Physicians' financial incentives and treatment choices in heart attack management

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Using a large set of private health insurance claims, we estimate how physicians' financial incentives affect their treatment choices in heart attack management. Different insurance plans pay physicians different amounts for the same services, generating the required variation in financial incentives. We begin by presenting evidence that, unconditionally, plans that pay physicians more for more invasive treatments are associated with a larger fraction of such treatments. To interpret this correlation as causal, we continue by showing that it survives conditioning on a rich set of diagnosis and provider-specific variables. We perform a host of additional checks to verify that differences in unobservable patient or provider characteristics across plans are unlikely to be driving our results. We find that physicians' treatment choices respond positively to the payments they receive, and that the response is quite large. If physicians received bundled payments instead of fee-for-service incentives, for example, heart attack management would become considerably more conservative. Our estimates imply that 20 percent of patients would receive different treatments, physician costs would decrease by 27 percent, and social welfare would increase.

KEYWORDS. Physician incentives, physician treatment choices, health insurance, heart attack management, fee-for-service payments.

JEL CLASSIFICATION. I11, I13, L20.

1. INTRODUCTION

The United States performs 96 percent more MRI (magnetic resonance imaging) exams, 81 percent more knee replacements, 25 percent more cesarean sections, and 101 percent more coronary angioplasties than the Organization for Economic Cooperation and Development (OECD) average (OECD (2011)).[1] While this is partly due to cross-country dif-

[1]Cesarean sections are per live births; other statistics are per capita.

ferences in health, income, and tastes, many believe that physicians' financial incentives also play a role (Orszag and Ellis (2007), Emanuel and Fuchs (2008), Garber and Skinner (2008)). Physicians may be responding to a fee-for-service payment system that rewards them for performing costly, sophisticated treatments. In the gray area of medicine where it is not clear what treatment is in the patient's best interest, financial incentives might prove decisive (Chandra, Cutler, and Song (2011)).

Our goal in this paper is to estimate these payment responses in the context of heart attack management, using a large administrative data set of private health insurance claims paid by insurers and self-insured firms. With a discrete choice model of physician behavior, we quantify how heart attack treatment decisions depend on the payments physicians expect to earn from each potential treatment, and how treatments would change in response to different financial incentives such as bundled payments.[2] The model also allows us to evaluate the likely effect of counterfactual payment regimes on the cost of care and social welfare. Our results suggest that fee-for-service incentives induce a substantial and social-welfare decreasing shift toward more expensive treatments.

Our empirical strategy is motivated by two key correlations in the data. First, different health insurance plan types, like health maintenance organizations and preferred provider organizations, pay different amounts for the same treatments. Second, treatment choices vary with this variation in payments. Some plan types tend to pay relatively more for aggressive treatments, and patients in those plan types tend to receive aggressive treatments more often.

Any attempt to interpret these simple correlations as causal has to confront the obvious concern that patients and providers are not assigned to insurance plan types at random. Plan types that pay more for aggressive treatments may be more likely to attract patients with severe heart attacks or may tend to contract with physicians who prefer to treat aggressively. Fortunately, our data contain a rich set of control variables about the patient, the heart attack episode, and the physician, mitigating much of the selection concerns. These variables include the kind of heart attack and where in the heart it occurs; comorbidities such as hypertension, diabetes, and obesity; previous diagnoses, treatments performed, and health care expenditures; and the provider's average resource use, as measured by inpatient expenditures and length of hospital stays. The main identifying assumption is that conditional on our observables, heart attack treatment choices are related to insurance plan type only to the extent that plan type changes the prices paid for physicians' services.

Following Cutler, McClellan, and Newhouse (2000), who make a related identifying assumption, we note that this is considerably weaker than ruling out adverse selection.[3] Because we condition on comprehensive health status data, we can allow for selection across plan types on diagnoses recorded in our data. The assumption does, however, place some restrictions on selection across plan types. A series of checks investigates whether our estimates might reflect selection, rather than the causal effect of prices on

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[2]Bundled payments are a fixed payment for the entire episode of care, regardless of which services the patient receives.

[3]Cutler et al. rule out patient selection on unobservables across plan types, as in our analysis, but make no assumption about the mechanism by which plan type affects treatments.

treatment choice. We analyze hospital admission rates across plan types for heart attacks and other serious conditions, and find that selection on unobservable sickness is unlikely to explain our results. We show that differences in cost-sharing across plan types are mostly irrelevant for heart attack treatment choice, as on the margin, the patient will generally not be contributing toward his health care bills. We examine the possibility that physicians with different practice styles are attracted to different plan types, beyond what our controls can capture, and find that our results do not support such selection.

We also explore physicians' response to financial incentives under a different exogeneity assumption, using variation in insurance plan type across *employers* rather than across individuals.[4] The assumption is that patients' choice of employer is unrelated to employers' plan type enrollment at the time of the patients' heart attack. This seems reasonable, especially because people often choose their employers long before they know they have heart disease, and changes in the health insurance industry make it difficult to predict what plans an employer would offer years in the future. We still reject the hypothesis that physicians are uninfluenced by financial incentives.

Inference in this setting is complicated by missing payment data. Health care providers do not submit claims for treatments that are not performed. We do not observe how much physicians would get paid for performing angioplasty on patients who actually receive medical management, yet the angioplasty payments may affect treatment choice. To measure the effect of payment on treatment choice, we need to estimate the "first stage," that is, how plan types affect payments. But if plan types affect payments, and payments affect choices, missingness of payments is correlated with plan type. Changing plan type has a causal effect on payments, but it also changes the patient mix receiving a given treatment. Regressing observed payments on plan types is thus subject to selection bias. While we study the privately insured, this problem would also be present with Medicare or Medicaid data.[5]

Each treatment is a collection of services. "Angioplasty" may involve electrocardiograms (ECGs), X-rays, and physician consultations, as well as the angioplasty itself. Our data have detailed information on the quantities used and unit prices of these underlying services. This disaggregated information plays a crucial role in dealing with the missing payments problem. We first estimate the effect of plan type on *service* prices. Together with data on how much of each service is typically used in each treatment, we show how these estimates allow us to infer the effect of plan type on overall treatment payments. With estimates of the first stage parameters, we can recover the effect of changing payments on treatment choices.

Physicians' treatment choice is represented by a multinomial probit model. We estimate the choice model and the payment equations simultaneously by Bayesian meth-

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[4]Cardon and Hendel (2001) test for adverse selection assuming employment choice is unrelated to health status. Polsky and Nicholson (2004) and Shafrin (2010) analyze the effect of across-employer plan type differences on utilization and surgery rates.

[5]The difficulty in estimating price responses when the prices of unchosen goods are missing arises in other contexts. Erdem, Keane, and Sun (1999) study the case of scanner panel data on household purchasing behavior.

ods, using the Gibbs sampler.[6] The results, which are robust in size across widely different specifications, indicate that increasing the price paid for a treatment increases the frequency with which it is performed. The own-price elasticities in the main specification vary from 0.3 to 0.9, depending on the treatment. Physicians' price responsiveness appears to decrease when they treat sicker patients.

Our model predicts that if physicians received bundled payments instead of facing fee-for-service incentives, heart attack management would be considerably more conservative: 18 percent fewer patients would receive angioplasty or bypass surgery. This is roughly equal to the difference in the incidence of these treatments between the United States and France, and half the difference between the United States and Israel, found in an international clinical trial (Gupta et al. (2003)). The cost of care would decline by about 27 percent. Extrapolating to the United States as a whole, this corresponds to a $5 billion reduction in health care expenditure each year. Our estimates account for the cost differences between the average and marginal treatment recipients.

Despite limited information on quality of life posttreatment, our model still permits welfare analysis: 20 percent of patients would receive different treatments under bundled payments and fee-for-service. We show how these patients' change in welfare from bundled payments depends on physicians' disutility of labor and thus their desire to shirk. A back-of-the-envelope calculation suggests that any welfare losses patients might experience from bundled payments are likely to be smaller than the cost savings from more conservative treatment choices.

Much evidence exists on physicians' response to financial incentives generally.[7] Rice (1983), Escarce (1993), Yip (1998), and Clemens and Gottlieb (2014) estimate how changes in Medicare reimbursement rates affect aggregate quantities of health care services. Helmchen and LoSasso (2010) and Melichar (2009) analyze the effect of fee-for-service payments on the number of patient encounters office-based physicians schedule and the time they spend per encounter. But possibly because of the challenge posed by missing payment data, very few studies explicitly quantify physicians' substitution between different treatments when their payments change, or develop a framework that can measure the overall effect of fee-for-service incentives on the distribution of treatments. Perhaps closest to this paper are Gruber, Kim, and Mayzlin (1999), who find that cesarean deliveries are more common if they are highly reimbursed relative to normal deliveries, and Dickstein (2012), who finds that capitated physicians tend to choose drugs that require fewer follow-up visits when treating depression. Cutler, McClellan, and Newhouse (2000) examine heart attack treatment choices across plan types, but do not estimate physicians' payment response.

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[6]The estimates can be understood as summarizing the posterior parameter distribution, but also have the usual frequentist interpretation. Other applications of Bayesian methods in health economics include Munkin and Trivedi (2003) and Deb, Munkin, and Trivedi (2006a, 2006b), who study the effect of insurance on health care utilization, and Geweke, Gowrisankaran, and Town (2003), who analyze hospital quality.

[7]A related literature treats hospitals' response to incentives (Hodgkin and McGuire (1994), Finkelstein (2007), Acemoglu and Finkelstein (2008), Dafny (2005), Kim (2011)). Ho and Pakes (2012) and Swanson (2012) examine how physician incentives affect the hospital referral decision. Ho and Pakes' study is of particular interest here, as they also use variation in financial incentives generated by health insurance.
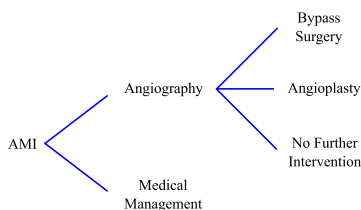
Figure 1. Acute myocardial infarction treatments. Adapted from Cutler, McClellan, and Newhouse (2000). Some patients may receive another treatment if the above are unsuitable or inadequate.

2. Heart attack management

Figure 1 (adapted from Cutler, McClellan, and Newhouse (2000)) presents the main treatment and diagnosis options for a heart attack, i.e., acute myocardial infarction (AMI). An AMI is caused by an arterial blockage interrupting blood flow to the heart. Treatments aim to restore the heart's blood supply. Medical management involves administering drugs, often including aspirin, beta-blockers, and thrombolytics. Angiography, or diagnostic catheterization, is an imaging technique in which a catheter is guided to the coronary arteries to inject an X-ray dye, allowing X-rays to show blood flow around the heart and reveal arterial occlusions. Depending on the severity of disease this reveals, physicians may choose no further intervention, angioplasty, or bypass surgery. In an angioplasty, or interventional catheterization, the cardiologist inflates a balloon at the site of the blockage to widen the interior of the artery and typically also leaves a stent to keep the vessel open. A coronary artery bypass surgery involves grafting a vein or artery taken from elsewhere in the body to the coronary artery, bypassing the blockage. Some patients have another form of major surgery because the other alternatives are unsuitable or because they also suffer from another heart condition.

The American College of Cardiology and the American Heart Association produce joint guidelines on AMI management (Antman (2004), Anderson et al. (2007)). Delays in coordinating personnel for catheterization or inexperienced interventional cardiologists make medical management more attractive, for example, while hypotension favors an initial angiography. If an angiography reveals minimal coronary artery disease, no further intervention may be necessary. Angioplasty is otherwise a common choice, but three vessel disease and diabetes increase the benefit from bypass surgery relative to angioplasty.

Even with such recommendations, it is not always clear what is in the patient's best interest. The guidelines themselves state as much: "Despite the wealth of reports on reperfusion for STEMI [ST elevation myocardial infarction], it is not possible to produce a simple algorithm, given the heterogeneity of patient profiles and availability of resources in various clinical settings at various times of day" (Antman (2004)). Doctors may not often recommend entirely inappropriate treatments purely for their own per-

sonal benefit.[8] But physicians with patients on the margin between treatments might be influenced by the payments attached to the options.[9,10]

3. Treatments and payments data

3.1 *Data sources*

Our primary data source is the Thomson Reuters MarketScan data base, a large administrative collection of claims paid by private health insurance companies and self-insured firms.[11] MarketScan's data are primarily on employees of large, Fortune 500 firms, with a focus on firms in the South and Midwest. The data base contains records on over 90 million person-years of insurance enrollment from 2002 to 2007, representing almost 10 percent of the population of Americans who obtain employer-sponsored health insurance.[12] We select inpatient admission records from this period where the primary diagnosis is AMI.

A key advantage of the MarketScan data base is that the payment variables are actual transaction prices, not list prices, which may not reflect insurer-specific discounts. Each record also has information on patient demographics and insurance plan type, the type and quantity of services used, and the corresponding payments to the hospital and physicians, and the physicians' diagnoses. Patients can be identified over time, so medical history is available for each patient from when they enter the sample. Appendix A describes in detail how the sample is constructed, and provides the diagnosis and service codes used to identify AMI and its treatments. The final sample contains 66,014 AMI. We use the Health Resources and Services Administration's Area Resource File for additional county-level information on demographics, ischemic heart disease mortality, hospital and physician characteristics, and per capita surgery rates.

There are four major insurance plan types in these data. In order of increasing restrictiveness of the provider network and decreasing patient choice, they are comprehensive, preferred provider organizations (PPOs), point of service (POSs), and health maintenance organizations (HMOs). They vary depending on whether patients are incentivized to use a particular network of providers, whether the insurer makes any con-

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[8]Although counterexamples exist (Devi (2011)). Chan et al. (2011) review over half a million angioplasties, and judge 12 percent of procedures for nonacute patients to be inappropriate, with a further 38 percent of uncertain value.

[9]Lucas et al. (2010) survey cardiologists and find that they almost uniformly deny being influenced by money. Others are skeptical that physicians are unaffected: "The United States is just about the only developed country where health care is delivered on a fee-per-service basis and we very liberally incentivize physicians for doing invasive procedures. The economic incentives are just too strong" (Steven Nissen, Chief of Cardiovascular Medicine, Cleveland Clinic, quoted in Devi (2011)).

[10]In other cases there is a clear consensus but it is not followed. Many patients, for example, fail to receive aspirin and beta-blockers post-AMI (Jencks et al. (2000), Baicker and Chandra (2004), Chandra and Staiger (2007)).

[11]The MarketScan data base is used extensively in the public health literature (see citations in Adamson, Chang, and Hansen (2008)). Studies in health economics using MarketScan include Dor, Grossman, and Koroukian (2004), Ho (2006, 2009), and Dickstein (2012, 2013).

[12]On average, about 158 million people received employer-sponsored health insurance yearly over this period (see Kaiser Family Foundation and HRET (2002, 2007)).

TABLE 1. Plan type characteristics.

	Network	Out-of-Network Coverage	Primary Care Physician
HMO	✓	x	✓
POS	✓	✓	✓
PPO	✓	✓	x
Comprehensive	x	n/a	x

Note: "Network" means insurers incentivize patients to use certain providers. "Out-of-Network Coverage" means plan types make some contribution to out-of-network expenses. "Primary Care Physician" means plan types require referrals to specialists to be made through primary care physicians.

tribution to out-of-network services, and whether a primary care physician controls referrals to specialists. Table 1 summarizes these differences.

3.2 *Insurance plan types and physician payments*

Fee-for-service billing   Insurers in the MarketScan data pay physicians and hospitals by fee-for-service or by capitation.[13] In 2007, over 95 percent of AMI patients in our data are covered by insurance that paid entirely by fee-for-service. Capitation is more common for primary care physicians than specialists like cardiologists and cardiac surgeons (Kongstvedt (2007)). We drop the few patients who are recorded as having insurance that pays by capitation.[14]

The provider group contracting with the insurer may be paid by fee-for-service, while individual physicians are paid in some other way. Most evidence suggests, however, that the fee-for-service incentives filter through to the physicians themselves. A 2007 survey found that 73 percent of cardiology practices were physician-owned (American College of Cardiology (2007)). For 84 percent of surgical specialists, reimbursements depend on the quantity of services they personally supply.[15]

Physicians submit claims to private health insurers by listing the services performed. Under the Health Insurance Portability and Accountability Act of 1996, these claims must use a standardized coding system called the Current Procedural Terminology (CPT). Medicare reimburses physicians by associating "relative value units" to the CPT codes and multiplying these units by a conversion factor to find the dollar payment amount. Private insurers' reimbursement schemes are generally modeled on this system (Ginsburg (2010)), but may differ in some respects. A large survey of private plans found that most use more than a single conversion factor (MedPAC (2003)), so their pay-

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[13]Capitation is a fixed payment from the insurer to the provider for each enrollee-month, irrespective of services used.

[14]MarketScan started recording whether services are paid by fee-for-service or capitation in 2007. For previous years there may remain a small fraction of patients in our sample whose insurance pays by capitation. This appears to be unimportant for our results: when we allow the price responsiveness coefficient β^p defined in Section 4 to vary by year, the estimate for 2007 is close to other years' estimates.

[15]Author's calculations from the Center for Studying Health System Change's Physician Survey, 2004–2005. See also Reschovsky and Hadley (2007).

ments need not be proportional to Medicare's. Using multiple conversion factors allows plans to control how they reimburse each category of service (e.g., evaluation and management versus surgery).

Physician–insurer negotiations and physician payments Insurers negotiate with in-network physicians over the prices for each service (defined by the CPT codes). For physicians who have limited market power, these "negotiations" take a simple form: the insurer makes a take-it-or-leave-it offer of a fee schedule, and the physician decides whether to join the network on those terms. Physicians sometimes combine into large groups to give themselves more bargaining power, and may succeed in extracting higher payments from the insurer (Kongstvedt (2007), Ginsburg (2012)). In a related work, Ho (2009) examines the formation of hospital–insurer networks.

Insurer bargaining power is likely to vary by plan type. The restrictiveness of the insurer's network is one determinant of the concessions it can extract from physicians.[16] The ability to better control costs may be a major reason why restrictive networks exist at all (Dranove, Shanley, and White (1993), Gal Or (1997), Town and Vistnes (2001)). HMOs have tighter networks than PPOs, so all else equal, exclusion from a HMO network is likely to lead to a greater fall in revenue for a physician than exclusion from a PPO network. More restrictive plan types should therefore be able to obtain more favorable contracts from physicians. Other factors like insurer market share and number of enrollees may also affect payments.

Using data on services billed, we assign each admission to one of the five treatment groups. Table 2 displays the mean and standard deviation of total physician payments, overall and by plan type and treatment. Physician payments are all payments the insurer makes to physicians involved in the treatment of that patient, which may include cardiologists, emergency physicians and cardiac surgeons. Averaging over patients, total physician payments for an AMI are around $3400. They are lower in the more restrictive HMO and POS plans than the less restrictive PPOs. The least restrictive comprehensive plans have the lowest payments. This is partly due to selection on observables across plan types, but also suggests a role for factors other than network structure in determining bargaining power.

For angioplasty, HMOs and PPOs both pay physicians slightly under $3000. But average payments for medical management are considerably higher in HMOs, at about $1800, than in PPOs, at $1200, and bypass payments are larger in PPOs, at $9100, than HMOs, at $8500. These patterns suggest that physicians' financial incentives to treat intensively vary by plan type. In particular, PPOs seem to pay relatively large amounts, and HMOs relatively small amounts, for aggressive interventions. Insurers may use their bar-

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[16]Sorensen (2003), Dor, Grossman, and Koroukian (2004), and Wu (2009) make this point in the context of insurer negotiations with hospitals, and show considerable price variation within a hospital across insurers.

TABLE 2. Total physician payments by insurance plan type and treatment.

	MM	A'graphy	A'plasty	Bypass	Other	All Treatments
HMO	1836	1782	2888	8497	7104	3291
	(2525)	(1980)	(2421)	(4182)	(4950)	(3463)
POS	1115	1687	2841	8787	7919	3432
	(1355)	(1345)	(1886)	(3841)	(4871)	(3388)
PPO	1152	1670	2958	9087	7852	3537
	(1405)	(1344)	(2153)	(4164)	(4956)	(3530)
Comprehensive	1048	1600	2506	8188	6670	3066
	(1367)	(1407)	(1812)	(3774)	(4771)	(3231)
All plan types	1264	1678	2878	8839	7604	3426
	(1697)	(1459)	(2128)	(4085)	(4941)	(3470)

Note: Total physician payments are the sum in dollars of all payments made to all physicians involved in treating the AMI, from the patient and the insurer. Each cell contains the mean and standard deviation of total payments for that plan type and treatment.

gaining power not just to reduce the level of payments, but also to induce physicians to treat more conservatively.[17, 18]

The finding that some plan types, like HMOs, pay relatively more for conservative treatments is quite robust. Insurers reimburse the treatment facility—usually a hospital—separately from physicians. Appendix Table A shows that similar patterns exist for facility payments too. HMOs, POSs, and PPOs pay comparable amounts to facilities for medical management, but PPOs pay more for the other treatments. Our Supplementary Appendix, available in a supplementary file on the journal website, http://qeconomics.org/supp/365/supplement.pdf, shows that differences in reimbursement rates across plan types occur not only at the treatment level, but also at the level of the individual service. In addition, it demonstrates that similar differences in relative payments exist for four other common conditions too (prostate cancer, breast cancer, inguinal hernia, and spinal disc herniation).

3.3 *Insurance plan types and treatment choices*

Just as payments vary by plan type, so does the distribution of AMI treatments. Table 3 is a cross-tabulation of the sample by plan type and treatment. Medical management, for example, is more popular in HMOs than PPOs (17 percent vs. 11 percent), and the opposite is true for angioplasty (52 percent vs. 57 percent). Table 4 shows that these differences remain after controlling for a rich set of observables, including state and year

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[17]In related work, Town, Feldman, and Kralewski (2011) find that physician groups in stronger bargaining positions are more likely to be paid by fee-for-service than capitation. Dafny, Duggan, and Ramanarayanan (2012) find that insurers may use their bargaining power to affect how care is provided by substituting nurses for physicians.

[18]As a point of comparison, the Medicare payments for these patients would be about 23 percent lower overall but only 12 percent lower for medical management, suggesting they tend to incentivize conservative treatment somewhat more than the average private insurer.

TABLE 3. Treatments and insurance plan types.

		MM	A'graphy	A'plasty	Bypass	Other Surgery	All Treatments
HMO	%	17.1	15.7	51.8	7.2	8.3	100.0
	N	1559	1432	4733	659	761	9144
POS	%	12.0	15.1	55.2	8.5	9.2	100.0
	N	846	1070	3906	600	652	7074
PPO	%	10.5	15.3	56.5	8.1	9.6	100.0
	N	4258	6233	22,968	3281	3913	40,653
Comprehensive	%	14.5	15.8	51.0	9.3	9.3	100.0
	N	1329	1448	4666	852	848	9143
All plan types	%	12.1	15.4	54.9	8.2	9.4	100.0
	N	7992	10,183	36,273	5392	6174	66,014

Note: The % rows display the percentage of AMI patients in the corresponding plan type who received the corresponding treatment. The N rows display the number of AMI patients with the corresponding plan type and treatment. Appendix A describes the codes used for categorizing diagnoses and treatments.

TABLE 4. Treatments and insurance plan types: regression results.

$N = 66{,}014$	MM	A'graphy	A'plasty	Bypass	Other Surgery
HMO	3.44	0.20	−2.80	−0.74	−0.10
	(0.39)	(0.44)	(0.58)	(0.34)	(0.35)
POS	2.84	−0.01	−2.62	−0.35	0.14
	(0.43)	(0.49)	(0.64)	(0.37)	(0.39)
Comprehensive	2.60	0.68	−3.08	−0.11	−0.08
	(0.40)	(0.45)	(0.60)	(0.35)	(0.36)

Note: An observation in these regressions is an AMI patient. Each column corresponds to a different linear probability model with 100 times the indicator for the corresponding treatment as the dependent variable. PPO is the omitted plan type category. All regressions control for the X_i described in Section 4.3, including clinical and provider covariates.

fixed effects, patient demographics and clinical information, and provider covariates.[19] This kind of treatment pattern suggests that payments affect treatment choice. For PPO patients, the expensive, invasive procedures are relatively more remunerative to physicians than cheaper, simpler alternatives. Physicians seem correspondingly more likely to treat PPO patients aggressively.

While these correlations are suggestive, our empirical strategy allows us to see if these patterns hold up, on average, when comparing all treatments across all plan types and after dealing with the problem of missing payment data for counterfactual treatments. It also allows us understand how counterfactual reimbursement structures influence choices, welfare, and costs.

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[19]Langa and Sussman (1993), Every et al. (1995), Sada et al. (1998), and Canto et al. (2000) also find that invasive treatments for AMI patients are less common in HMOs than other plan types; Cutler, McClellan, and Newhouse (2000) do not, attributing lower expenditures in HMOs to lower unit prices rather than treatment differences.

4. Empirical strategy

4.1 *Overview*

Our empirical strategy has two steps. First, we estimate how treatment payments vary by plan type. Simple ordinary least squares (OLS) would yield inconsistent estimates because payments are systematically missing. We account for the missing payments by using the detailed price and quantity data we have on the services that comprise a treatment. Intuitively, our strategy is similar to fixing the bundle of services that make up the typical angioplasty, for example, and evaluating the total payments for this bundle at the service price schedules of different plan types. This isolates the effect of service prices from service quantities. Second, we estimate physicians' responses to the across-plan type variation in treatment payments found in the first step.

4.2 *Model and assumptions*

Service prices and treatment payments   Let $p_{i,j}$ denote the total payments physicians would receive if they were to treat patient i with treatment j for $j = 1, \ldots, 5$ corresponding to medical management, angiography, angioplasty, bypass surgery, and other surgery. Let $\pi_{i,s}$ denote the service price patient i's insurance pays for service s and let $q_{i,s,j}$ denote the number of units i would receive of service s if treated with j. The total payment $p_{i,j}$ is the sum of all service revenues: $p_{i,j} = \sum_s \pi_{i,s} q_{i,s,j}$. For each service $s = 1, \ldots, S$, log service prices are given by

$$\ln \pi_{i,s} = W_i' \gamma_s^W + \text{Ins}_i' \gamma_s^{\text{Ins}} + v_{i,s}. \tag{4.1}$$

The variables in Ins_i are insurance plan type indicators interacted by region (Northeast, North Central, South, and West), so that the effect of plan type is allowed to vary geographically.[20],[21] The PPO is the omitted category in each region. The variable W_i collects other variables that might affect service prices, like state and year fixed effects. The $v_{i,s}$ term is determined by the particular insurance plan patient i has, as opposed to his general plan type. Reflecting actual reimbursement practices, $\pi_{i,s}$ is not indexed by j, as the amount a plan type pays for a service does not depend on the treatment of which it is part.

For each treatment j, log total payments are

$$\ln p_{i,j} = X_i' \alpha_j^X + \text{Ins}_i' \alpha_j^{\text{Ins}} + u_{i,j}. \tag{4.2}$$

Demographic, clinical, and provider covariates are collected in X_i. The variables in W_i and X_i are described fully in Section 4.3.

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[20]There is considerable geographic variation in plan type market shares (Baker (1999), Shen, Wu, and Melnick (2010)), suggesting that the effect of plan type on payments within a region varies by region.

[21]We include state fixed effects in pricing *and* treatment choice equations, so we do not use across region variation in the overall price level over all plan types to identify price responsiveness.

Treatment choices and treatment payments   Over financial outcomes, physicians are risk-averse expected utility maximizers with log Bernoulli utility. They know X_i, Ins_i, and the payment equations (4.2), and have a signal $s_{i,j}$ of the payment error $u_{i,j}$. Utility from financial incentives from patient i and treatment j is

$$\mathbb{E}(\ln p_{i,j} \mid X_i, \text{Ins}_i, s_{i,j}) = X_i' \alpha_j^X + \text{Ins}_i' \alpha_j^{\text{Ins}} + \mathbb{E}(u_{i,j} \mid s_{i,j}). \tag{4.3}$$

Utility from nonfinancial factors is $X_i' \beta_j^X + e_{i,j}^0$. Demographic, clinical, and provider covariates may affect how much a patient is likely to benefit from a treatment, and how much effort physicians must exert in providing that treatment. Both are captured by the term $X_i' \beta_j^X$. Overall physician utility is the sum of financial and nonfinancial components:

$$U_{i,j} = \mathbb{E}(\ln p_{i,j} \mid X_i, \text{Ins}_i, s_{i,j})\beta^p + X_i' \beta_j^X + e_{i,j}^0 \tag{4.4}$$

$$= X_i' \beta_j^X + \left(X_i' \alpha_j^X + \text{Ins}_i' \alpha_j^{\text{Ins}} \right)\beta^p + e_{i,j}, \tag{4.5}$$

where β^p is the weight physicians place on financial incentives and $e_{i,j} = e_{i,j}^0 + \mathbb{E}(u_{i,j} \mid s_{i,j})\beta^p$. Treatment j is chosen for i if and only if $U_{i,j} \geq U_{i,k}$ $\forall k$.[22] The plan type variables Ins_i only affect utility indirectly, through payments. The aim is to estimate β^p and the effect of changing payments on the distribution of treatments performed.[23]

The principal decision-maker in AMI treatment, whose utility is modeled by (4.5), is generally the cardiologist. But the cardiologist may not receive all physician fees associated with the treatment choice. Bypass surgery, for example, is performed by cardiac surgeons rather than cardiologists. Since the payment of others seems less likely to influence one's decision than one's own payment, the effect of a physician's own payments on his choices may be larger than our results will suggest. Our estimates are suited to the counterfactuals we explore, where changes in total payment for a treatment need not go solely to the principal decision-maker.

We allow payment and utility errors to be correlated. This is important for two reasons, corresponding to the two terms in the utility error $e_{i,j} = e_{i,j}^0 + \mathbb{E}(u_{i,j} \mid s_{i,j})\beta^p$. First, payments may be endogenous in the sense that they may be correlated with the benefits the treatment confers to the patient (correlation between $u_{i,j}$ and $e_{i,j}^0$). Second, payments may directly affect utility (correlation between $u_{i,j}$ and $\mathbb{E}(u_{i,j} \mid s_{i,j})\beta^p$).

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[22]In our model, physician payments affect treatment choice, but hospital payments do not. This is informed by the classical view of physician–hospital relations in the United States, according to which the hospital is the physician's workshop and can exert relatively little control over the physician, who has ultimate responsibility for treatment choices (Pauly and Redisch (1973), Starr (1982), Burns and Muller (2008), Reinhard (2008)). There are legal reasons for this: tort law and hospital bylaws often restrict hospitals' ability to interfere with physicians' decisions, even if the physicians are hospital employees (Elhauge (2010)). Note that the model can allow for physicians to receive payments from hospitals that are proportional to their nonhospital payments, as in this case hospital payments would not affect physicians' treatment choice.

[23]The parameter β^p is constant across j. This restriction is consistent with the data: we also estimate the model in which β_j^p may vary by j, and we fail to reject the hypothesis that β_j^p is constant across j (*p*-value of 0.75).

We observe $\ln p_{i,j}$ if and only if j is chosen for i. This is a multinomial generalized Roy model (Heckman and Vytlacil (2007)). There is a multinomial choice equation determining which payment is observed and, unlike the basic Roy model, payment is not the only determinant of choice. Observed payments are a selected sample of all payments, which makes OLS estimation of the payment equation (4.2) inconsistent. Changing plan types has a causal effect on service prices $\pi_{i,s}$ and thus treatment payments $p_{i,j}$, but it also changes the treatment cutoffs in the utility model and so changes the mean of the payment errors $u_{i,j}$.

As mentioned above, our empirical strategy proceeds in two steps. First, we estimate the effect of plan type on treatment payments, allowing for missing payments. We show how to obtain these estimates from estimates of the service price equations, that is, we use the γ_s^{Ins} in (4.1) to infer the α_j^{Ins} in (4.2). Second, we use the first step results to find physicians' payment response. Utilities can be written as $U_{i,j} = X_i'(\beta_j^X + \alpha_j^X \beta^p) + \text{Ins}_i'\alpha_j^{\text{Ins}}\beta^p + e_{i,j}$. Estimating the choice model with the α_j^{Ins} fixed at the values found in the first step gives an estimate of β^p. We now turn to the assumptions underpinning this empirical strategy.

Assumptions   Define $u_i = (u_{i,1}, \ldots, u_{i,5})$, $e_i = (e_{i,1}, \ldots, e_{i,5})$, and $v_i = (v_{i,1}, \ldots, v_{i,S})$. Let

$$w_{i,s,j,0} = \frac{\exp(W_i'\gamma_s^W + v_{i,s})q_{i,s,j}}{\sum_k \exp(W_i'\gamma_k^W + v_{i,k})q_{i,k,j}}$$

denote the share of total payment that would go to service s for patient i receiving treatment j if i were in the "base" plan type (so that $\text{Ins}_i'\gamma_s^{\text{Ins}} = 0$), where the choice of the base is arbitrary. We make the following assumptions throughout.

A1. The random variables (u_i, e_i, v_i) are independent across i.

A2. We have $(u_i, e_i) \sim N(0, \Sigma)$.

A3. We have $(u_i, e_i) \perp\!\!\!\perp (X_i, \text{Ins}_i)$.

A4. We have $v_{i,s} \perp\!\!\!\perp (W_i, \text{Ins}_i, i \text{ receives service } s)$ for all s.

A5. We have $w_{i,s,j,0} \perp\!\!\!\perp e_i \mid X_i, \text{Ins}_i$ for all j and s.

Assumption A1 is a standard assumption of independent sampling. Assumption A2 imposes normality of errors with unrestricted covariance matrix Σ, but the Supplementary Appendix gives a set of sufficient conditions for semiparametric identification. Assumption A3 implies independence of the utility errors and regressors. This ensures that the discrete choice model (4.5) can be estimated. In particular, the effect of payments on utility, β^p, can be estimated once the payment regression parameters α_j^{Ins} have been recovered. Assumption A3 requires, for example, that patients in different plan types who are identical on observables are not differently suited to the potential treatments. It also implies that plan type does not directly affect the service quantities in a particular treatment. If a patient would receive different kinds of angioplasties depending on whether he is in a PPO or a HMO, the angioplasty utility error would vary by plan type. Section 5 assesses this assumption.

As is typical with missing data problems, we must make assumptions about how what we observe relates to what we do not. We avoid the strong assumption that payments are missing at random conditional on the observables, instead requiring A4 and A5. Assumption A4 implies that there is no selection on the individual-specific service price errors $v_{i,s}$, so we can estimate the coefficients of the service price equations in (4.1) by simply running these regressions on the observed service data. The utilities $U_{i,j}$ depend on Ins_i, so treatment choice, and therefore service choice, may depend on plan *type*. Assumption A4, however, rules out physicians' service choice being influenced by the particular prices a patient's *plan* pays, which would introduce correlation between $v_{i,s}$ (which is determined by the patient's plan) and the event that i receives service s. This seems plausible, as a physician may perform dozens of different services for dozens of different plans. Keeping track of each service price for each plan would be a rather formidable task for the physician, and certainly much more difficult than learning how overall treatment payments vary by plan type.

Assumption A5 rules out selection on the service composition of treatments. The utility error $e_{i,j}$ may be correlated with the total payment $p_{i,j}$, but conditional on observables, it must be independent of the service composition of $p_{i,j}$ or the fraction of $p_{i,j}$ spent on any particular service. Physicians' decisions may be affected by the overall amount they stand to gain from the possible treatments, but not by the treatments' compositions. This means we can estimate the share of the total angioplasty payment that would go to chest X-rays on average across all patients, using only data on those patients who did in fact receive angioplasties. Assumption A5 greatly simplifies the analysis of physician choice. Instead of having to jointly model service quantity choice for almost 200 services, we can aggregate the services into treatments and focus only on the choice between treatments.

Assumption A4 means we can infer the effect of plan type on service prices, and A5 means we can infer the average service compositions of each treatment. With this information, we can approximate the effect of plan type on total treatment payments, α_j^{Ins}. Appendix B.1 presents the argument in detail, but the intuition is as follows: if we know the percentage change in each service price caused by plan types, and we know how important each service is on average in determining the overall treatment payment, we can infer the percentage change in the treatment payment caused by plan types.[24] Put otherwise, the service price regression parameters γ_s^{Ins} determine the percentage change in each of the terms in the sum $p_{i,j} = \sum_s \pi_{i,s} q_{i,s,j}$. With the average of the service composition terms $w_{i,s,j,0}$, we can find the average percentage change in the $p_{i,j}$. By A3, once these estimates of α_j^{Ins} are available, we can estimate the discrete choice model and the effect of payments on utility, β^p.

4.3 *Demographic, clinical, and provider covariates*

The covariates W_i are the service quantities (as insurers may charge different unit prices for different quantities), state and year fixed effects, and, where available, CPT modi-

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[24]Another approach to estimating the first stage would rely on instruments for treatment selection in addition to instruments for payments (Heckman and Vytlacil (2007)).

fier codes.[25] In all specifications, X_i includes state and year fixed effects, as well as a set of controls for the patient's age group (<40, 40–44, 45–49, 50–54, 55–59, >60), sex, and urban place of residence. The Health Resources and Services Administration's Area Resource File provides additional county-level information. All specifications control at the county level for the fraction of physicians reporting a medical (rather than a surgical) speciality, median age, median household income, ischemic heart disease mortality rates, the number of hospitals with adult interventional cardiac catheterization facilities per capita, the number of hospitals with adult cardiac surgery facilities per capita, the number of hospital beds for cardiac intensive care per capita, and the number of inpatient surgeries per capita.

In some specifications, X_i also includes clinical and provider covariates. Our clinical covariates contain information on the type of AMI (e.g., ST elevation myocardial infarction of anterolateral wall), all 29 Elixhauser comorbidities (Elixhauser et al. (1998)), cardiac dysrhythmia, cardiomyopathy, whether the patient is a smoker, and whether the record corresponds to the initial episode of care for the AMI.[26] Furthermore, many of the Elixhauser comorbidities have been linked in the medical literature with AMI severity, indicating that they may be good proxies for the kind of AMI suffered.[27]

Our covariates also contain information on medical and insurance history for the patients who are in-sample for at least six months prior to their AMI. We include an indicator variable for being in-sample during this period. For those who are, we include a continuous variable measuring inpatient expenditure, and indicator variables for incurring no inpatient expenditure, being admitted for any form of ischemic heart disease (not necessarily AMI) and the kind of treatment received, and changing insurance plan type.[28,29]

Plan types may contract with different kinds of physicians or hospitals. HMOs might prefer to contract with those who tend to treat conservatively. Including provider fixed effects to account for this selection is problematic. There are about 9000 different providers in the sample, so including fixed effects for each of the four normalized utility and five payment equations would involve estimating a nonlinear model with over

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[25]Physicians sometimes use CPT modifier codes to convey more information about how the procedure was performed. These codes may affect service prices.

[26]The Elixhauser comorbidities are congestive heart failure, valvular disease, pulmonary circulation disorders, peripheral vascular disease, hypertension, paralysis, other neurological disorders, chronic pulmonary disease, diabetes without chronic complications, diabetes with chronic complications, hypothyroidism, renal failure, liver disease, chronic peptic ulcer disease, HIV and AIDS, lymphoma, metastatic cancer, solid tumor without metastasis, rheumatoid arthritis/collagen vascular diseases, coagulation deficiency, obesity, weight loss, fluid and electrolyte disorders, blood loss anemia, deficiency anemias, alcohol abuse, drug abuse, psychoses, and depression.

[27]These comorbidities include congestive heart failure (Krumholz et al. (1999)), hypertension (Pedrinelli et al. (2012)), diabetes (Rytter, Troelsen, and Beck-Nielsen (1985)), chronic obstructive pulmonary disease (Kjøller et al. (2004)), peripheral vascular disease (Guerrero et al. (2005)), and renal failure (Beattie et al. (2001)).

[28]Because identical patients might receive different treatments and incur different inpatient expenditures in different plan types, the variable we use to control for inpatient expenditure is the patient's percentile among others in the same plan type, rather than the dollar amount.

[29]The International Classification of Diseases ICD-9 diagnosis codes used to identify any form of ischemic heart disease are 410, 411, 413, 414, and 786.

Table 5. Summary statistics: contemporaneous patient-level variables.

Age	54.45	Diabetes with chronic complications	0.02
	(7.28)	Obesity	0.05
Male	0.73	Smoker	0.14
Urban place of residence	0.75	Cardiac dysrhythmia	0.20
Congestive heart failure	0.13	Cardiomyopathy	0.03
Valvular disease	0.10	Initial episode of care	0.99
Hypertension	0.31	ST elevation	0.51
Chronic pulmonary disease	0.08	Some emergency dept. expenditure	0.72
Diabetes without chronic complications	0.15		
	$N = 66{,}014$		

Note: All variables are binary unless standard deviations are displayed. We use the Healthcare Cost and Utilization Project's description of Elixhauser comorbidities' ICD-9 codes to identify Elixhauser comorbidities.

80,000 parameters.[30] Because our sample size is around 66,000, we instead choose to control for provider characteristics parsimoniously, but in a way that is informed by the concerns about provider selection across plan types. We find each provider's percentile in the distribution of treatment intensity, for two measures of treatment intensity: mean inpatient expenditures and mean inpatient length of hospital stay. These two variables serve as our proxies for provider practice style.

Table 5 displays summary statistics of some variables contemporaneous with the AMI, including selected Elixhauser comorbidities. Because these patients are pre-Medicare, they are younger than the average AMI sufferer. The categories "ST elevation" and "Some Emergency Department Expenditure" are of particular interest. Section 7 presents evidence that physicians' payment responses are smaller for more severe AMI when severity is measured by these variables. Appendix Table B summarizes the contemporaneous country-level variables, as well as patients' histories prior to the AMI. Seventy-six percent of patients are in-sample for the six months preceding their AMI. About 15 percent of the sample were admitted for some form of ischemic heart disease. Insurance plan type changes in the period before the AMI are rare.

5. Assessing the identification strategy

Assumption A3 requires that insurance plan type does not directly affect treatment utilities. We consider whether this assumption is reasonable.

Selection on patients' unobservables

People may select into plan types on the basis of variables not observed in these data. HMO patients might be unobservably healthier than PPO patients, for example, making them relatively suited to medical management. This would lead to correlation between

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[30]By "provider," we refer to the hospital when hospital identifiers are available (45 percent of the sample), the principal physician when physician identifiers are available and hospital identifiers missing (31 percent of the sample), and the principal physician's practice zipcode when both hospital and physician identifiers are missing (24 percent of the sample).

the unobservable benefits from treatments and plan types, violating the restriction $e_i \perp\!\!\!\perp \text{Ins}_i$.

The clinical covariates described in the previous subsection are fairly rich. We observe the kind of AMI and where in the heart it occurs, as well as the full set of Elixhauser comorbidities, including hypertension, diabetes, and obesity. MarketScan is a panel data set, so we can also track medical history. Since all this is observable, it is less evident how selection on patients' unobservables might operate. At the time of choosing a plan type, people would have to know something about their health status, which affects the kind of AMI they are likely to suffer from, but which does not show up in their observed medical history (including the diagnostic, procedural, and expenditure data from previous inpatient visits and outpatient admissions) or recorded clinical information from the AMI itself. It is somewhat unclear what such factors might be, as Cutler, McClellan, and Newhouse (2000) note in their study of AMI treatment and managed care.

The assumption of no selection on unobservables in this setting is different from, and weaker than, the assumption of no adverse selection. Adverse selection typically refers to selection on variables on which the insurer does not price (Einav, Finkelstein, and Cullen (2010), Handel (2013)). We observe and control for many such variables, so patients may differ systematically by plan type along these dimensions. There may be any degree of adverse selection across plan types on the likelihood of suffering a health event that would be recorded in the data. Insurers do not freely price on these events almost by definition, since their purpose is to shield consumers from the full cost of medical bills. For example, we can allow for adverse selection on the probability of suffering an AMI, or on where in the heart the infarction occurs, or on whether it occurs in conjunction with diabetes.

One way to get a sense of how problematic selection on unobservable sickness is likely to be is to estimate how the probability of being admitted for various conditions, including AMI, varies by plan type. We estimate linear probability models of the form

$$\text{Adm}_i = V_i'\delta + \delta^{\text{Comp}}\text{Comp} + \delta^{\text{HMO}}\text{HMO}_i + \delta^{\text{POS}}\text{POS}_i + r_i.$$

An observation is a person-year in the MarketScan data enrolled in a comprehensive, HMO, POS, or PPO plan. The term Adm_i is 1000 if that person-year is admitted as an inpatient with a particular primary diagnosis and is 0 otherwise. Different primary diagnoses correspond to different regressions. The variable V_i includes controls for age, year, state, sex, urban place of residence, and the county-level data from the Area Resources File. Clinical data like hypertension and obesity are not available for all enrollees, as they are only recorded during a visit to a physician. The PPO is the omitted plan type.

The conditions we choose are among the leading causes of death in the United States.[31] Table 6 displays the estimates of the plan type coefficients from these regressions. The patterns are fairly consistent across diagnoses. Comprehensive and HMO enrollees are somewhat more likely to be admitted for these conditions, and POS enrollees

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[31]See http://www.cdc.gov/nchs/fastats/lcod.htm, accessed on 14 September 2012. Our chosen conditions cover seven of the ten leading causes of death. The other three are accidents, Alzheimer's disease, and suicide. These are less relevant as indicators of physical health for our sample of mostly under 65 year olds.

TABLE 6. Admission probabilities and insurance plan types: regression results.

(A) Heart Disease Related Conditions

$N = 60{,}321{,}748$	AMI	Other Acute & Subacute Ischemic Heart Disease	Angina Pectoris	Other Chronic Ischemic Heart Disease
HMO	0.092	0.009	0.006	0.130
	(0.010)	(0.004)	(0.003)	(0.016)
POS	−0.056	−0.025	−0.009	−0.125
	(0.013)	(0.004)	(0.003)	(0.019)
Comprehensive	0.052	0.034	0.007	0.126
	(0.020)	(0.008)	(0.005)	(0.032)
Mean of dependent variable	0.893	0.116	0.054	1.800

(B) Other Conditions

$N = 60{,}375{,}372$	Cancer	Chronic Obstructive Pulmonary Disease	Cerebrovascular Disease	Diabetes	Flu and Pneumonia	Kidney Disease
HMO	−0.023	0.132	0.068	0.051	0.041	0.033
	(0.016)	(0.012)	(0.011)	(0.010)	(0.013)	(0.006)
POS	−0.020	−0.049	−0.046	−0.017	−0.084	−0.001
	(0.019)	(0.015)	(0.013)	(0.011)	(0.016)	(0.007)
Comprehensive	0.017	0.302	0.137	0.170	0.245	0.093
	(0.029)	(0.023)	(0.021)	(0.017)	(0.023)	(0.011)
Mean of dependent variable	1.884	0.983	0.862	0.620	1.241	0.258

Note: An observation is a person-year and standard errors are clustered at the person level. The sample is restricted to those in the data for the full year. The dependent variable is 1000 if that person-year is admitted for the corresponding condition. PPO is the omitted plan type. Not shown are controls for year, age, state, urban place of residence, sex, and all variables in Appendix Table B(I). AMI is ICD-9 diagnosis code 410, other acute and subacute forms of ischemic heart disease are 411, angina pectoris is 413, other forms of chronic ischemic heart disease are 414, cancer is 140–208, chronic obstructive pulmonary disease (and allied conditions) is 490–496, cerebrovascular disease is 430–438, diabetes is 250, influenza and pneumonia are 480–487, and kidney disease (nephritis, nephrotic syndrome, and nephrosis) is 580–589.

are somewhat less likely, than PPO enrollees. There is no evidence that, on average, re-strictive plan types attract healthier people. In particular, there is no evidence that the relatively high rate of conservative AMI treatment in HMOs is because HMO patients tend to suffer from less severe AMIs. HMO patients suffer from AMIs at a somewhat *higher* rate than PPO patients. It is hard to pinpoint risk factors that would cause more frequent, but less severe AMIs.[32] It appears likely then that HMO patients in this sample are, if anything, more likely to suffer from severe AMI. These regressions control for de-mographics, but in the choice model, we include a much richer set of controls, including

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[32]The Framingham risk points, for example, draw no distinction between risk factors for less and more severe AMI (Pearson, Maron, Ridker, and Grundy (2001)).

comorbidities and medical history. This would likely narrow unexplained health differences across plan types further.

As a further check, we test the hypothesis that payments have no effect on treatment choice under a different exogeneity assumption, using variation in insurance plan type across employers rather than across individuals. The test is based on the individual's employer choices being unrelated to his employers' future plan type enrollment at the time of his AMI. This seems plausible, especially in view of the potentially long lag between employer choice and AMI, and the unpredictability of future plan type offerings. The results, in Section 7.2, easily reject the hypothesis that physicians do not respond to payments.

Finally, it is unclear even in principle why more restrictive plan types should attract healthier enrollees. Bundorf, Levin, and Mahoney (2012) argue that risk-based selection may not be a major characteristic of modern health insurance markets, and find evidence supporting the "horizontal" differentiation of plan types. Relative to the healthy, the sick might dislike having their choice of provider curtailed, but they might also prefer the lower rates of cost sharing that restrictive plan types typically impose. Breyer, Bundorf, and Pauly (2011) survey the literature starting from the 1990s and come to a similar conclusion: there is no pattern of restrictive plan types systematically attracting healthier enrollees.

Utilization management

The exclusion restriction $e_i \perp\!\!\!\perp \text{Ins}_i$ might fail because of differences in utilization management across plan types. Some plan types may encourage conservative treatments by reimbursing them at relatively high rates. If they also use nonfinancial means to encourage physicians to treat conservatively, we would attribute the combined effect of nonfinancial and financial incentives solely to financial incentives, and thus incorrectly estimate the extent to which physicians respond to payments.[33]

Insurers' attempts to directly influence treatments face serious legal obstacles. The corporate practice of medicine doctrine holds that insurers should not affect treatment decisions, because, unlike physicians, they lack medical licenses (Elhauge (2010)). Motivated by this doctrine, and responding to a public backlash against "managed care" insurers, between 1995 and 2001, states enacted a variety of patient protection laws that greatly restricted the number and scope of utilization management programs (Robinson (2001), Felt-Lisk and Mays (2002), Mays, Hurley, and Grossman (2003)). Some identify this as the biggest change in managed care insurance plans over this period (Hall (2005)).

To the extent that utilization management programs still existed when our data were collected (2002–2007), they primarily targeted either chronic or unusually expensive conditions (e.g., "disease management" for diabetes, asthma, and stable angina, or

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[33]A related concern is that even controlling for providers' mean inpatient expenditures and length of stays, plan type might be correlated with being treated in a hospital that does not have the facilities to perform some treatments. To test this, we construct indicators for whether a provider is ever recorded as treating an ischemic heart disease patient with angioplasty, bypass, or other surgery, and regress these indicators on the X_i and plan type variables. There is no evidence that HMO or POS patients are less likely to be treated in hospitals where angioplasty, bypass, or other surgery are available, using this measure.

"catastrophic case management" for transplants, spinal cord injuries, and some cancers) (Kongstvedt (2007)). AMI is one of the least likely candidates for utilization management. Its treatments are not experimental or extremely expensive, and have been well established and understood for decades. Further, quick treatment is crucial for AMI patients (Cannon et al. (2000), De Luca et al. (2004)), so there is limited scope for the insurer to require that the physician's proposed treatment be preauthorized. Even before the proliferation of legal restrictions on utilization management, when such programs were in full force, insurers rarely failed to cover procedures requested for cardiovascular disease patients (Remler et al. (1997), Lessler and Wickizer (2000)).[34] It seems unlikely that after further weakening of utilization management programs, an insurer would refuse to cover a standard procedure for AMI treatment such as bypass surgery.

Patients' influence on treatment choice

If patients influence the treatment they receive in a way that varies by plan type, this would also introduce correlation between e_i and Ins_i. This might occur because of cost sharing. If enrollees of one plan type pay more for more expensive procedures, they might push their doctor to treat conservatively.

In practice, it is improbable that this accounts for the variation in treatments by plan type. For some patients, the MarketScan data include information on deductibles, coinsurance rates, and individual out of pocket maximums. These variables determine the spending level beyond which the insurer completely covers the bill.[35] Table 7 presents this information, overall and by plan type. The mean spending threshold is \$9689, and the mean total payment for medical management is \$11,891 (the median is \$8326). Patients will often be paying zero on the margin, in which case they have no financial reason to prefer one treatment over another.

Some patients will not reach the threshold if they only receive medical management. Since they are still contributing to their medical bills, they might prefer less expensive treatments. Table 7 shows that the spending thresholds are much lower for HMO enrollees. This is because HMO enrollees are much less likely to face coinsurance payments (11 percent vs. 98 percent in other plan types). But despite the lack of cost sharing they face, the HMO patients are treated relatively conservatively. This suggests that if patient cost sharing affects treatment, it would tend to offset—and so lead us to underestimate—the physician payment response.[36]

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[34]Remler et al. (1997) survey physicians and find the overall denial rate for cardiac catheterization to be under 1 percent, and for surgical procedures to be under 2 percent. Lessler and Wickizer (2000) find that utilization management affects cardiac care patients' lengths of stay in hospital, but not their procedures. Just four of 1513 requests for procedural admission were denied in their data. These studies analyze cardiac care generally, and the effects of utilization management are likely to be smaller still for AMI.

[35]This is conservative, as spending by other family members may mean that the family out of pocket maximum is met sooner.

[36]MarketScan has no data on cost sharing provisions for most of the sample, but other evidence suggests that the basic picture is unchanged. Plans with looser networks tend to rely more on cost sharing to keep costs low. Kaiser Family Foundation and HRET (2007) find in a representative survey of firms' health plans that 65 percent of PPOs and 30 percent of POSs have coinsurance for hospital admissions, and only 18

TABLE 7. Spending thresholds.

$N = 7981$	Mean	Std. Dev.
HMO	554	(1278)
POS	11,728	(4438)
PPO	10,616	(5074)
Comprehensive	9399	(2944)
All plan types	9689	(5548)

Note: For patients for whom detailed coinsurance and copayment data are available, the table presents the dollar amount of total medical expenses (facility and physician) beyond which the patient bears zero financial liability.

Patient influence over treatment choice could potentially violate the exclusion restriction for other reasons. For example, patients who are risk-averse over money outcomes might prefer HMOs' low cost sharing, and might also be disinclined to choose riskier procedures. However, the majority of the evidence of patient influence over treatment choices concerns other, less urgent, settings like childbirth (Chou et al. (2006), Johnson and Rehavi (2013)). For urgent conditions like AMI, it seems less probable that a patient would be both willing and able to second-guess his physician's recommendation. Most angioplasties are, for instance, performed on an "ad hoc" basis, directly following the angiography and without an intervening opportunity to discuss treatment options with the patient (Hannan et al. (2009), Nallamothu and Krumholz (2010)).

Physicians and plan types

The assumption $e_i \perp \text{Ins}_i$ also restricts how a particular treatment varies by Ins_i. If a given patient would receive different kinds of angioplasties depending on whether he is in a PPO or a HMO, his angioplasty utility would vary by plan type. While plan type may change service prices, $e_i \perp \text{Ins}_i$ implies it does not affect the service quantities a patient would receive from a treatment. If these services can vary by plan type, this undermines our approach of aggregating services into treatments. An alternative would be to conduct the analysis at the service level. This would require modeling not only the level of each of the 195 services in the data, but also the covariances between them. Given that interpreting the estimation results would likely involve aggregating up to the treatment level anyway, the advantages of such an exercise seem limited. We choose instead to greatly reduce the dimension of the estimation problem by aggregating services into treatments.

A related concern is that different plan types may attract physicians with different practice styles, in a way that is not fully captured by our provider controls. Section 7.2 explores this possibility, noting that physician selection should lead to particularly large differences in treatments between providers dominated by a single plan type. We find no evidence of such differences. Our Supplementary Appendix provides further evidence

percent of HMOs do. Of those plans that do have coinsurance, patients pay about the same percentage in HMOs as on average across all plan types (15 percent vs. 17 percent).

that physician selection is unlikely to be driving our results. We form a hospital-level proxy for physicians' treatment propensities, and find no indication that PPOs tend to affiliate with physicians with propensities to treat aggressively.

6. Estimation

We estimate the model in two steps. The first step uses the service price data to recover an estimate of the effect of plan type in the total payment equations. The second step takes this estimate as given and finds the remaining payment and utility parameters.

The likelihood function for total payment and treatment choice data

The utility equations in (4.5) are not normalized for location or scale. Fixing the location of utility by subtracting $U_{i,1}$ from each $U_{i,j}$ gives

$$\overline{U}_{i,j} = X_i'\overline{\beta}_j^X + \big(X_i'(\alpha_j^X - \alpha_1^X) + \text{Ins}_i'(\alpha_j^{\text{Ins}} - \alpha_1^{\text{Ins}})\big)\beta^p + \overline{e}_{i,j} \qquad (6.1)$$

for $j = 1, \ldots, 5$, where we define $\overline{U}_{i,j} = U_{i,j} - U_{i,1}$, $\overline{\beta}_j^X = \beta_j^X - \beta_1^X$, and $\overline{e}_{i,j} = e_{i,j} - e_{i,1}$. We write the variance of the payment and utility errors $(u_{i,1}, \ldots, u_{i,5}, \overline{e}_{i,2}, \ldots, \overline{e}_{i,5})$ as $\overline{\Sigma} = \begin{pmatrix} \Sigma_{uu} & \Sigma_{u\overline{e}} \\ \Sigma_{\overline{e}u} & \Sigma_{\overline{e}\overline{e}} \end{pmatrix}$. Fixing $\text{tr}(\Sigma_{\overline{e}\overline{e}}) = 1$ sets the scale of utility.[37]

Patient i receives treatment j if and only if $X_i'(\overline{\beta}_j^X + \beta^p(\alpha_j^X - \alpha_1^X)) + \text{Ins}_i'\beta^p(\alpha_j^{\text{Ins}} - \alpha_1^{\text{Ins}}) + \overline{e}_{i,j} \geq \max_k X_i'(\overline{\beta}_k^X + \beta^p(\alpha_k^X - \alpha_1^X)) + \text{Ins}_i'\beta^p(\alpha_k^{\text{Ins}} - \alpha_1^{\text{Ins}}) + \overline{e}_{i,k}$. Define $\mathcal{E}_{i,j}$ to be the set of $\overline{e}_i = (\overline{e}_{i,2}, \ldots, \overline{e}_{i,5})$ satisfying this inequality. Let Y_i be the treatment that i receives. Abusing notation slightly by writing $u_i = (u_{i,Y_i}, u_{i,-Y_i})$, the likelihood contribution for i is

$$\int \mathbf{1}(\overline{e}_i \in \mathcal{E}_{i,Y_i})\phi\big(\ln p_{i,Y_i} - X_i'\alpha_{Y_i}^X - \text{Ins}_i'\alpha_{Y_i}^{\text{Ins}}, u_{i,-Y_i}, \overline{e}_i \mid \overline{\Sigma}\big)\, d(u_{i,-Y_i}, \overline{e}_i), \qquad (6.2)$$

where $\phi(u_i, \overline{e}_i \mid \overline{\Sigma})$ is the multivariate normal density with mean 0 and variance $\overline{\Sigma}$. The likelihood for the total payment and treatment choice data is the product of these terms over all i.

Estimating the effect of plan type on total payments

Under A4, conditional on service s being received, the distribution of $v_{i,s}$ is independent of W_i and Ins_i. The service price regressions in (4.1) can therefore be estimated by OLS. Knowing in addition the average share of total treatment payments that each service makes up allows us to determine how total treatment payments vary by plan type. Appendix B.1 presents this argument in full. The estimator $\hat{\alpha}_j^{\text{Ins}}$ is a weighted sum of the estimators $\hat{\gamma}_s^{\text{Ins}}$: it is the sample average in each region of $\sum_s \hat{\gamma}_s^{\text{Ins}} w_{i,s,j,0}$ over those i that receive treatment j.

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[37]Regardless of whether the scale of utility is fixed, the cross-payment error correlations are unidentified, because only one payment is observed at a time. None of the results we report rely on estimates of these parameters.

Physician compensation is determined by services rather than diagnoses. The payment for a chest X-ray does not depend on whether the patient suffers from AMI or angina. An advantage of using the services data is that we are not restricted to using data on AMI patients to estimate the first stage. Our procedure allows data from all diagnoses to be used in estimating the service price regressions, which gives more precise estimates of the effect of plan type on payments.

Estimating the remaining payment and utility parameters

We implement the second step by Gibbs sampling.[38] The Gibbs sampler draws variables in four substeps: first, the unobserved payments and utilities; second, the payment parameters $(\alpha_1^X, \ldots, \alpha_5^X)$; third, the utility parameters $(\beta_1^X, \ldots, \beta_5^X, \beta^p)$; and finally, the variance matrix of the error terms. Appendix B.2 goes into the details of the Gibbs sampler. Unlike the Bayesian analysis of the standard multinomial probit model (e.g., Train (2009)), here the data augmentation is not only of unobserved utilities, but also of unobserved payments.

Our estimator is asymptotically equivalent to a two step parametric M-estimator, in which the first step obtains $(\hat{\alpha}_1^{\text{Ins}}, \ldots, \hat{\alpha}_5^{\text{Ins}})$ and the second step maximizes the likelihood with respect to the remaining parameters. Showing consistency and asymptotic normality is standard (e.g., Wooldridge (2004, Section 14.2)).[39]

7. Results

7.1 *Payments and plan types*

Figure 2 shows the percentage effect of plan type on treatment payments, as estimated from the service data. These estimates are driven entirely by variation in service prices, not service quantities. Standard errors are computed by the nonparametric bootstrap, resampling at the inpatient-episode level. The effects in each region are relative to PPOs. The summary statistics in Table 2 suggested that physicians' treatment choice incentives vary by plan type. Figure 2 further supports this view, showing that these differences persist after controlling for the W_i and accounting for missing payment data. The amount PPOs pay in excess of HMOs or POSs, for example, tends to be smaller for medical management than angioplasty. The differences are large enough to plausibly affect physician

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[38]An advantage of Bayesian methods like Gibbs sampling relative to maximum simulated likelihood is that consistency and efficiency can be guaranteed under weaker conditions on the number of simulation draws (Train (2009)). Geweke, Keane, and Runkle (1994) find that Gibbs sampling slightly outperforms classical simulation methods in two Monte Carlo experiments. Gibbs sampling with data augmentation is due to Albert and Chib (1993), and is developed further in McCulloch and Rossi (1994) and Chib and Hamilton (2000).

[39]An alternative to using the service data to estimate α^{Ins} and Gibbs sampling the remaining parameters is estimating all parameters by Gibbs sampling. The disadvantage of this procedure is that a missing data problem would reemerge, since patients do not receive all services. The missing service price data would need to be augmented for each patient, which would increase by several hundred the number of latent variables in the model. The two step procedure avoids this issue, and, in particular, avoids having to estimate the high dimensional variance matrix of service price errors.
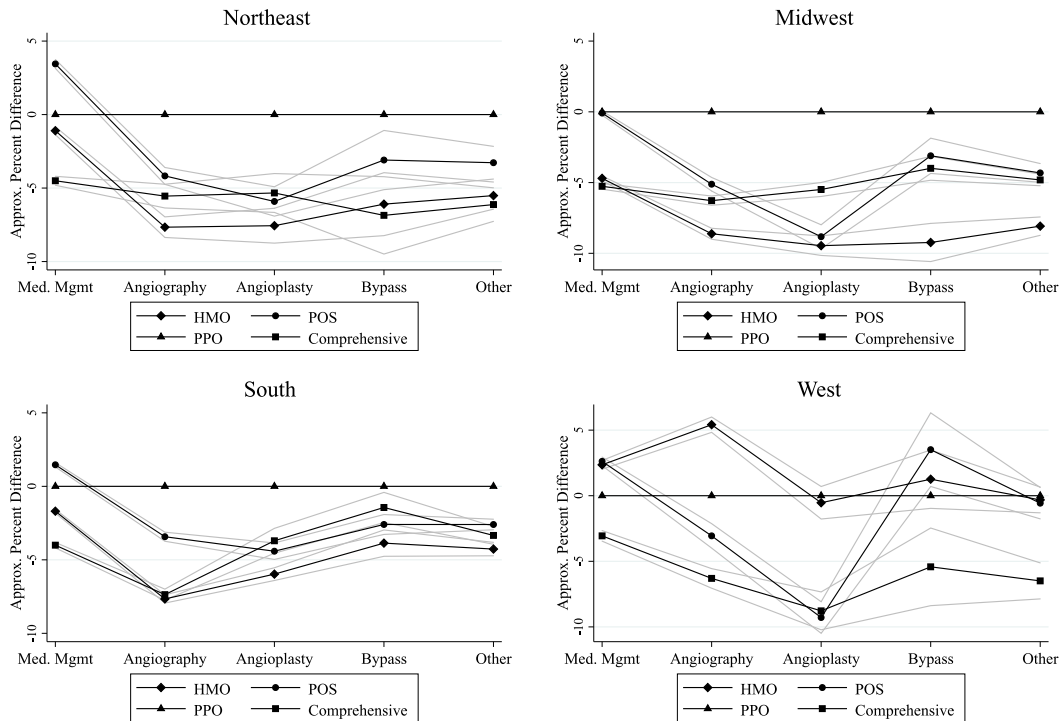
Figure 2. Effect of plan type on payments. The figure depicts estimates and 95 percent confidence intervals of the parameters in α^{Ins} from (4.2), as obtained from the service data. There are 22,078,493 service data observations. The regressors in the service price regressions are the plan type by region interactions Ins_i, the service quantities $q_{i,s,j}$, state and year fixed effects, and, where available, CPT modifier codes. PPO is the omitted category against which changes are measured in each region. Standard errors are computed from 50 bootstrap draws, where services are resampled at the inpatient-episode level.

behavior: a reimbursement differential of 9 percent for angioplasty, as with PPOs versus POSs in the Midwest, is about $250. Payment patterns in the West appear rather different from those in other regions. In our model, we allow the effect of plan type on payments to vary by region, to reflect this variation in the data.[40]

7.2 *Treatments and payments*

Magnitude of payment responses   As we are using the variation in total payments generated by differences in per-unit service prices, we use the terms "payment responses" and "price responses" interchangeably. Table 8 shows the average partial effects for the main specification, which includes all provider and clinical covariates. Increasing the payment to medical management by 1 percent increases the fraction of medical management cases by 0.0862 percentage points, and reduces the fraction of angioplasties by

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[40]When we also allow the price responsiveness parameter β^p to vary by region, we find positive and significant price reponses in each region, although slightly smaller in the West than elsewhere.

TABLE 8. Average partial effects of prices on treatments: main specification.

	MM	A'graphy	A'plasty	Bypass	Other
MM	8.62	−1.06	−5.44	−1.16	−0.95
	(2.89)	(0.37)	(1.83)	(0.44)	(0.32)
A'graphy	−1.06	8.52	−5.25	−1.45	−0.76
	(0.37)	(2.89)	(1.84)	(0.82)	(0.27)
A'plasty	−5.44	−5.25	18.75	−3.61	−4.45
	(1.83)	(1.84)	(6.34)	(1.23)	(1.56)
Bypass	−1.16	−1.45	−3.61	7.12	−0.90
	(0.44)	(0.82)	(1.23)	(2.46)	(0.43)
Other	−0.95	−0.76	−4.45	−0.90	7.06
	(0.32)	(0.27)	(1.56)	(0.43)	(2.42)

Note: The (j, k)th entry is 100 times the percentage point increase in the share of treatment j resulting from a 1 percent increase in the price paid for treatment k, averaged over patients. Choice probabilities are calculated by Geweke–Hajivassiliou–Keane (GHK) simulation. The included regressors are the demographic, clinical, and provider covariates described in Section 4.3.

0.0544 percentage points.[41] Specification 1 of Table 9 displays the corresponding own-price elasticities. There are 12.1 percent of cases treated by medical management, so the own-price elasticity is $8.62/12.1 \approx 0.71$. Increasing the payment to angioplasty by 1 percent results in a larger absolute increase in the number of angioplasties, but a smaller elasticity ($18.75/55 \approx 0.34$). Elasticities range from 0.34 (angioplasty) to 0.87 (bypass).

Across all specifications in Table 9, there is a positive physician price response. Comparing specification 1 with 2 and 3 shows how robust is the finding of a positive price response: not only does it remain after controlling for clinical and provider covariates, it does not even appear to decrease by much. Specification 4 mimics a "naive" procedure that ignores the selection bias in the first stage equations (4.2). It estimates α^{Ins} by OLS and imposes zero covariance between payment errors u_i and utility errors e_i. The direction of the bias from ignoring selection is ambiguous in theory. We find the price responses to be somewhat underestimated and their precision overstated.

Payment responses by AMI severity   The physician may not always know the patient's plan type. This is more likely for more severe AMI, because there may be less time to obtain the patient's insurance information. Physicians might also be less concerned with maximizing their income when treating severe AMI. Physicians' payment response may consequently be smaller for more severe AMI. We use two proxies for AMI severity to examine this effect in our data. The first is ST elevation. ST elevation AMI are more severe and have higher mortality rates than non-ST elevation AMI (Swanton (2003), Fox et al. (2007)). The second proxy is whether the patient has some recorded emergency department expenditure. Specifications 5 and 6 of Table 9 show the corresponding own-price elasticities. As expected, physicians appear to be more price responsive when treating less severe AMIs. The bypass price elasticity, for instance, is almost 50 percent higher for non-ST elevation AMI, and over 25 percent higher for patients with no emergency department expenditures.

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[41]Symmetry of the average partial effects matrix is a consequence of constant β^p's across treatments.

TABLE 9. Own-price elasticities: various specifications.

		MM	A'graphy	A'plasty	Bypass	Other
1.	Main specification	0.71	0.55	0.34	0.87	0.75
		(0.24)	(0.19)	(0.12)	(0.30)	(0.26)
2.	No provider covariates	0.74	0.45	0.18	0.67	0.38
		(0.36)	(0.21)	(0.09)	(0.32)	(0.18)
3.	No clinical covariates	1.19	0.36	0.24	1.10	1.08
		(0.45)	(0.14)	(0.09)	(0.42)	(0.41)
4.	No selection correction	0.62	0.39	0.25	0.68	0.49
		(0.14)	(0.08)	(0.06)	(0.14)	(0.12)
5.	ST elevation	0.45	0.37	0.20	0.70	0.58
		(0.23)	(0.18)	(0.10)	(0.35)	(0.29)
	Non-ST elevation	0.74	0.69	0.23	1.04	0.53
		(0.32)	(0.30)	(0.10)	(0.45)	(0.23)
6.	Some emergency spending	0.61	0.55	0.23	0.89	0.61
		(0.27)	(0.24)	(0.10)	(0.38)	(0.26)
	No emergency spending	0.72	0.60	0.24	1.13	0.62
		(0.29)	(0.24)	(0.10)	(0.45)	(0.25)
	High provider concentration	0.54	0.42	0.16	0.67	0.40
		(0.38)	(0.30)	(0.12)	(0.47)	(0.29)
7.	Medium provider concentration	0.56	0.46	0.19	0.82	0.51
		(0.35)	(0.30)	(0.13)	(0.52)	(0.33)
	Low provider concentration	0.59	0.47	0.20	0.86	0.55
		(0.35)	(0.29)	(0.13)	(0.51)	(0.34)

Note: All specifications include as regressors the demographic covariates described in Section 4.3. Unless stated otherwise, all specifications also include the clinical and provider covariates, corrected for selection by estimating α^{Ins} using the service data, and allow for correlation between payment and utility errors. "No Selection Correction" estimates α^{Ins} by running OLS on the first stage equations (4.2), and imposes independence of payment and utility errors. Specifications 5, 6, and 7 allow β^p to vary by patient group for the named groups.

Payment responses by provider plan type concentration   Specification 7 of Table 9 is informative about physician selection across plan types. If physician selection is an issue, there ought to be particularly large differences in treatments between providers dominated by a single plan type. Take, for instance, a comparison between a hospital that treats almost all HMO patients and one that treats almost all PPO patients. If physicians respond to prices, patients will receive more conservative treatments in the HMO-dominated hospital. If, in addition, the HMO hospital's physicians have more conservative practice styles, the treatment distributions in the HMO and the PPO hospital will be more different still. There is no evidence of this in the data. We calculate the Herfindahl index of plan type concentration for each provider in the data, and allow the price response parameter β^p to vary by the tertiles of the index. Price elasticities appear mildly *smaller* when comparing providers with high plan type concentration.

Testing for payment responses under a different exogeneity assumption   We form the fitted values $\widehat{\text{Ins}}_i$ from the regression of Ins_i on employer plan type shares and X_i. Utilities

are

$$U_{i,j} = X_i\big(\beta_j^X + \alpha_j^X \beta^p\big) + \widehat{\mathrm{Ins}}_i \alpha_j^{\mathrm{Ins}} \beta^p + r_{i,j}, \tag{7.1}$$

where $r_{i,j} = e_{i,j} + (\mathrm{Ins}_i - \widehat{\mathrm{Ins}}_i)\alpha_j^{\mathrm{Ins}}\beta^p$. Testing the null of $\beta^p = 0$ under the assumption that $e_i \perp\!\!\!\perp \widehat{\mathrm{Ins}}_i$ is straightforward, as the standard errors of β^p from the Gibbs sampler are valid.[42] As discussed in Section 5, this allows people to choose among their employer's offered plan types on the basis of their idiosyncratic unobservable characteristics, but requires that their choice of employer is unrelated to their employer's plan type offerings. The test rejects at any conventional significance level, with a t-statistic of 4.6.

7.3 *Counterfactuals and welfare*

Treatments and payments under counterfactual financial incentives   We model bundled payments by setting to zero the part of the conditional mean of utility corresponding to financial incentives, so that utilities are $U_{i,j} = X_i'\beta_j^X + e_{i,j}$.[43] The predicted outcomes under this counterfactual are in Table 10. The first four rows indicate that the in-sample model fit is reasonable. Our model predicts mean physician payments to within 1 percent of the actual value. The final rows suggests that bundling payments would greatly increase the fraction of AMI treated conservatively. Fifty percent more patients would receive medical management or angiography. Eighteen percent fewer would receive either angioplasty or bypass, and 61 percent fewer would receive bypass alone.[44] These numbers are fairly substantial relative to international treatment differences. In an international clinical trial, Gupta et al. (2003) find that in France and Israel 18 and 34 percent fewer AMI patients receive angioplasty or bypass. Moise and Jacobzone (2003) find that Finland and Sweden perform about 60–70 percent fewer bypass surgeries than the United States.

Conservative treatments are cheaper than aggressive ones, so the distribution of treatments under the bundled payments counterfactual should be cheaper than the distribution of treatments under the current payment regime. We quantify this difference in cost, using *current* prices. In performing this exercise, our model captures the difference in treatment cost between the average and marginal patients. A patient who was just sick enough to receive angiography under fee-for-service may only receive medical management if payments are bundled. Because we have estimated the covariance matrix Σ, when we simulate counterfactual outcomes, we can incorporate the utility and

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[42]Estimation is another matter, as if $\beta^p \neq 0$, the utility errors $r_{i,j}$ in this model are heteroskedastic and nonnormal. This precludes using the conjugate distributions, which greatly simplify the Gibbs sampler. Computing a test statistic under the null $\beta^p = 0$ presents no such difficulties.

[43]We keep the utility error variance matrix fixed in this counterfactual. It is not possible to identify how much of the utility error corresponds to physician financial incentives ($\mathbb{E}(u_{i,j} \mid s_{i,j})\beta^p$) and how much to other factors ($e_{i,j}^0$), since the data are only informative about the sum of the two. This benchmark of constant utility error variances corresponds to the case where physicians' signal s_i of the payment error is assumed to be constant, so that financial incentives do not enter into the utility errors.

[44]Hospitals' investment responses to bundled payments are unmodeled here, and they would likely incline physicians to treat even more conservatively, as bundled payments would make investing in angioplasty or bypass-specific capital less attractive.

TABLE 10. Treatments and physician payments: actual and predicted.

	MM	A'graphy	A'plasty	Bypass	Other	All Treatments
Actual payments	1264	1678	2878	8839	7604	3426
Actual shares	12.1	15.4	54.9	8.2	9.4	100.0
Predicted payments	1333	1744	2885	9242	7726	3462
	(27)	(36)	(34)	(331)	(140)	(53)
Predicted shares	12.1	17.4	53.2	8.2	9.1	100.0
	(0.2)	(0.3)	(0.4)	(0.4)	(0.2)	
Bundled payments, costs	1524	1796	2540	7815	6851	2530
	(86)	(53)	(114)	(1050)	(409)	(226)
Bundled payments, shares	18.9	26.3	46.9	3.2	4.8	100.0
	(2.2)	(3.1)	(2.9)	(1.1)	(1.2)	

Note: The table shows actual average physician payments and treatment shares, model predicted payments and shares from the main specification with β^p equal to its estimated value, and model predicted payments and shares from the main specification with $\beta^p = 0$ (the bundled payments counterfactual). We evaluate the costs of treatments provided under bundled payments using the predicted payments under the current fee-for-service system.

payment error correlation. The physician cost savings from bundled payments evaluated at current prices are 27 percent. If the savings on facility payments were of the same order, the average reduction in total costs would be around $7600 per patient. Extrapolating to the entire United States, this corresponds to a reduction in expenditure of approximately $5 billion per year.[45]

Our model also enables us to project how treatment patterns and total physician payments would change if the payment schedule for a given plan type applied for all patients. Appendix Table C shows these results by region. They correspond to the across-plan type payment differences of Figure 2. For example, relative to all patients in the Northeast being in PPOs, if all were in POSs, there would be 6 percent more medical management cases and total physician payments would be 4 percent lower.

Finally, physicians may face still stronger incentives to treat aggressively if they receive the hospital payment too, as is the case with physician-owned speciality hospitals (Casalino, Devers, and Brewster (2003), Kahn (2006)). Doubling payments from angioplasty and bypass surgery—not implausible given the sizes of the hospital payments—would lead to an 18 percent increase in angioplasties and a 29 percent increase in bypass surgeries, with almost three-quarters of AMI patients receiving one of the two procedures.

Welfare consequences of financial incentives   The estimates from the main specification imply that 20 percent of patients (with a 6 percent standard error) are marginal, in the sense that they would receive different treatments under fee-for-service and bundled payments. Even without posttreatment quality of life data, under some assumptions, the model allows us to use physicians' revealed preference to quantify these patients'

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[45]There are around 935,000 AMI in the United States each year (Roger et al. (2012)). About 25 percent result in sudden death; the vast majority of the remainder (around 700,000) are treated in hospital (Wennberg and Birkmeyer (1999)).

change in welfare. Whereas randomized controlled trials from the medical literature are informative about average effects, the effect of a payment reform depends on how the marginal patient is affected. Our model-based analysis has the advantage that we can study the effects on marginal patients' welfare.

Our welfare analysis captures the idea that physicians' disutility of labor is an important determinant of whether bundled payments are better for patients than fee-for-service. In our analysis, if physicians' payments always exactly cancel out their disutility of labor, they have no self-interested reason to choose one treatment over another, and their utility is aligned with their patients' utility.[46] Bundled payments, for instance, maximize patient welfare in the case where the physician's effort costs are constant across treatments. If costs are not constant, bundled payments may induce physicians to shirk, and fee-for-service incentives may be better for patients (Ellis and McGuire (1986)).

We assume utility can be decomposed as

$$U_{i,j} = X_i'\beta_j^X + \left(X_i'\alpha_j^X + \text{Ins}_i'\alpha_j^{\text{Ins}}\right)\beta^p + e_{i,j} \tag{7.2}$$

$$= \underbrace{X_i'\beta_j^X + e_{i,j} + \rho X_i'\alpha_j^X \beta^p}_{\text{patient welfare}} + \underbrace{\left(X_i'\alpha_j^X + \text{Ins}_i'\alpha_j^{\text{Ins}}\right)\beta^p}_{\text{physician utility from revenue}}$$

$$- \underbrace{\rho X_i'\alpha_j^X \beta^p}_{\text{physician disutility of labor}} \tag{7.3}$$

for some scalar ρ. The implicit assumptions here are that the utility error belongs to the patient and that physician disutility of labor is proportional to physician utility from PPO revenue (the omitted plan type). The case of $\rho = 0$ corresponds to physician disutility of labor, which is constant across treatments and can be normalized to 0. Given ρ, we can calculate the average value of the patient welfare term under bundled payments and fee-for-service. Because we have estimated how physician revenue translates into utility, we can express the welfare difference between these two regimes in dollar terms, as evaluated by the physicians in terms of their own revenue.

Figure 3 shows patients' average welfare gain from bundled payments relative to fee-for-service as a function of ρ, assuming that physicians value a dollar of patient welfare at one-tenth or one-fiftieth of the value they place on their own revenue. If $\rho < 0.57$, bundled payments are better for patients than fee-for-service. A simple back-of-the-envelope calculation suggests that bundled payments increase social surplus because of cost savings. If physicians value \$50 of patient welfare at \$1, and even if $\rho = 1$, the loss of patient welfare from bundled payments of slightly over \$5000 is outweighed by the reduction in total treatment costs of around \$7600.

This conclusion—that shifting to a less intensive distribution of treatments is likely to be welfare increasing—is based on an estimated model of physicians' treatment choices, and it is plausible given the extensive medical literature based on randomized trials. Angioplasty seems to confer benefits over medical management for some forms

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[46]We focus on disutility of labor rather than the financial costs of treatment (e.g., for medical supplies), which are typically borne by the hospital rather than the physicians.
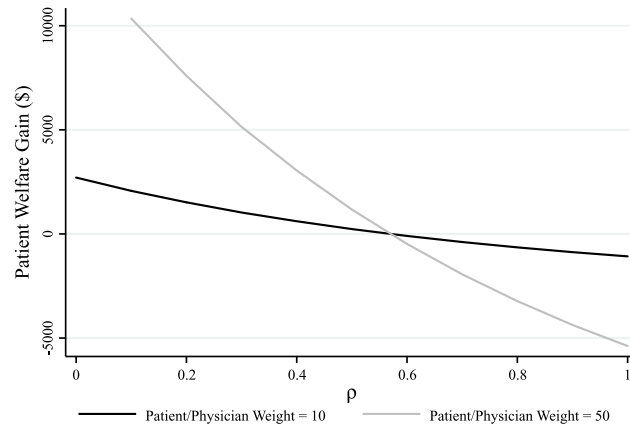
FIGURE 3. Patient welfare gain from bundled payments versus cost–payment ratio. Welfare losses from patient utility level P_1 to P_2 calculated as the certainty equivalent of physician payments for the chosen treatment j, which is multiplied by the percent change in payments corresponding to the welfare loss, and a factor representing the rate at which physicians trade off their patients' welfare for their own income: $d \exp(X_i'\alpha_j^X + \mathrm{Ins}_i'\alpha_j^{\mathrm{Ins}})[\exp((P_1 - P_2)/\beta^p) - 1]$, for $d = 10$ or 50.

of heart attacks (e.g., ST-elevation AMI (Keeley, Boura, and Grines (2003))), but not for others (e.g., intermediate and low risk non-ST-elevation AMI (Gibler et al. (2005))). Similarly, the benefits from bypass over angioplasty appear small (Rodriguez et al. (2005), Daemen et al. (2008)), while the costs are much greater.[47]

8. Conclusion

Financial incentives do appear to influence how physicians manage AMI, and the effects are particularly pronounced for less severely ill patients. Our estimates imply that it is quite common for patients to receive different treatments than they would if physicians received bundled payments. The costs of providing the more intensive treatments associated with fee-for-service payments are substantial and plausibly outweigh the welfare gain from those treatments.

The finding that treatment payments affect treatment choices in this setting is especially significant given that AMI is one of the conditions for which financial incentives are least likely to matter. Compared to other conditions AMI has fairly standardized treatment protocols. Moreover, it is an acute condition, and physician incentives are more likely to affect the treatment of chronic illnesses, like congestive heart failure and cancer (Wennberg (2010), Clemens and Gottlieb (2014)). If payments influence physicians even for AMI management, for other conditions, they may have even more substantial effects.

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[47]Further, because these trials compare the effect of different treatments on average patients, they are likely to overstate the change in welfare from assigning patients on the margin between treatments to the less intensive treatment.

We use two data sets to estimate the model. The first is at the patient level and the second is at the service level. All data are from the period 2002–2007. Replication files are available in a supplementary file on the journal website, http://qeconomics.org/supp/365/code_and_data.zip.

AMI patients

This sample is used to estimate the payment equations (4.2) and the discrete choice model (6.1) given the estimate of α^{Ins} from the first stage. It includes only those patients with AMI as their primary diagnosis (ICD-9 diagnosis code 410). Patients are categorized as receiving medical management if their admission has a nonsurgical diagnosis-related group (DRG) assigned and there is no record of their receiving angiography, angioplasty, or bypass surgery, and as receiving other surgery if they have a surgical DRG assigned but their principal procedure is not angiography, angioplasty, or bypass surgery. For angiography, angioplasty, and bypass surgery the codes are the following.

Coronary Angiography
ICD-9 Procedure Codes: 8850 8851 8852 8853 8854 8855 8856 8857 8858 3721 3722 3723.
CPT Codes: 93501 93503 93508 93510 93511 93514 93524 93526 93527 93528 93529 93539 93540 93541 93542 93543 93544 93545 93555 93556 93561 93562 93571 93572.
Coronary Angioplasty
ICD-9 Procedure Codes: 0066 3601 3602 3605 3606 3607 3609.
CPT Codes: 92980 92981 92982 92984.
Coronary Bypass Surgery
ICD-9 Procedure Codes: 3610 3611 3612 3613 3614 3615 3616 3617 3619.
CPT Codes: 33510 33511 33512 33513 33514 33516 33517 33518 33519 33521 33522 33523 33530 33533 33534 33535 33536.

Patients who receive multiple treatments are assigned to the most invasive of their treatments (where the order of "invasiveness" from least to most is medical management, angiography, angioplasty, bypass surgery, other surgery).

We keep the patients who belong to the largest four plan types—HMO, PPO, POS, and comprehensive—and drop the less than 4 percent of observations from the remaining plan types: exclusive provider organizations, capitated and partially capitated point of service plans, and consumer driven health plans. We also drop the few observations that are recorded as including capitated payments in 2007 (the only year MarketScan records capitation) and those that appear to have unreliable or missing data: those with zero or negative total inpatient expenditures, or missing geographical region data. Physician payments are winsorized at the 1st and 99th percentiles.

Services prices

This sample is used to estimate the service price regressions (4.1). We focus on the most common services, dropping the 3 percent of services that appear fewer than 500 times for our AMI patients; 195 different services remain. We keep services from all diagnoses,

not just AMI, as the prices paid for a service typically depend on the service itself and not on the patient's diagnosis. We only keep services received by patients in HMO, PPO, POS, and comprehensive plans, and drop the claims recorded as capitated. We omit the outliers, defined as those over 10 times larger or smaller than the median service price, which are more likely to reflect measurement error than true prices.

Appendix B: Estimation

B.1 *Estimating the effect of plan type on total payments*

We obtain a first-order approximation of the effect of plan type on total payments, given the effect of plan type on service prices. To reduce notational burden, we treat the case where there are just two plan types, so Ins_i is a binary variable; the extension to multiple plan types is straightforward. Service prices, treatment payments, and treatment utilities are given by equations (4.1), (4.2), and (4.5).

Let $\alpha_{i,j}^{\text{Ins}}$ denote the percent change in i's total payment for treatment j when i switches from $\text{Ins}_i = 0$ to $\text{Ins}_i = 1$, and let $w_{i,s,j,0} = \exp(W_i'\gamma_s^W + v_{i,s})q_{i,s,j}(\sum_k \exp(W_i'\gamma_k^W + v_{i,k})q_{i,k,j})^{-1}$ denote the share of i's physician bill that would be spent on service s were he to receive treatment j when in plan type $\text{Ins}_i = 0$. We write $x(\gamma^{\text{Ins}}) \approx y(\gamma^{\text{Ins}})$ if $x(\gamma^{\text{Ins}})$ and $y(\gamma^{\text{Ins}})$ are equal up to first order in γ^{Ins}, that is, if $\lim_{\gamma^{\text{Ins}} \to 0} \|x(\gamma^{\text{Ins}}) - y(\gamma^{\text{Ins}})\|/\|\gamma^{\text{Ins}}\| = 0$. The following proposition allows us to approximate the average effect of plan type on payments, given the observed, selected sample.

PROPOSITION. *Given the service price, treatment payment, and treatment utility equations* (4.1), (4.2), *and* (4.5), *and under assumptions* A3 *and* A5,

$$\mathbb{E}\big(\alpha_{i,j}^{\text{Ins}} \mid X_i, \text{Ins}_i, i \text{ receives } j\big) \approx \mathbb{E}\big(\alpha_{i,j}^{\text{Ins}} \mid X_i, \text{Ins}_i\big). \tag{B.1}$$

PROOF. We first note that the response of total payments to plan type can be written as a weighted sum of the responses of service prices to plan type:

$$
\begin{aligned}
\alpha_{i,j}^{\text{Ins}} &= \frac{\sum\limits_s \exp(W_i'\gamma_s^W + \gamma_s^{\text{Ins}} + v_{i,s})q_{i,s,j} - \sum\limits_s \exp(W_i'\gamma_s^W + v_{i,s})q_{i,s,j}}{\sum\limits_s \exp(X_i'\gamma_s^X + v_{i,s})q_{i,s,j}} \\
&= \sum_s \left\{ \frac{\exp(W_i'\gamma_s^W + \gamma_s^{\text{Ins}} + v_{i,s})q_{i,s,j} - \exp(W_i'\gamma_s^W + v_{i,s})q_{i,s,j}}{\exp(W_i'\gamma_s^W + v_{i,s})q_{i,s,j}} \right. \\
&\qquad\qquad \left. \times \frac{\exp(W_i'\gamma_s^W + v_{i,s})q_{i,s,j}}{\sum\limits_k \exp(W_i'\gamma_k^W + v_{i,k})q_{i,k,j}} \right\} \\
&\approx \sum_s \left\{ \gamma_s^{\text{Ins}} \frac{\exp(W_i'\gamma_s^W + v_{i,s})q_{i,s,j}}{\sum\limits_k \exp(W_i'\gamma_k^W + v_{i,k})q_{i,k,j}} \right\} \\
&= \sum_s \gamma_s^{\text{Ins}} w_{i,s,j,0}.
\end{aligned} \tag{B.2}
$$

The first equality is true because, by A3, changing plan type changes service prices only, not service quantities (see Section 5, "Physicians' Response to Plan Type" for discussion of this assumption). The second step is simple algebra, the third is a first-order approximation, and the fourth is true by definition. The proposition follows:

$$\mathbb{E}\big(\alpha_{i,j}^{\text{Ins}} \mid X_i, \text{Ins}_i, i \text{ receives } j\big)$$

$$= \mathbb{E}\big\{\mathbb{E}\big(\alpha_{i,j}^{\text{Ins}} \mid X_i, \text{Ins}_i, e_i\big) \mid X_i, \text{Ins}_i, i \text{ receives } j\big\}$$

$$\approx \mathbb{E}\bigg\{\mathbb{E}\bigg(\sum_s \gamma_s^{\text{Ins}} w_{i,s,j,0} \mid X_i, \text{Ins}_i, e_i\bigg) \mid X_i, \text{Ins}_i, i \text{ receives } j\bigg\}$$

$$= \mathbb{E}\bigg\{\mathbb{E}\bigg(\sum_s \gamma_s^{\text{Ins}} w_{i,s,j,0} \mid X_i, \text{Ins}_i\bigg) \mid X_i, \text{Ins}_i, i \text{ receives } j\bigg\}$$

$$\approx \mathbb{E}\big\{\mathbb{E}\big(\alpha_{i,j}^{\text{Ins}} \mid X_i, \text{Ins}_i\big) \mid X_i, \text{Ins}_i, i \text{ receives } j\big\}$$

$$= \mathbb{E}\big(\alpha_{i,j}^{\text{Ins}} \mid X_i, \text{Ins}_i\big).$$

The first step follows from the law of iterated expectations because the event $\{i \text{ receives treatment } j\}$ can be expressed in terms of (X_i, Ins_i, e_i), the second is true by (B.2), the third is implied by A5, the fourth is true by (B.2), and the final equality is straightforward. □

The effect of plan type is patient-specific: the coefficient $\alpha_{i,j}^{\text{Ins}}$ depends on the particular bundle of services that patient i consumes. By A4, the γ_s^{Ins} can be estimated by ordinary least squares. If bundles of services for counterfactual treatments were observed, then $\alpha_{i,j}^{\text{Ins}}$ could be estimated for every patient i and treatment j, and the model could be estimated with total payment regressions of the form $\ln p_{i,j} = X_i'\alpha_j^X + \text{Ins}_i'\alpha_{i,j}^{\text{Ins}} + u_{i,j}^0$. Counterfactual bundles are not observed, so we instead form total payment regressions featuring the average coefficients over patients: $\ln p_{i,j} = X_i'\alpha_j^X + \text{Ins}_i'\mathbb{E}(\alpha_{i,j}^{\text{Ins}} \mid X_i, \text{Ins}_i) + u_{i,j}$, where $u_{i,j} = \text{Ins}_i'(\alpha_{i,j}^{\text{Ins}} - \mathbb{E}(\alpha_{i,j}^{\text{Ins}} \mid X_i, \text{Ins}_i)) + u_{i,j}^0$. The proposition implies that a feasible, consistent estimator of $\mathbb{E}(\alpha_{i,j}^{\text{Ins}} \mid X_i, \text{Ins}_i)$ is the sample average of $\sum_s \hat{\gamma}_s^{\text{Ins}} w_{i,s,j,0}$ conditional on X_i, Ins_i, and i receiving j. Thus the service data allow the effect of plan type on total payments to be recovered. In practice, there is not enough data for each (X_i, Ins_i) combination to give precise estimates of $\mathbb{E}(\alpha_{i,j}^{\text{Ins}} \mid X_i, \text{Ins}_i)$. We estimate the mean of $\alpha_{i,j}^{\text{Ins}}$ by region only. Standard errors are calculated from 50 bootstrap draws, resampling the service data at the inpatient-episode level.

B.2 *Estimating the remaining payment and utility/parameters*

Let $\mathcal{Z}_i = (Y_i, X_i, p_{i,Y_i})$ denote the treatment and total payment data for the ith patient, let $\mathcal{Z} = \{\mathcal{Z}_i\}$ denote all treatment and total payment data, let $p_- = \{p_{i,-Y_i}\}$ denote all unobserved payments, and let $\overline{U} = \{\overline{U}_i\}$ denote all utilities. Define $\alpha^X = (\alpha_1^X, \ldots, \alpha_5^X)$, $\alpha^{\text{Ins}} = (\alpha_1^{\text{Ins}}, \ldots, \alpha_5^{\text{Ins}})$, and $\overline{\beta} = (\overline{\beta}_2^X, \ldots, \overline{\beta}_5^X, \beta^p)$. The Gibbs sampler draws parameters sequentially: first, $p_-, \overline{U} \mid \alpha^X, \overline{\beta}, \overline{\Sigma}, \hat{\alpha}^{\text{Ins}}, \mathcal{Z}$; second, $\alpha^X \mid p_-, \overline{U}, \overline{\beta}, \overline{\Sigma}, \hat{\alpha}^{\text{Ins}}, \mathcal{Z}$; third,

$\overline{\beta} \mid p_-, \overline{U}, \alpha^X, \overline{\Sigma}, \hat{\alpha}^{\mathrm{Ins}}, \mathcal{Z}$; and finally, $\overline{\Sigma} \mid p_-, \overline{U}, \alpha^X, \overline{\beta}, \hat{\alpha}^{\mathrm{Ins}}, \mathcal{Z}$. Repeated draws from these conditional distributions form a Markov chain with a strictly positive transition kernel (the kernel here is the product of truncated normal, untruncated normal, and inverse Wishart distributions). Standard results on Markov chains imply it converges to the posterior parameter distribution (Geweke and Keane (2001)).

We specify independent priors on $\alpha^X, \overline{\beta}, \overline{\Sigma}^{-1}$ as $\alpha^X \sim N(0, \psi_{\alpha^X}\mathcal{I})$, $\overline{\beta} \sim N(0, \psi_{\overline{\beta}}\mathcal{I})$, and $\overline{\Sigma}^{-1} \sim \mathrm{Wishart}(\zeta, \psi_{\overline{\Sigma}}\mathcal{I})$ (McCulloch and Rossi (1994)).[48] Large ψ_{α^X} and $\psi_{\overline{\beta}}$, and small ζ correspond to a diffuse prior. We choose $\psi_{\alpha^X} = \psi_{\overline{\beta}} = 100$, $\zeta = 10$, and $\psi_{\overline{\Sigma}} = 1$. The norm of the utility error variance matrix $\Sigma_{\overline{e}\overline{e}}$ is unrestricted, so the model is unidentified. The Gibbs sampler takes values in the unidentified parameter space, but we report results on the identified parameters by dividing the utility conditional mean parameter draws by $\mathrm{tr}(\Sigma_{\overline{e}\overline{e}})^{0.5}$ and the utility error variance by $\mathrm{tr}(\Sigma_{\overline{e}\overline{e}})$ (Geweke, Keane, and Runkle (1994), McCulloch and Rossi (1994)).

We draw from the conditional distributions as follows:

1. Draw $p_-, \overline{U} \mid \alpha^X, \overline{\beta}, \overline{\Sigma}, \hat{\alpha}^{\mathrm{Ins}}, \mathcal{Z}$. This is the data-augmentation part of the algorithm. It comprises eight substeps, one for each of the four missing utilities (one utility is normalized to zero) and four missing payments (one payment is observed). For each patient, we draw each latent variable or, equivalently, its corresponding error term, conditional on the values of all other error terms for that patient and on the observed choice. This is Gibbs sampling from a truncated multivariate normal distribution, as described by Geweke (1991).

2. Draw $\alpha^X \mid p_-, \overline{U}, \overline{\beta}, \overline{\Sigma}, \hat{\alpha}^{\mathrm{Ins}}, \mathcal{Z}$. This is a Bayesian seemingly unrelated regression system. Collecting the five payment equations in (4.2) across j gives

$$\ln p_i = (\mathcal{I}_5 \otimes X_i')\alpha^X + (\mathcal{I}_5 \otimes \mathrm{Ins}_i')\hat{\alpha}^{\mathrm{Ins}} + u_i,$$

$$u_i \sim N\big(\Sigma_{u\overline{e}}\Sigma_{\overline{e}\overline{e}}^{-1}\overline{e}_i, \Sigma_{uu} - \Sigma_{u\overline{e}}\Sigma_{\overline{e}\overline{e}}^{-1}\Sigma_{\overline{e}u}\big). \tag{B.3}$$

Because $\overline{e}_i$ is a function of $\overline{U}_i$ and β, the distribution of u_i is conditional on $\overline{e}_i$. Write the Cholesky decomposition of the inverse of the conditional variance as $(\Sigma_{uu} - \Sigma_{u\overline{e}}\Sigma_{\overline{e}\overline{e}}^{-1}\Sigma_{\overline{e}u})^{-1} = C_u C_u'$. Define $\ln\breve{p}_i = C_u'(\ln p_i - (\mathcal{I}_5 \otimes \mathrm{Ins}_i')\hat{\alpha}^{\mathrm{Ins}} - \Sigma_{u\overline{e}}\Sigma_{\overline{e}\overline{e}}^{-1}\overline{e}_i)$, $\breve{A}_i = C_u'(\mathcal{I}_5 \otimes X_i')$, and $\breve{u}_i = C_u'(u_i - \Sigma_{u\overline{e}}\Sigma_{\overline{e}\overline{e}}^{-1}\overline{e}_i)$, so that

$$\ln\breve{p}_i = \breve{A}_i\alpha^X + \breve{u}_i, \quad \breve{u}_i \sim N(0, \mathcal{I}_5) \tag{B.4}$$

or, stacking over i,

$$\ln\breve{p} = \breve{A}\alpha^X + \breve{u}, \quad \breve{u} \sim N(0, \mathcal{I}_{5n}). \tag{B.5}$$

Given the prior over α^X, the posterior $\alpha^X \mid p_-, \overline{U}, \overline{\beta}, \overline{\Sigma}, \hat{\alpha}^{\mathrm{Ins}}, \mathcal{Z}$ is normal with mean $(\breve{A}'\breve{A} + \psi_{\alpha^X}^{-1}\mathcal{I})^{-1}\breve{A}'\ln\breve{p}$ and variance $(\breve{A}'\breve{A} + \psi_{\alpha^X}^{-1}\mathcal{I})^{-1}$.

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[48]In this parameterization, ζ is the degrees of freedom and $\psi_{\overline{\Sigma}}\mathcal{I}$ is the scale matrix, so that the Wishart density $f(A)$ is proportional to $|A|^{(\zeta-p-1)/2}\exp(-\frac{1}{2}\mathrm{tr}(\psi_{\overline{\Sigma}}^{-1}A))$, where p is the dimension of A.

3. Draw $\overline{\beta} \mid p_-, \overline{U}, \alpha^X, \overline{\Sigma}, \hat{\alpha}^{\mathrm{Ins}}, \mathcal{Z}$. This is a Bayesian regression system. Define

$$B_i = \begin{pmatrix} X_i'(\alpha_2^X - \alpha_1^X) + \mathrm{Ins}_i'(\hat{\alpha}_2^{\mathrm{Ins}} - \hat{\alpha}_1^{\mathrm{Ins}}) \\ \mathcal{I}_4 \otimes X_i', \qquad\qquad \vdots \\ X_i'(\alpha_5^X - \alpha_1^X) + \mathrm{Ins}_i'(\hat{\alpha}_5^{\mathrm{Ins}} - \hat{\alpha}_1^{\mathrm{Ins}}) \end{pmatrix} \quad \text{and}$$

$$\overline{\beta} = (\overline{\beta}_1^{X\prime}, \ldots, \overline{\beta}_4^{X\prime}, \beta^p)'.$$

Collecting the four utility equations in (6.1) across j gives

$$\overline{U}_i = B_i \overline{\beta} + \overline{e}_i, \quad \overline{e}_i \sim N\big(\Sigma_{\overline{e}u}\Sigma_{uu}^{-1}u_i, \Sigma_{\overline{e}\overline{e}} - \Sigma_{\overline{e}u}\Sigma_{uu}^{-1}\Sigma_{u\overline{e}}\big). \tag{B.6}$$

Write the Cholesky decomposition of the inverse of the conditional variance as $(\Sigma_{\overline{e}\overline{e}} - \Sigma_{\overline{e}u}\Sigma_{uu}^{-1}\Sigma_{u\overline{e}})^{-1} = C_{\overline{e}}C_{\overline{e}}'$. Define $\breve{\overline{U}}_i = C_{\overline{e}}'(\overline{U}_i - \Sigma_{\overline{e}u}\Sigma_{uu}^{-1}u_i)$, $\breve{B}_i = C_{\overline{e}}'B_i$, and $\breve{\overline{e}}_i = C_{\overline{e}}'(\overline{e}_i - \Sigma_{\overline{e}u}\Sigma_{uu}^{-1}u_i)$, so that

$$\breve{\overline{U}}_i = \breve{B}_i\overline{\beta} + \breve{\overline{e}}_i, \quad \breve{\overline{e}}_i \sim N(0, \mathcal{I}_4) \tag{B.7}$$

or, stacking over i,

$$\breve{\overline{U}} = \breve{B}\overline{\beta} + \breve{\overline{e}}, \quad \breve{\overline{e}} \sim N(0, \mathcal{I}_{4n}). \tag{B.8}$$

Given the prior over $\overline{\beta}$, the posterior $\overline{\beta} \mid p_-, \overline{U}, \alpha^X, \overline{\Sigma}, \hat{\alpha}^{\mathrm{Ins}}, \mathcal{Z}$ is normal with mean $(\breve{B}'\breve{B} + \psi_{\overline{\beta}}^{-1}\mathcal{I})^{-1}\breve{B}'\breve{\overline{U}}$ and variance $(\breve{B}'\breve{B} + \psi_{\overline{\beta}}^{-1}\mathcal{I})^{-1}$.

4. Draw $\overline{\Sigma} \mid p_-, \overline{U}, \alpha^X, \overline{\beta}, \hat{\alpha}^{\mathrm{Ins}}, \mathcal{Z}$. This step is conditional on both u_i and $\overline{e}_i$. The conjugate prior on $\overline{\Sigma}^{-1}$ is Wishart$(\zeta, \psi_{\overline{\Sigma}}\mathcal{I})$, so the posterior is Wishart$(\zeta + n, (\psi_{\overline{\Sigma}}^{-1} + \sum_{i=1}^n (u_i', \overline{e}_i')'(u_i', \overline{e}_i'))^{-1})$ (Gelman, Carlin, Stern, and Rubin (2003)).

For α^{Ins} fixed at its true value, the Bernstein–von Mises theorem implies that the difference between the posterior mean and the maximum likelihood estimator is $o_p(n^{-1/2})$, and that the asymptotic distribution of the maximum likelihood estimator (MLE) is the same as the asymptotic posterior (van der Vaart (1998), Train (2009)). It also implies that the mean of the posterior is asymptotically equivalent to the MLE, and that the posterior variance can be used as an estimate of the sampling variance of the MLE. Moments of the posterior are all that is required for inference. The transition kernel is strictly positive and therefore ergodic, so the mean and variance of the posterior can be estimated by finding the sample mean and variance of the draws from each iteration of steps 2–4 (Geweke and Keane (2001) Section 2.4 collects the relevant results). We run the Gibbs sampler for 5000 draws and allow for "burn-in" by discarding the first half of the draws, following Gelman et al. (2003). Because α^{Ins} is not known but instead estimated in a first stage, standard errors need to be adjusted to account for the sampling variation in $\hat{\alpha}^{\mathrm{Ins}}$. We use the posterior variance from the Gibbs sampler together with the bootstrapped standard errors of $\hat{\alpha}^{\mathrm{Ins}}$ to calculate the overall standard errors.

TABLE A. Total facility payments, by insurance plan type and treatment.

	MM	A'graphy	A'plasty	Bypass	Other	All Treatments
HMO	10,319	10,209	20,643	41,633	48,824	21,107
	(12,496)	(10,447)	(17,001)	(29,476)	(37,369)	(22,467)
POS	11,040	10,863	23,759	43,412	51,136	24,478
	(12,849)	(9582)	(15,935)	(25,485)	(35,048)	(22,084)
PPO	11,004	12,652	25,075	49,448	53,154	26,366
	(13,665)	(11,487)	(16,943)	(30,023)	(35,390)	(23,702)
Comprehensive	9516	11,491	21,150	42,696	46,165	22,257
	(11,030)	(10,956)	(15,130)	(26,542)	(35,527)	(21,692)
All plan types	10,627	11,955	23,850	46,754	51,447	24,866
	(12,958)	(11,121)	(16,718)	(29,143)	(35,706)	(23,186)

Note: Total facility payments are the sum in dollars of all payments made to all facilities, including hospitals, involved in treating the AMI, from the patient and the insurer. Each cell contains the mean and standard deviation of total payments for that plan type and treatment.

TABLE B. Summary statistics: selected variables.

(I) Contemporaneous county-level variables, $N = 66{,}014$

Fraction of physicians with medical speciality	0.61	No. hospitals with adult cardiac cath. facilities per million	7.45 (5.54)
Median age	35.73 (3.20)	No. hospitals with adult cardiac surgery facilities per million	6.22 (4.87)
Median household income (thousands)	45.89 (11.79)	No. hospital beds for cardiac intensive care per million	93.64 (87.73)
Ischemic heart disease mortality per thousand	1.54 (0.56)	No. inpatient surgeries per thousand	53.04 (41.75)

(II) Medical and insurance history over 6 months prior to AMI, $N = 50{,}454$

No inpatient expenditure	0.79	Admitted for IHD and received angioplasty	0.04
Total inpatient expenditure (thousands)	4.38 (17.06)	Admitted for IHD and received bypass surgery	0.01
Admitted for IHD and received medical management	0.06	Admitted for IHD and received bypass surgery	0.01
Admitted for IHD and received angiography	0.03	Plan type change	0.01

Note: All variables are binary unless standard deviations are displayed. Contemporaneous county-level variables are obtained from the Area Resources File. "IHD" stands for ischemic heart disease, which corresponds to the ICD-9 codes 410, 411, 413, 414, and 786.

TABLE C. Treatment shares and payments for different payment schedules.

Region	Plan Type	N	MM	A'graphy	A'plasty	Bypass	Other	Payments
Northeast	HMO	969	16.7	16.0	52.0	6.8	8.5	3097
			(0.6)	(0.5)	(0.7)	(0.5)	(0.4)	(62)
	POS	1023	17.0	16.0	51.5	7.0	8.4	3179
			(0.6)	(0.5)	(0.8)	(0.5)	(0.4)	(61)
	PPO	2833	16.1	16.1	52.5	6.9	8.4	3320
			(0.6)	(0.5)	(0.7)	(0.5)	(0.4)	(62)
	Comp	943	16.2	16.1	52.5	6.8	8.4	3129
			(0.6)	(0.5)	(0.7)	(0.5)	(0.4)	(65)
Midwest	HMO	1738	10.0	16.6	55.4	8.1	9.9	3261
			(0.2)	(0.3)	(0.5)	(0.5)	(0.3)	(57)
	POS	1446	10.2	16.7	54.7	8.3	10.1	3359
			(0.3)	(0.3)	(0.6)	(0.5)	(0.3)	(59)
	PPO	11,782	9.6	16.6	55.8	8.1	9.9	3570
			(0.2)	(0.3)	(0.5)	(0.5)	(0.2)	(60)
	Comp	5375	9.6	16.6	55.7	8.2	9.9	3399
			(0.3)	(0.3)	(0.5)	(0.5)	(0.2)	(57)
South	HMO	3527	11.6	18.7	51.8	8.8	9.1	3341
			(0.2)	(0.4)	(0.6)	(0.5)	(0.2)	(58)
	POS	4120	11.6	18.9	51.6	8.8	9.1	3391
			(0.2)	(0.4)	(0.6)	(0.5)	(0.2)	(59)
	PPO	21,883	11.2	19.0	52.1	8.7	9.0	3498
			(0.2)	(0.4)	(0.5)	(0.5)	(0.2)	(60)
	Comp	2304	11.2	18.5	52.2	9.0	9.1	3423
			(0.2)	(0.4)	(0.5)	(0.5)	(0.2)	(59)
West	HMO	2910	17.7	15.1	52.0	7.4	7.9	3605
			(0.4)	(0.4)	(0.7)	(0.5)	(0.4)	(61)
	POS	485	18.4	14.8	50.6	7.9	8.2	3517
			(0.4)	(0.4)	(0.9)	(0.5)	(0.4)	(57)
	PPO	4115	17.5	14.8	52.5	7.3	7.9	3613
			(0.4)	(0.4)	(0.8)	(0.4)	(0.4)	(53)
	Comp	521	18.0	14.8	51.8	7.4	7.9	3367
			(0.4)	(0.4)	(0.7)	(0.4)	(0.4)	(56)

Note: Rows show the estimated treatment shares and total physician payments that would result from all patients in the corresponding region having insurance that pays like the corresponding plan type. The *N* column displays the number of patients in the corresponding region and plan type.

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