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THE ECONOMIC CONSEQUENCES OF HOSPITAL ADMISSIONS

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ABSTRACT

We examine some economic impacts of hospital admissions using an event study approach in two datasets: survey data from the Health and Retirement Study, and hospital admissions data linked to consumer credit reports. We report estimates of the impact of hospital admissions on out-of-pocket medical spending, unpaid medical bills, bankruptcy, earnings, income (and its components), access to credit, and consumer borrowing. The results point to three primary conclusions: non-elderly adults with health insurance still face considerable exposure to uninsured earnings risk; a large share of the incremental risk exposure for uninsured non-elderly adults is borne by third parties who absorb their unpaid medical bills; the elderly face very little economic risk from adverse health shocks.

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1 Introduction

Adverse health shocks are a major source of economic risk for adults in the United States. Protection against such risk has been a major rationale for health insurance policy in the United States. For example, speaking at the signing ceremony for Medicare, President Johnson declared, "No longer will illness crush and destroy the savings that [older Americans] have so carefully put away over a lifetime."[1] More recently, the United States has undertaken a major expansion of both public and private health insurance coverage through the 2010 Affordable Care Act, which particularly expanded coverage for non-elderly adults. As a result, the vast majority of American adults now have health insurance. Yet we know remarkably little about their exposure to economic risk from adverse health events.

Using an event study approach, we examine the economic impacts of hospital admissions in two complementary panel data sets. First, we use 20 years of the Health and Retirement Study (HRS) from 1992-2012 to analyze the impact of hospital admissions on out-of-pocket medical spending, income, and its components for about 10,000 hospitalized adults. Second, we construct a 10-year panel of credit reports (2002-2011) for adults in California with hospital admissions from 2003-2007 to analyze the impact on unpaid medical bills, bankruptcy, access to credit, and borrowing for about 1 million hospitalized adults.

Our primary focus is on non-elderly adults with health insurance (the "insured"). In the HRS these adults are ages 50-64 at the time of their hospital admission (average age 58). In the credit report data they are ages 25-64 (average age 49), although results are similar when restricted to the subset who are ages 50-64 as in the HRS. Additionally, we report a parallel set of analyses for elderly adults (age 65 and older) - all of whom are covered by Medicare - and for uninsured, non-elderly adults ages 25-64 (the "uninsured"). The analysis of the uninsured is limited to the credit report data due to insufficient sample size in the HRS. In both data sets, to focus primarily on health shocks, we restrict our analysis to non-pregnancy-related admissions and to adults who have not had a prior hospital admission for several years preceding the "index" admission.

In each data set, we find compelling visual evidence of sharp, on-impact effects of hospitalizations that in many cases persist - or even increase - over time. For insured adults, we find that hospital admissions increase out-of-pocket medical spending and unpaid medical bills, reduce earnings and income, reduce access to credit and consumer borrowing, and increase bankruptcy. The elderly experience similarly-sized impacts on out-of-pocket medical expenses and unpaid bills, but little or no impact on earnings and (presumably relatedly) on access to credit, borrowing, or bankruptcy. For uninsured adults, we find similar impacts on access to credit and borrowing to our insured sample, but much larger impacts on unpaid bills and bankruptcy.

Our results indicate that non-elderly insured adults in the US face considerable exposure to uninsured earnings risk from hospital admissions. Over the three years post admission, hospital admissions are associated with an average annual decline in labor market earnings of about $7,000, or about 17 percent of pre-admission earnings. By comparison, we estimate average annual out-of-pocket medical spending increases by about $1,000 in the three years post admission. Moreover, while the increase

[1] See http://www.presidency.ucsb.edu/ws/?pid=27123, last accessed July 2, 2015.

in out-of-pocket spending is relatively concentrated in the first year post admission, the decline in earnings appears permanent - indeed, likely increasing over time, at least over the approximately 7 years of post admission earnings we observe. Consistent with an increasing impact on earnings over time, we also find that hospital admissions decrease borrowing in the credit report data.

We estimate that about 30 percent of the earnings decline is insured through offsetting government transfers (particularly Social Security Retirement Income and Social Security Disability Income), and we find no evidence of a spousal labor supply response. Overall, total average, annual household income declines by about 11 percent in the first three years after a hospital admission for the insured non-elderly in the US. By contrast, Fadlon and Nielsen (2015) estimate that in Denmark, health shocks produce comparable (15-20 percent) declines in earnings but much smaller (2-4 percent) declines in income due to the greater role of social insurance.

Thus, while those with health insurance in the US have coverage for a large share of the medical expenses that hospital admissions incur, they have considerably less coverage for the labor market consequences of the hospital admission. A back-of-the-envelope calculation underscores this point. We estimate that health insurance covers over 90 percent of the medical expenses associated with a hospital admission. However, once earnings losses and insurance against such losses are also accounted for, our estimates suggest that only about 80 percent of the total economic consequences (medical expenses plus earnings declines) of a hospital admission in the first year are covered. Over time the share of economic costs covered declines further, since the subsequent labor market consequences loom larger than the continued medical expenses; in the third year post admission, for example, our estimates suggest that insured non-elderly adults have coverage for only about 60 percent of the total economic consequences of the hospital admission.

Our results also suggest that external parties bear an important share of the incremental economic consequences of hospital admissions for adults in the US who lack insurance. We find similar impacts for insured and uninsured adults on borrowing (about a 10 percent decline over four years) and borrowing limits (about a 5 percent decline), but much larger impacts for the uninsured on unpaid bills and bankruptcy. Four years post-admission, a hospital admission is associated with an increase in unpaid bills of about $6,000 for the uninsured, compared to $300 for the insured, and an increase in bankruptcy of 1.5 percentage points for the uninsured, compared to 0.4 percentage points for the insured.

Naturally one must be careful in drawing causal inference about the role of insurance from such comparisons. However, we provide some supportive evidence for a causal interpretation by presenting complementary results from a regression discontinuity (RD) analysis of the impact of the discrete change in health insurance when individuals are covered by Medicare at age 65 (in the spirit of Card et al. 2008, Card et al. 2009, and Barcellos and Jacobson 2015). Our findings complement other recent work suggesting that a large share of the medical costs for the "uninsured" are not, in fact, paid for by the uninsured, and that much of the economic benefits from insurance may accrue to external parties who bear the ultimate economic incidence of unpaid medical bills (Garthwaite et al. 2015; Finkelstein et al. 2015, Mahoney 2015).

More broadly, our paper relates to an existing literature studying the economic consequences of health shocks in the United States. Cochrane's (1991) classic study used panel survey data on food consumption from the Panel Study of Income Dynamics (PSID) to examine the covariance of food consumption changes and various shocks, concluding that individuals are imperfectly insured against illness. A subsequent literature has used the PSID to study the correlation between changes in self-reported health or disability and changes in earnings and (food) consumption (e.g., Charles 2003; Chung 2013; Meyer and Mok 2013), and the HRS to study the correlation between the onset of health problems and changes in income, assets, retirement, and disability (e.g., Cutler et al., 2011; Poterba et al. 2010; Smith 1999). Our analysis in the HRS is similar in spirit to this prior work, but focuses on the relatively sharp event of a hospital admission. By comparison, we know of very little work that, like us, uses rich administrative data and the sharp timing of health events to study the economic consequences of adverse health events in the United States.[2]

Finally, our findings contribute directly to the controversial, high-profile literature on "medical bankruptcies", which has concluded that medical events can explain between 17 and 62 percent of all consumer bankruptcies (Himmelstein et al. 2005, 2009; Dranove and Millenson 2006). Consistent with this "medical bankruptcy" literature, we estimate that hospital admissions are associated with statistically significant increased rates of consumer bankruptcy for non-elderly adults (but not for the elderly). Quantitatively, our estimates imply that hospital admissions are responsible for about 3 percent of bankruptcies for insured, non-elderly adults, and about 5 percent of bankruptcies for uninsured, non-elderly adults.

The rest of the paper proceeds as follows. Section 2 provides a simple conceptual framework in which health shocks can generate both uninsured medical expenses and reductions in wages, and discusses potential impacts on out-of-pocket medical costs, earnings, and credit report outcomes in this setting. Section 3 provides an overview of our data and empirical framework. Section 4 presents our main results from the Health and Retirement Survey on the impact of hospital admissions on out of pocket medical expenses and income. Section 5 presents our main results of the impact of hospital admissions on credit report outcomes. Section 6 discusses some implications of the findings. The last section concludes.

2 Economic framework

We develop a simple economic framework in which health shocks may generate both increases in out-of-pocket medical expenses and reductions in earnings; we will analyze these impacts using data from the HRS on out-of-pocket medical spending, earnings, and income. We also use the framework to help interpret the impact of health shocks on the various financial outcomes we will analyze in credit report data: borrowing, borrowing limits, unpaid medical bills, and borrowing costs.

[2]Indeed, we have been able to identify only three such papers. Morrison et al. (2013) and Gupta et al. (2014) use an event-study type approach to examine the impact of non-fatal automobile accidents in Utah and cancer diagnoses in Western Washington, respectively, on bankruptcy; they are unable to reject the null hypothesis of no effect. In follow-on work, Gupta et al. (2015) also examine the differential impact of cancer diagnoses on bankruptcy and foreclosures across individuals with (cross-sectionally) different pre-diagnosis access to liquidity.

2.1 Model setup

An individual lives for two periods. At the start of period 1, she faces an adverse health event with probability p; in what follows, we superscript outcomes in the state of the world in which the adverse health event has occurred with an S (for sick state), and we use H (healthy state) as superscript when health event has not occurred. After observing the period 1 health shock, she chooses her labor supply (h_t) in each period and her consumption path (c_t) subject to her lifetime budget constraint in order to maximize her state-specific utility (U^H and U^S).

Utility U^J in health state $J \in \{H, S\}$ is given by

$$U\left(c_1^J, h_1^J\right) + \frac{1}{1+\delta} U\left(c_2^J, h_2^J\right).$$

Here, δ is the discount rate. The per-period utility function $U(c_t^J, h_t^J)$ is defined as

$$U(c_t^J, h_t^J) = g(c_t^J) - f(h_t^J),$$

with $g()$ a concave utility function over consumption (c_t) and $f()$ a convex disutility function over hours worked (h_t).

The health event incurs exogenous medical expenses m and exogenously reduces the wage in each period from w_1 and w_2 to $(1-\alpha_1)w_1$ and $(1-\alpha_2)w_2$, with $0 < \alpha_t < 1$.[3] Of course, in principle the individual can choose how much health care to consume following a health shock (and we discuss this briefly in Section 6.1 below); nonetheless, the assumption of exogenous medical expenses seems a reasonable approximation in our empirical setting of hospital admissions. We assume that the total shock is bounded above by total income; i.e., $m + \alpha_1 w_1 h_1^H + \alpha_2 w_2 h_2^H < w_1 h_1^H + w_2 h_2^H$, which is a sufficient condition to ensure that the individual can choose positive consumption in both periods. Health insurance covers a share $\lambda_m \in [0,1]$ of medical costs m and replaces a share $\lambda_\alpha \in [0,1]$ of the reduction in wages in each period. A (weakly positive) insurance premium π is paid in every period and in every health state.

After observing the health shock and the amount of insurance, the individual chooses: (1) hours of work in each period (h_1 and h_2), (2) borrowing or savings in period 1 (b) at the interest rate $r(u,b)$, and (3) what amount of uninsured medical expenses $(1-\lambda_m)m$ to pay, with the remainder $u \leq (1-\lambda_m)m$ as unpaid medical bills.

The cost of borrowing $r(u,b)$ is strictly increasing in borrowing (b) and in unpaid bills (u). Borrowing is also limited by a maximum borrowing limit L. We model L as an increasing function of the present discounted value of maximum total income Y. Specifically, we assume

$$L = \gamma Y,$$

with $0 < \gamma \leq 1$ and $Y \equiv w_1\bar{H} + w_2\bar{H}/(1+r)$, where $\bar{H}$ is the maximum hours an individual can work each period. The parameter γ is a reduced-form representation of the supply side of the credit

[3] We show in Appendix A that our main results obtain in an alternative model where health shocks increase the disutility of hours worked rather than reduce the wage.

market, which may not let individuals borrow all the way up to their "natural borrowing limit" (e.g., Ljungqvist and Sargent 2004).

Finally, it is useful to define total income in each state:

$$y_t^H = w_t h_t^H$$
$$y_t^S = (1-(1-\lambda_\alpha)\alpha_t)w_t h_t^S.$$

The individual chooses h_1, h_2, b, and u to maximize utility subject to the state-specific budget constraints. These choices are associated with the following consumption choices in each health state and time period:

$$\begin{aligned} c_1^S &= y_1^S - \pi - (1-\lambda_m)m + u + b^S \\ c_2^S &= y_2^S - \pi - \left(1 + r(u, b^S)\right) b^S \\ c_1^H &= y_1^H - \pi + b^H \\ c_2^H &= y_2^H - \pi - \left(1 + r(0, b^H)\right) b^H. \end{aligned} \tag{1}$$

We also impose some additional technical conditions which we discuss in more detail in Appendix A. These conditions ensure interior solutions for b and u.

2.2 Impact of health shocks

We use Δ to compare outcomes when sick to outcomes when healthy (e.g., $\Delta b = b^S - b^H$, $\Delta y_1 = y_1^S - y_1^H$). We consider the impact of a health shock that is not "fully covered", by which we mean one with $m > 0$, $\alpha_1 > 0$, $\alpha_2 > 0$, $\lambda_m < 1$, and $\lambda_\alpha < 1$. These conditions imply that $(1-\lambda_m)m + (1-\lambda_\alpha)(\alpha_1 w_1 h_1^H + \alpha_2 w_2 h_2^H) > 0$.[4]

Proposition 1. *A health shock that is not fully covered generates $\Delta c_1 < 0$, $\Delta c_2 < 0$, $\Delta U < 0$, and $\Delta u > 0$; the signs of Δb, Δr, ΔL, Δy_1, and Δy_2 are ambiguous, but $\Delta b \neq 0$ and/or $\Delta r \neq 0$ and/or $\Delta L \neq 0$ and/or $\Delta y_1 \neq 0$ and/or $\Delta y_2 \neq 0$ reject full coverage.*

Proof. See Appendix A. □

Proposition 1 says that individuals who experience a health shock that is not fully covered will experience a decline in utility and consumption when sick; this is an intuitive result based on objects we do not directly observe. More usefully, Proposition 1 says that we can reject the null of full coverage through changes in outcomes we can observe or proxy for: income (y_1 and y_2), credit limits (L), borrowing (b), unpaid medical bills (u), and interest rates (r). A change in any of these outcomes following a health shock implies a rejection of full coverage because with full coverage ($\lambda_m = \lambda_\alpha = 1$), health shocks do not change either the level or time profile of wages or lifetime resources, and hence do not change labor supply choices, income, borrowing behavior, borrowing costs, or unpaid bills.

[4] For ease of exposition, our definition implies that $\lambda_m = \lambda_\alpha = 1$ provides "full coverage". Naturally equating consumption across states is not equivalent to full insurance (equating marginal utility of consumption across states), as the marginal utility of consumption may vary with health (Finkelstein et al., 2013).

Without full coverage, unpaid bills increase as they are 0 mechanically when healthy, and will be strictly positive when sick by the envelope theorem. While interest rates are increasing in u, the effect on interest rates is ambiguous because Δb is ambiguous and r depends on both u and b. The change in borrowing limits (ΔL) is also ambiguous because Δr is ambiguous.

More interestingly, Proposition 1 says that the sign of the impact of a health shock on borrowing and on earnings is a priori ambiguous. The intuition for why Δb could be of either sign without full coverage is more easily seen in an alternative simplified setting in which individuals cannot forgo paying medical bills ($u = 0$), interest rates are exogenously fixed at the discount rate ($r = \delta$), there are no insurance premiums ($\pi = 0$), and the borrowing limit is equal to available income ($\gamma = 1$). In this simplified case, solving the agent's optimization problem yields the following closed-form expression for the change in borrowing (see Appendix A for derivation):

$$\Delta b = \frac{1}{1+(1+r)} \left(\underbrace{(\Delta y_2 - \Delta y_1)}_{Relative\ change\ in\ income} + \underbrace{(1-\lambda_m)m}_{Uninsured\ medical\ expenses} \right). \tag{2}$$

Equation (2) shows that the sign of Δb depends on the importance of the uninsured medical cost shock, $(1 - \lambda_m)m$ compared to the relative income change, $(\Delta y_2 - \Delta y_1)$. Increases in out-of-pocket medical spending tend to increase borrowing, while declines in future income tend to decrease borrowing. Thus borrowing is more likely to decline following a health shock when uninsured wage shocks are more important relative to uninsured medical cost shocks, and when the resultant income decline grows over time. Indeed, if the health event only creates an uninsured medical cost shock (i.e., $m > 0$, $\lambda_m < 1$, and $\alpha_1 = \alpha_2 = 0$), this will increase borrowing ($\Delta b > 0$) because the individual will borrow from the future to smooth consumption across the two periods when faced with uninsured medical expenses in period 1. For borrowing to decline following a health shock, the income decline needs to be larger in later relative to earlier periods, so that the individual now wants to move consumption to later periods. Evidence of the impact of the health shock on borrowing will therefore complement our direct estimates of the impact of the health shock on out-of-pocket medical spending and income.

The intuition behind the ambiguous sign of Δy_1 and Δy_2 is similar. The health shock is both a negative shock to unearned income (uninsured medical expenses) and a negative shock to the wage in each period. If the health shock is primarily a medical expenses shock, then the negative wealth effect will tend to increase hours and (if wages don't change by very much) this will increase total labor income. Alternatively, if out-of-pocket medical expenses are small and wages are reduced by a lot, then this will decrease total labor income, although hours can either increase or decrease depending on the relative importance of income and substitution effects in labor supply in response to a health shock. We describe this trade-off more formally in Appendix A.[5]

[5] Specifically, under the additional assumption that wages are the same in both periods and decline by same amount

3 Data and Empirical Framework

3.1 Data

We analyze the impact of hospital admissions (the empirical analog of the "adverse health shock" in the model) using two complementary data sets to analyze many of the outcomes in Proposition 1. We analyze 11 bi-annual survey waves from 1992 through 2012 of the Health and Retirement Study (HRS), a nationally representative panel survey of the elderly and near-elderly in the United States.

We also analyze a sample of individuals discharged from hospitals in California between 2003 and 2007 whom we linked annually to their January credit reports from 2002-2011. We also link these individuals to information on all of their California hospitalizations between 2000 and 2010 and to mortality data (both in and out of hospital) from California vital statistics through 2010.[6] For confidentiality reasons, all of these analyses were conducted on a non-networked computer in the Sacramento office of California's Office of Statewide Health Planning and Development (OSHPD).

We provide a brief overview of the sample definition and key variables here. Appendix B provides considerably more details.

3.1.1 Analysis samples

In both data sets, to try to focus on health "shocks" we restrict attention to non-pregnancy related hospital admissions for individuals who have not had a recent hospital admission. In the HRS, we identify the survey wave in which the individual first reports having had a hospital admission over the last two years (hereafter, the "index" admission), and require that we observe the individual in the previous bi-annual interview without reporting an admission over the last two years; the index hospital admission, therefore, on average represents the first hospital admission in at least 3 years. In the California discharge data, we restrict attention to individuals who have not had a prior hospital admission in the three years preceding their index admission.

Our primary focus is on non-elderly adults with health insurance who had a hospital admission. In the HRS our non-elderly sample is 50-64 at admission; in the credit report analysis they are 25-64

(i.e., $w_1 = w_2$ and $\alpha_1 = \alpha_2$), we formally derive the following expression for the sign of change in income:

$$sign(\Delta y_1) \quad = \quad sign\left(\underbrace{(-\varepsilon^I)\frac{(1-\lambda_m)m}{1+(1+r)}}_{Uninsured\ medical\ expenses} \quad - \underbrace{(1+\varepsilon_{h,w})y_1^H((1-\lambda_\alpha)\alpha_1)}_{Wage\ change} \right)$$

where $\varepsilon^I = d(wh)/dm$ is the effect of wealth (and/or unearned income) on labor earnings and $\varepsilon_{h,w} = dlog(h)/dlog(w)$ is the uncompensated labor supply elasticity. Since the wealth effect is negative, the first term in the expression is the *increase* in labor income from uninsured medical expenses. The second term is the *decrease* in labor income from the decline in wages; the magnitude of this earnings decline depends on the uncompensated labor supply elasticity. The sign of the uncompensated labor supply elasticity ($\varepsilon_{h,w}$) is ambiguous and depends on the relative strength of income and substitution effects; however, $(1+\varepsilon_{h,w})$ is always positive given our assumptions on $g()$ and $f()$ described above (Keane 2011). Overall, the formula shows that labor income will decline ($\Delta y_1 < 0$) as long as the net-of-insurance change in wages ($(1-\lambda_\alpha)\alpha_1$) is large enough so that the earnings change from the decline in wages outweighs the labor supply response from the negative wealth shock coming from out-of-pocket medical costs ($(1-\lambda_m)m$).

[6] To ensure sufficient sample sizes for important sub-samples, we over-sampled certain types of admissions. In all of our analyses, we weight each individual by the inverse of their probability of being sampled.

at admission, although we also report (similar) results separately for those aged 50-64 at admission. We define an individual in the HRS as "insured" if he reports having private insurance or Medicaid in the interview prior to the one where he reports the index admission. In the California discharge data, we define an individual as "insured" if their primary payer for the index admission is private insurance or Medicaid. In both data sets, we exclude the approximately 15 percent of non-elderly adults on Medicare, because such individuals are disabled and therefore presumably have already had an "adverse health event".

Our baseline sample consists of approximately 4,400 non-elderly insured adults with a hospitalization in the HRS and 380,000 non-elderly insured adults with a hospitalization in the credit report data. We also report a parallel set of analyses in both data sets for the elderly (65 and older), analyzing about 5,800 elderly individuals with a hospitalization in the HRS and about 400,000 in the credit report data. Finally, in the credit report data we also analyze about 150,000 uninsured non-elderly adults with a hospitalization; these are individuals who are 25-64 at admission, whose "expected source of payment" is "self-pay". There is insufficient sample size for analysis of uninsured non-elderly adults in the HRS.[7]

Summary statistics Table 1 presents some basic summary statistics. Column 2 describes the non-elderly insured sample with a California discharge whom we analyze in the credit report data. 85 percent are privately insured, three-quarters are admitted to a non-profit hospital, and about half are admitted through the Emergency Department. The two most common reasons for the index admission (each of which are about 15 percent of admissions) are circulatory system and musculoskeletal conditions (see Appendix Table 2). The index hospital admission lasts an average of 4 days and incurs about $45,000 in list charges (which are notoriously higher than actual payments and thought to be significantly higher than actual costs). It is also associated with subsequent additional health care utilization: one-fifth are re-admitted to the hospital within 12 months and 36 percent are re-admitted within 48 months (see Appendix Table 1). There are also likely associated non-hospital medical expenses; estimates from the MEPS (described in Appendix B.3 and Appendix Table 36) suggest total medical payments in the 12 months post admission of about $18,000, of which $11,000 reflect the index admission, $3,200 reflect non-inpatient medical expenses, and the remainder reflect payments from re-admissions.

The remaining columns of Table 1 show statistics for the other samples. Naturally, the average age at admission for the non-elderly insured is much lower in the credit report sample (49) than in the HRS sample (58). The severity of the health shock, as measured by length of stay or charges, is larger for the elderly that the non-elderly. Importantly for interpreting the empirical findings, insurance status is persistent post-admission for the non-elderly insured but not the uninsured. For those uninsured at the index admission, only about 43 percent of subsequent hospital days over the next four years are uninsured, which may reflect post-admission incentives to take up insurance.

[7] Likewise, there is insufficient sample to analyze consumption in the HRS, which is measured for only a small subset of individuals and survey waves.

3.1.2 Key outcomes

We use the HRS to analyze the impact of a hospital admission on out-of-pocket medical spending ($(1-\lambda_m)m-u$), income (y_t), and several key components of income. Specifically, we examine respondent earnings ($w_t h_t$), and two measures of potential forms of earnings insurance (λ_α): spousal earnings and government transfers (unemployment insurance, social security disability insurance, supplemental security income, and social security retirement income).

All outcomes are derived from self-reports. Out-of-pocket spending is reported for the last two years; income and its components are reported for the last calendar year. We use the CPI to adjust all dollar amounts to 2005 levels (the mid-point of the credit report data), and censor all outcomes at the 99.95th percentile.

We use the credit report data to analyze the remaining key outcomes in the model: unpaid medical bills (u), borrowing (b), borrowing limits (L), and borrowing costs (r). All credit report measures are at the individual, rather than household level.[8] Once again, we censor all the continuous outcomes at the 99.95th percentile to purge the data of extreme outliers.

Our main measures of unpaid bills (u) come from collections - unpaid bills that have been sent to collection agencies for recovery attempts. We analyze both the "number of collections to date" (starting from 2002) and current unpaid collection balances. Usefully, we are able to observe medical and non-medical collection balances separately starting in 2005. In addition, we analyze consumer bankruptcy - specifically whether the individual has filed for consumer bankruptcy at any point back to 2002; this may be viewed as an extreme form of unpaid bills.

We analyze two measures of borrowing (b). Our primary measure ("credit card balances") is total revolving account balances, summed over all open revolving credit accounts the individual may have. We focus on revolving credit because we suspect it corresponds most closely to the function of b in the model; that is, the source of the marginal dollar borrowed in response to a health event. We also analyze balances for automobile installment loans, which are another major source of loans and may also be a proxy for motor vehicle consumption (e.g. Agarwal et al., 2015b).

Finally, we analyze two components of "access to credit": borrowing limits (L), and interest rates (r). We proxy for total borrowing limits (L) based on the individual's total credit limit across all open revolving accounts. We use the individual's credit score to proxy for the interest rate (r) faced by individuals. Credit scores are well-known determinants of individual borrowing costs (e.g. Einav et al. 2013a, Agarwal et al. 2015, Han et al. 2015), with higher credit scores corresponding to lower r. We analyze the VantageScore 2.0 credit scores, which ranges from a worst possible score of 501 to a best possible score of 990.[9]

3.2 Econometric models

We estimate both non-parametric and parametric event study models. The details naturally differ slightly across the two data sets. In particular, in the HRS we analyze bi-annual survey data while in

[8] We are unable to identify or link spouses in either the hospital data or the credit report data.

[9] Prior to hospital admission, about 5 percent of the insured sample and the elderly sample, and 15 percent of the uninsured sample do not have a credit score.

the credit report data we analyze the annual outcome data in terms of months relative to admission. At a broad level, however, they are quite similar.

3.2.1 Non-parametric event study

We analyze the coefficients on various indicator variables for time relative to the event ("relative time"). The primary advantage of this non-parametric event study is that it allows us to visually (and flexibly) assess the pattern of outcomes relative to the date of hospitalization. The basic non-parametric event study specification takes the form:

$$y_{it} = \gamma_t + X_{it}\alpha + \sum_{r=S}^{r=-2} \mu_r + \sum_{r=0}^{r=F} \mu_r + \varepsilon_{it} \tag{3}$$

where γ_t are coefficients on calendar time fixed effects, X_{it} represents a vector of other potential control variables, and μ_r are coefficients on indicators for time relative to the hospital admission. All analyses allow for an arbitrary variance-covariance matrix at the individual level and include the relevant sample weights. The key coefficients of interest are the pattern on the μ_r's which estimate the outcome at a given r relative to the omitted category μ_{-1}.

The identifying assumption behind these event study analyses is that conditional on having a hospital admission during our observation window and the included controls, the timing of the admission is uncorrelated with the outcome. One way this assumption would be violated is if there were an individual-specific component of the error term that is correlated with the timing of hospitalization; as a result, we report robustness to an alternative specification with individual fixed effects (which requires an additional normalization due to the collinearity of admission cohort, calendar time, and event time).

Another way the identifying assumption would be violated is if there are time-varying shocks that are correlated with both the timing of hospital admission and y_{it}; for example, if a negative economic shock - such as the loss of a job - caused health to deteriorate, and also had an independent (direct) effect on the economic outcome y_{it}. The relatively sharp information on the timing of the event and the relatively high frequency measurement of outcomes (particularly in the credit report data) help mitigate concerns about underlying, slow-moving secular trends for the individual that separately affect both economic and health outcomes; our restriction to individuals experiencing their first hospitalization in the last three years is likewise designed to mitigate the likelihood that individuals are on a downward trend prior to the hospitalization. We examine patterns in outcomes in the months leading up to the hospitalization to help assess the validity of the identifying assumption. Attrition - which in our setting occurs primarily because of mortality - poses yet another potential threat to our identifying assumption, and we show below that our results are robust to alternative specifications designed to address potential attrition concerns.

HRS specification In the bi-annual HRS data, event time r refers to the survey wave relative to the survey wave in which the index hospital admission is reported to have occurred in the last two years ($r = 0$). The $r = 0$ interview therefore occurs, on average, one year after the index admission.

We analyze up to three waves prior to the index admission ($S = -3$) and three waves post index admission ($F = 3$); the omitted category (μ_{-1}) reflects an interview conducted, on average, one year prior to the index admission. Our baseline specification includes bi-annual survey wave indicators that control for calendar time (γ_t) and, as additional covariates (X_{it}), a series of "HRS cohort" by wave dummies, because of the changes in sample composition over time as the HRS added additional birth cohorts for study (see Appendix B.1.1 for details). In some of the robustness analysis, we include individual fixed effects, in which case we omit an additional survey wave fixed effect.

Credit report specification In the annual credit report data, we observe each individual's credit report outcomes in January of each year. However, because individuals are admitted to the hospital in different months within the year, we can define event time r as the number of *months* relative to the hospital admission (which occurs at $r = 0$). Our baseline specification limits the sample to relative months -47 ($S = -47$) through 72 ($F = 72$). The omitted category (μ_{-1}) is the month prior to hospitalization. The γ_t are coefficients on calendar year fixed effects, and there are no additional covariates (X_{it}) in the the baseline specification. Because this is a slightly non-standard setup (involving monthly analysis of annual data) we discuss identification in more detail in Appendix C; we also describe there the additional normalizations required when we include individual fixed effects in some of the robustness analysis.

3.2.2 Parametric event study

We use the parametric event study to summarize the magnitude of estimated effects and their statistical significance. Our choice of functional form is guided by the patterns seen in the non-parametric event studies. In the figures below, we superimpose the estimated parametric event study on the non-parametric event study coefficients which allows for a visual assessment of our parametric assumptions.

HRS specification In the HRS, our baseline specification is:

$$y_{it} = \gamma_t^{'} + X_{it}\alpha^{'} + \delta r + \sum_{r=0}^{r=3} \mu_r^{'} + \varepsilon_{it}^{'}. \tag{4}$$

Equation 4 allows for a linear pre-trend in event time r (i.e., between bi-annual waves of the HRS). The key coefficients of interest, the $\mu_r^{'}$'s, show the change in outcome following an index admission relative to any pre-existing linear trend (δ). As before, we include "HRS cohort" by wave dummies as additional covariates (in X_{it}).

Credit report specification In the higher-frequency credit report data, we again allow for a linear pre-trend in event time r (now months before/after admission), but now impose a a cubic spline in post-admission event time:

$$y_{it} = \gamma_t^{''} + \beta_1 r + \beta_2 r^2 \{r > 0\} + \beta_3 r^3 \{r > 0\} + \beta_4 (r - 12)^3 \{r > 12\} + \beta_5 (r - 24)^3 \{r > 24\} + \varepsilon_{it}^{''} \tag{5}$$

Equation (5) allows for the second and third derivative of the relationship between outcome and event time to change after the event ($r > 0$), and for the third derivative to change further 12 months after the event ($r > 12$) and 24 months after the event ($r > 24$). The key coefficients of interest - β_2 through β_5 - allow us to summarize the change in outcome following an index admission relative to any pre-existing linear trend (β_1).

4 Impacts on Out-of-Pocket Medical Expenses and Income

4.1 Non-elderly insured

Figure 1 shows the impact of hospital admissions for insured non-elderly adults on out-of-pocket spending, earnings, spousal earnings, government transfers, and total household income. Out-of-pocket spending has a look-back period of the last two years, while earnings and income refer to the prior calendar year. For each outcome, we plot the estimated coefficients on event time (μ_r's) from the non-parametric event study regression (equation (3)), and the estimated pre-admission linear relationship between outcome and event time (δ) from the parametric event study regression (equation (4)).

Panel A of Table 2 summarizes the implied average annual effects of the hospital admission 1 year and 3 years after the index admission based on the estimates from the parametric event study regression; Appendix Table 5 reports the raw coefficients from this regression.

Out-of-pocket spending and earnings The impact of hospital admissions on out-of-pocket spending and earnings is visually apparent "immediately" (i.e., one year after the hospital admission), and persists in subsequent years. The figures suggest that a linear trend fits the pre-hospital admission trend remarkably well, presumably reflecting the fact that adverse health is one of the main forms of idiosyncratic variation in medical expenses and labor market activity for insured adults age 50-64.

Because of the survey design, it is not straightforward to read the time pattern of the impact of hospital admissions off of the raw, non-parametric event study coefficients. Roughly speaking, to make comparisons of the non-parametric estimates at different post-admission years, the estimates 1-year post hospital admission should be doubled. To be more precise, we calculate implied effects at different time periods post-admission based on the parametric event study coefficients; the inputs to these calculations are described in Appendix B.1.2.

A hospital admission increases average annual out of pocket spending by \$1,091 (standard error = \$126) in the three years post admission. The impact on out-of-pocket spending in the first year post admission (\$2,115, standard error = 186) is almost four times the impact in the third year post admission (\$580, standard error = 118). The fact that the hospital admission continues to have a statistically significant (albeit substantially smaller) impact on out-of-pocket spending in subsequent years likely reflects the fact that, as discussed above, the index hospital admission is associated with increased future medical expenses, as well.

A hospital admission reduces average annual earnings by \$7,206 (standard error = \$2,390) in the three years post admission. This represents about a 17 percent decline in average annual earnings relative to pre-admission average annual earnings.[10] The point estimates suggest that the impact of hospital admissions on earnings grows over time, although the estimates are not statistically distinguishable. For example, we estimate that a hospital admission decreases earnings by \$6,124 (standard error = \$2,701) in the first year post admission, and by \$7,931 (standard error = \$2,353) in the third year post-admission.

We examined some components of the earnings decline (see Appendix Table 8 and Appendix Figure 2). We focus our discussion on average annual effects 3 years post admission. A hospital admission decreases annual hours by about 240 (standard error = 39.7), or 16 percent relative to the pre-admission average.[11] At least some of the declines in hours and earnings happen through the extensive margin: a hospital admission decreases the probability of having any earnings by 11 percentage points (standard error = 1.5), or 14 percent relative to the pre-admission fraction with any earnings. Hospital admissions are also associated with a net exit from full-time work of 8.9 percentage points (standard error = 1.8) with little or no net impact on working part time or being unemployed, disabled, or not in labor force. Much or all of the reduction in full-time work represents transition to retirement; self-reported retirement increases by 7.5 percentage points (standard error = 1.5) and self-reported partial retirement increases by 1.7 percentage points (standard error = 1.0). Consistent with the declines in the full-time work reflecting the consequences of a hospital admission, hospital admissions are associated with a 5.2 percentage point (standard error = 1.7) increase in the portion of people who report that their ability to work for pay is limited by health.

Earnings insurance Total household income may fall by more or less than earnings, depending in part on the response of spousal earnings and government transfers. There is no statistical or substantive evidence of a response of spousal earnings.[12] There is evidence of an increase in average annual government transfers of \$1,951 (standard error = \$276) three years post admission; roughly three-quarters reflects increased Social Security Retirement Income Payments, with the rest from increased Social Security Disability Insurance Payments (see Appendix Table 9 and Appendix Figure 3). Total average annual household income falls by \$10,010 (standard error = \$4,606).[13]

Overall, about 30 percent of the earnings decline from a hospital admission is "insured" through

[10] Our earnings measure includes both labor market earnings and self-employment income, although it may undercount self-employment income that instead gets classified as "business or capital income" (see Appendix B.1.2 for more details). In Appendix Table 11 we show that the decline in earnings primarily reflects a decline in labor market earnings but there is also some evidence of a decline in measured self-employment income.

[11] We find no evidence of a change in log wages conditional on working, but the estimates are imprecise and would be difficult to interpret regardless because of potential compositional effects.

[12] Results are similar if we restrict to the three quarters of individuals who had a spouse in the survey wave prior to the hospital admission (see Table 1 column 1). We might expect spousal earnings to increase due to the income effect from the decline in respondent earnings, or to decline if spousal leisure is a complement to poor health. Consistent with the presence of such offsetting effects, Fadlon and Nielsen (2015) find in Denmark that spousal earnings increase substantially following a spouse's death, but exhibit a (statistically significant but economically modest) decline following a spouse's severe - but non-fatal - health shock.

[13] Total household income appears to fall by more than earnings, despite the offsetting government transfers. As we show in Appendix Table 7 (and Appendix Figure 1), this reflects statistically insignificant declines in "household business and capital income" and "other household income". As we discuss in Appendix B, some self-employment income may in fact be reported as business income, so that our baseline measure of earnings may well understate the decline in earnings for the self-employed.

government transfers. As a result, the 17 percent decline in earnings translates into about an 11 percent decline in income.[14]

Identifying Assumption and Robustness An implication of the identifying assumption is that there should be no trend in outcomes in the period leading up to the hospital admission. Our estimates indicate a pre-admission rise in annual out-of-pocket spending of about $50 per year, and a pre-admission decline in annual earnings of about $500 per year, which may reflect a gradual decline in health preceding the hospital admission. Neither pre-admission trend is statistically significant (see Appendix Table 5).

In the robustness analysis (which we present in detail in Section D) we present results for a number of alternative specifications; we find these generally reassuring. In particular, we explore alternative specifications which allow for weaker identifying assumptions by including additional covariates or individual fixed effects. In addition, to investigate the time pattern of results more carefully as well as address potential concerns on attrition, we estimate results on a balanced panel. We also report specifications that, unlike our baseline, include individuals who may have had a hospital admission within the 3 years prior to their index admission. Finally, given the high variance and right-skewness of earnings and income measures, we confirm that a proportional model (specifically, a quasi-maximum likelihood Poisson model) produces quantitatively similar proportional estimates, as does a model of log household income.

4.2 The elderly

We conducted a parallel set of analyses for elderly individuals with a hospital admission. The average age at admission for this sample is 75 (see Table 1, column 3). Figure 2 shows the results graphically; Table 2 Panel B summarizes the estimated effects; Appendix Table 6 reports the estimated coefficients directly. A parallel set of robustness analysis is presenting in Appendix D.

In the three years following a hospital admission, average annual out-of-pocket spending for the elderly increases by $675 (standard error = $120). This is slightly smaller than, but statistically indistinguishable from, the impact for 50-64 year olds. A similar impact on out-of-pocket spending is consistent with our back-of-the-envelope calculations from the Medical Expenditure Panel Survey (see Appendix B.3) that hospital admissions generate similar total medical spending for the elderly and the non-elderly insured, and that cost-sharing is also similar for these two groups. This is consistent with the elderly - all of whom are covered by Medicare and some of whom have supplemental insurance as well - having similar consumer cost-sharing to the non-elderly insured.

In contrast to the results for the non-elderly insured, there is no evidence of an impact of a hospital admission on earnings for the elderly. This is not surprising, given much lower labor force participation among the elderly. For example, less than 25 percent of the elderly report positive earnings in the

[14] In Table 2, the estimated level decline in total annual household income (column 5) corresponds to an 11 percent decline relative to average pre-admission total annual household income. We estimate a similar percentage decline in total household income if we estimate the impact of hospital admissions on log household income (see Appendix Table 7; here, we add 1 to the total household income variable before taking logs to deal with the 0.6 percent of the sample with zero household income (see Appendix Table 4).

survey wave prior to their hospital admission, compared to over three-quarters of the 50-64 year olds (Appendix Table 4).[15]

5 Impacts on Credit Report Outcomes

5.1 Non-elderly insured adults

Figures 3 and 4 present the event study analyses graphically for our main outcomes: collections, bankruptcy, credit limits, credit scores, credit card borrowing, and automobile loans. Once again, we plot the estimated coefficients on event time (μ_r's) from the non-parametric event study regression (equation (3)), and the estimated pre-admission linear relationship between outcome and event time (δ) from the parametric event study regression (equation (5)).[16] Tables 3 and 4 (panel A) summarize the implied effects of the hospital admission (from equation (4)) at 1 year and 4 years after the index admission. Appendix Table 13 reports the estimated coefficients directly.

Unpaid bills and bankruptcy There is a clear "on impact" effect of hospital admissions on collections (number and balances). Four years later, a hospital admission is associated with an increase in total collection balances of $302 (standard error = 37) or about 25 percent relative to pre-admission balances. The effect is most pronounced for medical collections, although there is some evidence of a smaller increase for non-medical collections, which may in fact reflect an increase in mis-classified medical collections.[17] The effect on medical collections increases initially over time and then appears to flatten out after about two years. This makes sense, since firms usually make several attempts over multiple months to get payment on a bill before sending it to a collection agency.

Hospital admissions are also associated with a statistically significant increase in consumer bankruptcy.[18] Four year later, a hospital admissions is associated with an increase in the probability of bankruptcy of 0.4 percentage points, or about 33 percent relative to the annual bankruptcy rate of 1.2 percent in this population.

[15]There is some puzzling evidence of a decline in total household income for the elderly that is borderline statistically significant (p-value = 0.08) and appears to be driven by declines in household business and capital income and "other income" (see Appendix Table 7). However, this estimated decline in total household income it is not robust to alternative specifications, such as analyzing log household income (see Appendix Table 7) or including individual fixed effects (Appendix Table 10).

[16]For many of the outcomes, there is visual evidence of a cyclical pattern to the non-parametric event study coefficients. The pattern is particularly pronounced post hospitalization, but also visible pre admission for some outcomes. This appears to reflect systematic variation in our sample by admission month since, recall, we observe each individual once every 12 months. The fact that that pattern is more pronounced post-hospitalization and (as we will see in the robustness analysis below) is usually still present after the inclusion of individual fixed effects suggests that the variation across admission months primarily reflects variation in treatment effect rather than mean outcome levels. Thus, the point estimates from our spline regressions should be viewed as an average of the impact of hospitalization across the groups admitted to the hospital in different months.

[17]While we can be fairly confident that "medical" collections reflect unpaid medical bills, the converse is less clear. Non-medical collections may reflect non-payment of non-medical bills (such as utility bills). But they may also reflect unpaid medical bills; for example, a medical bill that is charged to a credit card whose balances are then not paid would show up as a non-medical collection.

[18]We informally interpret consumer bankruptcy as an extreme case of "unpaid bills". For a formal model of personal bankruptcy, see Wang and White (2000).

Borrowing and access to credit Four years later, hospital admissions are associated with a decline in credit card balances (our primary proxy for borrowing b) of $1,208 (standard error = $253) - or about 9 percent. Automobile loan balances also decline in the four years post admission - by $507 (standard error = $71), or about 7 percent. The decline in borrowing is consistent with the persistent decline in future income following a hospital admission estimated in the HRS.

Hospital admissions are also associated with declines in access to credit. Four years after admission, credit limits have declined by $2,215 (standard error = $440), or about 5.5 percent relative to pre-admission levels - and credit scores by 1.8 (standard error = 0.5) - or about 0.2 percent - although the visual evidence for credit scores is not particularly compelling.[19]

The decline in credit limits is likely more consequential than the decline in credit score. The decline in credit limits following a hospital admission is over half the the decline in credit limits following an unemployment spell,[20] while our back-of-the-envelope calculations suggests that the increase in credit score may be associated with an increase in interest rates of less than 0.054 percentage points.[21] A larger impact of hospital admissions on borrowing limits (L) than interest rates (r) is consistent with our theoretical model in which the effect of a hospital admission on r was theoretically ambiguous due to two opposing forces: hospital admissions increase unpaid bills (u), which should serve to increase r, but also decrease b which should serve to decrease r.[22]

Heterogeneity We explored heterogeneity in the impacts of hospital admissions across different sub-samples of individuals and types of hospitalizations. Results are shown in Appendix Tables 17 through 19, and Appendix Figures 10 through 24. We find smaller impacts of a hospital admission for those on Medicaid than those with private insurance, which may reflect the lower labor force attachment for those on Medicaid; consumer-cost sharing is similar for these groups.[23] There is some evidence of larger impacts for admissions for chronic diseases and for admissions with higher predicted list charges; such admissions may have larger impacts on medical expenses and/or earnings.

To try to focus in on potentially unanticipated hospital admissions, we looked separately at admissions through the ER, and found similar impacts to those not through the ER. We also looked at admissions for particular conditions that may be less likely to be anticipated, such as heart attacks, car accidents, and external injuries; in some cases the samples get quite small, but there is no obvious pattern of differential effects for less anticipated admissions. Results also look similar for admission

[19]Since 96 percent of the sample has a credit score prior to hospitalization, we examined the impact of hospital admissions on the probability of having a credit score (see Appendix Figure 9 and Appendix Table 16). A hospital admission is associated with a statistically significant decline of 0.25 percentage points in the probability of having a credit score after 48 months.

[20]Bethune (2015) examines people who lose their job between 2007 and 2009, and estimates that unemployment is associated with a decline in credit card limits of $925 by 2009. By comparison, we estimate that a hospital admission associated with a $500 decline in credit limits 12 months later.

[21]Recent estimates suggest that, on average, a 100 point decline in credit score is associated with an increase in interest rates (r) of 100 to 300 basis points (Agarwal et al. 2015a, Han et al. 2015).

[22]The larger effect on credit limits may also reflect differences in how these instruments are used as screening devices for borrowers; indeed, consistent with our findings, Agarwal et al. (2015a) find that credit card companies will often impose large changes in borrowing limits without meaningful changes in interest rates as a function of credit score.

[23]In the 2000-2011 CPS, we estimate labor force participation rates in California of 85 percent and 40 percent for the privately insured and Medicaid recipients, respectively. In the 1999-2010 MEPS, we estimate only slightly lower consumer cost sharing for those covered by Medicaid (6.7 percent compared to 8.8 percent for the privately insured)

across different types of hospitals (public, non-profit and for profit), and for the five most common reasons for admission.

Finally, Appendix Table 20 moves beyond mean impacts to examine results from unconditional quantile regressions on the distribution of five continuous outcomes: total collection balances, credit limit, credit score, credit card balances, and automobile loan balances. Many of these are highly skewed variables (see Appendix Table 12). In general, the impacts at the 75th percentile are fairly similar to mean effects, and the 90th percentile impacts are often between two and three times larger than the mean impacts.

Identifying assumption and robustness Once again, we can look at the trends in outcomes leading up to the hospital admission as one way of assessing the identifying assumption. For some outcomes - such as collection balances, credit card borrowing, and credit limits - the pre-trends appear negligible. However, for others - particularly bankruptcy and credit score - they are quite pronounced.

In the robustness analysis (which we present in detail in Section D) we estimate a number of alternative specifications and in general find the results reassuring. These include many of the ones we explored in the HRS (with similar motivation) including individual fixed effects, balanced panels, including the approximately 15 percent of individuals who were excluded from the baseline analysis because they had a hospital admission within the 3 years before their index admission, and a proportional (quasi-maximum likelihood Poisson) model. As an additional check on potential attrition bias in these data, we also present results separately for the bottom quartile of predicted mortality.

5.2 The elderly and the non-elderly uninsured

We conducted a parallel set of analyses for elderly individuals and for uninsured non-elderly admissions. The results for the elderly are shown graphically in Figures 5 and 6, and for the uninsured in Figures 7 and 8. Implied effects are summarized in panels C and B, respectively, of Tables 3 and 4; estimated coefficients are presented directly in Appendix Tables 14 and 15. A parallel set of heterogeneity analysis is shown in Appendix Tables 21 and 22 for the elderly, and 24 and 25 for the uninsured. A complete set of robustness analyses for both sub-samples is presented in Appendix D.

Elderly For the elderly, the results suggest similar proportional (smaller absolute) impacts on collection outcomes as for the non-elderly insured, and limited or no impact - either visually or in the estimated implied effects - on other outcomes. In particular, there is no evidence of an impact on bankruptcy or credit limits; the point estimates are usually wrong-signed and substantively small compared to estimates for non-elderly adults. There is no evidence of a decline in credit card borrowing, and weak evidence of a small increase in automobile loans. There is a decline in credit score following a hospital admission that is similar in magnitude to the quantitatively trivial estimate for the non-elderly insured.

Uninsured For the uninsured, non-elderly, we find much larger impacts on collections and bankruptcy than for the insured non-elderly. For example, four years later, a hospital admission is associated with an increase in collection balances of $6,199 (standard error = $130) for the uninsured, compared to $302 (standard error = $39) for the insured. The right tail effects are also much larger for the uninsured, for example, the 90th percentile impact on collection balances is $23,000 for the uninsured, compared to $600 for the insured (see Appendix Tables 20 and 26 for quantile regressions). The impact on bankruptcy - another "tail" outcome - is also larger for the uninsured; a hospital admission is associated with a 1.4 percentage point (standard error = 0.14) increase in bankruptcy over four years, compared to a 0.4 percentage point increase for the insured (the pre-hospitalization annual bankruptcy rate is similar at about 1.2 percent).

By contrast, the four-year impacts on the other outcomes - credit card balances, credit limits, and automobile balances - are similar proportionally (and smaller in absolute terms) for the uninsured relative to the insured.[24] For example, the decline in credit limits is about 5 percent for each group, and the decline in borrowing about 9 percent; we find similar results when we estimate a proportional model directly (see the Poisson regression results in Appendix Tables 27 and 28).[25] As would be expected, the proportional impacts on these outcomes are relatively larger for the uninsured compared to the insured in the shorter run (i.e., one year after admission) - when medical expenses are a larger component of the total economic cost of the hospital admission- than in the longer run (i.e., four years post admission).

6 Implications

6.1 Coverage for the "insured"

The results suggest that the non-elderly insured still face considerable economic risk from hospital admissions, with the primary source being uninsured earnings consequences rather than uninsured medical expenses.In the first three years post-admission we estimate an average annual earnings decline about 17 percent of pre-hospitalization earnings. This earnings decline is similar in magnitude to estimates of earnings losses from job displacement (e.g., Jacobson, Lalonde, and Sullivan 1993, Sullivan and von Wachter 2009). The earnings decline appears permanent over the seven post-admission years we can analyze - indeed the point estimates suggest the impacts are increasing over time - and large relative to the (shorter run) increase in out-of-pocket medical spending.

The results from the credit reports complement and enrich this analysis. The post-admission decline in borrowing for the non-elderly insured that we estimate in the credit report data is (as discussed in Section 2) consistent with hospital admissions having an impact on income that is increasing over time

[24] The results for the uninsured on credit scores (Table 4 column 3 and Figure 8) are somewhat puzzling - suggesting a similar proportional decline to the insured at 12 months but a statistically significant *increase* at 48 months. However, given the potential endogeneity of presence of a credit score, we urge some caution in interpreting these results. As noted above, only 84 percent of the uninsured sample has a credit score prior to hospitalization. We find that a hospital admission is associated with a statistically significant decline of 0.85 percentage points in the probability of having a credit score after 48 months (see Appendix Figure 9 and Appendix Table 16).

[25] The declines for the uninsured may be mechanically dampened by the relatively large share with zero credit limits and credit card balances (50 percent, compared to about 20 percent for the insured; see Appendix Table 12). However, at higher quantiles where such censoring is less of a concern, the pattern of results across quantiles look similar to that for the insured; impacts for the uninsured are similar at the 75th percentile compared to the mean, and estimated effects at 90th percentile are roughly three times larger than the effects at the mean (see Appendix Table 26).

and large relative to the (front-loaded) increase in out-of-pocket medical costs. The credit report and HRS analyses are similarly consistent for the elderly: we find little impact on earnings in the HRS, and, consistent with this, little impact on credit limits or borrowing. These results suggest that the impacts on access to credit and borrowing for the non-elderly insured may in large part reflect the impact of hospital admission on earnings, since the elderly experience similar impacts on out-of-pocket medical spending, but no declines in earnings, access to credit, and borrowing.[26] Indeed, our back of the envelope calculation (described in detail in Appendix E.2) suggests that less than 30 percent of the four-year hospital impacts on credit limits and borrowing for the insured non-elderly can be explained by uncovered medical expenses, and that this number may reasonably be much closer to zero.

Nature and amount of insurance coverage Our findings highlight the nature of insurance against health shocks in the US. Our estimates imply that the impact of a hospital admission on total medical expenses is likely similar to its impact on earnings in the first few years, while over longer horizons the earnings decline is likely substantially larger than increase in total medical expenses.[27] Health insurance in the United States covers over 90 percent of the medical expenses associated with a hospital admission. However our results suggest that less than 30 percent of the earnings decline associated with the hospital admission is covered, so that annual total household income declines by about 11 percent following a hospital admission.

In other words, for those who have it, insurance for medical expenses (λ_m) is fairly comprehensive, while insurance for income declines (λ_a) is substantially less complete. As a result, the insured have less protection against the economic consequences of health shocks than the cost-sharing provisions of their insurance for medical expenses insurance would imply, and the degree of protection is declining over longer time horizons. For example, we estimate in the MEPS that about 92 percent of the medical expenses in the year following admission (including the medical expenses from the index admission itself) are covered by insurance. However, once earnings consequences are accounted for, only about 80 percent of the total economic costs (medical expenses plus earnings decline) of the hospital admission in the first year are covered. In the third year after admission, only about 60 percent of costs are covered, reflecting the growing impact on earnings and the declining impact on medical expenses.[28]

[26] Naturally, there are other differences between the elderly and non-elderly insured adults that could also contribute to the differential impacts of hospital admissions observed in the credit report data - such as the nature of their insurance or the causes of their hospital admissions. However, if anything the health shock itself appears more severe for the elderly (as measured by list charges or length of stay for the index admission in Table 1). Indeed, as we show in Appendix Table 29, when we re-weight the elderly sample to match the non-elderly insured sample on demographics (race and gender) and health conditions (diagnosis codes and length of stay), the results for the elderly become smaller.

[27] We estimate in the MEPS that the average co-insurance for insured non-elderly adults for medical expenses in the year including and following the admission is about 8 percent. Given our estimated annual increase in out of pocket medical spending of about $1,000 in the first three years, this implies average annual total medical expenses (m) associated with the hospital admission of about $12,500 in the first three years. By comparison, we estimate average annual declines in earnings of about $7,000 over the first few years, and these effects, unlike the out of pocket spending effects, do not appear to decline over time.

[28] These calculations are based on estimates of the impact of the admission on out-of-pocket spending, earnings and government transfers at various time periods. Appendix Table 5 presents the underlying parametric estimates and Appendix B.1.2 describes how these are transformed into implied effects at various time periods post admission. We reported implied effects in the first year in Table 2 and Section 4 and implied effects in the third year in Section 4. We assume based on our calculation in the MEPS (see Appendix B.3) that 92 percent of the incurred medical expenses are covered, and we assume based on our estimate in the HRS in Section 4 that 30 percent of the earnings loss is covered.

This stands in marked contrast to Fadlon and Nielsen's (2015) findings for Denmark; they analyze the impacts of non-fatal heart attacks and strokes and find declines in own earnings that are broadly similar to our estimates - about 15 to 20 percent - but only a 2-4 percent decline in household income; spousal labor supply does not provide informal insurance in their setting either, but household income is insured through government social insurance to a much greater degree in Denmark. This underscores the very different nature of insurance against the economic consequences of adverse health events in the two countries.

Welfare implications Of course, the welfare implications of a change in earnings and in out-of-pocket medical spending may not be the same. Suppose that the individual has no control over the size of the total medical cost shock m, but that she endogenously chooses her hours in response to the size of the wage shock ($\alpha_1 w_1$ and $\alpha_2 w_2$). These assumptions correspond to our economic framework in Section 2 and are in the spirit of our empirical strategy based on using hospital admissions as an exogenous shock to medical expenses. In this model, a given change in earnings reduces welfare in inverse proportion to the uncompensated labor supply elasticity, while any out of pocket medical expenses feed through directly (one for one) to welfare reductions.[29] "Consensus" estimates of the elasticity of hours with respect to a permanent, unanticipated change in wages range between -0.2 and 0.5 (Keane 2011). Using the upper bound estimate of 0.5, this suggests that, in the first three years, the welfare consequences of the roughly \$5,000 average annual decline in net earnings (i.e. the \$7,200 decline in earnings net of the \$2,000 increase in government transfers) is more than three times that of the roughly \$1,000 average annual increase in out of pocket medical spending. Moreover, since the net earnings decline appears permanent while the out of pocket spending increase appears front-loaded, we suspect that the relative welfare consequences of the earnings impact may loom larger over larger time horizons. The relative welfare consequences of earnings would also loom larger if - unlike our current model - we allowed some or all of the out-of-pocket spending to be an endogenous choice (involving, for example, a trade-off between the health benefits of medical spending and the foregone utility from non-medical consumption as in Einav et al. 2013b).

Implications for younger, insured adults Naturally, our results speak directly to the earnings and out-of-pocket medical spending consequences of hospital admissions only for non-elderly insured adults aged 50-64 whom we observe in the HRS. A priori, it is unclear whether to expect larger or smaller earnings effects of hospital admissions for younger, insured adults.

Earnings effects of hospital admissions might be smaller at younger ages if the elasticity of labor supply with respect to health shocks is smaller. For example, the substantial exit into retirement that we estimate is presumably more likely at older ages (although the reporting of non-employment as "retirement" is presumably also more common). In addition, the near-elderly have greater potential to access various social insurance programs - particularly Social Security.

[29] As we show in Appendix (A), a first-order approximation to the money-metric change in utility from a health shock is $\frac{\Delta U}{g'(c_1)} \approx \frac{\Delta y_1 + \Delta y_2}{1+\varepsilon_{h,w}} - (1-\lambda_m)m$, where $\varepsilon_{h,w} = dlog(h)/dlog(w)$ is the uncompensated labor supply elasticity and $g'(c_1)$ is the marginal utility of consumption in the first period.

However, there are two countervailing reasons to expect that earnings effects of hospital admissions could be larger at younger ages. First, hours worked are higher and so the impact of a given change in wages, holding behavior constant, is larger.[30] Second, while our stylized model considers only two periods, in practice, the relevant time horizon for potential earnings is larger for younger individuals, so that a given permanent decline in annual earnings would be integrated over a larger number of years of potential earnings.

While we cannot directly examine the impact of hospital admissions on earnings for individuals under age 50, we present two indirect analyses. These show no suggestion of smaller earnings effects of hospital admissions at younger ages. Indeed, if anything, there is suggestive evidence of potentially larger earnings effects at younger ages. First, in the HRS we analyzed the impact of hospital admissions for those aged 50-57 at admission and those aged 58-64. The results, shown in Appendix Figure 39 and Appendix Table 30, indicate similar effects on out of pocket earnings but about a three-fold larger decline in annual earnings for the 50-57 year olds. Second, in the credit report data, we analyzed the impact of hospital admissions separately for the near-elderly insured (ages 50-64) and compared them to the impacts for the full non-elderly insured sample (ages 25-64). These results, shown in Appendix Tables 31 through 33 and Appendix Figures 40 and 41, indicate similar-sized effects of a hospital admission on credit card limits and borrowing. This is consistent similar-sized impacts of the hospital admission on income and out-of-pocket medical spending, although of course there could also be offsetting differences.

6.2 Coverage for the "uninsured"

6.2.1 Differential impacts for insured vs. uninsured non-elderly

The results showed similar impacts of hospital admission on access to credit (i.e., credit limits) and borrowing for the insured and uninsured, with larger impacts for the uninsured limited to impacts on unpaid bills and bankruptcy. Naturally one must exercise caution in interpreting such comparisons as reflecting the causal effect of insurance per se; there may be other underlying differences between the two groups, such as the severity of the health event. To try to adjust for observable differences between the two groups, Appendix Table 29 shows results for the uninsured re-weighted to make the insured sample on demographics (age, race and gender) and health conditions (diagnosis codes and length of stay); this has little effect on the estimates.

To gain greater insight into the causal effects of insurance, we estimated the impact of insurance coverage using a regression discontinuity (RD) strategy based on the discrete change in health insurance when individuals are covered by Medicare at age 65 (in the spirit of Card et al. 2009 and Barcellos and Jacobson 2015). The RD strategy uses arguably more credible identifying variation than the simple difference-in-differences comparison of the impact of admission for insured relative to uninsured. However, it has much lower power, involves a distinct sample of adults, and requires making an assumption about how to define the "first stage" in terms of the change in insurance coverage (which, as emphasized by Card et al. 2009, may not be limited to the observed, extensive coverage margin).

[30] Employment rates are 79 percent for 25-49 year olds compared to 67 percent of 50-64 year olds and 15 percent for individuals 65 plus, according to the 2000-2011 pooled March CPS.

We present the RD results in detail in Appendix E.1. We limit the analysis to admissions for 60 to 70 year olds who are admitted to the hospital through the emergency room (ER); we show that the frequency of admission through the ER appears smooth through age 65, alleviating concerns about compositional effects of insurance on admissions. Both the visual evidence and the point estimates indicate an impact of consumer cost sharing on unpaid medical bills, but no impacts on credit limits or borrowing (although the latter are sufficiently noisy that we are unable to rule out large effects). These results are consistent with our difference-in-differences comparison of the impact of hospital admissions for uninsured and insured non-elderly adults. Quantitatively, the RD estimates imply that the impact of insurance coverage on unpaid bills may be about 75 percent larger than what we estimated above based on the difference-in-difference comparisons, although of course the estimates are based on different populations and so are not directly comparable.

6.2.2 Interpretation

Our results suggest that insurance reduces the impact of hospital admissions on unpaid bills. This is consistent with existing evidence that health insurance reduces measures of financial risk exposure and financial strain, including out-of-pocket medical spending, medical debt, and difficulty paying non-medical bills.[31] The welfare consequences for the patient of having larger unpaid medical bills is, however, less clear. The unpaid medical bills we measure (medical collections) are, for the most part, ultimately never paid (Avery et al., 2003). Increases in unpaid medical bills (u) therefore point to adverse effects on whatever external parties bear the ultimate economic incidence of these unpaid bills, such as charitable care provided by hospitals (e.g., Garthwaite et al. 2015). In our model, any impact of increased u on patient welfare is indirect; an increase in u raises welfare insofar as unpaid medical bills allow for increased consumption following the health shock, and decreases welfare insofar as it increases future borrowing costs r. Of course, in practice, there may also be other unmeasured and un-modeled channels by which u directly affects patient welfare, such as impacts of u on "peace of mind" (Mann and Porter 2010).

Our results also suggest that health insurance does not mitigate the decline in access to credit and borrowing due to a hospital admission. This is consistent with these declines primarily reflecting declines in earnings following a hospital admission, which health insurance does not cover. This interpretation was implied by our framework in Section 2 and consistent with the empirical results for the non-elderly insured and the elderly (see Section 6.1).

Declines in access to credit and borrowing likely reflect negative welfare consequences for the patient. In the framework in Section 2, declines in credit limits are assumed to proxy for declines in earnings potential. More broadly, a welfare-enhancing role for access to credit is a standard feature of many leading household finance models (Chatterjee et al. 2007; Kaplan and Violante 2014). Positive

[31] This literature includes evidence from Medicaid expansions (Finkelstein et al. 2012; Baicker et al. 2013), the Massachusetts health insurance expansion (Mazumder and Miller 2014), the introduction of Medicare (Finkelstein and McKnight 2008), and the introduction of Medicare Part D (Engelhardt and Gruber 2011). Most closely related to the empirical strategy we implement in Appendix E is recent work using the discontinuity in insurance coverage at age 65 when Medicare eligibility begins to examine the impact of Medicare on out-of-pocket spending and medical-related financial strain in survey data (Barcellos and Jacobson 2015).

welfare effects of higher credit limits are also consistent with the high estimated marginal propensity to consume out of liquidity in Gross and Souleles (2002) and the model-based estimates in Telyukova (2013) that point to an important role for demand for liquidity in understanding patterns of credit card usage.[32] Declines in borrowing, in the framework in Section 2, point to large declines in future earnings relative to smaller, transitory increases in out of pocket costs. Outside of this framework, one might also interpret the decline in borrowing as a proxy for consumption declines. This would be the case if credit cards are used primarily as a means of transaction rather than for consumption smoothing, and/or the decline in automobile balances could be interpreted as a decline in automobile consumption (as in Mian et al. 2013 or Agarwal et al. 2015b).

Taken together, therefore, our findings suggest that the economic impact of a hospital admission may be broadly similar for insured and uninsured non-elderly adults, and that a large share of the incidence of lack of insurance may be born by third party payers who ultimately incur the costs of the uninsured's unpaid medical bills. This is consistent with recent work suggesting that the nominally uninsured have a fair amount of implicit, informal insurance, and that a large share of the "uninsured's" medical costs are not, in fact, paid for by the uninsured (Mahoney 2015, Garthwaite et al., 2015, Finkelstein et al., 2015). In this vein, our findings provide some suggestive evidence of the magnitude of the benefits to health care providers from insurance coverage. A simple comparison of four-year impacts suggests that a hospital admission generates about $6,000 more in unpaid bills for the uninsured than the insured; the RD estimates suggest even larger causal effects of insurance on unpaid bills. Of course, unpaid bills may be based on charges (not hospital costs), which complicates the interpretation of the impact of insurance on unpaid bills, since charges (prices) may differ by insurance status.

6.3 Medical bankruptcies

A growing empirical literature examines the impact of various economic shocks on consumer bankruptcy (e.g., Domowitz and Sartain 1999; Sullivan et al. 1999; Fay et al. 2002; Warren and Tyagi 2003; Livshits et al. 2007; Keys 2010). A controversial, high-profile strain of this literature has examined the role of "medical bankruptcies". A study by Himmelstein et al. (2005) interviewing bankruptcy filers regarding the cause of their bankruptcy, found that 54 percent of bankruptcy filers self-reported "medical causes" as the reason for their bankruptcy. Follow-on studies using this same same basic method but varying in their definition of a "medical cause" have estimated rates of "medical bankruptcy" ranging from 17 percent (Dranove and Millenson 2006) to 62 percent (Himmelstein et al. 2009). These findings have attracted a great deal of attention from journalists, politicians, and policymakers (e.g., Obama 2009). However, self-reported "causes" among those who go bankrupt can be difficult to interpret. More promisingly, recent research by Morrison et al. (2013) and Gupta et al. (2014) have

[32] There is also other related work that provides some evidence of the welfare consequences of access to credit. Sullivan (2008) finds that that the negative consumption effects of unemployment shocks are largest for individuals with limited access to unsecured credit, who are not able to increase their borrowing to help smooth their consumption. Herkenhoff et al. (2015) finds suggestive evidence that access to credit causes longer unemployment durations but higher re-employment wages following a job loss. This finding is consistent with higher credit limits raising reservation wages of unemployed workers, which can be a sufficient statistic for welfare in a broad range of job search models (Shimer and Werning 2007).

performed event study analyses of the relationship between an adverse health shock and subsequent consumer bankruptcy, using a census of non-fatal automobile crashes in Utah and cancer diagnoses in 11 counties in western Washington State, respectively. However, both papers are unable to reject the null hypothesis of no causal effect of the medical event analyzed on bankruptcy.

Relative to this existing literature, our results provide evidence of a statistically significant impact of hospital admissions on bankruptcies - for both insured and uninsured non-elderly adults but not for the elderly. However, they suggest that the share of "medical bankruptcies" may be lower than the prior literature has concluded. Four years later, a hospital admission increases bankruptcy rates by 0.4 percentage points for the insured elderly, and 1.4 percentage points for the uninsured elderly; hospital admissions have no effect on bankruptcy for the elderly.

Our estimates imply that hospital admissions are pivotal for about 3 percent of bankruptcies for non-elderly insured adults, and 5 percent of bankruptcies for non-elderly uninsured adults, and do not contribute to bankruptcies for the elderly.[33] This is likely a lower bound on the total number of medically-induced bankruptcies, since it excludes index medical events not associated with a hospital admission. However, hospital admissions (and their sequelae) are likely a major cause of medical bankruptcies. Hospital spending alone is about 40 percent of total medical spending, and among individuals in the top 5 percent of annual medical spending, two-thirds have had a hospital admission in the last year; for those in top percentile of annual medical spending, almost 90 percent had a hospital admission (authors' calculations from MEPS).

We suspect that the driving force behind "medical bankruptcies" for insured non-elderly adults is lost labor market earnings. For the elderly - who do not experience "medical bankruptcies" - have similar insurance coverage and out of pocket medical spending, but no lost earnings. We suspect that the larger impact on bankruptcy for the uninsured non-elderly relative to the insured reflects the larger uncovered medical expenses for the uninsured. This is consistent with results using aggregate data from Gross and Notowidigdo (2011) and Mazumder and Miller (2014) that health insurance reduces the risk of bankruptcy.

7 Conclusion

The United States has recently engaged in a major expansion of public and private health insurance for non-elderly adults. This health insurance covers a substantial portion of medical expenses, but does not provide coverage for potential earnings losses from poor health. Using two complementary panel data sets, we have explored the economic consequences of hospital admissions for non-elderly adults with health insurance, as well as for non-elderly adults without health insurance and for the elderly. Our findings suggest that non-elderly insured adults still face considerable exposure to adverse economic

[33] In the MEPS, we estimate an annual non-childbirth hospitalization rate of 5.7 percent for insured adults, and 2.9 percent for uninsured adults. We estimate a 0.8 percent annual bankruptcy rate for the non-elderly by combining Census population estimates with the distribution of bankruptcy filers by age, which is compiled by the Department of Justice U.S. Trustee Program (www.justice.gov/ust). Since the pre-hospitalization bankruptcy rate is similar in our insured and uninsured samples, we assume that the bankruptcy rate is similar in the overall population of insured and uninsured non-elderly adults, as well. This is consistent with the results in Stavins (2000), which shows that the health insurance rates are similar between bankruptcy filers and non-filers.

consequences of hospital admissions through their impact on labor earnings. They also suggest that the nominally uninsured may face similar economic risks from hospital admissions despite their lack of formal insurance, due to their ability to simply not pay large portions of their medical costs. The elderly - who have health insurance through Medicare for medical expenses and relatively little labor market earnings - appear to suffer little or no economic consequences from hospital admissions.

These are positive, not normative, findings. Additional assumptions are required for drawing inferences about consumer welfare or optimal insurance design. For example, while our results would suggest that hospital admissions are associated with consumption declines for non-elderly adults, if the marginal utility of consumption is lower in poor health (Finkelstein et al., 2013), some decline in consumption is (ex ante) optimal. Moreover, in the presence of moral hazard effects of insurance - on health care utilization and/or labor market activity - the (constrained) optimal level of insurance would not involve fully equating marginal utility of consumption across health states.

Our findings underscore the nature of insurance - and the lack thereof - in the United States. Our estimates suggest that in the first few years, the total medical expense and earnings consequences of a hospital admission are similar for insured adults and that over a longer horizon the earnings consequences loom relatively larger. By design, however, insurance in the US covers (a large portion of) medical expenses and relatively little of the earnings decline. Employer provision of sick pay and private disability insurance is fairly sparse, and public disability insurance is available only after a lengthy application and approval process (Autor et al. 2015). By contrast, in many other countries, there is substantially more formal insurance for the labor market consequences of adverse health. For example, in Germany, an overnight hospital stay automatically produces wage replacement benefits from the Social Insurance System (Jager 2015); in Denmark, mandatory sick-pay benefits from employers combined with public and private disability insurance covers most of the adverse earnings consequences of a non-fatal health event (Fadlon and Nielsen 2015). On the other hand, for those lacking formal health insurance in the US, there appears to be fairly extensive informal insurance operating through unpaid bills.

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Table 1

Sample Characteristics

Sample	Non-Elderly Insured		Elderly		Non-Elderly Uninsured
Data Source	HRS	Credit Report Sample	HRS	Credit Report Sample	Credit Report Sample
	(1)	(2)	(3)	(4)	(5)
Panel A: Demographics					
Age at admission	57.6	48.5	74.9	77.0	45.1
Male (%)	48.2	45	42.9	46	62
Year of admission	2002.3	2005.0	2003.7	2005.0	2005.0
Has spouse in survey wave preceding hospitalization (%)	75.2	n/a	53.6	n/a	n/a
Panel B: Race/Ethnicity					
Hispanic (%)[d]	5.3	18.0	6.0	10.6	25.6
Black (%)	9.7	7.9	7.9	5.3	11.2
White (%)	86.0	63.0	88.7	75.8	54.7
Other Race (%)	4.2	11.0	3.4	8.3	8.5
Panel C: Index Hospitalization					
Length of Stay (days)	n/a	4.1	n/a	6.0	4.8
Hospital List Charges ($)[a]	n/a	45,580	n/a	56,609	37,789
	n/a	(189,598)	n/a	(162,445)	(82,718)
Medicaid (%)	6.2	13.7	6.5	1.0	0.5
Private (%)	93.9	86.3	36.0	9.4	0.3
Hospital Non Profit (%)	n/a	74.4	n/a	76.2	60.4
Hospital For Profit (%)	n/a	16.3	n/a	15.4	12.6
Hospital Public (%)	n/a	9.4	n/a	8.4	26.9
Admitted through Emergency Department (%)	n/a	47.9	n/a	58.6	79.8
Panel D: Subsequent Outcomes [c]					
Re-Admitted to Hospital Within 12 Months (%)	23.1	20.4	26.6	33.3	20.1
Re-Admitted to Hospital Within 48/36 Months (%)	36.0	36.0	44.4	56.4	35.1
Died within 12 Months (%)	0	3.2	0	15.2	3.9
Died within 48 Months (%)	n/a	6.3	n/a	30.6	7.7
Insured within 12 Months (%)[b]	92.9	97.6	99.2	99.7	40.6
Insured within 48/36 Months (%)[b]	92.6	96.6	99.0	99.7	52.5
Individuals	4,359	378,190	5,785	409,030	152,852

Notes: Age is defined at admission. Non-elderly are 50-64 in HRS and 25-64 in credit reports; elderly are 65 and older. Insurance status is defined at the index admission for the credit report sample and in the survey wave preceding the wave which reports the index admission for the HRS sample. "Insured" denotes coverage by Medicaid or private insurance. All proportions are multiplied by 100 and the analysis is weighted to adjust for oversampling of some groups for the credit report sample and using survey weights for the HRS sample. All hospitalizations that are pregnancy related (MDC = 14) have been dropped from the credit report sample.

[a] Charges are summed and insurance type is averaged (weighted by length of stay) for people that have a single hospitalization spread across more than one unit in a hospital or more than one hospital.

[b] Subsequent insurance status for the credit report sample is defined only if they are re-admitted to the hospital.

[c] In the HRS, survey waves are two years apart so we assume the index hospital admission occurs one year prior to its report. Subsequent outcomes 12-months later are therefore measured based on the survey wave reporting the index hospital admission and for 36-months later we use the survey wave subsequent tothe one that reports the index admission. In the credit report data we measure outcomes 12 and 48 months later. In the HRS, mortality is mechanically zero 12 months post admission, and thus the sample conditions on survival to the next survey.

[d] In the credit report sample, black, white, other race and Hispanic are mutually exclusive; in the HRS, "Hispanic" is asked separately from race.

Table 2
Impact of Hospitalization on Selected Outcomes in the HRS

	Out-of-Pocket Medical Spending	Respondent Earnings	Spousal Earnings	Household Government Transfers	Total Household Income
	(1)	(2)	(3)	(4)	(5)
Panel A. Non-Elderly Insured					
12-month effect[a]	2,115	-6,124	-1,029	1,592	-10,076
	(186)	(2,701)	(2,300)	(306)	(5,269)
	[<.001]	[.023]	[.65]	[<.001]	[.056]
Average annual effect after 36 months[b]	1,091	-7,206	-621	1,951	-10,010
	(126)	(2,390)	(2,037)	(276)	(4,606)
	[<.001]	[.0026]	[.76]	[<.001]	[.03]
Pre-hospitalization mean	2,159	41,935	28,077	3,408	91,336
Number of Individuals	4,359	4,359	4,359	4,359	4,359
Number of Observations	22,582	22,582	22,582	22,582	22,582
Panel B. Elderly					
12-month effect[a]	1,323	-1,717	-57	-531	-7,901
	(163)	(1,657)	(770)	(301)	(3,481)
	[<.001]	[.3]	[.94]	[.078]	[.023]
Average annual effect after 36 months[b]	675	-854	555	-431	-4,938
	(120)	(1,492)	(637)	(263)	(2,857)
	[<.001]	[.57]	[.38]	[.1]	[.084]
Pre-hospitalization mean	2,521	8,248	4,672	15,811	51,198
Number of Individuals	5,785	5,785	5,785	5,785	5,785
Number of Observations	29,441	29,441	29,441	29,441	29,441

Notes: Samples are the non-elderly insured (see Table 1, column 1) and elderly (see Table 1, column 3) in the HRS. All columns report effects based on OLS estimates of equation 4. Pre-hospitalization means are calculated using the survey wave preceding the hospitalization. Standard errors (clustered on the individual) are in parentheses and p-values are in brackets. All estimates are weighted using survey weights. All outcomes are reported for the past calendar year except for out-of-pocket medical spending which covers the two-years since the last interview.

[a] The 12-month effect is calculated as (5/3)*mu_0+(1/6)*mu_1 from equation 4 for all outcomes except for out-of-pocket medical spending, which is calculated as mu_0. The wave 0 interview occurs on average one year after the hospital admission and 6 months into the calendar year. For all outcomes except out-of-pocket medical spending, mu_0 therefore reflects changes relative to a linear trend for 6 months before and 6 months after the hospitalization on average, while mu_1 reflects the change relative to the linear trend for months 19 through 30 following the hospitalization. For out-of-pocket medical spending, mu_0 reflects the change relative to a linear trend for the 12 months before and 12 months after the hospitalization, on average.

[b] The average annual effect after 36 months is likewise calculated from equation (4) as (1/3)*[2*mu_0 + (11/6)*mu_1 + (1/6)*mu_2] for all outcomes except for out-of-pocket spending where it is (1/3)*(mu_0+mu_1). Note that mu_2 reflects changes in income relative to the linear trend for months 43 through 54 following the hospitalization.

Table 3
Impact of Hospitalization on Collections

	Number of Collections			Collection Balances		
	All	Medical	Non-Medical	All	Medical	Non-Medical
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Non-Elderly Insured						
12-month effect[a]	.11	.095	.011	122	127	18
	(.005)	(.002)	(.003)	(13)	(7)	(16)
	[<.001]	[<.001]	[.0011]	[<.001]	[<.001]	[.26]
48-month effect[b]	.21	.18	.034	302	271	101
	(.019)	(.008)	(.014)	(37)	(18)	(47)
	[<.001]	[<.001]	[.017]	[<.001]	[<.001]	[.03]
Pre-hospitalization mean	.92	.2	.72	1,230	292	1,086
Number of Individuals	383,718	383,718	383,718	383,718	375,844	375,844
Number of Observations	3,131,534	3,131,534	3,131,534	3,131,534	2,208,517	2,208,517
Panel B. Non-Elderly Uninsured						
12-month effect[a]	.97	.85	.12	4,469	4,259	246
	(.012)	(.008)	(.007)	(51)	(45)	(36)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
48-month effect[b]	1.3	1.2	.11	6,199	6,144	195
	(.045)	(.028)	(.028)	(130)	(102)	(100)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[.051]
Pre-hospitalization mean	2.3	.59	1.7	3,529	1,292	2,762
Number of Individuals	153,617	153,617	153,617	153,617	151,343	151,343
Number of Observations	1,256,759	1,256,759	1,256,759	1,256,759	913,516	913,516
Panel C. Elderly						
12-month effect[a]	.027	.026	0	24	17	4
	(.002)	(.001)	(.002)	(8)	(3)	(11)
	[<.001]	[<.001]	[.8]	[.0018]	[<.001]	[.74]
48-month effect[b]	.038	.049	-.011	84	37	39
	(.01)	(.004)	(.008)	(24)	(8)	(34)
	[<.001]	[<.001]	[.19]	[<.001]	[<.001]	[.25]
Pre-hospitalization mean	.24	.048	.19	428	75	422
Number of Individuals	414,547	414,547	414,547	414,547	387,839	387,839
Number of Observations	2,959,802	2,959,802	2,959,802	2,959,802	1,946,208	1,946,208

Notes: Samples are non-elderly insured and uninsured (see Table 1, columns 2 and 5) and Elderly (see Table 1, column 4). All columns report effects based on OLS estimates of equation 5. Pre-hospitalization means are calculated using the credit report from January of the calendar year preceding the hospitalization (between 12 and 23 months before the hospitalization). All variables are observed from 2002 to 2011, except medical and non-medical collection balances which are only observed beginning in 2005. Standard errors (clustered on the individual) are in parentheses and p-values are in brackets. All estimates are weighted to adjust for individuals' sampling probabilities.

[a] 12-month effect is calculated from equation (5) as 144*Beta_2_hat + 1,728*Beta_3

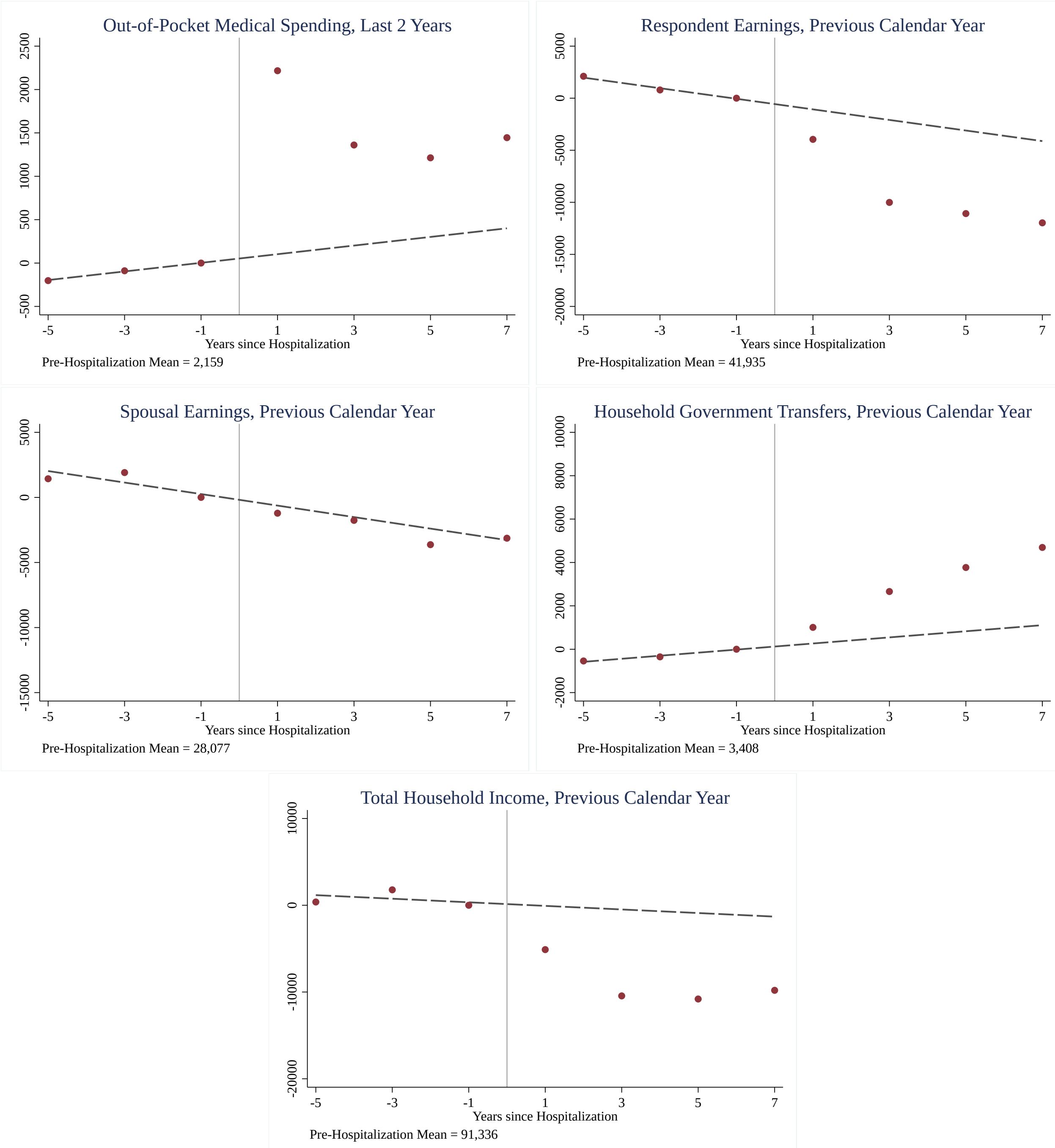
[b] 48-month effect is calculated from equation (5) as 2,304*Beta_2_hat+110,592*Beta_3_hat+46,656*Beta_4_hat+13,824*Beta_5_hat

Table 4
Impact of Hospitalization on Other Credit Report Outcomes

	Any Bankruptcy to Date	Credit Limit	Credit Score	Credit Card Balances	Automobile Loan Balance
	(1)	(2)	(3)	(4)	(5)
Panel A. Non-Elderly Insured					
12-month effect	.0013	-515	-1.6	-293	-102
	(.00031)	(154)	(.2)	(94)	(28)
	[<.001]	[<.001]	[<.001]	[.0018]	[<.001]
48-month effect	.0042	-2,215	-1.8	-1,208	-507
	(.00092)	(440)	(.5)	(253)	(71)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
Pre-hospitalization mean	.034	37,664	731	11,942	6,684
Number of Individuals	383,718	383,718	371,715	383,718	383,718
Number of Observations	3,131,534	3,131,534	2,942,253	3,131,534	3,131,534
Panel B. Non-Elderly Uninsured					
12-month effect	.0048	-678	-5	-264	-267
	(.00046)	(131)	(.3)	(83)	(29)
	[<.001]	[<.001]	[<.001]	[.0014]	[<.001]
48-month effect	.014	-690	6.6	-443	-349
	(.0014)	(353)	(.6)	(214)	(73)
	[<.001]	[.051]	[<.001]	[.038]	[<.001]
Pre-hospitalization mean	.037	15,145	655	5,376	3,981
Number of Individuals	153,617	153,617	137,913	153,617	153,617
Number of Observations	1,256,759	1,256,759	1,017,096	1,256,759	1,256,759
Panel C. Elderly					
12-month effect	-.00019	370	-1.4	72	69
	(.00022)	(138)	(.2)	(73)	(17)
	[.4]	[.0073]	[<.001]	[.32]	[<.001]
48-month effect	-.001	-448	-3.3	-30	194
	(.00072)	(393)	(.5)	(187)	(43)
	[.16]	[.25]	[<.001]	[.87]	[<.001]
Pre-hospitalization mean	.016	36,967	824	7,016	2,143
Number of Individuals	414,547	414,547	405,389	414,547	414,547
Number of Observations	2,959,802	2,959,802	2,833,027	2,959,802	2,959,802

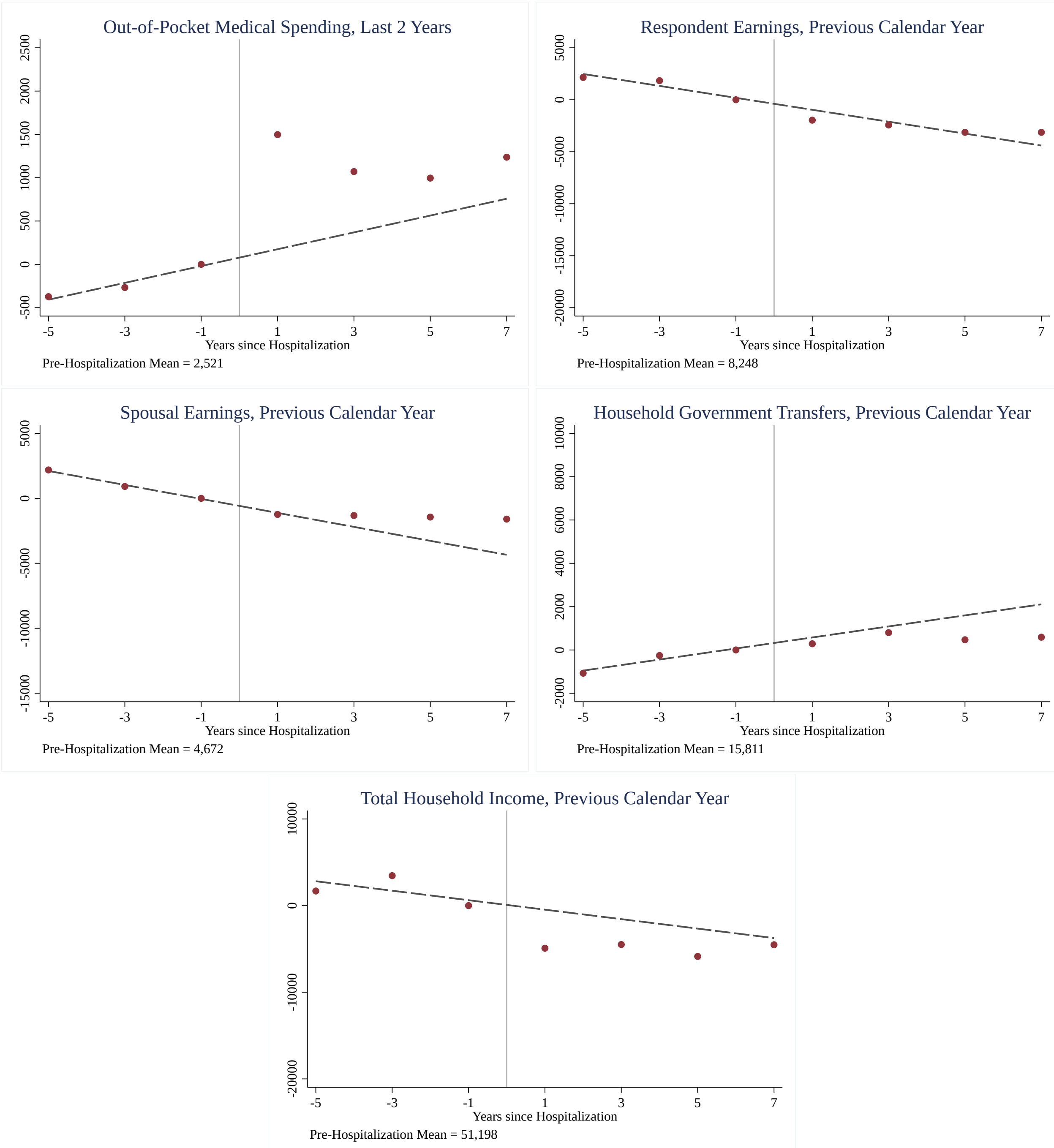
Notes: See notes to Table 3.

Figure 1: Impact of Hospitalizations on Selected Outcomes in the HRS, Non-Elderly Insured



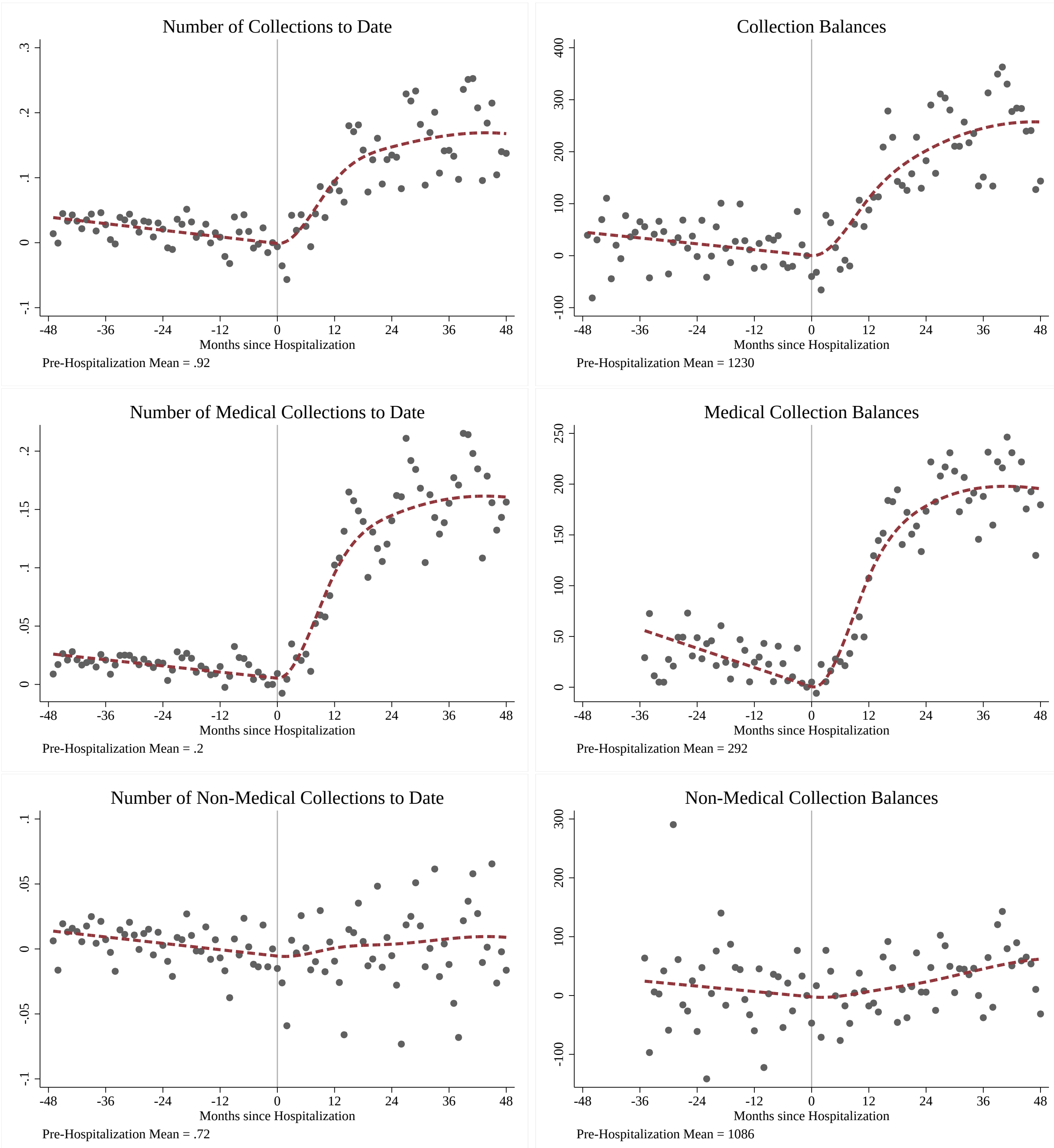
Notes: The sample is the non-elderly insured (see Table 1, column 1). The years on the x-axis are defined relative to the index admission. The points in each figure represent the estimated effects of event time (i.e. the μ_r's from the non-parametric event study in equation 3), with the year prior to admission normalized to zero. The dashed line represents the estimated pre-admission linear relationship between outcome and event time from the parametric event study in equation 4 with the level normalized to match the non-parametric estimates. All estimates are weighted using HRS survey weights.

Figure 2: Impact of Hospitalizations on Selected Outcomes in the HRS, Elderly



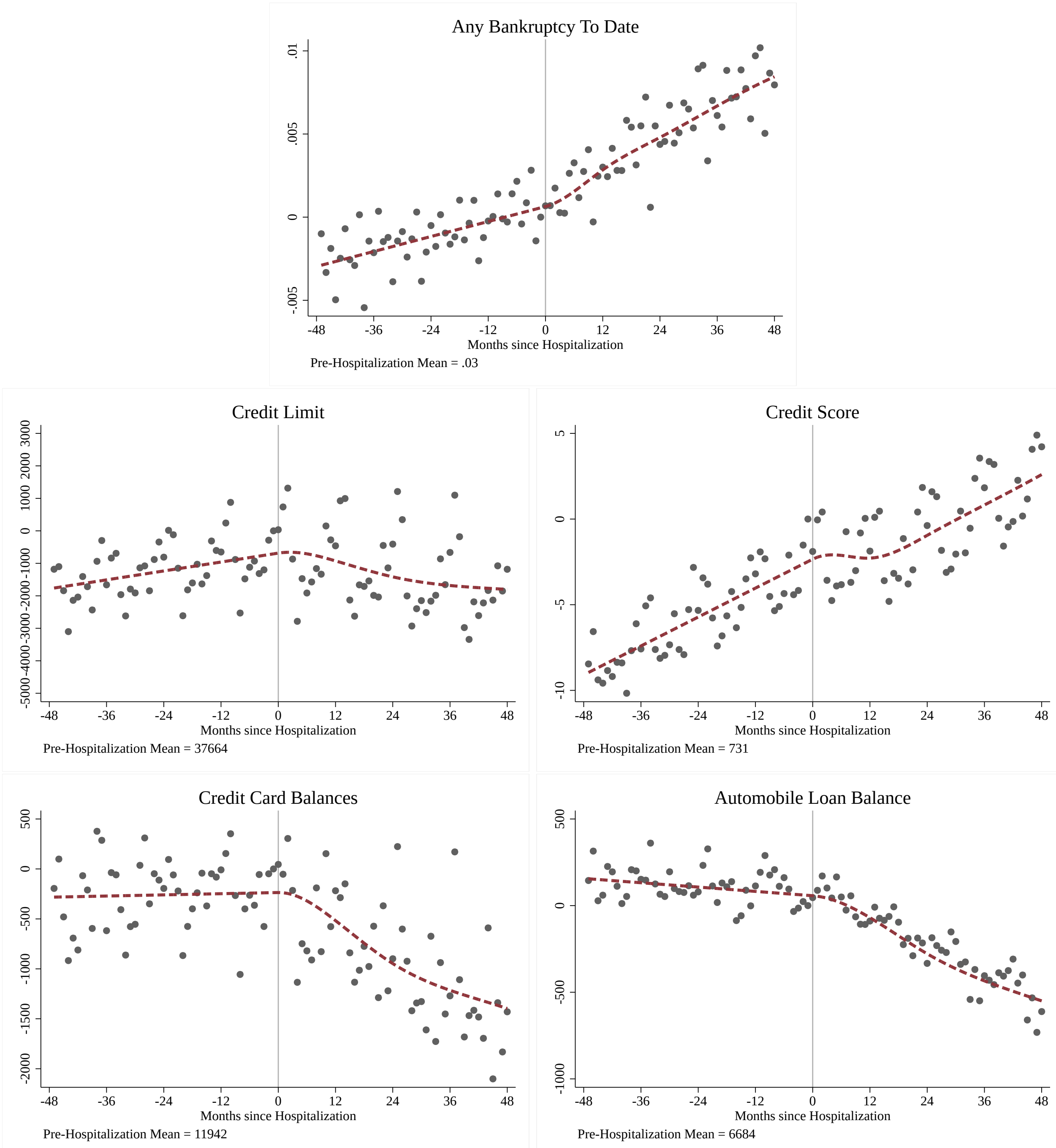
Notes: The sample is the elderly (see Table 1, column 3). See notes to Figure 1 for more details.

Figure 3: Impact of Hospitalizations on Collections, Non-Elderly Insured



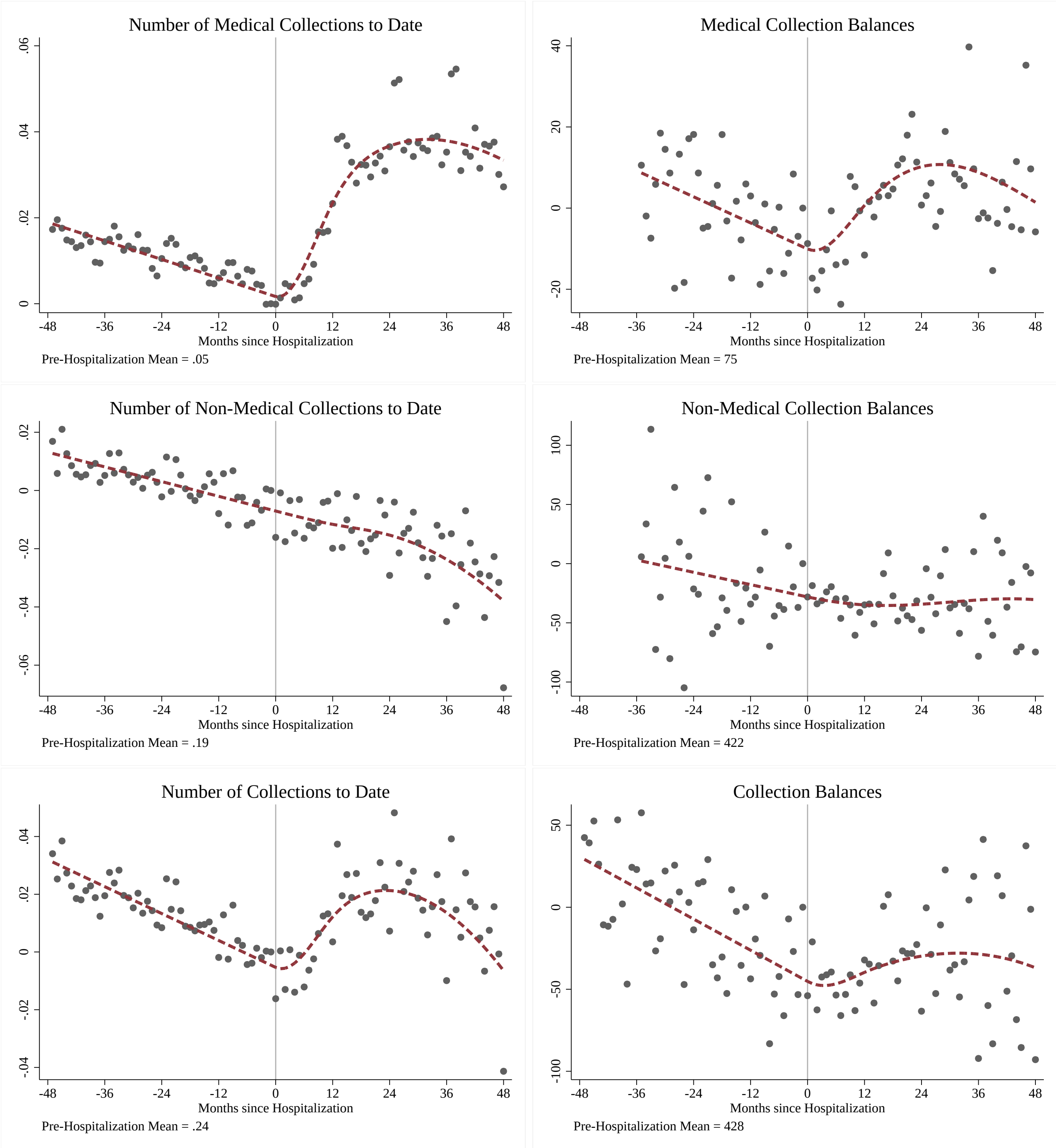
Notes: The sample is the non-elderly insured (see Table 1, column 2). The months on the x-axis are defined relative to the index admission. The points in each figure represent the estimated effects of event time (i.e. the μ_r's from the non-parametric event study in equation 3). The dashed line represents the estimated event study coefficients from the parametric event study in equation 5 with the level normalized to match the non-parametric estimates. All estimates are weighted to account for individuals' sampling probabilities. All variables are observed from 2002 to 2011, except medical and non-medical collection balances which are observed beginning in 2005.

Figure 4: Impact of Hospitalizations on Other Credit Report Outcomes, Non-Elderly Insured



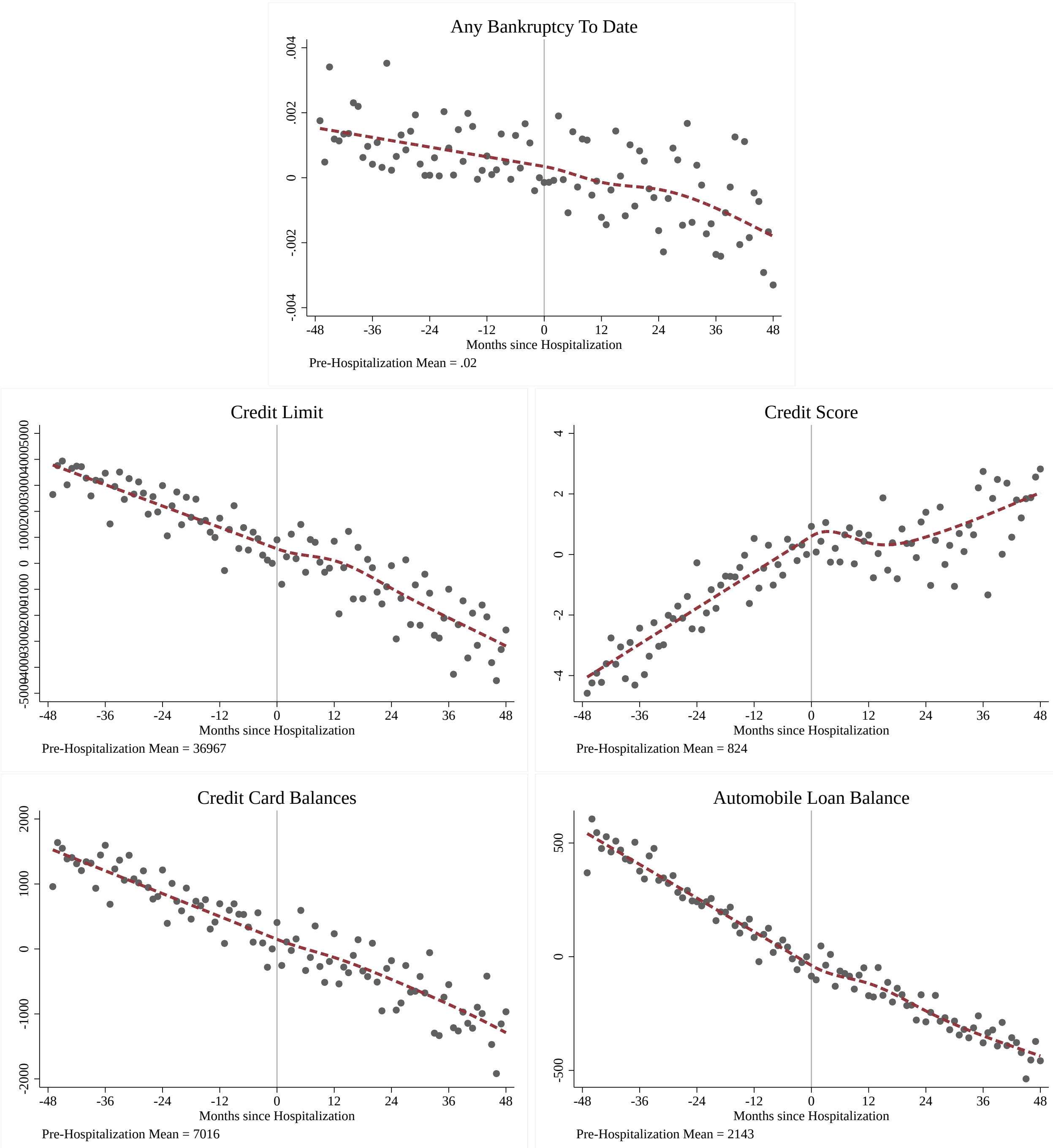
Notes: See notes to Figure 3.

Figure 5: Impact of Hospitalizations on Collections, Elderly



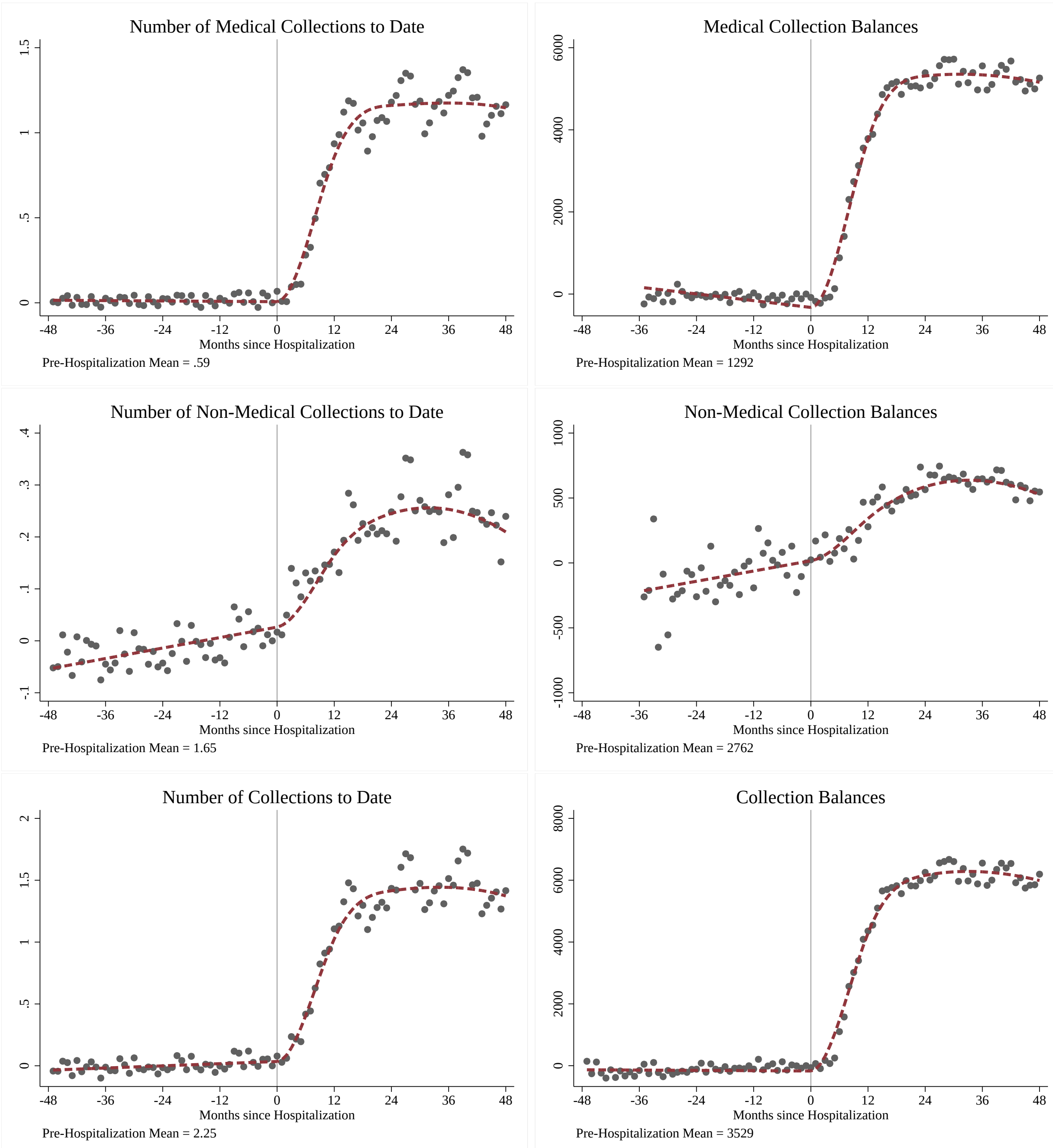
Notes: The sample is the elderly (see Table 1, column 4). See notes to Figure 3 for more details.

Figure 6: Impact of Hospitalizations on Other Credit Report Outcomes, Elderly



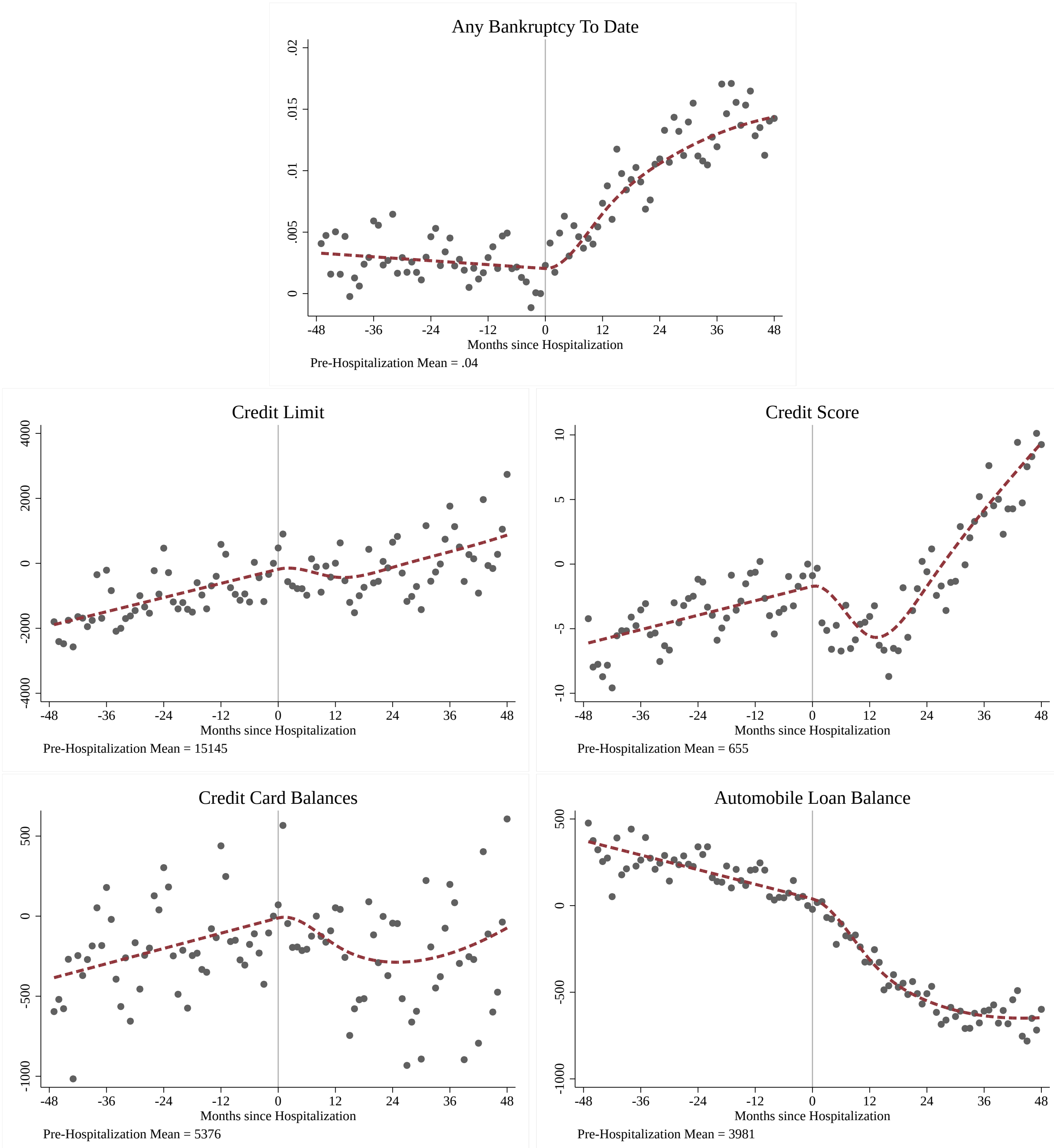
Notes: See notes to Figure 5.

Figure 7: Impact of Hospitalizations on Collections, Non-Elderly Uninsured



Notes: The sample is the non-elderly uninsured (see Table 1, column 5). See notes to Figure 3 for more details.

Figure 8: Impact of Hospitalizations on Other Credit Report Outcomes, Non-Elderly Uninsured



Notes: See notes to Figure 7.