The Efficiency Consequences of Health Care Privatization: Evidence from Medicare Advantage Exits

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Abstract

There is considerable controversy over the use of private insurers to deliver public health insurance benefits. We investigate the efficiency consequences of patients enrolling in Medicare Advantage (MA), private managed care organizations that compete with the traditional fee-for-service Medicare program. We use exogenous shocks to MA enrollment arising from plan exits from New York counties in the early 2000s, and utilize unique data that links hospital inpatient utilization to Medicare enrollment records. We find that individuals who were forced out of MA plans due to plan exit saw very large increases in hospital utilization. These increases appear to arise through plans both limiting access to nearby hospitals and reducing elective admissions, yet they are not associated with any measurable reduction in hospital quality or patient mortality.

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The Medicare program, which currently provides nearly universal health insurance coverage to 55 million elderly and disabled U.S. residents, was introduced in 1965 as a monopoly public insurance product that was run and financed by the Federal government (Commonwealth Fund, 2015). Over time, it has evolved into a hybrid of public insurance and publicly financed private insurance, along two channels: the Medicare Advantage (MA) program (Part C) and prescription drug coverage (Part D). The MA program allows Medicare recipients to enroll in a private health insurance plan (a Medicare HMO, PPO, or other coordinated care plan), which is then reimbursed by the federal government. The Part D program allows Medicare recipients to choose from a variety of private prescription drug insurance plans (KFF, 2014). More than 40 million Medicare recipients are now enrolled in Medicare Part C or Medicare Part D (CMS 2015).

The growing privatization of Medicare has been motivated by potential efficiencies from the 'care management' provided by private insurance companies. This is a particularly interesting topic in the context of Medicare Advantage, where private insurers provide coverage side-by-side with the government system. Evidence on the relative efficiency advantages of private Medicare Advantage plans, however, has been mixed (McGuire et al., 2011).

This mixed evidence arises from two key challenges faced by the previous literature. The first is the endogeneity of MA enrollment among seniors, for whom this is a choice – individuals have the option to enroll in or disenroll from an MA plan. There is a large body of past evidence which suggests that individuals don't enroll randomly into MA, but rather do so based on health status, leading to potential selection bias when evaluating the impact of MA (Morrissey et al 2012, Brown et al 2014). The second is the limited availability of data for those who are enrolled in MA. The Medicare claims data that is typically used for empirical work in this area only tracks utilization for those enrolled in the traditional FFS program, and not those in MA.

The purpose of our paper is to address these empirical concerns with two innovations. The first is to use hospital discharge data from New York State, which allows us to examine the

health care utilization of Medicare recipients both inside and outside of Medicare Advantage. A major advantage of these hospital data is that we obtained permission to longitudinally link it at the individual level to Medicare enrollment files, so that we can assess how an individual's utilization changes leading up to and following changes to that individual's MA enrollment status.

The second is to use these novel data to identify the causal impact of MA plan enrollment by studying counties in which MA plans completely exited in the early 2000s, and comparing them to counties where there was no exit. In these counties, enrollees who were previously in MA plans had no choice of remaining in MA, so our data allows us to study the utilization impact of moving exogenously from MA plans to the FFS Medicare program.

Doing so, we find that there is a substantial rise in inpatient hospital utilization after MA plan exit. We estimate that previous MA enrollees see their utilization of the hospital rise by about 60%, when moving back to the traditional FFS plan. This estimate is comparable to the corresponding estimate of 65% from the RAND Health Insurance Experiment of the 1970s, which randomly assigned patients to managed care plans. The finding is robust to specification checks and appears to be long-lasting, so that it does not simply reflect pent-up demand that caused a temporary increase in utilization. The increases appear across all types of hospitalizations, but are particularly pronounced for elective visits. We also find substantial reductions in the average distance traveled to the hospital when patients exogenously switch from MA to FFS following plan exit. This suggests that the mechanisms for lowering costs under MA plans are both reduced hospital availability and greater restrictions on elective care.

At the same time, we find no evidence to suggest that the quality of care is rising along any dimension. We find no change in the quality of hospitals used by enrollees as measured by typical Medicare metrics, and more significantly we see no reduction in mortality among those who are forced to move from MA to FFS. This suggests that MA plans were delivering care more efficiently than the FFS Medicare program by using fewer hospital resources.

Our findings therefore have important implications for Medicare and suggest that increased management of hospital care could lower costs without reducing the quality of care.

Our paper proceeds as follows. Part I provides background on the Medicare Advantage program and reviews the previous literature on MA. Part II describes our data. Part III explores the impact of plan exit on utilization and outcomes. Part IV discusses the implications of the findings for Medicare policy. Part V concludes.

1 Background on Medicare Advantage

Since 1982, Medicare recipients have had the option to enroll in private managed care plans. Enrollment in the plans has fluctuated in response to changes in the generosity of plan reimbursement and has varied substantially across geographic areas at any point in time (McGuire et al 2011). Throughout the 1980s and 1990s, plan payments for an enrollee were set to be 95 percent of a county's per-capita Medicare FFS expenditures, and were further adjusted based on the recipient's age, gender, disability status, Medicaid enrollment status, and nursing home status (Chaikind et al 2004). The program's name changed over time, beginning as Medicare managed care and then changing to Medicare+Choice in 1997 and then to Medicare Advantage after 2003. In the pages that follow, we refer to Medicare Part C as Medicare Advantage.

Research demonstrated that individuals enrolling in Medicare managed care plans tended to have significantly lower costs than the average and thus plan contracting actually increased Medicare spending.[2] In response to this, legislation was enacted reducing the future growth rate of private Medicare reimbursement, as part of the 1997 Balanced Budget Act. In this same legislation, the government introduced payment "floors" in counties with low per-capita FFS expenditures given substantially lower private Medicare penetration in those areas. The

[2] Studies from the mid 1990s found that utilization among private Medicare enrollees was 12% (Riley et al 1996) to 37% (PPRC 1996) lower than those of demographically comparable enrollees in FFS. While some of this could reflect treatment effects rather than selection, the PPRC study actually compared the two groups, during the time that both were still in FFS (they focused on the 6 months immediately preceding HMO enrollment).

Benefits Improvement and Protection Act of 2000 further increased payment floors in urban counties that had low per-capita FFS expenditures, as described below (Chaikind et al 2004). Despite these changes, private Medicare enrollment of 5.3 million in 2003 was approximately equal to its 1997 level (5.4 million) (KFF 2014); the increases in enrollment in floor counties were approximately offset by lower enrollment in other counties. These differential trends in enrollment were driven by the more modest reimbursement growth across non-floor counties.[3]

A large body of previous research has investigated the effect of Medicare Advantage on health care expenditures, the utilization of medical care, and health outcomes (see McGuire et al, 2011 for an excellent review). One challenge when estimating these effects is the endogeneity of MA enrollment – individuals have the option to enroll in or disenroll from an MA plan. To account for this, previous studies have taken a variety of approaches. One subset of research has estimated cross-sectional models that include a rich set of controls for individual's age, health status, and related factors, assuming that there are no remaining unobserved differences between those who choose to enroll in managed care and those who do not (Landon et al, 2012). Another branch of studies has used instrumental variable approaches, with their methods assuming that certain factors (e.g. MA penetration in the local market) influence plan choice but do not affect utilization (Mello et al, 2002). A final strand of the literature has used longitudinal data to follow individuals over time and compare the evolution of Medicare spending or other outcomes of interest among those switching between MA and traditional Medicare and those not switching; Brown et al (2014) examine

[3]Partly because of the stagnant MA enrollment growth, in 2003 the Medicare Modernization Act raised reimbursement in all areas. The government also moved to a system of risk adjustment that began in the early-2000s and that paid plans more for individuals with diabetes, pneumonia, or other medical conditions. In 2006 the government moved to a bidding system whereby plans could submit a bid for expected costs of providing coverage to recipients, which is comparable to traditional Medicare in scope. If a plan's bid fell below county-level benchmarks, the plan would rebate $\frac{3}{4}$ of the difference to enrollees in the form of enhanced benefits or reduced premiums, while the government would keep the remainder. If a bid was above the benchmark, the recipient would pay the full difference between the bid and benchmark, in the form of higher premiums (Chaikind et al, 2004). The Affordable Care Act has further transformed plan reimbursement by gradually reducing benchmarks from 2011 to 2017, with the largest reductions occurring in counties with the highest levels of per-capita FFS spending (Biles et al, 2012). This increase in reimbursement has led to a steady rise in Medicare Advantage enrollment since 2004, and it now stands at more than three times as many people (17 million) and more than twice as large a share of enrollment (31%) as 2004. Data available at http://kff.org/medicare/fact-sheet/medicare-advantage/

cases of voluntary switching, while Parente et al (2005) examine cases of switching following plan exit. Critically, plan exit in the latter study is incomplete, meaning that individuals can still remain in MA by switching to a plan that remains active; as such, in both cases, the switching decision between MA and traditional Medicare remains endogenous.

Altogether, the findings from this research are mixed, with most finding that Medicare Advantage does reduce utilization; however, it is difficult to disentangle these estimated effects from favorable selection into MA plans (Mello et al, 2003).

A related area of research has investigated the effect of plan reimbursement on MA enrollment and on the average characteristics of MA enrollees. These studies have exploited variation over time (Cawley et al, 2005; Afendulis et al, 2013) and across geographic areas (Cabral et al, 2014; Duggan et al, 2014) in the generosity of plan reimbursement and find that there is a strong positive effect on MA enrollment.

A third area of research has considered the effect of Medicare Advantage (or managed care among the privately insured) on utilization for those enrolled in traditional Medicare. The likely mechanism for such an effect is that health care providers may change the way that they treat individuals enrolled in traditional Medicare as more of their patients become enrolled in managed care (Glied and Graff Zivin, 2002). The results from this research suggest that reimbursement-induced increases in Medicare Advantage enrollment reduce utilization among those in traditional Medicare and that this effect partially offsets the greater spending on Medicare Advantage enrollees (Baicker et al, 2013; Afendulis et al. 2013).

There is a broader literature which has evaluated the impact of managed care on health care utilization. This literature follows the same type of approaches discussed above, such as controlling for observable differences across patients in FFS and managed care (Cutler et al 2000) and instrumenting for managed care enrollment using area factors such as the area penetration of managed care plans (Baker, 2000). These studies typically find that managed care plans lower utilization, but are subject to the caveats noted above.

There is, however, one source of exogenous experimental variation, which is an arm of the

famous RAND Health Insurance Experiment of the 1970s. Best known for the randomization of individuals across health insurance plans of differential generosity, the RAND HIE also randomized one set of individuals into the Group Health Cooperative of Puget Sound (an HMO) and another set into a fee-for-service plan (Manning et al., 1987). This study found very large reductions in inpatient care in the managed care plan, with roughly 65% higher inpatient utilization under FFS relative to managed care. At the same time, outpatient utilization was comparable across the two settings (Manning et al, 1985; Manning et al, 1987).

2 Data and Empirical Strategy

We use administrative datasets from CMS and New York State, which contain information on Medicare & Medicare Advantage enrollment status, individual-level utilization metrics for those in MA as well as FFS, and individual-level mortality indicators. Our ability to track individual-level hospital inpatient utilization in Medicare Advantage is unique, as is our ability to continuously track individual-level utilization for those switching between MA & FFS. The dearth of available Medicare Advantage claims data has hindered past research, and is an issue that we overcome here. In this section, we discuss the various data sources used for this analysis and sample selection restrictions imposed. Further details are provided in the Data Appendix.

2.1 Medicare & MA Enrollment Data

We obtain administrative Medicare data from CMS, in the form of a denominator file containing Medicare & Medicare Advantage enrollment status at a person-month level. This denominator file covers every person enrolled in Medicare, at any point during the 1998-2003 period, and is national in scope. This data also contains information on the demographic characteristics of each enrollee, including birth date and age, gender, race, state of residence,

and county of residence.

As the Medicare Denominator data only identifies overall MA enrollment status, and not the specific plan to which an individual may belong, we supplement the data with public-use files from CMS, containing national Medicare Advantage enrollment information at a plan-county-year level. Using this public-use file, we are able to identify the extent to which any US county experienced plan-exit, along with the timing of that exit. Specifically, we are able to identify those counties experiencing complete plan-exit, and the years in which this took place.

2.2 New York Utilization Data

Our primary measures of health care utilization relate to the inpatient setting, and cover New York State. These measures are compiled for all those in Medicare, including MA enrollees, by linking specialized New York State discharge-level hospital data to Medicare Denominator data. This linking is performed using Social Security Numbers, which are contained in both of the datasets (the inclusion of these SSN fields in the data required administrative permission from CMS as well as New York State).[4] Through this linking, we can construct an individual-level panel of inpatient hospital utilization, for the 1998-2003 period across New York State. This panel covers all individuals in Medicare, irrespective of whether they happen to be enrolled in Medicare FFS or Medicare Advantage at any given point, since hospitals compile uniform data across all payers. Given our approach for constructing this panel, individuals are included in the sample even if they've had no hospital utilization throughout the study period. With this data, we can bypass issues of sample selection that would plague any analysis that uses just stand-alone hospital discharge data.

We aggregate our measures of hospital utilization to the person-year level. We focus

[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 Medicare recipients over 99.9% of the time. The Medicare recipients that were not uniquely identified were excluded from the sample.

on cumulative, yearly metrics of the following: number of visits, number of days stayed, number of procedures performed, and the log of hospital charge amounts.[5] The means of these utilization measures, for our two cohorts of interest, are presented in Table 1. Among those initially in Fee-for-Service Medicare, these measures appear to be at least 60% higher than among those initially in Medicare Advantage; however, the extent to which this disparity is driven by patient composition, rather than by treatment differentials, is not readily discernible.

Unfortunately, we are unable to include outpatient data, as it is not collected in the same way in New York State. This limits our ability to speak to the impact of Medicare Advantage on total medical spending. That said, studies of non-Medicare HMO's have found no meaningful HMO effect on outpatient utilization, relative to a fee-for-service alternative (Manning et al 1987; Manning et al 1985).

2.3 Mortality Data

We use fields in CMS's Medicare Denominator data, to construct person-year mortality indicators. These data are national in scope, and cover the entirety of our sample period. In constructing our sample, we allow for sample attrition through mortality; as such, if an individual dies in 2002, their mortality indicator will be positive for that year, and the individual will not appear in the sample in the following year.

2.4 Sample Restrictions

We focus on the 1998-2003 period, given that subsequent increases to MA reimbursement resulted in a re-entry of plans to many counties that had previously experienced exit, with virtually no counties having complete exit of MA plans after this period. We restrict to Medicare recipients over 65, and restrict to those who were originally eligible for Medicare

[5] To avoid the undue influence of outliers, total charges for those above the 98th percentile are winsorized (thus replacing the charge value with the 98th percentile value).

by virtue of age, rather than disability. We also restrict to those already in Medicare in 1998; as such, we exclude those who aged into Medicare later in the study period. This allows us to construct a baseline measure of utilization for every individual in our sample at least two years before any of the MA exits that we study occurred. We construct cohorts based on individuals' Medicare Advantage/FFS enrollment status at the start of the study period, to combat bias from voluntary switching between the two at a later point.

Throughout our plan-exit analyses, our treatment group is made up of eight counties that saw complete plan exit, over either a one or two year period. Altogether, these counties accounted for about 3% of all Medicare Advantage enrollees in New York State, prior to plan-exit, and likewise accounted for about 7.5% of all FFS recipients. These eight exit counties have MA penetration rates of 6.7% on average, as opposed to an overall NY average of 15.2%. In six of the counties, exit is over a one year period; in the other two counties, it is over a two-year period. The exiting plans are a mix of national for-profit carriers (the largest enrollment was in Aetna) and local non-profit carriers (the second largest was in the Capital Area Community plan); altogether there were six plans exiting. Some carriers exited all New York counties in which they operated, while other carriers selectively exited certain counties and continued being active in others.

While we cannot fully explain plan exit, one cause of exit was clearly low reimbursement rates. A sizeable literature finds that the MA share of Medicare enrollment is strongly related to MA reimbursement rates (Afendulis et al 2013, Cawley et al 2005, Pope et al 2006). In the Appendix, we demonstrate that reimbursement changes over this period are strongly associated with the type of plan exits that we study. In particular, we find that each $100 per month rise in MA reimbursement leads to around a 5% decrease in the number of enrollees in exiting MA plans, as a fraction of Medicare recipients in that county. We are unable to use reimbursement changes as instruments for plan exit, however, as they have direct effects on the treatment of MA patients even in counties that do not see exit, and perhaps even spillover effects on the treatment of FFS patients. But we demonstrate below

that plan exit appears to be an exogenous shock to the counties that we study.

2.5 Empirical Strategy

As reviewed above, enrollment in an MA plan results from endogenous decisions by seniors that may be correlated with their health status. Thus any comparison of those who do and do not choose to join MA plans may be biased. Our approach instead is to look at a sample of individuals who exogenously lose access to MA plans: seniors residing in counties where all available MA plans have exited. Such seniors have no option of choosing an MA plan. For seniors who were previously enrolled in an MA plan, this results in an exogenous shift out of MA into FFS care. As part of our main approach, we do not consider "partial" plan exits, where some plans leave a county but others remain, due to the endogeneity of the decision to remain in an MA plan. The two counties with partial plan exit during our study period (Nassau and Suffolk) are excluded from the analysis.

We use the data described above to estimate regressions of the following form:

$$(1) \qquad UTIL_{ijt} = \alpha + \beta * Exit_{jt} + X_{ijt} * \gamma + \pi_j + \mu_t + \varepsilon_{ijt}$$

Where i indexes individuals, j counties and t years; UTIL is one of our measures of utilization and/or quality; EXIT is a dummy for whether the MA plans have exited county j in year t; X is a limited set of demographic controls (5 year age group dummies and gender); and π_j and μ_t are a full set of county and year fixed effects. For the two counties that exit over two years, the "EXIT" variable takes on a value of 0.5 in the first year and 1.0 in the second year. All standard errors are clustered at the county level.

3 Results

As discussed in Part II, here we examine the impact of plan exits on the utilization of health care. Our basic results are illustrated in Figure 1. This figure shows the trend in the

average annualized number of hospital admissions for those who are initially in MA plans in New York counties. The red line shows the number of visits for those who are in counties that do not see MA plan exit over this period, while the blue line shows visits for those in counties where MA plans exit. Both are trending up over time because the sample is aging given that we restrict to individuals who are 65 years old and up in our base year 1998. This graph includes counties in which exit occurred in both one year and over two years – the blue vertical line marks the start of the first year, while the red vertical line marks the start of the second year.

There is a steady upward trend for both sets of counties, but an enormous jump up for counties in which MA plans exit around the time of that exit. The trend is more rapid during the first year as the two year-exit counties are fully integrated, and then trends return to parallel. This previews our finding of a robust increase in inpatient utilization among those initially enrolled in MA in exit counties.

The regression analysis of the impact of plan exit is shown in Table 2, for the sample of individuals who are initially in an MA plan. We estimate the change in utilization in counties that see plan exit versus those that do not while controlling for a full set of county and year indicators. The coefficient of interest is multiplied by an indicator for being in an exit county interacted with the period after exit, controlling for a full set of county dummies and year indicators. Further, the standard errors for all our regression results are clustered at a county-level, to control for possible within-county serial correlation, since that's the level at which plan exit varies. Altogether, the results confirm the implications of Figure 1: there are very sizeable increases in utilization along every dimension.

In particular, we show that those MA enrollees who see plan exit in their county (and who therefore move to FFS Medicare) see their number of hospital admissions rise by an average of 0.105; relative to the ex-ante mean of 0.177, this represents an increase of approximately 60 percent. Total hospital days rise by 0.65 (48%), and the number of hospital procedures

rise by 0.13 (33%). Total charges rise by 53%.[6]

The results therefore suggest that exit of MA plans led to very sizeable increases in hospital utilization by former MA enrollees, with an estimated magnitude that is comparable to findings from the RAND Health Insurance Experiment (Manning et al 1987; Manning et al 1985). The rise in utilization appears to be mostly along the margin of admissions, with proportionally smaller increases in days and in the number of procedures. Given that sicker or more severely injured patients will tend to remain in the hospital for longer, this suggests that the marginal admission is for a less serious condition.

3.1 Specification Checks

We further explore these findings in Table 3, where we consider robustness tests along two dimensions. First, we present a specification that includes both lags and leads of the exit effect. The lead coefficient allows us to test for differential trends across treatment and control counties. The lags allow us to address the important question of whether these large effects simply represent pent-up demand by those who were treated less intensively under MA plans, which would then fade over time as enrollees become acclimated to the FFS environment.

The results of this specification are shown in the first panel of Table 3. We find no significant lead effect, consistent with no differential pre-trends across these different types of counties.[7] In addition, we find that the estimated utilization response occurs quickly and gets slightly stronger over the first three years. This is inconsistent with a pent-up demand

[6] We add 1 to charges given that nearly 90 percent of person-year observations have zero charges and would otherwise be dropped from the analysis. If we reestimate the model in the level of charges, including zeros (but allowing perhaps undue influence of outliers), we obtain an estimate that is similar in percentage terms.

[7] In a separate analysis, we find no evidence of increased attrition from exiting plans, in the months immediately preceding exit. The lack of attrition increases can be attributed to a number of factors. First, information on plan exits only became publicly available 3-4 months preceding exit; plans typically drop out at the end of each year, while upcoming-year plan availability is only made public in September of the previous year. In addition, there are some individual-level restrictions on MA-to-FFS switching, which typically can only be undertaken during open enrollment periods. Finally, there is substantial inertia in MA enrollment more generally.

explanation, at least over this three year window.

Another concern is that, given the relatively small number of exit counties (eight), there may be some other correlated factor that is changing at the same time as plan exit. To address this concern, we reestimate our models on the sample of FFS Medicare enrollees in these same NY counties over this same period. These enrollees should be impacted by other factors that impact medical demand or supply over this period, but should be largely unaffected by the MA exits. Of course, to the extent that there are important spillovers from MA onto treatment of FFS, then the reduced presence of MA in these counties could lead to increased treatment of FFS beneficiaries. But such an effect would be biased in the same direction as our findings, with those enrolled in FFS initially also using more care when MA plans exit.

The second panel of Table 3 shows the results of this exercise. In fact, we find no significant or sizeable impacts on those enrolled in FFS in our baseline year of 1998 in the counties with plan exit. This suggests that there are not broad trends towards less efficient care in this set of counties (as well as no significant spillovers) and that we are therefore accurately capturing the effect of MA enrollment.

3.2 Mechanisms

The striking increase in medical utilization from MA plan exit raises the question of how MA plans are able to restrain hospital inpatient utilization so effectively. In this section we explore the effects on several additional outcome variables, which point to the mechanisms through which managed care plans are restricting utilization.

One possible driver of utilization differences between MA and FFS could be cost-sharing differentials. The sign of these differentials is unclear ex-ante, however. This depends both on what individuals do for supplemental Medicare coverage when they lose their MA coverage, and on how generous that alternative is relative to Medicare Advantage.

We have investigated this issue using data from the Medicare Current Beneficiary Survey

(MCBS), which gathers data for a large nationally representative set of Medicare enrollees on their insurance coverage and medical spending over a two year period. We find that among those leaving Medicare Advantage over the 1998-2003 period, 29% chose not to purchase any supplemental coverage over the next year and therefore face the full extent of Medicare inpatient cost sharing (which is a large deductible for the first sixty days and a daily copayment after that). On the other hand, 21% obtained supplemental coverage through employer-sponsored insurance, 36% purchased supplemental coverage through the Medigap program, and 13% obtained supplemental coverage through government sources (Medicaid or the Military's Tricare program).

Turning to the generosity of coverage, we find that those in the MCBS with no supplemental coverage bear on average 9.3% of their inpatient hospital bills. On the other hand, those with some type of supplemental coverage bear about 2.5% of their bills, and that this is almost completely invariant to the type of coverage. Regression estimates of inpatient share of costs on dummies for insurance type show no significant difference among these supplemental alternatives, with or without controls. Therefore, on net, cost sharing among those leaving Medicare Advantage went up, which should if anything be reducing inpatient utilization (the opposite of what we find).

Another mechanism is through restricting choice of hospital, thereby eliminating the marginal hospitalization (which would be consistent with the severity results above). To assess this point, we measure the impact of MA plan exit on distance traveled and travel time to the hospital using the latitutude and longitude of the Medicare recipient's zip code of residence and the zip code of the hospital. As discussed in the Appendix, these distance/time calculations reflect driving rather than "as the crow flies" distances.

Table 4 shows the results for distance traveled. The sample here is restricted to those who actually use the hospital, reducing our sample size. We find that MA plan exit is associated with a sizeable reduction in distance traveled to the hospital: the average hospitalization is almost 5 miles and 7 minutes closer in driving time. These represent 76% and 39% of the

sample means, respectively.[8] Clearly, enrollees are taking advantage of the less restrictive network under FFS Medicare after MA plan exit. These results are robust to the inclusion of DRG fixed effects, suggesting that they are not driven by hospital visit composition differences across MA and FFS.[9]

Another source of reduced hospitalization under MA plans could be fewer hospitalizations among the least sick enrollees. To assess this we next explore the change in the types of hospitalizations that take place when MA plans exit. We look at a variety of different types of hospitalizations, and in each case we can compare the relative effects to the roughly 60% overall rise in hospital visits. The results described below are, again, robust to the inclusion of DRG fixed effects, suggesting that they are not driven by changes to visit composition.

We begin by looking at two different types of admissions. The first is "emergency" hospitalizations, which are inpatient admissions that initiate in the emergency room. We find that the proportional effects for emergency care (at 27%) are about half the magnitude of the full sample results (at around 60% jump). In the rows that follow, we divide hospitalizations into those that are elective and non-elective, as specified in the discharge data, which defines elective admissions as those where "the patient's condition permits adequate time to schedule the admission based on the availability of a suitable accommodation". We find that there is a much larger proportional rise in elective hospitalizations, which increase by 131% of their baseline value after MA plans exit. This is in contrast to non-elective hospitalizations, which rise by less than half (46%) of their baseline value.

Indeed, as the next set of rows show, there is a much larger proportional rise in the inten-

[8] One concern with this set of estimates is that MA affects the composition of hospitalizations. To the extent that the marginal admissions are to hospitals that are close to the patient's home, this would tend to mechanically lower the average distance when patients return to FFS. But given the magnitude of the decline in average distance, this change in composition would not be sufficient to explain the difference even if the average distance for marginal admissions was zero.

[9] Recent work on HMO's in the Medicaid setting provides further indication that distance to nearest hospital could be a driver of the effect estimated here; in New York's Medicaid program, the FFS option is not associated with reduced distance to the nearest hospital, and produces only 30% higher inpatient utilization (Vabson 2015), compared to the 60% increase that we estimate for Medicare. As such, the greater effect of HMO's under Medicare could be accounted for by a greater effect on distance to hospital (and other aspects of hospital networks).

sity with which elective hospitalizations are treated. The number of procedures performed rises by 94% for elective admissions, and only by 18% for non-elective ones.

These results therefore suggest two important mechanisms through which MA plans reduce hospital utilization. The first is to restrict patients to hospitals that involve considerably longer travel. The second is to more tightly restrict elective and non-urgent hospitalizations.

In Table 5, we examine these mechanisms in further depth, by comparing the effect of plan exit based on individuals' ex-ante distances to in-network MA hospitals. MA enrollees living close to in-network MA hospitals would experience a smaller decrease in hospital distance, following plan exit, compared to MA recipients living farther away. We break out our baseline sample of those initially in MA into two cohorts, based on each individual's distance to their nearest in-network MA hospital (pre-plan exit): the closest 50% and the farthest 50%. We measure whether a given hospital is in-network based on whether enrollees of MA plans in the corresponding county visit it at greater than de minimus rates.[10] We find that in these exit counties, 45% of hospitals on average (and 47% of hospital beds) are classified as in-network.

As the first column of Table 5 shows, both of these cohorts experienced a reduction in distance traveled to the hospital following plan exit, although obviously the reduction is larger in the group that lives farther from an in-network hospital. We also find that the cohort that was closer to an in-network hospital experienced a relatively larger increase in utilization, following plan-exit. The higher level of visits is driven almost entirely by a higher level of admissions from the emergency room. This suggests that restrictions on emergency admissions could have a disproportionate impact on those who would most likely use the ER, in this case those living closest to the hospital. The results further suggest that a higher threshold on admissions from the ER could be a more likely mechanism for

[10] For these purposes, we define de minimus as receiving fewer than 4% of MA hospital admissions, for a given county. In markets with exiting plans, there typically are only a limited number of hospitals, each of which would enjoy substantially higher market share, in the absence of network restrictions (we confirm this by looking at visits under FFS). We do not set these thresholds to zero, given that individuals will go to out-of-network hospitals even in the presence of network restrictions.

utilization reductions under MA, than increased distances to hospital.

3.3 Quality Impacts

If the exit of MA plans is causing such a substantial increase in utilization, a natural question is whether this is delivering benefits to enrollees through higher quality care or improved health outcomes. We explore this issue in Table 6 by examining a broad variety of quality indicators.

To measure the quality of care at the hospital level, we turn to two sets of standardized measures from the CMS Hospital Quality Initiative database. The first set of metrics consists of process measures, which are featured prominently as part of CMS's Hospital Compare tool; these capture the fraction of the time that a hospital follows 'best-practices', in the treatment of a listed condition. Possible best practices include the administration of beta blockers or antibiotics, for such conditions as heart failure, heart attacks, and pneumonia. Altogether, for this set of measures, higher ratings would imply better quality of care.

Meanwhile, the CMS Compare outcome measures are risk-adjusted mortality and readmission metrics for each hospital; these reflect the percentage of individuals dying/being readmitted in the 30-day period following discharge, for the following separate conditions: heart attacks, heart failure, and pneumonia. As such, these metrics are conditional on initial hospitalization. Altogether, for these measures, higher ratings would imply worse quality of care.

Using these measures, we do not see any consistent evidence of moving to higher quality hospitals, as seven of the nine measures are insignificant; further, one of the significant coefficients suggest higher quality (improved process for pneumonia) while the other suggests lower quality (worse outcomes for heart failure). Moreover, all of the coefficients are very small relative to mean values and precisely estimated, ruling out meaningful impacts.

We next turn to more direct process measures of outcomes created from our discharge data. One such measure, the 60-day hospital readmission rate, can proxy for quality given

that many readmissions result from either ineffective in-hospital or ineffective post-hospital care (Neal Axon et al, 2011). Another measure, preventable hospitalizations, identified those hospitalizations that are avertable under adequate outpatient care, such as visits involving chronic conditions. We identify these preventable hospitalizations using AHRQ's PQI algorithm, which works off the DRG codes and procedures associated with a given admission (DHHS, 2001). For our analyses, we aggregate the readmission and preventable hospitalization measures at a person-year level. Both of these measures are conditional on hospitalization, so that we can assess whether under FFS the marginal hospitalization is more likely to be a readmission or be preventable. As shown in Table 1, the number of readmissions is higher among those initially in FFS than in the initially MA cohort, consistent with the selection evidence discussed above, although the number of preventable hospitalizations is lower.

When MA plans exit, we find that both measures rise – that is, plan exit does not appear to be translating to more efficient care on net that is lowering readmissions or preventable admissions. The odds of readmission, conditional on an initial hospitalization, rises by about 15% among those initially in MA plans after plans exit. Meanwhile, the odds of a given hospitalization being preventable rises by 10%. By these measures, therefore, quality is falling for those initially enrolled in MA following the exit of MA plans.

Finally, we examine the impact on mortality. For measuring mortality, we can extend our analysis to consider not only the impacts in New York, but across the nation as a whole.[11] This allows us to substantially increase the precision of our estimates on exit effects.

The effects on mortality are shown at the bottom of Table 6. Both estimates are in fact positive, suggesting that plan exit leads to higher mortality, although neither estimate is significant. Most importantly, we can rule out a meaningful reduction in mortality associated with the higher hospital utilization under FFS plans. Even with the less precise New York

[11] In New York State, there were 8 counties in which plans completely exited (which comprised our treatment group), and 52 counties in which plans did not exit (which comprised our control group), along with 2 partial exit counties (which were dropped). Across the nation as a whole, the comporable figures are 401 complete exit counties, 2373 non-exit counties, and 430 partial exit counties.

only data, we can rule out a reduction in mortality rates in excess of .35% (with 95% confidence) from a baseline of 4.1%; with the more precise national data, meanwhile, we can rule out a reduction in excess of .10% (and also rule out an increase in excess of .14%), off a baseline of 4.4%. Given that utilization of the hospital goes up by more than 60%, this is a fairly tight bound.

The results from this section appear to indicate that there is a sizeable inefficiency in transitioning elders out of Medicare Advantage into the FFS program. Utilization of, and spending in, the hospital rises substantially, with no consistent indication of quality improvement (although travel to the hospital is greatly reduced). If anything, we find a reduction in quality, with readmissions, preventable hospitalizations and mortality (the last insignificantly) increasing after the shift out of managed care plans.

4 Conclusions

The role of private players in public insurance is the subject of a central debate in U.S. public policy. This debate is perhaps most heated around the role of Medicare Advantage plans. Advocates claim that the higher efficiency of such private options should push the government towards expanding the role of managed care plans. Opponents point to the sizeable positive selection faced by these plans (and their high baseline reimbursement, even independent of selection) to claim that they are over-reimbursed and are costing, rather than saving, government dollars.

Central to this debate is the question of whether MA plans actually deliver care more efficiently. Our paper contributes to the literature on this point in two important ways. First, we make use of data that tracks the treatment of both traditional Medicare (FFS) recipients and MA enrollees. Second, we make use of exogenous variation in MA availability, arising from county-level exit of MA plans. Using these empirical advantages, we document sizeable increases in hospital inpatient utilization along many dimensions when MA plans

exit a county. Hospital inpatient utilization rises by 60%, and total charges by more than 50%. We find that MA insurers may achieve this by reducing the use of the hospital for elective and emergency cases, and also by increasing the distance that a patient needs to travel to the nearest hospital. Moreover, we find no evidence that this is accompanied by reduced quality of care for Medicare patients when enrolled in MA; quality indicators, if anything, deteriorate when MA plans exit.

There are a number of caveats to these results. One concern is that the effects of plan exit-which we measure-may not be congruent to the effect from plan entry. That said, we do address one major difference between exit and entry, which is that exit could be accompanied by short-run pent up demand, which would dissipate over time. Examining utilization for the three years following plan exit, we find no evidence for pent-up demand, as the effects do not appear to fade over that timeframe. An additional caveat is that plan exits may be correlated with other factors that impact patient care, but the lack of pre-treatment effects, and the lack of effects for FFS patients, suggest no such effects.

There remain two other limitations to our analysis, however. First, we are only able to track inpatient care. It is possible that the main mechanism through which MA plans reduced hospital care was by increasing spending on primary and outpatient care. However, the evidence that we provide is not consistent with that interpretation: preventable hospitalizations as a share of hospitalizations do not appear to change when MA plans exit. Furthermore, the closest existing study of HMO's provide no evidence of offsetting increases to outpatient care, despite finding large decreases in the inpatient setting (Manning et al 1987). That said, we may still be overstating the efficiency gains associated with MA plans, by ignoring non-hospital care.

Second, our measure of outcomes is an extreme one, mortality. There may be other dimensions along which outcomes improve when MA plans exit that are not captured by our measures. We have documented one such outcome, distance traveled to the hospital. There may be others, such as treatment quality or palliative care, which are not well captured by

our coarse mortality measure.

With those caveats in mind, it is worth discussing the implications of our findings for government policy towards MA plans. Our results have subtle implications for MA reimbursement policy within the existing system. On the one hand, higher reimbursement leads to more MA plan entry and greater choice for consumers (Afendulis 2013, Cabral et al 2014, Duggan et al 2014). On the other hand, higher reimbursement increases inframarginal payments to plans that are already in the market. Existing evidence suggests that the MA plans themselves keep more than half of this reimbursement change (Cabral et al. 2014, Duggan et al. 2014), while much of what remains is a transfer to Medicare recipients. Optimal reimbursement must therefore weigh the social efficiencies of care for those newly enrolling in MA against the deadweight loss of raising the revenue to pay these higher rates for those already enrolled in the plan. When MA plans are scarce, it seems likely that there are efficiency gains given the findings we have here. But as the MA share grows, these efficiency gains may become small relative to the inframarginal transfers.[12]

On the other hand, our results suggest that there are large efficiencies from ensuring that at least some managed care option is available to enrollees. This could occur through a premium support system of the type discussed in CBO (2013), which would set up competitive exchanges through which private plans could compete with the government option. Alternatively, the government could establish a monopoly MA provider for each area, and auction off the number of MA slots for the area, in that way minimizing the reimbursement of MA plans while ensuring MA plan availability. Future work could usefully explore the tradeoffs of these alternatives.

[12] Of course, if there are spillovers from a growing MA share in terms of increased FFS efficiency, this offsets the counter-argument. Existing work suggests that such spillovers do occur, as noted earlier.

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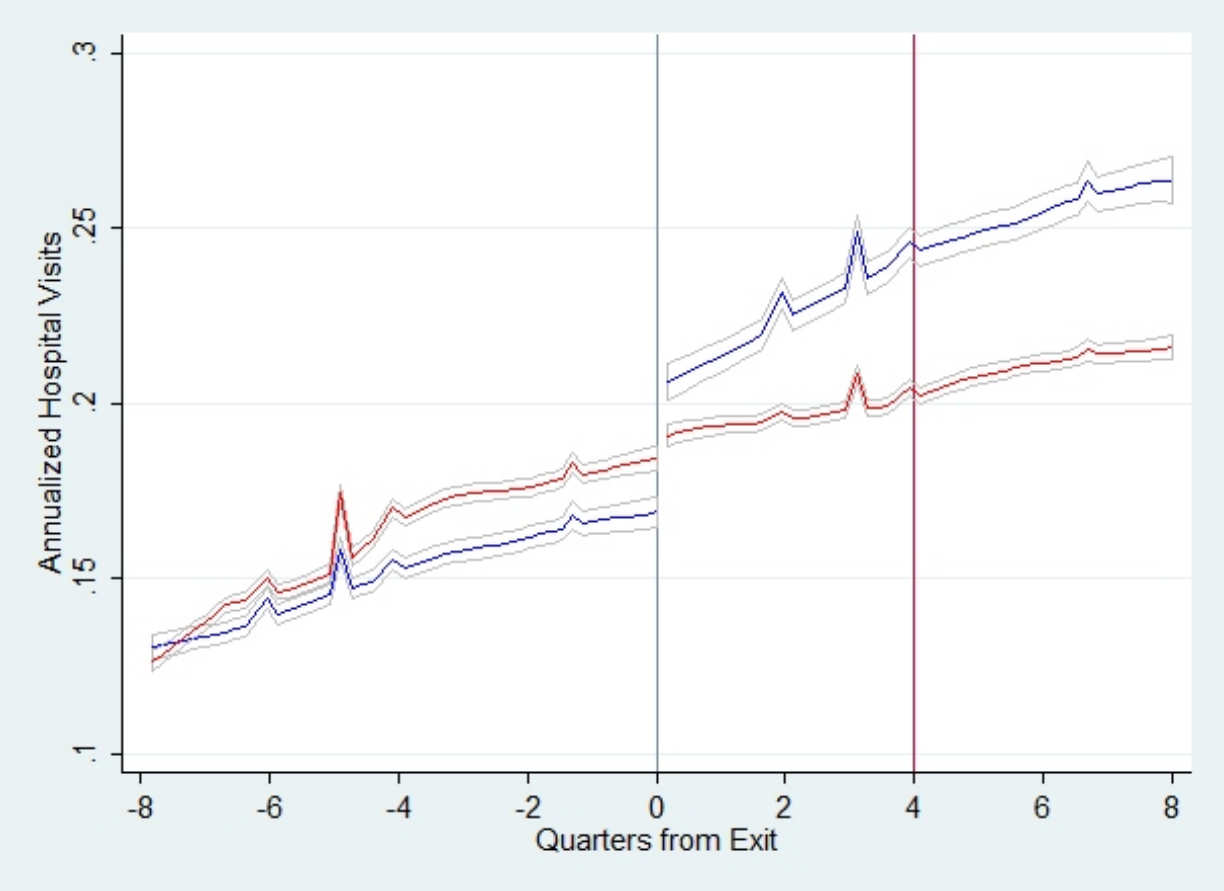
Figure 1: Effect of Plan Exit on Annualized Hosp Visits



Blue: Initially MA in Plan-Exit Counties (Treatment)
Red: Initially MA in Non-Plan Exit Counties (Control)

Table 1: Summary Statistics

	Initially MA	**Initially FFS**
Utilization:		
Visits	0.177	0.288
	(0.782)	(1.041)
Tot Days Stayed	1.349	2.333
	(8.462)	(11.606)
Tot Procs	0.391	0.635
	(2.395)	(3.309)
Tot Charges	4,159	5,427
	(29,320)	(31,654)
Quality:		
Mortality (perc)	4.193	5.844
	(20.045)	(23.458)
N	1,367,730	8,564,475
Other Quality Measures:		
Conditional Readmissions	0.202	0.235
	(0.402)	(0.424)
Conditional Preventable Visits	0.186	0.178
	(0.389)	(0.383)
Driving Time to Hospital	17.83	20.31
	(17.83)	(24.18)
Driving Distance to Hospital	6.32	8.36
	(10.46)	(14.91)
CMS Compare Hospital Rating (Ov)	0.86	0.85
	(0.05)	(0.05)
N	223,324	2,206,923

Notes: Table presents summary statistics for those in MA and FFS (as of 1998, respectively). The unit of observation is at the person-year level for the top 2 panels, and at the hospitalization-level for the bottom panel. The sample covers the 1998-2003 period. In addition, the sample is restricted to those over 65, who are also actively enrolled in Medicare. This data was constructed using discharge-level hospital data from New York State and person-month level Medicare enrollment records from CMS; these two datasets were linked using SSN and other fields.

Table 2: Effect of Plan Exit on Utilization

	(1) **Visits**	(2) **Tot Days Stayed**	(3) **Tot Procs**	(4) **Log Tot Charges**
Mean Value	0.177	1.349	0.391	0.939
Exit Cnty*Post-Exit	0.105***	0.654***	0.129*	0.533***
	(0.017)	(0.146)	(0.075)	(0.082)
N		1,367,730		
Sample & Controls:		Baseline: Initially MA in all NY Counties		

Notes: Table presents linear regression models, where outcome variables are annualized measures of individual inpatient utilization. The key variable of interest is Exit Cnty*Post-Exit, which captures the effect of involuntary switching from MA to FFS Medicare. Year, gender, age, and county fixed effects are included as part of the analysis, and standard errors are clustered at the county level. The unit of observation is at the person-year level, for the 1998-2003 period. The sample is restricted to those over 65, who are also actively enrolled in Medicare. In addition, the sample is restricted to those enrolled in Medicare Advantage, as of the start of the study period (1998). This data was constructed using discharge-level hospital data from New York State and person-month level Medicare enrollment records from CMS; these two datasets were linked using SSN and other fields, and subsequently aggregated to a person-year level. Sample inclusion is not conditional on utilization.

Table 3: Utilization Effect: Specification Checks

	(1) **Visits**	(2) **Tot Days Stayed**	(3) **Tot Procs**	(4) **Log Tot Charges**
Mean Value	0.177	1.349	0.391	0.939
Exit Cnty*Pre-2	-0.004	0.129	-0.014	-0.008
	(0.029)	(0.211)	(0.065)	(0.161)
Exit Cnty*Pre-1			Baseline	
Exit Cnty*First Yr of Exit	0.097***	0.711***	0.106	0.444***
	(0.026)	(0.184)	(0.087)	(0.152)
Exit Cnty*Post-1	0.113***	0.826***	0.124	0.529***
	(0.025)	(0.259)	(0.077)	(0.115)
Exit Cnty*Post-2+	0.100***	0.635***	0.130	0.579***
	(0.031)	(0.217)	(0.109)	(0.125)
N			1,367,730	
Sample & Controls:			Robustness Check: Baseline Sample, with Leads & Lags	
ExitCnty*Post-Exit	-0.005	-0.068	0.121	0.000
	(0.009)	(0.082)	(0.082)	(0.048)
N			8,564,476	
Sample & Controls:			Placebo Test: Initially FFS in all NY Counties	

Notes: Table presents linear regression models, where outcome variables are annualized measures of individual inpatient utilization. The key variable of interest is Exit Cnty*Post-Exit. Year, gender, age, and county fixed effects are included as part of the analysis, while standard errors are clustered at the county level. The unit of observation is at the person-year level, for the 1998-2003 period. The sample is restricted to those over 65, who are also actively enrolled in Medicare. In addition, the sample is restricted to those enrolled in Medicare Advantage (or FFS-as specified), as of the start of the study period (1998). This data was constructed using discharge-level hospital data from New York State and person-month level Medicare enrollment records from CMS; these two datasets were linked using SSN and other fields, and subsequently aggregated to a person-year level. Sample inclusion is not conditional on utilization.

Table 4: Mechanisms for Utilization Impact

	Exit Effect	***Mean***	***Percentage Effect***	***N***
Cohort Restriction:	*Initially MA*			
County Restriction:	*All NY Full-Exit or Non-Exit Cnties (Excl Part Exit)*			
Distance to Hospital: Miles	-4.780***	6.323	-75.6%	223,324
	(1.357)		(21.5%)	
Distance to Hospital: Time	-6.915***	17.825	-38.8%	223,324
	(2.334)		(13.1%)	
Emergency Hosp	0.036***	0.132	27.3%	1,367,730
	(0.013)		(9.8%)	
Non-Emergency Hosp	0.068	0.045	151.1%	1,367,730
	(0.010)		(22.2%)	
Elective Hosp	0.038***	0.029	131.0%	1,367,730
	(0.006)		(20.7%)	
Non-Elective Hosp	0.067***	0.147	45.6%	1,367,730
	(0.017)		(11.6%)	
Elective Procs	0.072***	0.076	94.7%	1,367,730
	(0.015)		(19.7%)	
Non-Elective Procs	0.057	0.315	18.1%	1,367,730
	(0.070)		(22.2%)	

Notes: Table presents linear regression models, where outcome variables are various measures of individual inpatient utilization. The key variable of interest is Exit Cnty*Post-Exit, which captures the effect of involuntary switching from MA to FFS Medicare. Year, gender, age, and county fixed effects are included as part of the analysis, while standard errors are clustered at the county level. The unit of observation is at the hospitalization level for the top panel, and at the person-year level for all the other panels. The data spans the 1998-2003 period. The sample is restricted to those over 65, who are also actively enrolled in Medicare. In addition, the sample is restricted to those enrolled in Medicare Advantage, as of the start of the study period (1998). This data was constructed using discharge-level hospital data from New York State and person-month level Medicare enrollment records from CMS; these two datasets were linked using SSN and other fields. For the person-year level sample, inclusion in the sample is not conditional on utilization.

Table 5: Exit Effect, Based on Distance from In-Network Hospital

VARIABLES	(1) Hosp Dist	(2) Visits	(3) Tot Days Stayed	(4) Tot Procs	(5) Log Tot Charges	(6) Elect. Visits	(7) Emerg. Visits
Exit Cnty*Post-Exit	-2.884***	0.129***	0.841***	0.157*	0.634***	0.040***	0.054***
	(.953)	(0.017)	(0.143)	(0.087)	(0.083)	(0.005)	(0.015)
Mean	5.059	0.189	1.459	0.409	0.999	0.030	0.143
N	112,154	686,981	686,981	686,981	686,981	686,981	686,981
Sample & Controls:	Closest 50% from In-Network MA Hospitals						
Exit Cnty*Post-Exit	-6.101***	0.078***	0.442***	0.097	0.419***	0.036***	0.018
	(1.885)	(0.022)	(0.158)	(0.067)	(0.096)	(0.007)	(0.014)
Mean	7.587	0.165	1.239	0.375	0.880	0.029	0.121
N	111,054	680,720	680,720	680,720	680,720	680,720	680,720
Sample & Controls:	Furthest 50% from In-Network MA Hospitals						

Notes: Table presents linear regression models, where outcome variables are various measures of individual inpatient utilization. The key variable of interest is Exit Cnty*Post-Exit, which captures the effect of involuntary switching from MA to FFS Medicare. Year, gender, age, and county fixed effects are included as part of the analysis, while standard errors are clustered at the county level. The unit of observation is at the hospitalization level for the distance measure, and at the person-year level for all other outcome measures. The top panel is restricted to individuals whose distance to the NEAREST in-network MA hospital was in the closest 50 %; note that this refers to distance, preceding the exit of MA plans. The bottom panel, meanwhile, is restricted to individuals in the furthest 50%. The data spans the 1998-2003 period. The sample is restricted to those over 65, who are also actively enrolled in Medicare. In addition, the sample is restricted to those enrolled in Medicare Advantage, as of the start of the study period (1998). This data was constructed using discharge-level hospital data from New York State and person-month level Medicare enrollment records from CMS; these two datasets were linked using SSN and other fields. For the person-year level sample, inclusion in the sample is not conditional on utilization.

Table 6: Effect of Plan Exit on Quality

	Exit Effect	***Mean***	***Percentage Effect***	***N***
Cohort Restriction:	*Initially MA*			
County Restriction:	*All NY Full-Exit or Non-Exit Cnties (Excl Partial Exit)*			
Outcome Ratings: CMS				
MI Mort	0.158	13.804	1.1%	166,960
	(0.172)		(1.2%)	
HF Mort	0.236***	10.433	2.3%	168,397
	(0.061)		(0.6%)	
PN Mort	0.030	10.873	0.3%	168,512
	(0.109)		(1.0%)	
MI Readm	0.067	19.300	0.3%	164,155
	(0.093)		(0.5%)	
HF Readm	0.158	24.788	0.6%	168,505
	(0.102)		(0.4%)	
Process Rating: CMS				
Overall	0.007	0.859	0.8%	172,286
	(0.005)		(0.6%)	
Heart Attack	-0.002	0.928	-0.2%	172,286
	(0.006)		(0.6%)	
Heart Failure	0.000	0.877	0.0%	172,286
	(0.005)		(0.6%)	
Pneumonia	0.024***	0.752	3.2%	172,286
	(0.008)		(1.1%)	
Disch Based Measures:				
Conditional Readm	0.029***	0.202	14.3%	228,283
	(0.009)		(4.4%)	
Conditional Preventable Hosp	0.019***	0.185	10.3%	1,367,730
	(0.006)		(2.7%)	
Mortality Results (perc)				
New York Only	0.048	4.084	1.2%	235,288
	(0.194)		(4.8%)	
National	0.021	4.413	0.5%	4,001,263
	(0.061)		(1.4%)	

Notes: Table presents linear regression models, where outcome variables are various measures of individual inpatient utilization. The key variable of interest is Exit Cnty*Post-Exit, which captures the effect of involuntary switching from MA to FFS Medicare. Year, gender, age, and county fixed effects are included as part of the analysis, while standard errors are clustered at the county level. The unit of observation is at the hospitalization level for the top two panels, and at the person-year level for the bottom two panels. The data spans the 1998-2003 period. The sample is restricted to those over 65, who are also actively enrolled in Medicare. In addition, the sample is restricted to those enrolled in Medicare Advantage, as of the start of the study period (1998). This data was constructed using discharge-level hospital data from New York State and person-month level Medicare enrollment records from CMS; these two datasets were linked using SSN and other fields. For the person-year level sample, inclusion in the sample is not conditional on utilization.

Appendix

In Appendix Table I, we consider the effect of MA reimbursement rates on Medicare Advantage's penetration of the Medicare market, for the 1998-2003 time period. More specifically, we investigate a possible mechanism for this effect, the exit of MA plans, and the sensitivity of exit to MA reimbursement rates.

Reimbursement amounts to MA plans, per enrollee, are linked to administratively set MA benchmarks, which vary based on an enrollee's county of residence. These reimbursement amounts are also linked to the demographic and health characteristics of each enrollee, since county-level benchmarks are risk-adjusted (based on each enrollee's characteristics) to arrive at the final payment rate.

Incidentally, MA county-level benchmarks are largely a function of each county's per capita FFS costs. Given this, it is necessary to construct an instrument for MA reimbursement, which would be uncorrelated with other factors that could also be affecting plan exit. To do so, we make use of policy-driven variation in county-level MA benchmarks, resulting from the Benefits Improvement and Protection Act of 2000.

One change legislated by the act, which we make use of, is an increase in the MA benchmark floor, from \$401 to \$475; benchmarks were set to the floor level across counties with per capita FFS costs under that floor. We make use of an additional change from the act: the introduction of a differentiated floor, which was set at \$525 and which applied to urban counties only; for this purpose, counties were classified as urban if they were part of metropolitan areas with populations exceeding 250,000. Our instrument is at a county-year level, and is defined as the difference between the actual benchmarks and the counterfactual benchmark that would have prevailed in the absence of these two changes; as such, the instrument effectively corresponds to the bump in benchmarks that certain counties received, from this legislation. Given this, the instrument is mechanically set to \$0 for all years preceding 2001. It is also set to \$0 for all counties for which the floor was not binding at any point, either pre or post 2001.

First, we examine the effect of MA reimbursement, using this instrument, on MA enrollment levels, as a fraction of all those in Medicare. The observation-level throughout these analyses is at a county-year level. Consistent with the existing literature (Afendulis et al 2013, Cawley et al 2005, Pope et al 2006), we find that an additional $100, per person-month, in MA reimbursement (or about a 20% increase, relative to average reimbursement) is associated with a 5.1% increase in the share of nationwide Medicare recipients in MA. This result, which is shown in Table A.1, remains unchanged when restricting to New York State only.

We then examine the effect of MA reimbursement on rates of plan exit, based on the share of all Medicare recipients in exiting MA plans (as of the time of plan exit). This plan exit measure is cumulative in nature, meaning that the measure for 2003 will reflect the cumulative number in exiting plans, from 1998 to 2003, as a fraction of 2003 Medicare enrollment levels. Altogether, the results suggest that plan exit is highly sensitive to MA reimbursement levels, with a $100 increase in MA reimbursement levels reducing the cumulative number in exiting plans-as a fraction of all those in Medicare-by between 3% and 6%.

Note that individuals in exiting MA plans will automatically drop out of MA if no other MA plans remain in their county (we focus on such counties in our main study). However, if other MA plans remain in their county of residence, which is often the case, some of those in exiting plans may switch to MA plans that didn't exit, instead of switching into FFS. To get at the rate at which individuals in exiting MA plans switch to other MA plans, we examine the relationship between the fraction of Medicare recipients in exiting plans, and MA penetration for a given county-year. Our estimates, which are presented in Table A.2, suggest that about half of those in exiting MA plans switch to other MA plans, while the other half drops out of MA entirely and goes into FFS.

Table A.1: Effect of MA Reimbursement on MA Enrollment and Plan Exit

	(1) Nationwide	(2) New York Only	Mean	
			National	*New York*
Coefficient on MA Benchmark Rate ($100/month):				
Share of Medicare in MA	.051***	.054**	0.155	0.172
	(0.006)	(0.022)	(0.152)	(0.103)
Share of Medicare in Exiting MA Plans	-.064***	-0.032	0.058	0.043
	(0.014)	(0.030)	(0.088)	(0.069)

Notes: Table presents linear regression models, where outcome variables correspond to the share of Medicare in MA, and the share of Medicare in exiting MA plans. The exit measure is a cumulative one, meaning that it represents the sum of enrollment in all exiting plans from 1998 through the year of observation, as a fraction of Medicare enrollment. The independent variable of interest, for which coefficient estimates are displayed, is an instrumented MA Reimbursement Rate (for plans), in hundreds of dollars per enrollee-month. County and year fixed effects are included, while standard errors are clustered at the county level. The unit of observation is at the county-year level. The data spans the 1998-2003 period, and is taken from publicly available CMS data.

Table A.2: Effect of MA Plan Exit on MA Enrollment

	MA Enrollment Levels *As Percent of Medicare*	
Exiting Plans (Cumulative)	-0.382***	-0.629***
As Share of Overall Medicare	(0.008)	(0.037)
Set of Cnties	**National**	**NY State**
	County, Year	
	County Medicare Population	
Mean	0.191	0.164
	(0.139)	(0.069)
N	5,478	255
R-Sq	0.960	0.927

Notes: Table presents linear regression models, where outcome variables correspond to the share of Medicare in MA. The key independent variable correspond to the share of Medicare in exiting MA plans; the exit measure is a cumulative one, meaning that it represents the sum of enrollment in all exiting plans from 1998 through the year of observation, as a fraction of Medicare enrollment. County and year fixed effects are included, while standard errors are clustered at the county level. The unit of observation is at the county-year level. The data spans the 1998-2003 period, and is taken from publicly available CMS data.

Data Appendix:

Inpatient Panel Data Construction:

Much of this study relies on an individual-year level panel that tracks inpatient hospital utilization, for private as well as FFS Medicare recipients.

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

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

Sample Restrictions; Treatment and Control Group Construction:

The sample is restricted to New York State; it is further restricted to those qualifying for Medicare on the basis of age, and excludes those qualifying by virtue of disability. For most of our analyses (and in the construction of treatment/control groups), we focus on those enrolled in Medicare, as of January 1998. As such, those who aged into Medicare at a later point in our study period would not be included as part of our study sample. In addition, for each Medicare recipient, the sample is restricted to those years during which they were in Medicare in NY State for at least one month; hence, some individuals may drop out of the sample as a result of death or change of residence.

Our primary treatment and control groups are further restricted to those in PRIVATE Medicare

as of January 1998; for these purposes, we define private Medicare enrollment status based on information in the CMS Medicare denominator data; this allows our analyses to be robust to possible miscoding of private Medicare status in the discharge files (such miscoding appears to be common).

We define county of residence (and by implication, whether an individual is in an 'exit county' and is assigned to the treatment or control group) based on their original county of residence as of January 1998. We exclude partial-exit counties from all of our results, which we define as counties that by 2003 lost between 25 and 90% of their original 1998 MA enrollment. In New York State, there are two such counties altogether (Nassau and Suffolk), whereas nationwide there are 430 such counties (out of over 3,000 in total).

Outcome Measures, From Individual Inpatient Panel:

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

Total Charges: Defined as raw inpatient charges; note that this does not reflect the amount actually paid to hospitals (or the negotiated rate), but is instead an accounting based measure that is uniform across payers. Note that when looking at the non-logged form of this measure, we winsorize the data at the 98th percentile, meaning that all person-year charge amounts in excess of that percentile would get set to the 98th percentile.

Log Total Charges: Defined as the log of (charges+1); as such, even observations with zero raw charges will still get included as part of the analysis.

Distance to Hospital, Miles/Minutes: This is calculated as the driving distance between the center of a patient's zip code of residence, and the center of the zip code in which a given hospital is located. These driving distances, in terms of minutes as well as miles, are calculated using Microsoft's MapPoint program; they reflect driving, rather than crow flies distances.

Elective: Hospital visits that are defined in the type of admission field in New York State's data as follows: 'The patient's condition permits adequate time to schedule the admission based on the availability of a suitable accommodation.'

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

Outcome Measures, From CMS Compare Data:

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

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

Outcome Measures, From CMS Denominator Data

Mortality: These measures are at an individual-year level, and are taken from CMS's Medicare Denominator data. They indicate whether a Medicare recipient died over the course of a given year.

Outcome Measures, from CMS Public Use Data

MA Enrollment Levels: These measures are at a county-year level, are national in scope, and are taken from CMS Public Use Files. They denote the number enrolled in Medicare Advantage for that county and year, as a fraction of all those in Medicare.