Efficiency Gains Under Incomplete Contracting: Evidence from Medicaid

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Abstract

While government contracting is pervasive, there is limited understanding of the magnitude of resulting efficiency gains, and particularly the degree to which incomplete contracting inhibits their pass-through to either governments or constituents. I examine these questions by looking to Medicaid contracting in New York, where the state pays private insurers to coordinate beneficiary care and reimburse providers, in lieu of doing so directly. These contracts exhibit incompleteness, as private insurers end up responsible for some but not all medical services, with the rest remaining under public provision. For causally identifying the effects of incomplete contracting, I leverage a change in contract completeness over the sample period, through the integration of previously excluded drug services. While I find evidence of efficiency gains, I find that incomplete contracting reduces their pass-through to governments, by leading private plans shift costs to medical services that remain under public provision. Eventual integration of these services into existing private contracts yields a 16% reduction in overall fiscal costs.

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1 Introduction

Governments contract a vast array of services to private firms, rather than administering these services directly, with contracting accounting for almost 10% of U.S. GDP altogether. One argument offered in favor of contracting is that private firms deliver services more efficiently, and that corresponding efficiency improvements would eventually get passed through to either governments or program recipients, via fiscal savings or program improvements (Savas 1977, Savas 1987). Unfortunately, there has been limited examination of contracting's actual efficiency impact, the magnitude of actual pass-through, and particularly the extent to which pass-through is limited by factors like incomplete contracting.

I examine contracting in the context of public health insurance, looking to Medicaid specifically, where governments pay fixed amounts to private insurers to coordinate and manage care for some beneficiaries. As part of this contracting, private insurers will then directly pay medical providers for patient care, in place of the government doing so. This setting uniquely positions me to examine fundamental issues in contracting, since it exhibits key features of contracting such as the outsourcing of provision to third parties (in this case private insurers), along with changes to the underlying nature of that provision (here through introduction of managed care).[2] Medicaid contracting also has policy relevance in its own right, given that private Medicaid plans covered almost 65% of all Medicaid beneficiaries and took in \$270 billion in government payments as of 2016 (KFF 2017), compared to only \$61 billion in 2008 (per Figure 1). Finally, by focusing on Medicaid contracting among the disabled specifically, this study may gain additional policy relevance. After all, the disabled remain mostly under public Medicaid and could consequently be targeted by future privatization initiatives, while furthermore accounting for a disproportionate 42% of all Medicaid expenditures (KFF 2017). In fact, health care makes up the government's single biggest expense item on the disabled, even exceeding spending on cash transfers (Autor et al 2011).

Despite the policy importance of Medicaid contracting, past work has largely focused on the fiscal impact of these policies, rather than their accompanying effect on health care use, patient well-being, or overall efficiency.[3] Moreover, while past work has not found meaningful fiscal savings from Medicaid contracting, it remains unclear whether this is due to limited

[2] Managed care is differentiated from the government's fee-for-service delivery system through greater intervention in patient care, as discussed further in Section 2.

[3] For example, Duggan (2004) and Duggan et al (2013) examine effects of private Medicaid on overall government spending, without closely examining accompanying effects on utilization or efficiency. Meanwhile, Aizer et al (2007) focus more directly on quality, finding reduced quality in terms of prenatal care and birth outcomes, but do not look beyond births. Further, they are unable to examine many direct measures of utilization (including overall inpatient and outpatient care), nor the mechanisms underlying the observed effects. Finally, Lee (2017) examines Medicaid Managed Care in New York primarily through the lens of inpatient hospital response.

pass-through of efficiency gains, or the absence of such gains altogether. An additional limitation of previous studies has come from their focus on lower-cost populations, which collectively account for only a third of Medicaid spending (KFF 2017) and may also not generalize to Medicaid as a whole.

One ongoing challenge to studying this topic has been limited availability of private Medicaid utilization and claims data. I overcome this issue by linking together several rich administrative datasets from New York State, including claims-level files. In doing so, I construct an individual-level panel on health utilization, which is unique in covering those in private as well as public Medicaid. This data is also unique in covering the full range of health care services, including outpatient and prescription drug use. Altogether, this data gives me a more complete picture of utilization relative to past work, and consequently allows for a fuller examination of relevant issues.

Another longstanding challenge stems from enrollment composition differences between private and public Medicaid, which makes it difficult to decompose program effects from those of underlying enrollment. I overcome this issue through a novel approach, focusing on those who were in Medicaid-only and not simultaneously in Medicare before 65, while also leveraging involuntary switching from private to public Medicaid at 65. Specifically, New York beneficiaries who were in Medicaid-only before 65 will converge to public Medicaid coverage after 65 and will simultaneously gain public Medicare coverage at that age, irrespective of whether they were in private or public Medicaid pre-65. This is driven by the confluence of two factors: the onset of Medicare coverage at 65 among those previously in Medicaid-only, and a New York state rule requiring those concurrently in Medicare to enroll in public Medicaid.[4] My analyses could be complicated by the concurrent onset of Medicare at 65, coinciding with the switch to public Medicaid, but this challenge is tackled as part of my analyses. By focusing on the disabled population, I am also able to avoid empirical issues that are specific to other Medicaid populations, such as frequent enrollment turnover.

A final challenge lies in identifying the effect of incomplete contracting on pass through and isolating it from potential confounding factors.[5] I overcome this issue by leveraging changes in contract completeness taking place during my study period, particularly in the scope of health care services covered. I consider the effect of making an existing contract more complete, through the 2011 integration of prescription drug services that were previously

[4] I also make use of a secondary identification strategy, leveraging involuntary switching from public to private Medicaid, driven by private Medicaid enrollment mandates. These mandates have been used in previous work on Medicaid privatization (Duggan 2004; Aizer et al 2007; Duggan, 2013).

[5] In the literature, incomplete contracting is defined as possible limitations in contractual monitoring, enforcement, or scope (Hart and Moore 1990, Hart 1995). While existing theory implies that the incomplete nature of contracts imposes costs, this theory has not been empirically tested, with the associated costs also not having been explicitly quantified (Hart 1995; Hart, Shleifer, Vishny 1997).

excluded.

Looking at private Medicaid's effect on care quantity and composition, I find a highly significant 15% reduction in overall inpatient utilization. This effect size is more modest than what has previously been estimated within the Medicare as well as commercial settings, where private/managed care plans appeared to reduce inpatient utilization by 40% relative to the public/non-managed care options (Duggan et al 2017, Manning et al 1987). This difference may be attributable to tighter regulatory constraints imposed on private Medicaid plans, since these constraints could limit the application of various care management tools. Under private Medicaid plans, I simultaneously find a striking 60% increase in non-institutional outpatient utilization, which in dollar terms offsets over half of the inpatient reduction.

Turning next to mechanisms, I find that some of the inpatient reductions could be attributable to increased outpatient care, which could reduce avoidable inpatient admissions in particular. Meanwhile, I find no evidence of direct substitution between the inpatient and outpatient settings, such as shifting of surgeries that can technically be performed in either space.[6] In addition, I posit that barriers to care under private Medicaid plans, such as prior authorization requirements, could help explain observed reductions in non-preventable and elective hospitalizations. Finally, increases in outpatient care, particularly in physician visits, are potentially attributable to improved provider access under private Medicaid plans. Such access is known to be deficient in public Medicaid, and could be particularly lacking in New York, given that its provider reimbursement rates rank in the bottom fifth of all states (KFF 2016).

I find that patients may benefit from Medicaid privatization, through increases in high-value outpatient care, including physician and office visits. Moreover, I find no evidence of adverse impact on patients through other forms of quality, at least to the extent these can be measured. Given that Medicaid privatization produces no observable harm to patients, and simultaneously results in lower overall medical utilization, I conclude that it may be efficiency improving.

Looking next to Medicaid privatization's direct impact on governments, I find that governments lose out through fiscal cost increases of 25%, consistent with estimates from past literature (Duggan et al 2004, Duggan et al 2013). I find that this cost increase is compounded by incomplete contracting, as spending on services originally excluded from private contracts was 30% higher under private plans. Moreover, integration of previously excluded drug services into private contracts resulted in a 29% drop in drug spending and 16% decrease in overall government spending on private Medicaid enrollees. This is consistent with

[6] Additional discussion of the substitutability of surgeries between the inpatient and outpatient settings can be found in AHA (2006) and MedPAC (2013).

existing theoretical work on the potential costs of incomplete contracting (Hart 1995; Hart, Shleifer, Vishny 1997), and to my knowledge serves as the first empirical test of that theory.

Altogether, my findings have substantial implications for Medicaid policy, given the prevalence of Medicaid contracting and given proposals to further expand it, particularly among higher-cost and disabled Medicaid recipients. I find that Medicaid contracting may be beneficial to program recipients, and I also identify the policy elements that could enhance overall pass-through of these benefits.

In Section 2, I review the basic characteristics of public and private Medicaid systems, and go over relevant institutional features of New York Medicaid. In Section 3, I review the administrative data from New York State underlying my analyses. In Section 4, I go over my empirical design and implementation. In Section 5 and 6, I discuss my empirical findings and concurrently test their robustness. In Section 7, I conclude.

2 Background

When Medicaid was initially introduced in 1965, only the "public" version of it was offered, which was directly administered by states and reimbursed providers on a fee-for-service basis. Starting in the 1980's however, state governments began contracting out Medicaid to private insurers, concurrent with similar efforts in Medicare (McGuire et al 2011). Such contracting was motivated partly by the theoretical benefits that could result from private competition (Hansmann 1980, Donahue 1989). Notwithstanding original intentions, previous studies on public insurance contracting suggest that it may have increased rather than reduced fiscal costs, within Medicaid (Duggan 2004, Duggan et al 2013) as well as Medicare (Brown et al 2014).

Currently, over 65% of all Medicaid recipients receive coverage through some type of private plan. Among higher-cost Medicaid groups such as the disabled, private Medicaid covers a somewhat lower 45% of beneficiaries, although this share has increased from only 20% in 2011 and is expected to grow further (KFF 2012).[7] Partly due to this increase in disabled enrollment, government expenditures on private plans have been rising in tandem, making up 50% of current Medicaid spending compared to 25% in 2011 (CMS 2017). Even in the face of these general trends, there remains significant variation across states in the design of their privatized Medicaid systems and the timetable by which they were introduced.[8]

[7] A number of states have contracted out Medicaid coverage for the disabled. Besides New York, these include California, Florida, Illinois, Louisiana, Ohio, and Texas (Sparer 2012).

[8] For example, there is significant variation in the types of insurers that states contract with, in terms of for-profit status, size, and other characteristics. There is also variation across states in ease of insurer entry into the private Medicaid market. Moreover, there is heterogeneity in the scope of services that private plans

New York State began contracting out Medicaid coverage in the 1990's, paying private plans fixed rates to provide health care coverage to beneficiaries. In New York, participating plans included a diverse set of carriers, in terms of for-profit status, size, as well as provider sponsorship, with each plan's payment levels determined through negotiation.[9] Initially, all New York Medicaid recipients eligible for the private option could remain under the public system, but New York eventually started making enrollment in private plans mandatory for certain populations. These requirements, referred to as "enrollment mandates", were rolled-out across counties under a pre-planned timetable.[10] At the same time, certain types of Medicaid recipients remained ineligible for private plans and instead had to receive coverage through public Medicaid; these included long-term nursing home residents & dual-eligibles (those Medicaid recipients who were simultaneously in Medicare). As a consequence of these eligibility rules, most private Medicaid enrollees would get involuntarily shifted onto public Medicaid at 65, given the typical onset of simultaneous Medicare-Medicaid enrollment at that age (Sparer 2008).

New York's private Medicaid plans initially covered inpatient, outpatient, and laboratory services. However, these plans were prohibited by law from covering certain ("carved-out") medical services such as prescription drugs, mental health, and long-term care, which continued to be directly paid for by the state. Starting in October 2011, however, prescription drug services were integrated into all of New York's existing private Medicaid contracts.

2.1 Characteristics of Private Plans

While private Medicaid plans are differentiated from public Medicaid in the source of their provision, with coverage administered by outside insurers rather than by the government directly, they also differ in the underlying nature of that provision. To this end, rather than operating under the same fee-for-service regime as public Medicaid, private plans are instead based off a capitated managed care framework. First, capitation specifically could foster sharper incentives, since plans would receive fixed payments that are invariant to the amount of care used. Second, in contrast to the traditional fee-for-service system, managed care takes

are allowed to cover. To this end, some states do not permit plans to cover services such as long-term care, prescription drugs, and mental health, with these services instead always administered by a state directly. Further, there is cross-state variation in whether enrollment in private Medicaid plans for any given group is required, optional, or even available (Duggan et al 2013).

[9] Prior to 2008, New York state did not explicitly risk-adjust payments based on individual-level health characteristics (Sparer 2008).

[10] These enrollment mandates initially applied only to children and TANF adults in New York Medicaid, but in 2005 were extended to disabled Medicaid recipients (Sparer 2008). Largely as a consequence of the mandates, enrollment in private Medicaid plans increased from 600,000 in the mid 1990's to 2.5 million in 2009 (New York Medicaid & Medicaid Managed Care Enrollment Data). .

a much more active and interventionist role in patient care, by placing restrictions on use of medical services, assigning primary care physicians (PCP's) a gatekeeper role, limiting the doctors patients can see through restricted networks, and engaging in care coordination.

Considering managed care's many moving parts, its impact could plausibly come through a variety of mechanisms. It could presumably achieve utilization reductions, first, through efforts at care coordination. For example, plans will designate a primary care provider to oversee patient care, especially for those with chronic conditions. Such coordination could mitigate acute health episodes, and thereby reduce preventable utilization. Managed care plans could also lower utilization by incentivizing and promoting greater use of preventive care, including PCP visits and essential prescription drugs (Glied 2000).

In addition, managed care could impact utilization by imposing barriers to care, in requiring special approval for certain visits and procedures. Some of these approvals would need to come from plans directly. Meanwhile, the approval of other services, such as hospital admissions or specialist visits, would need to come from PCP's through their gatekeeper role. Altogether, these barriers could reduce low-value or unwarranted care, though at the risk of simultaneously eliminating valuable care (Glied 2000). Unlike managed care plans in the commercial setting, private Medicaid plans are unable to employ cost-sharing as an additional barrier to care, given statutory requirements mandating de minimus co-pays and deductibles. This said, there have been some recent and isolated moves towards increasing Medicaid cost-sharing, in Indiana most notably (Saloner et al 2014).

Managed care could further shape utilization through construction of restricted provider networks, which limit the set of covered providers that patients can see. Such networks could reduce utilization by virtue of their limited size as well as their underlying composition (Geruso et al 2017; Gruber et al 2016).[11] In the commercial setting, provider network construction could produce the added benefit of lower negotiated reimbursement rates (Zwanziger et al 2000), as indicated in the existing literature (Cutler et al 2000). Within Medicaid however, private Medicaid networks may actually exhibit higher negotiated rates than the public system, given that public prices are often administratively set to be below the market-clearing rate.[12]

Altogether, studies on managed care in Medicaid have found some evidence of utilization decreases (Aizer et al 2007, Duggan 2004, Lee 2017), but this evidence has mostly been

[11] Limited network breadth could reduce utilization by reducing care access, such as by increasing distance to the nearest provider or lowering appointment availability. Limited networks could also reduce utilization by directing care to more efficient providers, by weighing network composition towards more efficient physicians.

[12] Based on conversations with the New York State Department of Health, negotiated rates among private Medicaid plans are typically higher than under fee-for-service. This is based on New York's analysis of internal administrative data, which tracks provider rates across both settings. Based on additional conversations, private Medicaid's negotiated rates appear to exceed public Medicaid ones across most states.

limited to newborns and inpatient care. Studies on managed care in other settings, looking at a more comprehensive set of utilization measures, have also found evidence of reduced utilization. For example, managed care has been found to lower inpatient utilization by around 40% in the commercial setting and by a similar magnitude in Medicare (Manning et al 1987, Duggan et al 2017). Unfortunately, these other studies could have limited generalizability to Medicaid specifically, given differences in the characteristics of the covered population, the plan and program environment, and the prevailing regulatory conditions.

3 Data

In this paper, I use several administrative datasets from CMS and New York State, containing information on public and private Medicaid enrollment status, along with inpatient, ER, outpatient, and drug utilization. Uniquely, this data allows for construction of an individual-level panel of utilization, which covers everyone in private as well as public Medicaid. Sample inclusion in these data also remains unconditional on utilization, in contrast to the standalone discharge data typically used for researching Medicaid. This data is also unique in tracking outpatient and prescription drug utilization, in contrast to past studies with access to inpatient data only, allowing for a more complete picture of patient care.

Using information contained in the administrative data, I can precisely construct cohorts that are relevant to my analyses. Specifically, I restrict to New York State residents actively enrolled in Medicaid, around the age of 65, who qualified for the program as a result of disability (formally, this group is referred to as non-elderly SSI recipients). From the sample, I also exclude those who were simultaneously in Medicare prior to 65.

3.1 Beneficiary Characteristics and Enrollment Information

To derive information on beneficiary characteristics and enrollment status, I use administrative data from the Centers for Medicare & Medicaid Services (CMS), covering New York State for the 1999-2010 period. The data contains person-month level demographic and related information, including information on overall Medicaid and Medicare enrollment status, which is broken down more granularly for public and private Medicaid, as well as each beneficiary's basis for Medicaid eligibility.

For those in private Medicaid plans, the CMS data also tracks the specific plan of enrollment through standardized ID's, which can be used for linking to external plan characteristics data.

3.2 Inpatient and ER Utilization

I track inpatient and emergency room usage for everyone in Medicaid, inclusive of those under private as well as public coverage. I do so by linking together Medicaid enrollment data (obtained from CMS) and visit-level hospital and ER data (obtained from New York State). This linking is facilitated through Social Security number information found in both datasets, which was obtained through a special administrative process.[13] The data track every single inpatient visit made by New York Medicaid recipients over the 1999-2010 period, along with every ER and ambulatory care visit made over the 2005-2010 period. The inpatient and ER settings are promising areas for study, as these represent particularly expensive forms of treatment, which private plans could plausibly aim to reduce.

This data tracks the timing of each hospital visit, at a month-year level. The data also provides visit-level information on treatment intensity and composition, including hospitalization type, length of hospital stay, set of procedures performed, and total (pre-discounted) hospital charges. Using fields contained in the data, it is also possible to classify hospitalizations into various relevant categories, including elective, emergency, and surgical. Finally, the data specifies the name and location of the hospital visited, allowing me to link to external hospital characteristics measures as well as to estimate each patient's hospital travel time.

For most of my analyses, I aggregate this data to a person-month level, and include those without any utilization as part of the sample (as such, sample selection is not conditional on having a hospital/ER visit). Information on each individual's public as well as private Medicaid status is taken from the original CMS files, rather than from the discharge data directly, given possible payer miscoding in the original discharge file.

In Table 1, I present average, annualized utilization measures for my main analytic sample (those between 63 and 67, who were Medicaid-only enrolled at 63). I break these measures out for two separate subpopulations, corresponding to my treatment and control groups: those in private and public Medicaid respectively, at 63. I find that the private Medicaid group has substantially lower utilization (by 20-30%) relative to the public, although the extent to which this is driven by enrollment composition rather than treatment differences is not readily apparent.

[13] Linking was conducted using a combination of the last four digits of individuals' SSN, date and year of birth, gender, as well as county of residence; in combination, these variables uniquely identify Medicaid recipients over 99.9% of the time. Medicaid recipients who could not be uniquely identified here were excluded from the sample.

These unique linking variables are contained in a special version of New York's SPARCS data, which was obtained through an application process. Through a separate application process, I obtained special CMS data, containing SSN identifiers for every Medicaid recipient.

3.3 Outpatient Data

I track outpatient hospital, physician, as well as non-hospital institutional care using claims-level information from CMS. This data covers all Medicaid recipients in New York State, for the 2008-2010 period, unfortunately encompassing fewer years than the inpatient data. As with the previous inpatient data, this data also covers those in private Medicaid plans, rather than being restricted just to public Medicaid. Similarly, this data covers those Medicaid recipients simultaneously enrolled in Medicare, along with those enrolled in Medicaid only. Altogether, this data allows me to get a more complete picture of each individual's health care usage than would be possible with inpatient data alone, particularly given that non-inpatient care accounts for over 65% of this population's medical spending. The data contains information on the site and date of care, along with the diagnoses and procedures associated with any given visit. Based on this information, I am able to classify care into different categories, including regular office visits, surgeries, as well as diagnostic and imaging procedures. I am also able to identify outpatient care that is of a preventive nature, using an accepted classification system among insurers.

Claims data on those enrolled in Medicaid only is taken from CMS's Medicaid files. Meanwhile, data for those simultaneously in Medicaid and Medicare is taken from Medicare files, as Medicare constitutes the primary payer for that population and thereby sees the full universe of outpatient utilization. Note that this data is linked together at an individual-level using common beneficiary ID's, allowing me to follow individuals over time, even if they switch from private plans to public Medicaid or go from being in Medicaid-only to being simultaneously in Medicare. Given that data on those in private Medicaid plans originally comes from the plans themselves, this introduces the risk of data quality issues and particularly visit underreporting, based on apparent incompleteness of past data turnovers (Lewin Group, 2012). These concerns could be mitigated by apparently higher outpatient usage under private Medicaid, since data quality issues would most plausibly produce the opposite effect, through undercounting of private Medicaid visits. As a consequence, actual outpatient usage under private Medicaid could be even higher than estimated here, suggesting that these results ought to be treated as a floor.

The outpatient data also includes information on actual cost amounts, specifically actual amounts paid by public Medicaid and Medicare to providers, although these amounts are unfortunately missing from all private Medicaid claims. To overcome this limitation, I construct a standardized cost measure applying to all of Medicaid, public as well as private. To do so, I mechanically set private prices to corresponding public Medicaid rates, with my approach described in further detail in Appendix D. While this is useful for measuring the relative intensity of outpatient care, one limitation is that it does not capture potential differences in unit prices across public Medicaid and private Medicaid plans, which could arise through private negotiation.

3.4 Pharmaceutical Data

I track prescription drug utilization, using claims-level data obtained from CMS. This data covers all public Medicaid as well as private plan enrollees in New York State, for the 1999-2011 period. It can be linked at a patient-level to all non-pharmaceutical claims data, with the help of common beneficiary-level identifiers. This data tracks individuals in Medicaid-only for the full sample period, along with those dually-enrolled in Medicare and Medicaid for all years after 2008.

Prescription drug data for those in dually-enrolled Medicaid and Medicare is mostly taken from Medicare Part D files, given that Medicare administers prescription drug coverage for this group following 2006. Meanwhile, prescription drug data for those in Medicaid-only is taken from CMS's Medicaid-specific files. The Medicaid data is comprised of drug claims from the public system as well as from private plans, particularly for the time period after October 2011, when private plans assumed responsibility for prescription drug coverage.[14] One concern with claims data turned over by private plans is that it might be incomplete and not contain the full universe of claims, given claims underreporting issues that have previously been identified (Lewin Group 2012). However, this concern seems unwarranted here, as private and public Medicaid drug utilization appears to differ primarily in terms of claims composition rather than in overall claims count.

This data includes information on the quantity and types of drugs used, including individual drug identifiers (NDC codes), which I link to external data tracking each drug's therapeutic class. In addition, I am able to track whether a drug targets acute conditions, chronic conditions, or is instead discretionary by nature, based on classifications compiled in Chandra et al (2010). In turn, these classifications allow me to identify drugs that have spillover effects on medical spending.

Finally, this data specifies the cost of each individual drug claim processed by public Medicaid. Unfortunately, no raw cost information is turned over in the private Medicaid claims, requiring imputation of these prices off public Medicaid rates, using an approach described in further detail in Appendix D. One limitation of this approach is that it fails to capture possible differences in negotiated drug rates between public and private Medicaid plans. An additional drawback is that none of the cost information is inclusive of negotiated drug rebates.

[14] Fortunately, all of these data are maintained in a common format, and as a result can be longitudinally linked.

3.5 Quality of Care Metrics

To measure quality of care, I rely on external hospital-level measures of quality. In addition, I evaluate care quality at an individual hospitalization level, using commonly accepted algorithms. Finally, I consider other traditional measures of quality, such as rates of readmission.

External measures of hospital-level quality are taken from CMS. These consist of risk-adjusted mortality and readmission rates for each hospital, for heart attacks, heart failure, and pneumonia. These also consist of process measures, which capture the extent to which a hospital follows best practices, such as the administration of Advil following heart attacks. I also make use of additional hospital characteristics, such as whether a hospital is classified as a major teaching center.

Discharge-level hospital measures, meanwhile, are meant to capture quality of care for each individual visit. One of these measures flags ambulatory-sensitive hospital visits, or those that could have been prevented through better outpatient treatment, based on an algorithm developed by AHRQ. Another measure flags ER visits that were non-emergency or preventable, and is based off an algorithm developed by the NYU Center for Health and Public Service Research.

Finally, I make use of traditional measures of care quality, such as rates of hospital readmission. Unfortunately, these measures and all the others used are incomplete indicators of care quality and patient well-being. The coarseness of these measures constitutes a meaningful limitation of my study, and prevents me from drawing more sweeping conclusions around the welfare consequences of private Medicaid plans.

3.6 Government Expenditures Metrics

I construct individual-level measures of government Medicaid expenditures, based off CMS's administrative data. First, I track overall fiscal spending under public Medicaid, while also breaking this spending measure down for specific service categories such as long-term care, inpatient care, and pharmaceuticals. Separately, I track fiscal spending on private Medicaid plans in the form of premiums, at a beneficiary-month level.

4 Identification and Empirical Strategy

Public and private Medicaid could differ in terms of underlying enrollment composition, given that healthier populations are more likely to enroll in these plans voluntarily and also more likely to be explicitly required to do so (Duggan 2004). As such, any naive comparison between public and private Medicaid may capture differences in patient composition rather than in treatment.

For my primary identification strategy, I focus on those who were in Medicaid-only while under 65, without being simultaneously in Medicare then. I leverage involuntary switching from private to public Medicaid at 65, as individuals will converge to public Medicaid coverage post-65, irrespective of whether they were under public or private Medicaid coverage pre-65. Alongside convergence to public Medicaid coverage, both these groups will also gain Medicare at 65, a complication that I try to account for in my analyses. To start, I employ a differences-in-discontinuities approach around the age 65 threshold, restricting to disabled beneficiaries in Medicaid-only prior to 65. Those in private Medicaid pre-65 are designated as the treatment group, while those in public Medicaid pre-65 make up the control. This control group is meant specifically to account for Medicare's concurrent onset at 65, and to tease out that effect from the separate impact of switching from private to public Medicaid. Not everyone in the treatment group will be subject to the actual treatment (a small fraction will remain in private Medicaid plans, post-65, as some are ineligible for Medicare post-65[15]), meaning that the results would capture an intent-to-treat effect.

Altogether, this involuntary switching from private to public Medicaid at 65 comes from the confluence of two factors: a New York State rule prohibiting private Medicaid enrollment among those also in Medicare, and the onset of Medicare eligibility at 65. With its reliance on the age 65 Medicare eligibility threshold, my approach is somewhat analogous to the research design in Card, Dobkin, and Maestas (2008).

The estimating equation for the primary analysis takes the following form, for individual i, at time t.

$$y_{it} = \alpha + \beta_0 * InitiallyPvt_i + \beta_1 * Post65_{it} + \beta_2 InitiallyPvt_i * Post65_{it} + X_{it} * \gamma + \varepsilon_{it}$$

(**Equation #1**)

I also include gender, quarter-year, and county fixed effects, along with a flexible control for age.

As discussed, the estimated value of β_2 will reflect the intent-to-treat impact, and will need to be scaled to reflect the actual effect of treatment, based on the fraction of those initially in private Medicaid plans actually switching into the public option, at 65.

[15] To be eligible for Medicare at 65, an individual must be a U.S. citizen or permanent resident, and must have resided in the U.S. for a minimum of five years.

4.1 Endogeneity and Other Empirical Concerns

My identification strategy is predicated on a number of assumptions, which would need to be satisfied for my results to be credible. I examine the validity of each of these assumptions, one-by-one.

As discussed earlier, my research design presumes that there are no other changes at 65, which differentially affect those in public and private Medicaid pre-65. Specifically, it presumes that Medicare's onset at 65 does not differentially impact those who were sicker, particularly insofar as this would differentially impact my treatment and control groups. In theory, onset of Medicare coverage among those already in Medicaid could affect overall care through a broader provider network, higher reimbursements to providers, and changes to prescription drug coverage. Meanwhile, Medicare's onset wouldn't affect care through either cost-sharing or scope of services covered, as these would remain unchanged. While other changes besides Medicare onset are common at 65, these should be less germane for my sample population. For example, this group's employment status should not meaningfully change at 65, given limited labor market activity among the disabled, and in particular among the SSI recipients on whom I focus.[16] Moreover, no other relevant changes are observed at 65, at least along dimensions such as medical coverage and disability status.

To address concerns relating specifically to Medicare's onset at 65, I first examine the effect of turning 65 on the control group, finding no significant impact on most outcome measures. This makes it more likely that Medicare onset (along with other unrelated changes at 65) similarly has no meaningful impact on the treatment group, although this should be seen as a suggestive rather than definitive indicator. To tackle this concern further, I re-run my analyses using an instrumented measure of private (as opposed to public) Medicaid status, immediately before 65. This instrument can be constructed using my secondary identification strategy, which is based on county-year variation in the introduction of private Medicaid enrollment requirements (otherwise known as mandates), and is covered in further depth in Appendix A. With this instrument, I can set private Medicaid status to be independent of health and other characteristics, and thereby satisfy the identifying assumption. Results from these analyses, which can be found in the main tables as well as in Appendix B, are comparable to those that are based off my primary specification. This provides validation of a key identifying assumption, and should mitigate concerns that my results are driven by onset of Medicare or alternative changes at 65.

As a final validation of my identification approach, I perform separate analyses using an alternative strategy, based again off the staggered introduction of enrollment mandates. The

[16] Only 5.7% of SSI recipients of working age were employed to any degree in 2008 (Hemmeter 2009).

corresponding findings, which are discussed in greater detail in Appendix A, are generally consistent with those from my primary identification strategy, although they are substantially less precise.

My strategy additionally presumes that no differential pre-trends exist between the treatment and the control groups. Across a number of analyses, I find no evidence of such differential pre-trends, confirming the validity of my identifying assumption. In addition, this result suggests that the observed effect is not coming from strategic delaying or hastening of care. I also find no evidence of effect attenuation over the post-period, suggesting that my results are not driven by pent-up demand.

Besides endogeneity issues, there could also be concerns around external validity and the generalizability of my results to Medicaid as a whole. For example, my study is focused on a single state, which differs from the rest of the country along various material dimensions, such as the generosity of its public Medicaid program (which has the highest per-enrollee spending in the nation) and the largely urban make-up of its beneficiaries. While I acknowledge this as a limitation of my study, I do not consider it prohibitive, as New York State's Medicaid program overlaps with the nation's in other important ways. First, New York State's private Medicaid plans are all structured as HMO's, consistent with plan make-up across other states. Second, these plans are operated by a diverse set of carriers, along such dimensions as for-profit status, size, and geographic scope. Third, as in many other states, Medicaid plans in New York State are afforded free-entry into the marketplace conditional on satisfying base requirements.

Besides its geographic limitations, my study is additionally constrained by its focus on disabled Medicaid recipients around 65, although I argue that this particular population might still generalize to Medicaid overall. First, this group accounts for a large and disproportionate share of Medicaid spending, despite making up a relatively smaller share of the beneficiary population. Moreover, any effects that I observe for this population could also hold true for lower-cost populations, given that successful care management could be most difficult to attain among a higher-cost group. A final concern relates to the short-run nature of my study, which could consequently fail to capture longer-run impacts. Although I cannot address these concerns fully, I do consider how effect sizes may change over the shorter-term, and do not find any evidence of attenuation.

5 Results

5.1 The Impact of Age 65 on Private Medicaid Enrollment Status

The basic results are documented in Figure 2, indicating a sharp drop at age 65 in the percentage of Medicaid beneficiaries in private plans, among those originally in private Medicaid at 63. The decline right at 65 appears much sharper than the preceding trend in private Medicaid enrollment rates. Further, little corresponding change is observed at 65, among the group initially in public Medicaid.

I present corresponding regression results in Table 2, which yield similar findings, indicating that two-thirds of those initially in private Medicaid switch to the public option, at age 65 (the relevant point estimate is found in column one, under the Init. Private*Post 65 term). These analyses are based off the baseline specification (Equation 1), where the outcome of interest is at a person-month level, and the sample is restricted to those who were disabled and in Medicaid-only at 63.

A number of explanations exist for why only two-thirds my original cohort switches to the public option, at 65. First, among the original group in private Medicaid, about 20% transitioned into public Medicaid prior to 65 (as implied by the point estimate on the Initially Private term, in column one). Further, many of those in private Medicaid are not forced into the public option at 65, as not all become Medicare-eligible at that age.[17] Given this, my main estimates will reflect an 'intent-to-treat' effect rather than the impact on those 'actually treated.' As such, to get at the actual treatment effect, the original estimates will need to be scaled by 1.5.

I perform an additional robustness check, examining whether the treatment and control groups gain additional Medicare coverage to the same degree, at 65. I find these groups gain this coverage at a comparable rate of 80%.

5.2 The Impact of Private Medicaid on Inpatient Care

Using changes in private Medicaid status at 65, I examine the effect of private Medicaid on overall inpatient utilization, while also decomposing how much of this attributable to extensive margin changes in number of hospitalizations. The hospital setting is a particularly suitable one for detecting utilization reductions and efficiency improvements, given that hospital care is relatively expensive compared to other medical services. Since private

[17] To be eligible for Medicare at 65, an individual must be a U.S. citizen or permanent resident, and must have resided in the U.S. for a minimum of five years.

Medicaid's impact on inpatient care might not be commensurate with its effects on other types of utilization, I will examine effects on outpatient and pharmaceutical care separately.

The inpatient utilization impacts of switching from private to public Medicaid are presented in Figures 3 and 4, showing a sharp and statistically significant jump in overall inpatient days and a comparable jump in the total number of inpatient visits. I also document an absence of differential pre-trends, while finding no evidence of effect attenuation over the post-period, suggesting that my results are not driven by pent-up demand. My sample restrictions remain unchanged from before, with the observation-level being at a person-month level and sample selection not being conditional on utilization.

Regression results, which are presented in Table 3, imply that switching from private to public Medicaid results in an approximately 20% increase in individual inpatient utilization. The effects on number of hospital visits and overall visit intensity (for example based on inpatient days) appear comparable, suggesting that much of the overall impact may come through extensive margin changes in hospitalization likelihood. However, decomposing the full effect into its extensive and inpatient margin components is difficult, given possible accompanying changes to visit composition. As shown in the bottom two panels, my estimates are robust to a tighter age restriction (64 to 66), and also to instrumenting for initial private status using an alternate strategy. The latter strategy is meant to rule out an empirical threat from the simultaneous introduction of supplementary Medicare coverage, at 65, which could bias my results if Medicare's impact differs across subpopulations.

In Table 3, the point estimates of interest fall under the Initially Private*Post 65 term, with these needing to be scaled by approximately 1.5 to get at the actual effect of transitioning from private to public Medicaid, given the share of the treatment group actually being treated. For example, for total days stayed, the point estimate of .335 in the top panel implies that switching from private to public Medicaid results in .50 (or 18%) more annual days in the hospital. In addition, I find that the point estimate on the Post 65 term is a relatively modest -.064, and is not highly significant. This suggests that the additional onset of Medicare eligibility, at 65, does not have a meaningful effect on utilization among the control group, which could lend additional credibility to my research design. Finally, I find that the point estimate on the Initially Private Medicaid term is a highly significant -1.201; this estimate captures the degree of advantageous selection into private Medicaid, relative to the public version.

To better understand the observed inpatient impact, I look at how this effect breaks down across different types of inpatient visits, specifically across visits classified as either elective, emergency, surgical, or as readmissions. This is meant to explore some, albeit not all of the potential mechanisms underlying the inpatient effect, and also to understand the potential welfare implications accompanying it. Figure 5 documents the effect of switching from private to public Medicaid for one particular class of visits, readmissions, indicating a sharp jump among the treatment group at 65, without any comparable change among the control group. Meanwhile, Figure 6 shows a more modest effect on non-readmissions, at least as a fraction of baseline levels. The figures also provide no evidence of differential trends across the treatment and control groups over the pre-period, while showing that the readmissions effect actually grows over the post-period.

These findings are confirmed by regression results presented in Table 4, which indicate a substantially greater proportional effect on readmissions compared to non-readmissions, as well as a more pronounced effect on non-surgical relative to surgical visits. Meanwhile, there appear to be comparable effects on elective relative to non-elective visits, as well as on emergency relative to non-emergency ones. These findings are robust to a tighter age restriction, as shown in Table B.1, as well as to a secondary identification strategy addressing the empirical threat from Medicare's concurrent onset at 65. The readmissions results in particular suggest that part of private Medicaid's effect on utilization may come through improved prevention. This mechanism of improved prevention is also consistent with the observed timing of the effect on readmissions, which is not entirely instantaneous but rather becomes more pronounced over the post-period, as documented in Table B.2. This timing fits with the narrative of improved prevention, as the benefits of prevention could also be expected to materialize over time.

In Table 4, I also examine the effect of private Medicaid on distance traveled to the hospital, conditional on hospitalization. I find such distances to be modestly lower under private Medicaid, which would be difficult to reconcile with narrower hospital networks. Consistent with this, I find that the breadth of hospitals actually visited is comparable across private and public Medicaid, and that private plans in New York appear to largely contract with the full universe of hospitals, at least over this time period.[18]

Altogether, these results suggest that inpatient utilization reductions under private Medicaid could be attributable to improved prevention, but cannot be explained by changes in distance to hospital or in hospital network composition more generally. In line with this, I separately find that changes in readmission rates cannot be accounted for by changes in initial site of hospitalization nor in initial type of visit, as discussed further in Appendix C. Additional mechanisms also appear likely however, given the observed effect on discretionary care, including elective and non-emergency visits. The set of possible mechanisms could in-

[18] In conversations, the New York Dept. of Health confirmed that most private Medicaid plans in the state did not adopt limited hospital networks over this time period, and instead contracted with the full universe of hospitals. According to state officials, this has changed in more recent years, with a growing number of private plans narrowing their networks.

clude utilization management techniques, such as prior authorization requirements, which are applied towards discretionary care in particular. Finally, cost-sharing can be ruled out as a possible mechanism in this setting unlike in others, given that it is effectively absent across all of Medicaid.

Contextualizing these results with respect to Medicare, private Medicare plans have also been found to reduce inpatient utilization, although to an even greater degree than what I estimate for private Medicaid. However, in Medicare, this reduction appears to come largely through discretionary visits, primarily through the mechanism of prior-authorization requirements (Duggan et al 2017). These points of difference between private Medicare and Medicaid appear plausible, given that private Medicare plans are granted relatively more scope for limiting utilization, which could translate into correspondingly greater utilization reductions.[19]

5.3 Private Medicaid's Impact on Outpatient Care

To get at the full impact of private Medicaid, the effect on inpatient utilization cannot be considered in isolation, but instead must be considered alongside the accompanying effect on outpatient care. Outpatient utilization is an important element of care in its own right, and at the same time could have spillover effects on inpatient care, potentially serving as an additional mechanism through which private Medicaid affects inpatient use. For example, certain hospitalizations could be prevented outright, through appropriate outpatient care. Outpatient care could also substitute for inpatient care more directly, as some services could be shifted from the inpatient to outpatient settings, including surgeries in particular (AHA 2006, MedPAC 2013). Ultimately, the share of the inpatient effect attributable to outpatient care is difficult to identify, as only a subset of outpatient services may produce inpatient spillovers. Moreover, inpatient visits could be affected by factors other than outpatient care.

In my analyses, I focus first on outpatient care delivered in the hospital setting, looking to ER and ambulatory care services in particular. As discussed in greater detail in Section 3, the data tracking this utilization is collected by New York State, for all payers, for the 2005-2010 period. Results presented in Table 5, under the first row of Panel A, imply 10% fewer ER visits under private Medicaid in comparison to the public version, inclusive of ER visits not resulting in inpatient admissions. Meanwhile, I find no significant effect on outpatient hospital ambulatory surgery visits. Altogether, the effects on these select outpatient services

[19]For example, private Medicare plans are able to institute prior authorization requirements more widely and stringently than plans in private Medicaid. Private Medicare plans are also known for adopting restricted hospital networks, unlike private Medicaid plans in New York State.

appear consistent with my previous inpatient estimates.

To further explore effects on outpatient care and their associated implications, I next turn to unique outpatient claims data, covering the public and private Medicaid settings as well as Medicare. As previously discussed, these data ought to comprehensively track outpatient utilization for my sample population. Unfortunately, this data covers a more limited time period than the inpatient data used in previous analyses, extending only from 2008 through 2010.

Using these data, I first examine effects on a standardized measure of non-institutional outpatient utilization, covering a core set of outpatient and physician office services.[20] In Figure 7, outpatient utilization appears to increase substantially at 65 for control group members gaining Medicare coverage, who simultaneously continue to be under public Medicaid. The onset of Medicare could result in a utilization increases through channels such as provider network composition, given that past studies have documented broader provider networks and greater outpatient provider availability under Medicare when compared to Medicaid (Boccuti 2015). Meanwhile, private Medicaid beneficiaries appear to experience no jump in outpatient utilization at 65, upon switching to public Medicaid and also gaining Medicare coverage. Altogether, this suggests that outpatient utilization is higher under private Medicaid relative to the public option.

Regressions results in Table 5 also suggest substantially higher outpatient utilization under private Medicaid, by 50% to 75%, across a wide range of services; the full set of coefficient estimates is presented in Table B.3.[21] These findings do not appear to be driven by data quality or other reporting issues, as data underreporting from private Medicaid would actually bias my estimates downward, and effectively understate any increases from private Medicaid. For additional context, claims data for private Medicaid plans come from a different source than the public Medicaid data, and may be prone to underreporting (Lewin Group 2012).

The increase in outpatient care appears to be particularly pronounced for office visits and other services of a more preventive and thus high-value nature, while being less pronounced for lower-value imaging services, meaning that the additional utilization could ultimately be welfare improving and of benefit to patients. Altogether, the coefficient estimates imply 75% higher rates of office visits under private Medicaid, while only 18% higher utilization of imaging services. Higher rates of preventive outpatient care under private Medicaid could

[20] This set is inclusive of all professional services performed in the outpatient setting, including physician office visits. It is also inclusive of tests and basic services provided in the office and outpatient hospital settings. It excludes most institutional outpatient costs, such as outpatient hospital procedures. It also excludes costs related to home, long-term, and personal care.

[21] Companion first-stage estimates for this particular sample are presented in Table B.4.

also factor into the simultaneous reduction seen in inpatient readmissions.

To quantify private Medicaid's impact on overall utilization and medical spending, I sum up the original inpatient and outpatient-specific estimates, after scaling them to reflect treatment non-compliance. These imply that private Medicaid's inpatient costs are $1350 lower, at the same time that its actual outpatient costs are $800 higher, based on standard cost-to-charge ratios.[22] This translates into net costs that are $550 lower under private Medicaid, implying that inpatient care reductions are substantially offset by outpatient care increases, at least based on the limited set of outpatient services captured here. While the overall cost estimate here is sensitive to the specific sample restrictions chosen, the magnitude of the outpatient offset should remain considerable under any sample alternative.

5.4 Quality of Care

To more completely identify the impact of private Medicaid, relative to the public version, its effect on utilization and cost must be considered in combination with its effect on patients. The preceding outpatient results suggest that private Medicaid improves care quality, specifically through increases in preventive care and physician office visits. To build on these results, I look to additional care quality indicators, including hospital-level measures of quality and patient-level measures of health outcomes. Based on these other measures, I find no evidence of quality deterioration. These findings should not be treated as definitive, however, given that the underlying measures suffer from limitations and may not capture quality fully. However, taken together with the other inpatient and outpatient findings, these results suggest that private Medicaid produces a net benefit for patients.

My quality-related analyses here focus on the inpatient setting. While the underlying unit of observation is at the individual visit level, I find that the results below are not driven by concurrent changes to visit composition; the estimates are robust to the inclusion of visit category fixed effects, which cover several hundred DRG-based conditions.

Looking to hospital quality measures, I find no significant difference between public and private Medicaid, in terms of hospitals' performance on core CMS measures or other observable indicators. Outcome-based results are presented in the first section of Table 6, while process-based results appear in the second section. The outcome measures reflect each hospital's mortality and readmissions averages, 30 days following admission, for heart attacks, heart failure, and pneumonia. Meanwhile, the process indicators cover a range of

[22] These calculations assume actual Medicaid costs as being 50% of inpatient charges. Further, while these calculations are inclusive of a subset of outpatient spending, they are not inclusive of outpatient services such as home care, long-term care, personal care, and outpatient surgeries.

conditions, such as heart attacks, heart failure, and pneumonia. I find no significant impact on an additional proxy for hospital quality, major teaching hospital status, as shown near the bottom of Table 6.

Finally, looking to more direct inpatient quality measures that are individual-specific, I find evidence of quality improvements under private Medicaid plans. Specifically, I find that readmissions likelihood is 1.4% lower under private plans, conditional on initial hospitalization. I also find no effect on preventable ER visits, again conditional on ER utilization. Unfortunately, I am unable to consider general mortality as part of these analyses, as it is not reliability tracked in CMS's Medicaid data. In analyses not shown here, I find no significant effects on an alternative mortality measure, in the form of in-hospital deaths.

5.5 Fiscal Impact on Governments

In addition to Medicaid privatization's effect on utilization and patient well-being, I also consider how it impacts governments' fiscal costs. This analysis fits into a broader examination of the incidence of Medicaid privatization's impact. For it, I turn to data tracking direct government costs under public Medicaid, including fee-for-service payments and premium payments to private plans. Separately, I also look at costs for "carved-in" services, or services covered directly by private plans, along with "carved-out" costs for services that are always covered and paid for by the government.[23]

Altogether, I find that overall fiscal costs are about 25% higher under private than public Medicaid. As shown in Table 7, this increase is disproportionately driven by carved-out services, whose costs increase by 30% compared to only 20% for carved-in services. Incidentally, the results also suggest that Medicare's fiscal costs are at least 25% higher than even private Medicaid's, for an equivalent patient group.

5.6 Incomplete contracting and its effect on pass-through

Under private Medicaid, the increased use of carved-out services could be attributable to cost-shifting and to incomplete contracting by extension. Theoretically, plans may increase use of carved-out services, such as prescription drugs, to reduce use of carved-in medical care. This would be consistent with drug-associated medical spillovers examined in Chandra et al (2010), Starc et al (2015), and Lavetti et al (2016). While prescription drugs could serve as substitutes for a range of medical services, acute care substitution could be most relevant

[23] Fiscal spending on carved-out services comes through fee-for-service payments, for all Medicaid enrollees; meanwhile, fiscal spending on carved-in services comes entirely in the form of premium payments for those in private Medicaid, and entirely through fee-for-service payments for those in the public version. The underlying unit of observation here is aggregated to the beneficiary-month level.

to this setting; drug substitution for behavioral health services may be less applicable here, given that behavioral health is carved-out of private Medicaid in New York.

However, since private Medicaid differs from public Medicaid in multiple respects, increased use of carved-out services could be due to factors other than incomplete contracting. For instance, private Medicaid is distinguished from public Medicaid in its use of managed care, which could lead to increased use of a service irrespective of its carved-in or carved-out status. In a similar paper on Medicare, Starc and Town are faced with an analogous empirical challenge, which they are unable to overcome directly.[24] To tackle this issue and decompose the effects of incomplete contracting from those of managed care, I leverage a 2011 change in the scope of private Medicaid contracts. Under this change, prescription drug services were integrated into existing plan contracts, while non-drug plan coverage remained unchanged.

Consistent with other carved-out services, prescription drug utilization is higher under private than under public Medicaid, over the 2008-2010 period preceding their eventual integration. The results, which are presented in Table 8, indicate that the percentage difference is most pronounced for discretionary drugs, based on drug classifications originally developed in Chandra et al (2010), while being less pronounced for acute and chronic drugs.[25]

In Table B.5, I present additional results that are consistent with my main findings, but which are based off alternate approaches. The first panel leverages the switch from private to public Medicaid at 65, but is restricted to the 1999-2005 period, when Medicaid-only and dual-eligible enrollees both received drug coverage through Medicaid; dual-eligibles only began receiving drug coverage through Medicare, rather than Medicaid, following the 2006 introduction of Part D. The bottom panel, meanwhile, leverages my secondary identification strategy, relating to private Medicaid enrollment mandates.[26]

Building on these results, I then consider the effect of the October 2011 prescription drug carve-in. To account for concurrent changes unrelated to the carve-in, I introduce

[24] Starc and Town (2015) examine differences between integrated and non-integrated drug benefits, for the Medicare rather than Medicaid setting, looking to MA-PD and stand-alone Part D plans respectively. They introduce a structural model to estimate counterfactual Part D spending, in the event drug spillovers were internalized by Part D plans. However, as in Medicaid, these two Medicare coverage types differ on additional dimensions apart from drug and medical cost integration, making it difficult to separate out that particular effect.

[25] Acute care drugs here are defined as those for which non-adherence could cause an adverse event within a couple of months. Chronic care drugs are ones for which non-adherence could cause an adverse event within a year. Finally, discretionary drugs are defined as those not typically associated with adverse events.

[26] The effect on spending appears to be more modest in the top panel than in the bottom, which could be attributable to differences in the underlying treatment groups. This could also be attributable to effects of going from private to public Medicaid (from the top panel) being non-symmetric to those going from public to private Medicaid (in the bottom panel). One explanation for this could be inertia, as it could go against downgrading to less expensive drugs, but not go against upgrading to more expensive ones.

a control group consisting of individuals in public Medicaid. I also restrict my analyses to the period immediately around the carve-in, throughout which the set of active plans effectively remains fixed, along with underlying plan enrollment; this ensures that my results are robust to potential changes in plan or enrollment composition. Unfortunately, as my data only extends through December 2011, my analyses will primarily capture short-run and not long-run effects. For these analyses, the underlying level of observation is at a person-month level, with outcome measures denoted accordingly. In addition, these analyses employ a standardized spending measure, based off prevailing drug rates under public Medicaid, so that measured rates are invariant to the identity of the payer (particularly, whether it is public or private).[27]

The effect of the carve-in is documented in Figure 8, indicating a substantial reduction in drug expenditures among those in private Medicaid, while showing no concurrent changes in public Medicaid. This finding is confirmed by additional regression results, presented in Table 9, which also indicate that the reduction is driven primarily by shifts to less expensive drugs and not by fewer prescriptions filled. Specifically, I find that prescription drug costs decrease by $78 (or 29%) as of three months following the carve-in, or by 17% when looking at spending in terms of logs. In line with theoretical expectations, I find that the reduction in spending is least pronounced for drugs likely to produce acute care offsets.[28] Finally, in results not shown, I find no differential effect on mental health related drugs in comparison to other non-acute care drugs, even though these drugs could theoretically substitute for analogous medical care; this result may be a function of the particular institutional setting, given that mental health services are carved-out of New York's private Medicaid contracts. I find no evidence of differential pre-trends, although I do observe a meaningful post-trend, which suggests the effect of the carve-in is not instantaneous.

To better understand the mechanisms driving this effect, I examine the concurrent effect on number of prescriptions filled. First, I find that the observed drug cost reductions are coming through changes in drug composition, particularly through greater use of generics, rather than through decreases in total drugs consumed. Specifically, as shown in Table 10, the carve-in appears to produce a 35% increase in generic prescriptions and a 20% increase in prescriptions altogether, alongside a 37% decrease in branded prescriptions. The decrease in branded drug usage appears to largely come from substitution to non-equivalent generics,

[27] Such standardization is also necessary given that actual prices are missing in the private Medicaid claims.

[28] Since theoretically, plans may shift utilization to carved-out prescription drugs to reduce utilization of carved-in services, the carve-in of drug services could plausibly result in a countervailing increase in medical costs. However, I find no empirical evidence for such an increase, particularly when it comes to inpatient services. Altogether, I can rule out an inpatient utilization increase in excess of 5% of the baseline. One notable limitation of this result is that capture short-run effects, and so does not rule out possible long-run offsets.

given that only about 20% of the overall branded decrease comes from drugs with generic bioequivalents. Substitution may also take place within the set of branded drugs, to relatively less expensive options, given that branded drug spending decreases by a greater percentage than total branded prescriptions.

In addition, in results not shown here, I consider additional mechanisms by which plans affect drug use. First, I find that the observed effect cannot be explained by a shift to different providers, as my results are not sensitive to the inclusion of provider fixed effects (corresponding to the ID of the prescribing physician). At the same time, I find that my results could plausibly be accounted for by formulary changes under the carve-in, from a uniform state design to a custom plan-specific one. First, plan-determined formularies are known to be relatively more restrictive in coverage of branded drugs, with the aim of encouraging generic substitution, consistent with the actual effect that I observe. Plan-determined formularies are also known to impose additional utilization restrictions on relatively costly drugs, including step therapy and authorization requirements. This could explain shifts that I observe to less expensive drugs, even within the branded drug category.

Finally, I find that the carve-in of prescription drugs, and the resulting decreases in drug costs, translate into lower overall fiscal spending by governments. As shown in the final column of Table 9, the carve-in results in a highly significant $163 (or 16%) decrease in monthly fiscal costs per beneficiary. This estimate provides empirical support for existing theory, showing that incomplete contracting can indeed substantially increase contracting costs. Moreover, since this estimate reflects only one type of incomplete contracting, it may understate the combined impact across all forms of incomplete contracting.

5.7 Heterogeneity Across Plans

There could be heterogeneity across contractors, within Medicaid as well as more broadly, in health utilization, outcomes, and overall efficiency. Differences in contractor performance could be driven by heterogeneity in a variety of underlying characteristics. For tractability, I focus my analyses on a single characteristic of theoretical well as policy interest: a private plan's for-profit status. The implications of for-profit status have been closely examined in the hospital context (Duggan 2000), but are less well understood in the insurance context (Dafny et al 2013) and in other areas of contracting. Unfortunately, in the private Medicaid setting, for-profit status is correlated with a variety of other characteristics, as for-profit insurers tend to be large, multi-state entities, while not-for-profit insurers tend to be smaller and have more limited geographic and enrollee scope (some only offer Medicaid products, for example). As such, these results on for-profit status should be viewed as suggestive, rather than causal, and considered as a starting point for future work.

I first examine whether for-profit plans attained lower hospital utilization in comparison to not-for-profit ones. To identify this effect, independent of patient composition differences across plan types, I leverage my primary identification strategy of involuntary disenrollment from private Medicaid, at 65; individuals will converge to the same type of coverage, post-65, regardless of whether they were in a for-profit or not-for-profit plan, pre-65. Using this strategy, I can identify treatment differences between these plans (based on differential changes at 65), as well as selection differences (based on different levels of utilization, post-65). In these analyses, I restrict the sample to those in Medicaid as of age 63, and also denote initial for-profit enrollment status as of that age.

I find that for-profit insurers are associated with lower inpatient utilization, as shown in Table 11. Altogether, the relevant point estimates imply 20-30% lower relative inpatient utilization under for-profit in comparison to not-for-profit plans (based on the relevant interaction term of Init. Priv and For-Profit). In Table 12, meanwhile, I show that these differences in inpatient utilization stem disproportionately from emergency and elective visits. These analyses are similar in flavor to the ongoing work of Geruso, Layton, and Wallace (2017), who examine cross-plan differences by leveraging random assignment of Medicaid beneficiaries across private plans. An advantage of my approach is that it is based off the full set of individuals in private Medicaid, immediately before 65, while a methodological drawback is lack of truly randomized plan assignment.[29]

6 Specification Checks

First, I look for possible differential pre-trends in my outcome variables, prior to 65. Consistent with my identifying assumptions, in Tables B.2, B.6, and B.7, I find no statistical evidence of such pre-trends across my treatment and control groups prior to 65. Further, I find that a sharp and discontinuous change only arises right at 65. Finally, these tables indicate that the main inpatient effect becomes more pronounced over the post-65 period. This suggests this effect isn't being driven by pent-up demand, under which the effect size would be expected to attenuate over time. To the contrary, for prevention-sensitive care such as readmissions, the effect could be expected to come with a lag, given that the benefits of prevention could similarly be delayed; this is consistent with the observed results for readmissions. Altogether, these findings suggest that the long run effect of switching from

[29] Specifically, the sample in Geruso, Layton, and Wallace is limited to beneficiaries randomly assigned to plans, due to lack of active choice, who comprise only about 5-10% of all those in private Medicaid in New York. Given that this sample accounts for a small fraction of all Medicaid enrollment, it may not be altogether generalizable to private Medicaid as a whole.

private to public Medicaid could exceed its shorter-run impact.

Consistent with Altonji et al (2005), I also examine the sensitivity of my main results to the age bandwidth and controls chosen. In Table B.8, I show that my results hold up when focusing in on narrower sets of bandwidths (64 to 66, and 64.5 to 65.5). Meanwhile, in Table B.9, I show that the results hold up under different types of age controls, including linear, quadratic, and cubic. Finally, as shown in Table B.10, I find the results to be robust to treatment and control group specific age controls, and also to separate age controls on either side of the discontinuity.

7 Conclusion

While government contracting is pervasive, there is limited understanding of the magnitude and incidence of resulting efficiency gains, as well as of the factors limiting their eventual pass-through to either governments or constituents. Looking to New York State's Medicaid program, I find that privatization may produce efficiency improvements through inpatient care reductions, although these reductions are partly offset by outpatient care increases. I also find that the inpatient reductions under private Medicaid are more modest than previously estimated for private Medicare, which I posit is due to tighter regulatory constraints on private Medicaid plans, specifically around when and how they can manage care. In addition, I find that program beneficiaries may benefit from Medicaid privatization, through improved outpatient care access and lack of adverse impact on other quality indicators. Simultaneously, I find that governments lose out through fiscal cost increases, which are compounded by incomplete contracting; to this end, exclusion of certain services from plan contracts leads to cost-shifting towards these same services, which is then reversed through their subsequent inclusion in contracts.

I also identify potential mechanisms for private Medicaid's effect on care, although further research on this is needed. Specifically, future work could decompose the effect of incentives (in this setting, capitation) from that of proprietary technology (in this setting, care management). Future work could also tackle a few of the notable limitations to my analyses. For example, I am unable to track administrative costs associated with private Medicaid plans, which could exceed those in public Medicaid.[30] I am also unable to track actual provider prices under private Medicaid, which is unfortunate given that these are germane to plan costs as well as provider revenues. Furthermore, my measures of care quality are incomplete, which prevents me from fully identifying effects on patients or on general welfare. Finally,

[30] In Medicare, for example, administrative expenses account for 9% of all private plan spending, compared to only 2% of traditional Medicare spending (MedPAC 2013)

my analysis focuses on a narrow population, looks primarily at the short-run, and is also specific to a single state. Given these limitations, future research can more closely identify heterogeneity in impact across different types of patients, plans, and geographies.

My study carries a number of policy implications for federal as well as state governments. First, these findings suggest that outpatient care access under public Medicaid is inadequate and in need of improvement, given substantially greater use of outpatient services under private Medicaid as well as traditional public Medicare. In addition, as contract completeness appear to be a key determinant of pass-through, this suggests that governments should broaden the set of medical services covered by private plans, while also expanding the set of performance standards in place. My results also point to possible benefits from greater technological insourcing, as this could allow states to enjoy the indirect benefits of contracting and at the same time avoid its inherent costs. For example, states could introduce elements of managed care directly into their public programs, in place of wholesale outsourcing to a private contractor, as Texas Medicaid has done through its PCCM initiative (Lewin Group 2004).

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Figure 1: National Medicaid Expenditures Over Time

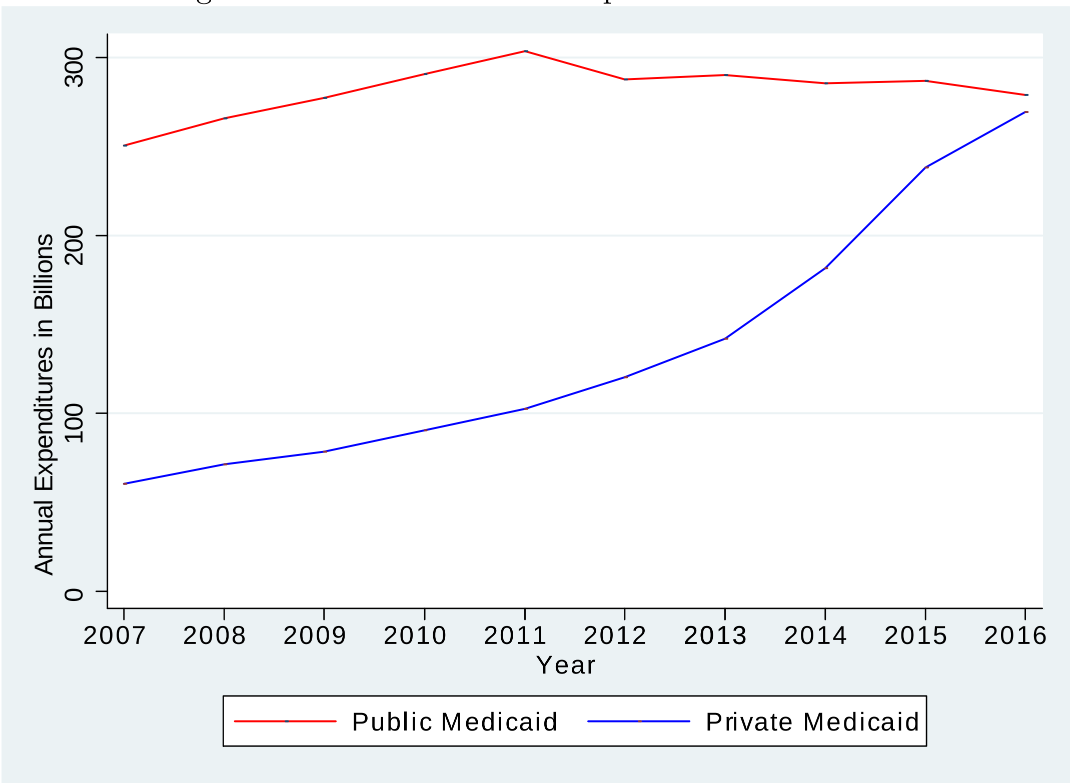

Figure 2: Private Medicaid Status Around Age Threshold

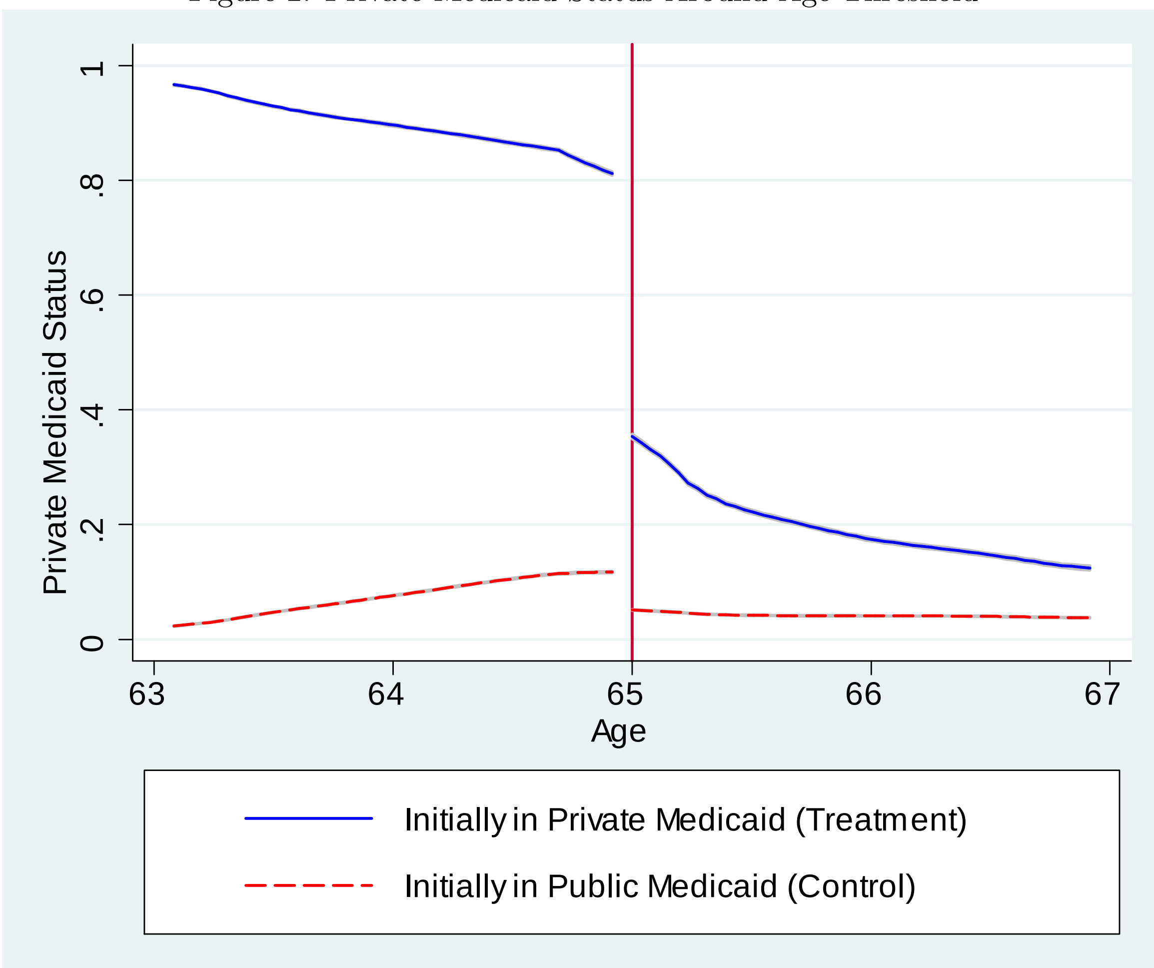

Figure 3: Total Inpatient Days Around Age Threshold

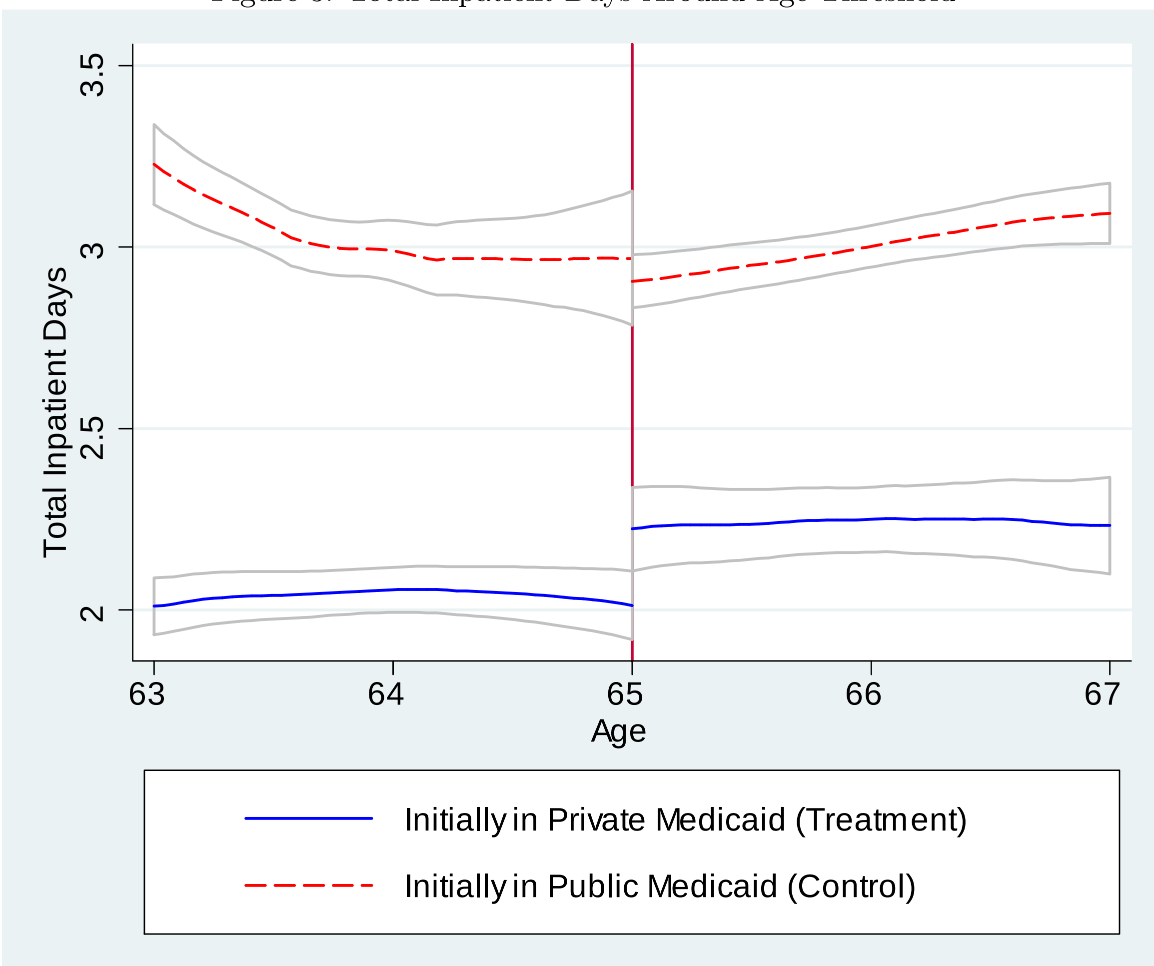

Figure 4: Number of Inpatient Visits Around Age Threshold

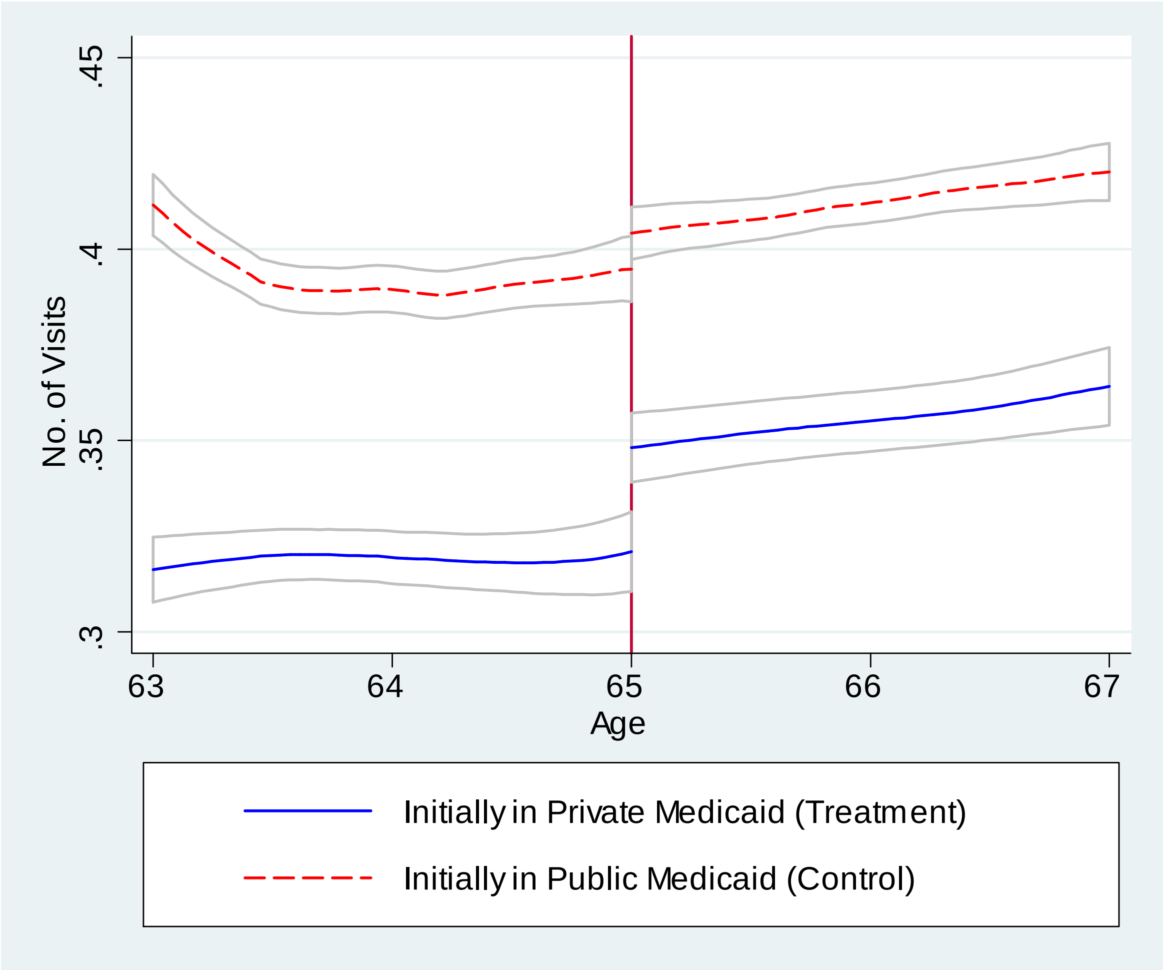

Figure 5: Number of Inpatient Readmissions Around Age Threshold

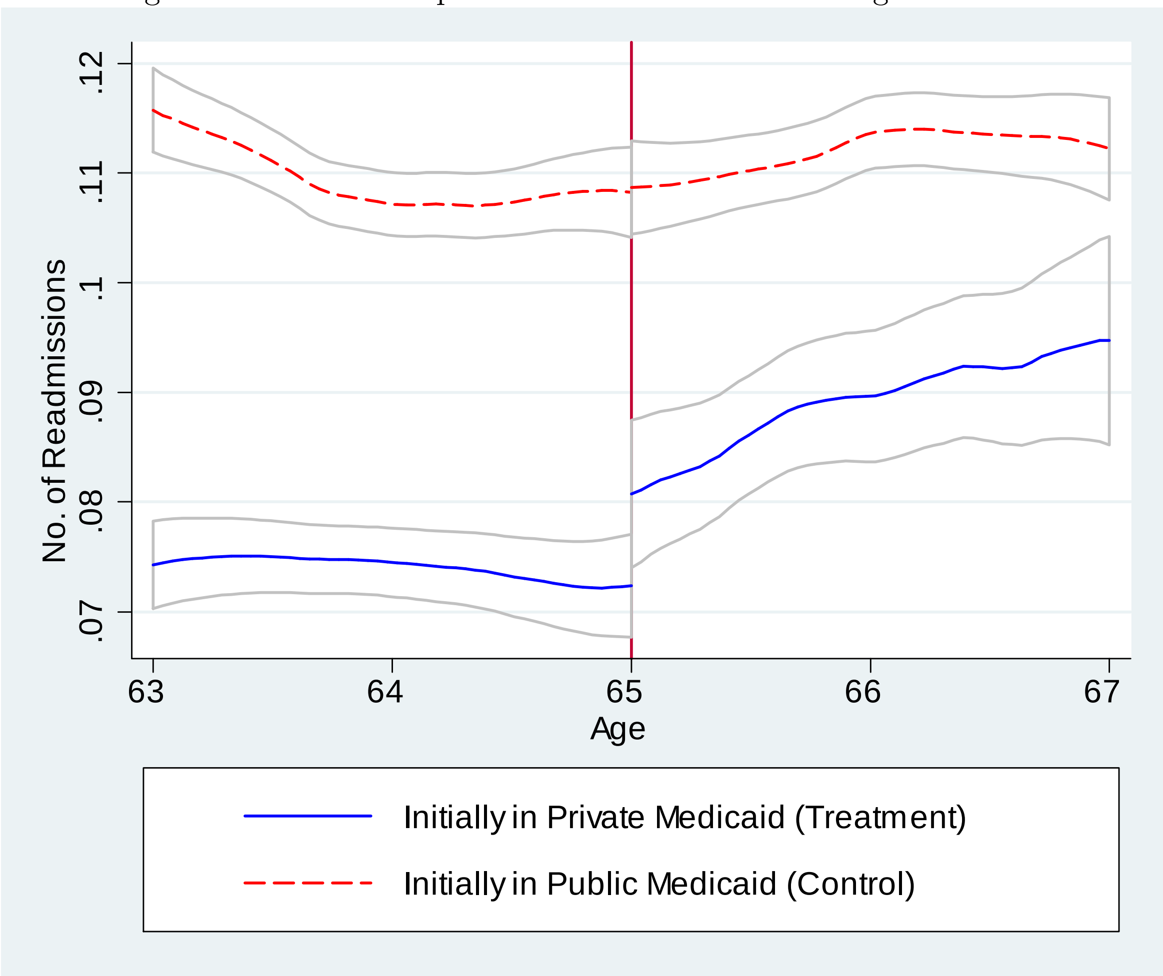

Figure 6: Number of Inpatient Non-Readmissions Around Age Threshold

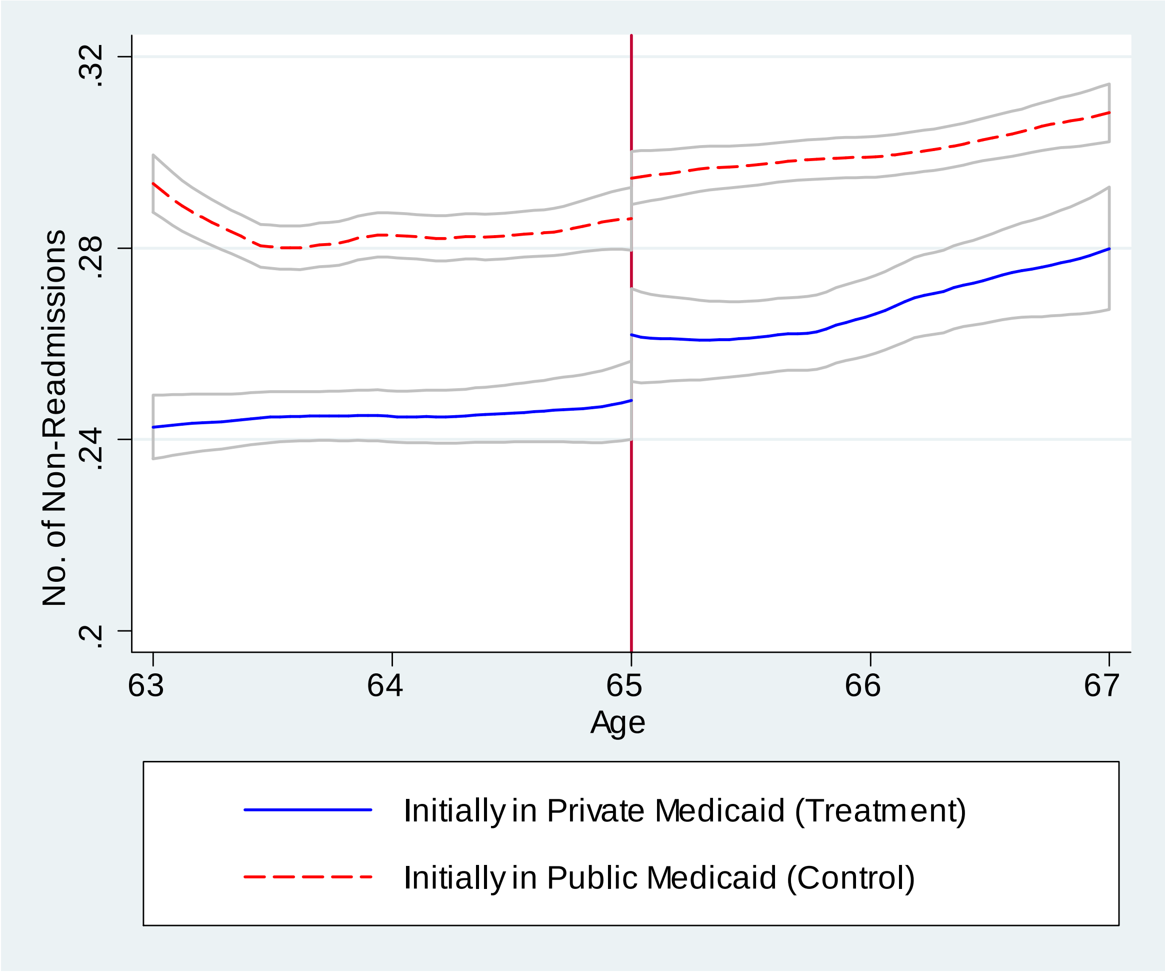

Figure 7: Core Outpatient Spending Around Age Threshold

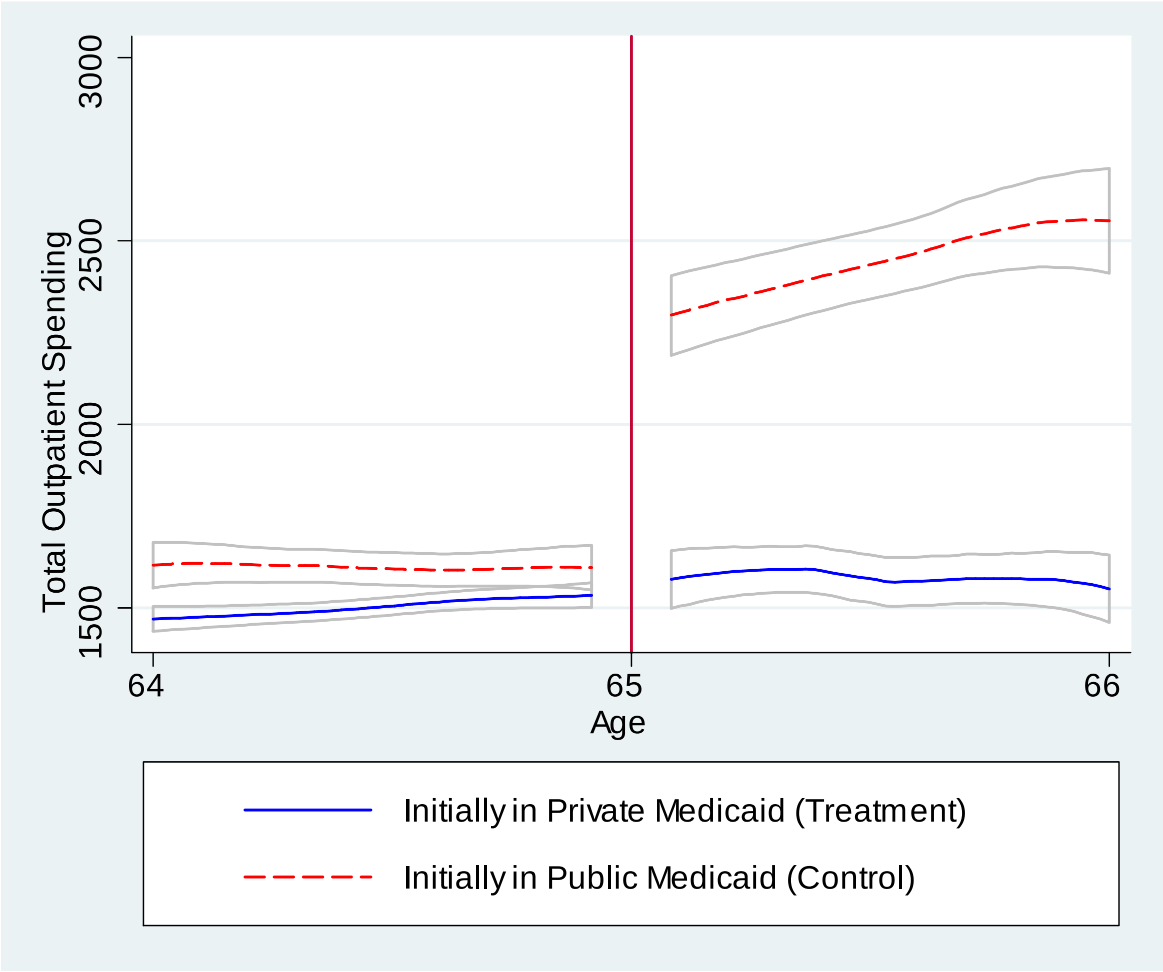

Figure 8: Prescription Drug Utilization: Pre and Post Carve-In

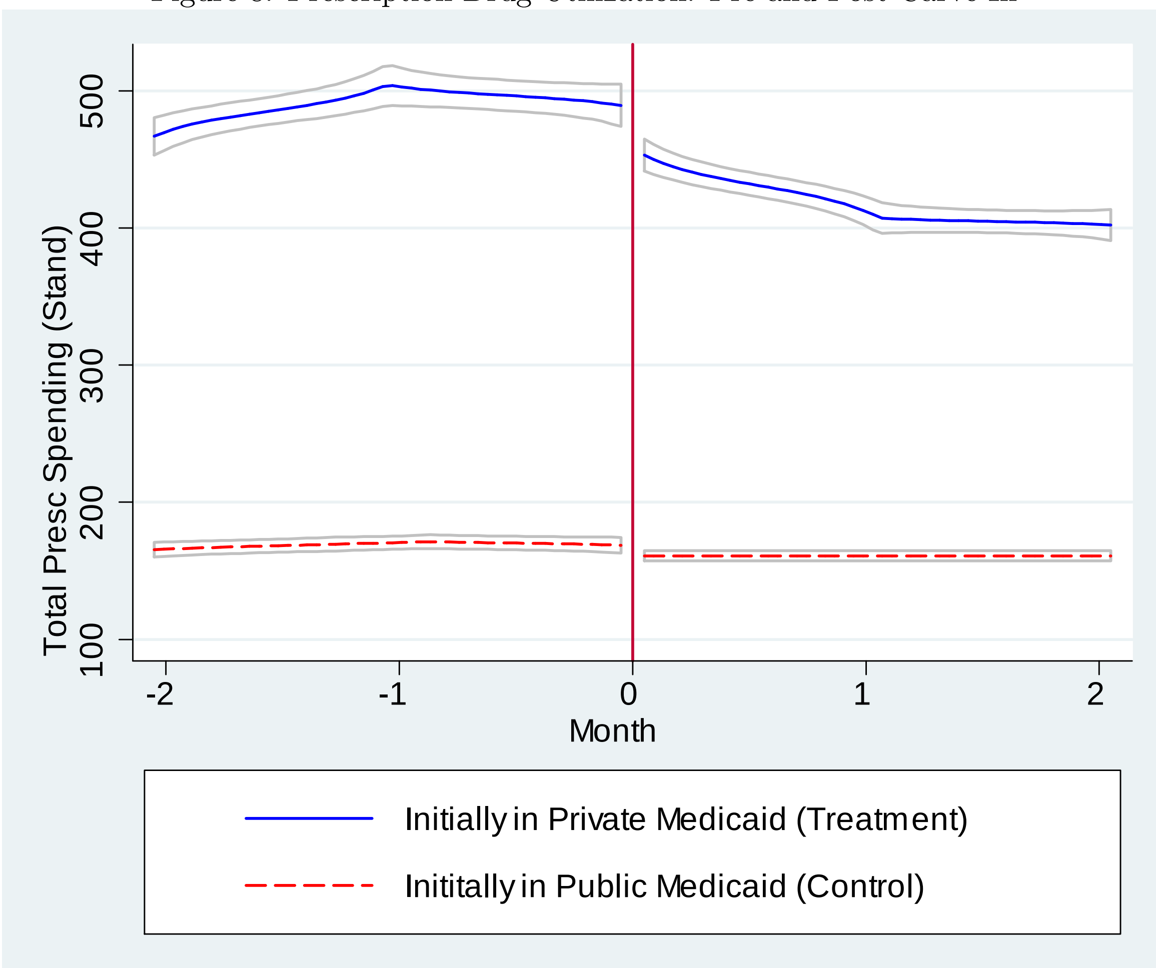

Table 1: Summary Statistics

	Initially Private	Initially Public
Inp Utilization		
Hosp Visits	0.331	0.402
	(2.209)	(2.474)
LOS	2.107	3.022
	(23.284)	(38.844)
Num Proc	0.722	0.849
	(7.047)	(7.626)
Charges	9,896	11,371
	(117,421)	(129,294)
Inp Composition		
Readmissions	0.079	0.111
	(1.141)	(1.391)
Surgeries	0.024	0.044
	(0.539)	(0.736)
Outpatient Utilization		
Office Visits	8.091	5.563
	(13.900)	(14,122)
Outp Serv Spending	1,470	1,784
	(4,485)	(4,729)
Outp ER Visits	0.666	0.770
	(3.496)	(3.863)
Overall Spending		
Total	14,652	44,693
	(24,623)	(110,323)
Carved-in Services	10,262	26,314
	(22,173)	(100,927)
Carved-out Services	4,390	18,380
	(8,777)	(43,379)
Pharma		
No. of Presc	4.477	4.736
	(4.364)	(4.823)
Presc Spending	3,171	3,555
	(4,766)	(5,538)
N	944,405	2,965,365

Notes: Panel presents summary statistics for my primary treatment and control groups (those in private and public Medicaid at age 63, respectively). This data covers the 1999-2010 period, and is aggregated at the person-month level; however, the measures shown here have been annualized. The sample is restricted to the age range between 63 and 67; it is further restricted to those who were in New York and in Medicaid-only at 63, by virtue of disability. This data was constructed using discharge-level hospital data from New York State, outpatient and pharmaceutical Medicaid claims data, and person-month level Medicaid enrollment records from CMS; these datasets were linked using SSN and other fields, and subsequently aggregated to a person-month level. Sample inclusion is not conditional on utilization.

Table 2: Effect of Age 65 of Private Medicaid Status

	(1) Private Medicaid Enrolled	(2) Dual Medicaid and Medicare Enrolled
Mean (Pre-65)	0.298	0.026
	(0.457)	(0.159)
Init. Private*Post 65	-0.659***	0.011*
	(0.008)	(0.006)
Post 65	-0.029***	0.789***
	(0.005)	(0.009)
Initially Private	0.798***	0.010***
	(0.006)	(0.002)
Restriction	Ages 63 to 67	
N	3,909,770	
Init. Private*Post 65	-0.662***	0.019***
	(0.003)	(0.003)
Post 65	0.012***	0.800***
	(0.001)	(0.002)
Initially Private	0.897***	0.008***
	(0.002)	(0.001)
Restriction	Ages 64 to 66	
N	2,329,769	
$Init.\widehat{Private * Post}65$	-0.646***	0.015***
	(0.006)	(0.005)
Post 65	0.010***	0.764***
	(0.002)	(0.002)
$Init,\widehat{Private}$	0.952***	0.050***
	(0.011)	(0.008)
Restriction	Ages 64 to 66	
N	2,329,769	

Notes: Table presents results of my first-stage regression, a linear model with private Medicaid enrollment status as the outcome and the interaction of Init. Private*Post 65 as the instrument of interest. Init Private is defined as those enrolled in private Medicaid, at the age of 63. The unit of observation is at the person-month level, for the 1999-2010 period. Year-quarter, county, and gender fixed effects are included as part of the analysis. The sample is restricted to those enrolled in Medicaid-only at 63, by virtue of disability; the sample is also restricted to New York State only. Finally, the sample is restricted to the 63 to 67 age range. Standard-errors are clustered at the individual level. The original Medicaid enrollment administrative data is taken from CMS.

Table 3: Effect of Private Medicaid on Inpatient Utilization

	(1) Tot Visits	(2) Tot Length Stay	(3) Tot Procs	(4) Tot Chrg	(5) Log Tot Chrg
Mean	0.385	2.801	0.818	11,015	0.345
	(2.413)	(35.714)	(7.491)	(126,530)	(2.040)
Init Private*Post 65	0.034***	0.335***	0.065***	1,303***	0.025***
	(0.008)	(0.088)	(0.024)	(402)	(0.007)
Post 65	0.010	-0.064	0.037**	335	0.011**
	(0.006)	(0.101)	(0.019)	(316)	(0.005)
Init. Private	-0.107***	-1.201***	-0.204***	-4,408***	-0.092***
	(0.007)	(0.067)	(0.018)	(284)	(0.006)
Sample Restriction			Ages 63 to 67		
N			3,909,770		
Init Private*Post 65	0.039***	0.479***	0.106***	1,859***	0.028***
	(0.009)	(0.103)	(0.026)	(441)	(0.007)
Post 65	0.010	-0.003	0.036	550	0.014**
	(0.008)	(0.139)	(0.023)	(393)	(0.006)
Init. Private	-0.133***	-1.398***	-0.270***	-5,180***	-0.113***
	(0.008)	(0.085)	(0.021)	(332)	(0.007)
Sample Restriction			Ages 64 to 66		
N			2,329,769		
$\widehat{Init.Private * Post}65$	0.058***	0.370*	0.098*	1,732*	0.041***
	(0.018)	(0.216)	(0.054)	(949)	(0.015)
Post 65	0.005	0.026	0.038	581	0.011
	(0.009)	(0.154)	(0.026)	(439)	(0.007)
$\widehat{Init.Private}$	0.092**	0.643	0.170	349	0.077**
	(0.042)	(0.429)	(0.107)	(1568)	(0.035)
Sample Restriction			Ages 64 to 66		
N			2,329,769		

Notes: Table presents linear regression models, where outcome variables are annualized measures of individual inpatient utilization. The key variable of interest is Init Private*Post 65, which captures the effect of involuntary switching from private to public Medicaid; the share of this group actually switching corresponds to about 65 %, based on my first stage estimates. Init Private is defined as those enrolled in private Medicaid, at the age of 63. Year-quarter, county, and gender fixed effects are included as part of the analysis. The unit of observation is at the person-month level, for the 1999-2010 period. The sample is restricted to those enrolled in Medicaid-only at 63, by virtue of disability; the sample is also restricted to New York State only. Finally, the sample is restricted to the 63 to 67 age range. This data was constructed using discharge-level hospital data from New York State and person-month level Medicaid enrollment records from CMS; these two datasets were linked using SSN and other fields, and subsequently aggregated to a person-month level. Sample inclusion is not conditional on utilization.

Table 4: Mechanisms for Inpatient Utilization Impact

	Treat Grp. Effect	Mean (Ex-Ante)	Percentage Effect	N
Age Restriction:		63 to 67		
Elective Hosp	0.006***	0.045	13.3%	3,909,770
	(0.002)		(4.4%)	
Non-Elective Hosp	0.028***	0.340	8.2%	3,909,770
	(0.008)		(2.4%)	
Emergency Hosp	0.027***	0.313	8.6%	3,909,770
	(0.007)		(2.2%)	
Non-Emergency Hosp	0.007**	0.072	9.7%	3,909,770
	(0.003)		(4.2%)	
Surgery Hosp	-0.002	0.039	-5.1%	3,909,770
	(0.002)		(5.1%)	
Non-Surgery Hosp	0.036***	0.346	10.4%	3,909,770
	(0.008)		(2.3%)	
Readmission Hosp	0.018***	0.103	17.5%	3,909,770
	(0.005)		(4.9%)	
Non-Readmission Hosp	0.016***	0.282	5.7%	3,909,770
	(0.005)		(1.8%)	
Distance to Hospital: Miles	0.237**	5.499	4.3%	264,166
	(0.113)		(2.1%)	
Distance to Hospital: Time	0.494**	15.706	3.1%	264,166
	(0.252)		(1.6%)	

Notes: Table presents linear regression models, where outcome variables are annualized measures of individual inpatient utilization. The coefficients presented correspond to the key variable of interest, Init Private*Post 65, which captures the effect of involuntary switching from private to public Medicaid; the share of this group actually switching corresponds to about 65 %, based on my first stage estimates. Init Private is defined as those enrolled in private Medicaid, at the age of 63. Year-quarter, county, and gender fixed effects are included as part of the analysis. The unit of observation is at the person-month level, for the 1999-2010 period. The sample is restricted to those enrolled in Medicaid-only at 63, by virtue of disability; the sample is also restricted to New York State only. Finally, the sample is restricted to the 63 to 67 age range. This data was constructed using discharge-level hospital data from New York State and person-month level Medicaid enrollment records from CMS; these two datasets were linked using SSN and other fields, and subsequently aggregated to a person-month level. Sample inclusion is not conditional on utilization.

Table 5: Effect on Outpatient Care

	Treat Grp. Effect	***Mean* (Ex-Ante)**	***Percentage* Effect**	***N***
	Panel A: Outpatient Hospital Setting			
Outp ER Visits	.055**	0.736	7.5%	2,294,206
	(0.020)		(2.7%)	
Amb Surg Visits	-0.001	0.206	.5%	2,294,206
	(0.007)		(3.4%)	
Year Range	2005-2010			
	Panel B: General Outpatient Setting			
Non-Instit. Outp Spend	-646.3***	1,626	-39.7%	180,003
	(105.5)		(6.5%)	
Outp Spend Breakdown				
Surg Amt	-83.7***	159	-52.6%	180,003
	(15.1)		(9.5%)	
Diag Amt	-57.2***	95	-60.3%	180,003
	(8.4)		(8.9%)	
Imaging Amt	-40.1	268	-15.0%	180,003
	(33.4)		(12.5%)	
Preventive Care	-21.6***	51	-42.5%	180,003
	(3.3)		(6.5%)	
Office Visits	-125.4***	240	-52.3%	180,003
	(10.2)		(4.3%)	
Outp Visit Breakdown				
Office Visits	-4.2***	7.2	-58.6%	180,003
	(0.2)		(4.0%)	
Minutes in Office	-74.0***	132	-56.1%	180,003
	(5.5)		(4.2%)	
Year Range	2008-2010			

Notes: Table presents linear regression models, where outcome variables are annualized measures of individual outpatient utilization. The coefficients presented correspond to the key variable of interest, Init Private*Post 65, which captures the effect of involuntary switching from private to public Medicaid; the share of this group actually switching corresponds to about 65 %, based on my first stage estimates. Year-quarter, county, and gender fixed effects are included as part of the analysis. The unit of observation is at the person-month level, for the 2008-2010 period. The sample is restricted to those enrolled in Medicaid-only at 64.5, by virtue of disability; the sample is also restricted to New York State only. Finally, the sample is restricted to the 64.5 to 65.5 age range. This data was constructed using outpatient Medicaid and Medicare claims data from New York State and person-month level Medicaid enrollment records from CMS; these datasets were linked using unique beneficiary ID's, and subsequently aggregated to a person-month level. Sample inclusion is not conditional on utilization.

Table 6: Effect on Quality of Care

	Treat Grp. Effect	***Mean* (Ex-Ante)**	***Percentage* Effect**	***N***
Age Range		*63 to 67*		
Outcome Measures: CMS				
MI Mort	0.065*	14.130	0.4%	208,490
	(0.036)		(0.3%)	
HF Mort	0.021	10.450	0.2%	214,669
	(0.023)		(0.2%)	
PN Mort	0.045	11.065	0.4%	214,789
	(0.034)		(0.3%)	
MI Readm	0.011	19.333	0.1%	192,103
	(0.023)		(0.1%)	
HF Readm	-0.012	25.277	0.0%	214,700
	(0.033)		(0.1%)	
Process Measures: CMS				
Overall	0.002**	0.853	0.2%	197,361
	(0.001)		(0.1%)	
Heart Attack	0.002**	0.927	0.2%	197,361
	(0.001)		(0.1%)	
Heart Failure	0.003**	0.882	0.3%	197,361
	(0.001)		(0.1%)	
Pneumonia	0.003	0.737	0.4%	197,361
	(0.002)		(0.3%)	
Hospital Char Measure				
Major Teaching Hospital	0.012	0.560	2.1%	228,642
	(0.008)		(1.4%)	
Disch Based Measures				
Conditional Readm	0.014**	0.215	6.5%	197,925
	(0.006)		(2.8%)	
Conditional Prevent ER	-0.001	0.493	-0.2%	266,640
	(0.006)		(1.2%)	

Notes: Table presents linear regression models, where outcome variables are measures of inpatient care quality, where the underlying unit of observation is at an individual hospitalization level. The coefficients presented correspond to the key variable of interest, Init Private*Post 65, which captures the effect of involuntary switching from private to public Medicaid; the share of this group actually switching corresponds to about 65 %, based on my first stage estimates. Init Private is defined as those enrolled in private Medicaid, at the age of 63. Year-quarter, county, and gender fixed effects are included as part of the analysis. The data covers the 1999-2010 period. The sample is restricted to those enrolled in Medicaid-only at 63, by virtue of disability; the sample is also restricted to New York State only. Finally, the sample is restricted to the 63 to 67 age range. This data was constructed using discharge-level hospital data from New York State and person-month level Medicaid enrollment records from CMS; these two datasets were linked using SSN and other fields. Sample inclusion is conditional on utilization.

Table 7: Effect on Fiscal Spending

	(1)	(2)	(3)
		Spending	
	Overall	*Carved-In*	*Carved-Out*
Init. Private*Pre 65	4,760***	2,640**	2,120***
	-1,370	(1,207)	(677)
Pre 65	-6,854***	-3,967***	-2,886***
	-1,319	(1,151)	(668)
Initially Private	-38,433***	-20,959***	-17,474***
	(1,484)	(1,217)	(828)
Mean	26,979	16,951	10,027
Percentage Effect	17.6%	15.6%	21.1%
	(5.1%)	(7.1%)	(6.8%)
Year Range:		2008-2010	
N		180,003	

Notes: Table presents linear regression models, where outcome variable is annualized government Medicaid spending, per-enrollee. The coefficients presented correspond to the key variable of interest, Init Private*Pre 65, which captures the effect of involuntary switching from public to private Medicaid; the share of this group actually switching corresponds to about 65 %, based on my first stage estimates. Init Private is defined as those enrolled in private Medicaid, at the age of 64.5. Year-quarter, county, and gender fixed effects are included as part of the analysis. The data covers the 2008-2010 period, with the unit of observation at the person-month level. The sample is restricted to those enrolled in Medicaid-only at 64.5, by virtue of disability; the sample is also restricted to New York State only. Finally, the sample is restricted to the 64.5 to 65.5 age range. This data was constructed using Medicaid and Medicare claims and premium payment records and person-month Medicaid enrollment records, both from CMS.

Table 8: Effect on Carved-Out Prescription Drug Spending

	(1)	(2)		
	All Presc	Presc; By Category		
		Acute	*Chronic*	*Discr.*
Panel A: Log Presc Spending				
Initially Private*Pre 65	0.512***	0.286***	0.294***	0.461***
	(0.065)	(0.067)	(0.061)	(0.065)
Pre 65	-0.402***	-0.279***	-0.318***	-0.352***
	(0.058)	(0.058)	(0.054)	(0.058)
Initially Private	0.057	-0.376***	0.221***	0.228***
	(0.076)	(0.079)	(0.072)	(0.075)
Mean	6.08	3.52	3.73	4.62
N	180,003			
Panel B: Number of Prescriptions				
Initially Private*Pre 65	0.593***	0.156***	0.183***	0.233***
	(0.102)	(0.034)	(0.036)	(0.060)
Pre 65	-0.240***	-0.194***	-0.240***	-0.199***
	(0.093)	(0.031)	(0.033)	(0.054)
Initially Private	-0.642***	-0.280***	-0.080*	-0.272***
	(0.119)	(0.039)	(0.042)	(0.072)
Mean	5.24	1.153	1.522	2.6
N	180,003			

Notes: Table presents linear regression models, where outcome variables are monthly measures of individual drug utilization. The unit of observation is at the person-month level. The coefficients presented correspond to the key variable of interest, Init Private*Pre 65, which captures the effect of involuntary switching from public to private Medicaid; the share of this group actually switching corresponds to about 65 %, based on my first stage estimates. Init Private is defined as those enrolled in private Medicaid, at the age of 64.5. Year-quarter, county, and gender fixed effects are included as part of the analysis. The data covers the 2008-2010 period. The sample is restricted to those enrolled in Medicaid-only at 64.5, by virtue of disability; the sample is also restricted to New York State only. Finally, the sample is restricted to the 64.5 to 65.5 age range. This data was constructed using Medicaid and Medicare drug claims data and person-month Medicaid enrollment records, both from CMS, and is subsequently aggregated to the person-month level.

Table 9: Effect from Carving-In Prescription Drugs on Spending

	(1) All	(2) Generic	(3) Branded	(4) Acute	(5) Chron.	(6) Discr.	(7) Tot Med. Spend
Panel A: Spending in Levels							
Pre-2 Mon*Init. Priv Med.	12.421**	3.904***	8.755	1.973	2.954	7.751***	14.62**
	(6.090)	(0.645)	(6.026)	(2.304)	(4.911)	(2.528)	(6.22)
Pre-1 Mon*Init. Priv Med.				Baseline			
Carve-in Mon*Init. Priv Med.	-29.482***	6.445***	-37.553***	-7.649***	-17.100***	-6.421**	-163.84***
	(6.390)	(0.668)	(6.250)	(2.362)	(4.819)	(2.987)	(7.70)
Post-1 Mon*Init. Priv Med.	-74.412***	11.512***	-87.356***	-19.485***	-25.046***	-31.499***	-163.18***
	(6.998)	(0.797)	(6.888)	(2.676)	(5.500)	(2.775)	(7.76)
Post-2 Mon*Init. Priv Med.	-78.350***	15.978***	-95.615***	-15.510***	-29.904***	-35.554***	-162.90***
	(7.119)	(0.829)	(7.000)	(2.506)	(5.750)	(2.677)	(7.73)
Mean	266	32	233	89	71	104	1023
N				673,872			
Panel B: Spending in Logs							
Pre-2 Mon*Init. Priv Med.	0.044***	0.067***	0.025	0.006	0.007	0.068***	0.033**
	(0.015)	(0.011)	(0.017)	(0.014)	(0.013)	(0.015)	(0.014)
Pre-1 Mon*Init. Priv Med.				Baseline			
Carve-in Mon*Init. Priv Med.	-0.067***	0.059***	-0.118***	-0.061***	-0.068***	0.023	-0.074***
	(0.016)	(0.012)	(0.018)	(0.015)	(0.014)	(0.015)	(0.016)
Post-1 Mon*Init. Priv Med.	-0.155***	0.145***	-0.392***	-0.100***	-0.133***	-0.116***	-0.162***
	(0.016)	(0.012)	(0.019)	(0.015)	(0.014)	(0.016)	(0.016)
Post-2 Mon*Init. Priv Med.	-0.175***	0.214***	-0.511***	-0.059***	-0.214***	-0.145***	-0.180***
	(0.016)	(0.013)	(0.020)	(0.015)	(0.014)	(0.016)	(0.016)
N				673,872			

Notes: Table presents linear regression models, where outcome variables are monthly measures of individual drug utilization. The unit of observation is at the person-month level. The measures of interest are monthly dummies, interacted with an indicator for whether someone was in private Medicaid as of July 2011; these measures denote prescription drug utilization among those initially in private Medicaid, relative to those initially in public Medicaid, relative to the baseline month (September 2011). The sample is restricted to those enrolled in Medicaid, by virtue of disability, in either the public or private option, as of July 2011. The sample is also restricted to New York State only. County and gender fixed effects are included as part of the analysis. This data was constructed using claim-level prescription drug utilization and person-month level Medicaid enrollment records from CMS; these two datasets were linked using beneficiary ID's, and subsequently aggregated to a person-month level. Sample inclusion is not conditional on utilization.

Table 10: Effect from Carving-In Prescription Drugs on Prescription Counts

	(1) Total	(2) Generic	(3) Branded	(4) Branded w/Sub	(5) Acute	(6) Chron.	(7) Discr.
Pre-3 Months*Init. Priv Med.	-0.052*	-0.021	-0.022*	-0.019***	-0.033***	-0.029***	0.022
	(0.030)	(0.022)	(0.013)	(0.004)	(0.009)	(0.010)	(0.018)
Pre-2 Months*Init. Priv Med.	0.073**	0.062***	0.019	-0.016***	0.003	0.016	0.061***
	(0.030)	(0.022)	(0.013)	(0.004)	(0.010)	(0.011)	(0.018)
Pre-1 Months*Init. Priv Med.	Baseline						
Carve-in Month*Init. Priv Med..	0.553***	0.367***	-0.070***	-0.050***	-0.006	-0.017	0.317***
	(0.034)	(0.024)	(0.013)	(0.004)	(0.010)	(0.011)	(0.020)
Post-1 Month*Init. Priv Med.	0.518***	0.561***	-0.307***	-0.069***	0.003	0.011	0.233***
	(0.034)	(0.025)	(0.014)	(0.004)	(0.010)	(0.011)	(0.020)
Post-2 Months*Init. Priv Med.	0.699***	0.800***	-0.371***	-0.062***	0.051***	0.025**	0.287***
	(0.035)	(0.026)	(0.014)	(0.004)	(0.010)	(0.011)	(0.021)
Cohort Restriction	Initially in Medicaid						
Age Restriction	60 to 65						
Mean	3.308	2.235	1.002	0.123	0.708	0.795	1.723
N	673,872						

Notes: Table presents linear regression models, where outcome variables are monthly measures of individual drug utilization. The unit of observation is at the person-month level. The measures of interest are monthly dummies, interacted with an indicator for whether someone was in private Medicaid as of July 2011; these measures denote prescription drug utilization among those initially in private Medicaid, relative to those initially in public Medicaid, relative to the baseline month (September 2011). The sample is restricted to those enrolled in Medicaid, by virtue of disability, in either the public or private option, as of July 2011. The sample is also restricted to New York State only. County and gender fixed effects are included as part of the analysis. This data was constructed using claim-level prescription drug utilization and person-month level Medicaid enrollment records from CMS; these two datasets were linked using beneficiary ID's, and subsequently aggregated to a person-month level. Sample inclusion is not conditional on utilization.

Table 11: Inpatient Utilization Differences Based on For-Profit Status

	(1) Tot LOS	(2) Tot Procs	(3) Tot Chrg	(4) Log Tot Chrg	(5) Tot Hosp Visits
For Profit Plan*Post 65	0.307*	0.110**	2,077**	0.030**	0.041**
	(0.157)	(0.052)	(908)	(0.014)	(0.017)
For Profit Plan	-0.527***	-0.157***	-1,940***	-0.069***	-0.080***
	(0.101)	(0.032)	(481)	(0.010)	(0.011)
Post 65	0.161	0.030	835	0.010	0.011
	(0.130)	(0.038)	(629)	(0.010)	(0.012)
Male	0.722***	0.238***	3,384***	0.073***	0.085***
	(0.086)	(0.026)	(407)	(0.008)	(0.010)
Age	2.057	0.658	14,301	-0.223	-0.406
	(3.171)	(0.974)	(15,572)	(0.278)	(0.328)
Age Sq	-0.016	-0.005	-110	0.002	0.003
	(0.025)	(0.008)	(121)	(0.002)	(0.003)
Cohort Restriction	Enrolled in Private Medicaid, at Age 63				
Age Restriction	63 to 67				
Mean	2.107	0.722	9,896	0.302	0.331
	(23.284)	(7.047)	(117,421)	(1.915)	(2.209)
N	936,200				

Notes: Table presents linear regression models, where outcome variables are annualized measures of individual inpatient utilization. The key variable of interest is For Profit*Post 65, which the differential impact of private to public Medicaid switching between those enrolled in for-profit and not-for-profit plans; the share of this group actually switching corresponds to about 65 %, based on my first stage estimates. Init Private is defined as those enrolled in private Medicaid, at the age of 63, and For Profit plan is also based on plan enrollment as of age 63. Year-quarter, county, and gender fixed effects are included as part of the analysis. The unit of observation is at the person-month level, for the 1999-2010 period. The sample is restricted to those enrolled in Medicaid-only at 63, by virtue of disability, who are also in private Medicaid at that age; the sample is also restricted to New York State only. Finally, the sample is restricted to the 63 to 67 age range. This data was constructed using discharge-level hospital data from New York State and person-month level Medicaid enrollment records from CMS; these two datasets were linked using SSN and other fields, and subsequently aggregated to a person-month level. Sample inclusion is not conditional on utilization.

Table 12: Inpatient Composition Differences Based on For-Profit Status

	(1) Elective	(2) Non-Elective	(3) Emergency	(4) Non-Emergency	(5) Surgery	(6) Non-Surgery
For Profit Plan*Post 65	0.008	0.033**	0.035**	0.006	0.004	0.037**
	(0.005)	(0.016)	(0.015)	(0.006)	(0.003)	(0.017)
Post 65	0.013***	-0.001	-0.002	0.013***	-0.004	0.007
	(0.004)	(0.011)	(0.011)	(0.005)	(0.002)	(0.012)
For Profit Plan	-0.006**	-0.075***	-0.076***	-0.005	0.004	-0.077***
	(0.003)	(0.010)	(0.010)	(0.003)	(0.003)	(0.011)
Male	0.006**	0.079***	0.070***	0.015***	.006***	0.079***
	(0.002)	(0.009)	(0.009)	(0.003)	(0.001)	(0.009)
Age	0.109	-0.516*	-0.460	0.053	0.049	-0.455
	(0.098)	(0.302)	(0.286)	(0.123)	(0.069)	(0.315)
AgeSq	-0.001	0.004*	0.004	-0.000	-0.000	0.004
	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)
Cohort Restriction	Enrolled in Private Medicaid, at 63					
Age Restriction	63 to 67					
Mean	0.041	0.289	0.269	0.062	0.024	0.307
Diff. Percentage Effect	19.5%	11.4%	13.0%	9.7%	16.7%	12.1%
	(12.2%)	(5.5%)	(5.6%)	(9.7%)	(12.5%)	(5.5%)
N	936,200					

Notes: Table presents linear regression models, where outcome variables are annualized measures of individual inpatient utilization. The key variable of interest is For Profit*Post 65, capturing the differential impact of private to public Medicaid switching between those enrolled in for-profit and in not-for-profit plans; the share of this group actually switching corresponds to about 65 %, based on my first stage estimates. Init Private is defined as those enrolled in private Medicaid, at the age of 63, and For Profit plan is also based on plan enrollment as of age 63. Year-quarter, county, and gender fixed effects are included as part of the analysis. The unit of observation is at the person-month level, for the 1999-2010 period. The sample is restricted to those enrolled in Medicaid-only at 63, by virtue of disability, who are also in private Medicaid at that age; the sample is also restricted to New York State only. Finally, the sample is restricted to the 63 to 67 age range. This data was constructed using discharge-level hospital data from New York State and person-month level Medicaid enrollment records from CMS; these two datasets were linked using SSN and other fields, and subsequently aggregated to a person-month level. Sample inclusion is not conditional on utilization.

Appendix

Appendix A: Secondary Identification Strategy

Research Design:

In addition to my primary empirical strategy featuring the age 65 threshold, I implement a secondary identification strategy, which leverages involuntary switching into private Medicaid plans under new enrollment requirements, or "mandates". Under these mandates, certain groups of Medicaid recipients were explicitly required to enroll in private plans. My identifying variation is based off county-time heterogeneity in the implementation of these enrollment requirements. For disabled Medicaid recipients, the introduction of these enrollment requirements began in 2005 and continued through the end of my study period.[31]

Altogether, the key instrument here is based on whether an enrollment mandate was already in effect in an individual's county of residence, applying specifically to the disabled population.[32] The timeline of these mandates' introduction across counties is presented in Table A.1. The introduction of these mandates coincided with a substantial increase in managed care enrollment among the disabled/SSI population, as shown in Table A.2.

The first stage regression, for estimating the effect of mandates on private Medicaid enrollment status takes the following form, for individual i, in county c, at time t. I also include flexible controls for age, along with a county specific time trend, as well as county, gender and quarter-year fixed effects.

$$PvtMedicaidStatus_{ict} = \alpha + \beta_0 * PostMandate_{ct} + X_{ict} * \gamma + \varepsilon_{ict}$$

(**Equation #A1**)

Meanwhile, the second stage regression takes the following form, for estimating the effect of private Medicaid on an outcome variable y, for individual i, in county c, at time t. I include a linear control for age, along with gender, year, and county fixed effects. I also include controls for county and group-specific linear trends, across the treatment and control groups.

$$y_{ict} = \alpha + \beta_0 PvtMe\widehat{dicaid}Status_{ct} + X_{ict} * \gamma + \varepsilon_{ict}$$

(**Equation #A2**)

The coefficient, β_0, captures the causal effect of private Medicaid enrollment on various outcomes of interest.

Even among this treatment group, of Medicaid-only enrollees, some individuals may be exempt from mandated private Medicaid plan enrollment. Unfortunately, the nature of these

[31] By the end of the study period, about 80% of disabled New York Medicaid recipients lived in a county with an 'enrollment' mandate in effect. Note that prior to the implementation of these mandates, Medicaid recipients had the option to voluntarily enroll in private Medicaid, which was taken up by about 35% of all beneficiaries.

[32] Here, I limit to individuals who were already enrolled in Medicaid (either public or private) at the start of 2004. This restriction is meant to guard against sample composition changes, given that the implementation of mandates could result in changing beneficiary selection into Medicaid (Currie and Fahr 2005).

exemptions makes it difficult to directly identify exempt beneficiaries, and exemption status could moreover be endogenous to health; as a result, individuals with exemptions remain included in the sample.[33]

For this identification strategy to be valid, on its own, mandate counties must be on parallel trends to non-mandate counties, at least with the inclusion of time controls.

Results

First, I explore the impact of enrollment mandates on switching from public Medicaid to private Medicaid plans. The regression results, presented in Table A.3, indicate that the imposition of mandates was associated with a 20-30% increase in the corresponding share of Medicaid recipients in private plans. These estimates are based on the baseline specification (from Equation A1), and are robust to the inclusion of additional controls and sample restrictions, including the use of dual-enrollees in Medicaid and Medicare as a control group (since these individuals are universally exempt from mandates).

These mandates have only a limited effect on private Medicaid enrollment, and also do not lead to 100% private Medicaid penetration, on account of several factors. First, a substantial share of beneficiaries were already in private Medicaid before the mandates, and would thereby remain unaffected. In addition, a significant share of beneficiaries were altogether exempted from the mandates, on the basis of existing conditions or institutional care status, and thereby could remain in public Medicaid.

Using mandates as an instrument for private Medicaid enrollment, I then consider the impact of private Medicaid on various measures of inpatient utilization. The regression results, presented in Table A.4, provide no evidence of a significant effect on overall inpatient utilization. Breaking down inpatient utilization into different types of visits, as shown in Table A.5, I also find limited evidence that private Medicaid impacted even a subset of inpatient utilization, such as elective or emergency care. These results are limited by a lack of precision, however, such that a utilization reduction of up to 20% cannot be ruled out, under a 95% confidence interval. Finally, in Table A.6, I examine the effect of private Medicaid on fiscal costs, specifically finding statistically significant increases in carved-out spending. Taken together, all these estimates ultimately do not contradict those from my primary identification strategy, particularly given their lack of precision.

[33] Exemptions were made for those qualifying as dual-eligibles, mental health patients, long-term nursing home residents, and for participants in a number of special treatment programs.

Table A.1: Timeline of NY Managed Care Mandates: Disabled/SSI

Date	**Areas/Counties Affected**
Nov. 2005	NYC
Oct. 2007	Nassau, Onondaga, Oswego, Suffolk, Westchester
Apr. 2008	Allegany, Cattaraugus, Chautauqua, Erie, Genesee, Niagara, Orleans, Rockland
Jun. 2008	Livingston, Monroe, Ontario, Seneca, Yates
Sep. 2008	Albany, Broome, Columbia, Cortland, Greene, Herkimer, Oneida
	Rensselaer, Saratoga
Oct. 2008	Dutchess, Fulton, Montgomery, Orange, Otsego, Putnam
	Schenectady, Sullivan, Ulster

Notes: This information is has been compiled using public data from the New York State Department of Health.

Table A.2: Population & Medicaid Enrollment Figures for NY State, by Year

Year	**Population**	**Private Medicaid Enrollment**		**Overall Medicaid Enrollment**	
		SSI	*Non-SSI*	*SSI*	*Non-SSI*
2000	18,976,457	48,346	606,868	1,270,892	2,371,011
2001	19,082,838	54,346	626,488	1,289,483	2,430,202
2002	19,137,800	59,595	771,835	1,318,894	2,928,692
2003	19,171,814	70,660	1,326,144	1,349,346	3,095,709
2004	19,171,567	75,783	1,694,806	1,355,664	3,339,993
2005	19,132,610	87,799	1,863,675	1,381,186	3,482,820
2006	19,104,631	102,050	1,922,745	1,459,118	3,473,079
2007	19,132,335	136,130	1,873,121	1,470,607	3,346,334
2008	19,212,436	211,531	1,900,232	1,500,781	3,272,951
2009	19,307,066	250,458	2,060,058	1,552,833	3,557,718
2010	19,378,102	278,470	2,409,256	1,617,300	4,080,285

Notes: This information is has been compiled using public data from the New York State Department of Health.

Table A.3: Alternate Identification Strategy: First Stage

	(1)	(2)	(3)	(4)	(5)
	Private Medicaid Enrollment Status				
Init. Non Dual*Post Mandate	0.224***	0.287***	0.230***	0.286***	0.232***
	(0.028)	(0.031)	(0.037)	(0.031)	(0.037)
Post Mandate		-0.055***	-0.015	-0.044**	-0.006
		(0.019)	(0.014)	(0.017)	(0.011)
Init. Non Dual		0.214***	0.153***	0.132***	0.037**
		(0.025)	(0.021)	(0.028)	(0.017)
Male	-0.064***	-0.049***	-0.049***	-0.046***	-0.046***
	(0.005)	(0.003)	(0.003)	(0.003)	(0.003)
Time Trends	X	X	X	X	X
Include Dual Eligibles as Control		X	X	X	X
Mandate Counties Only		X	X		
Group Specific Time Trends			X		X
Mean	0.209	0.301	0.301	0.189	0.189
	(0.403)	(0.454)	(0.454)	(0.388)	(0.388)
N	7,723,534	9,020,373	9,020,373	10,778,876	10,778,876
R-squared	0.156	0.229	0.233	0.241	0.248

Notes: Table presents results of my first-stage regression, a linear model with private Medicaid enrollment status as the outcome and the interaction of Init. Non Dual*Post Mandate as the instrument of interest. Init Non Dual is defined as those enrolled in Medicaid-only, as of 2004. The unit of observation is at the person-quarter level, for the 2004-2010 period. Year-quarter, county, age, and gender fixed effects are included as part of the analysis. The sample is restricted to those enrolled in Medicaid (or dually enrolled in Medicare), by virtue of disability, as of 2004; the sample is also restricted to New York State only. Standard-errors are clustered at the individual level. The original Medicaid enrollment data is taken from CMS.

Table A.4: Alt Identification Strategy: Effect on Inpatient Utilization

	(1)	(2)	(3)	(4)	(5)
	Tot LOS	Tot Hosp Visits	Tot Procs	Tot Chrg	Log Tot Chrg
$PrivateM\widehat{edicaid}Enrolled$	0.221	0.022	-0.097	1,852	0.030
	(0.358)	(0.037)	(0.074)	(1,317)	(0.070)
Medicaid Only Enrolled	-0.585***	-0.032***	-0.084***	-2,065***	-0.072***
	(0.080)	(0.007)	(0.027)	(313)	(0.014)
Male	0.189***	-0.019***	-0.036***	102	-0.072***
	(0.057)	(0.006)	(0.011)	(103)	(0.010)
Mean	2.693	0.343	0.646	9,928	0.699
	(20.854)	(1.578)	(4.263)	(77,102)	(2.742)
N			10,778,876		

Notes: Table presents linear regression models, where outcome variables are annualized measures of individual inpatient utilization. The instrument for private Medicaid enrollment is based off enrollment mandates. Year-quarter, county, age, and gender fixed effects are included as part of the analysis, along with an indicator for whether a mandate is in effect. I also include county and treatment group/control group specific time trends. The unit of observation is at the person-quarter level, for the 2004-2010 period. The sample is restricted to those enrolled in Medicaid (or dually enrolled in Medicare), by virtue of disability, as of 2004; the sample is also restricted to New York State only. This data was constructed using discharge-level hospital data from New York State and person-month level Medicaid enrollment records from CMS; these two datasets were linked using SSN and other fields, and subsequently aggregated to a person-quarter level. Sample inclusion is not conditional on utilization.

Table A.5: Alt Identification Strategy: Effect on Inpatient Util. Composition

	(1) *Elect*	(2) *Non-Elect*	(3) *Emerg*	(4) *Non-Emerg*	(5) *Surg*	(6) *Non-Surg*	(7) *Readm*	(8) *Non-Readm*
$Private\widehat{Medicaid}Enrolled$	-0.008	0.026	0.023	-0.005	.012***	0.006	*0.018*	0.000
	(0.012)	(0.032)	(0.028)	(0.018)	(0.003)	(0.035)	(0.017)	(0.024)
Medicaid Only Enrollee	-0.010***	-0.096***	-0.083***	-0.022***	-0.018***	-0.083***	-.011***	-0.069***
	(0.002)	(0.006)	(0.005)	(0.003)	(0.002)	(0.006)	(0.002)	(0.005)
Male	-0.013***	-0.045***	-0.038***	-0.020***	-0.007***	-0.049***	.011***	-0.057***
	(0.003)	(0.005)	(0.004)	(0.004)	(0.001)	(0.006)	(0.004)	(0.004)
Mean	0.042	0.261	0.235	0.069	0.029	0.279	0.085	0.218
	(0.464)	(1.381)	(1.288)	(0.617)	(0.364)	(1.446)	(0.933)	(0.971)
N	10,761,528							

Notes: Table presents linear regression models, where outcome variables are annualized measures of individual inpatient utilization. The instrument for private Medicaid enrollment is based off enrollment mandates. Year-quarter, county, age, and gender fixed effects are included as part of the analysis, along with an indicator for whether a mandate is in effect. I also include county and treatment group/control group specific time trends. The unit of observation is at the person-quarter level, for the 2004-2010 period. The sample is restricted to those enrolled in Medicaid (or dually enrolled in Medicare), by virtue of disability, as of 2004; the sample is also restricted to New York State only. This data was constructed using discharge-level hospital data from New York State and person-month level Medicaid enrollment records from CMS; these two datasets were linked using SSN and other fields, and subsequently aggregated to a person-quarter level. Sample inclusion is not conditional on utilization.

Table A.6: Alt Identification: Effect on Fiscal Spending

	(1)	(2)	(3)
	Fiscal Spending		
	Overall	*Carved-In*	*Carved-Out*
$Private\widehat{Medicaid}Enrolled$	730	-282	1,008***
	(1049)	(981)	(276)
Mean	24,274	17,644	6,631
Percentage Effect	3.0%	-1.6%	15.2%
	(4.3%)	(5.6%)	(4.2%)
Year Range	2004-2010		
N	1,607,790		

Notes: Table presents linear regression models, where outcome variables are annualized measures of individual inpatient utilization. The instrument for private Medicaid enrollment is based off enrollment mandates. Year, county, age, and gender fixed effects are included as part of the analysis, along with an indicator for whether a mandate is in effect. I also include county and treatment group/control group specific time trends. The unit of observation is at the person-quarter level, for the 2004-2010 period. The sample is restricted to those enrolled in Medicaid (or dually enrolled in Medicare), by virtue of disability, as of 2004; the sample is also restricted to New York State only. This data was constructed using person-year level Medicaid expenditure records and person-month level Medicaid enrollment records, all from CMS; these two datasets were linked using unique beneficiary ID's, and subsequently aggregated to a person-year level. Sample inclusion is not conditional on utilization.

Appendix B: Robustness Checks

Table B.1: Robustness Tests: Mechanisms Table

	(1) *Elect*	(2) *Non-Elect*	(3) *Emerg*	(4) *Non-Emerg*	(5) *Surg*	(6) *Non-Surg*	(7) *Readm*	(8) *Non-Readm*
Mean	0.045	0.340	0.313	0.072	0.039	0.346	0.103	0.282
Init. Private*Post 65	0.006***	0.028***	0.027***	0.007**	-0.002	0.036***	0.018***	0.016***
	(0.002)	(0.008)	(0.007)	(0.003)	(0.002)	(0.008)	(0.005)	(0.005)
Post 65	0.006***	0.004	0.001	0.009***	0.006***	0.005	0.002	0.008*
	(0.002)	(0.006)	(0.006)	(0.002)	(0.002)	(0.006)	(0.004)	(0.004)
Initially Private	-0.006***	-0.101***	-0.095***	-0.012***	-0.001	-0.106***	-0.048***	-0.059***
	(0.002)	(0.006)	(0.006)	(0.002)	(0.001)	(0.006)	(0.003)	(0.004)
Sample Restriction	63 to 67							
N	3,909,770							
Init. Private*Post 65	0.010***	0.029***	0.027***	0.012***	0.002	0.037***	0.025***	0.014**
	(0.003)	(0.008)	(0.007)	(0.003)	(0.002)	(0.008)	(0.005)	(0.006)
Post 65	0.004*	0.006	0.003	0.007**	0.003	0.006	-0.007	0.017***
	(0.002)	(0.007)	(0.007)	(0.003)	(0.002)	(0.007)	(0.004)	(0.005)
Ini Private	-0.009***	-0.123***	-0.114***	-0.018***	-0.005***	-0.128***	-0.058***	-0.074***
	(0.002)	(0.007)	(0.007)	(0.003)	(0.002)	(0.007)	(0.004)	(0.005)
Sample Restriction	64 to 66							
N	2,329,769							
Init. $\widehat{\text{Private}}$*Post 65	0.010**	0.048***	0.041***	0.017**	-0.003	0.061***	0.032***	0.026**
	(0.005)	(0.017)	(0.016)	(0.007)	(0.003)	(0.018)	(0.011)	(0.012)
Post 65	0.004	0.001	-0.001	0.006	0.005**	0.000	-0.009*	0.014**
	(0.003)	(0.008)	(0.007)	(0.004)	(0.002)	(0.008)	(0.005)	(0.006)
$\widehat{Init.Private}$	0.011	0.082**	0.054	0.039**	0.009	0.083**	0.016	0.077***
	(0.010)	(0.039)	(0.036)	(0.015)	(0.007)	(0.041)	(0.022)	(0.027)
Sample Restriction	64 to 66							
N	2,329,769							

Notes: Table presents linear regression models, where outcome variables are annualized measures of individual inpatient utilization. The key variable of interest is Init Private*Post 65, which captures the effect of involuntary switching from private to public Medicaid. The three different panels reflect different age restrictions. In the bottom panel, I also instrument for the Init Private*Post 65 term using a secondary identification strategy, as a robustness check. Year-quarter, county, and gender fixed effects are included as part of the analysis. The unit of observation is at the person-month level, for the 1999-2010 period. The sample is restricted to those enrolled in Medicaid-only at 63, by virtue of disability; the sample is also restricted to New York State only. This data was constructed using discharge-level hospital data from New York State and person-month level Medicaid enrollment records from CMS; these two datasets were linked using SSN and other fields, and subsequently aggregated to a person-month level. Sample inclusion is not conditional on utilization.

Table B.2: Robustness Test: Pre and Post Trends for Inpatient

	(1) Tot LOS	(2) Tot Hosp Visits	(3) Tot Readm	(4) Tot ER Admits	(5) Tot ER Visits
Mean	2.801	0.385	0.103	0.193	0.736
Initially Private*Pre-4	-0.126	-0.014	-0.007	-0.008	-0.030
	(0.160)	(0.012)	(0.007)	(0.008)	(0.024)
Initially Private*Pre-3	-0.050	-0.005	0.003	-0.004	0.012
	(0.160)	(0.012)	(0.007)	(0.008)	(0.022)
Initially Private*Pre-2	0.064	-0.011	-0.003	-0.003	0.013
	(0.156)	(0.011)	(0.006)	(0.008)	(0.021))
Initially Private*Pre-1			Baseline		
Initially Private*Switch Per	0.335**	0.012	0.012*	-0.001	0.019
	(0.164)	(0.012)	(0.007)	(0.008)	(0.024)
Initially Private*Post-1	0.387**	0.022	0.016**	0.011	0.040
	(0.184)	(0.014)	(0.008)	(0.010)	(0.026)
Initially Private*Post-2	0.089	0.020	0.009	0.013	0.060**
	(0.175)	(0.015)	(0.008)	(0.011)	(0.027)
Initially Private*Post-3	0.150	0.037**	0.024***	0.028**	0.096***
	(0.184)	(0.016)	(0.009)	(0.012)	(0.031))
N	3,909,770	3,909,770	3,909,770	3,909,770	2,294,206

Notes: Table presents linear regression models, where outcome variables are annualized measures of individual inpatient and drug utilization. The key variables track differences in utilization between the initially Public Medicaid and initially Private Medicaid cohorts (which are constructed based off Medicaid status at 63). The Pre and Post terms correspond to the number of half-yrs an individual is away from 65. Year-quarter, county, and gender fixed effects are included as part of the analysis. The unit of observation is at the person-month level, for the 1999-2010 period. The sample is restricted to those enrolled in Medicaid-only at 63, by virtue of disability; the sample is also restricted to New York State only. Finally, the sample is restricted to the 63 to 67 age range. This data was constructed using discharge-level hospital data from New York State, Medicaid drug data, and person-month level Medicaid enrollment records from CMS; these two datasets were linked using SSN and other fields, and subsequently aggregated to a person-month level. Sample inclusion is not conditional on utilization.

Table B.3: Effect on Outpatient Utilization-Full Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Spending Breakdown						**Visits Breakdown**	
	Core Outp	*Surg Amt*	*Diag Amt*	*Imaging Amt*	*Prev. Care*	*Office Visits*	*Office Visits No*	*Minutes in Office*
Initially Private*Post 65	-646.300***	-83.705***	-57.293***	-40.150	-21.621***	-125.440***	-4.260***	-74.053***
	(105.551)	(15.173)	(8.413)	(33.496)	(3.331)	(10.291)	(0.292)	(5.548)
Post 65	586.781***	75.275***	84.283***	88.934***	16.432***	107.461***	3.820***	74.032***
	(94.135)	(15.882)	(8.091)	(26.926)	(3.672)	(9.138)	(0.262)	(5.013)
Initially Private	-132.841**	22.514***	0.341	9.507	22.997***	120.915***	3.596***	64.580***
	(58.988)	(8.135)	(3.101)	(15.089)	(1.553)	(4.580)	(0.130)	(2.392)
Male	-84.259*	4.760	3.482	-23.619*	-39.185***	-39.030***	-1.102***	-21.298***
	(49.709)	(6.527)	(2.990)	(13.706)	(1.290)	(4.618)	(0.129)	(2.394)
Age	212.745***	21.162	-0.784	6.436	-1.325	43.795***	0.804***	12.312***
	(81.355)	(18.596)	(6.571)	(32.807)	(4.727)	(7.522)	(0.206)	(4.029)
Cohort Restriction	At age 64.5, enrolled in Medicaid AND NOT simultaneously enrolled in Medicare.							
Age Restriction	64.5 to 65.5							
Mean	1,626	159	95	268	51	240	7.27	132
N	180,003							

Notes: Table presents linear regression models, where outcome variables are measures of outpatient utilization. The coefficients presented correspond to the key variable of interest, Init Private*Post 65, which captures the effect of switching from private to public Medicaid; the share of this group actually switching is about 68%, based on my first stage estimates. Year-quarter, county, and gender fixed effects are included as part of the analysis. The unit of observation is at the person-month level, for the 2008-2010 period. The sample is restricted to those enrolled in Medicaid-only at 64.5, by virtue of disability; the sample is also restricted to New York State only. Finally, the sample is restricted to the 64.5 to 65.5 age range. This data was constructed using outpatient Medicaid and Medicare claims data from New York State and person-month level Medicaid enrollment records from CMS; these datasets were linked using unique beneficiary ID's, and subsequently aggregated to a person-month level. Sample inclusion is not conditional on utilization.

Table B.4: First Stage Results: 2008 to 2010 Time Period

	(1) Private Medicaid Enrolled	(2) Dually Enrolled in Medicare
Initially Private*Post 65	-0.684***	0.071***
	(0.007)	(0.010)
Post 65	0.016***	0.696***
	(0.004)	(0.008)
Initially Private	0.951***	0.006***
	(0.002)	(0.001)
Male	-0.001	-0.001
	(0.002)	(0.002)
Age	0.049***	0.049***
	(0.002)	(0.002)
Cohort Restriction	At age 64.5, non-dual Medicaid enrolled	
Age Restriction	64.5 to 65.5	
Mean (Pre-65)	0.195	0.543
N	180,003	

Notes: Table presents linear regression models, where outcome variables are measures of private Medicaid and dual enrollment status. The coefficients presented correspond to the key variable of interest, Init Private*Post 65, which captures the effect of reaching 65 among the treatment group. Year-quarter, county, and gender fixed effects are included as part of the analysis. The unit of observation is at the person-month level, for the 2008-2010 period. The sample is restricted to those enrolled in Medicaid-only at 64.5, by virtue of disability; the sample is also restricted to New York State only. Finally, the sample is restricted to the 64.5 to 65.5 age range. This data was constructed using outpatient Medicaid and Medicare claims data from New York State and person-month level Medicaid enrollment records from CMS; these datasets were linked using unique beneficiary ID's, and subsequently aggregated to a person-month level. Sample inclusion is not conditional on utilization.

Table B.5: Effect on Prescription Utilization-Alternate Approaches

	(1) No. of Presc	(2) Presc Spend	(3) Log Pharma Spend
Init. Private*Pre 65	0.015	-11	0.071*
	(0.047)	(54)	(0.036)
Pre 65	0.005	29	-0.013
	(0.017)	(22)	(0.014)
Init. Private	-0.349***	-521***	0.027
	(0.042)	(42)	(0.034)
Year Range:		1999-2005	
Mean	4.703	3,507	5.833
N		2,009,680	
$Priv.\widehat{Medicaid}Enrolled$	0.188	416**	0.196**
	(0.282)	(170)	(0.083)
Year Range:		2004-2010	
Mean	8.141	2,985	4.499
N		10,778,876	

Notes: Table presents linear regression models, where outcome variables are monthly measures of individual drug utilization. The top panel leverages my primary identification strategy, of involuntary switching from private to public Medicaid at 65. The sample is restricted to those between 63 and 67, who were in Medicaid-only by virtue of disability, at age 63. It is further restricted to the 1999 to 2005 The bottom panel leverages an alternate instrument for private Medicaid enrollment, based off mandates, the sample here is restricted to those in Medicaid-only as of 2004, by virtue of disability, and is further restricted to the 2004 to 2010 period. Year-quarter, county, and gender fixed effects are included as part of the analysis. The unit of observation is at the person-month level. This data was constructed using claim-level prescription drug utilization and person-month level Medicaid enrollment records from CMS; these two datasets were linked using beneficiary ID's, and subsequently aggregated to a person-month level. Sample inclusion is not conditional on utilization.

Table B.6: Robustness Tests: Pre and Post Trends for Outpatient Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Spending Breakdown						Visits Breakdown	
	Core Outp	*Surg Amt*	*Diag Amt*	*Imaging Amt*	*Prev. Care*	*Office Visits*	*Office Visits No*	*Minutes in Office*
Mean	1,626	159	95	268	51	240	7.27	132
Initially Private*Pre-2	33.187	11.952	-4.857	9.521	-0.897	-6.833	-0.091	-3.413
	(54.034)	(12.848)	(4.230)	(21.691)	(2.908)	(4.301)	(0.119)	(2.285)
Initially Private*Pre-1	Baseline							
Initially Private*Switch	-467.809***	-62.947***	-53.871***	21.966	-22.774***	-123.749***	-4.163***	-73.462***
	(113.796)	(18.304)	(8.984)	(36.931)	(4.355)	(10.849)	(0.311)	(5.965)
Initially Private*Post-1	-826.221***	-96.351***	-66.410***	-104.723**	-21.172***	-134.552***	-4.469***	-78.278***
	(143.207)	(22.300)	(11.800)	(51.753)	(5.011)	(12.823)	(0.357)	(6.814)
Cohort Restriction	At age 64.5, enrolled in Medicaid AND NOT simultaneously enrolled in Medicare.							
Age Restriction	64.5 to 65.5							
N	180,003							

Notes: Table presents linear regression models, where outcome variables are annualized measures of individual outpatient utilization. The key variables track differences in utilization between the initially Public Medicaid and initially Private Medicaid cohorts (which are constructed based off Medicaid status at 64.5). The Pre and Post terms correspond to the number of quarters an individual is away from 65. Year-quarter, county, and gender fixed effects are included as part of the analysis. The unit of observation is at the person-month level, for the 2008-2010 period. The sample is restricted to those enrolled in Medicaid-only at 64.5, by virtue of disability; the sample is also restricted to New York State only. Finally, the sample is restricted to the 64.5 to 65.5 age range. This data was constructed using claims-level Medicaid and Medicare outpatient data from CMS, and person-month level Medicaid enrollment records from CMS; these datasets were linked using common beneficiary ID's, and subsequently aggregated to a person-month level. Sample inclusion is not conditional on utilization.

Table B.7: Robustness Tests: Pre and Post Trends for Drug Results

	(1)	(2)		
	All Presc	Presc; By Category		
		Acute	*Chron.*	*Discr.*
Panel A: Log Spending				
Initially Private*Pre-2	-0.217***	-0.097***	-0.098***	-0.161***
	(0.030)	(0.029)	(0.026)	(0.029)
Initially Private*Pre-1		Baseline		
Initially Private*Switch	-0.492***	-0.294***	-0.307***	-0.453***
	(0.070)	(0.071)	(0.065)	(0.069)
Initially Private*Post-1	-0.662***	-0.375***	-0.378***	-0.635***
	(0.078)	(0.079)	(0.071)	(0.076)
N		180,003		
Panel B: Number of Prescriptions				
Initially Private*Pre-2	-0.143***	-0.041***	-0.042***	-0.057**
	(0.045)	(0.013)	(0.014)	(0.027)
Initially Private*Pre-1		Baseline		
Initially Private*Switch	-0.591***	-0.160***	-0.183***	-0.213***
	(0.107)	(0.036)	(0.038)	(0.064)
Initially Private*Post-1	-0.740***	-0.193***	-0.226***	-0.315***
	(0.120)	(0.040)	(0.041)	(0.071)
N		180,003		

Notes: Table presents linear regression models, where outcome variables are annualized measures of individual drug utilization. The key variables track differences in utilization between the initially Public Medicaid and initially Private Medicaid cohorts (which are constructed based off Medicaid status at 64.5). The Pre and Post terms correspond to the number of quarters an individual is away from 65. Year-quarter, county, and gender fixed effects are included as part of the analysis. The unit of observation is at the person-month level, for the 2008-2010 period. The sample is restricted to those enrolled in Medicaid-only at 64.5, by virtue of disability; the sample is also restricted to New York State only. Finally, the sample is restricted to the 64.5 to 65.5 age range. This data was constructed using claims-level Medicaid and Medicare drug data from CMS, and person-month level Medicaid enrollment records from CMS; these datasets were linked using common beneficiary ID's, and subsequently aggregated to a person-month level. Sample inclusion is not conditional on utilization.

Table B.8: Robustness Test: Bandwidth Sensitivities

	(1) Tot LOS	(2) Tot Hosp Visits	(3) Tot Readm.	(4) Tot ER Admits	(5) Tot ER Visits
Mean (Baseline)	2.801	0.385	0.103	0.193	0.736
Init. Private*Post 65	0.335***	0.034***	0.018***	0.017***	0.055***
	(0.088)	(0.008)	(0.005)	(0.006)	(0.020)
Post 65	-0.064	0.010	0.002	-0.009**	0.006
	(0.101)	(0.006)	(0.004)	(0.004)	(0.013)
Initially Private	-1.201***	-0.107***	-0.048***	-0.070***	-0.173***
	(0.067)	(0.007)	(0.003)	(0.005)	(0.016)
N	3,909,770	3,909,770	3,909,770	3,909,770	2,294,206
Baseline Bandwidth: 63 to 67					
Init. Private*Post 65	0.479***	0.039***	0.025***	0.011*	0.041**
	(0.103)	(0.009)	(0.005)	(0.006)	(0.019)
Post 65	-0.003	0.010	-0.007	-0.006	0.013
	(0.139)	(0.008)	(0.004)	(0.005)	(0.017)
Initially Private	-1.397***	-0.133***	-0.058***	-0.079***	-0.178***
	(0.085)	(0.008)	(0.004)	(0.005)	(0.018)
N	2,329,769	2,329,769	2,329,769	2,329,769	1,239,260
Narrower Bandwidth: 64 to 66					
Init. Private*Post 65	0.431***	0.034***	0.018***	0.004	0.022
	(0.141)	(0.010)	(0.006)	(0.007)	(0.021)
Post 65	0.160	0.015	(0.003)	0.003	0.032
	(0.170)	(0.010)	(0.006)	(0.007)	(0.023)
Initially Private	-1.601***	-0.156***	-0.067***	-0.091***	-0.199***
	(0.116)	(0.009)	(0.005)	(0.007)	(0.021)
N	1,312,489	1,312,489	1,312,489	1,312,489	670,760
Narrowest Bandwidth: 64.5 to 65.5					

Notes: Table presents linear regression models, where outcome variables are annualized measures of individual inpatient utilization. The panels present under variying bandwidths. Year-quarter, county, and gender fixed effects are included as part of the analysis. The unit of observation is at the person-month level, for the 1999-2010 period. The sample is restricted to those enrolled in Medicaid-only at the beginning of the specified bandwidth, by virtue of disability; the sample is also restricted to New York State only. Finally, the sample is restricted to the age range specified. This data was constructed using discharge-level hospital data from New York State and person-month level Medicaid enrollment records from CMS; these two datasets were linked using SSN and other fields, and subsequently aggregated to a person-month level. Sample inclusion is not conditional on utilization.

Table B.9: Robustness Test: Control Sensitivities

	(1) Tot LOS	(2) Tot. Hosp Visits	(3) Tot Readm.	(4) Tot ER Admits	(5) Tot ER Visits
Mean (Baseline)	2.801	0.385	0.103	0.193	0.736
Init. Private*Post 65	0.335***	0.034***	0.018***	0.017***	0.055***
	(0.088)	(0.008)	(0.005)	(0.006)	(0.020)
Post 65	-0.064	0.010	0.002	-0.009**	0.006
	(0.101)	(0.006)	(0.004)	(0.004)	(0.013)
Initially Private	-1.201***	-0.107***	-0.048***	-0.070***	-0.173***
	(0.067)	(0.007)	(0.003)	(0.005)	(0.016)
N	3,909,770	3,909,770	3,909,770	3,909,770	2,294,206
Baseline Spec: Quadratic					
Init. Private*Post 65	0.319***	0.033***	0.018***	0.016***	0.052***
	(0.088)	(0.009)	(0.005)	(0.006)	(0.020)
Post 65	-0.033	0.0127**	0.003	-0.007	0.008
	(0.103)	(0.006)	(0.004)	(0.004)	(0.013)
Initially Private	-1.196***	-0.106***	-0.047***	-0.069***	-0.172***
	(0.067)	(0.007)	(0.003)	(0.005)	(0.017)
N	3,909,770	3,909,770	3,909,770	3,909,770	2,294,206
Alternate Control: Linear					
Init. Private*Post 65	0.334***	0.034***	0.018***	0.017***	0.055***
	(0.088)	(0.009)	(0.005)	(0.006)	(0.020)
Post 65	-0.176	0.000	-0.005	-0.013**	-0.007
	(0.140)	(0.008)	(0.005)	(0.005)	(0.016)
Initially Private	-1.201***	-0.107***	-0.047***	-0.070***	-0.173***
	(0.067)	(0.007)	(0.003)	(0.005)	(0.017)
N	3,909,770	3,909,770	3,909,770	3,909,770	2,294,206
Alternate Control: Cubic					

Notes: Table presents linear regression models, where outcome variables are annualized measures of individual inpatient utilization. The table presents analyses, under different forms of age controls. The key variable of interest is Init Private*Post 65, which captures the effect of involuntary switching from private to public Medicaid; the share of this group actually switching corresponds to about 65 %, based on my first stage estimates. Init Private is defined as those enrolled in private Medicaid, at the age of 63. Year-quarter, county, and gender fixed effects are included as part of the analysis. The unit of observation is at the person-month level, for the 1999-2010 period. The sample is restricted to those enrolled in Medicaid-only at 63, by virtue of disability; the sample is also restricted to New York State only. Finally, the sample is restricted to the 63 to 67 age range. This data was constructed using discharge-level hospital data from New York State and person-month level Medicaid enrollment records from CMS; these two datasets were linked using SSN and other fields, and subsequently aggregated to a person-month level. Sample inclusion is not conditional on utilization.

Table B.10: Robustness Test: Control Sensitivities

	(1) Tot LOS	(2) Tot Hosp Visits	(3) Tot Readm	(4) Tot ER Admits	(5) Tot ER Visits
Mean (Baseline)	2.801	0.385	0.103	0.193	0.736
Initially Private*Post 65	0.332***	0.033***	0.018***	0.017***	0.054***
	(0.088)	(0.009)	(0.005)	(0.006)	(0.020)
Post 65	-0.274***	-0.009	-0.009**	-0.014***	-0.067***
	(0.081)	(0.007)	(0.004)	(0.005)	(0.016)
Initially Private	-1.199***	-0.107***	-0.048***	-0.070***	-0.173***
	(0.067)	(0.007)	(0.003)	(0.005)	(0.017)
Alternate Control: Linear on Either Side of Discontinuity					
Init. Private*Post 65	0.332***	0.034***	0.018***	0.017***	0.054***
	(0.088)	(0.009)	(0.005)	(0.006)	(0.020)
Post 65	-0.378***	-0.018**	-0.017***	-0.017***	-0.081***
	(0.106)	(0.009)	(0.005)	(0.006)	(0.020)
Initially Private	-1.198***	-0.107***	-0.048***	-0.070***	-0.173***
	(0.067)	(0.007)	(0.003)	(0.005)	(0.017)
Alternate Control: Quadratic on Either Side of Discontinuity					
Init. Private*Post 65	0.661***	0.036**	0.019**	0.011	0.029
	(0.153)	(0.014)	(0.008)	(0.010)	(0.031)
Post 65	-0.359***	-0.011	-0.010**	-0.014***	-0.066***
	(0.094)	(0.008)	(0.005)	(0.005)	(0.019)
Initially Private	-1.380***	-0.120***	-0.054***	-0.079***	-0.201***
	(0.102)	(0.009)	(0.005)	(0.006)	(0.022)
Alternate Control: Linear, Group-Specific on Either Side of Discontinuity					
Init. Private*Post 65	0.913***	0.059***	0.025**	0.024*	0.068* *
	(0.207)	(0.018)	(0.010)	(0.013)	(0.039)
Post 65	-0.543***	-0.026**	-0.020***	-0.020***	-0.096***
	(0.124)	(0.010)	(0.006)	(0.007)	(0.025)
Initially Private	-1.600***	-0.138***	-0.062***	-0.088***	-0.241***
	(0.12)	(0.01)	(0.01)	(0.01)	(0.03)
Alternate Control: Quadratic, Group-Specific on Either Side of Discontinuity					

Notes: Table presents linear regression models, where outcome variables are annualized measures of individual inpatient utilization. The table presents analyses, under different forms of age controls. The key variable of interest is Init Private*Post 65, which captures the effect of involuntary switching from private to public Medicaid; the share of this group actually switching corresponds to about 65 %, based on my first stage estimates. Init Private is defined as those enrolled in private Medicaid, at the age of 63. Year-quarter, county, and gender fixed effects are included as part of the analysis. The unit of observation is at the person-month level, for the 1999-2010 period. The sample is restricted to those enrolled in Medicaid-only at 63, by virtue of disability; the sample is also restricted to New York State only. Finally, the sample is restricted to the 63 to 67 age range. This data was constructed using discharge-level hospital data from New York State and person-month level Medicaid enrollment records from CMS; these two datasets were linked using SSN and other fields, and subsequently aggregated to a person-month level. Sample inclusion is not conditional on utilization.

Appendix C: Additional Analyses Around Mechanisms

In my main analyses, I find that private Medicaid plans reduce inpatient hospitalizations partly through a reduction in readmissions. Here, I extend these analyses by exploring a few plausible mechanisms behind this readmissions reduction. These potential mechanisms include changes to the site of hospitalization, as well as changes to hospitalization composition. One caveat is that the set of mechanisms considered here is not exhaustive, as I specifically do not look at outpatient or post-acute care spillovers, due to data limitations. As such, this examination of mechanisms should be treated as illustrative rather than all-encompassing. For these analyses, I structure my data to be at an individual hospitalization level and restrict the data to initial admissions. Otherwise, I incorporate the baseline sample restrictions and controls from before. The outcome variable of interest denotes the likelihood of readmission within 30-days of the initial admission, and as such is binary.

Altogether, I find the mechanisms examined may explain only about a third of the overall effect on readmissions, under private Medicaid plans. First, as shown in Table C.1, I find that this effect is attenuated by one-eighth when including diagnostic class (DRG) fixed effects, and that about one-third of the effect disappears when including more granular diagnosis (ICD-9) fixed effects. Finally, I find that these results are not sensitive to the inclusion of individual hospital fixed effects (corresponding to the site of initial hospitalization). These results leave two-thirds of the readmissions effect unaccounted for, in terms of mechanisms. Based on conversations with various stakeholders, additional plausible mechanisms could include changes to inpatient care at the individual hospital level. These mechanisms could also include changes to outpatient care, such as increases to home or primary care immediately following a hospitalization. In addition, these mechanisms could include more stringent authorization requirements for inpatient admissions generally, and readmissions specifically.

In Table C.2, I present some additional regression analyses, to more closely examine the effect on readmissions through one specific channel, a shift in initial site of hospitalization. Consistent with the previous analyses, I do not find that this mechanism plays a meaningful role. Specifically, I find that only 5% of the estimated effect on readmissions (or .001 readmissions, out of an increase of .024, per person-year), is attributable to shifts to alternative hospitals, and can rule out anything in excess of 10% with 95% confidence. To arrive at these estimates, I construct hospital-specific measures of conditional readmissions rates, which account for differences in patient demographics and visit composition. I then combine these with observed changes in visit distribution across hospitals, under private Medicaid plans. Incidentally, to construct conditional readmission rates, I look to the public Medicaid population and back out corresponding hospital-level fixed effects for readmissions, conditional on individual level demographics (age and race) and the nature of the initial hospitalization (ICD-9 designations).

Table C.1: Mechanisms for Private Medicaid's Effect on Readmissions

	(1)	(2)	(3)	(4)	(5)	(6)
	Readm. Rate, Conditional on Hospitalization					
Init. Private*Post 65	0.024***	0.021***	0.020**	0.017**	0.016**	0.022***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Initial DRG FE'S		X	X			
Initial ICD-9 FE's				X	X	
Initial Hosp FE's			X		X	X
Mean	0.218					
N	82,503					

Notes: Table presents linear regression models, where outcome variable is readmission status, following initial hospitalization. The coefficient reflects the value of my private Medicaid instrument, capturing the impact of private to public Medicaid switching (for the 65% of the initial cohort switching, at 65). The unit of observation is at the hospitalization level, for the 1999-2010 period. The sample is restricted to initial hospitalizations, for those enrolled in Medicaid-only at 63, by virtue of disability; the sample is also restricted to New York State only. Finally, the sample is restricted to the 63 to 67 age range. This data was constructed using discharge-level hospital data from New York State and person-month level Medicaid enrollment records from CMS; these two datasets were linked using SSN and other fields.

Table C.2: Decomposing Private Medicaid's Readmissions Effect

	(1)	(2)	(3)
	Hosp Readm. Index		
Init. Private*Post 65	0.001*	0.001*	0.001
	(0.001)	(0.001)	(0.001)
DRG FE'S		X	
ICD-9 FE's			X
Mean	-0.140		
N	82,503		

Notes: Table presents linear regression models, where outcome variable is a hospital-level readmission likelihood index, constructed previously by me. 'Coefficient' reflects the value of my private Medicaid instrument, capturing the impact of private to public Medicaid switching (for the 65% of the initial cohort switching, at 65). The unit of observation is at the initial hospitalization level, for the 1999-2010 period. The sample is restricted to initial hospitalizations, for those enrolled in Medicaid-only at 63, by virtue of disability; the sample is also restricted to New York State only. Finally, the sample is restricted to the 63 to 67 age range. This data was constructed using discharge-level hospital data from New York State and person-month level Medicaid enrollment records from CMS; these two datasets were linked using SSN and other fields.

Appendix D: Data Description

A: Data Construction:

Inpatient Panel: Much of this study relies on an individual-month level panel that tracks inpatient hospital utilization, for those in public Medicaid as well as in private Medicaid plans.

This individual-level panel is constructed through the linking of two distinct datasets: individual-year level Medicaid denominator data (obtained from CMS) and discharge-level hospital data (obtained from New York State's Department of Health). This linking is conducted using several identifying fields that are found in both data: the last four digits of SSN, full birth dates, gender, and county of residence. The combination of these fields uniquely identifies Medicaid recipients over 99.9% of the time. Those Medicaid recipients that are not uniquely identified are dropped from the sample.

Subsequently, these data are aggregated to a person-year level; given the nature of this data, sample inclusion is not conditional on utilization. To this end, I retain person-year level observations even in the absence of inpatient utilization; for person-year combos for which a Medicaid enrollment record exists, but an inpatient utilization record does not, I mechanically set inpatient utilization to zero.

Outpatient and Overall Spending Panel: This study also relies on an individual-month level panel that tracks outpatient utilization, across the office as well as facility settings, for private as well as public Medicaid enrollees.

This individual-level panel is constructed through the pooling and linking of a number of distinct datasets. These include Medicaid claims data, which encompasses public/fee-for-service as well as private/managed care (otherwise known as 'encounter') claims. In addition, these data are inclusive of fee-for-service Medicare claims. The Medicaid and Medicare data are pooled together, and then linked to a person-month level denominator file from Medicaid, which tracks enrollment information for Medicaid-only as well as dual Medicare-Medicaid enrollees.

For those in Medicaid-only, utilization information is taken only from Medicaid claims data, as these claims data would fully capture utilization for this population. Meanwhile, for those who are dually-enrolled, utilization information is taken solely from Medicare claims data, as Medicare would function as the primary payer for these individuals and would thereby completely track their utilization (with the notable exception of long-term care services and certain other treatments that are covered only by Medicaid and not Medicare, but these uncovered treatments are not included among our analyses). Following the linking, the data is aggregated to a person-month level. Incidentally, longitudinal linking is possible across all these data, thanks to common beneficiary ID's. Given that payment rates for a single service could vary between Medicaid and Medicare, I construct standardized rate measures that are uniform across payment settings, so as to track raw utilization differences independent of payment rate differences.

This study also relies on an individual-month level panel that tracks government expenditures on all services, including inpatient, outpatient, as well as pharmaceutical, for those enrolled under private as well as public Medicaid. For those in public Medicaid, government expenditures come in the form of fee-for-service payments for various services, which I track using corresponding claims data. Meanwhile, for those in private Medicaid plans, government expenditures arise in the form of premium payments, which I track using person-month level premium costs that are included in the data, as a special type of claim. For those in private Medicaid, government expenditures can also come in the form of fee-for-service payments, for services that are carved-out of managed care contracts, amounts which can be derived from fee-for-service claims data.

Pharmaceutical Panel Data: This study relies on an individual-month level panel that tracks pharmaceutical utilization, for those in private as well as public Medicaid.

For those in Medicaid-only and not simultaneously in Medicare, the individual-level panel is constructed by linking together claims-level Medicaid data on pharmaceutical utilization, in combination with person-month level Medicaid enrollment data. The claims data is taken from standardized Medicaid MAX data, where the claims are stored and represented in a uniform format, irrespective of whether they come from public or private Medicaid.

For those simultaneously in Medicare and Medicaid, claims information is taken from Medicare Part D data, given that Part D functions as the primary payer for these beneficiaries.

Ultimately, these linked data are aggregated at a beneficiary-month level. Given the nature of this data, sample inclusion is not conditional on utilization. Thanks to common beneficiary ID's, these different data can be longitudinally linked, making it possible to track utilization for beneficiaries who switch between different types of coverage.

Unfortunately, actual amounts paid are only available for claims processed by public Medicaid or Medicare Part D, and not available for claims processed under private Medicaid. To deal with this issue, and also to measure differences in cost independent of possible differences in payment rates, I construct a standardized cost measure. To do so, I take the unit payment rates prevailing under Medicaid fee-for-service, for every single NDC code. I then set these as the baseline payment rates for all claim types.

B. Sample Restrictions and Treatment and Control Group Construction:

The sample is restricted to New York State and is further restricted to those qualifying for Medicaid on the basis of disability. For my main analyses, I focus on those enrolled in Medicaid at age 63, and track their utilization forward through age 67. Consequently, those who are not enrolled in Medicaid at 63, but who become enrolled at a later age, would not be included as part of the sample. The sample will also specifically exclude individuals who are dually-enrolled in Medicaid and Medicare at age 63. In addition, for each Medicaid recipient, the sample is restircted only to the months in which they were enrolled. Hence, some individuals may eventually drop out of the sample as a result of death, loss of eligibility, or change of residence.

My primary treatment and control groups are further restricted to those enrolled in a fully-capitated private Medicaid plan, as of age 63; for these purposes, I define private Medicaid enrollment status based on information in CMS's data and not based on information in the discharge file, given that the discharge files often miscode payer type.

C: Outcome Measures

Inpatient Panel

Total Procs: This measure reflects the total number of inpatient procedures performed. Given that New York's discharge data can only track up to 15 procedures associated with a given inpatient visit, this measure should be considered a floor (although only a tiny fraction of all inpatient hospitalizations involve 15+ procedures).

Total Charges: This measure reflects raw inpatient charge amounts, which are an accounting-based measure uniform across payers, and which doesn't reflect the negotiated rate actually paid to hospitals.

Log Total Charges: This measure is defined as the log of (charges+1); as such, even records associated with zero raw charges will be assigned a defined value here, and so will get included as part of the analysis.

Distance to Hospital, Miles/Minutes: This measure reflects the driving distance between the center of a patient's zip code of residence, and the center of the zip code in which a given hospital is located. These reflect driving, and not 'crow flies' distances, and are calculated in terms of minutes and miles using Microsoft's MapPoint program.

Elective Visits: This measure covers inpatient hospital visits that are defined as follows, in the type of admission field in New York State's inpatient discharge data: 'The patient's condition permits adequate time to schedule the admission based on the availability of a suitable accommodation.'

Emergency Visits: This measure covers inpatient hospital visits that are defined as follows, in the type of admission field in New York State's inpatient discharge data: 'The patient requires immediate medical intervention as a result of severe, life threatening, or potentially disabling conditions.'

Surgery Visits: This measure covers inpatient hospital visits throughout which at least one surgical procedure was performed. Such visits are identified on the basis of the DRG code contained in the discharge record, using a Dartmouth Institute DRG classification system.

Readmissions: This measure covers inpatient hospital visits that take place within 30 days following an individual's initial discharge from the hospital.

Individual Outpatient Panel

Tot Outp Spending: This measure captures spending on a subset of outpatient services, relating specifically to non-institutional outpatient care. These are inclusive of physician services in the outpatient hospital as well as office settings, along with other services performed in the office setting. Excluded are non-basic outpatient hospital services, such as those subject to outpatient prospective payments. This measure also excludes home and long-term care. Altogether, the set of outpatient services included here is limited to those fitting under a standard HCPCS or CPT code. The spending measure here is standardized, so as to be invariant to rate differences across coverage settings. Specifically, ths field is set to the Medicaid fee-for-service payment amount, for all payers.

Surg Amt: This measure captures spending on surgeries taking place outside of the inpatient setting. Surgical procedures in this setting are defined as those with HCPCS codes of between 10000 and 70000. As before, spending amounts are standardized across payer settings, and set to correspond to payment rates under Medicaid fee-for-service.

Diag Amt: This measure captures spending on diagnostic testing, taking place outside of the inpatient setting. Such diagnostic services are defined as those with HCPCS codes of between 80000 and 90000. As before, spending amounts are standardized across payer settings, and set to correspond to payment rates under Medicaid fee-for-service.

Imaging Amt: This measure captures spending on imaging services, taking place outside of the inpatient setting. Such imaging services are defined as those with HCPCS codes of between

70000 and 80000. As before, spending amounts are standardized across payer settings, and set to correspond to payment rates under Medicaid fee-for-service.

Preventive Care Amt: This measure captures spending on a number of common preventive care services. This set of preventive care is based on definitions and corresponding codes originally compiled by BlueCross BlueShield. On the basis of these codes, I identify preventive care services appearing in the actual claims data. As before, spending amounts are standardized across payer settings, and set to correspond to payment rates under Medicaid fee-for-service.

Office Visits Amt: This measure captures spending on office visits, which are defined as claims with HCPCS codes of between 99001 and 99015. As before, spending amounts are standardized across payer settings, and set to correspond to payment rates under Medicaid fee-for-service.

Office Visits-Count: This measure captures the number of office visits, which are defined as claims with HCPCS codes of between 99001 and 99015. Each claim carrying such a HCPCS code is treated as a unique office visit, with the number of overall office visits thereby defined based on the number of such claims.

Office Visits-Minutes in Office: This measure represents office visit utilization in terms of the number of minutes actually spent in the office. Minutes are quanitified based on the underlying HCPCS code, each of which nominally corresponds to a visit of a certain length.

Outp ER Visits-Count: This measure captures the number of visits made to an outpatient ER setting. This visit count covers all ER visits, including those leading and not leading to an inpatient admission. Visit information is drawn from comprehensive statewide ER visit data that is maintained by New York, through its SPARCS system, and not from claims data.

Amb Surgery Visits-Count: This measure captures the number of ambulatory surgery visits, within the confines of a hospital facility. Visit information is drawn from comprehensive statewide ER visit data that is maintained by New York, through its SPARCS system, and not from claims data.

CMS Compare Measures

CMS Outcome Ratings: Outcome measures are at a hospital-level and are taken from CMS's 2014 Hospital Compare Data. They focus on visits involving heart attacks (MI), heart failure (HF), and pneumonia (PN). These rates reflect odds of death or readmission within 30-days, in percentage terms, conditional on initial hospitalization for the listed condition. For example, a heart attack mortality rate of 15% implies that if an individual is hospitalized for a heart attack, they have a 15% likelihood of death within 30 days of that hospitalization (at that particular hospital). In addition, these rates are risk-adjusted for hospital case-mix. Altogether, these measures are inversely related to quality, as higher rates correspond to greater rates of mortality and readmissions.

CMS Process Ratings: Process measures are at a hospital-level and are taken from CMS's 2014 Hospital Compare Data. They gauge the degree of adherence to medical guidelines for treatment of heart attacks, heart failure, and pneumonia. Among the subset of hospitalizations for which each specific measure is applicable (i.e.-heart attacks), these rates reflect the share of hospitalizations across which process was followed. For example, a rate of .85 for heart attacks implies that for a particular hospital, process was adhered to 85% of the time. Possible process guidelines include, for example, the timely and appropriate administering of Aspirin, antibiotics, beta-blockers, and vaccines. Altogether, these rates are directly proportional to quality, as higher rates correspond to greater process adherence.

Other Hospital Characteristics and Outcome Measures:

Major Teaching Hospital: This measure jof hospital status is constructed based on American Hospital Association classifications of major and non-major teaching hospitals, which are tracked in its annual hospital survey data. These original AHA definitions are linked to the inpatient claims data on the basis of AHA hospital codes.

Conditional Prevent ER: This measure identifies could have been avoided, either through improved outpatient care in the past, or which could have been treated in the outpatient setting even in the present. It is constructed using an NYU-developed algorithm, operating off diagnosis and procedure information.

Pharmaceutical Outcome Measures

Overall Prescription Count: This measure captures the number of prescriptions filled by an individual. It is derived from prescription claims data and corresponds to the number of total claims, given that each claim corresponds to a single prescription filled.

Overall Prescription Spending: This is a standardized measure of prescription drug spending. Across public Medicaid claims, this measure is based off the amount actually paid for a drug. Across private Medicaid claims, the spending amount is instead based on the public Medicaid unit rates for that same NDC code, given the absence of actual paid amounts in the private data. As such, this measure can be seen as a standardized measure of utilization and spending, since the rates used are invariant to payer.

Branded Presc. Count/Presc. Spending: These outcome variables relate to drugs that are classified as branded, based on whether their manufacturer originally held patent protection for the drug. This category is inclusive of drugs manufactured by the original patent holder, but for which patent protection has expired. This drug status is derived from Lexi-Comp drug characteristics data, and is linked to the original claims using NDC codes.

Generic Presc. Count/Presc. Spending: These outcome variables relate to medications that are manufactured by generic drug companies, which would not be subject to any patent protections. This drug status is derived from Lexi-Comp drug characteristics data, and is linked to the original claims using NDC codes.

Acute Drugs: This outcome variable relates to drugs that are classified as acute-oriented, based on their underlying therapeutic class in Lexi-Comp drug characteristics data. Altogether, this category consists of drugs which, if not taken, produce an increased probability of an adverse event within a month or two. These classifications coincide with the definitions of Chandra, Gruber, and McKnight (2010), which placed therapeutic classes such as antibiotics and antitoxins into this acute category.

Chronic Drugs: This outcome variable relates to drugs that are classified as chronic care oriented, based on their underlying therapeutic class in Lexi-Comp drug characteristics data. Altogether, this category consists of drugs which, if not taken, produce an increased probability of an adverse event within a year. These classifications coincide with the definitions of Chandra, Gruber, and McKnight (2010), which placed therapeutic classes such as beta-blockers, hypertension medications, and statins into this chronic category.

Discretionary Drugs: This outcome variable relates to drugs that are classified as discretionary oriented, based on their underlying therapeutic class in Lexi-Comp drug characteristics data. Altogether, this category consist of drugs which, if not taken, produce no increased risk of an adverse health-outcome. These classifications coincide with the definitions of Chandra, Gruber, and McKnight (2010), which placed therapeutic classes such as antihistamines and acne medications into this discretionary category.