The Impact of Hospital-Sponsored Health Plans on Readmission Rates

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Abstract

Provider-sponsored health plans (PSHPs) are a form of vertical integration (VI) between health care financing (plans) and health care delivery systems (hospitals). This paper studies the impact of PSHP on 30-day unplanned readmissions risk among Medicare Advantage beneficiaries and distinguishes between two margins of integration: treatment at an integrated hospital (VI-hospital) and enrollment in an integrated plan (VI-plan). We address both patient selection and supply side selection into VI status. To address patient selection, we adopt an identification strategy the leverages quasi-experimental variation in hospital assignment and plan enrollment. To address hospital selection into VI status, we include hospital and plan fixed effects, and exploit within hospital variation in the VI status over time. We find that in markets where hospital competition is stronger, patients admitted to an integrated hospital-plan setting have 9.5 pp lower readmission risk relative to those admitted to standard hospitals, and have also shorter length of inpatient stay. Identification of the impact of enrollment into a VI plan is weaker since we cannot include plan fixed affect as there is no within plan variation in the VI status. After controlling for the admitting hospital, we do not find evidence that enrollment in a vertically integrated health plan reduces readmission risk. Together, these findings indicate that, in competitive hospital markets, this lower readmission risk operate through hospital-wide care practice changes at an integrated hospital rather than through the enrollment in an integrated plan.

Keywords: Hospital Readmission, Provider-Sponsored Health Plans, Vertical Integration, Medicare Advantage.

1 Introduction

Provider-sponsored health plans (PSHPs) are health plans sponsored by healthcare providers and represent a form of integration between healthcare financing (the plan side) and healthcare delivery (the provider side). Proponents argue that PSHPs align incentives between payers and providers, with potentially positive effects on efficiency and quality. Critics emphasize that provider–insurer integration may also increase market power through patient steering, weaken competitive discipline, and alter incentives in ways that do not necessarily improve patient outcomes (Meyers et al. (2017); Park et al. (2023); Meyers et al. (2020a); Gaynor et al. (2015)).

Integration between providers and insurers is particularly important in the context of Medicare Advantage (MA), where MA plans receive a capitated payment from Medicare for each enrollee. This payment structure creates strong incentives for plans to manage care utilization and discipline providers to avoid inefficient care. We focus on plans sponsored by large hospital organizations (health systems) and study how hospital–plan integration affects 30-day unplanned readmission risk (RR) among MA beneficiaries. According to the Healthcare Cost and Utilization Project (HCUP), Medicare-payer stays had about a 17.0% readmission rate in the 2016–2019 period, and Medicare's average readmission cost was $18,100, implying aggregate hospital costs of roughly $36.2 billion per year. In the context of hospital-sponsored plans, readmission rates are especially informative because hospital–plan integration changes incentives to prevent readmissions. Under standard plans, both plans and hospitals bear some of the costs of readmissions, and insurers use negotiated rates to incentivize hospitals to prevent them. For patients enrolled in a hospital-sponsored plan, instead, hospitals internalize the full returns of improving post-discharge care management to reduce the readmissions risk.

In this paper, we refer to a hospital that sponsors a plan as a vertically integrated (VI) hospital, and to a plan sponsored by a health system as a vertically integrated plan. We study two distinct treatment margins: (i) the VI-hospital margin, that is, the effect of being admitted to a VI hospital rather than a standard hospital; and (ii) the VI-plan margin, that is, the effect of being enrolled in a plan sponsored by a hospital. We also investigate the combined effect of these two margins. In other words, we consider as a separate treatment margin the effect of being enrolled in a PSHP and receiving care from the hospital that sponsors that plan.

Distinguishing these margins is important both conceptually and empirically because hospital–plan integration can operate through different mechanisms on the provider side and on the insurer side. On the provider side, a hospital that both insures and treats its

own beneficiaries may have stronger incentives to invest in discharge planning, transitional care, monitoring, and care coordination because it internalizes more of the downstream cost of avoidable readmissions under capitated payment. If these investments take the form of hospital-wide protocols or organizational changes, they can improve outcomes for all patients treated at the hospital, not only for enrollees in the system's own plan. On the plan side, the effect may operate through different channels. VI plans may steer patients toward hospitals within the sponsoring system, where the integrated organization internalizes more of the benefit from preventing readmissions. They may also negotiate rates differently with outside hospitals: because the integrated plan has better information about the true cost of rehospitalization, it may be better able to design payment arrangements or utilization-management tools that strengthen hospitals' incentives to avoid preventable readmissions.

Recent evidence on vertically integrated MA plans is suggestive, but does not establish any causal effect. The literature focuses on plan characteristics, care experiences, or broad hospitalization patterns rather than readmissions from emergency episodes. Park et al. (2023) show that integrated MA plans have higher star ratings and lower non-claims costs than non-integrated plans, although they also charge higher premiums. Bejarano et al. (2024b) document the rapid growth of integrated MA plans and show that legacy-integrated plans tend to have higher star ratings and higher premiums than both non-integrated and newer integrated plans. On the patient-experience margin, Bejarano et al. (2024a) find that integrated MA plans overall are not associated with meaningfully better care experiences than non-integrated plans, with the strongest advantages concentrated in legacy-integrated plans. Finally, Meyers et al. (2020b) study provider-integrated MA arrangements and find differences in inpatient care patterns and marginally lower mortality, but no difference in readmissions. Taken together, the existing evidence suggests that vertical integration may affect quality, costs, and care processes, but it leaves open whether hospital-sponsored plans reduce readmissions and, if so, whether that effect operates through the hospital side, the plan side, or both.

We contribute to this literature by implementing an empirical strategy that separates hospital-side and plan-side effects on readmission risk. Estimating the treatment effects of these two margins requires addressing two conceptually distinct sources of endogeneity. The first is patient-side selection: beneficiaries may sort across hospitals and health plans based on unobservable health status that is correlated with readmission risk. We adopt an instrumental-variables design tailored to each margin. For the hospital-treatment margin, we follow the logic of Doyle Jr et al. (2015) and construct an ambulance-company-based instrument that exploits ambulance referral patterns in emergency episodes. For the plan-enrollment margin, we leverage plausibly random variation in the county-level availability

of VI plans across county borders. This logic is related to Shapiro (2022), who uses discontinuities in advertising exposure at television-market borders to obtain quasi-experimental variation in consumer information.

The second source of endogeneity is supply-side selection. Hospitals that differ in the VI status may differ in other quality dimension, that affect readmission risk. We include hospital fixed effects to absorb unobservable time-invariant differences across hospitals. Identification of the effect of admission to a VI hospital relative to a standard hospital therefore comes from within-hospital changes over time in VI status. Similarly, VI plans may differ systematically from non-VI plans along dimensions that are not fully observed. Since plan VI status is effectively time-invariant within plan, plan fixed effects are not feasible because they would absorb the treatment variable. As a result, the hospital-VI estimates admit a stronger causal interpretation than the VI-plan estimates, which should be interpreted more cautiously as evidence on the outcomes associated with instrument-induced enrollment into VI plans, conditional on admitting hospital and observed plan characteristics.

A large literature in health economics and industrial organization shows that hospital performance and quality incentives depend on market structure. Competition can strengthen hospitals' incentives to invest in quality and operational performance, while high concentration can weaken those incentives or allow organizations with market power to capture rents without delivering commensurate quality improvements. Kessler and McClellan (2000), Gaynor et al. (2015), Cooper et al. (2022), and Chandra et al. (2016) all emphasize the importance of competition for provider performance. Our paper also studies heterogeneity in the effect of vertical integration across markets with different levels of hospital competition. We do so by estimating our specifications separately in the full estimation sample and in the subsample of unconcentrated hospital markets.

We present two sets of empirical evidence. First, we use a long panel dataset of hospital-level readmission rate for Medicare Fee-for-Service beneficiaries. This setting allows us to avoid the endogeneity concerns due to plan selection, but it does not consent us to control for patient selection across hospitals. The results of the difference-in-difference design show that hospital VI-status adoption reduces readmission rates. We also document that hospital that later become a plan sponsor are not following a different pre-trend compared to standard hospitals.

The second set of evidence uses beneficiary level claims and enrollment data. This framework allow us the implement our IV design to control for patient selection across hospitals and plans, but also introduce another source of endogeneity that stems from unobservable plan-side effects. A key assumption that applies to both the DiD analysis and the IV framework is that the timing of the hospital VI-status adoption is exogenous to broader organizational

changes concurrent with the timing of plan sponsorship.

This second framework provides the main results of the paper. We construct a pooled sample of two years of administrative data on Medicare Advantage beneficiaries. We observe beneficiaries' plan enrollment and use the AHRQ's Compendium of Health Systems to determine whether an MA plan is sponsored by a hospital organization. We use MA claims-level data to obtain information on patient location, the category of the emergency event, ambulance transportation, the admitting hospital, the principal diagnosis, inpatient length of stay, and the 30-day all-cause unplanned readmission event. We implement an IV design embedded in a Two-Way Fixed Effect linear probability model.

After accounting for patient selection, we find little evidence of an average hospital-side effect in the full sample. However, once we focus on hospitals operating in unconcentrated markets, patients admitted to a plan-sponsoring hospital have a 9.57 percentage point lower readmission probability than those admitted to standard hospitals. This pattern is robust to a stricter definition of vertically integrated hospitals based on sponsor-plan enrollment relative to bed capacity. We also find that VI hospitals have shorter inpatient length of stay, suggesting that the readmission effects reflect more efficient discharge planning, care coordination, or post-acute management rather than longer inpatient stays.

The plan-side identification are weaker without plan fixed effects and suggests a pattern consistent with advantageous selection into vertically integrated plans. Conditioning on the admitting hospital, we find no evidence the enrollment in a VI plan significantly changes the readmission risk. Similarly, we find no evidence that beneficiaries who are both enrolled in a VI plan and treated at the sponsoring hospital experience an additional improvement beyond the effect of being treated at a VI hospital. While we cannot rule out plan-side effect, these findings suggest that the main benefits of hospital-plan integration is driven by hospital-wide changes in care delivery, especially in more competitive hospital markets, rather than by differential treatment of patients based on plan enrollment.

An important limitation of our empirical design is that its most credible identification comes from the subset of high-acuity, effectively non-deferrable emergency admissions. As in Doyle Jr et al. (2015) and subsequent ambulance-based applications such as Cooper et al. (2022), the identifying variation comes from pre-hospital ambulance assignment and routing, which are most plausibly exogenous when patients have little discretion over the timing and location of care. The resulting estimates therefore speak most directly to emergency inpatient episodes. They need not generalize one-for-one to elective admissions, chronic disease management, or the broader universe of inpatient utilization. Nevertheless, emergency episodes account for roughly 20 to 25% of all hospital admissions and are associated with some of the highest expected inpatient costs. The restriction to this sample therefore still

captures a quantitatively important and policy-relevant segment of hospital care, even if the resulting estimates should not be interpreted as applying mechanically to the full universe of admissions.

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting, the data used in the estimation, and the descriptive statistics. Section 3 discusses the sources of endogeneity and the instrumental variables. Section 4 presents evidence using hospital-level data on Fee-for-Service beneficiaries. Section 5 presents the main empirical setting and shows the evidence using MA beneficiaries claims and enrollment data. Section 6 concludes.

2 Institutional Setting and Data

This section describes the institutional environment of Medicare Advantage (MA), the role of provider-sponsored plans, and the data used in the empirical analysis. We first discuss the growth of vertically integrated provider-sponsored health plans and how these organizations combine payer and provider functions. We then explain why hospital readmissions provide a useful outcome for evaluating the effects of hospital–plan integration. Finally, we describe the administrative datasets used in the analysis and present descriptive statistics on readmission rates and patient episodes across different integration settings.

2.1 Medicare Advantage and Provider-Sponsored plans

Medicare Advantage (MA) is the alternative to traditional Traditional Medicare. Medicare advantage beneficiaries enroll in health plans offered by private insurance companies. Private plans are paid by Medicare a capitated amount per-beneficiary per month to provide coverage for TM Part A and Part B benefits. Plans are differentiated in terms of provider network and coverage, and negotiate with providers the payment rates for each service. Provider-sponsored health plans (PSHPs) are becoming a prominent feature of the Medicare Advantage (MA) market. PSHPs are vertically integrated entities in which a provider organization owns and operates its own insurance plan to provide healthcare services. This integration combines the payer-side (insurance) and provider-side (hospitals, doctors) functions within a single organization. Common ownership may align incentives across payer and provider, reducing transaction costs and enabling clinical integration, potentially raising quality and efficiency. Yet integration can also enable patient steering and increased market power. This vertically integrated organization often takes the form of a health system, but there are instances of health plans that are sponsored by provider organizations

that do not include hospitals. In this paper we will exclusively focus on plans sponsored by health systems. We will refer to hospitals that sponsor a plan as vertically integrated hospitals and their sponsored plans as VI plans.

2.2 Hospital readmission and hospital-plan integration

This paper focuses on hospital readmissions as a clinically and financially meaningful measure to evaluate the impact of vertically integrated plans. Recent evidence indicates that a substantial share of acute admissions are followed by a rehospitalization within 30 days, with aggregate spending associated with readmissions on the order of tens of billions of dollars annually in Medicare.[1] Readmissions are financially consequential for Medicare and hospitals, and they are explicitly targeted by Medicare's Hospital Readmissions Reduction Program (HRRP) [2].

In non-integrated arrangements, the insurer bears the marginal claims cost of rehospitalizations, while the hospital is largely reimbursed per admission and may not fully internalize downstream utilization costs that occur after discharge. When a hospital system sponsors an MA plan, the integrated entity captures a larger share of the surplus from preventing avoidable rehospitalizations, which strengthens incentives to invest in activities that require coordination across settings—for example, discharge planning, timely outpatient follow-up, medication reconciliation, and post-acute care management. Integration can also lower transaction and information frictions (e.g., shared data systems and unified care-management protocols), making these investments easier to implement at scale.

Readmissions are therefore an informative outcome for evaluating hospital–plan integration as they are widely viewed as a sentinel measure of care transitions and post-discharge management: they are sensitive to failures in communication, follow-up, and coordination with outpatient and post-acute providers, and they are financially consequential for both payers and hospitals. In addition, readmissions are a central target of Medicare quality policy through the Hospital Readmissions Reduction Program (HRRP), which explicitly frames communication and care coordination around discharge as channels for reducing rehospitalizations. These features make readmissions a natural metric for assessing whether PSHPs improve the efficiency of care coordination at the hospital–plan interface.

This focus is also consistent with the integrated-MA literature. Meyers, Mor, and Rahman (2020) motivate provider-integrated MA plans as a mechanism for improving coordination across payer and provider and evaluate differences in hospitalization processes and post-

[1] See, e.g., Ziedan and Kaestner (2021) for summary statistics and policy discussion.

[2] CMS evaluates hospital readmission performance over a multi-year window and applies payment reductions to hospitals with "excess" readmissions relative to national benchmarks. See Gupta (2021).

hospital outcomes including unplanned readmissions. Park, Langellier, and Meyers (2022) document that integrated MA plans differ from non-integrated plans along several dimensions, including the provision of supplemental benefits directly related to transitional care (e.g., post-discharge in-home medication reconciliation and readmission-prevention benefits). Related work on MA integration and beneficiary experiences emphasizes care coordination as a key dimension along which integrated arrangements may differ from non-integrated plans (Bejarano et al. 2024).

2.3 Data

Medicare Advantage Claims Data We use administrative data from the Centers for Medicare & Medicaid Services (CMS) for the years 2019 and 2022. The main data source for plan enrollment is the Master Beneficiary Summary File (MBSF), which contains individual-level data on a 5% random sample of Medicare beneficiaries. The MBSF records enrollment information for each beneficiary, including whether they are enrolled in Traditional Medicare or a Medicare Advantage (MA) plan.

The MBSF also includes detailed beneficiary characteristics such as age, gender, race, income measures, and county of residence. In 2019, total Medicare enrollment was approximately 61 million, so the 5% sample includes about 3 million individuals. Roughly 37% of beneficiaries were enrolled in MA plans. We exclude ESRD patients from the analysis and also special needs plans, pace plans, employer-sponsored plans or PFFS plans which have a different reimbursement scheme from CMS which is different from the standard plans.

The main data source for patients' acute-care utilization is the Inpatient Encounter Data. These files identify the admitting hospital (CCN) and, for ambulance-transported episodes, the ambulance provider. They include ICD-10 diagnosis and procedure codes, length of stay, and discharge disposition, which we use to construct episode definitions, risk adjusters, and readmission outcomes. In addition, we use the Inpatient, Outpatient, Skilled Nursing Facility (SNF), Home Health Agency (HHA), Durable Medical Equipment (DME), and Carrier Encounter files to measure broader utilization and to construct spending-based measures in supplementary analyses.

Provider-sponsored health plans and VI hospitals. To identify integrated MA plans sponsored by health systems, we use the AHRQ Compendium of Health Systems, which contains the list of all health plans offered by hospitals and health systems. We link the hospitals in the Compendium to their CMS provider identification numbers (CCN) in order to merge the VI classification to hospital-level datasets (including the AHA Annual Survey for hospital characteristics). To identify health plans sponsored by other provider organizations,

we additionally search through the full list of MA contracts and flag those whose sponsor is a provider organization. Throughout the descriptive analysis, we classify a hospital as vertically integrated in a given year if it sponsors at least one plan in that year. In the empirical section, we also consider a more robust definition of hospital VI integration status based on the number of beneficiaries enrolled in the hospital-plan relative to the capacity of the hospital.

Readmission measure We use the claims data to compute readmission at the individual level. A patient experiences an *unplanned* acute inpatient readmission if within 30 days of discharge from the *index* emergency admission, the patient is readmitted to an hospital for the same condition. We treat multiple rehospitalizations within the 30-day window as a single event, consistent with HRRP-style targeted measures, which are most naturally interpreted as readmission probabilities rather than counts.[3] Following CMS-style measurement conventions, we exclude same-day transfers and implement a planned-readmission algorithm to remove rehospitalizations that are predictably scheduled (e.g., selected elective procedures) rather than reflecting failures of post-discharge management.[4] Because our data are Medicare Advantage encounters, we can track readmissions to *any* acute-care hospital, not only the index hospital.

2.4 Summary Statistics

We begin by presenting descriptive statistics for both the estimation sample and the full inpatient sample. Our estimation sample is restricted to the emergency episodes and is the one used in the hospital-side instrumental-variables design, while the broader full inpatient sample is useful for assessing how restrictive that sample selection is in practice. Across both samples, the raw readmission patterns are remarkably similar: the overall 30-day unplanned readmission rate is 14.5 percent in the emergency sample and 14.5 percent in the full sample. This comparison is useful because it shows that the emergency restriction does not mechanically generate an unusual readmission environment.

Readmission Rates Table 1 reports mean 30-day all-cause unplanned readmission rates by the vertical integration status of hospitals and plans. The top panel reports the emergency-episode sample, and the bottom panel reports the broader inpatient sample. Two patterns

[3] Gupta (2021) notes that CMS does not differentiate between one and multiple readmissions within the 30-day period when constructing targeted readmission metrics.

[4] Ziedan and Kaestner (2021) describe the 30-day all-cause framework and common exclusions used in HRRP-style definitions.

are clear and they are nearly identical across the two panels. First, the readmission rates are essentially the same in vertically integrated and non-vertically integrated hospitals in both samples. Second, beneficiaries enrolled in vertically integrated plans have substantially lower unconditional readmission rates than beneficiaries enrolled in non-integrated plans.

Table 1: Readmission Rates by Vertical Integration

	Plan Integration		
Hospital Integration	Non-VI	VI	Total
Emergency episodes sample			
Non-VI Hospitals	14.7	12.1	14.5
VI Hospitals	14.9	13.2	14.5
Total	14.8	12.8	14.5
Full inpatient sample			
Non-VI Hospitals	14.7	12.1	14.5
VI Hospitals	14.9	13.2	14.5
Total	14.7	12.8	14.5

Notes: Entries report mean 30-day all-cause unplanned readmission rates (in percent). The top panel uses the emergency-episode estimation sample. The bottom panel uses the broader inpatient sample.

Looking at the joint cells in Table 1, the lowest readmission rates occur for beneficiaries enrolled in vertically integrated plans but treated at non-integrated hospitals, while the highest rates occur for beneficiaries in non-integrated plans treated at vertically integrated hospitals. Descriptively, then, the unconditional association appears stronger on the plan margin than on the hospital margin. Patient sorting across hospitals and plans may bias this raw comparison. We address this concern in the empirical section using an IV design.

Table 2 reports the same readmission comparisons across levels of hospital market concentration. Again, the emergency and full-sample panels look very similar. Raw readmission rates are highest in unconcentrated markets, lower in moderately concentrated markets, and lowest in highly concentrated markets. Across all concentration bins, vertically integrated plans continue to exhibit lower average readmission rates than non-integrated plans. By contrast, the raw association between admission to a vertically integrated hospital and readmission is not stable across market structures. In unconcentrated markets, readmission rates are slightly higher at VI hospitals than at non-VI hospitals, whereas in highly concentrated markets they are slightly lower. This instability in the unconditional hospital-side comparison suggests that simple raw differences are unlikely to isolate the causal effect of hospital integration. The empirical section therefore turns to an IV design that addresses patient

Table 2: 30-Day Unplanned Readmission across Market Concentration

Market Concentration	Hospital Integration		Plan Integration	
	Non-VI	VI	Non-VI	VI
Emergency episode sample				
HHI $<$ 1500	15.4	15.9	16.2	12.8
1500 $\leq$ HHI $<$ 2500	15.1	15.1	15.1	14.6
HHI $\geq$ 2500	14.1	13.7	14.2	12.3
Full inpatient sample				
HHI $<$ 1500	15.4	15.9	16.1	12.8
1500 $\leq$ HHI $<$ 2500	15.1	15.0	15.1	14.6
HHI $\geq$ 2500	14.1	13.7	14.2	12.3

Notes: Each cell reports the mean 30-day all-cause unplanned readmission rate (in percent). Market concentration is measured using county-level hospital HHI.

sorting into hospitals.

Distribution of Patient Admissions Table 3 reports how admissions are distributed across plan and hospital integration categories. In the emergency sample (top panel), 68.4 % of admissions for beneficiaries in non-integrated plans occur in non-integrated hospitals, while the remaining 31.6 % occur in hospitals that are vertically integrated with some other plan. For beneficiaries enrolled in vertically integrated plans, 44.5 % of emergency admissions occur at the sponsoring hospital, 19.2 % occur at a different vertically integrated hospital, and 36.4 % occur at non-integrated hospitals. Thus, even in emergency episodes, vertically integrated plan enrollees are more likely to be treated within integrated systems than non-VI enrollees, but a substantial share of their admissions still occurs outside the sponsoring hospital.

The full inpatient sample panel (bottom panel) points to an important external-validity distinction. In the full sample, admissions of beneficiaries enrolled in vertically integrated plans are much less concentrated at the sponsoring hospital: only 9.2 % of admissions are fully integrated, while 86.9 % occur at non-integrated hospitals. In other words, the emergency-episode sample and the full inpatient sample look quite similar in unconditional readmission rates, but they differ more noticeably in where vertically integrated plan enrollees receive care. This is one reason to be careful when extrapolating the emergency-based causal estimates to all hospital admissions.

Table 4 reports the distribution of admissions across integration categories by hospital market concentration. In the emergency sample, integrated arrangements are less prevalent

Table 3: Distribution of Admissions by Vertical Integration

Plan Integration	Hospital Integration Non-VI	VI	VI-Full	Total
Emergency episode sample				
Non-VI	127,381	58,978	0	186,359
	68.35%	31.65%	0.00%	100.00%
VI	10,143	5,341	12,406	27,890
	36.37%	19.15%	44.48%	100.00%
Total	137,524	64,319	12,406	214,249
	64.19%	30.02%	5.79%	100.00%
Full inpatient sample				
Non-VI	127,976	59,231	723,111	910,318
	14.06%	6.51%	79.43%	100.00%
VI	118,088	5,361	12,430	135,879
	86.91%	3.95%	9.15%	100.00%
Total	246,064	64,592	735,541	1,046,197
	23.52%	6.17%	70.31%	100.00%

Notes: Each cell reports the number of admissions and its percentage of the total. Hospital Non-VI denotes non-integrated hospitals, Hospital VI denotes hospitals vertically integrated with some plan but not the beneficiary's own plan, and Hospital VI-Full denotes hospitals vertically integrated with the beneficiary's own plan. Plan Non-VI denotes non-integrated plans and Plan VI denotes vertically integrated plans.

in more concentrated markets: the share of emergency episodes occurring in non-integrated hospitals rises from 57.4 % in unconcentrated markets to 67.8 % in highly concentrated markets, and the share of admissions in fully integrated hospitals falls from 8.7 % to 4.3 %. The same broad message carries over to the full sample: integrated arrangements are less common in more concentrated markets, although the broader inpatient sample exhibits a substantially different overall composition than the emergency sample. Together, these descriptive statistics suggest that hospital–plan integration is not distributed uniformly across markets: integrated arrangements are less common in more concentrated hospital markets, and admissions in those markets are less likely to occur in fully integrated settings.

Sample restriction, limitations, and generalizability The hospital-side instrumental-variables design relies on ambulance referral patterns, so the main estimation sample is restricted to high-acuity, effectively non-deferrable emergency admissions. This follows the logic in Doyle Jr et al. (2015) and related ambulance-based designs: the identifying assumption is most credible when pre-hospital assignment and ambulance routing, rather than

Table 4: Distribution of Admissions across Market Concentration

Market Concentration	Hospital Integration			Plan Integration	
	Non-VI	VI	VI-Full	Non-VI	VI
Emergency episode sample					
HHI $<$ 1500	57.36	33.98	8.66	84.28	15.72
1500 $\leq$ HHI $<$ 2500	58.24	33.34	8.42	85.56	14.44
HHI $\geq$ 2500	67.76	27.98	4.26	88.19	11.81
Full inpatient sample					
HHI $<$ 1500	26.94	7.93	65.13	82.76	17.24
1500 $\leq$ HHI $<$ 2500	21.60	6.21	72.18	86.57	13.43
HHI $\geq$ 2500	22.97	5.66	71.37	88.35	11.65

Notes: Each cell reports the number of admissions and its percentage of the total, separately for each market-concentration category. Hospital Non-VI denotes non-integrated hospitals, Hospital VI denotes hospitals vertically integrated with some plan but not the beneficiary's own plan, and Hospital VI-Full denotes hospitals vertically integrated with the beneficiary's own plan. Plan Non-VI denotes non-integrated plans and Plan VI denotes vertically integrated plans.

patient discretion, largely determine the admitting hospital. Subsequent work using the same design likewise focuses on emergency settings for exactly this reason (Cooper et al., 2022). The advantage of the restriction is internal validity. By concentrating on acute admissions for which location choice is least discretionary, the ambulance-based instrument is more plausibly orthogonal to patient unobservables.

The tradeoff is external validity. The hospital-side estimates should be interpreted as applying most directly to acute emergency inpatient episodes rather than to elective admissions, chronic disease management, or the full universe of inpatient utilization. The descriptive evidence is informative about the scope of this limitation. On the one hand, Table 1 shows that overall readmission levels and the raw plan-versus-hospital patterns are nearly unchanged when moving from the emergency sample to the broader inpatient sample. On the other hand, Table 3 and Table 4 show that the distribution of admissions across integrated settings differs more materially in the broader sample, especially for beneficiaries enrolled in vertically integrated plans. Accordingly, the emergency restriction should be viewed as a design choice that strengthens identification, not as a claim that the same treatment effects must hold uniformly for all hospital admissions.

3 Endogeneity and Identification Strategy

There are two sources of endogeneity concerns when estimating the effect of hospital-plan integration on readmission rates. We refer to the first as supply-side selection. Hospitals and plans may differ systematically in unobservable quality which also affects the readmission rate. For example, high-quality hospitals plans may be more likely to sponsor health plans compared to low-quality hospitals. The second concern is patient selection. Patients with ex-ante different readmission probability may be more or less likely to enroll in PSHP or being treated by plan-sponsor hospitals. In this section, we discuss how we address both of these challenges.

3.1 Supply side endogeneity and identification

Hospital side To address hospital selection into plan-sponsoring, we need to control for hospital level unobserved quality. We use hospital fixed effects to absorb all time-invariant hospital attributes, including baseline quality, discharge practices, coding intensity, and persistent post-acute referral patterns. County fixed effects absorb time-invariant local conditions that may affect both hospital choice and readmission risk. Year and diagnosis fixed effects absorb common shocks and differences in clinical composition.

Under the standard parallel trend assumption, we can identify the impact of VI hospital in a difference in difference design with hospital and year fixed effects. In this setting identification comes from within-hospital changes in plan integration status over time, net of aggregate time trends. Table 5 shows one of our key identifying variations. It summarizes the distribution of hospitals by their vertical integration status over time. A majority of hospitals (64%) are never vertically integrated during the sample period, while 20% remain vertically integrated throughout. Importantly, 16% of hospitals switch their vertical integration status at least once over time. This group of "switchers" provides the key source of identification in our difference-in-differences framework with hospital and year fixed effects, as it reflects within-hospital changes in integration status.

Table 5: Hospital Vertical Integration Status Over Time

	Frequency	Percentage (%)
Always VI	555	19.97
Always non-VI	1,776	63.91
Switchers	448	16.12
Total Hospitals	2,779	100.00

However, patient selection may still pose a concern if the composition of patients changes

in response to plan-integration. In particular, if healthier patients differentially sort into vertically integrated settings following changes in integration status, estimates may remain biased. To address this concern, we complement our difference-in-differences strategy with an instrumental variable approach. In section 3.2, we describe instrument we use to account for patient selection into hospitals.

Another endogeneity concern is that hospitals may adopt VI status as part of a broader organizational change—for example, improvements in care management, discharge planning, or system integration—that begins before formal adoption. In subsection 4.2 we present a validation exercise using hospital-level fee-for-service (FFS) hospital-episode-year data from 2016–2023. The scope of this exercise is to check whether plan-sponsor hospitals and standard hospitals were on different readmission trends prior to the hospital's VI adoption event.

Plan side The supply side endogeneity of PSHP stems from the fact that the latter may differ systematically from standard plans along unobserved quality dimensions. Ideally, one would address this by including plan fixed effects. However, this is not feasible in our setting because vertically integrated plans exists only after hospital-plan integration, leaving no within-plan variation in vertical integration status to exploit. As a result, we are unable to separately identify the effect of being enrolled in a PSHP from time-invariant unobservables using plan fixed effects.

A second concern is that beneficiaries may sort into provider-sponsored plans based on unobserved health, preferences for integrated delivery systems, anticipated utilization, or differential access to providers. These unobservables can directly affect readmission risk and may therefore bias our estimation results. We address patient selection into plans using an instrumental variable strategy. In the next section, we describe instrument we use to account for patient selection across plans.

The estimated effect of enrollment in a hospital-sponsored plan should not be given a fully causal interpretation since we are not able to control for plan-level unobservables. Thus, our estimates may reflect a combination of the effects of hospital-plan integration and residual differences in unobserved plan quality.

3.2 Instruments for Patient Selection

This section presents the instruments we use to address the possibly non-random selection of beneficiaries between hospitals and plans.

Ambulance-based instrument for hospital assignment In general, patients' choice over hospitals depends unobserved hospital quality which might also be correlated with read-

mission probability. However, in emergency episodes, the admitting hospital is heavily influenced by pre-hospital factors such as ambulance dispatch, ambulance availability, and patient location. Patients typically do not choose the ambulance company, and dispatch/rotation protocols together with mutual-aid agreements generate plausibly exogenous assignment to ambulance companies with heterogeneous hospital destination preferences (Doyle Jr et al., 2015). At the same time, ambulance companies often have stable tendencies to deliver patients to particular hospitals, reflecting historical routing patterns, contracts, operational convenience, or local familiarity.

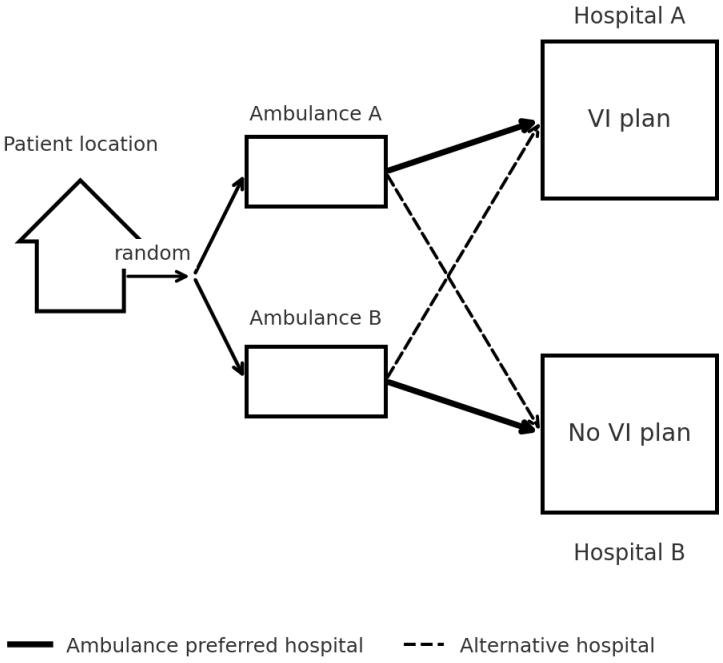

Figure 1: Hospital Assignment and Ambulance Preferences

Figure 1 illustrates the key source of quasi-random variation in hospital assignment. Two ambulances serve the same geographic area but differ in their propensity to transport patients to different hospitals. If one ambulance is systematically more likely to deliver patients to a VI hospital than another, then patients who are quasi-randomly assigned to those ambulances differ in their probability of admission to a VI hospital even though they face similar emergencies and originate from the same local area.

Let H_i be the indicator for *admission to a vertically integrated hospital* for beneficiary-episode i, and let $a(i)$ identify the ambulance company serving episode i for diagnosis cate-

gory $d(i)$. We construct the leave-one-out ambulance-level VI-hospital share:

$$Z_i^{A,H} = \frac{1}{N_{a(i),d(i),t(i)} - 1} \sum_{\substack{j: a(j)=a(i) \\ d(j)=d(i)}} H_j.$$

Thus, $Z_i^{A,H}$ is the leave-one-out share of episodes with the same ambulance company, diagnosis group, and year that are admitted to a VI hospital. Conditioning on diagnosis is useful because ambulance routing patterns may differ across emergency conditions.

The identifying intuition is that, among otherwise similar patients facing similar emergencies, assignment to an ambulance with a higher propensity to transport patients to VI hospitals raises the probability of admission to a VI hospital while remaining plausibly orthogonal to patient unobservables conditional on location and the control set.

Integrated plans availability across county borders To address beneficiaries selection into provider-sponsored plans, we first exploit the institutional fact that Medicare Advantage plans are offered at the county-year level: beneficiaries may enroll only in plans filed for their county of residence. Thus, otherwise similar beneficiaries living in close geographic proximity but on different sides of a county border may face different menus of provider-sponsored plans.

Let $c(i)$ and $z(i)$ denote beneficiary i's county and ZIP code, respectively. Wecompute a county-year PSHP availability index as the number of PSHP plans per thousand MA beneficiaries in county c and year t, scaled to lie in $[0, 1]$, and denote it by

$$Z_i^{A,P} \equiv \text{Avail}_{c(i),t(i)} \equiv \frac{\#\text{ VI plan in county } c(i) \text{ year } t(i)}{\#\text{ plans in county } c(i) \text{ year } t(i)}.$$

The key identifying variation is that beneficiaries who live in the same ZIP code, or in ZIP codes very close to a county border, can face different county-level MA plan menus. Wetherefore restrict attention to ZIP codes that either span multiple counties or lie within a short distance of a county border.

Figure 2 illustrates this source of variation. Beneficiaries living in close geographic proximity may face different PSHP choice sets simply because they reside in different counties. This institutional feature generates plausibly exogenous variation in the feasibility of enrolling in a VI plan.

Differential distance as a common geographic shifter I additionally use a geographic shifter based on relative proximity to VI and non-VI hospitals. Let $\text{dist}(r(i), h)$ denote distance from beneficiary i's residence (or pickup location) $r(i)$ to hospital h. Define the

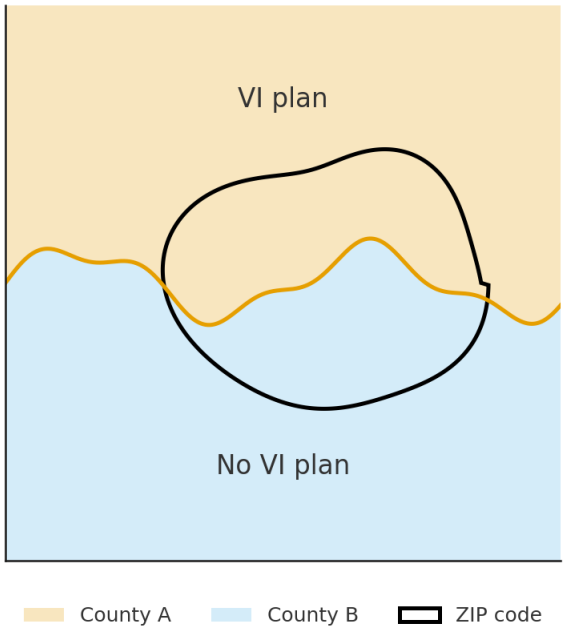

Figure 2: Within-ZIP variation in PSHP availability across county borders

nearest VI hospital $h^{VI}(i)$ and the nearest non-VI hospital $h^{NVI}(i)$, and construct

$$Z_i^{DD} \equiv \ln \text{dist}(r(i), h^{VI}(i)) - \ln \text{dist}(r(i), h^{NVI}(i)).$$

More negative values of Z_i^{DD} indicate greater relative proximity to a VI hospital.

This same differential-distance measure is useful for both endogenous margins, but with different interpretations. First, in the hospital-assignment equation, Z_i^{DD} shifts the probability that an emergency episode is delivered to a VI hospital, because distance is a first-order determinant of hospital destination in emergency care. Second, in the plan-enrollment equation, Z_i^{DD} shifts the relative attractiveness of enrolling in a provider-sponsored plan, because beneficiaries value proximity to in-network hospitals and providers. In that setting, proximity to a VI hospital system makes the affiliated plan more attractive. Thus, the same geographic shifter enters both the hospital and plan instrument sets, but its identifying role differs depending on whether the endogenous regressor is hospital assignment or plan enrollment.

3.3 Excluded instruments

Combining the margins above, the excluded instrument set for admission to a VI hospital is

$$Z_i^H \equiv \left(Z_i^{A,H},\, Z_i^{DD} \right).$$

The first component captures ambulance-specific destination preferences; the second captures relative geographic proximity to VI versus non-VI hospitals. The identifying assumption is that, conditional on the control set and fixed effects, the ambulance preference shifter and differential distance affect readmissions only through admission to a VI hospital. This assumption is most plausible in high-acuity emergency episodes, where patient discretion over the timing and location of care is limited.

A common stability assumption applies to the ambulance-based instruments used in this paper: conditional on diagnosis, geography, and the fixed effects, ambulance companies' destination preferences do not themselves change in response to a hospital becoming a plan sponsor in a way that is directly related to post-discharge outcomes. Put differently, hospital adoption of plan sponsorship do not induce ambulance providers to re-optimize routing toward that hospital for reasons other than the treatment margin being instrumented. This assumption is most plausible in high-acuity emergency episodes, where ambulance destination patterns are shaped primarily by operational protocols, capacity, and geography rather than by insurer ownership.

The excluded instrument set for enrollment in a VI plan is

$$Z_i^P \equiv \left(Z_i^{A,P},\, Z_i^{DD} \right).$$

The first component captures county-level PSHP availability; the second captures the relative convenience of proximity to a VI hospital system. For the plan-enrollment equation, the identifying assumption is that, conditional on beneficiary controls, plan characteristics, fixed effects, and admitting hospital, county-level PSHP availability and differential distance affect readmissions only through VI-plan enrollment. The hospital fixed effects in the plan regression are particularly important: they ensure that identification does not come from VI-plan enrollees being treated at systematically different hospitals on average. Rather, the design compares beneficiaries treated at the same hospital but facing different plan-enrollment incentives.

3.4 Estimation samples

The estimation samples differ across the hospital and plan analyses because the identifying variation is different in each case. For the hospital-treatment margin, we restrict attention to high-acuity, effectively non-deferrable emergency conditions—such as acute myocardial infarction, stroke, and hip fracture—for which the index admission is largely outside the patient's control. This sharpens the plausibility of the ambulance-based assignment design.

The emergency estimation sample spans a broad set of diagnosis categories. The most

common groups are circulatory-system conditions (23.25%), infectious diseases (17.12%), injuries and trauma (13.57%), respiratory-system conditions (10.77%), and digestive-system conditions (8.86%). Additional episodes come from genitourinary conditions (6.87%), endocrine disorders (4.48%), and nervous-system conditions (4.00%), with the remaining 11.08% classified as other diagnoses. Thus, while the empirical design is built around emergency and non-deferrable admissions, the estimation sample is not limited to a single clinical condition; rather, it covers a broad range of acute diagnoses for which readmission is a meaningful indicator of inpatient quality and post-discharge care coordination.

For the VI-plan analysis using the county-availability design, we restrict the sample to ZIP codes that either span multiple counties or lie within a short distance of a county border. This ensures that the identifying variation comes from geographically proximate beneficiaries facing different county-level MA plan menus. For specifications that use differential distance, we additionally require non-missing residential location information sufficient to compute distances.

4 Evidence from Hospital-level FFS data

Before turning to MA beneficiary-level analysis, I present evidence using a panel dataset on hospital-level for Fee-for-Service (FFS) beneficiaries from 2013 to 2023. We implement a difference-in-difference design to estimate the impact of hospital VI status on readmission rate for different admission diagnosis. This empirical framework has two main strengths. First, since FFS beneficiaries are covered by Traditional Medicare, endogeneity threat related to plan side selection does not apply. Secondly, having a longer panel allows us to both exploit more variation in hospital VI status over time, and also to test for differential pre-trends in readmission rates among VI hospitals and standard hospitals.

However, this design has two important limitations. First, aggregate hospital level data do not allow us to control for unobservable patient selection across hospital. Secondly, the introduction of a sponsored plan may still indirectly affect the composition of the observed FFS population through switching between Traditional Medicare and Medicare Advantage or through broader changes in referral patterns and system organization. For this reasons, we consider this analysis as complementary to that of Section 5 where we use patient level data and control for patient selection using the IV design discussed in Section 3. A key assumption that applies to both the DiD analysis and the IV framework is that the timing of the hospital VI-status adoption is exogenous. In other words, we cannot rule out that hospital VI adoption may coincide with broader organizational changes—for example, improvements in care management, discharge planning, or system integration—that affect readmissions

independently of plan sponsorship itself.

4.1 Difference-in-differences design

Let h index hospitals, e episode categories, $m(h)$ the hospital's local market, and t year. Let RR_{het} is the observed 30-day unplanned readmission rate for hospital h, episode e, and year t; VI_{ht} indicates whether hospital h sponsors a Medicare Advantage plan in year t. We implement a simple Difference-in-Difference test to test whether there is evidence of hospital-plan integration affecting the readmission rates among FFS beneficiaries. We run two separate specifications: one in the full FFS sample and one in the subsample of unconcentrated hospital markets, defined as county-year markets with hospital HHI below 1500. In the baseline specification, I estimate regressions of the form

$$\ln(RR_{het}) = \beta VI_{ht} + \rho \ln(ERR_{het}) + X'_{ht}\gamma + Z'_{m(h)t}\delta + \alpha_h + \nu_{r(h)} + \eta_e + \tau_t + \epsilon_{het}, \qquad (1)$$

where $\ln(ERR_{het})$ is the log expected 30-day readmission rate for hospital h, episode e, and year t. This is useful because hospitals differ over time in the underlying risk profile of the patients they treat. ERR_{het} provides a compact case-mix-adjusted benchmark for that cell, so conditioning on $\ln(ERR_{het})$ helps ensure that the estimated effect of VI adoption is not simply capturing predictable variation in readmissions driven by changes in patient severity or episode composition. The other controls are: X_{ht} denotes time-varying hospital controls; $Z_{m(h)t}$ denotes time-varying market controls; α_h are hospital fixed effects; $\nu_{r(h)}$ are HRR fixed effects; η_e are episode fixed effects; and τ_t are year fixed effects. Regressions are weighted by $\ln(\text{discharges}_{het})$, and standard errors are clustered at the hospital level.

Table 6 reports the results using FFS hospital–episode–year data, where the dependent variable is the log 30-day unplanned readmission rate. In the full sample, hospital VI adoption is associated with a statistically significant 1.0 percent decline in readmissions. We also estimate the specification in Equation 1 for the subset of hospitals that operate less concentrated markets (HHI < 1500). In unconcentrated markets, the coefficient of interest is also statistically significant and indicate a lower readmission rate of about 1.6 percent. These patterns are consistent with the hospital-side results from the IV analysis and suggest that the effects of vertical integration may be stronger in less concentrated markets. Because the analysis is conducted on FFS patients, the estimates are more naturally interpreted as reflecting hospital-wide changes in care delivery that apply to all patient treated at VI hospitals.

Table 6: Impact of Hospital VI Adoption on FFS Readmission Rates

	Full Sample	Unconcentrated Markets
	DiD	DiD
Hospital VI adoption	-0.0101**	-.01597**
	(0.0042)	(0.0068)
Observations	55,580	9,140
Hospital FE	Yes	Yes
HRR FE	Yes	Yes
Episode FE	Yes	Yes
Year FE	Yes	Yes
Hospital controls	Yes	Yes
Market controls	Yes	Yes

Notes: The dependent variable is the log 30-day unplanned readmission rate at the hospital–episode–year level. Hospital VI adoption equals one if the hospital sponsors a Medicare Advantage plan in year t. All specifications control for the log expected readmission rate, time-varying hospital characteristics, and time-varying market characteristics. Unconcentrated markets are county-year hospital markets with HHI < 1500. Regressions are weighted by ln(discharges), and standard errors clustered at the hospital level are reported in parentheses.

4.2 Pre-trend assessment

A key identifying assumption of the difference-in-differences design is that, absent adoption, hospitals that later become vertically integrated would not already have been on systematically different readmission trajectories. For example, hospitals that later become plan-sponsor could have already implemented improvement in care management that allowed to reduce readmission risk. In this case, our DiD estimation can be biased because lower readmission rates at hospitals observed as vertically integrated could partly reflect pre-existing trends rather than the effect of VI adoption itself. This concern applies not only to the DiD design but also to the IV design in the next section. However, since we only have two years of MA beneficiary-level data we cannot test for the absence of pre-trends in that case. Under the assumption that VI hospitals implement hospital-wide change in care delivery, we consider the pre-trend assessment using FFS data to be informative also for the MA populations.

To assess this threat, I estimate an event-study specification centered on each hospital's *first* observed year of VI adoption. Let G_h denote the first year hospital h becomes vertically

integrated, and define event time as $k = t - G_h$. I then estimate:

$$\begin{aligned} \ln(RR_{het}) = & \sum_{k \neq -1} \theta_k \mathbb{1}\{t - G_h = k\} \\ & + \beta \ln(ERR_{het}) + X'_{ht}\gamma + Z'_{m(h)t}\delta \\ & + \alpha_{he} + \lambda_{et} + u_{het}, \end{aligned} \tag{2}$$

where RR_{het} is the observed FFS readmission rate for hospital h, episode e, and year t; ERR_{het} is the expected readmission rate for that hospital–episode–year cell; X_{ht} denotes time-varying hospital controls; $Z_{m(h)t}$ denotes time-varying market controls; α_{he} are hospital-episode fixed effects; and λ_{et} are episode-year fixed effects. Standard errors are clustered at the hospital level. The omitted event-time category is $k = -1$, so each θ_k measures the difference in log readmission rates at event time k relative to the year immediately before first adoption.

In the event study design specified in Equation 2, hospital-episode fixed effects absorb all time-invariant differences across hospital-episode cells, while episode-year fixed effects absorb common all time-varying episode-specific shocks. Conditional on expected risk, hospital controls, market controls, and these high-dimensional fixed effects, the lead coefficients θ_k for $k < 0$ provide a direct test of whether hospitals that later adopt VI were already on different outcome trajectories before adoption.

Figure 3 plots the estimated event-time coefficients. The pre-adoption coefficients are small in magnitude and statistically imprecise. In the baseline event-study specification, the coefficients at $k = -3$ and $k = -2$ are approximately 0.0045 and 0.0012, respectively, with neither individually distinguishable from zero. More importantly, a joint test of the included lead coefficients fails to reject the null that they are jointly zero ($F = 0.55$, $p = 0.579$). Thus, within the observed pre-adoption window, the FFS data provide no evidence that future adopters were already on differential readmission trends before first VI adoption.

The post-adoption coefficients are negative and become more pronounced over time. The coefficient at $k = 0$ is close to zero, while the estimates at $k = 1$, $k = 2$, and $k = 3$ are negative, with the largest decline appearing three years after first adoption. In the current specification, the estimate at $k = 3$ is approximately -0.011 and statistically significant at conventional levels. Because the purpose of this exercise is primarily to evaluate pre-trends, I do not emphasize these lag coefficients as separate causal treatment effects. Nevertheless, their sign is consistent with lower readmission rates after adoption.

Taken together, this evidence yields three conclusions. First, within a hospital, VI adoption is associated with lower readmission rates. Second, this association is smaller in more concentrated markets. Third, the event-study evidence provides no indication that future

adopters were already on differential pre-adoption readmission trajectories in the observed pre-period. These findings strengthen the hospital-adoption margin as a useful benchmark and weaken the concern that the main hospital-side results are driven entirely by pre-existing differential trends.

In the next section, we implement a Two-way Fixed-effects design using MA beneficiary level data. These data allow us to implement an IV strategy to address endogeneity from patient selection across hospitals, but also introduce another source of endogeneity that stems from unobservable plan-side effects.

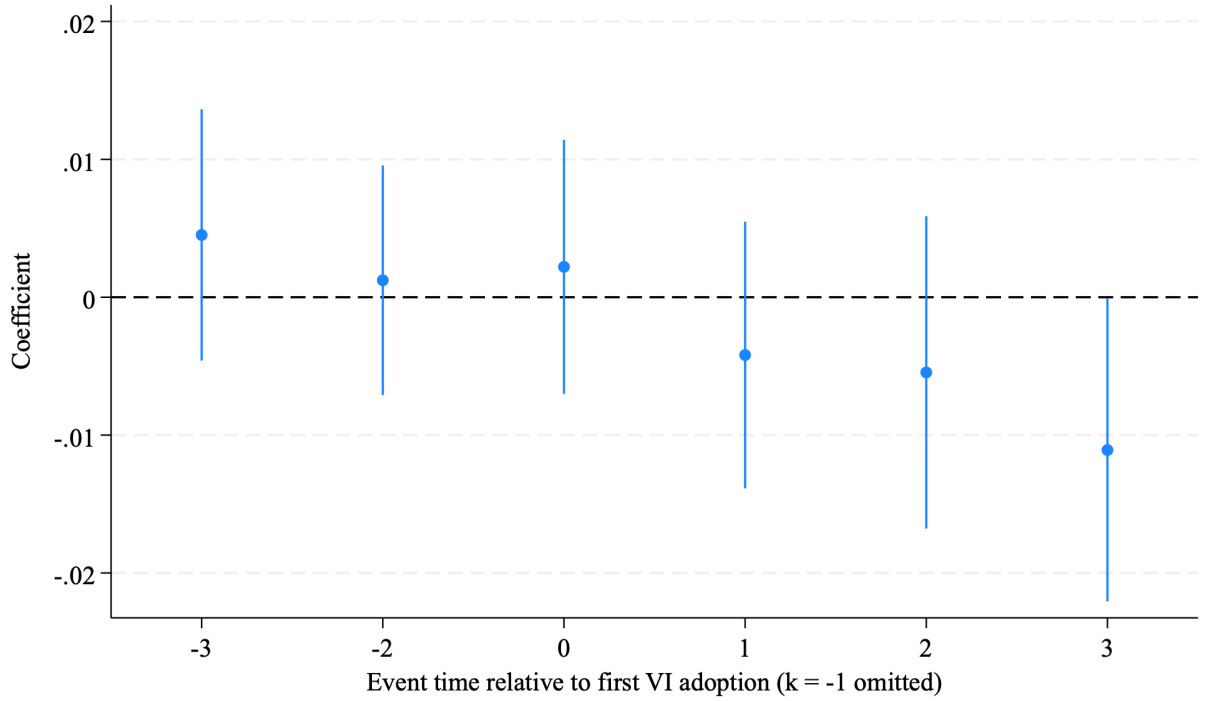

Figure 3: FFS event study around first hospital VI adoption

5 Evidence from beneficiary level MA data

The unit of observation is the beneficiary-by-episode, where an episode is defined as an inpatient admission originating in the emergency department (or an observation stay subsequently converted to inpatient) with no transfer-in from another acute-care facility. For each episode, we observe the beneficiary's Medicare Advantage (MA) plan enrollment, the admitting hospital, and whether the beneficiary experiences an unplanned readmission within 30 days for any cause.

Let i index the beneficiary-episode. The primary outcome is an indicator for 30-day all-cause unplanned readmission,

$$R_i = \begin{cases} 1 & \text{if beneficiary } i \text{ has an unplanned readmission within 30 days,} \\ 0 & \text{otherwise.} \end{cases}$$

Let $h(i)$ denote the admitting hospital, $p(i)$ the MA plan, and $t(i)$ the year. We study three treatment margins: (i) admission to a vertically integrated hospital, (ii) enrollment in a vertically integrated plan, and (iii) *full integration*, meaning enrollment in a VI plan and treatment at the sponsoring hospital. Define

$$H_i \equiv \mathbb{1}\{\text{beneficiary } i \text{ is admitted to a VI hospital in year } t(i)\},$$

$$P_i \equiv \mathbb{1}\{\text{beneficiary } i \text{ is enrolled in a VI plan in year } t(i)\},$$

and

$$F_i \equiv \mathbb{1}\{\text{beneficiary } i \text{ is enrolled in a VI plan and treated at the sponsoring hospital}\}.$$

Across all three treatment margins, I report specifications in two samples: the full estimation sample and the subsample of beneficiary episodes treated at hospitals operating in unconcentrated hospital markets, defined as county-year markets with $HHI < 1500$. The reason is that if vertical integration affects hospital-wide discharge planning, care coordination, or post-acute management, those responses may be stronger in less concentrated provider markets, where competitive pressure is greater.

LATE interpretation. The IV hospital estimates identify a local average treatment effect for beneficiaries whose hospital assignment is shifted by ambulance referral patterns and differential distance. Because the hospital specifications also include hospital fixed effects, they combine a complier interpretation on the patient-assignment margin with within-hospital variation over time in VI status.

The VI-plan estimates identify a local effect of instrument-induced enrollment into VI plans, conditional on the admitting hospital and observed plan characteristics. Because plan fixed effects are infeasible, those estimates should not be interpreted as within-plan ownership effects.

The full-integration estimates identify the additional effect of sponsor-plan enrollment among beneficiaries whose fully integrated treatment status is shifted by the product-based instrument, but those specifications turn out to be weakly identified in the data and are

therefore interpreted cautiously.

5.1 Admission to a VI hospital

We begin with admission to a vertically integrated hospital. The baseline outcome is either an indicator for 30-day unplanned readmission or length of stay (in logs). As noted above, we report each hospital-side specification in both the full sample and the subsample of unconcentrated hospital markets. We run the following specification:

$$Y_i = \beta_H H_i + X_i'\gamma + \alpha_h + \phi_p + \mu_c + \lambda_d + \tau_{t(i)} + \varepsilon_i, \tag{3}$$

where H_i indicates admission to a vertically integrated hospital, X_i contains beneficiary controls, α_h are hospital fixed effects, ϕ_p are plan fixed effects, μ_c are county fixed effects, λ_d are diagnosis fixed effects, and $\tau_{t(i)}$ are year fixed effects. The specification in Equation 3 does not have a time subscript because most beneficiaries appears only once in our sample. The hospital analysis uses the emergency-episode sample in which ambulance-based assignment shifters are available. Patient-side selection is addressed by instrumenting H_i with the leave-one-out ambulance preference shifter $Z_i^{A,H}$ and differential distance Z_i^{DD}. Hospital and plan fixed effects absorb all time invariant difference across hospitals and plans respectively. Under the assumption that the timing of hospital adoption of VI status is exogenous, the coefficient of interest, β_H, admits a a causal interpretation.

Table 12 and Table Table 13 in the appendix reports robustness analysis based on a stricter VI-hospital definition. In that alternative definition, a hospital-year is treated as vertically integrated only when sponsor-plan enrollment relative to bed capacity is above the median. The motivation is hospital-wide incentives: if the mechanism operates through organizational changes in discharge planning and care coordination, those changes should be more likely when the hospital has a larger mass of patients for whom it fully internalizes the gains from avoiding readmissions. We find that results are robust to this definition.

5.2 Enrollment in a VI plan

We use the following specification to estimate the treatment effect of enrollment into vertically integrated plan:

$$Y_i = \beta_P P_i + X_i'\gamma + \alpha_h + \mu_c + \lambda_d + \tau_{t(i)} + \varepsilon_i, \tag{4}$$

where P_i indicates enrollment in a vertically integrated plan. This specification is designed to isolate the plan-side margin. We instrument for patient selection across plans using

the county-level VI-plan availability $Z_i^{A,P}$ and the differential distance Z_i^{DD}. Since the first instrument is valid only for the zip codes the that are near a county border, we estimate Equation 4 on a restricted sample.

We include hospital fixed effects but not plan fixed effects because VI status is effectively time-invariant within plan and would be absorbed by plan fixed effects. Conditioning on the admitting hospital means that identification comes from differences in outcomes among patients treated at the same hospital but enrolled in different types of plans. However, because plan fixed effects are infeasible, these estimates should be interpreted cautiously: we cannot rule out the estimated effect by driven by other plan unobservables correlated with the VI status.

5.3 Full integration

$$Y_i = \beta_F F_i + X_i'\gamma + \alpha_h + \phi_p + \mu_c + \lambda_d + \tau_{t(i)} + \varepsilon_i, \tag{5}$$

where F_i indicates full integration: the beneficiary is enrolled in a VI plan and is treated at the hospital that sponsors that plan. The full-integration exercise asks whether there is an additional effect of sponsor-plan enrollment once the patient is already treated at a VI hospital. The omitted group therefore includes beneficiaries treated at VI hospitals who are not enrolled in the sponsoring plan. I instrument F_i using the interaction-based shifter, which is given by the product of the hospital-assignment and plan-enrollment instruments already described in Section 4.

Conceptually, this is the test of within-hospital differential treatment by plan ownership. If fully integrated patients performed better even relative to other patients treated at the same VI hospital, that would suggest that VI hospitals manage sponsor-plan enrollees differently from other patients. If not, the hospital-side effect is more naturally interpreted as a hospital-wide organizational response that benefits all patients treated at the VI hospital.

5.4 Results

Hospital Side Results Table 7 reports the results of the baseline hospital-side specification in Equation 3. The OLS and IV estimates point in very different directions. In the full sample, OLS implies that patients treated at VI hospitals have higher readmission rates, whereas the IV estimate is close to zero and statistically insignificant. We interpret that gap as evidence of adverse patient selection into VI hospitals. Once the sample is restricted to unconcentrated markets, however, the IV coefficient becomes negative and statistically significant. Therefore, in markets with higher hospital competition, we find that, conditional

on our set of controls, patients admitted to a VI hospital have 9.57 percentage point lower readmission risk relative to patients admitted to standard hospitals.

Appendix Table 12 shows that this pattern is robust to the stricter VI-hospital definition based on sponsor-plan enrollment relative to bed capacity. The full-sample IV estimate remains small and insignificant, while the unconcentrated-market IV estimate becomes even more negative. This robustness exercise strengthens the hospital-wide mechanism story: when a larger share of patients are enrolled in the hospital-sponsored plan, the incentives to make organization-wide investments in discharge planning and post-discharge coordination should be stronger.

Table 8 shows that, after accounting for patient selection, vertically integrated hospitals lead to 10.9% shorter length of stay as compared to standard hospitals. Appendix Table 13 reveals an even stronger pattern under the stricter hospital definition. In unconcentrated markets, vertically integrated hospitals reduce length of stay by 13.9% relative to non-integrated hospitals, a magnitude that is slightly larger than the effect estimated in the full sample. These findings suggest that the observed reduction in readmission rates at vertically integrated hospitals is not driven by longer inpatient stays, but rather reflects more efficient care delivery.

Table 7: Readmission: VI Hospital (Baseline Definition)

	Full Sample		Unconcentrated Markets	
	OLS	IV	OLS	IV
VI Hospital	0.0169***	-0.0084	0.0158	-0.0957**
	(0.0052)	(0.0171)	(0.0100)	(0.0376)
Observations	177,648	177,648	38,168	38,168
KP first-stage F		138.9690		40.1930
Year FE	Yes	Yes	Yes	Yes
Diagnosis FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Plan FE	Yes	Yes	Yes	Yes
Hospital FE	Yes	Yes	Yes	Yes

Notes: Columns (1)–(2) use the full estimation sample; columns (3)–(4) restrict to unconcentrated markets with hospital HHI < 1500. Hospital, plan, county, diagnosis, and year fixed effects are included in all specifications. The IV specifications instrument hospital treatment with differential distance and ambulance-based destination preferences. Standard errors clustered at the hospital level are reported in parentheses. Stars denote $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Plan Side Results Table 9 reports the results of the baseline plan-side specification in equation Equation 4. The plan-side readmission results are weaker and should be inter-

Table 8: Length of Stay: VI Hospital (Baseline Definition)

	Full Sample		Unconcentrated Markets	
	OLS	IV	OLS	IV
VI Hospital	-0.0179	-0.1094***	-0.0581**	-0.1390*
	(0.0113)	(0.0390)	(0.0259)	(0.0741)
Observations	117,333	117,333	24,740	24,740
KP first-stage F		139.4000		40.2070
Year FE	Yes	Yes	Yes	Yes
Diagnosis FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Plan FE	Yes	Yes	Yes	Yes
Hospital FE	Yes	Yes	Yes	Yes

Notes: The outcome is $\log(LOS)$. Columns (1)–(2) use the full estimation sample; columns (3)–(4) restrict to unconcentrated markets with hospital HHI < 1500. Hospital, plan, county, diagnosis, and year fixed effects are included in all specifications. The IV specifications instrument hospital treatment with differential distance and ambulance-based destination preferences. Standard errors clustered at the hospital level are reported in parentheses. Stars denote $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

preted more cautiously. We obtain negative OLS coefficients in both the full sample and the unconcentrated-market sample, but those differences disappear after controlling for patient selection in the IV specifications. This OLS–IV pattern suggests advantageous selection into VI plans. The unconcentrated-market first stage is especially weak, which further limits what should be inferred from those columns.

Table 10 extends the same comparison to length of stay. We find evidence of shorter LOS among patient enrolled in VI plan, but the absence of plan fixed effects makes these results less convincing than the hospital side findings. Moreover, the unconcentrated-market IV specification have a weak first-stage F statistics and, therefore, are no longer informative. We therefore would not emphasize the plan-side LOS results as a separate causal finding. Their main value is interpretive: even on the plan side, the evidence does not suggest that lower readmissions are being generated through longer inpatient stays.

Full Integration Results Table 11 reports the results of the baseline full integration specification in equation Equation 5. The comparison is made between beneficiaries who are enrolled in a VI plan and treated at the sponsoring hospital relative to a control group that includes both patients treated by standard hospitals and also patients treated at VI hospitals but not enrolled in the hospital-sponsored plan. The OLS coefficients are positive, but the IV estimates are highly imprecise and statistically insignificant in both samples, and has weak first stage. Although we cannot rule this out, the safest interpretation is that

Table 9: Readmission: VI Plan

	Full Sample		Unconcentrated Markets	
	OLS	IV	OLS	IV
VI Plan	-0.0092**	-0.1251	-0.0262***	-0.0344
	(0.0038)	(0.2472)	(0.0074)	(0.5579)
Observations	178,667	80,003	38,476	14,569
KP first-stage F		12.6250		2.7320
Year FE	Yes	Yes	Yes	Yes
Diagnosis FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Plan FE	No	No	No	No
Hospital FE	Yes	Yes	Yes	Yes

Notes: Columns (1)–(2) use the full estimation sample; columns (3)–(4) restrict to unconcentrated markets with hospital HHI < 1500. Hospital, county, diagnosis, and year fixed effects are included in all specifications. Plan fixed effects are not included because VI status is time-invariant within plan. The IV specifications instrument plan enrollment using county-level VI-plan availability and differential distance. Standard errors clustered at the hospital level are reported in parentheses. Stars denote $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table 10: Length of Stay: VI Plan

	Full Sample		Unconcentrated Markets	
	OLS	IV	OLS	IV
VI Plan	-0.0507***	-1.4000***	-0.1140***	0.8643
	(0.0089)	(0.5244)	(0.0151)	(1.2807)
Observations	178,667	80,003	38,476	14,569
KP first-stage F		12.6250		2.7320
Year FE	Yes	Yes	Yes	Yes
Diagnosis FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Plan FE	No	No	No	No
Hospital FE	Yes	Yes	Yes	Yes

Notes: The outcome is $\log(LOS)$. Columns (1)–(2) use the full estimation sample; columns (3)–(4) restrict to unconcentrated markets with hospital HHI < 1500. Hospital, county, diagnosis, and year fixed effects are included in all specifications. Plan fixed effects are not included because VI status is time-invariant within plan. The IV specifications instrument plan enrollment using county-level VI-plan availability and differential distance. Standard errors clustered at the hospital level are reported in parentheses. Stars denote $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

these results provide no evidence of an additional readmission benefit from enrollment in a hospital sponsor-plan once the patient is already treated at a VI hospital.

Appendix Table 14 leads to the same conclusion for length of stay. There is no evidence that fully integrated patients experience systematically different LOS once treatment at a VI hospital is already accounted for, and the first stage is again weakly identified. Taken together, the full-integration results are consistent with the idea that VI hospitals do not differentially manage patients based on whether they are enrolled in the sponsor's plan. The lower readmission risk in VI hospitals appear to reflect hospital-wide practice changes rather than differential care by plan enrollment.

Overall, these estimates support four conclusions. First, in markets where hospital competition is stronger, hospital-plan integration significantly reduce readmission risk. Second, the reduction in readmission risk is not driven by longer inpatient stay at the hospital. Third, the lower readmission risk is driven hospital-wide care practice changes at the VI hospital. Finally, while we cannot rule out an effect through VI-plan enrollment, our results provide no evidence of ad additional effect from enrollment in a VI-plan once we control for the admitting hospital.

Table 11: Readmission: Full Integration

	Full Sample		Unconcentrated Markets	
	OLS	IV	OLS	IV
Full integration	0.0232***	-0.0934	0.0278*	-1.5353
	(0.0083)	(1.0698)	(0.0154)	(1.7955)
Observations	177,648	79,076	38,168	14,303
KP first-stage F		2.2720		1.2860
Year FE	Yes	Yes	Yes	Yes
Diagnosis FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Plan FE	Yes	Yes	Yes	Yes
Hospital FE	Yes	Yes	Yes	Yes

Notes: The omitted group includes beneficiaries treated at VI hospitals who are not fully integrated. The IV specifications are exactly identified and use Z_{HP} as the excluded instrument. Columns (1)–(2) use the full estimation sample; columns (3)–(4) restrict to unconcentrated markets with hospital HHI < 1500. Hospital, county, diagnosis, and year fixed effects are included in all specifications. Plan fixed effects are not included because VI status is time-invariant within plan. Standard errors clustered at the hospital level are reported in parentheses. Stars denote $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

6 Conclusion

This paper studies how hospital–plan integration affects 30-day unplanned readmissions risk, and aims at disentangling the effect of two treatment margins: the admission to VI-hospital and the enrollment in a VI-plan. Aggregate hospital-level data on Fee-for-Service beneficiaries indicate that VI-status adoption leads to hospital-wide care delivery that reduce readmission rate. Moreover, the event study analysis rules out that these results are driven my differential trends that precede VI-status adoption. We assume that the latter is also informative for pre-trend among Medicare Advantage beneficiaries. A limitation of aggregate FFS data is that we cannot control for endogenous patient selection across hospitals.

We use a pooled sample of two years of beneficiary-level Medicare Advantage data and implement an IV design to address beneficiaries selection across hospital and plans. Our set of instruments generate quasi-experimental variation in VI hospital assignment and VI-plan enrollment. We control for plan fixed effect and hospital fixed effects, and we leverage within hospital variation in the VI-status over time to identify the effect of admission to a VI-hospital. After accounting for patient selection, we find little evidence of an average hospital-side effect in the full sample. However, in unconcentrated hospital markets, admission to a vertically integrated hospital substantially reduces readmission risk relative to admission to a comparable non-integrated hospital. Moreover, we also show that this lower readmission risk is not driven by longer inpatient length of stay.

The plan-side identification is weaker since we cannot control for plan fixed effects which would absorb the VI-plan effect. While we cannot rule out plan side effects, our results shows that, after conditioning on the admitting hospital, there is no evidence that enrollment in a VI plan significantly changes readmission risk. We also find no evidence that beneficiaries who are both enrolled in a VI plan and treated at the sponsoring hospital experience an additional improvement beyond the effect of treatment at a VI hospital. However, those full-integration estimates are weakly identified and should be interpreted cautiously. Taken together, this evidence suggests that the benefits of hospital–plan integration arise primarily through hospital-wide changes in care delivery rather than through differential treatment based on plan enrollment.

These findings contribute to the debate on the efficiency gains from hospital-insurer integration. Integration can raise legitimate concerns about hospital market power, but our results here suggest that, in competitive hospital markets, it reduces significantly readmission risk without increasing inpatient stay. More broadly, the paper shows why it is important to separate hospital-side from plan-side effects when evaluating integrated payer-provider organizations. Because the cleanest hospital-side identification comes from high-acuity emergency

admissions, the results speak most directly to that segment of inpatient care and need not generalize one-for-one to the full universe of hospital admissions. A natural next step is to examine whether similar effects arise in more discretionary episodes and for other outcomes, including post-acute spending, mortality, and total episode costs.

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A Additional Results Tables

This appendix collects the additional tables introduced in the revised draft. The first group reports validation exercises for the hospital-side margin using a stricter VI-hospital definition based on sponsor-plan enrollment relative to hospital beds. The second group reports length-of-stay results for the plan-side and full-integration margins. These tables support the interpretation in the main text: the hospital-side readmission effect is strongest in unconcentrated markets and is not driven by longer stays, whereas the evidence for additional plan-side or full-integration effects is substantially weaker.

Table 12: Readmission: VI Hospital (High Enrollment-to-Bed Share Definition)

	Full Sample		Unconcentrated Markets	
	OLS	IV	OLS	IV
VI Hospital (high share)	0.0135**	-0.0114	0.0163	-0.1151**
	(0.0053)	(0.0237)	(0.0109)	(0.0505)
Observations	177,648	177,648	38,168	38,168
KP first-stage F		88.4150		29.9150
Year FE	Yes	Yes	Yes	Yes
Diagnosis FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Plan FE	Yes	Yes	Yes	Yes
Hospital FE	Yes	Yes	Yes	Yes

Notes: This alternative VI-hospital definition follows the stricter hospital-year definition implemented in the log file, based on a higher plan-enrollment-to-bed ratio. Columns (1)–(2) use the full estimation sample; columns (3)–(4) restrict to unconcentrated markets with hospital HHI < 1500. Hospital, plan, county, diagnosis, and year fixed effects are included in all specifications. The IV specifications instrument hospital treatment with differential distance and ambulance-based destination preferences. Standard errors clustered at the hospital level are reported in parentheses. Stars denote $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table 13: Length of Stay: VI Hospital (High Enrollment-to-Bed Share Definition)

	Full Sample		Unconcentrated Markets	
	OLS	IV	OLS	IV
VI Hospital (high share)	-0.0331***	-0.1519***	-0.0835***	-0.1734*
	(0.0105)	(0.0544)	(0.0232)	(0.0963)
Observations	117,333	117,333	24,740	24,740
KP first-stage F		85.0100		29.2300
Year FE	Yes	Yes	Yes	Yes
Diagnosis FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Plan FE	Yes	Yes	Yes	Yes
Hospital FE	Yes	Yes	Yes	Yes

Notes: The outcome is $\log(LOS)$. This alternative VI-hospital definition follows the stricter hospital-year definition implemented in the log file, based on a higher plan-enrollment-to-bed ratio. Columns (1)–(2) use the full estimation sample; columns (3)–(4) restrict to unconcentrated markets with hospital HHI < 1500. Hospital, plan, county, diagnosis, and year fixed effects are included in all specifications. The IV specifications instrument hospital treatment with differential distance and ambulance-based destination preferences. Standard errors clustered at the hospital level are reported in parentheses. Stars denote $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table 14: Length of Stay: Full Integration

	Full Sample		Unconcentrated Markets	
	OLS	IV	OLS	IV
Full integration	-0.0143	4.1873	-0.0327	-0.3841
	(0.0171)	(3.5772)	(0.0357)	(1.9995)
Observations	177,648	79,076	38,168	14,303
KP first-stage F		2.2720		1.2860
Year FE	Yes	Yes	Yes	Yes
Diagnosis FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Plan FE	Yes	Yes	Yes	Yes
Hospital FE	Yes	Yes	Yes	Yes

Notes: The omitted group includes beneficiaries treated at VI hospitals who are not fully integrated. The IV specifications are exactly identified and use Z_{HP} as the excluded instrument. The outcome is $\log(LOS)$. Columns (1)–(2) use the full estimation sample; columns (3)–(4) restrict to unconcentrated markets with hospital HHI < 1500. Hospital, county, diagnosis, and year fixed effects are included in all specifications. Plan fixed effects are not included because VI status is time-invariant within plan. Standard errors clustered at the hospital level are reported in parentheses. Stars denote $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.