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How does Risk-selection Respond to Risk-adjustment? Evidence from the Medicare Advantage Program

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Abstract

Medicare administers a traditional public fee-for-service (FFS) plan while also allowing enrolles to join government-funded private Medicare Advantage (MA) plans. We model how selection and differential payments—the value of the capitation payments the firm receives to insure an individual minus the counterfactual cost of his coverage in FFS—change after the introduction of a comprehensive risk adjustment formula in 2004. Our model predicts that firm screening efforts along dimensions included in the model ("extensive-margin" selection) should fall, whereas screening efforts along dimensions excluded ("intensive-margin" selection) should increase. These endogenous responses to the risk-adjustment formula can in fact lead differential payments to increase. Using individual-level administrative data on Medicare enrollees from 1994 to 2006, we show that while MA enrollees are positively selected throughout the sample period, after risk adjustment extensive-margin selection decreases whereas intensive-margin selection increases. We find that differential payments actually rise after risk-adjustment, and estimate that they totaled $23 billion in 2006, or about six percent of total Medicare spending.

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1 Introduction

Since the 1980s, the Medicare program has offered beneficiaries the choice between a traditional, government-administered plan as well as government-funded private plans, known as Medicare Advantage (MA) plans, which today cover roughly a quarter of Medicare's 47 million members. Offering individuals a choice between a government-administered option and a government-funded but privately administered option is a common feature in government programs—for example, in the United States, parents can increasingly choose between traditional public schools and charter schools, and Britain's latest employer pension reform involves a default government option competing alongside plans offered by private insurance companies and fund managers.

Critics of traditional public provision often point to its minimal incentives for cost-control. The traditional Fee-for-Service (FFS) Medicare program, for example, pays hospitals, physicians and other health care providers on the margin for services rendered to enrollees, and thus some providers may deliver services with little clinical benefit if the payment covers more than their marginal cost. In contrast, the government pays MA plans a fixed capitation payment to cover an enrollee's expected health costs, giving plans an incentive to reduce the provision of low-value services.

But, as noted by Newhouse (1996) and others, this arrangement also gives plans the incentive to enroll individuals whose expected costs are lower than their capitation payment, which does not reduce total Medicare costs but merely transfers government funds to private plans and "over-priced" enrollees. To address this concern, in 2004 the Center for Medicare and Medicaid Services (CMS), the federal agency that administers the Medicare program, introduced a comprehensive risk-adjustment formula based on more than seventy disease categories.[1] The formula was used the generate a *risk score* for each individual, and capitation payments were determined by multiplying the risk score by a county-level cost factor. The objective of the formula was to reduce differential payments—the cost to the Medicare program of financing an individual's coverage when she is in a private MA plan minus the counterfactual cost to Medicare had the FFS program covered her directly.

This paper analyzes how introducing risk-adjustment changes selection patterns and differential payments. To our knowledge, we present the first comprehensive economic analysis of the effort, which directly affects reimbursement for more than 11 million Medicare recip-

[1]Private Medicare plans have had many official names since the 1980s: Medicare HMOs, Medicare-plus-choice plans, and, most recently, Medicare Advantage plans. Similarly, the Center for Medicare and Medicaid Services was until 2001 named the Health Care Financing Administration. For simplicity, in the paper we will always refer to private Medicare plans as Medicare Advantage (MA) plans and the agency that administers Medicare as CMS, regardless of the time period we are discussing.

ients. We analyze risk-adjustment more generally by developing a model of firm screening efforts, and describe conditions under which risk-adjustment can reduce differential payments, as well as conditions under which it can actually increase them. Empirically, we find that risk-adjustment as attempted in the Medicare Advantage program falls into the latter category, with differential payments rising substantially in the years after its adoption. More generally, our results show that using additional information to determine prices can actually aggravate problems associated with asymmetric information, as in Finkelstein and Einav (2010).

We begin by modeling how risk adjustment influences the optimal screening efforts of profit-maximizing MA plans. Before risk adjustment, firms have incentives to enroll the lowest-cost enrollees along all dimensions observable to them. After risk-adjustment, they reduce efforts to screen along dimensions included in the risk-adjustment formula and increase efforts along dimensions excluded from the formula. Thus, after risk-adjustment the prevalence of conditions compensated by the formula will rise among MA enrollees ("extensive-margin selection" falls), but their actual cost conditional on their risk score falls ("intensive-margin selection" increases), as they are now more powerfully selected along dimensions not included in the formula. Thus, both before and after risk-adjustment, MA enrollees are positively selected with respect to underlying cost, but due to different mechanisms.

The success of risk adjustment, however, does not rest on whether selection patterns change, but whether it reduces differential payments. We show that under mild assumptions (which the Medicare risk formula meets) risk-adjustment will narrow the gap between capitation payments and expected FFS costs when selection patterns do not change from their pre-period patterns. However, we show that in the presence of an increase in intensive selection, there are conditions under which risk-adjustment can increase differential payments. Thus, whether risk-adjustment "works" is an empirical question, and an important focus of the empirical work in the paper.

The empirical work begins, however, by providing support for the selection predictions of the model, using both individual- and county-level data on Medicare expenditures. First, we show that risk scores of individuals who select into MA plans increase after risk-adjustment, consistent with plans devoting less energy to screening along the variables included in the formula. Second, conditional on the risk score, those switching to MA plans have lower costs after risk-adjustment, suggesting plans intensify screening efforts along variables excluded from the formula. Indeed, costs are negatively correlated with the probability an FFS enrollee selects into MA both before and after risk-adjustment, though the relative importance of selection along different dimensions has changed.

We then focus on the effect of risk-adjustment on differential payments. We first show that, as predicted by our model, had selection patterns not changed from the pre-period, differential payments would have indeed fallen under risk-adjustment. Specifically, we find that before risk-adjustment, an individual switching from FFS to MA costs the Medicare program an additional $1,800 relative to the cost had she remained in FFS. However, this differential payment is nearly halved when we replace MA enrollees' actual capitation payments with their capitation payment had the HCC model been used.

Of course, selection patterns did change, and when we instead consider differential payments for those who actually switched to MA in the risk-adjustment era, we encounter a very different picture. Differential payments actually increase after risk-adjustment, even after adjusting MA payments downward in the post-period to account for temporarily enhanced payments given to plans to ease their transition to risk-adjustment.

In our model, MA plans expend resources in order to improve selection along different observable dimensions of health, but we do not specify or micro-found how they do so. One possible strategy would be to differentially retain profitable enrollees by treating them better than unprofitable ones in ways the government cannot observe or regulate. Throughout our sample period, we find that, relative to FFS enrollees, MA enrollees' satisfaction with their health care is more positively correlated with their self-reported health. This gradient appears for almost every measure recorded, from enrollees' perceptions regarding their out-of-pocket costs to their doctors' concern for their well-being. We interpret these results as strongly suggesting that MA plans dedicate resources to keeping the healthiest patients happiest, though we believe that future work on how MA plans target and treat enrollees based on health status and expected profits is warranted.

Given the evidence we present suggesting that risk adjustment "didn't work," a natural question is how the formula can be improved. We close the paper by discussing the challenges inherent in improving the current model. First, we show that any recalibration of the model will likely lead to upwardly biased payments to MA plans. Recall that after risk-adjustment, enrollees with the lowest costs conditional on their risk score join MA. Because the risk-adjustment model can only be calibrated with the remaining FFS population (MA plans do not report cost data to the government, as their doing so would perhaps weaken their incentives to cost-minimize), that population will now have, conditional on the risk score, the most expensive cases.

Second, we discuss the costs and benefits of simply including more risk categories. On the one hand, making the formula more detailed might limit intensive selection. On the other hand, it potentially exacerbates plans' incentives to "intensively code"—CMS's term for MA plans' tendency to more aggressively document disease conditions included in the risk-adjustment formula relative to FFS providers.[2]

In addition, adjusting the formula for every possible condition means the government would essentially reimburse MA plans for their marginal costs, exactly the situation the plans were created to overcome. Finally, adding more categories and thus having fewer individuals per category could increase estimation error in the government's risk adjustment coefficients, which would give MA plans an incentive to select beneficiaries with 'over-priced' conditions, aggravating the problem of differential payments.

While we focus on Medicare, our results speak to the challenges facing risk-adjustment more generally. The use of risk-adjustment in health insurance markets is set to increase substantially: the 2010 Affordable Care Act requires risk adjustment in the individual and small group health insurance market starting in 2014. Recent work (e.g., (Epple and Romano, 2008)) has even suggested conditioning school voucher payments on students' ability, the educational analogue to conditioning capitation payments on disease scores. By selecting individuals with low costs conditional on their risk scores, MA firms' behavior is analogous to the worker who focuses on the contractable task to the detriment of all others (as in Holmstrom and Milgrom 1991 and Baker 1992) or the instructor who "teaches to the test" at the expense of other educational goals (as in Jacob and Levitt 2003). Our results suggest that regulators of risk-adjusted markets should be alert to firms responding endogenously to the formula.

Our results also highlight distributional issues associated with "contracting out" social insurance programs. The selection and differential payment patterns suggest not only that Medicare may have overpaid for the coverage of MA enrollees relative to what they would have cost in the FFS program, but also that resources may have shifted from relatively higher-cost enrollees (who are more likely to remain in FFS) to relatively lower-cost enrollees (who are more likely to switch to MA). Moreover, consistent with our satisfaction results, the Kaiser Family Foundation (2010) documents that MA contracts provide many up-front benefits that healthy individuals enjoy (e.g., 57 percent offer free or discounted gym memberships), but worse cost-sharing than does FFS for serious medical conditions. As such, compared to the FFS population, benefits *among MA enrollees* are more skewed toward the healthy. Both of these distributional effects will tend to diminish the role of Medicare in providing social insurance to smooth the utility consequences of variation in health status.

Because of its sheer size, Medicare Advantage is of interest in its own right. Controlling Medicare costs is viewed as the key factor in improving the long-term fiscal position of the

[2]We do not model intensive coding because for the individuals in our sample we are able to calculate capitation payments that are independent of MA plans' coding of disease conditions, as we explain in detail in Section 6. As such, our results tend to underestimate payments to MA plans after 2003.

US. We estimate that differential payments to MA plans in 2009 totaled about \$40 billion annually, a not insignificant share of the roughly \$500 billion in total Medicare spending that year. Put differently, differential payments amount to over one-third the annual cost of the coverage expansion provisions in the ACA, which extend health insurance to 30 million uninsured Americans.

The remainder of the paper is organized as follows. Section 2 provides background information on the MA program and the risk-adjustment formula. Section 3 presents the model. Section 4 describes the data. Sections 5, 6 and 7 present the empirical results on selection, differential payments, and satisfaction, respectively. Section 8 discusses the challenges to improving risk-adjustment and Section 9 offers concluding remarks.

2 Background on Medicare Advantage capitation payments and risk-adjustment

Since establishing MA plans in the 1980s, CMS has generally focused on adjusting payments along two dimensions of cost: individual attributes and geography. Individual attributes are used to generate an individual-level *risk score*. This risk score is then multiplied by a county-level *benchmark*, and capitation payments for individual i in year t in county c are thus equal to $capitation\,payment_{itc} = Risk\,score_{it} \times Benchmark_{ct}$. Below we describe how the methodology for calculating risk scores has changed, as well as how county-level benchmarks have evolved.

2.1 Risk-adjustment before 2004

Throughout the 1980s and 1990s, county benchmarks were generally set to 95 percent of county FFS costs (generally calculated based on a moving average over the past eight years), as it was believed that MA plans should be able to deliver services more efficiently than FFS (Medicare Payment Advisory Commission, 2009). However, by the late 1990s, the strict link between benchmarks and FFS costs began to fray, as benchmarks were raised beyond FFS costs in areas of low MA penetration in an effort to expand access to MA plans. By the end of 2003, benchmarks were roughly 103 percent of average FFS spending (Medicare Payment Advisory Commission, 2004).

During the 1980s and 1990s, CMS used a "demographic model" to generate individual-level risk scores, so-called because it included only demographic variables (gender, age, and disability and Medicaid status) as opposed to disease or health conditions. As mentioned in the Introduction, MA plans do not report cost or claims data to CMS, so CMS used the FFS population to determine how each of these demographic factors contributes to average costs. CMS found that the model explained only one percent of the variation in medical costs among the FFS population (Pope *et al.*, 2004). Given the difficulties in out-of-sample prediction, it is unlikely that the model explained any more of the variation among the MA population.

Because of the low predictive power of the demographic model, there was substantial scope for MA plans to target enrollees who were healthier—and thus cheaper—than the demographic model would predict. Indeed, previous research has shown that during this period MA plans were able to attract patients who were far less costly than the FFS population in general or than predicted by the demographic model. Estimates suggest that individuals switching from traditional FFS to MA had medical costs between 20 and 37 percent lower than individuals who remained in FFS.[3]

Federal policymakers reacted to this evidence by enhancing the risk-adjustment procedure. In 2000, CMS introduced the principal inpatient diagnostic cost group (PIP-DCG) model. Due to the lack of MA cost data, the model used inpatient diagnoses (thus excluding outpatient and physician diagnoses) documented on FFS claims data to predict FFS costs the following year. As MA plans do not submit claims data, applying this model to the MA population required that MA plans submit "encounter data," which documents an enrollee's diagnoses. CMS found that the PIP-DCG model could explain 6.2 percent of the variation in FFS costs.

Between 2000 and 2003, risk scores were calculated as a 90/10 blend of the demographic and PIP-DCG models: $Risk\,score = 0.9 * Demographic\,score + 0.1 * PIP\text{-}DCG\ score$. Thus, the introduction of the PIP-DCG model raised the portion of MA cost variation explained by risk scores from one to $0.9 * 1 + 0.1 * 6.2 = 1.5$ percent.

2.2 Risk adjustment after 2003

The difference between county benchmarks and FFS costs continued to grow after 2003. By 2009, benchmarks reached 118 percent of county FFS costs (Medicare Payment Advisory Commission, 2009).

In 2004, CMS introduced a more comprehensive risk-adjustment regime that is based on the hierarchical condition categories (HCC) model. Like the PIP-DCG model, the HCC model uses claims data from the FFS population to calibrate a model that predicts FFS costs in the following year, though the HCC model accounts not just for inpatient claims, but physician and outpatient claims as well. The model distills the roughly 15,000 possible ICD-9 codes providers list on claims into seventy disease categories.[4] Initally, the HCC model

[3]See, for example, Langwell and Hadley, Commission (1997), Mello *et al.* (2003) and Batata (2004).

[4]ICD-9 is an abbreviation for International Statistical Classification of Diseases and Related Health Problems Version IX. CMS provides the file mapping ICD-9 conditions to HCC categories at `http://www.cms.`

was blended with the demographic model, with the HCC model accounting for 30, 50 and 75 percent of the total risk score in 2004, 2005, and 2006, respectively. From 2007 onward, risk scores were based entirely on the HCC model.

CMS found that when FFS data is used to calculate HCC scores, the HCC score explains eleven percent of FFS costs the following year (Pope *et al.*, 2004). Newhouse *et al.* (1997) and Van de ven and Ellis (2000) survey the literature and conclude that the lower bound on the percent of cost variation plans are able to predict is between 20 and 25 percent, suggesting there is still potential room for risk-selection even if the model were to perform as well on the MA population as it does on the FFS population. Moreover, both prospective reports commissioned by CMS in 2000 (Pope *et al.*, 2000) and more recent work using data from 2004 to 2006 (Frogner *et al.*, 2011) have found that the formula systematically under-predicts costs for those with above-average costs, and over-predicts costs for those with below-average costs.

Evaluation and application of the PIP-DCG and HCC models is complicated by the lack of cost or claims data from MA plans. Whether the model performs as well on the FFS population as it does on the MA population depends on at least two key assumptions: first, that that the coding practices MA plans use in generating encounter data to record the conditions used in the formula are the same as those used by FFS providers on claims data; second, that differences in the MA and FFS populations can be fully accounted for by these conditions.

CMS has done extensive research related to the first assumption. They have found that MA plans exhibit greater "coding intensity" in documenting disease conditions, so that an MA enrollee's risk score grows substantially faster than an FFS enrollee's risk score (Center for Medicare and Medicaid Services, 2008).[5] As risk scores are based on disease conditions the previous year, the year after an enrollee switches from FFS to MA, his risk

`gov/MedicareAdvtgSpecRateStats/Downloads/RAdiagnoses.zip`. The model coefficients and algorithms can be found at `http://www.cms.gov/MedicareAdvtgSpecRateStats/Downloads/HCCsoftware07.zip`. County benchmarks are published annually in the Medicare Advantage "ratebooks" and ratebooks from 1990 to 2011 are all available at: `http://www.cms.gov/MedicareAdvtgSpecRateStats/RSD/list.asp`.

[5]The CMS study focuses the difference in growth rates between those who stay in FFS at least two years in a row to those who stay in MA at least two years in a row, to eliminate the effect of compositional changes. One reason that MA risk scores might grow faster is that the health of MA "stayers" is deteriorating faster than their FFS counterparts. However, CMS explicitly dismisses this possibility: "As noted above, it is possible that beneficiaries enrolled in MA plans may be getting sicker faster than beneficiaries in FFS and this could be driving faster risk score growth for MA enrollees. Given the care coordination and disease management activities of MA plans, however, we do not find it reasonable to assume that MA stayers underlying health status is getting worse at a faster rate than stayers in FFS. CMS analysis has found that MA mortality rates during the study period do not explain rising risk scores." Indeed, all the evidence that we present in later sections point to MA enrollees being more healthy both before and after the HCC model was implemented in 2004.

score is based on FFS provider claims data and is thus free of intensive coding. However, any year after that, risk scores are based on MA plans' coding practices, which are far more aggressive than those assumed when the model was calibrated.

The HCC model will also offer a upwardly biased estimate of the counterfactual FFS costs of MA enrollees if these enrollees are positively selected along dimensions not included in the model. In fact, the introduction of risk-adjustment will incentivize firms to selectively target individuals who they expect to have low costs *conditional on their risk score*. The next section presents a formal model of how firms' screening incentives change upon the introduction of risk adjustment.

3 Model

In this section, we model the incentives that private insurers have to screen customers based on underlying cost and how these incentives change under risk-adjustment. To focus on predicting how firms will react to the introduction of risk adjustment, we take as given the basic contours of the risk-adjustment formula used by CMS as described in the previous section.[6]

3.1 Basic framework and assumptions

3.1.1 Cost of health insurance coverage

Let the expected cost of covering individual i in a given year be given by $m_i = b_i + v_i$, where b_i is a continuous composite measure of costs based on variables included in the risk-adjustment formula, and v_i is a similar composite measure composed of variables not included in the formula. As MA contracts have a year-long duration, the model is single-period, and we thus specify costs over a single year.[7] Both v and b are in units of absolute dollars.[8] While $\mathbb{E}(v|\, b) = 0$ for all b, the variance of v can vary with b, consistent with past work showing substantial heteroskedasticity in medical costs. We discuss the variance of v in greater detail when we lay out our assumptions regarding screening costs.

[6]There is a large literature on risk-adjustment, and Van de ven and Ellis (2000) and Ellis (2008) serve as excellent reviews. Recently, work has focused on "optimal" risk adjustment, following Glazer and McGuire (2000) who argue that mere predictive models (such as the HCC model) are fundamentally misguided because formula coefficients need to be chosen for their incentive, not predictive, properties. However, as noted by Ellis (2008), predictive models are by far the most common risk-adjustment models in use today and thus determining how firms react to them is an important empirical question.

[7]We return to the question of dynamics in Section 8 when we discuss recalibrating the risk-adjustment model over time.

[8]Note that m is the cost to the *insurer*—the cost of total medical care plus administrative costs, less the out-of-pocket costs paid by the individual—not total actual medical costs. As in Glazer and McGuire (2000), we do not model out-of-pocket costs in order to focus on selection, though return to the issue in Section 7.

In our baseline model, we assume that costs m are the same whether an individual is in FFS or MA. Of course, MA plans may be more or less efficient than FFS, and all of the results that follow hold when MA costs are proportional to FFS costs. However, we focus on the case where costs are identical: not only is the analysis slightly more simple, but it also allows us to more easily focus on the difference between payments to private firms for insuring person i and the counterfactual cost if the government directly covered her, which is a key parameter for evaluating the fiscal impact of private Medicare Advantage plans.[9]

3.1.2 Capitation payments and risk adjustment

Without risk-adjustment, firms receive a fixed payment $\bar{p}$ for each individual they enroll. We model risk-adjustment as replacing $\bar{p}$ with a function $p(b)$, $p' > 0$, which links the capitation payment to the value of b. For analytic convenience, we set $p'' = 0$.

Given that $\mathbb{E}(v|\, b) = 0$, total expected costs m are given by $\mathbb{E}\left(b + v|\, b\right) = b$ and thus the government would like to ideally set $p(b) = b$. However, based on the evidence presented in Section 2, the government is not able to perfectly estimate b. In particular, individuals with higher (lower) risk scores are under- (over-) compensated. As such, we assume that while the government's estimate of costs grows with b it grows less than one-for-one, so $p'(b) \in (0, 1)$. Note that the main results on how selection responds to risk-adjustment and the resulting effect on differential payments require only that $p' > 0$; assuming $p' < 1$ generates additional empirical predictions.[10]

We also make risk-adjustment be "payment-neutral," that is, $\mathbb{E}(p(b)) = \bar{p}$ for the Medicare population as a whole. In other words, if the entire population joined a private plan, the government would pay the same average capitation payment with or without risk adjustment.[11]

Finally, we want to allow for the degree of risk-adjustment to vary, which again mirrors the actual experience of the phasing-in of risk adjustment between 2004 and 2007. We define

[9]Whether the HMO model is actually more efficient than the fee-for-service model even absent selection effects is an open question. Duggan (2004) finds that when some California counties mandated their Medicaid recipients to switch from the traditional FFS system to an HMO, costs increased by 17 percent relative to counties that retained FFS. As, within a county, individuals did not select between FFS or an HMO, selection issues are unlikely to be driving the result.

[10]Specifically, Proposition 1—that selection along b falls and selection along v rises as a result of risk-adjustment—and Proposition 4—that the effect of risk-adjustment on differential payments is ambiguous—do not depend on $p' < 1$. We provide a simple microfoundation for why p' will be typically less than one in the Appendix. In particular, we assume that the government observes a mismeasured version of b, which results in the standard errors-in-variables attenuation bias.

[11]As we discuss in Section 2, firms were actually given temporary payments to ease the transition into risk-adjustment, but as a matter of theory, we are more interested in the steady-state results when the system returns to payment-neutral conditions. Section 6 reports our empirical results with and without these temporary payments.

capitation payments as $(1-\Omega)\bar{p} + \Omega p(b_i)$, where $\Omega \in [0,1]$ is the risk-adjusted share of the capitation payment.

As indicated in the introduction, the key objective of risk adjustment was to reduce the difference between a plan's capitation payment for covering an individual and the cost to the government had it directly covered him via FFS. Having defined how risk-adjustment affects capitation payments, we can make this concept slightly more precise.

Definition. *The "differential payment" for individual i equals*

$$\underbrace{(1-\Omega)\bar{p} + \Omega p(b_i)}_{\textit{capitation payment}} - \underbrace{(b_i + v_i)}_{\textit{FFS cost}}$$

3.1.3 Screening costs

Though we discuss profit-maximization in greater detail shortly, firm profits are obviously a function of $m = b + v$ and thus they will have preferences over the average b and v values of the population they enroll. However, enrolling only specific groups from the entire Medicare population entails screening costs. Costs may include targeted advertising and recruiting, writing contracts that specifically appeal to one profitable group but whose terms make it unattractive to another *ex ante* profitable group, or even the risk of government sanctions for violating open-enrollment requirements.

We assume that the per capita screening costs c a firm incurs depends on its enrollees' mean values of b and v. Since randomly enrolling individuals from the general population should require minimum screening costs, $c(\bar{b}, \bar{v})$ is a global minimum, where $\bar{b}$ and $\bar{v}$ are population averages. Enrolling individuals further from the mean is costly, so $c_x < 0$ for $x < \bar{x}$ and $c_x > 0$ for $x > \bar{x}$ for $x \in \{b, v\}$. We also make these costs everywhere convex.

Finally, we assume that $c_{bv} > 0$. This assumption implies that for higher values of b, the incremental cost of reducing v falls. This assumption rules out the possibility that screening in b and v are complements. Because the variance of medical costs is typically a positive function of expected costs (see, e.g., Lumley *et al.* 2002) and v is measured in absolute dollars, it should be easier to find, say, a cancer patient with costs \$100 below expectation than someone without a single documented disease condition with costs \$100 below expectation.

With screening costs thus defined, we can now specify a firm's profit function. In our baseline model, we make the simplifying assumption that firms cannot affect the number of individuals that they enroll, though we return to this assumption later in the section. Firms instead focus on maximizing the average profit per enrollee, which is a function of b and v. Thus, firms maximize the following expression:

$$\mathbb{E}(\pi) = (1-\Omega)\bar{p} + \Omega p(b) - b - v - c(b, v). \tag{1}$$

We now use this framework to prove a number of results regarding selection and differential payments.

3.2 Results

We begin with our central result, which characterizes how firms will react to a change in risk adjustment. The proof of this and all other results are in the Appendix.

Proposition 1. *The following two conditions hold when the risk-adjusted share Ω of the capitation payment increases:*

(i) Firms decrease screening efforts along the b margin and thus the average value of b among their enrollees rises.

(ii) Firms increase screening efforts along the v margin and thus the average value of v among their enrollees falls.

As risk-adjustment makes capitation payments a positive function of b, firms will spend less effort finding low-b enrollees and instead focus their efforts on finding low-v enrollees.[12] We show as a corollary to Proposition 1 that the effect of increasing risk adjustment on selection with respect to total costs $m = b + v$ is ambiguous—that is, it is possible that the increased screening along the v-margin can offset the decreased screening along the b-margin.

The next result demonstrates that increasing risk adjustment does not lead firms to indiscriminately enroll individuals with high b values.

Proposition 2. *For any $\Omega \in [0, 1]$ and any $p(.)$ such that $p' < 1$, firms will enroll only individuals with $b < \bar{b}$.*

This result is the first to depend on $p' < 1$. As we show in the Appendix, if $p' < 1$, then with or without risk-adjustment, individuals with $b > \bar{b}$ have capitation payments below their expected costs, and firms have the incentive to engage in screening to avoid such *ex ante* unprofitable enrollees.[13]

[12]The empirical work will focus on the government's observed risk score—that is, $p(b)$ in the parlance of the model—as b itself is not observable. But as $p'(b) > 0$, Proposition (1) (i) implies that $p(b)$ will increase as well, thus giving us the testable prediction that risk scores *as measured by the government* increase with an increase in risk-adjustment.

[13]Similar to the discussion in footnote 12, this prediction technically applies to b, whereas we can only observe $p(b)$. In a corollary to the proof, however, we show that $b < \bar{b} \Leftrightarrow p(b) < \mathbb{E}(p(b))$.

We show as a corollary to Proposition 2 that firms positively select with respect to overall costs $m_i = b_i + v_i$ both before and after an increase in risk adjustment. Of course, as shown in Proposition 1, the relative intensity of b versus v screening depends on the degree of risk-adjustment, but overall positive selection holds for any $\Omega \in [0, 1]$.

We now turn to examining how differential payments change with risk-adjustment. We begin by showing how increasing risk-adjustment affects a firm's differential payments if selection is held fixed—that is, the firm's b and v (the average b_i and v_i among the firm's enrollees) do not change in response to a change in risk-adjustment.

Proposition 3. *For $\Omega_0 < \Omega_1$, moving from Ω_0 to Ω_1 will always decrease differential payments if b and v are held fixed at their equilibrium values under Ω_0.*

This proposition shows that risk adjustment reduces differential payments if selection remains unchanged. Therefore, our assumptions regarding the risk-adjustment model $p(.)$ do not render it completely ineffective.

While the results and corollaries so far allow us to derive empirical tests of the model, they do not have direct public-policy relevance. After all, the key test of risk-adjustment from the government's perspective is not how it affects selection patterns (Propositions 1 and 2), or how it performs in the artificial scenario when firms cannot react (Proposition 3). Rather, it is whether risk-adjustment reduces the magnitude of differential payments when firms can optimally change their screening patterns, a question our final result examines.

Proposition 4. *The effect of increasing Ω on a firm's average differential payment is ambiguous.*

On the one hand, risk-adjustment reduces differential payments for enrollees with the lowest b values—who, by Proposition 2, we know disproportionately join MA plans. On the other hand, plans react to an increase in risk-adjustment by moving up the b distribution and down the v distribution, and the proof shows that these two effects can actually more than fully offset the decreased payments for low-b enrollees.

Note that this result holds despite our having ruled out ineffectual risk-adjustment models (Proposition 3). Recall that the capitation payment $p(b)$ is always a positive function of b, the health risk as observed by the government. We further require payment-neutrality, so the government is not merely handing extra money to private plans via risk-adjustment. But even under these conditions, the endogenous response of firms can thwart the goal of lowering differential payments.

3.3 Discussion

Some assumptions or omissions of the model deserve further scrutiny. First, we do not model firm competition. Competition might lead firms to have to out-bid each other over profitable enrollees. One might thus model screening costs $c(b, v)$ as including this cost and thus as endogenous to the risk-adjustment regime.[14]

Second, we have assumed firms can only affect the *types*, but not *number*, of people they enroll. As we show in the Appendix, endogenizing this margin does not change the results so long as the number of enrollees per firm is large relative to (*i*) a firm's ability to enlarge its enrollment by changing b or v; or (*ii*) the level of per capita profits. At least in relatively competitive settings—in which firms do not expand beyond some efficient size as instead excess demand is met by new firm entry and in which profits are minimal—these assumptions appear plausible. Moreover, empirically, the MA share of the Medicare population did not immediately change upon introduction of risk-adjustment, supporting our baseline assumption.

Moreover, this assumption allows us to focus on average cost and differential payments per beneficiary, which we can observe or construct using our individual- and county-level data on Medicare recipients, as opposed to firm's total profits, which we cannot. We now turn to describing our main data sources.

4 Data

Our empirical work relies heavily on individual-level data from the Medicare Current Beneficiary Survey (MCBS) Cost and Use series from 1994 to 2006 (the most current year available). The MCBS links CMS administrative data to surveys from a nationally representative sample of roughly 11,000 Medicare enrollees each year. It also provides complete claims data from hospital admissions, physician visits, and all other provider contact for all FFS enrollees in the sample, totaling about half a million claim-level observations each year.

A mix of cross-sectional and panel data, each year the MCBS follows a subsample of respondents for up to four years. During our sample period, the data comprise over 55,000 unique individuals and over 140,000 person-year observations. From this sample, we make only minimal sampling restrictions. First, we do not include the less than 0.25 percent of enrollees whose Medicare eligibility is based entirely on having end-stage-renal disease, as

[14] How market power affects risk-selection in the presence of adverse selection has only recently received much attention. As Olivella and Vera-Hernandez (2007) write, "most of the literature on adverse selection considers extreme cases: either perfect competition or monopoly." Recent attempts to model the effects of market power generally focus on settings with two firms and two types of consumers (e.g., Olivella and Vera-Hernandez 2007, Biglaiser and Ma 2003 and Jack 2006).

different risk-adjustment rules applied to them. Furthermore, we exclude the roughly two percent of individuals who only become eligible for Medicare partway through the year, as their survey data is often incomplete.

Table 1 reports summary statistics, separately for MA and FFS enrollees. We follow CMS and classify an individual as an MA enrollee in a given year if he spends the majority of his Medicare-eligible months in an MA plan, though we show our empirical results are robust to other specifications.[15] FFS enrollees are more likely to be disabled, and, conditional on not being disabled, are roughly a year older. While there are no significant differences with respect to gender and race, MA plans attract a disproportionate share of Hispanics. The share of MA respondents with annual income above $20,000 (roughly the median of the sample) is about three percentage points higher than that of FFS respondents.[16]

The last variable in Table 1 is *Total Medicare cost*, the total cost to Medicare for individual i in year t, whether it is covering her directly via FFS or paying an MA plan to cover her. We calculate this variable by summing the reported capitation payment each month an individual is in MA and any Part A or B payments the months she is not. Obviously, for those classified as being in MA, *Total Medicare cost* is determined entirely or mostly by capitation payments, and for those in FFS it is determined entirely or mostly by provider payments.

An important detail to discuss is that an individual's reported capitation payment in the MCBS is the *average capitation payment to an individual's MA plan*, not the capitation payment for the individual herself. As MCBS respondents are sampled so as to be representative of the Medicare population, the average *Total Medicare cost* for MA enrollees in Table 1 should equal in expectation *Total Medicare cost* for the average MA enrollee. As the Table reports, Medicare pays on average $800 more to cover the average MA enrollee than the average FFS enrollee.[17]

While the reporting of plan averages instead of individual payments still allows us to look at how average *Total Medicare cost* differs between MA and FFS, our empirical objective is to estimate how an individual's cost changes depending on whether she is in MA or FFS. Comparing, say, *Total Medicare cost* the last year an individual is in FFS to the first year she is in MA would be a natural way to begin investigating how MA status affects costs; unfortunately, given how MCBS actually records capitation payments, we would actually be comparing an individual's last year in FFS with the average costs of all individuals in her

[15] See Center for Medicare and Medicaid Services (2009). While enrollees can switch mid-year, well over 90 percent of individuals we classify as being enrolled in MA in a given year spend all twelve months in MA.

[16] We do not report the average because the survey topcodes above $50,000.

[17] Unless otherwise stated, all dollars amounts reported in the paper are adjusted to 2007 dollars using the CPI-U.

MA plan the following year.

However, we are greatly aided by the fact that HCC scores are based on diagnoses documented on medical claims in the *prior* year, and that each year an individual is in FFS, the MCBS collects all her claims data. As such, we can calculate her HCC score, and thus her capitation payment, her first year in MA.[18] In analyses where we do these imputations, we generally limit ourselves to those individuals who were in FFS all twelve months of a baseline year, so that we can be sure we have their complete claims history that year. Claims data are not available once an individual is in MA, so we cannot do this imputation in subsequent years. As we discuss later, given the evidence described in Section 2 on "intensive coding" by MA plans, limiting our analysis to the change in Medicare costs the first year an individual switches to MA likely understates any increase in differential payments.

Because we can only calculate capitation payments the first year an individual switches to MA, much of the empirical focuses on comparing *Total Medicare cost* the last year an individual is in FFS to *Total Medicare cost* the first year an individual is in MA. Appendix Table 1 shows the number of observations who are in FFS in year t and MA year $t+1$ as well as the number who are in FFS both years (these individuals often serve as a control group), and how these numbers change across our sample period. We have over 1,500 individuals who switch from FFS to MA, and over 70,000 who remain in FFS over the course of two years.

5 Changes in selection after risk adjustment

In this section we test the predictions from the model in Section 3 regarding selection into MA plans after risk-adjustment. The model offers several testable predictions. First, after risk adjustment, extensive margin selection should fall, thus leading to an increase in risk scores, at least for values of risk scores that are profitable in expectation. Second, intensive margin selection should increase; conditional on risk scores, total spending for those switching to MA relative to those staying in FFS should fall after risk-adjustment. Thus, third, MA recipients are positively selected with respect to cost both before and after risk-adjustment.

[18] To calculate the capitation payment, we follow the HCC risk-adjustment formula published on the CMS website to calculate the risk score and multiply it by the benchmark for the individual's county, also published on the website (see footnote 4 for the actual links). CMS provided us the actual risk scores of a sample of MCBS enrollees so we could check how accurately we imputed the risk scores: the correlation between actual and imputed risk scores is above 0.96.

5.1 Quantifying the selection incentives created by the HCC model

As discussed earlier in Section 4, we focus our analysis on those in FFS for all twelve months of the baseline year, which includes many who will switch to MA the following year. For each individual, we calculate two counterfactual capitation payments were they to indeed switch to MA the following year: the first based on the demographic formula and the second based on the HCC formula.

The first column of Table 2 compares these two capitation payments. The table includes only those in the pre-risk-adjustment period, before any selection endogenous to the risk scores would be incentivized. Col. (1) presents the average difference between the HCC-based capitation payment and the demographic-based capitation payment, grouped by quintile of HCC score. Of course, mechanically capitation payments must on average rise under the HCC formula for those with higher risk scores, and col. (1) merely presents the magnitudes. For example, the HCC capitation payment would, on average, pay \$3,267 less than the demographic-based capitation payment for the lowest quintile of risk score, but it would pay \$7,419 more for the highest quintile of risk score.

Col. (1) would make it seem as though plans would be incentivized to increase risk scores over the entire risk score distribution, but col. (2) shows that doing so would not always be profitable. Past work has documented that the HCC formula appears to systematically underestimate capitation payments for the seriously ill (Pope *et al.*, 2004), and indeed we find this pattern in our MCBS sample. For example, individuals in the highest quintile of risk scores represent on average an expected \$4,261 loss to an MA plan. More specifically, in our MCBS sample, those above a risk score of 2.0 (the 85th percentile of the distribution) would appear to be ex ante unprofitable. Increasing risk scores indiscriminately would lead plans to enroll many beneficiaries who are ex ante very unprofitable.

5.2 Evidence from individual-level data

5.2.1 Empirical strategy

To test whether firms react to the extensive margin incentives depicted in Table 2, we estimate the following specification on the sample of individuals who are in FFS all twelve months of a given year t:

$$Risk\,score_{it} = \beta MA_{i,t+1} \times After\,2002_t + \gamma MA_{i,t_1} + \delta_t + \epsilon_{it}, \tag{2}$$

where i indexes the individual, t the year, $MA_{i,t+1}$ indicates that the individual will switch to MA in the following year, $After\,2002$ indicates the baseline year is after 2002, and δ_t is a vector of year fixed effects. Note that while risk-adjustment begins in 2004, those in FFS in the baseline year of 2003 would be switching into MA the first year under the HCC model, and are thus part of the "post-period."

Based on Table 2 and the results from the model, plans should want to increase risk scores in regions only where it is profitable, and thus we estimate equation (2) on two different samples: those with risk-scores below 2.0 and those with risk-scores above 2.0. We predict that the coefficient on the interaction term should be positive for the first sample, and close to zero for the second sample.

To test the intensive margin prediction, we estimate the following equation:

$$Cost_{it} = \beta MA_{it} \times After\,2002_t + \gamma MA_{it} + \lambda Score_{it} + \delta_t + \epsilon_{it}, \tag{3}$$

where $Cost_{it}$ is the total FFS cost for individual i in year t, $Score$ is the HCC score of the individual, and all other notation follows that in equation (2). We predict a negative coefficient on the interaction term—conditional on the risk score, MA enrollees should be relatively cheaper after risk-adjustment.[19]

5.2.2 Results

The first three columns of Table 3 report results from estimating (2). The first two columns report results when the sample is restricted to beneficiaries who in expectation are profitable and unprofitable, respectively. As predicted, the risk scores of those joining MA increase after risk-adjustment, conditional on the risk scores being in the profitable region. The difference between the risk scores of those joining MA versus those staying in FFS increases by .057 points after risk-adjustment, or about seven percent. For the rest of the sample, the difference in risk scores between those joining MA is essentially unchanged after risk-adjustment—and increase of 0.6 percent with a p-value above 0.9. Col. (3) shows that the positive extensive margin result holds when instead of the MA dummy we use the share of months on MA.

The next two columns investigate intensive selection, and test the prediction that costs conditional on risk scores for those switching into MA relative to those staying in FFS should fall after risk-adjustment. Col. (4) estimates equation (3): the interaction term is negative and significant, indicating that, conditional on risk score, MA enrollees are more positively selected with respect to costs after risk-adjustment. Col. (5) shows the same result with the share-of-months variables.

[19]This specification is similar in spirit to the "unused observables" test of Finkelstein and Poterba (2006). Using the terminology of their framework, $Cost_{it}$ in equation (3) is an "ununsed observable" because it is positively related to future costs to the insurer but, conditional on a beneficiary's risk score, is not used to determine insurance premiums or capitation payments.

The fact that the main effect of MA in cols. (4) and (5) is close to zero suggests that differences in risk scores accounted for essentially all of the cost differences between those joining MA and those switching to FFS in the pre-period. As risk scores were designed to address exactly these differences and were obviously based on data from the pre-period, the result is not surprising. However, once risk score adjustment is instituted, MA plans have an incentive to enroll individuals who have low costs conditional on their risk scores, and the results in cols. (4) and (5) suggest that enrollment patterns follow exactly that pattern.

A third prediction is that MA enrollees should be positively selected with respect to cost both before and after risk-adjustment. Col. (6) shows this prediction holds by re-estimating (3) without including the risk-score control. The model offered no strict prediction on the sign of the interaction term, and while the point-estimate is negative it is nowhere near conventional levels of significance.

5.3 Evidence on selection from county-level data

One limitation of the results in Table 3 is that our data only cover the first three years of risk adjustment. We therefore augment this evidence with county-level data from 2000 to 2008.

5.3.1 Data and Empirical strategy

CMS publishes annual county-level data on per capita FFS spending on all beneficiaries 65 and older.[20] Average county-level FFS expenditures for the elderly during our nine-year study period are approximately $6,680 and the average change from one year to the next is $233. We merge these data to annual county-level data on the fraction of Medicare recipients enrolled in MA plans, and summarize our sources for both sets of data in the Data Appendix. Average MA enrollment during the same period is approximately 15.9 percent, with this declining from 17.4 percent to 12.6 percent from 2000 to 2004, and then increasing to 22.0 percent by 2008.

The positive selection with respect to overall costs we saw throughout the period—documented in col. (6) of Table 3—would suggest that when a county's Medicare population shifts from FFS to MA its average per capita FFS costs should increase, as more low-cost enrollees leave the FFS population, driving up the average cost of those remaining. Batata (2004) formalizes this intuition, using ideas developed by Berndt (1991) and Gruber *et al.* (1999) to estimate the difference between the average per capita cost among those switching

[20]See Data Appendix for sources. We follow Batata (2004) in focusing on aged beneficiaries, who account for 85 percent of all Medicare recipients.

between MA and FFS and the average per capita cost of those remaining in FFS. She demonstrates that, if one assumes the distribution of spending across counties differs only with respect to the mean, then the estimate for β_1 in the following specification yields the difference between the "switchers" and average per capita FFS costs, with j and t indexing county and year respectively:

$$\Delta Per\text{-}capita\, Cost_{jt}^{FFS} = \beta_0 + \beta_1 \Delta ln(FFS\, share\, of\, county_{jt}) + \epsilon + jt. \tag{4}$$

There are a number of challenges in applying this estimate to the predictions of the model, however, which is why we view these results more as secondary support for our earlier results and less as direct evidence on their own. First, because we only have county-year-level FFS means, we cannot investigate the specific extensive and intensive selection patterns documented in Table 3. We thus merely seek to establish, using a different data set and source of variation, that MA enrollees are positively selected with respect to overall costs and that this positive selection remains after risk-adjustment. As this result is on face surprising—the objective of the HCC formula was, after all, to eliminate the incentives for enrolling low-cost individuals—yet can be explained by our model, we want to provide readers further supporting evidence.

Second, and perhaps most obviously, equation (4) treats changes in MA penetration as exogenous, whereas in reality endogeneity stories are easy to imagine. Counties with high FFS cost growth may differentially attract MA plans, as insurers might believe that these high costs represent evidence of needless medical spending and thus opportunities for painless cost-cutting. Of course, the relative health of a county's Medicare recipients also determines cost, and, especially in the pre-risk-adjustment period, counties with high FFS costs due to having relatively sick enrollees would not attract MA plans.

We nonetheless find the county-level analysis useful in strengthening our early results for three reasons. First, the county-level results shed further light on what those switching to MA *would have* cost had they remained in FFS. Recall that the selection results in the final column of Table 3 essentially compare two individuals in FFS in year t: one who will switch to MA in year $t + 1$ and one who will remain in FFS. While we document that, relative to the "stayer," the year-t costs of the "switcher" is lower, we cannot with certainty assume this ordering holds for their $t + 1$ costs, as once an individual switches to MA we no longer know her actual costs. However, if MA "switchers" tend to have low costs the year before they switch but high costs the next year, then their switching to MA would actually remove a high-cost enrollee from the FFS population the following year, and counties with larger shares of their population switching from FFS to MA would then have lower FFS costs the following year. Thus, finding that β_1 in equation (4) is negative indicates that any differential regression-to-the-mean among MA switchers in $t+1$ is not large enough to outweigh the positive selection in year t we documented using individual-level data in the previous subsection.

Second, the individual-level analysis cannot pick up potential efficiency spillovers from MA to FFS, as documented in earlier work by Chernew *et al.* (2008) and the county-level results help us ascertain the importance of such an omission.[21] High rates of MA penetration in an area may lower FFS costs due to competition or because local providers, incentivized by MA plans to contain costs, begin to treat their FFS patients in a similar fashion. Such spillovers decrease the cost of FFS relative to MA, and thus comparisons may, unfairly, report higher relative MA costs when in fact the lower FFS costs are actually due to MA's "good influence." However, a negative estimate of β_1 in equation (4)—which includes both the effect of differential selection and any effect of efficiency spillovers due to local MA penetration—would indicate that the magnitude of any spillovers is relatively small.

Finally, although we do not have a perfectly satisfying answer to the endogeneity concerns—no instrument uncorrelated with FFS cost outside of its correlation with MA penetration—our identification benefits from the fact that MA penetration both rose and fell during our sample period. Certainly changes in penetration may be correlated with time-varying characteristics of a county that influence average FFS costs. However, to the extent that there are omitted factors influencing FFS expenditures differentially in counties with rapid MA growth, it is unlikely that these omitted factors would reverse direction when MA enrollment is declining.

5.3.2 Results

Table 4 report the results from estimating equation (4). The first column suggests that those switching from FFS to MA have Medicare costs roughly \$1,000 lower than those who remain in FFS, and when county-year trends are added in the second column the effect difference increases by about \$150. The second two columns suggest that this section pattern did not change after risk-adjustment. Whereas in col. (6) of Table 3 we saw a small, statistically insignificant increase in the positive selection of MA enrollees after 2003, the last two columns of Table 4 suggest a small statistically insignificant decrease in positive selection post risk-adjustment. But both tables point to substantial positive selection both before and after

[21] Nicholas (2009), however, finds that efficiency spillovers have not persisted in more recent years. Another possible reason for these differing results is that Chernew *et al.* (2008) drops the ten percent of Medicare recipients who are institutionalized. In our sample of the MCBS, these individuals are roughly three times as expensive as those in community settings and nearly three times as likely to be in FFS as opposed to MA, and as such their inclusion or exclusion could have large effects on cost comparisons between MA and FFS.

risk-adjustment, with no discernible effect of the policy change.

While the coefficients of interest in col. (6) of Table 3 are larger than the coefficients on $\Delta Log(FFSshare)$ in Table 4, as mentioned earlier, they are not directly comparable because the former relates to differences in spending the year before individuals switch to MA and the latter to the year after.[22] Moreover, recall that individuals switching from FFS to MA identify the earlier result, whereas those switching from FFS to MA *and* those switching from MA to FFS identify the county-level results. As we show in Section 7, MA enrollees in poor health are the most likely to switch to FFS, the opposite pattern we saw in col. (6) of Table 3, where those with lowest cost are the most likely to switch from FFS to MA. As such, variation in MA penetration due to switches from FFS to MA should lead to larger increases in FFS average costs than variation due to switches from MA to FFS, as the former group is more positively selected, leading to larger magnitudes in Table 3 than in Table 4.

5.4 Discussion

We take the results in Table 3 as confirming the predictions of the model in Section 3. First, firms have less incentive to engage in extensive-margin selection after risk-adjustment, and as a result risk scores rise for those switching into MA relative to those remaining in FFS. Moreover, this increase only occurs in regions of the risk-score distribution that support positive expected profits, providing further evidence that firms are indeed reacting to incentives. Second, conditional on the risk score, those switching to MA have lower baseline FFS spending relative to those staying in FFS after risk-adjustment, consistent with their being more intensely selected along dimensions excluded from the risk formula. In the MCBS data, these two effects cancel each other out, leaving the positive selection with respect to overall baseline costs unchanged after risk-adjustment. We find similar patterns—of positive selection along total costs, with no marked change after risk-adjustment—using county-level data.

Recall that our model shows that if firms increase their intensive margin selection efforts, differential payments can actually rise after risk-adjustment. While risk-adjustment will always decrease differential payments if selection patterns do not change, the impact on differential payments when selection patterns do change is an empirical question, which the next section explores.

[22] Of course, the differences in the sample period used Table 3 and Table 4 might explain the difference, but, in fact, the coefficients in col. (6) of Table 3 are essentially unchanged if we instead estimate it on data from 2000 to 2006, a period more closely aligned with the 2000-2008 county sample.

6 Did risk adjustment decrease differential payments to MA plans?

In this section, we focus on how an individual's *Total Medicare cost* in a given year changes as he switches from FFS to MA. Recall from Section 4 that *Total Medicare cost* is equal to total reimbursements to providers if he is in FFS, total capitation payments to MA plans if he is in MA, or the total of the two if he spent time in both programs. If risk-adjustment works perfectly—so that in expectation capitation payments are equal to an individual's FFS costs—then whether an enrollee switches between FFS and MA should have no effect on his total Medicare costs.

We make two adjustments to capitation payments after 2003 to isolate the effect of the introduction of risk-adjustment from other changes occurring around the same time. First, the growth rate of county benchmarks (the baseline value, which, multiplied by the risk score, yields capitation payments) rose after 2003, in some counties considerably so. We therefore calculate capitation payments holding the growth rate of each county's benchmark to its pre-2004 level. Second, in the years immediately following the introduction of risk-adjustment, plans received a so-called "budget-neutrality" adjustment (about a ten percent increase in capitation payments) to ease the transition to risk-adjustment, and we reduce payments to remove this effect. In both cases, these adjustments increased all capitation payments by a given percent and did not depend on underlying individual conditions or characteristics. The adjustments we make obviously decrease the likelihood we would observe an increase in differential payments after risk-adjustment.[23] Before examining whether differential payments fell after the introduction of risk-adjustment in 2004, we explore whether they would have decreased had selection into MA plans held to its pre-2004 patterns, a key prediction from Section 3.

6.1 Would risk-adjustment have worked had selection patterns not changed?

Figure 1 overlays the distribution of the change in total Medicare costs for those switching from FFS to MA between 1994 and 2003 and the distribution of that change from the same population *had the HCC model been in effect.* On average during the pre-period, actual Medicare costs increase by $2,991 for the MA joiners but by only $1,162 for the FFS stayers. The unconditional differential increase in Medicare costs is therefore $1,828. When we instead simulate the capitation payments under the HCC model for those switching to MA, differential payments shrink by over $800. This $800 decrease remains when we condition for the large set of control variables listed in the notes to Table 5.

[23] Also note that our model assumed that the average capitation payment were the entire Medicare population to join MA would be the same with or without risk-adjustment, so removing these extra payments to plans brings the empirical work in line with the theory.

In short, had selection patterns not changed, we predict that the introduction of the HCC formula would have substantially reduced differential payments. Of course, given that the formula was calibrated on this population, this is a relatively undemanding test of the risk adjustment model. Moreover, as Section 5 demonstrates, selection patterns changed substantially after risk-adjustment—first, MA enrollees' risk scores increased once capitation payments became a function of risk scores, and, second, conditional on risk scores, pre-period FFS costs for this group fell. As we showed in Section 3, whether the change in selection patterns can completely "un-do" the risk-adjustment model is an empirical question, to which we now turn.

6.2 Did risk-adjustment reduce differential payments after 2003?

6.2.1 Empirical strategy

As above, we begin with a sample of beneficiaries in FFS all twelve months of a given year $t-1$. To estimate the counterfactual Medicare cost for an MA joiner in year t had he remained in FFS, we examine the actual Medicare costs in year t for FFS stayers who are similar along observable dimensions. The estimating equation is:

$$Cost_{it} = \beta MA_{it} \times After\,2003_t + \gamma MA_i t + \lambda X_i t + \delta_t + f(Cost_{i,t-1}) + \epsilon_{it}, \tag{5}$$

where $Cost_{it}$ is total Medicare costs for person i in year t, $f(Cost_{i,t-1})$ is a flexible function of lagged Medicare costs, and all other notation follows that in previous equations. We prefer this specification to simply regressing $\Delta Cost_{it}$ as the lagged cost controls in equation (5) can better account for the fact that medical costs typical exhibit strong regression to the mean, though we show that results using changes look very similar.[24]

The coefficients β and γ estimate the change in total Medicare cost associated with an individual switching to MA relative to his having stayed in FFS. These estimated effects are consistent only if MA is uncorrelated with ϵ. This condition implies that, conditional on our control variables, the decision to join MA is not systematically related to time-varying shocks to an individual's expected cost to the Medicare program. If, for example, individuals join MA when their health is improving and thus their expected costs are falling, β will be biased toward zero. In fact, when we return to endogeneity concerns later in this section, we argue that they generally bias results against finding differential payments to MA plans.

[24] The lagged Medicare cost controls include: lagged Medicare costs and twenty quantiles of non-zero Part A costs and non-zero Part B costs (we found that regression to the mean differed depending on the type of costs). Note that regressing the *change* in spending is thus nested in the equation (5)—the two are equivalent if the coefficient on lagged spending is constrained to equal one and the coefficients on all other lagged spending variables are constrained to equal zero.

6.2.2 Results

The first column of Table 5 shows the results from merely regressing the change in Medicare spending on the MA indicator—which is allowed to have a different effect before and after risk-adjustment—and year fixed effects. Total Medicare costs increase by roughly $1,890 when an individual switches from FFS to MA before risk-adjustment, and by an additional $1,350 after risk-adjustment.

The second column regresses the level of spending on the flexible function of past spending. The MA main effect is lower than in the first column, suggesting that some of the differential payments in the pre-period may in fact have been differential regression to the mean among those switching to MA. However, the coefficient on the interaction term barely changes, and in fact grows slightly in magnitude. The third column adds controls for self-reported health in the previous year, as well as a large set of demographic and other controls, listed in the Table notes. These controls are important if, for example, older people tend to have higher spending growth and post risk-adjustment they are also more likely to join MA plans. In this case, we want to account for the fact that these older beneficiaries would have likely experienced high cost growth had they remained in FFS. That the coefficient on the interaction term increases by roughly a third suggests, as we hypothesized earlier, that selection endogeneity works against finding MA differential payments, at least in the post-period.

Col. (4) includes measures of current-year self-reported health. We prefer the previous specification, as health may indeed be endogenous to the care individuals receive in MA versus FFS. However, in practice, these controls have little apparent effect. Col. (5) is equivalent to col. (3), but replaces the MA dummy variables with the fraction of total Medicare-eligible months an individual spent that year in MA. We do not have a strong opinion on whether the dummy-variable specification in col. (3) is superior to using the more continuous measure, but take the former as our preferred specification because it gives more conservative coefficient estimates and is perhaps easier to interpret.

Col. (6) limits the sample to the years 1997 to 2006 to investigate whether the coefficient is being largely identified by comparing the post-2003 years to the earlier years in the sample and thus perhaps not reflecting the policy change. The coefficient on the interaction falls slightly, but is still positive and highly significant.

6.3 Calculating the total value of differential payments

To fully measure the fiscal impact of MA enrollment, we use the actual payments to plans in col. (7) of Table 5, including the budget-neutrality payments and allowing county benchmarks to grow at their actual rate. While the coefficient on the main MA effect remains unchanged, the coefficient on the interaction term grows substantially.

We use these results to estimate total differential payments in 2006, when total MA penetration was 16 percent and total Medicare enrollment was 43 million: $(899 + 2462) * 0.16 * 43$ million $=$ \$23 billion, or six percent of *total* Medicare spending on benefits in 2006. In 2009, with MA penetration at 23 percent and Medicare enrollment at 46.1 million, we estimate that total differential payments would be \$36 billion, or eight percent of total Medicare spending on benefits.[25]

6.4 Discussion

Given that our identification relies on those switching to MA from FFS, a natural question is whether the cost differences for this group are representative of the differences between the MA and FFS stock. For example, while readers may agree that we have indeed identified large differential payments the first year an individual is in MA, perhaps the cost-containment measures of MA plans slow cost growth thereafter, so that differential payments shrink after the initial year.

The MCBS does not allow us to answer this question, but CMS itself has provided evidence against this hypothesis (Center for Medicare and Medicaid Services, 2008). Specifically, they have found that the growth rate of risk scores in MA is faster than that in FFS. They attribute this phenomenon to "intensive coding"—enrollees in MA plans being diagnosed more thoroughly than their FFS counterparts. Because risk scores in the first year that a beneficiary is enrolled in MA depend on conditions recorded when he is still enrolled in FFS, the effect of intensive coding can begin no sooner than an individual's second year in MA. Given that the growth in risk scores is mechanically related to the growth in Medicare costs for the MA population, and that risk scores in the FFS population are, in expectation, equal to Medicare costs by the very construction of the risk score, this finding by CMS suggests that looking only at the first year understates the difference in the stock. After years of intensive coding on the part of MA plans, the difference between capitation payments and counterfactual costs in FFS should fan out further, not contract.

Turning to another potential concern, while we mentioned earlier that endogeneity in equation (5) likely works against finding the results in Table 5, readers may wish for fur-

[25] Medicare spending on benefits (i.e., excluding administrative costs) totaled \$385 billion and \$463 billion in 2006 and 2009 (both in 2007 dollars, to match the coefficient units), respectively. See Kaiser Family Foundation (2007) and Kaiser Family Foundation (2010), which give nominal estimates that we adjust with the CPI-U. Capitation payments between 2006 and 2009 would have been decreased by the phasing out of the budget-neutrality transition adjustment, but increased by the continued increase of county benchmarks during this period, so we suspect differential payments held roughly steady in the years after our sample period ends.

ther evidence. Any bias story working in the opposite direction must argue that while those switching to MA appear relatively healthy and low-cost the year before they switch, they systematically have higher medical costs their first year in MA—perhaps their health deteriorates or they put off an expensive medical procedure until joining MA—and thus would have also been expensive had they remained in FFS. We find this story unlikely for several reasons.

First, if those switching from FFS systematically experience a deterioration in health their first year in MA, then controlling for current-year health as we do in col. (4) should have substantially reduced the coefficients on the MA interaction and main effect. Second, individuals are unlikely to postpone expensive procedures until they join an MA plan because plans tend to have less generous cost-sharing arrangements for serious medical procedures than does FFS (Kaiser 2010). In fact, we find that individuals who switch to MA were no less likely to have an eye exam their last year in FFS, even though vision coverage is generally more generous in MA.[26] Consistent with there being no "Ashenfelter dip" the year before a switch to MA, when we control for the past two years of medical costs, instead of just one as in Table 5, the coefficients on the MA variables barely change, though become significant at only the ten-percent level due to the sample size falling by a half.

Considering the incentives MA plans face, these facts are not surprising. The least profitable enrollees for them in the post-period would be those who have little contact with the medical system their last year in FFS—and thus no documented HCC conditions and thus a low capitation payment—but suddenly become expensive their first year in MA. As we have shown throughout the last two sections, plans seem able to enroll the most profitable Medicare beneficiaries, though both the empirical work and model has not specified exactly how they do so. The next section seeks to shed light on a possible mechanism.

7 Enrollee satisfaction with their care as a function of health and MA status

The evidence in Section 5 shows that MA plans enrolled lower-cost individuals before and after risk adjustment. But how do they accomplish this selection, given that they must offer the same plans at the same rate to all Medicare beneficiaries in their geographical area of operation? We can imagine at least three strategies. First, they may target low-cost and thus more profitable individuals with advertisements or other recruiting efforts. Indeed, Bauhoff (2010) finds evidence that highly regulated German health insurance firms respond more quickly to enrollment requests from respondents residing in low-cost areas of the country, and MA plans have far greater flexibility than do German firms. Second, they can design

[26] The MCBS asks about eye exams the past year but not other specific examples of medical check-ups.

plans so that only healthy individuals will join, a possibility that Glazer and McGuire (2000) investigate theoretically for managed-care firms more generally. Finally, after individuals sign up for an MA plan, plans can treat healthy enrollees better than sick ones, so as to differentially retain the former group. Our data allow us to investigate this third hypothesis, though we think further work on these and other possible strategies is warranted.

7.1 Data and estimation strategy

The MCBS asks respondents to rate their satisfaction with their overall health care "last year" as well as specific aspects of it. As the question is asked in the fall, it is difficult to know whether individuals are answering based on their experience so far in the calendar year or the previous calendar year as well. As such, we generally sample those who did not switch (either from FFS to MA or from MA to FFS) the previous year. Thus, unlike the majority of analysis so far in the paper, identification comes from cross-sectional variation—comparing individuals in MA with individuals in FFS. Asking someone who, say, just switched from FFS to MA to rate their health care "last year" would likely shed little light on their experience so far in MA.

This sampling means we actually have little information on the medical spending of those in MA. Recall that after someone enters MA, the MCBS—and indeed the Medicare program itself—does not track their medical claims or costs, and without this information we cannot compute risk scores. As such, we cannot test whether the specific extensive- and intensive-margin selection patterns also arise with respect to satisfaction. We would have liked, for example, to see whether MA plans treat individuals with low costs relative to their risk scores better after risk-adjustment, but such detail is impossible given data limitations.

Instead, we focus on the fact that both before and after risk-adjustment, MA plans enroll individuals who are healthier than average. Before risk-adjustment, MA and FFS enrollees in our regression sample have mean self-reported health (from one, "poor," to five, "excellent") of 3.36 and 3.11, respectively. The differential shrinks slightly, to 3.34 and 3.13, after risk-adjustment, but the change is not close to being statistically significant. Given the results in Section 5 that selection along pre-period spending did not change, this result is not surprising.

One way plans might achieve this selection is to make their sicker enrollees unsatisfied with their health care and thus more likely to switch out of their plan (either to FFS or to a different MA plan). We thus estimate the following equation:

$$Satisfaction_{it} = \beta MA_{it} \times Health_{it} + \gamma MA_{it} + \mathbf{H}_{it} + \lambda X_i + \delta_t + \epsilon_{it}, \tag{6}$$

where $Satisfaction$ varies from one (very dissatisfied) to four (very satisfied), $Health$ is the five-category self-reported health variable described earlier, **H** are its corresponding fixed effects, and all other notation follows that used in previous equations. The health fixed effects account for the fact that in both MA and FFS, poor health might cause negative feelings toward one's health care, and thus the interaction term captures how much more or less sensitive enrollee satisfaction is to underlying health in MA versus FFS.

7.2 Results

Table 6 displays the results from estimating equation (6). We demean the Health variable in $MA \times Health$, so that the MA main effect represents the effect of MA enrollment for someone with mean self-reported health.[27] The first row reports results when overall satisfaction serves as the dependent variable. The MA main effect is negative—suggesting that someone of average health reports higher satisfaction in MA than in FFS. Of course, the type of person who joins MA might simply be harder to please—after all, FFS is the default and they chose to switch in the first place. As such, we do not take this coefficient to mean that MA plans in general deliver poorer services.

We instead focus on the interaction term, which is positive and significant, indicating that good health predicts satisfaction with MA plans more than it does satisfaction with FFS. In fact, relative to FFS enrollees, MA enrollees exhibit a more positive gradient of satisfaction with respect to health in all nine categories surveyed by the MCBS. In five of the nine categories (overall, out-of-pocket costs, doctor's concern for your health, questions answered over the phone, and receiving information about your medical condition) the coefficient is significant, and a sixth (having medical care provided in the same location) has a p-value of 0.113.

We interpret these results as suggesting that MA plans focus resources on keeping their healthier enrollees relatively happier than their sicker enrollees. Any cross-sectional bias story would have to argue that MA plans differentially attract disgruntled sick people or people who become especially disgruntled when they are sick. Not only does such a story require incredibly specific selection patterns, MA plans have no incentive to attract such individuals. We therefore believe our hypothesis, which is consistent with firms' profit maximization, offers a more likely explanation of the results in Table 6.

The last row of Table 6 investigates whether sicker MA enrollees "vote with their feet" and exit at higher rates than do sicker enrollees in FFS. Indeed, the same pattern emerges—not only are MA enrollees less likely to retain their current coverage status in general, but

[27] The sample size variation across different regressions arises from variation in the number of individuals who report not having enough experience to make a satisfaction rating as well as some questions not being asked in the earlier years.

this difference is especially pronounced for those in self-reported poor health.

The earlier results suggested that the probability of switching from FFS to MA was increasing in health, whereas this last result indicates that the probability of switching from MA to FFS is decreasing in health. As noted earlier, the county-level results are identified using both types of transitions, whereas the selection results in col. (6) of Table 3 are identified only by the first type of transition. The exit results are the basis for our earlier prediction that the county-level results would show less positive selection than the individual-level regressions based on those switching from FFS to MA.[28]

7.3 Discussion

The results in Table 6 begin to shed light on how MA plans actually risk-select, which our model treated in a very reduced-form manner. However, given the limitations mentioned earlier, we feel this topic warrants further work. Bauhoff (2010) uses an audit design in his work on German insurance firms, and applying his methods to MA plans would seem to us an excellent first step. Risk-selection will likely become an even more important public policy issue when the entire small group and individual health insurance markets will be subject to risk adjustment in 2014, as mandated by the Affordable Care Act.

8 Can risk adjustment be improved?

In this section we briefly outline the challenges we see in improving risk adjustment, first in terms of future re-calibrations of the HCC model, and then with respect to adding more categories to the formula.

8.1 Recalibrating the model

Given evolution in medical treatments and research, the costs associated with diseases change over time. As such, the coefficients in any risk-adjustment model need to be recalibrated.

However, the combination of intensive selection and the lack of cost data once beneficiaries leave FFS makes recalibration especially difficult in the context of Medicare Advantage. Recall the model in Section 3, where costs are defined as $m_i = b_i + v_i$. As we showed

[28] One might assume that because those exiting MA do not appear particularly healthy relative to the FFS stock, our selection results are over-estimated and thus our differential payment results may be over-stated as well. However, differential payments are a function not only of selection but also of capitation payments, and CMS has documented that risk scores grow faster in the MA stock than in the FFS stock due to intensive coding. So, at the point when they return to FFS, MA enrollees' capitation payments would have grown faster than their actual costs, and thus differential payments are in fact under-estimated by considering only those switching from FFS to MA. See Section 6 for further detail.

in Proposition 1, risk-adjustment will tend to increase the average value of b in the MA population, but decrease the average value of v. When the government wants to re-estimate costs conditional on b, they can only do so on the FFS population. But $E(v \mid b, FFS) > E(v \mid b)$, meaning that the government's estimate of $m_i = b_i + v_i$ will be biased upward for any value of b.

In fact, recalibration will likely exacerbate mispricing. If certain categories tend to be over-priced, MA plans have a greater incentive to recruit individuals in that category. As those individuals will generally be positively selected, their leaving FFS drives up the average FFS cost in that category. Upon the next recalibration, the category is more mispriced than before, and MA plans will have ever heightened incentives to recruit individuals with this condition.

8.2 Adding more categories to the formula

A natural reaction to the intensive-margin results in Table 3 is that the government should simply add more detail to the formula. The most obvious drawback to doing so is that it provides firms even more scope to "intensively code." If the only two conditions in a formula are heart attack and cancer, outside of actual fraud, MA plans cannot document that a patient has one of these conditions when he in fact does not. But "diabetes with complications" is far more open to interpretation; CMS reports that, relative to FFS, MA plans tend to interpret gray areas in a manner that results in higher risk scores. In addition, having extremely detailed categories would result in, essentially, paying MA plans on the margin for services performed, undercutting one of the primary rationales for the MA program.

A less obvious drawback is that having a more flexible HCC model would mean increasing the number of estimated coefficients for the risk adjustment model. While the sample of individuals on which CMS can perform its estimation is large, it is not unlimited. When choosing the complexity of a risk adjustment model, CMS faces a tradeoff between adding more parameters to their model (and explaining more of the variance in costs) and measuring each coefficient precisely. Even if each coefficient is unbiased, having mismeasured coefficients can lead to large differential payments if MA plans are able to estimate the model more precisely than is CMS and attract patients with the over-priced conditions.[29]

[29] To take an extreme example of how a very flexible risk adjustment model can increase differential payments due to mismeasurement of the model's coefficients suppose, for example, that CMS estimated a fully non-parametric version of the HCC model, where costs were estimated for every combination of the 70 HCC conditions. Such a model would have 2^{70} parameters, or trillions of times the number of FFS beneficiaries enrolled in a ten-year period. Here, many cells would have only 1 or 2 beneficiaries within them, and the corresponding coefficients would be measured with error. Given the skewness of medical costs, some cells would be wildly over-priced, and going forward, MA plans would have strong incentives to attract individuals with these specific conditions, leading to large differential payments.

9 Conclusion

This paper began by modeling the effects of an effort to lower differential payments to private Medicare plans by risk-adjusting their capitation payments. We showed that plans respond to risk-adjustment in two ways: they decrease their screening efforts along dimensions included in the model ("extensive-margin" selection), while increasing screening efforts along dimensions excluded from the model ("intensive-margin" selection). As the model demonstrates, these responses can actually thwart the government's objective of decreasing differential payments.

We find support for the extensive- and intensive-margin predictions using both individual- and county-level data on Medicare expenditures. Moreover, we find that differential payments to MA plans are substantial, and actually grew after risk-adjustment. We estimate that in 2009 they totaled $36 billion, eight percent of total Medicare expenditures. To put this number in a slightly different context, the Congressional Budget Office estimates that in 2016 the total cost of the Affordable Care Act's insurance expansion provisions (e.g., the Medicaid expansion up to 133 percent of the poverty line and the subsidies to low-income individuals in the state insurance exchanges) will total $114 billion. Thus, the total differential payments to MA plans are equivalent to nearly one-third of the total cost of extending insurance to over 30 million uninsured Americans.[30]

Of course, the increase in differential payments is not necessarily waste. It is passed on as revenue to insurance firms and potentially additional benefits to MA enrollees. Town and Liu (2003) estimate that between 1993 and 2000, nearly $3 of firm profits were generated for every $1 of consumer surplus the MA program created. How the surplus is split between profits, administrative costs and consumer benefits likely depends on the level of competition in the local MA market, which we do not analyze and might indeed make for interesting future work.

We close by returning to the potential distributional consequences of our results. Regardless of how the surplus described above is split, the MA program appears to expand the cost of Medicare while also transferring relative expenditures from the FFS population toward the financing of care for the MA population. As those switching into MA have, throughout the sample period, lower baseline costs and better self-reported health than do those remaining in FFS, the MA program transfers Medicare expenditures to those who likely have less need for it.

Moreover, as we show in Section 6, the gradient of satisfaction with one's health care is a

[30] The CBO estimates the cost to be $132 billion in 2016 dollars, and we use the CBO's forecasts for inflation to deflate this estimate to 2007 dollars, the units of the regression estimates.

more positive function of self-reported health for MA enrollees than FFS enrollees, consistent with MA treating their healthier (and thus more profitable) enrollees better so as to differentially retain them. Indeed, exit rates out of MA plans are differentially higher among those in poor health. Therefore, the MA program appears not only to transfer aggregate Medicare expenditures from the relatively higher-cost FFS population to the relatively lower-cost MA population, but it seems to effect a similar transfer *within the MA population*.

These results suggest that governments may wish to take special care in "contracting out" social insurance. Imperfect pricing—whereby the government overpays a private firm relative to the cost and quality of in-house production—is, of course, a potential concern every time governments contract with a private party and has received great attention in the literature (see, for example, Hart *et al.* 1997). In the case of, say, paving a road, the consequences of imperfect pricing would seem limited to whatever amount the government overpaid. With social insurance programs, however, imperfect pricing can induce private firms to cream-skim, exacerbating the utility consequences of the underlying inequality the program was initially intended to mitigate. At least in the case of Medicare, we find little evidence that risk-adjustment has solved this problem.

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Figure 1: Medicare cost increases for enrollees switching from FFS to MA, 1994 to 2003

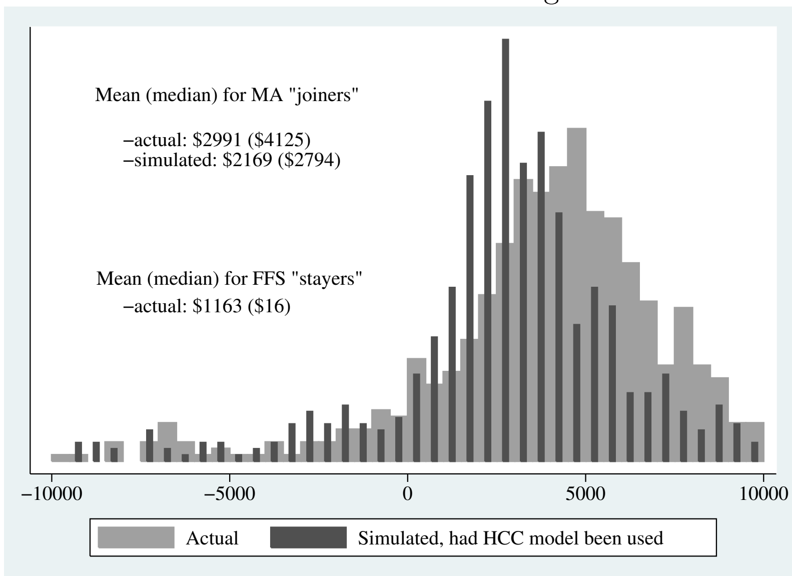

Table 1: Summary statistics, 1994-2006 Medicare Current Beneficiary Survey

	FFS	MA	Difference: FFS - MA
Disabled	0.13	0.067	0.061**
Age	72.8	74.0	-1.12**
Age, those not disabled	76.2	75.4	0.88**
Female	0.57	0.57	-0.0012
Black	0.092	0.098	-0.0057*
Hispanic	0.018	0.034	-0.015**
Self-reported health	3.12	3.35	-0.23**
Income > 20,000	0.45	0.48	-0.028**
Has a high school degree	0.75	0.74	0.0035
Total cost	7310.1	8044.8	-734.7**
Observations	131,339	19,320	

Table 2: Summarizing changes in incentives after risk-adjustment

Quintiles of HCC score	HCC payment minus Demographic payment	HCC payment minus actual Medicare cost
1	-3,267	1,391
2	-3,242	2,256
3	-2,027	2,494
4	-191	2,379
5	7,419	-4,261
Total	-265	813
Observations	55,367	55,367

Notes: All data taken from the "pre-period" before implementation of risk-adjustment, among the subsample of individuals who were in the FFS system all twelve months of the previous year. Both columns use claims data from the previous year to calculate capitation payments under the HCC model for each individual. The first column follows the formula of the demographic model to calculate capitation payments for all individuals. We only show five quantiles in the interest of space, but HCC payments minus actual costs becomes negative above the 85^{th} percentile, or roughly a risk score of 1.84.

Table 3: Changes in selection patterns after risk-adjustment

	Extensive margin			Intensive margin		
	(1) Score	(2) Score	(3) Score	(4) Cost	(5) Cost	(6) Cost
Will join MA next year	-0.118***	-0.420***		229.4		-2242.2***
	[0.0172]	[0.147]		[253.4]		[341.0]
Will join MA next year x After 2002	0.0643**	0.0566		-1045.3**		-448.2
	[0.0309]	[0.194]		[511.3]		[634.1]
Fraction of next year spent in MA			-0.150***		171.5	
			[0.0202]		[316.5]	
Fraction of next year in MA x After 2002			0.105***		-1217.9**	
			[0.0358]		[604.0]	
HCC score				9903.6***	9903.4***	
				[182.5]	[182.5]	
Mean of dept. var.	0.862	3.181	0.862	6491.9	6491.9	6491.9
Sample	$\mathbb{E}(\pi) > 0$	$\mathbb{E}(\pi) < 0$	$\mathbb{E}(\pi) > 0$	All	All	All
Observations	62,889	10,165	62,889	73,054	73,054	73,054

Notes: All observations are in FFS all twelve months of the given year. Year fixed effects included in all regressions. HCC score is calculated using FFS claims data. Standard errors are clustered by the individual.

Table 4: Changes in county per-capita FFS spending as a function of MA penetration

	Change in per capita FFS spending			
	(1)	(2)	(3)	(4)
Δ Log(FFS share of county)	-1022.5***	-1191.5***	-1060.3***	-1267.6***
	[153.1]	[181.4]	[223.7]	[249.5]
Δ Log(FFS share of county) x After 2003			78.36	166.4
			[264.7]	[284.5]
County fixed effects	No	Yes	No	Yes
Observations	24,878	24,878	24,878	24,878

Notes: Each column summarizes the results from a different specification in which the dependent variable (listed at the top of each column) is a measure of the change in a county's per-capita Medicare expenditures from one year to the next. Data includes all U.S. counties (with the exception of those in Alaska and a few others with missing data on Medicare enrollment in one or more years) in each year from 2000 through 2008 (and thus there are eight first-differences for each county). All specifications include eight year effects, 3,110 county effects, and are weighted by each county's share of the U.S. Medicare population in each year. Dollar amounts are adjusted to 2007 dollars using the CPI-U and standard errors are clustered at the county level.

Table 5: Changes in differential payments after risk-adjustment

	Dependent variable: Total Medicare Cost						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Enrolled in MA	1890.2***	937.3***	628.0***	683.7***		899.9***	899.2***
	[335.5]	[221.7]	[243.0]	[258.4]		[314.7]	[314.8]
Enrolled in MA x After 2003	1354.1*	1412.5**	1934.0***	1928.6***		1658.8**	2462.2***
	[757.5]	[647.4]	[683.0]	[702.0]		[711.8]	[729.9]
Fraction of year in MA					687.1**		
					[287.8]		
Fraction of year in MA x After 2003					2492.5***		
					[806.5]		
Dependent var is in...	Δs	Levels	Levels	Levels	Levels	Levels	Levels
Benchmark growth fixed	Yes	Yes	Yes	Yes	Yes	Yes	No
Budget-neutrality removed	Yes	Yes	Yes	Yes	Yes	Yes	No
Demog. controls	No	No	Yes	Yes	Yes	Yes	Yes
Health controls	No	No	No	Yes	No	No	No
Only 2000-2006	No	No	No	No	No	Yes	Yes
Observations	73,054	72,965	72,668	72,404	72,668	60,206	60,206

Notes: All observations are in FFS all twelve months of the previous year. Year fixed effects included in all regressions, and county fixed effects included in all regressions after col. (2). All regressions in "levels" include a once-lagged dependent variable, as well as dummy variables corresponding to 21 bins of lagged Part A and Part B spending (with zero as its own bin and 20 bins corresponding to 20 quantiles of positive spending). Standard errors are clustered by the individual. "Post-2003 adjustment" refers to reducing MA payments after 2003 in the following manner: the growth rate in benchmarks is constrained to match that of the pre-period, and the "budget neutrality" adjustment meant to ease the risk-adjustment process is eliminated. Both of these adjustments make it less likely that the interaction term would have a positive coefficient, as one can see from comparing cols. (5) and (6). "Controls" include the following: fixed effects for the five categories of current self reported health (excellent, very good, good, fair, poor) and five categories of self-reported change in health (much better, somewhat better, the same, somewhat worse, much worse); indicators for gender, race and Hispanic origin; income category fixed effects; marital status fixed effects; and fixed effects for eligibility status (disabled and old-age, with and without ESRD as a secondary condition).

Table 6: Effect of MA enrollment and health status on enrollee satisfaction

Dependent var:		OLS coefficient estimates (clustered SEs)	
Satisfaction rating (1-4)	Obs.	Enrolled in MA	MA x Health (demeaned)
Overall medical care	75,890	-0.0408***	0.0144**
		(0.00777)	(0.00661)
Out-of-pocket costs	75,315	0.0165*	0.0222***
		(0.00909)	(0.00745)
Follow-up care	69,770	-0.0191***	0.00573
		(0.00702)	(0.00603)
Doctor's concern for your health	74,717	-0.0333***	0.0151**
		(0.00748)	(0.00634)
Information about your medical condition	75,545	-0.0245***	0.0127**
		(0.00703)	(0.00593)
Access to specialists	57,193	-0.0497***	0.000594
		(0.00772)	(0.00646)
Questions answered over phone	48,622	-0.0279***	0.0195**
		(0.00926)	(0.00771)
Availability of care nights and weekends	44,507	-0.00920	0.00726
		(0.00916)	(0.00796)
Medicare care provided in same location	69,386	0.0208***	0.00868
		(0.00695)	(0.00574)
Retains coverage type next year (probit coefficients)	82,145	-0.545***	0.0787***
		(0.0274)	(0.0213)

Notes: Each row represents a regression of the form: $satisfaction\, category_i = \beta_1 MA_i + \beta_2 MA_i \times Health_i + \gamma \mathbf{H}_i + \lambda \mathbf{X}_i + \epsilon_i$, where $satisfaction$ takes values from one to four ("very dissatisfied," "dissatsifed," "satisfied," "very satisfied"), MA is a dummy variable for being enrolled in Medicare Advantage, $Health$ is a (demeaned) linear measure of the five-category self-reported health variable, $\mathbf{H}$ is a vector of fixed effect for the five health categories, and $\mathbf{X}$ is a vector of basic controls: age, state-of-residence, and year fixed effects, and indicator variables for being female, disabled, or on Medicaid. As the $Health$ variable is demeaned, the coefficient on the MA indicator variable represents the effect of being enrolled in MA for an enrollee with average health. A positive coefficient on $MA \times Health$ indicates that the relationship between satisfaction and health status for MA enrollees is greater ("more positive") than that for FFS enrollees.

Appendix Table 1: Frequency distribution of transitions between FFS and MA, 1994-2006

	Baseline year t equals...				Total
	1994-1996	1997-1999	2000-2002	2003-2005	t=1994-2005
FFS (year t) $\rightarrow$ FFS (year t+1)	19,017	18,539	18,305	17,329	73,190
FFS (year t) $\rightarrow$ MA (year t+1)	566	399	102	464	1,531
MA (year t) $\rightarrow$ FFS (year$t$+1)	102	165	457	125	849
MA (year t) $\rightarrow$ MA (year t+1)	1,457	3,282	2,805	2,496	10,040
In sample both years	21,142	22,385	21,669	20,414	85,610
Left sample after baseline year	13,883	14,301	14,983	14,284	57,451
Total observations (baseline year)	35,025	36,686	36,652	34,698	143,061

Notes: Unit of observation is a person in a year. Drops all person-year observations in which a person is eligible for only Part A or Part B for any part of the year of if the person is eligible for Medicare only because of having ESRD. An individual in a given year is classified as being on MA if she is on MA for more than half of the months for which she is Medicare eligible in that given year.

Proofs of Propositions

Proposition 1. *The following two conditions hold when the risk-adjusted share* Ω *of the capitation payment increases:*

(i) Firms decrease screening efforts along the b *margin and thus the average value of* b *among their enrollees rises.*

(ii) Firms increase screening efforts along the v *margin and thus the average value of* v *among their enrollees falls.*

Proof. We are required to show that $\frac{\partial b^*}{\partial \Omega} < 0$ and $\frac{\partial v^*}{\partial \Omega} > 0$, where b^* and v^* are a firm's optimal levels of b and v.

The first-order conditions from maximizing the profit expression in equation (1) with respect to b and v are given by

$$[b] \;:\; \Omega p'(b^*) - c_b(b^*, v^*) = 1 \tag{7}$$

$$[v] \;:\; -c_v(b^*, v^*) = 1 \tag{8}$$

Totally differentiating equation (7) with respect to Ω yields:

$$p'(.) + \Omega p''(.)\frac{\partial b^*}{\partial \Omega} - c_{11}(.,.)\frac{\partial b^*}{\partial \Omega} - c_{12}(.,.)\frac{\partial v^*}{\partial \Omega} = 0 \tag{9}$$

Similarly, equation (8) yields:

$$c_{12}(.,.)\frac{\partial b^*}{\partial \Omega} + c_{22}(.,.)\frac{\partial v^*}{\partial \Omega} = 0$$

or

$$\frac{\partial v^*}{\partial \Omega} = -\frac{c_{12}}{c_{22}}\frac{\partial b^*}{\partial \Omega}. \tag{10}$$

Substituting equation (10) into (9) gives:

$$p'(.) + \Omega p''(.)\frac{\partial b^*}{\partial \Omega} - c_{11}(.,.)\frac{\partial b^*}{\partial \Omega} - c_{12}(.,.)\left(-\frac{c_{12}}{c_{22}}\frac{\partial b^*}{\partial \Omega}\right) = 0.$$

We can now solve for $\frac{\partial b^*}{\partial \Omega}$ and sign many of the terms:

$$\frac{\partial b^*}{\partial \Omega} = \frac{\overbrace{p'}^{+\text{ as cap payments increase in } b}}{-\Omega p'' + \dfrac{\overbrace{(c_{bb}c_{vv}) - c_{bv}^2}^{+\text{ by convexity of } c(.,.)}}{\underbrace{c_{vv}}_{+\text{ by convexity of } c(.,.)}}} \tag{11}$$

By assumption, $p'' = 0$, so the entire denominator is positive.

As $\frac{\partial b^*}{\partial \Omega} > 0$ and $c_{bv}, c_{vv} > 0$, equation (10) gives the result in (ii). ■

Corollary 1. *The effect of increasing Ω on firms' overall insurance costs m is ambiguous.*

Proof. Differentiating total costs $m = c + b$ by Ω yields

$$\frac{\partial m}{\partial \Omega} = \frac{\partial b}{\partial \Omega} + \frac{\partial v}{\partial \Omega},$$

or, via substitution from equation (10),

$$\frac{\partial m}{\partial \Omega} = (1 - \frac{c_{12}}{c_{22}})\frac{\partial b}{\partial \Omega}.$$

While Proposition 1 guarantees $\frac{\partial b}{\partial \Omega}$ is positive, $(1 - \frac{c_{12}}{c_{22}})$ can be positive or negative. ■

Proposition 2. *For any $\Omega \in [0,1]$ and any $p(.)$ such that $p' < 1$, firms will enroll only individuals with $b < \bar{b}$.*

Proof. If it sufficient to prove this claim holds for $\Omega = 1$ since we know from Proposition 1 that $\frac{\partial b^*}{\partial \Omega} > 0$.

The first-order condition for b in equation (7) with $\Omega = 1$ is $p'(b^*) - c_b(b^*, v^*) = 1$. As $p' < 1$, $c_b(b^*, v^*)$ must be negative. By assumption c_b is negative if and only if $b < \bar{b}$ (recall that screening costs grow as a function of distance from the mean), so the first-order condition only holds for b below $\bar{b}$. ■

Corollary 2. *For any $\Omega \in [0,1]$ and any $p(.)$ such that $p' < 1$, firms will enroll individuals with $p(b) < \mathbb{E}(p(b))$.*

Proof. From Proposition 2, firms always choose $b < \bar{b}$. By linearity of $p(.)$, $\mathbb{E}(p(b)) = p(\bar{b})$, and since $p' > 0$, $p(b) < p(\bar{b})$ for all $b < \bar{b}$. ■

Corollary 3. *For any $\Omega \in [0,1]$ and any $p(.)$ such that $p' < 1$, firms always positively select with respect to total costs $m = b + v$.*

Proof. From Proposition 2, we know firms always positively select with respect to b so it is sufficient to show that they always positively select with respect to v as well. The first-order-condition with respect to v is $-c_v(b^*, v^*) = 1$. So, c_v must be negative, and $c_v < 0$ iff $v < \bar{v}$.
■

Proposition 3. *For $\Omega_0 < \Omega_1$, moving from Ω_0 to Ω_1 will always decrease differential payments if b and v are held fixed at their equilibrium values under Ω_0.*

Proof. The result is easy to show when $p(.)$ is linear. Recall that $p(.)$ is "payment-neutral," so that $\mathbb{E}(p(b)) = \bar{p}$. For linear p, $\mathbb{E}(p(b)) = p(\bar{b}) = \bar{p}$, so risk-adjustment does not change the payment for an individual with $b = \bar{b}$. As $p' > 0$, $p(b) < p(\bar{b}) = \bar{p}$ for all $b < \bar{b}$. From Proposition 2 we know that $b < \bar{b}$ in equilibrium, so $p(b) < \bar{p}$ for any equilibrium b. ■

Proposition 4. *The effect of increasing Ω on a firm's average differential payment is ambiguous.*

Proof. Let $\phi(\Omega)$ denote the differential payment the risk-adjusted share of the capitation payment is set to Ω and firms are at their optimal b and v values:

$$\phi(\Omega) = \underbrace{\Omega p(b^*(\Omega)) + (1-\Omega)\bar{p}}_{\text{capitation payment}} - \underbrace{(b^*(\Omega) + v^*(\Omega))}_{\text{actual costs}} \tag{12}$$

Differentiating with respect to Ω gives:

$$\phi'(\Omega) = \Omega p' \frac{\partial b^*}{\partial \Omega} + p(b^*) - \bar{p} - \frac{\partial b^*}{\partial \Omega} - \frac{\partial v^*}{\partial \Omega} \tag{13}$$

Rearranging and substituting $\frac{\partial v^*}{\partial \Omega} = \frac{c_{bv}}{c_{vv}} \frac{\partial b^*}{\partial \Omega}$ from equation (10) yields

$$\phi'(\Omega) = [p(b^*) - \bar{p}] + \frac{\partial b^*}{\partial \Omega}(\Omega p' - 1 - \frac{c_{bv}}{c_{vv}}) \tag{14}$$

We showed in the proof of Proposition 3 that $p(b^*) < \bar{p}$ for any equilibrium b^*, so the first term (in brackets) is negative. However, the second term is ambiguous. While Ω and p' are both by assumption less than one and $\frac{\partial b^*}{\partial \Omega} > 0$ by Proposition 1, if $\frac{c_{bv}}{c_{vv}}$ is sufficiently large in magnitude, the expressing can indeed be positive.

■

Endogenizing firm enrollment size

We now assume that firms maximize *total*, as opposed to per capita, profits which equal $q(b,v)\pi(b,v,\Omega)$, where π is average per capita profits as specified in equation (1) and q is the number of enrollees the firm has.

The first-order conditions with respect to b and v are now:

$$[b] \quad : \quad q_b(b,v)\pi(b,v,\Omega) + q(b,v)\underbrace{(\Omega p' - 1 - c_b(b,v))}_{\pi_b} \tag{15}$$

$$[v] \quad : \quad q_v(b,v)\pi(b,v,\Omega) + q(b,v)\underbrace{(-1 - c_v(b,v))}_{\pi_v} \tag{16}$$

Note that when the level of q is larger relative to (i) its partial derivatives or (ii) the level of per capita profits, then equations (15) and (16) reduce to the original first-order conditions of $\pi_b = \pi_v = 0$.

Why $p(.) \in (0,1)$

Here, we explain why we assume that although the government's estimate of costs grows with b it grows less than one-for-one, or $p'(b) \in (0,1)$.

First, assume that the government observes b and c for each individual i and wishes to create a payment function, $p(b)$ that best predicts c_i for each individual i. Since we assumed that $c = b + v$ and $\mathbb{E}(v|b) = 0$, if the government ran a regression of $c_i = \alpha_0 + \alpha_1 b_i + \epsilon_{1i}$, the goverment would find $\hat{\alpha}_1 \xrightarrow{p} 1$.[31] That is, without measurement error, the government would set $p'(.) = 1$.

Now, the government observes only a mismeasured version of $b, \hat{b} = b + \varepsilon$, with ε independent of b and v. This ε comes from the fact that the government risk adjusters have only finite sample size, so it cannot run a totally non-parametric regression of all of the HCC indicators on costs. By imposing the HCC model, the government ensures that its composite measure of health (the risk score; $\hat{b}$) does not perfectly equal the composite measure of health (b). Given the know value $\hat{b}$, the government wishes to construct a function $\tilde{p}\left(\hat{b}\right)$ that best predicts c_i for each individual i. If the government ran a regression of $c = \beta_0 + \beta_1 \hat{b} + \epsilon_{2i}$, the government would find $\hat{\beta}_1 \xrightarrow{p} \beta_1 < 1$ due to the measurement error in $\hat{b}$.[32] So, we know that $p'\left(\hat{b}\right) < 1$. In particular, $p\left(\hat{b}\right) = \beta_0 + \beta_1 \hat{b}$. For our model, we want to know $p(b)$, how the

[31] Mathematically, we know that

$$\begin{aligned}
\hat{\alpha}_1 \xrightarrow{p} \alpha_1 &= \frac{cov(c,b)}{var(b)} \\
&= \frac{cov(b+v,b)}{var(b)} \\
&= \frac{cov(b,b)}{var(b)} + \frac{cov(v,b)}{var(b)} \\
&= 1 + \frac{cov(v,b)}{var(b)} \\
&= 1
\end{aligned}$$

[32] Mathematically, we know that

$$\begin{aligned}
\hat{\beta}_1 \xrightarrow{p} \beta_1 &= \frac{cov\left(c,\hat{b}\right)}{var\left(\hat{b}\right)} \\
&= \frac{cov(c,b+\varepsilon)}{var(b+\varepsilon)} \\
&= \frac{cov(c,b)}{var(b+\varepsilon)} + \underbrace{\frac{cov(c,\varepsilon)}{var(b+\varepsilon)}}_{=0 \text{ since } \varepsilon \text{ is independent of } b \text{ and v}} \\
&= \frac{cov(c,b)}{\underbrace{var(b)+var(\varepsilon)}_{\text{no covariance term since } \varepsilon \text{ is independent of } b}} \\
&< 1 \text{ since the numerator is the same as above, but the denom is larger}
\end{aligned}$$

average cap payment for a person with a given b. This equals

$$
\begin{aligned}
p(b) &= \mathbb{E}\left(\tilde{p}(b+\varepsilon)\right) \\
&= \int_{-\infty}^{\infty} \tilde{p}(b+\varepsilon) \underbrace{f(\varepsilon)}_{\text{density of } \varepsilon} d\varepsilon \\
&= \int_{-\infty}^{\infty} \beta_0 + \beta_1 (b+\varepsilon) \cdot f(\varepsilon)\, d\varepsilon \\
&= \beta_0 + \beta_1 \int_{-\infty}^{\infty} (b+\varepsilon) \cdot f(\varepsilon)\, d\varepsilon \\
&= \beta_0 + \beta_1 b + \underbrace{\int_{-\infty}^{\infty} \varepsilon \cdot f(\varepsilon) d\varepsilon}_{\text{integrates to 0}} \\
&= \beta_0 + \beta_1 b
\end{aligned}
$$

So, $p'(b) = \beta_1 < 1$.