The Impacts of Privatization: Evidence from the Home Health Care Setting

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Abstract

In many sectors, privatization is pursued to improve efficiency but raises concerns about cost-cutting and underprovision. In health care, however, the dominant margin of response may be overprovision: higher spending driven by a strategic supply response billing and coding efforts. I study this mechanism in the context of Arkansas's 2016 sale of its statewide public home health agency network to a national for-profit chain. A stylized model predicts that the ownership transition induces a strategic supply response, operating through billing effort in documentation, coding, and service mix decisions, that raises the effective reimbursement per episode and expand care along an upward-sloping cost curve. Consistent with these predictions, difference-in-differences estimates using Medicare data from 2010–2021 show that privatization increased effective reimbursement per episode by 27 percent and statewide home health use by roughly 10 percent, with no evidence of reduced access for high-cost or rural populations. Spending rose substantially, but measurable quality and outcomes remained flat. These findings suggest that when payment is endogenous to provider behavior, privatization may reshape the trade-offs typically associated with ownership change, increasing spending and access without necessarily increasing the value of care.

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1 Introduction

Privatization has reshaped service delivery across sectors, from prisons and utilities to education, health care, and elder care. While shifting provision from public to private entities can potentially reduce costs and improve efficiency, profit motives may also distort provider behavior—encouraging cream-skimming, quality degradation, or underinvestment in hard-to-monitor dimensions (Hart et al., 1997; Duggan, 2000; Megginson and Netter, 2001). Healthcare privatization, however, often presents a different problem. Under prospective payment systems, providers face incentives not just to reduce costs but to optimize reimbursement by increasing service intensity and documenting greater patient complexity (Silverman and Skinner, 2004; Dafny, 2005; Geruso and Layton, 2020)—creating moral hazard through overprovision and upcoding rather than underprovision (Layton et al., 2022). Understanding how ownership interacts with these distinctive payment incentives is especially important given healthcare's ongoing shift toward private delivery (Horwitz, 2005; Horwitz and Nichols, 2022)

Home health care offers a compelling setting for examining how ownership interacts with payment incentives in practice. The sector has grown rapidly, driven by an aging population and demand for community-based care, yet its value proposition remains uncertain amid persistent concerns about fraud and overuse—challenges often attributed to the for-profit providers who now dominate the industry (O'Malley et al., 2023; Medicare Payment Advisory Commission, 2023, 2011; Geng et al., 2024). Crucially, home health provides a revealing lens for studying privatization because of its distinctive policy history: until 1980, Medicare explicitly barred for-profit providers from receiving funding. The lifting of that restriction triggered a sharp and sustained shift toward for-profit provision that continues today, making home health a sector where ownership transitions have been particularly consequential.

Furthermore, the shift towards private provision occurred within a distinctive policy environment. Medicare's prospective payment system (PPS) allowed home health agencies to influence their effective reimbursement through service mix and coding decisions - creating a strategic environment distinct from fixed-price settings. Under the PPS, Medicare paid a fixed rate per 60-day episode, with adjustments based on documented patient characteristics—functional limitations, clinical severity, comorbidities—and on the number of therapy visits delivered. Although nominally prospective, this payment system gave agencies considerable discretion to influence reimbursement. Agencies could increase payments by coding patients into higher-severity categories or by delivering therapy visits that crossed predetermined thresholds associated with higher payment rates. I refer to these activities collectively as *billing effort*: the strategic use of coding, documentation, and service mix to raise the *effective price* of care. Billing effort operates along two dimensions: (1) strategic care provision—adjusting the service mix and visit intensity to optimize payment formulas (e.g., increasing therapy visits, reducing aide visits)—and (2) strategic documentation—more thorough coding of comorbidities or functional limitations that increase case-mix weights. Due to the ability to serve patients more profitably through billing effort, private home health agencies may

also have an incentive to serve more patients on the extensive margin. Because beneficiaries face no cost-sharing, there is little from the demand-side to discipline this strategic supply response.

This mechanism contrasts sharply with the traditional Hart-Shleifer-Vishny (HSV) framework (Hart et al., 1997), which predicts that privatization leads to cost-cutting that may reduce quality or restrict access to high-cost patients. In the HSV model, private firms minimize costs in ways that can degrade non-contractible quality—predictions that imply contraction or cream-skimming. My billing effort mechanism, by contrast, predicts expansion: higher effective reimbursement allows providers to profitably serve additional patients, including those with higher marginal costs.

To test these predictions, I exploit a unique natural experiment in Arkansas, where in 2016 the state sold its publicly-run home health agency program to Kindred Healthcare, a large national for-profit chain. The sale transferred 74 Medicare-certified licenses across 69 of the state's 75 counties, effectively ending public provision of home health services in Arkansas and replacing the public home health agency network with a single private operator. At the time, Arkansas ran one of the last remaining statewide public home health programs, making this transition a rare and consequential example of large-scale privatization in healthcare delivery.

Several features make this policy change both empirically useful and substantively rich. The transition was abrupt and comprehensive: all licenses were transferred to Kindred at once. It occurred four years prior to Medicare's 2020 payment system change from the prospective payment system to the Patient Driven Groupings Model, minimizing confounding from concurrent reforms. Arkansas also maintains strict certificate-of-need (CON) laws, which limit entry by requiring state approval to open or expand a home health agency. As a result, privatization involved the transfer of existing licenses rather than new market entry or capacity expansion. Together, these features create a relatively clean natural experiment with which to study how ownership affects provider behavior, access, and outcomes under prospective payment.

My empirical approach uses comprehensive Medicare data from 2010 to 2021 and three complementary difference-in-differences designs. First, at the provider level, I compare outcomes for Kindred-acquired agencies to agencies in control states, examining changes in visit mix, patient composition, coding practices, and episode payments—the key indicators of billing effort. I separately analyze spillover effects on non-Kindred agencies in Arkansas. Second, at the state level, I implement difference-in-differences comparing Arkansas to control states that maintained stable ownership, supplemented by synthetic control methods. These state-level designs capture extensive-margin effects on utilization and aggregate spending. Third, I exploit within-Arkansas variation using a continuous treatment design, where treatment intensity is measured by Kindred's county-level market share expansion. This approach links the magnitude of privatization exposure to changes in both market-level utilization and spillover agency behavior.

I find that privatization fundamentally altered home health delivery, increasing Medicare payments by nearly $500 per episode (27 percent). This increase reflects changes along multiple margins. Consistent with the strategic supply response, Kindred-acquired agencies shifted service

delivery toward higher-reimbursement care: therapy visits per episode rose substantially while aide visits fell. Second, agencies documented greater patient complexity, with self-reported scores for activities of daily living (such as bathing, dressing, and eating) rising by 12%, a pattern consistent with strategic coding to increase reimbursement. Third, agencies served beneficiaries who were modestly sicker on observable dimensions: the number of chronic conditions (measured from prior-year claims) and comorbidity scores (a standardized measure of how many diseases patients have based on their medical records) both increased, though these changes were smaller in magnitude than the shifts in agency-reported measures. Because these comorbidity measures cannot be influenced by home health agency documentation, they suggest some genuine selection toward sicker patients, though the dominant driver of increased payments appears to be the strategic supply response, operating through billing effort in service mix and coding, rather than patient selection alone.

Privatization also expanded access along the extensive margin. The number of fee-for-service Medicare beneficiaries receiving home health care in Arkansas rose by nearly 10% relative to control states, with particularly large increases in urban areas and among non-dual-eligible patients. This growth was driven primarily by the acquired public agencies: while some of Kindred's expansion came at the expense of incumbent providers, overall market growth substantially outweighed business stealing. Newly acquired agencies also expanded their geographic reach, serving patients located 3 to 4 miles farther from the nearest agency on average—consistent with higher per-episode payments offsetting the additional delivery costs associated with serving more distant patients. Despite this market expansion, most of the increased Medicare expenditure stemmed from higher payments to inframarginal patients. A decomposition analysis shows that approximately 70% of the spending increase came from higher payments to beneficiaries who would have received care regardless of ownership, with the remaining 30% attributable to newly served patients. This implies that while privatization expanded access, the majority of additional spending represented transfers to providers for treating existing patients rather than the cost of serving new beneficiaries.

The privatization also generated spillover effects on the non-acquired home health agencies in Arkansas, though these effects were modest compared to the changes at directly privatized facilities. For-profit home health agencies showed small but statistically significant increases in therapy visits and decreases in aide visits, while nonprofit agencies exhibited no measurable response. The muted response from competitors to the acquired agencies suggests that the public agency was not acting as a disciplining 'public option' in this market. Because the primary channel of adjustment operates through reimbursement optimization rather than cost-cutting or quality competition, the transition did not significantly affect or induce a strategic response among the incumbent home health agencies.

Despite substantial shifts in how home health care was delivered, these changes did not translate into measurable improvements in standard clinical outcomes—specifically, hospitalization and mortality rates during and following home health episodes. I find no significant change in either

measure, and no consistent substitution away from institutional post-acute or long-stay nursing home care. Patient-reported experience ratings remained flat. One exception is an improvement in timely initiation of care for post-acute patients, a measure that likely reflects real operational gains in intake speed or scheduling. It is worth noting, however, that much of the value of home health care may lie in outcomes that are difficult to observe in administrative data, such as supporting independence or reducing caregiver burden. For inframarginal patients who would have received care regardless of ownership, increased billing effort likely raised reimbursement without improving outcomes, increasing spending without generating additional value

Importantly, the transition was not seamless. Trends of outcomes over time show volatility in volume and service delivery immediately following the sale's announcement, consistent with short-run disruption. This reflects a broader feature of privatization often neglected in empirical work: while conversion to private operators may improve efficiency or scale in the long run, the transition itself may entail costs, including disrupted continuity, workforce loss, or the breakdown of informal coordination channels, that shape how and for whom services are delivered.

This study advances the literature in several ways. To my knowledge, it is among the first to provide causal evidence on the privatization of home health agencies. While prior work has shown that ownership matters for provider incentives across a range settings, health care and beyond, (Hart et al., 1997; Duggan, 2000; Duggan et al., 2023), most studies focusing on the home health care sector rely on cross-sectional comparisons (Cabin et al., 2014; Kim and Norton, 2015) or potentially endogenous transitions (Zhang et al., 2024). I leverage a rare natural experiment in Arkansas that fully replaced public provision with a single for-profit operator, allowing me to isolate effects on home health care service provision, coding, and access.

Second, I show how ownership changes can affect not only provider behavior but also broader market outcomes. Prior research documents extensive strategic responses to payment incentives across healthcare settings, including pay-for-performance programs in hospitals (Werner et al., 2012) and payment reforms in post-acute care (Huckfeldt et al., 2014), with providers bunching visits near thresholds (McKnight, 2006), upcoding patient severity (Bowblis and Brunt, 2014; Geruso and Layton, 2020), adopting revenue-generating documentation practices (Sacarny, 2018), gaming payment contracts (Gupta et al., 2024), and shifting service mix toward high-margin inputs (Silverman and Skinner, 2004; Dafny, 2005). Crucially, empirical studies find that for-profit and private equity–owned providers are more responsive to these financial incentives across settings (Kim and Norton, 2015, 2017; Zhang et al., 2024; Gupta et al., 2021; Rahman, 2025). In the home health setting existing work shows that agencies bunch visits at therapy thresholds (McKnight, 2006), new entrants, both for-profit and non-profit, deliver more therapy than incumbents (Kim and Norton, 2015), and for-profit agencies adjust episode length and service intensity more aggressively than nonprofits (Kim and Norton, 2017).

Unlike cost-based reimbursement or pure capitation, where payment is either tied to realized costs or fixed per enrollee, home health's prospective payment system allows providers to increase

reimbursement through coding and service mix decisions. Under cost-based reimbursement, ownership mainly affects production efficiency, since payments track realized costs. Under pure capitation, providers are paid a fixed amount per enrollee and therefore face incentives to reduce service intensity or avoid high-cost patients. Theories of public versus private provision suggest that private entities face stronger incentives for revenue maximization (Hart et al., 1997), and in this setting, I show that the primary concern may not be under-provision, but rather moral hazard driven over-provision as private providers exploit payment system features to maximize revenue. Providers may also expand utilization directly by initiating episodes, managing referrals, or other marketing efforts—channels consistent with the supplier-induced demand (SID) literature (McGuire, 2000). While these mechanisms are difficult to disentangle empirically, my setting permits a joint examination of how ownership and payment incentives shape both the quantity and composition of care.

More broadly, my findings speak to ongoing debates about privatization and value in publicly financed care. When reimbursement systems allow providers to raise their effective price through billing effort, or induce care through marketing effort, ownership transitions can reshape not only how care is delivered, but also who receives it and at what cost. The evidence suggests that privatization expanded access to home health care in Arkansas, but also increased spending even for inframarginal patients. Whether this shift is welfare-improving depends not only on the value of marginal care relative to its cost, but also on the extent of overpayment for inframarginal patients—those who would have been served under public ownership but now generate higher reimbursement without better outcomes. These results highlight that the impacts of privatization depend not just on how providers respond at the intensive and extensive margins, but on how those responses interact with the structure of payment incentives—determining who gets served, how care is delivered, and whether public spending ultimately increases welfare.

Finally, Arkansas's decision to sell its public home health network was motivated in part by a desire to exit a program the state could no longer sustainably operate. Whether privatization achieved this goal — and at what cost to Medicare and to patients — is ultimately the question this paper addresses. The evidence suggests the answer is nuanced: the evidence suggests that privatization succeeded in a narrow fiscal sense for the operator, but through revenue optimization rather than efficiency improvement, with consequences for federal spending that the state's calculus did not internalize.

The remainder of the paper is organized as follows. Section 2 provides background on the institutional setting and policy context. Section 3 presents a conceptual framework for understanding provider effort under prospective payment. Section 4 describes the data and empirical strategy. Section 5 presents the main results. Section 6 discusses the broader implications, and Section 7 concludes.

2 Background and Institutional Setting

2.1 Medicare Home Health Payment and Incentives

Home health care is a core component of the broader home-based care landscape, which also includes personal care services and other types of home-based medical care. These categories differ in both medical content and insurance coverage. Personal care services involve non-medical assistance with daily tasks such as bathing, dressing, eating, toileting, and mobility. These activities are considered custodial or personal care because they do not require skilled clinical training and are not covered by Medicare when provided on their own. Instead they are typically financed through Medicaid long-term services and supports, state programs, or private payment. Medicare's home health care benefit is a medical benefit with specific requirements. To qualify, a beneficiary must be homebound, require skilled care such as intermittent nursing or therapy, and receive services under a physician-approved plan of care delivered by a Medicare-certified home health agency. Covered services include skilled nursing, physical therapy, occupational therapy, speech-language pathology, medical social work, and aide services when part of a skilled care plan. These services are covered because they are medically necessary to treat illness or injury, whereas stand-alone personal care without a medical need is not covered by Medicare.

This paper focuses on home health care in Traditional Medicare, which remains the largest payer for such services in the United States. In 2019, Medicare spent over $18 billion on home health, covering 3.3 million fee-for-service beneficiaries. The scale of this federal investment has made the sector a focus of both legitimate provider expansion and, at times, fraudulent activity (O'Malley et al., 2023). To qualify for Medicare home health, beneficiaries must meet two key criteria: they must be homebound, and they must require intermittent skilled services—criteria that distinguish home health from both personal care and broader home-based medical models.

During most of the study period, Medicare reimbursed home health agencies under a prospective payment system (PPS) that paid a fixed rate for each 60-day episode of care. Payments were adjusted for case-mix, local wages, and—critically—therapy volume. Because therapy use directly affected reimbursement, agencies faced strong incentives to increase therapy visits. Agencies could also increase payments by documenting greater functional impairment or additional comorbidities, both of which raised case-mix scores (Werner et al., 2012).

In 2020, Medicare implemented a major overhaul of the home health payment system with the Patient-Driven Groupings Model (PDGM). PDGM shortened the standard episode length from 60 to 30 days, eliminated therapy volume as a payment driver, and introduced new clinical groupings and functional scoring criteria. These changes substantially altered agency incentives. PDGM was also implemented in January 2020—just before the onset of the COVID-19 pandemic—making it difficult to isolate its effects from concurrent disruptions to care delivery and utilization.

2.2 The Shift Toward Privatization

The Medicare home health benefit was initially restricted to nonprofit and government-operated agencies. That changed with the Omnibus Reconciliation Act of 1980, which lifted the federal ban on for-profit participation. This policy shift marked a turning point in the structure of the industry: between 1980 and 2000, the number of Medicare-certified home health agencies more than doubled, with proprietary agencies accounting for most of the growth. By 2015, over 75% of HHAs nationwide were for-profit (Medicare Payment Advisory Commission (MedPAC), 2016).

Over the same period, many states began to reduce or eliminate their direct provision of home health care. Some states sold their public agencies to private operators, while others exited the sector entirely in response to fiscal constraints, staffing shortages, and the growing availability of private providers. For example, South Carolina sold its DHEC-operated system to Kindred in 2016, Oklahoma phased out its remaining public HHAs by the early 2000s, and Idaho's last public agency closed in 2022. North Carolina, Kentucky, Georgia, and others transitioned dozens of county- or state-run agencies to private hands during the 2010s. Alabama remains a rare exception, continuing to operate a statewide Medicare-certified public agency as of 2025 (South Carolina Department of Health and Environmental Control, 2018; Panhandle Health District, 2024; Alabama Department of Public Health, 2017).

These trends reflect a broader realignment in the delivery of publicly financed care, with ownership increasingly shifting from public agencies to private—particularly for-profit—operators. While the shift toward private ownership has been well documented in home health, few studies have examined its consequences in settings where public agencies continued to operate at statewide scale. As Table A.1 shows, Arkansas was one of the last states to maintain a large, centrally administered public home health system through the mid-2010s, making it a rare and informative case for studying privatization within this sector.

2.3 The Arkansas Public HHA System and Its Privatization

By the mid-2010s, Arkansas operated one of the nation's last extensive public home health systems, with the Arkansas Department of Health (ADH) maintaining Medicare-certified home health agencies (HHAs) in 69 of the state's 75 counties. These agencies operated through local public health units under the coordination of ADH's In-Home Services Division.

In 2015, shortly after Republican Governor Asa Hutchinson took office following his election in 2014, ADH announced plans to phase out its In-Home Services program—which then served several thousand patients with approximately 300 state employees and 1,500 contract workers. The privatization aligned with Hutchinson's conservative governance philosophy, as he explicitly stated: "We are not in the business of competing with the private sector" (Talk Business & Politics, 2016). State officials cited multiple rationales for the sale, including financial constraints facing the public system and the belief that the agencies would benefit from private-sector management approaches.

Following a competitive bidding process that attracted six bidders, the state selected Kindred Healthcare, Inc., a large national for-profit chain, as the buyer. Arkansas finalized the sale of its home health assets to Kindred for $39 million in June 2016, transferring 74 home health agency licenses spanning 69 counties.

Prior to this acquisition, Kindred had no presence in Arkansas until 2015, when it first entered the state through its purchase of Gentiva Health Services (see Appendix C). Through this acquisition, Kindred inherited three home health agencies in Arkansas. The 2016 ADH acquisition dramatically expanded Kindred's market presence, allowing it to operate in 69 of the state's 75 counties and become Arkansas's dominant home health provider with a combined total of 77 agencies (out of approximately 125 statewide). While 2015 could be considered a partial treatment year due to Kindred's initial entry, the dramatic expansion in geographic coverage and market share occurred with the 2016 privatization. The transaction also included ADH's hospice programs, though these services are excluded from the outcome measures analyzed in this study.

2.4 Regulation of Home Health Agencies and Certificate of Need Laws

Many states, including Arkansas, regulate the supply of home health services through Certificate of Need (CON) laws, which require agencies to demonstrate "need" before opening a new provider or expanding service areas. These laws are intended to prevent oversupply and promote cost control, but they also restrict market entry and expansion. As of 2020, 17 states had active CON requirements specifically for home health agencies (HHAs), with varying thresholds, geographic rules, and enforcement intensity.[1]

Arkansas has long maintained one of the most restrictive CON regimes for HHAs. Since the early 1980s, the state has required prior approval from the Arkansas Health Services Permit Agency for any new agency, service area expansion, or additional branch office. Approval depends on population size, projected utilization, and the existing supply of providers.[2]

The state's methodology is particularly strict. For example, an applicant must show that there are fewer than 2.5 full-time-equivalent home health nurses per 1,000 persons aged 65 and over in the county to be served.[3] Because this threshold is rarely met—especially in rural areas—the law effectively prohibits new agencies from entering many counties. In practice, few new home health licenses have been granted in Arkansas since the 1990s.

These constraints significantly shaped the dynamics of the 2016 ADH privatization. When the state exited the market, existing for-profit agencies could not easily expand into counties that had

[1]See National Conference of State Legislatures (2020), "Certificate of Need: State Health Laws and Programs."

[2]See Arkansas Code Title 20, Subtitle 2, Chapter 9 and related Health Services Permit Agency rules.

[3]See Arkansas Health Services Permit Commission rules, Section VII (Home Health Services Need Methodology).

previously been served by ADH unless they already held a license. Even for agencies operating in adjacent areas, expansion typically required a burdensome CON application. The sale of ADH's licenses to Kindred—a for-profit chain already operating in Arkansas—thus enabled an immediate path to statewide scale that would have otherwise taken years.

These constraints help explain why privatization resulted in a consolidation of care under Kindred rather than a broader diffusion of service provision. In a less regulated environment, other agencies might have entered or expanded to absorb former ADH patients. But in Arkansas, CON law acted as a structural barrier, shaping both the privatization process and downstream market adjustments.

Importantly, however, Kindred was rarely the only home health provider in a county, even during the ADH period. Most counties had at least one other agency operating alongside ADH/Kindred, and beneficiaries often received care from providers based in neighboring counties. Appendix Table A.2 shows that in both 2014 and 2019, Kindred served fewer than 80% of beneficiaries in nearly all counties, and was entirely absent from a minority of counties. Thus, while CON law constrained new entry, it did not confer exclusive control. This context is critical to interpreting the patterns observed post-privatization: in counties where Kindred exited, spillover providers did not dramatically expand; in counties where Kindred grew, other agencies reduced therapy and payments. These patterns reflect not just regulatory barriers to entry, but also the strategic realignment of services within a constrained and partially competitive landscape.

3 Conceptual Framework

This section presents a stylized model to guide interpretation of the empirical results. The framework formalizes how the strategic supply response under prospective payment, operating through billing effort in coding and service mix, can raise the effective reimbursement received from Medicare, thereby shaping both the intensity and reach of home health delivery. The model contrasts the traditional view of ownership from Hart et al. (1997) (hereafter HSV), which emphasizes cost-cutting and potential reductions in quality or access, with an alternative mechanism in which privatization induces a *strategic supply response*.

In HSV, profit motives induce managers to minimize costs in ways that can degrade non-contractible quality or reduce access to socially valuable services. In contrast, I model behavior in home health care as primarily reflecting a *strategic supply-side response* under prospective payment: agencies allocate effort toward activities that increase effective reimbursement through documentation and service mix decisions. Rather than cutting costs, providers can increase profits by optimizing revenue within the payment system by exerting *billing effort*—resources devoted to documentation, coding, and visit planning.

This effort operates along two dimensions: (1) *strategic care provision*: adjusting the service mix and visit intensity to optimize payment formulas—and (2) *strategic documentation*: more

intensive coding of comorbidities or functional limitations that increase case-mix weights. Other potential channels, including marketing or referral effort, cost reductions, and mission-driven public provision, are discussed later in this section. The following model formalizes these incentives and their implications for provider behavior under different ownership structures.

The goal of this stylized framework is not to derive the optimal choice of billing effort e^*, but rather to characterize comparative statics: how privatization shifts effort and supply behavior. I assume that firms satisfy first-order conditions for optimality and that the privately chosen effort level e^* is weakly greater than the normalized public level $e = 0$. This abstraction allows a clear comparison between ownership types while capturing the key prediction that privatization induces a weakly positive shift in billing effort and, through it, both intensive and extensive margin expansion.

3.1 Model Setup

Let q denote the number of home health episodes delivered by the firm. Patients are indexed in ascending order of cost to serve. Let $MC(q)$ represent the marginal cost of treating patient q, where $MC'(q) > 0$ reflects that patients with greater clinical complexity or geographic distance require more resources. This upward-sloping marginal cost function represents the firm's supply curve: at any given reimbursement level, the firm supplies care up to the point where reimbursement equals marginal cost. Throughout, billing effort affects only effective reimbursement, not $MC(q)$.

Let $e \geq 0$ denote *billing effort*, which affects both the service mix s and documentation (coding) intensity d:

$$s = s(e), \quad d = d(e), \qquad s'(e) > 0, \;\; d'(e) > 0.$$

Medicare pays agencies a prospective, per-episode rate that depends on observable characteristics such as clinical acuity, functional status, and therapy volume. Let r denote the average reimbursement per episode. Reimbursement is determined by two endogenous components: the service mix s (e.g., composition and number of visits) and documentation (coding) intensity d. Formally,

$$r \;=\; r(s, d), \qquad r_s > 0, \;\; r_d > 0,$$

where subscripts denote partial derivatives. The agency cannot set r directly but can influence it through effort. By the chain rule,

$$\frac{dr}{de} \;=\; r_s \, s'(e) \;+\; r_d \, d'(e) \;>\; 0.$$

Let $C(q)$ denote the firm's total cost of serving q patients, with $C'(q) = MC(q)$ and $C''(q) > 0$. Let $K(e)$ denote the monetized cost of billing effort (e.g., staff time for documentation/coding, managerial attention, compliance and reputational risk), which is not directly observed in claims data; assume $K'(e) > 0$ and $K''(e) > 0$. Total revenue is $R(q, e) = r(s(e), d(e)) \cdot q$. Both public and

private agencies maximize profit with respect to q and choose e subject to institutional constraints:

$$\max_{q,e} \ \pi \ = \ r\big(s(e), d(e)\big)\, q \ - \ C(q) \ - \ K(e).$$

First-order conditions (assuming an interior solution) are:

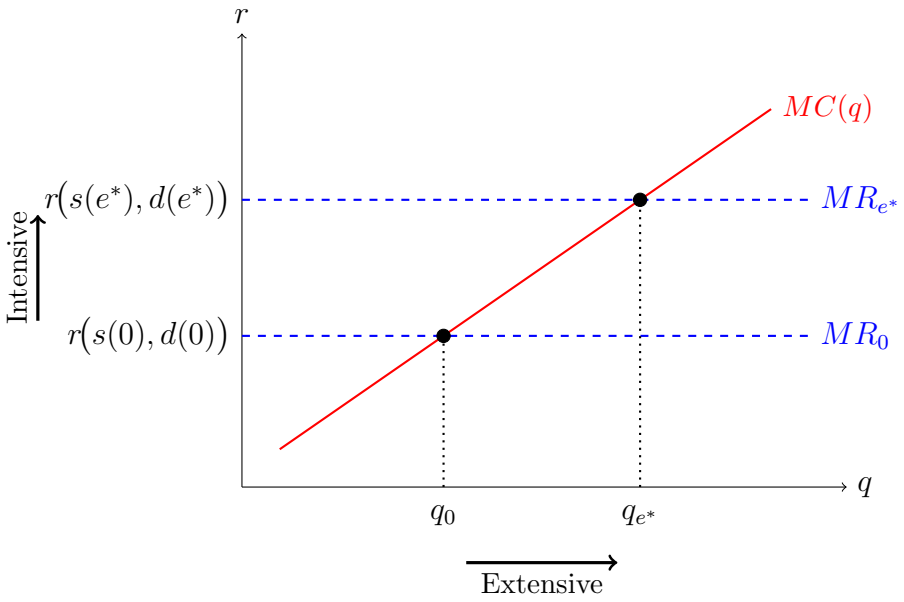
$$r\big(s(e), d(e)\big) \ = \ MC(q) \quad \text{(quantity choice: } MR = MC\text{)} \tag{1}$$

$$\big[r_s\, s'(e) + r_d\, d'(e)\big]\, q \ = \ K'(e) \quad \text{(effort choice)}. \tag{2}$$

Equation (1) states that agencies choose the number of patients to serve so that marginal revenue equals marginal cost. Equation (2) determines optimal billing effort: firms increase e until the marginal return (operating through service mix and documentation) equals the marginal cost of exerting effort. Public agencies are assumed constrained to $e = 0$ (institutional or mission-driven limitations), while private agencies choose $e^* \geq 0$. Privatization therefore shifts effort from $e = 0$ to $e = e^* > 0$, raising effective reimbursement from $r(s(0), d(0))$ to $r\big(s(e^*), d(e^*)\big)$ and expanding output from q_0 to q_{e^*}, where

$$r\big(s(0), d(0)\big) = MC(q_0) \quad \text{and} \quad r\big(s(e^*), d(e^*)\big) = MC(q_{e^*}).$$

This generates (i) *intensive-margin* growth (higher average reimbursement per episode) and (ii) *extensive-margin* growth, as the upward shift in marginal revenue leads the firm to expand output along its rising marginal cost curve.

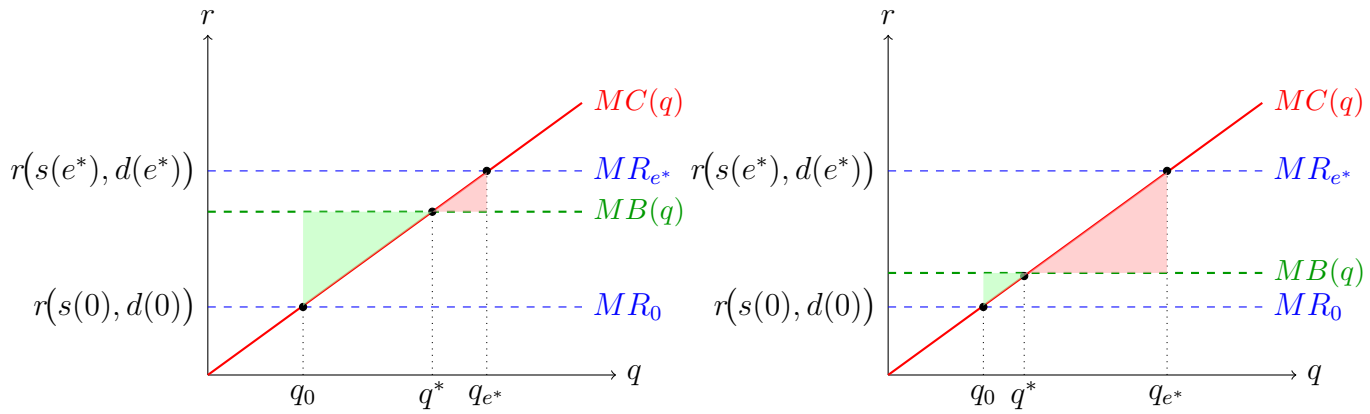


Effect of Privatization on Care Expansion. Privatization raises billing effort from e=0 to e=e^*, increasing $s(e)$ and $d(e)$ and shifting marginal revenue up from MR_0 to MR_{e^*}. The cutoff moves from q_0 to q_{e^*}, reflecting both intensive and extensive margin growth.

3.2 Welfare Interpretation

To assess welfare, I do not adopt a conventional willingness-to-pay framework in which welfare is evaluated using a demand curve. In home health, beneficiaries face no cost-sharing, so willingness to pay is not observed. Instead, I assume a flat marginal benefit curve $MB(q)$, which treats all eligible patients as equally likely to benefit if served. This simplification provides a normative benchmark that abstracts from heterogeneity in treatment effects. If $MB(q)$ were instead downward sloping, marginal patients induced by privatization would be expected to benefit less than inframarginal patients, making welfare losses from expansion more likely. Conversely, if $MB(q)$ were upward sloping, for example, if previously unserved patients had especially high unmet needs, the gains from expansion could be understated.

The two-panel figure below illustrates two stylized welfare scenarios. In both panels, privatization increases billing effort from $e = 0$ to $e = e^*$, raising the average reimbursement per episode and therefore marginal revenue. This shifts the marginal revenue line upward and the treatment threshold from q_0 to q_{e^*}. The welfare implications depend on whether the newly served patients have $MC(q) < MB(q)$ (left panel, welfare-increasing) or $MC(q) > MB(q)$ (right panel, welfare-decreasing). The empirical analysis examines whether the observed expansion was accompanied by improvements in clinical outcomes, which could be consistent with welfare-enhancing care expansion.



Effects of Privatization: Two Welfare Scenarios. Privatization raises billing effort from e=0 to e=e^*, increasing $s(e)$ and $d(e)$ and shifting marginal revenue up from MR_0 to MR_{e^*}, moving the cutoff from q_0 to q_{e^*}. The welfare consequences depend on the value of care delivered to newly served patients relative to their treatment costs. In the left panel, most expansion occurs where marginal cost is below marginal benefit, yielding a net welfare gain (green area exceeds red area). In the right panel, much expansion occurs beyond the efficient threshold q^* where marginal cost exceeds marginal benefit, resulting in a potential welfare loss (red area exceeds green area). The empirical analysis evaluates which scenario better characterizes observed expansion by examining outcomes among marginal patients.

3.3 Alternative Mechanisms and Extensions

Marketing or Referral Effort. In addition to billing effort, agencies may exert effort to increase patient volume directly through marketing, referral cultivation, or patient outreach. This channel differs from billing effort in both mechanism and observability. Whereas billing effort influences the effective reimbursement and is observable in claims-based indicators (e.g., therapy share, episode payments), marketing effort primarily affects patient acquisition and is less readily observed. However, secondary evidence, such as increases in star ratings or timely initiation of care, may suggest enhanced referral management or patient-facing outreach. While I do not formally model marketing effort in the main framework, it may act as a strategic complement to billing effort: higher effective reimbursement increases the return to acquiring additional patients.

Cost Differences. While this stylized model emphasizes a strategic supply response as the primary mechanism, it abstracts from potential differences in productive efficiency discussed in the extant literature. As discussed in previous sections, private agencies may achieve lower marginal costs through operational improvements, managerial expertise, or reduced bureaucratic constraints (Hart et al., 1997). If so, privatization could shift $MC(q)$ downward, allowing providers to profitably serve more patients even at a fixed reimbursement level. Figure D.1 in the Appendix illustrates this possibility: a downward shift from $MC_{pub}(q)$ to $MC_{priv}(q)$ expands the equilibrium quantity even without a change in $r(s(e), d(e))$. Both mechanisms—increased billing effort and improved cost efficiency—predict expansion, but the welfare implications differ. Real cost reductions increase social surplus, whereas billing-driven expansion may lead to overuse if $MC(q) > MB(q)$ at the margin. The model abstracts from efficiency differences across ownership types, but the results can be interpreted through either channel, or both operating in parallel.

Mission-Driven Public Provision. The model also abstracts from the possibility that public agencies may have served some patients at a financial loss—that is, beyond the profit-maximizing quantity, where reimbursement $r(s(0), d(0))$ fell below $MC(q)$. In practice, public providers may have internalized broader access goals or operated under non-financial objectives, leading them to serve beneficiaries who remained unprofitable even after accounting for potential billing optimization. Figure D.2 in the Appendix depicts this scenario, showing the subsidy implicit in public provision when agencies serve patients beyond the financially sustainable quantity. In contrast, privatization may raise effective reimbursement and/or reduce marginal costs, allowing private agencies to treat the same beneficiaries at a positive margin. This distinction helps explain how access could be maintained—or even expanded—despite the transition away from mission-driven public provision.

Inframarginal versus Marginal Effects of Privatization. The welfare analysis thus far focuses primarily on the extensive margin—newly served patients following privatization. However,

ownership changes also affect inframarginal patients—those who would have received care under either regime. Let $E = r(s(e^*), d(e^*)) \times q_{e^*}$ denote total Medicare home health expenditure. Following privatization, the change in expenditure can be decomposed as:

$$\Delta E = [r(s(e^*), d(e^*)) - r(s(0), d(0))] \times q_0 \; + \; r(s(e^*), d(e^*)) \times (q_{e^*} - q_0).$$

The first term represents higher payments for inframarginal patients, which may reflect pure documentation intensity (better coding without changing care) or real treatment intensity changes (e.g., more therapy visits). If primarily the former, this is a transfer from Medicare to providers without clinical benefit. The second term captures payments for marginal patients, where welfare depends on whether $MB(q) \geq MC(q)$ for these newly served beneficiaries.

Disentangling these effects is empirically important. When clinical outcomes remain flat despite higher spending and service intensity, much of the expenditure growth may represent transfers without commensurate benefit—consistent with the welfare-decreasing scenario in the figure above. Hence, the welfare assessment depends not only on whether care expanded, but on how billing practices changed for both existing and new patients.

Effects on Incumbent Agencies. While the model focuses on agencies directly affected by privatization, the empirical analysis also examines spillover effects on other home health providers operating in Arkansas during the same period. These agencies were already privately owned but varied in ownership form: some for-profit, others nonprofit. Although all faced the same prospective payment incentives, for-profit agencies may have been more responsive to local competitive conditions and more inclined to adjust service delivery or documentation practices in response to Kindred's expansion.

While I do not formally model these agencies' decisions, their behavior can be interpreted within the same (q, e) framework. For-profit competitors may re-optimize billing or service intensity in response to a different competitive environment. We might expect for-profit and non-profit non-acquired agencies to respond differently, if they have different objective functions or operate with less managerial or financial flexibility than their for-profit counterparts.

4 Data and Empirical Strategy

4.1 Data

This study uses rich administrative data from Medicare to evaluate the effects of privatizing publicly operated home health agencies in Arkansas. The sample spans 2010 through 2021 and includes all Medicare fee-for-service (FFS) beneficiaries, home health episodes, and all Medicare-certified home health agencies (HHAs) across the United States. I link multiple administrative sources to construct longitudinal measures of access, spending, patient composition, and service delivery.

Because PDGM and COVID-19 jointly introduced major structural and operational changes to the home health landscape, the main service-level analyses in this paper are restricted to the pre-PDGM period, ending in 2019. This restriction ensures that observed changes in visit mix and payment reflect the effects of privatization rather than shifts in the national payment model or pandemic-era shocks.

Home health episodes and spending are measured using 100% Medicare home health claims, which provide detailed information on service mix, visit counts, payment amounts, and episode characteristics. These claims are linked to the Master Beneficiary Summary File (MBSF), which offers beneficiary-level data on demographics, dual eligibility, ZIP code, and chronic conditions. Provider ownership and certification history are obtained from the Medicare Provider of Services (POS) files, tracking ownership transitions, and provider characteristics.

For clinical outcomes, I incorporate data from the Outcome and Assessment Information Set (OASIS), containing standardized patient evaluation metrics collected at the beginning and end of care episodes. Patient experience measures are drawn from the Home Health Consumer Assessment of Healthcare Providers and Systems (HHCAHPS) and Care Compare star ratings. Notably, the Quality of Patient Care star ratings, based on OASIS assessments and Medicare claims data, were first publicly reported in July 2015, while the Patient Survey star ratings, derived from HHCAHPS data, became available in January 2016. Consequently, analyses utilizing these measures have a limited pre-policy period for assessing trends.

To analyze agency-level financial performance and cost structures, I supplement the beneficiary-level claims data with Medicare Home Health Agency Cost Reports (HCRIS). These annual filings provide detailed financial information for all Medicare-certified agencies, including total operating costs, aggregate visit counts by discipline, and patient volume by payer type. I use these reports to construct measures of average variable cost per visit and to examine payer mix (e.g., Medicaid and private insurance) that are not visible in the Medicare claims data.

To characterize where beneficiaries are located, I link beneficiary ZIP codes and counties from the MBSF to external datasets. Rurality is determined using the Rural-Urban Continuum Codes (RUCC) from the USDA at the county level. Additional county-level socioeconomic and healthcare resource variables are sourced from the Area Health Resource File (AHRF). To assess spatial access and agency reach, distances between beneficiary ZIP codes and agency locations are calculated using centroid coordinates from the National Bureau of Economic Research (NBER) ZIP Code Tabulation Area (ZCTA) database.

The chronology of events as they relate to the analysis is illustrated in Figure 1. The timeline defines 2010–2014 as the pre-period, before any privatization announcements, and 2017–2021 as the post-period, after Kindred had completed the acquisition and assumed operations. The years 2015–2016 represent a transition period during which the privatization was announced and implemented. These transition years are excluded from the main difference-in-differences specifications to avoid contamination from anticipatory effects. Additionally, Gentiva, a Kindred subsidiary, began

operating in Arkansas in 2015, further justifying the classification of 2015-2016 as a partially-treated period.

Because PDGM and COVID-19 jointly introduced major changes to the home health landscape, the main service-level analyses in this paper are restricted to the pre-PDGM period, ending in 2019. This restriction ensures that observed changes in visit mix and payment reflect the effects of privatization rather than shifts in the national payment model or pandemic-era shocks. Market-level analyses of overall utilization and spending use the full 2017–2021 post-period to capture longer-term effects on access and aggregate expenditures.

4.2 Baseline Market Structure and Pre-Treatment Characteristics

To contextualize the empirical design, I summarize baseline characteristics of agencies and beneficiaries in the pre-period (2010–2014). Table 1 compares Kindred-acquired (formerly ADH) agencies, other Arkansas providers, and control-state agencies. Patients served by ADH agencies were more likely to be rural, dual-eligible, and Black compared to those in other groups. They had slightly lower average comorbidity scores and fewer cognitive or memory impairments.

Service delivery also differed sharply. ADH agencies provided more visits per episode (22.2) and far more aide visits (10.3), but fewer therapy visits (2.7) than spillover (5.8) or control (5.9) agencies. This pattern is consistent with a care model oriented toward support and maintenance rather than reimbursement-maximizing therapy. Correspondingly, ADH episodes were reimbursed at significantly lower levels ($1,787 vs. $2,340 to $2,720 elsewhere), despite similar or higher visit counts.

Table A.2 shows that ADH/Kindred served beneficiaries in nearly every Arkansas county in both 2014 and 2019 (73 and 75 of 75 counties, respectively). However, their market share was relatively modest in most local areas. In 2014, ADH had a share above 80% in only one county, and a share below 20% in 57 counties. These patterns persisted after privatization, indicating that Kindred's statewide expansion occurred not through exclusive control, but by scaling up within markets that already had multiple providers.

Figure 2 shows ADH and Kindred's share of home health episodes across Arkansas ZIP codes before and after privatization. In the pre-period (2010 to 2014), ADH's highest market shares were concentrated in rural parts of the state, particularly in the southern and northeastern regions. Urban areas such as Little Rock, Fort Smith, and Fayetteville had comparatively low public agency penetration. Following the 2016 transition, Kindred expanded its presence substantially in urban markets while maintaining broad rural coverage, reflecting both continuity and strategic growth.

These geographic patterns have important implications for identification. The transition shifted a publicly operated system with a rural footprint into private hands, enabling both reallocation of patients and potential net expansion. While ADH maintained near-statewide presence before the sale, Kindred's growth was uneven, with particularly large gains in urban ZIP codes. This variation in post-privatization market share, especially across geographies with differing population density,

infrastructure, and provider competition, provides a natural source of treatment intensity that I exploit in the within-Arkansas analyses that follow.

4.3 Empirical Strategy

My empirical strategy combines several complementary research designs to examine how the privatization of Arkansas's public home health agencies affected provider behavior and market outcomes. I begin at the provider level, comparing outcomes at Kindred-acquired agencies to agencies in control states, and separately analyzing spillover effects on non-Kindred agencies in Arkansas. These analyses focus on visit mix, episode payments, patient composition, and coding practices. I then turn to market-level outcomes, estimating state-year difference-in-differences and synthetic control models to capture extensive-margin effects on utilization and spending. Finally, I exploit within-Arkansas variation by constructing a continuous measure of Kindred's local market expansion, enabling county-level event studies that link treatment intensity to changes in use and spillovers. Together, these approaches provide complementary evidence on both the intensive and extensive margins of care delivery, as well as on competitive responses by incumbent providers.

4.3.1 Provider-Level Identification

At the agency level, I categorize providers into three groups. The treated group comprises former ADH agencies acquired by Kindred in 2016. The spillover group includes other Arkansas-based HHAs that may have been exposed to market changes following the transaction. The control group consists of agencies outside Arkansas that were not in states affected by privatizations during the study window.

I estimate both standard difference-in-differences models and dynamic event studies at the provider (agency) level, using quarterly data. The baseline DiD specification is:

$$Y_{it} = \alpha + \beta(Treated_i \times Post_t) + \theta Treated_i + \delta_t + \varepsilon_{it}, \tag{3}$$

where $Y_{i,t}$ is the outcome for provider i in quarter t; $Treated_i$ is an indicator for whether agency i is in the treated group; $Post_t$ equals 1 for periods after 2016; and δ_t are year-quarter fixed effects that absorb time shocks common to all agencies. Because δ_t fully absorb $Post_t$, the treatment effect is identified solely from the interaction term ($Treated_i \times Post_t$), capturing differential changes for treated agencies relative to controls after the ownership transition. I estimate this model separately for each treatment group: Kindred-acquired agencies versus controls, and spillover agencies versus controls.

To examine dynamic effects and test for pre-trends, I implement a quarterly event study specification:

$$Y_{it} = \alpha + \sum_{k \neq 2014Q3} \beta_k (Treated_i \times \mathbf{1}\{t = k\}) + \delta_t + \varepsilon_{it}, \tag{4}$$

where $\mathbf{1}\{t = k\}$ are quarter indicators and 2014Q3 serves as the omitted reference period. The model includes year-quarter fixed effects (δ_t) to control for aggregate time trends. Standard errors for both specifications are clustered at the provider level. Note that because the Arkansas Department of Health agencies were consolidated into fewer provider identifiers following the sale, a within-agency design is not feasible. Instead, I estimate specifications with fixed effects for the treated group as a whole, which absorb time-invariant differences between the privatized agencies and the control agencies. This corresponds to a group-level difference-in-differences design and is consistent with the institutional setting, since the public agencies operated under a common organizational structure prior to privatization.

I apply these models to outcomes including average number of visits per episode, payment per episode, patient composition, and coding behavior. The key identifying assumption for this approach is that, absent privatization, treated agencies would have followed trajectories parallel to control agencies. While this counterfactual is inherently unobservable, I address this assumption in several ways. First, I examine pre-treatment trends visually and statistically to confirm similar patterns before the privatization announcement. I also implement the analysis with different control groups to ensure results are not driven by the particular selection of comparison agencies.

Spillover Effects by Ownership Type To assess how different organizational forms respond to market changes, I stratify the spillover analysis by provider ownership type. I estimate the DiD specification separately for for-profit and nonprofit agencies, where the treated group consists of non-Kindred agencies in Arkansas, and the control group includes agencies of matching ownership type in control states.

The identification assumption is that, absent the privatization, non-Kindred agencies in Arkansas would have followed trajectories similar to their ownership-matched counterparts in other states. This approach enables me to isolate differential responses by ownership type.

4.3.2 Market-Level Identification Using Difference-in-Differences

To isolate the effect of the ownership change, I exclude from the control group any state that underwent substantial privatization of public HHAs between 2010 and 2021. These excluded states include South Carolina, Oklahoma, Kentucky, North Carolina, Georgia, and Idaho (see Table A.1). In these states, changes in public provision were typically gradual, geographically fragmented, or driven by county-level decisions rather than a single statewide policy change, making them less well suited for a clean difference-in-differences design. The remaining control states did not experience any known transitions away from public provision during the study period and thus serve as a comparison group for evaluating the effects of Arkansas's ownership change.

Inclusion of Alabama in the Control Group. A potential concern in constructing the control group is whether to include states that maintained publicly operated home health agencies throughout the study period. Alabama is one such case: unlike most other states, the Alabama Department of Public Health (ADPH) continued to operate a statewide, Medicare-certified home health program in all 67 counties from 2010 through 2021. Because Alabama did not experience a privatization event or structural change in ownership, it satisfies the core identification assumption of a stable counterfactual. Moreover, ADPH's organizational structure and geographic coverage closely resembled Arkansas's pre-2016 public program, making Alabama a valuable comparison for estimating the effects of ownership change. I include Alabama in the main control group and test robustness to its exclusion in sensitivity analyses. Results are substantively similar when Alabama is omitted (see Appendix Table A.7).

Estimation Strategies. I estimate two complementary specifications. The baseline DiD model is:

$$Y_{st} = \alpha + \beta(\text{Arkansas}_s \times \text{Post}_t) + \gamma_s + \delta_t + \varepsilon_{st} \tag{5}$$

where Y_{st} is the outcome in state s and year t, Arkansas_s is an indicator for Arkansas, Post_t is a post-treatment indicator, and γ_s and δ_t are state and year fixed effects. This specification captures the average treatment effect across all post-privatization years. Standard errors are clustered at the state level.

To examine dynamic effects and validate the parallel trends assumption, I also estimate an event study specification:

$$Y_{st} = \alpha + \sum_{t \neq 2014} \beta_t(\text{Arkansas}_s \times \mathbf{1}\{t = k\}) + \gamma_s + \delta_t + \varepsilon_{st} \tag{6}$$

where $t = 2014$ is the reference year. This specification examines year-by-year effects of privatization relative to the pre-announcement baseline, allowing me to assess whether treatment effects grew over time and to confirm the absence of pre-trends.

Subgroup Analysis. To examine heterogeneity in treatment effects across different beneficiary populations, I estimate the same specifications separately for each subgroup g (urban, rural, dual-eligible, etc.). For each analysis, the outcome $Y_{g,s,t}$ represents a subgroup-specific measure (e.g., home health users per 1,000 urban beneficiaries, home health users per 1,000 rural beneficiaries). This approach allows for direct comparisons of treatment effects across different segments of the Medicare population while maintaining consistent interpretation of the coefficients.

For market-level analyses, the key identifying assumption is that Arkansas, in the absence of privatization, would have experienced trends in home health utilization and spending similar to control states. This assumption is supported by parallel pre-treatment trends in the event study specifications and is further reinforced through the synthetic control approach described below,

which constructs a more tailored counterfactual.

4.3.3 Synthetic Control Method

To complement the DiD analysis, I construct a synthetic control for Arkansas using a weighted average of control states that best match pre-treatment trends in home health outcomes. Because Arkansas is the only treated unit, synthetic control provides a flexible and transparent approach to constructing a credible counterfactual.

Let Y_{1t} denote the observed outcome in Arkansas at time t, and let Y_{1t}^N denote the potential outcome that would have been observed in Arkansas in the absence of treatment ("no privatization"), following the notation of Abadie et al. (2010). Because Arkansas is exposed to treatment after 2016, $Y_{1t} = Y_{1t}^I$ (the outcome under intervention) for t in the post-treatment period, while Y_{1t}^N is unobserved and must be estimated.

The synthetic control estimator constructs this counterfactual as a weighted average of control states:

$$Y_{1t}^N = \sum_{j=2}^{J+1} w_j^* Y_{jt},$$

where J is the number of donor states, and the weights w_j^* are chosen to minimize pre-treatment prediction error subject to $w_j \geq 0$ and $\sum_j w_j = 1$. The estimated treatment effect at time t is then:

$$\hat{\alpha}_{1t} = Y_{1t} - Y_{1t}^N.$$

I assess significance using the pre-treatment standard deviation of the synthetic control gap, following Abadie et al. (2010). I also conduct placebo tests by applying the synthetic control method to each control state as if it were treated, producing a distribution of placebo effects for comparison. This approach accounts for serial correlation and heteroskedasticity in the absence of repeated treatment events.

The synthetic control analysis is implemented for key outcomes, including users per 1,000 beneficiaries, spending per beneficiary, and subgroup-specific use for duals, rural, and urban populations.

4.3.4 Within-Arkansas Continuous Treatment Design

To validate the state-level difference-in-differences design and decompose it further, I implement a within-Arkansas design that exploits county-level variation in exposure to privatization. To leverage variation in privatization intensity across counties, I use the change in Kindred's market share between 2014 and 2019 as a continuous treatment measure. This approach helps identify how the magnitude of Kindred's market expansion affected utilization at both the intensive and extensive margins. I implement difference-in-differences models that interact this continuous treatment

intensity with the post-privatization indicator:

$$Y_{ct} = \alpha + \delta_t + \beta(\text{ShareChange}_c \times \text{Post}_t) + \theta\text{ShareChange}_c + \delta_c + \varepsilon_{ct},$$

where Y_{ct} is the outcome in county c and year t, ShareChange_c measures Kindred's market share growth, and Post_t indicates post-2017 periods.

To examine dynamic effects and test for pre-trends, I also estimate event studies:

$$Y_{ct} = \alpha + \delta_t + \sum_{k \neq 2014} \beta_k \text{ShareChange}_c \times \mathbf{1}(t = k) + \theta\text{ShareChange}_c + \varepsilon_{ct},$$

This approach estimates how the effect of privatization varied with Kindred's penetration into local markets. I apply this model to market-level outcomes including home health users and episodes per 1,000 beneficiaries, which provides evidence on the extensive margin of care expansion. Standard errors are clustered at the county level.

To assess whether non-Kindred agencies adjusted their service patterns in response to privatization, I apply the continuous treatment design to outcomes measured specifically at spillover agencies. This approach tests whether agencies faced with greater competitive pressure from Kindred responded by changing their billing practices, visit mix, or coding behavior:

$$Y_{ct}^{\text{spillover}} = \alpha + \delta_t + \beta(\text{ShareChange}_c \times \text{Post}_t) + \theta\text{ShareChange}_c + \varepsilon_{ct},$$

With the corresponding event study:

$$Y_{ct}^{\text{spillover}} = \alpha + \delta_t + \theta\text{ShareChange}_c + \sum_{k \neq 2014} \beta_k \text{ShareChange}_c \times \mathbf{1}(t = k) + \varepsilon_{ct}.$$

Here, the coefficients capture how non-Kindred agencies in counties with greater Kindred expansion adjusted their service delivery along the intensive margin. I examine key outcomes including episode payments, therapy visits, aide visits, and nurse visits. This analysis complements the extensive margin results by showing whether privatization also induced strategic responses among incumbent providers facing new competitors.

To address potential endogeneity concerns regarding the market share change variable, I complement the main event study with an instrumental variables approach. The concern is that Kindred's post-acquisition market expansion decisions may respond to unobserved local market conditions that also influence home health demand. I instrument for share change using the square of baseline market share (2010–2014 average), which captures the strong non-linear relationship between pre-acquisition market structure and subsequent share changes. The instrument is valid under the assumption that this non-linear relationship reflects Kindred's strategic decisions based on pre-existing market positions rather than responses to contemporaneous local demand conditions.

4.3.5 Addressing Endogeneity Concerns

A natural concern in this setting is that the decision to privatize may have been endogenous to prior trends in outcomes—raising the possibility that changes attributed to ownership could instead reflect underlying deterioration in public agency performance. For example, if Arkansas privatized its home health system in response to worsening access, rising costs, or declining quality, then simple pre/post comparisons could conflate the effects of ownership with pre-existing trajectories.

Several features of the institutional context and research design help mitigate this concern. First, the privatization was politically motivated and tied to a change in gubernatorial administration rather than a sudden collapse in agency performance. Governor Asa Hutchinson, who took office in 2015, pursued privatization as part of a broader ideological agenda to reduce the state's role in direct service provision. While fiscal and operational pressures may have contributed to the decision, the timing aligns more closely with political turnover than with acute performance decline—suggesting that the event was plausibly exogenous to short-run changes in home health outcomes. Kindred also did not operate any skilled nursing facilities, acute care hospitals, or long-term care hospitals in Arkansas during the study period, further limiting the potential for vertical integration to confound downstream outcomes (see Appendix C).

Second, my event study models allow direct testing for pre-trends across treated and control units. In both the provider-level and market-level analyses, I find limited evidence of differential trends in core outcomes prior to the 2016 sale. For the most part, treated and control agencies followed similar trajectories in therapy use, payments, and patient mix in the years leading up to privatization, and state-level utilization trends moved in parallel until 2016. While pre-trend differences exist for some outcomes, the sharp discontinuity at the time of privatization supports a causal interpretation: the timing and magnitude of the shift are difficult to reconcile with continuation of prior dynamics alone.

Third, I strengthen inference by incorporating multiple comparison strategies. The difference-in-differences models compare Arkansas to a group of states that retained stable ownership structures, while the synthetic control approach constructs a counterfactual Arkansas using weighted pre-treatment trends in outcomes. The consistency of results across designs—both in timing and magnitude—reinforces the interpretation that the observed changes reflect a causal effect of privatization.

5 Results

This section presents the main empirical findings, organized to test the mechanisms proposed in the conceptual framework. I begin in Section 5.1 by isolating provider behavior at the intensive margin. Here, I analyze agency-level data on visit mix, coding intensity, and episode payments to characterize the strategic supply response and distinguish whether the observed changes in reimbursement were driven by patient selection or by strategic billing effort. In Section 5.2, I turn

to the extensive margin to examine how the ownership transition affected the aggregate supply of care. I test whether privatization led to market-wide expansion or contraction, validating these state-level patterns with a within-Arkansas analysis in Section 5.3. I then explicitly evaluate the "cream-skimming" hypothesis in Section 5.4 by analyzing changes in geographic reach and access for different patient populations. Section 5.5 assesses the broader market dynamics. I estimate spillover effects on incumbent home health agencies to determine whether the privatization of the public network altered the behavior for the remaining private competitors. Finally, Section 5.6 evaluates the welfare implications of these shifts. I contrast the observed changes in spending and utilization against objective clinical measures, including readmissions, mortality, and discharge outcomes.

5.1 Provider Behavior and The Strategic Supply Response

I begin by testing whether privatization altered how agencies responded to existing financial incentives, specifically, in their care delivery and documentation practices. Under Medicare's pre-2020 prospective payment system, agencies were paid a fixed amount per 60-day episode, with reimbursement increasing in therapy volume and documented case-mix severity. A central prediction of the conceptual framework is that privatization induces a strategic supply response—through billing effort in service delivery and documentation—that raises the effective price of an episode by delivering more reimbursable care and documenting greater patient complexity. If newly privatized agencies responded to these incentives, we would expect to observe changes in visit mix, case-mix coding, and payments per episode.

I test these predictions using agency-level difference-in-differences models comparing Kindred-acquired agencies in Arkansas to control agencies in other states that did not experience ownership changes.

Figure 3 shows a sharp and sustained increase in therapy visits per episode beginning in 2017. The event study indicates that treated agencies added roughly four therapy visits per episode on average relative to control agencies by 2019. Table A.3 quantifies the average effect pooled across the post-period years as 3.15 additional therapy visits per episode ($p < 0.01$), relative to a pre-treatment mean of 2.70—an increase of more than 100%. At the same time, aide visits fell sharply. Figure 4 shows a large decline in aide services at treated agencies, with the event study tracing a drop of nearly nine visits per episode. Table A.3 estimates an average reduction of 8.88 aide visits ($p < 0.01$), or roughly 90% of baseline aide volume. Nursing visits increased modestly (+2.12, $p < 0.01$), but the total number of visits per episode declined by 4.04 ($p < 0.01$), as shown in Figure 5. This effect reflects a discrete level shift concentrated in the quarters immediately preceding and during the privatization transition (2015–2016), with visit counts stabilizing thereafter. Taken together, the sharp increase in therapy visits and decline in aide services, with no commensurate increase in total visits, are consistent with a strategic re-optimization of service mix under the pre-2020 payment system to increase reimbursement.

Consistent with the strategic supply response, I also observe increases in reported patient acuity across several coding domains. Figure 6 shows a rise in recorded functional limitations (activities of daily living), which are reported by agencies themselves and directly feed into case-mix weights under the prospective payment system. Table A.5 estimates an increase of 0.30 points on the activities of daily living index ($p < 0.01$). While these shifts may partly reflect true changes in patient mix, the magnitude and timing are consistent with increased documentation effort by agencies seeking higher reimbursement. Furthermore, when looking at changes clinical complexity, as measured by Elixhauser comorbidity scores constructed by excluding home health claims (Figure A.1), the observed increases are not nearly as pronounced as the increases in activities of daily living self-reported by the newly privatized home health agencies.

I next examine whether these shifts in service mix and documentation translated into higher payments. Figure 7 shows a pronounced increase in average episode payments at treated agencies post-2016. Table A.3 estimates an increase of \$490 per episode ($p < 0.01$), a 27% increase from the pre-period mean of \$1,837. These payment gains cannot be fully explained by changes in observed demographics or diagnoses. As stated above, some of the increase reflects real growth in documented complexity, but the timing and structure of the changes suggest that increased billing effort, including more intensive documentation of case-mix.

Two additional analyses help rule out patient composition as the sole driver of these effects. First, Figure 8 plots episode payments and therapy use for a panel of beneficiaries observed in both the pre- and post-periods. Even among this stable population, treated agencies increased therapy visits and episode payments relative to controls, suggesting that provider behavior changed even when patient characteristics were held fixed. Second, Figures A.3 and A.4 present diagnosis-specific event studies for diabetes and COPD, showing similar post-treatment increases in therapy and payments within clinical categories. As a further check, Appendix Figure A.5 presents event study estimates using outcomes residualized on episode-level characteristics. Treated agencies continue to show large increases in therapy visits and payments and sharp reductions in aide visits, even after accounting for patient mix. These results reinforce the interpretation that changes were driven by the strategic supply response, specifically strategic coding and documentation, rather than shifts in who was served.

Appendix Figure A.6 further replicates the analyses using a control group that excludes Kindred- or Gentiva-affiliated agencies in other states, mitigating concerns about shared corporate practices. The resulting estimates closely mirror the baseline, supporting the conclusion that effects reflect Arkansas-specific privatization rather than corporate-wide changes.

Taken together, these results are consistent with privatization inducing a strategic supply response that raised the effective price of serving each patient. Agencies reallocated care toward reimbursable services and intensified documentation, leading to higher payments. These adjustments appear to be strategic responses to financial incentives, rather than changes in clinical need.

5.2 Market Expansion and the Extensive Margin

The conceptual framework predicts that privatization induces a strategic supply response, in which agencies adjust coding intensity and service mix to raise the effective reimbursement per episode. By shifting the marginal revenue curve upward, this behavior creates both the ability and the incentive for the for-profit agency to serve more patients, expanding care along the extensive margin until marginal revenue again equals marginal cost. In this section, I test whether this predicted expansion materialized by comparing utilization of home health care in Arkansas and control states before and after privatization.

I begin with a state-level difference-in-differences model comparing Arkansas to a control group of states with stable ownership structures during the study period. Figure 9(a) presents event-study estimates for the number of Medicare home health users per 1,000 beneficiaries. Prior to the 2016 sale, Arkansas and control states followed similar trends. Beginning in 2017, the treatment effect rises steadily, reaching 8–9 additional users per 1,000 by 2021. Table A.6 reports the average post-treatment effect as 6.61 additional users per 1,000 beneficiaries ($p < 0.01$), a 9.1% increase relative to the pre-period mean of 71.91.

Spending per beneficiary rose in parallel. Figure 9(b) shows a comparable upward shift in average Medicare home health spending per beneficiary in Arkansas relative to control states. This increase reflects both higher episode payments (documented in Section 5.1) and a greater number of episodes per beneficiary, consistent with supply-side expansion in response to higher effective reimbursement.

The post-privatization expansion was broad-based. Figure 10 reports subgroup difference-in-differences estimates that compare changes in home health use among specific beneficiary groups in Arkansas to the same groups in control states. Each panel therefore compares changes in a given subgroup in Arkansas to changes in the same subgroup in control states. Urban beneficiaries in Arkansas experienced the largest relative gains, approximately 13–14 additional users per 1,000 by 2021 compared to urban beneficiaries in control states. Rural beneficiaries also saw significant increases, on the order of 8–9 additional users per 1,000 relative to rural beneficiaries in control states. Among dual-eligible beneficiaries, use increased by approximately 9–11 per 1,000 relative to dual-eligible beneficiaries in control states. While the magnitude of expansion is greatest in urban areas, the positive and statistically significant estimates for rural and dual-eligible beneficiaries indicate that growth was not confined to the most profitable or densely populated markets.

I further test for changes in utilization in the most remote areas using USDA Rural–Urban Continuum Codes. Appendix Figure A.9 shows no evidence of reduced access even in very rural (RUCC codes 8–9) or extremely rural (RUCC code 9) counties. While estimated effects in these areas are small and not statistically significant, they indicate that private agencies maintained service availability for the most geographically isolated beneficiaries, even if they did not experience the same growth that occurred in denser and potentially more profitable markets.

To assess the robustness of these results and complement the difference-in-differences design, I

implement a synthetic control model using pre-treatment trajectories to construct a counterfactual for Arkansas. As shown in Figure A.7(a), the synthetic control closely tracks Arkansas through 2015, after which the actual series diverges upward, reaching a persistent gap of roughly 4–5 users per 1,000 by 2020. Numerical estimates for each post-treatment year are reported in Table A.10.

The subgroup panels show similar patterns. For urban and dual-eligible beneficiaries, post-privatization gaps are positive and broadly consistent with the magnitude and timing of the state-level difference-in-differences estimates. In rural areas, the synthetic control results show no evidence of reduced access following privatization; if anything, rural use remains stable or increases modestly relative to the counterfactual. Taken together, the synthetic control analysis corroborates the difference-in-differences findings and indicates that the post-2016 divergence is unlikely to be driven by differential pre-trends.

The results provide clear evidence of extensive-margin expansion following privatization. The state-level difference-in-differences estimates show a statistically significant increase in home health users per 1,000 beneficiaries after 2016, with an average post-treatment effect of approximately 6–7 additional users per 1,000. Event study estimates indicate little evidence of differential pre-trends prior to 2015, modest effects in 2015 consistent with partial exposure following Kindred's subsidiary acquisition, and a sharp and persistent increase beginning in 2017 after the full transfer of licenses.

Subgroup difference-in-differences estimates show positive and statistically significant increases in use among urban and dual-eligible beneficiaries, and positive increases for rural beneficiaries as well. While the magnitude of growth is largest in urban areas, there is no evidence in the state-level event studies of reduced access in rural areas at any point following privatization. The synthetic control estimates yield similar qualitative patterns: a strong post-2016 divergence in overall use and positive gaps for urban and dual beneficiaries, with no indication of contraction in rural areas.

Overall, these designs consistently point to broad-based expansion along the extensive margin following privatization. The absence of contraction among rural or higher-need populations suggests that the expansion did not operate through cream-skimming or withdrawal from less profitable groups, but rather through an outward shift in utilization consistent with increased effective reimbursement. In Section 5.4, I examine this mechanism directly by testing whether expansion occurred toward patients who are costlier to serve and whether changes in utilization reflect higher effective reimbursement rather than reductions in marginal cost.

5.3 Within-Arkansas Analysis and Decomposition

The cross-state analysis provides evidence of market expansion following privatization, but it abstracts from heterogeneity in exposure within Arkansas. I therefore complement the state-level results with a within-Arkansas analysis that exploits county-level variation in the intensity of privatization. The primary purpose of this exercise is to validate the extensive-margin growth documented above and to assess whether expansion was larger in counties where Kindred increased its market presence more aggressively. A secondary objective is to distinguish between net market

expansion and displacement of incumbent providers.

I measure treatment intensity using the change in ADH/Kindred's county-level market share between 2014 and 2019 and estimate difference-in-differences models that interact this continuous measure with a post-privatization indicator. Figures A.10(a) and (b) present event study estimates for home health users and episodes per 1,000 beneficiaries. Pre-treatment coefficients are small and statistically indistinguishable from zero, with no evidence of differential pre-trends across counties with higher versus lower subsequent Kindred expansion. Beginning in 2016 and becoming more pronounced in 2017, coefficients diverge upward, indicating that counties experiencing larger increases in Kindred's market presence also experienced larger increases in home health utilization.

Table A.9 reports pooled estimates for the 2017–2019 period. In the OLS specification, a 10 percentage point increase in Kindred's market share is associated with 1.03 additional home health users per 1,000 beneficiaries ($p < 0.05$) and 5.51 additional episodes per 1,000 beneficiaries ($p < 0.01$). Relative to baseline levels of approximately 75 users and 154 episodes per 1,000 beneficiaries, these effects correspond to increases of roughly 1–2 percent in user rates and 3–4 percent in episode rates per 10 percentage point increase in Kindred share.

A potential concern is that post-acquisition changes in Kindred's market share may reflect unobserved county-level shocks that also influence home health demand. To address this concern, I estimate instrumental variables specifications that use the square of baseline treated share as an instrument for post-period share expansion. The first-stage F-statistic equals 129, indicating a strong instrument.

The IV estimates are larger in magnitude but similar in sign to the OLS results. A 10 percentage point increase in Kindred's market share is associated with 2.17 additional users per 1,000 beneficiaries ($p < 0.01$) and 9.35 additional episodes per 1,000 beneficiaries ($p < 0.001$) during 2017–2019. The consistency in sign and statistical significance across OLS and IV specifications suggests that the positive relationship between Kindred expansion and home health utilization is not driven solely by endogenous demand shocks.

Having established that market expansion scales with local exposure to privatization, I next decompose changes in home health use within Arkansas to distinguish between genuine market growth and displacement of incumbent providers. Specifically, I decompose changes in utilization into users served by (1) Kindred-acquired agencies, (2) non-acquired incumbent agencies, and (3) the combined total, using the same continuous difference-in-differences framework.

Figure A.11 plots the dynamic effects. Kindred-acquired agencies show a sharp and sustained increase in users per 1,000 beneficiaries following privatization. Spillover agencies, by contrast, exhibit a modest decline beginning around 2018, concentrated in counties where Kindred's expansion was largest, suggesting some displacement of patients or referrals from incumbent providers.

Crucially, however, the total effect remains positive throughout the post-period. Even in counties where Kindred expanded most aggressively, overall home health use increased. This pattern indicates that privatization did not merely reallocate patients across providers. While some business

stealing occurred, it was outweighed by net market expansion.

This finding aligns with the model's prediction that an increase in the effective price per episode raises marginal revenue and shifts the treatment threshold outward. The modest decline in spillover agency volume reflects partial displacement, but the sustained increase in total users confirms that privatization primarily operated through real supply-side expansion rather than pure reallocation of beneficiaries across agencies.

5.4 Patient Selection and Cost Structure

The extensive-margin expansion documented above raises two related questions about mechanism. First, did agencies expand toward higher-cost patients, those with greater clinical complexity or longer travel distances, or did privatization induce cream-skimming away from unprofitable groups? This question matters because cream-skimming is itself a cost-reduction strategy in the HSV framework: by avoiding high-cost patients, private providers can lower costs without improving productive efficiency. By contrast, the conceptual framework developed here predicts the opposite pattern—higher effective reimbursement shifts marginal revenue upward, inducing expansion along an upward-sloping marginal cost curve toward patients who are costlier to serve. Finding expansion toward rather than away from costly patients would therefore be consistent with the billing effort mechanism while ruling out cream-skimming as an alternative cost-reduction channel. Second, was expansion enabled by higher effective reimbursement through billing effort, or by genuine reductions in the marginal cost of production? I examine both questions in turn, using changes in patient composition, geographic reach, access across patient groups, and per-visit costs.

Patient Composition. If privatization raised the effective price per episode, the model predicts that agencies should expand service to patients with higher marginal costs—specifically those with greater clinical complexity or longer travel distances. To test this prediction, I use several complementary indicators that vary in their sensitivity to documentation incentives. Figure 6 shows that ADL scores, which are self-reported by agencies and directly determine payment weights, rose sharply after privatization. Table A.5 estimates an average increase of 0.30 points ($p < 0.01$), or roughly 12% relative to the pre-period mean. Importantly, broader measures of acuity also rose, though more modestly. Chronic condition counts (derived from prior-year claims) and Elixhauser scores (derived from concurrent non-home health claims) increased significantly. The divergence between the sharp rise in coding-sensitive ADLs and the more gradual increase in claims-based measures suggests that while agencies intensified documentation, they also expanded access to a genuinely sicker and more complex patient population.

Geographic Reach. I also find evidence that the acquired agencies expanded to serve patients located geographically farther. By calculating the distance between current patients and the fixed 2014 branch locations, I isolate changes in the geographic dispersion of patients from changes in

branch office locations. Figure A.15 shows that the average distance increased by roughly 3–4 miles for treated agencies starting in 2017. Because Medicare pays an episode-based rate that does not vary with travel time, these patients represent higher marginal cost admissions. This willingness to travel farther is consistent with higher effective reimbursement justifying the service of geographically costlier patients.

Access Across Patient Groups. These results suggest expansion toward higher-cost patients on average, but they do not rule out the possibility that agencies selectively reduced care for patients who remained persistently unprofitable under Medicare's payment system. The analyses above address this concern using extensive-margin measures (such as users per 1,000 beneficiaries) and changes in the composition of patients served. I next examine whether agencies adjusted *conditional treatment intensity* for patients predicted to be low-margin.

To do so, I focus on a subgroup of patients predicted—based on pre-privatization models estimated in control states—to require high visit intensity while receiving low episode payments. Because expected reimbursement for these patients may be low relative to expected costs, they plausibly remain unattractive even after billing re-optimization. Figure A.12 presents event-study estimates of realized visits per episode for this subgroup. Because these estimates are conditional on receiving home health care, they do not identify changes in entry into care. Instead, they test whether agencies that continue to serve low-margin patients reduced delivered intensity. The results suggest that visit intensity for these patients did not decrease, though estimates are imprecise.

A related concern is that privatization may have shifted agency capacity toward higher-margin Medicare patients at the expense of lower-paying payers, such as Medicaid or privately insured beneficiaries. To examine this possibility, Appendix Figure A.14 plots total patient volume at ADH/Kindred agencies by payer source using Medicare cost report data. While Medicare patient volume increases substantially following privatization, there is no corresponding decline in non-Medicare patient volume. If anything, other-payer volume rises alongside Medicare volume. Although the cost reports aggregate Medicaid and private insurance into a single "other payer" category and therefore do not allow separate analysis of Medicaid patients, the absence of contraction in non-Medicare volume suggests that Medicare expansion did not come at the expense of access for lower-paying groups. This pattern is consistent with the broader evidence on rural and dual-eligible populations, which likewise shows no reduction in service availability following privatization.

Cost Structure. Having established that expansion occurred broadly with no evidence of cream-skimming, I turn to the efficiency mechanism directly. I examine the average variable cost per visit using Medicare cost report data in Figure A.16. The data reveal a sharp, temporary spike in costs during the 2015–2016 transition, likely reflecting administrative disruption, but post-2017 costs per visit stabilized at levels comparable to the pre-privatization baseline.

Importantly, the stability of per-visit costs does not contradict the finding that agencies expanded along an upward-sloping marginal cost curve. Rather, it distinguishes between a *shift* of the curve and *movement along* it. If privatization had generated significant efficiencies in care delivery (e.g., via improved logistics or staffing), we would expect the entire variable cost schedule of producing visits to shift downward, resulting in lower per-visit costs. The absence of such reductions suggests that the marginal cost schedule remained largely unchanged. Instead, the expansion appears to be driven by the strategic supply response raising effective revenue, which allowed agencies to profitably serve marginal patients—those who were costlier to reach or more complex to treat—who may have been unprofitable at the lower, pre-privatization effective reimbursement rate.

Overall, the evidence contrasts with the standard "cream-skimming" or "cherry-picking" prediction of the HSV framework, in which private providers seek to lower costs or reduce access. Instead, privatization coincided with expansion in care, including toward patients who were more clinically complex and more geographically costly to serve, without evidence of reduced access or systematic reductions in care for persistently low-margin patients. Growth was largest in more profitable urban markets, but with no reduction in access for less profitable rural or high-cost populations. These findings are consistent with expansion driven by movement along an upward-sloping marginal cost curve, enabled by the strategic supply response raising effective reimbursement through billing effort, rather than by an efficiency-driven reduction in (variable) costs.

5.5 Spillover Effects on Non-Acquired Agencies

Privatization may affect not only directly acquired agencies but also the non-acquired agencies operating in the same markets. In this section, I examine whether non-Kindred agencies in Arkansas, referred to here as spillover agencies, adjusted their service patterns following the 2016 ownership change. I estimate spillover effects using agency-level difference-in-differences regressions comparing non-Kindred agencies in Arkansas to agencies in control states.

Table A.4 presents baseline estimates pooling across ownership types. On average, spillover agencies reduced aide visits by 0.50 and increased therapy visits by 0.29 per episode, though only the decline in aide visits is statistically significant. Nurse visits rose modestly (+0.25, p=0.088p = 0.088 p=0.088), while total visits and episode payments remained essentially unchanged. These shifts are small relative to those observed at Kindred-acquired agencies (Table A.3), and the absence of a payment response suggests limited behavioral adjustment overall.

Because nonprofit and for-profit agencies may differ in their objective functions and responsiveness to different factors, I estimate the spillover analysis separately by ownership type, comparing spillover agencies in Arkansas to ownership-matched agencies in non-privatizing states. Table A.10 reports results separately for for-profit and nonprofit spillover providers. Nonprofit agencies exhibit no measurable response along any dimension. For-profit agencies, by contrast, show modest but statistically significant increases in therapy and nursing visits, as well as a small rise in total visits per episode, though again with no corresponding increase in episode payments.

This pattern is consistent with the broader mechanism documented in this paper. The primary channel through which privatization affected provider behavior was reimbursement optimization—adjusting service mix and documentation to raise the effective price per episode—rather than cost reduction or quality competition linked to patient demand. Because this channel does not operate through price or quality signals that competitors directly observe and respond to, it generates limited competitive pressure on incumbent providers. The muted and ownership-heterogeneous spillover response is therefore what the billing effort framework would predict: for-profit agencies, more attuned to payment incentives, made small adjustments at the margin, while nonprofits—less sensitive to financial incentives—did not respond at all.

Taken together, these findings suggest that the public agency was not functioning as a disciplining force on private competitors prior to privatization—a point taken up further in the Discussion, where I also consider the role of CON laws in limiting competitive responses. The transition reshaped billing incentives at directly acquired agencies without meaningfully altering the strategic environment faced by incumbents.

5.6 Patient Outcomes and Value of Care

This section examines whether the post-privatization increases in service intensity and access documented above were accompanied by improvements in patient outcomes. While earlier results show substantial changes in treatment patterns and Medicare spending, the welfare implications of these changes depend on whether additional care generated observable benefits in health or reduced downstream utilization.

I examine outcomes across five domains: mortality, hospitalization, reported quality and process measures, patient experience, and substitution away from institutional care. Mortality and hospitalization outcomes are estimated using provider-level difference-in-differences models comparing treated agencies to control providers. Market-level substitution outcomes are evaluated using state-level difference-in-differences designs, supplemented by synthetic control estimates.

Table 2 reports difference-in-differences estimates for mortality and hospitalization during and following home health episodes. Estimated effects on mortality during an episode and within 30 days of discharge are small and statistically indistinguishable from zero. At the 95 percent confidence level, the estimates rule out reductions larger than approximately 0.4 percentage points for in-episode mortality and 1.1 percentage points for 30-day post-discharge mortality, while remaining consistent with smaller effects in either direction.

Hospitalization outcomes exhibit a similar pattern. Estimated effects on hospital admissions during and within 30 days of a home health episode are negative but imprecisely estimated. The associated confidence intervals rule out reductions exceeding approximately 2.0 percentage points but allow for more modest changes. Taken together, these results indicate that the post-privatization increases in service intensity were not accompanied by large or precisely estimated improvements in survival or reductions in hospital use.

Event-study estimates for mortality and hospitalization are reported in the Appendix (Figures A.17 and A.18). These plots allow for an explicit assessment of potential short-run disruptions around the privatization transition. While there is some volatility in these outcomes over time, and especailly during the transition period, there is no systematic divergence between treated and control agencies following privatization. Post-period coefficients fluctuate around zero, and confidence intervals consistently span the null in the post-period, indicating no evidence of large or sustained declines. There is some evidence of reduced mortality during the transition period, but this could reflect reduced patient volume and different patient composition among the the beneficiaries who remained in care during the transition.

In contrast, reported quality measures increase sharply following privatization. Panel (a) of Figure 11 shows a large and immediate rise in Home Health Compare star ratings for Kindred-acquired agencies, while ratings for spillover and control agencies remain flat. Importantly, these star ratings are constructed primarily from agency-reported process and functional assessment data. As a result, they may be sensitive to changes in coding, documentation practices, or reporting effort, and may not fully reflect changes in underlying clinical quality.

Panel (b) of Figure 11 shows a similarly pronounced increase in timely initiation of care. In contrast to star ratings, this measure is derived from claims data and captures the share of episodes in which home health services begin promptly (within 2 days) following referral or discharge. As such, it is more likely to reflect genuine improvements in agency operations. The increase in timely initiation of care therefore suggests shorter wait times and more rapid access to post-acute services, consistent with improved scheduling and intake performance following privatization.

Despite these improvements in reported and process-based measures, there is no corresponding change in patient-reported experience or clinical outcomes. Appendix Figure A.19 shows no detectable change in HHCAHPS satisfaction scores, and larges changes in mortality and hospitalization outcomes can be ruled out as documented above. Taken together, these patterns indicate that while privatization was associated with improvements in observable operational performance and reported quality metrics, these changes did not translate into measurable gains in patient health or experience.

Finally, I examine whether expanded home health use substituted for institutional post-acute care. To do so, I look at skilled nursing facility use and nursing home use per 1000 beneficiaries in Arkansas relative to control states using the state-level difference-in-differences design. Difference-in-differences estimates in Table 3 show small and imprecise changes in skilled nursing facility and nursing home use. The confidence intervals rule out large shifts, exceeding roughly 5–8 users per 1,000 beneficiaries, but allow for smaller changes. Synthetic control estimates in Figure A.20 corroborate these findings. Nursing home use in Arkansas tracks the synthetic control closely throughout the entire sample period, with no visible divergence following privatization. Skilled nursing facility use tells a similar story: rather than Arkansas departing from its pre-period trend, it is the synthetic control that rises sharply in 2016 before converging back toward Arkansas by 2017,

leaving no clear evidence of a differential effect on SNF utilization attributable to the privatization. Because Kindred did not own institutional post-acute facilities in Arkansas (Appendix C), these patterns are unlikely to reflect vertical referral incentives, and together suggest that the expansion in home health use did not come at the expense of—or complement—institutional post-acute care in a detectable way.

Overall, privatization was not accompanied by measurable improvements in clinical outcomes, substitution away from institutional care, or patient-reported experience, despite substantial increases in Medicare spending. Treated agencies delivered more therapy, documented greater patient complexity, and expanded access, but these changes were not matched by observable gains in survival or reductions in hospitalization. Taken together, the findings suggest that privatization raised the quantity and cost of care without clear evidence of corresponding improvements in health. While modest benefits cannot be ruled out, the results indicate that average outcome changes in the post-privatization period were limited relative to the growth in spending.

6 Discussion

This paper examines how privatization affects provider behavior when reimbursement is endogenous to coding and service delivery decisions. Using Arkansas's 2016 sale of its statewide public home health network to a for-profit chain, I show that privatization increased effective reimbursement per episode by 27 percent and expanded statewide home health use by roughly 10 percent, with spending rising substantially but measurable quality and outcomes remaining flat. These findings reveal a distinctive mechanism through which ownership changes affect healthcare markets: rather than cutting costs or reducing access, privatization led to billing optimization, allowing providers to raise the effective reimbursement received per episode of care and expand treatment along an upward-sloping cost curve.

6.1 Privatization Through a Strategic Supply Response Rather Than Cost-Cutting

A central motivation for privatization across sectors is the expectation that private firms will deliver services more efficiently than public agencies. However, the canonical framework for understanding public versus private provision—articulated by Hart et al. (1997) and others—emphasizes a fundamental tradeoff: while private ownership may improve productive efficiency, profit motives can also induce cost-cutting that degrades non-contractible quality, reduces access to unprofitable populations, or leads to cream-skimming (Shleifer, 1998; Duggan, 2000; Megginson and Netter, 2001). The first-order concern in this literature is *under-provision*—too little care or too low quality, particularly for high-cost patients.

Healthcare settings, however, may present a different problem. Under prospective payment

systems where reimbursement can be influenced through coding, documentation, and service mix decisions, the dominant margin of response may be *over-provision*: higher spending driven by billing optimization rather than by increased value to patients (Silverman and Skinner, 2004; Dafny, 2005; Bowblis and Brunt, 2014; Geruso and Layton, 2020). Providers can increase profits not by cutting costs, but by raising their effective reimbursement through strategic responses to payment formulas. This mechanism has been documented across healthcare settings, including upcoding patient severity (Geruso and Layton, 2020; Sacarny, 2018), bunching at payment thresholds (McKnight, 2006), and adjusting service mix toward high-reimbursement inputs (Silverman and Skinner, 2004; Gupta et al., 2024). Importantly, for-profit and private equity–owned providers appear systematically more responsive to these financial incentives (Dafny, 2005; Kim and Norton, 2015, 2017; Gupta et al., 2021; Rahman, 2025).

The home health sector exemplifies these dynamics. Since Medicare lifted restrictions on for-profit participation in 1980, the industry has shifted dramatically toward proprietary ownership, raising persistent concerns about fraud and overuse (O'Malley et al., 2023; Medicare Payment Advisory Commission, 2011, 2023). Prior research shows that agencies bunch therapy visits at reimbursement thresholds (McKnight, 2006) and that for-profit agencies deliver more therapy and adjust episode characteristics more aggressively than nonprofits (Kim and Norton, 2015, 2017). Yet most existing evidence relies on cross-sectional comparisons or potentially endogenous ownership transitions (Zhang et al., 2024), leaving open questions about the causal effects of privatization in this setting.

This paper provides the first causal evidence on home health privatization by exploiting Arkansas's sudden, comprehensive sale of its statewide public network. The results strongly support a strategic supply response mechanism—distinct from traditional privatization channels of cost-cutting or cream-skimming. Following the ownership transition, treated agencies increased therapy visits by more than 100 percent while reducing aide visits by roughly 90 percent—a service mix reallocation that raised episode payments by $490 (27 percent). Agencies also documented greater patient complexity, with ADL scores rising 12 percent, far exceeding the modest increases in coding-insensitive comorbidity measures derived from prior-year claims.

Crucially, these patterns persist when conditioning on stable patient populations, diagnosis groups, or observable patient characteristics, indicating genuine behavioral change rather than patient selection alone. The timing and magnitude of changes in agency-reported measures (ADLs, service mix) substantially exceeded shifts in independently-coded indicators (prior claims-based comorbidities), pointing to strategic coding, documentation, and service mix decisions as the dominant mechanism.

Importantly, this billing effort operated through a fundamentally different margin than the cost-cutting channel emphasized in Hart et al. (1997). Cost report data show that per-visit variable costs exhibited no sustained decline post-privatization, remaining near pre-transition levels after accounting for temporary disruption during ownership transfer. Had privatization generated pro-

ductive efficiency gains—through improved logistics, staffing optimization, or reduced bureaucratic constraints—we would expect persistent reductions in production costs per unit of service. Instead, the flat variable cost trajectory combined with sharply higher effective prices indicates that Kindred invested resources in *revenue optimization* rather than *cost reduction*. While administrative restructuring may have yielded fixed cost savings, the marginal treatment decision appears driven by higher reimbursement enabling expansion along an unchanged marginal cost curve, not by a downward shift in that curve.

This finding contributes to ongoing debates about the mechanisms through which ownership affects provider behavior in healthcare. When payment is endogenous to provider documentation and coding decisions, privatization may reshape incentives not toward efficiency or quality competition, but toward strategic engagement with payment formulas. The result is higher spending without corresponding improvements in measured outcomes—a pattern that raises distinct welfare concerns from the traditional under-provision narrative.

Importantly, the strategic supply response documented here may be the most relevant channel through which ownership change matters across many healthcare settings. Recent work on hospital privatization shows similar patterns of billing optimization and service mix changes without corresponding quality improvements (Gupta et al., 2024). In Medicare Advantage, private insurers strategically upcode enrollee diagnoses to increase capitation payments (Geruso and Layton, 2020). In Medicaid managed care, plans engage in coding intensity adjustments and service utilization management that raise revenues without improving health outcomes (Layton et al., 2022). Across these contexts, endogenous payment systems create scope for strategic responses that may dominate the efficiency and quality margins emphasized in traditional privatization frameworks. This paper provides causal evidence that strategic supply responses operate powerfully in home health, suggesting they may be first-order considerations in evaluating ownership transitions throughout the healthcare sector.

6.2 Market Structure, Regulation, and Limited Competition

The modest spillover effects documented in Section 5.5 reflect two key features of this market: regulatory barriers to entry and limited competitive dynamics along traditional margins. Together, these characteristics help explain why the elimination of Arkansas's public home health network did not meaningfully alter incumbent provider behavior.

Arkansas's restrictive certificate-of-need laws shaped both the pre-existing market structure and the post-privatization dynamics in ways that are relevant for interpreting the results. Under Arkansas law, new home health agencies or service area expansions required state approval demonstrating unmet need—a threshold rarely satisfied given existing provider density, making new entry by private competitors difficult in many of the markets ADH served. The public network may therefore have been reaching populations that private agencies would have found difficult to serve under the existing regulatory environment, though the extent to which CON was the binding con-

straint, as opposed to underlying demand or cost conditions in rural areas, is difficult to establish definitively. What the regulatory environment does suggest more clearly is that the counterfactual to privatization was unlikely to have been seamless competitive entry by private agencies filling the gap. The 10 percent increase in statewide home health use documented in Section 5.2 is consistent with latent demand—patients who were clinically eligible but whom the public network either could not serve efficiently or private agencies could not legally reach. Alternatively, this expansion could reflect induced demand, where providers increased utilization beyond what patients valued. The results do not allow for a full disentangling of these mechanisms.

Beyond regulatory constraints, the structure of competition in home health fundamentally differs from markets where a public option might discipline private providers. First, home health agencies do not compete on price. Medicare sets reimbursement rates administratively, and beneficiaries face zero cost-sharing for home health services. This eliminates the primary channel through which a low-cost public provider might exert competitive pressure on private agencies.

Second, quality competition appears limited in this setting. Research on the home health star ratings program finds that higher-rated agencies gain essentially no market share relative to lower-rated ones, suggesting weak linkages between publicly reported quality metrics and patient or referrer choice (Li, 2024). This is consistent with the patterns observed here: star ratings for acquired agencies increased sharply post-privatization (Figure 11), yet patient experience scores (HHCAHPS) remained flat and clinical outcomes showed no improvement. The disconnect between reported quality metrics and substantive care quality, combined with evidence that patients do not strongly sort on observable ratings, suggests that quality competition was not a meaningful disciplining force on provider behavior in this market—undermining quality as a competitive margin even where public information was available.

6.3 Welfare Implications

The welfare assessment is ambiguous. Privatization expanded access to home health services—a 10 percent increase in statewide users with no evidence of reduced service to high-cost or rural populations. For marginal patients who gained access, the value may be substantial, particularly if home health substitutes for institutional care or improves health trajectories not captured in short-term outcomes.

However, Medicare spending increased substantially without detectable improvements in measurable quality or outcomes. The decomposition in the Appendix shows roughly 70 percent of increased spending flowed to patients who would have received care anyway. To the extent that outcomes for inframarginal patients were unchanged—consistent with the flat mortality and hospitalization estimates, which capture averages across both inframarginal and marginal patients—this spending represents transfers to providers without commensurate clinical benefit. Modest benefits for inframarginal patients cannot be ruled out, but the absence of detectable outcome improvements at the agency level is inconsistent with large gains.

This welfare ambiguity reflects the endogenous reimbursement structure: billing effort simultaneously raises payments and enables expansion. Whether marginal patients gained sufficient value depends on parameters difficult to estimate. However, two considerations suggest caution. First, clinical outcomes and patient experience measures showed no improvement despite 27 percent higher payments. While star ratings increased sharply, these gains likely reflect documentation improvements rather than substantive care quality changes—mortality, hospitalization, and HHCAHPS scores all remained flat. Second, the home health sector has been repeatedly flagged for overuse and fraud, particularly among for-profit providers, suggesting marginal expansion may push into regions of low or negative value.

6.4 Implications for Payment System Design

These findings have direct implications for Medicare payment reform. The case-mix adjustment system was designed to pay fairly for patient heterogeneity while constraining spending through fixed episode rates. This analysis reveals a key vulnerability: when providers can influence reimbursement through coding and documentation, and adjust service intensity to reach payment thresholds, "prospective" payment becomes partially illusory.

The results suggest ownership matters primarily through differential capacity to mount a strategic supply response, rather than through cost efficiency or quality differences. If these investments shift effective payment without corresponding patient value, they represent regulatory arbitrage undermining payment reform goals.

This dynamic is particularly relevant for the 2020 Patient-Driven Groupings Model reform, which eliminated therapy thresholds and moved the sector closer to a pure episode-based payment. While removing these volume incentives was designed to curb the 'revenue maximization' behaviors documented in this study, it also removes a key lever through which private agencies could exert effort to increase their revenues. By closing the channel for billing effort, the reform may inadvertently activate or increase the salience of the classic Hart-Shleifer-Vishny trade-off: when revenue is fixed, the primary lever to increase profit becomes cost containment. Thus, while the shift to strict episode-based payments may successfully reduce wasteful overprovision, it creates a new risk of underprovision, specifically, 'skimping' on unobserved quality or reducing access for higher-cost populations that the previous, gameable system inadvertently discouraged by rewarding higher service intensity.

6.5 Generalizability and Limitations

Several features of this setting deserve emphasis when considering the external validity of these findings. First, the Arkansas privatization was both comprehensive and sudden—the state sold its entire network to a single operator in a matter of months. This may have enabled larger or faster organizational changes than would occur through gradual entry or exit in a typical market.

Second, the buyer was a large, sophisticated national chain with established systems for billing optimization and operational management. Results might differ if the acquirer had been a smaller regional operator or a nonprofit agency with different objectives.

Third, the outcomes examined focus on short-term quality metrics and utilization patterns; longer-term effects on patient health trajectories, caregiver burden, or institutional care use may differ from the patterns observed here.

Fourth, the analysis of agency cost and payer mix outside of FFS Medicare relies on Medicare Cost Reports, which differ in data quality from administrative claims. Unlike claims data, which are generated systematically during payment processing, cost reports are self-reported financial disclosures filled out by the agencies themselves. Consequently, these data may be subject to inconsistent coding, human error, or variations in accounting practices across firms. While cost reports represent the most comprehensive publicly available source for observing the internal cost side of agency operations, estimates derived from them, particularly regarding per-visit variable costs, should be interpreted with caution given the potential for reporting noise and documentation bias.

Fifth, the CON environment limited competitive responses. In less regulated markets, privatization might generate different dynamics.

Finally, this study examines privatization within a public system that was likely operating under institutional constraints, state employment rules, procurement regulations, and budget limitations. The counterfactual is not an idealized public provider but rather a constrained public system. Whether alternative reforms to public provision might have achieved similar or better outcomes remains an open question.

7 Conclusion

This paper demonstrates that privatization can reshape healthcare delivery through a mechanism distinct from traditional concerns about cost-cutting or cream-skimming. When payment systems allow providers to influence effective reimbursement through coding and service mix decisions, privatization may redirect effort toward billing optimization rather than cost reduction or quality improvement. In the Arkansas home health care setting, this translated into dramatic service composition shifts, intensified documentation of patient complexity, substantially higher payments, expanded utilization, and increased Medicare spending—with no detectable improvement in clinical outcomes or patient-reported experience.

A stated objective of the transaction was to improve the financial sustainability of services the state could no longer efficiently operate, and by this measure, privatization succeeded. Kindred substantially increased revenue per episode, transforming a network that operated at reimbursement rates well below the national average into an ostensibly profitable enterprise. But the mechanism was revenue optimization rather than cost reduction, and the fiscal beneficiary was the operator

rather than the public: Medicare spending rose substantially while measured outcomes remained flat, suggesting the profitability gains may have largely represented a transfer from the federal government to the private provider.

The normative implications remain complex. Privatization expanded access for thousands of beneficiaries with no evidence of reduced service to high-cost or remote populations. Whether this represents efficient response to unmet need or demand inducement pushing into low-value regions cannot be definitively answered with available data. What is clear: expansion came at substantial fiscal cost, with roughly 70 percent of increased spending flowing to patients who would have received care anyway.

These findings contribute to several literatures. They add to research on healthcare privatization by identifying strategic supply responses, operating through billing effort, as a mechanism receiving limited attention relative to traditional access and quality concerns. They inform the upcoding and risk adjustment literature by showing how organizational form affects billing optimization intensity. They advance payment reform debates by revealing how prospective systems can become endogenous when providers influence classification and service intensity.

From a policy perspective, the results highlight tensions in payment system design. Features such as risk-adjustment or higher payments for higher service levels aim for fair payment across patient heterogeneity and access to services for higher-cost or higher-need populations, but create scope for actions that raise payments without necessarily improving outcomes.

Finally, the strategic supply response documented here likely generalizes to many settings in which payment is endogenous to provider behavior. Recent evidence suggests similar dynamics in hospital privatization (Gupta et al., 2024), Medicare Advantage (Geruso and Layton, 2020), and Medicaid managed care (Layton et al., 2022). Taken together, these findings suggest that billing optimization may be a first-order consideration in evaluating ownership transitions.

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8 Figures

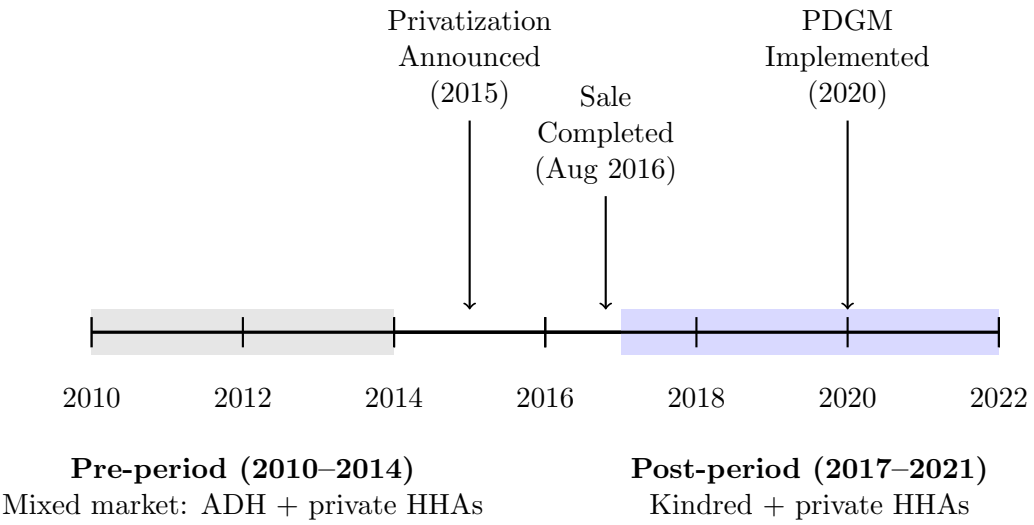
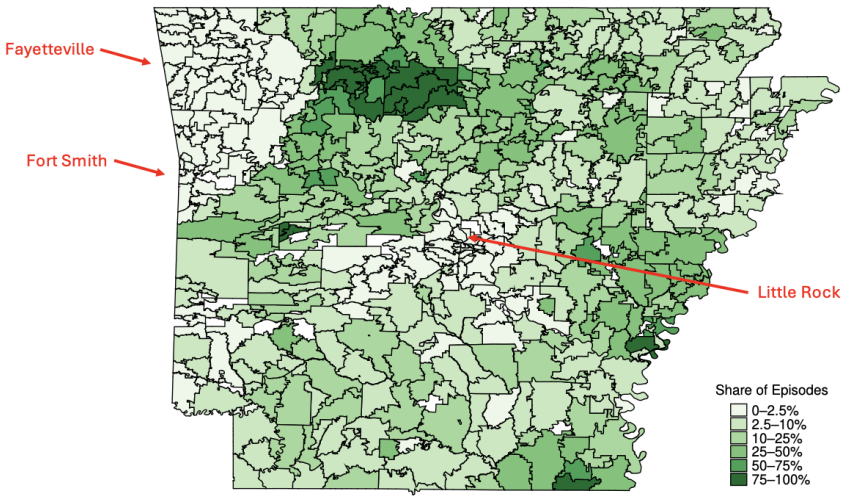
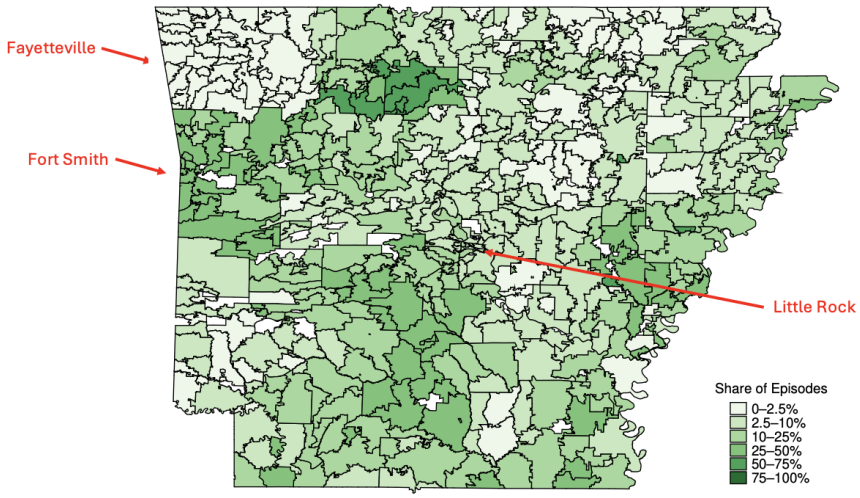


Figure 1: **Privatization and Policy Timeline.** Timeline of key events related to the Arkansas home health privatization. Shaded areas denote the pre-period (2010–2014) and post-period (2017–2022). Arrows indicate the announcement of privatization (2015), sale completion (2016), and the implementation of the PDGM payment model (2020).
Notes: The pre-period includes both public and private provision; the post-period reflects a transition to private and Kindred-operated agencies.

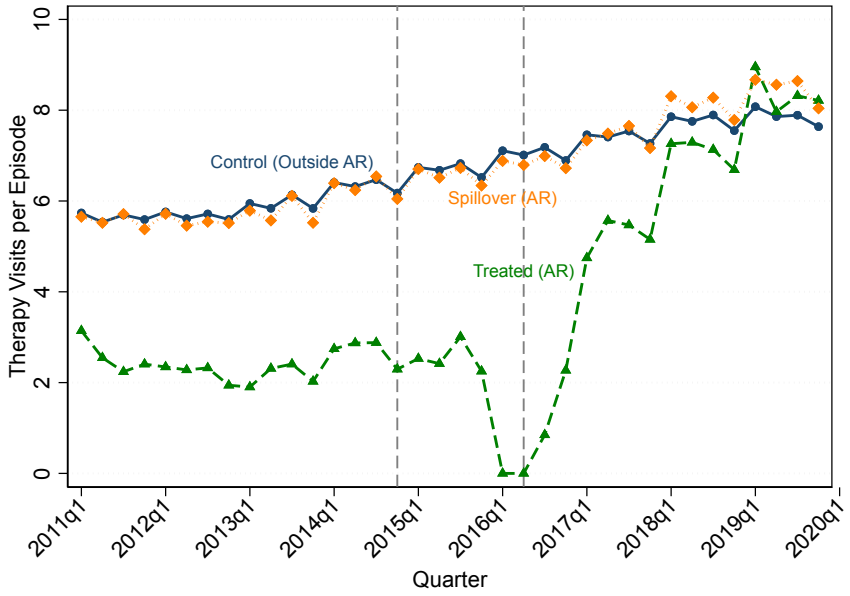


(a) ADH Market Share, 2010–2014

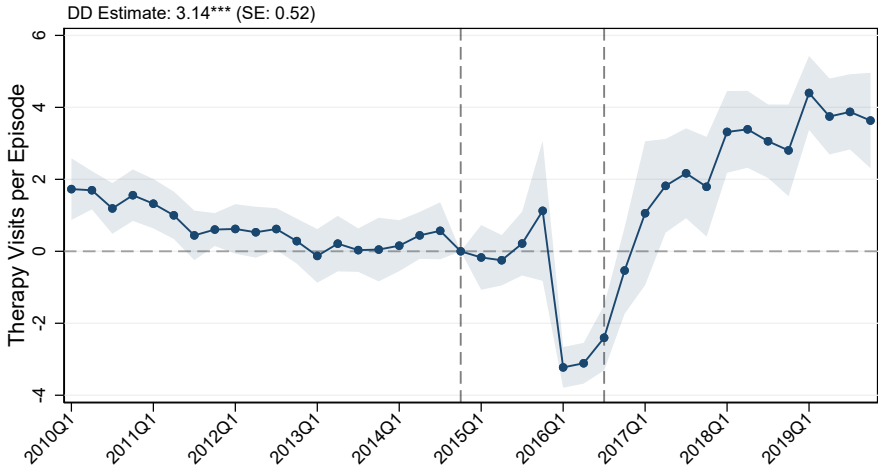


(b) Kindred Market Share, 2017–2021

Figure 2: **ADH and Kindred Market Share by ZIP Code, Before and After Privatization.** Panel (a) shows the share of Medicare home health episodes provided by the Arkansas Department of Health (ADH) before privatization (2010–2014). Panel (b) shows Kindred's market share after privatization (2017–2021). *Notes:* Shading represents the share of episodes attributable to ADH (2010–2014) or Kindred (2017–2021) within each ZIP code. Darker areas reflect higher market share. Red labels identify major metro areas. The maps illustrate that Kindred retained a broad rural footprint while expanding significantly into urban areas post-privatization.

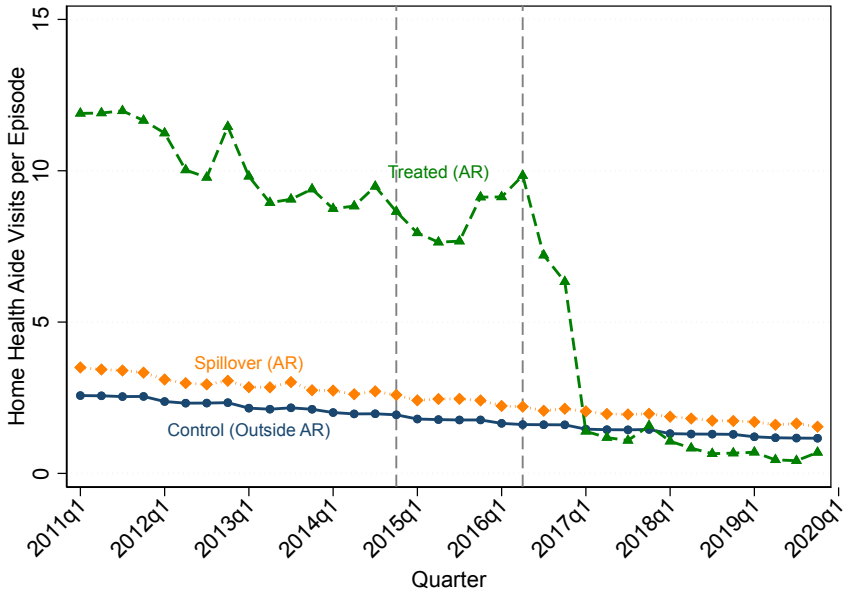


(a) Group-Level Means

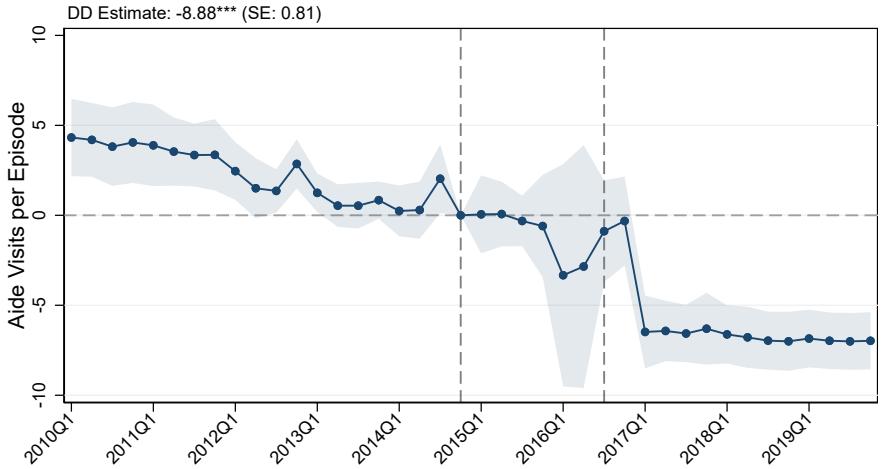


(b) Event Study Estimates

Figure 3: **Therapy Visits per Episode.** Panel (a) shows group-level averages of therapy visits per episode. Panel (b) presents event study estimates using quarterly data. *Notes:* Data are collapsed to the agency-quarter level and weighted by episodes. Shaded areas represent 95% confidence intervals. Estimates use provider-level difference-in-differences with year-quarter fixed effects. Vertical dashed lines denote the announcement (2014Q3) and implementation (2016Q3) of privatization.

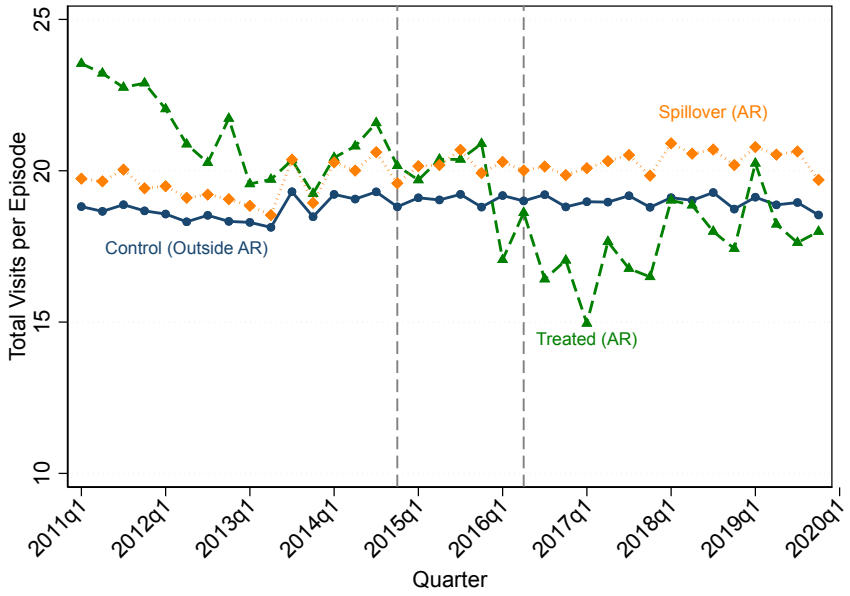


(a) Group-Level Means

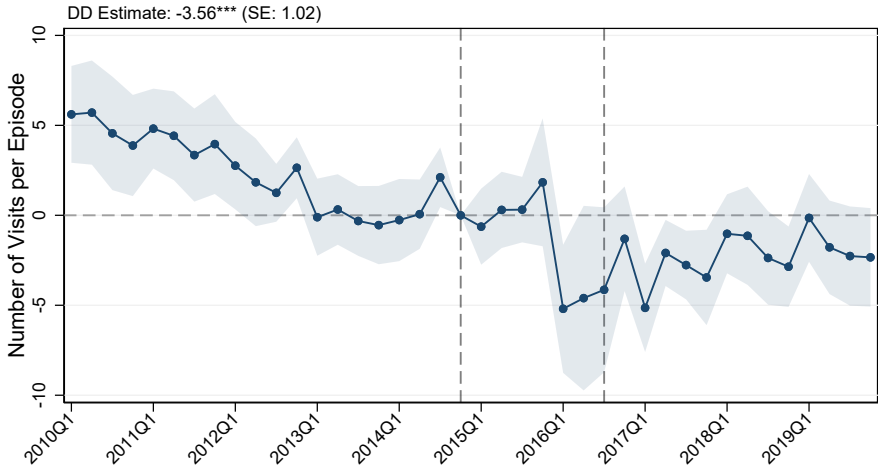


(b) Event Study Estimates

Figure 4: **Aide Visits per Episode.** Panel (a) shows group-level averages of aide visits per episode. Panel (b) presents event study estimates using quarterly data. *Notes:* Data are collapsed to the agency-quarter level and weighted by episodes. Shaded areas represent 95% confidence intervals. Estimates use provider-level difference-in-differences with year-quarter fixed effects. Vertical dashed lines denote the announcement (2014Q3) and implementation (2016Q3) of privatization.

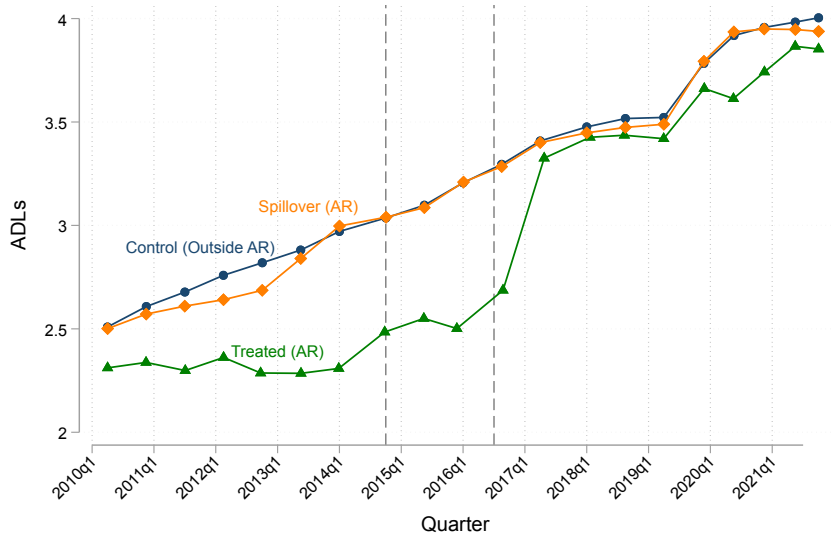


(a) Group-Level Means

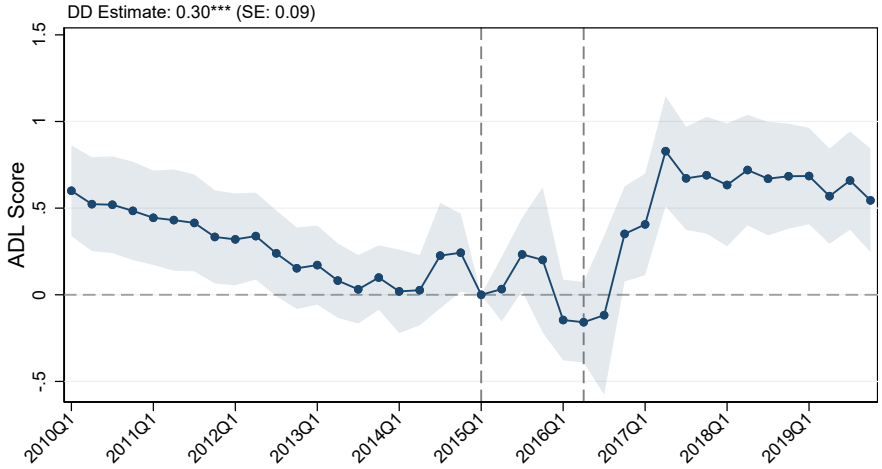


(b) Event Study Estimates

Figure 5: **Total Visits per Episode.** Panel (a) shows group-level averages of total home health visits per episode, including all visit types. Panel (b) presents event study estimates using quarterly data. *Notes:* Data are collapsed to the agency-quarter level and weighted by episodes. Shaded areas represent 95% confidence intervals. Estimates use provider-level difference-in-differences with year-quarter fixed effects. Vertical dashed lines denote the announcement (2014Q3) and implementation (2016Q3) of privatization.

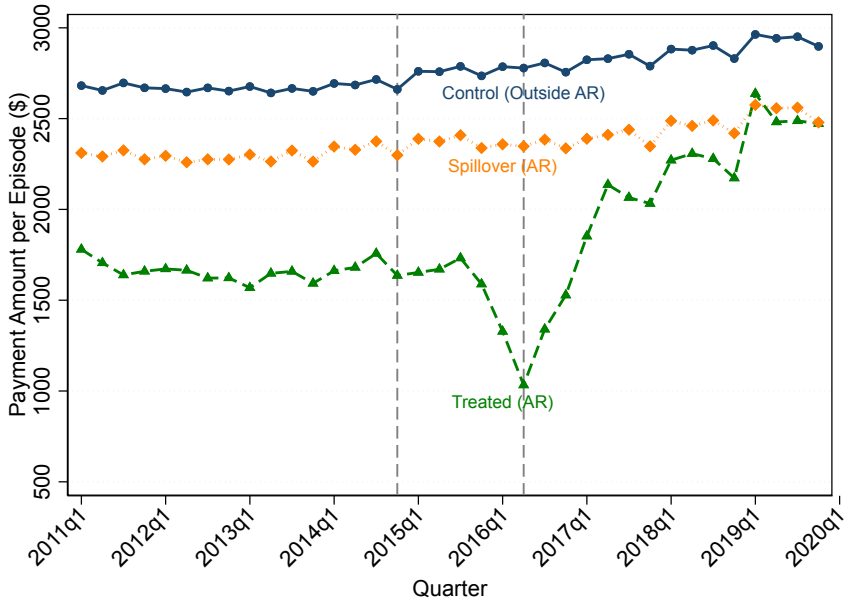


(a) Group-Level Means

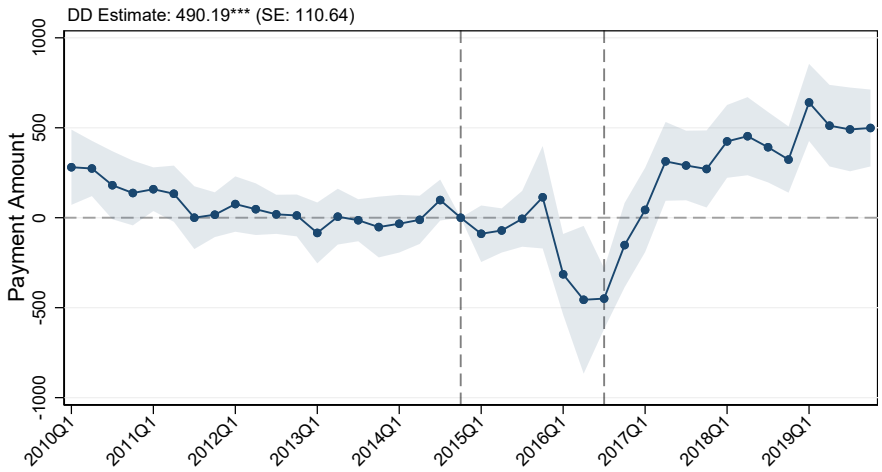


(b) Event Study Estimates

Figure 6: **Activities of Daily Living (ADLs).** Panel (a) shows group-level averages of coded limitations in ADLs per episode. Panel (b) presents event study estimates using quarterly data. *Notes:* In panel (a), the green line represents agencies formerly operated by the Arkansas Department of Health and acquired by Kindred, the orange line shows other agencies operating in Arkansas (spillovers), and the navy line shows agencies in control states. Data are collapsed to the agency-quarter level and weighted by episodes. Shaded areas in panel (b) represent 95% confidence intervals. Estimates use provider-level difference-in-differences with year-quarter fixed effects. Vertical dashed lines denote the announcement (2014Q3) and implementation (2016Q3) of privatization.



(a) Group-Level Means



(b) Event Study Estimates

Figure 7: **Average Episode Payments.** Panel (a) shows averages of payment per home health episode for ADH/Kindred home health agencies in Arkansas, spillover home health agencies in Arkansas, and home health agencies in control states. Panel (b) presents event study estimates. *Notes:* Data are collapsed to the agency-quarter level and weighted by episodes. Shaded areas represent 95% confidence intervals. Estimates use provider-level difference-in-differences with year-quarter fixed effects. Vertical dashed lines denote the announcement (2014Q3) and implementation (2016Q3) of privatization.

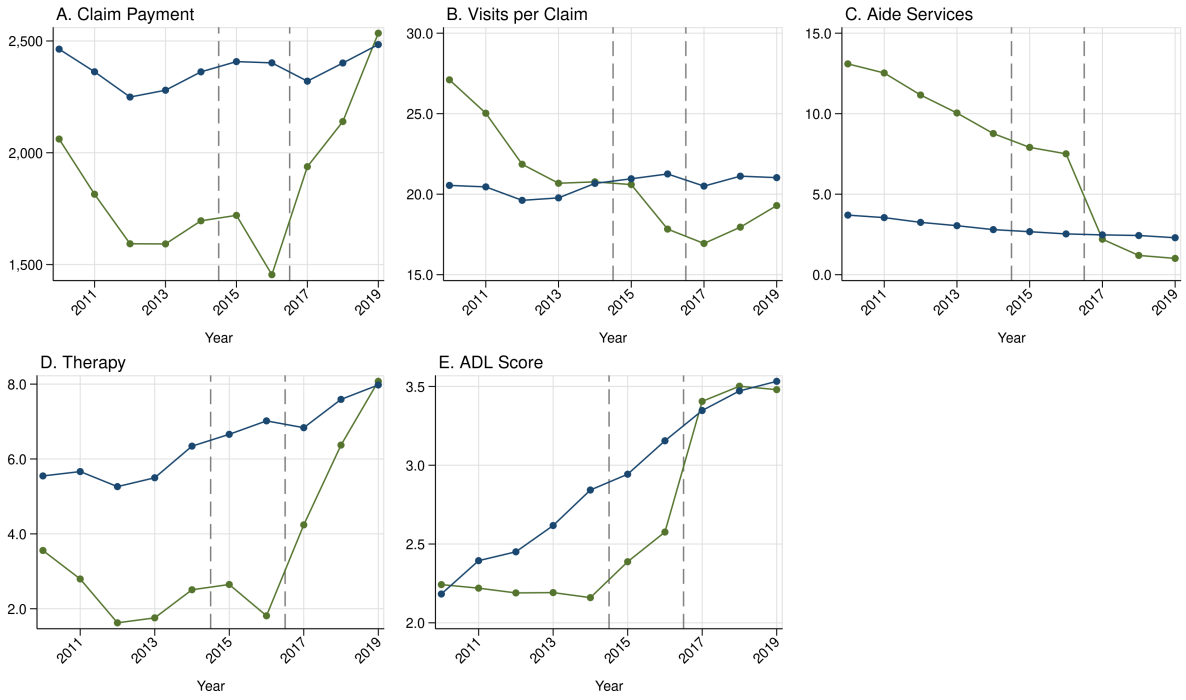
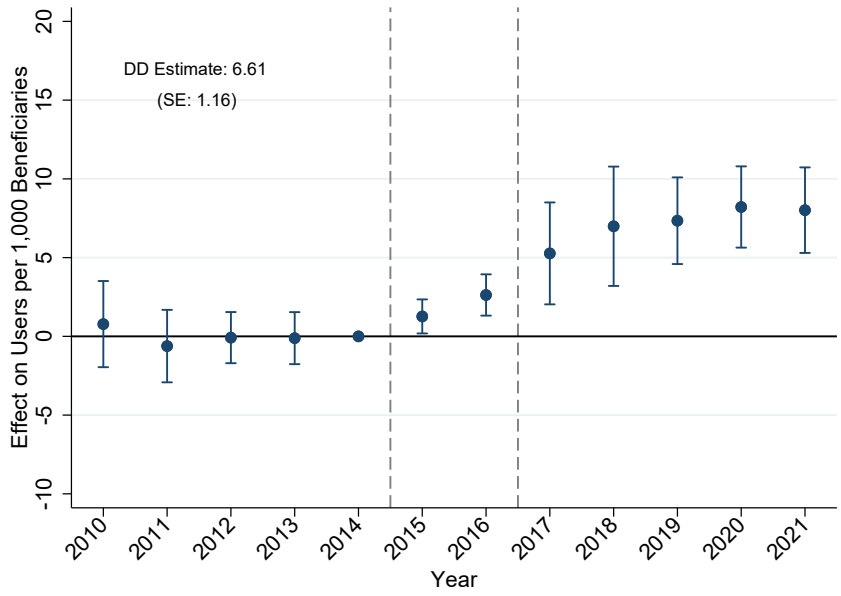
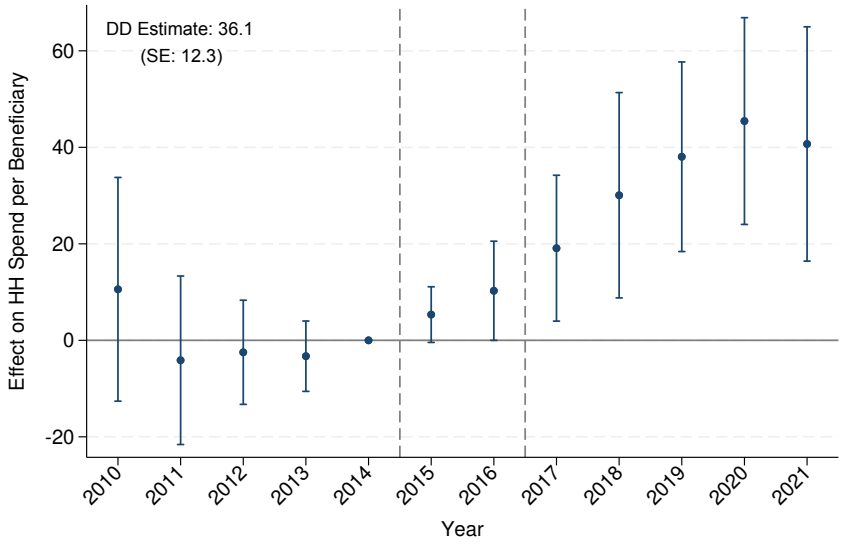


Figure 8: **Same Beneficiaries Over Time.** This figure shows average outcomes for a panel of beneficiaries observed both before and after privatization. *Notes:* In all panels, the green line represents agencies formerly operated by the Arkansas Department of Health and acquired by Kindred and the navy line shows other agencies in Arkansas (spillovers). The sample is restricted to beneficiaries with claims in both pre- and post-periods to hold patient composition constant. Data are collapsed to the beneficiary-quarter level and weighted by the number of home health episodes. Vertical dashed lines denote the announcement (2014Q3) and implementation (2016Q3) of privatization.

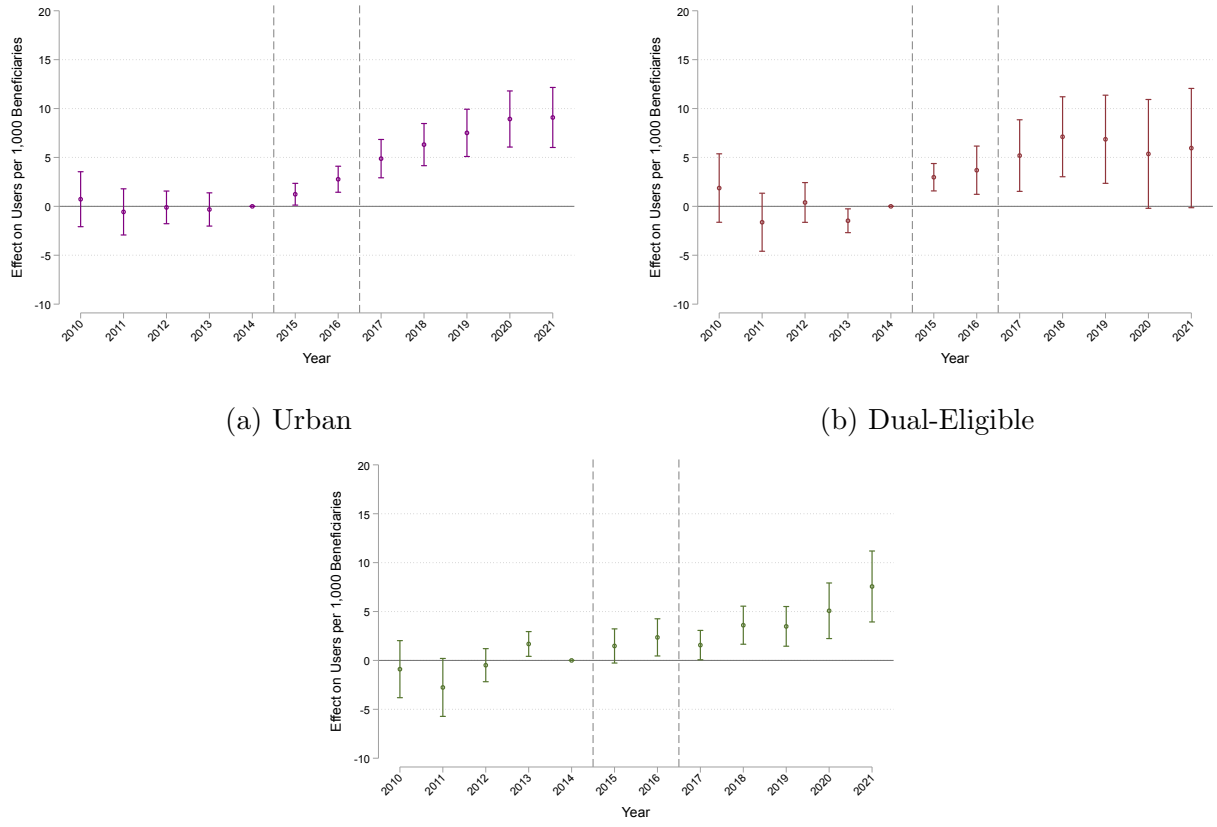


(a) Home Health Users per 1,000 Beneficiaries



(b) Spending per Beneficiary

Figure 9: **Extensive Margin: Users and Spending.** Event study plots showing changes in (a) the number of home health users per 1,000 Medicare beneficiaries, and (b) Medicare home health spending per beneficiary. *Notes:* Vertical dashed lines indicate the timing of privatization announcement and implementation.

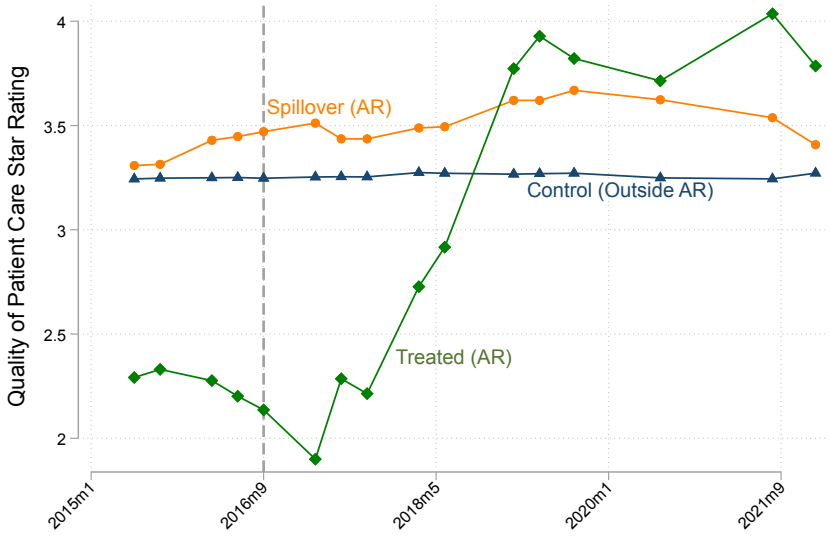


(a) Urban

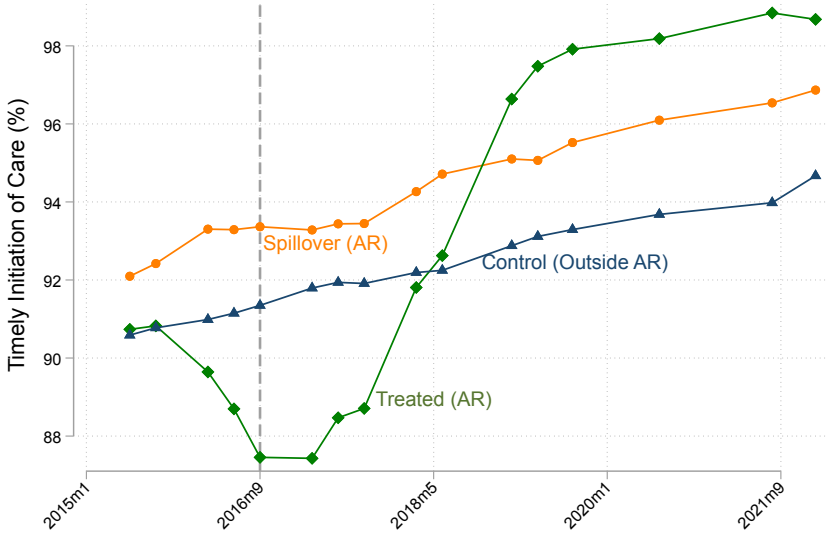
(b) Dual-Eligible

(c) Rural

Figure 10: **Extensive Margin: Heterogeneity.** Event study estimates showing the effect of privatization on the number of home health users per 1,000 Medicare beneficiaries, separately for urban (a), dual-eligible (b), and rural (c) subgroups. *Notes:* Estimates are based on difference-in-differences models comparing Arkansas to control states. Shaded areas represent 95% confidence intervals. Vertical dashed lines indicate the timing just before the privatization announcement (2014.5) and implementation (2016.5).



(a) Overall Star Rating



(b) Timely Initiation of Care

Figure 11: **Home Health Agency Quality Measures.** Panel (a) plots average overall star ratings from Medicare's HH Compare system. Panel (b) shows the share of home health episodes in which care was initiated within the CMS-defined timely window. *Notes:* In all panels, the green line represents agencies formerly operated by the Arkansas Department of Health and acquired by Kindred, the orange line shows other agencies in Arkansas (spillovers), and the navy line shows agencies in control states. Data are collapsed to the agency-quarter level. Panel (b) is weighted by the number of episodes. Vertical dashed line denotes the implementation (2016Q3) of privatization.

9 Tables

Table 1: Episode-Weighted Summary Statistics by Treatment Group, 2010-2014

	Treatment Group		
Variable	Gov't → Kindred	Spillover (AR)	Control (non-AR)
Patient Demographics			
Age	75.89 (3.74)	76.65 (1.95)	76.26 (3.53)
Share Age <65	0.16 (0.10)	0.14 (0.04)	0.15 (0.08)
Share Age 85+	0.24 (0.10)	0.24 (0.05)	0.25 (0.09)
Female (%)	0.65 (0.10)	0.64 (0.04)	0.62 (0.06)
Number of Chronic Conditions	5.27 (0.69)	5.69 (0.46)	6.05 (0.74)
Cognitive Function	0.44 (0.22)	0.63 (0.24)	0.71 (0.28)
Memory Deficit	0.09 (0.08)	0.16 (0.08)	0.18 (0.12)
Elixhauser Comorbidity	3.50 (0.69)	3.50 (0.51)	3.32 (0.64)
Share Black	0.13 (0.18)	0.11 (0.13)	0.16 (0.19)
Share Dual Eligible	0.49 (0.12)	0.34 (0.10)	0.38 (0.21)
Location Characteristics			
Rural (RUCA)	0.25 (0.32)	0.12 (0.19)	0.04 (0.10)
Referral Sources			
Share Community	0.50 (0.13)	0.45 (0.11)	0.52 (0.20)
Share Institutional	0.50 (0.13)	0.55 (0.11)	0.48 (0.20)
Service Utilization			
Claim Payment Amount ($)	1,786.71 (359.01)	2,340.41 (386.48)	2,719.89 (551.86)
Therapy Per Episode	2.69 (1.61)	5.80 (2.31)	5.86 (2.76)
Total Visits per Episode	22.19 (5.44)	19.73 (2.91)	19.03 (5.69)
Aide Visits Per Episode	10.26 (4.70)	3.18 (2.25)	2.40 (2.72)
Nurse Visits Per Episode	7.67 (2.52)	8.78 (1.36)	9.00 (4.04)
Total Episodes	35,545	316,595	33,660,069

Note: Table presents means with standard deviations in parentheses. Statistics weighted by number of episodes. Data pooled across years 2010-2014. Treatment groups: (1) Government-owned HHAs acquired by Kindred; (2) Other HHAs in Arkansas (spillover effect); (0) HHAs outside Arkansas (control group).

Table 2: Effect of Privatization on Mortality and Hospitalization Outcomes

	Death 30d Post (1)	**Death During HH** (2)	**Hosp During HH** (3)	**Hosp 30d Post** (4)
Treated × Post	−0.0040	−0.0013	−0.0086	−0.0090
	(0.0037)	(0.0015)	(0.0060)	(0.0058)
Mean of Dep Var	0.0310	0.0073	0.1152	0.1033
Observations (Agency-Quarters)	389,919	389,919	389,919	389,919
Year-Quarter FE	Yes	Yes	Yes	Yes

Notes: Each column reports the coefficient on the interaction term between treated agencies and the post-privatization period from a separate regression. Outcomes include mortality and hospitalization during or within 30 days of home health care. All models include year-quarter fixed effects and cluster standard errors at the provider level.
Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Skilled Nursing Facility and Nursing Home Use

	SNF Users per 1,000 Hospitalized Benes	**NH Users per 1,000 Benes**
Post × Arkansas	3.57	1.04
	(2.36)	(3.61)
Baseline Mean	132.22	86.67
Observations	540	540
Clusters (States)	46	46
State FE	Yes	Yes
Year FE	Yes	Yes

Notes: Each column reports the coefficient on the interaction between a post-treatment indicator and an Arkansas indicator from the state-level difference-in-differences model. All regressions include state and year fixed effects. Standard errors clustered at the state level are shown in parentheses. Baseline means reflect pre-treatment averages in Arkansas within the subgroup. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendices

A Appendix Tables

Table A.1: States with Publicly Operated Home Health Agencies During the Study Period (2010 - 2021)

State	Provision Type	Status in Analysis	Notes
Oklahoma	County/state HHAs via health departments	Excluded	Public HHAs phased out by early 2000s after post-BBA funding cuts.
Georgia	Regional and county HHAs	Excluded	Health districts exited home health by mid-2010s; services transitioned to private agencies.
South Carolina	State-operated HHA program (DHEC)	Excluded	DHEC sold its home health licenses to Kindred in 2016, ending public provision.
Kentucky	County-operated HHAs	Excluded	County health departments sold or closed HHAs from 2015–2020 due to budget and staffing constraints.
North Carolina	County-operated HHAs	Excluded	Most county HHAs privatized or shut down by 2020 amid rising competition and financial strain.
Idaho	Regional public health district HHAs	Excluded	Last remaining public HHA (Panhandle Health) closed in 2022 due to workforce shortages.
Alabama	Statewide program (ADPH)	Included	ADPH continued to operate a Medicare-certified HHA serving all 67 counties throughout the study period.
Arkansas	State-operated HHAs (pre-2016)	Treated	ADH operated HHAs in 69 of 75 counties until sold to Kindred in 2016. Included as the treated unit.

Notes: This table lists the subset of states that maintained publicly operated home health agencies at any point during the 2010–2021 study period. States were excluded from the control group if they experienced substantial transitions away from public provision. Alabama retained a statewide public program and is included in the control group. Arkansas is treated as the intervention state due to its 2016 privatization.

Table A.2: Kindred/ADH Beneficiary Share by County, 2014 and 2019

Category	**2014**	**2019**
Kindred served beneficiaries in county	73 / 75	75 / 75
Counties with ≥80% Kindred share (Dominant)	1	0
Counties with 20–79% Kindred share (Mixed)	17	20
Counties with <20% Kindred share (Marginal)	57	55

Notes: Share calculated as the percentage of beneficiaries in each county served by Kindred/ADH. Counties are classified as "Dominant" if Kindred served 80% or more of beneficiaries.

Table A.3: Difference-in-Differences Estimates: Service Characteristics

	Dependent Variable: Visits or Payments per Episode				
	Therapy	Aide	Nurse	Total Visits	Episode Payment
Treated × Post	3.14***	-8.88***	2.12***	-3.56***	490.19***
	(0.52)	(0.81)	(0.48)	(1.02)	(110.64)
Baseline Mean	2.70	9.59	8.27	22.18	1,837.23
Observations (Agency-Quarters)	389,919	389,919	389,919	389,919	389,919
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes

Notes: Each column reports the coefficient on the interaction between a post-privatization indicator and treatment group membership in a difference-in-differences regression. All models include year-by-quarter fixed effects. Standard errors clustered at the provider level are shown in parentheses. Baseline means refer to the pre-period among control (government-run) agencies. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4: Difference-in-Differences Estimates: Spillover Agencies

	Dependent Variable: Visits or Payments per Episode				
	Therapy	Aide	Nurse	Total Visits	Episode Payment
Treated × Post	0.29	-0.50***	0.25*	-0.04	36.72
	(0.20)	(0.18)	(0.15)	(0.33)	(33.13)
Baseline Mean	5.85	1.64	7.91	17.01	2,560.83
Observations (Agency-Quarters)	927,375	927,375	927,375	927,375	927,375
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes

Notes: Each column reports the coefficient on the interaction between a post-privatization indicator and spillover agency status (i.e., non-Kindred for-profit agencies in Arkansas) in a difference-in-differences regression. Models are weighted by the number of episodes and include year-by-quarter fixed effects. Standard errors clustered at the provider level are shown in parentheses. Baseline means refer to the pre-period among spillover agencies. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5: Difference-in-Differences Estimates: Beneficiary Characteristics

	Baseline Mean	Treated × Post Coefficient	
Outcome	Treated Group	2017-2019	2017-2021
Average Age	74.44	-1.23*	-0.34
		(0.64)	(0.81)
ADL Score	2.49	0.30***	0.18**
		(0.09)	(0.08)
Share Dual Eligible	0.51	-0.01	-0.034
		(0.03)	(0.03)
Number of Chronic Conditions	5.08	0.31**	0.54***
		(0.12)	(0.18)
Share Female	0.63	-0.02**	-0.02
		(0.01)	(0.01)
Elixhauser Comorbidity Index	3.51	0.34***	0.36***
	(0.13)	(0.15)	
Share Black	0.18	0.09**	0.05
		(0.04)	(0.04)
Institutional Referral	0.50	-0.02	-0.02
		(0.02)	(0.02)
Memory Deficit	0.09	0.03	0.02
		(0.02)	(0.02)
Share Age 85+	0.21	-0.03*	-0.00
		(0.02)	(0.02)
Share Age <65	0.20	0.02	0.01
		(0.02)	(0.02)
Year-Quarter FE		Yes	Yes

Notes: The coefficients reported are for the treated×post interaction term from difference-in-differences regression models comparing treated units to control units before and after the policy implementation. The Elixhauser Comorbidity Index is calculated using outpatient claims excluding home health claims. Number of chronic conditions is derived from the Medicare Beneficiary Summary File (MBSF) for the year prior to the home health episode. All models include year-by-quarter fixed effects and are weighted by episode count. Standard errors clustered at the provider level are shown in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.6: Difference-in-Differences Estimates: Extensive Margin Outcomes

	Users per 1,000 Beneficiaries				**Spending per Beneficiary**
	All Users	Dual Users	Urban Users	Rural Users	Spending
Post × Arkansas	6.61***	5.26**	6.82***	4.06***	36.10**
	(1.16)	(2.44)	(1.17)	(1.26)	(12.3)
Baseline Mean	71.91	102.54	70.62	84.46	303.91
Observations	540	540	540	516	540
Clusters (States)	46	46	46	44	46
State FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

Notes: Each column reports the coefficient on the interaction between a post-2017 indicator and an Arkansas indicator in a difference-in-differences model. All regressions include state and year fixed effects. Standard errors clustered at the state level are shown in parentheses. Baseline means reflect pre-treatment averages (2010-2014) in Arkansas within the subgroup. The main sample excludes six states that privatized public home health agencies during the study period: Georgia, Idaho, Kentucky, North Carolina, Oklahoma, and South Carolina. The rural panel additionally excludes Delaware and the District of Columbia due to insufficient rural beneficiary counts. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.7: Difference-in-Differences Estimates: Extensive Margin Outcomes (Excluding Alabama)

	Dependent Variable: Users or Spending per 1,000 Beneficiaries				
	All Users	Dual Users	Urban Users	Rural Users	Spending
Post × Arkansas	6.70 ***	5.22 **	6.88 ***	4.90 ***	30.97 ***
	(1.11)	(2.33)	(1.12)	(1.19)	(9.92)
Baseline Mean	71.91	102.54	70.62	84.46	329.86
Observations	528	528	528	504	528
Clusters (States)	44	44	44	42	44
State FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

Notes: Each column reports the coefficient on the interaction between a post-2017 indicator and an Arkansas indicator in a difference-in-differences model. All regressions include state and year fixed effects. Standard errors clustered at the state level are shown in parentheses. Baseline means reflect pre-treatment averages (2010–2014) in Arkansas.
The main sample excludes seven states that privatized public home health agencies during the study period: Alabama, Georgia, Idaho, Kentucky, North Carolina, Oklahoma, and South Carolina. The rural panel additionally excludes Delaware and the District of Columbia due to insufficient rural beneficiary counts. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.8: Synthetic Control-Based Estimates: Extensive Margin Outcomes

Year	Spending	All Users	Rural Users	Urban Users	Dual Users
2017	4.06 [3.98, 4.14]	5.68 [5.67, 5.68]	0.39 [0.37, 0.40]	1.83 [1.81, 1.84]	2.16 [1.78, 2.53]
2018	10.95 [10.87, 11.03]	7.73 [7.73, 7.73]	1.38 [1.37, 1.39]	2.98 [2.97, 3.00]	2.08 [1.71, 2.46]
2019	17.91 [17.83, 17.99]	7.95 [7.95, 7.95]	-0.06 [-0.07, -0.04]	4.30 [4.29, 4.31]	3.40 [3.02, 3.77]
2020	23.13 [23.05, 23.21]	8.54 [8.54, 8.54]	4.17 [4.16, 4.18]	6.05 [6.03, 6.06]	0.59 [0.22, 0.97]
2021	17.41 [17.33, 17.49]	9.08 [9.07, 9.08]	6.03 [6.02, 6.05]	6.85 [6.84, 6.86]	1.20 [0.83, 1.58]

Notes: Each cell reports the synthetic control estimate of the treatment effect for Arkansas in a given year, with 95% confidence intervals in brackets. Confidence intervals are based on the pre-treatment RMSE of the Arkansas–synthetic gap. Estimates are normalized relative to the average pre-treatment difference. Outcomes include Medicare home health spending per beneficiary (Spending) and the number of home health users per 1,000 beneficiaries, both overall and for subgroups (Rural, Urban, Dual-Eligible).

Table A.9: Within-Arkansas OLS and IV: Share Change and Home Health Use (Main: 2017–2019)

	Home Health Users per 1,000	Home Health Episodes per 1,000
	Post 2017–19	Post 2017–19
Panel A: OLS		
Share Change (10pp) × Post	1.033*	5.511**
	(0.531)	(1.913)
Panel B: IV		
Share Change (10pp) × Post	2.166**	9.351***
	(0.814)	(2.745)
Observations	750	750
County FE	Yes	Yes
Year FE	Yes	Yes
K-P F-stat (IV)	129.0	129.0

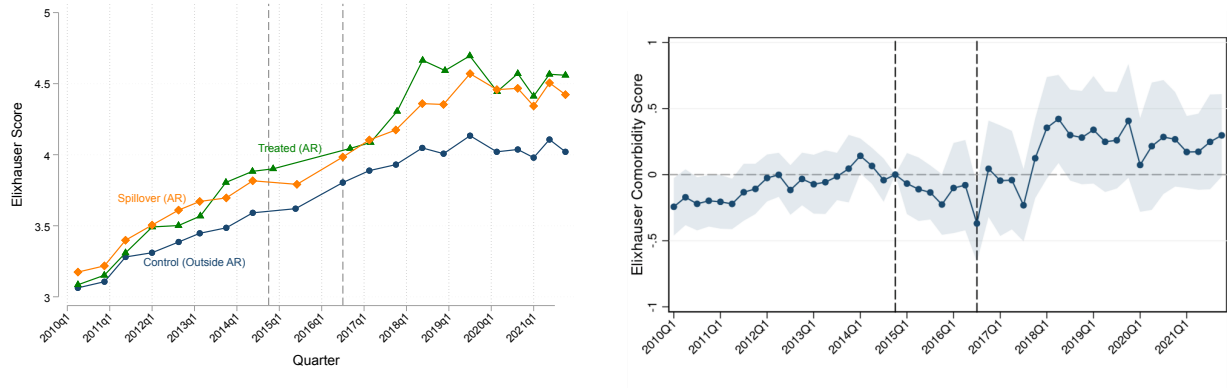
Notes: Notes: Unit of observation is county–year. Outcomes are per 1,000 beneficiaries. Share change is scaled to a 10 percentage point unit. OLS and IV specifications absorb county and year fixed effects and cluster standard errors at the county level. IV instrument uses baseline treated share squared interacted with the post indicator. * p<0.05; ** p<0.01; *** p<0.001.

Table A.10: Difference-in-Differences Estimates: Spillover Effects by Ownership Type

	Dependent Variable: Visits or Payments per Episode	
Outcome	Nonprofit Agencies	For-Profit Agencies
Therapy Visits	0.57	0.95***
	(0.44)	(0.25)
Aide Visits	-0.61	-0.42*
	(0.47)	(0.21)
Nurse Visits	0.27	0.82*
	(0.30)	(0.32)
Total Visits	-0.03	1.34**
	(0.71)	(0.48)
Episode Payment ($)	-44.37	47.42
	(71.13)	(38.78)
Quarter-Year FE	Yes	Yes

Notes: Estimates reflect provider-level difference-in-differences models comparing pre- and post-period outcomes for spillover agencies in Arkansas versus agencies in other states. Columns are stratified by ownership type. All models include year-by-quarter fixed effects. For-profit models are weighted by episode count. Standard errors clustered at the provider level are shown in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

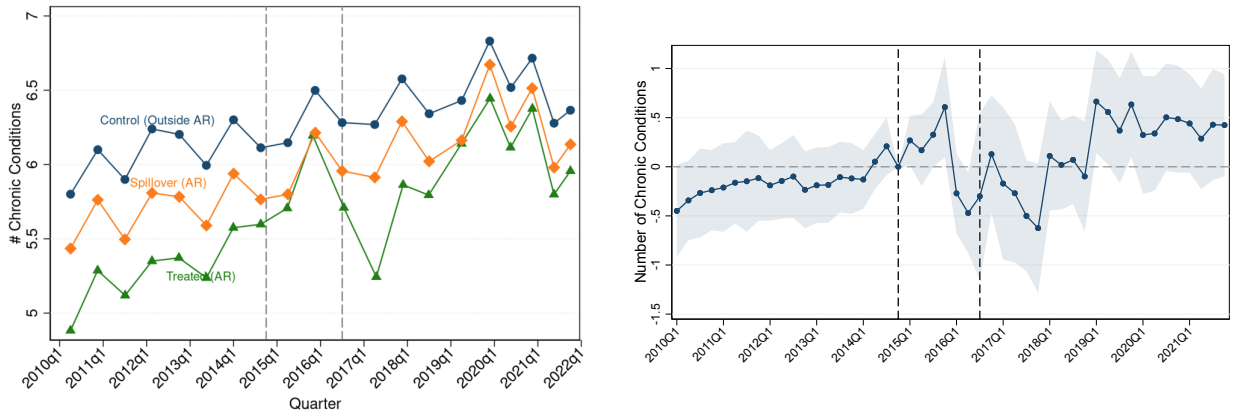
B Appendix Figures



(a) Group-Level Means

(b) Event Study Estimates

Figure A.1: **Elixhauser Comorbidity Score.** The left panel shows group-level averages of Elixhauser scores, a summary measure of clinical complexity. The right panel presents event study estimates using quarterly data. *Notes:* In panel (a), the green line represents agencies formerly operated by the Arkansas Department of Health and acquired by Kindred, the orange line shows other agencies operating in Arkansas (spillovers), and the navy line shows agencies in control states. Data are collapsed to the agency-quarter level and weighted by episodes. Shaded areas in panel (b) represent 95% confidence intervals. Estimates use provider-level difference-in-differences with year-quarter fixed effects. Vertical dashed lines denote the announcement (2014Q3) and implementation (2016Q3) of privatization.



(a) Group-Level Means

(b) Event Study Estimates

Figure A.2: **Number of Chronic Conditions.** The left panel shows group-level averages of the number of chronic conditions per episode. The right panel presents event study estimates using quarterly data. *Notes:* In panel (a), the green line represents agencies formerly operated by the Arkansas Department of Health and acquired by Kindred, the orange line shows other agencies operating in Arkansas (spillovers), and the navy line shows agencies in control states. Data are collapsed to the agency-quarter level and weighted by episodes. Shaded areas in panel (b) represent 95% confidence intervals. Estimates use provider-level difference-in-differences with year-quarter fixed effects. Vertical dashed lines denote the announcement (2014Q3) and implementation (2016Q3) of privatization.

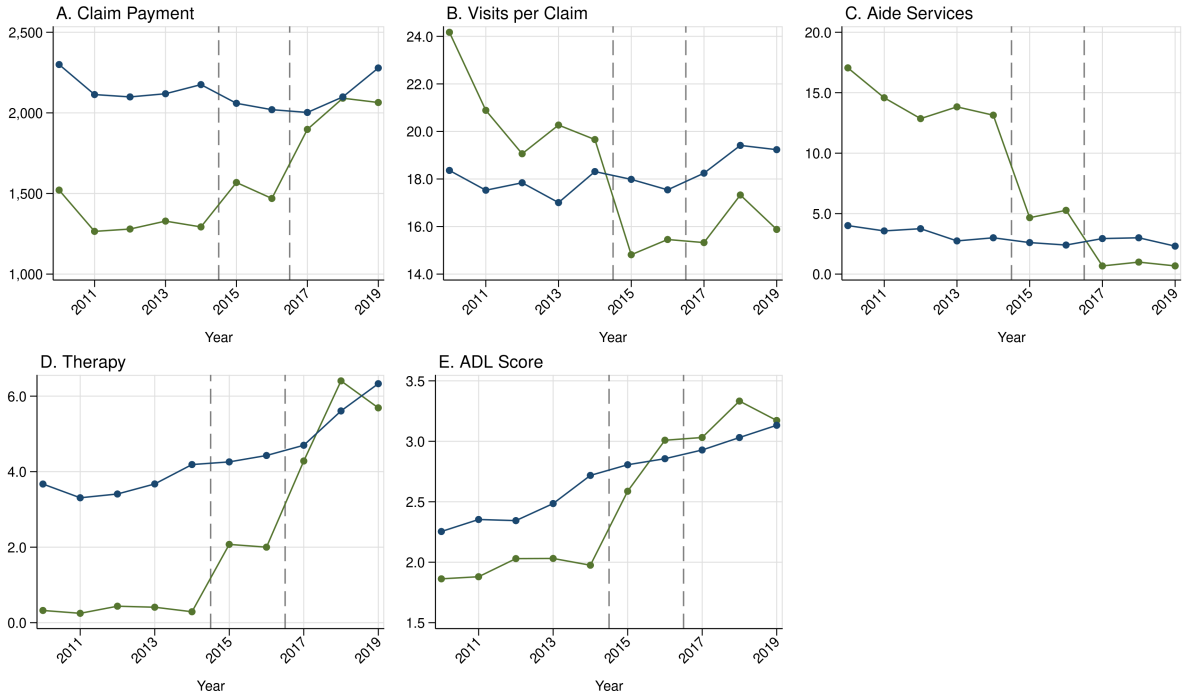


Figure A.3: **Principal Diagnosis: Diabetes.** This figure shows event study estimates for home health episodes in which diabetes was listed as the principal diagnosis. *Notes:* In all panels, the green line represents agencies formerly operated by the Arkansas Department of Health and acquired by Kindred and the navy line shows other agencies in Arkansas (spillovers). Data are restricted to episodes with diabetes as the primary condition. Estimates use provider-level difference-in-differences with year-quarter fixed effects and are weighted by episode counts. Shaded areas indicate 95% confidence intervals. Vertical dashed lines denote the announcement (2014Q3) and implementation (2016Q3) of privatization.

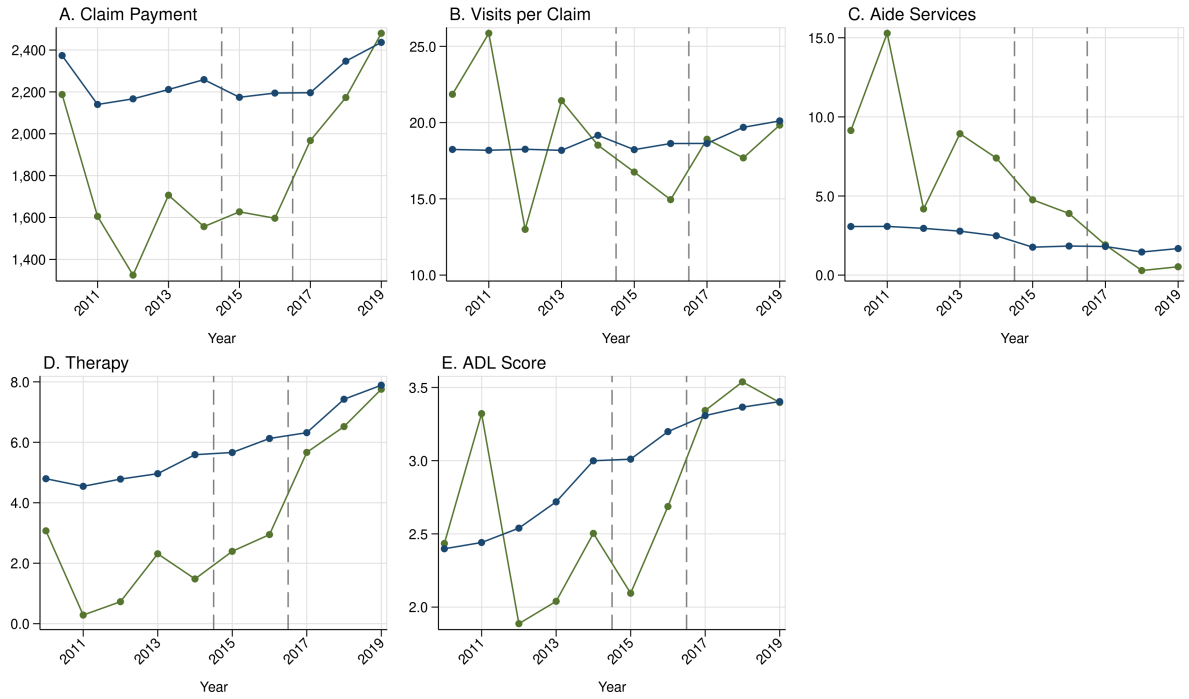
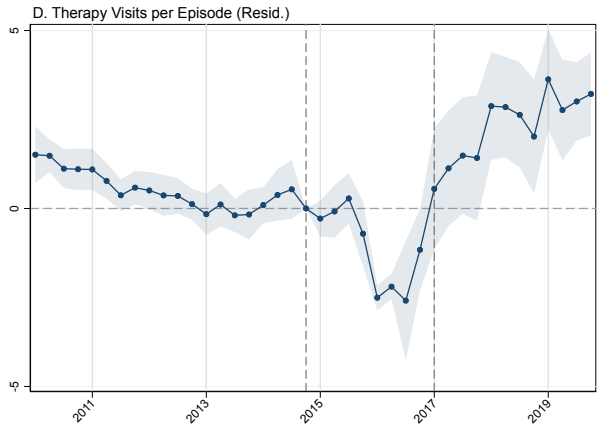
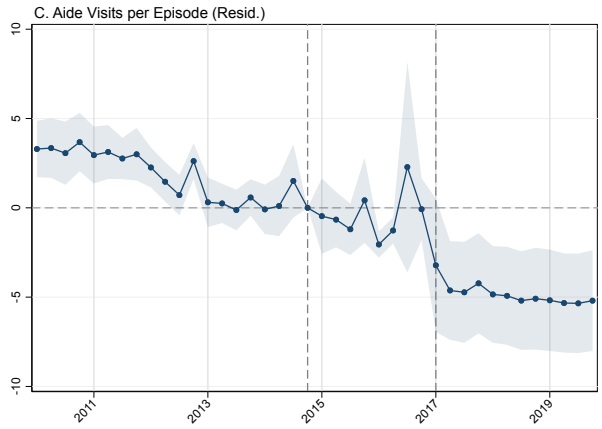


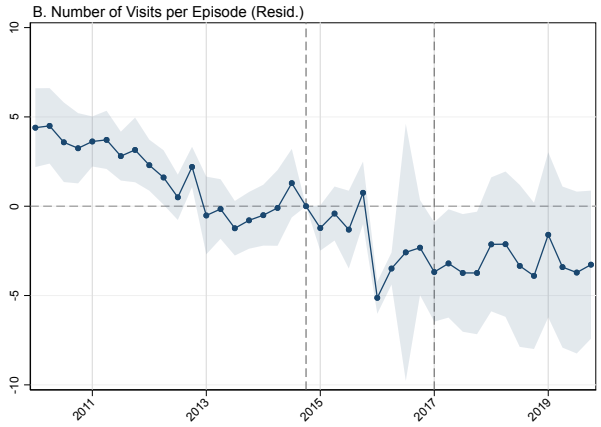
Figure A.4: **Principal Diagnosis: COPD.** This figure shows event study estimates for home health episodes in which chronic obstructive pulmonary disease (COPD) was listed as the principal diagnosis. *Notes:* In all panels, the green line represents agencies formerly operated by the Arkansas Department of Health and acquired by Kindred and the navy line shows other agencies in Arkansas (spillovers). Data are restricted to episodes with COPD as the primary condition. Estimates use provider-level difference-in-differences with year-quarter fixed effects and are weighted by episode counts. Shaded areas indicate 95% confidence intervals. Vertical dashed lines denote the announcement (2014Q3) and implementation (2016Q3) of privatization.



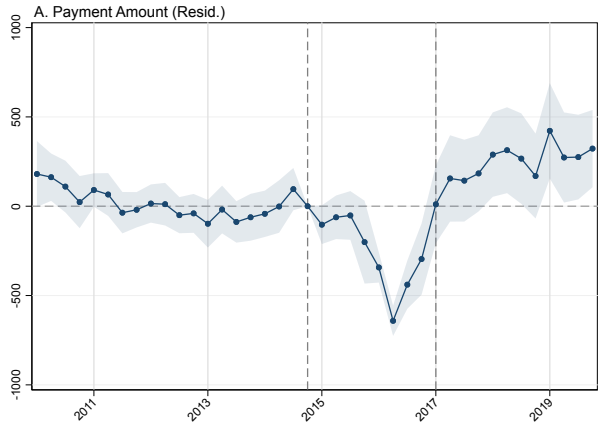
(a) Therapy Visits (Residualized)



(b) Aide Visits (Residualized)

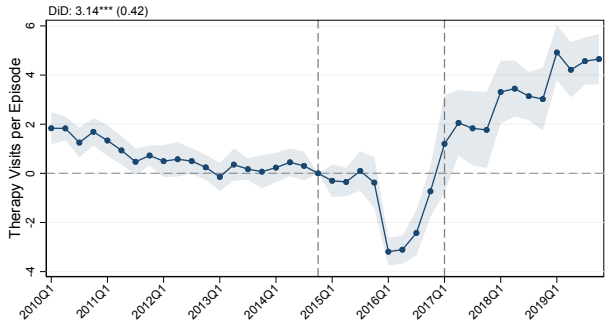


(c) Nurse Visits (Residualized)

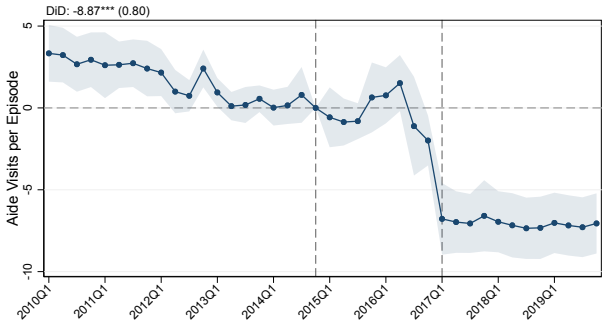


(d) Episode Payment (Residualized)

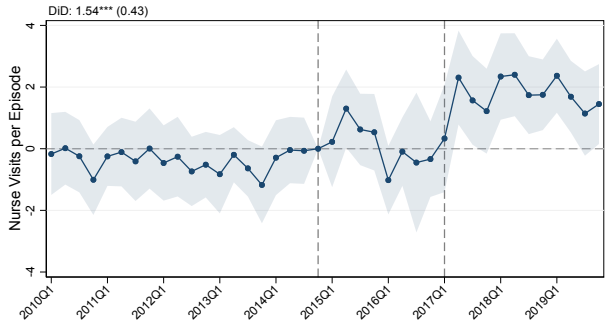
Figure A.5: **Event Study Estimates Using Residualized Outcomes.** This figure replicates the main event study models using outcomes residualized on a set of beneficiary-level covariates, including age, sex, dual eligibility, rurality, referral source, diagnosis group, and comorbidities.



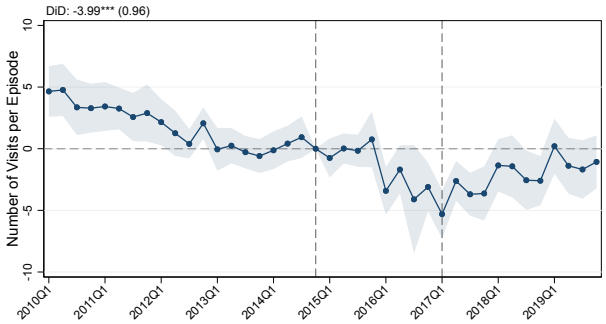
(a) Therapy Visits



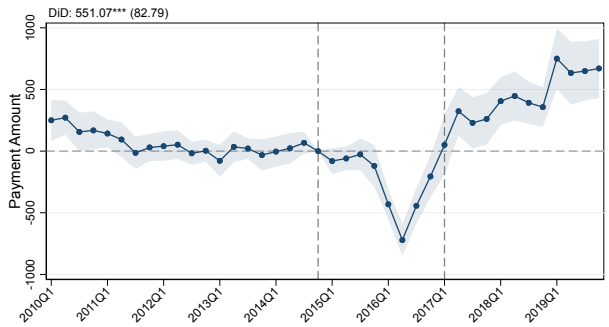
(b) Aide Visits



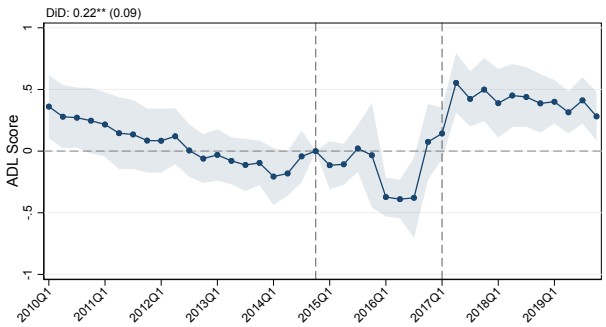
(c) Nurse Visits



(d) Total Visits

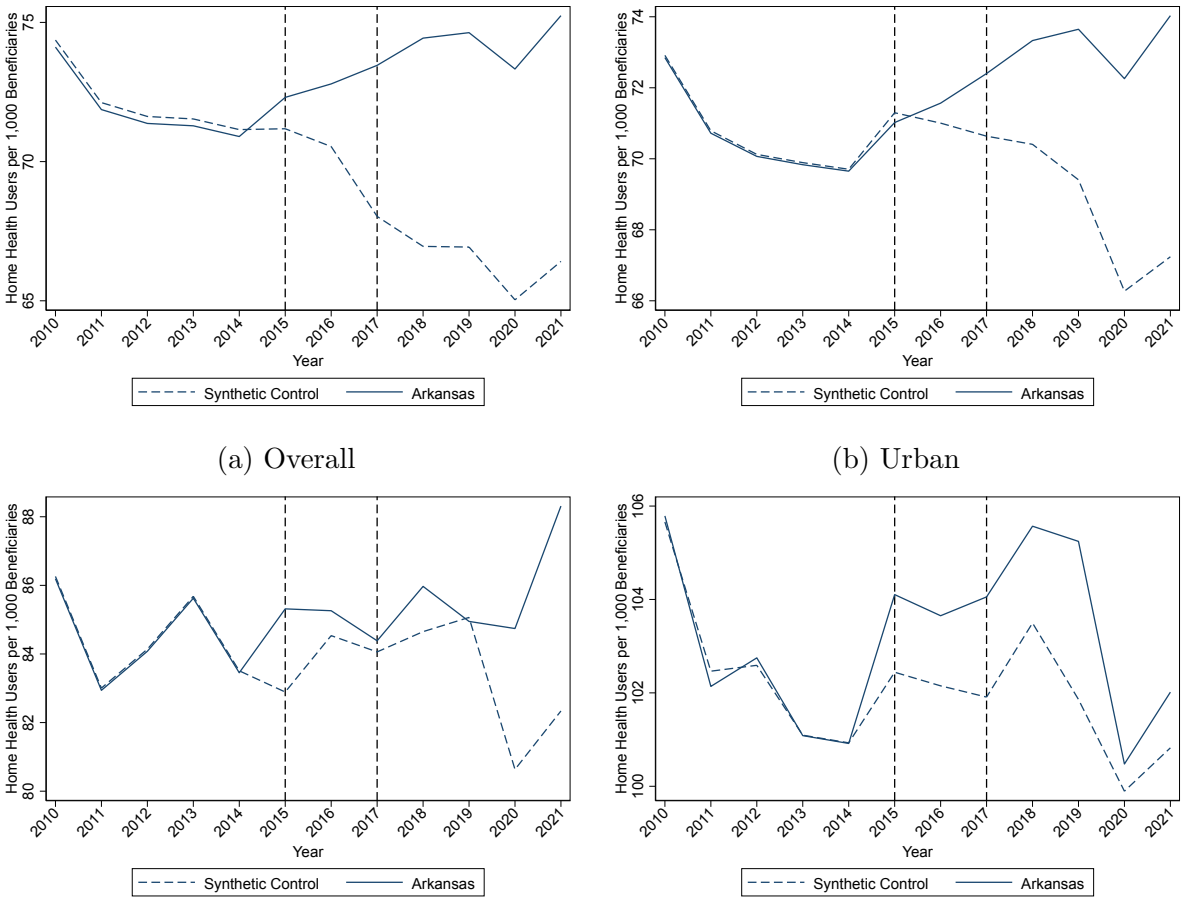


(e) Episode Payment



(f) ADL Score

Figure A.6: **Event Study Estimates: Excluding Kindred and Gentiva Agencies in Control States.** This figure replicates the main agency-level event studies using a control group that excludes all agencies owned by Kindred or Gentiva during the study period, with the exception of the treated agencies in Arkansas. Outcomes include therapy, aide, and nursing visits per episode; total visits; Medicare episode payment; and ADL score. Data are collapsed to the agency-quarter level and weighted by episodes. Estimates include year-quarter fixed effects and cluster standard errors at the provider level. Shaded regions show 95% confidence intervals.



(a) Overall

(b) Urban

(c) Rural

(d) Dual Eligible

Figure A.7: **Synthetic Control Estimates: Home Health Users per 1,000 Beneficiaries.** Each panel compares Arkansas to a synthetic control constructed from other states using pre-treatment trends in home health use. Panel (a) shows overall results, while panels (b)–(d) show subgroup estimates for urban counties, rural counties, and dual-eligible beneficiaries. *Notes:* Estimates are based on quarterly state-level data from 2010–2021. Vertical dashed lines denote the announcement (2014Q3) and implementation (2016Q3) of privatization.

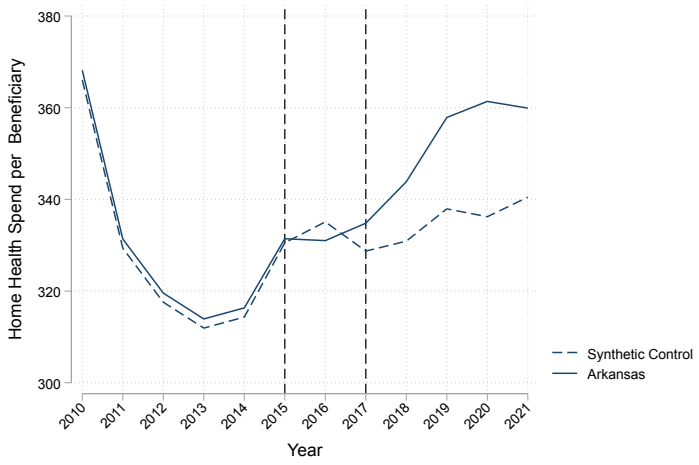
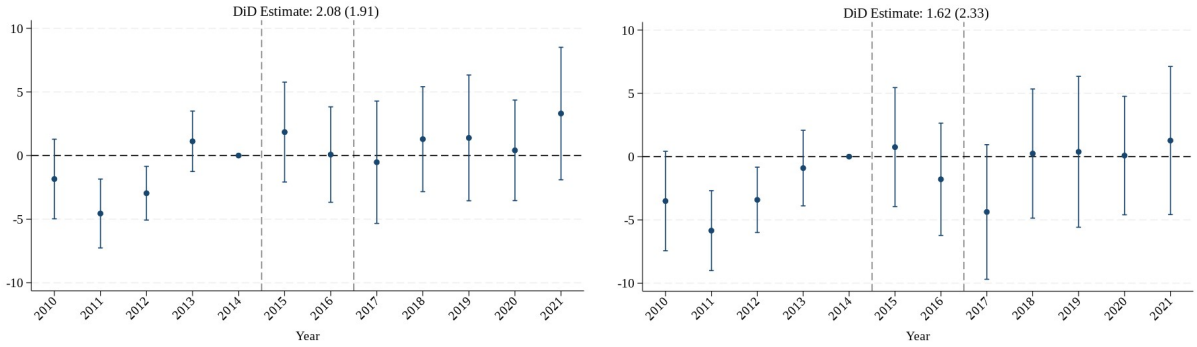


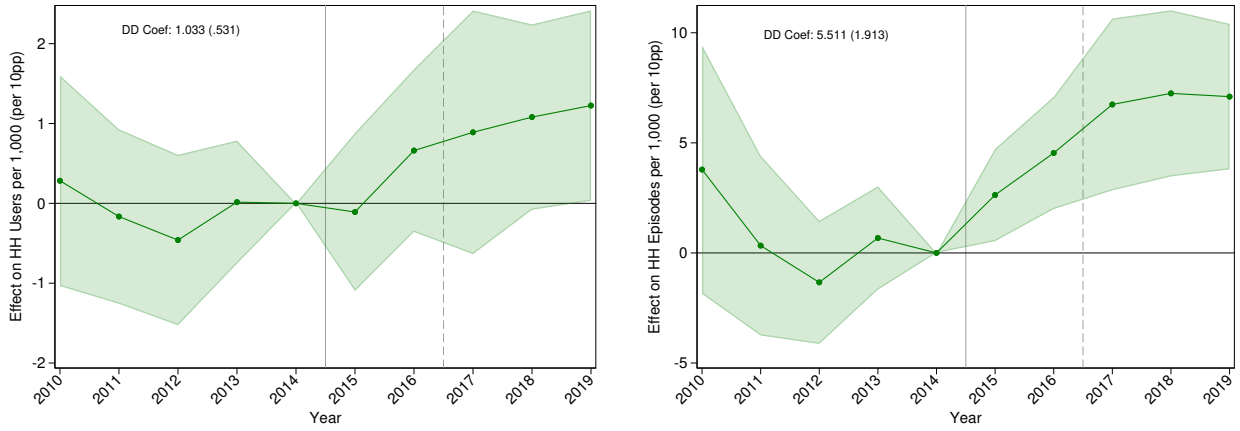
Figure A.8: **Synthetic Control Estimate: Home Health Spending per Beneficiary.** This figure compares quarterly home health spending per Medicare beneficiary in Arkansas to a synthetic control constructed from other states based on pre-treatment trends. *Notes:* Estimates are based on state-level data from 2010–2021. Vertical dashed lines denote the announcement (2014Q3) and implementation (2016Q3) of privatization. The solid line represents observed outcomes in Arkansas; the dashed line represents the synthetic control.



(a) Very Rural Counties

(b) Extremely Rural Counties

Figure A.9: **Visit Volume Effects by Rurality Definition.** Each panel shows difference-in-differences estimates of home health users per 1000 beneficiaries using alternate definitions of rurality. *Notes:* Rurality is defined using the USDA Rural-Urban Continuum Codes (RUCC). "Very rural" includes counties coded as RUCC 8 or 9 (completely rural or population <2,500); "extremely rural" includes only RUCC 9 counties (not adjacent to a metro area). Data are collapsed to the state-year level. Vertical dashed lines indicate the announcement (2014Q3) and implementation (2016Q3) of privatization.



(a) Users per 1,000 Beneficiaries

(b) Episodes per 1,000 Beneficiaries

Figure A.10: **Continuous Difference-in-Differences: Home Health Use by County-Level Kindred Expansion.** Event study estimates from a continuous difference-in-differences model where treatment intensity is defined as the change in Kindred's county-level market share from 2014 to 2019. *Notes:* Coefficients represent the effect of a 10 percentage point increase in Kindred's market share. The pooled difference-in-differences coefficient is displayed in the top left of each panel. Shaded areas represent 95% confidence intervals. Vertical dashed lines denote the period before the announcement (2014.5) and the period before implementation (2016.5) of privatization. Both panels show sustained increases post-2017, consistent with net market expansion in response to Kindred's growth.

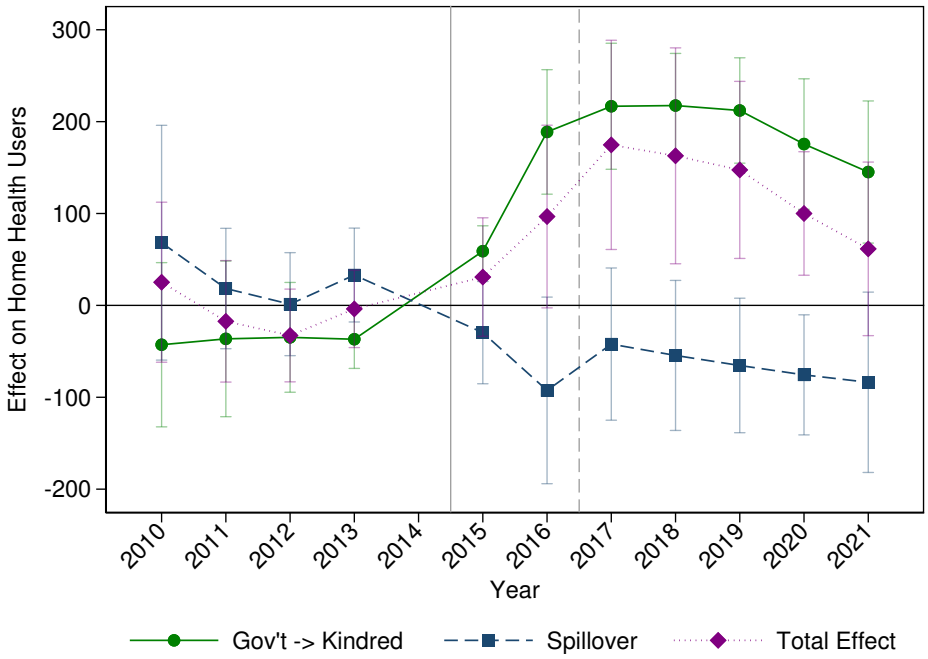


Figure A.11: **Decomposing Home Health Expansion by Provider Type.** This figure shows continuous difference-in-differences event study estimates for the change in home health users per 1,000 beneficiaries, decomposed by provider type. Treatment intensity is measured as the change in Kindred's market share between 2014 and 2019 at the county level. *Notes:* The green solid line shows the increase in users at Kindred-acquired agencies. The blue dashed line reflects changes at non-Kindred (spillover) agencies. The purple dotted line indicates the total market-level change across all agencies. Estimates represent the effect of a 100 percentage point increase in Kindred's county-level share. Results indicate that while some displacement occurred among spillover agencies, the total effect remained positive, suggesting net market expansion rather than pure business stealing. Vertical dashed lines denote the period before the announcement (2014.5) and the period before implementation (2016.5) of privatization.

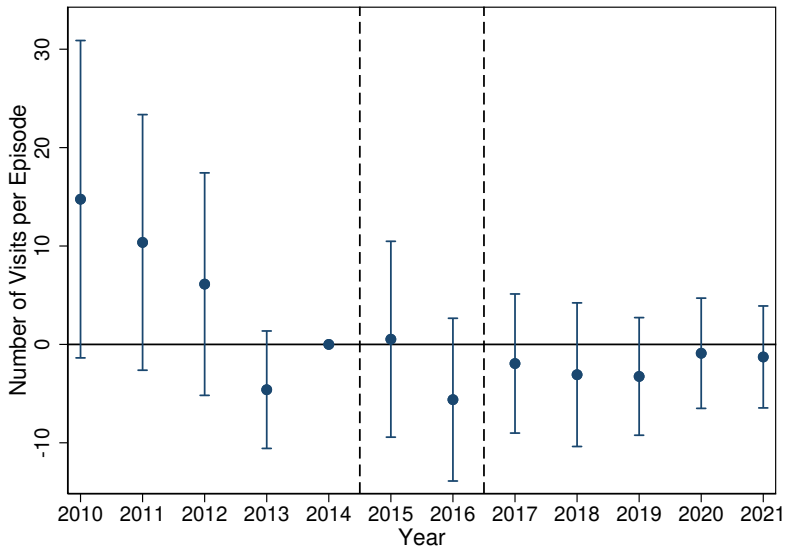
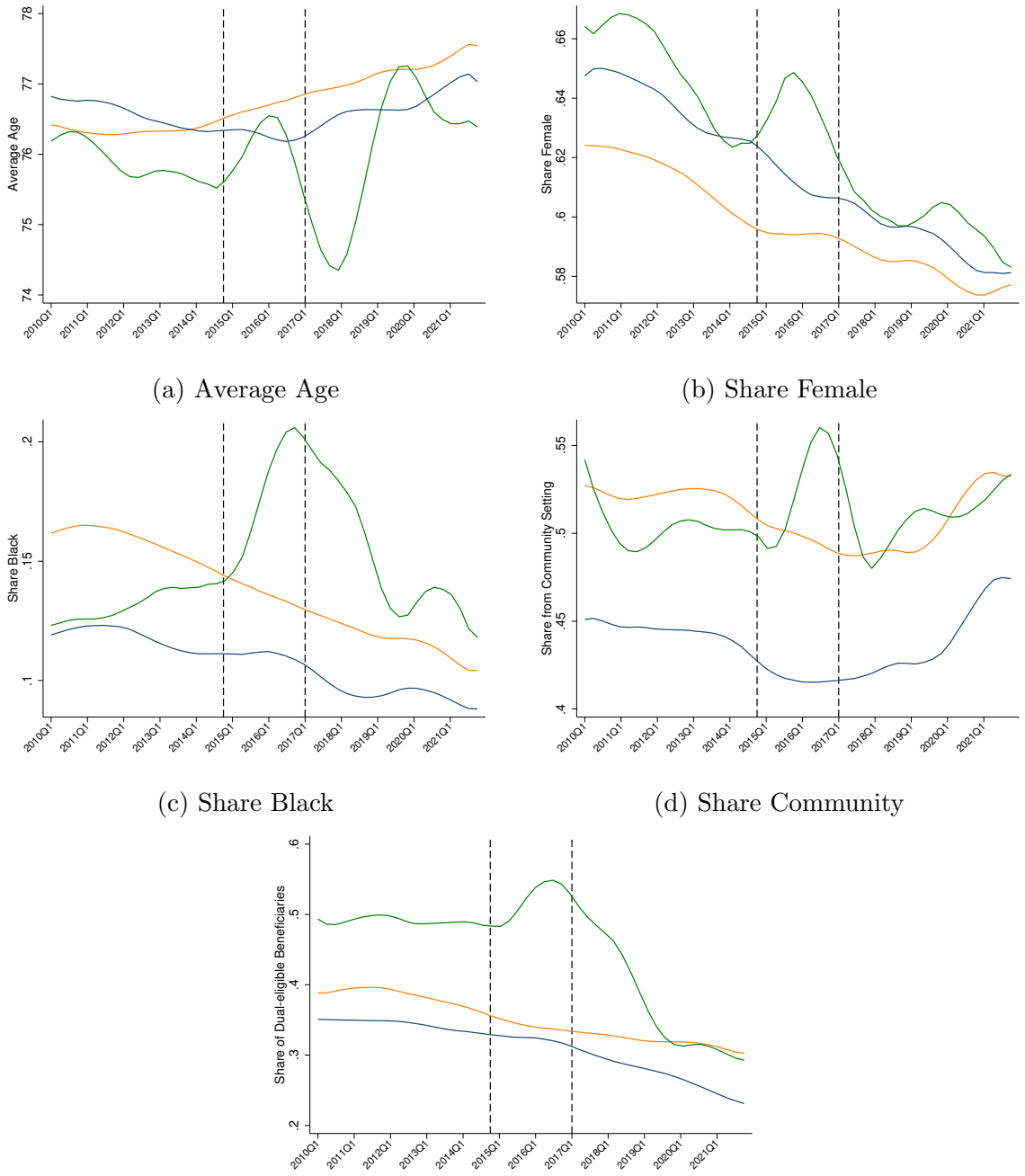


Figure A.12: **Actual Visits: High-Need, Low-Payment Patients.** This figure shows event study estimates of actual visits per home health claim, restricted to the subset of patients predicted (in control states) to require high visit volume and receive low payment. *Notes:* Subgroup defined using 2010 - 2014 control-state regression models of predicted visit intensity and payment. Estimates shown for treated agencies only. Vertical dashed lines denote the announcement (2014Q3) and implementation (2016Q3) of privatization.



(a) Average Age

(b) Share Female

(c) Share Black

(d) Share Community

(e) Share Dual Eligible

Figure A.13: **Beneficiary Characteristics: Group-Level Means.** These panels show trends in demographic and socioeconomic characteristics of home health beneficiaries, averaged at the agency-quarter level and weighted by episodes. *Notes:* Green line: Government-to-Kindred agencies; navy: other agencies in Arkansas; orange: control agencies in other states. Vertical dashed lines denote the announcement (2014Q3) and implementation (2016Q3) of privatization.

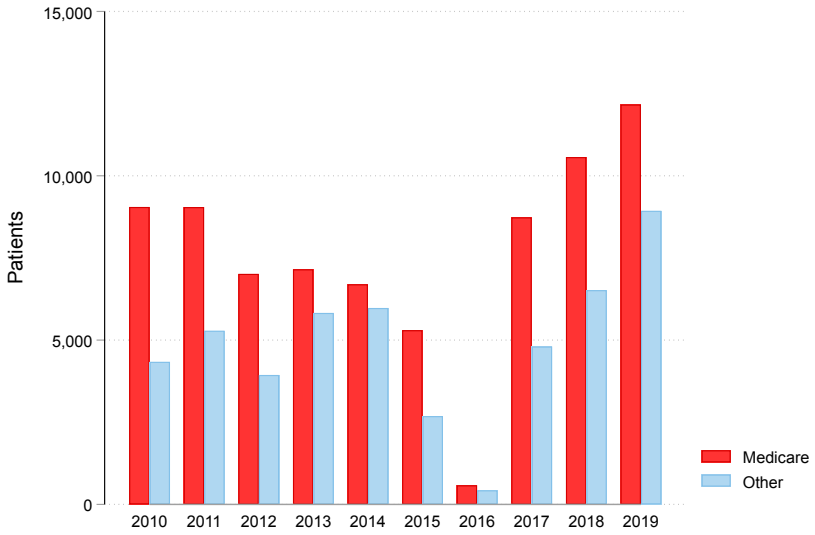


Figure A.14: **Patient Volume by Payer Source: ADH/Kindred Agencies.** This figure shows the total number of patients served annually by the formerly public agencies, disaggregated by payer source (Medicare vs. Medicaid and private insurance combined). *Notes:* Data are from Medicare cost reports (Worksheet S-3) for ADH (pre-2016) and Kindred (post-2016) agencies in Arkansas. Red bars indicate Medicare patients; blue bars indicate all other payers. The gap at 2015-2016 likely reflects incomplete cost report filing during the ownership transition.

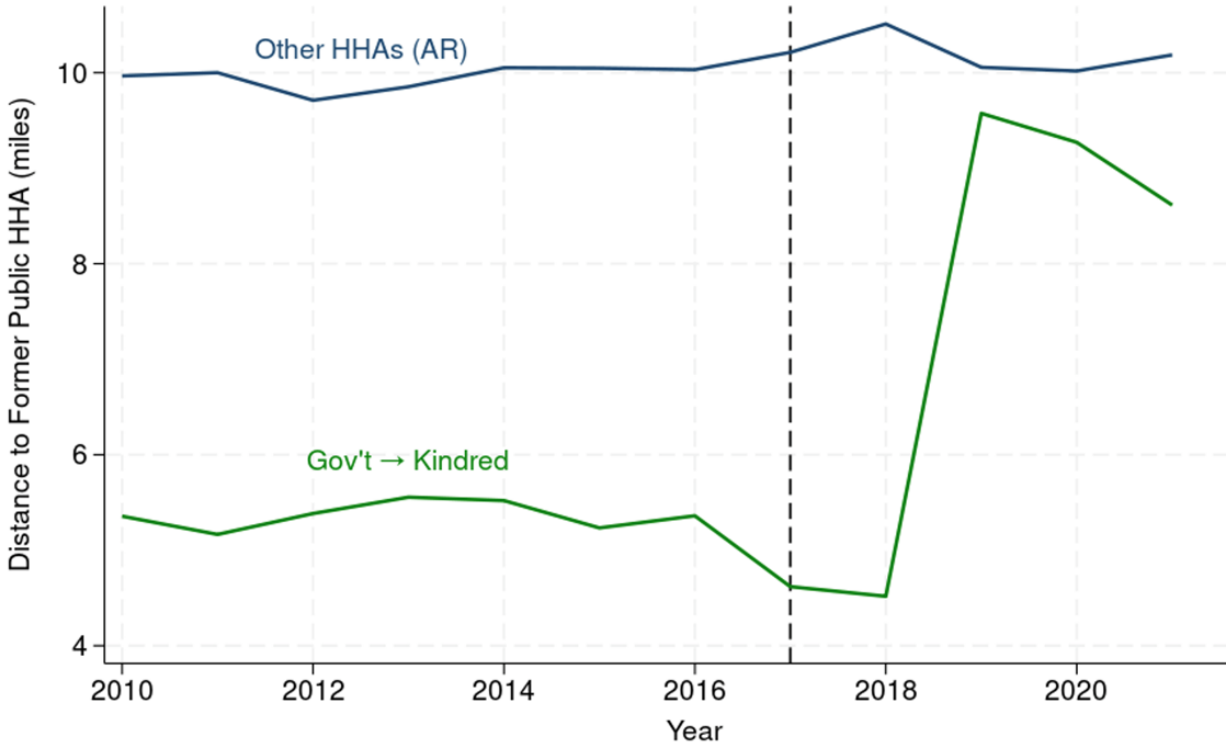


Figure A.15: **Distance to Former Public Home Health Agencies.** This figure shows the average ZIP-to-ZIP distance between beneficiaries and the nearest ZIP code of a government-operated home health agency as of 2014, by agency group. *Notes:* Distance is measured using fixed pre-privatization ZIP code geography from 2014, based on centroids. Treated agencies were formerly operated by the Arkansas Department of Health and acquired by Kindred after 2016. The green line shows treated agencies; the blue line shows other non-Kindred agencies in Arkansas. Data are collapsed to the agency-quarter level and weighted by the number of episodes. Vertical dashed lines denote the announcement (2014Q3) and implementation (2016Q3) of privatization.

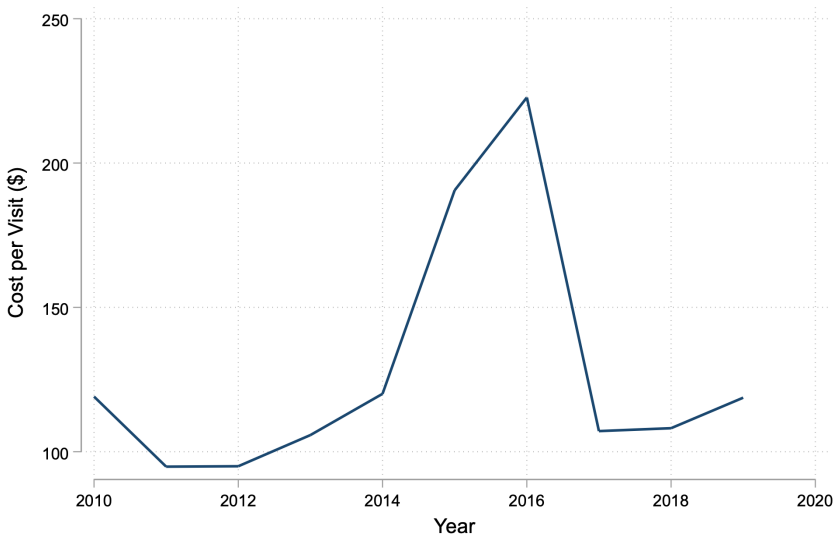
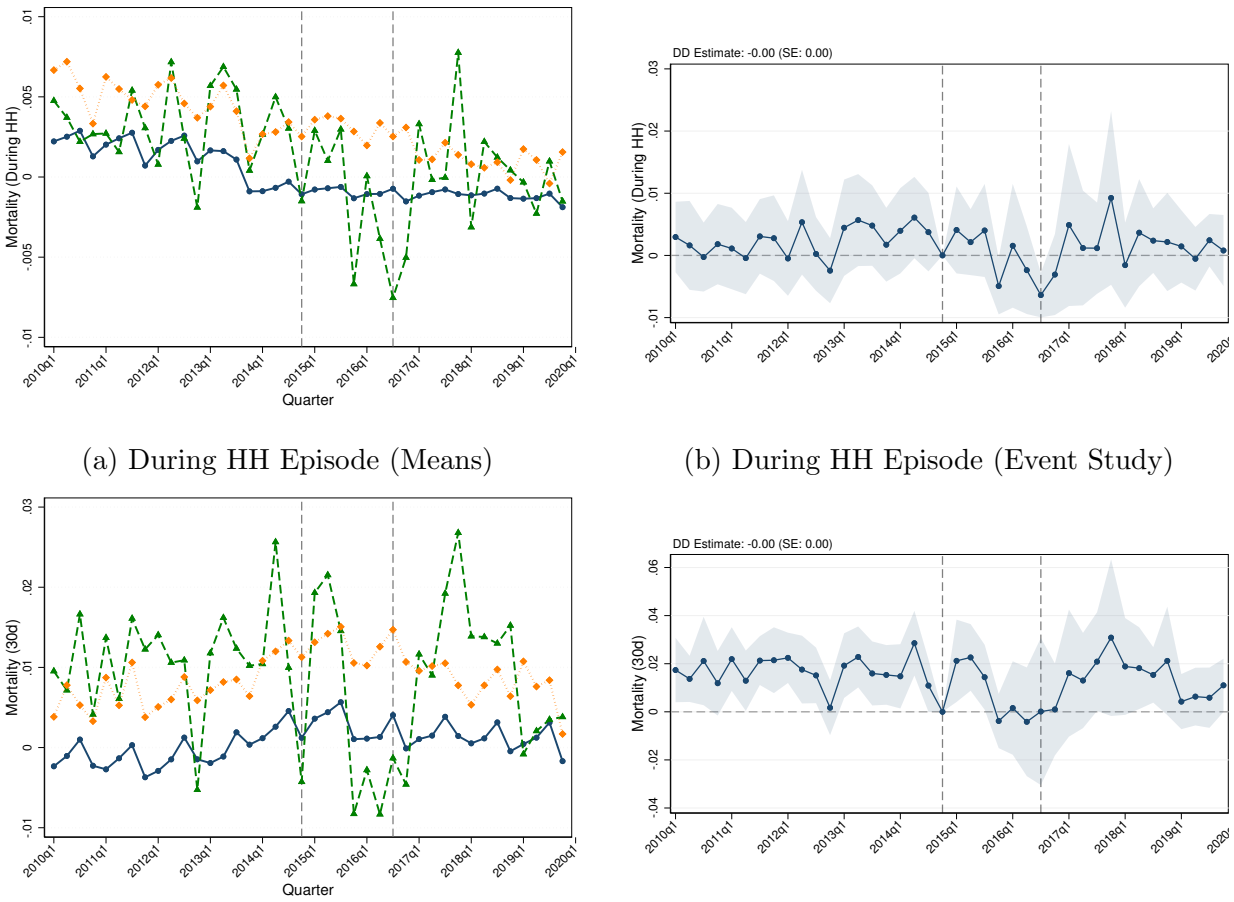


Figure A.16: **Average Cost per Visit: ADH/Kindred Agencies.** This figure shows trends in average cost per visit for the formerly public home health agencies (ADH pre-2017, Kindred post-2017), using data from Medicare cost reports. *Notes:* Data are from Medicare cost reports (Worksheet C) for ADH/Kindred agencies in Arkansas. Costs are weighted by episode counts from Worksheet S-3.



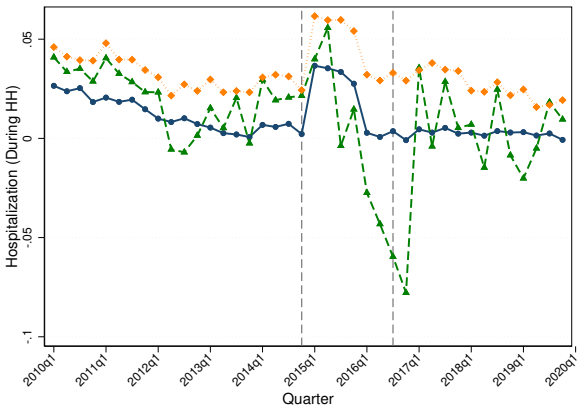
(a) During HH Episode (Means)

(b) During HH Episode (Event Study)

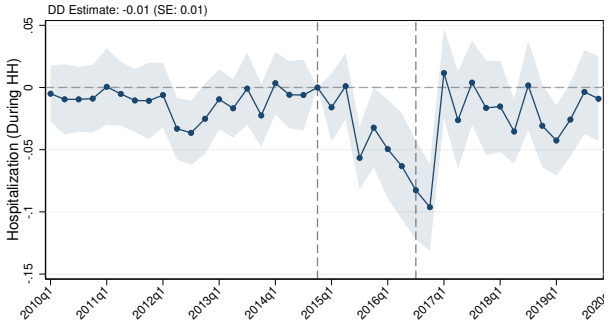
(c) 30-Day Post-Discharge (Means)

(d) 30-Day Post-Discharge (Event Study)

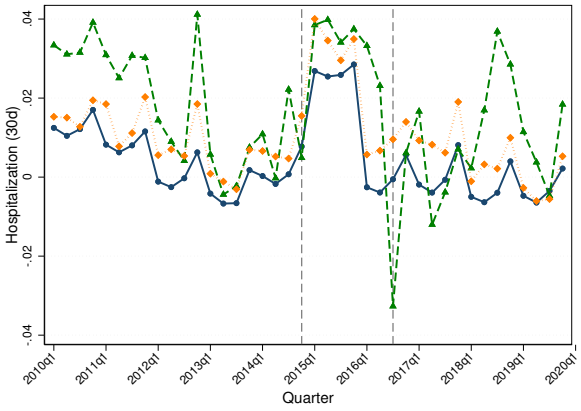
Figure A.17: **Mortality During and After Home Health Episodes.** The top row shows group-level means and event study estimates for mortality during the home health episode. The bottom row shows means and event study estimates for 30-day post-discharge mortality. *Notes:* In panels (a) and (c), the green line represents agencies formerly operated by the Arkansas Department of Health and acquired by Kindred, the orange line shows other agencies in Arkansas (spillovers), and the navy line shows agencies in control states. Data are collapsed to the agency-quarter level and weighted by episodes. Shaded areas in panels (b) and (d) represent 95% confidence intervals. Estimates use provider-level difference-in-differences with year-quarter fixed effects. Vertical dashed lines denote the announcement (2014Q3) and implementation (2016Q3) of privatization.



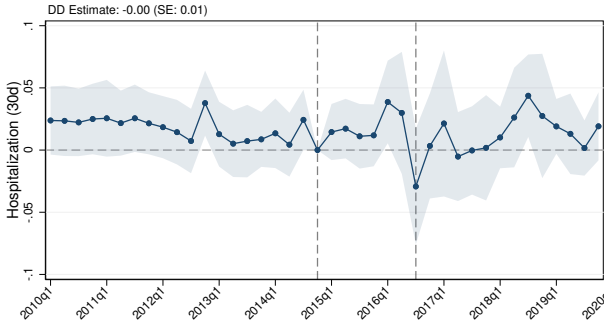
(a) During HH Episode (Means)



(b) During HH Episode (Event Study)



(c) 30-Day Post-Discharge (Means)



(d) 30-Day Post-Discharge (Event Study)

Figure A.18: **Hospitalization During and After Home Health Episodes.** The top row shows group-level means and event study estimates for inpatient admissions during the home health episode. The bottom row shows 30-day post-discharge hospitalizations. *Notes:*In panels (a) and (c), the green line represents agencies formerly operated by the Arkansas Department of Health and acquired by Kindred, the orange line shows other agencies in Arkansas (spillovers), and the navy line shows agencies in control states. Data are collapsed to the agency-quarter level and weighted by episodes. Shaded areas in panels (b) and (d) represent 95% confidence intervals. Estimates use provider-level difference-in-differences with year-quarter fixed effects. Vertical dashed lines denote the announcement (2014Q3) and implementation (2016Q3) of privatization.

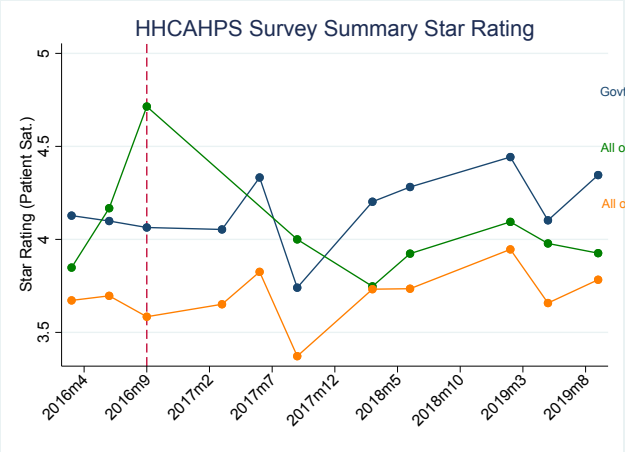
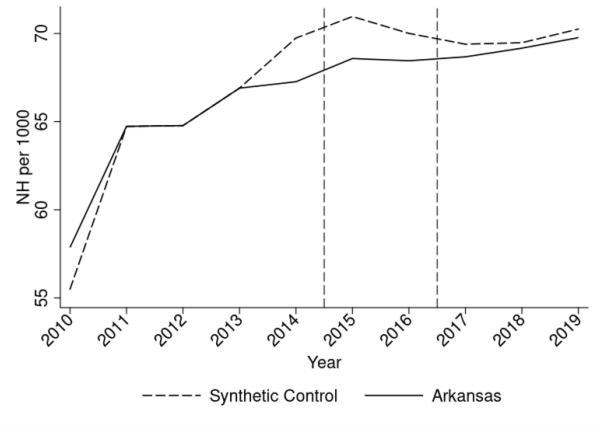
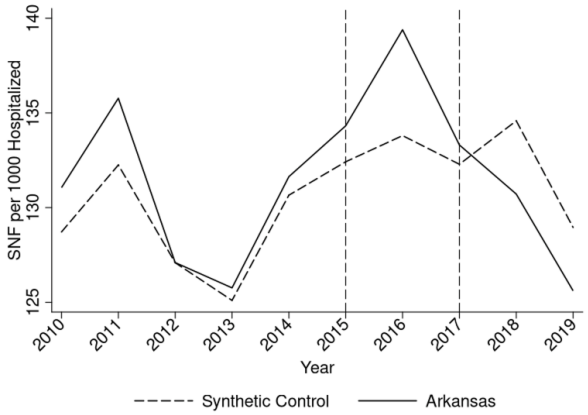


Figure A.19: **HHCAHPS Star Ratings.** This figure shows average Home Health Consumer Assessment of Healthcare Providers and Systems (HHCAHPS) star ratings by agency group over time. *Notes:* The green line represents agencies formerly operated by the Arkansas Department of Health and acquired by Kindred, the orange line shows other agencies in Arkansas (spillovers), and the navy line shows agencies in control states. Data are collapsed to the agency-quarter level. Vertical dashed lines denote the implementation (2016Q3) of privatization.

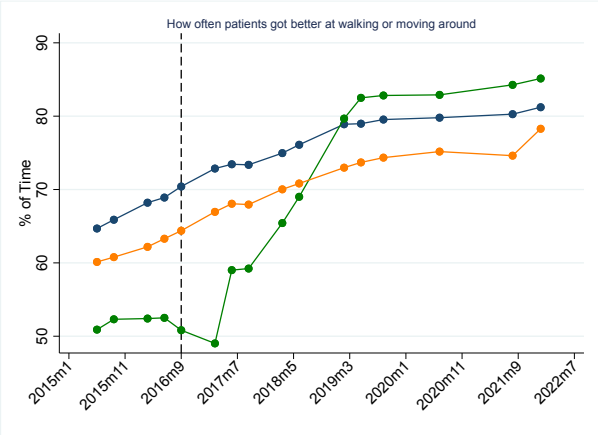


(a) Nursing Home Use per 1,000 Beneficiaries

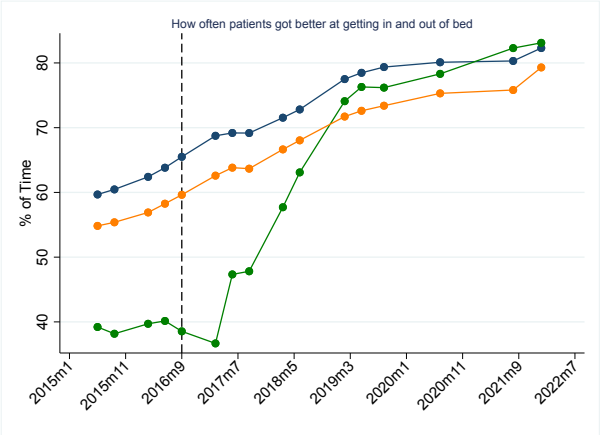


(b) SNF Use per 1,000 Hospitalized Beneficiaries

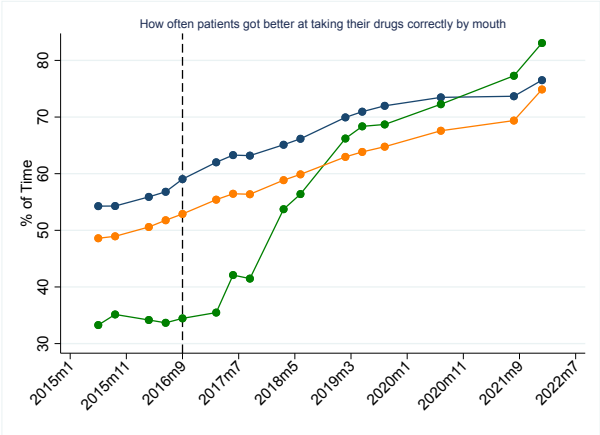
Figure A.20: **Synthetic Control Estimates: Institutional Care Use.** Each panel compares Arkansas to a synthetic control constructed from other states using pre-treatment trends. Panel (a) shows nursing home use per 1,000 beneficiaries. Panel (b) shows skilled nursing facility (SNF) use per 1,000 hospitalized beneficiaries. *Notes:* Vertical dashed lines denote the period prior to the privatization announcement and implementation. Solid lines reflect actual Arkansas trends; dashed lines represent synthetic control.



(a) Walking/Moving Around



(b) Getting in and out of Bed



(c) Taking Drugs Correctly by Mouth

Figure A.21: **Additional Home Health Agency Star Ratings.** Each panel plots average quality scores from Medicare's HH Compare star ratings system for specific functional improvement measures. *Notes:* In all panels, the green line represents agencies formerly operated by the Arkansas Department of Health and acquired by Kindred, the orange line shows other agencies in Arkansas (spillovers), and the navy line shows agencies in control states. Ratings are averaged at the agency-quarter level and include all agencies operating in Arkansas and control states. Vertical dashed line denotes the implementation (2016Q3) of privatization.

C Kindred Facility Ownership and Vertical Integration

To contextualize the outcome analyses—particularly hospitalizations, nursing home admissions, and skilled nursing facility use, I verified whether Kindred Healthcare or its affiliates operated any such facilities in Arkansas during the study's post period (2016–2021). This matters for interpretation, as vertical integration could confound spillover effects or affect incentives related to patient discharge and referral patterns.

Public records and press releases confirm that Kindred did *not* operate any acute care hospitals, SNFs, or long-term acute care hospitals (LTACHs) in Arkansas during this period. Although Kindred maintained hospice agencies statewide and later entered joint ventures to build inpatient rehabilitation hospitals (e.g., Mercy Rehabilitation Hospital Fort Smith, opened in 2021), these facilities are not directly implicated in the hospitalization or SNF outcomes analyzed here. Kindred exited the skilled nursing facility business nationally in 2017 through the sale of its entire SNF portfolio to BM Eagle Holdings, a private equity-backed buyer (Skilled Nursing News, 2017). None of the facilities involved in that transaction were located in Arkansas, and Kindred had no SNF capacity in the state before or after the sale.

Thus, Kindred did not own facilities involved in downstream care decisions during the study period. This supports the interpretation that observed changes in hospital or SNF use are unlikely to reflect vertical integration or ownership-driven referral behavior.

D Conceptual Model - Extensions

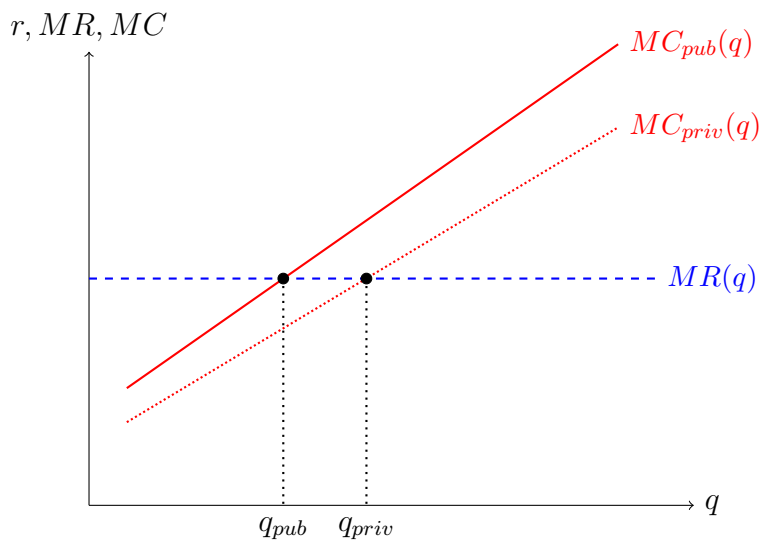


Figure D.1: **Cost Efficiency Mechanism.** Under a flat marginal revenue schedule $MR(q)$—as under fixed reimbursement—output is determined where $MR = MC$. If privatization reduces marginal costs, shifting the curve from $MC_{pub}(q)$ to $MC_{priv}(q)$, the intersection with MR moves from q_{pub} to q_{priv}, expanding output. Unlike a billing-effort mechanism that raises MR, this efficiency-based expansion reflects genuine cost reductions and increases social surplus.

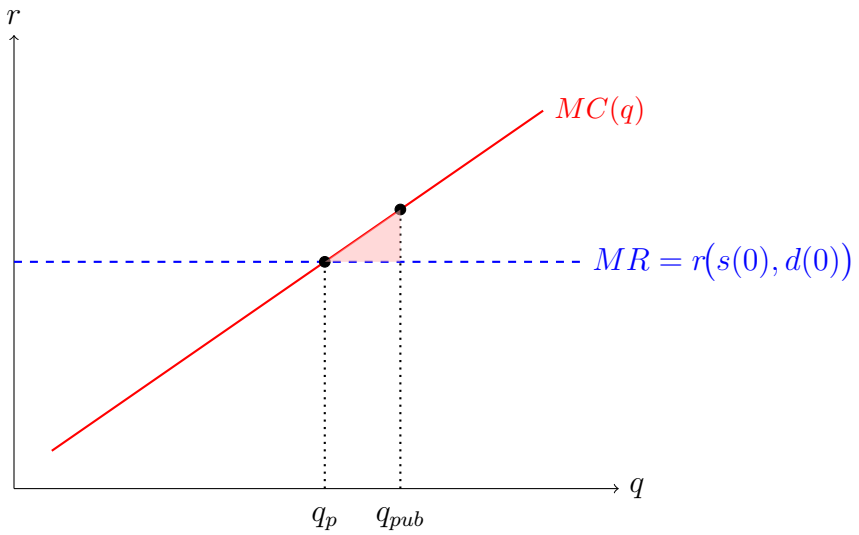


Figure D.2: **Alternative Mechanism: Mission-Driven Public Provision.** With a flat marginal revenue schedule $MR = r\big(s(0), d(0)\big)$, the profit-maximizing quantity is q_p where $MR = MC$. A mission-driven public agency may serve beyond q_p, up to q_{pub} where $MR < MC$, incurring losses. The shaded region is the implicit subsidy needed to cover those losses. Privatization removes such mission-driven provision, but higher effective revenue $r\big(s(e^*), d(e^*)\big)$ or lower $MC(q)$ could allow private agencies to treat these patients at a positive margin.

E Medicaid Expansion in Arkansas

Arkansas expanded Medicaid under the Affordable Care Act in January 2014, adopting a unique "private option" model that used federal funds to purchase private insurance plans for low-income adults. Although this policy substantially increased insurance coverage among low-income populations, its timing and structure make it unlikely to confound the effects of the 2016 home health privatization analyzed in this study.

First, the Medicaid expansion occurred more than two years prior to the ownership change. Second, the Medicare home health benefit is largely distinct from Medicaid coverage, and the population most affected by expansion—non-disabled adults under age 65—falls largely outside the traditional home health beneficiary pool, which is overwhelmingly elderly and enrolled in Medicare. Third, the difference-in-differences models include state-by-year or year fixed effects that account for broad temporal changes in state policy environments, including Medicaid expansion. Finally, subgroup analyses restricted to dual-eligible beneficiaries—who are more likely to be affected by Medicaid policy—if anything show smaller effects compared to the full fee-for-service Medicare population in Arkansas (see Section 5.2). Taken together, these considerations suggest that Medicaid expansion in Arkansas is unlikely to bias the estimated effects of home health privatization.

F Decomposing Fiscal Effects of Privatization

This appendix provides a back-of-envelope calculation to decompose the total increase in Medicare spending following privatization into inframarginal effects (higher payments for existing patients) and marginal effects (payments for newly served patients).

Given information from the main analysis:

- Pre-privatization average episode payment: \$1,837
- Post-privatization average episode payment: \$2,323 (\$1,837 + \$490)
- Increase in home health users: 9.1%

Let q_0 denote the pre-privatization number of users and q_e denote the post-privatization number, where $q_e = 1.091 \times q_0$. Let $p(0) = \$1,837$ denote the pre-privatization payment and $p(e^*) = \$2,323$ denote the post-privatization payment.

The total change in Medicare spending can be decomposed as:

$$\begin{aligned}
\Delta E &= p(e^*) \times q_e - p(0) \times q_0 && (7)\\
&= p(e^*) \times q_e - p(0) \times q_0 && (8)\\
&= \$2,323 \times (1.091 \times q_0) - \$1,837 \times q_0 && (9)\\
&= \$2,534.4 \times q_0 - \$1,837 \times q_0 && (10)\\
&= \$697.4 \times q_0 && (11)
\end{aligned}$$

This total change can be further decomposed into:
Inframarginal effect (higher payments for existing patients):

$$[p(e^*) - p(0)] \times q_0 = (\$2,323 - \$1,837) \times q_0 = \$490 \times q_0 \quad (12)$$

Marginal effect (payments for new patients):

$$p(e^*) \times (q_e - q_0) = \$2,323 \times (0.091 \times q_0) = \$211.4 \times q_0 \quad (13)$$

Relative proportions:

$$\text{Inframarginal share} = \frac{\$490 \times q_0}{\$697.4 \times q_0} = 69.7\% \quad (14)$$

$$\text{Marginal share} = \frac{\$211.4 \times q_0}{\$697.4 \times q_0} = 30.3\% \quad (15)$$

This decomposition shows that approximately 70% of the increased Medicare spending following privatization reflects higher payments per existing patient, driven by service mix reallocation and coding intensity, while about 30% reflects payments for newly served patients. The welfare implications of the 70% depend on whether the service mix changes generated clinical benefit for inframarginal patients.