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Managed care and medical expenditures of Medicare beneficiaries

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ABSTRACT

This paper investigates the impact of Medicare HMO penetration on the medical care expenditures incurred by Medicare fee-for-service (FFS) enrollees. We find that increasing penetration leads to reduced spending on FFS beneficiaries. In particular, our estimates suggest that the increase in HMO penetration during our study period led to approximately a 7% decline in spending per FFS beneficiary. Similar models for various measures of health care utilization find penetration-induced reductions consistent with our spending estimates. Finally, we present evidence that suggests our estimated spending reductions are driven by beneficiaries who have at least one chronic condition.

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1. Introduction

The 1990s saw a dramatic increase in the percentage of Medicare enrollees who joined an HMO. After the Balanced Budget Act of 1997, enrollment dropped dramatically, but following the Medicare Modernization Act of 2003, enrollment is again on the rise. By 2007, 20% of Medicare beneficiaries were enrolled in a privately administered health plan.¹ The Congressional Budget Office predicts further increases in Medicare HMO enrollment, suggesting enrollment in HMOs (excluding Private Fee-For-Service Plans and regional PPOs) will rise by about 50% by 2017.² While the rise of a meaningful managed care sector may affect both the financial health of the program and the physical health of Medicare enrollees, we focus on the former.³ In particular, we ask the question: Does Medicare HMO penetration affect total health care spending incurred by fee-for-service beneficiaries? Put differently, do the effects of HMO penetration spill over into fee-for-service Medicare?

Spillover effects refer to changes in the care delivered to fee-for-service enrollees that arise due to changes in HMO enrollment among Medicare beneficiaries, holding the health status of fee-for-service enrollees constant. There are several reasons to expect spillovers. For example, if physicians tend to practice similarly for all patients, more managed care enrollment may alter practice patterns for fee-for-service patients. Additionally, managed care enrollment may influence aspects of market

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¹ Source: Kaiser Family Foundation, Medicare Advantage Fact Sheet, June 2007.

² Peter R. Orszag, The Medicare Advantage Program: Trends and Options. CBO Testimony before the Subcommittee on Health Committee on Ways and Means U.S. House of Representatives, March 21, 2007.

³ In late 2000, the Health Care Financing Administration (HCFA), now called the Center for Medicare and Medicaid Services (CMS), convened a technical review panel to examine the assumptions used by the Office of Actuaries to assess the financial health of the Medicare Trust Funds. The panel concluded that these assumptions were in need of revision. One specific area was forecasting the impact of Medicare managed care on total Medicare costs.

structure such as the number of hospitals, beds or available services over time (Chernew, 1995a; Baker and Brown, 1999). In turn, these changes could impact practice patterns for all individuals in a given market. Overall, the notion behind the possibility of spillovers is that an increased managed care presence may change the manner in which fee-for-service patients are treated.

Accurate assessment of spillovers is important. In the current policy debate, it has been suggested that Medicare managed care plans are overpaid and there is some discussion of reducing payment rates.⁴ However, if spillovers are substantial, optimal payment rates from CMS to HMOs might be higher than they otherwise would be, to encourage greater HMO participation in the Medicare program. Conceptually, this would reflect some of the externality represented by savings to FFS Medicare stemming from Medicare managed care enrollment.

Even if CMS adopted a different payment approach for managed care, such as competitive bidding (which is used to some extent in the current Medicare managed care program) or proxy shopping as outlined by Havighurst (1970) and Rose-Ackerman (1983), the issue of externalities caused by spillovers is important. Plan bids and proxy prices would not capture these spillover externalities. Thus, more generally, if spillovers are significant, additional steps to increase enrollment in HMOs might be warranted.

Assessing the magnitude of spillovers is also important for assessing the fiscal impact of the Medicare managed care program. The direct fiscal impact of a Medicare beneficiary choosing to enroll in an HMO depends on Medicare's payment rates to HMOs, relative to what Medicare would have paid if they remained in the traditional fee-for-service system. Because payment rates for Medicare HMOs were historically tied to the local average costs in fee-for-service Medicare, and because HMOs tended to attract a relatively healthier population, analysts have felt that growth in HMO enrollment increases the total costs of Medicare. Any cost savings obtained by HMOs were either captured by the HMOs or competed away via more extensive benefit packages. Analysis by MedPAC suggests that spending by Medicare for HMO participants was 12% higher relative to demographically similar beneficiaries in traditional Medicare (MedPAC, 2008). However, if there are spillover effects from Medicare HMO penetration, the savings may offset the costs associated with favorable selection.

In this paper, we assess the spillover between Medicare HMO enrollment and expenditures on Medicare fee-for-service beneficiaries. Our basic approach is to regress spending by fee-for-service Medicare beneficiaries on the share of Medicare beneficiaries in their county who are enrolled in HMO plans. Because HMO penetration is potentially endogenous, we use county-level variation in Medicare payment policy as an instrument for Medicare-specific HMO penetration, which we also measure at the county-level on the assumption that a county geographically represents the relevant market. This approach has been used successfully in other contexts (c.f., Town and Liu, 2003; Gowrisankaran and Town, 2006). Our identification comes from longitudinal variation in payment rates over our study period (1994–2001) and reflects, in large part, reforms instituted in the Balanced Budget Act of 1997 (BBA) and idiosyncrasies in Medicare payment rules.

We find evidence of substantial spillover in a sample of fee-for-service Medicare beneficiaries. In particular, in instrumental variables models we find that a 1% point increase in county-level Medicare HMO penetration is associated with a .9% reduction in individual annual spending on fee-for-service beneficiaries. These estimates are larger in magnitude than corresponding least squares estimates, which also imply the existence of such spillovers. To investigate the validity of our findings, we also estimate models which examine the impact of Medicare HMO penetration on various categories of health care utilization. We find that increases in county-level Medicare HMO penetration reduce both inpatient and outpatient events, with larger effects found on intensive utilization margins. These estimates are consistent with our main finding that increased Medicare HMO penetration reduced spending by fee-for-service beneficiaries in that they provide a plausible mechanism for the spending reductions. Finally, we present evidence that this relationship is driven by individuals, who report at least one chronic condition. By contrast, we find no evidence of a systematic relationship for beneficiaries without any reported chronic conditions.

2. Background

The Tax Equity and Fiscal Responsibility Act (TEFRA), passed in 1982, directed the Health Care Financing Administration (HCFA) to contract with HMOs to provide a managed care option to Medicare enrollees. Under the Medicare HMO program, Medicare enrollees can forgo the traditional Medicare insurance program and enroll in a qualified HMO. The HMO agrees to provide health insurance that covers all Medicare-covered services (Parts A and B) in exchange for a per-capita fee, which varies at the county-level, from CMS. In addition, HMOs may offer benefits beyond those available to fee-for-service Medicare beneficiaries. The rationale underlying TEFRA is that HMOs may be more efficient at providing care thereby reducing federal Medicare expenditures.⁵ Beginning in the early 1990s and extending to the latter part of the decade, there was a surge in the share of Medicare beneficiaries who took advantage of this option.

A vast literature, including several reviews, documents the effects of managed care plans (Miller and Luft, 1997; Miller and Luft, 2002). One strand of that literature examines the impact of managed care enrollment on Medicare costs or utilization

⁴ See, for example, "Private Remedy: Insurers Fight to Defend Lucrative Medicare Business," *Wall Street Journal*, April 30, 2007.

⁵ HMO enrollment may be beneficial for enrollees, themselves, and the Medicare program if Medicare HMOs provide care more efficiently than the traditional fee-for-service system. More efficient care can manifest itself through lower costs of care, higher quality or through broader benefit coverage. If savings exist from HMOs, Medicare ultimately may save money and/or enrollees may receive enhanced benefits because of competition among plans.

(Baker and Corts, 1996; Baker, 1997; Baker and Shankarkumar, 1997; Cutler and Sheiner, 1997; Baker and McClellan, 2001; Cao and McGuire, 2003; Bundorf et al., 2004) as well as the somewhat larger literature examining the impact of overall HMO activity on the market as a whole (Robinson and Luft, 1988; Robinson, 1991; Melnick and Zwanziger, 1995; Wickizer and Feldstein, 1995; Robinson, 1996; Gaskin and Hadley, 1997; Hill and Wolfe, 1997). Overall, this research provides strong support for the general proposition that markets are connected and thus we may reasonably expect activities in the Medicare HMO market to influence the expenditures associated with treating Medicare fee-for-service beneficiaries.⁶

Much of the existing literature on spillovers ignores the potential endogeneity of HMO penetration. However, this strategy may be flawed if, for example, omitted area characteristics are correlated with Medicare HMO penetration and also have an independent impact on expenditures on fee-for-service enrollees.⁷ Baker (1997), Cao and McGuire (2003) and Mello et al. (2002) are exceptions as they report instrumental variables estimates. Baker (1997) and Cao and McGuire (2003) use cross-sectional models so their identification is fundamentally different from ours. Mello et al. (2002) examine utilization (not spending) using payment rate changes similar to our approach. They have a short panel from 1993 to 1996, prior to the BBA.

Our empirical strategy, discussed in detail in the next section, relies on a strong relationship between payment rates, which are specific to counties, and aggregate enrollment levels. Conceptually, higher payment rates induce more managed care plans to enter the Medicare market and induce plans to either offer more generous benefits or lower premiums, which attract beneficiaries. The findings of several studies support this intuition, suggesting that higher payment rates affect HMO participation in the Medicare program and the benefits offered (Cawley et al., 2002; Town and Liu, 2003; Pizer and Frakt, 2002). However, none of these studies directly measures the impact of payment changes on aggregated HMO enrollment at the county-level.

Because spillovers likely reflect the impact that managed care plans have on practice patterns, the magnitude of the spillover effect may depend on the exact mechanism by which plans attract beneficiaries and the characteristics of those attracted to managed care plans. For example, if managed care plans attract beneficiaries by offering more generous benefits, as opposed to by lowering premiums, those benefits may influence practice patterns and thereby affect spillovers. Similarly if managed care plans attract beneficiaries with certain clinical conditions, the impact on practice may be concentrated in those conditions and, depending on the distribution of beneficiaries remaining in the FFS Medicare program, may affect spillovers. Our analysis should thus be considered a reduced form analysis in which we do not try to disentangle the underlying mechanisms that drive spillovers.

3. Empirical strategy and related issues

3.1. Basic model

Using a sample of individuals enrolled in traditional fee-for-service Medicare, we estimate models of the form:

$$\text{LogExpenditure}_{ict} = \alpha_c + \gamma_t + \beta \text{MC}_{ct} + \lambda X_{it} + \varepsilon_{ict}, \quad (1)$$

where i indexes the individual fee-for-service beneficiary, c represents county of residence and t represents year of interview. *Expenditure* represents total annual medical care spending on fee-for-service beneficiaries enrolled in a given county in a given year.⁸ In later specifications, we replace spending with measures of health care utilization (e.g., inpatient and outpatient events, doctor visits, etc.) in an attempt to better understand the mechanism driving our spending estimates. *MC* represents the fraction of Medicare beneficiaries enrolled in an HMO in a given county in a particular year. Because we include county fixed effects (α_c) in our specification, we identify the impact of Medicare HMO penetration on spending via within-county changes in penetration. To the extent that there are unobserved characteristics that are correlated with both penetration and spending (e.g., county-level health status), this represents an improvement over cross-sectional estimation. In addition, we also include a vector of year effects (γ_t) to account for trends that are common across all counties in our sample. The vector X represents individual covariates that will affect demand for services. These include beneficiary demographic information as well as additional health status measures and other variables likely correlated with demand. In addition to self-reported health, additional covariates include experience with sixteen diseases/disorders as well as smoking status and body mass index.⁹ In our preferred specification, we add other county-level information including overall commercial HMO penetration and various measures of county-specific medical resources.

⁶ Note also that a series of studies by Zwanziger, Melnick and colleagues reach a similar qualitative conclusion using a somewhat different approach, emphasizing the importance of selective contracting on costs, without explicitly controlling for managed care penetration (Zwanziger and Melnick, 1988; Melnick et al., 1989a,b; Zwanziger et al., 1994).

⁷ Baker (1997), Cao and McGuire (2003) and Mello et al. (2002) are exceptions as they report instrumental variables estimates. Baker (1997) and Cao and McGuire (2003) use cross-sectional models so their identification is fundamentally different from ours. Mello et al. (2002) use payment rate changes, similar to our approach, using a short panel from 1993 to 1996, prior to the BBA. These latter authors, however, examine utilization and not spending.

⁸ This specification is similar to those found in the existing literature, though we use individual data.

⁹ The sixteen disease/disorder indicators are based on a central question which asks respondents if they have ever had: arthritis, rheumatoid arthritis, emphysema, Alzheimer's disease, hip fracture, cancer, skin cancer, Parkinson's disease, at least partial paralysis, psychiatric disorder, coronary heart disease, hypertension, diabetes, myocardial infarction, stroke or a hearing problem not included in this list.

The disturbance term in Eq. (1) is likely correlated with county-level Medicare HMO penetration. Specifically, there may be unobserved, time-varying county level traits that are correlated with both Medicare HMO penetration and spending, such as consolidation in the provider market or changes in employer demand. Assuming that HMOs tend to enter areas with rising fee-for-service spending (because they have greater potential to achieve savings), we would expect least squares estimates of β to be biased upwards. If the true effect of penetration on expenditures is negative, this means β will be biased towards zero.

3.2. Instrumental variables

We correct for this potential bias using an instrumental variables (IV) approach. In particular, we use county-level payment rates from CMS to HMOs as instruments to identify the effect of county-level Medicare HMO penetration.¹⁰ To the extent that these payment rates are correlated with county-level penetration, but are orthogonal to current fee-for-service expenditures, our IV estimates represent an improvement over corresponding OLS estimates. Given our expectations regarding HMO entrance into markets with relatively high cost growth in expenditures, and given our expectation that healthier enrollees chose HMOs, we expect the IV estimates to be more negative, and hence larger in magnitude, than our OLS estimates.¹¹

A formal model that highlights the underlying logic for a causal relationship between Medicare HMO penetration and payment rates can be found in *Gowrisankaran and Town (2006)*. In their framework Medicare HMOs enter and make premium and benefit design decisions based on the payment rate knowing that the benefit design may result in differential selection. In equilibrium, their simulations show Medicare HMO enrollments are a nonlinear and increasing function of the payment rate. The intuition is straightforward. All else equal, increasing the payments increases the profitability of the marginal enrollee. Thus, larger payments make it profitable for plans to incur the costs of entering the market and conditional on operating offering more generous benefits in order to increase enrollments.

Variation in county-level payment rates comes from two sources. First, prior to the BBA, Medicare based its payment to HMOs on the per capita costs of the fee-for-service enrollees in counties. This may seem to suggest that payment rates would be a poor instrument for HMO penetration in our model because of their apparent relationship with fee-for-service spending. However, payment rates at time t were based on average fee-for-service spending between periods $t-8$ to $t-3$.¹² The validity of county-level payment rates depends on the degree of autocorrelation in fee-for-service spending over time, which we explore below.

The other source of payment variation is the BBA of 1997, and subsequent refinements, which broke the link between payment rates and average local fee-for-service costs. The BBA fundamentally modified Medicare's payment methodology. While the changes in the payment formula are relatively technical, for our purposes, the important feature is that adjustments to county-level payments are now divorced from the Medicare fee-for-service experience in the county. Specifically, after the BBA, county rates were set equal to the maximum of three rates: (a) a blended input price which is a combination of an adjusted national rate and an area-specific rate, (b) a floor payment designed to increase the rates in low-paid counties, and (c) a minimum increase of 2% per year. Initially, most counties were either ceiling or floor counties, minimizing the variation in payment changes post-BBA. However, the subsequent refinements to the BBA payment formulas added greater variation in payments across counties. In most counties the post-BBA payment formula led to a substantial decrease in payment rates over what HMOs would have received prior to the BBA. It is estimated that the BBA methodology lowered payments to HMOs by an average of 6%.¹³ In addition to reducing the level of payments, the BBA also diminished the variance in payment rates across counties. For example, the standard deviation of the payment rate fell from roughly \$89 to \$60 from 1994 to 2001 in constant 1994 dollars. That said, the amount of within-county variation in payment rates is more relevant than the level of cross-sectional variation, given our identification strategy. To assess this, we regress the payment rate variable on a full set of county and year fixed effects. We then compute the variance inflation factor which, in this context, is the reciprocal of one minus the R -squared from this auxiliary regression. Conventionally, it is assumed that if the variance inflation factor is greater than 10, there is not sufficient independent variation. However, our auxiliary regression yields an R -squared small enough to imply sufficient variation in payment rates. In particular, the variance inflation factor is about 3.4.

3.3. Tests of the instruments

While the impact of the BBA on payment rates is likely unrelated to the error term in Eq. (1), the payment rate still may be a "weak" instrument. We test the strength of our instrument set via a standard F -test. As will be seen, all F -tests strongly reject the hypothesis that our instruments are unrelated to county-level Medicare HMO enrollment rates.¹⁴

¹⁰ Other potential instruments could be based on the distribution of firm sizes in an area, though this is most likely more relevant to commercial HMO penetration than Medicare-specific penetration. *Baker (1997)* advocates the use of such an instrument for commercial HMO penetration.

¹¹ Even with IV estimation, change in the composition of the FFS population remains possible. We discuss this later in this section.

¹² More specifically, these are 5-year averages, starting 8 years prior to time t .

¹³ Source: U.S. Congressional Budget Office (1999).

¹⁴ A standard rule-of-thumb is that this F -statistic be greater than 10. All of our F -statistics are greater than 37. In addition, we report the partial R -squared for each first-stage regression.

Table 1
Pre- and post-BBA growth in mean fee-for-service expenditure, by relative magnitude of payment growth

	Ratio of payment growth post-BBA to payment growth pre-BBA	
	Below median	Above median
% growth in mean FFS expenditure		
1994–1996	10.0%	9.2%
1998–2001	25.1%	16.7%

Notes: Payment growth pre-BBA is the percent increase in payment from 1994 to 1996 and payment growth post-BBA is the percent increase in payment from 1998 to 2001. The ratio of these percentages is computed for each sample county and the resulting distribution is divided into two groups—those counties below the median of this ratio and those counties above it. These two groups are represented in the columns above. Conceptually, counties below median are those whose payment rates are slowing, while those above median are counties whose payment rates are accelerating over time.

The validity of these county-level payments rates also requires payment changes to be unrelated to existing trends in spending across counties. For example, our identification strategy would be flawed if the counties that experienced relatively generous or stingy growth in payments due to the BBA would have had systematically different spending trends not captured by our covariates.

To examine this possibility, we divided counties in our sample into those whose payment growth was slowed following the BBA and those whose spending growth was accelerated.¹⁵ This taxonomy is based on the ratio of payment growth in each county post-BBA to growth pre-BBA. The results from this exercise are presented in Table 1. Prior to 1997, counties which were treated generously following the BBA (i.e., had above median relative payment growth) had roughly the same percent growth in expenditures as those counties which were treated less generously. In particular, the former counties experienced growth in spending on fee-for-service beneficiaries of 9.2%, while the latter counties experienced growth of 10%. This suggests spending trends prior to the BBA were similar across counties that later were differentially impacted by the BBA and subsequent payment regimes. After the BBA, and consistent with results we report below, counties whose payment growth was slowed following the BBA had higher percentage FFS spending growth (25.1%) relative to those counties whose payment growth was accelerated following the BBA (16.7%).

We more formally examine the relationship between lagged cost growth (which drives payment changes), and current cost growth by estimating a first-order autoregression of the residuals from a regression of log spending by fee-for-service beneficiaries on all of our exogenous variables, including the payment rates.¹⁶ The autocorrelation parameter appears to be sufficiently small to allow this to be a useful source of identifying variation. In particular, the parameter ranges from 0.04 to 0.07 and is not statistically different from zero. Analysis of Dartmouth Atlas data, based on costs measured at the Healthcare Referral Regions (HRRs) supports this analysis. The correlation between cost increases in FFS Medicare between 1996 and 2000 and cost increases between 2002 to 2003 is minimal ($\rho = -.08$) and not statistically significant, suggesting that correlation between current payment changes (which are based on lagged cost increases) and contemporaneous cost changes is near zero.

3.4. Selection effects

The measurement of spillovers is complicated by selection concerns. Selection effects refer to the impact of non-random sorting of beneficiaries into Medicare managed care. A common concern is that relatively healthier individuals will opt out of fee-for-service Medicare. The concern has fiscal implications. In particular, if healthier beneficiaries systematically enroll in Medicare HMOs, the costs for those remaining in the fee-for-service sector will rise because that population will be, on average, less healthy. Conditional on such sorting, costs will be higher in markets with high HMO penetration, even if care for any given fee-for-service patient is unaffected by managed care penetration. In contrast to the spillover story, if fee-for-service costs were regressed on Medicare HMO penetration, the estimated coefficient would be positive.

In our IV context, the issue is similar, but we are concerned with whether enrollment shifts *induced* by payment changes are systematically related to health status or other enrollee traits that may affect spending. If the FFS beneficiaries who are induced by payment changes to leave the FFS system for HMOs are healthier than the typical FFS beneficiary, then the remaining FFS population may become less healthy on average. Such movement would generate estimates that would underestimate spillover effects.¹⁷ Recent evidence, however, indicates that there is no association between favorable selection into Medicare HMOs and county-level HMO penetration (Mello et al., 2003), suggesting that at the margin, shifts in HMO penetration associated with payment changes do not substantially alter the health status of fee-for-service enrollees. However, Cao and McGuire (2003), using service-level variation, find evidence of systematically healthier beneficiaries joining HMOs in markets with HMO penetration rates below 15%.

¹⁵ The figures that follow are generated from our sample of counties. See Section 4.2 for details on our analysis sample, including selection of counties.

¹⁶ This required collapsing the residuals to county-year cells, so the residuals used in the autoregression are averaged over all sample individuals in a given county in a particular year.

¹⁷ Of course, if less healthy beneficiaries are induced to leave fee-for-service Medicare for HMOs as payments change, then our measured spillover effect may overstate the magnitude of the true effect.

Despite the lack of clear evidence, we address this issue in several ways. First, we estimated models with a large set of health status controls, including covariates for general health status and a set of 16 disease indicators. Additionally, we investigate the association between payment changes and changes in the composition of our fee-for-service sample. In particular, we estimate models that replace spending with age and health status measures in order to test whether payment-induced changes in Medicare HMO penetration affected the composition of this group. Here, a finding that the fee-for-service population became younger or healthier, as payment rates alter penetration, would indicate that selection may taint our estimates in ways described above. Similarly, a finding that the fee-for-service population got older or less healthy would also represent compositional change. However, as we discuss below, we find no systematic evidence of any such compositional changes, implying that selection effects are not significantly affecting our estimates.

4. Data

4.1. Data description

We use data from the annual Cost and Use files of the Medicare Current Beneficiary Survey (MCBS) for the years 1994–2001. This period predates the rise in private FFS plans, which have grown rapidly but are not likely to generate the substantial spillovers. The MCBS is a nationally representative survey of Medicare beneficiaries which gathers information on respondents via a rotating panel. While the sampling frame includes elderly and disabled beneficiaries, we limit our analysis to individuals aged 65 and older. In addition, we exclude the roughly 10% of respondents who completed “facility” interviews, which were administered to individuals who could not complete the interview on their own and required a proxy to do so. Since we examine potential spillovers associated with Medicare managed care, we include only individuals who were consistently enrolled in fee-for-service Medicare in each wave of the survey.

The MCBS contains information on respondent health care utilization and expenditure. With respect to the latter, respondents are linked with claims data to ensure the accuracy of individual spending measures. The MCBS staff uses this information, in conjunction with information provided by respondents, to construct each respondent's total annual expenditure, which is our outcome of interest.¹⁸ We focus on total spending, rather than just fee-for-service Medicare spending, because spillovers may be wide-ranging. That said, fee-for-service Medicare expenditure accounts for about two-thirds of total expenditures in our samples. Indeed, though not reported, when we estimate models that replace total expenditure with Medicare-specific expenditures, our estimates provide slightly stronger evidence of spillovers. To better understand our findings, we also examine the impact of Medicare HMO penetration on selected categories of health care utilization including inpatient events, outpatient events, medical provider events and office visits.¹⁹

Our key covariates of interest are county-level estimates of Medicare HMO enrollment and the county-specific payment rates CMS uses to compensate managed care companies. Both of these variables are obtained from CMS.

We include a wide range of other covariates. From the MCBS, we include respondent demographics (e.g., income, race, living arrangements), health status (e.g., self-reported health status, past experience with a variety of diseases and disorders). We also add other county-specific variables including commercial HMO penetration and various measures of local medical resources as covariates, obtained from the Area Resource File. We merge all of this county-level information to our data using geographic identifiers available in restricted-use versions of the MCBS.

4.2. Analysis samples

Our primary sample eliminates the relatively few individuals with zero total annual expenditure. As a sensitivity check, we estimate models that include these individuals, assigning such respondents an expenditure of one dollar since we model log expenditure in our spending models. As mentioned, we eliminate proxy respondents and those under 65 years old which results in a sample of 77,963 individuals. Limiting our sample to those enrolled in fee-for-service Medicare for the entire year reduces this figure to 60,844 and missing information on key variables further reduces our sample size to 58,231. Excluding individuals with zero expenditure further drops the sample by about 2.6% to 56,754.

Since the MCBS contains several counties with relatively few individuals, we restrict our analysis to individuals in counties that contribute at least 15 observations over the 8 years of data we examine.²⁰ This restriction reduces the sample that excludes zero expenditure individuals to 53,188. Table 2 presents means and standard deviations for four samples. The first two columns represent samples we use to generate regression estimates, while the latter two columns represent ones that include all counties, regardless of the number of observations they contribute. Comparing the first and third columns as well as the second and fourth ones, it is apparent that there are no substantial differences associated with our restrictions. However, as expected, there are differences in average expenditure between samples that do and do not contain zero expenditure individuals, but these are slight given the relatively small fraction of individuals with zero expenditure.

¹⁸ In particular, we use the variable PAMTTOT which aggregates expenditures from 11 different sources to construct a measure of total expenditures.

¹⁹ Medical provider events include doctor visits, surgical and laboratory services, or purchases of medical equipment and supplies.

²⁰ Counties contributing fewer than 15 observations contribute an average of less than 4 observations over the 8 years in question or less than one-half of 1 observation per year, on average.

Table 2
Selected means and standard deviations

	With county restrictions		Without county restrictions	
	(1) Without zeroes (N = 53,188)	(2) With zeroes (N = 54,552)	(3) Without zeroes (N = 56,754)	(4) With zeroes (N = 58,231)
Total annual expenditures	8021 (14,469)	7821 (14,341)	8137 (14,657)	7930 (14,517)
Fraction zero expenditure	–	0.025 (0.156)	–	0.026 (0.159)
Medicare HMO penetration	8.40 (11.98)	8.39 (11.97)	8.58 (12.06)	8.56 (12.04)
Payment rate	444.88 (100.73)	444.84 (100.74)	447.77 (103.17)	447.72 (103.20)
Age	76.88 (7.48)	76.81 (7.49)	76.88 (7.50)	76.81 (7.51)
Female	0.591 (0.492)	0.588 (0.492)	0.589 (0.492)	0.587 (0.492)
Excellent health	0.147 (0.354)	0.149 (0.356)	0.146 (0.353)	0.149 (0.356)
Very good health	0.272 (0.445)	0.274 (0.446)	0.271 (0.445)	0.273 (0.445)
Good health	0.323 (0.468)	0.322 (0.467)	0.323 (0.468)	0.321 (0.467)
Fair health	0.182 (0.386)	0.180 (0.384)	0.183 (0.387)	0.181 (0.385)
Poor health	0.074 (0.262)	0.073 (0.260)	0.075 (0.263)	0.074 (0.261)

Notes: All four samples include only beneficiaries enrolled in traditional fee-for-service Medicare. The first two columns represent the samples we use to generate regression estimates. The second two columns relax our restriction that a county contribute at least 15 observations over the 8 years of data in question. Total annual expenditures and payment rates are in nominal dollars. Standard deviations are in parentheses.

5. Results

5.1. Main estimates

In Table 3, we present OLS and IV estimates of the impact of Medicare HMO penetration on the expenditure of fee-for-service enrollees. In the OLS models, presented in the first two columns, the estimated coefficients on Medicare HMO penetration are small, relative to the IV estimates we will present. For example, we estimate that a 1% point increase in Medicare HMO penetration is associated with a decrease of about 0.3% in expected expenditures by fee-for-service enrollees. Despite their relatively small magnitudes, the signs of these coefficients are consistent with the existence of spillovers.

Table 3 also presents our IV spending estimates. Across the specifications presented, the estimated coefficient on Medicare HMO penetration is negative and relatively large in magnitude. Columns 3 and 4 present a base specification, first without zero expenditure individuals and then including such individuals, respectively. These estimates imply that a 1% point increase in Medicare HMO enrollment is associated with a reduction in expected fee-for-service expenditure of between 0.7 and 0.8%. Over our sample period, mean Medicare HMO penetration increased by approximately 8% points. By extrapolation, these estimates imply that the rise of managed care reduced fee-for-service expenditure by about 6%, relative to the level that would have obtained in the absence of such penetration. It is also worth noting that, consistent with recent work, we estimated versions of these specifications that allowed for a quadratic in Medicare HMO penetration. However, the squared term was consistently close to zero and insignificant, suggesting no improvement over our linear parameterization.

Of course, the reliability of our estimates is only as good as the validity of our instruments. In Table 3, we present some additional evidence on this issue. First, our instruments explain a significant amount of the variation in Medicare HMO penetration, controlling for county fixed-effects and other right-hand-side variables. In particular, the partial R^2 is at least 0.14 in all specifications and the F -test that the coefficients on the instruments are all zero is over 37 in all specifications,

Table 3
Estimated effect of Medicare HMO penetration on log annual spending—OLS and IV estimates

	OLS			IV		
	(1) Base w/ o zeros	(2) Base w/ zeros	(3) Base w/ o zeros	(4) Base w/ zeros	(5) Preferred specification	(6) Excludes CA & FL
Medicare HMO penetration	–0.00353 (0.00157) [2.26] {8.4%}	–0.00296 (0.00204) [1.45] {8.4%}	–0.00784 (0.00368) [2.13] {8.4%}	–0.00756 (0.00532) [1.42] {8.4%}	–0.00939 (0.00409) [2.29] {8.4%}	–0.00896 (0.00456) [1.97] {6.5%}
First stage F -statistic	–	–	37.22	37.26	45.36	39.54
Partial R^2	–	–	0.16	0.16	0.14	0.17
N	53,188	54,552	53,188	54,552	53,188	47,853

Notes: Dependent variable is log of annual spending. In addition to county and year fixed effects, all models control for age, age squared, race, income, household size, marital status, general health status, sixteen disease indicators, smoking status and body mass index. Model (1) is our OLS base specification without zero expenditure individuals, Model (2) includes these zero expenditure individuals, adding one dollar of expenditure so that logs may be taken, Model (3) is our IV base specification without zero expenditure individuals, Model (4) adds zero expenditure individuals, Model (5), our preferred specification, adds controls for county-level commercial HMO penetration, county-level medical resources, and the availability of supplemental coverages to Model (3), and Model (6) is a sensitivity check which excludes California and Florida observations. The over-identification test statistic is distributed $\chi^2(1)$. Absolute values of t -ratios in brackets and mean Medicare HMO penetration in curly brackets. Standard errors, in parentheses, are adjusted for clustering at the county-level.

Table 4
Estimated effect of Medicare HMO penetration on health care utilization—IV estimates

Dependent variable	Any	Number	Number >0
Inpatient events	−0.00212 (1.84) {0.222} [53,188]	−0.00471 (1.58) {0.390} [53,188]	−0.00565 (0.61) {1.709} [11,793]
Outpatient events	−0.00113 (0.51) {0.712} [53,188]	−0.05103 (1.78) {3.939} [53,188]	−0.04988 (1.48) {5.536} [37,870]
Medical provider events	−0.00029 (0.74) {0.981} [53,188]	−0.11005 (1.06) {24.987} [53,188]	−0.11316 (1.07) {25.494} [52,179]
Office visits	0.00093 (0.70) {0.861} [53,188]	0.02568 (1.03) {5.976} [53,188]	0.02002 (0.81) {7.051} [45,806]

Notes: This table presents estimates from twelve separate IV regressions. Columns represent the model specification and rows represent the dependent variable in question. In particular, the column labeled “any” presents models where the dependent variable equals one if the individual has experienced an event indicated by a given row, the column labeled “number” presents models where the dependent variable is the number of relevant events, and the column labeled “number >0” present models where the sample is restricted to individuals with strictly positive events. Models correspond to our most preferred specification which is represented by Column 5 of Table 3. Absolute values of *t*-ratios in parentheses, dependent variable mean in curly brackets, and sample sizes in square brackets. Standard errors adjusted for clustering at the county level.

relative to a rule-of-thumb of 10. Thus, there is no evidence that our estimates suffer from a weak instrument problem. When combined with the diagnostic results of minimal autocorrelation in spending growth among fee-for-service beneficiaries and similar spending growth prior to the BBA in counties treated more and less generously by it, we believe these are reasonable instruments.²¹

Column 5 presents an estimate of β from our most preferred specification. It adds a set of county-level controls as well as information on supplemental coverage to the specification presented in Column 3. In particular, this specification adds controls for county-level commercial HMO penetration, county-specific medical resources, including measures of hospital beds, total medical doctors, medical specialists, hospice beds and long term beds, as well as person-specific supplemental coverage information including the availability of employer-sponsored health insurance coverage and Medicaid eligibility. As can be seen in Column 5, our estimate of the impact of a 1% point change in Medicare HMO penetration rises about 25% when area controls are added, to nearly 1%. This figure represents an economically significant effect that continues to imply non-trivial spillover. It implies that over our sample period, the rise of managed care reduced fee-for-service expenditure by about 7%, relative to the level that would have obtained in the absence of such penetration. Given the fullness of this model, this is our preferred estimate.

Finally, Column 6 presents a specific robustness check. In particular, it eliminates observations from California and Florida, areas where Medicare managed care grew rapidly in the 1990s. The concern is that estimates from our preferred specification may be driven by changes in these areas. However, the estimate in Column 6 suggests that the estimate from our preferred model is not dependent on the California and Florida experience.²² In particular, this coefficient on Medicare HMO penetration, −0.00896, is precisely estimated and also implies an effect of just under 1%, quite similar to our preferred estimate.²³

5.2. Utilization models

In order to better understand the nature of our spending estimates, we estimate IV specifications of the impact of Medicare HMO penetration on the following measures of utilization: inpatient events, outpatient events, medical provider events and office visits. Corresponding estimates, from models that exclude zero expenditure individuals and implement our most

²¹ Additionally, the Hansen test of the over-identifying restrictions does not reject in any specification. However, the over identifying restrictions are the consequence of adding nonlinear transformation of the payment rate to the instruments set (which are statistically significant in the first stage). Thus while we believe the Hansen test is informative, the test statistic must be interpreted recognizing it relies on non-linear transformation of payments to generate the over identification.

²² Though not reported, estimates from models that exclude California and Florida separately also yield similar estimates.

²³ Finally, we also estimate our preferred specification for the years 1996–1999, inclusive, to capture variation in payment rates stemming primarily from the BBA of 1997. The resulting estimate is quite similar to our preferred model estimate though it is no longer statistically different from zero, likely because we lose roughly half of our original sample in restricting to 1996–1999.

Table 5
Estimated effect of Medicare HMO penetration on log annual spending—IV estimates by plausibly “high” and “low” use Medicare beneficiaries

Structural equation estimates	Without zeroes	With zeros
Preferred models	–0.00939 (2.29) [53,188]	–0.01028 (1.76) [54,552]
“High-Use” individuals	–0.01084 (2.30) [40,307]	–0.01503 (2.34) [40,829]
“Low-Use” individuals	–0.00689 (0.65) [12,881]	0.00456 (0.32) [13,723]

Notes: Dependent variable is log of annual spending. “High-Use” individuals are those with at least one chronic condition as described in the text, while “Low-Use” individuals include those without any of these conditions. Models “without zeroes” drop individuals with zero expenditure, while models “with zeroes” assign one dollar of spending to these individuals and include a dummy variable indicating if an individual has zero expenditure. Models correspond to our most preferred specification which is represented by Column 5 of Table 3. Absolute values of *t*-ratios in parentheses and sample sizes in brackets. Standard errors adjusted for clustering at the county-level.

preferred specification, are presented in Table 4.²⁴ Since the distributions of these events are skewed, we estimate three sets of models that correspond to different specifications of the dependent variable. The three specifications indicate: (a) whether an individual experienced a given event, (b) the number of events, and (c) the number of events, conditional upon the number being greater than zero. The estimates indicate that the impact of Medicare HMO penetration on utilization appears to be occurring on the intensive margins of outpatient and medical provider events. The magnitude of these effects is not trivial. Conditional on having an outpatient event, a 1% point increase in Medicare HMO penetration reduces the expected number of visits by nearly 1% when evaluated relative to the mean of the dependent variable. There is also some evidence that penetration impacts inpatient events, on both extensive and intensive margins. While not as statistically precise as estimates in Table 3, our findings with respect to utilization are consistent with our main finding that Medicare HMO penetration reduces expenditures by fee-for-service enrollees in that they provide a mechanism for such reductions.

5.3. Exploring our main estimates in more detail

We next allow the impact of Medicare HMO penetration to vary by the level of individual health care use. In particular, we are interested in whether the effect of penetration differs across plausibly high- and low-use individuals. To this end, we proxy “high-use” and “low-use” by whether the individual reports ever having been told they have one of four chronic conditions which include coronary heart disease, arthritis, diabetes or some “other” heart problem. We separate respondents into two groups – those who report at least one of these conditions and those who report none – and refer to the former as “high-use” and the latter as “low-use”. Mean spending levels support this characterization—individuals with at least one chronic condition had an average annual expenditure of \$7776, while those individuals who report none of these chronic conditions had an average expenditure of \$4686.²⁵ We hypothesize that the effects of HMO penetration will be larger in the population with chronic disease because HMOs target chronic disease and because care management for these conditions may be more prone to systematic approaches and thus spillover. For example, Chernew (1995b) reports that the impact of HMO on diagnostic testing was much greater for patients with chronic diseases.

We explore this possibility in Table 5. As can be seen, the implied spending reductions for higher-use individuals are much larger in magnitude than their low-use counterparts. In particular, while the implied reduction for the former group ranges from 1.1 to 1.5%, we find no systematic relationship for low-use individuals. This suggests that the savings associated with increasing Medicare HMO penetration are derived from individuals with relatively higher use and expenditure. That said, our data do not allow us to distinguish whether reductions among high-use beneficiaries represent reductions in superfluous or necessary care.²⁶

5.4. Are changes in composition of FFS beneficiaries driving our spillover estimates?

Despite the advantages of instrumental variables estimation and the quality of our instruments, the issue of who is induced to switch between FFS and HMOs remains. Recall that if fee-for-service beneficiaries who select into HMOs are, on average, healthier than the FFS population, then our estimates may be due to the change in the composition of this group, rather than true spillover. In this case, we would be overestimating the true spillover effect. Conversely, if the FFS beneficiaries who

²⁴ Estimates from models that include zero expenditure individuals yield similar estimates.

²⁵ These figures are computed from our 1994 sample and include the relatively few beneficiaries with zero expenditure. Corresponding figures from our sample without individuals with zero expenditure are \$7908 and \$5041, respectively.

²⁶ Finally, as a sensitivity check to what we present in Section 5, we re-estimated our preferred specification including proxy respondents, whose number is roughly 10% of our main sample. The relevant estimate is nearly identical to the corresponding estimate from our preferred specification.

Table 6

Estimated effects of Medicare HMO penetration on selected demographic and health-related characteristics of FFS sample

Dependent variables	Without zeroes (N = 53,188)	With zeroes (N = 54,552)
Age (in years)	0.04348 (1.75)	0.03771 (1.46)
Age \geq 75	0.00165 (0.89)	0.00125 (0.67)
Excellent health reported	−0.00130 (1.07)	−0.00083 (0.36)
Poor health reported	0.00005 (0.05)	0.00107 (0.31)

Notes: This table reports coefficients on Medicare HMO penetration variable from eight separate regressions. The regressions all include a full set of covariates as described in Table 3 and correspond to our most preferred specification which is represented by Column 5 in Table 3. Absolute values of *t*-ratios, in parentheses, are based on standard errors adjusted for clustering at the county-level.

leave are, on average, less healthy than the FFS population, we may underestimate this effect. We test for such compositional change by investigating the relationship between Medicare HMO penetration and various demographic and health-related variables to assess the likelihood of such compositional change. In particular, we estimate our most preferred specification, replacing the dependent variable with age and health-related measures, preserving our basic empirical strategy.

As seen in Table 6, we find no evidence that FFS recipients in our sample became less healthy over time. Indeed, there is some evidence that our FFS sample became older as a result of payment-induced increases in Medicare HMO penetration, perhaps suggesting that the remaining FFS population became less healthy, which, in principle, should bias our strategy against finding evidence of spillover effects. However, the estimated effects are practically very small. For example, the estimates suggest that a 1% point increase in Medicare HMO penetration is associated with roughly a 0.04 year increase in age, on average. Moreover, there is no evidence that the fraction of the FFS population at least 75 years old increased. Perhaps most directly, we find no systematic relationship between “excellent” and “poor” health and penetration, suggesting no compositional change with regard to health status. These findings are consistent with Mello et al. (2003) who find no systematic evidence of an association between favorable selection into Medicare HMOs and county-level HMO penetration.

6. Conclusions

The impact of managed care enrollment on spending on fee-for-service beneficiaries has important implications for the Medicare program. Substantial spillovers suggest that policy makers should encourage HMO enrollment. In the context of current payment systems, this would imply greater payment to managed care plans than implied simply by the cost of serving beneficiaries in HMOs. Yet the externality would be important to consider even if other payment systems were used.

This paper suggests that such spillovers are substantial. Using IV models that correct for the endogeneity of HMO penetration changes across counties, we estimate that a 1% point increase in county-level Medicare HMO penetration is associated with nearly a 1% reduction in individual-level annual spending by fee-for-service enrollees. The findings are robust to several sensitivity checks and a number of diagnostic exercises suggest that our instruments are valid. Our spending estimates are also supported by utilization models which suggest a mechanism through which Medicare HMO penetration affects spending in the fee-for-service sector. Finally, the spending reductions implied by our estimates seem to be derived from less healthy and consequently high-cost beneficiaries, as opposed to their healthier counterparts.

There are several limitations to our work. First, our findings should be interpreted as applying to the range of HMO penetration influenced by payment policy. Given their substantial magnitude, we suspect additional large changes in penetration might translate into somewhat smaller effects. Second, our results do not apply to private FFS plans, which have benefited from generous payment and do not likely generate substantial spillovers. Third, our analysis predates the Medicare Modernization Act (MMA) of 2003. The MMA expanded the set of plans available to Medicare beneficiaries, added a prescription drug benefit, and enhanced the system of risk adjustment. Spillovers may differ in the post MMA era if these changes alter the way managed care plans behave or the set of people who selected Medicare managed care plans. Fourth, the nature of our empirical specification assumes that payment-induced changes in Medicare HMO penetration affect relevant outcomes (i.e., spending and utilization) contemporaneously. In other words, our specification identifies short-run impacts of Medicare HMO penetration. To address this, we attempted to estimate a model that included current Medicare HMO penetration and 1-year lagged Medicare HMO penetration, both as endogenous variables. Following our present empirical strategy, we instrumented each with payment rate for the relevant period. Unfortunately, these estimates and their corresponding standard errors “blow up” somewhat due to high (intertemporal) correlations between Medicare HMO penetration and payment rates within counties on a year-to-year basis. As such, we can shed little light on the temporal evolution of the effects we have identified.

Yet given the present estimated effects, policy makers might well be advised to consider spillovers in policy debates. Some of the costs of increased payments to plans is likely offset by savings in the fee-for-service system, and possibly the health care system overall. Continued work to assess the magnitude of spillovers is needed to allow policy makers to balance the benefits of HMO penetration with the payment rates necessary to encourage plan participation in the program.

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