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ESSAYS IN HEALTH ECONOMICS

by

NAYAN KRISHNA JOSHI

DISSERTATION

Submitted to the Graduate School

of Wayne State University,

Detroit, Michigan

in partial fulfillment of the requirements

for the degree of

DOCTOR OF PHILOSOPHY

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Approved By:

Advisor	Date

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DEDICATION

To my father Arjun Krishna Joshi

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I would like to thank my advisor, Professor Allen C. Goodman, for teaching me how to do research and how to publish research. He gave me the freedom to do research and provided constructive feedback at various stages of my research . His insightful suggestions made it possible for me to publish the part of my second chapter of my dissertation and my term paper, that I originally wrote for his Health Economics course.

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CHAPTER 1 HOUSING WEALTH, FINANCIAL WEALTH, AND HEALTHCARE SPENDING

1.1 Introduction

This chapter examines the impacts of housing and financial wealth on healthcare spending using the matched household data constructed from the Consumer Expenditure Survey (CES) and the Survey of Consumer Finances (SCF). The human capital model of demand for health (Grossman, 1972, 2000) suggests that wealth has a positive effect on healthcare spending as higher wealth will relax the budget constraint of individuals and allows individuals to consumer more healthcare, *ceteris paribus* (Kim and Ruhm, 2012).[1] This implies that prior studies which use only income as a proxy for economic resources do not provide a complete picture of how resources affect healthcare spending (Goodman, 1989). Additionally, the exclusion of wealth raises the concern of endogeneity[2] (Feinstein, 1993). Furthermore, literature in macroeconomics and finance indicates that wealth effect might differ depending upon the forms of wealth, for example, housing wealth and financial wealth, because of difference in features such as uncertainty of shocks, liquidity, tax treatment, and mental accounting by households with respect to the forms of wealth they hold (Case, Quigley, and Shiller, 2005; Bostic, Gabriel, and Painter, 2009; Belsky, 2010). However, in what direction the housing wealth effect differs from the financial wealth effect is an empirical issue.

Data problems aside, studies focusing on effects of housing and financial wealth on healthcare spending need to take into account a large fraction of zero values in healthcare spending. In this paper, I address this issue by using dependent double-hurdle model

[1]Anecdotal evidence in the media also indicates that households reduce healthcare spending following the loss of income and wealth (Lowrey, 2013).

[2] Feinstein (1993) states "...the problem of reverse causality is less likely to afflict household wealth than household income measures, primarily because wealth accumulates over time and hence is less affected by a single episode of sickness".

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(Jones, 1989; Okunade, Suraratdecha, and Benson, 2010). The double-hurdle model permits the possibilities that healthcare spending might be zero, first, because households decide not to participate in healthcare spending and second, because households decide to participate in healthcare spending but may not be able to afford the spending at current price, income, and wealth. I begin the empirical analysis by estimating the model for pooled sample (1989-2010) using gross measure of household wealth. The results indicate significant housing and financial wealth and relatively large housing wealth effect. Next, I estimate the model separately by survey years of the SCF to examine how housing and financial wealth effects have changed over time. The data suggest that housing wealth effects are significant for all years and relatively large, but financial wealth effects are insignificant in most cases. Analysis by SCF survey years also reveals the diminished housing wealth effect but increased financial wealth effect following the Great Recession when compared to the survey year of the SCF before the Great Recession. In addition, subgroup analyses by age group and credit-constrained show that the housing wealth effect is most pronounced among older aged households and credit-constrained households, whereas the financial wealth effect is significant only for older aged households and unconstrained households. In general, the results are qualitatively similar when I use net wealth measure. However, housing and financial wealth effects using net wealth specification are smaller in magnitudes compared to those using gross wealth specification. In other words, households are less sensitive to net wealth measure than to gross wealth measure. [3]

The contribution of the paper to the health economics literature can be summarized as follows. First, it provides an empirical examination of relationship between healthcare

[3]These findings are consistent with the behavioral theory that argue that households form "mental accounts" that make them more likely to consume assets held in some ways that in others (Poterba, 2000; Thaler, 1990).

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spending and housing and financial wealth. This analysis is relevant as households may respond differently to fluctuations in housing wealth and financial wealth. In fact, the paper finds housing wealth effects to be larger than financial wealth effects. Second, it recognizes the household debt and compares wealth effect of gross measure of housing wealth or financial wealth with that of net (gross wealth minus debt) measure. Third, it uses recent data to examine the impacts of housing and financial wealth after Great Recession. Fourth, to the best of my knowledge, it is the first study to use the CES to examine the effect of wealth on healthcare spending. Foster (2010) compares the healthcare spending estimate from the CES with the Medical Expenditures Panel Survey (MEPS) and find the CES healthcare spending data to be consistent.

Section 1.2 outlines the conceptual framework. Section 1.3 describes the data and method. Section 1.4 presents the empirical results, and Section 1.5 provides robustness check and extension. Section 1.6 concludes.

1.2 Conceptual Framework

This study begins with the human capital model of demand for health (Grossman, 1972, 2000). In the static version of this model, the individuals derive utility from consumption and health, and produce health by combining time inputs with healthcare. Furthermore, individuals face a single full wealth constraint and maximize the utility subject to this constraint. Higher wealth will relax the constraint of individuals and since health is almost certainly a normal good, it will cause individuals to consume more healthcare, *ceteris paribus* (Smith, 1999; Kim and Ruhm, 2012). In addition, higher wealth individuals are more likely to have access to enhanced insurance coverage which will allow them to purchase more healthcare (Fichera and Gathergood, 2013; Goldman and Maestas, 2013; Kennickell, 2008).

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Recent studies that examine the effect of wealth on healthcare spending include Okunade, Suraratdecha, and Benson (2010), Kim and Ruhm (2012), and Lusardi, Schneider, and Tufano (2015). Okunade, Suraratdecha, and Benson (2010), using the Thailand Socio-Economic Surveys, measure wealth as household ownership of durable goods (e.g., air conditioner, motorbike, car, television etc) and find that there is a significant positive relationship between wealth and out-of-pocket (OOP) spending. Kim and Ruhm (2012) use an elderly sample from Health and Retirement Survey and document that wealth shocks associated with inheritances of greater than or equal to $10,000 have positive and significant impact on expected OOP spending, conditional on positive OOP spending. Using data on individuals between ages of 18 and 65 from the TNS Global Economic Crisis Survey (2009), Lusardi, Schneider, and Tufano (2015) find that economic shocks generated by the economic crisis resulted in relatively large reduction in the use of routine non-emergency medical care for United States households than those for Canadian, French, German, and British households. These studies, however, do not differentiate between the effects of housing and financial wealth on healthcare spending, though the literature in macroeconomics and finance suggests that they may exist.

The literature in macroeconomics and finance takes a life-cycle consumption model of consumption as a starting point (Case, Quigley, and Shiller, 2005; Paiella, 2009). The model suggests that households accumulate and spend their wealth to smooth consumption and that the marginal propensity to consume out of all wealth, irrespective of its form, should be the same small number (Case, Quigley, and Shiller, 2005; Paiella, 2009; Belsky, 2010).[4] However, marginal propensity to consume might differ, for example, be-

[4]The model does not distinguish between different forms of wealth and assumes that households do not face liquidity constraint (Belsky, 2010). Galama, Hullegie, Meijer, and Outcault (2012) indicate that consumption smoothing is also applicable to healthcare spending.

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tween housing wealth [5] and stock wealth, for at least four reasons (Case, Quigley, and Shiller, 2005; Bostic, Gabriel, and Painter, 2009; Belsky, 2010). First, households may view changes in some kinds of wealth as temporary or uncertain (Case, Quigley, and Shiller, 2005). For example, if households view change in house prices as permanent or less volatile than change in stock prices then consumption effect associated with housing wealth will be relatively large (Belsky, 2010). Second, households form mental accounts that make them more likely to consume wealth held in some ways than in others (Thaler, 1990; Poterba, 2000) .[6] Bostic, Gabriel, and Painter (2009) apply this concept to illustrate the difference in sensitivity of households to gross and net measures of housing wealth or financial wealth.[7] Third, households may find difficult to measure or liquefy certain form of wealth (Bostic, Gabriel, and Painter, 2009; Case, Quigley, and Shiller, 2005). For example, transaction costs associated with selling or borrowing against stock wealth are lower than that related with selling or borrowing against housing wealth which suggests that housing wealth effect will be smaller than stock wealth effect (Bostic, Gabriel, and Painter, 2009; Belsky, 2010). Fourth, differences in the tax treatment of housing and stock wealth may result in difference in wealth effects (Belsky, 2010). For example, if households have a bequest motive and if the housing wealth receives a preferential tax treatment over stock wealth at bequest, then households may hold housing wealth until

[5] Theoretically, it is not clear whether there should be a large housing wealth effect. For example, for the unconstrained homeowner who expects to live in his house for a long time, an increase in house prices does not make him richer because it also increases the implicit rental cost of housing, and without any substitution effect (that is change in the housing), it doesnt exert any effect on consumption (Poterba, 2000; Mian and Sufi, 2011; Campbell and Cocco, 2007; Sinai and Souleles, 2005). However, as an asset, housing can serve as collateral in a loan and thus an increase in house prices increase the homeowner's access to "cash on hand" if borrowers are willing to lend against the higher collateral value (Mian and Sufi, 2011, 2014a). The cash-on-hand effect stimulates the spending , especially for credit constrained homeowners (Mian and Sufi, 2014a).

[6] With regard to the stock wealth effect, Poterba (2000) argues that propensity to consume of household is higher for stocks that are held directly than for stocks that are held in the retirement accounts, since the later are considered as long term assets.

[7] Specifically, they argue that households are more sensitive to a dollar windfall capital gain in wealth (housing or stock) that to the dollar increase in existing wealth.

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death and thus the housing wealth effect may be lower than stock market wealth (Case, Quigley, and Shiller, 2005; Belsky, 2010).

One may also expect housing wealth effect to differ from the stock market wealth effect over time for three reasons (Poterba, 2000). First, there have been changes in the composition of wealth shocks (Poterba, 2000). For example, in the current recession, both the stock and house prices fell. However, in the 2001 recession, stock prices fell but that was offset by a rise in house prices. In the 1990-1991 recessions, home values dropped but that was offset by an increase in stock market wealth (Moore and Palumbo, 2010). The second factor is the increasing share both of households owning equities and of equities in households total assets relative to housing wealth. Moore and Palumbo (2010) show that between 1989 and 2007, the share of households owing equities increased by 19 percentage points compared to the 4 percentage points for housing. The third is the institutional change such as decreasing cost of leaving bequests (Poterba, 2000). McGarry (2013) notes that US economy has experienced significant changes in both the size of an estate, that is tax exempt, and in the top marginal rate. For instance, in the late 1980s, the top marginal estate tax rate was 55 percent and the maximum allowable tax credit was $0.6 million. This contrasts with the rate of 40 percent and $5.25 million in 2013 (McGarry, 2013; Jacobson, Raub, and Johnson, 2007)[8] . The reduction in the estate tax is more likely to make bequest attractive to high net worth households and thus they may decrease the current marginal propensity to consume out of wealth (Poterba, 2000).

Recent studies have used panel data and micro data to examine the consumption effects of housing and stock market wealth. Case, Quigley, and Shiller (2005) and Case, Quigley, and Shiller (2013) use a panel of US states and show a stronger housing wealth

[8]The tax was eliminated in 2010, returned in 2011.

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than stock wealth effect .[9] Consistent with Kahneman and Tverskys prospect theory, Case, Quigley, and Shiller (2013) also show that the housing wealth effect is higher for the falling market than that for the rising market (elasticity of 0.10 versus 0.032). Similar to Case, Quigley, and Shiller (2005) and Case, Quigley, and Shiller (2013), Calomiris, Longhofer, and Miles (2013) use a panel of US states and find smaller stock market wealth effects which they attribute to relatively high volatility of stock wealth and the relatively lower rate of participation by households in the stock market. They also document that that consumption effect of housing wealth depends on age composition, poverty rates, and the housing wealth shares.

Using a unique matched data set constructed from the SCF and the CES, Bostic, Gabriel, and Painter (2009) find that housing and financial wealth have significant positive effects on consumption, but housing wealth effects are larger than financial wealth effects. Guo and Hardin III (2014) use the Panel Study of Income Dynamics and find that consumption by households with greater share of wealth in financial wealth is not affected by housing wealth whereas consumption by households with greater share of wealth in housing wealth is not impacted by financial wealth.

Most of the existing empirical test of wealth effects on consumption, using micro data, focus on non-durable consumption (Attanasio, Hurst, and Pistaferri, 2012; Attanasio and Weber, 1995).[10] None have tried to empirically identify the impacts of housing and financial wealth on healthcare spending. Additionally, in what direction housing wealth effect differs from financial wealth effect is an empirical matter.

[9]Poterba (2000) argues that the consumption effects of stock market wealth are likely to be small for most households because of highly skewed distribution of stock ownership.
[10]The exception is Bostic, Gabriel, and Painter (2009), who report the wealth effects for both total consumption and durable goods consumption.

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1.3 Data and Method

1.3.1 Data

Ideally, one may want to use the survey data that contain detailed information on wealth and healthcare spending for non-elderly population. Unfortunately, such a database is not publicly available .[11] To overcome this data limitation, I follow Bostic, Gabriel, and Painter (2009) and use nonparametric statistical matching to combine two different data sources: the CES and the SCF . [12] Specifically, I obtain household-level healthcare spending data from the CES and then use the SCF to impute household-level wealth and its components for households in the CES. In the following subsections, I briefly describe these data sources and statistical matching process used to combine these datasets.

Consumer Expenditures Survey

The CES data come from Bureau of Labor Statistics (BLS). The data are available from the start of 1980 to the first quarter of 2012 and consist of two different components: an interview component and a diary component, each with a different data collection technique and sample.[13] This study employs the interview component of the CES. In the interview component, each household [14]is interviewed five consecutive times, once

[11]A longitudinal or a cross sectional survey that consists of detailed information on healthcare spending, health status, and wealth for non-elderly population is presently not available in the US. There exists the Health and Retirement study, but it focuses only on older population (50 years and above). The Survey of Income and Program Participation (SIPP) is a potential data source in this regard; however, its wealth data are not reliable when compared with the SCF (Czajka, Jacobson, and Cody, 2003). In addition, the SIPP data have not been subjected to the same, detailed evaluations as the medical expenditure data from the CES (Foster, 2010) . Moreover, a data gap in the SIPP overlaps with the Great Recession of 2007-2009 (Hacker et al., 2014).

[12]See also Salotti (2012).

[13]The interview component covers up to 95 percent of total expenditures. For details on two different components, please refer to the Chapter 16 of the BLS (2013).

[14]A household in the CES is called a consumer unit which is defined as 1) all members of a particular housing unit who are related by blood, marriage, adoption, or some other legal arrangement, such as foster children; 2) a person living alone or sharing a household with others, or living as a roomer in a private home, lodging house, or in permanent living quarters in a hotel or motel, but who is financially independent; or 3) two or more unrelated persons living together who pool their income to make joint expenditures decisions. Students living in university-sponsored housing are also included in the sample

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per quarter. The first interview is a contact interview, whereas the second through fifth interviews collect the spending made by households for three months prior to the month of interview (Malloy, Moskowitz, and Vissing-Jorgensen, 2009). Households report not only the healthcare spending, but also the spending on housing, transportation, and education. The interviews also gather the information on demographic variables and health insurance status as of the time of interview. In addition, households report the information about the employment and income for the previous twelve months in the second and fifth interviews, and information on stock of assets for the previous month and changes in that stock from the month one year ago, in the fifth interview. The healthcare spending definition and sample selection criteria are described in Appendix A.1. The healthcare spending and income variables are converted into 2010 dollar terms using all items of CPI-U of the BLS. Foster (2010), using the data of 1996-2006, compares the healthcare spending estimate from the CES with the MEPS and find the CES healthcare spending data to be consistent. Specifically, he finds CES-MEPS ratio for healthcare spending to be 0.93.

Survey of Consumer Finances

The study uses the SCF to impute the wealth data into the CES as the coverage of wealth data in the CES is not reliable (Bostic, Gabriel, and Painter, 2009; Kennickell and Woodburn, 1999). Kennickell and Woodburn (1999) note that the SCF has relatively good coverage of wealth data compared to other surveys conducted in US.[15] The SCF is conducted every three years by Federal Reserve Board and is available from the start of 1983 to 2010. [16] The SCF employs a dual sampling technique. First, it uses a multistage

as separate consumer units (BLS, 2013).

[15]See also Czajka, Jacobson, and Cody (2003) for difference in wealth estimates using the Panel Study of Income Dynamics (PSID), the Survey of Income and Program Participation (SIPP), and the SCF.

[16]A household in a SCF is consists of primary economic unit (PEU)— the family—and everyone else in the household. The PEU is an economically dominant single person or couple (whether married or

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area-probability sample to provide good coverage of broadly distributed variables such as homeownership and credit card debt (Kennickell, 2008; Kennickell and Woodburn, 1999). Second, it uses a special sample design (called as a supplemental sample or a list sample) to include relatively wealthy families disproportionately so as to provide good coverage of narrowly held variables such as corporate stock (Kennickell and Woodburn, 1999). The supplemental sample is drawn from a tax list of Internal Revenue Service (Ackerman, Fries, and Windle, 2012). The SCF handles missing data problem by employing multiple imputation method. Under this method, which began with the 1989 survey, the SCF provides five different estimates (called implicates) for each missing data point , resulting in a total data set with five times the actual number of households. Imputed values differ across implicates to represent the sampling uncertainty inherent in the imputation. To account for multiple imputations in the SCF data, the estimation results follow repeated imputation inference (RII) method of Rubin (1987).

The wealth concept used here includes the gross financial wealth, gross housing wealth, and gross other real estate wealth (Bostic, Gabriel, and Painter, 2009). Gross financial wealth is defined as sum of: i) liquid assets (checking accounts other than money market + savings account+ money market accounts+ call accounts at brokerages) ii) certificates of deposit iii) mutual funds iv) stocks and bonds (not including bond funds or savings bonds) v) quasi-liquid retirement accounts (individual retirement accounts, thrift-type accounts, and future pensions) vi) savings bonds vii) cash value of whole life insurance, and viii) other managed assets (trusts, annuities and managed investment accounts in which household has equity interest). Gross housing wealth is the gross value of primary residence and gross other real estate wealth is the gross value of all residential real estate

living together as partners) and everyone else in the household is financially interdependent with PEU (Ackerman, Fries, and Windle, 2012).

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other than the primary residence.

The study also incorporates the information on the liabilities in the SCF and compute net financial wealth, home equity, and other residential real estate equity. Net financial wealth is the gross financial wealth minus the financial debt[17] , home equity is the gross housing wealth minus mortgages and home equity loans, and other real estate equity is the gross other real estate wealth minus the mortgages and equity loans. The SCF also provides the information on demographic characteristics and employment history which can be used for matching with the CES. The wealth and income variables are expressed in 2010 dollar terms using all items of CPI-U of the BLS. Although both the CES and the SCF are available since 1983, I use the data only from 1989 because of changes in the SCF question frame prior to that year (Bostic, Gabriel, and Painter, 2009).

Statistical Matching

For each SCF survey year, I use a nonparametric statistical matching algorithm to obtain wealth components for CES households.[18] The matching process starts with the harmonization of the two surveys. Harmonization consists of two major phases: (i) harmonization of population and unit denitions, and (ii) harmonization of denitions of variables (D'Orazio, Di Zio, and Scanu, 2006). Since the CES and the SCF are both representative samples of US population, the first phase may not be a problem. The second phase involves creating new variables using the available information and recoding some variables. After the harmonization, both CES and SCF samples are partitioned into cells based on seven categorical variables: race, marital status, education, occupation, family size, tenure, and health insurance, that are known to be highly-correlated with

[17]Total debt excluding mortgage debt, other lines of credit and residential debt (i.e. financial debt= credit card debt + installment debt +other debt).

[18]Recall that SCF survey years are 1989, 1992, 1995, 1998, 2001, 2004, 2007, and 2010.

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variation in the healthcare spending:

(a) Race: white, black, other

(b) Marital status: married/living with partner, other

(c) Education: twelfth grade or less, high school, some college or more

(d) Occupation: unemployed, managers and professional, administrative (administrative support, technical, sales), service, operators (operator, assembler, laborer), other (precision, production, craft, repairing, farming, forestry, and fishing)

(e) Family size: one, two, three, four or more

(f) Tenure: homeowner, other

(g) Health insurance: insured (private/public), not insured

Partitioning ensures that matches are only allowed for households that agree exactly on these variables (exact matching). Within each cell, each household in the CES is matched with the closest SCF household according to a Mahalanobis distance measure[19] computed using common variables age and income (D'Orazio, Di Zio, and Scanu, 2006).[20]

$$\mathbf{d}_{ab} = (\mathbf{x}_a - \mathbf{x}_b)' \sum_{\mathbf{xx}}^{-1} (\mathbf{x}_a - \mathbf{x}_b)$$

where, $\mathbf{x}$ include the age and income, and $\sum$ is the estimated covariance matrix of $\mathbf{x}$. The distance takes into account the relationship among the $\mathbf{x}$ variables .[21] When two or more SCF households are equally distant from a CES household, I choose one of them at random. I then use wealth components of the SCF household as imputations for wealth components of the CES household. I also refine the matching by dropping the CES household whose Mahalanobis distance is greater than 95 percentile. This ensures

[19]The Mahalanobis distance is suitable when there are relatively few covariates and when the covariates are normally distributed (Stuart, 2010). These criteria are fulfilled in this study, since I use only age and logarithm of income, which are more likely to be normally distributed.

[20]Income, family size, and insurance status are defined at household level. All other variables refer to the household head.

[21]In this study, the SCF is considered as the donor and the CES is considered as the recipient.

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that the matched households do not differ significantly across age and income. The match process yielded 4997 observations in 2010.

The validity of statistical matching is examined using two different ways. First I examine the correlation between the two measures of income (income in the CES and income in the SCF)[22] and between the income measures and healthcare spending (appears only in CES) and the wealth variable (appears only in the SCF) for the matched data corresponding to SCF survey years. The correlation in the Table 1.1 suggests that two measures of income are highly correlated across SCF survey years. In addition, the correlation between CES healthcare spending and SCF log income is similar in magnitude to that of the correlation between CES healthcare spending and CES log income for all SCF survey years. This relationship is also observed for SCF wealth variables for most of the survey years, where the CES log income correlations are similar in magnitude to those of the SCF. Second, I examine the probability density functions of net worth for the matched and the original SCF datasets (Kum and Masterson, 2010). Figure 1.1 shows that the probability density functions appear to be identical for the matched and the original SCF datasets.[23]

1.3.2 Empirical Model

Healthcare spending studies using the micro data need to take account of two issues: a) a large proportion of zero observations for healthcare spending and b) right-skewness for positive observations of healthcare spending. Two-part, sample selection, and hurdle models have been used to deal with zero observations, whereas logarithmic, Box-Cox, and inverse hyperbolic sine transformations of healthcare spending have been used to address the skewness (and non-normality). Following an earlier study on healthcare spending

[22] The matched dataset consists of both measures of income for each observation.

[23] Similar correlations and graphs are obtained for other years.

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(Okunade, Suraratdecha, and Benson, 2010), I focus on the use of dependent double-hurdle model.[24]

The double-hurdle model, originally proposed by Cragg (1971), assumes that households must overcome two hurdles before being observed with a positive level of healthcare spending: first, they have to decide whether or not to participate in healthcare spending (participation decision) and second, they must decide the level of healthcare spending (consumption decision).

The rationale for separating the decision processes is (Garcia and Labeaga, 1996): first, if households decide not to participate in healthcare spending due to noneconomic reasons (a behavioral zero), economic variables like price, income, and wealth will be irrelevant for these households and second, if households decide to participate in healthcare spending, then they may choose not to spend for certain levels of economic variables (corner solution).[25]

Jones (1989) extends the Cragg's double-hurdle model by allowing the dependence in the shocks to participation and consumption decisions. Following Jones (1989), the double-hurdle model can be written as:

$$y_i = \begin{cases} y_{2i}^* & \text{if } y_{1i}^* \succ 0 \text{ and } y_{2i}^* \succ 0 \\ 0 & \text{otherwise} \end{cases} \tag{1.1}$$

where, y_i is the observed healthcare spending and y_{1i}^* and y_{2i}^* are the latent variables corresponding to the participation decision and consumption decision, respectively, and can be expressed as linear functions of explanatory variables, $\mathbf{x}_{1i}$ and $\mathbf{x}_{2i}$, respectively.

[24] The model has also been used in demand analysis for tobacco (e.g. (Jones, 1989)), migrant remittance (Bettin, Lucchetti, and Zazzaro, 2012), and labor market studies (e.g., (Blundell and Meghir, 1987).

[25] That is the corner solution of zero consumption is the utility-maximizing choice for these households, given current prices, income, and wealth (Garcia and Labeaga, 1996; Madden, 2008).Thus, the double-hurdle model permits the use of different set of determinants in each of the decision stages (Garcia and Labeaga, 1996).

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$$y^*_{1i} = \mathbf{x}^*_{1i}\beta_1 + u_{1i}$$
$$y^*_{2i} = \mathbf{x}^*_{2i}\beta_2 + u_{2i} \tag{1.2}$$

The error terms u_{1i} and u_{2i} are distributed with a bivariate normal distribution (BVN):

$$[u_{1i}, u_{2i}] \sim \mathrm{BVN}\,(0, \Sigma) \quad \text{and} \quad \Sigma = \begin{pmatrix} 1 & \sigma_{12} \\ \sigma_{12} & \sigma^2 \end{pmatrix}$$

If the sample is divided into those with zero healthcare spending and those with positive healthcare spending, the likelihood for the double-hurdle model is given by (Jones, 1989, 1992):

$$\begin{aligned} L &= \prod_{i=1}^{n} \left[1 - P\left(y^*_{1i} \succ 0, y^*_{2i} \succ 0\right)\right]^{1-I_i} \prod_{i=1}^{n} \left[P\left(y^*_{1i} \succ 0, y^*_{2i} \succ 0\right) f\left(y_i | y^*_{1i} \succ 0, y^*_{2i} \succ 0\right)\right]^{I_i} \\ &= \prod_{i=1}^{n} \left[1 - \Psi\left(\mathbf{x}'_{1i}\beta_1, \tfrac{\mathbf{x}'_{2i}\beta_2}{\sigma}, \rho\right)\right]^{1-I_i} \prod_{i=1}^{n} \left[\frac{1}{\sigma}\Phi\left(\frac{\mathbf{x}'_{1i}\beta_1 + \frac{\rho \times (y^T_i - \mathbf{x}'_{2i}\beta_2)}{\sigma}}{(1-\rho^2)^{1/2}}\right)\phi\left(\frac{y^T_i - \mathbf{x}'_{2i}\beta_2}{\sigma}\right)\right]^{I_i} \end{aligned} \tag{1.3}$$

where, $f(y_i|.,.)$ is the conditional density function, I_i is a dichotomous variable that takes a value of 1 if $y_i \succ 0$, and is 0 otherwise,ρdenotes the correlation coefficient and is given by $\rho = \frac{\sigma_{12}}{\sigma}$, $\phi(.)$ is the univariate standard normal probability density function, $\Phi(.)$ is the univariate standard normal cumulative distribution functions, and $\Psi(.)$ is the bivariate standard normal cumulative distribution function. Jones (1989) presents following three special cases with regard to the dependent double-hurdle model.

(a) **Independence**:

If u_{1i} and u_{2i} are independent, i.e., $\rho = 0$, then the Jones's double-hurdle model reduces to Cragg's double-hurdle model. The likelihood function in this case is:

$$L = \prod_{i=1}^{n} \left[1 - \Phi(\mathbf{x}'_{1i}\beta_1)\Phi\left(\frac{\mathbf{x}'_{2i}\beta_2}{\sigma}\right)\right]^{1-I_i} \prod_{i=1}^{n} \left[\frac{1}{\sigma}\Phi(\mathbf{x}'_{1i}\beta_1)\phi\left(\frac{y^T_i - \mathbf{x}'_{2i}\beta_2}{\sigma}\right)\right]^{I_i} \tag{1.4}$$

(b) **First hurdle dominance**: If participation decision dominates the consumption

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decisions, then the Jones's double-hurdle model reduces to Heckman's generalized Tobit or sample selection model (henceforth referred to as the Heckman's model). Dominance implies that the observed zero healthcare spending doesn't arise from a standard corner solution and once the first hurdle is passed Tobit censoring is no longer relevant (Jones, 1989) .[26] First hurdle dominance implies: $P\left(y_{1i}^{*} \succ 0, y_{2i}^{*} \succ 0\right) = P\left(y_{1i}^{*} \succ 0\right)$ and $f\left(y_i | y_{1i}^{*} \succ 0, y_{2i}^{*} \succ 0\right) = f\left(y_i | y_{1i}^{*} \succ 0\right)$. The likelihood function in this case is:

$$L = \prod_{i=1}^{n} [1 - \Phi(\mathbf{x}'_{1i}\beta_1)]^{1-I_i} \prod_{i=1}^{n} \left[\frac{1}{\sigma}\Phi\left(\frac{\mathbf{x}'_{1i}\beta_1 + \frac{\rho \times (y_i^T - \mathbf{x}'_{2i}\beta_2)}{\sigma}}{(1-\rho^2)^{1/2}} \right) \phi\left(\frac{y_i^T - \mathbf{x}'_{2i}\beta_2}{\sigma} \right) \right]^{I_i} \tag{1.5}$$

(c) **Complete dominance**: If independence is also assumed ($\rho = 0$) along with the first hurdle dominance, then Jones's double-hurdle model reduces to two-part model.[27] The likelihood function in this case is:

$$L = \prod_{i=1}^{n} [1 - \Phi(\mathbf{x}'_{1i}\beta_1)]^{1-I_i} \prod_{i=1}^{n} \left[\frac{1}{\sigma}\Phi(\mathbf{x}'_{1i}\beta_1)\phi\left(\frac{y_i^T - \mathbf{x}'_{2i}\beta_2}{\sigma} \right) \right]^{I_i} \tag{1.6}$$

To choose the best - fitting model, likelihood ratio (LR) test is used when the models are nested and Vuong (1989) and Clarke (2003) tests are used when the models are non-tested. Vuong's test is: [28]

$$V = \frac{n^{1/2}\bar{m}}{\hat{\omega}_m} \sim N(0,1) \tag{1.7}$$

where,

$$m_i = \ln L_{i,1} - \ln L_{i,2}{}^{29} \quad , \quad \bar{m} = \frac{1}{n}\sum_{i=1}^{n} m_i \quad \text{and} \quad \hat{\omega}_m^2 = \frac{1}{n}\sum_{i=1}^{n} m_i^2 - \bar{m}^2$$

If V is greater than the critical value, e.g., 1.96, the first model is preferred; if V is less than -1.96, the second model is preferred; and otherwise neither model is preferred (Yen, 2005). The Clarke test is a non-parametric test and applies the paired sign test to the differences in the individual log-likelihoods from two nonnested models.

[26] In other words, the observed zero consumption is only determined by the participation decision and not the consumption decision.

[27] The two-part model doesn't have latent variable representation. In addition, there is no assumption about the unconditional mean, only about the conditional /selected sample (Jones, 2000).

[28] See also Greene (2012) for Vuong test.

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To provide the economic interpretation, I compute the average partial effects (APE) of probability, conditional level, and unconditional level with respect to binary variables and average elasticities of probability, conditional level, and unconditional level with respect to continuous variables (Long, 1997). For statistical inference, I calculate the standard error for estimated elasticities and estimated APE using nonparametric bootstrapping.

1.4 Empirical Analysis

In this section, I report the results for the double-hurdle model. Before that, I present the description of the variables in Table 1.2 and descriptive statistics of these variables in Table 1.3.

Table 1.3 provides the summary statistics (mean and standard deviation) separately for the total sample and for sub-samples with non-zero healthcare spending and zero healthcare spending. The sample is restricted to household heads under 65 years of age to avoid heterogeneous spending resulting from retirement, social security income eligibility, and Medicare eligibility for those 65 years old and over (Galama et al., 2012).[30] Since there are five different matched datasets corresponding to five imputed SCF datasets, there will be five sets of summary statistics which are then combined according to the Rubin's rule (Rubin, 1987).

Households with positive healthcare spending and zero healthcare spending differ in terms of age, marital status, education, race, household size, occupation, health insurance status, income, and wealth components. Irrespective of the gross or net wealth measures, households with positive healthcare spending have higher income and higher financial wealth , housing wealth, and other real estate wealth. In addition, they are more likely

[30] I do not drop renters; I assign housing wealth of $1 for all renters. This is necessary because excluding renters would result in the drop of about one-fourth of total households with positive financial wealth which may bias the estimated financial wealth effect.

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to be older, white, married, and insured, and have higher education and larger family size (Table 1.3).

On the basis of variables defined in Table 1.2, the explanatory variables in the first stage and second stage equations of the double-hurdle model can be written as [31]:

$$\begin{aligned}
\mathbf{x}'_{1i}\boldsymbol{\beta_1} =& \beta_{1,0} + \beta_{1,1}\text{Age 36-49} + \beta_{1,2}\text{Age 50-64} + \beta_{1,3}\text{Married} + \beta_{1,4}\text{High school}+ \\
& \beta_{1,5}\text{Some school} + \beta_{1,6}\text{African American} + \beta_{1,7}\text{Other race}+ \\
& \beta_{1,8}\text{Household size 2} + \beta_{1,9}\text{Household size 3} + \beta_{1,10}\text{Household size 4}+ \\
& \beta_{1,11}\text{Managers and professional} + \beta_{1,12}\text{Administrative} + \beta_{1,13}\text{Service}+ \\
& \beta_{1,14}\text{Operators} + \beta_{1,15}\text{Other} + \beta_{1,16}\text{Insured} + \beta_{1,16}\text{Insured} \\
\mathbf{x}'_{2i}\boldsymbol{\beta_2} =& \beta_{2,0} + \beta_{2,1}\text{Age 36-49} + \beta_{2,2}\text{Age 50-64} + \beta_{2,3}\text{Married} + \beta_{2,4}\text{High school}+ \\
& \beta_{2,5}\text{Some school} + \beta_{2,6}\text{African American} + \beta_{2,7}\text{Other race}+ \\
& \beta_{2,8}\text{Household size 2} + \beta_{2,9}\text{Household size 3} + \beta_{2,10}\text{Household size 4}+ \\
& \beta_{2,11}\text{Managers and professional} + \beta_{2,12}\text{Administrative} + \beta_{2,13}\text{Service}+ \\
& \beta_{2,14}\text{Operators} + \beta_{2,15}\text{Other} + \beta_{2,16}\text{Insured} + \beta_{2,17}\text{Income}+ \\
& \beta_{2,18}\text{Financial wealth} + \beta_{2,19}\text{Housing wealth} + \beta_{2,20}\text{Other real estate wealth}
\end{aligned}$$

The important issue in the double-hurdle model is the selection of explanatory variables in the first stage and second stage equations. However, theory provides no guidance in this regard (Newman, Henchion, and Matthews, 2003). In most empirical studies, the first-stage (participation) equation of the double-hurdle model includes the demographic variables and other non-economic variables (Aristei, Perali, and Pieroni, 2008; Okunade, Suraratdecha, and Benson, 2010; Yen, 2005). The exclusion of the economic variables in

[31] Both the first and second stage equations also include year dummies, but these are not shown in the equations above.

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the first stage equation is motivated by the discrete random preference theory which suggests that sample selection is determined exclusively by non-economic factors (Aristei, Perali, and Pieroni, 2008; Yen, 2005). The second stage equation augments the variables in first stage equation with the economic variables (income and wealth variables).[32] Throughout the analysis economic (income and wealth) variables are logged. For, households reporting zero for wealth components, log-values are recoded to zero. To be specific, the components of gross wealth are transformed using ln(gross wealth) + 1, whereas the components of net wealth are transformed using sign(net wealth) × ln(|net wealth| + 1) (S. Brown and Taylor, 2008; Bricker and Bucks, 2013). [33]

The dependent variable in the second stage equation is transformed using natural logarithm to take into account positive skewness and thick tails (Manning, 1998; Aristei, Perali, and Pieroni, 2008; Zhang, Huang, Lin, and Epperson, 2008; Okunade, Suraratdecha, and Benson, 2010).[34] Histogram and kernel density plots for levels of healthcare spending and natural logarithm of positive healthcare spending are shown in Figures 1.2a and 1.2b, respectively. Figure 1.2b shows that the distribution of the natural logarithm of positive healthcare spending is more likely to be normally distributed. [35]

[32]The current income, wealth, and demographic and human capital variables in the second stage proxy household permanent income (Bostic, Gabriel, and Painter, 2009). This specification permits the examination of independent effects of housing and financial wealth on healthcare spending. Alternatively, one can compute household permanent income by regressing current income on wealth and demographic and human capital variables as in Goodman and Kawai (1982) and Goodman (1989). However, this specification is not useful here.

[33] The derivative of transformed net wealth (netwealth*) with respect to the untransformed net wealth(netwealth) is given by,

$$\frac{\partial \text{netwealth}^*}{\partial \text{netwealth}} = \frac{1}{|\text{netwealth}| + 1}$$

[34]There is only one observation with healthcare spending less than one dollar in which case logarithmic transformation generates negative value. I set this to zero. Dropping the observation, however, didn't change the result.

[35]These graphs are based only on the first imputation. Similar graphs are obtained for other four imputations.

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1.4.1 Specification Test

In this analysis, I use the LR test and Vuong test and Clarke nonparametric test to test the dependent double-hurdle model against its alternative nested and non-nested models. Table 1.4 presents the results of the tests. The first row of Table 1.4 shows that LR tests rejects the hypothesis of independence of the error terms at 0.01 level of significance. The second and third rows report the results from Vuong test and Clarke test. Both tests reject the null hypothesis of "no difference" between dependent double-hurdle model and Heckman model in favor of the dependent double-hurdle model at 0.01 significance level. In addition, the result from LR test suggests that Heckman model is preferred to two-part model. Overall, results indicate that the dependent double-hurdle model is preferable. [36]

1.4.2 Analysis for Pooled Data

Table 1.5 summarizes the result of the maximum likelihood estimation of the dependent double-hurdle model using gross wealth specification for pooled sample (1989-2010)[37][38]. To account for multiple imputations in the data, the regression results use repeated imputation inference (RII) method of Rubin (1987). The results shows that correlation coefficient between residuals from the participation equation and consumption equation (rho) is -0.905 and statistically significant at 0.01 level. This suggests the dependence of error and justifies that dependent double-hurdle model is preferred to the

[36]Clarke's statistic is reported as number of positive values minus its expectation. The results reported in Table 1.4 are based on the data using imputation 1. The results are similar when using other imputed data .

[37] Regarding the selection mechanism, the specification test favors dependent double-hurdle model over independent double-hurdle mode, Heckman model, and two-part model. However, I also estimated the Heckman model and find similar results (available upon request) .

[38]Following Solon, Haider, and Wooldridge (2013), the results are not weighted because the matched data is not representative of US population. This is because annual household spending from CES is based on the households who completed all five interviews and CES doesn't provide sample weights to adjust for the bias resulting from the excluding the households who did not complete all interviews.

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independent double-hurdle model.[39]

Table 1.5 shows that the non-economic explanatory variables have different effects on the participation and consumption equations. For example, occupation dummies (Managers and professional, Administrative, Service, Operators, Other) are significant and positive in the participation equation, but significant and negative in the consumption equation. With reference to the other non-economic variables, dummy variables related to age, marital status, education, race, household size, and health insurance have expected signs and mostly significant in both participation and consumption equations. For example, coefficients on education dummies suggest that higher education increases both the probability and level of household healthcare spending. The pooled result also shows that most of the estimates of year specific fixed effects are significant in the participation equation, whereas all are significant in the consumption equation. This suggests that stages of the economic cycle have different effects on the participation and consumption equations. Moreover, the consumption equation of the double-hurdle model shows that healthcare spending rise with the increase in income, housing wealth, and financial wealth. This confirms the importance of total household resources or permanent income in the model of household healthcare spending.

To provide the economic interpretation of the estimated parameters, I calculate three types of marginal effects for binary variables and three types of elasticities for continuous variables. The standard errors in each case are computed using non parametric bootstrapping. Since there are five different regression results corresponding to five different datasets, there will be five sets of average marginal effects and average elasticities which are then combined according to the Rubin's rule (Rubin, 1987).

[39] See also results from Section 1.4.1.

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Table 1.6 reports the average elasticities for the income and wealth variables. The results show that unconditional elasticities with respect to the income, financial wealth, and housing wealth are positive and significant.[40] In particular, the income elasticity indicates that healthcare is a necessity good - the elasticity is well below unity. Table 1.6 also suggests that estimated elasticity of healthcare spending is larger for housing wealth than for financial wealth. A 1 percent increase in housing wealth leads spending to increase by 0.022 percent, whereas the same amount of increase in financial wealth causes the spending to increase by about 0.010 percent. Table 1.6 also shows that other real estate wealth doesn't have any significant effect on the healthcare spending.

Table 1.7 presents the APE for the binary variables. The APE for probability indicates the absolute change in participation probability, while conditional and unconditional APE indicate relative change in healthcare spending since the dependent variable in the second stage is the natural logarithm of healthcare spending. The results in Table 1.7 suggest that middle aged households (age 36-49) are about 3.09 percent (on average) more likely to have positive healthcare spending than other households, *ceteris paribus*. Middle aged households spend 29.78 percent [41] more on healthcare conditional on participation, and overall, spend 7.05 percent more than others. The corresponding figures are 5.60 percent, 78.03 percent, and 14.07 percent for older households (age 50-64).

Table 1.7 also indicates that married households, educated households, insured households, and households with large family size are individually associated with higher probability of positive healthcare spending and higher percentage of spending, conditional and unconditional on positive spending. Among households of different ethnicities, African-Americans are about 5.06 percent less likely to have positive spending than others, and

[40] Since these variables do not enter in participation equation, the probability elasticities are close to zero and so conditional and unconditional elasticities are nearly the same for these variables.

[41] This is given by $(\exp(0.26063) - 1) * 100$

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conditional on positive spending, consume about 22.65 percent less healthcare than others. The unconditional level suggests that African - Americans consume about 9.43 percent less than others. As to the occupation variable, households whose head is manager and professional are 4.20 percent more likely to consume healthcare than other but, conditional on consumption, consume about 0.61 percent less than others (not significant). Overall, the average effect of the unconditional level indicates that households whose head is manager and professional consume 4.63 percent more than others. Table 1.7 also shows that other occupation dummies are significant for probability and conditional, although mostly insignificant for unconditional level.

1.4.3 Analysis by SCF Survey Years

In order to examine how the income and wealth elasticities have changed over time, separate double-hurdle models are estimated for each survey years of the SCF. Table 1.8 presents the corresponding elasticities using gross wealth specification. [42] Several interesting results emerge. First, the income elasticity is statistically significant for all survey years and ranges from 0.094 to 0.275.[43] Second, housing wealth elasticity is relatively large than financial wealth elasticity for all years. Third, financial wealth is statistically significant only for 1998 and 2010 and appears to be trending up after 2004. Fourth, housing wealth elasticity is statistically significant for all survey years and trend up from 0.0174 in 1998 to 0.0309 in 2007 and then trend down in 2010. Analysis by SCF survey years also reveals the diminished housing wealth effect but increased financial wealth effect following the Great Recession, when compared to the survey year of SCF before the Great Recession. However, other real estate wealth elasticity is insignificant for all survey years.

[42]APE for binary variables are not shown here, but are available upon request.
[43]Note that SCF is a triennial survey. So, wealth data are available only after 3 years.

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1.5 Robustness Check and Extension

In this section, I explain the results for the subgroup analysis by credit-constrained and age and robustness check using net wealth specification.

1.5.1 Extension: Subgroup Analysis by Credit-Constrained

In order to assess how the income and wealth elasticities differ between credit constrained and unconstrained households, separate double-hurdle models are estimated. For this purpose, I use questions on SCF relating to whether households have been denied credit or discouraged from applying. In particular, the SCF asks the following questions about households' access to credit:

(i) In the past five years, has a particular lender or creditor turned down any request you (or your husband/wife/partner) made for credit, or not given you as much credit as you applied for?

(ii) Were you later able to obtain the full amount you or your (husband/wife/partner) requested by reapplying to the same institution or by applying elsewhere?

(iii) Was there any time in the past five years that you or your (husband/wife/partner) thought of applying for credit at a particular place, but changed your mind because you thought you might be turned down?

Following Dogra and Gorbachev (2013) and Jappelli, Pischke, and Souleles (1998), the households are defined as credit-constrained if they answer yes to question 1 and no to question 2, or if they answer yes to question 3. That is households are defined as credit-constrained if they were turned down for credit or received less that they applied for, or were discouraged from applying for credit because they believed that they would be turned down. Credit-constrained households are hypothesized to be more sensitive to income and wealth than unconstrained households (Jappelli, Pischke, and Souleles, 1998;

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Bostic, Gabriel, and Painter, 2009).

Table 1.9 reports the income and wealth elasticities for credit-constrained and unconstrained households using gross wealth specification. Consistent with the expectation, housing wealth and income elasticities are higher for credit-constrained households. However, financial wealth elasticity is significant for only unconstrained households.

1.5.2 Extension: Subgroup Analysis by Age

In this sub-section, I examine how the income and wealth elasticities differ across age groups. In particular, I divide the households into three different age groups: aged 18-35, aged 36-49, and aged 50-64. Separate estimates for three different age groups are shown in Table 1.10 for gross wealth specification . Results in Table 1.10 show that income effect is stronger for households aged 18-34 compared to older age groups (aged 36-49 and aged 50-64)[44] , whereas housing wealth effect is stronger for households aged 50-64 compared to younger age groups (aged 18-35 and aged 36-49). Moreover, financial wealth effect is significant only for households aged 50-64. The fact that households aged 50-64 have large housing wealth elasticities makes sense. These households are in near retirement stage and are likely to use wealth for life-cycle reasons (Gourinchas and Parker, 2002; Lehnert, 2004). Moreover, the higher housing wealth elasticity of households aged 18-34 compared to households aged 35-49 is also consistent with the economic theory: younger households are more likely to be credit-constrained and thus use wealth purely as a buffer stock (Gourinchas and Parker, 2002; Lehnert, 2004).

1.5.3 Robustness Check: Net Wealth instead of Gross Wealth

In this sub-section, I estimate the sensitivity of results in Sections 1.4.2, 1.4.3, 1.5.1, and 1.5.2 by re-estimating the models using net wealth measures. For each forms of

[44] One possible explanation for higher income elasticity of the younger age groups is that they are more likely to be credit-constrained.

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wealth, the net wealth is computed as the gross wealth minus debt. For example, home equity is obtained as gross housing wealth minus mortgages and home equity loan. Appendix Tables A.2.2 and A.2.3 present the average elasticities for economic variables and APE for binary variables based on estimated parameters from the double-hurdle model in Appendix Table A.2.1 using pooled sample. In general, the results appear to be qualitatively similar. However, the estimated financial wealth and housing wealth elasticities using net wealth specification are smaller in magnitudes compared to those using gross wealth specification. In other words, households are less sensitive to net wealth measures than to gross wealth measures. For example, a 1 percent increase in home equity results in the spending to increase by about 0.015 percent compared to 0.022 percent for gross housing wealth (Table 1.6 and Appendix Table A.2.2). The lower elasticities for net wealth measure relative to gross wealth measure suggest the possibility of measurement error in assessing net wealth positions by households (Bostic, Gabriel, and Painter, 2009). In general, these results also hold for each SCF survey years (Appendix Table A.2.4), subgroup analysis by credit-constrained (Appendix Table A.2.5) , and subgroup analysis by age (Appendix Table A.2.6).

1.6 Conclusion

This study uses a matched data set constructed from the SCF and the CES to analyze the impacts of housing and financial wealth on healthcare spending for the period 1989-2010. Using the dependent double-hurdle model for the pooled sample, I find significant housing and financial wealth effects. In addition, I document that the housing wealth effect is larger than the financial wealth effect. These findings generally hold across all survey years of the Survey of Consumer Finances. In particular, the estimates of housing wealth elasticities range from 0.017 to 0.031 over the period of 1989-2010 and are

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highly significant throughout. In contrast, the estimates of financial wealth elasticities are insignificant in most cases and range from -0.001 to 0.017. Analysis by SCF survey years also reveals the diminished housing wealth effect but increased financial wealth effect following the Great Recession, when compared to the survey year of the SCF before the Great Recession. Furthermore, subsample analyses by age group and credit-constrained show that the housing wealth effect is most pronounced among older aged households and credit-constrained households, whereas the financial wealth effect is significant only for older aged households and unconstrained households.

In general, the results are qualitatively similar when I use net wealth measure. However, the housing and financial wealth effects based on net wealth measure are smaller compared to those using gross wealth measure. Overall, the study sheds light on differences in wealth effects from different types of wealth. The findings in the study are also consistent with the previous literature that argues that model of household healthcare spending should incorporate both current income and wealth (e.g., (Okunade, Suraratdecha, and Benson, 2010)).

The estimated effects cannot be interpreted as causal. An endogeneity problem is likely because there may be unobservable variables that are correlated with both healthcare spending and housing and financial wealth (for example, rates of time preference) or there may be reverse causality whereby healthcare spending determines housing and financial wealth , for example, higher healthcare spending may deplete the housing and financial wealth. One way to address this is to instrument wealth variables such that instruments are correlated with wealth variables but uncorrelated with the unobserved factors that affect healthcare spending. For example, one could instrument the housing wealth with local house prices. However, it is not possible to exploit the local house

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prices as an instrument since the geographic information (state/region) of households is not available in the SCF . Nevertheless, the results on housing and financial wealth effects in the paper provide new evidence on the existence of differences in the importance of various forms of wealth for household healthcare spending.

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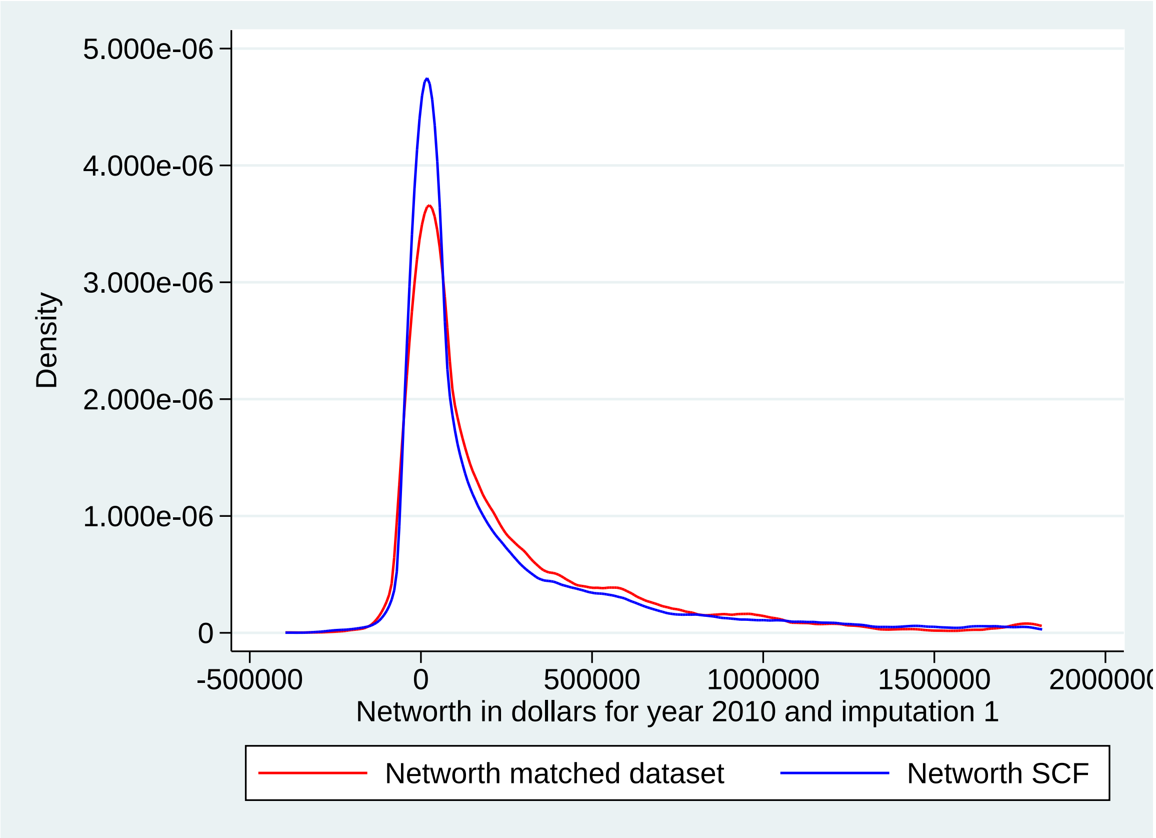

Figure 1.1: **Distribution of net worth in matched and Survey of Consumer Finances datasets**

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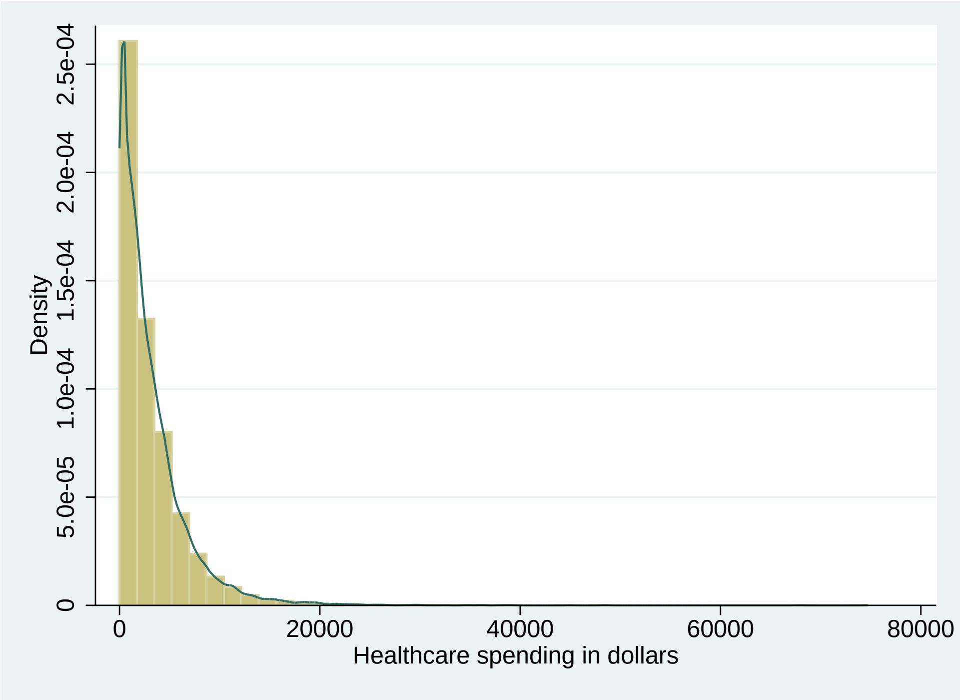

Figure 1.2a: **Histogram and kernel density plot for levels of healthcare spending**

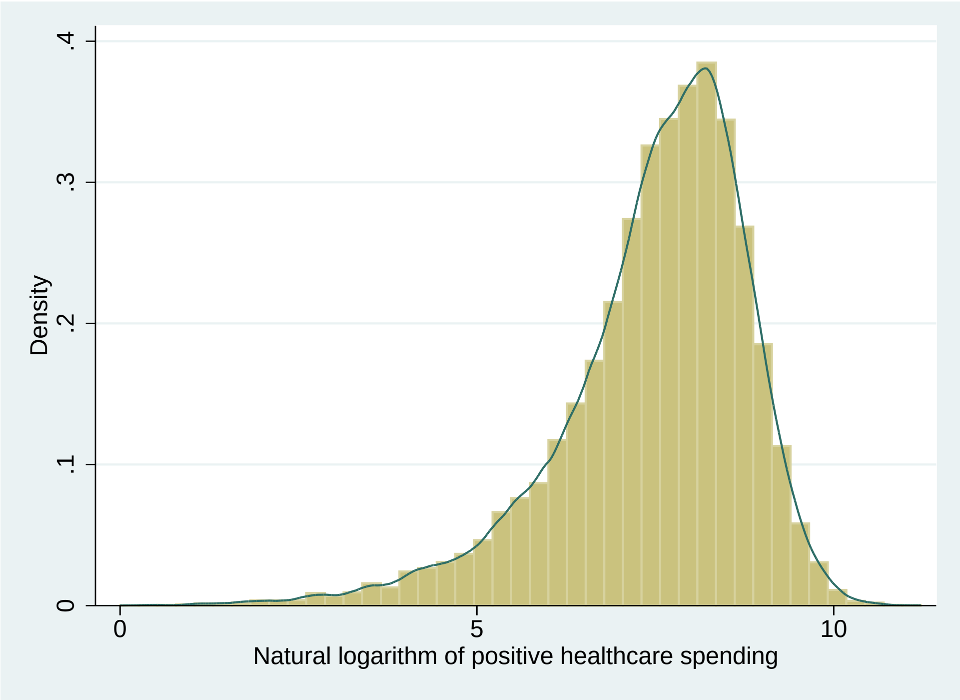

Figure 1.2b: **Histogram and kernel density plot for natural logarithm of positive healthcare spending**

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Table 1.1: **Correlation coefficients for variables by survey years of SCF**

	1989		1992	
	CES log income	SCF log income	CES log income	SCF log income
CES log income	1.000	0.781***	1.000	0.719***
SCF log income	0.781***	1.000	0.719***	1.000
Healthcare spending	0.122***	0.118***	0.161***	0.150***
Gross financial wealth	0.100***	0.163***	0.102***	0.273***
Gross housing wealth	0.277***	0.401***	0.266***	0.438***
Gross other real estate wealth	0.157***	0.233***	0.120***	0.205***
Net financial wealth	0.097***	0.166***	0.098***	0.268***
Home equity	0.210***	0.331***	0.192***	0.329***
Other real estate equity	0.037**	0.054***	0.097***	0.137***
	1995		**1998**	
	CES log income	SCF log income	CES log income	SCF log income
CES log income	1.000	0.804***	1.000	0.794***
SCF log income	0.804***	1.000	0.794***	1.000
Healthcare spending	0.167***	0.176***	0.165***	0.172***
Gross financial wealth	0.096***	0.136***	0.034**	0.121***
Gross housing wealth	0.143***	0.198***	0.222***	0.303***
Gross other real estate wealth	0.131***	0.188***	0.082***	0.170***
Net financial wealth	0.091***	0.130***	0.033**	0.120***
Home equity	0.090***	0.139***	0.147***	0.213***
Other real estate equity	0.093***	0.186***	0.013	0.049***
	2001		**2004**	
	CES log income	SCF log income	CES log income	SCF log income
CES log income	1.000	0.799***	1.000	0.817***
SCF log income	0.799***	1.000	0.817***	1.000
Healthcare spending	0.166***	0.156***	0.244***	0.231***
Gross financial wealth	0.087***	0.096***	0.146***	0.204***
Gross housing wealth	0.289***	0.394***	0.346***	0.437***
Gross other real estate wealth	0.127***	0.198***	0.139***	0.186***
Net financial wealth	0.086***	0.095***	0.143***	0.201***
Home equity	0.193***	0.282***	0.179***	0.238***
Other real estate equity	0.097***	0.149***	0.093***	0.120***
	2007		**2010**	
	CES log income	SCF log income	CES log income	SCF log income
CES log income	1.000	0.743***	1.000	0.829***
SCF log income	0.743***	1.000	0.829***	1.000
Healthcare spending	0.181***	0.191***	0.261***	0.264***
Gross financial wealth	0.115***	0.161***	0.143***	0.259***
Gross housing wealth	0.278***	0.399***	0.232***	0.375***
Gross other real estate wealth	0.077***	0.175***	0.136***	0.163***
Net financial wealth	0.112***	0.157***	0.140***	0.256***
Home equity	0.201***	0.306***	0.145***	0.280***
Other real estate equity	0.097***	0.192***	0.055***	0.080***

Notes: Superscripted notations next to the coefficients indicate the level of statistical significance from a two-tailed t- test. ***,**,*denote significance at 1 % , 5 %, 10 % level, respectively. See Table 1.2 for definitions of variables.

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Table 1.2: **Definitions of the variables**

Variable	**Definition**
Dependent variable	
Healthcare spending†	Total healthcare spending
Explanatory variables	
A. Binary variables	
Age 18-35‡	1 if $\leq$ 18 age of the household head (HH) $\leq$ 35
Age 36-49	1 if $\leq$ 36 age of the HH $\leq$ 49
Age 50-64	1 if $\leq$ 50 age of the HH $\leq$ 64
Married	1 if the HH is married/living with partner
Less than high school‡	1 if the HH has an education of twelfth grade or less
High school	1 if the HH is a high school graduate
Some college or more	1 if the HH has an education of some college or more
White‡	1 if the HH is white
African American	1 if the HH is African American
Other race	1 if the HH is not white or African American
Household size 1‡	1 if the family size is 1
Household size 2	1 if the family size is 2
Household size 3	1 if the family size is 3
Household size 4	1 if the family size is 4 or more
Unemployed‡	1 if the HH is unemployed
Manager and professional	1 if the HH's job category is manager and professional
Administrative	1 if the HH's job category is administrative support, technical, sales
Service	1 if the HH's job category is service
Operators	1 if the HH's job category is operator, assembler, laborer
Other	1 if the HH's job category is other⋆
Insured	1 if at least one of the household members is covered by insurance
Year 1989‡	1 if the SCF survey year is 1989
Year 1992	1 if the SCF survey year is 1992
Year 1995	1 if the SCF survey year is 1995
Year 1998	1 if the SCF survey year is 1998
Year 2001	1 if the SCF survey year is 2001
Year 2004	1 if the SCF survey year is 2004
Year 2007	1 if the SCF survey year is 2007
Year 2010	1 if the SCF survey year is 2010
B. Continous variables	
Income†	Annual income
Gross financial wealth†	Gross financial wealth
Gross housing wealth†	Gross value of primary residence (houses)
Gross other real estate wealth†	Gross value of all residential real estate other than the primary residence
Net financial wealth†	Net financial wealth
Home equity†	Gross housing wealth minus mortgages and home equity loans
Other real estate equity†	Gross other real estate wealth minus the mortgages and equity loans

Notes: † indicates that the variable is expressed in 2010 US dollars.‡ is the omitted category in the model.⋆includes precision, production ,craft, repairing, farming, forestry, and fishing.

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Table 1.3: **Summary statistics**

	Full sample		**Non zero HCS**		**Zero HCS**	
Variables	Mean	Stdev	Mean	Stdev	Mean	Stdev
Healthcare spending	3052.87	3537.55	3281.60	3563.88	0.00	0.00
Age 18-35	0.26	0.44	0.25	0.43	0.45	0.50
Age 36-49	0.42	0.49	0.43	0.49	0.35	0.48
Age 50-64	0.32	0.47	0.33	0.47	0.20	0.40
Married	0.64	0.48	0.67	0.47	0.32	0.47
Less than high school	0.10	0.31	0.09	0.29	0.26	0.44
High School	0.26	0.44	0.26	0.44	0.31	0.46
Some college or more	0.63	0.48	0.65	0.48	0.43	0.50
White	0.88	0.32	0.89	0.31	0.73	0.44
African American	0.08	0.27	0.07	0.25	0.21	0.40
Other race	0.04	0.20	0.04	0.19	0.06	0.24
Household size 1	0.22	0.41	0.20	0.40	0.42	0.49
Household size 2	0.29	0.45	0.29	0.45	0.20	0.40
Household size 3	0.18	0.38	0.18	0.38	0.14	0.34
Household size 4	0.32	0.47	0.33	0.47	0.25	0.43
Unemployed	0.17	0.37	0.16	0.37	0.30	0.46
Manager and professional	0.34	0.47	0.35	0.48	0.15	0.36
Administrative	0.22	0.42	0.23	0.42	0.17	0.38
Service	0.09	0.29	0.09	0.28	0.17	0.37
Operators	0.10	0.30	0.10	0.30	0.12	0.32
Other	0.08	0.26	0.07	0.26	0.09	0.28
Insured	0.67	0.47	0.68	0.47	0.52	0.50
Income	68932.73	61498.40	71707.02	61855.92	31902.80	41479.69
Gross financial wealth	252797.42	2229769.86	265731.28	2298388.31	80162.74	894388.27
Gross housing wealth	211204.54	481837.24	221734.63	492809.13	70654.13	261208.98
Gross other real estate wealth	44143.98	249055.11	46626.07	257159.99	11014.25	78031.96
Net financial wealth	236982.03	2228912.04	249486.96	2297913.84	70072.40	881045.54
Home equity	129512.86	433811.71	135954.04	444394.62	43539.22	237127.80
Other real estate equity	35125.72	325059.84	37242.75	336522.05	6868.68	60118.37
No. of observations	23361	23361	21732	21732	1628	1628

Notes: See Table 1.2 for definitions of the variables. HCS indicates healthcare spending.

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Table 1.4: **Specification tests**

Model	Test type	Test value	P-value
Dependent DH model versus independent DH model	LR	466.01	0.00
Dependent DH model versus Heckman model	Vuong	15.24	0.00
Dependent DH model versus Heckman model	Clarke		0.00
Heckman model versus two-part model	LR	466.01	0.00

Notes: DH stands for double-hurdle model and LR stands for likelihood ratio test. All tests are conducted using gross wealth specification for pooled sample (1989-2010)

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Table 1.5: **ML estimation of the dependent double-hurdle model using gross wealth specification**

Dependent Variable:	log(healthcare spending)	
	Participation equation	Consumption equation
Age 36-49	0.2398*** (0.0285)	0.2006*** (0.0217)
Age 50-64	0.4756*** (0.0352)	0.4664*** (0.0249)
Married	0.3332*** (0.0357)	0.2398*** (0.0279)
High School	0.2313*** (0.0375)	0.1424*** (0.0325)
Some college or more	0.4053*** (0.0381)	0.1911*** (0.0328)
African American	-0.3241*** (0.0368)	-0.1617*** (0.0334)
Other race	-0.1952*** (0.0568)	-0.0534 (0.0440)
Household size 2	0.1582*** (0.0381)	0.1876*** (0.0311)
Household size 3	0.1365*** (0.0446)	0.2360*** (0.0351)
Household size 4	0.1579*** (0.0437)	0.2448*** (0.0350)
Manager and professional	0.3461*** (0.0401)	-0.0888*** (0.0291)
Administrative	0.2552*** (0.0396)	-0.1238*** (0.0295)
Service	0.1389*** (0.0428)	-0.2138*** (0.0358)
Operators	0.1769*** (0.0454)	-0.2830*** (0.0350)
Other	0.1128** (0.0500)	-0.1840*** (0.0387)
Insured	0.2831*** (0.0251)	0.0439** (0.0180)
Year 1992	-0.1459** (0.0570)	0.0662* (0.0390)
Year 1995	-0.1937*** (0.0603)	0.3299*** (0.0408)
Year 1998	-0.0829 (0.0550)	0.2937*** (0.0348)
Year 2001	-0.1327** (0.0530)	0.3071*** (0.0341)
Year 2004	-0.2216*** (0.0560)	0.2751*** (0.0378)
Year 2007	-0.3257*** (0.0525)	0.3275*** (0.0349)
Year 2010	-0.2684*** (0.0515)	0.2908*** (0.0350)
Log of income		0.2018*** (0.0111)
Log of gross financial wealth		0.0099*** (0.0031)
Log of gross housing wealth		0.0223*** (0.0017)
Log of gross other real estate wealth		-0.0020 (0.0020)
Constant	0.4547*** (0.0650)	4.2654*** (0.1093)
Sigma	1.2566*** (0.0066)	
Rho	-0.9051*** (0.0067)	

Notes: All estimates are from the dependent double-hurdle model using gross wealth specification for pooled sample (1989-2010). Columns 1 and 2 present the estimates for the participation equation and consumption equation, respectively . The omitted categories for age, education, race, household size and occupation are age 18-35, less than high school, white, household size 1, and unemployed, respectively. Standard errors are in parentheses. Number of observation varies from 23349 to 23376 depending on the imputation number. Superscripted notations next to the coefficients indicate the level of statistical significance from a two-tailed t-test. ***,**,*denote significance at 1 % , 5 %, 10 % level, respectively. See Table 1.2 for definitions of variables.

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Table 1.6: **Average elasticities for pooled sample with respect to continuous variables using gross wealth specification**

	Unconditional level
Income	0.2018*** (0.0120)
Gross financial wealth	0.0099*** (0.0030)
Gross housing wealth	0.0223*** (0.0020)
Gross other real estate wealth	-0.0020 (0.0020)

Notes: Unconditional level average elasticity based on the estimates are from double-hurdle models using gross wealth specification. Since the continuous economic variables appear only in consumption equation, probability average elasticity is close to zero and therefore, conditional level average elasticity is the same as the unconditional level average elasticity. Number of observation varies from 23349 to 23376 depending on the imputation number. Bootstrapped standard errors are in parentheses. Superscripted notations next to the coefficients indicate the level of statistical significance from a two-tailed t-test. ***,**,*denote significance at 1 % , 5%, 10 % level, respectively. See Table 1.2 for definitions of variables.

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Table 1.7: **Average partial effects for pooled sample with respect to binary variables using gross wealth specification**

	Probability	**Conditional level**	**Unconditional level**
Age 36-49	0.0309*** (0.0040)	0.2606*** (0.0200)	0.0705*** (0.0050)
Age 50-64	0.0560*** (0.0040)	0.5768*** (0.0230)	0.1407*** (0.0050)
Married	0.0459*** (0.0050)	0.3283*** (0.0270)	0.0965*** (0.0060)
High School	0.0286*** (0.0050)	0.1983*** (0.0340)	0.0595*** (0.0060)
Some college or more	0.0571*** (0.0060)	0.3004*** (0.0340)	0.1065*** (0.0080)
African American	-0.0506*** (0.0070)	-0.2568*** (0.0330)	-0.0943*** (0.0090)
Other race	-0.0288*** (0.0110)	-0.1081** (0.0420)	-0.0483*** (0.0140)
Household size 2	0.0198*** (0.0040)	0.2263*** (0.0300)	0.0530*** (0.0050)
Household size 3	0.0169*** (0.0050)	0.2691*** (0.0350)	0.0553*** (0.0070)
Household size 4	0.0200*** (0.0050)	0.2838*** (0.0350)	0.0609*** (0.0070)
Manager and professional	0.04200*** (0.0040)	-0.0062 (0.0270)	0.0463*** (0.0060)
Administrative	0.0307*** (0.0040)	-0.0634** (0.0280)	0.0261*** (0.0060)
Service	0.0170*** (0.0050)	-0.1805*** (0.0370)	-0.0055 (0.0070)
Operators	0.0212*** (0.0050)	-0.2412*** (0.0330)	-0.0092 (0.0060)
Other	0.0139** (0.006)0	-0.1567*** (0.0360)	-0.0057 (0.0080)
Insured	0.0394*** (0.0040)	0.1197*** (0.0170)	0.0615*** (0.0050)

Notes: Average partial effects (APE) based on the estimates from double-hurdle models using gross wealth specification for pooled sample (1989-2010). Probability APE indicates absolute change in participation probability, conditional level and unconditional level APE indicates relative change in health-care expenditures. Bootstrapped standard errors are in parentheses. Number of observation varies from 23349 to 23376 depending on the imputation number . Bootstrapped standard errors are in parentheses. Superscripted notations next to the coefficients indicate the level of statistical significance from a two-tailed t-test. ***,**,*denote significance at 1 % , 5 %, 10 % level, respectively. See Table 1.2 for definitions of variables.

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Table 1.8: **Average elasticities by SCF survey years with respect to economic variables using gross wealth specification**

	Unconditional level
Panel A: 1989 (No. of observations : 2254-2261)	
Income	0.1797*** (0.0410)
Gross financial wealth	0.0123 (0.0120)
Gross housing wealth	0.0242*** (0.0060)
Gross other real estate wealth	-0.0036 (0.0060)
Panel B: 1992 (No. of observations : 2147-2159)	
Income	0.2161*** (0.0450)
Gross financial wealth	0.0152 (0.0100)
Gross housing wealth	0.0204*** (0.0060)
Gross other real estate wealth	-0.0038 (0.0070)
Panel C: 1995 (No. of observations : 1803-1812)	
Income	0.0943** (0.0410)
Gross financial wealth	0.0159 (0.0120)
Gross housing wealth	0.0188*** (0.0060)
Gross other real estate wealth	-0.0062 (0.0060)
Panel D: 1998 (No. of observations : 3391-3397)	
Income	0.1513** (0.0280)
Gross financial wealth	0.0146 (0.0090)
Gross housing wealth	0.0174*** (0.0050)
Gross other real estate wealth	-0.0036 (0.0050)
Panel E: 2001 (No. of observations : 3955-3975)	
Income	0.1650*** (0.0280)
Gross financial wealth	0.0070 (0.0080)
Gross housing wealth	0.0210*** (0.0050)
Gross other real estate wealth	0.0034 (0.0040)
Panel F: 2004 (No. of observations : 2257-2566)	
Income	0.2751*** (0.0480)
Gross financial wealth	-0.0012 (0.0100)
Gross housing wealth	0.0240*** (0.0050)
Gross other real estate wealth	-0.0020 (0.0050)
Panel G: 2007 (No. of observations : 3586-3600)	
Income	0.2469*** (0.0290)
Gross financial wealth	0.0016 (0.0090)
Gross housing wealth	0.0309*** (0.0060)
Gross other real estate wealth	-0.0058 (0.0040)
Panel H: 2010 (No. of observations : 3633-3637)	
Income	0.2666*** (0.0350)
Gross financial wealth	0.0165* (0.0090)
Gross housing wealth	0.0240*** (0.0050)
Gross other real estate wealth	-0.0007 (0.0050)

Notes: Unconditional level average elasticity based on the estimates from double-hurdle models using gross wealth specification for each SCF survey years (1989, 1992, 1995, 1998, 2001, 2004, 2007, and 2010) . Since the continuous economic variables appear only in consumption equation, probability average elasticity is close to zero and therefore, conditional level average elasticity is the same as the unconditional level average elasticity. Bootstrapped standard errors are in parentheses . Superscripted notations next to the coefficients indicate the level of statistical significance from a two-tailed t-test. ***,**,*denote significance at 1 % , 5%, 10 % level, respectively. See Table 1.2 for definitions of variables.

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Table 1.9: **Average elasticities for constrained and unconstrained households with respect to economic variables using gross wealth specification**

	Unconditional level
Panel A: Unconstrained households (No. of observations : 18774-18827)	
Income	0.1883*** (0.0130)
Gross financial wealth	0.0092** (0.0040)
Gross housing wealth	0.0211*** (0.0020)
Gross other real estate wealth	-0.0021 (0.0020)
Panel B: Constrained households (No. of observations : 4528-4587)	
Income	0.2392*** (0.0410)
Gross financial wealth	0.0074 (0.0070)
Gross housing wealth	0.0243*** (0.0040)
Gross other real estate wealth	0.0019 (0.0060)

Notes: Unconditional level average elasticity based on the estimates from double-hurdle models using gross wealth specification for unconstrained and constrained households. Since the continuous economic variables appear only in consumption equation, probability average elasticity is close to zero and therefore, conditional level average elasticity is the same as the unconditional level average elasticity. Households are defined as credit-constrained if they were turned down for credit or received less that they applied for, or were discouraged from applying for credit because they believed that they would be turned down. Bootstrapped standard errors are in parentheses. Superscripted notations next to the coefficients indicate the level of statistical significance from a two-tailed t-test. ***,**,*denote significance at 1 % , 5%, 10 % level, respectively. See Table 1.2 for definitions of variables.

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Table 1.10: **Average elasticities by age groups with respect to economic variables using gross wealth specification**

	Unconditional level
Panel A: Age 18-35 (No. of observations :6131-6139)	
Income	0.3392*** (0.0380)
Gross financial wealth	0.0107 (0.0070)
Gross housing wealth	0.0201*** (0.0040)
Gross other real estate wealth	-0.0046 (0.0050)
Panel B: Age 36-49 (No. of observations : 9806-9816)	
Income	0.1824*** (0.0180)
Gross financial wealth	0.0029 (0.0050)
Gross housing wealth	0.0186*** (0.0030)
Gross other real estate wealth	0.0010 (0.0030)
Panel C: Age 50-64 (No. of observations : 7404-7421)	
Income	0.1537*** (0.0180)
Gross financial wealth	0.0141** (0.0060)
Gross housing wealth	0.0264*** (0.0040)
Gross other real estate wealth	-0.0024 (0.0030)

Notes: Unconditional level average elasticity based on the estimates from double-hurdle models using gross wealth specification for Age 18-35, Age 36-49, and Age 50-64. Since the continuous economic variables appear only in consumption equation, probability average elasticity is close to zero and therefore, conditional level average elasticity is the same as the unconditional level average elasticity. Bootstrapped standard errors are in parentheses. Superscripted notations next to the coefficients indicate the level of statistical significance from a two-tailed t-test. ***,**,*denote significance at 1 % , 5%, 10 % level, respectively. See Table 1.2 for definitions of variables.

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CHAPTER 2 LOCAL HOUSE PRICES AND HEALTH

2.1 Introduction

This chapter examines the impact of local house prices on health and health behaviors of the individuals using the Behavioral Risk Factor Surveillance System (BRFSS) from 2001 to 2012. The conceptual approach is based on the pure wealth mechanism. Under this mechanism, a decrease in local level house prices increases the likelihood that homeowners will face a negative wealth shock. A negative wealth shock could result in the poor health of the homeowners through the reduction in expenditures on preventive healthcare and physical activities which are considered as normal goods (Xu, 2013; Yilmazer, Babiarz, and Liu, 2015; Lusardi, Schneider, and Tufano, 2015). However, the shock could also result in the reduction of the risky healthy behaviors such as smoking and alcohol consumption (normal goods) which could positively impact homeowners' health (Fichera and Gathergood, 2013; Van Kippersluis and Galama, 2013). The directional impact on health is thus ambiguous for homeowners. On the one hand, if house prices and rents are positively correlated in a given area, renters face a positive wealth shock as house prices fall, because it reflects that renting or potential buying become inexpensive. On the other hand, the health effect for renters depends on whether the improved health resulting from the increase in expenditures on preventive healthcare and physical activity dominates the deterioration in health resulting from the rise in risky health behaviors or vice-versa.

The health effect of the local house prices is identified by within-county variation in house prices, relative to the fluctuations occurring in other counties. I interact the local house prices with the education level, where lower level of education proxies the renters and higher level of education proxies the homeowners, to allow the different health effects

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on homeowners and renters. In addition, I separately focus on housing boom period (January 2001- October 2006) and housing bust period (November 2006-December 2012) to allow asymmetric responses to increase and decrease in local house prices. I also examine heterogeneity in the health effect of the local house prices by stratifying the sample by gender.

The results indicate the evidence of relationships for weight-related health, physical and mental health, and health behaviors. In particular, among those likely to be homeowners, I find the evidence of positive effects of house prices on poor mental health and binge drinking but a negative effect on physical activity. However, for those likely to be renters, I document a positive impact of house prices on body mass index but negative effects on physical health and smoking.

The subsample analysis also suggests substantial heterogeneity of the health effects across gender. Additionally, I show that local house prices have asymmetric effects on health outcomes. In particular, local house prices have relatively large and significant impacts on binge drinking and physical activity during the housing bust period. Summarizing the papers contribution, it first provides an empirical examination of the relationship between local house prices and health and health behaviors by using the United States (US) data covering a period of housing boom and housing bust. Second, it considers the possibility of the asymmetry in health effects by separately focusing on boom and bust of housing cycle.

In the paper, Section 2.2 provides the conceptual framework, and Section 2.3 presents the previous literature. Section 2.4 describes the data, and Section 2.5 presents the econometric model. Section 2.6 discusses the empirical results, and Section 2.7 presents the extensions. Section 2.8 concludes.

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2.2 Conceptual Framework

Conceptually, local house prices can affect the health through the pure wealth mechanism. Under this mechanism, the decrease (increase) in local house prices increases the likelihood that homeowners in a given area will face a negative (positive) wealth shock. A wealth shock may affect peoples health by affecting the consumption of health-related commodities such as preventive healthcare, physical activities, and risky healthy behaviors which are considered normal goods (Xu, 2013; Fichera and Gathergood, 2013).[1] Consider, for example, homeowners who experience negative wealth shocks resulting from decreases in local house prices. These shocks may lead to the increase in the likelihood of homeowners reducing the expenditures on preventive healthcare such as physician visits and physical activities which may negatively impact the homeowners' health (Yilmazer, Babiarz, and Liu, 2015; Lusardi, Schneider, and Tufano, 2015; Xu, 2013). However, the shocks may also result in the reduction of the risky healthy behaviors such as smoking and alcohol consumption which may positively impact homeowners' health (Van Kippersluis and Galama, 2013; Fichera and Gathergood, 2013). So, the overall effect on health of a decrease in local house prices is ambiguous for homeowners. Similarly, an increase in local house prices also has ambiguous effect on the health of the homeowners.

Note, however, that individuals who are not homeowners in a given area may also experience health effects because of local house prices changes. For example, if the house prices and rents are positively correlated in a given area, local house price decreases represent a positive wealth shock to the renters because it reflects that renting or for

[1]Empirical studies show that decrease in house prices may decrease the non-housing consumption by decreasing households perceived wealth, or by tightening borrowing constraints (Campbell and Cocco, 2007). Mian and Sufi (2014b) document that households use a part of money borrowed out of home equity to finance home improvement. If this is the case, then a fall in the local house prices may increase the likelihood of the unmet home repairs and maintenance and thus, may lead to poor health (Rohe and Lindblad, 2013).

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those planning to buy a house in the future, buying become inexpensive (Fichera and Gathergood, 2013; Ratcliffe, 2015). A positive wealth shock is therefore expected to have positive impact on renters' health through the increase in spending on preventive healthcare or physical activities. However, this may be offset by the increase in the risky healthy behaviors. Thus, the net effect on health of a change in local house prices is also ambiguous for renters.

The directional impact of the local house prices on health will be more complex if local house prices have spillover effects on unemployment. For example, Appendix Figure B.1.1 shows that the increase in the unemployment rate from 2006 to 2013 for the counties with the big decline in house prices from 2006 to 2009 was twice as high as the rise in counties with the smallest decline in house prices.[2] Further, the health effect of local house prices may be asymmetric. For example, during a housing bust period, many households may find the values of their homes dropping below the value of their mortgages.[3] This may result in the increased psychological stress as these underwater households face a dilemma of whether to continue staying in their homes. [4] Moreover, the foreclosures have negative externality and thus may push local house prices further down as prices of all the homes in the given area suffer.[5] Health effects of homeowners and renters who are insulated from the foreclosures may also deteriorate as a result of the negative externality of foreclosure (Currie and Tekin, 2015).

[2] See also, http://houseofdebt.org/2014/05/22/employment-scars-of-housing-bust.html

[3] In 2011, 23 percent of all mortgaged house had negative equity and many of these have continued to stay below (Mian and Sufi, 2014a).

[4] Households may continue stay in their homes if they exhibit nominal loss aversion (Kahneman and Tversky, 1979; Engelhardt, 2003), in which case they will experience problems in making mortgage payments (Rohe and Lindblad, 2013) or walk away and allow the bank foreclose (Mian and Sufi, 2014a).

[5] This occurs because of the fire sale of the foreclosed property by the bank. The fire-sale price is then used by other home buyers and appraisers to estimate the prices of all other homes in the given area (Mian and Sufi, 2014a).

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2.3 Previous Literature

The present work is related to the literature that investigates the health effects of local house prices and foreclosures (e.g., Fichera and Gathergood (2013); Ratcliffe (2015); Tekin, McClellan, and Minyard (2015)). Three recent papers have considered the effects on health. Fichera and Gathergood (2013), using the British Household Panel Survey (BHPS) from 1991 to 2008, look at the effect of the local house price movements on health. They find that the increase in home equity reduces the likelihood of homeowners having different kinds of health conditions. They further show that health effects occur through two complementary channels: an increase in purchase of private medical insurance and an increase in physical activity and leisure as a result of a decrease in work hours. Ratcliffe (2015) also uses the BHPS and documents a positive correlation between house prices and the mental wellbeing of both homeowners and nonhomeowners which is inconsistent with the pure wealth mechanism. Tekin, McClellan, and Minyard (2015) investigate the impact of foreclosures on health using the data from four US states - Arizona, California, Florida, and New Jersey that have been among the hardest hit by the foreclosure crisis. They document that zip codes with increases in foreclosures are associated with the rise in the urgent and unscheduled hospital and emergency room visits during the period 2005 to 2009.

This paper extends the literature in two ways. First, I also use the US data and focus on local house price fluctuations which have the potential to affect a larger population of renters and homeowners compared to an event like foreclosure that affects only a subset of homeowners. Second, I examine the health effects for the boom and bust of the housing cycle separately, to accommodate a possible asymmetric effects of house prices.

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2.4 Data

To analyze the relationship between local house prices and health and health behaviors, I use the Behavioral Risk Factor Surveillance System data. The BRFSS is an ongoing monthly telephone survey administered by the US Centers for Disease Control and Prevention to track health conditions and risk behaviors of the individuals age 18 and over. The BRFSS questionnaire mainly consists of core component and optional modules. The core component consists of the questions asked by all states and includes questions about height, weight, physical and mental health, health behavior (cigarette smoking, alcohol consumption, and physical activity), and demography.

The optional modules consist of the questions on specific topics such as cancer survivorship, mental illness, and stigma which are asked by some states but not others. In the present study, I focus on the core component of the BRFSS from 2001 to 2012. Although the BRFSS is available since 1984, I do not use the data before 2001 because of significant changes to the survey in that year (Barbaresco, Courtemanche, and Qi, 2015). In addition, I restrict the sample to the individuals for whom county of residence is publicly available to obtain a more localized measure of housing market conditions.[6] This restriction results in a reduction of overall BRFSS sample by 8.3% to 20.5%, depending on the year in question.[7] In the following sections, I discuss the construction of the variables and analysis sample used in the study.

2.4.1 Variable Construction

I categorize the variables into three groups: i) health variables, which serve as the dependent variables; ii) housing market variable which serves as the explanatory variable

[6]For the period of 2001 to 2012, the BRFSS does not identity the county of residence for individuals residing in a county with fewer than 50 respondents or a county with adult populations less than or equal to 10,000 residents.

[7]BRFSS 2013 does not report county of residence.

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that is central to this paper; and iii) control variables that include demographic variables (age, sex, education, marital status and race), group average income, local unemployment rate, and national stock market index. In the following paragraphs, I discuss the measurement of these variables. [8]

(i) **Health variables** : In line with the existing literature that investigates the impact of business cycle [9], I focus on three main measures of individual health. I group the variables into three general categories: weight-related health, physical and mental health, and health behaviors (Charles and DeCicca, 2008). Weight-related health and health behaviors are the channels through which the local house prices can affect health, whereas the physical and mental health measure the individual's reported health directly (Tekin, McClellan, and Minyard, 2015).

- **Weight-related health**: I use body mass index (BMI) as a proxy for weight-related health. BMI is a standard measure of fatness (Cawley, 2004) and is defined as weight in kilograms divided by height in meters squared. BMI is constructed using self-reported height and weight. I transformed BMI using natural logarithm to take into account positive skewness and thick tails.
- **Physical and mental health**: With respect to the physical health, I construct a categorical variable 'Physical health' from the response to the following question: 'Would you say that in general your health is'. The variable takes values of 1, 2, 3, 4, and 5 if responses are poor, fair, good, very good, and excellent respectively. As to the mental health, I construct a variable 'Poor mental health' from the response to the following question: 'Now thinking about your

[8] Appendix Table Table B.2.1A also provides the definitions of the variables and the data sources for the variables.

[9] Ruhm (2005) and Charles and DeCicca (2008) use aggregate (state or MSA-level) unemployment rate, while Tekin, McClellan, and Minyard (2015) use aggregate employment rate as a proxy for business cycle.

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mental health, which includes stress, depression, and problems with emotions, for how many days during the past 30 days was your mental health not good?'

- **Health behaviors**: This category refers to cigarette smoking, alcohol consumption, and physical activity. Under cigarette smoking, I use the response to the following question: 'Do you now smoke cigarettes every day, some days or not at all?' and create binary variables: 'current smoker': that takes a value of 1 if respondents are nondaily (current) smokers (those who answer 'some days'), and 'daily smoker' that takes a value of 1 if respondents are daily smokers (those who answer 'every day'). For alcohol consumption, I construct the variable 'Binge drinking' from the response to the following questions : 'During the past 30 days, on the days when you drank, about how many drinks [10] did you drink on the average?'.Finally, I create a binary variable reflecting the physical activity from the response to the following question: 'During the past month, other than your regular job, did you participate in any physical activities or exercises such as running, calisthenics, golf, gardening, or walking for exercise?'. The variable takes a value of one if the individuals have participated in any physical activities or exercises in the past month, and zero otherwise.

(ii) **Housing market variable**: I use the Zillow Home Value Index (ZHVI) from Zillow.com, a website that provides house price data, as a proxy for local house prices. The ZHVI is a hedonic index [11] and is based on detailed information about the property (such as the size of the house, the number of bedrooms, and the number of bathrooms) collected from public records (Guerrieri, Hartley, and Hurst,

[10] One drink is equivalent to a 12-ounce beer, a 5-ounce glass of wine, or a drink with one shot of liquor.
[11] The index is seasonally adjusted. For details on methodology, see http://www.zillow.com/research/zhvi-methodology-6032/

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2010).[12] An advantage of the ZHVI data relative to the Federal Housing Finance Agency (FHFA) house price index is that it allows the examination of the impact of the local house prices monthly instead of quarterly, and at the county level instead of metropolitan or state level. Previous empirical studies find the ZHVI to have a good accuracy. For example, Mian and Sufi (2009) show that for 2248 zip codes, the house price changes for Fiservs Case Shiller Weiss indices and Zillow have a correlation coefficient of 0.91. Huang and Tang (2012) also document similar coefficient for 210 metropolitan areas for the house price changes for the FHFA House Price Index and ZHVI.[13] Figure 2.1a plots the national measure of monthly ZHVI (nominal) for single primary residence for the sample period 2001 to 2012. The sample period includes the housing market boom that started in January 2001 and peaked in November 2006 and housing market bust that started in November 2006 before it began to recover in January 2012.[14] In nominal terms, house prices increased 59.30 percent during the boom period and declined 20.65 percent during the bust period. The overall house prices increase was 26.41 percent. The numbers are similar when house prices are expressed in real terms. After converting the ZHVI into January 2012 dollar terms using all items of CPI-U of BLS, house prices increased 33.51 percent during the boom period and declined 29.60 during the bust period. The overall house prices decrease was 6.01 percent. A similar pattern is observed as shown in Figure 2.1b when I plot the monthly average county-level

[12] The indices are available at seven geographic levels: neighborhood, ZIP code, city, congressional district, county, metropolitan area, state and the nation and are also available for home type, price tier and number of bedrooms.

[13] Guerrieri, Hartley, and Hurst (2010) use the ZHVI for robustness check and document results similar to that for the Case-Shiller index.

[14] The start of the housing bust is identified by looking at when prices peaked and then began to fall. This was November 2006 for national measure of the ZHVI and October 2006 for average county measure of the ZHVI. This is similar to that identified by Cohen, Coughlin, and Lopez (2012) using Standard and Poors /Case-Shiller house price index and the FHFA Purchase-Only House Price Index.

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ZHVI (nominal) for single primary residence. For the analysis, I use the average of real ZHVI for single family residence during the three months ending with the survey month or negative binomial models for count data.

(iii) **Control variables**: In line with previous studies, I include the following control variables: age, gender, race and ethnicity, marital status, education and income levels. Group average is used for income since individual income is likely to be simultaneously determined with health status (Ruhm, 2005). [15]The BRFSS reports the income (in $) in the ranges: less than 10,000, 10,000-14,999, 15,000-19,999, 20,000-24,999, 25,000-34,999, 35,000-49,999, 50,000-74,999, and 75,000 and over. For the estimation purpose, respondent's income is assumed to be at the midpoint of each range and 150 % of the top category and is then converted to 2012 year dollars using the all-items of CPI-U of the Bureau of Labor Statistic (BLS) (Ruhm, 2005). The weighted average incomes are calculated for 36 demographic groups stratified by sex (male versus female), age (25-29, 30-34, 35-39, 40-44, 45-49, and 50-55), and education (less than high school, high school or some college, and college) (Tekin, McClellan, and Minyard, 2015). I also control for local macroeconomic condition to alleviate the concern that it might be driving the relationship between local house prices and health (Ruhm, 2005). Monthly county-level unemployment rate from the BLS Local Area Unemployment Statistics database is used as a proxy for local macroeconomic condition .[16] In addition, I control for stock market condition using the natural log of the monthly mean daily market closing Dow Jones Industrial Average indexes (DJIA) (Tekin, McClellan, and Minyard, 2015).[17] Although the

[15]Estimates utilizing group-level variations are unlikely to suffer from omitted variable and endogeneity bias, but will be less precisely estimated (Ruhm, 2005).

[16]I use the average of unemployment rates during the three months ending with the survey month.

[17]See http://measuringworth.com/datasets/DJA/index.php

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BRFSS provides information on individual unemployment, I do not control for it because it is more likely to be endogenous (Ruhm, 2005).

2.4.2 Analysis Sample

The analysis sample after excluding the individuals for whom county of residence is not publicly available in the BRFSS consists of approximately 3.9 million individual-month observations for the years 2001-2012 inclusive. The sample size is further reduced after I merge the ZHVI to the BRFSS data by county of residence and month of the interview of the respondents. This is because the ZHVI is available only for 857 to 998 counties , depending on the year in question. The merged sample consists of approximately 2.67 million individual-month observations. The sample is further limited to individuals less than age 65. After excluding missing data on control variables, I have a total sample size of 1.67 million individual-month observations. However, analysis sample differs across the outcome variables because of the omission of these variables. Table 2.1 provides the descriptive statistics for the health outcome variables and the explanatory variables for the full sample period (January 2001-December 2012).

2.5 Empirical Model

To measure the effect of local house price on individual health, I use the following model:

$$\begin{aligned} Health_{ijmt} =& \beta_0 + \beta_{11}\text{lnHousePrices}_{jmt} \times \text{HighSchool}_{ijmt} + \beta_{12}\text{lnHousePrices}_{jmt} \times \\ & \text{SomeSchool}_{ijmt} + \beta_{13}\text{lnHousePrices}_{jmt} \times \text{College}_{ijmt} + \\ & \beta_{21}\text{SomeCollege}_{ijmt} + \beta_{22}\text{College}_{ijmt} + \delta \text{X}_{ijmt} + \mu_j + \alpha_m + \lambda_t + \epsilon_{ijmt} \end{aligned} \tag{2.1}$$

Here, i denotes the individual, j denotes county of residence, m denotes month of the interview, and t denotes year of the interview. Health represents one of the health vari-

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ables, lnHousePrices denotes log of the county-level house price [18], HighSchool, SomeCollege, and College refer to high school or less than high school, some college, and college and are used as the proxy for tenure status [19], and X is a vector of control variables (individual and county specific covariates). The house price is interacted with the education level to allow the effect of the house prices to differ across those likely to be homeowners and renters. In equation 2.1, I also include county fixed effects, μ , which control for any fixed differences across counties.

Regarding fixed effects, α is a vector of month fixed effect and takes into account the seasonal variations in some of the health outcome variables such as physical activity (Ruhm, 2005). Variable λ represents fixed year effects, which control for any nationwide trends and shocks that may influence health outcomes such as calorie content in national chain restaurants or federal health policies (Tekin, McClellan, and Minyard, 2015). ϵ represents an idiosyncratic random error term. I use the linear regression model for all dependent variables. [20] I report robust standard errors clustered at the county and month, assuming that observations are independent across counties and calendar months, but not within counties in a given month (Ruhm, 2005; Cameron, Gelbach, and Miller, 2011).[21].

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[18]Taking the log of house prices takes into account the diminishing marginal effects of house prices at higher price levels, which is consistent with diminishing marginal utility of housing wealth (Ratcliffe, 2015).

[19]Ideally, one would want to use the information on the tenure variable, but this information is not collected before 2009 in the BRFSS. Moreover, information for 2009 and 2010 contain considerable missing data. A more serious issue, however, is that the tenure variable is likely to be endogeneous to the health outcomes. One imperfect solution for this issue is to examine the effects across the level of education as in Farnham, Schmidt, and Sevak (2011), where the individuals with lower level of education (less than high school or high school) are assumed to be more likely to be renters and those with higher level of education (some college or college graduate) to be more likely to be homeowners. This requires that education to be exogenous with respect to the health outcomes as in previous studies (e.g., Ruhm (2005); Charles and DeCicca (2008); Tekin, McClellan, and Minyard (2015)).

[20]For ease in the interpretation, I use the linear specification. However, I also estimate binary probit models and find similar marginal effects.

[21]Clustering by county and month is important because house prices take the same values for respondents interviewed in a county during a given month and year Ruhm (2005).

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The coefficients of primary interest are, β_{11} and β_{12}. β_{11} (or β_{12}) gives the effect of the 1 % change in the local house prices for those with high school or lower than high school education (some college or college), all else held constant. Inclusion of county fixed effects implies that I use within-county variation in house prices to identify health effects (Ruhm, 2005). To examine the existence of sufficient within county-variation in house prices, I first regress monthly county house prices on year and month fixed effects. The resulting regression yields an R-squared of only 0.0012. Second, I regress monthly county house prices on county, year, and month fixed effects; the R-squared from this regression is 0.9753. Thus, national trends alone explain only around 1.2% of the variation in county house prices, and approximately around 2.5% of the variation in county house prices cannot be explained. [22] The data is weighted using the BRFSS sampling weights. In addition, the usefulness of county house prices depends on the existence of variation that is independent of state house prices. This condition is satisfied. For instance, the R-squared for an equation regressing county house prices on state house prices, year, and month fixed effects is just 0.1612. Hence, there appears to be substantial variation in county house prices, relative to corresponding state house prices.

For each health outcome variables as described in the Section 2.4.1, equation 2.1 is estimated for the full sample period (January 2001- December 2012). I also estimate equation 2.1 separately for the housing boom period (January 2001-October 2006) and bust period (November 2006-December 2012) to allow an asymmetric impact of house price increases and decreases on individual health and health behaviors.

Tekin, McClellan, and Minyard (2015) describe two sources of endogeneity: a) statistical endogeneity that results when the unobservable variables are correlated with both

[22] McInerney and Mellor (2012) also use the similar approach to examine the existence of sufficient within-state variations.

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health outcome variables and local house prices and b) structural endogeneity that occurs when individual health outcomes determines the local house prices (reverse causality). The present study is less likely to suffer from either types of endogeneity, since it is less likely that individual unobservables will be correlated with the local house prices or that individual health causes the fluctuations in local house prices.[23] In other words, the assumption that local house price changes are exogenous to an individual health is more likely to hold here.

2.6 Results

Column 2 of Panel A of Table 2.2 presents the estimated effect of local house price on weight-related health for the full sample period. The results suggest that local house prices have a statistically significant impact only for the individuals with high school degree. In particular, the coefficient estimate on the interaction of house prices and high school term implies that a one percent increase in local house prices increases BMI by 0.007 percent. Thus for those most likely to be renters, a negative wealth shock resulting from the increase in local house prices is likely to worsen weight problem. One shortcoming with the estimates using the full sample period is that they mask differences across the housing boom period and bust period. To alleviate this concern, I estimate the model separately for the housing boom period (January 2001-October 2006) and bust period (November 2006-December 2012). Column 3 presents results for the housing boom period, and column 4 displays the results for the housing bust period. The coefficient estimates do not indicate significant asymmetric relationship between local house prices and BMI.

Panel B of Table 2.2 displays results related to the physical and mental health. Results

[23] For example, one would expect that those with better health would move to counties with higher house prices.

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in column 2 suggest that local house prices have a significant negative impact on physical health for the individuals with high school degree and significant positive impact on poor mental health for the individuals with college degree. Looking at the results for boom and bust in columns 3 and 4, there appears to be significant asymmetric relationship between local house prices and physical health but not poor mental health. For example, the results in columns 3 and 4 indicate that for the individuals with high school degree, physical health worsens when local house prices increase (boom period), but the decrease in local house prices (bust period) has no statistically significant impact on physical health.

Panel C of Table 2.2 shows the estimated effect of local house prices on health behaviors. The estimates in column 2 indicate that for those likely to be homeowners, the local house prices have a positive and significant impact on binge drinking but a negative and significant impact on physical activity. The results also suggest that for those likely to be renters, local house prices have a negative and significant impact on daily smoking. Moreover, the coefficient estimates in columns 3 and 4 indicate an asymmetry in health effects for binge drinking and physical activity. In particular, local house prices have relatively larger (in magnitude) impacts on binge drinking and physical activity during the bust . In addition, impacts are more pronounced among those likely to be homeowners for binge drinking in the boom and the bust and physical activity in the bust.

2.7 Extension

Previous study (Fichera and Gathergood, 2013) documents that health effects of local house price are stronger for female than male. In the light of these findings, this section examines whether the health effects vary by gender. As before, I present the results separately for the full sample period, boom period, and bust period.

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The estimated effects of local house price on health and health behaviors are presented in Table 2.3 for males and Table 2.4 for females. As before, Panels A-C present the estimates for weight-related health, physical and mental health, and health behaviors, respectively, and columns 2-4 report the estimates for the full sample period, boom period, and bust period, respectively .

The results in column 2 of Panel A of Tables 2.3 and 2.4 suggest that an increase in local house prices significantly raises the BMI for females and those most likely to be renters, but has no significant effect for males and those most likely to be renters. This finding is consistent with Schmeiser (2009) who documents the significant positive income effect on BMI for low-income females with Earned Income Tax Credit (EITC) eligible earnings but not for low-income males with EITC-eligible earnings. A separate analysis for the housing boom and the bust in columns 3 and 4 (Panel B of Tables 2.3 and 2.4) show the absence of asymmetric relationship between local house prices and BMI for both male and female.

Panel B of Tables 2.3 and 2.4 present the estimated effect of local house prices on the physical and mental health for male and female, respectively. Column 2 in Panel B of Table 2.3 shows that for male and those likely to be renters, local house price have negative and significant impact on physical health, whereas column 2 in Panel B of Table 2.4 reveals that for female and those likely to be homeowners, local house price have positive and significant impact on poor mental health. A separate analysis for the boom and the bust in columns 3 and 4 (Panel B of Tables 2.3 and 2.4) shows that there is an asymmetric relationship between local house prices and physical health for only male.

Panel C of Tables 2.3 and 2.4 display the estimated effect of local house prices on health behaviors. The results in column 2 in Table 2.3 imply that among male and those

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likely to be homeowners, local house prices have positive and significant impact on binge drinking and smoking but negative and significant impact on physical activity. For male renters, the local house prices have a positive impact on binge drinking, with the impact being smaller than those likely to be homeowners. Among female and those likely to be homeowners (renters), there is significant negative relationship between house prices and physical activity (smoking).

Looking at the boom and the bust, the results in columns 3-4 in Panel C of Tables 2.3 and 2.4 reveal different patterns for male and female. For the male homeowners, house prices are positively and significantly related to the smoking both in the boom and the bust, but the impact is relatively larger in the bust. Results also suggest that binge drinking decreases, but physical activity increases, with the fall in house prices during the bust for both homeowners and renters. Closer inspection also show that the impact is larger for those likely to homeowners than those likely to be renters. Among female in the bust, a decrease in local house prices increases smoking for those likely to be renters and decreases drinking for those likely to be homeowners. In addition, the significant negative impact on physical activity is observed for the female in the bust, with the impact being relatively larger for those likely to be homeowners.

2.8 Conclusion

In this paper, I investigate the impact of local house prices on health and health behaviors of the individual using the Behavioral Risk Factor Surveillance System data from 2001 to 2012. The results indicate the evidence of relationship for weight-related health, physical and mental health, and health behaviors. In particular among those likely to be homeowners, I find the evidence of positive effects of local house prices on poor mental health and binge drinking but negative effect on physical activity. However,

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for those likely to be renters, I document a positive impact of house prices on body mass index but negative effects on physical health and smoking. The impacts of local house prices on drinking and smoking are consistent with a pure wealth effect.

I also find substantial heterogeneity of the health effects across gender. Specifically for male homeowners, local house prices have a positive and significant impact on binge drinking and smoking, but a negative and significant impact on physical activity. Among female homeowners, local house prices have a positive and significant impact on poor mental health but a negative and significant impact on physical activity. Moreover, local house prices have a positive (negative) impact on binge drinking (physical health) for male renters but a negative (positive) impact on smoking (body mass index) for female renters.

Additionally, I show that local house prices have an asymmetric effect on health. In particular, local house prices have relatively large and significant impacts on binge drinking and physical activity during the housing bust period. The asymmetric effect is, however, absent for weight-related health, physical health, and poor mental health. I also find that health effects are different for the boom and the bust when the subgroup analysis is conducted by gender.

Overall, the findings suggest the relationship between local house prices and individual's health and health behaviors. The results also imply that health effects depend on which aspects of economic upturns or downturns are being considered (Ruhm, 2005; Fichera and Gathergood, 2013; Charles and DeCicca, 2008; Tekin, McClellan, and Minyard, 2015).

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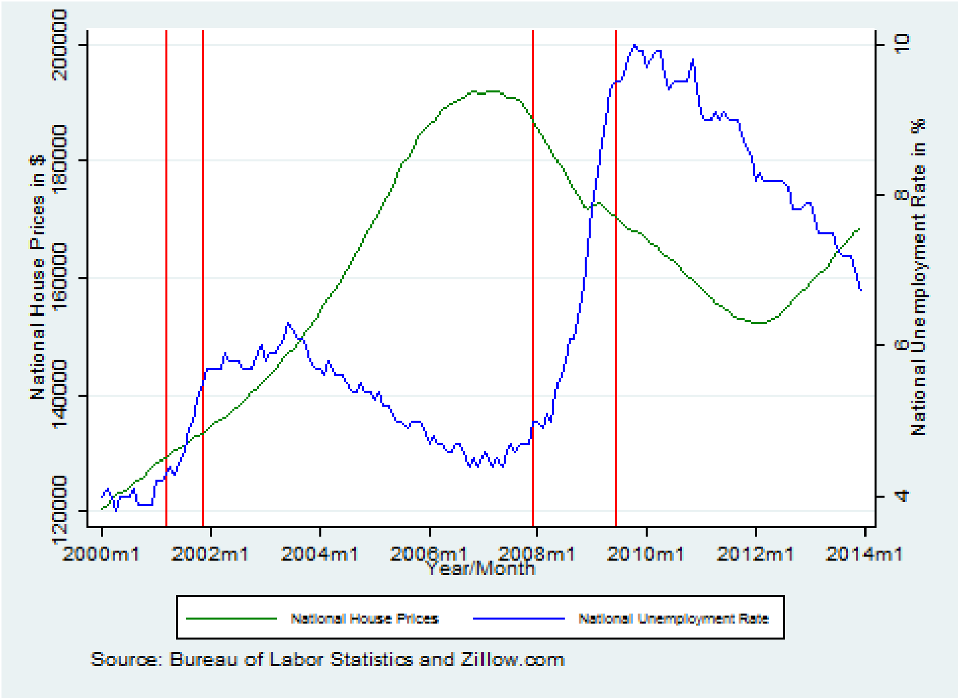

Figure 2.1a: **National measure of monthly house price and unemployment rate**

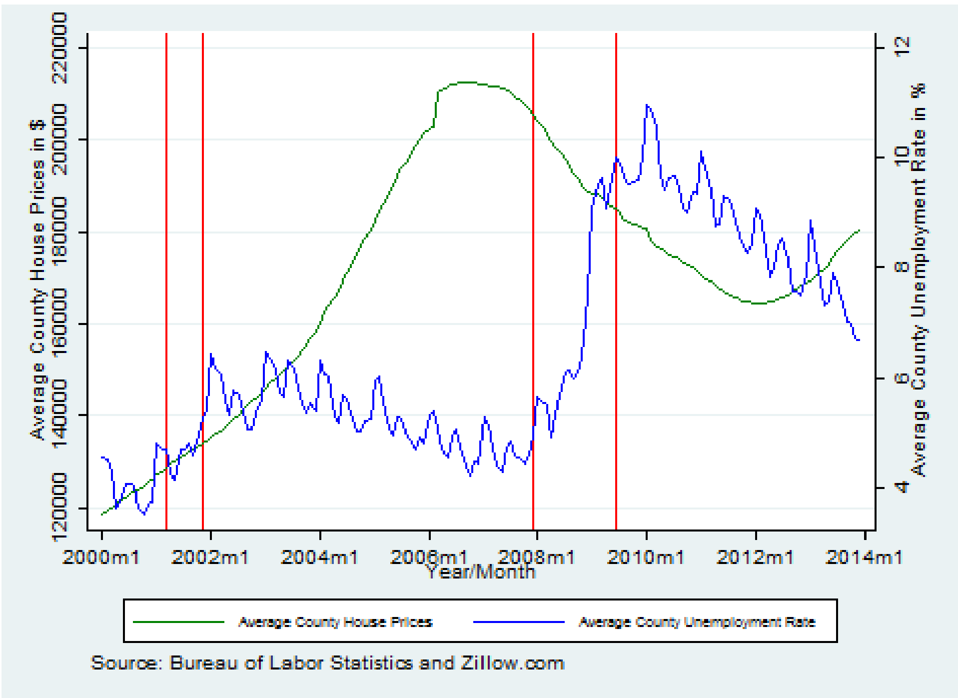

Figure 2.1b: **County measure of monthly house price and unemployment rate**

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Table 2.1: **Descriptive Statistics**

	Observations	**Mean**	**Std. Dev.**
Health Outcome Variables			
BMI	1534800	3.293	0.204
Physical Health	1666579	3.639	1.057
Poor Mental Health	1598792	3.877	7.977
Binge Drinking	880315	1.081	3.390
Current Smoker	755782	0.457	0.498
Daily Smoker	755782	0.337	0.473
Physical Activity	1662951	0.788	0.409
Housing Market Variable			
County House Price (in '000)	1671016	252.228	287.645
Control Variables			
County Unemployment Rate	1671016	6.499	2.704
Stock Market Index	1671016	9.296	0.147
High School	1671016	0.313	0.464
Some College	1671016	0.278	0.448
College	1671016	0.409	0.492
Age	1671016	45.054	12.002
Married	1671016	0.575	0.494
Divorced	1671016	0.152	0.359
Widowed	1671016	0.033	0.179
Other Marital Status	1671016	0.240	0.427
Mean Income (in '000)	1671016	68.706	26.950
White	1671016	0.754	0.431
African American	1671016	0.096	0.294
Hispanic	1671016	0.084	0.278
Other Race	1671016	0.067	0.250
Male	1671016	0.405	0.491

Notes: Descriptive statistics for the full sample period (January 2001-December 2012). See Appendix Table 2.1 for definitions of variables.

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Table 2.2: **Estimated effect of local house prices on health and health behaviors**

	All	Boom	Bust
Panel A: Estimated effect of local house prices on weight-related health			
Log of BMI	(N = 1534800)	(N = 547855)	(N = 986945)
Log of County House Price × High School	0.00686* (0.0041)	0.00717 (0.0052)	0.00424 (0.0061)
Log of County House Price × Some College	0.00144 (0.004)	0.0064 (0.0044)	-0.00383 (0.0057)
Log of County House Price × College	-0.00319 (0.0041)	-0.00211 (0.0044)	-0.00665 (0.0055)
County Unemployment Rate	0.00012 (0.0003)	0.0027*** (0.0007)	0.00037 (0.0005)
Log of Dow Jones Industrial Average	-0.00106 (0.003)	-0.00843 (0.0094)	0.00029 (0.0041)
Panel B: Estimated effect of local house prices on physical and mental health			
Physical Health	(N = 1666579)	(N = 645746)	(N = 1020833)
Log of County House Price × High School	-0.04068** (0.0153)	-0.05601* (0.0306)	0.02213 (0.0315)
Log of County House Price × Some College	0.0061 (0.0141)	-0.02183 (0.0298)	0.07517*** (0.0274)
Log of County House Price × College	-0.01515 (0.0144)	-0.03667 (0.0282)	0.05029 (0.0317)
County Unemployment Rate	0.00058 (0.002)	-0.00144 (0.0044)	0.00208 (0.0025)
Log of Dow Jones Industrial Average	0.01431 (0.0171)	-0.09167** (0.0407)	0.05828*** (0.0204)
Poor Mental Health	(N = 1598792)	(N = 585140)	(N = 1013652)
Log of County House Price × High School	-0.00574 (0.1348)	0.07764 (0.2554)	-0.08417 (0.1707)
Log of County House Price× Some College	0.17779 (0.1475)	0.24694 (0.2717)	0.08985 (0.1901)
Log of County House Price× College	0.30194** (0.1295)	0.26978 (0.2435)	0.26047 (0.1863)
County Unemployment Rate	0.00546 (0.0178)	-0.00117 (0.0171)	0.01566 (0.0166)
Log of Dow Jones Industrial Average	-0.30703* (0.1739)	-0.1554 (0.4208)	-0.36661* (0.1912)
Panel C: Estimated effect of local house prices on health behaviors			
Binge Drinking	(N = 880315)	(N = 303561)	(N = 576754)
Log of County House Price× High School	0.02244 (0.054)	-0.0265 (0.1591)	0.36966** (0.1676)
Log of County House Price× Some College	0.131** (0.0641)	0.17369 (0.162)	0.42948** (0.1748)
Log of County House Price× College	0.16483** (0.0713)	0.23707 (0.1716)	0.45239*** (0.1637)
County Unemployment Rate	-0.0085 (0.0064)	-0.01227 (0.0162)	-0.01003 (0.0106)
Log of Dow Jones Industrial Average	-0.12162 (0.1088)	0.07063 (0.1275)	-0.19358 (0.1486)
Current Smoker	(N = 755782)	(N = 302774)	(N = 453008)
Log of County House Price× High School	0.00022 (0.0079)	0.01871 (0.0234)	0.00144 (0.0216)
Log of County House Price× Some College	0.00885 (0.0099)	0.02042 (0.025)	0.01333 (0.0226)
Log of County House Price× College	0.01607 (0.011)	0.02941 (0.0225)	0.01819 (0.0226)
County Unemployment Rate	-0.0019 (0.0015)	-0.00462 (0.0029)	-0.00229* (0.0013)
Log of Dow Jones Industrial Average	-0.02818* (0.0144)	-0.00255 (0.0252)	-0.03592** (0.0177)
Daily Smoker	(N = 755782)	(N = 302774)	(N = 453008)
Log of County House Price× High School	-0.01218** (0.0054)	0.00158 (0.0151)	-0.00695 (0.0158)
Log of County House Price× Some College	0.00415 (0.0064)	0.0114 (0.0155)	0.01482 (0.0152)
Log of County House Price× College	0.01537 (0.0095)	0.02759 (0.0176)	0.02321 (0.0161)
County Unemployment Rate	-0.00162 (0.0012)	-0.00211 (0.0026)	-0.00172 (0.0017)
Log of Dow Jones Industrial Average	-0.03141*** (0.0112)	-0.00502 (0.0246)	-0.03743** (0.0176)
Activity	(N = 1662951)	(N = 646592)	(N = 1016359)
Log of County House Price× High School	-0.01349 (0.0147)	0.02437 (0.0189)	-0.03689*** (0.0104)
Log of County House Price× Some College	-0.01756 (0.013)	0.007 (0.0171)	-0.03592*** (0.0112)
Log of County House Price× College	-0.02724** (0.0118)	0.00486 (0.0168)	-0.05031*** (0.0116)
County Unemployment Rate	0.00447*** (0.0014)	0.00332* (0.0019)	0.00241** (0.001)
Log of Dow Jones Industrial Average	0.01149 (0.0101)	-0.01197 (0.0154)	0.01183 (0.0128)
Month Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes

Notes: All estimates are from linear models and uses the sample weights provided by the BRFSS. Columns 2, 3, and 4 present the estimates for the full sample period, housing boom period, and housing bust period, respectively. Panels A, B, and C display the estimates for the weight-related health (BMI(log of body mass index), physical and mental health, and health behaviors (binge drinking, current smoker, daily smoker, and activity), respectively. Models include controls for education (omitted category is high school or less than high school), county unemployment rate, log of Dow Jones Industrial Average, marital status (omitted category is single/separated), group average income, race (omitted category is white), and male. Standard errors clustered at the county and month level are in parenthesis. Number of observation (N) varies across the health outcomes. Superscripted notations next to the coefficients indicate the level of statistical significance from a two-tailed t-test. ***,**,*denote significance at 1 % , 5 %, 10 % level, respectively. See Appendix Table B.2.1 for definitions of variables.

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Table 2.3: **Estimated effect of local house prices on health and health behaviors for male**

	All	Boom	Bust
Panel A: Estimated effect of local house prices on weight-related health			
Log of BMI	(N = 635976)	(N = 227907)	(N = 408069)
Log of County House Price × High School	0.00218 (0.0049)	0.00699 (0.0079)	0.00535 (0.0066)
Log of County House Price × Some College	-0.00132 (0.005)	0.00993 (0.0077)	-0.00207 (0.0068)
Log of County House Price × College	-0.00598 (0.0054)	-0.00242 (0.0079)	-0.00307 (0.007)
County Unemployment Rate	0.04848** (0.0181)	-0.03205 (0.0372)	0.09705*** (0.0255)
Log of Dow Jones Industrial Average	0.00217*** (0)	0.00209*** (0.0001)	0.00221*** (0.0001)
Panel B: Estimated effect of local house prices on physical and mental health			
Physical Health	(N = 674571)	(N = 263811)	(N = 410760)
Log of County House Price × High School	-0.05581*** (0.015)	-0.09897** (0.0393)	0.03925 (0.0397)
Log of County House Price × Some College	-0.00925 (0.0149)	-0.07502** (0.0301)	0.09793*** (0.0327)
Log of County House Price × College	-0.02148 (0.0148)	-0.08074** (0.0328)	0.08186** (0.0372)
County Unemployment Rate	-0.37442** (0.143)	-0.08483 (0.217)	-0.53107*** (0.1876)
Log of Dow Jones Industrial Average	-0.01859*** (0.0003)	-0.01778*** (0.0004)	-0.01919*** (0.0004)
Poor Mental Health	(N = 646228)	(N = 238240)	(N = 407988)
Log of County House Price × High School	0.04404 (0.1612)	0.19114 (0.4274)	0.08726 (0.2244)
Log of County House Price × Some College	-0.02435 (0.1916)	0.17974 (0.4652)	-0.0247 (0.2031)
Log of County House Price × College	0.14139 (0.1825)	0.21958 (0.4532)	0.19346 (0.2172)
County Unemployment Rate	0.54939 (0.7855)	-0.19024 (2.1672)	1.1087 (1.0862)
Log of Dow Jones Industrial Average	0.00917*** (0.0025)	0.00244 (0.0041)	0.01339*** (0.002)
Panel C: Estimated effect of local house prices on health behaviors			
Binge Drinking	(N = 397489)	(N = 139425)	(N = 258064)
Log of County House Price × High School	0.13856** (0.0681)	0.14156 (0.2621)	0.56706** (0.2372)
Log of County House Price × Some College	0.26863*** (0.0763)	0.42086 (0.275)	0.622** (0.2428)
Log of County House Price × College	0.30311** (0.1135)	0.48124 (0.294)	0.64623** (0.252)
County Unemployment Rate	-2.16674*** (0.6959)	-4.01843*** (1.4614)	-1.23945 (0.7807)
Log of Dow Jones Industrial Average	-0.02485*** (0.0015)	-0.0307*** (0.0022)	-0.02097*** (0.0018)
Current Smoker	(N = 333100)	(N = 134132)	(N = 198968)
Log of County House Price × High School	0.00946 (0.0144)	0.02834 (0.0366)	0.03241 (0.0281)
Log of County House Price × Some College	0.02347 (0.0159)	0.03572 (0.0428)	0.04949 (0.0303)
Log of County House Price × College	0.02949** (0.0144)	0.04057 (0.0357)	0.05475* (0.029)
County Unemployment Rate	-0.23666** (0.0884)	-0.15141 (0.1526)	-0.27634** (0.1099)
Log of Dow Jones Industrial Average	-0.00763*** (0.0001)	-0.00853*** (0.0002)	-0.00694*** (0.0002)
Daily Smoker	(N = 333100)	(N = 134132)	(N = 198968)
Log of County House Price × High School	-0.00284 (0.0101)	0.00942 (0.0235)	0.02207 (0.0275)
Log of County House Price × Some College	0.01444 (0.01)	0.01942 (0.0263)	0.04513 (0.0284)
Log of County House Price × College	0.02964** (0.0118)	0.04194* (0.0232)	0.05607** (0.0272)
County Unemployment Rate	-0.29795*** (0.0953)	-0.20991 (0.1641)	-0.36621** (0.164)
Log of Dow Jones Industrial Average	-0.00336*** (0.0002)	-0.0039*** (0.0003)	-0.00294*** (0.0002)
Activity	(N = 672892)	(N = 264161)	(N = 408731)
Log of County House Price × High School	-0.01001 (0.0166)	0.04856* (0.0265)	-0.0323* (0.0192)
Log of County House Price × Some College	-0.01631 (0.0165)	0.0237 (0.0259)	-0.02999 (0.0208)
Log of County House Price × College	-0.02575* (0.0139)	0.02765 (0.0246)	-0.04819** (0.0196)
County Unemployment Rate	0.16144*** (0.0466)	0.39898*** (0.1153)	0.04963 (0.0748)
Log of Dow Jones Industrial Average	-0.0039*** (0.0001)	-0.00383*** (0.0002)	-0.00399*** (0.0002)
Month Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes

Notes: All estimates are from linear models and uses the sample weights provided by the BRFSS. Columns 2, 3, and 4 present the estimates for the male sub-sample of full sample period, housing boom period, and housing bust period, respectively. See notes of Table 2.2 for others.

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Table 2.4: **Estimated effect of local house prices on health and health behaviors for female**

Panel A: Estimated effect of local house prices on weight-related health			
Log of BMI	(N = 898824)	(N = 319948)	(N = 578876)
Log of County House Price × High School	0.01018** (0.0048)	0.00907 (0.0105)	0.0023 (0.0076)
Log of County House Price × Some College	0.00444 (0.0046)	0.00404 (0.0086)	-0.00408 (0.0072)
Log of County House Price × College	-0.00105 (0.0052)	-0.00128 (0.01)	-0.00934 (0.0085)
County Unemployment Rate	0.06227*** (0.0138)	0.05286 (0.0391)	0.07072** (0.0286)
Log of Dow Jones Industrial Average	0.00341*** (0)	0.00356*** (0.0001)	0.00331*** (0.0001)
Panel B: Estimated effect of local house prices on physical and mental health			
Physical Health	(N = 992008)	(N = 381935)	(N = 610073)
Log of County House Price × High School	-0.02234 (0.0194)	-0.0138 (0.0409)	0.00505 (0.035)
Log of County House Price × Some College	0.02303 (0.0165)	0.0295 (0.0377)	0.05061 (0.0358)
Log of County House Price × College	-0.00595 (0.0168)	0.00788 (0.0396)	0.01807 (0.0352)
County Unemployment Rate	-0.34576** (0.1651)	-0.32143 (0.2101)	-0.34738** (0.1545)
Log of Dow Jones Industrial Average	-0.01609*** (0.0004)	-0.01559*** (0.0004)	-0.01647*** (0.0005)
Poor Mental Health	(N = 952564)	(N = 346900)	(N = 605664)
Log of County House Price × High School	-0.07424 (0.1906)	-0.03564 (0.3317)	-0.29621 (0.3484)
Log of County House Price × Some College	0.38658* (0.2267)	0.33996 (0.331)	0.20075 (0.4017)
Log of County House Price × College	0.46056** (0.1851)	0.33762 (0.3085)	0.31046 (0.3793)
County Unemployment Rate	-6.04041*** (1.154)	-5.03292*** (1.6033)	-6.4563*** (1.2558)
Log of Dow Jones Industrial Average	0.00056 (0.002)	-0.00956** (0.0039)	0.00755*** (0.0024)
Panel C: Estimated effect of local house prices on health behaviors			
Binge Drinking	(N = 482826)	(N = 164136)	(N = 318690)
Log of County House Price × High School	-0.06986 (0.069)	-0.16405 (0.1621)	0.17223 (0.1352)
Log of County House Price × Some College	-0.02376 (0.0612)	-0.11338 (0.1324)	0.20605* (0.1042)
Log of County House Price × College	0.00889 (0.0513)	-0.04947 (0.1391)	0.22538** (0.1022)
County Unemployment Rate	-0.75503 (0.5218)	-0.77728 (0.7768)	-0.62969 (0.7716)
Log of Dow Jones Industrial Average	-0.0148*** (0.0003)	-0.01494*** (0.0009)	-0.0147*** (0.0007)
Current Smoker	(N = 422682)	(N = 168642)	(N = 254040)
Log of County House Price × High School	-0.01307 (0.0099)	0.00508 (0.0164)	-0.03884** (0.0185)
Log of County House Price × Some College	-0.00549 (0.0145)	0.0072 (0.0178)	-0.02792 (0.021)
Log of County House Price × College	0.00059 (0.0123)	0.01816 (0.0126)	-0.02611 (0.0236)
County Unemployment Rate	-0.17368* (0.0883)	-0.10421 (0.1204)	-0.21642* (0.1113)
Log of Dow Jones Industrial Average	-0.0067*** (0.0001)	-0.00709*** (0.0003)	-0.00638*** (0.0002)
Daily Smoker	(N = 422682)	(N = 168642)	(N = 254040)
Log of County House Price × High School	-0.0245** (0.0091)	-0.00317 (0.0129)	-0.04536*** (0.0159)
Log of County House Price × Some College	-0.00609 (0.0128)	0.01071 (0.0082)	-0.02139 (0.0191)
Log of County House Price × College	-0.00181 (0.0109)	0.01665 (0.0163)	-0.01845 (0.0198)
County Unemployment Rate	-0.32488*** (0.1015)	-0.27848* (0.1376)	-0.38432*** (0.1116)
Log of Dow Jones Industrial Average	-0.00384*** (0.0002)	-0.004*** (0.0002)	-0.00369*** (0.0002)
Activity	(N = 990059)	(N = 382431)	(N = 607628)
Log of County House Price × High School	-0.01606 (0.0143)	-0.00079 (0.0189)	-0.03864*** (0.0103)
Log of County House Price × Some College	-0.0177 (0.0117)	-0.00971 (0.0149)	-0.03956*** (0.0099)
Log of County House Price × College	-0.02746** (0.0112)	-0.01794 (0.0167)	-0.04996*** (0.0115)
County Unemployment Rate	0.11333 (0.0734)	0.21285 (0.135)	0.09646 (0.0814)
Log of Dow Jones Industrial Average	-0.0027*** (0.0001)	-0.00251*** (0.0002)	-0.00284*** (0.0001)
Month Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes

Notes: All estimates are from linear models and uses the sample weights provided by the BRFSS. Columns 2, 3, and 4 present the estimates for the female sub-sample of full sample period, housing boom period, and housing bust period, respectively. See notes of Table 2.2 for others.

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CHAPTER 3 LONG-TERM IMPACT OF MEDICAID EXPANSIONS ON FINANCIAL OUTCOMES

3.1 Introduction

The fundamental goal of Medicaid expansions is to increase the health insurance coverage of the poor. A large literature examines the effect of these expansions on health outcomes (Buchmueller, Ham, and Shore-Sheppard, 2015). However, the question of how these expansions impact financial outcomes has received little attention in the literature. Recent studies suggest that such impacts may be important. For example, Gross and Notowidigdo (2011) focus on Medicaid expansions from 1992 to 2004 and find that the expansions reduce personal bankruptcies. Finkelstein et al. (2012) focus on Medicaid expansion in Oregon and show that the expansion decreases medical debt. Similarly, Mazumder and Miller (2014) find the evidence of improvement in credit scores and reductions in delinquencies, personal bankruptcy in the last 24 months, and fraction of debt past due, in response to the 2006 Massachusetts health care reform.

One limitation of these studies is that they focus only on short time impact. In this paper, I combine the March Current Population Survey (CPS) with the American Community Survey (ACS) to examine the long-term impact of the Medicaid expansions that took place in the 1980s and 1990s on three financial outcomes - homeownership rate, mortgage ownership rate, and financial market participation rate, that have not been investigated previously. Specifically, I use the 1980-2008 CPS to construct the average of actual Medicaid eligibility over all ages from 0 to 17 by state of birth, birth cohort, and race, for the birth cohorts 1980-1991. Next, I use the 2005-2013 ACS to construct financial outcomes, by state of birth, birth cohort, race, age, and calendar years for these birth cohorts when they are 22-30 years old. I then match these financial outcomes in

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adulthood with average childhood Medicaid eligibility using state of birth, birth cohort, and race. The main concern with the use of actual eligibility is that it depends on states demographic and economic characteristics. To address this concern, I follow Currie and Gruber (1996) and Cohodes, Grossman, Kleiner, and Lovenheim (2014), and use fixed simulated Medicaid eligibility as an instrument for actual eligibility. To construct this instrument, I select a fixed (20 %) nationally representative sample from the 1986 CPS and then assign that same sample to each state. I then use this fixed national sample to calculate the average of child's Medicaid eligibility over all ages from 0 to 17 by state of birth, birth cohort, and race, for the birth cohorts 1980-1991. Since the sample is national, the fixed simulated Medicaid eligibility depends only on states eligibility rules and is independent of other characteristics.

The results suggest that Medicaid eligibility during childhood increases homeownership rate, mortgage ownership rate, and financial market participation rate later in life. In particular, a one-percentage-point increase in eligibility is associated with a 0.0071 percentage point increase in the homeownership rate, a 0.0088 percentage point increase in the mortgage ownership rate, and a 0.0024 percentage point increase in the financial market participation rate. These imply a 2.02 % increase in homeownership rate, a 2.90 % increase in mortgage ownership rate, and a 2.89 % increase in financial market participation rate, with respect to their mean values. I also show that these results are not driven by endogenous state Medicaid eligibility expansions. Exploiting only federal variation, that is arguably exogenous, yields results similar to the main results that use state variation as well. In addition, main results are robust to the inclusion of state-year and age-year fixed effects in the model. I also provide the empirical evidence of the heterogeneity in the effect of Medicaid eligibility across races (whites and nonwhites).

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This paper is related to two strands of literature. By analyzing the long term impact of Medicaid expansions on various outcomes, this paper is closely related to recent studies by Cohodes, Grossman, Kleiner, and Lovenheim (2014), D. W. Brown, Kowalski, and Lurie (2015), and Miller and Wherry (2015). These studies examine educational outcomes, tax outcomes, and health outcomes, but they do not analyze homeownership, mortgage ownership, and financial market participation rates. By studying the homeownership rate, this paper complements research by Engelhardt (2008). A key difference is that this paper is concerned with the impact of public health insurance expansion on the young adult homeownership rate, whereas Engelhardt (2008) focuses on the impact of social security program on elderly homeownership.

The rest of the paper proceeds as follows. Section 3.2 presents background on Medicaid expansions. Section 3.3 reviews the relevant literature, and Section 3.4 provides an overview of the data. Section 3.5 discusses the identification strategy. Section 3.6 presents the empirical results. Section 3.7 concludes.

3.2 Background on Medicaid Expansions

Medicaid is the largest health insurance program for low-income individuals that began in 1965. Medicaid for non disabled children was traditionally linked to the Aid to Families with Dependent Children (AFDC). Beginning in the mid-1980s, it expanded, first by relaxing some of the family structure requirements (under Deficit Reduction Act of 1984) and then by income requirement: starting in 1986, income requirement was linked to the federal poverty line rather than to the AFDC limits, and thus, coverage was extended to families with income above the AFDC limits (Buchmueller, Ham, and Shore-Sheppard, 2015). These expansions are described in Appendix Table C.1.2. Because of these expansions, by the mid-1990s, most children below the federal poverty line, and

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all young children below 133% of the federal poverty line, and in certain states, their parents, were eligible for Medicaid (Gruber and Simon, 2008; Gross and Notowidigdo, 2011). In 1997, Congress augmented Medicaid program for the children with the State Children's Health Insurance Program (SCHIP). Under the SCHIP, a majority of the states provided health care coverage to low-income children whose family income level makes them ineligible for Medicaid but lack private health insurance. For example, under the SCHIP, children in New York were eligible up to 400% of the federal poverty line. Because states expanded Medicaid eligibility at different times, and with different levels of generosity, these Medicaid expansions resulted in a substantial variation in health insurance eligibility limit by age within states, and also the variation in magnitude of within state variation across states (Gruber and Simon, 2008; Buchmueller, Ham, and Shore-Sheppard, 2015).

3.3 Previous Literature

A large number of studies have examined the impacts of Medicaid expansion on various outcomes, e.g., health insurance coverage, health care utilization, health status, financial well-being, labor supply, education, and others (Buchmueller, Ham, and Shore-Sheppard, 2015).[1] Most of these studies, however, focus on the short-term outcome by examining the impact of Medicaid exposure on contemporaneous outcomes or outcomes after a few years (Wherry, Miller, Kaestner, and Meyer, 2015). For example, Gruber and Simon (2008) exploit cross-state variation in Medicaid expansions from 1992 to 2004 and show that the expansions reduce personal bankruptcies. Finkelstein et al. (2012) use randomized control experiment in Oregon and document that Medicaid expansion to low-income and uninsured adult population increase health care utilization, decrease out-of-pocket

[1]Buchmueller, Ham, and Shore-Sheppard (2015) provide a comprehensive review of these.

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medical expenditures and medical debt (including fewer bills sent to collection), and improve self-reported physical and mental health. In a related study, Mazumder and Miller (2014) examine the impact of 2006 Massachusetts health care reform on a broad set of financial outcomes for those who were uninsured before the reforms.[2] They find the improvement in credit scores and reductions in delinquencies, personal bankruptcy in the last 24 months, and fraction of debt past due, in response to the health reform.

Recent studies, however, begin to focus on the long-term outcome by examining the impact of Medicaid exposure when young on later life outcomes. For examples, Cohodes, Grossman, Kleiner, and Lovenheim (2014) find that Medicaid exposure when young decreases high school dropout and increases college attendance and college completion. Similarly, Miller and Wherry (2015) provide evidence that Medicaid eligibility in childhood results in fewer hospitalizations and emergency department visits in adulthood (age 25) for blacks. Moreover, D. W. Brown, Kowalski, and Lurie (2015) show that children exposed to Medicaid pay more in cumulative taxes by age 28, collect less money from the government in the form of Earned Income Tax Credit, and experience decreases in mortality and increases in college attendance. One advantage associated with these studies is that they potentially address concern of legislative endogeneity relative to studies that focus on contemporaneous outcomes (D. W. Brown, Kowalski, and Lurie, 2015).

Despite these studies, the evidence of Medicaid eligibility on long-term financial outcomes remains limited. Gruber and Simon (2008), Finkelstein et al. (2012), and Mazumder and Miller (2014) focus only on short-term financial outcomes. Using the census data, I focus on following long-term financial outcomes that have not yet been examined in the literature: home ownership, mortgage ownership, and financial market

[2]Mazumder and Miller (2014) focus on entire population of uninsured residents. In contrast, Finkelstein et al. (2012) focus on only low-income and uninsured adult population, while Gross and Notowidigdo (2011) focus on low-income pregnant and children.

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participation.

3.4 Data

This paper uses two datasets: the March Current Population Survey (CPS) and the American Community Survey (ACS). I use the CPS to construct Medicaid eligibility, the main explanatory variable of interest, and the ACS to construct financial outcome variables: home ownership rate, mortgage ownership rate, and financial market participation rate.

3.4.1 March Current Population Survey

I use the March Current Population Survey to construct Medicaid eligibility for the birth cohorts 1980-1991 that are between the ages of 0-17. [3] In particular, I follow Currie and Gruber (1996), Gruber and Simon (2008), and Cohodes, Grossman, Kleiner, and Lovenheim (2014) to construct two measures of Medicaid eligibility: actual eligibility and fixed simulated eligibility. Actual eligibility is defined as the fraction of children of age a and race r that are eligible for Medicaid in state s and calendar year t based on family income and demographics (marital status, gender of household head, number of children, ages of children).[4]For example, I calculate the fraction of nonwhite aged 1 that are eligible in Michigan in year 1980, in Michigan in year 1981, and so on for every state and every year between 1980 and 2008.

The major concern with actual eligibility is that it depends on state's economic and demographic characteristics. For example, if the state had the highest fraction eligible of any state, it might reflect both the generosity of the state's program and the relative

[3]I select 1991 as the youngest cohort because data on outcomes (see section 3.4.2) are available for this cohort at age 22. Similar to Cohodes, Grossman, Kleiner, and Lovenheim (2014), I select 1980 as the oldest cohort since Medicaid eligibility was very low before 1980.

[4]I define group cells by race (nonwhite and white) , in addition to state, year, and age, because nonwhite children are more likely to affected by Medicaid expansions. See section 3.5.

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poverty of its residents. To address this concern, I follow Currie and Gruber (1996) and use fixed simulated eligibility as an instrument for actual eligibility. This instrument depends only on the state's legislative rules and is independent of its demographic characteristics. To construct this instrument, I use a 20% national sample from the 1986 CPS and then assign that same sample to each state . I then use this fixed national sample to calculate the fraction of children of age a and race r that would be eligible for Medicaid in state s and calendar year t based on family income and demographics.[5]. For example, I take a nationally representative sample of nonwhite aged 1 and calculate the proportion that would be eligible for Medicaid if they lived in Michigan in year 1980 (with inflation adjustments [6]), in Michigan in year 1981 (with inflation adjustments), and so on for every state and every year between 1980 and 2008. The use of fixed national sample assures that the fraction eligible in this sample depends only on state eligibility rules, and is independent of other characteristics of states.[7]As described by Currie and Gruber (1994, 1996), this instrument provides a convenient parameterization of the generosity of Medicaid laws for each race-age-state-year cell. Next, following Cohodes, Grossman, Kleiner, and Lovenheim (2014)[8], I calculate mean of child's actual eligibility and fixed simulated eligibility over all ages from 0 to 17 by state-of-birth, birth cohort, and race as below:

$$\text{ActualElig}_{scr} = \frac{1}{18}\sum_{i=0}^{17}\overline{ActualElig}_{scri} \tag{3.1}$$

$$\text{SimulatedElig}_{scr} = \frac{1}{18}\sum_{i=0}^{17}\overline{SimulatedElig}_{scri} \tag{3.2}$$

[5]This national sample consists of 1,592,373 (31223 times 51 states) observations and remains the same for years 1980-2008. The income variable is deflated (inflated) to years 1980-2008 (from the year 1986) for which eligibility is calculated using Consumer Price Index for All Urban Consumers (CPI-U) from the Bureau of Labor Statistics (BLS).

[6]Inflation adjustment is needed because I am using the 1986 CPS sample. For example, when computing the eligibility for 1980, income variable is deflated, but when calculating the eligibility for 1987, income variable is inflated. I use CPI-U from the BLS for the purpose of inflation adjustment.

[7]See Currie and Gruber (1994) for details.

[8]I thank Michael F. Lovenheim for helping me understand this measure.

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Here, s is state, c is birth cohort , r is race, t is calendar year, and i is age.$\overline{ActualElig}_{scri}$ is the average actual eligibility, and $\overline{ActualElig}_{scri}$ is the average fixed simulated eligibility for each state-birth year-race-age cell. ActualElig_{scr} is the main explanatory variable of interest and $\text{SimulatedElig}_{scr}$ is the instrument for the main explanatory variable.[9]

Figure 3.1 shows the value of the instrument, $\text{SimulatedElig}_{scr}$, by state. Panel A presents the value for the oldest cohort (1980 birth cohort) and panel B presents the value for the youngest cohort (1991 birth cohort). As demonstrated in the panel A, fixed simulated Medicaid eligibility varies considerably across states within the 1980 birth cohort, with Alabama having a value of 0.1125 (or 11.25% [10]) and Hawaii having a value of 0.2125 (21.25%). The panel B also demonstrates the substantial variation across states within the 1991 birth cohort, with Vermont having a value of 0.4227 (42.27%) and California having a value of 0.6716 (67.16%).

3.4.2 American Community Survey

I use the 2005-2013 American Community Survey to construct three financial outcomes: home ownership rate, mortgage ownership rate, and financial market participation rate by state of birth (s)[11], birth cohort (c), race (r), and survey year (t), for the birth cohorts 1980-1991 that are between the ages of 22-30 [12] in 2005- 2013.[13]

[9]A simulated eligibility of 0.30 (or 30%) for the Michigan-1980 birth cohort-nonwhite group indicates that the national sample of nonwhites born in 1980 in Michigan would be eligible for Medicaid for 30% of childhood years (birth to age 17), or, for approximately 5.1 years, under Michigan's Medicaid rules from 1980 to 1997.

[10]Higher value indicates relatively more generous Medicaid laws. For example, a simulated value of 40% means that the Medicaid rules of the state is two times more generous than the rules of another state with the value of 20%.

[11]I use state of birth as it is unrelated to Medicaid rules (Cohodes, Grossman, Kleiner, and Lovenheim, 2014).

[12]I select 30 as the oldest age because that is the last age for which I have the data on all four cohorts (at age 33 I observe only individuals born in 1980, at age 32 I observe individuals born in 1980 and 1981, at age 31 I observe individuals born in 1980, 1981, and 1982, and at age 30 I observe individuals born in 1980,1981,1982, and 1983. See Appendix Table C.1.1 for details.

[13]Similar to Wherry and Meyer (2012), I include only those observations of children born in the U.S. with residence in the 50 U.S. states and the District of Columbia.

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To construct homeownership rate and mortgage ownership rate, I use the information on the family's homeownership status and mortgage status [14] in the ACS. Using this information, I define homeownership variable as an indicator for whether an individual owns the housing unit and mortgage ownership variable as an indicator for whether an individual owns the housing unit with a mortgage or loan.

As to the financial market participation variable, I follow the approach of Cole, Paulson, and Shastry (2014) and use the information on investment income in the ACS. In the ACS, investment income is defined as the "income from interest, dividends, net rental income, royalty income, or income from estates and trusts received during the previous year". Despite limited information on financial wealth in the ACS, Cole, Paulson, and Shastry (2014) show that investment income in the ACS serves as a good proxy for broader financial market participation.

Similar to Cole, Paulson, and Shastry (2014), I define the financial market participation variable as an indicator for whether an individual reports a nonzero investment income (positive or negative). [15] I then compute means for these three financial outcomes by state of birth, birth cohort, race, and ACS survey year, using census weights. This gives homeownership rate, mortgage ownership rate, and financial market participation rate by state of birth, birth cohort, race, and survey year. I also compute mean state-birth year-race-survey year values for following variables: male, married using census weights.[16]Next, I match them onto the main explanatory variable and instrument (see equations 3.1 and 3.2) by state of birth, birth cohort, and race.

[14]I restrict the sample to noninstitutional, civilian household heads not living in group quarters. In addition, all of the socio-demographic characteristic variables are those of the household head.
[15]Investment income can be negative or positive. Also, households are instructed to report even small amounts credited to an account Cole, Paulson, and Shastry (2014).
[16]I use household weight for all financial outcomes.

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3.5 Empirical Model

To examine the impact of increased Medicaid eligibility on financial outcomes, I follow Cohodes, Grossman, Kleiner, and Lovenheim (2014) and estimate regression model of the following form:

$$y_{scart} = \alpha + \beta \text{ActualElig}_{scrt} + \lambda X_{scart} + \gamma_s + \eta_t + \theta_a + \epsilon_{scart} \tag{3.3}$$

Here the unit of observation is a state-of-birth (s)/birth-year (c)/age (a)/race (r)/survey year (t). The dependent variable y is one of three financial outcome measures (home-ownership rate, mortgage ownership rate, and financial market participation rate) . The key explanatory variable ActualElig is the average actual eligibility (see equation 3.1), and X is a vector of controls, including percent male, percent married , and an indicator for whether the observation is for the nonwhite sample or not. I include state of birth, age, and survey year fixed effects and cluster standard errors at the state-of-birth level. The state fixed effects account for determinants of financial outcomes that differ across states but are time-invariant (such as housing supply elasticity). The age fixed effects account for the possibility that younger individuals may be less likely to own home or own mortgage or participate in the financial market and the year fixed effects controls for those that vary uniformly across states over time (such as innovations in mortgage finance). Variable ϵ represents unobserved shocks.

The parameter of interest is β, which tells us the impact of average Medicaid eligibility during childhood from birth to age 17 on the financial outcomes at adulthood. This impact is identified using variation in actual Medicaid eligibility within state across birth cohorts over time (Cohodes, Grossman, Kleiner, and Lovenheim, 2014). This variation comes from differences in state's Medicaid eligibility rules as well as differences in its demographic and economic characteristics.

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The OLS estimate from equation 3.3 will likely to be biased toward demonstrating adverse impacts of Medicaid on outcomes because unobservable factors affecting eligibility are likely to be correlated with unobservable factors affecting financial outcomes. In addition, it is not possible to control all demographic and economic factors that determine both eligibility and financial outcomes. The inclusion of state and year fixed effects cannot address this concern if these factors are not constant within a state or across states within a year. To overcome this problem, I follow Currie and Gruber (1996) and use fixed simulated eligibility as an instrument for actual eligibility. As described in the previous section, this instrument is constructed using a fixed national random sample and so it isolates variation based on state's Medicaid eligibility rules that is purged of variation in its demographic and economic characteristics. Specifically, I estimate the first stage equation of the following form:

$$\text{ActualElig}_{scart} = \alpha + \delta \text{SimulatedElig}_{scrt} + \lambda \text{X}_{scart} + \gamma_s + \eta_t + \theta_a + \nu_{scart} \tag{3.4}$$

The coefficient and t statistic on the fixed simulated eligibility are 0.89 and 12.16, respectively (see Appendix Table C.2.1 for the first-stage estimates). This suggests that fixed simulated eligibility is strongly correlated with actual eligibility. In addition, the Cragg-Donald Wald F statistic (2011.80) and the Kleibergen-Paap Rank statistic (17.45, $p < 0.001$) indicate that the instrument is neither weak nor underidentified.

The one potential problem with the instrument is that it relies on the exogeneity of state legislation. For example, there could be differential trends in financial outcomes across cohorts at the state level that are correlated with, but not caused by, fixed simulated eligibility (Currie and Gruber, 1996; Cohodes, Grossman, Kleiner, and Lovenheim, 2014; Gross and Notowidigdo, 2011). Although I cannot incontrovertibly establish the validity of this assumption, I follow Cohodes, Grossman, Kleiner, and Lovenheim (2014)

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and conduct two robustness checks to assess their validity in the data. First, I include state $\times$ year and age $\times$ year in equation 3.3 to capture any omitted time trends in financial outcomes that vary by age or state and are correlated with fixed simulated eligibility. Second, I exploit only federal variation that is arguably exogenous since it relies only on federal rules compared to variation that uses both state and federal rules. [17]

To account for the possible heterogeneity in the effect of Medicaid expansions across races (whites and nonwhites), I include Nonwhite $\times$ ActualElig interaction term in equation 3.3.[18] There are at least three reasons why the effect of Medicaid expansions can be heterogeneous across races. First, nonwhite children are more likely to benefit from the Medicaid expansions because of their distribution of family income (Wherry and Meyer, 2012). As depicted in Figures 3.3a and 3.3b, for every birth cohort from 1980 to 1991, nonwhites have higher average Medicaid eligibility between the ages of 0-17 than whites. Numerically, the percent of nonwhites that were eligible for Medicaid between the ages of 0-17 was 55.08 percent for the birth cohort 1991 and 20.96 percent for the birth cohort 1980. However, the percent of whites that were eligible for Medicaid between the ages of 0-17 was 38.22 percent for the birth cohort 1991 and 7.55 percent for the birth cohort 1980. Second, existing evidence on long-term health outcomes suggests that Medicaid eligibility during childhood is more likely to lead to better health outcomes later in life for nonwhite children than for whites. [19]Third, whites and nonwhites also tend to differ

[17]The construction of actual federal Medicaid eligibility and fixed simulated federal Medicaid eligibility are similar to actual Medicaid eligibility and fixed simulated federal Medicaid eligibility. The only difference is that for the federal eligibility, I fix AFDC rules in each state as of 1980 and consider only federal mandatory Medicaid rules (see Appendix Table C.2.1 for these rules).

[18]In this case, there will be two endogenous variables: actual eligibility and Nonwhite $\times$ ActualElig. As before, I instrument actual eligibility with simulated eligibilty and following Balli and Sørensen (2013) , I instrument Nonwhite $\times$ ActualElig with Nonwhite $\times$ SimulatedElig.

[19]For example, Wherry, Miller, Kaestner, and Meyer (2015) find that Medicaid eligibility in childhood is associated with fewer hospitalizations and emergency department visits for blacks at age 25. Wherry and Meyer (2012) also provide evidence that Medicaid eligibility in childhood results in a significant decrease in the internal mortality rate for black at ages 15-18.

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in terms of financial outcomes. For example, nonwhites generally have lower homeownership rates than whites. Accordingly, I estimate the following modified form of equation 3.3 :

$$\begin{aligned} y_{scart} =& \alpha + \beta_1 \text{ActualElig}_{scrt} + \beta_2 \text{Nonwhite} \times \text{ActualElig}_{scrt} + \lambda X_{scart} + \gamma_s + \\ & \eta_t + \theta_a + \epsilon_{scart} \end{aligned} \tag{3.5}$$

The coefficient β_2 gives the difference in the impact of Medicaid eligibility on the financial outcome between nonwhites and whites.

3.6 Empirical Results

This section presents results for three financial outcomes. I first present the OLS relationship between Medicaid expansions and financial outcomes (equation 3.3). I then report IV estimates of equation 3.3 for these outcomes. Next, I address endogeneity of state Medicaid expansions and provide results from robustness checks. Finally, I present OLS and IV estimates of equation 3.4 that allows for the interaction of eligibility and race.

3.6.1 Main Results

Table 3.2 presents the main results. Panel A of Table 3.2 presents the OLS estimates, whereas Panel B presents the IV estimates. In panels A and B, columns 2, 3, and 4 provide estimates for homeownership rate, mortgage ownership rate, and financial market participation rate, respectively. The OLS estimates suggest that actual Medicaid eligibility has a statistically negative impact on all financial outcomes but significant only for homeownership rate and mortgage ownership rate. This is presumably because of the omitted factors. So, in my preferred specification, I instrument actual Medicaid eligibility with simulated Medicaid eligibility. Columns 2, 3, and 4 in panel B report the IV estimates. Unlike the OLS estimates, the IV estimates are positive but remain statistically

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significant. In addition, the magnitude of an IV coefficient is larger than the OLS for all outcomes.

The IV estimates suggest that Medicaid eligibility during childhood increases homeownership rate, mortgage ownership rate, and financial market participation rate later in life. Specifically, a ten percentage point increase in eligibility is associated with a 0.0071 percentage point increase in the homeownership rate, a 0.0088 percentage point increase in the mortgage ownership rate, and a 0.0024 percentage point increase in the financial market participation rate. Given the average values of homeownership rate of 35.18 %, mortgage ownership rate of 30.39 %, and financial market participation rate of 8.31 %, these estimates correspond to increases in homeownership rate of 2.02 %, mortgage ownership rate of 2.90 %, and financial market participation rate of 2.89 %. These magnitudes are economically significant. To see this, consider the increase in Medicaid eligibility between the 1980 and 1991 birth cohorts (Figure 3.2); the eligibility increased by 32.29 percentage points. Using estimates from columns 2 through 4 (panel B), this implies that the change would have increased homeownership rate, mortgage ownership rate, and financial market participation rate by 6.52 percent, 9.35 percent, and 9.33 percent, respectively.

3.6.2 Robustness Checks

The IV estimates in panel B of Table 3.2 assumes the exogeneity of the instrument. To test the validity of this assumption, I conduct two robustness checks as described in 3.5. First, I include state by year and age by year fixed effects in the models. Table 3.3 presents the results of this test. Panel A shows that such fixed effects have little effect on the OLS estimates. Panel B shows that the IV estimates are still positive and statistically significant; however, these estimates are substantially larger that the baseline

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IV estimates (panel B of Table 3.2).

An alternative test for the concern regarding the endogeneity of the instrument is presented in Table 3.4 that uses only federal variation. The results are similar to the baseline IV estimates (panel B of Table 3.2); the point estimates are slightly higher than the baseline IV estimates. For example, the baseline IV estimate for homeownership rate (column 2 of panel B of Table 3.2) implies that using all Medicaid variation, a ten percentage point rise in eligibility is associated with a 0.0071 percentage point (2.02%) increase in the homeownership rate. However, the IV estimate in column 2 of Table 3.4 suggests that using only federal Medicaid variation, the increase is 0.014 percentage points (3.98%).

3.6.3 Extension: Race Interactions

Table 3.5 presents the OLS and IV estimates from equation 3.5. As before, columns 2 through 4 of panel A and panel B provide OLS and IV estimates for homeownership rate, mortgage ownership rate, and financial market participation rate, respectively. Both the OLS and IV estimates show that the interaction term between Medicaid eligibility and nonwhites is positive and statistically significant for all financial outcomes. This implies that nonwhites are more responsive to Medicaid expansions than whites as to financial outcomes. One way to get a sense of the magnitude of the interaction effect is to examine the partial derivative with respect to Medicaid eligibility using estimates from columns 2, 3, and 4 (panel B):

$$\frac{\partial \text{Own}_{scart}}{\partial \text{ActualElig}_{scrt}} = -0.0313 + 0.2462\text{Nonwhite}$$
$$\frac{\partial \text{Mortgage}_{scart}}{\partial \text{ActualElig}_{scrt}} = -0.0158 + 0.2498\text{Nonwhite}$$
$$\frac{\partial \text{Fin}_{scart}}{\partial \text{ActualElig}_{scrt}} = -0.0098 + 0.0806\text{Nonwhite}$$

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These partial derivatives suggest that the increase in eligibility leads to increases in homeownership rate, mortgage ownership rate, and financial market participation rate for nonwhites, but decreases in homeownership rate, mortgage ownership rate, and financial market participation rate for whites. Specifically, for nonwhites, a ten percentage point higher eligibility increases the homeownership rate by 0.022 percentage point (6.25 %), mortgage ownership rate by 0.023 percentage point (7.57 %), and financial market participation rate by 0.007 percentage points (8.42 %). For whites, a ten percentage point increase in eligibility is associated with a decrease in the homeownership rate by 0.0031 percentage point (0.88 %) , mortgage ownership rate by 0.0016 percentage point (0.53 %), and financial market participation rate by 0.001 percentage points (1.20 %). Table 3.6 shows that the interaction effect is robust to the inclusion of state by year and age by year fixed effects in the models.

3.7 Conclusion

This paper provides the evidence on the long term impact of Medicaid expansions that took place in the 1980's and 1990's on three financial outcomes - homeownership rate, mortgage ownership rate, and financial market participation rate. Using the simulated eligibility instrumental variables approach, I find that Medicaid eligibility during childhood increases homeownership rate, mortgage ownership rate, and financial market participation rate later in life. Specifically, a ten percentage point increase in eligibility is associated with a 0.0071 percentage point increase in the homeownership rate, a 0.0088 percentage point increase in the mortgage ownership rate, and a 0.0024 percentage point increase in the financial market participation rate. These magnitudes are economically significant. The estimates indicate that the increase in Medicaid eligibility by the magnitude similar to the increase between the 1980 and 1991 birth cohorts would increase

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homeownership rate, mortgage ownership rate, and financial market participation rate by 6.52 percent, 9.35 percent, and 9.33 percent, respectively. This finding is robust to only use of variation provided by federal Medicaid expansions.

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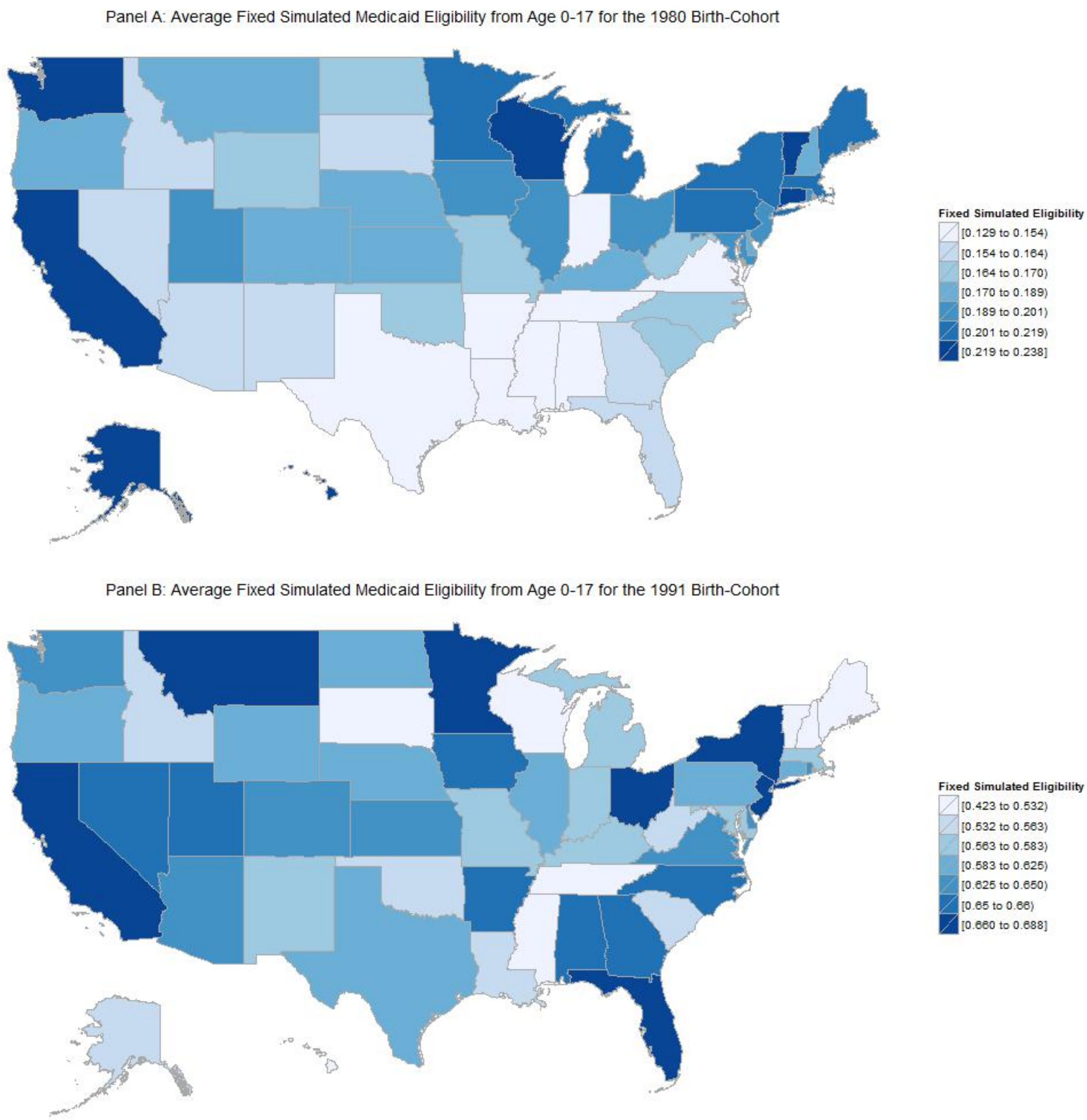

Figure 3.1: **Average Fixed Simulated Medicaid Eligibility for the Birth-Cohorts 1980 and 1991**

Notes: Figure shows fixed simulated Medicaid eligibility, constructed using fixed national sample from the 1986 March Current Population Survey, for the birth cohorts 1980 and 1991 by state. Medicaid eligibility is defined as the average Medicaid eligibility during childhood from birth to age 17. See text for details.

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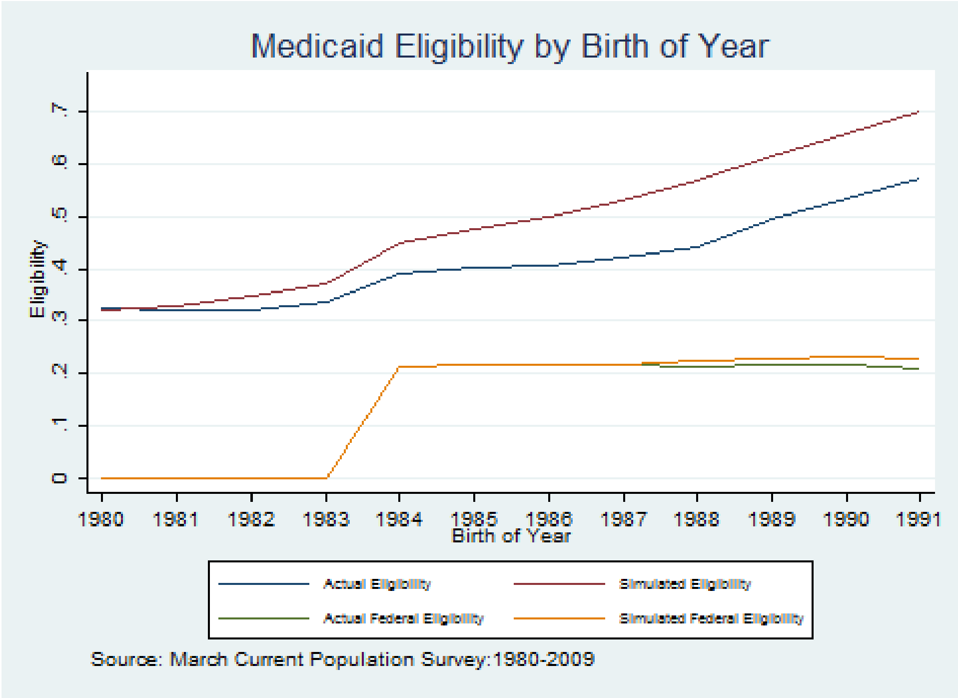

Figure 3.2: **Average Medicaid Eligibility by Birth-Cohorts**

Notes: Figure shows Medicaid eligibility by birth-cohorts and race using the March Current Population Survey (CPS) data from 1980-2009. Medicaid eligibility is defined as the average Medicaid eligibility during childhood from birth to age 17. Actual eligibility uses both state variation and federal variation, whereas federal eligibility uses only federal variation. Simulated eligibility and simulated federal eligibility use 20% national sample from the 1986 CPS. See text for details.

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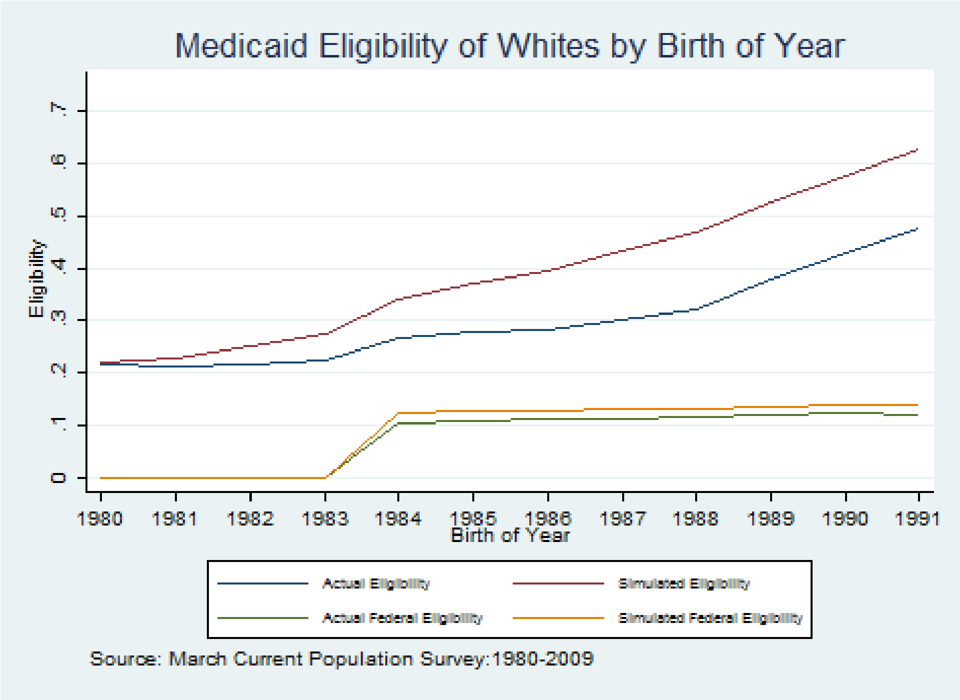

Figure 3.3a: **Average Medicaid Eligibility by Birth-Cohorts for Whites**

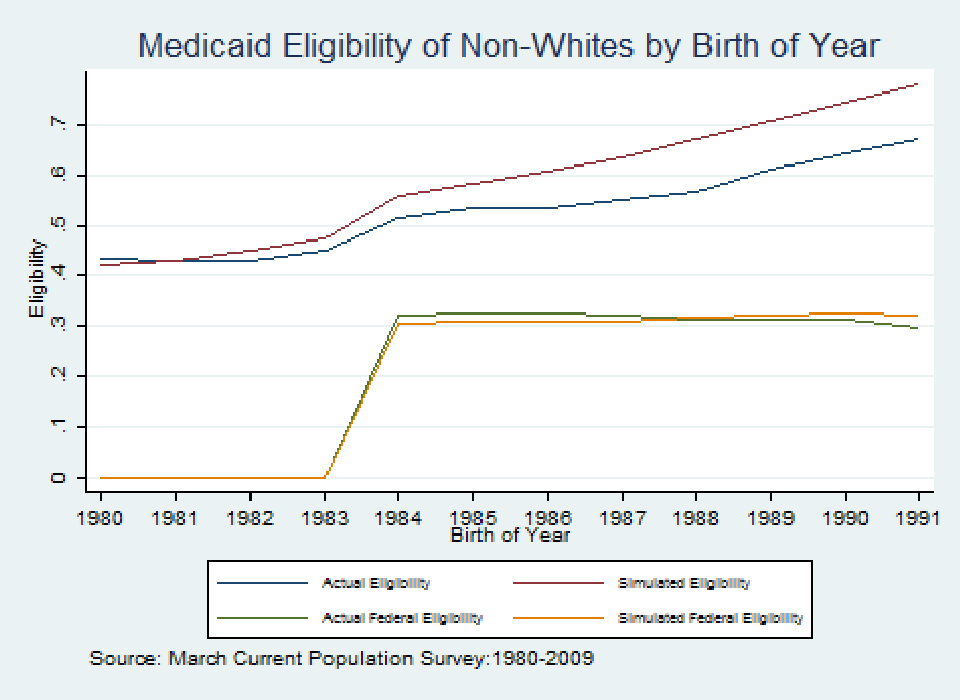

Figure 3.3b: **Average Medicaid Eligibility by Birth-Cohorts for Non-whites**

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Table 3.1: **Descriptive Statistics**

	All	White	Nonwhite
Homeownership rate	0.3518	0.3956	0.2017
	(0.1545)	(0.1393)	(0.1002)
Mortgage ownership rate	0.3039	0.3456	0.1610
	(0.1465)	(0.1326)	(0.0913)
Financial market participation rate	0.0831	0.0955	0.0405
	(0.0437	(0.0358)	(0.0413)
Age	25.8652	25.8821	25.8074
	(2.3882)	(2.3817)	(2.4103)
Male	0.4567	0.4864	0.3549
	(0.0812)	(0.0491)	(0.0868)
White	0.7739	1.0000	0.0000
	(0.4184)	(0.0000)	(0.0000)
Black	0.1360	0.0000	0.6012
	(0.2820)	(0.0000)	(0.2682)
Other race	0.0902	0.0000	0.3988
	(0.2100)	(0.0000)	(0.2682)
Married	0.3572	0.397	0.2212
	(0.1376)	(0.1205)	(0.1009)
Eligibility age 0-17	0.1997	0.1651	0.3178
	(0.1275)	(0.1056)	(0.1250)
Fixed simulated eligibility age 0-17	0.238	0.2022	0.3607
	(0.1362)	(0.1157)	(0.1296)
Federal eligibility age 0-17	0.0742	0.0589	0.1266
	(0.0932)	(0.0680)	(0.1381)
Federal fixed simulated eligibility age 0-17	0.0769	0.0612	0.1305
	(0.0900)	(0.0640)	(0.1344)
Observations	6638	3366	3272

Notes: Eligibility is defined as the average Medicaid eligibility during childhood from birth to age 17. Eligibility uses both state variation and federal variation, whereas federal eligibility uses only federal variation. See text for details.

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Table 3.2: **Effect of childhood Medicaid eligibility on financial outcomes**

	Homeownership rate (2)	Mortgage ownership rate (3)	Financial market participation rate (4)
Panel A: OLS estimates			
Eligibility	-0.0620**	-0.0397*	-0.0012
	(0.0262)	(0.0216)	(0.0130)
Panel B: IV estimates			
Eligibility	0.0712*	0.0883**	0.0238*
	(0.0404)	(0.0331)	(0.0128)

Notes: N=6638. Table shows estimates of childhood Medicaid eligibility on financial outcomes in adulthood (aged 22-30 years) (Equation 3.3). Columns 2 through 4 report estimates for homeownership rate, mortgage ownership rate, and financial market participation rate, respectively. Panel A presents the OLS estimates, and panel B presents the IV estimates. Childhood Medicaid eligibility is defined as the average actual Medicaid eligibility during childhood from birth to age 17. All specifications include state fixed effects, year fixed effects, age fixed effects, percent of each state-of-birth, birth cohort, and race cell who are male and married as well as an indicator for the cell being non-white or not. The instrumental variables specifications use the average fixed simulated Medicaid eligibility as an instrument for the average actual Medicaid eligibility in the first stage. Standard errors clustered at the state-of-birth level are in parentheses. Superscripted notations next to the coefficients indicate the level of statistical significance from a two-tailed t-test. ***,**,*denote significance at 1 % , 5 %, 10 % level, respectively.

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Table 3.3: **Effect of childhood Medicaid eligibility on financial outcomes: including state-year and age-year fixed effects**

	Homeownership rate (2)	Mortgage ownership rate (3)	Financial market participation rate (4)
Panel A: OLS estimates			
Eligibility	-0.0942**	-0.0704**	-0.0075
	(0.0363)	(0.0328)	(0.0158)
Panel B: IV estimates			
Eligibility	0.1819*	0.1757**	0.0770***
	(0.0973)	(0.0861)	(0.0233)

Notes: N=6638. Table shows estimates of childhood Medicaid eligibility on financial outcomes in adulthood (aged 22-30 years) (Equation 3.3). Columns 2 through 4 report estimates for homeownership rate, mortgage ownership rate, and financial market participation rate, respectively. Panel A presents the OLS estimates, and panel B presents the IV estimates. Childhood Medicaid eligibility is defined as the average actual Medicaid eligibility during childhood from birth to age 17. All specifications include state fixed effects, year fixed effects, age fixed effects, state-year fixed effects, age-year fixed effects, percent of each state-of-birth, birth cohort, and race cell who are male and married as well as an indicator for the cell being non-white or not. The instrumental variables specifications use the average fixed simulated Medicaid eligibility as an instrument for the average actual Medicaid eligibility in the first stage. Standard errors clustered at the state-of-birth level are in parentheses. See text for details.Superscripted notations next to the coefficients indicate the level of statistical significance from a two-tailed t-test. ***,**,*denote significance at 1 % , 5 %, 10 % level, respectively.

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Table 3.4: **Effect of childhood Medicaid eligibility on financial outcomes: using fixed simulated federal eligibility as an instrument**

	Homeownership rate (2)	Mortgage ownership rate (3)	Financial market participation rate (4)
Eligibility	0.1420***	0.1416***	0.0713***
	(0.0406)	(0.0362)	(0.0226)

Notes: N=6638. Table shows the IV estimates of childhood Medicaid eligibility on financial outcomes in adulthood (aged 22-30 years) (Equation 3.3). Columns 2 through 4 report estimates for homeownership rate, mortgage ownership rate, and financial market participation rate, respectively. Childhood Medicaid eligibility is defined as the average actual Medicaid eligibility during childhood from birth to age 17. All specifications include state fixed effects, year fixed effects, age fixed effects, percent of each state-of-birth, birth cohort, and race cell who are male and married as well as an indicator for the cell being non-white or not. The instrumental variables specifications use the average fixed simulated federal Medicaid eligibility as an instrument for the average actual Medicaid eligibility in the first stage. Standard errors clustered at the state-of-birth level are in parentheses. See text for details .Superscripted notations next to the coefficients indicate the level of statistical significance from a two-tailed t-test. ***,**,*denote significance at 1 % , 5 %, 10 % level, respectively.

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Table 3.5: **Effect of childhood Medicaid eligibility on financial outcomes: including race interactions**

	Homeownership rate (2)	Mortgage ownership rate (3)	Financial market participation rate (4)
Panel A: OLS estimates			
Eligibility	-0.1627***	-0.1363***	-0.0317***
	(0.0292)	(0.0242)	(0.0103)
Nonwhite × Eligibility	0.2113***	0.2025***	0.0641***
	(0.0381)	(0.0363)	(0.0145)
Nonwhite	-0.1235***	-0.1237***	-0.0621***
	(0.0208)	(0.0196)	(0.0078)
Panel B: IV estimates			
Eligibility	-0.0313	-0.0158	-0.0098
	(0.0456)	(0.0401)	(0.0143)
Nonwhite × Eligibility	0.2462***	0.2498***	0.0806***
	(0.0446)	(0.0429)	(0.0137)
Nonwhite	-0.1528***	-0.1560***	-0.0707***
	(0.0275)	(0.026)	(0.0075)

Notes: N=6638. Table shows estimates of childhood Medicaid eligibility on financial outcomes in adulthood (aged 22-30 years) (equation 3.5). Columns 2 through 4 report estimates for homeownership rate, mortgage ownership rate, and financial market participation rate, respectively. Panel A presents the OLS estimates, and panel B presents the IV estimates. Childhood Medicaid eligibility is defined as the average actual Medicaid eligibility during childhood from birth to age 17. All specifications include state fixed effects, year fixed effects, age fixed effects, percent of each state-of-birth, birth cohort, and race cell who are male and married. The instrumental variables specifications use the average fixed simulated Medicaid eligibility as an instrument for the average actual Medicaid eligibility and Nonwhite average fixed simulated Medicaid eligibility as an instrument for the Nonwhite average actual Medicaid eligibility in the first stage. Standard errors clustered at the state-of-birth level are in parentheses. See text for details. Superscripted notations next to the coefficients indicate the level of statistical significance from a two-tailed t-test. ***,**,*denote significance at 1 % , 5 %, 10 % level, respectively.

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Table 3.6: **Effect of childhood Medicaid eligibility on financial outcomes: including race interactions and state-year and age-year fixed effects**

	Homeownership rate (2)	Mortgage ownership rate (3)	Financial market participation rate (4)
Panel A: OLS estimates			
Eligibility	-0.2240***	-0.1954***	-0.0439***
	(0.0492)	(0.0463)	(0.0117)
Nonwhite × Eligibility	0.2304***	0.2220***	0.0645***
	(0.0381)	(0.0366)	(0.0143)
Nonwhite	-0.1212***	-0.1226***	-0.0606***
	(0.0217)	(0.0205)	(0.0081)
Panel B: IV estimates			
Eligibility	-0.0083	-0.0206	0.0210
	(0.1148)	(0.1122)	(0.0264)
Nonwhite × Eligibility	0.2532***	0.2614***	0.0745***
	(0.0451)	(0.0448)	(0.0131)
Nonwhite	-0.1573***	-0.1594***	-0.0726***
	(0.0299)	(0.0274)	(0.0080)

Notes: N=6638. Table shows estimates of childhood Medicaid eligibility on financial outcomes in adulthood (aged 22-30 years) (equation 3.5). Columns 2 through 4 report estimates for homeownership rate, mortgage ownership rate, and financial market participation rate, respectively. Panel A presents the OLS estimates, and panel B presents the IV estimates. Childhood Medicaid eligibility is defined as the average actual Medicaid eligibility during childhood from birth to age 17. All specifications include state fixed effects, year fixed effects, age fixed effects, state-year fixed effects, age-year fixed effects, percent of each state-of-birth, birth cohort, and race cell who are male and married. The instrumental variables specifications use the average fixed simulated Medicaid eligibility as an instrument for the average actual Medicaid eligibility and Nonwhite average fixed simulated Medicaid eligibility as an instrument for the Nonwhite × average actual Medicaid eligibility in the first stage. Standard errors clustered at the state-of-birth level are in parentheses. See text for details. Superscripted notations next to the coefficients indicate the level of statistical significance from a two-tailed t-test. ***,**,*denote significance at 1 % , 5 %, 10 % level, respectively.

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APPENDIX A

A.1 CES sample choice and definition of healthcare spending

In the CES, annual healthcare spending for each household do not correspond exactly with the calendar year for two important reasons.[1] First, households in each quarterly interview report the healthcare spending for the previous three months. More importantly, each quarterly interview is uniformly spread over the months of the quarter. So, the two households interviewed at the two different months of the same quarter, would report the healthcare spending for the different prior three months. Second, the CES is a rotating panel survey with households in each quarterly interview consisting of both new and old households. Each quarter, one-fifth of the household interviewed is new and replaces the old households who have finished the fifth interview in the previous quarter. Because of the rotating nature of the CES, a number of households do not finish all five interviews.

I address these problems in two ways. First, I focus only on the households who completed all five interviews and reported valid income data. Second, I construct approximate annual spending for calendar year t[2] by including only those households that entered the survey between the second quarter of calendar year t and second quarter of calendar year *t+1* (Johnson and Li, 2009; Attanasio and Weber, 1995). For example, to construct healthcare spending for year 2010, I include only households that entered the survey between the second quarter of 2010 and second quarter of 2011. Thus, year 2010 includes the data that spans from the third quarter of 2010 to the second quarter of 2012.[3] In order to compute the annual healthcare spending at the household level, each

[1] Household health care expenditures include only out-of-pocket spending and do not include those items that are covered by health insurance (Attanasio and Weber, 1995).

[2] The calendar year t refers to the Survey of Consumer Finance (SCF) survey year. The SCF is conducted every three years and is available from the start of 1983 to 2010.

[3] The objective is to include only the households that have at least two quarters of the calendar year expenditures data. Note that the first interview doesnt collect information on expenditures (Johnson and Li, 2009).

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household is first matched across the quarters by consumer unit identifying variable. The annual healthcare spending for each household is then obtained as the sum of its previous quarter (PQ) and current quarter (CQ) variables.[4] Household healthcare spending in the Interview Survey consist of four components[5]: health insurance, medical services, prescription drugs, and medical supplies (Foster and Kreisler, 2011).[6]

(a) Health insurance includes premiums paid by consumers for private health insurance and Medicare. Private insurance includes coverage obtained individually or through a group plan sponsored by an employer or other organization.[7] Medicare outlays are premiums paid for Medicare Part B and Medicare Part D coverage. Medicare Part B (Medical Insurance) helps cover physicians services and outpatient care and Medicare Part D covers prescription drugs.

(b) Medical services include physicians, dental, eye care, and other professional services; inpatient hospital care; lab tests and x-rays; other medical care services, such as hospital outpatient and emergency room care; and nursing home care.

(c) The prescription drug spending category is for outlays that are not connected to inpatient hospitalization.

(d) Medical supplies include the purchase of hearing aids, eyeglasses and contact lenses, and the purchase or rental of medical equipment for general use.

The data on healthcare spending (and its components), demographic variables ,and employment and income are obtained from FMLY file.[8] The health insurance variable is

[4]If household was interviewed in June, then PQ variable refers to the expenditures of March of previous quarter, and CQ refers to the expenditures of April and May of current quarter. See pps.24, 73, and 77 of Interview users documentation.

[5]The study does not include health care expenditures collected in the Diary Survey which consists of nonprescription drugs, nonprescription vitamins, topical and dressings, and medical equipment.

[6]CES provides components of healthcare expenditures only from the year 1994. For years prior to 1994, CES gives only total healthcare expenditures.

[7]The CES does not include the premiums paid by the employers on behalf of households (Gruber and Levy, 2009).

[8]I use measure of final income before taxes. For the CES, I use the imputed income data for 2004 Q1 to

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obtained by merging IHB file (for private insurance) and IHC file (for public insurance) of detailed spending files with FMLY file . The health insurance and medical service components of health spending are expressed in terms of 2010 dollars using the medical care services component of Medical care group of CPI-U of the BLS whereas prescription drug and medical supplies components are expressed in terms of 2010 dollars using the medical care commodities component of Medical care group of CPI-U of the BLS. The healthcare spending is then obtained as the sum of these converted components.[9]

2005 Q4 and non-imputed income data for other years. The annual income in CES is obtained as the average of the annual income reported in interviews two to five. The demographic variables pertain to the third interview (these don't change for most of households across the four interviews).

[9]For years prior to 1994, I use total healthcare spending in the CES and convert it into 2010 dollar terms using all items of CPI-U of the BLS.

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A.2 Robustness check: net wealth instead of gross wealth

Table A.2.1: **ML estimation of the dependent double-hurdle model using net wealth specification**

Dependent Variable:	log(healthcare spending)	
	Participation equation	Consumption equation
Age 36-49	0.2401*** (0.0285)	0.2190*** (0.0218)
Age 50-64	0.4823*** (0.0353)	0.4936*** (0.0249)
Married	0.3341*** (0.0358)	0.2692*** (0.0276)
High School	0.2312*** (0.0375)	0.1693*** (0.0324)
Some college or more	0.4087*** (0.0381)	0.2288*** (0.0324)
African American	-0.3273*** (0.0368)	-0.1903*** (0.0334)
Other race	-0.2021*** (0.0569)	-0.0727* (0.0440)
Household size 2	0.1604*** (0.0381)	0.1936*** (0.0313)
Household size 3	0.1428*** (0.0446)	0.2431*** (0.0352)
Household size 4	0.1627*** (0.0438)	0.2550*** (0.0350)
Manager and professional	0.3465*** (0.0400)	-0.0705** (0.0290)
Administrative	0.2534*** (0.0396)	-0.1109*** (0.0296)
Service	0.1406*** (0.0428)	-0.2104*** (0.0359)
Operators	0.1792*** (0.0455)	-0.2722*** (0.0351)
Other	0.1148** (0.0510)	-0.1672*** (0.0388)
Insured	0.2860*** (0.0252)	0.0561*** (0.0182)
Year 1992	-0.1479*** (0.0575)	0.0660* (0.0390)
Year 1995	-0.1963*** (0.0604)	0.3327*** (0.0408)
Year 1998	-0.0869 (0.0550)	0.3054*** (0.0349)
Year 2001	-0.1310** (0.0530)	0.3196*** (0.0341)
Year 2004	-0.2232*** (0.0560)	0.2830*** (0.0379)
Year 2007	-0.3297*** (0.0526)	0.3386*** (0.0349)
Year 2010	-0.2662*** (0.0516)	0.3191*** (0.0352)
Log of income		0.2182*** (0.0108)
Net financial wealth		0.0008 (0.0010)
Home equity		0.0150*** (0.0015)
Other real estate equity		0.0011 (0.0020)
Constant	0.4478*** (0.0651)	4.1568*** (0.1092)
Sigma	1.2566*** (0.0066)	
Rho	-0.9051*** (0.0067)	

Notes: All estimates are from the dependent double-hurdle model using net wealth specification for pooled sample (1989-2010). Columns 1 and 2 present the estimates for the participation equation and consumption equation, respectively .The components of net wealth are transformed using sign(net wealth) $\times$ ln(|net wealth| + 1). The omitted categories for age, education, race, household size and occupation are age 18-35, less than high school, white, household size 1, and unemployed, respectively. Standard errors are in parentheses. Number of observation varies from 23349 to 23376 depending on the imputation number. Superscripted notations next to the coefficients indicate the level of statistical significance from a two-tailed t-test. ***,**,*denote significance at 1 % , 5 %, 10 % level, respectively. See Table 1.2 for definitions of variables.

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Table A.2.2: **Average elasticities for pooled sample with respect to continuous variables using net wealth specification**

	Unconditional level
Income	0.2182*** (0.0120)
Net financial wealth	0.0003 (0.0000)
Home equity	0.0100*** (0.0010)
Other real estate equity	0.0001 (0.0000)

Notes: Unconditional level average elasticity based on the estimates are from double-hurdle models using net wealth specification. Since the continuous economic variables appear only in consumption equation, probability average elasticity is close to zero and therefore, conditional level average elasticity is the same as the unconditional level average elasticity. Number of observation varies from 23349 to 23376 depending on the imputation number . Bootstrapped standard errors are in parentheses. Superscripted notations next to the coefficients indicate the level of statistical significance from a two-tailed t-test. ***,**,*denote significance at 1 % , 5%, 10 % level, respectively. See Table 1.2 for definitions of variables..

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Table A.2.3: **Average partial effects for pooled sample with respect to binary variables using net wealth specification**

	Probability	Conditional level	Unconditional level
Age 36-49	0.0309*** (0.0040)	0.2791*** (0.0210)	0.0731*** (0.0050)
Age 50-64	0.0566*** (0.0040)	0.6054*** (0.0230)	0.1452*** (0.0050)
Married	0.0459*** (0.0050)	0.3581*** (0.0270)	0.1006*** (0.0060)
High School	0.0286*** (0.0050)	0.2252*** (0.0340)	0.0631*** (0.0060)
Some college or more	0.0575*** (0.0060)	0.3392*** (0.0330)	0.1124*** (0.0080)
African American	-0.0511*** (0.0070)	-0.2865*** (0.0330)	-0.0991*** (0.0090)
Other race	-0.0299*** (0.0110)	-0.1295*** (0.0420)	-0.0525*** (0.0140)
Household size 2	0.0201*** (0.0050)	0.2329*** (0.0300)	0.0541*** (0.0060)
Household size 3	0.0176*** (0.0050)	0.2777*** (0.0350)	0.0572*** (0.0070)
Household size 4	0.0205*** (0.0050)	0.2952*** (0.0350)	0.0631*** (0.0070)
Manager and professional	0.0420*** (0.0040)	0.0123 (0.0270)	0.0488*** (0.0060)
Administrative	0.0305*** (0.0040)	-0.0509* (0.0280)	0.0276*** (0.0060)
Service	0.0171*** (0.0050)	-0.1767*** (0.0370)	-0.0047 (0.0070)
Operators	0.0215*** (0.0050)	-0.2300*** (0.0330)	-0.0073 (0.0060)
Other	0.0141** (0.0060)	-0.1394*** (0.0370)	-0.0030 (0.0080)
Insured	0.0398*** (0.0040)	0.1327*** (0.0170)	0.0637*** (0.0050)

Notes: Average partial effects (APE) based on the estimates from double-hurdle models using net wealth specification for pooled sample (1989-2010). Probability APE indicates absolute change in participation probability, conditional level and unconditional level APE indicates relative change in healthcare expenditures. Bootstrapped standard errors are in parentheses. Number of observation varies from 23349 to 23376 depending on the imputation number . Bootstrapped standard errors are in parentheses. Superscripted notations next to the coefficients indicate the level of statistical significance from a two-tailed t-test. ***,**,*denote significance at 1 % , 5 %, 10 % level, respectively. See Table 1.2 for definitions of variables.

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Table A.2.4: **Average elasticities by SCF survey years with respect to economic variables using net wealth specification**

	Unconditional level
Panel A: 1989 (No. of observations : 2254-2261)	
Income	0.1961*** (0.0410)
Net financial wealth	-0.0011 (0.0010)
Home equity	0.0145*** (0.0040)
Other real estate equity	0.0009 (0.0010)
Panel B: 1992 (No. of observations : 2147-2159)	
Income	0.2288*** (0.0460)
Net financial wealth	0.0009 (0.0010)
Home equity	0.0123*** (0.0030)
Other real estate equity	-0.0001 (0.0010)
Panel C: 1995 (No. of observations : 1803-1812)	
Income	0.0994** (0.0420)
Net financial wealth	-0.0002 (0.0010)
Home equity	0.0129*** (0.0040)
Other real estate equity	0.0004 (0.0010)
Panel D: 1998 (No. of observations : 3391-3397)	
Income	0.1601*** (0.0280)
Net financial wealth	0.0017 (0.0010)
Home equity	0.0086*** (0.0030)
Other real estate equity	0.0005 (0.0010)
Panel E: 2001 (No. of observations : 3955-3975)	
Income	0.1736*** (0.0280)
Net financial wealth	-0.0001 (0.0010)
Home equity	0.0128*** (0.0030)
Other real estate equity	0.0002 (0.0010)
Panel F: 2004 (No. of observations : 2257-2566)	
Income	0.2902*** (0.0470)
Net financial wealth	-0.0002 (0.0010)
Home equity	0.0128*** (0.0040)
Other real estate equity	-0.0009 (0.0010)
Panel G: 2007 (No. of observations : 3586-3600)	
Income	0.2524*** (0.0300)
Net financial wealth	0.0007 (0.0010)
Home equity	0.0172*** (0.0040)
Other real estate equity	0.0002 (0.0010)
Panel H: 2010 (No. of observations : 3633-3637)	
Income	0.3078*** (0.0340)
Net financial wealth	-0.0005 (0.0010)
Home equity	0.0047*** (0.0020)
Other real estate equity	-0.0002 (0.0000)

Notes: Unconditional level average elasticity based on the estimates from double-hurdle models using net wealth specification for each SCF survey years (1989, 1992, 1995, 1998, 2001, 2004, 2007, and 2010) . Since the continuous economic variables appear only in consumption equation, probability average elasticity is close to zero and therefore, conditional level average elasticity is the same as the unconditional level average elasticity. Bootstrapped standard errors are in parentheses . Superscripted notations next to the coefficients indicate the level of statistical significance from a two-tailed t-test. ***,**,*denote significance at 1 % , 5%, 10 % level, respectively. See Table 1.2 for definitions of variables.

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Table A.2.5: **Average elasticities for constrained and unconstrained households with respect to economic variables using net wealth specification**

	Unconditional level
Panel A: Unconstrained households (No. of observations : 18774-18827)	
Income	0.2030*** (0.0120)
Net financial wealth	0.0005 (0.0010)
Home equity	0.0096*** (0.0010)
Other real estate equity	0.0001 (0.0000)
Panel B: Constrained households (No. of observations : 4528-4587)	
Income	0.2559*** (0.0420)
Net financial wealth	0.0001 (0.0000)
Home equity	0.0079*** (0.0010)
Other real estate equity	0.0004 (0.0000)

Notes: Unconditional level average elasticity based on the estimates from double-hurdle models using net wealth specification for unconstrained and constrained households. Since the continuous economic variables appear only in consumption equation, probability average elasticity is close to zero and therefore, conditional level average elasticity is the same as the unconditional level average elasticity. Households are defined as credit-constrained if they were turned down for credit or received less that they applied for, or were discouraged from applying for credit because they believed that they would be turned down. Bootstrapped standard errors are in parentheses. Superscripted notations next to the coefficients indicate the level of statistical significance from a two-tailed t-test. ***,**,*denote significance at 1 % , 5%, 10 % level, respectively. See Table 1.2 for definitions of variables.

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Table A.2.6: **Average elasticities by age groups with respect to economic variables using net wealth specification**

	Unconditional level
Panel A: Age 18-35 (No. of observations :6131-6139)	
Income	0.3627*** (0.0400)
Net financial wealth	0.0000 (0.0000)
Home equity	0.0065*** (0.0020)
Other real estate equity	0.0000 (0.0000)
Panel B: Age 36-49 (No. of observations : 9806-9816)	
Income	0.1929*** (0.0180)
Net financial wealth	-0.0002 (0.0010)
Home equity	0.0090*** (0.0020)
Other real estate equity	0.0002 (0.0000)
Panel C: Age 50-64 (No. of observations : 7404-7421)	
Income	0.1709*** (0.0180)
Net financial wealth	0.0011 (0.0010)
Home equity	0.0135*** (0.0020)
Other real estate equity	0.0000 (0.0000)

Notes: Unconditional level average elasticity based on the estimates from double-hurdle models using net wealth specification for Age 18-35, Age 36-49, and Age 50-64. Since the continuous economic variables appear only in consumption equation, probability average elasticity is close to zero and therefore, conditional level average elasticity is the same as the unconditional level average elasticity. Bootstrapped standard errors are in parentheses. Superscripted notations next to the coefficients indicate the level of statistical significance from a two-tailed t-test. ***,**,*denote significance at 1 % , 5%, 10 % level, respectively. See Table 1.2 for definitions of variables.

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APPENDIX B

B.1 Increase in county unemployment rate by size of housing crash

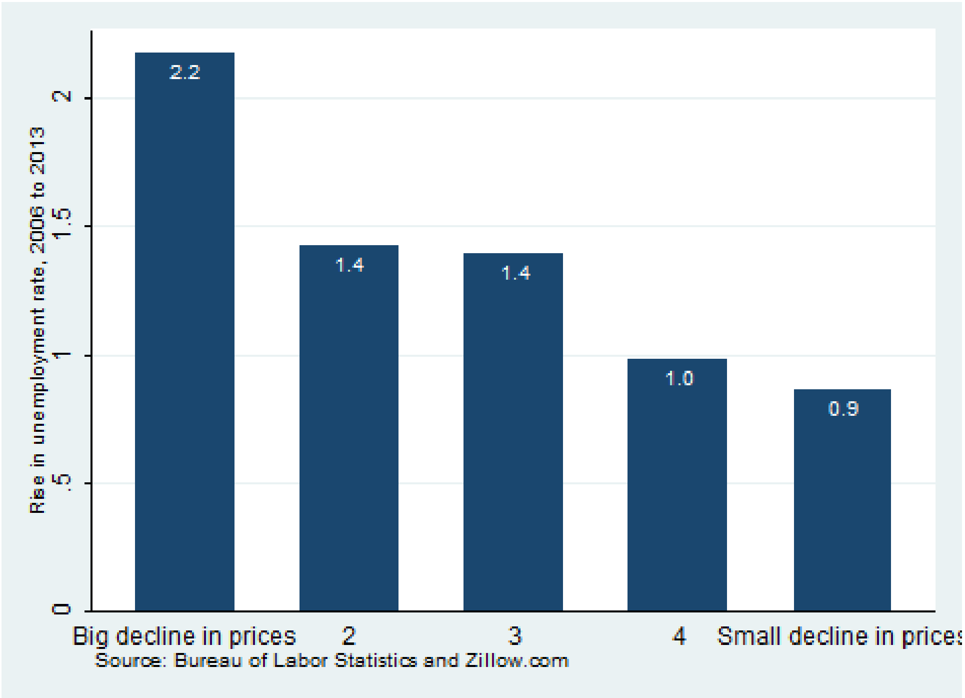

Figure B.1.1: **Increase in county unemployment rate, 2006-2013 by size of housing crash, 2006-2009**

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B.2 Definitions of the variables

Table B.2.1: **Definitions of the variables**

Variable	Definition	Source
Health Variables		
Log of BMI	Natural logarithm of Body Mass Index (BMI)	BRFSS
Physical Health	Physical health which takes values of 1, 2, 3, 4, and 5 if respondent reports physical health as poor, fair, good, very good, and excellent respectively	BRFSS
Poor Mental Health	Number of days during the previous month for which respondent reports having poor mental health	BRFSS
Binge Drinking	Number of days for which respondents report having at least one occasion with five (four for women) drinks in the previous month	BRFSS
Current Smoker	1 if respondents report smoking cigarettes some day; 0 otherwise	BRFSS
Daily Smoker	1 if respondents report smoking every day; 0 otherwise	BRFSS
Physical Activity	1 if the respondent reports participating in any physical activities or exercises in the previous month; 0 otherwise	BRFSS
Housing Market Variable		
County House Price (in '000)	Average of real Zillow House Value Index for single family residence at the county level during the three months ending with the survey month	Zillow
Control Variables		
County Unemployment Rate	Average of unemployment rate at the county level during the three months ending with the survey month	BLS
Log of Dow Jones Industrial Average†	Natural log of the monthly mean daily market closing Dow Jones Industrial Average	
High School	1 if respondent has an education of high school or less than high school; 0 otherwise	BRFSS
Some College	1 if respondent has an education of some college; 0 otherwise	BRFSS
College	1 if respondent is a college graduate; 0 otherwise	BRFSS
Age	Age of the respondents in years	BRFSS
Married	1 if the respondent is married; 0 otherwise	BRFSS
Divorced	1 if the respondent is divorced; 0 otherwise	BRFSS
Widowed	1 if the respondent is widowed; 0 otherwise	BRFSS
Other Marital Status	1 if the respondent is separated or never married; 0 otherwise	BRFSS
Mean Income (in '000)	Weighted average income (see text for details)	BRFSS
White	1 if the respondent is White; 0 otherwise	BRFSS
African American	1 if the respondent is Black; 0 otherwise	BRFSS
Hispanic	1 if the respondent is Hispanic; 0 otherwise	BRFSS
Other Race	1 if the respondent is of other races; 0 otherwise	BRFSS
Male	1 if the respondent is male; 0 otherwise	BRFSS

Notes: † from measuringworth.com. Summary of observations without non-responses from the 2001-2012 waves of Behavioral Risk Factor Surveillance System.

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APPENDIX C

C.1 Birth cohorts by age and ACS year and Medicaid legislation

Table C.1.1: **Birth cohorts by age and ACS year**

Age	ACS year								
	2005	**2006**	**2007**	**2008**	**2009**	**2010**	**2011**	**2012**	**2013**
22	1983	1984	1985	1986	1987	1988	1989	1990	1991
23	1982	1983	1984	1985	1986	1987	1988	1989	1990
24	1981	1982	1983	1984	1985	1986	1987	1988	1989
25	1980	1981	1982	1983	1984	1985	1986	1987	1988
26		1980	1981	1982	1983	1984	1985	1986	1987
27			1980	1981	1982	1983	1984	1985	1986
28				1980	1981	1982	1983	1984	1985
29					1980	1981	1982	1983	1984
30						1980	1981	1982	1983

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Table C.1.2: **Medicaid legislation**

1	Deficit Reduction Act of 1984 (DEFRA)	Required states to provide Medicaid to children up to age 5 born after September 30, 1983, who met Aid to Families with Dependent Children (AFDC) financial standards but not the categorical tests for eligibility.
2	Omnibus Budget Act of1986 (OBRA 86)	Permitted states to cover pregnant women and infants under age 1 (and, on a phased basis, children up to age 5) meeting a state established income standard as high as 100 percent of the federal poverty level.
3	Omnibus Budget Act of 1987 (OBRA 87)	Permitted states to cover pregnant women and infants with family incomes up to 185 percent of federal poverty level and allowed more rapid phase-in of coverage of children aged 1 through 5 with incomes below 100 percent of poverty. Extended to age 7 (or at the state's option, to age 8) the required coverage of children born after September 30, 1983, who met financial but not categorical eligibility standards.
4	Medicare Catastrophic Coverage Act of 1988 (MCCA)	Required states to phase in coverage of pregnant women and infants under age 1 with incomes under 100 percent of federal poverty level.
5	Omnibus Budget Reconciliation Act of 1989 (OBRA 89)	Required states to cover pregnant women and children up to age 6 born after September 30, 1983, with incomes under 133 percent of federal poverty level
6	Omnibus Budget Reconciliation Act of 1990 (OBRA 90)	Required states to cover all children under age 19 born after September 30, 1983, with family incomes under 100 percent of federal poverty level.
7	Balanced Budget Act of 1997 (BBA 97)	Established the State Children's Health Insurance Program (SCHIP, later referred to as CHIP), allowing states to cover uninsured children in families with incomes below 200 percent of federal poverty level who were ineligible for Medicaid. .

Source: Congressional Research Service (1993), pp. 35-37 and Buchmueller, Ham, and Shore-Sheppard (2015).

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C.2 First stage regressions

Table C.2.1: **First stage regressions**

	Baseline (2)	S-Y and A-Y fixed effects (3)	Federal eligibility only (4)
Fixed simulated eligibility	0.8923***	1.0402***	
	(0.0734)	(0.1303)	
Federal fixed simulated eligibility			0.3588***
			(0.0349)
R-squared	0.9041	0.9142	0.8661
Cragg-Donald F statistic	2011.8	779.88	451
Kleibergen-Paap rank LM statistic	17.45***	11.71***	14.48***

Notes: N=6638. Table shows estimates from first-stage regression of average actual Medicaid eligibility on the instrument (equation 3.4). Columns 2 and 3 use fixed simulated eligibility as an instrument, whereas Column 4 uses federal fixed simulated eligibility as an instrument. All specifications include state fixed effects, year fixed effects, age fixed effects, percent of each state-of-birth, birth cohort, and race cell who are male and married. Column 3 also includes state-year fixed effects and age-year fixed effects. Standard errors clustered at the state-of-birth level are in parentheses. See text for details. Superscripted notations next to the coefficients indicate the level of statistical significance from a two-tailed t-test. ***,**,*denote significance at 1 % , 5 %, 10 % level, respectively.

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ABSTRACT

ESSAYS IN HEALTH ECONOMICS

by

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May 2016

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Major: Economics

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My dissertation consists of three essays in health economics. In the first essay, I use the matched data set constructed from the Consumer Expenditure Survey and the Survey of Consumer Finances for period 1989-2010 to investigate the impacts of housing and financial wealth on healthcare spending. The results indicate significant housing and financial wealth effects and relatively large housing wealth effects for the pooled sample. Results for each survey years of the Survey of Consumer Finances further suggest that housing wealth effects are significant for all years and relatively large, but financial wealth effects are insignificant in most cases. Analysis by survey years also reveals the diminished housing wealth effect following the Great Recession. Moreover, the housing wealth effect is significant and most pronounced among older aged households and credit-constrained households. In general, the results are qualitatively similar when I use net wealth measure.

In the second essay, I investigate the relationship between local house prices and health and health behaviors of the individuals using the Behavioral Risk Factor Surveillance System for 2001-2012, a period that includes both the housing boom and housing bust. I find evidence of positive relationship for poor mental health and binge drinking but negative association for physical health for those likely to be homeowners. However,

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among those likely to be renters, I document positive impact of house prices on body mass index but negative effect on physical health and smoking. The subgroup analysis also suggests substantial heterogeneity of the health effects across gender. Additionally, I show an asymmetry in the relationship between house prices and health. For example, the impact of house prices on binge drinking and physical activity are relatively large during the housing bust period.

In the third essay, I examine the impacts of the public health insurance eligibility during childhood on longer-term financial outcomes. Exploiting the variation provided by the Medicaid expansions that took place in the 1980's and 1990's and applying the simulated eligibility instrumental variables approach, I show that Medicaid eligibility during childhood increases homeownership rate, mortgage ownership rate, and financial market participation rate later in life. This finding is robust to only use of variation provided by federal Medicaid expansions.

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