Abstract

We examine some economic impacts of hospital admissions using an event study approach in two datasets: the Health and Retirement Study, and hospital admissions linked to credit reports. For insured, non-elderly 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. In the three years following a hospital admission, the decline in average annual earnings is about $17,000, and the decline in household income is similar; they are an order of magnitude larger than the increase in out-of-pocket medical spending. For the elderly, we find similar-sized effects on out-of-pocket medical expenses and unpaid bills, and little or no little effect on earnings, income, access to credit, borrowing or bankruptcy. For the uninsured non-elderly, we find similarly-sized effects on access to credit and borrowing as for insured non-elderly, but substantially larger impacts on unpaid medical bills and bankruptcy; for example, four years later, the average hospital admission has generated $300 in unpaid medical bills and increased bankruptcy rates by 0.4 percentage points for insured non-elderly adults, compared to $6,000 in unpaid medical bills and a 1.4 percentage point increase in bankruptcy rates for the uninsured non-elderly. Taken together, our results suggest that insured non-elderly adults face considerable exposure to uninsured earnings risk from adverse health, while a large share of the incremental risk exposure for uninsured non-elderly adults may be born by third parties who absorb their unpaid medical bills.

JEL codes: I10, I13, D14.

Keywords: Health insurance, consumer finance, consumption smoothing, bankruptcy.


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.” More recently, the United States has undertaken a major expansion of both public and private health insurance coverage through the 2010 Affordable Care Act; this expansion particularly affected non-elderly adults, as existing public health insurance disproportionately covered the elderly and children.

The vast majority of Americans now have health insurance. Yet we know remarkably little about how much exposure to economic risk from adverse health events that adults with health insurance face. We investigate this using an event study approach to examine the economic impacts of hospital admissions in two, complementary panel data sets. We use 20 years (11 waves) of the Health and Retirement Study (HRS) to analyze the impact of hospital admissions on out of pocket medical spending, income, and its components for about 10,000 adults. We construct a 10-year panel of credit reports for adults in California with hospital admissions to analyze the impact on unpaid medical bills, bankruptcy, access to credit, and borrowing for about 1 million adults. 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 hospitalization for several years preceding the index hospitalization. Our primary focus is on non-elderly adults with health insurance; 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). However we report a parallel set of analyses for elderly (age 65 plus) adults and for uninsured non-elderly adults; the analysis of the uninsured is limited to the credit report data due to insufficient sample size in the HRS. In each data set, we find compelling visual evidence of sharp, on-impact effects of hospitalizations that persist – and in some cases increase – over time.

Our baseline estimates in the HRS indicate that for insured non-elderly adults, 3 years later, an initial hospital admission is associated, on average with an average annual increase in out-of-pocket medical spending of about $1,200, and an average annual decline in labor market earnings of about $17,000, which represents a one-third decline relative to pre-admission mean earnings. Average household total income declines by a similar amount; there is evidence of a slight (about $1,000) offset from government transfers (particularly Social Security Retirement Income and Social Security Disability Income), but no response of spousal earnings. In the credit report data, our baseline estimates for non-elderly insured adults indicate that, four years later, an initial hospital admission is associated, with, to date: (1) an increase in unpaid bills (measured as bills sent to collection agencies) of about $300 (25 percent); (2) an increase in consumer bankruptcy of 0.4 percentage points (33 percent), (3) a decrease in access to credit - measured by a $2,000 (5 percent) decline in credit limits and a 1.8 (0.2 percent) decline in credit score; and (4) a decrease in borrowing - measured by a decline in credit card balances of $1000 (9 percent) and a decline in automobile installment loans of $450 (7

\footnote{See http://www.presidency.ucsb.edu/ws/?pid=27123, last accessed July 2, 2015.}
percent).

Our findings suggest that for insured non-elderly adults ages 50-64, the uninsured income consequences of hospital admissions are an order of magnitude more important than uninsured medical expenses. The declines in access to credit and borrowing and the increase in bankruptcy following a hospital admission that we estimate in credit report data therefore likely stem primarily from the uninsured income consequences of the health shock; consistent with this, for the elderly, we find similarly-sized effects of hospital admissions on out-of-pocket medical expenses and unpaid bills, and little or no little effect on earnings, income, access to credit, borrowing or bankruptcy. Interestingly, our back-of-the-envelope calculation suggests that total medical expenses and income consequences of health shocks are likely roughly similar for non-elderly insured adults; however, by design, health insurance covers (a portion of) medical expenses but not earnings losses.

Our findings also suggest that for non-elderly adults without health insurance (the “uninsured”), external parties bear an important part of the economic consequences of their hospital admission. For non-elderly adults without insurance, the analysis of the credit report data indicates similar impacts of hospital admissions on access to credit and borrowing as for our insured sample, but much larger impacts on unpaid bills and bankruptcy; four years later, a hospital admission is associated with a $6,000 (170 percent) increase in unpaid bills and an increase in consumer bankruptcy of 1.4 percentage points (120 percent). Naturally one must be careful in drawing causal inference about the role of insurance per se from comparisons of impacts for the insured and uninsured. We provide some supportive evidence for our 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. 2009 and Barcellos and Jacobson forthcoming). Consistent with the simple comparison of impacts for insured and uninsured adults, the RD analysis also finds impacts of consumer cost-sharing on unpaid bills but not on access to credit or borrowing. These findings are consistent with 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 accrue to external parties who bear the ultimate economic incidence of unpaid medical bills (Garthwaite et al. 2015; Finkelstein et al. 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. In Denmark, Fadlon and Nielsen (2015) estimate that health shocks produce comparable (15-20 percent) decline in earnings but much smaller (2-4 percent) declines in income; this contrast underscores the relative paucity of insurance (private or public) for the labor market consequences of adverse health in the US.

Finally, our findings also relate 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 non-elderly insured adults, and 5 percent of bankruptcies for non-elderly, uninsured adults.

The rest of the paper proceeds as follows. Section 2 provides a simple conceptual framework in which health shocks can generate both medical expenses and lost income, and discusses potential impacts on 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 the two channels considered in the model: out of pocket medical expenses and income. Section 5 shows 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 out-of-pocket medical expenses and reductions in income; we will analyze these impacts using data from the HRS on out-of-pocket medical spending, and household income (in total and its components). We use the framework to help interpret the impact of health shocks on the financial outcomes we will analyze in credit report data: borrowing ($b$), credit card limits ($L$), unpaid medical bills ($u$), and borrowing costs ($r$).

2.1 Model setup

Individuals live for two periods. At the start of period 1, the individual 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 $S$ (sick state) and ones in which it has not with an $H$ (healthy state). The health event incurs medical expenses $m$ and reduces income from $y_1$ and $y_2$ to $(1 - \alpha_1)y_1$ and $(1 - \alpha_2)y_2$. Thus, in our set-up, a health shock can directly affect the individual’s budget constraint in two ways: through medical expenses $m$ and through a decrease in income $\alpha_1y_1 + \alpha_2y_2$.

2Indeed, 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.
For modeling purposes, we treat both medical expenses $m$ and the decrease in income $(\alpha_1 y_1 + \alpha_2 y_2)$ as exogenous. In practice, as we will show below, the reduction in income reflects reduced earnings, which presumably reflects endogenous choices in response to declines in the marginal product of labor and/or increases in the marginal disutility of labor.

The total “size” of the health shock is therefore $m + \alpha_1 y_1 + \alpha_2 y_2$. We assume that the health shock is bounded above by total income; i.e., $m + \alpha_1 y_1 + \alpha_2 y_2 < y_1 + y_2$. The size restriction ensures that the individual will be able to choose positive consumption in both periods.

Health insurance covers a share $\lambda_m \in [0, 1]$ of medical costs $m$ and reimburses a share $\lambda_u \in [0, 1]$ of reduced income $\alpha_1 y_1 + \alpha_2 y_2$. A (weakly positive) insurance premium $\pi$ is paid in every period and in every health state.

**Definition 1.** We define a health shock that is not fully insured as a health shock that is non-zero – i.e., $m > 0$ and/or $\alpha_1 > 0$ and/or $\alpha_2 > 0$ - and for which the individual is not fully insured – i.e., $(1 - \lambda_m)m + (1 - \lambda_u)(\alpha_1 y_1 + \alpha_2 y_2) > 0$.

This definition ignores the possibility of state-dependent utility (or, equivalently, defines “full insurance” $(\lambda_m = \lambda_u = 1)$ as smoothing consumption across states rather than marginal utility of consumption). We do this for ease of exposition, and we explicitly consider the potential implications of state-dependent utility for the normative implications of our findings in Section 5.1.3.

The individual has a per-period concave utility function defined over consumption, $u(c)$, and a discount rate $\delta$. She can choose to save or borrow in period $1$ at an interest rate $r$; we denote this amount by $b^H$ or $b^S$, depending on whether she is in the (H)ealthy or (S)ick state. She can also choose to pay only a share of her out-of-pocket medical expenses $(1 - \lambda_m)m$, and leave an amount $u \leq (1 - \lambda_m)m$ unpaid. The cost of unpaid medical bills comes in the form of higher future borrowing costs $r(u, b)$.

After observing the size of any period $1$ health shock, the individual chooses her consumption path (i.e., $b$ and $u$) subject to her lifetime budget constraint to maximize her state-specific utility ($U^H$ and $U^S$), where utility in health state $J \in \{H, S\}$ is given by $u(c_1^J) + \frac{1}{1+\delta} u(c_2^J)$. The optimal borrowing choice ($b^J$) is limited by a maximum borrowing constraint ($L^J$) that is an increasing function of the presented discounted value of net income $(Y^J)$ according to $L^J = \gamma Y^J$, with $0 < \gamma \leq 1$. This modeling choice is a reduced-form representation of the supply side of the credit market, which may not let individuals borrow all the way up to their “natural borrowing limit” (e.g., Ljungqvist and Sargent 2004). The borrowing choices together with the budget constraint determine consumption in each health state and time period as follows:

\[
\begin{align*}
  c_1^S & = (1 - (1 - \lambda_u) \alpha_1) y_1 - (1 - \lambda_m) m + u - \pi + b^S \\
  c_2^S & = (1 - (1 - \lambda_u) \alpha_2) y_2 - \pi - (1 + r(u, b^S)) b^S \\
  c_1^H & = y_1 - \pi + b^H \\
  c_2^H & = y_2 - \pi - (1 + r(0, b^H)) b^H.
\end{align*}
\]

We assume that the interest rate function $r(u, b)$ is strictly increasing in $b$ and $u$, and we define the
total cost of borrowing as \( g(u, b) = (1 + r(u, b))b \). We impose the following assumption on the total cost expression:

**Assumption 1.** The marginal cost of additional borrowing is increasing in the total amount to be repaid: \( g(u', b') > g(u, b) \) implies \( \frac{\partial g}{\partial b}g(u', b') > \frac{\partial g}{\partial b}g(u, b) \).

This assumption implies that the total amount to be repaid is a sufficient statistic for the marginal cost of additional borrowing, whether the total borrowing costs come from unpaid medical bills or from regular debt.

### 2.2 Impact of health shocks

We will use \( \Delta \) to compare outcomes when sick to outcomes when healthy (e.g., \( \Delta b = b^S - b^H \)).

**Proposition 1.** A health shock that is not fully insured generates \( \Delta c_1 < 0, \Delta c_2 < 0, \Delta U < 0, \Delta L < 0, \Delta u > 0 \); the signs of \( \Delta b \) and \( \Delta r \) are ambiguous, but \( \Delta b \neq 0 \) and/or \( \Delta r \neq 0 \) reject full insurance.

**Proof.** See Appendix A \( \square \)

Proposition 1 says that, without full insurance, individuals will experience a decline in utility and consumption when sick; this is a straightforward result based on objects we do not directly observe. More usefully, Proposition 1 says that we can reject the null of full insurance through changes in outcomes we can observe: credit limits \( (L) \), borrowing \( (b) \), unpaid medical bills \( (u) \), and interest rates \( (r) \). Without full insurance, credit limits fall when individuals become sick \( (\Delta L < 0) \), because of a decline in lifetime resources which (by assumption) are positively related to credit limits. A change in borrowing, unpaid bills, or interest rate following a health shock \( (\Delta b \neq 0, \Delta u \neq 0, \text{ or } \Delta r \neq 0) \) implies a rejection of full insurance because, with full insurance \( (\lambda_m = \lambda_a = 1) \), health shocks do not change either the level or time profile of lifetime resources, and hence do not change borrowing behavior, borrowing costs, or unpaid bills. The sign of the impact of a health shock on \( r \) is ambiguous; \( r \) is increasing in unpaid bills \( u \) (which health shocks increase\(^4\)) and in \( b \), but the impact of a health shock on \( b \) is of indeterminate sign.

The intuition for why \( \Delta b \) could be of either sign without full insurance is more easily seen in a simplified setting in which individuals cannot forgo paying medical bills \( (u^S = 0) \), interest rates are exogenously fixed at the discount rate \( (r = \delta) \), and the borrowing limit is equal to net income (i.e., \( \gamma = 1 \) so that \( L^S = Y^S \)).\(^5\) In this special case, solving the agent’s optimization problem yields the following closed-form expression (see Appendix A for derivation):

\[
\Delta b = \frac{-(1 - \lambda_a)(\alpha_2 y_2 - \alpha_1 y_1) + (1 - \lambda_m)m}{1 + (1 + r)}.
\]  

\(^3\)In order to ensure an interior solution for \( u \) and an optimal choice of \( b \) that is strictly less than the borrowing limit \( L = \gamma Y \), we make several additional technical assumptions on the \( r(u, b) \) function. We assume that the interest rate is strictly increasing and convex in \( b \) and \( u \) (i.e., \( \frac{\partial^2 r}{\partial b^2} > 0, \frac{\partial^2 r}{\partial u^2} > 0, \frac{\partial^2 r}{\partial b \partial u} > 0 \)) and that the cross-partial \( \frac{\partial r}{\partial b} \) is positive. We also assume that \( \frac{\partial r}{\partial u} \geq 0 \) at \( u = 0 \) and is strictly increasing in \( u \) for \( u > 0 \), and that the limit of \( \frac{\partial r}{\partial u} \) as \( u \) approaches \( 1 - \lambda_m \) is infinity. We similarly assume that the limit of \( \frac{\partial r}{\partial b} \) as \( b \) approaches \( L \) is infinity.

\(^4\)We can sign the effect of incomplete insurance on unpaid medical bills \( (\Delta u > 0) \) because \( u^H = 0 \) by definition (i.e., the individual has no medical bills in the healthy state).

\(^5\)The result, however, is true in the more general setting (see A).
Equation (2) shows that the sign of $\Delta b$ depends on the relative importance of the uninsured medical cost shocks, $(1 - \lambda_m) m$, and the uninsured income shock, $(1 - \lambda_\alpha) (\alpha_1 y_1 + \alpha_2 y_2)$.$^6$ The more important are uninsured income shocks relative to uninsured medical cost shocks, the more likely is $\Delta b$ to be negative. Indeed, if the health event only creates a medical cost shock (i.e., $m > 0$, $\lambda_m < 1$, and $\alpha_1 = \alpha_2 = 0$), then it will increase borrowing (i.e., $\Delta b > 0$) because the individual will borrow from the future to smooth consumption across the two periods when faced with uninsured medical expenses in period 1. The fact that we find that hospital admissions decrease borrowing ($\Delta b < 0$) is therefore a piece of suggestive piece of evidence on the relative importance of uninsured income shocks.$^7$

2.3 Welfare implications

We can use this stylized framework to help interpret the impact of hospital admissions on our various measured outcomes ($L, r, b, u$) in terms of their consequences for individual welfare.

Access to credit ($L, r$)

The ability to directly measure access to credit via credit limits $L$ is a key strength of the credit report data; moreover, credit scores, which we observe in the data, are a useful proxy for borrowing costs $r$. Reductions in access to credit due to health shocks (i.e., $\Delta L < 0$, $\Delta r > 0$) reduce individual well-being ($\partial U / \partial L > 0, \partial U / \partial r < 0$). Reduced access to credit increases the costs of smoothing the decline in consumption following the health shock across periods. In a more general model, it increases the cost of smoothing consumption changes in response to other shocks as well. This welfare-enhancing role of access to consumer credit as an (imperfect) way of smoothing consumption in the face of uninsured, idiosyncratic shocks is a standard result of basic economic theory. Not surprisingly, therefore, there is a rich and active literature examining the consequences of access to credit.$^8$ For example, Sullivan (2008) finds 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 consumption. More recent work has investigated other benefits of higher credit limits, such as longer unemployment durations and higher re-employment wages following job loss, both of which suggest that higher credit limits raise reservation wages (Herkenhoff et al. 2015).$^7$

$^6$Note that while $\Delta b \neq 0$ allows us to reject full insurance, the converse is not true; $\Delta b = 0$ does not imply full insurance, since (as shown in equation (2), the uninsured medical cost shock and the uninsured decline in income could exactly offset.)

$^7$Deriving a closed-form expression for $\Delta b$ is not feasible once we allow for unpaid medical bills and endogenize borrowing costs. The same intuition, however, regarding the implications of $\Delta b < 0$ for the relative importance of uninsured income consequences (compared to uninsured medical expenses) remains. If $\Delta b < 0$, then this will generally be associated with uninsured income changes in the future being more important than uninsured medical expenses, even in the more general model with unpaid medical bills and endogenous borrowing costs.

$^8$This includes both calibrated models of the welfare effects of policies that affect credit access (e.g. Chatterjee et al. 2007) and reduced form empirical examination of a range of potential benefits to consumers from increased access to credit (e.g. Karlan and Zinman 2010).
Consumption changes ($c$)

Declines in consumption directly reduce individual well-being. Ideally, we would directly measure consumption changes following a health shock, in much the same way that an older empirical literature has examined the change in (food) consumption upon unemployment (e.g., Gruber 1997; Browning and Crossley 2001; Stephens 2001; East and Kuka 2014). Unfortunately, to our knowledge there does not exist a potential source of high-quality panel consumption data for a large sample of individuals in the U.S. experiencing identifiable health shocks. However, the above framework, together with our estimates of $\Delta L$ and $\Delta b$ can provide bounds on the change in first period consumption following a health shock $\Delta c_1$:

**Proposition 2.** If there is a non-zero health shock and individuals are not fully insured, we can bound the change in consumption following a health shock by $\Delta L/\gamma < \Delta c_1 \leq \min(\Delta b, 0)$.

**Proof.** See Appendix A

Proposition 2 provides bounds on $\Delta c_1$. Assuming the change in consumption is negative, an upper bound on the decline in consumption is $\Delta L/\gamma$, which is equal to the change in present discounted value of net income ($\Delta Y$). Intuitively, the decline in consumption in the first period ($\Delta c_1$) must be smaller in magnitude than $\Delta Y$. Since $\Delta L = \gamma \Delta Y$, the upper bound on decline $\Delta c_1$ is given by $\Delta L/\gamma$.

A lower bound on the decline in consumption $\Delta c_1$ is $\min(0, \Delta b)$. Intuitively, health shocks decrease borrowing when they create a large uninsured decline in second period income relative to first period income and (first period) uninsured medical costs expenses. In the extreme where there are no uninsured medical costs and no uninsured shock to first period income, then first period consumption declines only because individuals borrow less from the future to finance first-period consumption due to their attempt to smooth the decline in second period income across periods. In a less extreme case where there are uninsured first period income or medical cost shocks, the individual will now have a countervailing incentive to increase borrowing to bring resources into this first period, so the decline in borrowing only provides a lower bound on the magnitude of the decline in first period consumption, rather than a point estimate. 

Unpaid medical bills ($u$)

In discussing our empirical results, we make a distinction between impacts on unpaid medical bills ($u$), and other outcomes ($L$, $r$ and $b$). This is because, unlike declines in access to credit or consumption, increases in unpaid medical bills may not have a direct negative impact on individual welfare if they do not affect access to credit. 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

---

9As we discuss in the Appendix, both Proposition 1 and Proposition 2 are still valid with pre-existing savings, which we model as unearned income in period 1 that does not depend on health status. However, for individuals with large savings and low or no borrowing before the health event, the bound formed by $\Delta b$ may be a very weak upper bound. There is an additional measurement issue which is that we measure borrowing but not savings, so declines in savings are the same as increases in net borrowing in the model, but we will not be able to measure this in our data. This will further work to weaken the estimated bound formed by $\Delta b$ by attenuating our estimates.
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). Any impact of increased $u$ on patient welfare is indirect. In the model, the impact of $u$ on patient welfare would operate through either consumption and access to credit (i.e., raising welfare insofar as unpaid medical bills allow for increased consumption following the health shock, and decreasing welfare insofar as they increase future borrowing costs $r$). Of course, in practice, there may also be other unmeasured and un-modeled channels such as impacts of $u$ on peace of mind (Mann and Porter 2010).

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) in two complementary data sets. We provide a brief overview of each data set here. Considerably more detail on the definition of each analytical sample and variables is provided in Appendix B.

First, we analyze the Health and Retirement Study (HRS), a nationally representative panel survey of the elderly and near-elderly in the United States. We analyze 11 bi-annual survey waves from 1992 through 2012.\(^{10}\) In each survey, respondents are asked whether they had any overnight hospital stay since the last interview. We define the survey wave in which the individual first reports a hospital admission (hereafter: the index admission) as wave 0; wave -1 refers to the survey wave prior to the index admission and wave 1 the survey wave subsequent to it (and so on). On average, about 16 percent of the non-elderly insured sample report a hospital admission in a given survey wave (i.e. an admission over the last two years).

Second, we analyze annual credit report outcomes from TansUnion’s Consumer Credit Database for a panel of individuals in California over 10 years during which they experiences a hospitalization.\(^{11}\) Specifically, we link a sample of individuals who were hospitalized in California over the five-year period between 2003 and 2007 to their annual, January credit reports from 2002-2011. We also link these hospitalized individuals to information on all of their California hospitalizations between 2000 and 2010 and to mortality data from California vital statistics through 2010; the mortality data include deaths both in and outside the hospital. The hospital data are standard, discharge-level hospital discharge data; they include the exact dates of admission and discharge, the source of the admission, detailed information on the patients’ diagnoses, and basic demographics. In addition, the data indicate the “expected source of payment” which is based on the patient’s primary insurer. To ensure sufficient sample sizes for certain types of admissions given cost considerations, 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. For confidentiality reasons, all of these analyses were conducted on a non-networked

---

\(^{10}\) This includes the entire available HRS data with the exception of the first two waves of the AHEAD survey in 1993 and 1995.

\(^{11}\) In practice, given the age range of our HRS sample (see Table 1 below), the HRS analysis is likewise effectively restricted to non-pregnancy related admissions.
computer in the Sacramento office of California’s Office of Statewide Health Planning and Development (OSHPD).

In both data sets, to try to focus on health “shocks” we restrict attention to index non-pregnancy related hospital admissions for individuals who have not had a recent hospital admission. In the HRS, we require that we observe the individual in at least one wave before the index admission in which they do not report an an overnight hospital stay in the prior two years; the index hospital admission (in wave 0) therefore, on average represents the first hospital admission in at least 3 years. Likewise, in the 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 (25-64 year old at admission) adults with health insurance who had a hospital admission. We define an individual’s health insurance in the HRS based on their self-reported contemporaneous insurance in wave -1; we define an individual as “insured” if he reports private insurance or Medicaid. In the discharge data, we define an individual’s insurance status based on their primary insurer for the index admission; once again, we define the individual as “insured” if the primary payer is Medicaid or private insurance.\(^{12}\)

Our baseline sample consists of approximately 4,400 non-elderly insured adults in the HRS and 380,000 non-elderly insured adults in the credit report data. We also report a parallel set of analyses in both data sets for the elderly - defined as those 65 and over. As with our non-elderly adults, we restrict attention to individuals who have not had a prior hospital admission over the relevant look-back period. We analyze about 5,800 elderly individuals 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; these are individuals who are 25-64 at admission and whose “expected source of payment” is “self-pay” and who also have not had a prior hospital admission in the last three years. There is insufficient sample size for analysis of uninsured non-elderly adults in the HRS.\(^{13}\)

3.1.1 Characteristics of the sample and the “event”

Table 1 presents some basic demographic statistics on our samples of interest in both data sets. Specifically, columns 1 and 2 describe the non-elderly insured samples in HRS and credit report data, respectively, columns 3 and 4 describe the elderly (65+) sub-samples in these two data sets, and column 5 describes the uninsured non-elderly sample in the credit report data. Appendix B provides additional information, including an analysis of the impact of our various sample restrictions.

Naturally, the HRS and credit report samples are not perfectly comparable on demographics. They involve different areas (national or California), different sampling frames (for example, the credit report data require that the individual have a credit report), slightly different years, different measurement conventions (e.g. insurance in the wave prior to the wave with the index admission as opposed to

\(^{12}\)In addition to the individuals with Medicaid or private insurance as a primary payer, another approximately 15 percent of individuals under 65 have Medicare as a primary payer and a few have “other” insurance (which includes various small programs such as indigent care and workers’ compensation). Because individuals under 65 on Medicare are disabled - and therefore represent a group that already presumably has had an “adverse health event”, we exclude them from our analyses.

\(^{13}\)Likewise, there is insufficient sample to analyze consumption in the HRS, which is measured for only a subset of individuals and survey waves.
insurance at the time of the index admission) and, perhaps most importantly given the nature of the sampling in the HRS, different ages for the non-elderly insured. Average age of admission for our “non-elderly” insured sample is 49 in the credit report compared to 58 in the HRS; indeed, the first percentile of age in the HRS is 52, compared to a 10th percentile age at admission of 34 in the credit report sample. The ages for the 65+ sample are much more comparable.

Focusing on the non-elderly insured adult sample in the credit report data (column 2), about 85 percent of the non-elderly insured in the credit report sample are privately insured; the remainder are on Medicaid. About 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) are circulatory system and musculoskeletal conditions (see Appendix Table 12). The index hospital admission is associated with subsequent additional health care utilization, which affects the magnitude and time patterns of our event study analyses: one-fifth are re-admitted to the hospital within 12 months and 36 percent are re-admitted within 48 months; the index hospital admission lasts an average of 4 days; subsequent to the index admission, the individual spends, on average, an additional 2 days in the hospital over the next 12 months and an additional 4.5 days over the next 48 months. Mortality is 3.2 percent in the 12 months following the index admission, and 6.4 percent in 48 months.\textsuperscript{14}

We endeavored to get a sense of the total medical payments associated with the index admission and subsequent medical use (both hospital and non-hospital). The hospital discharge data only report list charges (which are higher than transacted prices) and only include hospital costs. Therefore, we drew on data from a sub-sample of individuals from the Medicare Expenditure Panel Survey (MEPS) designed to approximate our three analysis samples; Appendix C provides more details on our calculations and additional results. Our rough approximation is that for insured adults, the index hospital admission is associated with between $23,000 and $33,000 in total medical payments in the first 48 months.

Finally, a key issue for interpreting our results is whether insurance status is persistent over time. For our non-elderly insured sample, it is: when we look 12 months out and 48 months out, over 95 percent still have insurance, and three to four years later. However, insurance status is much less persistent for our uninsured sample (column 5) is much less persistent (presumably reflecting post-admission incentives to take up insurance); only about 43 percent of subsequent hospital days over the next 4 years are uninsured for individuals who are uninsured for the index admission.

3.1.2 HRS outcomes

We use the HRS primarily to analyze the direct budget impact of a hospital admission on the two outcomes considered in the model: out-of-pocket medical expenses ($1 - \lambda m)m \text{ and income (y). All outcomes are derived from self-reports. Appendix Table 3 shows the distribution of outcome variables in wave -1 (the wave prior to the reporting of the index hospital admission) for both the non-elderly insured adults and the elderly.}

\textsuperscript{14}We describe the robustness of our findings to various potential threats posed by attrition in Appendix E below.
We analyze the direct impact of the hospital admission on the respondent by analyzing their total out-of-pocket medical spending over the past two years, their earnings in the last calendar year, whether they had any positive earnings in the last calendar, and whether (contemporaneously) they report their work is limited by health, their labor force status (work full time, work part time, unemployed, partly retired, retired, disabled or not in labor force), and whether they received any government transfers (Unemployment insurance, Social Security Disability Insurance, Supplemental Security Income, or Social Security Retirement Income) in the last calendar year. We also analyze the impact of the hospital admission on their total household income in the previous calendar year, and on each of its components: the respondent’s earnings, spousal earnings, household pension and annuity income, household government transfers (in total and each component), household business and capital income, and other household income. We use the CPI to adjust all dollar amounts to 2005 levels (the mid-point of the credit report data).

3.1.3 Credit report outcomes

Credit bureaus like TransUnion collect vast data that aims to cover virtually all U.S. consumer borrowing. Credit report data are derived from public records (such as bankruptcy filings), collection agencies, and “trade lines” such as credit card balances; the data do not generally capture informal borrowing or non-traditional lenders such as loans from relatives, pawnbrokers, or pay-day lenders. Credit reports are primarily used by prospective creditors to assess the credit-worthiness of potential customers. All credit report measures are at the individual, rather than household level.\textsuperscript{15}

Analysis of credit report data is still relatively rare in the economics literature. Existing work has primarily used them to study the impact of access to credit on outcomes like credit scores and credit limits (e.g., Gross and Souleles 2002; Bhutta et al. 2013). To our knowledge, Finkelstein et al. (2012) and Mazumder and Miller (2014) - who use credit report data to study the impact of health insurance on financial distress - are the only prior papers analyzes credit report data in a health context.\textsuperscript{16}

In the remainder of this section, we briefly define our key outcomes. Avery et al. (2003) provide an excellent, detailed discussion of credit bureau data; unless otherwise noted our description of the various credit measures and references to their role in a general population are based on this paper.

Appendix Table 14 reports pre-hospitalization summary statistics for our three analysis samples. The outcomes correspond to the ones analyzed in the model: unpaid medical bills ($u$), credit card balances and automobile installment loans ($b$), credit card limits ($L$), and credit scores ($r$).\textsuperscript{17} In addition, we analyze consumer bankruptcy, which may be viewed as an extreme form of unpaid bills ($u$). We now provide more detail on each in turn. We focus our discussion of pre-hospitalization summary statistics on those for insured adults. Not surprisingly, prior to hospitalization, the uninsured

\textsuperscript{15}We are unable to link to identify spouses in either the hospital data or the credit report data. We discuss some implications of this for our quantitative estimates in Section 5.1.3 below.

\textsuperscript{16}Several recent papers have also used related survey measures that capture late payments on bills as measures of financial distress in both the health care context (Barcellos and Jacobson 2014; Finkelstein et al. 2012) and other settings (Melzer 2011).

\textsuperscript{17}We censor all the continuous outcomes at the 99.95th percentile, an approach that makes no substantive difference to our empirical estimates below but purges the data of extreme outliers. Credit scores are already censored at 990.
look worse off on all measures than the insured; the elderly tend to appear slightly better off than insured adults.\footnote{In addition, to place our analysis samples in context relative to the general population in their age group, Appendix Table 21 compares the credit report outcomes pre-hospitalization for our three analysis samples to a randomly selected sample of Californians in 2005 (the average pre-hospitalization year) in the same age group. This sample excludes the sample of individuals with hospital admissions from 2003-2007 described above that we tried to match to credit reports. As a result, this sample of Californians whom we report in Appendix Table 21 has a non-zero, but lower than typical hospitalization rate. Although we would not necessarily expect our analysis sample of individuals with a hospital admission to be representative of the general California population in their age range, in practice, both our insured, hospitalized adult population and our elderly hospitalization population looks quite similar to the general population in their age range on outcomes other than unpaid bills; however the uninsured hospitalized adult population looks substantially worse off than the general adult population.}

**Unpaid bills** Our measures of unpaid bills are all based on collections. These reflect unpaid bills that have been sent to collection agencies for recovery attempts. We observe the number of new collections in the last 12 months and the total current unpaid balances across all collection accounts.

Collections correspond closely to the notion of unpaid bills (u) in the model: They are considered major derogatories and can have an important effect on one’s credit score, which we use as a proxy for \(r\). Moreover, only about 10 percent of collection balances are ever paid off.

Usefully, we are able to observe medical and non-medical collections separately. Medical collections refer to unpaid medical bills sent to a collection agency. They are the most common kind of collection, accounting for about half of all collection balances in a general population. Non-medical collections refer to any other unpaid bills sent to a collection agency; the most common are utility bills, accounting for about half of non-medical collection balances; non-medical collections may potentially reflect unpaid medical bills if, for example, these were charged to a revolving credit card and then not paid.

We use the flow measure of “new collections in the last 12 months” to construct a cumulative “stock” measure of “number of collections to date” by summing within the individual starting from the 2002 credit report file; this measure is therefore mechanically increasing with calendar time over our sample period.

We also measure unpaid collection balances. Prior to the hospital admission, an insured adult has about $1,100 in unpaid collection balances prior to hospitalization. We only observe the breakdown of collection balances into medical and non-medical for years 2005 forward; approximately one-quarter of pre-hospitalization collection balances are medical.

**Bankruptcy** Our data contain an indicator variable for whether an individual filed for consumer bankruptcy in the previous 12 months. On average, prior to the hospital admission, about 1.25 percent of our insured adult sample had filed for bankruptcy in the previous 12 months. As with the number of collections measure, we convert this “flow” measure into a “stock” by defining a cumulative indicator variable that indicates whether the individual has ever filed for bankruptcy during the sample period. Because individuals must wait at least eight years before being able to fully discharge their debts again through bankruptcy, we do not observe individuals filing for bankruptcy multiple times during our sample period. In the (on average) three years of data we observe for each individual prior to their hospitalization, we find that that 3.4 percent of the insured sample went bankrupt. This implies that
a very large share of our sample is “at risk” for bankruptcy in the years following their hospitalization.

**Credit limit** ($L$) We observe the total credit limit across all open revolving accounts; all else equal, a higher credit limit implies more access to credit. For our insured sample (Panel A), prior to hospitalization, the average credit limit is about $37,000, the median is $14,000 and about 20 percent of our sample has no access to revolving credit (a credit limit of 0). We interpret our measure of credit limits as a (likely incomplete) measure for the total credit limit $L$ the individuals faces; we discuss this more in some of our quantitative exercises below and in Appendix G.

**Credit score** ($r$) We use credit scores to proxy for the interest rate faced by individuals. The credit score provides a measure of the market’s assessment of the individual’s credit-worthiness; it is used by lenders to determine whether and at what terms to lend. Credit scores are well-known determinants of individual borrowing costs (e.g. Einav et al. 2013, Agarwal et al. 2015, Han et al. 2015). A higher credit score corresponds to a lower $r$.

We analyze the “VantageScore 2.0” credit risk score provided to us by the credit bureau. It can range from a low of 501 (the worst) up to a high of 990 (the best). Scores have a letter grade attached to them ranging in 100 point increments from “A” (901-990) to “E” (501-600), (see e.g., http://www.mortgagefit.com/credit-rating/vantagescore.html). Prior to hospitalization, the mean and median credit score for our insured sample (Panel A) are both around 730, which corresponds to a C (“prime”) rating. Roughly 8% have an A (“Super Prime”) rating indicating they will qualify for the best terms available on loans, 25% have a D rating (“Non Prime”) suggesting they can get access to credit but on less favorable terms, and about 17% have an E rating (“High Risk”) implying they are likely to be turned down by borrowers (not shown).

Not everyone in our data has a credit score. Approximately 5% of the insured sample does not have a credit score in the year prior to hospitalization; the number is similar for the elderly sample. A non-existent credit score is not equivalent to a bad credit score. Rather, it reflects insufficient information on the person to generate a credit score, which in turn reflects a lack of both revolving credit and major derogatories; unpaid bills that are sent to collection agencies or generate liens against the individual (e.g., medical bills, utility bills, property taxes) will generate a credit score even if the individual has no access to revolving credit. We show below that a hospitalization does not have a substantive impact on the probability of having a credit score for the insured or the elderly, and therefore we feel reasonably comfortable analyzing credit scores on the insured sample who have them. We are somewhat less comfortable analyzing credit scores for the uninsured sample, as about 15 percent do not have a credit score prior to hospitalization, and hospitalizations appear to further reduce the probability of having a credit score by another 1 percentage point.

**Borrowing** ($b$) We observe two measures of borrowing. Our primary measure is total revolving account balances (“credit card balances”), summed over all revolving credit accounts the individual may have. A “revolving” balance account is an account with a minimum monthly payment and

---

19This is consistent with Consumer Financial Protection Bureau estimates that about 8 percent of individuals with a credit record are “unscorable” (Consumer Financial Protection Bureau 2015).
credit limit for which the balance can be carried over from one month to the next (“revolve”). These defining features of a credit card account differ from installment accounts (debts with a set number of monthly payments) and non-revolving (or “charge”) accounts (similar to credit card accounts except the balance must be paid in full each month). Revolving accounts are by far the most common form of credit account, followed by installment accounts; non-revolving accounts are quite uncommon. 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 shock. We will additionally analyze balances for the primary source of installment loans, automobile loans. As with credit limits, we believe our borrowing measures are likely an incomplete measure of total borrowing, and discuss this more below.

3.2 Econometric models

We estimate both non-parametric and parametric event studies. The details naturally differ slightly across the two data sets due to differences in their formats. In particular, in the HRS we analyze bi-annual survey data while in 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. We describe the basic models here. More details - both on the baseline specification and alternative specifications used in the robustness analysis - is provided in Appendix D.

3.2.1 Non-parametric event study

The non-parametric event study analyzes the coefficients on various indicator variables for time relative to the event (“relative time”). The primary advantage of the 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:

$$y_{i,t} = \gamma_t + X_t \alpha + \sum_{r=-S}^{r=-2} \mu_r + \sum_{r=0}^{r=F} \mu_r + \varepsilon_{i,t}$$

(3)

where $\gamma_t$ are coefficients on calendar time fixed effects which control for any secular trends in the outcome, $X_t$ represents a vector of other potential control variables, and $\mu_r$ 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 $\mu_r$’s which estimate the outcome at a given $r$ relative to the the omitted category $\mu_{-1}$.

In the bi-annual HRS analysis, we include calendar time fixed effects for all but one of the survey waves, and include as additional controls $X_t$ the individual’s gender, age at admission and HRS cohort. Event time $r$ in the HRS refers to the survey wave relative to the index hospital admission; $r = 0$ refers to the survey wave in which the index hospital admission is reported (which on average is 12 months after the index admission). We analyze up to 7 waves per individual: at most 3 waves prior to admission ($S = 3$) and at most 3 waves after the index admission ($F = 3$); the omitted wave is wave
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 admissions (which occurs at \( r = 0 \)). Our baseline specification limits the sample to relative months -47 (\( S = -47 \)) through 72 (\( F = 72 \)).\(^{20}\) The omitted category, \( \mu_{-1} \) is the month prior to hospitalization. The \( \gamma_t \) are coefficients on calendar year fixed effects, and there are not additional \( X_i \) covariates in the baseline specification.

The identifying assumption behind these event study analyses is that conditional on having a hospital admission during our observation window, the timing of the admission is uncorrelated with the outcome, conditional on the included covariates in the regression (such as calendar time fixed effects). One way this assumption would be violated would be if there is 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 \( y_{i,t} \); for example, if a negative economic shock - such as the loss of a job - caused health to deteriorate and produce a hospital admission, and also had an independent (direct) effect on credit report outcomes, this would violate our identifying assumption. The relatively sharp information on the timing of the event and the relatively high frequency measurement of outcomes (particularly in the credit report data) helps 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 trends in outcomes in the months leading up to the hospitalization to diagnose the validity of the identifying assumption. Attrition - which in our setting occurs because of mortality - poses yet another potential threat to our identifying assumption, and we explore this as well in the robustness section below.

### 3.2.2 Parametric event study

A parametric event study is useful for summarizing the magnitude of the impacts of a hospital admission and assessing their statistical significance. Our choice of functional form for our parametric event study 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 study which allows for a visual assessment of our parametric assumptions.

For the HRS analysis, our baseline parametric specification is:

\[
y_{i,t} = \gamma_t' + X_i \alpha' + \delta r + \sum_{r=0}^{3} \mu_r' + \varepsilon_{i,t}'.
\]  

\(^{20}\)For non-medical collection balances and medical collection balances - which we only observe in calendar years 2005 forward - we limit the sample to individuals in relative months -35 through 72.
Equation 4 allows for a linear trend in event time (i.e. over waves of the HRS) \( r \). The key coefficients of interest show the change in outcome relative to that trend in the wave of the index hospital admission \( (u_0) \) and the subsequent 3 waves. In the HRS tables below, we use the parametric event study coefficients to derive the implied impact of a hospital admission at 12 months post admission and at 30 or 36 months post admission, with the exact derivation of these implied effects and their time frame dependent on the reference period for the outcome, as we detail in Appendix B and in the table notes; note that all these effects are estimated relative to any pre-existing linear trend \( (\delta) \).

In the higher-frequency credit report data, we again allow for a linear trend in event time (now months since admission) \( r \), but we specify the post-period differently. Specifically, we estimate a cubic spline in post-admission event time:

\[
y_{i,t} = \gamma_0 + \beta_1 r + \beta_2 r^2 \{ r > 0 \} + \beta_3 (r - 12)^3 \{ r > 12 \} + \beta_5 (r - 24)^3 \{ r > 24 \} + \varepsilon_{i,t} (5)\]

Equation (5) allows for a linear trend in event time \( r \), and it 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 \( (r > 24) \).

In the credit report tables below we use the parametric event study to report the estimated impact of the hospitalization at 12 months \( (= \beta_2 \times (12^2) + \beta_3 \times (12^3) \) \) and the impact of the hospitalization at 48 months \( (= \beta_2 \times (48^2) + \beta_3 \times (48^3) + \beta_4 \times (36^3) + \beta_5 \times (24^3) \); note that these effects are all estimated relative to any pre-existing linear trend \( (\beta_1) \).

## 4 Impact of Hospital Admissions on Medical Expenses and Income

We begin by exploring the direct costs of hospital admissions on the two channels considered in the model: out of pocket medical spending and income. This analysis uses the HRS. We focus on the non-elderly insured, but also present a parallel set of results for the elderly.

### 4.1 Non-elderly insured

Figure 1 presents results for insured non-elderly adults. For each outcome, we plot the estimated coefficients on the bi-annual survey wave relative to hospital admission (the \( \mu_r \)’s) from the non-parametric event study regression in equation (3). Survey wave 0 is the wave in which the index hospital admission is reported; the coefficient on survey wave -1 \( (\mu_{-1}) \) is omitted from the regression and normalized to 0 for graphical presentation. The figures also plot the pre-admission linear relationship between outcome and event time implied by the estimates from the parametric event study regression in equation (4) (i.e. \( \delta \)), with the level normalized to match the non-parametric estimate. Panel A of Table 2 summarizes the implied effects at various points after the index admission based on the parametric event study regression in equation (4)\(^{21}\); recall these implied effects are all relative to the estimated pre-period linear trend. Out-of-pocket spending has a look-back period of the last two years, while earnings and income refer to the prior calendar year.

\(^{21}\)Appendix Table 4 reports the raw coefficients from this regression.
The first two outcomes we present are out-of-pocket spending and respondent earnings. The figures suggest that a linear trend fits the pre-hospital admission trend in the three waves (6 years) prior to admission 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 aged 50-64. The figures also show an impact of the hospital hospital admission that is visually apparently “immediately” after the hospital admission (i.e. by survey wave 0, which occurs on average 12 months after the hospital admission). The impact on out-of-pocket spending declines slightly at later waves but remains noticeably above trend, consistent with the fact that, as discussed in Section 3.1.1 above, the index hospital admission is associated with increased future medical expenses as well. The decline in earnings persists - and grows slightly - by wave 1 (which on average represents the impact on earnings or income in months 19 - 30 post admission) beyond which point the effects seem to roughly stabilize (see wave 2 and wave 3 coefficients). While the figures show a sharp departure from the pre-admission patterns, there is evidence of a pre-admission rise in out-of-pocket spending of about $120 over two years, and a pre-admission decline in earnings of about $1,000 per year; although neither trend is statistically significant, they are consistent with the hospital admission being preceded by some gradual decline in health.

Quantitatively, the estimates in Table 2 suggest that, on average, out-of-pocket medical expenses increase relative to trend by $2,162 (standard error = 182), 12 months following the index hospital admission; by 36 months after hospital admission, average annual out-of-pocket spending has increased by $1,184 (standard error = $144), or about $3,500 of cumulative increase. The decline in earnings is about an order of magnitude larger: average annual earnings fall by $16,594 (standard error = $8,525) in the three years post-admission, or about one-third relative to pre-admission mean earnings. This decline in earnings occurs partly through the extensive margin; Appendix Table 7 shows that a hospital admission decreases the probability of having any earnings by 4.3 percentage points (standard error = 0.95) or about 5 percent relative to the pre-admission fraction employed; 36 months post admission, the probability of working has declined by 9.5 percentage points (standard error = 1.5), or about 12 percent. The larger proportional decline in earnings than the share with any earnings may reflect larger impacts for higher earnings, or it may reflect declines in earnings among those who remain employed.

Appendix Table 7 also shows that the decline in earnings reflects changes in self-reported labor

---

22Since out-of-pocket medical spending is asked with a two-year look back period, and the hospital admission on average occurs one year earlier, in principle \( \mu_0 \) reflects any change in out-of-pocket spending in the 12 months prior to the admission as well as the 12 months post admission. Likewise, since earnings is reported for the prior calendar year, \( \mu_0 \) represents the impact for 6 months prior to and 6 months after the hospital admission (see Appendix B for more detail). In principle, therefore, the wave 0 effects may capture some pre-admission changes in outcomes. As noted, we limited our sample to individuals who have not been to the hospital for at least 3 years prior to wave 0, which should minimize (but not eliminate) increases in out of pocket spending in the 12 months prior to admission and declines in earnings in the 6 months prior to admission. This issue does not arise for outcomes such as self-reported labor force status which are contemporaneous measures for the time of the interview.

23As seen in Appendix Table 4, the linear trend in annual earnings is -$1,875 (standard error=2,312) in the bi-annual analysis.

24Our earnings measure includes both labor market earnings and self employment income; see Appendix B for more details. In Appendix Table 10 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 self-employment income.
market activity; Appendix Figure 33 shows the parallel event study graphs. There is a net exit from full-time work - about 7.6 percentage points (standard error = 1.8) after 36 months, with little or no net impact on working part time, being unemployed, disabled, or not in labor force; about three-quarters of the reduction in the full-time work represents substitution to self-reported retirement. Consistent with the declines in the full-time work reflecting the consequences of a hospital admission, the final column of Appendix Table 7 shows a similar-sized increase in the portion of people who report that their ability to work for pay is limited by health.

Total household income may fall more or less than earnings, depending in part on the response of spousal earnings and government transfers which may provide informal or formal insurance against earnings losses. There is no evidence of a response of spousal earnings. There is evidence of an increase in government transfers of $1,311 (standard error = $291); Appendix Table 6 (and Appendix Figure 34) shows that two-thirds of the increase in government transfers reflects increased Social Security Retirement Income Payments, while one-third reflects increased Social Security Disability Insurance Payments. Thus is appears that 5 to 10 percent of the decline in earnings is “insured” through government transfers. Total household income appears to fall by slightly more than earnings - after 3 years the average annual decline is $18,123 (standard error $9,221), despite the slight offset of government transfers. This reflects statistically insignificant declines in “household business and capital income” and “other household income” (see Appendix Table 9 and Appendix Figure 40); as we discuss in Appendix B, it is possible that some self-employment income is in fact reported as business income.

Our results suggest that the primary source of economic risk from adverse health faced by non-elderly adults with insurance is earnings risk rather than medical expense risk. For the three years post-admission, we estimate that average annual out of pocket medical expenses increase by $1,200, or less than one-tenth the magnitude of the $17,000 decline in average annual earnings. Interestingly, the total size of the medical expense shock is likely similar to that of the earnings shock; we estimate in the MEPS that the average co-insurance is about 8 percent, suggesting average annual total medical expenses (m) associated with the hospital admission of about $15,000. Health insurance in the United States, however, covers a large portion of these costs. Our results suggest, by contrast, little insurance against the adverse labor market consequences of health events in the US, either through formal insurance (private or public) or informal insurance through spousal labor supply. 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 virtually non-existent. 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 earnings do not respond in their setting either, but rather household income is insured through public and private insurance (particularly Disability Insurance). This underscores the very different nature of insurance against the economic consequences of adverse health events in the two countries.

Of course it is important to bear in mind that these results are limited to individuals age 50-64. While hospital admissions disproportionally hit older individuals among those 25-64 year olds, they
are not limited to 50-64 year olds; in the California discharge data, we estimate an average age of admission among individuals age 25-64 of 49 and a 10th percentile admission age of 34.

It is unclear whether the impact of a hospital admission on earnings is larger for the near-elderly (50-64 year olds) than for 25-64 year olds. On the one hand, the impact on earnings might be lower among the near-elderly since labor force participation rates (and hence the “earnings at risk”) are lower in this population and since - to the extent the results in Figure 1 suggest a permanent decline in earnings due to a hospital admission - the number of years with earnings potential are lower. On the other hand, the elasticity of labor market activity with respect to health may be larger at older ages; in particular, 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). Relatedly, the near-elderly have greater potential to access various social insurance programs - particularly Social Security - which may cushion the net income consequences of any earnings effect relative to what a younger cohort might experience; thus a given earnings shock may be better insured for the near-elderly. In the credit report data below, we find similar-sized effects of a hospital admission on credit card limits and borrowing for the near-elderly insured and the full 25-64 year old non-elderly insured samples, which is consistent with similarly-sized impacts on income.

**Robustness**

We explored the robustness of these findings to a number of alternative specifications, including individual fixed effects, balanced panels, and adding back in individuals who may have had a hospital admission within the past 3 years before their index admission; these results were generally reassuring. Given the high variance and right-skewness of earnings and income measures, we were also reassured that those results are robust to estimating a proportional model (specifically, a quasi-maximum likelihood Poisson model) instead; indeed, the estimated proportional declines in earnings are slightly larger (average annual decline at 3 years of 40 percent) and more precise (p value = 0.02) than in our baseline linear model. All of this robustness analysis is discussed in detail in Section E, Appendix Table 8, and Appendix Figures 35 to 39).

**4.2 The elderly**

We conducted a parallel set of analyses for elderly individuals with a hospital admission. The bottom panel of Table 2 summarizes the estimated effects; Appendix Figure 41 shows the results graphically. As expected, given the relative importance of labor market consequences of hospital admissions for our 50-64 year old insured individuals, the impacts of hospital admissions for individuals 65 plus look very different. This population has an average age at admission of 75, and less than 20 percent report working (i.e. positive earnings) in the survey wave prior to their hospital admission (compared to almost three-quarters of the 50-64 year olds). The increase in out of pocket spending is slightly smaller but statistically indistinguishable from the impact on 50-64 year olds; we estimate that for the elderly the average annual increase in out-of-pocket spending 3 years later of $709 (standard error

25 Appendix Table 5 reports the estimated coefficients directly.
= $129), compared to $1,184 (standard error $144) for 50-64 year olds. This is consistent with our back-of-the-envelope calculations from the MEPs that hospital admissions for the elderly generate total medical spending and out of pocket medical spending that is similar to that for non-elderly insured adults (see AppendixC). There is no evidence of declines in employment, or in earnings or income, for the elderly following a hospital admission. There are some puzzling wrong-signed coefficients that are sometimes statistically significant - such as increases in earnings; however there is no evidence of a change in total household income, and the increase in earnings is not robust in an alternative Poisson specification (see Appendix Table 8). Overall, the picture suggests similar impacts on out of pocket spending and no declines in earnings or income for this older population.

5 Impact of Hospital Admissions on Credit Report Outcomes

We now use the credit report data to analyze some of the “downstream consequences” of the direct economic consequences of a hospital admission for the household budget that we have just seen in the prior section. We focus on non-elderly insured adults, but also present parallel results for the elderly and for non-elderly uninsured adults.

5.1 Non-elderly insured adults

5.1.1 Unpaid bills

For the main analysis sample of non-elderly insured adults, Figure 2 presents results for our main outcomes graphically. Specifically, for each outcome, we plot the estimated coefficients on months relative to hospitalization (the \( \mu_r \)'s) from the non-parametric event study regression in equation (3) (see also (12) for the credit-report specific version). The non-parametric event time coefficients identify the change in outcomes relative to the month prior to hospitalization; \( \mu_{-1} \) is omitted from the regression and normalized to 0 for graphical presentation. The figures also plot the relationship between outcome and event time implied by the estimates from the parametric event study regression in equation (5), with the level normalized to match the non-parametric estimates. Primarily for space reasons, we show the post-hospitalization effects only out to 48 months (which is also the maximum follow-up period we can observe for all hospitalizations); in the robustness analysis below we present the results for longer follow up periods, both on our baseline sample and on a sub-sample with a balanced panel of admissions with longer post-hospitalization follow-up (see Appendix Figures 9 through 15). Panel A of Tables 3 and 4 summarize the implied effects of the hospitalization at 12 months and 48 months based on the parametric event study coefficients; Appendix Table 27 presents the underlying parametric event study coefficients. Since we will present a large number of outcomes using a similar format, we discuss the first outcome with relative more detail, and then summarize more quickly the results for other outcomes.

We consider first the impact of a hospital admission on the number of medical collections to date. The top left panel of Figure 2 presents the result graphically. It shows a clear “on impact” effect of the hospital admission on medical collections. This effect 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.\textsuperscript{26}

As discussed, our identifying assumption is that there are no omitted factors that are correlated with $y_{i,t}$ and the timing of hospital admission. One implication of this is that there should be no trend in outcomes in event time prior to admission. The event study for the number of medical collections to date shows some evidence of a pre-trend; it is statistically significant (see Appendix Table 27) but quantitatively very small. As discussed, we report all implied effects relative to a linear pre-trend.

The implied effects for the number of medical collections (Column 1 of Table 3, panel A) indicate that a hospitalization is associated with a (statistically significant) increase in the number of medical collections of 0.09 in the 12 months following a hospitalization and 0.16 in the 48 months following a hospitalization. Relative to the pre-hospitalization mean of 0.19 medical collections, this indicates a 50 percent increase in medical collections in the 12 months following the hospital admission and an 85 percent increase in the 48 months following the hospital admission.

The remaining panels of Figure 2 and columns of Table 3 report our results for our other collection measures. Hospital admissions are also associated with a statistically significant increase in non-medical collections, although the magnitude is considerably smaller: an increase of about 0.01 (1 percent) non-medical collections in the 12 months following a hospitalization, and an increase of about 0.03 is the 48 months following a hospitalization. In terms of the dollar amount of collections owed, hospitalizations are associated with a statistically significant increase in total collection balances of about $300 (25 percent) in the 48 months following a hospitalization. The result is disproportionately concentrated in medical collection balances, although there is some increase in non-medical collection balances.\textsuperscript{27} For simplicity, the model in Section 2 allows for unpaid medical bills only. It is possible that the small increase in non-medical collections reflects (un-modeled) non-payment of non-medical bills (such as utility bills). However, it is also possible that the increase in non-medical collections actually captures unpaid medical bills. While we can be fairly confident that “medical” collections reflect unpaid medical bills, the converse is less clear. Some non-medical collections may also reflect

\textsuperscript{26}In the collection analyses in Figure 2, there is visual evidence of a cyclical pattern to the non-parametric event study coefficients. Though the pattern is particularly pronounced post hospitalization, it is also visible in the pre-period for some outcomes; it also discernible in many of the non-collection outcomes analyzed in Figure 3. Recall that we observe each individual once every 12 months. Therefore, if there are systematic differences across admission months in characteristics that are correlated with the level of the outcome or the treatment effect of hospitalization on the outcome, that could produce this pattern. Chance sampling variation in who happened to go to the hospital in different months could also produce it. To explore whether these are systematic differences or chance sampling variation, we randomly split our sample in half, estimated the non-parametric event study on the two random samples, and computed the correlation of the residuals. We did this 30 times for each outcome analyzed in Figure 2, and found the residuals were usually highly correlated. This suggests there is systematic variation in our sample by admission month. 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 differences across admission months are primarily in the treatment effect rather than the pre hospitalization level of the outcome. 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.

\textsuperscript{27}We focus our discussion in total collection balances since, as discussed in Section 3, we can only measure medical and non medical collection balances separately starting in 2005 (as opposed to 2002 for all our other outcomes). We see a fairly pronounced pre-trend for medical balances in Figure 2, which may be somewhat of a mechanical reflection of our sample selection (conditioning on no prior hospitalizations in the past 3 years); as noted, our parametric specification reports effects relative to any pre-hospitalization (linear) trend.
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.

Finally, the top panel of Figure 3 and the first column of Table 4 (Panel A) shows the impact of hospital admissions on consumer bankruptcy, arguably the ultimate “unpaid bill.” We find that a hospital admission is associated with a (statistically significant) 0.13 percentage point increase in the probability of a bankruptcy within 12 months and a 0.4 percentage point increase within 48 months. Relative to the annual bankruptcy rate of 1.2 percent in this population, these results suggest that an index hospital admission raises for an insured adult raises annual bankruptcy risk by 33 percent.

5.1.2 Access to credit and borrowing

The remainder of Figure 3 and Table 4 (Panel A) shows the results for our other outcome measures. We estimate statistically significant declines in borrowing, credit limits, and credit scores. Consistent with our identifying assumption, credit card balances are flat in the period prior to hospitalization; however they are trending strongly upward (relative to the size of the post admission changes) for credit limits, credit scores, and bankruptcy.

We find that a hospital admission is associated with a decline in credit card balances (our primary proxy for borrowing) of about $300 (2.5 percent) in the 12 months following the admission, and about $1,000 (9 percent) over the 48 months following the admission. We also find that 48 months after a hospitalization, there is a decrease in the amount of automobile loan balances of $450 (7 percent). As discussed in the model in Section 2, a decline in borrowing following a health shocks suggests a relatively important role for uninsured income consequences compared to uninsured medical costs.

Credit limits \((L)\) decline by even more than balances - about $530 (1.5 percent) in the first 12 months and $2,000 (5.5 percent) after 48 months. A larger decline in credit limits than credit borrowing is also consistent with the model discussed in in Section 2, as is a decline in credit limits that is substantially less than the decline in PDV income that we estimated in the HRS. For perspective on the magnitude, the decline in credit limits following a hospital admission is over half the the decline in credit limits following an unemployment spell. As an alternative benchmark, Herkenhoff et al.’s (2015) estimates of the impact of credit limits in helping to dampen the adverse consequences of a job loss suggest that our 48-month estimated decline in credit limits would be associated with 0.2 to 0.6 percent lower re-employment wage following job loss.

Hospital admissions are associated with a decline in credit scores of 1.6 (.2 percent) over the first 12 months and a similar magnitude about 48 months. An impact on credit scores is consistent

---

28In terms of the framework from Section 2, bankruptcy can be thought of an extreme case of “unpaid bills”. We have not, however, formally modeled the costs of bankruptcy and the individual’s optimal bankruptcy decision. For a simple model of personal bankruptcy, see Wang and White (2000).

29Bethune (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 for insured non-elderly adults, and a $700 decline for uninsured non-elderly adults.

30As noted above, 96 percent of the sample has a credit score prior to hospitalization. We examined whether hospital admission affects the probability of having a credit score. Appendix Figure 1 and Appendix Table 20 column 1 report the results. 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.
with our findings of the impact of hospitalizations on collections and bankruptcies, both of which are considered “major derogatories that can affect the price of credit” (Avery et al. 2003). The decline in credit score is expected to raise borrowing costs ($r$). Quantitatively, our estimates suggest that the impact of a hospital admission on interest rates is likely to be small. The relationship between credit scores and the price of borrowing is non-linear; on average, however, recent estimates suggest that 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. 2015, Han et al. 2015); this suggests that our estimated decline in credit scores for non-elderly insured adults 48 months after a hospital admission of 1.8 would be associated with an increase in interest rates of less than 0.054 percent.

Thus far, our analysis has focused on the mean impacts of hospital admissions, which may mask important heterogeneity. Table 5, therefore reports 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 14). In general, the impacts at the 75th percentile tend to be fairly similar to the effects at the mean, and impacts at higher percentiles tend to be larger. Looking at collection balances (Panel A), at 48 months the mean increase is $302, the 90th percentile increase is $591, and the 95th is $1,292. For the other outcomes, the remaining panels suggest declines at the 90th percentile that are about three times the mean decline. For example, at 48 months after a hospital admission, the decline in credit card balances is $3,848 at the 90th percentile compared to the mean decline of $1,208. Likewise, the decline in credit card limits after 48 months is $5,624 at the 90th percentile, compared to $2,215 at the mean.

### 5.1.3 Consumption implications

We do not directly observe consumption. In Appendix G we try a variety of approaches to derive implied changes in consumption following a hospital admission. First, we use the framework from Section 2 (specifically, Proposition 2), together with our estimates of changes in credit limits ($\Delta L$) and credit card borrowing ($\Delta b$) to bound the change in average annual consumption following a health shock, relative to average annual household consumption (estimated from the Consumer Expenditure Survey). Second, we develop a number of alternative ways to proxy for the change in consumption. For example, under the assumptions that credit cards are used primarily as a means of exchange rather than a way to move consumption inter-temporally, and that the consumption financed on credit cards responds proportionally to other types of household consumption, we can interpret the percent decline in credit card balances directly as the percent change in consumption. Another approach we use follows on prior work using automobile expenditures as a proxy for total consumption (Mian et al., 2013) and

---

31 Our data use agreement prohibits our engaging in a quantitative analysis of the likely impact of various derogatories on credit scores.

32 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: we find that hospital admissions increase unpaid bills ($u$), which should serve to increase $r$, but also decrease $b$ which should serve to decrease $r$. It may also reflect differences in how these instruments are used as screening devices for borrowers; indeed, consistent with our findings, Agarwal et al. (2015) 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.
looks at declines in automobile loans as a proxy for automobile spending (and hence consumption).

Across a variety of conceptual approaches and implementation decisions, the results suggest a decline in average annual consumption following a hospital admission of 1 to 2 percent for non-elderly insured adults. The impacts at the 90th percentile on the outcomes we estimated above suggest 90th percentile impacts on consumption that are roughly 3 times these mean impacts. Interestingly, our 1 to 2 percent estimated average decline in consumption is in the same range as estimates of the impact of major illness on consumption in Indonesia. 33 As another point of comparison, average US unemployment spell is associated with a 6 to 8 percent decline in (food) consumption in the year following the unemployment shock (Gruber 1997; East and Kuka 2014); accounting for the pre-unemployment drop in consumption by those anticipating an unemployment spell, Hendren (2015) estimates the actual impact of unemployment on consumption may be as large as a 12 percent decline. Looking more broadly at job displacements, Stephens (2001) similarly estimates a 9 percent average annual decline in (food) consumption following a job displacement, with the effect relatively constant throughout the time horizon in the data (up to 6 years post displacement). 34 Consistent with consumption being smoother than net resources, Stephens (2001) estimates that the impact of job displacement on net of tax family income is about 45 percent larger than its impact on (food) consumption; in our context, the 1 to 2 percent average annual decline in consumption is coming from a “direct impact” of the hospital admission on average net annual income (out of pocket medical expenses and income) of about 20 percent (based on our HRS estimates which are of course not a directly comparable population to the credit report population).

Of course, consumption declines following a hospital admission are not inconsistent with individuals being fully insured, in the sense of having marginal utility of consumption equated across health states. If the marginal utility of consumption is lower in poor health, then full smoothing of marginal utilities across health states would not involve full smoothing of consumption. Indeed, a crude back-of-the-envelope calculation using the estimates of negative health state dependent utility in Finkelstein et al. (2013) suggests that optimal change in consumption following a hospital admission may be about 0.5 percent. 35 This raises the possibility that a meaningful share of overall consumption decline may

---

33 Using changes in self-reported non-medical consumption for approximately 4,000 individuals over a 2 year panel, Gertler and Gruber (2002) estimate that a minor illness (in which the head of the household reports experiencing any symptoms or symptoms that last more than a month) has no effect on household consumption, however a major illness (as measured by a change in self-reported activities of daily living (ADL)) does; specifically, they estimate that a change from the household head from being able to unable to perform one ADL lowers household non-medical consumption by 1.8 percent. Naturally, the health events and the measures are not directly comparable, but the broad comparison is still of interest.

34 A more direct comparison to estimated impacts of unemployment on outcomes we measure directly (credit card balances and credit card limits) similarly suggests that the impact of a hospital admission on these outcomes is about one-tenth to one-half that of becoming unemployed. Bethune (2015) uses the Survey of Consumer Finances (SCF) to estimate the impact on 2009 credit limits and credit card balances of becoming unemployed between 2007 and 2009. He estimates that, relative to a comparison group who remains employed, unemployment is associated with a decline in credit card balances of $2,500 and a decline in credit card limits of $925. By comparison, we estimate in Table 4 that for a non-elderly insured adult, the average hospital admission was associated with a decline in credit card balances of about $300 and a decline in credit card limits of about $500 over the first year following the hospital admission.

35 The stylized model in Finkelstein et al. (2013) implies that the optimal change in consumption following a health shock under health state dependence can be approximated by: \( \Delta \log(c) \approx (1/\theta) \times p \times \varphi \), where \( 1/\theta \) represents the inter-temporal elasticity of substitution, \( p \) represents the probability of health shock, and \( \varphi \) is the state-dependent utility parameter which represents the change in the marginal utility of consumption following the health shock. We choose
not represent “incomplete insurance” but rather an optimal response to expected negative health state dependence following a hospitalization.

5.1.4 Heterogeneity and robustness

We explored heterogeneity in effects across different types of hospitalizations and different types of individuals. Results for various subsamples are shown in Appendix Tables 32 through 37; corresponding event studies are shown in Appendix Figures 2 through 8 and 23 through 30. Our insured sample includes both privately insured and Medicaid covered individuals; we find smaller impacts of a hospital admission for those on Medicaid than those with private insurance. This may reflect the lower labor force attachment for those on Medicaid; consumer-cost sharing is similar for these groups.36 Authors’ calculation from MEPs and CPS. There is some evidence of larger impacts for admissions for chronic diseases, which would be consistent with these having larger subsequent impacts on either medical expenses or labor market participation.37 In a similar spirit, we find larger effects for hospital admissions with higher predicted list charges.

To try to focus in on potentially unanticipated hospital admissions, we looked separately at admissions through the ER - finding similar impacts to those not through the ER (Appendix Table 32) - and at admissions for particular conditions that may be less likely to be anticipated, such as heart attacks, car accidents, and external injuries (Appendix Table 35). In some cases the samples get quite small, but there is an obvious pattern of differential effects for these types of admissions. Results also look similar for admission across different types of hospitals (public, non-profit and for profit), and for the five most common reasons for admission.

Finally, because our analysis of impacts on out-of-pocket medical spending, earnings and income in the HRS is limited to 50-64 year olds, we produced a parallel set of results in the credit report data for insured individuals age 50-64 (see Appendix Tables 43-46 and Appendix Figures 44 and 45). Overall, the impacts are quantitatively similar. This would be consistent with similar-sized impacts of the hospital admission on income and out-of-pocket medical spending, although of course there could also be offsetting differences.

We also confirmed the robustness of our results to a number of alternative specifications. These included adding individual fixed effects, exploring potential bias due to attrition from mortality, examining the time pattern of the effects in more detail through various balanced-panel specifications, and adding back to the analysis the approximately 15 percent of individuals who had a prior hospital admission in the last three years. These analyses and results are discussed in detail in Appendix E.

1/ \theta = 0.5 following Gruber (2006), \rho = 0.06 for insured adults aged 25-64 (authors’ calculation from the MEPS), and \phi = −0.179 which represents the baseline estimate in Finkelstein et al. (2013) of the effect of one additional chronic disease on the marginal utility of consumption for the elderly; we (crudely) assume that a typical hospital admission for an insured adult has the same effect on the shape of the utility function. This gives approximate decline in consumption of Δc ≈ 0.5 \times 0.06 \times −0.179 = −0.005, or an average annual decline in consumption of roughly 0.5 percent.

36 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).

37 We classify individual ICD-9 diagnosis codes as chronic or non-chronic using a crosswalk developed by the Health Care Utilization Project (HCUP) available at https://www.hcup-us.ahrq.gov/toolssoftware/chronic/chronic.jsp#download [accessed on September 22, 2015]; this follows the method developed by Hwang et al. (2001).
5.2 The elderly

We conducted a parallel set of empirical analyses of the impact of hospital admissions for individuals 65 and over. Appendix Figures 42 and 43 present the event study analysis; Panel C of Tables 3 and 4 summarize the implied effects at 12 months and 48 months.\textsuperscript{38}

Overall, we interpret the results as showing limited or no impact of a hospital admission on non-collection outcomes for the elderly; estimated effects at higher quantiles are also generally small in magnitude, statistically insignificant, and fairly similar to the estimated effects at the mean (see Appendix Table 41). The one place where we find robust and quantitatively non-trivial estimates are for the impact of a hospital admission on collections, which is similar to the effect size for insured adults proportionally, although much smaller in levels (see Table 3).\textsuperscript{39} We find no substantive or statistical evidence of any impact of hospital admissions - in either the visual evidence of the estimated implied effects - on an increase in bankruptcy, on a decline in either borrowing measure or credit limits.\textsuperscript{40} The one non-collection outcome where there appears to be an effect for the elderly is on credit scores; there is robust evidence of an impact of hospital admissions for credit score both in the visual evidence and the point estimates, and the magnitude of the decline is similar to that for insured adults which, as we discussed above, is quantitatively unimportant.

This limited impact of hospital admissions on non-collection outcomes for the elderly underscores the relative importance of the uninsured income consequences of hospital admissions compared to uninsured medical expenses for non-elderly insured adults. Both groups have similar consumer cost-sharing (see authors’ calculations from the MEPs in Appendix Table 42) but very different labor force participation; adults in California aged 25-64 have a labor force participation rate of 80 percent, compared to only 15 percent for those 65 and older (authors’ calculations using 2000-2011 March Current Population Survey). As noted, in the HRS we found relatively little impact of hospital admissions on income for the elderly, but impacts for insured non-elderly that were 10 times larger than the impacts on out of pocket medical spending.

Naturally there are other differences between the elderly and non-elderly insured adults besides their labor force participation rates 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. Still, the lack of much of an impact for the elderly is striking, particularly given that, if anything the health shock itself appears more severe for the elderly; the index admission is associated with 50 percent longer length of stay and 25 percent higher list charges for the elderly than for non-elderly insured adults (see Table 1). Indeed, as we show in Appendix Table 23, when we re-weight the elderly sample to match the non-elderly insured sample on demographics (race and

\textsuperscript{38}The underlying parametric event study coefficients are reported in Appendix Table 29. A parallel set of sub-sample analyses by types of hospitalizations is shown in Appendix Tables 34 and 37, and a parallel set of robustness analysis is presenting in Appendix E.

\textsuperscript{39}Once again, for proportional estimates we can compare the estimated impact to the pre-hospitalization mean in Table 3, or consider the results from a proportional model using the quasi-maximum likelihood Poisson estimates in Appendix Tables 25 and 26.

\textsuperscript{40}We note a puzzlingly wrong-signed and statistically significant impact on automobile loans. But there are not statistically significant effects on bankruptcy, credit card borrowing or credit limits; the point estimates are usually wrong signed and substantively small compared to the estimates for non-elderly insured adults.
gender) and health conditions (diagnosis codes and length of stay), the results for the elderly on unpaid bills become smaller.

5.3 Non-elderly uninsured adults

As noted, there are insufficient sample sizes in the HRS to estimate the impact of hospital admissions on the uninsured non-elderly. A rough calculation in the MEPs data (see Appendix C for details), however, suggests that for an uninsured non-elderly adult, the average hospital admission is associated with about $2,700 in out of pocket medical spending over the first 12 months; the same calculation for insured non-elderly adults suggests 12-month out-of-pocket spending of $1,000 (which is roughly similar to our event-study estimate above in the HRS).

We can conduct a parallel set of empirical analyses of the impact of hospital admissions on credit report outcomes for uninsured non-elderly adults. Compared to insured non-elderly adults, we found much larger impacts on collections and on bankruptcy, but similar impacts on other outcomes. Figures 4 and 5 presents the analogous event study analysis for our sample of non-elderly uninsured adults; Panel B of Tables 3 and 4 summarize the implied effects at 12 months and 48 months. 41

The impact of a hospital admission on unpaid bills is much larger for the uninsured. For example, 48 months later, a hospital admission is associated with an increase in collection balances of $6,000 (170 percent) for the uninsured, compared to an increase of $270 (about 25 percent) for the insured. The right tail effects are also much larger for the uninsured, as shown in the quantile regression results in Appendix Table 40; the 90th percentile impact on collection balances is $23,000 for the uninsured (compared to $600 for the insured); at the 99th percentile, collection balances increase by over $80,000 for the uninsured, compared to $7,000 for the insured. Relatedly, the impact of a hospital admission on bankruptcy is also higher for the uninsured; 48 months later, a hospital admission is associated with a 1.4 percentage point increase in bankruptcy, compared to a 0.4 percentage point increase for the insured (the pre-hospitalization annual bankruptcy rate is similar at about 1.2 percent).

Interestingly, the impact of a hospital admission on the other outcomes in Table 4 - credit card balances, credit limits, and automobile balances - is quantitatively much more similar for the uninsured and insured than the impacts on unpaid bills (collections or bankruptcy). 42 In absolute terms, the impacts are smaller for the uninsured. For example, after 48 months a hospital admission is associated with a $700 decline in credit limits for the uninsured, and a $450 decline in credit card balances, compared to $2,215 and $1,208 for the insured. Of course, the pre-hospitalization mean outcomes for the insured are larger than the uninsured. In percentage terms, the impacts of hospital admissions are roughly similar for the insured and the uninsured at 48 months; results are similar when we estimate a

41 Appendix Table 28 presents the underlying parametric event study coefficients. A parallel set of sub-sample analyses by types of hospitalizations is shown in Appendix Tables 33 and 36 and a parallel set of robustness analyses is presented in Appendix E.

42 The results for the uninsured on credit scores (Table 4 column 3 and Figure 5) 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 examined whether hospital admission affects the probability of having a credit score. Appendix Figure 1 and Appendix Table 20 report the results. 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.
proportional model directly are similar (see the Poisson regression results in Appendix Tables 25 and 26). 12 months after the hospital admission, however, the percent decline in these outcomes is 2 to 4 times larger for the uninsured compared to the insured. It is natural for the differences in the impact of a hospital admission between insured and uninsured individuals to be greater in the shorter run (when medical expenses are a larger component of the total economic cost of the hospital admission) than in the longer run (when uninsured income consequences become a more important component of the total).

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 25 percent for the insured; see Appendix Table 14). 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 40).

6 Implications

We discuss the implications of these findings for the effective amount of coverage for those with formal health insurance and those without it. We also discuss the implications of our finding for rates of “medical bankruptcy”.

6.1 Coverage for the “uninsured”

The results are suggestive of relatively little difference in the economic impacts of a hospital admission for insured and uninsured non-elderly adults. Impacts on access to credit and borrowing are similar for the two groups at 48 months. Differences are confined to unpaid bills and bankruptcy where, as discussed, it is not clear how much these directly negatively impact the patient; by the same token, these results suggest that the incidence of lack of health insurance is falling at least partly on external parties who bear the costs of the unpaid bills.

Naturally one must exercise caution in interpreting any differential impacts of hospital admissions for insured and uninsured adults as the causal effect of insurance per se. There are other underlying differences between the groups, such as in the underlying severity of the health event, or differential effects on labor earnings. The credit outcomes we analyze may also have differential economic importance for the two groups due, for example to differential levels of per-existing savings. As noted, the uninsured also have a much higher share with zero initial credit limits or credit balances, which may mechanically depress declines in mean outcomes, although the similar effects for the insured and the uninsured also appeared at higher quantiles where such censoring is less of a concern. To try to adjust for observable differences between the two groups, Appendix Table 23 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 forthcoming). We limit the analysis to admissions occurring through the emergency room; we do not detect an impact on utilization age age 65.

We present these results in detail in Appendix F. The RD research strategy uses arguably more credible identifying variation than what our implicit difference-in-difference comparison of estimated impacts for the two groups. It has several downsides however: it has much lower power, involves a demographically distinct sample of elderly adults (rather than non-elderly adults as in our baseline sample), and requires 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.

Both the visual evidence (Appendix Figures 31 and 32) and the point estimates (Appendix Table 24) from the RD analysis indicate an impact of consumer cost sharing on unpaid medical bills, but no impacts on credit limits or borrowing. These results are consistent with our simple comparison of the impact of hospital admissions for uninsured and insured non-elderly adults, suggesting that the main differences in impacts are in unpaid bills. Quantitatively, the RD estimates imply that the impact of insurance coverage on unpaid bills may be about 50 percent larger than what we estimated above based on the difference-in-difference comparisons.

Overall, our findings both from the RD analysis and the simple comparison of impacts for insured and uninsured suggest much larger differential impact for the uninsured relative to the insured of a hospital admission on unpaid bills (collections and bankruptcy) than on access to credit or borrowing. This suggests that the declines in access to credit and borrowing that we find for the insured do not follow “mechanically” from the increase in unpaid bills that they experience, as the uninsured experience larger increases in unpaid bills without concomitantly larger changes on these other outcomes.

These findings are related to several strands in the literature. First, the larger impact on unpaid bills for the uninsured is consistent with the existing empirical literature that has documented that health insurance reduces measures of financial risk exposure and financial strain, including: out of pocket medical spending, medical debt, difficulty paying non-medical bills, and bankruptcy. Second, our findings that the nominally uninsured in fact have a fair amount of implicit, informal insurance is consistent with other work documenting sources of implicit insurance for the uninsured (e.g. Mahoney 2015). Third, and relatedly, the lack of larger differences in the impact of hospital admissions for adults with and without insurance for outcomes other than unpaid bills is also consistent with recent work suggesting that a large share of the “uninsured’s” medical costs are not, in fact, paid for by the uninsured; this in turn may dampen the benefits of formal insurance coverage to the nominally uninsured (Garthwaite et al. 2015; Finkelstein et al. 2015). In this vein, our findings provide some suggestive evidence of the benefits to health care providers from insurance coverage. In particular, we estimate that for a non-elderly insured adult, the average hospital admission generates about $300 in

---

43This literature includes evidence from Medicaid expansions (Gross and Notowidigdo 2011; 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 F 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 forthcoming).
unpaid bills. By contrast, we estimate the average hospital admission generates over $6,000 in unpaid bills for uninsured adults; as discuss, the RD estimates suggest even larger causal effects of insurance. Of course unpaid bills are based on charges (not hospital costs) which complicates interpretation of the impact of insurance on unpaid bills, since charges (prices) may also differ by insurance status.

6.2 Lack of coverage for the “insured”

The results also suggest that those with formal health insurance still face considerable economic risk from hospital admissions. We considered two potential sources of the “incomplete insurance” for those with health insurance: uncovered medical expenses and uninsured income consequences of the hospital admission.

Several of our findings have suggested an important role for uninsured income consequences. First, in the HRS analysis, we estimated an impact of hospital admission on household income for non-elderly insured adults that was over ten times larger than the impact on out of pocket medical spending. Relatedly, our estimates imply that the underlying medical expense shock is the same order of magnitude as the income shock, but that health insurance (by design) covers a large portion of medical expenses but no portion of lost income. Second, in the HRS we estimated similar impacts on out of pocket medical spending for the elderly but little impact on household income. Third, and presumably relatedly, in the credit report data we found little impact on credit limits or borrowing for the elderly. Fourth, we found much greater differences in the impact of a hospital admission for the insured relative to the uninsured in the shorter run (i.e. 12 months) - when medical expenses are a larger component of the total economic cost of the hospital admission - than in the longer run (i.e. 48 months) - when uninsured income consequences become a more important component of the total. Finally, we estimate that hospital admissions decrease borrowing for non-elderly adults (both insured and uninsured) which, as discussed in Section 2 suggests a relatively important role for uninsured income consequences compared to uninsured medical costs.

To try to quantify the role of uncovered medical expenses in contributing to the estimated impacts of a hospital admission on credit limits and borrowing for insured non-elderly adults, we estimate what the (counter-factual) impact of a hospital admission would be if individuals with health insurance had coverage for all medical expenses (i.e., \( \lambda_m = 1 \)); we denote this counter-factual change in outcome by \( \Delta y|\lambda_m=1 \). We define the share (s) of the impact for insured individuals (\( \Delta y|\lambda_{insured} \)) that is accounted for by uninsured medical expenses:

\[
s = 1 - \frac{\Delta y|\lambda_m=1}{\Delta y|\lambda_{insured}}. \tag{6}
\]

Since we do not observe anyone in the data with complete coverage of medical expenses (i.e., \( \lambda_m = 1 \)), we linearly extrapolate from our estimated impact of a hospital admission for the uninsured (\( \Delta y|\lambda_{uninsured} \)) in the numerator of equation (6) as follows:

\[
\Delta y|\lambda_m=1 = \Delta y|\lambda_{insured} + (1 - \lambda_m^{insured}) \frac{\Delta y|\lambda_{insured} - \Delta y|\lambda_{uninsured}}{\lambda_m^{insured} - \lambda_m^{uninsured}}. \tag{7}
\]

Intuitively, we extrapolate to the impact of a hospital admission on a change in outcome for those with
full medical insurance ($\lambda_m = 1$), based on our estimates of the impact of hospital admissions for those with insurance ($\Delta y|_{\lambda^{\text{insured}}_m}$) and those without insurance ($\Delta y|_{\lambda^{\text{uninsured}}_m}$), and data from the MEPS on the share of medical costs covered for those with insurance ($\lambda^{\text{insured}}_m$) and those without insurance ($\lambda^{\text{uninsured}}_m$). We estimate $s$ using both 12-month and 48-month estimates of the impact of hospital admissions.

We use two approaches to estimating $\Delta y|_{\lambda^{\text{uninsured}}_m}$. First, we simply use the raw changes in outcomes for the uninsured, implicitly assuming that any differences from the insured represent the causal effects of consumer cost sharing (i.e. $\lambda_m$). As we will see, this approach will produce quite small estimates of $s$, because of the similarity in our estimates of the percentage decline in outcomes following a hospital admission for the uninsured and the insured. To allow for a potentially larger role for consume cost-sharing we report a second approach to estimating $\Delta y|_{\lambda^{\text{uninsured}}_m}$, in which we adjust our estimates to account for the fact that RD estimates of the impact of insurance coverage tend to find (where we have power) a 50 percent larger effect than the “naive” difference-in-difference comparison (see Section 5.3). Specifically, we scale up the magnitude of all estimated impacts for the uninsured by 50 percent. This likely overstates the impacts for uninsured’s (and hence the role of consumer cost-sharing) since the RD estimates in fact suggest no effect of insurance coverage on $L$ or $b$.

We use data from the MEPS to estimate $\lambda^{\text{insured}}_m$ and $\lambda^{\text{uninsured}}_m$. We assume $\lambda^{\text{insured}}_m = 0.08$. We report results for a wide range of estimates for $\lambda^{\text{uninsured}}_m$ (0.35 to 0.82), reflecting the considerable uncertainty about the degree of informal insurance held by the nominally “uninsured”. Our estimate of $s$ is driven thus both by the relatively similar impacts of hospital admissions for those with and without insurance that we discussed in Section 5.3, and by the degree of consumer cost-sharing that insurance - and lack of it - provides. We present our approach and results in detail in Appendix F and Appendix Table 39.

Overall, our results suggest a limited role for uncovered medical expenses in contributing to the economic impact of a hospital admission for a non-elderly insured adults. The role of uncovered medical expenses is larger in the shorter run (12 months) than in what might be more reasonably assumed to be steady state (48 months). This follows directly from our estimates in Table 4, and is what we would expect given the front-loaded nature of medical expenses compared to any income consequences of a hospital admission. Looking across the range of estimates and assumptions, we consistently find that, 48-months later, uncovered medical expenses can explain less than 20 percent of the decline in outcomes for insured adults, and that this number may reasonably be much closer to zero; most of the estimates suggest a role for uninsured medical expenses on the order of explaining 0 to 5 percent of the decline in outcomes. This is broadly consistent with our estimates (for an older population) in the HRS that the direct out of pocket medical expenses associated with a hospital admission for the insured are less than one-tenth that of the income consequences.

### 6.3 Medical bankruptcies

A growing empirical literature examines the impact of various economic shocks on consumer bankruptcy (e.g., 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 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 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. Both papers are unable to reject the null hypothesis of no causal effect of the medical event analyzed on bankruptcy.

We can use the empirical results on the impact of a hospital admission on bankruptcy (Table 4) to analyze the role of hospitalizations in contributing to bankruptcy rates. Our estimates indicate that, for the baseline, non-elderly insured adults, a non-childbirth hospital admission is associated with a (statistically significant) 0.13 percentage point increase in the probability of a bankruptcy within 12 months and a 0.4 percentage point increase within 48 months. The estimates are substantially larger for uninsured adults; a non-childbirth hospital admission is associated with a (statistically significant) 0.5 percentage increase in the probability of bankruptcy within 12 months and a 1.4 percentage point increase in the probability of bankruptcy within 48 months. If one were to interpret the differences in estimates for insured and uninsured causally, it would suggest a large change in bankruptcy risk due to health insurance at the time of hospitalization, consistent with results using aggregate data from Gross and Notowidigdo (2011) and Mazumder and Miller (2015) that health insurance reduces the risk of bankruptcy. Finally, we find no evidence of a statistically significant effect of hospital admissions on bankruptcies for individuals aged 65 and over; this is consistent with the main channel for the economic impacts of hospital admissions coming from labor market consequences.

These estimates imply that hospital admissions are pivotal for about 3 percent of bankruptcies for non-elderly insured adults, given their annual bankruptcy rate of 0.8 percent and an annual non-childbirth hospitalization rate of 5.7 percent (authors’ calculations from MEPS). For uninsured adults aged 25-64 - who have a similar annual bankruptcy rate but a lower annual non-childbirth hospitalization rate of 2.9 percent (authors’ calculations from MEPS) - the estimates suggest that hospital admissions are pivotal in about 5 percent of bankruptcies. As noted, we estimate no impact of hospital admissions on bankruptcy for the elderly. Our interpretation of these results is that the driving force behind “medical bankruptcies” for insured non-elderly adults is the lost labor market earnings, and for uninsured non-elderly adults the impact is higher because there are also uncovered

---

44 We estimate the non-elderly bankruptcy rate 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.
medical expenses; the elderly have both considerable coverage of medical expenses (like the non-elderly insured) and little earnings risk, and therefore do not experience “medical bankruptcies.”

Our estimates are 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 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). Overall, our results provide evidence of a statistically significant impact of hospital admissions on bankruptcies - for both insured and uninsured non-elderly adults - but suggest that the share of “medical bankruptcies” may be lower than the prior literature on this subject has concluded.

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 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 findings underscore the nature of insurance - and the lack thereof - in the United States. Health insurance is designed to cover (a portion of) the medical expenses associated with a health event. There is a relatively sparse patchwork of insurance against the earnings consequences of these health shocks through voluntary employer provision of sick pay and private disability insurance; 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.

Our findings are positive, not normative. Additional assumptions are required for drawing inferences about consumer welfare or optimal insurance design. For example, while our results 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 of the 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.

8 References


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

Out-of-Pocket Medical Spending

Respondent Earnings

Spousal Earnings

Household Government Transfers

Total Household Income

Notes: The sample is the non-elderly insured (see Table 1, column 1). The points in each figure represent the estimated effects of event time (i.e. the $\hat{\mu}_t$'s from equation 3); the dashed line represents the estimated trendline from equation 4 with the level normalized to match the non-parametric estimates. All estimates are weighted using survey weights designed for population representativeness following the Current Population Survey guidelines.
Figure 2: Impact of Hospitalizations on Collections, Non-Elderly Insured

Notes: The sample is the non-elderly insured (see Table 1, column 2). The points in each figure represent the estimated effects of event time (i.e. the \( \hat{\mu}_r \)'s from equation 3); the dashed line represents the estimated event study coefficients from 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 only observed beginning in 2005.
Figure 3: Impact of Hospitalizations on Other Credit Report Outcomes, Non-Elderly Insured

Notes: See notes to Figure 2.
Figure 4: Impact of Hospitalizations on Collections, Non-Elderly Uninsured

Notes: The sample is the non-elderly uninsured (see Table 1, column 5). The points in each figure represent the estimated effects of event time (i.e. the $\hat{\mu}_t$’s from equation 3); the dashed line represents the estimated event study coefficients from equation 5 with the level normalized to match the non-parametric estimates. All estimates are weighted to adjust for oversampling of some groups. All variables are observed from 2002 to 2011, except medical and non-medical collection balances which are only observed beginning in 2005.
Figure 5: Impact of Hospitalizations on Other Credit Report Outcomes, Non-Elderly Uninsured

Notes: See notes to Figure 4.
### Table 1: Sample Characteristics

<table>
<thead>
<tr>
<th>Data Source</th>
<th>Non-Elderly Insured</th>
<th>Elderly Insured</th>
<th>Non-Elderly Uninsured</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>HRS (1)</td>
<td>HRS (3)</td>
<td>Credit Report Sample (5)</td>
</tr>
<tr>
<td>Age at admission</td>
<td>57.6</td>
<td>74.9</td>
<td>77.0</td>
</tr>
<tr>
<td>Male (%)</td>
<td>48.2</td>
<td>42.9</td>
<td>46.0</td>
</tr>
<tr>
<td>Year of admission</td>
<td>2002.3</td>
<td>2003.7</td>
<td>2005.0</td>
</tr>
<tr>
<td>Has spouse in survey wave preceding hospitalization (%)</td>
<td>75.2</td>
<td>53.6</td>
<td>n/a</td>
</tr>
<tr>
<td>Hispanic (%)</td>
<td>5.3</td>
<td>6.0</td>
<td>10.6</td>
</tr>
<tr>
<td>Black (%)</td>
<td>9.7</td>
<td>7.9</td>
<td>5.3</td>
</tr>
<tr>
<td>White (%)</td>
<td>86.0</td>
<td>88.7</td>
<td>75.8</td>
</tr>
<tr>
<td>Other Race (%)</td>
<td>4.2</td>
<td>3.4</td>
<td>8.3</td>
</tr>
<tr>
<td>Length of Stay (days)</td>
<td>n/a</td>
<td>4.1</td>
<td>6.0</td>
</tr>
<tr>
<td>Hospital List Charges ($)</td>
<td>n/a</td>
<td>45,580</td>
<td>56,609</td>
</tr>
<tr>
<td>Medicaid (%)</td>
<td>6.2</td>
<td>6.5</td>
<td>1.0</td>
</tr>
<tr>
<td>Private (%)</td>
<td>93.9</td>
<td>94.4</td>
<td>9.4</td>
</tr>
<tr>
<td>Hospital Non Profit (%)</td>
<td>n/a</td>
<td>74.4</td>
<td>76.2</td>
</tr>
<tr>
<td>Hospital For Profit (%)</td>
<td>n/a</td>
<td>16.3</td>
<td>15.4</td>
</tr>
<tr>
<td>Hospital Public (%)</td>
<td>n/a</td>
<td>9.4</td>
<td>8.4</td>
</tr>
<tr>
<td>Admitted through Emergency Department (%)</td>
<td>n/a</td>
<td>47.9</td>
<td>58.6</td>
</tr>
<tr>
<td>Re-Admitted to Hospital Within 12 Months (%)</td>
<td>23.1</td>
<td>0</td>
<td>20.1</td>
</tr>
<tr>
<td>Re-Admitted to Hospital Within 48/36 Months (%)</td>
<td>36.0</td>
<td>0</td>
<td>35.1</td>
</tr>
<tr>
<td>Died within 12 Months (%)</td>
<td>0</td>
<td>15.2</td>
<td>3.9</td>
</tr>
<tr>
<td>Died within 48 Months (%)</td>
<td>n/a</td>
<td>6.3</td>
<td>30.6</td>
</tr>
<tr>
<td>Insured within 12 Months (%)</td>
<td>92.9</td>
<td>99.2</td>
<td>99.7</td>
</tr>
<tr>
<td>Insured within 48/36 Months (%)</td>
<td>92.6</td>
<td>99.0</td>
<td>99.7</td>
</tr>
<tr>
<td>Individuals</td>
<td>4,359</td>
<td>378,190</td>
<td>409,030</td>
</tr>
</tbody>
</table>

**Notes:** Age is defined at admission. Non-elderly adults 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 Current Population Survey (CPS) non-institutionalized population weights for the HRS sample. All hospitalizations that are pregnancy related (MDC = 14) have been dropped from the credit report sample.

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

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 to the 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 is not accurately measured at later points in time.

In the credit report sample, black, white, other race and Hispanic are mutually exclusive; in the HRS, “Hispanic” is asked separately from race.

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.
### Table 2
Impact of Hospitalization on Selected Outcomes in the HRS

<table>
<thead>
<tr>
<th></th>
<th>Out-of-Pocket Medical Spending</th>
<th>Respondent Earnings</th>
<th>Spousal Earnings</th>
<th>Household Government Transfers</th>
<th>Total Household Income</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
</tr>
<tr>
<td><strong>Panel A. Non-Elderly Insured</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>12-month effect&lt;sup&gt;a&lt;/sup&gt;</td>
<td>2,162</td>
<td>-17,127</td>
<td>-858</td>
<td>795</td>
<td>-19,827</td>
</tr>
<tr>
<td></td>
<td>(182)</td>
<td>(10,156)</td>
<td>(2,636)</td>
<td>(320)</td>
<td>(11,076)</td>
</tr>
<tr>
<td>Average annual effect after 36 months&lt;sup&gt;b&lt;/sup&gt;</td>
<td>1,184</td>
<td>-16,594</td>
<td>-169</td>
<td>1,311</td>
<td>-18,123</td>
</tr>
<tr>
<td></td>
<td>(144)</td>
<td>(8,525)</td>
<td>(2,293)</td>
<td>(291)</td>
<td>(9,221)</td>
</tr>
<tr>
<td>Pre-hospitalization mean</td>
<td>2,159</td>
<td>46,673</td>
<td>28,328</td>
<td>3,410</td>
<td>95,345</td>
</tr>
<tr>
<td>Number of Individuals</td>
<td>4,359</td>
<td>4,359</td>
<td>4,359</td>
<td>4,359</td>
<td>4,359</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>22,582</td>
<td>22,582</td>
<td>22,582</td>
<td>22,582</td>
<td>22,582</td>
</tr>
<tr>
<td><strong>Panel B. Elderly</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>12-month effect&lt;sup&gt;a&lt;/sup&gt;</td>
<td>1,314</td>
<td>1,748</td>
<td>381</td>
<td>-1,506</td>
<td>-4,766</td>
</tr>
<tr>
<td></td>
<td>(164)</td>
<td>(1,564)</td>
<td>(835)</td>
<td>(309)</td>
<td>(3,166)</td>
</tr>
<tr>
<td></td>
<td>[&lt;.001]</td>
<td>[.26]</td>
<td>[.65]</td>
<td>[&lt;.001]</td>
<td>[.13]</td>
</tr>
<tr>
<td>Average annual effect after 36 months&lt;sup&gt;b&lt;/sup&gt;</td>
<td>709</td>
<td>2,640</td>
<td>1,072</td>
<td>-1,507</td>
<td>-1,874</td>
</tr>
<tr>
<td></td>
<td>(129)</td>
<td>(1,376)</td>
<td>(705)</td>
<td>(269)</td>
<td>(2,579)</td>
</tr>
<tr>
<td></td>
<td>[&lt;.001]</td>
<td>[.055]</td>
<td>[.13]</td>
<td>[&lt;.001]</td>
<td>[.47]</td>
</tr>
<tr>
<td>Pre-hospitalization mean</td>
<td>2,521</td>
<td>8,281</td>
<td>4,683</td>
<td>15,816</td>
<td>51,243</td>
</tr>
<tr>
<td>Number of Individuals</td>
<td>5,785</td>
<td>5,785</td>
<td>5,785</td>
<td>5,785</td>
<td>5,785</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>29,441</td>
<td>29,441</td>
<td>29,441</td>
<td>29,441</td>
<td>29,441</td>
</tr>
</tbody>
</table>

**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 CPS non-institutionalized population 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.

<sup>a</sup> 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 $\mu_0$. 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.

<sup>b</sup> 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

<table>
<thead>
<tr>
<th>Number of Collections</th>
<th>Collection Balances</th>
</tr>
</thead>
<tbody>
<tr>
<td>Medical</td>
<td>Non-Medical</td>
</tr>
<tr>
<td>(1)</td>
<td>(2)</td>
</tr>
</tbody>
</table>

Panel A. Non-Elderly Insured

<table>
<thead>
<tr>
<th>12-month effect(^a)</th>
<th>.095</th>
<th>.011</th>
<th>.11</th>
<th>127</th>
<th>18</th>
<th>122</th>
</tr>
</thead>
<tbody>
<tr>
<td>(.002)</td>
<td>(.003)</td>
<td>(.005)</td>
<td>(7)</td>
<td>(16)</td>
<td>(13)</td>
<td></td>
</tr>
<tr>
<td>[&lt;.001]</td>
<td>[.0011]</td>
<td>[&lt;.001]</td>
<td>[&lt;.001]</td>
<td>[.26]</td>
<td>[&lt;.001]</td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>48-month effect(^b)</th>
<th>.18</th>
<th>.034</th>
<th>.21</th>
<th>271</th>
<th>101</th>
<th>302</th>
</tr>
</thead>
<tbody>
<tr>
<td>(.008)</td>
<td>(.014)</td>
<td>(.019)</td>
<td>(18)</td>
<td>(47)</td>
<td>(37)</td>
<td></td>
</tr>
<tr>
<td>[&lt;.001]</td>
<td>[.017]</td>
<td>[&lt;.001]</td>
<td>[&lt;.001]</td>
<td>[.03]</td>
<td>[&lt;.001]</td>
<td></td>
</tr>
</tbody>
</table>

Pre-hospitalization mean | .2 | .72 | .92 | 292 | 1,086 | 1,230 |

Number of Individuals | 383,718 | 383,718 | 383,718 | 375,844 | 375,844 | 383,718 |

Number of Observations | 3,131,534 | 3,131,534 | 3,131,534 | 2,208,517 | 2,208,517 | 3,131,534 |

Panel B. Non-Elderly Uninsured

<table>
<thead>
<tr>
<th>12-month effect(^a)</th>
<th>.85</th>
<th>.12</th>
<th>.97</th>
<th>4,259</th>
<th>246</th>
<th>4,469</th>
</tr>
</thead>
<tbody>
<tr>
<td>(.008)</td>
<td>(.007)</td>
<td>(.012)</td>
<td>(45)</td>
<td>(36)</td>
<td>(51)</td>
<td></td>
</tr>
<tr>
<td>[&lt;.001]</td>
<td>[&lt;.001]</td>
<td>[&lt;.001]</td>
<td>[&lt;.001]</td>
<td>[&lt;.001]</td>
<td>[&lt;.001]</td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>48-month effect(^b)</th>
<th>1.2</th>
<th>.11</th>
<th>1.3</th>
<th>6,144</th>
<th>195</th>
<th>6,199</th>
</tr>
</thead>
<tbody>
<tr>
<td>(.028)</td>
<td>(.028)</td>
<td>(.045)</td>
<td>(102)</td>
<td>(100)</td>
<td>(130)</td>
<td></td>
</tr>
<tr>
<td>[&lt;.001]</td>
<td>[&lt;.001]</td>
<td>[&lt;.001]</td>
<td>[&lt;.001]</td>
<td>[.051]</td>
<td>[&lt;.001]</td>
<td></td>
</tr>
</tbody>
</table>

Pre-hospitalization mean | .59 | 1.7 | 2.3 | 1,292 | 2,762 | 3,529 |

Number of Individuals | 153,617 | 153,617 | 153,617 | 151,343 | 151,343 | 153,617 |

Number of Observations | 1,256,759 | 1,256,759 | 1,256,759 | 913,516 | 913,516 | 1,256,759 |

Panel C. Elderly

<table>
<thead>
<tr>
<th>12-month effect(^a)</th>
<th>.026</th>
<th>0</th>
<th>.027</th>
<th>17</th>
<th>4</th>
<th>24</th>
</tr>
</thead>
<tbody>
<tr>
<td>(.001)</td>
<td>(.002)</td>
<td>(.002)</td>
<td>(3)</td>
<td>(11)</td>
<td>(8)</td>
<td></td>
</tr>
<tr>
<td>[&lt;.001]</td>
<td>[.8]</td>
<td>[&lt;.001]</td>
<td>[&lt;.001]</td>
<td>[.74]</td>
<td>[.0018]</td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>48-month effect(^b)</th>
<th>.049</th>
<th>.011</th>
<th>.038</th>
<th>37</th>
<th>39</th>
<th>84</th>
</tr>
</thead>
<tbody>
<tr>
<td>(.004)</td>
<td>(.008)</td>
<td>(.01)</td>
<td>(8)</td>
<td>(34)</td>
<td>(24)</td>
<td></td>
</tr>
<tr>
<td>[&lt;.001]</td>
<td>[.19]</td>
<td>[&lt;.001]</td>
<td>[&lt;.001]</td>
<td>[.25]</td>
<td>[&lt;.001]</td>
<td></td>
</tr>
</tbody>
</table>

Pre-hospitalization mean | .048 | .19 | .24 | 75 | 422 | 428 |

Number of Individuals | 414,547 | 414,547 | 414,547 | 387,839 | 387,839 | 414,547 |

Number of Observations | 2,959,802 | 2,959,802 | 2,959,802 | 1,946,208 | 1,946,208 | 2,959,802 |

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 2304*Beta_2_hat+110592*Beta_3_hat+46656*Beta_4_hat+13824*Beta_5_hat
### Table 4
Impact of Hospitalization on Other Credit Report Outcomes

<table>
<thead>
<tr>
<th></th>
<th>Any Bankruptcy to Date</th>
<th>Credit Limit</th>
<th>Credit Score</th>
<th>Credit Card Balances</th>
<th>Automobile Loan Balance</th>
</tr>
</thead>
<tbody>
<tr>
<td>Panel A. Non-elderly Insured</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>12-month effect</td>
<td>.0013 (0.00031)</td>
<td>-515</td>
<td>-1.6</td>
<td>-293</td>
<td>-102</td>
</tr>
<tr>
<td>48-month effect</td>
<td>.0042 (0.00092)</td>
<td>-2,215</td>
<td>-1.8</td>
<td>-1,208</td>
<td>-507</td>
</tr>
<tr>
<td>Pre-hospitalization mean</td>
<td>.034 (0.00031)</td>
<td>37,664</td>
<td>731</td>
<td>11,942</td>
<td>6,684</td>
</tr>
<tr>
<td>Number of Individuals</td>
<td>383,718</td>
<td>383,718</td>
<td>371,715</td>
<td>383,718</td>
<td>383,718</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>3,131,534</td>
<td>3,131,534</td>
<td>2,942,253</td>
<td>3,131,534</td>
<td>3,131,534</td>
</tr>
<tr>
<td>Panel B. Non-Elderly Uninsured</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>12-month effect</td>
<td>.0048 (0.00046)</td>
<td>-678</td>
<td>-5</td>
<td>-264</td>
<td>-267</td>
</tr>
<tr>
<td>48-month effect</td>
<td>.014 (0.0014)</td>
<td>-690</td>
<td>6.6</td>
<td>-443</td>
<td>-349</td>
</tr>
<tr>
<td>Pre-hospitalization mean</td>
<td>.037 (0.00046)</td>
<td>15,145</td>
<td>655</td>
<td>5,376</td>
<td>3,981</td>
</tr>
<tr>
<td>Number of Individuals</td>
<td>153,617</td>
<td>153,617</td>
<td>137,913</td>
<td>153,617</td>
<td>153,617</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>1,256,759</td>
<td>1,256,759</td>
<td>1,017,096</td>
<td>1,256,759</td>
<td>1,256,759</td>
</tr>
<tr>
<td>Panel C. Elderly</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>12-month effect</td>
<td>-.00019 (.00022)</td>
<td>370</td>
<td>-1.4</td>
<td>72</td>
<td>69</td>
</tr>
<tr>
<td>48-month effect</td>
<td>-.001 (.00072)</td>
<td>-448</td>
<td>-3.3</td>
<td>-30</td>
<td>194</td>
</tr>
<tr>
<td>Pre-hospitalization mean</td>
<td>.016 (.00022)</td>
<td>36,967</td>
<td>824</td>
<td>7,016</td>
<td>2,143</td>
</tr>
<tr>
<td>Number of Individuals</td>
<td>414,547</td>
<td>414,547</td>
<td>405,389</td>
<td>414,547</td>
<td>414,547</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>2,959,802</td>
<td>2,959,802</td>
<td>2,833,027</td>
<td>2,959,802</td>
<td>2,959,802</td>
</tr>
</tbody>
</table>

**Notes:** See notes to Table 3.
Table 5
Quantile Effects, Non-Elderly Insured

<table>
<thead>
<tr>
<th></th>
<th>Mean Effect Estimates (OLS)</th>
<th>Unconditional Quantile Effect Estimates</th>
<th>75th Percentile</th>
<th>90th Percentile</th>
<th>95th Percentile</th>
<th>99th Percentile</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td></td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
</tr>
<tr>
<td>Panel A: Collection Balances</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>12-Month Effect Estimate</td>
<td>122</td>
<td>102</td>
<td>318</td>
<td>476</td>
<td>2,427</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(13)</td>
<td>(5.8)</td>
<td>(33)</td>
<td>(73)</td>
<td>(463)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>[&lt;.001]</td>
<td>[&lt;.001]</td>
<td>[&lt;.001]</td>
<td>[&lt;.001]</td>
<td>[&lt;.001]</td>
<td></td>
</tr>
<tr>
<td>48-Month Effect Estimate</td>
<td>302</td>
<td>153</td>
<td>591</td>
<td>1,292</td>
<td>7,021</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(37)</td>
<td>(17)</td>
<td>(94)</td>
<td>(212)</td>
<td>(1,383)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>[&lt;.001]</td>
<td>[&lt;.001]</td>
<td>[&lt;.001]</td>
<td>[&lt;.001]</td>
<td>[&lt;.001]</td>
<td></td>
</tr>
<tr>
<td>Pre-hospitalization mean/percentile</td>
<td>1,230</td>
<td>336</td>
<td>3,625</td>
<td>8,093</td>
<td>28,024</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Panel B: Credit Limit</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>12-Month Effect Estimate</td>
<td>-515</td>
<td>-657</td>
<td>-2,265</td>
<td>-3,234</td>
<td>-1,847</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(154)</td>
<td>(203)</td>
<td>(632)</td>
<td>(1,522)</td>
<td>(4,741)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>[&lt;.001]</td>
<td>[.0012]</td>
<td>[&lt;.001]</td>
<td>[.034]</td>
<td>[.7]</td>
<td></td>
</tr>
<tr>
<td>48-Month Effect Estimate</td>
<td>-2,215</td>
<td>-1,990</td>
<td>-5,624</td>
<td>-7,552</td>
<td>-7,004</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(440)</td>
<td>(571)</td>
<td>(1,759)</td>
<td>(4,345)</td>
<td>(14,642)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>[&lt;.001]</td>
<td>[&lt;.001]</td>
<td>[.0014]</td>
<td>[.082]</td>
<td>[.63]</td>
<td></td>
</tr>
<tr>
<td>Pre-hospitalization mean/percentile</td>
<td>37,664</td>
<td>49,818</td>
<td>118,200</td>
<td>192,700</td>
<td>419,220</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Panel C: Credit Score</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>12-Month Effect Estimate</td>
<td>-1.6</td>
<td>-1.6</td>
<td>-.87</td>
<td>-.43</td>
<td>0</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(.18)</td>
<td>(.38)</td>
<td>(.34)</td>
<td>(.52)</td>
<td>(0)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>[&lt;.001]</td>
<td>[&lt;.001]</td>
<td>[.011]</td>
<td>[.42]</td>
<td>[.]</td>
<td></td>
</tr>
<tr>
<td>48-Month Effect Estimate</td>
<td>-1.8</td>
<td>-1.7</td>
<td>-.13</td>
<td>0.013</td>
<td>0</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(.45)</td>
<td>(1)</td>
<td>(.89)</td>
<td>(1.4)</td>
<td>(0)</td>
<td></td>
</tr>
<tr>
<td>Pre-hospitalization mean/percentile</td>
<td>731</td>
<td>847</td>
<td>903</td>
<td>933</td>
<td>990</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Panel D: Credit Card Balances</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>12-Month Effect Estimate</td>
<td>-293</td>
<td>-245</td>
<td>-1,207</td>
<td>-2,593</td>
<td>-5,783</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(94)</td>
<td>(74)</td>
<td>(362)</td>
<td>(824)</td>
<td>(3,060)</td>
<td></td>
</tr>
<tr>
<td>48-Month Effect Estimate</td>
<td>-1,208</td>
<td>-910</td>
<td>-3,848</td>
<td>-10,947</td>
<td>-9,393</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(253)</td>
<td>(190)</td>
<td>(947)</td>
<td>(2,207)</td>
<td>(8,748)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>[&lt;.001]</td>
<td>[&lt;.001]</td>
<td>[&lt;.001]</td>
<td>[&lt;.001]</td>
<td>[.28]</td>
<td></td>
</tr>
<tr>
<td>Pre-hospitalization mean/percentile</td>
<td>11,942</td>
<td>10,533</td>
<td>37,998</td>
<td>77,506</td>
<td>223,819</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Panel E: Automobile Loan Balances</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>12-Month Effect Estimate</td>
<td>-102</td>
<td>-233</td>
<td>-360</td>
<td>-240</td>
<td>-81</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(28)</td>
<td>(80)</td>
<td>(110)</td>
<td>(174)</td>
<td>(489)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>[&lt;.001]</td>
<td>[0.0034]</td>
<td>[0.01]</td>
<td>[.17]</td>
<td>[.87]</td>
<td></td>
</tr>
<tr>
<td>48-Month Effect Estimate</td>
<td>-507</td>
<td>-1,326</td>
<td>-1,488</td>
<td>-2,061</td>
<td>-3,161</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(71)</td>
<td>(202)</td>
<td>(258)</td>
<td>(394)</td>
<td>(1,022)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>[&lt;.001]</td>
<td>[&lt;.001]</td>
<td>[&lt;.001]</td>
<td>[&lt;.001]</td>
<td>[.002]</td>
<td></td>
</tr>
<tr>
<td>Pre-hospitalization mean/percentile</td>
<td>6,684</td>
<td>9,128</td>
<td>21,745</td>
<td>30,633</td>
<td>54,001</td>
<td></td>
</tr>
</tbody>
</table>

Notes: This table presents results of unconditional quantile effect estimates for the main outcomes reported in Tables 3 and 4 for the non-elderly insured credit report sample (see Table 1, column 2). The estimation follows the two-step procedure described in Firpo et al. (2009). Standard errors are clustered by individual and reported in parentheses; associated p-values are in brackets. The maximum credit score occurs below the 99th percentile, so that there is no impact on the 99th percentile.