Essays on the Economics of Health Insurance

by

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B.A. Economics, Russian Studies
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Abstract:

This thesis brings together three essays on issues in the economics of health insurance. The first study considers the effects of average per-patient caps on Medicare reimbursement for home health care, which took effect in October 1997. I use regional variation in the restrictiveness of per-patient caps to identify the short-run effects of this reimbursement change on home health agency behavior, beneficiary health care utilization, and health status. The empirical evidence suggests that agencies responded to the caps by shifting the composition of their caseload towards healthier beneficiaries. In addition, I find that decreases in home care utilization were associated with an increase in outpatient care, and had little adverse impact on the health status of beneficiaries.

In the second paper, I examine the impact of Medicare balance billing restrictions on physician behavior and on beneficiary spending. My findings include a significant decline in out-of-pocket expenditures for medical care by elderly households, but no impact on the quantity of care received or in the duration of office visits.

The third paper (written with Jonathan Gruber) explores the causes of the dramatic rise in employee contributions to employer-provided health insurance over the past 20 years. We find that there was a large impact of falling tax rates, rising eligibility for insurance through the Medicaid system and through spouses, and deteriorating economic conditions (in the late 1980s and early 1990s). We also find more modest impacts of increased managed care penetration and rising health care costs. Overall, this set of factors can explain about one-quarter of the rise in employee contributions over the 1982-1996 period.

Thesis Supervisor: Jonathan Gruber

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Chapter 1: Home Care Reimbursement, Long Term Care Utilization, and Health Outcomes

1. Introduction

Long-term care is a policy issue of growing importance in the United States. In 2000, combined home care and nursing home costs for the elderly totaled $98 billion, with Medicare and Medicaid bearing 56% of these costs (U.S. Congress 2000). Moreover, demand for long-term care is expected to increase dramatically over the coming decades. For example, estimates by the Lewin Group suggest that the number of elderly people requiring assistance with activities of daily living will increase by 42% between 2000 and 2020 (U.S. Congress 2000). Many of these elderly people will require long-term care, in the form of nursing home care or home health care.

The 1980s and 1990s saw a notable shift in the utilization of both types of care. Nursing home care decreased substantially between 1985 and 1995, with an 8.2% decline in the share of elderly who reported staying overnight in a nursing facility on a given day (Bishop 1999). Home care utilization, in contrast, increased dramatically over the same period, with an 82% increase in the share of Medicare beneficiaries who used home care and a 208% increase in the number of home care visits per user (U.S. Congress 2000). These facts naturally lead to several important questions: first, did the increased use of home health care during this period lead to the reduction in nursing home use? Second, given the lower costs associated with providing home care, did the increased use of home health care reduce overall expenditures on long-term care? Finally, what was the impact of increased home care utilization on the health status of the elderly?

This paper addresses these three critical questions using evidence from the dramatic reversal of home care utilization growth rates. Specifically, I examine the short-run impact of the sharp decline in home care usage that resulted from a substantial change to Medicare reimbursement for home care in October 1997. The policy change had dramatic aggregate effects, causing an immediate 30% decline in Medicare expenditures for home care. The reimbursement change, which involved the imposition of average per-patient reimbursement caps, also fundamentally changed the incentives faced by home care agencies. In this paper, I also analyze the incentives provided by the new reimbursement policy and provide evidence that agencies changed their procedures in response to the new incentives.

I describe a model of a profit-maximizing home health agency with two types of patients – long-term and short-term patients. Using this model, I show that the imposition of an average per-patient cap on Medicare reimbursement, under reasonable assumptions, could lead the home care agency to intentionally shift the composition of its patients towards short-term patients and to provide a lower intensity of care to its long-term patients. This prediction is consistent with anecdotal evidence that particularly unhealthy Medicare beneficiaries have experienced difficulty with access to home care since 1997.

Next, I turn to data from the 1992-1998 Medicare Current Beneficiary Survey (MCBS) to provide empirical evidence on the composition shift in home care utilization as well as substitution towards other forms of care and impacts on health outcomes. To identify effects of the reimbursement change, I utilize state variation in the restrictiveness of the per-patient caps. In particular, caps were constructed as a weighted average of the historical costs per home care user in each state and the mean historical costs per home care user in each state's Census division. As a consequence of this formula, states with otherwise similar utilizations patterns faced differential incentives to cut back on per-patient costs after the Medicare reimbursement policy change. For example, Tennessee and Utah provided the same average amount of care to their users in 1996, but Utah faced more stringent per-patient caps than Tennessee, due to the regional component of the per-patient cap calculation.

Using this identification strategy, I find significant declines in the utilization of home health care in the post-policy period, which are consistent with the aggregate declines and confirm the validity of my empirical strategy. The decline in usage among relatively healthy beneficiaries is insignificantly different from zero, while the decline among relatively unhealthy beneficiaries is significant and negative, suggesting that the declines were driven primarily by relatively unhealthy beneficiaries.

I also examine the impact of the policy change on utilization of other forms of care, providing evidence on the substitutability of home care for nursing home and other types of care. I find evidence of a significant offsetting increase in outpatient expenditures (and an insignificant increase in outpatient events), which is concentrated among relatively healthy patients. This finding is consistent with shifting the location of physical therapy or other short-term care from the home to an outpatient setting. I find no evidence of offsetting increases in nursing home or inpatient care; if anything, the results suggest the possibility of a *decline* in institutional care associated with declines in home care utilization. This result is consistent with home care agencies providing referrals or otherwise enabling access to nursing home care.

Finally, I turn to the question of how declines in home care utilization affected health outcomes. I use various measures of health, including mortality, self-reported health, body mass index and reported difficulty performing activities of daily living to test for effects on health outcomes. However, the only results suggesting an adverse impact of the decline in home care utilization on Medicare beneficiaries is a significant increase in obesity. These variables are, of course, coarse measures of overall health, but the results are suggestive that the declines in home care utilization did not have a substantial impact on health. However, because my findings characterize the immediate impact of a decline in home care utilization, they may not be representative of the long-term impact on beneficiary health.

The paper proceeds as follows. I provide background information on the change in home care utilization in Section 2 and describe related previous research in Section 3. In Section 4, I present a model of home health agency behavior. Section 5 describes my data source, the Medicare Current Beneficiary Survey, and explains my empirical strategy. In Section 6, I present results regarding home care utilization and, in Section 7, I present results for other forms of care and health outcomes. Section 8 discusses specification checks and Section 9 concludes.

2. Background

The early 1990s witnessed unprecedented growth in Medicare expenditures for home health care, with expenditures skyrocketing from $2.5 billion in 1989 to $18.1 billion in 1996. This growth was precipitated by a liberalization of the Medicare home care benefit rules, following the settlement of a class action lawsuit in 1988. The rapid rise in expenditures was driven by substantial increases in both the percentage of Medicare beneficiaries who were using home care and in the number of visits provided to each home care user. Between 1989 and 1996, the percentage of Medicare beneficiaries who used home care almost doubled—from 5.1% to 9.5%—and the number of visits per user almost tripled—from 27 to 79 (U.S. Congress 2000).

In response to rapidly rising expenditures and the concern that agencies had no incentive to provide care efficiently, Congress mandated the development of a prospective payment system (PPS) for home care in the Balanced Budget Act of 1997 (BBA 97). In order to slow the growth of home health expenditures in the interim before the PPS was ready to be implemented, BBA 97 also created an "Interim Payment System" (IPS) that took effect in October 1997.

Prior to BBA 97, home care agencies were reimbursed for their reasonable costs, subject to a per-visit cap. The per-visit cap, which was equal to 112% of the national average cost for each type of visit, was applied to aggregate agency payments. As the *2000 Green Book* explains, "an aggregate cost limit was set for each agency equal to the sum of the agency's limit for each type of service multiplied by the number of visits of each type provided by the agency." This reimbursement policy was criticized for providing no incentives for agencies to provide home care visits efficiently, because agencies were reimbursed for every marginal visit to a patient. Furthermore, there were no financial constraints on demand, because beneficiaries face no copayments for home care.

Beginning in October 1997, the IPS added an additional limit to the calculation of agency reimbursements: a per-patient cap. This new, agency-specific cap was calculated as a weighted average of each agency's 1994 average per-patient costs and the 1994 regional average per-patient costs. The agency's own average per-patient costs comprised 75% of the cap, with the regional average per-patient cost comprising the remaining 25%. Thus, those agencies that had above-average per-patient costs within their region in 1994 received per-patient caps that were *lower* than their 1994 per-patient costs; agencies that had below-average per-patient costs in 1994 received caps that were *higher* than their 1994 per-patient costs. In a regulatory impact statement in March 1998, HCFA projected that 58% of agencies would exceed the per-patient cap (*Federal Register* 1998).

Following the introduction of the IPS, home care utilization declined substantially, with decreases in both the share of beneficiaries who used home care and in the number of visits per user. In January 1998, CBO had projected that BBA 97 would lead to a slowing of the growth rate of Medicare's home care expenditures (CBO 1998). However, as Figure 1 shows, home health expenditures actually plummeted by almost $6 billion in 1998. Figures 2 and 3 show that both the dramatic pre-1997 increase in expenditures and the striking post-1997 decline in expenditures were driven primarily by changes in the number of visits per Medicare beneficiary, rather than changes in costs per visit. Data from GAO (2000) indicate that the striking decline in visits per Medicare beneficiary reflected significant decreases in both the share of beneficiaries who used home care and in the number of visits per user. As Senator Susan Collins of Maine told the *New York Times*, "The Medicare home health cutbacks have been far deeper and more wide-reaching than Congress ever intended" (Pear 2000).

Various reports and anecdotes have suggested that agencies responded to the IPS per-patient caps by cutting back on their care to the sickest patients. Discharge planners and advocates for the aged told the GAO that "patients with intensive skilled nursing needs and patients needing a significant number of visits over a long period of time (rather than patients, for example, with short-term rehabilitation needs) were the most difficult to place in home health services" after BBA 97 (GAO 1998).

Furthermore, some observers have complained that declines in home care have led to increases in the utilization of other forms of health care. For instance, a hospital administrator told the *New York Times*, "Our hospital has been busier since the cutbacks in home health care. We attribute quite a bit of that to the fact that we can't provide adequate home care. Patients are admitted or readmitted to the hospital or to a nursing home, and both of those are more expensive than home care" (Pear 2000). Indeed, it is plausible that Medicare could lose money by cutting back on home care reimbursement if, for example, the policy change led patients to substitute relatively expensive inpatient care for less expensive home care. Alternatively, Medicaid or individual patients could bear financial costs if patients moved from home care to nursing homes as a consequence of BBA 97. Understanding the impact on other forms of care is, therefore, critical for evaluating the consequences of the IPS. If the decrease in Medicare spending on home care was offset by increases in spending on other forms of care, then the "savings" from the IPS could be illusory. This paper responds to the anecdotal reports by examining the empirical evidence on substitution between home care and other forms of care.

3. Previous work

This paper contributes to a literature on the trade-off between efficiency in theproduction of medical care and selection of patients in prospective and retrospective payment systems. Newhouse (1996) provides theoretical background and an overview of this literature. As he explains, providing a lump-sum payment provides an incentive for health care providers to provide care in the most cost-effective manner and, in that sense, provides an incentive for efficiency in production of medical care. On the other hand, providing a lump-sum payment transfers risk to the health care provider and therefore gives him an incentive to select the healthiest patients for treatment. Newhouse reviews empirical evidence on this issue and argues that full prospectivity is unlikely to be optimal, due to the welfare loss from increased patient selection. These arguments are also applicable to the case of the IPS and suggest that, while the reimbursement change provided an incentive for agencies to provide care efficiently, it also provided an incentive for them to select the healthiest patients for care.

Much of the existing empirical evidence on prospective payment systems comes from the literature on Medicare's transition to prospective payment for hospital care. Chalkley and Malcomson (2000) provide a review of this literature. The findings include clear evidence of declines in utilizations and mixed evidence of effects on health outcomes, such as readmission rates and mortality, in response to prospective payment. These findings suggest that the IPS could be expected to cause a decline in utilization; other conclusions of the PPS literature – especially impacts on health outcomes – are suggestive, but are not as easily extrapolated to the case of home care.

Empirical evidence about the response of HHAs to a transition from fee-for-service to prospective reimbursement is available from an experiment in the early 1990s. Cheh (2001) evaluates the impact of the experimental prospective payment system on patient selection, health care utilization and health outcomes. She finds strong evidence of declines in home care utilization among the prospectively paid treatment group, but little evidence that agencies in the treatment group made an effort to select healthier patients for care. Cheh also reports little evidence of adverse health effects or increased utilization of other forms of health care. However, there are several reasons that these conclusions may not generalize to the situation of BBA 97. First, several features of the experimental reimbursement system—notably, the use of adjustments for agency case-mix— were not used in the IPS. Second, and more significantly, agencies participated voluntarily in the experiment and were insured against 97-99% of any losses that were generated as a result of the experimental reimbursement system. In contrast, the IPS was mandatory and did not offer any insurance against agency losses. As a result, agencies may have reacted more strongly to the IPS than they did to the experimental PPS. Cheh's study provides interesting experimental evidence; this paper provides complementary evidence from a nationwide policy change.

The more general issue of substitution between home care and other forms of care has long been a question of interest to health economists. In the early 1980s, the well-known

National Long Term Care Demonstration project was implemented, providing case management and additional community services to a treatment group. Kemper (1988) summarizes the conclusions of the evaluation. He reports that, despite the fact that clients and informal care-givers in the treatment group were more satisfied with care arrangements and quality of life, the additional services led to higher net costs. These higher costs were caused by the fact that the costs of additional services were not offset by decreases in the costs of other forms of care utilization, notably nursing home care. The findings of other, smaller demonstrations have suggested that there may be some opportunities for home care to substitute for other care, especially if increases in home care usage are well-targeted.[1] However, in the twenty years since these experiments were completed, many aspects of the health care system have changed in ways that are likely to have impacted home care utilization patterns. For example, the implementation of the Medicare prospective payment system for hospital inpatient reimbursement in 1983 may have led to earlier hospital discharges and more home care utilization. This paper provides updated evidence that is more relevant in the current health care environment.

4. Theoretical Framework

The intention behind the IPS legislation was to provide an incentive for agencies to provide care efficiently. Lawmakers intended that "payments on behalf of patients whose costs were lower than average would 'subsidize' more costly patients; the balance of low and high cost patients would determine whether an agency would exceed its aggregate per beneficiary cap" (U.S. Congress 2000). However, a simple model, evaluated under reasonable assumptions, suggests that agencies had an incentive to respond to the IPS by favoring patients who appeared likely to incur low costs. The following model illustrates the incentives of the pre-policy period and how these incentives changed when the IPS was implemented.

Pre-policy

Suppose there are two types of patients, those with short-term needs, type S, and those with long-term needs, type L. An agency chooses a level of care intensity for short-term patients, I_S, and a level of care intensity for long-term patients, I_L, to maximize its profits. It is

[1] Hughes, Susan, Larry Manheim, Perry Edelman, and Kendon Conrad (1987). Kemper (1988) cites additional evidence from Blackman, D. et al (1985). *South Carolina Community Long Term Care Project: Report of Findings*. Spartanburg: South Carolina State Health and Human Services Commission.

convenient to think of I_i as the number of home health care visits provided to a patient of type i. The agency receives a fixed reimbursement rate, P, for each unit of I_i provided to either type of patient. The cost of providing a unit of care intensity, C_i, varies with patient type and with the level of intensity. An increase in I_i also leads to an increase in the number of patients, N_i, who choose to obtain services from the agency.[2] The agency's profits are equal to the number of patients of each type, N_i, multiplied by the per-patient profit for each type of patient:

(1) $$\pi = N_S(I_S)\cdot(P\cdot I_S - C_S(I_S)\cdot I_S) + N_L(I_L)\cdot(P\cdot I_L - C_L(I_L)\cdot I_L)$$

The marginal cost of an additional unit of intensity is assumed to increase with intensity, reflecting the costs of hiring new workers, the psychic costs of providing more care than medically necessary, or the increased risk of fraud and abuse allegations from Medicare. I assume that C_S increases more rapidly than C_L, yielding the intuitive implication that short-term patients always receive less intensive care – or fewer visits – than long-term patients. P is fixed at Medicare's per-visit cap for each visit and does not depend on I_S or I_L.

The first-order conditions for profit maximization are therefore:

(2) $$\frac{d\pi}{dI_S} = \frac{dN_S}{dI_S}\cdot(PI_S - C_S I_S) + N_S\cdot\left(P - \frac{dC_S}{dI_S}I_S - C_S\right) = 0$$

and:

(3) $$\frac{d\pi}{dI_L} = \frac{dN_L}{dI_L}\cdot(PI_L - C_L I_L) + N_L\cdot\left(P - \frac{dC_L}{dI_L}I_L - C_L\right) = 0$$

The first term in each first-order condition represents the benefit of increased intensity to the agency due to increased demand for their services. These benefits must be balanced against the second term, which reflects decreased per-patient profit as a result of increasing marginal costs. In this setting, the intensity of care provided to each type is chosen independently of the intensity of care provided to the other type.

Post-policy

The IPS can be incorporated into this model by adding an aggregate per-patient cap, $\overline{X}$, to the calculations, so that:

[2] This feature has been used in previous work by Hodgkin and McGuire (1994). It is consistent with suggestions from HCFA officials that agencies competed by providing additional visits, since the lack of copayments left no scope for price competition (GAO 1996). It is also consistent with the standard assumption of monopolistic competition in models of physician behavior.

$$(4) \qquad \left(\frac{N_S}{N_S+N_L}\right)\cdot P\cdot I_S+\left(\frac{N_L}{N_S+N_L}\right)\cdot P\cdot I_L\le \overline{X}$$

That is, the weighted sum of per-patient reimbursement for patients of type L and type S must be less than or equal to $\overline{X}$. Since agencies for whom the per-patient cap is not a binding constraint continue to behave as they did in the pre-policy period, I assume that this condition holds with equality for illustrative purposes. Under the assumption that the per-patient cap is a binding constraint, P becomes a function of I_S, I_L, N_S and N_L:

$$(5) \qquad P=\frac{\overline{X}}{\left(\frac{N_S}{N_S+N_L}\right)I_S+\left(\frac{N_L}{N_S+N_L}\right)I_L}$$

Taking the derivative of P with respect to I_L and assuming that I_S is less than I_L, I find that P is decreasing in I_L:

$$(5) \qquad \frac{dP}{dI_L}=\overline{X}\cdot\left[\frac{N_S\cdot\frac{dN_L}{dI_L}\cdot(I_S-I_L)-N_L\cdot(N_S+N_L)}{(N_S\cdot I_S+N_L\cdot I_L)^2}\right]<0$$

Taking the derivative of P with respect to I_S and again assuming that I_S is less than I_L, I find that the relationship between P and I_S is ambiguous. If I_L is substantially larger than I_S, N_S is very responsive to I_S, or N_S is relatively small, then P increases with I_S; otherwise, P decreases with I_s.

$$(6) \qquad \frac{dP}{dI_S}=\overline{X}\cdot\left[\frac{N_L\cdot\frac{dN_S}{dI_S}\cdot(I_L-I_S)-N_S\cdot(N_S+N_L)}{(N_S\cdot I_S+N_L\cdot I_L)^2}\right]\overset{?}{>}0$$

I shall assume that $(I_L\text{-}I_S)$ is always sufficiently large that the derivative of P with respect to I_S is greater than zero.[3]

The agency's new maximization problem under the IPS is the same as in the pre-policy problem, except that the price received by Medicare has become a negative function of I_L and a positive function of I_S:

$$(7) \qquad \pi = N_S(I_S)\cdot\left(P(I_S,I_L)\cdot I_S - C_S(I_S)\cdot I_S\right) + N_L(I_L)\cdot\left(P(I_S,I_L)\cdot I_L - C_L(I_L)\cdot I_L\right)$$

The new first-order conditions for profit maximization are therefore:

$$(8) \qquad \frac{d\pi}{dI_S} = \frac{dN_S}{dI_S}\cdot\left(PI_S - C_S I_S\right) + N_S\cdot\left(\frac{dP}{dI_S} + P - \frac{dC_S}{dI_S}I_S - C_S\right) + N_L\cdot\frac{dP}{dI_S}\cdot I_L = 0$$

and:

$$(9) \qquad \frac{d\pi}{dI_L} = \frac{dN_L}{dI_L}\cdot\left(PI_L - C_L I_L\right) + N_L\cdot\left(\frac{dP}{dI_L}P - \frac{dC_L}{dI_L}I_L - C_L\right) + N_S\cdot\frac{dP}{dI_L}\cdot I_S = 0$$

These first-order conditions are different from the pre-policy first-order conditions for two reasons. First, the second term has been modified to account for the fact that marginal reimbursement per patient of a given type decreases with intensity provided to that type. Under the assumption that P increases with I_S and decreases with I_L (and holding all else equal), this adjustment leads to a higher equilibrium level of I_S and a lower equilibrium level of I_L. Second, there is a new third term, $\left(N_{-i}\cdot\frac{dP}{dI_i}\cdot I_{-i}\right)$, which represents the externality imposed on the profitability of patients of type $-i$ when I_i increases. That is, under the assumption that P increases with I_S and decreases with I_L, the third term accounts for the fact that I_S imposes a positive financial externality on the per-patient profitability of type L patients, whereas I_L imposes a negative financial externality on the per-patient profitability of type S patients. Because I_S exerts a positive externality on the profitability of all type L patients, this third term

[3] This is an important assumption. However, it appears consistent with the pattern of utilization among those beneficiaries who report any home care spending in the MCBS; 25% of the observations spend \$638 or less whereas 25% spend \$5211 or more.

has the effect of making increases in I_S more attractive to agencies. Likewise, increases in I_L become less attractive, due to the negative externality on the profitability of all type S patients.

The insight provided by this model is that, under reasonable assumptions, agencies may attempt to increase the share of short-term patients and may provide higher intensity of care to those patients in order to attract more of them. Agencies are also likely to decrease the share of long-term patients and to decrease the intensity of care provided to them. Moreover, because the per-patient cap was designed to be substantially lower than the average per-patient price received during the immediate pre-policy years, it is likely that the overall number of patients would decrease in response to the policy.

Several authors have pointed out that some agencies were confused about the per-beneficiary limits, either not knowing what their limits were or not understanding that the limits applied to aggregate reimbursement. Indeed, the final rules for calculating per-beneficiary caps were not published in the *Federal Register* until March 1998. Since this cap was effective for agencies with fiscal years that began as early as October 1997, the timing of the publication meant that some agencies faced uncertainty about their caps for the first six months of the fiscal year. In addition, there are reports that some agencies interpreted the limits as actual caps on how much they could spend on each user, rather than caps on average per-patient reimbursement (MedPAC 1999, U.S. Congress 2000). The confusion that surrounded the implementation of this policy suggests that agencies could have responded somewhat differently than predicted. If agencies interpreted the limits as actual caps on how much they could spend on each user, there would be a substantial cutback on long spells of home care. If agencies did not know what their per-beneficiary limits would be, they might either over-react or under-react to the caps, depending on whether they were too pessimistic or too optimistic in their expectations.

5. Data and Empirical Strategy

I use data from the Medicare Current Beneficiary Survey (MCBS) to consider the questions that I have posed about agency responses to the IPS and about resulting changes in beneficiary utilization of care and health status. The MCBS surveys a rotating panel of Medicare beneficiaries, with an over-sampling of older beneficiaries. An important feature of the MCBS is its inclusion of all beneficiaries, regardless of whether they live in a nursing home; this feature makes it possible to analyze the impact of the IPS on nursing home utilization. Another advantage of the MCBS is that it combines administrative data from Medicare claims with survey data from several interviews with beneficiaries (or proxies, if the beneficiary is unable to participate in an interview) over the course of a year. The resulting data set provides detailed information on utilization and costs of medical care, in addition to information on demographics and health status. Utilization and costs are categorized based on the type of care and the setting; categories include facility care, institutional, inpatient, outpatient, medical provider, home health, hospice, and prescription drugs. Facility care and institutional care may both include nursing home care; they are distinguished by the expected length of the care. Facility events are intended to represent long-term care, whereas institutional events represent care that is expected to be short-term or has concluded.

I use the annual number of events and the total annual expenditures for each type of care as key dependent variables in my analysis. Home health "events" are defined as home health visits. For institutional and inpatient care, "events" refer to admissions. Facility "events" are stays and outpatient "events" are outpatient visits. Expenditures are all inflated to real 1999 dollars.

The MCBS was conducted annually beginning in 1992. I use data through 1998; the 1999 data will be added to my analysis when it becomes available. The limited post-policy period is a limitation of the current analysis and precludes any conclusions about long-term impacts of the decline in home care utilization. My complete 1992-1998 data set includes observations for 85,359 Medicare beneficiaries, including 13,022 observations that report some home health utilization during the year. Of the home health users, 1,896 were in the post-policy period.[4]

Summary statistics are provided in Table 1. The first column shows statistics for the full sample. These statistics show that 15% of the observations in the MCBS use home health care and about 10% use facility care in any given year. The second column provides summary statistics for observations that are predicted—based on their characteristics and on pre-policy utilization patterns—to have higher home care costs than the median beneficiary. Not surprisingly, this group has higher utilization levels of all forms of medical care than the corresponding predicted low-cost beneficiaries, reflecting the relatively poor health of observations in the predicted high-cost group. In the empirical analysis, I test for differential impacts of the IPS on these two groups of patients.

[4] 4,977 observations were excluded from the main analysis because of missing values.

This paper uses variation in the restrictiveness of IPS per-patient caps to identify the effect of the caps on agency behavior and beneficiary utilization of care. Variation in restrictiveness comes from the fact that the per-patient cap was based on both the agency's historical costs and the region's average historical costs. Thus, agencies that had above-average costs within their region were penalized by the regional component of the cap and faced more restrictive caps. On the other hand, agencies that had below-average costs within their region benefited from the regional component of the cap and faced less restrictive caps. I therefore rely on the geographic variation in the restrictiveness of the IPS, using the fact that agencies that are located in states that had higher average per-patient costs in 1994 than other states in their region were more strongly impacted than other states in their region.

For my empirical analysis, I create a measure of restrictiveness based on the 1994 state average visits per user, as reported in GAO (2000). From each state's average number of visits per user in 1994, I subtract the average number of visits per user in that state's Census division, the relevant region for calculating per-patient caps. The resulting measure of restrictiveness ranges from -40 to +34 visits. In the analysis below, I use this measure, interacted with a dummy variable indicating the post-policy period, to test for the effects of the new reimbursement policy on agency behavior and on beneficiary utilization of care and health outcomes. I define 1998 as the post-policy period; the last 3 months of 1997 are technically part of the post-policy period but, because the data is annual, these months are included with pre-policy data in my analysis.

To graphically illustrate the basis for my identification strategy, I have classified states into "high", "medium" and "low" restrictiveness states, based on my continuous measure of restrictiveness. "High", "medium" and "low" are therefore defined relative to a state's region. Figures 4 and 5 shows that states with relatively highly restrictive caps had trends that were similar to states with relatively unrestrictive caps, but experienced substantially larger post-policy declines in utilization. The difference in the post-policy declines is particularly striking relative to the pre-policy trends in the different types of states, especially in the case of users per-beneficiary. States with relatively restrictive caps had larger declines in users and visits per user in the post-policy period, with a 28% decline in users per beneficiary and a 47% decline in visits per user. In contrast, states with relatively unrestrictive caps had a 19% decline in users and a 36% decline in visits per user.

The basic estimating equation takes the following form:

$$Y_{ist} = \alpha + \gamma_1 \mathrm{Re}\,strict_s * Post_t + X_{ist}\beta + \sum_s \gamma_s State_s + \sum_t \gamma_t Year_t + \sum_s \delta_s State_s * trend_t + \varepsilon_{ist} \tag{10}$$

The coefficient of interest, γ_1, is on the interaction between the state-level measure of restrictiveness and the post-policy dummy variable; this coefficient is shown in the first row of each column. This coefficient measures the impact of living—during the post-policy period—in a state that provided an additional one visit per user above the regional average during the pre-policy period. I control separately for state and year fixed effects, state trends, and individual characteristics and diagnoses[5]; coefficients for some of these control variables are shown in the lower rows of each table.

I generally estimate my equations using OLS. When my primary dependent variables are measures of utilization and spending, however, my coefficients combine effects on the extensive and intensive margins. Therefore, I look separately at the probability of a value greater than zero for these measures. I do not, however, show results that are conditional on having a value greater than zero, because these results have no causal interpretation when the participation margin is affected. For regressions with binary dependent variables, I report marginal effects from Probit models; results using logit and linear probability models are similar, but not reported.

The critical identifying assumption of this empirical strategy is that there are no differential trend in states that faced relatively high restrictiveness due to the IPS. For example, if there were mean reversion in home care utilization, states with high-pre-policy utilization would have decreases in utilization in the post-policy period, even in the absence of any policy change. Because states that faced relatively high restrictiveness also had relatively high pre-policy utilization levels, there is a possibility that my measure of restrictiveness simply captures the mean reversion of high utilization states. I address this concern by using the fact that my measure of restrictiveness depends on a state's pre-policy utilization relative to other states in its region, not relative to the rest of the country. States which have similar pre-policy utilization may face the same degree of mean reversion, but would face different IPS restrictiveness depending on whether their utilization is higher or lower than other states in the division. For

[5] Twelve diagnosis dummy variables indicate whether an individual has ever received a diagnosis of Alzheimer's disease, cancer, diabetes, emphysema, hypertension, mental retardation, mental disorders, osteoporosis, paralysis, Parkinson's disease, stroke, or amputation of an arm or leg.

instance, in 1994, home health users in Georgia received an average of 103 visits and users in Oklahoma received a comparable 105. The IPS was substantially more restrictive for Georgia, because home health users in Georgia received 33.80 more visits on average in 1994 than the average user in the region; users in Oklahoma, in contrast, received only 2.80 more visits than the average user in the region. So, although Georgia and Oklahoma should have faced a similar degree of mean reversion in the post-policy period, Georgia should have faced more pressure to decrease utilization due to the formula for calculating per-patient caps. Likewise, states that have very similar measures of restrictiveness in my data have very different utilization levels in 1994. The restrictiveness measures for Kansas and Mississippi are 8.64 and 8.45, respectively, but users in Kansas in 1994 received an average of 56 visits, whereas users in Mississippi received an average of 113 visits. Due to the formula for calculating per-patient caps under the IPS, the financial incentive to decrease utilization was similar in these two states, despite the fact that pre-policy utilization in Mississippi was over twice as high as utilization in Kansas.

In order to formally account for the possibility of mean reversion in my empirical analysis, I run my regressions both with and without a "mean reversion" term. The mean reversion term is an interaction between the 1994 average visits per user in each state and a dummy variable for the post-policy period. This additional term accounts for the fact that states with high utilization in the pre-policy period may have decreased their utilization even in the absence of the IPS. My restrictiveness measure, then, captures variation in IPS restrictiveness that does not depend on the pre-policy level of utilization, but rather on the pre-policy level of utilization *relative* to other states in the region. As discussed below, the majority of my results are not sensitive to the inclusion of this mean reversion term.

The estimating equation is identical to Equation 10, except that it includes an additional term, which is an interaction between 1994 state average visits per home care user and a post-policy dummy variable. The coefficient on this interaction term captures the extent to which states with relatively high utilization before BBA 97 decreased their average usage. The remaining variation that is exploited in my identification strategy depends only on the pre-policy level of utilization *relative* to utilization in other states in the same region. This specification takes the following form:

$$Y_{ist} = \alpha + \gamma_1 Restrict_s * Post_t + \gamma_2 Visits94 * Post_t + X_{ist}\beta + \sum_s \gamma_s State_s \tag{11}$$
$$+ \sum_t \gamma_t Year_t + \sum_s \delta_s State_s * trend_t + \varepsilon_{ist}$$

The next section presents basic results for home care utilization. In Section 8, I address potential concerns with this framework, such as the endogeneity of pre-policy differences and the effects of concurrent policy changes.

6. Effect on Home Health Care Utilization

The results for home care utilization in Table 2 confirm the evidence from aggregate data: agencies in states that had above-regional-average visits per user in 1994 had larger declines in visits per user and in users per Medicare beneficiary in the post-policy period. In particular, for every one visit difference from the regional average in 1994, home care utilization fell by an additional 0.1 to 0.2 visits per-beneficiary in the post-policy period. The first column shows results for a regression that only controls for state and year fixed effects. The second column shows that the results are robust to inclusion of individual-level covariates. The third column, which corresponds to the basic estimating equation specified in Equation 10, adds controls for state-level trends. The magnitude of the coefficient in the third column is larger than in the first two columns, suggesting that states with highly restrictive IPS caps tended to be states where utilization was trending upward relatively rapidly. Finally, the fourth column, which corresponds to Equation 11, adds a control for mean reversion and suggests that almost half of the impact in column 3 is attributable to mean reversion.

In Table 3, I replicate the regressions in Table 2, allowing for the possibility that there are differential effects for patients with relatively high predicted home care costs, by interacting $Restrict_s * Post_t$ with a dummy variable for "high" predicted costs.[6] However, because this specification requires covariates to control for the main effect of "high" predicted costs, I do not replicate column 1 of Table 2. Predicted costs were imputed based on each observation's characteristics and the coefficients from a regression of total home care costs on individual characteristics among those observations that had home health visits in the pre-policy period.

[6] I also add controls for having high predicted costs and for interactions between high predicted costs and both $post_t$ and $restrict_s$.

The characteristics that were used to predict home care costs include age, gender, marital status, race, difficulties with walking, writing, lifting and stooping, as well as the 12 diagnosis control variables. Thus, the predicted costs are a measure of the costs that each person was likely to have incurred if they had used home health care during the pre-policy period; those with relatively high predicted costs are the less healthy patients, who are more likely to incur high home care costs. I define "high cost" patients as those patients whose predicted costs are above $2267, the median prediction for all Medicare beneficiaries in the sample.

The results in Table 3 reveal that patients with higher predicted costs had significantly larger declines in home care utilization and expenditures in the post-policy period as well as significantly larger declines in the probability of receiving any care. These larger declines could reflect a mechanical effect, arising from the fact that patients with higher predicted costs use more home care than those with low predicted costs and therefore had a greater opportunity to decrease the number of visits. The larger declines could also reflect a behavioral effect, arising from agencies' efforts to cut back on care to patients who were most likely to exceed per-patient caps. The fact that the negative effects of the IPS on all of the home care utilization measures are driven primarily by significant negative effects among the predicted high cost beneficiaries is very suggestive of a behavioral impact.

Distinguishing between the mechanical and behavioral effects is not a straightforward task, because it is not clear what would constitute "equivalent" declines in utilization among the relatively unhealthy and healthy groups. One possibility is that the mechanical effect would lead to an equal percentage decline in utilization among the two groups. The basic results in column 2, which include covariates and state trends, suggest that relatively unhealthy beneficiaries faced declines that were roughly four times the magnitude of declines faced by relatively healthy beneficiaries. However, the summary statistics in Table 1 indicate that the relatively unhealthy beneficiaries typically use approximately seven times as many home health visits as the relatively health beneficiaries. These results, then, suggest a higher *percentage* decline among relatively healthy beneficiaries, providing no evidence of selection effects. On the other hand, the results in column 3, which account for mean reversion, show an insignificant *increase* in utilization among relatively healthy beneficiaries which, under any reasonable assumption about the magnitude of the mechanical effect for healthy beneficiaries, would imply a behavioral effect.

In Tables 4 and 5, I show basic results – with and without mean reversion controls – for additional measures of home health utilization. The first two columns show results for the extensive margin, the probability of having any home care at all. These results have a more straightforward interpretation than those in Tables 2 and 3, because they do not confound effects on the extensive and intensive margins. They provide additional evidence that agencies responded to the IPS by differentially cutting back on care to relatively unhealthy beneficiaries; relatively healthy beneficiaries face an insignificant increase in the likelihood of receiving any home care, whereas relatively unhealthy beneficiaries face a significant decrease in the likelihood of receiving any home care. Columns 3 and 4 show results for home health expenditures.

The overall results for home care utilization are also robust to different definitions of "high" predicted costs. For example, when I define "high" predicted costs to be greater than \$3770, the 75^{th} percentile of predicted costs, I find that the number of home health visits provided to the relatively unhealthy beneficiaries declined eighteen times more than the number among relatively healthy beneficiaries (a coefficient of -.37 for the less healthy beneficiaries and -.02 for the healthier beneficiaries), although the mean utilization is only 6.5 times as high (32.17 visits for the less healthy beneficiaries, as opposed to 5.08 for the less healthy beneficiaries).

7. Effects on Other Usage and Health Outcomes

Tables 6 through 10 provide results from regressions that are similar to those discussed above, but they use the measures of facility, institution, inpatient and outpatient utilization as dependent variables. The impact of IPS restrictiveness on facility care, without controls for mean reversion, is shown in Table 6. Because the most important margin for nursing home utilization decisions is the extensive margin, the regressions for nursing home usage focus only on whether the beneficiary currently resides in a facility (columns 1 and 2), whether the beneficiary has used any facility care during the year (columns 3 and 4) and total expenditures on facility care (columns 5 and 6). The coefficients in Table 6 indicate no significant effect of the IPS on nursing home utilization or expenditures. However, the coefficients in Table 7, which controls for mean reversion, suggest the possibility of a significant *negative* effect on facility utilization. Such a finding could reflect the fact that home care agencies provide an important link between Medicare beneficiaries and the long-term care industry. For example, some home health agencies are affiliated with nursing homes; such an association may increase awareness of other care options for home care recipients. This result echoes a finding from the RAND Health Insurance Experiment that declines in outpatient utilization due to higher copayments were associated with declines in inpatient utilization (Newhouse 1993). In the case of my facility care regressions, the results depend on the specification and are, therefore, more suggestive than conclusive. However, the results for institutional utilization, in Table 8, are similar to those for the longer-term facility care results and are not sensitive to specification. They confirm that declines in home care lead to a decline in residential care.

Table 9 shows results for inpatient utilization, from regressions that include the mean reversion term. Results without the mean reversion term are not shown in the tables, but are not substantively different from the results that are shown. There is no indication that the decline in home care utilization had a significant impact on use of inpatient care. The results for outpatient care in Table 10, in contrast, suggest a significant increase in utilization, which is concentrated among relatively healthy beneficiaries. This finding suggests that relatively healthy patients shifted the location of short-term care from their homes to outpatient settings. This result may seem inconsistent with the generally insignificant impact of the IPS on home care utilization among beneficiaries with "low" predicted costs. However, the confidence intervals in home care utilization regressions never exclude the possibility of small declines in usage among these relatively healthy beneficiaries.

Health Outcomes

In light of my finding that only healthier beneficiaries experienced an offsetting increase in other forms of utilization, the next logical question is whether the decreases in home care utilization affected health outcomes. I use several measures of health, including self-reported health, body mass index, difficulty with ADLs and mortality. In results that are not shown in tables, I find no significant impact on self-reported health. Table 11 shows the impact on body mass index, the probability of being underweight (defined as having a BMI that is less than 18.5) and the probability of being obese (defined as having a BMI that is greater than 30). The results suggest a significant increase in the probability of obesity associated with declines in home care usage among relatively unhealthy beneficiaries. In particular, beneficiaries living in the state that faced the most restrictive cap had a 5 percentage point higher increase in the likelihood of being obese in the post-policy period than beneficiaries living in the state that faced the least restrictive cap. This increase is substantial relative to the mean obesity rate of 17% in the sample. Table 12 shows the effects of home care declines on reported difficulties with four activities: stooping or kneeling, lifting 10 pounds, writing and walking 2-3 blocks.[7] The dependent variable in each column is a dummy variable for reporting a lot of difficulty or an inability to perform the activity. In most cases, there is no significant effect of the IPS on ADLs; the one exception is a significant decrease in the probability that a relatively healthy beneficiary reported difficulty writing. Finally, in Table 13, I show effects on the mortality hazard, using a variety of hazard models. None of the hazard models suggest any impact on death rates. These measures of health outcomes are obviously quite coarse and, thus, should be interpreted as suggestive rather than conclusive. Nevertheless, the results suggest limited adverse health outcomes, in the form of an increased likelihood of obesity, associated with declines in home care.

Caveats to the Findings

One concern about the home care substitution and health outcome results is the issue of external validity. If the IPS led agencies to cut back on precisely those home care benefits that had the lowest marginal clinical value for the unhealthy beneficiaries, it would not be surprising to find that there were no offsetting increases in other care for them or substantial declines in health status, even if there is a very high overall level of substitutability between home care and other care. Likewise, if the IPS led agencies to cut back on precisely those home care benefits that had the highest marginal clinical value for the healthy beneficiaries, it is plausible that further cuts in care to relatively healthy beneficiaries would not incur the same magnitude of increases in outpatient expenditures. The present data, unfortunately, do not allow us to ascertain the marginal clinical value of the lost home care visits. Thus, an important caveat to the present results is that it is plausible that additional cuts could have a different impact.

Another caveat to the present results is that they represent short-run outcomes. It is entirely plausible that many agencies required a year to fully ascertain the implications of the

[7] These regressions necessarily exclude ADL measures from the set of explanatory variables and from the home care cost prediction equations. This change raises the question of whether ADLs should be used as control variables in *any* regressions, since they are potential outcomes. Results that are not shown in the tables reveal that my findings are not sensitive to the exclusion of ADL control variables.

reimbursement changes or to fully incorporate changes to their procedures. Likewise, it is possible that some effects on health outcomes would not become immediately apparent. If so, the results that are obtained here could be an overestimate or underestimate of long-run responses to the IPS.

8. Identification Concerns and Specification Checks

Endogeneity of pre-policy differences?

Given the substantial state-to-state variation in pre-policy utilization that underlies my measure of restrictiveness, a natural question is what generated these regional differences. For example, if this variation was driven entirely by differences in the health status of state residents, my restrictiveness measure could be simply measuring the differential trends in utilization by beneficiaries with different health statuses. However, there is little evidence – either in my data or in other research on this topic – that the large pre-policy differences were generated by differences in patient characteristics. Using 1994 data from the National Home and Hospice Care Survey, which includes information on characteristics of home care agencies and patients, I was able to explain only 9% of the variation in my restrictiveness measure with observable characteristics of the agency, patient and type of care. Other researchers have been similarly unsuccessful at explaining the pre-policy regional variation in utilization using such characteristics and have pointed to regional differences in practice styles as a likely source of the observed variation. As William Scanlon of the GAO told Congress in 1998 testimony, "these extremes are more likely due to differences in practice style and efficiency among agencies rather than patient mix."

In one analysis of the geographic variation, GAO (1996) provided evidence that some states consistently provided more care than other states in data from the early 1990s. Kentucky and Tennessee, for instance, are in the same Census division, but provided dramatically different average levels of care to their home care patients. The GAO reported that the average user in Tennessee received 106 visits, compared to 60 visits for the average user in Kentucky. If there were substantially fewer home care patients in Tennessee than in Kentucky, we might attribute the difference in visits per user to Tennessee's selection of the most unhealthy beneficiaries to receive home care. However, the GAO reported Tennessee actually provided care to approximately 50% more of its beneficiaries than Kentucky did. Moreover, when the GAO compared patients with the same diagnosis in the two states, the patients in Tennessee consistently received more care. Patients with diabetes in Tennessee in the early 1990s received an average of 54 visits, whereas those in Kentucky received an average of 37. Patient with a hip fracture in Tennessee received 39 visits per user, whereas those in Kentucky received only 25. The evidence suggests, therefore, that differences in utilization patterns were not generated by underlying differences in beneficiary health.

The explanation for this historical regional variation in home care utilization was explored in greater detail by Schore (1994). Using detailed data about patient characteristics and diagnoses as well as regional and agency characteristics, she was able to explain about one-third of the regional variation in the number of visits per episode of care in her data from the early 1990s. She proposes differences in physician and agency practice patterns, differences in the availability of nursing home or home- and community-based care, and unobservable patient characteristics as potential explanations for the remaining variation. Differences in physician and agency practice patterns seem particularly plausible. She observes that "agencies with a philosophy of teaching self-care focus on instructing patients (or caregivers) to provide their own care, while other agencies tend to provide all needed care to patients, with less emphasis on instruction and eventual independence" (p. 9). Likewise, her data shows substantial regional variation in some components of treatment plans, such as orders for activity restrictions, for patients who otherwise appear very similar.

Differences in practice style are a plausible – but unfortunately untestable – explanation for historical variation in home care utilization patterns. If this explanation is justified, the reimbursement changes of BBA 97 can be viewed as simply providing a financial incentive for providers with high-usage practice styles to move towards the practice patterns of low utilization states.

Concurrent Changes

Another concern with my identification strategy is the issue of separating the effects of the IPS from the effects of other concurrent changes. While there were several other relevant policy changes around the time of BBA 97, none are likely to have generated the substantial aggregate effects that were observed after 1997. Specifically, the changes that could have contributed to the aggregate decline in home care utilization include the fact that BBA 97 eliminated eligibility for home care based solely on venipuncture and decreased the per-visit reimbursement limit from 112% to 106% of the national average cost. In addition, 1996 legislation added financial penalties for physicians who falsely certify that a patient needs home care, which may have led to lower home care admission rates. One final possibility is that concurrent efforts to reduce fraud and abuse caused the decline in home care utilization. Each of these possible explanations for the large aggregate decline in home care utilizations is discussed below.

The first issue is the fact that BBA 97 eliminated eligibility for home care based only on the need for a skilled nurse to draw blood. Venipuncture – as this procedure is called – is sometimes necessary for patients who are taking blood thinners, heart medications or insulin (Schore 1994). There were some suggestions that doctors had been requesting a nurse to draw blood from their patients at home on a one-time basis in order to qualify them for subsequent home care services, which were then used as a substitute for long-term care. Schore (1994) reports that, in her sample from the early 1990s, venipuncture was a planned treatment at the beginning of 24% of episodes, with substantial regional variation, ranging from 9.9% in New England to 50.4% in East South Central states. I test the sensitivity of my empirical analysis to this eligibility change, by including an interaction between Schore's division-level pre-policy venipuncture rate and a post-policy dummy variable in my regressions. This term does not have a significant impact on the utilization of home health care nor does its inclusion in the regressions affect my empirical results. I therefore conclude that the elimination of home care eligibility on the basis of a need for venipuncture does not contaminate my analysis.

A second concern is the fact that BBA 97 lowered the per-visit limit on reimbursement from 112% of the national average cost to 106% of the national average cost. This change decreased the marginal revenue for providing an additional visit. It is not clear that this change should have led to a decrease in the share of Medicare beneficiaries who used home care, rather than simply decreasing the number of visits that each user received. Moreover, the implied price elasticity of attributing the entire decline in visits per user to this reimbursement change would be implausibly large.[8] Furthermore, HCFA estimated that the per-patient cap, not the per-visit cap,

[8] The implied decrease in the price of a visit caused by this change in the per-visit cap is 5%. GAO (2000) reported a 44% decrease in the number of visits per home care user between 1996 and 1999. Attributing the entire decline to the change in the per-visit cap would imply a price elasticity of almost 9.

was the binding constraint for the majority of agencies in the post-policy period. (*Federal Register* 1998, U.S. Congress 2000)).

The Health Insurance Portability and Accountability Act of 1996 (enacted in August of that year) included financial penalties for physicians who falsely certified that a Medicare patient needed home care. This policy change supposedly had a "chilling effect on physician referrals" (U.S. Congress 2000, p. 139). However, tabulations from the National Home and Hospice Care Survey suggest that the share of home care patients who had been referred by a physician or hospital remained constant at roughly 80% from 1992 through 2000.

Increased fraud and abuse detection efforts and case review were implemented, beginning with a demonstration project "Operation Restore Trust." The project was initially concentrated in California, Florida, Illinois, New York and Texas in 1995 and expanded to 10 additional states in 1997. At the time of IPS implementation, HCFA required its claims processors to implement a newly intensified case review process (U.S. Congress 2000). Such efforts could be responsible for some of the observed decreases, but the overall initiative to decrease fraud and abuse clearly pre-dated the rapid declines in utilization that began in 1998.

In contrast to the potential explanations that were explored above, the IPS can plausibly explain the observed aggregate change in home care utilization. The model in Section 4 suggests that agencies should respond to the IPS with both a decline in (high-cost) users and a decline in service intensity. In fact, by their own account, agencies responded strongly to the IPS. According to a 1999 MedPAC survey, 39% of agencies indicated that the IPS had directly affected their admission decisions, 31% of agencies indicated that the IPS had affected their discharge decisions, and 71% said that they had decreased the total number of visits per patient provided to Medicare beneficiaries since the IPS (MedPAC 1999). To the extent that these other, concurrent policy changes affected any agency decisions, I may overstate the impact of the IPS on agency behavior. However, results about substitution between home care and other forms of care are likely to be valid, regardless of which combination of policies caused the overall decline in home care utilization.

9. Conclusions

In general, my empirical findings about short-run agency behavioral responses to the IPS are consistent with the predictions of the simple model. The evidence generally suggests that agencies responded to the caps by shifting their case-mix towards healthier, less costly patients. Declines in utilization were driven primarily by declines among relatively unhealthy beneficiaries, despite the fact that there was scope for substantive declines among the 7% of relatively healthy beneficiaries who use home care in a typical year. These findings confirm anecdotal reports of decreased access to home care among relatively unhealthy Medicare beneficiaries.

Given the substantial declines in home care utilization, especially among relatively unhealthy beneficiaries, it is important to assess the issue of substitution towards other forms of care. This paper examines the issue of substitution, but finds no increases in facility, institution or inpatient utilization in the years immediately following the policy change. In fact, there is some evidence of a *decline* in both facility and institutional care. This finding may suggest that home care provides an entrée into the residential care industry, perhaps providing patients with new information about the options that are available and leading to increases in the use of these options.

I do find that there is some substitution between home care and outpatient care. Specifically, I find a significant increase in outpatient expenditures among relatively healthy beneficiaries, in specifications with and without mean reversion adjustments. Since Medicare provides substantial reimbursement for outpatient care, this finding suggests that some of the savings that were generated by the IPS were offset by increases in other Medicare expenses.

Finally, I address the issue of whether the decline in home care utilization led to short-term changes in the health status of beneficiaries. I examine measures of self-reported health, body mass index, reports of difficulty with ADLs and mortality; the only apparent adverse impact on health is a significant increase in the probability of obesity associated with the decline in home health care usage.

In sum, I document the decline in home care utilization among Medicare beneficiaries in the wake of the imposition of an average per-patient cap on reimbursement. Consistent with a simple model of agency behavior, this policy change led agencies to shift their case-mix towards healthier patients. The decline in home care utilization was offset, among relatively healthy beneficiaries, by an increase in outpatient care. Among relatively unhealthy beneficiaries, there is no evidence of an offsetting increase in other forms of care, although there is some evidence of a decline in nursing home utilization. Despite all of the changes in utilization that were induced by the imposition of the average per-patient cap, I find limited evidence of adverse consequences for beneficiary health status.

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Table 1: Summary statistics

Variable	Full sample	Predicted High-Cost Beneficiaries	Predicted Low-Cost Beneficiaries
Predicted home care costs (2001 $)	2800	4370	1229
	(2211)	(2088)	(696)
Home health events	12.69	21.99	3.584
	(62.14)	(82.79)	(28.681)
Any home health events	.153	.234	.070
	(.360)	(.423)	(.259)
Home health events, conditional on any	83.15	94.13	49.73
	(139.49)	(150.18)	(95.51)
Expenditures	482	867	97
	(3466)	(4783)	(920)
Facility events	.111	.141	.026
	(.344)	(.379)	(.180)
Any events	.102	.131	.023
	(.302)	(.337)	(.151)
Events, conditional on any	1.09	1.078	1.124
	(.321)	(.297)	(.404)
Expenditures	3034	5167	725
	(13174)	(16468)	(7591)
Institutional events	.088	.150	.021
	(.516)	(.681)	(.215)
Any events	.042	.069	.014
	(.202)	(.254)	(.116)
Events, conditional on any	2.068	2.161	1.556
	(1.477)	(1.530)	(1.003)
Expenditures	352	606	78
	(2480)	(3293)	(970)
Inpatient events	.376	.492	.243
	(.958)	(1.099)	(.741)
Any events	.218	.274	.154
	(.413)	(.446)	(.361)
events, conditional on any	1.73	1.798	1.577
	(1.37)	(1.437)	(1.205)
Expenditures	2867	3766	1895
	(9626)	(11199)	(7441)
Outpatient events	3.68	4.287	2.950
	(9.24)	(10.457)	(7.790)
Any events	.654	.692	.610
	(.476)	(.462)	(.488)
events, conditional on any	5.64	6.196	4.837
	(10.94)	(12.092)	(9.508)
Expenditures	804	924	650
	(2849)	(3152)	(2442)

Table 1: Summary Statistics, continued

Variable	Full sample	Predicted High-Cost Beneficiaries	Predicted Low-Cost Beneficiaries
Age	71.95	73.09	70.45
	(14.55)	(15.41)	(13.07)
Male	.435	.300	.579
	(.496)	(.458)	(.494)
Married	.469	.416	.542
	(.499)	(.493)	(.498)
Body Mass Index	25.8	25.79	25.7
	(5.34)	(6.07)	(4.47)
Underweight	.05	.07	.03
	(.22)	(.26)	(.16)
Obese	.17	.19	.14
	(.38)	(.40)	(.35)
Difficulty stooping	.15	.29	.01
	(.36)	(.45)	(.09)
Difficulty lifting	.15	.28	.01
	(.35)	(.45)	(.08)
Difficulty writing	.03	.05	0
	(.16)	(.22)	(0)
Difficulty walking	.21	.39	.01
	(.41)	(.49)	(.11)
Died	.06	.09	.02
	(.23)	(.28)	(.16)

Table 2: Home Health Utilization

Independent variable	(1) OLS: Visits	(2) OLS: Visits	(3) OLS: Visits	(4) OLS: Visits
Post-BBA 97*Restrict	-.168**	-.158**	-.220**	-.119**
	(.058)	(.058)	(.066)	(.052)
Personal Characteristics				
Male		.236	.274	.270
		(.888)	(.889)	(.889)
Married		-2.748**	-2.781**	-2.789**
		(1.044)	(1.048)	(1.049)
Male*Married		-1.049	-1.010	-.994
		(1.270)	(1.271)	(1.273)
Diabetes		3.268**	3.238**	3.239**
		(.875)	(.868)	(.866)
Stroke		3.688**	3.652**	3.664**
		(1.153)	(1.157)	(1.158)
Alzheimer's		-4.547	-4.612	-4.607
		(3.272)	(3.288)	(3.289)
Hypertension		.766	.781	.780
		(.472)	(.468)	(.468)
Year fixed effects	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes
State trends	No	No	Yes	Yes
Visits94*Post	No	No	No	Yes
Observations	86,101	80,382	80,382	80,382

Other control variables include year, state, state trends, age group, race, education, income, and other health conditions (cancer, mental retardation, mental disorders, osteoporosis, emphysema, paralysis and amputation of arm or leg) and level of difficulty with walking, writing, lifting and kneeling.

Table 3: Home Health Utilization

Independent variable	(1) OLS: Visits	(2) OLS: Visits	(3) OLS: Visits
Post-BBA 97*Restrict	-.028	-.087*	.010
	(.034)	(.046)	(.044)
Post-BBA 97*Restrict *Predicted High Cost	-.236**	-.243**	-.239**
	(.091)	(.089)	(.091)
Personal Characteristics			
Male	.291	.329	.326
	(.888)	(.889)	(.889)
Married	-1.639	-1.672	-1.681
	(1.229)	(1.235)	(1.235)
Male*Married	-2.446	-2.410	-2.392
	(1.484)	(1.489)	(1.491)
Diabetes	3.728**	3.698**	3.698**
	(.934)	(.926)	(.925)
Stroke	3.925**	3.889**	3.901**
	(1.197)	(1.201)	(1.202)
Alzheimer's	-4.596	-4.663	-4.655
	(3.283)	(3.299)	(3.300)
Hypertension	.508	.524	.523
	(.470)	(.468)	(.467)
Year fixed effects	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes
State trends	No	Yes	Yes
Visits94*Post	No	No	Yes
Observations	80,382	80,382	80,382

Other control variables include year, state, state trends, age group, race, education, income, and other health conditions (cancer, mental retardation, mental disorders, osteoporosis, emphysema, paralysis and amputation of arm or leg) and level of difficulty with walking, writing, lifting and kneeling. Regressions that show interactions with high predicted costs also include a control for high predicted cost as well as an interaction of high predicted cost with post-BBA 97. High predicted cost means predicted costs > $2267. Standard errors are clustered on state.

Table 4: Home Health Utilization

Independent variable	(1) Probit Any Events	(2) Probit Any Events	(3) OLS Expenditures	(4) OLS Expenditures
Post-BBA 97*Restrict	-.0004*	.0002	-6.28*	-2.62
	(.0003)	(.0004)	(3.19)	(2.46)
Post-BBA 97*Restrict *Predicted High Cost		-.0010**		-6.00**
		(.0003)		(2.63)
Personal Characteristics				
Male	.012**	.012**	4.12	7.79
	(.004)	(.004)	(43.95)	(43.05)
Married	-.026**	-.028**	-41.88	37.61
	(.006)	(.006)	(73.05)	(80.09)
Male*Married	-.005	-.001	-44.31	-144.10
	(.010)	(.010)	(87.18)	(94.97)
Diabetes	.040**	.047**	284.70**	317.68**
	(.005)	(.006)	(57.18)	(59.82)
Stroke	.031**	.047**	205.75*	223.35**
	(.005)	(.007)	(104.37)	(106.20)
Alzheimer's	-.046**	-.079**	-258.52	-264.62
	(.006)	(.013)	(173.90)	(175.00)
Hypertension	.015**	.012**	25.57	7.03
	(.003)	(.003)	(32.66)	(33.02)
Observations	80,382	80,382	80,382	80,382

Other control variables include year, state, state trends, age group, race, education, income, and other health conditions (cancer, mental retardation, mental disorders, osteoporosis, emphysema, paralysis and amputation of arm or leg) and level of difficulty with walking, writing, lifting and kneeling. Regressions that show interactions with high predicted costs also include a control for high predicted cost as well as an interaction of high predicted cost with post-BBA 97. High predicted cost means predicted costs > $2267. Marginal effects are shown for Probit results. Standard errors are clustered on state.

Table 5: Home Health Utilization

Controlling for Mean Reversion

Independent variable	(1) Probit Any Events	(2) Probit Any Events	(3) OLS Expenditures	(4) OLS Expenditures
Post-BBA 97*Restrict	-.0003	.0004	-.55	2.80
	(.0003)	(.0004)	(2.18)	(1.71)
Post-BBA 97*Restrict *Predicted High Cost		-.0010**		-5.78**
		(.0003)		(2.71)
Personal Characteristics				
Male	.012**	.012**	3.90	7.61
	(.004)	(.004)	(43.90)	(43.01)
Married	-.026**	-.028**	-43.40	37.08
	(.006)	(.006)	(72.92)	(79.96)
Male*Married	-.005	-.001	-43.40	-143.14
	(.010)	(.010)	(87.11)	(94.88)
Diabetes	.040**	.039**	284.75**	317.67**
	(.005)	(.005)	(57.21)	(59.85)
Stroke	.031**	.031**	206.44*	223.97**
	(.005)	(.005)	(104.40)	(106.22)
Alzheimer's	-.046**	-.046**	-258.22	-264.17
	(.006)	(.006)	(173.76)	(174.82)
Hypertension	.015**	.015**	25.46	6.96
	(.003)	(.003)	(32.63)	(33.00)
Observations	80,382	80,382	80,382	80,382

Other control variables include year, state, state trends, age group, race, education, income, and other health conditions (cancer, mental retardation, mental disorders, osteoporosis, emphysema, paralysis and amputation of arm or leg) and level of difficulty with walking, writing, lifting and kneeling. Regressions that show interactions with high predicted costs also include a control for high predicted cost as well as an interaction of high predicted cost with post-BBA 97. High predicted cost means predicted costs > $2267. Marginal effects are shown for Probit results. Standard errors are clustered on state.

Table 6: Facility Utilization

Independent variable	(1) Probit Facility Resident	(2) Probit Facility Resident	(3) Probit Any Events	(4) Probit Any Events	(5) OLS Expenditures	(6) OLS Expenditures
Post-BBA 97*Restrict	-.0002	.00004	-.0002	-.0001	10.04	3.61
	(.0001)	(.00024)	(.0001)	(.0002)	(7.51)	(9.16)
Post-BBA 97*Restrict *Predicted High Cost		-.0004		-.0004		18.03
		(.0003)		(.0003)		(15.43)
Personal Characteristics						
Male	.007**	.007**	.007**	.007**	612.67**	610.66**
	(.001)	(.001)	(.001)	(.001)	(281.38)	(283.21)
Married	-.012**	-.012**	-.012**	-.012**	-1040.54**	-941.83**
	(.002)	(.002)	(.002)	(.002)	(184.44)	(184.46)
Male*Married	-.003	-.003	-.003	-.002	170.93	48.47
	(.003)	(.003)	(.003)	(.003)	(267.49)	(279.77)
Diabetes	-.001*	-.001*	-.001*	-.001*	581.98	-536.99**
	(.002)	(.002)	(.002)	(.002)	(172.79	(173.45)
Stroke	.008**	.009**	.008**	.008**	122.62	149.22
	(.002)	(.002)	(.002)	(.002)	(160.27)	(160.16)
Alzheimer's	.119**	.121**	.115**	.117**	9885.18**	9845.36**
	(.011)	(.011)	(.011)	(.011)	(898.13)	(894.04)
Hypertension	-.008**	-.008**	-.007**	-.008**	725.52**	-750.39**
	(.001)	(.001)	(.001)	(.001)	(166.58)	(167.00)
Observations	80,382	80,382	80,382	80,382	80,382	80,382

Other control variables include year, state, state trends, age group, race, education, income, and other health conditions (cancer, mental retardation, mental disorders, osteoporosis, emphysema, paralysis and amputation of arm or leg) and level of difficulty with walking, writing, lifting and kneeling. Regressions that show interactions with high predicted costs also include a control for high predicted cost as well as an interaction of high predicted cost with post-BBA 97. High predicted cost means predicted costs > $2267. Marginal effects are shown for Probit results. Standard errors are clustered on state.

Table 7: Facility Utilization
Controlling for Mean Reversion

Independent variable	(1) Probit Facility Resident	(2) Probit Facility Resident	(3) Probit Any Events	(4) Probit Any Events	(5) OLS Expenditures	(6) OLS Expenditures
Post-BBA 97*Restrict	-.0003**	-.0001	-.0003**	-.0001	1.48	-11.06
	(.0001)	(.0002)	(.0001)	(.0002)	(7.46)	(10.74)
Post-BBA 97*Restrict *Predicted High Cost		.0003		-.0003		17.44
		(.0003)		(.0003)		(15.70)
Personal Characteristics						
Male	.007**	.007**	.007**	.007**	611.95**	611.15**
	(.001)	(.001)	(.001)	(.001)	(281.50)	(283.43)
Married	-.012**	-.012**	-.012**	-.012**	-1039.11**	-940.39**
	(.002)	(.002)	(.002)	(.002)	(184.05)	(183.80)
Male*Married	-.003	-.003	-.002	-.002	169.76	45.86
	(.003)	(.003)	(.003)	(.003)	(267.79)	(280.22)
Diabetes	-.001	-.001	-.001	-.001	-581.62**	-536.96**
	(.002)	(.002)	(.002)	(.002)	(172.84)	(173.55)
Stroke	.008**	.009**	.008**	.008**	121.74	147.53
	(.002)	(.002)	(.002)	(.002)	(160.16)	(160.07)
Alzheimer's	.119**	.121**	.115**	.117**	9881.05**	9844.15**
	(.011)	(.011)	(.011)	(.010)	(898.12)	(893.72)
Hypertension	-.008**	-.008**	-.007**	-.008**	-726.17**	-750.21**
	(.001)	(.001)	(.001)	(.001)	(166.59)	(167.05)
Observations	80,382	80,382	80,382	80,382	80,382	80,382

Other control variables include year, state, state trends, age group, race, education, income, and other health conditions (cancer, mental retardation, mental disorders, osteoporosis, emphysema, paralysis and amputation of arm or leg) and level of difficulty with walking, writing, lifting and kneeling. Regressions that show interactions with high predicted costs also include a control for high predicted cost as well as an interaction of high predicted cost with post-BBA 97. High predicted cost means predicted costs > $2267. Marginal effects are shown for Probit results. Standard errors are clustered on state.

Table 8: Institutional Utilization

Controlling for Mean Reversion

Independent variable	(1) OLS Institution Events	(2) OLS Institution Events	(3) Probit Any Events	(4) Probit Any Events	(5) OLS Expenditures	(6) OLS Expenditures
Post-BBA 97*Restrict	-.0007**	.0001	-.0001	.0001	-3.14	.55
	(.0003)	(.0003)	(.0001)	(.0001)	(2.28)	(1.91)
Post-BBA 97*Restrict *Predicted High Cost		-.002**		-.0003		-7.14**
		(.001)		(.0002)		(3.08)
Personal Characteristics						
Male	.019**	.019**	.009**	.008**	62.45**	62.78**
	(.006)	(.006)	(.001)	(.001)	(25.62)	(25.72)
Married	-.017**	-.015**	-.009**	-.009**	-67.55**	-60.87**
	(.006)	(.007)	(.003)	(.003)	(26.87)	(29.78)
Male*Married	-.008	-.010	.002	.003	-17.93	-26.72
	(.007)	(.007)	(.005)	(.005)	(27.98)	(31.75)
Diabetes	.028**	.029**	.004*	.004*	118.58**	121.37**
	(.005)	(.005)	(.002)	(.002)	(27.36)	(27.61)
Stroke	.046**	.046**	.006**	.006**	153.88**	154.95**
	(.009)	(.009)	(.002)	(.002)	(32.48)	(31.94)
Alzheimer's	.128**	.128**	.010**	.010**	457.69**	458.32**
	(.019)	(.019)	(.002)	(.002)	(74.26)	(74.32)
Hypertension	-.008**	-.008**	-.0002	-.0001	-25.42*	-26.77**
	(.004)	(.004)	(.0015)	(.0015)	(13.10)	(12.84)
Observations	80,382	80,382	80,339	80,339	80,382	80,382

Other control variables include year, state, state trends, age group, race, education, income, and other health conditions (cancer, mental retardation, mental disorders, osteoporosis, emphysema, paralysis and amputation of arm or leg) and level of difficulty with walking, writing, lifting and kneeling. Regressions that show interactions with high predicted costs also include a control for high predicted cost as well as an interaction of high predicted cost with post-BBA 97. High predicted cost means predicted costs > $2267. Marginal effects are shown for Probit results. Standard errors are clustered on state.

Table 9: Inpatient Care Utilization

Controlling for Mean Reversion

Independent variable	(1) OLS Inpatient Events	(2) OLS Inpatient Events	(3) Probit Any Events	(4) Probit Any Events	(5) OLS Expenditures	(6) OLS Expenditures
Post-BBA 97*Restrict	-.0007	-.0008	-.000001	.0001	-2.56	-1.37
	(.0009)	(.0009)	(.0003)	(.0005)	(9.80)	(11.50)
Post-BBA 97*Restrict *Predicted High Cost		.0001		-.0002		7.54
		(.0012)		(.0007)		(8.93)
Personal Characteristics						
Male	.098**	.098**	.047**	.046**	1004.83**	1002.68**
	(.014)	(.014)	(.004)	(.004)	(158.75)	(158.21)
Married	-.007	-.012	-.004	-.007	-34.54	-73.60
	(.014)	(.014)	(.005)	(.006)	(138.89)	(148.97)
Male*Married	-.036*	-.030	-.016*	-.011**	-235.67	-185.99
	(.019)	(.020)	(.008)	(.008)	(228.97)	(228.68)
Diabetes	.167**	.165**	.069**	.067**	1329.10**	1313.20**
	(.017)	(.017)	(.006)	(.006)	(218.34)	(219.08)
Stroke	.149**	.148**	.067**	.066**	1074.25**	1066.43**
	(.022)	(.022)	(.007)	(.007)	(197.96)	(199.65)
Alzheimer's	-.052*	-.051*	.003	.004	-498.79*	-498.75*
	(.027)	(.027)	(.008)	(.008)	(277.16)	(277.42)
Hypertension	.055**	.056**	.028**	.029**	478.99**	487.39**
	(.007)	(.007)	(.003)	(.003)	(81.76)	(81.35)
Observations	80,382	80,382	80,382	80,382	80,382	80,382

Other control variables include year, state, state trends, age group, race, education, income, and other health conditions (cancer, mental retardation, mental disorders, osteoporosis, emphysema, paralysis and amputation of arm or leg) and level of difficulty with walking, writing, lifting and kneeling. Regressions that show interactions with high predicted costs also include a control for high predicted cost as well as an interaction of high predicted cost with post-BBA 97. High predicted cost means predicted costs > $2267. Marginal effects are shown for Probit results. Standard errors are clustered on state.

Table 10: Outpatient Care Utilization

Controlling for Mean Reversion

Independent variable	(1) OLS Outpatient events	(2) OLS Outpatient Events	(3) Probit Any Events	(4) Probit Any Events	(5) OLS Expenditures	(6) OLS Expenditures
Post-BBA 97*Restrict	.011	.025*	.0006	.001	8.78**	11.92**
	(.011)	(.013)	(.0007)	(.001)	(2.51)	(3.76)
Post-BBA 97*Restrict *Predicted High Cost		-.027**		-.0013**		-5.86
		(.013)		(.0005)		(3.98)
Personal Characteristics						
Male	.095	.096	-.016**	-.016**	108.90*	109.65*
	(.207)	(.206)	(.007)	(.007)	(61.76)	(61.58)
Married	.360	.369	.019**	.017*	148.23*	162.77*
	(.215)	(.222)	(.009)	(.009)	(79.14)	(82.00)
Male*Married	-.446	-.457	-.024	-.021	-242.60	-260.99
	(.420)	(.423)	(.016)	(.016)	(156.29)	(159.85)
Diabetes	1.692**	1.696**	.073**	.073**	412.38**	418.44**
	(.228)	(.228)	(.007)	(.007)	(78.58)	(79.53)
Stroke	.860**	.862**	.044**	.044**	216.03**	219.09**
	(.196)	(.197)	(.008)	(.008)	(60.34)	(60.92)
Alzheimer's	-.217	-.215	.014	.014	-34.61	-34.96
	(.211)	(.211)	(.017)	(.017)	(61.83)	(61.84)
Hypertension	.709**	.706**	.048**	.048**	171.41**	167.96**
	(.148)	(.148)	(.005)	(.005)	(48.56)	(49.12)
Observations	80,382	80,382	80,369	80,369	80,382	80,382

Other control variables include year, state, state trends, age group, race, education, income, and other health conditions (cancer, mental retardation, mental disorders, osteoporosis, emphysema, paralysis and amputation of arm or leg) and level of difficulty with walking, writing, lifting and kneeling. Regressions that show interactions with high predicted costs also include a control for high predicted cost as well as an interaction of high predicted cost with post-BBA 97. High predicted cost means predicted costs > $2267. Marginal effects are shown for Probit results. Standard errors are clustered on state.

Table 11: Body Mass Index (BMI)

Controlling for Mean Reversion

Independent variable	(1) OLS: BMI	(2) OLS: BMI	(3) Probit: Underweight (BMI<18.5)	(4) Probit: Underweight (BMI<18.5)	(5) Probit: Obese (BMI>30)	(6) Probit: Obese (BMI>30)
Post-BBA 97*Restrict	.001	-.001	.00003	.0002	.0007**	.014
	(.003)	(.007)	(.00019)	(.0002)	(.0003)	(.014)
Post-BBA 97*Restrict *Predicted High Cost		.004		-.0002		.0011**
		(.010)		(.0003)		(.0005)
Personal Characteristics						
Male	-.712**	-.712**	-.008**	-.008**	-.070**	-.070**
	(.120)	(.119)	(.003)	(.003)	(.007)	(.007)
Married	-.407**	-.424**	.008*	.008**	-.008	-.010*
	(.120)	(.121)	(.003)	(.002)	(.006)	(.006)
Male*Married	1.365**	1.387**	-.025**	-.025**	.033**	.036**
	(.201)	(.199)	(.004)	(.003)	(.010)	(.011)
Diabetes	1.451**	1.444**	-.016**	-.016**	.082**	.080**
	(.106)	(.107)	(.002)	(.002)	(.007)	(.007)
Stroke	-.675**	-.678**	.011**	.011**	-.027**	-.027**
	(.118)	(.118)	(.003)	(.003)	(.006)	(.006)
Alzheimer's	-1.208**	-1.205**	.025**	.025**	-.034**	-.034**
	(.155)	(.156)	(.005)	(.005)	(.009)	(.009)
Hypertension	1.503**	1.507**	-.020**	-.020**	.074**	.075**
	(.064)	(.064)	(.002)	(.002)	(.004)	(.004)
Observations	79,472	79,472	79,472	79,472	79,472	79,472

Other control variables include year, state, state trends, age group, race, education, income, and other health conditions (cancer, mental retardation, mental disorders, osteoporosis, emphysema, paralysis and amputation of arm or leg) and level of difficulty with walking, writing, lifting and kneeling. Regressions that show interactions with high predicted costs also include a control for high predicted cost as well as an interaction of high predicted cost with post-BBA 97. High predicted cost means predicted costs > $2267. Marginal effects are shown for Probit results. Standard errors are clustered on state.

Table 12: Problems with Activities of Daily Living
Controlling for Mean Reversion

Independent variable	(1) Probit Stooping	(2) Probit Stooping	(3) Probit Lifting	(4) Probit Lifting	(5) Probit Writing	(6) Probit Writing	(7) Probit Walking	(8) Probit Walking
Post-BBA 97*Restrict	-.0001	-.00001	-.0002	-.0002	-.0001	-.0003**	.0001	.0004
	(.0003)	(.0004)	(.0004)	(.004)	(.0001)	(.0001)	(.0005)	(.0006)
Post-BBA 97*Restrict *Predicted High Cost		-.0002		-.00002		.0003		-.0004
		(.0004)		(.0007)		(.0002)		(.0004)
Personal Characteristics								
Male	-.034**	-.034**	-.040**	-.039**	.002*	.002*	-.035**	-.035**
	(.006)	(.006)	(.004)	(.004)	(.001)	(.001)	(.007)	(.007)
Married	-.024**	-.025**	-.021**	-.022**	.0005	.0002	-.040**	-.041**
	(.008)	(.008)	(007)	(007)	(.0023)	(.0022)	(.008)	(.008)
Male*Married	.040**	.041**	.047**	.049**	.004	.004	.080**	.080**
	(.011)	(.011)	(.013)	(.013)	(.004)	(.003)	(.012)	(.012)
Diabetes	.037**	.036**	.015**	.014**	.0006	.0006	.046**	.046**
	(.007)	(.007)	(.007)	(.007)	(.0021)	(.0020)	(.006)	(.007)
Stroke	.028**	.028**	.037**	.036**	.010**	.009**	.033**	.033**
	(.009)	(.009)	(.010)	(.010)	(.004)	(.004)	(.012)	(.012)
Alzheimer's	.114**	.114**	.135**	.134**	.041**	.040**	.147**	.146**
	(.016)	(.016)	(.022)	(.022)	(.010)	(.010)	(.020)	(.020)
Hypertension	.029**	.029**	.017**	.017**	-.002**	-.002**	.046**	.046**
	(.004)	(.004)	(.003)	(.003)	(.001)	(.001)	(.004)	(.004)
Observations	81,829	81,829	81,817	81,817	81,631	81,631	81,824	81,824

Other control variables include year, state, state trends, age group, race, education, income, and other health conditions (cancer, mental retardation, mental disorders, osteoporosis, emphysema, paralysis and amputation of arm or leg). Regressions that show interactions with high predicted costs also include a control for high predicted cost as well as an interaction of high predicted cost with post-BBA 97. High predicted cost means predicted costs > $2267. Marginal effects are shown for Probit results. Standard errors are clustered on state.

Table 13: Mortality Hazard Models

Controlling for Mean Reversion

Independent variable	(1) Cox Proportional Hazards Model	(2) Cox Proportional Hazards Model	(3) Log-logistic Model	(4) Log-logistic Model	(5) Log-normal Model	(6) Log-normal Model
Post-BBA 97*Restrict	-.0007	.001	.00004	-.00007	-.0001	-.0004
	(.0045)	(.010)	(.00035)	(.00071)	(.0005)	(.0009)
Post-BBA 97*Restrict *Predicted High Cost		-.003		.0002		.0004
		(.009)		(.0006)		(.0009)
Personal Characteristics						
Male	.754**	.752**	-.061**	-.061**	-.104**	-.104**
	(.057)	(.057)	(.005)	(.005)	(.008)	(.008)
Married	1.816**	1.809**	-.068**	-.068**	-.057**	-.056**
	(.100)	(.100)	(.004)	(.004)	(.006)	(.006)
Male*Married	-3.424**	-3.414**	.123**	.123**	.099**	.098**
	(.173)	(.174)	(.006)	(.006)	(.010)	(.010)
Diabetes	1.636**	1.635**	-.068**	-.067**	-.057**	-.057**
	(.061)	(.061)	(.003)	(.003)	(.004)	(.004)
Stroke	1.100**	1.101**	-.036**	-.036**	-.018**	-.018**
	(.082)	(.082)	(.004)	(.004)	(.006)	(.006)
Alzheimer's	.767**	.774**	-.023**	-.023**	-.0001	-.0005
	(.070)	(.071)	(.004)	(.004)	(.0068)	(.0069)
Hypertension	-.688**	-.688**	.025**	.025**	.020**	.020**
	(.053)	(.053)	(.002)	(.002)	(.004)	(.004)
Observations	80,382	80,382	80,382	80,382	80,382	80,382

Other control variables include year, state, state trends, age group, race, education, income, and other health conditions (cancer, mental retardation, mental disorders, osteoporosis, emphysema, paralysis and amputation of arm or leg). Regressions that show interactions with high predicted costs also include a control for high predicted cost as well as an interaction of high predicted cost with post-BBA 97. High predicted cost means predicted costs > $2267. Marginal effects are shown for Probit results. Standard errors are clustered on state.

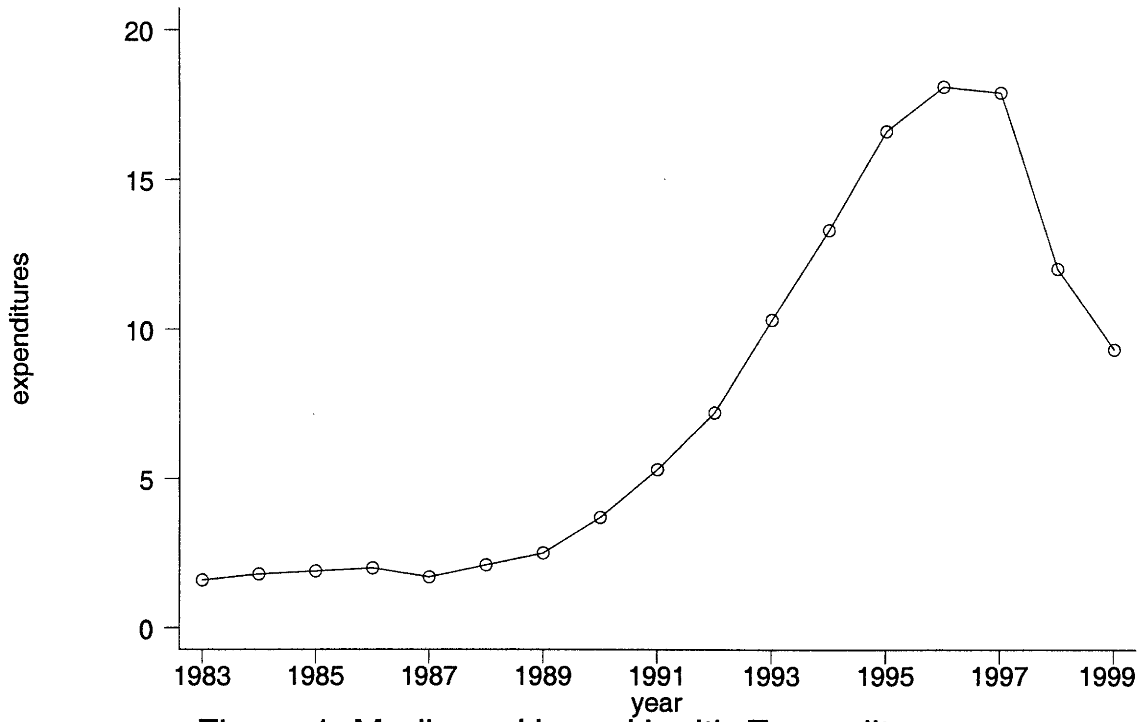


Figure 1: Medicare Home Health Expenditures

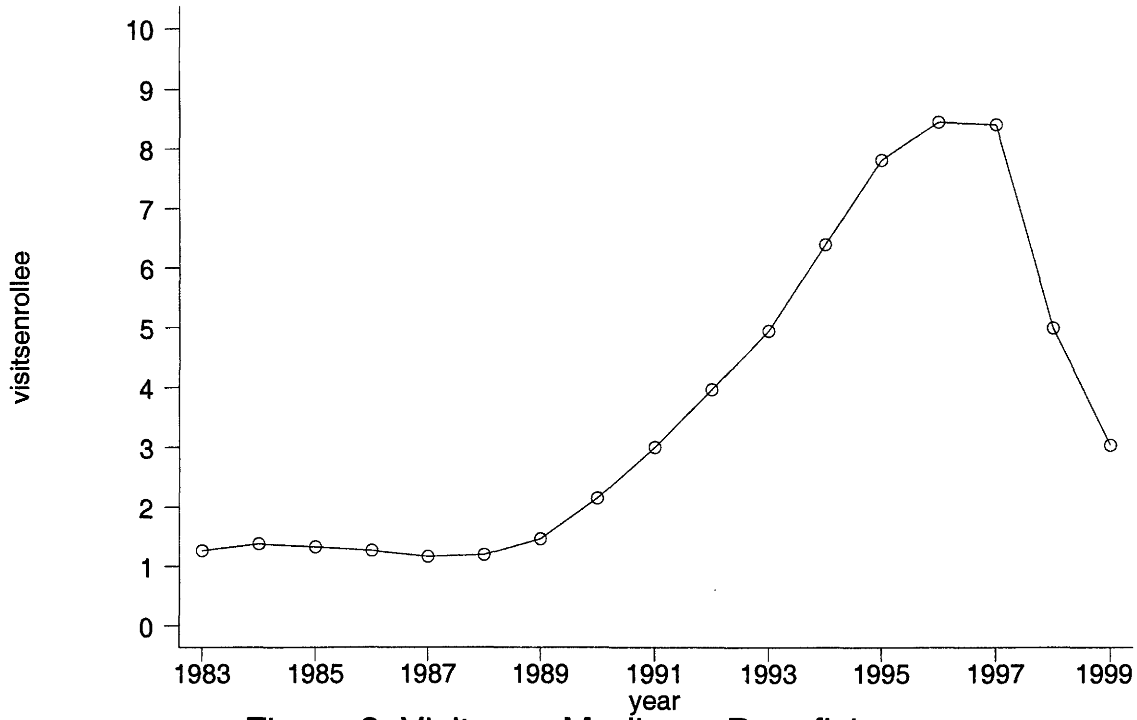


Figure 2: Visits per Medicare Beneficiary

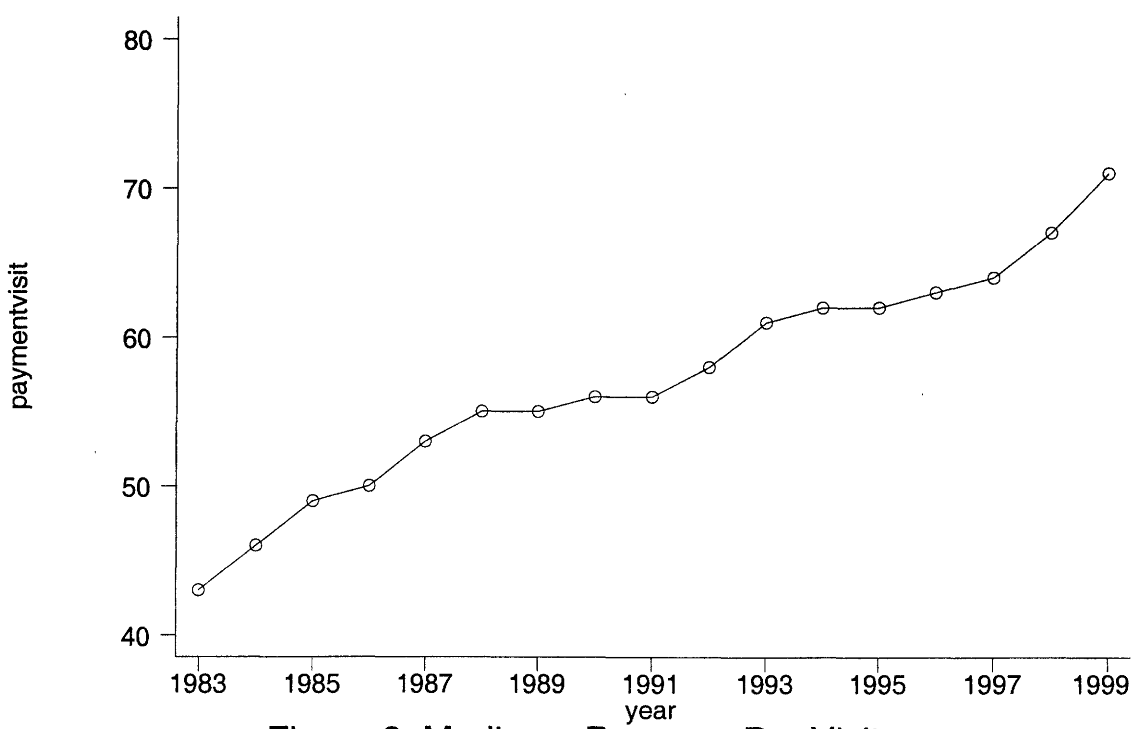


Figure 3: Medicare Payment Per Visit

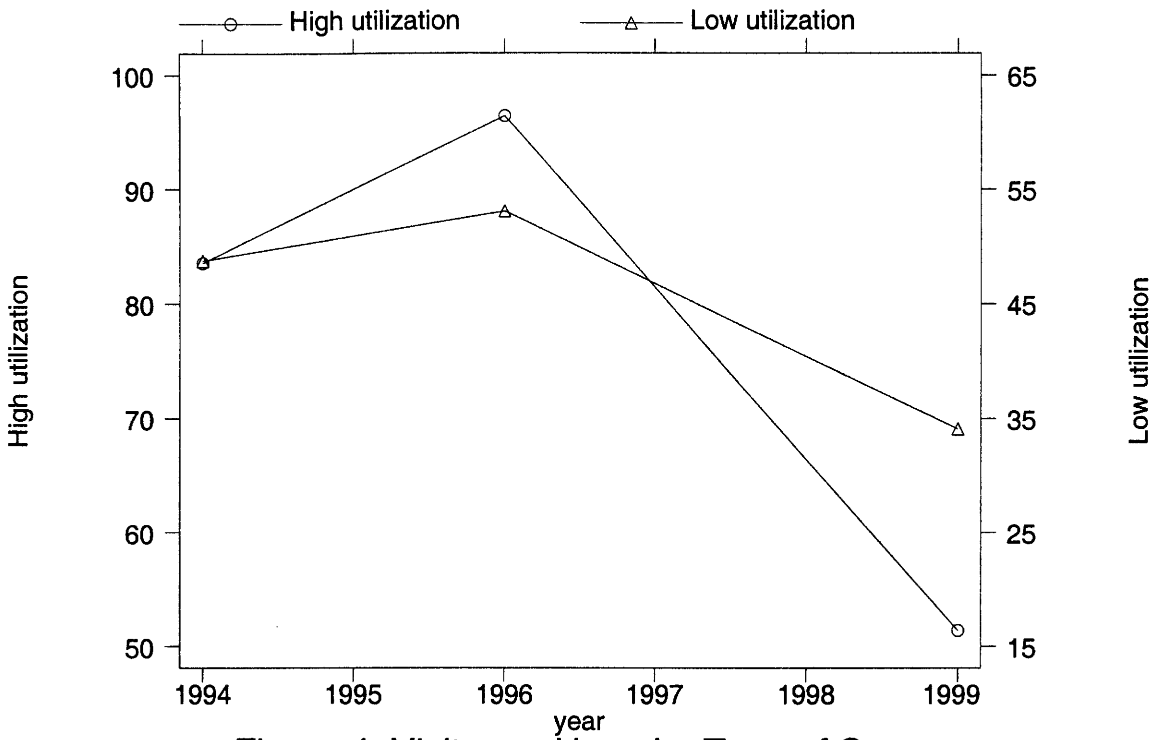


Figure 4: Visits per User, by Type of State

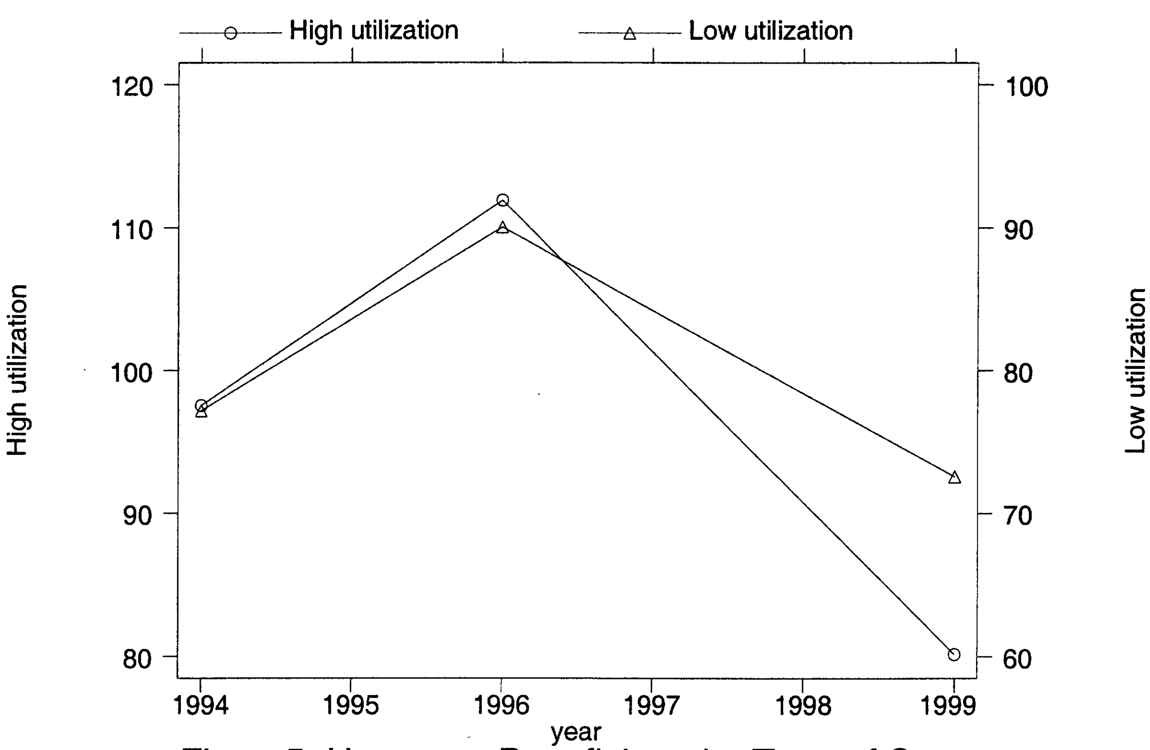


Figure5: Users per Beneficiary, by Type of State

Chapter 2: Medicare Balance Billing Restrictions: Impacts on Physicians and Beneficiaries

1. Introduction

Medicare balance billing is the practice of billing Medicare beneficiaries for physician charges in excess of the copayment and reimbursement amounts approved by Medicare. During the late 1980s and early 1990s, in an effort to protect beneficiaries from out-of-pocket liabilities, state and federal policies restricted the ability of physicians to balance bill Medicare beneficiaries. These restrictions raised concerns about whether restricting the price that physicians can charge to beneficiaries would restrict access to care or the quality of care provided. More recently, similar questions have arisen in the context of "concierge physicians" who require substantial out-of-pocket payments in excess of reimbursement provided by insurance companies.

Economic theory suggests that physicians may have responded to restrictions on balance billing by adjusting either the quantity or quality of services they provided to Medicare beneficiaries. Theory does not, however, provide unambiguous predictions about the direction of the effect on physician behavior. Depending on whether the model incorporates quality of care as a choice variable or allows for features such as demand inducement, physician income targeting or demand constraints, the predictions of the model may vary. Furthermore, empirical research on the effects of balance billing restrictions has been quite limited. GAO (1989) analyzed data from the first four states that implemented policies, but concluded that the available data covered "too short of a time to determine whether physicians modified their behavior in response to the laws" (p. 37).

In this paper, I provide new empirical evidence on physician responses to Medicare balance billing restrictions. I use variation in the timing, location and eligibility requirements of restrictions to identify the effects of the restrictions. Some of the initial state policies, for example, only applied to beneficiaries with income below certain levels. Likewise, individuals under the age of 65 were typically not affected by any of the policies, because they are not age-eligible for Medicare; this slightly younger group provides a good control in my empirical analysis for secular trends in medical expenditures and utilization that may have also affected Medicare beneficiaries.

I begin by quantifying the effects of balance billing restrictions on household out-of-pocket medical expenditures, using data from the Consumer Expenditure Survey (CEX). My analysis indicates that balance billing restrictions led to an annual decline of approximately $120 in out-of-pocket expenditures for physician services among households with elderly members. This decline represents a 9% decrease in overall spending for medical services among elderly households. The results also indicate that these spending declines were uniform across household of different income levels, suggesting that high-income households benefited as much as low-income households from the decline in physician reimbursement.

Next, I consider the effects of balance billing restrictions on the quantity of care received by Medicare beneficiaries. Because balance billing restrictions decreased the marginal reimbursement for providing an additional medical service to the subset of Medicare beneficiaries who were previously paying balance bills, physicians may have responded by changing the supply of care available to Medicare patients. Using data from the National Health Interview Survey (NHIS), I investigate this issue and find no evidence that the number of doctor visits provided to Medicare beneficiaries changed.

However, even if the number of visits was unchanged, the location of care may have changed in response to changes in balance billing policy. Baker and Royalty (2000) have previously observed that physicians in some settings, such as emergency rooms, have less ability to turn away relatively unprofitable patients than physicians in other settings, such as private offices. In fact, Baker and Royalty report that increases in Medicaid reimbursement rates did not change the quantity of care received by Medicaid recipients, but did cause a shift in the location of care away from public hospitals and clinics and towards private offices. On the other hand, Medicare beneficiaries who are concerned about the out-of-pocket expense of a medical visit may delay care until it becomes an emergency. Evidence from the NHIS supports this latter hypothesis, indicating a significant decline in the likelihood that a doctor visit occurs in the emergency room. However, the NHIS results also indicate an offsetting increase in the likelihood of a doctor visit in the hospital, so it is not clear that the shift in visit location is substantively important.

Next, I turn to a survey of doctor visits, the National Ambulatory Medical Care Survey (NAMCS), to assess the effects of balance billing restrictions on the duration of doctor visits and the planned follow-up. Such variables may be interpreted as proxies for the quality of care.

Balance billing restrictions have no significant impact on the duration of doctor visits, but do have a significant, negative impact on the likelihood of planning a follow-up telephone call. The result reflects a decision by physicians to spend less time with their Medicare patients in response to balance billing restrictions. Thus, although there is no evidence that Medicare beneficiaries experienced a change in the quantity of medical care after balance billing restrictions were imposed, it appears that they experienced a small decline in the quality of the care.

Finally, I consider the possibility of general equilibrium effects in the market for physicians. Using aggregate data on the number of physicians of each specialty across states and over time, I find no significant evidence that the supply of physicians was affected by the balance billing restrictions.

To summarize, I find that Medicare patients – of all income ranges – benefited from lower out-of-pocket expenditures as a result of balance billing restrictions. However, the restrictions also led to a decline in the probability that a physician schedule a follow-up telephone conversation with his patient. In addition, balance billing restrictions were associated with a shift away from medical care provided in an emergency room.

This paper proceeds as follows. I begin, in Section 2, by providing the legislative history of balance billing restrictions. In Section 3, I present a simple model of physician behavior and discuss the predictions of the effects of balance billing restrictions. Section 4 describes related previous theoretical and empirical research. I describe my data sources in Section 5 and my identification strategy in Section 6. Section 7 presents my empirical results and Section 8 concludes.

2. Background and Legislative History

Medicare historically reimbursed physicians for their "customary, prevailing and reasonable" fee, which meant that physicians were reimbursed by Medicare for the lower of "(1) the actual charge (the billed amount), (2) the physician's customary charge (the median charge of all charges by that physician for that service over the previous 12 months), or (3) the prevailing charge (sufficient to cover the customary charge for three out of four bills for all physicians in the geographic area)" (GAO 1989, p. 9). Before 1984, doctors had a choice of "accepting assignment" or not. If the doctor accepted assignment, he would receive 80% of the Medicare allowed charge directly from Medicare and could bill the patient for the 20% copayment, but was not permitted to balance bill. If a doctor did not accept assignment, he would bill the patient for the full cost of the service and the patient would be reimbursed by Medicare for 80% of Medicare's allowed charge.[9] Hence, physicians who did not accept assignment were permitted to balance bill, but ran the risk of receiving no payment for any of their charges; in contrast, physicians who did accept assignment were guaranteed payment of at least 80% of the Medicare fee, but were not permitted to balance bill.

In the 1980s, there was growing concern about the financial liability faced by Medicare beneficiaries. In 1982, liability for balance billing had grown to 22 percent of the total part B out-of-pocket liability faced by beneficiaries (McMillan, Lubitz and Newton 1985). As a result, a number of measures were taken to encourage physicians to accept assignment. In 1984, the "Participating Physician and Supplier Program" was introduced, which defined a "participating physician" as a doctor who agreed to always accept assignment for Medicare patients. Between 1984 and 1990, numerous efforts were made to persuade doctors to "participate". Efforts included publishing a directory of participating doctors for Medicare beneficiaries and offering a 5% higher Medicare allowed charge to participating doctors than to non-participating doctors. Also, the Omnibus Budget Reconciliation Act of 1986 (OBRA 86) restricted the growth of billed charges for non-participating doctors whose charges were greater than 115% of the national average prevailing charge for the procedure to a nominal growth rate of 1% per year.

Effective in spring 1986, doctors in Massachusetts were required to accept assignment or lose their license to practice in the state. This law (and subsequent laws that restricted balance billing in other states) did not require doctors to treat Medicare beneficiaries; it only required that, if they chose to treat Medicare beneficiaries, they could not balance bill them.

In 1987, Connecticut, Vermont and Rhode Island implemented mandatory assignment laws that applied to lower-income beneficiaries. Based on their income, 68% of Connecticut beneficiaries, 49% of Rhode Island beneficiaries, and 90% of Vermont beneficiaries were eligible for mandatory assignment (GAO (1989)). Effective January 1, 1990, Rhode Island's

[9]Medigap policies typically have not covered balance bills, so balance bills represent additional out-of-pocket costs to beneficiaries (GAO 1989, Rice (1984)).

mandatory assignment law was expanded to cover all beneficiaries. Pennsylvania required all doctors to accept assignment, effective Sept. 8, 1990.

The Omnibus Budget Reconciliation Act of 1989 (OBRA 89) legislated a new Medicare fee schedule, which was implemented beginning in 1992, and imposed restrictions on balance billing, which were implemented beginning in 1991. For each procedure/region, there is a "recognized payment amount" for non-participating physicians, which is 95% of the recognized payment amount for participating physicians. There is also a "limiting charge" which is the upper bound on billed charges by non-participating physicians. In 1991, the limiting charge was 125% of the recognized payment amount; this limit decreased to 120% in 1992 and 115% in 1993. Since the fee for non-participants is 95% of the fee for participants, physicians have effectively been permitted to bill their patients only 9.25% above the Medicare participating physician fee since 1993. New York implemented a more stringent limiting charge of 115% of the recognized payment amount beginning in 1991; New York's limiting charge fell to 110% in 1992.

Advocates have argued that balance billing restrictions would lead to greater access to medical care for the elderly. In particular, they claimed that the elderly would be more likely to obtain necessary medical care if they did not face any uncertainty about out-of-pocket costs. Uncertainty arises from the fact that patients do not always have the option to choose their specialists and from the fact that an individual physician treating an individual patient may choose to accept assignment on one visit, but not another (GAO 1989, PPRC 1988). In addition, advocates pointed out that roughly half of Medicare beneficiaries did not understand the term "assignment" and approximately three-quarters had not heard of the Participating Physician and Supplier program (GAO 1989). Given these facts, advocates argued that it was unreasonable to expect beneficiaries to lower their out-of-pocket costs by finding and using a participating physician. Thus, they anticipated that restrictions on balance billing would increase access to care by the elderly.

Opponents argued that balance billing restrictions would have the opposite effect, reducing access to care for Medicare beneficiaries. In particular, they suggested that physicians would be less willing to treat Medicare patients and, when balance billing regulations had been enacted in only a few states, physicians might move to states with less restrictive policies (GAO 1989). In 1987, William McDermott of the Massachusetts Medical Society said that, in response to Massachusetts' balance billing restriction, "you're going to find a lessening of access for elderly patients" (UPI 1987). Likewise, Kirk Johnson of the American Medical Association suggested that, under such policies, beneficiaries might receive inferior treatment (Wald 1987). Concern about the adverse affects of balance billing restrictions was sufficiently strong that, when the Puget Sound Council of Senior Citizens sponsored a public referendum in Washington to ban balance billing, the state chapter of the AARP opposed it (PPRC 1988).

3. Theoretical Framework

A simple model of the physician as an income-maximizer provides insights into how physicians might respond to restrictions on balance billing. Assume that a physician acts to maximize his income:

$$(1) \qquad I = p(Q_{Priv}, f) \cdot Q_{Priv} + f \cdot (Q_{Total} - Q_{Priv}) - c(Q_{Total}) \cdot Q_{Total}$$

where p is the price charged to "private" (non-Medicare and Medicare non-assigned) patients, Q_{Priv} is the number of "private" (non-Medicare and Medicare non-assigned) patients, f is the Medicare fee, Q_{Total} is the total number of patients, and c is the cost of treating a patient. Note that Q_{Priv} is composed of two distinct groups of patients: non-Medicare patients and Medicare non-assigned patients. When balance billing is incorporated in this model, one of the two groups – the Medicare non-assigned – will be shifted out of Q_{Priv}.

I assume that the cost of seeing patients increases with the number of patients seen, due to actual costs of treatment and the physician's demand for leisure (i.e. $dc/dQ_{Tot}>0$). I also make the assumption that the private price increases with Medicare fee (i.e. $dp/df>0$), which reflects the fact that Medicare non-assigned patients care only about the out-of-pocket costs. If a non-assigned Medicare patient has met his deductible, his net out-of-pocket cost is the standard copayment (20% of the Medicare fee, f) plus the balance bill (p-f). That is, the net price to a non-assigned Medicare patient is $p-(0.8*f)$. Since an increased Medicare fee offsets part of the net out-of-pocket cost, non-assigned Medicare patients are willing to pay higher p to remain at the same level of out-of-pocket cost for any quantity of services. To the extent that the non-assigned market is dominated by Medicare patients, dp/df may be close to 0.8; to the extent that the non-assigned market is dominated by private non-Medicare patients, dp/df will be close to zero. Finally, I assume that the physician faces a downward sloping demand curve for private patients (i.e. $dp/dQ_{Priv}<0$). This assumption reflects the notion that physicians are monopolistic competitors, due to product differentiation.

The physician chooses Q_{Priv} and Q_{Total} to maximize income. The two first-order conditions are:

(2) $$\frac{dI}{dQ_{Priv}} = p + Q_{Priv}\frac{dp}{dQ_{Priv}} - f = 0$$

and:

(3) $$\frac{dI}{dQ_{Total}} = f - c - Q_{Total}\frac{dc}{dQ_{Total}} = 0$$

The first of these conditions, equation 2, indicates that a physician will provide services to private patients until the marginal revenue from an additional private patient $\left(p + Q_{Priv}\frac{dp}{dQ_{Priv}}\right)$ is equal to the marginal revenue from an additional Medicare assigned patient (f). Rewriting equation 2 yields the elasticity of price with respect to private demand:

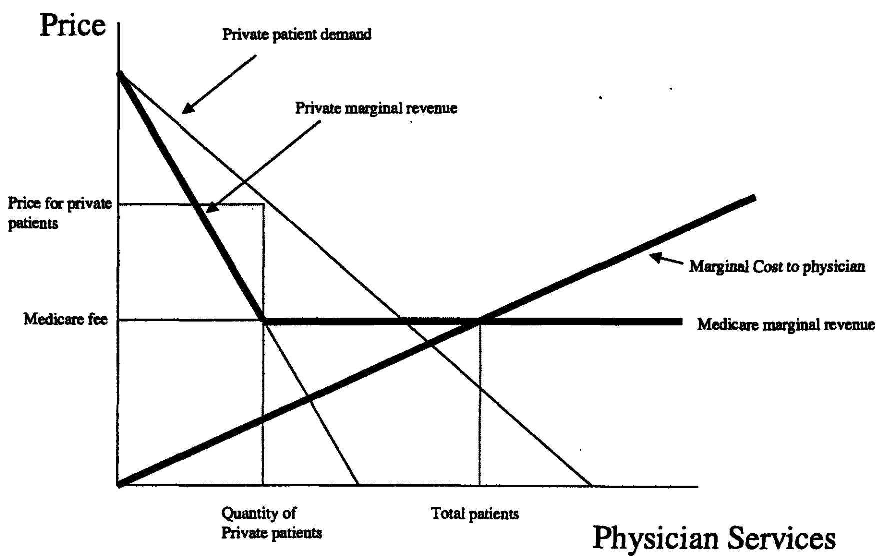
(4) $$\eta_p = \frac{dP}{dQ_{Priv}} \cdot \frac{Q_{Priv}}{P} = \frac{f-p}{p}$$

This equation implies that the elasticity of price with respect to private demand, which is always negative in equilibrium, increases with f and decreases with p.

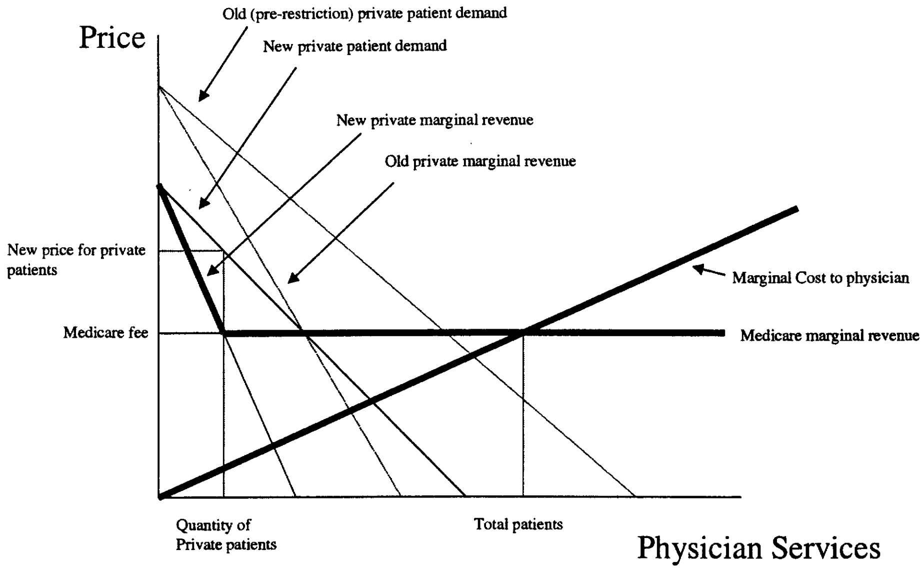
Equation 3 indicates that the physician will provide services to patients until the marginal cost of providing services to an additional patient $\left(c + Q_{Total}\frac{dc}{dQ_{Total}}\right)$ is equal to the marginal revenue from providing services to an additional patient (f). Rewriting this first-order condition, we have the elasticity of cost with respect to Q_{Total}:

(5) $$\eta_c = \frac{dC}{dQ_{Total}} \cdot \frac{Q_{Total}}{C} = \frac{f-c}{c}$$

The following graph, based on earlier work by Mitchell and Cromwell (1982), represents the physician's maximization problem. As above, he stops seeing private patients when the marginal revenue from private patients is equal to the marginal revenue of Medicare assigned patients; thus, Q_{Priv} is established at the point where the two marginal revenue curves cross and the price for private patients is set by the demand curve at that point. The point at which the physician stops seeing Medicare assigned patients is given by the intersection of the marginal cost and marginal Medicare revenue curves. Note that it is possible for the physician's marginal cost curve to be sufficiently high that it intersects the private marginal revenue curve at a price above the Medicare marginal revenue curve. In such a case, the physician chooses to never treat assigned Medicare patients; his only Medicare patients will be those patients who are willing to be balance-billed.



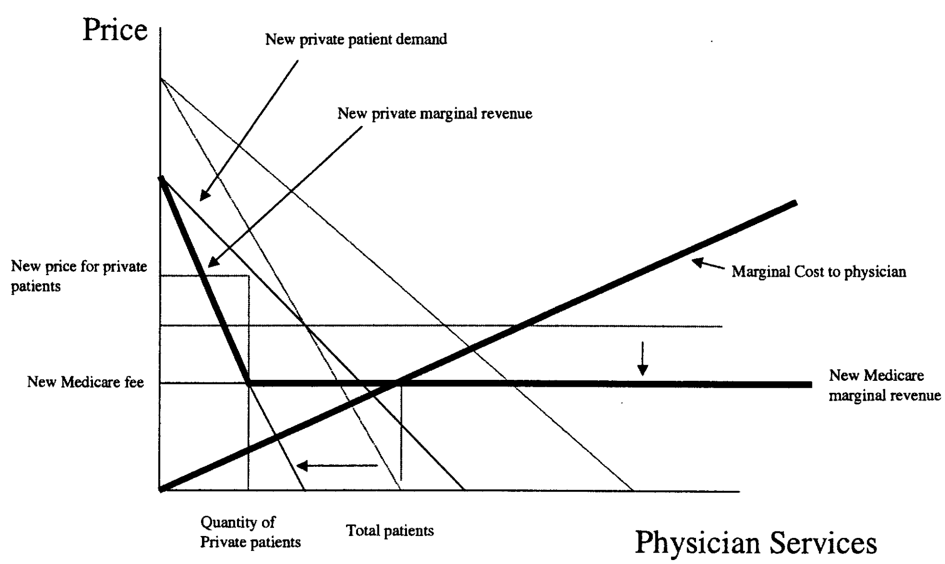
What does this theoretical framework predict about the effect of restricting balance billing? In the extreme case of banning any balance billing of Medicare beneficiaries, the policy can be viewed as restricting the demand for physician services by private patients at any given price level; that is, a ban on balance billing would force the Medicare non-assigned component of Q_{Priv} to join the Medicare assigned patients, thereby decreasing the demand from private patients and increasing the demand from Medicare assigned patients. Assuming that the physician was previously treating some Medicare assigned patients, this change will decrease the number of private patients seen by a physician, without changing the total number of patients seen. In other words, the previously non-assigned Medicare patients will simply become assigned Medicare patients and the overall quantity of care will remain the same.



However, if the physician was not previously seeing Medicare assigned patients (but was seeing Medicare non-assigned patients at private-market prices), he may respond to balance billing restrictions by treating fewer patients in total. Such a physician has a sufficiently steep marginal cost curve that, in the pre-policy period, his marginal cost curve intersected the downward-sloping marginal revenue curve. After the imposition of balance billing restrictions, he – like other physicians – faces inward shifts in the private demand and private marginal revenue curves; unlike other doctors, he determines Q_{Total} by the intersection of the *private* marginal revenue and marginal cost curves and, therefore, may decrease Q_{Total} in response to the restrictions

Implementation of balance billing restrictions in the United States generally occurred in an environment where Medicare fees were falling relative to prices from private payers. Indeed, part of the motivation for restricting balance billing was concern that, as the federal government decreased reimbursement rates to physicians, these decreases would be passed through to beneficiaries in the form of increases in balance billing. According the Physician Payment Review Commission, Medicare fees in 1991 were 65%, on average, of the level that private payers and insurance companies were paying for the same procedures. This was a decline from 71% just two years earlier.

The following figure illustrates the changes that physicians faced during the time that Medicare balance billing restrictions were imposed. In response to the simultaneous decline in Medicare fees and in demand for non-assigned Medicare services, this simple model suggests that physicians would treat fewer "private" patients (and at a lower price) and would treat fewer total patients. Depending on the relative magnitudes of the decline in private demand and the decline in Medicare fees, a physician might increase or decrease the number of assigned Medicare patients that he treats. Thus, the model could provide theoretical support for either the advocates or opponents of balance billing restrictions, depending on the parameters of the model.



One caveat to the preceding model is that it assumes that demand does not constrain the physician's choice of the quantity of services provided. This assumption may be unrealistic, because beneficiaries always face out-of-pocket costs and, therefore, do not have unlimited demand for physician services. If demand were a constraint in the initial pre-policy equilibrium, restrictions on balance billing could cause demand to expand due to the decreased marginal costs of obtaining physician services. As a result, the equilibrium quantity of services provided could increase. This scenario roughly corresponds to the perspective of advocates of the balance billing restriction policies.

The overall insight from the theoretical framework is that the impact of balance billing restrictions is ambiguous. Theoretical work by other authors, discussed in the next section, adds more ambiguities. The goal of this paper, therefore, is to provide some empirical evidence on the direction and magnitude of the effect of balance billing restrictions.

4. Previous Literature

Theoretical

Numerous papers have utilized models that are similar to the income-maximizing model explored in the previous section. For example, Mitchell and Cromwell (1982), Paringer (1980) and Rodgers and Musacchio (1983) use the model to analyze the physician assignment decision. Zuckerman and Holahan (1991) use the model to examine the issue of balance billing graphically and conclude that balance billing restrictions "may in fact reduce the financial burden for many beneficiaries, but that it is also likely to reduce access to some segment of the physician population" (p.143).

Several papers point to ways that the simple income-maximizing framework could be modified. These papers raise significant questions about the appropriate model of physician behavior, but do not provide a clear consensus on the predicted effects of price controls, in general, or balance billing restrictions, in particular. For example, Feldman and Sloan (1988) and Glazer and McGuire (1993) use models that incorporate both quantity and quality of care as choice variables. Wedig, Mitchell, and Cromwell (1989) highlight the potential issue of income targeting by physicians, which could create a scenario where price controls lead to increases in the quantity and quality of services. In addition, Wedig, Mitchell and Cromwell (1989), McGuire (2000), and numerous other authors have debated the possibility of demand inducement by physicians, which could also cause price controls to lead to increases in quantity or quality of care. In short, theoretical work on models of physician behavior has raised important issues that increase the ambiguity of the predictions in the previous section.

In addition to providing theoretical predictions about physician response to fee policy changes, several articles analyze the welfare implications of placing price controls on physician fees. In a simple model of the physician as a monopolist, price controls would be welfare-improving, because they would increase production from the sup-optimally low level that a monopolist produces. However, when quality is included in the model, the effect on production is ambiguous. Feldman and Sloan (1989) conclude that "the only case that can be ruled out is overproduction of both quality and quantity" and argue that "price controls may not contribute to a second-best welfare solution to the monopoly problem" (pages 253 and 258). Wedig, Mitchell and Cromwell (1989) point out an additional flaw in the view that price controls may be welfare-improving; they argue that, due to moral hazard induced by the health insurance market, price controls could still be welfare-enhancing if they lead to decreases in the quantity or quality of medical care. They observe, however, that these potential welfare improvements could be compromised if demand inducement or physician income targeting caused the physician to respond to price controls by increasing quantity or quality of care. These articles are not directly applicable to the case of Medicare balance billing, because they address price controls that affect the entire market, rather than one subset of patients.

Empirical

Associated with the theoretical literature discussed in the previous section, there is an empirical literature on the determinants of physician assignment or participation, mostly utilizing data from the late 1970s and early 1980s. This literature seeks to explain differences in physician willingness to voluntarily accept patients at the Medicare fee, rather than charging the usual market rate to all patients. Overall, these papers reach the sensible conclusions that the level of the Medicare fee, the level of the physician's usual price, the physician's philosophical and political leanings, and the competitive environment are important determinants of voluntary assignment decisions. The Medicare fee is typically a positive, significant predictor of assignment rates, with estimated elasticities ranging from .31 to 5.[10] Likewise, the physician's usual fee (or the difference between the physician's charge and the Medicare fee) tends to have a significant, negative effect on assignment rates.[11] Physicians with "liberal" views are significantly more likely to accept assignment for their Medicare patients.[12] Surgeons tend to be

[10] Mitchell and Cromwell (1982) find an elasticity of 1.5, Mitchell, Rosenbach and Cromwell (1988) find an elasticity of 0.95, Paringer (1980) reports an elasticity of 5 for physicians who do not participate in Medicaid, Rice (1984) reports an elasticity of 0.31 for medical service, and Rodgers and Musacchio (1983) find an elasticity of 0.384.

[11] Mitchell and Cromwell (1982), Paringer (1980), Rodgers and Musacchio (1983). One exception is Mitchell, Rosenbach and Cromwell (1988), which finds no effect of the carrier reduction rate on the participation decision.

[12] Mitchell and Cromwell (1982) find that "liberals" have higher assignment rates and that physicians who disagreed strongly with the statement that "medical care is a right" have lower assignment rates than their peers. Mitchell, Rosenbach and Cromwell (1988) find that the percentage of state residents voting for Walter Mondale in the 1984 election is positively correlated with physician participation rates. They also cite evidence about why physicians signed or didn't sign Medicare participation agreements from an unpublished Rosenbach, Hurdle and Cromwell report: "The single most important reason for signing was altruism, either towards Medicare patients or the Federal

more likely to accept assignment; this finding has been attributed to the fact that surgical procedures tend to be more expensive, leading surgeons in the late 1970s and early 1980s to prefer the lower Medicare fee level to the risk of receiving no payment at all.[13] Finally, authors have found that physicians who face stronger private demand for their services are less likely to accept assignment.[14] These results provide evidence on the likelihood that a physician will voluntarily accept assignment, but they don't provide any evidence about how physicians will react to mandatory assignment or balance billing restrictions.

Empirical evidence on the effects of balance billing restrictions is limited. The GAO completed a study in 1989, based on the initial evidence from restrictions in Massachusetts, Connecticut, Rhode Island and Vermont. Analyzing Medicare claims for these states between 1985 and 1987, the GAO found evidence of a decrease in out-of-pocket spending by the elderly. However, the authors concluded that insufficient time had passed since the policies had been implemented to draw any conclusions about physician behavior. The short length of time between policy implementation and evaluation is a particular concern if we believe that long-run physician responses may be stronger than short-run responses. In this paper, I provide evidence on longer-term responses, using data that extends as far as 10 years beyond the first policy change in Massachusetts.

5. Data

For my empirical analysis of the effects of balance billing restrictions, I turn to several survey data sets. Unfortunately, no single data source provides information on out-of-pocket expenditures, quantity of medical care and quality of medical care during the time period that corresponds to balance billing policy changes. As a result, I use three different data sets, each of which provides evidence on an important outcome that may be affected by balance billing restrictions. In addition, I use aggregate data on the number of physicians of different specialties

Government (reported by one-fourth of participants). Among non-participants, economic reasons dominated, but philosophical opposition was the next most important (reported by one-fifth of nonparticipants)" p.25.

[13] Mitchell and Cromwell (1982), Paringer (1980). Paringer finds that surgeons have a higher voluntary (non-Medicaid) assignment rate, but a lower total assignment rate, perhaps reflecting less willingness among surgeons to treat Medicaid recipients.

[14] Rice (1984) finds that the change in physician density is positively correlated with assignment rates . Mitchell, Rosenbach and Cromwell (1988) report that the elasticity of physician participation with respect to HMO enrollments is 0.14.

who are active in each state and year, in order to assess general equilibrium effects of the restrictions.

The first survey data set that I use is the Consumer Expenditure Survey (CEX), which provides detailed quarterly household expenditure information. I use CEX data from 1984 to 1996, which allows me to analyze the effects of restrictions on out-of-pocket medical expenditures by households with at least one elderly (aged 65 or over) member. Households with heads between the age of 55 and 64, but no elderly members, are included in my data set as a control group. I exclude households that are income-eligible for Medicaid, because there may be differences in Medicaid reimbursement rates across states and over time that could affect my dependent variables. A disadvantage of the CEX is that state identifiers are suppressed for smaller states. As a result, my sample includes only 38 states and the District of Columbia. In particular, two of the states that passed balance billing restrictions in 1987, Rhode Island and Vermont, are not represented in my CEX data set. The final sample includes 33,840 observations on elderly households and 25,104 observations on non-elderly control group households. Categories of expenditures in the CEX are very detailed, so I am able to separately analyze expenditures on physician services, prescription drugs, hospital services and numerous other components of out-of-pocket medical spending. In addition, the CEX provides data on household income, which permits analysis of the differential effects of balance billing restrictions by income level.

The National Health Interview Survey (NHIS) provides annual data about the health care utilization of individuals. I use the 1984-1994 data sets to provide evidence about the effects of balance billing restrictions on health care utilization among Medicare beneficiaries. I use two key variables from this survey as dependent variables in my analysis: the number of doctor visits in the past 12 months and the number of doctor visits in the two weeks before the interview. The NHIS provides additional details about any visits in the previous two weeks, including the type of doctor visited and the setting for the visit (e.g. office, emergency room, etc.). I utilize this additional information in my analysis, to determine whether balance billing restrictions differentially changed access to any particular physician specialty or had an impact on the location of care. I include all individuals over the age of 54 in my sample, except for individuals who are income-eligible for Medicaid. The resulting data set includes 90,598 observations on people aged 65 or over and 85,479 observations on people between the ages of 55 and 64.

Finally, I use the National Ambulatory Medical Care Survey (NAMCS), which provides data on a sample of doctor visits. This data set includes information on the length of the doctor visit and any plans for a follow-up to the visit; I use these variables as proxies for quality of care in my analysis. The NAMCS also includes detailed information on the reason for the doctor visit as well as patient demographics, which are used as control variables in my regressions. Patient income and, more importantly, state identifiers are not currently available for the NAMCS, so I implement a slightly different empirical strategy when I use this data. My analysis includes survey data for the years 1985 and 1989 through 1994, including observations for patients aged 55 and over; no data was collected from 1986 to 1988. Because all of the states that initiated balance billing restrictions before 1991 are located in the northeast and I am unable to identify these states due to the lack of state identifiers, I exclude observations from the northeast of the United States. The resulting data set includes observations on 52,636 visits by patients aged 65 or over and 25,453 visits by patients between the ages of 55 and 64.

6. Empirical Strategy

To identify the effects of balance billing restrictions, I exploit variation in balance billing policy across states, over time, and between patient age and income groups. Control groups for the Medicare beneficiaries who are affected by balance billing restrictions include:

1) Patients of slightly younger ages (age 55-64) who are not yet age-eligible for Medicare and therefore are not affected by balance billing restrictions.
2) Beneficiaries of the same age and in the same state, but in earlier years, who are not yet affected by restrictions.
3) Beneficiaries of the same age and in the same state and year, who are unaffected by restrictions because they do not meet income eligibility requirements.
4) Beneficiaries of the same age and in the same year, but in states that are not yet affected by restrictions.

I use all four controls groups for my analysis in the CEX and the NHIS. (Because state identifiers are not available in the NAMCS, I am only able to use the first two groups in that analysis. I discuss identification in the NAMCS in greater detail below.) The independent variable of interest in the CEX and NHIS regressions is a dummy variable for being in a state and year with a balance billing restriction in place and being in the appropriate age group and income range. I control separately for the direct effects of age, income, state and year, and rely on the interactions for identification. The basic regression takes the following form:

$$Y_{ist} = \alpha + \delta \text{Re}striction_{ist} + X_{ist}\beta + \sum_s \gamma_s State_s + \sum_t \gamma_t Year_t + \varepsilon_{ist} \tag{6}$$

where Y_{ist} measures a dependent variable for individual *i* in state *s* and year *t*. Dependent variables include measures of out-of-pocket expenditures for physician services and quantity of services. $Restriction_{ist}$ is a dummy variable that equals one for any person who lives in a state and year with a balance billing restriction in place and who is income-eligible and age-eligible for those restrictions. $State_s$ and $Year_t$ are fixed state and year effects, respectively. X_{ist} is a vector of covariates, which includes age group, gender, marital status, race, education, and income categories. CEX regressions include additional controls for quarter of interview and size of consumption unit.

Policy Endogeneity Concerns

The possibility of policy endogeneity is a source of concern for this identification strategy. For example, it is possible that balance billing restrictions were first implemented in states that had particular reasons to be concerned about the financial liabilities of their elderly residents or in states where mandatory assignment would not be a binding constraint. This concern is ameliorated by the fact that, although only six states actually passed balance billing restrictions before the federal government did, many other states considered such restrictions, including twelve states that rejected proposals in 1987 alone.[15] Moreover, pre-policy assignment rates in states that passed restrictions varied widely, from 58% in Connecticut to 94% in Massachusetts and Rhode Island. However, the mean assignment rate among states that passed restrictions, at 78%, was higher than the national average of 60%. This fact suggests that balance billing restrictions were less of a constraint in states that first passed restrictions, so that my findings may represent an underestimate of the impact of balance billing restrictions in a typical state.

[15] Proposals failed in Arkansas, California, Florida, Illinois, Iowa, Maryland, Montana, New Hampshire, New Jersey, Ohio, Oregon and Washington during the 1987 legislative session (PPRC 1988).

7. Empirical Results

Consumer Expenditure Survey

Summary statistics for the CEX are provided in Table 1. The first column provides statistics about the treatment sample of elderly Medicare households, the second column provides statistics about the younger sample that is used as a control group, and the third column provides statistics for the pooled sample. For almost all of the categories of medical expenditures, with the exception of prescription drugs and nursing home and ambulance care, the means for the 55-64 year olds are virtually indistinguishable from those of the 65+ households.

The first set of results, shown in Table 2, is from the CEX. These results provide an important test of the validity of my empirical strategy. If my empirical strategy is truly measuring the effects of balance billing, it should show a negative relationship between out-of-pocket spending for physician services and balance billing restrictions. Indeed, I find this empirical relationship in the CEX, with magnitudes that are consistent with aggregate changes in balance billing. Specifically, I find a quarterly decrease in out-of-pocket household expenditures on "physician services" of about $30 (in real 1999 dollars) for the treatment group. This coefficient is consistent with the aggregate data, which suggests that annual per-beneficiary balance billing liability decreased by about $89 between 1985 and 1995 (U.S. Congress 1994). Out-of-pocket expenditures on "total medical expenses," which include physician services and numerous other categories of services, show an effect of similar magnitude. As a share of expenditures for physician services, the impact of balance billing restrictions is a substantial 46%; as a share of total medical expenditures, the restrictions cause a 9% decline. The other categories of expenditures generally show no significant effects.

These findings raise the issue of the distributional consequences of balance billing restrictions. While restrictions may have been enacted to protect the elderly from high out-of-pocket medical expenses, they presumably protected some beneficiaries who have a high income and did not have an obvious need for the protection of balance billing restrictions. Table 3 provides evidence on this issue, from regressions that interact a dummy for having "high" income with $Restriction_{ist}*Post_t$.[16] "High" income is defined as any income over $23,145 (in real 1999 dollars), the median for the elderly households in the data set. A significant, negative

[16] These regressions also control directly for having high income and include interactions between $Restriction_{ist}$ and the high-income dummy as well as interactions between $post_t$ and the high-income dummy.

coefficient on the interaction term would imply that high-income beneficiaries benefited differentially from the restrictions. The results provide no significant evidence that high-income beneficiaries enjoyed relatively larger declines in out-of-pocket expenditures, although the point estimates for the high-income group are negative. The 95% confidence intervals allow for the possibility of differential effects among the relatively high-income households ranging from –$33 to +$14.

National Health Interview Survey

I next turn to the NHIS to analyze effects on the quantity of care provided to elderly beneficiaries. Table 4 shows summary statistics for the various samples in the NHIS. The elderly sample, not surprisingly, reports higher means for virtually all categories of physician services.

Table 5 presents evidence about how balance billing restrictions affected the quantity of physician care received by the elderly. The regression coefficients in the first 2 rows provide no evidence that Medicare beneficiaries received any more or less care as a result of balance billing restrictions. The point estimates for the number of doctor visits are all positive, but statistically insignificant. The confidence intervals for the OLS coefficient on the number of doctor visits in the past 12 months allow for the possibility that balance billing restrictions decreased the number of visits by no more than 11% and increased the number of visits by no more than 16%. In short, the restrictions do not appear to have affected the quantity of care received by Medicare beneficiaries but, if they did affect the quantity, the effect was relatively small.

Table 5 also shows results from separate regressions for the number of visits in the past two weeks by type of physician specialty. Paxton (1987) reported wide variation among specialties in physician dependence on Medicare fore income. He found that Medicare accounted for 24% of the average physician's income, but that this percentage ranged from 2% for pediatricians to 43% for thoracic surgeons. A physician with a high income share from Medicare should react more strongly to balance billing restrictions than a physician with a low income share from Medicare, because the physician with a high income share faces a stronger decline in private demand. To capture differential effects by specialty, Table 5 shows different regressions for the number of visits in the past 2 weeks to six different types of specialists. Medicare income shares for each specialty (from Paxton (1987)) are shown in the last column of the table. The empirical evidence in Table 5 does not suggest that balance billing restrictions affected the quantity of care received, regardless of physician specialty.

In Table 6, I examine the effect on the number of visits in the past two weeks to doctors in various settings. Baker and Royalty (2000) previously found that increases in Medicaid reimbursement did not impact the quantity of care received by recipients, but did shift the site of care from clinics to private offices. Likewise, restrictions on balance billing could affect the location of care, even if it did not affect quantity. The evidence in Table 6 suggests a significant decline in visits at emergency rooms, with offsetting increases in visits at home and in the hospital. One interpretation for these results is that, as a result of balance billing restrictions, Medicare beneficiaries obtain care in a more timely manner and are thereby able to avoid emergency room visits. However, the offsetting increases in visits at the hospital suggest that this shift away from emergency room may reflect changes that are not particularly substantive.

National Ambulatory Medical Care Survey

Table 7 shows summary statistics for the NAMCS. The critical dependent variables in this data set include the duration of the doctor visit, as reported by the physician, and the follow-up plans that were arranged. These variables are proxies for the quality of care received. The statistics are shown separately for the 55-64 year old control group in column 1, for the 65-75 year-old treatment group in column 2, and the 75 and older treatment group in column 3. The 75 and older patients are excluded from some of the regressions in order to make the treatment and control groups more comparable. As the summary statistics show, excluding the oldest age groups creates a sample that appears to be more homogeneous.

The identification strategy for the NAMCS differs from the basic regression framework because state identifiers are not currently available in the NAMCS. In this case, the potential controls groups are limited to:

1) Beneficiaries of the same age, but in earlier years, who are not yet affected by the federal restrictions.
2) Patients of slightly younger ages (55-64) who are not Medicare beneficiaries and are therefore not affected by balance billing restrictions.

The younger age group is generally not a strong control group for older Medicare beneficiaries. It is not necessarily reasonable, for example, to assume that a 55-year-old in Pennsylvania in

1991 would have the same number of doctor's appointments or a doctor's appointment of the same length as a 75-year-old in Pennsylvania in 1991. However, this assumption is more reasonable if the 55-year-old and the 75-year-old were suffering from the same health problem. So, although the NAMCS does not currently allow use of geographic variation in balance billing policies, it does provide fairly detailed information about reasons for physician visits and diagnoses, which makes the use of variation in age more palatable.

The framework for analyzing the effects of balance billing on Medicare beneficiaries in the NAMCS is as follows:

(7) $$Y_{it} = \alpha + \delta Age65_{it} * Post_t + \gamma_a Age65_{it} + X_{it}\beta + \sum_t \gamma_t Year_t + \varepsilon_{it}$$

In this empirical framework, $Age65_{it}$ is a dummy variable for being aged 65 or over. The coefficient of interest is δ, which represents the effect of being older than age 65 in the post-policy period. X_{it} is a vector of covariates, which includes the physician specialty and primary reason for the patient's visit. The identifying assumption is that, conditional on the reason for a visit and other covariates, there are no differential trends in the dependent variable among the two age groups. An alternative regression, which may reduce concerns about differential trends in the age groups, excludes observations over the age of 74 who are most likely to be different from the 55-64 age group.

The results in Table 8 do not show substantial evidence of changes in the quality of health care provision. The only significant results indicate that physicians are less likely to arrange a follow-up telephone call for the older age group after 1991. The coefficient of -.006 represents a 20% decline in follow-up phone calls relative to the mean. This result is present in the full sample as well as the younger, more homogeneous sub-sample. The various types of follow-up plans are not mutually exclusive, so it is not unreasonable that there is no significant offsetting increase in another category of follow-up. This result is suggestive of a small, negative impact on quality of care for Medicare beneficiaries after balance billing restrictions were imposed.

In Table 9, I take advantage of variation in physician specialty. A physician with a high income share from Medicare may react more strongly to balance billing restrictions than a physician with a low income share from Medicare, because the physician with a high income share would face a stronger decline in private demand. In order to capture this effect, I interact the specialty-specific income shares presented in Paxton (1987) with $Age65_{ist}*Post_t$. The coefficients on this interaction terms are always insignificant but the standard errors are so large that it is impossible to rule out the possibility of some effects. The results in Table 9 are, therefore, inconclusive.

Effects on Aggregate Physician Supply

One final and important issue is the general equilibrium effects of balance billing restrictions. The restrictions could have led to a decrease in the supply of physicians through numerous mechanisms, including increases in physician retirement rates, physician migration between states or slowdowns in physician immigration from foreign countries. Opponents of balance billing restrictions suggested that the supply of physicians would, in fact, decline as a result of policies.

Using aggregate data from numerous editions of the AMA's publication, *Physician Characteristics and Distribution*, I consider whether there is an impact of balance billing policies on the number of physicians in a given state. Using data from 1981 to 1993 on the number of doctors of each specialty in each state and year, I test whether the supply of physicians in specialties that are particularly reliant on Medicare income was more likely to decline in states with balance billing restrictions. I interact the Medicare income share of each specialty with $Restriction_{st}$. Specialties observed include: general surgeons, internists, neurosurgeons, obstetrician-gynecologists, ophthalmologists, orthopedists, pediatricians, plastic surgeons, psychiatrists, radiologists and thoracic surgeons. Of these specialties, pediatricians were least reliant on Medicare income, with an income share of only 2%, whereas thoracic surgeons were most reliant on Medicare income, with an income share of 43%.

The results, shown in Table 10, provide no evidence of a decline in the number of log doctors in Medicare-reliant specialties relative to less Medicare-reliant specialties in states with balance billing restrictions. The first two columns restrict the effect of balance billing restrictions to be the same in every post-policy years, while the second two columns allow for a gradual effect. Regardless of the specification, there are no significant effects. However, the standard errors are again sufficiently large that it is impossible to rule out sizeable effects.

8. Conclusion

The empirical results in this paper do not provide any significant evidence that physicians changed their behavior in response to the balance billing restrictions that were imposed in the late 1980s and early 1990s. At most, there is evidence that physicians reduced telephone follow-up calls in response to restrictions. This finding does not provide strong support for the views of either the advocates or the opponents of balance billing restrictions.

The empirical results do, however, suggest a decline in out-of-pocket spending of roughly \$120 per elderly household per year or a 9% decline in overall medical spending. This decline in spending amounts to a transfer from physicians to Medicare patients, which raises issues about equity implications. The decline in spending appears to have been roughly uniform among high- and low-income beneficiaries, suggesting that physicians were obligated to subsidize health care for both low-income and high-income beneficiaries as a result of the balance billing restrictions. Whether such redistribution is an optimal government policy is unclear.

The findings of this paper have implications for the recent debates about "concierge physicians" who do not accept insurance reimbursement as payment in full for their services. One of the frequent concerns that is raised about concierge medical plans is the possibility that patients who cannot afford to pay the extra fees will lose access to medical care. The empirical results in this paper, however, provide no evidence that payers who are unable to pay extra fees or balance bills suffer any substantial decline in access to care.

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Table 1: CEX Summary Statistics

Variable	Mean (Standard Deviation) Aged 65+	Mean (Standard Deviation) Aged 55-64	Mean (Standard Deviation) All age groups
Physician services expenditures	65.90 (284.68)	64.40 (386.85)	65.22 (332.06)
Prescription drugs expenditures	113.83 (206.73)	63.12 (145.40)	92.23 (184.84)
Hospital services expenditures	29.06 (558.08)	34.79 (687.41)	31.50 (616.48)
Eye exams, treatment and surgery	13.62 (105.84)	10.95 (110.44)	12.48 (107.83)
Medical supplies	20.11 (81.93)	20.08 (82.15)	20.10 (82.02)
Dental services	68.46 (284.70)	68.35 (283.46)	68.41 (284.17)
Labs, tests, x-rays	10.16 (81.59)	12.18 (87.45)	11.02 (84.14)
Care in nursing homes, ambulances, etc	28.74 (472.17)	6.08 (119.16)	19.19 (366.29)
Other medical services expenditures	23.65 (395.97)	13.19 (158.32)	339.25 (1026.36)
Total medical services expenditures	373.53 (1058.24)	293.05 (979.87)	339.25 (1026.36)
Restrict	.449 (.497)	.433 (.495)	.442 (.497)
Male	.581 (.493)	.688 (.463)	.626 (.484)
Married	.516 (.500)	.625 (.484)	.562 (.496)
Age	71.53 (9.68)	59.39 (2.88)	66.36 (9.67)
Real household income	32,619 (29,890)	39,498 (39,881)	35,506 (34,605)
Household size	1.98 (1.19)	2.32 (1.34)	2.13 (1.27)
Observations	33,840	25,104	58,944

Table 2: Medical Expenditures

Dependent variable (in 1999 $)	(1) OLS	(2) Probit Any Exp	(3) OLS Log exp.	(3) Median
Physician services	-31.70**	-.043	-.161**	
	(9.25)	(.030)	(.063)	
Prescription drugs	-2.48	-.020	.088	
	(10.64)	(.042)	(.106)	
Hospital	-18.82	.020	1.38**	
	(18.18)	(.019)	(.533)	
Eye exams and treatment	-13.14	-.009	-.151	
	(13.14)	(.017)	(.199)	
Medical supplies	-.622	-.025*	-.033	
	(3.20)	(.014)	(.207)	
Dental services	-20.46	-.018	-.028	
	(14.24)	(.024)	(.174)	
Labs, tests, x-rays	-2.10	.003	-.299	
	(3.96)	(.017)	(.234)	
Care in nursing homes, ambulances, etc	-45.49	-.019**	.588	
	(36.92)	(.005)	(.662)	
Other expenses	-6.20	.012	-.023	
	(10.89)	(.018)	(.268)	
Total medical expenses	-141.01*	-.006	-.171*	-35.76**
	(76.48)	(.024)	(.097)	(16.71)
Number of observations	58,944	58.944	Varies	58,944

Note. Standard errors in parentheses; they are clustered on state and year. Each cell contains the coefficient from a different regression. Controls include state, year, state trends, quarter of interview, real income, household size and demographic characteristics (gender, marital status, gender*marital status, age, race and education) of the household head. Regressions also allow for interactions between each of the control variables and a dummy for having a household member aged 65 or over. Households that are income-eligible for Medicaid are excluded from the sample.

Table 3: Medical Expenditures, by income category

Dependent variable (in 1999 $)	(1) OLS Physician Expenditures	(2) OLS Total Medical Expenditures
Restrict	-26.67**	-128.19
	(10.52)	(78.13)
Restrict*High Income	-9.69	-23.49
	(11.81)	(37.21)
Number of observations	58,944	58,944

Note. Standard errors in parentheses; they are clustered on state and year. Each column contains the coefficients from a different regression. Controls include state, year, state trends, quarter of interview, real income, household size and demographic characteristics (gender, marital status, gender*marital status, age, race and education) of the household head. Regressions also allow for interactions between each of the control variables and a dummy for having a household member aged 65 or over. Households that are income-eligible for Medicaid are excluded from the sample.

Table 4: NHIS Summary Statistics

Variable	Mean (Standard Deviation) Aged 65+	Mean (Standard Deviation) Aged 55-64
Doctor visits in past 12 months	6.06 (14.88)	5.08 (13.70)
Doctor visits in past 2 weeks	.368 (.971)	.285 (.854)
Doctor visits in office setting	.211 (.534)	.158 (.492)
Doctor visits in outpatient setting	.029 (.293)	.031 (.320)
Doctor visits in own home	.036 (.527)	.008 (.276)
Doctor visits in hospital	.010 (.123)	.009 (.134)
Doctor visits in ER	.005 (.079)	.006 (.185)
Doctor visits in clinic	.003 (.069)	.004 (.094)
Doctor visits via telephone	.037 (.247)	.034 (.240)
Restrict	.413 (.492)	.390 (.488)
Male	.431 (.495)	.469 (.499)
Married	.590 (.492)	.756 (.429)
Age	73.85 (6.25)	59.44 (2.85)
High school dropout	.413 (.492)	.301 (.459)
High school graduate	.335 (.472)	.392 (.488)
Some college	.128 (.334)	.144 (.351)
Observations	90,598	85,479

Table 5: Quantity of Medical Care

Dependent variable	(1) OLS Number of visits	(2) Probit Dummy for any visits	(3) OLS, conditional on any visits	(4) Robust	(5) Median	(6) Medicare Income Share
Doctor visits in past 12 months	.688 (.589)	.008 (.010)	.781 (.697)	.120 (.109)	.155 (.117)	
Doctor visits in past 2 weeks	.039 (.030)	.020* (.012)	.042 (.086)			
Ophthalmologist visits	-.006 (.004)	.003 (.003)	-.104 (.217)			42%
Internal medicine visits	-.008 (.009)	-.004 (.007)	-.057 (.111)			37%
Radiology visits	.015 (.009)	.005 (.003)	.605 (1.82)			29%
Orthopedic surgery visits	.003 (.007)	.0003 (.0030)	.187 (.541)			23%
General practice visits	.007 (.015)	.007 (.015)	.026 (.108)			22%
Psychiatry visits	-.003 (.003)	-.003 (.002)	.597 (.580)			8%
Number of observations	176,077	176,077	Varies	176,077	176,077	

Note. Standard errors in parenthesis; they are clustered on state and year. Each cell contains the coefficient from a different regression. Controls include state, year, real income, gender, marital status, gender*marital status, age, race and education. Regressions also allow for interactions between each of the control variables and a dummy for being aged 65 or over. Households that are income-eligible for Medicaid are excluded from the sample.

Table 6: Location of Medical Care

Dependent variable	(1) OLS Number of visits	(2) Probit Dummy for any visits	(3) OLS, conditional on any visits in past two weeks
Doctor visits in office setting	-.001 (.017)	.0003 (.012)	-.099* (.054)
Doctor visits in outpatient	.011 (.008)	.005 (.005)	.043 (.044)
Doctor visits in own home	.019* (.012)	.006** (.003)	.105** (.049)
Doctor visits in hospital	.007** (.003)	.004* (.002)	.030 (.018)
Doctor visits in ER	-.006** (.003)	-.004** (.002)	-.032** (.015)
Doctor visits in clinic	-.003 (.003)	.00004 (.0013)	-.018 (.018)
Doctor visits via telephone	.003 (.008)	.004 (.005)	-.012 (.042)
Number of observations	176,077	176,077	36,886

Note. Standard errors in parenthesis; they are clustered on state and year. Each cell contains the coefficient from a different regression. Controls include state, year, real income, gender, marital status, gender*marital status, age, race and education. Regressions also allow for interactions between each of the control variables and a dummy for being aged 65 or over. Households that are income-eligible for Medicaid are excluded from the sample.

Table 7: NAMCS Summary Statistics

Variable	Mean (Standard Deviation) Aged 55-64	Mean (Standard Deviation) Age 65-75	Mean (Standard Deviation) Age 75+
Duration of visit	18.82 (14.16)	18.51 (13.68)	18.19 (13.33)
No follow-up planned	0.066 (.249)	.052 (.221)	.046 (.210)
Return at specified time	.678 (.467)	.720 (.449)	.739 (.439)
Return if needed	.185 (.389)	.158 (.365)	.150 (.357)
Telephone follow-up planned	.032 (.175)	.032 (.176)	.033 (.177)
Refer to other physician	.034 (.180)	.031 (.174)	.031 (.174)
Return to referring physician	.021 (.143)	.021 (.145)	.019 (.135)
Admit to hospital	.015 (.123)	.017 (.128)	.017 (.128)
Restrict	0 (0)	.483 (.500)	.501 (.500)
Medical specialty	.511 (.500)	.465 (.499)	.447 (.497)
Male	.446 (.497)	.449 (.497)	.407 (.491)
Age	59.63 (2.89)	69.87 (3.12)	81.16 (4.35)
Observations	25,453	32,296	20,340

Table 8: Quality of Medical Care

Dependent variable	OLS, controlling for visit reason	OLS, controlling for visit reason and including ages 55-75	Robust, controlling for visit reason and including ages 55-75
Duration of visit (in minutes)	.003 (.198)	-.075 (.222)	.057 (.138)
No follow-up planned	.003 (.004)	.003 (.004)	
Return at specified time	-.001 (.007)	-.003 (.008)	
Return if needed	.0003 (.006)	.003 (.006)	
Telephone follow-up planned	-.006** (.003)	-.007** (.003)	
Refer to other physician	.003 (.003)	.004 (.003)	
Return to referring physician	-.001 (.002)	-.002 (.002)	
Admit to hospital	-.002 (.002)	-.002 (.002)	
Number of observations	78,089	55,071	55,071

Note. Standard errors in parenthesis; they are clustered on physician specialty. Each cell contains the coefficient from a different regression. Controls include age, sex, race, region, year, physician specialty, primary reason for visit, and interactions between over age 65, post-policy and the Medicare income share. Patients with Medicaid as an expected source of payment are not included in the sample.

Table 9: Quality of Medical Care

Shows coefficients on interaction with Medicare income share

Dependent variable	OLS, controlling for visit reason	OLS, controlling for visit reason and including ages 55-75	Robust, controlling for visit reason and including ages 55-75
Duration of visit (in minutes)	.819 (2.20)	.578 (2.66)	.467 (1.702)
No follow-up planned	-.004 (.058)	-.016 (.065)	
Return at specified time	.120 (.106)	.140 (.110)	
Return if needed	-.119 (.071)	-.092 (.079)	
Telephone follow-up planned	-.015 (.037)	-.006 (.028)	
Refer to other physician	-.052 (.034)	-.054 (.034)	
Return to referring physician	.0007 (.025)	.010 (.026)	
Admit to hospital	-.015 (.024)	-.024 (.021)	
Number of observations	48,953	34,331	34,331

Note. Standard errors in parenthesis; they are clustered on physician specialty. Each cell contains the coefficient from a different regression. Controls include age, sex, race, region, year, physician specialty, primary reason for visit, and interactions between over age 65, post-policy and the Medicare income share. Patients with Medicaid as an expected source of payment are not included in the sample.

Table 10: Physician Supply, by Specialty

Independent variable, interacted w/ Medicare income share	(1) OLS Log doctors	(2) Median, Log doctors	(3) OLS Log doctors	(4) Median Log doctors
Restrict	.095	.092		
	(.065)	(.080)		
Restrict, t=0			.038	.088
			(.056)	(.078)
Restrict, t=1			.066	.125
			(.075)	(.080)
Restrict, t>=2			.049	.095
			(.112)	(.080)
Observations	6219	6219	6219	6219

Note. Controls for physician specialty, physician specialty trends, state, state trends and year.

Chapter 3: Why Did Employee Health Insurance Contributions Rise?[17]

The dominant feature of the health insurance market in the U.S. is the provision of private health insurance through the workplace. But the past two decades have been a period of substantial reduction in both the scope and generosity of employer-provided health insurance. In 1982, roughly 80% of workers were covered by employer-provided health insurance. By 1998, this had fallen to 73%. Similarly, in 1982, 44% of those who were covered by their employer-provided health insurance had insurance that was fully financed by their employer. But by 1998, this had fallen to 28%.[18]

There has been a voluminous literature in recent years on the causes and consequences of the decline in employer-provided health insurance coverage. But there has been virtually no work on the parallel time trend of declining employer payments for health insurance. This is a particularly glaring omission in light of recent evidence which shows that most of the time trend in private insurance coverage appears to be reductions not in employer offering of insurance, but in employee takeup of insurance conditional on offering (Cooper and Schone, 1997; Farber and Levy, 2000). Thus, the key dimension along which employers appear to be adjusting their health insurance spending is through the generosity of what they contribute. Moreover, this raises the possibility that it is reductions in employer generosity that are responsible for declining insurance coverage.

In this paper, we attempt to model the set of factors that may be driving employers to shift their health insurance costs to their employees. We begin by discussing the theory of why employers might shift premiums to their employees. There are two classes of explanations. The first is that employers are shifting premiums in order to induce employees to choose the cost effective option from the range of insurance choices offered by the employer. The second is that premium sharing results from imperfect worker sorting across firms; with heterogeneity in tastes among co-workers, premium contributions become a useful tool for separating worker types. By requiring contributions, the firm can provide insurance only to those who demand it, and can pass the savings back to employees in the form of higher wages.

[17] This chapter is joint work with Jonathan Gruber.
[18] Source for all figures is author's tabulations of March Current Population Survey data.

We then turn to estimating the role of a number of factors which fit into these categories of explanations, most of which also have the attractive feature that they operated most strongly in the late 1980s and early 1990s, the period over which the shift from employer to employee financing was most pronounced. Our summary for the first explanation is the rise in managed care penetration. A key determinant of employer premium sharing should be the range of choices offered by the employer; as there are more managed care options available in a state, then there will be more incentive to make employees bear insurance costs on the margin in order to motivate them to choose the low cost plan.

The second and third factors are the rise in spousal labor supply and the expansion of eligibility for the public Medicaid insurance program for women and children. A key prediction of the imperfect sorting model is that, as there are more outside insurance options available to workers, firms should increase employee contributions to insurance. Both of these factors represent a rise in such outside options: more spousal labor supply means more opportunity for spouses to be covered by insurance; and more Medicaid entitlement means more chance for coverage by public insurance.

The fourth factor is health insurance costs. In the presence of workplace heterogeneity and imperfect individual-specific wage shifting, rising medical costs will increase the pressure on firms to shift the costs of insurance to their workers. Likewise, the reductions in marginal tax rates through the tax reforms of the 1980s could be playing an important causal role. A central feature of employer-provided health insurance in the U.S. is its subsidization through the tax code. If employees are paid in wages, they must pay taxes on those wages; but, if paid in health insurance, it is tax free. Since employee contributions for health insurance are usually made in a post-tax form, higher tax rates would lead to a stronger incentive for employers to finance these costs rather than shifting premiums to employees. Thus, tax subsidies to insurance are the fifth factor.

The final factor we consider is cyclical conditions. The most dramatic rise in employee premium sharing was at the end of the 1980s and beginning of the 1990s, during which the economy went through a significant downturn. A variety of models, such as recruitment models with imperfect worker understanding of wage shifting, or rent-sharing between workers and firms that is partially through insurance premium sharing, would suggest that when the economy performs worse, there is more premium shifting to employees.

We investigate the role of these six factors using the only nationally representative annual data on premium sharing that covers this period of rapidly rising employee premium contributions: the Current Population Survey (CPS). These data provide only a crude measure of premium sharing, based on a question of covered employees as to whether their employers pay all, some, or none of premiums. Compared to more comprehensive sources available for particular years, however, these crude data capture both variation across jobs/places and over time in the propensity to share costs between employers and employees. Moreover, this disadvantage is counterbalanced by the significant advantage that we can match to these data job and locational variation in our measures of interest. Based on these matches, we can investigate the role of these factors in driving the rise in employee premium sharing.

Our results suggest that this set of factors are all related to employer contribution decisions, but the results for some of the factors (spousal labor supply, taxes, Medicaid and unemployment rates) are much more robust than others (managed care penetration and medical costs). Interestingly, many of these factors changed significantly in the late 1980s and early 1990s in a manner that is consistent with rising employee contributions. We find that the time trend in these influences corresponds quite strikingly to that of employee contributions, but that overall these factors can only explain about a quarter of the rise in employee contributions over the entire 1982-1996 period.

Our paper proceeds as follows. We begin, in Part I, by providing background on employer and employee contributions for health insurance. We also discuss heuristically the theoretical issues involved in thinking about the tradeoff between employer and employee-financed insurance payments. Part II lays out our data and empirical framework for testing hypotheses about this shift. Part III presents our results, and assesses the extent to which the factors we investigate can explain this time series trend. Part IV concludes.

Part I: Background

Time Series Trends

Group health insurance provided through the workplace has been the dominant source of private health insurance coverage in the U.S. at least since an IRS ruling in the 1940s that health insurance costs were deductible from employer costs, but were not taxable income to employees. In 1998, over 90% of the privately insured received their coverage through employers.

But, as noted in the introduction, employer provided coverage has been declining precipitously over the past two decades. Figure 1 graphs the share of workers who have group coverage over time.[19] Coverage was flat until 1982, then slowly declined until 1988, when the decline was rapid before stabilizing after 1992. Over the entire period, group coverage declined by 7 percentage points, or almost 10%. This significant decline is the primary driver behind the sizeable rise in the share of the non-elderly without any health coverage, although this rise in uninsurance is smaller than the decline in private coverage due to the mitigating role of Medicaid.

Several recent studies have attempted to decompose this decline in employer coverage. Cooper and Schone (1997) find that the decline over the 1987-1996 period is completely driven by reduced employee takeup of employer-provided coverage; they estimate that firm offering of insurance actually rose over this period. Farber and Levy (2000) also estimate that offering has risen between 1988 and 1997, and that the decline in coverage can be attributed to both reduced insurance takeup and reduced eligibility for insurance among those offered.

Why has insurance takeup declined over time? One reason may be the significant increase in required employee contributions towards employer-provided health insurance. Figure 2 superimposes the decline in the share of employers paying all of the cost of employer-provided coverage, from the CPS, on the decline in employer coverage over this period. The series are normalized to fit on the same scale, and they show remarkable consistency over time, with both series flat until the early 1980s, slowly declining until 1987, rapidly declining over the next five years, and then flattening out again. The correlation between the series is 0.98, and the residual correlation after partialing a time trend out of both series is 0.66. A time series regression of group coverage against the share of employers paying all of the costs of health insurance yields a coefficient of 0.44 (0.02), and the relationship is significant even when controlling for time trend, with a coefficient of 0.36 (0.10).

Micro-data evidence on the impact of employer contribution policy on employee insurance decisions has not, to date, yielded evidence consistent with this time series correlation. Chernew, Frick and McLaughlin (1997) and Blumberg and Nichols (2001) both model employee

[19]There was a major redesign of the health insurance questions in the March 1995 CPS that results in a significant upward jump in the share of the population with group coverage. We have assumed that coverage was flat from 1993 to 1994 and used the ratio of these years to reverse benchmark the earlier figures. Our regression models below will all contain year dummies to capture such shifts in survey methodology.

takeup as a function of employer premium sharing. Both papers find that there is little impact of employee premiums on insurance takeup decisions, with the highest estimated elasticities in the range of -0.1. This evidence is not fully convincing, as employer contribution policies may themselves be endogenous to tastes for employee insurance takeup. The bias from this potential endogeneity is not obvious ex ante. If employees who are likely to take up select firms with low employee premiums, this would lead to an upward bias to the estimated elasticity of takeup. But if employers cover a larger share of health insurance premiums when employees don't have tastes for insurance coverage (either through paternalism or to meet insurer requirements on employee takeup), then this could lead to a downward bias. Regardless, this striking time series correspondence is highly suggestive and highlights the value of understanding what drove the trend towards employees paying more for their health insurance.

Analytical Framework

In this section, we lay out an analytical framework for thinking about the determinants of employee premium sharing. We do not propose a new model here, but rather summarize and extend some of the insights of Dranove, Spier and Baker (2000) and Levy (1998).

As noted by Pauly (1986), the presence of <u>any</u> employee contributions suggests imperfect worker sorting across firms, because in general employer contributions for health insurance are excluded from taxation while employee contributions are not. In a growing share of firms with IRS Section 125 plans, employee contributions can also be excluded from taxation, but such protection of employee contributions is far from complete. The data on the prevalence of such arrangements is sketchy. The most recent available data, from a survey of employers by the Kaiser Family Foundation, suggests that half of all workers are in firms that offered such flexible benefit plans. These data also suggest that in the last year of our sample, 1996, the figure was higher (65%) (Kaiser Family Foundation, 2000). At the same time, earlier surveys by the Bureau of Labor Statistics suggest a much lower prevalence. Data on large firms from the Bureau of Labor Statistic's Employee Benefit Survey (EBS) show that in 1993 only 32% of workers in large firms had tax free employee contributions, and in 1992 only 20% of workers in small firms had such arrangements.

In principle, every firm should also set up a Section 125 plan to further maximize the size of the pie by making employee contributions pre-tax as well. The reason for less than full coverage of this generous tax benefit in practice is unclear, but some of it may have to do with extensive IRS regulation of these arrangements to ensure that they are not abused. For example, the regulations state that no more than 25% of the benefits of a plan can be attributed to any "highly compensated" employee, essentially ruling out the availability of section 125 plans for very small firms. Moreover, there are strict and complicated rules that limit the flexibility of employees to switch sources of insurance coverage during the year if they are paying their health insurance contributions on a pre-tax basis.

Levy (1997) highlights two possible explanations for the existence of employee contributions. The first is the "fixed subsidy" model, whereby employers with multiple insurance plans ask their employees to contribute funds towards insurance in order to incentivize employees to choose the lowest cost insurance plan. If this were the only motivation for employee contributions, employers would contribute the amount of the minimum cost plan, and employers with only one plan would never have employee contributions. In fact, as Levy (1997) points out, the second of these conditions does not hold in practice: more than half of firms with only one plan require an employee contribution. Overall, she finds that only about one-sixth of employee contributions are paid by workers who have the option of a cheaper plan with no contribution required.

Alternative explanations for employee contributions rely on imperfect worker sorting across firms, and Levy (1997) and Dranove, Spier, and Baker (2000) present two different models of this imperfect sorting. The key notion behind these models is that there is not perfect worker-by-worker shifting of insurance costs to wages, so that with heterogeneity in tastes premium contributions become a useful tool for separating worker types. By requiring contributions, the firm can provide insurance only to those who demand it, and can pass the savings back to employees in the form of higher wages.

Models such as these have a number of interesting predictions; we follow here Dranove, Spier and Baker's discussion of comparative statics. First, in the absence of taxation, there should be 100% employee contributions for insurance, to maximize the ability to separate those who want and do not want insurance. As the tax rate rises, employee contributions fall, due to the tax subsidy to employer spending only. Second, as the premium rises, employee contributions rise, as the value of sorting to the firm is increasing. Third, as outside insurance options increase, employee contributions rise, since there is more possibility of shifting employees to other sources of coverage, raising the wages that can be paid to employees.

These hypotheses have been the subject of some limited testing in these previous articles. Levy (1997) shows that contributions fall with a proxy for insurance demand, worker age, and that firms where workers have higher tax rates are less likely to require a contribution. Dranove, Spier and Baker (2000) show that contributions are larger at smaller firms, are higher for firms with more female workers, are lower at firms with more older male workers, and are higher at firms with more part-time workers (a proxy for higher premium costs).

One difficulty with previous tests, however, is distinguishing the worker sorting story from a simple alternative model that high quality jobs provide higher compensation along many dimensions, including lower employee contributions. Firms with more older workers, fewer female workers, higher employee wages and thus tax rates, fewer part-time workers, and more total employees are all the type of high quality jobs that are likely to compensate their workers highly. Given imperfect controls in these models for job quality, this could easily explain the finding that such jobs require smaller employee contributions. In the empirical work presented below, we will endeavor through instrumental variables strategies to avoid such problems of interpretation in our measures of determinants of employee contributions.

Part II: Data and Empirical Framework

Data

Our primary data for this analysis is the CPS data on premium sharing used in Table 2. As noted above, the CPS only provides information on whether the employer pays some, all, or none of the premium. An additional limitation is that this information is only provided conditional on being covered by insurance, and only for the policy through which one is covered. We cannot condition on having insurance in our regression analysis of premium sharing, since the factors that we examine may (and in fact, in some cases, do) have effects on the coverage decision itself. Therefore, our dependent variables will be unconditional, measuring (for example) the share of all workers for whom the employer pays all the costs of health insurance. This variable may change for four reasons. First, employers may shift the financing of their health insurance plans. Second, changes in employer offering may be differentially concentrated in high or low employee contribution firms. Third, changes in employee takeup may be concentrated in differentially high or low employee contribution firms. Finally, since this measure refers to the plan held by the employee, employees may be moving across plans of different contribution levels. We will address these issues in the interpretation of our results below.

These limitations raise fundamental issues of the applicability of our CPS results, however: do shifts that we observe in the share of employers paying different amounts for insurance in the CPS accurately capture shifts in employer-financing more generally? To address this concern, we have compared the CPS data with two other sources which have more complete information on premium sharing. The first is the Bureau of Labor Statistics Employee Benefits Survey (EBS). The EBS surveys were sporadically carried out since the early 1980s, alternating in recent years between small private firms, medium/large private firms, and government workplaces. They also provide data on the share of employees required to pay some of the cost of their insurance; workers required to pay all of the costs are not counted as insured for their purposes and so not included in the survey. The EBS unfortunately only provide time series data and no micro-data or cross-tabulations; we use the summary of their time series data from EBRI (2000).

Table 1 provides a comparison through time of our CPS and EBS results. We focus on the EBS results for medium and large firms, since this is the only long time series available. Since the CPS only has data on firm size beginning with the 1988 survey (data for 1987), we compare the EBS time series both to the overall CPS patterns and the patterns over all years, and the patterns for medium and large CPS firms for 1987 onwards.

There is a rough time series correspondence between these two sources of data. Both sources show small changes in the early 1980s. The EBS shows a much larger rise from 1985 to 1988 than does the CPS. Then, from 1988 to1993, both sources show a large rise, although it is larger in the EBS than in the CPS. The series for family premium sharing is then fairly flat in both data sources. For singles, the EBS shows a much larger rise since 1993 than does the CPS. Overall, the time series correspondence seems reasonable, particularly for family policies.

The second source is data on the share of costs of insurance for family and single plans that are borne by firms, from unpublished data tabulations purchased from the benefits consulting firm KPMG. These data have the advantage that they represent a more complete measure of premium sharing, the actual percentage of costs borne by the firm. But we were only able to obtain cross-tabulations of these data, by region, industry, and firm size, and only for years from 1991 to the present.

Therefore, to compare these data, we have collapsed our CPS data into comparable year, region, industry, and firm size cells, and examined the correlation between our CPS measure of percent of firms paying all of premiums and the KPMG measures of percent of costs borne by firms. We find a correlation for family premium sharing of 0.33, and for individual premium sharing of 0.23. Figure 3 illustrates this correlation for family premium sharing; there is a strong positive correlation with only two notable outliers. The correlations suggest that the CPS data contain real information about the degree of premium sharing.

Our CPS sample for this analysis consists of all adult workers (age 21-64). We exclude the self-employed and the federal government employees. We use CPS data from March 1983 (referring to calendar year 1982) through March 1997 (referring to calendar year 1996).[20] We will focus on several dependent variables related to firms' health insurance provision. The first three are whether the firm pays some, all, or none of the costs of health insurance. As noted above, this is measured by a dummy which is equal to one if the employer pays all/some/none, and zero otherwise, not conditional on whether the individual has insurance. To interpret these findings it is also important to measure what is happening to overall insurance coverage. We therefore also examine the impact of these factors on whether the worker has insurance on their job at all.

Measurement of Key Independent Variables

As noted earlier, we consider the role of six key potential explanations for the time series trend in employer contributions. For all concepts, we would ideally measure their impact on insurance decisions at the level of the firm. But the CPS does not provide any detail on an individual's firm composition, other than their industry, location, and (from 1988 onwards) firm size.

We considered two proxies for firm-based measures of our key incentive variables. The first was to aggregate the CPS data by various combinations of state, industry, and year in order to form "synthetic firms". The alternative is to simply use the CPS respondent's information to

[20]The CPS data on premium sharing in the March 1995 survey, for calendar year 1994, are not useful for our purposes since they lump together firms paying all and some of the costs of insurance.

form the measures, as a proxy for the characteristics of their firm. As part of earlier work (Gruber and Lettau, 2000), one of us has investigated both of these options using internal Bureau of Labor Statistics data, the Employment Compensation Index (ECI) data, which gathers information on both firm characteristics and the wages of workers in the firm. The data show that, for predicting the average wage of a firm, the individual worker's wage has much more predictive power than does an average wage formed by aggregating like firms into synthetic firms. We therefore create our measures at the level of the worker, as a proxy for that worker's firm characteristics.

As noted above, the "fixed subsidy" model of employee premium sharing suggests that such premium sharing arises as a mechanism to ensure efficient worker choice of health plan. This model suggests that, as new lower cost insurance alternatives become available to workers, firms should be more likely to pass premium costs to employees in order to cause them to choose these lower cost alternatives. Of course, we do not know about the insurance choices available to each of the workers in the CPS. But we can proxy for the availability of these new lower cost alternatives that might induce premium sharing by the managed care penetration rate in the worker's state. This is defined as the share of privately insured persons enrolled in HMO plans in the state, and the data come from Laurence Baker, who has compiled them for his work on HMO penetration. This is of course a somewhat crude proxy, but it should capture the introduction of low cost options that would cause employers to want to induce price sensitivity in plan choice among their employees.

In terms of the imperfect worker sorting model, we test four predictions. The first is that premium sharing should rise with the outside insurance options available to workers. We use two proxies for outside insurance options. Our first is spousal labor force participation. Our regressor here is a dummy variable for whether the worker has a spouse who works at least 17 hours per week. We explored alternative measures that tried to use information on the quality of the spouse's job, and the results were quite similar to those reported here.

The second measure of outside options is entitlement to Medicaid. Here, we use the simulation program developed for earlier work by one of us, and described in more detail in Currie and Gruber (1996a,b), Cutler and Gruber (1996), and Gruber (2000). This program uses information on women and children in the CPS to compute their eligibility for Medicaid coverage given state eligibility rules. We then, following Cutler and Gruber (1996) use the computed eligibility for all women and children to calculate the percentage of each family's medical spending that is eligible for Medicaid, which we call MES (Medicaid eligible share). This is calculated according to:

(2) $MES = (\Sigma_k SPEND_k * ELIG_k * NUM_k) / (\Sigma_k SPEND_k * NUM_k)$

where k indexes single year age groups of children, and broader age groups for adults.[21] $SPEND_k$ is the expected health spending in a year for that age group based on data from the 1987 National Medical Expenditure Survey (NMES); the appendix to Cutler and Gruber (1996) presents these figures.

The second prediction of the imperfect sorting model is that premium sharing should fall with the relative subsidy to employer spending on insurance. We test this hypothesis by computing the tax price of insurance for workers, which measures the tax subsidy to insurance purchase through the firm. This is computed as:

(1) $$TP = \frac{(1 - \tau_f - \tau_s - \tau_{ss} - \tau_{mc})}{(1 + \tau_{ss} + \tau_{mc})}$$

where τ_f is the federal income tax marginal rate; τ_s is the state income tax marginal rate; τ_{ss} is the marginal payroll tax rate for the OASDI program; and τ_{MC} is the marginal payroll tax rate for the Medicare HI program.[22] We differentiate the latter two programs because, beginning in the early 1990s, the taxable maximum for the HI program was increased above that for the OASDI program (and was eventually removed altogether); the marginal rate is zero above the taxable maximum for payroll taxation. As the tax price of insurance rises (or as tax rates fall), there will be less pressure to pay for insurance through the firm, and therefore more premium sharing as a means of dealing with imperfect worker sorting.

To compute the marginal tax rate for each worker, we use the NBER's TAXSIM model, which inputs information on the major elements of taxable income and computes both a federal

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[21]We divide adults into those age 19-29, 30-39, 40-49, 50-59, and 60-64. We further divide women into ages 40-44 and 45-49 because pregnancy is assumed to occur only in the first group.

[22]The reason that the payroll tax rate is additive in the denominator is that the employer is indifferent between purchasing one dollar of benefits or paying wages of $1/(1+ \tau_{ss} + \tau_{mc})$, since each dollar of wages requires a payroll tax payment as well.

and state marginal tax rate.[23] Virtually all of the elements of taxable income that we need are reported in the CPS, with the major exception of any information on the itemization behavior of the household. We therefore used data from the Statistics of Income (SOI) data to impute both the odds of itemization and the amount itemized by state and family earnings level. For each person, we compute their tax rate as a non-itemizer, and as an itemizer with average itemization equal to the imputed amount from the SOI. We then take a weighted average of the resulting tax rates, where the weights are the predicted rate of itemization based on state and earnings.

The third prediction of the imperfect sorting model is that premium sharing should rise as premium costs increase, since this raises the value of sorting. Once again, we do not know the firm's actual insurance costs. Thus, as a proxy for insurance costs, we measure average spending on medical care per capita by state, from the Health Care Financing Administration.

Finally, we consider the role of cyclical conditions. This factor is not addressed in theoretical models of premium sharing, which are full employment models. But there are a variety of rent-sharing theories which suggest that firms and workers share in the benefits of firm success (and the costs of firm failure); Budd and Slaughter (2000) provide a review of this literature and some convincing new evidence. If there is rent sharing in wages, then there may also be rent sharing through health insurance contributions as well. Alternatively, another link between health insurance contributions and economic conditions could be employee recruitment. To the extent that potential employees pay particular attention to whether they have to contribute to their health insurance plan at a prospective new firm, and do not understand that it is likely that lower contributions for insurance also generally will imply lower wages, when the labor market is tight firms may choose to pay all of the costs of insurance. But, as unemployment rises, there is less pressure on firms to use low employee contributions as a recruitment tool. Thus, we include in the model the state/year unemployment rate, from the Bureau of Labor Statistics, to capture cyclical effects on premium sharing decisions.

Identification Concerns

While each of the measures laid out above captures the influences of these factors on employer behavior, the measures suffer from two important potential limitations. The first is

[23]For more information about TAXSIM, see Feenberg and Coutts (1993). A public use version of TAXSIM is available at www.nber.org/taxsim . Marginal rates are computed by first computing the tax bill, then adding $1000 to earned income and recomputing the tax bill, and taking the difference divided by $1000.

measurement error; these are very noisy proxies for the characteristics of a given worker's firm. The second is omitted variables bias. For each of these measures, there are potential correlates of both the measures and the firm's insurance decisions that could bias estimated relationships. A critical omitted variable is firm-specific economic shocks. For example, if a given firm is subject to a downturn, then both wages and employer contributions for health insurance may fall. A decline in wages will also lead to a decline in tax rates and therefore the subsidy to employer-provided spending, to a rise in Medicaid eligibility, and potentially to a rise in spousal labor supply, biasing all three of these coefficients in favor of finding the expected explanatory role for contribution shifts.

To address these concerns, we use instrumental variables for the first three of our measures. For spousal labor supply, we instrument actual spousal work with predicted spousal work using the characteristics of the spouse. That is, we estimate in each year a model of labor supply (separately) for married women and married men as a function of age, race, education, and interactions of these variables. We then use the resulting coefficients to form a predicted measure of work for each spouse of each worker in our sample, and use this as our instrument. In our regression models, we control for the spouse's age, education and race directly. So this instrument is identified only by interactions of race and age, race and education, and age and education, and interactions of all of these with year of survey. All of these seem plausibly exogenous to the premium contribution of a given employee.

For Medicaid eligibility, we follow Cutler and Gruber (1996) in instrumenting the Medicaid Expenditure Share with a "simulated" MES. This is computed by using a measure of "simulated" Medicaid eligibility. To create this measure, we first select a random sample of 250 married families and 250 single persons in each decile of their marital-status specific income distributions in each year's CPS. These same 5000 observations are then assigned to each state, and the relevant odds of Medicaid eligibility are computed for each family in the sample. The average MES is then computed for each income decile/marital status/state/year cell, and this is used as an instrument for all persons in that cell.

This instrument varies only by income decile by marital status, state, and year. Each of these factors is controlled for linearly in the model, so that identification comes only from their interactions. Thus, this instrument purges any omitted variables bias other than that arising potentially from those interactions. One obvious concern with this approach is that there may be changes in employee premium sharing by income group over time. Thus, in the basic model we also include a full set of income decile by marital status by year interactions. Another concern is that there may be time trends by state that are correlated with factors such as HMO penetration, and likewise correlated with employers' decisions on premium contributions. To control for such time trends, we include in the model a set of interactions between each state dummy and a linear time trend variable.

For our measure of the tax subsidy, we use a similar approach. We once again draw a national sample of families by income by marital status, and assign them to every state in that year. We then use that sample to compute tax prices, and use the average by income decile by marital status*state*year cell as our instrument.

For our remaining measures, managed care penetration, medical spending, and unemployment, we do not have readily available instruments. For unemployment, this is not likely to be an important issue, as the state/year unemployment rate can reasonably be taken as exogenous to the firm's decision on premium sharing. But this is a more important issue for our other measures.

For medical spending, the reverse causality may arise because rising employee contributions cause falling medical spending by making employees more sensitive to the cost of medical care. Fortunately for us, however, this biases <u>against</u> the hypothesis of interest, which is that higher medical costs lead to more employee contributions, so if we find the hypothesized relationship it should be convincing. For managed care penetration, the reverse causality may arise because managed care plans may choose to expand in places where employees pay a larger share of their premiums, since they will be most successful in such price sensitive environments. This bias is more problematic because it goes directly in favor of the hypothesis we are attempting to test.

Regression Framework

We will incorporate these measures of interest into a regression framework of the following form:

$$Y_{kjt} = \alpha + \beta_1 HMO_{jt} + \beta_2 SPLS_{kjt} + \beta_3 MES_{kjt} + \beta_4 TP_{kjt} + \beta_5 SPEND_{jt} + \beta_6 UNEM_{jt} + \beta_7 X_{kjt} + \beta_8 \eta_j + \beta_9 \tau_t + \beta_{10} \eta_j * TIME + \varepsilon \quad (3)$$

where k indexes individuals, j indexes states, and t indexes years; Y is one of our insurance measures; HMO is our managed care penetration measure; SPLS is average spousal labor supply for the cell; MES is the average Medicaid Eligible Share for the cell; TP is the average tax price for the cell; SPEND is state/year medical spending; UNEM is the state/year unemployment rate; X is a set of individual covariates; and η_j, and τ_t are sets of fixed effects for state, and year, respectively. The individual covariates in the model include own and spouse's age, race, and education; sex, marital status, and an interaction of these; occupation dummies; a set of 10 income decile dummies for married and 10 for single persons; interactions of these 20 income by marital status dummies with year dummies; and a separate linear time trend for each state (η_j*TIME).

Our key regressor is whether your employer pays all of the cost of your health insurance. For each of the coefficients β_1 through β_6, the hypothesis is that the coefficient of interest will be negative; each of these factors is hypothesized to raise premium sharing with employees. The impacts on whether the employer pays some of the cost of insurance are ambiguous. On the one hand, if employers are moving from paying all of the contributions to paying some, then these coefficients should all be positive when the dependent variable is employer pays some of the cost. On the other hand, to the extent that employers react to these forces by moving from paying some of the costs to none of the costs, then the coefficient may be negative. Moreover, it is important to recall that we are using unconditional measures of premium sharing here. So if employers are reacting to these forces by simultaneously reducing insurance coverage and premium sharing, then there could be reductions in both the "employer pays all" and "employer pays some" coefficients; the reduction in the latter would reflect the net of shifting to employees and dropping insurance altogether.

The means of our data are presented in Table 2. 62% of our sample of workers has health insurance coverage through their own employer. For roughly 2/5 of these workers, the employer pays all of the cost of insurance; for the other 3/5, the employer pays some, with very few employees having employers who pay none of the costs of insurance. On average over our sample period, 12% of the privately insured are in HMOs, although this figure is rising rapidly over time. Only 3% of Medical spending for our full sample is eligible for Medicaid on average, although this figure is once again rising rapidly. Roughly half of spouses work, and on average the tax subsidy to insurance is about one-third of the price of insurance (a tax price of insurance, relative to wages, of 0.65). Medical spending per capita in the states averages $2450, and the average unemployment rate is 7%.

Part IV: Results

Basic Results

Our basic regression results are shown in Table 3. The first three columns show the results for the odds that the employer pays all, some, or none of the cost of insurance. The coefficients across these columns need not add to zero because these are unconditional measures; rather, the coefficients add to the net change in insurance coverage induced by that factor. The final column therefore shows the impact on having coverage at all through your employer. All regressions are estimated as linear probability models for consistency of our instrumental variables estimates; results are similar if probit models are used instead. The standard errors are corrected for within state-year clustering.

The most striking feature of the first column of Table 3 is that all of our predictors have the expected (negative) sign; in every case, a stronger incentive for more premium sharing reduces the odds that employers pay all of the cost of insurance. This is a striking confirmation of the role of economic incentives in this employer decision.

But only four of the six coefficients are statistically significant. The first coefficient of interest is that on HMO penetration. There is a negative impact of HMO penetration on premium sharing, indicating that for each 10 percentage point rise in HMO penetration, the share of employers paying all of the cost of health insurance falls by 0.74 percentage points. There is a corresponding rise in the share of employers paying some of the cost, with little effect on overall coverage. But none of these coefficients are significant, and the impacts are substantively quite small; the elasticity of full employer financing of insurance with respect to managed care penetration is less than 0.04. Thus, the results here confirm the intuition from Levy's (1997) facts: the fixed subsidy model cannot explain much of the time trend in premium sharing.

The next two coefficients of interest are those on Medicaid and on spousal labor supply. Both show sizeable and highly significant negative impacts on the odds that an employer pays all of the cost of health insurance, which is consistent with the contention of the imperfect sorting model that raising outside insurance options will lead to more premium sharing. In the case of Medicaid, the results indicate that for each 10 percentage points increase in the Medicaid eligible share, the share of employers paying all of the cost of insurance falls by 1.7 percentage points. There is a corresponding 0.77 percentage point rise in the share of employers paying some of the cost and a 0.28 percentage point rise in the share paying none of the cost. In addition, there is a 0.64 percentage point decline in the odds that the individual is covered at all by employer-provided insurance (which is consistent with the "crowdout" results in Cutler and Gruber (1996)).

The fact that there is some reduction in total insurance coverage makes interpretation of the impacts on premium sharing somewhat difficult, because without longitudinal data we can not infer the premium sharing arrangement that existed for those losing (or dropping) coverage.[24] A conservative assumption would be that those that lost or dropped coverage were distributed across the all/some/none categories in proportion to the full sample. This is conservative since it seems likely that those firms that would drop coverage in response to Medicaid expansions, or those workers that would stop taking up, would be much more likely to come from the pool of firms paying some or none of the costs of insurance, not from the pool of firms paying all of the cost.

Under this assumption, 38% of those employees losing coverage previously were in jobs where the employer paid all of the costs of insurance, 57% were in jobs where the employer paid some of the costs, and 5% were in jobs where the employer paid none of the costs. These proportions would suggest that 0.24% of the 1.77% reduction in employers paying all comes from reduced coverage, so that on net a 10% rise in Medicaid entitlement led to a 1.53% shift from employers paying all of the cost of insurance to employers paying some or none. But this is likely a lower bound, for the reasons noted above.

For spousal labor supply, there is a 1% reduction in the odds of an employer paying all of the costs of insurance for each 10% rise in the odds of having a working spouse. But there is an even larger 1.2% decline in the odds of an employer paying some of the cost, with little effect on the odds of paying none of the cost, for a total reduction in employer coverage of 2.2%. In this case, interpreting the impact on actual changes in premium sharing is more difficult. But under the conservative assumption that coverage reductions are in proportion to the initial shares of premium contributions, then there is a slight shift in financing of roughly 0.1% for each 10% rise

[24]In the context of Medicaid, the available evidence suggests that the overall reduction in coverage arises mostly from a reduction in insurance takeup conditional on offering, not from reduced employer offering.

in spousal labor supply. Thus, there is a wide range of possible impacts of spousal labor supply on premium sharing decisions of firms, but in any case the estimated impact appears fairly small.[25]

The fourth row shows the impact of the tax price. Once again, as with the case of spousal labor supply, here we find negative effects on paying all and on paying some of the cost of insurance, and even a significant negative impact on the odds of paying none of the cost, with a resultant very sizeable decline in overall insurance coverage; this result mirrors the price sensitivity of employer-provided insurance coverage documented in Gruber and Lettau (2000) and Gruber (2002). Once again, this overall negative impact makes interpretation somewhat difficult. Under the conservative assumption used thus far, each 10% increase in the tax price leads to a 1.7% shift from employers paying all of the costs of insurance to employers paying some or none. So the impact of tax changes on premium sharing appears quite large; the effect varies from 1.7% to 3.7% per each 10% change in tax price.

The fifth row shows the effect of state/year medical costs; the coefficient is that on the level of costs divided by 1000. We find here a negative, but not significant, relationship between medical costs and premium sharing.

Finally, there is a very significant negative effect of the unemployment rate on the odds that the employer pays all of the cost of insurance. This coefficient indicates that for a 10 percentage point rise in the unemployment rate, 1.7 percent fewer firms pay all of the cost of insurance. There is a rise in the odds of a firm paying some or none of the cost of insurance by 0.5 percent, and an overall reduction in coverage of 1.2 percent. Thus, under the conservative approach pursued thus far, we say that each 10 percent rise in unemployment leads to a 1.2 to 1.7 percent reduction in the odds that an employer pays all of the costs of insurance.

Endogenous Incomes

We argued above that our instruments likely purged these models of omitted variables bias, because our instruments only vary by income group, marital status, state, and year, and we are controlling for main effects of all four factors, as well as interactions of income, marital

[2525]It is interesting to note that while we find that higher levels of spousal labor supply reduce the odds of own insurance coverage, we find no effects on the odds of having <u>any</u> employer-provided coverage. Thus, it appears that when spouses work, they reduce the insurance coverage on their spouse's job, but equally raise the odds of insurance through their own job.

status, and year. But there is an additional concern that is not addressed by this approach: endogeneity of income groups. The consensus in the health economics literature is that there is full or close to full shifting of health insurance costs to wages (Gruber, 2000). As a result, if firms change their insurance contributions, that should be reflected in wages, which will in turn feed back to our instruments. This creates a problematic endogenous correlation between our instruments and the dependent variables in these models.

We have addressed this endogeneity concern by recreating our instruments using not actual income but *predicted* income. That is, we predict income for each household as a function of age, sex, race, education, sex*education, sex*race, race*education, and dummies for number of children. We then use these predictions to create *predicted* income deciles, and classify households based on these predicted income deciles for the purposes of making our instruments. This approach results in instruments which are free of the potential endogeneity bias from using actual incomes, although they are also, by definition, less efficient.

The results of using this alternative IV approach are shown in Table 4. As would be expected, there is relatively little impact on the regressors where there was no change in instruments; the coefficient in the "employer pays all" regression is down somewhat for spousal labor supply, and up for the unemployment rate. There is also remarkably little impact of this instrument on the Medicaid coefficient on premium sharing, although the overall coverage coefficient is now insignificant. There is a much larger impact on the tax price coefficient, which has almost doubled in size. This is partly due to a larger overall effect on insurance coverage, and partly due to a larger concentration of the effect in the employer pays all (rather than the employer pays some) category. Using the same type of calculation that we pursued above, we now estimate that for each 10% rise in the tax price, there is a 3% reduction in the odds that employers pay all of the costs of health insurance, a quite large effect.

Implications for Time Series Trends

Our paper began with the question of what factors can explain the time series trend in rising employer contributions for health insurance. We can now return to this question by applying our estimated coefficients to the time series trends in our key independent variables, and comparing the predicted time series trends that result to the actual trend in premium contributions by employers.

The results of doing so are presented in Figure 4. The figure shows two lines, which have been rescaled so that the time patterns can be easily compared. The first line is the actual time trend in the share of employers paying all of the cost of insurance. The second line is the predicted time trend, based on the time trend in our six key independent variables, times the coefficients of each in our basic Table 3 regression.

There is a remarkably close correspondence between the time series in the actual and predicted time series. The key features of the time series are replicated here: a slow decline through 1985, a much more rapid decline through 1992, and then a flattening in the mid-1990s. The figure is very similar if, instead, the conditional share of firms paying all of insurance costs is compared to the implied conditional effects from our regressions (e.g. using our conservative assumption above to obtain the impact on premium shifting).

While the correspondence between the series is close over time, however, the magnitudes implied by our model are not large enough to explain the overall time series shift. In Table 5, we illustrate this by dividing our data into three periods: 1982-1985; 1985-1992; and 1992-1996. From 1982-1985, the share of employers paying all of the cost of health insurance fell by 2.2 percentage points. The predicted decline from our model was 0.4 percentage points, or 18% as large a decline. From 1986-1992, the share of employers paying all of the cost fell by 9.8 percentage points. The predicted decline 2.7 percentage points, or 28% as large a decline. From 1993-1996, the share of employers paying all of the costs fell by 0.7 percentage points. The prediction over this period was acutally a rise of 0.3 percentage points. Over the entire period, the actual decline was 12.8 percentage points, and the predicted decline was 2.8 percentage points, or 22% as large a decline. Thus, we conclude that the factors in our model match fairly well the time series pattern of employer contributions, but that they can only explain about a quarter of the overall movements over this period.

Part V: Conclusions

The large and growing literature on the determinants of health insurance coverage of the U.S. population has been focused primarily on the decision of employers to offer health insurance. But there is a growing recognition in health economics that employee takeup decisions may be the more important margin for explaining the large declines in coverage that we have witnessed over the past two decades. This contention is bolstered by the fact that there was such an enormous shift in premium costs from employers to employees over this time period. Yet, to date, there has been no explanation for this dramatic and potentially important trend.

In this paper, we have investigated six possible determinants of this trend, drawing on the theoretical arguments for why, in the face of tax subsidized employer premiums, employers would shift premium costs to employees. Five of these six determinants (spousal labor supply being the exception) also have the attractive feature that the incentives for employee financing grow most rapidly in exactly the time period when the shift to employee financing was most pronounced, the late 1980s and early 1990s. We find that, for all six factors, we obtain the expected relationship with employee financing, although this relationship is only significant in four of the six cases. In terms of timing of changes over this period, these factors do an excellent job. But, in terms of the overall trend over this period, they explain less than one-quarter.

These findings, particularly the strong effect for tax incentives, suggest that premium financing is a price sensitive decision for firms. This implies that policies that subsidize the employer-provision of health insurance may not only increase insurance offering, but also reduce the burden of premium payments for employees. This provides an additional factor that must be included in cost-benefit analysis of employer versus individual subsidies as a means of expanding insurance coverage.

These results also raise two further research questions. First, what other factors explain the trend towards increased employee premium sharing over this period? Future research with data that has more continuous measures of premium sharing should be employed to understand more fully this important trend. Second, what are the implications of these rising employee contributions? As noted earlier, the existing small literature on employee takeup suggests that it is not very price elastic, suggesting that this premium shift has only distributional consequences. But further work is needed to confirm or refute this contention.

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Table 1: Comparison of CPS and BLS Data on Percent of Employees Contributing to their Plans

Year	CPS All		EBS Medium/Large		CPS Medium/Large	
	Family	Single	Family	Single	Family	Single
1981	54%	48%				
1982	54%	47%				
1983	55%	48%	54%	33%		
1984	56%	49%				
1985	56%	49%	56%	36%		
1986	57%	50%				
1987	58%	49%			62%	55%
1988	58%	50%	64%	44%	63%	56%
1989	61%	52%			66%	58%
1990	63%	53%			68%	60%
1991	65%	57%	69%	51%	70%	65%
1992	67%	60%			73%	67%
1993	67%	59%	76%	61%	73%	67%
1994	...	....			....	....
1995	66%	60%	78%	67%	73%	67%
1996	70%	60%			75%	68%
1997	69%	60%	80%	69%	75%	68%
1998	70%	63%			76%	70%

Table 2: Means

Variable	Mean	Standard Deviation
Own Group Coverage	0.62	0.48
Employer Pays All	0.24	0.43
Employer Pays Some	0.36	0.48
Employer Pays None	0.03	0.17
Manage Care Penetration	0.12	0.09
Spousal Labor Supply	0.46	0.50
Medicaid Eligible Share	0.03	0.12
Tax Price	0.65	0.10
Medical Spending ($1000)	2.45	0.63
Unemployment Rate	0.07	0.02
Number of Obs	850,541	850,541

Table 3: Basic Results				
Variable	Employer Pays All	Employer Pays Some	Employer Pays None	Own Group Coverage
Managed Care Penetration	-.074 (.086)	.092 (.089)	-.049 (.028)	-.037 (.071)
Medicaid Eligible Share	-.167 (.034)	.077 (.047)	.028 (.012)	-.064 (.045)
Spousal Labor Supply	-.104 (.026)	-.122 (.030)	.008 (.011)	-.215 (.032)
Tax Price	-.367 (.098)	-.127 (.077)	-.060 (.022)	-.554 (.091)
Medical Spending	-.005 (.008)	-.003 (.009)	-.004 (.003)	-.011 (.008)
Unemp. Rate	-.170 (.071)	.065 (.080)	-.017 (.025)	-.120 (.074)
Number of Obs	850,541	850,541	850,541	850,541

Note: Dependent variable listed in top row. Standard errors in parentheses. Regressions also include controls for: own and spouse's age, race, and education; sex, marital status, and an interaction of these; occupation dummies; a set of 10 income decile dummies for married and 10 for single persons; interactions of these 20 income by marital status dummies with year dummies; state and year fixed effects; and a separate linear time trend for each state.

Table 4: Results Using Predicted Income Instrument				
Variable	Employer Pays All	Employer Pays Some	Employer Pays None	Own Group Coverage
Managed Care Penetration	-.088 (.088)	.076 (.096)	-.048 (.028)	-.066 (.081)
Medicaid Eligible Share	-.163 (.067)	.180 (.096)	.012 (.024)	.025 (.094)
Spousal Labor Supply	-.094 (.025)	-.074 (.031)	.009 (.010)	-.156 (.033)
Tax Price	-.667 (.183)	-.066 (.192)	-.078 (.043)	-.808 (.183)
Medical Spending	.0001 (.008)	.003 (.009)	-.005 (.003)	-.001 (.009)
Unemp. Rate	-.254 (.076)	-.014 (.085)	-.009 (.025)	-.276 (.085)
Number of Obs	850,541	850,541	850,541	850,541

Note: Dependent variable listed in top row. Standard errors in parentheses. Regressions also include controls for: own and spouse's age, race, and education; sex, marital status, and an interaction of these; occupation dummies; a set of 10 income decile dummies for married and 10 for single persons; interactions of these 20 income by marital status dummies with year dummies; state and year fixed effects; and a separate linear time trend for each state.

Table 5: Comparing Predicted vs. Actual Trends

Time Period	Actual	Predicted
1982-1985	- 2.2 %	- 0.4%
1985-1992	- 9.8 %	- 2.7%
1992-1996	- 0.7 %	0.3 %
1982-1996	- 12.8 %	-2.8 %

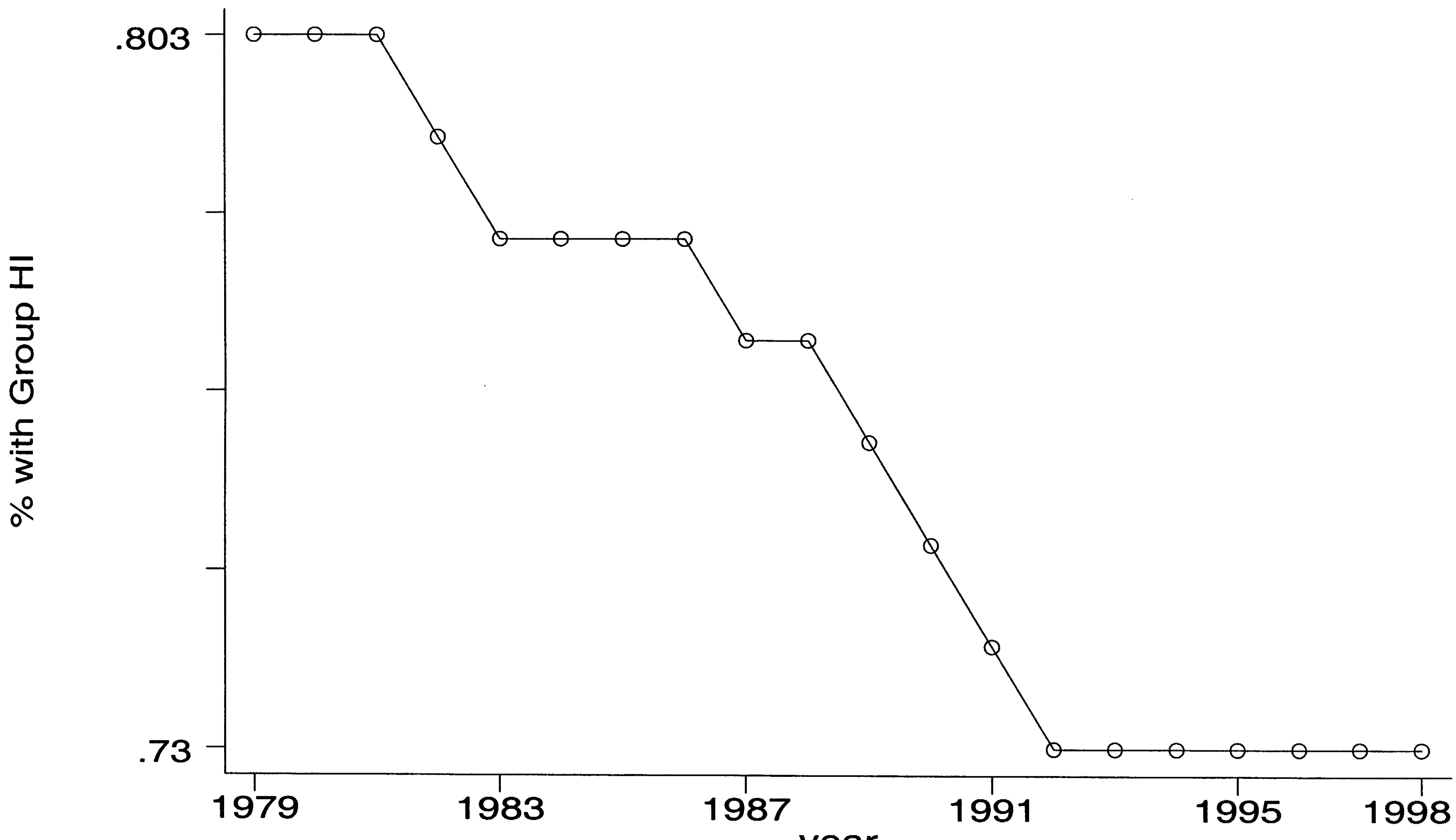

Figure 1: Percent of Workers with Group Health Insurance

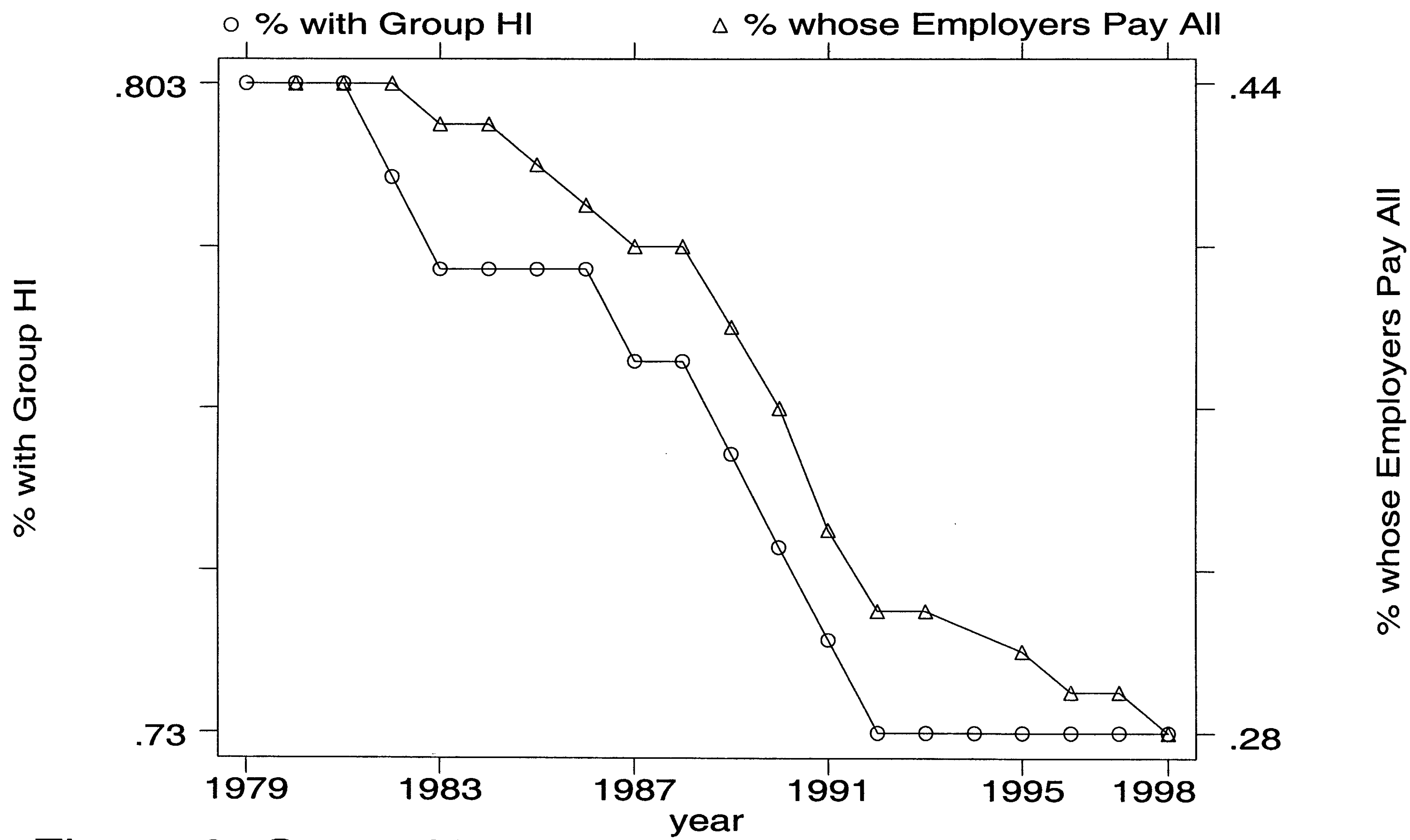

Figure 2: Group Health Insurance vs. Employer Pays All

Figure 3: Comparison of CPS and KPMG Premium Sharing

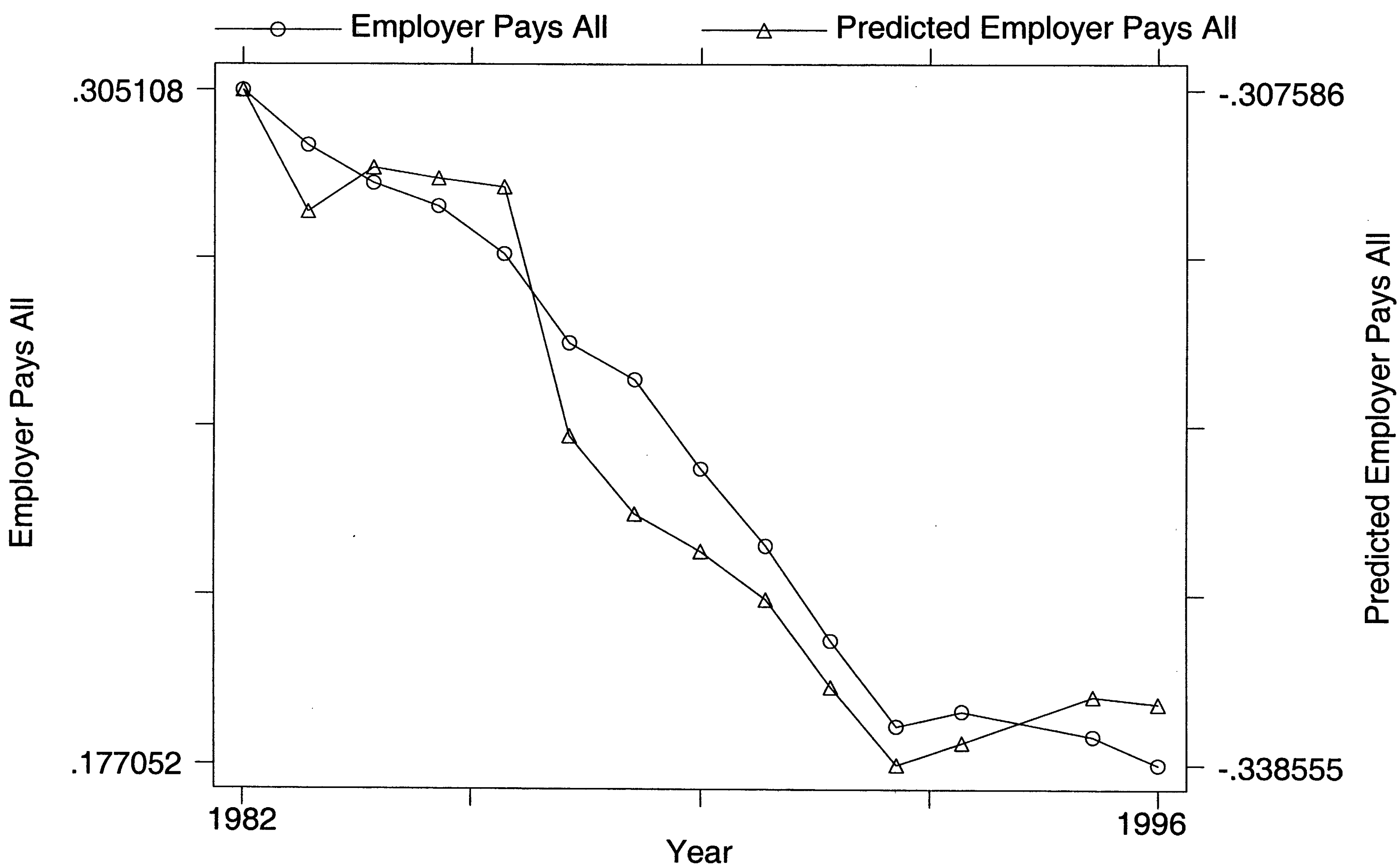

Figure 4: Actual vs. Predicted Time Series of Employer Pays All

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