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

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There is considerable controversy over the use of private insurers to deliver public health insurance benefits. We investigate the consequences of patients enrolling in Medicare Advantage (MA), privately 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. (*JEL* G22, I11, I12, I13, I18)

The Medicare program, which currently provides nearly universal health insurance coverage to 55 million elderly and disabled US residents, was introduced in 1965 as a form of monopolized insurance coverage that was run and financed by the federal government (Davis, Schoen, and Bandeali 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, which is then reimbursed by the federal government. Prior to 2006, Part C offerings were limited to HMO plans, although additional plan types (such as PPOs) were subsequently introduced, as a result of the Medicare Modernization Act. The Part D program, meanwhile, 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).

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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 (MA), where private insurers provide coverage side by side with the traditional fee-for-service (FFS) system. Evidence on the relative efficiency advantages of private Medicare Advantage plans, however, has been mixed (McGuire, Newhouse, and Sinaiko 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 do not 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. 2013, Brown et al. 2014). The second is the limited availability of data for those who are enrolled in MA. The Medicare claims' data that are typically used for empirical work in this area only track 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 in 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 allow 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 following MA plan exit. We estimate that those originally in MA see their hospital utilization rise by about 60 percent, when moving back to the traditional FFS plan. This estimate is comparable to the corresponding estimate of 65 percent 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, suggesting 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 lower utilization under MA could arise through the mechanisms of reduced hospital availability as well as increased restrictions on elective care.

At the same time, we find no evidence that higher FFS utilization is accompanied by higher quality of care, along any dimension. We find no change in the quality of hospitals used by beneficiaries, as measured by typical Medicare metrics. More significantly, we find no reduction in mortality among those who are forced to move from MA to FFS. Taken together, this suggests that MA plans were delivering care more efficiently than the FFS Medicare program, by using fewer hospital resources. Consequently, our findings have important implications for Medicare, suggesting that increased management of hospital care could lower costs without reducing quality of care.

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

I. Background on Medicare Advantage

Since 1982, Medicare recipients have had the option to enroll in privately 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, Newhouse, and Sinaiko 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 and Morgan 2005). 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, suggesting that plan contracting actually increased Medicare spending.[1] 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 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 and Morgan 2005). Despite these changes, the 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 partly by more modest reimbursement growth across non-floor counties.

Partly because of stagnating MA enrollment levels, in 2003, the Medicare Modernization Act raised reimbursement across all areas. The government also

[1] Studies from the mid-1990s found that utilization among private Medicare enrollees was 12 percent (Riley et al. 1996) to 37 percent (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 six months immediately preceding HMO enrollment).

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, based on their expected costs of providing traditional Medicare equivalent coverage. If a plan's bid fell below county-level benchmarks, the plan would rebate three-fourths of the difference to enrollees, in the form of enhanced benefits or reduced premiums, while the government would keep the remainder. If a bid fell above the benchmark, the recipient would pay the full difference between the bid and benchmark, in the form of higher premiums (Chaikind and Morgan 2005). The Affordable Care Act has further transformed plan reimbursement, by gradually reducing benchmarks between 2011 and 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 following 2004, with overall MA enrollment levels increasing by a factor of 3 (17 million) since then, and the share of Medicare beneficiaries in MA increasing by a factor of 2 (31 percent).[2]

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, Newhouse, and Sinaiko 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 set 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, Stearns, and Norton 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 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 literature has examined the effect of MA more generally, in terms of consumer surplus and overall welfare. Town and Liu find that overall marginal costs under FFS are about 40 percent higher than under MA, which is comparable to our own estimate (granted, our estimate applies to inpatient costs only). Meanwhile,

[2] Data available at http://kff.org/medicare/fact-sheet/medicare-advantage/.

Curto et al. find that FFS costs could be around 14 percent higher than MA, but this estimate is inclusive of administrative costs under MA (which our estimates, of course, are not). Further, Town and Liu assume that there is no advantageous selection into MA. Notably, Town and Liu and Curto et al. both identify differences in overall cost rather than differences in utilization; given that unit price levels are likely to be higher under MA than under FFS, by up to 15–20 percent, the cost estimates in these papers would need to be scaled accordingly to reflect the impact on utilization. Altogether, by accounting for selection and isolating the effect on medical utilization from that on overall costs, our paper moves the existing literature forward.

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 cross-time variation (Cawley, Chernew, and McLaughlin 2005; Afendulis, Chernew, and Kessler 2013) and cross-geography variation (Cabral, Geruso, and Mahoney 2015; Duggan, Starc, and Vabson 2016) in the generosity of plan reimbursement and find a strong positive effect on MA enrollment.

A third area of research has considered the effect of Medicare Advantage on utilization for those enrolled in traditional Medicare. The likely mechanism could come through individual health care providers, whose practice style across all patients, inclusive of traditional Medicare, could be a function of the share of their patient load in managed care (Glied and Graff Zivin 2002). This research suggests 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, Chernew, and Robbins 2013; Afendulis, Chernew, and Kessler 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, McClellan, and Newhouse 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 percent higher inpatient utilization under FFS relative to managed care. At the same time, outpatient utilization was comparable across the two settings (Manning et al. 1984, Manning et al. 1987).

II. Data and Empirical Strategy

We use administrative datasets from CMS and New York State, which contain information on Medicare and Medicare Advantage enrollment status, individual-level utilization metrics for those in MA as well as FFS, and individual-level mortality indicators. We have the unique ability to track individual-level inpatient utilization in Medicare Advantage for every individual in a state and to continuously track individual-level utilization for those switching between MA and 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.

A. *Medicare and MA Enrollment Data*

We obtain administrative Medicare data from CMS, in the form of a denominator file containing Medicare and 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. These data also contain 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 and the characteristics of existing plans. Specifically, we are able to identify those counties experiencing complete plan exit, and the years in which this took place.

B. *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).[3] 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 all-inclusive data across all payers. Given our approach for constructing this panel, individuals are included in the sample even if they have 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. Further, given that these data are of uniform coding and completeness

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

Table 1—Summary Statistics

	Initially MA	Initially FFS
Panel A. Utilization		
Visits	0.177	0.288
	(0.782)	(1.041)
Total days stayed	1.349	2.333
	(8.462)	(11.606)
Total procedures	0.391	0.635
	(2.395)	(3.309)
Total charges	4,159	5,427
	(29,320)	(31,654)
Panel B. Quality		
Mortality (percent)	4.193	5.844
	(20.045)	(23.458)
Observations	1,367,730	8,564,475
Panel C. 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	0.86	0.85
	(0.05)	(0.05)
Observations	223,324	2,206,923

Notes: The 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 two 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. These data were 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.

across payers, any cross-payer differences in utilization would not be driven by differences in underlying data quality; rather, the centralized tracking of these data by hospitals, rather than by individual payers, ensures uniform data quality.

We aggregate our measures of hospital utilization to the person-year level. We focus 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.[4] 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 percent higher than among those initially in

[4] To avoid the undue influence of outliers, total charges for those above the ninety-eighth percentile are winsorized (thus replacing the charge value with the ninety-eighth percentile value).

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 part of our analyses, as New York State does not collect it centrally. This limits our ability to speak to the impact of Medicare Advantage on total medical spending and overall efficiency. However, past studies on MA suggest that this data limitation may not be problematic, as these studies suggest that the outpatient effect might be modest or absent. For example, Parente et al. (2005) found that semi-involuntary MA to FFS switching produces no significant effect on outpatient charges, although they do find a 10 percent reduction to physician visits.[5] Meanwhile, the one experimental study of non-Medicare HMOs found no meaningful HMO effect on outpatient utilization, relative to a fee-for-service alternative (Manning et al. 1987, Manning et al. 1984).

We have conducted some additional analyses using our own data, to compensate for the lack of outpatient claims, by focusing on visits that are especially susceptible to offsets. For example, we show limited relative reductions in preventable inpatient visits, specifically ambulatory-sensitive visits, which could be especially responsive to increases in outpatient care. We also show that the impacts we find are not particularly concentrated in surgical admissions, for which substitutability to an outpatient setting is most well documented (Avalere Health 2006, MedPAC 2013).

Finally, reductions of inpatient care may be efficiency improving, even if the reductions are offset one for one by increases to outpatient utilization (the same type of care may be more efficient to deliver in the outpatient than inpatient setting). This gives us confidence that there is value in studying inpatient spending, even with a lack of corresponding outpatient data.

C. *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. These data are short term in nature—we are only assessing mortality within two years of plan exit—and so they are not comprehensive or perfect measures of care quality. We discuss this point further below.

D. *Sample Restrictions*

We focus on the 1998–2003 period, given that subsequent increases to MA reimbursement resulted in a reentry 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

[5] However, the study may be inconclusive, as it is potentially biased by selection (individuals typically have the option of switching to another MA plan in the county, rather than going into FFS, as not all plans in the county will exit).

originally eligible for Medicare 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 percent of all Medicare Advantage enrollees in New York State, as of 1998 (which precedes plan exit in every county), and likewise accounted for about 7.5 percent of all FFS recipients. For that year, these eight exit counties had average MA penetration rates of 6.7 percent, compared to an overall New York average of 15.2 percent. In six of the counties, exit is over a one-year period; in the other two counties, it is over a two-year period. Meanwhile, our control group consists of counties that experienced effectively no plan exit, excluding those counties with partial-plan exit over our study period.[6]

E. *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 do not have the 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; this is done by excluding the two counties experiencing partial-plan exit from our sample.

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

$$UTIL_{ict} = \alpha + \beta \times Exit_{ct} + X_{ict} \times \gamma + \pi_c + \mu_t + \varepsilon_{ict}, \tag{1}$$

where i indexes individuals, c 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 c in or before year t; X is a limited set of demographic controls (five-year-age group dummies and gender); and π_c 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. In New York, there are 62 counties altogether, of which 8 are exit counties (thereby part of the treated), 52 are counties without substantial exit (thereby part of the control group), and 2 are

[6] The two partial-plan exit counties were the two suburban Long Island counties of Nassau and Suffolk.

counties with partial exit (and are excluded from most analyses). All standard errors are clustered at the county level.

One concern is that disenrollment from MA could result not just from plan exit at a county level, but also from voluntary disenrollments at an individual level, which could require adjustment to our estimates. However, we find the magnitude of such switching to be modest, across treatment as well as control groups. For the treatment group, we find that about 20 percent of those initially in MA had voluntarily disenrolled from it, over the multiyear period preceding plan exit; this implies that 80 percent of the originally designated treatment group was actually subject to the treatment. We also find that the control group experienced MA disenrollment of similar magnitude during the pre-period, but did not experience meaningful changes in MA enrollment status, right when plan exit was taking place in the other counties. Altogether, this means that all of our coefficient estimates should be divided by 0.8, to reflect the effect of treatment on those actually treated.

F. *Endogeneity and Generalizability Concerns*

A natural concern is that such plan exit is not exogenous with respect to underlying health care utilization or health status. We address this concern in several ways as part of our empirical work.

First, we include county fixed effects, so that we control for any fixed differences across counties that do or do not experience exits. Second, we investigate whether there are differential trends before the exit "event" itself. Below, we show as well that the pre-trends in our key outcomes are very similar between the two groups of counties, and that there are no corresponding changes in outcomes for the FFS population.

Another approach here would be to find an instrument for plan exit. Unfortunately, no instrument is readily available. One apparent cause of exit was low reimbursement rates. A sizable literature finds that the MA share of Medicare enrollment is strongly related to MA reimbursement rates (Afendulis, Chernew, and Kessler 2013; Cawley, Chernew, and McLaughlin 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 percent 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 could have direct effects on the treatment of inframarginal MA patients, including those in non-exit counties, and perhaps even spillover effects on the treatment of FFS patients.

Our estimates around plan exit can be validated by examining the effects of an opposite phenomena, plan entry, and showing whether entry is accompanied by utilization decreases. To perform this exercise, we define our treatment group as those originally in MA, prior to plan exit, in plan-exit counties. Our control group, meanwhile, is defined to include those originally in MA, in non-exit counties; this group would have largely remained in MA as of the time of additional plan entry, with its enrollment in MA thereby little affected by the entry. While we find plan reentry to have a highly significant impact on the MA enrollment status of the treatment group, relative to the control group, the impact is modest: following plan exit, most of those

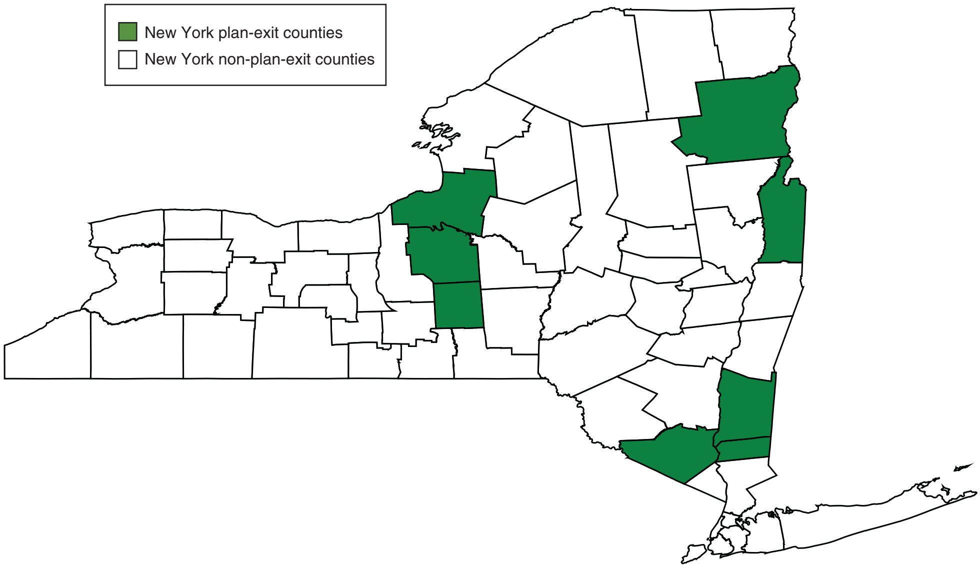

FIGURE 1. MAP OF NEW YORK PLAN-EXIT COUNTIES

in the treatment group were disenrolled into FFS, whereas following plan reentry, only about 15 percent of the treatment group ended up back in MA. As a result, the confidence intervals for our utilization measures included both zero and our original exit estimates. Altogether, given this lack of power, our results here are ultimately inconclusive.

In addition to endogeneity, another concern is the generalizability of our results. One issue is that exit counties may not be representative of counties more generally, in terms of geography and demographics, composition of beneficiaries, and composition of MA plans. Looking first at geography, we show the locations of New York's exit counties in Figure 1. The map indicates that these counties are found all over the state, although most lie on its eastern border. Turning to demographic and other metrics in Table 2, we find that counties with and without plan exit appear quite similar, along most dimensions. Exit counties have comparable percent urban and percent white to non-exit ones, along with similar MA penetration rates and average incomes. At the same time, exit counties are smaller and less densely populated, on average.

To complement these analyses, we examine the effect of plan exit across the suburban Long Island counties of Nassau and Suffolk, which are more populated and dense than typical exit counties, but which experienced only partial-plan exit and were thereby excluded from our main sample. To this end, these two counties saw exit of a substantial, but not the full, set of their Medicare Advantage plans, resulting in MA disenrollment for about 40 percent of those originally in MA. Examining the impact of plan exit in these counties, we find implied effects that are comparable to our main results for full-plan exit counties. This provides support to the generalizability of our findings.

Table 2 also shows a comparison of our New York counties to counties nationwide, to get a sense of national representativeness. Both exit and non-exit counties

Table 2—County Profiles

County type	Total population (1)	Percent urban (2)	Population density (3)	Percent white (4)	Average household income (5)	1998 MA pen rate (6)
New York: Exit county	180,808	57%	261	91%	55,974	8.2%
New York: Non-exit county	324,629	55%	3,324	88%	51,156	7.7%
National: All	88,608	41%	268	84%	44,357	4.4%

Notes: The table presents summary statistics for different groups of counties, based on state and plan-exit status. The unit of observation is at the county level, with the MA enrollment statistics applying for 1998, and all other statistics being for 2000. These data were constructed using county-level characteristics data from the 2000 US census and county-year level Medicare enrollment records from CMS.

in New York are much more urban, more white, have higher incomes, and have higher MA penetration than the nation as a whole. This suggests that while our within-New York comparisons may be valid, there could be some concern in applying these results nationally.

Our results may also not generalize if the MA carriers exiting a county are somehow idiosyncratic. For example, if MA exit is due to an insurer going out of business, they may behave differently than general MA insurers (e.g., be less likely to approve nonurgent surgeries). However, far from being idiosyncratic, the set of insurers exiting treatment counties appears to be broad and diverse. In particular, national carriers such as Aetna and WellPoint accounted for over half of all exiting MA plans in New York, at least in terms of pre-exit enrollment. Given that these carriers are only leaving a limited number of markets, for limited lines of business, rather than departing more broadly, we might not expect the same "going-out-of-business" effects that might otherwise materialize.

A final limitation is that our results cover the 1998–2003 period, which might not be completely generalizable to the present day, given the substantial changes that have since taken place in Medicare Advantage (in terms of reimbursement, introduction of Part D, and introduction of new plan types in addition to HMOs). That said, even with the introduction of new plan types, around two-thirds of MA enrollees continue to be in HMOs, somewhat aiding generalizability. In addition, past research indicates that reimbursement levels may have limited effect on cost sharing and utilization, conditional on an individual's enrollment in MA (Cabral, Geruso, and Mahoney 2015; Duggan, Starc, and Vabson 2016).

III. 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 2 panels A–C, with the underlying unit of observation here being at a person-month level.[7] Figure 2, panel A shows the trend in the average annualized number of hospital admissions

[7] The level of observation here differs from our regression results, in which observations are aggregated to a person-year level. To validate these person-month graphs, we have separately rerun our statistical analyses on data aggregated at a person-month level. In doing so, we obtain regression results that match what is implied by the graphs.

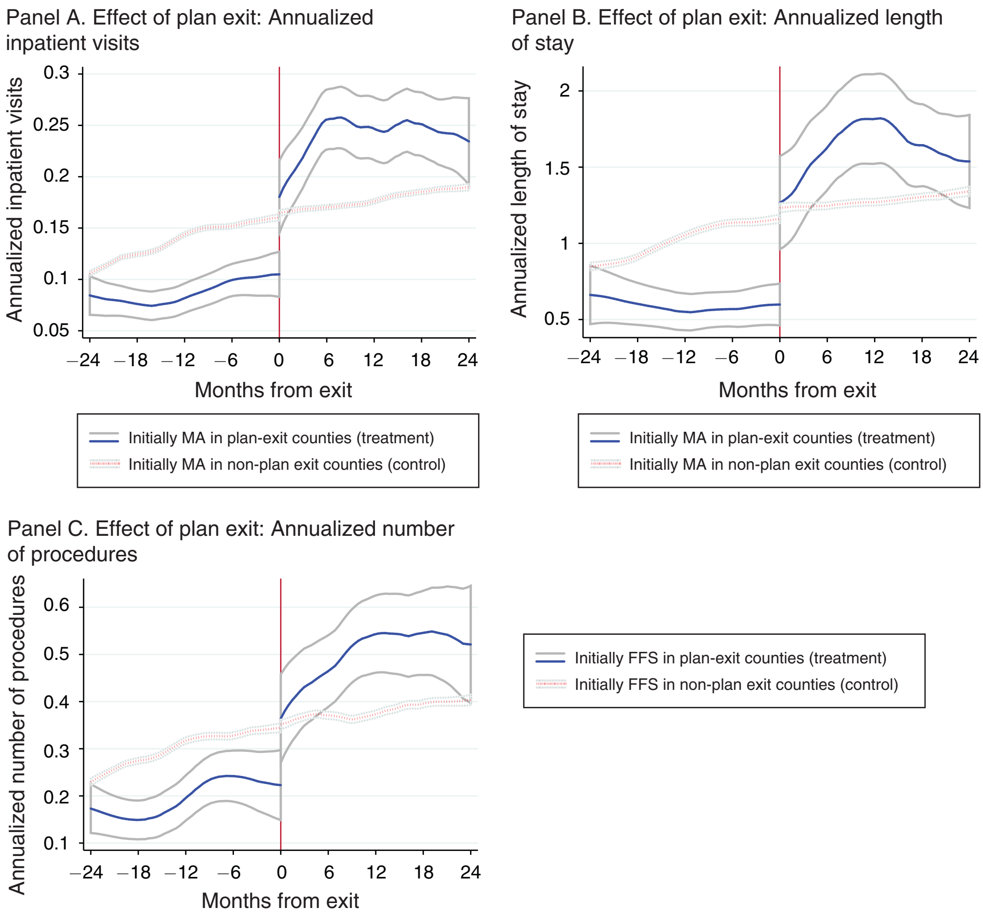

Figure 2

for those who are initially in MA plans in New York counties. The dashed line shows the number of visits for those who are in counties that do not see MA plan exit over this period, while the solid 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 in Medicare as of 1998. 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. Further, over the post-exit period, hospital visits increase more rapidly in exit counties, relative to the pre-trend in exit counties as well as relative to the post-trend in non-exit ones. This previews our finding of a robust increase in inpatient utilization among those initially enrolled in MA in exit counties, with part of this increase materializing immediately following plan exit and the remainder emerging gradually over the post-exit period.

Table 3—Effect of Plan Exit on Utilization

	Visits (1)	Total days stayed (2)	Total procedures (3)	log total charges (4)
Mean value	0.177	1.349	0.391	0.939
Exit Cnty × Post-Exit	0.105 (0.017)	0.654 (0.146)	0.129 (0.075)	0.533 (0.082)
Observations	1,367,730			
Sample and controls:	Baseline: Initially MA in all New York counties			

Notes: The 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). These data were 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.

Figure 2, panels B–C replicate the format of Figure 2, panel A for the other outcome variables that we study: length of stay and number of procedures. In each case, the pattern is similar: roughly flat pre-trends with a very large jump at the month of exit, along with additional increases over subsequent months.

The regression analysis of the impact of plan exit is shown in Table 3, 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 corresponds to an interaction term, for being in an exit county and being in the period after exit. 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 is the level at which plan exit varies. Altogether, the results confirm the implications of Figure 2, panels A–C: there are very sizable increases in utilization along every dimension, with some of the increase happening right at the time of exit and the remainder emerging gradually.

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 percent), and the number of hospital procedures rises by 0.13 (33 percent). Total charges rise by 53 percent.[8]

It is useful to compare our estimates, which (we argue) reflect true differences in the intensity of care across individuals who are exogenously assigned to one

[8] We add one 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.

type of plan or another, to the total difference between FFS and MA plans, which also captures patient selection. Focusing on plan exit counties and looking at 1998 (since that year precedes all exits), we find that those initially in MA experience 80 percent fewer yearly hospitalizations relative to the FFS population; by contrast, our main estimated effect (independent of selection) implies 40 percent lower inpatient utilization under MA than FFS. This suggests that about half of the overall cross-sectional difference between FFS and MA utilization in our baseline year is attributable to treatment effects (MA reducing utilization, even holding population fixed) with the remaining half attributable to selection (MA attracting a relatively healthier population). Consistent with this, the MA population appears healthier even in terms of observable characteristics, such as age, given that MA enrollees are on average two years younger than those in FFS.

The results therefore suggest that the exit of MA plans led to very sizable 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. 1984). 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 relatively less serious conditions.

A. *Specification Checks*

We further explore these findings in Table 4, 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 reflect 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 4. We find no significant lead effect, consistent with no differential pre-trends across these different types of counties.[9] 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 explanation, at least over this three year window. Moreover, it suggests that much of the effect of MA disenrollment is instantaneous and implies that some of the mechanisms underlying MA's effect are activated immediately and likewise have immediate impact. However, given that the effect size grows over time, mechanisms with non-immediate effect appear also to be prevalent.

[9] 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.

Table 4—Utilization Effect: Specification Checks

	Visits (1)	Total days stayed (2)	Total procedures (3)	log total charges (4)
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)
Observations	1,367,730			
Sample and controls:	Robustness check: Baseline sample, with leads and lags			
Exit Cnty × Post-Exit	−0.005	−0.068	0.121	0.000
	(0.009)	(0.082)	(0.082)	(0.048)
Observations	8,564,476			
Sample and controls:	Placebo test: Initially FFS in all New York counties			

Notes: The 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). These data were 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.

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 in these same counties. To address this concern, we reestimate our models on the sample of FFS Medicare enrollees in these same New York 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 the 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.

Figure 3, panels A–C show the same analysis for FFS enrollees that we showed for MA enrollees in Figure 2, panels A–C. There is a small jump at the time of plan exit, but it is tiny compared to what we see for MA, and it is reflected as well in the non-exit counties. This effect may reflect the spillovers discussed above, but even if reflecting other factors, the effect is very small relative to what we see for the MA population.

This is reflected in regression form in the second panel of Table 4. As expected from the figures, we find no significant or sizable impacts on those enrolled in FFS

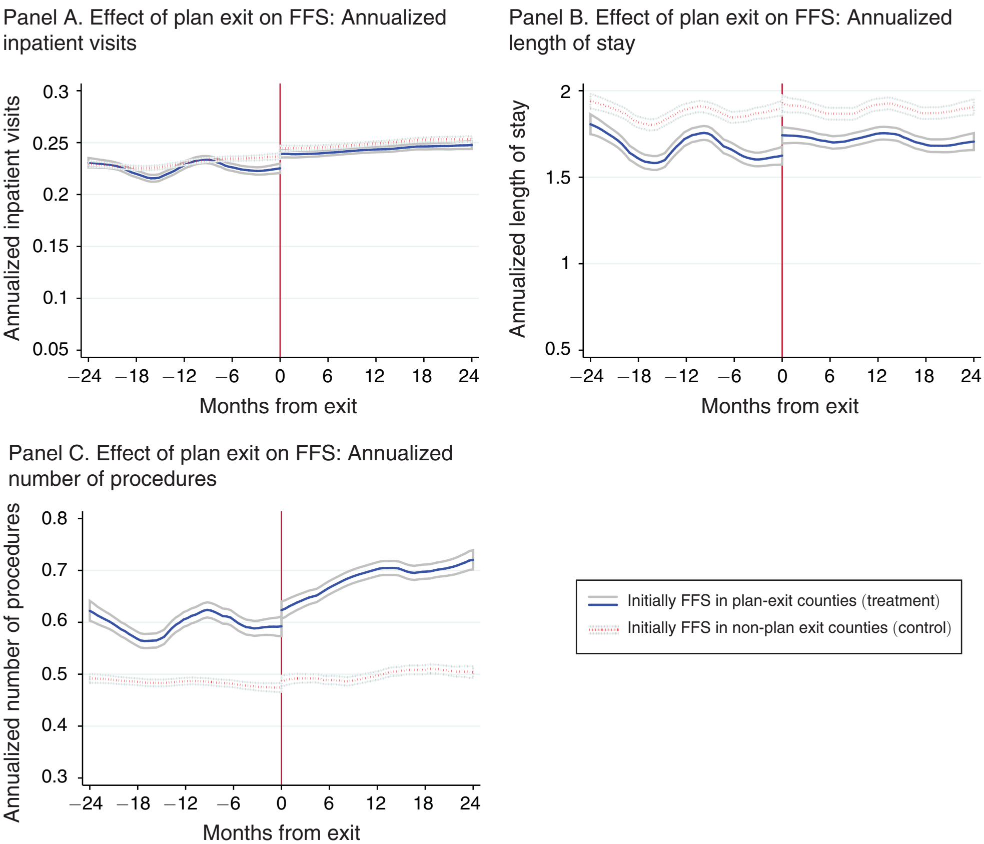

FIGURE 3

in our baseline year of 1998 in the counties with plan exit. This suggests that there are not broad trends toward 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.

A final concern is that our inpatient data are limited to New York State only and fail to track visits to hospitals in surrounding states. This could result in biased estimates, in the event that MA enrollees in New York have differential rates of out-of-state inpatient usage, relative to those in FFS. We perform two different robustness checks, which involve the exclusion of populations more likely to use out-of-state hospitals. In one test, we exclude beneficiaries who live in exit counties and simultaneously reside within ten miles of a state border. In another test, we take a more systematic approach to identifying potential out-of-state hospital users by leveraging hospital service area (HSA) definitions; we exclude those living in exit counties AND simultaneously living in a zip code that is in a non-New York HSA. These hospital service area designations reflect actual patterns of hospital use under FFS, as they are based on actual hospital utilization in Medicare FFS, are granular at a zip-code level, and were originally compiled by the Dartmouth Atlas.

Ultimately, we find that our original estimates remain unchanged under these robustness tests, suggesting that our estimates are not meaningfully affected by the absence of non-New York hospital data. For example, when looking at the impact of MA disenrollment on the number of annual hospital visits, we obtain a point estimate of 0.105 under our main specification, compared to a point estimate of 0.108 when excluding non-New York HSA areas within exit counties, and a point estimate of 0.105 when excluding parts of exit counties within ten miles of the state border.

B. *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. Inpatient cost sharing in MA plans typically comes in the form of per day co-payments, whereas cost sharing under regular FFS consists primarily of an inpatient (Part A) deductible; daily inpatient co-pays also appear under traditional FFS, but only for inpatient days in excess of 60, over a single benefit period. Meanwhile, if an individual in FFS opts for supplemental Medicaid insurance, that supplemental insurance will cover some portion of traditional FFS cost sharing. Altogether, the sign of cost-sharing differentials between MA and FFS is unclear ex ante, given that it is dependent 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 percent 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 60 days and a daily co-payment after that). However, 21 percent obtained supplemental coverage through employer-sponsored insurance, 36 percent purchased supplemental coverage through the Medigap program, and 13 percent 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 percent of their inpatient hospital bills. However, those with some type of supplemental coverage bear about 2.5 percent of their bills, and 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 all supplemental alternatives, including Medicare Advantage, with or without controls. Therefore, on net, cost sharing among those leaving Medicare Advantage did not change, which means that this cannot explain the reduction in inpatient hospital utilization.

Table 5—Mechanisms for Utilization Impact

	Exit effect	Mean	Percentage effect	Observations
Cohort restriction:			Initially MA	
County restriction:		All New York full-exit or non-exit counties (excludes 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 hospitalization	0.036	0.132	27.3%	1,367,730
	(0.013)		(9.8%)	
Nonemergency hospitalization	0.068	0.045	151.1%	1,367,730
	(0.010)		(22.2%)	
Elective hospitalization	0.038	0.029	131.0%	1,367,730
	(0.006)		(20.7%)	
Nonelective hospitalization	0.067	0.147	45.6%	1,367,730
	(0.017)		(11.6%)	
Elective procedures	0.072	0.076	94.7%	1,367,730
	(0.015)		(19.7%)	
Nonelective procedures	0.057	0.315	18.1%	1,367,730
	(0.070)		(22.2%)	
Surgery hospitalization	0.017	0.031	54.8%	1,367,730
	(0.004)		(12.9%)	
Non-surgery hospitalization	0.089	0.146	60.9%	1,367,730
	(0.014)		(9.6%)	

Notes: The 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 span 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). These data were 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.

Another mechanism is through restriction of hospital choice, thereby eliminating the marginal hospitalization (which would be consistent with our results showing more modest percentage effects on charges or days stayed than on number of visits). Given that one measure for breadth of hospital choice is distance traveled to hospital (conditional on hospitalization), we examine the impact of MA plan exit on distance traveled and travel time to the hospital, based on 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 5 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 sizable reduction in distance traveled to the hospital: the average hospitalization is almost five miles and seven minutes closer in driving time. These represent 76 percent and 39 percent of the sample means, respectively.[10] 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 diagnosis-related group (DRG) fixed effects, suggesting that they are not driven by compositional differences in hospital visits across MA and FFS.[11]

One question raised by these results is the share of the effect attributable to greater travel distance between patients' place of residence and the hospital. In our analyses, we leverage within-person variation in distance to the nearest MA hospital in the non-exit counties (to avoid confounding from the direct effects of MA plan exit on hospital utilization). In particular, we estimate a regression of hospital visits on distance to the nearest hospital in the MA network; this regression is restricted to non-exit counties only, with individual fixed effects included to control for fixed locational differences, and the regression being identified only off within-county network changes over time.[12]

From this, we estimate the effect of distance to the nearest hospital on utilization, finding that each additional mile from the closest MA network hospital lowers the number of visits by -0.003. We then multiply this estimate by the estimated effect of MA on distance to the nearest hospital, which allows us to derive the effect of MA through this specific channel of distance. Altogether, we find an estimated reduction in hospital admissions of 0.015 from this channel (relative to 0.105 lower admissions overall), implying that this particular mechanism accounts for about 15 percent of the overall effect of MA.

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 percent overall rise in hospital visits.

We begin, in the second panel of Table 5, by looking at two different types of admissions. The first is "emergency" hospitalizations, which are defined as those requiring immediate medical interventions. We find that the proportional effects for emergency care (at 27 percent) are about half the magnitude of the full sample

[10] 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.

[11] Recent work on HMOs in the Medicaid setting provides further indication that distance to the 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 percent higher inpatient utilization (Vabson 2015), compared to the 60 percent increase that we estimate for Medicare. As such, the greater effect of HMOs under Medicare could be accounted for by a greater effect on distance to the hospital (and other aspects of hospital networks).

[12] One limitation of our approach is that we focus only on distance to the nearest in-network hospital and not on broader measures of hospital accessbility. For example, as shown in Ho and Pakes (2014), not all in-network hospitals may be equally accessible to managed care enrollees, since capitated PCPs may end up referring only to a subset of in-network hospitals that are further away and of lower cost. That said, this limitation should be less applicable to New York than to California, which is the focus of Ho and Pakes (2014), given that capitated PCPs appear to be relatively less common in New York State as of the beginning of our study period (Kongstvedt 2001).

results (at around 60 percent jump), while, correspondingly, there is a much larger rise in nonemergency hospitalizations relative to the sample mean (151 percent).

In the rows that follow, we divide hospitalizations into those that are elective and nonelective, 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 percent of their baseline value after MA plans exit. This is in contrast to nonelective hospitalizations, which rise by less than half (46 percent) of their baseline value.

Indeed, as the next set of rows show, there is a much larger proportional rise in the intensity with which elective hospitalizations are treated. The number of procedures performed rises by 94 percent for elective admissions, and only by 18 percent for nonelective 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 nonurgent hospitalizations. These mechanisms are consistent with what has been previously reported on MA plans, in terms of their use of limited provider networks, as well as their implementation of prior authorization requirements and other utilization management techniques (Blue Cross Blue Shield 2016). These mechanisms are also consistent with the instantaneous timing of the estimated effect, given that the removal of these MA restrictions would immediately follow MA disenrollment and could furthermore have immediate impact.

As noted earlier, one concern given the nature of our data is that we are capturing only increases in inpatient care, and not potential offsetting reductions in outpatient care, when patients move from MA to FFS. While we cannot measure outpatient care, we can consider the type of inpatient care which is most substitutable for outpatient care: surgical admissions. Avalere Health (2006) noted the enormous trend around our sample period in shifting surgeries from inpatient to outpatient settings, and MedPAC (2013) further documents that the shift from inpatient to outpatient care was focused on surgeries, with inpatient surgeries declining 3 percent per year from 2005–2011, compared to total inpatient discharges declining only 1 percent per year.

The final rows of Table 5 therefore split the results into surgical and nonsurgical admissions. In fact, we find that the results are, if anything, stronger for nonsurgical admissions, although the results are similar when taken as a share of the respective admission rate. Therefore, there is no evidence of a particularly strong shift in the surgical admissions that are more substitutable for outpatient care.

In Table 6, 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 (preplan exit): the closest 50 percent and the furthest 50 percent. We measure whether a given hospital is in network based on whether enrollees of MA plans

Table 6—Exit Effect, Based on Distance from In-Network Hospital

Variables	Hospital distance (1)	Visits (2)	Total days stayed (3)	Total procedures (4)	log total charges (5)	Elective visits (6)	Emergency visits (7)
Exit Cnty × Post-Exit	−2.972	0.122	0.831	0.178	0.607	0.041	0.052
	(0.845)	(0.017)	(0.142)	(0.088)	(0.080)	(0.005)	(0.016)
Mean	5.167	0.186	1.416	0.402	0.988	0.031	0.140
Percent effect	−57.5%	65.6%	58.7%	44.3%	61.4%	132.3%	37.1%
	(16.4%)	(9.1%)	(10.0%)	(21.9%)	(8.1%)	(16.1%)	(11.4%)
Observations	116,727	682,108	682,108	682,108	682,108	682,108	682,108
Sample and controls:	Closest 50% from in-network MA hospitals						
Exit Cnty × Post-Exit	−6.818	0.089	0.477	0.080	0.459	0.036	0.022
	(1.817)	(0.022)	(0.191)	(0.066)	(0.104)	(0.007)	(0.013)
Mean	7.577	0.168	1.284	0.382	0.892	0.028	0.124
Percent effect	−90.0%	53.0%	37.1%	20.9%	51.5%	128.6%	17.7%
	(24.0%)	(13.1%)	(14.9%)	(17.3%)	(11.7%)	(25.0%)	(10.5%)
Observations	106,481	684,166	684,166	684,166	684,166	684,166	684,166
Sample and controls:	Furthest 50% from in-network MA hospitals						

Notes: The 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 percent; note that this refers to distance, preceding the exit of MA plans. The bottom panel, meanwhile, is restricted to individuals in the furthest 50 percent. The data span 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). These data were 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.

in the corresponding county visit it at greater than de minimus rates.[13] We find that in these exit counties, 45 percent of hospitals on average (and 47 percent of hospital beds) are classified as in network.

Table 6 shows that the cohort that was closer to an in-network hospital experienced a relatively larger increase in utilization, following plan exit. There is both an absolutely and proportionally larger increase in visits, procedures, days stayed, and charges.

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

[13] For these purposes, we define de minimus as receiving fewer than 4 percent 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.

could have a disproportionate impact on those who would most likely use the ER, in this case those living closest to the hospital.

This result might seem counterintuitive, since the impact of relaxing network restrictions would be smallest on those already living near an in-network hospital. However, that particular mechanism is not the only one operating, and there could be concurrent mechanisms with countervailing effects, such as utilization management. For example, utilization management could have a larger effect on those already located near a hospital, given that these individuals may be unobservably more inclined to undertake emergency visits of a relatively "discretionary" nature absent these restrictions, compared to those further away. Altogether, the presence of these two different mechanisms, and the fact that the underlying impact of utilization restrictions could itself be contingent on distance to hospital, may ultimately account for this set of results.

C. *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 7 by examining a broad variety of quality indicators. All of these quality measures have substantial limitations, but taken together, they paint a fairly consistent picture of no meaningful impacts on care quality.

We also examine how much of our estimated quality effect (and effect on distance traveled) is driven by changes to visit composition, including changes to patient characteristics and diagnoses, as well as the underlying services rendered. As such, we look to the DRG code associated with an admission, which assigns visits to one of several hundred categories, based on the diagnoses and procedures associated with that visit, as well as patients' demographics. Altogether, we find that our results are robust to the inclusion of DRG fixed effects, suggesting that they are not driven by changes to visit composition.

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 values 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 values would imply worse quality of care. For all CMS measures, we use data from 2014. For the process measures, data that are closer to our sample period (from 2005) are available, and they yield similar results.

Table 7—Effect of Plan Exit on Quality

	Exit effect	Mean	Percentage effect	Observations
Cohort restriction:	Initially MA			
County restriction:	All New York full-exit or non-exit counties (excludes partial exit)			
Panel A. Outcome ratings: CMS				
MI mortality	0.158	13.804	1.1%	166,960
	(0.172)		(1.2%)	
HF mortality	0.236	10.433	2.3%	168,397
	(0.061)		(0.6%)	
PN mortality	0.030	10.873	0.3%	168,512
	(0.109)		(1.0%)	
MI readmission	0.067	19.300	0.3%	164,155
	(0.093)		(0.5%)	
HF readmission	0.158	24.788	0.6%	168,505
	(0.102)		(0.4%)	
Panel B. 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%)	
Panel C. Discharge-based measures				
Conditional readmission	0.028	0.203	13.7%	176,374
	(0.009)		(4.4%)	
Conditional preventable hospitalization	0.018	0.186	9.7%	228,403
	(0.006)		(3.2%)	
Panel D. Mortality results (percent)				
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: The 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 span 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). These data were 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.

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 suggests 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. Of course, these are noisy measures and capture only quality changes from switching hospitals, but the consistency is strongly suggestive of no quality effects.

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 the quality given that many readmissions result from either ineffective in-hospital or ineffective post-hospital care (Axon and Williams 2011). Another measure, preventable hospitalizations, identified those hospitalizations that are avoidable under adequate outpatient care, such as visits involving chronic conditions.[14] We identify these preventable hospitalizations using AHRQ's PQI algorithm, which works off the DRG codes and procedures associated with a given admission (AHRQ 2001). Both of these measures are conditional on hospitalization, allowing us to assess whether marginal hospitalizations under FFS disproportionately consist of readmissions or preventable visits. 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, rise by about 15 percent among those initially in MA plans after plans exit. Meanwhile, the odds of a given hospitalization being preventable rise by 10 percent. 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. This allows us to substantially increase the precision of our estimates, given that the entire country has 50 times as many exit counties as New York alone. Further, nationwide rates of plan exit appear to be similar to New York's, suggesting that mortality results for the country as a whole could also be applicable to New York specifically. To this end, 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). Nationwide, the comparable figures are 401 complete exit counties, 2,373 non-exit counties, and 430 partial-exit counties.

The effects on mortality are shown at the bottom of Table 7. 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 only data, we can rule out a reduction in mortality rates in excess of 0.35 percent (with 95 percent confidence) from a baseline of 4.1 percent; with the more precise national data, meanwhile, we can rule out a reduction in excess of 0.10 percent (and also rule out an increase in excess of 0.14 percent), off

[14] However, a reduction in preventable hospitalizations may come through increased outpatient care, leading the overall efficiency consequences to be mixed.

a baseline of 4.4 percent. Given that utilization of the hospital goes up by more than 60 percent, this is a fairly tight bound.

Another way to interpret the magnitude of these mortality results is to translate them into dollar terms, based on the statistical value of a life year. One complicating factor is that our mortality analyses deal with overall yearly death rates, rather than providing additional insight on the accompanying impact on overall lifespan. As such, in using our mortality results on death rates, we must also make assumptions on counterfactuals, in terms of when these deaths would otherwise occur. Looking at mortality nationwide, the estimated effect of MA on death rates has a 95 percent confidence interval of −0.1 to 0.14 (in percentage terms). Given a statistical value of a life year of $100,000, this translates to an effect between −$100 and $140 in dollar terms, if assuming that the timing of death only gets shifted by a year. Meanwhile, if assuming that the timing of death gets shifted by 10 years, it would imply an effect of between −$750 and $1,040 in dollar terms (assuming an annual discount factor of 3 percent).

We then compare these estimates to the estimated magnitude of financial savings from MA. These financial estimates are based on our estimate of 53 percent higher inpatient charges under FFS, relative to MA; we assume that MA and FFS spending are identical for all other types of care. Given mean charges of around $4,100, and assuming a cost-to-charge ratio of two-thirds, we find that MA is associated with between $1,000 and $1,900 in annual savings (the 95 percent confidence interval). With these estimates, the financial benefits of MA appear to outweigh the potential dollarized costs of MA, even toward the outside of our confidence intervals.

The results from this section appear to indicate that there is a sizable inefficiency in transitioning elders out of Medicare Advantage into the FFS program. Utilization of, and spending in, the hospital rises substantially, with no clear or consistent evidence 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.

Of course, our quality measures are imperfect. We are capturing only short term mortality, and any reductions in care under MA plans may show up only over longer periods (although the impact from inpatient reductions could be relatively near term). And, most importantly, we do not have any quality of life measures for patients, which could capture more of the costs of managed care for patients. While we cannot perfectly ascertain whether MA's impact on patient well-being is outweighed by its financial benefits, from our utilization results we can infer the quality range under which this would be true, and under which MA would be of overall benefit.

IV. Conclusions

The role of private firms in public insurance is the subject of a central debate in US 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 toward expanding the role of managed care plans. Opponents point to the sizable 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 sizable increases in hospital inpatient utilization along many dimensions when MA plans exit a county. Hospital inpatient utilization rises by 60 percent, and total charges by more than 50 percent. We find that MA insurers may achieve this by differentially reducing the use of the hospital for elective and nonemergency 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 time frame. An additional caveat is that plan exits may be correlated with other factors that impact patient care, but the lack of pretreatment effects, and the lack of effects for FFS patients, suggest no such effects.

There remain four 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 and readmissions, as a share of all hospitalizations, do not appear to change when MA plans exit. In addition, the effects on surgical admissions are comparable to those for all other visit types, even though their substitutability to outpatient care is well-documented. Furthermore, the closest existing study of HMOs provides 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 nonhospital care.

Second, our main 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.

A third limitation of our analysis is that we cannot fully explain the reasons for plan exit. In particular, if MA plans are so much more efficient than traditional Medicare, then why are they leaving the program? There are no noticeable differential pre-trends in reimbursement; over the period preceding plan exit, relative MA reimbursement from county to county effectively remained constant, with 2 percent annual increases across all counties. This increase lagged well behind medical inflation during this period, which put exit pressure on all plans. This pressure may have been felt particularly by plans in the exit counties, since their reimbursement started at a lower baseline; in 1998, MA reimbursement benchmarks were 101 percent of FFS in the exit counties, compared to 109 percent in non-exit counties.

In addition, administrative costs are much higher for MA plans than for FFS, accounting for 9 percent of MA spending (MedPAC 2013) versus less than 2 percent of FFS spending. As well, MA appears to pay higher provider rates, particularly where individual MA plans have less market power than the FFS program. Discussions with officials at New York's Department of Health indicate that MA rates could be an average of 15–20 percent higher. Moreover, we cannot rule out some offsetting costs' increases on the outpatient side that is not measured in our analysis. Finally, MA plans might have minimum profit thresholds or requirements, which may lead them to exit a market even if they are marginally profitable.

A final limitation is that our analysis is limited to a somewhat older time period, for a set of New York counties only. We have shown that these counties are fairly representative of the state, but they do appear to be substantively different on observables from the national average. This suggests the value of additional analyses of this type, which can investigate whether the effects are similar in other areas and at other times.

With those caveats in mind, it is worth discussing the implications of our findings for government policy toward 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, Chernew, and Kessler 2013; Cabral, Geruso, and Mahoney 2015; Duggan, Starc, and Vabson 2016). 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, Geruso, and Mahoney 2015; Duggan, Starc, and Vabson 2016), 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.[15]

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.

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

Appendix

In Appendix Table A1, 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, Chernew, and Kessler 2013; Cawley, Chernew, and McLaughlin 2005; Pope et al. 2006), we find that an additional \$100, per person month, in MA reimbursement (or about a 20 percent increase, relative to average reimbursement) is associated with a 5.1 percent 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 percent and 6 percent.

Table A1—Effect of MA Reimbursement on MA Enrollment and Plan Exit

	Nationwide (1)	New York only (2)	Mean: National	Mean: New York
Coefficient on MA benchmark rate ($100/month):				
Share of Medicare in MA	0.051	0.054	0.155	0.172
	(0.006)	(0.022)	(0.152)	(0.103)
Share of Medicare in exiting MA plans	−0.064	−0.032	0.058	0.043
	(0.014)	(0.030)	(0.088)	(0.069)

Notes: The 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 span the 1998–2003 period and are taken from publicly available CMS data.

Table A2—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)
	County, year	
	County Medicare population	
Set of counties	National	New York state
Mean	0.191	0.164
	(0.139)	(0.069)
Observations	5,478	255
R^2	0.960	0.927

Notes: The table presents linear regression models, where outcome variables correspond to the share of Medicare in MA. The key independent variable corresponds 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 span the 1998–2003 period and are taken from publicly available CMS data.

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 A2, 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.

Data Appendix

A. *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 percent 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.

B. *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 New York 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 percent 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).

C. *Outcome Measures, from Individual Inpatient Panel*

Total Procedures.—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 ninety-eighth percentile, meaning that all person-year charge amounts in excess of that percentile would get set to the ninety-eighth 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."

D. *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 percent implies that if an individual is hospitalized for a heart attack, they have a 15 percent 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 0.85 for heart attacks implies that for a particular hospital, process was adhered to 85 percent 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.

E. *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.

F. *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.

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