How does risk selection respond to risk adjustment? New evidence from the Medicare Advantage Program*

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Abstract

To combat adverse selection, governments increasingly base payments to health plans and providers on enrollees' scores from risk-adjustment formulae. In response to evidence of plan overpayments due to selection, in 2004 Medicare began to risk-adjust capitation payments to private Medicare Advantage (MA) plans. But because the variance of medical costs increases with the predicted mean, incentivizing enrollment of individuals with higher risk scores can increase the scope for enrolling "over-priced" individuals with costs significantly below the formula's prediction. Indeed, after risk adjustment, MA plans enrolled individuals with higher scores but significantly lower costs *conditional* on their score. We find that overpayments on net rise and only limited evidence that consumer surplus increased.

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1 Introduction

Recent health care reforms have attempted to move away from the fee-for-service (FFS) payment model—which economists have long argued incentivizes over-provision of services—by paying providers or insurers fixed capitation payments rather than reimbursing them for each service. The success of such reforms hinges on correctly aligning capitation payments with a patient's expected cost. Otherwise, plans and providers will have incentives to cream-skim over-priced cases instead of competing on quality or cost.

To more accurately equate payments with expected costs, governments and other insurance sponsors have increasingly turned to "risk adjustment"—setting payments to insurers or providers to take account of an individual's past and current health conditions. For example, the Affordable Care Act of 2010 (ACA) relies heavily on risk adjustment.[1] However, empirical research on these attempts to risk-adjust has been limited.[2]

In this paper, we provide an assessment of the largest risk-adjustment effort to date in the U.S. health care sector—Medicare's risk adjustment of capitation payments to private Medicare Advantage (MA) plans, which the ACA suggests as the model for risk adjustment in the state-run insurance exchanges—on selection into MA plans and on the government's total cost of financing Medicare benefits. Since the 1980s, Medicare enrollees have been able to enroll in either the traditional fee-for-service (FFS) program or in an MA plan, which can provide additional services but must cover the basic benefits guaranteed by traditional Medicare. For an individual in an MA plan, the government pays the plan a capitation payment meant to cover the cost of providing her Medicare benefits. Today, more than one-fourth of Medicare's 51 million enrollees receive their care through a private MA plan.

Before 2004, an MA enrollee's capitation payment was, essentially, based on the average cost of FFS enrollees with the same demographic characteristics in her county and was not adjusted for health conditions. Despite regulations requiring MA plans to offer the same plan at the same price to all Medicare beneficiaries in its geographical area of operation, researchers found that less costly individuals were much more likely to enroll in an MA plan.[3]

[1] Approximately 25 million people are projected to join the insurance exchanges established by the ACA, in which private insurers will receive capitation payments adjusted for enrollees' health status.

[2] There is a large, mostly theoretical or statistical, 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 one used by Medicare, on which we focus the empirical work) 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 their effect on selection and costs is a central policy question. On the empirical side, Bundorf *et al.* (2012) provides estimates on the welfare gains to risk adjustment of health insurance *premiums*.

[3] See, e.g., Langwell and Hadley (1989), Physician Payment Review Commission (1997), Mello *et al.* (2003) and Batata (2004).

Reacting to this evidence of "differential payments" to MA plans—payments in excess of the expected cost of covering a beneficiary in traditional FFS—in 2004 Medicare began to base capitation payments on an individual's "risk score," generated by a risk-adjustment formula accounting for more than seventy disease conditions.

We develop a simple model to show that plans' endogenous response to risk adjustment can undo the intended goal of reducing overpayments and test it using data from the Medicare Current Beneficiary Survey (MCBS). Before risk adjustment, MA plans had an incentive to enroll individuals who were low cost on all dimensions. After risk adjustment, plans no longer need to avoid beneficiaries with conditions *included in the formula*. In a difference-in-differences model, we show that, relative to individuals who remain in FFS, risk scores of those joining MA increase after risk adjustment, consistent with our model's predictions.

However, our model emphasizes how selection can take place on different margins. While risk adjustment indeed decreases plans' scope for advantageous selection along the dimensions *included* in the formula, it increases the incentive to find individuals who are positively selected along dimensions *excluded* from the formula and are thus "cheap for their risk score." Indeed, as the model predicts, we find that actual costs *conditional on the risk score* of those joining MA fall substantially after 2003, relative to those remaining in FFS.

Finally, the model makes clear that the former effect (the decrease in selection along dimensions included in the formula) can be more than offset by the latter effect (the increased selection conditional on the risk score). The key insight is that because the variance of medical costs increases with the expected mean, there are more cases of extremely high overpayments among those with high risk scores. Figure 1, which plots average medical costs along with the 10th and 90th percentile, shows how the variance of medical costs increases with a patient's risk score. Given that costs are bounded below by zero, overpayments to those with a risk score of 0.5 are bounded above by $3,500, whereas if plans can manage to avoid the costliest ten percent of enrollees with a risk score of two (five), their overpayments for this group would average over $5,000 ($9,000).[4]

Due to this increase in variance, the ability of firms to enroll individuals with costs substantially below the formula's prediction—whether through targeted advertising or designing benefits packages that differentially appeal to certain people—can actually increase after risk adjustment, and with it the government's total cost of financing the Medicare program. To take but one example from our data, pre-risk-adjustment, Hispanics were roughly $1,200 cheaper on average than their (non-risk-adjusted) capitation payments; after risk adjustment, Hispanics with a history of congestive heart failure (one of the most common condi-

[4]For these calculations, we use average FFS costs as an estimate for plan per-enrollee payments. The next section discusses modifications to this formula over time.

tions included in the risk formula) are on average \$3,500 cheaper than their (risk-adjusted) capitation payments. Intuitively, before risk adjustment MA plans fished in a pond of relatively healthy enrollees with little cost variance. Risk adjustment allows them to fish in a pond of enrollees who have higher costs on average but also highly *variable* costs. Indeed, we find that after risk adjustment, overpayments are higher, an increase equal to roughly nine percent of average Medicare per capita spending.

This counterintuitive consequence of risk adjustment has, to the best of our knowledge, not been noted by other researchers, but is related to the literature on the unintended consequences of increasing the specificity of incomplete contracts. 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 other tasks (as in Holmstrom and Milgrom, 1991) or the instructor who "teaches to the test" at the expense of other educational goals (as in Lazear, 2006). More generally, our results suggest that using additional information to determine prices can sometimes aggravate problems associated with asymmetric information, as in Einav and Finkelstein (2011).

While we find little evidence that risk adjustment accomplished the goal of reducing overpayments, we also examine whether the increased overpayments led to greater consumer or producer surplus. Our results suggest little to no improvement in several alternative measures of beneficiary satisfaction and quality of care. These results are consistent with recent research regarding the incidence of MA reimbursement generosity (Cabral *et al.*, 2013), with additional advertising expenditures absorbing much of the additional Medicare spending (Mehrotra *et al.*, 2006 and Duggan *et al.*, 2013). Perhaps because of these additional marketing costs, benefits to plans were also limited, with CMS actually increasing plan reimbursement to cushion the expected negative effect of risk adjustment on insurers' profits.

The remainder of the paper is organized as follows. Section 2 provides background information on the MA program and the risk-adjustment formula Medicare currently uses. Section 3 presents the intuition and results from the model. Section 4 describes the data. Sections 5 and 6 present the empirical results on selection and differential payments, respectively. Section 7 explores potential mechanisms by which MA plans might be able to differentially select certain enrollees. Section 8 explores the welfare consequences of risk adjustment and discusses ways to improve it, and Section 9 concludes.

2 Medicare Advantage capitation payments and risk adjustment

Since the 1980s, Medicare enrollees have had the choice between the traditional FFS program and private MA plans (previously known as Medicare+Choice or Part C plans). The evolution of MA enrollment as a share of total Medicare enrollment during our sample period is plotted in Appendix Figure 1.

Plans must accept all applicants residing in their areas of operation and provide benefits that are covered under traditional Medicare. MA plans have considerable latitude in creating their hospital and physician networks. Many offer extra benefits such as vision care, dental care, and gym memberships. Plans can also charge a monthly premium, reduce enrollees' Medicare Part B premiums, or vary copayments.

The Medicare program pays MA plans a fixed capitation payment to cover these costs (excluding hospice care, which FFS covers), and plans are, essentially, the residual claimants if actual costs are above or below the capitation payment. Since 2006, Medicare Part D has provided enrollees coverage for prescription drugs, though all of our analysis will focus on Part A (hospital and inpatient) and B (physician and outpatient), as these are the services MA plans are required to provide.[5]

The capitation payment to an MA plan for covering an individual is based on the estimated Part A and B payments had FFS Medicare covered her directly. During the 1980s and 1990s, the Center for Medicare and Medicaid Services (CMS)—the agency that administers Medicare—used a "demographic model" to perform this estimation, so-called because it included primarily demographic variables (gender, age, and disability, Medicaid and institutional status) as opposed to disease or health conditions. The demographic model would output a "risk score" (with mean one) that when multiplied by a county-level "benchmark" would determine the capitation payment. Then as now, CMS did not require MA plans to report cost or claims data, so it used FFS data to regress total Part A and B spending on these demographic factors, finding that one percent of FFS expenditures were explained by the risk score (Pope *et al.*, 2004).

In response to research showing that MA plans enrolled beneficiaries who were significantly cheaper than the demographic model predicted, CMS revised its risk-adjustment procedure.[6] In 2000, CMS made ten percent of capitation payments dependent on inpatient claims data, raising the effective R^2 of the formula from 1.0 to 1.5 percent. More significantly, in 2004—which for simplicity we term the "start" of risk adjustment—CMS introduced the hierarchical condition categories (HCC) model, still in use today. The HCC model, like the demographic model, uses data from the FFS population to predict FFS costs in the following year, but instead of relying only on demographic data, it also accounts for the disease conditions included on FFS providers' claims. The model distills the roughly 15,000 ICD-9 codes

[5]MA plans that provide prescription drug coverage receive a separate capitation payment in return.

[6]Estimates from Langwell and Hadley (1989), Physician Payment Review Commission (1997), Mello *et al.* (2003) and Batata (2004) suggest that individuals switching from traditional FFS to MA had medical costs between 20 and 37 percent lower than observably similar individuals who remained in FFS.

that providers can list on claims into seventy disease-category indicator variables, the most common of which are described in Appendix Table 1. By definition, these variables are the same whether a person has 1 or 100 claims for a certain condition. Initially, the HCC model was blended with the demographic model, and accounted for 30, 50, 75, and 100 percent of the total risk score in, respectively, 2004, 2005, 2006, and 2007 or later. To help plans adjust to the new system, CMS increased payments across the board to MA plans after risk adjustment (we discuss the potential effects of these payments in the next section).

CMS found that within the FFS population, the HCC risk score explained eleven percent of FFS expenditures 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 expenditure variation that insurers are able to predict is between 20 and 25 percent, suggesting there is still room for risk selection even if the model performs as well on the MA population as it does on the FFS population. Similarly, reports commissioned by CMS in 2000 and 2004 (Pope *et al.*, 2000 and Pope *et al.*, 2004) and more recent work (Frogner *et al.*, 2011) have found that—again, looking only at the FFS population—the formula systematically under-predicts spending for those with the most serious health conditions.

It is worth noting that, for at least three reasons, the spending prediction from the HCC model is likely to perform worse on the MA population than on those in FFS. First, out-of-sample prediction is more difficult than in-sample prediction. Second, CMS has found that MA plans exhibit greater "coding intensity" in documenting disease conditions than do FFS providers. For example, what an FFS provider codes as "diabetes" an MA plan might code "diabetes with complications," thus increasing the enrollee's capitation payment (CMS 2010). Third, MA plans may target beneficiaries for whom the formula over-predicts costs. Indeed, as the model in the next section demonstrates, risk adjustment incentivizes insurers to enroll individuals whom they expect to have low costs *conditional on their risk score*.

3 Theoretical framework

Our model of how plans will respond to risk adjustment relies on a simple, under-appreciated fact about medical costs: as its expectation rises, so does the variance around that expectation. One might paraphrase and say that healthy people are all alike, but sick people are each sick in their own way.

Before risk adjustment, when plans were roughly getting about $8,000 per enrollee, regardless of medical history, it made little financial sense for a plan to enroll someone with a risk-score of, say, five, meaning expected costs of $40,000. Yet, as Figure 1 shows, because of the substantial variance such an individual exhibits, post risk-adjustment the margin between the capitation payment for these individuals and actual costs can be significant if plans can engage in even modest risk selection. As noted in the introduction, merely avoiding the costliest ten percent of enrollees within a risk group nets substantial margins (e.g., roughly \$7,500 on average for those with risk scores between three and four).

The idea of large potential margins among those with high risk scores is captured nicely in the below quote from Thomas Scully, the director of CMS from 2001 to 2003 and currently a general partner in a private equity firm focusing on health care:

> If you get paid \$10,000 per year for everybody [as in the pre-risk-adjustment regime], you are going to find healthy people and avoid the sick people. Well, now we have risk adjustment in Medicare...[Insurance plans] want to find a \$50,000 patient because . . . you can't make an \$8,000 margin when Medicare is paying you \$8,000. Risk adjustment has totally flipped all of the incentives in Medicare for insurance companies.[7]

The quote emphasizes that, perhaps ironically, the margin between capitation payments and medical costs (i.e., differential payments) can actually increase post-risk adjustment, now that plans can "fish" in a high-variance, high-expected-mean "pond."

3.1 Illustrating the theory with a simple example

We begin with a simple three-type example that can show all the key insights of the model, and then describe how these results are generalized in the mathematical Appendix.

Basic set-up. There are three types of individuals, one of whom is "healthy" and two of whom are "sick." Each type represents a third of the population. To fix ideas, type A is "healthy" and has no documented health conditions. He has expected costs of 5 were he to be covered directly by FFS, and there is no cost variation within members of type A: recall, healthy people are all alike. "Sick" types have cancer, and are not all alike. Type B has cancer that is in remission and has costs of 6; type C is receiving chemotherapy and has costs of 13. Table 1 displays this information. For simplicity, we assume that medical costs of treating each type is the same in FFS as for an MA plan.[8]

Before risk adjustment, capitation payments are set equal to average FFS costs across all types, or 8 ($\frac{5+6+13}{3}$). After risk adjustment, the government pays plans the average cost in each *risk category*—that is, no conditions (type A) and cancer (types B and C). For simplicity, we assume that the risk score is equal to average cost, so the risk score for those with no conditions (type A) equals 5 and the risk score for those with cancer (types B and C) equals $\frac{6+13}{2} = 9.5$. Note that risk adjustment is "payment-neutral" in the sense that, if

[7]From an April 28, 2011 conference at Columbia Business School. A full video of his remarks can be found at `http://www.youtube.com/watch?v=r_apgZpHhh8&feature=relmfu`.

[8]In practice, MA plans may affect the utilization of health care or negotiate different prices with providers.

the entire Medicare population joined MA, total capitation payments would be the same before and after risk adjustment, $3 \times 8 = 24$ and $5 + 9.5 + 9.5 = 24$, respectively.

While MA plans must accept any individual who wishes to join, we assume that a plan can—at some cost—influence the characteristics of its enrollees. These screening costs could include targeted advertising or additional benefits that appeal to certain groups. We assume that it is costlier to screen *within* a risk category than *across* categories. Applying these assumptions to our example, we set screening costs for type A individuals at just 1 while costs for type B (or C) individuals are 2. Put another way, it costs less to attract *generally* healthy individuals than to attract *relatively* healthy cancer patients. However, *the cost of screening within a risk category falls with the the category's mean cost.* In our example, the cost of screening *among* Type A individuals is infinite, as "healthy people are all alike." The cost of screening among those in the cancer risk category is positive but finite. If, instead, firms decline to influence their enrollment and merely open their door to all comers, screening costs are assumed to be zero.

Enrollment before risk adjustment. Profits are defined as capitation payment less medical and screening costs (if any). If plans do not screen, they make zero expected profits, as capitation payments and medical costs are equal in expectation. If they screen, as Table 1 shows, profits pre-risk-adjustment are 2, 0 and -7 when plans selectively enroll types A, B and C, respectively. We assume that insurers only enroll profitable individuals, so plans will choose to selectively enroll type A individuals and earn profits of 2.

Enrollment after risk adjustment. Again, if plans do not screen, they make zero expected profits because both pre- and post-risk adjustment, medical costs and capitation payments are equal in expectation. If they screen, profits are -1, 1.5 and -5.5 for selectively enrolling types A, B and C, respectively. Plans thus enroll type B and earn profits of 1.5.

Results. The first outcome of note is that the *risk scores of those enrolled in MA increase after risk adjustment*, specifically from 5 (A's risk score) in the pre-period to 9.5 (B's risk score) in the post-period. Intuitively, post risk adjustment there is no longer a penalty for enrolling individuals with high risk scores, so plans no longer expend the screening costs to avoid such individuals. We term selectively enrolling those with low risk-scores "extensive-margin screening" and our model thus predicts that it falls after risk adjustment.

The second outcome of note is that *for those enrolled in MA, medical costs conditional on the risk score fall after risk adjustment.* Specifically, in the pre-period, medical costs less the risk score were $5 - 5 = 0$, falling to $6 - 9.5 = -3.5$ in the post-period. The key to this result is that within-risk-score screening costs fall with the risk score itself. To paraphrase the quote, it is impossible to find someone with a $10,000 margin when they have a low risk score and thus, say, an $8,000 capitation payment. But, for patients with high risk scores, such a margin is possible because of the high variance. Again, in our example, the cost of screening within Type A ($risk\,score = 5$) is infinite, as no variation exists.[9] We term selectively enrolling those with costs below their risk score "intensive-margin selection."

A third outcome is that *risk adjustment would have reduced differential payments had the population joining MA remained fixed*. In our example, only Type A joins MA before risk adjustment, and risk adjustment reduces differential payments for this population to zero.

A fourth outcome from our example is that the government's differential payments (capitation payments less medical costs) actually rise under risk adjustment. In the pre-period, differential payments were $8 - 5 = 3$, rising to $9.5 - 6 = 3.5$ in the post-period.

A fifth outcome is that profits fall. Because risk adjustment changes the screening costs that insurers pay, differential payments and plan profits need not move together. In our original example, profits actually fall from 2 to 1.5 given the increased screening costs. As we do not have data on MA-specific insurer profits, we cannot directly test this result, but we return to it when we discuss welfare in Section 8.[10]

In the appendix, we go from three types to a continuum of types and allow the predictive power of risk adjustment to vary continuously as well. As we show, all five results from our three-type discrete set-up hold, with an important exception: the effect of risk adjustment on overpayments is ambiguous. The reason for the ambiguity reinforces the main theme of the paper: the success of risk adjustment depends crucially on how much medical cost variance increases with the risk score. Suppose in our example that instead of ranging from 6 to 13, the costs of those with cancer range only from 7 (type B) to 12 (type C). In the post-period, it is still the case than plans only enroll type B. Our two selection results hold: for those in MA, risk scores rise from 5 to 9.5 ("extensive-margin" selection falls) and costs less the risk score falls, in this case from $5 - 5 = 0$ to $7 - 9.5 = -2.5$ ("intensive-margin" selection increases). However, risk adjustment in this case has accomplished its goal of reducing the government's differential payments, from 3 in the pre-period to $9.5 - 7 = 2.5$ in the post-period.

[9] We make this assumption so as not to need four types to illustrate the point, but as we show in the Appendix, the result of increasing intensive-margin selection does not depend on the particular screening costs we choose here.

[10] Finally, we note that our model yields ambiguous predictions on how the average medical costs of MA enrollees should change. On the one hand, after risk adjustment MA enrollees will have higher risk scores. On the other hand, their costs conditional on their score will fall. In the Appendix, we show that the second effect can dominate and that risk adjustment can cause average costs of MA enrollees to fall. We hasten to add that this result is not general—indeed, in our example medical costs among those in MA increase from 5 to 6—but is possible.

3.2 Discussion of assumptions

First, a central though seemingly innocuous assumption of the model is *payment neutrality*, that if MA plans were to enroll all Medicare enrollees (or a random sample thereof), total payments would be the same before and after risk adjustment. If, instead, risk adjustment is accompanied by an increase in what we term "statutory" overpayments—that is, overpayment related to the government's decision to systematically overpay MA plans on average, even absent risk-selection—then our predictions need not hold. Suppose that along with risk adjustment, Medicare decided to increase all capitation payments by twenty percent, as we illustrate in Appendix Table 2. Results are unchanged in the pre-period, but now plans can either engage in screening, in which case it is most profitable to differentially enroll type B for a profit of $9.5 * 1.2 - 6 - 2 = 3.4$. Or, they can open their doors to all comers and gain $1.2 * (5 + 9.5 + 9.5) - 5 - 6 - 13 = 4.8$, as they no incur no screening costs. As such, if risk adjustment is accompanied with large increases in statutory overpayments, plans will at some point lose any incentive to find those who are cheap conditional on their risk score.

This point is empirically important because statutory overpayments have increased substantially over time. From 2001-2003, MA plans would receive 104 percent of FFS costs, absent any risk-selection, as policy-makers began to set county "benchmarks" above average county FFS spending.[11] From 2004-2006 (our post-period), these payments rose to 108 percent, both because the county "benchmarks" increased at a faster rate than did FFS spending and because CMS explicitly gave MA plans so-called "budget neutrality" payments (plans argued they would need these extra payments to compensate them for the expected revenue loss due to risk adjustment). From 2007-2009, benchmark increase and budget-neutrality payments led to statutory overpayments between 113 and 114 percent.[12] For this reason, we choose a rather short post-period, when the change in these statutory overpayments is still relatively limited. Consistent with the prediction that once statutory overpayments are sufficiently large plans will be less selective, MA enrollment between 2006 and 2010 increased by 63 percent (from 6.8 million to 11.1 million).[13]

[11] Throughout the 1980s and early 1990s, policy-makers argued that MA plans should be more efficient than FFS and thus systematically set benchmarks at 95 percent of county FFS per capita spending. The earlier work we cited suggests that plans were still able to enroll individuals who were on average less than 95 percent of average FFS costs, meaning net overpayments were still high, but during this period none of the net overpayments were due to statutory overpayments, unlike recent years.

[12] All figures come from various MedPAC reports, which annually document statutory overpayments.

[13] Indeed, while in Section 5 we document evidence of significant intensive-margin screening from 2004 to 2006, evidence from more recent years is mixed. In its 2012 annual report to Congress, MedPAC, citing a working-paper version of our study, found nearly identical levels of intensive-margin selection levels using the universe of Medicare enrollees in 2007 and 2008 (MedPAC, 2012). Results from McWilliams *et al.* (2012), however, suggest that risk-selection decreased in 2007-2008 relative to 2004-2006, consistent with higher statutory overpayments diminishing risk-selection incentives.

Second, our assumptions regarding the manner in which screening costs vary across risk categories are obviously central to the model, but we are rather silent on what, in practice, these screening costs might entail. How do plans differentially attract "over-priced" consumers? We empirically explore some possibilities in Section 7. Strictly speaking, how they do so is irrelevant to the government's bottom line, which is our main focus. Of course, it is highly relevant to consumer surplus, which we explore when we discuss welfare in Section 8.

Third, we are also rather silent on plan competition. We believe competition is likely second-order in determining the cost to the government, as MA capitation payments are set by the risk-adjustment formula and not competitive bidding. Again, however, competition likely affects how producer and consumer surplus change as a result of risk adjustment and so we explore this topic in Section 8.[14]

4 Data

Our empirical work relies chiefly on individual-level data from the Medicare Current Beneficiary Survey (MCBS) Cost and Use series from 1994 to 2006. 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 Medicare-covered provider contact for all FFS enrollees in the sample, totaling about 0.5 million claim-level observations annually. The MCBS follows a subsample of respondents for up to three or four years, thus creating a mix of cross-sectional and panel data. During our sample period, the data comprise more than 55,000 unique individuals and 150,000 person-year observations.[15]

The MCBS records whether an individual is in an MA plan or FFS each month he is in the sample. As noted in Section 2, MA plans do not submit claims or costs to CMS, and thus the MCBS only contains claims and health care cost data for those in FFS. Otherwise, all demographic and survey data are recorded for both MA and FFS enrollees. Consistent with past work, we find that, relative to their FFS counterparts, MA enrollees are more likely to live in metro areas, are less likely to be on Medicaid or Social Security Disability Insurance (SSDI) and, conditional on not being on SSDI, are younger.

Some of the key predictions from the theoretical framework in Section 3 involve enrollees'

[14]Past studies have explored consumer surplus, profits, and competition in the MA market, though all are from the pre-risk-adjustment era. Hall (2011) finds that between 1999 and 2002, annual consumer surplus surpassed $12 billion. Town and Liu (2003) estimate that between 1993 and 2000, the MA program generated over $18 billion in consumer surplus, and nearly three times that amount in insurer profits.

[15]We exclude the 0.25 percent of enrollees whose Medicare eligibility is based on having end-stage-renal disease, as different MA rules apply to them. We also exclude the roughly 2 percent of observations in which the person joins the MCBS in the middle of the year because much of their data are imputed.

risk scores, which the MCBS does not report. We obtained risk scores from 2004 to 2006 for all MCBS respondents directly from CMS. However, testing two of our key predictions also involves knowing what individuals' risk scores *would have been* in the earlier years had the HCC formula been in place, which we must generate ourselves. As described above, an individual's risk score in year t is based on diagnoses documented on claims from year $t-1$. As such, using CMS's algorithm for converting claims data into risk scores, we simulate the risk score for all MA enrollees the year immediately after they switch from FFS. As we know the actual risk scores from 2004 to 2006, we check our simulation in these years: the correlation between our simulated risk scores and CMS's actual scores is more than 0.96.

The need to calculate HCC scores in the pre-period means we limit some of our analysis to those individuals who were in FFS all twelve months of a baseline year, so that we observe their complete claims history that year. Table 2 shows the number of observations who are in FFS in year t and in MA in year $t+1$, as well as the number who are in FFS both years, and how these numbers change across our sample period. Of the more than 85,000 cases in which we observe a person in both year t and year $t+1$, more than 1,500 involve switches from FFS to MA. One limitation of the focus on FFS-to-MA switchers is that it ignores those who join MA immediately upon their Medicare eligibility. Our MCBS data demonstrate, however, that more than 3-in-4 new MA enrollees come from FFS, as opposed to joining when first enroll in Medicare.[16]

5 How did selection patterns into MA change after risk adjustment?

In this section, we empirically test our model's predictions regarding the effect of risk adjustment on both extensive-margin selection (did MA beneficiaries' risk scores rise?) and intensive-margin selection (did their costs conditional on their risk score fall?).

5.1 Quantifying the selection incentives created by the HCC model

Col. (1) of Table 3 presents the average difference between the HCC-based capitation payment and the traditional demographic-based capitation payment using our MCBS data, with this difference broken down by percentiles of the HCC risk score.[17] Mechanically, capitation

[16] In Table 2, we show that 2.1 percent of FFS recipients switch into MA the next year. With an average of 35 million in Medicare FFS during our study period, that represents 735,000 per year. During our period, about 2 million individuals become eligible for Medicare each year, and about 12 percent $(240,000)$ of them are enrolled in MA in that first year.

[17] We estimate these payments using pre-2004 data so that selection in reaction to the HCC model has not taken place. After 2003 MA plans enjoyed higher benchmarks as well as additional payments to ease the transition to risk adjustment, which we remove for the purposes of this table. As such, it reflects the change in incentives from "payment-neutral" risk adjustment as defined in Section 3.

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 about \$3,000 less than the demographic-based capitation payment for individuals with HCC scores in the lowest quartile, but would pay roughly \$7,000 more for the individuals in the top quartile.

Col. (1) suggests that insurers would have an incentive to increase risk scores over the entire risk score distribution, but col. (2), which reports actual costs minus the HCC capitation payment, shows that doing so would not always be profitable. For example, individuals with the highest one percent of risk scores represent, on average, a nearly \$6,000 loss to an MA plan, consistent with the research cited earlier showing that the HCC formula under predicts costs for enrollees with the most severe disease conditions. Plans might thus be reluctant to draw from the extreme right tail of the risk-score distribution.

Though not shown in the table, we also calculate that the share of individuals who have actual baseline costs less than their risk scores would predict is 77 percent under both the demographic and the HCC model. This result arises because of the extreme right-skew of health costs—the vast majority of the distribution falls below the mean (or conditional mean, in the case of risk adjustment). Thus, risk adjustment does not actually decrease the number of individuals who are "over-priced"—though it obviously changes their likely characteristics—and indeed we find little change in MA market share after the HCC model is introduced.[18]

5.2 Empirical strategy

Our prediction that "extensive-margin" selection should fall after the shift to risk adjustment would imply a positive estimate for β in the following difference-in-differences specification:

$$Risk\,score_{it} = \beta MA_{it} \times After\,2003_t + \gamma MA_{it} + \delta_t + \epsilon_{it}, \tag{1}$$

where i indexes the individual, t the year, $Risk\,score_{it}$ is the individual's HCC score (which, by definition, uses year $t-1$ claims data to predict Medicare expenditure in year t), MA_{it} the share of her Medicare-eligible months that the individual spends in MA in year t, $After\,2003_t$ the post-period indicator, and δ_t a vector of year fixed effects.[19] We estimate this regression on the sample of individuals who are in FFS all twelve months of the baseline year $t-1$ so

[18]This analysis does not imply 77 percent of individuals are potentially *profitable*, as there are screening costs and MA might be more or less efficient than FFS in providing the basic Medicare benefits package. Note also that MA enrollments increase substantially after our post-period, in reaction to statutory overpayments averaging over 13 percent.

[19]Both equations (1) and (2) are parsimonious in that they do not control for demographic or other characteristics of the beneficiaries. This choice is deliberate, and it reflects the fact that MA plans are paid based on the risk scores of their beneficiaries, not their risk scores conditional on, say, age.

that we can use their complete claims data that year to calculate year t risk scores.[20]

We next investigate our "intensive-margin" prediction that after risk adjustment, plans will enroll individuals who have low baseline costs conditional on their risk score. This would imply a negative estimate for β in the following specification:

$$Expenditure_{i,t-1} = \beta MA_{it} \times After\,2003_t + \gamma MA_{it} + \lambda Risk\,score_{it} + \delta_t + \epsilon_{it}, \qquad (2)$$

where $Expenditure_{i,t-1}$ is the total FFS expenditure for individual i in year $t-1$ and all other notation and sampling follows that in equation (1).

5.3 Results

We begin by exploring how the difference in average baseline Medicare spending changes after risk adjustment among those switching to MA versus those remaining in FFS, and then decompose this effect into its extensive- and intensive-margin components. Col. (1) of Table 4 shows that before risk adjustment, those switching to MA have average Medicare spending \$2,847 below those who remain in FFS, consistent with positive selection into MA. The statistically insignificant estimate of -\$173 for the $After\,2003$ interaction suggests that risk adjustment has little effect on this difference.

The next five columns of Table 4 explore the first component of the decomposition. We report the mean of the dependent variable (roughly 1.1) and Appendix Figure 2 displays a histogram. Col. (2) suggests that while individuals switching into MA before risk adjustment had average risk scores roughly 0.305 points lower than those remaining in FFS, risk scores of those switching into MA rise significantly (by .106) after risk adjustment is introduced, making up about one-third of the difference.

Based on the results from Table 3 that outliers in the right-tail are still underpriced by the HCC formula, we expect the effect on the mean to be muted, as plans would still find it unprofitable to enroll those with extreme risk scores. Indeed, in col. (3), merely dropping observations with risk scores above the 99$^{\text{th}}$ percentile increases the magnitude of the estimate. Estimating a median regression (col. 4) on the entire sample increases the coefficient by nearly one-third (to .140). While we prefer to use a long pre-period to improve precision by increasing the number of individuals in the pre-period switching from FFS to MA, col. (5) shows that excluding observations before 1997 does not change the results. Finally, in col. (6), we show the result is robust to controlling for $MA \times year$ pre-trends.[21]

[20] While we could use the actual risk scores provided by CMS for the post-period, we instead use our simulated risk scores in both the pre- and post-periods so that any change in risk scores will not be driven by differences in how they are calculated. Using actual risk scores in the post-period increases the magnitudes and statistical significance of the coefficients of interest in both the extensive- and intensive-margin analyses.

[21] As risk adjustment phases in between 2004-2006, we see an increasing trend in the post-period. As

Following Bitler *et al.* (2006), to get a clearer picture of the effects across the entire risk score distribution, we estimate quantile regressions for the first through 99$^{\text{th}}$ quantiles, and plot the resulting coefficients in Figure 2. As predicted (because of the greater variance at higher risk scores), the estimate is generally increasing with the risk score. But it falls close to zero right before the 99$^{\text{th}}$ quantile, consistent with outliers being substantially underpriced by the formula, though of course precision is more limited at the highest percentiles.

Given that average Medicare spending of those switching to MA relative to those remaining in FFS does not change after 2003 while their risk scores rise, intensive-margin selection must have increased. As expected, col. (7) shows that, relative to the pre-risk-adjustment period, after 2003 individuals switching into MA versus those remaining in FFS have baseline costs over $1,200 less than their risk scores would predict. As with the extensive-margin results, the coefficients of interest are robust to excluding years before 1997 (col. 8) and controlling for pre-trends (col. 9).[22]

In the final specification, we focus exclusively on the 2004 through 2006 period and use the actual risk scores provided to us by CMS instead of a simulated risk scores. Here the intensive margin results are even stronger and more precisely estimated, demonstrating that those joining MA after the shift to risk adjustment have significantly lower costs than their HCC risk scores would predict. This larger effect is expected, as our simulated risk-scores (though highly correlated to the official risk scores provided to us by CMS) presumably contain some error and lead to attenuation bias. Finally, note that the main effect of MA status in the intensive-margin regressions is of theoretical interest. The fact that it is close to zero suggests that, among beneficiaries switching to MA in the pre-period, the HCC risk score successfully predicts costs. This result supports the model's prediction that risk adjustment *would have* reduced selection had the population of individuals joining MA not changed in response to the policy.

5.4 Discussion and further verification

One drawback of our identification strategy is that, because we need to calculate risk-scores in the pre-period, we focus on individuals who are in FFS in a baseline year and identify our coefficients off of those who switch the next year from FFS to MA.[23] As noted earlier, CMS

such, in col. (6) we estimate pre-period trends and project them forward to the post-period. The coefficient (p-value) on the $MA \times year$ variable in the pre-period is -.0027 (0.875), suggesting essentially no pre-trends.

[22] Like the extensive-margin analysis, $MA \times year$ trends in the pre-period are essentially zero: a coefficient of -20.69 with a p-value of 0.925.

[23] In the same report that we cited in footnote 13, MedPAC also examined MA-to-FFS switchers from 2007-2008, whereas this group is too small in the MCBS for us to examine. Consistent with our model, they find that MA enrollees who switch back to FFS are *expensive for their risk score*, the parallel result to finding that FFS-to-MA switchers are *cheap for their risk score*.

provided us with actual risk scores for *all* individuals in the MCBS from 2004 to 2006. While we cannot replicate the specification in Table 4, we can determine whether—consistent with our findings for MA switchers—risk scores among all MA enrollees are growing faster than those in FFS during this period. We find that in the CMS administrative data, the average MA risk score increased by 12 percent from 2004 to 2006, versus just 1 percent for those in FFS. As such, our estimates above comparing FFS-to-MA "switchers" versus FFS "stayers" closely correspond to comparisons using the *stock* of individuals in MA versus FFS.

In Table 5, we move beyond the MCBS to verify our finding of no overall cost differences between FFS and MA enrollees after risk adjustment, as we found in col. (1) of Table 4. We gathered over 8,000,000 hospital discharge records from the twelve states between 2000 and 2006 that require hospitals to record FFS/MA status. Because these states are populous, they represent 42 percent of Medicare beneficiaries during this period. In col. (2) of Table 5, the average MA patient had \$1,500 less in charges than his FFS counterpart before 2004—consistent with positive selection in the pre-period—with only \$85 (p=.835) of this difference being made up in the post-period—consistent with our results that risk adjustment did not lead MA plans to enroll higher-cost individuals. Interestingly, when we do our best to replicate this regression with the MCBS switcher-analysis by using only annual Part A charges in 2000 to 2006, we find very similar point-estimates (col. 3).

Finally, we follow Batata (2004) and use county-level data to estimate changes in MA selection. She shows that regressing county-level changes in average FFS spending on the change in county-level MA penetration yields a measure of the difference in costs between the *marginal* person switching between MA and FFS and the FFS stock. While slightly different than our switcher regressions—which compare the *average* person switching from FFS to MA with the *average* person staying in FFS—one would expect these two selection measures to move in the same direction. As the final column of Table 5 shows, this difference is negative before risk adjustment, reflecting the fact that those on the margin of switching between MA and FFS have lower costs than those in FFS. Consistent with the switcher analysis and the rest of the table, selection along this margin does not change after risk adjustment (in fact, the point estimate suggests increased selection), though our precision here is somewhat limited.

In summary, our evidence from several datasets indicates that, with respect to actual health costs, those in MA are as positively selected after risk adjustment as before.

6 Did risk adjustment decrease differential selection?

One might assess risk adjustment by estimating how an individual's annual *Total Medicare expenditure* changes when he switches from FFS to MA, and then compare this change before and after risk adjustment. *Total Medicare expenditure* is the total annual cost to Medicare for covering an individual, whether from claims (for FFS enrollees) or capitation payments (for MA enrollees). Under perfect risk adjustment (i.e., capitation payments equal to an individual's expected FFS costs) whether an enrollee switches between FFS and MA should in expectation have no effect on this variable.

This approach has important limitations. First, comparing the government's costs as individuals switch between MA and FFS obviously requires focusing only on "switchers," and thus only a subsample of the data. Yet our model tells us that risk adjustment will decrease overpayments on the *stock* of those who joined MA before risk adjustment, while having ambiguous effects on those who join after, meaning that looking only at post-period "switchers" could mask the ability of risk adjustment to decrease overpayments. On the other hand, using risk scores only from those who were in FFS the previous year ignores "intensive coding" (as risk scores that first year are based on FFS claims), a serious drawback of risk adjustment that a "switcher" analysis cannot measure.

Second, after 2003, the MCBS does not provide individual capitation payments. In principle, one can recreate them by multiplying the simulated HCC risk scores by county-level benchmarks. However, recall that county benchmarks grow more rapidly post risk adjustment and plans also receive additional "budget-neutrality" adjustments to ease the transition to risk adjustment. As such, one must take a stand on the counterfactual evolution of benchmark payments to isolate the effects of risk adjustment from these coincident payment increases.

In Appendix B, we make assumptions about how each of these missing pieces affects our calculation and conclude that overpayments do not fall post-risk adjustment. We in fact find a small positive effect—a significant increase in overpayments among those who enroll in MA post-2003 of between \$1,500 and \$2,000, somewhat but not fully offset by a \$700 decrease in overpayments among the still larger population MA "incumbents" who had first joined before 2004–though given the above concerns large error bands must be assumed. Note also that this blended effect should become more positive over time as the incumbents comprise a shrinking share of the MA population given its substantial flux.

In the rest of the section, we focus on a specification that allows us to examine the change in differential selection for *all* MA and FFS enrollees—not just switchers—before and after risk adjustment, in a manner independent of changes in underlying benchmarks. Specifically, we use mortality as a proxy for costs—which, unlike costs, is both recorded for everyone in the MCBS and independent of how the government changes benchmarks—and regress it on MA status and the risk score. A negative coefficient on the MA variable indicates that, even conditional on the risk score, MA enrollees are positively selected. We compare whether this selection conditional-on-the-risk-score is greater in the pre-period (using the demographic risk score) or the post-period (using the HCC risk score).[24]

Below, we show that our demographic and HCC scores predict the correct amount of variance in the FFS data; that mortality is indeed an excellent proxy for costs in FFS data; and then proceed to the main empirical test.

6.1 Empirical results

Initial steps. As noted in Section 2, the demographic risk score was shown by CMS to account for one percent of FFS cost variance. Col. (1) of Appendix Table 4 shows that the demographic risk scores we calculate account for 1.23 percent of cost variance, using pre-period FFS data. Similarly, CMS calculated that the HCC score accounts for 11 percent of pre-period FFS cost variance. Indeed, in col. (2) our R^2 value using MCBS FFS data is also 11.0 percent. Col. (3) regresses annual costs on whether an MCBS respondent died in a given year. The R^2 value is 15.1 percent, showing that death is in fact a stronger predictor of FFS costs than the HCC risk score itself.

Comparing selection before and after risk adjustment. The first three columns of Table 6 show that—unconditional on the risk score—those in MA are very positively selected with respect to mortality. They are 1.4 percentage points (more than 25 percent) less likely to die in a given year, relative to their FFS counterparts, which holds relatively constant before and after risk adjustment (cols. 2 and 3). The nearly identical mortality advantage of those in MA pre- and post-risk adjustment adds to the evidence shown in Table 5 that the differences in overall, unconditional health status between MA and FFS enrollees do not change after risk adjustment.

To test risk adjustment, however, we compare *conditional* differences. In the pre-period, the demographic risk score was more effective in reducing this positive selection—the MA coefficient, while still negative and significant, falls in magnitude by roughly 75 percent in col. (4) relative to col. (2). In the post-period, conditioning on the HCC risk score in col. (5) only modestly reduces the positive selection into MA—the coefficient on the MA variable falls by less than thirty percent and remains highly significant.

In col. (6), we combine the regressions in cols. (4) and (5) so that we can more easily

[24]From CMS, we have HCC risk scores for everyone (both switchers and stayers) in the post-period, and the demographic risk scores can be easily calculated with the background information collected by the MCBS and does not require claims data.

compare the MA coefficients in the pre- and post-periods. Indeed, the coefficient on MA in the post-period is significantly "more negative" than that in the pre-period, suggesting that *conditional on the risk score that MA plans faced at the time*, being in MA indicates a lower conditional probability of dying and thus more positive selection. The final columns shows that this relationship holds when we compare the post-period to a shorter pre-period.

6.2 Discussion

We can translate these effects into overpayments by using the estimated effect of mortality on total annual Medicare costs. This exercise yields an increase in overpayments of \$317. An advantage of the mortality analysis is that it provides an estimate of differential payments independent of the extra statutory overpayments MA plans received, but as a policy matter, MA plans did indeed enjoy higher statutory overpayments in the post-period. Including them yields total overpayment increases between \$736 and \$988, the smaller estimate equal to roughly nine percent of average per capita FFS annual spending.[25]

7 How does selection into MA plans take place?

7.1 Why are low-cost individuals more likely to be in MA plans?

The evidence in Section 5 shows that MA plans enrolled lower-cost individuals both before and after risk adjustment. But how do such patterns emerge when plans must offer the same plans at the same rate to all Medicare beneficiaries in their geographical area of operation? We first explore whether among all MA enrollees, the healthy ones are more satisfied with their care and less likely to return to FFS. This pattern might arise because plans actively treat healthy enrollees better than sick ones so as to differentially retain the former group, or simply because sick individuals do not like the HMO model of care. Through reputation effects, such a result could feed back into patterns of switching into MA as well. This latter pattern could also be driven by targeted advising, with previous studies finding that advertisements for MA plans target healthy people (Mehrotra et al, 2006; KFF, 2008).

[25]The monthly increase in costs the year an individual dies is \$4308 (App. Table 4, col. 3). The increase in conditional selection with respect to mortality after risk adjustment is 0.00613 (Table 6, col. 6), suggesting an increase in overpayments of $\$4308 * 12 * 0.00613 = \$317$ (assuming the relationship between costs and mortality is the same for MA and FFS). The increase in statutory overpayments depends on which pre-period to use as a counter-factual. As noted in Section 3, in the three years before the reform, statutory overpayments averaged 103 percent of FFS costs, rising to 108 percent in our post-period. Taking the entire pre-period, where in the early years capitation payments were set to equal 95 percent of per capita FFS costs, we can estimate that capitation payments were roughly 100 percent of FFS costs. Given that per capita FFS costs were \$8385 in 2004, the statutory overpayment increases are between $0.05 * \$8385 = \$419$ and $0.08 * \$8385 = \$671$, so, adding the increase in overpayments from differential selection alone gives a total overpayment increase between \$736 and \$988.

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 current year or in the previous calendar year. As such and in contrast to our previous analyses, here we focus on those who did not switch (either from FFS to MA or from MA to FFS) the previous year by comparing individuals in MA in both years with those in FFS in both years.

Ideally, we would explore whether satisfaction is higher among MA recipients who are more profitable to insurers. But because we do not have health care cost or claims data for those in MA, we use self-reported health as an admittedly imperfect proxy and investigate whether good health predicts satisfaction with one's health care in MA more than in FFS:

$$Satisfaction_{it} = \beta MA_{it} \times Health_{it} + \gamma MA_{it} + \mathbf{H}_{it} + \lambda X_i + \delta_t + \epsilon_{it}, \tag{3}$$

In this specification, $Satisfaction$ measures individuals' reported satisfaction with different aspects of their health care and varies from one (very dissatisfied) to four (very satisfied), $Health$ is a five-category self-reported health variable, $\mathbf{H}$ 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 correlates with negative feelings toward one's health care. Thus the interaction term explores how much more or less sensitive enrollee satisfaction is to underlying health in MA versus FFS. We control for demographic characteristics in X because different groups may assess their health and health care differently. If MA plans treat healthier enrollees better, we would expect $\beta > 0$.

Table 7 displays the results from estimating equation (3) via OLS. We demean the $Health$ variable in $MA \times Health$, so that the MA main effect represents the association with MA enrollment for someone with mean self-reported health. 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 lower satisfaction in MA than in FFS. This estimate is surprising given that MA enrollees self-selected into MA and given the large overpayments to MA plans, which could lead to additional benefits. But then again MA enrollees may simply be harder to please.

We instead focus on the coefficient 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, only among those who report being in "excellent" health do MA plans receive a higher rating than FFS (not shown). Moreover, 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, and for a majority of categories this difference is statistically significant.

The last row of Table 7 investigates whether sicker MA enrollees "vote with their feet" and exit at higher rates than do sicker enrollees in FFS. Instead of satisfaction ratings, we regress whether an individual changes his coverage status—to MA if he is currently in FFS, to FFS if he is in MA—on the same set of explanatory variables.[26] Indeed, the same pattern emerges—not only are MA enrollees less likely to retain their current coverage status in general, but this difference is especially pronounced for those in self-reported poor health. As such, among MA patients, the sickest are the most likely to return to FFS each year.

7.2 Possible mechanisms underlying the changes after risk adjustment

The results in Table 7 provide an explanation for why higher-cost enrollees tend to be in FFS, but they do not explain how individuals with low costs relative to their risk score found their way into MA plans after risk adjustment. While we cannot provide a definitive answer given the available data, we believe several factors are at work. First, insurers have a wealth of data, both on their own MA enrollees and from their operations in the non-Medicare market. In fact, because insurers (unlike CMS) have data on *the medical claims and costs of MA beneficiaries*, along this dimension plans have more information than the government.

Second, CMS does not adjust for factors such as race, ethnicity, and income, which are not only related to health costs but, through targeted advertising, are also relatively easy for MA plans to select on. Given that the variance in costs grows with the risk score, demographic differences that are small on average could be very large for groups with high risk scores or for a specific disease category. Plans have the data to determine that, for example, Hispanics with heart disease are \$3,500 cheaper than their risk score would suggest and then target advertising accordingly. If demographic or other observable factors explain how costs vary from the risk score's prediction within a disease category, then plans may have the ability to differentially enroll people who are low cost for their risk score.[27]

We explore empirically whether after risk adjustment MA plans appear to engage in selection along profitable $Disease \times Demographic\ Group$ margins. We start with a specific example. Consider HCC10, "Breast, Prostate, and Colorectal Cancer," which, unlike the other major categories (the ten most common of which are listed in Appendix Table 1) combines multiple diseases. Given the prevalence rates of these diseases, the large majority of women (men) in this category will have breast (prostate) cancer. Moreover, past medical research (Yabroff *et al.*, 2008) has shown the annual cost of breast cancer treatment to be

[26]For this analysis, we expand our MCBS sample to include individuals who switch from FFS to MA (or vice-versa) in each year.

[27]See recent work by Kim and Aizawa (2013) using advertising spending by MA plans to document that such spending is higher in localities where the cost differences between the sick and healthy are largest and thus the benefits of risk-selection are greatest.

roughly \$1,900 cheaper than that of prostate cancer even after accounting for demographic differences between the two groups.[28] Therefore, we have a common disease group in which we can identify a large subgroup of individuals (women) who were under-priced before risk adjustment and are over-priced after. While all individuals in Category 10 were less likely than other Medicare beneficiaries to join MA in the pre-period, women—but not men—in this category are more likely to join after risk adjustment ($p < .02$).

Next, we explore whether, more generally, MA enrollment in the post-period appears to correspond to $Disease\,Category \times Gender$ "errors" in the risk-adjustment formula. Because of power constraints, we consider only the largest ten disease categories and use gender because other categories create unequal splits of disease groups and thus very small cells. While HCC10 provides an especially nice example given that two diseases are combined, it is also the case that men and women appear to differ systematically in the costs they incur for the other nine disease groups. For each of the twenty cells, we estimate the "error" in the risk-adjustment formula using pre-period FFS data and pre-period benchmarks.[29] As Appendix Figure 3.A shows, while the errors are roughly centered on zero, there exists substantial variance. On the y-axis, we plot the probability an individual in each cell switches from FFS to MA in the post-period. Indeed, there is a positive correlation between overpayment errors and the post-period FFS-to-MA transition probability. Appendix Figure 3.B shows that, by contrast, there is no such correlation between the payment errors and pre-period transition probability—we would expect none, since risk adjustment did not exist in this period and thus risk-adjustment errors would be meaningless to plans.

These results provide additional evidence that insurers responded to the policy-induced change in financial incentives by enrolling those Medicare recipients whose profitability increased most after the shift to risk adjustment.

8 Welfare and Policy Implications

While we have provided evidence suggesting that risk adjustment did not accomplish its goal of reducing the government's overpayments, a full welfare analysis would include the effects on producer and consumer surplus. While the evidence presented in this section is hardly definitive, it begins to shed light on the policy's wider implications.

[28] In our data, the gender difference in costs among those with HCC 10 is roughly \$1,200, likely because of attenuation bias from colorectal cancer.

[29] To capture as closely as possible the errors MA plans would encounter, we use data from 1997 to 2002, as the risk-adjustment formula was calibrated using 1999-2000 FFS data and we add two years on either side to gain precision.

8.1 The effect of risk adjustment on plan profits

We do not have access to actual MA-specific profit data from insurers, a limitation that appears to be shared by all papers in the MA literature. However, our model does speak directly to plan profits—after risk adjustment, profits fall (even if overpayments rise) because screening costs increase. Note also this prediction refers to "payment-neutral" risk adjustment—which, recall, was our term for a risk-adjustment model in which, essentially, the government is not trying to systematically under- or overpay plans on average.

While we do not have actual profit data to verify the model's prediction, evidence from plans' reactions to risk adjustment suggest they believed payment-neutral risk adjustment would hurt their profits. The implementation of the mild precursor to the HCC model (which, recall, explained 1.5 percent of cost variation) was explicitly payment-neutral. Many private insurers formally called on CMS—which tacitly agreed the reform would hurt plans—to delay the implementation of risk adjustment or to provide extra "budget-neutrality" payments to compensate for risk adjustment.[30] Indeed, perhaps to avoid a similar backlash, as we explain in Sections 2 and 3, CMS increased the risk-adjusted capitation payments by roughly ten percent as the HCC risk-adjustment formula was phased in. Thus, when risk adjustment is not augmented with additional payments, both CMS and the private insurers expected profits to fall, consistent with our model's predictions.

In our model, risk adjustment can increase government spending while decreasing plan profits because it increases insurers' screening costs. The model assumes that strategies that allow insurers to find, for example, the cheapest diabetics (an optimal strategy after risk adjustment) are costlier on a per-enrollee basis than avoiding diabetics altogether (optimal before risk adjustment). This extra money could be spent in ways that increase consumer welfare, like improving the quality of medical care, or in ways that likely do not, such as engaging in targeted advertising or devising complicated screening strategies. Ultimately, the extent to which this extra spending benefits consumers is an empirical question.

8.2 The effect of risk adjustment on consumer welfare

In this subsection, we explore whether risk adjustment improved consumer welfare along a variety of observable dimensions. To conserve space we only briefly describe the data and estimating equations and refer readers to the Appendix for greater detail. It is worth emphasizing here that, through the accelerated growth of county benchmarks and "budget neutrality" payments, MA plans received increases in capitation payments in the post-period

[30] See `http://www.cms.gov/MedicareAdvtgSpecRateStats/Downloads/Announcement2000.pdf` This same document shows that the PIP-DCG model was meant to be explicitly payment neutral.

beyond those that we attribute to their endogenous reaction to risk adjustment. As such, if some of these overpayments were passed on to MA enrollees after 2003, our estimates serve as an upper bound for benefits to consumers from risk adjustment alone.

Comparing MA v. FFS before and after risk adjustment. We begin by examining the simple question of whether MA enrollees report higher satisfaction after risk adjustment. We regress each of our satisfaction measures on $MA \times After$ 2003, its lower-order terms and the controls in Table 7. As shown in Appendix Table 5, the coefficient on $MA \times After$ 2003 is negative for five and positive for four, though rarely statistically significant, suggesting that post-risk-adjustment, MA enrollees do not report relative increases in their satisfaction.

We probe further on these results by controlling for differential trends among MA recipients prior to the shift to risk adjustment (panel B). In contrast to our previous results for extensive and intensive margin selection, which were unaffected by the inclusion of these trends, here the point estimates generally increase and suggest that MA recipients experienced an increase in satisfaction following the shift to risk adjustment. The increase in the estimated impacts is driven by a negative trend in satisfaction with MA prior to 2004, which is somewhat arrested after 2003. However, in the post-period, MA recipients still report lower satisfaction in eight of the nine categories the MCBS collects (not shown). As such, while there might be some improvement relative to trend, it is not large enough to close the MA-FFS satisfaction gap despite the increase in payments to MA plans in the post-period.

Nor did the positive $MA \times Health$ satisfaction gradient documented in Table 7 shift post-risk-adjustment, as one would have predicted had MA plans began to cater to those in worse health. In Appendix Table 6 we interact $MA \times Health$ with a post-risk-adjustment indicator variable, and the only significant effect is a further positive tilt in one category and no statistically significant effect on the others.

Did risk adjustment improve the Medicare program more broadly? To fully assess risk adjustment we need to consider its effects on the entire Medicare program. Suppose, for example, that risk adjustment allowed for better sorting of beneficiaries between MA and FFS, and thus everyone was better off. The above analysis would obscure such an effect by not considering the benefits to FFS enrollees as well. Moreover, because the types of people switching from FFS to MA change after 2003 (they have higher risk scores, e.g.), the analysis above—comparing MA versus FFS enrollees after 2003—could be contaminated by compositional changes. We thus perform a number of analyses not subject to these critiques.

As envisioned by policy-makers, risk adjustment would increase insurers' incentives to expand health care options for those in poor health. As such, one would expect those in poor health to be relatively more satisfied with their health care options post-risk adjustment. We thus estimate our various satisfaction measures as a function of $Health \times After$ 2003, its lower-order terms, and the controls included in Table 7. As shown in Appendix Table 7, the coefficient on the interaction term is positive for all nine categories and more often than not significant, meaning that after risk adjustment those in poor health are relatively *less* satisfied with their health care.

This result is central to the welfare effects of risk adjustment because the gradient of satisfaction with respect to health status speaks to the insurance value of Medicare. Put differently, a system in which healthy people receive the highest quality health care would not seem to allocate resources from the "good" to the "bad" state, as consumption-smoothing requires. The positive coefficients on $Health \times After$ suggest this allocation of resources between the good and bad states was potentially made worse after risk adjustment.

We next turn to the National Health Interview Survey to explore whether measures of Medicare enrollees' health care quality improved after risk adjustment relative to similar individuals outside the program. Using data from 2000 to 2006, we compare the "young elderly" (65-74 year-olds) to the "near elderly" (55-64 year-olds). We examine all variables in the dataset related to patient satisfaction and preventive care. Appendix Table 8 shows that none of these measures improve for the young-elderly relative to the near-elderly, as one would have expected if risk adjustment improved care generally for Medicare enrollees.

Finally, we investigate whether MA-intensive counties saw mortality improvements for the young-elderly relative to the near-elderly after 2003. All else equal, in counties with greater MA penetration initially, risk adjustment represents a larger intervention to the Medicare program. We obtained county-level data on mortality by age from the National Center for Health Statistics for 2000-2006. As Appendix Table 9 shows, we find no evidence of differential mortality improvements among the young elderly in counties most affected by risk adjustment—the coefficients vary in sign but are essentially zero. Nor do changes in MA penetration after risk adjustment predict greater mortality gains for the young-elderly relatively to the near-elderly than they did before risk adjustment. We also examine "long differences" to allow the effect a few years to materialize, and if anything, after 2003, increases in MA penetration are associated with a small increase in mortality for the young-elderly relative to the near-elderly, though these results are not quite statistically significant.

In summary, the results from this and the previous subsection suggest little improvement in the care of those in MA versus FFS, nor improvements in the care of Medicare enrollees more generally after risk adjustment.[31]

[31] Our result adds to recent work showing that increases in plan reimbursement have not led to substantial increases in consumer surplus. Cabral *et al.* (2013) find that about 12 cents for every dollar of MA overpayment increase is passed on to consumers, while Duggan *et al.* (2013) find that less than a third are passed on.

8.3 Improving risk adjustment

Our results suggest at least two concerns about future attempts to improve risk adjustment. The first relates to the methodology for recalibration of the risk-adjustment formula, which, essentially, just reruns the cost regressions on the HCC dummies using more recent FFS data. Our results in Table 4 on intensive-margin selection indicate that, after risk adjustment, the lowest-cost cases of each disease category become more likely to exit FFS and join MA. As such, using more recent FFS data will create even greater positive bias for a disease category, as the FFS enrollees with the disease are more adversely selected. Put differently, if a disease condition was over-priced initially, recalibration will *exacerbate* the mis-pricing.

The second note of caution is a more general reminder that we believe bears further emphasis: the R^2 of a risk-adjustment formula is not a sufficient statistic for its welfare effects (or even its effect on government payments). As noted, the model shows that the effect of an increase in R^2 on differential payments is indeed ambiguous. We thus add to the argument first made by Glazer and McGuire (2000) that the risk adjustor needs to consider not a formula's "predictiveness," but also the incentives it creates.

Our results do suggest some potential improvements. First, our framework can help predict which disease categories might be especially vulnerable to selection. The results in Section 7 suggest that disease categories that have greater predictable variance along dimensions plans could easily observe and target (e.g., gender, ethnicity) will be especially problematic. The risk adjustor may wish to pay special attention to selection in these categories and could consider, for example, conditioning on the variables that empirically predict an individual's cost. In particular, our results suggest including interactions between demographics and certain diseases could be beneficial.

Alternatively, instead of trying to improve the predictiveness of the formula using FFS data, CMS may wish to incorporate the information embodied in MA enrollment patterns into the formula. If a certain disease group begins to "differentially disappear" from FFS and "re-appear" in MA after risk adjustment, then it is a signal that low-cost individuals in that disease group are, for whatever reason, easy to "skim," and thus CMS could reduce the capitation payments for that disease category. Note that such an approach would have the exact opposite effect of the current recalibration procedure, which leads to payment increases for disease groups that experience differential migration to MA.

9 Conclusion

We developed a simple model for understanding how risk selection would respond to an attempt to decrease differential payments to MA plans via risk adjustment. We predicted that MA plans would enroll more Medicare recipients with conditions included in the formula ("extensive-margin" selection falls), but would increase efforts to enroll those with low costs conditional on the risk score ("intensive-margin" selection rises). Using individual-level data on Medicare expenditures and comparing the selection patterns for those switching to MA with those remaining in FFS, we confirmed both predictions. Our framework also shows that because the variance of medical costs increases with the risk score, risk adjustment can increase the scope for selecting individuals with costs below their capitation payment. Indeed, we find that changes in selection led to a small increase in overpayments after risk adjustment and, combined with increases in across-the-board average payments to MA plans, actual overpayments to MA plans increase meaningfully after risk adjustment. We find little evidence that these overpayment increases improved enrollees' quality of care or satisfaction.

The MA program and Medicare more generally have recently been the target of regulation and reform, and as data become available, future work might examine how MA selection and differential payments change in response. The Affordable Care Act (ACA) directly affects the MA program by lowering many county benchmarks while at the same time linking capitation payments to measures of plan quality.

The ACA also requires risk adjustment in the federal and state-run insurance exchanges. While Medicare has access to claims and cost data for its FFS recipients to calibrate a risk adjustment model, no such "public option" will exist in the exchanges, and thus there may be insufficient data to estimate a model.[32] The lack of a public option is important for another reason. Recall from Section 7 that both before and after risk adjustment, MA enrollees in poor health express greater dissatisfaction with their care than do their counterparts in FFS, and differentially migrate back to FFS. Plans in the insurance exchanges would seem to face similar incentives not to retain under-priced enrollees and thus might devote limited resources to their care. But these enrollees would not have a public, FFS-like plan to which to return. Thus, while the cost of imperfect pricing in the MA context is primarily borne by taxpayers via higher Medicare spending, we speculate that the under-priced enrollees themselves may bear more of this cost in the exchanges or other settings without a public option.

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[32] Currently, risk-adjustment formulae are calibrated using Marketscan data, but whether this sample mirrors the costs of the exchange population is unknown.

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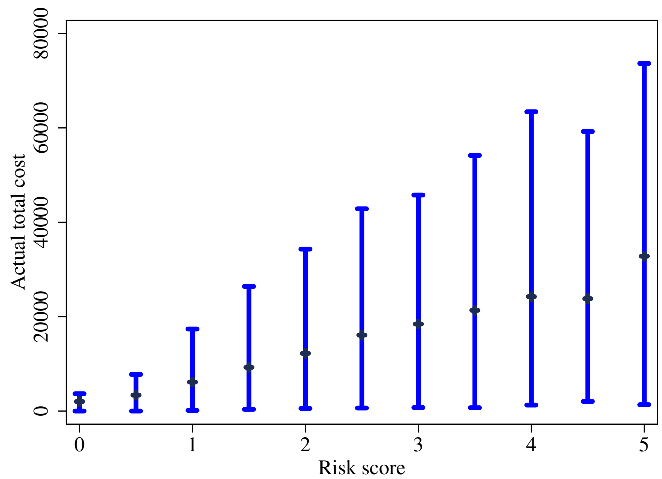
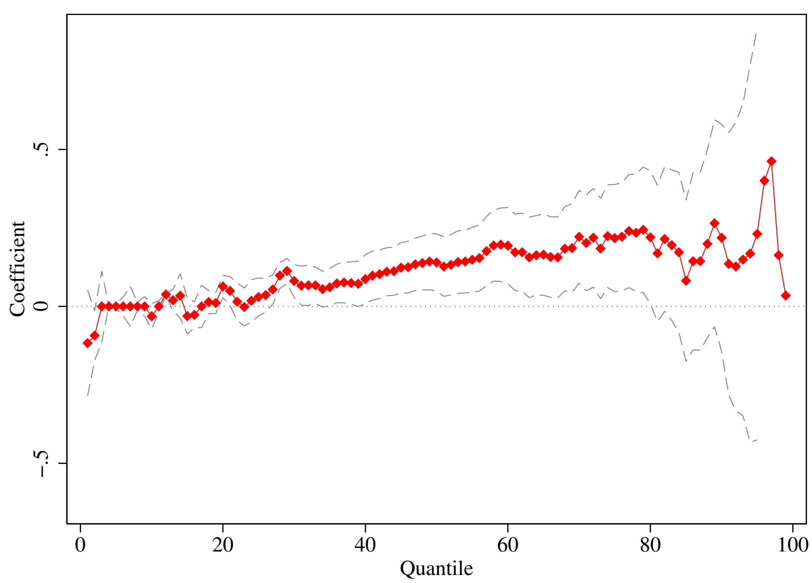
Figure 1: Means, 10^{th} and 90^{th} percentiles of total costs, by risk score



Notes: All observations spent all twelve months of the previous year in FFS (so that current-year risk scores can be calculate) and no months of the current year in MA (so that all current cost data can be observed). Observations are taken only from the pre-period so that the sample is unlikely to be selected with respect to the risk score. Sample weights provided by the MCBS are used.

Figure 2: Coefficients and 90-percent confidence intervals from quantile "extensive-margin" regressions



Notes: Each point is the coefficient β from a quantile regression of the form $Risk\,score_{it} = \beta MA_i \times After_t + \gamma MA_i + \delta_t + \epsilon_{it}$. See Section 5 for further detail. Note that the confidence-intervals for quantiles greater than the 96^{th} are suppressed so as not to compress the scale of the figure.

Table 1: Example illustrating model predictions

	No Conditions	Has Cancer	
		In remission	In treatment
Model Fundamentals			
True costs	5	6	13
Screening	1	2	2
Risk score	5	9.5	9.5
Residual (cost - r. score)	0	-3.5	3.5
Not Risk Adjusted			
Capitation Payments	8	8	8
Differential Payments	3	2	-5
Profits	2	0	-7
Risk Adjusted			
Capitation Payments	5	9.5	9.5
Differential Payments	0	3.5	-3.5
Profits	-1	1.5	-5.5

Notes: Boxes indicate the type of enrollee that will join MA under each regime.

Table 2: 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) → FFS (year t+1)	19,017	18,539	18,305	17,329	73,190
FFS (year t) → MA (year t+1)	566	399	102	464	1,531
MA (year t) → FFS (year$t$+1)	102	165	457	125	849
MA (year t) → 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: An individual in a given year is classified as being on MA if she is on MA for at least half of the months for which she is Medicare eligible in that given year.

Table 3: Summarizing changes in incentives after risk adjustment

HCC score	HCC payment minus demographic payment	HCC payment minus actual Medicare expenditure
0-25th percentile (lowest scores)	-2,993	67
25-50th percentile	-2,406	198
50-75th percentile	-342	549
75-99th percentile	6,701	893
99-100th percentile (highest scores)	29,789	-5,907
Total	491	359
Observations	54,369	54,369

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. Dollar amounts are adjusted to 2007 dollars using the CPI-U. Sample weights provided by the MCBS are used.

Table 4: Changes in selection patterns after risk adjustment

		Extensive-margin					Intensive-margin			
	(1) Cost	(2) Score	(3) Score	(4) Score	(5) Score	(6) Score	(7) Cost	(8) Cost	(9) Cost	(10) Cost
Share of year in MA	-2847.0***	-0.305***	-0.241***	-0.233***	-0.236***	-0.250***	171.5	191.6	106.6	
	[396.9]	[0.0355]	[0.0350]	[0.000161]	[0.0510]	[0.0350]	[316.5]	[467.2]	[316.5]	
Share of year in MA x After 2003	-172.7	0.106*	0.133**	0.140***	0.129*	0.155**	-1217.9**	-1280.3*	-1052.4*	-1656.9***
	[713.4]	[0.0614]	[0.0604]	[0.000413]	[0.0709]	[0.0604]	[604.0]	[691.8]	[604.0]	[516.1]
HCC score (calculated from claims)							9903.4***	9691.0***	9903.4***	
							[182.5]	[202.2]	[182.5]	
HCC score from CMS										10653.2***
										[452.3]
Mean, dept. var.	6315.6	1.149	1.091	1.149	1.115	1.091	6315.6	6482.0	6315.6	7372.1
Estim. method	OLS	OLS	OLS	Q. reg.	OLS	OLS	OLS	OLS	OLS	OLS
Outliers trimmed	No	No	Yes	No	Yes	Yes	No	No	No	No
1998-2006 only	No	No	No	No	Yes	No	No	Yes	No	No
Trend control	No	No	No	No	No	Yes	No	No	Yes	No
Pre- & post-periods?	Both	Both	Both	Both	Both	Both	Both	Both	Both	Post
Observations	73,054	73,054	72,274	73,054	54,029	72,274	73,054	54,646	73,054	17,680

Notes: All observations are in FFS all twelve months of the given year. Year fixed effects included in all regressions. The outcome in cols. (1), and (7) through (10) is an individual's current year total Medicare expenditure. The outcome in cols. (2) through (6) is an individual's HCC score the following year, which is based on current-year claims. "Q. reg" refer to median regressions. "Outliers trimmed" excludes individuals with risk scores above the 99^{th} percentile (where percentiles are calculated separately by year). "Trend controls" adjust for $MA \times year$ based on pre-period trends. In all columns except the last, we calculate HCC scores from the MCBS claims data; the final column uses the HCC score provided directly from CMS (unavailable for the pre-period). Sample weights provided by the MCBS are used. Dollar amounts are adjusted to 2007 dollars using the CPI-U. Standard errors are clustered by the individual. $^{*}p < 0.1,^{**} p < 0.05,^{***} p < 0.01$

Table 5: Changes in total baseline Medicare expenditure for MA versus FFS enrollees after risk adjustment

	(1) Costs	(2) Charges	(3) Part A	(4) Δln(FFS cost)
Share of year in MA	-2847.0***		-1279.5	
	[396.9]		[882.3]	
Share of year in MA x After 2003	-172.7		-235.9	
	[713.4]		[997.5]	
In MA at discharge		-1497.5***		
		[278.4]		
In MA at discharge x After 2003		85.50		
		[409.2]		
Δln(FFS share)				-711.9**
				[329.0]
Δln(FFS share) x After				-86.06
				[697.0]
Mean, dept. var.	6315.6	25559.6	3489.3	298.4
Data set used	MCBS	Hosp.	MCBS	County FFS
Years	1995-2006	2000-2006	2000-2006	2000-2006
Observations	73054	8217647	42051	18658

Notes: Col. (1) replicates col. (1) of Table 4. Col. (2) uses hospital discharge data and estimates total charges as a function of MA status, hospital and year fixed effects. Col. (3) replicates col. (1) of Table 4 but uses only part A costs and years from 2000-2006, to be more comparable to the hospital discharge analysis. Col. (4) uses county-level data and replicates the analysis in Batata (2004). She shows that in the following regression $\Delta ln(Avg.\ FFS\ costs)_{ct} = \alpha + \beta \Delta FFS\ share_{ct} + \epsilon_{ct}$, where $Avg.\ FFS\ costs_{ct}$ is the average per capita costs for all FFS enrollees in county c in year t and $FFS\ share_{ct}$ is the share of county c's Medicare enrollees in FFS in year t, β is an estimate for the difference between the costs of the *marginal* enrollee switching between MA and FFS and the *average* FFS enrollee. All regressions are estimated via OLS and for regressions using MCBS data the provided weights are used. $^{*}p < 0.1, ^{**}p < 0.05, ^{***}p < 0.01$

Table 6: Changes in differential selection before and after risk adjustment

	Dept. Variable: Died within the calendar year (x100)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Share of months in MA	-1.414*** [0.158]	-1.440*** [0.182]	-1.345*** [0.319]	-0.310* [0.181]	-0.923*** [0.312]	-0.310* [0.181]	-0.253 [0.231]
Share of months in MA x After 2003						-0.613* [0.361]	-0.669* [0.389]
Demographic score				4.675*** [0.0906]			
HCC score					5.563*** [0.226]		
Demogr. score x Pre-period						4.675*** [0.0906]	4.659*** [0.123]
HCC score x Post-period						5.563*** [0.226]	5.563*** [0.226]
Mean, dept. var.	0.0497	0.0495	0.0502	0.0495	0.0502	0.0497	0.0495
Period	Both	Pre	Post	Pre	Post	Both	Both
Short pre-period	No	No	No	No	No	No	Yes
Observations	137769	105364	32405	105364	32405	137769	91802

Notes: Observations are no longer restricted to being in FFS the previous year. The demographic risk score is calculated for all MCBS observations using the formula provided by CMS. The HCC risk score is provided directly by CMS for all MCBS respondents after 2003. Year effects included in all regressions. Sample weights provided by the MCBS are used. Standard errors are clustered by the individual. $^{*}p < 0.1,^{**} p < 0.05,^{***} p < 0.01$

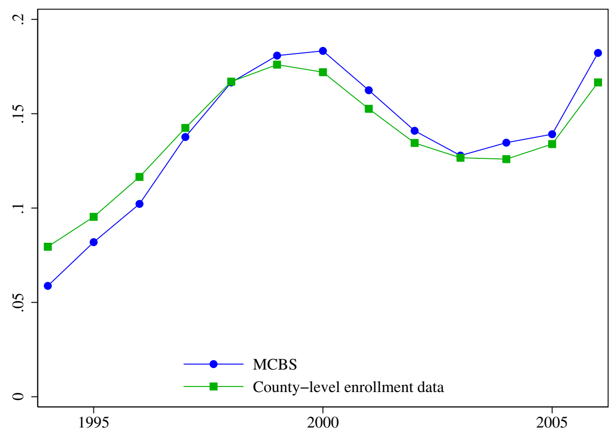
Table 7: Effect of MA enrollment and health status on enrollee satisfaction

Dependent var:			Coeffs. (SEs)	
Satisfaction rating (1-4)	Mean	Obs.	In MA	MA x Health
Overall medical care	3.26	75,884	-0.0168**	0.0158**
			(0.00727)	(0.00659)
Out-of-pocket costs	3.01	75,309	0.0648***	0.0238***
			(0.00848)	(0.00742)
Follow-up care	3.16	69,764	0.00505	0.00721
			(0.00657)	(0.00604)
Doctor's concern for your health	3.15	74,711	-0.0126*	0.0163***
			(0.00697)	(0.00633)
Information about your medical condition	3.12	75,539	-0.00114	0.0137**
			(0.00658)	(0.00592)
Access to specialists	3.17	57,187	-0.0178**	0.00142
			(0.00721)	(0.00653)
Questions answered over phone	3.06	48,616	-0.0167*	0.0198**
			(0.00868)	(0.00778)
Availability of care nights and weekends	3.11	44,502	0.0115	0.00955
			(0.00865)	(0.00803)
Medicare care provided in same location	3.10	69,380	0.0403***	0.00973*
			(0.00646)	(0.00580)
Retains coverage type next year	0.97	84,160	-0.0578***	0.00901***
			(0.00315)	(0.00283)

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," "dissatisfied," "satisfied," "very satisfied"), MA is a dummy variable for being enrolled in Medicare Advantage at least half of all Medicare-eligible months in a give year, $Health$ 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 (one, "poor," up to five, "excellent"), 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. Note that the sample size varies across regressions because not all questions are asked each year and there is variation in the number of individuals who respond that they do not have enough information to answer. Except for the last outcome, these regressions include only people with the same MA status in both the baseline year and the previous year. Sample weights provided by the MCBS are used and standard errors are clustered by individual. $^{*}p < 0.1, ^{**}p < 0.05, ^{***}p < 0.01$

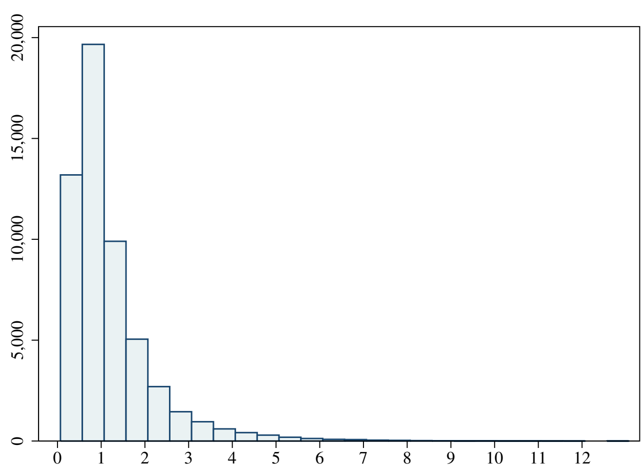
Appendix A: Supplementary Figures and Tables

Appendix Figure 1: Share of Medicare enrollees in a Medicare Advantage plan, 1994-2006



Notes: The first series is based on our MCBS sample. The second series is from annual county-level MA penetration data published by CMS.

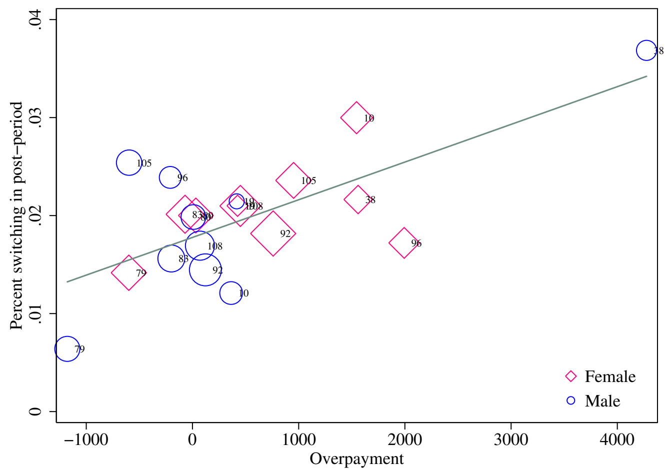
Appendix Figure 2: Histogram of HCC risk scores



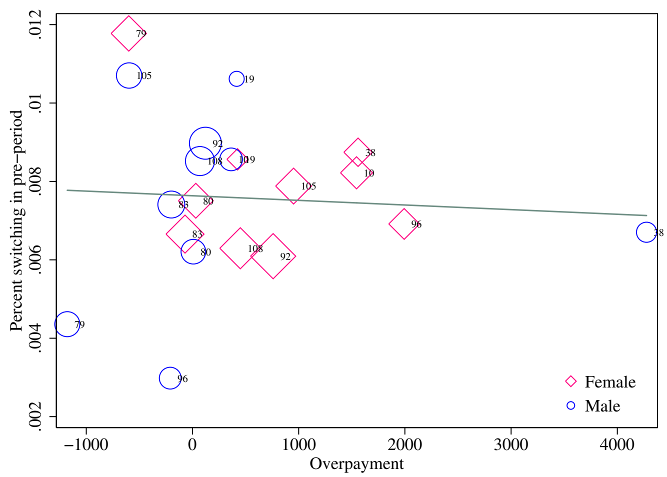
Notes: The sample for this figure are all pre-period MCBS observations enrolled in FFS all year. The risk scores we display are those we simulate from claims data (not those from CMS, only available in the post period.)

Appendix Figure 3: HCC overpayments among $Disease\,Category \times Gender$ cells and FFS-to-MA switching probability

(a) In the post-period



(b) In the pre-period



Notes: Each point in the scatter plot represents one of the top ten HCC conditions crossed by gender (females are represented by diamonds, males by circles), weighted by the number of cases in the MCBS. The x-axis is identical for both subfigures. It indicates the "overpayment" for each $Category \times Gender$ cell, which we estimate (for each cell) by subtracting total expenditures from the estimated capitation payment based on the HCC score for those in FFS all of the previous year and in FFS all Medicare-eligible months of the current year (which can be less than twelve for those who die). We estimate capitation payments using the HCC model, by calculating HCC risk scores and using pre-period benchmarks. We use the years 1997 to 2002, as the HCC formula was benchmarked using 1999-2000 data (we use the two years surrounding those years to gain additional precision). The y-axis is based on the estimated amount of time on MA among those who spent the entire previous year on FFS (essentially, a switching probability, adjusted slightly for the fact that someone may not spend the entire year in MA). These probabilities are estimated in the post-period for subfigure (A) and in the pre-period for subfigure (B).

Appendix Table 1: The ten most common conditions in the HCC formula

Category	Prevalence	Description	HCC weight
80	0.126	Congestive Heart Failure	0.417
108	0.124	Chronic Obstructive Pulmonary Disease	0.376
19	0.120	Diabetes without Complication	0.200
92	0.097	Specified Heart Arrhythmias	0.266
105	0.094	Vascular Disease	0.357
10	0.063	Breast, Prostate, Colorectal Cancers	0.233
83	0.046	Angina Pectoris/Old Myocardial Infarction	0.235
96	0.045	Ischemic or Unspecified Stroke	0.306
38	0.039	Rheum. Arthritis and	
		Inflam. Connective Tissue Disease	0.322
79	0.038	Cardio-Respiratory Failure and Shock	0.692

Notes: This table is based on the FFS population, 1993-2006. The weight associated with each HCC condition is added to a person's total risk score. Given that the average benchmark is roughly \$9,345—average per capita FFS expenditure (\$8,344) multiplied by the benchmark-to-FFS markup in 2006 (1.12)—in 2006, having been diagnosed with congestive heart failure in the previous year would mean an individual's capitation payment is increased by $0.417 * \$9,345 = \$3,897$.

Appendix Table 2: Example of changes in selection after risk adjustment and 20 percent statutory overpayment

	No Conditions	Has Cancer	
		Remission	Treatment
Model Fundamentals			
True medical costs	5	6	13
Screening	1	2	2
Payment-neutral risk score	5	9.5	9.5
Residual (Cost - R. score)	0	-3.5	3.5
Not Risk Adjusted			
Capitation Payments	8	8	8
Differential Payments	3	2	-5
Profits	2	0	-7
$Avg. r.score = 5$			
$Avg. residual = 0$			
$Total\ profits = 2$			
Risk Adjusted, plans screen			
Capitation Payment (R. score $\times$ 1.2)	6	11.4	11.4
Profits, by type	0	3.4	-3.6
$Total\ profits = 3.4$			
Risk Adjusted, plans do not screen			
Capitation Payment (R. score $\times$ 1.2)	6	11.4	11.4
Screening	0	0	0
Profits, by type	1	5.4	-1.6
$Total\ profits = 1 + 5.4 - 1.6 = 4.8$			
$Avg. r.score = \frac{5+9.5+9.5}{3} = 8$			
$Avg. residual = \frac{0-3.5+3.5}{3} = 0$			

Notes: Boxes indicate the type of enrollee that will join MA under each regime.

Appendix Table 3: Changes in differential payments after risk adjustment

	Dependent variable: Total Medicare expenditure						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Share of months in MA x After 2003	1733.4** [747.0]	2089.8*** [790.7]	2127.1*** [776.4]	2031.7** [810.9]	1779.4** [745.6]	1968.7** [883.3]	1563.0** [776.2]
Share of months in MA	905.2*** [256.5]	879.2*** [289.8]	873.4*** [290.1]	943.6*** [353.7]	1356.3*** [311.5]	1010.3** [500.1]	978.1*** [341.9]
Mean, dept. var.	7,640	7,621	7,601	7,586	7,207	7,791	7,586
Baseline controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Lagged health controls	No	No	Yes	Yes	Yes	Yes	Yes
Health controls	No	No	No	Yes	Yes	Yes	Yes
Dept. var windsorized	No	No	No	No	Yes	No	No
Only 1998-2006	No	No	No	No	No	Yes	No
Trend controls	No	No	No	No	No	No	Yes
Observations	73,054	72,930	72,638	72,375	72,375	54,120	72,375

Notes: All observations are in FFS all twelve months of the previous year. Year fixed effects are included in all regressions, and county fixed effects included in col. (2) - (7). All regressions include a once-lagged dependent variable, as well as dummy variables corresponding to eleven bins of lagged Part A and B expenditure (with zero as its own bin and ten bins corresponding to ten deciles of positive Part A and B expenditure, calculated separately for each year). "Baseline controls" include the following: individual's predicted capitation payment based on the demographic model; race and Hispanic origin; gender; age-in-year fixed effects; fixed effects for eligibility status (disabled and old-age, with and without end-stage-renal disease as a secondary condition); Medicaid status; the interaction of disability status and Medicaid status; income category fixed effects; months of Medicare eligibility; and education category fixed effects. "Lagged health controls" includes fixed effects for the five categories of lagged self reported health (excellent, very good, good, fair, poor), the lagged share of the year spent in an institution, and the lagged risk score. "Health controls" include the following: five categories of current self-reported health, the difference between current and previous-year self-reported health, an indicator variable for being alive the entire year, and the share of the year spent in an institution. The dependent variable is windsorized at the 99$^{\text{th}}$ percentile in col. (5). Col. (6) uses the shorter pre-period and col. (7) controls for pre-trends in $MA \times year$. Sample weights provided by the MCBS are used. Dollar amounts are adjusted to 2007 dollars using the CPI-U. Standard errors are clustered by the individual. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Appendix Table 4: Background regressions for mortality analysis

	Dept. Variable: Medicare costs		
	(1)	(2)	(3)
Demographic score	258.2***		
	[12.73]		
HCC risk score (from CMS)		959.5***	
		[59.94]	
Died in calendar year			4308.2***
			[151.4]
Mean, dept. var.	755.1	906.9	794.5
Sample period	Pre	Post	Both
R-squared	0.0126	0.110	0.155
Observations	54369	17153	71529

Notes: All observations are in FFS all months of the previous year and spend all Medicare-eligible months on FFS the current year (not always twelve months as some die). We have normalized both the demographic and HCC risk scores to have a standard deviation of one to make coefficient comparisons easier. The dependent variable in all regressions is total Medicare spending in the current year divided by Medicare-eligible months (as plans are not paid after patients die). Sample weights provided by the MCBS are used. Year effects included in all regressions. Dollar amounts are adjusted to 2007 dollars using the CPI-U. Standard errors are clustered by the individual. $^{*}p < 0.1,^{**} p < 0.05,^{***} p < 0.01$

Appendix Table 5: Satisfaction measures for MA versus FFS before and after risk adjustment

(a) Without MA-specific trends

	(1) gen	(2) costs	(3) follow	(4) concern	(5) info	(6) specialist	(7) phone	(8) avail	(9) sameloc
In MA majority of year x After 2003	0.00324	-0.108***	-0.0128	-0.0307**	-0.0152	-0.000904	0.0335*	-0.0135	-0.0111
	[0.0163]	[0.0189]	[0.0150]	[0.0157]	[0.0147]	[0.0156]	[0.0190]	[0.0194]	[0.0148]
Mean, dep var	3.257	3.015	3.162	3.147	3.121	3.168	3.060	3.115	3.105
Observations	76047	75469	69913	74866	75701	57315	48715	44591	69518

(b) With MA-specific trends

	(1) gen	(2) costs	(3) follow	(4) concern	(5) info	(6) specialist	(7) phone	(8) avail	(9) sameloc
In MA majority of year x After 2003	0.0440***	0.131***	0.0368**	0.0190	0.0561***	0.0534***	0.0841***	0.0426**	0.0492***
	[0.0163]	[0.0188]	[0.0150]	[0.0157]	[0.0147]	[0.0156]	[0.0190]	[0.0193]	[0.0148]
Mean, dep var	3.257	3.015	3.162	3.147	3.121	3.168	3.060	3.115	3.105
Observations	76047	75469	69913	74866	75701	57315	48715	44591	69518

Notes: Each column represents a regression of the form: $satisfaction\,category_i = \beta_1 MA_i + \beta_2 MA_i \times After_i + \lambda \mathbf{X}_i + \epsilon_i$, where $satisfaction$ takes values from one to four ("very dissatisfied," "dissatisfied," "satisfied," "very satisfied"), MA is a dummy variable for being enrolled in Medicare Advantage at least half of all Medicare-eligible months in a give year, 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. The abbreviations in the column labels refer, in the same order, to the nine satisfaction categories described in Table 7. Note that the sample size varies across regressions because not all satisfaction questions are asked each year and there is variation in the number of individuals who respond that they do not have enough information to answer. This regression focuses only on people with the same MA status in both the baseline year and the previous year. Sample weights provided by the MCBS are used and standard errors are clustered by individual. $^{*}p < 0.1, ^{**}p < 0.05, ^{***}p < 0.01$

Appendix Table 6: Differential satisfaction change by health and MA status, before and after risk adjustment

(a) Without trends

	(1) gen	(2) costs	(3) follow	(4) concern	(5) info	(6) specialist	(7) phone	(8) avail	(9) sameloc
MA x Health status x After 2003	0.0176	0.0135	0.0169	0.0122	0.0196	-0.0133	0.0323*	0.00511	0.000902
	[0.0147]	[0.0176]	[0.0137]	[0.0144]	[0.0133]	[0.0145]	[0.0173]	[0.0182]	[0.0131]
Mean, dep var	3.257	3.015	3.162	3.147	3.121	3.168	3.060	3.115	3.105
Observations	75884	75309	69764	74711	75539	57187	48616	44502	69380

(b) With $MA \times year$, $Health \times year$ and $MA \times Health \times year$ trends

	(1) gen	(2) costs	(3) follow	(4) concern	(5) info	(6) specialist	(7) phone	(8) avail	(9) sameloc
MA x Health status x After 2003	-0.0121	-0.0193	-0.00421	-0.00213	-0.00625	-0.0152	0.0409**	-0.0168	-0.0107
	[0.0148]	[0.0177]	[0.0138]	[0.0145]	[0.0133]	[0.0145]	[0.0174]	[0.0182]	[0.0131]
Mean, dep var	3.257	3.015	3.162	3.147	3.121	3.168	3.060	3.115	3.105
Observations	75884	75309	69764	74711	75539	57187	48616	44502	69380

Notes: Each column represents a regression of the form: $satisfaction\,category_i = \beta_1 MA_i + \beta_2 MA_i \times After_i + \lambda \mathbf{X}_i + \epsilon_i$, where $satisfaction$ takes values from one to four ("very dissatisfied," "dissatisfied," "satisfied," "very satisfied"), MA is a dummy variable for being enrolled in Medicare Advantage at least half of all Medicare-eligible months in a give year, 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. The abbreviations in the column labels refer, in the same order, to the nine satisfaction categories described in Table 7. Note that the sample size varies across regressions because not all satisfaction questions are asked each year and there is variation in the number of individuals who respond that they do not have enough information to answer. This regression focuses only on people with the same MA status in both the baseline year and the previous year. Sample weights provided by the MCBS are used and standard errors are clustered by individual. $^{*}p < 0.1,^{**} p < 0.05,^{***} p < 0.01$

Appendix Table 7: Self-reported health and satisfaction with health care before and after risk adjustment

(a) Without $Health \times year$ trends

	(1) gen	(2) costs	(3) follow	(4) concern	(5) info	(6) specialist	(7) phone	(8) avail	(9) sameloc
Self-reported health x After 2003	0.00832*	0.00630	0.00606	0.00757	0.00956**	0.0198***	0.0150***	0.00626	0.0137***
	[0.00466]	[0.00595]	[0.00452]	[0.00469]	[0.00452]	[0.00467]	[0.00560]	[0.00571]	[0.00454]
Mean, dep var	3.257	3.015	3.162	3.147	3.121	3.168	3.060	3.115	3.105
Observations	75884	75309	69764	74711	75539	57187	48616	44502	69380

(b) With $Health \times year$ trends

	(1) gen	(2) costs	(3) follow	(4) concern	(5) info	(6) specialist	(7) phone	(8) avail	(9) sameloc
Self-reported health x After 2003	0.0181***	0.00850	0.0191***	0.0233***	0.0245***	0.0211***	0.0153***	0.0278***	0.0176***
	[0.00466]	[0.00595]	[0.00452]	[0.00469]	[0.00452]	[0.00467]	[0.00560]	[0.00571]	[0.00454]
Mean, dep var	3.257	3.015	3.162	3.147	3.121	3.168	3.060	3.115	3.105
Observations	75884	75309	69764	74711	75539	57187	48616	44502	69380

Notes: Each column represents a regression of the form: $satisfaction\, category_i = \beta_1 Health_i + \beta_2 Health_i \times After_i + \lambda \mathbf{X}_i + \epsilon_i$, where $satisfaction$ takes values from one to four ("very dissatisfied," "dissatisfied," "satisfied," "very satisfied"), $Health$ is a (demeaned) linear measure of the five-category self-reported health variable, 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. Note that the sample size varies across regressions because not all questions are asked each year and there is variation in the number of individuals who respond that they do not have enough information to answer. Sample weights provided by the MCBS are used and standard errors are clustered by individual. $^{*}p < 0.1, ^{**}p < 0.05, ^{***}p < 0.01$

Appendix Table 8: Quality of care measures among 55-74 year-olds in the 2000-2006 NHIS

(a) Without $Elderly \times Year$ trends

	Unhappy with access to care regarding...				
	(1) Phone	(2) Appointment	(3) Waiting	(4) Hours	(5) Rec'd flu shot
Age 65-74 x After 2003	-0.00101 [0.00270]	0.000845 [0.00385]	0.00186 [0.00378]	-0.00198 [0.00252]	0.00888 [0.00901]
Observations	48,675	48,675	48,673	49,107	48,279

(b) With $Elderly \times Year$ trends

	Unhappy with access to care regarding...				
	(1) Phone	(2) Appointment	(3) Waiting	(4) Hours	(5) Rec'd flu shot
Age 65-74 x After 2003	-0.000943 [0.00536]	-0.00362 [0.00763]	-0.00103 [0.00750]	-0.00536 [0.00500]	0.0289 [0.0179]
Observations	48,675	48,675	48,673	49,107	48,279

Notes: All regressions take the form $outcome_{it} = \beta Age\,65-75_i \times After\,2003_t + \alpha_i + \delta_t + \epsilon_{it}$, where α_i are a vector of age-in-year fixed effects and δ_t are a vector of year fixed effects. The first four outcomes measure self-reported dissatisfaction (a binary variable in the NHIS) with, respectively, reaching health care providers over the phone, making a timely appointment, time spent in the waiting room, and providers' hours of operation. The final variable is a binary variable for whether the respondent received a flu shot in the past twelve months. The lack of any improvement among the young elderly after risk adjustment is robust to the following specification checks. First, we excluded any of the near-elderly who are on disability, as many will qualify for Medicare. Second, we excluded those who report having no contact with health professionals in the past two years, given that otherwise it is difficult to separate the effect of not being dissatisfied with simply not seeking care. Third, we included data from 2007 to 2008 (beyond our MCBS sample period). Results from each of these checks is available from the authors.

Appendix Table 9: Mortality rates for near elderly (55-64) and young elderly (65-74) before and after risk adjustment

	Log mort. rate		Δ log mort. rate		Δ_2 log mort. rate	
	(1)	(2)	(3)	(4)	(5)	(6)
Pre-period MA share x	0.000247	-0.0127				
Elderly x After	[0.0164]	[0.0176]				
Δ MA share x			0.00364	0.0256		
Elderly x After			[0.159]	[0.169]		
Δ_2 MA share x					0.111	0.107
Elderly x After					[0.0898]	[0.0961]
Lagged dept. var.?	No	Yes	No	No	No	No
County FE?	Yes	Yes	No	Yes	No	Yes
Observations	30,996	25,453	25,453	25,453	22,140	22,140

Notes: Data taken from county-level vital statistics data. "Pre-period MA share" is a county's average MA share between 2000 and 2003. Δ $MA\ share_{it}$ is defined as $MA\ share_{it} - MA\ share_{i,t-1}$ and Δ_2 $MA\ share_{it}$ is defined as $MA\ share_{it} - MA\ share_{i,t-2}$ for county i in year t, with Δ $ln(mortality\ rate)$ and Δ_2 $ln(mortalityrate)$ defined analogously. "Elderly" is an indicator variable for being age 65-74 as opposed to 55-64. For all specifications, all lower-order terms of the triple interaction terms are also included but are not reported. We also explored results using data through 2008 (beyond the MCBS sample period) and also found no effect on elderly mortality after 2003 (results available upon request).

Appendix B: Supplementary overpayment analysis using estimated capitation payments

In this section, we focus on how an individual's annual *Total Medicare expenditure* changes as he switches from FFS to MA. We calculate this variable by summing the reported capitation payment each month an individual is in MA and any Part A or B payments incurred over the year. Obviously, for those classified as being in MA, *Total Medicare expenditure* is determined entirely or mostly by capitation payments, and for those in FFS it is determined entirely or mostly by provider payments. 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 expenditure levels.

Constructing capitation payments

While summing capitation payments and Part A and B payments is in principle very simple, another limitation of the MCBS is that, perhaps for confidentiality reasons, capitation payments reported after 2003 do not consistently reflect individual-level variation in HCC scores (see the information we provide at the end of this Appendix Section). We thus try to reconstruct capitation payments ourselves, using our simulated risk scores from FFS claims data from the previous year.

To isolate the effect of the introduction of risk adjustment from other changes occurring around the same time, we make two adjustments to capitation payments after 2003. First, the growth rate of county benchmarks (the baseline value, which, multiplied by the risk score, yields capitation payments) began to rise more rapidly in the later years of our sample period. We thus calculate each county's benchmark growth rate in the pre-period and then have the county's benchmarks grow at this slower rate for the post-period as well. Second, in the years immediately following the introduction of risk adjustment, plans received so-called "budget-neutrality" adjustments (about a ten percent increase in the risk-adjusted portion of capitation payments) to ease the transition to risk adjustment, and we mechanically 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.

Empirical strategy and results

There are two groups of MA enrollees in the post-period: those who joined during the post-period and those "incumbent" enrollees who joined in the pre-period. Our model suggests that the effect of risk-adjustment will be very different for the two groups and we analyze them separately. We begin with our standard "switcher" analysis, which examines those who switch from FFS to MA and thus, in the post-period, only picks up the effect of those who are joining *after* the new policy and not the effect on "incumbent" MA enrollees.

Switcher analysis: Empirical strategy. Consider the sample of beneficiaries in FFS all twelve months of a given year $t-1$. To estimate the counterfactual Medicare expenditure 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:

$$Expenditure_{it} = \beta MA_{it} \times After\,2003_t + \gamma MA_{it} + \lambda X_{it} + \delta_t + f(Expenditure_{i,t-1}) + \epsilon_{it}, \quad (4)$$

where $Expenditure_{it}$ is total Medicare expenditure for person i in year t, $f(Expenditure_{i,t-1})$ is a flexible function of lagged Medicare expenditure, and all other notation follows that in previous equations.[33] Note that in the intensive-margin regression we modeled an individual's Medicare expenditure the year *before* joining MA—hypothesizing that individuals who have low baseline FFS spending conditional on their risk score would be highly attractive to MA plans after risk adjustment—whereas here we model current Medicare expenditure. While lagged Medicare expenditure is highly correlated with current Medicare expenditure and thus serves as an obvious factor on which plans would try to screen, it is the current expenditure that an MA plan must actually cover once someone has joined and thus current expenditure is what matters for estimating differential payments.

Switcher analysis: Results. The first column of Table 10 shows the results from regressing the level of total Medicare spending on the MA variable, which is allowed to have a different effect before and after risk adjustment, the lagged spending variables, and year fixed effects. Total Medicare expenditure increases by roughly \$905 when an individual switches from FFS to MA (for the entire year) before risk adjustment, and by an additional \$1,733 after risk adjustment.

The second column adds county fixed effects as well as demographic and other basic controls (all listed in the table notes). The coefficient on the interaction term increases to \$2,081. 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. Col. (3) includes measures of lagged health indicators, which has essentially no effect on the coefficient on the interaction term. Col. (4) includes health indicators from the current year. While self-reported health is not a perfect proxy for current-year health costs, this specification better accounts for potential regression to the mean in health status—if enrollees typically experience a deterioration in their health upon joining MA, then comparing current to previous year's spending will overstate MA differential payments; however, current-year health status is endogenous to the care individuals receive in MA versus FFS and thus including it may be "over-controlling." In practice, the two estimates are very similar.[34]

[33] We prefer this specification to simply regressing $\Delta Expenditure_{it}$ as the lagged expenditure controls in equation (4) can better account for the fact that medical costs typically exhibit strong regression to the mean, though results using $\Delta Expenditure_{it}$ look very similar and are available from the authors. The lagged Medicare expenditure controls include: lagged Part A and B expenditure and deciles of non-zero Part A payments and non-zero Part B payments as well as indicator variables for zero Part A and B payments (we found that regression to the mean differed depending on the type and level of costs). The results are not sensitive to controlling more coarsely or finely than deciles for lagged Part A and B expenditure.

[34] We explore whether individuals tend to join MA just as their health is about to deteriorate or as their spending is about to rise for other reasons, which would cause us to underestimate the costs MA plans actually face and thus to overestimate overpayments. First, if this effect were important, we should have seen a large decrease in β and γ after current health measures were added in col. (4). Second, individuals

Cols. (5) and (6) subject the estimation in col. (4) to robustness checks. Windsorizing the data based on the 99th percentile in col. (5), dropping years before 1998 in col. (6), or including a pre-trend control in col. (7) leave the results largely unchanged. Though our estimates vary somewhat based on specification and standard errors are substantial, in general we see a doubling of overpayments after risk-adjustment among those switching from FFS to MA relative to those stahing in FFS.

Incumbent analysis. While we cannot look at the "stock" of MA enrollees post-risk-adjustment, we instead use pre-period data to examine how risk-adjustment *would have affected* those who joined MA pre-risk-adjustment and project this effect onto the incumbent MA enrollees, who, by definition, themselves switched pre-risk-adjustment.

Using our simulated risk scores and pre-period benchmarks and CMS's "rescaling factors," we can estimate capitation payments had the HCC score been used in the pre- period.[35] We then subtract this value from the pre-period capitation payments used in Appendix Table 10 and weight this difference by each observation's share of months in MA (so, those in FFS all twelve months do not contribute to the calculation). This calculation leads to a difference of \$694. That is, had the HCC formula and pre-period benchmarks been used to calculate capitation payments among the pre-period MA population, overpayments would have fallen by just under \$700, relative to using the demographic model.

We thus assume that, among those who switched to MA before 2004 but who remain there in the post-period, overpayments would have fallen by \$694. Note that this group would be subject to intensive coding, which our above estimate cannot include. As such, assuming that incumbent MA enrollees would see the full \$694 decease in their capitation payments assumes plans do not intensively code them in the post-period, and as such serves as an upper bound on the effectiveness of risk-adjustment in reducing overpayments to this population.

Discussion and aggregate spending calculations

We now combine the effects on MA switchers with MA incumbents to assess how overpayments change after risk-adjustment. We take the coefficient on col. (4) of Appendix Table 10 as our estimate of the increase in overpayments among MA switchers, stripped of the effect of increased benchmarks and budget-neutrality payments. And we take -\$694 as the effect on MA incumbents.

Because of the significant flux in the MA population, by 2006, 32 percent of those in MA in the MCBS had switched at some point in the post-period, whereas 68 percent were MA incumbents who joined before 2004. As such, we estimate that the overall effect of risk-adjustment is $-\$694 * 0.68 + \$2032 * 0.32 = \$178$.

are unlikely to postpone expensive treatments until they join an MA plan because plans tend to have less generous cost-sharing for serious procedures than does FFS (see Kaiser's report on MA benefits, `http://www.kff.org/medicare/upload/8047.pdf`). Third, we actually find no evidence of strategic timing of services for which MA is more generous than FFS, such as vision exams. Finally, we find no evidence of an "Ashenfelter dip" the year before a switch to MA—controlling for two years of lagged cost data instead of one has minimal effect on the point-estimates, though standard errors increase due to the smaller sample.

[35]Rescaling factors are used to convert benchmarks used for the demographic model to benchmarks used for the HCC model.

To add back in the effect of increasing benchmarks and budget neutrality payments, recall that MedPAC estimated that these factors increased MA payments to 108 percent of FFS costs in 2004-2006, assuming risk-selection worked perfectly. Roughly speaking, this ratio was about 100 percent in the pre-period (95 percent in the early years, rising to 103 percent from 2001-2003). Finally, average FFS spending in 2004 is $8385.

As such, taking 2001-2003 as the baseline, overpayments increase by $.05 * 8385 + \$178 = \$597$. Taking the entire pre-period as the baseline, the estimate rises to $.08 * 8385 + \$178 = \$849$.

Of course, the share of MA incumbents, while still just under one-third in 2006, will fall over time, making the blended average between switcher and incumbents more positive over time.

Appendix Table 10: Changes in differential payments after risk adjustment

	Dependent variable: Total Medicare expenditure						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Share of months in MA x After 2003	1733.4** [747.0]	2089.8*** [790.7]	2127.1*** [776.4]	2031.7** [810.9]	1779.4** [745.6]	1968.7** [883.3]	1563.0** [776.2]
Share of months in MA	905.2*** [256.5]	879.2*** [289.8]	873.4*** [290.1]	943.6*** [353.7]	1356.3*** [311.5]	1010.3** [500.1]	978.1*** [341.9]
Mean, dept. var.	7,640	7,621	7,601	7,586	7,207	7,791	7,586
Baseline controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Lagged health controls	No	No	Yes	Yes	Yes	Yes	Yes
Health controls	No	No	No	Yes	Yes	Yes	Yes
Dept. var windsorized	No	No	No	No	Yes	No	No
Only 1998-2006	No	No	No	No	No	Yes	No
Trend controls	No	No	No	No	No	No	Yes
Observations	73,054	72,930	72,638	72,375	72,375	54,120	72,375

Notes: All observations are in FFS all twelve months of the previous year. Year fixed effects are included in all regressions, and county fixed effects included in col. (2) - (7). All regressions include a once-lagged dependent variable, as well as dummy variables corresponding to eleven bins of lagged Part A and B expenditure (with zero as its own bin and ten bins corresponding to ten deciles of positive Part A and B expenditure, calculated separately for each year). "Baseline controls" include the following: individual's predicted capitation payment based on the demographic model; race and Hispanic origin; gender; age-in-year fixed effects; fixed effects for eligibility status (disabled and old-age, with and without end-stage-renal disease as a secondary condition); Medicaid status; the interaction of disability status and Medicaid status; income category fixed effects; months of Medicare eligibility; and education category fixed effects. "Lagged health controls" includes fixed effects for the five categories of lagged self reported health (excellent, very good, good, fair, poor), the lagged share of the year spent in an institution, and the lagged risk score. "Health controls" include the following: five categories of current self-reported health, the difference between current and previous-year self-reported health, an indicator variable for being alive the entire year, and the share of the year spent in an institution. The dependent variable is windsorized at the 99^{th} percentile in col. (5). Col. (6) uses the shorter pre-period and col. (7) controls for pre-trends in $MA \times year$. Sample weights provided by the MCBS are used. Dollar amounts are adjusted to 2007 dollars using the CPI-U. Standard errors are clustered by the individual. $^{*}p < 0.1, ^{**}p < 0.05, ^{***}p < 0.01$

Note: Capitation payments in the MCBS

Two pieces of information support our conclusion that after 2003 the MCBS capitation payment variable does not reflect variation in the risk scores. First, after risk adjustment when capitation payments were based on an individual's risk score, there is extremely little variation in the capitation payments recorded in the MCBS for beneficiaries in the same age group, calendar year, gender, disability, Medicaid status, institutional status, plan, and county cells. For example, in 2004 and 2005, consider all individuals who are (1) enrolled in an MA plan in May of that year and (2) are in a cell (as defined above) with at least one other beneficiary in the MCBS. Of these more 1,000 individuals, more than 92 percent have capitation payments that are within $1 of all other individuals in their cell. Second, using the actual risk scores provided to us by CMS, we show that individuals in the same cell (as defined above) who have *different* risk scores are recorded as receiving the *same* capitation payment. In 2006, the MCBS does not include plan identifiers, but the payment variable in the MCBS still does not appear to represent the actual amount of money an MA plan received. For example, there are twelve individuals who are enrolled in MA all months in 2006 and have exactly the same very low annual capitation payment ($913.58). Yet these individuals have substantially different risk scores (one has a risk score of 1.03 while another has a risk score of 4.67) and different ages (one is 68 years old while another is 95). We speculate that the MCBS may not include capitation payments that reflect an individual's risk score because such information would allow researchers to back out an individual's risk score, a variable that is not included in the MCBS and that we needed to access directly from CMS itself. Nonetheless, as we show in Table 10, using the uncorrected capitation payments from the MCBS has little impact on our results.

Documents needed to calculate risk scores and capitation payments from FFS claims data

CMS provides the file mapping ICD-9 conditions to HCC categories at `http://www.cms.gov/MedicareAdvtgSpecRateStats/Downloads/RAdiagnoses.zip`. The model coefficients and algorithms can be found at `http://www.cms.gov/MedicareAdvtgSpecRateStats/Downloads/HCCsoftware07.zip`. To calculate final capitation payments, these risk scores are multiplied by "county benchmarks," which are published annually in the Medicare Advantage "ratebooks," and ratebooks from 1990 to 2011 are available at: `http://www.cms.gov/MedicareAdvtgSpecRateStats/RSD/list.asp`.

Appendix C: Theoretical Framework

In this Appendix, we formalize the intuition provided in Section 3. The purpose of this model is to understand how adopting risk adjustment will influence total costs to the government from offering MA plans. We therefore take as given the basic contours of the risk-adjustment formula used by CMS, as opposed to exploring the optimal formula, as in Glazer and McGuire (2000) and others.

While an MA plan must be open at the same price to all individuals in the plan's geographic area of operation, the model assumes that, as shown in earlier work, plans have at least *some* scope to encourage individual with certain characteristics to enroll. For example, by differentially advertising in *Diabetes Forecast* (a publication of the American Diabetes Association), MA plans could increase the probability that diabetics enroll.

We emphasize that this process does not necessarily imply that the plan have access to information about the characteristics of any individual Medicare beneficiaries. Instead, plans could use information on the conditional distribution of costs in the Medicare population and employ strategies, such as targeted advertising or changing the quality of physicians in their network, to encourage beneficiaries with certain conditions to enroll. Beneficiaries, who have private information on their health type, choose to enroll in MA based on the perceived costs and benefits of the plan.[36]

To keep the model tractable, we do not model the consumer side of the enrollment decision and instead focus on plans' decision to incur the costs associated with these screening activities in return for enrolling a selected subsample from the Medicare population. In our model, plans have an incentive to target individuals for whom the difference between capitation payments and expected costs is the greatest, and risk adjustment changes this set of individuals by changing how capitation payments are calculated.

9.1 Basic framework and assumptions

9.1.1 Cost of health insurance coverage

Let the cost of covering individual i in a given year be given by $m_i = b_i + v_i$, where b_i is an individual's expected cost conditional on the variables included in the risk-adjustment formula used by the government, and v_i is the residual. As MA contracts have a year-long duration, the model is single-period, and we thus specify costs over a single year.[37] Both v and b are in units of absolute dollars.[38] While $\mathbb{E}(v|\, b) = 0$ for all b, the conditional variance of v can vary with b, consistent with past work showing substantial heteroskedasticity in

[36]Note that the model does not rule out the possibility that plans use some information to actively encourage some individuals to enroll in their plan. For example, MA plans may respond more quickly to enrollment requests from respondents residing in low-cost areas, as Bauhoff (2012) finds in the German context.

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

[38]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 we present results on individuals' satisfaction with their out-of-pocket costs in Section 7.

medical costs. We assume that costs m are the same whether an individual is in FFS or MA. Of course, MA plans may be better or worse at controlling costs 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. This assumption not only simplifies the analysis, but also allows us to more easily focus on the difference between payments to private plans 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.[39]

9.1.2 Capitation payments and risk adjustment

Without risk adjustment, plans receive a fixed payment $\bar{p}$ for each individual they enroll. We model risk adjustment as replacing $\bar{p}$ with a function $p(b)$, $p' > 0$, so that capitation payments become an increasing function of b. While our main results on selection and differential payments do not require that risk- adjusted payments are linear in b, this assumption corresponds to the MA setting where capitation payments are calculated by multiplying risk scores by a fixed county factor. As it allows us to generate additional empirical predictions and also simplifies the analysis, we take as a baseline assumption that $p''(\cdot) = 0$.[40]

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.[41]

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 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*

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

[39]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.

[40]In particular, our proofs of Proposition 1 (that risk adjustment causes selection to fall along the b margin and rise along the v margin) and Proposition 3 (that the effect of risk adjustment on differential payments is ambiguous) do not depend on the linearity of $p(\cdot)$.

[41]As we discuss in Section 2, plans 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.

9.1.3 Screening costs

Though we discuss profit-maximization in greater detail shortly, plan profits are obviously a function of an individual's cost $m_i = b_i + v_i$, and thus plans will have preferences over the b and v values of their enrollees, even if the plan is unable to observe b and v for any potential beneficiary. However, MA plans are required to accept any patient in their geographic coverage area who chooses to enroll, and selectively encouraging certain individuals to enroll will entail screening costs. Thus, even though plans cannot directly control the characteristics of their beneficiaries, because plans can indirectly influence the population who signs up, we assume that b and v are choice variables on the part of the plan.

We assume that the per capita screening cost c a plan incurs is given by $c(b, v)$, where b and v are its enrollees' average values of b_i and v_i. 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 (recall we assume $\bar{v} = 0$). Encouraging individuals to enroll who are 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 assume that the cost function is 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 Figure 1) and v is measured in absolute dollars, it should be easier to attract, say, a cancer patient with costs \$100 below what her risk score would predict than someone without a single documented disease condition with costs \$100 below what her risk score would predict.

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

$$\mathbb{E}(\pi) = \underbrace{(1-\Omega)\bar{p} + \Omega p(b)}_{\text{capitation payment}} - \left(\underbrace{b + v}_{\text{FFS cost}}\right) - \underbrace{c(b, v)}_{\text{screening cost}} . \tag{5}$$

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

9.2 Main Results

We begin with our main selection result, which characterizes how plans will react to a change in risk adjustment.

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

(i) Plans decrease screening along the b margin and thus the average value of b among their enrollees rises ("extensive-margin" selection decreases).

(ii) Plans increase screening along the v margin and thus the average value of v among their enrollees falls ("intensive-margin" selection increases).

This proposition formalizes the result from the Theoretical Framework that (1) "the risk scores of those enrolling in MA will increase relative to those remaining in FFS" (2) "actual costs conditional on the risk score will fall among those enrolling in MA relative to those remaining in FFS."

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 plan's optimal levels of b and v. The first-order conditions from maximizing the profit expression in equation (5) with respect to b and v are given by

$$[b] \;:\; \Omega p'(b^*) - c_b(b^*, v^*) = 1 \tag{6}$$

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

Totally differentiating equation (6) with respect to Ω yields

$$p'(\cdot) + \Omega p''(\cdot)\frac{\partial b^*}{\partial \Omega} - c_{11}(\cdot,\cdot)\frac{\partial b^*}{\partial \Omega} - c_{12}(\cdot,\cdot)\frac{\partial v^*}{\partial \Omega} = 0 \tag{8}$$

Similarly, equation (7) yields:

$$c_{bv}(\cdot,\cdot)\frac{\partial b^*}{\partial \Omega} + c_{vv}(\cdot,\cdot)\frac{\partial v^*}{\partial \Omega} = 0$$

or

$$\frac{\partial v^*}{\partial \Omega} = -\frac{c_{bv}}{c_{vv}}\frac{\partial b^*}{\partial \Omega}. \tag{9}$$

Substituting equation (9) into (8) gives:

$$p'(\cdot) + \Omega p''(.)\frac{\partial b^*}{\partial \Omega} - c_{bb}(\cdot,\cdot)\frac{\partial b^*}{\partial \Omega} - c_{bv}(\cdot,\cdot)\left(-\frac{c_{bv}}{c_{vv}}\frac{\partial b^*}{\partial \Omega}\right) = 0.$$

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

$$\frac{\partial b^*}{\partial \Omega} = \frac{\overbrace{p'}^{+\text{ as cap payments increase in } b}}{-\Omega p''(\cdot) + \dfrac{\overbrace{(c_{bb}c_{vv}) - c_{bv}^2}^{+\text{ by convexity of } c(\cdot,\cdot)}}{\underbrace{c_{vv}}_{+\text{ by convexity of } c(\cdot,\cdot)}}} \tag{10}$$

By assumption, $p''(\cdot) = 0$, so the entire denominator is positive. As $\frac{\partial b^*}{\partial \Omega} > 0$ and $c_{bv}, c_{vv} > 0$, equation (9) gives the result in (ii). ■

As risk adjustment makes capitation payments a positive function of b, plans will spend less effort finding low-b enrollees and instead focus on finding low-v enrollees. We term the first result "extensive-margin" selection as it relates to the government's risk score, which is an approximate measure of actual cost; we term the second result "intensive-margin" selection because it relates to how intensely individuals are selected conditional on the risk score.[42]

Proposition 2. *For $\Omega_0 < \Omega_1$, moving from Ω_0 to Ω_1 will always decrease differential payments if (1) b and v are held fixed at their equilibrium values under Ω_0 and (2) if individuals are positively selected with respect to b under Ω_0.*

This proposition formalizes the result from the Theoretical Framework that, "applying the risk-adjustment formula to the pre-risk-adjustment population of MA enrollees would have decreased the total capitation payments the government would have made on their behalf."

Proof. The result is easy to show when $p(\cdot)$ is linear. Recall that $p(\cdot)$ 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 $b < \bar{b}$. So, as long as individuals are positively selected with respect to b under Ω_0 ($b < \bar{b}$), the proposition holds. ■

Proposition 3. *The effect of increasing Ω on a plan's average differential payment is ambiguous.*

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

$$\phi(\Omega) = \underbrace{\Omega p(b^*(\Omega)) + (1-\Omega)\bar{p}}_{\text{capitation payment}} - \underbrace{(b^*(\Omega) + v^*(\Omega))}_{\text{actual costs}} \tag{11}$$

Differentiating with respect to Ω gives:

$$\phi'(\Omega) = \Omega p' \frac{\partial b^*}{\partial \Omega} + p(b^*) - \bar{p} - \frac{\partial b^*}{\partial \Omega} - \frac{\partial v^*}{\partial \Omega} \tag{12}$$

Rearranging and substituting $\frac{\partial v^*}{\partial \Omega} = -\frac{c_{bv}}{c_{vv}} \frac{\partial b^*}{\partial \Omega}$ from equation (9) yields

$$\phi'(\Omega) = [p(b^*) - \bar{p}] + \frac{\partial b^*}{\partial \Omega}(\Omega p' - 1 + \frac{c_{bv}}{c_{vv}}) \tag{13}$$

We showed in the proof of Proposition 2 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

[42] 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 the testable prediction that risk scores *as measured by the government* increase with an increase in risk adjustment.

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 large, the expression can indeed be positive. This condition requires c_{bv} to be sufficiently 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 (5) and q is the number of enrollees the firm has.

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

$$[b] \quad : \quad q_b(b,v)\pi(b,v,\Omega) + q(b,v)\underbrace{(\Omega p' - 1 - c_b(b,v))}_{\pi_b} \tag{14}$$

$$[v] \quad : \quad q_v(b,v)\pi(b,v,\Omega) + q(b,v)\underbrace{(-1 - c_v(b,v))}_{\pi_v} \tag{15}$$

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 (14) and (15) reduce to the original first-order conditions of $\pi_b = \pi_v = 0$.

Overall Cost Selection

In this section, we explore how the move to risk adjustment changes selection along overall FFS costs $(b+v)$. We are interested in $\frac{d(v^*+b^*)}{d\Omega}$.

Proposition 4. *If, beginning at no risk adjustment* $(\Omega = 0)$*, increasing* Ω *increases a firm's average differential payment, then the effect of increasing* Ω *on the overall FFS costs of beneficiaries* $(b+v)$ *is negative. That is,* $\left.\frac{d(b^*+v^*)}{d\Omega}\right|_{\Omega=0} < 0$.

Proof. From (9), we know that $\frac{dv^*}{d\Omega} = -\frac{c_{bv}}{c_{vv}}\frac{db^*}{d\Omega}$. So, we know that

$$\begin{aligned} \frac{d(b^*+v^*)}{d\Omega} &= \frac{db^*}{d\Omega} + \frac{dv^*}{d\Omega} \\ &= \frac{db^*}{d\Omega} - \frac{c_{bv}}{c_{vv}}\frac{db^*}{d\Omega} \\ &= \frac{db^*}{d\Omega}\left(1 - \frac{c_{bv}}{c_{vv}}\right). \end{aligned}$$

From Proposition 3, we know that if increasing risk adjustment causes overpayments to increase, it must be the case that $\Omega p' - 1 + \frac{c_{bv}}{c_{vv}} > 0$. This implies that

$$\Omega p' > 1 - \frac{c_{bv}}{c_{vv}}.$$

If there is no risk adjustment $(\Omega = 0)$, this equation implies that $1 - \frac{c_{bv}}{c_{vv}} < 0$. Now, by Proposition 1, we know that moving to risk adjustment causes the average risk score of enrollees to rise: $\frac{db^*}{d\Omega} > 0$. Hence $\frac{d(b^*+v^*)}{d\Omega} < 0$. ∎

Note that this result is true only for small changes in Ω evaluated at $\Omega = 0$. The empirical section of this paper and in the simplified model in the main text, by contrast, involves moving Ω by a large amount, starting from 0. In this case, there is no guarantee that $\frac{d(b^*+v^*)}{d\Omega} < 0$. Nonetheless, this result highlights the fact that risk adjustment can cause overall cost selection to increase.

Firm Profits

In this section, we explore the effect of changing risk adjustment on firm profits.

Proposition 5. *Increasing Ω decreases a firm's average per enrollee profits so long as their enrollees are positively selected with respect to b.*

Proof. The simplest way to verify this claim is to use the envelope theorem. Write profits as a function of the amount of risk adjustment (Ω) and the characteristics of the average enrollees (b^*, v^*), which indirectly depend on Ω. The envelope theorem says that to understand how profits change with Ω, one can ignore how changing Ω influences the optimal choices of b and v. Recall from (11) that firm profits are given by

$$\pi(\Omega) = \underbrace{(1-\Omega)\bar{p} + \Omega p(b^*(\Omega))}_{\text{capitation payment}} - \underbrace{(b^*(\Omega) + v^*(\Omega))}_{\text{FFS cost}} - \underbrace{c(b^*(\Omega), v^*(\Omega))}_{\text{screening cost}}$$

Differentiating with respect to Ω, and ignoring the dependence of b^* and v^* on Ω, we see that

$$\pi'(\Omega) = p(\cdot) - \bar{p} \tag{16}$$

As we showed in the proof of Proposition 2, this expression is negative as long as individuals are positively selected with respect to b. ∎

Corollary. *So long as plans' enrollees are positively selected with respect to b, if overpayments increase after risk adjustment, then plans' screening costs must also increase.*

Proof. From Proposition 5, we know that profits fall under these conditions. As such, the only way for overpayments to increase and for profits to fall is for screening costs to have risen. This result can also be shown analytically. ∎

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