.42	.000	-8.1	5,914,067
Any family planning service										
Within 6 months	9.17	1.14***	0.12	.000	12.5	-1.24***	0.24	.000	-13.5	3,825,628
Within 12 months	12.79	0.42***	0.13	.001	3.3	-0.63**	0.28	.023	-5.0	3,825,628
Any NEMT service										
Within 6 months	7.98	-2.33***	0.05	.000	-29.3	5.10***	0.62	.000	63.9	5,914,067
Within 12 months	13.79	-1.61***	0.06	.000	-11.7	10.18***	0.69	.000	73.8	5,914,067
Number of services or encounters										
Vision services										
6 months	0.15	0.01***	0.13	.000	3.4	-0.01***	0.32	.001	-7.3	5,914,067
12 months	0.31	0.02***	0.21	.000	6.2	-0.00	0.52	.582	-0.9	5,914,067
Any dental service										
Within 6 months	0.33	-0.04***	0.27	.000	-12.1	-0.05***	0.95	.000	-14.7	5,914,067
Within 12 months	0.68	-0.13***	0.48	.000	-18.4	-0.11***	1.75	.000	-15.885	5,914,067
Family planning services										
6 months	0.27	0.08***	0.47	.000	30.3	-0.08***	0.79	.000	-29.4	3,825,628
12 months	0.58	0.11***	0.89	.000	19.4	-0.12***	1.51	.000	-19.8	3,825,628
NEMT services										
6 months	0.13	-0.04***	0.09	.000	-33.4	0.09***	1.46	.000	72.4	5,914,067
12 months	0.30	-0.05***	0.18	.000	-16.5	0.19***	2.27	.000	61.6	5,914,067

Source: Mathematica analysis of administrative data from 2014–2017 for Iowa and New Hampshire (demonstration states); and Kentucky, New Mexico, Ohio, Pennsylvania, and West Virginia (comparison states). Years included for each state depend on the date of demonstration implementation or coverage expansion and on data availability.

Table III.3 (*continued*)

Notes: Arkansas was excluded from the DD model because the premium assistance demonstration was in effect for all four years of the study, 2014–2017. We controlled for beneficiaries' individual characteristics (sex, age, living in a rural location, and, where possible, CDPS score). CDPS score was only created for, and included in, the 12-month models.

We report results for regression analyses where the demonstration group includes all beneficiaries in demonstration states during active premium assistance demonstrations. We report analyses that are restricted to QHP beneficiaries in Appendix Table G.4.

Marginal effects were estimated using logistic and negative binomial regressions. We calculated the average of the estimated difference in outcomes, using the covariate distribution of the demonstration group.

The comparison group mean is the unadjusted comparison sample mean, presented for reference. The average marginal effect should not be added to the comparison group mean to calculate use by enrollees in demonstration states during active demonstrations.

**Significantly different from zero at the .05 level, two-tailed test.

***Significantly different from zero at the .01 level, two-tailed test.

CDPS = Chronic Illness and Disability Payment System; NEMT = non-emergency medical transportation.

Table III.4. Cross-sectional regression results for utilization of wraparound benefits

Outcome variable	Comparison group mean	Average marginal effect of demo in Arkansas	Standard error	*p*-value	Percent change	Average marginal effect of demo in Iowa	Standard error	*p*-value	Percent change	Average marginal effect of demo in New Hampshire	Standard error	*p*-value	Percent change	N
Any use of services (percent)														
Any vision service														
Within 6 months	11.79	-3.62***	0.04	.000	-30.7	1.73***	0.00	.000	14.7	-1.73***	0.12	.000	-14.7	6,854,400
Within 12 months	20.93	-6.77***	0.05	.000	-32.4	2.76***	0.00	.000	13.2	-2.59***	0.15	.000	-12.4	6,854,400
Any dental service														
Within 6 months	17.91	-12.46***	0.03	.000	-69.5	5.05***	0.00	.000	28.2	-9.64***	0.11	.000	-53.8	6,854,400
Within 12 months	28.51	-19.92***	0.04	.000	-69.9	6.20***	0.00	.000	21.7	-15.09***	0.14	.000	-52.9	6,854,400
Any family planning service														
Within 6 months	9.17	-1.27***	0.05	.000	-13.8	3.31***	0.00	.000	36.1	1.92***	0.16	.000	21.0	4,440,242
Within 12 months	12.79	-1.13***	0.06	.000	-8.9	2.84***	0.00	.000	22.2	2.26***	0.18	.000	17.7	4,440,242
Any NEMT service														
Within 6 months	7.98	-5.26***	0.02	.000	-65.9	-4.56***	0.00	.000	-57.2	-5.33***	0.06	.000	-66.8	6,854,400
Within 12 months	13.79	-8.51***	0.03	.000	-61.7	-6.72***	0.00	.000	-48.7	-8.09***	0.09	.000	-58.7	6,854,400
Number of services or encounters														
Vision services														
6 months	0.15	-0.05***	0.00	.000	-30.9	0.01***	0.00	.000	7.3	-0.02***	0.00	.000	-14.1	6,854,400
12 months	0.31	-0.10***	0.00	.000	-32.9	0.02***	0.00	.000	5.4	-0.04***	0.00	.000	-11.9	6,854,400
Any dental service														
Within 6 months	0.33	-0.24***	0.00	.000	-71.7	0.10***	0.00	.000	30.0	-0.19***	0.00	.000	-59.0	6,854,400
Within 12 months	0.68	-0.51***	0.00	.000	-74.2	0.26***	0.00	.000	37.9	-0.42***	0.00	.000	-61.4	6,854,400
Family planning services														
6 months	0.27	-0.11***	0.00	.000	-40.1	0.08***	0.00	.000	29.2	0.03***	0.01	.000	12.7	4,440,242
12 months	0.58	-0.26***	0.00	.000	-43.8	0.10***	0.01	.000	17.1	0.09***	0.01	.000	16.0	4,440,242
NEMT services														
6 months	0.13	-0.11***	0.00	.000	-84.6	-0.09***	0.00	.000	-70.2	-0.10***	0.00	.000	-74.1	6,854,400
12 months	0.30	-0.28***	0.00	.000	-92.9	-0.21***	0.00	.000	-69.4	-0.21***	0.00	.000	-69.4	6,854,400

Table III.4 (*continued*)

Source: Mathematica analysis of administrative data from 2014–2017 for Arkansas, Iowa, and New Hampshire (demonstration states); and Kentucky, New Mexico, Ohio, Pennsylvania, and West Virginia (comparison states). Years included for each state depend on the date of demonstration implementation or coverage expansion and on data availability.

Notes: We controlled for beneficiaries' individual characteristics (sex, age, living in a rural location, and, where possible, CDPS score). CDPS score was only created for, and included in, the 12-month models.

We report results for regression analyses where the demonstration group includes all beneficiaries in demonstration states during active premium assistance demonstrations. We report analyses that are restricted to QHP beneficiaries in Appendix Table G.5.

Marginal effects were estimated using logistic and negative binomial regressions. We calculated the average of the estimated difference in outcomes, using the covariate distribution of the demonstration group.

The comparison group mean is the unadjusted comparison sample mean, presented for reference. The average marginal effect should not be added to the comparison group mean to calculate use by enrollees in demonstration states during active demonstrations.

**Significantly different from zero at the .05 level, two-tailed test.

***Significantly different from zero at the .01 level, two-tailed test.

CDPS = Chronic Illness and Disability Payment System; NEMT = non-emergency medical transportation.

b. Limitations of analyses of service utilization

First, cross-sectional regression estimates are potentially biased by unmeasured differences between states that are unrelated to the demonstration. This issue particularly affects Arkansas, because we could only include that state in cross-sectional analyses (as opposed to the more rigorous DD analyses) due to data limitations.

Next, the analysis of service utilization is also limited by our use of data from several different sources. For Iowa, we obtained data directly from the state for 2014 and 2015 and from TAF for 2016 and 2017. Arkansas data for the full study period and for all beneficiaries came from the state's APCD. New Hampshire data on QHP beneficiaries came from the state's APCD, and data for non-QHP beneficiaries came from MAX, Alpha-MAX, and TAF, which were also the sources of data for comparison states. We developed coding procedures to standardize these formats as much as possible, but there are some differences in reporting that might have affected our results.[22] In particular, the variables used to construct the outcomes measures varied depending on whether they were based on APCD data or Medicaid administrative data. We used available information in the APCD to construct close matches to the Medicaid administrative data, but not all variables aligned perfectly.[23]

c. What have other studies found?

Findings from academic studies on premium assistance demonstrations. One of the goals of premium assistance is to ensure equal access to care for Medicaid beneficiaries and privately insured patients. However, published studies of changes in access to care by coverage type report inconsistent findings. A secret shopper study that assessed access to care for traditional Medicaid beneficiaries versus those enrolled in QHPs found greater availability of new patient appointments to primary care practices for QHP beneficiaries in Arkansas and Iowa than for traditional Medicaid beneficiaries. These differences were statistically significant (Basseyn et al. 2016).

Sommers, Blendon, and Orav (2016) surveyed beneficiaries in Arkansas's premium assistance demonstration, Kentucky's traditional Medicaid expansion, and Texas's Medicaid program, which did not expand coverage. They found that expansion was related to a significant increase in outpatient utilization beginning in the second year post-expansion, but that outpatient utilization was similar for both Arkansas (premium assistance demonstration) and Kentucky (traditional Medicaid expansion). A more recent analysis of survey data through the end of 2016 for the same three states had similar results (Sommers et al. 2017).

[22] For example, we used the claim submitter to identify QHP beneficiaries in the Arkansas APCD data. It is possible this method did not capture enrollment in a QHP in the same way that enrollment data do in New Hampshire or in other states.

[23] For example, the APCD data did not have an equivalent for the "type of service" code available in Medicaid administrative data, and APCD data were not organized into outpatient (OT) and inpatient (IP) files as are MAX, Alpha-MAX, and TAF data.

One way that states expected QHPs to improve access was through their higher physician reimbursement rates compared with fee-for-service Medicaid. A recent policy change, the Medicaid "fee bump," increased Medicaid payments to primary care physicians by over 70 percent on average in 2013, presenting an opportunity to test this hypothesis. Two studies revealed that the increase in reimbursement had no detectable effect on provider participation in the Medicaid program (Decker 2018; Mulcahy et al. 2018). However, a recent study of the same policy change was based on more detailed data; it revealed that the fee bump increased access to physicians—as measured by office visits—for Medicaid patients and improved self-reported health (Alexander and Schnell 2019).[24]

Findings from state-based evaluation reports on section 1115 demonstrations. Iowa's state-based interim evaluation examined use of physician services and prescription benefits by beneficiaries enrolled in QHPs. Among members enrolled for at least 11 months in 2014, 76 percent of beneficiaries who were enrolled in QHPs had a preventive/ambulatory care visit, which was less than the 82 percent for other Iowa expansion enrollees and 87 percent for parents who were not in the expansion eligibility group (Damiano et al. 2015). This finding is not consistent with our finding of higher physician office visit rates by Iowa beneficiaries than by beneficiaries in comparison states, but the findings are not directly comparable because the state evaluation did not include a comparison group outside Iowa. After controlling for beneficiary characteristics, evaluators found that Iowa QHP beneficiaries were significantly less likely to have a prescription claim in 2014 than parents enrolled in traditional Medicaid coverage were (Damiano et al. 2015). This corresponds with our finding for those with similar enrollment durations (12 months).

Iowa's state-based evaluators found that those enrolled in a QHP had lower unmet need for non-emergency medical transportation than those enrolled in state plan Medicaid coverage (who had lower incomes than those in the expansion eligibility group) (Damiano et al. 2015). We found that beneficiaries in Iowa during its premium assistance demonstration had lower rates of use of non-emergency medical transportation than expansion beneficiaries in comparison states did, but it is unclear whether lower use corresponds to greater unmet need, because we cannot observe beneficiaries' transportation needs.

Arkansas's state-based summative evaluation of the Private Option revealed that, compared with Medicaid beneficiaries who were not enrolled in a QHP, a higher proportion of QHP beneficiaries had an outpatient visit within 30 and 90 days (Arkansas Center for Health Improvement 2018). Our cross-sectional analysis yielded different results: Arkansans were less likely than those in comparison states to have any physician office visits within 2, 6, or 12 months. Because Arkansas' demonstration was in effect for the entire time period we studied—

[24] The physician fee bump affected all states equally in 2014 and 2015. The only state in our analysis that opted to keep the higher rate after that was New Mexico, one of our comparison states. However, reimbursements in New Mexico changed relatively little as a result of the fee increase, so we opted to consider the state's physician fees as one of the many ways states vary in their Medicaid policy environment, and not control separately for it in the analyses.

2014–2017—we could not use a DD design to account for baseline differences between Arkansas and comparison states in our analysis. Therefore, unobserved differences, such as variation in medical care utilization patterns across states, could account for at least part of the difference in our results. Other independent studies have not supported the state evaluation's finding of higher service utilization among QHP beneficiaries in Arkansas (Sommers, Blendon, and Orav 2016; Sommers et al. 2017).

Arkansas's state-based evaluation found that QHP beneficiaries reported fewer non-emergency transportation issues than Medicaid beneficiaries who were not enrolled in QHPs did. In addition, there were no differences between QHP beneficiaries, and a different within-state comparison group made up of Medicaid beneficiaries who were eligible before the expansion and did not complete a health needs questionnaire (Arkansas Center for Health Improvement 2018). Controlling for beneficiary characteristics, we found that QHP beneficiaries in Arkansas had lower rates of using non-emergency medical transportation than expansion beneficiaries in comparison states.

New Hampshire's state-based summative evaluation did not find statistically significant results on access and was inconclusive as to whether the premium assistance model provided better access than the traditional Medicaid program (Health Services Advisory Group 2019). The evaluation did analyze non-emergency medical transportation, finding that transportation requests were fulfilled at similar rates for QHP and non-QHP beneficiaries. Our DD analysis found that beneficiaries in New Hampshire were more likely than beneficiaries in comparison states to use any non-emergency medical transportation within 6 or 12 months.

2. Unmet need for medical care

Similar to the expectation that premium assistance would increase utilization of services covered by QHPs, higher provider reimbursement under QHPs—and resulting differences in physician participation relative to traditional Medicaid coverage—could be expected to reduce unmet need for care. Beneficiaries might perceive an unmet need for care despite receiving services if, for example, there is a long wait for appointments with physicians who accept their insurance. Higher provider reimbursement could affect not only whether beneficiaries ever receive needed care, but whether beneficiaries receive it in a way that they feel meets their needs. We expected that beneficiaries in all three premium assistance demonstrations would report better access to care and fewer unmet needs than beneficiaries in other expansion states.

a. Regression analysis of survey data on unmet need for medical care

We analyzed BRFSS data from 2012 to 2017 to assess unmet need for medical care in Arkansas, Iowa, and New Hampshire (demonstration states) and in Kentucky, Nevada, New Mexico, North Dakota, Ohio, Oregon, Pennsylvania, Washington, and West Virginia (comparison states). We considered respondents' answers to three questions: (1) whether they had a routine doctor's visit within the past year, (2) whether they have a personal doctor or health care provider, and (3) whether they had an unmet medical need due to cost in the previous year. More affirmative

answers to the first two questions would indicate better access to care (or a decrease in unmet need for care), whereas a higher number of affirmative answers to the last question would indicate a decline in access to care (an increase in unmet need for care). We conducted regression analyses to assess the probability of an affirmative response to each question in demonstration and comparison states, controlling for observable demographic characteristics (age, sex, race/ethnicity, education, marital status, employment status, income, and indicators for disability and for the presence of a child in the household). We limited the sample to respondents who were ages 18 to 64 and who reported annual household incomes under $35,000. These respondents were not necessarily enrolled in their state's Medicaid program (either direct coverage or premium assistance). These analyses therefore reflect the potential population-level effects of an expansion featuring a premium assistance model on unmet need for medical care. Appendix E contains the full model specifications and descriptive statistics for BRFSS data, including demographic characteristics for this sample (Appendix Table E.2).

From pre-expansion to post-expansion, the percentage of respondents with a checkup in the last year increased more for adults in demonstration states than for those in comparison states (Table III.5). Arkansas had the largest improvement after its demonstration was implemented, with a 5.5 percentage point increase that was statistically significant. Arkansas also had the largest percentage point decrease in reported unmet medical need due to cost after expanding Medicaid, but the estimated relationship between residing in Arkansas and reporting unmet need was not statistically significant. New Hampshire showed a larger increase post-expansion in the percentage of adults with a personal provider, 4.4 percentage points, than Arkansas, Iowa, or the comparison states—a result that was statistically significant at the 5 percent level. Taken together, these results suggest that the premium assistance model could diminish the level of unmet need for care, although we did not find statistically significant relationships for all outcomes in all states.

Table III.5. Percentage of respondents who reported having a checkup within the last year, a personal provider, or unmet medical need due to cost, and association of change in these measures with premium assistance demonstrations

	Pre-expansion unadjusted mean (percent)	Post-expansion unadjusted mean (percent)	Regression-adjusted average marginal effect (s.e.)	*p*-value
Checkup within last year				
Arkansas	51.7	62.9	5.5*** (1.8)	.002
Iowa	60.5	62.3	3.6** (1.7)	.035
New Hampshire	58.3	61.1	-1.1 (2.2)	.633
Comparison	58.1	62.5		
N = 61,367,748				
Personal provider				
Arkansas	65.0	72.2	2.9 (1.6)	.070
Iowa	71.0	70.3	2.8 (1.6)	.073
New Hampshire	74.3	79.4	4.4** (2.0)	.030
Comparison	65.9	68.7		
N = 61,989,941				

Table III.5 (*continued*)

	Pre-expansion unadjusted mean (percent)	Post-expansion unadjusted mean (percent)	Regression-adjusted average marginal effect (s.e.)	*p*-value
Unmet need due to cost				
Arkansas	40.4	28.3	-2.4 (1.6)	.130
Iowa	21.4	19.1	-0.9 (1.5)	.538
New Hampshire	29.1	24.5	3.4 (2.1)	.108
Comparison	31.9	23.6		
N = 62,142,936				

Source: Mathematica analysis of Integrated BRFSS data, 2012–2017, for Arkansas, Iowa, and New Hampshire (demonstration states); and Kentucky, Nevada, New Mexico, North Dakota, Ohio, Oregon, Pennsylvania, Washington, and West Virginia (comparison states).

Notes: Marginal effects were estimated using logistic regressions. We calculated the average of the estimated difference in outcomes, using the covariate distribution of the demonstration group. Control variables included age, sex, race/ethnicity, education, marital status, employment status, income, and indicators for disability and for the presence of a child in the household.

**Significantly different from zero at the .05 level, two-tailed test.

***Significantly different from zero at the .01 level, two-tailed test.

s.e. = standard error.

b. Limitations of analyses of unmet need for medical care

One limitation of the analyses of unmet need is that we do not know what proportion of the BRFSS sample in each state was enrolled in a Medicaid expansion. Likewise, in demonstration states we cannot distinguish between respondents in traditional Medicaid coverage and those in QHPs. Thus, although we found some evidence of improvement in measures of access to care and unmet need that exceed those observed in traditional Medicaid expansion states, these are population-level effects, and we cannot quantify the effect of the demonstration on these measures for the subset of BRFSS respondents who were actually enrolled in QHPs. Although a QHP-specific effect is diluted in these population-level estimates, they are better at capturing any potential spillover effect from changes among QHP beneficiaries that affect other adults with low incomes. The QHP-specific effect is likely to be more diluted in Iowa, where only those earning 100 to 133 percent of FPL were enrolled in QHPs, than in Arkansas or New Hampshire, where a larger proportion of the population earning less than 133 percent of FPL was enrolled in QHPs.

A second limitation is that measures of unmet need for care rely on survey data, which can be subject to various sources of bias. People who chose to respond to surveys could be different from those who chose not to respond. This type of survey bias is a particular concern when response rates are low. BRFSS response rates for 2017 for the states included in this analysis ranged from 39.6 percent (in Washington) to 56.9 percent (in North Dakota) (Centers for Disease Control and Prevention 2018).

c. What have other studies found?

Findings from academic studies on unmet need for care. Sommers, Blendon, and Orav (2016) compared the premium assistance demonstration in Arkansas, the traditional Medicaid expansion

in Kentucky, and the Medicaid program in Texas (which did not expand coverage). The authors found that, in 2014, respondents in Kentucky had a larger decline in the amount of difficulty they had paying medical bills than respondents in Arkansas did, but there were no other significant differences between these two states in terms of access to care. Sommers et al. (2017) updated the analysis using data through 2016, and with a longer post-expansion period, they found significant improvements for both expansion states in terms of having a personal doctor, skipping care due to cost, and receipt of a checkup in the past 12 months. However, outcomes in the two expansion states were not significantly different from each other.

Similarly, Kirby and Vistnes (2016) found that, among those who were uninsured in 2013, improvements in access were similar for new Medicaid and Marketplace enrollees. Sommers, Blendon, Orav, and Epstein (2016) found that beneficiaries in Kentucky had higher rates of diabetic glucose testing than those in Arkansas, but otherwise there were no statistically significant differences. Our BRFSS analysis, which is based on a larger set of comparison states than Sommers et al. used, finds generally more positive effects of the QHP programs on access across the demonstration states. People with low incomes in Arkansas, Iowa, and New Hampshire had a statistically significant improvement in at least one measure of increased access, suggesting that QHP demonstrations improved access to care more than Medicaid programs in comparison states did.

Findings from state-based evaluation reports on section 1115 demonstrations. We reviewed state evaluation reports for Arkansas, Iowa, and New Hampshire to explore unmet need for medical care in premium assistance demonstrations, as reflected by metrics corresponding to the BRFSS measures.

Arkansas reported statistically significant differences in perceived access to health care between QHP enrollees and one within-state comparison group—a group of Medicaid beneficiaries not enrolled in QHPs because they had exceptional health needs (Arkansas Center for Health Improvement 2018). Specifically, QHP enrollees reported greater ability to get care right away and greater ease of obtaining needed care than beneficiaries in the other group. The proportion of enrollees who always received care when they needed it right away was 72 percent for QHP enrollees versus 53 percent for beneficiaries with exceptional health needs (regression-adjusted estimates). However, similar proportions of both groups said they always got an appointment for a check-up or routine care or for a specialist as soon as they needed it. There were no differences between QHP beneficiaries and a different within-state comparison group of Medicaid beneficiaries who were eligible before the expansion and who did not complete a health needs questionnaire.

In Iowa, QHP beneficiaries reported similar levels of unmet need for care compared to traditional Medicaid beneficiaries, but they were less likely to have a regular source of care. Comparable percentages of beneficiaries enrolled in QHPs and traditional Medicaid coverage reported unmet need for urgent, routine, preventive, mental/emotional care, and prescriptions, although QHP beneficiaries had lower rates of unmet need for specialist care than beneficiaries

enrolled in state plan Medicaid coverage (who had lower incomes than those in the expansion eligibility group) (Damiano et al. 2015). Damiano and colleagues used a CAHPS composite measure to assess timely access to care and timely access to information and found that reported timely access to care and information was similar for QHP beneficiaries and those enrolled in traditional Medicaid coverage. However, QHP beneficiaries were less likely to report having a regular source of care than other demonstration and traditional Medicaid beneficiaries were (74 percent, compared to 81 percent for both within-state comparison groups) (Damiano et al. 2015; Momany et al. 2019).

In New Hampshire's state-based evaluation report, QHP enrollees were more likely to report getting an appointment with a specialist as soon as they needed it than those enrolled in traditional Medicaid coverage were (Health Services Advisory Group 2019). However, the evaluators found inconclusive results on whether QHP enrollees were more likely to report (1) getting care as soon as they needed to when they needed care right away and (2) getting an appointment for a check-up or routine care as soon as needed.

3. Potential for continuity between Medicaid/Marketplace coverage

Fluctuations in the income and household composition of adults with low incomes can affect their eligibility for Medicaid versus Marketplace subsidies and cause them to "churn" between public and private coverage. Smoothing these coverage transitions is one rationale for choosing to implement a premium assistance demonstration instead of a traditional Medicaid expansion. Beneficiaries who transition between coverage types are more likely to be able to stay enrolled with the same issuer and keep the same providers when the same issuer offers plans in both settings in a given state.

To examine the potential for continuity of coverage across settings, we used information on issuers' participation in Medicaid and the Marketplace in the years 2014–2018 to assess the overlap between QHP issuers offering plans both to Medicaid premium assistance beneficiaries and in the Marketplace, as well as the overlap between issuers offering Medicaid managed care (MMC) plans and QHPs in the states that served as comparisons to the demonstration states.

a. Analysis of Medicaid and Marketplace issuer participation

We compiled lists of issuers in the demonstration states that participated in premium assistance, along with lists of issuers in all demonstration and comparison states that offered coverage via MMC or the Marketplace. For each state, we determined the number and percentage of issuers that overlapped, both between premium assistance and the Marketplace and between MMC and the Marketplace. We included Arkansas, Iowa, and New Hampshire as demonstration states; Kentucky, Nevada, New Mexico, North Dakota, Ohio, Oregon, Pennsylvania, Washington, and West Virginia were the comparison states. We categorized comparison states by market size and focused our assessment of issuer participation by comparing New Mexico, North Dakota, and West Virginia to New Hampshire (small markets), and Kentucky, Nevada, and Oregon to Arkansas and Iowa (medium markets).

Premium assistance versus Marketplace issuers. Arkansas and New Hampshire had complete overlap between Medicaid premium assistance and Marketplace issuers, whereas Iowa had less overlap. The Arkansas and New Hampshire insurance departments require issuers that offer QHPs in the Marketplace to also offer plans that qualify for the premium assistance program.[25] As a result, beneficiaries who churned between premium assistance and Marketplace plans had the option of staying with the same issuer in those states. Iowa did not have this requirement; only two of its four Marketplace issuers in 2014 and one of its three issuers in 2015 offered both Marketplace and premium assistance plans (Table III.6). The fact that several of Iowa's Marketplace issuers opted out of participating in premium assistance suggests that high levels of overlap may only be achievable through active state intervention—either through regulation or incentives.

Medicaid managed care versus Marketplace issuers. From 2014 to 2018, none of the states we examined required Marketplace issuers to also offer MMC plans, nor were issuers of MMC plans required to also have Marketplace plans. New Hampshire only had an issuer offering plans in both the Marketplace and MMC settings in one year (2018). One issuer in Iowa offered both Marketplace and MMC plans in 2016, when Iowa transitioned all demonstration enrollees into Medicaid managed care; however, that issuer dropped out of the Marketplace in 2017 (Table III.6). Arkansas does not contract with managed care plans for its Medicaid program. Two of the three small-market comparison states had minimal overlap or no overlap in issuers. In the third small-market comparison state (New Mexico), three out of six issuers offering either MMC or Marketplace plans participated in both markets from 2015 to 2016, and two out of six participated in both markets in 2017 and 2018. In medium-market comparison states, between one and four issuers participated in both settings, representing 5 to 67 percent of all Marketplace and/or MMC issuers.

[25] New Hampshire's premium assistance program was replaced with Medicaid managed care beginning in January 2019, after the end of this analysis period. The New Hampshire insurance department requirements applied while the premium assistance program was active.

Table III.6. Number of overlapping issuers and percentage of overlapping issuers out of all participating issuers, by state: 2014–2018

States	2014 n (%)	2015 n (%)	2016 n (%)	2017 n (%)	2018 n (%)
Small markets (<150K potential Marketplace enrollees)					
Demonstration states					
New Hampshire					
Premium Assistance and Marketplace issuers	n.a.	n.a.	5 (100)	4 (100)	3 (100)
MMC and Marketplace issuers	0 (0)	0 (0)	0 (0)	0 (0)	1 (25)
Comparison states (MMC and Marketplace issuers)					
New Mexico	1 (20)	3 (50)	3 (50)	2 (33)	2 (33)
North Dakota	1 (33)	1 (33)	1 (33)	1 (33)	1 (33)
West Virginia	0 (0)	0 (0)	0 (0)	0 (0)	0 (0)
Medium markets (200–300K potential Marketplace enrollees)					
Demonstration states					
Arkansas					
Premium Assistance and Marketplace issuers	3 (100)	4 (100)	5 (100)	4 (100)	4 (100)
MMC and Marketplace issuers	n.a.	n.a.	n.a.	n.a.	n.a.
Iowa					
Premium Assistance and Marketplace issuers	2 (50)	1 (33)	n.a.	n.a.	n.a.
MMC and Marketplace issuers	0 (0)	0 (0)	1 (14)	0 (0)	0 (0)
Comparison states (MMC and Marketplace issuers)					
Kentucky	2 (33)	3 (43)	4 (50)	2 (33)	1 (17)
Nevada	1 (20)	1 (17)	1 (20)	1 (17)	2 (67)
Oregon	2 (8)	2 (8)	2 (9)	1 (5)	1 (5)
Large markets (>450K potential Marketplace enrollees)					
Comparison states (MMC and Marketplace issuers)					
Ohio	4 (29)	5 (31)	6 (38)	4 (31)	4 (44)
Pennsylvania	5 (19)	5 (12)	4 (11)	2 (15)	2 (12)
Washington	3 (21)	4 (33)	4 (33)	3 (27)	3 (27)

Source: Mathematica analysis of CMS Center for Consumer Information & Insurance Oversight Service Area Public Use Files, 2014–2018; 2014–2017 CMS Medicaid Managed Care Enrollment and Program Characteristics; 2018 National Committee for Quality Assurance Medicaid Health Insurance Plan Rating; 2014–2018 Oregon Health Plan Data and Reports; Iowa 1115 Demonstration Waiver Marketplace Choice quarterly monitoring reports; and Kaiser Family Foundation's "2018 Medicaid MCOs and Their Parent Firms." For the states that did not use healthcare.gov for their Marketplace, issuer information came from state exchange websites, press releases, and reports as of November 5, 2018.

Notes: For the premium assistance and Marketplace overlap percentages, we used the total number of unique Marketplace and/or premium assistance issuers as the denominator. For the MMC and Marketplace overlap percentages, we used the total number of unique Marketplace and/or MMC issuers as the denominator. Among states included in our analysis, the number of unique issuers ranges from 3 to 7 in states with small markets; 3 to 25 in states with medium markets; and 9 to 42 in states with large markets.

In cases where we found discrepancies between CMS MMC (which lists issuers as of July 1 each year) and NCQA (which lists issuers as of June 30 each year) data sources, we used the information from CMS.

Table III.6 (*continued*)

Market sizes were categorized according to the number of potential 2015 Marketplace enrollees for each state as reported by the Kaiser Family Foundation (KFF 2016).
The CMS Public Use Files did not include information on whether certain issuers in North Dakota only offered off-exchange stand-alone dental plans. We used information from the Kaiser Family Foundation (2017) to confirm the number of issuers offering health plans on the Marketplace, excluding those not identified by Kaiser to ensure consistent exclusion of stand-alone dental plans across states.
The managed care organizations counted for Oregon are coordinated care organizations.
Arkansas does not have MMC. Iowa's premium assistance program, Marketplace Choice, closed on December 31, 2015. New Hampshire's premium assistance program did not begin until 2016 and closed after 2018.

MMC = Medicaid managed care; n.a. = not applicable.

b. Limitations of the analysis on Medicaid and Marketplace plan participation

We did not systematically examine issuers by market share, so overlap between multiple small issuers could affect fewer enrollees than overlap for one large issuer would. Certain issuers participating in both Marketplace and MMC settings have large shares of the individual insurance market. For example, Kentucky had 33 percent overlap between issuers in 2014, reflecting the fact that two issuers offered both Marketplace and MMC plans out of six unique issuers participating in the Marketplace and Medicaid managed care. One of these issuers was Anthem, which covered 51 percent of the individual market in 2014 (Kaiser Family Foundation n.d. [a]). To the extent that share in the individual market is representative of an issuer's share of other markets, large issuers can cover a high proportion of both the MMC and Marketplace populations in each state, representing an opportunity for continuity of coverage for more beneficiaries.

In addition, some issuers offer plans only in certain parts of a state, and we did not examine issuer participation in geographic areas smaller than the state. Thus, even in states with significant issuer overlap, residents in certain areas might not have the option of staying enrolled with the same issuer. For example, Arkansas's state-based interim evaluation reports that the number of Marketplace issuers by region changed over time. In 2014, only three out of seven market regions in Arkansas had more than two participating issuers. By 2016, all seven market regions had at least five carriers offering coverage (Arkansas Center for Health Improvement 2018). We also note that the aggregate issuer overlap statistics in Table III.6 mask changes in individual issuer participation in the Marketplace that can affect whether premium assistance beneficiaries remain enrolled with the same issuer from one year to the next, even if they do not have eligibility changes. (See Natzke and Chao [2018], which presents an earlier version of this analysis, for a more detailed discussion.)

Finally, we used a variety of sources to identify issuers because no single source had the information we needed for every year in the study. For example, we relied on the CMS Medicaid Managed Care Enrollment and Program Characteristics source for a list of MMC issuers for 2014 through 2017, and on the NCQA source for MMC issuers in 2018, because data are not available for 2018 from the CMS Medicaid Managed Care Enrollment and Program Characteristics source. In comparing the lists of issuers from the two sources for years in which data were available from both, we found differences in terms of the number and identity of issuers. When we found discrepancies between these two sources, we used the information from

CMS. Because the CMS Medicaid Managed Care Enrollment and Program Characteristics data are not yet available for 2018, the number of issuers we list for each state in 2018 might not reflect all participating issuers, or could include issuers that were consolidated or liquidated or had terminated their contracts early. We attempted to minimize this limitation in the 2018 data by comparing the list of NCQA issuers with a list generated by Kaiser Family Foundation for 2018. We compared information from the CMS Center for Consumer Information & Insurance Oversight Service Area Public Use Files: 2014–2017 with a Kaiser Family Foundation report (2017), and found that the number of Marketplace issuers differed for some states. The CMS data, which are based on issuer data submissions, generally include more issuers than the number reported by Kaiser Family Foundation, which are based on state insurance filings. We used the CMS data when there was a discrepancy.

c. What have other studies found?

Findings from state-based evaluation reports on section 1115 demonstrations. State evaluation reports for Arkansas and Iowa did not address this topic. The New Hampshire state report found that more Medicaid plans entered the health insurance marketplace after the start of the premium assistance demonstration. There was inconclusive evidence, however, on whether premium assistance beneficiaries maintained continuous enrollment with the same health plans and providers when Medicaid eligibility changed (Health Services Advisory Group 2019).

Other relevant literature. The premium assistance model has the potential to smooth transitions between premium assistance and Marketplace coverage. Recent studies have estimated that 20–25 percent of adults with low incomes have fluctuations in their health insurance coverage each year (Sommers, Gourevitch, and Maylone et al. 2016; Maylone and Sommers 2017). Smoother transitions between coverage types could help them.

Demonstration states that require Marketplace issuers to also offer QHPs to the premium assistance population have complete overlap in participating issuers, enabling continued enrollment with the same issuer over time. However, the ability to maintain continuous relationships with providers when beneficiaries change coverage types also depends on whether the plans offered to Marketplace and Medicaid beneficiaries have comparable provider networks (McQueen 2013). Issuers participating in either the Marketplace or Medicaid managed care could find it difficult to create a provider network in the other market (Rosenbaum 2015). This challenge would need to be addressed to increase participation in both settings and support comparable networks. Furthermore, one study found multiple barriers to Marketplace participation among Medicaid insurers, such as the lack of sophisticated in-house actuarial knowledge, lack of a system to collect premiums from individuals or employers, costs of capital reserve requirements, lack of experience competing for beneficiaries, and increased financial risk due to changes in Marketplace conditions and rules (Burton, Wengle, and Elmendorf 2019). We did not find any studies of issuer participation in Marketplaces versus premium assistance.

At the national level, the Association for Community Affiliated Plans (ACAP) estimated that about 93 out of 192 Marketplace insurers (48 percent) offered both Marketplace and Medicaid plans in the same states in 2018, an increase over the previous four years, when the percentage overlap ranged from 39 to 44 percent (ACAP 2018). Although ACAP used a different method than we did to calculate overlap, our finding on the percentage of overlap between Marketplace and MMC issuers in two of the comparison states (Kentucky and New Mexico) is similar to ACAP's. In comparison with this national figure, we found a much lower percentage overlap between Marketplace and MMC in other comparison states and in the demonstration states.

C. Spending in premium assistance demonstrations

In premium assistance demonstrations, Medicaid paid QHP issuers a capitated monthly rate to cover medical care for enrolled beneficiaries instead of paying for care on a fee-for-service basis or contracting with Medicaid managed care plans. Where provider reimbursement is higher for QHPs than for Medicaid, it is possible that Medicaid premium payments to QHPs would be higher than what Medicaid would pay under other coverage arrangements. Some state and federal policymakers might consider higher costs under premium assistance acceptable if there are also gains in access to appropriate care and in overall health status. To assess spending in premium assistance models, we examined one research question, listed as Question 2a in Appendix Table A.1: How do states supporting QHP enrollment compare with other Medicaid expansion states in terms of total Medicaid spending?

1. Regression analysis of administrative data on expenditures

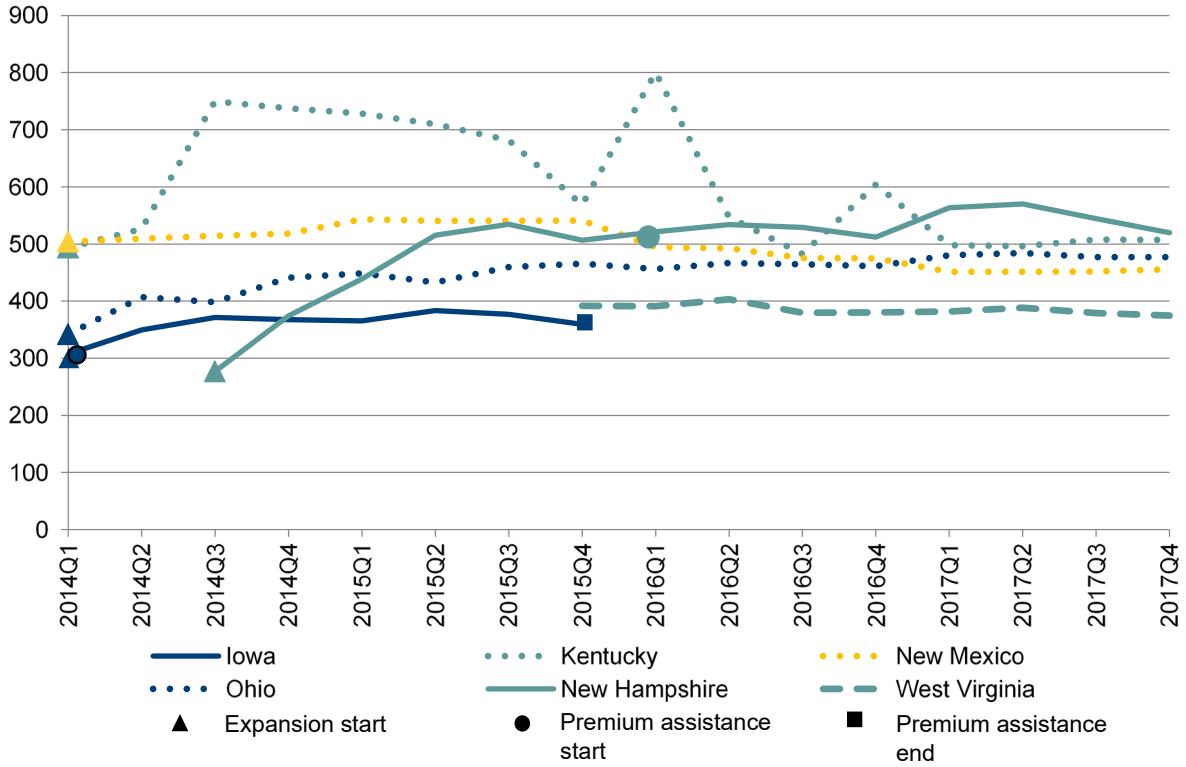
We used administrative data and all-payer claims databases to conduct regression analyses of quarterly expenditures from 2014–2017 for adult expansion Medicaid beneficiaries, controlling for beneficiary characteristics. We studied two demonstration states (Iowa and New Hampshire) in this analysis, including both beneficiaries enrolled in QHPs and those covered directly by Medicaid from these two states. Iowa was included only for its demonstration period (2014–2015) because payment data for the non-demonstration period (2016–2017) were unreliable. We could not include Arkansas in analyses of Medicaid expenditures because the APCD data lacked information on capitation payments paid to QHPs and could not be linked to other data sources containing capitation payment records. We also included four comparison states (Kentucky, New Mexico, Ohio, and West Virginia).[26]

We calculated per-member per-month (PMPM) spending by the Medicaid program on direct medical services and capitation payments, including payments to QHPs, in each quarter in the years 2014 through 2017 (See Figure III.1). For each of our outcome variables, we recoded all values above the 99th percentile to the value of the 99th percentile to minimize the effects of extreme outliers, a common statistical technique called "Winsorizing."

[26] We included West Virginia data from October 2015 through December 2017 only, which is the period covered by TAF in that state. We excluded MAX and Alpha-MAX data for West Virginia because we had concerns about the quality of the data. Pennsylvania was excluded because payment variables were unreliable in administrative data sources.

We used two regression models: (1) a difference-in-differences (DD) model and a (2) cross-sectional model. New Hampshire was the only state for which we had usable payment data covering demonstration and non-demonstration periods after the state expanded Medicaid, and was therefore the only demonstration state included in the DD analysis. The DD model exploited the timing of New Hampshire's switch from traditional Medicaid for all expansion beneficiaries (August 2014 to December 2015) to enrolling most beneficiaries in QHPs (January 2016–December 2017). The cross-sectional model, which does not require data covering non-demonstration periods, allowed us to include Iowa. However, cross-sectional analysis cannot control for baseline differences in expenditures across states, and therefore might be confounded by unobserved differences across states. In both regression models, we controlled for sex, age, living in a rural area, and length of enrollment span. In data quality checks preceding our analysis, we saw consistent dips in the number of claims in the quarter preceding the transition to TAF data, likely due to a limited run-out for Alpha-MAX data. We included a control variable in the regression analysis to account for this pattern. Appendix C contains full model specifications and sample characteristics.

Figure III.1. Mean PMPM spending on direct medical services and capitation payments for all adult Medicaid expansion beneficiaries, 2014 to 2017



Source: Mathematica analysis of administrative data from 2014–2017 for Arkansas, Iowa, and New Hampshire (demonstration states); and Kentucky, New Mexico, Ohio, Pennsylvania, and West Virginia (comparison states). Years included for each state depend on the date of demonstration implementation or coverage expansion and on data availability. We do not report Iowa results for 2016–2017 due to anomalously low expenditures in those years.

PMPM = per-member per-month.

2. Regression results

In our main DD regression model, which controlled for beneficiary characteristics and baseline differences in expenditures across states, we found that New Hampshire's premium assistance demonstration increased Medicaid expenditures. The change in expenditures after the introduction of the premium assistance program in New Hampshire was $56 PMPM larger than the change in expenditures over the same time period in comparison states (Table III.7). The results from the cross-sectional regression analysis were mixed. We found a larger PMPM increase (of $123) in spending in New Hampshire, but we also found that the premium assistance demonstration in Iowa was associated with PMPM expenditures that were $148 lower than in comparison states, which could signal savings from premium assistance (Table III.8). This model did not control for baseline differences across states, and therefore either estimate could reflect differences in healthcare utilization patterns or the cost of medical care across states rather than the impact of premium assistance.

Table III.7. Regression results for expenditures (difference-in-differences model) for expansion Medicaid beneficiaries in New Hampshire, 2014 to 2017

Outcome variable	Comparison group baseline mean	New Hampshire: Average marginal effect	Standard error	*p*-value	Percent change	N
PMPM expenditures ($)	508	56***	1	.000	11	32,132,991

Source: Mathematica analysis of administrative data from 2014–2017 for New Hampshire (demonstration state); and Kentucky, New Mexico, Ohio, and West Virginia (comparison states). Years included for each state depend on the date of demonstration implementation or coverage expansion and on data availability.

Notes: We controlled for beneficiaries' individual characteristics (sex, age, living in a rural area, and length of enrollment span). We also controlled for the dips in PMPM expenditures around the transitions to TAF, which were found in the descriptive analysis.

Marginal effects were estimated using a generalized linear regression model. We calculated the average of the estimated difference in differences, using the covariate distribution of the demonstration group.

**Significantly different from zero at the .05 level, two-tailed test.

***Significantly different from zero at the .01 level, two-tailed test.

Table III.8. Regression results for expenditures (cross-sectional model) for expansion Medicaid beneficiaries in Iowa and New Hampshire, 2014 to 2017

Outcome variable	Comparison group baseline mean	IA Average marginal effect	Standard error	*p*-value	Percent change	NH Average marginal effect	Standard error	*p*-value	Percent change	N
PMPM expenditures ($)	508	-148***	1	.000	-29	123***	1	.000	24	32,132,991

Source: Mathematica analysis of administrative data from 2014–2017 for New Hampshire and Iowa (demonstration states); and Kentucky, New Mexico, Ohio, and West Virginia (comparison states). Years included for each state depend on the date of demonstration implementation or coverage expansion and on data availability.

Notes: We controlled for beneficiaries' individual characteristics (sex, age, living in a rural area, and length of enrollment span). We also controlled for the dips in PMPM expenditures around the transitions to TAF, which were found in the descriptive analysis.

Marginal effects were estimated using a generalized linear regression model. We calculated the average of the estimated difference in differences, using the covariate distribution of the demonstration group.

**Significantly different from zero at the .05 level, two-tailed test.

***Significantly different from zero at the .01 level, two-tailed test.

3. Limitations of spending analysis

There are several limitations to note for the expenditure analysis. First, cross-sectional regression estimates are likely biased by unmeasured and therefore uncontrolled-for differences across states. The DD model does a better job of accounting for these differences, but our results might not generalize beyond New Hampshire. Furthermore, for New Hampshire, PMPM expenditures were increasing relative to comparison states in 2014, before the start of the premium assistance demonstration, which does not support the parallel trends assumption required for difference-in-differences estimation and biases our estimates upwards (toward larger estimated effects of premium assistance).[27] Second, as noted in Section III.B.1.b, differences in data sources could also introduce error in comparing Iowa (data from the state Medicaid agency) and New Hampshire (data from the APCD) with comparison states (for which we used MAX, Alpha-MAX, and TAF data). Given these limitations, the results from the expenditures analysis should be interpreted with caution.

4. What have other studies found?

Findings from state-based evaluation reports on section 1115 demonstrations. We analyzed cost to Medicaid, including premium payments (or capitation payments) to plans and fee-for-service payments for wraparound benefits. We could not find comparable studies in state-based evaluations. However, all three state-based evaluators analyzed service payments to providers to study the cost of providing services to Medicaid beneficiaries under different coverage approaches. In both Arkansas and Iowa, evaluators noted that the cost of covering beneficiaries was higher through QHPs than traditional Medicaid; differences were mainly attributed to higher provider payment rates (Arkansas Center for Health Improvement 2018; Damiano et al. 2015; see also a MACPAC summary by Buderi 2017). The New Hampshire state-based evaluation found that the cost of providing care to QHP beneficiaries was higher than it would have been under other Medicaid coverage, a result driven by provider reimbursement rates and administrative costs (Health Services Advisory Group 2019).

D. Enrollment in premium assistance demonstrations

We addressed two subsidiary research questions designed to assess how states with premium assistance demonstrations compared to other Medicaid expansion states in terms of enrollment (listed as Questions 3a and 3b in Appendix Table A.1):

- How does the take-up rate among likely eligible individuals in premium assistance states compare to states with traditional Medicaid expansions?

[27] In a sensitivity check, we dropped 2014 data from New Hampshire due to the trend of increasing spending, and we found results similar to those in our main DD analysis, although somewhat smaller in magnitude. We found that the demonstration in New Hampshire was associated with higher spending of $34 ($p < 0.001$) PMPM as opposed to $56.

- Are there patterns in the timing of Medicaid beneficiary enrollment in premium assistance states that may be related to the Marketplace open enrollment period, even though Medicaid beneficiaries are not subject to open enrollment periods?

For the first question, we used administrative enrollment data from 2014 through 2017 and IPUMS-ACS data over the same time period. To answer the second question, we analyzed Medicaid administrative enrollment data from 2014 through the 2017–2018 Marketplace open enrollment period.

1. Take-up among the likely eligible population

QHPs could be an appealing option to people who believe a stigma is attached to enrolling in Medicaid. We might therefore expect that take-up would be higher in demonstration states than comparison states. We conducted a descriptive analysis of take-up rates by comparing levels of Medicaid enrollment in states with premium assistance demonstrations to those in comparison states, overall and for demographic subgroups. Take-up rates were calculated as unadjusted annual estimates of the proportion of the likely eligible population enrolled in Medicaid (including premium assistance programs). As described in Chapter II, the numerator for each state is the average number of non-disabled, non-dual–eligible, non-elderly beneficiaries enrolled in a year.[28] Denominators are estimates of the total eligible population in each state and are based on IPUMS-ACS data.

a. Take-up rates by state

Table III.9 shows take-up rates by state from 2012 to 2017. Most states implemented their Medicaid expansions in January 2014, with two exceptions: New Hampshire (August 2014) and Pennsylvania (January 2015). States with premium assistance demonstrations had lower take-up than most comparison states in most years. Pennsylvania is an outlier among the comparison states, with relatively low take-up. It is also notable that both Iowa and New Hampshire had low take-up relative to comparison states even in years when they did not have premium assistance, suggesting the presence of confounding state factors that we cannot disentangle from premium assistance policies.

[28] The numerator for Arkansas's take-up rates for 2014 through 2017 includes the expansion population only. We used data from Arkansas's All-Payer Claims Database as the source for all analyses of Arkansas administrative data because it is likely there were errors in the enrollment data in Arkansas's T-MSIS submissions. We did not obtain data from the All-Payer Claims Database for adults in eligibility groups other than the expansion group. However, Arkansas had relatively low income limits for non-disabled adults before expanding coverage (as reflected by low take-up rates in 2012 and 2013), suggesting that the take-up rate for the entire non-disabled, non-dual–eligible, non-elderly population would only be slightly higher than the take-up rate for the expansion population in 2014–2017.

Table III.9. Take-up rates by state among adults likely eligible according to income eligibility levels following expansion

Expansion date	State	2012	2013	2014	2015	2016	2017
Demonstration states							
January 2014	Arkansas	0.06	0.06	**0.42[a]**	**0.51*[a]**	**0.62*[a]**	**0.65*[a]**
	Iowa	0.31	0.32	**0.45**	**0.50***	**0.53**	**0.57**
August 2014	New Hampshire	0.09	0.09	**0.27**	**0.35**	**0.44***	**0.43***
Comparison states							
January 2014	Kentucky	0.11	0.11	**0.53**	**0.68**	**0.74**	**0.82**
	New Mexico	0.23	0.21	**0.58**	**0.74**	**0.80**	**0.82**
	Ohio	0.28	0.28	**0.49**	**0.63**	**0.68**	**0.68**
	West Virginia	0.11	0.12	**0.54**	**0.62**	**0.60**	**0.60**
January 2015	Pennsylvania	0.17	0.16	0.15	**0.38**	**0.46**	**0.53**

Source: Mathematica analysis of take-up estimates, calculated using administrative data to arrive at average monthly enrollment in the numerator and Integrated Public Use Microdata Sample (IPUMS) data to estimate likely eligible population based on income guidelines following coverage expansion in the denominator.

Notes: Take-up estimates in expansion years exclude the first three months post-expansion. For New Hampshire, expansion took place midyear, so the take-up estimate in 2014 also excludes months in that year that were before the expansion.

Bolded numbers indicate post-expansion year.

*Shaded estimates indicate that premium assistance was in effect in that year.

[a] Numerator includes the expansion population only.

b. Take-up by demographic subgroup

Next, we constructed take-up rates by demographic subgroups to determine whether there were patterns in enrollment by age, sex, race, and ethnicity. Take-up rates by demographic subgroups did not reveal a clear association between the use of QHPs to expand Medicaid and enrollment (see Figures F.1–F.3 in Appendix F). However, we did find differences between demographic subgroups in the propensity to take up coverage, and these differences applied to all states. For example, in all states and years, take-up was higher for women than for men (Figure F.1), and was highest among those ages 27–35 and 36–45 (Figure F.2). Chapter IV has an extended discussion of the differences in take-up rates by demographic group and the limitations to the take-up rates analysis.

2. Patterns in enrollment related to Marketplace open enrollment periods

a. Descriptive analysis of administrative data

We conducted descriptive analyses of Medicaid administrative enrollment data to assess whether there was an association between patterns in Medicaid enrollment and Marketplace open enrollment periods. In all states, public outreach about Marketplace open enrollment periods encourages people to apply for coverage, and some of them are determined Medicaid-eligible, resulting in a bump in adult Medicaid enrollment during the open enrollment period, even though people can enroll in Medicaid at any time during the year. If Medicaid-eligible adults in premium assistance states understand they are likely to enroll in QHP coverage, but do not understand that

they can initiate this coverage at any time, the bump in adult Medicaid enrollment during open enrollment could be higher in premium assistance states. We calculated unadjusted counts of monthly enrollment for adult expansion beneficiaries by state from 2014 to 2017 (Figure III.2 and Appendix Table G.3; Appendix Table C.6 provides demographic characteristics of beneficiaries included in this analysis).

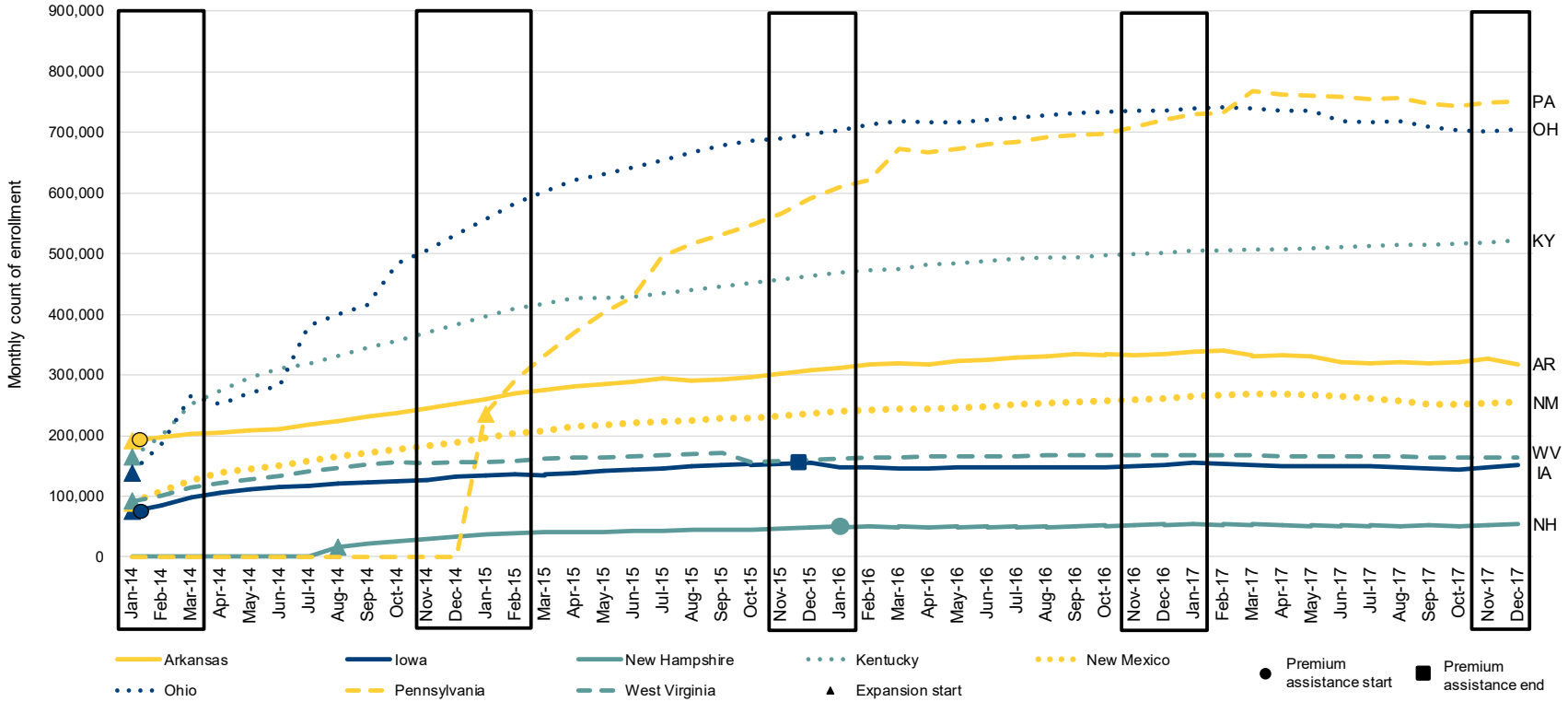
Figure III.2 shows that enrollment increased during the early months of each state's expansion. Because the first open enrollment period coincided with the expansion's implementation in most states, it is not possible to distinguish any effect of open enrollment from January–March 2014. Similarly, there is a jump in enrollment in Pennsylvania during the second open enrollment period, when that state expanded Medicaid. In many, states, including the demonstration states, enrollment plateaued or declined in 2017.

There are no cases where states saw pronounced increases in enrollment, relative to trends, during open enrollment periods. In rare cases, there was a decrease during the open enrollment period. In Iowa, for example, during the open enrollment period for calendar year 2016 (November 2015–January 2016), there was a 4 percent decrease in enrollment, but this coincided with Iowa ending its QHP demonstration and a switch in data sources from state administrative data to TAF. Overall, we found no evidence of a unique relationship between changes in enrollment and Marketplace open enrollment periods in premium assistance states.

b. Limitations of the analysis on enrollment timing

The key limitations for the enrollment analysis are a consequence of using multiple data sources. Different states switched from MAX to Alpha-MAX or Alpha-MAX to TAF at different times, which could have added "noise" to the enrollment counts. We also used state administrative data for Iowa (2014–2015) and APCD data for Arkansas (2014–2017).

Figure III.2. Monthly counts of adult non-disabled, non-elderly, non-dual–eligible Medicaid beneficiaries during the study period, 2014–2017, and Marketplace open enrollment periods



Source: Mathematica analysis of administrative data from 2014–2017 for Arkansas, Iowa, and New Hampshire (demonstration states); and Kentucky, New Mexico, Ohio, Pennsylvania, and West Virginia (comparison states). Years included for each state depend on the date of demonstration implementation or coverage expansion and on data availability.

Notes: We include all states with available data for the newly eligible population for 2014 to 2017. This includes Iowa data in 2016, after its premium assistance program ceased operations, and New Hampshire data in 2014 and 2015, before the premium assistance aspect of its demonstration began in January 2016.

Rectangles indicate months of the Marketplace open enrollment periods; the open enrollment period was the same nationwide. Markers on each line indicate the beginning of the state's expansion.

3. What have other studies found?

Although several studies have examined the effects of Medicaid expansion on take-up rates (for example, Wehby and Lyu 2017; Sommers et al. 2014), we are not aware of any that distinguish between expansion states by demonstration type or examine Medicaid enrollment patterns vis-à-vis Marketplace open enrollment periods. Medicaid enrollment has been shown to increase during those periods even though Medicaid beneficiaries can enroll at any time of the year (Colby and Croake 2016).

E. Discussion

We first discuss our findings on access to care in premium assistance demonstrations in Arkansas, Iowa, and New Hampshire compared with access in other Medicaid expansion states. We assessed service use, provider participation, unmet need for medical care, and the potential for continuity of coverage when switching between Medicaid and Marketplace coverage.

Access and health outcomes. First, we assessed the use and promptness of physician office visits, prescriptions, vision, dental, family planning, and non-emergency medical transportation services. We found that rates of physician office visits in Iowa and New Hampshire were higher during their premium assistance demonstration than rates in comparison states, suggesting greater access to physician care in those two states. This finding was statistically significant. Beneficiaries in Arkansas, however, had fewer physician office visits than beneficiaries in comparison states. Prescription drug fill rates were higher in Iowa, but lower in Arkansas and New Hampshire than in comparison states. Findings on beneficiaries' use of vision, dental, family planning and non-emergency medical transportation services were mixed: some services were used at higher rates in demonstration states, and some were used at lower rates.

The Arkansas results on access to care should be interpreted with caution. We could not use the stronger DD research design that we used for Iowa and New Hampshire because the Arkansas demonstration was in effect for the entire post-expansion period. Estimates from cross-sectional regression models are potentially biased by unobserved confounding variables, such as variation in health care utilization patterns across states, that are better controlled for in DD regressions. Differences in our findings could be the result of differences in the effects of the programs or unobserved differences in data quality and completeness. In addition, Arkansas data came from the state's APCD, and we could not link them to Medicaid administrative data as we could with New Hampshire. Other differences in the data structure led us to use comparable but different definitions of some outcome variables, which may further explain some of the difference.

Another important caveat to our findings on utilization is that, in addition to premium assistance, Iowa implemented beneficiary engagement strategies, including financial incentives for beneficiaries to have an annual wellness exam and health risk assessment (Domain 3), and those could affect our evaluation of the outcomes studied in Domain 1. Iowa also required monthly payments (Domain 2) starting in 2015, which could have had an effect on outcomes observed in Domain 1, through changes that might have taken place in take-up. Because these components of

the demonstration were all implemented at the same time, it is not possible to isolate the effects of premium assistance from the effects of these other components. For example, rates of physician office visits might have been affected both by higher reimbursement rates and financial incentives for patients.

Our findings, except for the one on lower rates of physician office visits in Arkansas, align with our main hypothesis: we expected that higher levels of physician reimbursement under premium assistance would give beneficiaries better access to care than traditional Medicaid coverage would. We did not expect the same changes for wraparound services, because they were provided as a fee-for-service Medicaid benefit in both demonstration and comparison states.

Second, we used BRFSS data to assess unmet need for medical care. We found that beneficiaries living in Arkansas, Iowa, and New Hampshire during premium assistance demonstrations reported better access to care than beneficiaries in comparison states. The probability of having had a checkup in the last year (Arkansas and Iowa) or of having a personal provider (New Hampshire) was higher during a premium assistance demonstration than it was in comparison states without such a demonstration. The BRFSS findings were consistent with state-based evaluation findings, but conflict with results for Arkansas in our cross-sectional regression analysis of administrative data. Different evaluation methodologies (difference-in-differences vs. cross-sectional), and different data sets with different variable definitions could explain some or all of the conflicting findings. Overall, these findings suggest that the premium assistance demonstrations helped improve access to care in each demonstration state more than traditional Medicaid expansions did in comparison states.

Finally, we assessed the potential for continuity between Medicaid and Marketplace coverage. We found complete overlap in the sets of issuers who participated in premium assistance and in the Marketplace in Arkansas and New Hampshire per state regulations, but there was much less overlap in Iowa, where participation in premium assistance was optional for Marketplace issuers. Complete issuer overlap in Arkansas and New Hampshire increases the potential for beneficiaries to stay enrolled with the same issuer as their eligibility for Medicaid expansion or Marketplace subsidies fluctuates. This degree of overlap seems unlikely to exist in the absence of state intervention—either through regulation or incentives—given Iowa's experience and the limited overlap between Marketplace and Medicaid Managed Care plans that we observed in most states.

Total Medicaid spending. We assessed how the total cost to Medicaid for coverage during premium assistance demonstrations in New Hampshire and Iowa compared to other states that enrolled all beneficiaries in traditional Medicaid coverage. We were unable to include Arkansas in this analysis due to limitations of the Arkansas data. Arkansas data came from the state's APCD, and we could not link them to Medicaid administrative data to obtain information on capitation payments as we were able to do with data from New Hampshire. We found that Medicaid expenditures were higher in New Hampshire than in comparison states, but lower in Iowa. We used a DD regression design in New Hampshire, but not in Iowa due to data quality

issues outside of Iowa's demonstration period (2014–2015). As was the case in the analysis of utilization outcomes, without controlling for baseline differences, cross-sectional regression estimates were possibly biased by unobserved differences across states, such as variation in health care prices or utilization, and this could bias our estimates for Iowa.

Enrollment in premium assistance demonstrations. We assessed how premium assistance demonstrations were associated with health insurance take-up rates and enrollment timing. We found that states with premium assistance demonstrations had lower take-up rates than most comparison states in most years. The timing of Marketplace open enrollment did not appear to be associated with the timing of Medicaid enrollment in premium assistance demonstrations or in states with traditional Medicaid expansions. We hypothesized that QHPs would be appealing to some people who believe a stigma is attached to enrolling in Medicaid, and that take-up of Medicaid coverage would therefore be higher in premium assistance states than in comparison states, but we did not find this to be the case. We also hypothesized that enrollment in demonstration states would align more closely to the health insurance marketplace's open enrollment periods than it does in comparison states, but found no evidence to support this hypothesis.

Policy takeaways. Overall, although our results were not the same for each state and analysis, they do suggest that premium assistance increases access to physician office visits. Using difference-in-differences regression models, an analytically strong approach, we found higher rates of physician office visits in both Iowa and New Hampshire during their premium assistance demonstrations, and this result was statistically significant. Using less rigorous cross-sectional regression models, we found similar results. We were able to include Arkansas only in cross-sectional models and found that beneficiaries in Arkansas had fewer physician office visits and other services than beneficiaries in the comparison states did. However, the results on access to care for Arkansas should be interpreted with caution, because cross-sectional models cannot control for confounding factors as well as the difference-in-differences models do.

Our analyses of national survey data were consistent with findings from difference-in-differences models based on administrative data. We found that beneficiaries living in Arkansas and Iowa during premium assistance demonstrations had a higher probability of having had a checkup in the last year, and those living in New Hampshire had a higher probability of having a personal provider. Results of our analysis of states' expenditures were mixed but suggest that premium assistance probably costs more than traditional Medicaid coverage. Finally, states with premium assistance demonstrations have lower take-up, but this could be due to confounding state factors that we cannot disentangle from premium assistance policies.

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IV. DOMAIN 2: PREMIUMS OR OTHER MONTHLY CONTRIBUTIONS (MONTHLY PAYMENTS)

Highlights of Domain 2 findings

- Regression models based on national household survey data from 2012 through 2017 revealed a statistically significant negative association between living in a state that requires monthly payments and the probability of enrolling in Medicaid (even for people whose incomes are too low to be subject to a monthly payment). Actually owing a monthly payment was also negatively associated with the probability of enrolling in Medicaid. The reduction in the probability of enrollment was highest for those with the highest estimated payment amounts ($31 or more).
- Unadjusted take-up rates (that is, the proportion of the likely eligible population that is enrolled in Medicaid) based on combined survey and administrative data suggest a negative relationship between monthly payment requirements and enrollment, but results were not conclusive.
- Regression estimates based on administrative data from 2014 through 2017 did not show a clear negative relationship between monthly payment policies and enrollment continuity during the first enrollment year or subsequent potential 12-month enrollment spans (the longest a beneficiary could be enrolled before needing to renew). Regression estimates of renewals after the first enrollment year revealed a relatively low probability of renewal in three of the five states with monthly payment policies (Iowa, Indiana, and Michigan), but the probability of renewal in the other two (Arkansas and Montana) was higher than in comparison states.
- Descriptive analyses of data received from Indiana revealed that the majority of those who disenrolled did not re-enroll within an 11-month observation period and were lost to follow-up. Those who were disenrolled for nonpayment and subject to a non-eligibility period were more likely to be lost to follow-up than those who disenrolled for other reasons (other than moving out of state).
- Regression estimates of continuous enrollment throughout the four-year observation period after states expanded Medicaid revealed wide variation in both demonstration and comparison states. It is possible that payment policies in Iowa and Michigan explain their relatively low long-term continuous enrollment rates, but West Virginia (a comparison state) had similarly low rates. Montana had higher continuous enrollment rates than other states with monthly payments.
- Finally, an accelerated failure time model based on administrative data revealed that people who were likely to owe monthly payments had enrollment lengths that were 84 percent of the median time to disenrollment among those without monthly payments, controlling for other variables.

Five states—Arkansas, Indiana, Iowa, Michigan, and Montana—operated section 1115 Medicaid demonstrations during our study period that required or encouraged monthly payments from Medicaid beneficiaries with incomes below 133 percent of the federal poverty level (FPL).

The principal concern with monthly payments is that they might discourage people with limited incomes from enrolling in Medicaid or from staying enrolled. On the other hand, having to pay for Medicaid coverage could signal that it is valuable, which in turn could encourage some people to enroll or to stay enrolled. This evaluation was designed to assess the relationship between monthly payment requirements and enrollment by asking the following primary research questions:

1. To what extent do requirements for monthly payments affect enrollment patterns?
2. What effects do monthly payments appear to have on continuity of coverage?

We use four distinct analytic approaches to answer these questions: (1) regression models of the likelihood of enrollment in Medicaid, based on Integrated Public Use Microdata Sample (IPUMS) data from the American Community Survey (ACS), (2) Medicaid "take-up" rates that estimate the proportion of adults who were likely eligible under the expansion and who actually enrolled in each state, based on both IPUMS-ACS data and administrative data, (3) descriptive analyses and regression models of enrollment continuity and renewal outcomes, based on administrative data, and (4) descriptive regressions and an accelerated failure time model of enrollment continuity across the four-year study period. In Appendix Table A.2, we summarize the analytical approach and data sources for all subsidiary research questions.

In the following sections, we describe key features of the monthly payment policies in effect in demonstration states during the 2014–2017 study period (Section A) and present our detailed findings (Sections B and C). Section B has findings related to the first of the two primary research questions, focusing on the relationships between monthly payments and the likelihood of enrollment. Section C has findings related to the second primary research question, focusing on the relationship between monthly payments and the likelihood of enrollment continuity during the first year of enrollment, as well as at renewal. We close with a synthesis of our results.

A. Key design features of monthly payments in evaluation states

States with section 1115 authority to collect monthly payments from adults with incomes below 133 percent of the FPL designed payment policies that vary in terms of amount, timing, and the income level at which payments are required or encouraged.[29] Table IV.1 summarizes these payment policies during the 2014–2017 study period. Other features of states' monthly payment policies—such as consequences for nonpayment, enforcement of those consequences, and rewards such as gift cards or enhanced benefits—are more challenging to assess, but could also affect initial and continued enrollment in Medicaid. Next, we briefly summarize relevant features of these policies in the five states that are the subject of our evaluation. More details can be found in Appendix B.

[29] Title XIX of the Social Security Act normally prohibits states from requiring monthly payments from Medicaid beneficiaries with family incomes under 150 percent of the FPL, with certain exceptions—such as working people with disabilities who are eligible under the Ticket to Work and Work Incentives Improvement Act. Section 1115 authority is therefore relevant when monthly payments are collected from adults who are not disabled and have incomes under 150 percent of the FPL, but alternative Medicaid expansion demonstrations only include adults with incomes up to 133 percent.

Table IV.1. Overview of monthly payment policies in effect during the 2014–2017 study period

	Arkansas		Indiana	Iowa	Michigan	Montana
	Health Care Independence Program	Arkansas Works	Healthy Indiana Plan (HIP) 2.0[a]	Iowa Health and Wellness Plan	Healthy Michigan Plan	Health and Economic Livelihood Partnership
Expansion date	Jan. 2014	n.a.	Feb. 2015	Jan. 2014	April 2014	Jan. 2016
Effective dates of monthly payments in the study period[b]	Jan. 2015–April 2016	Jan. 2017–Dec. 2017	Feb. 2015–Dec. 2017	Jan. 2015–Dec. 2017	Oct. 2014–Dec. 2017	Jan. 2016–Dec. 2017
Monthly payment amounts for income 0–100% FPL	$0	$0	0–5% FPL: $1 6%–100% FPL: 2% of income, equivalent to $1–$20[c]	0–49% FPL: $0 50%–100% FPL: $5	$0	0–49% FPL: $0 50%–100% FPL: 2% of income, equivalent to $10–$20[c]
Monthly payment amounts for income >100% FPL	>100%–115% FPL: $10 >115%–133% FPL: $15	2% of income, equivalent to $20-$27[c]	2% of income, equivalent to $20–$27 ($100 maximum)[c]	$10	2% of income, equivalent to $20-$27[c]	2% of income, equivalent to $20–$27[c]

[a] Payment amounts are for HIP Plus beneficiaries. HIP Basic beneficiaries do not make monthly payments.
[b] Start dates refer to the enrollment month in which monthly payments first became effective in each demonstration. Beneficiaries in Arkansas, Indiana, and Montana are subject to payments upon enrollment, whereas payments begin after 6 months of enrollment in Michigan and after 12 months of enrollment in Iowa. End dates listed as December 2017 refer to the end of the study period, although monthly payment requirements may have been ongoing in the demonstrations.
[c] This dollar estimate is calculated for a family of one using 2017 FPL ($12,060/year, or about $1,005 per month).
FPL = federal poverty level; n.a. = not applicable.

Arkansas. From January 2015 through April 2016,[30] Arkansas collected monthly payments through individual accounts known as Independence Accounts. The state set the monthly payment amounts at $10 for beneficiaries with incomes above 100 percent of the FPL up to 115 percent, and $15 for beneficiaries with incomes above 115 percent of the FPL. Beneficiaries who made payments to their Independence Account in one month could present their MyIndyCard at the point of service to cover all co-payments and co-insurance costs required by their qualified health plan (QHP) in the following month. Individuals with incomes above 100 percent of the FPL who did not make monthly payments were required to pay co-payments and co-insurance at the point of service, but did not lose Medicaid coverage. Arkansas closed the Independence Accounts in June 2016, citing the administrative costs of operating them. The state later received approval for a new set of demonstration policies under the name Arkansas Works, which is ongoing during the period from 2017 through 2021. The first year of Arkansas Works is included in our study period. Arkansas Works requires beneficiaries with incomes above 100 percent of the FPL to make monthly payments equal to 2 percent of income and pay cost-sharing at the

[30] These dates are based on information gathered from key informant interviews with state officials on July 8, 2015, and on August 16, 2016.

point of service regardless of whether they make monthly payments. The state is allowed to attempt to collect unpaid premiums, but there are no formal consequences for nonpayment.

Indiana. From 2015 through 2017, Healthy Indiana Plan (HIP) Plus beneficiaries with incomes above 5 percent of the FPL made monthly payments of 2 percent of their income into individual accounts called Personal Wellness and Responsibility, or POWER, accounts.[31] HIP Plus beneficiaries with incomes at or below 5 percent of the FPL paid $1 per month. After beneficiaries were determined eligible, they had 60 days to make their first monthly payment to enroll in HIP Plus. Adults with incomes above the poverty line who did not make a payment did not receive coverage—the first monthly contribution was a requirement for enrollment into HIP for this income group. In addition, adults with incomes above the poverty line who stopped making payments once they were enrolled in HIP Plus were disenrolled for six months. Those at or below the poverty line who did not make an initial payment, or who stopped making payments, were enrolled in a different coverage plan called HIP Basic, which required point-of-service co-payments and offered more limited benefits. HIP Basic beneficiaries could not be disenrolled for nonpayment, and they could move back to HIP Plus at the next renewal period if they resumed monthly payments. In 2018, Indiana changed monthly payment amounts from 2 percent of income to fixed amounts in four tiers ($1, $5, $10, and $15). The analyses in this report focus on enrollment during the period when amounts were 2 percent of income.

Iowa. Under Iowa's section 1115 demonstration authority during the 2014–2017 period, Iowa Health and Wellness Plan[32] beneficiaries with incomes at or above 50 percent of the FPL were required to make monthly payments beginning in the 13th month of enrollment if they did not engage in specified health behaviors. The monthly payment amount was $5 for beneficiaries with incomes between 50 and 100 percent of the FPL, and $10 for beneficiaries above 100 percent of the FPL. Beneficiaries who had an annual physician visit or dental wellness exam and completed an annual health risk assessment were exempted from monthly payments in the following year. At the beginning of each new enrollment year, there was a 30-day grace period for making the first payment or accomplishing the two health behavior goals.

Iowa expanded Medicaid in January 2014, and the first monthly payments were required in 2015 for beneficiaries who successfully renewed coverage. After a 90-day grace period, beneficiaries with incomes above 100 percent of the FPL who were required to make monthly payments because they did not engage in the two health behaviors could be disenrolled if they failed to make those payments, but they could re-enroll the following month. Beneficiaries whose income

[31] The maximum monthly POWER account contribution is $100.

[32] The Iowa Health and Wellness Plan comprised two different demonstrations during 2014 and 2015: the Iowa Wellness Plan and Marketplace Choice. Marketplace Choice was a premium assistance program that supported the purchase of QHPs by non-exempt beneficiaries with incomes above the federal poverty level. Marketplace Choice was effectively closed on December 31, 2015, although the state retained its authority to operate the program through December 2016. One of Iowa's two participating QHP carriers became insolvent in late 2014, and the other stopped accepting new Medicaid beneficiaries in 2015. The state received approval in January 2016 to modify eligibility for the Iowa Wellness Plan to include the population formerly enrolled in premium assistance. Although the care delivery mechanism changed, monthly payment requirements did not.

was between 50 and 100 percent of the FPL and who failed to make payments were not disenrolled; however, unpaid monthly payments could become a collectible debt. Beneficiaries in either of these income categories were able to request a hardship exemption and avoid both the payment and the consequences of nonpayment for any month.

Michigan. During the 2014–2018 Healthy Michigan Plan demonstration, beneficiaries with incomes above 100 percent of the FPL were required to make monthly payments of 2 percent of their income into individual MI Health accounts after the first six months of enrollment. Because Michigan's demonstration began in April 2014, the first monthly payment invoices were distributed after October 2014. Beneficiaries were billed quarterly, and payments could be made quarterly, but most beneficiaries made monthly installment payments. The state did not disenroll beneficiaries from the program for nonpayment, but was allowed to garnish state tax refunds for missed payments in excess of $50.

Montana. Beneficiaries enrolled in Montana's ongoing 2016–2020 Health and Economic Livelihood Partnership (HELP) demonstration who have incomes at or above 50 percent of the FPL are required to make monthly payments of 2 percent of their income upon enrollment. The state issues the first monthly payment bill in the first or second month of enrollment, depending on the exact enrollment date. HELP beneficiaries with incomes above 100 percent of the FPL can be disenrolled from active benefit status if they do not make monthly payments, although those meeting two or more exemption conditions, such as enrollment in college or in a substance use disorder treatment program, continue to receive benefits. The state provides a 30-day window after notice of overdue payment, followed by a 90-day grace period, after which benefits are suspended. Unpaid monthly payments can become a collectible debt for all beneficiaries with incomes at or above 50 percent of the FPL. Beneficiaries can resume active benefit status upon payment or when the debt is assessed.

B. Relationship between monthly payments and likelihood of enrollment in Medicaid

To assess the relationship between monthly payments and the enrollment decisions of eligible adults, we compared enrollment in Medicaid (or premium assistance programs) among likely eligible adults in states with and without payment requirements. In this section, we present analyses that address three subsidiary research questions (listed as Questions 1a, 1b, and 1d in Appendix Table A.2):

- Do eligible adults in states with required monthly payments enroll in Medicaid (or premium assistance programs) at the same rate as eligible adults in other states?
- Do eligible adults in key demographic groups who live in states with required monthly payments enroll in Medicaid (or premium assistance programs) at the same rate that eligible adults in other states do?
- How do monthly payment amounts affect take-up of coverage?

We used two analytical approaches to answer each question. Both approaches were designed to examine enrollment among the total population of non-disabled, non-elderly, non-dual–eligible adults that we estimated would be eligible for the expansion in each state, both before and after Medicaid expansions were implemented in demonstration and comparison states.

First, working with IPUMS-ACS data, we used cross-sectional time series models with a comparison group and state and year fixed effects to model the probability of Medicaid enrollment from 2012 to 2017. Although this is an analytically strong approach, it does not definitively establish causality. These models included two key explanatory variables and a set of demographic controls. The first key variable indicates whether an observation was from a state and year in which monthly contributions were required from any beneficiaries, whereas the second key variable reflects an individual's expected monthly contribution based on his or her estimated modified adjusted gross income (MAGI) and the state's required monthly contributions in that year. (Appendix D contains information on the population definition, model specifications, and descriptive statistics.)

Second, we conducted a descriptive analysis by constructing take-up rates for each state and each year from 2012 to 2017. These are annual estimates of the proportion of the likely eligible population enrolled in Medicaid for both demonstration and comparison states. Numerators were derived from state enrollment data or Medicaid Analytic eXtract (MAX), Alpha-MAX, and Transformed Medicaid Statistical Information System Analytic File (TAF) data, and denominators were derived from IPUMS-ACS data.

1. Regression models of Medicaid enrollment

a. Overall estimates

Table IV.2 presents estimates of the marginal difference in the probability of Medicaid enrollment when a person (1) lives in a state with a monthly payment requirement, or (2) owes monthly payments (by payment amount), controlling for demographic characteristics.[33]

The existence of a monthly payment amount was negatively associated with the probability of Medicaid enrollment—on average, the probability of enrollment was 0.8 percentage points lower in states with a monthly payment—even if a person was expected to owe no payments at all. This suggests a potential "chilling effect," whereby implementing a monthly payment requirement for some people reduces the probability of Medicaid enrollment for everyone in that state.

The negative association between the probability of enrollment and the payment amount was statistically significant at $p < 0.01$ for all monthly payment amounts except at an estimated payment amount of $11 to $20. The reduction in the probability of enrollment for those in the

[33] Table IV.2 presents average marginal effects, which are interpretable as the average across all marginal effects computed at each individual's values for all other variables in the model.

highest estimated payment category ($31 or more) is notably larger than it is for other payment amounts.

These estimates—for any payment and for the payment amount—are additive. For example, for an adult who is likely to be eligible, lives in a state with a monthly payment requirement, and is expected to have a $10 payment, the likelihood of enrolling in Medicaid was an estimated 5.5 percentage points lower than the enrollment rate in comparison states.[34,35]

Table IV.2 also presents the results of sensitivity tests that reveal a stronger negative relationship between monthly payments and enrollment for Indiana compared with other demonstration states, which we might expect given relatively strong payment enforcement mechanisms under HIP 2.0.[36] For example, the estimated reduction in the probability of enrollment for those residing in a state with payment requirements decreased from 0.8 percentage points in the full model to 0.6 when we excluded Indiana, and the coefficient was no longer statistically significant. Similarly, in a model based only on Indiana, the estimated reduction in the probability of enrollment for those with a $6–$10 monthly payment increased to 5.7 percentage points from 4.7 percentage points in the full model.

These sensitivity tests highlight an important feature of this analysis: the estimates in Table IV.2 are based on payment policies with a wide range of enforcement mechanisms. For example, it was largely optional to make monthly payments in Arkansas during the period when Independence Accounts were operational. If beneficiaries made the monthly payments, they avoided co-payments in the following month, but there were no consequences for nonpayment. In Indiana, however, beneficiaries could be disenrolled for six months for nonpayment.

These models partially control for unique state policies with state fixed effects, but not perfectly so. Because policy design and enforcement vary from one state to the next, these estimates of the relationship between monthly payments and Medicaid enrollment in the primary model underestimate the likely impact of monthly payments that are truly mandatory, as they were for some beneficiaries in Indiana. Likewise, these estimates overstate the likely impact of payments that are optional, like those implemented in Arkansas from 2015 to 2016.

[34] This estimated reduction of 5.5 percentage points in the probability of enrolling in Medicaid is the total of the marginal effect of residing in a state with a monthly payment requirement (a 0.8 percentage point reduction) plus the marginal effect of having a $10 monthly payment (a 4.7 percentage point reduction). The baseline enrollment rate is the percentage of the likely eligible population enrolled in Medicaid in expansion states without monthly payment requirements in 2014–2017.

[35] Estimated associations are robust to several alternative specifications and variable coding choices. For example, we obtained similar results from models that (1) truncated estimated monthly payment amounts at $75, and (2) considered Arkansas as having monthly payments in 2016 and Michigan as having monthly payments in 2014. In both states, monthly payments were in effect for fewer than half of these years: Arkansas's Independence Accounts closed in April 2016, and Michigan beneficiaries were not subject to monthly payments until October 2014. See Appendix D for a full description of these sensitivity tests.

[36] It might also be the case that differences in enrollment verification policies contribute to our results, although we have not found documentation indicating that Indiana's enrollment verification procedures are stricter than those of other states.

Table IV.2. Estimated relationship between monthly payments and the probability of reported enrollment in Medicaid among likely eligible adults

Model description/key variable	Average marginal effect (percentage points)	Standard error	*p*-value
Full model, comparing relationship between key explanatory variables and probability of enrollment			
Residing in state with monthly payments	-0.8**	0.4	.030
Estimated monthly payment amount:			
$1–$5	-2.1***	0.6	.001
$6–$10	-4.7***	0.8	.000
$11–$20	-0.2	0.7	.773
$21–$30	-2.9***	0.6	.000
$31+	-10.1***	0.7	.000
N = 68,733,280 (weighted)			
Model excluding Indiana			
Residing in state with monthly payments	-0.6	0.4	.127
Estimated monthly payment amount:			
$1–$5	1.8	1.3	.160
$6–$10	-5.1***	1.1	.000
$11–$20	-1.4	1.3	.310
$21–$30	-2.0***	0.7	.006
$31+	-9.4***	0.9	.000
N = 62,271,583 (weighted)			
Model with Indiana only[a]			
Estimated monthly payment amount:			
$1–$5	-5.1***	0.7	.000
$6–$10	-5.7***	1.0	.000
$11–$20	-1.7**	0.8	.030
$21–$30	-6.0***	0.9	.000
$31+	-12.7***	0.9	.000
N = 51,899,588 (weighted)			

Source: Mathematica analysis of Integrated Public Use Microdata Sample data from the American Community Survey, 2012–2017, for Arkansas, Indiana, Iowa, Michigan, and Montana (demonstration states); and Kentucky, Nevada, New Mexico, North Dakota, Ohio, Oregon, Pennsylvania, Washington, and West Virginia (comparison states).

Notes: Marginal effects for payment amounts shown in the table were estimated using logistic regression models and are relative to a $0 payment amount, the base category.
Control variables included Medicaid expansion status, state fixed effects, year fixed effects, age, sex, race, Hispanic/Latino ethnicity, education level, employment status, and indicators for disability and the presence of a child in the household.
The baseline enrollment rate for all models shown is 39.2, which is the percentage of the likely eligible population enrolled in Medicaid in expansion states without monthly payment requirements in 2014–2017.

[a] The variable reflecting residence in a state with monthly payments is not included in this model due to collinearity. (All observations for Indiana are coded as residing in a state with monthly payments and owing a payment amount.) All comparison states are included in this model along with Indiana as the only demonstration state.

**Significantly different from zero at the .05 level, two-tailed test.

***Significantly different from zero at the .01 level, two-tailed test.

b. Estimates by demographic subgroup

Table IV.3 presents the estimated relationship between having monthly payments of any amount and enrolling in Medicaid by demographic subgroup. (See Appendix D for regression specifications.) Estimates in Table IV.3 are based on the same likely eligible population used for estimates in Table IV.2. (The likely eligible population is described in Chapter II). Results in Table IV.3 reveal that the estimated negative relationship was true for all demographic subgroups: estimates for different subgroups were all statistically significant at $p < 0.01$.

We caution that estimates are not directly comparable across subgroups. For example, the estimated negative relationship between monthly payments and the probability of enrollment by men and women may not represent the true difference in the probability of enrollment based on gender, because we used two separate regression models to generate the estimates. However, larger estimated associations tend to correspond with higher baseline enrollment, suggesting that groups with higher baseline enrollment include some people who are more sensitive to price, and that groups with low baseline enrollment may primarily be composed of people with price-insensitive demand for health insurance. For example, the negative relationship between monthly payment requirements and enrollment was higher for people with disabilities than it was for people without them,[37] but people with disabilities also had a much higher enrollment rate at baseline (in the absence of a monthly payment policy). This pattern does not always hold; for example, unemployed adults have a baseline probability of enrollment that is similar to that of adults who are not in the labor force, but there is a stronger relationship between monthly payments and enrollment for those who are unemployed. This result suggests that unemployed adults are more sensitive to price than adults who are not in the labor force.

It is also worth noting that the estimates in Table IV.3 were negative and statistically significant across a range of demographic groups that might be subject to various policy interventions. For example, both employed and unemployed individuals were less likely to enroll if it meant owing monthly payments.

[37] Because people with a disability who receive Social Security Income were excluded from the eligible population, disability status as operationalized here does not reflect Medicaid eligibility status. Disabilities as operationalized here are based on self-reports in the ACS of having one or more of the following conditions: cognitive difficulty, ambulatory difficulty, difficulty with independent living, difficulty with self-care, difficulty seeing, or difficulty hearing.

Table IV.3. Estimated relationship between owing monthly payments of any amount and the probability of reported Medicaid enrollment among likely eligible adults, by demographic group

Demographic group	Frequency in millions (%)	Baseline enrollment rate	Average marginal effect (s.e.)	*p*-value
Age				
19–26	27.8 (40.4)	20.6	-1.5*** (0.6)	.006
27–35	13.3 (19.3)	42.0	-7.2*** (0.9)	.000
36–45	10.3 (15.0)	42.7	-8.7*** (1.0)	.000
46–55	9.5 (13.8)	40.1	-6.4*** (1.0)	.000
56–64	7.8 (11.4)	36.9	-7.9*** (1.0)	.000
Sex				
Female	35.2 (51.2)	37.8	-4.7*** (0.5)	.000
Male	33.5 (48.8)	27.2	-4.9*** (0.5)	.000
Race				
White	51.4 (74.7)	30.7	-4.5*** (0.4)	.000
Black	10.3 (15.0)	40.8	-6.9*** (1.1)	.000
Other	7.1 (10.3)	34.7	-6.3*** (1.5)	.000
Ethnicity				
Hispanic	6.6 (9.6)	35.9	-5.3*** (1.7)	.002
Non-Hispanic	62.2 (90.4)	32.3	-4.9*** (0.4)	.000
Education				
Less than high school	9.2 (13.4)	46.0	-6.8*** (1.2)	.000
High school	31.6 (46.0)	35.5	-5.1*** (0.6)	.000
Some college	20.9 (30.4)	26.4	-4.3*** (0.6)	.000
4+ years of college	7.0 (10.2)	20.8	-3.5*** (1.0)	.000
Employment status				
Employed	31.6 (46.0)	25.9	-4.4*** (0.5)	.000
Unemployed	8.5 (12.4)	36.6	-8.9*** (1.3)	.000
Not in labor force	28.6 (41.6)	38.9	-4.7*** (0.6)	.000
Children in household				
Yes	18.5 (26.9)	51.4	-8.7*** (0.8)	.000
No	50.3 (73.1)	25.7	-3.4*** (0.4)	.000
Disability				
Yes	12.3 (17.9)	51.5	-6.0*** (0.9)	.000
No	56.5 (82.1)	28.5	-4.6*** (0.4)	.000

Source: Mathematica analysis of Integrated Public Use Microdata Sample (IPUMS) data from the American Community Survey, 2012–2017, for Arkansas, Indiana, Iowa, Michigan, and Montana (demonstration states); and Kentucky, Nevada, North Dakota, Ohio, Oregon, Pennsylvania, Washington, and West Virginia (comparison states).

Notes: Marginal effects for payment amounts shown in the table were estimated using logistic regression models and are relative to a $0 payment amount, the base category.

Control variables included Medicaid expansion status, state fixed effects, year fixed effects, age, sex, race, Hispanic/Latino ethnicity, education level, employment status, and indicators for disability and the presence of a child in the household. We excluded sociodemographic variables in models segmented by categories for that variable. For example, we did not include age as a covariate in regressions segmented by age group.

The baseline enrollment rate is the percentage of the likely eligible population enrolled in Medicaid in expansion states without monthly payment requirements in 2014–2017, shown separately for each demographic group.

***Significantly different from zero at the .01 level, two-tailed test.

s.e. = standard error.

c. Limitations of estimation approach

Estimates of the relationship between Medicaid enrollment and monthly payments and payment amounts have several limitations. As noted, key features of monthly payment policies, including enforcement mechanisms, are not reflected in the analyses. Also, several demonstration states coupled monthly payment requirements with opportunities for beneficiaries to avoid paying them. Three states implemented incentives for healthy behaviors that, once fulfilled, waived (Iowa) or reduced (Indiana and Michigan) monthly payments. To the extent that beneficiaries understood these incentives, the ability to reduce or waive payments could attenuate the relationship between monthly payments and enrollment. In addition, states with section 1115 authority to require monthly payments of Medicaid beneficiaries adopted other alternative expansion approaches that could have affected the decision to enroll. For example, Arkansas and Iowa supported Medicaid enrollment by enrolling some or all newly eligible beneficiaries in QHPs, an expansion approach that could have affected some beneficiaries' enrollment decisions independent of payment policies.[38]

Another limitation of the regression models is that national household surveys are known to undercount enrollment in Medicaid, causing measurement error in our dependent variable. People who respond to the ACS, on which IPUMS is based, do not always report their coverage status accurately, although the estimated degree of error compares favorably with that of other national surveys (Boudreaux et al. 2015). In recent work, Boudreaux and colleagues found that the Medicaid undercount is more pronounced for Medicaid expansion states than non-expansion states. They estimated an "expansion effect" of 9.2 percentage points over the 2010–2016 period, with a 10.64 percent undercount in expansion states in 2016 versus a 0.02 percent undercount in non-expansion states in the same year (Boudreaux et al. 2019). However, the authors found similar trends in the undercount in four section 1115 demonstration states—Arkansas, Indiana, Iowa, and Michigan—compared to all states that expanded coverage in 2014. If the expansion effect is similar for demonstration and comparison states, it is unlikely to be a significant source of bias in our estimates of the relationship between monthly payments and enrollment.

2. Take-up among the likely eligible population

The take-up rates in this section are unadjusted annual estimates of the proportion of the likely eligible population enrolled in Medicaid. As described in Chapter II, take-up rates are enrollment ratios and not regression estimates, and thus do not control for differing population characteristics over time or across states. The numerator for each state is the average number of non-disabled, non-dual–eligible, non-elderly adult beneficiaries enrolled in a year.[39]

[38] Iowa's premium assistance demonstration operated from January 1, 2014, through December 31, 2015. Beneficiaries enrolled in QHPs transitioned to the Iowa Wellness Plan demonstration in January 2016.

[39] Due to data limitations, the numerator for Arkansas's take-up rates for 2014 through 2017 include the expansion population only. We used data from Arkansas's All-Payer Claims Database as the source for all analyses of Arkansas administrative data due to likely enrollment errors in Arkansas's TAF submissions. We did not obtain data from the All-Payer Claims Database for adults in eligibility groups other than the expansion group. However, (continued)

Denominators are estimates of the total eligible population in each state, based on income eligibility levels following expansion. They are computed using IPUMS-ACS data.

a. Take-up rates by state

Table IV.4 shows take-up rates by state from 2012 to 2017. Shaded cells indicate estimates for demonstration states that had monthly payment requirements in effect for all or part of the year.

Table IV.4 illustrates several patterns worth noting. First, there was increased take-up in all states the year after they expanded Medicaid. The exact magnitude of this jump varied from one state to the next. Among demonstration states, the largest jump in the take-up rate was in Arkansas: from 0.06 to 0.42 in the first post-expansion year; Arkansas had one of lowest income eligibility thresholds for adults prior to expansion.

Second, take-up rates for demonstration states in the first year post-expansion remained lower than take-up rates for a majority of comparison states, although not all demonstration states required monthly payments in the first year. Take-up rates for demonstration states in the first year post-expansion ranged from 0.29 in Indiana to 0.48 in Montana. For Arkansas and Iowa, the 2014 rates represent post-expansion take-up before any monthly payments were required. Michigan's 2014 take-up rate reflects enrollment both before and after some beneficiaries began making monthly payments in October of that year.[40] Indiana and Montana are the only demonstration states that expanded Medicaid and implemented monthly payment requirements at the same time, in 2015 and 2016, respectively. The rates for comparison states in their first years post-expansion ranged from 0.38 to 0.58, although three of the five states had take-up rates of over 0.5, and the rate for a fourth, Ohio, was 0.49. The rate in Pennsylvania in its first post-expansion year (2015) was an outlier among the comparison states.[41]

Third, Table IV.4 reveals that take-up continued to increase in Arkansas, Iowa, and Michigan into 2015, when monthly payment requirements were in effect for the full year in each state. Likewise, take-up continued to grow in Indiana and Montana in the second year post-expansion. However, of the demonstration states, only Montana had a take-up rate in its second year post-expansion that was comparable to the majority of comparison states in their second years. The increase in take-up in Iowa from the first year post-expansion to the second, when monthly payments took effect, was smaller than for all other states. In 2016, Indiana had a lower take-up

Arkansas had relatively low income limits for non-disabled adults before expanding coverage (as reflected by low take-up rates in 2012 and 2013), suggesting that the take-up rate for the entire non-disabled, non-dual–eligible, non-elderly population would only be slightly higher than the take-up rate for the expansion population in 2014–2017.

[40] Michigan's take-up rate in 2014 reflects enrollment in the second half of the year; the state expanded Medicaid in April, and we exclude each state's first three expansion months in our estimates to reflect steady-state enrollment.

[41] Pennsylvania's implementation experience was also different from that of other comparison states included here; the state first expanded coverage via a section 1115 demonstration in January 2015, but a new gubernatorial administration transitioned the demonstration to a traditional expansion later in the same year. (See https://www.kff.org/medicaid/fact-sheet/medicaid-expansion-in-pennsylvania/.)

rate than all other states. Take-up rates increased again in 2017 for all states other than Ohio and West Virginia, where they remained constant.

Table IV.4. Take-up rates by state among adults likely eligible according to income eligibility levels following expansion

Expansion date	State	2012	2013	2014	2015	2016	2017
Demonstration states							
January 2014	Arkansas	0.06	0.06	**0.42[a]**	**0.51*[a]**	**0.62*[a,b]**	**0.65*[a]**
	Iowa	0.31	0.32	**0.45**	**0.50***	**0.53***	**0.57***
April 2014	Michigan	0.20	0.21	**0.41***	**0.52***	**0.57***	**0.63***
February 2015	Indiana	0.15	0.14	0.11	**0.29***	**0.38***	NA[c]
January 2016	Montana	0.07	0.07	0.08	0.13	**0.48***	**0.66***
Comparison states							
January 2014	Kentucky	0.11	0.11	**0.53**	**0.68**	**0.74**	**0.82**
	New Mexico	0.23	0.21	**0.58**	**0.74**	**0.80**	**0.82**
	Ohio	0.28	0.28	**0.49**	**0.63**	**0.68**	**0.68**
	West Virginia	0.11	0.12	**0.54**	**0.62**	**0.60**	**0.60**
January 2015	Pennsylvania	0.17	0.16	0.15	**0.38**	**0.46**	**0.53**

Source: Mathematica analysis of take-up estimates, calculated using administrative data to arrive at average monthly enrollment in the numerator and Integrated Public Use Microdata Sample (IPUMS) data to estimate likely eligible population based on income guidelines following coverage expansion in the denominator.

Notes: Take-up estimates in expansion years exclude the first three months post-expansion. For Michigan and Indiana, expansion occurred midyear, so take-up estimates in 2014 (for Michigan) and 2015 (for Indiana) also exclude months that were before the expansion.
Bolded numbers indicate post-expansion year.

*Shaded estimates indicate that the state required monthly payments for all or part of the year.

[a] Numerator includes the expansion population only.

[b] Arkansas stopped asking beneficiaries to make monthly payments in April 2016 and implemented a new monthly payment policy in January 2017.

[c] Take-up estimate is not available because we did not include Indiana's 2017 TAF in the analysis.

b. Take-up by demographic subgroup

Next, we constructed take-up rates by demographic subgroups to determine whether there are patterns in enrollment by age, sex, and race/ethnicity. Take-up rates by demographic subgroups do not reveal a clear relationship between monthly payments and enrollment (see Figures F.1–3 in Appendix F).

Take-up was higher for women than for men in all states and in all years (Figure F.1). These differences by sex were similar before and after expansions were implemented.

Take-up rates were highest among people ages 27–35 and 36–45 in most states for most years (Figure F.2). Differences between age groups in their take-up rates increased post-expansion. In 2015, for example, take-up by age groups in Montana ranged from 0.02 (for people ages 56–64) to 0.26 (for people ages 36–45), a difference of 0.24. In 2017, the range was 0.42 (for those ages

19–26) to 0.96 (for both people ages 26–35 and those ages 36–45), a difference of 0.54. These trends do not appear to be associated with the presence of premiums.

Take-up rates for race/ethnicity (Figure F.3) also do not show a clear pattern. We excluded Arkansas, Iowa, Kentucky, New Mexico, and West Virginia because of the poor quality of the race and ethnicity variable in the available administrative data. Of all the race/ethnicity groups in Michigan, Montana, and Ohio, take-up in the "other" race/ethnicity category increased at the fastest rate. Several states had a high percentage of beneficiaries with unknown race/ethnicity that we categorized as "other" for the purpose of this analysis.

c. Limitations of take-up estimates

A key limitation of the take-up estimates is that Medicaid eligibility data were drawn from different sources in different states and, in the case of Arkansas and Iowa, from different sources for different years. We obtained data directly from Arkansas for 2014 through 2017 and from Iowa for 2014 and 2015. We used MAX, Alpha-MAX, and TAF for all other states and years (see Table C.1 in Appendix C for administrative data sources by state and year). As a result, we received data in five different formats. We developed coding procedures to standardize these formats as much as possible, but there could be underlying differences in reporting that introduce variation in the number of enrolled non-disabled, non-elderly, non-dual–eligible adult Medicaid beneficiaries from one state to the next. A similar limitation is that states use different processes for coding race and ethnicity, making it more challenging to compare take-up by these demographic subgroups. This is true even for the states for which we used MAX, Alpha-MAX, and TAF data, because states must cross-walk information maintained in their eligibility systems—which often contain different configurations of race/ethnicity categories—into the data submission formats from which these data sources are constructed.

In addition, possible error in the survey-based estimates of the likely eligible population for some states or demographic groups, coupled with possible reporting error in the Medicaid administrative data, can result in implausible take-up estimates of over 1.0 in some instances, as in the case of race/ethnicity estimates presented in the appendix.

3. What have other studies found?

There are few studies of the effects of monthly payments on adults' Medicaid enrollment. More common are studies focusing on children or on disenrollment by adults and children. (We review findings on enrollment continuity in Section IV.C.) The most relevant studies of adult enrollment used Current Population Survey data to model the effect of premiums on the probability of public, private, and no coverage among childless adults with low incomes (Guy et al. 2012) and parents with low incomes (Guy et al. 2017). Using data from 2000 to 2013, the latter study found that a $500 annual increase in public premiums (equivalent to $41.67 per month) was associated with a 1.9 percentage point reduction in the probability of parents having public insurance. In households where no one was employed, a $500 annual increase in public premiums resulted in a 9.8 percentage point reduction in the probability of parents having public insurance. This finding

echoes our estimate of the relationship between the highest premium amount category and adult enrollment in Medicaid, which was based on our analysis of IPUMS-ACS data. Another recent study focused on adult enrollment in Indiana, using data from the 2009–2016 waves of the American Community Survey (Freedman et al. 2018). In findings similar to the results of our take-up analysis, the authors reported that coverage did not increase as quickly in Indiana as it did in other expansion states in the region, but noted that some expansion states outside the Midwest had even smaller gains in coverage.

Studies evaluating the effect of monthly payments in the Children's Health Insurance Program (CHIP) have also found that higher payment amounts are associated with a lower probability of enrollment in Medicaid or CHIP (Abdus et al. 2014; Nikolova and Stearns 2014; Gresenz et al. 2013; Hadley et al. 2006). To the extent that the coverage decisions parents make for their children are based on price, we can draw a parallel to the adult expansion beneficiaries, who are the focus of our evaluation. For example, Abdus and coauthors (2014) analyzed 1999–2010 Medical Expenditure Panel Surveys (MEPS) data and found that a $10 increase in monthly payments was associated with a 6.7 percentage point reduction in Medicaid or CHIP coverage for children with family incomes from 101 to 150 percent of FPL. This estimate is similar to our regression-adjusted estimates based on IPUMS-ACS data. In another recent study, researchers analyzed MEPS data and found that monthly payment increases of $1 decreased the probability of CHIP enrollment by 1.4 to 2.1 percentage points, depending on family income (Nikolova and Stearns 2014). Hadley and colleagues (2006), using Community Tracking Study data from 1996 to 2003, estimated that imposing an annual premium of $120 and increasing existing premiums by $120 (or $10 per month) would decrease public coverage among children by 3.1 percentage points, a slightly lower estimate than our estimates from the IPUMS-ACS analysis.

C. Relationship between monthly payments and continued and renewed enrollment

We used administrative data to conduct a variety of descriptive analyses to assess the effects of monthly payments on continuity of coverage among adults enrolled in the Medicaid expansion group. Analyses in this section address five distinct research questions (listed as Questions 2a–2e in Appendix Table A.2):

- Is there a relationship between midyear disenrollments and the timing of monthly payment policies?
- Is there a relationship between monthly payment requirements and renewals?
- What is the effect of payment enforcement rules such as non-eligibility periods before re-enrollment?
- Is there a relationship between monthly payment requirements and long-term enrollment continuity?
- Is there a relationship between monthly payment requirements and enrollment duration?

To assess the relationship between **midyear disenrollments** and the timing of monthly payments, we constructed midyear disenrollment rates for adult expansion beneficiaries (Section IV.1.a, below) and estimated state-specific regression models to compare enrollment outcomes at specific policy-relevant time points (Section IV.1.b). We also calculated midyear disenrollments for nonpayment in Indiana (Section IV.1.c).

To assess the effects of monthly payments on **renewals after 12 continuous months of enrollment**, we estimated state-specific regression models to compare the probability of renewing into expansion eligibility groups, renewing into a different Medicaid eligibility category, and not renewing Medicaid coverage at all (Section IV.2).

To assess the effect of payment enforcement, we conducted a descriptive analysis of **non-eligibility periods** as a consequence of nonpayment, using administrative data from Indiana (Section IV.3).

We also used descriptive regressions to examine **enrollment continuity over multiple years.** This was designed to assess the relationship between monthly payments and long-term Medicaid enrollment (Section IV.4). We used an accelerated failure time model for a survival analysis of the **duration of continuous enrollment** (Section IV.5).

Each of these analyses is based on individual-level state administrative data on adult expansion beneficiaries who began an enrollment spell in 2014, 2015, 2016, or 2017. Appendix C contains model specifications and descriptive statistics for each state.

1. Continuous enrollment within potential 12-month spans

a. Proportion of adult expansion beneficiaries disenrolled midyear

To assess the relationship between monthly contributions and enrollment continuity among adults enrolled in the Medicaid expansion group, we first examined the proportion of adult expansion beneficiaries in their first enrollment year who disenrolled before they reached 12 months of continuous enrollment in each state (Table IV.5). To construct these proportions, we used the number of adults enrolled for at least two months for whom it was possible to observe 12 months of potential enrollment. Iowa serves as a point of comparison for the first span because monthly payments were not required until the second enrollment year.

The results in Table IV.5 reveal that, on average, the proportion of beneficiaries who disenrolled before the end of the first enrollment year was similar in the demonstration states and the comparison states (group-level averages of 29 percent versus 28 percent), and there was no clear relationship between monthly payment policies and midyear disenrollments. There was a high degree of between-state variability in disenrollment proportion, with both the lowest and highest first-year disenrollment rates in the demonstration states: 13 percent for Arkansas and 38 percent for Michigan. Kentucky had a disenrollment rate resembling that of Arkansas. Pennsylvania, a comparison state, had a disenrollment rate comparable to Michigan's and Indiana's.

In later spans, demonstration states did not have consistently higher or lower disenrollment rates than comparison states did. In fact, the highest and lowest disenrollment rates in the second, third, and fourth spans were found in comparison states, with the demonstration states generally occupying places in the middle of the distribution. Two states, Arkansas (a demonstration state) and Pennsylvania (a comparison state), had notable decreases in enrollment continuity from the third to the fourth span. In Arkansas's case, several issues with the state's eligibility system caused a backlog in renewal processing and income verification from 2012–2015; the state did not process renewals regularly until 2017.[42] It is possible that the reduction in enrollment continuity from the third to the fourth span is related to the fact that the state processed relatively more renewals later in the study period.

A limitation of this analysis is that any relationship between monthly payments and disenrollment rate that might exist could be obscured by limitations in the data that prevent us from distinguishing those who did and did not owe monthly payments in demonstration states. Arkansas and Michigan collected payments only from beneficiaries with incomes above 100 percent of the FPL, and Iowa and Montana only from those with incomes above 50 percent of the FPL.

[42] Communication with CMS staff in the Children and Adults Health Programs Group, January 7, 2020. See also a fact sheet on this issue (Arkansas Center for Health Improvement 2015) at https://achi.net/wp-content/uploads/2018/01/Medicaid-Eligibility-Redetermination.pdf or Arkansas's summative evaluation report for the Arkansas Health Care Independence Program (Arkansas Center for Health Improvement 2018) at https://www.medicaid.gov/Medicaid-CHIP-Program-Information/By-Topics/Waivers/1115/downloads/ar/Health-Care-Independence-Program-Private-Option/ar-works-private-option-summative-eval-20180630.pdf.

Table IV.5. Proportion of adults disenrolled midyear, by enrollment span and by state

Span		Demonstration states					Comparison states				
		Arkansas	Indiana	Iowa	Michigan	Montana	Kentucky	New Mexico	Ohio	Pennsylvania	West Virginia
1	Number with at least 12 months of potential enrollment	458,864	461,243	310,058	1,223,699	89,966	703,096	407,332	1,160,564	1,161,318	321,541
	Average number of months in first span	11.2	9.3	10.5	9.8	10.3	10.8	10.2	10.8	9.8	10.2
	Proportion disenrolled before 12 months (%)	13.0	37.3	29.0	38.2	28.1	17.5	29.9	20.2	37.9	32.2
2	Number with at least 12 months of potential enrollment	340,415	12,714	162,658	648,213	28,997	484,599	271,794	720,357	540,936	190,369
	Average number of months in second span	11.0	10.7	9.9	10.2	10.8	11.5	10.4	10.7	9.7	10.1
	Proportion disenrolled before 12 months (%)	17.9	25.2	34.1	31.2	19.9	8.5	27.8	22.6	36.8	31.7
3	Number with at least 12 months of potential enrollment	218,862	57	70,319	286,665	95	339,422	153,986	367,953	115,911	100,801
	Average number of months in third span	11.0	10.1	10.4	10.5	10.8	11.6	10.6	10.8	10.0	10.5
	Proportion disenrolled before 12 months (%)	19.0	31.6	26.7	26.0	20.0	8.1	24.7	22.3	32.2	25.5
4	Number with at least 12 months of potential enrollment	123,459	NA	20,675	21,170	n.a.	89,724	43,809	77,614	528	33,901
	Average number of months in fourth span	9.9	NA	10.7	10.3	n.a.	11.7	10.4	10.7	9.0	10.8
	Proportion disenrolled before 12 months (%)	31.0	NA	21.7	28.9	n.a.	6.1	29.6	23.3	48.5	21.9

Source: Mathematica analysis of administrative data from 2014–2017. Years included for each state depend on the date of demonstration implementation or coverage expansion. 2017 data for Indiana were not included.

Notes: Although Iowa's demonstration required monthly payments, Iowa serves as a comparison state for the first span because monthly payments were not required until a beneficiary had successfully renewed coverage after 12 months of enrollment.

There were no fourth spans for which it was possible to observe at least 12 months for Montana, which implemented its demonstration in January 2016 and had two years of observable data. Three spans were possible to observe in a two-year period if one or both of a person's first two spans were shorter than 12 months. There were also no fourth spans for Indiana because we did not include Indiana's 2017 data in the analysis.

NA = data not available; n.a. = not applicable.

b. Probability of continuous enrollment within potential 12-month enrollment spans

Next, we modeled each of three midyear enrollment outcomes separately for each state, which allowed us to closely examine whether disenrollment patterns might be related to the timing of monthly payment policies. We estimated enrollment continuity at 3 months after initial enrollment because monthly payments were due in the second month in Arkansas[43] and Montana. (In Indiana, payments were required to complete enrollment.) We estimated enrollment continuity at 8 months as well. Invoices were distributed in Michigan in the 7th month. In Montana, disenrollment for people with incomes above 100 percent of the FPL who did not make payments occurred at 7 months.[44] We also estimated enrollment continuity at 12 months, for comparison with 3- and 8-month continuity and with the proportions disenrolled mid-span in Table IV.5. As was the case in the previous analysis, Iowa serves as a point of comparison for the first span because payments were not required until the second enrollment year. Appendix C contains model specifications and descriptive statistics for each state.

Table IV.6 presents the predicted probability of enrollment at 3, 8, and 12 months for each state among beneficiaries enrolled in the Medicaid expansion, by enrollment span. Enrollment continuity at 3 months was high (over 90 percent) for the first span in all states but Indiana, where, controlling for population characteristics, only 84.1 percent of beneficiaries were still enrolled at 3 months. There was a noticeable decrease in the probability of continued enrollment between 3 and 8 months in all states, although the magnitude of the decreases varied and does not appear related to payment policies. The largest decrease, of 20.6 percentage points, occurred in Pennsylvania, a comparison state.

We note that the probability of continued enrollment at each milestone in the first span in Montana, a state with monthly payments, is similar to the probability at each milestone in Iowa, a state where monthly payments were not in effect in the first span. Enrollment continuity is slightly higher in Montana in the second span. Montana is the only state in this study that had a 12-month continuous eligibility policy for adults, meaning that income fluctuations would not cause people to be disenrolled within an enrollment year. It is possible that the continuous eligibility policy reduces the mid-span disenrollments that might otherwise occur and that an effect of monthly payments would be more apparent in the absence of that policy.

In the second and third spans, for all demonstration states but Iowa, we would expect that those who made monthly payments in an earlier span and who renewed coverage might be less likely

[43] This was the timing for Independence Account payments from 2015 to 2016. We do not have information on the timing of invoices and payments for the Arkansas premiums that began in 2017. There were no monthly payments in Arkansas in 2014 or May–December 2016.

[44] The special terms and conditions for Montana's demonstration state that people with incomes above 100 percent of the FPL may be disenrolled for nonpayment "after appropriate notice and a 90-day grace period." In practice, the state issues the first monthly payment bill in the first or second month of enrollment, provides a 30-day window after notice of overdue payment, and then applies the 90-day grace period. Beneficiaries are then flagged for disenrollment from active benefit status in the seventh month, with suspension of benefits following in the next month. The first disenrollments from active benefit status took effect in July 2016.

to separate from Medicaid because of payment obligations. Indeed, Table IV.6 shows that enrollment continuity at 3 months in the second span was over 90 percent for all states, including Indiana. Onset of monthly payment obligations in the second span in Iowa does not make a noticeable difference in the probability of remaining enrolled at 3 months compared to the same milestone in the first span. However, Iowa had the largest decrease in enrollment continuity at 8 months from the first span to the second span, at 7.2 percentage points. In the third span, the probability of remaining enrolled at all three time points was slightly higher than for the same time points in the second span for all comparison states, and in Iowa and Michigan. There were slight decreases in enrollment continuity in the third span for Indiana. Arkansas and Montana stayed about the same.

Estimates in Table IV.6 should be interpreted with caution because we did not have the necessary data to segment enrollment outcomes for people who did and did not owe monthly payments in demonstration states. In addition, these descriptive regressions omitted income. Another limitation of this analysis is that estimated probabilities are for first, second, or third spans that occurred at any point in the study period. As noted, Arkansas did not have monthly payments in 2014 or from May to December 2016. When we restricted the analysis of enrollment in Arkansas to first spans occurring in calendar year 2017, when monthly payments restarted under Arkansas Works, estimated continuous enrollment rates are noticeably lower at 3, 8, and 12 months (at 94.6 percent, 78.3 percent, and 69.1 percent, respectively) compared to any Arkansas spans shown in Table IV.6. Lower enrollment continuity in 2017 could be partly attributable to the monthly payment policy effective in that year. However, as noted in the discussion of mid-span disenrollments in the previous section, the state also had several eligibility systems issues that caused a backlog in renewal processing and income verification from 2012–2015, and did not process renewals regularly until 2017. Thus it is also possible that changes in eligibility systems over the study period contributed to the difference in observed enrollment continuity in 2017 compared to the entire study period.

Table IV.6. Descriptive regression analyses of enrollment retention among adults in Medicaid expansions

Probability of enrollment	Demonstration states					Comparison states				
	Arkansas	Indiana	Iowa	Michigan	Montana	Kentucky	New Mexico	Ohio	Pennsylvania	West Virginia
First span only										
At 3 months (%)	98.4	84.1	97.2	94.4	96.1	97.5	95.8	96.1	95.7	96.5
N	518,573	634,778	343,467	1,368,469	116,371	757,686	452,606	1,320,868	1,346,107	348,978
At 8 months (%)	90.3	69.9	83.8	75.5	80.2	85.4	79.7	86.1	75.1	80.6
N	488,989	548,967	326,163	1,288,135	102,730	727,622	427,205	1,230,610	1,251,161	334,103
At 12 months (%)	87.0	62.7	71.0	61.8	71.9	82.5	70.1	79.8	62.1	67.8
N	458,864	461,243	310,058	1,223,699	89,966	703,096	407,332	1,160,564	1,161,318	321,541
Second span only										
At 3 months (%)	98.0	97.2	96.2	95.2	97.4	99.0	96.1	96.9	94.3	95.5
N	390,522	238,960	192,165	792,330	59,503	536,229	318,991	848,653	769,889	218,330
At 8 months (%)	87.4	86.7	76.6	79.2	85.8	94.0	81.1	85.8	74.9	79.9
N	365,633	172,933	176,521	717,801	43,579	506,845	294,520	780,687	673,209	203,592
At 12 months (%)	82.1	74.8	65.9	68.8	80.1	91.5	72.2	77.4	63.2	68.3
N	340,415	12,714	162,658	648,213	28,997	484,599	271,794	720,357	540,936	190,369
Third span only										
At 3 months (%)	98.0	97.5	96.9	95.9	97.3	99.2	96.6	97.2	94.8	96.4
N	264,068	6,283	96,067	430,087	2,627	406,829	204,704	500,107	328,236	129,679
At 8 months (%)	88.6	83.4	81.3	82.8	87.2	94.9	82.5	86.1	77.9	83.2
N	238,691	688	82,579	353,124	475	373,796	180,298	433,556	223,656	114,695
At 12 months (%)	81.0	66.7	73.3	74.0	79.3	91.9	75.3	77.7	67.8	74.5
N	218,857	51	70,316	286,663	92	339,440	153,986	367,951	115,910	100,799

Source: Mathematica analysis of administrative data from 2014–2017. Years included for each state depend on the date of demonstration implementation or coverage expansion. 2017 data for Indiana were not included.

Note: Results are predicted probabilities from logistic regression models. Control variables included a flag for initial enrollment month, age, sex, and race/ethnicity (only available for Michigan, Montana, Ohio, and Pennsylvania). Appendix C includes full model specifications.

Although Iowa's demonstration requires monthly payments, Iowa serves as a comparison state for the first span because monthly payments are not required until a beneficiary has successfully renewed coverage after 12 months of enrollment.

c. Proportion of adults disenrolled for nonpayment

Next, we examined the proportion of adults in Indiana who were disenrolled for nonpayment of POWER account contributions, using Medicaid administrative data obtained directly from Indiana. Adults in HIP 2.0 with incomes above 100 percent of the FPL were subject to disenrollment as a consequence for nonpayment.

Table IV.7 presents the proportion of adults enrolled in their first, second, and third spans in HIP 2.0 who disenrolled before they reached 12 months of continued enrollment, overall and for nonpayment. As above, these proportions were constructed using the number of beneficiaries enrolled for at least two months for whom it was possible to observe 12 months of potential enrollment for a span. Analyses in Tables IV.5 and IV.6, which were based on TAF, excluded Indiana's 2017 data. The analyses in Table IV.7, using HIP data, cover a longer period, through January 2018, and allow us to observe as many as three full 12-month spans for a beneficiary. HIP 2.0 beneficiaries' first spans could start as early as February 2015 or as late as February 2017.

Table IV.7 shows that the overall proportion of HIP 2.0 beneficiaries with a midyear disenrollment was largest in the first span and decreased for each successive span. The table also shows that the proportion of spans that end midyear as a consequence of nonpayment was much smaller than the overall proportion of spans that end midyear, ranging from 1.8 to 2.8 percent. About half of the adults disenrolled midyear for nonpayment were disenrolled from HIP Plus and therefore subject to a six-month non-eligibility period; others who were disenrolled for nonpayment were HIP Basic members whose income increased and who did not make an initial monthly payment to enroll in HIP Plus. These beneficiaries could re-enroll at any time by making a payment. For example, 13,659 adults in their first HIP 2.0 span experienced midyear disenrollment for nonpayment and, of those, 8,502 were subject to a six-month non-eligibility period.

We note that when TAF data are used for analysis, the proportions of beneficiaries who disenrolled midyear (in Table IV.5) are higher than those based on analysis of data obtained directly from the state (in Table IV.7). For example, the proportion of midyear disenrollments in the first span based on TAF is 37.3, versus 26.4 based on data obtained directly from the state. These differences are attributable to differences in the underlying data and the fact that we use only 2015 and 2016 data for the TAF analysis.[45,46] We also note that Indiana is the only state for

[45] HIP 2.0 data include a small number of adults in HIP enrollment subgroups not included in the expansion adult eligibility group in TAF. After accounting for this group, 90.6 percent of monthly records are in both HIP 2.0 and TAF enrollment files. Differences in the available years for each data source have a relatively large effect on disenrollment estimates for the second and third spans because there are fewer of these spans in the TAF. When we limit the HIP 2.0 analysis to 2015 and 2016 only, making the time period more comparable to years available in TAF, the midyear disenrollment estimates from state data are slightly closer to those based on TAF.

[46] Monthly counts in TAF are less than 10 percent different than counts reported by the state in the Medicaid Budget & Expenditure System (MBES) for all included months. Counts in the state data met that threshold for all months in 2016 and 2017. Counts in the state data do not meet that threshold for the first three months of 2015, but are closer to MBES than the TAF counts from June to December 2015.

which we make a direct comparison of TAF and state data, but it is possible that there would be comparable differences for other states as well.

Table IV.7. Proportion of Healthy Indiana Plan 2.0 enrollees disenrolled midyear, overall and for nonpayment, by enrollment span

	First enrollment span	Second enrollment span	Third enrollment span
Total number of spans	585,716	303,278	77,300
Proportion (N) disenrolled midyear, any reason	26.4 (154,339)	21.4 (64,947)	17.5 (13,499)
Average number of months in span among those disenrolled mid-year, any reason	6.3	6.4	6.5
Proportion (N) disenrolled midyear for nonpayment	2.3 (13,659)	2.8 (8,412)	1.8 (1,417)
Nonpayment of PAC (HIP Plus only)	1.5 (8,502)	1.4 (4,175)	1.0 (761)
Increased income and nonpayment of PAC (HIP Basic only)[a]	0.9 (5,157)	1.4 (4,237)	0.8 (656)
Missing reason	3.3 (19,181)	2.9 (8,861)	1.3 (986)

Source: Mathematica analysis of Indiana HIP 2.0 data, February 2015–January 2018.

Note: Midyear disenrollments are defined as spans with at least two but fewer than 12 months of enrollment for which it was possible to observe 12 months of potential enrollment. Disenrollment reasons are from disenrollment records in the HIP 2.0 data that correspond with the last month of enrollment. In cases where there is not a disenrollment record at the end of the span, the disenrollment reason may be based on a disenrollment record in the month prior to, or the month after, the end of the span. Those in the "missing reason" category have fewer than 12 months of enrollment and a missing disenrollment record.

[a] HIP Basic enrollees whose income increases above 100 percent of the FPL are moved to HIP Plus if they contribute to their POWER account. Enrollees whose income increases above 100 percent of the FPL but do not make an initial contribution to their POWER account are disenrolled from HIP 2.0 without a non-eligibility period. Some enrollees who meet certain criteria, such as those who are medically frail, may continue in the program even though their income increased above 100 percent of the FPL and they did not contribute to their POWER account.

FPL = federal poverty level; HIP = Healthy Indiana Plan; PAC = Personal Wellness and Responsibility (POWER) account contribution.

2. Probability of renewal after 12-month spans

To examine renewal outcomes for beneficiaries who were continuously enrolled for 12 months, we estimated probabilities of renewed enrollment into the expansion eligibility group, renewal into a different Medicaid eligibility group, and no renewal. We estimated these probabilities separately for each state (Table IV.8). These estimates were based on 14-month observation periods; renewals were counted if they occurred in month 13 or 14.

Because we needed to observe 14 months of data for each enrolled adult, we observed only two re-enrollment periods for Montana, which implemented its demonstration in January 2016, and for Indiana, because we did not include Indiana's 2017 TAF in the analysis. For other demonstration and comparison states, we observed up to three re-enrollment periods. Iowa serves as a demonstration state for outcomes for all three spans, in contrast with previous analyses, because the payment requirement took effect after renewal after the first span. We could not observe enrollment into a different Medicaid eligibility group for Arkansas because we had data only for the expansion group, and not for all non-disabled, non-elderly adults in Medicaid. Estimates in Table IV.8 should be interpreted with caution because we did not have the

necessary data to segment renewal outcomes for people who did and did not owe monthly payments in demonstration states. In addition, these descriptive regressions omitted income, an unobservable variable in most states during the period included in these analyses. Appendix C contains model specifications and descriptive statistics for each state.

After all three spans, the majority of beneficiaries who remained enrolled for 12 months renewed into the expansion eligibility group, adjusting for beneficiary characteristics. Of the beneficiaries who did not re-enroll in the expansion, most failed to renew Medicaid coverage. A small proportion (fewer than 2 percent) renewed their Medicaid coverage, but in a different eligibility group. On average, rates of renewing coverage in the expansion group among demonstration states were 3.7 percentage points lower than those among comparison states after the first span and 4.5 percentage points lower after the second span. There was no noticeable difference between demonstration and comparison states after the third span, suggesting that those who remain enrolled through a third span value coverage highly, are accustomed to making monthly payments, or both. Beneficiaries with experience from three enrollment spans in demonstration states were unlikely to be affected by payment obligations going into their fourth. West Virginia, a comparison state, had the lowest re-enrollment rate after the first span, but Indiana, a demonstration state, had the lowest re-enrollment rate after the second.

Among demonstration states, Iowa, Indiana, and Michigan had noticeably lower renewal rates than Arkansas and Montana after the first span. These three states also had lower renewal rates than all comparison states other than West Virginia after the first span. It is possible that the relatively lower probability of renewing into the expansion group after the first span in Iowa, Indiana, and Michigan is related to monthly payment requirements. Iowa is the only state in which the timing of the first monthly payment coincided with renewal for beneficiaries who did not achieve the encouraged healthy behavior goals in the previous enrollment year. This pattern did not persist in later spans.

Iowa, Indiana, and Montana all applied disenrollment consequences to adults with incomes above 100 percent of the FPL who did not make monthly payments. However, the presence of payment enforcement in Montana did not appear to preclude high rates of re-enrollment among beneficiaries who reached 12 continuous months of enrollment—rates comparable to those in Arkansas, where monthly payments were essentially optional. The only consequence of not making Independence Account payments in Arkansas in 2014–2016 was that beneficiaries were required to pay cost-sharing at the point of service, and there was no consequence at all for nonpayment in 2017.

In addition, Arkansas delayed eligibility redeterminations that were initially scheduled for 2014 to mid-2015 due to a lengthy transition to a new eligibility system, with the result that many beneficiaries in their first spans were automatically enrolled for more than 12 months.[47] Likewise, it is also important to note that several comparison states had known eligibility systems issues during the study period. These issues caused delays in application and renewal

[47] See https://achi.net/wp-content/uploads/2018/01/Medicaid-Eligibility-Redetermination.pdf.

processing, which could have artificially inflated enrollment continuity for those who were enrolled.[48]

[48] Communication with CMS staff in the Children and Adults Health Programs Group, January 7, 2020. Comparison states with known issues include Kentucky, New Mexico, and Ohio. However, many states experienced eligibility systems issues during the study period even if there was no documented backlog in processing renewals. The precise effect of such eligibility systems issues on enrollment continuity in each demonstration and comparison state is unknown.

Table IV.8. Descriptive regression analyses of renewal outcomes among adults in Medicaid expansions

	Demonstration states					Comparison states				
Probability of renewal	Arkansas	Indiana	Iowa	Michigan	Montana	Kentucky	New Mexico	Ohio	Pennsylvania	West Virginia
After the first span										
Into the expansion group (%)	98.1	80.4	78.5	80.0	97.4	97.1	96.8	87.7	94.2	76.7
Into a different eligibility group (%)	NA	0.0	1.9	1.4	0.2	0.5	0.7	1.9	1.0	1.2
Not renewing (%)	1.9	19.6	19.6	18.6	2.5	2.9	2.5	10.4	4.8	22.1
N	387,049	386,496	212,336	727,033	56,711	557,223	276,206	897,085	684,232	211,848
After the second span										
Into the expansion group (%)	97.5	75.8	84.3	90.0	97.3	97.9	96.8	91.7	95.1	85.9
Into a different eligibility group (%)	NA	0.0	1.2	0.9	0.6	0.8	0.6	0.7	0.6	0.5
Not renewing (%)	2.5	24.2	14.5	9.1	2.6	2.1	2.6	7.6	4.3	13.6
N	263,973	4,718	101,424	412,481	2,146	425,629	187,598	527,923	303,376	124,106
After the third span										
Into the expansion group (%)	95.9	NA	93.8	93.8	n.a.	97.7	97.4	93.3	94.3	90.1
Into a different eligibility group (%)	NA	NA	0.7	0.6	n.a.	0.8	0.4	0.3	0.9	0.3
Not renewing (%)	4.1	NA	5.5	5.6	n.a.	2.1	2.2	6.5	4.8	9.6
N	164,919	NA	46,081	178,659	n.a.	288,191	106,031	257,526	13,211	68,687

Source: Mathematica analysis of administrative data from 2014–2017. Years included for each state depend on the date of demonstration implementation or coverage expansion. 2017 data for Indiana were not included.

Note: Results are predicted probabilities from logistic regression models. Control variables included a flag for initial enrollment month, age, sex, and race/ethnicity (only available for Michigan, Montana, Ohio, and Pennsylvania). Appendix C includes full model specifications.

Some outcomes were estimated using fewer than the total number of observations for a given span/year because the model excluded some observations due to lack of variation in outcomes within subgroups. For example, in some cases all beneficiaries who initially enrolled in the same month in a given state renewed into the same eligibility group, so these observations were dropped from the model.

NA = data not available; n.a. = not applicable given demonstration start date.

3. Effect of payment enforcement rules

To assess the relationship between enforced non-eligibility periods and gaps in coverage, we conducted a descriptive analysis using administrative data from Indiana. Adults enrolled in HIP Plus who failed to make a POWER account contribution within a 60-day grace period were disenrolled and subject to a six-month non-eligibility period, whereas HIP Plus and HIP Basic beneficiaries disenrolled for other reasons were not subject to the non-eligibility period.[49]

Table IV.9 presents HIP 2.0 enrollment activity following disenrollment, including the percentage of disenrollments after which beneficiaries did and did not re-enroll within 11 months, as well as the average number of months between spans for those who re-enrolled within this time frame. Table IV.9 shows these data points separately for each of eight disenrollment reasons—two of which are related to nonpayment of POWER account contributions—and for those that are missing a reason for disenrollment.

Table IV.9 shows that a majority of spans that ended in a disenrollment did not result in re-enrollment within 11 months—that is, they were lost to follow-up. Other than moving out of state, the disenrollment reason with the greatest loss to follow-up was disenrollment for nonpayment with a non-eligibility period. HIP Plus beneficiaries who were disenrolled for nonpayment and who subsequently re-enrolled also had the longest average gap between spans (7.0 months). However, the length of this gap indicates that most HIP Plus beneficiaries who did re-enroll within 11 months did so shortly after the non-eligibility period was over. Thus, beneficiaries who were disenrolled for nonpayment and subject to a six-month non-eligibility period tended to have one of two distinct outcomes—most failed to re-enroll for 12 months or longer, but some re-enrolled shortly after they regained eligibility.

[49] Adults who were verified as being medically frail or who experienced a qualifying event (that is, obtaining and subsequently losing private insurance coverage, loss of income after disqualification due to increased income, taking up residence in another state and later returning, being a victim of domestic violence, or residing in a county subject to a disaster declaration) could return to HIP before fulfilling the six-month non-eligibility period.

Table IV.9. HIP 2.0 enrollment activity following a disenrollment, by reason for disenrollment

	Percentage not re-enrolled within 11 months[a]	Percentage re-enrolled within 11 months	Average number of months between spans[b]
Disenrollments for nonpayment, with non-eligibility periods			
Nonpayment of PAC (HIP Plus only) (N = 15,713)	78.0 (12,262)	22.0 (3,451)	7.0
Disenrollments for nonpayment, without non-eligibility periods			
Increased income and nonpayment of PAC (HIP Basic only)[c] (N = 13,554)	72.1 (9,772)	27.9 (3,782)	5.5
Disenrollments for other reasons, without non-eligibility periods			
Increased income to over 133% FPL[d] (N = 115,626)	77.3 (89,366)	22.7 (26,260)	5.1
Did not submit paperwork for redetermination (N = 96,516)	67.0 (64,634)	33.0 (31,882)	5.0
Moved to a non-demonstration Medicaid category (N = 38,682)	61.4 (23,747)	38.6 (14,935)	4.8
Failure to verify information (N = 35,023)	67.6 (23,693)	32.4 (11,330)	5.0
Moved out of state (N = 28,526)	81.4 (23,228)	18.6 (5,298)	5.4
Other[e] (N = 24,240)	71.2 (17,255)	28.8 (6,985)	4.4
Missing reason (N = 42,063)	40.2 (16,929)	59.8 (25,134)	2.9

Source: Mathematica analysis of Indiana HIP 2.0 data, February 2015–September 2018. Spans included in this analysis end no later than October 31, 2017, to allow observation of the following 11 months.

Note: Analysis includes adults re-enrolling within 11 months after disenrolling, regardless of enrollment length. The 10-month gap in enrollment enables observation of gaps longer than the six-month non-eligibility period.

[a] Enrollees are considered lost to follow-up if they do not re-enroll in HIP 2.0 by the 11th month after disenrollment.

[b] Those returning after more than 11 months are not included because we restricted spans to those that are separated by minimum of 1 month and maximum of 10 months. We imposed this maximum so that we used consistent measurement of a potential re-enrollment window for the latest spans in our analysis. This likely underestimates the average number of months between spans that are separated by a gap.

[c] HIP Basic beneficiaries whose income increased above 100 percent of the FPL were moved to HIP Plus if they contributed to their POWER account. Beneficiaries whose income increased above 100 percent of the FPL and who did not make an initial contribution to their POWER account were disenrolled from HIP 2.0 without a non-eligibility period. Some beneficiaries who met certain criteria, such as those who were medically frail, could continue in the program even though their income increased above 100 percent of the FPL, and they did not contribute to their POWER account.

[d] The Affordable Care Act established a 5 percent income disregard that increased the effective income limit from 133 to 138 percent of the FPL.

[e] Other reasons for disenrollment include death, incarceration, and voluntary withdrawal.

FPL = federal poverty level; HIP = Healthy Indiana Plan; PAC = Personal Wellness and Responsibility (POWER) account contribution.

4. Long-term enrollment continuity

Next, we estimated the probability of remaining enrolled in a Medicaid expansion long-term, using all available years of data for each state. Although many beneficiaries "churn" between public and private health insurance or between insurance and non-insurance, some retain eligibility for several years. We studied the probability of remaining continuously enrolled for periods longer than a full year—18, 24, 36, and 48 months—for the first enrollment span in each state's Medicaid expansion. We used models similar to the ones we used to study continuous enrollment within a yearlong span (Section 1.b, and models and descriptive statistics in Appendix C). We can observe longer-term outcomes for states that implemented their expansions earlier in the study period.

Enrollment continuity beyond 12 months reflects patterns we observed in the above analyses of enrollment continuity within 12-month spans and of renewal (Table IV.10). There was wide variation across both demonstration and comparison states. Arkansas, a demonstration state, and Kentucky, a comparison state, had the two highest continuous enrollment rates at every milestone. Iowa and Michigan, both demonstration states, consistently had the lowest continuous enrollment rates, although enrollment rates in Pennsylvania and West Virginia were comparable to Iowa and Michigan at 24 and 36 months. It is possible that payment policies in Iowa and Michigan explain their relatively low long-term continuous enrollment rates. However, Montana, which required premiums and had a disenrollment consequence for nonpayment, had higher continuous enrollment rates. Overall, enrollment duration beyond 24 months was relatively infrequent in both demonstration and comparison states; fewer than 50 percent of beneficiaries remained continuously enrolled at 36 months in a majority of states for which we could observe this outcome.

Table IV.10. Descriptive regression analyses of long-term enrollment outcomes for adults in Medicaid expansions

Probability of enrollment	Demonstration states					Comparison states				
	Arkansas	Indiana	Iowa	Michigan	Montana	Kentucky	New Mexico	Ohio	Pennsylvania	West Virginia
At 18 months (%)	80.6	46.5	38.8	38.3	61.0	75.2	57.6	60.6	44.2	41.9
N	414,934	321,417	279,784	1,102,278	62,600	647,447	371,903	1,040,265	990,616	297,023
At 24 months (%)	71.9	NA	31.5	30.9	55.8	72.4	49.1	53.8	35.3	34.0
N	372,707	NA	252,092	981,176	35,864	603,400	334,798	918,109	775,520	271,868
At 36 months (%)	60.4	n.a.	19.7	21.0	n.a.	64.2	38.1	41.3	52.3	24.2
N	278,845	n.a.	172,443	615,281	n.a.	480,301	237,665	583,435	228,206	202,775
At 48 months (%)	42.4	n.a.	16.2	n.a.	n.a.	51.0	26.3	30.3	n.a.	20.5
N	190,420	n.a.	73,908	n.a.	n.a.	160,492	85,233	136,125	n.a.	89,672

Source: Mathematica analysis of administrative data from 2014–2017. Years included for each state depend on the date of demonstration implementation or coverage expansion. 2017 data for Indiana were not included.

Note: Results are predicted probabilities from logistic regression models. Control variables included a flag for initial enrollment month, age, sex, and race/ethnicity (only available for Michigan, Montana, Ohio, and Pennsylvania). Appendix C includes full model specifications.

NA = data not available; n.a. = not applicable given demonstration start date.

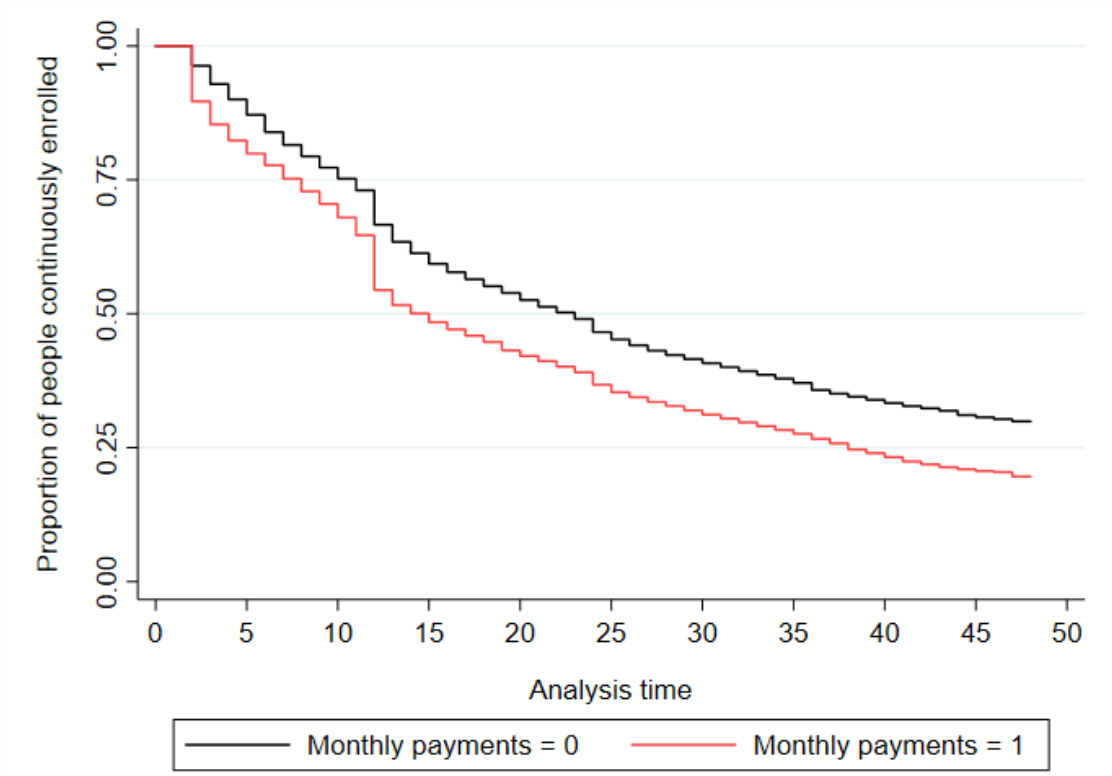
5. Survival analysis

Finally, we conducted a survival analysis of continuous enrollment, also using all available years of data for each state. In contrast with regression models that estimate whether an outcome took place, survival models estimate *when* the outcome took place. This approach enabled us to estimate the effect of monthly payments on the length of time a person remained enrolled ("survived") in the Medicaid expansion group before disenrollment, the event of interest. The survival function is the probability of remaining enrolled through the current month (month t). The probability is conditional on being enrolled the previous month (month $t-1$) and decreases over time. We compared survival functions for those who were estimated to owe monthly payments and those who were not on the basis of residing in a demonstration state and time enrolled—for example, a person in Michigan was estimated to owe a monthly payment beginning in Enrollment Month 6, and in Iowa in Enrollment Month 13. The functions can be adjusted to account for individual characteristics that change over time, including onset of monthly payment obligations and age, and characteristics that we do not observe changing over time, such as state of residence and sex.

We present the survival data in two ways, first with a visual representation of unadjusted probabilities and second with covariate-adjusted results from a regression model. Figure IV.1 plots the unadjusted probability of remaining enrolled for each month in the 48-month analysis time period, estimating the probability separately for those who are estimated to owe a monthly payment in a given month and those who do not. Month 0 is the time origin for the first Medicaid expansions, representing January 1, 2014. The probability of enrollment in each month thereafter reflects whether people in the data set remained enrolled at the end of the month; Month 48 is December 2017.

The figure shows two distinct points in time when the probability of staying enrolled drops at a faster rate for beneficiaries who were estimated to owe a monthly payment than it does for other beneficiaries. These time points for those who enrolled in the Medicaid expansions in January 2014 are (1) soon after initial enrollment and (2) at renewal after the first enrollment span. The decrease in the probability of remaining enrolled around Month 13 in the study period is consistent with our finding that Iowa—one of only two demonstration states that implemented its coverage expansion in January 2014—had a relatively low renewal rate after the first enrollment span. Except for these two points, the slopes of the two lines are similar, indicating that the probability of staying enrolled changes over time at the same rate for both groups. The probability of staying enrolled remains lower for those estimated to owe monthly payments throughout the rest of the study period.

Figure IV.1. Kaplan-Meier survival plot of enrollment



Source: Mathematica analysis of administrative data from 2014–2017 for Arkansas, Indiana, Iowa, Michigan, and Montana (demonstration states); and Kentucky, New Hampshire, New Mexico, Ohio, Pennsylvania, and West Virginia (comparison states). Years included for each state depend on the date of demonstration implementation or coverage expansion.

Because the survival probabilities in Figure IV.1 do not control for available covariates, we also estimated a multivariate accelerated failure time model with a generalized gamma distribution and state fixed effects.[50] Accelerated failure time models are less common than Cox proportional hazards models, but they are easier to interpret. The results in Table IV.11 take the form of time ratios, which reflect each variable's effect on continuous enrollment time—time ratios of less than 1.0 indicate shorter enrollment lengths. Because people in the data set could have more than one enrollment span, we used robust standard errors to adjust for clustering of observations by person.

Table IV.11 shows that people estimated to have a monthly payment had shorter enrollment lengths, controlling for other variables. The time ratio for monthly payments is 0.84, meaning that the median time to disenrollment among those with monthly payments is 84 percent of the median time to disenrollment among those without monthly payments. This estimate is statistically significant. All other covariates are statistically significant predictors of enrollment length. For example, men are likely to stay enrolled longer than women, and people in the youngest age group (19–26, the reference category) are likely to drop out sooner than older

[50] We estimated both accelerated failure time models and proportional hazards models and found consistent results.

people. We re-estimated the model without Indiana as a sensitivity check on the estimated effect of monthly payments. When we excluded Indiana, the effect of monthly payments on enrollment time remained negative. The time ratio increased slightly from 0.84 to 0.88 but remained statistically significant.

As with other analyses of administrative data in this chapter, the survival analyses presented here should be interpreted with caution because we did not have data on income. Without income data, we cannot distinguish those in each state who actually owed monthly payments from those who did not. For purposes of the analysis, we assumed that monthly payments started at the same point in an enrollment span for all beneficiaries in a demonstration state even if only some of those beneficiaries would have owed monthly payments. This generalization likely underestimates the actual effect of monthly payments on enrollment time. In addition, income is an important but omitted control variable in the accelerated failure time model, as is race/ethnicity. The quality of these variables in the TAF was poor for several states.

Table IV.11. Accelerated failure time model of enrollment duration in Medicaid expansion states, 2014–2017

Model variable	Time ratio (s.e.)	*p*-value
Monthly payment (reference is no monthly payment)	0.84 (0.00)***	.000
Age		
19–26	(reference)	
27–35	1.02 (0.00)***	.000
36–45	1.11 (0.00)***	.000
46–55	1.29 (0.00)***	.000
56-64	1.06 (0.00)***	.000
Sex		
Male	(reference)	
Female	0.96 (0.00)***	.000
Year		
2014	(reference)	
2015	1.09 (0.00)***	.000
2016	0.97 (0.00)***	.000
2017	0.95 (0.00)***	.000
N = 20,305,364		

Source: Mathematica analysis of administrative data from 2014–2017 for Arkansas, Indiana, Iowa, Michigan, Montana (demonstration states); and Kentucky, New Mexico, Ohio, Pennsylvania, and West Virginia (comparison states). Years included for each state depend on the date of demonstration implementation or coverage expansion.

Notes: Time ratios shown in the table were estimated using accelerated failure time models and are relative to omitted categories shown in parentheses. Controls include state (not shown).

***Significantly different from 1.0 at the .01 level, two-tailed test.

s.e. = standard error.

6. Limitations of analyses of enrollment continuity, disenrollment, renewals, and long-term enrollment

The most significant limitation of all analyses of enrollment continuity based on federal administrative data is that we are unable to distinguish between those who actually do and do not owe monthly payments in demonstration states because our data sources did not have reliable income data. Arkansas and Michigan collected payments only from those with incomes above 100 percent of the FPL, and Iowa and Montana only from those with incomes above 50 percent of the FPL. In addition, income is an important variable omitted in our models.

Another limitation is that several states in our analyses had documented eligibility systems issues that caused delays in processing applications, renewals, and changes in circumstance (for example, income changes) during our study period. Delays in processing renewals and changes in circumstance could have increased enrollment continuity beyond what it would have been if processing had occurred on time. For example, if a person's income exceeded income eligibility at renewal, they would normally not renew coverage, but delays in this determination would mean that they remain enrolled. The exact extent of these eligibility systems issues in each state, and their effect on enrollment continuity, is unknown. To the extent that more comparison states had delays in processing renewals than did demonstration states, lower enrollment continuity observed in states with premiums may be as least partly due to systems issues, but discussions with CMS staff did not suggest this concern was systematically more common among comparison states.

Another limitation, as noted in the discussion of take-up rates, is that the Medicaid administrative data we obtained from various sources for this analysis were not standardized in content or format. There could be differences in the file formats and our resulting data processing routines that affect estimates of the number of people enrolled in each Medicaid expansion program at different time points. In addition, models shown in Tables IV.6, IV.8, and IV.10 are all state-specific and include different control variables for different states, limiting the comparability of state estimates. For example, models for Arkansas, Iowa, Kentucky, New Mexico, and West Virginia do not include a race/ethnicity variable because the quality of the variable was poor in those states. Race/ethnicity is included for Montana, but is defined as "white, non-Hispanic" and "other/unknown" because there were not enough people in the "black, non-Hispanic" and "Hispanic/Latino" categories to create separate groups for analysis. However, we also estimated models that combined all states, including only the control variables that were the same across states, and found similar results.

A different challenge for these models of enrollment retention is that, because MAX and Alpha-MAX do not contain uniform eligibility codes for adult expansion beneficiaries, there may be some error in our identification of those beneficiaries in comparison states. We identified a set of the most likely eligibility codes for adult expansion beneficiaries (see Appendix Table C.3), but it is possible that states used additional eligibility categories for smaller numbers of adult expansion beneficiaries who are consequently missing in our data set. It is also possible that

these state-specific eligibility groups include some adults who were eligible before the state expansions. The TAF include eligibility codes for adult expansion beneficiaries, but not all expansion states are using these new codes consistently. We also used state-specific eligibility codes to impute eligibility codes for adult expansion beneficiaries whose data were missing in the TAF.

7. What have other studies found?

Other findings on enrollment continuity. Multiple single-state studies have examined the effect of monthly payments on enrollment continuity among adults with low incomes and children in low-income households. For example, Dague (2014) examined enrollment continuity in Wisconsin to assess the effect of premiums instituted in 2008 for adult beneficiaries with incomes above 150 percent of the FPL. Using a regression discontinuity model, the author found that a change from a monthly payment of $0 to $10 resulted in an estimated 1.3-month reduction in the length of enrollment. Results were similar for children. (There was no effect for most increases above $10, suggesting that owing any premium could be a bigger factor than the size of the monthly payment.) Although we do not model reductions in enrollment length in the same way, Dague's results are consistent with our finding that the predicted probability of enrollment at three months is lower for adult expansion beneficiaries in their first enrollment span in Indiana than in other states, and that Iowans in their second enrollment span (after monthly payment requirements took effect) were less likely to remain enrolled at eight months than they were in their first span (when monthly payments were not yet required). They are also consistent with our finding that Iowa and Michigan both had relatively low long-term continuous enrollment rates.

Dague's results are similar to findings from a number of studies focusing on enrollment continuity among children enrolled in CHIP. Marton (2007) and Herndon et al. (2008) conducted survival analyses of enrollment in single states and found that monthly payment requirements reduced enrollment lengths, although these effects were concentrated in the periods immediately after the payment policies were implemented. These results are generally consistent with the results of our survival analysis. Multi-state studies found that the introduction of premiums or premium increases were associated with higher probabilities of program exit (Kenney et al. 2006/2007; Kenney et al. 2007; Marton et al. 2010), although Kenney and colleagues (2006/2007) found no effect on disenrollment of a monthly payment requirement in Kansas, possibly because nonpayment did not result in program termination until eligibility recertification after 12 months of enrollment.

There are also a number of studies of the effects of a monthly payment change implemented in 2003 for Oregon Health Plan beneficiaries, although most of these studies are either qualitative or do not use strong comparison strategies. For example, Wallace and colleagues (2010) derived estimates from a simple pre-post research design and did not control for individual characteristics. In that study, the researchers found that average monthly rates of disenrollment increased after the implementation of the new policy, which increased premiums by $6 to $20

per person depending on income and implemented stricter payment enforcement, including disenrollment for nonpayment for six months. They found increases in disenrollment rates of 6.1 percent to 10.8 percent. These findings are generally consistent with those in other literature, although the methods differ.

Other findings on non-eligibility periods. In addition to the monthly payment change, Oregon also instituted a six-month non-eligibility period for nonpayment of premiums in 2003. A study by the Office for Oregon Health Policy and Research in 2005 (cited in Oberlander 2007) found that most Oregon Health Plan beneficiaries (72 percent) who lost coverage remained uninsured. This is similar to our finding that 76 percent of spans ending in disenrollment and a non-eligibility period were not followed by a new span within 11 months following disenrollment. Non-eligibility periods among former Oregon Health Plan beneficiaries were associated with declines in access to care (Wright et al. 2005). Among HIP Plus beneficiaries who re-enrolled, most did so shortly after the non-eligibility period was over. Although it is unclear whether these adults were uninsured during the non-eligibility period, it is likely that many faced a break in care continuity as a result of coverage churn.

Other findings on renewals. Few studies are designed to specifically examine the effects of monthly payments on renewals, as opposed to the effect of factors like renewal procedures. We found one study that examined the effect of an increase in annual premiums of $50 on renewals among children with family incomes above 100 percent of the FPL in Alabama's separate CHIP (Morrisey et al. 2012). This premium increase took place in 2003, at the same time that co-payments were increased by small amounts. Using a time series model of observations from 1998 to 2009, the authors estimated that the premium increase reduced the probability of immediate renewal by 8.3 percent and the probability of renewal within six months by 6.9 percent. However, the analysis did not consider transitions from CHIP to Medicaid (or vice versa). This result is generally consistent with our finding that the probability of renewal into the demonstration group in Iowa, Indiana, and Michigan was relatively low after the first span, although our methods are not directly comparable to the methods used by Morrisey and colleagues because we did not examine differences in the probability of renewals before and after monthly payment requirements were implemented.

Findings from state-based evaluation reports on section 1115 demonstrations. Of the three states with available summative evaluation reports (Arkansas, Iowa, and Michigan), only Michigan directly examined the effects of monthly payments on enrollment. Evaluators found that beneficiaries enrolled in the Healthy Michigan Plan for at least six months had a higher likelihood of disenrollment within the next 11 months if they had incomes over 100 percent of the FPL (and were therefore subject to monthly payments) compared to those with lower incomes (Hirth et al. 2018). The evaluators also compared beneficiaries on either side of the income threshold at 100 percent of the FPL, using a regression discontinuity design, and found a 2.6 percentage point jump in the probability of disenrollment within 13 months of initial enrollment.

Summative evaluation reports for Arkansas and Iowa, both premium assistance states, examined enrollment continuity with a focus on transitions in and out of different types of coverage, rather than as a function of monthly payments. Arkansas evaluators found high rates of continuous coverage from January 2014 through June 2015, before a large eligibility re-determination effort took place in July 2015: 98.6 percent of those enrolled in a QHP and 85.3 percent of those enrolled in fee-for-service Medicaid in June 2015 had no enrollment gap during this period (Arkansas Center for Health Improvement 2018). These percentages dropped to 82.1 and 79.3, respectively, for those enrolled in 2016. Arkansas's findings highlight the need to interpret our results with caution because we focus on individual-level 12-month enrollment spans occurring at any point in the 2014–2017 study period. As noted, a sensitivity analysis of continuous enrollment in Arkansas that we restricted to data from calendar year 2017 showed less enrollment continuity than our main results.

Similarly, Iowa's draft summative report contains analyses of enrollment churn between eligibility groups as well as transitions between QHPs, fee-for-service Medicaid, Medicaid managed care plans, and no coverage, by calendar year (Momany 2019).[51] Evaluators found that the percentage of demonstration beneficiaries with at least one gap in coverage in calendar years 2016 and 2017 was similar to the percentage of other Medicaid beneficiaries with a gap, and that gap lengths were comparable. This analysis did not distinguish between demonstration beneficiaries who did and did not owe premiums. The state planned to produce a measure of disenrollment as a result of not completing healthy behaviors or paying monthly premiums, but did not report this in its summative report, or in a related report on disenrolled beneficiaries' experiences and understanding of program requirements (Askelson et al. 2017).

Summative reports for Indiana and Montana were not available in time to be included in this report. Montana's interim report does not contain analyses of administrative data (Kowlessar et al. 2019), but Indiana's interim evaluation report for its 2015–2017 demonstration period contains relevant preliminary findings on enrollment continuity and monthly payment requirements. Evaluators found that about 15 percent of beneficiaries disenrolled from the demonstration in its first year (Lewin Group 2016). This percentage is smaller than the one we found on midyear disenrollment during the first HIP 2.0 enrollment span (regardless of calendar year), using either Indiana's TAF (37.3 percent) or HIP 2.0 data from the state (26.4 percent), because the statistic in the interim report is cross-sectional rather than span-based and includes beneficiaries who enrolled at any point during the first demonstration year in the denominator. The evaluators also found that 5.9 percent of HIP 2.0 beneficiaries with incomes above 100 percent of the FPL were disenrolled for nonpayment in the first demonstration year. This data point is not directly comparable to our finding on the percentage of disenrollments for nonpayment in Indiana (2.3 percent for the first enrollment span) because our denominator included all HIP 2.0 beneficiaries both above and below the poverty line. We did so because

[51] At the time of writing, Iowa's summative report was not yet finalized and posted to Medicaid.gov, but was available on the state's website at https://dhs.iowa.gov/sites/default/files/Final%20Interim%20Report%20IHAWP%20Eval%20Summative%20April%202019.pdf?050820191529.

those with incomes under 100 percent of the FPL and enrolled in HIP Basic can be disenrolled for nonpayment if their income increases and they are moved to HIP Plus, although these beneficiaries are not subject to a non-eligibility period.

D. Discussion

Enrollment in Medicaid. Our results point to a negative relationship between monthly payments and enrollment, although some individual analyses are inconclusive. Each analytic strategy in this evaluation has strengths and limitations. Survey data allow us to estimate who did and did not owe premiums in demonstration states and to include the largest number of comparison states. Survey data also provide information on the total pool of people who are likely eligible for coverage, whether or not they have chosen to enroll. This is a critical perspective when assessing the effect of policies that could deter enrollment. However, many people could be misreporting their Medicaid coverage, leading to an undercount of those who enrolled that can differ across states and over time. Conversely, administrative data provide a complete record of those who have been enrolled in coverage and allow us to understand continuity of coverage within that group, but cannot provide perspective on those who never attempted to enroll. The available administrative data also did not allow us to disaggregate by income level those who did and did not owe monthly payments. Cross-state analyses do not account for differences in the strength of payment requirements or the consequences for nonpayment.

First, regression models based on IPUMS-ACS survey data revealed a negative association between living in states with monthly payments and the probability of Medicaid enrollment, regardless of whether a given person is expected to owe any payments at all. There was also a negative association between owing a monthly payment and the probability of Medicaid enrollment. The reduction in the probability of enrollment for those in the highest estimated payment category ($31 or more) was notably higher than it was at other payment amounts. Nearly all estimates were statistically significant, and the negative relationship persisted across demographic subgroups. Although there are limitations to these models—most notably that they do not account for variation in the payment incentives and nonpayment consequences of different demonstration states—our findings are robust to multiple sensitivity tests and consistent with findings in published research.

Estimated take-up rates, which combine IPUMS-ACS and administrative data to estimate enrollment in Medicaid among the likely eligible population, reveal a pattern that is consistent with our regression models of IPUMS-ACS data. Take-up in states with monthly payments was lower than in most comparison states in most years, but the pattern is not conclusive. Take-up in demonstration states in 2015, a year when premiums were in effect for all demonstration states except Montana (which had not yet expanded coverage), was lower than take-up in most comparison states in the same year. In 2016, take-up in all demonstrations was lower than in Kentucky, New Mexico, and Ohio, but was comparable to take-up in West Virginia and Pennsylvania. Indiana had the lowest take-up rate of any state in 2016, a year when all states had

expanded coverage. (A take-up estimate is not available for Indiana in 2017 because we did not include administrative data for Indiana in the analysis for that year.)

Continuity of coverage. The results of our analyses of the relationship between monthly payment policies and enrollment continuity within the first, second, and third coverage year were inconclusive. Regression estimates of renewals after the first enrollment year revealed a relatively low probability of renewal in three of the five states with monthly payment policies (Iowa, Indiana, and Michigan), but the probability of renewal in the other two (Arkansas and Montana) was higher than it was for comparison states. Differences between demonstration and comparison states disappeared by the third enrollment year, suggesting that those who remain enrolled through a third span value coverage highly, are accustomed to making monthly payments, or both. Available administrative data did not permit us to distinguish between those who did and did not owe monthly payments based on their incomes.

A focused analysis of payment enforcement rules in Indiana sheds light on the relationship between non-eligibility periods imposed for non-payment and gaps in coverage. Adults with incomes above the poverty level who failed to make a monthly payment within a 60-day grace period were disenrolled and subject to a six-month non-eligibility period, whereas beneficiaries disenrolled for other reasons were not subject to the non-eligibility period. We found that beneficiaries who were subject to the non-eligibility period were more likely to be lost-to follow-up (that is, to not re-enroll within an 11-month observation period) than those who disenrolled for any other reason, except for moving out of state. They were also more likely to be lost to follow-up than to re-enroll. Beneficiaries who were disenrolled for nonpayment and who subsequently re-enrolled also had the longest average gap between spans (7.0 months). However, the length of this gap indicates that most HIP Plus beneficiaries who re-enrolled within the 11-month observation period did so shortly after the non-eligibility period was over. Thus, beneficiaries who were disenrolled for non-payment either failed to re-enroll for 12 or more months or they re-enrolled shortly after regaining eligibility.

Finally, we examined long-term enrollment continuity, estimating the probability of remaining continuously enrolled for periods longer than a full year and conducting a survival analysis of the time to disenrollment throughout the study period. Arkansas, a demonstration state, and Kentucky, a comparison state, had the two highest continuous enrollment rates at 18, 24, 36, and 48 months. Iowa and Michigan, both demonstration states, consistently had the lowest continuous enrollment rates, although enrollment rates in Pennsylvania and West Virginia were comparable to Iowa and Michigan at 24 and 36 months. It is possible that payment policies in Iowa and Michigan explain their relatively low long-term continuous enrollment rates. However, Montana, which had a similar monthly payment policy, had higher continuous enrollment rates, likely because of its high renewal rates. Results of an accelerated failure time model were more conclusive, showing that people assumed to have a monthly payment had enrollment lengths that were 84 percent as long as those assumed not to owe a monthly payment, controlling for other variables. This estimate was statistically significant and consistent with other findings that monthly payments reduce enrollment continuity among adults and children, but it should be

interpreted with caution because we were unable to distinguish between those who did and did not owe premiums within each state.

Policy takeaways. Taken together, our results point to a negative relationship between monthly payments and enrollment. Regression models based on national survey data revealed a negative association between living in states with monthly payments and the probability of Medicaid enrollment, regardless of whether a given person is expected to owe any payments at all. There was also a negative association between owing a monthly payment and the probability of Medicaid enrollment; the largest payment was associated with the largest decrease in take-up. Analyses of enrollment continuity, drawing on administrative data, were inconsistent, and limited by the lack of an income variable that would allow us to segment analyses by those who did and did not owe payments in demonstration states. However, a survival analysis revealed a statistically significant negative relationship between enrollment duration, or length of continuous enrollment spans, and owing a monthly payment (based on the timing of states' monthly payment policies and the onset of payment obligations). A separate analysis of non-eligibility periods as a consequence of nonpayment suggests that this form of payment enforcement could extend the period prior to reenrollment and reduce the number of people who return to Medicaid coverage.

V. DOMAIN 3: BENEFICIARY ENGAGEMENT STRATEGIES TO ENCOURAGE HEALTH BEHAVIORS

Highlights of Domain 3 findings

- For beneficiaries in Indiana, Iowa, and Michigan, financial incentives for wellness visits were associated with a higher probability of having a wellness visit compared with beneficiaries in comparison states.
- All three demonstrations were associated with increased use of preventive services, with Iowa's demonstration associated with the highest rates of wellness visits, and Indiana's and Michigan's demonstrations associated with higher rates of other preventive services, such as screenings.
- Findings on management of chronic conditions were mixed; in general, outcomes in demonstration states did not differ greatly from those in comparison states.
- Only Michigan beneficiaries used primary care at higher rates than beneficiaries in comparison states; but all three demonstrations were associated with more use of specialty care.
- Behavior incentives in Iowa and Michigan were associated with reduced use of inefficient care, such as using the emergency department for a non-emergency, but not with increased use of more efficient services such as urgent care. Indiana's demonstration was associated with increased use of the emergency department.
- Analyses based on national survey data did not reveal any population-level effects that might have resulted from beneficiary incentives in Indiana, Iowa, and Michigan, such as diffusion of healthy behavior practices to peers and relatives of beneficiaries who were not enrolled themselves.

Demonstration beneficiaries in three states—Indiana, Iowa, and Michigan—were given financial incentives to engage in healthy behaviors (such as receiving preventive care) and, in some cases, to avoid inappropriate care (such as emergency department visits for non-emergency care) or to manage the cost of their care. The effectiveness of these incentives could vary depending on a number of factors, including the monetary amounts of available rewards, how easy it was to engage in the incentivized behavior, and what administrative processes were associated with getting credit for it. Effectiveness also depends on beneficiaries understanding what the incentivized behavior entails and realizing they can get financial rewards if they practice it. All three states contracted with health plans under a capitation system to provide care for at least part of their expansion populations and to implement the beneficiary engagement strategies outlined in their demonstration designs.

This portion of the evaluation is designed to understand the relationship between financial incentives and beneficiary behavior by asking the following primary research questions:

1. What strategies are states using to educate beneficiaries about preferred health behaviors?
2. To what extent are Medicaid enrollees responsive to explicit program incentives?
3. How do the incentives affect overall access to and use of care?
4. Are population-level effects observed from Medicaid demonstration policies?

We used three distinct analytic approaches to address these questions. To address the first question, we synthesized state-reported data on beneficiary education methods. To address the second and third questions, we used administrative data for descriptive analyses, including regression models, of the preventive, chronic, and emergency care that beneficiaries received. We also analyzed state-reported information on health account utilization. To address the fourth question, we used BRFSS data for descriptive analyses and difference-in-differences regression analyses of self-reported health behaviors. A summary of our analytical approach and data sources for all Domain 3 research questions can be found in Appendix Table A.3.

We begin by describing key features of the beneficiary engagement policies in demonstration states (Section A). We then present our findings on state strategies for educating beneficiaries about their incentives (Section B), the effect of beneficiary engagement programs on beneficiaries' participation in their care (Section C), the effect of incentives on receipt of care (Section D), and whether demonstration incentives have population-level effects (Section E). We discuss limitations (Section F) and results from related literature (Section G). We close by discussing and synthesizing our results (Section H).

A. Key design features of beneficiary engagement policies

In this section, we briefly summarize relevant features of these policies in the three demonstration states during the 2014–2017 study period. More details can be found in Appendix B.

Indiana's Healthy Indiana Plan (HIP) 2.0 encouraged beneficiaries to: (1) get a preventive service recommended for people of their age and gender, and (2) manage their health care costs. Although beneficiaries could fulfill the preventive service requirement with a one-time action—for example, having a mammogram—HIP 2.0 engaged them in managing their health care costs throughout the year by providing a Personal Wellness and Responsibility (POWER) account that served as a deductible jointly financed by the state and the individual. HIP 2.0 beneficiaries used POWER accounts to pay for the first $2,500 of their annual medical expenses, except for the cost of preventive care, which was not deducted from the account. Preventive services were also exempt from co-payments. HIP Plus beneficiaries funded part of the $2,500 through monthly contributions of 2 percent of their income (or a minimum of $1 for beneficiaries with no income or incomes up to 5 percent of the federal poverty level [FPL]). For HIP Basic beneficiaries, the state paid the entire amount.[52]

Beneficiaries had an incentive to spend POWER account funds judiciously because they could be eligible to roll over a portion of any funds remaining at the end of an enrollment year into their account for the next year, thereby reducing or even eliminating their required monthly

[52] Analyses based on TAF did not include an income variable or payment amounts. This data limitation applied to all included states. Because all three demonstration states structured their incentives for healthy behaviors as reductions in monthly payments for at least some of the expansion population, not being able to observe or infer monthly payment amounts is a limitation of this evaluation.

contributions in the next year.[53] The state doubled the rollover amount for HIP Plus beneficiaries who received at least one recommended preventive service during the plan year. Beneficiaries enrolled in the HIP Basic plan who got the recommended preventive care also had the opportunity to reduce future contributions by up to 50 percent if they chose to move to HIP Plus at renewal, but they could not earn reduced payments if they failed to get the recommended preventive care.

Iowa's Iowa Health and Wellness Plan (IHAWP)[54] encouraged beneficiaries to (1) complete an annual health risk assessment (HRA) and (2) have an annual wellness visit. If beneficiaries did both in one enrollment year, they were exempt from monthly payments in the next enrollment year. The monthly payment for beneficiaries with incomes between 50 and 100 percent of the FPL was $5, and the monthly payment for those with incomes above 100 percent of the FPL was $10. At first, only a comprehensive physical satisfied the requirement for an annual wellness exam, but over the course of 2014, Iowa began to count routine medical exams and visits to a physician's office for acute care. In January 2015 the state began to count dental wellness visits as well.

In addition, IHAWP featured escalating dental benefits that rewarded consistent dental wellness visits throughout the year. All beneficiaries received coverage for core dental services as part of the demonstration. Core services included diagnostic and preventive services, emergency services, and stabilization services. Beneficiaries who returned for a periodic exam 6 to 12 months after their first visit qualified for enhanced benefits, including restorative services, endodontic care, and certain oral surgery services, among others. Those who returned for a second periodic exam 6 to 12 months after the first qualified for additional enhanced benefits, including crowns, tooth replacements, and gum surgery. Earned benefits were maintained by adhering to an annual or semiannual schedule of exams.

Michigan's Healthy Michigan Plan (HMP) demonstration used an individually targeted strategy that incentivized beneficiaries to (1) complete a health risk assessment and (2) agree to address or maintain a healthy behavior of their choosing with the help of a primary care provider. The need to involve physicians in these activities was an implicit financial incentive for a primary care office visit. By engaging in the two incentivized behaviors, beneficiaries with incomes

[53] In January 2018, the state switched from enrollment-year to calendar-year resolution of POWER account balances. All active POWER account balances were closed on December 31, 2017, and rolled over on January 1, 2018.

[54] IHAWP comprised two different demonstrations during 2014 and 2015: the Iowa Wellness Plan and Marketplace Choice. The Iowa Wellness Plan originally covered only IHAWP beneficiaries with incomes at or below 100 percent of the FPL. Marketplace Choice was a premium assistance program that supported the purchase of QHPs by non-exempt beneficiaries with incomes above the FPL. Marketplace Choice was effectively closed on December 31, 2015, although the state retained its authority to operate the program through December 2016. One of Iowa's two participating qualified health plan carriers became insolvent in late 2014, and the other stopped accepting new Medicaid beneficiaries in 2015. The state received approval in January 2016 to modify eligibility for the Iowa Wellness Plan to include the population formerly enrolled in premium assistance. In April 2016, all beneficiaries began receiving care through managed care organizations that are part of the Iowa Wellness Plan.

above 100 percent of the FPL could earn a 50 percent reduction in required contributions to beneficiary accounts, which were called MI Health accounts. Account contribution amounts were normally 2 percent of income. Beneficiaries with incomes at or below 100 percent of the FPL, who were not required to make account contributions, could earn a $50 gift card. MI Health accounts were intended to teach beneficiaries about the costs of care and prepare them to pay regular premiums for commercial coverage in the future. Beneficiaries at all income levels who engaged in the two incentivized behaviors also earned a 50 percent reduction in co-payments once they spent 2 percent of their annual income on cost-sharing. (Co-payments stopped entirely when they reached the 5 percent out-of-pocket maximum.) Preventive services and services for the management of chronic conditions (such as diabetes) were fully exempt from co-payments.

B. State strategies to educate beneficiaries

States and health plans play an important role in educating beneficiaries about a demonstration's incentives. For incentives to yield their intended results, beneficiaries must understand what they have to do to earn rewards and why those activities are worthwhile. To shed light on educational activities that took place, we summarized information from a rapid-cycle report that synthesized findings from two rounds of interviews with key informants (state Medicaid officials and health plan staff) in Indiana, Iowa, and Michigan that took place in 2016 and 2017 (Contreary and Miller 2017). Next, to describe the evidence on beneficiary engagement, we updated our findings from a second rapid cycle report that summarized analyses of beneficiary survey data that were presented in the demonstration evaluation reports from the three states' independent evaluators in early 2017 and 2018 (Miller et al. 2017). The approaches used by states and health plans and the perceptions of beneficiaries may have changed since these data were collected, but the insights they shared at that time provide important information on how the programs were implemented. In this section, we present analyses that addressed one subsidiary research question (listed as Question 1a in Appendix Table A.3):

- What strategies are states using to explain incentives and disincentives? Which strategies are perceived to be effective?

Although all three states contractually required health plans to communicate with their beneficiaries, the plans typically provided education above and beyond what the state required. Sharing the responsibility to educate beneficiaries resulted in variation both between and within states in how individual beneficiaries learned about the incentives, how they were reminded of potential financial rewards, and ultimately, how they experienced the effects of these policies on health outcomes.

1. Education strategies

The primary strategies used to educate beneficiaries were:

- **Communicating directly with beneficiaries about the desired behaviors and rewards.** Beneficiaries were first informed of demonstration incentives when they enrolled with the state Medicaid agency. After they joined a health plan, the plan took over primary responsibility for communicating with and educating the beneficiaries.[55] States retained the right to review all communication materials distributed to beneficiaries, however, and, in some cases, to develop materials that the plans distributed with minimal changes. Communication methods contractually required by states included welcome packets, member handbooks, and call centers. Plans typically layered a wide range of other communications initiatives on top of the state requirements, and devoted significant resources to communicating with beneficiaries .(See the box: Reported Plan Points of Contact with Beneficiaries.)

Reported plan points of contact with beneficiaries

- Welcome packet
- Member handbook
- Plan website
- Welcome calls
- Monthly or quarterly general and program-specific newsletter
- Calls or mailings reminding people to access state-encouraged services
- Calls or mailings reminding people to access plan-specific services
- Topic-specific awareness or reminder calls or mailings (for example, for "Heart Health Month")
- Calls triggered by HRA responses
- Targeted calls or mailings from care/disease management programs that beneficiaries were assigned to
- Calls triggered by a lapse in care as identified via claims data
- Calls triggered by an emergency department visit as identified via claims data
- Reminders about services when beneficiaries called the plan call center with a question
- Monthly, weekly, or daily contacts for beneficiaries with severe or chronic conditions
- Emails or texts instead of phone calls (some plans were exploring these methods)

- **Monitoring and giving extra encouragement for behaviors that earn rewards.** Plans in all three states reported that they gave incentives over and above those built into the demonstrations. The rewards were usually financial or material, often involving gift cards or rewards cards that could be credited with amounts ranging from $5 to $25 when beneficiaries received a recommended preventive service or engaged with their care management program. In some cases, the rewards were restricted to purchases of health-related items; in other cases, beneficiaries could use them for any purchase in participating stores. Some Michigan managed care organizations (MCOs) reportedly conducted targeted

[55] As of July 2017, this was true of all three states; however, before the 2016 managed care transition, Iowa's demonstration operated a little differently. Beneficiaries enrolled in Marketplace Choice (who had incomes greater than 100 percent of the FPL) received care through QHPs, and communicating with them about the demonstration was left to the state. Beneficiaries in the Iowa Wellness Plan (whose incomes were at or below 100 percent of the FPL) received regular communications from their managed care organization (MCO). After the state's contracts ended with the QHPs and initial MCOs, but before the managed care transition in 2016, the state was responsible for all communications with beneficiaries about the demonstration.

campaigns (for example, to boost mammogram rates) by using raffles for goods such as iPads.

- **Involving providers.** To varying degrees, states and plans reported that they enlisted providers in engaging beneficiaries, in some cases by offering them direct financial rewards. In all three states, communication with providers took place mainly at the plan level. Communication typically involved (1) detailing the specific incentivized services beneficiaries should receive to ensure providers were familiar with the requirements of the demonstration, and (2) describing any rewards providers could earn by ensuring beneficiaries completed healthy behaviors. Plans reported using diverse methods to engage providers, such as provider toolkits, regular mailings and newsletters, "rounding" or information-sharing visits, regular fax updates, online provider portals, and information distributed at webinars, seminars, and meetings of Medicaid providers or other professional associations.

2. Evidence on beneficiaries' engagement

State surveys revealed that, for the most part, beneficiaries had a limited understanding of demonstrations' features and incentives, especially during the early years of the demonstrations. In Iowa, fewer than 30 percent of beneficiaries surveyed in 2014[56] were aware of the HRA/wellness visit incentive, and actual completion rates were even lower. Although different data sources reported different completion rates, records of the Iowa Department of Human Services showed that only 8 percent of demonstration beneficiaries with incomes over 100 percent of the FPL, and 17 percent of beneficiaries with incomes at or below 100 percent of the FPL, completed an HRA *and* had a wellness exam in 2014 (Askelson et al. 2016).

The HRA completion rate in 2015 in Michigan was about 14 percent,[57] comparable to Iowa's rate in 2014. However, Michigan implemented a multi-step process for completing the HRA: (1) the beneficiary worked on part of the HRA with an enrollment representative, (2) the beneficiary finished the HRA with his or her provider, and (3) the provider submitted the completed HRA to the beneficiary's MCO. State Medicaid officials reported in 2016 that the majority of new beneficiaries had a provider visit within 150 days of enrollment, but that HRA completion rates did not reflect this. Beneficiaries who did not understand that completing an HRA could lower their payments might have been less likely to ask their physician to complete and submit the HRA during their visit, and consequently they missed out on financial rewards. Michigan beneficiaries' understanding of the policy did not seem to improve over time. In 2017, fewer than 30 percent of beneficiaries surveyed knew that completing an HRA would result in lower monthly payments (Dorr Goold et al. 2018).

In Indiana, findings from a 2015[58] state survey of beneficiaries revealed that fewer than 10 percent of beneficiaries were aware they could receive preventive care without a co-payment,

[56] The most recent Iowa state report noted limited beneficiary understanding of the healthy behavior program but did not present new survey results on this topic.

[57] As reported in a key informant interview with state Medicaid officials January 27, 2016.

[58] The latest report contained no new survey results on this topic.

with around 40 percent reporting they did not know whether they faced cost-sharing for preventive services. A larger proportion (52 percent of HIP Plus beneficiaries and 35 percent of HIP Basic beneficiaries) understood that receiving preventive care would mean that their annual POWER account rollover amount would be higher (Lewin Group 2016).

3. Effectiveness of beneficiary engagement strategies

Even though states and plans committed significant amounts of time and resources to reaching out to beneficiaries and providers and educating them about demonstration incentives, there were still large gaps in beneficiaries' understanding of the incentives and rewards available to them. Although we could not draw definitive conclusions, interviews with key informants from states and health plans, combined with an analysis of state beneficiary survey data, shed some light on which strategies could be more effective than others in driving behavior.

Informants told us they need to remind beneficiaries regularly about incentivized behaviors and the rewards for completing them. In the early years of Iowa's demonstration, beneficiaries received only occasional reminders of the financial rewards for completing an HRA and a wellness visit, starting with the information the state gave them about the healthy behaviors program when they enrolled. In 2014, the state mailed reminder postcards, but in 2015, no reminders were sent. This could have contributed to low completion rates for the HRA as of 2016, although Iowa beneficiaries received wellness visits at comparable rates to beneficiaries in other states. In 2017, letters were sent to members reminding them to engage in their healthy behaviors so as to waive monthly contributions for the following year.[59]

In Michigan, the multi-step process required to get the reward for an HRA (as of 2016) meant there were multiple points where it could break down. Under this design, earning a reward ultimately depended on the beneficiary's provider taking action and submitting the completed HRA to the plan. Informants in Michigan expressed their frustration with the process for completing an HRA, saying it probably contributed to the low completion rate. Streamlining the processes for obtaining rewards could help more beneficiaries complete the HRA and earn the reward.[60]

[59] We did not have access to HRA completion data in Iowa past 2016, so we could not assess whether HRA completion rates rose after the new reminder policy went into effect.

[60] We did not conduct key informant interviews after 2016, and thus cannot comment directly on whether the state changed its practices. However, a recent report from the state noted that as of December 2018, health care providers also had to choose between four statements to attest to whether the beneficiary had achieved or made significant progress toward the previous year's healthy behavior goal. Only those beneficiaries whose providers attested to both significant progress and the selection of a new goal were eligible for the incentive (Michigan DHS 2019).

C. Evidence that incentives encourage Medicaid beneficiaries to participate in their care

To assess the effect of program incentives on Medicaid enrollees' participation in their health care, we analyzed their completion of healthy behaviors that were specifically incentivized in the demonstration states in 2014–2017. This section has analyses that address a subsidiary research question (listed as Question 2 in Appendix Table A.3), "To what extent are Medicaid enrollees responsive to explicit program incentives?"

We considered four outcomes related to completing incentivized behaviors:

- Having a wellness visit
- Time it took to have a wellness visit
- Completing an HRA
- Engaging with health accounts, as reflected by account management and cost-conscious use of care

An annual wellness visit is recommended for all adults, but having one only yielded financial rewards from the state Medicaid agency in the demonstration states we analyzed. Our comparison states (Kentucky, New Mexico, Ohio, Pennsylvania, and West Virginia) have not offered financial incentives for wellness visits as a matter of policy, though their contracted health plans may have implemented incentives independently, just as health plans in the demonstration states often layered on incentives beyond those that were part of the demonstration. To apply a consistent definition across states, we examined the number of people who had an adult wellness exam as it is defined in the Healthcare Effectiveness Data and Information Set (HEDIS).[61]

We used administrative data for descriptive regressions designed to estimate the association between offering financial incentives for a wellness visit and actually having such a visit, and to estimate the length of time between enrollment and the wellness visit. We limited our analysis to beneficiaries for whom we could observe a full 12 months of enrollment data. Differences between demonstration states and comparison states are reflected in the average marginal effect for each outcome variable in each demonstration state. Control variables included age, sex, Chronic Illness and Disability Payment System (CDPS) score,[62] and an indicator for residing in a

[61] The specific incentivized service differed depending on the state. Indiana gave rewards for completion of any recommended preventive service, including a wellness visit. Iowa initially required a comprehensive annual physical, but over time, the state relaxed the requirement to have a broader set of provider office visits qualify a beneficiary for the reward. Michigan did not explicitly incentivize a wellness visit, but did provide financial rewards for completing an HRA and selecting a health goal, both of which must be done with the help of a primary care provider. There was therefore an implicit incentive to have a wellness visit.

[62] Chronic Illness and Disability Payment System (CDPS) scores serve as a proxy for health, with higher scores indicating worse health. We Winsorized CDPS scores at 1 percent at the top (that is, limited more extreme values in the data to the 99th percentile value) to reduce the influence of outliers.

rural area. Our approach enabled us to control for the confounding effects of beneficiaries' demographic and health characteristics, but it does not establish causality.

We used a model that estimated separate effects for each state's demonstration.[63] The demonstrations differed in important ways, including the timing of the reward for a wellness visit. Michigan beneficiaries were rewarded in the same enrollment year they had the visit, whereas beneficiaries in Indiana and Iowa were rewarded only upon renewal. This difference in timing of the reward could be reflected in different amounts of time it took to complete the required behavior for the reward. More information on the methods used to analyze receipt of wellness visits can be found in Appendix C. Appendix G contains descriptive analyses of administrative data on wellness visits and other service use.

Next, we used unadjusted frequencies to examine how many beneficiaries completed the HRA in Iowa in 2014–2016. Completing an HRA was one of two healthy behaviors incentivized in both Iowa and Michigan, although we were only able to obtain data on HRA completion in Iowa for the first three years of the demonstration.

Finally, we conducted a series of analyses to shed light on beneficiaries' understanding and use of the health accounts in Indiana and Michigan. We synthesized findings from state monitoring reports and interim and final evaluation reports for both states, as well as a 2016 key informant interview with Michigan Medicaid officials. We also conducted descriptive analysis of administrative data obtained directly from Indiana on the rate at which beneficiaries received account rollover rewards for having completed the incentivized preventive service.

1. Having a wellness visit

After controlling for individual demographic and health characteristics, all three demonstration states had higher levels of wellness visits than comparison states did, although there were substantial differences between the three demonstration states. Table V.1 shows our estimates of the differences in the probability of having a wellness visit that are associated with being enrolled in the Indiana, Iowa, and Michigan demonstrations.

Iowa's demonstration was associated with the highest rate of wellness visits (8.2 percentage points higher than the rate among expansion beneficiaries in comparison states, controlling for beneficiary characteristics). Indiana and Michigan demonstration beneficiaries were 4.5 and 3.0

[63] We tested a number of alternative specifications to ascertain whether our results were robust to different modeling choices. Results were similar across all specifications. First, we ran separate regressions for each demonstration state, excluding the other demonstration states. Next, we restricted the effect of beneficiary engagement policies to be the same across the three demonstration states by running a regression using a single indicator variable for residing in one of the three demonstration states. Then, we included fixed effects for all states in the analysis (demonstration and comparison), and generated state-specific predicted values for all outcomes. Finally, for the primary model specification and these alternative specifications, we allowed the effect of demographic and health characteristics to vary by state or by demonstration status by including interactions between the state-specific indicator variables or the single demonstration indicator and each of the control variables. The states differ in their demographic and health composition, and this approach allows the association between, for example, age and receipt of a wellness visit to differ by state.

percentage points more likely to have made a wellness visit than comparison state beneficiaries were. Moreover, beneficiaries in the demonstration states scheduled their wellness visits sooner after enrollment, on average, than beneficiaries in the comparison states: visits occurred 10 days sooner on average in Indiana and Iowa, and 9 days in Michigan (Table V.1). For both outcomes, the estimates for the three states were all statistically significantly different from zero and from each other at conventional levels ($p <. 05$).

Table V.1. Estimated association between incentives offered in demonstration states and having a wellness visit in 2014–2017

		Indiana				Iowa				Michigan				
Measure	Comparison group mean	Average marginal effect	Standard error	*p*-value	Percent difference	Average marginal effect	Standard error	*p*-value	Percent difference	Average marginal effect	Standard error	*p*-value	Percent difference	N
Had a wellness visit (percent)	70.7	4.5***	0.1	.000	6.4	8.2***	0.1	.000	11.6	3.0***	0.0	.000	4.2	7,505,336
Length of time between enrollment and wellness visit (days)	102.6	-9.9***	0.2	.000	-9.6	-10.4***	0.2	.000	-10.2	-9.2***	0.1	.000	-8.9	5,400,442

Source: Mathematica analysis of administrative data, 2014–2017, for Indiana, Iowa, and Michigan (demonstration states); and Kentucky, New Mexico, Ohio, Pennsylvania, and West Virginia (comparison states). Years included for each state depend on the date of demonstration implementation or coverage expansion and on data availability.

Notes: Marginal effects were estimated using logistic and ordinary least squares regressions. We calculated the average of the estimated difference in outcomes between the demonstration group and the comparison group, using the covariate distribution of the demonstration group. Control variables included age, sex, CDPS score, and an indicator for residing in a rural area.
The comparison group mean is the unadjusted comparison sample mean, presented for reference. The average marginal effect should not be added to the comparison group mean to calculate use by enrollees in demonstration states.
Length of time between enrollment and wellness visit was calculated only for individuals who had a wellness visit.

*** Significantly different from zero at the .01 level, two-tailed test.

2. Completing the HRA

Table V.2 shows HRA completion rates in Iowa in 2014–2016, overall and for different demographic subgroups. The overall completion rate was low. We considered 12-month enrollment spans (the longest a beneficiary could be enrolled before needing to renew), and in only 21.6 percent of the spans did beneficiaries receive credit for completing an HRA. Older people, women, and people with higher CDPS scores were, on average, more likely to complete the HRA.

Table V.2. HRA completion rates in Iowa, overall and by demographic subgroups, 2014–2016

Subgroup	HRA completion (%)
Overall	21.6
Age	
19–26	13.8
27–35	17.3
36–45	21.0
46–55	28.0
56–64	35.6
Sex	
Female	23.8
Male	19.2
Urban/rural residence	
Urban	21.6
Rural	21.7
CDPS score	
0 to < 0.5	14.8
0.5 to < 1.0	21.1
1.0 to < 1.5	25.0
≥ 1.5	27.1
Number of observations	388,110

Source: Mathematica analysis of Iowa state administrative data from January 2014–December 2016.
Note: Beneficiaries could receive credit for completing an HRA in a number of ways, including by telling the state they filled one out. These figures could therefore represent an upper bound on actual HRA completion rates.
CDPS = Chronic Illness and Disability Payment System; HRA = health risk assessment.

3. Use of health accounts

Indiana and Michigan used health accounts as another way to engage beneficiaries. The accounts were designed to educate beneficiaries about the cost of care and encourage them to practice cost-conscious behavior when they sought health care. We found only limited evidence that health accounts might influence care-seeking behavior. Few surveyed HIP 2.0 beneficiaries said they were aware of their POWER accounts, and similarly few reported asking their providers about the price of treatment. In Michigan, large percentages of beneficiaries surveyed in 2016

and 2018 reported that they were aware of their statements and sought information about the cost of their care, but fewer reported that cost information led them to change their health care purchasing decisions.

Indiana

Findings from state report. In Indiana, survey results from 2015 suggest that few beneficiaries checked their POWER account balances regularly. Although all beneficiaries had a POWER account, only 66 percent of HIP Plus and 46 percent of HIP Basic beneficiaries reported ever hearing of the POWER account. Of those who had heard of the account, 72 percent of HIP Plus and 76 percent of HIP Basic beneficiaries knew they had an account of their own. Of those who knew they had an account, 51 percent of HIP Plus and 57 percent of HIP Basic beneficiaries said they checked their account balances every few months or more often. This means that only about 24 percent of HIP Plus and 18 percent of HIP Basic beneficiaries who were surveyed both knew they had a POWER account and checked its balance at least every few months (Lewin Group 2016).

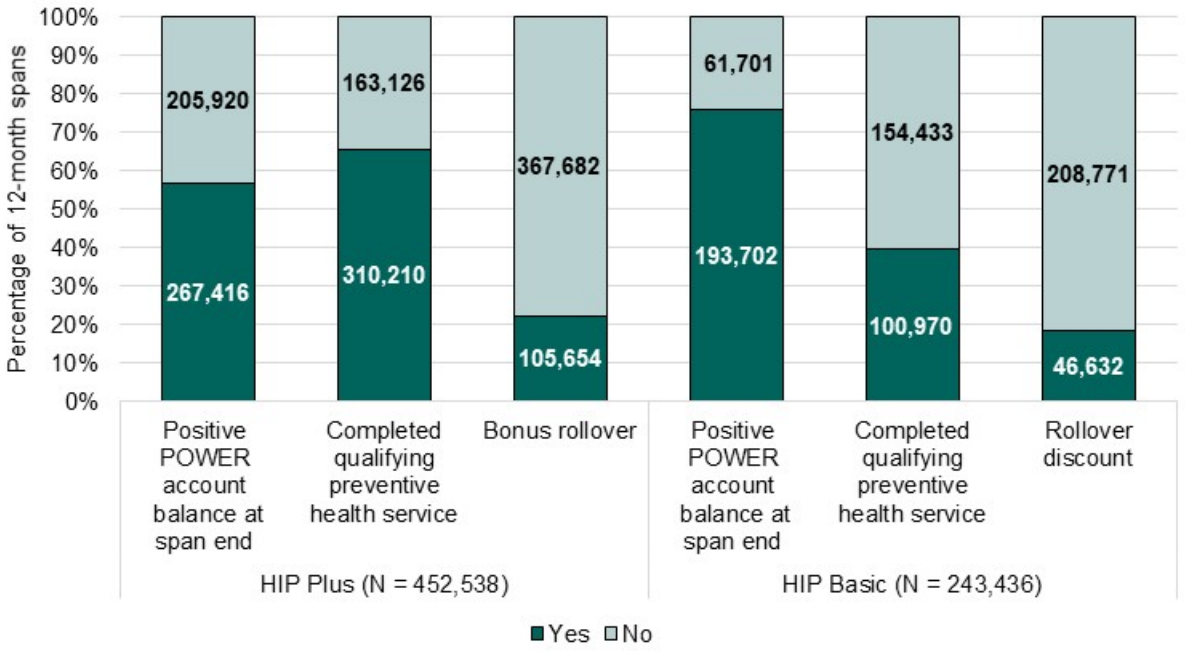
Likewise, survey results suggest that POWER accounts did not motivate most beneficiaries to seek information about the price of health care services they needed. HIP Plus beneficiaries could maximize their POWER account rollover dollars by maintaining a balance after the costs of non-preventive care were deducted from their accounts. However, only about one-quarter of all surveyed HIP Plus beneficiaries (27 percent) responded affirmatively to a question about whether they asked their provider how much any needed care would cost. If beneficiaries are to consider price in making choices about their health care consumption, they need to know how much any recommended services will cost. If most beneficiaries did not pursue this information, it suggests they were not considering how their choices would affect their POWER account balance.

Finally, beneficiaries also earned rewards if they received preventive care—HIP Plus beneficiaries earned a doubled account rollover, and HIP Basic beneficiaries qualified for reduced account payments if they moved to HIP Plus at renewal. Awareness of this incentive was comparable to overall awareness of the POWER account: 52 percent of HIP Plus and 35 percent of HIP Basic beneficiaries understood that receiving preventive care would impact their annual rollover. Although survey data suggest an imperfect understanding of the demonstration's incentives, Indiana beneficiaries had high rates of preventive service use. Analysis of claims data in the state's interim evaluation report revealed that 74 percent of HIP 2.0 beneficiaries who were enrolled for at least 10 months by winter 2015 received a qualifying preventive service (Lewin Group 2016). Factors other than understanding the demonstration's incentives, such as intrinsic motivation, prompts from care providers, or financial rewards from health plans could have contributed to these high rates.

Analysis of administrative data. Using HIP 2.0 data from Indiana, which were extracted from the POWER account reconciliation files that the managed care entities (MCEs) submit to the state

fiscal agent, we calculated the percent of 12-month spans in which beneficiaries met the criteria for a reward. Figure V.1 shows the number and percentage of spans in which beneficiaries met each criterion, by plan type. Among spans ending in HIP Plus enrollment, 56 percent had a positive POWER account balance at the end of the span and 66 percent received qualifying preventive care. Nevertheless, less than a quarter (22 percent) of these spans had a bonus rollover for meeting both criteria. The difference between rates of qualifying preventive service use and bonus rollovers might result from a tendency for beneficiaries who receive preventive services to utilize more care in general, reducing their likelihood of retaining a positive balance at the end of the span. A larger proportion of spans ending in HIP Basic enrollment had a positive POWER account balance at the end of the span (76 percent), but HIP Basic beneficiaries were less likely to receive qualifying preventive care than HIP Plus beneficiaries (40 percent). Eighteen percent of spans ending in HIP Basic enrollment met both criteria to qualify for a rollover discount in the next span.

Figure V.1. Number and percentage of 12-month spans with bonus rollover or rollover discount, by plan type at the end of span



Source: Mathematica analysis of Indiana HIP 2.0 data, February 2015–January 2018.

Notes: Enrollees who complete age- and sex-appropriate preventive care and have a positive POWER account balance at the end of the span qualify for a bonus rollover (HIP Plus only) or rollover discount (HIP Basic only). Managed care entities reported using different requirements for completion of qualifying preventive care in 2016.

Information on preventive care, POWER account balances, bonus rollovers, and rollover discounts are from POWER account reconciliation records that correspond with the last month of enrollment. In cases where there are no POWER account reconciliation records at the end of the span, the information may be based on a record in the month prior to or the month after the end of the span.

Some beneficiaries move between HIP Plus and HIP Basic during their span; the reward type in these cases (bonus rollover or rollover discount) is based on the benefit plan at the end of the span.

POWER = Personal Wellness and Responsibility.

The rate of preventive service completion reported here, which used HIP 2.0 data (reported by MCEs), is similar to the rate in the state's interim evaluation report, which used claims data and found that between 36 and 51 percent of HIP Basic beneficiaries and between 52 and 64 percent of HIP Plus beneficiaries completed a preventive service (Lewin Group 2016).

Michigan

Findings from state reports. In Michigan, there was greater awareness of the MI Health account and the account statement than there was for other HMP policies designed to engage beneficiaries. Most beneficiaries who responded to the state follow-up survey in 2017 (78 percent) reported receiving an MI Health account statement; of these, 85 percent agreed or strongly agreed that they carefully reviewed each statement to see how much they owed, and 83 percent agreed or strongly agreed that the statements made them more aware of the cost of health care (Dorr Goold 2018). Almost three-fourths (72 percent) of all respondents to the 2016 survey said they were somewhat or very likely to find out how much they might have to pay for a health service before they got it, and 67 percent said they were somewhat or very likely to talk with their doctor about how much different health care options would cost (Dorr Goold 2017).[64]

Differences in the co-payment structure may help to explain why Michigan beneficiaries were more likely than Indiana beneficiaries to say they asked their doctors about the cost of care. HIP Plus beneficiaries effectively paid for care in advance by making POWER account contributions, which were then drawn down to pay for services. They were exempt from point-of-service co-payments, and the opportunity to roll over unspent funds at renewal could have arisen many months from the point of care, which is when beneficiaries were deciding between treatment options. Michigan beneficiaries, in contrast, were billed quarterly for incurred copayments via the MI Health account statement, potentially making the cost of treatment more salient at the point of care.

There is some evidence that the MI Health account program drove changes in care-seeking behavior among HMP beneficiaries: 31 percent of HMP beneficiaries agreed or strongly agreed that the information in the MI Health account statement led them to change some of their health care decisions (Dorr Goold 2018). However, without knowing how much discretion they had in their care decisions, it is difficult to know whether this figure represents a large or small shift in beneficiaries' decision making. More research is needed to understand what types of health care decisions beneficiaries were prompted to reconsider, and what conditions—such as advance information about health care prices or the ability to consult with a provider—were necessary to enable those decisions.

[64] These questions were not asked in the follow-up survey.

D. Evidence that program incentives change behavior in desired ways

We examined use of several health care services to assess whether beneficiaries made positive changes to their health behavior in ways that might be expected to follow from the services directly incentivized by demonstrations (completing an HRA and having a wellness visit in Iowa and Michigan, and receiving recommended preventive care in Indiana). In all three demonstration states, the incentivized behaviors could be completed by establishing a primary care relationship.

Demonstration incentives might influence beneficiary utilization patterns in a number of ways. For example, incentives to establish a relationship with a primary care provider could lead to more preventive care visits.[65] We also expect that, by establishing such a relationship and receiving recommended preventive care, beneficiaries might manage their chronic conditions more effectively.

We first tested whether receipt of primary care increased as a result of incentives for wellness visits or preventive screenings. We also analyzed the impact of program incentives on specialty care, which could be expected to increase or decrease with greater use of primary care. Finally, we tested whether establishing a primary care relationship and improving management of chronic conditions would lead to less use of inefficient care, such as the emergency department when urgent care or primary care would have been appropriate.[66]

In this section, we present findings from analyses designed to address three subsidiary research questions (listed as Questions 3a, 3c, and 3b in Appendix Table A.3):

- Do behavior incentives yield gains in preventive care and chronic condition management?
- How do behavior incentives affect volume of and access to care?
- Do behavior incentives yield reductions in disincentivized care (that is, non-emergent emergency department visits)?

We used administrative data to conduct descriptive regressions of service use among demonstration beneficiaries, using the same model described in Section V.C. We limited the analysis to beneficiaries for whom we could observe a full 12 months of enrollment data; all such beneficiaries enrolled by January 2017. For services recommended for people in specific age/sex categories, or for outcomes that apply only to individuals with specific conditions, we also limited the analysis to beneficiaries in the specified categories. For Iowa beneficiaries, we also

[65] In Iowa and Michigan, the (explicitly or implicitly) incentivized behavior was a physician visit. In Indiana, the incentivized behavior was a preventive service, which could be accessed via a physician visit or, in some cases, through self-referral.

[66] Indiana's demonstration also involved a direct disincentive for inefficient care in the form of graduated co-payments for non-emergency use of the emergency department, but co-payment enforcement was left to the hospitals and was sporadic (Lewin Group 2017). The program disincentives were therefore not expected to strongly influence beneficiaries' behavior.

estimated how completing an HRA was associated with utilization outcomes, controlling for individual characteristics. (Appendix G contains descriptive analyses of administrative data on preventive care, chronic condition management, and other service use.)

1. Use of preventive care

We found that all three demonstrations were associated with greater use of preventive care, relative to the comparison states. Iowa's demonstration was associated with the largest increase in wellness visits, whereas Indiana's and Michigan's demonstrations were associated with greater increases in receipt of other preventive services, such as cancer screening. All three demonstrations were also associated with higher rates of receiving any preventive care, and with higher rates of completing all services recommended for the beneficiary's age and sex.

Our outcome measures were:

- Receipt of individual age- and sex-specific preventive services within a 12-month enrollment span, and receipt of any recommended service
- Completion of all age- and sex-specific recommended preventive services within a 12-month enrollment span
- Time to completion of all age- and sex-specific recommended preventive services

Table V.3 shows the preventive services recommended by age and sex that we included in our analysis. The list of services and their definitions were adapted from HEDIS and from the Core Set of Adult Health Care Quality Measures for Medicaid-Eligible Adults (Adult Core Set).

Table V.3. Recommended preventive services by age, sex, and risk profile

			Recommended preventive services				
Sex	Age	Sexually active	Wellness visit	Mammogram	Cervical cancer screening	Chlamydia screening	Colorectal cancer screening
M	19–49		X				
M	50–64		X				X
F	19–24	No	X		X		
F	19–24	Yes	X		X	X	
F	25–49		X		X		
F	50–64		X	X	X		X

Note: Chlamydia screening is a recommended preventive service for women ages 19–24 who are sexually active. We defined sexual activity using adapted Adult Core Set methodology, flagging as sexually active women who have claims indicating pregnancy, use of hormonal contraception, or chlamydia screening.

Regression results. Controlling for demographic and health characteristics, demonstration beneficiaries in all three states had higher rates of preventive service use than expansion beneficiaries in comparison states did. Table V.4 shows the results from our descriptive regression models, with effects estimated separately for the three demonstration states. All estimated average marginal effects were statistically significant at conventional levels. With the exception of time to completion of all preventive services, all differences between states were also statistically significant.

As noted, Iowa's demonstration was associated with the largest positive difference from the comparison states in terms of wellness visits. However, Iowa's demonstration was associated with smaller increases in use of other preventive services than the other two demonstrations; the increases in Iowa were between 1 and 10 percentage points smaller than the increases for demonstration beneficiaries in Indiana and Michigan.

Beneficiaries in Indiana were 9.8 percentage points more likely to receive a mammogram than beneficiaries in comparison states, and also 9.8 percentage points more likely to be screened for chlamydia. They were 5.1 and 5.7 percentage points more likely to be screened for cervical and colorectal cancer, respectively.

Iowa beneficiaries were 5.4 percentage points more likely than beneficiaries in comparison states to receive a mammogram, and 3.0 percentage points more likely to be screened for chlamydia. They were also more likely to receive cervical and colorectal cancer screening (0.8 and 2.7 percentage points, respectively).

Beneficiaries in Michigan were 7.0 percentage points more likely to receive a mammogram, 12.6 percentage points more likely to be screened for chlamydia, 5.6 percentage points more likely to receive a cervical cancer screening, and 4.3 percentage points more likely to receive a colorectal cancer screening than beneficiaries in the comparison states.

The completion rate for wellness visits in each state drives the rate of having any recommended preventive service, as beneficiaries typically access screenings through a wellness visit. Iowa's demonstration was correspondingly associated with the largest increase in the rate of completing any preventive service and, because a wellness visit is the only recommended preventive service for men ages 19 to 49, the state also had the largest increase in the probability of completing all recommended preventive services. The Indiana and Michigan demonstrations were also associated with a higher probability of completing all recommended preventive services than in comparison states. In those two states, however, the increases came more from higher screening rates than from higher rates of wellness visits. Among beneficiaries who completed all recommended preventive services for their age and sex, beneficiaries in Iowa completed them about 10 days earlier than beneficiaries in comparison states did. The difference was 9 days for beneficiaries in Indiana and Michigan.

Table V.4. Estimated association between beneficiary engagement policies in demonstration states and completion of preventive services, 2014–2017

		Indiana				Iowa				Michigan				
Measure	Comparison group mean (percent)	Average marginal effect	Standard error	*p*-value	Percent difference	Average marginal effect	Standard error	*p*-value	Percent difference	Average marginal effect	Standard error	*p*-value	Percent difference	N
Wellness visit	70.7	4.5***	0.1	.000	6.4	8.2***	0.1	.000	11.6	3.0***	0.0	.000	4.2	7,505,336
Mammogram	30.7	9.8***	0.3	.000	31.9	5.4***	0.2	.000	17.6	7.0***	0.1	.000	22.7	1,020,492
Chlamydia screening	43.7	9.8***	0.4	.000	22.5	3.0***	0.4	.000	6.8	12.6***	0.2	.000	28.8	399,544
Cervical cancer screening	16.9	5.1***	0.1	.000	29.8	0.8***	0.1	.000	4.9	5.6***	0.1	.000	33.3	3,848,784
Colorectal cancer screening	11.3	5.7***	0.2	.000	50.3	2.7***	0.1	.000	24.0	4.3***	0.1	.000	38.2	1,906,327
Any preventive service	71.0	4.6***	0.1	.000	6.5	8.2***	0.1	.000	11.5	3.0***	0.0	.000	4.2	7,505,336
Completed all preventive services	29.6	5.6***	0.1	.000	18.9	4.9***	0.1	.000	16.7	4.0***	0.0	.000	13.6	7,505,336
Time to completion of all preventive services (days)	135.0	-9.1***	0.3	.000	-6.8	-10.1***	0.3	.000	-7.5	-9.4***	0.2	.000	-7.0	2,294,408

Source: Mathematica analysis of administrative data, 2014–2017, for Indiana, Iowa, and Michigan (demonstration states); and Kentucky, New Mexico, Ohio, Pennsylvania, and West Virginia (comparison states). Years included for each state depend on the date of demonstration implementation or coverage expansion and on data availability.

Notes: Marginal effects were estimated using logistic and ordinary least squares regressions. We calculated the average of the estimated difference in outcomes between demonstration and comparison groups, using the covariate distribution of the demonstration group. Control variables included age, sex, CDPS score, and an indicator for residing in a rural area.

The comparison group mean is the unadjusted comparison sample mean, presented for reference. The average marginal effect should not be added to the comparison group mean to calculate use by enrollees in demonstration states.

Length of time between enrollment and completion of all recommended preventive services was calculated only for individuals who completed all recommended preventive services.

*** Significantly different from zero at the .01 level, two-tailed test.

CDPS = Chronic Illness and Disability Payment System.

Descriptive results on HRAs. To assess whether completing an HRA was associated with higher rates of using preventive services, we conducted descriptive regressions (controlling for demographic and health characteristics) to compare utilization rates for Iowa beneficiaries who did and did not receive credit for completing an HRA.

Completing an HRA was strongly correlated with getting preventive services (Table V.5). Beneficiaries who were credited with an HRA were at least 7.7 percentage points more likely to complete a wellness visit, a mammogram, cervical cancer screening, and colorectal cancer screening. The exception was chlamydia screening for sexually active women ages 19–24; the estimated difference was small and not statistically significant. Compared with beneficiaries who had not done an HRA, beneficiaries who completed an HRA were more likely to have had any preventive service and were also more likely to have had all recommended preventive services for their age and sex. None of these results imply causality; the observed correlations could simply be documenting that people with an HRA were generally more engaged in their health care.

Table V.5. Estimated association between completing an HRA and getting preventive services among Iowa beneficiaries in 2014–2016

Measure	Mean among those who did not complete HRA (percent)	Difference among beneficiaries completing HRA	Standard error	*p*-value	Percent difference	N
Wellness visit	77.1	9.2***	0.1	.000	11.9	366,215
Mammogram	29.3	17.9***	0.5	.000	61.4	49,355
Chlamydia screening	45.9	0.1	0.9	.954	0.1	22,924
Cervical cancer screening	16.2	8.9***	0.2	.000	54.8	191,666
Colorectal cancer screening	11.6	7.7***	0.3	.000	66.3	92,119
Any preventive service	77.3	9.1***	0.1	.000	11.8	366,215
Completed all preventive services	34.2	8.3***	0.2	.000	24.1	366,215
Time to completion of all preventive services (days)	125.0	-2.4***	0.6	.000	-2.0	126,308

Source: Mathematica analysis of state administrative data from 2014–2016 for Iowa.

Notes: Differences (average marginal effects) were estimated using logistic and ordinary least squares regressions. We calculated the average of the estimated difference in outcomes between those who completed an HRA and those who did not, using the covariate distribution of the group that completed an HRA. Control variables included age, sex, CDPS score, and an indicator for residing in a rural area.

*** Significantly different from zero at the .01 level, two-tailed test.

HRA = health risk assessment. CDPS = Chronic Illness and Disability Payment System.

2. Managing chronic conditions

Although we found that demonstration incentives increased the likelihood of accessing preventive services, we did not find consistent evidence that demonstrations were associated with better management of chronic conditions in demonstration states. The proportion of beneficiaries who received an HbA1c test was higher in Michigan and Indiana and lower in Iowa than it was in the comparison states. Hospitalization rates for chronic conditions were not consistently associated—positively or negatively—with any demonstration. Rates of follow-up physician visits after a hospitalization were also inconsistent across states.

Our outcome measures for these analyses were:

- Among diabetic beneficiaries: completion of an HbA1c test and admission to hospital for diabetes treatment
- Among all beneficiaries: admission to hospital for heart failure
- Among all beneficiaries ages 19−39: admission to hospital for asthma treatment
- Among all beneficiaries ages 40−64: admission to hospital for chronic obstructive pulmonary disease (COPD) treatment
- Among beneficiaries with an acute inpatient hospitalization: follow-up visit with physician within 30 days of discharge
- Among beneficiaries with an acute inpatient hospitalization for mental illness: follow-up visit with mental health professional within 30 days of discharge

Only one measure (HbA1c test) is a service that beneficiaries can use to actively manage their chronic condition, so any increase represents a positive change. The next three measures are on hospitalizations to treat complications of chronic conditions that are considered preventable with adequate primary care. Thus, increased use of these measures is viewed as a negative: if beneficiaries in demonstration states managed chronic conditions better, we would expect to see lower rates of avoidable hospitalizations in those states when controlling for other factors. When there is an acute hospitalization, prompt follow-up in the month after discharge is considered the standard of care. The final two measures examine how often these follow-up visits take place; increases in these measures are indicative of improved access to care.

Regression results. The results from our descriptive regression models (shown in Table V.6) do not tell a consistent story about the demonstrations' effects on managing chronic conditions. In some cases, we found no changes in outcomes associated with the demonstrations. For example, there did not appear to be a strong association between any of the three demonstrations and hospitalization for respiratory conditions (asthma for beneficiaries ages 19–39 and COPD for beneficiaries ages 40–64).

Some of the statistically significant changes had the expected associations, whereas others moved in unexpected directions. For example, Iowa beneficiaries with diabetes had, on average, a lower probability of receiving an HbA1c test during a 12-month enrollment span than beneficiaries with diabetes in comparison states (an average marginal effect of −4.1 percentage points, and a statistically significant result). In contrast, Indiana and Michigan beneficiaries with diabetes had a significantly higher probability of receiving an HbA1c test than beneficiaries in the comparison states did (8.7 percentage points and 9.5 percentage points higher, respectively).

Demonstration beneficiaries in Iowa were also more likely to be admitted to the hospital for diabetes and for heart failure than beneficiaries in comparison states were, controlling for demographic and health characteristics.[67] Beneficiaries in Indiana were statistically as likely as comparison beneficiaries to be admitted for diabetes, but less likely to be admitted for heart failure. There was no statistically significant association between Michigan's demonstration and diabetes or heart failure admissions.

The three states differed in their rates of physician follow-up visits after an acute hospitalization. Beneficiaries in Indiana were statistically no more or less likely than beneficiaries in comparison states to have a follow-up visit after any hospitalization, but 21.4 percent more likely to have a follow-up visit after a hospitalization for mental health.[68] Beneficiaries in Iowa were 8.8 percentage points more likely than beneficiaries in comparison states to have a follow-up visit after any hospitalization and 4.6 percentage points more likely to have a mental health follow-up visit after a hospitalization for a mental health issue. The average marginal effects for these two states were statistically significant at conventional levels. Beneficiaries in Michigan were 2.9 percentage points more likely to have a follow-up visit after any hospitalization, but 1.6 percentage points less likely to have a follow-up visit after a hospitalization for mental health than beneficiaries in comparison states were. All differences between states in rates of follow up were statistically significant.

[67] Although the magnitude of differences in hospitalization rates were small, the baseline probability of hospitalization was also small. Small differences in the probability of hospitalization can therefore represent large percentage differences. Hospitalization is rare, but our sample included enough hospitalizations for each chronic condition that our maximum likelihood estimate was unlikely to suffer from small-sample bias.

[68] Indiana Medicaid has a Behavioral & Primary Healthcare Coordination Program which offers coordination of health care services to manage mental health needs of eligible recipients, including HIP 2.0 beneficiaries (https://www.in.gov/medicaid/members/204.htm). These coordination services could explain the high rate of connecting beneficiaries with outpatient mental health care following a hospitalization.

Table V.6. Estimated association between beneficiary engagement policies in demonstration states and chronic condition management in 2014–2017

Measure	Comparison group mean (percent)	Indiana				Iowa				Michigan				N
		Average marginal effect (percentage points)	Standard error	*p*-value	Percent difference	Average marginal effect (percentage points)	Standard error	*p*-value	Percent difference	Average marginal effect (percentage points)	Standard error	*p*-value	Percent difference	
Diabetes														
HbA1c test	76.72	8.70***	0.28	.000	11.34	-4.13***	0.35	.000	-5.39	9.45***	0.15	.000	12.32	496,632
Short-term hospital admission for diabetes	1.66	-0.13	0.09	.175	-7.67	0.60***	0.11	.000	36.00	-0.03	0.05	.629	-1.53	496,632
Heart failure														
Short-term hospital admission for heart failure	0.12	-0.02***	0.01	.008	-16.06	0.04***	0.01	.000	29.86	0.00	0.00	.355	2.58	7,505,336
Respiratory conditions														
Short-term hospital admission for asthma (ages 19–39)	0.05	0.01	0.01	.063	22.00	-0.01	0.01	.266	-12.01	0.01***	0.00	.001	21.28	4,123,844
Short-term hospital admission for COPD (ages 40–64)	0.41	-0.03	0.02	.135	-7.46	-0.04***	0.02	.009	-10.50	-0.02***	0.01	.008	-6.07	3,381,492
Follow-up after hospitalization														
Follow-up with physician within 30 days of hospital discharge	58.11	0.57	0.31	.062	0.99	8.83***	0.29	.000	15.19	2.93***	0.17	.000	5.04	588,212
Follow-up with mental health professional within 30 days of hospital discharge for mental health	23.63	21.39***	0.90	.000	90.49	4.64***	0.74	.000	19.64	-1.56***	0.52	.003	-6.60	76,410

Source: Mathematica analysis of administrative data, January 2014–December 2017, for Indiana, Iowa, and Michigan (demonstration states) and Kentucky, New Mexico, Ohio, Pennsylvania, and West Virginia (comparison states). Years included for each state depend on the date of demonstration implementation or coverage expansion and on data availability.

Notes: Marginal effects were estimated using logistic and ordinary least squares regressions. We calculated the average of the estimated difference in outcomes between demonstration and comparison, using the covariate distribution of the demonstration group. Control variables include age, sex, CDPS score, and an indicator for residing in a rural area.

The comparison group mean is the unadjusted comparison sample mean, presented for reference. The average marginal effect should not be added to the comparison group mean to calculate use by enrollees in demonstration states.

*** Significantly different from zero at the .01 level, two-tailed test.

CDPS = Chronic Illness and Disability Payment System; COPD = chronic obstructive pulmonary disease.

Descriptive results on HRAs. In general, Iowa beneficiaries who completed an HRA managed their chronic conditions more effectively than beneficiaries who did not (Table V.7). Beneficiaries with diabetes who got credit for completing the HRA had a higher probability (4.0 percentage points) of receiving an HbA1c test and a lower probability (-0.5 percentage points) of a hospital admission for diabetes during a 12-month enrollment span compared with diabetic beneficiaries who did not get credit for the HRA. Completing an HRA was also associated with a 39 percent lower probability of a hospital admission for heart failure, but was not significantly associated with hospitalizations for respiratory conditions. Iowa beneficiaries who completed an HRA were more likely to see a physician within 30 days of being discharged from the hospital for an acute condition (4.8 percentage points), and also more likely to have a follow-up visit after a discharge from the hospital for a mental health condition (10.5 percentage points).

Table V.7. Estimated association between completing an HRA and managing a chronic condition among Iowa beneficiaries in 2014–2015

Measure	Mean among those who did not complete HRA (percent)	Difference for beneficiaries completing HRA	Standard error	*p*-value	Percent difference	N
HbA1c test	72.30	4.03***	0.61	.000	5.58	24,388
Short-term admission for diabetes	2.61	-0.54***	0.16	.001	-20.66	24,388
Short-term admission for heart failure	0.12	-0.05***	0.02	.006	-39.11	366,215
Short-term admission for asthma (ages 19–39)	0.05	-0.01	0.01	.306	-27.67	203,873
Short-term admission for COPD (ages 40–64)	0.33	-0.03	0.03	.359	-9.72	162,342
Follow-up within 30 days of discharge	65.63	4.77***	0.65	.000	7.27	25,908
Follow-up within 30 days (mental health)	24.21	10.46***	2.11	.000	43.22	3,771

Source: Mathematica analysis of state administrative data from Iowa, January 2014–December 2016.
Notes: Differences (average marginal effects) were estimated using logistic and ordinary least squares regressions. We calculated the average of the estimated difference in outcomes between those who completed an HRA and those who did not, using the covariate distribution of the group that completed an HRA. Control variables include age, sex, CDPS score, and an indicator for residing in a rural area.
*** Significantly different from zero at the .01 level, two-tailed test.
CDPS = Chronic Illness and Disability Payment System: COPD = chronic obstructive pulmonary disease; HRA = health risk assessment.

3. Receipt of primary or specialty care from a physician

As noted, the incentives to receive a wellness visit and complete an HRA constitute an implicit incentive to form a primary care relationship. However, the broad definition of a wellness visit (both the HEDIS definition we use and the set of services allowed by Iowa in its demonstration) could obscure the rate at which beneficiaries receive comprehensive primary care. We used

administrative data to explore whether the beneficiary engagement demonstrations are associated with receipt of an evaluation and management (E&M) visit with a primary care physician.

We also explored the association of each state's demonstration with receipt of an E&M visit by a specialty physician. Especially in Iowa and Indiana, where no demonstration incentive involves a primary care physician, a specialty physician visit could count toward completion of the incentivized behaviors. Thus, specialty care utilization could increase as a direct result of demonstration incentives. If beneficiary engagement incentives are effective in increasing primary care, however, the indirect effect on specialty care utilization could go in either direction. First, regular primary care can result in increased preventive care and better management of chronic conditions, such that there is less need for specialty care for advanced conditions (Reschovsky et al. 2012). Second, an increase in the number of visits with primary care physicians who direct care and make appropriate referrals could decrease self-referrals to specialists, leading to reductions in specialty care utilization.[69] On the other hand, especially in the early stages of expansion, there might be a "discovery effect," that is, increased primary care utilization could lead to the discovery of previously undiagnosed or untreated conditions requiring specialty care (Donabedian 1976).

Finally, we explored the volume of care received in each of these categories. Indiana's demonstration involved an explicit incentive to reduce utilization of care through its POWER account rollover structure. Surveys did not indicate that beneficiaries were strongly affected by this incentive. However, to the extent some were, we would expect to observe lower levels of utilization, especially of expensive services, among Indiana beneficiaries.

Our outcome measures were:

- Probability of a primary care service in an ambulatory setting, and number of such visits
- Probability of a specialty care service in an ambulatory setting, and number of such visits

Regression results. Only beneficiaries in Michigan used primary care more than beneficiaries in comparison states (11.9 percentage points). Beneficiaries in Indiana and Iowa had less use of primary care than beneficiaries in comparison states (3.5 and 2.6 percentage points, respectively). In contrast, all three state demonstrations were associated with greater specialty care utilization. However, the average marginal effect for Indiana was almost three times as large as that for Michigan (10.2 percentage points versus 3.6), and the effect for Iowa was almost twice as large (6.2 percentage points). All results were statistically significant at conventional levels (Table V.8).

Only Michigan's demonstration required the involvement of a primary care provider to complete the incentivized behaviors and earn a financial incentive, as a primary care provider must assist with completion of the HRA and sign off on a beneficiary's health goal. In Indiana and Iowa, the

[69] A number of other factors also contribute to utilization of specialty care, including coverage by managed care organizations, local supply of specialist physicians, and regional practice patterns.

incentivized behaviors could be completed by seeking care from a range of providers, including specialists. The finding that Michigan, with greater use of primary care than comparison states, also had greater specialty care use than comparison states suggests that the discovery effect likely occurred in the expansion population. However, the finding that Michigan had lower specialty care use than the other demonstration states, which did not have explicit primary care incentives, suggests that greater primary care use can lead to reduced specialty care use, possibly through one of the other proposed mechanisms above.

Our finding that Indiana had lower primary care use and higher specialty care use than other (demonstration and comparison) states suggests that incentives to control health expenditures did not shift utilization from more to less expensive categories of service, or indeed reduce the overall volume of care.

Table V.8. Estimated association between beneficiary engagement policies in demonstration states and receipt of primary and specialty care in 2014–2017

		Indiana				Iowa				Michigan			
Measure	Comparison group mean	Average marginal effect	Standard error	*p*-value	Percent difference	Average marginal effect	Standard error	*p*-value	Percent difference	Average marginal effect	Standard error	*p*-value	Percent difference
Primary care visit (percent)	43.7	-3.5***	0.090	.000	-7.9	-2.6***	0.089	.000	-5.9	11.9***	0.051	.000	27.2
Number of primary care visits	1.6	-0.1***	0.006	.000	-4.5	-0.2***	0.006	.000	-13.9	0.6***	0.004	.000	38.5
Specialty care visit (percent)	56.9	10.2***	0.082	.000	18.0	6.2***	0.079	.000	10.9	3.6***	0.046	.000	6.4
Number of specialty care visits	3.2	1.6***	0.017	.000	49.2	0.1***	0.015	.000	4.0	0.1***	0.009	.000	3.2

Source: Mathematica analysis of administrative data, 2014–2017, for Indiana, Iowa, and Michigan (demonstration states); and Kentucky, New Mexico, Ohio, Pennsylvania, and West Virginia (comparison states). Years included for each state depend on the date of demonstration implementation or coverage expansion and on data availability.

Notes: Marginal effects were estimated using logistic and ordinary least squares regressions (7,505,336 observations). We calculated the average of the estimated difference in outcomes between demonstration and comparison, using the covariate distribution of the demonstration group. Control variables included age, sex, CDPS score, and an indicator for residing in a rural area.

The comparison group mean is the unadjusted comparison sample mean, presented for reference. The average marginal effect should not be added to the comparison group mean to calculate use by enrollees in demonstration states.

*** Significantly different from zero at the .01 level, two-tailed test.

4. Use of emergency department for non-emergency visits

An additional expected benefit of promoting primary care relationships is reduced reliance on the emergency department (ED) for conditions that could be treated by primary care providers. We used administrative data to explore use of the ED, which is considered an inefficient venue of care for non-emergency conditions. In this subsection, we present analyses that address one subsidiary research question (listed as Question 3b in Appendix Table A.3), "Which behavior incentives yield the greatest reductions in disincentivized care (that is, non-emergent ED visits)?"

Indiana's demonstration involved a graduated co-payment structure[70] for non-emergency use of the emergency department, which was intended to divert beneficiaries to more efficient sources of care. Therefore we also explored utilization of urgent care, which is an alternative and less expensive venue for beneficiaries who need immediate medical care that does not rise to the level of an emergency.

Our outcome measures were:

- Probability of an ED visit during the 12-month span, and number of ED visits
- Probability of a non-emergent ED visit during the 12-month span, and number of non-emergent ED visits
- Probability of an urgent care visit during the 12-month span, and number of urgent care visits (for Indiana and Michigan only)[71]

Iowa's and Michigan's demonstrations were associated with reduced use of emergency care in 2014–2017, with differences of 1.7 and 3.2 percentage points, respectively. The probability of visiting an ED for a non-emergent condition was also lower in both states than in the comparison states (2.6 and 2.7 percentage points). Indiana's demonstration was associated with higher ED use than the comparison state expansions (2.8 percentage points), and also with higher ED use for non-emergent conditions (7.4 percentage points). These differences in probabilities were also reflected in lower (higher) numbers of trips to the ED both overall and for non-emergent care.

Beneficiaries in Indiana and Michigan were 1.0 and 4.3 percentage points less likely to use urgent care, respectively. Results on ED and urgent care use are shown in Table V.9.

Indiana's demonstration had two pathways to achieve reduced ED use—as a downstream consequence of increased primary care use, and independently through the disincentive from the graduated ED co-payment structure. However, Indiana's demonstration was not associated with increased primary care utilization and, as of 2017, it was difficult to judge the implementation of the copayment disincentive due to data limitations. Most (72 percent) beneficiaries with an ED

[70] Co-payments for ED use were $8 for the first non-emergency visit, increasing to $25 per visit after that.

[71] Our definition of urgent care was unsuccessful in identifying urgent care utilization in Iowa, so we limited the analysis to Indiana and Michigan.

visit had their copayment waived, although it was not possible to know if it was waived according to the demonstration's cost-sharing exclusions or for other reasons (Lewin Group 2017). Therefore, it is possible that neither mechanism for reduced ED use held in Indiana during the first three years of the demonstration, and perhaps unsurprisingly, we did not find reduced ED use among the expansion population. It is not clear why Indiana beneficiaries would use ED services at a substantially higher rate than comparison state beneficiaries did.

Descriptive results on HRA completion. Completing an HRA was correlated with reduced use of the emergency department (Table V.10). Beneficiaries who were credited with HRA completion had a 5.9 percentage point lower probability of visiting the ED during the 12-month span, and a 2.7 percentage point lower probability of visiting the ED for a non-emergency condition. The average number of emergency department trips, both overall and for non-emergencies, was lower among those who completed HRAs. These differences were statistically significant at conventional levels.

Table V.9. Estimated association between beneficiary engagement policies in demonstration states and use of the ED and urgent care in 2014–2017

		Indiana				Iowa				Michigan			
Measure	Comparison group mean	Average marginal effect	Standard error	*p*-value	Percent difference	Average marginal effect	Standard error	*p*-value	Percent difference	Average marginal effect	Standard error	*p*-value	Percent difference
ED visit (percent)	41.9	2.84***	0.089	.000	6.8	-1.74***	0.085	.000	-4.2	-3.21***	0.049	.000	-7.7
Number of ED visits	1.1	0.17***	0.005	.000	15.3	-0.06***	0.004	.000	-5.3	-0.14***	0.002	.000	-12.5
Non-emergency ED visit (percent)	20.4	7.36***	0.083	.000	36.0	-2.60***	0.070	.000	-12.7	-2.74***	0.040	.000	-13.4
Number of non-emergency ED visits	0.3	0.19***	0.002	.000	57.9	-0.05***	0.002	.000	-14.1	-0.05***	0.001	.000	-16.2
Urgent care visit (percent)	8.5	-1.03***	0.052	.000	-12.2	NA	NA	NA	NA	-4.29***	0.023	.000	-50.7
Number of urgent care visits	0.1	-0.01***	0.001	.000	-6.2	NA	NA	NA	NA	-0.08***	0.001	.000	-53.8

Source: Mathematica analysis of administrative data, 2014–2017, for Indiana, Iowa, and Michigan (demonstration states); and Kentucky, New Mexico, Ohio, Pennsylvania, and West Virginia (comparison states). Years included for each state depend on the date of demonstration implementation or coverage expansion and on data availability. Iowa was excluded from the urgent care analyses for implausibly low visit counts.

Notes: Marginal effects were estimated using logistic and negative binomial regressions (7,505,336 observations). We calculated the average of the estimated difference in outcomes between demonstration and comparison, using the covariate distribution of the demonstration group. Control variables included age, sex, CDPS score, and an indicator for residing in a rural area.

The comparison group mean is the unadjusted comparison sample mean, presented for reference. The average marginal effect should not be added to the comparison group mean to calculate use by enrollees in demonstration states.

*** Significantly different from zero at the .01 level, two-tailed test.

CDPS = Chronic Illness and Disability Payment System; ED = emergency department; NA = not available.

Table V.10. Estimated association between HRA completion and use of the emergency department among Iowa beneficiaries, 2014–2015

	Mean among those who did not complete HRA	Difference among beneficiaries who completed HRA	Standard error	*p*-value	Percent difference
Likelihood of any ED visit (%)	41.5	-5.9***	0.18	.000	-14.2
Number of ED visits	1.1	-0.3***	0.01	.000	-25.6
Likelihood of ED visit for non-emergent care (%)	18.5	-2.7***	0.15	.000	-14.6
Number of ED visits for non-emergent care	0.3	-0.1***	0.00	.000	-19.6

Source: Mathematica analysis of state administrative data from Iowa, 2014–2016.

Notes: Differences (average marginal effects) were estimated using logistic and ordinary least squares regressions (366,215 observations). We calculated the average of the estimated difference in outcomes between those who completed an HRA and those who did not, using the covariate distribution of the group that completed an HRA. Control variables include age, sex, CDPS score, and an indicator for residing in a rural area.

*** Significantly different from 0 at the .01 level, two-tailed test.

ED = emergency department; HRA = health risk assessment.

E. Evidence that demonstration incentives have population-level effects

In this section, we present analyses reflecting the following research question (listed as Question 4 in Appendix Table A.3):

- Are population-level effects observed from Medicaid demonstration policies?

We used BRFSS data from 2012 to 2017 to assess whether the demonstrations were associated with any population-level changes in self-reported receipt of care and health behaviors such as tobacco use and physical activity. Such population-level effects could arise if impacts for Medicaid beneficiaries were sufficiently large or diffused into the broader population through contact between beneficiaries and their peers and relatives who are not directly enrolled. Although we limited the BRFSS sample to respondents who reported under $35,000 in annual household income, the individuals represented are a broader group than that represented in the Medicaid administrative data. Specifically, surveyed individuals could have been enrolled in their state's Medicaid demonstration, they could have been enrolled in another Medicaid program, or they could not have been covered by Medicaid at all. More information on our methods for using BRFSS data can be found in Appendix E.

1. Preventive care and healthy behaviors

Our outcome measures were:

- Receipt of a checkup within the last year
- Having cholesterol checked in the last year
- Receipt of a flu shot in the last 12 months
- Tobacco use
- Engagement in physical activity in the last month

Regression results. Survey respondents could have interpreted the term "checkup" in various ways, although most would likely count a wellness visit (as defined above) as a checkup. We found that the estimated rate of reporting a checkup in the previous year increased in Michigan. We did not find statistically significant changes in reported checkups in Indiana or Iowa. These findings differ from our results using administrative data, where we found that beneficiaries received wellness visits at much higher rates in Iowa than in Michigan.

We found a small estimated decrease in the rate of self-reported cholesterol checks within the past year among respondents in Indiana. The decrease was statistically significant at conventional levels, controlling for individual characteristics (Table V.11).

For other outcomes, we found that residing in Indiana, Iowa, or Michigan after demonstration implementation was not associated with different outcomes than were reported in the comparison states.[72] We did not find strong evidence of an association between the states' demonstrations and self-reported receipt of a flu shot, smoking, or physical activity.

Table V.11. Estimated association of self-reported health behaviors with residence in a state with a beneficiary engagement demonstration

Measure/state	Pre-expansion unadjusted mean (percentage)	Regression-adjusted average marginal effect	Standard error	*p*-value
Checkup within last year				
Indiana	53.6	1.7	1.3	.203
Iowa	56.9	1.4	1.4	.316
Michigan	58.6	4.3***	1.2	.000
Comparison	58.1			
N = 75,146,298				
Had cholesterol checked in last year				
Indiana	67.5	-4.6**	1.7	.008
Iowa	66.4	-1.0	2.0	.624
Michigan	64.0	3.0	1.7	.084
Comparison	66.2			
N = 25,555,829				
Had a flu shot in last 12 months				
Indiana	26.3	-0.1	1.3	.945
Iowa	34.3	-3.1	1.6	.057
Michigan	24.4	0.5	1.2	.658
Comparison	28.2			
N = 70,771,097				

[72] We conducted tests to assess whether our results were sensitive to different modeling decisions. Results were similar across all specifications. First, we ran separate regressions for each demonstration state, excluding the other two demonstration states. Then, we restricted the effect of beneficiary engagement policies to be the same across the three demonstration states.

Table V.11 (*continued*)

Measure/state	Pre-expansion unadjusted mean (percentage)	Regression-adjusted average marginal effect	Standard error	*p*-value
Tobacco use				
Current smoker				
Indiana	39.1	-1.4	1.3	.282
Iowa	33.7	1.5	1.5	.305
Michigan	38.1	0.4	1.2	.735
Comparison	36.6			
N = 74,155,588				
Tried to quit in last 12 months				
Indiana	61.2	-1.4	2.3	.556
Iowa	57.1	-2.0	2.8	.463
Michigan	65.8	-3.0	2.2	.167
Comparison	60.0			
N = 26,283,163				
Engaged in physical activity or exercise in last month				
Indiana	64.6	-0.4	1.3	.789
Iowa	68.1	2.6	1.6	.098
Michigan	69.1	-0.4	1.2	.724
Comparison	69.8			
N = 72,962,421				

Source: Mathematica's analysis of Behavioral Risk Factor Surveillance System data, 2012–2017. Comparison states included Kentucky, Nevada, New Mexico, North Dakota, Ohio, Oregon, Pennsylvania, Washington, and West Virginia.

Notes: We estimated average marginal effects using logistic regressions. We calculated the average of the estimated difference in differences, using the covariate distribution of the demonstration group. Control variables included age, sex, race/ethnicity, education, marital status, employment status, income, and indicators for disability and for the presence of a child in the household.

** Significantly different from zero at the .05 level, two-tailed test.

*** Significantly different from zero at the .01 level, two-tailed test.

2. Cancer screening

Our outcome measures were:

- Receipt of cancer screenings in the past year (mammograms, Pap tests, prostate-specific antigen tests, blood stool tests, colonoscopies)

Regression results. We found no consistent association between self-reported receipt of cancer screenings and residence in a state with an active beneficiary engagement demonstration. Respondents in Michigan were significantly more likely to have received a Pap test (cervical cancer screening) in the past year than respondents in the comparison states. Respondents in Iowa were statistically significantly more likely to have had a prostate-specific antigen test in the past year than comparison state respondents were (Table V.12).

Table V.12. Estimated association of self-reported receipt of cancer screenings with residence in a state with a beneficiary engagement demonstration

Measure	Pre-expansion unadjusted mean (percentage)	Regression-adjusted average marginal effect	Standard error	*p*-value
Received mammogram in past year				
Indiana	44.9	1.1	3.4	.751
Iowa	54.9	4.0	3.8	.292
Michigan	51.2	1.8	3.1	.567
Comparison	51.1			
N = 11,415,828				
Received Pap test in past year				
Indiana	44.0	-1.5	2.8	.582
Iowa	52.3	0.8	3.1	.794
Michigan	50.2	5.7**	2.5	.022
Comparison	51.0			
N = 19,185,598				
Had prostate-specific antigen test in past year				
Indiana	48.0	9.6	6.8	.158
Iowa	43.8	15.1**	7.4	.040
Michigan	55.5	2.3	5.8	.692
Comparison	54.6			
N = 2,745,607				
Had blood stool test in past year				
Indiana	29.1	-5.5	5.1	.277
Iowa	26.0	-6.2	5.7	.275
Michigan	26.8	-1.0	4.0	.806
Comparison	33.3			
N = 3,545,690				
Had sigmoidoscopy/colonoscopy in past year				
Indiana	24.8	2.0	3.6	.584
Iowa	25.1	-2.2	4.2	.592
Michigan	27.8	-2.7	2.7	.328
Comparison	24.9			
N = 7,675,543				

Source: Mathematica's analysis of Behavioral Risk Factor Surveillance System data, 2012–2017. Comparison states included Kentucky, Nevada, New Mexico, North Dakota, Ohio, Oregon, Pennsylvania, Washington, and West Virginia.

Notes: We estimated marginal effects using logistic regressions. We calculated the average of the estimated difference in differences, using the covariate distribution of the demonstration group. Control variables included age, sex, race/ethnicity, education, marital status, employment status, income, and indicators for disability and for the presence of a child in the household. Models for breast cancer screening, Pap tests, and prostate cancer screening exclude sex as a control variable because only female or male respondents were asked about the applicable screenings.

** Significantly different from zero at the .05 level, two-tailed test.

3. Management of chronic conditions

Our outcome measures were:

- Currently taking blood pressure medication
- Diabetes management (currently takes insulin, has seen a doctor in the past year about diabetes, had A1c checked in the last year)

Regression results. We did not find evidence that demonstrations in Indiana, Iowa, or Michigan were associated with better management of chronic conditions through 2017 relative to comparison states. After we controlled for individual characteristics, rates of taking medication to control high blood pressure were unchanged in the demonstration states relative to the comparison states (Table V.13). Among survey respondents with diabetes, we found no difference in rates of two measures of self-reported diabetes treatment (taking insulin and having HbA1c checked) associated with residing in Indiana, Iowa, or Michigan after Medicaid expansion at conventional significance levels.

In Michigan, respondents with diabetes were less likely to report having had a diabetes-related doctor visit than respondents in the comparison states. This finding contrasts with other findings indicating that respondents in Michigan had a greater likelihood of having a checkup and of receiving a Pap test in the past year (Tables V.11 and V.12). Diabetic respondents might have had trouble accessing care for their diabetes, but our results indicate that respondents did not have trouble accessing other types of care. Our estimates might be influenced by the fact that the population included in the analysis is not limited to demonstration beneficiaries. Alternatively, it could be that respondents were less likely to report seeing a doctor specifically for diabetes if they had a general wellness visit, which might explain the opposing directions of those two findings in Michigan.

Table V.13. Estimated association of self-reported management of high blood pressure and diabetes with residence in a state with a beneficiary engagement demonstration

Measure	Pre-expansion unadjusted mean (percentage)	Regression-adjusted average marginal effect	Standard error	*p*-value
Currently taking blood pressure medication				
Indiana	68.2	-0.7	2.6	.772
Iowa	65.0	0.3	3.0	.915
Michigan	61.6	2.7	2.5	.293
Comparison	66.3			
N = 11,111,181				
Diabetes management				
Currently takes insulin				
Indiana	38.7	-1.8	4.6	.704
Iowa	44.6	-2.8	6.6	.673
Michigan	40.5	3.0	5.8	.608
Comparison	37.5			
N = 4,730,931				
Has seen a doctor in the past year about diabetes				
Indiana	85.0	-2.3	3.2	.485
Iowa	90.9	-2.1	2.7	.437
Michigan	90.7	-6.9**	2.8	.014
Comparison	86.8			
N = 4,632,778				
Had A1c checked in the last year				
Indiana	87.3	-3.3	3.2	.301
Iowa	94.3	-0.7	2.8	.813
Michigan	86.1	-2.8	3.3	.408
Comparison	86.4			
N = 4,352,854				

Source: Mathematica's analysis of Behavioral Risk Factor Surveillance System data, 2012–2017. Comparison states included Kentucky, Nevada, New Mexico, North Dakota, Ohio, Oregon, Pennsylvania, Washington, and West Virginia.

Notes: We estimated marginal effects using logistic regressions. We calculated the average of the estimated difference in differences, using the covariate distribution of the demonstration group. Control variables included age, sex, race/ethnicity, education, marital status, employment status, income, and indicators for disability and for the presence of a child in the household.

** Significantly different from zero at the .05 level, two-tailed test.

F. Limitations

Administrative data. A common limitation to the analyses based on administrative data is that the Medicaid administrative data were drawn from different sources. For Indiana, we used the T-MSIS Analytic File (TAF) for all included years (2015–2016). We obtained data directly from Iowa for 2014 and 2015, and used TAF for 2016 and 2017. For Michigan, we used MAX data in 2014, Alpha-MAX and TAF in 2015, and TAF in 2016 and 2017. For our comparison states, we used a mix of MAX, Alpha-MAX, and TAF, with the transitions between those sources occurring at different points during our study period for different states. We standardized the data from these disparate sources so they could be used together, but residual differences in data structure and content likely remain that could introduce systematic differences in how our outcome measures were constructed. For example, as discussed in Chapter II, we noted that in each state claim counts were lower in the quarters immediately preceding the TAF changeover, especially so in the final quarter before the changeover. This means that the claim record may be incomplete in some quarters, but which quarters are affected differs by state. As our analysis relies on construction of spans which may cross the MAX-TAF changeover period, we cannot eliminate this data issue and note it as an important limitation.

A second limitation concerns our restriction to 12-month enrollment spans. We imposed this restriction to observe the full period of time over which demonstration beneficiaries in Indiana, Iowa, and Michigan can complete their incentivized behaviors and earn financial rewards, but it limits our analysis in an important way. A large proportion of beneficiaries are enrolled for less than 12 months.[73] By limiting to 12-month spans we excluded between 13 and 69 percent of enrollment spans in each state. In Michigan, as long as beneficiaries were enrolled six months or longer, they could benefit financially from completing the incentivized behaviors and receiving the reward. In Indiana and Iowa, although the financial rewards only applied to beneficiaries who renewed their enrollment after the first year, the incentives may have affected behavior in shorter spans if beneficiaries expected to remain enrolled for longer than 12 months.[74]

Most important, because our analyses were cross-sectional and limited to the period after states expanded Medicaid—with or without a section 1115 demonstration—we cannot infer causality from the results. A number of state-specific factors may have influenced outcomes among expansion beneficiaries, and even though the comparison states were useful as a point of contrast, we could not control for differences between the states that predated the demonstrations. Therefore, the comparison states do not constitute an adequate counterfactual for attributing observed differences in utilization after the demonstration to the beneficiary engagement features of the demonstrations. In addition, although we controlled for some observable individual-level demographic and health characteristics, our data did not allow us to control for race and ethnicity or for income. There are also other personal characteristics that are

[73] This is true of both demonstration and comparison states, but may differentially affect beneficiaries who are required to make monthly payments (see Chapter IV).

[74] As a robustness check, we repeated these analyses using 6-month enrollment spans. Results were similar to those using 12-month enrollment spans.

important drivers of health care utilization that we could not observe, such as intrinsic motivation and risk aversion. Our analyses of how HRA completion correlated with outcomes were particularly affected by this shortcoming, as individuals who were motivated to complete an HRA were likely also motivated to undertake other health behaviors such as preventive care.

Survey data. Our analyses based on survey data were designed to identify population-level effects. Recognizing that Medicaid beneficiaries experience high levels of coverage churn, we looked for evidence that behaviors learned in the Medicaid demonstrations could persist after enrollment ends. We have no way to assess how many people in our sample were exposed to the demonstrations—either themselves as enrollees or through contact with beneficiaries (for example, other household members)—so it is impossible to accurately gauge the strength of the intervention and therefore the expected size of any spillover effects. Despite a lack of noteworthy impact estimates, we cannot rule out the possibility that the demonstrations influenced behavior among former enrollees and their peers and relatives.

A second limitation is that survey data are subject to various sources of bias. People who chose to respond to surveys could be different from those who chose not to respond. This type of survey bias is a particular concern when response rates are low. BRFSS response rates for 2017 for the states included in this analysis ranged from 39.3 percent (in Indiana) to 56.9 percent (in North Dakota) (Centers for Disease Control and Prevention 2018).

G. What have other studies found?

Findings from state-based evaluation reports on section 1115 demonstrations. At the time of this report writing, a summative evaluation report was available for Michigan, and interim evaluation reports were available for Indiana and Iowa. Michigan's report on health behaviors, utilization, and health outcomes (Clark et al. 2018) focused on demonstration beneficiaries who maintained continuous enrollment over a two-year period, and revealed that primary care utilization was high among this population, with over 70 percent of the study population making annual primary care visits. Preventive care utilization was also high (over 80 percent), particularly among beneficiaries who received regular primary care. ED use and inpatient hospitalizations moved in opposite directions for beneficiaries with and without chronic conditions. Those with chronic conditions saw decreases in both outcomes over the study period; those without chronic conditions saw increases. The authors conclude that promoting regular primary care and health risk assessments is associated with reduced ED use and hospitalization. The study population for the summative evaluation is not comparable to our study population, because it limits attention to beneficiaries with two years of continuous enrollment, whereas we include all beneficiaries with a 12-month enrollment span. However, their findings of high primary and preventive care use among the HMP population are consistent with our results.

Iowa's draft state-based evaluation report on demonstration performance in 2016–2017 (Momany 2019) includes findings on several outcomes comparable to those we studied. The comparison group for the state evaluation is fee-for-service Medicaid beneficiaries within Iowa, whereas our comparison group comprises expansion beneficiaries in states without

demonstrations. Like Michigan's study, the Iowa study tracks outcomes over time for the expansion population, but as repeated cross-sections; this is in contrast to restricting the analysis to continuously enrolled beneficiaries. The authors find that utilization of preventive services, including wellness visits and screenings, increased over time, particularly following the managed care transition in 2016, and that demonstration beneficiaries had equal or greater access to most primary and preventive services as FFS Medicaid beneficiaries. This is consistent with our findings that demonstration beneficiaries in Iowa had greater utilization of preventive care than the expansion population in comparison states. The state evaluation further finds that demonstration beneficiaries had higher rates of hospitalization and less non-emergency use of the ED than FFS Medicaid beneficiaries. We, too, find higher rates of hospitalization for certain chronic conditions and lower rates of non-emergency use of the ED among the demonstration population.

Indiana's interim evaluation report (Lewin Group 2016) focused on comparing outcomes for HIP Plus and HIP Basic beneficiaries and did not compare outcomes for HIP 2.0 beneficiaries to a comparison group. Using claims data, the evaluators found that between 36 and 64 percent of beneficiaries completed at least one preventive service, depending on income and whether they were enrolled in HIP Plus or HIP Basic. These numbers are similar to what we found. We found that HIP 2.0 beneficiaries were more likely to use specialty than primary care; the state report found the same—between 17 and 31 percent used primary care and between 30 and 47 percent used specialty care. Around 25 percent of emergency department visits were considered non-emergent. A separate evaluation of the emergency room co-payment policy found no differences in utilization between beneficiaries subject to the escalating co-payment schedule and those who were not, and indeed found that few beneficiaries incurred any ED co-payment for non-emergency use (Lewin Group 2017).

Other studies on demonstrations in Indiana, Iowa, and Michigan. Several studies published in academic journals, some by state evaluators, have examined the effects of section 1115 demonstrations involving beneficiary engagement. One study of the Iowa Health and Wellness Plan found that in 2014 and 2015, over 80 percent of beneficiaries failed to complete the two incentivized activities (wellness visit and health risk assessment), and this failure would subject them to premiums in the following enrollment year (Wright et al. 2018). The authors found that 40 percent of Wellness Plan members and 37 percent of Marketplace Choice members received a wellness examination, well below our estimates. However, our findings include later years, after the state's managed care transition. In addition, we use a different and possibly broader definition of wellness visit. A pair of studies analyzing telephone survey data in Michigan from 2016 found that beneficiaries reported greater access to primary care and preventive services after enrolling in the Healthy Michigan Plan (Goold et al. 2019), and that beneficiaries who received primary care were also more likely to complete an HRA and commit to a healthy behavior, but that knowledge of the demonstration's financial incentives was limited (Kelley et al. 2019). Another study used BRFSS data to look at changes over time in health behaviors in Florida, Indiana, Iowa, and Michigan using a difference-in-differences framework, similar to our approach. The authors found no change in smoking patterns or obesity in the early years of

implementation (through 2016), but did find some evidence of small increases in preventive health visit rates (Huf et al. 2018).

Studies on financial incentives in other states. Beginning in the early 2000s, several states implemented programs that offered financial incentives to encourage Medicaid beneficiaries to engage with their health care. Three programs that are similar to those in our three demonstration states are Florida's Enhanced Benefits Rewards Program, Idaho's Preventive Health Assistance Program, and West Virginia's Mountain Health Choices (MHC) Program. Smaller, plan-specific beneficiary engagement programs have also been implemented in states including Minnesota and Wisconsin (Wisconsin DHS 2013). These states designed their programs primarily for low-income parents and their children, and often included incentivized behaviors for both adults and children. In contrast, the incentive programs in Indiana, Iowa, and Michigan were designed for the adult expansion population. Indiana's, Iowa's, and Michigan's demonstrations rewarded beneficiaries mainly through reduced or waived monthly payments, whereas the earlier state programs rewarded beneficiaries mainly through earned credits, gift cards, or enhanced benefits.[75]

Our findings suggest that direct financial incentives are effective in promoting specific wellness activities, and research on these earlier Medicaid programs generally supports our results. Studies on the programs in Idaho and Minnesota showed that financial incentives are effective at prompting behavior change for activities that require a single decision or action, such as making a wellness visit or getting a flu shot (Greene 2011; Kenney et al. 2011; Nyman et al. 2013).[76] Financial incentives in Idaho and Florida appear less effective at promoting ongoing lifestyle changes such as smoking cessation or weight loss (Blumenthal et al. 2013; Hall et al. 2013).

West Virginia's MHC program was designed to encourage healthy behaviors and discourage non-emergent use of the ED, and shared some features with Indiana, Iowa, and Michigan's demonstrations. MHC beneficiaries could opt into an enhanced benefit plan by signing a member responsibility agreement to not use the ED for non-emergent care and by entering into a health improvement contract with their doctor to engage in better health behaviors. Researchers found that beneficiaries in the more basic plan were more likely to have non-emergent ED visits than those in the enhanced benefit plan were (Gurley-Calvez et al. 2012, Walsh et al. 2014), but given that enrollment in enhanced benefits was optional, the findings may be biased by selection effects.

[75] In a rapid-cycle report (Contreary and Miller 2017), we found that health plans in Indiana, Iowa, and Michigan layer additional financial incentives—which are often gift cards earned for receiving preventive services or engaging with care management—on top of demonstration incentives. These health plan incentives are similar to those offered in earlier state incentive programs. However, we have no data about beneficiaries' receipt of these supplementary incentives and cannot assess their impact on care-seeking behaviors or health outcomes.

[76] Research on low-income adults without Medicaid coverage reveals similar results. A recent randomized controlled trial found that beneficiaries who were offered small cash incentives were 6 to 9 percentage points more likely to visits their primary care provider than comparison beneficiaries were (Bradley et al. 2017).

H. Discussion

We used several analytic approaches and drew on different data sources to evaluate the effect of beneficiary engagement strategies on the use of preventive care, management of chronic conditions, and use of inappropriate care. Each approach has strengths and limitations, but using different strategies allowed us to explore whether the results told a consistent story about the effects of incentives on healthy behaviors and about utilization of specific types of care.

Response to explicit financial incentives. Our quantitative analyses suggest that more people might engage in healthy behavior if they were given financial incentives for specific actions. Regression models based on administrative data revealed that living in a state with a financial incentive to have a wellness visit was associated with a higher likelihood of making such a visit. We also found that beneficiaries in one demonstration state (Michigan) were more likely to self-report having received a checkup in the past year. These findings are consistent with our hypothesis and with previous research showing that financial incentives for one-time actions can be effective.

Utilization of services that might follow from incentives. Our findings were more mixed on the question of whether financial incentives to have a wellness visit and HRA are associated with other desired utilization outcomes. In all three states, utilization of preventive services was consistently higher than it was in the comparison states. However, our results on management of chronic conditions were mixed, as were our results on primary and specialty care utilization and utilization of emergency services.

- In Iowa, beneficiaries with diabetes had lower rates of HbA1C testing and higher rates of diabetes-related hospitalization than comparison state beneficiaries, whereas the opposite was true for beneficiaries in Indiana and Michigan.
- Rates of follow-up after hospitalization varied widely from one demonstration state to the next and were not consistently associated with demonstration policies.
- Although Michigan's demonstration was associated with increased use of primary care, Indiana's and Iowa's were associated with decreased use and substantially higher use of specialty care.
- Similarly, although Iowa's and Michigan's demonstrations were associated with slightly less use of the emergency department, Indiana's was associated with higher use.
- In both demonstration states where we measured urgent care use (Indiana and Michigan) it was less common than it was in comparison states.

It is possible that distinct demonstration features account for the estimated differences discussed above. For example, Michigan's demonstration required beneficiaries to set a health care goal with a primary care physician to receive the financial reward, whereas no feature of Indiana's or Iowa's demonstrations required the involvement of a primary care physician. This could at least partially explain why Michigan's demonstration was associated with more primary care use and

less use of the emergency department, although it does not explain why the other two demonstrations were associated with reduced primary care use, or why Indiana's demonstration was associated with increased emergency department use. Similarly, distinct features of states' Medicaid programs other than the demonstrations could be associated with the variation in utilization patterns. The fact that Indiana had substantially higher rates of follow-up mental health visits after hospitalization could be attributable to the fact that HIP 2.0 beneficiaries were eligible for the state's Behavioral and Primary Healthcare Coordination Program, which offers mental health care coordination services. This benefit is independent of the section 1115 demonstration, but it likely influences use of mental health services by demonstration beneficiaries.

Another possible reason why demonstration incentives did not consistently result in more positive changes than those seen in comparison states concerns the structure of each state's Medicaid expansion. During the years covered by this study, several of the comparison states contracted with managed care organizations.[77] These types of health plans often do active outreach to beneficiaries to encourage them to receive preventive care and to manage their chronic conditions. In addition, managed care organizations often offer plan-specific financial incentives (for example, gift cards) to increase the likelihood that beneficiaries will obtain preventive care (Contreary and Miller 2017). If managed care organizations in comparison states offered similar incentives for preventive care, promoted establishing a primary care relationship, and/or did outreach to reduce inefficient care, it might have lessened the policy contrast between demonstration and comparison states.

In Iowa, for part of the study period, some beneficiaries may even have received less outreach than comparison beneficiaries. Iowa covered a portion of its expansion population under a premium assistance program for the first two years of the demonstration. We conducted a key informant interview with representatives of one of the qualified health plans involved in Iowa's Marketplace Choice premium assistance program, who reported that the QHP did not conduct outreach related to demonstration incentives or preventive care. Premium assistance beneficiaries likely were the subjects of less outreach encouraging preventive care and chronic condition management than managed care beneficiaries were. In this case, our finding that completion rates for wellness visits were higher in Iowa than in the comparison states supports even more strongly the suggestion that financial incentives for one-time actions can be effective, because beneficiaries completed wellness visits at high rates without being regularly prompted to by their health plans.

Unfortunately, data limitations prevent us from distinguishing the effects of demonstration features from any of the above alternative explanations, or between either of these and a remaining possible explanation—that observed differences between states are attributable to state-level characteristics that are unrelated to features either of the demonstrations or the expansion.

[77] These organizations sometimes go by another name—in Indiana, for example, they are known as managed care entities.

Our analyses of HRA completion in Iowa showed that completing an HRA was associated with using desired health care services. Iowa beneficiaries who completed an HRA were more likely to also receive preventive services, manage their diabetes, and avoid emergency department visits. However, this finding does not constitute conclusive evidence that HRA completion promotes other healthy behaviors. Completing an HRA may have given beneficiaries information that encouraged them to remain engaged with their health care, but it is also possible that the observed association was driven by beneficiaries who were generally more motivated to manage their health and were therefore more likely to receive preventive and chronic condition management services regardless of any external incentives.

Population-level effects. Estimates from difference-in-differences models using BRFSS data did not indicate the presence of population-level impacts on a variety of self-reported utilization and health outcomes resulting from state demonstration policies. In general, we found no consistent and statistically significant differences in such outcomes between demonstration and comparison states.

Policy takeaways. Combining our various analytic approaches, we found evidence that financial incentives for specific healthy behaviors can prompt beneficiaries to engage in those incentivized behaviors, particularly if the incentives are easy to understand and beneficiaries have control over completing the necessary actions. We found some evidence that incentives to form a relationship with a physician promote receipt of preventive care, but more mixed evidence that such incentives promote receipt of primary care or management of chronic conditions. We found mixed evidence that beneficiary engagement policies discouraged non-emergent use of the emergency department, and no evidence that they shifted care to more appropriate venues such as urgent care clinics. We also did not find evidence suggesting population-level effects of beneficiary engagement policies, either spillover effects from Medicaid beneficiaries to their peers and family or effects resulting from persistence of behaviors encouraged through financial incentives.

Our findings suggest, therefore, that although incentives for specific healthy behaviors can increase use of the services most directly related to the healthy behavior, use of downstream services is not systematically affected positively or negatively relative to comparison states. It is important to note that our analyses faced several data quality limitations that prevented us from controlling for important state- and beneficiary-level factors that influence service utilization. However, based on our findings, states wishing to influence utilization of particular services might choose to design their incentive programs to directly target those services.

VI. DISCUSSION

A. Summary of findings across the different domains

Between 2014 and 2017, six states—Arkansas, Indiana, Iowa, Michigan, Montana, and New Hampshire—expanded Medicaid coverage to people with incomes up to 133 percent of the federal poverty level (FPL) by using section 1115 authority to test new approaches to administering Medicaid programs. These states have implemented (1) premium assistance programs that enroll Medicaid beneficiaries in qualified health plans (QHPs), (2) monthly payment requirements similar to those in commercial health insurance, and/or (3) programs that encourage specific health behaviors. Several states tested more than one of these strategies in the same demonstration, making it more complicated to assess the outcomes of each one.

Although each strategy borrows elements from earlier, smaller-scale policy experiments, these demonstrations are the first to test these approaches on a large pool of adult enrollees who qualify for Medicaid on the basis of income rather than disability or other categorical reasons, such as pregnancy or caretaker status. Collectively, the six demonstrations enrolled over 1.7 million people in 2017, nearly one-third of the total Medicaid-covered population in these states.

For this summative evaluation report, we organized research questions into domains that corresponded to the three approaches. We used several data sources and a variety of qualitative and quantitative analytic methods, each of which has strengths and limitations. For example, we used Medicaid administrative data from 2014 through 2017 to examine health care use patterns and enrollment histories among those who successfully enrolled in the demonstrations. These analyses allowed us to analyze enrollment, service use, and expenditures among beneficiaries who were actually enrolled in their states' expansions and demonstrations.

Because data on utilization were not available before Medicaid expansion, however, we were limited to descriptive analyses of key outcomes. In addition, data quality issues reduced the reliability of our estimates. To balance these limitations, we also used national household survey data from 2012 through 2017 to understand coverage take-up rates among likely eligible adults, as well as changes in health behaviors and unmet health care needs among adults with low incomes. These data allowed us to use the most expansive set of demonstration and comparison states and yielded information on the total pool of individuals who are likely eligible for coverage. However, survey data are also subject to different types of nonresponse bias, and surveys are known to undercount the number of adults enrolled in Medicaid. By capitalizing on the benefits of different data sets and employing different analytic strategies, we could explore whether different analyses told a consistent story about the main effects of key policies.

This discussion summarizes the main findings from each domain, with a focus on analyses that yielded statistically significant or otherwise meaningful results. It is important to note, however, that when comparing demonstrations to traditional Medicaid expansions, a lack of significantly different results might be an acceptable outcome. In several cases, no differences were observed

because in both direct and alternative Medicaid expansion models, utilization of recommended services was high among the newly covered, and reductions in unmet need for care were consistently realized. Such results comport with a body of work by Sommers and colleagues that draws on surveys of low-income adults in Arkansas, Kentucky, and Texas, and finds similar improvements in Arkansas (a demonstration state) and Kentucky (a direct expansion state) but not in Texas, which did not expand Medicaid (Sommers, Blendon and Orav 2016; Sommers et al. 2017). When program outcomes are statistically indistinguishable from each other, states and CMS might want to consider other factors in deciding whether to pursue a demonstration or a traditional Medicaid expansion, such as the administrative costs of demonstration operations or the value of allowing state policies to vary in response to the political views and preferences of different state constituencies.

Collectively, results across the three research domains suggest that different approaches to expanding Medicaid coverage vary in their ability to meaningfully influence outcomes. Our most notable findings are as follows:

Domain 1: Premium assistance programs that enroll beneficiaries in QHPs. Overall, our results suggest that premium assistance increases access to physician office visits. Using difference-in-differences regression models, an analytically strong approach, we found statistically significant, higher rates of physician office visits in both Iowa and New Hampshire during their premium assistance demonstrations. Using less rigorous cross-sectional regression models, we found similar patterns for physician office visits Iowa and New Hampshire. We were able to include Arkansas only in cross-sectional models and found that beneficiaries in Arkansas had fewer physician office visits and other services relative to comparison states. However, the results on access to care for Arkansas should be interpreted with caution because cross-sectional models cannot control for confounding factors as well as the difference-in-differences models do, and we used a different source of administrative data for each state. Our analyses of national survey data found that beneficiaries living in Arkansas and Iowa during premium assistance demonstrations had a higher probability of having had a checkup in the last year, and those living in New Hampshire had a higher probability of having a personal provider. Results of our analysis of states' expenditures were mixed but suggest that premium assistance probably costs more than traditional Medicaid coverage. Finally, states with premium assistance demonstrations have lower rates of Medicaid coverage take-up.

Domain 2: Monthly financial contributions. Taken together, our results point to a negative relationship between monthly payments and enrollment. Regression models based on national survey data revealed a negative association between living in states with monthly payments and the probability of Medicaid enrollment, regardless of whether a given person is expected to owe any payments at all. There was also a negative association between owing a monthly payment and the probability of Medicaid enrollment. The largest payment ($31+) was associated with the largest decrease in take-up. Estimated take-up rates, or the proportion of the likely eligible population enrolled in Medicaid, were generally consistent with our regression models of survey data, but their pattern was not conclusive. Analyses of enrollment continuity using a survival

analysis revealed a statistically significant negative relationship between enrollment duration, or length of continuous enrollment spans, and owing a monthly payment (based on the timing of states' monthly payment policies and the onset of payment obligations). However, for all analyses of administrative data, we lacked an income variable, so we could not disaggregate enrollment continuity based on income for those who did and did not owe premiums in each state. Notably, enrollment duration beyond 24 months was relatively infrequent in both demonstration and comparison states; fewer than 50 percent of beneficiaries remained continuously enrolled this long in a majority of states for which we could observe long-term enrollment. A separate analysis of non-eligibility periods as a consequence of nonpayment suggests that this form of payment enforcement could extend the period prior to reenrollment and reduce the number of people who return to Medicaid coverage.

Domain 3: Incentivized healthy behaviors. Combining multiple analytic approaches, we found evidence that financial incentives for specific healthy behaviors can increase use of the services most directly related to the healthy behavior, although use of downstream services is not systematically affected either positively or negatively relative to comparison states. Specifically, we found some evidence that incentives to form a relationship with a physician promote receipt of preventive care, but mixed evidence that such incentives promote receipt of primary care or management of chronic conditions. For some outcomes related to management of chronic conditions, we found no changes associated with demonstrations. For others, statistically significant associations moved in different directions for different demonstration states. In only one state (Michigan) did we find that use of primary care was greater than in comparison states; in the other two it was lower. We also found mixed evidence that beneficiary engagement policies implemented in Indiana, Iowa, and Michigan discouraged non-emergent use of the emergency department or shifted care to more appropriate venues such as urgent care clinics. Emergency department utilization was higher in Indiana than in comparison states, but lower in Iowa and Michigan.

We found no evidence that beneficiary engagement policies had spillover effects—habits or behavior that pass from Medicaid beneficiaries to their peers and family—or other population-level effects that would have resulted from the persistence of behaviors encouraged through financial incentives, although we could not definitively reject that there were such effects. State-based evaluation findings suggest that many beneficiaries do not appear to understand their incentives, and that incentives might work best if they are easy to understand and beneficiaries have control over completing the necessary actions. Based on our findings, states wishing to influence utilization of particular services might choose to design their incentive programs to focus directly on those services. Incentives to have general contact with the health system are unlikely to affect downstream utilization and health outcomes. Findings from Domain 2 analyses indicating that most adult expansion beneficiaries have enrollment durations of less than two years also suggest that Medicaid programs have short time horizons within which they can influence care-seeking behavior. States could consider tailoring behavioral incentives to the subset of beneficiaries with more persistent enrollment (to the extent those individuals have unique characteristics), or design incentives meant to influence short-term behaviors.

B. Conclusions

The analyses in this summative report are the first cross-state research findings that include the first six states that used section 1115 authority to implement novel approaches to expanding Medicaid. Although few findings in this report are conclusive, they provide valuable evidence on considerations for both demonstration designs and implementation, evidence that CMS and states can use to support states' success.

These findings also highlight the value of weaving together results from different analytic approaches and different data sources, particularly when faced with limitations in the availability or quality of data. An additional contribution of this work is that it combines information on demonstration implementation and outcomes to assess demonstration performance. In particular, the national evaluation team's rapid-cycle reports to CMS on demonstration implementation have generated nuanced information about which outcomes might be likely to change and why outcomes might have differed across states that implemented similar policies. For example, key informant interviews in Michigan and Indiana highlighted the active role that managed care organizations have played to promote healthy behavior among demonstration enrollees.

Collectively, this body of work will help both CMS and state officials understand cross-state differences in outcomes and shed some light on the degree to which observed differences are policy-driven versus reflective of the idiosyncrasies of each state's health care system. With our findings, policymakers will have better tools for shaping Medicaid programs to best serve qualifying adults.

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