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The Effect of Medicare Home Health Care Payment on Informal Care

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Abstract

We assess the effect of payment caps for Medicare home health care on the informal care use of older adults with functional limitations. We find that individuals exposed to more restrictive payment caps offset reductions in Medicare home health care with increased informal care, although we only observe this effect for lower-income individuals. This suggests that home care payment restrictions my increase caregiving burden on some low income families, but that many higher income families were able to either forgo the care or finance it privately. Home care payment policies should reflect these effects, balancing costs of the program with the desire to protect families from the burdens associated with providing informal home care.

1. Introduction

As the US population ages, policymakers must be prepared to address a growing demand for long-term care services for older adults with functional limitations. In the coming decades there may be a significant increase in the demand for long-term care as the number of elderly Americans increases and because of current adverse health trends such as obesity (Lakdawalla, Bhattacharya, and Goldman 2004; Congressional Budget Office 1999). This projected future demand is in spite of evidence that disability rates at old age have improved somewhat over the past 10–15 years (Freedman, Martin, and Schoeni 2002; Manton, and Gu 2001).

A major component of the long-term care continuum for older adults with functional limitations is home care. The broad goals of home care are to provide services and supports to individuals so that they may avoid institutionalization (which is more expensive and less desirable for many individuals) and to provide respite to family caregivers.

The increased demand for home care has important implications for public budgets, which finance 75% of all home care (Catlin et al. 2007), as well as for families who pay for home care privately or provide direct care. In 2005, home care services accounted for 28% of total long-term care expenditures and 2.5% of total US health expenditures (Catlin et al. 2007). Furthermore, home care was the fastest growing category of national health care expenditures between 2003–2005 (Catlin et al. 2007) and spending on Medicaid Home and Community Based Services Waivers increased from $2.7 million to $14.1 million between 1992 and 2001 (Kitchener et al. 2005). Policymakers have already acted to address the

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financial pressures associated with publicly funded paid home care. For example, after rapid growth of Medicare home health services in the early to mid-1990's, Congress instituted payment caps to the Medicare home health payment system as part of the 1997 Balanced Budget Act. These caps (discussed in greater detail later) resulted in a dramatic decrease in Medicare home health care utilization.

Another major component of the LTC continuum is informal care. Informal care is home care that is delivered without payment, and is generally provided by family and friends of the care recipient. An important limitation of the aforementioned data on the levels and trends of home care costs is that they only include paid home care, while excluding informal care, which accounts for the majority of LTC that is delivered in the US. There is evidence that the economic value of informal care is considerably greater than the combined expenditures on nursing homes and paid home care (Arno, Levine, and Memmott 1999).

Policymakers must consider several issues when determining how much home care to fund with public dollars and how publicly-funded home care services will be reimbursed. For instance, any short-term cost savings that would result from a potential policy to reduce the generosity of publicly-funded home care must be weighed against the effects on recipients' health and probability of institutionalization. Additionally, the analysis should consider the effects on informal caregivers, who may shoulder additional burdens as the result of the policy.

Another important consideration for policymakers is the potential distributional consequences of such policies and whether they are consistent with societal preferences for distributing publicly-funded home care. For instance, the growth in Medicare home health services in the 1990's was disproportionately distributed to individuals with greater informal support (Langa et al. 2001), whereas some observers support targeting publicly-funded home care to those at greatest risk of negative outcomes (e.g., mortality, institutionalization, increased disability) (Weissert, Chernew, and Hirth 2001) and/or those with less informal support (Wolf 1999). In addition, distributional issues related to income and disparities may be important. Specifically, relative to high-income individuals, lower-income individuals may be at greater risk of institutionalization due to poorer health and functional status and due to a greater likelihood of Medicaid eligibility via spend-down provisions. Lower-income individuals may respond to reductions in paid home care by relying more on unpaid care, if paid care is unaffordable. In this paper, we assess the effect of a major change in the way that Medicare paid for home health services on informal care use and whether that effect varied by individuals' income.

2. Background

The effect of publicly-funded home care generosity on informal care is a central issue for home care policy. Policymakers face a difficult tradeoff in decisions about the generosity of publicly-funded home care, because of the potential for substitution between paid home care and informal care. Specifically, the question of whether and how publicly-funded home care generosity affects informal care implicitly hinges on whether and to what extent paid home care substitutes for informal care. There are sociological and economic conceptual arguments that suggest that paid home care may or may not be a substitute for informal care (Muramatsu and Campbell 2002; Noelker and Bass 1989). Some prior economic conceptual models of informal care are ambiguous regarding whether informal care substitutes for paid home care (Pezzin, Kemper, and Reschovsky 1996), while other models predict that the two forms of care will be substitutes (Sloan, Hoerger, and Picone 1996; Van Houtven and Norton 2004). Because these conceptual models do not make strong predictions of the extent of substitution, this question must be resolved empirically.

Empirical research in this area faces methodological challenges and is not definitive. For example, a randomized trial of paid home care services from the early 1980's provided mixed evidence on the relationship between paid and informal home care (Christianson 1988; Kemper 1992; Pezzin et al. 1996), although those data were not nationally representative and are now nearly 25 years old. More recent research has utilized instrumental variables methods and found that paid home care and informal care are substitutes, although the degree of substitution reported is inconsistent (Pezzin et al. 1996; Van Houtven and Norton 2004, 2007). Although this research is valuable for understanding the relationship between paid and informal care, it does not provide direct evidence of the effects of changes in publicly-funded home care on informal care use.

Direct estimates of the effect of publicly-funded home care on informal care are more relevant for this work. Research using data from Canada exploits inter-provincial variation in publicly-funded home care policy generosity and finds that more generous home care policies were associated with a significant reduction in informal caregiving (Stabile, Laporte, and Coyte 2006). Data were limited in only having information on whether or not informal care was delivered, and not on the total hours of informal care delivered. In addition, it is questionable whether inter-provincial variation in publicly-funded home care generosity is truly exogenous, as it is plausible that if there are fewer informal caregivers per province there may be pressure to expand publicly-funded home care. This concern is salient because although a Hausman test of OLS consistency was not significant, the point estimates from their IV analysis indicated no effect of publicly-funded home care generosity on the probability of informal caregiving.

Home Care Policy in the U.S

In the early to mid-1990's, Medicare paid for over half of the total home care costs for older adults in the US (54% in 1996), even though Medicare home health services were only originally intended to be post-acute care options (Spector, Cohen, and Pesis-Katz 2004). Medicare also experienced explosive growth in home health services between 1990 and 1996. The number of home health visits per 1,000 beneficiaries increased from 2,054 to 7,857 and Medicare home health expenditures increased from $3.7 billion to $16.75 billion (Health Care Financing Administration 2001). This increase was fueled by a cost-based reimbursement system wherein home health agencies had incentives to provide more services and by expansions of the types of conditions and home health services eligible for reimbursement from Medicare.

Policymakers responded to rapidly increasing Medicare home health costs in several ways. Possibly the most important response was imposing a prospective payment system as part of the 1997 Balanced Budget Act (BBA). An interim payment system (IPS) was put in place in October of 1997 because the newly mandated prospective payment system would not be ready to be implemented until 2000. The IPS imposed annual per-patient caps for reimbursement on home health agencies. Seventy-five percent of the cap came from the agency's average per-patient costs in 1994, and 25% of the cap came from the regional average per-patient costs in 1994 (McCall et al. 2001). The IPS payment caps changed home health agencies' incentives in two ways: agencies had incentives to provide care more efficiently so that per-patient costs would not exceed the payment caps, and agencies also had an incentive to avoid high-cost patients altogether (McKnight 2006). In addition to changing the payment system, the federal government also became more active in reviewing Medicare home health claims for fraud and in penalizing physicians who fraudulently certified Medicare beneficiaries as being eligible for home health services (McCall et al. 2001).

These policies had dramatic effects, resulting in considerable drops in the percentage of Medicare beneficiaries receiving any home health services and in the number of visits per home health care user (Figures 1 & 2) (Health Care Financing Administration 2001). Furthermore, there is evidence that the IPS had strong effects independent of the other concurrent policy changes. Specifically, individuals who faced more restrictive IPS payment caps received significantly fewer Medicare home health services after the implementation of the IPS, although they did not increase their nursing home or Medicaid home health care use, or experience poorer health outcomes (McKnight 2006). It is difficult to assess the extent to which the services that were reduced were fraudulent or unnecessary. However, the fact that higher-income beneficiaries offset most, but not all (63%) of the reduced Medicare home health with out-of-pocket care may imply that beneficiaries did not fully value all of the services that were reduced (McKnight 2006). These important findings raise the question, to what extent were reductions in paid home care absorbed by increasing informal care use?

This research addresses this question, as well as the question of whether individuals' responses to the Medicare home health payment change varied by level of income. Our research builds on the prior literature in several important ways. First, although some studies focus only on the probability of using any informal and paid home care, we are also able to look at the effects of publicly-funded paid home care policy on total hours of informal care use. Second, our data come from a nationally representative sample of older adults. Third, we exploit a plausibly exogenous source of variation in the generosity of publicly-funded paid home care to assess the effects of the policy on informal care use.

3. Data and methods

3.1. Data

We use data from the 1993, 1995, 1998 and 2000 waves of the Asset and Health Dynamics among the Oldest-Old Survey (AHEAD) and data from the 1996, 1998, and 2000 waves of the Health and Retirement Study (HRS). AHEAD and HRS are nationally representative longitudinal studies of the non-institutionalized (at baseline) population of older Americans. AHEAD collects data from adults who were aged 70 and older in 1993 and their spouses, while the HRS cohort includes individuals who were 51–61 years old in 1992 and their spouses. We only include individuals from these datasets who were older than 65 in a given wave, because our analyses relate to changes in Medicare policy. To ensure comparability of our measures, we only include unmarried individuals in our sample, because data on informal care delivered from spouses were not collected in 1995 or 1998. We also restrict our sample to individuals with at least one activity of daily living (ADL) or instrumental activity of daily living (IADL) limitation in a given wave, because paid and informal home care questions were only asked if the respondent reported a limitation.

One sample complication relates to the timing of the implementation of the Medicare home health Prospective Payment System (PPS). Because the IPS was replaced in October 2000 with the PPS, it is possible that the incentives of the IPS did not strongly affect the observations in the 2000 wave of the data. In fact, to the extent that home health agencies were aware that a new PPS was to be instituted, the agencies may have had less of an incentive to avoid high-cost patients as the change to the PPS drew closer. Specifically, high-cost patients would be less likely to exceed the maximum IPS payment cap if the IPS were only binding for a limited period of time. To address this issue, we exclude observations from 2000 that were interviewed in or after July (when the final PPS regulations were published). After excluding observations with missing data on any covariates or sample weights, the final sample includes 1,686 observations in 1993, 1,589 observations in 1995, 51 observations in 1996, 1,950 observations in 1998, and 1,425

observations in 2000. This yields a final sample size of 6,701 observations from 3,621 unique individuals. Each individual could contribute between one and four observations to the sample, with an average of 1.85 observations per person.

3.2. Key measures

The data on informal home care use are based on self report. All home care questions were asked in reference to ADL and IADL limitations. For each ADL or IADL limitation that was reported, the respondent was asked if he or she received help with that limitation, how much help was received, who delivered the help, and whether the helper was paid. These measures were calculated from the average number of days per week and the average hours per day that a respondent reported receiving home care, with missing data imputed (Langa et al. 2001). The measure of informal care use therefore only captures non-medically-skilled home care, namely home-based help or personal care for functional limitations. The final measure of informal care hours is the average number of hours of unpaid home care for ADL or IADL assistance per week, over the month prior to interview. Scholars have recognized the HRS/AHEAD for having among the best available data on informal caregiving for nationally-representative surveys of older Americans (Wolf, Freedman, and Soldo 1997). Our measure of informal care use is comparable to the measures in other recent research that uses this dataset (Van Houtven and Norton 2004).

3.3 Identification strategy and empirical specification

To identify the effect of Medicare home health generosity on informal care use while avoiding problems of endogeneity of publicly-funded home care generosity, we exploit a natural experiment that emerged from the implementation of the IPS for Medicare home health services. McKnight (2006) observed that the formula that determined IPS home care payment caps was implemented in a way so that the average restrictiveness of the caps were plausibly exogenous at the state level. Because 25% of a home health agency's IPS payment caps was derived from the 1994 regional average for Medicare home health use, the average payment caps in a given state were higher if the other states in the census region had lower levels of Medicare home health use. This implies that two states in different census regions could have had very similar levels and trends of Medicare home care use before the IPS, but could have received average payment caps that were very different because the caps depended in part on the states' regional levels of home care use.[1] Using data from the Medicare Current Beneficiary Survey, McKnight found that individuals who lived in states with more restrictive caps received significantly less Medicare paid home care. Two other findings from McKnight's research are also relevant. First, the reductions were greatest among individuals with poorer health and more functional limitations, ostensibly indicating that home health agencies indeed responded to the new incentive to avoid patients with higher predicted costs. Second, the overall reductions in paid home care were concentrated within individuals with lower incomes, as beneficiaries with higher incomes offset most of the reductions in Medicare home care with out-of-pocket home care.

We extend McKnight's analysis by looking at the effect of the restrictiveness of the IPS on informal care use. If there is substitution between Medicare home care and informal care, then we would expect to find that individuals in more restrictive states experienced increases in their informal care use after the implementation of the IPS. Our basic regression specification is as follows:

[1]Additionally, for the restrictiveness of the payment caps to be truly exogenous, high-and low-restrictiveness states must have had similar home care trends prior to the IPS. In her original analyses, McKnight did not find any evidence of different trends across states' restrictiveness.

$$\text{Informal}_{ist}=\beta_0+\beta_1\text{Restrict}*\text{PostBBA}_{st}+\sum_{s=1}^{S}\beta_2\text{State}_s+\sum_{t=1}^{T}\beta_3\text{Year}_t+\sum_{s=1}^{S}\beta_4\text{State}_s*\text{lineartime}+\beta_5 X_{ist}+u_{ist}$$

We estimate a reduced form equation for weekly informal care hours (Informal_{ist}). Following McKnight, we create a state-level variable that measures the restrictiveness of IPS caps by subtracting each state's 1994 Census region average Medicare home health visits per user from each state's 1994 average Medicare home health visits per user. This yields a continuous variable ranging from −41 to +35 (mean = −.51, SD = 12.23), with higher values indicating a more restrictive IPS cap. This variable was adjusted in the 2000 wave to reflect that IPS caps were relaxed by one-third in 1999 and 2000 for agencies with payment caps that were more restrictive than the national median (Federal Register 1999). The key independent variable is $\text{Restrict*PostBBA}_{st}$, which is the interaction between the state's level of IPS restrictiveness and an indicator of whether the observation is before or after the implementation of the IPS. If the estimated coefficient for this variable is positive, that would indicate that individuals substituted informal care for Medicare home health care.

The reduced form equation includes state and year fixed effects and a set of variables that measure state-specific linear time trends in informal care use. The state linear time trends are interactions between the state fixed effects and a continuous measure of the year of observation. We include the state time trend variables to control for any pre-existing state trends in informal care use and to be consistent with McKnight's main specifications (although we re-estimate without the state time trends in a sensitivity analysis). We also include the following individual-level covariates in X_{ist}: sociodemographics (gender, age, race, income, education), health status (hypertension, heart disease, cancer, lung disease, dementia, stroke, psychiatric disorder, arthritis), and functional status variables (number of ADL and number of IADL limitations). We estimate this equation for the full sample and then separately for the subsamples of individuals who were above or below the poverty line to assess whether there was a differential impact of the IPS by level of income. Because our sample is restricted to those with at least one ADL or IADL limitation, we are focusing on the subset of the elderly population with the greatest need for long-term care. This sample restriction may be comparable to McKnight's designation of Medicare beneficiaries who have high predicted home care costs due to poorer health and functional status.

3.4. Statistical analysis

Because the dependent variable, weekly informal care hours, is non-negative with a large zero mass and a skewed positive distribution, we estimate the effect of IPS restrictiveness using two-part models (Duan et al. 1984). The first part of the model is a probit equation estimating the probability of any informal care use, and the second part of the model is an OLS regression of logged informal care hours, restricted to those with positive informal care hours. The two-part models were estimated with Norton's two-part probit program in Stata 9.2 (Norton 2005) and standard errors are clustered on the state. We used a smearing estimator in the re-transformation of the logged informal care hours when calculating marginal effects, due to heteroskedasticity in some of the independent variables (Duan 1983). Standard errors and confidence intervals of marginal effects are estimated by bootstrapping and are clustered at the state level to adjust for observations that are correlated at the state level and at the individual level (nearly all individuals did not change their state of residence over the study period). All analyses are conducted using the HRS/AHEAD sampling weights.

4. Results

Table 1 displays the description of the sample. Fifty-one percent of the full sample reported using any informal care over the month prior to interview and the average weekly hours of informal care for the full sample is 13 hours. Fifty-eight percent of the low-income subsample reported using any informal care over the month prior to interview; the average weekly hours of informal care for the low-income subsample is 15 hours. Forty-eight percent of individuals above the poverty line reported using any informal care; their average weekly hours of informal care is 12.3 hours per week. The higher levels of informal care in the low-income subsample likely reflect the fact that lower income individuals have poorer health status or that they have fewer available resources to pay for home care out-of-pocket. These estimates are consistent with recent research using the HRS/AHEAD (Van Houtven and Norton 2004). However, they are somewhat lower than those from the 1994 National Long-Term Care Survey, in which 66% of older adults with functional limitations used any informal care in the prior week (Spillman and Pezzin 2000). This discrepancy may be because our data exclude married individuals who may use more informal care due to the availability of spousal support.

Table 2 includes the results of the two-part models. For the full study population, a higher level of state IPS restrictiveness is associated with a higher probability of using any informal care in the first part of the two-part model, although the coefficient (0.008) is not statistically significant (p=.106). In the conditional equation of the two-part model, there is no association between level of state IPS restrictiveness and logged informal care hours. After splitting the sample by observations above and below the poverty line, the results of the two-part models are considerably stronger for the low-income subsample, compared to the higher-income subsample. For the higher-income subsample, the probit coefficient on the interaction of IPS restrictiveness and post-IPS implementation from the first part of the model is less than half as big as for the low-income subsample (0.0037), and is insignificant (p=.590). There is no association between state IPS restrictiveness and logged informal care hours in the conditional equation within the higher-income subsample. Within the subsample of low-income individuals, there is a statistically significant association between state IPS restrictiveness and the probability of using any informal care in the post-IPS period (coefficient=.028, p=.072). The coefficient for logged informal care hours is also positive for the low-income subsample, but is far from statistically significant (coefficient=.0034, p=.763).

To give a more intuitive interpretation of our results, we also report the marginal effect of a one-unit increase in state IPS restrictiveness in the post-IPS period on total informal care hours (Table 3). We present marginal effects separately for the full sample and for the subsamples of individuals above and below the poverty line. To test for the significance of these marginal effects, we report the bias-corrected, bootstrapped confidence intervals, based on 500 bootstrap replications. For the full sample, a one-unit increase in the IPS restrictiveness measure results in a statistically significant .002 increase in the probability of using any informal care and a non-significant increase of one minute per week in informal care hours. In the higher-income subsample, the marginal effect of a .0009 increased probability of using any informal care was less than half as strong and the effect on total informal care hours was negative, although neither of the effects was statistically significant. However, in the low-income subsample, the effect of a one-unit increase in the IPS restrictiveness measure is a 0.0062 increased probability of using any informal care, which is significant at p<.05.

Combining the two parts of the model, the marginal effect of the IPS restrictiveness measure is an increase of 0.24 informal care hours (about 15 minutes) per week, although this

estimate is not statistically significant (90% CI: −.0643 – .9740). While this marginal effect seems small, it is useful to compare a change from a relatively low level of IPS restrictiveness (−12) to a relatively high level of IPS restrictiveness (+12). This difference is approximately equivalent to comparing a one standard deviation difference in our measure of IPS restrictiveness above the mean with a one standard deviation difference below the mean. The effect of going from low IPS restrictiveness to high IPS restrictiveness for the low-income subsample is a 15% increase in the probability of using any informal care and an increase of 5.87 informal care hours per week. Relative to the .583 mean probability of using informal care and the 15 hours per week mean informal care use for the low-income population, this represents a 26% relative increase in the probability of using informal care and a 38% relative increase in informal care hours.

To put this finding in perspective, McKnight's findings imply that a change from the same levels of low IPS restrictiveness to high IPS restrictiveness would result in a decrease of 21 Medicare paid home care visits per year for low-income, high-predicted-costs beneficiaries. We are limited in our comparisons with McKnight's results because we used different units of measurement for the dependent variables (weekly hours vs. annual visits) and because our study populations are different. Nevertheless, we can bound the relative reduction in paid home care for low-income, high-predicted costs beneficiaries between a 41% and a 62% relative reduction in annual visits.[2] Even comparing the less conservative estimate of McKnight's relative reduction in paid home care with our estimate of the relative increase in informal care suggests considerable substitution between the two forms of care.

4.1. Sensitivity Analyses

The results from these analyses were largely robust to several alternative specifications and sample definitions (Table 4). We focused on the low-income subsample, as that is where the effects of the IPS are concentrated. When the analyses were conducted without sampling weights (column a), the marginal effect on the probability of using any informal care was reduced, while effect on total informal care hours increased, although neither effect is significant. The reduction in the marginal effect on the probability of using any informal care is not surprising, because the unweighted analysis includes individuals who were institutionalized at the time of the interview, and thus were not at risk for using any informal care.[3]

The marginal effects are qualitatively similar but weaker if the state time trend variables are excluded (column b), although our preferred specification includes these variables to control for any possible correlation between state trends in informal care and IPS cap restrictiveness. However, the marginal effect of IPS restrictiveness on any informal care use was still significant when state time trend variables were not used (marginal effect = .0022, 90% CI = .0003 – .0053). This finding is consistent with McKnight (2005), who also found weaker effects of IPS cap restrictiveness on paid home care use when state time trends were not included.

In another sensitivity analysis where the dependent variable was still non-spousal informal care, we included married individuals, and added a covariate for marital status (column c). The results from this model were qualitatively similar, but were smaller in magnitude and

[2]McKnight (2006) reports that high-predicted cost beneficiaries used an average of 34 home care visits/year, and that low-income beneficiaries used an average of 19.2 visits/year relative to 12.6 visits/year for all beneficiaries. To calculate a lower bound of average visits/year for low-income, high-predicted costs users we assume that this group has the same number of visits as all high-predicted cost beneficiaries. To calculate an upper bound of average visits/year for low-income, high-predicted costs users we assume that the home care visits are independently distributed across low-income and high-predicted cost individuals. This leads to an upper bound estimated average of 34 * (19.2/12.6) = 51.8 visits/year.

[3]Institutionalized individuals have sample weights of zero, and thus are not included in the original analyses

non-significant. This is a plausible result since married individuals rely on less paid home care and less non-spousal informal care than do unmarried individuals, and thus we would expect the effects to be attenuated for this group.

In a fourth sensitivity analysis, we expanded our low-income sample to include all observations in the lower half of the sample's income distribution (column d). The results from these analyses were somewhat weaker than for the sample of observations below the poverty line. The marginal effect of a one-unit increase in the IPS restrictiveness measure is a 0.0038 increased probability of using any informal care, which is significant at the $p<.10$ level (90% CI: .0007 – .0012). The marginal effect on total informal care hours is a non-significant increase of .107 hours.

In a final sensitivity analysis, we re-estimated the main model for the low-income subsample after restricting to observations that were ages 70 or older. In the main analysis, individuals ages 65–69 only enter into the sample starting in 1998. This sensitivity analysis assesses whether our main results are robust to using a consistent age group across all study years. Our point estimates in this analysis are very close to those from the main analysis, although they are estimated less precisely, possibly due to a smaller sample size.

5. Discussion and Conclusions

This research has used the introduction of the IPS to assess the effect of a change in Medicare payment policy for home health care services on informal care use. Although prior research has documented that the IPS dramatically reduced Medicare home health care use, there is no prior evidence of the IPS's effect on informal care.

We find evidence that IPS-induced changes in paid home care resulted in changes in informal care at the extensive margin for the overall population of older adults with functional limitations. After stratifying by income, we find no effects for the subset of that population that was above the federal poverty line. This finding may reinforce McKnight's conclusion that higher-income individuals did not fully value the Medicare home health services that were reduced by the IPS. However, we do find that low-income older adults were more likely to offset IPS-induced decreases in paid home care with additional informal care, although our estimate for the intensive margin is not precise. This finding appears consistent with McKnight's findings that the IPS had a disproportionately strong effect on lower-income beneficiaries.

We propose several potential explanations for this finding which are not mutually exclusive. First, individuals with greater financial resources replaced Medicare-funded home health care by paying for private home care services out-of-pocket, as McKnight (2006) observed. Second, the potential family caregivers of higher income individuals had higher opportunity costs of time, which made them less likely to deliver informal care. Third, prior research suggests that paid home care use increased disproportionately faster for higher-wealth individuals than lower-wealth individuals over the early to mid-1990's (Langa et al. 2001). If some of the marginal reductions in Medicare home health services that resulted from the implementation of the IPS were not fully valued by higher-income individuals, we would not necessarily expect that they would be substituted with informal care.

These findings provide further support for the hypothesis that individuals can and do substitute informal care for publicly-funded home care, at least to a certain extent. Our conservative estimates of the extent of substitution suggest that a 62% relative decrease in home health services led to a 26% relative increase in the probability of informal care use and a 38% relative increase in informal care hours. This is considerably greater than has been reported in recent research. For example, Van Houtven and Norton (2004) report that a

10% increase in informal care hours leads to a .87% decrease in the probability of using any paid home care. The difference between these results and our own may be explained because we include only individuals with functional limitations in our analyses, while Van Houtven and Norton included all unmarried respondents, many of whom may not have been at risk of using paid home care if they had no functional limitations.

Furthermore, our findings provide interesting insight into the distributional consequences of Medicare policies. In this case, a change in Medicare payments affected higher income families differently than lower income families. Our results suggest that lower income families without the immediate financial resources available to purchase home care services responded to the payment change by increasing time transfers to the care recipients. This response is of substantive interest, as there may be considerable opportunity costs associated with increased caregiving in the form of lost wages (Ettner 1996; Heitmueller, and Inglis 2007) or less time available invest in other family members, such as younger children. In addition, informal caregiving is associated with increased risks for mortality (Schulz and Beach 1999) and poorer physical and mental health for caregivers (Schulz et al. 1997).

This study has several limitations. Our sample only includes unmarried individuals. While this limits the study's generalizability, a focus on unmarried individuals is useful because it avoids data problems in the HRS/AHEAD associated with measuring informal care for delivered by spouses and because the proportion of the elderly population that is unmarried will grow in the coming decades due to increases in divorce rates. We also limited our analyses to individuals with functional limitations. Although this may also limit our generalizability, the focus on individuals with functional limitations is appropriate because this is the population most directly affected by long-term care policy.

We also are limited because we do not directly observe Medicare home care use with our data, which prevents us from explicitly calculating the level at which low-income individuals substituted informal care for Medicare home health services. We also cannot observe whether Medicare home care services were for medically-skilled services or for less-skilled help with functional limitations. This is relevant because the degree to which informal care and paid home care are substitutes likely varies depending on the comparability of the care that is delivered. In addition, the HRS/AHEAD only collected data from respondents every-other year over the study period. Our lack of more frequent data points may partially explain the lack of precision in our findings.

In spite of the limitations, this study is important in several respects. First, there is little research investigating the effects of Medicare home health care policy on informal care, even though Medicare is responsible for a substantial proportion of home care expenditures. This research shows that Medicare policies do have a considerable effect on informal care and that the effects of these policies vary with income level Even though policymakers may not institute a payment system that involves IPS-style payment caps for home health care again, plausibly exogenous sources of variation in publicly-funded home care generosity are rare, implying that our analyses may hold important lessons for future policy action. Finally, this study complements other research that suggests that the benefits of paid home care accrue not only to care recipients, but may also benefit potential and actual family caregivers. Policymakers should be careful to balance the financial consequences of changing publicly-funded home care generosity and/or payment systems with the effects that those changes may have on the informal care use of lower-income families.

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Figure 1.
Medicare Home Health Users per 1000 Beneficiaries

Figure 2.
Medicare Home Health Visits per Home Health User

Table 1

Sample Means

	Full Sample (n=6701)	Higher-Income Subsample (n=4533)	Low-Income Subsample (n=2168)
Any Informal Care	0.507	0.478	0.579
Informal Care Hours	13.29	12.43	15.43
Age	81.07	81.19	80.79
Male	0.187	0.206	0.140
Education (years)	9.98	10.80	7.97
Income ($)	17768	22658	5757
Black	0.130	0.100	0.204
Other Race	0.031	0.019	0.058
High Blood Pressure	0.617	0.596	0.668
Diabetes	0.185	0.172	0.218
Cancer	0.156	0.168	0.128
Lung Disease	0.156	0.150	0.169
Heart Disease	0.407	0.400	0.426
Stroke	0.189	0.187	0.194
Psychiatric Disorder	0.189	0.179	0.213
Arthritis	0.682	0.671	0.708
Dementia	0.176	0.157	0.224
# ADL Limitations	2.05	2.01	2.15
# IADL Limitations	1.41	1.34	1.56

Note: Sample statistics weighted by HRS/AHEAD sampling weights

Table 2

Two-Part Models of Informal Care Use: Coefficients and t-values

	Full Sample		Higher-Income		Low-Income	
	Probit: Any informal care	**OLS: Logged informal care hours**	**Probit: Any informal care**	**OLS: Logged informal care hours**	**Probit: Any informal care**	**OLS: Logged informal care hours**
Restrict*PostBBA	0.0081 (1.62)	−.0011 (−.12)	.0037 (.54)	−.0029 (−.28)	.0228 (1.80)	.0034 (.30)
Age	.0109 (3.49)	.0017 (.14)	.0134 (3.11)	−.0021 (−.36)	.0095 (1.85)	.0061 (.79)
Education	−.0286 (−3.82)	−.0234 (−2.84)	−.0377 (−5.15)	−.0276 (−3.67)	−.0132 (−1.02)	−.0177 (−1.02)
Income ($1,000's)	.00004 (.06)	.0006 (.82)	.0007 (.94)	.0007 (1.03)	−.02 (−1.22)	.03 (1.20)
Male	−.2260 (−4.18)	−.1310 (−1.63)	−.2511 (−3.63)	−.2971 (−3.18)	−.1341 (−1.49)	.2226 (1.08)
Black	.0194 (.29)	.1379 (2.52)	.0582 (.57)	.2100 (3.13)	−.0769 (−.88)	.0359 (.33)
Other Race	.0149 (.07)	.3418 (2.72)	.1361 (.51)	.0310 (.13)	−.0211 (.10)	.4611 (2.69)
Hypertension	.0920 (2.16)	.1617 (3.00)	.0961 (1.71)	.1439 (2.05)	.0990 (1.69)	.2304 (2.10)
Diabetes	.1771 (2.65)	.1058 (1.45)	.2137 (2.54)	.0810 (0.66)	.1277 (1.01)	.2243 (2.64)
Cancer	−.1285 (−1.74)	.0489 (.60)	−.0537 (−.63)	.0106 (.12)	−.3332 (−3.28)	.2151 (1.62)
Lung Disease	−.0185 (−.23)	.0413 (.59)	−.0353 (−.40)	.0936 (1.00)	.0142 (.10)	−.0741 (−.51)
Heart Disease	.0084 (.22)	.0048 (.06)	.0254 (.56)	−.0657 (−.76)	−.0093 (−.11)	.1367 (.88)
Stroke	.1118 (1.39)	.1368 (1.80)	.0612 (.62)	.1704 (1.69)	.2825 (2.27)	.0477 (.45)
Psychiatric	.0390	−.1971	.0588	−.2293	−.0254	−.1351
Disorder	(.39)	(−3.62)	(.54)	(−2.67)	(−.19)	(−1.03)
Arthritis	−.0067 (−.11)	−.0442 (−.65)	−.0056 (−.08)	−.0415 (−.56)	.0126 (.14)	−.0943 (−1.01)
Dementia	.0224 (.29)	.3085 (5.43)	.1013 (1.06)	.3654 (4.70)	−.1016 (−1.04)	.1874 (1.61)
# ADL	−.0286	.1093	−.0149	.1200	−.0562	.0719
Limitations	(−1.78)	(7.06)	(−.76)	(5.79)	(−2.26)	(1.98)
# IADL	.7437	.2829	.7963	.2647	.6831	.2906
Limitations	(14.48)	(12.90)	(17.33)	(9.26)	(9.21)	(6.93)

Note: Regressions include state and year fixed effects and state linear time trends. Robust standard errors are clustered on the state.

Table 3

Marginal Effects from Two-Part Models

	Marginal Effect	Bootstrapped, bias-corrected 90% CI[a]	Shifting from a low restrictiveness state to a high restrictiveness state[b] results in…
Full Sample			
Pr(any informal care)	.0021**	.0004 – .0052	5.0% increased probability of informal care use
E(informal hours)	.0174	−.2920 — .3111	.42 increased informal care hours/week
Higher-Income Sample			
Pr(any informal care)	.0009	−.0018 — .0054	2.2% increased probability of informal care use
E(informal hours)	−.0293	−.3205 — .3585	.70 decreased informal care hours/week
Low-Income Sample			
Pr(any informal care)	.0062**	.0015 – .0163	14.9% increased probability of informal care use
E(informal hours)	.2446	−.0643 — .9740	5.87 increased informal care hours/week

*p<.10

**p<.05

[a] Confidence intervals are clustered at the state level

[b] Low restrictiveness is approximately one standard deviation below mean restrictiveness (−12), and high restrictiveness is approximately one standard deviation above the mean (+12)

Table 4

Marginal Effects and 90% Confidence Intervals[a] from Sensitivity Analyses

	a. Low-income sample without weights	b. Low-income sample without state time trends	c. Married & unmarried individuals below the poverty line	d. Lower 50% of income (unmarried only)	e. Low-income sample restricted to ages 70 and older
Pr(any informal care)	.0007 (−.0022 – .0094)	.0022* (.0003 – .0053)	.0041 (−.0010 – .0090)	.0038* (.0007 – .0120)	.0071* (.0014 – .0163)
E(informal hours)	.2102 (−.1019–1.964)	.1058 (−.0801 – .3186)	.1751 (−.2998 – .6387)	.1066 (−.1774 – .6417)	.1977 (−.3469 – .7271)
N	2354	2035	2469	3146	1838

*p<.10

**p<.05

[a]Confidence intervals are bias-corrected, based on 500 bootstrap replications, and clustered at the state level