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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 enrollees. We find that increasing penetration leads to reduced health care spending on fee-for-service beneficiaries. In particular, a one percentage point increase in Medicare HMO penetration reduces such spending by .9 percent. We estimate similar models for various measures of health care utilization and 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 — 20% of Medicare beneficiaries are currently 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 structure such as

[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.

the number of hospitals, beds or available services over time (Chernew, 1995a). 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. More generally, additional steps to increase enrollment in HMOs might be warranted, if it encouraged savings in FFS Medicare or more broadly.

Economists' interest in such spillover effects is captured in the growing body of work examining the impact of managed care enrollment on Medicare costs or utilization (Baker and Corts, 1996; Baker, 1997; Baker and Shankarkumar, 1997; Cutler and Sheiner, 1997; McClellan and Baker, 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

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

activities in the Medicare HMO market to influence the expenditures associated with treating Medicare fee-for-service beneficiaries.[5]

Although the overall body of literature supports the existence of spillover effects, research explicitly examining the impact of Medicare HMO enrollment on expenditures by fee-for-service beneficiaries is relatively small and contributions to this literature tend to ignore the potential endogeneity of HMO penetration, treating it as exogenous. 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.[6]

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 of selection effects and 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, 2002; Gowrisankaran and Town, 2004). Our identification comes from longitudinal variation in payment rates over our study period (1994-2001) and reflects, in

[5] 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; Melnick et al., 1989b; Zwanziger et al., 1994).
[6] 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 1993-1996, prior to the BBA. These latter authors, however, examine utilization and not spending.

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 one percentage point increase in county-level Medicare HMO penetration is associated with a .9 percent 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 broad 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.

In the following section, we provide background on the progression of Medicare managed care and its relation to our work. Section 3 presents our empirical strategy, which relies on county-specific payment rates as instruments for county-level Medicare HMO penetration and discusses relevant issues, including the possibility that beneficiary selection affects our spillover estimates. Section 4 describes our data, including the construction of key variables and detailed descriptions of the samples we analyze. Section 5 presents our estimates and Section 6 concludes.

2. Background

In 1982, Congress passed the Tax Equity and Fiscal Responsibility Act (TEFRA). Under this statute, the Health Care Financing Administration (HCFA) was directed to contract with HMOs to provide a managed care option to Medicare enrollees. Under Medicare+Choice, 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.[7] 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.

An important lever that Medicare has to influence beneficiary participation in HMOs is payment policy. 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. The findings of several studies suggest payment rates affect HMO participation in the Medicare program (Cawley, Chernew and McLaughlin, 2002;

[7] 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.

Town and Liu, 2002). However, none of these studies directly measures the impact of payment changes on aggregated HMO enrollment at the county-level.

In addition to estimating the impact of payments on enrollment, it is important for forecasting and policy purposes to understand the fiscal impact of Medicare HMO enrollment on the program. Medicare HMO enrollment has both direct and indirect impacts on the Medicare 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 the dollar value of care individuals would have used had they remained in the traditional fee-for-service system. Because payment rates for Medicare HMOs were historically tied to the local costs of care for enrollees in the fee-for-service portion of Medicare, and because HMOs tended to attract a relatively healthier population, analysts have felt that growing HMO enrollment would increase the total costs of the Medicare program. Any cost savings obtained by HMOs were either captured by the HMOs, themselves, or competed away via more extensive benefit packages to beneficiaries. For example, analysis by MedPAC suggests that spending by Medicare for HMO participants was four percent higher relative to demographically similar beneficiaries in traditional Medicare (MedPAC, 2002). Yet, this calculation does not adjust for potential spillover effects. If there are spillover effects from Medicare HMO penetration, such efficiencies may reduce the cost for caring for individuals who do not enroll in Medicare HMOs. To some extent, these savings may offset the direct effect of Medicare HMO enrollment.

3. Empirical Strategy and Related Issues

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

$$LogExpenditure_{ict} = \alpha_c + \gamma_t + \beta MC_{ct} + \lambda X_{it} + \varepsilon_{ict}, \qquad (1)$$

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

[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 hear problem not included in this list.

including overall commercial HMO penetration and various measures of county-specific medical resources.

The disturbance term in equation (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.

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]

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-

[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.

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-services 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. To explore the potential for using payment as an instrument, we estimated 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.[13] 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 at conventional levels of significance.

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 two percent 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

[12] More specifically, these are five-year averages, starting eight years prior to time t.
[13] 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.

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 six percent.[14] In addition to reducing the level of payments, the BBA also diminished the variance in payment rates across counties.

While the impact of the BBA on payment rates is likely unrelated to the error term in equation (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.[15] The validity of these county-level payments rates also requires payment changes to be unrelated to existing trends in spending across counties. In particular, the counties that experienced relatively generous or stingy growth in payments due to the BBA might differ systematically in this regard.

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.[16] 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

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[14] Source: Congressional Budget Office (1999).

[15] A standard rule-of-thumb is that this F-statistic be greater than ten. All of our F-statistics are greater than thirty-seven. In addition, we report the partial R-squared for each first-stage regression.

[16] 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.

experienced growth in spending on fee-for-service beneficiaries of 9.2 percent, while the latter counties experienced growth of 10 percent. 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 percent) relative to those counties whose payment growth was accelerated following the BBA (16.7 percent).

Finally, we note that 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 concern 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 FFS beneficiaries who are healthier, on average, than the initial FFS population are induced by payment changes to leave the FFS system for HMOs, 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, suggests 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 selection in markets with HMO penetration rates below fifteen percent.

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 sixteen disease indicators. Additionally, we investigate the association between payment changes and changes in the composition of our fee-for-service sample over time. 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 be driving compositional changes that could 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 in section 5.4, we find no systematic evidence of any such compositional changes, implying that our estimates represent true spillover.

4. Data

4.1 Data description

[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.

We use data from the annual Cost and Use files of the Medicare Current Beneficiary Survey (MCBS) for the years 1994 to 2001, inclusive. This period pre-dates 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 sixty-five and older. In addition, we exclude the roughly ten percent 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 detailed information on 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) as well as information on 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

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

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] Another set of key variables included county-level estimates of Medicare HMO enrollment and the county-specific payment rates CMS uses to compensate managed care companies, both of which are available from CMS. We also add other county-specific variables including commercial HMO penetration and various measures of local medical resources as covariates. 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 institutionalized individuals and those under sixty-five 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 percent to 56,754.

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

Since the MCBS contains several counties with relatively few individuals, we restrict our analysis to individuals in counties that contribute at least fifteen observations over the eight 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.

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 one percentage point increase in Medicare HMO penetration is associated with a decrease of about 0.3 percent 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. As noted earlier, since it is likely that the error terms of these equations are correlated

[20] Counties contributing fewer than fifteen observations contribute an average of less than four observations over the eight years in question or less than one-half of one observation per year, on average.

with county-level Medicare HMO penetration, OLS may provide biased estimates of the true relationship. For reasons also discussed earlier, this bias is likely to be negative; if so, the true impact of HMO penetration on fee-for-service expenditure will be understated or biased towards zero.

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 one percentage point increase in Medicare HMO enrollment is associated with a reduction in expected fee-for-service expenditure of between 0.7 and 0.8 percent. Over our sample period, mean Medicare HMO penetration increased by approximately eight percentage points. By extrapolation, these estimates imply that the rise of managed care reduced fee-for-service expenditure by about six percent, 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 thirty-seven in all specifications, relative to a rule-of-thumb of ten. 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 coverages' 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.[22] As can be seen in Column 5, our estimate of the impact of a one percentage point change in Medicare HMO penetration rises about 25% when area controls are added, to nearly one percent. This figure represents an economically significant effect that continues to imply non-trivial spillover. Again, given the fullness of this model, it is our preferred specification.

Finally, Column 6 presents a specific robustness check. In particular, it eliminates observations from California and Florida, areas where Medicare managed care grew

[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] Information on county-level medical resources was drawn from the appropriate versions of the Area Resource File (ARF).

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. In particular, this coefficient on Medicare HMO penetration, -0.00896, is precisely estimated and also implies an effect of just under one percent, 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 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 one percentage point increase in Medicare HMO penetration reduces the expected number of visits by nearly one percent when evaluated relative to the mean of the dependent variable. There is also some evidence that penetration impacts inpatient events, on both

[23] Though not reported, estimates from models that exclude California and Florida separately also yield similar estimates.
[24] Estimates from models that include zero expenditure individuals yield similar estimates.

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 $7,776, while those individuals who report none of these chronic conditions had a similar expenditure of $4,686.[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.

[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 $7,908 and $5,041, respectively.

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 percent, 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.

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 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 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 changes in Medicare HMO penetration, perhaps suggesting that the remaining sample 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 one percentage 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 seventy-five 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

Quantifying the impact of managed care enrollment on spending on fee-for-service beneficiaries is an important policy exercise, especially since such spillovers are generally ignored in considering future program costs. 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 one percentage point increase in county-level Medicare HMO penetration is associated with nearly a one percent 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.

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. Moreover, our results do not apply to Private FFS plans, which have benefited from generous payment and do not likely generate substantial spillovers. Yet given the present estimated effects, policy makers might well be advised to encourage greater HMO presence because at least some of the costs of increased payments to plans would be offset by savings in the fee-for-service system, and likely the health care system overall. At a minimum, the possibility of such spillovers should be considered in policy debates.

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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.

Table 2. Selected means and standard deviations.

	With county restrictions		Without county restrictions	
	(1)	(2)	(3)	(4)
	Without Zeroes (N=53,188)	With Zeroes (N=54,552)	Without Zeroes (N=56,754)	With Zeroes (N=58,231)
Total annual expenditures	8,021 (14,469)	7,821 (14,341)	8,137 (14,657)	7,930 (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 fifteen observations over the eight years of data in question. Total annual expenditures and payment rates are in nominal dollars. Standard deviations are in parentheses.

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

	OLS		IV			
	Base w/o Zeros	Base w/ Zeros	Base w/o Zeros	Base w/ Zeros	Preferred Specification	Excludes CA & FL
	(1)	(2)	(3)	(4)	(5)	(6)
Medicare HMO Penetration	-0.00353	-0.00296	-0.00784	-0.00756	-0.00939	-0.00896
	(0.00157)	(0.00204)	(0.00368)	(0.00532)	(0.00409)	(0.00456)
	[2.26]	[1.45]	[2.13]	[1.42]	[2.29]	[1.97]
	{8.4%}	{8.4%}	{8.4%}	{8.4%}	{8.4%}	{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	-0.00471	-0.00565
	(1.84)	(1.58)	(0.61)
	{0.222}	{0.390}	{1.709}
	[53,188]	[53,188]	[11,793]
Outpatient events	-0.00113	-0.05103	-0.04988
	(0.51)	(1.78)	(1.48)
	{0.712}	{3.939}	{5.536}
	[53,188]	[53,188]	[37,870]
Medical provider events	-0.00029	-0.11005	-0.11316
	(0.74)	(1.06)	(1.07)
	{0.981}	{24.987}	{25.494}
	[53,188]	[53,188]	[52,179]
Office visits	0.00093	0.02568	0.02002
	(0.70)	(1.03)	(0.81)
	{0.861}	{5.976}	{7.051}
	[53,188]	[53,188]	[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.

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

	Without Zeroes	With Zeros
Structural equation estimates		
Preferred Models	-0.00939	-0.01028
	(2.29)	(1.76)
	[53,188]	[54,552]
"High-Use" Individuals	-0.01084	-0.01503
	(2.30)	(2.34)
	[40,307]	[40,829]
"Low-Use" Individuals	-0.00689	0.00456
	(0.65)	(0.32)
	[12,881]	[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.

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	0.03771
	(1.75)	(1.46)
Age ≥ 75	0.00165	0.00125
	(0.89)	(0.67)
Excellent Health Reported	-0.00130	-0.00083
	(1.07)	(0.36)
Poor Health Reported	0.00005	0.00107
	(0.05)	(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.