PATIENT COST-SHARING, HOSPITALIZATION OFFSETS, AND THE DESIGN
OF OPTIMAL HEALTH INSURANCE FOR THE ELDERLY

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ABSTRACT

Patient cost-sharing for primary care and prescription drugs is designed to reduce the prevalence of
moral hazard in utilization. Yet the success of this strategy depends on two factors: the elasticity of
demand for those medical goods, and the risk of downstream hospitalizations by reducing access to
beneficial health care. Amazingly, we know little about either of these factors for the elderly, the most
intensive consumers of health care in our country. We remedy both of these deficiencies by studying
a policy change that raised patient cost-sharing for retired public employees in California. We find
that physician office visits and prescription drug utilization are very price sensitive; while direct comparison
is difficult, the price sensitivity appears to greatly exceed that of the famous RAND Health Insurance
Experiment (HIE). Moreover, unlike the HIE, we find large "offset" effects in terms of increased hospital
utilization in response to the combination of higher copayments for physicians and prescription drugs.
These offset effects are concentrated in patients for whom medical care is presumably efficacious:
those with a chronic disease. Finally, we find that the savings from increased cost-sharing accrue mostly
to the supplemental insurer, while the costs of increased hospitalization accrue mostly to Medicare;
thus, there is a fiscal externality associated with cost-sharing increases by supplemental insurers. Our
findings suggest that optimal insurance should be tied to underlying health status, with chronically
ill patients facing lower cost-sharing. We also conclude that the externalities to Medicare from supplemental
insurance coverage may be more modest than previously suggested due to these offsets.

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The elderly are the most intensive consumers of health care in the United States today. Individuals over age 65 consume 36% of health care in the U.S., despite representing only 13% of the population (CMS, 2005). The Medicare program that insures the nation's elderly (as well as the disabled) is the third largest expenditure item for the Federal government, and is projected to exceed Social Security by 2024 (Board of Trustees, 2005). This rapid growth in program expenditures was reinforced by the introduction last year of Medicare Part D, a new plan providing coverage for the outpatient prescription drugs used by Medicare beneficiaries.

The Federal government has undertaken a variety of strategies to control Medicare program growth on the supply side, from the introduction of prospective reimbursement for hospitals to reductions in provider reimbursement rates. Yet Medicare spending growth has continued unabated. Recently, therefore, there has been a growing interest in demand-side approaches to controlling system costs, through higher patient costs which would induce more price sensitivity in medical spending.

Using demand-side approaches to constrain the growth of Medicare spending is hampered by three difficulties. First, surprisingly, we have no evidence on the price sensitivity of the elderly to point-of-service medical costs. For the non-elderly, the question of the sensitivity of medical consumption to its price was addressed by the famous RAND Health Insurance Experiment, one of the most important pieces of social policy research of the post-war period. The RAND HIE randomized individuals across health insurance plans of differing generosity with respect to patient costs, and the results showed that higher patient payments significantly reduced medical care utilization, without any adverse health outcomes on average (Manning et al. (1987), Newhouse (1993)). While this evidence is now almost thirty years old, it has remained the "gold standard" for

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evaluating the impact of patient-side cost-sharing and the design of insurance benefits.

However, the HIE estimates the elasticity of demand in a fee-for-service environment that may exhibit very different responsiveness to prices than the world of managed care. Moreover, changes in the practice of medicine, including the increased use of prescription drugs, the growth of imaging and diagnostic technology and the development of minimally invasive surgery, may imply a structural change in the elasticity of medical demand and the health impacts of any utilization reductions. Finally, even absent other changes, it is unclear whether the results of the HIE apply to the elderly population that was not included in this social experiment.

Second, there may be particular concern that increased cost-sharing for the elderly can cause offsetting rises in medical costs elsewhere in the system. The HIE did test this "offset effect" for the non-elderly, and found no evidence, for example, that higher outpatient cost-sharing led to more use of inpatient services. Recent research on prescription drug utilization by the non-elderly, however, has shown that higher copayments lead to lower utilization of maintenance drugs for chronic illness, which implies (although it has yet to be proved) that an offset effect may exist (Goldman et al., 2006). Once again, this is a major concern for the elderly who are the most intensive users of expensive medical services.

A final complication is the fact that Medicare already has quite substantial cost-sharing requirements, with an uncapped 20% coinsurance rate for physician services and a high deductible for hospital utilization. As a result, most enrollees have some form of supplemental coverage for their medical spending, either provided by an employer, purchased on their own, or provided through state Medicaid programs. Thus, introducing demand-side price sensitivity requires involving these supplemental insurers who control the first-dollar exposure of the elderly to medical

costs. This can be difficult because the interests of the first-dollar insurer and the last-dollar insurer may not be readily aligned. In particular, there are major concerns about the fiscal externality on Medicare from supplemental coverage; by providing first-dollar coverage, these policies increase utilization, raising the costs to Medicare. This is a similar insight to that developed by Goldman and Philipson (2007), who argue that optimal health insurance should account for the cross-price elasticities between the insured good and other services; if these other services are subsitutes for the insured good, then copayments ought to be lower than in a model that ignores the interdependency.

In this paper, we provide new evidence on all three of these issues by studying a major set of copayment changes in a modern, managed care environment. Facing mounting fiscal pressure from health plan cost increases, the California Public Employees Retirement System (CalPERS) Board, the third largest purchaser of health insurance in the United States, enacted a series of substantial copayment increases in February 2001. For the remainder of 2001, copayments were increased for the Preferred Provider Organizations (PPOs) that were offered by the state to its employees. Then, beginning in 2002, copayments were increased for the remaining enrollees in Health Maintenance Organizations (HMOs) operating under contract to CalPERS. Importantly, these changes occurred not just for active employees, but for retirees receiving supplemental insurance through the plans as well. The result was a staggered set of copayment changes that allow us to carefully evaluate their impact on the medical care utilization of the elderly.[1]

To evaluate these policy changes, we have compiled (with the assistance of CalPERS) a comprehensive database of all medical utilization data[2] for those enrolled continuously in several of the CalPERS plans from January, 2000 through September, 2003. This database has information on

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[1] We can, of course, also study utilization of the non-elderly, but this is of less interest because of existing studies. We intend to investigate responses among the non-elderly in future work.
[2] All claims information used was de-identified prior to receipt for both CalPERS and study researchers.

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every medical contact, allowing us to comprehensively study the effect of these copayment policy changes on a wide variety of medical utilization outcomes.

We have a number of interesting findings. First, we find that both physician office visits and prescription drug utilization are very price sensitive among the elderly. While direct comparisons to the RAND HIE are difficult, due to the very different nature of these price changes, the implied elasticities are much larger than those found in earlier work. Second, unlike the HIE, we find significant "offset" effects in terms of increased hospital utilization in response to the combination of higher copayments for physicians and prescription drugs. These offset effects are concentrated, however, in the most ill populations, particularly those who had a chronic illness or who had high previous medical spending. Finally, we find that the savings from increased cost-sharing accrue mostly to the supplemental insurer, CalPERS, while the costs of increased hospitalization accrue mostly to Medicare. As a result, when supplemental insurance cost-sharing rises, the net fiscal benefits for Medicare are much smaller than previously recognized.

Our paper proceeds as follows. Part I provides some background on previous work in this area and on the policy change we are studying. Part II describes the data and empirical strategy. Part III presents our basic set of results on price sensitivity. Part IV then extends the results in a variety of directions. Part V concludes.

Part I: Background

Previous Work on Price Sensitivity of Medical Demand

The previous literature on the impact of copayment changes can be broken down into three "eras". The first is the work on elasticities of demand that pre-dated the RAND HIE. Most of this

work simply regressed medical care spending on the level of coinsurance, either at the family or the state level (e.g. Feldstein, 1971; Rosett and Huang, 1973). This work faced the fundamental problem that the level of coinsurance is a function both of choice of insurance plan and of previous medical spending (since the level of coinsurance varies by whether individuals are below or above the deductible level, for example), and both of these are quite likely to be correlated with contemporaneous medical spending. One of the earliest studies to use quasi-experimental variation was Phelps and Newhouse (1972). This study examined the impact of a change in cost-sharing in the Stanford medical plan, but had no control group to capture general time trends in demand that might confound their analysis.

The second era is the RAND HIE, which is summarized in Manning et al. (1987) and Newhouse (1993). In the wake of the unconvincing evidence from the first era of the literature, the government, in the mid-1970s, funded a massive social experiment to evaluate the impact of cost-sharing on demand. Families who enrolled in the HIE were randomly assigned to plans with widely varying coinsurance and deductible amounts. This random assignment guaranteed that the results could be used to cleanly infer the impacts of varying coinsurance rates on demand. The HIE showed that medical services were modestly price responsive, with an overall estimated elasticity of medical spending in the range of -0.2.

There were a variety of other important findings that are summarized in Newhouse (1993), but three stand out for our purposes. First, the reduction in medical utilization was relatively uniform: for almost every category of care, utilization fell for both (in theory) "effective" and "ineffective" care. Second, despite the reduced medical care that resulted from high patient coinsurance, those in high coinsurance plans were in no worse health, on average. Even among

subgroups such as the poor or the unhealthy, there was no systematic relationship between patient coinsurance and health. There is one exception that has received much attention: the study found sizeable advantages of free care for the share of the sample that was both low income (bottom 20% of the income distribution) and chronically ill (least healthy 25% of the sample). In particular, such individuals saw large declines in their blood pressure on the free care plan relative to the coinsurance plans; improvements in respiratory functioning; and improvements in vision and dental health.[3] Finally, the study found no "offset effects": there was no evidence that high coinsurance, by causing individuals to forgo efficacious preventive care, would raise costs through inappropriate care later on (particularly at the hospital). On the experimental plan with higher outpatient coinsurance, but no inpatient charges, hospital utilization actually fell rather than rose.

The third era includes studies subsequent to the HIE: Cherkin, Grothaus and Wagner (1989) studied the introduction of a $5 copayment rate for state employees enrolled in an HMO in Washington state, in the mid-1980s, relative to federal employees. They find a sizeable reduction in office visits. Selby, Fireman and Swain (1996), examined the introduction of an emergency room copayment for some firms insured by the Kaiser Permanente HMO plan in the early 1990s, relative to a control group of those firms who did not see this copayment increase. They find a significant decline in emergency room utilization, with no evidence of adverse impacts on health. Eichner (1996) studied the impact on utilization in a fee-for-service plan, using the fact that adults in a family face different prices as injuries to their children push them above out-of-pocket cost sharing limits, and he finds an elasticity of spending of -0.3 to -0.4.

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[3] While this potential caveat has garnered much attention, however, it is important to recognize its limitations. First, none of the estimated effects on the health of this group are statistically significant in a traditional sense; the samples are simply too small to have much confidence in these results. Second, as Newhouse (1993) emphasizes, there is no evidence of overall health benefits for this group, just evidence along these three measures; for other measures, such as varicose veins, health is worse in the free care group.

There is also a recent and sizeable literature that focuses on the price elasticity of demand for prescription drugs. Studies such as those by Goldman et al. (2004) and Landsman et al. (2005) find that prescription drug use is price sensitive, with low elasticities of around -0.1 for drugs used to maintain chronic conditions (such as ACE inhibitors or statins), but much higher elasticities of around -0.3 to -0.4 for drugs used for acute conditions (such as Cox-2 inhibitors for pain management).

These studies have two important limitations. First, they are exclusively focused on the non-elderly, ignoring the elderly patients who are responsible for a massive, and growing, share of public health spending. There are studies that focus on elders, as reviewed by Rice and Matsuoka (2004), but virtually all of these studies are either cross-sectional comparisons of elders with and without generous drug coverage, or simple before/after comparisons of drug copayment changes (which suffer from the problem of uncontrolled trends in drug utilization). The one exception is Johnson et. al. (1997), who studied an increase in copayments in one Medicare HMO, using a different Medicare HMO as a control. This study finds no consistent impact of changes in copayments on drug utilization. The second major limitation of this previous literature is that it does not go on, as RAND did, to examine the implications of changing utilization for either health or utilization elsewhere in the medical system.[4]

There is a related literature on the elderly's medical utilization, which examines the fact that

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[4]Recent work by Goldman et al. (2006) argues that higher copayments for prescription drugs leads to more hospitalizations, but this claim is based on combining the fact that higher copayments lead to less drug utilization with an estimate of the relationship between drug utilization and hospitalization. The latter parameter is obtained by comparing hospitalization rates among those who do and do not comply with their drug regimes. This may not reflect the reduction in drug use due to copayments so much as the type of individuals who do and do not comply.

some elders cover the "holes" in Medicare through supplemental "Medigap" insurance plans.[5] These studies find a sizeable impact of Medigap coverage on medical spending. None of the papers, however, are able to fully surmount the important selection problem in the holding of Medigap policies documented by Ettner (1997). Studies such as Lillard and Rogowski (1995) use past work history as an instrument for employer-provided Medigap coverage, but past work history is itself likely correlated with demand for medical care and health. Moreover, none of these studies assesses the impact of varying copayments for the elderly, only the aggregate impact of having or not having supplemental insurance.

This literature does, however, raise the important issue of the potential *externality* exerted by supplemental coverage on the traditional Medicare program. If copayments do lower utilization, then, by covering copayments, supplemental coverage raises utilization. Much of this increased utilization, however, comes in the spending range covered by Medicare, so that supplemental insurance raises Medicare costs. Despite the importance of this fiscal externality, there are no convincing estimates of its magnitude.

A new paper by Finkelstein (2007) paints a more dramatic picture by looking at the introduction of the Medicare program. Using variation in pre-existing levels of insurance coverage before the program was introduced, Finkelstein finds an enormous response of spending to the provision of insurance coverage, with an implied elasticity that is six times larger than in the HIE. This suggests either that the elderly are more price-sensitive than the non-elderly, or that a system-wide change of this nature had a bigger effect than individual randomization.

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[5] References include McCall et al. (1991), Wolfe and Goodeeris (1991), Lillard and Rogowski (1995), Hurd and McGarry (1997), and Lillard, Rogowski, and Kington (1999).

The Institutional Setting

Our analysis focuses on changes in copayments for the health plans offered to public employees by the state of California through the CalPERS program. This program provides insurance to 1.2 million of California's active and retired civil servants and their dependents, making it the third largest purchaser of health insurance in the nation (behind the Federal Government and General Motors) (CalPERS, 2006). Employees are offered a choice of two state-run Preferred Provider Organizations (PPOs), PersCARE and PersChoice, as well as a variety of state-sanctioned HMO choices that have changed over time. Choices are made every year during the month-long open enrollment period, between mid-September and mid-October. For retirees, our population of interest, the state provides supplemental insurance that "wraps around" the coverage provided by the Medicare program.

The empirical approach of this project revolves around the policy change put in place by CalPERS. The genesis of this policy change was the rapidly rising health care costs in the self-funded CalPERS plans. There was a 47% increase in PPO enrollment from 1997 through 2000, with another 25% increase expected for 2001, due partly to movements out of the HMO plans and lack of HMO availability in some areas. There were also significant rises in costs per member, due to both increased provider prices (reflecting the nationwide trend towards tougher bargaining by providers with managed care organizations) and increased utilization, particularly of prescription drugs. As a result, the plan reserves had fallen from a level equal to 6.3 months of claims experience to basically zero (so that premium income was roughly equal to claims expenditures) (CalPERS, 2000).

In response to these pressures, the board of CalPERS in December 2000 voted to raise the

copayments in their PPO plans as of February 2001. They then decided, in April 2001, to follow suit for the HMO plans that contract through CalPERS, beginning in January 2002. This policy change included a variety of copayment changes for the non-supplemental population. For the supplemental population, there were two changes:

1. A rise in physician office visit copayments for the HMOs from $0 to $10 in 2002. There was no corresponding change for PPOs in 2001.

2. A rise in copayments for prescription drugs for all PPO plans from $5 for generics and $10 for name brands to $5 for generics, $15 for formulary name brands, and $30 for non-formulary name brand prescriptions, as well as a rise in mail order prescription copayments from $5 to $10/$25/$45, with a $1000 stop-loss in 2001. HMOs had a similar rise in copayments for prescription drugs in 2002; for HMOs, however, copayments prior to the policy change were $1 for almost all enrollees, and did not differ for generics and name brands.[6]

Part II: Data and Empirical Strategy

Data

For our analysis, we use a comprehensive database of all medical utilization data for four of the CalPERS plans between January, 2000 and September, 2003. We focus on a panel of Medicare supplemental plan members who were continuously enrolled in their plan during our sample period. The resulting data set includes information on medical utilization by 70,912 members; 93% of these members were over the age of 65 in January 2000.

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[6] The copayment was $1 for one of our HMOs and $5 for the other, but the former HMO represents 98% of the continuously enrolled HMO sample.

By selecting a sample of continuously enrolled individuals, we risk potentially mismeasuring the population responsiveness of individuals to copayment changes. If individuals switch out of plans raising copayments, and the individuals who switch have a sensitivity of medical care utilization with respect to price that is higher (lower) than average, then our estimated elasticities in this sample will be biased downward (upward). Such switching, however, does not seem to be of sufficient importance to bias our results. We find, for example, that 93% of the members who were enrolled in our PPOs in January 2000 remained enrolled in the same plan in January 2001, despite the anticipated copayment increases in February 2001. Indeed, this 93% retention rate was slightly *higher* than the 91% retention rate, over the same period, at HMOs that were not expecting a copayment increase. Similarly, 92% of the members who were enrolled in our HMOs in January 2001 remained enrolled in the same plan in January 2002. Again, despite the January 2002 copayment increase, the HMO retention rate was slightly *higher* than the 90% retention rate for the PPOs over the same period. As a result, when we re-estimate our models using the full sample of enrollees (including individuals who both switch plans and exit CalPERS), we get almost identical results to those presented here.[7]

For each individual, we measure office visit utilization by the number of medical encounters which occurred in an office or outpatient setting during the month. We measure drug utilization by the number of prescriptions filled during the month. We measure hospital utilization with an indicator for whether or not the individual spent any days in the hospital during the month.

In our analysis, we compute an average of each utilization measure for each plan in each

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[7] One exception to this statement is our results for hospitalizations. In the full sample, we get a small negative effect on overall hospitalizations for the 2002 change, rather than the small positive effect reported below. But this appears to reflect pre-existing trends in the full sample of data, and our key results on hospitalization for the chronically ill are very similar. All the full-sample results are available from the authors upon request.

month, which yields 180 observations in our final data set (45 months*4 plans). Thus, each of the 180 observations on a utilization measure in our final data set reflects the average utilization per person among all continuously enrolled members of a single plan in a given month. In order to document the impact of the policy change on copayments, we also calculate average copayments and deductibles across all visits (or prescription drug purchases) for each plan in each month. Each of the 180 observations on copayments in our final data set reflects the average copayment per visit (or prescription drug purchase) in a single plan in a single month.[8]

We analyze the impact of these policy changes on medical spending as well as utilization. Ideally, we would do this by having the payments associated with every medical encounter, and we would redo our analysis using these payments as the dependent variable. In practice, however, there is not any financial information on payments attached to HMO claims. We therefore pursue an alternative approach where we use available data on total payments from PPO claims to impute total payments to each claim based on the primary diagnosis category of the office visit (or the NDC code of the prescription drug), and the average payment for an office visit with that diagnosis category (or for that specific drug) among all PPO claims. Following Medicare Part B policy, we assume that Medicare made 80% of the total payments for office visits and that the CalPERS supplemental insurance plan paid the difference between the remaining 20% and any patient copayments. For drugs, we assume that the CalPERS supplemental insurance plan made all payments above the patient copayments.

For hospitalizations, where Medicare's share of total payments is not a fixed percentage, we followed a different methodology. Total hospital payments are the sum of imputed CalPERS

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[8] When we analyze the 2002 copayment increase for office visits, we exclude office visit data from 2000. One of the plans changed their coding of office visits between 2000 and 2001, generating a discontinuity in the data that is unrelated to any policy changes. To avoid any spurious results based on this reporting change, we simply exclude all office visit data prior to 2001.

insurance payments, imputed Medicare payments, and any actual cost-sharing paid by the patient. We imputed the first component based on the diagnosis code of the hospitalization and the average insurance payment for hospitalizations with the same diagnosis in the PPO data. We imputed Medicare's payments based on the diagnosis code of the hospitalization and the average Medicare payment for hospitalizations with that diagnosis code, using the universe of Medicare claims in the state of California during our sample period.[9] For cost-sharing, we used the actual amounts that were reported in the data. We then constructed total hospital payments as the sum of these three payments.

A clear concern with our approach for physician and hospital visits is that we assign the *average* payments per diagnosis, whereas ideally we would like to use the *marginal* payments for the given admission. If we had complete data on all procedures performed at the hospital and at the office, we could do this, but our data include limited information on procedures. The bias from using average rather than marginal charges is unclear, but we have explored one exercise to assess its importance. Among those admitted to the hospital, we have regressed length of hospital stay on the policy dummy in our difference-in-difference framework. If the marginal patients admitted due to higher physician/drug copayments are very different than the average patient admitted, then we should see a marginal change in the length of stay for hospital patients. In fact, there is no significant effect on length of stay, providing at least crude evidence that the marginal and average

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[9] We used a 100% sample of Medicare Part A payments for patients whose residence was in California at the time of the hospitalization; Medicare patients who reside in other states but were in California at the time of hospitalization were excluded. For this sample of patients, we constructed average Medicare payments by principal diagnosis code for hospitalizations that occurred between January 2000 and September 2003. In order to reduce sampling error associated with rare diagnoses, diagnosis codes were first aggregated to levels indicated by the Clinical Classification System (CCS). To capture payments made to physicians for in-hospital services rendered during these hospital stays, we used a 20% random sample of the above Part A sample for whom we had Part B claims. We further restricted these claims to those whose dates corresponded to a hospitalization (including the dates of admission and discharge) and whose service place was an overnight inpatient visit. We merged payments for these services with the corresponding Part A hospitalization. This analysis was performed at the Center for the Evaluative Clinical Sciences at Dartmouth Medical School, and the relevant SAS programs are available from the authors on request.

patients are not very different.

Empirical Strategy

Our analysis of this quasi-experimental change in CalPERS policy is fairly straightforward. We begin by estimating difference-in-difference models of the form:

$$UTIL_{pt} = \alpha + \beta HIPAY_{pt} + \delta_p + \lambda_t + \varepsilon_{pt}$$

Where UTIL is a measure of utilization (or out-of-pocket costs) for plan p in month t, α is a constant term, HIPAY is a dummy for increased copayments in plan p in month t (specifically, an interaction of an indicator variable for being in a plan where greater cost-sharing is instituted and an indicator for being in the post-increase era), and δ_p and λ_t are plan and month fixed effects, respectively. In this model, the effect of the copayment change is identified by β, which measures the change in utilization in the plans with a copayment change relative to those without. For each type of utilization, we estimate two models of this type: one which separately identifies effects on PPOs (focusing on data from January 2000 to December 2001), and another which separately identifies effects on HMOs (focusing on data from February 2001 to September 2003). Regressions are weighted by the number of plan members who are continuously enrolled during our sample period in each plan.

There are two potential problems with this approach. First, utilization is likely autocorrelated within plans; this autocorrelation causes the standard errors in ordinary regressions to be understated. To address this issue, we estimate our regressions using Generalized Least Squares (GLS) allowing for plan-specific autocorrelation as well as plan-specific

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heteroskedasticity.[10]

Second, there may be underlying trends in utilization, which can confound the estimation of the causal effects that we are interested in. A particularly worrisome source of such trends for the HMO analysis is the earlier PPO policy change. That is, our difference-in-difference analysis for the HMO policy change compares the change in utilization in the HMOs between the pre-policy period (February 2001 – December 2001) and the post-policy period (January 2002 – September 2003) to the change in utilization in the PPOs over the same periods. If the policy change in the PPOs had immediate effects in February, 2001, this is not a problem. But it is possible that utilization in the PPOs adjusted slowly to the PPO policy change, with full adjustment only by the end of 2001. If this were the case, absent any other change, there would be a negative utilization difference between 2001 and 2002 for the PPO, leading to a spurious positive difference-in-difference estimate of the effect of the 2002 policy change on HMO utilization. To deal with this concern, we present dynamic models of the policy change effect, estimating separate treatment effects for each quarter before and after the policy change.[11] In this way, we can examine whether any changes in utilization in 2002 represent the effects of a slow-moving relative trend or a sharp break when the policy is put in place. If the dynamic model indicates the latter, it suggests that we are not just picking up the dynamic effects of the PPO policy change.

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[10] This is a non-trivial issue that we have explored from a variety of angles. The GLS procedure reports $\hat{\beta} = (X'\hat{\Omega}^{-1}X)^{-1}X'\hat{\Omega}^{-1}y$, and its variance as $\text{var}(\hat{\beta}) = (X'\hat{\Omega}^{-1}X)^{-1}$. The relevant variance-covariance matrix is estimated as $\hat{\Omega} = \Sigma \otimes I_{TxT}$, and operationalized with $\hat{\Sigma}_{i,j} = \hat{e}_i \hat{e}_j / T$ (where T is the number of time-periods and Σ is a square matrix of dimension equal to the number of plans). The most natural solution to our problem would be to follow the insights of Bertrand, Duflo and Mullainathan (2004) and cluster on the four plans. However, the approach relies critically on the asymptotic justification that the number of clusters goes to infinity; with too few clusters the cluster-robust standard errors are biased downwards severely. Empirically, we found that this approach produced standard-errors that were 0%-35% smaller than those obtained from our GLS procedure. Alternatively, if we ignore the potential auto-correlation and cluster on plan-by-month (45 months x 4 plans = 180 clusters) we obtain standard-errors that are similar, though 0%-20% smaller, to those obtained from the more conservative GLS procedure. We thank Doug Staiger and Doug Miller for helpful conversations on this topic .

[11] In addition, we tested the sensitivity of our 2002 policy change results to excluding data from the first 3 months after the 2001 policy change when into effect (February, March, and April of 2001) and found that the results were unaffected by this exclusion.

Means

Table 1 presents the means of the data. We show the mean utilization rate and coinsurance

rates for each type of utilization for HMOs and PPOs in each year. Once again, we have no pre-

period data for the PPO policy change for office visits due to data limitations. Several

discontinuities that preview our ensuing difference-in-difference specifications are apparent in these

tabulations. First, average copayments for an office visit jump from $0.14 in 2001 to $10.11 in

2002 for the HMOs, while they remain flat at below $1 in the (control) PPO plans over time.[12] Over

the same period, average office visits fell by 0.03 per member per month in the HMOs (relative to

an increase of 0.07 visits per member per month in the PPOs, which experienced no copayment

increase). Thus, the means suggest a differential decline in office visits for HMO members at the

time that office visit copayments increased for them.

Average out-of-pocket payments for a prescription drug increased from $6.93 in 2000 to

$13.50 in 2001 for PPOs. Over the same period, average drug utilization rose by 0.09 prescriptions

per member per month in the PPOs, relative to an increase of 0.16 prescriptions per member per

month in the HMOs, which had not yet experienced a drug copayment increase. These means, then,

suggest the possibility of a relative decline in drug utilization resulting from the increased drug

copayments for the PPOs in 2001. For the HMO policy change, the means show an increase in

prescription drug copayments from $1.27 in 2001 to $7.63 in 2002. They also show a decrease of

0.09 prescriptions per member per month in the HMOs between 2001 and 2002, relative to an

increase of 0.14 drugs per member per month in PPOs during the same period. Once again, the

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[12] The fact that the average copays in our data are not *precisely* $0 for the HMOs in 2001 or for the PPOs throughout the sample period may reflect the fact that some of the encounters that we identify as "office visits" were misclassified or it may reflect some minor misreporting of copayments in the data.

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means suggest a relative decline in drug utilization resulting from the increased drug copayments for the HMOs in 2002.

Finally, we show means for hospitalizations for each year and type of plan. All hospitalization rates in Table 1 and in later regression tables are multiplied by 10,000, in order to make the numbers easier to read. Hospitalization rates increase over time for both types of plans throughout the sample period, presumably reflecting the aging of our panel. Table 1 does not suggest an obvious increase or decrease in hospitalizations associated with the policy changes

Part III: Basic Results

Office Visits

We begin our analysis by examining the effect of the 2002 copayment increase on office visits in the HMOs. This policy change increased copayments from a base of $0 to $10 for those in the supplemental plan. The results of our analysis are shown in Table 2. Each cell reports a coefficient and standard error (in parentheses).

The first column shows the basic difference-in-difference estimate of the impact on copayments per visit. The statistically significant coefficient of 10.06 is virtually identical to the $10 increase that we expect.

The second column of the table shows the difference-in-difference estimate of the impact on office visit utilization. There is a sizeable and highly statistically significant reduction of 0.132 office visits per member per month. Relative to the pre-period mean of 0.753, this is a 17.5% decline in office visits. This is a very large response: a $10 copayment increase led to an almost 20% decline in visits.

The implied effects of this result are much larger than those produced by the RAND HIE.

The $10 copayment rise represents a 9.2% rise in coinsurance rates relative to the baseline mean cost of a doctor's visit of $108.42. Thus, relative to this 9.2% rise in coinsurance rates, the 17.5% decline in visits implies a very large elasticity of -1.90. This elasticity is much larger than the HIE, but is in line with the aggregate elasticity implied by Finkelstein (2007). But this comparison may be misleading. Under the HIE, individuals were paying an explicit coinsurance rate, whereas here they are paying a flat copayment and may not even be aware of the coinsurance rate represented by that copayment. Therefore this large elasticity may not extrapolate to marginal changes in copayments beyond $10.

The third column explores the dynamic pattern of coefficients by showing the effects for each of the three quarters before, the quarter of, and all quarters after the policy change; the results are shown graphically in Figure 1. All coefficients for the dynamic model are measured relative to the omitted quarter, which is 2 quarters prior to the policy change. There is a large rise in office visits in the quarter before the policy change, which presumably reflects anticipation of the change, and then a large and immediate drop in utilization in the quarter after the policy change. This graph clearly suggests a causal impact of the copayment increase itself, and not a spurious trend in utilization.

Much of the large rise immediately before and fall immediately after the copayment change likely represents a one-time shift in the timing of office visits, and not a fundamental change in utilization patterns in response to this higher patient cost. Thus, in the last column of the table, we show the difference-in-difference estimate excluding the quarter immediately before and after the policy change (the estimated change in copayment and coinsurance is almost identical when we excluded these quarters). There is a sizeable decline in the coefficient on office visits to -0.095,

although it remains highly significant. The implied elasticity is -1.38. We will use this specification as our baseline for the remaining results on office visits.

Prescription Drugs

There were large increases in copayments for prescription drugs in both 2001 (for PPOs) and 2002 (for HMOs), as described earlier. Unlike physician office visits, however, the exact magnitude of the copayment change for each member depends on the mix of drugs they use, as the copayments changed differently for different types of drugs.

Table 3 shows estimated effects for the 2001 and 2002 policy changes. The effects of these changes on copayments per drug are shown in columns 1 and 5, respectively. For the 2001 policy change, we estimate a $7.25 rise in average copayment. For the 2002 policy change, the estimated change in out-of-pocket charges is slightly smaller at $6.74. However, these first stage estimates confound two effects: the static effect of rising copayments, holding constant the pre-policy mix of drugs, and the dynamic effect of consumers shifting to less expensive drugs in response. We believe that the correct first stage estimate would include only the first, static part of this response and ignore the second portion. To estimate this static copayment change, we measure utilization in the pre-policy period, multiply by both old and new copayments, and calculate the difference in copayments for this fixed set of drugs. Using this methodology, we get larger estimates of the copayment increases -- $8.06 for the 2001 change, and $7.26 for the 2002 change.

The effects of these changes on the number of prescriptions are shown in the next columns (columns 2 and 6), using the basic difference-in-difference specification. In each case, there is a negative and significant effect on the average number of prescriptions filled. For PPO enrollees in

2001, there is a reduction in prescriptions filled of 0.110 per member per month, which is 5.6% of the pre-policy mean of 1.97. The $8.06 rise in average copayments in 2001 represented 12.3% of the average drug payment of $65.30 by PPOs before the copayment increase. This implies an elasticity of approximately -0.46 of drug utilization with respect to its patient cost. For HMO enrollees in 2002, there is a reduction in prescriptions filled of 0.276, which is 20% of the pre-period mean for that group of 1.39. The $7.26 rise in copayments for this population represents 14.2% of the average payment for drugs of $50.96 before the copayment increase. This implies an elasticity of -1.4.[13]

The dynamics of these responses are reported in columns 3 and 7 in Table 3 and are shown graphically in the two panels of Figure 2. The results clearly indicate that there were no pre-existing trends towards less drug use. Thus, the estimated utilization decline appears to be a causal impact of the policy change. For the 2001 change, however, we once again see that there is an anticipation effect in the quarter before the policy change. To avoid such timing effects, we once again drop the quarter before and after the policy change, and show the results in Columns 4 and 8 of Table 3. In the case of the 2001 policy change, the estimated effect decreases by about a half, so that the implied elasticity falls to only -0.20.

Once again, it is important to note that these elasticities are not directly comparable to the HIE to the extent that individuals aren't aware of full drug prices and are just reacting to copayments. The main conclusion here is that there is substantial price sensitivity to the 2002 change, and much less for the 2001 change. There are at least two explanations for the larger response in 2002. The first is that there are interactions with the 2002 rise in physician copayments

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[13] Using the alternative estimates of the copayment increase, which incorporate both the static and dynamic effects of policy change, the implied elasticities are -0.5 for the 2001 policy change and -1.5 for the 2002 policy change. Therefore, the use of the more sophisticated (and correct) method of estimating copayment changes does not dramatically alter our results.

that led to a fall in physician visits – and may, as a result, have led to fewer prescriptions as well. We can test this hypothesis by examining the impact of the copayment increase for prescription drugs on new versus refilled prescriptions. Physician visits are not required for refilled prescriptions, so if the effect is solely on refills, then it suggests that the impact is purely on compliance with previously prescribed regimes, and not through reduced physician contact. In fact, that is exactly what we find: no effect on new prescriptions, with all of the effect concentrated in prescription refills.[14] Thus, the interaction with rising physician copayments does not seem to be the cause of this fall in prescription drug utilization.

A second potential explanation is the different nature of the copayment changes for prescriptions in the PPO and HMO plans. In the PPO plans, there was no change in the copayments for generic prescriptions, and a tripling of copayments for branded drugs on the formulary. In the HMO plans, there was a large rise in generic copayments (from roughly $1 to $5), in addition to a very large rise in branded formulary copayments (from roughly $1 to $15). The very different nature of these changes might lead to larger HMO effects in two ways.

First, it may be that individuals respond differently to much larger percentage changes in copayments than to smaller percentage changes. For instance, HMO members experienced 400% increases in branded formulary copayments, while PPO members experienced only 50% increases in branded copayments, so the HMO members may have perceived their copayment increases to be much larger than PPO members. Indeed, if we calculate elasticities not on the basis of coinsurance rates (as in the HIE), but rather as a function of the percentage change in copayments, we find that

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[14] We classify each prescription as "new" or as a "refill," based on whether the individual has a prior claim for the same drug. We then use these variables as dependent variables in our difference-in-difference regression framework, excluding data from January - June 2000, due to the over-classification of "new" drugs at the beginning of the sample period. For the 2001 policy change, we find a small, insignificant increase in new prescriptions and a statistically significant decline of 0.087 refills per member per month (5% of the pre-policy PPO mean). For the 2002 policy change, we again find a small, insignificant increase in new prescriptions and a statistically significant decline of .223 refills per member per month (20% of the pre-policy HMO mean).

the elasticities for the two policy changes are quite similar: -0.048 in 2001 and -0.035 in 2002.

Second, the fact that only some drugs became more expensive for the PPO plans, while all drugs were more expensive for the HMO plans, might have led to more substitution out of drug use in the HMOs. That is, individuals taking generic drugs in the PPO plan saw no price change, while there was a proportionally large price change for HMO customers taking generic drugs in the HMO plan. To assess this issue further, we divided the prescription drugs in our analysis into formulary tiers and examined the behavioral response to copayment changes in the each tier.[15] Consistent with the notion that, in the PPO policy change, the increase in the price of higher-tier drugs (with no change in the price of generics) should induce greater use of generics, we found a statistically significant *increase* of 0.032 generic drugs per member per month. Simultaneously there were statistically significant reductions of 0.028 and 0.041 drugs per member per month for the 2nd (formulary) and 3rd (non-formulary, retail) tiers, respectively. In contrast, when we estimated our difference-in-difference models for the HMO policy change, we noted statistically significant decreases of 0.064 and 0.1664 drugs per person per month for generics and formulary tiers respectively (there was no third formulary tier for the HMOs).

Our experiment also allows us to explore the behavioral response to different types of drugs. By assembling a panel of three practicing physicians and two pharmacists, we classified each drug into one of three categories:[16] (1) Acute care drugs are those that, if not taken, will increase the probability of an adverse health event within a month or two (examples are anticonvulsants, anti-

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[15] Specifically, we estimated separate DD models for the 2001 and 2002 policy changes by formulary tier. For each policy change, we defined the control group of drugs as being the same drugs as those whose copayment changed by virtue of the change in the formulary in the treatment group. For example, in the PPO policy change, if Lipitor was classified as a formulary drug (whose copayment went from $5 to $15), then we defined the corresponding HMO treatment group comprising this drug.

[16] Consistent with the clinical literature on expert physician panels, our panel exhibited considerable variance of opinion, especially in the distinction between acute and chronic care drugs. We classified a drug as belonging to a certain class if the majority of experts believed that it was in that class. During their deliberations, our panels had access to the Physician Desk Reference and the internet to inform their choices.

malarials and anti-angials, coronary vasodilators and thrombolytics). (2) Chronic care medications are designed to treat more persistent conditions that, if not treated, will result in a potentially adverse health event within the year (examples include analgesics, antivirals, ACE inhibitors, antigout medications, beta-blockers, hypertension drugs, statins, and glaucoma medications). The final category includes medications which, while necessary to improve the quality of life, will not result in an adverse health event if not taken, by virtue of their providing symptom relief as opposed to affecting the underlying disease process (examples are acne medications, antihistamines, motion sickness medications, cold remedies, relief of pain drugs).[17] Each category contained a mix of drugs from the various formulary tiers, so the specific elasticities, by themselves, are not entirely informative. However, we found substantial responsiveness for *all* three types of drugs, suggesting that members decreased their use of drugs that, according to our expert panel, were likely preventing short-term and long-term adverse health events.

Hospital Utilization

As highlighted above, a major concern with increased patient cost-sharing is the so-called "offset effect": by raising the cost of going to the doctor or filling prescriptions, and therefore reducing physician or drug use, increased cost-sharing may delay necessary care and increase hospitalizations. Indeed, the fact that individuals in our sample decreased utilization, even for drugs that were classified as "acute care" by our panel of experts (or "essential" by Tamblyn et al. (2001)), increases this concern. The RAND HIE found no evidence for such offset effects, but

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[17] We also examined a simpler classification based on the work Tambyn et. al (2001). This approach classifies drugs based on whether a drug was "essential" or not. This classification, however, excludes many drugs with uncertain classification (for example, antibiotics and drugs for gastritis). For this reason, we preferred our own classification, but note that the results are similar to those reported in the manuscript. These results are available from the authors upon request.

there has been little subsequent investigation of this question, particularly for the elderly. This policy change provides an excellent laboratory for testing for such offset effects, in response to reduced prescription drug utilization (in 2001) and in response to both reduced office visits and reduced prescription drug utilization (in 2002).

We explore the effect of both policy changes on hospital utilization in Table 4. The difference-in-difference estimates of the impact of the two policy changes on hospital utilization are shown separately in columns 1 and 4. Interestingly, both coefficients are positive, although only that for the 2002 policy change is statistically significant. For the 2002 policy change, we find an increase in the probability of any hospital days during the month of 0.078%, relative to the pre-policy mean of 1.30%, or a rise of 6.0%. As the dynamic results show, both in Table 4 and graphically in Figure 3, these effects do not appear to reflect pre-existing trends. However, the coefficients for the 2001 changes are small and statistically insignificant. The larger response in 2002 may be due to the rise in physician copayments under the HMO plans and the larger reduction in prescription drug use. These results suggest some potential "offset" effect of the changes in coverage for physician and prescription services. Part IV below further explores the nature of this offset effect and its implications for insurance design.

Summary

To summarize our basic results, we have found that there is clear price responsiveness for the elderly to coinsurance for both physician visits and prescription drugs. The implied price elasticities of demand are large, but are difficult to interpret since these were not direct changes in co-insurance rates. Moreover, we also found evidence of an "offset effect" of higher coinsurance in

terms of increased hospitalizations. These offset effects may mitigate any costs savings from lower office visit or drug utilization; the magnitude of such an offset is explored below.

Part IV: The Offset Effect and Optimal Insurance

Perhaps the most striking finding from our analysis thus far is the strong evidence of an offset effect in 2002, with hospital visits rising as physician and prescription drug utilization falls. In this section, we explore this effect further by moving from utilization measures to our (imputed) payment measures. All of the results presented below exclude the quarters before and after the policy change; this exclusion lowers the response of physician visits in 2002, but has no effect on either prescription drug or hospitalization estimates.

Quantifying the Offset

The results of this exercise are presented in the first panel of Table 5, which presents the same difference-in-difference regressions as in the earlier tables, but replacing utilization measures with expenditures. For physician visits, we find a decline in imputed expenditures of $13.16, which is 14.1% of the base cost of $93.25 per person per month. This effect is similar to the 12.6% effect that we found using visit counts. For prescription drugs, we find a reduction in payments of $23.06 for the 2002 policy change, which is 32% of the base spending on drugs. Thus, in total, we find a reduction in physician plus prescription drug payments of $36.22 for the 2002 policy change. At the same time, we find an increase in hospitalization payments of $7.23, which is 5.4% of baseline hospital expenditures. These results suggest that increases in hospital spending offset 20% of the savings from higher copayments on physicians and prescription drugs. While this offset is sizeable,

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it is unlikely to be enough to reverse any conclusions about the efficacy of copayment changes.

Payer-Specific Offsets and the Fiscal Externality of Supplemental Insurance

Importantly, however, this offset does not operate equally for all payers. The next panel of Table 5 divides these responses into payments by Medicare and payments by the CalPERS supplemental insurance plans. Medicare, in this time period, did not cover prescription drugs, so the only effects on the Medicare program are lower physician costs and higher hospital costs.

The results of this division by payer are quite interesting. The physician savings accrue roughly equally to Medicare and to CalPERS, despite the fact that Medicare pays 80% of the charges for physician visits and CalPERS pays only 20%. This is because CalPERS obtains all the benefit from the shifting of payments from insurer to patient, while Medicare obtains only 80% of the savings from lower physician utilization (with CalPERS also getting the rest). Thus, when the HMOs introduce the $10 physician copayment, there is a total reduction in spending on physicians of $13.16, of which Medicare receives roughly 80% (0.8*$13.16 = $10.53). Meanwhile, CalPERS obtains the other 20% of savings from reduced utilization (0.2*13.16 = $2.63), plus the transfer of costs from the supplemental insurance plans to patient ($10 per visit, with an average of 0.86 visits per enrollee per month, equals $8.60), for a total of $11.23.

The savings from reduced prescription drug utilization, of course, accrue totally to CalPERS. These savings are equal to the total reduction in dollars spent on prescription drugs, plus the cost-shift from CalPERS to enrollees through the increased copayments. At the same time, Medicare bears most of the increased cost of hospitalizations. Of the $7.23 in extra hospital costs, Medicare pays $5.58, and CalPERS pays only $1.49. Thus, on net, Medicare saves $10.53 on

physician visits, but pays $5.58 more in hospitalization costs, so that the offset effect for the Medicare program in isolation is 53%. At the same time, the offset for CalPERS is close to zero, since CalPERS pays relatively little of the hospital costs but receives half of the savings in physician visits and all of the savings on prescription drugs. Thus, while the offset is small overall, it is a major consideration for the Medicare program.

Heterogeneity in Offset

While this overall offset effect is fairly modest, there is heterogeneity in the offset that has important implications for insurance design. Table 6 divides our sample in five ways to explore heterogeneity by type of enrollee: by income (terciles of the income distribution, based on zip code median income); by age (65 to 74, 75 to 84, and 85 and older); by intensity of spending in 2000 (terciles of the overall spending distribution in the period before any policy changes); by Charlson index (a measure of patient health described below); and by categories of chronic illness (described further below).

Table 6 has panels for each of these types of heterogeneity, with rows for each category within the type. The first three columns show the results for spending on physicians, prescription drugs, and hospitalizations. The fourth column shows the total offset effect, calculated directly from these first three columns. The fifth and six columns show the offset effect for Medicare and CalPERS, respectively, which are calculated from payer-specific spending regressions. These regressions are not shown, but follow exactly the same pattern as in Table 5: increases in hospital spending accrue about 80% to Medicare and 20% to CalPERS; reductions in prescription drug spending accrue completely to CalPERS (and are higher than reductions in total drug spending due

to the cost shift from enrollees).

The first panel shows results by income tercile. Here we find that there are relatively flat effects across income groups. The effect on hospitalizations is somewhat smaller for the highest income tercile, and the effect on prescription drug utilization somewhat larger, so that the offset declines with income. None of these differences, however, are statistically significant. Thus, there is little heterogeneity by income.

The results by age, however, suggest more heterogeneity. As age increases, we find similar or even smaller effects on physician and prescription drug spending – but much larger effects on hospital spending. As a result, the implied offset is less than 20% for those 65-74, but over 40% for those 85 and older. For the Medicare program in isolation, there is a 70% offset for those 75-84, and an offset well above 100% (i.e. Medicare is losing money from this policy change) for those in the oldest group. Once again, however, the hospital results are relatively imprecise across age groups, so that these differences are not statistically significant.

The results by (ex-ante) spending tercile are even more striking. There is relatively little variation in physician spending effects by tercile, and a rising reduction in prescription drug spending by tercile. Most striking, however, is the hospitalization effect: the effect is negative for the first tercile, small and positive for the second tercile, and very large for the third tercile, implying that hospital spending rose by $23 per enrollee per month as a result of this policy. Overall, there is an offset of almost 50% for the top tercile of spenders: for every dollar society saves in prescription and physician spending, hospitalization costs increase by 45 cents. Unsurprisingly, this implies an enormous offset for the Medicare program in isolation: the Medicare program loses $1.19 in increased hospitalization expenditures for each $1 in physician spending

savings.

These results are echoed in the findings for the Charlson index, which we divide into three categories. The Charlson index is a weighted sum of indicators for the presence of certain conditions, where the weights reflect relative increases in one-year mortality risk for a person; mortality risk increases with the index. To calculate the index, we use diagnosis codes from each individual's claims history to identify the presence of each of 19 conditions, and then apply the weights described in Charlson et al. (1987). We then run regressions separately for individuals with Charlson indices of 0 (i.e. individuals with none of the conditions), 1-3, and 4 or greater. We find a modest increase, in absolute value, in the physician and prescription drug effects by Charlson index, but the differential effect on hospital spending is even more striking. Indeed, the hospital spending effect is enormous for those who are unhealthiest by this measure, with hospital spending increasing by almost $2 for every $1 saved on other spending – and Medicare's hospital spending increasing by more than $6 for every $1 saved on physician spending!

Finally, the last panel in the table explores the findings by chronic illness. Again, we use the diagnosis codes from each individual's claims history to identify individuals who suffer from each of seven chronic illnesses.[18] We then run regressions separately for the sub-samples with and without chronic illnesses. The first two rows compare those with any of the seven chronic illnesses to those with none of the chronic illnesses. In each category the effects are much larger for those with any chronic illness. Yet the most striking difference is in hospitalization effects; there is no effect on hospital payments for those with no chronic illness, while there is an $18.25 per person per month increase in hospitalization payments for those with chronic illness. Overall, for those with chronic illness, there is a roughly 43% offset; for the Medicare program in isolation, the offset

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[18] Our chronic illness definitions follow Goldman et al. (2004).

is over 100%.

The remaining rows of the table consider each of the seven chronic illnesses separately. The results vary across type of illness, with particularly large offsets for those with hypertension, diabetes, arthritis, and affective disorders, and relatively little offset for those with hypercholesterol, asthma, and gastritis. In no category does the overall offset approach one; that is, in every single case, the overall medical system saved money when CalPERS raised copayments. But in many cases, the Medicare program was a significant loser: the most striking case is affective disorders, where most of the savings are on prescription drugs, so that Medicare is losing almost $8 in increased hospitalization spending for each $1 it is saving on physician visits.

Part V: Conclusions and Implications

The rapid rise in both the share of our population that is elderly, and the spending per elderly person, has placed pressure on both public and private insurers to find new ways to control medical costs in this population. A number of approaches have been suggested, but forefront among them is increased consumer cost-consciousness. While Medicare has relatively high copayments and deductibles, these "holes" are filled by supplementary insurance for most elderly. Thus, the elderly typically face very low copayments on most medical utilization.

Our results show that higher copayments for office visits and prescription drugs can have a real effect in terms of reducing medical spending by the elderly. The effects are large in absolute terms, suggesting that moving from a $0 to a $10 copayment led to a 20% reduction in physician visits, and that a roughly $6 copayment rise, on average, reduced prescription drug use by 20%. The implied elasticities are substantially larger than those reported in the RAND HIE for the non-

elderly, often exceeding 1 in absolute value, but they may not be directly comparable due to the very different nature of the price changes in our example.

Most interestingly, we also document the first convincing evidence for an offset effect of higher copayments. While previous work has conjectured that such an offset might be present, the only convincing evidence, from the RAND HIE, rejected the notion of an offset, suggesting instead that physician and hospital care were complements. Overall, we find a rather modest offsetting rise in hospital care when physician and prescription drug copayments are raised. But we find large offsets for the sickest populations with chronic diseases, suggesting that, for chronically ill populations, there is little financial gain to higher copayments.

These findings have several important policy implications. The first is that the fiscal externality associated with supplemental insurance coverage may be more modest than originally presumed. This is because the increase in physician and drug spending arising from supplemental coverage is substantially offset (for the Medicare program) by the fall in hospital costs. This finding also has implications for the design of Medicare's new prescription drug benefit. One of the most controversial features of Medicare's new prescription drug benefit is the so-called "doughnut hole": the gap in prescription drug coverage for beneficiaries with total drug costs that exceed $2,250 per year, until their drug costs exceed $5,100 per year. Our results suggest that this "doughnut hole," by increasing coinsurance rates to 100% for some of the most chronically ill Medicare beneficiaries, could *increase* Medicare's costs. In particular, those beneficiaries who are affected by the "doughnut hole" are likely to be similar to the chronically ill sub-populations where we found that increased Medicare hospital spending exceeded any savings from reduced prescription drug and office visit utilization. Future research should carefully explore the full

system-wide implications of this design feature of the Part D program.

More generally, these findings have important implications for the optimal design of health insurance. The results from RAND clearly implied that optimal health insurance would feature high patient coinsurance up to an income-related out-of-pocket limit. Our findings qualify that result, suggesting further that optimal insurance would be tied to underlying health status, with chronically ill patients facing lower cost-sharing. The cost-sharing increases in our data were sufficiently modest that income-related out-of-pocket limits alone would not accomplish this goal, so specific targeting of copayments may be required.

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Table 1: Means of Key Dependent Variables
By Type of Plan and Year

	PPOs				HMOs			
	Pre-policy	Post-policy			Pre-policy		Post-policy	
	2000	2001	2002	2003	2000	2001	2002	2003
Office Visits Average copayment per visit (in dollars)	-	$0.68	$0.61	$0.59	-	$0.14	$10.11	$9.89
Visits per member per month	-	1.07	1.14	1.19	-	0.75	0.72	0.75
Prescription Drugs Average copayment per drug (in dollars)	$6.93	$13.50	$13.82	$13.29	$1.36	$1.27	$7.63	$7.43
Drugs per member per month	1.98	2.07	2.21	2.44	1.27	1.43	1.34	1.50
Hospitalizations Share of members with any hospital days during the month (x10,000)	156.7	169.8	182.2	206.7	119.5	131.0	149.0	174.3

Table 2: Effects of 2002 HMO Office Visit Copayment Increase
On Office Visit Utilization

Independent variable	Copayment (dollars per drug)	Utilization (Number of office visits per member per month)		
	(1)	(2)	(3)	(4)
HIPAY	$10.06** (0.05)	-.132** (.018)		-.095** (.012)
HIPAY$_{t-4}$			.016 (.018)	
HIPAY$_{t-3}$			.0002 (.016)	
HIPAY$_{t-1}$			.130** (.016)	
HIPAY$_t$			-.036** (.016)	
HIPAY$_{t+1}$			-.094** (.016)	
HIPAY$_{t+2}$			-.071** (.016)	
HIPAY$_{t+3}$			-.082** (.021)	
HIPAY$_{t+4}$			-.101** (.016)	
HIPAY$_{t+5}$			-.113** (.016)	
HIPAY$_{t+6}$			-.029** (.016)	
N	128	128	128	104

** denotes significance at the 5% level.
Each column shows coefficients from a different regression; standard errors are reported in parentheses. The dependent variable is indicated on the column heading; the independent variable is indicated on the row label. Regressions control for plan and month fixed effects. They are estimated by GLS, allowing for plan-specific autocorrelation and plan-specific heteroskedasticity. Regressions include data from February 2001 through September 2003. Column 4 excludes data from the 3 months before and after the policy change, in order to eliminate the effects from any temporary shifts in the timing of care.

Table 3: Effects of Drug Copayment Increases
On Drug Utilization

Independent variable	2001 Policy Change				2002 Policy Change			
	Copayment (dollars per drug)	Utilization (Number of drugs per member per month)			Copayment (dollars per drug)	Utilization (Number of drugs per member per month)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
HIPAY	$7.25** (0.06)	-.111** (.020)		-.048** (.014)	$6.74** (0.09)	-.276** (.016)		-.261** (.021)
$HIPAY_{t-5}$			-.030 (.034)					
$HIPAY_{t-4}$			-.075** (.020)				.005 (.029)	
$HIPAY_{t-3}$			-.023 (.021)				.016 (.017)	
$HIPAY_{t-1}$			.093** (.021)				.039** (.017)	
$HIPAY_t$			-.101** (.020)				-.236** (.016)	
$HIPAY_{t+1}$			-.073** (.020)				-.219** (.016)	
$HIPAY_{t+2}$			-.082** (.020)				-.220** (.016)	
$HIPAY_{t+3}$			-.052 (.034)				-.189** (.016)	
$HIPAY_{t+4}$							-.320** (.016)	
$HIPAY_{t+5}$							-.312** (.016)	
$HIPAY_{t+6}$							-.269** (.016)	
N	80	80	80	58	124	124	124	100

** denotes significance at the 5% level.

Each column shows coefficients from a different regression; standard errors are reported in parentheses. The dependent variable is indicated on the column heading; the independent variable is indicated on the row label. Regressions control for plan and month fixed effects. They are estimated by GLS, allowing for plan-specific autocorrelation and plan-specific heteroskedasticity. Columns 1-4 include data from January 2000 through November 2001, and Columns 5-8 include data from March 2001 through September 2003. Columns 4 and 8 exclude data from the 3 months before and after the relevant policy change, in order to eliminate the effects of any temporary shifts in the timing of care.

Table 4: Effects of Copayment Increases on the Probability That a Member Experiences Any Hospital Days During the Month

	2001 Policy Change			2002 Policy Change		
	(1)	(2)	(3)	(4)	(5)	(6)
HIPAY	3.69		5.31**	7.79**		7.16**
	(2.61)		(2.70)	(2.20)		(2.29)
HIPAY$_{t-5}$		-0.22				
		(6.83)				
HIPAY$_{t-4}$		-8.96			3.79	
		(4.33)			(5.00)	
HIPAY$_{t-3}$		4.51			-4.81	
		(4.43)			(4.48)	
HIPAY$_{t-1}$		-4.67			-4.20	
		(4.43)			(4.48)	
HIPAY$_{t}$		-4.01			-0.66	
		(4.32)			(4.31)	
HIPAY$_{t+1}$		7.10			8.33*	
		(4.32)			(4.32)	
HIPAY$_{t+2}$		3.17			1.09	
		(4.32)			(4.32)	
HIPAY$_{t+3}$		-0.85			14.22**	
		(4.94)			(4.32)	
HIPAY$_{t+4}$					6.21	
					(4.32)	
HIPAY$_{t+5}$					4.72	
					(4.32)	
HIPAY$_{t+6}$					7.00	
					(4.35)	
N	96	96	72	128	128	104

** denotes significance at the 5% level.
* denotes significance at the 10% level.
Each column shows coefficients from a different regression; standard errors are reported in parentheses. The dependent variable is indicated on the column heading; the independent variable is indicated on the row label. Regressions control for plan and month fixed effects. They are estimated by GLS, allowing for plan-specific autocorrelation and plan-specific heteroskedasticity. Columns 1-3 include data from January 2000 through December 2001, and Columns 4-6 include data from February 2001 through September 2003. Columns 3 and 6 exclude data from the 3 months before and after the relevant policy change, in order to eliminate the effects of any temporary shifts in the timing of care. Note that the dependent variable is multiplied by 10,000.

Table 5: Effects of 2002 Copayment Increases on Medical Payments Per Member Per Month
By Source of Payment

	2002 Policy Change			
	(1) Office Visit Payments (dollars)	(2) Drug Payments (dollars)	(3) Hospital Payments (dollars)	(4) Offset (Percent)
All Sources	-13.16** (1.18)	-23.06** (1.85)	7.23** (2.60)	20.0%
Payment Source: Medicare	-10.53** (0.95)	--	5.58** (2.25)	53.0%
Supplemental Insurance	-11.24 (0.26)	-29.20** (1.67)	1.49** (0.38)	3.7%
N	104	100	104	

** denotes significance at the 5% level.
* denotes significance at the 10% level.
Each cell shows the coefficient from a separate difference-in-difference regression; standard errors are reported in parentheses. The dependent variable is indicated on the column heading; the payment source is indicated on the row label. Regressions control for plan and month fixed effects. They are estimated by GLS, allowing for plan-specific autocorrelation and plan-specific heteroskedasticity. All regressions exclude data from the 3 months before and after the relevant policy change, in order to exclude the effects of any temporary shifts in the timing of care. There are 4 fewer observations used for the drug payment regressions, because months that were affected by timing shifts in drug purchases due to other policy changes were excluded. Specifically, the regressions exclude February 2001, due to unusually large, one-time decreases in drug purchases in the PPOs resulting from the February 2001 policy change.

  Column 4 shows the increase in hospital payments as a percentage of the decrease in office visit and drug payments. That is, it is equal to [-Column 3/ (Column 1 + Column 2)].

Table 6: Heterogeneity in Effects of 2002 Copayment Increases
on Payments per Member per Month, by Type of Plan Member

Type of Plan Member	2002 HMO Policy Change			2002 Offset Effects		
	(1) Office Visit Payments (Dollars)	(2) Drug Payments (Dollars)	(3) Hospital Payments (Dollars)	(4) Total Offset (Percent)	(5) Medicare Offset (Percent)	(6) Supplemental Insurance Offset (Percent)
Income Tercile: Low Income	-11.13** (1.59)	-22.10** (1.50)	6.16 (6.39)	18.5%	52.0%	3.2%
Middle Income	-15.07** (1.42)	-23.65** (1.54)	7.14 (8.29)	18.4	41.2	5.3
High Income	-14.03** (1.38)	-29.76** (2.80)	4.41 (5.28)	9.4	33.6	1.1
Member Age: 65-74	-13.67** (1.47)	-23.98** (1.70)	6.81 (4.38)	18.1	48.4	3.3
75-84	-15.54** (1.35)	-23.64** (2.06)	11.07* (6.65)	28.3	70.3	4.8
85+	-9.54** (2.30)	-22.54** (2.86)	13.96 (19.44)	43.5	144.0	6.9
Spending Tercile: Low Spending	-11.08** (1.53)	-11.69** (1.80)	-7.95** (3.00)	-34.9	-83.9	-3.1
Middle Spending	-12.59** (1.61)	-23.40** (1.60)	7.24 (5.17)	20.1	54.6	3.9
High Spending	-15.43** (1.60)	-35.02** (2.21)	22.63** (8.41)	44.9	119.2	6.0
Charlson Index: 0	-7.53** (0.87)	-11.78** (1.00)	4.34** (1.78)	22.5	61.0	2.8
1-3	-11.05** (1.40)	-24.24** (1.33)	12.89** (6.08)	36.5	117.6	4.9
4+	-13.68** (2.39)	-34.68** (2.28)	85.93** (31.19)	177.7	652.4	21.2

| Chronic condition						
Any of the following	-15.19**	-27.15**	18.25**	43.1	122.4	6.4
	(1.35)	(1.76)	(4.22)			
None of the following	-3.27**	-8.19**	-0.78	-6.8	42.7	1.7
	(1.09)	(1.77)	(4.19)			
Hypertension	-13.72**	-27.27**	25.02**	61.0	188.3	8.6
	(1.58)	(1.88)	(7.43)			
Hypercholesterolemia	-15.55**	-28.69**	3.14	7.1	13.7	2.4
	(1.73)	(1.14)	(7.61)			
Asthma	-18.69**	-46.91**	13.90	21.2	68.4	4.3
	(2.20)	(2.04)	(28.18)			
Diabetes	-20.13**	-32.97**	29.93**	56.4	153.4	8.5
	(1.58)	(2.59)	(11.93)			
Arthritis	-16.94**	-27.94**	30.66**	68.3	191.1	8.5
	(1.77)	(2.45)	(5.54)			
Affective disorders	-5.92	-58.20**	44.60**	69.6	764.8	10.0
	(4.14)	(5.37)	(17.60)			
Gastritis	-18.58**	-33.68**	-5.59	-10.7	-33.1	-1.2
	(3.36)	(2.23)	(14.48)			
N	104	100	104			

** denotes significance at the 5% level.
* denotes significance at the 10% level.
Each cell in Columns 1-3 shows the coefficient from a separate difference-in-difference regression; standard errors are reported in parentheses. The dependent variable is indicated on the column heading; the sub-sample is indicated on the row label. Regressions control for plan and month fixed effects. They are estimated by GLS, allowing for plan-specific autocorrelation and plan-specific heteroskedasticity. All regressions exclude data from the 3 months before and after the relevant policy change, in order to eliminate the effects of any temporary shifts in the timing of care. There are 4 fewer observations used for the drug regressions, because we exclude February 2001, due to unusually large, one-time decreases drug purchases in the PPOs resulting from the February 2001 policy change.

Column 4 shows the increase in total hospital payments as a percentage of the decrease in office visit and drug payments. That is, it is equal to [-Column 3/ (Column 1 + Column 2)]. Columns 5 and 6 show the same calculations for payments by Medicare and by supplemental insurance plans. Coefficients underlying the results in Columns 5 and 6 are available from the authors upon request.

**Figure 1: Effect of 2002 HMO Policy Change on Office Visits**
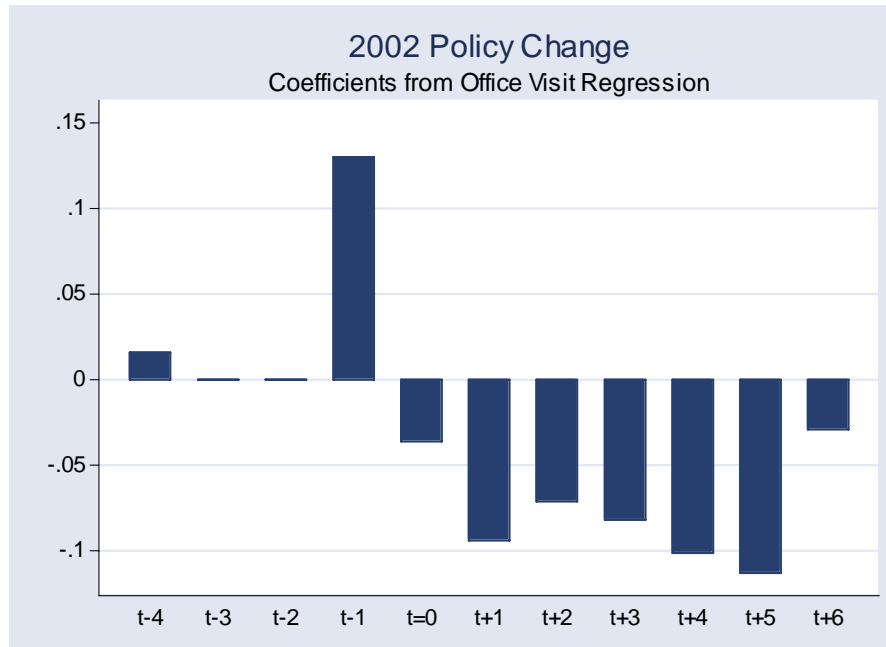
The bars show the point estimates from Column 3 of Table 2.

Figure 2: Effect of Drug Copayment Policy Changes on Drug Utilization

Panel A: 2001 PPO Policy Change

Panel B: 2002 HMO Policy Change
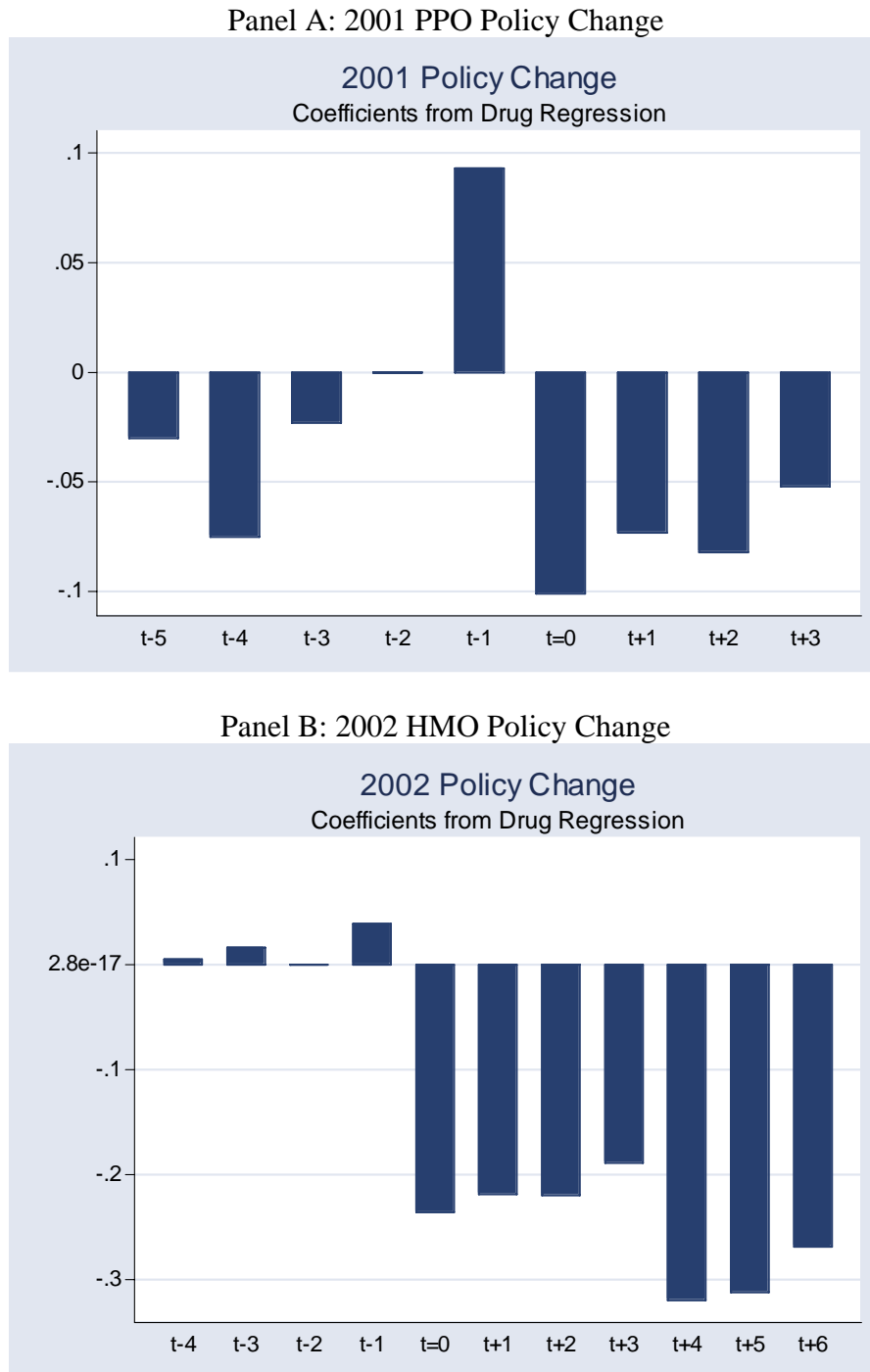
The bars show the point estimates from Columns 3 and 7 of Table 3.

Figure 3: Effect of Policy Changes on Hospitalizations

Panel A: 2001 PPO Policy Change

Panel B: 2002 HMO Policy Change
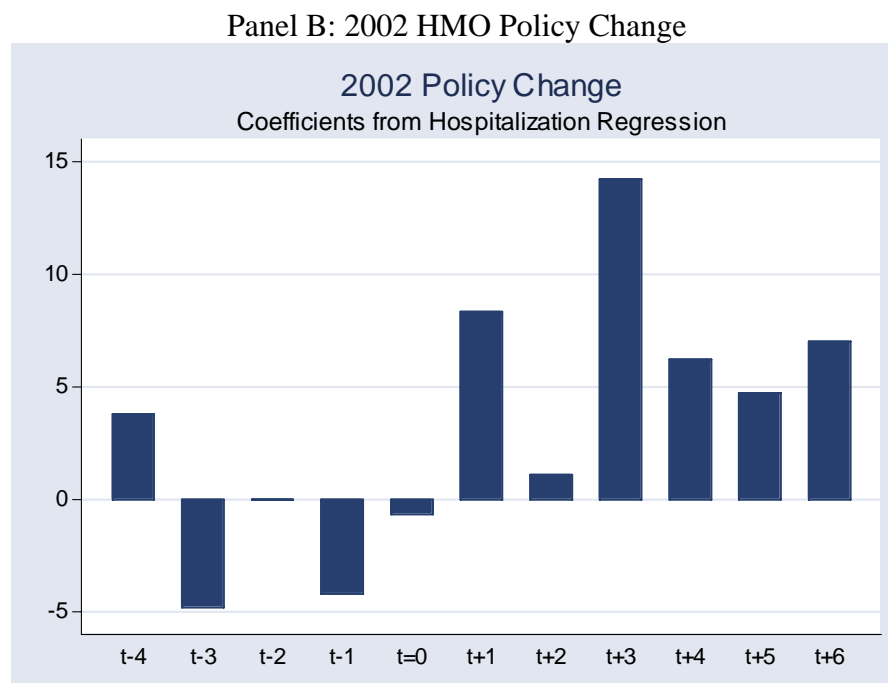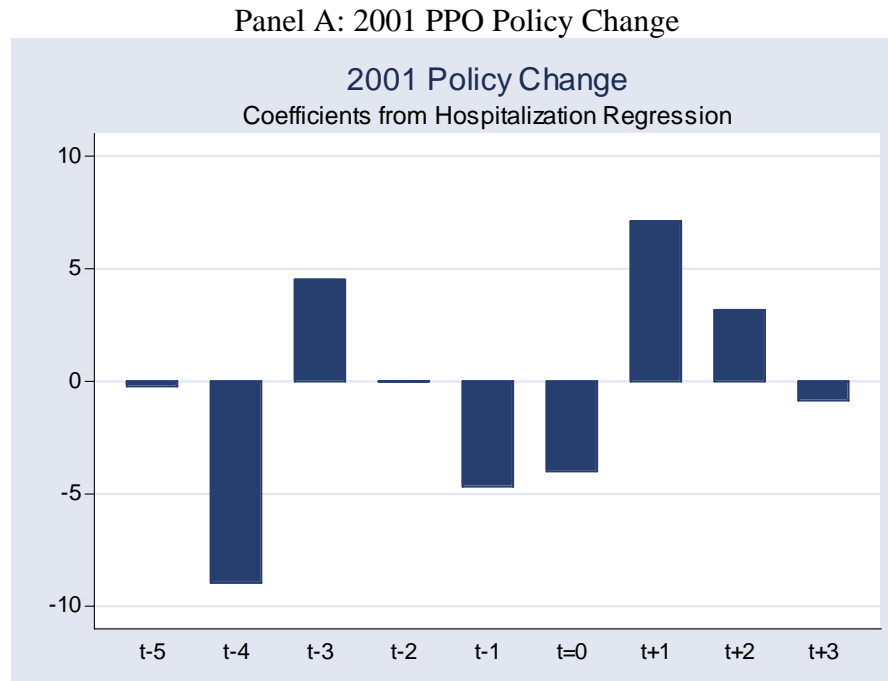
The bars show the point estimates from Column 2 and 5 of Table 4.
Note that all values in this figure and in Table 4 are multiplied by 10,000.