ESSAYS ON LONG-TERM CARE AND AGING

by

Ezra Golberstein

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Doctoral Committee:

Professor Richard A. Hirth, Chair
Professor Paula M. Lantz
Professor Robert F. Schoeni
Associate Professor Kenneth M. Langa
Professor Michael E. Chernew, Harvard University

To the memory of my Grandfather,
who taught me to pursue my own ideas and enabled those pursuits by supporting my education;

And to Sarah,
for her love.

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Abstract

This dissertation contains three empirical papers on important health policy issues. The first paper looks at the effect of changes in levels of Medicare home health care services on the informal care use of disabled, older adults. I estimate two-part models of informal care use, where the key independent variable is a measure of Medicare home health payment restrictiveness. Individuals who lived in states with less Medicare home health care services increased their use of informal care, although this effect is only observed among low-income individuals.

The second paper assesses the effects of changes in employment incentives on the supply of informal support from adult children to their disabled, older parents. This study focuses on one specific form of informal support, co-residence with a disabled parent. I compare changes in co-residence patterns between 1990 and 2000 across groups of middle aged women whose co-residence patterns were arguably comparable, and who experienced very different changes in employment incentives. Results from difference-in-difference models provide support to the hypothesis that increasing employment incentives reduces the supply of informal support to disabled parents.

The third paper looks at the effect of education on health status. This paper builds on the extensive research literature on this topic by measuring health in terms of the number of Quality-Adjusted Life Years experienced by individuals over an extended period of time. This measure captures overall health status over time, which is arguably a more important measure than point-in-time measures of specific health conditions or longevity alone. I use changes in compulsory schooling laws to identify the causal effect of education on health. Instrumental variables models provide evidence that the health returns to education are both statistically significant and substantial.

Chapter 1

Introduction

This dissertation is comprised of three empirical research papers on the effects of diverse policies and factors on important health policy outcomes. Several threads tie these three papers together. First is the prevailing context of population aging. Two of the following papers deal with long-term care, which is a focal point of concern as the U.S. population grows older. Long-term care is especially interesting for several reasons. One of those reasons is the sheer magnitude of the issue, in terms of the public and private costs associated with long-term care, and in terms of the number of people who will require long-term care directly or may deliver long-term care directly. Another reason is that the nature of the technology of long-term care is such that it can be provided in a formal health care setting or informally in the community, which raises interesting questions about the optimal way to organize and create incentives for long-term care.

Second is the importance of establishing causal inference in policy research. While it is easy to applaud the virtues of implementing policies that are informed by research that identifies their probable causal effects, composing research designs that identify causal effects from observational data is a major challenge. All three dissertation papers employ quasi-experimental research designs where the treatment and control groups are defined by policy changes that created variation in the independent variable of interest that was plausibly unrelated to the outcome of the study. Exogenous variation in a variable of interest can be quite difficult to come by when randomized trials are unavailable. To wit, in two of my papers, I import sources of variation in the independent variable that are most commonly used in the empirical labor economics literature.

Third is an expansive view of health policy. Health services and health policy researchers generally focus on questions about variables that pertain directly to health or the health care system. I contend that non-health factors, policies, and trends may hold major import for health and the health care system, and that some important outcomes of health policies go beyond health status and the health care system per se.

My first paper (which is co-authored with David Grabowski, Ken Langa, and Mike Chernew) is entitled "The Effect of Medicare Home Health Care Payment on Informal Care Use." In this paper, we evaluate the effect of a major change in Medicare home health care policy on an important outcome that exists outside of the formal health care system: informal care use. An important contribution of this paper is that it deals specifically with the way society and the health care system approach long-term care, which is one of the biggest health policy concerns associated with population aging. But this paper also speaks to some of the most fundamentally interesting and important issues in health policy. For instance, how do different payment mechanisms affect the patterns of care that individuals receive? To what extent does the public provision of health care services "crowd-out" the private provision of health care services? And what are the distributional consequences of different health policies?

We employ a research design that exploits variation in the restrictiveness of Medicare home health payment after the policy change. Prior research has found that individuals who lived in states with more restrictive home health care payments after the policy change received significantly less paid home care. Our results suggest that lower-income individuals who lived in more restrictive states did offset reductions in publicly-funded home care by using more informal care.

The second paper is entitled, “The Effect of Women’s Employment Incentives on Co-Residence with Older, Disabled Parents.” This paper follows from the first paper in terms of its substantive focus on policy issues related to supporting older adults with disabilities. But, whereas the first paper looked at policies that changed the supply of formal long-term care services, this paper examines the effects of policies that may change the supply of informal supports to disabled older adults. My research question is, how do policies that increase employment incentives for middle-age adult children affect the informal support that they deliver to their disabled parents? Although the relationship between employment and delivering informal support has received a lot of attention in the research literature, there have been no studies that have utilized a research design that captured plausibly exogenous variation in the employment incentives of individuals who may also need to support their disabled parents.

This is an important issue for health and social policy, both in the U.S. and internationally. Policymakers may experience pressure to enact policies to encourage employment and also to encourage the delivery of informal support to disabled older adults. However, policies that address one of those policy goals may have the unintended consequence of undermining the other policy goal. This relates to two points that I wish to advance as a health policy researcher. First, health policy research may benefit from taking a more expansive perspective that encompasses non-health factors that are potentially important for the health care system. And second, that social policies may have important (and possibly unintended) consequences for health and health policy.

In this paper, I look at one form of informal support: co-residence with older, disabled parents. I assess whether policies that encourage employment, such as the Earned Income Tax Credit and welfare reforms, affect the probability that a middle-aged child lives with a disabled parent. My research design takes a difference-in-difference approach and compares groups of women who experienced major changes in employment incentives over the 1990s. My findings suggest that women who were faced with increased incentives to work were significantly less likely to co-reside with a disabled parent.

My third paper (co-authored with Paula Lantz) is entitled, "The Effect of Education on Health: A Cost-Utility Approach." This paper shifts focus from the previous two and looks at a different health policy issue: the social determinants of health status. Nevertheless, this paper shares a theme with the prior paper, which is that social policies and factors may have important consequences for health policy. We focus on education as one specific social factor that may influence health. This is a timely contribution, as many commentators suggest that increasing or improving education could be one of the most effective and feasible ways to improve population health. However, policy decisions about investing in education to improve population health should be at least partially informed by evidence on the health returns to education and on the cost-effectiveness of using education to improve population health.

This paper aims to improve the evidence base on the health returns to years of education. Specifically, we combine survey data from a 15-year longitudinal study with detailed mortality data to assess the effects of an additional year of education on the number of Quality-Adjusted Life Years experienced over a 15-year period of time. To identify the causal effect of education on health, we utilize changes in compulsory schooling laws over the 20th century as a natural experiment. Our instrumental variables analyses indicate that an additional year of schooling does significantly increase the number of Quality-Adjusted Life Years experienced. Furthermore, our estimates imply that there are considerable health returns to education (relative to the income returns to education), and that increasing education may in fact be a cost-effective intervention to improve population health (relative to other medical interventions).

Chapter 2

The Effect of Medicare Home Health Care Payment on Informal Care

2.1. 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).

Home care is a major component of the long-term care continuum for older adults with functional limitations. 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. Recent major policy initiatives and demonstrations that support the use of paid home care as a potential substitute for institutional long-term care illustrate the importance of home care in the context of the demographic and disability trends that are affecting the long-term care system. For example, 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).

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). Policymakers have already acted to address the 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. Our results suggest that policies that change the generosity of publicly-funded home care have important consequences for informal caregivers. Low-income individuals who experienced more restrictive Medicare home health payment caps moderately increased their informal care use, although this effect is not estimated precisely and is not observed for higher-income individuals. These findings have important implications for publicly-funded home care policy. They suggest that the benefits of publicly-funded home care accrue to potential informal caregivers, along with the care recipients. Also, reducing publicly-funded home care may have particularly strong effects on lower-income families due to the high opportunity costs of caregiving.

2.2. Background

The relationship between paid home care and informal care and the extent to which the two forms of care are substitutes is a central issue for home care policy. Policymakers face a potentially difficult tradeoff. If a decision to increase the availability of publicly-funded home care results in individuals substituting the publicly-funded care for informal care that would otherwise have been provided by family or friends, that could be considered a misallocation of public resources. On the other hand, substitution could be seen as a beneficial outcome if it allows caregivers to be relatively more productive in other ways, such as by participating in the labor force. Substitution may also have the additional benefit of reinforcing informal caregiving roles by providing support and respite to caregivers. Conversely, if publicly-funded home care were cut and families responded by substituting with informal care, the resultant cost savings should be weighed against the extent to which any increased burden of informal caregiving is borne by the family and the opportunity costs of the family's time.

2.2.1. Previous literature

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 is complicated, however, because of the endogeneity and simultaneity of paid home care and informal care. Changes in one form of home care use can be caused by changes in the other, and it is also possible that any changes in both paid and informal care are the result of some other, unobserved factors. These complications make it difficult to assess any casual relationships between paid and informal home care.

A few studies have addressed these methodological challenges. 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. For example, researchers have used family structure characteristics (e.g., number of children and birth order of daughters) to instrument for the supply of informal caregivers. They find that increases in informal care use result in statistically significant but modest decreases in the probability of using paid home care and in Medicare home health expenditures (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. Other recent research uses Canadian data to estimate the effect of publicly-funded home care on informal care more directly (Stabile, Laporte, and Coyte 2006). That research 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. However, their 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.

2.2.2. 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 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 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.

2.3. Data and methods

2.3.1. 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 representing individuals who 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.

2.3.2. 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).

2.3.3 Identification strategy and empirical specification

To identify the effect of Medicare home health generosity on informal care use while avoiding problems of endogeneity, 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

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[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.

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:

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

We estimate a reduced form equation for weekly informal care hours ($\text{Informal}_{\text{ist}}$). 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 $\text{Restrict*PostBBA}_{\text{st}}$, 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. 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.

2.3.4. 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, because conventional estimation of these statistics is not possible when combining both parts of the two-part model (Cameron and Trivedi 2005, chapter 11). Those statistics are derived empirically by approximating their distributions from 1000 bootstrap samples, which are clustered at the state level to adjust for observations that are correlated at the state level and at the individual level (most individuals do not change their state of residence). All analyses are conducted using the HRS/AHEAD sampling weights.

2.4. 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 be more likely to use informal care due to the availability of spousal support.

Table 2 displays 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 stratifying 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 as in the full sample, the effect on total informal care hours was negative, and 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 34% 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.

2.4.1. 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

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[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 10.7 visits/year for higher-income 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 * (19.2/10.7) = 61.24 visits/year.

were conducted without sampling weights (column a), the marginal effect on the probability of using any informal care was reduced, while the 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). 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 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 final 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

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

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.

2.5. Discussion and Conclusions

This research has assessed the effect of a change in Medicare payment policy for home health care services on informal care use. It is particularly important to understand the consequences of payment policies for publicly-funded home care because such policies may be appealing loci for interventions to control long-term care expenditures. Such interventions should be informed by an understanding of the probable effects on all relevant outcomes and of the potential distributional consequences. One such intervention was the introduction of the IPS to rein in rapidly rising Medicare home health expenditures. 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 stronger 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.

A notable finding from our two-part models is that more restrictive IPS payment caps had a clearer effect on the probability of using any informal care than on the number of hours of informal care used, conditional on using any care. One possible explanation for this finding is that the individuals who replaced Medicare home care with informal care due to the policy change had been totally reliant on Medicare home health services and used no informal care. If these were potentially high-cost users who home health agencies avoided after the IPS was implemented, it is possible that they substituted informal care only along the extensive margin. However, this is only one possible explanation. The interpretation of this finding is complicated because changes in the composition of both Medicare home health and informal care users may have resulted in changes in average intensity of home care use in ways that are difficult to assess.

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. In fact, although we cannot provide an accurate estimate of the amount of substitution, even our conservative estimates suggest considerably greater substitution between paid home care and informal care 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.

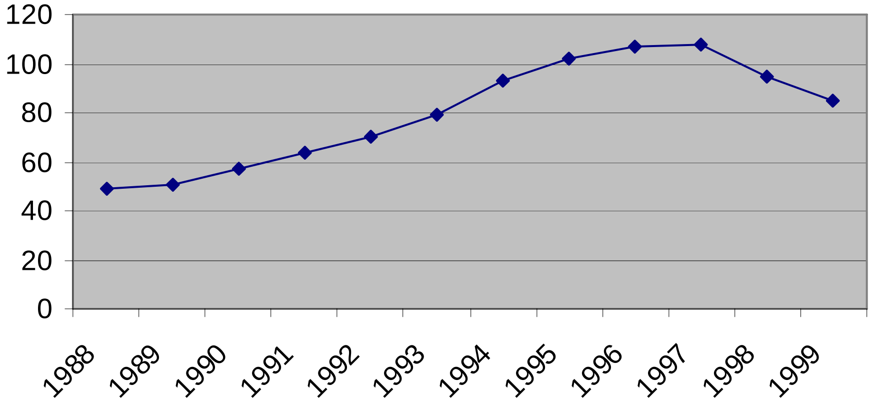
Furthermore, our findings provide interesting insight into the distributional consequences of Medicare policies. In this case, a change in Medicare payments affected higher income families differently than lower income families. Our results suggest that lower income families without the immediate financial 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 to 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 (Schulz et al. 1997) for caregivers.

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 the data problems in the HRS associated with measuring informal care 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. Another issue is the generalizability of the IPS experience. On the one hand, policymakers may not institute a payment system that involves IPS-style payment caps for home health care again. But on the other hand, plausibly exogenous sources of variation in publicly-funded home care generosity that affect the whole country (IPS restrictiveness was not localized in any geographical region) are rare, which implies that these analyses may hold important lessons for future policy actions.

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. We hope to address these two limitations in future research. 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. Future research using more detailed home care data may be able to produce more precise estimates of the extent of substitution between paid home care and informal care.

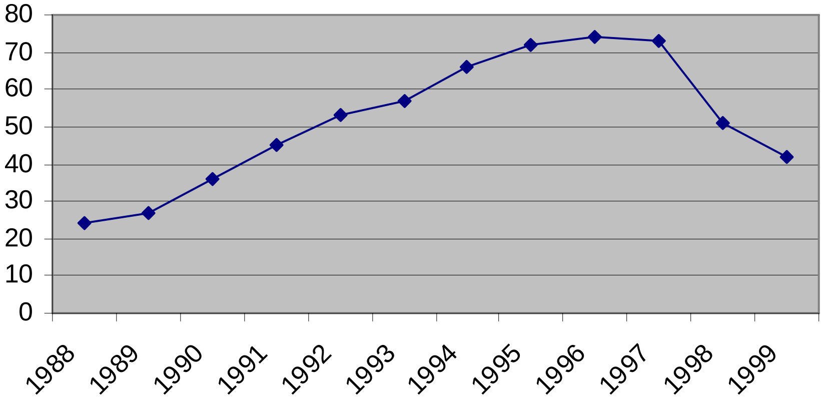
In spite of the limitations, this study is important in several respects. First, it further confirms the qualitative conclusion that there is substitution between paid home care and informal care. Second, it complements other research that suggests that the benefits of paid home care accrue not only to care recipients, but may also benefit potential and actual family caregivers. And third, 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. Policymakers should consider these heterogeneous effects when designing future home care policy. This research is not able to estimate overall welfare effects of the IPS, even though that is a central issue for designing publicly-funded home care policies. Estimating potential welfare changes involving informal care is complicated because the overall social costs and benefits of informal caregiving are not known. Future research that assesses the overall societal value of informal caregiving is needed to inform such analyses.

Figure 1: Medicare Home Health Users per 1000 Beneficiaries



2001 Health Care Financing Review Statistical Supplement

Figure 2: Medicare Home Health Visits per Home Health User



2001 Health Care Financing Review Statistical Supplement

Table 2.1. 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 2.2. 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	-.0011	.0037	-.0029	.0228	.0034
	(1.62)	(-.12)	(.54)	(-.28)	(1.80)	(.30)
Age	.0109	.0017	.0134	-.0021	.0095	.0061
	(3.49)	(.14)	(3.11)	(-.36)	(1.85)	(.79)
Education	-.0286	-.0234	-.0377	-.0276	-.0132	-.0177
	(-3.82)	(-2.84)	(-5.15)	(-3.67)	(-1.02)	(-1.02)
Income ($1,000's)	.00004	.0006	.0007	.0007	-.02	.03
	(.06)	(.82)	(.94)	(1.03)	(-1.22)	(1.20)
Male	-.2260	-.1310	-.2511	-.2971	-.1341	.2226
	(-4.18)	(-1.63)	(-3.63)	(-3.18)	(-1.49)	(1.08)
Black	.0194	.1379	.0582	.2100	-.0769	.0359
	(.29)	(2.52)	(.57)	(3.13)	(-.88)	(.33)
Other Race	.0149	.3418	.1361	.0310	-.0211	.4611
	(.07)	(2.72)	(.51)	(.13)	(.10)	(2.69)
Hypertension	.0920	.1617	.0961	.1439	.0990	.2304
	(2.16)	(3.00)	(1.71)	(2.05)	(1.69)	(2.10)
Diabetes	.1771	.1058	.2137	.0810	.1277	.2243
	(2.65)	(1.45)	(2.54)	(0.66)	(1.01)	(2.64)
Cancer	-.1285	.0489	-.0537	.0106	-.3332	.2151
	(-1.74)	(.60)	(-.63)	(.12)	(-3.28)	(1.62)
Lung Disease	-.0185	.0413	-.0353	.0936	.0142	-.0741
	(-.23)	(.59)	(-.40)	(1.00)	(.10)	(-.51)
Heart Disease	.0084	.0048	.0254	-.0657	-.0093	.1367
	(.22)	(.06)	(.56)	(-.76)	(-.11)	(.88)
Stroke	.1118	.1368	.0612	.1704	.2825	.0477
	(1.39)	(1.80)	(.62)	(1.69)	(2.27)	(.45)
Psychiatric Disorder	.0390	-.1971	.0588	-.2293	-.0254	-.1351
	(.39)	(-3.62)	(.54)	(-2.67)	(-.19)	(-1.03)
Arthritis	-.0067	-.0442	-.0056	-.0415	.0126	-.0943
	(-.11)	(-.65)	(-.08)	(-.56)	(.14)	(-1.01)
Dementia	.0224	.3085	.1013	.3654	-.1016	.1874
	(.29)	(5.43)	(1.06)	(4.70)	(-1.04)	(1.61)
# ADL Limitations	-.0286	.1093	-.0149	.1200	-.0562	.0719
	(-1.78)	(7.06)	(-.76)	(5.79)	(-2.26)	(1.98)
# IADL Limitations	.7437	.2829	.7963	.2647	.6831	.2906
	(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 2.3. 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 2.4. 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)
Pr(any informal care)	.0007 (-.0022 - .0094)	.0022* (.0003 - .0053)	.0041 (-.0010 - .0090)	.0038* (.0007 - .0120)
E(informal hours)	.2102 (-.1019 – 1.964)	.1058 (-.0801 - .3186)	.1751 (-.2998 - .6387)	.1066 (-.1774 - .6417)
N	2354	2035	2469	3146

*p<.10 **p<.05

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

References for Chapter 2

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Chapter 3

The Effects of Women's Employment Incentives on their Co-residence with Older, Disabled Parents

1. Introduction

As population aging progresses, determining how to support older adults with disabilities becomes a more pressing concern. Older adults with disabilities generally require some form of long-term care, which can be very expensive. For example, nursing home and home health care already account for 8.4% of national health care expenditures (Catlin et al. 2008). Long-term care expenditures have special policy importance because the majority of long-term care is publicly-funded, and policymakers are concerned about the solvency and sustainability of publicly-funded health care programs like Medicare and Medicaid.

Population aging also raises questions about how families will support disabled older adults. The majority of long-term care is delivered informally by family members. However, increases in the proportion of older adults without spouses and lower fertility rates among the baby boomer cohorts call into question whether the family can and/or will continue to be the primary source of long-term care.

This brief discussion serves to highlight some of the issues and questions that loom for policymakers. In order to develop optimal policies for supporting disabled older adults, policymakers need to answer normative and empirical questions. Designing optimal policies involves answering the normative question of how much of the burden of supporting disabled older adults should be borne by the family versus the public. This relates to an equally important empirical question of how different policies and trends interact to affect the extent to which the burden of caregiving is borne privately by families or by public health care systems like Medicare, Medicaid, and state and local programs. Because long-term care that is provided by the family can (to a certain degree) substitute for long-term care provided by the health care system (and vice versa), policies and trends that affect family caregiving will have important consequences for the health care system (and vice versa).

More generally, this discussion suggests that trends and policies that focus on social and/or economic factors may have important implications for health policy. To the extent that social and economic factors affect health policy, policymakers must consider the possibility that social and economic policies may have unintended health policy consequences. One of the goals of this paper is to evaluate whether social policies had inadvertent effects on an important health policy outcome: the supply of informal supports to disabled older adults.

In this paper I focus on one important factor that affects patterns of intra-family support for disabled older adults: women's employment. Specifically, I estimate the effect of women's employment incentives on their probability of co-residing with an older, disabled parent. Co-residence with a disabled parent is a key form of informal support, and may also be a reasonable proxy measure of informal caregiving. This empirical analysis uses data from the 1990 and 2000 US Census to exploit the dramatic shifts in patterns of women's labor force participation over the 1990s. Certain groups of women experienced major increases in incentives to work (and disincentives for not working) over the 1990s due in significant part to a combination of changes in income tax policies, and changes in welfare policies. Because these policy changes were plausibly uncorrelated with the probability of co-residing with disabled parents, I use this source of variation to identify the effect of increased employment incentives on co-residence.

Evidence from difference-in-difference and triple-difference models support the conclusion that increases in women's employment incentives over the 1990s resulted in a decreased probability of co-residing with a disabled parent, although I am unable to decompose whether this is due to substituting time spent in informal support with time spent working, or whether this is due to increases in income. I find that single women with children experienced a reduction in the probability of co-residing with a disabled parent between 1990-2000, relative to single women without children. This specification of treatment and control groups is used because expansions of the Earned Income Tax Credit over the 1990s went largely to single-parent families with children, and because welfare reforms had their greatest effect on AFDC recipients, who were largely single-parent families with children. I also find that this result is driven by lower-skilled women, who were most strongly affected by the pro-labor force policies of the 1990s. Although the effects that I detect are small in an absolute sense, they correspond to large relative effects on co-residence with disabled parents. Furthermore, the effects become more meaningful when compared with data from other studies on the proportion of women in this age group who are at risk of co-residing with a disabled parent.

The rest of the paper is organized as follows. Section two provides detailed background on trends in old age disability and general social and economic trends that may potentially have important implications for caregiving and living arrangements of older adults with disabilities. Section three reviews the literature on the relationship between employment and delivering informal support to disabled older adults. Section four describes the conceptual model of decisions to co-reside with disabled parents. Section five describes the empirical methods and econometric specification. The results are described in section six, and section seven concludes.

3.2. Background

3.2.1. Population Aging and Long-Term Care

As the baby boomer generation grows into old age, the absolute number of Americans who are 65 and older and the size of that age group relative to the general population will reach unprecedented magnitudes. Currently, persons aged 65 and older represent 12.4% of the US population, and this group is projected to represent 20% of the US population in 2030 (U.S. Department of Health and Human Services 2006). This demographic shift raises major concerns about rapidly expanding expenditures for the health and long-term care (LTC) of older adults, much of which is publicly-financed through Medicare and Medicaid.

The Congressional Budget Office (1999) estimated that LTC spending for the elderly would be $123 billion in 2000 and would rise to $207 billion by 2020. More recent estimates find that LTC accounted for 8.4% of total US health care expenditures in 2006 (Catlin et al. 2008) and that real LTC expenditures increased by 21% from 2000 to 2006 (author's calculation from Catlin 2008). It is also notable that home health care has been the category of national health expenditures with the greatest annual growth for each year since 2004 (Catlin et al. 2008). Along with being concerned about overall LTC costs, policymakers are also concerned that these costs are disproportionately borne by public budgets. For example, LTC comprised 35% of Medicaid expenditures in 2003 (Holahan and Ghosh 2005).

Researchers have speculated about whether population aging will cause sustained increases in LTC use in the future and whether these increases will overburden publicly-financed systems of support. Some research suggests that recent declines in disability might prevent health and long-term care costs from becoming excessively high (Cutler 2001). However, other research questions whether disability improvements will necessarily translate to reduced health and LTC costs (Congressional Budget Office 1999; Spillman 2004) and also whether recent improvements in disability will indeed be sustained in the future (Lakdawalla, Bhattacharya, and Goldman 2004). Uncertainties over the extent of the effects of disability trends on health care and long-term care costs notwithstanding, it will be very important for policymakers to understand the factors that affect the health and LTC use of older adults.

A shortcoming of the aforementioned data on long-term care use and expenditures is that they do not include informal sources of support for disabled adults that are delivered from within the family. Informal help is assistance to disabled older adults that is unpaid and is generally delivered by family and friends of the care recipient. This assistance encompasses a broad set of activities, including personal care (e.g., helping with bathing or dressing), helping with housework, and helping to manage finances and medical services. A few points about informal help are particularly important. First, informal care is very prevalent, and care delivered by spouses and children are the most common forms of long-term care. In 1999, 66% of older adults with functional limitations used some unpaid, informal care (which was largely delivered by family members), while only 25% used paid (or formal) sources of LTC (Spillman and Black 2005). Second, it is widely assumed disabled older adults much prefer receiving informal care than formal LTC services, although it is not known whether the quality of informal care is better or worse than paid home care services. Third, the economic value of informal care is substantial, as researchers estimate that the total value of informal care is between half and 1.7 times as much as total expenditures for paid LTC (Arno, Levine, and Memmott 1999; Johnson 2007). In addition, informal care from children has been demonstrated to significantly decrease the use of paid LTC services (Lo Sasso and Johnson 2002; Van Houtven and Norton 2004, 2007).

Fourth, adult children of older adults with functional limitations are a major source of informal support. In 1999, 41% of all primary informal caregivers for older adults were children of the care recipient, up from 36% in 1989 (Wolff and Kasper 2006). In addition, 57.5% of child caregivers were 54 years old or younger in 1999, suggesting that caregiving might often conflict with employment or household responsibilities that are common in middle ages (Spillman and Black 2005). Finally, there are major gender differences in informal caregiving. Sixty-seven percent of all primary caregivers are female, and 73% of primary caregivers who are children of the care recipient are females (Wolff and Kasper 2006).

One of the most important ways that adult children support older parents with functional limitations is through co-residing in the same household. Co-residence may make the delivery of informal care more efficient, both by reducing the fixed costs of travel time to a parent's residence, as well as through possibilities for joint production of normal household tasks with helping parents (for example, in cooking and cleaning for the entire intergenerational household). Data exist which support the notion that co-residence with adult children is a common and important source of support for older adults with functional limitations. In 1999, 22% of disabled adults ages 65 or older co-resided with an adult child, and 51% of child primary caregivers co-resided with their disabled parent (Wolff and Kasper 2006). In addition, in absence of data on actual informal caregiving, co-residence with an older parent with functional limitations may be used as a proxy variable for informal caregiving (for example, see (Schoeni 1998)). For example, data from the 1989 National Long Term Care Survey (NLTCS) show that older adults with disabilities who lived with an adult child received an average of 26 hours of informal care per week from their adult children (compared to 3 hours per week for those who lived independently) (Hoerger, Picone, and Sloan 1996). More recent data from the Health and Retirement Study (HRS) indicate that unmarried, disabled older adults who live with an adult child received an average of 230 hours of informal care per month (compared to 88 hours per month for those who lived independently) (Johnson and Weiner 2006).

Despite the importance of co-residence in delivering informal support to disabled parents, older adults have become far less likely to co-reside with their children. In 1900, the average 65-year-old women spent 63% of her remaining years in co-residence with a child. This percentage dropped to 47% by 1940, and was down to 16% in 1990 (Schoeni 1998). It is important to consider the various social, economic, and demographic trends that may affect both informal caregiving arrangements and the frequency with which adult children co-reside with their older, disabled parents.

3.2.2. Trends Affecting Informal Support

One important trend that may affect the supply of informal support to the disabled elderly is the reduced fertility of the Baby Boom generation, relative to prior generations (Dye 2005). This trend implies a future reduction in the supply of children as sources of informal support for older Baby Boomers. This is significant because prior research finds that having more children increases the amount of informal care used by older adults, which substitutes for paid home care use and nursing home use (Van Houtven and Norton 2004).

Another important trend is the number of older adults with a spouse, which includes trends of the number of older adults who are widowed and the number of older adults who are divorced. On the one hand, mortality reductions among older adults may decrease the number of older adults who are widowed, which could lead to a reduced demand for nursing home care (Lakdawalla and Philipson 2002). On the other hand, divorce rates among the Baby Boom generation have significantly increased. As the proportion of older adults who are unmarried is projected to rise over the next several decades (Johnson, et al. 2007), there may be a reduction in the supply of spousal caregivers. This would increase the demand for informal support from children. Data from the 2002 HRS show that 64% of unmarried, disabled older adults receive informal care from a daughter, compared to only 20% of married, disabled older adults (Johnson and Weiner 2006). In addition, increases in unmarried older adults could lead to increased demand for paid long-term care services, particularly if accompanied by lower fertility which leads to a reduced supply of children.

Another relevant trend is the increased real income of older adults. The median real income (expressed in 2000 dollars) for individuals aged 65 and over increased from $15,000 in 1976 to $19,000 in 2000 (Hungerford et al. 2003). Increased income has an ambiguous effect on the demand for informal care. On the one hand, higher income may indirectly decrease the demand for informal care by increasing the demand for paid home health services that allow the individual to maintain an independent living arrangement. But on the other hand, more income may directly increase the demand for informal care from children if that care is preferred to paid home care. Also, it may increase the supply of informal care if children are motivated to deliver informal care by a potentially larger bequest (although the results of empirical research in this area are mixed (see Norton and Van Houtven 2006 for an example and review)). Furthermore, recent research has found that independent living is a normal good for older adults and is very sensitive to income (Englehart, Gruber, and Perry 2005), although it is not clear whether increases in independent living are due to the effect of income on disability or on the ability to pay for support to maintain independent living.

Trends within the children of the Baby Boom generation may also have important effects on informal caregiving and living arrangements for older adults with functional limitations. First, starting with the Baby Boom generation and continuing through subsequent cohorts, women have significantly delayed their childbirth relative to earlier generations. This implies that more women will have child care responsibilities into their late-middle ages. Thus, the number of women with child care responsibilities and concurrent demands of supporting older parents with functional limitations is expected to grow. In fact, in 1994, 22% of women with at least one child younger than 15 were potential caregivers (Spillman and Pezzin 2000). These women may be less likely to supply informal supports to disabled parents if they have competing demands of caring for their families through paid employment or through working in the household.

Second, and relatedly, women's employment has dramatically increased over the past several decades. A number of factors have driven this increase, including changing norms of gender roles, increases in single woman-headed households, increased educational attainment, reduced and delayed fertility (Blau 1998), as well as more recent changes in tax incentives and welfare policies (Meyer and Rosenbaum 2001). Furthermore, promoting women's labor force participation continues to be a major policy goal in the US, and may even be a greater priority in other countries with higher unemployment (e.g., the European Union's Lisbon Agenda[4]). Increases in women's employment may have consequences for whether or not women deliver informal support to their disabled parents, since a significant proportion of working-age women may have a parent in need of informal support. In 1994, 48% of women who worked at least 30 hours per week were potential caregivers (Spillman and Pezzin 2000). The focus of this paper is the potential effects of employment on the delivery of informal support to their disabled parents. I review the potential implications of women's increased labor force participation in my conceptual model below.

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[4] http://ec.europa.eu/growthandjobs/faqs/background/index_en.htm

3.2.3. Prior Research on Employment and Informal Support

Many researchers have studied the effects of caregiving and co-residing with older disabled parents on employment outcomes. A recent systematic review of this research literature concludes that caregiving is not a clear predictor of labor force participation, although co-residence with the care recipient and more intensive caregiving do appear to be associated with reduced labor force participation (Lilly, Laporte, and Coyte 2007). However, a major complication in this area of research is the endogenous and simultaneous relationship between employment and caregiving or co-residence with a disabled parent. In addition, there may be unobserved variables that are correlated with both employment and caregiving decisions. This suggests that the findings from empirical studies which adjust for endogeneity deserve closer attention.

Those studies that address endogeneity generally find that caregiving reduces employment and/or hours worked, although the estimates of these effects vary considerably and may be moderated by the gender of the potential caregiver, intensity of caregiving, and by whether the caregiver co-resides with the care recipient (Ettner 1996; Heitmueller 2007; Wolf and Soldo 1994). Heitmueller (2007) takes two approaches to identify the effect of caregiving on employment: cross-sectional instrumental variables (IV), and panel data fixed effects models. Both approaches find that co-residential caregiving reduces employment more than extra-residential caregiving. Ettner (1996) uses IV methods and finds that giving informal care to a non-resident parent reduces women’s employment by over 12 hours per week, while giving informal care to a co-residing disabled parent reduces women’s employment by over 27 hours per week (although the latter estimate is not statistically significant). Bolin and colleagues (Bolin, Lindgren, and Lundborg 2008) find that co-residential caregiving has a stronger negative effect on probability of employment than extra-residential caregiving. Finally, one other study focuses exclusively on the effect of co-residence with a parent on women's employment. That study, while limited in that it treats co-residence as an exogenous variable, finds that living with a disabled parent is associated with reducing the number of hours worked (Kolodinsky and Shirey 2000).

While these studies come to the same qualitative conclusion that caregiving reduces employment and hours worked, these studies come to different conclusions about the magnitude of the relationship. One explanation for inconsistent findings about the magnitude of the relationship between labor market participation and caregiving may be that different studies use different strategies to account for endogeneity, with varying degrees of success. In some of these studies, endogeneity tests (such as Durbin-Wu-Hausman) do not reject the null hypothesis that caregiving is exogenous to employment outcomes (Bolin et al 2008). In fact, one study comes to different conclusions about the endogeneity of co-residential caregiving depending on the method of estimation (Heitmueller 2007). Another explanation may be that these studies look at populations in very different contexts with respect to availability of publicly-funded LTC, paid-work leave policies, and cultural norms of family responsibilities.

A notable gap in this literature is that there is little research assessing the effects of women's employment on caregiving and co-residence with older, disabled parents. In a review of the literature on the supply of informal care, Norton notes that, "Although conceptually it is easier to model a working woman's decision to provide care or not, empirically it is easier to identify and survey a group of caregivers and then estimate the probability of working," and cites the few papers that take this approach (Norton 2000). In addition, the relatively few papers that assess the effects of employment on caregiving generally do not address the endogeneity between employment and caregiving. One notable exception dealt with the endogeneity issue by using lagged measures of employment as instruments for current employment, and found negative but insignificant effects of employment on caregiving after controlling for endogeneity (Stern 1995). However, the author notes that this identification strategy is based on the possibly unrealistic assumption that children do not make forward-thinking employment decisions with respect to caregiving. Also, it is not clear that this approach deals with the possibility that lagged employment is directly correlated with caregiving due to unobserved preferences for household and market production. Finally, to my knowledge, there are no papers that directly assess the effects of employment on the probability of co-residing with a disabled parent.

That question is the focus of this research. I investigate whether women who were influenced to increase their employment by social policies of the 1990s changed their patterns of co-residence with older, disabled parents as a consequence of these changes. This study builds on prior research by using plausibly exogenous variation in incentives to work as a source of variation in employment.

3.3. Conceptual Model and Predictions

My conceptual model considers the co-residence decisions of an altruistic, utility-maximizing adult child (hereafter called the "child"). The child maximizes utility over consumption and leisure, and is altruistic in the sense that his or her utility also increases with his or her disabled parent's well-being (I assume an individual parent for expositional purposes) (Becker 1974). For simplicity, suppose that one's parent's well-being increases with the parent's health and varies by the parent's living arrangement (either in the community or institutionalized). Informal care and paid LTC are inputs to the parent's health. Caregiving can also affect the child's utility directly but ambiguously, following research in sociology suggesting that caregiving may confer distinct benefits or burdens for the caregiver (England 2005). Finally, the child's utility function depends on whether or not he or she co-resides with a disabled parent. For example, if a child values privacy, he or she may derive more utility from consumption or leisure if the child does not co-reside with an older parent. The child's co-residence decision depends on whether his or her utility is higher in an independent household, or in a shared household with a disabled parent.

The present research question is, how might increased incentives to work affect the child's decision to co-reside with a disabled parent? Different types of incentives can affect this decision in complex ways. For example, static labor supply theory unambiguously predicts that an increase in wages raises the probability that a child works (but has ambiguous predictions for how much he or she works). Increased wages result in a combination of income and substitution effects that make it difficult to predict the effect on co-residence with a disabled parent. On the one hand, higher wages imply greater opportunity costs of caregiving, which could cause the child to substitute his or her time away from caregiving to employment. Yet on the other hand, if one's parent's well-being is a normal good, having more earned income would predict greater investments in the parent's health, possibly via more informal care (which could improve the parent's health more efficiently under co-residence) or via purchasing home care services. Also, the income effect could promote co-residence if the parent is happier co-residing than living in a nursing home. Alternatively, if privacy is a normal good, the income effect would make the child less likely to co-reside. Thus, the income effect on caregiving has an ambiguous sign. This is because this income effect is composed of a direct effect that may be positive or negative, along with an indirect effect through the parent's health, which may also be positive or negative (since the child can choose between direct caregiving or paying for home care).

Changes to a child's non-wage income may also alter that child's employment incentives. Static labor supply theory predicts that a reduction in non-wage income increases the probability of working. However, the effect of this changed incentive on co-residence with a disabled parent is ambiguous, as it involves a complex interaction of income and substitution effects, similar to the case of a wage increase.

This discussion is intended to illustrate that it is not possible to make unambiguous predictions about how changes in employment incentives affect the child's co-residence decisions, without making restrictive, and possibly unrealistic, assumptions about the child's utility function. This ambiguity suggests that empirical analysis is needed to assess how employment incentives affect co-residence decisions.

Some previous research on informal supports for older adults notes that the decision to co-reside is most appropriately conceptualized as a joint decision between a woman and her older parent. For example, Pezzin and Schone (1999) propose a Nash bargaining model in which co-residence occurs only if both the daughter and the parent have higher utilities in a joint living arrangement than in separate living arrangements.

While this conceptual model adds important nuance to living arrangement decisions, its empirical implementation is not drastically different from models of the daughter's living arrangement choice that assume unitary preferences within families. For example, in Pezzin and Schone's reduced form empirical model of living arrangements, the main difference due to explicitly modeling the bargaining process is the inclusion of parent's health characteristics, parent's unearned income, parent's living arrangement preferences, and housing costs. If one makes the assumption (as I do) that changes in the incentives for the child to work are uncorrelated with these parent-level variables, then my estimates of the effects of these changes on co-residence should be unbiased, even though I do not explicitly model the bargaining process.

The conceptual model described above (along with other prior research) suggests that the price and availability of substitutes for informal care (such as paid home care or nursing home care) may also affect informal caregiving and co-residence decisions. Empirical research demonstrates that availability of paid home care services reduces informal caregiving and co-residence (Golberstein et al. 2007; Pezzin, Kemper, and Reschovsky 1996; Stabile, Laporte, and Coyte 2006). On the other hand, while increased availability of nursing home care could conceivably reduce informal caregiving and co-residence, this result has not been borne out in empirical research (Grabowski and Gruber 2007). Nevertheless, the potential importance of paid sources of support for disabled parents suggests that variables measuring the supply of these services ought to be included in empirical models of co-residence.

Although it is difficult to unambiguously predict the effects of increases in work incentives on co-residence decisions using theory alone, I hypothesize that these changes will result in a reduced likelihood that women will co-reside with an older, disabled parent. I also hypothesize that this effect will be strongest among women with lower levels of education, as they are the most likely to respond to these changes by increasing their labor force participation (greater detail on the study design is in Section 5.3).

3.4. Empirical Methods

3.4.1. Data description

The data for these analyses come from the Integrated Public Use Microdata Series (IPUMS) 1% samples of the 1990 and 2000 US Censuses (King et al. 2004). I restrict my sample to unmarried women ages 30-50. I focus on women because the bulk of informal care given to disabled parents is delivered by daughters. Furthermore, the large changes in labor force participation over the 1990s occurred for women, and not for men. More specifically, the changes in employment incentives over the 1990s were strongest for unmarried women. I focus on the ages of 30-50 years old as those are the women who are most likely to have competing demands of work, children, and older parents. I restrict my sample to women who are not reported to have any disabilities that prevent them from working, as women with disabilities may not have been affected by the policies that increased women's labor force participation over the 1990's.[5] I also exclude women who were reported to be living in institutions at the time of the census. After

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[5] This exclusion criterion may be objectionable because responses to work disability questions are known to vary with work and wage opportunities, possibly introducing sample selection bias into my analysis. To assess this, I tested for whether groups faced with greater work incentives were less likely to report any work-related disability, and found that this was the case. I also tested for whether women with work disabilities were more or less likely to co-reside with a disabled parent, and found that they were significantly less likely to live with a disabled parent. Taken together, this suggests that to the extent that excluding observations with work disabilities introduces sample selection bias, it actually understates the main findings.

these restrictions, my sample includes 97,626 observations from 1990 and 123,951 observations from 2000, for a total sample size of 221,577.

Data from the 1990 and 2000 censuses are well-suited for these analyses for several reasons. First, the proportion of women ages 30-50 who co-reside with a disabled parent is rather low. Therefore, the large sample size from the census is needed to pick up changes that may be small on an absolute scale, despite being quite large on a relative scale. Second, the timing of the censuses is ideal for these analyses, as the 1990 census occurred just before the large increase in women's labor force participation and the 2000 census occurred just after women's labor force participation peaked. Third, these two waves of the census included questions that directly measure functional limitations and disability.

3.4.2. Measures

The dependent variable in these analyses is whether a woman co-resides with a disabled, older parent. Individuals are defined as having a disability based on two questions from both the 1990 and 2000 censuses. The designated household head reports the disability status of all individuals in the household, implying that measures of disability include both self-reported cases and cases that were assessed by the household head. The first question asks if the person has any difficulty going outside the home alone to shop or visit a doctor's office. The second question asks if the person has any difficulty with bathing, dressing, or getting around inside the house. Both questions in both census years specify that the difficulty must be because of a health condition lasting 6 months or more. Although the disability questions were similar in 1990 and 2000, they were not identical. In 1990, the two questions refer to difficulties due to a "health condition," and also define what is meant by "difficulty." In 2000, the two questions refer to difficulties due to "a physical, mental, or emotional condition," and no definition of "difficulty" is provided. It seems unlikely that these differences in the question wording will bias the results. The levels of reported disability are similar across the two years, and more importantly, to bias the results the question wording must have differentially affected the responses across the different comparison groups (described below), which seems implausible.

I define a parent as having a disability if he or she is reported to have difficulties for either of these questions. To maintain the focus on old-age disability, I also require a parent to be age 60 or older to qualify as disabled. It is worth noting that the census disability questions are framed in terms of ADL limitations, but not IADL limitations, which may also require substantial personal care. Thus, the measure of the dependent variable may underestimate true levels of co-residence with disabled, older parents.

The key independent variables in these analyses are whether women are observed in the 1990 or 2000 census and whether or not they have children. A woman is defined as having a child if she is reported to have at least one of her own children under the age of 18 living in the same household. I measure employment with a dummy variable for whether or not the individual worked at all in the past year. I also include a number of covariates in the models, including women's age and age-squared, race/ethnicity (white, black, Asian/Pacific Islander, Hispanic/Latino, other races and ethnicities), and whether the household is located in an area that is not defined as urban or suburban. I measure educational attainment as three categories of less than a high school education, a high school education only, or education beyond the high school level. These variables are all taken directly from the IPUMS census data files (King et al. 2004). I also include data on state long-term care policies that may have affected women's probabilities of co-residing with a disabled parent. To measure the state-level supply of formal long-term care, I use Medicaid reimbursement rates for a day of nursing home care (in real 2000 dollars), the number of certified nursing home beds per 1000 adults age 65 and over, and whether a state had a certificate of need law or moratorium on nursing home construction in place.[6] I also use measures of state Medicaid policies that may affect the demand for nursing home care.[7] These measures include a dummy variable for whether a state had a spend-down provision for Medicaid eligibility in a given year, and the income standard and assets standards for a single adult for Medicaid eligibility (expressed in real 2000 dollars). To measure the availability of publicly-funded home care, I use Medicaid home and community-based services expenditures per 1000 adults age 65 and older (in real 2000 dollars). I use state unemployment rates as a measure of macroeconomic conditions that may also influence women's co-residence decisions. I also include two other variables that may be related to co-residence decisions in some of the empirical models. I measure housing values as the median rent for a one-bedroom apartment in the household's metropolitan area.[8] I also include a variable for whether the woman lived in her state of birth as a proxy for distance from one's parents. All analyses are weighted to represent the US population and the standard errors are clustered on the state level (Bertrand, Duflo, and Mullainathan 2004).

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[6] I am grateful to Charlene Harrington for sharing some of these data with me.

[7] I am grateful to David Grabowski and Jonathan Gruber for sharing these data with me.

[8] This variable is computed at the level of the "public use metropolitan area" from the 5% census samples in 1990 and 2000.

3.4.3. Study Design

The main econometric strategy is to estimate difference-in-difference (D-D) and triple-difference (DDD) models of the probability of co-residing with a disabled parent. D-D estimators are commonly used in situations where some definable group is affected by a policy and another one is not. The validity of the D-D estimator depends heavily on the specification of treatment and control groups that did not experience any changes, other than the policy change, over the study period that would have affected the relevant outcome.

Following prior research on the effects of the Earned Income Tax Credit (EITC) and welfare reforms on women's employment, I compare the co-residence trends for single mothers with the trends for single women with no children. These comparison groups are used because those policy changes significantly changed the employment incentives for single women with children, but barely changed employment incentives for single women without children. In addition, along with estimating the D-D models for the full sample of women, I run separate D-D models for women with more than a high school education and for women with a high school education or less. I do these stratified analyses (which are analogous to a DDD approach) because labor supply theory predicts that lower skilled and lower income workers are more likely to be affected by policy changes like the EITC and welfare reform (a prediction that is consistently demonstrated in the empirical literature).

An extensive body of research assesses women's employment over the 1990s and how it was affected by changes in the EITC and welfare reforms. Single mothers experienced a dramatic increase in labor force participation over the 1990s, particularly in comparison to single women without children. Figure 1 illustrates that annual employment for single mothers ages 30-50 increased from 81% in 1990 to 92% in 2000, while employment for single women without children stayed even at 92% (Author's calculations from CPS data). Another important observation is that the increase in single mothers' employment was driven by employment increases of lower-skilled women (Figure 2).

It is very complicated to assess the exact causes of this increase, because it was likely a combination of specific social policies (some of which likely had interactive effects) and a context of a booming economy. Nevertheless, prior research provides strong evidence that the EITC and welfare reforms did have major effects on the increase in women's employment over the 1990's. For example, Grogger (2003) reports that 69% of single women worked in 1993 and 83% worked in 1999. He estimates that 33% of this increase is attributed to EITC expansions and 20% of this increase is due to welfare policy changes (and that an additional 20% is attributed to improved macroeconomic conditions, as measured by the unemployment rate). These findings are comparable to other research that estimates that for single women, tax policy changes (including the EITC) accounted for 35% of employment increases from 1992-1996, while changes in welfare policies accounted for an additional 24-36% of those increases (Meyer and Rosenbaum 2001). Although the exact causes of these trends in women's employment are unclear, I justify my choices of treatment and comparison groups because the changes in employment incentives across these groups between 1990 and 2000 were exogenous with respect to the probability of co-residing with a disabled parent.

A possible concern with this D-D approach is that there were other trends and changes taking place that affected the co-residence or employment of the treatment group differently than the control group. To address this concern, the models include variables that measure other factors that could differentially affect the two groups. These variables include measures of state long-term care policies and measures of state macroeconomic conditions. I also estimate some models where I include interactions of these variables with the measure of whether a woman had a child present to test for whether women with children are differentially affected by these policies in a way that might explain any observed trends.

It is important to note that the probability of a woman co-residing with a disabled parent is conditional upon a) having a living parent, and b) having a living parent who is disabled. I make the assumption throughout that the *change in the* probability of having a living parent who is disabled between 1990 and 2000 is identically distributed across the treatment and control groups,[9] so that the probability of co-residing with a disabled parent can be considered unconditionally. I make this assumption because I know of no evidence which suggests that parents' mortality or disability status varied between the single women with and without children between 1990 and 2000. In fact, there is evidence that from 1990 to 2000 low education women were *increasingly likely* to have a disabled parent than higher education women, which would bias *against* finding that women with increased employment incentives would be less likely to co-reside (I return to this assumption later).

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[9] Note that this assumption is weaker than assuming that the probability of having a living, disabled parent is identically distributed across treatment and control groups.

3.4.4. Econometric Models

I first estimate a simple regression model of co-residence with a disabled parent on employment, conditioning on age, which is an obvious confounder. This model can be considered "naïve" in the sense that it ignores the simultaneity of employment and co-residence decisions and because unobserved variables may be correlated with employment decisions and the error term. For these reasons, the naïve model will not deliver a consistent estimate of the effect of employment on co-residence. Next, I assess the effect of increased employment incentives on co-residence by estimating reduced form models of co-residence. In the reduced form models, I compare co-residence across groups who were faced with significantly different incentives to work. These models take the following general form:

$$Coresidence_{ist} = \beta_0 + \beta_1 Child * 2000_{ist} + \beta_2 Child_{ist} + \beta_3 2000_t + \gamma X_{ist} + \lambda LTCpolicies_{st} + \eta_s + \varepsilon_{ist}$$

In this model, *i* indexes individuals, *s* indexes states, and *t* indexes year. *Co-residence* is the dummy variable for whether a woman co-resided with a disabled parent. The key coefficient is β_1, and β_2 and β_3 are dummies for whether a child is present and for whether the observation is in year 2000, respectively. X includes individual-level covariates, *LTCPolicies* includes the state-level variables for formal LTC services, along with other state-level variables, and η denotes state fixed effects. A negative coefficient for β_1 would suggest that women who were faced with greater incentives to work were less likely to co-reside with a disabled parent. I also stratify my analyses across women with greater than high school education and women with a high school education or less, because the lower-education women were the ones who were most affected by the changed incentives to work, while these changed incentives did not strongly affect employment for higher-education women (Figure 1a). Stratifying by education is analogous to estimating a DDD model of co-residence.

I estimate the reduced form models as linear probability models in OLS, because this approach allows for a clear interpretation of the coefficients. A non-linear model such as probit is generally preferable for estimating models of binary dependent variables, especially when the mean of the dependent variable is close to zero or one, as is the case with my data. However, the key dependent variable in my models is an interaction term, and interaction terms in nonlinear models do not have a straightforward interpretation (Ai and Norton 2003). To test whether linear probability models produce accurate results, I compare the key coefficients from the first set of reduced form linear probability models with the marginal effects from the same model, estimated in probit. I use the inteff program for Stata to calculate the marginal effect of the key dependent variable, along with the standard error, which is estimated via the delta method (Norton, Wang, and Ai 2004). The results of this comparison are in appendix table A1, and show that both the marginal effects and the statistical significance are nearly identical between the linear probability models and the probit models, so I proceed with estimating my models with OLS.[10]

3.5. Results

3.5.1 Descriptives

Tables 1 and 2 show the summary statistics for the sample. The sample is weighted to represent the population of all single women ages 30-50 in the US in 1990

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[10] The results from the probit models are included in the appendix, in Table A2

and 2000. The mean age is slightly higher in 2000 than in 1990 and there are more women with greater than a high school education in 2000. Also, the percentage of women who are white dropped between 1990 and 2000, while the percentage of women who were Hispanic/Latino or “other race” went up. The biggest difference across the census years was that far fewer women lived in non-metropolitan areas in 2000. With respect to state-level variables (Table 2), the mean maximum assets and income for Medicaid eligibility (in real terms) both increased between 1990 and 2000. There was a small decrease in the proportion of the population in states with certificate-of-need or nursing home moratorium policies between 1990 and 2000. The average Medicaid nursing home reimbursement rate increased between 1990 and 2000, although the average nursing home beds per 1000 individuals 65 and older was unchanged. Also, the average Medicaid home health spending per 1000 individuals 65 and older increased substantially between 1990 and 2000, while the average unemployment rate dropped.

Table 3 shows the mean of the dependent variable. For the full sample, 2.4% of women co-reside with an older, disabled parent. This proportion is higher among lower education women, is lower among women who have children under 18, and is higher in 2000 than in 1990. There are also significant racial and ethnic differences in the probability of co-residence. For example, Asians/Asian-Americans are over twice as likely to co-reside with a disabled parent as whites. Not surprisingly, women who live in their state of birth are more likely to co-reside with a disabled parent. Figure 3 illustrates that the probability of co-residence increases with women’s age.

Table 4 shows the probability of co-residing with a disabled parent across the treatment and control groups, which can be interpreted as unadjusted D-D estimates.

These proportions show that the probability of co-residence increased more for women without children between 1990 and 2000 than for women with children. In addition, this result is stronger for the subsample of lower-education women. Furthermore, there is virtually no difference in trends of co-residence between women with and without children for the higher higher-education subsample.

3.5.2 Reduced Form Results

The results from the naïve co-residence model are consistent with prior literature documenting a negative correlation between employment and delivering informal support. Specifically, the naïve model estimates that working in the past year is associated with a statistically significant 1.9% reduced probability of co-residing with a disabled parent ($p<.001$) (Table 5).

Table 6 contains the results of the reduced form co-residence models. Several models are presented to address possible alternative explanations for the results. The first model (Table 6a, model 1) is essentially the raw D-D estimate, controlling for state fixed effects only, and is nearly identical to the D-D comparisons in Table 4. β_1 is estimated to be -.0045 for the full sample ($p=.003$). After stratifying by education, the effect is stronger in the low-education sample ($\beta_1=-.0077$, $p<.001$) (Table 6b, model 1), and non-existent for the higher-education sample ($\beta_1=-.002$, $p=.29$) (Table 6c, model 1). These results support the hypothesis that women who were faced with greater changes in the incentives to work reduced their co-residence with disabled parents. But, this result may be due to changes in the composition of comparison groups over time. In the second model, I include the individual-level covariates to control for compositional change.

After conditioning on these covariates, β_1 remains nearly unchanged. β_1 is estimated to be -.004 (p=.007) for the full sample (Table 6a, model 2), -.0077 (p<.001) for the low-education sample (Table 6b, model 2), and -.0016 (p=.414) for the higher-education sample (Table 6c, model 2). Another potential explanation for these findings is that the women who were faced with greater incentives to work lived in states with fewer publicly-funded LTC resources, or that macroeconomic conditions might explain these results. Thus, I add state LTC policy variables and unemployment rates to the third model (Tables 6a-6c, model 3). The results remain virtually unchanged after adding these other variables. Finally, I add interaction terms to allow for the possibility that women with children in their household are differentially affected by state LTC policy variables and unemployment rates. Adding these variables eliminates the effect of having a child present in 2000 for the full sample (β_1=-.0019, p=.395) (Table 6a, model 4), but does not change the finding for the low-education subsample (β_1=-.008, p=.032) (Table 6b, model 4).

Although these coefficients are small on an absolute scale, they take on more meaning when interpreted on a relative scale. Compared to the mean of the dependent variable for the full sample of 2.4%, the β_1 coefficient from model 3 implies an 18% reduction in the probability of co-residing with a disabled parent. For the low-education subsample, the coefficient from model 3 implies a 30% reduction in the probability of co-residing when compared to the sub-sample mean of 2.9%.

There are several other notable results in these models. Focusing on model 3 for the full sample, I find only one significant relationship between state LTC policy and co-residence with a disabled parent (Table 6a). A higher asset allowance for Medicaid eligibility is significantly associated with a lower probability of co-residing with a disabled parent (p=.004). This is consistent with a story that more liberal Medicaid eligibility could make disabled parents more likely to enter nursing homes, or to live independently with Medicaid home care. After adding interactions between the policy variables and whether there is a child present in model 4, a greater supply of nursing home beds significantly reduces the probability of co-residing with a disabled parent, but only for women with children (β=-.005, p<.001, not shown in table).

When focusing on the low-education subsample (Table 6b), which may be more likely to be affected by publicly-funded LTC policies, there are more interesting relationships. As in the full sample, a higher asset allowance for Medicaid eligibility is significantly associated with a lower probability of co-residing with a disabled parent (p=.002). In addition, the presence of certificate-of-need or nursing home construction moratorium policies increased the probability of co-residing by a non-trivial .011 (p=.038). Also, higher Medicaid reimbursement for nursing homes reduced the probability of co-residing, although this coefficient was only marginally significant (p=.053). Similar to the full sample, after adding interactions between the policy variables and whether there is a child present in model 4, a greater supply of nursing home beds significantly reduces the probability of co-residing with a disabled parent, but only for women with children (β=-.004, p=.051, not shown in table).

These findings imply that nursing home use may in fact be somewhat elastic with respect to public LTC generosity, and that those policies which affect the availability and generosity of public programs may therefore affect the children of disabled older adults. The former implication is in contrast to Gruber and Grabowski's (2007) findings that these policies did not significantly affect nursing home use, although their results were in the same direction as mine. However, it is difficult to compare our studies, since I use fewer time points and have a much larger sample size.

3.5.3 Falsification Test and Robustness Check

Thus far, my empirical results support the conclusion that single women are less likely to co-reside with a disabled parent because they are working. However, these results could be consistent with another story, which is that single women are less likely to co-reside with a parent because they are working, regardless of their parents' disability status. To test whether this is the case, I constructed a new dependent variable. This variable equals one if a woman co-resided with any parents *and* if those parents did not have any disabilities, and equals zero otherwise. I then ran a reduced form D-D model of this new variable for the lower education group of single women. If this model produced results that were similar to my main results, it may suggest that this alternative story is a more plausible explanation of my findings. The results from this model are presented in table 8. I found that having a child in 2000 actually *increased* the probability of co-residing with a non-disabled parent (although the coefficient was not significantly different from zero). This result lends further support to the hypothesis that labor market participation reduces the supply of informal support to disabled parents.

I included two potentially endogenous variables in my main analyses. To the extent that living in one's birth state proxies for proximity to one's parents, this variable could be endogenous because individuals might choose to live closer to disabled parents to facilitate the delivery of informal support. In addition, it is possible that individuals might choose to live in areas with lower housing rents in order to facilitate co-residence with a disabled parent without many financial resources. After re-estimating my reduced form models without these independent variables, my results are not changed (Table 9).

3.5.4 Potential Unobserved Heterogeneity

The biggest issue in my research design is whether my choice of treatment and control groups is valid. In other words, is having a child in the 2000 period a valid measure for increased employment incentives that is uncorrelated with other things that may directly affect co-residence decisions? I have thus far argued that conditional on all of my covariates, there are no unobserved characteristics of this group that are directly correlated with co-residence decisions.

Nevertheless, the possibility of unobserved heterogeneity remains a concern which is impossible to rule out entirely via empirical methods. Thus, it is useful to consider potential sources of unobserved heterogeneity, and whether they could realistically affect my results in any meaningful way. One possible source of heterogeneity that could bias my results is if conditional on my covariates, single women with children had a larger decrease in the probability of having a disabled parent from 1990 to 2000, relative to single women without children. If there were survey data collected at approximately these two points in time that asked middle-age women about their parents' disability status, this could resolve the issue. Unfortunately, such data do not exist. However, it is possible to derive "back of the envelope" estimates of how much parents' disability rates would have had to have changed across the study groups to explain my findings. I can then compare that estimate with the evidence on the levels and trends in old-age disability between 1990 and 2000 to get a sense for whether it is a plausible explanation for my findings.

To estimate the proportion of single women ages 30-50 who have at least one disabled parent, I use data from the 1993 National Survey of Families and Households (NSFH), a nationally representative survey of family demographic issues. The NSFH includes detailed information about family living arrangements and disability status. I estimate that in 1993, 8.0% of single women ages 30-50 with a high school education or less had at least one living parent who needed help moving around inside the house or with personal care such as eating, bathing, dressing, or going to the bathroom. There was no significant difference in the unadjusted probability of having a disabled parent between low-education single women with children and single women without children (9.5% vs. 5.9%, respectively, p=.207). While this non-significant (though non-trivial) difference may be further evidence that single women with and without children are suitable comparison groups for my analysis, the more important issue is whether the difference in the probability of having a disabled parent between these groups changed between 1990 and 2000.

To get a sense for how much the probability of having a disabled parent must have changed to explain my findings, I make the assumptions that the 1993 NSFH data can be compared to the 1990 data and that the underlying probability of co-residence, conditional on having a disabled parent, was unchanged for low-education single women (at 34%) between 1990 and 2000. Under these assumptions, the change in probability of having a disabled parent between 1990 and 2000 must have been 2.6 percentage points lower for single women with children than for single women without children to explain the findings from my D-D models. This is a very substantial change, and compared to the baseline probability of having a disabled parent it implies a 32% relative difference in the trends of having a disabled parent between single women with and without children.

To put that estimate in perspective, most research suggests that there were decreases in old-age disability between 1990 and 2000. However, these decreases were not very large in relative magnitude. A review of recent trends in old-age disability finds that the percent of non-institutionalized individuals age 70 and older with personal care needs dropped from 8.1% in 1994 to 7.5% in 2000, (a 7.4% reduction), and that the percent of individuals age 70 and older with at least one ADL limitation dropped from 35% in 1992 to 30% in 2000 (a 14.3% reduction) (Freedman et al. 2004). Given these magnitudes, it is unlikely that there would be a differential trend in parents' disability across my study groups that would be strong enough to account for my findings. Nevertheless, although I consider it unlikely, it is still possible that differential trends in the probability of having a disabled parent could affect my findings, although the direction of any resultant bias is ambiguous. That possibility is a limitation of this research.

It is also possible that there were other differences in the composition of single women with children compared to single women without children between 1990 and 2000 that could account for my findings. To address this concern, I included a number of sociodemographic controls in some of my models. Nevertheless, it is still possible that any observed differential changes may be explained by compositional changes along unobservable characteristics that are uncorrelated with the sociodemographic variables in my model. I explore two other possible explanations here, based on their potential to affect co-residence decisions. First, if single women with children had on average more children in 2000 than in 1990, that could reduce the amount of time available for delivering informal care to a disabled parent. However, the data from the 1990 and 2000 census show that there was no significant difference in the number of children for single women between 1990 and 2000 (1.81 and 1.83, respectively). Second, it is possible that I do not observe single women's number of siblings, and having more siblings could reduce the probability of co-residence. To test whether this is a plausible explanation for my findings, I look at data from the 1990 and 2000 General Social Surveys. These data suggest that among single women ages 30-50, having children is indeed associated with having more siblings, but that the difference between groups did not change between 1990 and 2000 (results not shown). I take these two findings as further evidence that it is unlikely that there are unobserved differences across single women with and without children between 1990 and 2000 that could otherwise explain my results.

It is also possible that there were differential trends in the possibility of having a disabled parent across the comparison groups defined by educational attainment. There is more direct evidence available to address this possibility. For this to be a valid explanation, there would have to have been larger decreases in parents' disability between 1990 and 2000 for lower education women than for higher education women. However, there is evidence that recent decreases in old-age disability were significantly greater for higher education individuals than for lower education individuals (Schoeni et al. 2005). Assuming that there is intergenerational correlation in education, this would suggest that my reduced form DDD estimates of the probability of co-residence are actually conservative.

3.6. Discussion

3.6.1. Summary of Findings

Population aging is a major policy concern because of the strains it may place on families and LTC systems. Much research focuses on the consequences of different health policies on the long-term care and living arrangements of the elderly, but less work focuses on the social policies that can also affect these outcomes. This research addresses a social factor that has potentially great consequences for the LTC and living arrangements of older adults: women's employment. I find evidence to support the hypothesis that the increases in women's employment over the 1990's reduced the probability that middle-age, single women co-resided with an older, disabled parent. Specifically, I estimate that the single women who were most affected by changing employment incentives (low education women with children in 2000) had a statistically significant reduction in the probability of co-residing with a disabled parent of approximately 0.8 percentage points. While small on an absolute scale, that reduction is considerable when compared to the average level of co-residence in that population (2.9%). These findings suggest that policies to increase women's labor force participation may have had the unintended consequence of reducing the amount of informal support that is available to older adults with functional limitations.

A limitation of using census data is that I am not able to distinguish the women who are actually at risk for co-residing with a disabled parent. A consequence of this shortcoming is that I am studying proportions that are seemingly small. However, these proportions become much more meaningful when compared to the proportion of single women ages 30-50 who are actually at risk of co-residence by dint of having at least one living, disabled parent. In separate analyses of the nationally-representative 1993 NSFH, I find that 87% of single women ages 30-50 have at least one living parent, and 8.5% have at least one living parent with functional limitations. These estimates roughly imply that while co-residence with a disabled parent is relatively uncommon among all middle age women, approximately 28% of women ages 30-50 who have at least one parent with a functional limitation co-reside with a disabled parent.

In addition to these main findings, I also find evidence that LTC policies may affect co-residence patterns. Specifically, I find that more liberal Medicaid eligibility for older adults and higher Medicaid nursing home reimbursement rates reduced the probability of co-residing with a disabled parent, while the presence of certificate-of-need or nursing home construction moratoria increased the probability of co-residing with a disabled parent. This suggests that prior research that did not find that these policies had an effect on nursing home use (Grabowski and Gruber 2007) may have estimated those effects imprecisely due to a smaller sample size and/or that those policies affect the residence of disabled older adults along two margins: whether or not to live in a nursing home, and whether or not to co-reside with children.

3.6.2. Limitations

There are a number of important limitations to this research. First, I am limited in using only census data. Unfortunately, there is a paucity of data sources that include comprehensive information on women and information on the disability status of the members of their households with a sufficiently large sample size to test for group differences in co-residence. Because I do not have annual data, I am limited in my ability to examine how specific policies, like EITC expansions and welfare reforms, affected the probability of co-residing with a disabled parent by increasing labor force participation.

Second, I am not able to decompose the effect of women's labor force participation on co-residence into the income effect and substitution effects which both likely exist. The relative magnitudes of such effects have important policy implications. For example, if policymakers were to enact further policies to induce women's labor force participation, but the prevailing economic context did not lead to these policies causing higher incomes, then there may not be a difference in co-residence if my results are due mostly to income effects. In fact, women's well-being may decrease in such a situation, if they are dually burdened with working and supporting a disabled parent without additional resources.

Third, my focus only on single women raises questions about generalizability. It is unclear how my results would generalize to married women, who are clearly an important source of informal support for disabled older adults. Analyzing the relationship between employment and co-residence is more complicated for married women because it introduces dynamics of in-laws and the division of household and non-household activities with the spouse. However, even if my results are not generalizable to married women, single women are substantively important, since 43% of child primary caregivers are unmarried (Wolff and Kasper 2006). Other research has also reported that men are becoming increasingly important sources of informal support for disabled older adults, although my research does not speak to the effects of employment for that population.

A further limitation is that my strongest inferences are based on lower-education women. It is not clear how my findings might generalize to higher-education women. On the one hand, lower-education families are more likely to be eligible for publicly-funded LTC services that may substitute for informal support, suggesting that co-residence is more sensitive to employment than in higher-education families. But on the other hand, lower-education families may rely on intergenerational resource pooling more heavily, suggesting that co-residence is less sensitive to employment than in higher education families.

Fourth, my findings may be questioned because the choice of treatment and control groups may be correlated with other trends that could affect women's co-residence with disabled parents. To minimize this concern, I controlled for other factors, such as changes in the supply of nursing homes, publicly-funded home care generosity, and other long-term care policies that could affect co-residence decisions. To be certain, the large increase in women's labor force participation resulted not only from major policy changes, but also from a booming economy. To the extent that economic conditions differentially affected co-residence decisions of my comparison groups via mechanisms not involving employment, that would challenge the validity of my findings.

Another concern related to compositional change is that the EITC expansions and welfare reforms changed the incentives for marital and childbearing decisions. To the extent that this occurred, the treatment and control groups may have changed in unobservable ways between 1990 and 2000, resulting in potential sample selection bias. However, the empirical evidence on the effects of labor market incentives on family structure is mixed, and the effects (when observed) are generally small in magnitude or concentrated within certain subgroups (Bitler et al. 2004; Bitler, Gelbach, and Hoynes 2006; Fitzgerald and Ribar 2004). This implies that any potential bias would likely be small and has an ambiguous sign.

3.6.3. Implications

This research has several important implications for policy. First, it implies that projections of future LTC use among older adults should consider trends that may affect the availability of informal support, such as levels of women's employment. In general, projections of future LTC use have not taken levels of women's employment into consideration (a notable exception is the Urban Institute's recent DYNASIM3 model). That omission may lead to inaccurate projections because the availability of informal support has been shown to significantly reduce paid home care and nursing home use. If levels of women's employment remain high and children continue to be relatively important sources of informal support, this could signal a potentially increased demand for paid LTC services in the future, much of which may be publicly-funded.

A second implication is that policies to promote labor force participation may have the unintended consequence of reducing the supply of informal supports to older adults with disabilities. Reducing the supply of informal supports could come at a substantial cost, because informal support has been shown to significantly reduce both nursing home use and paid home care use (Van Houtven and Norton 2004). The average annual nursing home costs per user (in 2004) were $68,000, and the average annual Medicaid Home and Community-Based services costs per user (in 2001) were $10,300. Hence, even relatively small increases in the number of disabled older adults could lead to substantial increases in LTC expenditures (Kitchener et al. 2005; National Center for Health Statistics 2007).

Policies to increase labor force participation may not be a major issue currently in the US, as levels of employment are high and there is somewhat limited potential to further increase labor force participation. However, this may be an important consideration in other countries where promoting women's labor force participation is a major policy goal and where population aging may increase future demands for LTC. For instance, one way to raise revenue to support publicly-financed LTC may be to increase income and payroll taxes by encouraging women's labor force participation. Policymakers should take care in balancing potential tax revenue increases with reductions in the supply of informal supports when considering such policies.

3.6.4. Conclusion

In spite of this study's limitations, this research does make an important contribution. To my knowledge, it is the first study to exploit plausibly exogenous variation in employment incentives to test for whether employment incentives affect the supply of informal supports to older adults with functional limitations. I also find evidence that LTC policies may have an effect on women's co-residence with disabled parents, which may be an important outcome for policymakers to consider. Future research should examine the effects of employment on the supply of informal supports to disabled older adults in greater depth. For example, studying the effects of employment on more direct measures of informal caregiving could be a fruitful area for research. Future research could also assess the effects of employment hours on co-residence and/or informal caregiving decisions and could try to separate out the effects of changes in employment versus changes in income. Also, it could be valuable to more directly assess the effects of specific policies (e.g., the EITC, specific welfare reforms) on the supply of informal support. Finally, future research should assess whether these findings are observed in other contexts. For instance, does employment affect co-residence in other countries where publicly-funded home care is more readily available and generous? Such research would be invaluable in informing policy decisions to be made about designing incentives to promote optimal levels of employment and social support.

Figure 3.1. Probability of Working in Past Year, All Education Levels (CPS)

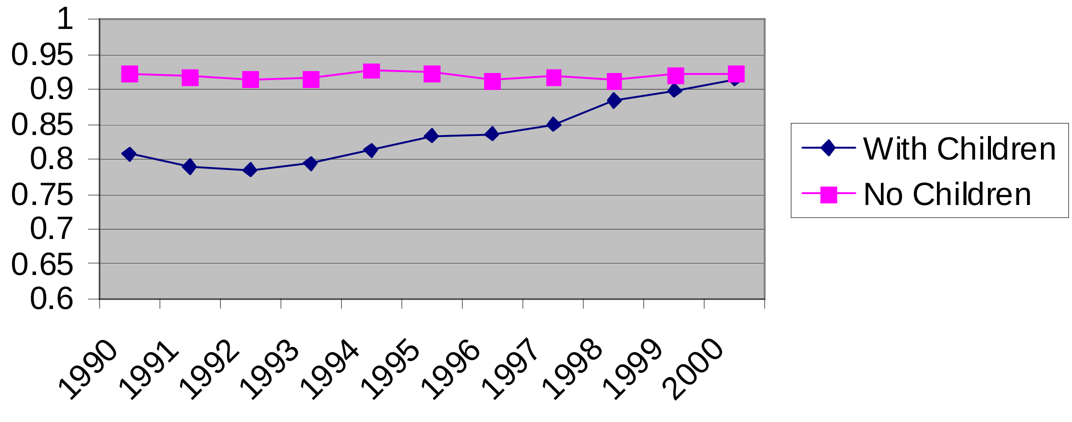

Figure 3.2. Probability of Working in Past Year, for Single Women with Children (CPS)

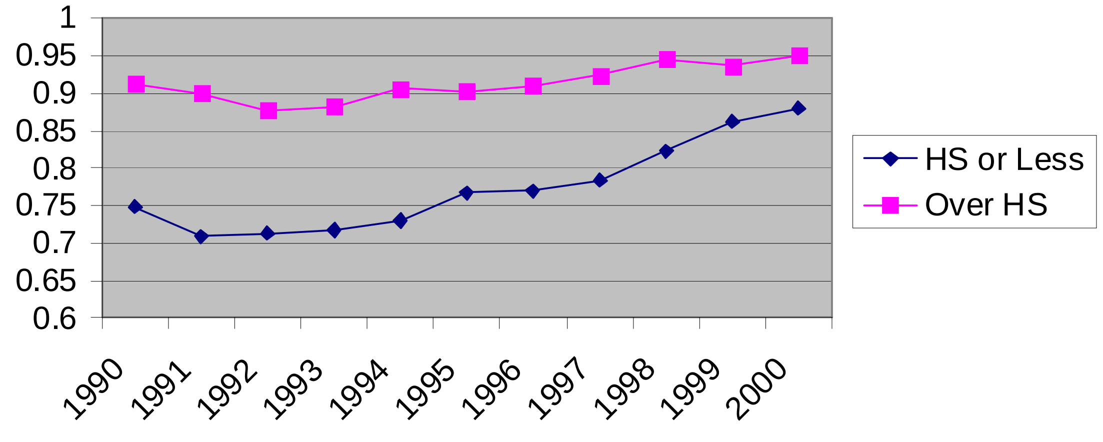

Figure 3.3. Probability of Co-Residing with a Disabled Parent

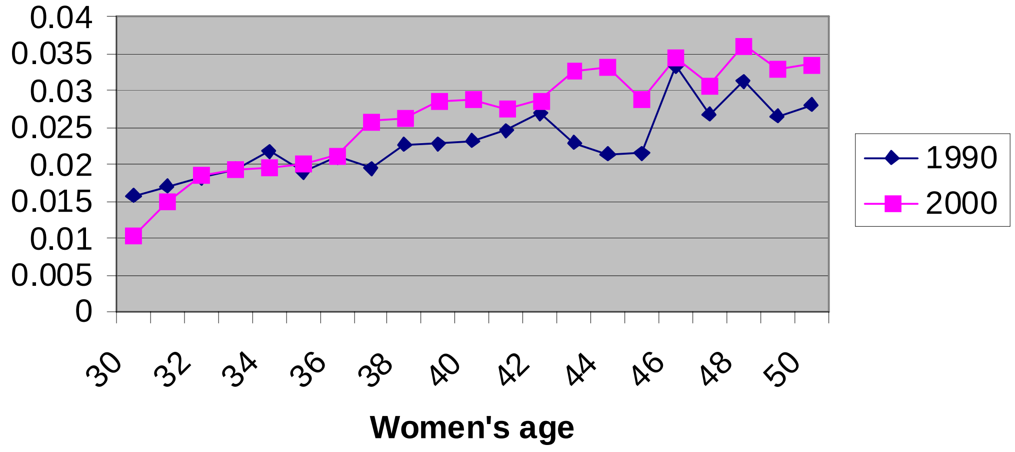

Table 3.1. Sample Descriptive Statistics for sociodemographic variables

Variable	**1990**		**2000**		**Combined**	
	Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**
Age	38.46	5.91	39.63	5.97	39.11	5.98
Age-squared	1514.49	466.43	1606.37	476.59	1565.48	474.30
Child present	0.36		0.34		0.35	
Education						
Less than HS	0.13		0.11		0.12	
HS/GED	0.32		0.30		0.31	
More than HS	0.55		0.59		0.57	
Race						
White	0.66		0.62		0.64	
Black	0.22		0.21		0.21	
Asian	0.02		0.03		0.03	
Hispanic	0.09		0.11		0.10	
Other Race	0.01		0.03		0.02	
Urban/Rural						
Metro/mixed area	0.85		0.97		0.92	
Not metro/mixed	0.15		0.03		0.08	
Lives in birth state						
Yes	0.55		0.55		0.55	
No	0.45		0.45		0.45	
Median rent (2000 $)	456.34	193.37	476.07	158.58	469.01	172.09
Sample size	97626		123951		221577	

Table 3.2. Sample Descriptive Statistics, State-level variables

	1990		**2000**		**Combined**	
Variable	Mean	SD	Mean	SD	Mean	SD
Medicaid asset test for single-person HH	2175.34	781.32	2312.40	850.67	2254.56	824.90
Medicaid income test for single-person HH	258.56	458.87	335.68	606.89	301.82	548.19
CoN/NH Moratorium in place	0.83		0.77		0.80	
Medicaid NH reimbursement rate	85.15	25.69	108.92	22.12	98.28	26.56
NH beds per 65+ capita	52.21	15.45	51.19	15.19	51.65	15.31
Medicaid Home Health $ per 65+ capita	211077.80	287984	271260.70	273808	244473.70	281797
State unemployment Rate	5.62	0.85	4.03	0.76	4.74	1.13

Note: All dollar values expressed in real 2000 dollars

Table 3.3. Means of the Dependent Variable

Overall sample mean	0.024
Year	
1990	0.022
2000	0.026
Child present	0.017
No Child present	0.028
Education	
Less than HS	0.031
HS/GED	0.029
More than HS	0.020
Race/Ethnicity	
White	0.021
Black	0.031
Asian	0.046
Hispanic	0.029
Other Race	0.027
Urban/Rural	
Metro/mixed area	0.024
Not metro/mixed	0.027
Lives in birth state	
Yes	0.030
No	0.018

Table 3.4. Differences in Co-Residence

Full sample

	1990	**2000**	**Difference**	**D-D**
No child present	0.025	0.030	0.005	
			(0.0009)	
Child present	0.0168	0.0176	0.0008	
			(0.001)	
Difference	-0.008	-0.013		-0.0045
	(0.001)	(0.001)		

Over High School Education

	1990	**2000**	**Difference**	**D-D**
No child present	0.0201	0.0249	0.0047	
			(0.0011)	
Child present	0.0135	0.0165	0.0029	
			(0.0014)	
Difference	-0.0066	-0.008		-0.00181
	(0.001)	(0.001)		

High School or less

	1990	**2000**	**Difference**	**D-D**
No child present	0.0323	0.0392	0.007	
			(0.002)	
Child present	0.0199	0.0191	-0.0008	
			(0.002)	
Difference	-0.0123	-0.0201		-0.0078
	(0.0016)	(0.0016)		

Note: Standard errors are in parentheses

Table 3.5. Naïve co-residence regression model

	Coef	SE	t	P
Worked in past year	-0.019	0.0013	-14.32	<.001
Age	0.0009	0.0001	15.16	<.001
Constant	0.0051	0.0026	1.97	0.049
R-squared	0.003			
Sample size	221577			

Note: Estimated as a linear probability model. Standard errors are clustered at the state.

Table 3.6a. Reduced form co-residence models: Full sample

	MODEL 1				**MODEL 2**				**MODEL 3**				**MODEL 4**			
	Coef	se	t	p	Coef	se	t	p	Coef	se	t	p	Coef	se	t	p
Child present	-0.008	0.001	-6.6	<.001	-0.009	0.001	-6.81	<.001	-0.008	0.002	-5.54	<.001	-0.004	0.013	-0.28	0.783
2000	0.005	0.001	4.22	<.001	0.005	0.001	4.41	<.001	0.011	0.004	2.86	0.006	0.010	0.004	2.58	0.013
Child*00	-0.004	0.001	-3.1	0.003	-0.004	0.001	-2.82	0.007	-0.004	0.002	-2.58	0.013	-0.002	0.002	-0.86	0.395
Age					0.004	0.001	5.63	<.001	0.005	0.001	5.82	<.001	0.005	0.001	5.82	<.001
Age-squared					0.000	0.000	-4.59	<.001	0.000	0.000	-4.74	<.001	0.000	0.000	-4.75	<.001
HS					0.000	0.002	-0.08	0.94	0.000	0.002	-0.01	0.993	0.000	0.002	-0.02	0.983
Over HS					-0.007	0.002	-3.8	<.001	-0.007	0.002	-3.87	<.001	-0.007	0.002	-3.89	<.001
Black					0.008	0.001	6.26	<.001	0.008	0.001	5.7	<.001	0.008	0.001	5.68	<.001
Asian					0.031	0.007	4.64	<.001	0.035	0.007	4.86	<.001	0.035	0.007	4.83	<.001
Hispanic					0.010	0.002	4.83	<.001	0.011	0.002	4.56	<.001	0.011	0.002	4.5	<.001
Other race					0.008	0.002	3.48	0.001	0.009	0.002	3.6	0.001	0.009	0.002	3.62	0.001
Nonmetro					0.005	0.002	3.17	0.003	0.002	0.002	1.5	0.139	0.002	0.002	1.55	0.128
Same state					0.013	0.001	11.87	<.001	0.013	0.001	10.29	<.001	0.013	0.001	10.22	<.001
Medicaid asset test for single-person HH ($1000s)									-0.016	0.005	-3.06	0.004	-0.016	0.005	-3.08	0.003
Medicaid income test for single-person HH ($1000s)									-0.003	0.003	-0.88	0.385	-0.003	0.003	-0.84	0.407
CoN/Moratorium in place									0.004	0.003	1.27	0.209	0.005	0.003	1.38	0.175
Medicaid NH reimb rate ($100s)									-0.010	0.009	-1.08	0.284	-0.008	0.010	-0.8	0.426
NH beds per 65+ capita (100s)									-0.013	0.017	-0.78	0.437	-0.008	0.017	-0.46	0.651
Medicaid Home Health $ per 65+ capita ($1000s)									0.000	0.000	-1.03	0.306	0.000	0.000	-1.13	0.262
State unemployment rate									0.003	0.001	1.87	0.068	0.002	0.002	1.38	0.175
Median rent ($100s)									-0.001	0.0005	-2.32	0.025	-0.001	.0004	-2.33	0.024
Policy*child interactions	no				no				no				Yes			
R-Squared	0.0028				0.0073				0.008				0.0081			
Sample size	221577				221577				185528				185528			

Note: Models estimated with OLS. State fixed effects and interaction terms are suppressed. Standard errors are clustered on the state.

Table 3.6b. Reduced form co-residence models: High school education or less

	MODEL 1				MODEL 2				MODEL 3				MODEL 4			
	Coef	se	t	p	Coef	se	t	p	Coef	se	t	p	Coef	se	t	p
Child present	-0.012	0.002	-6.91	<.001	-0.011	0.002	-5.35	<.001	-0.010	0.003	-3.70	0.001	-0.014	0.023	-0.61	0.543
2000	0.007	0.002	4.29	<.001	0.007	0.002	4.70	<.001	0.024	0.007	3.34	0.002	0.023	0.007	3.16	0.003
Child*00	-0.008	0.002	-4.18	<.001	-0.008	0.002	-4.26	<.001	-0.009	0.002	-3.60	0.001	-0.008	0.004	-2.21	0.032
Age					0.005	0.001	3.27	0.002	0.006	0.002	3.35	0.002	0.005	0.002	3.33	0.002
Age-squared					0.000	0.000	-2.78	0.008	0.000	0.000	-2.90	0.006	0.000	0.000	-2.88	0.006
HS					-0.001	0.002	-0.83	0.411	-0.001	0.002	-0.75	0.458	-0.001	0.002	-0.77	0.443
Over HS					-				-				-			
Black					0.005	0.002	2.84	0.006	0.006	0.002	2.81	0.007	0.006	0.002	2.81	0.007
Asian					0.034	0.008	4.22	<.001	0.039	0.009	4.55	<.001	0.039	0.009	4.52	<.001
Hispanic					0.005	0.002	1.86	0.069	0.004	0.002	1.91	0.061	0.004	0.002	1.87	0.067
Other race					0.005	0.004	1.27	0.211	0.007	0.004	1.59	0.118	0.007	0.004	1.61	0.113
Nonmetro					0.005	0.002	2.34	0.023	0.003	0.002	1.08	0.284	0.003	0.002	1.11	0.273
Same state					0.016	0.001	11.27	<.001	0.016	0.002	10.42	<.001	0.016	0.002	10.46	<.001
Medicaid asset test for single-person HH ($1000s)									-0.028	0.009	-3.22	0.002	-0.029	0.009	-3.26	0.002
Medicaid income test for single-person HH ($1000s)									-0.001	0.005	-0.3	0.766	-0.002	0.005	-0.44	0.664
CoN/Moratorium in place									0.011	0.005	2.13	0.038	0.013	0.005	2.53	0.015
Medicaid NH reimb rate ($100s)									-0.033	0.016	-1.98	0.053	-0.034	0.018	-1.93	0.059
NH beds per 65+ capita (100s)									0.005	0.025	0.2	0.84	0.009	0.025	0.35	0.726
Medicaid Home Health $ per 65+ capita ($1000s)									0.000	0.000	-1.1	0.278	0.000	0.000	-1.02	0.314
State unemployment rate									0.004	0.002	2.19	0.033	0.004	0.002	1.61	0.114
Median rent ($100s)									-0.001	0.007	-0.72	0.474	-0.005	0.007	-0.72	0.476
Policy*child interactions	no				no				no				yes			
R-Squared	0.0043				0.0072				0.0079				0.0043			
Sample size	97146				97146				80786				80786			

Note: Models estimated with OLS. State fixed effects and interaction terms are suppressed. Standard errors are clustered on the state.

Table 3.6c. Reduced form co-residence models: Over high school education

	MODEL 1				MODEL 2				MODEL 3				MODEL 4			
	Coef	se	t	p	Coef	se	t	p	Coef	se	t	p	Coef	se	t	p
Child present	-0.006	0.001	-4.24	<.001	-0.007	0.002	-4.64	<.001	-0.008	0.002	-4.8	<.001	0.001	0.011	0.09	0.929
2000	0.005	0.001	3.91	<.001	0.003	0.001	2.86	0.006	0.002	0.005	0.36	0.721	0.001	0.005	0.17	0.868
Child*00	-0.002	0.002	-1.07	0.29	-0.002	0.002	-0.82	0.414	-0.001	0.002	-0.47	0.641	0.002	0.003	0.75	0.455
Age					0.004	0.001	4.48	<.001	0.004	0.001	3.32	0.002	0.004	0.001	3.34	0.002
Age-squared					0.000	0.000	-3.44	0.001	0.000	0.000	-2.41	0.02	0.000	0.000	-2.43	0.019
HS					-				-				-			
Over HS					-				-				-			
Black					0.011	0.002	6.69	<.001	0.010	0.002	5.92	<.001	0.010	0.002	5.92	<.001
Asian					0.030	0.006	4.62	<.001	0.033	0.007	4.81	<.001	0.034	0.007	4.81	<.001
Hispanic					0.017	0.002	8.1	<.001	0.019	0.003	6.58	<.001	0.019	0.003	6.57	<.001
Other race					0.009	0.003	2.68	0.010	0.010	0.004	2.56	0.014	0.010	0.004	2.57	0.013
Nonmetro					0.005	0.002	2.52	0.015	0.002	0.002	0.99	0.325	0.002	0.002	1.02	0.314
Same state					0.011	0.002	6.8	<.001	0.010	0.002	6.05	<.001	0.010	0.002	6.04	<.001
Medicaid asset test for single-person HH ($1000s)									-0.006	0.005	-1.05	0.298	-0.006	0.005	-1.06	0.297
Medicaid income test for single-person HH ($1000s)									-0.003	0.003	-1.2	0.238	-0.003	0.003	-0.97	0.337
CoN/Moratorium in place									-0.004	0.002	-2.68	0.01	-0.005	0.002	-2.25	0.029
Medicaid NH reimb rate ($100s)									0.009	0.013	0.67	0.508	0.012	0.013	0.9	0.371
NH beds per 65+ capita (100s)									-0.025	0.022	-1.12	0.269	-0.019	0.023	-0.85	0.399
Medicaid Home Health $ per 65+ capita ($1000s)									0.000	0.000	-0.26	0.797	0.000	0.000	-0.42	0.679
State unemployment rate									0.001	0.002	0.81	0.421	0.001	0.002	0.61	0.542
Median rent ($100s)									-0.001	.0004	-2.86	0.006	-0.001	.0004	-2.84	0.007
Policy*child interactions	no				no				no				yes			
R-Squared	0.0028				0.0074				0.008				0.0081			
Sample size	124431				124431				104742				104742			

Note: Models estimated with OLS. State fixed effects and interaction terms are suppressed. Standard errors are clustered on the state.

Table 3.7. Falsification Test: Co-residence with non-disabled parents, low-education subsample

	Coef	se	T	p
Child present	-0.058	0.008	-7.63	<.001
2000	-0.005	0.015	-0.36	0.721
Child*00	0.005	0.007	0.77	0.446
Age	-0.025	0.004	-7.09	<.001
Age-squared	0.000	0.000	5.03	<.001
HS	0.019	0.004	5.45	<.001
Black	-0.009	0.004	-2.39	0.021
Asian	0.045	0.012	3.62	0.001
Hispanic	-0.013	0.009	-1.45	0.153
Other race	0.001	0.010	0.13	0.899
Nonmetro	-0.010	0.005	-2.14	0.037
Same state	0.039	0.005	7.2	<.001
Medicaid asset test for single-person HH ($1000s)	0.003	0.018	0.16	0.871
Medicaid income test for single-person HH ($1000s)	0.009	0.009	0.95	0.349
CoN/Moratorium in place	-0.005	0.009	-0.58	0.563
Medicaid NH reimb rate ($100s)	0.047	0.041	1.13	0.264
NH beds per 65+ capita (100s)	-0.001	0.001	-1	0.320
Medicaid Home Health $ per 65+ capita ($1000s)	-0.000	0.000	-0.57	0.572
State unemployment rate	0.006	0.004	1.26	0.214
Median rent ($100s)	0.004	0.002	2.15	0.036
R-squared	0.0315			
Sample size	80786			

Note: Model also includes state fixed effects. Standard errors are clustered on the state.

Table 3.8. Reduced form co-residence models, excluding potentially endogenous regressors

	1				2				3			
All Observations	**Coef**	**se**	**t**	**P**	**Coef**	**Se**	**t**	**P**	**Coef**	**se**	**t**	**p**
Child present	-0.008	0.001	-6.46	<.001	-0.008	0.001	-6.35	<.001	-0.004	0.012	-0.29	0.775
2000	0.005	0.001	4.55	<.001	0.009	0.004	2.28	0.027	0.008	0.004	2.04	0.047
Child*00	-0.004	0.001	-2.83	0.007	-0.005	0.001	-3.16	0.003	-0.003	0.002	-1.13	0.263
		1				**2**				**3**		
High school or less	**Coef**	**se**	**t**	**P**	**Coef**	**Se**	**t**	**p**	**Coef**	**se**	**t**	**p**
Child present	-0.011	0.002	-5.05	<.001	-0.010	0.002	-4.56	<.001	-0.017	0.021	-0.8	0.429
2000	0.008	0.002	4.78	<.001	0.019	0.007	2.68	0.01	0.019	0.007	2.64	0.011
Child*00	-0.008	0.002	-4.35	<.001	-0.008	0.002	-4.04	<.001	-0.009	0.003	-2.66	0.011
		1				**2**				**3**		
Over High School	**Coef**	**se**	**t**	**P**	**Coef**	**se**	**t**	**P**	**Coef**	**Se**	**t**	**p**
Child present	-0.007	0.002	-4.42	<.001	-0.006	0.002	-4.07	<.001	0.002	0.010	0.2	0.843
2000	0.003	0.001	3.05	0.004	0.001	0.005	0.31	0.758	0.000	0.004	0	0.998
Child*00	-0.002	0.002	-0.81	0.421	-0.002	0.002	-1.16	0.252	0.002	0.003	0.6	0.550
Sociodemographics		yes				yes				Yes		
State policy variables		no				yes				Yes		
Policies*kids interactions		no				no				Yes		

Note: All models estimated as linear probability models. All models include state fixed effects. Standard errors are clustered on the state.

Chapter 3 Appendix

Table 3.A1. Comparing Ai & Norton marginal effects with linear probability model coefficients

	1			**2**			**3**			**4**		
	Coef	se	t	Coef	se	t	Coef	se	t	Coef	se	t
Ai & Norton	-0.005	0.001	-3.13	-0.004	0.002	-2.72	-0.007	0.002	-3.18	0.000	0.005	-0.13
LPM	-0.004	0.001	-3.1	-0.004	0.001	-2.82	-0.004	0.002	-2.58	-0.002	0.002	-0.86
Sociodemographics		No			Yes			Yes			Yes	
State Policies		No			No			Yes			Yes	
Policies*Kids interactions		No			No			No			Yes	

Note: All models include state fixed effects. Standard errors are clustered on the state level.

Table A2a. Reduced form co-residence probit models, all observations

	1				2				3				4			
	Coef	se	t	p	Coef	se	t	p	Coef	se	t	p	Coef	se	t	P
Child present	-0.167	0.025	-6.66	<.001	-0.170	0.026	-6.51	<.001	-0.167	0.032	-5.17	<.001	-0.245	0.265	-0.93	0.355
2000	0.087	0.021	4.13	<.001	0.077	0.018	4.14	<.001	0.202	0.081	2.5	0.012	0.157	0.082	1.92	0.055
Child*00	-0.064	0.030	-2.11	0.035	-0.058	0.031	-1.89	0.059	-0.062	0.034	-1.83	0.068	0.068	0.051	1.33	0.182
Age					0.097	0.014	6.72	<.001	0.101	0.015	6.94	<.001	0.102	0.015	6.97	<.001
Age-squared					-0.001	0.000	-5.85	<.001	-0.001	0.000	-6.03	<.001	-0.001	0.000	-6.06	<.001
HS					0.002	0.028	0.07	0.946	0.004	0.030	0.15	0.883	0.003	0.030	0.11	0.912
Over HS					-0.117	0.029	-4.11	<.001	-0.119	0.028	-4.26	<.001	-0.120	0.028	-4.3	<.001
Black					0.146	0.023	6.38	<.001	0.141	0.024	5.74	<.001	0.141	0.025	5.66	<.001
Asian					0.498	0.070	7.15	<.001	0.546	0.069	7.87	<.001	0.549	0.070	7.82	<.001
Hispanic					0.203	0.034	5.89	<.001	0.206	0.035	5.81	<.001	0.205	0.036	5.77	<.001
Other race					0.158	0.036	4.41	<.001	0.170	0.038	4.51	<.001	0.172	0.037	4.59	<.001
Nonmetro					0.095	0.029	3.34	0.001	0.041	0.028	1.47	0.143	0.041	0.028	1.47	0.141
Same state					0.256	0.021	11.93	<.001	0.245	0.023	10.59	<.001	0.245	0.023	10.52	<.001
Medicaid asset test									0.000	0.000	-2.98	0.003	0.000	0.000	-2.94	0.003
Medicaid income test									0.000	0.000	-0.58	0.561	0.000	0.000	-0.46	0.647
CoN/Moratorium in place									0.068	0.044	1.55	0.12	0.069	0.045	1.52	0.129
Medicaid NH reimb rate									-0.002	0.001	-1.31	0.19	-0.001	0.002	-0.91	0.364
NH beds per 65+ capita									-0.003	0.004	-0.82	0.41	-0.001	0.004	-0.41	0.685
Medicaid Home Health $ per 65+ capita									0.000	0.000	-0.46	0.644	0.000	0.000	-0.5	0.621
State unemployment rate									0.050	0.027	1.85	0.064	0.030	0.030	0.99	0.32
Median rent									0.000	0.000	-2.25	0.025	0.000	0.000	-2.26	0.024
Policy*child interactions	no				no				no				yes			
Psuedo R-Squared	0.013				0.033				0.036				0.037			
Sample size	221577				221577				185528				185528			

Table A2a. Reduced form co-residence probit models, high school education or less

	1				2				3				4			
	Coef	se	t	p	Coef	se	t	p	Coef	se	t	p	Coef	se	t	p
Child present	-0.210	0.031	-6.76	<.001	-0.191	0.036	-5.38	<.001	-0.167	0.044	-3.8	<.001	-0.292	0.348	-0.84	0.40
2000	0.092	0.022	4.23	<.001	0.092	0.020	4.49	<.001	0.356	0.121	2.95	0.003	0.315	0.126	2.51	0.012
Child*00	-0.105	0.035	-2.99	0.003	-0.106	0.034	-3.09	0.002	-0.131	0.041	-3.16	0.002	-0.023	0.065	-0.36	0.723
Age					0.085	0.022	3.94	<.001	0.097	0.024	4.07	<.001	0.097	0.024	4.06	<.001
Age-squared					-0.001	0.000	-3.47	0.001	-0.001	0.000	-3.62	<.001	-0.001	0.000	-3.61	<.001
HS					-0.020	0.024	-0.83	0.406	-0.021	0.027	-0.78	0.437	-0.023	0.027	-0.83	0.406
Over HS					-				-				-			
Black					0.079	0.027	2.94	0.003	0.086	0.029	2.95	0.003	0.086	0.030	2.93	0.003
Asian					0.461	0.069	6.69	<.001	0.504	0.067	7.51	<.001	0.508	0.069	7.38	<.001
Hispanic					0.080	0.038	2.08	0.037	0.075	0.033	2.28	0.023	0.074	0.033	2.26	0.024
Other race					0.093	0.063	1.48	0.139	0.112	0.060	1.86	0.063	0.115	0.060	1.93	0.053
Nonmetro					0.078	0.033	2.36	0.018	0.032	0.035	0.9	0.367	0.032	0.035	0.91	0.363
Same state					0.266	0.022	12.13	<.001	0.266	0.022	11.89	<.001	0.265	0.022	11.97	<.001
Medicaid asset test									0.000	0.000	-2.77	0.006	0.000	0.000	-2.82	0.005
Medicaid income test									0.000	0.000	0.01	0.993	0.000	0.000	-0.02	0.985
CoN/Moratorium in place									0.157	0.065	2.44	0.015	0.174	0.061	2.83	0.005
Medicaid NH reimb rate									-0.005	0.002	-2.01	0.045	-0.005	0.003	-1.78	0.076
NH beds per 65+ capita									0.000	0.004	0.11	0.913	0.002	0.004	0.4	0.691
Medicaid Home Health $ per 65+ capita									0.000	0.000	-0.97	0.331	0.000	0.000	-0.86	0.39
State unemployment rate									0.069	0.032	2.16	0.031	0.049	0.036	1.36	0.174
Median rent									0.000	0.000	-0.8	0.424	0.000	0.000	-0.82	0.415
Policy*child interactions		no				no				no				yes		
Psuedo R-Squared		0.017				0.029				0.031				0.032		
Sample size		97146				97146				80786				80786		

Table A2c. Reduced form co-residence probit models, over high school education

	1				2				3				4			
	Coef	Se	t	p	Coef	se	t	p	Coef	se	t	p	Coef	se	t	p
Child present	-0.153	0.037	-4.18	<.001	-0.165	0.038	-4.33	<.001	-0.196	0.046	-4.31	<.001	-0.196	0.285	-0.69	0.492
2000	0.094	0.025	3.8	<.001	0.058	0.023	2.52	0.012	0.042	0.122	0.35	0.73	-0.004	0.112	-0.03	0.975
Child*00	-0.017	0.047	-0.36	0.721	-0.009	0.049	-0.19	0.85	0.020	0.052	0.37	0.709	0.170	0.080	2.11	0.035
Age					0.107	0.019	5.62	<.001	0.105	0.024	4.39	<.001	0.105	0.024	4.43	<.001
Age-squared					-0.001	0.000	-4.68	<.001	-0.001	0.000	-3.56	<.001	-0.001	0.000	-3.6	<.001
HS					-				-				-			
Over HS					-				-				-			
Black					0.209	0.029	7.3	<.001	0.191	0.031	6.18	<.001	0.192	0.031	6.15	<.001
Asian					0.527	0.075	7.01	<.001	0.578	0.074	7.76	<.001	0.580	0.074	7.82	<.001
Hispanic					0.346	0.036	9.73	<.001	0.358	0.041	8.68	<.001	0.358	0.041	8.72	<.001
Other race					0.208	0.058	3.57	<.001	0.213	0.065	3.29	0.001	0.215	0.065	3.31	0.001
Nonmetro					0.110	0.043	2.59	0.01	0.048	0.044	1.1	0.274	0.048	0.044	1.09	0.274
Same state					0.235	0.037	6.4	<.001	0.217	0.037	5.87	<.001	0.217	0.037	5.86	<.001
Medicaid asset test									0.000	0.000	-0.92	0.357	0.000	0.000	-0.88	0.381
Medicaid income test									0.000	0.000	-1	0.317	0.000	0.000	-0.73	0.468
CoN/Moratorium in place									-0.095	0.045	-2.13	0.033	-0.112	0.045	-2.5	0.012
Medicaid NH reimb rate									0.001	0.003	0.35	0.728	0.002	0.003	0.65	0.515
NH beds per 65+ capita									-0.007	0.007	-1.01	0.312	-0.005	0.007	-0.75	0.45
Medicaid Home Health $ per 65+ capita									0.000	0.000	0.17	0.864	0.000	0.000	0.05	0.96
State unemployment rate									0.026	0.037	0.7	0.481	0.007	0.036	0.2	0.842
Median rent									0.000	0.000	-2.72	0.007	0.000	0.000	-2.7	0.007
Policy*child interactions	No				No				no				yes			
Psuedo R-Squared	0.014				0.038				0.04				0.041			
Sample size	124091				124091				104596				104596			

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Chapter 4

The Effects of Education on Health: A Cost-Utility Approach

4.1. Introduction

Extensive research across several disciplines documents and attempts to explain social patterns in health. A recurrent finding from this literature is that socioeconomic position is strongly and negatively correlated with health status across many health outcomes, including mortality, specific diseases, mental health, and disability. There is less agreement, however, over whether and the extent to which socioeconomic position has a causal effect on health. Taking the example of educational attainment as a measure of socioeconomic position, increased education is hypothesized to improve health through a number of different pathways, such as by facilitating higher income and more positive work environments, along with improving cognitive skills and improving health behaviors (Cutler and Lleras-Muney 2006; Link and Phelan 1995). On the other hand, it is possible that educational attainment is correlated with other characteristics that may affect future health outcomes, such as underlying ability or time preferences (Fuchs 2004). While it is difficult to dismiss any of these arguments on conceptual grounds, recent evidence suggests a considerable causal effect of education on future health outcomes (Lleras-Muney 2005).

Many commentators argue that investing in education may be one of the most promising ways to improve population health (Mechanic 2007; Monheit 2007). But, policy decisions about increasing education should be informed (in part) by estimates of the extent of the health returns to education. Few studies have attempted to explicitly quantify these returns. For example, one recent estimate suggests that an additional year of education leads to an additional 0.6 years of life expectancy, which is valued between $13,500-$44,000 (depending on discount rates), assuming that the value of a year of life (from a societal perspective) is valued at $75,000 (Cutler and Lleras-Muney 2006).

In this research we attempt to improve the knowledge about the overall magnitude of the relationship between education and health, without assessing the mechanisms through which education affects health. To that end, we apply a familiar metric, the Quality-Adjusted Life Year (QALY), to studying differences in longitudinal health profiles across educational groups. The QALY measure is fundamental to cost-utility analysis. The basic idea behind the QALY metric is that individuals assign different levels of utility to different health states. Prior research has linked data on specific health states to health-related quality of life (HRQL) weights, which represent the proportion of a year of "healthy life" that is experienced under different health conditions (Nyman et al. 2007). HRQL weights can be combined with mortality data to describe the number of QALYs experienced over a set period of time.

This research builds on the existing literature in several ways. First, we assess the magnitude of the relationship between education and health using QALYs, which account for the fact that both longevity and health-related quality of life are valuable health outcomes that may be affected by education. Second, we use several waves of longitudinal survey data combined with detailed mortality data to construct profiles of QALYs experienced over a 15-year period of time. Third, we use a natural experiment in years of education to assess the causal effects of education on health. We believe that these three features of our study allow us to deliver some of the most comprehensive estimates of the health returns to education to date.

In our descriptive analyses, we find a strong monotonic association between years of education and the number of QALYs experienced over a 15-year study period. To deal with the potential endogeneity of education with other unobserved factors that could also affect health outcomes, we use changes in compulsory schooling laws as a plausibly exogenous source of variation in educational attainment. Results from instrumental variables models support the hypothesis that education has a causal effect on health. Those results are consistent with other comparable research in suggesting that the effect of education on QALYs may be understated in OLS models, although the instrumental variables results are estimated with somewhat less precision.

The rest of the paper proceeds as follows. Section 2 reviews the evidence on the effect of education on health. Section 3 describes compulsory schooling and child labor laws and reviews their applications in empirical research on the effects of education. Section 4 describes the methods used in this study, and section 5 reports the results. Section 6 discusses the policy implications of our findings and concludes.

4.2. Prior Studies on the Effect of Education on Health

Many studies find robust evidence that higher education is correlated with better health outcomes (for reviews, see Cutler and Lleras-Muney 2006; Grossman and Kaestner 1997), and the strength of this association may be growing (Meara, Richards, and Cutler 2008). We focus in this section on the studies that attempt to empirically assess the causal effect of education on health using methods that treat education as being a potentially endogenous variable. The challenge in this area of research is to find variation in education that is plausibly exogenous with respect to health outcomes. In the past decade, a small literature has emerged that uses instrumental variables methods to assess the effect of education on health. The results from this literature generally point to a causal effect of education on health, although not all the relevant studies support this conclusion.

Some researchers have used the random element of risk of being drafted into the Vietnam war as an instrument for higher education attainment, with the intuition being that men in cohorts with a higher probability of being drafted were more likely to enroll in college to avoid induction and that the risk of being drafted was otherwise uncorrelated with health outcomes (de Walque 2007; Grimard and Parent 2007). Those studies focus on smoking and find evidence that higher education reduces the probability of smoking and may increase smoking cessation among smokers.

Other researchers have exploited variation in birth timing as an instrument for educational attainment, with the intuition being that individuals born just before and after a cut-off point for compulsory enrollment in school or staying in school have different levels of education, and that birth timing should not have direct effects on health. For example, one study uses quarter-of-birth as an instrument for education and finds evidence of a causal effect of education on health at old ages (although these results are questionable because quarter-of-birth is a weak predictor of education in his data) (Adams 2002). Clark and Royer (2007) took a more refined analytic approach using data on exact date-of-birth from the UK, and found little to no evidence that increased education led to better adult health status or reduced mortality.

A final group of studies uses changes in state compulsory education and child labor laws to identify the effect of education on health. The intuition behind this approach is that these laws induced individuals to stay in school longer than they would have otherwise and that changes in these laws were uncorrelated with factors that would have affected health outcomes. Lleras-Muney (2005) used changes in these laws between 1915 and 1939 as instruments for education, finding that education resulted in lower aggregate mortality rates and that the IV results were stronger than the comparable OLS results.

In a subsequent study, Mazumder (2007) replicates Lleras-Muney's aggregate mortality analysis with more data and also applies her instruments to microdata from the Survey of Income and Program Participation (SIPP). Notably, Mazumder finds that Lleras-Muney's aggregate-level mortality findings are not statistically significant after including state time trends. In his micro-level IV analyses, however, he finds that education has a significant effect on general health status that is robust to the inclusion of state time trends. But, he finds mixed evidence of the effect of education on specific health conditions. However, this analysis is complicated by focusing on older adults (with a mean age of 72), for whom mortality selection is an important consideration. In addition, the extent to which the micro-level findings represent the causal effects of education is questionable because none of the identifying instruments significantly predicted education at the $p<.05$ level in the first-stage, with corresponding F-tests indicating very weak instruments.[11] He also finds that the laws had less of an effect on education from 1915-1926 than they did from 1927-1939, suggesting that it may be valuable to look at the effects of changes in compulsory schooling laws in later cohorts, for whom the laws may have mattered more.

Another shortcoming in the literature on the relationship between social position and health is that few datasets document patterns in health within individuals over extended periods of time. Following individuals over time is useful for establishing the cumulative burden of poor health across groups, which may be a more meaningful outcome from a policy perspective than point-in-time measures of health status. Some exceptions do exist, including the National Longitudinal Study of Youth (NLSY), and the Health and Retirement Study (HRS). But these datasets are limited in focusing on a restricted period of the life span (youth or old age, respectively) and in examining a relatively small set of health outcomes (NLSY).

Along with these data limitations, most longitudinal studies of social patterns in health generally focus on one specific outcome; for example, mortality, disability, or a specific health condition. That approach, while valuable, has the limitation of not assessing people's cumulative health profiles. For instance, a study of the effect of education on mortality does not take into consideration the extent of morbidity prior to death, which is also a very important outcome. A longitudinal study that focuses on a single outcome, such as a single disease, may overstate or understate overall educational differences in health. Such a study could overstate those differences if different educational groups get sick from different things, but all ultimately get sick at comparable rates. On the other hand, it could understate social disparities if certain social

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[11] We are grateful to Bhash Mazumder for graciously sharing his first-stage results with us.

groups are more likely to get sick from lots of things, implying that total morbidity and/or mortality is concentrated in those groups. This suggests the potential utility of using dependent variables that integrate multiple measures of disease and mortality.

4.3. Compulsory Schooling Laws

There was considerable variation across states in compulsory schooling and child labor laws from 1914-1974. These laws dictated the maximum ages for school entry, the minimum ages for school exit, and the minimum age or level of schooling to obtain a work permit. Most states changed their laws at least once over this time period, many states changed their laws several times, and while the general trend was in the direction of stricter laws, there were a number of instances where laws were actually relaxed. Figure 1 shows the variation of compulsory schooling laws within states over time. Importantly, these laws appear to be uncorrelated with local tastes for education. For instance, these laws only operate by increasing levels of education through high school. If these laws were correlated with local tastes for educational attainment, we would expect that they would also predict educational attainment beyond high school, but this is not the case (Acemoglu and Angrist 2000). Another concern is that stricter laws may have emerged in states that already had higher levels of education or that already had upward trends in level of education. This does not seem to be the case, however, as levels of education have been found to predict contemporaneously stricter laws, but not stricter laws in the future (Lleras-Muney 2002). This, along with evidence of flat trends in education in the years immediately before and after the laws' enactments (Lochner and Moretti 2004) suggest that higher education resulted from the laws, and not vice versa.

Possibly more important for this study is the potential concern that these laws may have been correlated with other factors that may have directly affected health. A strength of using these laws as an instrument for education is that in order to violate the IV assumptions, any other factors that were correlated with changes in compulsory education laws that occurred when an individual was 14 must have had a direct effect on that individual's health many years (indeed decades) later. Lleras-Muney (2005) supports this assumption by noting that, "there is no evidence that the laws included any clauses or restrictions that would have affected health independently. For example, there were no lunch programmes provided as part of school attendance. Also the states that led in education during this period (the prairie states) were not the same states that led in health (north-eastern states) (p.207)." Furthermore, there is no evidence of a relationship between the strength of compulsory education laws and a measure of states' health resources, the number of doctors per capita (as described below).

Mazumder questions whether compulsory schooling laws were actually uncorrelated with other factors that might directly affect subsequent health. Specifically, he proposes that the laws may affect health because of smallpox vaccination in schools (2007) or that, contrary to Lleras-Muney's assertion, stricter compulsory schooling laws were in fact correlated with school lunch programs (Mazumder 2008). However, in the one year of data for which the strength of smallpox vaccination laws are available, the strength of these laws are in fact uncorrelated with the strength of compulsory schooling laws (author's calculations from data in Mazumder (2007)). Additionally, it is implausible that smallpox could have had more than a negligible effect on health later in life. The vast majority of US smallpox cases after 1900 were caused by the less virulent of the two smallpox strains, for which long-term health effects were extremely rare. Even for the more virulent strain of smallpox (which was all but eradicated in the US by 1927), long-term health consequences were very uncommon, causing blindness and encephalitis in less than 1% of survivors in the 20th century (Fenner et al. 1988).

The claim that compulsory schooling laws were correlated with school lunch programs is also quite questionable. Before the implementation of the National School Lunch program in the late 1940's, school lunch programs were mostly at the discretion of local authorities. There is little data available on the extent of these programs, but a visual comparison of the few local programs mentioned in Mazumder's source with changes in compulsory schooling laws does not suggest any clear correlation with stricter state-level compulsory schooling laws. Further, data available from after 1949 on the percent of students in each state receiving school lunches suggests that stricter compulsory schooling laws were *negatively* correlated with school lunch usage, which is in the opposite direction of the relationship that Mazumder proposes (author's calculations from data from the Statistical Abstracts of the United States).

Along with not having a direct effect on later-life health outcomes, using compulsory schooling laws as an instrument necessitates that the laws did in fact increase educational attainment. Although these laws were responsible for only a small share of the increase in educational attainment over the 20th century, a number of studies have demonstrated that these laws are strong predictors of education and have used these laws as instruments to study the effect of education on mortality (Lleras-Muney 2005, Mazumder 2007), income (Acemoglu and Angrist 2000; Oreopoulos 2006), criminal behavior (Lochner and Moretti 2004), and voting (Milligan, Moretti, and Oreopoulos 2004). The strength of the instrument will also be assessed empirically in this research.

4.4. Methods

4.4.1. Data

Our data come from the Americans Changing Lives (ACL) surveys. The ACL study interviewed a sample of 3,617 individuals in 1986 that was nationally-representative of the 1986 U.S. population of non-institutionalized adults ages 25 and older. The sample was interviewed again in 1989, in 1994, and in 2001. Of the 3,617 respondents, 49 were excluded because they were born before 1895, for whom measures of the identifying instruments are not available. An additional 231 respondents were excluded who were born outside of the US or did not report one single state where they lived in until age 16. We also excluded 252 more observations that were nonrespondents at waves 3 and 4 and who did not die before wave 4. This leaves a sample of 3,085 observations.

In addition, detailed mortality data were collected for the entire sample and timing of death was recorded to the month. Information on deaths among sample members was obtained primarily from the National Death Index and secondarily from informant reports, with probable deaths being certified after receiving a death certificate from a state vital registration office. Between Wave 1 and Wave 4, 1,184 of the original 3,617 ACL respondents died (32.7%), with 1,159 of those deaths (97.9%) certified with a death certificate. The remaining 25 deaths were carefully reviewed, and death appears certain in all cases even though a death certificate could not be located. For these cases, the timing of death was estimated based on the information provided by the informant who reported the death.

4.4.2. Dependent Variables

Measures of QALYs are generated by combining information on life span with the health-related quality of life (HRQL) experienced over that life span. Measures of HRQL weights are commonly derived from preference elicitation methods (such as time trade-off or standard gamble techniques). While these methods are integral for determining the HRQL weights that are associated with certain health conditions, they are less useful for understanding patterns of QALYs at a population level or in response to a given health intervention. To address this concern, researchers have developed survey instruments that assess general health status. These instruments (e.g., the EQ-5D, the HUI, the SF-6D) can be used in cost-utility analysis because of valuation studies that have related these instruments to HRQL weights using preference elicitation techniques.

However, many major health surveys do not include survey instruments for general health status that have been directly linked to HRQL weights. Such surveys often use other measures of health status, such as the common self-rated health item, or self-reports of specific diseases or disabilities. Our data source, the ACL study, falls into this category. Our goal is to construct estimates of QALYs experienced by the ACL sample over the 15-year study period, and we create two versions of this dependent variable.

The two dependent variables are constructed with the methods proposed by Nyman and colleagues (Nyman et al. 2007). Nyman and colleagues used a large, nationally-representative health survey (the Medical Expenditures Panel Survey) that included the EQ-5D, along with self-rated health and 10 specific self-reported health conditions identified. They then equated those more conventional measures of health status to the HRQL weights associated with the EQ-5D that had been previously established in a study that linked the EQ-5D with HRQL weights using time trade-off methods (Shaw, Johnson, and Coons 2005). This allowed Nyman et al. to predict how much each condition and each self-rated health category lowers the HRQL weight for a given year of life. For example, they estimate that the HRQL weight for someone with joint pain is .156 lower than someone without joint pain, and that the HRQL weight for someone with poor self-rated health is .511 lower than someone with excellent self-rated health.

Several features of Nyman and colleague's analysis warrant mention. First, they provide two separate methods for deriving HRQL weights from survey data. One method uses the five-level self-rated health (SRH) question, and the other method uses self-reported data on 10 common health conditions. Second, they estimate the marginal effect of different health conditions on a HRQL weight by estimating regression models where the health conditions enter in to the equation additively. That is, the effect of a given condition on the HRQL weight does not vary with any other characteristics or conditions. Third, they give estimates of the marginal effect on HRQL weights of 10 conditions that were defined as, "priority conditions," by AHRQ (e.g., diabetes, heart diseases, lung diseases, hypertension). However, they did not include other important conditions in their analysis, such as disability or mental disorders. We will return to these points in greater detail in the discussion section.

We use data available from all four ACL waves to construct the number of QALYs experienced by our sample over the 15-year study period. To derive a HRQL weight for each of the years between ACL surveys, we assumed linear trends in each of the health conditions between survey waves. For instance, if a respondent reported having diabetes in two contiguous survey waves, he/she was imputed to have diabetes in each of the intervening years. If he/she did not report having diabetes in one wave, but did report it in the next wave, then he/she was imputed to have diabetes in the latter half of the intervening years between survey waves. If a respondent was not interviewed in a given wave, we used data on the adjacent waves to impute health status in the same way that we treated the years between contiguous survey waves. We also assumed that health status did not change between the last survey measurement and death, which likely leads to overestimating health status in the years prior to death. These methods allow us to assign HRQL weights η_{it} to each person-year of observation. More specific details of how HRQL weights were assigned to each year of data are described in Appendix 1.

Our dependent variables are a respondent's sum of HRQL -weighted years lived over the 15-year study period. These variables are constructed according to the following formula:

$$\sum_{t=1}^{15}(\eta_t * Alive_t),$$

where *Alive* is the proportion of year *t* that was lived by the respondent. Survival is measured to the month. For instance, individuals who died six months into a year were coded as having lived for .5 of that year.

4.4.3. Independent Variables

We measure education in terms of completed years of education. Race/ethnicity is coded as a categorical variable of white, black, or other race/ethnicity (the small sample size of the latter group prevents us from disaggregating into more specific categories). Our models also include a dummy variable for sex, and we use year-of-birth fixed effects to allow for a fully flexible effect of age.

Several sources of data on compulsory schooling and child labor laws are available. We use data from Acemoglu & Angrist (2001) because their time frame covers most of the ACL respondents. Consistent with prior empirical research, we assign each individual to the compulsory schooling and child labor laws that were in their state of residence when they were age 14. We follow Acemoglu and Angrist's algorithm for creating two sets of dummy variables to summarize the compulsory education and child labor laws (the algorithm is described in Appendix 2). Compulsory schooling laws are coded as dummy variables for 7 years or less, 8 years, 9 years, 10 years, and 11 years or more of compulsory education. Child labor laws are coded as dummy variables for 6 years of education or less, 7 years, 8 years, 9 years, 10, or 11 years of education or more required to receive a work permit. This coding is very similar to that used in other studies, except that our coding adds an additional category to each dummy variable because there is sufficient variation in the laws in our data to support the additional categories.

Most researchers use state-of-birth data to assign individuals to their state of residence at age 14, and allow that this may lead to measurement error in the first stage (an exception is Milligan et al.'s (2004) analysis of National Election Survey data), and possibly in the second stage as well. We are able to reduce that measurement error because ACL respondents were asked which state was their primary home until age 16. We are also able to assess the effect of this measurement error because the ACL also included a question about place of birth.

4.4.4. Statistical Analysis

Our analysis proceeds in three steps. First, we present descriptive statistics for our sample, including the distribution of compulsory education and child labor laws across states and across time and non-parametric graphs of the unadjusted relationship between years of education and 15-year QALYs. Second, we estimate OLS models of 15-year QALYs. The OLS models of QALYs take the following form:

$$QALY_i = \alpha_0 + \beta Education_i + \gamma X_i + \phi_1 Age_i + \varepsilon_i$$

Education is measured as number of completed years of education and is top-coded at 17. *X* includes sex and race (measured as white/black/other). We use year-of-birth dummies (*Age*) to account for age in the most flexible way.

Because of the potential correlation between education and the error term (ε,) in our third set of analyses we estimate IV models of the effect of education on 15-year QALYs. The IV models take the following form:

$$Education_i = \alpha_0 + \eta Z_{st} + \gamma X_i + \theta_1 State_s + \theta_2 Cohort_t + \mu_i$$

$$QALY_i = \alpha_0 + \delta \widehat{Education}_{ist} + \gamma X_i + \phi_1 State_s + \phi_2 Cohort_t + \varepsilon_i$$

The vector *Z* includes all of the identifying instruments. Identification of the effect of education on health comes from changes over time in the compulsory schooling and child labor laws within a given state. Empirically, this means including state-of-youth (*State*) and birth cohort (*Cohort*) fixed effects in the first-stage regression of education on compulsory schooling and child labor laws. The key identifying assumption is that μ is uncorrelated with ε. While some of the studies that use compulsory schooling laws to identify the effect of education include region-time trends (Lleras-Muney 2005) or state-time trends (Mazumder 2007), we do not include such variables in our specification (similar to Lochner & Moretti (2004) and Milligan et al. (2004), for example). We justify this specification primarily because there is no evidence that stricter compulsory schooling laws were simply part of pre-existing trends towards higher education (Lleras-Muney 2002; Lochner and Moretti 2004) and because we know of no credible evidence that increases in compulsory schooling were correlated with other factors that could have directly improved health status in later life. We also avoid including these variables because given our relatively small sample size, region- or state-time trends may pick up too much variation that is actually exogenous due to the laws (this is even more of a concern with state-time trends, which may be estimated quite imprecisely due to small cell sizes). Nevertheless, we do explore the inclusion of region-time trends in our sensitivity analyses.

Because of our relatively small sample size, we keep our models as parsimonious as possible. One possible problem with the current specification is that increases in compulsory schooling may have been correlated with greater local health care resources, which might affect health later in life. To test whether this is a major concern, we regressed a measure of state health care resources, doctors per capita, on the compulsory schooling laws, along with state and year fixed effects (Table A1). We find no relationship between the laws and doctors per capita, improving our confidence in the assumption that the compulsory schooling laws are not correlated with the error term in the QALY equation.

Although two-stage least squares (2SLS) is a conventional IV estimator, it is not the ideal estimator in the present application. First, 2SLS is inefficient in the presence of multiple instruments and heteroskedasticity, in which case GMM is the most efficient estimator (Baum, Schaffer, and Stillman 2003). Second, because our sample size is much smaller than most studies that use compulsory schooling and child labor laws as instruments,[12] the instruments are relatively weaker in our application than in other studies. If the instruments are relatively weak, 2SLS has greater potential bias than the limited-information maximum likelihood (LIML) estimator. The literature is less clear on the optimal estimator when both of these conditions exist, so we follow the suggestion of Stock, Wright, and Yogo (2002) and estimate with the continuously-updating GMM estimator (which is equivalent to the LIML estimator under homoskedasticity and is available as the ‘cue’ option in Stata’s ivreg2 procedure). All analyses are clustered at the state-of-youth and birth year level to adjust for potential dependence between observations.

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[12] Other studies that use these instruments have samples that are larger by up to three orders of magnitude.

4.5. Results

4.5.1. Descriptive Statistics

The description of the sample is found in Table 1. The average number of QALYs over the 15-year study period ranged from 11.4 to 11.7, depending on the measure. The average age of the sample is 54 years old at baseline. The average number of years of education is 11.5 and there is substantial variation in educational attainment, as one-fifth of the sample did not have any high school education and two-thirds of the sample did not have any education beyond high school. There is also considerable variation in the compulsory schooling laws that were in place when the respondent was age 14.

To get a sense of the unadjusted relationship between years of education and QALYs over the 15-year study period, we graph QALYs by education, using a local polynomial smoother (Figures 4.2a and 4.2b). The relationship between education and QALYs is positive, monotonic, and relatively smooth for both measures of the dependent variable.

4.5.2. OLS results

The OLS results confirm the basic relationship that is illustrated in Figure 2, controlling for sex, race/ethnicity, and age. As presented in Table 2, depending on the dependent variable used, one additional year of education is associated with an increase of 0.15 – 0.20 QALYs over the 15-year study period (both estimates are significant at $p<.001$). In addition, women experienced significantly more QALYs than men, and Blacks and other races/ethnicities experienced significantly fewer QALYs than whites.

4.5.3. Instrumental Variables results

Before discussing the IV estimates of the effect of education on health, we review the first-stage estimates of the effect of the instruments on education, since the first stage has important implications for interpreting the IV estimates. We conducted first-stage regressions using just the compulsory schooling laws, just the child labor laws, and both sets of dummy variables to see which instruments predicted education most robustly in our data. After controlling for state and year fixed effects and clustering the standard errors on state of youth and year of birth, the dummy variables for compulsory schooling and child labor laws did predict education, although the compulsory schooling laws appear to be the main driver. When we included only the 4 compulsory schooling dummy variables, all four coefficients have the expected signs and are significant at $p<.001$, and the F-test for the joint significance of the 4 variables is 7.59. The coefficients from the first-stage model are presented in Table 3. When we only included the 4 child labor law dummy variables, all four coefficients have the expected signs, but only three of the coefficients are significant at $p<.05$ and the F-test for the joint significance of the 4 variables is a much weaker 3.12 (results not shown). When we included the compulsory schooling and child labor law dummies, it is clear that the robust predictors of education are the compulsory schooling laws. The coefficients on those laws are all significant at $p=.001$ or lower, while the child labor laws have no effect on education, conditional upon the compulsory schooling laws (results not shown). For this reason, we rely only on the compulsory schooling laws in our IV analyses.

Although the F-test for the joint significance of the compulsory schooling law instrument is lower than the commonly-cited benchmark of F>10 for a strong instrument, Stock and Yogo (2004) conclude that if one estimates the IV model with LIML, our F-statistic is large enough to reject the null hypothesis of being a weak instrument (the critical value for rejection with 4 instruments is 5.44). So while our instruments are not overwhelmingly strong, we have confidence that our estimates suffer from, at most, limited weak-instrument bias.

Other diagnostic tests support the use of our instruments. The partial R-squared associated with the instruments is .012, suggesting that the instruments have substantial predictive power. We can reject the null hypothesis that the IV model is under-identified (p<.001). In addition, Hansen-J tests for overidentification do not reject the null hypothesis that the instruments are valid, implying that we are correct in excluding the instruments from the QALY equation (although the test for the SRH-based QALY model does come close to rejection, with p=.083). Finally, tests for heteroskedasticity all strongly reject the null hypothesis of homoskedasticity (all tests reject with p<.001), suggesting that GMM estimation is warranted (Baum et al. 2003).

Both of the IV estimates of the effect of education on disease-based and SRH-based QALYs over the 15-year study period point to a stronger effect of education than the OLS models suggested (Table 4). For the model of SRH-based QALYs, an additional year of education results in 0.38 additional QALYs (p=.065). For the model of disease-based QALYs, an additional year of education results in 0.40 additional QALYs (p=.041). These estimates are less precise than the OLS estimates, as is common in IV models. Nevertheless, they do indicate a significant causal effect of education on the number of QALYs experienced over the 15-year study period.

4.5.4. Sensitivity analyses

We also tested to see how robust our results were against different model specifications and different sample specifications (Table 5). First, we re-estimated our IV models after including region-linear time trends. An important caveat of these models is that the continuously-updated GMM estimator would not converge under this model specification, so we estimated with 2SLS, which likely results in a loss of efficiency. As expected (and consistent with Lochner & Moretti's (2004) analyses), including region-time trends soaks up much of the variation in compulsory schooling laws, and the first stage F-statistics are much smaller, indicating weak instruments. The IV estimate for the disease-based QALY model is comparable in magnitude to the main results, but the estimate for the SRH-based QALY model is smaller than the main IV results (but still larger than the original OLS estimate), and both coefficients are estimated with much less precision.

Second, we re-estimated our models with the subset of respondents who responded to all four ACL waves, or responded to all waves until they died. We also were unable to estimate with the preferred continuously-updated GMM due to the smaller sample size, so these models are estimated with the less-efficient 2SLS. Under this smaller sample size of 2,382, the first-stage estimates are somewhat weaker, and the point estimates are nearly identical to the main results, but are estimated somewhat less precisely.

Third, we re-estimated our models after including individuals who did not die during the ACL period but were survey nonrespondents at waves three and four or at waves two, three, and four. We imputed the HRQL weights in the unobserved latter period of the ACL to be the same as their latest available measurement, which assumes that those individuals did not experience changes in health status as they aged. This increased the sample size to 3,334 and raised the strength of the instruments to F>8.50. However, the point estimates under this sample specification were smaller than the main IV results (but still larger than the OLS estimates), and were estimated less precisely than the main IV results. This may be because the HRQL weights for the imputed years for those non-respondents were assumed to not change after their latest ACL measurement (either in wave 1 or 2), whereas ACL respondents who did not respond at later waves had poorer health.

In our fourth sensitivity analysis, we test whether our estimates are considerably different if we use state-of-birth to assign individuals to the instruments, which is the standard approach in the literature. Interestingly, 15% of our sample reported that their primary state of residence until age 16 was different from their state of birth. Not surprisingly, the first stage estimates when using state-of-birth to assign compulsory schooling laws were a little weaker, reflecting measurement error in the instruments. The joint F-statistic for the significance of the instruments was 7.11, which is slightly weaker than in the main analyses (F=7.59). The coefficients for both versions of the dependent variables were slightly higher than in the main specification, and the p-values were slightly lower, and may be due to weaker instruments slightly weaker instruments.

In our final sensitivity analyses, we stratified the sample by median age, to see whether the effects of education on health are comparable earlier and later in life. The results of these analyses actually suggest very divergent patterns by age. The older age groups have qualitatively similar effects as the main analyses. But the younger age groups have a negative effect of education on health. While these results may suggest that the health returns to education accrue mostly at older ages, neither of these results were close to being statistically significant, and the first-stage F statistics were both extremely small, calling into question how much we can learn from these stratified analyses.

4.5.5. Estimating Lifetime Returns to Education

Finally, we produce back-of-the-envelope calculations of the overall health returns to education. Specifically, we estimate the marginal effect of an additional year of education for a hypothetical individual who is 50 years old at the start of the ACL (which is slightly below the sample mean of 54 years old at the start of the ACL), considering the number of QALYs that would have been experienced from age 18 until the end of the ACL study. To compute this estimate, we assumed that all individuals have perfect health at age 18 (HRQL weight = 1) and that the IV estimates of the effect of education on QALYs do not vary by age. First, we estimated IV models (using the same specification as the main analyses) where the dependent variable is the HRQL weight at the first ACL wave. We found that a year of education results in a 0.01 higher HRQL weight (regardless of which measure of HRQL we use). We then used that model to predict the wave-1 HRQL weight for a hypothetical individual who was 50 years old at the start of the ACL. We then assigned a HRQL weight for each year between age 18 and 50 by assuming that HRQL decayed at an exponential rate between age 18 and the first ACL wave.[13] Finally, we augment those estimates with the main IV estimates of the effect of education on QALYs from the 15 years of ACL data, and sum the total predicted QALYs from age 18 through the end of the ACL period.

If we do not discount QALYs, then our IV results suggest that for someone who entered the ACL at age 50, one year of education results in an additional 0.47 – 0.51 QALYs (depending on which measure of QALYs is used) from age 18 through the ACL study period. If we assume (conservatively) that a QALY is valued at $75,000, then the QALY gain from a year of education is monetarily equivalent to an additional $35,400 - $38,400. If we discount QALYs at a 3% rate starting from age 18, our results suggest a return of 0.15 – 0.17 QALYs to a year of education, which has a monetary value of $11,400- $12,450 (again assuming that a QALY is valued at $75,000).

4.6. Discussion

Ideally, to assess the effect of an additional year of education on lifetime QALYs, one would want to observe the health status of a sample from the time of completing education until mortality. In the absence of such data, we combined survey data from a 15-year longitudinal study with detailed mortality data to study the effect of education on QALYs. We found that more education is significantly related to more QALYs, and our IV models suggest that there is in fact a causal effect of education on QALYs. Although

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[13] Note that this is more conservative than assuming a linear decline in HRQL between age 18 and the start of the ACL.

this study did not aim to unpack the specific mechanisms through which education affects health, it does suggest that the potential health returns to education may be substantial.

Very few researchers have explicitly estimated the monetary value of health returns to education. Interestingly, our estimates of the health returns to an additional year of education are strikingly similar to those reported by Cutler and Lleras-Muney (2006), even though their analysis did not adjust for the endogeneity of education and looked only at life expectancy. They note that an additional year of education leads to roughly $80,000 in additional discounted lifetime earnings. Based on that figure, our estimates of the discounted health returns to education suggest that the overall value of an additional year of education is at least 14% higher if health is also included (and valued at a conservative $75,000/QALY). However, our estimates likely understate the overall health returns to education, because we only observe each individual in our sample for a maximum of 15 years. This implies that we do not observe the full effects of education on life expectancy, since two-thirds of the ACL sample were still alive at wave 4, and there is strong evidence of a causal effect of education on mortality (Lleras-Muney 2004).

We can also use our estimates to assess the cost-effectiveness of an additional year of education in producing health. It is difficult to estimate the incremental societal costs of a year of education, because that cost varies by whether one considers secondary or post-secondary education, and by the target population. For example, one might imagine that the costs associated with raising education through implementing and enforcing stricter compulsory schooling laws increase with the number of years of required education, due to progressively higher opportunity costs for those who would otherwise not attend school. With those caveats in mind, we compare our estimates of discounted health returns to a year of education for an individual who was 50 at the start of the ACL with the direct expenditures per pupil (expressed in 2007 dollars) for a year of secondary school in 1955 (when that cohort was finishing high school) as a measure of the incremental cost of a year of education.[14] Those estimates imply a cost-effectiveness ratio of $17,300-19,600/QALY, which is considered “a bargain” in the medical cost-effectiveness literature. If we use current levels of direct expenditures for secondary school per pupil,[15] our estimates imply a cost-effectiveness ratio of $67,500-76,500/QALY, which is still well within the range that is generally considered “cost-effective” for health interventions (Hirth et al. 2000).

This study has several important limitations. First, our measures of HRQL weights are far from ideal. While it would be preferable to construct weights directly from an established health utility measure, such data are not available in large-scale, longitudinal studies with detailed mortality information. Second, while our IV estimates help to improve the understanding of the causal effect of education on overall health, these estimates likely do not represent population average treatment effects. Because compulsory schooling laws affect educational attainment only through high school, the IV estimates only provide evidence of the effects of education on health for lower levels of education. Whether the causal effect of higher education on health is greater or less than the effect of lower levels of education is not well understood. Other research has found that for some health measures, the relationship between education and health is roughly linear at all levels of education, while for other health measures, the relationship

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[14] Our estimates imply that increasing education by 5.9-6.7 years adds one (discounted) QALY for an individual age 50 at the start of the ACL. Total expenditures per pupil in 1955-56 were $2,939 (in real 2006-2007 dollars) (National Center for Education Statistics 2007).

[15] Total expenditures per pupil in 2004-05 were $11,470 (in real 2006-20077 dollars) Ibid.

between education and health is only apparent at higher levels of education (after roughly 10 years of school) (Cutler and Lleras-Muney 2006). This suggests that the health returns to higher education could potentially be greater than what we observe. In addition, there is evidence from smoking behavior that the relationship between college education and health may in fact be causal (de Walque 2007; Grimard and Parent 2007; MacInnis 2006). Third, even though we justify our primary model specifications by citing evidence that compulsory schooling laws did not emerge out of trends towards higher education and noting the lack of evidence that changes in compulsory schooling laws were correlated with other factors that might have directly affected health in later life, our IV results are sensitive to the inclusion of region-time trends. The relatively small sample size of the ACL may explain the reduced strength of the instruments after including region-time trends. Finally, although we reject that our instruments are formally defined as "weak" under our choice of estimator, instrument strength is nevertheless remains a concern.

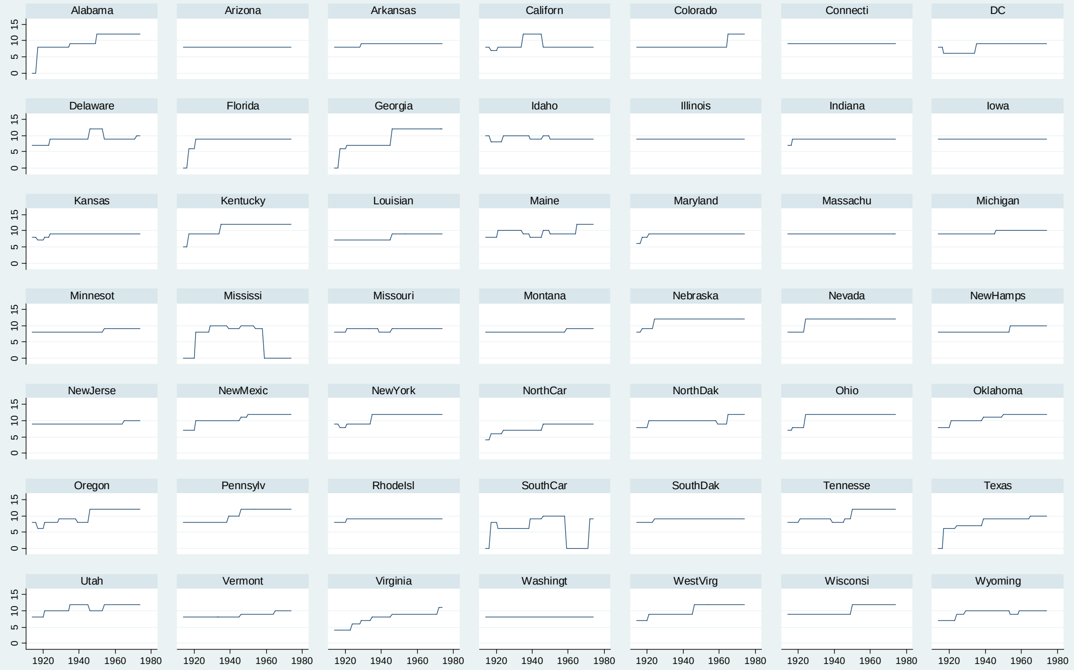
Our findings are consistent with the literature that uses compulsory education laws as instruments for education in that, contrary to a priori expectations, the IV coefficient on education is often substantially larger than the OLS coefficient. An explanation is that the IV coefficients represent the local average treatment effect (LATE) for a very select group of people who were affected by the instruments, and that these LATE estimates are probably considerably stronger than the population average treatment effect (ATE) (Card 1999). Oreopolous (2006) notes that this explanation does not necessarily hold, by demonstrating that IV coefficients on *income* are very similar across situations where the LATE represents a more unique subpopulation and where the

LATE converges upon the ATE. Nevertheless, in the same paper he finds that for *health* outcomes, the IV coefficient is much stronger than the OLS coefficient when the LATE corresponds to a unique subpopulation, whereas the IV coefficient is identical to the OLS coefficient when the LATE converges to the ATE. So it may be premature to reject Card's explanation of heterogeneous treatment effects, and it remains important to consider the different dimensions in which the propensity to respond to the instrument varies with individuals' characteristics.

Even though understanding the specific mechanisms through which education affects overall health status is not the specific aim of this paper, it may nevertheless have important policy implications. Conceptual models of health suggest that education may improve health status by increasing one’s endowment of resources to improve health by raising income, by choosing a mix of consumption and activities that lead to better health, or by producing health more efficiently from a given allocation of consumption and activities (Grossman 1972) and that education may affect different health conditions differently (Link and Phelan 1995). Empirical research suggests that different pathways may be more relevant for some health conditions than others. For example, there is evidence that education affects mortality only through its effect on income (Lantz et al. 1998) and that education may directly affect the onset of disease, but not the progression of disease (Herd, Goesling, and House 2007). An implication of this literature is that income redistribution policies may have comparable effects as educational interventions for certain measures of health status in low-SES populations. Another important policy implication emerges if education only affects health by increasing income. Specifically, if policy interventions were to raise overall levels of education across a population distribution, then marginal educational increases would lead to diminishing income returns (because in spite of higher productivity, education attainment, relative to the whole population may be unchanged). In that scenario, the health benefits of policy action to increase education may be more limited.

This study aimed to improve the understanding of the overall extent to which education affects health status. Understanding the causal effects of education on health is far from just a trivial academic exercise. The extent to which education affects health has important implications for policy. From a health policy perspective, our estimates suggest that investing in education may be a relatively cost-effective approach to promoting population health. From a general policy perspective, our estimates suggest that the overall returns to education may be considerably understated if health outcomes are not included as potential returns. Although cost-effectiveness or cost-utility analyses do not justify policy action on their own, our findings can be taken as one argument for expanding public investment in education.

Figure 4.1. Years of Compulsory Education, 1914-1974



Data are from Acemoglu & Angrist (2001)

Figure 4.2A. Education and QALYs with local polynomial smoother

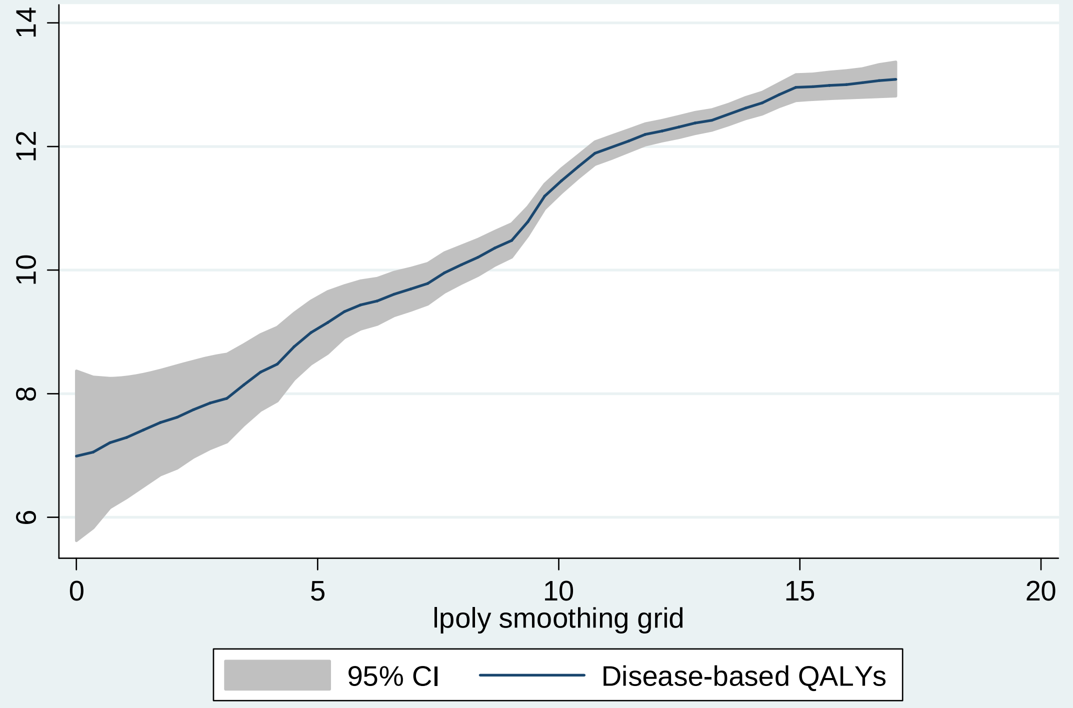


Figure 4.2B. Education and QALYs with local polynomial smoother

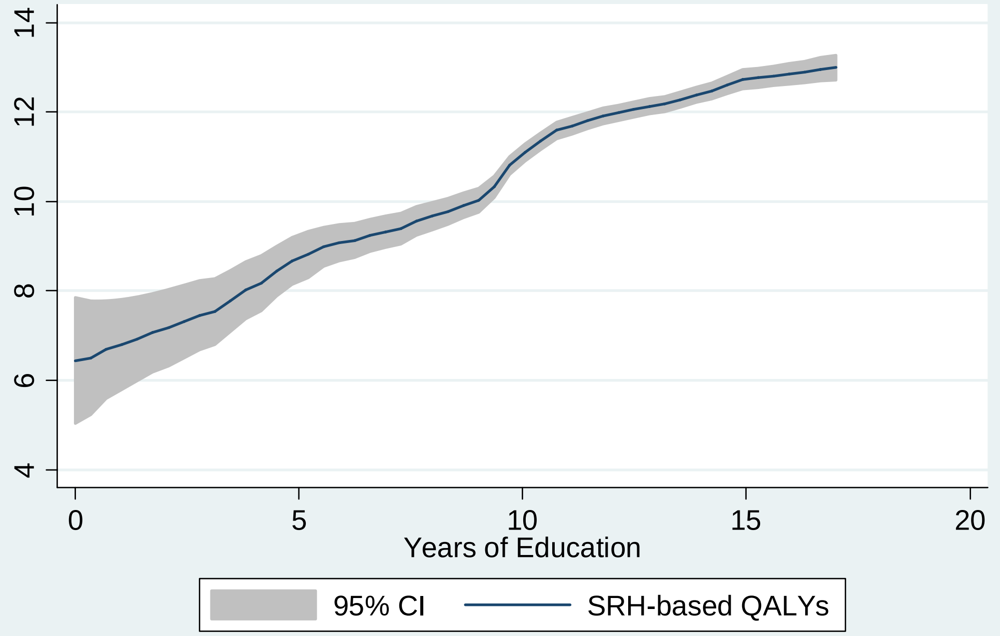


Table 4.1. Sample Descriptives

Independent Variables	**Mean**	**SD**	**Min**	**Max**
Education	11.53	3.41	0	17
Female	0.62		0	1
White	0.66		0	1
Black	0.32		0	1
Other race	0.02		0	1
Age	54.19	17.10	24	86
Dependent Variables				
SRH-QALYs	11.40	4.08	0.041	15
Disease-QALYs	11.70	3.97	0.067	15
Years of Compulsory Schooling				
7 or less	0.12		0	1
8	0.16		0	1
9	0.41		0	1
10	0.08		0	1
11 or more	0.22		0	1

Distribution of years of education

	N	%	Cumulative %
0	17	0.55	0.55
1	14	0.45	1
2	14	0.45	1.46
3	42	1.36	2.82
4	35	1.13	3.95
5	52	1.69	5.64
6	70	2.27	7.91
7	109	3.53	11.44
8	253	8.2	19.64
9	143	4.64	24.28
10	196	6.35	30.63
11	194	6.29	36.92
12	910	29.5	66.42
13	202	6.55	72.97
14	303	9.82	82.79
15	99	3.21	86
16	216	7	93
17	216	7	100

Table 4.2. OLS results

	1. Disease-based QALYs				2. SRH-based QALYs			
	Coef	SE	T	P	Coef	SE	T	P
Education	0.147	0.021	7.13	<.001	0.203	0.022	9.27	<.001
Female	0.871	0.126	6.94	<.001	0.948	0.134	7.06	<.001
Black	-0.787	0.138	-5.71	<.001	-0.838	0.144	-5.82	<.001
Other race	-1.195	0.409	-2.92	0.004	-1.201	0.454	-2.64	0.008
N	3083				3085			
R2	0.3745				0.3286			

Note: Models also include age dummies.
Standard errors are clustered on state-of-youth and year-of-birth.

Table 4.3. First-Stage Results

	Coef	se	t	P
Compschool8	1.106	0.320	3.45	0.001
Compschool9	1.099	0.265	4.15	<.001
Compschool10	1.300	0.316	4.12	<.001
Compschool11	1.740	0.320	5.44	<.001
Female	0.099	0.113	0.88	0.38
Black	-1.232	0.157	-7.86	<.001
Other race	-1.476	0.475	-3.10	0.002
N	3085			
R2	0.3134			
			Statistic	P-value
F			7.59	<.001
Partial R2			0.012	
Underidentification test (null=underidentified)			37.26	<.001

Note: Models also include age and state-of-youth dummies. Standard errors are clustered on state-of-youth and year-of-birth.

Table 4.4. IV Results								
	1. Disease-based QALYs				**2. SRH-based QALYs**			
	Coef	SE	T	P	Coef	SE	T	P
Education	0.402	0.197	2.04	0.041	0.384	0.208	1.84	0.065
Female	0.840	0.129	6.52	<.001	0.934	0.136	6.42	<.001
Black	-0.622	0.300	-2.07	0.038	-0.739	0.317	-2.33	0.020
Other race	-0.666	0.521	-1.28	0.201	-0.794	0.554	-1.43	0.152
N	3082				3084			
R2	0.3538				0.3242			
Hansen-J	p=.2161				p=.0834			

Note: Models also include age and state-of-youth dummies.
Standard errors are clustered on state-of-youth and year-of-birth.

Table 4.5. Sensitivity Analyses

	Coefficient	S.E.	P	1st-stage F	N
With region trends[+]					
Disease-based QALYs	0.370	.328	.259	3.39	3082
SRH-based QALYs	0.285	.343	.406	3.38	3084
Excluding all non-respondents[+]					
Disease-based QALYs	0.410	.226	.070	6.35	2382
SRH-based QALYs	0.384	.234	.101	6.35	2382
Including all non-respondents					
Disease-based QALYs	0.335	.177	.059	8.57	3334
SRH-based QALYs	0.265	.186	.153	8.51	3336
State-of-birth first stage					
Disease-based QALYs	0.456	.203	.025	7.11	3053
SRH-based QALYs	0.407	.209	.052	7.10	3055
Less than median age[+]					
Disease-based QALYs	0.361	.458	.430	0.78	1509
SRH-based QALYs	0.167	.492	.735	0.78	1510
Greater or equal to median age+					
Disease-based QALYs	-0.488	.526	.353	2.07	1573
SRH-based QALYs	-0.456	.547	.405	2.07	1574

Note: All models include controls for sex and race, and state-of-youth and birth cohort dummies. Standard errors are clustered on the state-of-youth and year-of-birth.
[+] Estimated with 2SLS.

Appendix 1.

The assignment of HRQL weights to each year in the ACL study is somewhat complicated, and involved creating HRQL weights for each of the ACL survey waves and then imputing HRQL weights for the years between interviews and the years between an interview and death. We created two sets of HRQL weights using the algorithms provided by Nyman et al. (2007). One algorithm defines HRQL weights based upon self-rated health, and the other defines HRQL weights based upon the self-report of 10 specific conditions. The 10 conditions included in Nyman et al.'s algorithm for computing HRQL weights are 12-month measures of diabetes, asthma, coronary heart disease, angina, myocardial infarction, other heart disease, hypertension, stroke, emphysema, and joint pain. At each wave, the ACL included questions about whether the respondent experienced the following conditions over the past 12 months: arthritis or rheumatism, lung disease, hypertension, heart attack or other heart trouble, diabetes, and stroke.

To make Nyman et al.'s effects of conditions on HRQL weights concordant with the ACL conditions, we created weighted averages of the marginal effects of heart conditions and lung conditions from Nyman et al.'s estimates. Nyman et al. include the prevalence of their 10 conditions along with the estimated effects on HRQL weights. To create an estimated effect of having a heart attack or other heart trouble on HRQL weights, we use information from the four specific heart conditions (coronary heart disease, angina, myocardial infarction, other heart disease) described in Nyman et al. Our estimate is the weighted average of Nyman et al.'s effects of these four conditions, where the weights are the proportion of heart conditions attributable to the specific condition. For example, “other heart disease” is the most common of the four specific heart conditions and thus receives the most weight. Our approach to creating an estimated effect of lung disease was similar, except we used the two specific lung diseases (asthma and emphysema) included in Nyman et al. It is important to note that our method for estimating effects on QALYs for aggregated heart and lung diseases makes the assumption that the specific diseases in each of these categories are distributed independently of each other. In other words, the method is valid if having, say, a heart attack does not predict having another specific heart condition (say, angina). Because this assumption is likely unrealistic, we note that our estimated effects of heart and lung diseases on HRQL weights are likely to be underestimates. The final effects of health conditions on HRQL weights are -.036 for diabetes, -.017 for hypertension, -.022 for heart attack or other heart trouble, -.071 for stroke, -.023 for lung disease, and -.156 for arthritis or rheumatism (which we assume is directly comparable to the MEPS “joint pain” item). In addition, the effects of self-rated health categories on HRQL weights are -.153 for good health, -.200 for fair health, and -.511 for poor health.

To determine whether how many and which years following the baseline interview each respondent had each health condition we use a two-step process incorporating two sources of information from the ACL. First, we look at the four waves of survey data. If a respondent indicated that they had a given condition in contiguous survey waves (e.g., waves 1 and 2, or waves 3 and 4), then we coded the respondent as having had that condition for each year between those two waves. If a respondent indicated that they did not have a given condition in contiguous survey waves, then we coded the respondent as not having had that condition for each year between those two waves. If a respondent reported having a given condition in only one of two contiguous waves, we assumed a uniform distribution of incidence and recovery for the condition. That means that we assume that on average, the incidence/recovery of the condition occurred halfway through the interval, and we code the intermediate years accordingly. When a respondent died between intervals, we assumed that the respondent had only the conditions that were reported in the most recent ACL wave up until time of dead.

Second, we also use data on the timing of each health condition that were collected in the fourth ACL wave. For each condition, the respondent is asked if he/she was ever told by a doctor or other health care provider that they had the specific condition. If the respondent responded affirmatively, then the respondent was asked for the year in which they were first told that they had the condition. We use these data to specify the year of incidence for the situations where a respondent reported having not a given condition in one wave, but reported having the condition in the following wave. When there was missing data for the year when first told about a condition (most frequently because the respondent reported not knowing), we maintained our original assumption of a uniform distribution of incidence between waves.

Appendix 2.

We followed the algorithm described in Acemoglu & Angrist (2000) for creating our measure of the number of years of compulsory schooling. The measure of years of compulsory schooling is computed from data on maximum age by which a child must enroll in school, the minimum years of schooling a child had to obtain before dropping out, and the minimum age for dropping out of school. If a state had no specified maximum enrollment age or minimum dropout age, then the years of compulsory schooling was equal to the minimum years of schooling a child had to obtain before dropping out (if that number was positive), or zero otherwise. If a state had a maximum enrollment age and a minimum dropout age, then the years of compulsory schooling was equal to the greater of a) the difference between the minimum dropout age and the maximum enrollment age or b) to the minimum years of schooling a child had to obtain before dropping out.

Table 4.A1. Doctors per capita and compulsory schooling

		Coefficient	se	P
Compschool8		0.0000164	1.67E-05	0.328
Compschool9		-6.57E-07	1.55E-05	0.966
Compschool10		2.45E-06	1.83E-05	0.893
Compschool11		6.62E-07	1.89E-05	0.972
N	2917			
R2	0.871			

Note: Unit of analysis is the state-year. Includes state and year fixed effects.
Data are from Lleras-Muney (2005), the AMA American Medical Directory, and the Statistical Abstracts of the United States.

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Chapter 5

Conclusion

Policymakers face difficult decisions about how to optimally allocate societal resources in order to meet a diverse set of health policy goals. In this dissertation, I have focused on three separate issues that are important to health policy, with the hope that the research will help inform difficult policy decisions. In my first paper, I find evidence that decreasing Medicare payments for home health care services causes low-income, disabled older adults to use more informal care. Opportunities to use plausibly exogenous variation in publicly-funded home care generosity are rare, and I contend that this paper represents the best evidence to date of the effect of publicly-funded home care on informal care use.

In my second paper, I find evidence suggesting that middle-aged single women respond to increased incentives to work by reducing their co-residence with older disabled parents. This paper is possibly the only study to use plausibly exogenous variation in employment incentives to test for how increased employment incentives may affect the delivery of informal support.

In my third paper, I find evidence of a causal effect of education on health that suggests that increasing education may be a relatively cost-effective way to improve population health. While the research design used in this paper is not necessarily innovative, the approach that I propose to measuring the overall health returns to education represents an important step forward in this area of research.

A major theme that emerges from this dissertation is the difficulty in devising research designs that can identify causal effects validly. Quasi-experimental research often involves a tension between finding clean sources of identification on the one hand and asking interesting research questions or producing generalizable findings on the other hand. I have addressed this tension in different ways across my three papers. The first paper probably involves the fewest trade-offs, because the research design uses a policy change that affected a large and important (from a policy perspective) population: Medicare beneficiaries with functional limitations. The second paper probably involved the most trade-offs. In order to answer the research question, I make some assumptions about the comparison groups that I argue are reasonable, but are ultimately unverifiable. Even with those assumptions, I sacrificed external validity somewhat in order to maximize internal validity, since the variation that I study affected only a small subset of the overall population. I also trade off external validity in favor of internal validity in the third paper. Similar to the second paper, I use variation in the independent variable that only applied to a small subset of the overall population, and it is unclear how findings based on that variation apply to the broader population, and in particular, how they apply to changes in levels of education above secondary school. In spite of the trade-offs that I made, I feel that I never sacrificed the importance of the research questions that I have addressed in any of the three papers.

If there is one point that this dissertation has aimed to advance, it is that health services and policy research can benefit from taking a broader perspective that looks beyond what happens only in the health care system. All three of my papers come to important conclusions for health policy that involve more than just the health care system. Health policy is obviously only one facet of public policy. The research in this dissertation suggests that the interactions between health policy and other areas of public policy are important, and may be a fruitful area for future research.