1-1-2014

Essays in Health Economics and Public Finance

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Abstract
This dissertation focuses on topics in health economics and public finance. I deal with questions that have importance for health policy, and that are simultaneously of general economic interest; in particular, I consider the efficiency impact of privatization, the effects of competition in health care markets, and the effects of incomplete contracting and imperfect competition on rates of pass-through to consumers and governments.

In Chapter One, I examine the extent to which contracting out by governments yields efficiency improvements, by looking to Medicaid contracting in New York State. To identify the efficiency impact of private, relative to public Medicaid, I exploit involuntary switching between the two; primarily, I leverage age-based rules forcing individuals to switch from private to public Medicaid at 65. I also leverage unique administrative data, which longitudinally tracks individual utilization across the public and private Medicaid settings. I find evidence that private Medicaid yields efficiency improvements, but find no evidence that these improvements are passed on to either governments or patients. Instead, I find that pass-through is substantially limited by incomplete contracting, with plans shifting costs to medical services that remain under government provision.

In Chapter Two, I examine the effects of cost-sharing among a previously understudied population-those dually enrolled in Medicaid and Medicare. I leverage an exogenous court ruling that resulted in loss of Medicaid coverage in Tennessee, among 25,000 individuals who had previously been dually-enrolled. This disenrollment resulted in an increase in average cost-sharing rates, from around 0% to around 20%. I find that this cost-sharing increase resulted in a utilization reduction of about 30%, implying an arc-elasticity in spending of about -.2.

In Chapter Three, with Mark Duggan and Amanda Starc, we examine how contracts are affected by their generosity, by looking to the Medicare Advantage program. In doing so, we exploit a substantial policy-induced increase in MA reimbursement in metropolitan areas with a population of 250,000 or more relative to MSAs below this threshold. Our findings also reveal that about one-eighth of the additional reimbursement is passed through to consumers in the form of better coverage.

Degree Type
Dissertation

Degree Name
Doctor of Philosophy (PhD)

Graduate Group
Applied Economics

First Advisor
Mark G. Duggan

Keywords
Cost-sharing, health care policy, Managed Care, Medicaid, Medicare, Privatization

Subject Categories
Economics | Health and Medical Administration

ESSAYS IN HEALTH ECONOMICS AND PUBLIC FINANCE

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A DISSERTATION

in

Applied Economics

For the Graduate Group in Managerial Science and Applied Economics

Presented to the Faculties of the University of Pennsylvania

in

Partial Fulfillment of the Requirements for the

Degree of Doctor of Philosophy

2015

Supervisor of Dissertation

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ESSAYS IN HEALTH ECONOMICS AND PUBLIC FINANCE

Dedication

For my parents, who gave me zest for life, abundant curiosity, audacious ambition, and a moral compass.

Acknowledgements

This dissertation could not have been possible, first and foremost, without the many efforts of my parents. They grew up in a totalitarian Soviet society that was bleak for everyone, but especially bleak for Jewish families like ours. But rather than extinguish their love of life and knowledge, that darkness made it all the brighter and illuminates my life still. When I was five, they left the Soviet Union and brought me to America, in search of a better life. My parents began their American journey with nothing, and yet were ultimately able to give me everything. Thanks to them, over a twenty-five year timespan, I could go from being a Medicaid recipient to a Medicaid scholar. It is thanks to them that I also had a great amount of fun along the way, growing up in a beautiful Maine town, with all the amusements that this entailed: fishing in the summer, skiing in the winter, and lots more in between. While their expectations for me were perenially high, my parents simultaneously allowed me to succeed on my own terms, and to define success for myself.

I must thank the many others who helped me on the way.

My undergraduate advisors from Dartmouth (Doug Staiger, Eric Zitzewitz, Chris Snyder, and Jon Zinman) first sparked my interest in economic research, and helped me develop economic intuition and data skills that will long serve me well. Doug Staiger and Jon Skinner also continued to be of immense help long after Dartmouth, providing me with data access and research support through the Dartmouth Institute; such support has been critical to the completion of this dissertation.

Thanks must also go to the members of my dissertation committee, particularly Mark Duggan and Jon Gruber. Mark and Jon have taught me a substantial amount about the conceptual as well as methodological aspects of research, through our shared co-authored work as well as through their oversight on my sole-authored papers. They have been a joy to work with, personally as well as professionally. Mike Sinkinson, Amanda Starc, and Bob Town also have provided fantastic research support. In addition, I would like to thank Jon Kolstad, who has been a great research mentor and also a great friend (and who would have been on my committee, absent his move to Berkeley!)

I would like to thank my fellow pre-docs at NBER, particularly Adam Sacarny and Mark Shepard, for all that they taught me and for their many insights and feedback. But of all of my fellow graduate students, I would most like to thank Jacob Wallace, who has taught me a great amount about Medicaid and whose research has done much to enhance my own; most importantly, he has been an incredible friend.

I would like to thank Colleen Fiato at New York State's Department of Health for her help with obtaining SPARCS discharge data. I would also like to thank Jean Roth for her help with data applications and data processing.

Finally, I must thank my friends, classmates, and other special people who made graduate school full of fun and joy. In no particular order, they incude Jihae Shin, Andy Wu, Andrew Johnston, Teddy Turesky, Miguel Garrido, Tyler Jorgensen, Eric Moore, Alex Zevelev, Artem Shvartsbart, Emil Pitkin, Timothy Stafford, Alan Kwan, Matt Miller, and Cullen Roberts.

ABSTRACT

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This dissertation focuses on topics in health economics and public finance. I deal with questions that have importance for health policy, and that are simultaneously of general economic interest; in particular, I consider the efficiency impact of privatization, the effects of competition in health care markets, and the effects of incomplete contracting and imperfect competition on rates of pass-through to consumers and governments.

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CHAPTER 1: The Magnitude and Incidence of Efficiency Gains Under Contracting: Evidence from Medicaid

Boris V. Vabson

1 Introduction

Governments contract a vast array of services to private firms, rather than administering these services directly; in the United States alone, contracting accounts for almost 10% of GDP.

Governments contract with the expectation that private firms produce services more efficiently, and that efficiency gains from contracting could be passed through to either governments (via cost-savings) or program recipients (via quality improvements) (Savas 1977, Savas 1987). Unfortunately, there has been limited examination of contracting's efficiency impact, along with the magnitude of pass-through to various parties.

To examine these questions, I focus on contracting in the public health insurance setting, specifically within Medicaid. While this setting should be generalizable to other forms of contracting, Medicaid also has substantial policy importance, in its own right. Medicaid expenditures for 2013 stood at $449 billion, or 2.5% of GDP, and Medicaid currently covers more individuals (55 million) than any other insurance program in America, including Medicare (CMS, 2013). In addition, contracting has been pervasive in this setting, with private plans covering 60% of all Medicaid enrollees (KFF, 2011) and taking in $130 billion annually (DHHS, 2013); by comparison, typical annual spending on unemployment insurance is $30-40 billion (Whittaker et al, 2014).

This paper has additional policy relevance, given that it focuses on Medicaid contracting among a specific group-the disabled. This group accounts for a disproportionate 40% of Medicaid spending (while making up only 15% of enrollees), yet has been understudied relative to lower cost groups. Moreover, government expenditures on disabled programs are substantial, with health care accounting for the largest share of this spending (surpassing spending on cash transfers).

As is the case elsewhere, contracting within Medicaid involves the outsourcing of provision to third parties (outside insurers); simultaneously, it involves a change in the nature of that provision, with a shift from a fee-for-service to a capitated managed care set-up.

To start, I examine the extent to which private Medicaid, through this combination of managed care and capitation-yields efficiency improvements, and the mechanisms through which it does so. Here, I define efficiency improvements as reductions to care that patients wouldn't want (such as unnecessary or preventable visits), as opposed to care that patients would want (such as elective surgeries, which could improve patient welfare). In doing so, I contribute to an existing literature (Cutler and Sheiner 1998, Cutler et al 2000, Landon 2012, Pinkovskiy 2013), while overcoming two issues that have hindered past research.

One issue is an identification problem (enrollment composition differences between managed & non-managed care), which I overcome through a novel approach. I leverage a New York rule barring most Medicaid recipients over 65 from private Medicaid; this rule results in substantial involuntary switching from private to public Medicaid at the age of 65. For my secondary identification strategy, I focus on cases of involuntary switching in the reverse direction-from public to private Medicaid.[1] Finally, where possible, I combine these two strategies as a robustness check, by instrumenting for pre-65 private Medicaid status.

Another issue is data-related (limited information on activity within managed care plans), which I overcome by linking together several unique administrative data sets from New York State. In doing so, I construct an individual-level panel on hospital and prescription drug utilization, across the private and public Medicaid

[1] This was driven by Medicaid managed care enrollment mandates, which have been featured in previous literature (Duggan 2004; Aizer et al 2007; Duggan, 2013).

settings. Critically, inclusion in this panel is not conditional on utilization. Detailed information in the data allows me to precisely construct treatment & control groups, and to implement a rich set of controls.

I also examine the incidence of efficiency gains under Medicaid contracting, while measuring pass-through in a broader and more precise fashion than the existing literature (Duggan 2004, Aizer et al 2007, Duggan et al 2013). Further, I consider how factors such as incomplete contracting limit the pass-through of efficiency gains.[2] In this paper, I focus on a particular form of incomplete contracting (of limited scope), and identify the benefit from broadening an existing contract along that dimension.

Finally, I consider whether the for-profit status of a contracted insurer affects the size of efficiency gains, contributing to an existing literature on for-profit status across insurance and other firm types (Dafny et al 2013, Duggan 2000).

In examining these questions, I look to disabled Medicaid recipients in New York during the period between 1999 and 2010. I focus on the disabled population, given that efficiency improvements may be easier to detect (and also bring about) among a high-cost group, and also given that Medicaid policies pertaining to them remain unsettled. I also focus on this population for empirical reasons; the disabled are the primary group enrolled in Medicaid immediately pre-65 (the age discontinuity on which I focus), and also experience less churn into and out of Medicaid than other populations. Finally, I focus on New York, on account of unique administrative data covering that state.

I first consider private Medicaid's effect on inpatient and ER care, since this would be a likely setting for efficiency improvement. I find that private Medicaid results in

[2] 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, such theory has not been empirically tested (Hart 1995; Hart, Shleifer, Vishny 1997).

a highly significant, 15% reduction in overall inpatient utilization. Four-fifths of this decrease can be attributed to a reduction in prevention sensitive visits, including a 30% reduction in hospital readmissions, while non-preventable visits see a more modest reduction. This utilization reduction does not appear to come from shifts to more efficient hospitals, but may instead arise from changes to outpatient care, or from within-hospital changes to treatment. Given that these utilization reductions do not appear to adversely affect patient welfare, I conclude that they are efficiency improving.

In addition, I examine the incidence of efficiency gains under contracting, finding no evidence of pass-through to governments via cost savings. Altogether, I can rule out a reduction in government spending in excess of 6%, with 95% confidence. Further, I examine how incomplete contracting reduces pass-through, by identifying counterfactual cost levels under a more complete contract. To do so, I look to medical services, particularly prescription drugs, that were not financially integrated into private Medicaid contracts, and that remained covered by New York State. I find evidence that private plans cost-shifted to these services, as their use increased by about 15% following involuntary switching into private plans. In addition, eventual financial integration of these services into existing private contracts resulted in a comparable 15% drop in drug utilization (through the implementation of a formulary), and in a 4% decline in overall contracting costs (that is, in overall government spending).

Finally, I find suggestive evidence that for-profit contractors (relative to not-for-profits) are more efficient, as they achieve lower inpatient utilization, and do so primarily through reductions to prevention-sensitive visits.

Altogether, my findings have substantial implications for Medicaid policy, given the prevalence of Medicaid contracting and given proposals to further expand it. I find that Medicaid contracting may produce substantial efficiency improvements, and

I also identify the policy design that could maximize pass-through of these gains.

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 Contracting out of Medicaid Services

When Medicaid was initially introduced in 1965, only the 'public' version of it was offered, which was administered directly by states, through a fee-for-service framework. In the 1980's, however, governments increasingly began to contract out Medicaid (along with other government services) to third parties, given theoretical benefits that could result from capitation and from competition (Hansmann 1980, Donahue 1989). However, as governments contracted out Medicaid coverage, they not only changed the source of that provision, but also the very nature of it; while public Medicaid was based on a fee-for-service framework, private Medicaid was largely based on capitated managed care. As such, the impact of contracting out Medicaid services is not only dependent on the economics of contracting, but also dependent on the economics of capitation & managed care. Below, I go over the basic characteristics of managed care and fee-for-service delivery frameworks. I also go over institutional aspects of the New York Medicaid program that are relevant to my subsequent analyses.

2.1 Background on Managed Care

Capitated managed care plans, which first appeared in the 1980's, are characterized by an active involvement in patient care, and by the variety of strategies they adopt for improving efficiency. Managed care plans are also characterized by the sharp incentives they face, through capitation; under capitation, plans receive fixed payments for each enrollee, irrespective of the amount of health care the enrollee uses. In these respects, managed care differs substantially from fee-for-service (FFS) delivery systems, in which an insurer acts as little more than an intermediary, and does not proactively shape enrollees' care.

Managed care plans could achieve efficiency improvement, first, through efforts at care coordination; plans usually 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. In addition, managed care could achieve efficiency improvement by requiring special approval for certain visits and procedures; this occurs through a process called 'utilization review'.

Finally, managed care plans could improve efficiency by featuring more efficient providers in their networks. Given that enrollees would have to either pay higher cost sharing for out-of-network utilization (or in some cases, be prohibited from it entirely), this could directly translate into more efficient care. More generally, provider network formation could also reduce provider prices, through negotiation (Zwanziger et al 2000); however, within Medicaid, provider pricing is actually around 15% higher under the private option, given administrative rate setting under the public option.

While previous literature has found evidence of lower provider prices under managed care (Cutler et al 2000), evidence on its quantity impact is more mixed. The most accepted findings may come from the RAND Health Insurance Experiment, which concluded that managed care reduces hospital utilization by around 40% (Manning et al 1987). However, the RAND experiment findings are dated and could also have limited external validity.

Previous studies have been constrained by enrollment differences between managed and non-managed care; these enrollment differences, along with advantageous selection into managed care, have been extensively documented (Morrissey 2012, Brown et al 2014). Further, there is limited individual-level data available on treatment within managed care plans, particularly in Medicaid and Medicare. Private managed care plans participating in these programs are typically not required to turn over claims data, and the limited data that are turned over appear to be of questionable quality (Lewin Group 2012).

2.2 Private Medicaid Nationwide

Governments began contracting out Medicaid provision in the early 1980's, with an aim towards quality improvements and cost savings. However, previous studies have offered no evidence of cost savings to governments, and in fact some evidence of cost increases, as a result of Medicaid contracting (Duggan 2004, Song et al 2012, Duggan et al 2013).

Medicaid currently serves over 65% of enrollees through some form of managed care, although managed care's share is highest among lower-cost groups such as children (at around 80%) and lowest among the disabled (at around 25%) (KFF 2012). Given the limited number of high-cost Medicaid enrollees enrolled in managed care, payments to these plans account for only about 20% of Medicaid expenditures. However, a number of states have or are in the process of shifting high-cost Medicaid enrollees (including the disabled), into managed care plans.[3]

[3] Besides New York, these include California, Florida, Illinois, Louisiana, Ohio, and Texas (Sparer 2012).

Incidentally, Medicare contracting also began during this period, with private Medicare currently covering about 30% of Medicare recipients (McGuire et al 2011).

There is significant heterogeneity across states in terms of the design of their privatized Medicaid systems. For example, there is significant variation in the types of insurers that states contract with, such as for-profits, and heterogeneity in ease of market entry. Moreover, there is heterogeneity in the scope of services that managed care plans are allowed to cover. To this end, some states do not permit private plans to cover long-term care or prescription drug care, or mental health care, and instead administer these service on behalf of all Medicare recipients (including those in the private option). Further, there is cross-state variation in whether enrollment in private Medicaid is required, optional, or even available for a given group (Duggan et al 2013).

2.3 Private Medicaid in New York State

New York State began contracting out Medicaid coverage in the 1990's, but retained a public version of Medicaid alongside the private option. Initially, all Medicaid recipients eligible for the private option could remain under the public system. Furthermore, certain types of Medicaid recipients were ineligible for the private option; these included long-term nursing home residents & dual-eligibles (those Medicaid recipients who were simultaneously in Medicare). Consequently, most of those who were enrolled in private Medicaid plans would get disenrolled at the age of 65, given the typical onset of dual-Medicare/Medicaid enrollment at that age (Sparer 2008).

To increase the share of Medicaid recipients in private plans, New York started making enrollment in these plans mandatory-rather than optional-beginning with non-disabled Medicaid recipients. These requirements, referred to as 'enrollment mandates', were rolled-out under a pre-planned timetable, county-by-county. The enrollment mandates initially applied only to children and TANF adults in New York Medicaid. Disabled Medicaid recipients were not subject to these mandates until 2005, when they were introduced specifically for this group (Sparer 2008). Just as with the previous non-disabled 'mandates', these were rolled out on a pre-planned timetable, and on a county by county basis (Sparer 2008). Concurrent with these 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).

In terms of scope of coverage, New York's private Medicaid plans initially covered inpatient, outpatient, and certain long-term care services; however, by law, these plans could not cover particular medical services, such as prescription drugs and mental health. These 'carved-out' services continued to be directly administered and paid for by the state. Starting in October 2011, however, New York State integrated prescription drug services into all existing private Medicaid contracts.

In terms of market access, insurers generally enjoyed free entry into the marketplace, so long as they provided basic proof of competence and qualification. Insurers could enter the market at the county-level, and thereby be active in some counties and not in others. While entry was open to all forms of insurers, including for-profit and not-for-profit, the preponderance of active insurers were not-for-profit.

Payment levels to each insurer were determined through negotiation between that insurer and the state government. Prior to 2008, New York state did not explicitly risk-adjust payments, to account for the health status of each Medicaid recipient (Sparer 2008).

3 Data

In this paper, I use several administrative datasets from CMS and New York State, containing information on overall Medicaid & on private Medicaid enrollment status, along with inpatient, ER, and drug utilization. Uniquely, this utilization data is structured as an individual-level panel, tracking those in private Medicaid, public Medicaid, as well as those who switch between the public & private options. Another unique feature of the data is that sample selection is not conditional on utilization; even those with zero inpatient/ER activity remain included.

The structure of this data allows me to overcome issues hampering previous research. First, the data source typically used in Medicaid research, stand-alone discharge data, suffers from a sample selection issue; inclusion in it is conditional on hospitalization (on a related point, stand-alone discharge data has a cross-sectional, rather than panel structure). Further, the alternative-private Medicaid utilization data-has been difficult to obtain and has been of questionable quality even when available (Lewin Group, 2012).

Using information contained in the administrative data, I can precisely construct cohorts that are relevant to my analyses. First, I restrict to New York State Medicaid recipients, who qualified for the program as a result of disability (formally, this group is referred to as non-elderly SSI recipients), and who were enrolled during that particular month & year.

For my analyses of utilization effects, I focus on those Medicaid recipients who were not simultaneously-enrolled in Medicare, pre-65. I further restrict to individuals who were in the original sample just before they reached the age of 65, while focusing on the age range immediately around 65.

For my analyses of government spending effects, for which I leverage mandates, 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 entry/exit into Medicaid (Currie and Fahr 2005).

3.1 Individual Characteristics and Enrollment Information

I use administrative data from CMS (Centers for Medicare & Medicaid Services), which covers New York State for the 1999-2010 period. The data contains person-month level demographic and Medicaid/Medicare enrollment information; it specifies private Medicaid status, the general reason for Medicaid eligibility, and concurrent Medicare enrollment status. Using this information, I can restrict to those who are in Medicaid by virtue of disability. In addition, I can track whether individuals were simultaneously eligible for Medicare. Finally, I can control for certain demographic characteristics such as county of residence, age, and date of birth.

For those in private Medicaid, the CMS data also tracks the specific plan that someone's enrolled in, at a person-month level, based on a 'Plan ID'. Using supplemental data obtained from New York State, I identify which of these plans are for-profit and which are not-for-profit.

3.2 Inpatient and ER Utilization Metrics

I track inpatient and emergency room usage for everyone in Medicaid, including those enrolled in the private option. I do so by linking together Medicaid enrollment data (obtained from CMS) and visit-level hospital/ER data (obtained from New York State). This linking is facilitated through Social Security number information found in both data, which was obtained through special administrative permissions.[4] The data contains every single inpatient visit made by New York Medicaid recipients, throughout the 1999-2010 period; the data on ER and ambulatory care visits, meanwhile, covers the 2005-2010 period.

Unfortunately, I do not have visit-level information on outpatient activities, which account for 65% of health care spending for this population. This said, the inpatient and ER settings could be likely sites for efficiency improvements, as these represent particularly expensive forms of treatment. As such, even if reductions in inpatient utilization are offset by increases in outpatient utilization (offsets which-unfortunately-I can't explicitly measure), such reductions should still be worthwhile. Further, various information in the inpatient data can proxy for outpatient utilization, as I will describe.

This data provides information on the timing of each hospital visit, at a month-year level. The data also provides visit-level information on treatment intensity and composition, including the length of hospital stay, types and number of procedures performed, and total (pre-discounted) hospital charges. I also identify surgical and non-surgical visits, using a DRG cross-walk obtained from the Dartmouth Institute for Health Policy and Clinical Practice. In addition, this data provides information on the source of care, such as whether a hospital admission originated from ER. Finally, the data specifies the name and location of the hospital visited, allowing me to compile additional hospital characteristics measures, and also measure each patient's travel time to the hospital (in miles and minutes).

[4] Linking was conducted using a combination of the last four digits of individuals' SSN, dates & years of birth, gender, and county of residence; in combination, these variables uniquely identify Medicaid recipients over 99.9% of the time. The Medicaid recipients that were not uniquely identified were excluded from the sample.

To obtain this identifiable information, I applied for a special version of New York's SPARCS data, containing these aforementioned fields. I also applied for special CMS data, containing SSN identifiers for every Medicaid recipient.

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 Medicaid status (as well as private Medicaid status) is taken from the original CMS files, rather than from the discharge data; in doing so, I bypass possible issues of payer mischaracterization/miscoding in the discharge data.

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 groups-those initially in private Medicaid and those in the public option-who correspond to our treatment and control groups. I find that those initially in private Medicaid have substantially lower utilization (by 20-30%) than those in the public option, although the extent to which this is driven by enrollment composition rather than treatment differences is not readily apparent.

3.3 Inpatient Quality of Care Metrics

To measure quality of care, I rely on outside measures of hospital quality. and also construct metrics using existing algorithms.

To gauge inpatient care quality, I look to CMS quality measures. These measures consist of risk-adjusted mortality and readmission rates for each hospital, for heart attacks, heart failure, and pneumonia. As an additional metric of quality, I use the 60-day hospital readmission rate, which can proxy for the quality of inpatient as well as outpatient care.

Given that I do not have outpatient claims data, I cannot directly identify the quality of outpatient care. Instead, to gauge this, I combine my discharge and ER data with existing algorithms. First, I use an algorithm developed by AHRQ, which identifies hospital visits that could have been prevented through improved outpatient treatment. In addition, I identify ER visits that were non-emergency or preventable, using an algorithm developed by the NYU Center for Health and Public Service Research.

In Table 1, I present average, annualized quality of care measures for my treatment & control groups. These measures indicate that those in public Medicaid have higher rates of readmissions and preventable ER visits, although this could partly be a function of their higher overall levels of utilization.

3.4 Government Expenditures Metrics

I construct individual-level measures of government Medicaid expenditures, using fields from the CMS administrative data. I track Medicaid spending at a person-month level, including the overall level and spending on various subcategories of care (such as long-term care, inpatient care, and pharmaceuticals). Separately, I track government payments to private Medicaid plans, at a person-month level, for each private Medicaid enrollee

3.5 Pharmaceutical Data

I track prescription drug utilization, using claims-level data obtained from CMS. This data covers all Medicaid recipients in New York (including those in private Medicaid plans), for the 1999-2011 period. Prior to October 2011, prescription drug utilization was tracked directly by the state, for all Medicaid recipients (including private ones). For the period subsequent to October 2011, the data also covers public and private enrollees, but it comes from two different sources-New York state and private plans.[5]

[5] At that point, private plans started covering drug services directly, and also became responsible for tracking these drug services, while the state tracked drug activity for public enrollees; fortunately, all of these data make use of a standard format, and can be linked across time.

This data includes information on the types of drugs bought, including individual drug-identifiers (NDC codes), as well as generic vs. non generic status. In addition, the data specifies the quantity of each drug included in a prescription, the overall cost of that drug, the fill date associated with each claim, along with the ID of the prescribing physician. Finally, the data includes beneficiary-level identifiers, which facilitate linking to other administrative Medicaid data.

3.6 Additional Data

I obtain county-level data, from the New York State Department of Health, on the timetable of New York's private Medicaid Enrollment Mandates. These mandates were implemented in a staggered fashion, across counties and across years. New York State implemented two different types of enrollment mandates-one specific to children & non-disabled adults (non-SSI) and the other specific to disabled & other populations (SSI). For the timetable of disabled (SSI) mandates, see Appendix Table 11. Note that the mandates were fully implemented about one year subsequent to the listed 'official' dates.

4 Identification and Empirical Strategy

Public and private Medicaid not only differ in the treatment they provide patients, but also in their enrollment composition; in fact, when the choice between public and private Medicaid is voluntary, private Medicaid typically attracts a healthier set of enrollees (Glied 2000, Morrissey 2012, Brown et al 2014). As such, any naive comparison between public and private Medicaid may capture patient composition differences between the two, rather than possible treatment differences. To decompose the effects of treatment from those of patient composition, I focus on situations where individuals involuntarily switch between the public and private options; in such situations, only private Medicaid enrollment status will change, while patient composition will remain fixed

For my primary identification strategy, I implement a differences-in-discontinuities strategy. This strategy leverages involuntary switching from private to public Medicaid at the age of 65, among those initially in the private option. This involuntary switching is driven by a New York State rule prohibiting private Medicaid enrollment among those simultaneously in Medicare; Medicare eligibility, meanwhile, typically arises at 65. Given that my identification strategy relies, at least indirectly, on the the age 65 Medicare eligibility rule, it relates to the approach taken by Card, Dobkin, and Maestas (2008) in their study on Medicare. Altogether, those initially in the private option, pre-65, will make-up the treatment group, while those initially in the public option will form the control group. Both the treatment and control groups will be restricted to those who were only in Medicaid (and not simultaneously enrolled in Medicare), pre-65.

Not everyone in the treatment group will be subject to the actual treatment (a small fraction will remain in private Medicaid, post-65, as some are ineligible for Medicare post-65 [6]); as such, the results would capture an intent-to-treat effect, and would need scaling to reflect the effect of the actual treatment.

At 65, those switching from private to public Medicaid will concurrently gain Medicare coverage, requiring that I separate out the effect of Medicare. My research design facilitates this, given that the control group gains Medicare coverage, but has unchanging Medicaid status. As such, the differential between the treatment and control group effects would reflect the impact of private Medicaid disenrollment.

[6] 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.

The primary identifying assumption is that Medicare's effect is identical across the privately and publicly Medicaid enrolled. This identifying assumption could be threatened if Medicare's effect is heterogeneous across sickness levels. To address this concern, I re-run all of these analyses using an instrumented measure of private (as opposed to public) Medicaid status, immediately pre-65; this instrument can be constructed using my secondary identification strategy, which I review below. With this instrument, I can set private Medicaid status to be independent of health and other characteristics, and thereby satisfy the identifying assumption.

Another identifying assumption is that no differential pre-trends exist between the treatment and control groups. As part of this, the assumption is that individuals are not strategically delaying or hastening care, in anticipation of coverage changes at 65. To confirm the validity of this assumption, I check for visual as well as statistical evidence of differential pre-trends.

A final identifying assumption here is that no concurrent changes are taking place at 65 (apart from those mentioned above), which would differentially impact the treatment group. One potential concern, that individuals' employment status often changes at that age, should not be applicable to the sample here; the treatment as well as control groups are made up entirely of SSI disability recipients, who have limited labor market activity. No other relevant changes appear to take place then, such as to medical coverage, disability status, or employment.

Altogether, 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. For this baseline specification, the sample is restricted to disabled Medicaid enrollees in New York in 1999-2010.

Since the estimated value of β_2 will reflect the intent-to-treat impact, it needs to be scaled to reflect the actual effect of treatment, based on the fraction of those initially in private Medicaid actually switching to the public option, at 65.

As part of my secondary identification strategy, I focus on involuntary switching from public to private Medicaid plans; this takes place under newly introduced enrollment requirements-or 'mandates'-that required certain groups of Medicaid recipients to enroll in the private option. As previously discussed, I also combine this secondary strategy with my primary strategy, as a robustness test of the results from the primary strategy alone. 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.[7]

Even among this treatment group, of Medicaid-only enrollees, some individuals may be exempt from mandated private Medicaid enrollment. Unfortunately, the nature of these exemptions makes it difficult to identify who is and who isn't exempt; moreover, since exemption status may be endogenous to health status, individuals with exemptions remain as part of the sample.[8]

Altogether, the key instrument here is based on whether an enrollment mandate was ALREADY in effect in an individual's county of residence. Below, I present the first stage regression for this, estimating the effect of mandates on private Medicaid

[7]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 before the implementation of these mandates, Medicaid recipients had the option to voluntarily enroll in private Medicaid, and about 35% of them did so.

[8]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.

enrollment status, 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 #2**)

In the second stage regression, shown below, I estimate the effect of private Medicaid on 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.

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

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

(**Equation #3**)

For this identification strategy to be valid, on its own, mandate counties must be on parallel trends to non-mandate counties, or the non-parallel trends should be fully captured by my linear and trend controls. However, when this strategy is combined with my primary one, these identification assumptions can be substantially relaxed.

5 Results

5.1 The Impact of Age 65 & of Enrollment Mandates on Managed Care Enrollment Status

The Impact of Age 65

In Figure 1, I graphically document a sharp drop in private Medicaid enrollment rates-at age 65-among those initially in private Medicaid (as of age 63). This decline at 65 is much sharper than the prevailing pre-65 downward trend in Medicaid enrollment rates. Further, no corresponding change is observed at 65, among the group initially in public Medicaid.

Next, I statistically document this relationship, and find that about two-thirds of those initially in private Medicaid switch to the public option, at age 65 (the relevant point estimate is found in Table 2, column one, under the Post65*InitiallyPrivate term). For these analyses, I use 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 in Medicaid-only (by virtue of disability) at age 63, and to the 63 to 67 age range.

The measured effect is two-thirds, rather than complete (that is, not all of those initially in private Medicaid involuntarily switch to the public option, at 65). This can be partly attributed to ongoing switching from private to public Medicaid over the pre-period, among 20% of the original cohort (as implied by the point estimate on the Initially Private term, in column one). Further, many of those in private Medicaid will not be forced to switch to the public option at 65, as not all will be Medicare-eligible at that age (which is the underlying driver of private Medicaid disenrollment).[9] Given that private Medicaid disenrollment only affects part of my 'treatment' group, at 65, 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, my results will need to be scaled by 1.5.

I perform an additional robustness check, examining whether the treatment and

[9]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.

control groups gain supplemental Medicare coverage at the same rate, at 65. I find no statistically significant difference in the rates at which these groups obtain such coverage at 65 (around 80% gain coverage, as reported in column 2).

The Impact of Enrollment Mandates

I also explore the impact of enrollment mandates on switching from public to private Medicaid. Altogether, I find that the imposition of mandates was associated with a 20-30% increase in the corresponding share of Medicaid recipients in the private option. The results are presented in Appendix Table 1, based on the baseline specification (from Equation 2). These estimates 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 exempt from mandates).

The effect of mandates on private Medicaid status is somewhat limited, since certain Medicaid recipients (such as long-time nursing home residents) are not subject to them. Unfortunately, there is no way to precisely identify individuals who are exempt, based on the information contained in the administrative data, resulting in some exempt individuals being assigned to my treatment group.

5.2 The Impact of Private Medicaid on Overall Inpatient Utilization

Using my primary instrument for private/public Medicaid status, I consider its effect on overall inpatient hospital use, as well as inpatient use on the extensive margin. While hospital care accounts for over one-third of health care spending, the effects observed in this setting might not carry over to other settings; rather, hospital care reductions may be offset by outpatient care increases. However, previous studies provide no evidence of such offsets (Manning et al 1987), meaning that any osensible efficiency improvements in this setting might not be an artifact of efficiency deteriorations elsewhere.

Further, the hospital setting may be a sensible setting for detecting efficiency improvement, given that hospital care is expensive relative to other medical services. As a corollary, the magnitude of efficiency improvement in the inpatient setting-from managed care-might not be representative of the impact on other care settings.

In my main results, my instrument for private (as opposed to public) Medicaid enrollment status is based on whether someone initially in private Medicaid has reached 65. 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. I present a companion set of results, based off my secondary instrument (enrollment mandates), in the Appendix.

In Figures 1 and 2, I document a sharp jump in annual inpatient days and total hospital visits, at 65, among those initially in private Medicaid. Further, I show that this discontinuity is absent among those initially in public Medicaid. Finally, I document an absence of differential pre-trends and find no evidence of differential post-trends (suggesting that my results are not driven by pent-up demand). Since the colored bands in this graph reflect 95% confidence intervals, these effects appear to be statistically significant.

I proceed to statistically examine the effect of age 65 on various annualized measures of inpatient utilization, such as the number of visits and the inpatient days stayed. My estimates, 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 other utilization metrics (such as annualized days in hospital) appear comparable, suggesting that much of overall impact may be through the extensive margin. However, decomposing the extensive (vs intensive) margin effects could be challenging, given possible extensive margin changes to visit composition. As shown in the bottom two panels, my estimates are also robust to using a narrower age window (64 to 66), and also to instrumenting for initial private status using my secondary strategy. The latter strategy is meant to confirm that the main effect is not driven by differential impact of supplementary Medicare coverage, particularly across health levels.

In Table 3, the key point estimates are found under the Initially Private*Post 65 term, and need to be scaled by 1.5 to get at the actual effect of switching from private to public Medicaid (given that 65% of those initially in private Medicaid switched to public Medicaid, at 65). For example, for total days stayed (under column 1), 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. Given the corresponding standard error, I can rule out an increase under 9%, or above 27%, with 95% confidence. 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. This limited impact should ease concerns about possible threats to identification, from onset of supplemental Medicare eligibility. Finally, I find that the point estimate on the Initially Private Medicaid term is a highly significant -1.201; this estimate reflects the magnitude of advantageous selection into private Medicaid.

In Appendix Table 3, I present results for these same outcome variables, using an alternate identification strategy (enrollment mandates). While the main point estimates for these results are not significant, this could be largely a function of larger standard errors. In fact, the 95% confidence intervals from this approach contain the point estimates from my primary identification strategy.

5.3 The Effect of Private Medicaid on Efficiency of Inpatient Care

I then examine whether lower hospital utilization under private Medicaid comes from efficiency improvements, or instead comes from reduced patient welfare. To do so, I divide hospital visits into prevention-sensitive and non-prevention sensitive categories. Prevention sensitive visits, such as readmissions or ER visits, could be reduced through improved inpatient & outpatient care, and without adverse impact to patients. Meanwhile, a reduction in non-prevention sensitive visits, such as joint surgeries, may not reflect increased efficiency and would be more likely to adversely impact patients. The sample selection and empirical approach here is consistent with the previous section's.

In Figure 3, I document a sharp jump in readmissions among the treatment group, at 65, without any corresponding change among the control group. In addition, the figure provides no evidence of differential pre-trends across the treatment and control groups, and no evidence of attenuation of the effect over the post-period.

In statistical analyses, I find that switching from private to public Medicaid is accompanied by a substantial increase in prevention-sensitive visits, which include readmissions, admissions from the ER, and 'avoidable' admissions (per AHRQ's classification). Simultaneously, I find a much less pronounced increase in non-prevention sensitive care, such as general surgeries and joint/hip replacements. These results are presented Table 4, with the estimates being consistent across the three panels. For example, I find that switching from private to public Medicaid is accompanied by an increase in person-year readmissions of .027 (or 25%), given the point estimate of .018 on the Initially Private*Post 65 term in the top panel (which needs to be scaled by 1.5 to reflect the actual treatment effect). Hospital readmission reductions could result from improved inpatient as well as outpatient care. Meanwhile, I find that under switching, non ER and non-readmission visits, per person-year, increase only by about .010 (or 7%), which represents a much more modest increase.

I also statistically examine the effect of switching from private to public Medicaid on additional measures of patient welfare, such as distance traveled to hospital and the quality of hospitals visited. Given that these measures are conditional on hospitalization, I structure the analytic data to be at a hospitalization (rather than at a person-month) level. I also include DRG (diagnosis group) fixed effects as part of these analyses, so that my estimates are robust to potential compositional changes to hospitalizations. In the results, which are shown in Table 5, I find that such switching has no significant impact on patients' travel times to hospitals, and can rule out a change of greater than 2%, with 95% confidence. More generally, in results not shown here, I find no effect from private Medicaid on hospital network breadth, as private plans' networks appear comparable in completeness to the FFS network. I also find no evidence of a meaningful effect on the quality or type of hospital visited, based on hospital-level measures such as risk-adjusted mortality and readmissions rates. For these measures, I find the estimated effects to be insignificant, and can generally rule out effects of greater than 1% of the baseline, with 95% confidence.

Altogether, these results suggest that reduced inpatient utilization under private Medicaid may reflect efficiency improvement, rather than reduced patient welfare.

In Appendix Table 3, I focus on these same outcome variables, while using an alternate instrument for private Medicaid enrollment status (enrollment mandates). There, I find no significant effect of private Medicaid enrollment on these measures; however, the standard errors are again quite large, with the 95% confidence intervals again encompassing my main estimates.

5.4 Private Medicaid's Impact on Quantity and Efficiency of Outpatient Hospital Care

I also examine the causal effect of private vs. public Medicaid enrollment on outpatient hospital utilization; given that some outpatient hospital care is relatively expensive and also potentially preventable, a reduction in such visits could also reflect efficiency improvements. As previously, my measures reflect annualized utilization at a person-month level; I also implement the baseline controls and sample restrictions from before.

I statistically examine the effect of switching from private to public Medicaid on prevention sensitive types of outpatient visits (such as ER ones), along with visits that are less sensitive to prevention (such as outpatient surgeries). I find a substantial and highly significant effect on overall ER Visits (including those that do not result in inpatient admissions), along with an effect on a subclass of ER visits that is designated as particularly preventable. Meanwhile, I find no significant effect on outpatient surgeries. Altogether, this suggests that private Medicaid affects outpatient hospital utilization primarily through reductions to prevention-sensitive visits. The results are reported in Table 6.

For general ER visits, the point estimate of .055 on the key term of interest, Init Private*Post 65, and when properly scaled implies that there about .082 (or about 11%) more ER visits per year under public than under private Medicaid. In addition, for outpatient hospital surgeries, the key point estimate is insignificant, and suggests that a reduction of more than 10% can be ruled out with 95% confidence.

5.5 Mechanisms Behind Private Medicaid's Efficiency Impact

In the previous tables, I provided some evidence that private Medicaid enhances efficiency. I proceed by focusing on a specific type of prevention sensitive visit-inpatient readmissions.

First, I show that most of the reduction in these readmissions-under private Medicaid-cannot be explained by compositional changes to initial visits. Rather, the effect on readmissions remains even when holding the composition of initial admissions fixed; this suggests that reduced readmission rates could reflect improved efficiency, rather than something unrelated. In addition, I show that the reduction in readmissions cannot be explained by shifts to more efficient hospitals.

In 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 60-days of the initial admission, and as such is binary. I present estimates for various analyses in Table 7, for the key variable, Initially Private*Post 65; these need to be further scaled by 1.5 to reflect the actual effect of switching from private to public Medicaid.

When not conditioning on either the site of the initial hospitalization, nor on the type of initial hospitalization, the point estimate (in column one) implies that the conditional probability of a readmission is 3.6% higher under public than under private Medicaid (the baseline likelihood is 16%). 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). Since the aforementioned mechanisms may explain only about a third of private Medicaid's effect, the mechanisms for the remaining two-thirds of the effect remain unaccounted for. Based on conversations with various stakeholders, these mechanisms may include supply shifts, in terms of changing within-hospital behavior. These mechanisms could also include changes to outpatient care, such as increased home care immediately following a hospitalization.

I perform additional statistical analyses on whether this readmissions effect could be explained by shifts to more efficient hospitals. As part of my implementation, I first construct a hospital-level measure of readmissions rates. This measure is derived by looking to the public Medicaid population, regressing on likelihood of readmission (conditional on initial admission), and subsequently backing out hospital-level fixed effects. In identifying these hospital-level fixed effects, I also control for individual level demographics (age and race) and for the nature of the initial hospitalization (using ICD-9 fixed effects). The results, which are presented in Table 8, suggest that only 5% of the estimated effect on readmissions (or .001 readmissions out of .024, per person-year), are attributable to shifts to other hospitals.

Unfortunately, I am unable to further delve into these mechanisms, given my current data.

5.6 Pass-Through to Governments

In the previous section, I provided evidence of efficiency gains, under Medicaid privatization, and also showed that there's no evidence of pass-through to patients. I proceed to further explore the incidence of efficiency gains under Medicaid privatization, by examining possible pass-through to governments, in the form of cost savings.

Unfortunately, I cannot identify privatization's effect on spending, using my primary identification strategy, as I am unable to track spending among those concurrently in Medicare (hence, I am largely unable to track spending in the post-period). Instead, I instrument for private Medicaid status using my secondary strategy, of enrollment mandates. In constructing a measure of total government spending, at a person-quarter level, I include government spending on all those in public Medicaid (in the form of fee-for-service payments) and those in private Medicaid (in the form of premium payments to insurers). I also include government spending on medical services that remain directly administered and paid for by the government, even for those in private Medicaid plans (such services are referred to as 'carve-outs'). Given that managed care implementation may alter the overall composition of enrollees in Medicaid (Currie and Fahr 2005), I restrict my sample to those in Medicaid in the year preceding the mandates (2004). The results of these analyses are presented in Table 9.

First, I find no evidence of a reduction in overall government spending, under privatization. Rather, given the point estimate and standard error shown in column 1, I can rule out a spending reduction from privatization in excess of 5%, with 95% confidence. This result is robust to different time trends.

In addition, I examine the effect of Medicaid privatization on government spending on 'carved-in' services; such services are financially and logistically covered by private plans (for anyone enrolled in them). By focusing on these services, I am able to get at privatization's fiscal impact, independent of certain manifestations of incomplete contracting (such as cost-shifting to services that are universally covered by the government, even for those in private Medicaid). I find that when looking to such 'carved-in' spending, the point estimate on my instrument is not statistically significant, even though it is negative. Part of the limited cost-savings observed here-even in the absence of 'cost shifting'-could be attributable to differences in provider reimbursement levels, which are about 15% higher under private than public Medicaid. As such, limited pass-through to governments could partly be explained by provider capture of some portion of efficiency gains.

I also find evidence that pass-through could be further limited by incomplete contracting. To this end, under privatization, I identify a 15% increase in expenditures on 'carved-out' services, or services that were covered by the government (even for those in private Medicaid). This increase could be an attempt by plans to cost-shift towards services for which they don't bear costs, while substituting away from those services for which they are financially responsible. In the following section, I further explore this phenomenon, and examine whether it is a causal result of incomplete contracting, or an artifact of something unrelated.

5.7 Identifying the Extent to Which Incomplete Contracting Reduces Pass-Through

While spending on carved-out services, such as prescription drugs, is higher under private than under public Medicaid, this isn't necessarily indicative of cost shifting by plans. For instance, such carved-out services may coincide with relatively efficient forms of care; as such, private Medicaid plans (or managed care plans generally) may use more of these services, even if incurring the complete cost. To decompose the effects of incomplete contracting from those of private/managed care, I focus on prescription drug services, which accounted for the plurality of all carved-out services spending. I first examine the effect of private Medicaid (relative to public) on the use of these services, while these were excluded from private Medicaid contracts; the result of this analysis captures the combined effects of managed care and incomplete contracting. In addition, I consider how utilization of these services changes in

October 2011, when they were bundled into existing contracts (and plans were made financially responsible for them); here, only the effect of incomplete contracting would be captured, separate from that of managed care.

Effect of Incomplete Contracting: Under Switch Between Public & Private Medicaid

When examining the impact of private Medicaid enrollment on prescription drug utilization, I instrument for private Medicaid status using enrollment mandates (my secondary identification strategy, involving involuntary shifts from public to private Medicaid). The results, which can be found in the top panel of Table 10, indicate that individual-level switching from public to private Medicaid is associated with a highly significant 15-20% increase in prescription drug spending (by the government). This increase is not accompanied by a corresponding change in the number of prescriptions, and I can rule out an increase in excess of 9%, with 95% confidence. As such, increases in drug spending could instead come through changing prescription composition, such as the use of more branded or of fewer generic drugs. These more expensive drugs could more effectively substitute for other types of care, such as hospital or outpatient services; as such, plans would have every incentive to use these expensive drugs (given they don't bear their cost), since these could substitute for services that plans would otherwise have to pay for.

I also instrument for private Medicaid status using my primary strategy, involving age 65. However, the results from this strategy might not reflect the true magnitude of cost changes; individuals here would be switching from private to public Medicaid, and inertia could bias against downgrading to less expensive drugs, but might not bias against upgrading to more expensive ones. This said, while the estimates here are more modest, they still imply 10% higher drug expenditures under public than under private Medicaid, with the results presented in the bottom panel. Note that to ensure consistency with the top panel, I invert the coefficient of interest to be Initially Private*Pre 65 (rather than Initially Private*Post 65). While the top panel focuses on the mandate period of 2004-2010, the bottom panel extends from 1999 to 2005. The bottom panel excludes the subsequent period, given the 2006 shift in drug coverage from Medicaid to Medicare (from the intro of Part D), for those with supplementary Medicare coverage.

Effect of Incomplete Contracting: Under Bundling of Prev. Excluded Services

I proceed to examine the effect of bundling prescription drug services into existing private Medicaid contracts, which was implemented in October 2011. I try to capture the difference between an incomplete contract and a (more) complete counterfactual (at least with respect to prescription drug services), while holding other factors constant. For example, the set of active contracts does not change throughout the period on which I focus (three months pre and post), making my results robust to compositional changes in contracts. Further, I hold fixed the set of enrollees, focusing on disabled individuals who were enrolled in private Medicaid three months prior to the carve-in (July 2011), and still enrolled as of December 2011 (note that this amounts to about 95% of the original July cohort). Unfortunately, since my data only extends through December 2011, I will mostly be capturing the short-run and not the long-run effect.

Altogether, I find that bundling reduces prescription drug expenditures, and that this reduction is driven primarily by shifts to less expensive drugs (rather than by a decrease in the overall number of prescriptions). I also demonstrate that these results are not driven by pre-trends. This said, I do find evidence of a post-trend, which suggests the effect of bundling isn't instantaneous. The estimates using this approach-which isolates the effect of incomplete contracting-are consistent with those from previous approach (which captures the combined effect of incomplete contracting and managed care). Throughout these analyses, which are shown in Table 11, all observations and outcome variables are set at a person-month level. In addition, all point estimates are denoted relative to the baseline month-September 2011-which immediately precedes the carve-in.

Looking at changes in person-month prescriptions from September to December 2011, I find an increase of .366 in overall prescriptions (amounting to 15%), which breaks down to a decrease in branded prescriptions of .212 (or about 20%), and an increase in generics of .441 (also about 20%).[10] To measure utilization, I also construct a standardized measure of drug costs[11], and find a decrease of about $57 per person-month (or 17%) from September to December 2011.

Finally, I find that actual government spending, per individual in private Medicaid, decreases by a highly significant $35/month (or 4%) from September to December 2011. This result suggests that just one form of incomplete contracting-the exclusion of prescription drugs from private Medicaid contracts-increased contracting costs by 4%. This estimate provides empirical support for existing theory, showing that incomplete contracting can indeed substantially increase contracting costs. In addition, since I look at only one type of incomplete contracting, this finding should be viewed as a floor estimate for incomplete contracting's overall impact.

In results not shown here, I examine whether the reduction in drug spending, through the carve-in, is accompanied by an inpatient utilization offset. I find no significant change to inpatient utilization following the change, and can rule out an increase in excess of 5% of the baseline, although this only reflects short-run impact.

[10] Note that these classifications were not available for all drugs, and hence that the sum of the following estimates won't correspond to the main result.

[11] This reflect average drug prices in public Medicaid. As such, this measure is not affected by potential price differences (pre & post-carve in) for the same drug.

In other results not shown here, I consider the mechanisms by which plans affect drug use, following bundling. Anecdotally, these effects could come from restrictive formularies implemented by plans, placing more stringent limits on use of branded and expensive drugs than exist under the public option.

I also consider the extent to which an additional mechanism drives these results-a shift to different providers. To do so, I use prescription-level data, and look at whether the effect of the carve-in on proportion of branded drugs is at all sensitive to the inclusion of provider FE's (corresponding to the prescribing provider); I find that the results are not sensitive to the inclusion of these FE's, suggesting that the effect is mediated through within-provider behavioral changes, rather than shifts to different providers. The imposition of stringent formularies, which was mentioned previously, could explain some of this changing physician behavior.

5.8 Is Efficiency Dependent on For-Profit Status?

On the whole, private Medicaid appears to produce efficiency improvements, relative to the public option; however, there could be heterogeneity across contractors-within Medicaid as well as elsewhere-in their capacity for efficiency improvements. Differences in contractor performance could be driven by heterogeneity in a variety of underlying contractor characteristics, and here I focus in on a specific characteristic: the contractor's for-profit status. 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 more limited in terms of geography, and also in the patients they serve (some only offer Medicaid products). As such, these results on the effects of for-profit status should be viewed as suggestive, rather than causal.

I first examine whether for-profit plans produced lower hospital utilization than not-for-profit ones; such analyses are complicated by potential patient composition differences across plan types. To identify this effect, independent of patient composition, 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). Throughout these analyses, I make use of my baseline, person-month level sample, and denote initial for-profit enrollment status as of age 63.

Figure 4 provides evidence that for-profit plans are much more effective at reducing utilization than not-for-profits, as it documents a sharp jump in days stayed among those initially in for-profit plans, at 65, alongside a substantially more modest jump among the not-for-profit cohort. Simultaneously, the figure does not provide any evidence of differential pre-trends.

In statistical analyses, I confirm this result and find that for-profit insurers appear to achieve greater utilization reductions across various utilization measures; when properly scaled, the relevant point estimates imply 20-30% lower relative inpatient utilization under for-profit, than under not-for-profit plans (based on the relevant interaction term). The results for these analyses are presented in Table 12.

In these analyses, I am also able to identify selection differences between for-profit and not-for-profit plans, independent of treatment. In doing so, I find that not-for-profit plans attract healthier enrollees (relative to not-for-profits). This information can be gleaned from the For Profit Status term, which identifies post-65 differences in utilization, among those originally enrolled in for-profit and not-for-profit plans, respectively. After all, 80% of the original for-profit and not-for-profit populations experience the same coverage type (public Medicaid), post-65.

Finally, I examine whether the reduction in inpatient utilization, under for-profit plans, reflects possible efficiency improvements or instead might come at the expense of patient welfare. Following the same approach as in my primary analyses, I find that the effect comes disproportionately through reductions to prevention sensitive visits (which could reflect efficiency improvement), with the results presented in Table 13.

6 Robustness

My primary identifying assumption is the absence of differential trends across my treatment and control groups. An additional identifying assumption is that the effect of supplemental Medicare coverage (at 65) on the treatment group is no greater than it is on the control group. In the following section, I demonstrate that the effect of supplemental Medicare coverage is consistent across the treatment and control groups. In addition, I document the absence of differential pre-trends across my treatment and control groups; I also find only modest evidence of differential post-trends, suggesting that my results aren't being driven by pent-up demand. Finally, I also show that my results are robust to additional controls and to narrower bandwidth restrictions.

6.1 Validity of Instrument

One concern about my primary identification strategy is that private Medicaid disenrollment, at 65, is accompanied by the addition of supplementary Medicare coverage; to address this concern, I have constructed a control group that also gains supplementary Medicare coverage, at 65, but that remains in public Medicaid (pre as well as post 65). This strategy presumes that the impact of supplementary Medicare coverage is the same across the treatment and control group (or those initially in private and public Medicaid, respectively); given that the treatment group is on average healthier, this presumes that the effect of supplementary Medicare is not heterogeneous across health status.

Critically, I find no significant impact on the control group, from supplemental Medicare coverage at 65; this suggests that these could be second-order concerns, even if supplementary Medicare does have a differential impact on the treatment group.

As an additional robustness check, I implement my two identification strategies in tandem. I construct an instrument for an individual's private Medicaid status, pre-65, using my secondary identification strategy. I then examine the age-65 effect on utilization, and how its magnitude varies with instrumented pre-65 private Medicaid status (note that this instrumented term is not significantly correlated with health status). As noted previously and as shown in Tables 2, 3, and 4, this approach and my primary one yield comparable estimates.

6.2 Differential Trends

One of my identifying assumptions is that there are no differential pre-trends in the outcome measures, immediately preceding 65, which affect the treatment but not the control group.

However, I find no statistical evidence of differential pre-trends across my treatment and control groups, when looking to my baseline sample. Rather, there appears to be sharp jump in utilization among my treatment group at 65, relative to the control group, and the spread between the two appears to increase further over the post-period. This suggests that my main result is not being driven by differential pre-trends, and also that it isn't being driven by pent-up demand. Rather, these findings suggest that the long-run effects of switching from private to public Medicaid may actually exceed those from the short-run; as such, my main estimates, which reflect the short-run effect, could understate the long-run impact.

The findings discussed above are presented in Appendix Table 5, with the coefficients reflecting the utilization spread between the initially private and public Medicaid cohorts for different half year time periods; this is relative to the spread in the half-year preceding 65.

6.3 Bandwidth Tests and Control Sensitivities

Consistent with Altonji et al (2005), I also examine whether my primary results-on the utilization and efficiency effects of private Medicaid-are sensitive to the bandwidth and controls chosen. In Appendix Table 7 and Appendix Table 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 Appendix Table 9 and Appendix Table 10, I show that the results hold up under different types of age controls, including linear, quadratic, cubic controls. Finally, these results hold up when including treatment and control group specific age controls, and also when incorporating separate age controls on either side of the discontinuity.

7 Conclusion

While government contracting is pervasive, there is limited understanding of whether it improves efficiency, whether these efficiency improvements are passed-through, and whether certain contract designs are optimal. Looking to the Medicaid setting, I show that private contractors are able to reduce inpatient utilization, relative to government provision. I find that this utilization reduction is driven by reductions to preventable/unnecessary visits, and not by reductions to surgeries or other valuable care. As such, my findings suggest that private contractors (at least in this setting) reduce costs through efficiency improvement, rather than at the expense of enrollee welfare. Additional research is needed on privatization's impact across other health care settings (such as non-inpatient), and on the attenuating or magnifying effects of market structure. Further research is also needed to identify the mechanisms driving efficiency improvement, and to decompose the effect of incentives (in this setting, capitation) from that of proprietary technology (in this setting, care management).

In this draft, I also show that incomplete contracting substantially limits pass-through of efficiency gains. As such, this research points to the value of broader contracts, at least in terms of the scope of services covered. As I focus on only a single form of incomplete contracting, future work could examine other types of incomplete contracting.

Finally, I find that the magnitude of efficiency improvement is greater under for-profit contractors, relative to not-for-profits, although factors other than for-profit status could be at play here. Additional work is needed on the relationship between a contractor's characteristics, including for-profit status, and that contractor's performance.

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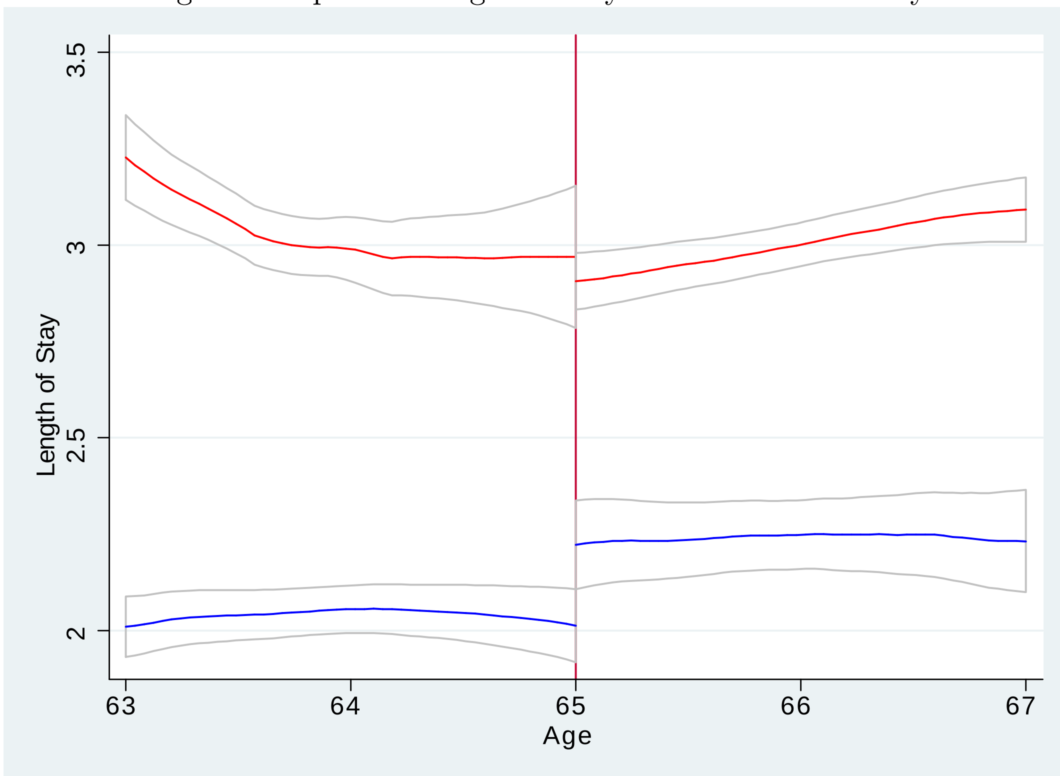
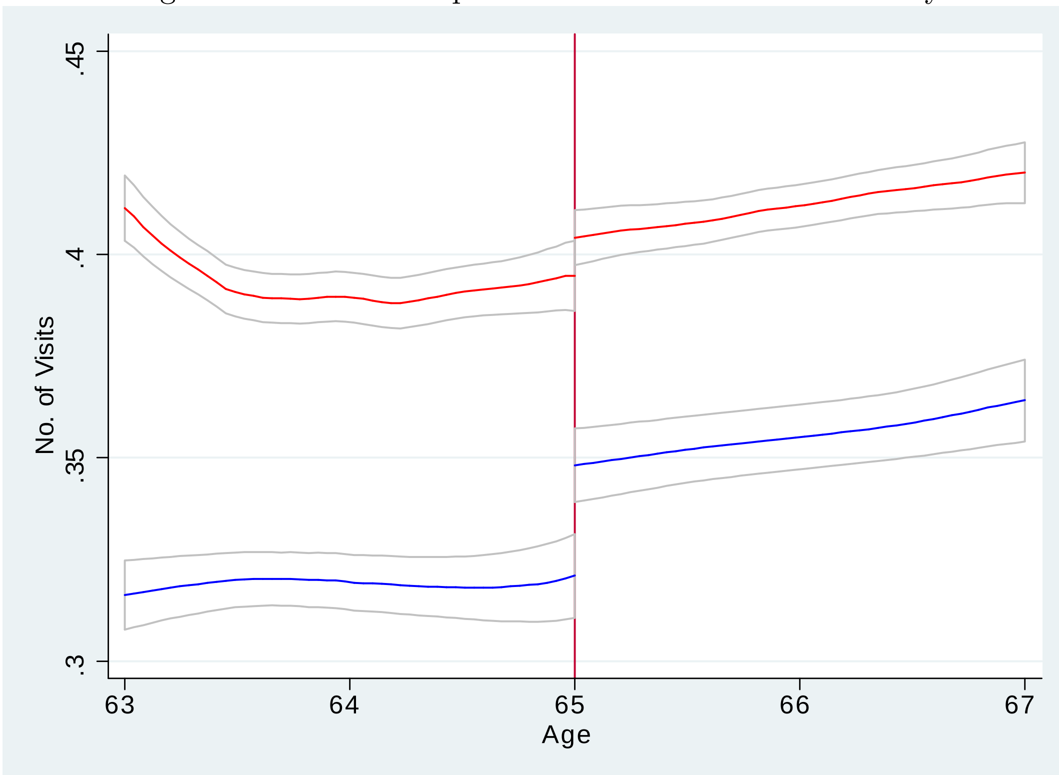
Figure 1: Inpatient Length of Stay Around Discontinuity



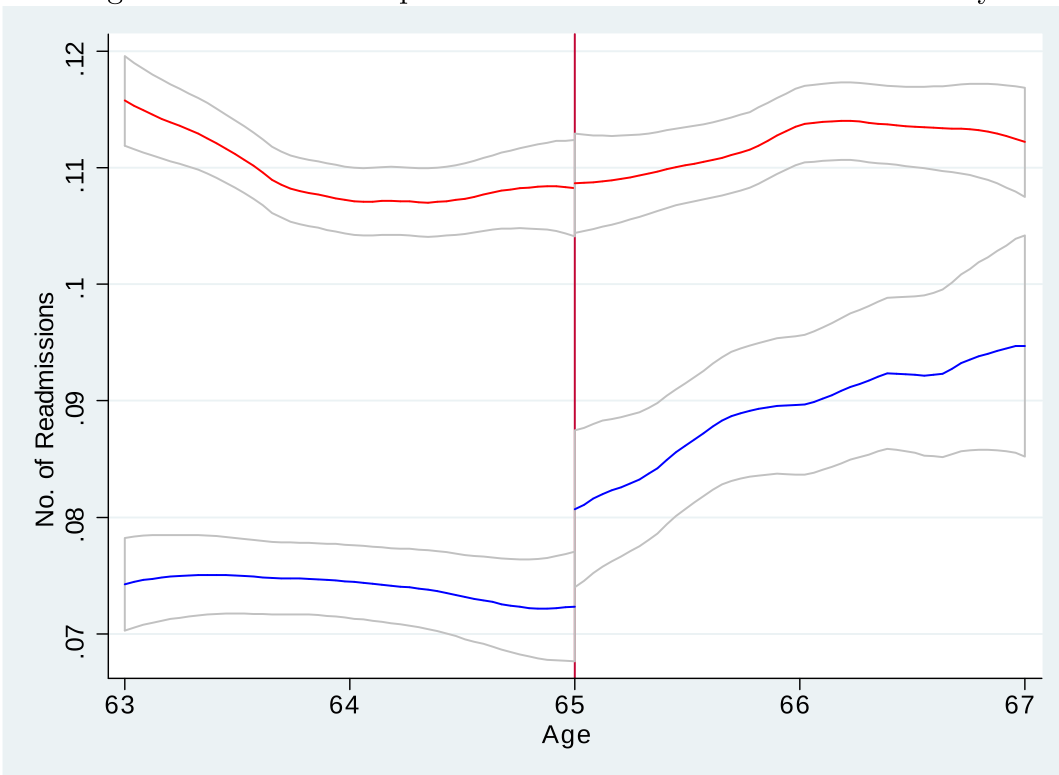
Blue: Initially in Private Medicaid (Treatment)
Red: Initially in Public Medicaid (Control)

Figure 2: Number of Inpatient Visits Around Discontinuity



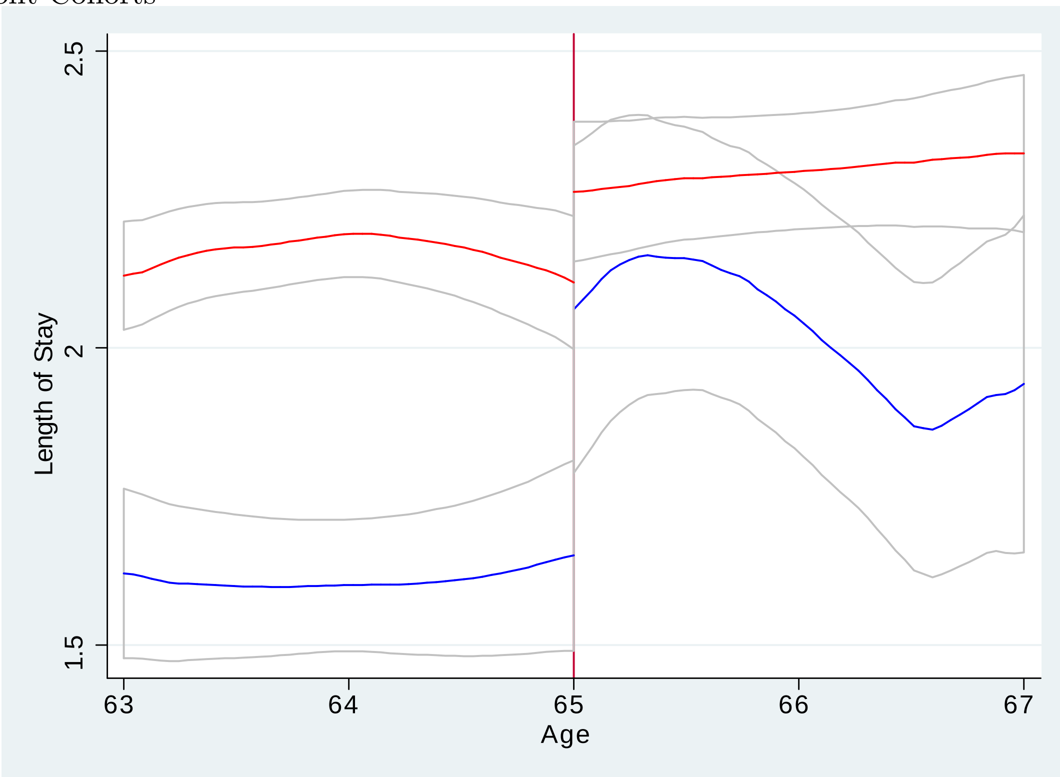
Blue: Initially in Private Medicaid (Treatment)
Red: Initially in Public Medicaid (Control)

Figure 3: Number of Inpatient Readmissions Around Discontinuity



Blue: Initially in Private Medicaid (Treatment)
Red: Initially in Public Medicaid (Control)

Figure 4: Inpatient Length of Stay Around Discontinuity, for For-Profit and Not-For-Profit Cohorts



Blue: Initially in For-Profit Medicaid Plans (Treatment)
Red: Initially in Not-For-Profit Medicaid Plans (Control)

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)
ER Admits	0.191	0.194
	(1.613)	(1.648)
Charges	9,896	11,371
	(117,421)	(129,294)
Inp Composition		
Readmissions	0.079	0.111
	(1.141)	(1.391)
Prevent Hosp	0.071	0.093
	(0.966)	(1.128)
Surgeries	0.024	0.044
	(0.539)	(0.736)
ER Utilization		
ER Visits	0.666	0.770
	(3.496)	(3.863)
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 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 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	
$\widehat{Init.Private}$*Post 65	-0.646***	0.015***
	(0.006)	(0.005)
Post 65	0.010***	0.764***
	(0.002)	(0.002)
$\widehat{Init, Private}$	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 Len Sty	(2) Tot Procs	(3) Tot Chg	(4) Log Tot Chg	(5) Tot Visits
Mean	2.801	0.818	11,015	0.345	0.385
	(35.714)	(7.491)	(126,530)	(2.040)	(2.413)
Init Private*Post 65	0.335***	0.065***	1,303***	0.025***	0.034***
	(0.088)	(0.024)	(402)	(0.007)	(0.008)
Post 65	-0.064	0.037**	335	0.011**	0.010
	(0.101)	(0.019)	(316)	(0.005)	(0.006)
Init. Private	-1.201***	-0.204***	-4,408***	-0.092***	-0.107***
	(0.067)	(0.018)	(284)	(0.006)	(0.007)
Sample Restriction			Ages 63 to 67		
N			3,909,770		
Init Private*Post 65	0.479***	0.106***	1,859***	0.028***	0.039***
	(0.103)	(0.026)	(441)	(0.007)	(0.009)
Post 65	-0.003	0.036	550	0.014**	0.010
	(0.139)	(0.023)	(393)	(0.006)	(0.008)
Init. Private	-1.398***	-0.270***	-5,180***	-0.113***	-0.133***
	(0.085)	(0.021)	(332)	(0.007)	(0.008)
Sample Restriction			Ages 64 to 66		
N			2,329,769		
$\widehat{Init.Private}$*Post 65	0.370*	0.098*	1,732*	0.041***	0.058***
	(0.216)	(0.054)	(949)	(0.015)	(0.018)
Post 65	0.026	0.038	581	0.011	0.005
	(0.154)	(0.026)	(439)	(0.007)	(0.009)
$\widehat{Init.Private}$	0.643	0.170	349	0.077**	0.092**
	(0.429)	(0.107)	(1568)	(0.035)	(0.042)
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: Effect of Private Medicaid on Preventable and Non-Preventable Inpatient Visits

	(1)	(2)	(3)	(4)	(5)	(6)
	Prevention Sensitive			Prevention Non-Sensitive		
	Readmits	*Avoidable Admits*	*ER Admits*	*Non ER/Readmits*	*All Surg*	*Elective Surg*
Mean	0.103	0.088	0.193	0.136	0.039	0.043
Init. Private*Post 65	0.018***	0.003	0.017***	0.007**	-0.002	-0.004
	(0.005)	(0.003)	(0.006)	(0.003)	(0.002)	(0.007)
Post 65	0.002	-0.003	-0.009**	0.015***	0.006***	0.017***
	(0.004)	(0.003)	(0.004)	(0.003)	(0.002)	(0.005)
Init. Private	-0.048***	-0.021***	-0.070***	-0.017***	-0.001	0.014***
	(0.003)	(0.003)	(0.005)	(0.003)	(0.001)	(0.004)
Sample Restriction	Ages 63 to 67					
N	3,909,770					
Init. Private*Post 65	0.025***	0.004	0.011*	0.012***	0.002	0.000
	(0.005)	(0.003)	(0.006)	(0.004)	(0.002)	(0.008)
Post 65	-0.007	0.001	-0.006	0.017***	0.003	0.009
	(0.004)	(0.003)	(0.005)	(0.004)	(0.002)	(0.007)
Initially Private	-0.058***	-0.024***	-0.079***	-0.027***	-0.005***	0.006
	(0.004)	(0.003)	(0.005)	(0.003)	(0.002)	(0.005)
Sample Restriction	Ages 64 to 66					
N	2,329,769					
$\widehat{Init.Private}$*Post 65	0.032***	0.006	0.013	0.017**	-0.003	-0.001
	(0.011)	(0.008)	(0.013)	(0.008)	(0.003)	(0.002)
Post 65	-0.009*	0.001	-0.007	0.015***	0.005**	0.001
	(0.005)	(0.004)	(0.005)	(0.004)	(0.002)	(0.001)
$\widehat{Init.Private}$	0.016	0.040**	-0.010	0.071***	0.009	0.000
	(0.022)	(0.018)	(0.030)	(0.017)	(0.007)	(0.003)
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. Note, however, that the elective surgery measure is 10x magnified. 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 5: Effect of Private Medicaid on Quality of Inpatient Care

	(1) Dist to Hospital		(2) Hospital Quality Measures					(3) Hospital Char.
	Miles	*Time*	*MI Mort*	*HF Mort*	*PN Mort*	*MI Readm*	*HF Readm*	*Major Teach. Hospital*
Init. Private*Post 65	0.138	0.283	0.065*	0.021	0.045	0.011	-0.012	0.012
	(0.109)	(0.242)	(0.036)	(0.023)	(0.034)	(0.023)	(0.033)	(0.008)
Post 65	-0.036	-0.052	-0.041**	-0.009	-0.037**	-0.018	-0.022	-0.013***
	(0.074)	(0.148)	(0.018)	(0.012)	(0.017)	(0.011)	(0.017)	(0.004)
Initially Private	-0.499***	-1.098***	0.030	-0.004	0.029	0.003	0.039	-0.022***
	(0.073)	(0.165)	(0.026)	(0.017)	(0.024)	(0.017)	(0.024)	(0.006)
Male	-0.007	-0.049	0.056***	0.040***	0.051***	-0.028***	-0.003	0.002
	(0.051)	(0.107)	(0.014)	(0.009)	(0.014)	(0.009)	(0.013)	(0.003)
Age	2.034	6.071	0.618	-0.265	0.468	0.283	-0.033	0.165
	(2.014)	(4.063)	(0.503)	(0.341)	(0.478)	(0.318)	(0.460)	(0.118)
AgeSq	-0.016	-0.047	-0.005	0.002	-0.004	-0.002	0.000	-0.001
	(0.016)	(0.031)	(0.004)	(0.003)	(0.004)	(0.002)	(0.004)	(0.001)
Cohort Restriction	At age 63, enrolled in Medicaid AND NOT simultaneously enrolled in Medicare.							
Age Restriction	63 to 67							
Mean	5.499	15.706	14.130	10.450	11.065	19.333	25.277	0.560
	9.228	17.593	1.832	1.575	1.781	1.146	1.864	0.496
N	264,166	264,166	208,490	214,669	214,789	192,103	214,700	228,642

Notes: Table presents linear regression models, where outcome variables are the characteristics of an admitting hospital, conditional on hospitalization. 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. DRG, year-quarter, county, and gender fixed effects are included as part of the analysis. The unit of observation is at the hospitalization 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.

Table 6: Effect of Private Medicaid on Outpatient Hospital Utilization

	(1)	(2)	(3)
	ER Visits		**Outp Surg Visits**
	All	*Avoidable*	*All*
Init. Private*Post 65	**0.055*** **	**0.021***	**-0.001**
	(0.020)	**(0.011)**	**(0.007)**
Post 65	0.006	0.007	0.015***
	(0.013)	(0.008)	(0.005)
Initially Private	-0.173***	-0.061***	0.009*
	(0.016)	(0.009)	(0.005)
Male	0.052***	-0.025***	-0.026***
	(0.015)	(0.008)	(0.004)
Age	-0.619*	-0.328	0.049
	(0.354)	(0.210)	(0.139)
AgeSq	0.005*	0.002	-0.000
	(0.003)	(0.002)	(0.001)
Cohort Restriction	At age 63, enrolled in Medicaid AND NOT simultaneously enrolled in Medicare.		
Age Restriction	63 to 67		
Mean	0.736	0.370	0.206
	(3.746)	(2.372)	(1.686)
N	2,294,206		

Notes: Table presents linear regression models, where outcome variables are annualized measures of individual outpatient hospital 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 visit-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 7: 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 8: 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.

Table 9: Pass-Through of Efficiency Gains to Government

	(1)	(2)	(3)
		Spending	
	Overall	*Carved-In*	*Carved-Out*
$Priv.\widehat{Medicaid}Enrolled$	730	-282	1,008***
	(1,049)	(981)	(276)
Year Range		2004-2010	
Mean	24,274	17,644	6,631
N		1,607,790	

Notes: Table presents linear regression models, where outcome variable is annualized government Medicaid spending, per-enrollee. The private Medicaid instrument is constructed based off enrollment mandates. Year, county, gender, and age fixed effects are also included, along with various time-trend controls. The unit of observation is at the person-year level, for the 2004-2010 period. The sample is restricted to those enrolled in Medicaid-only as of 2004, by virtue of disability; the sample is also restricted to New York State only. This data was constructed using person-year Medicaid spending records and person-month Medicaid enrollment records, both from CMS.

Table 10: Effect on Utilization of Excluded Prescription Services

	(1) No. of Presc	(2) Presc Spend	(3) Log Pharma Spend
$Priv.\widehat{Medicaid}Enrolled$	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	
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	

Notes: Table presents linear regression models, where outcome variables are monthly measures of individual drug utilization. The top panel leverages an 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 11: Effect of Carve-In of Previously Excluded Prescription Services

	(1)			(2)	(3)
	Number of Prescriptions			Presc. Cost	Ov. Medicaid Spending
	Total	*Branded*	*Generic*		
Pre-3 Months	-0.120***	-0.037***	-0.073***	-18.240***	-15.60***
	(0.006)	(0.003)	(0.005)	(1.843)	(2.04)
Pre-2 Months	0.050***	0.015***	0.041***	4.203**	7.45***
	(0.006)	(0.003)	(0.005)	(1.929)	(1.85)
Pre-1 Months				Baseline	
Carve-in Month	0.244***	-0.068***	0.187***	-35.498***	-34.49***
	(0.007)	(0.003)	(0.005)	(3.596)	(3.99)
Post-1 Month	0.263***	-0.187***	0.320***	-59.112***	-35.02***
	(0.007)	(0.003)	(0.005)	(3.243)	(3.99)
Post-2 Months	0.366***	-0.212***	0.441***	-57.143***	-35.75***
	(0.007)	(0.003)	(0.006)	(3.159)	(4.01)
Mean	3.374	1.080	2.230	340.77	930.96
N				2,643,684	

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, denoting prescription drug utilization relative to the baseline month (September 2011). The sample is restricted to those enrolled in Medicaid, by virtue of disability, who are enrolled in the private option as of July 2011 and who remain in the private option as of December 2011. The sample is also restricted to New York State only, for the period from July to December 2011. 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 12: Overall Utilization Differences Based on For-Profit Status

	(1)	(2)	(3)	(4)	(5)
	Tot LOS	Tot Procs	Tot Chrg	Log Tot Chrg	Tot 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 13: Identifying Efficiency Differences, Based on For-Profit Status

	(1) Prevention Sensitive			(2) Prevention Non-Sensitive		
	Readmits	*Prevent Visits*	*ER Admits*	*Non-Readm/ER*	*All Surg*	*Elec. Surg*
For Profit Plan*Post 65	0.023**	-0.001	0.019	0.010	0.004	0.008
	(0.010)	(0.006)	(0.012)	(0.006)	(0.003)	(0.015)
Post 65	-0.023***	-0.023***	-0.037***	-0.028***	-0.004**	0.002
	(0.005)	(0.004)	(0.008)	(0.004)	(0.002)	(0.008)
For Profit Plan	0.005	-0.006	-0.017*	0.021***	0.004	0.028**
	(0.007)	(0.005)	(0.009)	(0.005)	(0.003)	(0.013)
Male	0.032***	0.013***	0.049***	0.020***	0.006***	-0.036***
	(0.005)	(0.004)	(0.007)	(0.004)	(0.001)	(0.005)
Age	-0.195	-0.344***	-0.541**	0.034	0.049	0.199
	(0.179)	(0.133)	(0.242)	(0.134)	(0.069)	(0.320)
Age Sq	0.002	0.003***	0.004**	-0.000	-0.000	-0.002
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)
Cohort Restriction	At age 63, enrolled in Medicaid AND NOT simultaneously enrolled in Medicare					
Age Restriction	63 to 67					
Mean	0.079	0.071	0.191	0.102	0.024	0.055
	(1.141)	(0.966)	(1.613)	(1.103)	(0.539)	(2.575)
N	936,200					

Notes: Table presents linear regression models, where outcome variables are annualized measures of individual inpatient utilization (with the exception of elective surgeries, which are represented 10x). 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

Table A.1: 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.2: Alt Identification Strategy: Effect on Inpatient Utilization

	(1) Tot LOS	(2) Tot Hosp Visits	(3) Tot Procs	(4) Tot Chrg	(5) Log Chrg
$Private\widehat{Medicaid}Enrolled$	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.3: Alt Identification Strategy: Effect on Inpatient Utilization

	(1)	(2)	(3)	(4)	(5)	(6)
	Prevention Sensitive			**Prevention Non-Sensitive**		
	Readmits	*Avoidable Admits*	*ER Admits*	*Non-Prevent*	*All Surg*	*Elective Surg*
$PrivateM\widehat{edicaid}Enrolled$	0.018	0.016	0.072	-0.035	0.012***	-0.000
	(0.017)	(0.010)	(0.063)	(0.030)	(0.003)	(0.000)
Medicaid Only Enrollee	-0.011***	-0.001	-0.030***	-0.004	-0.018***	-0.001
	(0.002)	(0.002)	(0.004)	(0.006)	(0.002)	(0.000)
Male	0.011***	-0.007***	-0.008**	-0.018***	-0.007***	-0.001
	(0.004)	(0.001)	(0.004)	(0.003)	(0.001)	(0.000)
Mean	0.085	0.042	0.216	0.087	0.029	0.003
	(0.933)	(0.504)	(1.187)	(0.609)	(0.364)	(0.110)
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.4: Robustness Test: Pre and Post Trends

	(1) Tot LOS	(2) Tot Hosp Visits	(3) Tot Readm	(4) Tot ER Admits	(5) Tot ER Visits	(1) Tot No Presc	(2) Tot Pharma Spend	(3) Log Pharma Spend
Mean	2.801	0.385	0.103	0.193	0.736	4.703	3,507	5.833
Initially Private*Pre-4	-0.126	-0.014	-0.007	-0.008	-0.030	0.099**	20	0.104***
	(0.160)	(0.012)	(0.007)	(0.008)	(0.024)	(0.045)	(58)	(0.038)
Initially Private*Pre-3	-0.050	-0.005	0.003	-0.004	0.012	0.102**	73	0.100***
	(0.160)	(0.012)	(0.007)	(0.008)	(0.022)	(0.040)	(57)	(0.035)
Initially Private*Pre-2	0.064	-0.011	-0.003	-0.003	0.013	0.072**	59	0.064**
	(0.156)	(0.011)	(0.006)	(0.008)	(0.021)	(0.032)	(46)	(0.028)
Initially Private*Pre-1			Baseline				Baseline	
Initially Private*Switch Per	0.335**	0.012	0.012*	-0.001	0.019	0.025	0	-0.027
	(0.164)	(0.012)	(0.007)	(0.008)	(0.024)	(0.036)	(46)	(0.032)
Initially Private*Post-1	0.387**	0.022	0.016**	0.011	0.040	0.064	94	0.024
	(0.184)	(0.014)	(0.008)	(0.010)	(0.026)	(0.049)	(67)	(0.042)
Initially Private*Post-2	0.089	0.020	0.009	0.013	0.060**	0.031	24	0.008
	(0.175)	(0.015)	(0.008)	(0.011)	(0.027)	(0.061)	(80)	(0.050)
Initially Private*Post-3	0.150	0.037**	0.024***	0.028**	0.096***	0.171**	121	0.066
	(0.184)	(0.016)	(0.009)	(0.012)	(0.031)	(0.074)	(89)	(0.058)
N	3,909,770	3,909,770	3,909,770	3,909,770	2,294,206		3,909,770	

Notes: Table presents linear regression models, where outcome variables are annualized measures of individual inpatient 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 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 A.5: Robustness Test: Bandwidth Sensitivities

	(1) Tot LOS	(2) Tot Hosp Visits	(3) Tot Readm.	(4) Tot ER Adm	(5) Tot ER Vis
Mean (Baseline)	2.801	0.385	0.103	0.193	0.736
Init. Priv*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. Priv*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. Priv*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. TYear-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 A.6: Robustness Test: Bandwidth Sensitivities

	(1) Tot No Presc	(2) Tot Pharma Spend	(3) Log Pharma Spend
Mean (Baseline)	4.703	3,507	5.833
Initially Private*Post 65	-0.015	11	-0.071*
	(0.047)	(55)	(0.036)
Post 65	-0.005	-30	0.013
	(0.017)	(23)	(0.014)
Initially Private	-0.349***	-521***	0.027
	(0.042)	(42)	(0.034)
N		2,009,680	
Baseline Bandwidth: 63 to 67			
Initially Private*Post 65	-0.044	-34	-0.094***
	(0.033)	(44)	(0.029)
Post 65	0.016	-17	0.015
	(0.014)	(19)	(0.013)
Initially Private	-0.374***	-537***	0.011
	(0.042)	(45)	(0.034)
N		1,305,288	
Narrower Bandwidth: 64 to 66			
Initially Private*Post 65	-0.037	-11	-0.053**
	(0.028)	(38)	(0.027)
Post 65	-0.022	-30	-0.0311**
	(0.015)	(19)	(0.013)
Initially Private	-0.446***	-620***	-0.039
	(0.042)	(45)	(0.035)
N		758,085	
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. TYear-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 A.7: Robustness Test: Control Sensitivities

	(1) Tot LOS	(2) Tot. Hosp Visits	(3) Tot Readm.	(4) Tot ER Admits	(5) Tot ER Visits	(1) Tot No Presc	(2) Tot Pharma Spend	(3) Log Pharma Spend
Mean (Baseline)	2.801	0.385	0.103	0.193	0.736	4.703	3,507	5.833
Init. Private*Post 65	0.335***	0.034***	0.018***	0.017***	0.055***	-0.015	11	-0.071*
	(0.088)	(0.008)	(0.005)	(0.006)	(0.020)	(0.047)	(55)	(0.036)
Post 65	-0.064	0.010	0.002	-0.009**	0.006	-0.005	-30	0.013
	(0.101)	(0.006)	(0.004)	(0.004)	(0.013)	(0.017)	(23)	(0.014)
Initially Private	-1.201***	-0.107***	-0.048***	-0.070***	-0.173***	-0.349***	-521***	0.027
	(0.067)	(0.007)	(0.003)	(0.005)	(0.016)	(0.042)	(42)	(0.034)
N	3,909,770	3,909,770	3,909,770	3,909,770	2,294,206		2,009,680	
Baseline Spec: Quadtratic								
Init. Private*Post 65	0.319***	0.033***	0.018***	0.016***	0.052***	-0.016	9	-0.071*
	(0.088)	(0.009)	(0.005)	(0.006)	(0.020)	(0.047)	(54)	(0.036)
Post 65	-0.033	0.0127**	0.003	-0.007	0.008	-0.002	-19	0.013
	(0.103)	(0.006)	(0.004)	(0.004)	(0.013)	(0.017)	(22)	(0.014)
Initially Private	-1.196***	-0.106***	-0.047***	-0.069***	-0.172***	-0.349***	-521***	0.027
	(0.067)	(0.007)	(0.003)	(0.005)	(0.017)	(0.042)	(42)	(0.034)
Alternate Control: Linear								
Init. Private*Post 65	0.334***	0.034***	0.018***	0.017***	0.055***	-0.016	11	-0.071*
	(0.088)	(0.009)	(0.005)	(0.006)	(0.020)	(0.047)	(54)	(0.036)
Post 65	-0.176	0.000	-0.005	-0.013**	-0.007	0.000	(30)	-0.003
	(0.140)	(0.008)	(0.005)	(0.005)	(0.016)	(0.017)	(23)	(0.015)
Initially Private	-1.201***	-0.107***	-0.047***	-0.070***	-0.173***	-0.349***	-521***	0.027
	(0.067)	(0.007)	(0.003)	(0.005)	(0.017)	(0.042)	(42)	(0.034)
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 A.8: Robustness Test: Control Sensitivities

	(1) Tot LOS	(2) Tot Hosp Visits	(3) Tot Readm	(4) Tot ER Admits	(5) Tot ER Visits	(1) Tot No Presc	(2) Tot Pharma Spend	(3) Log Pharma Spend
Mean (Baseline)	2.801	0.385	0.103	0.193	0.736	4.703	3,507	5.833
Initially Private*Post 65	0.332***	0.033***	0.018***	0.017***	0.054***	-0.015	11	-0.071*
	(0.088)	(0.009)	(0.005)	(0.006)	(0.020)	(0.047)	(54)	(0.036)
Post 65	-0.274***	-0.009	-0.009**	-0.014***	-0.067***	0.204***	(4)	0.132***
	(0.081)	(0.007)	(0.004)	(0.005)	(0.016)	(0.026)	(32)	(0.021)
Initially Private	-1.199***	-0.107***	-0.048***	-0.070***	-0.173***	-0.349***	-522***	0.026
	(0.067)	(0.007)	(0.003)	(0.005)	(0.017)	(0.042)	(42)	(0.034)
Alternate Control: Linear on Either Side of Discontinuity								
Init. Private*Post 65	0.332***	0.034***	0.018***	0.017***	0.054***	-0.015	11	-0.071*
	(0.088)	(0.009)	(0.005)	(0.006)	(0.020)	(0.047)	(54)	(0.036)
Post 65	-0.378***	-0.018**	-0.017***	-0.017***	-0.081***	0.223***	0.736	0.124***
	(0.106)	(0.009)	(0.005)	(0.006)	(0.020)	(0.027)	(33)	(0.022)
Initially Private	-1.198***	-0.107***	-0.048***	-0.070***	-0.173***	-0.349***	-521***	0.0264
	(0.067)	(0.007)	(0.003)	(0.005)	(0.017)	(0.042)	(42)	(0.034)
Alternate Control: Quadratic on Either Side of Discontinuity								
Init. Private*Post 65	0.661***	0.036**	0.019**	0.011	0.029	-0.135**	-49	-0.180***
	(0.153)	(0.014)	(0.008)	(0.010)	(0.031)	(0.063)	(78)	(0.052)
Post 65	-0.359***	-0.011	-0.010**	-0.014***	-0.066***	0.2195***	3	0.146***
	(0.094)	(0.008)	(0.005)	(0.005)	(0.019)	(0)	(33)	(0)
Initially Private	-1.380***	-0.120***	-0.054***	-0.079***	-0.201***	-0.298***	-522***	0.083**
	(0.102)	(0.009)	(0.005)	(0.006)	(0.022)	(0.044)	(48)	(0.036)
Alternate Control: Linear, Group-Specific on Either Side of Discontinuity								
Init. Private*Post 65	0.913***	0.059***	0.025**	0.024*	0.068*	-0.042	-12	-0.154***
	(0.207)	(0.018)	(0.010)	(0.013)	(0.039)	(0.066)	(82)	(0.056)
Post 65	-0.543***	-0.026**	-0.020***	-0.020***	-0.096***	0.227***	3	0.134***
	(0.124)	(0.010)	(0.006)	(0.007)	(0.025)	(0.028)	(35)	(0.023)
Initially Private	-1.600***	-0.138***	-0.062***	-0.088***	-0.241***	-0.328***	-558***	0.061*
	(0.12)	(0.01)	(0.01)	(0.01)	(0.03)	(0.04)	(49)	(0.04)
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.

Table A.9: Timeline of NY's Managed Care Mandates: Disabled/SSI Recipients

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

Table A.10: Population & Medicaid Enrollment Figures for NY State, by Year

Year	Population	Private Medicaid Enrollment		Overall Medicaid Enrollment	
		Disabled	*Non-Disabled*	*Disabled*	*Non-Disabled*
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

CHAPTER 2: Cost Sharing Amongst Those Who Can't Pay: Evidence from Medicaid Disenrollments

Boris V. Vabson

1 Introduction

Medicaid and Medicare together constitute the two largest government programs in America, accounting for almost a trillion dollars of annual spending (or over 5% of GDP). Both programs were introduced 50 years ago, as part of the Social Security Amendments Act. Medicaid was created as a health care program for the poor, while Medicare was primarily meant to serve the elderly, although both now serve a sizeable number of disabled individuals.

While Medicaid and Medicare were not originally designed to work together, there are presently over 9 million individuals enrolled in both. Those qualifying for both programs are typically either disabled individuals (who simultaneously receive SSI and SSDI), or the poor elderly (who receive SSI only). These individuals, who are referred to as 'dual-eligibles', account for a disproportionate 34% of Medicaid and Medicare spending, while amounting to only 13% of enrollees (CBO, 2013).

Given the substantial amount of federal as well as state funds spent on dual-eligibles (almost 2% of GDP), this population constitutes an important topic for public finance and policy research. Moreover, this population's experiences could be generalizable to Medicare recipients with other types of secondary insurance, since such insurance is carried by almost 90% of all Medicare recipients. As such, this research may relate to existing Medicare work on employer sponsored coverage (Chandra et al, 2010) and Medigap (Cabral et al, 2014).

In addition, dual-eligible care relates to questions of more general economic and theoretical interest. For example, cost-sharing is an important theme in dual-eligible care, given that this population is subject to minimal amounts of it. As such, identifying cost sharing's impact on this population's spending constitutes an important and unanswered empirical question, especially since the impact of cost-sharing might be different for this population than for others; to this end, the impact of cost sharing could be heterogeneous across health and economic status.

Moreover, this research ties into existing theory on cost-sharing, particularly on the theoretical mechanisms driving cost-sharing's effects. Cost sharing could reduce utilization through reduced moral hazard (Manning et al 1987), but also increase it through reduced preventative care and resulting offsets in preventable utilization (Chandra et al, 2010), and it is valuable to understand the relative importance of each mechanism. Moreover, the magnitude of these offsets could vary by health status, and be greatest for sicker individuals (Chandra et al, 2010).

To start, I examine the effect of supplemental Medicaid coverage on those already in Medicare. Supplemental Medicaid coverage would affect treatment composition and spending primarily through the effective elimination of cost-sharing, which would otherwise be at around 20% under Medicare-only coverage (MedPAC, 2004).

Past work has been hindered by a number of empirical issues, which I attempt to overcome in this paper. First, given compositional differences between those simultaneously in Medicare and Medicaid, and those in Medicare-only, it has been difficult to separate out the effect of supplemental Medicaid coverage from that of underlying enrollee characteristics. As part of a novel identification strategy, I leverage involuntary disenrollments from Medicaid among those who were previously dually-enrolled in Medicaid and Medicare. Critically, disenrollment is not concurrent with individual-level changes to health or economic status. Rather, the disenrollments resulted from a 2009 Tennessee court decision, which allowed Tennessee to check the eligibility of existing Medicaid enrollees, and to disenroll anyone who no longer met the eligibility requirements.Prior to this ruling, Tennessee could not check the eligibility of a subset of Medicaid recipients (those who initially qualified via SSI), nor could it disenroll those no longer eligible, as a result of a 20-year long court prohibition (Wadhwani, 2010).[1]

In my analyses, I focus on Tennessee residents who were simultaneously in Medicaid and Medicare as of 2008. Using a difference-in-difference approach, I examine health utilization among those who were and those who weren't exogenously disenrolled, before and after disenrollment,. As part of my analysis, I focus on those disenrollees who no longer met the economic requirements for Medicaid, but who otherwise still met the disability qualifications. By construction, this group is better off than typical Medicaid or typical dual-enrollees, but probably only marginally so, meaning that my findings could still have external validity. After all, these individuals maintained their Medicaid coverage for an extended period of time, when they could have voluntarily dropped coverage. Throughout these analyses, I make use of Medicare administrative data, which comprehensively tracks health utilization for those in Medicare-only as well as for those dually-enrolled in Medicare and Medicaid (seeing as Medicare functions as the primary payer for both groups).

Altogether, I find that Medicaid disenrollment is associated with a 25-30% decrease in overall utilization and spending, which is highly significant. My results also suggest that moral hazard dominates over prevention-driven offsets in this setting, although both are present to some extent. To this end, I find that Medicaid disenrollment is associated with a substantial, 30% decrease in outpatient care, driven disproportionately by a reduction in elective care; at the same time, I find evidence of offsets-albeit less pronounced-in the form of increased inpatient utilization, disproportionately driven by increases to potentially avoidable care. Unfortunately, I am not able to effectively identify the accompanying effect on health outcomes or overall patient welfare.

[1] Incidentally, the 2009 Medicaid disenrollment is distinct from the one used in Garthwaite et al (2014), given that the 2009 episode is judicially rather than legislatively driven (although coincidentally, both take place in the same state).

These results have substantial relevance for policy, suggesting that part of the high spending on dual-enrollees in Medicaid and Medicare is attributable to a lack of cost-sharing. The absence of cost sharing also results in increases to potentially low-value care. While my results imply that the imposition of cost-sharing could reduce spending and improve care efficiency, even for low-income populations, this justifiably raises concerns about affordability (and also about health outcomes and patient welfare, which I do not address here). One worthwhile approach could be to offset increased cost-sharing with cash voucher payments (which could also be used for non-health expenses), and thereby financially incentivize efficient care while ensuring that it remains affordable. A similar approach has already been adopted in Indiana, and proposed in several other states (Saloner et al 2014).

In Section 2, I go over relevant institutional features of Medicaid, Medicare, as well as overlaps between them. I also review the dynamics of Medicaid disenrollment in Tennessee. In Section 3, I go over the data used in these analyses. In Section 4, I review my empirical design and implementation. In Section 5, I go over the results. In Section 6, I conclude.

2 Medicaid and Medicare

2.1 Background

Medicare and Medicaid were created in 1965, through the Social Security Amendments act. Medicare is federally financed and controlled, and has a uniform program design across all states. Medicaid, meanwhile, is directly administered by individual states, with each state having some latitude over program design, subject to federally imposed limits. Each state also bears up to 50% of the cost of Medicaid, with the remainder of the cost shouldered by the federal government (KFF, 2012).

While Medicaid and Medicare had little overlap at their time of formation, subsequent expansions have yielded a sizeable population that is enrolled in both, currently amounting to over 9 million people. This simultaneous enrollment (referred to as 'dual-eligibility') is most prevalent among the under-65 disabled who receive SSI and SSDI, as well as poor elderly (over 65) who are the recipients of SSI (CBO, 2013).

To this end, Medicare was originally restricted to elderly individuals over 65, who had been legal U.S. residents for over five years. Meanwhile, Medicaid was initially restricted to poor families with children, who were the recipients of cash assistance (through AFDC). In 1972, both programs were expanded to cover individuals who were disabled, with Medicare covering disabled recipients of SSDI[2], and Medicaid covering disabled recipients of SSI[3]; as a result, those receiving SSDI and SSI could be in Medicaid and Medicare simultaneously (SSA, 2011).

Meanwhile, Medicare originally covered only inpatient care (through Part A) and outpatient care (through Part B), along with short-term nursing home stays that take place within 30 days of a hospital discharge (through Part A). In 2003, the program was expanded to also cover prescription drugs, through Part D (SSA, 2011). Meanwhile, Medicaid was originally designed to cover all types of care, including long term nursing homes stays. However, for services such as home care, there is not a clear delineation of payment responsibilities, leading to coordination issues between Medicaid and Medicare for those dually enrolled; further, Medicare pays for all nursing home costs in the 30 days following inpatient hospitalization for duals, while Medicaid pays for all other nursing home care (MedPAC, 2011)

For dual-eligibles, Medicare functions as the primary payer for most services, with

[2]To qualify for SSDI, individuals needed to be disabled and also have a certain amount of work experiences, with the aforementioned work requirement varying by age.

[3]Some states retained more stringent financial requirements than exist for SSI, which had been in place prior to 1972.

Medicaid serving as the secondary payer. For these individuals, Medicare would pay its typical share of expenses (about 80% of the underlying cost) for traditionally covered services, and 0% for uncovered services. Medicaid, meanwhile, would pick up cost sharing expenses on the patient's behalf (such as deductibles and co-pays), which equate to about 20% of medical costs. Further, Medicaid would also cover 100% of the cost of services outside the scope of Medicare (such as long-term institutional care). Those who are dual-eligible, meanwhile, would largely not be responsible for their medical expenses (Carpenter, 1998).

While dual-eligibles make up about 15% of Medicare and Medicaid enrollees, they account for a disproportionate 35% of all Medicaid and Medicare spending. Such high spending could be partly attributable to the population's health characteristics. To this end, the dual-eligible population consists almost entirely of the elderly or disabled, and has higher accompanying rates of disease than the general Medicare or Medicaid population (CBO, 2013). That said, the high spending on this population could also be partly attributable to issues of program design. One specific culprit could be the complete absence of cost-sharing among dual-eligibles, which contrasts with meaningful cost sharing among the Medicare-only population.[4]

Some states, particularly Indiana, have increased the exposure of their Medicaid and dual-eligible populations to cost-sharing, in attempt at reducing unnecessary and wasteful care. As such, understanding the effects of cost-sharing on the dual eligible population can provide insight on the overall effectiveness of these policies (Saloner et al, 2014).

Other factors, such as poor coordination of care between Medicaid & Medicare, along with cost-shifting and provided-based gaming, could also partly account for

[4] Note that since 90% of Medicare-only recipients are enrolled in secondary insurance, cost-sharing rates for this population will typically not be the full 20% of traditional Medicare, since some out of pocket costs will be picked up by the secondary insurer.

dual-eligibles' disproportionately high spending (MedPAC, 2011).[5] That said, these issues would be most salient for an institutionalized population, which is not the focus of the study here.

2.2 Medicaid Disenrollment in Tennessee

Since 1972, SSI recipients in Tennessee qualified automatically for Medicaid. Likewise, for this group, the loss of SSI receipt would mean automatic disenrollment from Medicaid. Some of those disenrolled in this way filed a lawsuit in the 1980's, claiming that they were unjustifiably dropped from Medicaid (this case became designated as 'Cluster Daniels', in reference to the main plaintiff). In 1987, a federal court issued an injunction, pending final resolution of the case, which prevented Tennessee from dropping anyone from Medicaid who had originally qualified for it through SSI (even if that particular individual was no longer on SSI). Following this decision, the state could not drop these individuals from Medicaid, although individuals could drop out on their own accord (Wadhwani, 2010).

In January 2009, this case was finally resolved, allowing Tennessee to check on the Medicaid eligibility of those in the protected class (individuals originally qualifying for Medicaid through SSI); these individuals now accounted for about 150,000 Medicaid recipients (or about 13% of all Tennessee Medicaid enrollees) and about $1.2 billion in annual Medicaid spending. Some members of the group were enrolled in Medicaid-only, whereas others were dual-eligibles and simultaneously in Medicaid and Medicare; as such, this group included elderly as well as non-elderly individuals, and those who

[5] Anecdotally, some of cost-shifting between the two programs could be driven by ambiguity on whether Medicaid or Medicare is primarily responsible for covering certain services, particularly relating to home care. Provided-based gaming, meanwhile, could be driven by higher nursing reimbursement rates under Medicare rather than Medicaid. This would incentivize hospital readmissions for nursing home providers, given that the Medicare, and not Medicaid, reimbursement rate would apply for the first 30 days following return from the hospital.

qualified for Medicare by virtue of SSDI rather than by age (Wadhwani, 2010).

Of the 150,000 Medicaid recipients in the protected class, about 60,000 individuals were ultimately disenrolled following the ruling (presumably, the rest were found to be eligible); of these 60,000, about 25,000 had also been Medicare enrolled.

Tennessee implemented this disenrollment about seven to eight months following the court ruling (in August and September of 2009). Given this interval between notification and disenrollment, there may have been some anticipatory effects, as Medicaid recipients may have loaded up on care on the expectation of eventual disenrollment.

3 Data

In this paper, I use several administrative datasets from CMS, covering Tennessee for the 2008-2011 period. These datasets contain information on demographic characteristics, inpatient and outpatient utilization, and on concurrent Medicaid enrollment status. Critically, this data comprehensively tracks utilization among those in Medicare-only, and also among those concurrently in Medicare and Medicaid ('dual-eligibles'), since Medicare serves as the primary payer for both groups. In addition, I am able to structure the data as an individual-level panel, ensuring that sample selection is not conditional on utilization.

My research design, in combination with this existing data, allows me to overcome issues hampering previous Medicaid research. For example, previous research on Medicaid disenrollments focused on those who were not concurrently enrolled in Medicare, and hence could not make use of Medicare claims data (Finkelstein et al, 2012). Instead, these studies made use of stand-alone discharge data, along with patient-reported data; however, discharge data specifically suffers from a sample selection issue, as inclusion is conditional on hospitalization.

Using information contained in the CMS administrative data, I can precisely construct cohorts that are relevant to my analyses. First, I restrict to residents of Tennessee who were simultaneously enrolled in Medicaid and Medicare, as of the beginning of my study period (early 2008). I further restrict to those who originally qualified for Medicaid (and SSI) by virtue of disability, and who remained disabled as of the start of the study period. In addition, I restrict to those who were not enrolled in the private version of Medicare (Medicare Advantage) at any point in the study period.[6]

Finally, throughout my primary analyses, I focus on the under-65 population, for reasons that I will describe in the next section. In secondary analyses, meanwhile, I focus on the over-65 population. Throughout both analyses, I focus on the period from 2008 to 2011, which corresponds to the six quarters preceding and following the Tennessee Medicaid disenrollment.

3.1 Individual Characteristics and Enrollment Information

This data comes from CMS, and contains person-month level Medicare/Medicaid enrollment information; it specifies Medicare enrollment status, along with concurrent Medicaid status, and the reasons for Medicaid and Medicare eligibility (such as age or disability). This data also specifies private Medicare enrollment status. Finally, it includes various demographic information, which allows me to control for characteristics such as county of residence, age, and date of birth.

While this data tracks enrollment into and out of Medicare, it does not provide the explicit reason for any disenrollments. As such, I identify those who were disenrolled as a result of the 'Cluster Daniels' matter based on the timing of that disenrollment

[6] This restriction is necessary, given that claims data from private Medicare plans is not provided by CMS.

(that is, based on whether this disenrollment took place in July, August, or September of 2009). This approach will not produce a completely accurate measure, given that some disenrollments over that period might not be 'Cluster Daniels' related. That said, the rate of Medicaid disenrollment for the sample population (particularly, disabled individuals) should be sufficiently low to make such mismeasurement of second-order concern.[7]

3.2 Inpatient and Outpatient Utilization Metrics

I track inpatient and outpatient usage for everyone in Medicare, regardless of whether they are concurrently in Medicaid. I do so by linking together person-year level Medicare enrollment data, claims-level inpatient data, and claims-level outpatient data. This linking is facilitated through beneficiary ID information found across all these data.

This data contains every single inpatient and outpatient claim made by Tennessee Medicare recipients, throughout the 2008-2011 period. This data provides information on the timing of each visit, at a month-year level. The data also provides visit-level information on treatment intensity and composition, including the length of stay (for inpatient visits), types and number of procedures performed, and total cost of the visit. Note that the cost measures used in these analyses correspond to administrative Medicare charges, and reflect the amounts that providers actually got paid; since these are inclusive of cost sharing and remain constant across Medicare-only and dual-coverage, they are not mechanically affected by changes to coverage status.

For most of my analyses, I aggregate this data to a person-quarter level, and include those without any utilization as part of the sample (as such, sample selection

[7] Based on prevailing rates of Medicaid disenrollment for this population, I estimate that fewer that 5% of the disenrollments during this period were unrelated to 'Cluster Daniels'.

is not conditional on having an inpatient or outpatient visit).

In Table I, I present average, quarter-based utilization measures for my main analytic sample (those in Tennessee simultaneously enrolled in Medicare and Medicaid, as of the start of 2008). I break these measures out for two separate groups-those who were involuntarily (and exogenously) disenrolled from Medicaid as a result of 'Cluster Daniels' and those who weren't thus disenrolled. I find that those who were disenrolled have substantially lower overall utilization (~30%) than those who weren't, although the extent to which this is attributable to enrollment composition rather than to treatment differences is not readily apparent.

3.3 Utilization Composition Metrics

To measure treatment composition and treatment effectiveness, I rely on existing fields in the claims data, and also construct additional metrics using internal algorithms.

For one measure of treatment composition, I look to data fields specifying the site of care, and separate out visits that are inpatient-hospital based, outpatient-hospital based (including ER visits), and those that take place in non-institutional outpatient settings (such as physician offices or labs). Heterogeneity in the impact of Medicaid disenrollment across these setting could provide insight on the mechanisms for its effect.

As another measure of treatment composition, I look to the types of care performed, based on the 'BETOS' code assigned to a claim. In doing so, I separate out procedure-oriented, test-oriented, and primary care oriented claims from all others, and construct utilization measures accordingly. These measures are geared towards identifying the effect of Medicaid disenrollment on elective care, relative to its effect on preventative and preventable care.

Using algorithms put together by the Dartmouth Institute, I construct even more explicit identifiers of preventable and elective care. First, I measure rates of preventable care by identifying the inpatient visits that could be avoided through improved outpatient care, based on the output from this algorithm. I also identify inpatient visits that are elective, using an alternate Dartmouth algorithm.

4 Identification and Empirical Strategy

Those in Medicare-only and those simultaneously in Medicaid and Medicare (dual-eligibles) differ not only in the cost-sharing they are subject to, but also in their underlying health and demographic characteristics; dual-eligibles typically suffer from worse health and a greater number of co-morbidities than Medicare-only enrollees, even within the disabled population (CBO, 2013). As such, any naïve comparison between those in Medicare-only and those simultaneously in Medicare and Medicaid may reflect patient composition differences between the two, rather than reflecting differences in cost-sharing requirements. To decompose the effect of cost-sharing from that of underlying patient composition, I focus on situations where individuals involuntarily switch from enrollment in both programs to enrollment in Medicare-only, where the timing of that switch is exogenous; in such situations; only Medicaid enrollment status will change, while patient composition will remain fixed.

As part of my identification strategy, I implement a differences-in-differences approach, which leverages involuntary disenrollment from Medicaid among those previously in Medicaid and Medicare. This identification approach is somewhat analogous to the research approach of Finkelstein et al (2012), which instrumented for Medicaid status using Oregon's lottery for Medicaid coverage. This approach is also analogous to that of Garthwaite et al (2014), which used a legislative-driven Medicaid disenrollment in a study on labor outcomes.[8]

Here, the treatment group is made up of individuals who were simultaneously in Medicare and Medicaid, as of the start of the study period (2008), who were disabled, and who were disenrolled from Medicaid following the 'Cluster Daniels' decision (from being found ineligible). The treatment group was not disenrolled from Medicaid for health reasons-as they remained disabled at the time-but rather due to economic ineligibility.[9] The control group, meanwhile, is made up of those simultaneously in Medicaid and Medicare as of the start of the study period, who were not disenrolled following 'Cluster Daniels'. My primary analyses focus on the under-65 population, while a companion set of analyses focuses on those over 65.

Not everyone in the treatment group will be subject to the actual treatment at all points in the post-period (a small fraction will re-enroll in Medicaid, after again becoming eligible); as such, the results would capture an intent-to-treat effect, and would need scaling to reflect the effect of the actual treatment.

The primary identifying assumption is that the timing of Medicaid disenrollment is exogenous, and does not coincide with health developments or other changes among the treatment group. This assumption could be considered reasonable, since the treatment group's disenrollment is entirely driven by economic ineligibility (rather than being health-based); to this end, the treatment group consists of those who remain disabled, at the time of the disenrollments. Further, while the treatment group was no longer economically eligible for Medicaid, few members of the group probably experienced financial improvement at the time of disenrollment, and most

[8] Coincidentally, that disenrollment also took place in Tennessee, although it was driven by legislative action, rather than judicial process. Further, that disenrollment affected a very different population from the one examined here, as it impacted adults who weren't disabled and who weren't simultaneously in Medicare.

[9] These economic requirements, which coincide with the requirements for SSI, are income as well as asset based.

likely experienced them far before. After all, Tennessee could not unilaterly kick members of this group off Medicaid for 20 years-from 1987 through 2009-meaning that many probably became economically ineligible many years before 2009.

A related identifying assumption is that no concurrent developments were taking place in Tennessee at the time of disenrollment, which would differentially impact the treatment group. This identifying assumption is aided by the nature of the disenrollments that I focus on, since they were judicially driven rather than legislatively driven. The judicial, rather than political roots of these disenrollments, makes it less likely that their timing is endogenous to concurrent developments in Tennessee. To further test the validity of this identifying assumption, I check for differential pre-trends using visual as well as statistical approaches.

An addiitonal implicit assumption is that dual-eligibles' disenrollment from Medicaid affects care through the imposition of cost-sharing, and not through any other mechanisms. However, dual-eligible care could also differ from Medicare-only care in terms of program interactions. These interactions could manifest themselves in cost-shifting between Medicaid and Medicare, and also in provider gaming of care (given Medicare's higher rates of nursing home reimbursement, institutions would profit from having their residents readmitted to hospitals at higher rates).[10] That said, these program interaction issues are likely a second order concern; such interactions would be most material to long-term nursing home residents, who are very unlikely to be part of the treatment group.

A potential threat to my research design is the availability of other forms of secondary insurance, in the absence of Medicaid coverage. By enrolling in other forms of secondary insurance, following Medicaid disenrollment, individuals could continue

[10] Medicaid would continue paying for the resident's nursing home while they're in hospital; further, for the first 30 days following a hospital discharge, Medicare-rather than Medicaid-has responsibility for nursing home reimbursement (MedPAC, 2011)

being subject to minimal cost sharing.[11] Among the Medicare population, the most common types of secondary insurance are employer-sponsored commercial plans and Medigap plans (employer as well as non-employer sponsored). While Medicare Advantage could also qualify as a form of secondary insurance, it is not material to this analysis, as Medicare Advantage enrollees have been excluded from the sample[12]

However, there are compelling reasons to not consider the availability of secondary insurance, in the form of employer-sponsored or Medigap coverage, as a first-order threat to my research design. Turning first to employer-sponsored coverage, rates of enrollment conditional on not being in Medigap or Medicare Advantage stand at around 35% (Jacobson et al, 2014); however, rates among my study population are likely to be substantially lower, given socioeconomic considerations. Turning next to Medigap, aggregate-level enrollment data from Tennessee show no change in Medigap enrollment rates following this large-scale Medicaid disenrollment (AHIP, 2009-2011).[13] Finally, for members of my study population under 65, Medigap is likely to have been fairly inaccessible throughout the study period, given that it was not offered on a guaranteed issue basis.Altogether, this suggest that even under a conservative approach, we can assume that fewer than 35% of my study population will be subject to secondary insurance. As such, to obtain an upper bound for the effect of cost-sharing cessation, my results existing estimates should be further scaled by 1.5.

An additional concern about this research design is that the results might not be generalizable to all dual-eligibles, and instead might be specific to the study popula-

[11]Unfortunately, the Medicare administrative data does not track individuals' enrollment status in secondary insurance.

[12]Among those Medicare recipients not enrolled in Medicaid or Medicare-Advantage, about 35% were covered by employer-sponsored secondary insurance, while about 32% were covered by Medigap; meanwhile, about 20% were not covered by any kind of secondary insurance (Jacobson et al, 2014).

[13]Medigap enrollment rates stayed flat in Tennessee over the 2008-2010 period.

tion. After all, the dual-eligibles on which I focus were no longer financially eligible for Medicaid, and so by construction are not representative of all dual-eligibles. That said, the study population is not likely to be substantially wealthier than typical dual-eligibles, given that it also consists of disabled individuals and furthermore consists of those who did not voluntarily drop Medicaid coverage. Altogether, these considerations augment the external validity of my results.

Furthermore, these results might not be generalizable to all dual-eligibles, given treatment non-compliance over the course of the post-period, in the form of some re-enrolling into Medicaid; while we scale our results to correspond to the impact on those 'actually' treated, those who are actually treated (that is, those who don't end up re-enrolling into Medicaid) might not be perfectly representative of dual-eligibles as a whole.

Altogether, the estimating equation for my analysis takes the following form, for individual i, at time t, where the underlying data is aggregated to an individual-quarter level. Also included below are gender, quarter-year, county, and age fixed effects.

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

Since the estimated value of β_2 will reflect the intent-to-treat impact, it needs to be scaled to reflect the actual effect of treatment, based on the fraction of those initially disenrolled from Medicaid that remain disenrolled, at any given point in the post period. To obtain an upper-bound estimate of the effect of cost-sharing cessation, these results would need to be further scaled by a factor of 1.5, per earlier discussion.

5 Results

5.1 Effect of Disenrollment on Medicaid, Drug, and Misc. Coverage Rates

In Figure 1, I document a precipitous drop in Medicaid coverage rates, at the time of the 'Cluster Daniels' disenrollments (taking place Summer of 2009), among my treatment group; note that this is by construction, given how the treatment group is defined. Further, among this treatment group, I document a steady increase in Medicaid coverage rates over the course of the post-period, implying that some of those disenrolled later regained eligibility for Medicaid. Meanwhile, among the control group, which consists of Tennessee dual eligibles who weren't Medicaid disenrolled through 'Cluster Daniels', Medicaid coverage rates are stable and are close to 100% over the pre as well as post-periods.

I proceed by statistically documenting this relationship, and find that 97.5% of my treatment group loses Medicaid coverage at the time of the 'Cluster Daniels' disenrollments' (between the 2nd and 3rd quarters of 2009). The relevant point estimate can be found in Table 2, under column one, under Medicaid Dis.*Q of Disenroll. For these analyses, I use my baseline specification, where the outcome of interest is at a person-quarter level, and the sample is restricted to those under 65 and disabled, who are dually-enrolled in Medicare and Medicaid in Tennessee, as of the beginning of 2008; it is further restricted to those who weren't in Medicare Advantage at any point in the pre or post period. Finally, individuals are only included in the sample for the quarters in which they're Medicare enrolled.

I also find that the treatment's effect on Medicaid status attenuates over the course of the post-period, with the point estimates suggesting that only 2/3 of the original treatment group remains outside of Medicaid, five quarters after the initial disenrollment; meanwhile, at that point, about 1/3 of the cohort has re-enrolled in Medicaid. This can be attributed to individuals becoming re-eligible for Medicaid, as a result of deteriorating finances (such deterioration could, in turn, be driven by deteriorating health). Given that an incomplete fraction of the original treatment group remains Medicaid disenrolled, over the course of the post-period, my main estimates will reflect an 'intent-to-treat' effect rather than the impact on those 'actually treated.' As such, my main estimates will need to be accordingly scaled, to get at the actual treatment effect; for the last quarter of the post period, the appropriate scaling factor would be 1.5, given that 2/3 of the original treatment group is Medicaid disenrolled then.

I perform an additional robustness check, examining how individuals' drug coverage is affected by the loss of Medicaid; while dual-eligibles as well as Medicare-only enrollees would receive drug coverage through Part D for the study period, only for dual-eligibles is such coverage automatic; to this end,Medicare-only enrollees could theoretically opt out of such coverage. However, a statistical examination indicates that drug coverage (Part D) rates are not meaningfully impacted by Medicaid disenrollment, and that drug coverage remains near universal even among those in Medicare-only. One lingering concern is that the type of drug coverage under Part D may be changing, even if its prevalence is not; while dual-eligibles are randomly assigned to a Part D plan, those in Medicare-only can actively select a plan from a number of options. This is not a concern I further address here, although it is may be of second-order importance given that all Part D plans adhere to a standardized benefit design.

Given that my primary sample excludes those in Medicare Advantage at any point in the study period, I perform an additional robustness check, examining whether Medicaid disenrollment among dual-eligibles precipitates enrollment into Medicare

Advantage. Estimates from this analysis, presented in Table A.1, suggest that Medicaid disenrollment among this dual population does not appear to increase Medicare Advantage enrollment rates in excess of 2%, with this precise to within 1% with 95% confidence.

In Table A.2, I examine the effects of 'Cluster Daniels' disenrollments on Medicaid and drug coverage for an alternate sample: the over-65 population (in contrast to the under-65 population, which is the focus of my main analyses). The results suggest that the effect of the 'Cluster Daniels' disenrollments across these two populations is comparable, at least when it comes to Medicaid and drug coverage.

5.2 Effect of Disenrollment on Utilization and Spending

Using involuntary Medicaid disenrollment as an instrument, I consider the effect of supplemental Medicaid coverage on spending and utilization measures. Given that supplemental Medicaid coverage, in this setting, is primarily associated with a cost sharing reduction, I view such involuntary Medicaid disenrollment as an effective instrument for cost sharing. In my main results, my sample restrictions remain the same as before, with the observation-level being at a person-quarter level, and the outcome measures also reflecting person-quarter level utilization and spending. In addition, as before, my main results focus on individuals under the age of 65.

In Figure 2, I document a sharp drop in overall spending (logged) among the treatment group, immediately following involuntary Medicaid disenrollment (which corresponds to the right-most vertical line, while the leftmost corresponds to the time of disenrollment notification). Meanwhile, the control group over this period experiences only a steady upward trend in spending, which remained unchanged at the time of disenrollment notification, as well as at the time of actual disenrollment.

In Figures 3 and 4, I break out spending based on whether it takes place in the inpatient or outpatient setting. Looking at the treatment group relative to the control, I document a substantial drop in spending at the time of actual disenrollment, across the inpatient as well as outpatient settings. However, I document a precipitous rise in inpatient, following disenrollment notification; this could reflect anticipatory effects, as individuals push forward care (such as surgeries) to avoid future cost-sharing. Further, when comparing spending preceding disenrollment notification to spending following actual disenrollment, I find that there is an overall decrease in inpatient spending alongside an increase in outpatient spending.

I proceed to statistically examine the effect of Medicaid disenrollment, and of cost-sharing imposition by extension, on these spending measures, with the results shown in Table 3. These analyses indicate that Medicaid disenrollment results in a 25-30% decrease in overall spending. If assuming average cost-sharing under Medicare-only coverage of 20%, as is postulated in previous literature (Cabral et al 2014)[14], these estimates would imply an arc-elasticity of -.175[15]; this arc-elasticity is consistent with the -.2 estimate from RAND's health insurance experiment (looking at a commercial setting) and Cabral et al (2014)'s estimate of -.11 (looking at Medicare and Medigap).

Given that individuals may have secondary insurance that covers cost-sharing, even in the event of Medicaid disenrollment, these estimates should be viewed as a lower bound for cost sharing's effects, with these estimates and the implied arc-elasticity needing to be further scaled by about 1.5 to arrive at an upper bound. Furthermore, these results are not tainted by possible anticipatory effects, given that I define the pre-period as preceding disenrollment notification, and the post period

[14] This represents an approximation, given that a 20% coinsurance applies only to outpatient Medicare services, while a copay applies for inpatient services and is charged for each day in hospital. Further, deductibles are in place for Part A as well as Part B services.

[15] The arc-elasticity is given by [q2-q1/((q2+q1)/2)]/[(p2-p1)/(p2+p1)/2)], where q1 and p1 correspond to dual-eligibles' utilizations and prices, and q2 and p2 correspond to those of Medicare-only enrolled; note that for dual-eligibles, p1 is defined as 0.

as following actual disenrollment. Finally, this effect does not appear to be driven by differential pre-trends, and also does not appear to attenuate over the post-period. While I do find increases in inpatient spending for the treatment group between disenrollment notification and actual disenrollment, these could reflect attempts to push care forward, particularly since they're concentrated in procedure-based inpatient care (as I determine in separate analyses).

I proceed by decomposing this effect based on care setting, finding a spending decrease of 30% for the outpatient setting, accompanied by a spending increase on inpatient care of about 10%. The increases in inpatient spending, while not always statistically significant, are accompanied by substantial extensive margin increases to inpatient utilization, as documented in Table A.3.

Table 3 presents the specific estimates for the key interaction term from the difference-in-difference specification, which need to be scaled to reflect treatment noncompliance for each quarter; these estimates are broken out for each quarter, relative to a baseline period-the quarter immediately before the time of disenrollment notification (and three quarters before actual disenrollment). For example, for log of total spending, the point estimate of -.189 on Medicaid Dis.*Post-5Q suggests that being disenrolled from Medicaid results in a 28.5% reduction in overall spending from the baseline time period (with scaling by 1.5, based on estimates from Table 2). Given the corresponding standard error, I can rule out a decrease in excess of 38% or below 18%, with 95% confidence.

5.3 Mechanisms Driving Cost-Sharing Effects

The opposing effects I estimate across the inpatient and outpatient margins could reflect the effects of two mechanisms operating simultaneously: those of moral hazard (which would be reduced following Medicaid disenrollment) and preventive care (which could decrease following Medicaid disenrollment, yielding increases in preventable care). After all, moral hazard may manifest itself most in outpatient care, given that typical outpatient treatment is relatively more discretionary. Likewise, the effects of decreased prevention could be most pronounced in the inpatient setting, given that inpatient care is more prevention sensitive. To further test this, I examine the effects of cost sharing on the composition of care within each setting, rather than merely across settings.

Focusing first on outpatient care, I look at whether the effect is heterogeneous across care types, particularly among procedures, tests, and other forms of spending. Procedures and tests are generally considered less discretionary forms of outpatient care, and are generally thought to be physician driven (Finkelstein et al 2014). Meanwhile, other types of outpatient care, such as specialist visits, are thought to be more patient-driven, and hence more discretionary. The results, which are displayed in Table 4, suggest that outpatient spending reductions, under cost sharing, are driven disproportionately by discretionary care (non test and non procedure based); my estimates imply a 30% reduction in such care following Medicaid disenrollment, compared to a 10% reduction in procedure spending, and a 15% reduction in spending on tests.

I then turn to inpatient care, and examine whether the effect is heterogeneous across preventable and non-preventable care types. I gauge the preventability of an admission based on whether the admission originated from the ER; ER admissions are generally thought to be more prevention-sensitive than non-ER ones. In addition, I classify whether an admission is preventable based on whether it could have been averted through improved outpatient care (that is, whether it is ambulatory-sensitive), using a DRG-based Dartmouth algorithm.

In the results, which are presented in Table 5, I find that the effect of Medicaid disenrollment is disproportionately higher across prevention sensitive admissions; my estimates imply that such disenrollment results in a increase in the number of ER admissions, per person-quarter of .02 (or about 28% of the baseline), the results also imply that the effect is between 16% and 40% of the baseline, with 95% confidence. Meanwhile, non-ER admissions only increase by about 2% of their baseline. The same pattern holds when comparing ambulatory-sensitive visits to non-ambulatory sensitive ones. When looking at these measures in terms of spending (regardless of whether in log or level forms), these differentials become much less pronounced; the effects are comparable across prevention and non-prevention sensitive care. Given that preventable care increases much more substantially in terms of visit numbers than spending levels, the marginal prevention-sensitive visit may be relatively low-cost.

In Table 6, I follow up on these results by breaking out inpatient visits into elective and non-elective types; for these results to be consistent with the previous ones, there would need to be a disproportionate increase in non-elective visits, given that non-elective care is more prevention-sensitive. Meanwhile, the actual results imply that Medicaid disenrollment is associated with an approximately 20% increase in the number of non-elective inpatient visits, and a 15% increase in non-elective inpatient spending. Furthermore, there does not appear to be a statistically significant change to the number of elective inpatient visits.

Finally, in Table 7, I look for evidence of decreases to preventative care from Medicaid disenrollment, which could help explain the preceding findings of increases in prevention-sensitive care. Focusing specifically on outpatient primary care and outpatient specialist visits, I find no evidence of decreases in primary care visit numbers, and can rule out a reduction in excess of 5% with 95% confidence. Meanwhile, I do find evidence of decreases to the number of specialist visits, with reductions on the order of 10-15%. However, I am not able to ascertain whether these reductions are associated with decreases in preventative care.

5.4 Heterogeneity in Cost Sharing Effects

Previous research has found that the magnitude of prevention-sensitive offsets, under cost-sharing, could be greater for sicker patients (Chandra et al 2010). To examine whether this pattern holds true among dual-eligibles, I turn my attention to those over 65, who are generally sicker than the under-65 population comprising my primary sample; average spending among this over-65 cohort, relative to those under-65, is over 50% higher. Further, the over-65 dual population is of substantial policy interest, in its own right, given that it makes up a significant share of Medicare/Medicaid enrollment and an even higher share of spending. I present the results of analyses on the effects of Medicaid disenrollment for this population, along with the associated underlying mechanisms, in Table A.4. These results should be viewed as suggestive, rather than causal, given that the under and over-65 populations could differ in other respects besides health status.

First, I find that Medicaid disenrollment leads to an approximately 35% decrease in this population's overall spending; this estimate is based on the original point estimates in Table A.4, which I subsequently scale to reflect the degree of treatment compliance in the post-period (that is, the share of those disenrolled that remain outside of Medicaid, given the estimates in Table A.3). This estimate implies an arc-elasticity of -.21. I then turn my attention to outpatient spending and find that the implied effect corresponds to a 40% reduction. Looking at inpatient spending, I find an implied 20-30% spending increase for the over-65 population, compared to a 10% increase for those under-65. As such, the cohort that's sicker-those over 65-also happens to experience greater inpatient (or prevention sensitive) offsets, consistent with Chandra et al (2010).

One empirical concern is that treatment non-compliance, over the post-period, could be heterogeneous across the under and over 65 groups. This concern is already partially addressed above, given that the results are scaled to reflect such non-compliance, and thereby reflect treatment's effect on those actually treated. However, there could be different selection dynamics for non-compliance, for each of these populations. For example, among those over-65, the compliant set (those remaining outside of Medicaid) might consist of those most susceptible to prevention-based offsets. Meanwhile, among those under-65, the compliant set might consist of those less susceptible to such offsets. As such, for these analyses to reflect the impact of sickness (as opposed to differences in non-compliance), differential selection for non-compliance must be absent across these populations. One factor supporting this assumption is these populations' comparable rates of non-compliance.

In future analyses, I hope to study heterogeneous effects across health status more systematically, by comparing individuals based on their ex-ante health (as measured by their Charlson Co-Morbidity Index). Again, heterogeneity in non-compliance could be a threat to such a research approach.

6 Conclusion

I examine the effects of cost-sharing among a previously understudied population-those dually enrolled in Medicaid and Medicare-a population that critically accounts for 35% of all Medicare and Medicaid spending. While cost-sharing has previously been examined in other settings, my results are nonetheless useful from a policy and economic perspective, since the effects of cost-sharing may not generalizable from one setting to another. I also undertake needed work to identify the mechanisms behind cost-sharing's effects; I find suggestive evidence that spending reductions-under cost sharing-come through reduced moral hazard, but that these reductions are somewhat offset by increases in prevention-sensitive care. I also find that these offsets are greater among a sicker population, those over-65. However, one limitation of this study is that it does not identify the impact of cost-sharing on patient outcomes, nor on patient welfare more generally.

Future work is needed to better understand heterogeneity in cost sharing's effects across health and socioeconomic status, while also better gauging the impact on patient welfare. Such work could inform more effective cost-sharing policies in Medicaid, Medicare, and other settings, which could be tailored based on individuals' characteristics as well as the type of treatment.

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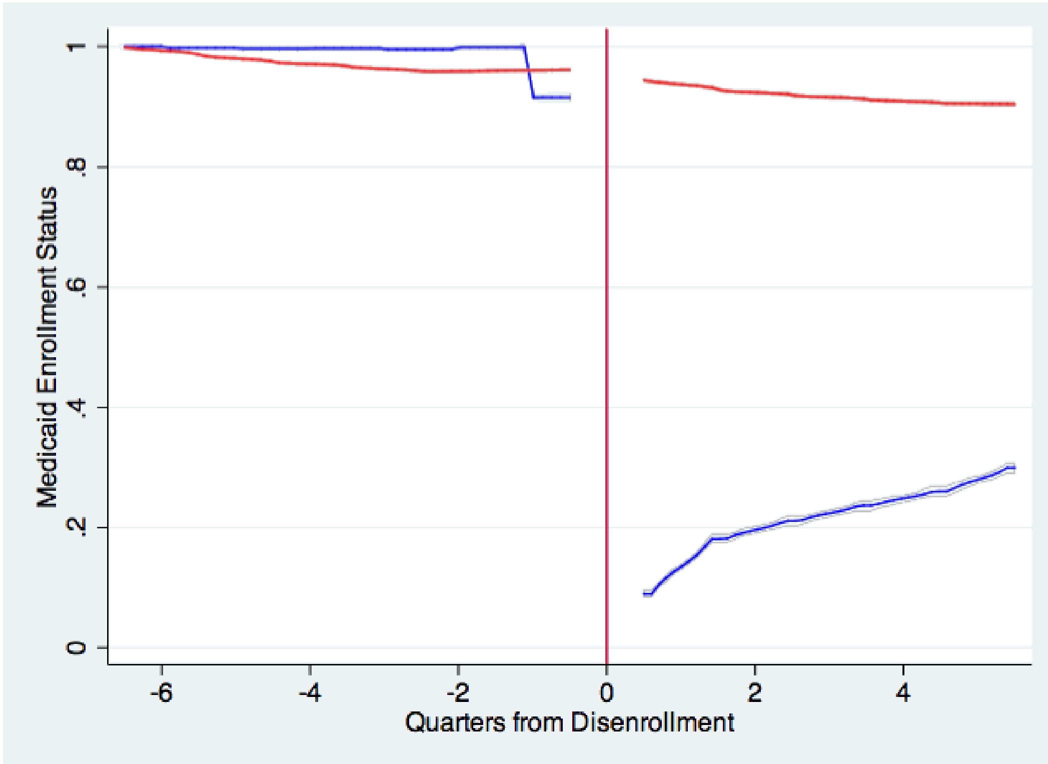

Figure 1: Effect of 'Cluster Daniels' on Medicaid Enrollment Status

Blue: Invol. & Exogenously Disenrolled from Medicaid, via Cluster Daniels (Treatment)
Red: Not Disenrolled via Cluster Daniels (Control)

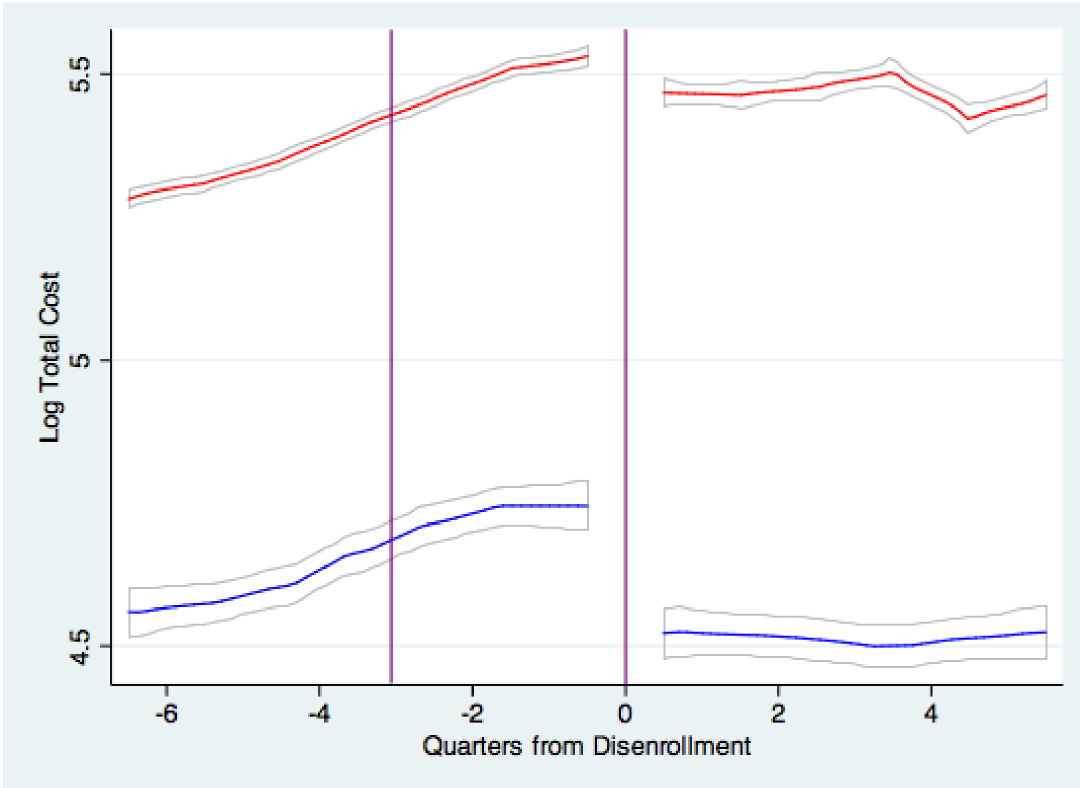

Figure 2: Effect of Medicaid Disenrollment on Overall Medical Spending (Logged)

Blue: Invol. and Exogenously Disenrolled from Medicaid, via Cluster Daniels (Treatment)
Red: Not Disenrolled via Cluster Daniels (Control)

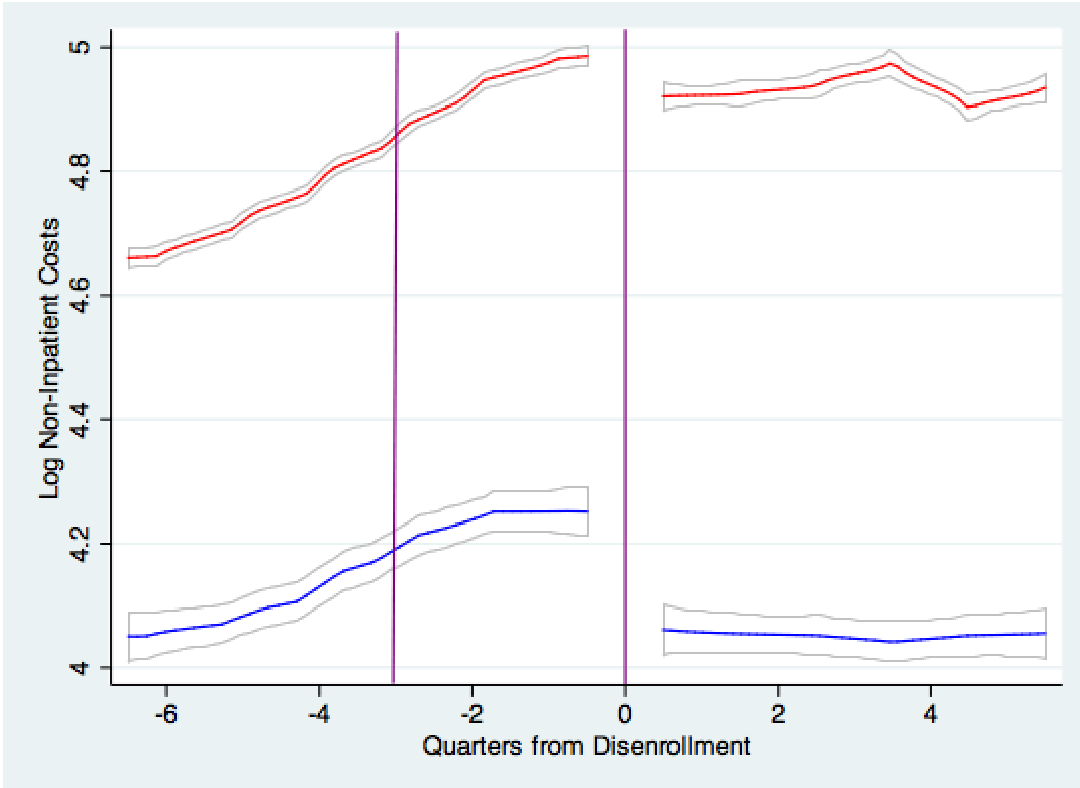

Figure 3: Effect of Medicaid Disenrollment on Non-Inpatient Spending (Logged)

Blue: Invol. and Exogenously Disenrolled from Medicaid, via Cluster Daniels (Treatment)
Red: Not Disenrolled via Cluster Daniels (Control)

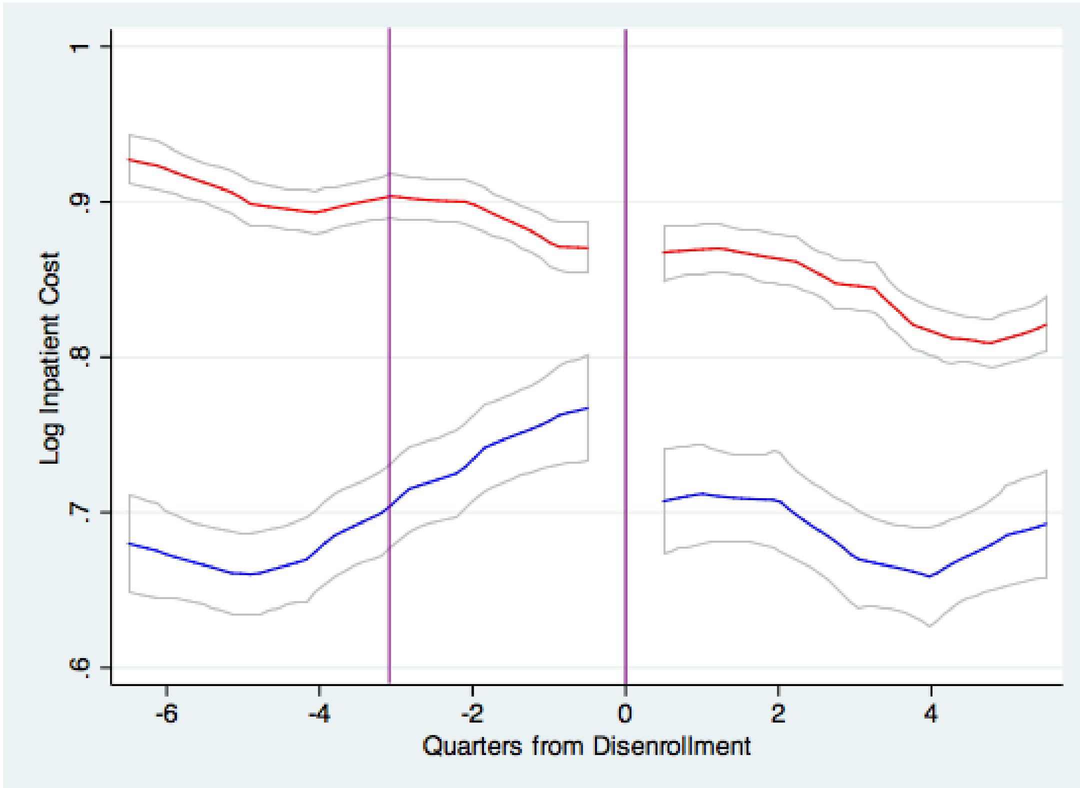

Figure 4: Effect of Medicaid Disenrollment on Inpatient Spending (Logged)

Blue: Invol. & Exog. Disenrolled from Medicaid, through Cluster Daniels (Treatment)
Red: Not Disenrolled via Cluster Daniels (Control)

Table 1: Summary Statistics

	Disenroll Group	Non-Disenroll Group
Coverage		
Medicaid Enrollment Status	0.585	0.944
	(0.493)	(0.228)
Drug Coverage Status	0.999	0.999
	(0.016)	(0.015)
Total Utilization		
Total Spending	2,078	-2,824
	(7,415)	(8,585)
Inpatient Spending	978	1,363
	(5,751)	(6,658)
Outpatient Spending	1,100	1,461
	(2,912)	(3,342)
Electability of Care		
Outp Procedure Spending	235	306
	(739)	(827)
Outpatient Non-Procedure Spending	865	1,155
	(2,492)	(2,908)
Inp Elective Spending	298	444
	(2,910)	(3,461)
Inp Non-Elective Spending	680	920
	(4,493)	(5,041)
Prevention		
Primary Care Visits	0.893	1.200
	(1.973)	(2.354)
Inp Preventable Spending	141	201
	(1,471)	(1,821)
Inp Non-Preventable Spending	837	1,162
	(5,407)	(6,192)
N	144,158	682,314

Notes: Table presents summary statistics for various outcome variables of interest, where the unit of observation is at an individual-quarter level, for the 2008-2011 period. Summary statistics are broken out for those who were and who weren't disenrolled from Medicaid as part of the 'Cluster Daniels' matter (in mid 2008). The sample is restricted to those under 65 and disabled, who were dually-enrolled in Medicaid and Medicare at the start of 2008; the sample is also restricted to Tennessee only. In addition, the sample is restricted to those Standard-errors are clustered at the individual level. Data is taken from CMS Administrative files.

Table 2: Effect of Disenrollment on Medicaid and Drug Status

	Simult. in Medicaid	Drug Coverage
Medicaid Disen.*Pre-5 Q	-0.006***	0.000
	(0.001)	(0.000)
Medicaid Disen.*Pre-4 Q	0.009***	-0.000**
	(0.001)	(0.000)
Medicaid Disen.*Pre-3 Q	Baseline	
Medicaid Disen.*Pre-2 Q	0.007***	-0.001**
	(0.001)	(0.000)
Medicaid Disen.*Pre-1 Q	0.009***	-0.001**
	(0.001)	(0.000)
Medicaid Disen.*Q of Disenroll	-0.975***	-0.001***
	(0.001)	(0.000)
Medicaid Disen.*Post-1 Q	-0.810***	-0.000
	(0.004)	(0.000)
Medicaid Disen.*Post-2 Q	-0.753***	-0.000
	(0.004)	(0.000)
Medicaid Disen.*Post-3 Q	-0.717***	0.000
	(0.004)	(0.000)
Medicaid Disen.*Post-4 Q	-0.686***	-0.000
	(0.004)	(0.000)
Medicaid Disen.*Post-5 Q	-0.664***	-0.001**
	(0.005)	(0.000)
Mean	0.881	0.999
N	826,472	

Notes: Table presents linear regression models, where the outcome variables include Medicaid and drug coverage status. The terms of interest are Medicaid Disen., interacted with pre and post terms. Medicaid Disen. is defined as those dual-eligibles involuntarily disenrolled from Medicaid as a result of 'Cluster Daniels', but who remain in Medicare. The unit of observation is at the individual-quarter level, for the 2008-2011 period. Year-quarter, county, age, and gender fixed effects are included as part of the analysis. The sample is restricted to those under 65 and disabled, who were dually-enrolled in Medicaid and Medicare at the start of 2008; the sample is also restricted to Tennessee only. Finally, the sample is restricted to those who were not in Medicare Advantage at any point in the pre or post period. Standard-errors are clustered at the individual level. Data is taken from CMS Administrative files.

Table 3: Effect of Disenrollment on Medical Spending

	Log Spending		
	Outp.+Inp.	*Outpatient*	*Inpatient*
Medicaid Disen.*Pre-5 Q	-0.004	-0.006	-0.043
	(0.027)	(0.025)	(0.030)
Medicaid Disen.*Pre-4 Q	0.047*	0.039	0.013
	(0.027)	(0.025)	(0.031)
Medicaid Disen.*Pre-3 Q		Baseline	
Medicaid Disen.*Pre-2 Q	-0.008	-0.022	0.055*
	(0.028)	(0.026)	(0.031)
Medicaid Disen.*Pre-1 Q	-0.068**	-0.094***	0.111***
	(0.030)	(0.028)	(0.032)
Medicaid Disen.*Q of Disen.	-0.341***	-0.361***	0.011
	(0.032)	(0.030)	(0.032)
Medicaid Disen.*Post-1 Q	-0.182***	-0.208***	0.061*
	(0.032)	(0.030)	(0.033)
Medicaid Disen.Post-2 Q	-0.266***	-0.293***	0.022
	(0.033)	(0.031)	(0.033)
Medicaid Disen.*Post-3 Q	-0.276***	-0.290***	0.004
	(0.034)	(0.032)	(0.033)
Medicaid Disen.*Post-4 Q	-0.193***	-0.220***	0.058*
	(0.034)	(0.032)	(0.033)
Medicaid Disen.*Post-5 Q	-0.189***	-0.219***	0.089***
	(0.035)	(0.033)	(0.034)
Mean	5.24	5.1	0.83
N		826,472	

Notes: Table presents linear regression models, where the outcome variables are person-quarter aggregated spending measures. The terms of interest are Medicaid Disen., interacted with pre and post terms. Medicaid Disen. is defined as those dual-eligibles involuntarily disenrolled from Medicaid as a result of 'Cluster Daniels', but who remain in Medicare. The unit of observation has been aggregated to the individual-quarter level, for the 2008-2011 period. Year-quarter, county, age, and gender fixed effects are included as part of the analysis. The sample is restricted to those under 65 and disabled, who were dually-enrolled in Medicaid and Medicare at the start of 2008; the sample is also restricted to Tennessee only. Finally, the sample is restricted to those who were not in Medicare Advantage at any point in the pre or post period. Standard-errors are clustered at the individual level. Data is taken from CMS Administrative files.

Table 4: Moral Hazard: Medicaid Disenrollment's Effect on Various Outp Spending

	Log Outp Spending		
	Proc	*Tests*	*Non Proc/Tests*
Medicaid Disen.*Pre-5 Q	-0.003	0.000	0.003
	(0.026)	(0.027)	(0.024)
Medicaid Disen.*Pre-4 Q	0.028	0.031	0.020
	(0.026)	(0.027)	(0.024)
Medicaid Disen.*Pre-3 Q		Baseline	
Medicaid Disen.*Pre-2 Q	-0.028	0.006	-0.033
	(0.028)	(0.028)	(0.024)
Medicaid Disen.*Pre-1 Q	-0.056**	-0.063**	-0.084***
	(0.028)	(0.029)	(0.026)
Medicaid Disen.*Q of Disen.	-0.114***	-0.183***	-0.308***
	(0.028)	(0.029)	(0.027)
Medicaid Disen.*Post-1 Q	-0.096***	-0.089***	-0.174***
	(0.028)	(0.029)	(0.028)
Medicaid Disen.Post-2 Q	-0.124***	-0.163***	-0.261***
	(0.029)	(0.030)	(0.029)
Medicaid Disen.*Post-3 Q	-0.138***	-0.156***	-0.270***
	(0.029)	(0.030)	(0.029)
Medicaid Disen.*Post-4 Q	-0.078***	-0.068**	-0.209***
	(0.029)	(0.030)	(0.030)
Medicaid Disen.*Post-5 Q	-0.061**	-0.104***	-0.204***
	(0.030)	(0.031)	(0.030)
Mean	1.22	2.309	4.264
N		826,472	

Notes: Table presents linear regression models, where the outcome variables are person-quarter aggregated spending measures. The terms of interest are Medicaid Disen., interacted with pre and post terms. Medicaid Disen. is defined as those dual-eligibles involuntarily disenrolled from Medicaid as a result of 'Cluster Daniels', but who remain in Medicare. The unit of observation has been aggregated to the individual-quarter level, for the 2008-2011 period. Year-quarter, county, age, and gender fixed effects are included as part of the analysis. The sample is restricted to those under 65 and disabled, who were dually-enrolled in Medicaid and Medicare at the start of 2008; the sample is also restricted to Tennessee only. Finally, the sample is restricted to those who were not in Medicare Advantage at any point in the pre or post period. Standard-errors are clustered at the individual level. Data is taken from CMS Administrative files.

Table 5: Prevention Offsets: Inpatient Setting

	Inp Visits					Log Inpatient Spending				
	All	*ER*	*Non-ER*	*Prev*	*Non-Prev*	All	ER	Non-ER	Prev	Non Prev
Mcd. Disen.*Pre-5 Q	-0.011**	-0.008**	-0.004	0.000	-0.011**	-0.043	-0.036	-0.013	-0.047***	-0.015
	(0.005)	(0.004)	(0.003)	(0.002)	(0.005)	(0.030)	(0.024)	(0.024)	(0.015)	(0.028)
Mcd. Disen.*Pre-4 Q	0.002	0.004	-0.002	0.004*	-0.002	0.013	0.005	0.021	-0.019	0.026
	(0.005)	(0.004)	(0.003)	(0.002)	(0.005)	(0.031)	(0.025)	(0.024)	(0.016)	(0.029)
Mcd. Disen.*Pre-3 Q	Baseline									
Mcd. Disen.*Pre-2 Q	0.013**	0.010***	0.003	0.008***	0.005	0.055*	0.031	0.044*	0.015	0.048
	(0.005)	(0.004)	(0.003)	(0.002)	(0.005)	(0.031)	(0.025)	(0.024)	(0.016)	(0.030)
Mcd. Disen.*Pre-1 Q	0.022***	0.018***	0.004	0.007***	0.014***	0.111***	0.104***	0.027	-0.001	0.115***
	(0.006)	(0.004)	(0.003)	(0.002)	(0.005)	(0.032)	(0.026)	(0.024)	(0.017)	(0.030)
Mcd. Disen.*Q of Dis.	0.013**	0.014***	-0.000	0.005**	0.008	0.011	0.028	-0.002	-0.004	0.018
	(0.006)	(0.004)	(0.003)	(0.002)	(0.005)	(0.032)	(0.026)	(0.024)	(0.017)	(0.029)
Mcd. Disen.*Post-1 Q	0.013**	0.016***	-0.003	0.008***	0.005	0.061*	0.054**	0.031	0.007	0.058*
	(0.006)	(0.004)	(0.003)	(0.002)	(0.005)	(0.033)	(0.027)	(0.025)	(0.017)	(0.031)
Mcd. Disen.Post-2 Q	0.012**	0.014***	-0.001	0.007***	0.006	0.022	0.037	-0.001	-0.003	0.022
	(0.006)	(0.004)	(0.003)	(0.002)	(0.005)	(0.033)	(0.027)	(0.025)	(0.017)	(0.031)
Mcd. Disen.*Post-3 Q	0.009	0.008*	0.002	0.004*	0.006	0.004	0.005	0.016	-0.016	0.018
	(0.006)	(0.004)	(0.004)	(0.002)	(0.005)	(0.033)	(0.027)	(0.025)	(0.017)	(0.031)
Mcd. Disen.*Post-4 Q	0.010*	0.005	0.005	0.004	0.007	0.058*	0.039	0.046*	-0.003	0.069**
	(0.006)	(0.004)	(0.003)	(0.002)	(0.005)	(0.033)	(0.027)	(0.024)	(0.017)	(0.031)
Mcd. Disen.*Post-5 Q	0.015**	0.014***	0.001	0.007***	0.007	0.089***	0.079***	0.050**	0.020	0.084***
	(0.006)	(0.004)	(0.003)	(0.002)	(0.005)	(0.034)	(0.028)	(0.025)	(0.018)	(0.031)
Mean	0.128	0.076	0.052	0.026	0.102	0.831	0.523	0.436	0.204	0.695
N	826,472									

Notes: Table presents linear regression models, where the outcome variables are person-quarter aggregated spending and visitmeasures. The terms of interest are Medicaid Disen., interacted with pre and post terms. Medicaid Disen. is defined as those dual-eligibles involuntarily disenrolled from Medicaid as a result of 'Cluster Daniels', but who remain in Medicare. The unit of observation has been aggregated to the individual-quarter level, for the 2008-2011 period. Year-quarter, county, age, and gender fixed effects are included as part of the analysis. The sample is restricted to those under 65 and disabled, who were dually-enrolled in Medicaid and Medicare at the start of 2008; the sample is also restricted to Tennessee only. Finally, the sample is restricted to those who were not in Medicare Advantage at any point in the pre or post period. Standard-errors are clustered at the individual level. Data is taken from CMS Administrative files.

Table 6: Prevention Offsets: Inpatient Setting

	Inpatient Visits			Log Inp Spending		
	All	*Elect*	*Non-Elect*	*All*	*Elect*	*Non-Elect*
Mcd. Disen.*Pre-5 Q	-0.011**	-0.003	-0.008*	-0.043	-0.023	-0.024
	(0.005)	(0.002)	(0.004)	(0.030)	(0.020)	(0.027)
Mcd. Disen.*Pre-4 Q	0.002	-0.001	0.003	0.013	0.008	0.023
	(0.005)	(0.003)	(0.004)	(0.031)	(0.020)	(0.027)
Mcd. Disen.*Pre-3 Q	Baseline					
Mcd. Disen.*Pre-2 Q	0.013**	0.002	0.012**	0.055*	-0.002	0.061**
	(0.005)	(0.003)	(0.005)	(0.031)	(0.020)	(0.028)
Mcd. Disen.*Pre-1 Q	0.022***	0.003	0.019***	0.111***	0.009	0.115***
	(0.006)	(0.003)	(0.005)	(0.032)	(0.020)	(0.029)
Mcd. Disen.*Q of Dis.	0.013**	0.001	0.013***	0.011	-0.026	0.047*
	(0.006)	(0.003)	(0.005)	(0.032)	(0.020)	(0.028)
Mcd. Disen.*Post-1 Q	0.013**	-0.001	0.013***	0.061*	0.002	0.074**
	(0.006)	(0.003)	(0.005)	(0.033)	(0.021)	(0.029)
Mcd. Disen.Post-2 Q	0.012**	-0.001	0.013***	0.022	-0.003	0.035
	(0.006)	(0.003)	(0.005)	(0.033)	(0.021)	(0.029)
Mcd. Disen.*Post-3 Q	0.009	0.002	0.007	0.004	-0.007	0.020
	(0.006)	(0.003)	(0.005)	(0.033)	(0.020)	(0.030)
Mcd. Disen.*Post-4 Q	0.010*	0.000	0.010**	0.058*	-0.015	0.086***
	(0.006)	(0.003)	(0.005)	(0.033)	(0.020)	(0.029)
Mcd. Disen.*Post-5 Q	0.015**	0.001	0.014***	0.089***	0.012	0.106***
	(0.006)	(0.003)	(0.005)	(0.034)	(0.021)	(0.030)
Mean	0.128	0.031	0.097	0.831	0.284	0.649
N	826,472					

Notes: Table presents linear regression models, where the outcome variables are spending and utilization measures, aggregated at a person-quarter level. The terms of interest are Medicaid Disen., interacted with pre and post terms. Medicaid Disen. is defined as those dual-eligibles involuntarily disenrolled from Medicaid as a result of 'Cluster Daniels', but who remain in Medicare. The unit of observation has been aggregated to the individual-quarter level, for the 2008-2011 period. Year-quarter, county, age, and gender fixed effects are included as part of the analysis. The sample is restricted to those under 65 and disabled, who were dually-enrolled in Medicaid and Medicare at the start of 2008; the sample is also restricted to Tennessee only. Finally, the sample is restricted to those who were not in Medicare Advantage at any point in the pre or post period. Standard-errors are clustered at the individual level. Data is taken from CMS Administrative files.

Table 7: Effect on Outpatient Preventative Care

	Outp Visits	
	Prim Care Visits	*Spec Visits*
Medicaid Disen.*Pre-5 Q	0.003	-0.026
	(0.021)	(0.024)
Medicaid Disen.*Pre-4 Q	0.019	-0.001
	(0.021)	(0.026)
Medicaid Disen.*Pre-3 Q		
Medicaid Disen.*Pre-2 Q	0.043*	0.011
	(0.023)	(0.026)
Medicaid Disen.*Pre-1 Q	0.024	-0.017
	(0.024)	(0.029)
Medicaid Disen.*Q of Disen.	-0.017	-0.058*
	(0.024)	(0.030)
Medicaid Disen.*Post-1 Q	0.038	-0.034
	(0.025)	(0.030)
Medicaid Disen.Post-2 Q	-0.032	-0.088***
	(0.023)	(0.031)
Medicaid Disen.*Post-3 Q	-0.038	-0.094***
	(0.024)	(0.031)
Medicaid Disen.*Post-4 Q	-0.002	-0.062**
	(0.025)	(0.030)
Medicaid Disen.*Post-5 Q	0.000	-0.014
	(0.026)	(0.034)
Mean	1.147	1.248
N	826,472	

Notes: Table presents linear regression models, where the outcome variables are outpatient utilization measures, aggregated at a person-quarter level. The terms of interest are Medicaid Disen., interacted with pre and post terms. Medicaid Disen. is defined as those dual-eligibles involuntarily disenrolled from Medicaid as a result of 'Cluster Daniels', but who remain in Medicare. The unit of observation has been aggregated to the individual-quarter level, for the 2008-2011 period. Year-quarter, county, age, and gender fixed effects are included as part of the analysis. The sample is restricted to those under 65 and disabled, who were dually-enrolled in Medicaid and Medicare at the start of 2008; the sample is also restricted to Tennessee only. Finally, the sample is restricted to those who were not in Medicare Advantage at any point in the pre or post period. Standard-errors are clustered at the individual level. Data is taken from CMS Administrative files.

Appendix

Table A.1: Effect of Medicaid Disenrollment on MA Enroll.

	MA Enrollment Status
Medicaid Disen.*Pre-5 Q	-0.003***
	(0.001)
Medicaid Disen.*Pre-4 Q	-0.002**
	(0.001)
Medicaid Disen.*Pre-3 Q	Baseline
Medicaid Disen.*Pre-2 Q	-0.011***
	(0.001)
Medicaid Disen.*Pre-1 Q	-0.017***
	(0.002)
Medicaid Disen.*Q of Disen.	0.019***
	(0.002)
Medicaid Disen.*Post-1 Q	-0.002
	(0.003)
Medicaid Disen.Post-2 Q	-0.013***
	(0.003)
Medicaid Disen.*Post-3 Q	-0.018***
	(0.003)
Medicaid Disen.*Post-4 Q	-0.019***
	(0.003)
Medicaid Disen.*Post-5 Q	-0.004
	(0.004)
Mean	0.213
N	1,409,420

Notes: Table presents linear regression models, where the outcome variable is Medicare Advantage enrollment status, at a person-quarter level. The terms of interest are Medicaid Disen., interacted with pre and post terms. Medicaid Disen. is defined as those dual-eligibles involuntarily disenrolled from Medicaid as a result of 'Cluster Daniels', but who remain in Medicare. The unit of observation has been aggregated to the individual-quarter level, for the 2008-2011 period. Year-quarter, county, age, and gender fixed effects are included as part of the analysis. The sample is restricted to those under 65 and disabled, who were dually-enrolled in Medicaid and Medicare at the start of 2008; the sample is also restricted to Tennessee only. Standard-errors are clustered at the individual level. Data is taken from CMS Administrative files.

Table A.2: Over 65: Effect of Disenrollment on Medicaid and Drug Cov

	Simult. in Medicaid	Drug Coverage
Medicaid Disen.*Pre-5 Q	-0.006***	-0.000
	-0.002	(0.000)
Medicaid Disen.*Pre-4 Q	0.010***	-0.000*
	-0.002	(0.000)
Medicaid Disen.*Pre-3 Q		
Medicaid Disen.*Pre-2 Q	0.010***	0.001
	-0.002	(0.001)
Medicaid Disen.*Pre-1 Q	0.006***	0.001
	-0.002	(0.001)
Medicaid Disen.*First Q of Disenroll	-0.978***	0.001
	-0.002	(0.001)
Medicaid Disen.*Post-1 Q	-0.809***	0.001
	-0.009	(0.001)
Medicaid Disen.*Post-2 Q	-0.768***	0.001
	-0.01	(0.001)
Medicaid Disen.*Post-3 Q	-0.739***	0.001
	-0.01	(0.001)
Medicaid Disen.*Post-4 Q	-0.714***	0.001
	-0.011	(0.001)
Medicaid Disen.*Post-5 Q	-0.689***	0.001
	-0.011	(0.001)
Mean	0.915	0.999
N	204,394	

Notes: Table presents linear regression models, where the outcome variables are Medicaid and drug coverage status, at a person-quarter level. The terms of interest are Medicaid Disen., interacted with pre and post terms. Medicaid Disen. is defined as those dual-eligibles involuntarily disenrolled from Medicaid as a result of 'Cluster Daniels', but who remain in Medicare. The unit of observation has been aggregated to the individual-quarter level, for the 2008-2011 period. Year-quarter, county, age, and gender fixed effects are included as part of the analysis. The sample is restricted to those over 65 and disabled, who were dually-enrolled in Medicaid and Medicare at the start of 2008; the sample is also restricted to Tennessee only. Finally, the sample is restricted to those who were not in Medicare Advantage at any point in the pre or post period. Standard-errors are clustered at the individual level. Data is taken from CMS Administrative files.

Table A.3: Effect of Medicaid Disenrollment on Inp. Utilization

	Inpatient Utilization		
	Hospitalizations	*LOS*	*Procedures*
Medicaid Disen.*Pre-5 Q	-0.011**	-0.063	-0.005
	(0.005)	(0.045)	(0.010)
Medicaid Disen.*Pre-4 Q	0.002	0.081	0.007
	(0.005)	(0.049)	(0.010)
Medicaid Disen.*Pre-3 Q		Baseline	
Medicaid Disen.*Pre-2 Q	0.013**	0.141***	0.021**
	(0.005)	(0.049)	(0.010)
Medicaid Disen.*Pre-1 Q	0.022***	0.238***	0.068***
	(0.006)	(0.054)	(0.012)
Medicaid Disen.*Q of Disen.	0.013**	0.140***	0.040***
	(0.006)	(0.051)	(0.011)
Medicaid Disen.*Post-1 Q	0.013**	0.165***	0.021*
	(0.006)	(0.053)	(0.011)
Medicaid Disen.Post-2 Q	0.012**	0.162***	0.031***
	(0.006)	(0.052)	(0.011)
Medicaid Disen.*Post-3 Q	0.009	0.128**	0.040***
	(0.006)	(0.054)	(0.012)
Medicaid Disen.*Post-4 Q	0.010*	0.132**	0.024**
	(0.006)	(0.052)	(0.011)
Medicaid Disen.*Post-5 Q	0.015**	0.189***	0.035***
	(0.006)	(0.056)	(0.013)
Mean	0.128	0.894	0.166
N		826,472	

Notes: Table presents linear regression models, where the outcome variable is Medicare Advantage enrollment status, at a person-quarter level. The terms of interest are Medicaid Disen., interacted with pre and post terms. Medicaid Disen. is defined as those dual-eligibles involuntarily disenrolled from Medicaid as a result of 'Cluster Daniels', but who remain in Medicare. The unit of observation has been aggregated to the individual-quarter level, for the 2008-2011 period. Year-quarter, county, age, and gender fixed effects are included as part of the analysis. The sample is restricted to those under 65 and disabled, who were dually-enrolled in Medicaid and Medicare at the start of 2008; the sample is also restricted to Tennessee only. Standard-errors are clustered at the individual level. Data is taken from CMS Administrative files.

Table A.4: Over 65: Effect of Disenrollment on Spending

	Log Spending		
	Outpatient+Inpatient	*Outpatient*	*Inpatient*
Medicaid Disen.*Pre-5 Q	-0.100	-0.114**	0.034
	(0.062)	(0.055)	(0.087)
Medicaid Disen.*Pre-4 Q	0.041	0.020	0.042
	(0.061)	(0.054)	(0.090)
Medicaid Disen.*Pre-3 Q		Baseline	
Medicaid Disen.*Pre-2 Q	0.149**	0.066	0.456***
	(0.065)	(0.057)	(0.095)
Medicaid Disen.*Pre-1 Q	0.113*	-0.010	0.597***
	(0.068)	(0.060)	(0.098)
Medicaid Disen.*Q of Disen.	-0.662***	-0.732***	0.078
	(0.081)	(0.074)	(0.091)
Medicaid Disen.*Post-1 Q	-0.248***	-0.281***	0.112
	(0.077)	(0.069)	(0.099)
Medicaid Disen.Post-2 Q	-0.247***	-0.295***	0.196**
	(0.078)	(0.070)	(0.098)
Medicaid Disen.*Post-3 Q	-0.235***	-0.284***	0.167*
	(0.078)	(0.071)	(0.099)
Medicaid Disen.*Post-4 Q	-0.266***	-0.324***	0.179*
	(0.079)	(0.071)	(0.098)
Medicaid Disen.*Post-5 Q	-0.237***	-0.292***	0.206**
	(0.083)	(0.075)	(0.102)
N		204,394	

Notes: Table presents linear regression models, where the outcome variables are medical spending measures, aggregated at a person-quarter level. The terms of interest are Medicaid Disen., interacted with pre and post terms. Medicaid Disen. is defined as those dual-eligibles involuntarily disenrolled from Medicaid as a result of 'Cluster Daniels', but who remain in Medicare. The unit of observation has been aggregated to the individual-quarter level, for the 2008-2011 period. Year-quarter, county, age, and gender fixed effects are included as part of the analysis. The sample is restricted to those over 65 and disabled, who were dually-enrolled in Medicaid and Medicare at the start of 2008; the sample is also restricted to Tennessee only. Finally, the sample is restricted to those who were not in Medicare Advantage at any point in the pre or post period. Standard-errors are clustered at the individual level. Data is taken from CMS Administrative files.

CHAPTER 3: Who Benefits when the Government Pays More? Pass-Through in the Medicare Advantage Program

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1 Introduction

Governments often contract with private firms to provide publicly financed goods and services. The scope of these contracting arrangements is large, representing 10% of GDP in the U.S. in 2008 (OECD, 2011). The range of industries, goods and services is also vast, ranging from defense contractors making military helicopters to landscaping companies mowing the lawns of publicly-owned property. Private firms are also increasingly involved in social services such as education and health care. Theoretically, "contracting out" could lead to improved efficiency, given that private firms have powerful incentives to control costs. Additionally, if the government contracts with multiple firms (or includes a government option), consumers may have access to more choice. This can improve consumer surplus in two ways: additional competition can lead to quality improvements and private firms may more effectively cater to heterogeneous consumer preferences.

An important example of "contracting out" can be seen in the Medicare program, which currently provides health insurance to 55 million U.S. residents, with total expenditures estimated to have exceeded $600 billion in 2013 (CMS, 2013; CBO, 2013). For most Medicare recipients, the federal government directly reimburses hospitals, physicians, and other health care providers on a fee-for-service basis. However, for 17 million (and 31 percent of all) Medicare recipients, the federal government instead contracts with private insurers and other organizations to coordinate and finance medical care as part of the Medicare Advantage (MA) program. This paper examines the MA market and explores how the quality of private provision changes as the generosity of the contract increases.

A large body of previous research has investigated the effect of Medicare Advantage on Medicare expenditures, health care utilization, and health outcomes (Afendulis et al. 2013, Landon et al. 2012, Lemieux et al. 2012). A related strand of research has explored how MA enrollment is affected by the generosity of plan reimbursement (Cawley et al. 2005, Pope et al. 2006). Yet, surprisingly little research has investigated how the characteristics of Medicare Advantage coverage vary with the generosity of plan reimbursement.[1] Theoretically, one would expect plan payment rates to influence both the quality of coverage offered by private insurers and the entry decisions of some insurers. This gap in the literature is unfortunate, given that a key feature of the recently enacted Affordable Care Act gradually lowers reimbursement to MA plans by an estimated \$156 billion from 2013-22 (CBO, 2012). While the Congressional Budget Office and others have estimated that these lower payment rates will reduce MA enrollment, there is little evidence on how the number of options and the quality of coverage will change for those who remain in the program.

In this study, we aim to partially fill this gap in the literature by exploiting policy-induced variation in the generosity of MA plan reimbursement. In counties with relatively low Medicare Fee-for-Service (FFS) spending, benchmarks are set at a payment floor, so that payments to MA plans do not fall below a certain level. The payment floor is 10.5 percent higher in counties that belong to metropolitan areas with more than 250,000 residents than in all other counties. We exploit this cross-sectional variation using data for the 2007 through 2011 period to explore the impact of the additional reimbursement on MA enrollment and on the generosity of MA coverage. This period represents a substantial expansion of the MA program, as show in Figure 1. We compare outcomes in urban counties (which are in metropolitan areas with a population of 250,000 or more) with similar counties below this threshold. Our specifications control flexibly for both the county and the MSA population and for county per-capita Medicare FFS expenditures. To obtain a more comparable set of urban and non-urban counties, we focus on counties in metropolitan areas with populations between 100,000 and 600,000 while probing the sensitivity of our results to alternative sample definitions. The differential payments for urban counties are in effect throughout our sample period and apply to a substantial percentage of counties, as shown in Figure 2.

Our first set of empirical results demonstrate that in counties with the additional

[1] Gowrisankaran et al. (2011) consider the effect of MA plan reimbursement on the presence of drug coverage. However, they do not examine the effects on other plan characteristics. In a more recent paper, Cabral et al. (2014) consider a broader set of outcomes during the 1998 through 2003 period. We discuss this paper further below.

reimbursement (due to the urban floor), there are on average 1.8 additional insurers and that the average HHI is lower by 873. These effects are substantial, given that our non-urban control counties have an average of 5.4 insurers and an HHI of 4,308. This first set of results indicates that the more generous reimbursement induces more insurers to enter the MA market and that individuals enrolled in MA then have more plans from which to choose. We next estimate the effect of the additional reimbursement on the fraction of Medicare recipients enrolling in MA. All else equal, a higher level of reimbursement would make the marginal MA enrollee more profitable for health insurers, which would lead insurers to aim for higher enrollment. Plans might achieve this by, for example, improving the quality of their coverage or by advertising more intensively. Consistent with this, we estimate that the 10.5 percent increase in plan reimbursement in urban counties leads to a 13.1 percentage point increase in enrollment in MA plans.

One limitation with these analyses is that MA enrollment and insurer entry in urban counties might differ from non-urban counties for reasons unrelated to MA reimbursement generosity. To explore this possibility, we estimate two sets of difference-in-differences specifications. In the first, we use non-floor counties, in which FFS expenditures are relatively high. In these counties, urban status would not affect MA reimbursement. Consistent with this, we find no evidence of higher MA enrollment or greater competition in these counties. Additionally, we estimate a similar set of specifications using the period before urban counties received differential payments. As expected, we find no evidence of a significant relationship between urban status and our outcome variables of interest in this earlier period. We include both the broader set of counties and the larger time period in a set of triple-difference specifications and our primary results are unchanged.

Given this evidence of greater competition in markets with higher MA payments, we next explore the impact on plan price and quality. Here, we find much more modest effects. For example, we find that the 10.5 percent increase in reimbursement resulting from a county's urban status does not translate into significantly lower monthly premiums. Estimates that incorporate additional expected out-of-pocket costs to consumers suggest that around one-eighth of the additional reimbursement is passed through, and we can rule out pass through of more than 49 percent at the 95 percent level of confidence. These findings suggest that less than half of the additional reimbursement is passed on to consumers through lower premiums, deductibles, or co-payments. Despite evidence of limited pass-through on average, we also find substantial heterogeneity, with greater pass-through in more competitive counties.

One possible explanation for our low estimated pass-through is a difference in the composition of insurers across urban and non-urban floor counties. To investigate this issue, we split the sample between Humana, which is the largest provider of MA coverage and operates in virtually all of our markets, and all other insurers. If the additional insurers that enter in response to the enhanced reimbursement offer less generous coverage than those already operating, we would expect to find greater pass-through for Humana plans. Consistent with this, our estimates imply significant pass-through of 19 percent for Humana plans versus (an insignificant) 0.5 percent for all other plans. We also find the greatest pass-through in the most competitive counties as measured by the county-level HHI for insurer-level MA enrollment.

Of course, plans may respond to reimbursement increases by improving the quality of medical care, rather than decreasing their enrollees' financial costs. For example, plans could contract with better providers, cover additional services, or expand the breadth of their provider networks in response to the additional revenues. To investigate this possibility, we use detailed individual-level data from the Consumer Assessment of Healthcare Providers and Systems (CAHPS), which contains information on MA plan satisfaction ratings, utilization, and health outcomes for approximately 160,000 MA enrollees per year. We find no evidence of increased patient satisfaction or increased utilization of care in urban floor counties, relative to their non-urban counterparts. Similarly, we find no impact on self-reported (overall or mental) health or satisfaction with care. Finally, while selection and composition effects could partially explain our low estimated pass-through, we find no evidence of significant compositional differences between MA recipients in urban and non-urban floor counties.

Taken together, our results indicate that the increased reimbursements paid to urban floor counties substantially increase the number of enrollees in Medicare Advantage, even though plan quality is not substantially different. How could insurers increase enrollment in counties above the MSA population threshold, without making significant changes to plan quality? We present suggestive evidence that firms accomplish this by advertising more aggressively in counties with higher benchmarks.[2]

The recently enacted Affordable Care Act instituted many changes to the Medicare Advantage program, including a reduction in the generosity of MA reimbursement, with the magnitude of these reductions growing steadily over time. Our estimates indicate that the financial incidence of these cuts will fall to a significant extent on the supply side of the market. While we cannot measure the direct impact on firm profitability, we can look to stock returns as a proxy. In April 2013, following reversals of planned cuts to the MA program, the stock market valuation of major health insurers rose substantially (see Figure 3).[3] At the same time, the stock price of the largest publicly traded hospital operator (HCA) was unchanged. Insurers, rather than providers, appear to be the primary beneficiaries of MA reimbursement increases.

The paper is organized as follows. Section 2 describes the Medicare Advantage program, while Section 3 describes the data on Medicare Advantage enrollment, cost, and quality along with insurer participation; it also outlines our identification strategy. Section 4 presents our main results, and Section 5 describes the impacts of benchmarks on plan quality. Section 6 presents results on firm advertising and returns. Section 7 concludes.

[2]The increase in advertising spending, meanwhile, suggests that not all of the rents associated with market power are captured by insurers. To the extent that the market for hospital or physician services is imperfectly competitive, some of the benefits of additional reimbursement may be passed through to them as well.

[3]See Al-Issis and Miller (2013) for an examination of the effect of the Affordable Care Act on the stock prices of a broader set of firms in the health care sector.

2 The Medicare Advantage Program

First introduced in 1982 as Medicare Part C, the forerunners to contemporary Medicare Advantage plans allowed consumers to opt out of traditional fee-for-service (FFS) Medicare and into private managed care plans. The federal government hoped to achieve quality as well as cost improvements by harnessing competition between private insurers (see McGuire, Newhouse, and Sinaiko 2011, for a comprehensive history). In contrast to the FFS framework used by Medicare, private Medicare Advantage plans provide care through a managed care model. Under traditional FFS, patients have substantial freedom in selecting physicians as well as treatment options, with relatively few restrictions placed on the scope of care. Under managed care, greater restrictions exist on physician access, with consumers often limited to a plan's provider network. Furthermore, many managed care plans require special approval for specialist visits and certain procedures. They may also make efforts to coordinate patient care, which could yield efficiency improvements.

2.1 Plan Description

While all Medicare Advantage plans must cover the services that are included under traditional Medicare Parts A and B, individual plans can differ in the supplemental benefits that they provide, such as vision or prescription drug coverage. Plans can also differ in their financial characteristics, including the premium charged and consumer co-payments (which affect the level and variance of predicted out-of-pocket costs). Private insurers can enter county-level markets by offering a variety of plans, and an insurer can selectively introduce a Medicare Advantage plan to certain counties and not to others. An insurer can offer multiple plans within the same county and vary the characteristics of these plans. However, Medicare Advantage plans are guaranteed-issue, and the insurer is required to offer coverage to all interested Medicare recipients in the counties in which a given plan is active.

Plans can also differ in the specific type of managed care framework that they utilize. All Medicare Advantage plans were operated as health maintenance organizations (HMOs) through 2003. However, following the passage of the Medicare Modernization

Act, these plans could also operate as POS (point of service), PPO (preferred provider organization), or PFFS (private fee-for-service). HMO, POS, and PPO plans all rely on provider networks, while PFFS plans were not required to construct networks prior to 2011. Medicare Advantage HMO plans do not allow enrollees to see physicians or hospitals outside of their provider network, barring a medical emergency. POS enrollees, meanwhile, have the option of visiting physicians and hospitals outside of the network, but require explicit approval to do so. Under PPO plans, out-of-network physician visits would not require plan approval, but would entail greater cost sharing. Finally, as part of PFFS plans, enrollees would have the option to visit any physician, so long as that physician accepts the payment terms of the PFFS plan (cost sharing terms for the patient would remain the same across all physicians). Differences between these plan types could ultimately shape insurers' market entry decisions, in terms of the plan types offered within a county. For instance, given that PFFS plans were not required to form provider networks during our study period, the fixed costs of market entry for PFFS plans could be much lower than for other types of plans.

2.2 Plan Reimbursement

Payments to Medicare Advantage plans are based on payment benchmarks, which correspond to a given enrollee's county of residence. The benchmark payment is risk-adjusted for that enrollee's demographic and health characteristics. Originally, county-level payment benchmarks for Medicare Advantage plans were set at 95% of a county's per enrollee, risk-adjusted Medicare fee-for-service spending. The Centers for Medicare and Medicaid Services (CMS) introduced a payment floor in 1998, primarily to encourage plan entry to rural counties. As a result, government spending on MA enrollees in many counties (particularly rural ones) substantially exceeded spending on similar enrollees in Medicare FFS. In 2001, CMS introduced a second payment floor, which was set at an approximately 10.5 percent premium to the existing floor, and which applied only to urban counties. CMS defined a county as "urban" if the metropolitan area in which it is included had a population of 250,000 or more.

The relationship between a county's average per-capita fee-for-service spending and its benchmark, as of 2004, can be seen in Figure 4. As this figure shows, counties with relatively low FFS spending had benchmarks set at the payment floor. More specifically, a non-urban county with average per-capita FFS spending below \$555 per month had a floor of \$555 while an urban county with average per-capita FFS spending below \$613 had a floor of \$613. Counties with per-capita FFS spending above \$613 were in this year essentially unaffected by the payment floor while only urban counties between \$555 and \$613 are affected. As the figure shows, the magnitude of the impact of the payment floor is quite substantial for some counties. Consider an urban county with per-capita FFS spending of \$500. Its benchmark is 23 percent greater than it would be in the absence of the payment floor. The corresponding gap is considerably smaller for an urban county with per-capita FFS spending of \$600, where the floor increases the benchmark by just 2 percent.

Our analysis focuses on the 2007-2011 period, throughout which payment floors continue to be functionally (albeit not formally) present; benchmarks after 2004 were set at the highest of the previous year's benchmark (adjusted for inflation) or a county's average FFS level. As such, 2004 floor counties would have 2007-2011 benchmarks set at the inflation adjusted 2004 floor rates, so long as the inflation adjusted floor, from 2004, exceeded that county's contemporaneous FFS costs. Ultimately, over 90% of the original, 2004 floor counties remained floors in the subsequent period. The relationship between benchmarks and a county's average per-capita fee-for-service spending, for this period, can be seen in Figure 5; as expected, this relationship is largely consistent with what was observed in 2004, though it becomes somewhat less tight.[4]

In 2003, the Medicare Modernization Act introduced an additional component to the reimbursement mechanism, in the form of a bidding system. Beginning in 2006, if a firm placed a bid that was lower than the existing reimbursement benchmark, 25% of the

[4] To the extent that a county's FFS level rose above the floor level in one or more years, its benchmark would subsequently exceed the inflation-adjusted floor. This explains why some counties in 2007 have a benchmark above the linear relationships displayed in Figure 4. Similarly, counties with non-binding 2004 floors would have subsequent rates that always exceeded the corresponding, inflation adjusted floor level, irrespective of their subsequent FFS costs. After 2004, a county can go from being floor to non-floor, but cannot go from being non-floor to floor.

difference got returned to the federal government. The remaining 75% got passed back to plans, and had to fund services not covered by traditional Medicare or be passed on to consumers. In the first year of these bids, CMS estimated that 65% of these rebates went towards part A and B cost-sharing reductions, 14% towards providing non-traditional benefits (vision, etc.), 4% towards reducing part B premiums, and 16% towards part D benefits and premium reductions (CHS 2006).[5]

We focus on the 2007-2011 period for a number of reasons. First, the introduction of Medicare Part D altered the market. Second, the Medicare Modernization Act led to a shift in risk adjustment, a bidding system for Medicare Advantage, and higher reimbursements for MA plans.[6] As a result, the program grew dramatically during our sample period. Finally, we believe 2006 represents a transition period as consumers became accustomed to the prescription drug benefit; however, our results are robust to the inclusion of 2006. By focusing on 2007-2011, we can analyze a period in which MA exists in nearly every county (eliminating concerns about selection) under a stable set of policies (after the introduction of stand-alone prescription drug products but before the implementation of the ACA).

A number of papers highlight the beneficial effects of competition in Medicare Advantage, on characteristics such as premium costs (Town and Liu 2003, Lustig, 2010) and out-of-pocket payment levels (Dunn 2011). Separately, a literature has examined firm entry in this market (Chernew et al. 2005, Pizer and Frakt 2002, and Frakt, Pizer, and

[5] Song et al. (2013) explore the effect of benchmark changes on plan bids. They instrument for the county benchmark with the growth of FFS spending in other counties in the state and with the national changes in benchmarks (which in dollar terms are larger for those counties with higher baseline FFS spending). However, this identifying variation is unlikely to be exogenous, given the many factors with which initial benchmark levels & state-level FFS growth rates may be associated. One of the many outcome variables that we consider below is the plan rebate, which is three-fourths of the difference between the bid and the benchmark.

[6] These significant changes to the MA program may have affected the degree of pass-through from this earlier period. Also whereas essentially all counties have at least one MA plan in recent years, during the 1998 through 2003 period, just one-in-eight counties had an MA plan in all years. The incidence of the payment floors for this select set of counties - not all of which were floor counties - may be different from the broader set of floor counties that were ultimately affected. Similarly, while MA enrollment declined steadily during their study period (from 18 percent in 1999 to 13 percent by 2003), it has steadily increased in more recent years, from 13 percent in 2005 to 30 percent in 2014. It is possible that the average incidence of reimbursement in floor counties is different during a period of program contraction from during a period of expansion. The authors also do not have data on enrollees' ratings of their health plans, which would incorporate factors such as broader or better provider networks.

Feldman 2009), and a broad literature has considered other aspects of the program, including consumer choice (Dafny and Dranove 2008), and disparities in health care (Balsa, Cao, and McGuire 2007). A number of papers have examined the impact of MA enrollment on mortality: Gowrisankaran, Town and Barrette (2011) find no effect for plans with drug coverage and increased mortality for plans without drug coverage, which we measure. By contrast, in a later period, Afendulis, Chernew, and Kessler (2013) find evidence of reduced mortality. Our paper adds to this literature by examining the effect of policy-induced variation in plan generosity on market structure, MA plan enrollment, and on the financial and non-pecuniary generosity of MA coverage.

Our paper also adds to an expanding literature on the role of insurance market competition in shaping negotiations with providers (Ho and Lee 2013, Gowrisankaran, Nevo, and Town 2013), and premiums (Dafny 2010, Dafny, Duggan, and Ramanaryan 2012). Furthermore, our paper is similar in spirit to a number of papers that evaluate the impact of the Medicare program on private insurers and consumers (see Cabral and Mahoney 2013 and Starc 2014 on Medigap, Abaluck and Gruber, 2011, Ketcham et al. 2012, Kling et al. 2012, or Einav, Finkelstein, and Schrimpf 2013 on demand in Medicare Part D, and Clemens and Gottleib 2013 on the relationship between public and private reimbursement). Finally, Gaynor and Town (2012) provide a nice summary of competition in health care markets more broadly.

In a complementary study to the current one, Cabral et al. (2014) examine the effect of the payment floor for urban counties on plan premiums and on other measures of plan quality. In that study, the authors focus on the 1998 through 2003 period, and examine within-county changes in plan characteristics following the introduction of urban floor payments. Their findings suggest that 45 percent of the additional reimbursement passes through to consumers and that the additional reimbursement had little impact on insurer entry. However, the program is significantly different today from what it was then. For example, whereas essentially all U.S. counties were served by one or more MA plans during our more recent study period, only one in five floor counties had non-zero MA enrollment during their study period. Additionally, the introduction of the Part D program in 2006 and the move to risk adjustment and plan bidding in Medicare Advantage around the same time affected firm incentives. Furthermore and as shown in Figure 1, enrollment was substantially higher in our more recent study period, partially reflecting more generous program reimbursement. Our study complements theirs by exploring pass-through across a wider range of floor counties and during a time of MA growth. Despite these differences, Cabral et. al. estimate of a 45 percent average pass-through rate lies (just) within our confidence interval. Both studies argue that imperfect competition plays an important role in determining the effect of the program on consumers.

3 Data and Identification Strategy

We use a number of administrative datasets from CMS that contain MA plan enrollment levels, the number and type of MA plans, plans' financial generosity, survey measures of plan quality and patient utilization, government payment amounts to Medicare Advantage plans, and FFS spending levels per enrollee. We construct measures of MA enrollee composition at a plan, county, as well as year level, using CAHPS survey data and Medicare enrollment data. These data are nationwide in scope, covering more than three thousand US counties.

We initially differentiate between three types of counties - those with monthly per-capita FFS spending below \$662 in 2007, those between \$662 and \$732, and finally those above \$732. For the first group, for any given level of FFS spending, the benchmark is typically 10.5 percent higher in urban counties than in non-urban counties and is set at the payment floor. For the third group, the benchmarks are essentially the same in each of the two types of counties for any given level of FFS spending. And for the second group, the gap in benchmarks between the two types of counties declines linearly from about \$70 at per-capita FFS spending of \$662 to 0 by \$732. Urban counties in this group typically have their benchmarks set at the payment floor while non-urban counties do not.

Figure 4 shows the relationship between average fee-for-service expenditures and county benchmarks for the three types of counties as of 2004, while Figure 5 presents the comparable relationship for 2007.[7] As these figures show, the effect of being designated an urban county is largest for those with average fee-for-service spending below $662 and this effect declines steadily from that threshold to the threshold of $732, at which point the floor no longer binds for urban counties. It is worth noting that a county's floor status can change from one year to the next. More specifically, a floor county in which per-capita FFS spending grows relatively rapidly may move out of the floor category. This is of course more likely for counties close to the kinks in the schedule displayed in Figure 4. Rather than redefining the floor "treatment" each year, we use a county's 2007 FFS expenditures and its status as an urban or non-urban county in that year as our primary source of variation in the generosity of plan reimbursement below.

Table 1 provides summary statistics for all counties and then separately for each of these three types of counties. For each county, we calculate the annual average of each variable during the 2007 to 2011 period, and then take the (unweighted) average across all counties. Both the fraction of Medicare recipients enrolled in MA plans and the average HHI concentration index are comparable across the three types of counties. However, the composition of MA enrollment is quite different, with PFFS plans relatively more important in low-FFS counties. Additionally, counties with high FFS spending have greater populations on average and , as expected, substantially higher MA benchmarks. The last several rows of the table provide average financial characteristics for MA plans, including plan premiums, rebate payments, and average out-of-pocket costs. For these averages, each plan is weighted by its share of MA enrollment in the county, and each year from 2007 to 2011 receives an equal weight. However, one might be concerned that other factors correlated with urban status are biasing our estimates. We therefore consider whether other county characteristics, unrelated to MA policy, might differ across the urban threshold. Undertaking a balance test, we show in Table 2 that demographic and other county characteristics are stable around the population threshold, at the same time that MA benchmarks and MA market share differ substantially.

[7] As of 2007, a number of of counties-approximately 7%-no longer have benchmarks determined in the same manner as in 2004. The reasons for this are described in Section 2.2.

3.1 Plan Enrollment Data

We obtain Landscape files from CMS on Medicare Advantage enrollment levels for the combination of the following: county, month, insurer, and the insurance package offered by that insurer (which has the technical term contract). Our final dataset is at the county-year-insurance contract level. For any given year, we exclude contracts with 10 or fewer enrollees, as CMS does not report enrollment for these contracts. In addition, we obtain information on county-year level Medicare enrollment levels, which allows us to calculate Medicare Advantage's share of each county's Medicare population. For counties with 10 or fewer MA enrollees, this number is not reported. Given the small number of counties in our analysis sample missing this data, our empirical results below are not sensitive to whether we exclude these counties from our sample or assume that MA enrollment there is equal to 0.

Across all counties nationwide with MA enrollment exceeding ten, the average number of insurers offering an MA plan is 4.0 and the average HHI concentration index is 5,117. These market measures treat PFFS, HMO, and PPO types of Medicare Advantage similarly. In Table 3, we list the most active insurers in the MA market, based on the number of county-years in which they operate from 2007 through 2011, and also break out each firm's activity by county type as in Table 1. As Table 3 shows, Humana is by far the most active MA insurer, in terms of county-years in which it is present (comprising 87 percent of all possible markets) and in terms of the number of enrollees it covers.

3.2 Plan Characteristics Data

To measure plan financial characteristics, we draw on plan-year level data from the CMS landscape files for measures of monthly plan premiums and whether each plan provides prescription drug coverage.[8] To calculate an average for each county in each year, we weight

[8] We also obtain information from CMS on the parent companies operating each specific insurance plan, as well as the type of coverage offered (HMO/HMOPOS, PFFS, or PPO). Following the literature, we consider the plan with the lowest plan ID to be most representative of the insurance contract as a whole (Hall, 2007 and Nosal, 2012). In matching contract enrollments to individual plan characteristics, we match enrollments to the characteristics of the lowest plan ID within the contract.

each plan by its share of county-specific MA enrollment in that same year. As shown in Table 1, the average monthly MA plan premium during our study period is approximately \$31, while the average fraction of MA enrollees in a county with drug coverage is 69 percent. The table also lists an average monthly plan rebate amount. Beginning in 2006, if an insurer bid below the county benchmark for providing its coverage to an enrollee with an average risk score, it was required to devote 75 percent of the difference to improving consumer benefits in the form of a rebate of added benefits (CHS, 2006). Thus if a plan submitted a bid of \$680 per month when a county's benchmark was \$720, the monthly rebate would be equal to \$30. Plans typically allocate rebates to decreasing the Part B premium paid by consumers, towards reduced cost-sharing, or to supplementary benefits like drug coverage. When the estimated cost of supplementary benefits exceeds the rebate amount, plans can charge consumers an additional premium: there exist many plans that receive rebates, yet simultaneously charge a premium.

We obtain additional data for each plan-year on an MA recipient's total expected out-of-pocket costs as compiled by CMS. These figures are featured as part of the Medicare Compare database that is used by many Medicare recipients, and, therefore, are likely to be salient to consumers. To the extent that a plan provides drug coverage or subsidizes a portion of the Part B premium, it would be captured by this measure (though the plan-specific premium is not included in this measure). In addition to measures of overall expected out-of-pocket costs, this data includes estimated costs for individual components (such as Part B premiums, inpatient hospital costs, and prescription drugs). Further, these data break down expected out-of-pocket costs across different demographics by age as well as self-reported health status. For example, the database provides an expected out-of-pocket cost for a 65-72 year old in excellent health, enrolled in a given insurance contract. We average these estimates across demographic groups to construct a single composite metric. As with the other plan-year measures, variation across counties in this measure is driven by differences in the relative share of each plan in each county.

3.3 Plan Quality Data

For measures of plan quality, we rely on the Consumer Assessment of Healthcare Providers and Systems (CAHPS) survey data, which contains enrollees' ratings of plans, self-assessments of health status, and other measures of plan experience, such as the self-reported number of physician visits. The CAHPS survey is administered yearly, and covers every Medicare Advantage plan that is at least one year old (including HMO, PPO, as well as PFFS plans). As part of the survey, 600 individuals from each MA contract are selected for questioning (if a contract has fewer than 600 enrollees, then all of its enrollees are selected). While 600 are selected for questioning, fewer respond and the average non-response rate is approximately 25%.

Our individual-level data include responses from approximately 160,000 MA enrollees in each year from 2007 through 2011. This CAHPS data identifies the insurance contract in which each survey respondent is enrolled, along with that respondent's age, race, education, and county of residence. Additionally, the data contains the respondent's answers to each of the survey questions. The first column of Table 4 provides the average measures (on a 0-10 scale) for several quality measures including overall satisfaction with health plan and with primary care physician. As this table shows, MA enrollees are on average quite satisfied with their plans, with especially high ratings for the two physician measures.

3.4 Identification Strategy

To estimate the effect of MA reimbursement on several outcome variables of interest, we make use of the federal policy described above that induces higher reimbursement in urban (metropolitan population of 250,000 and up) than in non-urban counties. For our empirical analyses, we focus mainly on counties in metropolitan areas close to the 250,000 population threshold so as to have a more comparable set of counties with which to estimate our effects of interest. More specifically, we restrict to counties belonging to metropolitan areas with populations between 100,000 and 600,000. The population range is set larger above the threshold because the density of metropolitan area populations is somewhat thicker in the range below the threshold than above. These criteria yield a sample of 576 counties, with 304 below the population threshold and 272 above. These 576 counties are included in 280 metropolitan areas, with approximately half of the metro areas having just 1 county, 20 percent having exactly two counties, and the remaining 30 percent having between 3 and 6 counties.

As shown in Figure 4, only those MA plans in counties with relatively low fee-for-service spending would receive the full 10.5 percent reimbursement increase as a result of urban status. Figure 5 reveals that the relationship between a county's fee-for-service spending is somewhat noisier in 2007 than in 2004. This continues into subsequent years and reflects the effect of a provision that set a minimum growth rate for the benchmark from one year to the next beginning in 2004. As a result, even if a county saw a substantial decline in its average FFS expenditures from one year to the next, its benchmark would not fall. This explains why many of the data points in Figure 5 lie above the payment floors and the 45 degree line.

More than 60 percent of counties have average monthly FFS spending in 2007 less than \$662 and would therefore tend to receive the full 10.5 percent increase if they are urban. We refer to these counties as "group one" counties. Of the 576 counties with metro populations between 100,000 and 600,000, approximately 60 percent (348) are below this expenditure threshold. These 348 counties represent our primary analysis sample.

As shown in Figure 5, both urban and non-urban counties with per-capita FFS expenditures of \$662 or less in 2007 typically had benchmarks set at the urban or non-urban floor. In contrast, the payment floor did not bind in counties above \$732 in per-capita FFS spending. Urban counties between these two thresholds usually had benchmarks at the urban floor while the non-urban floor was not binding in comparable counties in metropolitan areas with a population of less than 250,000. We refer to counties with average 2007 FFS expenditures of \$662 to \$732 as group two and counties above \$732 as group three.

Our key sources of variation is the urban population threshold, which causes benchmarks to be 10.5 percent higher in urban than in non-urban floor counties. To account for the possibility that other factors vary smoothly with population, we control flexibly for both the county population and for the population of the county's metropolitan area. We also include each county's per-capita FFS expenditures. As FFS expenditures increase among floor counties, the gap between the floor and the county benchmarks declines. All else equal, this change would have the opposite effect of the policy-induced increase in benchmarks at the urban population threshold. But because many other factors - such as patient preferences and provider treatment patterns - are likely to co-vary with per-capita FFS expenditures, we do not assign a causal interpretation to our estimates for the coefficient on this covariate.

We begin by estimating the effect of urban status on the level of benchmarks and then proceed to estimate the effect on market outcomes such as the number of insurers and the HHI concentration index along with measures of plan quality such as plan premiums and enrollee satisfaction with care. While the observation level in our data is at a county-year, our identifying variation stems from each county's associated metro population, and our sample restrictions are also based on metro population. To prevent metro areas with equal populations but a greater number of constituent counties from being mechanically over-represented in our sample, we inverse weight our regressions based on the number of counties making up a given metropolitan area. We control for a county's per-capita level of FFS expenditures and flexibly for both the county and metropolitan area population when estimating specifications of the following type:

$$Y_{jt} = b_0 + b_1 * FFS_{j,2007} + b_2 * Urban_j + f(CountyPop_{j,2007}) + g(MetroPop_{j,2007}) + g_t. \quad (1)$$

In this equation, our coefficient of particular interest is b_2, which represents our estimate of the average impact of urban status on outcome variable Y_{jt}.

One concern with this equation is that there may be other factors associated with urban status - being part of a relatively large metropolitan area - that are not adequately captured by our controls for county and metropolitan area population and fee-for-service expenditures. This concern is to some extent reduced by focusing on a smaller and more comparable set of counties that are close to the population threshold. To probe the robustness of our results, we estimate additional specifications that vary the range of the population window that is included in our analysis sample and also vary our method of controlling for county and metropolitan population. In addition to these cross-sectional analyses, we perform difference-in-differences and triple difference specifications. First, we compare high FFS and low FFS counties. We control for our urban definition and a "low FFS" variable that captures the extent to which floor payments bind. This variable takes on a one if FFS costs are below the rural floor ($662/month) and a zero if FFS costs are above the urban floor ($732/month). We assign counties with FFS costs between the two floors a value between 0 and 1 that captures the linear interpolation of the two endpoints. We estimate the following equation:

$$\begin{aligned} Y_{jt} &= b_0 + b_1 * FFS_{j,2007} + b_2 * Urban_j + b_3 * Low_j + b_4 * (Urban_j * Low_j) && (2) \\ &\quad + f(CountyPop_{j,2007}) + g(MetroPop_{j,2007}) + g_t, && (3) \end{aligned}$$

where the coefficient of interest is b_4, on the interaction of the urban indicator and the low variable. Similarly, we estimate difference-in-difference specifications using the pre-2001 period as a control group. We construct the variable "Post" to take on a one after the differential floors take effect. We estimate the following equation:

$$\begin{aligned} Y_{jt} &= b_0 + b_1 * FFS_{j,2007} + b_2 * Urban_j + b_3 * Post_{jt} + b_4 * (Urban_j * Post_{jt}) && (4) \\ &\quad + f(CountyPop_{j,2007}) + g(MetroPop_{j,2007}) + g_t, && (5) \end{aligned}$$

where again the coefficient of interest is b_4, on the interaction of the two indicators. Finally, we combine these two analyses in a triple difference specification.

One final concern could be the indirect manner through which county benchmarks affect plan reimbursement; insurers submit bids for how much it would cost to provide traditional Medicare services, for an average enrollee, with the county benchmark serving as an important reference point. Insurers can bid up to the county benchmark. However, they have some incentive to bid below the benchmark, as they can then allocate 75 percent of the difference between the bid and benchmark towards additional services, which could help attract additional enrollees. In Table 5, we show that a $1 increase in the county benchmark in urban relative to non-urban counties is accompanied by a $0.91 average increase in plan bids. Given this, we argue that county benchmark increases are transmitted almost fully to insurers, even in the presence of this bidding mechanism. As such, we can abstract away from this bidding structure, for the remainder of our analyses.

Under perfect competition and constant marginal costs (perfectly elastic supply), we expect full pass-through of reimbursements to consumers.[9] However, competition may be imperfect and there may be (adverse or advantageous) selection, even conditional on risk adjustment, leading to incomplete pass-through. Our research design allows us to identify pass-through by exploiting three primary sources of variation. First, we compare urban and non-urban "floor" counties to estimate the effect of the policy-induced increase of 10.5 percent in MA county benchmarks. Second, we explore whether our estimates for urban status are similar in high FFS counties in which urban counties do not receive additional reimbursement in a set of difference-in-differences estimates. Finally, we explore the relationship between urban status and our outcome variables of interest in our analysis sample before the urban increase was introduced in 2001. These multiple approaches allow us to obtain a credible estimate of the impact of policy-induced variation in reimbursement on several outcome variables of interest in this rapidly growing area of the health care sector.

[9] Therefore, the reimbursement is optimal when the marginal consumer in Medicare Advantage places a value on the additional coverage provided at an amount equal to the shadow price of public funds. A more detailed theoretical treatment can be found in the appendix.

4 Results

4.1 The Impact on County Plan Benchmarks

To investigate the effect of urban status on county benchmarks and on other outcome variables of interest, we primarily focus on the 2007 through 2011 period. We do this because Medicare Advantage changed substantially in 2006 with the introduction of Medicare Part D (and our results are quite similar if we include 2006 as well). Additionally, the shift to full risk adjustment in 2007 and the introduction of bidding in 2006 suggest that the MA program is quite different during our study period than in the preceding years. For the reasons outlined in the preceding section, our analysis sample includes counties in metropolitan areas with populations between 100,000 and 600,000, though we test the sensitivity of our results to alternative sample definitions.

The first column of Table 6 summarizes the results of a specification for "group one" counties - those with average FFS expenditures below $662 in 2007.[10] As discussed above, the effect of urban status should be largest for these counties. The specification also controls (with a linear and quadratic term) for both the county population and the metropolitan area population along with monthly FFS expenditures. Standard errors are clustered at the metropolitan area level given the level of variation of the urban indicator. The point estimate of 68.57 for the urban coefficient is very precisely estimated and suggests an increase of more than 10 percent in the average monthly MA benchmark. None of the four coefficients on the population variables are statistically significant. The estimate for the FFS expenditure coefficient is statistically significant though the magnitude of the estimate (0.04) is small. The positive point estimate reflects the fact that counties with spending close to $662 are more likely to rise above this floor in 2008 and later years.

The next column repeats this specification though focuses on "group two" counties - those with average 2007 FFS expenditures between $662 and $732. The statistically significant point estimate of 21.81 for our key explanatory variable indicates that urban

[10] By using the 2007 floor definitions, we guarantee a balanced panel. If we used the contemporaneous payment rate to define the sample, we would lose 25 counties in 2009 and 2010.

counties in this intermediate range of per-capita FFS spending did experience an increase in their monthly benchmarks relative to their non-urban counterparts. Not surprisingly given the noisy relationship between benchmarks and FFS spending in this range displayed in Figure 5, this coefficient estimate is less precise, with a standard error that is approximately five times larger than for group one counties.

The analysis sample for the third specification in Table 6 includes counties with per-capita FFS expenditures above $732 per month. For these counties, urban status should not lead to an increase in monthly benchmarks, as payment floors do not bind for either type of county. Consistent with this, the coefficient estimate is actually negative though is even less precisely estimated than for group two counties. When we pool together group 2 and group 3 counties in the final specification, we find little evidence of an increase in monthly benchmarks resulting from urban status. Taken together, the results in this table strongly suggest that relatively low FFS counties in urban areas experience a large policy-induced increase in monthly MA benchmarks while high FFS counties do not.

The urban payment floor for MA plans, which is 10.5 percent higher than the non-urban payment floor, was introduced in 2001. To the extent that our estimates are capturing a causal effect of this policy, we should detect little relationship between urban status and monthly MA benchmarks in the preceding years. To investigate this issue, we estimate a specification analogous to the first one in Table A.1 for the 1998 through 2000 period with the same sample of group one counties. The results from this specification are summarized in the first column of Table A.1. The point estimate of -4.11 is insignificant and precisely estimated.

We next estimate this same specification using data from the 2001 through 2003 period, the period just following the increase in MA reimbursement, with the results summarized in the third column of Table A.1. The point estimate for b_2 of 24.69 is precisely estimated though is considerably smaller than the corresponding one estimated for the 2007 through 2011 period. This is primarily because CMS categorized counties somewhat differently during this period, so that many counties with an urban designation after 2003 did not have an urban designation previously. In specification 4 we account for this by adding an indicator variable with the pre-2004 definition. The point estimate for the coefficient on this second indicator variable is approximately twice as large at 49.58. Figure 6 describes the evolution of the coefficient on the urban indicator in year-specific benchmark specifications over time. Before 2001, there is no significant difference in benchmarks between urban and non-urban counties. Between 2001 and 2003, the urban indicator coefficient is significantly positive, and this approximately doubles in 2004. In that same year, both the urban and rural floors were increased, leading to a higher proportion of our sample being classified as a floor county. Furthermore, it may take time for firms to submit new bids and consumers to react to changes in reimbursement. As seen in Figure 7, we only see large effects of the urban dummy on MA enrollment in 2006 and beyond.

While we do not have enough counties near the urban threshold to employ the techniques of a standard regression discontinuity design, Figure 8 presents a graphical illustration of the monthly change in benchmarks for group one counties using a uniform kernel and the optimal bandwidth of Imbens and Kalaynaraman (2012). The figure shows a clear discontinuity in payment rates at the urban threshold.[11]

The results presented in this section demonstrate that urban counties with relatively low FFS spending had significantly higher MA benchmarks than did comparable non-urban counties. We find no similar relationship for counties with high FFS spending, in which the payment floors rarely bind. Additionally, our results using data from an earlier period (and the year-by-year estimates shown in Figure 7) reveal that this relationship did not exist before 2001 and emerged immediately after urban floors were introduced in that year. As shown in Table 2, with respect to demographic characteristics, average income, and average fee-for-service expenditures, the two sets of counties are quite similar. In the subsequent sections, we explore how this policy-induced variation in the generosity of plan reimbursement affects market outcomes and the quality of MA coverage.

[11] The specification in these figures is slightly different and more flexible than any in the tables, yet the pattern is similar.

4.2 Market Structure and MA Enrollment

We next explore the effect of the policy-induced increase in MA plan reimbursement on two measures of market structure - the number of insurers and the HHI concentration index. As our model above suggests, increases in the generosity of reimbursement may cause additional firms to enter the MA market and incumbent firms to increase the quality of their product in response. Here, we consider counties in the first group described above, with FFS expenditures per enrollee below $662 in 2007. For this group of counties, the average number of insurers offering an MA plan during the 2007 through 2011 period was 6.5 and the average HHI concentration index 3,907 (measured on a 10,000 scale). We once again control for both county population and metropolitan area population (with both a linear and quadratic term) and for average per-capita FFS expenditures in 2007.

The first specification summarized in the first panel of Table 7 considers the effect of urban status on the number of insurers. The point estimate of 1.78 for the urban indicator variable represents more than 25 percent of a county's mean number of insurers for our analysis sample. This estimate is highly significant with a t-statistic of 3.8. The significantly negative point estimate of -.69 for the per-capita FFS expenditures variable suggests that fewer insurers enter as a county's fee-for-service expenditures gets closer to the $662 monthly FFS spending upper bound for group one counties. This makes sense as the gap between the plan reimbursement and FFS expenditures is declining in that measure (as shown in Figure 4), though we emphasize that other factors may vary with per-capita FFS expenditures and thus stop short of a causal interpretation for this estimate.

The second specification yields a similar picture by considering the effect of urban status on the HHI concentration index. Urban counties in metropolitan areas with a population of 250,000 or more are significantly less concentrated, with the point estimate of -873 representing almost one-fourth the mean HHI in our analysis sample. The HHI increases as FFS spending rises and thus the gap between this and the payment floor declines. As expected, the point estimates in column 2 have the opposite sign to those for the previous specification given that here a larger number represents fewer insurers operating. Our HHI measures are the least robust across specifications. This is not surprising, as HHI is a highly non-linear measure and the effect of additional entrants is not necessarily large. In a companion set of results not summarized here, we find that the percentage of plans sold by the three largest insurers in a market is not significantly different in urban counties. Therefore, our results suggest that higher reimbursement leads additional fringe insurers to enter, but not to capture large market shares.

Columns 1 and 2 in the first panel of Table 7 suggest that the additional reimbursement available to plans in counties with the urban designation leads to more entry and a reduction in concentration. The specifications summarized in the next three columns investigate whether and to what extent the additional reimbursement leads to more MA enrollment. The third column shows that the fraction of Medicare recipients enrolled in Medicare Advantage HMO or PPO plans increases by 7.1 percentage points as a result of the greater reimbursement, while column 4 shows a corresponding increase of 5.9 percentage points in the share enrolled in MA private fee-for-service plans. Both estimates are substantial relative to the sample means for our analysis sample. Figure 9 presents a graphical illustration of the effect of the urban threshold on MA penetration. MA penetration averages 10 percent immediately to the left of the threshold and 20 percent immediately to the right, providing additional evidence of a causal effect.

In subsequent panels of Table 7, we test the robustness of these results in a number of ways. The second and third panels use narrower population windows when constructing the analysis sample. The advantage of the wide range used in the preceding specifications (100,000-600,000) is that roughly one-fourth of Medicare eligibles are in metropolitan areas in this population range. The disadvantage is that by including such a broad population range, we may introduce bias. The specifications summarized in the second panel include only counties in metropolitan areas with populations from 150,000 to 350,000. All of our results are qualitatively similar (though the estimate in the HHI specification is no longer statistically significant) and suggest that the policy-induced increase in reimbursement leads to substantially more entry and an increase in MA enrollment in urban counties. Table A.2

shows that our results are also robust to alternative methods of controlling for population and the inclusion of the race and gender controls described in Table 2 (as suggested by Altonji et al. 2005). Finally, we present coefficients on the population variables in Table A.3.

We also present difference-in-differences and triple difference specifications. First, we compare floor counties in our sample to non-floor counties in the same population range. Because urban status does not lead to additional reimbursement in non-floor counties, this allows us to account for the possibility that market outcomes are different for other reasons in urban counties. The estimates for the coefficient on the interaction between urban status and being a floor county are displayed in the fourth panel of Table 7 (with the full results reported in Table A.4). The coefficient estimate of 1.53 in column 1 reveals that there is a significantly larger difference in the number of insurers between urban and non-urban floor counties than between the corresponding non-floor counties. Similarly, urban floor counties have an average HHI that is lower by 1087 points and MA penetration that is higher by 4.7%. These results strongly support the estimates that use only floor counties and all of the estimates are statistically significant.

Next, we investigate the pre-2001 period for our analysis sample. In this earlier time period, there were no differential floors by urban status. By comparing the results for this period to those for our study period, we can control for time-invariant features of urban floor relative to non-urban floor counties that may be driving our results. Our results are largely consistent with Table 7, with the exception of HHI. This is largely a compositional issue, as 67% of floor counties had no MA insurers pre-2001. If we replace these missing values with a monopoly-level HHI, we obtain a negative (though not statistically significant) coefficient.

Finally, we implement triple difference specifications that utilize both the non-floor counties and the earlier time period. The results from these specifications are summarized in Appendix Table A.5. The results are also consistent with Table 7 with the exception of HHI. If we replaced missing HHI values with the monopoly values, the results would be large, but not statistically significant (a reduction of 1280 points). Taken together, these results provide additional evidence that the differences in market structure and MA enrollment between urban and non-urban floor counties are due to differential reimbursement rather than other unobserved factors.

4.3 Financial Characteristics of Plans

We next consider how the financial generosity of MA coverage varies with the additional policy-induced reimbursement. As discussed above, insurers may respond to the higher benchmarks in urban counties and to the resulting increase in competition by reducing their premiums or out-of-pocket costs or by offering additional services. To test this possibility, we begin by exploring the relationship between urban status and the monthly MA plan premium, which has an average value of approximately $30 in our analysis sample. This premium data is available at the county-plan-year level, and our county-year measures are enrollment weighted-averages. As shown in the first column of the first panel of Table 8, the point estimate for the urban indicator is very small in magnitude (-0.88) and statistically insignificant. This suggests that despite the substantially higher benchmarks in urban counties, MA enrollees do not benefit in the form of much lower premiums.

In the second column we consider the effect on the amounts that insurers allocate toward supplemental Medicare services through the rebates they are provided by CMS (if and when their bids fall below the benchmarks). We only have rebate data for 2007 through 2010, and so our analysis sample is 20 percent smaller as a result, and the average value of this variable for our analysis sample is $55 per month. Consistent with our estimate for the premium measure, our results provide little evidence to suggest that significantly greater plan reimbursement leads to substantial additional benefits to enrollees. The point estimate of 3.38 for the benchmark represents about 5 percent of the additional reimbursement and we can rule out an increase in the rebate of more than $12 at the 95 percent level of confidence.

In the third column, we investigate the effect on out-of-pocket costs (OOPC). To the extent that an insurer responds to the additional reimbursement by, for example, reducing deductibles or offering supplemental services such as vision coverage, it would be reflected in this measure. This measure weights by MA enrollment, and the average OOPC in our analysis sample is approximately $365 per month. The point estimate of -7.02 for the urban coefficient is statistically insignificant. With this point estimate, we can rule out an out-of-pocket cost reduction of more than $24 per month at the 95 percent level of confidence.[12]

In the fourth column, the outcome variable is a measure of total expected costs for the enrollee, based on the sum of premiums and out-of-pocket costs indicators and with rebates subtracted out (given that higher values represent more generous coverage). The statistically insignificant point estimate of -8.30 suggests only about one-eighth of the additional reimbursement is passed on to consumers and we can rule out a benefit of more than $34 (49 percent of the benchmark effect) at the 95 percent level of confidence.

In the fifth column, we consider the provision of drug coverage and - consistent with the previous measures - find little evidence that this benefit is more likely to be offered by plans in urban counties, as the point estimate on the urban indicator is actually negative. And as with the OOPC variable, Part D coverage seems if anything to be less generous in counties with lower FFS reimbursement, where more insurers enter. This could once again reflect marginal entrants being less generous than incumbent firms on this dimension.

For all five of the outcome variables considered here, we weight by each plan's share of county-specific MA enrollment in the year. If MA recipients in urban counties were, for example, less likely to choose low-premium plans or plans with generous cost sharing, our estimates could provide a misleading estimate of average plan quality. To investigate this possibility, we estimate a companion set of specifications in which we weight each plan in a county-year with non-zero MA enrollment equally. As shown in Table A.6, our point estimates for the urban indicator are qualitatively quite similar and also suggest limited

[12] The statistically significant negative estimate for the FFS variable in the OOPC specification suggests that, as the wedge between the floor and FFS spending grows, plans become less generous. However, as we emphasize above, other factors likely vary with FFS expenditures, and thus we do not assign a causal interpretation to this estimate.

pass-through.[13]

We probe the robustness of these results in a number of ways. For example, in the next two panels we investigate whether the results are similar for narrower population ranges than in our main analysis sample. While the point estimates become less precise, they remain small in magnitude. For example, the insignificant point estimate of 5.80 for the sum of plan premiums and other OOPC and with rebates subtracted out in the fourth column actually suggests plans in urban counties offer somewhat less generous coverage. Our estimates in the fourth and fifth panels, which represent difference-in-differences specifications for high FFS spending counties and in the pre-period, respectively, further support our findings, though we do not have data on all of the outcome variables of interest in the pre-2001 period. While our earlier results provide evidence of a significant effect of MA reimbursement on MA penetration and market structure, these results suggest that more generous reimbursement has little impact on the financial features of MA plans.

4.4 Heterogeneity

We can also explore heterogeneity in the effect of reimbursement across insurers and markets. First, we restrict our analysis to Humana, the market leader. Humana operates in 87% of markets and 86% of floor markets, nearly twice the number of the next largest insurer, UnitedHealth. Humana also captures 18% of the national MA market. The results in Table 9 show that Humana plans are more generous in urban floor counties than non-urban floor counties. The sum of premiums and OOPC are $14 lower in urban floor counties. This indicates that more of the benchmark increase is being passed through to Humana consumers.

What drives the difference between these estimates and those in Table 8? Increased benchmarks may be attracting marginal insurers who are not as efficient as incumbents or

[13] Interestingly, the point estimates for the coefficient on FFS expenditures in the OOPC specifications change sign in these unweighted specifications, and now indicate that as the wedge between the floor and FFS spending declines, plans become less generous. This suggests that MA recipients in areas with higher FFS spending are more likely to choose plans with generous cost-sharing. As with the other point estimates for this FFS variable, we do not assign a causal interpretation.

must incur fixed costs of entry. These new insurers attract consumers who prefer their plans due to differential networks, idiosyncratic errors, or behavioral biases (Stockley et al. 2014). While these consumers are made better off by the increased reimbursement, the plans chosen are not necessarily better in purely financial terms. Therefore, we are cautious about drawing conclusions about welfare from our results.

These results also suggest that the effects of benchmarks may be both heterogeneous and non-linear. Therefore, we also replicate our analysis across more and less competitive markets. Table A.10 presents the results of this analysis. We find nearly full pass-through in the most competitive quintile of markets, but limited effect of benchmark generosity outside of this subset, consistent with findings in Cabral et al. (2014). These specifications support our basic results and provide additional evidence on mechanisms and heterogeneity.

5 Plan Quality

5.1 Quality Characteristics

Higher MA reimbursements could also be passed on to consumers in the form of quality improvements. To identify possible changes to the quality of health care coverage (as distinct from the financial measures considered above), we use respondent-level survey data from the federal government's Consumer Assessment of Healthcare Providers and Systems for the 2007 through 2011 period. These data contain information on respondents' counties of residence, allowing us to examine the relationship between county-level reimbursement variation and the measures included in the CAHPS. We have nearly 82,000 person-year level observations for the counties in our analysis sample during our study period.

We examine the impact of additional plan reimbursement on respondents' overall ratings of plan quality along different dimensions: health care received, the primary care provider, specialists seen, and the plan overall. We run our results on data aggregated to a county-year level, while restricting to counties in the 100,000 to 600,000 metro population range, with 2007 FFS values below the $662 monthly amount described above. The main results are displayed in Table 10. We find no significant relationship between a county's urban status and each of these rating measures, with the exception of ratings for primary care physicians. Using the approach introduced in Kling et. al. (2007), we calculate standardized treatment effects, to examine whether urban status has an impact on these ratings measures, as a collective. These results also indicate no significant relationships between higher MA benchmarks and plan ratings.

We further examine the effect on plan quality by looking to plan-level quality measures ("star ratings") compiled by CMS, relating to health outcomes, chronic care management, customer service, and the plan overall. These results, displayed in Table A.7, also show no significant relationship between a county's urban status and various metrics of plan quality. We can rule out a percentage increase in consumer's rating of "Overall Health Plan" of greater than 3.1 percent at a 95 percent level of confidence.

We also consider the impact on measures of utilization and outcomes contained in the CAHPS, such as number of specialist visits, number of personal MD visits, and self-reported health status. To the extent that additional reimbursement leads plans to expand access to care or to improve enrollee health more, it would potentially be captured by these estimates. These results, which are presented in Table 11, provide no evidence of a significant relationship between urban status and utilization or health outcomes across the counties in our analysis sample. These results - and those presented in Table 10 - are robust to sample definition as shown in Tables A.8 and A.9.

It is worth noting that these results on quality and intensity of care could be biased if the increase in MA enrollment that we find in urban counties leads to a significant change in the composition of enrollees. If, for example, MA plans in urban counties had patients who were substantially sicker on average, that might bias our estimate of the effects of additional reimbursement on enrollees satisfaction and other measures of plan quality. This motivates our analyses in the next section.

5.2 Compositional Effects

We do not find evidence of increased reimbursements being passed through to consumers to a significant extent in the form of lower cost-sharing or increased quality. However, our results could be biased by reimbursement-driven changes to enrollee composition within Medicare Advantage. As we showed in Table 7, the 10.5 percent increase in benchmarks for floor counties resulting from urban status leads to a substantial increase in MA enrollment. As such, we test for possible compositional changes to MA enrollment, which could accompany these increases to enrollment.

Using data from the CAHPS, we once again restrict to counties in the 100,000 to 600,000 metro population range, and with average per-capita FFS expenditures in 2007 of less than \$662. We then compile demographic and health metrics for enrollees in urban and non-urban counties, respectively. As shown in Table 12, we do not find substantial differences in age, gender, or race across enrollees in these counties. This does not definitively rule out the possibility of unobserved differences between the marginal and average MA enrollee. But we find very little evidence that compositional differences could be driving the very large difference between our results for a substantial effect on market structure and very little effect on the measured quality of MA coverage. In Table A.7, we consider additional metrics - the average risk score of MA enrollees (which is increasing with the number of conditions that a Medicare recipient has) and the average costs of those remaining in FFS - to test for possible reimbursement-driven changes to enrollee composition. These results also do not provide any evidence of significant changes in enrollee composition as a result of increased reimbursement. Finally, Table 5 presents the results of a regression with bids as a fraction of the benchmark, rather than prices or measures of quality, as the outcome of interest. There is no significant difference between urban and non-urban counties on this measure, indicating that bids average 90 percent of benchmarks in both types of floor counties. This suggests that firms are attuned to benchmarks, especially in floor counties, and bid to maximize payments.

6 Advertising and Firm Returns

Our empirical results show that larger subsidies to Medicare Advantage plans lead to significantly more insurers operating and to less concentrated insurance markets. Furthermore, more generous subsidies lead to higher Medicare Advantage penetration rates. One might assume that the higher subsidies are passed on to consumers in the form of lower premiums, out-of-pocket costs, or higher quality. Our empirical results do not support this conclusion. While higher margins seem to stimulate competition, competition has a limited effect on the price and quality of MA plans.

Firms in this market may compete on advertising, rather than price or quality. Numerous studies suggest that both framing and advertising can substantially impact consumers making complicated financial decisions. There is substantial evidence that seniors have a hard time choosing cost minimizing Medicare Part D plans (Abaluck and Gruber 2011). Furthermore, informational interventions that inform consumers that lower cost plans are available can have a substantial impact (Kling et al. 2012). Search frictions, comparison frictions, and behavioral biases can impact the health insurance purchase decision. The complexity of reimbursement in this market may lead firms to advertise zero premium plans, rather than focusing on the overall expected medical costs, including rebates, and the variation in those costs. Furthermore, advertising may help firms select favorable risks (Aizawa and Kim 2014).

Advertising competition is an important feature of the market for a wide range of complex financial products. Hastings et al. (2013) find that exposure to sales personnel in the market for investment funds decreases price sensitivity and increases brand loyalty. Taken together, these effects increase fees paid by consumers. Guren et al. (2013) show that mortgage lenders who advertise more tend to sell more expensive mortgages, target unsophisticated borrowers, and advertise teaser, rather than reset rates. These studies are consistent with a theoretical literature highlighting the impact of complex pricing rules (primarily add-on pricing, but similar logic could be applied to cost sharing or interest rates). Complex pricing rules can arise from incentives to price discriminate (Ellison 2005) or behavioral biases such as myopia (Laibson and Gabaix 2006).

To explore the impact of reimbursements on firm advertising, we utilize data from Kantar Media. Kantar Ad$pender contains advertising data at the media-product-year-designated market area (DMA) level. Because DMAs are bigger than counties, we need to aggregate our reimbursement data. We create variables that represent the percentage of Medicare beneficiaries in a DMA that live in an urban, urban floor, and floor county. We examine the impact of these variables on TV spot advertising spending per Medicare beneficiary in a DMA. We define this measure in two ways. In the first, we restrict our analysis to products with "Medicare" in their name. This includes Medicare Advantage plans, but also Part D and Medicare supplement plans as well. Furthermore, not all carriers report a specific Medicare line. The Kantar data does not allow us to distinguish between these products and data may be an overestimate or an underestimate of the amount of advertising for MA plans. However, we have no reason to believe that advertising for Medicare supplement or Part D plans would vary with floor status. Average spending per Medicare enrollee is $5.90 per year. In the second definition and panel of Table 13, we take Kantar definition of "health insurance" as given, noting that not all Medicare products are denoted by name. While we would prefer to restrict to only Medicare Advantage products within health insurance, the products are not coded finely enough in the data. However, Medicare products comprise the bulk of individual insurance plans sold (and, presumably, targeted advertising) within all DMAs.The dependent variable is skewed, with only about half of DMAs having advertising, but total spending in the 90th percentile of DMAs is $2.2 million per year.

In Panel A of Table 13, we summarize the results from specifications of the following type:

$$Y_{jt} = b_1 + b_2 * \%\, Urban_j + b_3 * \%\, Urban\ Floor_j + b_4 * \%\, Floor_j + d * FFS_j + g(MetroPop_{j,2007}) + \gamma * X_{jt} + \epsilon_{jt}.$$

In all specifications, we include year fixed effects as well as a spline that controls for the DMA-year population. There are 210 DMAs and we observe four years of advertising data (2007 through 2010), giving us 840 total observations, and we therefore cluster our standard errors at the DMA level. If more generous MA reimbursements in urban floor counties leads to increases in advertising, we would expect a positive estimate for b_3. It is important to note that, due to the level of aggregation in the advertising data, we are unable to restrict attention to the counties in metropolitan areas with populations between 100 and 600 thousand as we did in the preceding sections. Instead, the analysis sample in these specifications includes essentially all geographic areas in the U.S., which could make it more difficult to disentangle the effect of MA reimbursement from other factors.

The first specification summarized in Panel A indicates that urban floor counties have significantly higher advertising for Medicare products. The estimate of \$6.35 is substantial, as it slightly exceeds the mean of our dependent variable, though its precision is limited with a standard error of \$2.23. This is not surprising given that we have just 210 DMAs and the dependent variable is highly skewed. The corresponding estimate in Panel B, which uses the broader health insurance measure as the dependent variable, is also large in magnitude and statistically significant. Both estimates are robust to the inclusion of ad prices (specification 2) and per-capita FFS expenditures (specification 3) in the DMA-year.

One concern with this first set of estimates is that urban floor counties may attract more advertising for reasons unrelated to MA reimbursement generosity. To address this concern, in the fourth specification we add a control for per-capita credit card advertising in the DMA-year. This variable should not be affected by the generosity of MA reimbursement though should control for unobserved factors that influence the intensity of advertising in an area. While this variable is significantly positively related with both of our dependent variables, it has little impact on our coefficient estimates of interest.

In the fifth and final specification, we introduce controls for the share of a county residing in an urban county and in a floor county. This reduces both the magnitude and the precision of our key coefficient estimate in Panel A. However, it has essentially no impact on the estimate that uses the broader measure of health insurance as our advertising measure, which remains statistically significant and economically large.

Taken together, the results in this section suggest that the more generous reimbursement given to MA plans in urban floor counties leads to substantially more advertising. We believe these results can rationalize much of the increase in firm entry and MA enrollment in urban floor counties. While the precision of our estimates is limited due to the level of aggregation in the advertising data, it provides some insight as to why pass-through of MA reimbursement may be limited, and suggests that increased benchmarks need not accrue to insurers. Additionally, our findings are consistent with much previous literature regarding the importance of advertising in the market for complex financial products.

Despite dissipation of some rents through marketing costs, it is plausible that insurers also capture part of the increased benchmarks. Figure 3 shows dramatic increases in stock prices for the four publicly traded health insurers with the most MA enrollment (Humana, United, Cigna, and Aetna) as a result of a surprisingly large increase in benchmarks on April 1, 2013. Interestingly, it is Humana, the most active insurer in the Medicare Advantage market from Table 3, with the biggest increase. A simple pre-post comparison of market capitalization for these four firms, which accounted for about 44 percent of MA enrollment at the time of the policy change, indicates a market capitalization increase of approximately \$2.7 billion. The announced benchmarks represented an increase of approximately 5.6 percent relative to what otherwise was specified by legislation. Multiplying this percentage by our estimate of baseline MA revenues for each insurer (calculated by multiplying enrollment weighted benchmarks for each insurer by the average risk score of its enrollees) yields an estimated increase in annual MA revenue of about \$2.9 billion.

It is important to note that investors apparently expected a significant increase in benchmarks around this time. For example, according to Humana's press release, the firm had expected a 4.4 percent increase in benchmarks instead of 5.6 percent. If one assumes that this also accurately captures the assumptions of investors, this would suggest that just \$0.62 billion of the \$2.9 billion increase in annual MA revenues was a surprise. Using a discount rate of 5 percent, this implies an increase in the present value of MA revenues of approximately \$12.4 billion. Combining our estimate of a \$2.7 billion increase in market capitalization with the $12.4 billion increase in the present value of MA revenues, we estimate that 22 percent of the increase in benchmarks is passed through to insurers in the form of higher profits. Of course, the precision of this estimate is necessarily more speculative than our estimates relating to consumers. But the sharp stock market reaction to changes in the level of MA reimbursement strongly suggests that insurers capture much of the benefit of policy-induced increases in plan reimbursement.[14]

Our estimates and back of the envelope calculations indicate that at most 45 percent of the increased reimbursement goes to consumers and approximately 22 percent goes to insurers. Our advertising results suggest that some of the increased expenditure is dissipated through marketing costs. Theory suggests that hospitals, physicians, and other health care providers could also capture some of the increased reimbursements, by virtue of market power.[15] We believe that a combination of increased financial generosity, increased insurer profits, and increased marketing account for nearly all of the increased government expenditures.

7 Conclusion

Our results strongly suggest that increased subsidies for private insurance in the Medicare Advantage market result in increased insurer advertising, but little additional monetary or medical benefit for consumers.[16] Low pass-through cannot be attributed to selection and

[14] The benchmark increase of 5.6% applied not only to 2014 benchmarks, but also to all future year benchmarks; for 2014, this resulted in a benchmark that was 1.2% higher than the expectation. In our calculations, we thereby assume that all future year benchmarks would also be 1.2% higher than expected. However, for some of these years, higher benchmarks may have already been anticipated; congressional action on Medicare SGR policies would produce a benchmark increase of commensurate magnitude and would supercede CMS's action. While CMS preempted such legislation through its unilateral action, following any Congressional legislation, past CMS action (or lack thereof) would not affect subsequent benchmarks. In our calculations, we do not account for this possibility. As such, our estimate of the unexpected revenue increase, from CMS's action, represents an upper-bound, meaning that our estimated pass-through rate to insurers represents a lower-bound.

[15] However, the aforementioned calculations leave relatively little for providers. The absence of stock price reaction from the largest publicly-owned hospital operator, HCA, on April 1, 2013, is also suggestive of limited benefits to providers.

[16] The advertising is clearly market expanding if Medicare Advantage is the relevant market. However, the extent to which this is welfare enhancing depends on the view of advertising. We simply highlight that insurers in this market, as well as other insurance markets (Starc, 2014), tend to compete on advertising, rather than plan generosity or innovative benefit packages.

is, more likely, a result of market power. Altogether, the results indicate that incidence of the subsidy falls primarily on the supply side of the market. This finding is further supported by insurer stock price movements throughout the passage and implementation of the Affordable Care Act.

While our results indicate that insurers capture much of the increase in reimbursements (similar to Curto et al. 2015), we are hesitant to draw conclusions about welfare. For example, MA plans may be more efficient than traditional Medicare by reducing low-value care or improving health status. This would imply that increased reimbursements induce consumers to switch to more cost-effective plans, even if the primary mechanism is increased advertising. Furthermore, while we find no direct evidence that benchmarks meaningfully benefit consumers, such benefits could exist. Additional choice, due to insurer entry, could lead to meaningful gains in consumer welfare through better matching. Given that MA penetration rates increase alongside reimbursements, a revealed preference argument would imply that MA is more valuable to consumers when the benchmark is higher. The impact on consumer surplus may also depend on the welfare consequences of advertising. Furthermore, higher benchmarks may improve treatment quality and health outcomes in ways that we are unable to measure. Finally, our analysis focuses on low FFS counties and may not be applicable to the one-third of counties with FFS spending significantly higher than the floor thresholds. All of this notwithstanding, the measures of plan financial characteristics and quality that we use suggest that only about one-eighth of the policy-induced increase in plan reimbursement is captured by consumers.

While reimbursement increases have an ambiguous welfare impact on consumers, they unambiguously increase costs, through increased numbers of MA enrollees and through increased government spending per MA enrollee. A back-of-the-envelope estimate suggests that this additional spending amounted to approximately $6.7 billion during the final year of our sample period.[17] Therefore, given the deadweight loss associated with taxation, policy-

[17] Approximately 5.0 million MA enrollees resided in floor counties in 2011. In non-floor counties, the benchmark is on average 6.1 percent higher than the lagged 5-year average FFS expenditure measure. If this same 6.1 percent ratio existed in floor counties, monthly (annual) benchmarks would be $63.09 ($757.08) lower and spending for the 5.0 million MA enrollees would be $3.8 billion lower. Additionally, our estimates

makers should carefully weigh the possible gains in consumer welfare against the costs to the federal government. Future work should attempt to quantify the full welfare benefit of increased reimbursements and quantify the costs and benefits of alternative policies, including vouchers that allow Medicare beneficiaries to actively opt into traditional Medicare or private plans.

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for the effect of benchmarks on MA enrollment suggest the benchmark increase leads to about a 13 percentage point increase in MA enrollment. With 20.1 million Medicare recipients in floor counties, this represents about 2.6 million additional MA recipients. Recent research (Brown et al., forthcoming) indicates that switching into MA increases Medicare spending by more than $1,200 per recipient because of favorable selection and this suggests about $2.9 billion more in Medicare spending.

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9 Tables and Figures

Table 1: Summary Statistics: County & Financial

	All	*Sub-Group 1*	*Sub-Group 2*	*Sub-Group 3*
Per Cap 2007 FFS Rest:	**None**	***<662***	***≥662 & <732***	***≥732***
Metro Pop (thousands)	472	239	616	1,109
	(1,209)	(661)	(1,103)	(2,180)
County Pop (thousands)	96	50	104	246
	(306)	(105)	(243)	(631)
Monthly Per Cap. FFS	652.2	590.4	702.2	805.6
	(99.6)	(57.1)	(27.8)	(68.8)
Medicare Enroll (thousands)	14.79	8.33	15.98	35.98
	(39.66)	(14.93)	(33.28)	(79.47)
MA Penetration Rate	0.12	0.12	0.11	0.14
	(0.10)	(0.10)	(0.09)	(0.13)
PFFS Penetration Rate	0.05	0.06	0.04	0.02
	(0.04)	(0.05)	(0.03)	(0.03)
HHI Index	5,117	5,168	5,070	5,002
	(2,212)	(2,244)	(2,168)	(2,155)
Floor Status (2007)	0.62	0.92	0.27	0.00
	(0.49)	(0.26)	(0.44)	(0.00)
MA Benchmark (Monthly)	733.0	696.5	740.3	852.2
	(72.1)	(30.8)	(30.8)	(80.9)
N	3044	1850	667	527
Out of Pocket Costs (monthly)	365.3	376.5	355.1	338.5
	(37.3)	(31.4)	(31.7)	(44.9)
Rebate Payment (monthly)	56.6	52.2	57.6	71.1
	(21.8)	(15.4)	(17.2)	(35.8)
Premium (monthly)	30.8	29.7	31.8	33.1
	(19.0)	(17.7)	(18.4)	(23.5)
Premium+OOPC	396.2	406.5	386.9	371.6
	(41.8)	(34.1)	(36.5)	(56.9)
Premium+OOPC-Rebate Pmt	345.6	361.2	333.9	304.9
	(59.1)	(43.4)	(49.8)	(88.7)
Percent Offering Drug Cov	68.9	65.1	73.4	76.6
	(23.5)	(23.8)	(21.5)	(22.2)
N	3028	1840	666	522

Notes: The first panel presents summaries of demographic, MA penetration, and other characteristics for different sets of counties. The second panel presents summaries of the financial characteristics of MA plans, across different sets of counties. Measures are denoted per enrollee, per month. These measures cover the 2007-2011 period, and are at a county level. All financial measures are inflation-adjusted, and represented in 2007 dollars. The source data, which is at a plan level, is first aggregated to the county-year level; weighting is done based on plan enrollment levels. The county-year data is then aggregated to a county-level, with each year weighed equally; thus the final observation level is at a county level. The original data is obtained from publicly available CMS files, including simulated out of pocket cost information, premium metrics, as well as other data.

Table 2: Covariate Balance for Full Analytic Sample

	Full Sample		**Boundary Analysis**			
	Mean	*Std. Dev.*	*Low Side Mean*	*High Side Mean*	*Difference in Means*	*Test of Difference*
County Bnchmk	747	46	714	784	70	51.06
Market Structure						
No of Insurers	6.17	2.64	5.41	7.02	1.61	4.95
HHI Index	3,970	1,634	4,308	3,592	-716	-3.16
MA Share	0.167	0.104	0.128	0.211	0.083	4.93
PFFS Share	0.074	0.049	0.063	0.087	0.024	3.14
PPO Share	0.039	0.040	0.029	0.051	0.022	3.4
HMO Share	0.053	0.080	0.036	0.072	0.036	2.59
Financial Chars						
Drug Cov	0.66	0.20	0.63	0.69	0.06	1.74
Rebate Pmt	55.7	13.1	53.2	58.5	5.3	2.88
Prem	29.0	17.6	30.3	27.5	-2.9	-0.98
OOPC	367.4	27.5	371.6	362.8	-8.8	-1.97
OOPC+ Prem	396.4	31.3	401.9	390.2	-11.7	-2.40
OOPC+Prem-Rebate	348.0	40.3	355.2	340.1	-15.1	-2.56
County Chars						
5 yr FFS	591.7	47.0	590.4	593.2	2.8	0.40
Medicare Enroll	15,666	14,920	12,877	18,795	5,918	3.87
County Pop	97,827	97,344	80,178	117,627	37,449	3.79
Metro Pop	273,234	139,752	160,358	399,873	239,515	16.62
Percent White	88.22	14.15	88.45	87.95	-0.50	-0.23
Percent Black	7.98	13.16	7.13	8.92	1.79	0.83
Percent Hispanic	5.99	10.45	7.07	4.78	-2.29	-1.89
Percent Female (Among 65+)	0.57	0.02	0.57	0.57	0.00	0.81
Personal Income	28,415	4,681	27,935	28,921	986	1.66
Number of Counties	*348*		*184*	*164*		

Notes: Table presents a test for covariate balance between urban and non-urban counties in our sample. The unit of observation is at the county-level, and is aggregated across the 2007-2011 period. All financial measures are inflation-adjusted, and represented in 2007 dollars. We restrict to counties in the baseline analytic sample; this limits to counties in 100-600k metro population range, and with 2007 FFS levels below the lowest floor value. Counties classified as 'High Side' are those in metro areas with populations of 250-600k, while those classified as 'Low Side' have population of 100-250k. The original data is obtained from publicly available CMS files, including enrollment and other data. The original data is aggregated first to a county-year level, while weighing by plan enrollment; subsequently, it is aggregated to a county-level, while weighing all years equally.

Table 3: Most Active Firms in Markets of Interest

	All	*Sub-Group 1*	*Sub-Group 2*	*Sub-Group 3*
Per Cap. 2007 FFS:	**None**	***Blw 662***	***Above 662 & Blw 732***	***Above 732***
Humana Inc.	12,998	8,094	2,840	2,064
UnitedHealth Group, Inc.	7,146	4,444	1,407	1,295
Universal American Corp.	5,844	3,511	1,356	977
Coventry Health Care Inc.	5,463	3,427	1,121	915
WellPoint, Inc.	5,100	3,303	1,082	715
Aetna Inc.	4,042	1,826	1,077	1,139
XLHealth Corporation	2,099	974	677	448
WellCare Hlth Plans, Inc.	1,910	980	410	520
BCBS of Michigan	1,466	620	425	421
	15,020	**9,430**	**3,160**	**2,430**

Notes: Table presents number of county-year units through which any given firm offers contracts, where enrollment exceeds 10. This analysis extends for the period 2007-2011. The original data is obtained from publicly available CMS files, including contract-county level enrollment data and contract characteristics data.

Table 4: Summary Statistics: CAHPS Data

	All	100-600k	**100-600k, FFS Blw Floors**	
			Urban	*Non-Urban*
Overall Healthcare Received	8.45	8.49	8.52	8.48
	(0.71)	(0.49)	(0.38)	(0.60)
Primary Care Physician	9.00	9.02	9.03	9.03
	(0.58)	(0.37)	(0.31)	(0.45)
Specialist Physicians Seen	8.85	8.90	8.92	8.89
	(0.79)	(0.50)	(0.32)	(0.69)
Overall Health Plan	8.30	8.34	8.42	8.30
	(0.80)	(0.58)	(0.43)	(0.70)
Prescription Drug Benefits	8.32	8.33	8.40	8.27
	(0.86)	(0.57)	(0.38)	(0.69)
Specialists Seen	1.66	1.70	1.67	1.64
	(0.44)	(0.31)	(0.24)	(0.35)
Visits to Personal MD	2.01	1.97	1.94	1.89
	(0.63)	(0.51)	(0.39)	(0.59)
Visits for Routine Care	2.29	2.33	2.28	2.29
	(0.66)	(0.48)	(0.35)	(0.62)
Self-Reported Overall Health Status	2.96	2.95	2.93	2.94
	(0.41)	(0.31)	(0.26)	(0.37)
Self-Reported Mental Health Status	2.27	2.27	2.27	2.28
	(0.42)	(0.31)	(0.26)	(0.37)
No. Obs	2,923	560	167	195

Notes: This panel presents summaries of self-reported plan ratings, utilization, and outcomes for MA enrollees, across different sets of counties. The unit of aggregation is at the county-year level. The original measures were denoted for each enrollee, per year. The original data is taken from the CAHPS and is originally provided at an individual respondent level. Plan ratings are coded on a 0-10 scale, while self-reported health ratings are coded on a 1-5 scale. CAHPS survey data only covers plans that are at least a year old. As such, counties that have only new MA plans or no MA plans whatsoever do not appear in the data. SRH refers to self-reported health.

Table 5: MA Bid Analysis

	(1)	(2)
	Bid As	
	Fraction of Benchmark	Total Amount
Urban	**0.004**	
	(0.008)	
Instr. County Benchmark		**0.906*** **
		(0.080)
2007 FFS 5yr (in 100s)	0.004	2.538
	(0.003)	(2.351)
Metro Pop (100k)	-0.006	-4.007
	(0.009)	(6.047)
Metro Pop (100k) Sq	0.001	0.496
	(0.001)	(0.783)
Cnty Pop (100k)	0.001	0.759
	(0.003)	(2.327)
Cnty Pop (100k) Sq	-0.001	-0.530
	(0.001)	(0.667)
Counties	Metro 100-600k, & 2007 FFS 5 yr Blw 662	
Mean	0.897	645.25
	(0.034)	(44.73)
N	1,360	1,360
R-squared	0.339	0.815

Notes: Table presents linear regression model; outcome variable include plan bids, represented as fractions of county benchmarks and in absolute monthly terms. The unit of observation is aggregated to the county-year level. The underlying data is from CMS and covers the 2007-2010 period. In our sample construction, we exclude counties whose adjusted FFS level-as of 2007-was above that of the lowest possible floor. Further, we restrict to those counties in the 100-600k metro population band. We include year-level indicators and also control for 5-yr per capita Medicare FFS spending, from 2007. Populations are stated in terms of 100k. Standard errors are clustered at the metro-level.

Table 6: First Stage Regression Results

	(1)	(2)	(3)	(4)
VARIABLES		**County Benchmark**		
Urban	68.57***	21.81*	-21.23*	0.75
	(2.27)	(11.08)	(12.42)	(9.27)
County Pop (100k)	-1.61	-1.59	-7.97	-6.32
	(1.44)	(4.90)	(7.52)	(4.62)
County Pop (100k) Sq	0.49	0.17	0.59	0.69
	(0.46)	(0.91)	(1.39)	(0.87)
Metro Pop	0.59	1.55	22.04*	12.92
	(2.90)	(13.03)	(12.77)	(10.10)
Metro Pop Sq	-0.09	-0.27	-2.14	-1.35
	(0.36)	(1.54)	(1.67)	(1.28)
2007 FFS 5-yr	0.04***	0.53***	1.02***	1.00***
	(0.01)	(0.13)	(0.06)	(0.04)
FFS Restriction	**Group 1**	**Group 2**	**Group 3**	**Groups 2 & 3**
Observations	1,740	650	490	1,140
R-squared	0.96	0.51	0.77	0.84

Notes: Table presents results of our first-stage regression, a linear model with County-Level Monthly MA Benchmarks as the outcome variable. Benchmark values are inflation-adjusted, and represented in 2007 dollars. The Urban variable serves as the instrument of interest. The unit of observation is at the county-year level, for the 2007-2011 period. The regression results are weighed inversely with the number of counties in a metro area, such that each metro area is equally represented in the regression. Year fixed effects are included in the analysis. The county sample is restricted to a variety of FFS cost groupings, with benchmark floors typically binding for Group 1, partially binding for Group 2, and not binding for Group 3. We include year-level indicators and also control for 5-yr per capita Medicare FFS spending, from 2007. Standard errors are clustered at the metro level. The original data is obtained from publicly available CMS files, including enrollment and other data. Note that populations are stated in terms of 100k.

Table 7: Reimbursement Impact: Market Structure and Plan Penetration

VARIABLES	(1) Insurers	(2) HHI	(3) HMO+PPO Sh.	(4) PFFS Sh.	(5) MA Sh.
Mean (Baseline Sample)	*6.49*	*3,907*	*0.097*	*0.068*	*0.166*
Urban	1.78***	-873**	0.071***	0.059***	0.131***
	(0.47)	(370)	(0.019)	(0.013)	(0.023)
2007 FFS 5-yr (100s)	-0.69***	558***	-0.041***	-0.031***	-0.072***
	(0.23)	(187)	(0.015)	(0.008)	(0.015)
N	1,740	1,728	1,740	1,740	1,740
Sample	*Baseline: 100-600k Metros, 2007-2011, 2007 FFS < 662*				
Urban	1.89***	-541	0.070***	0.041**	0.111***
	(0.52)	(373)	(0.021)	(0.016)	(0.026)
2007 FFS 5-yr (100s)	-0.35	486*	-0.034***	-0.021**	-0.054***
	(0.37)	(252)	(0.011)	(0.010)	(0.015)
N	750	739	750	750	750
Sample	*Robustness: 150-350k Metros*				
Urban	1.98***	-29	0.070**	0.026	0.096***
	(0.66)	(503)	(0.028)	(0.022)	(0.030)
2007 FFS 5-yr (100s)	-0.10	421	-0.023	-0.024*	-0.048*
	(0.47)	(361)	(0.017)	(0.014)	(0.024)
N	395	386	395	395	395
Sample	*Robustness: 200-300k Metros*				
Urban*Low	1.53***	-1,087***	0.028	0.019*	0.047*
	(0.46)	(370)	(0.027)	(0.009)	(0.028)
2007 FFS 5-yr (100s)	-0.50**	407***	-0.032**	-0.021***	-0.053***
	(0.21)	(144)	(0.013)	(0.006)	(0.013)
N	2,880	2,855	2,880	2,880	2,880
Sample	*Diff-in-Diff: Comparing Low and High FFS*				
Urban*Post	1.14***	6	0.050***		0.050***
	(0.30)	(506)	(0.017)		(0.017)
2007 FFS 5-yr (100s)	-0.38*	388**	-0.046***		-0.051***
	(0.20)	(187)	(0.016)		(0.013)
N	2,784	2,072	2,784		2,784
Sample	*Diff-in-Diff: Comparing Pre and Post*				

Notes: Table presents linear regression models, where outcome variables are measures of MA market structure. The unit of observation is aggregated at the county-year, for the period specified, with the aggregation weighed by plan enrollment. The regression results are weighed inversely with the number of counties in a metro area, such that each metro area is equally represented in the regression. The original data is obtained from publicly available CMS files, including enrollment, landscape, OOPC, and other data. For the baseline sample, we exclude counties whose adjusted FFS level-as of 2007-was above that of the lowest possible floor, and also focus on the 2007 to 2011 post-period. We also restrict to those counties within the specified population band. In one alternate specification shown, we expand the baseline sample to include High FFS counties; in another, we expand the baseline sample to include the pre-2001 period (while still excluding the 2001 to 2006 period). In these alternate specifications, high FFS counties/pre-2001 observations serve as a control group, given that Urban status would not explicitly impact benchmarks for those observations. Prior to 2004, only HMO plans could be offered, meaning that the MA share and HMO+PPO share levels are identical for the pre-period analysis. We include year-level indicators and also control for 5-yr per capita Medicare FFS spending, from 2007. We also include quadratic population controls, for counties as well as metros. Populations are stated in terms of 100k. Standard errors are clustered at the metro-level.

Table 8: Reimbursement Impact: Plan Characteristics

VARIABLES	(1) Premium	(2) Rebate	(3) OOPC	(4) Premium+OOPC-Reb	(5) Drug Coverage
Mean (Baseline Sample)	*31.96*	*54.80*	*364.92*	*348.82*	*0.66*
Urban	-0.88	3.38	-7.02	-8.30	-0.064
	(5.97)	(4.11)	(8.48)	(12.87)	(0.070)
2007 FFS 5-yr (100s)	2.18	-1.00	-9.73**	-7.86	0.106***
	(3.23)	(1.93)	(4.16)	(6.48)	(0.032)
N	1,701	1,360	1,701	1,360	1,701
Sample	*Baseline: 100-600k Metros, 2007-2011, 2007 FFS < 662*				
Urban	1.37	-1.15	2.58	5.80	0.020
	(7.38)	(5.87)	(11.75)	(17.87)	(0.098)
2007 FFS 5-yr (100s)	0.44	2.87	-24.43***	-30.06***	0.099*
	(4.41)	(3.04)	(5.56)	(8.55)	(0.050)
N	711	568	711	568	711
Sample	*Robustness: 150-350k Metros*				
Urban	7.27	5.55	0.40	3.51	-0.050
	(8.07)	(8.58)	(17.98)	(26.67)	(0.147)
2007 FFS 5-yr (100s)	4.20	-2.18	-25.95***	-18.47	0.081
	(4.82)	(4.42)	(7.35)	(11.88)	(0.073)
N	361	288	361	288	361
Sample	*Robustness: 200-300k Metros*				
Urban*Low	6.56	-5.71	1.02	11.2	-0.036
	(6.35)	(6.86)	(8.79)	(17.32)	(0.050)
2007 FFS 5-yr (100s)	-0.81	2.68	-12.38***	-16.40**	0.096***
	(2.74)	(3.22)	(3.95)	(7.79)	(0.025)
N	2,809	2,246	2,809	2,246	2,809
Sample	*Diff-in-Diff: Comparing Low and High FFS*				
Urban*Post	-2.28				0.063
	(7.09)				(0.105)
2007 FFS 5-yr (100s)	0.63				0.091***
	(3.25)				(0.034)
N	1,786	1,360	1,701	1,360	1,786
Sample	*Diff-in-Diff: Comparing Pre and Post*				

Notes: Table presents linear regression models, where outcome variables are measures of the financial characteristics of MA plans. All outcome measures are inflation-adjusted, and represented in 2007 dollars. The unit of observation is aggregated at the county-year, for the period specified, with the aggregation weighed by plan enrollment. The regression results are weighed inversely with the number of counties in a metro area, such that each metro area is equally represented in the regression. The original data is obtained from publicly available CMS files, including enrollment, landscape, OOPC, and other data. For the baseline sample, we exclude counties whose adjusted FFS level-as of 2007-was above that of the lowest possible floor, and also focus on the 2007 to 2011 post-period. We also restrict to those counties within the specified population band. In one alternate specification shown, we expand the baseline sample to include High FFS counties; in another, we expand the baseline sample to include the pre-2001 period (while still excluding the 2001 to 2006 period). In these alternate specifications, high FFS counties/pre-2001 observations serve as a control group, given that Urban status would not explicitly impact benchmarks for those observations. Prior to 2004, only HMO plans could be offered, meaning that the MA share and HMO+PPO share levels are identical for the pre-period analysis. We include year-level indicators and also control for 5-yr per capita Medicare FFS spending, from 2007. We also include quadratic population controls, for counties as well as metros. Populations are stated in terms of 100k. Standard errors are clustered at the metro-level.

Table 9: Effect on Insurers

VARIABLES	(1) HMO+PPO Sh.	(2) PFFS Sh.	(3) MA Sh.	(1) Prem	(2) Rebate	(3) OOPC	(4) Prem+ OOPC	(5) Prem+ OOPC-Reb	(6) Drug Cov
Restriction	**Top Insurer by Market Penetration (Humana)**								
Urban	0.008	0.011*	0.018**	-0.50	2.34	-13.83	-14.33*	-13.29**	-0.002
	(0.005)	(0.006)	(0.009)	(3.61)	(1.96)	(9.39)	(8.04)	(6.29)	(0.002)
2007 FFS 5 yr	0.004	-0.008***	-0.004	2.769**	-0.71	-5.34	-2.57	-1.02	0.001
(in 100s)	(0.002)	(0.003)	(0.004)	(1.23)	(0.71)	(3.54)	(3.31)	(2.80)	(0.000)
Mean	0.014	0.024	0.038	30.44	41.34	395.39	425.83	393.23	1.000
	(0.028)	(0.025)	(0.035)	(30.65)	(23.20)	(52.00)	(40.43)	(44.01)	(0.007)
N	1,740	1,740	1,740	1,623	1,316	1,623	1,623	1,316	1,623
Restriction	**Non-Top Insurer by Market Penetration**								
Urban	0.064***	0.048***	0.112***	-0.85	-0.11	-3.05	-3.74	-0.35	-0.007
	(0.020)	(0.011)	(0.023)	(7.24)	(5.75)	(10.19)	(13.57)	(16.88)	(0.086)
2007 FFS 5 yr	-0.045***	-0.023***	-0.068***	3.02	0.50	-13.10***	-10.08*	-13.87*	0.10**
(in 100s)	(0.015)	(0.007)	(0.015)	(3.64)	(2.37)	(4.45)	(5.65)	(7.41)	(0.040)
Mean	0.084	0.044	0.128	32.10	59.17	353.51	385.64	332.06	0.499
	(0.100)	(0.051)	(0.106)	(28.48)	(19.71)	(40.49)	(50.18)	(57.87)	(0.321)
N	1,740	1,740	1,740	1,681	1,344	1,682	1,681	1,344	1,682
Additional FEs	Year								
Counties	Metro 100-600k, FFS 5 yr Under 662 (from 2007)								

Notes: Table presents linear regression models, where outcome variables are measures of MA market structure and the financial characteristics of MA plans. All financial measures are inflation-adjusted, and represented in 2007 dollars. The regression results are weighed inversely with the number of counties in a metro area, such that each metro area is equally represented in the regression. In the top panel, the sample is restricted to the MA insurer with the greatest geographic penetration: Humana. The results here capture the impact on MA enrollment across these two insurers, as a share of all Medicare, along with the impact on characteristics of this insurer's plans. In the bottom panel, the sample is restricted to all insurers, excluding Humana, with the results accordingly capturing the impact across non-top insurer. Altogether, these results capture whether pass-through differs across incumbent insurers and new insurer entrants. The unit of observation is aggregated at the county-year, for the period specified, with the aggregation weighed by plan enrollment. The original data is obtained from publicly available CMS files, including enrollment, landscape, OOPC, and other data. For the baseline sample, we exclude counties whose adjusted FFS level-as of 2007-was above that of the lowest possible floor, and also focus on the 2007 to 2011 post-period. We also restrict to those counties within the specified population band. We include year-level indicators and also control for 5-yr per capita Medicare FFS spending, from 2007. We also include quadratic population controls, for counties as well as metros. Populations are stated in terms of 100k. Standard errors are clustered at the metro-level.

Table 10: CAHPS Ratings

	(1) Overall Health Plan	(2) Overall Healthcare	(3) PCP	(4) Specialist Seen	(5) Drug Benefits
Mean (Baseline)	8.39	8.51	9.02	8.9	8.35
Urban	-0.044	-0.177	-0.204**	-0.050	-0.067
	(0.157)	(0.126)	(0.083)	(0.116)	(0.126)
2007 FFS 5-yr (100s)	0.061***	0.010	-0.000	0.003	0.023
	(0.018)	(0.015)	(0.014)	(0.019)	(0.018)
Stand. Treat. Effect			-0.119		
			(.083)		
N	1,657	1,641	1,625	1,545	1,588
Urban*Low	-0.200	-0.146	-0.040	-0.011	0.119
	(0.127)	(0.096)	(0.057)	(0.073)	(0.111)
2007 FFS 5-yr (100s)	0.041**	-0.008	-0.009	0.000	0.012
	(0.017)	(0.014)	(0.012)	(0.019)	(0.017)
Stand. Treat. Effect			-0.065		
			(0.056)		
N	2,607	2,584	2,565	2,455	2,525

Notes: Table presents linear regression models, where outcome variables are enrollee-reported ratings of plan quality. The unit of observation is at the county-year level, for the 2007-2011 period. The regression results are weighed inversely with the number of counties in a metro area, such that each metro area is equally represented in the regression. The original data is obtained from CMS, from the CAHPS survey of MA enrollees; while the data was originally at an individual respondent level, we aggregate this data to the county-year level for purposes of our analysis, with each observation weighed equally. We exclude counties whose adjusted FFS level-as of 2007-was above that of the lowest possible floor. Further, we restrict to those counties within the 100-600k metro population band. Plan ratings are coded on a 0-10 scale, while self-reported health ratings are coded on a 1-5 scale; higher corresponds to better. CAHPS survey data only covers plans that are at least a year old. As such, counties that have only new MA plans or no MA plans whatsoever do not appear in the data. We include year-level indicators and also control for 5-yr per capita Medicare FFS spending, from 2007. We also include controls for age categories, race, and gender. In addition, we include quadratic population controls, for counties as well as metros. Populations are stated in terms of 100k. Standard errors are clustered at the metro-level. Standardized treatment effects are calculated consistent with the approach in Kling et al (2007) and Finkelstein et al (2012).

Table 11: CAHPS Utilization and Health

	(1) Specialist Visits	(2) Personal MD Visits	(3) Routine Visits	(4) SRH Overall	(5) SRH Mental Health
Mean (Baseline)	1.66	1.90	2.27	2.92	2.26
Urban	-0.029	0.054	-0.070	0.094	0.121
	(0.080)	(0.110)	(0.121)	(0.081)	(0.084)
2007 FFS 5yr (in 100s)	0.011	0.007	-0.001	-0.012	-0.005
	(0.012)	(0.016)	(0.017)	(0.009)	(0.009)
Stand. Treat. Effect			0.071		
			(0.110)		
N	1,554	1,651	1,661	1,661	1,662
Urban*Low	0.059	0.041	0.084	0.064	-0.020
	(0.050)	(0.094)	(0.085)	(0.066)	(0.061)
2007 FFS 5yr (in 100s)	0.011	0.014	-0.001	-0.004	-0.002
	(0.013)	(0.017)	(0.017)	(0.009)	(0.009)
Stand. Treat Effect			0.020		
			(0.061)		
N	2,467	2,598	2,611	2,612	2,614

Notes: Table presents linear regression models, where outcome variables are enrollee-reported utilization levels and health status. The unit of observation is at the county-year level, for the 2007-2011 period. The regression results are weighed inversely with the number of counties in a metro area, such that each metro area is equally represented in the regression. The original data is obtained from CMS, from the CAHPS survey of MA enrollees; while the data was originally at an individual respondent level, we aggregate this data to the county-year level for purposes of our analysis, weighing each observation equally. We exclude counties whose adjusted FFS level-as of 2007-was above that of the lowest possible floor. Further, we restrict to those counties in the 100-600k metro population band. Plan ratings are coded on a 0-10 scale, while self-reported health ratings are coded on a 1-5 scale; higher corresponds to better. CAHPS survey data only covers plans that are at least a year old. As such, counties that have only new MA plans or no MA plans whatsoever do not appear in the data. We include year-level indicators and also control for 5-yr per capita Medicare FFS spending, from 2007. We also include controls for age categories, race, and gender. In addition, we include quadratic population controls, for counties as well as metros. Populations are stated in terms of 100k. Standard errors are clustered at the metro-level. Standardized treatment effects are calculated consistent with the approach in Kling et al (2007) and Finkelstein et al (2012).

Table 12: MA Enrollment: Demographic Composition Analysis

	(1)					(2)	
VARIABLES	**Age**					**Demographics**	
	65-74	*75-80*	*81-84*	*85+*	*Unknown*	*White*	*Female*
Urban	0.02	-0.03	-0.00	-0.02	-0.01	0.01	0.01
	(0.02)	(0.02)	(0.01)	(0.02)	(0.01)	(0.04)	(0.02)
2007 FFS 5yr (in 100s)	0.00	0.00	-0.02**	-0.02**	-0.01**	-0.06**	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.03)	(0.01)
Metro Pop (100k)	-0.04	0.04	0.01	0.03	0.01	-0.01	-0.01
	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.05)	(0.03)
Metro Pop (100k) Sq	0.01**	-0.00	-0.00	-0.01*	-0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)
Cnty Pop (100k)	0.00	0.01	0.03**	0.03**	0.01	-0.09**	0.01
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.05)	(0.02)
Cnty Pop (100k) Sq	-0.00	-0.00	-0.00*	-0.00	-0.00	-0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)
Additional Controls	**Year**						
Counties	**Metro 100-600k, FFS 5 yr Under 662 (from 2007)**						
Mean	0.296	0.222	0.143	0.104	0.061	0.895	0.535
	(0.196)	(0.158)	(0.128)	(0.115)	(0.094)	(0.190)	(0.186)
N	1,618	1,618	1,618	1,618	1,618	1,618	1,618
R-squared	0.25	0.03	0.11	0.05	0.13	0.04	0.02

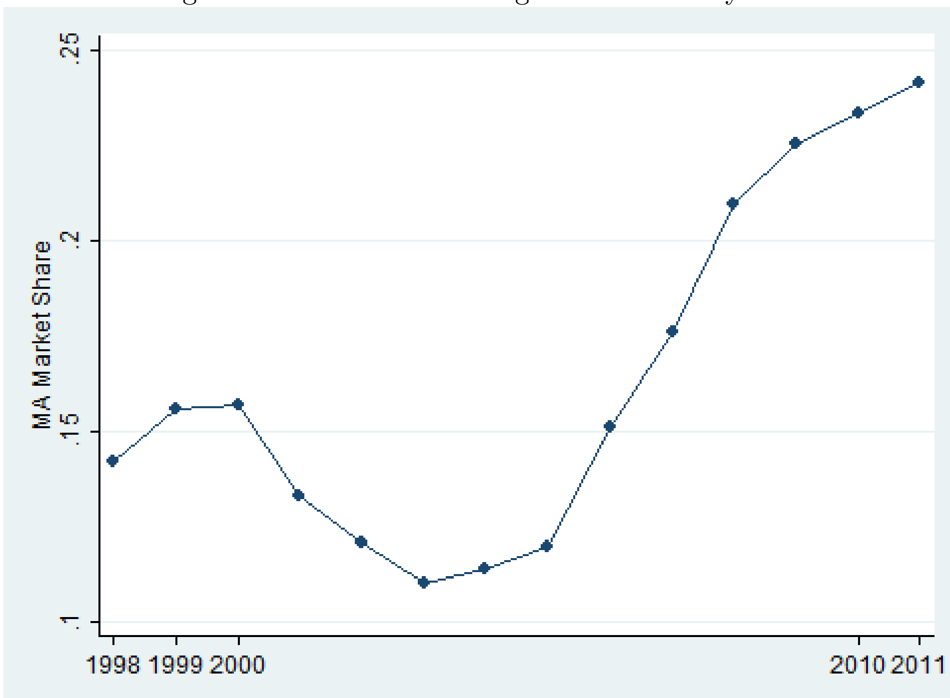
Notes: Table presents linear regression models, where outcome variables are enrollee-reported demographic characteristics. The unit of observation is at the county-year level, for the 2007-2011 period. The regression results are weighed inversely with the number of counties in a metro area, such that each metro area is equally represented in the regression. The original data is obtained from CMS, from the CAHPS survey of MA enrollees; while the data was originally at an individual respondent level, we aggregate this data to the county-year level for purposes of our analysis, weighing each observation equally. We exclude counties whose adjusted FFS level-as of 2007-was above that of the lowest possible floor. Further, we restrict to those counties in the 100-600k metro population band. CAHPS survey data only covers plans that are at least a year old. As such, counties that have only new MA plans or no MA plans whatsoever do not appear in the data. We include year-level indicators and also control for 5-yr per capita Medicare FFS spending, from 2007. In addition, we include quadratic population controls, for counties as well as metros. Populations are stated in terms of 100k. Standard errors are clustered at the metro-level.

Table 13: Advertising Spending in $ per Medicare Beneficiary

	(1)	(2)	(3)	(4)	(5)
Panel A: Health Insurance Products denoted "Medicare" Lines					
% Urban Floor	6.352***	7.263***	6.024***	5.176***	2.805
	(2.228)	(2.188)	(2.269)	(1.952)	(2.252)
% Urban					3.722
					(2.501)
% Floor					-0.399
					(1.235)
FFS Spending			-1.730	-1.346	-1.904
			(1.263)	(1.138)	(1.210)
Credit Card Ad Spending				0.0837*	0.0859**
				(0.0440)	(0.0427)
Observations	840	840	840	840	840
R-squared	0.059	0.109	0.113	0.156	0.160
Panel B: All Health Insurance Products					
% Urban Floor	20.69***	25.92***	20.50***	14.70***	15.80**
	(7.455)	(6.720)	(6.390)	(5.263)	(7.810)
% Urban					1.527
					(9.292)
% Floor					-4.356
					(3.092)
FFS Spending			-7.571*	-4.943	-6.522*
			(4.155)	(3.244)	(3.345)
Credit Card Ad Spending				0.573***	0.575***
				(0.140)	(0.139)
Observations	840	840	840	840	840
R-squared	0.097	0.204	0.213	0.406	0.407
Advertising Price Controls	no	yes	yes	yes	yes

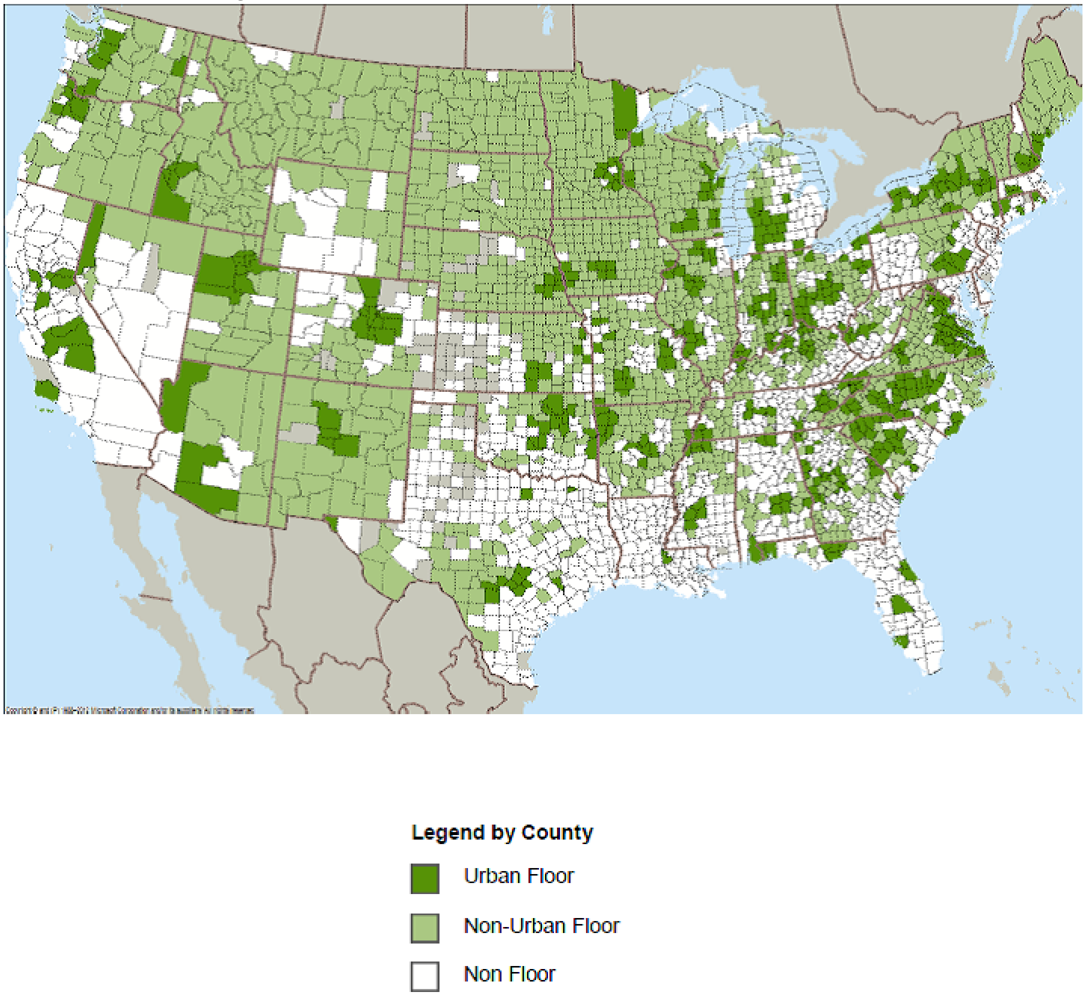
Notes: Table present results of an OLS regression with TV spot advertising expenditures for Medicare insurance products per Medicare beneficiary per year as the dependent variable. All measures are inflation-adjusted, and represented in 2007 dollars. The unit of observation is the DMA-year. The key explanatory variables of interest, % Urban Floor and % Rural Floor are aggregated from the county-level dataset using the same crosswalk provided by Gentzkow and Shapiro (2008). Population controls include a quadratic in metro-area population, and are included in all specifications. Advertising price index is given in SQAD points.

Figure 1: Medicare Advantage Penetration by Year



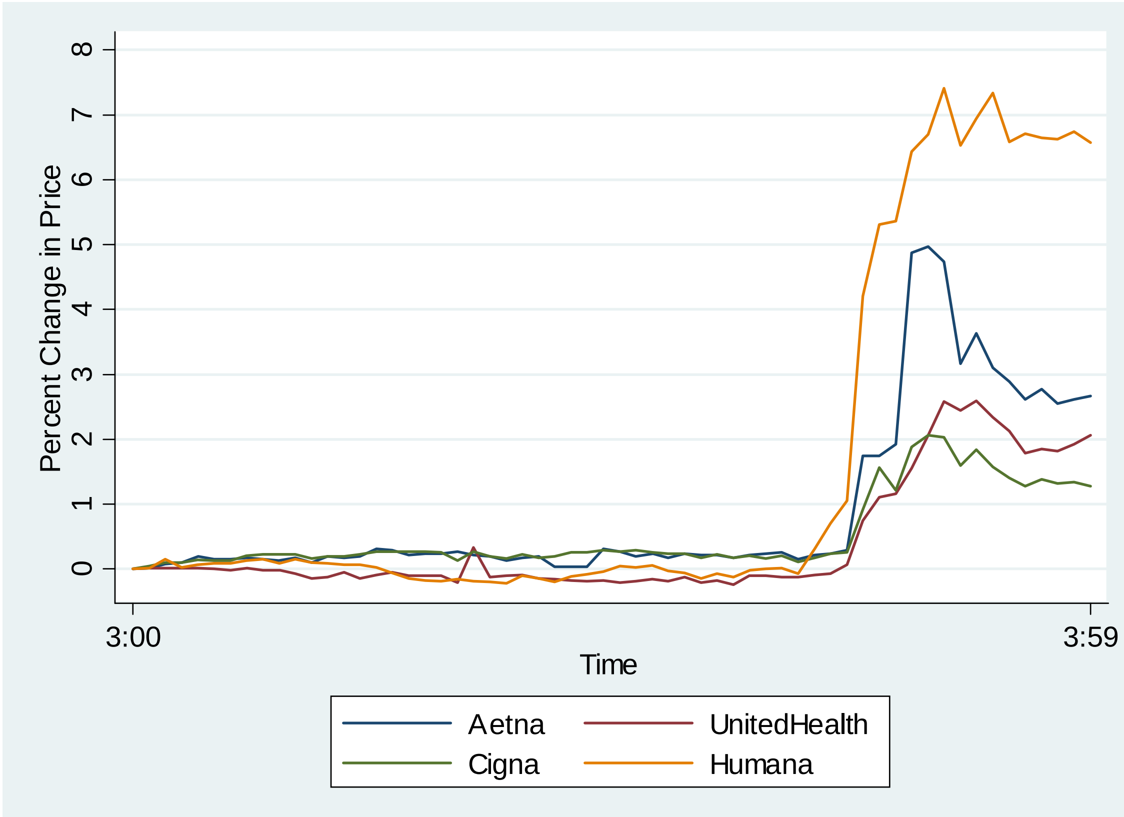
Note: Enrollment data are taken from publicly available CMS files and aggregated to the year level. The X-axis denotes year, while the Y-axis denotes the % of Medicare recipients enrolled in Medicare Advantage plans.

Figure 2: Nationwide Distribution of Floor Counties



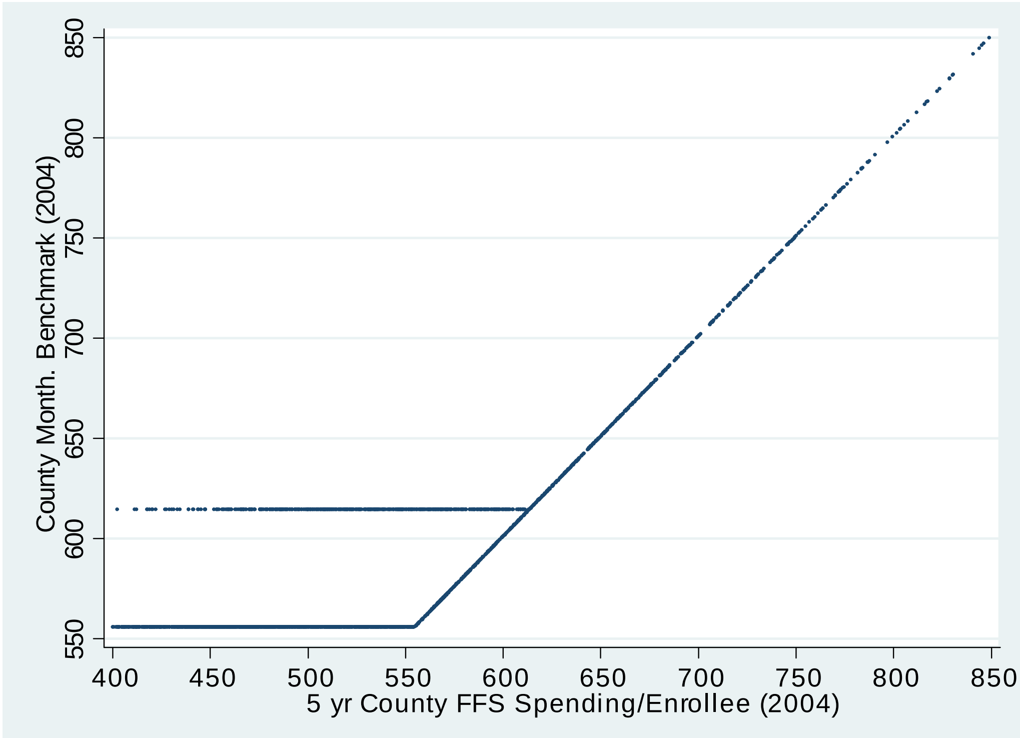
Note: Benchmark data are taken from publicly available CMS files. Dark and light green counties correspond to urban and non-urban floor counties, respectively. Meanwhile, white areas correspond to non-floor counties.

Figure 3: Stock Returns of Major MA Insurers, 3-4 pm on April 1, 2013



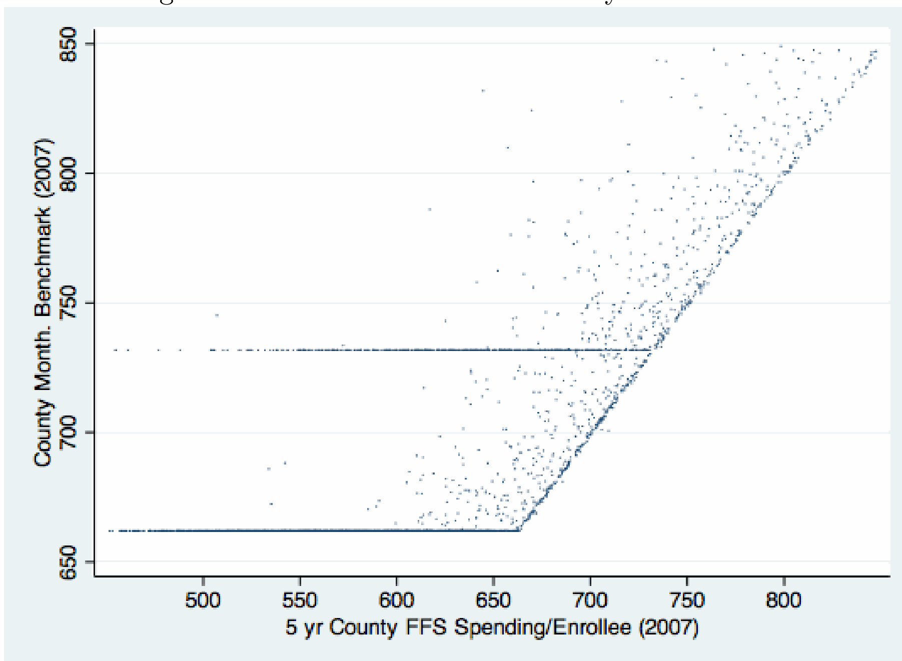
Note: Figure plots stock returns on April 1, 2013, when CMS announced a reversal to a planned cut to MA benchmarks (at 3 pm). The stock price change observed among health-insurance stocks-over this period-was absent for other firm types. Stock price data is taken from CRSP.

Figure 4: County Benchmark and FFS Costs in 2004



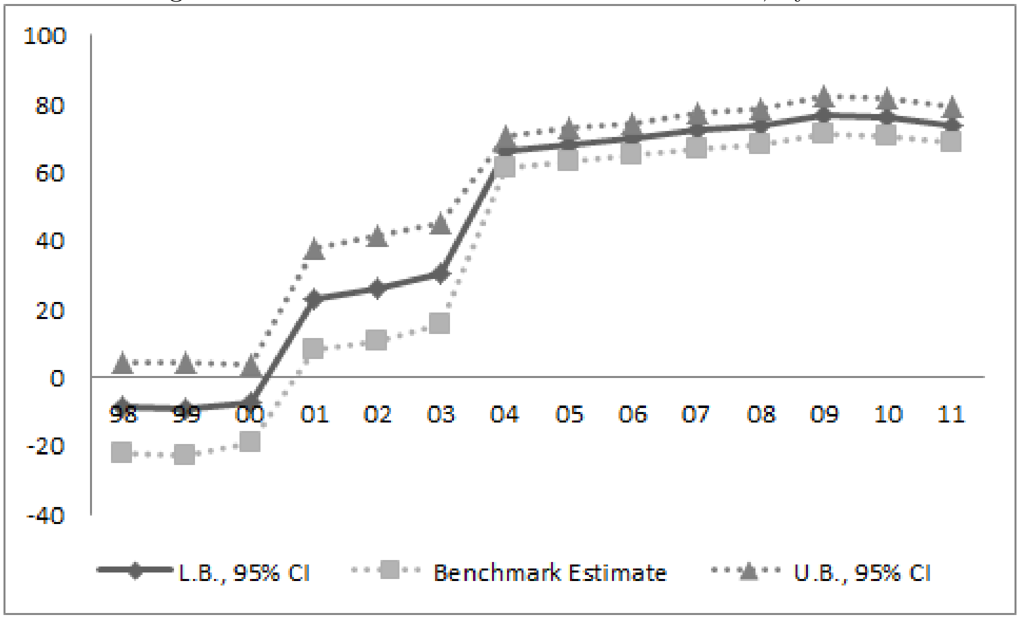
Note: FFS cost and benchmark data are taken from publicly available CMS files. The X-axis denotes 2004 FFS costs (based on CMS's 5-yr look-back average), while the y-axis denotes the 2007 benchmark payment amount.

Figure 5: 2007 FFS Costs and County Benchmarks



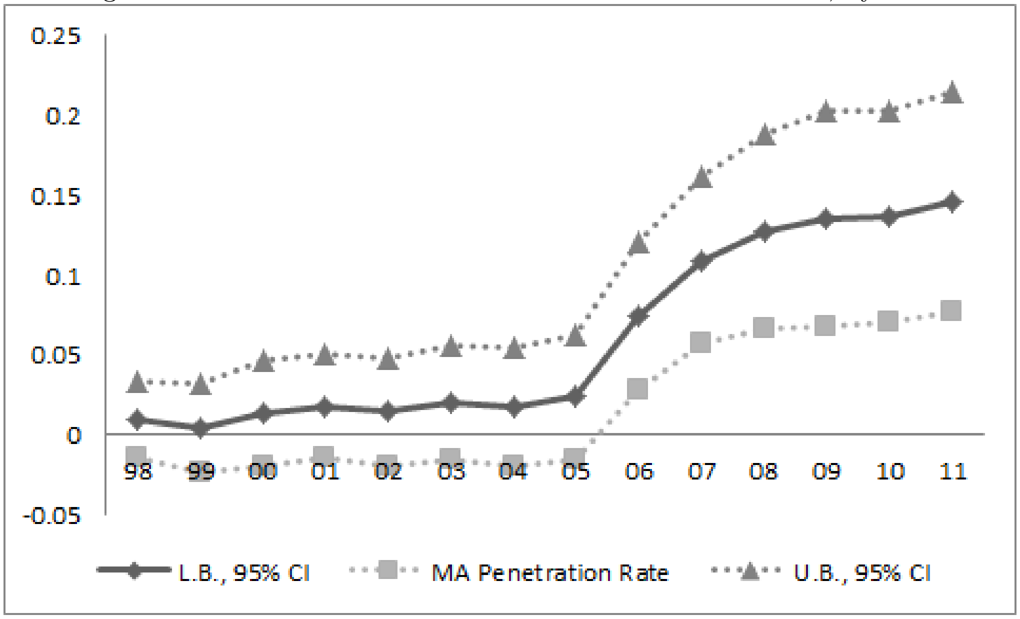
Note: FFS cost and benchmark data are taken from publicly available CMS files. The X-axis denotes 2007 FFS costs (based on CMS's 5-yr look-back average), while the y-axis denotes the contemporaneous benchmark payment amount.

Figure 6: Effect of Urban Status on Benchmarks, by Year



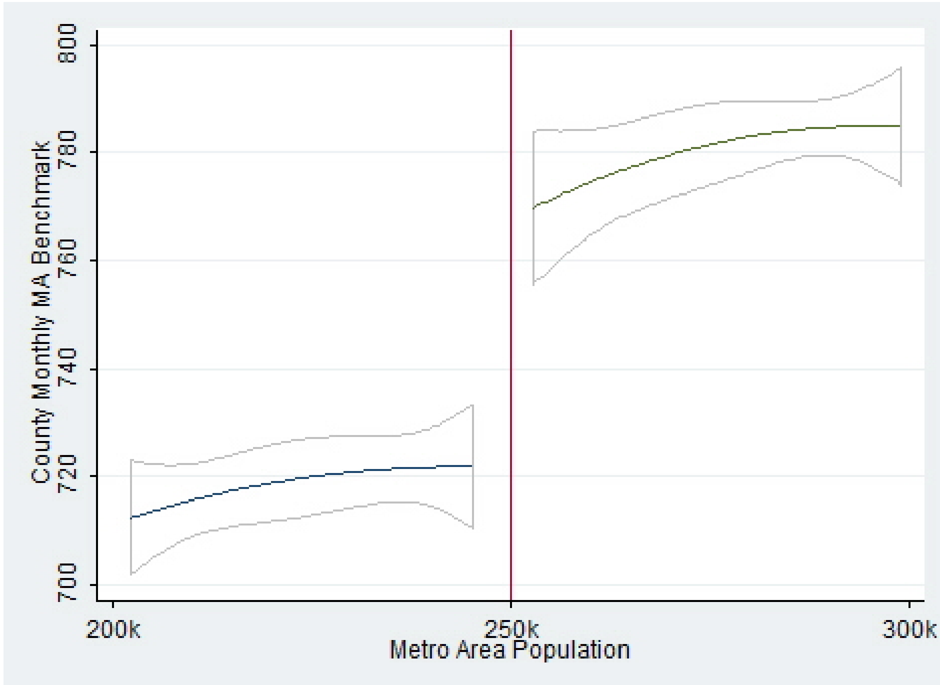
Note: Benchmark data are taken from publicly available CMS files and aggregated to the county-year level. Each point represents the coefficient on the "urban" dummy in a specification analogous to that in Equation (1), with benchmark as the dependent variable, restricted to the year denoted in the x-axis. Here, we also plot the 95% confidence intervals.

Figure 7: Effect of Urban Status on MA Penetration Rates, by Year



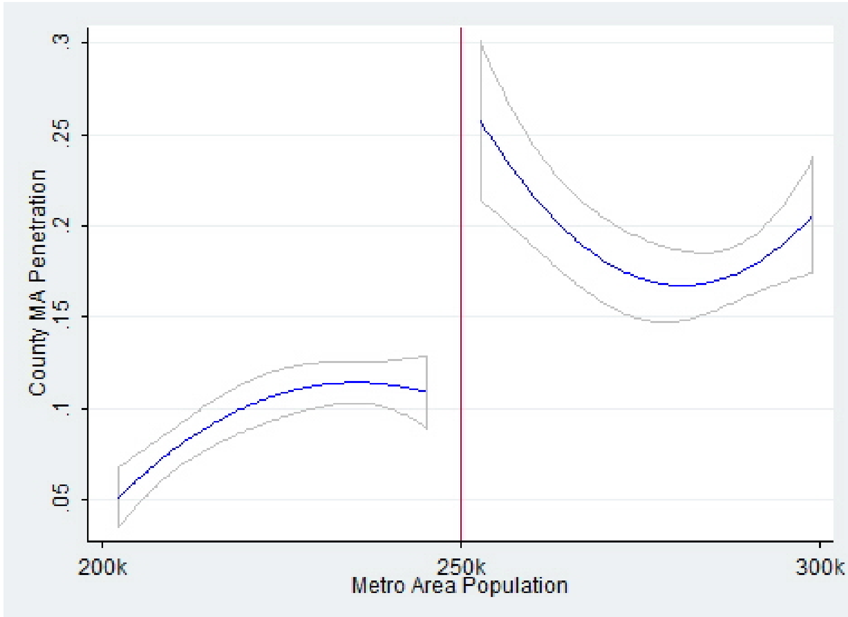
Note: Enrollment data are taken from CMS enrollment files and aggregated to the county-year level. Each point represents the coefficient on the "urban" dummy in a specification analogous to that in Equation (1), with MA penetration rates as the dependent variable, restricted to a single year denoted in the x-axis. Here, we also plot the 95% confidence intervals.

Figure 8: Effect of Urban Status on Benchmarks



Note: Benchmark data are taken from CMS and aggregated to the county-year level. Plot lines are constructed separately for each side of the discontinuity, using a second degree polynomial and an epanechnikov kernel. Alongside this line, we also plot the 95% confidence interval.

Figure 9: Effect of Urban Status on MA Penetration



Note: Market structure data are taken from CMS and aggregated to the county-year level. Plot lines are constructed separately for each side of the discontinuity, using a second degree polynomial and an epanechnikov kernel. Alongside this line, we also plot the 95% confidence interval.

Appendix: For Online Publication

A.1 Theory

This section describes the theoretical framework that informs the empirical specifications and highlights the fact that incidence depends on the degree of competition in the market as well as selection. For simplicity, we consider the case of linear demand. Just as manufacturers face upward sloping supply curves because the last plant location is not as efficient as the first plant location, insurance companies may face upward sloping average cost curves as well. If there is advantageous selection, then the marginal Medicare Advantage consumer is sicker and more costly to insure than the average. The average cost curve for a plan traces out costs from those who value the plan the most to those who value it least. Under advantageous selection, the low cost enrollees have the highest valuation for Medicare Advantage plans. In this case, we should expect a pass-through rate of less than one. As the amount of the subsidy increases, Medicare Advantage penetration rates increase, and sicker consumers begin to enroll in plans. As a result a dollar increase in the subsidy must fund the health costs of the sicker enrollees in addition to providing additional benefits to existing enrollees. Figure A.1 illustrates that incomplete pass-through under advantageous selection into Medicare Advantage policies.[18] Let AC_1 be average costs under initial reimbursement generosity. If generosity increases by some amount m, there is a downward shift in the insurer's average cost curve to AC_2. If demand were completely inelastic, the price would fall from p_1 to $p_1 - m$. However, if demand is not completely inelastic, the price will fall to some intermediate level p_2: the incidence of the increased generosity depends on the relative elasticity of supply (determined by selection) and demand.[19]

[18] We collapse this average out-of-pocket cost to an effective price p and assume no differences in plan quality. We will relax this assumption in the empirical section and explore the relationship between contract generosity and plan quality.

[19] The intuition is reversed if there is adverse selection. Pass-through is greater than one because the increased subsidy serves to internalize part of the asymmetric information problem. If there is relatively little selection (and thus a flat AC curve) and the market for MA plans is perfectly competitive, then virtually all of the additional spending passes through to consumers in the form of a lower premium.

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Furthermore, various studies (Dafny, 2010, Lustig, 2010, and Starc, 2014) have argued that perfect competition is a poor benchmark in insurance markets, and the incidence of the MA subsidy also depends on market structure. Consider pass-through under monopoly. Figure A.2 shows a downward shift of the average cost curve and assumes no selection; the marginal consumer and average consumer are the same. When the monopolist sets price equal to marginal revenue, the decrease in price is smaller than under perfect competition because the marginal revenue curve is steeper than the demand curve. In our example with constant marginal costs, linear demand would imply a pass-through rate of one-half, as the marginal revenue curve is twice as steep as the demand curve. Advantageous selection amplifies this effect. Therefore, both advantageous selection and imperfect competition theoretically reduce pass-through rates. Weyl and Fabinger (2013) expand this analysis to intermediate cases and more flexible models of demand. They find that the less competitive the conduct in a market is, the smaller the pass-through rate.[20]

In addition, more firms may enter as a result of increased plan generosity. If entry is costly, then an increase in government benefits could induce additional firms to enter. This is socially beneficial if the benefits to consumers in the form of increased competition and product variety are greater than the additional fixed costs incurred and the deadweight loss of taxation to fund any increase in Medicare spending. However, if increased generosity spurs excess entry, fixed and marketing expenditures are real economic costs. A model describing the full strategic interaction of imperfectly competitive firms is outside the scope of this paper; however, we can describe the strategic decisions made by insurers.

First, the firm must decide which markets to enter. Second, conditional on being active in a market, they must design insurance products, and then set premiums for those insurance products. Finally, the firm may choose to make ongoing quality investments over the course of the year, and earn variable profits on each policy. If the discounted sum of

little selection (and thus a flat AC curve) and the market for MA plans is perfectly competitive, then virtually all of the additional spending passes through to consumers in the form of a lower premium.

[20] Similarly, Mahoney and Weyl (2013) specifically consider the case of selection markets.

future variable profits is higher than the fixed cost of entry, the firm enters the market.[21] Therefore, in order to predict firm entry and the associated increase in competitive pressure, we are interested in a comparative static that links benchmarks to firm variable profits. This comparative static depends on four effects.

The first is the direct effect, where increased benchmarks lead to higher reimbursements for firms. The second is a price effect: for the same vector of bids, an increased benchmark means a lower price for consumers, depending on the pass-through rate.[22] Third, there is a cost effect, where higher benchmarks could change the composition of enrollees. For example, increasing penetration rates may lead to firms attracting sicker consumers, increasing costs, if there is advantageous selection in the market. Finally, there is a market power effect, in which high benchmarks may lead to more entry. As more firms enter, consumers have access to more plans that may prove to be closer substitutes, driving down markups. The overall effect of more generous plan reimbursement is ultimately an empirical question.

[21] A firm f may have a number of products j in market m. The firms variable profits from that policy can be written as:

$$\pi_{jm} = \sum_i (b_m + p_j - c_{ijm})\, s_{ijm}$$

where b_m is the benchmark (which in practice is adjusted by the individual's risk score), p_{jm} the plan's premium (if any), c_{ijm} the cost of individual i covered by plan j in market m, and s_{ijm} the probability that the same consumer purchases the plan. In order to get firm-level variable profits in a given market, aggregate over all plans within a market offered by the firm and subtract any fixed or sunk cost of entry.

[22] A higher benchmark need not change the competitive environment or optimal prices; increased benchmarks may simply affect firm profits by increasing quantity, as decreased premiums may increase Medicare penetration rates, and, therefore profits.

A.2 Tables and Figures

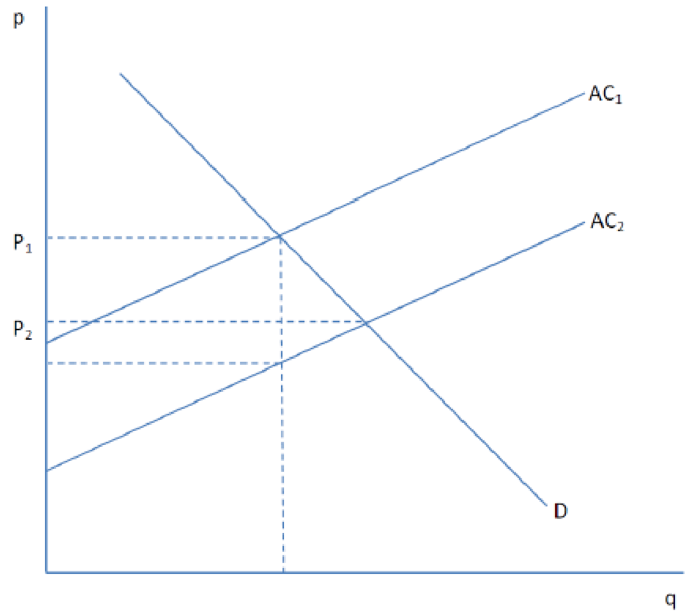

Figure A.1: Pass-Through Under Advantageous Selection

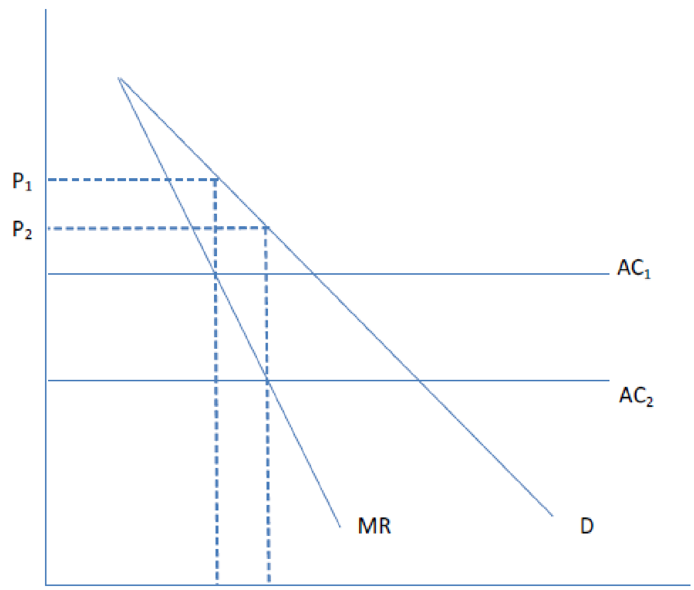

Figure A.2: Pass-Through Under Constant Average Cost and Monopoly

Table A.1: First Stage Regression Results: Pre-Period

	(1)	(2)	(3)	(4)
VARIABLES	**County Benchmark**			
Urban	-4.11	-4.97	24.69***	1.11
	(7.28)	(7.34)	(5.59)	(1.60)
Pre-2003 Urban		1.82		49.58***
		(4.89)		(1.29)
County Pop (100k)	-4.41	-4.72	7.93***	-0.32
	(4.10)	(4.16)	(2.43)	(0.88)
County Pop (100k) Sq	1.35	1.38	-0.63	0.19
	(1.28)	(1.28)	(0.57)	(0.27)
Metro Pop	5.15	4.98	3.33	-1.15
	(8.36)	(8.36)	(3.57)	(2.08)
Metro Pop Sq	-0.63	-0.62	-0.15	0.15
	(1.11)	(1.10)	(0.55)	(0.26)
2007 FFS 5-yr	0.29***	0.29***	0.07***	0.03***
	(0.03)	(0.03)	(0.02)	(0.01)
FFS Restriction	**Group 1**			
Population Restriction	100-600k Metro Areas			
Year Range	*1998-2000*		*2001-2003*	
Observations	1,044	1,044	1,044	1,044
R-squared	0.41	0.41	0.66	0.94

Notes: Table presents results of our first-stage regression, a linear model with Monthly, County-Level MA Benchmark as the outcome measure; urban serves as the instrument of interest. The unit of observation is at the county-year level, for the period specified. The regression results are weighed inversely with the number of counties in a metro area, such that each metro area is equally represented in the regression. The sample is restricted to counties in the 100-600k metro population range, as well as to counties with 2007 FFS levels below the lowest floor value. We include year-level indicators and also control for 5-yr per capita Medicare FFS spending, from 2007. The original data is obtained from publicly available CMS files, including enrollment and other data. Note that populations are stated in terms of 100k.

Table A.2: Stability Table

	(1) Insurers	(2) HHI	(3) HMO/PPO Sh.	(4) PFFS Sh.	(5) MA Sh.
Urban	1.64***	-756**	0.068***	0.054***	0.122***
	(0.44)	(324)	(0.021)	(0.013)	(0.024)
Controls	Linear Metro Population				
Urban	1.78***	-873**	0.071***	0.059***	0.131***
	(0.47)	(370)	(0.019)	(0.013)	(0.023)
Controls	Quadratic Metro Population				
Urban	1.61***	-698*	0.051**	0.058***	0.109***
	(0.50)	(398)	(0.022)	(0.015)	(0.026)
Controls	Cubic Metro Population				
Urban	1.74***	-593	0.063***	0.050***	0.112***
	(0.52)	(420)	(0.023)	(0.017)	(0.027)
Controls	Quartic Metro Population				
Urban	1.88***	-494	0.065***	0.034*	0.098***
	(0.51)	(472)	(0.024)	(0.018)	(0.020)
Controls	Spline Metro Population				
Urban	1.48***	-351	0.053**	0.039***	0.092***
	(0.48)	(382)	(0.025)	(0.014)	(0.026)
Controls	Linear Metro Population Trend on Each Side of Discontinuity				
Urban	2.03***	-406	0.077***	0.037**	0.114***
	(0.56)	(414)	(0.025)	(0.018)	(0.022)
Controls	Quadratic Metro Population Trend on Each Side of Discontinuity				
Urban	2.00***	-1,008***	0.074***	0.055***	0.129***
	(0.47)	(370)	(0.020)	(0.013)	(0.023)
Controls	Quadratic Metro Population, Demographic Controls				

Notes: Table presents linear regression models, where outcome variables include measures of MA plan penetration, market structure, and plan financial characteristics, as specified by the column. All financial measures are inflation-adjusted, and represented in 2007 dollars. Each panel presents results using a different type of control. The unit of observation is aggregated at the county-year, for the 2007-2011 period, with aggregation weighed by plan enrollment. The regression results are weighed inversely with the number of counties in a metro area, such that each metro area is equally represented in the regression. The original data is obtained from publicly available CMS files, including enrollment, landscape, OOPC, and other data. We exclude counties whose adjusted FFS level-as of 2007-was above that of the lowest possible floor. In addition, we restrict to those counties within the metro population band of 100,000 to 600,000. We include quadratic controls in county and metro-area population. We also control for 2007 per capita Medicare FFS spending, and include year-level indicators. Standard errors are clustered at the metro-area level.

Table A.3: Market Structure Baseline Analysis-Expanded

VARIABLES	(1) Insurers	(2) HHI	(3) HMO+PPO Sh.	(4) PFFS Sh.	(5) MA Sh.
Urban	1.78***	-873**	0.071***	0.059***	0.131***
	(0.47)	(370)	(0.019)	(0.013)	(0.023)
2007 FFS 5yr (in 100s)	-0.69***	558***	-0.041***	-0.031***	-0.072***
	(0.22)	(187)	(0.015)	(0.008)	(0.015)
Metro Pop (100k)	-0.57	435	-0.014	-0.022*	-0.036
	(0.59)	(465)	(0.023)	(0.013)	(0.024)
Metro Pop (100k) Sq	0.05	-38	0.001	0.002	0.003
	(0.09)	(65)	(0.003)	(0.002)	(0.004)
Cnty Pop (100k)	0.37	-574	-0.001	-0.003	-0.004
	(0.49)	(407)	(0.021)	(0.012)	(0.023)
Cnty Pop (100k) Sq	-0.38***	169**	-0.005	-0.001	-0.005
	(0.07)	(77)	(0.004)	(0.002)	(0.003)
Mean	*6.49*	*3,907*	*0.097*	*0.068*	*0.166*
	(3.072)	(1,802)	(0.105)	(0.060)	(0.108)
N	1,740	1,728	1,740	1,740	1,740
R-squared	0.61	0.167	0.303	0.277	0.320

Notes: Table presents linear regression models, where outcome variables are measures of MA market structure and the financial characteristics of MA plans. The unit of observation is aggregated at the county-year, for the 2007-2011 period, with aggregation weighed by plan enrollment. The regression results are weighed inversely with the number of counties in a metro area, such that each metro area is equally represented in the regression. The original data is obtained from publicly available CMS files, including enrollment, landscape, OOPC, and other data. We exclude counties whose adjusted FFS level-as of 2007-was above that of the lowest possible floor. Further, we restrict to those counties within the 100-600k metro population band. We include a control for 5-yr per capita Medicare FFS spending, from 2007, and also include year-level indicators. We also include quadratic population controls, for counties as well as metros. Populations are stated in terms of 100k. Standard errors are clustered at the metro-level.

Table A.4: Difference-in-Differences Specifications

	(1) Cnty Bnchmk	(2) Insurers	(3) HHI	(4) MA Sh.	(5) Prem	(6) Drug Cov
Urban*Low	70.326***	1.529***	-1,087***	0.047*	6.561	-0.036
	(8.379)	(0.464)	(370)	(0.028)	(6.355)	(0.050)
Urban	-12.460	-0.529	650*	0.043	-8.735	0.035
	(9.089)	(0.484)	(385)	(0.028)	(7.980)	(0.066)
Low	-27.476***	-0.630	241	-0.062*	-1.725	0.051
	(9.679)	(0.584)	(401)	(0.032)	(6.233)	(0.061)
2007 FFS 5-yr (100s)	30.472***	-0.502**	407***	-0.053***	-0.810	0.096***
	(4.673)	(0.207)	(144)	(0.013)	(2.740)	(0.025)
N	2,880	2,880	2,855	2,880	2,809	2,809
Urban*Post	66.271***	1.136***	6	0.050***	-2.276	0.063
	(4.369)	(0.303)	(506)	(0.017)	(7.094)	(0.105)
Urban	-3.522	0.358	-794	0.053**	1.224	-0.090
	(5.512)	(0.422)	(545)	(0.022)	(8.143)	(0.118)
Post	184.964***	3.014***	-4,017***	0.124***	-29.184***	0.099
	(3.284)	(0.223)	(332)	(0.012)	(5.470)	(0.078)
2007 FFS 5-yr (100s)	15.734***	-0.381*	388**	-0.051***	0.626	0.091***
	(1.924)	(0.204)	(187)	(0.013)	(3.245)	(0.034)
N	2,784	2,784	2,072	2,784	1,786	1,786

Notes: Table presents linear regression models, where outcome variables are measures of MA market structure and the financial characteristics of MA plans. All financial measures are inflation-adjusted, and represented in 2007 dollars. The unit of observation is aggregated at the county-year, for the 2007-2011 period, with aggregation weighed by plan enrollment. The regression results are weighed inversely with the number of counties in a metro area, such that each metro area is equally represented in the regression. The original data is obtained from publicly available CMS files, including enrollment, landscape, OOPC, and other data. We exclude counties whose adjusted FFS level-as of 2007-was above that of the lowest possible floor. Further, we restrict to those counties within the 100-600k metro population band. We include a control for 5-yr per capita Medicare FFS spending, from 2007, and also include year-level indicators. We also include quadratic population controls, for counties as well as metros. Populations are stated in terms of 100k. Standard errors are clustered at the metro-level.

Table A.5: Triple Difference

VARIABLES	(1) Cnty Bmk	(2) Insrs	(3) HHI	(4) MA Sh.	(5) Prem	(6) Drug Cov
Urban*Low FFS*Post	82.127***	1.921***	-1,283	0.065**	14.720	0.056
	(13.751)	(0.651)	(825)	(0.026)	(11.856)	(0.159)
Urban*Post	-16.006	-0.604	1,418**	-0.015	-14.964	0.005
	(12.598)	(0.568)	(643)	(0.020)	(9.521)	(0.117)
Low FFS*Post	-28.403***	0.102	-565	0.034**	-21.934**	-0.013
	(9.226)	(0.351)	(503)	(0.016)	(8.553)	(0.126)
Urban*Low FFS	-20.717	-0.445	156	-0.017	-9.047	-0.085
	(15.093)	(0.457)	(701)	(0.024)	(12.214)	(0.164)
Urban	9.422	0.387	-691	0.047**	8.120	0.030
	(14.363)	(0.420)	(513)	(0.021)	(10.997)	(0.134)
Low FFS	-23.456*	-0.193	632	-0.068***	18.407*	0.084
	(14.108)	(0.372)	(528)	(0.023)	(9.908)	(0.138)
Post	204.410***	3.776***	-3,284***	0.022	-4.965	0.112
	(8.504)	(0.303)	(399)	(0.013)	(6.408)	(0.097)
2007 FFS 5yr (in 100s)	0.450***	-0.255	177	-0.030***	-0.021	0.089***
	(0.052)	(0.162)	(137)	(0.011)	(2.603)	(0.024)
Metro Pop (100k)	15.713*	-0.274	79	-0.032*	-2.169	0.030
	(8.116)	(0.350)	(299)	(0.019)	(5.702)	(0.061)
Metro Pop (100k) Sq	-1.989*	0.046	-9	0.005*	0.295	-0.002
	(1.102)	(0.050)	(43)	(0.003)	(0.781)	(0.008)
Cnty Pop (100k)	-1.388	0.660***	-631***	0.011	4.603	-0.026
	(4.446)	(0.230)	(205)	(0.013)	(3.496)	(0.025)
Cnty Pop (100k) Sq	0.806	-0.280***	145***	-0.001	-1.518**	0.009*
	(0.933)	(0.047)	(39)	(0.003)	(0.667)	(0.005)
Additional Controls				Year		
Counties				Metro 100-600k		
Mean	668.88	4.270	4,813	0.113	33.880	0.691
	(119.41)	(3.698)	(2,509)	(0.115)	(26.710)	(0.260)
N	4,608	4,608	3,535	4,608	2,975	2,975
R-squared	0.901	0.739	0.424	0.351	0.132	0.134

Notes: Table presents linear regression models, where outcome variables are measures of MA market structure and the financial characteristics of MA plans. All financial measures are inflation-adjusted, and represented in 2007 dollars. The triple interaction of Urban, Low FFS, and Post serves as the key instrument. The unit of observation is aggregated at the county-year, with aggregation weighed by plan enrollment. The regression results are weighed inversely with the number of counties in a metro area, such that each metro area is equally represented in the regression. The Post period corresponds to 2007-2011, while Low FFS counties correspond to those with 2007 5-yr FFS below the lowest floor. For counties with 2007 5-yr FFS in between the two floors, we scale the Low FFS coefficient accordingly. The original data is obtained from publicly available CMS files, including enrollment, landscape, OOPC, and other data. The market structure data covers the period from 1998-2000 and 2007-2011; meanwhile, the financial measures are only available for 2000, and 2007-2011. We restrict to counties in metros with population of 100-600k. We include a control for 5-yr per capita Medicare FFS spending, from 2007, and also include year-level indicators. We also include quadratic population controls, for counties as well as metros. Populations are stated in terms of 100k. Standard errors are clustered at the metro-level.

Table A.6: Plan Financial Characteristics Results, Not Weighted by Enrollment

VARIABLES	(1) Prem	(2) OOPC	(3) Premium+OOPC	(4) Rebate	(5) Drug Cov
Mean (Baseline Sample)	33.29	*362.90*	*396.19*	*53.50*	*0.570*
Urban	0.451	-12.512**	-12.060*	-0.575	-0.010
	(3.512)	(5.843)	(7.175)	(3.552)	(0.050)
2007 FFS 5yr (in 100s)	1.755	-5.876**	-4.121	1.677	0.038**
	(1.689)	(2.588)	(2.943)	(1.201)	(0.019)
N	1,701	1,701	1,701	1,360	1,701
Sample	*Baseline: 100-600k Metros, 2007-2011, 2007 FFS < 662*				
Urban	2.210	-12.946	-10.736	-1.627	-0.013
	(4.414)	(8.356)	(9.835)	(5.508)	(0.062)
2007 FFS 5yr (in 100s)	1.479	-12.743***	-11.264**	3.139	0.031
	(2.464)	(3.619)	(4.482)	(2.232)	(0.039)
N	711	711	711	568	711
Sample	*Robustness: Narrower Bandwidth Sample (150-350k Metros)*				
Urban	6.243	-1.547	4.696	2.903	-0.026
	(6.363)	(13.659)	(15.859)	(8.790)	(0.092)
2007 FFS 5yr (in 100s)	0.683	-12.081**	-11.398**	2.846	-0.012
	(3.012)	(5.099)	(5.517)	(3.444)	(0.050)
N	361	361	361	288	361
Sample	*Robustness: Narrower Bandwidth Sample (200-300k Metros)*				
Urban	0.231	-2.998	-2.767	-1.441	0.045
	(6.200)	(7.172)	(11.374)	(5.216)	(0.050)
2007 FFS 5yr (in 100s)	0.050	-4.753*	-4.703	3.073	0.023*
	(1.746)	(2.709)	(3.898)	(2.151)	(0.012)
N	1,108	1,108	1,108	886	1,108
Sample	*Falsification: High FFS Cnty Sample (2007 FFS > 662)*				

Notes: Table presents linear regression models, where outcome variables are financial characteristics of MA plans. All financial measures are inflation-adjusted, and represented in 2007 dollars. The unit of observation is aggregated at the county-year, for the 2007-2011 period, with the variables NOT weighed by plan enrollment. The regression results are weighed inversely with the number of counties in a metro area, such that each metro area is equally represented in the regression. The original data is obtained from publicly available CMS files, including enrollment, landscape, OOPC, and other data. We restrict to counties with associated metro pop of 100-600k. Further, we exclude counties whose adjusted FFS level-as of 2007-was above that of the lowest possible floor. Finally, we restrict to those counties within the specified population band. We include year-level indicators and also control for 5-yr per capita Medicare FFS spending, from 2007. We also include quadratic population controls, for counties as well as metros. Populations are stated in terms of 100k. Standard errors are clustered at the metro-level.

Table A.7: Additional Metrics

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	FFS Costs	Risk Scores	Star Ratings			
			Health Outcomes	*Chronic Care Mgmt*	*Cust Service*	*Plan Ratings*
Urban	1.11	0.021	0.147	0.099	-0.061	0.271*
	(14.03)	(0.025)	(0.145)	(0.144)	(0.116)	(0.139)
2007 FFS 5yr (in 100s)		0.065***	0.019	-0.025	0.027	0.029
		(0.009)	(0.026)	(0.025)	(0.026)	(0.026)
Metro Pop (100k)	26.97	0.013	-0.057	-0.035	0.078	-0.184
	(16.79)	(0.009)	(0.159)	(0.142)	(0.126)	(0.148)
Metro Pop (100k) Sq	-3.51	-0.003	0.007	0.001	-0.010	0.020
	(2.21)	(0.002)	(0.023)	(0.019)	(0.018)	(0.019)
Cnty Pop (100k)	7.52	0.005	0.176	0.225*	-0.011	0.054
	(14.81)	(0.029)	(0.129)	(0.121)	(0.075)	(0.141)
Cnty Pop (100k) Sq	2.13	-0.002	-0.037**	-0.028*	0.005	-0.024
	(2.02)	(0.004)	(0.019)	(0.017)	(0.012)	(0.018)
Additional Controls	Year					
Counties	Metro 100-600k, FFS 5 yr Below 662 (from 2007)					
Mean	616.84	0.886	3.147	2.460	3.760	3.300
	(56.14)	(0.081)	(0.570)	(0.570)	(0.630)	(0.580)
N	1,740	1,724	1,270	1,224	1,387	1,394
R-squared	0.11	0.197	0.139	0.281	0.268	0.213

Notes: Table presents linear regression models, where outcome variables include county-year level measures of MA & FFS enrollee composition, along with measures of MA plan quality. These include measures of per capita FFS spending, MA risk scores, as well as plan star ratings. FFS costs are inflation adjusted, and represented in 2007 dollars. The unit of observation is aggregated at the county-year. The regression results are weighed inversely with the number of counties in a metro area, such that each metro area is equally represented in the regression. The enrollee composition measures cover the 2007-2011 period, while the star ratings cover 2007-2010. The original data is obtained from publicly available CMS files, including enrollment, star ratings, and other data. We exclude counties whose adjusted FFS level-as of 2007-was below that of the lowest possible floor. We restrict to those counties within the metro population band of 100,000 to 600,000, also as of 2007. We include quadratic controls in county and metro-area population. We also control for 2007 per capita Medicare FFS spending, and include year-level indicators. Standard errors are clustered at the metro-area level

Table A.8: CAHPS Ratings, 150-350k Sample

	(1) Overall Health Plan	(2) Overall Healthcare	(3) PCP	(4) Specialist Seen	(5) Drug Benefits
Urban	-0.018	0.064	-0.081	0.074	-0.021
	(0.198)	(0.200)	(0.108)	(0.145)	(0.157)
2007 FFS 5yr (in 100s)	0.022	0.042	0.012	0.017	0.038
	(0.033)	(0.046)	(0.020)	(0.030)	(0.026)
Metro Pop (100k)	-0.026	0.295	-0.254	0.450	-0.257
	(0.930)	(0.736)	(0.414)	(0.594)	(0.623)
Metro Pop (100k) Sq	-0.001	-0.089	0.052	-0.111	0.038
	(0.181)	(0.150)	(0.080)	(0.118)	(0.116)
Cnty Pop (100k)	0.068	-0.014	-0.079	0.212	-0.122
	(0.242)	(0.225)	(0.170)	(0.200)	(0.197)
Cnty Pop (100k) Sq	0.029	0.089	0.079	0.003	0.093
	(0.066)	(0.065)	(0.049)	(0.059)	(0.057)
Standarized Treatment Effect			-0.002		
			(0.104)		
Mean	8.41	8.51	9.04	8.88	8.39
	(0.99)	(0.82)	(0.62)	(0.93)	(0.94)
N	650	642	635	598	622
R-squared	0.039	0.037	0.029	0.011	0.044

Notes: Table presents linear regression models, where outcome variables are enrollee-reported levels of plan quality, levels of utilization, and health status. The unit of observation is at the county-year level, for the 2007-2011 period. The original data is obtained from CMS, from the CAHPS survey of MA enrollees; while the data was originally at an individual respondent level, we aggregate this data to the county-year level for purposes of our analysis. The regression results are weighed inversely with the number of counties in a metro area, such that each metro area is equally represented in the regression. We exclude counties whose adjusted FFS level-as of 2007-was above that of the lowest possible floor. Finally, we restrict to those counties within the 150-350k population band. We include a control for 2007 per capita Medicare FFS spending and include year-level indicators. We also include quadratic population controls, for counties as well as metros. Populations are stated in terms of 100k. Standard errors are clustered at the metro-level. Standardized treatment effects are calculated consistent with the approach in Kling et al (2007) and Finkelstein et al (2012). All specifications include controls for age categories, race, and gender.

Table A.9: CAHPS Utilization and Health, 150-350k Sample

	(1) Specialist Visits	(2) Personal MD Visits	(3) Routine Visits	(4) SRH Overall	(5) SRH Mental Health
Urban	0.099	-0.111	-0.142	-0.087	0.006
	(0.100)	(0.159)	(0.137)	(0.115)	(0.116)
2007 FFS 5yr (in 100s)	-0.011	-0.042	-0.018	-0.013	0.007
	(0.032)	(0.028)	(0.030)	(0.016)	(0.016)
Metro Pop (100k)	0.237	0.202	0.417	-0.517	-0.267
	(0.394)	(0.597)	(0.597)	(0.426)	(0.453)
Metro Pop (100k) Sq	-0.059	-0.011	-0.060	0.130	0.061
	(0.078)	(0.123)	(0.112)	(0.082)	(0.088)
Cnty Pop (100k)	0.030	-0.206	-0.244	-0.196	0.032
	(0.115)	(0.199)	(0.258)	(0.201)	(0.180)
Cnty Pop (100k) Sq	-0.014	0.043	0.063	0.007	-0.022
	(0.037)	(0.063)	(0.077)	(0.056)	(0.055)
Standardized Treatment Effect				-0.057	
				(0.155)	
Mean	1.70	1.89	2.28	2.93	2.25
	(0.55)	(0.69)	(0.78)	(0.48)	(0.47)
N	602	645	651	652	652
R-squared	0.009	0.038	0.024	0.055	0.016

Notes: Table presents linear regression models, where outcome variables are enrollee-reported levels of plan quality, levels of utilization, and health status. The unit of observation is at the county-year level, for the 2007-2011 period. The regression results are weighed inversely with the number of counties in a metro area, such that each metro area is equally represented in the regression. The original data is obtained from CMS, from the CAHPS survey of MA enrollees; while the data was originally at an individual respondent level, we aggregate this data to the county-year level for purposes of our analysis. We exclude counties whose adjusted FFS level-as of 2007-was above that of the lowest possible floor. Finally, we restrict to those counties within the 150-350k metro population band. We include a control for 2007 per capita Medicare FFS spending and include year-level indicators. We also include quadratic population controls, for counties as well as metros. Populations are stated in terms of 100k. Standard errors are clustered at the metro-level. Standardized treatment effects are calculated consistent with the approach in Kling et al (2007) and Finkelstein et al (2012). All specifications include controls for age categories, race, and gender.

Table A.10: Pass-Through By HHI Quintile

	(1) Prem	(2) Rebate	(3) OOPC	(4) Premium+OOPC-Reb	(5) Drug Cov
Mean (Baseline Sample)	*31.96*	*54.80*	*364.92*	*348.82*	*0.66*
Urban	-15.36	7.03	-44.30**	-58.683***	0.119
	(15.80)	(6.17)	(17.03)	(21.717)	(0.086)
2007 FFS 5yr (in 100s)	-7.81	5.40	-6.00	-20.071	0.123***
	(11.57)	(4.12)	(7.29)	(12.403)	(0.043)
Observations	348	278	348	278	348
Restriction:			First Quintile		
Urban	0.36	0.08	-7.69	-3.245	-0.041
	(7.35)	(6.54)	(15.75)	(22.970)	(0.107)
2007 FFS 5yr (in 100s)	9.16***	-0.14	-13.44**	-5.842	0.099**
	(3.07)	(2.23)	(5.33)	(7.836)	(0.044)
Obs	338	271	338	271	338
Restriction:			Second Quintile		
Urban	-0.30	11.51*	-3.21	-9.213	-0.289**
	(7.31)	(5.96)	(14.67)	(20.461)	(0.110)
2007 FFS 5yr (in 100s)	3.75	-7.83**	-6.49	7.310	-0.013
	(6.40)	(3.48)	(8.04)	(13.406)	(0.065)
Obs	343	274	343	274	343
Restriction:			Third Quintile		
Urban	14.78*	0.50	-8.80	0.587	0.077
	(8.12)	(8.49)	(19.41)	(28.537)	(0.102)
2007 FFS 5yr (in 100s)	6.10	-3.16	-9.17	-1.179	-0.006
	(5.04)	(5.60)	(11.08)	(18.689)	(0.053)
Obs	333	266	333	266	333
Restriction:			Fourth Quintile		
Urban	-3.34	-8.17	-2.93	9.867	0.078
	(15.82)	(14.35)	(20.40)	(31.257)	(0.217)
2007 FFS 5yr (in 100s)	-11.90*	-0.14	-7.84	-22.905**	0.203**
	(7.01)	(3.74)	(7.85)	(11.018)	(0.086)
Observations	339	271	339	271	339
Restriction:			Fifth Quintile		

Notes: Table presents linear regression models, where outcome variables are financial characteristics of MA plans. The panels present results for different counties, based on the quntile in which their HHI falls. The unit of observation is aggregated at the county-year, for the 2007-2011 period, with the variables weighed by plan enrollment. The regression results are weighed inversely with the number of counties in a metro area, such that each metro area is equally represented in the regression. Financial measures are inflation adjusted, and represented in 2007 dollars. The original data is obtained from publicly available CMS files, including enrollment, landscape, OOPC, and other data. We exclude counties whose adjusted FFS level-as of 2007-was above that of the lowest possible floor. Finally, we restrict to those counties with metro pop of 100-600k. We include year-level indicators and also control for 5-yr per capita Medicare FFS spending, from 2007. We also include quadratic population controls, for counties as well as metros. Populations are stated in terms of 100k. Standard errors are clustered at the metro-level.