# Redeterminations: What Happens When an Insurance Subsidy is Lost?*

Laurence Baker† Margarita Khvan‡ Alexander Lutsenko§ Maria Polyakova¶

September 2026

**Abstract**

We exploit discontinuities in the schedule for redetermining eligibility for Medicare's low-income prescription drug subsidy to estimate the effect of redeterminations and subsidy loss on insurance take-up, drug utilization, and health outcomes. Beneficiaries subject to redetermination are 4 percentage points more likely to lose the subsidy, a 182% relative increase. Losing the subsidy in turn increases the probability of dropping drug insurance by 15 percentage points; among those who retain coverage, out-of-pocket cost-sharing for drugs rises. Aggregate drug consumption is unchanged, but purchases decline in several therapeutic classes, including cardiovascular and mental health medications. Over two years of follow-up, we find no evidence of offsetting health effects. We detect no changes in the rates of hospitalizations (including admissions for heart attacks or strokes), ER visits, ICU admissions, or outpatient visits; we observe a reduction in SNF and Nursing Home stays. Overall, redeterminations produce no detectable change in Medicare's aggregate non-drug spending; we can rule out changes larger than 2%. Within the drug program, redeterminations reduce Medicare spending by approximately $1 million annually, a reduction financed in large part by shifting cost onto beneficiaries.

*Funding from the National Institute on Aging (K01-AG05984301) and Agency of Healthcare Research and Quality (T32-HS026128) is gratefully acknowledged.

†Stanford University and NBER; lbaker@stanford.edu

‡Stanford University; mkhvan@stanford.edu

§Northwestern University, aleksandr.lutsenko@kellogg.northwestern.edu

¶Stanford University and NBER; mpolyak@stanford.edu

# 1 Introduction

Health insurance subsidies is one of the largest categories of social spending in the United States (KFF, 2025; Finkelstein et al., 2019b). As in most public programs, health insurance subsidy eligibility is often means-tested. Complex administrative processes in turn exist to evaluate and re-evaluate eligibility based on income and assets. Social Security Administration (SSA) that conducts a lot of means-tested reviews spends nearly $2 billion per year, or 10% of its budget, on the review of continued eligibility for all programs it administers. Rich evidence across many means-tested programs suggests that the administrative complexity of initial eligibility verification is expensive to run, affects take up, and may introduce economic distortions (Currie and Gahvari, 2008; Moffitt, 2015). In comparison, there is substantially less evidence on the role of continued eligibility verifications, and especially on the longer-run non-fiscal implications of losing eligibility (Deshpande et al., 2025).

In this paper, we estimate the effect of losing means-tested health insurance subsidies through the re-determination process. Our context is the Medicare's low-income program (LIS) for Medicare prescription drug coverage (Medicare Part D) in years 2010 to 2022. The federal government provides extra subsidies for plan premiums and cost-sharing to enrollees with income under 150% of the federal poverty level (FPL) and limited assets. LIS is administered by the Social Security Administration (SSA). SSA estimates that the average value of the subsidy package is $5,700 per year, or nearly a quarter of annual income at the individual income eligibility cutoff (SSA, 2026). An important advantage of this context is that since the re-determination process applies only to a part of the beneficiary's health insurance package, we do not lose beneficiaries from the data when their subsidy eligibility lapses. This allows us to consider a rich set of outcomes over the course of three years since the initial subsidy receipt, including the beneficiaries' take-up of insurance, drug utilization, and health outcomes, in addition to fiscal implications. We find that the subsidy loss leads some beneficiaries to discontinue drug coverage altogether, while others reduce purchases of cardiovascular and mental health drugs. Surprisingly, we find no evidence of health offsets over the course of a two year follow up.

Our analysis relies on a birth date regression discontinuity design (RDD). We take advantage of quasi-experimental variation created by the intersection of Medicare's enrollment and LIS redetermination rules. For people enrolling in LIS right when they become eligible for Medicare, i.e. during the Initial Enrollment Period, the LIS enrollment month will be highly

correlated with the month of birth. The earliest time beneficiaries become eligible for Medicare (and LIS) due to old age is the month they turn 65.[1] At the same time, SSA selects beneficiaries for eligibility redeterminations based on annual cycles that use May 1 as a cutoff. Beneficiaries enrolled in LIS on or before April 30 of the current year can be selected to verify their LIS eligibility in September of the current year, while those who enrolled on or after May 1 will be included in the next cycle. These initial eligibility and LIS redetermination rules create a discontinuity in the probability that an LIS beneficiary could be selected for a redetermination review around May 1 birth dates. Beneficiaries with birth dates before May 1 and enrolling in LIS during the Initial Enrollment Period can be selected for LIS redetermination in their first year of enrollment, while those born after May 1 cannot.[2]

Within the RDD framework, we zoom in onto the universe of Medicare beneficiaries who were born in the 90-day window around May 1 and enrolled in Medicare and LIS during their Initial Enrollment Period (i.e. at age 65) in years 2010 to 2021 (this implies our sample consists of "Baby Boomer" cohorts born between 1945 and 1956). We follow these individuals for the first three years they are in the Medicare program.[3]

We start by evaluating different notions of the first stage in the data. We first test whether individuals born earlier in the year are indeed more likely to lose subsidies in years two and three after they first enroll in Medicare and LIS. The probability of losing the subsidy sharply changes around May 1, and is both statistically and economically significant. In the second and third year after LIS enrollment, being born before May 1 increases the probability of subsidy loss by 4 and 6 percentage points, respectively. This corresponds to, respectively, 182% and 44% increase relative to the mean probability of subsidy loss among beneficiaries born after May 1. The subsidy loss translates into a substantial drop in insurance demand. Beneficiaries born before May 1 are 0.6 and 1.1 percentage points more likely to drop prescription drug coverage in the second and third years since LIS enrollment, respectively, implying increases of 15 and 18 percentage points among compliers.

As anticipated by the administrative process, the subsidy discontinuation immediately

[1] We exclude individuals born on the 1st of the month, as they face different Initial Enrollment Period rules.

[2] Beneficiaries can still lose LIS throughout the year due to subsidy changing events rather than redetermination cycles, but those instances are rare in our sample. Subsidy changing events include marriage, death of a cohabiting spouse, divorce, annulment, or separations. These events can be reported by third parties or beneficiaries can self-report them to SSA, though it is not mandatory.

[3] To be able to track individuals for three years, we condition on survival until age 68. We also exclude individuals who are ever dual eligible during these three years, as they are considered automatically eligible for LIS, and hence, are not subject to LIS redetermination. Our empirical checks suggest that none of these sample restrictions generate differential selection around May 1 birthday cutoff.

translates into a reduction in Medicare's payments: in the year following the year of LIS enrollment, LIS premium payments for beneficiaries born just before May 1 decline by $6.9 (or 4%), while LIS cost-sharing payments go down by $42.4 (or 6%) per beneficiary per year. These reductions persist in the subsequent year and amount to large changes for the complier population of those beneficiaries who actually lose the subsidy as a result of the redetermination process: two stage least squares (TSLS) estimates imply a $109 decline in LIS premium payments and $938 in cost-sharing.

To move beyond the evaluation of fiscal impacts of re-determinations, we proceed to analyze the downstream changes in the utilization of prescription drugs as well as measures of health outcomes. The impact of losing subsidy varies substantially across therapeutic classes. While we do not detect changes in the utilization patterns of many classes of drugs and in the aggregate number of fills, we find reductions in the probability of filling prescriptions for some cardiovascular and mental health medications, including antipsychotics and sedatives. We also find declines in systemic antihistamines, obstructive airway diseases medications, and primarily asthma and COPD. Our are broadly consistent with the notion that prescription drugs tend to generally have inelastic demand Einav et al. (2018a), however the limited aggregate response contrasts with findings in several other settings that document a more pronounced behavioral response to subsidy lapses in health insurance Chandra et al. (2024); Hollrah (forthcoming); Gross et al. (2022). A likely explanation is that we are considering beneficiaries who are affected by the subsidy redetermination process and are hence by definition marginal to being eligible for subsidies and may exhibit more limited behavioral responses than an average infra-marginal beneficiary.

Using the richness of Medicare data, we move on to consider non-drug measures of healthcare utilization and, implicitly, health. Despite the pronounced changes in subsidies in the first stage, surprisingly we find no evidence of spillovers onto broader healthcare utilization, which we would expect to see if the increase in out of pocket costs of drugs and changes in drug utilization led to deteriorating health as has been documented in (Chandra et al., 2024). Beneficiaries who lose the subsidy are no more likely to be hospitalized, to have an ICU stay, to be admitted through the emergency department, or to be hospitalized for a cardiovascular condition. We similarly detect no changes in the probability of a physician visit, a primary care visit, a cardiologist visit, or an emergency room visit, nor in combined inpatient, outpatient, and carrier spending. The one exception is institutional long-term care: the probability of a nursing home

stay *falls* by 0.2 percentage points in both years (22% relative to the control mean in year 3), statistically significant in each, and the probability of a skilled nursing facility (SNF) stay falls by 0.1 percentage points in both years (14% relative to the control mean in year 3), reaching statistical significance in the third year.

Taken together, our results suggest that in the context of 65 to 68 year old lower-income adults who are on the margin of eligibility for means-tested Medicare support, losing prescription drug insurance leads to no detectable deterioration in health. These results are consistent with the findings of limited clinical effects of *gaining* insurance coverage for marginally eligible (albeit much younger) adults in the Oregon Health Insurance Experiment (Baicker et al., 2013). Our results contrast with Chandra et al. (2024, 2010a), who find evidence of significant health deterioration from disruptions in prescription drug (especially cardiovascular) fills in the Medicare—albeit on average older—population. Because we detect no changes in the non-drug utilization or Medicare spending, the fiscal implications of the redetermination program amount only to the direct savings on the LIS program, which we estimate to be $1.06 million per enrollment cohort net of any administrative costs of conducting the redeterminations.

Our analysis contributes to multiple strands of the literature. First, we contribute to a rich literature on cost-sharing and moral hazard in health insurance (Chandra et al., 2010a,b; Baicker et al., 2015; Aron-Dine et al., 2015; Einav et al., 2018a; Shigeoka, 2014; Brot-Goldberg et al., 2017; Einav and Finkelstein, 2018; Baicker et al., 2015; Hollrah, forthcoming; Einav et al., 2015; Chandra et al., 2024, among others). The closest in this space is Hollrah (forthcoming) and Roberts et al. (2025) that both consider the effect of prescription drug subsidy losses in the dual eligible Medicare-Medicaid population and find substantial reductions in drug purchases as well as increases in mortality in response. Our results are consistent with Fung et al. (2024) who document an association between LIS losses and an increase in out-of-pocket costs in the non-dual-eligible population.

Second, our work has implications for the broader literature on means-tested subsidies in a variety of healthcare contexts, including but not limited to subsidies to health insurance plans (Carey, 2021; Cabral et al., 2018; Duggan et al., 2016; Decarolis et al., 2020; Decarolis, 2015; Tebaldi, 2017), hospitals and physicians (Einav et al., 2018b; Dafny, 2005), and to consumers (Finkelstein et al., 2019b; Gross et al., 2022; Aron-Dine et al., 2013; Manning et al., 1987; Newhouse, 1993; Finkelstein et al., 2019a). Our findings underscore that subsidy effects may be highly heterogeneous across different types of consumers, which makes understanding the

returns for marginal beneficiaries central for policy-making (Hendren and Sprung-Keyser, 2020; Finkelstein and Hendren, 2020).

Finally, outside of the healthcare context, our work contributes to the expansive literature on the design of means-tested public benefits. An important strand of this literature examines how administrative burden may contribute to low take-up of social benefit programs (e.g., Finkelstein and Notowidigdo, 2019; Deshpande and Li, 2019; Ericson et al., 2025). Redetermination processes of the kind we exploit in our empirical design are likely to contribute to low take-up and are also extremely costly to run. A relevant policy question is whether such redetermination efforts are worthwhile. While our results are consistent with marginal beneficiaries being nearly indifferent and hence subsidy discontinuation may have muted effects on healthcare utilization, we also find that removing these subsidies generates limited savings.

The rest of the paper proceeds as follows. In section 2, we discuss the empirical context of Medicare prescription drug insurance, our data, and sample selection. Section 3 summarizes our regression discontinuity approach. Section 4 reports the results and Section 5 briefly concludes.

# 2 Setting and Data

## 2.1 Institutional Primer

Medicare is a federal health insurance program in the United States, primarily covering adults age 65 and older who have made mandatory payroll contributions to the program during their working lives. Contributions typically make beneficiaries eligible to enroll in Medicare Part A (for inpatient care) without premiums, while Medicare Part B (for outpatient services) and Part D (for prescription drugs) require paying premiums. In addition to premiums, all parts of the program have cost-sharing that beneficiaries have to pay when utilizing healthcare.

Medicare eligibility starts in the month an individual turns 65. To avoid penalties for late enrollment, beneficiaries must enroll during the Initial Enrollment Period (IEP)—a 7-month enrollment window that includes the birthday month as well as three months before and after.[4] Individuals can choose to receive their Medicare benefits either through traditional fee-for-service Medicare, in which the federal government directly reimburses providers of healthcare, or through a managed care Medicare Advantage plan, in which a private insurance plan administers the

[4]For individuals born on the first of the month, eligibility starts in the month before the month they turn 65. To make sure that the timing of eligibility is continuous with respect to the day of birth, we exclude these beneficiaries from the sample.

benefit. Medicare Part D program has no fee-for-service analogue and is always administered by a private insurer. Part D plans can be purchased as stand-alone prescription drug plans to supplement Traditional Medicare (TM), or as part of a Medicare Advantage (MA) plan.

Means-testing of benefits has become increasingly common in these different parts of Medicare. Starting in 2007, income-related Medicare premiums (also known as the Income Related Monthly Adjustment Amount) were enacted in Part B as part of the Medicare Modernization Act (MMA) of 2003. Further, several state-run Medicare Savings Program (MSP) initiatives that reduce premiums and cost-sharing have seen enrollment steadily increase over the past decade (MACPAC, 2024). The focus of our study are means-tested subsidies in Medicare Part D, which the federal government has provided since the program's inception in 2006.

Medicare Part D means-tested subsidies are known as Extra Help or Low Income Subsidy (LIS). SSA administers the LIS program. SSA deems many beneficiaries automatically eligible for LIS through participation in other programs, primarily Medicaid and Supplemental Security Income. Individuals can also apply for LIS and be approved or denied by SSA after review. LIS eligibility is based on modified income and asset tests. In 2026, single beneficiaries with less than $18,090 in resources and less than $23,475 in annual income (circa 150% FPL) were eligible. If approved, beneficiaries get a subsidy to defray the cost of plan premiums and cost-sharing. For example, in 2026, co-pays of beneficiaries with LIS for a one-month supply of generic and branded drugs cannot exceed $5.1 and $12.7, respectively, up until $2,100 after which the copay becomes $0 (CMS, 2026). Further, LIS covers 100% of premiums for a selection of Part D plans. SSA estimates that the average value of the subsidy package is $5,700 per year, which is equal to nearly a quarter of annual income at the income eligibility cutoff (SSA, 2026). SSA regularly reviews continued eligibility for LIS.

## 2.2 Data

The primary data source for this study are administrative Medicare data files provided by the Centers for Medicare and Medicaid Services (CMS). The files cover enrollment, plan characteristics, and prescription drug, inpatient, and outpatient records for years 2010-2022.

**Beneficiary Characteristics** For each beneficiary, 100% Master Beneficiary Summary Files (MBSF) provides information on the basic demographics and enrollment status for the universe of Medicare beneficiaries: age, sex, race, the SSA standard two-digit state code of residence, the

reason for original and current Medicare entitlement, whether a beneficiary is receiving benefits through the TM or MA program, and enrollment in Medicare Part D if any. For beneficiaries with prescription drug coverage, we also observe monthly data on each beneficiary's plan type and cost-sharing group. The latter allows us to determine if a beneficiary has any type of LIS in a given month.[5]

**Part D Plan Characteristics** CMS publishes annual reference files describing every Part D contract and plan benefit package offered in a given year. We use these files to observe two premium-related measures. The first is the plan's total monthly premium net of manufacturer rebates, which we assign to each beneficiary-month by matching their monthly contract and plan identifiers from the MBSF files to the corresponding plan-year record. The second is the Part D LIS premium subsidy: the portion of the premium paid on the beneficiary's behalf by LIS. The Plan Characteristics files report this subsidy separately for each of the four LIS benefit tiers (25%, 50%, 75%, and 100%), so we assign the applicable amount using the beneficiary's monthly cost-sharing group from the MBSF. Both measures are observed for the full baseline sample.

**Prescription Drug Purchases** When a Medicare beneficiary with Part D coverage fills a prescription, the prescription drug plan submits a record to CMS. Part D Event (PDE) files include all transactions by the random 20% sample of Medicare beneficiaries. Drug fills are observed for beneficiaries enrolled in stand-alone Prescription Drug Plans (PDPs) as well as those enrolled in Medicare Advantage Prescription Drug Plans (MA-PDs). The detailed administrative records contain key elements necessary to track drug fills and spending, such as drug name, dosage form, quantity purchased, date of service, and payments made by the beneficiary, plan, and third parties, including the LIS program. We assign drugs to therapeutic classes by mapping National Drug Codes (NDCs) observed at the claim level to Anatomical Therapeutic Chemical (ATC) classifications (up to ATC-4) using the RxNorm API provided by the National Library of Medicine.[6]

**Physician Services** For a 20% random sample of beneficiaries in Traditional Medicare, we observe professional charges by physicians and other clinical providers in Fee-for-Service Carrier

[5] In addition to the distinction between "deemed eligible" and "not deemed eligible" beneficiaries, individuals can receive partial LIS that provides a 15% coinsurance and a more limited premium subsidy.

[6] We are grateful to Fabrício Kury for developing and publicly sharing programs that query RxNorm API.

files. These files cover outpatient office visits and procedures, as well as physician fees for inpatient services.

**Hospital Outpatient Care** The Outpatient file contains institutional claims submitted by hospital outpatient departments and other facility providers for Fee-For-Service beneficiaries. We observe the date of service, total charges, the amount paid by Medicare, and the amount paid by the beneficiary through deductibles and coinsurance. Claims also carry revenue center codes identifying the hospital department delivering the service, which allows us to flag visits to the emergency room.

**Hospitals and SNFs** To trace the broader health consequences of prescription drug subsidies, we include the utilization of inpatient care in our analysis. The 100% Medicare Provider Analysis and Review (MedPAR) files contain full records of short- and long-term acute hospital and skilled nursing facility (SNF) stays for TM beneficiaries, as well as informational records (without payments) for MA beneficiaries.[7] We observe dates of admission and discharge, the type of admission (emergency or elective), the use of ICU, and a diagnosis-related group (DRG) code classifying stays by diagnosis and complexity for billing purposes.

**Long-Term Care** To track the use of long-term care nursing facilities, we leverage 100% Long Term Care Minimum Data Set (MDS) files, which contain records for regular health status screenings of all residents of nursing facilities certified by CMS, including beneficiaries in Traditional Medicare and Medicare Advantage.

### 2.3 Sample Construction

Our baseline sample starts with the universe of beneficiaries who first enrolled in Medicare Parts A and B at age 65 due to old age eligibility between 2010 and 2021. For each beneficiary, we find their first LIS spell if any. We consider this first spell to be the month of LIS enrollment. We exclude beneficiaries who are "deemed eligible" for LIS at any point within the first three calendar years following their LIS enrollment as they are automatically enrolled by SSA and are not subject to the same redetermination process (SSA, 2017).[8]

[7] MedPAR is estimated to capture nearly 90% of MA-covered hospitalizations (Cotterill, 2023).

[8] This primarily excludes beneficiaries who were dual eligible at any point during the year of LIS enrollment or the following two years. The probability of being dual-eligible exhibits no discontinuities at May 1 birth date.

We restrict the resulting sample to beneficiaries who applied and enrolled in LIS during their Initial Enrollment Period and who were born in the 90-day window around May 1.[9] All these individuals enrolled in Medicare and LIS around the time when they turned 65 and first became eligible. We follow these individuals for the first three years they are in the Medicare program.[10] The first year is the enrollment year, which is partial: it captures only the months from LIS enrollment through December. The subsequent two years are full calendar years. We restrict our baseline sample to a balanced panel of individuals who survive until 68 years of age.[11]

Our design also lets us distinguish three groups within the baseline sample: always-takers, never-takers, and compliers. Always-takers are beneficiaries born after May 1 who lose the subsidy despite being in the later redetermination cycle, and never-takers are those born before May 1 who retain the subsidy despite being in the earlier redetermination cycle. The remainder are compliers: beneficiaries who lose LIS if born before May 1 but not if born after. For these beneficiaries, the May 1 cutoff moves the first eligibility review forward by one cycle, and with it the timing of subsidy loss. The last paragraph in Section 3.1 describes how we recover complier outcome means.

### 2.4 Summary Statistics

Table 1 reports the basic facts about our baseline sample. We observe a total of 128,600 individuals, roughly equally split between those born 90 days before versus 90 days after May 1. The two groups are closely comparable on observable characteristics. As expected given our focus on the Initial Enrollment Period, the average age at LIS enrollment is 65 in both groups. In both, 60% of beneficiaries are women, 54% are non-Hispanic White, and 6% are Asian or Pacific Islander. Among beneficiaries born before May 1, 19% are Hispanic and 18% are Black; the corresponding shares among those born after May 1 are 21% and 17%. We find no economically meaningful differences in demographic characteristics across the cutoff. Medicare Advantage enrollment is

[9] We exclude beneficiaries born on the first day of any calendar month, as their Initial Enrollment Period is shifted by one month and they can enroll in Medicare one month before they turn 65. We do this to ensure a continuous relationship between date of birth and Medicare eligibility start date. We further exclude very few beneficiaries who drop Part D coverage before the end of the first calendar year of enrollment, i.e., by December of the enrollment year. This excludes beneficiaries who churn out of LIS immediately after initial enrollment.

[10] To facilitate a clean three-year follow-up window, we only keep beneficiaries who continuously reside in one of the 50 U.S. states or Washington D.C. during the first three years following LIS enrollment.

[11] Survival rates to age 68 are very high and we did not uncover any evidence of selection that this restriction introduces. See Appendix for details as well as robustness checks that probe the sensitivity of our results to all key sample restrictions. For the 2021 enrollment cohort, the post-enrollment observation window is truncated by data availability until 2022. We require survival through the end of 2022 rather than to age 68.

similarly balanced at the time they first join Medicare: 42% and 43% of beneficiaries born before and after May 1 enroll in Medicare Advantage rather than Traditional Fee-for-Service Medicare.

# 3 Empirical Approach

## 3.1 Regression Discontinuity Design

There are two institutional facts that we leverage in our empirical approach. First, beneficiaries who enroll in LIS right when they become eligible for Medicare — i.e., during the Initial Enrollment Period — most commonly enroll in LIS in their month of birth or (less commonly) in the three months right after their birth month.

Second, SSA conducts annual LIS redeterminations to verify continued eligibility for non-deemed LIS beneficiaries. Initial redeterminations happen right away in the first year of LIS eligibility and cyclical redeterminations happen in subsequent years.[12] Crucially, SSA anchors its redetermination cycles around May 1 eligibility cutoff. Initial redeterminations in a given calendar year are done only among beneficiaries who became eligible for LIS from May of the previous year to April of the redetermination year. Those who became eligible on or after May 1 fall into the next year's redetermination cycle. Since beneficiaries who enroll in LIS during the Initial Enrollment Period cannot enroll before their 65th birthday, those born after May 1, will not be included in SSA's initial redetermination cycle until the second calendar year of their enrollment. Those born before May 1, in contrast, can be subject to initial redeterminations in the first calendar year and thus can lose LIS at the start of their second year of enrollment.

This combination of Medicare enrollment and LIS eligibility verification rules creates as good as random assignment of the probability of losing LIS around May 1 birthday, in the second and third calendar year after enrollment. This setup lends itself well to using a regression discontinuity design (RDD) to analyze the effect of losing subsidies on a variety of outcomes.

To operationalize the RDD idea empirically, we estimate a parametric regression discon-

[12] In both cases, SSA does not run the redetermination process for all beneficiaries; instead it randomly selects subsets of beneficiaries among those who satisfy the relevant criteria. Beneficiaries selected for redeterminations are sent a letter in early September with SSA form 1026 (SSA-1026-OCR-SM-REDE) that asks beneficiaries to provide information to verify their LIS eligibility. Beneficiaries have 30 days to respond to this letter with the possibility of a one-time 30-day extension that must be requested during the original response period. If the form is returned before November 30, then any changes are effective January 1. If the form is returned after November 30, then any changes are effective the first day of the second month following the month the form is returned. For those who do not respond to the eligibility verification request, a local SSA office follows up in January-February of the following year (usually by calling them) and if there is still no response, then the subsidy is terminated effective April 1. Beneficiaries are entitled to appeal the SSA's decision and their LIS coverage may be extended until SSA responds to the appeal.

tinuity specification with the week of birth as a running variable, allowing both the slope and intercept to vary on each side of the May 1 birthday cutoff:

$$Y_i = \beta_0 + \beta_1 \mathbf{1}[w_i < 0] + \beta_2 w_i + \beta_3 w_i \mathbf{1}[w_i < 0] + \varepsilon_i \tag{1}$$

where $Y_i$ is the outcome of beneficiary $i$ who enrolled in LIS. $w_i$ is the week of birth, centered at zero for the week of May 1–May 7, which is the earliest week of birth unaffected by LIS redetermination in the year after LIS enrollment. Our coefficient of interest is $\beta_1$, which captures the discontinuity at the May 1 cutoff. Since beneficiaries born *before* May 1 are more likely to be affected by redeterminations, $\beta_1$ is the coefficient on an indicator for having an *earlier* birthday relative to the May 1 cutoff. Coefficients $\beta_2$ and $\beta_3$ allow for linear trends in the outcome, with different slopes on the two sides of the cutoff. We restrict the estimation sample to beneficiaries born within 90 days, or 13 weeks, on either side of the cutoff. Standard errors are clustered at the week of birth, the level of the running-variable bin, which yields 26 clusters within the estimation window.

There are several identifying assumptions needed to support this RD approach. First, individuals with birthdays close to May 1 need to not vary systematically in their eligibility for health insurance subsidies, health, and healthcare utilization except for their treatment by the SSA's LIS redetermination rules. Second, individuals born before versus after May 1 cannot manipulate their birthdays. And finally, individuals born before versus after May 1 need to not manipulate the timing of outcomes.

All of these assumptions are highly likely to hold in our setting. As we observe in Table 1, there are no differences in basic demographics between beneficiaries born before May 1 versus after May 1. The inability to manipulate the date of birth is mechanical. Further, for non-prescription drug outcomes, coverage does not change and hence there is no incentive to change the timing of healthcare utilization. For prescription drug outcomes, there is an incentive for individuals born before May 1 to reallocate their drug consumption towards the first year of their program enrollment. In practice, this kind of reallocation is unlikely for two reasons. First, while the May 1 cutoff increases the probability of being asked to verify eligibility, individuals cannot know with certainty whether they will be selected. Moreover, beneficiaries likely have limited or no knowledge of the details of the SSA's administrative process. Second, even if beneficiaries had perfect information and foresight, their ability to reschedule prescription fills

is constrained by limits on days of supply dispensed at pharmacies. Nevertheless, to assess the plausibility of such re-timing of fills, we check that prescription drug consumption in the first year of enrollment is not higher among those born before May 1. We observe no such differences.

We estimate specification 1 separately for outcomes in the first and second full calendar years following the year of LIS enrollment. In the year following LIS enrollment, beneficiaries born before May 1 and selected for initial LIS redetermination may lose LIS, while beneficiaries born after May 1 are unaffected by the LIS redetermination process. In the third year since LIS enrollment, beneficiaries born before May 1 may experience cyclical LIS redetermination, whereas those born after May 1 may be selected for initial LIS redetermination. As a placebo exercise, we also estimate our regressions for outcomes in the calendar year of LIS enrollment, when all beneficiaries are not yet affected by LIS redetermination. We track beneficiaries from the month of their enrollment through December and annuitize utilization and spending measures to account for differences in the length of their enrollment in that year.

We consider several outcomes of interest, moving from the first stage that measures the presence of subsidies, to decisions about maintaining prescription drug coverage that we expect to be directly affected by subsidies, to utilization of prescription drugs. We then expand our set of analyses to non-drug utilization of healthcare that provides us with claims-based measures of health outcomes.

**Instrumental Variables**

Crossing the May 1 threshold does not deterministically remove the subsidy; it raises the probability that a beneficiary's eligibility is reviewed and, conditional on review, that the subsidy is lost. The design is therefore a fuzzy regression discontinuity, and we use birth before May 1 as an instrument for subsidy loss in order to recover the effect of losing LIS on those induced to lose it.

Let $D_{i(t)}$ indicate that beneficiary $i$, who enrolled in LIS in calendar year $t$, is ever without a subsidy over the relevant horizon. Using the same running variable, bandwidth, and control function as in equation (1), we estimate by two-stage least squares

$$D_i = \pi_0 + \pi_1 \mathbf{1}[w_i < 0] + \pi_2 w_i + \pi_3 w_i \mathbf{1}[w_i < 0] + \nu_i, \tag{2}$$

$$Y_i = \gamma_0 + \tau^{\text{LATE}} \widehat{D}_i + \gamma_2 w_i + \gamma_3 w_i \mathbf{1}[w_i < 0] + \eta_i, \tag{3}$$

where the excluded instrument is the indicator for birth before the May 1 cutoff, $\mathbf{1}[w_i < 0]$, and standard errors are clustered at the week of birth. Outcomes are measured in the second and third years since LIS enrollment.

The IV interpretation requires an exclusion restriction: that being born before versus after May 1 affects outcomes only through the timing of eligibility redetermination. This assumption is likely to hold, as We do not expect any systematic seasonal discontinuities in drug use or health around May 1 birthdate cutoff. Monotonicity further requires that no beneficiary is made less likely to lose the subsidy by being born before May 1. This holds by the institutional design of the redetermination cycle: birth before the cutoff adds an opportunity for eligibility review, namely selection into the September cycle of the enrollment year, and never removes one. We exclude beneficiaries born on the first of the month throughout, as they are subject to different Initial Enrollment Period rules and their birth date therefore maps differently into the month of LIS enrollment.

Under these conditions, $\tau^{\text{LATE}}$ is the average effect of subsidy loss among compliers: beneficiaries born before May 1 who are selected for redetermination in their first year of LIS enrollment and lose the subsidy as a result. This is the population of direct policy relevance, since it consists precisely of those whom the redetermination process removes from the program. It is also, by construction, the population who is marginal to the subsidy eligiblity thresholds.

The two-stage least squares estimates above are local average treatment effects, so their interpretation depends on who the compliers are. We therefore recover their outcome means directly. On either side of the cutoff, the observed mean is a share-weighted average of always-taker, never-taker, and complier means. Always-takers are beneficiaries born after May 1 who lose the subsidy despite being in the later LIS redetermination cycle, and never-takers are those born before May 1 who retain it despite being in the earlier redetermination cycle. We estimate their shares and means by pooling across the estimation window on each side, which assumes these quantities do not vary systematically with week of birth. Subtracting their share-weighted contributions from the observed outcome and dividing by the complier share recovers the complier mean. We plot this mean by week of birth and estimate the discontinuity using the specification in Equation 1. Results are reported in Appendix Figures A3, A5, and A4.

### 3.2 Outcomes

Using the RDD methodology outlined in Section 3.1, we examine outcomes across three time periods: the LIS enrollment year (from enrollment month through December), the subsequent year, and the year thereafter. We analyze each period separately. The specific outcome measures are described below.

**First Stage** We examine how LIS redetermination rules — which subject beneficiaries enrolled before May 1 to earlier redetermination — affect LIS enrollment duration. Our first-stage outcomes capture the extensive margin of subsidy loss, identified using the monthly Part D cost-sharing group variable from the Medicare Master Beneficiary Summary Files (MBSF). The subsidy loss indicator equals one if a beneficiary experiences at least one month in a given year with either Medicare Part D coverage without LIS or no Part D coverage. See Appendix Figure A1 for an alternative measure of subsidy loss, the share of months per calendar year during which beneficiaries lacked LIS coverage.

**Insurance Take-Up and Premiums** Our indicator for lacking LIS includes both beneficiaries with Part D coverage but no subsidy and those who disenroll from Part D entirely. To separate these effects, we examine the probability of complete Part D disenrollment, defined as having no months of Part D coverage in a given year, and the share of months per year without Part D coverage.

We also observe two Part D premium measures from the Part D Plan Characteristics files. The Part D list premium is the monthly premium of the plan in which a beneficiary is enrolled, net of manufacturer rebates, summed over months a beneficiary is alive and annualized. Because more expensive plans typically offer broader formularies and lower cost-sharing, we use it as a proxy for plan generosity, allowing us to test whether beneficiaries who lose the subsidy switch to more or less generous plans. The Part D LIS premium subsidy is the portion of that premium paid on the beneficiary's behalf by the LIS program. It takes the value zero in any month without LIS, whether the beneficiary has Part D coverage without a subsidy or has dropped Part D altogether, and is then annualized by the number of months alive. Since LIS covers premiums in full for a selection of benchmark plans, this measure captures the mechanical value of the premium subsidy a beneficiary receives.

**Prescription Drug Utilization and Cost-Sharing** We further quantify the immediate effects on who pays for prescription fills using Medicare Part D Event files, specifically the dollar amount of drug expenditures covered by LIS and the amount paid out of pocket by beneficiaries. Changes in these measures reflect both the mechanical loss of LIS coverage and behavioral responses in drug consumption, which we examine next.

We first examine whether subsidy loss affects aggregate drug consumption measured by total standardized fills and total prescription drug costs per year. Beyond aggregate measures, we analyze the effects of subsidy loss on prescription drug fills in individual ATC-2 therapeutic classes.[13] We define the outcome variable as the probability, in percent, of at least one fill in each therapeutic class.

**Inpatient and Outpatient Care** We explore spillover effects of increased premiums and cost-sharing in Part D on broader healthcare utilization. For inpatient healthcare utilization, we examine hospital, SNF, and long-term care facility stays using Medicare Provider Analysis and Review (MedPAR) files and Long Term Care Minimum Data Set (MDS) files. In addition to looking at any hospital admissions, we focus separately on emergency admissions and ICU stays, as these typically involve urgent medical needs rather than elective care, and on hospital stays for circulatory conditions, which are common among elderly patients and potentially preventable through medication adherence.

For outpatient healthcare utilization, we examine the probability of having any outpatient physician visit and, separately, primary care physician and cardiology visits using Carrier Fee-For-Service files. We also measure emergency room (ER) visits, which we identify from positive ER payments in MedPAR together with any ER revenue-center record in the Outpatient file. We complement this analysis with aggregate non-drug healthcare spending, defined as the sum of inpatient spending from MedPAR and outpatient spending from the Outpatient and Carrier files. We report the total and its division between Medicare and beneficiary out-of-pocket payments.

Due to data restrictions, the outpatient analysis includes only beneficiaries enrolled in Traditional Medicare during the analyzed timeframe. To demonstrate that this sample restriction does not create selection bias, we report Traditional Medicare enrollment rates across all study periods and the share of months with Traditional Medicare coverage in Appendix Figure A2.

[13] We exclude ATC-2 classes with fewer than 500 beneficiaries in our baseline sample having at least one claim in that therapeutic class in the second year since LIS enrollment. We also exclude A11 (Vitamins) and A12 (Mineral Supplements), as these are frequently purchased over-the-counter, V03 (All Other Therapeutic Products), and N07 (Other Nervous System Drugs).

# 4 Results

## 4.1 First Stage

We begin by examining whether the administrative rules governing LIS redetermination generate meaningful differences in subsidy receipt between beneficiaries born just before and just after May 1. Figure 1 plots the raw relationship between week of birth and LIS loss, separately for each of the three years following initial enrollment. Panel A of Tables 2 and 3 report the corresponding regression discontinuity estimates from Equation 1, estimated for outcomes in years 2 and 3 since LIS enrollment, respectively.

The empirical patterns are consistent with our understanding of SSA's redetermination rules. In the year of initial LIS enrollment and there is no discontinuity at the cutoff, subsidy loss is rare regardless of birth date. This is expected, as no beneficiaries in our sample are subject to redetermination cycles in their enrollment year.

A sharp discontinuity emerges in the second year since enrollment. Among beneficiaries born after May 1, only 2.2% lose their subsidy. Being born before May 1 raises this probability by 4.0 percentage points (CI: [3.4, 4.6]), a 182% increase over the control mean.[14].

These differences persist and widen in the third year. As beneficiaries born after May 1 face their first initial redetermination, subsidy loss in the control group rises to 14.2%. However, early-birthday beneficiaries undergo their first cyclical redetermination, producing an even larger discontinuity of 6.2 percentage points (44% relative to the control mean) in the probability of losing LIS.

## 4.2 Insurance Take-Up and Premiums

Losing the low-income subsidy raises both beneficiaries' premium and out-of-pocket payments. A combination of these financial pressures may affect the extensive margin of prescription drug insurance, leading some beneficiaries to drop Part D coverage altogether. As Panels (a)–(b) of Figure 2 demonstrate, beneficiaries in the treatment group are 0.6 p.p. and 1.1 p.p. more likely to have no Part D coverage in at least one month of the second and third year since enrollment, respectively, and spend, on average, 0.3 p.p. and 0.8 p.p. less time with Part D coverage in

[14]The 2.2% loss rate among beneficiaries born after May 1 likely reflects SSA reassessment processes that operate outside the schedule of initial and cyclical redeterminations. In particular, beneficiaries may lose LIS outside the annual redetermination cycle if they report subsidy-changing events (e.g., marriage, divorce, or the death of a spouse) or experience changes in income, assets, or household composition that affect eligibility (Social Security Administration, 2017).

the second and third years. While these reduced-form estimates of the extensive margin are small, they become economically meaningful when scaled by the first stage, which implies that compliers are 15 and 18 percentage points more likely to drop Part D coverage, against control means of 1.9% and 3%.

Panel B of Tables 2 and 3 reports the effect of subsidy loss on the list premium of a beneficiary's Part D plan, which we use as a proxy for plan generosity. We find no discontinuity at the May 1 cutoff in either year (year 2 RD coefficient: –0.9, CI: [–3.99, 2.19], control mean: $224.1; year 3 RD coefficient: +0.6, CI: [–2.49, 3.69], control mean: $222.1). These estimates are precise enough to rule out changes larger than about $4 per year in either direction, indicating that beneficiaries who lose the subsidy and retain coverage do not move to systematically more or less expensive (and, likely, generous) plans.

The premium subsidy itself, by contrast, falls sharply. Being born just before May 1 reduces the amount LIS pays toward the Part D premium by $6.9 per person per year in the second year and $6.7 in the third, both statistically significant and corresponding to roughly 4% of the control means of $183 and $163.3. Two-stage least squares estimates imply declines of $172.2 and $108.6 for compliers in the second and third year since LIS enrollment, respectively. This decline is largely mechanical: beneficiaries who lose LIS no longer receive a premium subsidy, and those who drop Part D coverage altogether receive none for the months they are unenrolled.

### 4.3 Prescription Drug Utilization and Cost-Sharing

The loss of the low-income subsidy immediately affects LIS payments towards drug cost-sharing. In year 2, beneficiaries born before May 1 receive $42.4 (5.8% relative to the control mean) less annually. Similarly, in year 3, beneficiaries born before May 1 receive $57.4 (7.5% relative to the control mean) less annually. Both estimates are marginally significant. Two-stage least squares estimates imply declines of $933.3 and $937.7 for compliers in the second and third year since LIS enrollment, respectively.

The corresponding increase in what beneficiaries pay out-of-pocket is positive but not statistically distinguishable from zero. Patient pay amounts for Part D drugs rise by $5.2 in the second year (95% CI: [−$3.5,+$13.9], control mean: $92.4) and $4.9 in the third year (95% CI: [−$15.7,+$25.5], control mean: $125.5). The point estimates are of the expected sign and correspond to 3.9–5.6% of the control means, but the confidence intervals are wide enough that

we cannot distinguish them from no change.

A decrease in cost-sharing subsidies could reduce total drug consumption. Panels (a)–(b) of Figure 3 and Panel C of Tables 2 and 3 present evidence on this margin. We find no such behavioral response in our sample. In the second year since enrollment, the reduced-form effect of an early birthday on the number of standardized fills is +0.0 (95% CI: $[-1.6, +1.6]$) over a baseline of 35 fills. The effect on total annual drug cost is \$25.7 (95% CI: $[-\$237.3, +\$288.7]$) over a baseline of \$2,344.2. The third-year estimates are similarly small. In other words, a beneficiary who is marginally eligible for LIS does not meaningfully decrease their overall drug consumption in response to losing the subsidy.

We next explore whether the aggregate null effects on utilization mask changes in the composition of drugs consumed.

Figure 4 estimates separate RD coefficients for the probability of filling prescriptions for drugs in each ATC-2 therapeutic class. The figure reports estimates for the second year since LIS enrollment. Across most classes, the discontinuity at the May 1 cutoff is estimated to be zero. However, there is evidence of a decline in the use of psycholeptics — medications with calming effects, including antipsychotics, sedatives, and hypnotics. We also observe a decrease in the use of beta blockers, drugs for obstructive airway diseases (primarily, asthma and COPD), systemic antihistamines, and systemic corticosteroids. This is suggestive evidence of potential substitution patterns, but not full discontinuations of treatments.

### 4.4 Inpatient and Outpatient Care

We next ask whether changes in prescription drug subsidies have spillover effects on other dimensions of healthcare utilization and, implicitly, health. If subsidy loss causes beneficiaries to reduce adherence to medically necessary medications, we might expect to see increases in acute care — a pattern documented in related settings (e.g. Chandra et al., 2010a, 2024). Moreover, changes in drug affordability may affect beneficiaries' propensity to seek care that involves prescription drugs.

Figure 5 and Panel D of Tables 2 and 3 present the results for outpatient physician visits. We find no significant discontinuities in the probability of an office visit with any provider (year 2 RD coefficient: −0.1 p.p., control mean: 66.8%), with a primary care provider (year 2 RD coefficient: +2.9 p.p., control mean: 51.3%), or a cardiologist (year 2 RD coefficient: −0.9 p.p.,

control mean: 10.9%).[15] Similarly, losing the subsidy did not increase the probability of an ER visit (year 2 RD coefficient: −0.4 p.p., control mean: 20.1%). These null effects persist in the third year.

Figure 6 and Panel D of Tables 2 and 3 report the results for inpatient care utilization. The loss of LIS does not lead to a change in inpatient hospitalization rates in years 2 (RD coefficient: –0.1 p.p., CI: [–0.72, 0.52], control mean: 7.9%) and 3 (RD coefficient: –0.2 p.p., CI: [–0.82, 0.42], control mean: 8.3%). Exploring particular types of hospitalizations — stays involving the use of ICU, emergency admissions, and stays for circulatory system conditions — we find a similar absence of discontinuity at the May 1 cutoff in both years.

Despite no detectable changes in inpatient hospital stays, we find reduced utilization of post-acute and long-term care facilities following subsidy loss. The probability of a skilled nursing facility (SNF) stay falls by 0.1 p.p. relative to a control mean of 0.5% in the second year and 0.7% in the third year, reaching statistical significance in the third year. The probability of a nursing home stay falls by 0.2 p.p. in both years, relative to control means of 0.7% in the second year and 0.9% in the third year, and is statistically significant in each.

Panel E of Tables 2 and 3 reports combined inpatient, hospital outpatient, and carrier spending, in total and separately by payer. We find no significant discontinuities in total spending in either the second year (RD coefficient: –279.6, CI: [–1215.2, 656.0], control mean: $4,699.8) or the third (RD coefficient: –892.4, CI: [–2351.0, 566.2], control mean: $5,335.3). The same holds when spending is split by payer: neither the amount paid by Medicare (year 2: –141.6; year 3: –802.2) nor the amount paid out of pocket by beneficiaries (year 2: –57.6; year 3: –136.9) changes significantly at the May 1 cutoff. These estimates are, however, less precisely estimated than our utilization results.

### 4.5 Fiscal Implications for Medicare

Our estimates allow a back-of-the-envelope calculation of what LIS redetermination recovers for the Medicare program. We use our second-year RD estimates and consider the counterfactual of delaying a beneficiary's first eligibility redetermination by one cycle.

Our sample contains 128,600 beneficiaries born within 90 days of May 1 across 12 enrollment cohorts (2010-2021). Scaling to a full birth year and a single enrollment-year cohort

[15] Our measures of office visits are based on Fee-for-Service Carrier files and exclude stays by beneficiaries in Medicare Advantage (MA). Notably, we observe no changes in MA enrollment rates around May 1 discontinuity.

(which in our case is equivalent to a birth-year cohort) gives 21,433 beneficiaries per enrollment year. Applying our reduced-form estimates, being subject to the earlier redetermination cycle reduces the Part D LIS premium subsidy by $6.90 per beneficiary-year (Table 2) and its share of prescription drug costs by $42.40 (Table 2), for a combined $49.30 per beneficiary-year, or $1.06 million per enrollment cohort. We detect no offsetting change in non-drug Medicare spending: our estimates of combined inpatient, outpatient and carrier Medicare payments are statistically indistinguishable from zero. This calculation implies that the redetermination process on marginal LIS recipients recovers about 5% of the subsidy spending. While that saving is non-trivial, it is not clear whether it generates net fiscal savings. The $1.06 million in savings needs is generated by a high administrative burden of conducting redeterminations that involves multiple mailings, phone line availability across multiple SSA field offices, and the paperwork review process itself.

# 5 Discussion and Conclusion

In this paper, we estimate the effects of the redetermination process for marginal low-income recipients of subsidies in the Medicare Part D prescription drug program. Exploiting quasi-experimental variation generated by the intersection of Medicare initial enrollment rules and SSA's LIS redetermination schedule, we find that beneficiaries born just before May 1 are disproportionately more likely to lose their low-income subsidy shortly following enrollment.

Taken together, our analysis of downstream effects suggest limited behavioral responses to this loss of subsidy. Some beneficiaries respond by dropping prescription drug coverage. Yet the behavioral response beyond this insurance margin is strikingly limited. We find no statistically significant reductions in aggregate drug utilization, and the reductions we do observe are confined to a small number of ATC-2 therapeutic classes. The null effects extend to non-drug healthcare utilization, including hospitalizations, ICU stays, emergency admissions, physician and specialist visits, emergency room visits, and combined inpatient, outpatient, and carrier spending. The one exception is institutional long-term care: both nursing home and SNF stays decline, pointing toward less institutional care.

These findings are somewhat surprising in light of a large literature documenting substantial demand responses to cost-sharing and health offsets from drugs to inpatient and outpatient utilization. Several features of our setting may help explain the more muted responses we ob-

serve. First, our estimates are identified from individuals who are marginal to LIS eligibility. These are beneficiaries whose income and assets place them near the eligibility threshold and who are thus subject to the administrative process of eligibility redetermination. The fact that we observe limited demand responses at this margin is consistent with the subsidy program being designed — whether by intention or not — so that the marginal recipient is one for whom the subsidy has limited effects on utilization. Second, even after losing LIS, beneficiaries in our sample retain their eligibility for Medicare Part D that is partially funded by the federal government for all beneficiaries. Only a limited fraction of beneficiaries choose to drop Part D altogether when faced with higher premiums. For the vast majority who remain enrolled, cost sharing increases but does not become prohibitive — particularly for generic drugs, which constitute a large share of fills in our population. The limited response we document is then consistent with the view that the most common (often generic) prescription drugs tend to have relatively inelastic demand, particularly for chronic medications in the elderly population (Einav et al., 2018a).

The limited behavioral response by beneficiaries suggests that Medicare's fiscal savings from redeterminations are primarily funded by a transfer of cost onto the beneficiaries. This in turn implies that if the redetermination process itself is costly, the social welfare returns to spending on this process are limited even if it may be cost-effective fiscally.

Several caveats are worth noting. Our estimates are local to a specific margin — beneficiaries who enroll in LIS during the Initial Enrollment Period and whose income and assets are near the eligibility threshold. Responses may differ for beneficiaries who are infra-marginal to eligibility, enter LIS through automatic enrollment pathways, or for populations with different health profiles. Our analysis is also limited to the first three years following LIS enrollment; longer-run effects on medication adherence, health, and mortality remain an important open question.

## References


**Aron-Dine, Aviva, Liran Einav, Amy Finkelstein, and Mark Cullen**, "Moral Hazard in Health Insurance: Do Dynamic Incentives Matter?," *Review of Economics and Statistics*, 2015, *97* (4), 725–741.

**_ , _ , and _** , "The RAND Health Insurance Experiment, Three Decades Later," *Journal of Economic Perspectives*, 2013, *27* (1), 197–222.

**Baicker, Katherine, Sarah L Taubman, Heidi L Allen, Mira Bernstein, Jonathan H Gruber, Joseph P Newhouse, Eric C Schneider, Bill J Wright, Alan M Zaslavsky, and Amy N Finkelstein**, "The Oregon experiment—effects of Medicaid on clinical outcomes," *New England Journal of Medicine*, 2013, *368* (18), 1713–1722.

**_ , Sendhil Mullainathan, and Joshua Schwartzstein**, "Behavioral Hazard in Health Insurance," *The Quarterly Journal of Economics*, 2015, *130* (4), 1623–1667.

**Brot-Goldberg, Zarek C, Amitabh Chandra, Benjamin R Handel, and Jonathan T Kolstad**, "What Does a Deductible Do? The Impact of Cost-Sharing on Health Care Prices, Quantities, and Spending Dynamics," *The Quarterly Journal of Economics*, 2017, *132* (3), 1261–1318.

**Cabral, Marika, Michael Geruso, and Neale Mahoney**, "Do Larger Health Insurance Subsidies Benefit Patients or Producers? Evidence from Medicare Advantage," *American Economic Review*, 2018, *108* (8), 2048–87.

**Calonico, Sebastian, Matias D. Cattaneo, and Rocio Titiunik**, "Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs," *Econometrica*, 2014, *82* (6), 2295–2326.

**_ , _ , Max H. Farrell, and Rocío Titiunik**, "Rdrobust: Software for Regression-discontinuity Designs," *The Stata Journal*, 2017, *17* (2), 372–404.

**Carey, Colleen**, "Sharing the burden of subsidization: Evidence on pass-through from a subsidy revision in Medicare Part D," *Journal of Public Economics*, 2021, *198*, 104401.

**Chandra, Amitabh, Evan Flack, and Ziad Obermeyer**, "The Health Costs of Cost Sharing," *The Quarterly Journal of Economics*, 05 2024, *139* (4), 2037–2082.

**_ , Jonathan Gruber, and Robin McKnight**, "Patient Cost-Sharing and Hospitalization Offsets in the Elderly," *American Economic Review*, 2010, *100* (1), 193–213.

**_ , _ , and _** , "Patient Cost Sharing in Low Income Populations," *American Economic Review*, 2010, *100* (2), 303–308.

**CMS**, "Costs for Medicare Drug Coverage," 2026.

**Cotterill, Philip G**, "An assessment of completeness and medical coding of Medicare Advantage hospitalizations in two national data sets," *Health Services Research*, 2023, *58* (6), 1303–1313.

**Currie, Janet and Firouz Gahvari**, "Transfers in Cash and In-Kind: Theory Meets the Data," *Journal of Economic Literature*, 2008, *46(2)*, 333–383.

**Dafny, Leemore S.**, "How Do Hospitals Respond to Price Changes?," *American Economic Review*, December 2005, *95* (5), 1525–1547.

**Decarolis, Francesco**, "Medicare Part D: Are Insurers Gaming the Low Income Subsidy Design?," *American Economic Review*, 2015, *105(4)*, 1547–1580.

_ , **Maria Polyakova, and Stephen P Ryan**, "Subsidy Design in Privately Provided Social Insurance: Lessons from Medicare Part D," *Journal of Political Economy*, 2020, *128* (5), 1712–1752.

**Deshpande, Manasi and Yue Li**, "Who Is Screened Out? Application Costs and the Targeting of Disability Programs," *American Economic Journal: Economic Policy*, November 2019, *11* (4), 213–48.

_ , **Greg Kaplan, and Tobias Leigh-Wood**, "How Do Disability Recipients Cope with the Loss of Benefits? The Role of Family Support, Public Programs, and Health Care," *NBER Working Paper No. w33745*, 2025.

**Duggan, Mark, Amanda Starc, and Boris Vabson**, "Who benefits when the government pays more? Pass-through in the Medicare Advantage program," *Journal of Public Economics*, 2016, *141*, 50–67.

**Einav, Liran, Amy Finkelstein, and Maria Polyakova**, "Private Provision of Social Insurance: Drug-Specific Price Elasticities and Cost Sharing in Medicare Part D," *American Economic Journal: Economic Policy*, 2018, *10* (3), 122–153.

_ , _ , **and Neale Mahoney**, "Provider Incentives and Healthcare Costs: Evidence From Long-Term Care Hospitals," *Econometrica*, 2018, *86* (6), 2161–2219.

_ , _ , **and Paul Schrimpf**, "The Response of Drug Expenditure to Nonlinear Contract Design: Evidence from Medicare Part D," *Quarterly Journal of Economics*, 2015, *130* (2), 841–899.

_ **and** _ , "Moral Hazard in Health Insurance: What We Know and How We Know It," *Journal of the European Economic Association*, 05 2018, *16* (4), 957–982.

**Ericson, Keith Marzilli, Timothy J Layton, Adrianna McIntyre, and Adam Sacarny**, "Reducing administrative barriers increases take-up of subsidized health insurance coverage: evidence from a field experiment," *Review of Economics and Statistics*, 2025, pp. 1–32.

**Finkelstein, Amy and Matthew J Notowidigdo**, "Take-Up and Targeting: Experimental Evidence from SNAP*," *The Quarterly Journal of Economics*, 05 2019, *134* (3), 1505–1556.

_ **and Nathaniel Hendren**, "Welfare Analysis Meets Causal Inference," *Journal of Economic Perspectives*, November 2020, *34* (4), 146–67.

_ , _ , **and Erzo F.P. Luttmer**, "The Value of Medicaid: Interpreting Results from the Oregon Health Insurance Experiment," *Journal of Political Economy*, 2019, *127* (6), 2836–2874.

_ , _ , **and Mark Shepard**, "Subsidizing Health Insurance for Low-Income Adults: Evidence from Massachusetts," *American Economic Review*, April 2019, *109* (4), 1530–67.

**Fung, Vicki, Mary Price, David Cheng, Tej A. Patel, Zhiyou Yang, John Hsu, Margarita Alegria, and Joseph P. Newhouse**, "Associations Between Annual Medicare Part D Low-Income Subsidy Loss and Prescription Drug Spending and Use," *JAMA Health Forum*, 2024, *5* (2), e235152–e235152.

**Gross, Tal, Timothy Layton, and Daniel Prinz**, "The Liquidity Sensitivity of Healthcare Consumption: Evidence from Social Security Payments," *American Economic Review: Insights*, 06 2022, *4*, 175–190.

**Hendren, Nathaniel and Ben Sprung-Keyser**, "A Unified Welfare Analysis of Government Policies," *The Quarterly Journal of Economics*, 2020, *135* (3), 1209–1318.

**Hollrah, Christopher**, "Prioritizing Benefit? How Low-Income Patients Respond to Prescription Cost-Sharing," *American Economic Journal: Economic Policy*, forthcoming.

**KFF**, "What Does the Federal Government Spend on Health Care?," 2025.

**MACPAC**, "Medicare Savings Programs: Enrollment Trends," *Medicaid and CHIP Payment and Access Commission*, June 2024.

**Manning, Willard G, Joseph P Newhouse, Naihua Duan, Emmett B Keeler, and Arleen Leibowitz**, "Health Insurance and the Demand for Medical Care: Evidence from a Randomized Experiment," *The American Economic Review*, 1987, pp. 251–277.

**Moffitt, Robert**, "Economics of means-tested transfer programs: Introduction," *National Bureau of Economic Research*, 2015.

**Newhouse, Joseph P.**, *Free for All: Lessons from the RAND Health Insurance Experiment*, Cambridge, MA: Harvard University Press, 1993.

**Roberts, Eric T., Jessica Phelan, Aaron L. Schwartz, Ellen Meara, Dominic Ruggiero, Lilly Estenson, Rachel M. Werner, and José F. Figueroa**, "Loss of Subsidized Drug Coverage and Mortality among Medicare Beneficiaries," *New England Journal of Medicine*, 2025, *392* (20), 2025–2034.

**Shigeoka, Hitoshi**, "The Effect of Patient Cost Sharing on Utilization, Health, and Risk Protection," *American Economic Review*, 2014, *104* (7), 2152–2184.

**Social Security Administration**, "Subsidy-Changing Event (SCE) and Other Event," https://secure.ssa.gov/poms.nsf/lnx/0603050025 2017. HI 03050.025. Accessed: March 24, 2026.

**SSA**, "Redetermination of Eligibility for Medicare Part D Extra Help (Low-Income Subsidy)," Program Operations Manual System (POMS), HI 03050.020 2017. Revised: September 13, 2017; accessed: March 18, 2026.

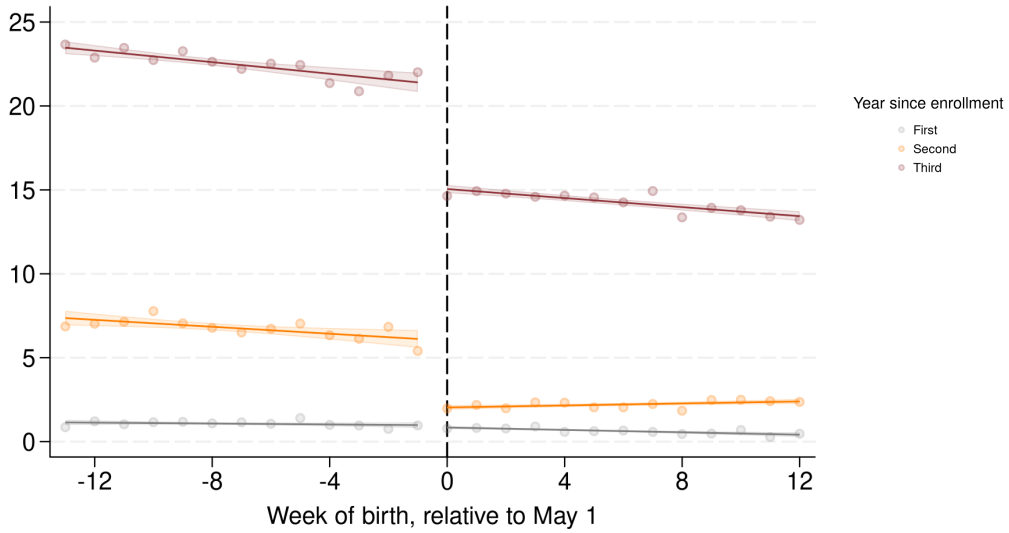
**SSA**, "Understanding the Extra Help with your Medicare Prescription Drug Plan," *Social Security Administration*, 2026.

**Tebaldi, Pietro**, "Estimating Equilibrium in Health Insurance Exchanges: Price Competition and Subsidy Design under the ACA," *University of Chicago Working Paper*, 2017.

# Figures and Tables

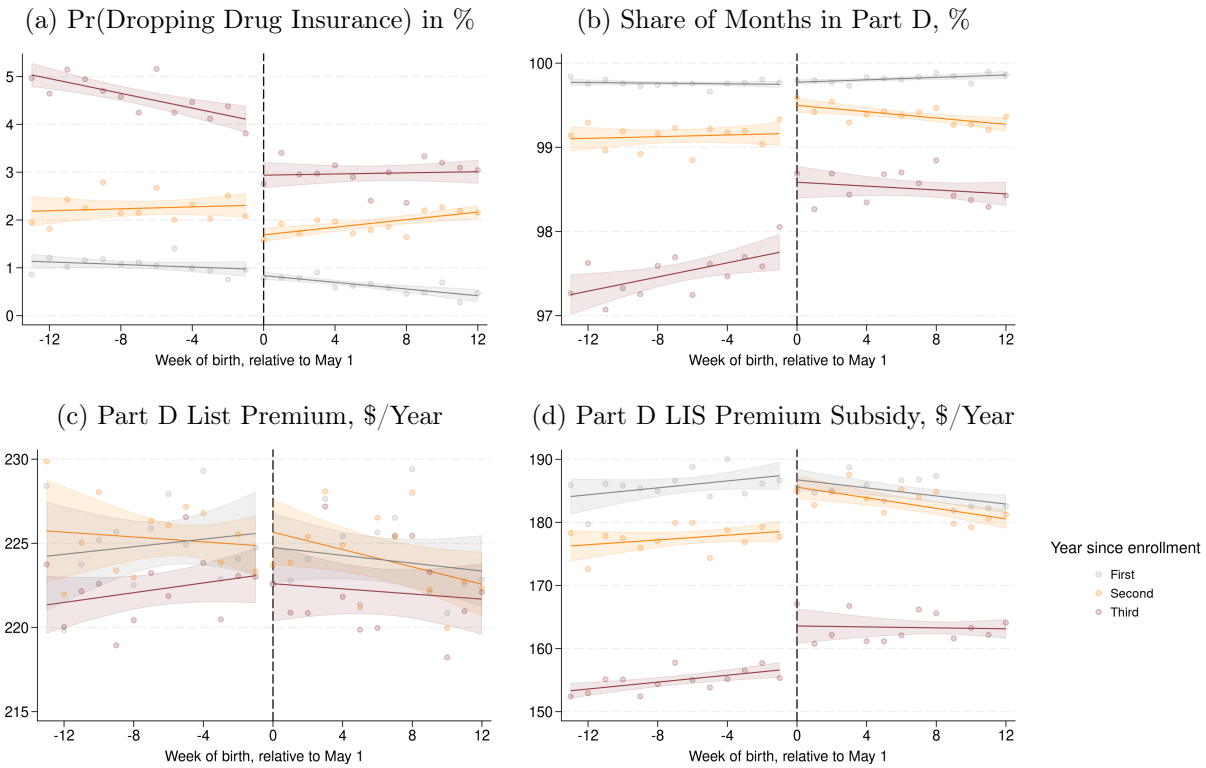
Figure 1: Rederminations and Low-Income Subsidy Loss

(a) Pr(Losing Subsidy) in %



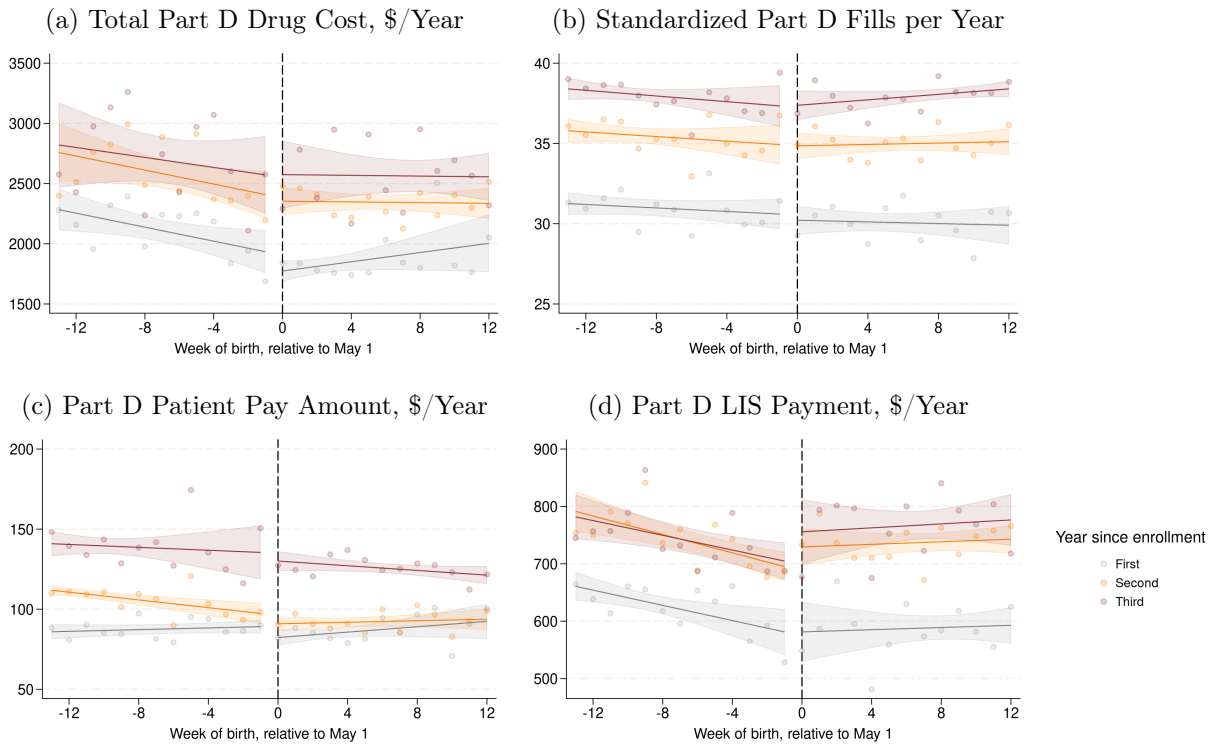

*Notes.* The figure illustrates the relationship between a beneficiary's week of birth (on the x-axis) and the probability of losing LIS prescription drug subsidy. Beneficiaries are aggregated into 7-day bins based on their day of birth within the year. We index the May 1–7 week-of-birth bin as zero. The outcomes are reported separately for the first (partial) year of LIS enrollment, second and third full years since LIS enrollment. The sample includes the universe of Medicare beneficiaries who enrolled in the low-income subsidy (LIS) during the Initial Enrollment Period upon turning 65 in 2010–2021, born within a 90-day window around May 1. See Section 2.3 for sample construction and 3.2 for outcome variable definitions. $N$ = 128,600 beneficiaries are observed in the year of enrollment and in the second year since enrollment. Of these, $N$ = 120,436 beneficiaries are also observed in the third year since enrollment after cohorts enrolling in 2021 are excluded as our data are only available up until 2022. In years 2–3, the outcomes are tracked for the full year for all beneficiaries. In year 1, the outcomes are tracked only from the month of LIS enrollment onward. Lines of linear best fit and 95% confidence intervals are included, weighted by the population in each week of birth without controls. Tables 2 and 3 present the corresponding regression discontinuity estimates from Equation 1.

Figure 2: Insurance Take-Up and Premiums



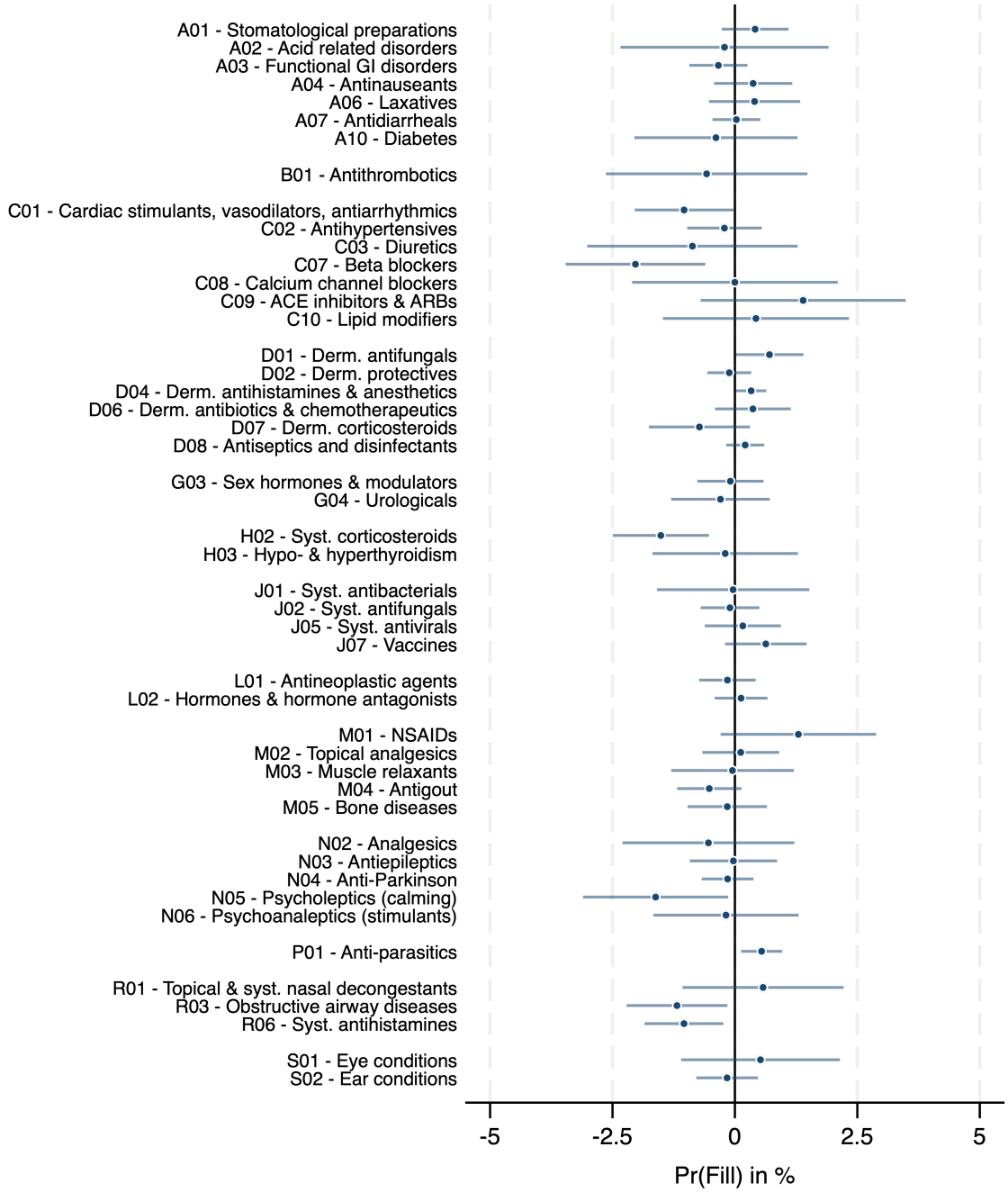

*Notes.* The figure illustrates the relationship between a beneficiary's week of birth (x-axis) and measures of Medicare Part D insurance discontinuation (Panels a–b), the Part D plan's list premium, net of rebates, which is used as a proxy for plan generosity (Panel c), and the value LIS pays towards the Part D plan premium (Panel d). Beneficiaries are aggregated into 7-day bins based on their day of birth within the year. We index the May 1–7 week-of-birth bin as zero. The outcomes are reported separately for the first (partial) year of LIS enrollment, second and third full years since LIS enrollment. Panels (a)–(b) use MBSF data, available for the full baseline sample of LIS enrollees. Panels (c)–(d) use Plan Characteristics data, also available for the full baseline sample of LIS enrollees. See Section 2.3 for sample construction and 3.2 for outcome variable definitions. All panels include all $N = 128{,}600$ beneficiaries in the baseline sample ($N = 120{,}436$ in year 3 that excludes beneficiaries enrolled in 2021). Lines of linear best fit and 95% confidence intervals are included, weighted by the population in each week of birth without controls. Tables 2 and 3 present the corresponding regression discontinuity estimates from Equation 1.

Figure 3: Prescription Drug Utilization



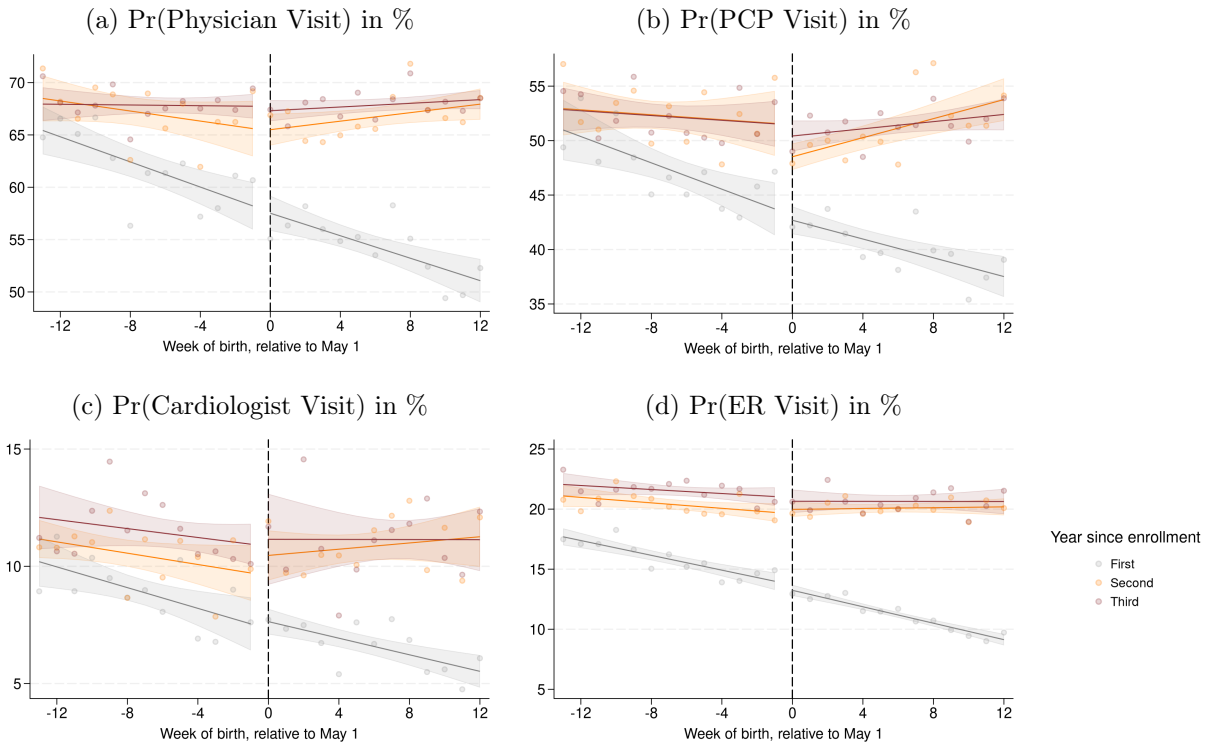

*Notes.* The figure illustrates the relationship between a beneficiary's week of birth (x-axis) and measures of Part D drug utilization (Panels a–b) and cost allocation by payer (Panels c–d). Beneficiaries are aggregated into 7-day bins based on their day of birth within the year. We index the May 1–7 week-of-birth bin as zero. The outcomes are reported separately for the first (partial) year of LIS enrollment, second and third full years since LIS enrollment. All panels use Part D Event data, available for the random 20% subsample of the baseline sample of LIS enrollees. See Section 2.3 for sample construction and 3.2 for outcome variable definitions. All panels include $N$ = 26,560 beneficiaries in the 20% subsample for which Part D files are available ($N$ = 24,845 in year 3 that excludes beneficiaries enrolled in 2021). Lines of linear best fit and 95% confidence intervals are included, weighted by the population in each week of birth without controls. Tables 2 and 3 present the corresponding regression discontinuity estimates from Equation 1.

Figure 4: Drug Utilization by Therapeutic Class (ATC-2)



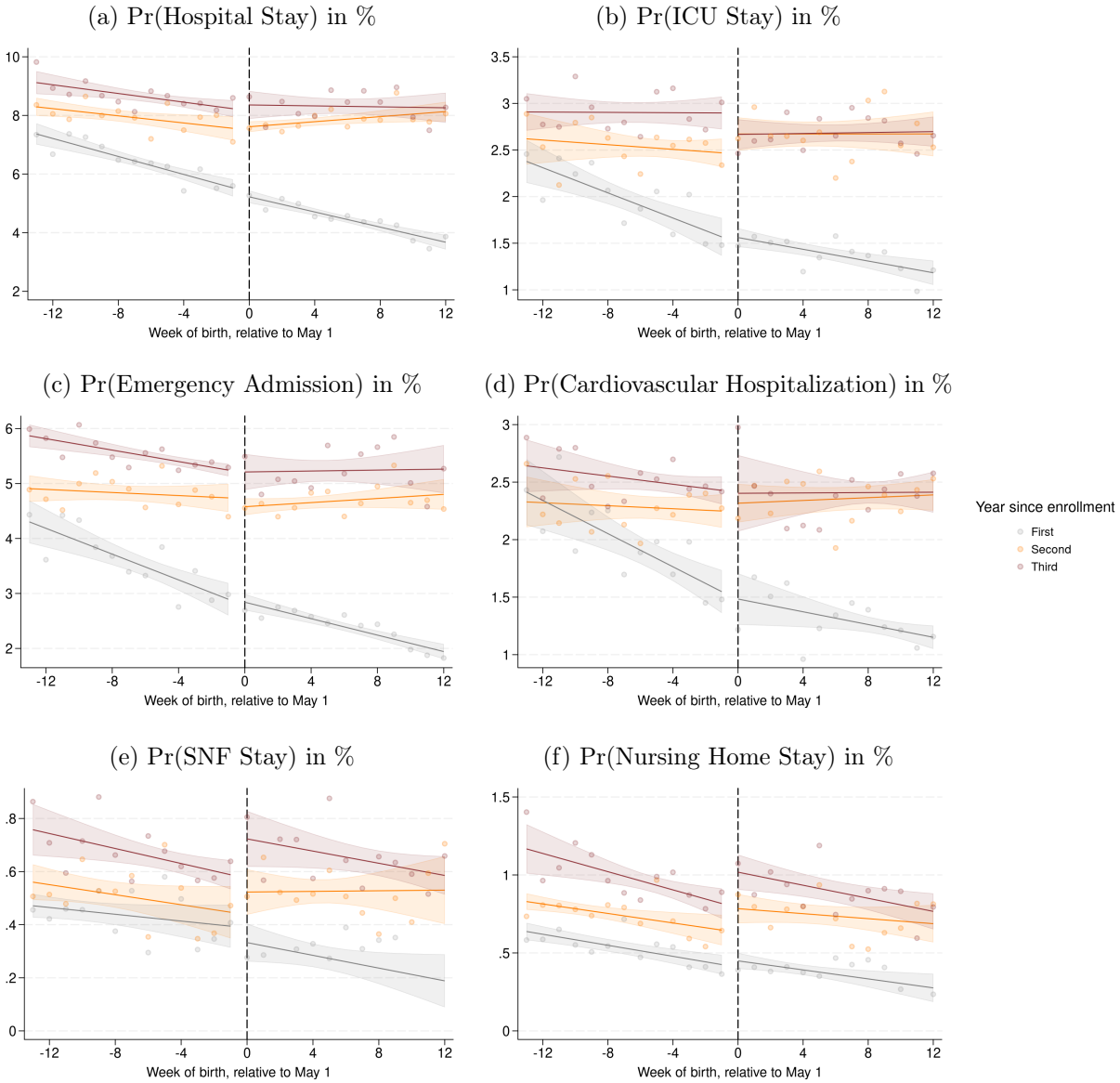

*Notes.* The figure illustrates regression discontinuity estimates from Equation 1 for year 2 since LIS enrollment. Separate regressions are estimated for each outcome. Outcomes are indexed on the y-axis and measure the probability of filling a prescription in most common therapeutic classes at the ATC-2 code level. The figure includes ATC-2 classes filled by at least 500 beneficiaries in the estimation sample in year 2 since LIS enrollment. All specifications use Part D Event data, available for the random 20% subsample of the baseline sample of LIS enrollees. See Section 2.3 for sample construction and 3.2 for outcome variable definitions. $N$ = 26,777 beneficiaries.

Figure 5: Outpatient Healthcare Utilization




*Notes.* The figure illustrates the relationship between a beneficiary's week of birth (x-axis) and measures of outpatient healthcare utilization. Beneficiaries are aggregated into 7-day bins based on their day of birth within the year. We index the May 1–7 week-of-birth bin as zero. The outcomes are reported separately for the first (partial) year of LIS enrollment, second and third full years since LIS enrollment. Data for panels (a)–(c) are based on 20% Fee-for-Service Carrier files. Data for Panel (d) come from 100% MedPAR and Outpatient files and is restricted to Fee-For-Service beneficiaries. In year 1, we only observe utilization after LIS enrollment, which induces mechanical negative trends by week of birth. See Section 2.3 for sample construction and 3.2 for outcome variable definitions. Panels (a)–(c) include $N = 14{,}316$ beneficiaries in year 1, 12,940 beneficiaries in year 2, and 11,514 beneficiaries in year 3. Panel (d) includes $N = 69{,}706$ beneficiaries in year 1, 63,000 beneficiaries in year 2, and 56,585 beneficiaries in year 3. Lines of linear best fit and 95% confidence intervals are included, weighted by the population in each week of birth without controls. Tables 2 and 3 present the corresponding regression discontinuity estimates from Equation 1.

Figure 6: Inpatient Healthcare Utilization




*Notes.* The figure illustrates the relationship between a beneficiary's week of birth (x-axis) and several measures of inpatient healthcare utilization. Beneficiaries are aggregated into 7-day bins based on their day of birth within the year. We index the May 1–7 week-of-birth bin as zero. The outcomes are reported separately for the first (partial) year of LIS enrollment, second and third full years since LIS enrollment. Data on inpatient hospital stays in Panels (a)–(d) and SNF stays in Panel (e) are based on Medicare Provider Analysis and Review (MedPAR) files, available for the full baseline sample. Data on long-term care facility utilization in Panel (f) come from the Long Term Care Minimum Data Set 3.0. In year 1, we only observe utilization after LIS enrollment, which induces mechanical negative trends by week of birth. See Section 2.3 for sample construction and 3.2 for outcome variable definitions. All panels include all $N = 128{,}600$ beneficiaries in the baseline sample ($N = 120{,}436$ in year 3 that excludes beneficiaries enrolled in 2021). Lines of linear best fit and 95% confidence intervals are included, weighted by the population in each week of birth without controls. Tables 2 and 3 present the corresponding regression discontinuity estimates from Equation 1.

Table 1: Summary Statistics

| | Born Before May 1 Mean Value (1) | Born After May 1 Mean Value (2) |
|---|---|---|
| Age at LIS enrollment | 65.1 | 65.1 |
| Female | 0.60 | 0.60 |
| Race / Ethnicity | | |
| Hispanic | 0.19 | 0.21 |
| White non-Hispanic | 0.54 | 0.54 |
| Black | 0.18 | 0.17 |
| Asian / Pacific Islander | 0.06 | 0.06 |
| American Indian / Alaska Native | 0.00 | 0.00 |
| In MA at enrollment | 0.42 | 0.43 |
| Number of Beneficiaries | 63,480 | 65,120 |

*Notes.* This table provides summary statistics for the baseline sample of the universe of Medicare beneficiaries who enrolled in the low-income subsidy (LIS) during the Initial Enrollment Period upon turning 65 in 2010–2021. Only beneficiaries born within a 90-day window around May 1 of any year, and not on the first day of a month, are included in the sample. The full sample size is 128,600 beneficiaries. See Section 2.3 for sample construction. Beneficiary characteristics are constructed from Medicare Beneficiary Summary Files (MBSF). All statistics are computed at the beneficiary level at the time of LIS enrollment.

Table 2: Regression Discontinuity Estimates, Year 2 Since LIS Enrollment

| | Control mean (1) | Difference in means (2) | RD coef. (3) | RD p-value (4) | IV coef. (5) | $N$ (6) |
|---|---|---|---|---|---|---|
| **Panel A. Loss of drug subsidy** | | | | | | |
| Pr(Losing Subsidy) in % | 2.2 | 4.5 | 4.0 | 0.00 | | 128600 |
| | | | (0.3) | | | |
| **Panel B. Drug Insurance Demand** | | | | | | |
| Pr(Dropping Drug Insurance) in % | 1.9 | 0.3 | 0.6 | 0.00 | | 128600 |
| | | | (0.2) | | | |
| Share of Months in Part D, % | 99.4 | –0.3 | –0.3 | 0.00 | | 128600 |
| | | | (0.1) | | | |
| Part D List Premium, $/Year | 224.1 | 1.2 | –0.9 | 0.56 | –21.6 | 128600 |
| | | | (1.5) | | (36.7) | |
| Part D LIS Premium Subsidy, $/Year | 183.0 | –5.6 | –6.9 | 0.00 | –172.2 | 128600 |
| | | | (1.4) | | (38.4) | |
| **Panel C. Prescription Drug Utilization** | | | | | | |
| Total Part D Drug Cost, $/Year | 2344.2 | 240.9 | 25.7 | 0.84 | 565.2 | 26560 |
| | | | (127.7) | | (2741.7) | |
| Standardized Part D Fills per Year | 35.0 | 0.4 | 0.0 | 0.99 | 0.2 | 26560 |
| | | | (0.8) | | (17.6) | |
| Part D Patient Pay Amount, $/Year | 92.4 | 12.3 | 5.2 | 0.23 | 114.9 | 26560 |
| | | | (4.2) | | (92.0) | |
| Part D LIS Payment, $/Year | 736.5 | 7.3 | –42.4 | 0.07 | –933.3 | 26560 |
| | | | (22.1) | | (500.6) | |
| **Panel D. Inpatient and Outpatient Healthcare Utilization** | | | | | | |
| Pr(Hospital Stay) in % | 7.9 | 0.0 | –0.1 | 0.65 | –2.9 | 128600 |
| | | | (0.3) | | (6.3) | |
| Pr(ICU Stay) in % | 2.7 | –0.1 | –0.2 | 0.11 | –5.4 | 128600 |
| | | | (0.1) | | (3.3) | |
| Pr(Emergency Admission) in % | 4.7 | 0.1 | 0.1 | 0.38 | 3.7 | 128600 |
| | | | (0.2) | | (3.9) | |
| Pr(Cardiovascular Hospitalization) in % | 2.4 | –0.1 | –0.1 | 0.54 | –1.9 | 128600 |
| | | | (0.1) | | (3.0) | |
| Pr(SNF stay) in % | 0.5 | –0.0 | –0.1 | 0.23 | –2.2 | 128600 |
| | | | (0.1) | | (1.7) | |
| Pr(Nursing Home Stay) in % | 0.7 | 0.0 | –0.2 | 0.05 | –3.8 | 128600 |
| | | | (0.1) | | (1.8) | |
| Pr(Physician Visit) in % | 66.8 | 0.3 | –0.1 | 0.93 | –3.2 | 12940 |
| | | | (1.7) | | (37.9) | |
| Pr(PCP Visit) in % | 51.3 | 1.0 | 2.9 | 0.12 | 67.9 | 12940 |
| | | | (1.8) | | (45.2) | |
| Pr(Cardiologist Visit) in % | 10.9 | –0.4 | –0.9 | 0.32 | –20.0 | 12940 |
| | | | (0.9) | | (20.8) | |
| Pr(ER Visit) in % | 20.1 | 0.3 | –0.4 | 0.50 | –9.2 | 63000 |
| | | | (0.5) | | (13.4) | |
| **Panel E. Inpatient, Outpatient, and Carrier Spending** | | | | | | |
| Inpatient, Outpatient, and Carrier Total Spending, $/Year | 4699.8 | 310.2 | –279.6 | 0.54 | –6438.4 | 12940 |
| | | | (454.3) | | (10252.9) | |
| Inpatient, Outpatient, and Carrier OOP Spending, $/Year | 816.9 | 48.0 | –57.6 | 0.53 | –1326.0 | 12940 |
| | | | (89.9) | | (2016.4) | |
| Inpatient, Outpatient, and Carrier Medicare Spending, $/Year | 3828.5 | 260.8 | –141.6 | 0.71 | –3262.4 | 12940 |
| | | | (378.6) | | (8532.7) | |

*Notes.* This table reports the estimates of $\beta_1$ and $\tau^{\text{LATE}}$, the coefficients of interest from Equations 1 and 3, for all outcomes. The table is restricted to outcomes measured in the second year since LIS enrollment. Each row of the table is a separate regression with outcomes defined in row titles. Column (1) reports the mean of the outcome among beneficiaries born in the 90 days after May 1 ("control" beneficiaries). Column (2) reports the raw differences in means between beneficiaries born in the 90 days before May 1 and those born in the 90 days after May 1. Column (3) reports the point estimate and clustered standard error for $\beta_1$ from Equation 1. Column (4) reports the p-value for the estimate in (3). Column (5) reports the point estimate and clustered standard error for $\beta_1$ from equation 3. Column (6) reports the sample size.

Table 3: Regression Discontinuity Estimates, Year 3 Since LIS Enrollment

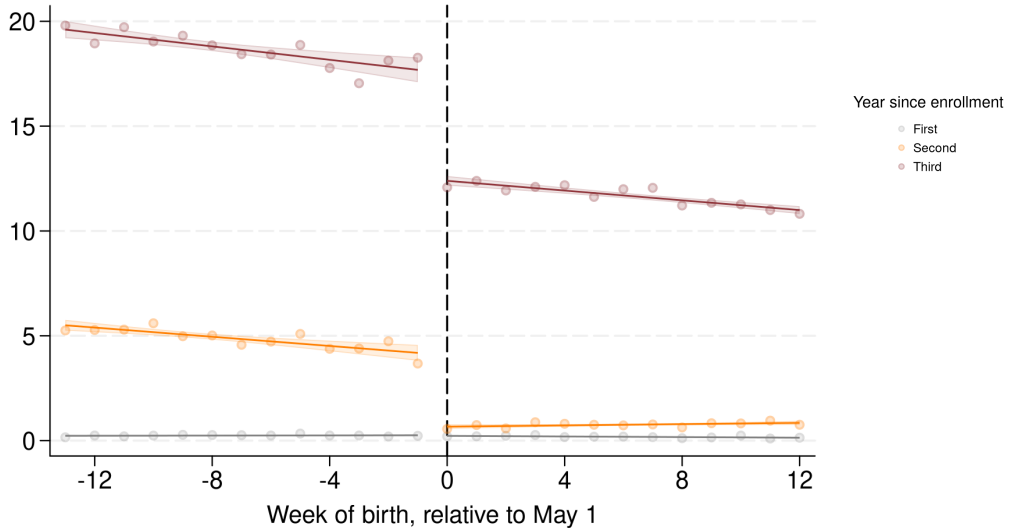
| | Control mean (1) | Difference in means (2) | RD coef. (3) | RD p-value (4) | IV coef. (5) | $N$ (6) |
|---|---|---|---|---|---|---|
| **Panel A. Loss of drug subsidy** | | | | | | |
| Pr(Losing Subsidy) in % | 14.2 | 8.2 | 6.2 | 0.00 | | 120436 |
| | | | (0.4) | | | |
| **Panel B. Drug Insurance Demand** | | | | | | |
| Pr(Dropping Drug Insurance) in % | 3.0 | 1.6 | 1.1 | 0.00 | | 120436 |
| | | | (0.2) | | | |
| Share of Months in Part D, % | 98.5 | –1.0 | –0.8 | 0.00 | | 120436 |
| | | | (0.2) | | | |
| Part D List Premium, $/Year | 222.1 | 0.1 | 0.6 | 0.67 | 10.1 | 120436 |
| | | | (1.5) | | (23.1) | |
| Part D LIS Premium Subsidy, $/Year | 163.3 | –8.4 | –6.7 | 0.00 | –108.6 | 120436 |
| | | | (1.6) | | (23.5) | |
| **Panel C. Prescription Drug Utilization** | | | | | | |
| Total Part D Drug Cost, $/Year | 2564.9 | 132.4 | –23.1 | 0.92 | –376.6 | 24845 |
| | | | (233.0) | | (3745.7) | |
| Standardized Part D Fills per Year | 37.9 | –0.0 | –0.1 | 0.88 | –2.1 | 24845 |
| | | | (0.9) | | (13.7) | |
| Part D Patient Pay Amount, $/Year | 125.5 | 12.7 | 4.9 | 0.63 | 80.6 | 24845 |
| | | | (10.0) | | (161.2) | |
| Part D LIS Payment, $/Year | 766.5 | –23.0 | –57.4 | 0.10 | –937.7 | 24845 |
| | | | (34.0) | | (586.1) | |
| **Panel D. Inpatient and Outpatient Healthcare Utilization** | | | | | | |
| Pr(Hospital Stay) in % | 8.3 | 0.4 | –0.2 | 0.50 | –3.3 | 120436 |
| | | | (0.3) | | (4.7) | |
| Pr(ICU Stay) in % | 2.7 | 0.2 | 0.2 | 0.09 | 3.7 | 120436 |
| | | | (0.1) | | (2.0) | |
| Pr(Emergency Admission) in % | 5.2 | 0.3 | –0.0 | 0.92 | –0.3 | 120436 |
| | | | (0.2) | | (2.8) | |
| Pr(Cardiovascular Hospitalization) in % | 2.4 | 0.1 | 0.0 | 0.97 | 0.1 | 120436 |
| | | | (0.2) | | (2.9) | |
| Pr(SNF stay) in % | 0.7 | 0.0 | –0.1 | 0.02 | –2.4 | 120436 |
| | | | (0.1) | | (1.0) | |
| Pr(Nursing Home Stay) in % | 0.9 | 0.1 | –0.2 | 0.01 | –3.7 | 120436 |
| | | | (0.1) | | (1.3) | |
| Pr(Physician Visit) in % | 67.9 | –0.0 | 0.4 | 0.63 | 7.2 | 11514 |
| | | | (0.8) | | (14.6) | |
| Pr(PCP Visit) in % | 51.5 | 0.7 | 1.0 | 0.48 | 17.6 | 11514 |
| | | | (1.4) | | (25.7) | |
| Pr(Cardiologist Visit) in % | 11.1 | 0.4 | –0.3 | 0.79 | –5.3 | 11514 |
| | | | (1.1) | | (19.1) | |
| Pr(ER Visit) in % | 20.6 | 0.9 | 0.3 | 0.65 | 5.6 | 56585 |
| | | | (0.7) | | (11.7) | |
| **Panel E. Inpatient, Outpatient, and Carrier Spending** | | | | | | |
| Inpatient, Outpatient, and Carrier Total Spending, $/Year | 5335.3 | 457.5 | –892.4 | 0.22 | –15651.7 | 11514 |
| | | | (708.2) | | (13413.6) | |
| Inpatient, Outpatient, and Carrier OOP Spending, $/Year | 900.5 | 69.4 | –136.9 | 0.25 | –2401.1 | 11514 |
| | | | (116.0) | | (2239.4) | |
| Inpatient, Outpatient, and Carrier Medicare Spending, $/Year | 4379.7 | 338.0 | –802.2 | 0.23 | –14069.5 | 11514 |
| | | | (647.7) | | (12197.9) | |

*Notes.* This table reports the estimates of $\beta_1$ and $\tau^{\text{LATE}}$, the coefficients of interest from Equations 1 and 3, for all outcomes. The table is restricted to outcomes measured in the third year since LIS enrollment. Each row of the table is a separate regression with outcomes defined in row titles. Column (1) reports the mean of the outcome among beneficiaries born in the 90 days after May 1 ("control" beneficiaries). Column (2) reports the raw differences in means between beneficiaries born in the 90 days before May 1 and those born in the 90 days after May 1. Column (3) reports the point estimate and clustered standard error for $\beta_1$ from Equation 1. Column (4) reports the p-value for the estimate in (3). Column (5) reports the point estimate and clustered standard error for $\beta_1$ from equation 3. Column (6) reports the sample size.

# Appendix

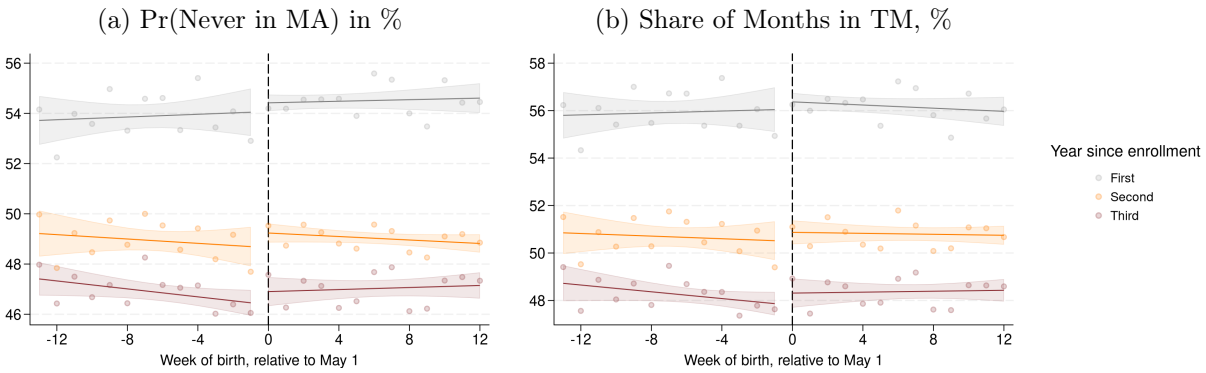
Figure A1: Low-Income Subsidy Loss

(a) Beneficiary-Months with No Subsidy, %



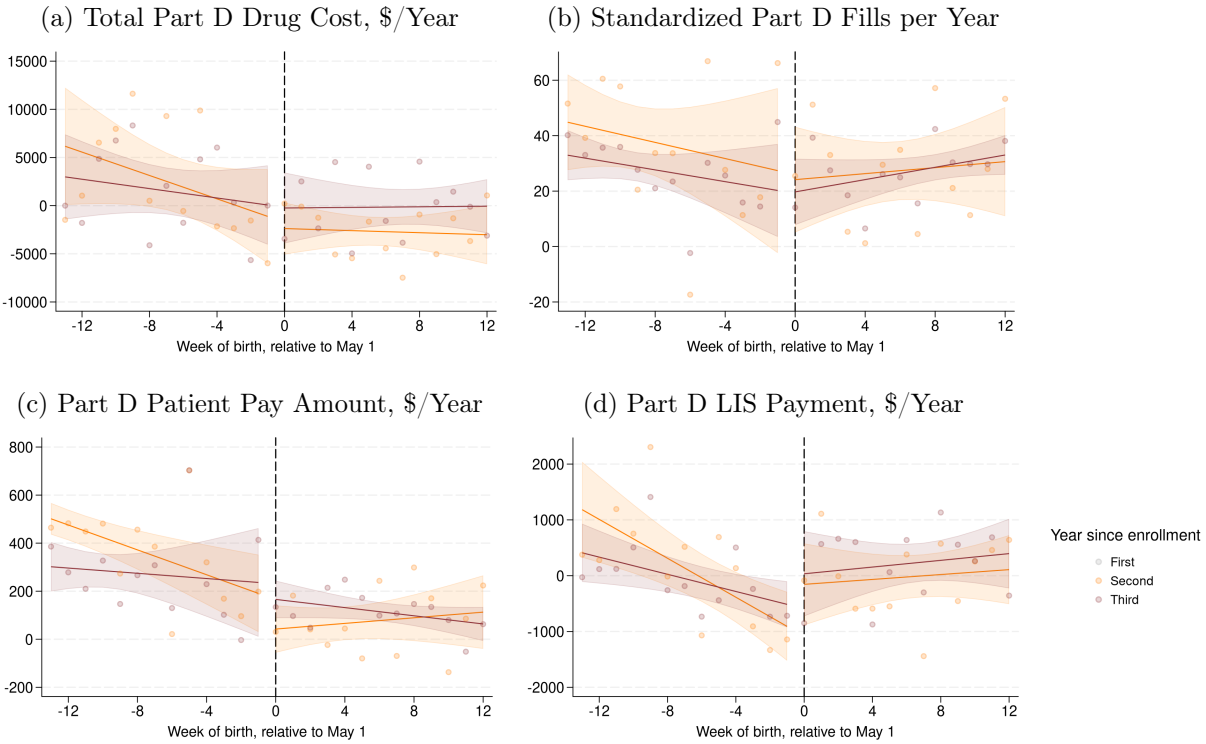

*Notes.* The figure illustrates the relationship between a beneficiary's week of birth (on the x-axis) and the share of months in a calendar year without a subsidy. Beneficiaries are aggregated into 7-day bins based on their day of birth within the year. We index the May 1–7 week-of-birth bin as zero. The outcomes are reported separately for the first (partial) year of LIS enrollment, second and third full years since LIS enrollment. The sample includes the universe of Medicare beneficiaries who enrolled in the low-income subsidy (LIS) during the Initial Enrollment Period upon turning 65 in 2010–2021, born within a 90-day window around May 1. See Section 2.3 for sample construction and 3.2 for outcome variable definitions. $N$ = 128,600 beneficiaries are observed in the year of enrollment and in the second year since enrollment. Of these, $N$ = 120,436 beneficiaries are also observed in the third year since enrollment after cohorts enrolling in 2020 are excluded. In years 2–3, outcomes are tracked for the full year for all beneficiaries. In year 1, outcomes are tracked only from the month of LIS enrollment onward. We first compute the share of months between LIS enrollment and December at the beneficiary level and then aggregate these shares with equal weights. Lines of linear best fit and 95% confidence intervals are included, weighted by the population in each week of birth without controls. Tables 2 and 3 present the corresponding regression discontinuity estimates from Equation 1.

Figure A2: Traditional Medicare Enrollment



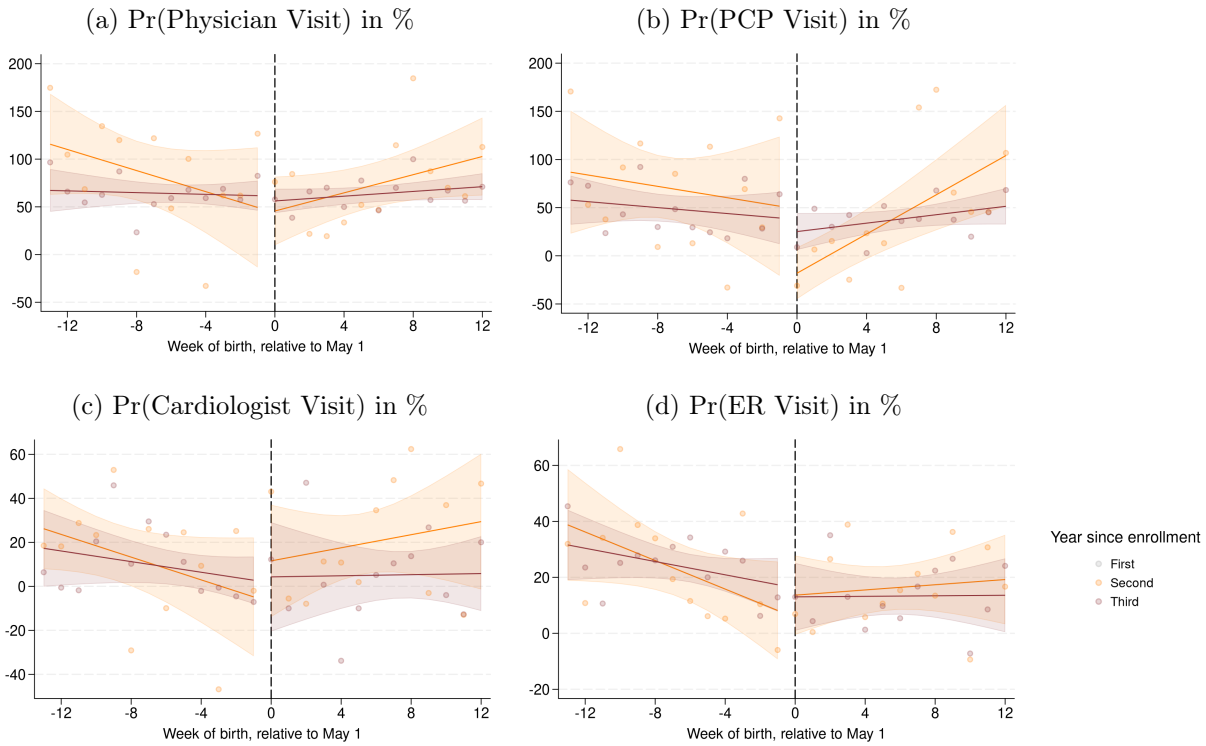

*Notes.* The figure illustrates the relationship between a beneficiary's week of birth (x-axis) and Traditional Medicare enrollment. Beneficiaries are aggregated into 7-day bins based on their day of birth within the year. We index the May 1–7 week-of-birth bin as zero. The outcomes are reported separately for the first (partial) year of LIS enrollment, second and third full years since LIS enrollment. Data are based on 100% MBSF files. See Section 2.3 for sample construction and 3.2 for outcome variable definitions. Lines of linear best fit and 95% confidence intervals are included, weighted by the population in each week of birth without controls. Tables 2 and 3 present the corresponding regression discontinuity estimates from Equation 1.

Figure A3: Prescription Drug Utilization Among Compliers



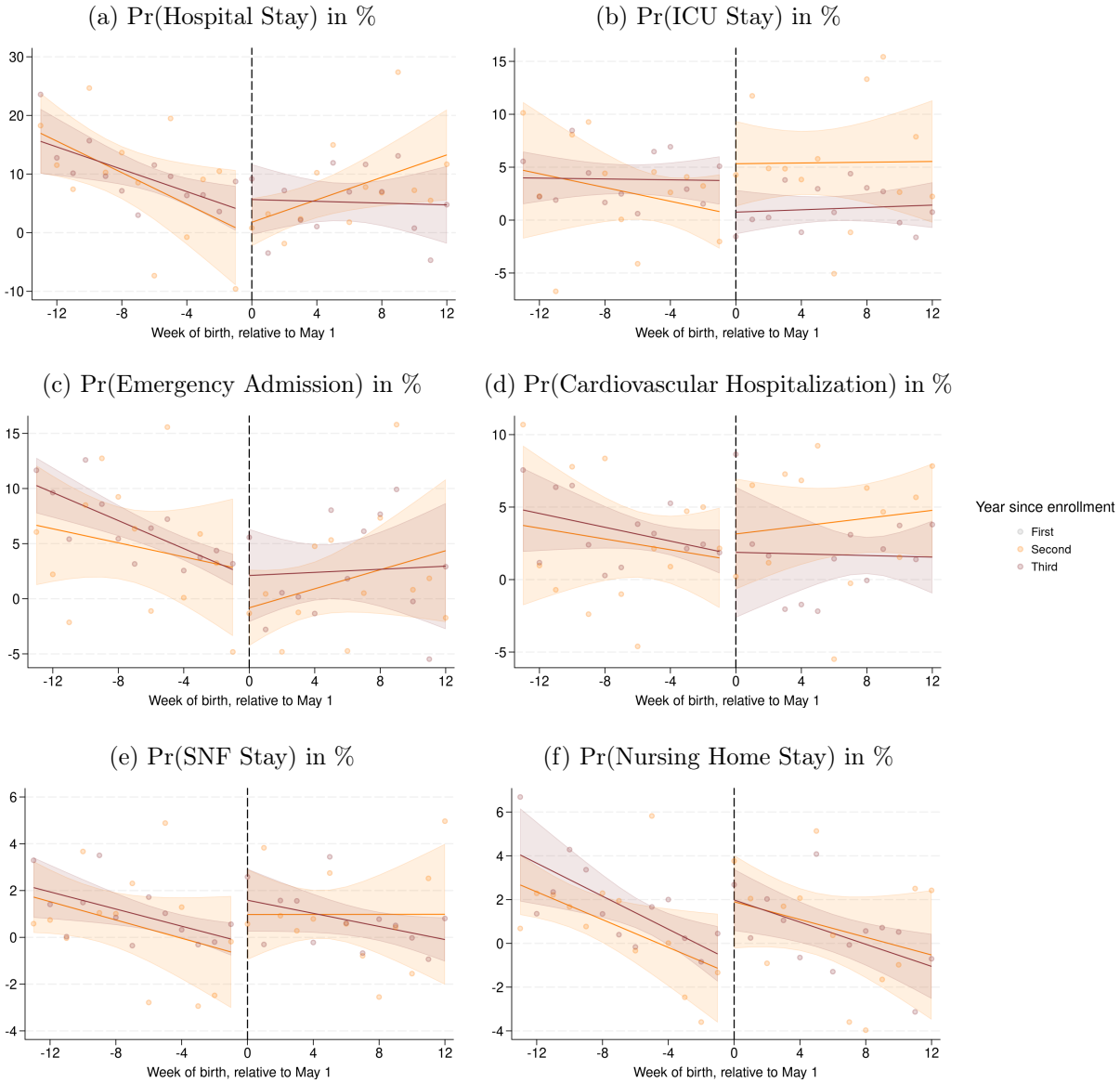

*Notes.* The figure illustrates the relationship between a beneficiary's week of birth (x-axis) and measures of Part D drug utilization (Panels a–b) and cost allocation by payer (Panels c–d) among *compliers* — beneficiaries who lose LIS if born before May 1 but retain it if born after. Beneficiaries are aggregated into 7-day bins based on their day of birth within the year. We index the May 1–7 week-of-birth bin as zero. Because compliers cannot be identified individually, we recover their mean outcome in each bin by subtracting the share-weighted contributions of always-takers and never-takers from the observed bin mean and dividing by the complier share. Always-takers are beneficiaries born after May 1 who lose the subsidy despite the later review schedule, and never-takers are those born before May 1 who retain it despite the earlier one; we estimate their shares and means by pooling across the estimation window on each side of the cutoff, separately for each year. The outcomes are reported separately for the second and third full years since LIS enrollment. All panels use Part D Event data, available for the random 20% subsample of the baseline sample of LIS enrollees. See Section 2.3 for sample construction and 3.2 for outcome variable definitions. Lines of linear best fit and 95% confidence intervals are included, weighted by the population in each week of birth without controls. The discontinuity is estimated using the specification in Equation 1 with the complier mean as the outcome.

Figure A4: Outpatient Healthcare Utilization Among Compliers




*Notes.* The figure illustrates the relationship between a beneficiary's week of birth (x-axis) and measures of outpatient healthcare utilization among *compliers* — beneficiaries who lose LIS if born before May 1 but retain it if born after. Beneficiaries are aggregated into 7-day bins based on their day of birth within the year. We index the May 1–7 week-of-birth bin as zero. Because compliers cannot be identified individually, we recover their mean outcome in each bin by subtracting the share-weighted contributions of always-takers and never-takers from the observed bin mean and dividing by the complier share. Always-takers are beneficiaries born after May 1 who lose the subsidy despite the later review schedule, and never-takers are those born before May 1 who retain it despite the earlier one; we estimate their shares and means by pooling across the estimation window on each side of the cutoff, separately for each year. The outcomes are reported separately for the second and third full years since LIS enrollment. Data for panels (a)–(c) are based on 20% Fee-for-Service Carrier files. Data for Panel (d) come from 100% MedPAR and Outpatient files and is restricted to Fee-For-Service beneficiaries. See Section 2.3 for sample construction and 3.2 for outcome variable definitions. Lines of linear best fit and 95% confidence intervals are included, weighted by the population in each week of birth without controls. The discontinuity is estimated using the specification in Equation 1 with the complier mean as the outcome.

Figure A5: Inpatient Healthcare Utilization Among Compliers




*Notes.* The figure illustrates the relationship between a beneficiary's week of birth (x-axis) and several measures of inpatient healthcare utilization among *compliers* — beneficiaries who lose LIS if born before May 1 but retain it if born after. Beneficiaries are aggregated into 7-day bins based on their day of birth within the year. We index the May 1–7 week-of-birth bin as zero. Because compliers cannot be identified individually, we recover their mean outcome in each bin by subtracting the share-weighted contributions of always-takers and never-takers from the observed bin mean and dividing by the complier share. Always-takers are beneficiaries born after May 1 who lose the subsidy despite the later review schedule, and never-takers are those born before May 1 who retain it despite the earlier one; we estimate their shares and means by pooling across the estimation window on each side of the cutoff, separately for each year. The outcomes are reported separately for the second and third full years since LIS enrollment. Data on inpatient hospital stays in Panels (a)–(d) and SNF stays in Panel (e) are based on 100% MedPAR files. Data on long-term care facility utilization in Panel (f) come from the 100% Long Term Care Minimum Data Set 3.0. See Section 2.3 for sample construction and 3.2 for outcome variable definitions. All panels include all $N = 128{,}600$ beneficiaries in the baseline sample ($N = 120{,}436$ in year 3 that excludes beneficiaries enrolled in 2021). Lines of linear best fit and 95% confidence intervals are included, weighted by the population in each week of birth without controls. The discontinuity is estimated using the specification in Equation 1 with the complier mean as the outcome.

Table A1: RD Robustness, Year 2 Since LIS Enrollment

| | Control mean (1) | RD coef. (2) | RD p-value (3) | RDRobust coef. (4) | RDRobust p-value (5) | $N$ (6) |
|---|---|---|---|---|---|---|
| **Panel A. Loss of drug subsidy** | | | | | | |
| Pr(Losing Subsidy) in % | 2.2 | 4.0 | 0.00 | 3.6 | 0.00 | 128600 |
| | | (0.3) | | (0.4) | | |
| **Panel B. Drug Insurance Demand** | | | | | | |
| Pr(Dropping Drug Insurance) in % | 1.9 | 0.6 | 0.00 | 0.3 | 0.23 | 128600 |
| | | (0.2) | | (0.3) | | |
| Share of Months in Part D, % | 99.4 | –0.3 | 0.00 | –0.3 | 0.02 | 128600 |
| | | (0.1) | | (0.1) | | |
| Part D List Premium, $/Year | 224.1 | –0.9 | 0.56 | –0.3 | 0.94 | 128600 |
| | | (1.5) | | (3.6) | | |
| Part D LIS Premium Subsidy, $/Year | 183.0 | –6.9 | 0.00 | –6.9 | 0.02 | 128600 |
| | | (1.4) | | (3.0) | | |
| **Panel C. Prescription Drug Utilization** | | | | | | |
| Total Part D Drug Cost, $/Year | 2344.2 | 25.7 | 0.84 | –360.4 | 0.31 | 26560 |
| | | (127.7) | | (356.7) | | |
| Standardized Part D Fills per Year | 35.0 | 0.0 | 0.99 | 0.7 | 0.64 | 26560 |
| | | (0.8) | | (1.5) | | |
| Part D Patient Pay Amount, $/Year | 92.4 | 5.2 | 0.23 | 2.9 | 0.69 | 26560 |
| | | (4.2) | | (7.3) | | |
| Part D LIS Payment, $/Year | 736.5 | –42.4 | 0.07 | –79.3 | 0.21 | 26560 |
| | | (22.1) | | (62.8) | | |
| **Panel D. Inpatient and Outpatient Healthcare Utilization** | | | | | | |
| Pr(Hospital Stay) in % | 7.9 | –0.1 | 0.65 | 0.0 | 0.97 | 128600 |
| | | (0.3) | (0.5) | | | |
| Pr(ICU Stay) in % | 2.7 | –0.2 | 0.11 | –0.4 | 0.23 | 128600 |
| | | (0.1) | (0.3) | | | |
| Pr(Emergency Admission) in % | 4.7 | 0.1 | 0.38 | –0.0 | 0.91 | 128600 |
| | | (0.2) | (0.4) | | | |
| Pr(Cardiovascular Hospitalization) in % | 2.4 | –0.1 | 0.54 | 0.2 | 0.49 | 128600 |
| | | (0.1) | (0.3) | | | |
| Pr(SNF stay) in % | 0.5 | –0.1 | 0.23 | –0.2 | 0.20 | 128600 |
| | | (0.1) | (0.1) | | | |
| Pr(Nursing Home Stay) in % | 0.7 | –0.2 | 0.05 | –0.3 | 0.09 | 128600 |
| | | (0.1) | (0.2) | | | |
| Pr(Physician Visit) in % | 66.8 | –0.1 | 0.93 | 2.6 | 0.34 | 12940 |
| | | (1.7) | (2.7) | | | |
| Pr(PCP Visit) in % | 51.3 | 2.9 | 0.12 | 6.7 | 0.02 | 12940 |
| | | (1.8) | (2.9) | | | |
| Pr(Cardiologist Visit) in % | 10.9 | –0.9 | 0.32 | –1.1 | 0.54 | 12940 |
| | | (0.9) | (1.8) | | | |
| Pr(ER Visit) in % | 20.1 | –0.4 | 0.50 | –0.4 | 0.71 | 63000 |
| | | (0.5) | (1.1) | | | |
| **Panel E. Inpatient, Outpatient, and Carrier Spending** | | | | | | |
| Inpatient, Outpatient, and Carrier Total Spending, $/Year | 4699.8 | –279.6 | 0.54 | –155.2 | 0.83 | 12940 |
| | | (454.3) | | (702.4) | | |
| Inpatient, Outpatient, and Carrier OOP Spending, $/Year | 816.9 | –57.6 | 0.53 | 42.8 | 0.68 | 12940 |
| | | (89.9) | | (105.2) | | |
| Inpatient, Outpatient, and Carrier Medicare Spending, $/Year | 3828.5 | –141.6 | 0.71 | –184.1 | 0.77 | 12940 |
| | | (378.6) | | (621.8) | | |

*Notes.* This table reports the estimates of $\beta_1$, the regression discontinuity coefficient of interest from Equation 1, for all outcomes. The table is restricted to outcomes measured in the second year since LIS enrollment. Each row of the table is a separate regression with outcomes defined in row titles. Column (1) reports the mean of the outcome among beneficiaries born in the 90 days after May 1 ("control" beneficiaries). Column (2) reports the point estimate and clustered standard error for $\beta_1$ from Equation 1. Column (3) reports the p-value for the coefficient in (2). Column (4) reports the point estimate and clustered standard error for the estimation using *rdrobust* (Calonico et al., 2014, 2017) with the 90-day bandwidth. Column (5) reports the p-value for the estimate in (4). Column (6) reports the sample size.

Table A2: RD Robustness, Year 3 Since LIS Enrollment

| | Control mean (1) | RD coef. (2) | RD p-value (3) | RDRobust coef. (4) | RDRobust p-value (5) | $N$ (6) |
|---|---|---|---|---|---|---|
| **Panel A. Loss of drug subsidy** | | | | | | |
| Pr(Losing Subsidy) in % | 14.2 | 6.2 | 0.00 | 6.9 | 0.00 | 120436 |
| | | (0.4) | | (0.7) | | |
| **Panel B. Drug Insurance Demand** | | | | | | |
| Pr(Dropping Drug Insurance) in % | 3.0 | 1.1 | 0.00 | 0.6 | 0.06 | 120436 |
| | | (0.2) | | (0.3) | | |
| Share of Months in Part D, % | 98.5 | –0.8 | 0.00 | –0.5 | 0.02 | 120436 |
| | | (0.2) | | (0.2) | | |
| Part D List Premium, $/Year | 222.1 | 0.6 | 0.67 | 0.4 | 0.92 | 120436 |
| | | (1.5) | | (3.9) | | |
| Part D LIS Premium Subsidy, $/Year | 163.3 | –6.7 | 0.00 | –9.1 | 0.00 | 120436 |
| | | (1.6) | | (3.1) | | |
| **Panel C. Prescription Drug Utilization** | | | | | | |
| Total Part D Drug Cost, $/Year | 2564.9 | –23.1 | 0.92 | –95.2 | 0.79 | 24845 |
| | | (233.0) | | (359.6) | | |
| Standardized Part D Fills per Year | 37.9 | –0.1 | 0.88 | 0.9 | 0.59 | 24845 |
| | | (0.9) | | (1.6) | | |
| Part D Patient Pay Amount, $/Year | 125.5 | 4.9 | 0.63 | 11.2 | 0.42 | 24845 |
| | | (10.0) | | (13.8) | | |
| Part D LIS Payment, $/Year | 766.5 | –57.4 | 0.10 | –35.0 | 0.59 | 24845 |
| | | (34.0) | | (65.1) | | |
| **Panel D. Inpatient and Outpatient Healthcare Utilization** | | | | | | |
| Pr(Hospital Stay) in % | 8.3 | –0.2 | 0.50 | 0.4 | 0.48 | 120436 |
| | | (0.3) | | (0.5) | | |
| Pr(ICU Stay) in % | 2.7 | 0.2 | 0.09 | 0.5 | 0.11 | 120436 |
| | | (0.1) | | (0.3) | | |
| Pr(Emergency Admission) in % | 5.2 | –0.0 | 0.92 | 0.3 | 0.48 | 120436 |
| | | (0.2) | | (0.4) | | |
| Pr(Cardiovascular Hospitalization) in % | 2.4 | 0.0 | 0.97 | –0.4 | 0.21 | 120436 |
| | | (0.2) | | (0.3) | | |
| Pr(SNF stay) in % | 0.7 | –0.1 | 0.02 | –0.1 | 0.47 | 120436 |
| | | (0.1) | | (0.2) | | |
| Pr(Nursing Home Stay) in % | 0.9 | –0.2 | 0.01 | –0.2 | 0.31 | 120436 |
| | | (0.1) | | (0.2) | | |
| Pr(Physician Visit) in % | 67.9 | 0.4 | 0.63 | 3.2 | 0.27 | 11514 |
| | | (0.8) | | (2.9) | | |
| Pr(PCP Visit) in % | 51.5 | 1.0 | 0.48 | 4.0 | 0.20 | 11514 |
| | | (1.4) | | (3.1) | | |
| Pr(Cardiologist Visit) in % | 11.1 | –0.3 | 0.79 | –3.0 | 0.12 | 11514 |
| | | (1.1) | | (1.9) | | |
| Pr(ER Visit) in % | 20.6 | 0.3 | 0.65 | –0.9 | 0.41 | 56585 |
| | | (0.7) | | (1.1) | | |
| **Panel E. Inpatient, Outpatient, and Carrier Spending** | | | | | | |
| Inpatient, Outpatient, and Carrier Total Spending, $/Year | 5335.3 | –892.4 | 0.22 | –318.2 | 0.82 | 11514 |
| | | (708.2) | | (1368.4) | | |
| Inpatient, Outpatient, and Carrier OOP Spending, $/Year | 900.5 | –136.9 | 0.25 | 33.9 | 0.86 | 11514 |
| | | (116.0) | | (194.7) | | |
| Inpatient, Outpatient, and Carrier Medicare Spending, $/Year | 4379.7 | –802.2 | 0.23 | –318.3 | 0.80 | 11514 |
| | | (647.7) | | (1225.8) | | |

*Notes.* This table reports the estimates of $\beta_1$, the regression discontinuity coefficient of interest from Equation 1, for all outcomes. The table is restricted to outcomes measured in the third year since LIS enrollment. Each row of the table is a separate regression with outcomes defined in row titles. Column (1) reports the mean of the outcome among beneficiaries born in the 90 days after May 1 ("control" beneficiaries). Column (2) reports the point estimate and clustered standard error for $\beta_1$ from Equation 1. Column (3) reports the p-value for the coefficient in (2). Column (4) reports the point estimate and clustered standard error for the estimation using *rdrobust* (Calonico et al., 2014, 2017) with the 90-day bandwidth. Column (5) reports the p-value for the estimate in (4). Column (6) reports the sample size.