”How Does Risk-Selection Respond to Risk-Adjustment?

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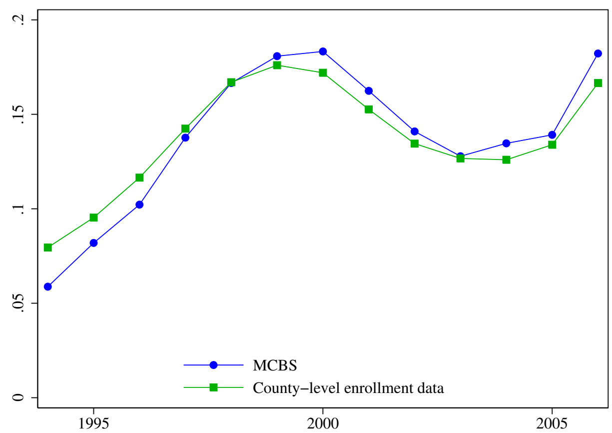
ONLINE APPENDIX

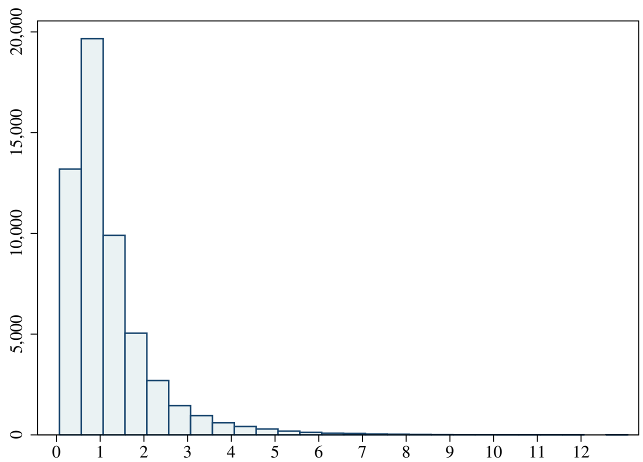
Appendix A: Supplementary Figures and Tables

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.

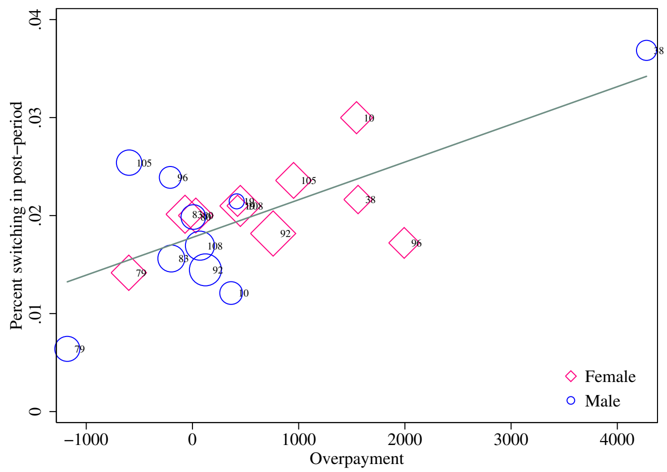
Appendix Figure 2: Histogram of HCC risk scores



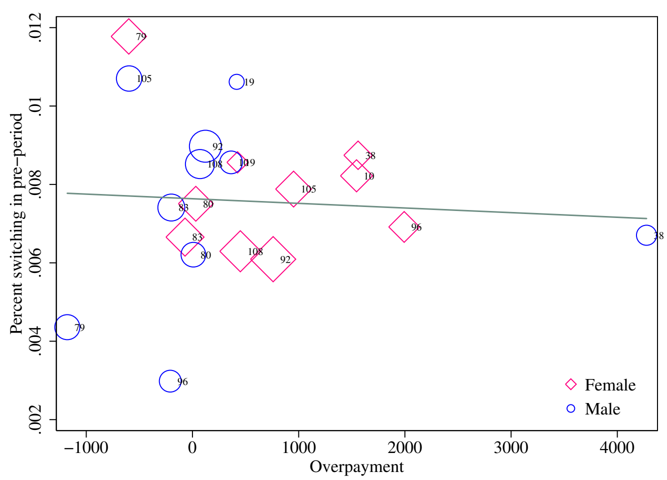
Notes: The sample for this figure are all pre-period MCBS observations enrolled in FFS all year. The risk scores we display are those we simulate from claims data (not those from CMS, only available in the post period.)

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	0.149	Congestive Heart Failure	0.417
108	0.147	Chronic Obstructive Pulmonary Disease	0.376
19	0.139	Diabetes without Complication	0.200
92	0.117	Specified Heart Arrhythmias	0.266
105	0.114	Vascular Disease	0.357
10	0.073	Breast, Prostate, Colorectal Cancers	0.233
83	0.054	Angina Pectoris/Old Myocardial Infarction	0.235
96	0.053	Ischemic or Unspecified Stroke	0.306
38	0.046	Rheum. Arthritis and Inflam. Connective Tissue Disease	0.322
79	0.045	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^{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
MA x (After 2003)	0.0218	-0.0932***	0.00283	-0.0164	-0.00513	0.0123	0.0327*	-0.00694	-0.00193
	[0.0155]	[0.0185]	[0.0146]	[0.0153]	[0.0142]	[0.0152]	[0.0187]	[0.0191]	[0.0145]
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-specific trends

	(1) gen	(2) costs	(3) follow	(4) concern	(5) info	(6) specialist	(7) phone	(8) avail	(9) sameloc
MA x (After 2003)	0.0653***	0.141***	0.0492***	0.0318**	0.0658***	0.0667***	0.0900***	0.0525***	0.0597***
	[0.0155]	[0.0184]	[0.0146]	[0.0153]	[0.0142]	[0.0152]	[0.0187]	[0.0191]	[0.0145]
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, year, female, race, disabled, education, income and Medicaid status. 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, year, female, race, disabled, education, income and Medicaid status. . 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, year, female, race, disabled, education, income and Medicaid status. 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 Elderly x After	0.000247	-0.0127				
	[0.0164]	[0.0176]				
Δ MA share x Elderly x After			0.00364	0.0256		
			[0.159]	[0.169]		
Δ_2 MA share x Elderly x After					0.111	0.107
					[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:

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

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.[1] 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.[2]

[1] We prefer this specification to simply regressing $\Delta Expenditure_{it}$ as the lagged expenditure controls in equation (1) 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.

[2] 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 99^{th} 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.[3] 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.

[3] 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 II. 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 **?** 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.[4]

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.

1.1 Basic framework and assumptions

1.1.1 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.[5] Both v and b are in units of absolute dollars.[6] 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 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

[4] 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 **?** finds in the German context.

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

[6] 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 **?**, 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 VI.

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.[7]

1.1.2 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 $p''(\cdot) = 0$.[8]

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.[9]

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*

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

.

[7]Whether the HMO model is actually more efficient than the fee-for-service model even absent selection effects is an open question. **?** 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.

[8]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 $p(\cdot)$.

[9]As we discuss in Section I, 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 V reports our empirical results with and without these temporary payments.

1.1.3 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., **?** 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:

$$\mathbb{E}(\pi) = \underbrace{(1-\Omega)\bar{p} + \Omega p(b)}_{\text{capitation payment}} - \left(\underbrace{b+v}_{\text{FFS cost}}\right) - \underbrace{c(b,v)}_{\text{screening cost}} . \tag{2}$$

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

1.2 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 (2) with respect to b and v are given by

$$[b] \quad : \quad \Omega p'(b^*) - c_b(b^*, v^*) = 1 \tag{3}$$

$$[v] \quad : \quad -c_v(b^*, v^*) = 1 \tag{4}$$

Totally differentiating equation (3) with respect to Ω yields

$$p'(\cdot) + \Omega p''(\cdot) \frac{\partial b^*}{\partial \Omega} - c_{11}(\cdot, \cdot) \frac{\partial b^*}{\partial \Omega} - c_{12}(\cdot, \cdot) \frac{\partial v^*}{\partial \Omega} = 0 \tag{5}$$

Similarly, equation (4) yields:

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

or

$$\frac{\partial v^*}{\partial \Omega} = -\frac{c_{bv}}{c_{vv}} \frac{\partial b^*}{\partial \Omega}. \tag{6}$$

Substituting equation (6) into (5) gives:

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

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

$$\frac{\partial b^*}{\partial \Omega} = \frac{\overbrace{p'}^{+\text{ as cap payments increase in } b}}{-\Omega p''(\cdot) + \frac{\overbrace{(c_{bb} c_{vv}) - c_{bv}^2}^{+\text{ by convexity of } c(\cdot,\cdot)}}{\underbrace{c_{vv}}_{+\text{ by convexity of } c(\cdot,\cdot)}}} \tag{7}$$

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 (6) 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.[10]

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:

$$\phi(\Omega) = \underbrace{\Omega p(b^*(\Omega)) + (1-\Omega)\bar{p}}_{\text{capitation payment}} - \underbrace{(b^*(\Omega) + v^*(\Omega))}_{\text{actual costs}} \tag{8}$$

Differentiating with respect to Ω gives:

$$\phi'(\Omega) = \Omega p' \frac{\partial b^*}{\partial \Omega} + p(b^*) - \bar{p} - \frac{\partial b^*}{\partial \Omega} - \frac{\partial v^*}{\partial \Omega} \tag{9}$$

Rearranging and substituting $\frac{\partial v^*}{\partial \Omega} = -\frac{c_{bv}}{c_{vv}} \frac{\partial b^*}{\partial \Omega}$ from equation (6) yields

$$\phi'(\Omega) = [p(b^*) - \bar{p}] + \frac{\partial b^*}{\partial \Omega}(\Omega p' - 1 + \frac{c_{bv}}{c_{vv}}) \tag{10}$$

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 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. ■

[10]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.

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 (2) and q is the number of enrollees the firm has.

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

$$[b] \quad : \quad q_b(b,v)\pi(b,v,\Omega) + q(b,v)\underbrace{(\Omega p' - 1 - c_b(b,v))}_{\pi_b} \tag{11}$$

$$[v] \quad : \quad q_v(b,v)\pi(b,v,\Omega) + q(b,v)\underbrace{(-1 - c_v(b,v))}_{\pi_v} \tag{12}$$

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 (11) and (12) 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 (6), 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 (8) 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

$$\pi'(\Omega) = p(\cdot) - \bar{p} \tag{13}$$

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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