Improving Productivity in Healthcare: The Case of Ambulatory Surgical Centers*

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March 2026

PRELIMINARY. DO NOT CIRCULATE.

Abstract

Policymakers aim to encourage innovative new care delivery models that reduce medical spending while maintaining quality. Yet care new models can be difficult to evaluate, and regulatory frictions may impede their growth. We illustrate these challenges using the case of Ambulatory Surgical Centers (ASCs), which offer a lower cost alternative to hospitals for low and moderate complexity procedures. Although ASCs have been active for several decades, there is still no consensus on their causal impacts on spending and health. This paper uses national data on Medicare patients and an instrumental variable approach to overcome this empirical challenge. Correcting for selection reduces estimated episode savings relative to hospitals only by 5% and reveals no detectable difference on most health outcomes. Instead, ASC patients are less likely to have a postoperative emergency department visit, a signal of improved quality and source of savings beyond the focal procedure. Heterogeneity tests reveal that episode savings are 40% larger in states that do not impose certificate of need restrictions, suggesting that such requirements not only impede access to care but also may inhibit the savings potential of new care delivery models.

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Improving the productivity of the U.S. healthcare system is a policy objective of first-order importance. With healthcare expenditures consuming nearly one-fifth of GDP, and evidence of substantial inefficiencies and variation in care delivery, there is a great need of new care models that lower medical spending while maintaining quality. In an influential article, Cutler (2011) argued that inefficiency in U.S. healthcare persists due to a lack of innovation in the organization of care delivery. He identified two key barriers to encouraging such innovation. First, since care quality is notoriously difficult to observe, market participants and regulators don't appropriately reward high quality firms nor punish low quality ones. In particular, the quality of new lower cost care models is always under suspicion. Second, the growth and diffusion of new care models may be inadvertently impeded by regulatory frictions and payer inertia. This paper assesses the importance of these challenges using the case of Ambulatory Surgical Centers (ASCs), an innovative healthcare delivery model that offers outpatient procedures in specialized low-cost facilities. Although ASCs have been in existence since the 1970s and serve millions of patients each year, their effects on spending and health, and the extent to which regulation impedes their potential, are poorly understood.

ASCs offer an ideal case study to examine the challenges facing business model innovation in care delivery for several reasons. The ASC is a prominent and successful innovation in care delivery in the US. ASCs leverage advances in surgical technology and anesthesia to provide safe same-day procedures in a lower cost setting outside hospitals. ASCs have successfully expanded in number and versatility since Medicare started reimbursing them for procedures in 1982. There were more than 8,000 ASCs in the US by 2020, about 40% more than hospital outpatient departments (HOPDs), the traditional option for outpatient surgery.

The success of ASCs stems from their advantageous cost structure. Health insurers reimburse ASCs a significantly lower facility fee for a procedure compared to HOPDs. To illustrate the cost advantage, consider Medicare reimbursement rates, which are publicly available, for a colonoscopy, one of the most common procedures performed at ASCs. While HOPDs are reimbursed $710 on average, ASCs receive $370—a saving of nearly 50%. Across a representative set of 15 high-volume procedures studied in this paper, we estimate that the average Medicare payment is approximately 38% higher in HOPDs than in ASCs. Note that lower payments for ASCs translate not only into savings for Medicare, but also lower out-of-pocket costs for beneficiaries.

However, payers and physicians have been conservative in allowing ASCs to expand their range of services and patient mix toward more complex procedures because of concerns about the risk to patient safety and health. This concern has lingered because it is difficult to estimate the causal impact of ASCs on patient health. ASCs tend to treat healthier, less complex patients. As we document using rich Medicare claims data, ASC patients have fewer comorbidities, fewer prior emergency department visits, and lower historical healthcare spending compared to their HOPD counterparts. Hence, payers and regulators legitimately worry about patient selection on unobserved risk. Furthermore, if treatment at ASCs leads to the patient needing more postoperative care (e.g., emergency care), it may offset the apparent cost savings on the focal procedure. The concern about making unbiased quality comparisons is reflected in the most recent report to Congress by the Medicare Payment Advisory Commission (MedPAC), "Al-

though some studies suggest that the quality of care at ASCs is comparable to that at HOPDs for certain procedures, Medicare lacks a comprehensive set of quality measures that would enable robust comparisons." (MedPAC 2025). Hence, information frictions, one of the key barriers discussed above, underlie the hesitation to expand the eligibility of ASCs to perform more procedures.

Care delivery is understandably a highly regulated industry, subject to both federal and state requirements. While ASCs have to comply with a plethora of regulations, our empirical focus in this paper is on entry restrictions imposed by states, known as certificate of need (CON) requirements. These laws require healthcare providers to obtain approval from a state agency before building new facilities or expanding existing capacity. These restrictions were initially adopted by some states as a cost control tool under the rationale that uncontrolled entry led to excess health care capacity. This represents wasteful deployment of capital and in turn leads providers to induce low-value care. During our sample period, 25 states and Washington D.C. required CON approval for ASC entry or expansion, and 34 states (and D.C.) required CON approval for at least one provider type (AHPA 2016). However, recent empirical studies have shown that CON restrictions reduce access to ASCs and may have led to differentially elevated cancer mortality (Cantor et al. 2025; Stratmann, Bjoerkheim, and Koopman 2025). We show in our sample, which is composed of Medicare beneficiaries, that consumers enjoy greater access to ASCs in states without CON restrictions. 58% of all cases are performed at ASCs in CON states, against 66% in states without such restrictions. Therefore, it is plausible that these restrictions suppress productivity gains from greater competition and the growth of new lower-cost care models.

Our main dataset is medical claims and enrollment information on a random sample of Medicare fee-for-service (FFS) beneficiaries over 2011–2020. We begin our empirical analysis by testing whether ASCs lead to a net expansion in care use or primarily substitute away from hospitals. Meaningful effects on the extensive margin of care would necessitate a careful market-level analysis to assess the total effects of ASC availability, such as Currie, Karpova, and Zeltzer (2023) perform for Urgent Care Centers (UCC). Following Einav, Finkelstein, and Mahoney (2023), we leverage ASC entry into new markets during 2012–18 to quantify the effects of ASCs on the extensive margin. We compare trends for counties that experienced ASC entry during this period to those for a set of comparison counties that did not experience entry until the end of the sample using a difference-in-differences design. We detect a sharp increase in the share of procedures performed at an ASC when a county experiences entry, and a corresponding decrease in mean spending per procedure. However, we find that the total number of procedures per Medicare beneficiary remains flat after ASC entry. Specifically, our estimate implies a small and statistically insignificant change in total volume averaged over 4 years after entry. This finding is very similar to the result reported by Yu (2023) who examines the effects of ASC entry after CON deregulation in Missouri. We conclude that ASCs effect medical spending and patient health primarily by substituting away from hospitals and we develop a research design to study this margin.

To obtain unbiased estimates of the causal effects of ASCs relative to HOPDs we require a research design that can circumvent potential patient selection on unobserved risk. We employ an instrumental variables (IV) strategy that uses a patient's relative

proximity to an ASC as an instrument for the choice of treatment site. Our instrument—the difference in distances between a patient's residence and the nearest ASC versus nearest HOPD performing the relevant procedure—is strongly predictive of facility choice, with a first stage f-statistic of approximately 500. This "differential" distance also meaningfully affects ASC choice: an increase in differential distance of one mile to the nearest ASC reduces the likelihood of ASC use by 0.71 percentage points.

Economists have long used differential distance as an instrument for provider choice because it is well established that patients strongly prefer more proximate providers (e.g., McClellan, McNeil, and Newhouse 1994a; Grabowski et al. 2013; Gupta et al. 2024). However, to our knowledge we are the first to apply this instrument to study ASC care. We therefore show comprehensive evidence to support the assumption that by using this instrument, patients are as good as randomly assigned to ASCs after conditioning on observables: patients living closer to ASCs are observationally similar to those living closer to HOPDs on a wide range of demographic and pre-treatment healthcare utilization metrics. A rich vector of such patient attributes are jointly uncorrelated with differential distance (p-value 0.27), while they strongly predict ASC use (p value < 0.001). Approximately 40% of the patients in our sample are compliers, i.e., they choose an ASC if they are located closer to one, otherwise they would not. Compliers are moderately complex in terms of health risk and similar to the average ASC patient in terms of demographics.

The IV results provide new evidence on the causal effects of ASCs on care utilization, quality, and spending. We find that undergoing a procedure at an ASC leads to a 32% reduction in Medicare payments for the focal procedure, slightly lower than the unadjusted difference in reimbursement schedules. More importantly, the total cost of care over the 90-day post-procedure period, which we refer to as the "episode" spending, is also lower for ASC patients by 16%. Since mean episode spending at an HOPD is $3,892, this implies a saving of approximately $630. Of note, the savings implied by the IV estimates are only slightly smaller (5%) in magnitude than those implied by the OLS coefficients. Note that total episode savings are substantially larger than the saving just on the focal procedure, $485. The additional savings are driven by a reduction in the likelihood of costly emergency department (ED) use, which falls by 2.5 percentage points (a 20% decline relative to the mean). The decrease in ED use is partly offset by an increase in visits to the physician's office and urgent care facilities. However, these are much less costly and potentially preventive alternatives for addressing postoperative concerns.

Crucially, we find no evidence that these cost savings come at the expense of patient health. Rates of surgical complications, hospital admissions, and mortality within 90 days of the procedure are statistically indistinguishable between ASC and HOPD patients in our IV models. The coefficients are also typically small and *negative* in sign, suggesting minor improvements. Overall, our results imply that ASCs deliver equivalent quality at lower cost and should be considered a productivity-enhancing innovation in healthcare.

We then explore the role of state-level regulations by stratifying our sample into states with and without prevailing CON laws. The cost savings associated with ASC care are substantially larger in non-CON states: total episode payments are reduced by

18% versus 14%. Moreover, we find that the overall reduction in ED visits is driven entirely by ASC patients in non-CON states, who experience a large reduction in ED visits (over 40%) with little substitution by urgent care use. In contrast, ASC patients in CON states experience no decrease in ED use but yet increase their use of urgent care clinics. These findings imply that not only do CON restrictions reduce the availability of ASCs, but also they reduce their cost saving effectiveness. Further, back of the envelope calculations suggest that the impact of CON restrictions on the aggregate savings of ASCs operate primarily by reducing the per-case savings rather than by reducing access.

This paper contributes to three strands of the literature. An active literature, mostly published in clinical journals, has quantified the effects of ASCs on spending and patient health (e.g., Chukmaitov et al. 2008; Billig et al. 2020; Wang et al. 2022; Chow et al. 2023). These studies document patterns descriptively or use regression models to control for differences in patient observables and typically conclude that ASCs are less costly and yield patient outcomes comparable to HOPDs. However, these studies do not address concerns about unobserved selection, a key challenge in estimating causal effects (Winter 2003; David and Neuman 2011).

To our knowledge, only two prior studies have attempted to address unobserved selection. Munnich and Parente (2018) leverage predicted changes in Medicare payment to instrument for ASC use, using 2007–2009 Medicare claims data. They find large improvements in postoperative outcomes—for example, a 15 percentage point reduction in ED visits within 8–30 days post-procedure, relative to a mean of 4.3%. Aouad, Brown, and Whaley (2019) use claims data from a private insurer in one state, California, during 2011–13 and leverage a price change in one procedure, colonoscopy, to instrument for ASC use. They find that ASCs reduce spending by roughly the observed difference in reimbursement, with no adverse or beneficial effects on patient health. These two studies differ in their conclusions regarding the effects of ASCs on patient health and it is difficult to pin down the reasons. Notably, both studies do not examine other changes in postoperative care and the interaction with state regulations.

More generally, economists have tried to quantify the causal effect of greater medical spending on patient health in other settings. Doyle et al. (2015) leverage quasi-random ambulance assignment of patients to hospitals and find that higher-spending hospitals reduce mortality rates for Medicare patients admitted through the Emergency Department (ED). Silver (2021) finds that ED physicians who spend more on medical treatment also produce lower mortality rates. In contrast, we find that ASCs are indistinguishable in their health outcomes from those of HOPDs and cost Medicare 16% less per episode. This might represent a rare example of a care delivery model that provides equivalent quality at a lower cost.

A second strand has quantified the causal effects of innovation in medical technology on patient health or spending. A series of papers by David Cutler and colleagues studied the costs and benefits of new heart attack treatments (Cutler et al. 1998; Cutler and McClellan 2001). More recent work has applied similar analysis to a wider set of new treatments and drugs (Dunn, Hall, and Dauda 2022). These studies have found that new medical technologies cost more but improve health outcomes sufficiently so that they can be considered cost effective. In contrast, our work shows that ASCs, which are primarily a care delivery and organization innovation, *reduce* medical spending while

maintaining quality. Our results also differ from recent evidence on the effects of urgent care centers (UCCs), a more recent care delivery innovation in healthcare. Both Wang, Mehrotra, and Friedman (2021) and Currie, Karpova, and Zeltzer (2023) find that UCCs lead to an increase in utilization of hospital care and medical spending even though they may substitute for some low-acuity ED visits.

Finally, the paper also relates to a growing literature examining the effects of capacity restrictions imposed by regulators. In healthcare, the most prominent example is the use of Certificate of Need (CON) requirements by some states for new facilities to receive an operating license. These restrictions were originally mandated by the federal government due to concerns about the costs of building excessive capacity in healthcare facilities like hospitals and nursing homes or the potential for low quality entrants (Courtemanche and Garuccio 2025). However, prior studies have shown that these requirements suppress the entry of new firms and limit access to care for patients (Cutler, Huckman, and Kolstad 2010; Yu 2023; Stratmann, Bjoerkheim, and Koopman 2025). Our results provide new empirical evidence to bolster this line of criticism.

The rest of the paper is organized as follows. Section 1 provides the necessary information about the outpatient care, ASCs, and CON restrictions. Section 2 describes the key data sources, sample and variable construction, and summarizes key descriptive statistics. We examine the extensive margin effects of ASCs in Section 3. Section 4 describes the research design to study the effects of ASC relative to hospital care per episode. Section 5 presents the corresponding results on the effects of ASCs on utilization, spending, and health along with robustness checks. Section 6 presents evidence from heterogeneity tests. Finally, Section 7 concludes.

1 Background

1.1 Outpatient Care Models

Outpatient surgery, also known as ambulatory surgery, refers to scheduled, nonemergent procedures that allow patients to undergo an operation and be discharged on the same day, provided their clinical condition permits. This segment of the surgical market has grown substantially over the past few decades, with outpatient procedures now accounting for more than 70% of all surgeries in the United States (Gabriel et al. 2021).

This growth has been driven in part by economic pressures to contain healthcare costs. Outpatient procedures tend to be substantially less expensive than their inpatient counterparts due to lower facility, personnel, and supply costs (Richter and Diduch 2017; Lynch et al. 2021). For example, outpatient knee and hip replacements are estimated to be 37% and 28% less costly, respectively, than those performed in inpatient settings (BCBS 2019). These cost differentials have positioned outpatient surgery as a more affordable alternative for appropriate patients, reducing financial burdens for both payers and patients. Technological advancements have further enabled this shift, as improvements in anesthesia, postoperative pain management, and minimally invasive surgical techniques have expanded the range of procedures that can be safely performed on an ambulatory basis (Abebe et al. 2022; Crews 2002; Tsui, Klein, and Garabrant 2013).

Ambulatory Surgery Centers (ASCs) and Hospital Outpatient Departments (HOPDs)

represent distinct models for delivering less complex outpatient procedures. HOPDs are owned by and typically co-located with hospitals, granting them access to broader clinical infrastructure, including emergency and intensive care services, advanced anesthesia care, and coordination with other hospital departments (Chukmaitov et al. 2008). This allows HOPDs to manage patients with higher risk profiles or more complex medical needs. ASCs, by contrast, are standalone facilities focused exclusively on outpatient surgical care. Their reimbursement from Medicare is limited to procedures that are included in the ASC Covered Procedures List maintained by the Centers for Medicare and Medicaid Services (CMS), which narrows the scope of services that can be offered (CMS 2024a). ASCs are typically smaller, more specialized, and staffed by teams with focused expertise in the procedures performed (Witiw et al. 2020).

1.2 Ambulatory Surgery Centers

ASC and HOPD growth. Within the outpatient care market, ambulatory surgery centers have experienced particularly rapid growth. Hollenbeck et al. (2014) document an increase in the share of outpatient procedures performed at ASCs from 28.5% in 2001 to 37.4% in 2010, accompanied by a 59.5% increase in ASC operating rooms—from 7,036 to 11,223—over the same period. By 2022, ASCs accounted for more than half of all outpatient procedures in the U.S., with the number of Medicare-certified ASCs exceeding that of hospital outpatient departments by approximately 40% (Morse 2022; PressGaney 2023).

Figure 1 illustrates these trends using data from the CMS Provider of Services files. Panel (a) shows that between 2011 and 2020, the number of ASCs grew by 27%, reaching 8,833, while the number of ASC operating rooms increased by 37%. In contrast, Panel (b) shows a flat trend in the number of HOPDs, which remained stable at around 5,850 over the same period. In terms of service volume, the Medicare Payment Advisory Commission (MedPAC) reported an increase in ASC utilization among Medicare fee-for-service (FFS) beneficiaries, from 180.5 procedures per 1,000 beneficiaries in 2014 to 201.6 procedures per 1,000 beneficiaries in 2019, including a 2.7% increase between 2018 and 2019 (MedPAC 2022). While the MedPAC report concludes at 2019, ASC use is likely to have continued rising in subsequent years.

ASC and HOPD costs. ASCs and HOPDs operate under distinct reimbursement structures, with generally lower rates at ASCs (BCBS 2023). This is particularly evident in Medicare reimbursements, where HOPDs are reimbursed under the Outpatient Prospective Payment Systems (OPPS), and ASCs are reimbursed under the ASC fee schedule. Differences in CMS relative weights, conversion factors, and wage index adjustments contribute to reimbursement variations. These differences can be substantial. For example, a colonoscopy (Current Procedural Terminology (CPT) code 45378) has an allowable payment rate of $710 at HOPDs compared to $370 at ASCs (McMillan, Long, and Payne 2019), representing savings of 48%. Lower reimbursement at ASCs also typically translates into lower out-of-pocket spending for beneficiaries, since Traditional Medicare covers only 80% of the total cost of outpatient care, whether it is performed in the HOPD or in an ASC (CMS 2024b).

Figure A.1 compares average Medicare payments between ASC and HOPD settings

for selected procedures. These fifteen procedures represent the most common ASC procedures for Medicare beneficiaries in the Medicare patient sample during our study period, as described in Section 2.2, and are therefore also the focus of our analyses. Spending amounts were obtained from Medicare claims data, as described in Section 2.1. Across all procedures, HOPD payments consistently exceeded those for ASCs. Taking a simple average across these procedures, Medicare pays approximately $820 (38%) less per procedure when the service is delivered in an ASC.

Panel (c) of Figure 1 illustrates the potential for ASCs to constrain average procedure costs over time. The panel shows average Medicare payments for three common surgical procedures, indexed to their 2011 levels. Cataract and glaucoma surgeries are examples of high-volume procedures frequently performed in both ASCs and HOPDs. Average Medicare spending for these procedures declined approximately 10–15% from 2011 to 2020. In contrast, Medicare payments for laparoscopic appendectomies—reimbursable only in the HOPD setting during this period—increased by nearly 50%. The divergence in spending trajectories suggests that broader site-of-service eligibility may help place downward pressure on Medicare spending growth.

1.3 Certificate of Need Regulations

Certificate-of-Need (CON) laws are state-level regulations requiring health care providers to obtain approval from a state agency before undertaking major capital projects such as building new facilities or expanding services. These laws were initially adopted as a cost-control measure: policymakers believed that limiting duplicative health facilities and equipment would prevent an expensive medical "arms race" and align new investments with genuine community needs (Ohlhausen 2015). Under the U.S. National Health Planning Act of 1974, the federal government mandated that states implement CON programs; by 1982 nearly all states had a CON process overseeing hospitals, nursing homes, and even ASCs. The federal mandate was repealed in 1987, but as of 2025, 35 states and Washington D.C. still maintain CON regimes. Proponents of CON regulation argued that unregulated competition could lead to excess capacity – for example, too many hospital beds or surgical centers – which in turn might drive up costs as providers try to cover the fixed expenses of underutilized infrastructure by inducing low-value care. By allowing state planners to approve only projects deemed necessary, CON laws aimed to curb wasteful spending and also to encourage health providers to serve underserved populations (since some states tie CON approvals to requirements for charity care or rural services). Many states applied CON review to physician-owned outpatient facilities as well; 25 states currently require a CON to establish an ambulatory surgery center (ASC), partly to prevent such centers from "cream skimming" lucrative, well-insured patients and jeopardizing hospitals' ability to cross-subsidize less profitable services.

Critics argue that entry restrictions stifle competition by protecting incumbent providers and imposing burdensome hurdles on new entrants, which can ultimately undermine cost efficiency. Empirical research has found little evidence that CON laws achieve their stated goals of cost control or quality improvement. On the contrary, limiting competition may actually reduce supply and access: for example, states with CON regulations on ASCs have been found to host about 14% fewer surgery centers per capita

than states without such laws (Stratmann, Bjoerkheim, and Koopman 2025). Federal antitrust authorities have likewise noted that CON programs tend to suppress beneficial competition without delivering offsetting public benefits (Ohlhausen 2015). By deterring potentially more efficient entrants and new care models, overly stringent CON regimes risk stifling innovation and locking in higher-cost modes of service delivery.

2 Data

2.1 Data Sources

We use patient-level administrative data from CMS spanning the years 2011 to 2020. These data include both institutional and non-institutional claims for Traditional Medicare, or fee-for-service (FFS), beneficiaries and provide a comprehensive view of healthcare services billed by physicians, ASCs, hospitals, and other providers. Detailed demographic information is also available for each beneficiary. Access to these confidential data was obtained through a Data Use Agreement with CMS.

The analysis relies on several core files. The Carrier file contains claims submitted by professional providers, including physicians and some organizational providers such as ASCs. We have access to this file for a 20% random sample of Medicare FFS beneficiaries. The Outpatient file includes claims submitted by institutional outpatient providers, such as hospital outpatient departments. Both the Carrier and Outpatient files contain detailed information on each claim, including procedure codes, service dates, beneficiary and provider zipcodes, associated diagnoses, provider National Provider Identifiers (NPIs), and claim payment amounts. We supplement our analysis of outpatient surgeries with two additional datasets from CMS. First, the Medicare Provider Analysis and Review (MedPAR) dataset provides detailed information on inpatient hospital stays, enabling us to link inpatient utilization before and after the focal outpatient procedure. Second, we link beneficiaries to the CMS Master Beneficiary Summary File, which provides rich demographic and enrollment data, including age, race, sex, dual eligible status, and mortality. For all files except the except the Carrier file, we have access to the universe of Medicare FFS beneficiaries.

Since we observe Carrier claims only for 20% of FFS beneficiaries, we restrict the analytic sample to individuals observed in this file. This approach ensures that we observe medical care and spending comprehensively, including non-institutional care, for all beneficiaries in our sample. This is crucial given our goal to assess the causal effects of ASCs—a non-institutional provider type.

Beyond CMS files, we extend the scope of our analysis by incorporating information from several additional data sources. We use two tools from the Healthcare Cost and Utilization Project (HCUP): the Clinical Classifications Software (CCS) (HCUP 2024a), which aggregates thousands of Current Procedural Terminology (CPT) codes into clinically meaningful categories, and the Surgery Flags Software for Services and Procedures (HCUP 2024b), which classifies CPT codes as major therapeutic, minor therapeutic, or diagnostic procedures. We use Health Service Areas (HSAs), created by the US Census to study healthcare utilization, to define local markets for outpatient surgeries (Makuc et al. 1991). HSAs consist of one or more counties and were constructed to be relatively self-contained in terms of hospital utilization for Medicare

beneficiaries. Nationally, there are approximately 900 HSAs, implying that the average HSA contains about 3-4 counties, 6-7 HOPDs, and 9-10 ASCs. We also incorporate zipcode-level demographic information, such as median household income, using public-use data from the US Census. Appendix Section A.1 describes each data source in detail.

2.2 Sample Construction

To have a more homogeneous sample suited to our empirical strategy, we impose several restrictions based on procedure type, patient characteristics, and patient residential zipcode. The key restrictions are summarized below, with full details provided in Appendix Section A.2.

First, to limit heterogeneity across procedures and patients, we restrict our analysis to 15 high-volume CCS procedures performed at ASCs during the study period among FFS beneficiaries. Figure 2 lists these procedures and the corresponding patient volumes in our analysis sample.[1] Each procedure has at least 5% of its total volume performed in both HOPDs and ASCs, collectively, they account for 81% of ASC procedure volume of total Medicare ASC claims sample during this period.

Each observation in our data represents an outpatient encounter for a unique patient-procedure-site combination. Patients may appear more than once if they received multiple qualifying procedures; however, we require at least one calendar year between procedures to allow clear attribution of health and spending outcomes to an episode. We also exclude patients that had an inpatient admission in the 90 days prior to their outpatient procedure to avoid confounding from recent hospitalizations.

At the patient level, we limit the sample to individuals aged 65 and older residing in the continental United States who received one of the 15 procedures of interest at either an HOPD or an ASC. Patients were continuously enrolled in FFS Medicare for at least one year prior to the index procedure and for at least three months following the procedure to ensure measurement of utilization without censoring.

We calculate distance to facility for each patient using the spherical projection, in miles, between the centroids of the beneficiary's residential zipcode and the zipcode of the treating facility. This implies that distance to a given facility does not vary between two patients living in the same zipcode who visit the facility in the same year.[2] Using a similar method, we also calculate the distance from each patient zipcode to the nearest ASC and HOPD. Because ASCs often specialize in a narrow range of services, these distances are calculated separately by CCS category. For example, for a patient receiving cataract surgery (CCS 15), we compute the distance to the nearest ASC and HOPD observed performing cataract procedures in the sample. From these values, we derive the differential distance (DD), defined as the distance to the nearest relevant ASC minus the distance to the nearest relevant HOPD. A negative DD indicates relative proximity to an ASC; a positive DD indicates relative proximity to an HOPD.

We then impose sample restrictions based on distance. We exclude patients who travel more than 50 miles for their procedure, which is well above the 90th percentile

1. If a visit involves multiple procedures, we assign the primary procedure based on the highest-charge claim.

2. Distances may vary across years within zipcodes due to facility entry and exit over time.

distance in the sample (36 miles), and may reflect the presence of unobserved differences in access or patient preferences. Second, we restrict the sample to patients residing in zipcodes with DD values between -10 and +10 miles. Patients in zipcodes with greater differentials are highly unlikely to choose the more distant provider type, and small changes in relative distance are unlikely to affect their choice of provider. This restriction also limits heterogeneity in the types of neighborhoods included in the sample since they are not too far from either type of sites. Zipcodes with extreme DD values are overwhelmingly far from one type of provider and may have unobserved features that make them less appealing. Finally, to improve balance in patient characteristics across zipcodes, we exclude zipcodes that are outliers on demographic characteristics. Appendix Figure A.2 presents a flow diagram of sample construction algorithm.

2.3 Outcome Measures

We study a range of healthcare utilization, quality, and spending outcomes. For utilization, our primary outcomes include 90-day office visits and a subset of "related" office visits. Office visits are identified using place of service and revenue center codes in both the Carrier and Outpatient files, as detailed in Appendix Table A.1. Related office visits are flagged by examining the primary diagnosis code on each visit; if the code matches one of the procedure-specific diagnosis codes listed in Appendix Table A.2, the office visit is classified as related. We examine changes in office visits (related and all) on the extensive and intensive margins.

Given the low mortality associated with outpatient procedures (Fleisher et al. 2004), we include a set of quality indicators with varying levels of severity to improve sensitivity to differences in clinical quality. These include indicators for a hospital admission, unscheduled need for care in the form of an urgent care visit or emergency department (ED) presentation, surgical complication, and death occurring within 90 days of the index procedure. Surgical complications are identified using International Classification of Diseases, Ninth Revision (ICD-9) and Tenth Revision (ICD-10) diagnosis codes based on definitions from previously published literature (Rosas et al. 2021).

For spending outcomes, we measure total Medicare spending on the focal procedure, as well as the spending including the focal procedure and the 90 days following. All spending measures include both facility and physician fees and are inflation-adjusted to 2021 dollars. Since a nontrivial fraction of patients have zero spending in the follow-up period, we also examine changes in the probability of having nonzero spending.

2.4 Descriptive Statistics

Figure 2 presents the distribution of procedures in our sample, grouped by CCS code and ranked by volume in the ASC setting. We include the 15 highest volume ASC procedures among FFS beneficiaries, although the top four—cataract surgeries, colonoscopies, endoscopies, and spinal injections (including stimulator insertions)—account for the majority of procedure volume. Across all 15 procedure groups, the average share performed in ASCs is 54%. Cataract surgeries have the highest ASC share at 78%, while extracorporeal lithotripsies have the lowest at 26%.

Table 1 summarizes procedure and patient characteristics by site of service. Panel A presents the top five procedures, ordered by patient volume. Colonoscopies and cataracts together account for about two-thirds of total ASC volume. Colonoscopies also contribute a disproportionate share of HOPD procedures, although HOPD volume is more evenly distributed across the remaining procedures. These differences in procedure mix between the two types of settings suggest the importance of controlling for procedure type in the empirical analysis.

Panels B and C present evidence on patient and zipcode demographics, respectively. ASC patients appear similar to their HOPD counterparts on average age, and the proportions that are female or black. Consistent with prior work, however, ASC patients are less likely to be dually eligible for Medicaid and tend to reside in zipcodes with slightly higher levels of affluence (Janeway et al. 2020; Zabinski 2023).

Panel D presents evidence on patient health risk based on diagnoses and utilization in the 90 days before the focal procedure. We calculate comorbidity scores using the Quan adaptation (Quan et al. 2011) of the Charlson Comorbidity Index, applied to all diagnoses observed during the 90-day pre-procedure window. On average, ASC patients have substantially lower comorbidity scores, consistent with risk selection into lower-acuity outpatient settings. For example, ASC patients are 7 percentage points (25%) less likely to have two or more Charlson comorbidities, which we define as "high-risk" for the purpose of this analysis. Similarly, they are 3 percentage points (30%) less likely to have visited an emergency department in the prior 90 days, and they incur $150 (11%) lower total healthcare spending over that period as well. These relatively large differences in prior utilization and spending suggest the presence of unobserved differences as well. Complications are identified using ICD codes defined by Mull et al. (2018).

Panel E presents evidence on the key outcomes of interest. ASC patients have higher rates of office and urgent care visits in the 90 days following surgery but have lower rates of ED visits. Taking into account the procedure mix, ASC patients incur $360 less in spending than their HOPD counterparts on the focal procedure and $670 less in total 90-day spending—a 17% reduction relative to the HOPD mean. However, given ASC patients' lower baseline medical complexity, these differences likely reflect a combination of selection and treatment effects, highlighting the importance of using a quasi-experimental research design to isolate the impact of ASCs on patient outcomes.

3 ASC effect on total utilization

A concern with the expansion of any outpatient care site that substitutes for costly hospital care is that while it reduces resource use per episode, it may lead to an increase in the total use of care by local residents. This concern is also relevant in the case of ASCs because they exclusively perform non-emergent procedures which patients may choose to delay or avoid. For example, patients may be more willing to act on a recommendation and get a procedure such as a colonoscopy done if it is not in a hospital setting and therefore less intimidating or disruptive to their routine. In addition, physicians may change their clinical practices in response to ASC availability in the local market and recommend more marginal patients to get the procedure. Hence, the *total* effect of ASCs on spending depends not only on their effect for each case but also on whether they induce new patients to get a procedure who otherwise would not have.

To be clear, a net increase in procedures per capita may improve consumer welfare if the marginal procedure performed due to ASC availability is medically valuable. Hence, the concern is mainly that we will miss a potentially important piece of the puzzle if we do not examine the extensive margin effect of ASC availability.

The prior literature offers an example of a low-cost care model that seems to increase total spending. Currie, Karpova, and Zeltzer (2023) examine the effects of entry by an Urgent Care Center (UCC) in a zipcode. Using Medicare FFS beneficiary claims data over 2006–16, they compare zipcodes experiencing entry to never-treated zipcodes and show that UCC entry leads to an increase in total care utilization by Medicare beneficiaries and a 4% increase in annual spending. Hence, instead of reducing total medical spending, UCCs lead to an increase in spending, with much of the increase coming from incremental hospital inpatient spending. It is therefore worth testing the equivalent concern in the case of ASCs.

It should be noted that patients must be referred to an ASC by a physician for a specific procedure due to medical need unlike in the case of UCCs which provide walk-in care for urgent conditions. This key difference in the institutional setting may mitigate concerns about the extensive margin effects of ASCs. A recent study by Yu (2023) on the effects of ASC entry finds results consistent with this conjecture. He leverages a CoN deregulation in Missouri in 2002 as an exogenous policy shock to examine the effects of ASC entry. Using claims data on Medicare FFS beneficiaries over 1998–2013 and a difference-in-differences design comparing markets in Missouri to those in states that continue CoN restrictions on ASCs, he finds that ASC entry increases the share of procedures performed at ASCs but the total number of procedures performed remains flat. Since our sample of Medicare claims is national in scope, we can test this hypothesis beyond Missouri by leveraging ASC entry into local markets during our sample period.

We follow Einav, Finkelstein, and Mahoney (2023) and estimate the causal effect of ASC entry in a market on utilization of care by Medicare FFS beneficiaries. We use (provider) counties to define markets. Although healthcare use patterns don't necessarily follow county boundaries, using counties permits more granular analysis than hospital referral regions (HRRs) or health service areas (HSAs) which are more commonly used market definitions in the literature.[3] We identify ASC entry into counties using information in the public-use National Plan and Provider Enumeration System (NPPES) data provided by CMS. We infer an ASC's entry year by observing the year it is issued a unique National Provider Identifier (NPI). Since we also observe the zipcode for providers in the NPPES data, we can link ASC entry to specific counties. A nice feature of this approach is that it does not infer entry based on claims, which can mechanically generate changes in county-level outcomes. Using this approach, we identify 197 counties which experienced ASC entry (across specialties) during the period 2012–2018.[4] The comparison group consists of 1,009 counties that did not experience ASC entry through the end of 2019. Due to concerns about the effects of the COVID pandemic on the volume of elective procedures, we exclude 2020 from this analysis. 316

3. There are 306 HRRs and approximately 900 HSAs in the US, against over 3,000 counties.

4. We include in this tally counties that last experienced entry in 2007 or earlier, allowing a 4-year moratorium period so that utilization trends in these counties are stable by 2012. If we limit to counties that have *never* experienced ASC entry then only 41 counties would be treated between 2012 and 2018.

counties experienced entry in the period 2008–11 and are considered "always treated" for this analysis. A fourth group of 14 counties experienced entry in 2019 and are also excluded from the sample. These four groups account for over 1,530 counties and all the ASCs and HOPDs in the US were located in these counties during our sample period. The remaining approximately 1,600 counties are not in the sample because they had neither an HOPD nor an ASC during the sample period.

We estimate a staggered difference-in-difference model of the form presented in Equation 1 below.

$$Y_{ct} = \alpha_c + \alpha_t + \beta\, 1(entry)_{ct} + \epsilon_{ct}. \tag{1}$$

The coefficient of interest in this model is β, the average treatment effect of ASC entry in the affected counties (ATT). Although we observe treated counties for up to 7 years after ASC entry, the average post-treatment period across treated counties is 4.5 years. Identification of the ATT relies on the parallel trends assumption that imposes common trends between treated and untreated counties in the absence of treatment. To assess the plausibility of this assumption we also estimate dynamic pre- and post-treatment coefficients using Equation 2 and present these in event study plots in Figure 3.

$$Y_{ct} = \alpha_c + \alpha_t + \sum_{s=-7}^{s=7} \beta_s\, 1(entry)_c \cdot 1(t = s) + \epsilon_{ct}. \tag{2}$$

Although we estimate a fully saturated specification in event time we present coefficients only up to 4 years around the year of entry since we observe later post-treatment years for relatively few treated counties. This approach follows the guidance against binning by the recent econometrics literature on staggered difference-in-differences (Baker, Larcker, and Wang 2022). Standard errors are clustered by county since treatment (ASC entry) is defined at the county level in this analysis. Finally, due to concerns about bias due to staggered timing of treatment and likely heterogeneity in treatment effects, we use the estimator proposed by Callaway and Sant'Anna (2020) which is robust to these concerns and recovers the sample-weighted ATT. To ensure that the ATT estimate corresponds to the event study plots, we average over effects in the same event window.

Figure 3 Panel (a) presents the effect of ASC entry in a county on the share of relevant procedures performed at an ASC. We consider this equivalent to a "first-stage" outcome since ASC entry should lead to an increase in the share of procedures performed at ASCs. Indeed, the figure shows that the share of procedures performed at ASCs increases sharply after entry and the effect stabilizes at 10 percentage points 3 years after entry. This increase is about 20% of the baseline ASC share prior to entry, about 50 percentage points. Importantly, the pre-treatment dynamic effects strongly suggest that treated and untreated counties were trending in parallel prior to entry. The estimated ATT is 0.078 with a standard error of 0.015. This evidence reassures us that ASC entry has both an economically meaningful and statistically significant effect on ASC use in our sample.

Figure 3 Panel (b) presents the corresponding effect on mean Medicare spending per procedure in the county after entry. This plot suggests a decrease in mean spending, that stabilizes around $50 per procedure. The ATT estimate is -$42, with a standard error of $16. As reported in Section 1, the average difference in the Medicare procedure reimbursement between HOPD and ASC during this period was about $800. Hence, a 8% switch would predict a decrease of $64, well within the confidence interval of this estimate. Hence, the effects on these two outcomes jive well together.

Finally, Figure 3 Panel (c) presents the effect of ASC entry on the total number of procedures per 10,000 FFS beneficiaries. Unlike Panel (a), this plot suggests that the total number of procedures performed remains flat up to 4 years after ASC entry. The estimated ATT is 29 with a standard error of 42. Not only this statistically imprecise, it is also small relative to the baseline procedure rate of 839 per 10,000 beneficiaries, about 4%. Reassuringly, our estimate is close in relative magnitude to the 3% medium-run increase (also statistically insignificant) reported by Yu (2023) for Missouri.

Taken together, this evidence implies that ASC entry into a market leads patients to substitute from the HOPD to the lower cost ASC setting, but does not lead to a detectable expansion in the use of care by beneficiaries. This further implies that ASCs impact medical spending and patient health primarily through their substitution of hospital care. Accordingly, the rest of the empirical analysis focuses on estimating the causal effect of ASC substitution of hospital care.

4 Empirical Strategy

Estimating the causal effect of ambulatory surgery centers on patient care outcomes remains a challenge due to well-documented concerns about patient selection into ASCs based on unobserved differences in health or complexity. Descriptive evidence from Table 1 supports this concern, as ASC patients appear systematically healthier than those treated in HOPDs. Prior work has also documented selection along both clinical and economic dimensions. For example, ASCs tend to treat patients with lower clinical risk and more favorable reimbursement profiles (Gabel et al. 2008; Mathis et al. 2013), and David and Neuman (2011) find that physicians refer higher-risk patients to HOPDs while reserving ASC care for healthier individuals. Although Medicare claims offer detailed data on diagnosis codes, demographics, utilization history, and clinical updates at the encounter level, they likely do not capture all the information available to physicians at the point of referral or shared decision-making.

Selection bias poses a significant obstacle to the estimation of causal effects. To illustrate this, Equation 3 below presents a linear model to estimate the effect of ASCs on patient outcomes.

$$Y_{ihjt} = \alpha_{ht} + \alpha_j + \beta\, ASC_{ihjt} + X_i'\,\delta + W_{z(i)t}'\,\tau + \epsilon_{ihjt}, \tag{3}$$

where Y_{ihjt} represents the outcome of interest, typically related to utilization of care or medical spending. ASC_{ihjt} denotes the regressor of interest, which is an indicator set to one if patient i in HSA h receives a procedure j in year t at an ASC. We define procedures granularly using CPT codes (instead of CCS groups) to compare patients within very narrowly defined cells. X_i is a vector that controls for patient-level characteristics,

including age, gender, race, dual eligibility status, Charlson comorbidity score, and indicator for prior healthcare utilization (HOPD, ASC, emergency department, or urgent care visit in the 90 days prior to surgery). $W_{z(i)t}$ is a vector of characteristics of the patient's zipcode, $z(i)$, in year t, including median household income, Area Deprivation Index (ADI), total procedure volume, the share of procedures performed at ASCs, and the share of patients with an office visit in the 90 days prior to surgery. We include HSA-by-year fixed effects α_{ht} to account for unobserved, time-varying HSA-specific factors and allow HSAs to flexibly evolve along different time trends. We also include fixed effects for the CPT code fixed effects α_j to control for unobserved procedure-specific differences. ϵ_{ihjt} represents idiosyncratic unobserved time-varying factors.

The preceding description shows that this is already a very flexible model with a rich set of covariate controls. However, we anticipate that estimating this model via ordinary least squares (OLS) is susceptible to selection bias concerns. The coefficient $\hat{\beta}$ recovers a combination of the true average treatment effect on the treated (ATT) and a bias term. For example, Table 1 Panel D shows that ASC patients had about $150 lower spending than HOPD patients in the 90 days prior to surgery, suggesting that they are healthier and would have required less care in the postoperative period relative to an average HOPD patient, even if they were treated at an HOPD. The OLS estimate of β with regards to the effect of ASC on spending partially reflects this baseline difference in complexity and will *overstate* the savings due to ASCs. A similar argument can be made with regards to estimates of the beneficial effects of ASCs on health.

Thus, to address the potential endogeneity of facility choice, we leverage a two-stage least squares (2SLS) instrumental variables (IV) approach. In the first stage, we estimate ASC use, ASC_{ihjt}, using differential distance, D_i, as an instrument (Equation 4). The fitted values of the outcome ($\hat{ASC}_{ihjt}$) from this regression, purged of spurious factors, are then used instead of the endogenous observed ASC indicator in the second stage model in Equation 5. Formally, the first- and second-stage regressions are:

$$ASC_{ihjt} = \alpha_{ht} + \alpha_j + \gamma\, D_i + X_i'\,\zeta + W_{z(i)t}'\,\lambda + \eta_{ihjt}, \tag{4}$$

$$Y_{ihjt} = \alpha_{ht} + \alpha_j + \beta_{IV}\, \hat{ASC}_{ihjt} + X_i'\,\delta + W_{z(i)t}'\,\tau + \epsilon_{ihjt}. \tag{5}$$

All terms here have the same meaning as in Equation 3. D_i represents the differential distance between the patient's residence and the nearest ASC compared to the nearest HOPD. In practice, we estimate both models simultaneously using 2SLS. Standard errors are clustered by patient zipcode, $z(i)$, to account for potential unobserved correlation between people living in the same neighborhood.

Next, we describe the instrument, differential distance, and assess the plausibility of the standard IV assumptions being satisfied in this setting. Under these assumptions, the coefficient β_{IV} recovers the local average treatment effect (LATE) of ASC use for the subset of patients whose provider choice is influenced by their differential distance, or relative proximity, to an ASC.

4.1 Distance-based Instrument

We construct a differential distance instrument to overcome endogenous patient selection into ASCs (McClellan, McNeil, and Newhouse 1994b; Grabowski et al. 2013;

Gupta et al. 2024). This approach leverages the documented tendency of patients to prefer geographically proximate healthcare providers, including for surgical care (Kessler and McClellan 2000). Specifically, distance to facility has long been known as a key nonclinical factor influencing patient decisions for surgical care (Yahanda et al. 2016). As previously described, in this study the differential distance is the difference in miles between a patient's residence and the nearest ASC observed to be performing the focal procedure, minus the distance to the nearest HOPD performing the same procedure. Lower values indicate greater relative proximity to an ASC, and zipcodes where the closest ASC is geographically closer than the closest HOPD have negative values. Intuitively, we expect that patients with a lower value of D_i are more likely to choose an ASC, all else equal. Although such distance-based instruments have been used by several studies to overcome selection for other types of care, we are the first to apply it to ASCs. We therefore carefully assess the validity of this instrument in our setting. We discuss the results of these tests below.

Instrument Relevance. A valid instrument for ASC use must strongly predict ASC choice after conditioning on covariates and fixed effects. This is the relevance condition, which can be tested by estimating the first stage model in Equation 4. Table 2 presents the corresponding results. We present the coefficient γ on D_i obtained from three different specifications. The first column presents the result from a model that includes all relevant fixed effects and zip-code covariates but does not adjust for differences in observed patient risk. The coefficient indicates that a one-mile increase in differential distance is associated with a 0.73 percentage point decrease in the probability of receiving care at an ASC. The coefficient is precisely estimated and the corresponding F-statistic is over 500, implying a very strong first stage relationship. Apart from statistical significance, the relationship is also economically meaningful. Patients residing in a zipcode with one standard deviation lower differential distance (3.4 miles) are predicted to have a 2.5 percentage point higher probability of choosing an ASC. This represents 5.2% of the standard deviation in ASC use across zipcodes.

Column 2 presents our preferred or baseline model which also adjusts for differences in patient risk in addition to the covariates present in column 1. This addresses concerns about adjusting for differences in observed risk between ASC and HOPD patients. The inclusion of patient covariates does not materially affect the first stage coefficient. Column 3 presents the result from a model which also includes state fixed effects. This ensures that we compare ASC patients to their HOPD counterparts within the same state. Since states vary substantially in their encouragement of ASCs and, more generally, of competition, controlling for this eliminates a potentially important confounder. Again, the coefficient remains unaffected, implying that differential distance is similarly predictive of ASC choice even if we limit the variation to markets within a state. In the appendix, we show that the results are robust to using more granular county by year fixed effects instead of HSA by year (see Table A.3).

Figure 4a presents a binned scatter plot that visually depicts the first-stage relationship between differential distance and choosing an ASC. The plot is constructed by separately regressing both the indicator for surgery at an ASC and differential distance on the patient controls and fixed effects in our preferred model. Residuals from these regressions are then binned into 10 equal-sized bins and the mean values in each decile

are plotted. The observed downward slope indicates a negative correlation between differential distance and surgery at an ASC. This nonparametric approach also provides support for a linear specification since the probability of choosing an ASC declines approximately linearly with an increase in distance.

Conditional Random Assignment. A key untestable assumption underlying the LATE is conditional random assignment, sometimes also known as instrument balance. This requires that the instrument be uncorrelated with the second-stage error term after covariate adjustment. This condition subsumes the exclusion restriction, which states that differential distance affects outcomes of interest only through its influence on the choice of an ASC. Although conditional random assignment cannot be verified directly, we assess its plausibility using three types of evidence. All three tests probe whether the instrument is balanced across observable characteristics, which lends credibility to the assumption that it is also uncorrelated with unobserved confounders. This is equivalent to testing covariate balance between the treated and control groups in a randomized controlled trial.

First, Table A.4 compares patient characteristics across three groups defined by their proximity to ASCs: patients living closer to ASCs (DD < 0), equidistant to both settings (DD = 0), and closer to HOPDs (DD > 0). Consistent with a valid instrument, ASC use declines monotonically with differential distance, from 74% in the ASC-proximate group to 53% in the HOPD-proximate group. In terms of balance, patient demographics and risk measures are nearly identical across groups. Charlson comorbidity scores are higher among patients farther from ASCs, but other risk proxies—including prior emergency department or urgent care use and 90-day preoperative spending—show negligible differences. However, these are unadjusted means, which may partly reflect differences in markets where these individuals reside. This motivates the next two tests.

Second, we use binned scatter plots to assess the correlation between differential distance and various measures of patient risk or complexity. For example, Figure 4b shows that the probability of the patient being classified as high-risk (Charlson comorbidity score $\geq$ 2) does not vary with differential distance. The slope of the fitted line is small and statistically insignificant, suggesting good balance in patient risk across different values of distance. Table 2 further supports this interpretation, as the coefficient on differential distance is nearly unchanged when patient controls are added in Column 2. Beyond Charlson scores, we examine additional potential indicators of risk. These include indicators for patient demographics (Black race), dual eligibility status (a marker of low-income status), a continuous measure of 90-day preoperative healthcare spending, and prior healthcare utilization (emergency department and urgent care visits). Binned scatter plots and slope estimates for selected measures are presented in Appendix Figure A.3. Of all measures tested, only dual eligibility shows a correlation with the instrument. However, the coefficient is small in magnitude (0.0002), indicating only a modest difference in the share of dual-eligible beneficiaries between zipcodes relatively closer to and farther from ASCs. Although these models partial out HSA by year and CPT fixed effects, they are still not as rich as our preferred specification since they do not control for zipcode characteristics and are tests of bivariate correlation.

Table 3 presents the correlation between patient risk factors and both ASC use (Column 1) and differential distance (Column 2) using our preferred specification. Column

1 reports linear probability estimates of the correlation between ASC utilization and patient characteristics, with zipcode-level controls, fixed effects, and clustered standard errors as specified in our baseline specification (Equation 5). Consistent with expectations, ASC use is significantly associated with all observable measures of patient risk. Strikingly, all coefficients in Column 2, including on dual eligibility, are small and statistically insignificant. Discussing an example will help illustrate the benefit of using the IV approach. Column 1 shows that ASC patients have \$60 lower preoperative spending on average, about 5% of the mean spending level. This suggests that ASC patients would also have lower post-operative spending regardless of where they get their procedure done, and is a potential measure of the extent of selection into ASC use. However, when we compare across different levels of differential distance, the corresponding coefficient in column 2 is small and statistically insignificant. This is consistent with the idea that distance quasi-randomly assigns patients to ASCs or HOPDs and eliminates potential selection. This statement can be made jointly about all the measures of demographics and risk assessed here. A joint F-test of all covariates yields a p-value of 0.27, supporting the interpretation that differential distance is conditionally orthogonal to individual-level observables. Taken together, this evidence strongly suggests that differential distance is a well balanced instrument in our setting.

Monotonicity. The second untestable assumption that needs to be satisfied to recover an unbiased estimate of the LATE is monotonicity. Under this assumption, an increase in differential distance must weakly decrease the likelihood of receiving surgery at an ASC for all patients. The assumption is defined at a patient level in terms of potential outcomes, which are unobserved, and is therefore untestable. The binned scatter plot depicting the first-stage confirms that this is true on average for Medicare patients in our sample. The assumption is intuitive and plausible in this setting of patients choosing a health care provider.

4.2 Compliers

Our 2SLS estimates represent the LATE for marginal patients—those whose choice of facility is influenced by their relative proximity to an ASC versus an HOPD. These patients, known as "compliers" in the LATE literature, may differ from the average ASC patient in the sample. To better interpret the IV coefficients, we first present evidence on the share of patients in the sample who are compliers and characterize their attributes relative to the average patient. For this exercise, we follow the methodology used by Agan, Doleac, and Harvey (2023) in their recent study of the effects of misdemeanor prosecution, in which they deployed an IV approach.

In addition to compliers, the LATE literature defines two other subgroups of individuals. The treatment choices of individuals in both subgroups are insensitive to the value of differential distance they face. "Always takers" will always choose an ASC regardless of relative proximity, while "Never takers" will never do so. We estimate the share of always-takers in the sample as the percentage of patients residing in zipcodes at the 99^{th} percentile of differential distance (9.27 miles) and still choosing an ASC even though ASCs are substantially less accessible than HOPDs. Along similar lines, we estimate the share of never-takers as the percentage of patients residing in zipcodes at the

1^{st} percentile of differential distance (-8.83 miles) and choosing an HOPD even though ASCs are relatively much more accessible. The proportion of compliers is derived as the remaining share, calculated by subtracting the shares of always-takers and never-takers from 100%. This strategy hinges on the validity of the conditional independence and monotonicity assumptions discussed above.

Applying this framework, we estimate that 43% of the patients in our sample are always-takers, 19% are never-takers, and 38% are compliers. The sizable share of compliers is reassuring since it suggests that proximity plays an important role in site selection, further bolstering the relevance of our instrument. This estimate likely represents a lower bound, as our distance measure—calculated between zipcode centroids rather than exact patient and facility locations—suffers from measurement error that could attenuate the importance of distance in predicting ASC choice.

Table 4 presents the characteristics of always-takers, never-takers, and compliers on demographics and measures of risk. The results reveal intuitive patterns of sorting into ASCs based on patient health status. For example, we expect that always-takers will be healthier and have lower levels of prior medical spending than the other two groups since they prefer ASCs regardless of the travel "cost." Indeed, we find that always-takers are healthier on multiple dimensions, with lower Charlson comorbidity scores (0.71 vs. 0.8 in the full sample), lower likelihood of recent urgent care use (0.02 vs. 0.03), and \$90 (about 8%) lower preoperative spending. This pattern is consistent with risk selection into ASCs for healthier, lower-cost patients.

In contrast, never-takers exhibit more complex health profiles, with higher Charlson scores (0.99), much more likely to be classified as high-risk (0.27), more likely to have visited an ED recently (0.10), and \$70 higher preoperative spending (6%) than the average patient in the sample. Their preference for hospital-based care likely reflects appropriate risk-based selection given their greater clinical complexity.

Compliers are similar to never-takers in their health status, with similar Charlson scores (1.01), likelihood of being high-risk (0.27), and ED utilization patterns (0.09). This suggests that our LATE estimates are particularly relevant to moderately complex patients who might traditionally receive care in hospital settings. Compliers are also demographically similar to the full sample, with only modest differences in age, gender, and dual eligibility-and nearly identical racial mix. This demographic resemblance enhances the generalizability of the LATE to the broader Medicare population and suggests that differential distance affects site of care primarily through logistical convenience, a factor that is plausibly orthogonal to health risk. When we discuss the IV estimates in Section 5, we use the terms "patients" and "compliers" interchangeably since the IV coefficients represent the causal effect only for complier patients.

4.3 Interpreting the OLS estimate

We will primarily focus on the IV estimates of getting a procedure at an ASC relative to an HOPD. However, it is useful to briefly discuss the OLS estimate, which serves as a point of comparison for the IV coefficients. An unbiased OLS estimate recovers the average treatment on the treated (ATT), where the treatment is receiving the procedure at an ASC instead of at an HOPD. Simply put, the OLS estimator compares ASC patients to HOPD patients, and recovers the difference in outcome means after

adjusting for fixed effects and covariates. Within the IV framework, ASC patients are a mix of always-treated and compliers (who happen to live close to an ASC), while HOPD patients are a mix of never-takers and compliers who live further away from an ASC. The descriptive evidence on the three sub-groups discussed above shows that always takers are much healthier and likely to have lower healthcare use and spending than never takers and compliers. Hence, *unbiased* OLS estimates of the utilization and spending reductions associated with ASC care will be larger than the IV estimates, which are estimated by comparing ASC and HOPD patients only *within* the complier group. In addition, if there is selection into ASC use on unobserved factors correlated with healthcare use, as suggested by the results in Table 3, the OLS estimates will be biased and will not recover the ATT.

5 Results

This section presents the results on the causal effects of ASCs on healthcare use, clinical quality, and spending. We present OLS and IV estimates for all three types of outcomes.

5.1 Utilization of care

We begin by examining whether getting a procedure at an ASC instead of an HOPD affects healthcare use in the 90-day period immediately following the focal procedure. Specifically, we focus on physician consultation visits in this section, which includes visits to primary care physicians (PCPs) and specialists. Table 5 presents the corresponding point estimates, with OLS estimates in Panel A and IV estimates in Panel B. Figure 5 presents binned scatter plots showing the reduced form relationship between select outcomes of interest and the instrument. These follow the same format as the plots in Figure 4.

Columns 1–3 examine the effect on physician office visits, regardless of whether they are related or unrelated to the focal procedure. We examine the effect on the total number of visits (column 1), the extensive margin effect (column 2), and the intensive margin effect for patients that have at least one visit during the 90-day period (column 3). For each outcome, we also report the mean value for untreated compliers, i.e., those who receive the focal procedure at an HOPD. This serves as a benchmark for the treatment effect estimated for compliers.

The OLS and IV estimates both imply that ASCs lead to more physician office visits following the focal procedure, with increases on both extensive and intensive margins. However, the IV estimates are an order of magnitude larger than the corresponding OLS coefficients. For example, the IV coefficient implies ASC patients experience 1.2 more office visits than hospital patients, an increase of nearly 30% relative to the mean. Figure 5 Panel (a) presents the corresponding binned scatter plot, showing a roughly linear decrease in the number of office visits as patients are located relatively further away from ASCs.

In contrast, the corresponding OLS coefficient implies an increase of only 0.14 visits. Both coefficients are estimated very precisely and their confidence intervals do not overlap. It is intuitive that the OLS estimates are of lower magnitude since they partly

reflect the experience of much healthier always-takers who likely have lower need to visit a physician. Similar patterns follow for the extensive and intensive margin coefficients. It is noteworthy that ASC patients are nearly 4 percentage points more likely to have any follow-up visit than HOPD patients. Since follow-up visits facilitate care coordination between the patient and physicians, thus helping to prevent more expensive emergency care, we interpret greater likelihood of having a visit as a benefit of ASC use for patients. Figure 5 Panel (b) visually shows the relationship between the likelihood of having any follow-up visit and differential distance.

To further help interpret the finding on office visits following the focal procedure, we investigate whether ASC patients have more visits for reasons that can be *directly* linked to the focal procedure. An increase in such visits is more likely a function of differences between ASC and HOPD providers in the discharge routines or follow-up protocols they practice. Using the diagnoses codes recorded for the office visits and in consultations with expert physicians, we determine whether a visit is "related" to the focal procedure. Table A.2 lists the codes we classify as related for each focal procedure in our sample. We deem approximately a quarter of all office visits as related using this approach, and only 43% of patients have a related visit at all. A caveat with this exercise is that it almost surely understates the share of visits directly related to the focal procedure. For example, general evaluation and management visits are not tagged as related since they cannot be definitively linked to the focal procedure. Yet, some of them are surely follow-up visits recommended by the provider at the time of discharge from the focal procedure.

Table 5 columns 4–6 present the corresponding results on the number of related visits, following the same format as in columns 1–3. The patterns of the OLS and IV results for related visits are generally similar to those for all visits, with a little more divergence between the OLS and IV coefficients here. The IV coefficients imply that ASC patients experience more related visits on the extensive and intensive margins. Although imprecisely estimated, the coefficient on the extensive margin effect for related visits in column 5 is only slightly smaller than that for all visits in column 2. This suggests that most of the difference between ASC and HOPD patients in the likelihood of having a follow-up visit is on account of ASC patients being more likely to have a related visit at all. In contrast, the OLS estimates are much smaller and inconsistent in sign since they imply an increase on the extensive margin but a decrease on the intensive margin.

5.2 Care quality

Next, we examine whether getting a procedure at an ASC relative to a HOPD results in greater incidence of adverse events for patients in the following 90 days, a standard approach used by insurers, regulators, and economists to infer clinical quality (Chandra et al. 2016; Gupta 2021). The conventional quality metrics used in hospital performance pay programs and studied in the economics literature are short-term mortality and readmission rates, and we consider these in our analysis. Since the focal procedure does not involve an inpatient admission in the first place, we examine the probability of an inpatient admission instead of a readmission. In addition, since the focal procedures are typically less complex in nature and a majority of patients are in good health (only

$\approx 20\%$ of patients have a Charlson comorbidity index greater than 1), we also examine other measures which represent more frequently occurring events and are therefore likely to be more sensitive to changes in care quality. These are, in decreasing order of severity, the likelihood of experiencing a surgical complication, having an ED visit, and an urgent care visit. We also define a composite outcome of unscheduled care, which is a flag for experiencing an urgent care or ED visit.

Table 6 presents the corresponding OLS and IV results for these outcomes, ordered roughly in decreasing order of severity. The OLS coefficients are uniformly negative and statistically significant, implying that ASCs deliver higher care quality. In contrast, the IV coefficients are usually imprecisely estimated and closer to zero or even positive in case. Again, this divergence is intuitive since the OLS coefficients partly compare healthier always takers to much sicker never takers. Unobserved selection into ASCs based on health risk may be biasing OLS coefficients further downward.

The IV coefficients imply no detectable differences between ASC and HOPD patients on the probability of death, inpatient admission, surgical complication, and in receiving unscheduled care in the 90 days following the focal procedure. The latter coefficient is negative and 3% of the mean value, suggestive of a modest decline. However, the imprecisely estimated effect on unscheduled care masks dramatic differences in the underlying effects on ED and urgent care. ASC patients are 2.5 percentage points (21%) less likely to have an ED visit but 2.1 percentage points (91%) more likely to have an urgent care visit. Figure 5 Panels (c) and (d) present the corresponding reduced form binned scatter plots, again suggesting linear relationships with differential distance. These patterns strongly suggest that, relative to HOPD patients, ASC patients substitute ED care with less expensive urgent care.

One possible explanation is geographic: patients in zipcodes closer to ASCs are also, on average, closer to urgent care facilities. We explore this co-location hypothesis by examining the proximity of patients to urgent care facilities based on differential distance. Patients in zipcodes with negative differential distance are, on average, 1.4 miles closer to urgent care centers than patients in zipcodes with positive differential distance (mean distances: 4.4 vs. 5.8 miles; $p<0.001$). Another possibility is clinical guidance: ASC patients may be advised to visit urgent care centers for postoperative concerns, whereas HOPD patients may be directed back to hospital-affiliated services. However, this hypothesis cannot be directly tested within our data. While we are unable to distinguish between these mechanisms, the substitution from ED to urgent care likely contributes to downstream cost savings, given the lower intensity and price of urgent care services. Overall, however, we interpret the evidence as suggesting no important difference in care quality between ASCs and hospital outpatient departments.

5.3 Medical spending

Table 7 presents the results related to healthcare spending. Since there is a lot of heterogeneity in spending levels across the different procedures included in our sample, we prefer to examine the effects on log-transformed values of spending. This allows us to directly interpret the estimated coefficients in percentage terms. Appendix Table A.5 presents the corresponding set of results using spending in levels. The results are qualitatively similar regardless of whether we model spending in levels or logs.

Column 1 presents the effect on total payment for the focal procedure. Since Medicare reimburses ASCs a lower amount for the same procedure (about 38% on average), we expect to find lower average spending on the focal procedure for ASC patients. This is the only outcome we study where we do not expect a substantial difference between the OLS and IV estimates. Medicare's payment is entirely determined by the CPT code of the focal procedure. In other words, both always takers and compliers will incur the same amount for the same CPT code at an ASC, and similarly compliers and never takers will incur the same amount at an HOPD.[5] This eliminates one source of difference between the OLS and IV estimates. Further, by construction, for any risk factor to affect payment, it must be recorded on the billing claim and therefore is observed by the econometrician. Hence, there is little scope for unobserved selection affecting this particular outcome. Intuitively, we find that the OLS and IV coefficients are similar for this outcome. Since spending is expressed in logs, the implied effect in percent terms is obtained by exponentiating the coefficients. The OLS and IV coefficients imply, respectively, that the focal procedure costs ASC patients 30% and 32% less than if they went to an HOPD. Applying the latter estimate to the mean cost of a focal procedure, $1,504, implies an average saving of $486 per ASC patient. Figure 5 Panel (e) presents the corresponding binned scatter plot of the reduced form relationship and clearly shows a linear increase in procedure cost as patients are more distant from ASCs.

Next, we investigate whether ASC patients also incur lower spending in the ensuing 90 days. This estimate will be determined by differences in healthcare utilization between ASC and HOPD patients during this period. It is difficult to predict ex-ante since the results in the previous sections have shown that ASC patients have more physician consultations, which may increase spending, but also have a lower likelihood of going to the ED, which should reduce spending. Column 2 presents the effect on the likelihood of having any spending in this period. We obtain a positive and statistically significant IV coefficient implying ASC patients are nearly 3 percentage points more likely to have nonzero spending. This is consistent with the result in Table 5 that they are 4 percentage points more likely to have a physician office visit. Column 3 presents the effect on spending in the 90 days following the focal procedure among patients with nonzero spending. The IV coefficient is positive but imprecise, so we do not emphasize it. In contrast, the OLS coefficient implies a reduction in spending on the intensive margin among ASC patients.

Finally, we quantify the difference in spending between ASC and HOPD patients over the entire episode–which includes the focal procedure and all care received in the following 90 days. Column 6 presents the corresponding results. The IV coefficient indicates that ASC patients have 16% lower spending (exponentiating the estimate), which implies an average saving of $628 on a base of $3,892. It is noteworthy that nearly a quarter of the implied total saving amount ($142) is due to differences in healthcare utilization in the follow-up period and not mechanically due to differences in CMS reimbursement rates.

5. The payment also varies across markets because of differences in costs of key inputs such as real estate and labor. However, since our models include HSA x year fixed effects, these differences are eliminated.

5.4 Falsification and Robustness Checks

This section provides further evidence in support of our empirical design by assessing the plausibility of the exclusion restriction and the sensitivity of the main results to changes in sample construction or models. We begin with a test in the spirit of falsification, following Gupta et al. (2023) and Grennan et al. (2024). Under the exclusion restriction, differential distance affects the outcomes of interest only through its effect on the likelihood of choosing an ASC. This assumption cannot be directly tested. However, an implication of the restriction is that in subsamples where the instrument does not influence ASC use, it should not predict changes in the outcomes of interest. Therefore, we implement this test by estimating IV and reduced form models on such a subsample.

We restrict the sample to patients for whom the differential distance between the nearest ASC and HOPD lies within a narrow window (-0.5 to 0.5 miles). These patients are nearly equidistant to both ASCs and HOPDs, and hence relative distance should have little predictive power for eventual ASC use. Table 8 presents the corresponding IV and reduced form results using this subsample. As expected, the first-stage F-statistic drops from 519 in the main sample to 1.3 in this subsample, confirming that differential distance is not predictive of ASC choice here. With such a weak instrument, the 2SLS coefficients are unreliable but we present them for the sake of completeness. In this exercise, the reduced form coefficients are of more interest. With the exception of the outcome related to office visits, they are small, statistically insignificant, and sometimes of the wrong sign. For example, the coefficient on ED visits is negative, implying that patients relatively distant from ASCs (in this subsample) are less likely to have an ED visit. Overall, these results reassure us that differential distance affects the outcomes primarily through its influence on ASC use.

Next, we consider three robustness tests, the results of which are presented in Table 9. In the interest of brevity, we present only the IV estimates from the robustness checks. First, we re-estimate our main models using a 30-day rather than 90-day postoperative window. This is the preferred duration for some quality measures used by CMS in hospital quality programs, such as mortality and readmission rates. The results are presented in Panel A and are qualitatively consistent with the baseline estimates, and are sometimes larger in percent terms. Consistent with baseline findings, ASC use increases the number of physician office visits (26% for any visit; 29% for related visits) and reduces ED visits while increasing urgent care use. However, while the baseline estimated decrease in likelihood of ED visits is 21%, the corresponding implied effect at 30 days is 24%. Further, the decrease in likelihood of ED visits at 30 days is much greater than the offsetting increase in likelihood of urgent care visits, suggesting that ASCs cause a net decrease in the use of ED care by this measure. The effect on total episode spending is also more pronounced in the 30-day window (approximately 21% versus 16% in the baseline), which aligns with our finding that some of the cost savings from ASC use are gradually offset by longer-run postoperative physician office visits.

Second, to address potential concerns about unobserved geographic heterogeneity, we replace HSA-by-year fixed effects with more granular county-by-year fixed effects. There are approximately 900 HSAs in the US compared to more than 3,100 counties, hence we expand the number of market fixed effects by more than a factor of 3. This

modification is motivated by prior research documenting substantial variation in health outcomes within HSAs (Rosenberg et al. 2016). For example, if income disparities within HSAs are significant, and ASCs locate in more affluent neighborhoods of the HSA, then including HSA fixed effects may not be sufficient to eliminate variation in unobserved health risk due to income disparities. For example, Montgomery County, PA (median household income: $107,000) and Philadelphia County, PA (median household income: $53,000) share the same HSA despite large economic and health differences. Panel B presents the corresponding coefficients obtained using the modified models. The first stage F-statistic drops in magnitude only slightly due to the inclusion of much more granular market fixed effects. The IV results remain robust to this alternative specification, suggesting that unobserved variation across counties within HSAs does not confound the relationship between ASC use and patient outcomes.

Finally, we test the sensitivity of our results to the exclusion of patients with exactly zero differential distance (DD = 0). This analysis removes those for whom proximity provides no clear incentive to favor one facility type over another. Since our 2SLS estimates identify the LATE for compliers—patients whose facility choice is influenced by differential distance—excluding patients with DD = 0 should not materially affect our results. Indeed, the first-stage F-statistic remains strong (464), and the second-stage estimates are virtually unchanged. This stability strengthens confidence in our identification strategy and supports the interpretation of our results as capturing the causal effect of ASC treatment on patients whose site of care is influenced by relative proximity.

6 Heterogeneity in Effects

This section presents evidence on heterogeneity in the causal effects of ASC use on the outcomes of interest. We have two main goals for this section. First, it is useful to assess whether some patient subgroups are disproportionately better or worse off receiving care at an ASC instead of in the hospital. For example, an important concern for critics of ASCs is whether a marginally frail patient requires more comprehensive hospital resources and may experience adverse effects if they are treated at an ASC instead. Using our IV research design, we can answer this question well by examining heterogeneity in effects on key outcomes for patients based on comorbidity and demographic factors (age, sex, and race).

Our second goal is to assess whether differences in states' regulation of ASCs may modulate their effectiveness in reducing healthcare spending among Medicare patients. As discussed in Section 1.3, states vary in their willingness to impose restrictions on the entry of new healthcare providers or the expansion of existing providers. By construction, fewer patients will be able to use ASCs in states where they are less prevalent and therefore more difficult to access. However, we would like to test whether the causal effects of ASCs *per patient* also vary with their penetration level. This could occur for several reasons, two of which we discuss briefly to illustrate potential channels. States that allow freer entry or growth of ASCs may also do the same for other healthcare provider types, such as corporate physician practices, urgent care centers, retail clinics, etc. Since ASCs partly reduce medical spending through greater follow-up preventive care that help avoid ED visits or substitute them with visits to urgent care centers,

greater availability of this "support ecosystem" in states with more liberal regulatory regimes may lead to relatively larger spending reductions there. Another possible explanation could be that ASCs in more lenient states enjoy greater acceptance from both referring physicians and patients, allowing them to serve larger case volumes and develop higher levels of operational and clinical productivity over time. These channels are not mutually exclusive and may interact, i.e., ASCs in states with more permissive environments may be both more productive and benefit from a more supportive provider ecosystem. Although we cannot precisely quantify the roles of these channels without additional sources of exogenous variation, we offer some suggestive evidence.

6.1 Patient health risk and demographics

We begin by examining the causal effects of ASC use for different patient subgroups based on comorbidity risk and demographics. In the interest of brevity, we focus on 6 key outcomes where we found changes due to ASC use in the main results, which were also presented in the robustness checks. Overall, we find the same qualitative pattern within each patient subgroup, where we compare ASC and HOPD patients of the same subgroup. Across the board, SC patients experience weakly more postoperative physician office visits (including those related to the focal procedure), a weakly lower probability of visiting the ED, some of which is offset by increased urgent care use, and incur both lower procedural and episode spending. Since there is no dramatic heterogeneity in treatment effects, we report these results in Table A.7 in the appendix. That being said, we briefly note two points below.

We first examine whether riskier patients, defined as patients with a Charlson Comorbidity Index (CCI) of greater than 0, have substantially different outcomes than patients with a comorbidity index of 0. Table A.7 Panel A presents the corresponding results. Riskier patients experience a similar percent increase in postoperative office visits but have substantially higher unscheduled care use than low-risk patients. They do not benefit from a decline in ED care but increase their use of urgent care. This divergence in unscheduled care also explains why they have a much smaller decline in episode spending compared to low-risk patients. These results suggest that it is difficult to reduce unscheduled care for more complex patients. In unreported results, we find no change in mortality, readmission, or complication rates among more complex patients. Hence, while the saving benefits of ASCs are smaller for complex patients, there is no evidence to suggest they experience more adverse events.

The main results suggest that episode savings exceed the price differential in the focal procedure because of the decrease in costly ED care, with potentially some substitution to inexpensive urgent care. Note that post-procedure spending decreases despite an increase in follow-up visits to physicians. The benefit of the rich heterogeneity analysis is that we can test for similar patterns across eight patient subgroups (two categories each of comorbidity prevalence, age, sex, and race). Indeed, we find that in 3 out of the 4 subsample analyses, the subgroup which experiences a greater decrease in ED use (net of increase in urgent care) also generates a larger saving in episode saving. The only exception is the analysis by gender. Women have a nominally larger decrease in ED care than men, but this advantage is completely offset by a sizable increase in urgent care use and a larger increase in physician office visits.

Differences by race are quite pronounced. White patients treated at ASCs experience substantially larger increases in office visits—29.2% for general and 23.3% for related visits—while estimates for non-White patients are smaller and imprecise. Both groups exhibit similar savings on episode payments; however, White patients experience lower reductions in total 90-day spending (16.0% vs. 29.2% for non-White patients), potentially due to increased follow-up care. Additionally, the substitution from ED to urgent care use also appears concentrated among White patients; the corresponding estimates for non-White patients are small and imprecise.

6.2 State CON requirement

This section examines heterogeneity in ASC effects by state-level Certificate of Need (CON) regulation. As described in Section 1.3, CON laws restrict facility expansion by requiring state-level approval for new ASC development. As of 2016, 26 states maintained CON requirements for ASCs.[6] The CON policy environment was very stable during our sample period, with only one state, New Hampshire, making a change to (repealing) its CON restrictions in 2016.[7] Due to the lack of changes in state policies, we cannot directly examine the effects of imposing or removing CON restrictions on the treatment effects from ASCs. Instead, we estimate our IV models separately in CON and non-CON states and compare the results to draw conclusions about the potential role of these restrictions. Strictly speaking, these estimates do not reflect the causal effect of CON restrictions and must be interpreted with caution.

In theory, imposing CON restrictions on the growth of ASCs (and other types of providers) may affect the aggregate savings from ASCs through two channels. First, CON restrictions are designed to reduce entry and capacity additions and will therefore reduce access to ASCs for patients in CON states relative to states without such restrictions. Second, the per-case savings from ASCs may also change due to CON restrictions, which we refer to as the "efficiency" effect. For example, if CON restrictions also reduce access to urgent care clinics, then patients will find it difficult to visit one after a procedure and be more likely to go to an ED instead. This will reduce post-procedure savings due to ASC care.

CON restrictions appear to meaningfully affect ASC presence. In our sample, the average market in CON states has 2.6 ASCs per 100,000 Medicare beneficiaries, while the corresponding mean for non-CON states is 16% higher at 3. In fact, the mean values understate the difference in accessibility between the two groups of states since there are some markets in CON states with an unusually high number of ASCs. Figure 6 plots histograms of ASC penetration for CON and non-CON states, respectively, and highlights this point. While the greatest density of markets in CON states is found below 1.5 ASCs, for non-CON states it lies around 3. Here, it must be noted that the difference in accessibility is partly offset by the fact that ASCs in CON states have greater scale than their counterparts in non-CON states. The average ASC in CON states serves 38.2 Medicare FFS cases per year, against 34.7 cases in non-CON states. Still, Medicare

6. States with CON laws during our sample period include: AL, AK, CT, DE, DC, GA, HI, IL, IA, KY, ME, MD, MA, MI, MS, MT, NV, NY, NC, RI, SC, TN, VT, VA, WV, and WA.

7. NH passed a law in 2016 repealing all CON restrictions. We include it in the non-CON group for simplicity.

beneficiaries get more procedures at ASCs in non-CON states on average (0.61 vs. 0.57 procedures per FFS beneficiary).

CON restrictions could also affect the growth in HOPD capacity. Therefore, the differences in ASC penetration may not accurately reflect the difference in access. It may be instructive to compare the fraction of procedures performed at ASCs in states with CON restrictions to those without. These figures also tell a similar story. ASCs contribute 58% of total case volume in CON states, against 66% in non-CON states. Hence, CON restrictions are associated with lower use of ASCs.

Next, we investigate whether the average savings generated by an ASC differs between CON and non-CON states. Table 10 presents results for states with and without CON restrictions, respectively, using our IV research design. The coefficients have the same interpretation as in the main models and they strongly indicate that ASCs generate higher savings in non-CON states. ASC treatment is associated with larger increases in general office visit use (36% vs. 21% in CON states) and larger reductions in both the procedure payment (36% vs. 28%) and total 90-day spending (18% vs. 13%). We detect substantial reductions in ED visits (44%) for ASC patients in CON states, whereas we fail to detect a change in ED visits in CON states and the point estimate is positive, suggesting, if anything, an increase. Since the level of episode spending is higher in non-CON states, the dollar savings are even larger than the proportional estimates imply. These estimates imply savings of $697 in non-CON states versus $481 in CON states.

We use these estimated effects in back of the envelope calculations to help illustrate the relative magnitudes of the access and efficiency effects of CON restrictions on savings from ASCs. The observed mean spending values for HOPD and ASC patients and their respective volume shares imply that the average episode costs Medicare $3,466 in non-CON states, against $3,576 in CON states, or $110 more. If we eliminate the difference in ASC share of cases – the access effect – the predicted episode spending in CON states would decline to $3,538, a small change. Instead, if we apply the higher per-unit savings estimated for non-CON states (the efficiency channel), we predict an average spending of $3,453, a substantial drop of around $120. If we eliminate both access and efficiency channels, then the predicted spending drops further to below $3,400. Eliminating both channels therefore predicts a saving of 5% in episode spending [(3576-3397)/3576]. Nearly 70% of this difference is accounted for by the efficiency channel alone. Hence, the effect of CON restrictions on per-case savings is quantitatively more important than their effect on access to ASCs.

Overall, these findings strongly suggest that ASCs are more effective at reducing medical spending and ED utilization in less regulated, more competitive markets. This evidence has important implications for policymakers considering reforms to regulatory structures such as CON restrictions, which may limit the cost-saving potential of ASCs.

7 Conclusion

Ambulatory Surgical Centers (ASCs) represent an innovation in the delivery of outpatient surgical care, with the potential to reduce spending while maintaining quality. However, estimating the causal effects of ASC use on spending and patient health is challenging due to the potential for unobserved patient selection. This has raised doubts

about their effects and limited support for their growth. This paper estimates causal effects using national claims data on fee-for-service Medicare patients.

We first confirm that ASCs primarily affect spending and patient health by substituting care away from hospital outpatient departments without increasing utilization on the extensive margin. We then show that ASC use reduces spending not only on the focal procedure itself, but also on total spending including all care consumed over the next 90 days. In fact, our results imply that ASC use also reduces spending in the next 90 days, driven by substitution away from costly ED visits toward lower cost visits to urgent care clinics. As a result, the savings on total episode costs is greater than on the focal procedure alone. This occurs even though ASC patients are more likely to have follow-up office visits with physicians over the next 90 days. Finally, we estimate economically and statistically insignificant changes in adverse outcomes, such as hospitalization, complications, and death. Together, we interpret these results as implying that ASCs reduce Medicare spending without adversely affecting quality of care. The results remain robust to a battery of specification checks.

We also document meaningful heterogeneity in the effects of ASC use on a multitude of dimensions. Of these, the most significant finding is that total episode savings due to ASCs are 40% larger (18% versus 13%) in states that do not require a Certificate of Need (CON) for new ASCs to enter the market or increase capacity. The differential savings arise due to both lower procedure payments and larger reductions in postoperative ED use. This result suggests that CON restrictions not only limit access to ASC care, but also potentially reduce their savings potential. As a result, aggregate savings for Medicare due to ASCs are much lower in CON states. Given that a central rationale for CON laws is to contain healthcare spending, this result raises important questions about whether such regulations may, in practice, undermine that objective.

While we have provided novel estimates on the causal effects of ASC use, several promising paths remain for future work. For instance, although we show that ASC patients have fewer ED visits in the follow-up period, we have not pinned down the precise mechanisms that help avoid ED use. Another key question that remains is whether ASCs improve physician performance through learning-by-doing effects that come with high-volume and focused practices. A better understanding of these potential mechanisms will further help understand how ASCs improve productivity of the healthcare system.

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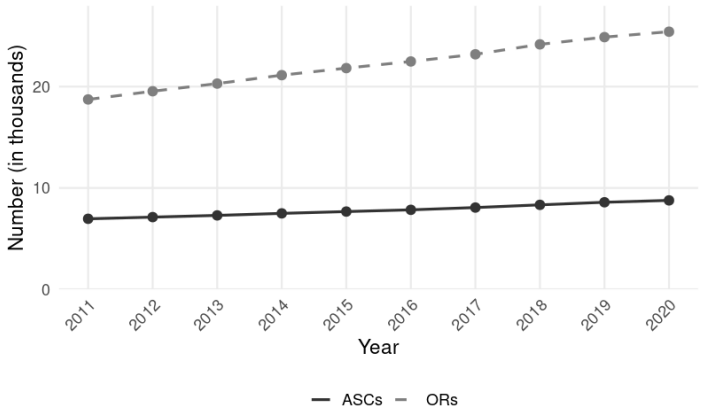
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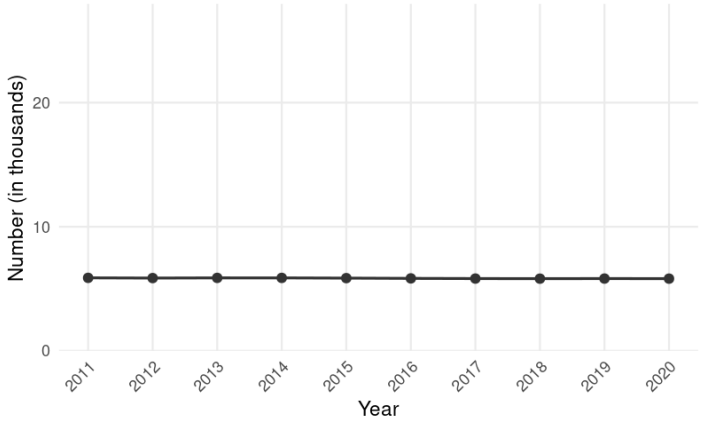
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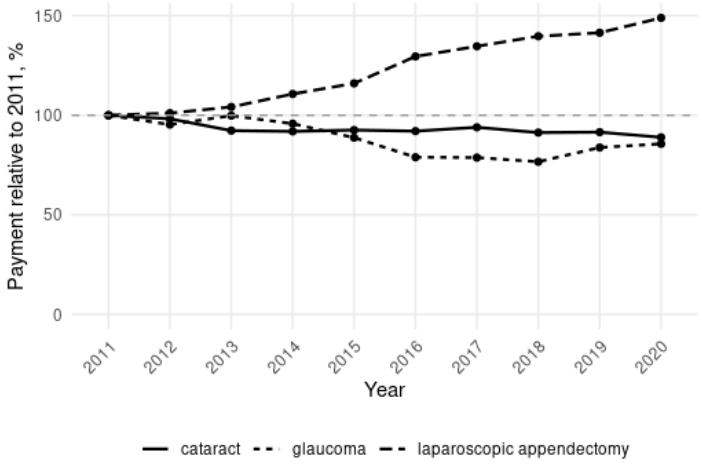
Figure 1: ASC Growth and Cost Advantage



(a) Number of ASCs and ASC Operating Rooms (ORs) over time



(b) Number of HOPDs over time



(c) Mean payment change over time

Notes: Panel (a) presents the annual number of ASCs and ASC operating rooms (ORs) in the United States from 2011 to 2020. Panel (b) shows the number of HOPDs during the same period. Panel (c) compares average Medicare payments for cataract and glaucoma procedures—eligible for ASC delivery—with laparoscopic appendectomies, which were limited to the HOPD setting. Procedure costs are defined as described in Section 2.3. Data for Panels (a) and (b) are sourced from the Centers for Medicare and Medicaid Services (CMS) Provider of Service files.

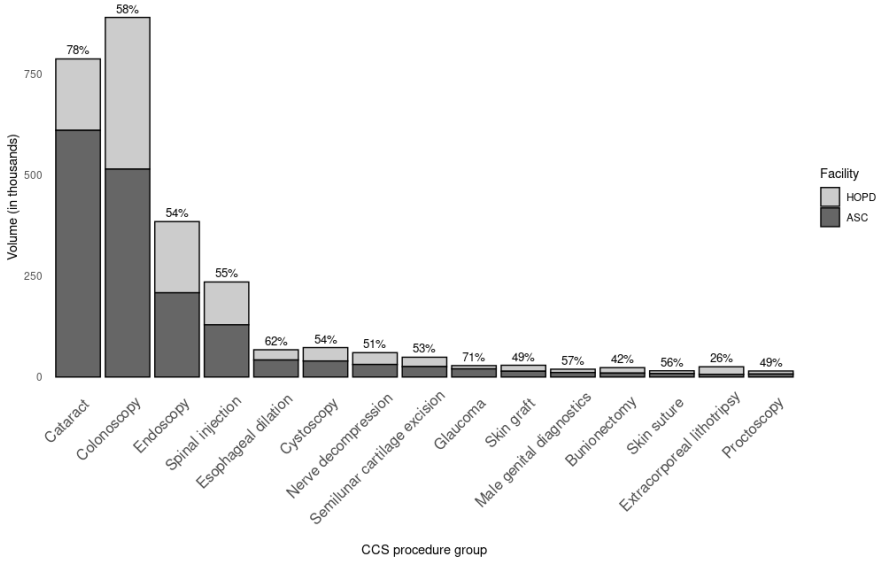
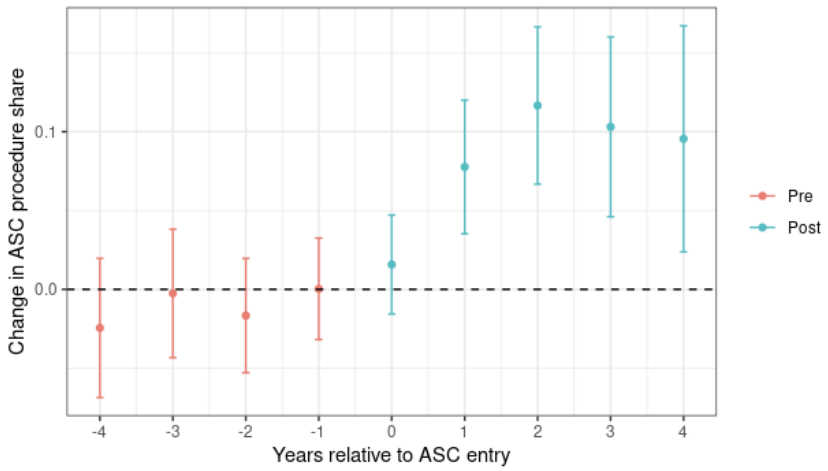
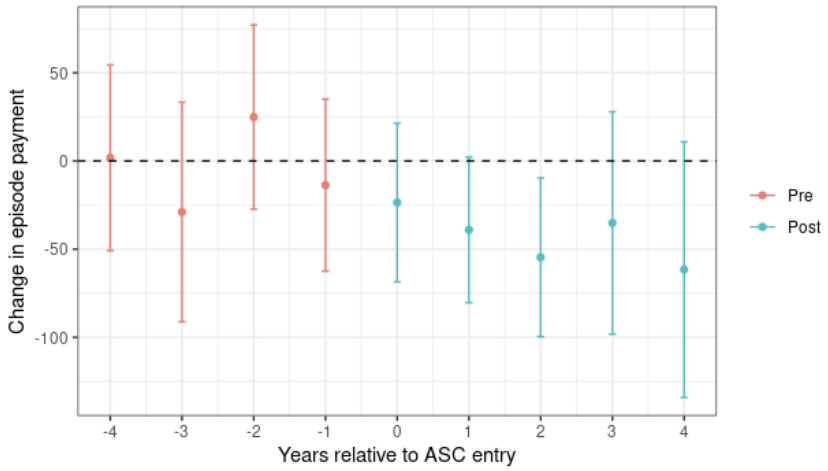


Figure 2: Volume of each CCS procedure group, by facility type

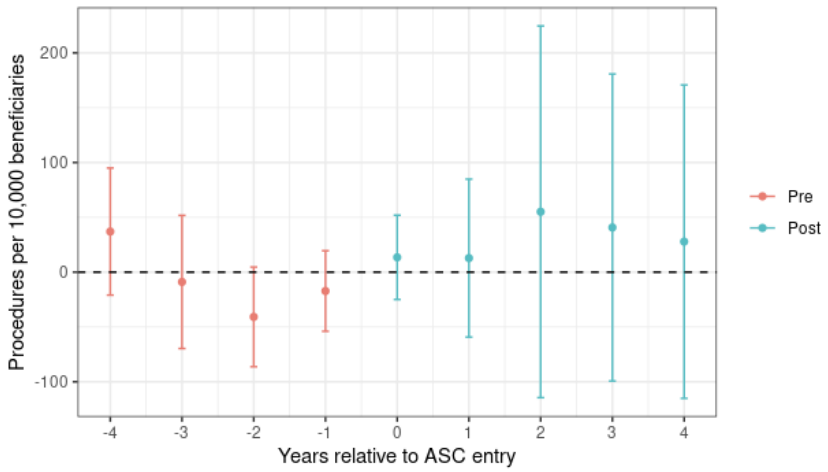
Notes: This figure details the ASC and HOPD volume distribution for the top 15 CCS procedure groupings in our data sample, comprising 81% of the total FFS ASC procedure volume. Procedures are arranged on the X-axis from left to right according to ASC volume, with volume displayed on the Y-axis in thousands. The combined height of the stacked bars indicates the total procedure volume across both settings. The percentages shown above each bar represent the share of procedures performed at the ASC setting (i.e., the share of the dark grey portion).



(a) ASC share of procedures



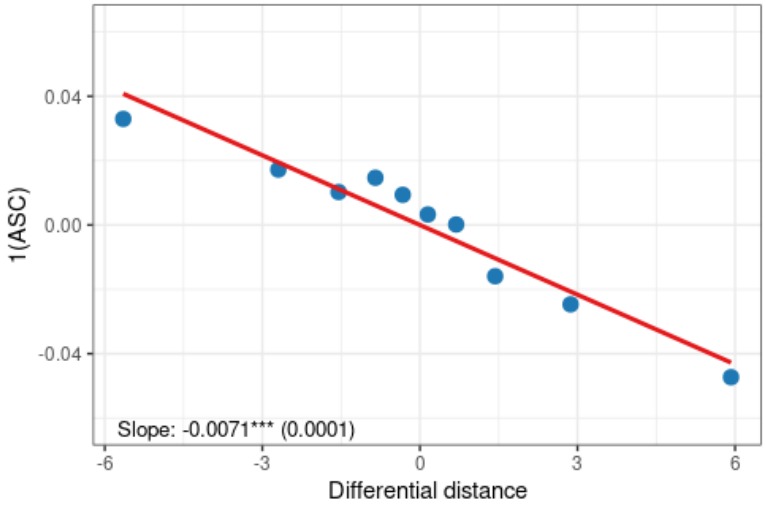
(b) Mean spending per procedure



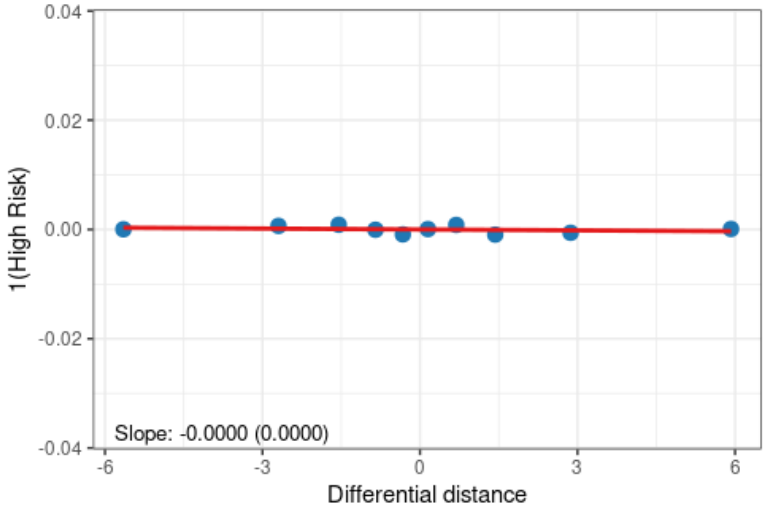
(c) Total procedures per capita

Figure 3: The effects of ASC entry

Notes: This figure presents event study plots associated with the effects of ASC entry in a county. Section 3 describes the research design. The figures plot the effect of ASC entry on (a) the share of total procedures performed at an ASC; (b) mean spending per procedure; and (c) the number of procedures performed per 10,000 FFS beneficiaries residing in the county. The figures also display 95% confidence intervals. Standard errors are clustered by county.



(a) Surgery at an ASC

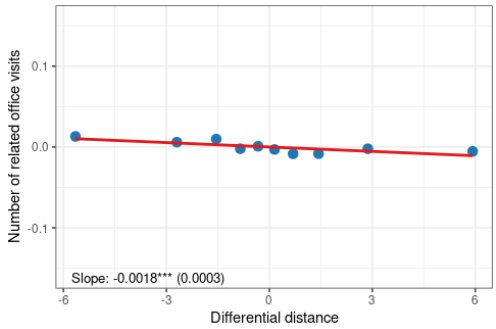


(b) High-risk patients

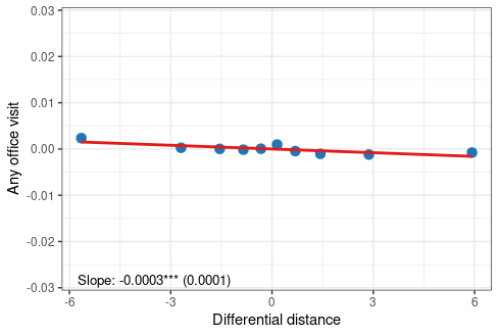
Figure 4: Binned residual plots, first stage

Notes: This figure presents binned scatter plots of the first stage results. Panel (a) is constructed by separately regressing both the indicator for surgery at an ASC and patient differential distance on controls (patient-level and patient ZIP-level controls) and fixed effects (HSA x Year and CPT code fixed effects) from our preferred specification. Residuals from these two regressions are binned into 10 bins and plotted. Means of the residuals from the regression with differential distance as the dependent variable are plotted on the X-axis, and means of residuals from the regression with the ASC indicator as the dependent variable are plotted on the Y-axis. Panel (b) takes the same approach, except the dependent variable of the Y-axis regression is an indicator for a high-risk patient (i.e., the patient has a Charlson comorbidity score greater than or equal to 2). Both panels present linear fitted lines for these plots, as well as the slope coefficient (per mile of differential distance) with the corresponding standard error. Y-axes are standardized using 0.15 times the standard deviation of the residuals. Standard errors are clustered by procedure CPT code.

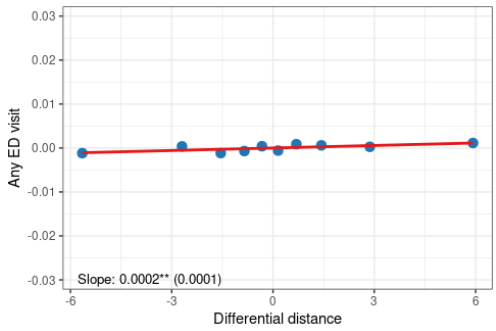
Figure 5: Reduced form binned scatter plots



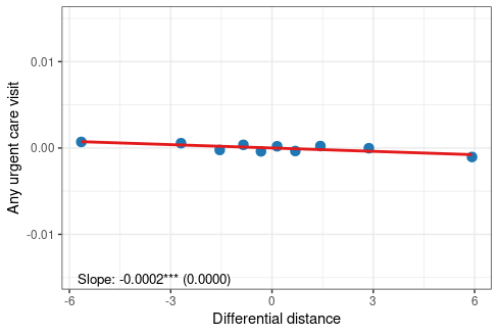
(a) Number of 90-day office visits



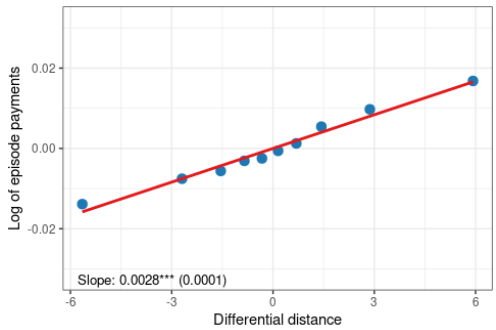
(b) 1(90-day Office Visits)



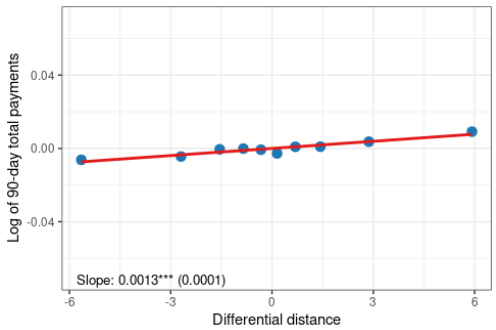
(c) 1(90-day ED visits)



(d) 1(90-day urgent care visits)



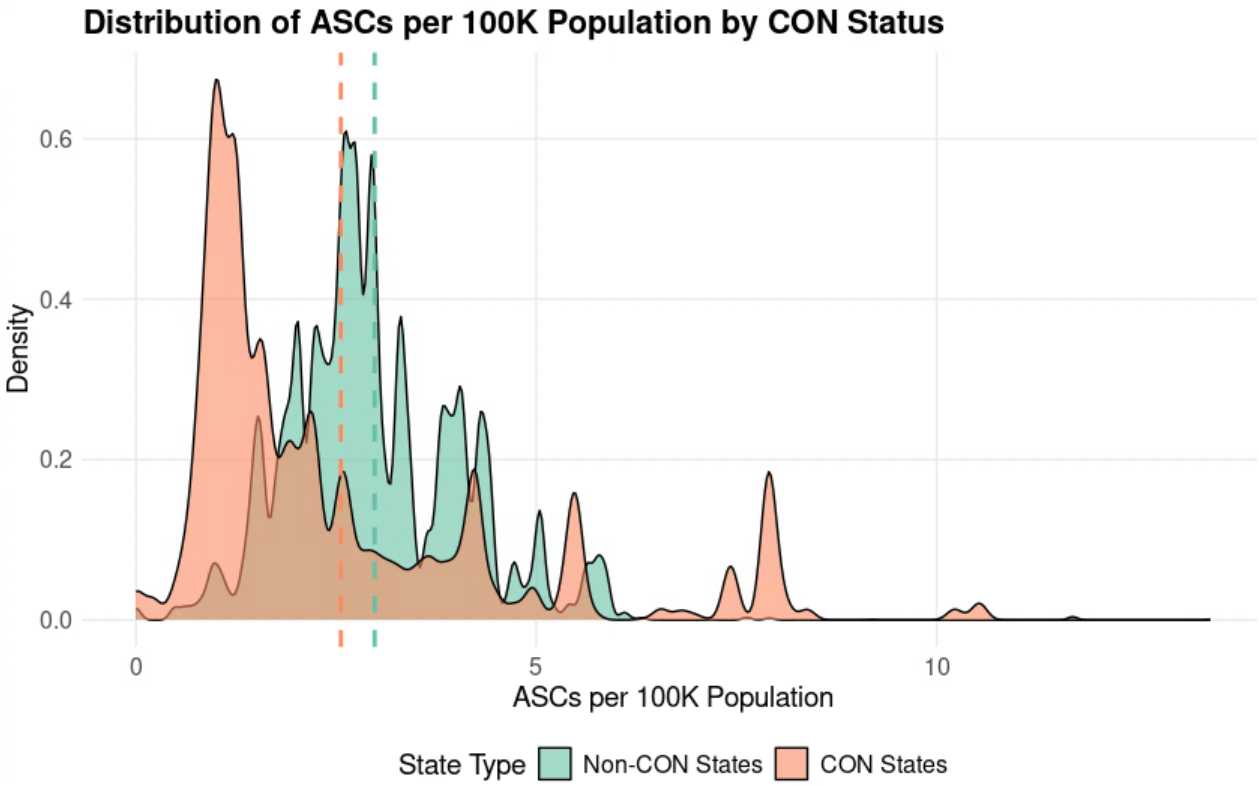
(e) Log(procedure payment)



(f) Log(Episode payment)

Notes: Y-axes are standardized using 0.10 times the standard deviation of the residuals. Standard errors are clustered by procedure CPT code.

Figure 6: ASC penetration and CON restrictions



Notes: The figure plots histograms of ASC facility penetration in markets by CON and non-CON states. The vertical dashed lines indicate the means for the two groups.

Table 1: Patient and Procedure Characteristics

	HOPD (N = 968,479)	ASC (N = 1,638,010)	All Patients (N = 2,606,489)
A: Top 5 procedures			
Colonoscopy	0.38 (0.48)	0.31 (0.46)	0.33 (0.47)
Cataract procedures	0.18 (0.38)	0.37 (0.48)	0.29 (0.46)
Endoscopy	0.17 (0.38)	0.12 (0.33)	0.14 (0.35)
Spinal stimulator	0.11 (0.31)	0.08 (0.27)	0.09 (0.28)
Cystoscopy	0.03 (0.17)	0.02 (0.15)	0.03 (0.16)
B: Demographics			
Age, mean (SD)	74.6 (6.54)	74.4 (6.30)	74.5 (6.39)
Female	0.57 (0.50)	0.58 (0.49)	0.57 (0.50)
Race, Black	0.04 (0.20)	0.04 (0.19)	0.04 (0.19)
Dual eligible	0.09 (0.28)	0.06 (0.25)	0.07 (0.26)
C: Patient zipcode characteristics			
Median income, (thousands)	79.9 (23.8)	79.3 (23.9)	79.5 (23.8)
Area Deprivation Index	45.9 (21.7)	44.9 (21.3)	45.3 (21.4)
D: Health risk			
Charlson comorbidity score	0.97 (1.52)	0.70 (1.24)	0.80 (1.36)
High risk (Charlson score ≥ 2)	0.27 (0.44)	0.20 (0.40)	0.23 (0.42)
Office visit in past 90 days	0.96 (0.19)	0.97 (0.17)	0.97 (0.18)
Office visit in past 90 to 180 days	0.77 (0.42)	0.76 (0.43)	0.76 (0.43)
ED visit in past 90 days	0.10 (0.30)	0.07 (0.26)	0.08 (0.28)
Urgent care in past 90 days	0.02 (0.15)	0.03 (0.16)	0.03 (0.16)
Healthcare spending in past 90 days	1270 (1560)	1120 (1430)	1180 (1480)
E: Outcomes			
90-day office visit	0.91 (0.29)	0.91 (0.28)	0.91 (0.28)
90-day ED visit	0.12 (0.33)	0.09 (0.28)	0.10 (0.30)
90-day urgent care visit	0.02 (0.15)	0.02 (0.15)	0.02 (0.15)
Procedure payment ($)	1500 (789)	1140 (577)	1270 (687)
90-day episode payment ($)	3890 (4610)	3220 (3960)	3470 (4220)

Notes: This table provides patient-level descriptive statistics for key variables used in the analysis. The values are computed for the analysis sample, described in Section 2.2. Columns 1 and 2 present mean (standard deviation) values for each characteristic for HOPD and ASC patients, respectively. Column 3 presents the mean (standard deviation) for the overall sample. The Charlson comorbidity score is calculated using comorbidities recorded in hospital inpatient and outpatient claims within 90 days prior to the focal procedure. We generated the Charlson comorbidity score using the `comorbidity` command from the *comorbidity* package in R (https://cran.r-project.org/web/packages/comorbidity/comorbidity.pdf). "Procedure payment" represents the sum of payments made within one day of the focal procedure date, allowing for the capture of ancillary related claims around the time of the focal procedure. "90-day episode payment" encompasses hospital inpatient and outpatient spending over the 90 days following the focal procedure, including the focal procedure payments. All payment amounts are winsorized at the 99% level and expressed in 2021 dollars.

Table 2: First Stage Results

	(1) 1(ASC)	(2) 1(ASC)	(3) 1(ASC)
Differential distance (miles)	-0.0073^{***}	-0.0071^{***}	-0.0071^{***}
	(0.0003)	(0.0003)	(0.0003)
HSA x year FEs	X	X	X
CPT FEs	X	X	X
Patient state FEs			X
Patient zipcode controls	X	X	X
Patient controls		X	X
SE clustered by beneficiary zip code	X	X	X
Observations	2,606,489	2,606,489	2,606,489
Y-mean	0.63	0.63	0.63
f-stat	518	502	502

Notes: This table presents estimates of the relationship between undergoing a procedure at an ASC and the patient's differential distance. The cells present the coefficient γ obtained by estimating Equation 4. The independent variable is the difference between the distance in miles to the nearest ASC and the nearest HOPD for the patient. The outcome variable is an indicator for whether the patient had their surgery at an ASC (=1 if at an ASC, 0 if at an HOPD). Column 1 controls for patient market (Health Service Area) x Year fixed effects, Current Procedural Terminology (CPT) code fixed effects, and patient zipcode controls. Column 2 (our preferred specification) additionally includes controls for patient characteristics and risk (age, race, sex, an indicator for dual eligible status, Charlson comorbidity score, and prior ASC, HOPD, ED or urgent care use). Column 3 further tests first stage robustness by including state fixed effects. Standard errors are clustered by patient zipcode. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table 3: Test of Instrument Balance

	(1) ASC	(2) Differential distance	(3) Sample mean
Age 70–79	-0.009***	-0.002	0.520
	(0.001)	(0.006)	
Age 80+	-0.038***	0.000	0.218
	(0.001)	(0.011)	
Female	0.003***	0.002	0.572
	(0.001)	(0.005)	
Race, Black	-0.024***	-0.007	0.039
	(0.002)	(0.031)	
Dual Eligible	-0.063***	0.027	0.073
	(0.002)	(0.017)	
High risk (Charlson ≥ 2)	-0.056***	0.010	0.226
	(0.001)	(0.006)	
90d pre-procedure spending, $, thousands	-0.006***	-0.000	1.18
	(0.000)	(0.002)	
Office visit 90 days before procedure	0.034***	-0.017	0.968
	(0.002)	(0.010)	
Office visit 90 to 180 days pre-procedure	-0.007***	-0.008	0.763
	(0.001)	(0.005)	
ED or urgent care 90 days pre-procedure	-0.011***	0.009	0.104
	(0.001)	(0.008)	
Observations	2,606,489	2,606,489	2,606,489
Joint F-Test p-value	<0.001	0.272	

Notes: Column (1) presents estimates from an OLS regression of ASC utilization on the listed covariates, indicators for prior ASC or HOPD visit, zipcode-level controls, HSA-by-year fixed effects, and CPT code fixed effects, with standard errors clustered at the patient zipcode level. Column (2) reports results from the same OLS model, but with differential distance as the dependent variable. The p-values reported at the bottom of each column correspond to an F-test of the joint significance of the listed covariates. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table 4: Characteristics of Always-Takers, Never-Takers, and Compliers

	(1) Full sample	(2) Always-Takers	(3) Never-Takers	(4) Compliers	(5) Compliers/Full Sample Ratio
Demographics					
Age, mean (SD)	74.5	74.3	74.6	74.6	1.00
Female	0.57	0.56	0.57	0.59	1.03
Race, White	0.91	0.93	0.92	0.91	1.00
Race, Black	0.04	0.03	0.04	0.04	1.03
Dual eligible	0.07	0.06	0.07	0.06	0.86
Risk measures					
Charlson comorbidity score	0.80	0.71	0.99	1.01	1.27
High risk indicator (Charlson score $\geq$2)	0.23	0.21	0.27	0.27	1.18
ED visit in past 90 days	0.08	0.08	0.10	0.09	1.09
Urgent care visit in past 90 days	0.03	0.02	0.02	0.03	1.09
Total spending in past 90 days (thousands)	1.18	1.09	1.25	1.27	1.08
Percentage of Sample (%)	100.0	42.6	19.1	38.3	

Notes: This table describes the observable characteristics of the always-taker, never-taker, and complier samples, relative to the full sample. Column (5) shows the ratio of Column (4) divided by Column (1).

Table 5: Postoperative follow-up care

	(1) Office visits, #	(2) Office visit, any	(3) Office visits, # (of any)	(4) Related office visits, #	(5) Related office visit, any	(6) Related visits, # (of any)
A. OLS						
1(ASC)	0.1360***	0.0067***	0.1130***	0.0143***	0.0118***	-0.0359***
	(0.0067)	(0.0005)	(0.0067)	(0.0034)	(0.0009)	(0.0053)
B. IV						
1(ASC)	1.223***	0.0377***	1.134***	0.2579***	0.0318	0.2474**
	(0.1647)	(0.0097)	(0.1618)	(0.0746)	(0.0220)	(0.0949)
Observations	2,606,489	2,606,489	2,378,587	2,606,489	2,606,489	1,180,502
Y-mean	4.43	0.909	4.87	1.11	0.431	2.56

Notes: This table presents estimates of the relationship between undergoing a procedure at an ambulatory surgery center (ASC) and postoperative follow-up care. Panel A presents OLS estimates, with each cell reporting the coefficient β from estimating Equation 3. The independent variable is an indicator for the patient having a procedure at an ASC. Office visits are identified according to the codes listed in Appendix Tables A.1 and A.2. Spending outcomes are reported in 2021 dollars. Panel B provides the coefficient β estimate for the same outcomes, obtained through 2SLS estimation of Equation 5, using differential distance to the nearest ASC versus the nearest HOPD as the instrument for undergoing a procedure at an ASC. "Y-mean" refers to the mean of untreated compliers. All regressions control for procedure CPT code and patient HSA x Year fixed effects, patient characteristics, and patient zipcode characteristics. Standard errors are clustered by zipcode. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table 6: Quality Outcomes

90-day Quality Outcomes

	(1) Mortality	(2) Inpatient admission	(3) Complication	(4) Unscheduled care	(5) ED Visit	(6) Urgent Care Visit
A. OLS						
1(ASC)	-0.0001**	-0.0060***	-0.0055***	-0.0215***	-0.0227***	0.0007***
	(0.0000)	(0.0003)	(0.0002)	(0.0005)	(0.0005)	(0.0002)
B. IV						
1(ASC)	0.0002	-0.0002	-0.0018	-0.0048	-0.0254**	0.0214***
	(0.0006)	(0.0060)	(0.0039)	(0.0100)	(0.0090)	(0.0062)
Observations	2,606,489	2,606,489	2,606,489	2,606,489	2,606,489	2,606,489
Y-mean	0.001	0.054	0.022	0.139	0.121	0.023

Notes: This table presents estimates of the relationship between undergoing a procedure at an ambulatory surgery center (ASC) and health outcomes. Panel A presents OLS estimates, with each cell reporting the coefficient β from estimating Equation 3. The independent variable is an indicator for the patient having a procedure at an ASC. Panel B provides the coefficient β estimate for the same outcomes, obtained through 2SLS estimation of Equation 5, using differential distance to the nearest ASC versus the nearest HOPD as the instrument for undergoing a procedure at an ASC. "Y-mean" refers to the mean of untreated compliers. All regressions control for procedure CPT code and patient HSA x Year fixed effects, patient characteristics, and patient zipcode characteristics. Standard errors are clustered by zipcode. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table 7: Spending Outcomes

	(1) log(procedure payment)	(2) Any 90-day payment	(3) log(Non-zero 90-day payment)	(4) log(total episode payment)
A. OLS				
1(ASC)	-0.3539***	0.0003	-0.0537***	-0.2315***
	(0.0011)	(0.0004)	(0.0024)	(0.0014)
B. IV				
1(ASC)	-0.3898***	0.0280***	0.0636	-0.1763***
	(0.0201)	(0.0077)	(0.0482)	(0.0269)
Observations	2,606,489	2,606,489	2,449,368	2,606,489
Y-mean ($, prob.)	1,504	0.941	2,538	3,892

Notes: This table presents estimates of the relationship between undergoing a procedure at an ambulatory surgery center (ASC) and spending outcomes. Panel A presents OLS estimates, with each cell reporting the coefficient β from estimating Equation 3. The independent variable is an indicator for the patient having a procedure at an ASC. Spending outcomes are reported in 2021 dollars. Panel B provides the coefficient β estimate for the same outcomes, obtained through 2SLS estimation of Equation 5, using differential distance to the nearest ASC versus the nearest HOPD as the instrument for undergoing a procedure at an ASC. "Y-mean" refers to the mean of untreated compliers. All regressions control for procedure CPT code and patient HSA x Year fixed effects, patient characteristics, and patient zipcode characteristics. Standard errors are clustered by zipcode. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table 8: Falsification

	(1) Office visits, #	(2) Related office visits, #	(3) log(proc. payment)	(4) log(90-day payments)	(5) ED Visit	(6) Urgent Care Visit
Panel A: 2SLS Results						
1(ASC)	10.59	1.65	-0.3722	0.6170	0.3412	0.0289
	(9.68)	(2.06)	(0.3358)	(0.8536)	(0.3661)	(0.1160)
Panel B: Reduced Form Estimates						
Differential	-0.1735**	-0.0270	0.0061	-0.0101	-0.0056	-0.0005
distance	(0.0582)	(0.0253)	(0.0070)	(0.0088)	(0.0032)	(0.0019)
Observations	1,173,169	1,173,169	1,173,169	1,173,169	1,173,169	1,173,169
Y-mean	4.50	1.10	1473	3859	0.125	0.023
F-stat	1.28	1.28	1.28	1.28	1.28	1.28

Notes: This table presents results from a test in the spirit of falsification. We narrow the sample to include only patients with differential distance between -0.5 and 0.5 miles, inclusive. All regressions include procedure CPT code fixed effects, patient HSA × Year fixed effects, and standard errors clustered by zipcode.

Table 9: Robustness Checks

	(1) Office visits, #	(2) Related office visits, #	(3) ED Visit	(4) Urgent Care Visit	(5) log(proc. payment)	(6) log(episode payment)
A. 30-day outcomes						
1(ASC)	0.4254***	0.1487**	-0.0140*	0.0074*	-0.3898***	-0.2388***
	(0.0733)	(0.0526)	(0.0058)	(0.0030)	(0.0201)	(0.0263)
Observations	2,606,489	2,606,489	2,606,489	2,606,489	2,606,489	2,606,489
Y-mean	1.61	0.521	0.058	0.008	1504	2332
F-stat	502	502	502	502	502	502
B. County x Year fixed effects						
1(ASC)	1.058***	0.2357***	-0.0183*	0.0234***	-0.3949***	-0.1662***
	(0.1475)	(0.0715)	(0.0084)	(0.0056)	(0.0185)	(0.0254)
Observations	2,606,489	2,606,489	2,606,489	2,606,489	2,606,489	2,606,489
Y-mean	4.43	1.11	0.121	0.023	1504	3892
F-stat	491	491	491	491	491	491
C. Excluding DD = 0						
1(ASC)	1.184***	0.2810***	-0.0228*	0.0216***	-0.3924***	-0.1824***
	(0.1751)	(0.0795)	(0.0095)	(0.0066)	(0.0211)	(0.0288)
Observations	1,567,066	1,567,066	1,567,066	1,567,066	1,567,066	1,567,066
Y-mean	4.41	1.12	0.119	0.022	1530	3916
F-stat	464	464	464	464	464	464

Notes: This table presents results from robustness checks on the relationship between surgery performed at an ASC and select patient health and spending outcomes. Panel A presents results using a 30-day post-procedure window (rather than 90 days). Panel B replaces the HSA × Year fixed effects with county × Year fixed effects. Panel C excludes patients in zipcodes equidistant from both types of facilities (DD = 0). All regressions include procedure CPT code fixed effects, and except for those in Panel B, patient HSA × Year fixed effects. Standard errors are clustered by zip code. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table 10: Heterogeneity by Certificate-of-Need status

	(1) Office visits, #	(2) Related office visits, #	(3) log(proc. payment)	(4) log(Episode payment)	(5) ED Visit	(6) Urgent Care Visit
A. CoN required	0.9366***	0.1831*	-0.3220***	-0.1332***	0.0029	0.0225**
	(0.2015)	(0.0858)	(0.0238)	(0.0335)	(0.0114)	(0.0074)
Observations	1,110,286	1,110,286	1,110,286	1,110,286	1,110,286	1,110,286
Y-mean	4.43	1.03	1,520	3,855	0.116	0.021
B. CoN not required	1.587***	0.3307**	-0.4492***	-0.1955***	-0.0548***	0.0186
	(0.2603)	(0.1230)	(0.0325)	(0.0418)	(0.0140)	(0.0097)
Observations	1,496,302	1,496,302	1,496,302	1,496,302	1,496,302	1,496,302
Y-mean	4.43	1.17	1,489	3,926	0.126	0.024

Notes: This table presents results from a heterogeneity test of the relationship between ASC use and selected patient health and spending outcomes. We stratify the sample by the presence of state-level Certificate of Need (CON) regulations for ASCs. States with CON laws include: AL, AK, CT, DE, DC, GA, HI, IL, IA, KY, ME, MD, MA, MI, MS, MT, NV, NY, NC, RI, SC, TN, VT, VA, WV, and WA. All regressions include CPT code fixed effects and HSA-by-year fixed effects. Standard errors are clustered by zipcode. HSA = Health Service Area. CON = Certificate of Need. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

A Appendix

A.1 Data Sources

This study integrates multiple publicly available data sources to construct variables for analysis. Specifically, we utilize: (1) Dartmouth Atlas data for geographic classifications, (2) the American Community Survey (ACS) for ZIP code-level demographic characteristics, (3) the Area Deprivation Index (ADI) for measures of neighborhood socioeconomic disadvantage, and (4) the Healthcare Cost and Utilization Project (HCUP) Clinical Classifications Software (CCS) for procedure code categorizations. This appendix provides an overview of each data source, outlining its scope and application within our study.

Dartmouth Atlas. Dartmouth Atlas data serve as our primary source for the delineation of Hospital Service Areas (HSAs). Developed by the Dartmouth Institute for Health Policy & Clinical Practice, these data define standardized geographic regions based on patient flow patterns for hospital care (Dartmouth Atlas 2024). We use Dartmouth Atlas data to link beneficiaries' counties of residence to their corresponding HSAs, allowing us to analyze healthcare utilization within defined service areas and account for geographic variation in healthcare markets.

American Community Survey. To incorporate demographic information at the ZIP-code level, we use data from the American Community Survey (ACS), conducted by the U.S. Census Bureau (United States Census Bureau 2022). Specifically, we use the 2016–2020 ACS 5-Year Estimates. From this dataset, we extract median household income for each beneficiary's residential ZIP code, using it as a proxy for local economic conditions.

Area Deprivation Index. We further refine our socioeconomic measures using the Area Deprivation Index (ADI), a composite metric of neighborhood disadvantage. Originally developed by the Health Resources & Services Administration (HRSA) and later refined by researchers at the University of Wisconsin-Madison, the ADI incorporates multiple socioeconomic indicators to assess relative deprivation (Kind and Buckingham 2018). While the ADI is constructed at the nine-digit ZIP code level, we aggregate these values to the five-digit ZIP code level by computing simple averages.

HCUP Clinical Classifications Software. To categorize healthcare procedures, we use the Healthcare Cost and Utilization Project (HCUP) Clinical Classifications Software (CCS), developed by the Agency for Healthcare Research and Quality (AHRQ). The CCS provides a standardized framework for grouping thousands of distinct Current Procedural Terminology (CPT) codes into clinically meaningful categories. To improve sample homogeneity within each CCS category, we restrict our analysis to the three most frequently observed CPT codes per category, capturing over 90% of all visits within each CCS-defined group.

A.2 Sample Construction Details

To construct the analytic sample, we begin by identifying all outpatient surgical encounters between 2011 and 2020 involving Medicare fee-for-service (FFS) beneficiaries. We focus on procedures performed in either hospital outpatient departments (HOPDs) or ambulatory surgery centers (ASCs).

To reduce procedural heterogeneity and ensure overlap in treatment settings, we restrict the analysis to the 15 most common procedures—defined using Clinical Classifications Software (CCS) codes—observed in FFS ASC claims over the study period. At least 5% of total volume for each selected procedure is performed in both ASCs and HOPDs. Together, these categories comprise 81% of all ASC procedures in the initial claims sample. Within each CCS category, we further limit the sample to the top three most frequently billed CPT codes, which account for over 90% of procedure volume in each group. For encounters involving multiple procedures, we assign the primary procedure based on the claim with the highest total charges. We exclude procedures coded under residual or "Other" CCS categories, which are difficult to interpret and span heterogeneous clinical indications.

After identifying eligible procedures, we apply several exclusions. We remove beneficiaries and providers located outside the 50 states and Washington, D.C. to maintain consistency in regulatory and geographic context. Claims with zero payments made are excluded, as these may reflect billing errors, denials, or services not covered by Medicare. Beneficiaries with any Medicare Advantage (MA) enrollment during the measurement window are dropped, given the lack of complete claims data for MA enrollees. We also exclude patients under age 65 and those not continuously enrolled in both Medicare Part A and B for at least 12 months prior to and three months following the index procedure, to ensure full visibility into healthcare utilization and spending. Finally, we exclude observations with missing Health Service Area (HSA) information, which is required for defining geographic markets.

Each observation in the dataset represents a unique patient-procedure pair. Patients may appear more than once if they undergo multiple qualifying procedures, but a minimum spacing of one calendar year is required between procedures to avoid overlapping follow-up periods. Additionally, we exclude patients with inpatient admissions in the 90 days preceding their outpatient surgery to isolate cases unrelated to recent hospital discharges.

To construct the key geographic instrument, we calculate distances using spherical (great-circle) distance between the centroids of the beneficiary's zipcode and provider zipcode. Distances are computed separately for each CCS procedure group to reflect the specialization of ASCs. For example, a patient undergoing cataract surgery is assigned the distance to the nearest ASC and HOPD performing cataract procedures, based on observed procedure volume during the study period. We then compute differential distance (DD) as the distance to the nearest ASC minus the distance to the nearest HOPD, where negative values indicate closer proximity to an ASC.

We impose two additional distance-based restrictions. First, we exclude patients who travel more than 50 miles to receive care, a threshold that exceeds the 90th percentile of the sample and likely reflects unobservable preferences or access barriers. Second, we restrict the sample to patients residing in zipcodes with DD values between

-10 and +10 miles. This range helps identify marginal patients for whom proximity plausibly influences provider choice, and avoids extrapolating to areas dominated by a single provider type.

Finally, to improve covariate balance across zipcodes with differing provider proximity, we trim the sample to exclude zipcodes that fall outside the overlapping range of the 5th to 95th percentiles for three key demographic variables: median household income, percent Black residents, and percent dual-eligible beneficiaries. These exclusions are applied separately within each procedure category to preserve within-group comparability.

Figure A.2 presents a flow diagram of sample construction, including initial case counts and each exclusion step. The final analytic sample includes over 2.7 million outpatient surgical episodes that meet all inclusion criteria.

Appendix figures

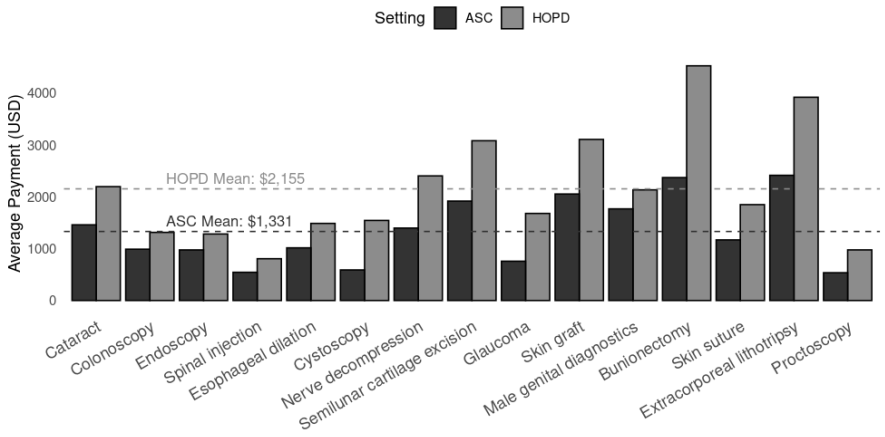


Figure A.1: Average ASC vs. HOPD procedure costs

Notes: This figure compares total average costs for 15 of the most frequently performed ASC procedures for Medicare patients (see Section 2.2). Episode costs were defined as described in Section 2.3.

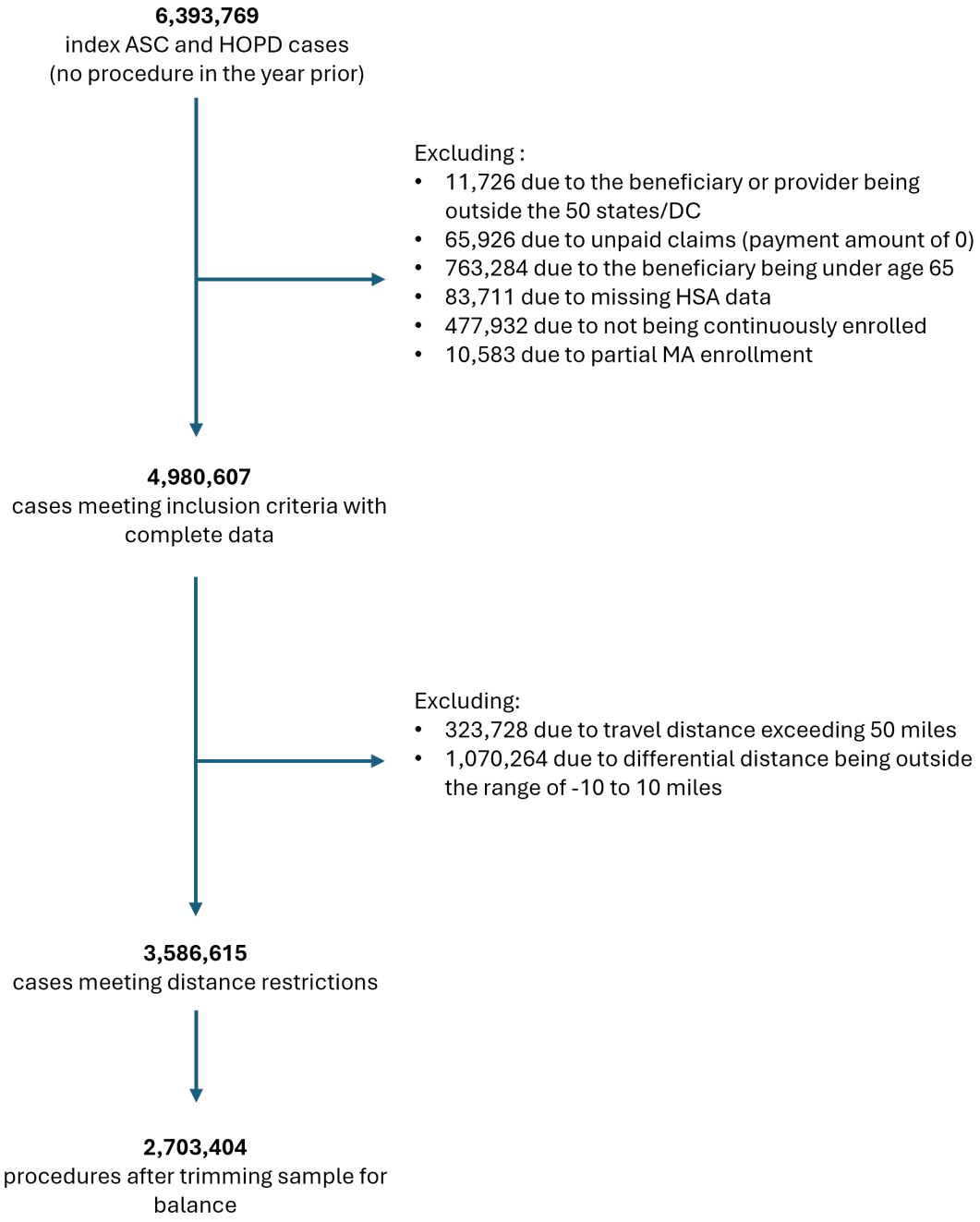
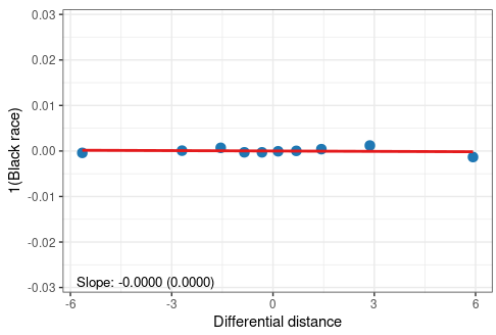


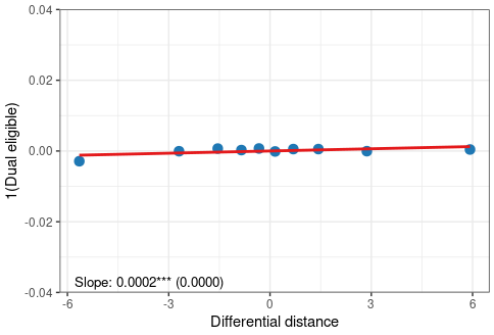
Figure A.2: Sample construction

Notes: This figure summarizes the sequential restrictions used to construct the analytic sample. See Sections 2.2 and A.2 for details.

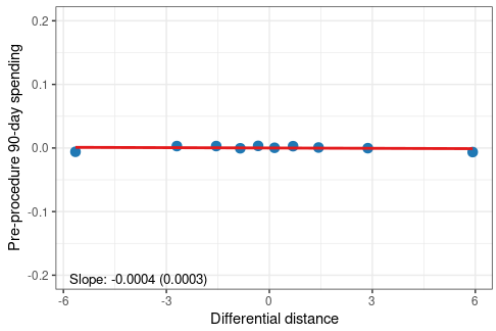
Figure A.3: Binned residual plots, alternative measures of risk



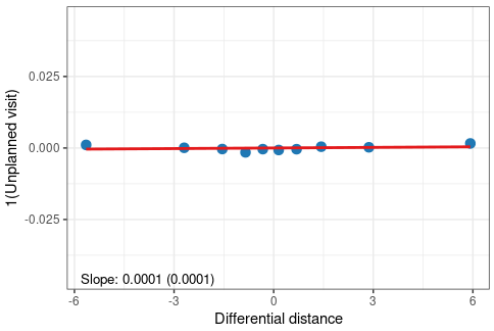
(a) 1(Black race)



(b) 1(Dual eligibility)



(c) 1(Pre-procedure spending)



(d) 1(ED or urgent care visit)

Appendix Tables

Table A.1: Identification of Visit Types in Medicare Claims Data

Visit Type	**Outpatient Revenue Center Codes**	**Carrier File Codes**
Office Visits	0500–0515, 0517–0525, 0527–0529	Place of Service Code = 11
Emergency Department	0450–0459, 0981	
Urgent Care	0516, 0526	Place of Service Code = 20

Note: Visit types are identified using both outpatient revenue center codes and carrier file place of service codes. ED visits are also identified through IP admissions from the MedPAR file with ED flag = 1.

Table A.2: Diagnosis Codes Used to Identify Related Office Visits

Procedure	**CCS**	**Codes and Description**
Colonoscopy	76	211/D23: Benign neoplasm of colon
		V58: Encounter for aftercare
		562: Diverticula of small intestine
Cataracts	15	366/H25: Cataract
		Z96/V43: Presence of intraocular lens
Endoscopy	70	530/K21: Reflux
		787: Digestive symptoms
		789: Abdominal pain/other symptoms
Spinal injection	5	724/M54: Back disorders
		722/M51: Disc disorders
		721/M47: Spondylosis
		M48: Other spondylopathies
Cystoscopy	100	599: UTI
		788: Urinary symptoms
		N40: BPH
		188: Malignant neoplasm of bladder
Esophageal dilatation	69	530/K22: Esophageal disorders
		K21: GERD
		787/R13: Dysphagia
Nerve decompression	6	354/G56: Mononeuritis of upper limb (e.g., carpal tunnel)
		719/M25: Joint pain
Semilunar cartilage excision	151	836/S83: Meniscal tear
		717: Internal derangement of knee
		719/M25: Pain in knee
		M17: OA of the knee
		Z47: Orthopedic aftercare
Glaucoma	172	365/H40: Glaucoma
		366: Cataract
		362: Retinal disorders
Skin graft	14	173/C44: Malignant neoplasm of skin
		V58: Post-procedure aftercare
Extracorporeal lithotripsy	107	592/N20: Kidney/ureter stones
		Z87: Hx of urinary calculi
		599: UTI
		788: Urinary symptoms
Bunionectomy	143	735/M20: Toe deformities, including bunion
		V58/V67/Z47: Aftercare/orthopedic aftercare
		727: Synovial and tendon disorders
		729: Pain in limb
		M79: Pain in foot
		719: Joint symptoms

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Procedure	**CCS**	**Codes and Description**
Male genital diagnostics	116	185/C61: Prostate cancer
		600/N40: BPH
		790/R97: Elevated PSA
		788: Urinary symptoms
Skin suture	171	Z48: Removal of sutures
		173/C44/Z85: Malignant skin neoplasm
		V58: General aftercare
Proctoscopy	77	787: Diarrhea
		562: Diverticulosis
		569: Other disorders of intestine
		Z79: Long-term drug therapy
		787: GI symptoms
		V58: Aftercare

Notes: This table lists the ICD-9 and ICD-10 diagnosis codes used to classify follow-up office visits as "related" to the index outpatient procedure. A visit is flagged as related if the primary diagnosis matches one of the codes associated with the patient's procedure. CCS refers to Clinical Classifications Software categories. Where applicable, ICD-9/ICD-10 code pairs are listed together.

Table A.3: First Stage Robustness, County Fixed Effects

	(1)	(2)	(3)
	1(ASC)	1(ASC)	1(ASC)
Differential distance (miles)	-0.0079^{***}	-0.0077^{***}	-0.0077^{***}
	(0.0003)	(0.0003)	(0.0003)
County x year FEs	X	X	X
CPT FEs	X	X	X
Patient state FEs			X
Patient zip code controls	X	X	X
Patient controls		X	X
SE clustered by beneficiary zip code	X	X	X
Observations	2,703,404	2,703,404	2,703,404
Y-mean	0.62	0.62	0.62
F-stat	519	508	508

Notes: This table presents estimates of the relationship between undergoing a procedure at an ASC and the patient's differential distance. The cells present the coefficient γ obtained by estimating Equation 4. The independent variable is the difference between the distance in miles to the nearest ASC and the nearest HOPD for the patient. The outcome variable is an indicator for whether the patient had their surgery at an ASC (=1 if at an ASC, 0 if at an HOPD). Column 1 controls for patient County x Year fixed effects, Current Procedural Terminology (CPT) code fixed effects, and patient zip code controls. Column 2 (our preferred specification) additionally includes controls for patient characteristics and risk (age, race, sex, an indicator for dual eligible status, Charlson comorbidity score, and prior ASC, HOPD, ED or urgent care use). Column 3 further tests first stage robustness by including state fixed effects. Standard errors are clustered by procedure CPT code. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A.4: Balance of Patient Characteristics

	DD -10 to <0 (N $= 676,431$)	DD $= 0$ (N $= 1,039,423$)	DD >0 to10 (N $= 890,635$)
Differential distance (miles)	-3.41 (2.45)	0 (0)	3.77 (2.62)
ASC	0.74 (0.44)	0.65 (0.48)	0.53 (0.50)
Demographics			
Age <70	0.25 (0.43)	0.27 (0.44)	0.27 (0.44)
Age 70-79	0.52 (0.50)	0.52 (0.50)	0.52 (0.50)
Age, 80+	0.23 (0.42)	0.21 (0.41)	0.22 (0.41)
Female	0.58 (0.49)	0.57 (0.50)	0.57 (0.50)
Race, Black	0.04 (0.19)	0.04 (0.19)	0.04 (0.20)
Dual eligible	0.07 (0.25)	0.07 (0.26)	0.08 (0.27)
Risk measures			
Charlson comorbidity score	0.76 (1.31)	0.79 (1.36)	0.83 (1.38)
High risk indicator (Charlson score ≥ 2)	0.22 (0.41)	0.22 (0.42)	0.24 (0.43)
Office visit in past 90 days	0.97 (0.16)	0.97 (0.18)	0.97 (0.18)
Office visit in past 90 to 180 days	0.76 (0.43)	0.76 (0.43)	0.76 (0.43)
ED visit in past 90 days	0.07 (0.26)	0.09 (0.28)	0.09 (0.28)
Urgent care visit in past 90 days	0.03 (0.16)	0.03 (0.16)	0.02 (0.15)
90-day pre-procedure spending	1180 (1480)	1170 (1490)	1180 (1480)

Notes: This table presents the balance of patient attributes with respect to the differential distance (DD) instrument. Patients are categorized into three groups based on whether their differential distance is less than, equal to, or greater than zero miles. Differential distance is calculated as the difference between the distance (in miles) to the nearest ambulatory surgery center (ASC) and the nearest hospital-based outpatient department (HOPD). Column 1 presents the mean (standard deviation) values for each characteristic for patients with DD less than zero (i.e., those living closer to an ASC). Column 2 presents the mean (standard deviation) values for patients with DD equal to zero (i.e., those living equidistant from an ASC and an HOPD). Column 3 presents the mean (standard deviation) values for patients with DD greater than zero (i.e., those living closer to an HOPD).

Table A.5: Spending Outcomes in Levels

	(1) Procedure payment	(2) Any 90-day payment	(3) Non-zero 90-day payment	(4) Total Episode payment
A. OLS				
1(ASC)	-455.5***	0.0003	-204.1***	-644.9***
	(1.882)	(0.0004)	(6.889)	(7.029)
B. IV				
1(ASC)	-585.7***	0.0280***	-92.72	-615.0***
	(28.66)	(0.0077)	(122.9)	(123.2)
Observations	2,606,489	2,606,489	2,449,368	2,606,489
Y-mean ($,prob.)	1,504	0.941	2,538	3,892

Table A.6: ED Spending Outcomes in Levels

	(1) procedure payment	(2) ED payments	(3) Non-ED payments
2SLS: IV estimates			
1(ASC)	-585.7***	26.37	-56.67
	(28.66)	(29.77)	(107.2)
Observations	2,606,489	2,606,489	2,606,489
Y-mean	1504	236	2154

Table A.7: Results by Patient Demographics

	(1) Office visits, #	(2) Related office visits, #	(3) log(proc. payment)	(4) log(90-day payments)	(5) ED Visit	(6) Urgent Care Visit
1. Risk						
CCI = 0	1.093***	0.2980***	-0.4112***	-0.2096***	-0.0358***	0.0131*
	(0.1760)	(0.0863)	(0.0214)	(0.0323)	(0.0105)	(0.0066)
Observations	1,635,407	1,635,407	1,635,407	1,635,407	1,635,407	1,635,407
Y-mean	3.92	1.11	1539	3815	0.105	0.022
CCI > 0	1.511***	0.2093*	-0.3833***	-0.1263**	-0.0030	0.0344***
	(0.2313)	(0.0930)	(0.0225)	(0.0414)	(0.0159)	(0.0084)
Observations	1,067,997	1,067,997	1,067,997	1,067,997	1,067,997	1,067,997
Y-mean	5.30	1.25	1620	6149	0.177	0.023
2. Age						
Age $\leq$ 73	1.156***	0.2418**	-0.4126***	-0.1470***	-0.0100	0.0163*
	(0.2076)	(0.0931)	(0.0233)	(0.0361)	(0.0122)	(0.0077)
Observations	1,362,450	1,362,450	1,362,450	1,362,450	1,362,450	1,362,450
Y-mean	4.33	1.12	1568	4552	0.115	0.023
Age > 73	1.276***	0.2515**	-0.3881***	-0.1998***	-0.0323*	0.0258***
	(0.1915)	(0.0865)	(0.0208)	(0.0356)	(0.0126)	(0.0073)
Observations	1,340,954	1,340,954	1,340,954	1,340,954	1,340,954	1,340,954
Y-mean	4.77	1.22	1583	5198	0.161	0.022
3. Gender						
Men	1.008***	0.2102*	-0.4066***	-0.1851***	-0.0112	0.0172*
	(0.2101)	(0.0932)	(0.0228)	(0.0400)	(0.0137)	(0.0071)
Observations	1,162,487	1,162,487	1,162,487	1,162,487	1,162,487	1,162,487
Y-mean	4.45	1.12	1587	5048	0.134	0.019
Women	1.400***	0.2857***	-0.3950***	-0.1634***	-0.0311**	0.0240**
	(0.1926)	(0.0857)	(0.0212)	(0.0323)	(0.0118)	(0.0075)
Observations	1,540,917	1,540,917	1,540,917	1,540,917	1,540,917	1,540,917
Y-mean	4.63	1.21	1567	4742	0.141	0.025
4. Race						
Non-white	0.1362	-0.0392	-0.4158***	-0.2921***	-0.0426	0.0080
	(0.3937)	(0.1657)	(0.0419)	(0.0780)	(0.0304)	(0.0137)
Observations	255,115	255,115	255,115	255,115	255,115	255,115
Y-mean	4.12	1.02	1616	4785	0.142	0.018
White	1.338***	0.2768***	-0.3978***	-0.1603***	-0.0210*	0.0223***
	(0.1702)	(0.0778)	(0.0202)	(0.0290)	(0.0096)	(0.0063)
Observations	2,448,289	2,448,289	2,448,289	2,448,289	2,448,289	2,448,289
Y-mean	4.59	1.19	1571	4884	0.137	0.023

Notes: This table presents heterogeneity in the effects of surgery performed at an ASC on select patient quality and spending outcomes. Columns 1–6 present the coefficient β and its standard error obtained from 2SLS estimation of Equation 5, with differential distance to the nearest ASC versus the nearest HOPD serving as the instrument for undergoing a procedure at an ASC. The median age in our sample of patients was 73. All regressions include procedure CPT code fixed effects, patient HSA × Year fixed effects, and patient and ZIP code controls. Standard errors are clustered by CPT code. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.