Online Appendix to
"The Economic Consequences of Hospital Admissions"

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A Proofs of Propositions, Derivations of Formulas, and Model Extensions

In the first section of this appendix, we derive the results used in the main text. Specifically, we derive Proposition 1 from Section 1, the analytic expressions for Δb and $sign(\Delta y)$ from Section 1, and an approximation for the money-metric utility cost of a health shock, which we discussed in Section 5.2. In the second section of this appendix, we show that both Proposition 1 and the money-metric formula apply under an alternative model in which the health shock affects the disutility of hours of work rather than wages, as in our baseline model.

Throughout, we assume that the marginal cost of additional borrowing is increasing in the total amount to be repaid, which we define as $k(u, b) = (1 + r(u, b))b$; i.e., $k(u', b') > k(u, b)$ implies $\frac{\partial}{\partial b}k(u', b') > \frac{\partial}{\partial b}k(u, b)$. 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.

Additionally, as noted in the main text, we impose some additional technical conditions to make sure we get an interior solution. Specifically, we assume that the interest rate is strictly increasing and convex in b and u and that the cross-partial $\frac{\partial^2 r}{\partial u \partial b}$ is positive. We also assume that $\frac{\partial r}{\partial u}$ is strictly between 0 and 1 at $u = 0$, is strictly increasing in u for $u > 0$, and that the limit of $\frac{\partial r}{\partial u}$ as u approaches $(1 - \lambda_m)m$ is infinity. We similarly assume that the limit of $\frac{\partial r}{\partial b}$ as b approaches L is infinity. These assumptions ensure that the borrowing limit is never strictly binding, which allows us to avoid analyzing corner solutions. We also assume that the individual does not take into account the effects of her choices on L; in principle, her choices regarding u and b can affect L via their impact on Y via their impact on r. This simplifies the first-order conditions in the individual's optimization problem.

A.1 Results from economic framework in Section 1

A.1.1 Proof of Proposition 1

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

Proof.

The optimization problem in the sick state is the following:

$$\max_{b^S,u,h_1^S,h_2^S} U^S \equiv g(c_1^S) - f(h_1^S) + \frac{1}{1+\delta}(g(c_2^S) - f(h_2^S))$$

where $c_1^S = (1-(1-\lambda_\alpha)\,\alpha_1)\,w_1 h_1^S - (1-\lambda_m)\,m - \pi + u + b^S$ and $c_2^S = (1-(1-\lambda_\alpha)\,\alpha_2)\,w_2 h_2^S - \pi - \left(1 + r(u, b^S)\right) b^S$. The optimal choices of u and b^S are given by the following two first-order conditions:

$$\begin{aligned}
(u)\;\; g'(c_1^S) &= \frac{1}{1+\delta} g'(c_2^S) \frac{\partial r(u,b^S)}{\partial u} \\
(b^S)\;\; g'(c_1^S) &= \frac{1}{1+\delta} g'(c_2^S)(1 + r(u,b^S) + b^S \frac{\partial r(u,b^S)}{\partial b})
\end{aligned}$$

Combining these conditions gives the following indifference condition which equates the marginal cost of additional unpaid bills with the marginal cost of additional borrowing:

$$\frac{\partial r(u,b^S)}{\partial u} = 1 + r(u,b^S) + b^S \frac{\partial r(u,b^S)}{\partial b}$$

Given the assumptions needed for an interior solution (so that the equation above is a necessary condition for the optimal choice of u and b), we can immediately determine that $\Delta u > 0$. While $\Delta u > 0$, the ambiguous sign of Δb passes through $r = r(u, b)$ so the sign of Δr is ambiguous as well. Lastly, the sign of ΔL is ambiguous because the borrowing limit depends on r:

$$\begin{aligned}
L^H = \gamma Y^H &= \gamma(w_1 H + \frac{w_2 H}{1 + r(0, b^H)}) \\
L^S = \gamma Y^S &= \gamma((1-(1-\lambda_\alpha)\alpha_1) w_1 H + \frac{(1-(1-\lambda_\alpha)\alpha_2) w_2 H}{1 + r(u, b^S)})
\end{aligned}$$

To show that the sign of Δb is ambiguous, consider the specific version of the model where $\pi = 0$ and $\gamma = 1$ and $r(0,0) = \delta$ and $w_1 = w_2$. In this case, these parameters imply that $b^H = 0$, and so the sign of Δb is ambiguous if b^S is ambiguous. Next, take λ_m arbitrarily close to 1 so that u is close to 0, and take $\lambda_\alpha = 0$. The first-order condition for borrowing is given by the following:

$$(b^S) \;\; g'((1-\alpha_1)w_1h_1^S + b^S) = \frac{1}{1+\delta}g'((1-\alpha_2)w_2h_2^S - (1+r(u,b^S))b^S)(1+r(u,b^S) \\ + b^S\frac{\partial r(u,b^S)}{\partial b})$$

This first-order condition can yield $b^S > 0$ if α_1 is close to 1 and α_2 is close to 0. Similarly, this first-order condition will yield $b^S < 0$ if α_1 is close to 0 and α_2 is close to 1.

To show that the sign of Δy_1 and Δy_2 are both ambiguous, consider the specific version of model where $\pi = 0$ and the borrowing costs increase very sharply with both u and b so that the optimal choice of b is approximately 0 in both the sick state and the healthy state. Then, with no intertemporal consumption smoothing, the optimal choice of hours in each period and health state is given by the following:

$$\begin{aligned} (h_t^S) \;\; g'((1-(1-\lambda_\alpha)\alpha_t)w_t - (1-\lambda_m)m) &= \frac{1}{(1-(1-\lambda_\alpha)\alpha_t)w_t}f'(h_t^S) \\ (h_t^H) \;\; g'(w_th_t^H) &= \frac{1}{w_t}f'(h_t^H) \end{aligned}$$

In the above scenario, one can take λ_m close to 1 and then use fact that $\Delta c_t < 0$ to conclude that $\Delta y_t < 0$. Intuitively, with only an uninsured wage shock, earnings must fall otherwise consumption will not fall. Alternatively, one can take λ_α close to 1 and then one can use the fact that $\Delta c_t < 0$ to conclude that $\Delta y_t > 0$. Intuitively, with only a medical expenses shock, earnings (and hours) must increase to satisfy the first order condition (since wages do not change, the uninsured medical expenses raise hours and earnings due to the negative wealth effect).

The result $\Delta U < 0$ comes from simple revealed preference arguments and proof by contradiction. Since a health shock that is not "fully covered" reduces wages and imposes uninsured medical expenses, this mechanically reduces utility (relative to healthy state) if the individual chooses the same hours and borrowing as before. Now, assume that utility increases when sick; this must result from consumer making different choices when sick as compared to healthy, since otherwise we know utility decreases mechanically. However, if these alternative choices increased utility, then that would imply that the consumer could have made those same choices when healthy and could have had higher utility mechanically. This contradicts the assumption that hours and borrowing were chosen optimally when healthy.

This leaves proving $\Delta c_1 < 0$ and $\Delta c_2 < 0$. While these comparative statics are intuitive, they are not straightforward to prove because the sign of Δb is ambiguous (as described above) and the signs of Δy_1 and Δy_2 are ambiguous (also described above). To prove these propositions, we first define the first-order conditions for optimal hours choices in each period and in each state:

$$\begin{aligned}
(h_1^S) \;\; g'(c_1^S) &= \frac{1}{(1-(1-\lambda_\alpha)\alpha_1)w_1} f'(h_1^S) \\
(h_2^S) \;\; g'(c_2^S) &= \frac{1}{(1-(1-\lambda_\alpha)\alpha_2)w_2} f'(h_2^S) \\
(h_1^H) \;\; g'(c_1^H) &= \frac{1}{w_1} f'(h_1^H) \\
(h_2^H) \;\; g'(c_2^H) &= \frac{1}{w_2} f'(h_2^H)
\end{aligned}$$

To prove these two inequalities for consumption, we consider four cases, based on whether or not Δh_t is positive or negative.

Case 1: $\Delta h_1 > 0$ and $\Delta h_2 > 0$

In this case, we can use the first order conditions above for hours choices to immediately conclude that $\Delta c_1 < 0$ and $\Delta c_2 < 0$. This is because the function $f(h)$ is convex, and wages weakly decline in sick state in each period relative to healthy state. Thus, the RHS on each period's first-order condition goes up in sick state relative to healthy state. Thus, the marginal utility of consumption in each period much go up in sick state relative to healthy state, so consumption must fall.

Case 2: $\Delta h_1 < 0$ and $\Delta h_2 < 0$

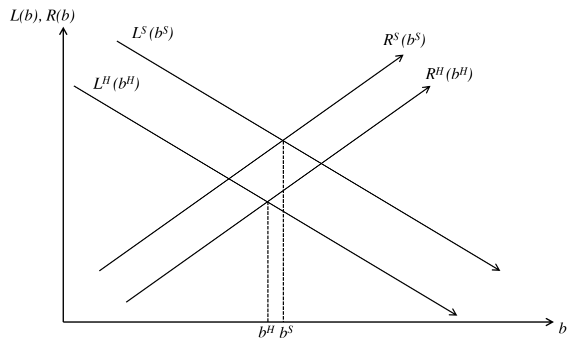
In this case, we have income declining in both periods ($\Delta y_1 < 0$ and $\Delta y_2 < 0$). In this case, we begin with the first-order conditions for borrowing in the healthy and sick states:

$$\begin{aligned}
g'(y_1 - \pi + b^H) &= \frac{1}{1+\delta} g'(y_2 - \pi - \left(1 + r(0, b^H)\right) b^H) * \\
&\quad \left(1 + r(0, b^H) + b^H \frac{\partial r(0, b^H)}{\partial b^H}\right) \\
g'(y_1 - (1-\lambda_\alpha)\alpha_1 y_1 - (1-\lambda_m)m - \pi + u + b^S) &= \frac{1}{1+\delta} g'(y_2 - (1-\lambda_\alpha)\alpha_2 y_2 \\
&\quad -\pi - \left(1 + r(u, b^S)\right) b^S) * \\
&\quad \left(1 + r(u, b^S) + b^S \frac{\partial r(u, b^S)}{\partial b^S}\right)
\end{aligned}$$

We re-define the two expressions above as follows:

$$\begin{aligned} L^H(b^H) &= R^H(b^H) \\ L^S(b^S) &= R^S(b^S) \end{aligned}$$

With these re-defined functions, we note that $L^H(b)$ and $L^S(b)$ are decreasing in b, while $R^H(b)$ and $R^S(b)$ are increasing in b. Since $u > 0$ in the sick state at the optimum, we have $L^H(b) < L^S(b)$ and $R^H(b) < R^S(b)$ for all values of b that yield positive consumption in both periods. These results imply that at the optimal choices of $b^H$and b^S, we have that $R^S(b^S) > R^H(b^H)$. This is easiest to see graphically in the following figure:



Since $R^S(b^S) > R^H(b^H)$ at the optimal choices of b^S and b^H, this implies that $L^S(b^S) > L^H(b^H)$, which in turn implies that $\Delta c_1 < 0$.

Next, to prove that $\Delta c_2 < 0$, we consider two cases. The first case is where $(1 + r(0, b^H))b^H \leq (1 + r(u, b^S))b^S$. In this case, it follows immediately that $\Delta c_2 < 0$. The second case is where $(1 + r(0, b^H))b^H > (1 + r(u, b^S))b^S$. In this case, by the hypothesis regarding the $k(u, b)$ function in the proposition, we have the following inequality:

$$1 + r(u, b^S) + b^S \frac{\partial r(u, b^S)}{\partial b^S} < 1 + r(0, b^H) + b^H \frac{\partial r(0, b^H)}{\partial b^H}$$

With the inequality above, and the fact that $R^S(b^S) > R^H(b^H)$, we have that $g'(c_2^H) < g'(c_2^S)$, which implies that $c_2^S < c_2^H$ and thus $\Delta c_2 < 0$. This completes the proof, which allows for any value of Δb.

Case 3: $\Delta h_1 > 0$ and $\Delta h_2 < 0$

In this case, we have $\Delta c_1 < 0$ following the same logic as in Case 1. We then have $\Delta c_2 < 0$ following the same logic as Case 2 (using the same assumption on the marginal cost of additional borrowing).

Case 4: $\Delta h_1 < 0$ and $\Delta h_2 > 0$

In this case, we have Δc_2 following the same logic as in Case 1. To prove $\Delta c_1 < 0$, we can follow Case 2, but we substitute marginal utility of consumption in period 2 in each state with the marginal disutility of hours worked. Then, since we have $\Delta h_2 > 0$, we know that the augment $R^H(b)$ and $R^S(b)$ functions behave the same way as in Case 2.

A.1.2 Formulas for Δb and $sign(\Delta y)$

We derive the analytic formula for Δb and $sign(\Delta y)$ discussed in Section 1. To do so, we analyze a simplified model in which individuals have no option to forgo paying some of their medical bills ($u^S = 0$), insurance premiums are zero ($\pi = 0$), the interest rate r is fixed exogenously at $r = \delta$, and borrowing constraints are determined by the present discounted value of maximum available resources (i.e., $\gamma = 1$ so that $L^J = Y^J$).[36] In the healthy state, the consumer's optimization problem is:

$$\max_{h_1^H, h_2^H, b^H} U^H \equiv U(w_1 h_1^H - \pi + b^H, h_1^H) + \frac{1}{1+r} U(w_2 h_2^H - \pi - (1+r)\, b^H, h_2^H)$$

which yields the condition for b^H:

$$b^H = \frac{y_2^H - y_1^H}{1 + (1+r)}.$$

Likewise, for the sick state, the consumer's optimization problem is:

$$\begin{aligned} \max_{b^S} U^S \equiv\ & U((1 - (1-\lambda_\alpha)\alpha_1) w_1 h_1^S) - (1-\lambda_m)m - \pi + b^S, h_1^S) + \\ & \frac{1}{1+r} u((1 - (1-\lambda_\alpha)\alpha_2) w_2 h_2^S) - \pi - (1+r)\, b^S, h_2^S) \end{aligned}$$

which yields the condition:

$$b^S = \frac{y_2^S - y_1^S + (1-\lambda_m)m}{1 + (1+r)}.$$

Combining the above expressions for b^H and b^S yields the change in borrowing following a health shock (Δb):

$$\Delta b = \frac{(\Delta y_2 - \Delta y_1) + (1-\lambda_m)m}{1 + (1+r)}. \tag{6}$$

[36] The last assumption on the borrowing limit ensures that it is never binding, which replaces the alternative technical assumptions that we assume in the general model with endogenous interest rates.

where Δy_t is change in income in each period. This equation indicates that with full coverage (i.e., $\lambda_\alpha = \lambda_m = 1$), the agent will choose $\Delta b = 0$. This implies that $\Delta b \neq 0$ rejects full coverage. It is also clear that sign of Δb is ambiguous, and depends on the relative importance of uninsured medical expenses and uninsured changes in labor income in each period.

To derive the expression for $sign(\Delta y_1)$, we make the additional assumptions that wages are same in both periods and decline by same amount in both periods following health shock ($w_1 = w_2$; $\alpha_1 = \alpha_2$).

In the healthy state, the consumer's optimization problem is:

$$\max_{b^H, h_1^H, h_2^H} \quad g(wh_1^H + b^H) - f(h_1^H) + \frac{1}{1+r}(g(wh_2^H - (1+r)b^H) - f(h_2^H))$$

This leads to first-order conditions:

$$\begin{aligned} g'(c_1^H) &= \frac{1}{w} f'(h_1^H) \\ g'(c_2^H) &= \frac{1}{w} f'(h_2^H) \\ g'(wh_1^H + b^H) &= g'(wh_2^H - (1+r)b^H) \end{aligned}$$

From these first-order conditions, we have $h_1^H = h_2^H$ and $b^H = 0$. Now, turning to sick state; the maximization problem is the following:

$$\begin{aligned} \max_{b^S, h_1^S, h_2^S} \quad & g((1-(1-\lambda_\alpha)\alpha)wh_1^S - (1-\lambda_m)m + b^S) - f(h_1^S) + \\ & \frac{1}{1+r}(g((1-(1-\lambda_\alpha)\alpha)wh_2^S - (1+r)b^S) - f(h_2^S)) \end{aligned}$$

This leads to following first order conditions:

$$\begin{aligned} g'(c_1^S) &= \frac{1}{(1-(1-\lambda_\alpha)\alpha)w} f'(h_1^S) \\ g'(c_2^S) &= \frac{1}{(1-(1-\lambda_\alpha)\alpha)w} f'(h_2^S) \\ g'((1-(1-\lambda_\alpha)\alpha)wh_1^S - m + b^S) &= g'((1-(1-\lambda_\alpha)\alpha)wh_2^S - (1+r)b^S) \end{aligned}$$

These first-order conditions imply that borrowing will be chosen to smooth consumption, and hours worked will be same in both periods. This results in the following:

$$b^S = \frac{(1-\lambda_m)m}{1+(1+r)}$$

Starting from $m = 0$ and $\alpha = 0$ (which is the healthy state), we have the following expression for change in earnings in first period coming from health shock dS (which causes change in wages and change in borrowing due to medical expenses):

$$\frac{dy_1}{dS} = (1+\varepsilon_{h,w})w_1 h_1 \frac{dlog(w_1)}{dS} + \varepsilon^I \frac{db}{dS}$$

This can be re-written as follows:

$$\frac{dy_1}{dS} = -\varepsilon^I \frac{(1-\lambda_m)m}{(1+(1+r))} - \alpha(1-\lambda_\alpha)(1+\varepsilon_{h,w})y_1^H$$

This gives the expression for sign of change in income:

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

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

A.1.3 Money-Metric Change in Utility from a Health Shock

We derive the following approximation to the money-metric change in utility from a health shock:

$$(7) \qquad \frac{\Delta U}{g'(c_1^H)} \approx \frac{\Delta y_1 + \Delta y_2}{1 + \varepsilon_{h,w}} - ((1 - \lambda_m)m)$$

where $\varepsilon_{h,w}$ is the uncompensated (Marshallian) labor supply elasticity $(dlog(h)/dlog(w))$, which represents the change in hours to a permanent change in wage. The formula is a first order approximation, making use of the envelope theorem so that there are no first order effects on utility of any health-shock induced changes in borrowing or unpaid bills (or of their effects, in turn, on r or L). The derivation also assumes that the discounted marginal utility of consumption is approximately equal across the two periods in both the healthy and sick states (i.e., $g'(c_1^J) \approx \frac{1}{1+\delta} g'(c_2^J)$); this will not hold exactly in the general model where we do not impose $r = \delta$.

Proof.

Recall utility in health state J is given by

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

where $U(c_t^J)$ is the per-period utility function defined as

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

Consider a "small" health shock dS, which perturbs individual from healthy state to sick state with "small" change in out-of-pocket costs and "small" change in wages. Then, using the expressions derived in equation (1) for consumption in each health state and time period for an individual optimally choosing labor supply and borrowing subject to her budget constraint, we can calculate the change in utility as follows:

$$\frac{dU}{dS} = \frac{\partial U}{\partial w_1}\frac{dw_1}{dS} + \frac{\partial U}{\partial w_2}\frac{dw_2}{dS} + \frac{\partial U}{\partial h_1}\frac{dh_1}{dS} + \frac{\partial U}{\partial h_2}\frac{dh_2}{dS} + \frac{\partial U}{\partial m}\frac{dm}{dS} + \frac{\partial U}{\partial u}\frac{du}{dS} + \frac{\partial U}{\partial b}\frac{db}{dS}$$

Since the health shock is "small", we can use envelope theorem to approximate the formula above:

$$\frac{dU}{dS} \approx \frac{\partial U}{\partial w_1}\frac{dw_1}{dS} + \frac{\partial U}{\partial w_2}\frac{dw_2}{dS} + \frac{\partial U}{\partial m}\frac{dm}{dS}$$

The approximation above assumes that there is no first-order effect on utility of changes in hours worked (in either period), changes in borrowing, or changes in unpaid bills by the standard envelope theorem argument that those were chosen optimally. Note that when healthy the unpaid bills is $u^H = 0$ by assumption, while when sick the unpaid bills are $u^S > 0$ due to first-order condition that sets optimal choice of u^S. However, the term du/dS is still going to have no first-order effect on utility since optimal choice of u^S approaches 0 as m approaches 0. Substituting for partial derivatives yields following:

$$\frac{dU}{dS} \approx g'(c_1)h_1\frac{dw_1}{dS} + \frac{1}{1+\delta}g'(c_2)h_2\frac{dw_2}{dS} - g'(c_1)\frac{dm}{dS}$$

Next, assuming that $g'(c_1) \approx \frac{1}{1+\delta}g'(c_2)$, then the money-metric utility change can be written as follows:

$$\frac{dU/dS}{g'(c_1)} \approx y_1\frac{dlog(w_1)}{dS} + y_2\frac{dlog(w_2)}{dS} - \frac{dm}{dS}$$

Lastly, we can use labor supply theory to relate change in earnings to change in wages in terms of uncompensated labor supply elasticity. We do this because, as is standard in the literature, we estimate changes in earnings rather than changes in hours (Keane 2011). We assume that the earnings changes (in response to health shock) are well approximated by the following relationship:

$$\frac{dlog(y_t)}{dS} \approx (1+\varepsilon_{h,w})\frac{dlog(w_t)}{dS}$$

where $\varepsilon_{h,w}$ is the uncompensated (Marshallian) labor supply elasticity - i.e. the change in hours - in response to a permanent change in wage.

Note that the approximation formula above assumes that earnings changes are due to changes in wages induced by health shock and not the unearned income shock coming from out-of-pocket medical expenses. This is a reasonable approximation if the uninsured medical expenses are small relative to wage changes and income effects in labor supply are not large. If that was non-negligible, then there would be an additional wealth effect adjustment needed to infer appropriate change in wages. Given above approximation, we can then substitute into above formula, which yields the following:

$$\frac{dU/dS}{g'(c_1)} \approx \frac{1}{1+\varepsilon_{h,w}}\left(\frac{dy_1}{dS} + \frac{dy_2}{dS}\right) - \frac{dm}{dS}$$

Lastly, we can replace derivatives with changes in response to health shock (i.e., replace $\frac{dy_1}{dS}$ with Δy_1), which gives formula in main text:

$$\frac{\Delta U}{g'(c_1)} \approx \frac{\Delta y_1 + \Delta y_2}{1+\varepsilon_{h,w}} - (1-\lambda_m)m$$

The adjustment for income effects would be the following:

$$\frac{dy_t}{dS} \approx (1+\varepsilon_{h,w})\frac{dlog(w_t)}{dS}y_t + \varepsilon^I \frac{dm}{dS}$$

This adjustment shows that the implied change in wages is more negative when taking into account wealth effect, because medical expenses reduce wealth which raises labor earnings. This results in following adjusted formula:

$$\frac{\Delta U}{g'(c_1)} \approx \frac{\Delta y_1 + \Delta y_2}{1+\varepsilon_{h,w}} - (1-\lambda_m)m + \frac{\varepsilon^I}{1+\varepsilon_{h,w}}(1-\lambda_m)m$$

Since the income effect is negative, this formula is slightly more negative, though since the wealth effect is small in magnitude ($\varepsilon^I \approx -0.1$ according to Imbens, Rubin, and Sacerdote (2001) and Cesarini et al. (forthcoming)), this term is ignored in the main formula.

A.2 Alternative Model: Health Shock Increases Disutility of Work

We show that both Proposition 1 and the money metric formula apply under an alternative model in which the health shock affects the disutility of hours of work rather than wages. To do this, we first modify notation so that utility is now defined as $g(c) - \beta f(h)$, and we consider changes in β from health shock. We define $\beta^S > \beta^H$ as disutility in each health state. In this setting, there is no effect of health shock on wages, only on disutility of work and uninsured medical expenses.

A.2.1 Proof of Proposition 1 in Alternative Model

Proof.

The optimization problem in the sick state is the following:

$$\max_{b^S,u,h_1^S,h_2^S} U^S \equiv g(c_1^S) - \beta^S f(h_1^S) + \frac{1}{1+\delta}(g(c_2^S) - \beta^S f(h_2^S))$$

where $c_1^S = w_1 h_1^S - (1-\lambda_m)\,m - \pi + u + b^S$ and $c_2^S = w_2 h_2^S - \pi - \left(1 + r(u,b^S)\right) b^S$. The optimal choices of u and b^S are given by the following two first-order conditions:

$$\begin{aligned}
(u)\;\; g'(c_1^S) &= \frac{1}{1+\delta} g(c_2^S)\frac{\partial r(u,b^S)}{\partial u} \\
(b^S)\;\; g'(c_1^S) &= \frac{1}{1+\delta} g(c_2^S)(1 + r(u,b^S) + b^S\frac{\partial r(u,b^S)}{\partial b})
\end{aligned}$$

Combining these conditions gives the following indifference condition which equates the marginal cost of additional unpaid bills with the marginal cost of additional borrowing:

$$\frac{\partial r(u,b^S)}{\partial u} = 1 + r(u,b^S) + b^S \frac{\partial r(u,b^S)}{\partial b}$$

Given the assumptions needed for an interior solution (so that the equation above is a necessary condition for the optimal choice of u and b), we have immediately that $\Delta u > 0$. Although $\Delta u > 0$, the sign ofΔb is ambiguous as described above. As a result, the sign of Δr is ambiguous, since $r = r(u,b)$. Lastly, the sign of ΔL is ambiguous since borrowing limit depends on r (as above).

The result $\Delta U < 0$ comes from the same arguments as in other model. This leaves proving $\Delta c_1 < 0$ and $\Delta c_2 < 0$. While these comparative statics are intuitive, they are not straightforward to prove because the sign of Δb is ambiguous (as described above) and the signs of Δy_1 and Δy_2 are ambiguous. To prove these propositions, we first define the first-order conditions for optimal hours choices in each period and in each state:

$$\begin{aligned}
(h_1^S) \;\; g'(c_1^S) &= \frac{1}{w_1}\beta^S f'(h_1^S) \\
(h_2^S) \;\; g'(c_2^S) &= \frac{1}{w_2}\beta^S f'(h_2^S) \\
(h_1^H) \;\; g'(c_1^H) &= \frac{1}{w_1}\beta^H f'(h_1^H) \\
(h_2^H) \;\; g'(c_2^H) &= \frac{1}{w_2}\beta^H f'(h_2^H)
\end{aligned}$$

To prove these two inequalities for consumption, we consider four cases, based on whether or not Δh_t is positive or negative.

Case 1: $\Delta h_1 > 0$ and $\Delta h_2 > 0$

In this case, we can use first order conditions above for hours choices to immediately conclude that $\Delta c_1 < 0$ and $\Delta c_2 < 0$. This is because the function $f(h)$ is convex, and β is higher in sick state in each period relative to the healthy state. Thus, the RHS on each period's first-order condition goes up in the sick state relative to the healthy state. and the marginal utility of consumption in each period much go up in the sick state relative to healthy state, so consumption must fall.

Case 2: $\Delta h_1 < 0$ and $\Delta h_2 < 0$

In this case, we have income declining in both periods ($\Delta y_1 < 0$ and $\Delta y_2 < 0$) since wages are the same. This is the same as Case 2 in the model above where a health shock reduces wages.

Case 3: $\Delta h_1 > 0$ and $\Delta h_2 < 0$

In this case, we have $\Delta c_1 < 0$ following the same logic as in Case 1. We then have $\Delta c_2 < 0$ following the same logic as Case 2 (using same assumption on marginal cost of additional borrowing).

Case 4: $\Delta h_1 < 0$ and $\Delta h_2 > 0$

This is same as Case 4 in model above where health shock reduces wages.

A.2.2 Approximation to money-metric change in utility from health shock in alternative model

To derive the approximation formula for money-metric change in utility from health shock, we again consider a "small" health shock dS, which perturbs individual from healthy state to sick state with "small" change in out-of-pocket costs and "small" change in disutility of work. In this case, we can calculate the change in utility as follows:

$$\begin{aligned}\frac{dU}{dS} &= \frac{\partial U}{\partial log(\beta_1)}\frac{dlog(\beta_1)}{dS} + \frac{\partial U}{\partial log(\beta_2)}\frac{dlog(\beta_2)}{dS} + \frac{\partial U}{\partial w_1}\frac{dw_1}{dS} + \frac{\partial U}{\partial w_2}\frac{dw_2}{dS} + \frac{\partial U}{\partial h_1}\frac{dh_1}{dS} + \frac{\partial U}{\partial h_1}\frac{dh_1}{dS} + \\ &\quad \frac{\partial U}{\partial m}\frac{dm}{dS} + \frac{\partial U}{\partial u}\frac{du}{dS} + \frac{\partial U}{\partial b}\frac{db}{dS}\end{aligned}$$

Since the health shock is "small", we can use the envelope theorem to approximate the formula above:

$$\frac{dU}{dS} \approx \frac{\partial U}{\partial \beta_1}\frac{d\beta_1}{dS} + \frac{\partial U}{\partial \beta_2}\frac{d\beta_2}{dS} + \frac{\partial U}{\partial m}\frac{dm}{dS} + \frac{\partial U}{\partial u}\frac{du}{dS}$$

The approximation above assumes that there is no first-order effect of changes in hours worked (in either period) or changes in borrowing on utility. We cannot do the same for unpaid bills because there is no first-order condition for unpaid bills in the healthy state (they are set to 0 by assumption). Substituting for partial derivatives yields the following:

$$\frac{dU}{dS} \approx -f(h_1)\frac{d\beta_1}{dS} - \frac{1}{1+\delta}f(h_2)\frac{d\beta_2}{dS} - g'(c_1)\frac{dm}{dS} + g'(c_1)\frac{du}{dS}$$

Next, assuming that $g'(c_1) \approx \frac{1}{1+\delta}g'(c_2)$ and using the first-order condition for hours worked in each period, the money-metric utility change can be written as follows:

$$\begin{aligned}\frac{dU/dS}{g'(c_1)} &\approx -\frac{w_1 f(h_1)}{\beta_1 f'(h_1)}\frac{d\beta_1}{dS} - \frac{w_2 f(h_2)}{\beta_2 f'(h_2)}\frac{d\beta_2}{dS} - \frac{dm}{dS} + \frac{du}{dS} \\ \frac{dU/dS}{g'(c_1)} &\approx -w_1 h_1 \frac{f(h_1)}{h_1 f'(h_1)}\frac{dlog(\beta_1)}{dS} - w_2 h_2 \frac{f(h_2)}{h_2 f'(h_2)}\frac{dlog(\beta_2)}{dS} - \frac{dm}{dS} + \frac{du}{dS}\end{aligned}$$

Lastly, we can use labor supply theory to relate change in earnings to change in disutility of work. Start from the first-order condition in each period:

$$g'(c_t) = \frac{1}{w_t}\beta_t f'(h_t)$$

Differentiating above with respect to β yields the following expression (note that this ignores unearned income, medical expenses, and the effect of change in β on borrowing levels or borrowing costs, so this will be an approximation):

$$\begin{aligned}
(w_t)^2 g''(w_t h_t)\frac{dh_t}{d\beta_t} &= f'(h_t) + \beta_t f''(h_t)\frac{dh_t}{d\beta_t} \\
w_t \frac{w_t g''(w_t h_t)}{g'(w_t h_t)}\frac{dh_t}{d\beta_t} &= \frac{w_t}{\beta} + w_t \frac{f''(h_t)}{f'(h_t)}\frac{dh_t}{d\beta_t} \\
\frac{dlog(h_t)}{dlog(\beta_t)} &= \frac{1}{c_t g''/g' - h_t f''/f'}
\end{aligned}$$

We can follow the same steps to get an expression for the uncompensated labor supply elasticity:

$$1 + \frac{dlog(h_t)}{dlog(w_t)} = 1 + \varepsilon_{h,w} = \frac{1 + h_t f''/f'}{-c_t u''/u' + h_t f''/f'}$$

Combining the two above expressions gives the following:

$$\frac{dlog(h_t)}{dlog(\beta_t)} = -\frac{1 + \varepsilon_{h,w}}{1 + h_t f''/f'}$$

Therefore, a change in earnings coming from change in β is given by:

$$\frac{d(w_t h_t)}{dlog(\beta_t)} = -\frac{1 + \varepsilon_{h,w}}{1 + h_t f''/f'} w_t h_t$$

Now, assuming that $1 + hf''/f' \approx hf'/f$, we can simplify approximation formula as follows:

$$\begin{aligned}
\frac{dU/dS}{u'(c_1)} &\approx -w_1 h_1 \frac{1}{1 + h_1 f''/f'}\frac{dlog(\beta_1)}{dS} - w_2 h_2 \frac{1}{1 + h_2 f''/f'}\frac{dlog(\beta_2)}{dS} - \frac{dm}{dS} + \frac{du}{dS} \\
\frac{dU/dS}{u'(c_1)} &\approx \frac{1}{1 + \varepsilon_{h,w}}\left(\frac{dy_1}{dS} + \frac{dy_2}{dS}\right) - \frac{dm}{dS} + \frac{du}{dS}
\end{aligned}$$

Lastly, we can replace derivatives with changes in response to health shock (e.g., replace $\frac{dy_1}{dS}$ with Δy_1), which yields the formula in main text:

$$\frac{\Delta U}{u'(c_1)} \approx \frac{\Delta y_1 + \Delta y_2}{1 + \varepsilon_{h,w}} - (1 - \lambda_m)m$$

The money-metric welfare change is the same whether the health shock reduces wages or increases disutility of work. As a result, if the health shock does both, the same formula will be obtained. Specifically, suppose a share σ of earnings change comes from wage shock and the remaining share $(1-\sigma)$ comes from increase in disutility of work, then the same formula will be obtained for any $0 \le \sigma \le 1$. Note that the assumed approximation $1 + hf''/f' \approx hf'/f$ will hold exactly in the standard model in Keane (2011) where utility is CRRA in consumption and the disutility of labor is isoelastic; i.e., $u(c,h) = c^{1+\eta}/(1+\eta) - \beta h^{1+\gamma}/(1+\gamma)$ implies that $1 + hf''/f' = hf'/f$.

B Data

B.1 Health and Retirement Survey

B.1.1 Data and sample definitions

The HRS is a nationally representative panel survey of the elderly and near-elderly in the United States. It began in 1992 with an initial HRS cohort of individuals in born between 1931 and 1941. Over subsequent survey waves they added additional birth cohorts including the AHEAD (born before 1924) in 1993, the Children of the Depression (born 1924-1930) and War Babies (born 1942-1947) in 1998, the Early Baby Boomers (born 1948-1953) in 2004, and the Mid Baby Boomers (born 1954-1959) in 2010. As a result, the cohort-composition of the sample changes substantially over survey waves (see Appendix Table 1).

We analyze all existing survey waves (1992 - 2012), with the exception of the 1993 and 1995 survey ways of the AHEAD cohort. As a result, the data are bi-annual and we have 11 waves from 1992 through 2012. All of our analyses use the HRS sample weights which are designed to be representative of the non-institutionalized population in this age group. Our analysis is primarily based on the RAND version of the Health and Retirement Survey (HRS) which can be downloaded here: http://hrsonline.isr.umich.edu.[37]

Appendix Table 2 gives definitions for key variables. We use the CPI to adjust all dollar amounts to 2005 levels (the mid-point of the credit report data). We censor all the continuous outcomes at the 99.95th percentile to purge the data of extreme outliers.

Our sample is limited to individuals who have an inpatient hospital admission. In each

[37] We also use the RAND HRS Income and Wealth Imputations supplemental data to facilitate some breakdowns within "household business and capital income" as described below.

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). We exclude a few individuals who have a zero survey weight in wave 0. Since the interviews are bi-annual, the index admission occurs, in expectation, one year prior to the wave 0 interview; we do not observe the date of admission. For purposes of our sample definition and analyses, we infer age at the index admission based on reported age at the wave 0 interview, minus 1 year.

We define an individual as "insured" if he has private insurance or Medicaid. Because insurance status may change after a health event (see e.g., Table 1), we define insurance status as of wave -1 (the interview prior to the index admission). This means that we must limit our sample to individuals whom we observe in at least 1 interview without reporting a hospital admission. This restriction is desirable not only for purposes of defining insurance coverage but also because it ensures that the index hospital admissions are all for individuals who have not had a prior hospital admission for the last 3 years in expectation (since they do not report a hospital admission in wave -1). For this reason, for our 65+ sample (where we do not need to define insurance) we likewise require that we observe them for at least 1 interview prior to the index admission without reporting an admission. Appendix Table 4 shows the impact of these sample restrictions.

Our main analysis sample is of insured, "non-elderly" individuals under 60 who have a hospital admission; we also analyze individuals 65 and over who have a hospital admission. The "non-elderly" insured are aged 50-59 at the time of the index admission; we therefore assume all admissions are non-pregnancy related. 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).

B.1.2 Key outcomes and their reference periods

In this section, we present a description of the HRS data structure and variable definitions, paying particular attention to the reference windows used in the questions on hospitalizations, income, and labor force status which are additionally presented in Panel B of Appendix Table 2. Variable reference periods will affect how we compute implied effects based on the parametric event study coefficients estimated in equation (4). The methods used to transform those coefficients will be described in detail in Appendix C. Appendix Table 5 presents summary statistics on the distribution of outcome variables in wave -1.

Out-of-pocket-medical expenses Respondent's out-of-pocket medical expenses are reported "since the last interview" which on average is two years. Of course, in principle the coefficient on wave 0 in equation (4) (i.e., μ_0) could reflect changes in out-of-pocket spending prior to the hospitalization, depending on the timing of the interview and hospitalization. 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 months prior to admission. Appendix C presents a full discussion of the way the coefficients from equation (4) translate to the reported effects at 12 months and the annual average effects over 48 months.

Out of pocket medical expenses are defined in the survey to include essentially every type of medical expense the individual might have to pay. This includes payments for: hospitals, physicians, nursing homes, dentists, outpatient surgery, prescription drugs, home health care, and special facility costs.

Income and earnings Earnings and income are reported for the calendar year prior to the interview.[38] We analyze household income and its components: respondent earnings, spousal earnings, household business and capital income, household social insurance payments, household pension and annuity income, and other household income. We discuss the construction of each component in turn.

We define earnings (either respondent earnings or spousal earnings) as the sum of labor market earnings and self-employment earnings. We also define a binary outcome "any earnings" which takes the value 1 if the respondent has positive earnings in the previous calendar year.

It is possible that our earnings measure misses some self-employment earnings. In the first two surveys (1992 and 1994), the HRS reports earnings as the sum of labor market earnings and self-employment earnings; we cannot separate them. In subsequent surveys, labor market earnings and self-employment earnings are reported separately; however in the RAND version of the HRS, self-employment earnings are reported in an aggregate called "household capital income" that also includes business income and unearned capital income. In order to separate the self-employment earnings component of "household capital income," we merged in the "RAND Income and Wealth Imputation File" to identify self-employment earnings in 1996 and subsequent waves and create a consistent measure of self-employment and labor market earnings over time. In robustness analysis, we analyze labor market earnings and self-employment earnings separately in surveys 1996 and later.

[38] An exception is pension and annuity income, which can either be reported for the previous calendar year or scaled up to to an annual amount based on a report for the prior month.

We additionally decompose earnings into wages and hours. Annual hours are defined as the usual number of hours worked per week (censored at 80) times the usual number of weeks worked per year at the respondent's main job. Our measure of annual hours includes those with zero annual hours. Wages can be reported at any frequency the respondent prefers (e.g. hourly, weekly, bi-weekly) and are normalized to an hourly wage. We analyze the logarithm of the hourly wage, conditional on positive hours worked.

Social insurance payments are the sum of Social Security Retirement Income, Supplemental Security Income, Social Security Disability Income, and Unemployment Insurance in the previous calendar year; in some analyses we analyze these components separately. We analyze social insurance payments for the household as well as the respondent. We also define a binary variable that is 1 if the respondent reports receiving any social insurance payments in the previous calendar year. Likewise, we analyze household pension and annuity income and respondent pension and annuity income. Finally, we analyze "other household income" (which includes alimony, lump sumps from insurance, pension, and inheritance, and other income).

We define "household business and capital income" as the RAND "household capital income" measure minus respondent and spousal self-employment earnings. In principle, this is supposed to measure business income and unearned capital income. In practice, the distinction between self-employment income and business income is not always clear, and it is possible that this measure includes some self-employment income.

Our baseline measure of "total household income" is the sum of respondent earnings, spousal earnings, household social insurance payments, and household pension and annuity income. This baseline measure excludes "household capital and business income" and "other household income" because these variables are extremely skewed, and may be measured with considerable error. Our estimates suggest large but extremely imprecise (standard error more than double the point estimate) potential impact of hospital admission on these outcomes. We therefore prefer to exclude them from our baseline "total household income" measure. In Appendix Table 9 however, we show that including these components of total income does not have a substantive effect on the analysis.

Employment and work limited by health The HRS records the contemporaneous (at the time of interview) labor force status of the respondent into the following mutually exclusive and exhaustive self-reported categories: working full-time, working part-time, unemployed, partly-retired, retired, disabled or not in the labor force. In addition to analyzing these categories individually, we also use the working part-time and working full-time variables to construct an indicator variable for the extensive margin "employment". The HRS also records the contemporaneous (at the time of interview) respondent answer to the question "Do you have any impairment or health problems that limits the kind or amount of paid work you can do?", which is analyzed alongside labor force status in Appendix Table 10.

B.2 Linked Hospital Discharge and Credit report data

B.2.1 Data and sample definitions

The credit reports come from TransUnion's Consumer Credit Database; TransUnion is a global risk information solutions company, and is one of the three primary credit reporting agencies. To maintain confidentiality, the TransUnion records were drawn using a list of Social Security Numbers (SSNs) of individuals with a hospital discharge embedded in a sample of randomly generated Social Security Numbers. The Social Security Numbers were then dropped from the file leaving a randomly generated unique identifier generated by a third party as a linking variable. The files were then securely transferred to the Sacramento office of California's Office of Statewide Health Planning and Development (OSHPD). At no point did the TransUnion staff have access to any medical data. The financial records were linked to information on all of the individual's California hospitalizations between 2000 and 2010 and to mortality data from California vital statistics through 2010 using the randomly generated unique identifier. At no point did the authors have access to any individual identifying information. For confidentiality reasons, the merging and all of our analysis was conducted on a non-networked computer at OSHPD's Sacramento office; prior to disclosing results outside of OSHPD, all generated output was reviewed on-site by OSHPD staff to confirm that privacy was protected.[39]

The hospital data are standard, discharge-level hospital discharge data made available to researchers by OSHPD. Similar discharge data – from California and from other states – have been used to study the impact of health insurance on hospital use (e.g., Doyle 2005; Card, Dobkin, and Maestas 2008, 2009; Anderson, Dobkin, and Gross 2012; Finkelstein et al. 2012), as well as to analyze the impact of other policies on hospital use (e.g., Dobkin and Nicosia 2009). The data include a unique individual identifier which we use to identify multiple hospital admissions by the same individual and to link the individual to external data on mortality and credit reports.

The hospital data include the exact dates (day, month, and year) of admission and discharge, the source of the admission (i.e., whether it was through the emergency department), detailed information on the patients' diagnoses, and basic demographics age, race, and sex). In addition, the data indicate the "expected source of payment" which is based

[39]Full details of the merging procedure are available from the authors upon request

on the patient's primary insurer. We use this to classify the insurance coverage for that admission as either Medicare, Medicaid, privately insured, uninsured ("self-pay"), or "other" (which includes various small programs such as indigent care, and workers' compensation). Throughout our analysis, we classify individuals' insurance status based on their primary insurer for the index admission.

We sample from the census of all non-pregnancy-related admissions for individuals aged 25 and over with a valid SSN during the five-year period between January 1, 2003 and December 31, 2007.[40] For cost reasons, and to ensure sufficient sample size for certain types of admissions, we over-sampled certain types of admissions. Specifically, we selected all admissions for the uninsured, all admissions through the emergency department (ED) for 60-70 year olds, a random sub sample of 20 percent of admissions through the ED from the remaining set of ED admissions (for those who are not uninsured or 60-70), and a random sub sample of 10 percent of admissions not through the ED. In all of the analyses, we weight each individual analyzed by the inverse of their probability of being sampled. All statistics reported in the paper use these weights.

We report results for three separate analysis samples. Our primary focus is on insured non-elderly individuals aged 25-64. We define the "insured" as those with Medicaid or private insurance. This excludes the self-pay (whom we analyze separately) and approximately 20 percent of hospitalizations with other forms of insurance (such as Medicare (the most common, accounting for about one-third of the hospitalizations with other forms of insurance), workers' compensation, the Veteran's Administration, and County Indigent Programs). Our two other analysis samples are uninsured ("self-pay") non-elderly individuals aged 25-64, and elderly individuals (ages 65 and older).

For each of these three analysis samples, we impose two (common) key sample restrictions.[41] Appendix Tables 13 and 14 shows the size of the drops and changes in sample composition at each step. We focus our discussion on the non-elderly adult insured sample (columns 1-3). Column 1 shows our sample of non-pregnancy related hospitalizations for insured individuals aged 25-64.

In column 2 we limit our analysis to individuals whom we can match to their credit reports. This reduces our sample of individuals by about 14 percent. We would lose individuals at this stage if they do not have a credit record, or if we are unable to match them on SSN (due either to an error in SSN in either record or a missing SSN in the credit report

[40]The restriction to non-pregnancy is based on diagnosis codes for admission.

[41]The sample we start with is restricted to hospitalizations that have an SSN, since this is required to match to the TransUnion data. From the public use files, we calculate that this excludes about 2 percent of hospitalizations from the sample.

data).[42] The Consumer Financial Protection Bureau (2015) estimated that about 11 percent of adults have no credit record; this estimate increases to 30 percent of adults in low-income neighborhoods. Consistent with this, we estimate a lower match rate for those on Medicaid (72 percent) than for the privately insured (89.5 percent).

Finally in order to focus our analysis on an "initial" health shock, in column 3 we further exclude from our index admissions any admissions that occur within three years of a prior admission (including a prior child birth admission, and even if that prior admission occurred prior to our 2003-2007 period); this excludes about 17 percent of individuals and about 50 percent of admissions.[43] From an economic perspective, the restriction is designed to focus on what is more likely the initial onset of adverse health, rather than capturing an individual midway through an ongoing series of health problems; in terms of our event study analyses, we expect this restriction to help minimize the extent of pre-trends in our outcomes.[44] Of course, many of the individuals in our baseline sample will go on to have subsequent hospitalizations and we consider these sequelae in interpreting our results.

Our baseline sample of 380,000 insured adults with a non-childbirth hospital admission is shown in column 2 of Table 1 (and Appendix Tables 13 and 14). Average age at index admission is 49; the sample is 45 percent male, 63 percent white non-Hispanic, and 18 percent Hispanic. 86 percent are privately insured at the time of the index admission; the rest 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. Mortality is 3.2 percent in the 12 months following the index admission, and 6.3 percent in 48 months. About one-fifth of the sample are re-admitted to the hospital within 12 months, and about 35 percent within 48 months.

Columns 6 and 9 present summary statistics for our baseline samples of 150,000 uninsured adults and 410,000 elderly individuals, respectively. Not surprisingly, there are some marked differences across the analysis samples (compare columns 3, 6 and 9). In particular, the share

[42]There is of course the possibility of a false positive as well with the matches. Indeed, we find that 10 to 20 percent of the matched individuals have a different birth date in the hospital discharge data and the credit report data, although our investigation suggests that this at least partially reflects data entry errors in birthdate in one of the files. In Appendix Table 22, we show that our main results are robust to limiting the sample to individuals where the birth-date is an exact match across the two data sources.

[43]In addition, we drop less than 1 percent of hospitalizations that represent a hospitalization for an individual that is subsequent to their first qualifying (index) hospitalization within the 2003 - 2007 window but also in the 2003-2007 window (so that each individual has exactly one hospital admission between 2003 and 2007).

[44]In the robustness analysis, we show that including individuals with prior hospitalizations in our analysis often exacerbates pre-trends in outcomes, but that it has little consequence for our implied effects of hospital admissions (which are all estimated relative to the pre-existing trend).

male, admitted through the emergency room, or admitted to public hospitals is substantially higher for the uninsured; length of stay, list charges and mortality are substantially higher for the elderly.

The consequences of our two key sample restrictions are broadly similar in all three analysis samples: they tend to increase the share white, reduce average length of stay, and reduce subsequent mortality. For the insured sample, they also increase the share with private insurance. The restrictions are a bit more binding for the uninsured and elderly than for the non-elderly insured, primarily reflecting a 5 to 10 percentage point lower match rate to credit report data, which is in the direction we would expect.

B.2.2 Key outcomes and their definitions

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.[45] Once again, we censor all the continuous outcomes at the 99.95th percentile to purge the data of extreme outliers. Credit scores are already censored at 990.

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, Skiba, and Tobacman 2015). 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.[46]

Avery, Calem, and Canner (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 15 reports pre-hospitalization summary statistics for our three analysis samples.

We focus our discussion of pre-hospitalization summary statistics on those for insured adults. Not surprisingly, prior to hospitalization, the uninsured look worse off on all measures

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

[46] 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 2015; Finkelstein et al. 2012) and other settings (Melzer 2011).

than the insured; the elderly tend to appear slightly better off than insured adults.[47]

Unpaid bills (u) 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 \$1,200 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,

[47] In addition, to place our analysis samples in context relative to the general population in their age group, Appendix Table 16 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 16 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.

about 1.2 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 expect to observe many 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.

Borrowing 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 \$38,000, 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.

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, Jenkins, and Levin 2013a; Agarwal et al. 2015a; Han, Keys, and Li 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 everyone in our data has a credit score. Approximately 5 percent of the insured sample does not have a credit score in the year prior to hospitalization; the number is similar for the elderly sample.[48] 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 in Appendix Table 34 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 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.

B.3 Medical Expenditure Panel Survey (MEPS)

Data from the Medical Expenditure Panel Survey (MEPS) allow us to gauge the total and out of pocket payments associated with the index hospital admission and its sequelae, as well as to estimate consumer cost-sharing.

The MEPS is a nationally representative survey of households with an overlapping panel design. It includes approximately 12,000 families (32,500 individuals) each year, and follows them for five interviews over the course of 2 calendar years. The data include information on

[48] This 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).

all sources of health care expenditures, including inpatient, outpatient, emergency room, and prescription drug. Data are collected from reports by individuals, their medical providers, and their employers. Self-reports from households provide the initial data which is supplemented and/or replaced when the survey follows up with the medical providers cited by the households. Data on insurance coverage for the household, including premiums, contributions, eligibility, and benefits, are provided by employer interviews. We use reported insurance coverage from the annual consolidated file. An individual is defined as insured if they were covered by either Medicaid or private insurance at any point throughout the calendar year, and defined as uninsured if they were not covered by any type of insurance at any point in the year (also including TRICARE, Medicare, and other types of public insurance). We drop individuals classified as uninsured who have positive payments from unlisted, "other private insurance" in their inpatient claims.

We use 11 panels of the MEPS, beginning each year from 1999-2010. We limit the sample to individuals who have at least one inpatient admission in the first year of the panel, whom we observe for the full two years, and who did not have a childbirth-related claim during their survey period. This sample approximates the California discharge data but - in at least two important ways - does not match it exactly. First, given the (two-year) nature of the MEPS, we are unable to restrict to individuals who have no prior hospitalizations in the last three years. Second, due to the absence of state identifiers and to boost sample size as with the HRS, our estimates are based on a national sample, rather than restricted to California.

We estimate an annual, non-childbirth hospital admission rate of 5.7 percent for insured adults age 25-64, 2.9 percent for uninsured adults aged 25-64, and 17.3 percent for adults aged 65+. Appendix Table 37 presents data on medical costs for this sample of individuals who have an "index hospital admission". Column 1 presents our approximation to our baseline sample: insured individuals aged 25-64. Columns 2 and 3 break down the baseline sample separately for those with private insurance and Medicaid. Column 4 shows results for the uninsured (ages 25-64), and column 5 shows results for the 65+.

Index event

The MEPS data suggest that the average total inpatient payments for the index hospital admission were about $11,000 for the insured sample, or about one-quarter of the $46,000 average list charge per hospital admission in the CA discharge data.[49] Payments for the index admission were similar for the elderly but somewhat lower ($8,500) for the uninsured,

[49]This is broadly consistent with our calculations - using data from the American Hospital Association - that, on average, across all admissions (and payers) the cost-to-charge ratio in California is about one-third, noticeably lower than the rest of the country.

even though total charges for the index event are quite similar in the MEPS for insured and uninsured individuals.

12-month total medical payments

We can estimate total medical payments (including inpatient, outpatient, emergency room, and prescription drug expenditures) over the 12 month period beginning with (and including) the inpatient stay directly in the MEPS. Accounting for subsequent hospitalizations, as well as expenditures on emergency room visits, outpatient visits and prescription drugs, we estimate that, for an insured individual, the index hospital admission is associated with about $18,500 in total medical payments in the first 12 months (including the index admission). About $11,000 represents payments for the index admission, about $3,200 represents non-inpatient medical spending (doctors, drugs and emergency rooms), and the remainder represents payments associated with subsequent admissions. Compared to total medical payments over the first 12 months of $18,500 for the non-elderly insured, we estimate total medical payments of about $11,000 for the uninsured and about $20,000 for the elderly.

To arrive at these estimates, we start with the observation that, as shown in Appendix Table 37, total medical payments over the 12 month period is $18,660 for insured adults. However, in the MEPS, we cannot restrict to individuals who have not had a hospital admission in the prior three years. As a result, the MEPS sample of insured adults includes, on average, 1.4 hospital discharges in the year following the index event (including that event) which is virtually identical to the 37 percent re-admission rate we see in the CA discharge data before restricting to individuals who have not had a prior hospital admission in the last 12 months (see Appendix Table 13 column 2), but double the 20 percent re-admission rate of our baseline sample, which restricts to individuals who have not had a hospital admission in the last 3 years (see Appendix Table 13, column 3). Therefore, for subsequent inpatient spending, we use the estimate from the CA discharge data of 0.32 additional hospital stays on average in the year following the index event (Appendix Table 14, column 3), multiplied by the average payments of $10,839 per hospital admission in the MEPS (Appendix Table 37) to estimate average total medical spending over the 12 month period beginning with the index hospital admission of $17,516 (i.e., $10,839 + 0.32*10,839 + $3,209 in non-inpatient spending) or about 160 percent of the index admission spending.

We can do a similar calculation for the uninsured and the elderly. For the uninsured, the relevant inputs are $6,938 in spending for the index event in the MEPS data (Appendix Table 37, column 4), 0.34 for hospital stays on average over the next 12 months in the CA discharge data (Appendix Table 14, column 6), and $1,548 in non-inpatient spending (emergency room, outpatient, and prescription drugs) in the MEPS over the next 12 months. This suggests total 12 month payments for the uninsured (including the index event), of \$10,845 (\$6,938 + 0.34*6,938 + 1,548). For the elderly, the relevant inputs are \$11,182 in spending for the index event in the MEPS data (Appendix Table 37, column 5), 0.54 for hospital stays over the next 12 months (Appendix Table 14, column 9), and \$3,194 in non-inpatient spending in the MEPS over the following 12 months. This yields 12 month payments for the elderly of \$20,414 (\$11,182 + 0.54*11,182 + 3,194).

Out-of-pocket medical expenses

Out-of-pocket costs for the index event are \$362 for the insured, \$1,363 for the uninsured, and \$212 for the elderly. We estimate total out of pocket costs for the 12 months following the index admission (inclusive of the index admission) of \$865 for the non-elderly insured, \$2,682 for the non-elderly uninsured, and \$1,001 for the elderly.

We calculate out of pocket costs over the 12 months following the index admission using the same logic as we do for total medical payments. Non-inpatient out of pocket costs (ER, OP, Rx) over the next 12 months are \$451 for the insured, \$901 for the uninsured, and \$735 for the elderly (Appendix Table 37, columns 1, 4, and 5). We use the number of subsequent inpatient admissions from Appendix Table 14 as above, and assuming that subsequent inpatient admissions incur the same out of pocket costs as the index admission.

Consumer cost-sharing

Appendix Table 37 also reports estimates from the MEPS on consumer cost-sharing. To calculate the share of medical expenses paid out of pocket (i.e., $1-\lambda_m^{insured}$), we sum total and out of pocket payments for an individual in the MEPS and take the ratio. The average share of out of pocket spending is the average across individual ratios. We estimate an average out of pocket share of 5.5 percent for the index hospital admission, and 8.4 percent for all medical spending in the 12-month period beginning with the index admission.

C Deriving implied effects in the HRS

The HRS data is collected in repeated waves that are on average two years apart. In each wave respondents are asked if they have had a hospitalization since the last interview. Recall from Section B.1.2 our basic parametric estimating equation (4):

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

where r measures the interview wave relative to the interview in which the hospital admission is reported, which we define as wave 0. Our focus here is how to translate the relative wave effects (μ_r') into estimates of the impact a given number of months after the hospital admission occurs. We define the treatment effect at month e relative to hospitalization as β_e where e denotes the number of months between the hospital admission and the interview wave and β_e therefore denotes the effect of the hospital admission in the effect on the outcome in e^{th} month following the admission. Specifically, we derive the implied effects we report in Table 2: impacts 12 months post admission (β_{12}), 36 months post admission (β_{36}), and the average annual effect three years post admission $\beta_{3yr\ avg}$.

Since, as discussed in Appendix B.1.2, our primary outcomes have different look-back period, the mapping from relative wave effects (μ_r') to effects in event time (β_e) will vary across outcomes. We therefore discuss separately the formula and derivation for the (β_e) for: earnings and other income (whose look back period is the prior calendar year); out-of-pocket medical expenses (whose looks back period is "since the last interview"); and labor market status (where interview asks about "current status").

C.1 Earnings and Income.

Respondents are asked in the interview to report their earnings and other income "in the last calendar year". Consider the interview in wave 0 (i.e., the interview in which the hospital admission is reported). The interview occurs in a (known) calendar month. The hospital admission occurred since the last interview (i.e., sometime in the previous 24 months). And earnings and income are reported for the last calendar year.

To assist in visualizing this, Appendix Figure 1 presents a table for the 24 months before the interview (when the hospitalization could have occurred) against the month in the year in which the interview occurred. Because the survey asks about earnings and other income in the prior calendar year, individuals interviewed in January will have a reference window for income that starts the month prior to the interview and extends back one year, while those interviewed in December report income from one year to two years prior to the interview. Each cell in the table presented in Appendix Figure 1 represents a given combination of hospitalization dates and interview months, and the value in the cell represents the month of hospitalization relative to month of the interview.

The (μ_r') coefficients from equation (4) will be the weighted sum of treatment effects for different durations. Consider the estimated effect of relative wave 0 (μ_0'). Depending on when the month of the interview and when the hospitalization occurred, all, some, or none of the reference window will capture time after the hospitalization. Individuals whose hospitalization occurred in the lower right triangle of the table in Appendix Figure 1 will not contribute to the estimate of the treatment effect post-admission since at interview wave 0 we only observe their earnings pre-hospitalization. Individuals whose time of interview and hospitalization puts them in the upper left triangle will have their entire year of reported income in the post period though at different lengths of time after the hospitalization. The remaining group whose hospitalization occurs in the gray parallelogram, will have only part of the period they are reporting income for post hospitalization. The estimated effect at relative wave 0 ($\hat{\mu_0}$) is therefore the weighted average of these different treatment effects. The same logic applies to estimated effects at other relative waves. Indeed we can derive:

$$
\begin{aligned}
\hat{\mu_0} &= \sum_{e=-11}^{23} \omega_e \beta_e = \sum_{e=0}^{23} \omega_e \beta_e \\
\hat{\mu_1} &= \sum_{e=13}^{47} \omega_e \beta_e \\
\hat{\mu_2} &= \sum_{e=37}^{71} \omega_e \beta_e
\end{aligned} \tag{8}
$$

where the second equality in the first line imposes identifying assumption that pre-period effects (β_e for $e < 0$) are 0 and ω_e are sample weights which indicate what share of the estimation sample has the indicated relative event time value. The sample weights ω_e can be calculated numerically using information on distribution of interview months and hospitalization times. We observe the empirical distribution of interview times in the HRS (they are roughly normally distributed, centered around July); we do not observe hospitalization dates in the HRS but assume that they are uniformly distributed over the prior 24 months; a uniform hospital admission rate by month is a very close approximation to reality based on the hospital admission information in our California discharge data. Appendix Table 3 shows these weights.

For earnings and income β_e reflects the impact of the hospital admission at the e^{th} month following the admission on earnings or income in the prior 12 months. Our baseline assumption is that β_e is a piecewise linear spline as a function of event time (e), with knot points at 0, 12 and 36 months. Under this assumption (and we show robustness to other assumptions below) we can derive the following expressions for the β_e as a function of objects we can estimate in the data:

$$
\begin{aligned}
\beta_{12} &= 2.227\hat{\mu_0} - 0.118\hat{\mu_1} + 0.0056\hat{\mu_2} \\
\beta_{36} &= -0.954\hat{\mu_0} + 1.550\hat{\mu_1} - 0.073\hat{\mu_2} \qquad (9) \\
\beta_{3yr\ avg} &= 0.636\hat{\mu_0} + 0.716\hat{\mu_1} - 0.034\hat{\mu_2}.
\end{aligned}
$$

The β_e derived in equation (9) are reported as our implied effects for income and earnings in our Tables (e.g. Table 2).

Derivation We discuss the derivation of equation (9). To derive expressions for β_e in terms of objects we can estimate in the data, we begin by assuming that the true evolution of the outcome follows a piecewise linear spline as a function of relative event time (e) measured in months:

$$
\beta_e = \alpha_0 * (e > 0) * e + \alpha_1 * (e > 12) * (e - 12) + \alpha_2 * (e > 36) * (e - 36) \qquad (10)
$$

The key assumption is known knot points (at 0 months, 12 months, and 36 months). We show below that our results are robust to alternative locations for the knot points.

Because of the structure of HRS data, the relationship between the $\mu's$ are linear combinations of the $\alpha's$. We can derive the relationship by substituting equations (8) into the linear spline equation (10):

$$
\hat{\mu_0} = \left(\sum_{e=0}^{23} \omega_e * (e > 0) * e \right) \alpha_0 + \left(\sum_{e=13}^{23} \omega_e * (e > 12) * (e - 12) \right) \alpha_1
$$

Note that there is no α_2 term because there are no relative months greater than 24 used to estimate the first wave fixed effect ($r = 0$). Similar calculations can be used to define μ_1 and μ_2, which can then be stacked together to arrive at the following matrix representation:

$$
\begin{bmatrix} \hat{\mu_0} \\ \hat{\mu_1} \\ \hat{\mu_2} \end{bmatrix} = \begin{bmatrix} A & B & C \\ D & E & F \\ G & H & I \end{bmatrix} \begin{bmatrix} \alpha_0 \\ \alpha_1 \\ \alpha_2 \end{bmatrix}
$$

where the elements in the 3x3 matrix are defined as follows:

$$A = \left(\sum_{e=0}^{23} \omega_e * (e > 0) * e \right), \quad B = \left(\sum_{e=13}^{23} \omega_e * (e > 12) * (e - 12) \right), \quad C = 0$$
$$D = \left(\sum_{e=13}^{47} \omega_e * (e > 0) * e \right), \quad E = \left(\sum_{e=13}^{47} \omega_e * (e > 12) * (e - 12) \right),$$
$$F = \left(\sum_{e=24}^{47} \omega_e * (e > 36) * (e - 36) \right), \qquad G = \left(\sum_{e=37}^{71} \omega_e * (e > 0) * e \right),$$
$$H = \left(\sum_{e=37}^{71} \omega_e * (e > 12) * (e - 12) \right), \quad I = \left(\sum_{e=37}^{71} \omega_e * (e > 36) * (e - 36) \right)$$

Each of the above elements can be calculated using only the sample weights, and then this allows researcher to recover spline estimates from wave fixed effects using simple matrix algebra. First, can define inverse of 3x3 matrix as follows:

$$M = \begin{bmatrix} A & B & C \\ D & E & F \\ G & H & I \end{bmatrix}^{-1} = \begin{bmatrix} a & b & c \\ d & e & f \\ g & h & i \end{bmatrix}$$

Then, we can recover estimates of spline parameters as follow:

$$\begin{bmatrix} \alpha_0 \\ \alpha_1 \\ \alpha_2 \end{bmatrix} = \begin{bmatrix} a & b & c \\ d & e & f \\ g & h & i \end{bmatrix} \begin{bmatrix} \hat{\mu_0} \\ \hat{\mu_1} \\ \hat{\mu_2} \end{bmatrix}$$

With spline parameters, we can recover the effects at 12, 24, and 36 months to be defined as follows

$$\begin{aligned}
\beta_{12} &= 12\alpha_0 = & 12(a\hat{\mu_0} + b\hat{\mu_1} + c\hat{\mu_2}) \\
\beta_{24} &= 24\alpha_0 + 12\alpha_1 = & (24a + 12d)\hat{\mu_0} + (24b + 12e)\hat{\mu_1} + (24c + 12f)\hat{\mu_2} \\
\beta_{36} &= 36\alpha_0 + 24\alpha_1 = & (36a + 24d)\hat{\mu_0} + (36b + 24e)\hat{\mu_1} + (36c + 24f)\hat{\mu_2} \\
\beta_{3yr\ avg} &= (1/3)(\beta_{12} + \beta_{24} + \beta_{36}) = & (24a + 12d)\hat{\mu_0} + (24a + 12d)\hat{\mu_1} + (24a + 12d)\hat{\mu_2}
\end{aligned}$$

Using sample weights formed by assuming uniform distribution of hospitalizations and using the empirical distribution of interview months, we obtain the following formulas to translate the event study coefficients into the relevant implied effects

$$\begin{aligned} \beta_{12} &= 2.227\hat{\mu_0} - 0.118\hat{\mu_1} + 0.0056\hat{\mu_2} \\ \beta_{36} &= -0.954\hat{\mu_0} + 1.550\hat{\mu_1} - 0.073\hat{\mu_2} \\ \beta_{3yr\ avg} &= 0.636\hat{\mu_0} + 0.716\hat{\mu_1} - 0.034\hat{\mu_2} \end{aligned}$$

C.2 Out of pocket expenses and labor market status

Respondents are asked about their out of pocket expenses "since the last interview". For labor market status (such as whether respondent is employed, disabled, retired, etc.), the interview question refers to current labor market status. For these outcomes, note that time between and interview wave and interview wave 0 (r) and months between the interview wave and the hospital admission (e) now has a very simple relationship, since we assume that hospital admissions are uniformly distributed and therefore occur on average 12 months before wave 0 interview. This simplifies the analysis, since the weights don't depend on the distribution of interview dates as they did for earnings or income that are reported "for the last calendar year". Under the assumption of uniformly distributed hospitalizations, all of the weights are (1/24)th.

We maintain the assumption that the true evolution of the outcome follows a piecewise linear spline as a function of relative event time (e), with knots at 0, 12, and 36 months (see equation (10)). Under this assumption, we can follow the same algebra as above with these different weights to recover the following expressions for out-of-pocket medical expenses:

$$\begin{aligned} \beta_{12} &= 1.627\hat{\mu_0} - 0.293\hat{\mu_1} + 0.039\hat{\mu_2} \\ \beta_{36} &= -0.248\hat{\mu_0} + 1.373\hat{\mu_1} - 0.182\hat{\mu_2} \\ \beta_{3yr\ avg} &= 0.460\hat{\mu_0} + 0.360\hat{\mu_1} - 0.048\hat{\mu_2} \end{aligned} \tag{11}$$

The β_e derived in equation (11) are reported as our implied effects for out of pocket medical expenses in our Tables (e.g., Table 2). Note that the effect at wave 0 μ_o is on average 12 months after the hospitalization but is reporting a 24-month look back period. If we had a knot point at 24 months rather than 12 months, then β_{12} would just be μ_0, but because we have a knot point at 12 months we get the above formula.

And for labor market status variables, we can follow the same algebra as above to recover the following expressions

$$
\begin{aligned}
\beta_{12} &= 1.627\hat{\mu_0} - 0.293\hat{\mu_1} + 0.0388\hat{\mu_2} \\
\beta_{36} &= -0.248\hat{\mu_0} + 1.373\hat{\mu_1} - 0.182\hat{\mu_2} \\
\beta_{3yr\ avg} &= 0.689\hat{\mu_0} + 0.540\hat{\mu_1} - 0.072\hat{\mu_2}
\end{aligned}
\tag{12}
$$

The β_e derived in equation (12) are reported as our implied effects for out of pocket medical expenses in our Tables (e.g., Table 2, see outcome "working part or full time").

Intuition for "negative weights"

One feature that may seem surprising is that some of the wave fixed effects have "negative weights" in the expressions above. This seems to be a general feature of using points estimated along a spline to figure out slope changes in spline function at known knot points. For example, suppose that there is linear spline with knot points at 12 months and 24 months, and the estimate of $\hat{\delta_0}$ is exactly equal to β_6, $\hat{\delta_1}$ is exactly equal to β_{18}, and $\hat{\delta_2}$ is exactly equal to β_{30}. Using same algebra as above, β_{12} can be estimated as $2\hat{\mu_0}$. More interestingly, β_{24} can be estimated as follows:

$$
\begin{aligned}
\beta_{24} &= \beta_{12} + (12) * ((\hat{\mu_1} - \beta_{12})/6) \\
\beta_{24} &= 2\hat{\mu_0} + 2 * (\hat{\mu_1} - 2\hat{\mu_0}) \\
\beta_{24} &= -2\hat{\mu}_0 + 2\hat{\mu_1}
\end{aligned}
$$

This is same result one obtains from following matrix algebra above, and it shows the negative weight on $\hat{\mu_0}$ is needed to recover an unbiased estimate of 24-month effect.

C.3 Robustness to alternative knot points

For consistency, we use knot points at 0, 12 months, and 36 months for all outcomes in the main analysis. We also carry out sensitivity analysis for alternative knot points in Appendix Table 7. These alternative knot points imply alternative weights of wave fixed effects given matrix algebra above (intuitively, the 3x3 matrix has different values because the wave fixed effects are weighted averages of different spline function parameters depending on assumption of knot point locations).

D Identification in Credit Report Data

D.1 Standard set-up

This identification problem is a specific example of a more general one studied in Borusyak and Jaravel (2016). To build intuition, first consider the following data set (which is a richer version of data set that is actually used): a large individual-level panel data set, with observations every year-month, and each individual is only hospitalized once. Define the individual's admission cohort, a, as the year-month of the hospitalization and relative event time, r, as the number of months between calendar time (t) and admission time (a); i.e., $r = t - a$. With this notation, we can define a fully nonparametric model of outcome y_{it} as follows:

$$y_{i,t} = \delta_a + \gamma_t + \mu_r + \varepsilon_{it} \tag{13}$$

where δ_a are admission cohort (i.e., admission year-month) fixed effects, γ_t are calendar time (i.e., calendar year-month) fixed effects, and μ_r are coefficients on indicators for months relative to the hospital admission, which occurs at relative month 0.

Given this setup, the three sets of fixed effects are not separately identified, since admission time is collinear with the combination of calendar time and relative event time. For the same reason, individual fixed effects cannot be included and estimated along with a full set of calendar time fixed effects and event time fixed effect, since individual fixed effects subsume admission cohort fixed effects. This is a well-known problem.

In order to identify equation (13) above, at least one pair of the fixed effects must be assumed to be the same. This normalization (by assumption) is necessary for identification. One way to do this would be to assume that some of the cohort fixed effects are equal, as in Card and Lemieux (2001). Another strategy would be to assume that the calendar time fixed effects are the same across months within a year (in other words, defining γ_t to be calendar year fixed effects instead of year-month fixed effects). In either case, the full set of relative event time fixed effects can now be separately identified with the appropriate normalizations.

D.2 Actual credit report data

D.2.1 Baseline non-parametric event study

In the actual data set that we use in paper, we do not observe outcomes in every year-month; we only observe outcomes in January each year. However, we observe the year-month of each hospitalization. As a result, there is no distinction between calendar year fixed effects and calendar year-month fixed effects. Our data require that we impose the normalization that calendar time effects are the same across months within a year.

As a result, we need an additional assumption (beyond our assumption that calendar time effects are constant across months within the year) in order to identify equation (13). In our baseline specification we assume that there are no admission cohort effects.[50] We estimate an equation of the form:

$$y_{i,t} = \gamma_t + \mu_r + \varepsilon_{i,t} \tag{14}$$

This equation implicitly assumes that outcomes are the same in expectation regardless of one's cohort (admission year-month); in other words, all admission year-month fixed effects are equal. Specifically, our baseline non-parametric event study is:

$$y_{i,t} = \gamma_t + \sum_{r=-47}^{r=-2} \mu_r + \sum_{r=0}^{r=72} \mu_r + \varepsilon_{i,t} \tag{15}$$

where γ_t are coefficients on calendar year fixed effects and μ_r are coefficients on indicators for months relative to the hospital admission which occurs at relative month 0. The key coefficients of interest are the pattern on the μ_r's which estimate the outcome at a given r relative to the month prior to hospitalization, μ_{-1}, which is omitted. The calendar year fixed effects control for any secular trends in the financial outcomes. All analyses include the sample weights.

The same identification issues inform our parametric spline model (see equation (5)).

D.2.2 Individual fixed effects

Our baseline specification did not allow for an individual-specific component of the error term that is correlated with the timing of hospitalization. If, for example, individuals of different admission cohorts have different levels of outcomes, this would violate our identifying assumption. In our robustness analysis we therefore explore an alternative specification with individual fixed effects.

We first consider the non-parametric event study. Since we now allow expected outcomes to differ across individuals (including across individuals in different admission cohorts), we now require an alternative normalization for identification. We restrict the first 13 relative event time fixed effects to be the same (i.e., we impose $\mu_r = 0$ for $r = -47$ to $r = -35$, which

[50] Alternatively, we could have assumed that a single pair (or a set of pairs) of year fixed effects are the same; this seems undesirable, however, given that our data set spans the Great Recession and its aftermath.

is one more restriction than in our baseline specification which only imposes $\mu_{-1} = 0$):

$$y_{i,t} = \alpha_i + \gamma_t + \sum_{r=-34}^{r=72} \mu_r + \varepsilon_{i,t} \tag{16}$$

This specification assumes there are no pre-trends in outcome $y_{i,t}$ in the months leading up to the hospitalization event between $r = -47$ and $r = -35$. If, contrary to our assumption, there are pre-trends during the excluded event time window, this can cause bias in all of the other estimated event time dummies; however if the true data generating process has a linear pre-trend (from -47 to -1) then we will not get bias in the estimated post-hospitalization effects relative to that linear pre-trend.

For the parametric event study, to be able to include individual fixed effects in equation (5), we must include the same kind of normalization that is discussed above. We do this by once again imposing that there are no pre-trends between $r = -47$ and $r = -35$, but keeping the rest of the cubic spline model otherwise identical. This gives the following estimating equation:

$$\begin{aligned} y_{i,t} &= \alpha_i + \gamma_t + \beta_1 (r-35)\{r > -35\} + \beta_2 r^2 \{r > 0\} + \beta_3 r^3 \{r > 0\} \\ &\quad + \beta_4 (r-12)^3 \{r > 12\} + \beta_5 (r-24)^3 \{r > 24\} + \varepsilon_{i,t} \end{aligned} \tag{17}$$

This equation implicitly imposes a spline with slope of 0 between $r = -47$ and $r = -35$.

E RD estimates of insurance coverage

As one rough way to gauge the validity of the insured-uninsured "difference-in-differences" comparison as an estimate of the causal impact of insurance coverage, we compare those results to the estimated 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 (Card, Dobkin, and Maestas 2008, 2009; Barcellos and Jacobson 2015). The RD research strategy is arguably more credible and reliable since it relies on weaker identifying assumptions. However, relative to the difference-in-difference estimates, the RD estimates have much lower power and also must be estimated on a demographically distinct sample of elderly adults (rather than non-elderly adults as in our baseline sample). The interpretation of the estimates will also depend on how the "first stage" effect of turning 65 on insurance coverage is parameterized.

To implement the RD analysis, we focus on a sample of a sample of 60-70 year olds admitted to the hospital through the emergency department in California between 2003 and

2007. As in our prior analyses, we restrict to individuals without recent prior hospital admissions. Appendix Table 13 provides details of sample construction and summary statistics. We estimate the following RD specification:

$$(18)\quad y_i = \beta_0 + \beta_1 age_i + \beta_2 age_i^2 + \beta_3 1\{age_i \geq 65\} age + \beta_4 1\{age_i \geq 65\} age_i^2 + \delta 1\{age_i \geq 65\} + \epsilon_i$$

where age_i is the age of individual i (in months) at the time of their hospital admission. Equation (18) is a standard RD equation, that allows for a quadratic relationship between the outcome and the running variable (age), and allows the slope of that quadratic relationship to change at the discontinuity (age 65). The key coefficient of interest in the RD analysis is δ, which reports the level shift in the outcome associated with turning 65. We estimate equation (18) on the sample of individuals hospitalized within 5 years of age 65 (on either side). All estimates are weighted by the inverse probability an individual was sampled, and we cluster the standard errors on age (in months).

We use the RD to estimate the impact of insurance coverage at admission on outcomes in the credit reports approximately one year (i.e., 1 to 12 months) and approximately four years (i.e., 37 - 48 months) post admission. For the former, we use the January credit report from the calendar year following the admission; for the latter we use the January credit report from the fourth January following admission. We focus, however, on the "one year" effects, however, since with longer time horizons more of the "control" group that is less than 64 ends up aging past 65 and becomes treated (insured) for subsequent hospital admission.

Appendix Figures 29 and 30 show graphically the polynomial estimated in equation (18), as well as (non-parametrically) the relationship between mean outcomes "one year" after admission and age in months at hospitalization; Appendix Table 36, columns 1 and 2, report the estimated coefficient δ on credit report outcomes analyzed one year and four years after admission. For added precision, columns 3 and 4 repeat the same analyses but using as the outcome variable a "differenced" variable - Δy_i - that is the change in the individual's outcome from their credit report in the year prior to the hospitalization (measured between 12 and 23 months before the hospitalization) and the post-hospitalization credit report outcome. The results are similar across the columns, with a slight increase in precision for the differenced variables.

The first rows of Appendix Table 36 show that being over 65 is associated with a statistically significant increase in the number of admissions of about 300 admissions over our four year analysis period (2003-2007), or about 7 percent relative to the 64 year old average . This is consistent with prior findings using this design (Card, Dobkin, and Maestas 2008, 2009 as well as others) that insurance increases the probability of hospital admissions. It raises the potential for contaminating effects of insurance coverage on the composition of people admitted to the hospital. To try to avoid such issues, we restrict the sample to admissions that occur through the emergency room, which are arguably less discretionary. Consistent with this intuition, we see that restricting attention to admissions that occur through the ER reduces any impact of being 65 on admissions through the ER to a (statistically insignificant) 60 admissions, or about 3 percent relative to the 64 year old average. All of the subsequent results are limited to this sample of hospital admissions that come through the ER. Row 3 shows that being 65 is associated with a decrease in the probability of the index admission through the ER being uninsured by 6 percentage points.

The remaining rows of the table show how insurance affects the impact of admissions through the ER on various credit report outcomes. We estimate a statistically significant decline in unpaid bills (number of collections or collection balances) associated with Medicare coverage; the visual evidence in Appendix Figure 29 also suggests an impact on collections. For all other outcomes however, the results lack sufficient precision to be meaningfully interpreted. The visual evidence in Appendix Figures 29 and 30 does not suggest an impact of Medicare, but again the results are noisy.

For collections, where we do have precision, the implied impact of insurance coverage is several times larger than a simple difference-in-differences comparison between the insured and uninsured in Table 5 would suggest. Appendix Table 36 (columns 1 or 3) indicates that turning 65 is associated with a statistically significant decrease in collection balances "one year" after a hospital admission of about \$450 (In fact, this is probably a lower bound on the "1 year" estimate since it averages effects 1 - 12 months post admission and the results from the event study analysis in the main text suggest that the impact of admissions on collections grows over time during the first year).

We can turn this into an implied effect of insurance coverage using the "first stage" estimate in row 3 that being 65 is associated with a 6 percentage point decline in the probability of the admission being uninsured. This may represent too low a first stage, as the impact of Medicare coverage is likely not limited to simply the extensive margin of insurance coverage; as emphasized by Card, Dobkin, and Maestas (2009), Medicare coverage at age 65 is also associated with a change in the nature of insurance coverage (including, for example, a decline in the share of individuals whose primary coverage is managed care). A larger first stage would reduce our estimated impact of insurance coverage, so we use the smaller first stage to get a potential upper bound on the impact of insurance coverage. Doing so suggests that, "1 year" later, insurance is associated with a decreased impact of a hospital admissions on collection balances of about \$7,500.

We compared the RD estimate to what a simple comparison of the event study impact on the insured relative to the uninsured might imply for the "impact" of insurance. The 12-month results in Table 5, column 4 suggest that the uninsured experience a about a $4,300 greater increase in collection balances when they have a hospital admission than the insured do (i.e., they experience an increase in collection balances of $4,469 compared to $122 for the insured). Thus the RD estimates imply that the impact of insurance coverage may be about 75 percent larger than what we would estimate based on the difference-in-difference comparisons. Of course, these two approaches may vary for many reasons; among other things, they are estimated on populations that differ along such dimensions as age and source of admission.

F Additional Appendix References

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G. Appendix Tables and Figures

G.1 Health and Retirement Survey

G.1.1 Data Details

Appendix Table 1. Timing and Cohort Structure

Wave \ Cohort	Initial HRS	AHEAD	Children of the Depression	War Babies	Early Baby Boomers	Mid Baby Boomers
Birth Years	1931-1941	Before 1924	1924-1930	1942-1947	1948-1953	1954-1959
1	1992					
2	1994	Not Used	Not Available	Not Available		
3	1996				Not Available	
4	1998	1998	1998	1998		
5	2000	2000	2000	2000		Not Available
6	2002	2002	2002	2002		
7	2004	2004	2004	2004	2004	
8	2006	2006	2006	2006	2006	
9	2008	2008	2008	2008	2008	
10	2010	2010	2010	2010	2010	2010
11	2012	2012	2012	2012	2012	2012

Notes: Columns are each of the cohorts included in the HRS, with the birth years of survey respondents included in the survey below. The first column lists the full set of survey waves of the HRS. The years in the table indicate the survey year for each survey wave and when each cohort entered the HRS.

Appendix Table 2. Variable Definitions

Variable	RAND HRS Extract Variable Name	Reference Period	Definition
Panel A. Demographic, Hospitalization, and Insurance Variables			
Unique respondent identifier	hhidpn	n/a	Unique ID, it is the household ID*1000 + the person number
Interview status	rWiwstat	Time of Interview	Whether the respondent was alive and responded to the interview. All person-wave observations that were not "Responded, Alive" were dropped
Age at hospitalization	rWagey_b	Contemporaneous	Respondent's age in years at the beginning of the interview (subtract 1 to get average age at hospitalization)
Gender	ragender	n/a	Respondent gender
Race	raracem	n/a	Respondent race (only includes white, black, and other)
Hispanic	rahispan	n/a	Whether the respondent is Hispanic or not, independent of the entry for raracem
Cohort	hacohort	n/a	Entry cohort in which the household was originally sampled
Sampling Weight	rWwtresp	Contemporaneous	Person-level analysis weights structured to match the CPS living, non-institutionalized respondent weights. This implies nursing home residents have weights of zero. These weights are used in all of the analysis we do of the HRS.
Hospitalization indicator	rWhosp	Since the last interview (~2 years)	Whether the respondent reports any overnight hospital stay in the reference period
Year of hospitalization	rWagey_b, rWagyear	Produced	We add ragey_b (age at beginning of interview) and ragyear (year of the respondent's birth) to get a rough sense of the year of the hospitalization, substracting 1 to obtain the age at hospitalization on average (because hospitalizations occur one year prior to interview on average based on the sampling frame
Insured	rWhigov, rWcovr, rWcovs	Contemporaneous	Indicates whether the respondent is covered by any government health insurance program (Medicare, Medicaid, VA, or other) or by employer-provided health insurance through their employer (rcovr) or their spouse (rcovs)
Uninsured	rWhenum, rWhigov, rWcovr, rWcovs, rWhiothp	Produced	Produced from several variables; a respondent is classified as uninsured if the number of health insurance plans is 0, they report no government insurance, no private insurance, and no other insurance (a catch-all for non-government and non-private insurance)
Panel B. Outcomes			
Out-of-Pocket medical spending	rWoopmd	Since the last interview (~2 years)	Total out-of-pocket medical expenditure in the reference period, summed across individual categories of spending. All components (e.g., hospital costs, doctor visit costs, prescription drug costs) are asked and imputed (when necessary) separately.
Any Earnings / Respondent Earnings	rWiearn, rWisemp	Previous calendar year	Total wage/salary/bonuses/tips earnings based on rWiearn in the core HRS data, plus self-employment income (available beginning in wave 3 from the Wealth and Income Imputation Supplement from RAND)
Spousal Earnings	sWiearn, sWisemp	Previous calendar year	Total wage/salary/bonuses/tips earnings based on sWiearn in the core HRS data, plus self-employment income (available beginning in wave 3 from the Wealth and Income Imputation Supplement from RAND)
Works full-time	rWlbrf	Contemporaneous	Indicates whether the respondent is working full-time. The categories working full-time, working part-time, unemployed (must be seeking work), partly-retired (working part-time but retirement mentioned, we reclassify these as part-time), retired, disabled, or not in the labor force (is not working and does not mention retirement, disability, or looking for work) are exhaustive and mutually exclusive
Work limited by health	rWhlthlm	Contemporaneous	Indicates the respondent's answer to the following question "Now I want to ask how your health affects paid work activities. Do you have any impairment or health problem that limits the kind or amount of paid work you can do?"
Retired	rWlbrf	Contemporaneous	Indicates whether the respondent is retired
Receive Social Insurance Payments / Respondent Social Insurance Payments	rWissdi, rWiunwc, rWisret	Previous calendar year	Government income that includes all income categories outside of earnings and pension and annuity income (SSDI, SSI, UI, and SS retirement income) in the core HRS sample
Household Social Insurance Payments	rWissdi, rWiunwc, rWisret, sWissdi, sWiunwc, sWisret	Previous calendar year	Government income that includes all income categories outside of earnings and pension and annuity income (SSDI, SSI, UI, and SS retirement income) in the core HRS sample
Respondent pension and annuity income	rWipena	Previous calendar year	Pension and annuity income
Household pension and annuity income	rWipena, sWipena	Previous calendar year	Pension and annuity income
Household capital and business income	hWicap, rWisemp, sWisemp	Previous calendar year	The sum of household business or farm income, business income, gross rent, dividend and interest income, trust funds or royalties, and other asset income. Note that this variable is hWicap in the core HRS which includes self-employment income, and we merge rWisemp and sWisemp from the RAND Wealth and Income Imputation Supplement to reallocate self-employment income to earnings.
Other Household Income	hWiothr	Previous calendar year	The sum of alimony (until wave 7), other income, and lump sums from insurance, pension, and inheritance
Total Household Income	rWiearn, rWisemp, sWiearn, sWisemp, rWissdi, rWiunwc, rWisret, sWissdi, sWiunwc, sWisret, rWipena, sWipena	Previous calendar year	The sum of respondent and spousal earnings, household social insurance payments, and household pension and annuity income.

Notes: The structure of the RAND variable names lists the relevant person or persons first ('r' for respondent, 's' for spouse, and 'h' for household), the survey wave W, and the variable name last. All variables are inflation adjusted using the CPI to 2005 dollars. Indicator variables are multiplied by 100 to display results as percentages.

Appendix Table 3. Weights to Convert Wave Effects to Spline Parameters

Calendar year look-back variables (e.g., earnings, household income)

Wave 0 indicator		Wave 1 indicator		Wave 2 indicator	
e	ω_e	e	ω_e	e	ω_e
-11	0.09	13	0.09	37	0.09
-10	0.23	14	0.23	38	0.23
-9	0.46	15	0.46	39	0.46
-8	0.75	16	0.75	40	0.75
-7	1.27	17	1.27	41	1.27
-6	1.99	18	1.99	42	1.99
-5	2.79	19	2.79	43	2.79
-4	3.44	20	3.44	44	3.44
-3	3.80	21	3.80	45	3.80
-2	4.03	22	4.03	46	4.03
-1	4.10	23	4.10	47	4.10
0	4.17	24	4.17	48	4.17
1	4.17	25	4.17	49	4.17
…	…	…	…	…	…
11	4.17	35	4.17	59	4.17
12	4.17	36	4.17	60	4.17
13	4.07	37	4.07	61	4.07
14	3.93	38	3.93	62	3.93
15	3.71	39	3.71	63	3.71
16	3.42	40	3.42	64	3.42
17	2.89	41	2.89	65	2.89
18	2.18	42	2.18	66	2.18
19	1.38	43	1.38	67	1.38
20	0.73	44	0.73	68	0.73
21	0.37	45	0.37	69	0.37
22	0.13	46	0.13	70	0.13
23	0.06	47	0.06	71	0.06

Implied matrix used to map wave FEs to spline params

$$\begin{bmatrix} 6.8455 & 0.8503 & 0 \\ 29.991 & 17.991 & 0.8503 \\ 53.991 & 41.991 & 17.991 \end{bmatrix}$$

Current status variables and look-back variables defined relative to interview date (e.g., labor force status, out-of-pocket costs)

Wave 0 indicator		Wave 1 indicator		Wave 2 indicator	
e	ω_e	e	ω_e	e	ω_e
1	4.17	25	4.17	49	4.17
2	4.17	26	4.17	50	4.17
…	…	…	…	…	…
23	4.17	47	4.17	71	4.17
24	4.17	48	4.17	72	4.17

Implied matrix used to map wave FEs to spline params

$$\begin{bmatrix} 12.5 & 3.25 & 0 \\ 36.5 & 24.5 & 3.25 \\ 60.5 & 48.5 & 24.5 \end{bmatrix}$$

Notes: This table reports the sample weights used to convert wave fixed effects to spline parameters. See Appendix C for details. The weights sum to 100 across each column, and are used with formulas in Appendix C to define the matrix to the right of the table which is used to recover spline parameter estimates. For the earnings variables, the weights are defined based on the empirical distribution of interview dates assuming a uniform distribution of hospitalizations across months. For the current status variables, the weights are uniform under the assumption of a uniform distribution of hospitalizations across months.

Appendix Figure 1. Visualization of Event Study Estimate at Wave 0

																									Interview Month
23	22	21	20	19	18	17	16	15	14	13	12	11	10	9	8	7	6	5	4	3	2	1	0	1	January
22	21	20	19	18	17	16	15	14	13	12	11	10	9	8	7	6	5	4	3	2	1	0	-1		February
21	20	19	18	17	16	15	14	13	12	11	10	9	8	7	6	5	4	3	2	1	0	-1	-2		March
20	19	18	17	16	15	14	13	12	11	10	9	8	7	6	5	4	3	2	1	0	-1	-2	-3		April
19	18	17	16	15	14	13	12	11	10	9	8	7	6	5	4	3	2	1	0	-1	-2	-3	-4		May
18	17	16	15	14	13	12	11	10	9	8	7	6	5	4	3	2	1	0	-1	-2	-3	-4	-5		June
17	16	15	14	13	12	11	10	9	8	7	6	5	4	3	2	1	0	-1	-2	-3	-4	-5	-6		July
16	15	14	13	12	11	10	9	8	7	6	5	4	3	2	1	0	-1	-2	-3	-4	-5	-6	-7		August
15	14	13	12	11	10	9	8	7	6	5	4	3	2	1	0	-1	-2	-3	-4	-5	-6	-7	-8		September
14	13	12	11	10	9	8	7	6	5	4	3	2	1	0	-1	-2	-3	-4	-5	-6	-7	-8	-9		October
13	12	11	10	9	8	7	6	5	4	3	2	1	0	-1	-2	-3	-4	-5	-6	-7	-8	-9	-10		November
12	11	10	9	8	7	6	5	4	3	2	1	0	-1	-2	-3	-4	-5	-6	-7	-8	-9	-10	-11	12	December
24								Hospitalization Months Prior to Interview															1		

Notes: This figure shows all of the combinations of months of interview and months of hospitalization relative to interview. Each cell reports the month of interview relative to the month of hospitalization, which in wave 0 ranges from 11 months before hospitalization to 23 months after the hospitalization. The grey area represents the share of sample where only part of the period of reported income is after hospitalization, while the lower right triangle represents individuals whose hospitalization occurred after the entire period of reported income. The upper left triangle represents individuals whose reported income covers the period entirely after hospitalization.

G.1.2 Summary Statistics

Appendix Table 4. Sample Composition

	Non-Elderly Insured			Ages 60 to 64 Insured			Elderly		
	Never Hospitalized	Hospitalized	Hospitalized with Pre-Period Observation [Baseline]	Never Hospitalized	Hospitalized	Hospitalized with Pre-Period Observation [Baseline]	Never Hospitalized	Hospitalized	Hospitalized with Pre-Period Observation [Baseline]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Sampling Cohort									
AHEAD Cohort (Born Before 1924)	.12	.21	.18	.15	.17	.1	15	44	33
Children of the Depression (CODA) Cohort (Born 1924 to 1930)	.099	.019	.093	.1	.16	.08	14	21	20
Initial HRS Cohort (Born 1931 to 1941)	25	22	34	45	57	59	53	28	39
War Babies Cohort (Born 1942 to 1947)	30	29	30	41	28	31	16	4.7	6.4
Early Baby Boomers Cohort (Born 1948 to 1953)	45	30	27	13	13	9.9	.12	.55	.27
Mid Baby Boomers Cohort (Born 1954 to 1959)	0	19	8.9	0	1.1	.49	0	.32	.21
Panel B: Demographics									
Age at admission	56	54	56	61	62	62	71	75	75
Male	50	48	48	49	50	49	43	43	43
Year of admission	2003	2003	2002	2004	2003	2002	2005	2002	2004
Has spouse in survey wave preceding hospitalization	77	74	77	76	75	78	62	54	55
Panel C: Race/Ethnicity									
Hispanic	6.4	5.9	5	6.1	7.6	5.8	7.2	5.7	6
Black	8.8	12	9.7	7.5	10	9.7	9.2	8.3	7.9
White	86	82	85	89	86	88	86	89	89
Other Race	4.9	5.9	5	3.7	3.4	2.5	4.6	3.1	3.4
Panel D: Index Hospitalization									
Medicaid[a]	2.6	12	5.9	2.2	13	6.7	4	9.4	6.5
Private[a]	94	88	94	93	87	93	42	32	36
Panel E: Subsequent Outcomes									
Admitted to Hospital Within 12 Months	.	27	24	.	26	22	.	31	27
Admitted to Hospital Within 36 Months	0	38	37	0	35	34	0	45	44
Insured within 12 Months	89	100	93	87	100	93	99	99	99
Insured within 36 Months	86	93	92	90	94	94	98	99	99
Individuals	3,606	4,271	2,732	2,359	1,791	1,627	2,729	9,015	5,785

Notes: Statistics are estimated for the survey wave reporting the hospitalization for columns 2 and 3. The never hospitalized sample is comprised of individuals who never report a hospitalization over their full panel in the HRS, and statistics are estimated on a randomly drawn observation within the the relevant age range (under 60 at the time of interview). Hospitalizations are defined using a two-year look-back window in between surveys for the HRS, so age is defined as age at interview minus one year. All estimates are weighted using survey weights.

[a] Insurance status (Medicaid or Private) is defined using the survey wave preceding the hospitalization for the baseline and never hospitalized samples. Insurance status for the hospitalized sample (without restricting to a pre-period observation, columns 2, 5, and 8) is defined using the survey wave reporting the hospitalization. Percentages may not sum to 100 if insurance coverage comes from another government source (e.g., coverage through the VA or Medicare).

Appendix Table 5. Pre-Hospitalization Summary Statistics

	Mean	Std Dev	Median	Share Zero	90th Percentile
	(1)	(2)	(3)	(4)	(5)
Panel A. Non-Elderly Insured (Ages 50 to 59)					
Out-of-Pocket Medical Spending	2,133	(4,363)	984	.11	4,830
Working Part or Full-Time	74	(44)	100	.28	100
Respondent Earnings	45,327	(67,533)	32,992	.22	90,000
Spousal Earnings	30,718	(51,613)	16,775	.4	77,369
Household Social Insurance Payments	2,649	(6,327)	0	.72	10,720
Total Household Income	82,512	(98,486)	62,000	.015	150,218
Panel B. Insured Ages 60 to 64					
Out-of-Pocket Medical Spending	2,210	(4,198)	1,017	.11	5,269
Working Part or Full-Time	55	(50)	100	.45	100
Respondent Earnings	35,284	(50,803)	24,264	.32	82,000
Spousal Earnings	22,863	(40,753)	2,206	.5	62,155
Household Social Insurance Payments	4,895	(8,828)	0	.56	15,566
Total Household Income	70,374	(70,480)	55,730	.016	136,936
Panel C. Elderly (Ages 65 and Older)					
Out-of-Pocket Medical Spending	2,521	(5,218)	1,195	.1	5,790
Working Part or Full-Time	11	(32)	0	.89	100
Respondent Earnings	8,248	(36,344)	0	.77	19,853
Spousal Earnings	4,672	(22,054)	0	.85	8,647
Household Social Insurance Payments	15,811	(9,823)	14,556	.048	26,470
Total Household Income	38,287	(52,395)	25,429	.0044	72,749

Notes: Summary statistics are calculated using the survey wave preceding the first wave which reports a hospitalization. N = 2,732 unique individuals for the non-elderly insured, 1,627 for the insured ages 60 to 64, and 5,785 for the elderly. All estimates are weighted using survey weights.

G.1.3 Regression Coefficients

Appendix Table 6. Regression Coefficients from Parametric Specifications for the Insured

Regressor	Event Time	Wave 0 Effect	Wave 1 Effect	Wave 2 Effect	Wave 3 Effect	Constant
	(1)	(2)	(3)	(4)	(5)	(6)
Out-of-Pocket Medical Spending	131	2,217	1,268	989	1,234	2,212
	(109)	(258)	(337)	(431)	(531)	(192)
	[.23]	[<.001]	[<.001]	[.022]	[.02]	[<.001]
Working Part or Full-Time	-3.5	-7.2	-12	-18	-21	70
	(1)	(1.4)	(2.2)	(3.2)	(4.3)	(1.6)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
Respondent Earnings	167	-3,383	-9,765	-11,483	-13,024	44,470
	(1,968)	(1,951)	(3,406)	(4,637)	(6,050)	(2,448)
	[.93]	[.083]	[.0041]	[.013]	[.031]	[<.001]
Spousal Earnings	-2,031	-150	942	208	2,613	29,302
	(1,452)	(1,855)	(3,055)	(4,219)	(5,595)	(2,275)
	[.16]	[.94]	[.76]	[.96]	[.64]	[<.001]
Household Social Insurance Payments	191	278	1,083	2,097	3,549	3,726
	(145)	(183)	(358)	(504)	(693)	(257)
	[.19]	[.13]	[.0025]	[<.001]	[<.001]	[<.001]
Total Household Income	-492	-4,198	-8,150	-10,127	-6,950	82,696
	(2,891)	(3,317)	(5,577)	(7,483)	(9,555)	(4,000)
	[.87]	[.21]	[.14]	[.18]	[.47]	[<.001]

Notes: The sample is the non-elderly insured (see Table 1, column 1) in the HRS survey. The table reports the regression coefficients for the primary regressors included in equation (4), which are used to estimate implied effects in Table 2. Standard errors (clustered on the individual) are in parentheses and p-values are in brackets. All proportions are multiplied by 100 and all estimates are weighted using survey weights. N = 2,732.

G.1.4 Robustness

Appendix Table 7. Robustness to Selection of Knots for Transformation to Calendar Time Effects

Specification	Knots at 0, 12, and 36 months [Baseline]	Knots at 0, 6, and 36 months	Knots at 0, 12, and 24 months	Knots at 0, 6, and 24 months
	(1)	(2)	(3)	(4)
Panel A: Out-of-Pocket Medical Spending				
12-month effect	3,274	2,596	3,669	2,682
	(373)	(297)	(447)	(313)
	[<.001]	[<.001]	[<.001]	[<.001]
Average annual effect over 36 months	1,428	1,237	1,648	1,318
	(202)	(189)	(213)	(186)
	[<.001]	[<.001]	[<.001]	[<.001]
Pre-hospitalization mean	2,133	2,133	2,133	2,133
Panel B: Working Part or Full-Time				
12-month effect	-8.9	-7.7	-9.6	-7.8
	(1.8)	(1.5)	(2.1)	(1.6)
	[<.001]	[<.001]	[<.001]	[<.001]
Average annual effect over 36 months	-10	-12	-13	-9.4
	(1.9)	(2.2)	(2.3)	(1.8)
	[<.001]	[<.001]	[<.001]	[<.001]
Pre-hospitalization mean	74	74	74	74
Panel C: Respondent Earnings				
12-month effect	-6,445	-5,664	-6,310	-5,270
	(4,024)	(3,111)	(4,142)	(2,252)
	[.11]	[.069]	[.13]	[.019]
Average annual effect over 36 months	-8,753	-8,533	-8,732	-8,441
	(3,415)	(3,204)	(3,429)	(3,018)
	[.01]	[.0077]	[.011]	[.0052]
Pre-hospitalization mean	45,327	45,327	45,327	45,327
Panel D: Spousal Earnings				
12-month effect	-444	-169	-570	270
	(3,851)	(2,946)	(3,982)	(2,074)
	[.91]	[.95]	[.89]	[.9]
Average annual effect over 36 months	572	651	550	784
	(3,114)	(2,907)	(3,129)	(2,723)
	[.85]	[.82]	[.86]	[.77]
Pre-hospitalization mean	30,718	30,718	30,718	30,718
Panel E: Household Social Insurance Payments				
12-month effect	503	480	515	440
	(379)	(292)	(392)	(217)
	[.18]	[.1]	[.19]	[.042]
Average annual effect over 36 months	881	875	884	863
	(338)	(322)	(338)	(312)
	[.009]	[.0066]	[.0089]	[.0057]
Pre-hospitalization mean	2,649	2,649	2,649	2,649
Panel F: Total Household Income				
12-month effect	-8,443	-6,797	-8,557	-5,281
	(6,857)	(5,294)	(7,059)	(3,793)
	[.22]	[.2]	[.23]	[.16]
Average annual effect over 36 months	-8,161	-7,694	-8,184	-7,270
	(5,709)	(5,332)	(5,735)	(4,987)
	[.15]	[.15]	[.15]	[.14]
Pre-hospitalization mean	82,512	82,512	82,512	82,512
Number of Individuals	2,732	2,732	2,732	2,732
Number of Observations	13,286	13,286	13,286	13,286

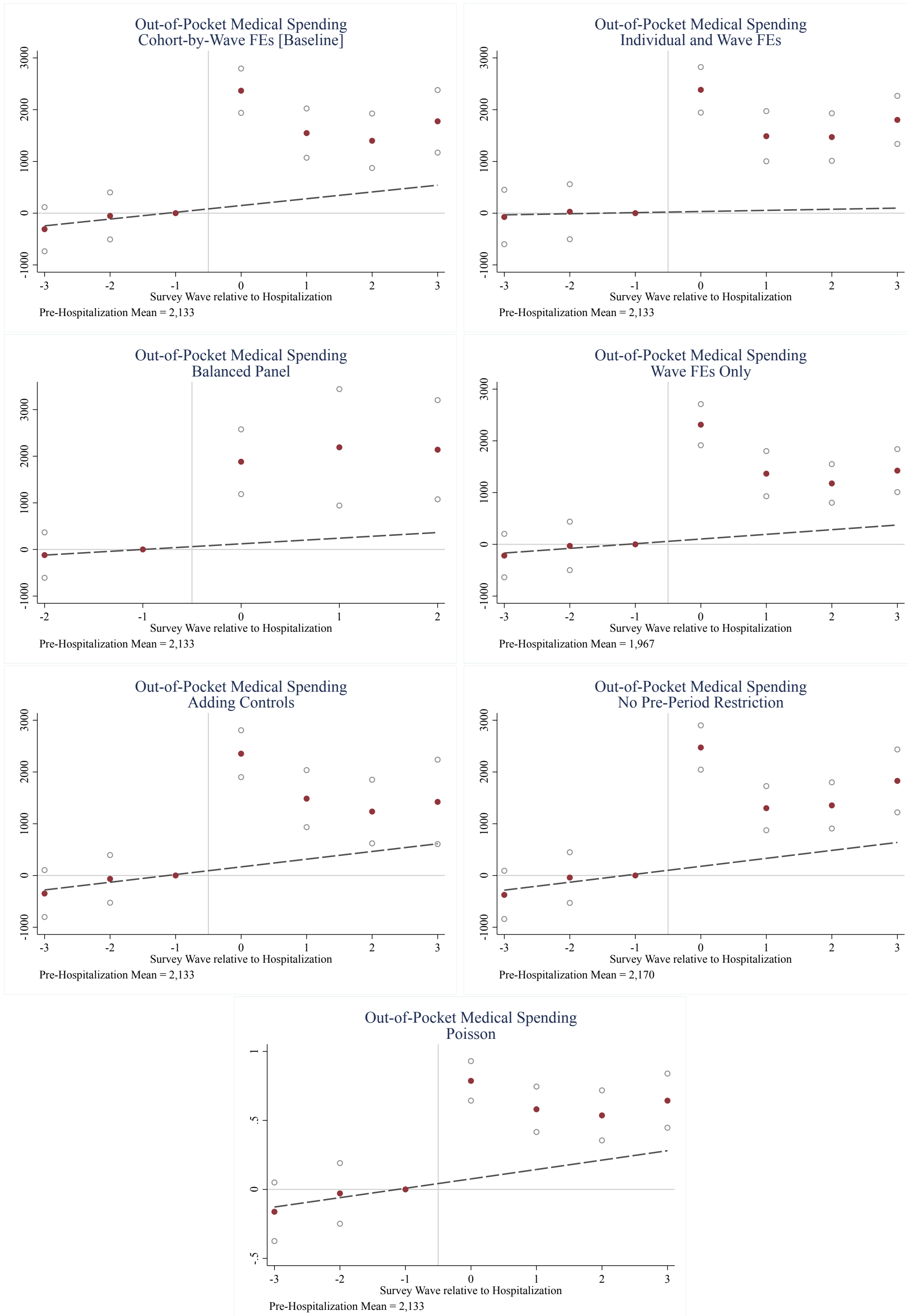
Notes: Sample is the non-elderly insured (see Table 1, column 1). Column 1 reports 12-month effects and average annual effects over 36 months calculated as described in Table 2 and Appendix C, assuming a piecewise linear function form with knots at 12 and 36 months. Columns 2 through 4 explore robustness of the estimates to the selected knot points used to transform event time coefficients from equation (4) into calendar time effects. See Appendix C for full details on these transformations. Standard errors (clustered on the individual) are in parentheses and p-values are in brackets. All estimates are weighted using survey weights.

Appendix Table 8. Robustness of Earnings Impact to Variable and Sample Definitions

Variable and Sample Definition	Respondent Earnings (Includes self-employment income, all waves)	Respondent Earnings (Includes self-employment income, waves 3 forward)	Labor Market Earnings (waves 3 forward)	Self-employment Earnings (waves 3 forward)
	(1)	(2)	(3)	(4)
12-month effect	-6,445	-9,174	-11,930	520
	(4,024)	(5,419)	(4,742)	(3,366)
	[.11]	[.09]	[.012]	[.88]
Effect at 36 months	-11,071	-13,008	-14,369	-749
	(3,475)	(4,790)	(3,967)	(2,415)
	[.0014]	[.0066]	[<.001]	[.76]
Average annual effect over 36 months	-8,753	-11,085	-13,141	-114
	(3,415)	(4,776)	(4,048)	(2,487)
	[.01]	[.02]	[.0012]	[.96]
Pre-hospitalization mean	45,327	48,734	41,593	7,577
Number of Individuals	2,732	2,674	2,674	2,674
Number of Observations	13,286	10,832	10,832	10,832

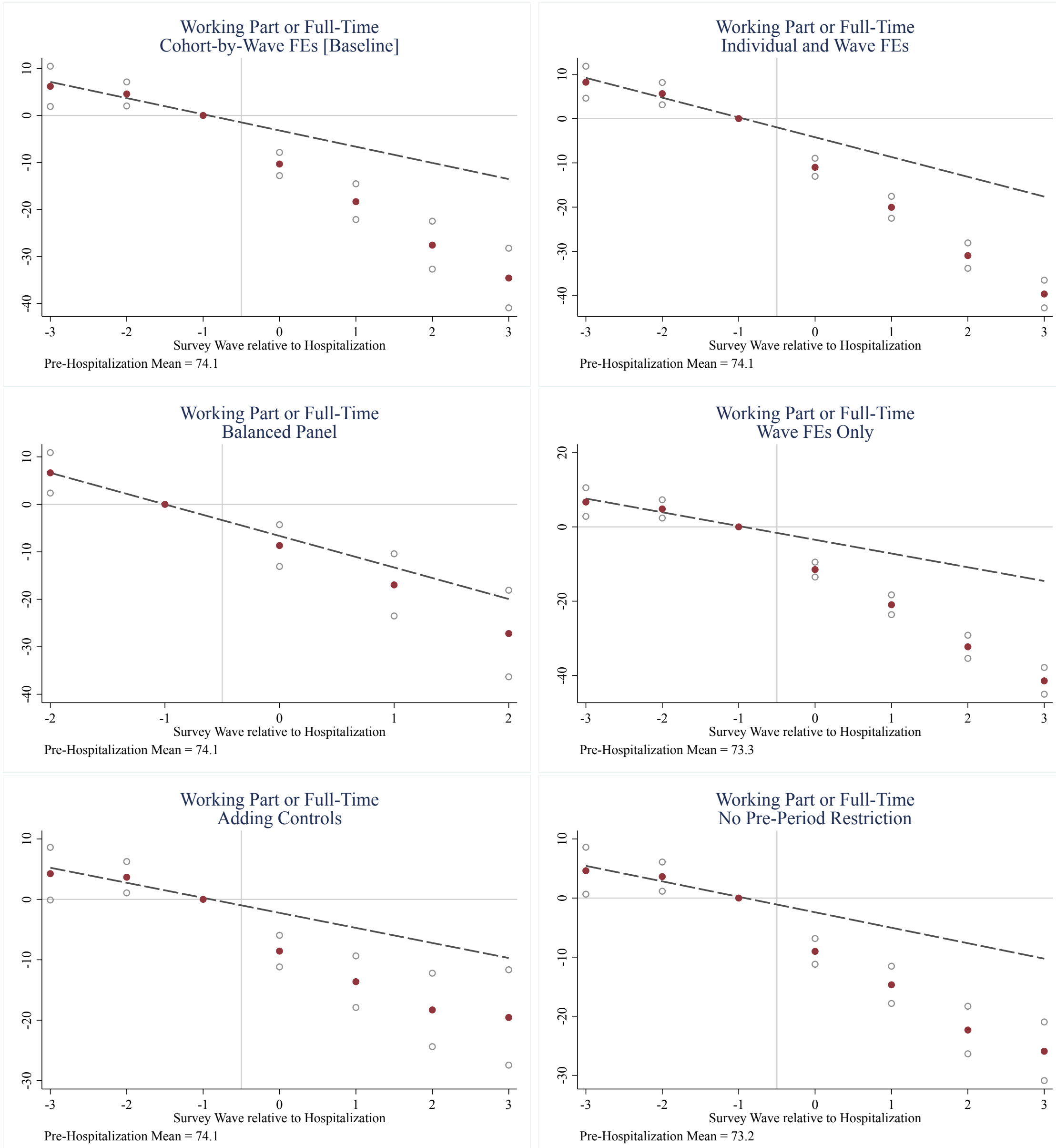
Notes: Sample is the non-elderly insured (see Table 1, column 1) in the HRS. All columns report effects based on OLS estimates of equation (4). Standard errors (clustered on the individual) are in parentheses and p-values are in brackets. All estimates are weighted using survey weights. The top row indicates whether or not self-employment income is included in the definition of earnings, and whether or not the first two waves of the HRS are included in the regression.

Appendix Figure 2. Impact of Hospitalization on Out-of-Pocket Medical Spending for the Insured, Robustness



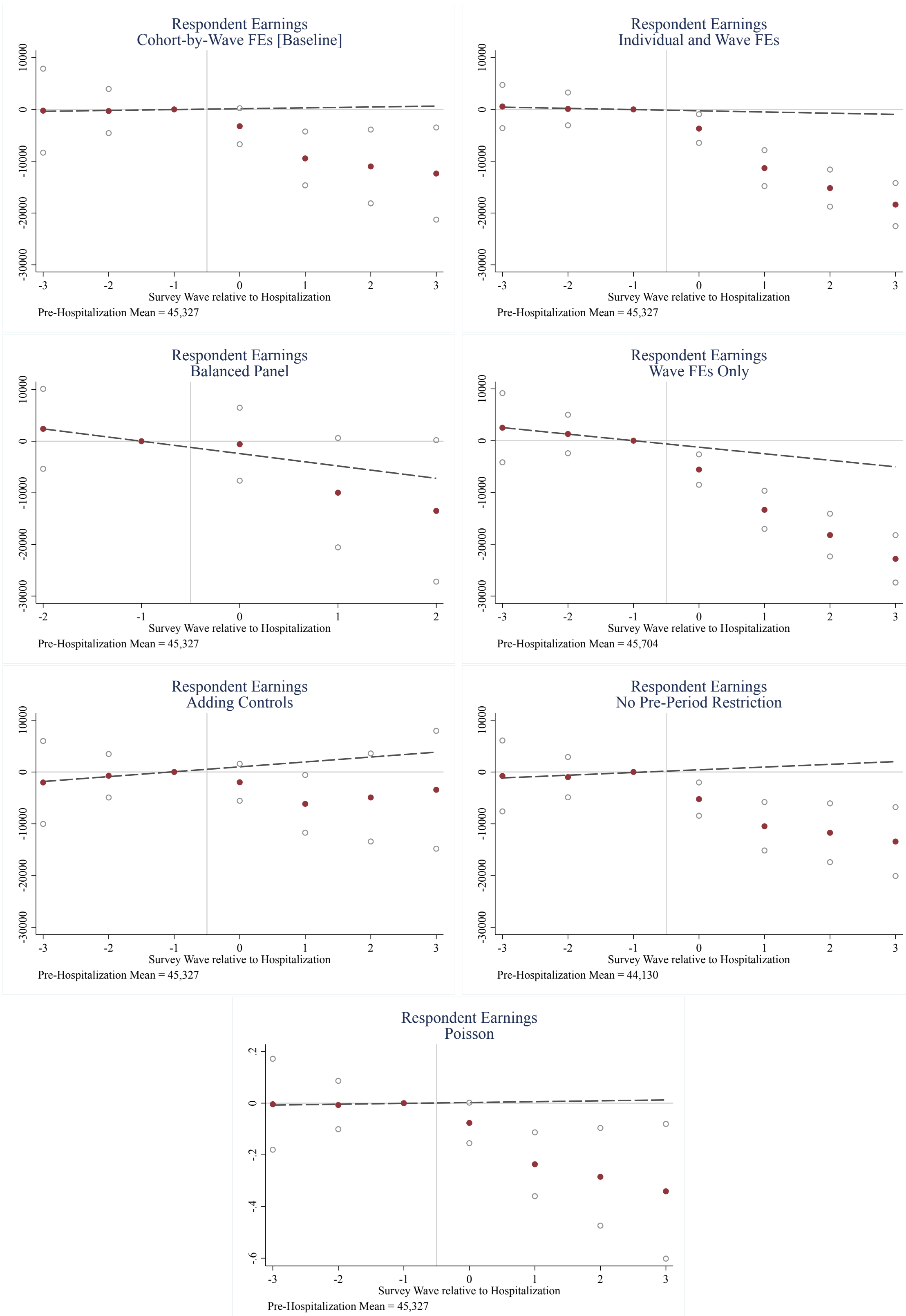
Notes: The baseline sample is the non-elderly insured (see Table 1, column 1). The top left figure displays the baseline specification. All other figures indicate specific departures from the baseline as follows: replacing cohort-by-wave fixed effects with individual and wave fixed effects; limiting to a balanced panel of individuals with non-missing data for the two years before and four years after their hospitalization; replacing cohort-by-wave fixed effects with wave fixed effects; adding a cubic in age, male dummy, race dummies and education dummies along with the cohort-by-wave fixed effects; dropping the requirement of a pre-period survey wave observation reporting no hospitalization; and, estimating equation (4) with a Poisson, rather than a linear, regression.

Appendix Figure 3. Impact of Hospitalization on Working Part or Full-Time for the Insured, Robustness



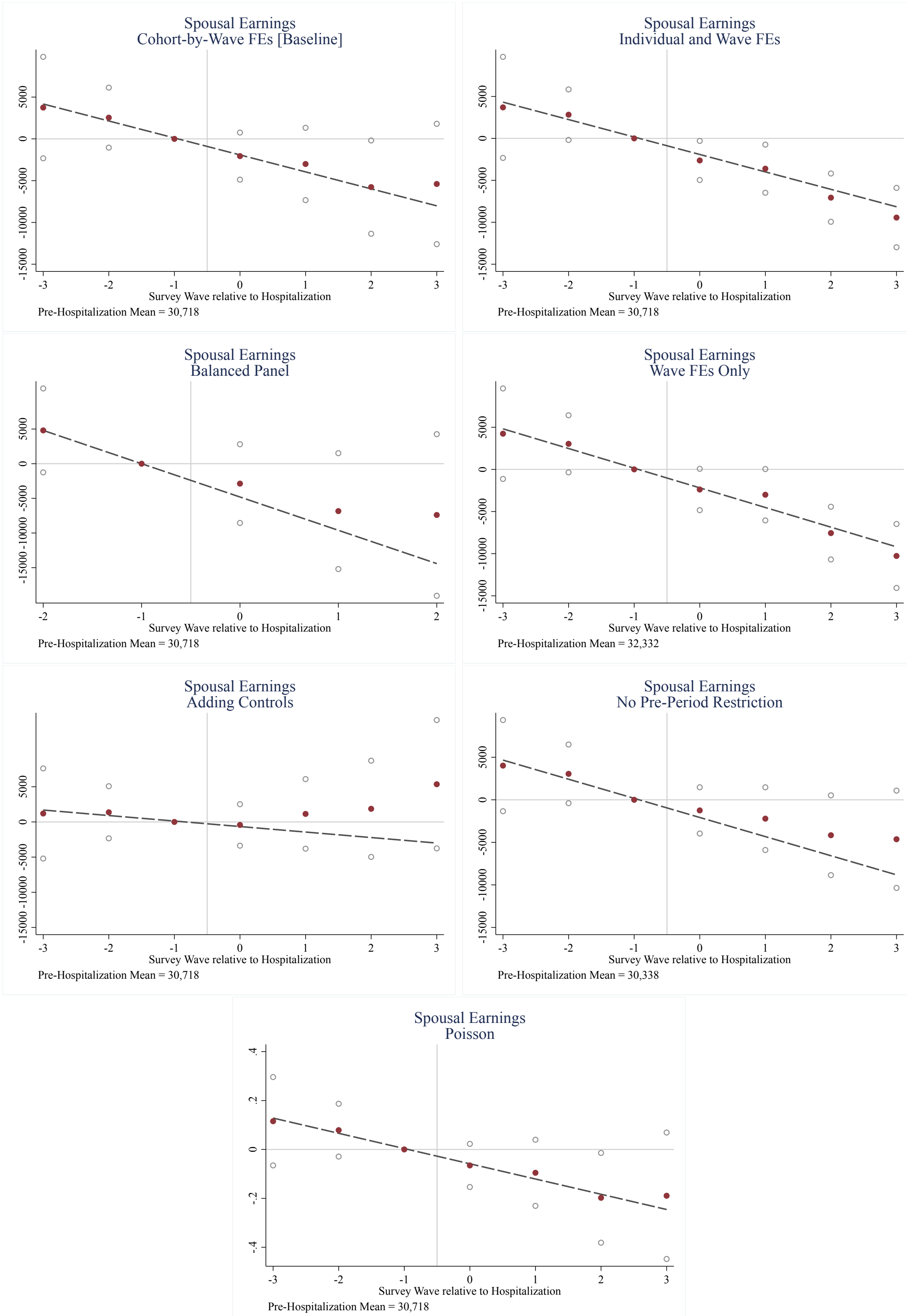
Notes: See notes to Appendix Figure 2.

Appendix Figure 4. Impact of Hospitalization on Respondent Earnings for the Insured, Robustness



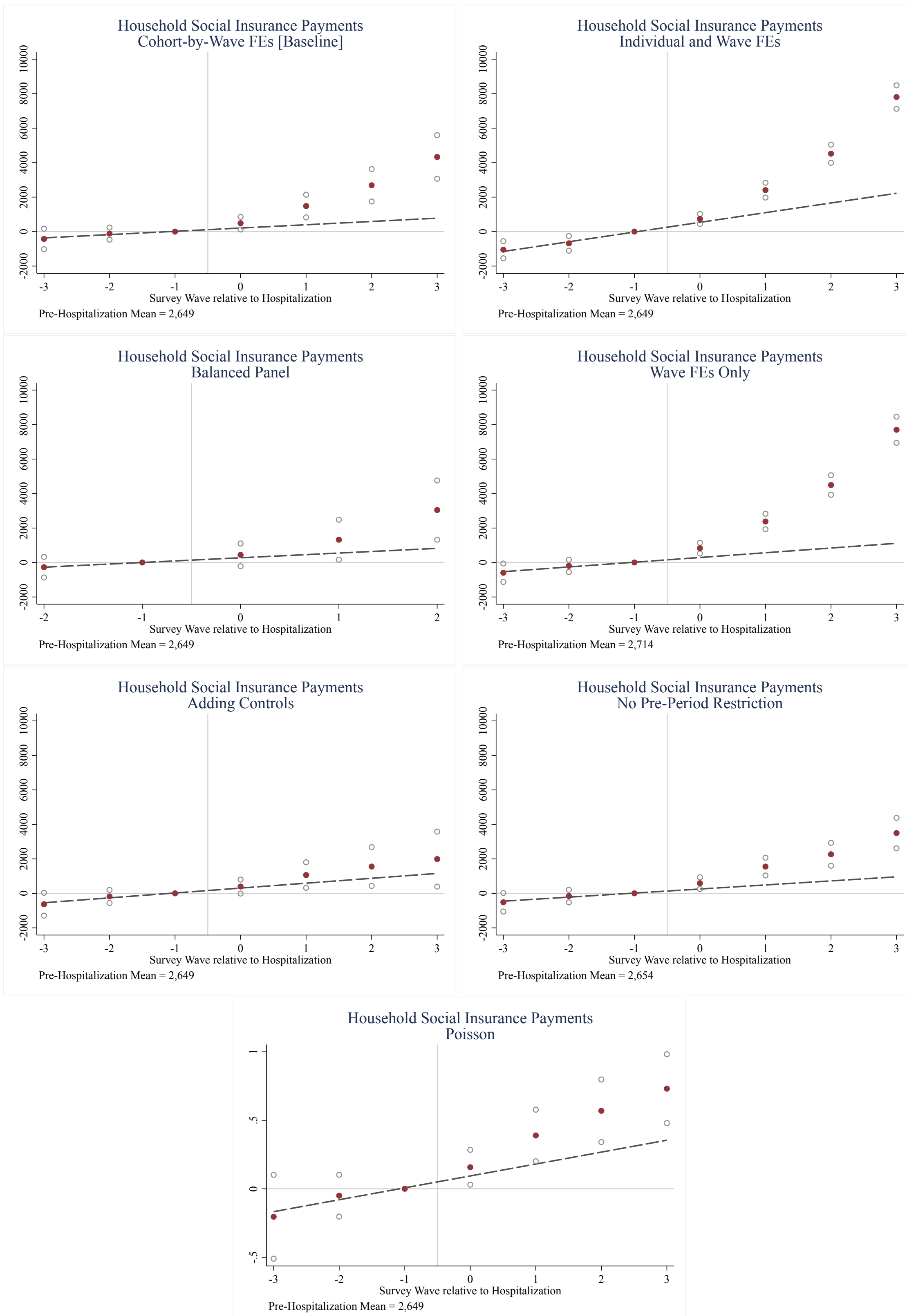
Notes: See notes to Appendix Figure 2.

Appendix Figure 5. Impact of Hospitalization on Spousal Earnings for the Insured, Robustness



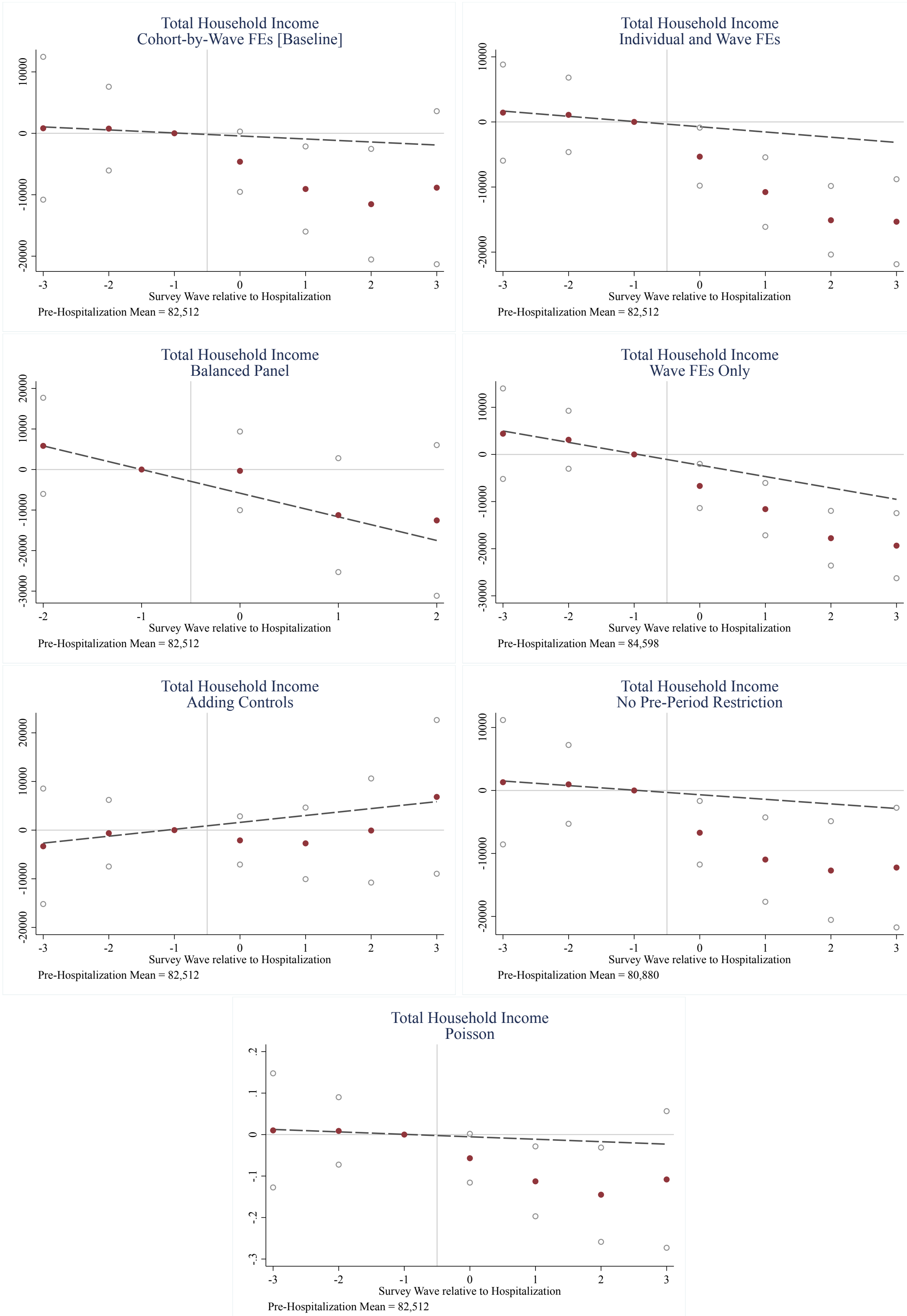
Notes: See notes to Appendix Figure 2.

Appendix Figure 6. Impact of Hospitalization on Social Insurance Payments for the Insured, Robustness



Notes: See notes to Appendix Figure 2.

Appendix Figure 7. Impact of Hospitalization on Total Household Income for the Insured, Robustness



Notes: See notes to Appendix Figure 2.

G.1.5 Additional Results

Appendix Table 9. Impact of Hospitalization on Earnings and Income for the Insured

	Respondent Earnings	Spousal Earnings	Respondent Social Insurance Payments	Household Social Insurance Payments	Respondent Pension and Annuity Income[a]	Household Pension and Annuity Income[a]	Total Household Income	Log(Total Household Income)	Household Capital and Business Income	Other Household Income	Total Household Income Plus Business, Capital, and Other Income	Log(Total Household Income Plus Business, Capital, and Other Income)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
12-month effect	-6,445	-444	612	503	392	655	-8,443	-.21	-367	1,371	-10,599	-.24
	(4,024)	(3,851)	(260)	(379)	(649)	(911)	(6,857)	(.16)	(2,690)	(2,783)	(8,232)	(.11)
	[.11]	[.91]	[.019]	[.18]	[.55]	[.47]	[.22]	[.18]	[.89]	[.62]	[.2]	[.031]
Effect at 36 months	-11,071	1,588	1,200	1,261	1,109	1,402	-7,890	-.15	-839	-1,399	-11,647	-.18
	(3,475)	(3,110)	(262)	(411)	(550)	(746)	(5,527)	(.11)	(1,605)	(1,878)	(6,597)	(.087)
	[.0014]	[.61]	[<.001]	[.0022]	[.044]	[.06]	[.15]	[.19]	[.6]	[.46]	[.077]	[.042]
Average annual effect over 36 months	-8,753	572	905	881	750	1,028	-8,161	-.18	-603	-14.6	-11,116	-.21
	(3,415)	(3,114)	(211)	(338)	(506)	(670)	(5,709)	(.12)	(1,889)	(2,070)	(6,789)	(.081)
	[.01]	[.85]	[<.001]	[.009]	[.14]	[.12]	[.15]	[.12]	[.75]	[.99]	[.1]	[.0095]
Pre-hospitalization mean	45,327	30,718	1,342	2,649	1,649	3,048	82,512	10.8	6,357	4,304	94,660	11
Number of Individuals	2,732	2,732	2,732	2,732	2,732	2,732	2,732	2,732	2,732	2,732	2,732	2,732
Number of Observations	13,286	13,286	13,286	13,286	13,286	13,286	13,286	13,286	13,286	13,286	13,286	13,286

Notes: Sample is the non-elderly insured (see Table 1, column 1) in the HRS. All columns report effects based on OLS estimates of equation (4). Standard errors (clustered on the individual) are in parentheses and p-values are in brackets. All estimates are weighted using survey weights.

[a] Pension and annuity income can be reported for the previous calendar year like all other outcomes in this table) or scaled up to an annual amount based on the prior month. For consistency, we treat the timing as if all respondents reported it for the prior calendar year.

Appendix Table 10. Impact of Hospitalization on Labor Force Status for the Insured

	Annual Hours	Log(Hourly Wage)[a]	Working Part or Full-Time	Working Full-Time	Working Part-Time	Unemployed	Partly Retired	Retired	Disabled	Not in Labor Force	Work Limited by Health
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
12-month effect	-214	.0029	-8.9	-8.3	-.63	-.38	.33	5.6	2.9	.41	9.5
	(42)	(.05)	(1.8)	(1.9)	(1.3)	(1.3)	(.94)	(1.5)	(1.1)	(.95)	(2.1)
	[<.001]	[.95]	[<.001]	[<.001]	[.63]	[.77]	[.73]	[<.001]	[.0094]	[.67]	[<.001]
Effect at 36 months	-228	.021	-11	-10	-.93	-1.8	1	10	.26	1.5	3.4
	(54)	(.059)	(2.3)	(2.5)	(1.7)	(1.4)	(1.3)	(1.8)	(1.2)	(1.2)	(2.5)
	[<.001]	[.72]	[<.001]	[<.001]	[.58]	[.2]	[.43]	[<.001]	[.82]	[.21]	[.18]
Pre-hospitalization mean	1,637	3	74	67	7.6	3	3.1	9.8	3.9	6.2	22
Number of Individuals	2,732	2,201	2,732	2,732	2,732	2,732	2,732	2,732	2,732	2,732	2,732
Number of Observations	13,109	7,641	13,286	13,286	13,286	13,286	13,286	13,286	13,286	13,286	13,286

Notes: Sample is the non-elderly insured (see Table 1, column 1) in the HRS. All columns report effects based on OLS estimates of equation (4). Standard errors (clustered on the individual) are in parentheses and p-values are in brackets. Columns 2 through 8 are the mutually exclusive and exhaustive values taken by the rWlbrf variable in the RAND HRS extract. All outcomes are contemporaneous. All estimates are weighted using survey weights.

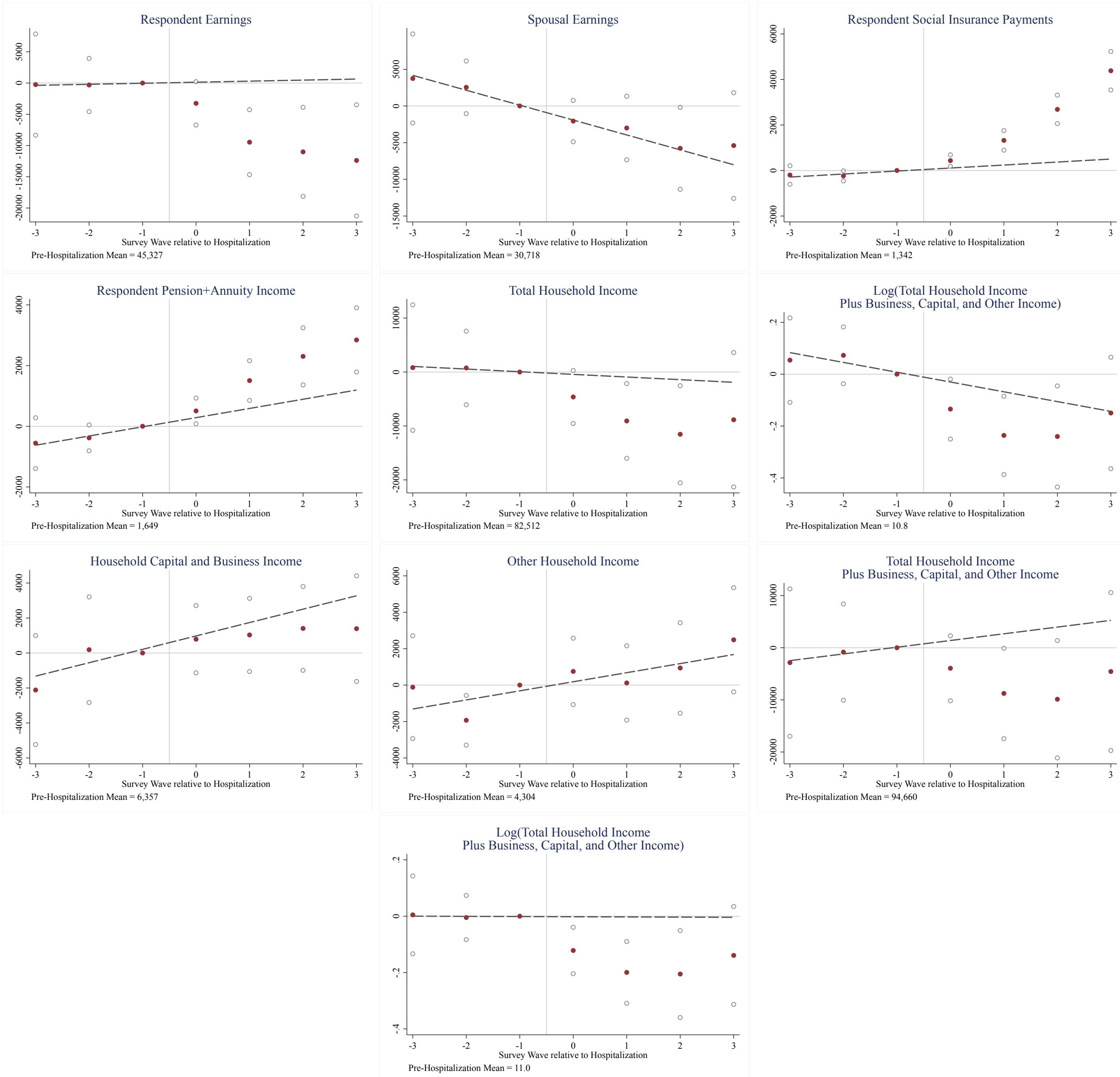
[a] Log(Hourly Wage) is conditional on positive hours.

Appendix Table 11. Impact of Hospitalization on Social Insurance Payments for the Insured

	Household UI Income		Total Household Social Insurance Payments		Social Security Income (All Programs)		Household SSDI		Household SSI		Household SS Retirement Income	
	Any	Amount	Any	Amount	Any	Amount	Any	Amount	Any	Amount	Any	Amount
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A. Non-Elderly Insured (Ages 50 to 59)												
12-month effect	-3.5	-131	4.3	503	5.7	312	3.2	6.5	-.62	-7.1	2.7	286
	(2.4)	(134)	(3.2)	(379)	(2)	(272)	(1.6)	(197)	(1.1)	(74)	(1.6)	(191)
	[.15]	[.33]	[.19]	[.18]	[.0039]	[.25]	[.048]	[.97]	[.56]	[.92]	[.087]	[.13]
Effect at 36 months	-1.4	-56	7	1,261	8.7	1,306	2.1	434	-.45	-19	8.3	912
	(1.8)	(91)	(2.6)	(411)	(2)	(302)	(1.5)	(204)	(.86)	(64)	(1.6)	(201)
	[.44]	[.54]	[.0079]	[.0022]	[<.001]	[<.001]	[.16]	[.033]	[.6]	[.77]	[<.001]	[<.001]
Average annual effect over 36 months	-2.4	-94	5.6	881	7.2	808	2.7	220	-.54	-13	5.5	598
	(1.8)	(93)	(2.5)	(338)	(1.7)	(242)	(1.3)	(168)	(.83)	(60)	(1.3)	(159)
	[.18]	[.31]	[.027]	[.009]	[<.001]	[<.001]	[.046]	[.19]	[.52]	[.83]	[<.001]	[<.001]
Pre-hospitalization mean	7.7	322	26	2,649	13	1,592	6.6	742	3.4	172	5.1	553
Number of Individuals	2,732	2,732	2,732	2,732	2,732	2,732	2,732	2,732	2,732	2,732	2,732	2,732
Number of Observations	13,286	13,286	13,286	13,286	13,286	13,286	13,286	13,286	13,286	13,286	13,286	13,286
Panel B. Insured Ages 60 to 64												
12-month effect	-1.3	-275	21	3,940	2	250	1.3	85	24	3,467	26	4,215
	(2.5)	(183)	(4.3)	(715)	(1.8)	(210)	(1.3)	(82)	(3.8)	(517)	(3.7)	(539)
	[.59]	[.13]	[<.001]	[<.001]	[.26]	[.23]	[.33]	[.3]	[<.001]	[<.001]	[<.001]	[<.001]
Effect at 36 months	-3.3	-125	20	5,779	1.3	219	-1.4	-47	26	5,512	24	5,947
	(1.9)	(145)	(3.1)	(609)	(1.6)	(189)	(1)	(71)	(3)	(512)	(3)	(521)
	[.081]	[.39]	[<.001]	[<.001]	[.41]	[.25]	[.17]	[.51]	[<.001]	[<.001]	[<.001]	[<.001]
Average annual effect over 36 months	-2.3	-200	21	4,856	1.6	234	-.07	19	25	4,487	25	5,078
	(1.9)	(147)	(3.1)	(542)	(1.4)	(158)	(.96)	(49)	(2.8)	(382)	(2.7)	(407)
	[.22]	[.17]	[<.001]	[<.001]	[.24]	[.14]	[.94]	[.7]	[<.001]	[<.001]	[<.001]	[<.001]
Pre-hospitalization mean	6.5	305	42	4,895	5.3	531	3.1	135	25	2,523	31	3,515
Number of Individuals	1,627	1,627	1,627	1,627	1,627	1,627	1,627	1,627	1,627	1,627	1,627	1,627
Number of Observations	9,296	9,296	9,296	9,296	9,296	9,296	9,296	9,296	9,296	9,296	9,296	9,296

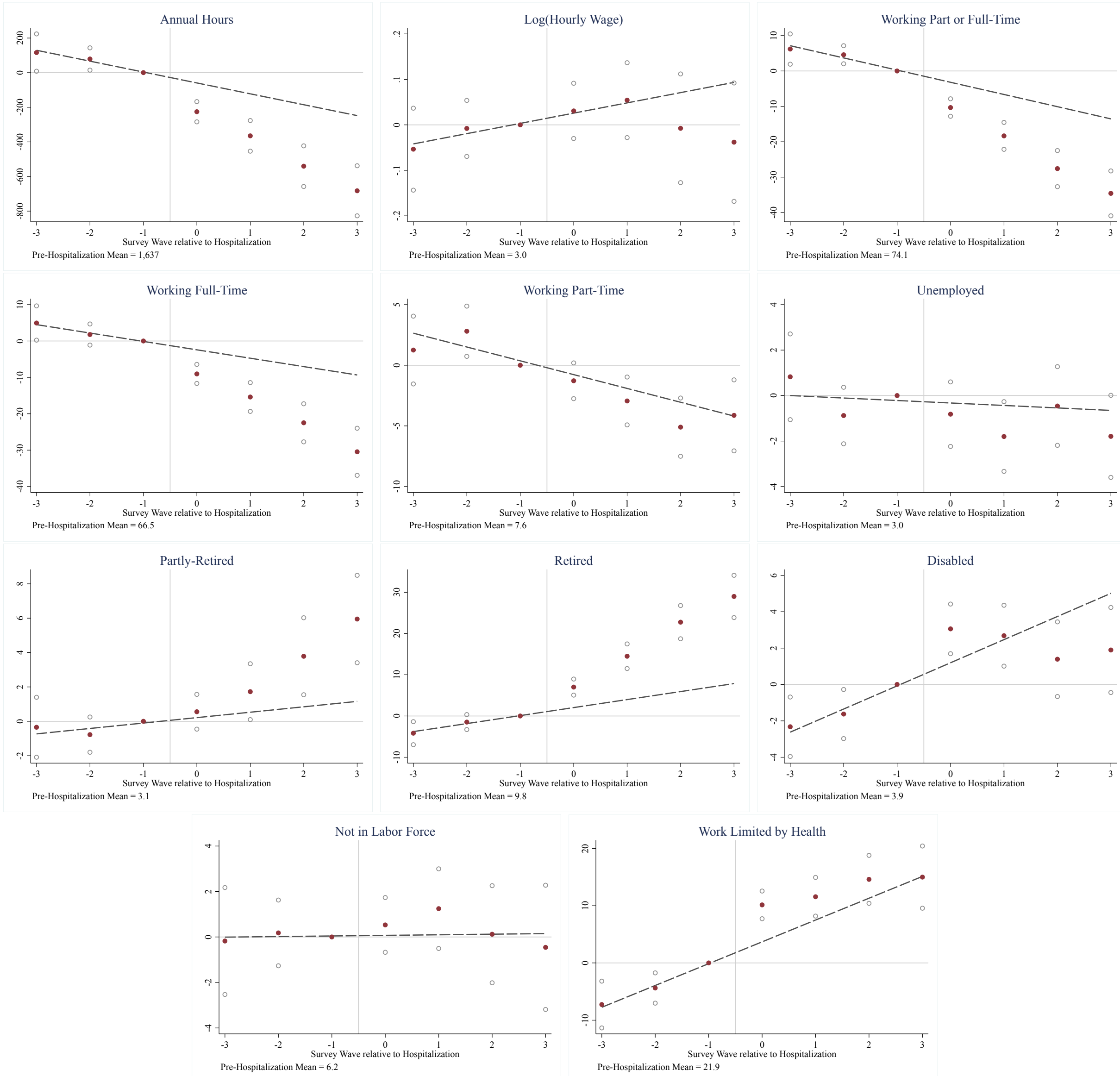
Notes: Samples are the non-elderly insured (see Table 1, column 1) and the insured ages 60 to 64 (see Appendix Table 4, column 6). All columns report effects based on OLS estimates of equation (4). Standard errors (clustered on the individual) are in parentheses and p-values are in brackets. All outcomes are measured over the prior calender year. All estimates are weighted using survey weights.

Appendix Figure 8. Impact of Hospitalization on Earnings and Income for the Insured



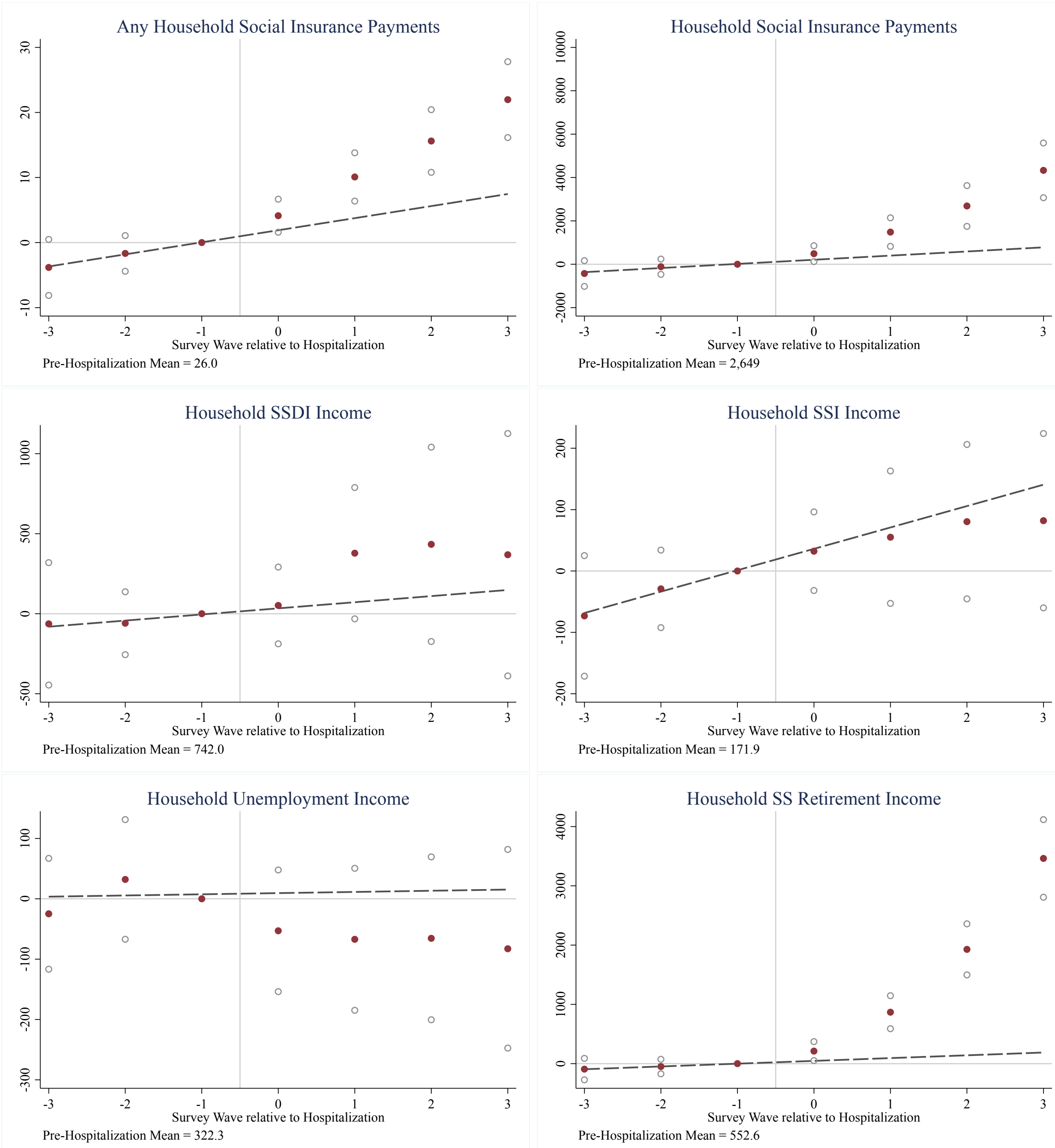
Notes: The sample is the non-elderly insured (see Table 1, column 1). The points in each figure represent the estimated effects of event time (i.e., the μ_r's from the non-parametric event study in equation (3)), with the survey wave reporting the hospitalization normalized to zero. Survey waves are biannual; we assume the hospitalization occurs halfway between survey waves (12 months prior to survey wave zero) on average. The hollow circles present the 95% confidence intervals. The dashed line represents the estimated pre-admission linear relationship between outcome and event time from the parametric event study in equation (4) with the level normalized to match the non-parametric estimates. All estimates are weighted using survey weights.

Appendix Figure 9. Impact of Hospitalization on Labor Force Status for the Insured



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 μ_r's from the non-parametric event study in equation (3)), with the survey wave reporting the hospitalization normalized to zero. Survey waves are biannual; we assume the hospitalization occurs halfway between survey waves (12 months prior to survey wave zero) on average. The hollow circles present the 95% confidence intervals. The dashed line represents the estimated pre-admission linear relationship between outcome and event time from the parametric event study in equation (4) with the level normalized to match the non-parametric estimates. All estimates are weighted using survey weights. Log(Hourly Wage) is conditional on positive hours worked.

Appendix Figure 10. Impact of Hospitalization on Social Insurance Payments for the Insured



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 μ_r's from the non-parametric event study in equation (3)), with the survey wave reporting the hospitalization normalized to zero. Survey waves are biannual; we assume the hospitalization occurs halfway between survey waves (12 months prior to survey wave zero) on average. The hollow circles present the 95% confidence intervals. The dashed line represents the estimated pre-admission linear relationship between outcome and event time from the parametric event study in equation (4) with the level normalized to match the non-parametric estimates. All estimates are weighted using survey weights.

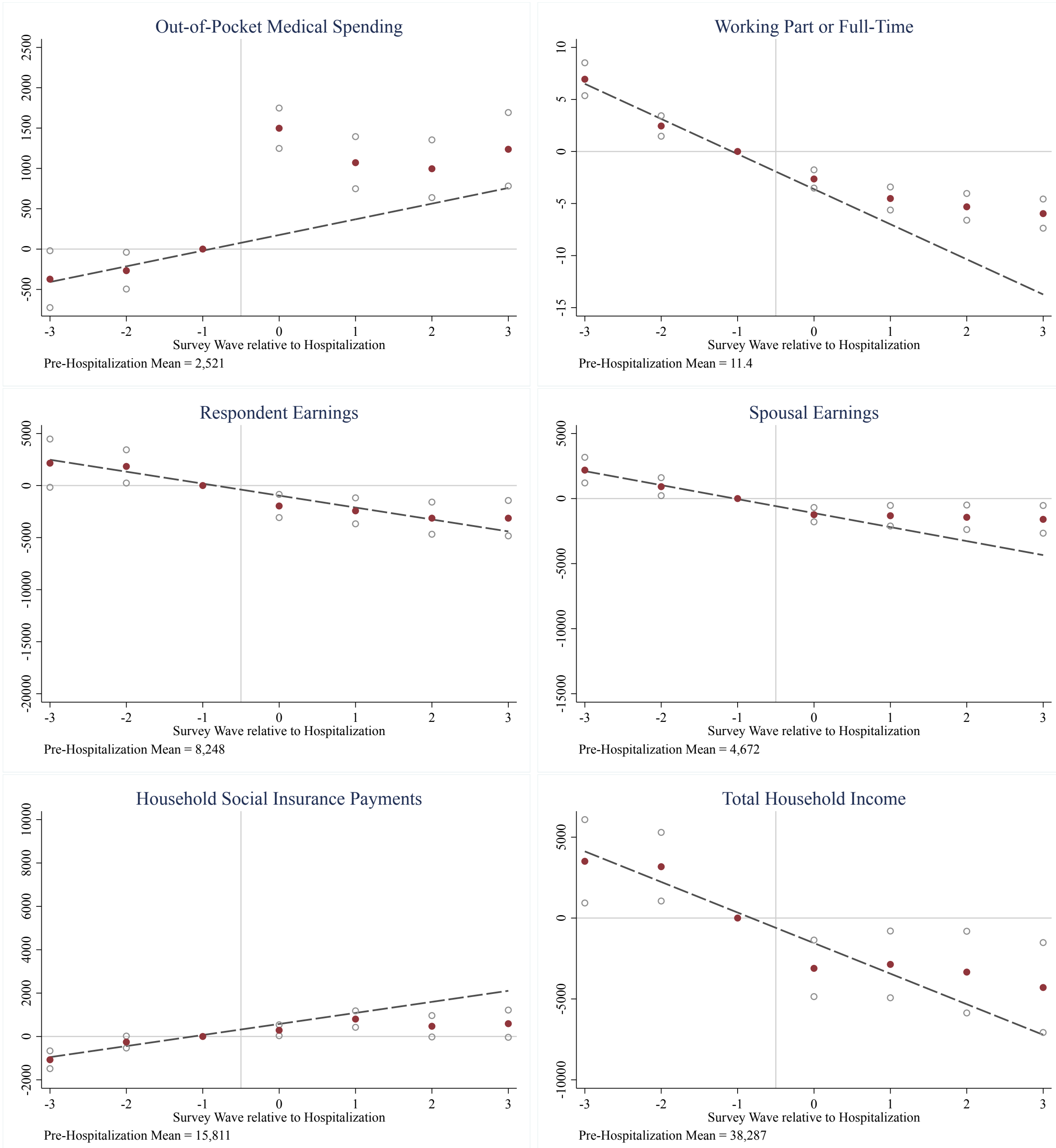
G.1.6 Heterogeneity Analysis

Appendix Table 12. Impact of Hospitalization, by Wealth and Marital Status

Specification	[Baseline]	Lowest Quartile Wealth	Highest Quartile Wealth	Below Median Wealth	Above Median Wealth	Married	Single
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			Panel A: Out-of-Pocket Medical Spending				
12-month effect	3,275	2,431	4,003	2,806	3,660	3,433	2,454
	(373)	(731)	(791)	(483)	(549)	(422)	(854)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[.0041]
Average annual effect over 36 months	1,429	1,553	1,412	1,562	1,350	1,450	1,285
	(202)	(493)	(403)	(296)	(271)	(233)	(393)
	[<.001]	[.0016]	[<.001]	[<.001]	[<.001]	[<.001]	[.0011]
Pre-hospitalization mean	2,133	2,150	2,245	2,027	2,222	2,086	2,295
			Panel B: Working Part or Full-Time				
12-month effect	-8.9	-13	-11	-10	-7.9	-8	-12
	(1.8)	(3.7)	(3.5)	(2.5)	(2.6)	(2)	(4.1)
	[<.001]	[<.001]	[.0022]	[<.001]	[.0022]	[<.001]	[.0036]
Average annual effect over 36 months	-10	-14	-12	-10	-10	-8.8	-14
	(1.9)	(3.8)	(3.6)	(2.6)	(2.6)	(2.1)	(4.2)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
Pre-hospitalization mean	74	66	76	72	76	75	70
			Panel C: Respondent Earnings				
12-month effect	-6,445	-2,518	-20,464	-1,416	-11,607	-5,728	-9,029
	(4,024)	(3,387)	(12,339)	(2,538)	(6,954)	(4,660)	(8,138)
	[.11]	[.46]	[.097]	[.58]	[.095]	[.22]	[.27]
Average annual effect over 36 months	-8,753	-4,126	-24,248	-2,723	-14,705	-8,250	-10,217
	(3,415)	(2,581)	(10,534)	(2,038)	(5,910)	(4,034)	(6,844)
	[.01]	[.11]	[.021]	[.18]	[.013]	[.041]	[.14]
Pre-hospitalization mean	45,327	24,141	72,804	29,425	58,635	47,271	38,667
			Panel D: Spousal Earnings				
12-month effect	-444	6,392	-10,080	5,780	-5,686	-2,334	9,400
	(3,851)	(3,582)	(11,077)	(2,771)	(6,538)	(4,868)	(2,305)
	[.91]	[.074]	[.36]	[.037]	[.38]	[.63]	[<.001]
Average annual effect over 36 months	572	3,556	-8,260	3,737	-2,456	-352	7,144
	(3,114)	(2,392)	(8,693)	(2,169)	(5,303)	(3,951)	(1,679)
	[.85]	[.14]	[.34]	[.085]	[.64]	[.93]	[<.001]
Pre-hospitalization mean	30,718	12,143	51,317	16,603	42,531	39,683	0
			Panel E: Household Social Insurance Payments				
12-month effect	503	162	1,279	146	841	324	1,264
	(379)	(734)	(686)	(549)	(521)	(444)	(702)
	[.18]	[.83]	[.062]	[.79]	[.11]	[.47]	[.072]
Average annual effect over 36 months	881	799	1,761	410	1,299	708	1,566
	(338)	(669)	(587)	(515)	(454)	(398)	(602)
	[.009]	[.23]	[.0027]	[.43]	[.0042]	[.075]	[.0093]
Pre-hospitalization mean	2,649	3,736	1,948	3,348	2,065	2,832	2,023
			Panel F: Total Household Income				
12-month effect	-8,443	4,386	-37,897	4,929	-20,559	-9,028	-3,710
	(6,857)	(4,449)	(21,002)	(3,364)	(11,995)	(8,032)	(13,566)
	[.22]	[.32]	[.071]	[.14]	[.087]	[.26]	[.78]
Average annual effect over 36 months	-8,161	475	-36,468	2,230	-18,223	-8,242	-4,686
	(5,709)	(3,277)	(17,515)	(2,641)	(10,010)	(6,826)	(10,614)
	[.15]	[.88]	[.037]	[.4]	[.069]	[.23]	[.66]
Pre-hospitalization mean	82,512	40,962	133,892	51,051	108,844	93,895	43,512
Number of Individuals	2,732	683	681	1,366	1,366	2,129	603
Number of Observations	13,286	3,075	3,480	6,374	6,912	10,446	2,840

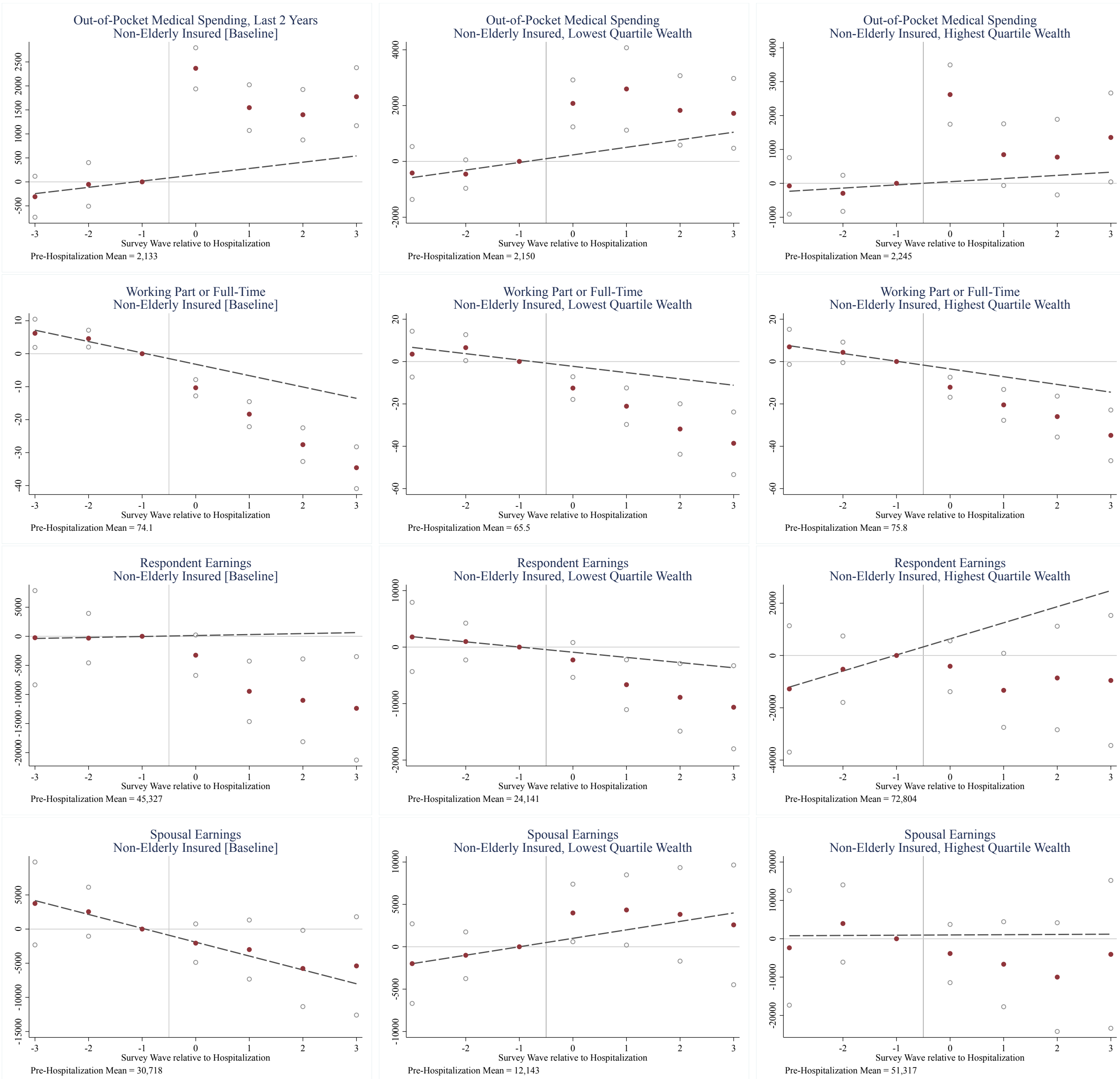
Notes: Samples are the non-elderly insured (see Table 1, column 1), additionally split by household net worth and marital status as measured in the survey wave preceding the hospitalization. All columns report effects based on OLS estimates of equation (4). Standard errors (clustered on the individual) are in parentheses and p-values are in brackets. All estimates are weighted using survey weights.

Appendix Figure 11. Impact of Hospitalization for the Elderly



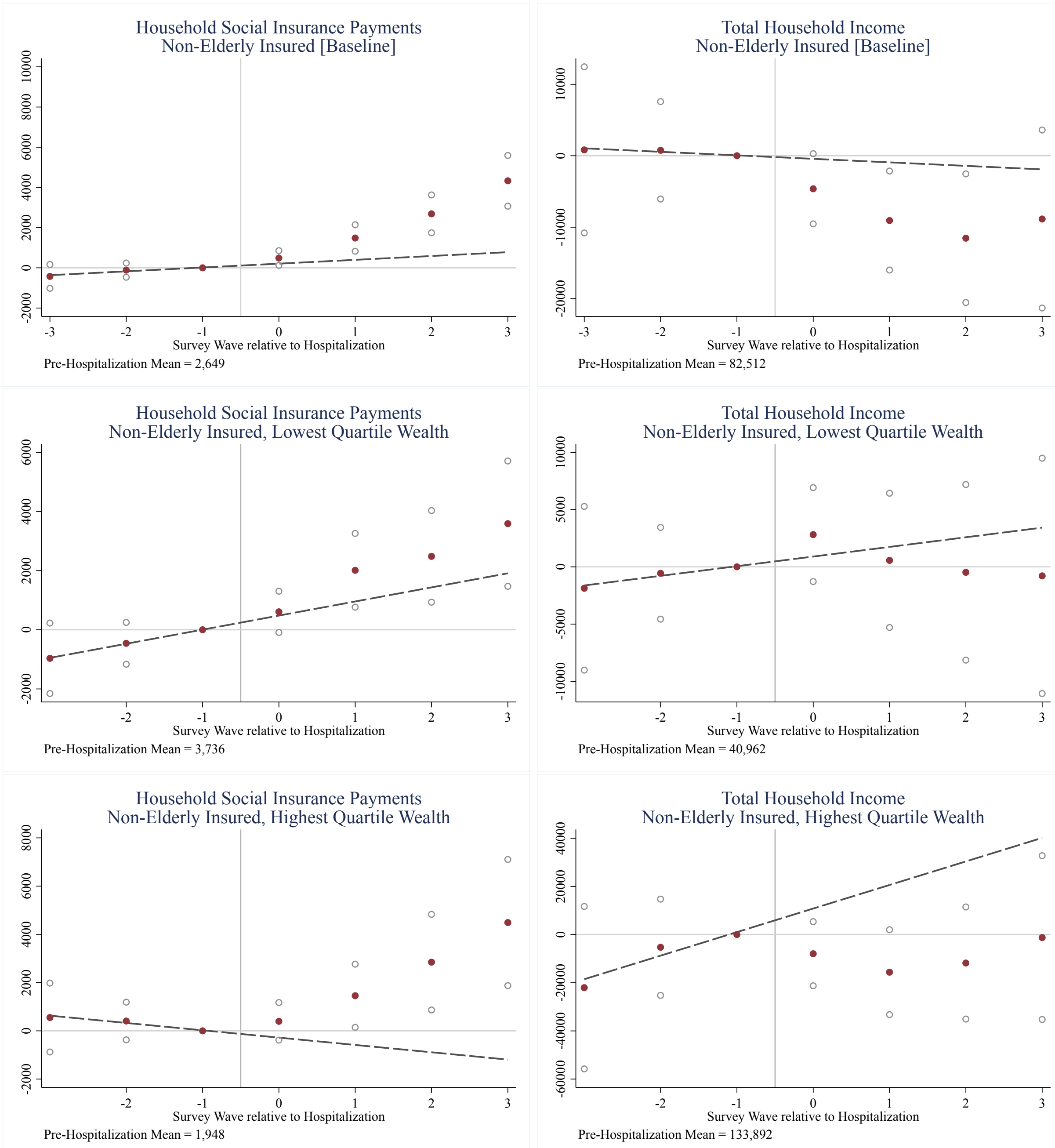
Notes: The sample is the elderly (see Table 1, column 3). The points in each figure represent the estimated effects of event time (i.e., the μ_r's from the non-parametric event study in equation (3)), with the survey wave reporting the hospitalization normalized to zero. Survey waves are biannual; we assume the hospitalization occurs halfway between survey waves (12 months prior to survey wave zero) on average. The hollow circles present the 95% confidence intervals. The dashed line represents the estimated pre-admission linear relationship between outcome and event time from the parametric event study in equation (4) with the level normalized to match the non-parametric estimates. All estimates are weighted using survey weights.

Appendix Figure 12. Impact of Hospitalization, by Pre-Hospitalization Wealth



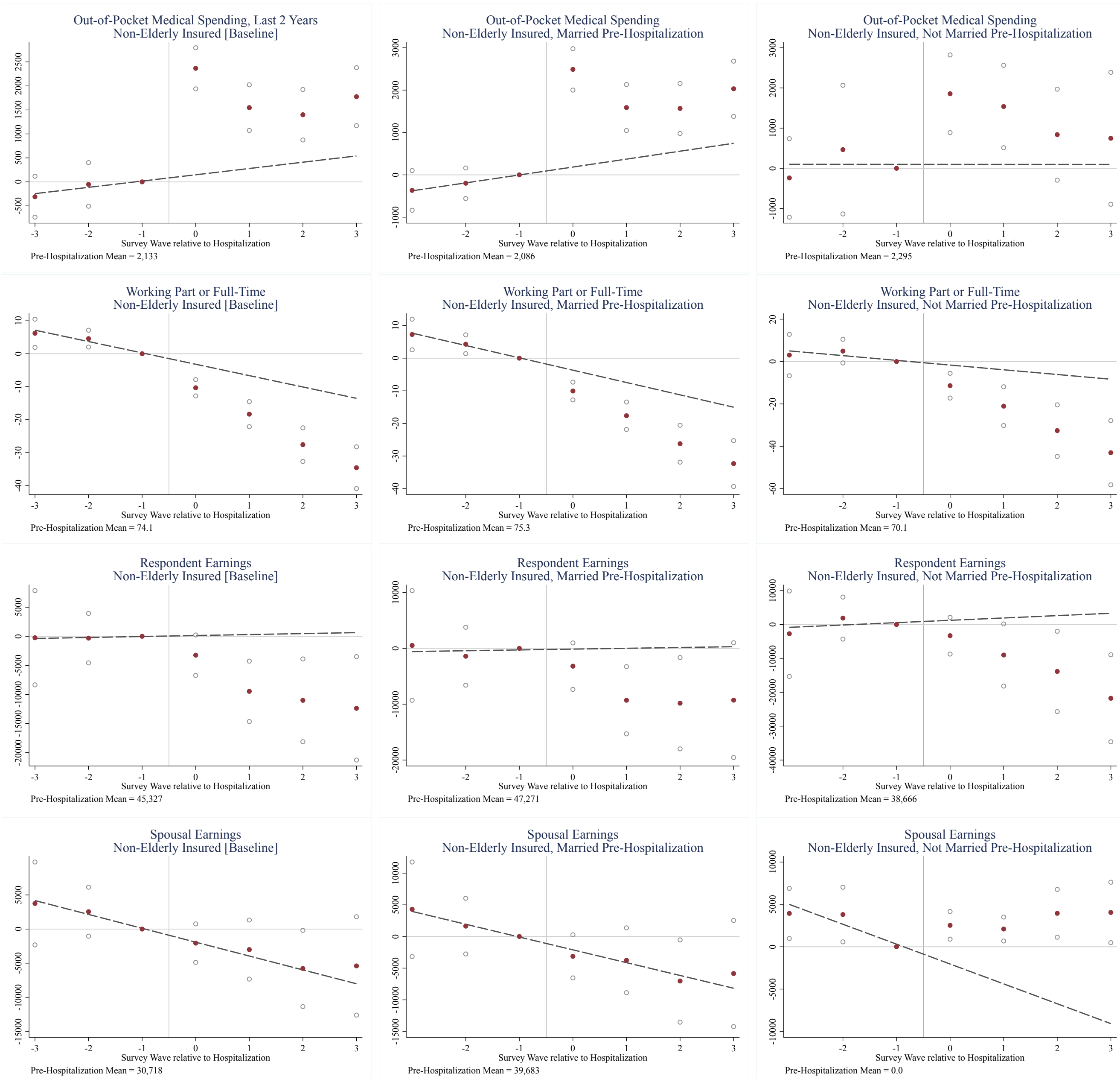
Notes: The sample is the non-elderly insured (see Table 1, column 1), additionally split into the bottom and top quartiles of the household net value of total wealth as measured in the survey wave preceding the hospitalization. The points in each figure represent the estimated effects of event time (i.e., the μ_r's from the non-parametric event study in equation (3)), with the survey wave reporting the hospitalization normalized to zero. Survey waves are biannual; we assume the hospitalization occurs halfway between survey waves (12 months prior to survey wave zero) on average. The hollow circles present the 95% confidence intervals. The dashed line represents the estimated pre-admission linear relationship between outcome and event time from the parametric event study in equation (4) with the level normalized to match the non-parametric estimates. All estimates are weighted using survey weights.

Appendix Figure 13. Impact of Hospitalization, by Pre-Hospitalization Wealth



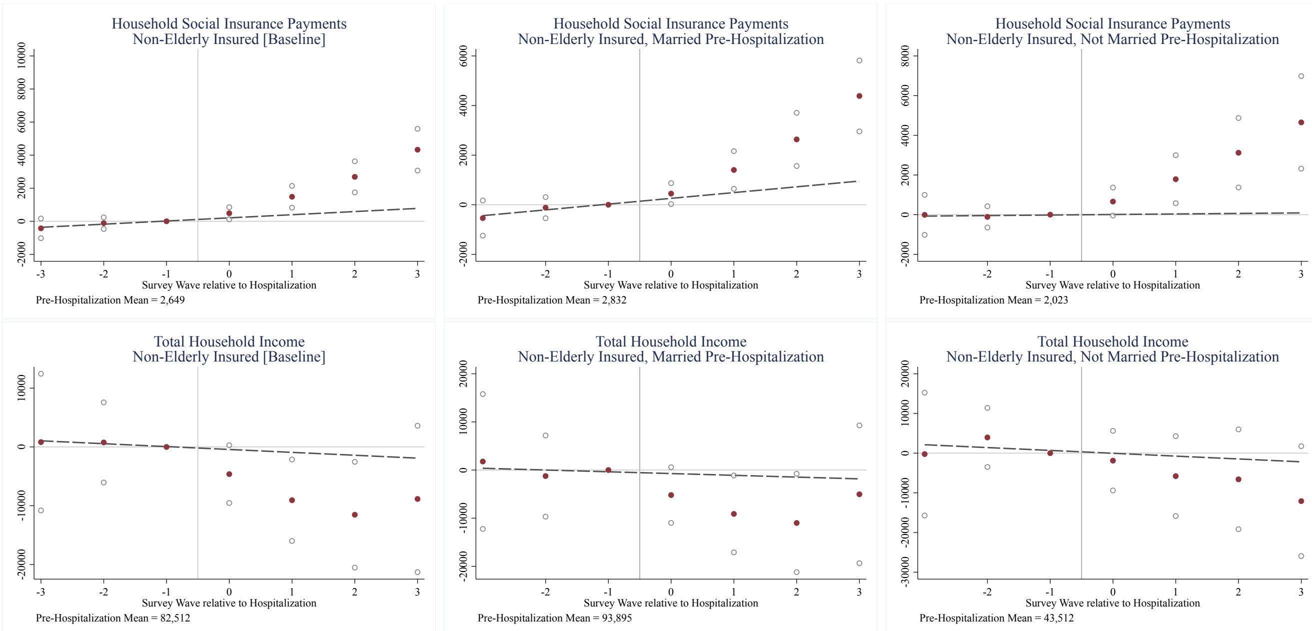
Notes: The sample is the non-elderly insured (see Table 1, column 1), additionally split into the bottom and top quartiles of the household net value of total wealth as measured in the survey wave preceding the hospitalization. The points in each figure represent the estimated effects of event time (i.e., the μ_r's from the non-parametric event study in equation (3)), with the survey wave reporting the hospitalization normalized to zero. Survey waves are biannual; we assume the hospitalization occurs halfway between survey waves (12 months prior to survey wave zero) on average. The hollow circles present the 95% confidence intervals. The dashed line represents the estimated pre-admission linear relationship between outcome and event time from the parametric event study in equation (4) with the level normalized to match the non-parametric estimates. All estimates are weighted using survey weights.

Appendix Figure 14. Impact of Hospitalization, by Pre-Hospitalization Marital Status



Notes: The sample is the non-elderly insured (see Table 1, column 1), additionally split by the pre-hospitalization marital status of the individual reporting a hospitalization in the subsequent survey wave. The points in each figure represent the estimated effects of event time (i.e., the μ_r's from the non-parametric event study in equation (3)), with the survey wave reporting the hospitalization normalized to zero. Survey waves are biannual; we assume the hospitalization occurs halfway between survey waves (12 months prior to survey wave zero) on average. The hollow circles present the 95% confidence intervals. The dashed line represents the estimated pre-admission linear relationship between outcome and event time from the parametric event study in equation (4) with the level normalized to match the non-parametric estimates. All estimates are weighted using survey weights.

Appendix Figure 15. Impact of Hospitalization, by Pre-Hospitalization Marital Status



Notes: The sample is the non-elderly insured (see Table 1, column 1), additionally split by the pre-hospitalization marital status of the individual reporting a hospitalization in the subsequent survey wave. The points in each figure represent the estimated effects of event time (i.e., the μ_r's from the non-parametric event study in equation (3)), with the survey wave reporting the hospitalization normalized to zero. Survey waves are biannual; we assume the hospitalization occurs halfway between survey waves (12 months prior to survey wave zero) on average. The hollow circles present the 95% confidence intervals. The dashed line represents the estimated pre-admission linear relationship between outcome and event time from the parametric event study in equation (4) with the level normalized to match the non-parametric estimates. All estimates are weighted using survey weights.

G.2 Hospital Discharge Data Linked to Credit Reports

G.2.1 Summary Statistics

Appendix Table 13. Sample Composition and Demographic Summary Statistics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Sample	Non-Elderly Insured			Non-Elderly Uninsured			Elderly			Ages 60-70 Through ED		
Drop if not in TU	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Drop if not first hospitalization in three years	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
			[Baseline]			[Baseline]			[Baseline]			[Baseline]
Panel B: Demographics												
Age at Admission	48.6	48.7	48.5	45.0	45.2	45.1	78.5	78.0	77.1	65.0	65.0	64.8
Male	44.9	44.3	44.9	62.1	61.0	61.7	43.7	45.9	45.8	50.1	50.5	51.2
Median Income for Zip Code of Residence	63,550	64,454	66,766	58,099	58,613	58,957	64,225	65,479	66,590	60,930	61,991	64,640
Race/Ethnicity												
Hispanic	20.0	18.7	18.1	26.4	24.2	25.6	14.0	11.6	10.6	19.7	17.6	16.6
Asian	6.5	5.8	6.8	4.2	3.7	4.4	8.1	4.9	5.4	7.6	6.0	7.2
Black	11.1	10.9	7.9	12.6	12.8	11.2	6.5	6.7	5.2	11.0	11.4	8.2
White	58.8	61.0	63.0	53.0	55.7	54.7	68.7	74.5	75.9	58.6	62.2	64.4
Other Race	3.7	3.6	4.2	3.8	3.6	4.1	2.7	2.3	2.9	3.2	2.8	3.6
Panel C: Index Hospitalization												
Length of Stay	5.4	5.1	4.1	5.0	4.9	4.8	7.1	6.7	5.9	6.9	6.6	5.1
Hospital List Charges	48,304	47,688	45,565	36,363	35,837	37,729	57,429	57,181	56,638	59,349	58,661	49,626
	(165,921)	(159,657)	(189,815)	(76,229)	(75,858)	(83,056)	(142,072)	(138,777)	(162,249)	(150,171)	(147,911)	(129,897)
Medicaid	29.1	24.8	13.7	0.5	0.5	0.4	3.9	1.3	1.0	14.4	10.4	6.2
Private	70.7	75.1	86.2	0.3	0.3	0.3	6.9	7.9	9.4	26.6	30.3	40.5
Hospital Non Profit	70.0	71.8	74.4	58.9	59.6	60.5	72.7	75.0	76.3	68.9	71.1	71.8
Hospital For Profit	17.7	17.3	16.3	12.5	12.5	12.6	18.2	16.6	15.3	17.2	16.5	15.8
Hospital Public	12.3	11.0	9.3	28.5	27.9	26.9	9.1	8.3	8.3	13.9	12.4	12.4
Admitted through Emergency Department	53.0	52.0	48.0	80.7	79.9	79.8	66.3	64.6	58.8	100.0	100.0	100.0
Panel D: Subsequent Outcomes												
Admitted to Hospital Within 12 Months	37.9	36.9	20.5	33.3	33.9	20.2	47.7	47.2	33.5	48.7	48.2	26.7
Admitted to Hospital Within 48 Months	54.0	53.2	36.1	48.7	49.9	35.3	67.0	67.2	56.6	66.1	66.0	48.4
Died within 12 Months	8.5	7.8	3.1	6.0	5.7	3.8	26.6	25.2	15.1	19.5	18.7	9.5
Died within 48 Months	15.6	14.7	6.2	11.8	11.7	7.5	47.9	46.4	30.6	34.9	33.9	17.8
Individuals	552,854	469,784	383,718	211,663	167,762	153,617	764,954	571,591	414,547	243,498	193,470	131,446
Weighted Proportion of Individuals Remaining		85.8	71.2		79.9	58.9		75.7	57.8		80.9	45.7
N (Hospital Records)	1,677,886	1,389,703	383,718	333,935	269,064	153,617	2,714,345	2,021,630	414,547	731,668	579,030	131,446

Notes: Insurance status and age are classified at the time of the index admission; "insured" indicates an individual was covered by Medicaid or private insurance. 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. All proportions are multiplied by 100 and the analysis is weighted to adjust for oversampling of some groups. The categories Asian, white and black all exclude Hispanic. All hospitalizations that are pregnancy related (MDC = 14) have been dropped from the sample. Median Income for Zip Code of Residence is from the 2007-2011 American Community Survey and reflect the mean value of the median household income for each individual's zip code of residence.

Appendix Table 14. Sample Composition and Hospitalization Summary Statistics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Sample	Non-Elderly Insured			Non-Elderly Uninsured			Elderly			Ages 60-70 Through ED		
Drop if not in TU	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Drop if not first hospitalization in three years	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
			[Baseline]			[Baseline]			[Baseline]			[Baseline]
Panel B: Index Hospitalization Ten Most Common Major Diagnosis Codes												
Circulatory System	16.1	16.3	16.0	15.9	16.1	15.4	26.0	26.4	25.6	28.5	29.0	32.7
Musculoskeletal System and Connective Tissue	10.5	10.9	13.4	6.6	6.5	8.1	12.0	12.9	17.3	5.2	5.3	6.9
Female Reproductive System	7.9	8.2	12.8	1.8	1.8	2.3	1.1	1.2	2.1	0.2	0.2	0.2
Digestive System	12.4	12.6	12.8	11.3	11.0	11.8	11.2	11.3	11.3	12.6	12.8	14.7
Nervous System	6.1	6.1	6.1	6.7	6.6	7.4	8.1	8.2	9.5	8.5	8.5	10.1
Respiratory System	7.5	7.2	5.7	7.8	7.8	7.5	14.5	13.7	10.8	16.3	16.0	11.4
Hepatobiliary System and Pancreas	6.2	6.0	5.4	7.2	7.0	6.3	3.1	3.0	3.3	5.2	5.1	5.2
Endocrine, Nutritional and Metabolic System	5.0	5.1	5.4	5.2	5.3	5.1	3.8	3.6	3.1	4.0	3.8	3.0
Skin, Subcutaneous Tissue and Breast	3.9	3.8	4.2	8.1	8.2	9.2	2.4	2.4	2.6	2.5	2.4	2.6
Mental Diseases and Disorders	7.4	6.8	4.0	7.6	7.6	5.7	1.4	1.3	1.0	1.5	1.3	0.9
Other MDC	17.2	17.1	14.2	21.7	21.9	21.1	16.4	16.0	13.5	17.2	17.2	17.2
Panel C: Subsequent Admissions												
Over 12 Months after index hospitalization (not including index stay)												
Admitted to Hospital Within 12 Months	37.9	36.9	20.5	33.3	33.9	20.2	47.7	47.2	33.5	48.7	48.2	26.7
Hospital Stays Within 12 Months	1.1	1.0	0.3	0.9	0.9	0.3	1.0	1.0	0.5	1.2	1.2	0.4
Total Hospital List Charges (Conditional on Any)	144,057	140,976	87,721	105,308	104,751	76,736	130,829	127,472	96,975	168,628	166,612	93,482
Total Days in Hospital (Conditional on Any)	19.7	18.6	9.6	16.5	16.3	10.9	16.9	16.0	11.5	20.4	19.6	10.4
Medicaid	39.7	34.6	18.3	23.4	22.6	21.3	4.3	1.7	1.1	16.7	12.7	8.6
Private	52.3	57.4	75.5	9.6	10.3	12.1	5.9	6.8	8.0	19.6	22.7	33.6
Medicare	4.0	4.1	2.3	3.6	3.6	2.6	89.0	90.8	90.3	60.0	61.1	51.7
Other insurance	1.5	1.5	1.5	5.3	5.3	4.6	0.5	0.5	0.3	1.3	1.3	1.8
County Medically Indigent	1.0	1.0	0.9	14.9	15.0	16.1	0.0	0.0	0.0	1.2	1.1	2.3
Self Pay	1.5	1.5	1.5	43.3	43.4	43.4	0.2	0.2	0.2	1.2	1.2	2.0
Over 48 Months after index hospitalization (not including index stay)												
Admitted to Hospital Within 48 Months	54.0	53.2	36.1	48.7	49.9	35.3	67.0	67.2	56.6	66.1	66.0	48.4
Hospital Stays Within 48 Months	2.5	2.4	0.8	2.3	2.3	0.9	2.2	2.2	1.3	2.8	2.8	1.2
Total Hospital List Charges (Conditional on Any)	239,816	233,804	121,824	190,334	191,538	122,489	210,484	204,736	145,042	287,419	284,277	159,896
Total Days in Hospital (Conditional on Any)	31.0	29.1	12.3	27.2	27.2	15.5	25.1	23.8	15.9	32.4	31.2	16.0
Medicaid	33.5	28.8	16.0	26.1	25.1	22.9	3.9	1.5	1.0	13.9	10.2	6.8
Private	52.3	56.7	71.7	13.2	14.1	17.3	6.0	6.8	7.8	18.3	20.9	28.3
Medicare	9.1	9.4	6.8	8.1	8.3	7.1	89.3	90.9	90.5	64.6	65.7	60.2
Other insurance	1.9	1.9	2.1	5.6	5.5	5.1	0.5	0.5	0.4	1.3	1.3	1.7
County Medically Indigent	1.3	1.3	1.3	13.6	13.6	14.5	0.0	0.0	0.0	0.9	0.8	1.5
Self Pay	1.9	2.0	2.2	33.5	33.3	33.0	0.2	0.2	0.2	1.0	1.0	1.5
One Hospitalization in Next 48 Months	17.2	17.4	18.0	15.9	16.1	16.2	20.2	20.3	22.0	17.6	17.7	19.5
Two Hospitalizations in Next 48 Months	8.4	8.4	6.5	7.6	7.8	6.3	12.7	12.8	11.7	10.7	10.8	8.9
Three Hospitalizations in Next 48 Months	4.9	4.8	2.8	4.3	4.4	2.9	8.1	8.1	6.2	7.1	7.0	4.6
Four or More Hospitalizations in Next 48 Months	19.1	18.3	5.2	17.1	17.7	6.5	20.9	20.6	10.4	25.7	25.3	9.4
Individuals	552,854	469,784	383,718	211,663	167,762	153,617	764,954	571,591	414,547	243,498	193,470	131,446
N (Hospital Records)	1,677,886	1,389,703	383,718	333,935	269,064	153,617	2,714,345	2,021,630	414,547	731,668	579,030	131,446

Notes: Insurance status and age are classified at the time of the index admission; insured indicates an individual was covered by Medicaid or private insurance. 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. All proportions are multiplied by 100 and the analysis is weighted to adjust for oversampling of some groups. For subsequent admissions, days in hospital and charges are summed across all subsequent hospitalizations in the reference window and insurance coverage is the weights sum of type of insurance an individual is covered by over subsequent stays where the weights are the proportion of the total days in the hospital over the reference window covered by each type of insurance. Average subsequent list charges, days in hospital and insurance type are conditional on having a subsequent hospital stay. The major diagnosis codes listed above are the ten most common in the baseline sample.

Appendix Table 15. Pre-Hospitalization Summary Statistics

	Mean (1)	Std Dev (2)	Median (3)	Share Zero (4)	90th Percentile (5)
Panel A. Non-Elderly Insured					
Number of Collections to Date	.92	(2.5)	0	.72	3
Number of Medical Collections to Date	.2	(.99)	0	.9	0
Number of Non-Medical Collections to Date	.72	(2)	0	.75	2
Collection Balances	1,230	(6,022)	0	.67	2,578
Medical Collection Balances	292	(2,225)	0	.83	304
Non-Medical Collection Balances	1,086	(5,517)	0	.72	2,086
Any Bankruptcy To Date	.034	(.18)	0	.97	0
Credit Limit	37,664	(69,215)	14,344	.21	98,140
Credit Score	731	(120)	729	0	891
Credit Card Balances	11,942	(33,405)	2,151	.25	28,600
Automobile Loan Balance	6,684	(12,414)	0	.64	22,427
Panel B. Non-Elderly Uninsured					
Number of Collections to Date	2.3	(4.1)	1	.47	6
Number of Medical Collections to Date	.59	(2.1)	0	.78	2
Number of Non-Medical Collections to Date	1.7	(3)	0	.52	5
Collection Balances	3,529	(10,272)	395	.4	8,692
Medical Collection Balances	1,292	(5,629)	0	.64	2,578
Non-Medical Collection Balances	2,762	(8,875)	150	.46	6,562
Any Bankruptcy To Date	.037	(.19)	0	.96	0
Credit Limit	15,145	(47,016)	300	.48	41,300
Credit Score	655	(110)	631	0	825
Credit Card Balances	5,376	(22,818)	0	.52	11,559
Automobile Loan Balance	3,981	(9,820)	0	.75	15,194
Panel C. Elderly					
Number of Collections to Date	.24	(1.2)	0	.9	0
Number of Medical Collections to Date	.048	(.45)	0	.97	0
Number of Non-Medical Collections to Date	.19	(.96)	0	.91	0
Collection Balances	428	(3,746)	0	.87	85
Medical Collection Balances	75	(1,262)	0	.94	0
Non-Medical Collection Balances	422	(3,673)	0	.89	0
Any Bankruptcy To Date	.016	(.13)	0	.98	0
Credit Limit	36,967	(61,330)	20,734	.097	81,500
Credit Score	824	(102)	860	0	921
Credit Card Balances	7,016	(25,599)	675	.23	15,300
Automobile Loan Balance	2,143	(7,206)	0	.85	7,729

Notes: Summary statistics are calculated for the non-elderly insured, the non-elderly uninsured, and the elderly (see Appendix Table 13, columns 3, 6, and 9) 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. Collections "to date" reflect, on average, about a 3 year look back window. N = 371,061 unique individuals (except for medical and non-medical collection balances where N = 127,384, and credit score where N = 351,960) for the insured sample, 144,234 (58,141 for medical/non-medical collection balances and 120,890 for credit score) for the uninsured, and 406,613 (135,059 for medical/non-medical collection balances and 395,007 for credit score) for the elderly sample. All estimates are weighted to account for individuals' sampling probabilities.

Appendix Table 16. Comparison to Credit Reports Not Matched to Hospitalization

	Non-Elderly Insured			Non-Elderly Uninsured			Non-Elderly Not Matched to Hospitalization			Elderly			Elderly Not Matched to Hospitalization		
	Mean	*Std Dev*	*Fraction Zero*	*Mean*	*Std Dev*	*Fraction Zero*	*Mean*	*Std Dev*	*Fraction Zero*	*Mean*	*Std Dev*	*Fraction Zero*	*Mean*	*Std Dev*	*Fraction Zero*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Any Collection, Last 12 Months	.17	(.37)	.83	.37	(.48)	.63	.15	(.36)	.85	.052	(.22)	.95	.076	(.27)	.92
Any Medical Collection, Last 12 Months	.043	(.2)	.96	.11	(.32)	.89	.032	(.18)	.97	.011	(.1)	.99	.027	(.16)	.97
Any Non-Medical Collection, Last 12 Months	.14	(.35)	.86	.31	(.46)	.69	.13	(.34)	.87	.044	(.2)	.96	.058	(.23)	.94
Collection Balances	1,230	(6,022)	.67	3,529	(10,272)	.4	1,220	(5,844)	.73	428	(3,746)	.87	617	(4,540)	.86
Medical Collection Balances	292	(2,225)	.83	1,292	(5,629)	.64	141	(989)	.89	75	(1,262)	.94	117	(1,805)	.93
Non-Medical Collection Balances	1,086	(5,517)	.72	2,762	(8,875)	.46	1,072	(5,642)	.77	422	(3,673)	.89	494	(3,868)	.89
Any Bankruptcy, Last 12 Months	.012	(.11)	.99	.013	(.11)	.99	.0072	(.085)	.99	.0057	(.075)	.99	.0048	(.069)	1
Credit Limit	37,664	(69,215)	.21	15,145	(47,016)	.48	42,313	(77,116)	.19	36,967	(61,330)	.097	37,136	(65,304)	.11
Credit Score	731	(120)	0	655	(110)	0	741	(118)	0	824	(102)	0	816	(105)	0
Credit Card Balances	11,942	(33,405)	.25	5,376	(22,818)	.52	13,734	(37,968)	.25	7,016	(25,599)	.23	7,662	(28,104)	.28
Automobile Loan Balance	6,684	(12,414)	.64	3,981	(9,820)	.75	6,736	(12,780)	.63	2,143	(7,206)	.85	2,021	(7,098)	.86
Median Income for Zip Code of Residence	66,984	(24,439)	0	59,138	(22,032)	0	66,519	(24,172)	0	66,712	(24,851)	0			

Notes: Columns 1 through 6 present summary statistics for the non-elderly insured and uninsured (see Appendix Table 13, columns 3 and 6) using the credit report from January of the calendar year preceding the hospitalization (between 12 and 23 months before the hospitalization). Columns 10 through 12 present the same statistics for the elderly at the time of hospitalization (see Appendix Table 13, column 9). Columns 7 through 9 and 13 through 15 present 2005 summary statistics for a random sample of the California residents split by age with social security numbers that were not matched to a hospitalization meeting the sample restriction requirements between 2003 and 2007. The baseline sample sizes for the non-elderly insured, uninsured, and the elderly are the same as Appendix Table 15. Sample sizes for the sample not matched to a hospitalization are 819,631 ages 25 to 64 (785,833 for credit score) and 581,347 ages 65 and older (566,886 for credit score).

Appendix Table 17. Pre-Hospitalization Summary Statistics, by Post-Hospitalization Survival

	Non-Elderly Insured					Non-Elderly Uninsured					Elderly				
By Ex-Post Mortality	All	Died 12 Months	Alive 12 Months	Died 48 Months	Alive 48 Months	All	Died 12 Months	Alive 12 Months	Died 48 Months	Alive 48 Months	All	Died 12 Months	Alive 12 Months	Died 48 Months	Alive 48 Months
Number of Collections to Date	.92	.98	.92	1	.92	2.3	2.1	2.3	2.3	2.3	.24	.25	.24	.24	.24
	(2.5)	(2.5)	(2.5)	(2.6)	(2.5)	(4.1)	(4.1)	(4.1)	(4.4)	(4)	(1.2)	(1.2)	(1.2)	(1.1)	(1.2)
Number of Medical Collections to Date	.2	.21	.2	.22	.2	.59	.53	.6	.64	.59	.048	.05	.047	.049	.047
	(.99)	(1.1)	(.99)	(1.1)	(.98)	(2.1)	(2.1)	(2.1)	(2.4)	(2)	(.45)	(.43)	(.46)	(.45)	(.46)
Number of Non-Medical Collections to Date	.72	.77	.72	.79	.72	1.7	1.6	1.7	1.6	1.7	.19	.2	.19	.19	.19
	(2)	(2)	(2)	(2.1)	(2)	(3)	(2.9)	(3)	(3)	(3)	(.96)	(.98)	(.95)	(.95)	(.96)
Collection Balances	1,230	1,595	1,218	1,644	1,202	3,529	3,769	3,519	4,054	3,484	428	458	422	443	421
	(6,022)	(7,593)	(5,963)	(7,674)	(5,892)	(10,272)	(11,627)	(10,212)	(11,908)	(10,121)	(3,746)	(3,780)	(3,740)	(3,689)	(3,771)
Medical Collection Balances	292	421	287	451	281	1,292	1,336	1,290	1,585	1,267	75	79	74	72	76
	(2,225)	(3,535)	(2,168)	(3,627)	(2,097)	(5,629)	(5,826)	(5,621)	(6,508)	(5,548)	(1,262)	(1,333)	(1,248)	(1,231)	(1,275)
Non-Medical Collection Balances	1,086	1,380	1,076	1,485	1,059	2,762	2,804	2,760	3,001	2,742	422	484	411	459	406
	(5,517)	(6,824)	(5,468)	(7,102)	(5,393)	(8,875)	(9,754)	(8,836)	(9,991)	(8,774)	(3,673)	(3,866)	(3,636)	(3,665)	(3,676)
Any Bankruptcy To Date	.034	.035	.034	.036	.034	.037	.035	.037	.034	.037	.016	.015	.017	.015	.017
Credit Limit	37,664	33,030	37,816	33,289	37,960	15,145	13,782	15,201	13,270	15,303	36,967	29,093	38,390	29,834	40,162
	(69,215)	(62,172)	(69,429)	(62,559)	(69,632)	(47,016)	(33,279)	(47,498)	(36,696)	(47,780)	(61,330)	(48,437)	(63,276)	(48,695)	(65,961)
Credit Score	731	732	731	730	731	655	666	655	659	655	824	823	824	823	824
	(120)	(123)	(120)	(123)	(120)	(110)	(111)	(110)	(111)	(110)	(102)	(100)	(102)	(100)	(102)
Credit Card Balances	11,942	9,735	12,015	9,969	12,076	5,376	4,730	5,402	4,550	5,445	7,016	4,998	7,380	5,092	7,877
	(33,405)	(29,118)	(33,534)	(30,174)	(33,607)	(22,818)	(16,264)	(23,049)	(18,411)	(23,150)	(25,599)	(20,238)	(26,436)	(19,988)	(27,703)
Number of Automobile Loans	1.7	1.4	1.7	1.5	1.8	1.1	.96	1.1	.93	1.1	.79	.6	.82	.61	.87
	(2)	(1.9)	(2)	(1.9)	(2)	(1.7)	(1.6)	(1.7)	(1.6)	(1.7)	(1.4)	(1.2)	(1.4)	(1.2)	(1.5)
Automobile Loan Balance	6,684	5,139	6,735	5,184	6,785	3,981	3,104	4,017	3,049	4,060	2,143	1,429	2,272	1,453	2,452
	(12,414)	(11,117)	(12,451)	(11,043)	(12,495)	(9,820)	(8,425)	(9,871)	(8,591)	(9,912)	(7,206)	(5,729)	(7,434)	(5,735)	(7,755)
N	371,061	18,363	352,698	40,741	330,320	144,234	5,730	138,504	11,202	133,032	406,613	65,091	341,522	149,162	257,451

Notes: Summary statistics are calculated for the non-elderly insured and uninsured (see Appendix Table 13, columns 3 and 6) and elderly (see Appendix Table 13, column 9) using the credit report from January of the calendar year preceding the hospitalization (between 12 and 23 months before the hospitalization) and additionally split by whether individuals died within 12 and 48 months following the hospitalization. Standard deviations are in parentheses. All estimates are weighted to account for individuals' sampling probabilities.

Appendix Table 18. Eventual Death Rates, by Predicted Mortality

Sample	Non-Elderly Insured		Non-Elderly Uninsured		Elderly	
By Predicted Mortality	Baseline	Lowest Mortality Quartile	Baseline	Lowest Mortality Quartile	Baseline	Lowest Mortality Quartile
Died Ever	.0784	.0131	.0964	.0185	.3812	.1185
Died within 12 Months	.0318	.0024	.0397	.0041	.1531	.0232
Died within 48 Months	.0633	.0087	.0777	.0133	.3093	.0813
N	371,061	93,224	144,234	36,104	406,613	101,665

Notes: Samples are non-elderly insured and uninsured (see Appendix Table 13, columns 3 and 6) and elderly (see Appendix Table 13, column 9). The table compares realized death rates based on predicted mortality using age and diagnosis related group from the index admission. Regression results for the lowest mortality quartile can be found in Table 7 and Appendix Tables 31 and 33.

G.2.2 Regression Coefficients

Appendix Table 19. Regression Coefficients from Parametric Specifications for the Insured

Coefficient	β_1	β_2	β_3	β_4	β_5	
Regressor	Pretrend (*r*)	r^2 x 1{r>0}	r^3 x 1{r>0}	$(r\text{-}12)^3$ x 1{r>12}	$(r\text{-}24)^3$ x 1{r>24}	Constant
	(1)	(2)	(3)	(4)	(5)	(6)
Number of Collections to Date	-.00085	.0014	-.000054	.000069	-.000016	.26
	(5.4e-08)	(4.7e-09)	(1.1e-11)	(3.2e-11)	(9.9e-12)	(.000062)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
Number of Medical Collections to Date	-.00044	.0012	-.000049	.000062	-.000013	.048
	(8.0e-09)	(1.1e-09)	(2.6e-12)	(7.3e-12)	(2.1e-12)	(9.3e-06)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
Number of Non-Medical Collections to Date	-.00041	.00014	-5.6e-06	7.9e-06	-3.3e-06	.21
	(3.7e-08)	(2.6e-09)	(5.9e-12)	(1.7e-11)	(5.4e-12)	(.000043)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
Collection Balances	-.94	1.5	-.054	.063	-.0092	992
	(.42)	(.037)	(.000083)	(.00021)	(.000048)	(690)
	[.026]	[<.001]	[<.001]	[<.001]	[<.001]	[.15]
Medical Collection Balances	-1.6	1.6	-.061	.074	-.012	238
	(.093)	(.0095)	(.00002)	(.000048)	(9.7e-06)	(22)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
Non-Medical Collection Balances	-.76	.2	-.0062	.0079	-.0034	1,060
	(.8)	(.052)	(.0001)	(.00023)	(.00004)	(180)
	[.34]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
Any Bankruptcy To Date	.000075	.000016	-6.1e-07	8.1e-07	-2.4e-07	.017
	(3.8e-10)	(2.1e-11)	(4.5e-14)	(1.2e-13)	(2.9e-14)	(5.3e-07)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
Credit Limit	23	-5.2	.14	-.099	-.055	29,667
	(53)	(5.1)	(.011)	(.025)	(.005)	(81,888)
	[.67]	[.31]	[<.001]	[<.001]	[<.001]	[.72]
Credit Score	.14	-.023	.001	-.0014	.0004	729
	(.00019)	(7.2e-06)	(1.6e-08)	(3.9e-08)	(8.1e-09)	(.36)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
Credit Card Balances	.98	-2.9	.074	-.044	-.045	8,562
	(11)	(2)	(.0043)	(.01)	(.0021)	(15,407)
	[.93]	[.14]	[<.001]	[<.001]	[<.001]	[.58]
Automobile Loan Balance	-2.1	-.92	.018	-.0021	-.021	6,012
	(1.8)	(.17)	(.00035)	(.00079)	(.00013)	(2,992)
	[.26]	[<.001]	[<.001]	[.0065]	[<.001]	[.045]

Notes: Sample is the non-elderly insured (see Table 1, column 2). Coefficients are based on OLS estimates of equation (5); all regressions include calendar year fixed effects. 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.

Appendix Table 20. Regression Coefficients from Parametric Specifications for the Uninsured

Coefficient	β_1	β_2	β_3	β_4	β_5	
Regressor	Pretrend (r)	$r^2 \times 1\{r>0\}$	$r^3 \times 1\{r>0\}$	$(r-12)^3 \times 1\{r>12\}$	$(r-24)^3 \times 1\{r>24\}$	Constant
	(1)	(2)	(3)	(4)	(5)	(6)
Number of Collections to Date	.0015	.013	-.00055	.00072	-.00018	.73
	(2.9e-07)	(3.1e-08)	(6.7e-11)	(1.8e-10)	(5.2e-11)	(.00036)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
Number of Medical Collections to Date	-.00018	.012	-.00049	.00065	-.00016	.18
	(8.3e-08)	(1.4e-08)	(3.0e-11)	(8.0e-11)	(2.1e-11)	(.0001)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
Number of Non-Medical Collections to Date	.0017	.0015	-.000061	.000074	-.000013	.55
	(1.5e-07)	(1.1e-08)	(2.3e-11)	(6.3e-11)	(1.8e-11)	(.00018)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
Collection Balances	-.8	61	-2.5	3.2	-.73	2,636
	(2.4)	(.6)	(.0013)	(.0031)	(.00063)	(3,777)
	[.74]	[<.001]	[<.001]	[<.001]	[<.001]	[.49]
Medical Collection Balances	-14	58	-2.4	3.1	-.74	911
	(2.1)	(.47)	(.001)	(.0024)	(.00046)	(794)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[.25]
Non-Medical Collection Balances	6.6	3.1	-.11	.12	-.0066	2,912
	(3.7)	(.26)	(.00049)	(.0011)	(.00018)	(1,012)
	[.077]	[<.001]	[<.001]	[<.001]	[<.001]	[.004]
Any Bankruptcy To Date	-.000026	.000058	-2.0e-06	2.3e-06	-3.1e-07	.014
	(7.3e-10)	(4.5e-11)	(9.3e-14)	(2.3e-13)	(5.0e-14)	(1.1e-06)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
Credit Limit	36	-9.7	.42	-.58	.18	13,703
	(43)	(3.7)	(.0077)	(.018)	(.0036)	(75,097)
	[.4]	[.0085]	[<.001]	[<.001]	[<.001]	[.86]
Credit Score	.093	-.083	.004	-.0059	.0019	657
	(.00033)	(.000014)	(3.0e-08)	(7.3e-08)	(1.5e-08)	(.66)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
Credit Card Balances	7.9	-3.3	.12	-.13	.011	4,518
	(8.3)	(1.5)	(.0033)	(.0079)	(.0016)	(13,239)
	[.34]	[.033]	[<.001]	[<.001]	[<.001]	[.73]
Automobile Loan Balance	-7	-3.5	.13	-.16	.024	3,504
	(2)	(.17)	(.00034)	(.00076)	(.00012)	(3,503)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[.32]

Notes: Sample is the non-elderly uninsured (see Appendix Table 13, column 6). Coefficients are based on OLS estimates of equation (5); all regressions include calendar year fixed effects. 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.

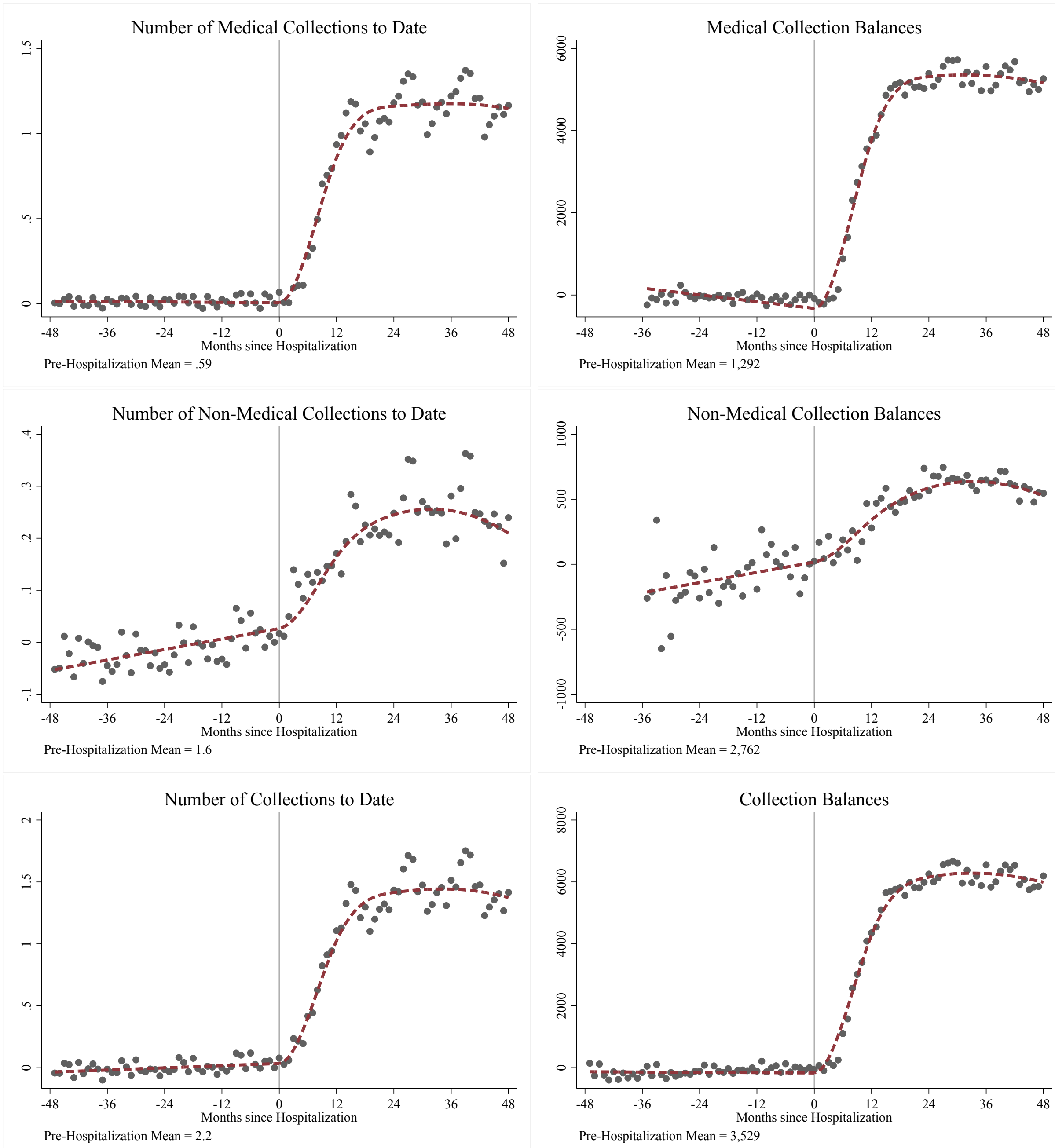
Appendix Table 21. Regression Coefficients from Parametric Specifications for the Elderly

Coefficient	β_1	β_2	β_3	β_4	β_5	
Regressor	Pretrend (*r*)	r^2 x $1\{r>0\}$	r^3 x $1\{r>0\}$	$(r-12)^3$ x $1\{r>12\}$	$(r-24)^3$ x $1\{r>24\}$	Constant
	(1)	(2)	(3)	(4)	(5)	(6)
Number of Collections to Date	-.00078	.00034	-.000013	.000015	-1.8e-06	.042
	(1.2e-08)	(1.3e-09)	(3.0e-12)	(8.7e-12)	(2.7e-12)	(.000014)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
Number of Medical Collections to Date	-.00036	.00033	-.000013	.000015	-2.4e-06	.0029
	(2.0e-09)	(2.7e-10)	(6.4e-13)	(1.8e-12)	(5.4e-13)	(2.2e-06)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
Number of Non-Medical Collections to Date	-.00042	8.0e-08	2.7e-07	-1.1e-06	8.9e-07	.039
	(8.3e-09)	(8.1e-10)	(1.8e-12)	(5.3e-12)	(1.6e-12)	(9.1e-06)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
Collection Balances	-1.6	.27	-.0087	.009	-.0005	306
	(.17)	(.014)	(.000029)	(.000072)	(.000016)	(260)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[.24]
Medical Collection Balances	-.54	.2	-.0069	.0071	.00034	60
	(.022)	(.0019)	(3.8e-06)	(9.0e-06)	(1.7e-06)	(6.3)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
Non-Medical Collection Balances	-.87	.024	.00014	-.00063	-.00016	403
	(.44)	(.025)	(.000047)	(.0001)	(.000017)	(83)
	[.047]	[.34]	[.0023]	[<.001]	[<.001]	[<.001]
Any Bankruptcy To Date	-.000025	-3.4e-06	1.7e-07	-3.1e-07	1.8e-07	.0056
	(1.8e-10)	(1.1e-11)	(2.3e-14)	(5.9e-14)	(1.4e-14)	(2.3e-07)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
Credit Limit	-69	6.4	-.32	.48	-.18	30,158
	(49)	(4.2)	(.0088)	(.021)	(.0044)	(71,628)
	[.16]	[.13]	[<.001]	[<.001]	[<.001]	[.67]
Credit Score	.099	-.018	.00068	-.00084	.00015	826
	(.00013)	(6.5e-06)	(1.4e-08)	(3.5e-08)	(7.3e-09)	(.23)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
Credit Card Balances	-29	1	-.044	.057	-.017	4,694
	(8.6)	(1.2)	(.0027)	(.0065)	(.0013)	(11,721)
	[<.001]	[.4]	[<.001]	[<.001]	[<.001]	[.69]
Automobile Loan Balance	-12	.97	-.041	.06	-.021	1,684
	(.69)	(.064)	(.00013)	(.0003)	(.00005)	(1,080)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[.12]

Notes: Sample is the elderly (see Appendix Table 13, column 9). Coefficients are based on OLS estimates of equation (5); all regressions include calendar year fixed effects. 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.

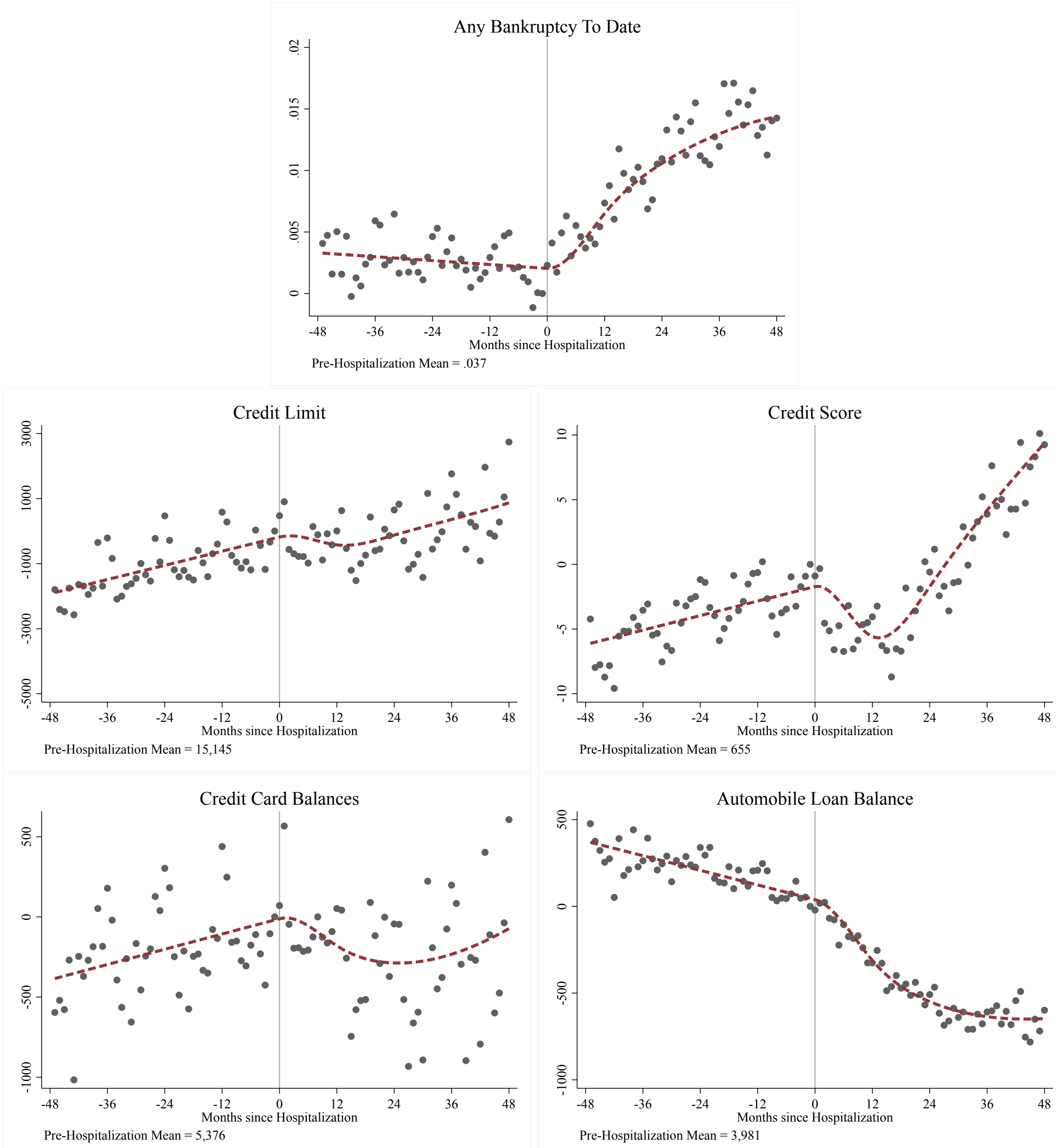
G.2.3 Results for the Uninsured and the Elderly

Appendix Figure 16. Impact of Hospitalization on Collections for the Uninsured



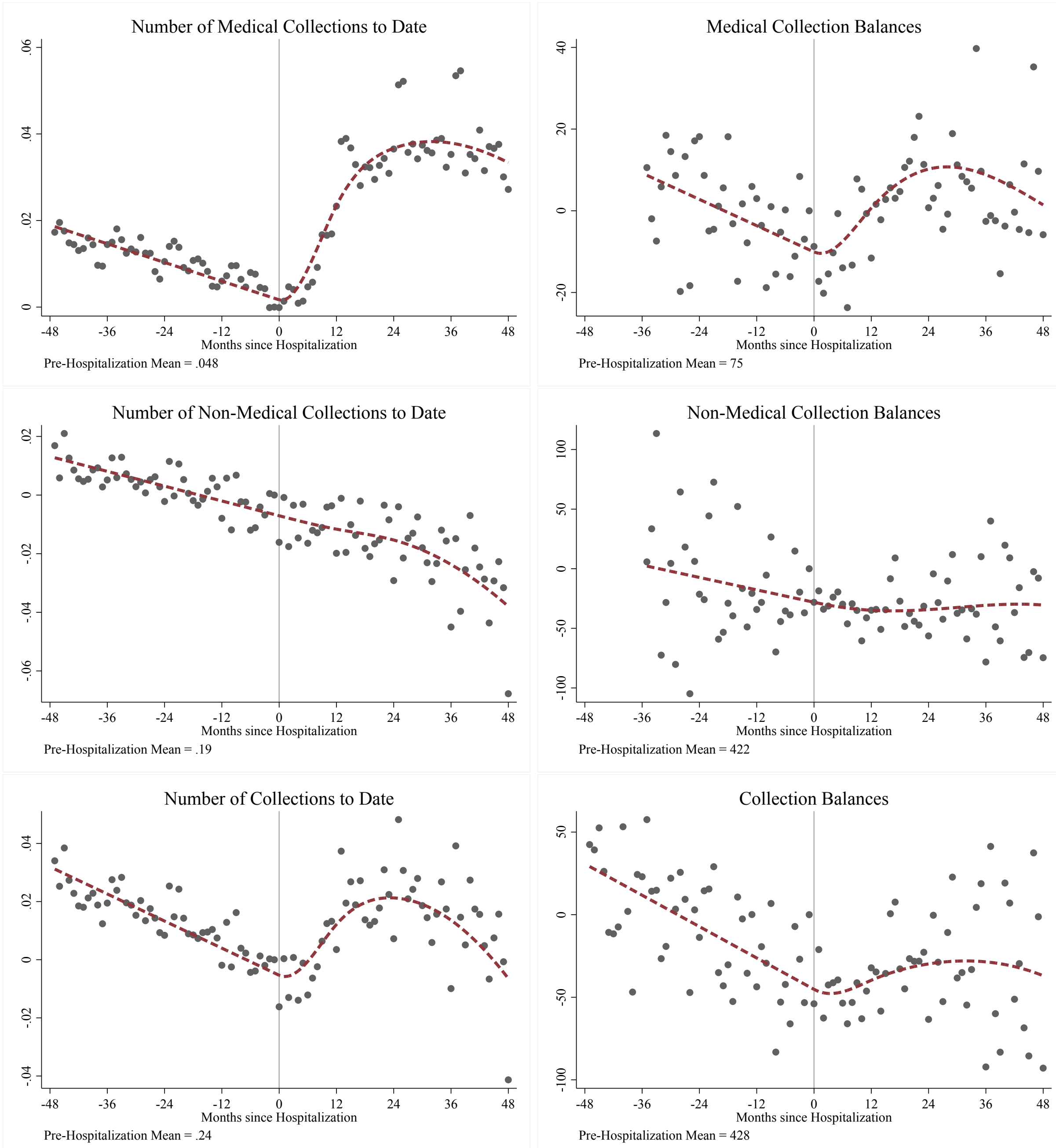
Notes: The sample is the non-elderly uninsured (see Appendix Table 13, column 6). The months on the x-axis are defined relative to the index admission. The points in each figure represent the estimated effects of event time (i.e., the μ_r's from the non-parametric event study in equation (3)). The dashed line represents the estimated event study coefficients from the parametric event study in equation (5) with the level normalized to match the non-parametric estimates. All estimates are weighted to account for individuals' sampling probabilities.

Appendix Figure 17. Impact of Hospitalization on Other Credit Report Outcomes for the Uninsured



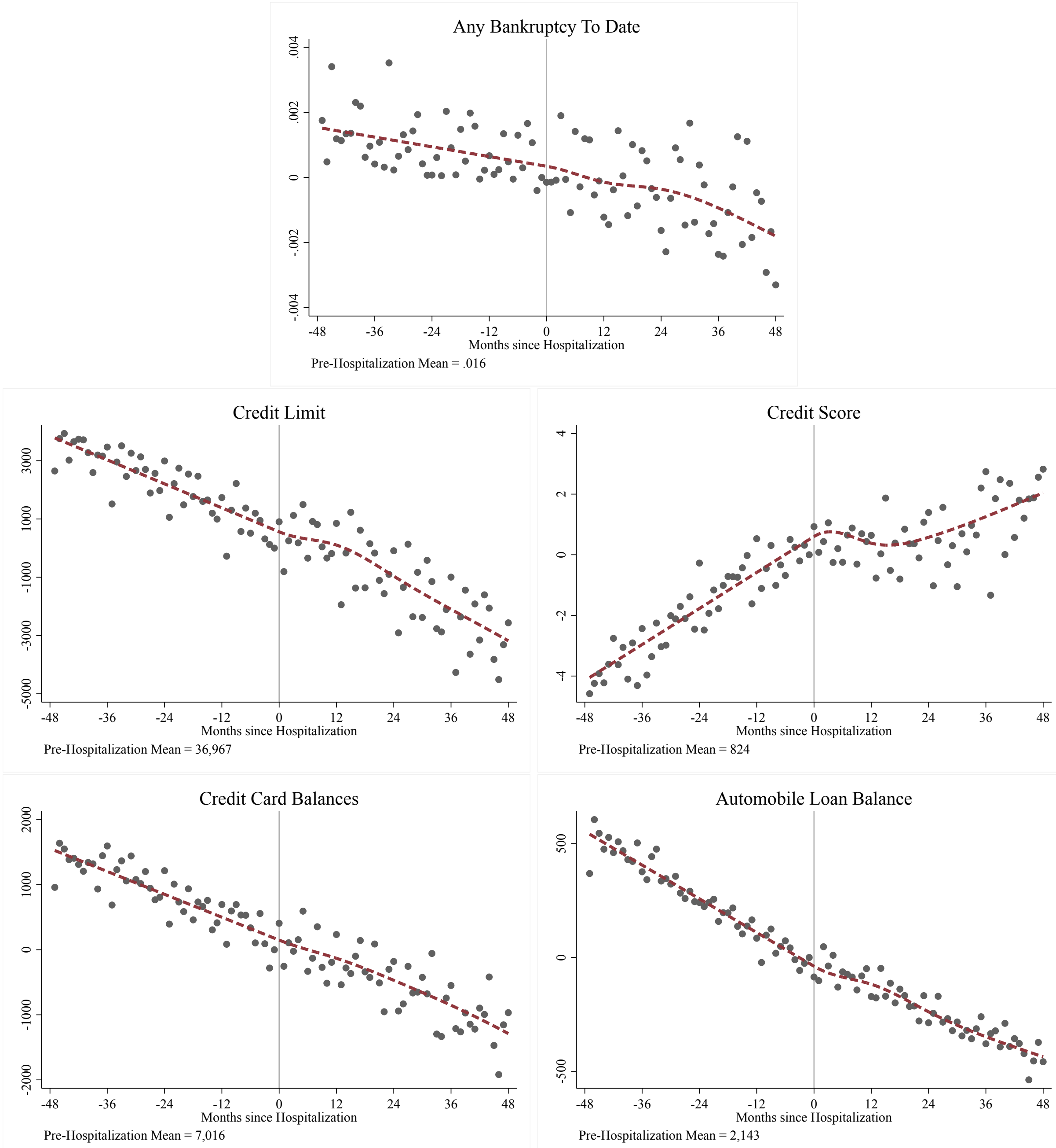
Notes: The sample is the non-elderly uninsured (see Appendix Table 13, column 6). The months on the x-axis are defined relative to the index admission. The points in each figure represent the estimated effects of event time (i.e., the μ_r's from the non-parametric event study in equation (3)). The dashed line represents the estimated event study coefficients from the parametric event study in equation (5) with the level normalized to match the non-parametric estimates. All estimates are weighted to account for individuals' sampling probabilities.

Appendix Figure 18. Impact of Hospitalization on Collections for the Elderly



Notes: The sample is the elderly (see Appendix Table 13, column 9). The months on the x-axis are defined relative to the index admission. The points in each figure represent the estimated effects of event time (i.e., the μ_r's from the non-parametric event study in equation (3)). The dashed line represents the estimated event study coefficients from the parametric event study in equation (5) with the level normalized to match the non-parametric estimates. All estimates are weighted to account for individuals' sampling probabilities.

Appendix Figure 19. Impact of Hospitalization on Other Credit Report Outcomes for the Elderly



Notes: The sample is the elderly (see Appendix Table 13, column 9). The months on the x-axis are defined relative to the index admission. The points in each figure represent the estimated effects of event time (i.e., the μ_r's from the non-parametric event study in equation (3)). The dashed line represents the estimated event study coefficients from the parametric event study in equation (5) with the level normalized to match the non-parametric estimates. All estimates are weighted to account for individuals' sampling probabilities.

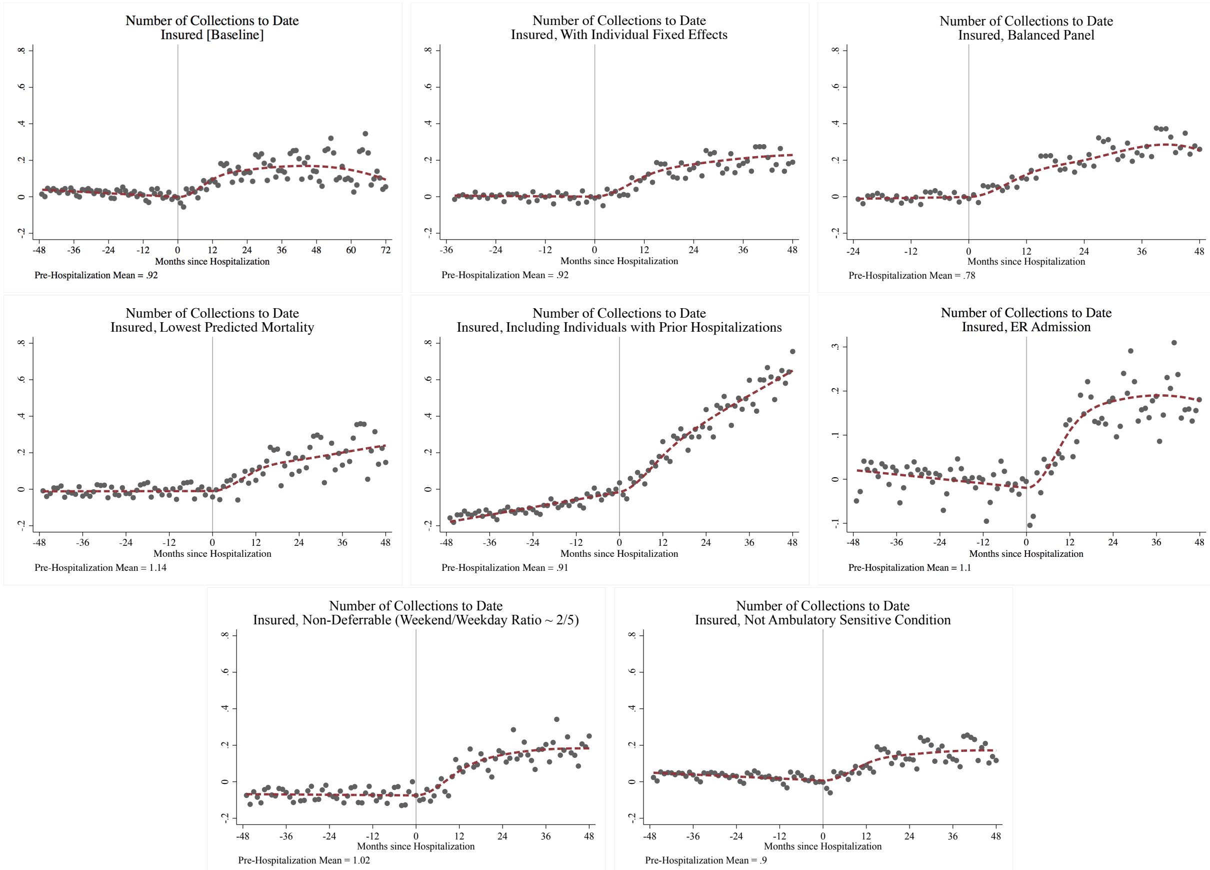
G.2.4 Robustness for Insured

Appendix Table 22. Robustness to Dropping Potential Bad Credit Report-Hospitalization Matches

Sample	Non-Elderly Insured	Non-Elderly Insured	Non-Elderly Uninsured	Non-Elderly Uninsured	Elderly	Elderly
	[Baseline]	Drop Mismatched Birthdays	[Baseline]	Drop Mismatched Birthdays	[Baseline]	Drop Mismatched Birthdays
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Number of Collections to Date						
12-month effect	.11	.11	.97	1.1	.027	.027
	(.005)	(.005)	(.012)	(.013)	(.002)	(.002)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
48-month effect	.21	.23	1.3	1.4	.038	.03
	(.019)	(.02)	(.045)	(.05)	(.01)	(.01)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[.0036]
Pre-hospitalization mean	.92	.92	2.3	2.4	.24	.22
Panel B: Collection Balances						
12-month effect	122	118	4,469	4,919	24	21
	(13)	(13)	(51)	(57)	(8)	(8)
	[<.001]	[<.001]	[<.001]	[<.001]	[.0018]	[.0097]
48-month effect	302	326	6,199	6,848	84	79
	(37)	(39)	(130)	(145)	(24)	(27)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[.003]
Pre-hospitalization mean	1,230	1,226	3,529	3,699	428	402
Panel C: Any Bankruptcy to Date						
12-month effect	.0013	.0013	.0048	.0057	-.00019	-.00017
	(.00031)	(.00033)	(.00046)	(.00051)	(.00022)	(.00025)
	[<.001]	[<.001]	[<.001]	[<.001]	[.4]	[.5]
48-month effect	.0042	.0044	.014	.016	-.001	-.0011
	(.00092)	(.00099)	(.0014)	(.0015)	(.00072)	(.0008)
	[<.001]	[<.001]	[<.001]	[<.001]	[.16]	[.16]
Pre-hospitalization mean	.034	.035	.037	.037	.016	.016
Panel D: Credit Limit						
12-month effect	-515	-493	-678	-649	370	246
	(154)	(166)	(131)	(141)	(138)	(152)
	[<.001]	[.0029]	[<.001]	[<.001]	[.0073]	[.11]
48-month effect	-2,215	-2,218	-690	-558	-448	-958
	(440)	(471)	(353)	(377)	(393)	(441)
	[<.001]	[<.001]	[.051]	[.14]	[.25]	[.03]
Pre-hospitalization mean	37,664	38,157	15,145	14,182	36,967	37,834
Panel E: Credit Score						
12-month effect	-1.6	-1.7	-5	-5.3	-1.4	-1.4
	(.18)	(.19)	(.25)	(.27)	(.17)	(.18)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
48-month effect	-1.8	-1.9	6.6	7.2	-3.3	-3.4
	(.45)	(.48)	(.63)	(.68)	(.45)	(.49)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
Pre-hospitalization mean	731	731	655	649	824	826
Panel F: Credit Card Balances						
12-month effect	-293	-293	-264	-268	72	43
	(94)	(101)	(83)	(89)	(73)	(81)
	[.0018]	[.0037]	[.0014]	[.0026]	[.32]	[.59]
48-month effect	-1,208	-1,205	-443	-323	-30	-171
	(253)	(270)	(214)	(227)	(187)	(209)
	[<.001]	[<.001]	[.038]	[.16]	[.87]	[.41]
Pre-hospitalization mean	11,942	12,115	5,376	5,185	7,016	7,137
Panel G: Automobile Loan Balance						
12-month effect	-102	-118	-267	-300	69	71
	(28)	(30)	(29)	(30)	(17)	(19)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
48-month effect	-507	-538	-349	-403	194	192
	(71)	(76)	(73)	(78)	(43)	(48)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
Pre-hospitalization mean	6,684	6,754	3,981	3,856	2,143	2,120
Number of Individuals	383,718	340,024	153,617	131,517	414,547	337,990
Number of Observations	3,131,534	2,776,577	1,256,759	1,074,189	2,959,802	2,419,300

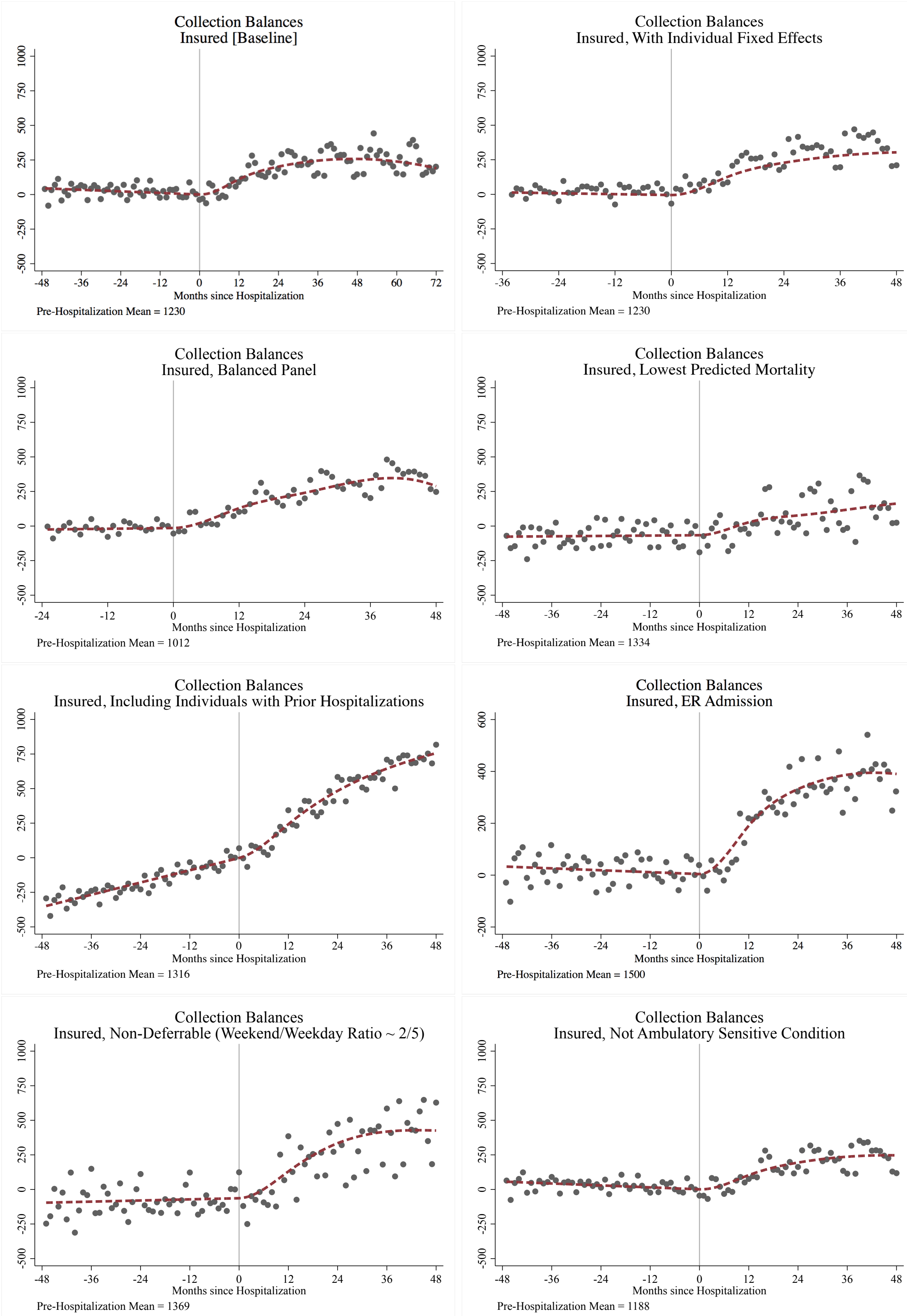
Notes: Columns 1, 3, and 5 replicate results for the non-elderly insured and uninsured (see Appendix Table 13, columns 3 and 6) and elderly (see Appendix Table 13, column 9). Columns 2, 4, and 6 includes only individuals who have the same birth year and birth month in the credit report and hospitalization data. All columns report effects based on OLS estimates of equation (5). 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.

Appendix Figure 20. Impact of Hospitalization on Number of Collections for the Insured, Robustness



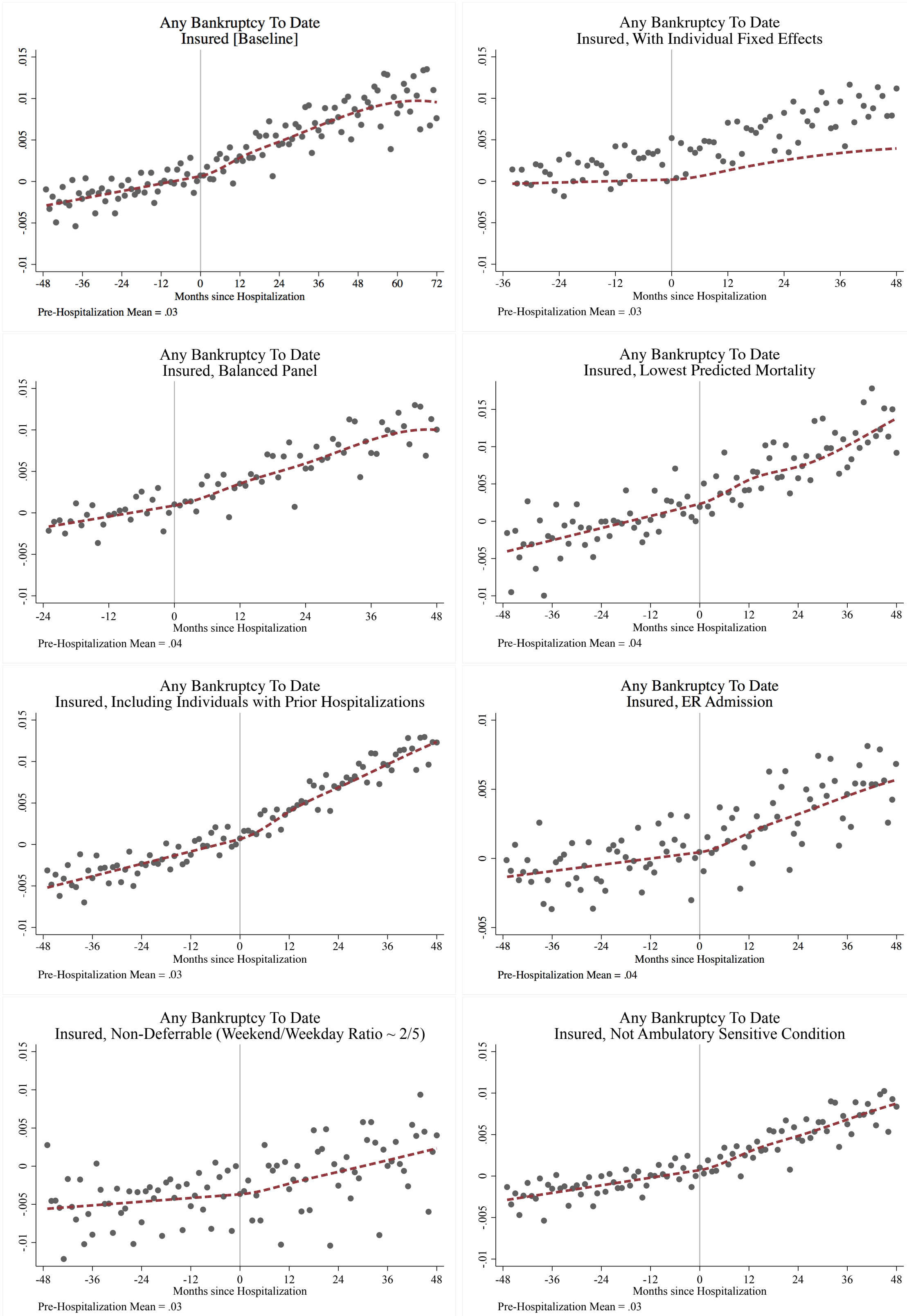
Notes: The baseline sample is the non-elderly insured (see Table 1, column 2). The top left figure displays the baseline specification. All other figures indicate specific departures from the baseline as follows: adding individual fixed effects; limiting to a balanced panel of individuals with non-missing data for the two years before and four years after their hospitalization; restricting to individuals in the lowest quartile of predicted mortality risk based on age and diagnosis-related group; adding insured individuals who had a prior hospital admission within the last three years; restricting to non-deferrable admissions; and, excluding admissions for "ambulatory care sensitive conditions." The parametric line may match the non-parametric event time dummies poorly for the individual fixed effects specification, due to the normalization requirements for identification (see Appendix Section D.2.2 for details).

Appendix Figure 21. Impact of Hospitalization on Collection Balances for the Insured, Robustness



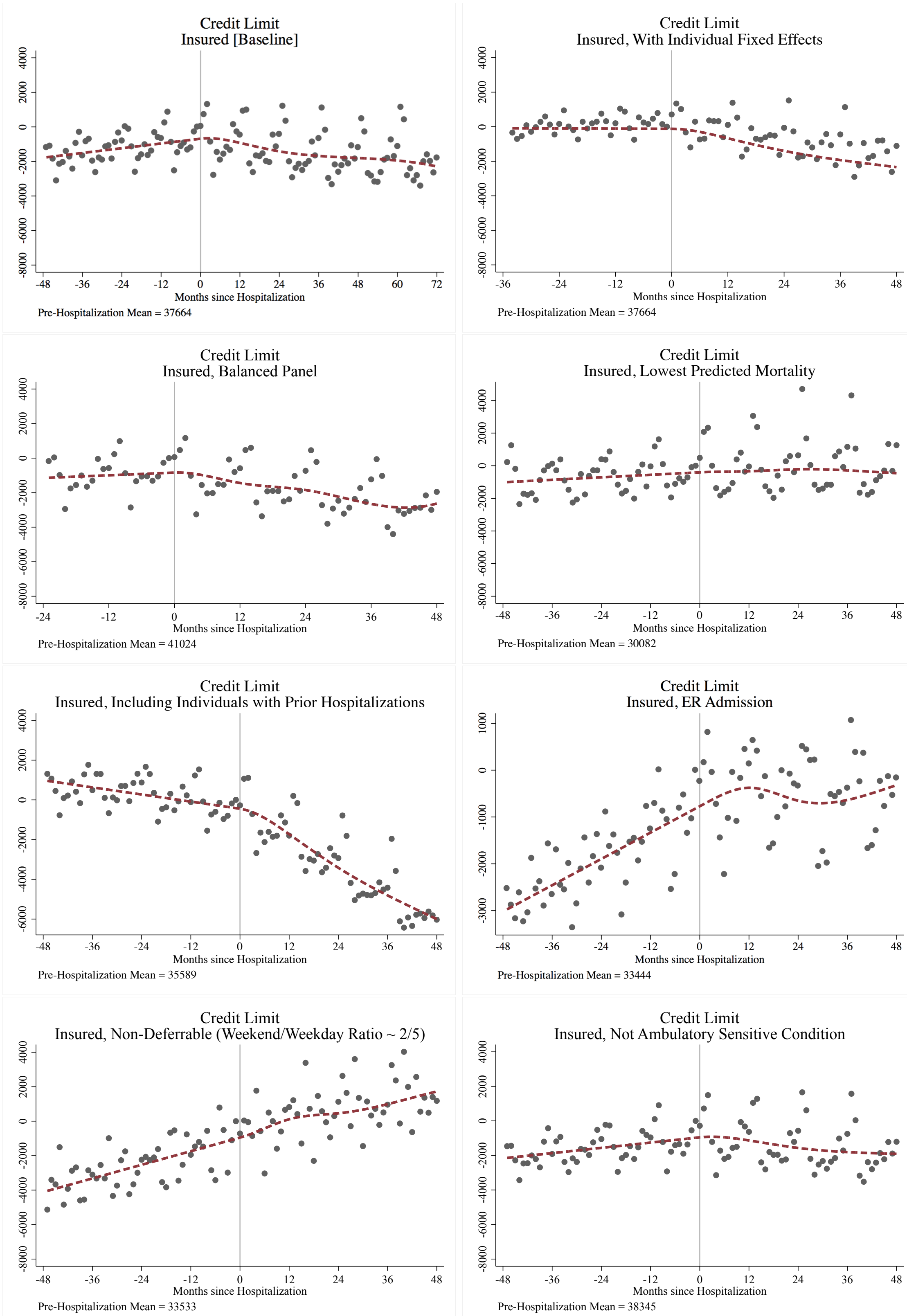
Notes: See notes to Appendix Figure 20.

Appendix Figure 22. Impact of Hospitalization on Consumer Bankruptcy for the Insured, Robustness



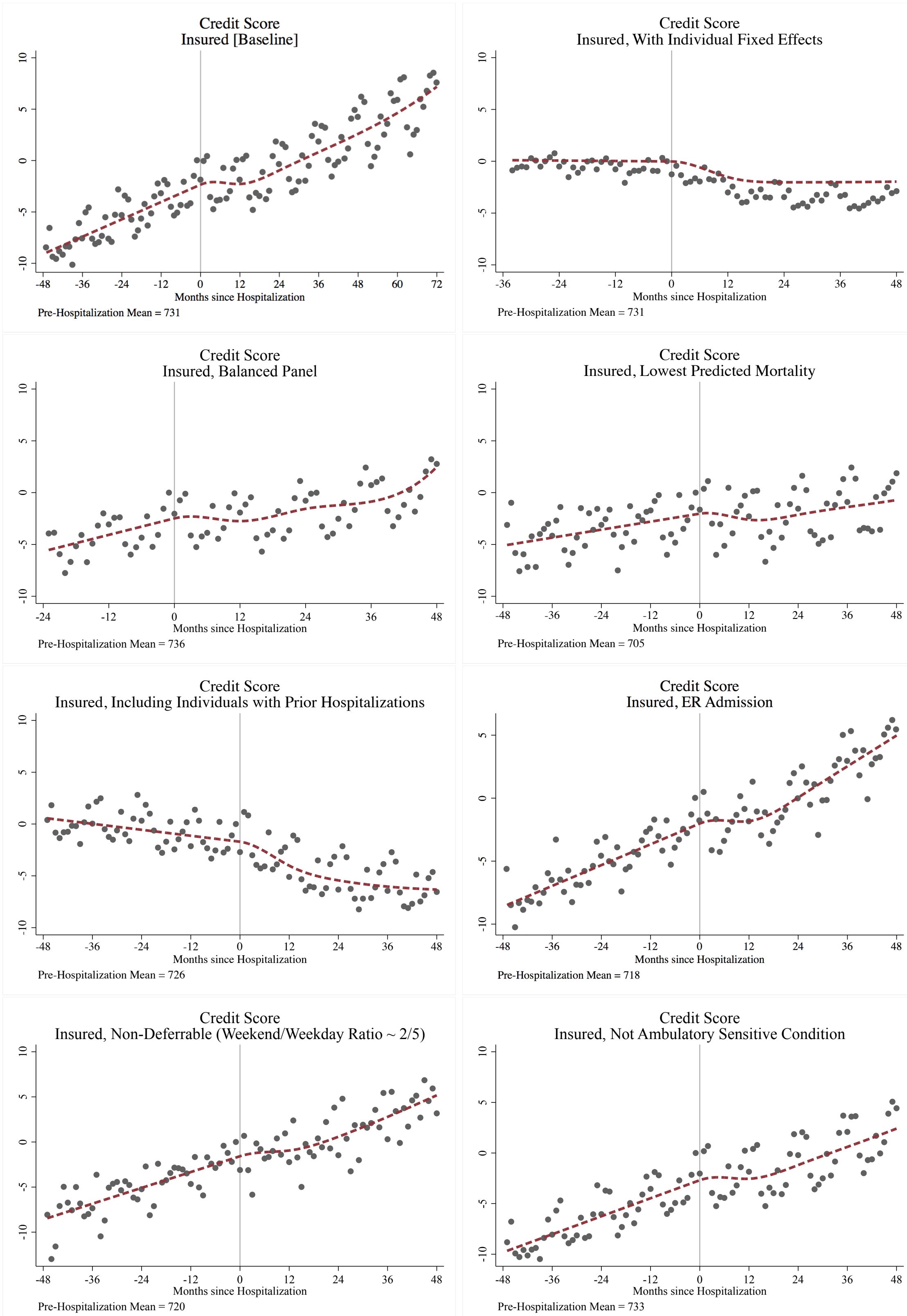
Notes: See notes to Appendix Figure 20.

Appendix Figure 23. Impact of Hospitalization on Credit Limit for the Insured, Robustness



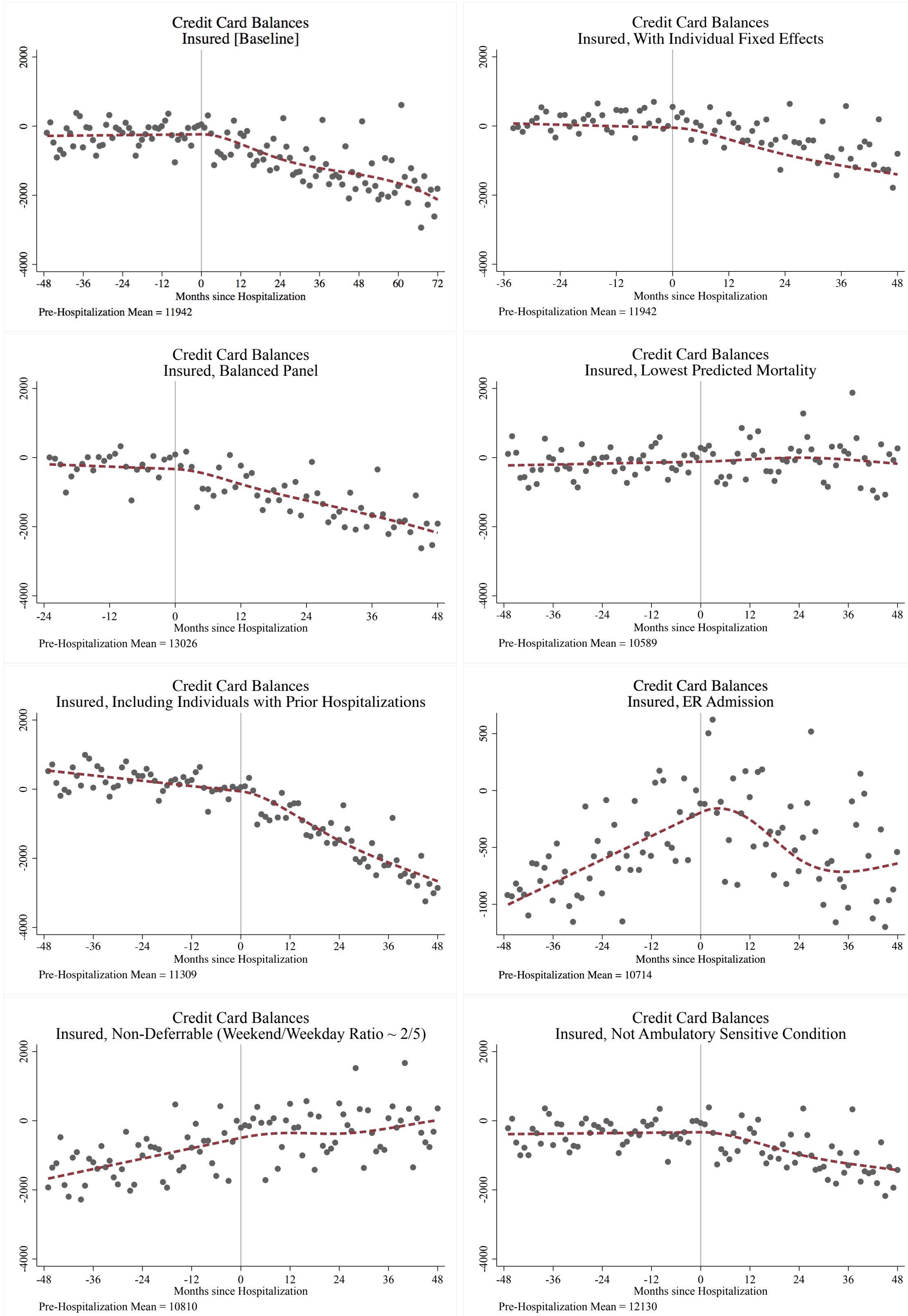
Notes: See notes to Appendix Figure 20.

Appendix Figure 24. Impact of Hospitalization on Credit Score for the Insured, Robustness



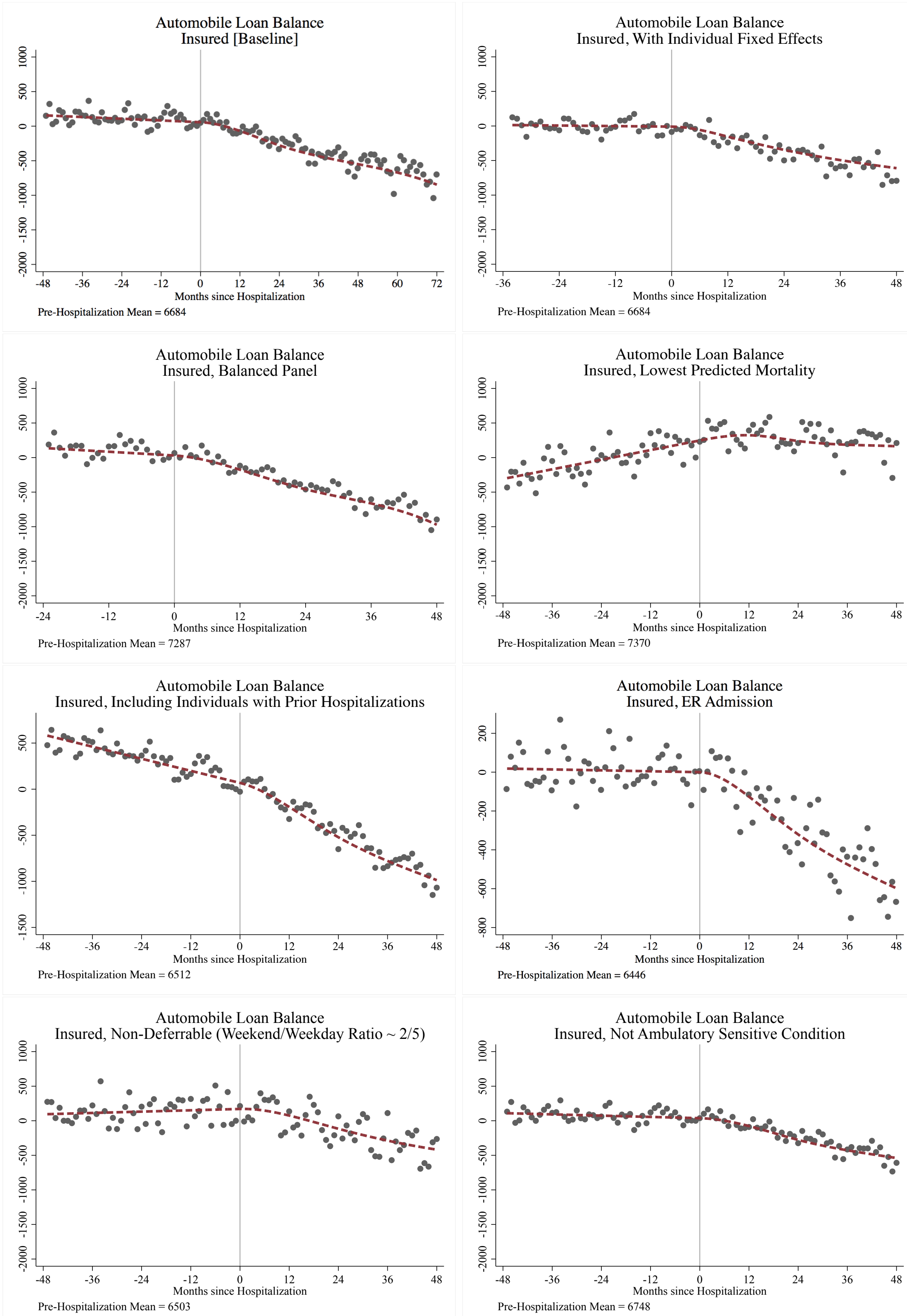
Notes: See notes to Appendix Figure 20.

Appendix Figure 25. Impact of Hospitalization on Credit Card Balances for the Insured, Robustness



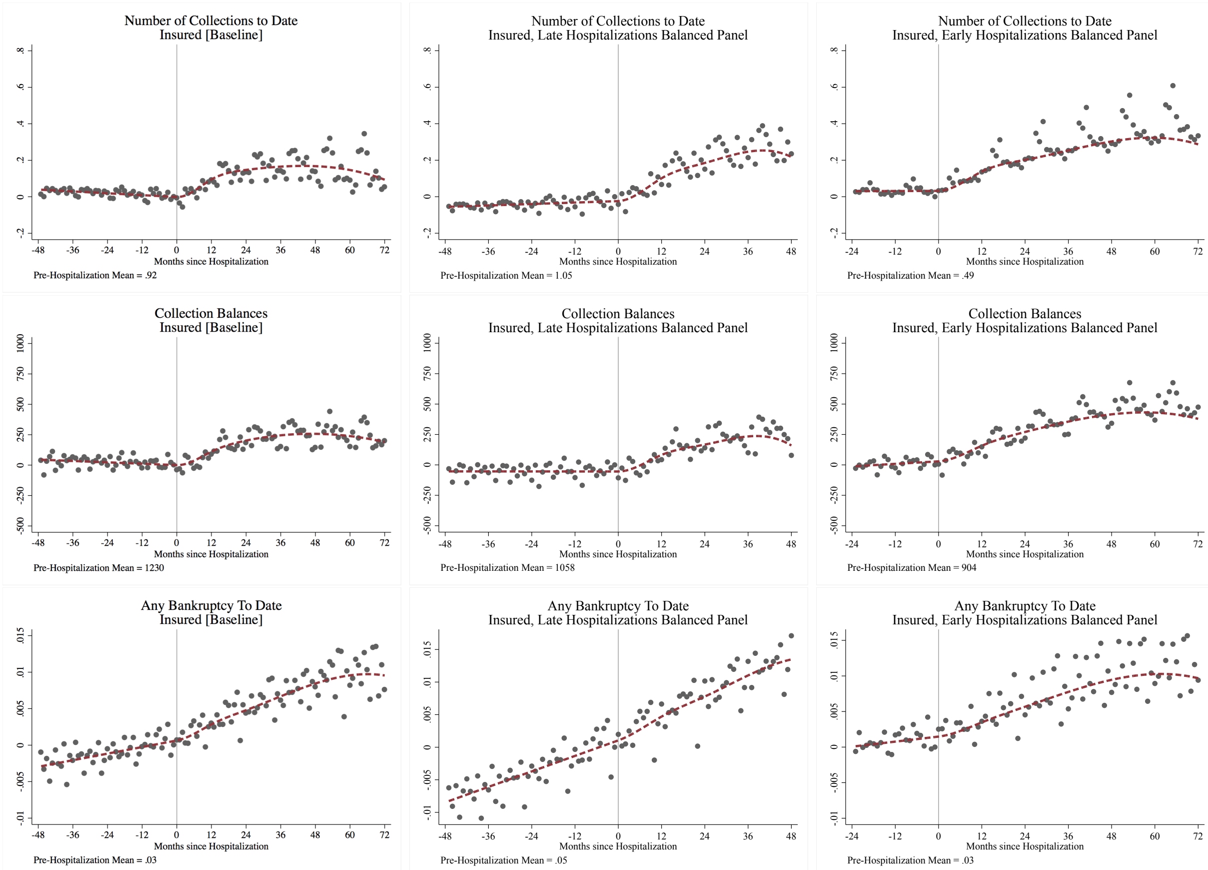
Notes: See notes to Appendix Figure 20.

Appendix Figure 26. Impact of Hospitalization on Automobile Loan Balance for the Insured, Robustness



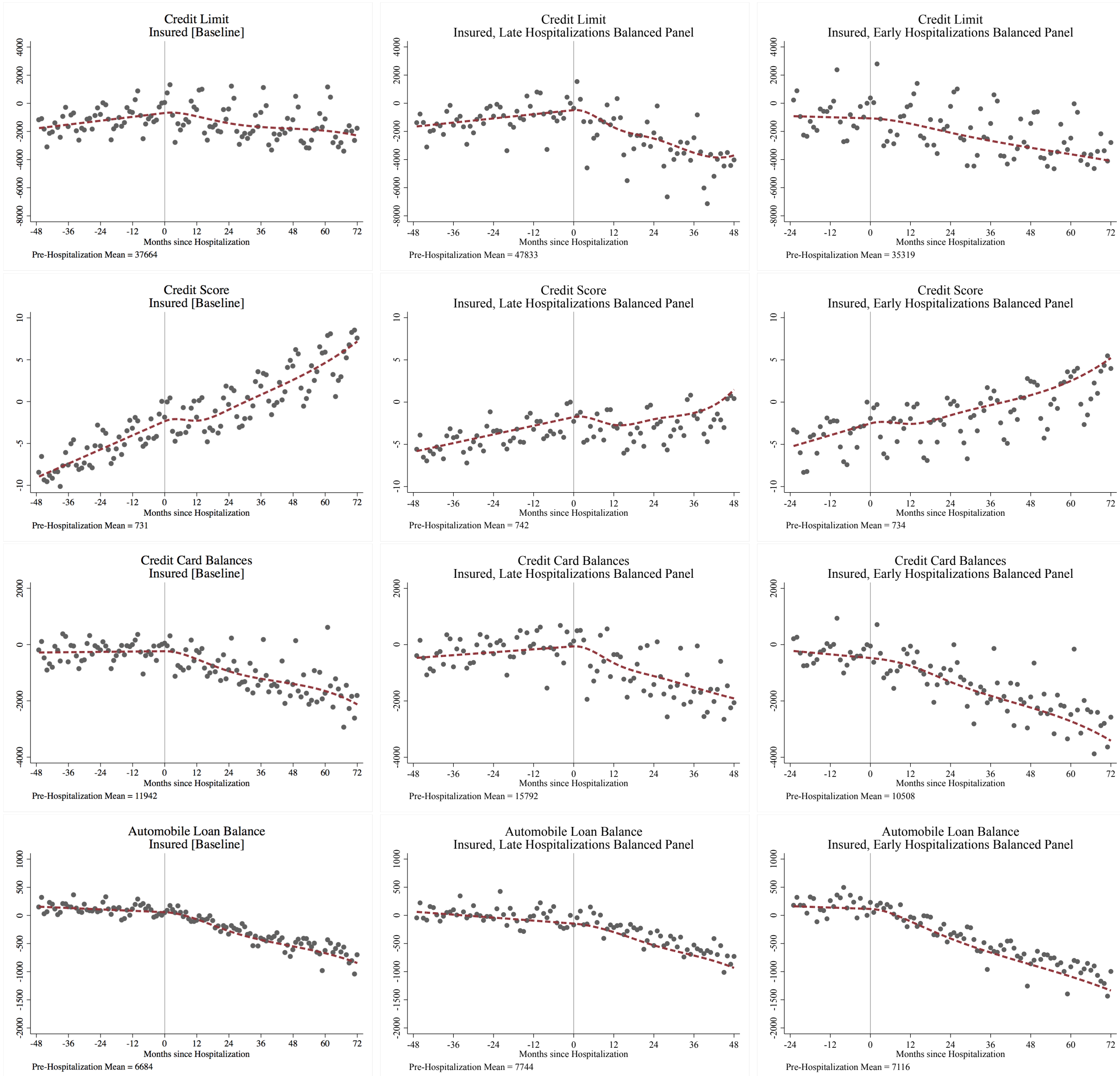
Notes: See notes to Appendix Figure 20.

Appendix Figure 27. Impact of Hospitalization on Collections and Bankruptcy,
Early and Late Hospitalizations Balanced Panels



Notes: The baseline sample is the non-elderly insured (see Table 1, column 2). The top left figure displays the baseline specification. The two departures from the baseline include limiting the sample to late hospitalizations (2005-2007), so that we observe at least 4 years of pre-hospitalization outcomes and limiting the sample to a balanced panel of early hospitalizations (2003-2005), so that we observe at least 6 years of post-hospitalization credit report outcomes.

Appendix Figure 28. Impact of Hospitalization on Other Credit Report Outcomes,
Early and Late Hospitalizations Balanced Panels



Notes: The baseline sample is the non-elderly insured (see Table 1, column 2). The top left figure displays the baseline specification. The two departures from the baseline include limiting the sample to late hospitalizations (2005-2007), so that we observe at least 4 years of pre-hospitalization outcomes and limiting the sample to a balanced panel of early hospitalizations (2003-2005), so that we observe at least 6 years of post-hospitalization credit report outcomes.

G.2.5 Heterogeneity Analysis for Insured

Appendix Table 23. Impact of Hospitalization, by Insurance Status and Chronic Diagnosis

	[Baseline]	Non-Elderly Privately Insured	Non-Elderly Medicaid	Insured, Chronic Primary Diagnosis	Insured, Some Chronic Diagnosis	Insured, No Chronic Diagnosis
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Number of Collections to Date			
12-month effect	.11	.11	.15	.11	.11	.11
	(.005)	(.004)	(.017)	(.007)	(.005)	(.011)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
48-month effect	.21	.23	.1	.28	.22	.2
	(.019)	(.019)	(.069)	(.028)	(.021)	(.048)
	[<.001]	[<.001]	[.14]	[<.001]	[<.001]	[<.001]
Pre-hospitalization mean	.92	.71	2.3	.87	.94	.83
			Panel B: Collections Balances			
12-month effect	122	118	201	134	129	91
	(13)	(13)	(45)	(19)	(14)	(30)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[.0026]
48-month effect	302	320	234	377	344	105
	(37)	(38)	(130)	(56)	(41)	(88)
	[<.001]	[<.001]	[.071]	[<.001]	[<.001]	[.23]
Pre-hospitalization mean	1,230	948	3,119	1,213	1,260	1,058
			Panel C: Any Bankruptcy to Date			
12-month effect	.0013	.0015	.00019	.0021	.0014	.0012
	(.00031)	(.00033)	(.00083)	(.00047)	(.00034)	(.00074)
	[<.001]	[<.001]	[.82]	[<.001]	[<.001]	[.11]
48-month effect	.0042	.0048	-.00014	.0057	.004	.0065
	(.00092)	(.00099)	(.0025)	(.0014)	(.001)	(.0022)
	[<.001]	[<.001]	[.95]	[<.001]	[<.001]	[.0035]
Pre-hospitalization mean	.034	.034	.038	.035	.035	.028
			Panel D: Credit Limit			
12-month effect	-515	-692	-178	-1,119	-675	143
	(154)	(176)	(114)	(236)	(167)	(404)
	[<.001]	[<.001]	[.12]	[<.001]	[<.001]	[.72]
48-month effect	-2,215	-2,698	365	-3,458	-2,571	-1,473
	(440)	(501)	(346)	(667)	(474)	(1,178)
	[<.001]	[<.001]	[.29]	[<.001]	[<.001]	[.21]
Pre-hospitalization mean	37,664	42,256	6,954	39,693	38,032	35,510
			Panel E: Credit Score			
12-month effect	-1.6	-2	-1.7	-1.9	-1.7	-1.4
	(.2)	(.2)	(.4)	(.3)	(.2)	(.5)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[.0015]
48-month effect	-1.8	-2.8	2.1	-2.3	-1.8	-2.4
	(.5)	(.5)	(1.1)	(.7)	(.5)	(1.2)
	[<.001]	[<.001]	[.057]	[<.001]	[<.001]	[.041]
Pre-hospitalization mean	731	743	634	736	732	728
			Panel F: Credit Card Balances			
12-month effect	-293	-359	-111	-637	-347	-134
	(94)	(107)	(71)	(142)	(101)	(257)
	[.0018]	[<.001]	[.12]	[<.001]	[<.001]	[.6]
48-month effect	-1,208	-1,418	53	-2,063	-1,424	-634
	(253)	(288)	(198)	(382)	(272)	(681)
	[<.001]	[<.001]	[.79]	[<.001]	[<.001]	[.35]
Pre-hospitalization mean	11,942	13,364	2,437	12,505	12,046	11,338
			Panel G: Automobile Loan Balance			
12-month effect	-102	-118	-104	-119	-133	30
	(28)	(32)	(43)	(42)	(30)	(79)
	[<.001]	[<.001]	[.015]	[.0041]	[<.001]	[.7]
48-month effect	-507	-637	195	-486	-542	-509
	(71)	(80)	(112)	(105)	(77)	(195)
	[<.001]	[<.001]	[.082]	[<.001]	[<.001]	[.0091]
Pre-hospitalization mean	6,684	7,288	2,646	6,666	6,628	7,010
Number of Individuals	383,718	314,393	69,325	187,160	335,930	47,788
Number of Observations	3,131,534	2,597,860	533,674	1,499,636	2,723,765	407,769

Notes: Column 1 replicates results for the non-elderly insured (see Tables 5 and 6); columns 2 and 3 report results respectively for the sub-samples with private insurance and Medicaid at the time of the index hospital admission; in columns 4 through 6, we additionally split the sample by whether an individual's hospitalization had an ICD-9 diagnosis code associated with a chronic illness. We classify individual ICD-9 diagnosis codes as chronic or non chronic using a crosswalk developed by the Health Care Utitlization 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). All columns report effects based on OLS estimates of equation (5). 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.

Appendix Table 24. Impacts for the Insured, by Type of Hospitalization

	[Baseline]	Lowest Quartile of Predicted Charges	Highest Quartile of Predicted Charges	Public Hospital	Non-Profit Hospital	For-Profit Hospital	Late Hospitalizations (2005-2007)	Early Hospitalizations (2003-2005)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Number of Collections to Date								
12-month effect	.11	.093	.17	.18	.089	.13	.12	.11
	(.005)	(.008)	(.01)	(.017)	(.005)	(.012)	(.009)	(.005)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
48-month effect	.21	.19	.27	.23	.19	.25	.21	.27
	(.019)	(.034)	(.044)	(.072)	(.021)	(.052)	(.048)	(.031)
	[<.001]	[<.001]	[<.001]	[.0018]	[<.001]	[<.001]	[<.001]	[<.001]
Pre-hospitalization mean	.92	.83	1	1.4	.84	1	1.1	.49
Panel B: Collection Balances								
12-month effect	122	111	187	274	83	211	136	108
	(13)	(23)	(31)	(47)	(14)	(33)	(23)	(17)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
48-month effect	302	217	434	447	234	501	215	304
	(37)	(64)	(86)	(129)	(43)	(94)	(89)	(65)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[.016]	[<.001]
Pre-hospitalization mean	1,230	1,106	1,451	1,877	1,120	1,367	1,058	904
Panel C: Any Bankruptcy to Date								
12-month effect	.0013	.0014	.0013	.004	.0011	.00072	.0013	.0013
	(.00031)	(.00056)	(.00067)	(.0011)	(.00035)	(.00078)	(.00054)	(.00049)
	[<.001]	[.015]	[.048]	[<.001]	[.0023]	[.35]	[.018]	[.0087]
48-month effect	.0042	.0044	.0045	.008	.004	.0026	.003	.005
	(.00092)	(.0017)	(.002)	(.0031)	(.0011)	(.0024)	(.0022)	(.002)
	[<.001]	[.0094]	[.027]	[.01]	[<.001]	[.28]	[.17]	[.011]
Pre-hospitalization mean	.034	.033	.034	.04	.034	.035	.045	.027
Panel D: Credit Limit								
12-month effect	-515	20	-1,286	-839	-526	-208	-1,520	-299
	(154)	(310)	(300)	(339)	(186)	(369)	(305)	(250)
	[<.001]	[.95]	[<.001]	[.013]	[.0047]	[.57]	[<.001]	[.23]
48-month effect	-2,215	-1,328	-4,078	-2,455	-2,461	-532	-4,405	-1,764
	(440)	(860)	(864)	(986)	(530)	(1,044)	(1,046)	(896)
	[<.001]	[.12]	[<.001]	[.013]	[<.001]	[.61]	[<.001]	[.049]
Pre-hospitalization mean	37,664	40,693	34,454	23,179	40,071	34,870	47,833	35,319
Panel E: Credit Score								
12-month effect	-1.6	-1.6	-2.2	-2	-1.5	-2	-1.9	-1.4
	(.18)	(.33)	(.39)	(.55)	(.21)	(.44)	(.29)	(.29)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
48-month effect	-1.8	-1.7	-2.7	1.1	-2	-2.1	-.87	-2.2
	(.45)	(.83)	(1)	(1.4)	(.52)	(1.1)	(.96)	(.93)
	[<.001]	[.044]	[.0073]	[.46]	[<.001]	[.058]	[.36]	[.017]
Pre-hospitalization mean	731	737	726	693	737	723	742	734
Panel F: Credit Card Balances								
12-month effect	-293	-200	-609	-257	-329	-122	-695	-147
	(94)	(191)	(176)	(206)	(114)	(217)	(193)	(157)
	[.0018]	[.29]	[<.001]	[.21]	[.0039]	[.57]	[<.001]	[.35]
48-month effect	-1,208	-906	-1,593	-1,094	-1,393	-249	-2,266	-1,241
	(253)	(499)	(476)	(571)	(304)	(606)	(662)	(513)
	[<.001]	[.069]	[<.001]	[.055]	[<.001]	[.68]	[<.001]	[.015]
Pre-hospitalization mean	11,942	13,058	10,561	7,648	12,641	11,182	15,792	10,508
Panel G: Automobile Loan Balance								
12-month effect	-102	-127	-151	-138	-83	-171	-98	-196
	(28)	(55)	(55)	(82)	(33)	(74)	(53)	(49)
	[<.001]	[.02]	[.0056]	[.091]	[.012]	[.021]	[.066]	[<.001]
48-month effect	-507	-672	-264	-501	-466	-710	-575	-889
	(71)	(138)	(137)	(207)	(83)	(182)	(162)	(162)
	[<.001]	[<.001]	[.053]	[.016]	[<.001]	[<.001]	[<.001]	[<.001]
Pre-hospitalization mean	6,684	7,138	5,889	5,629	6,737	7,049	7,744	7,116
Number of Individuals	383,718	102,015	97,270	42,280	279,141	62,206	158,321	182,050
Number of Observations	3,131,534	855,120	753,075	337,166	2,284,893	508,702	1,266,568	1,456,400

Notes: All samples are subsets of the sample of non-elderly insured (see Table 1, column 2). Column 1 replicates results in Tables 5 and 6. Charges are predicted using an individual's Major Diagnostic Category (MDC) code and length of stay. The remaining subsamples are defined using characteristics of the index hospitalization. All columns report effects based on OLS estimates of equation (5). 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.

Appendix Table 25. Impacts for the Insured, by Diagnosis for Admission

	[Baseline]	MDC5: Circulatory Systems	MDC6: Digestive Systems	MDC8: Musculoskeletal Systems	MDC4: Respiratory System	MDC1: Nervous System	Car Accidents	External Injuries	Cancer	Acute Myocardial Infarction
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Number of Collections to Date										
12-month effect	.11	.063	.069	.092	.16	.15	.25	.2	.022	.067
	(.005)	(.009)	(.011)	(.011)	(.02)	(.017)	(.031)	(.012)	(.011)	(.021)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[.058]	[.0013]
48-month effect	.21	0	.13	.25	.35	.28	.37	.41	-.021	.033
	(.019)	(.04)	(.047)	(.046)	(.082)	(.072)	(.13)	(.051)	(.048)	(.089)
	[<.001]	[.99]	[.0069]	[<.001]	[<.001]	[<.001]	[.0029]	[<.001]	[.66]	[.71]
Pre-hospitalization mean	.92	.84	.83	.7	1.1	.95	1.1	1	.63	.72
Panel B: Collections Balances										
12-month effect	122	91	44	39	167	284	463	267	-19	277
	(13)	(31)	(33)	(30)	(61)	(52)	(102)	(36)	(36)	(92)
	[<.001]	[.0032]	[.18]	[.19]	[.0066]	[<.001]	[<.001]	[<.001]	[.6]	[.0027]
48-month effect	302	31	1	204	395	856	991	658	-42	382
	(37)	(90)	(100)	(90)	(152)	(148)	(288)	(101)	(101)	(235)
	[<.001]	[.73]	[1]	[.024]	[.0092]	[<.001]	[<.001]	[<.001]	[.68]	[.1]
Pre-hospitalization mean	1,230	1,309	1,098	954	1,514	1,254	1,464	1,352	801	1,196
Panel C: Any Bankruptcy to Date										
12-month effect	.0013	.00059	.0016	.0018	.0018	.00087	.00009	.002	-.0015	.0017
	(.00031)	(.00068)	(.00075)	(.00081)	(.0012)	(.0012)	(.0018)	(.00078)	(.00088)	(.0016)
	[<.001]	[.38]	[.032]	[.028]	[.14]	[.46]	[.96]	[.009]	[.086]	[.28]
48-month effect	.0042	.0012	.0063	.0081	.0076	.0036	.0075	.0072	-.0021	.0073
	(.00092)	(.002)	(.0023)	(.0024)	(.0038)	(.0035)	(.0056)	(.0024)	(.0027)	(.0048)
	[<.001]	[.56]	[.005]	[<.001]	[.043]	[.3]	[.18]	[.0023]	[.43]	[.13]
Pre-hospitalization mean	.034	.036	.03	.029	.037	.035	.035	.033	.031	.033
Panel D: Credit Limit										
12-month effect	-515	-265	376	-1,259	-873	-1,148	-1,582	-647	-354	-186
	(154)	(369)	(407)	(505)	(563)	(561)	(726)	(379)	(493)	(829)
	[<.001]	[.47]	[.36]	[.013]	[.12]	[.041]	[.029]	[.088]	[.47]	[.82]
48-month effect	-2,215	-1,390	-459	-3,996	-5,955	-4,391	-4,346	-2,131	-1,082	-3,023
	(440)	(1,017)	(1,159)	(1,476)	(1,640)	(1,580)	(2,199)	(1,108)	(1,440)	(2,206)
	[<.001]	[.17]	[.69]	[.0068]	[<.001]	[.0055]	[.048]	[.054]	[.45]	[.17]
Pre-hospitalization mean	37,664	40,583	38,857	46,203	33,376	36,226	28,401	35,902	43,864	40,408
Panel E: Credit Score										
12-month effect	-1.6	-.81	-1.1	-1.6	-3	-2.4	-1.7	-2.7	-.6	-.48
	(.18)	(.4)	(.45)	(.51)	(.7)	(.69)	(1)	(.47)	(.53)	(.98)
	[<.001]	[.043]	[.016]	[.0023]	[<.001]	[<.001]	[.11]	[<.001]	[.25]	[.62]
48-month effect	-1.8	.95	-1.7	-3.1	-6.6	-2.4	6.7	-1.4	-5.7	-1.1
	(.45)	(1)	(1.2)	(1.3)	(1.8)	(1.8)	(2.6)	(1.2)	(1.4)	(2.4)
	[<.001]	[.35]	[.14]	[.016]	[<.001]	[.18]	[.012]	[.23]	[<.001]	[.65]
Pre-hospitalization mean	731	737	736	752	720	729	704	725	755	743
Panel F: Credit Card Balances										
12-month effect	-293	-319	191	-650	-267	-422	-1,099	-378	38	-726
	(94)	(221)	(252)	(304)	(326)	(346)	(499)	(234)	(291)	(527)
	[.0018]	[.15]	[.45]	[.032]	[.41]	[.22]	[.028]	[.11]	[.9]	[.17]
48-month effect	-1,208	-1,074	61	-2,565	-1,798	-2,792	-2,717	-681	-105	-1,617
	(253)	(574)	(684)	(825)	(891)	(897)	(1,271)	(647)	(795)	(1,241)
	[<.001]	[.061]	[.93]	[.0019]	[.044]	[.0019]	[.033]	[.29]	[.89]	[.19]
Pre-hospitalization mean	11,942	12,598	12,147	14,473	10,233	11,558	9,782	11,518	13,149	12,214
Panel G: Automobile Loan Balance										
12-month effect	-102	-132	-205	-105	31	-201	149	-62	-144	-158
	(28)	(66)	(75)	(86)	(1.0e+02)	(99)	(188)	(75)	(85)	(153)
	[<.001]	[.046]	[.0061]	[.22]	[.75]	[.043]	[.43]	[.41]	[.088]	[.3]
48-month effect	-507	-640	-747	-423	-488	-442	-1,487	-643	-439	-783
	(71)	(163)	(193)	(212)	(254)	(251)	(439)	(183)	(214)	(389)
	[<.001]	[<.001]	[<.001]	[.046]	[.054]	[.078]	[<.001]	[<.001]	[.04]	[.044]
Pre-hospitalization mean	6,684	7,184	6,949	7,083	5,871	6,314	7,235	6,415	6,451	7,329
Number of Individuals	383,718	70,289	54,259	42,577	29,082	28,182	9,854	56,937	42,111	13,186
Number of Observations	3,131,534	585,171	447,302	359,616	221,266	223,632	83,291	463,004	310,528	109,425

Notes: All samples are subsets of the non-elderly insured (see Table 1, column 2). Column 1 replicates results for the non-elderly insured (see Tables 5 and 6). Subsamples are defined using characteristics of the index hospitalization. All columns report effects based on OLS estimates of equation (5). 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.

Appendix Table 26. Quantile Effects for the Insured

	Mean Effect Estimates (OLS)	Unconditional Quantile Effect Estimates					
		25th Percentile	Median	75th Percentile	90th Percentile	95th Percentile	99th Percentile
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Collection Balances							
12-month effect	122	.8	.8	102	318	476	2,427
	(13)	(.042)	(.042)	(5.8)	(33)	(73)	(463)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
48-month effect	302	1.2	1.2	153	591	1,292	7,021
	(37)	(.12)	(.12)	(17)	(94)	(212)	(1,383)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
Pre-hospitalization mean/percentile	1,230	0	0	336	3,625	8,093	28,024
Panel B: Credit Limit							
12-month effect	-515	-55	-386	-657	-2,265	-3,234	-1,847
	(154)	(12)	(78)	(203)	(632)	(1,522)	(4,741)
	[<.001]	[<.001]	[<.001]	[.0012]	[<.001]	[.034]	[.7]
48-month effect	-2,215	-242	-1,354	-1,990	-5,624	-7,552	-7,004
	(440)	(36)	(229)	(571)	(1,759)	(4,345)	(14,642)
	[<.001]	[<.001]	[<.001]	[<.001]	[.0014]	[.082]	[.63]
Pre-hospitalization mean/percentile	37,664	800	15,000	49,818	118,200	193,700	419,220
Panel C: Credit Score							
12-month effect	-1.6	-3.1	-1.1	-1.6	-.87	-.43	0
	(.18)	(.42)	(.39)	(.38)	(.34)	(.52)	(0)
	[<.001]	[<.001]	[.0049]	[<.001]	[.011]	[.42]	[.]
48-month effect	-1.8	-2.1	.6	-1.7	-1.3	.013	0
	(.45)	(1.1)	(1.1)	(1)	(.89)	(1.4)	(0)
	[<.001]	[.046]	[.57]	[.094]	[.16]	[.99]	[.]
Pre-hospitalization mean/percentile	731	635	737	847	903	933	990
Panel D: Credit Card Balances							
12-month effect	-293	0	-59	-245	-1,207	-2,593	-5,783
	(94)	(0)	(17)	(74)	(362)	(824)	(3,060)
	[.0018]	[.]	[<.001]	[<.001]	[<.001]	[.0017]	[.059]
48-month effect	-1,208	0	-273	-910	-3,848	-10,947	-9,393
	(253)	(0)	(44)	(190)	(947)	(2,207)	(8,748)
	[<.001]	[.]	[<.001]	[<.001]	[<.001]	[<.001]	[.28]
Pre-hospitalization mean/percentile	11,942	0	2,039	10,533	37,998	77,506	223,819
Panel E: Automobile Loan Balances							
12-month effect	-102	0	0	-233	-360	-240	-81
	(28)	(0)	(0)	(80)	(110)	(174)	(489)
	[<.001]	[.]	[.]	[.0034]	[.001]	[.17]	[.87]
48-month effect	-507	0	0	-1,326	-1,488	-2,061	-3,161
	(71)	(0)	(0)	(202)	(258)	(394)	(1,022)
	[<.001]	[.]	[.]	[<.001]	[<.001]	[<.001]	[.002]
Pre-hospitalization mean/percentile	6,684	0	0	9,128	21,745	30,633	54,001

Notes: This table presents results of unconditional quantile effect estimates for the continuous outcomes reported in Tables 5 and 6 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 (clustered on the individual) are in parentheses and p-values are in brackets. The maximum credit score occurs below the 99th percentile, so that there is no impact on the 99th percentile. All estimates are weighted to adjust for individuals' sampling probabilities.

Appendix Table 27. Poisson Regression Impacts on Collections

	Number of Collections to Date			Collection Balances		
	All	Medical	Non-Medical	All	Medical	Non-Medical
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A. Non-Elderly Insured			
12-month effect	.07	.24	.013	.088	.35	.017
	(.0022)	(.0052)	(.0022)	(.0089)	(.018)	(.014)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[.24]
48-month effect	.12	.37	.038	.2	.66	.086
	(.0074)	(.016)	(.0078)	(.026)	(.052)	(.042)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[.039]
Pre-hospitalization mean	.92	.2	.72	1,230	292	1,086
Number of Individuals	383,718	383,718	383,718	383,718	375,844	375,844
Number of Observations	3,131,534	3,131,534	3,131,534	3,131,534	2,208,517	2,208,517
			Panel B. Non-Elderly Uninsured			
12-month effect	.2	.5	.031	.66	1.1	.08
	(.0025)	(.0057)	(.0022)	(.0079)	(.014)	(.012)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
48-month effect	.18	.47	-.0018	.79	1.1	.054
	(.008)	(.018)	(.0076)	(.02)	(.043)	(.034)
	[<.001]	[<.001]	[.81]	[<.001]	[<.001]	[.12]
Pre-hospitalization mean	2.3	.59	1.7	3,529	1,292	2,762
Number of Individuals	153,617	153,617	153,617	153,617	151,343	151,343
Number of Observations	1,256,759	1,256,759	1,256,759	1,256,759	913,516	913,516
			Panel C. Elderly			
12-month effect	.092	.34	.022	.061	.24	.0091
	(.0048)	(.011)	(.0049)	(.017)	(.04)	(.026)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[.73]
48-month effect	.19	.64	.056	.2	.51	.094
	(.016)	(.035)	(.016)	(.05)	(.11)	(.079)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[.23]
Pre-hospitalization mean	.24	.048	.19	428	75	422
Number of Individuals	414,547	414,547	414,547	414,547	387,839	387,839
Number of Observations	2,959,802	2,959,802	2,959,802	2,959,802	1,946,208	1,946,208

Notes: Samples are non-elderly insured, uninsured, and the elderly (see Appendix Table 13, columns 3, 6, and 9). All columns report the implied effects at 12 and 48 months based on Poisson regression estimates of equation (5). 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.

Appendix Table 28. Poisson Regression Impacts on Other Credit Report Outcomes

	Credit Limit	Credit Score	Credit Card Balances	Automobile Loan Balance
	(1)	(2)	(3)	(4)
Panel A. Non-Elderly Insured				
12-month effect	-.019	-.014	-.0023	-.014
	(.0061)	(.0033)	(.00024)	(.0042)
	[.0015]	[<.001]	[<.001]	[<.001]
48-month effect	-.067	-.052	-.0026	-.079
	(.016)	(.0089)	(.00061)	(.011)
	[<.001]	[<.001]	[<.001]	[<.001]
Pre-hospitalization mean	11,942	37,664	731	6,684
Number of Individuals	383,718	383,718	371,715	383,718
Number of Observations	3,131,534	3,131,534	2,942,253	3,131,534
Panel B. Non-Elderly Uninsured				
12-month effect	-.045	-.045	-.0077	-.068
	(.013)	(.0073)	(.00038)	(.0074)
	[<.001]	[<.001]	[<.001]	[<.001]
48-month effect	-.086	-.067	.01	-.093
	(.031)	(.019)	(.00097)	(.019)
	[.0064]	[<.001]	[<.001]	[<.001]
Pre-hospitalization mean	5,376	15,145	655	3,981
Number of Individuals	153,617	153,617	137,913	153,617
Number of Observations	1,256,759	1,256,759	1,017,096	1,256,759
Panel C. Elderly				
12-month effect	.022	.013	-.0017	.029
	(.0092)	(.0033)	(.0002)	(.0085)
	[.017]	[<.001]	[<.001]	[<.001]
48-month effect	.041	.006	-.004	.05
	(.023)	(.0092)	(.00054)	(.022)
	[.08]	[.51]	[<.001]	[.024]
Pre-hospitalization mean	7,016	36,967	824	2,143
Number of Individuals	414,547	414,547	405,389	414,547
Number of Observations	2,959,802	2,959,802	2,833,027	2,959,802

Notes: Samples are non-elderly insured, uninsured, and the elderly (see Appendix Table 13, columns 3, 6, and 9). All columns report the implied effects at 12 and 48 months based on Poisson regression estimates of equation (5). 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.

Appendix Table 29. Impact of Hospitalization on Collections, Alternate Age Restriction for the Insured

	Number of Collections to Date			Collection Balances		
	All	Medical	Non-Medical	All	Medical	Non-Medical
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Non-Elderly Insured Ages 50 to 59						
12-month effect	.056	.064	-.007	65	92	1
	(.006)	(.003)	(.005)	(21)	(11)	(30)
	[<.001]	[<.001]	[.12]	[.0022]	[<.001]	[.97]
48-month effect	.062	.094	-.031	155	152	70
	(.025)	(.011)	(.019)	(64)	(27)	(89)
	[.013]	[<.001]	[.098]	[.015]	[<.001]	[.43]
Pre-hospitalization mean	.63	.14	.49	1,016	212	905
Number of Individuals	123,505	123,505	123,505	123,505	120,509	120,509
Number of Observations	999,610	999,610	999,610	999,610	701,085	701,085
Panel B. Non-Elderly Insured Ages 25 to 64						
12-month effect	.11	.095	.011	122	127	18
	(.005)	(.002)	(.003)	(13)	(7)	(16)
	[<.001]	[<.001]	[.0011]	[<.001]	[<.001]	[.26]
48-month effect	.21	.18	.034	302	271	101
	(.019)	(.008)	(.014)	(37)	(18)	(47)
	[<.001]	[<.001]	[.017]	[<.001]	[<.001]	[.03]
Pre-hospitalization mean	.92	.2	.72	1,230	292	1,086
Number of Individuals	383,718	383,718	383,718	383,718	375,844	375,844
Number of Observations	3,131,534	3,131,534	3,131,534	3,131,534	2,208,517	2,208,517

Notes: Samples are the baseline non-elderly insured credit report sample (see Table 1, column 2) and the non-elderly insured additionally restricted to those ages 50 to 59 at the time of hospitalization to match the HRS sample restriction. All columns report effects based on OLS estimates of equation (5). 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.

Appendix Table 30. Impact of Hospitalization on Other Credit Report Outcomes, Alternate Age Restriction for Non-Elderly Insured

	Any Bankruptcy to Date	Credit Limit	Credit Score	Credit Card Balances	Automobile Loan Balance
	(1)	(2)	(3)	(4)	(5)
Panel A. Non-Elderly Insured Ages 50 to 59					
12-month effect	.001	-137	-.4	-376	-62
	(.0005)	(292)	(.3)	(172)	(49)
	[.04]	[.64]	[.16]	[.029]	[.2]
48-month effect	.0027	-530	1.3	-1,238	-446
	(.0015)	(840)	(.8)	(470)	(122)
	[.077]	[.53]	[.11]	[.0085]	[<.001]
Pre-hospitalization mean	.031	46,578	756	14,114	6,792
Number of Individuals	123,505	123,505	119,798	123,505	123,505
Number of Observations	999,610	999,610	946,387	999,610	999,610
Panel B. Non-Elderly Insured Ages 25 to 64					
12-month effect	.0013	-515	-1.6	-293	-102
	(.00031)	(154)	(.2)	(94)	(28)
	[<.001]	[<.001]	[<.001]	[.0018]	[<.001]
48-month effect	.0042	-2,215	-1.8	-1,208	-507
	(.00092)	(440)	(.5)	(253)	(71)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
Pre-hospitalization mean	.034	37,664	731	11,942	6,684
Number of Individuals	383,718	383,718	371,715	383,718	383,718
Number of Observations	3,131,534	3,131,534	2,942,253	3,131,534	3,131,534

Notes: Samples are the baseline non-elderly insured credit report sample (see Table 1, column 2) and the non-elderly insured additionally restricted to those ages 50 to 59 at the time of hospitalization to match the HRS sample restriction. All columns report effects based on OLS estimates of equation (5). 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.

G.2.6 Robustness and Additional Analysis for the Uninsured and the Elderly

Appendix Table 31. Robustness to Alternative Specifications and Sample Restrictions for the Uninsured

	[Baseline]	Individual FEs	Balanced Panel	Lowest Predicted Mortality Quartile	Including Individuals with Prior Hospitalizations	ER Admissions	Non-Deferrable (Weekend/Weekday Ratio ~ 2/5)	Excluding Ambulatory Care Sensitive Conditions
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A: Number of Collections to Date				
12-month effect	.97	.98	.89	.91	1.1	1.1	1.1	.97
	(.012)	(.011)	(.013)	(.023)	(.012)	(.014)	(.027)	(.012)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
48-month effect	1.3	1.3	1.1	1	1.9	1.5	1.6	1.3
	(.045)	(.045)	(.061)	(.093)	(.045)	(.052)	(.1)	(.048)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
Pre-hospitalization mean	2.3	2.3	1.9	2.4	2.2	2.5	2.4	2.2
				Panel B: Collection Balances				
12-month effect	4,469	4,487	3,820	3,813	4,786	5,162	5,564	4,458
	(51)	(55)	(63)	(84)	(50)	(61)	(125)	(55)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
48-month effect	6,199	6,331	5,386	4,554	7,170	7,285	7,752	6,142
	(130)	(138)	(181)	(215)	(128)	(155)	(312)	(138)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
Pre-hospitalization mean	3,529	3,529	2,690	3,190	3,658	3,897	3,781	3,444
				Panel C: Any Bankruptcy to Date				
12-month effect	.0048	.0055	.0083	.0044	.0049	.0049	.0071	.0048
	(.00046)	(.00047)	(.00071)	(.0009)	(.00044)	(.00053)	(.0011)	(.00049)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
48-month effect	.014	.014	.016	.013	.013	.014	.018	.014
	(.0014)	(.0014)	(.0022)	(.0027)	(.0013)	(.0016)	(.0031)	(.0015)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
Pre-hospitalization mean	.037	.037	.046	.032	.036	.039	.037	.037
				Panel D: Credit Limit				
12-month effect	-678	-462	-955	-249	-807	-908	-1,212	-690
	(131)	(139)	(228)	(289)	(122)	(96)	(190)	(143)
	[<.001]	[<.001]	[<.001]	[.39]	[<.001]	[<.001]	[<.001]	[<.001]
48-month effect	-690	-378	-1,022	-564	-1,374	-899	-1,231	-801
	(353)	(396)	(684)	(767)	(330)	(269)	(544)	(385)
	[.051]	[.34]	[.14]	[.46]	[<.001]	[<.001]	[.024]	[.037]
Pre-hospitalization mean	15,145	15,145	21,185	13,535	14,634	10,320	10,583	15,868
				Panel E: Credit Score				
12-month effect	-5	-3.8	-3.6	-2.7	-4.9	-5.3	-6.5	-5
	(.25)	(.24)	(.36)	(.46)	(.24)	(.27)	(.53)	(.26)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
48-month effect	6.6	8.4	10	9.8	7.1	6.8	4.4	6.7
	(.63)	(.64)	(.99)	(1.2)	(.6)	(.7)	(1.4)	(.67)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[.0012]	[<.001]
Pre-hospitalization mean	655	655	667	642	653	641	643	658
				Panel F: Credit Card Balances				
12-month effect	-264	-210	-411	-108	-332	-434	-532	-268
	(83)	(92)	(149)	(184)	(78)	(63)	(130)	(90)
	[.0014]	[.022]	[.006]	[.56]	[<.001]	[<.001]	[<.001]	[.003]
48-month effect	-443	-408	-570	-355	-793	-582	-774	-532
	(214)	(250)	(440)	(477)	(197)	(171)	(353)	(232)
	[.038]	[.1]	[.2]	[.46]	[<.001]	[<.001]	[.029]	[.022]
Pre-hospitalization mean	5,376	5,376	7,520	5,178	5,224	3,740	3,872	5,608
				Panel G: Automobile Loan Balance				
12-month effect	-267	-233	-260	-339	-279	-262	-233	-273
	(29)	(30)	(48)	(63)	(27)	(29)	(59)	(31)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
48-month effect	-349	-314	-461	-508	-360	-270	-321	-412
	(73)	(84)	(141)	(161)	(69)	(76)	(152)	(78)
	[<.001]	[<.001]	[.0011]	[.0016]	[<.001]	[<.001]	[.035]	[<.001]
Pre-hospitalization mean	3,981	3,981	5,526	4,565	3,929	3,539	3,733	4,066
Number of Individuals	153,617	153,617	94,419	36,104	170,350	122,513	32,282	137,600
Number of Observations	1,256,759	1,256,759	566,514	314,542	1,386,227	996,209	262,138	1,126,876

Notes: Column 1 replicates results for the non-elderly uninsured (see Tables 5 and 6 and notes to Table 5 for details). All other columns indicate specific departures from the baseline sample and specification as follows: Column 2 adds individual fixed effects to the estimating equation (see equation (17)). Column 3 limits the analysis to a balanced panel of individuals with non-missing data for the two years before and four years after their hospitalization. Column 4 restricts the sample to individuals in the lowest quartile of predicted mortality risk based on age and diagnosis-related group for the index admission. Column 5 adds back to the baseline sample insured individuals who had a prior hospital admission within the last three years. Column 6 restricts the sample to admissions through the emergency room. Column 7 restricts to non-deferrable admissions, which are limited to the subset of admissions that originate through the ER and have an ICD-9 code as the primary diagnosis that has weekend to weekday frequencies closest to the 2:5 ratio that we would expect if there is no delay in care. Column 8 excludes admissions for "ambulatory care sensitive conditions."

Appendix Table 32. Quantile Effects for the Uninsured

	Mean Effect Estimates (OLS)	Unconditional Quantile Effect Estimates					
		25th Percentile	Median	75th Percentile	90th Percentile	95th Percentile	99th Percentile
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Collection Balances							
12-month effect	4,469	139	1,038	6,191	17,184	22,684	41,969
	(51)	(2.6)	(15)	(68)	(218)	(404)	(1,732)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
48-month effect	6,199	93	786	6,790	22,752	32,969	83,358
	(130)	(6.8)	(38)	(177)	(587)	(1,120)	(5,044)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
Pre-hospitalization mean/percentile	3,529	0	1,091	6,715	22,159	38,624	97,173
Panel B: Credit Limit							
12-month effect	-678	0	0	-1,397	-2,345	-4,178	2,234
	(131)	(0)	(0)	(104)	(365)	(922)	(4,025)
	[<.001]	[.]	[.]	[<.001]	[<.001]	[<.001]	[.58]
48-month effect	-690	0	0	-930	-1,849	-3,905	13,285
	(353)	(0)	(0)	(305)	(1,039)	(2,541)	(11,634)
	[.051]	[.]	[.]	[.0023]	[.075]	[.12]	[.25]
Pre-hospitalization mean/percentile	15,145	0	0	8,700	42,000	82,500	261,300
Panel C: Credit Score							
12-month effect	-5	-4.9	-6.1	-7	-7.1	-2.8	-.075
	(.3)	(.34)	(.52)	(.68)	(.94)	(.72)	(1.9)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[.97]
48-month effect	6.6	8.6	11	8.8	-2.2	-2.6	10
	(.6)	(.83)	(1.3)	(1.9)	(2.5)	(1.9)	(5.3)
	[<.001]	[<.001]	[<.001]	[<.001]	[.38]	[.17]	[.055]
Pre-hospitalization mean/percentile	655	567	627	729	834	881	949
Panel D: Credit Card Balances							
12-month effect	-264	0	0	-373	-864	-1,548	-3,738
	(83)	(0)	(0)	(30)	(149)	(472)	(3,925)
	[.0014]	[.]	[.]	[<.001]	[<.001]	[.001]	[.34]
48-month effect	-443	0	0	-163	-1,648	-4,115	-3,843
	(214)	(0)	(0)	(78)	(380)	(1,209)	(10,724)
	[.038]	[.]	[.]	[.036]	[<.001]	[<.001]	[.72]
Pre-hospitalization mean/percentile	5,376	0	0	1,891	11,355	26,394	124,528
Panel E: Automobile Loan Balances							
12-month effect	-267	0	0	0	-1,005	-1,078	-1,387
	(29)	(0)	(0)	(0)	(129)	(195)	(616)
	[<.001]	[.]	[.]	[.]	[<.001]	[<.001]	[.024]
48-month effect	-349	0	0	0	-1,241	-1,596	-2,891
	(73)	(0)	(0)	(0)	(323)	(464)	(1,369)
	[<.001]	[.]	[.]	[.]	[<.001]	[<.001]	[.035]
Pre-hospitalization mean/percentile	3,981	0	0	0	13,915	22,114	43,896

Notes: Sample is the non-elderly uninsured (see Appendix Table 13, column 6). This table presents results of unconditional quantile effect estimates for the continuous outcomes reported in Tables 5 and 6. The estimation follows the two-step procedure described in Firpo et al. (2009). 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.

Appendix Table 33. Robustness to Alternative Specifications and Sample Restrictions for the Elderly

	[Baseline]	Individual FEs	Balanced Panel	Lowest Predicted Mortality Quartile	Including Individuals with Prior Hospitalizations	ER Admissions	Non-Deferrable (Weekend/Weekday Ratio ~ 2/5)	Excluding Ambulatory Care Sensitive Conditions
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A: Number of Collections to Date				
12-month effect	.027	.031	.035	.014	.048	.045	.037	.023
	(.002)	(.002)	(.002)	(.004)	(.002)	(.003)	(.006)	(.003)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
48-month effect	.038	.055	.063	.008	.13	.089	.063	.026
	(.01)	(.009)	(.011)	(.018)	(.009)	(.013)	(.024)	(.011)
	[<.001]	[<.001]	[<.001]	[.67]	[<.001]	[<.001]	[.0091]	[.014]
Pre-hospitalization mean	.24	.24	.19	.26	.21	.27	.26	.23
				Panel B: Collection Balances				
12-month effect	24	28	27	9	38	48	52	22
	(8)	(8)	(10)	(15)	(7)	(9)	(18)	(9)
	[.0018]	[<.001]	[.0076]	[.54]	[<.001]	[<.001]	[.0033]	[.0093]
48-month effect	84	89	96	31	100	125	112	77
	(24)	(27)	(38)	(49)	(22)	(29)	(47)	(26)
	[<.001]	[<.001]	[.012]	[.53]	[<.001]	[<.001]	[.017]	[.0034]
Pre-hospitalization mean	428	428	353	476	432	478	462	422
				Panel C: Any Bankruptcy to Date				
12-month effect	-.00019	-.00031	-.00023	-.0002	.00014	.00019	-.00047	-.00027
	(.00022)	(.0002)	(.00028)	(.00043)	(.0002)	(.00027)	(.00055)	(.00024)
	[.4]	[.13]	[.42]	[.64]	[.47]	[.48]	[.39]	[.26]
48-month effect	-.001	-.0013	-.0027	-.00047	-.00027	-.00027	-.00063	-.00082
	(.00072)	(.00067)	(.00092)	(.0013)	(.00065)	(.00088)	(.0017)	(.00077)
	[.16]	[.053]	[.003]	[.72]	[.68]	[.76]	[.71]	[.29]
Pre-hospitalization mean	.016	.016	.017	.02	.015	.018	.017	.016
				Panel D: Credit Limit				
12-month effect	370	-419	-899	-432	156	395	231	269
	(138)	(144)	(207)	(315)	(121)	(132)	(256)	(152)
	[.0073]	[.0036]	[<.001]	[.17]	[.2]	[.0028]	[.37]	[.077]
48-month effect	-448	-1,936	-3,566	-2,926	-936	-313	-931	-1,001
	(393)	(415)	(595)	(839)	(349)	(386)	(716)	(427)
	[.25]	[<.001]	[<.001]	[<.001]	[.0073]	[.42]	[.19]	[.019]
Pre-hospitalization mean	36,967	36,967	42,474	47,071	35,194	33,050	32,857	38,000
				Panel E: Credit Score				
12-month effect	-1.4	-1.3	-1.9	-.8	-1.7	-2.4	-1.4	-1.3
	(.17)	(.16)	(.23)	(.31)	(.15)	(.2)	(.42)	(.18)
	[<.001]	[<.001]	[<.001]	[.01]	[<.001]	[<.001]	[<.001]	[<.001]
48-month effect	-3.3	-3.6	-6.5	-3.5	-3.4	-4	-2.9	-3.6
	(.45)	(.42)	(.65)	(.79)	(.41)	(.54)	(1.1)	(.48)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]	[.0071]	[<.001]
Pre-hospitalization mean	824	824	829	818	822	818	819	825
				Panel F: Credit Card Balances				
12-month effect	72	-99	-293	-218	36	61	-159	35
	(73)	(82)	(114)	(169)	(63)	(68)	(132)	(80)
	[.32]	[.23]	[.011]	[.2]	[.57]	[.37]	[.23]	[.66]
48-month effect	-30	-306	-751	-870	-83	16	-398	-194
	(187)	(219)	(326)	(426)	(164)	(179)	(341)	(203)
	[.87]	[.16]	[.021]	[.041]	[.61]	[.93]	[.24]	[.34]
Pre-hospitalization mean	7,016	7,016	8,318	10,294	6,537	6,152	6,146	7,235
				Panel G: Automobile Loan Balance				
12-month effect	69	-10	5.3	-2.6	67	78	96	52
	(17)	(19)	(26)	(40)	(15)	(17)	(32)	(19)
	[<.001]	[.58]	[.84]	[.95]	[<.001]	[<.001]	[.0024]	[.0055]
48-month effect	194	32	58	100	248	166	181	159
	(43)	(52)	(76)	(101)	(38)	(44)	(80)	(47)
	[<.001]	[.54]	[.45]	[.32]	[<.001]	[<.001]	[.024]	[<.001]
Pre-hospitalization mean	2,143	2,143	2,584	3,277	2,044	1,923	1,880	2,201
Number of Individuals	414,547	414,547	238,366	101,665	562,020	289,322	78,974	354,306
Number of Observations	2,959,802	2,959,802	1,430,196	843,383	3,826,695	2,025,696	558,314	2,559,651

Notes: Column 1 replicates results for the elderly (see Tables 5 and 6 and notes to Table 5 for details). All other columns indicate specific departures from the baseline sample and specification as follows: Column 2 adds individual fixed effects to the estimating equation (see equation (17)). Column 3 limits the analysis to a balanced panel of individuals with non-missing data for the two years before and four years after their hospitalization. Column 4 restricts the sample to individuals in the lowest quartile of predicted mortality risk based on age and diagnosis-related group for the index admission. Column 5 adds back to the baseline sample insured individuals who had a prior hospital admission within the last three years. Column 6 restricts the sample to admissions through the emergency room. Column 7 restricts to non-deferrable admissions, which are limited to the subset of admissions that originate through the ER and have an ICD-9 code as the primary diagnosis that has weekend to weekday frequencies closest to the 2:5 ratio that we would expect if there is no delay in care. Column 8 excludes admissions for "ambulatory care sensitive conditions."

G.2.7 Additional Results

Appendix Table 34. Impact of Hospitalization on Non-Missing Credit Score and HELOC

Sample	Non-Elderly Insured	Non-Elderly Uninsured	Elderly
	(1)	(2)	(3)
	Panel A. Has Credit Score		
12-month effect	-.00016	-.0043	-.00015
	(.00037)	(.00094)	(.00035)
	[.66]	[<.001]	[.68]
48-month effect	-.0028	-.0085	-.0021
	(.00097)	(.0024)	(.00099)
	[.0038]	[<.001]	[.032]
Pre-hospitalization mean	.96	.84	.97
Number of Individuals	383,718	153,617	414,547
Number of Observations	3,131,534	1,256,759	2,959,802
	Panel B. Home Equity Line of Credit		
12-month effect	-.002	-.002	.003
	(.001)	(.001)	(.001)
	[.018]	[<.001]	[<.001]
48-month effect	-.009	-.007	0
	(.002)	(.002)	(.002)
	[<.001]	[<.001]	[.81]
Pre-hospitalization mean	.18	.064	.13
Number of Individuals	383,718	153,617	414,547
Number of Observations	3,131,534	1,256,759	2,959,802

Notes: Samples are non-elderly insured, uninsured, and the elderly (see Appendix Table 13, columns 3, 6, and 9). All columns report effects based on OLS estimates of equation (5). 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.

G.2.8 Comparing Results Across Groups

Appendix Table 35. Impact of Hospitalization for Reweighted Samples

	Non-Elderly Insured	Non-Elderly Uninsured	Uninsured, Reweighted	Elderly	Elderly, Reweighted
	(1)	(2)	(3)	(4)	(5)
	Panel A: Number of Collections to Date				
12-month effect	.11	.97	.83	.027	.029
	(.005)	(.012)	(.015)	(.002)	(.005)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
48-month effect	.21	1.3	1	.038	.036
	(.019)	(.045)	(.057)	(.01)	(.02)
	[<.001]	[<.001]	[<.001]	[<.001]	[.065]
Pre-hospitalization mean	.92	2.3	1.9	.24	.27
	Panel B: Collections Balances				
12-month effect	122	4,469	3,966	24	14
	(13)	(51)	(64)	(8)	(15)
	[<.001]	[<.001]	[<.001]	[.0018]	[.36]
48-month effect	302	6,199	5,423	84	55
	(37)	(130)	(160)	(24)	(49)
	[<.001]	[<.001]	[<.001]	[<.001]	[.26]
Pre-hospitalization mean	1,230	3,529	3,018	428	485
	Panel C: Any Bankruptcy to Date				
12-month effect	.0013	.0048	.0051	-.00019	-.00047
	(.00031)	(.00046)	(.00069)	(.00022)	(.00037)
	[<.001]	[<.001]	[<.001]	[.4]	[.21]
48-month effect	.0042	.014	.013	-.001	-.0015
	(.00092)	(.0014)	(.0021)	(.00072)	(.0012)
	[<.001]	[<.001]	[<.001]	[.16]	[.22]
Pre-hospitalization mean	.034	.037	.04	.016	.017
	Panel D: Credit Limit				
12-month effect	-515	-678	-787	370	615
	(154)	(131)	(429)	(138)	(246)
	[<.001]	[<.001]	[.067]	[.0073]	[.013]
48-month effect	-2,215	-690	-190	-448	-217
	(440)	(353)	(960)	(393)	(658)
	[<.001]	[.051]	[.84]	[.25]	[.74]
Pre-hospitalization mean	37,664	15,145	21,080	36,967	36,970
	Panel E: Credit Score				
12-month effect	-1.6	-5	-5.4	-1.4	-1.6
	(.2)	(.3)	(.4)	(.2)	(.3)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
48-month effect	-1.8	6.6	4.9	-3.3	-3.7
	(.5)	(.6)	(1)	(.5)	(.9)
	[<.001]	[<.001]	[<.001]	[<.001]	[<.001]
Pre-hospitalization mean	731	655	677	824	818
	Panel F: Credit Card Balances				
12-month effect	-293	-264	-219	72	248
	(94)	(83)	(169)	(73)	(134)
	[.0018]	[.0014]	[.19]	[.32]	[.064]
48-month effect	-1,208	-443	6.6	-30	45
	(253)	(214)	(555)	(187)	(312)
	[<.001]	[.038]	[.99]	[.87]	[.88]
Pre-hospitalization mean	11,942	5,376	7,165	7,016	7,160
	Panel G: Automobile Loan Balance				
12-month effect	-102	-267	-165	69	43
	(28)	(29)	(50)	(17)	(35)
	[<.001]	[<.001]	[<.001]	[<.001]	[.22]
48-month effect	-507	-349	-308	194	75
	(71)	(73)	(120)	(43)	(86)
	[<.001]	[<.001]	[.01]	[<.001]	[.38]
Pre-hospitalization mean	6,684	3,981	4,220	2,143	2,293
Number of Individuals	383,718	153,617	146,659	414,547	408,244
Number of Observations	3,131,534	1,256,759	1,200,774	2,959,802	2,914,677

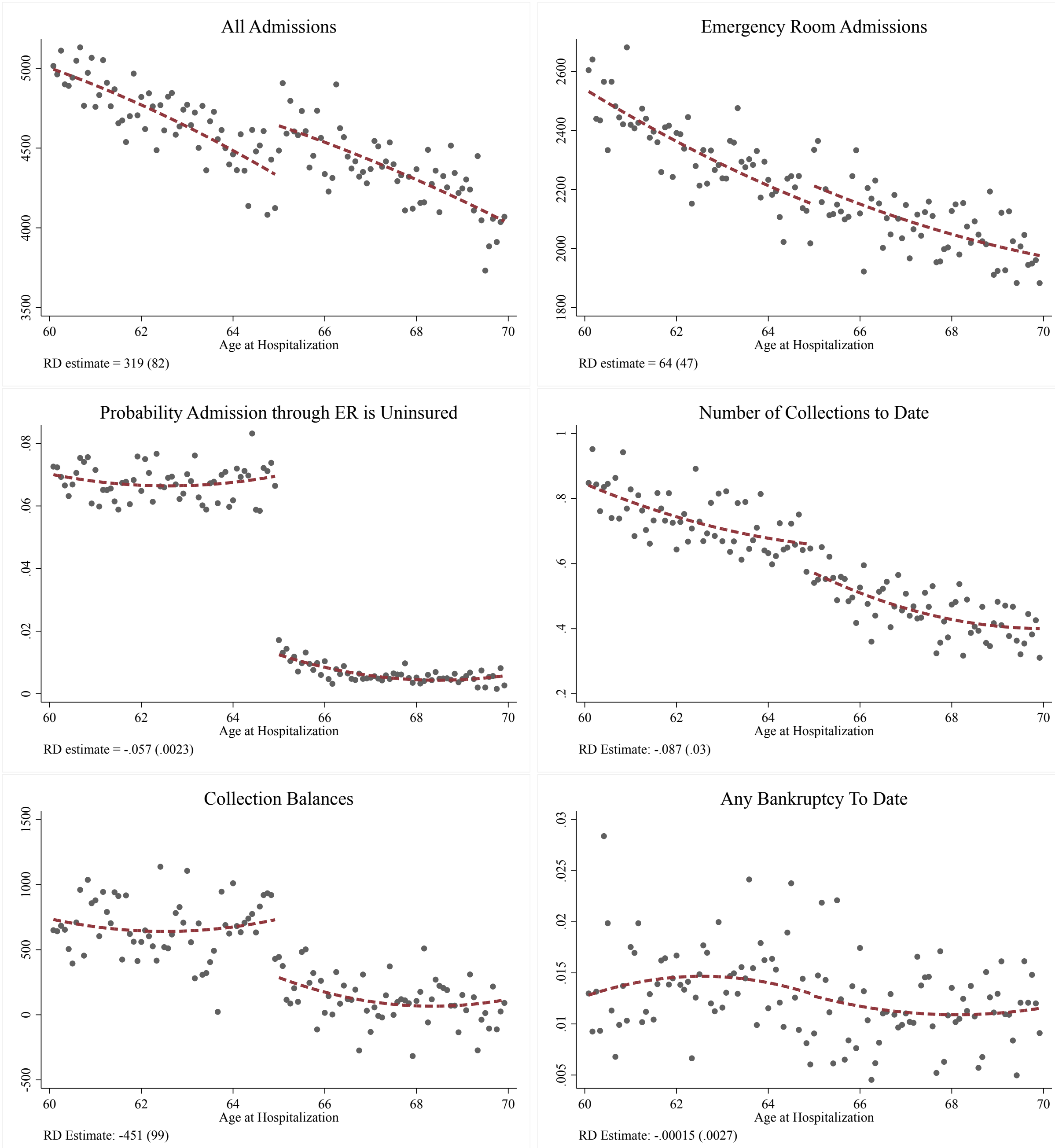
Notes: Columns 1, 2 and 4 replicate the baseline results (see Tables 5 and 6); columns 3 and 5 report results which reweight the uninsured and elderly samples to match the insured sample based on MDC codes, race, gender, median household income for zip code of residence, and whether the hospitalization included a chronic diagnosis. All columns report effects based on OLS estimates of equation (5). 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.

Appendix Table 36. Regression Discontinuity

Outcome	"One Year" Impact	"Four Year" Impact	"One Year" Impact First-Differenced	"Four Year" Impact First-Differenced
	(1)	(2)	(3)	(4)
All Admissions	319	304		
	(82)	(63)		
	[<.001]	[<.001]		
Admissions through ER	64	61		
	(47)	(37)		
	[.17]	[.097]		
Probability that Admission through ER is Uninsured	-.057	-.056		
	(.0023)	(.0027)		
	[<.001]	[<.001]		
Number of Collections to Date	-.074	-.053	-.087	-.12
	(.063)	(.081)	(.03)	(.069)
	[.24]	[.51]	[.0033]	[.078]
Collection Balances	-457	-574	-451	-666
	(103)	(162)	(99)	(193)
	[<.001]	[<.001]	[<.001]	[<.001]
Any Bankruptcy To Date	.0027	-.0012	-.00015	-.0024
	(.0035)	(.0055)	(.0027)	(.0045)
	[.44]	[.82]	[.96]	[.59]
Credit Limit	1,182	1,850	246	-80
	(1,753)	(1,770)	(1,190)	(2,034)
	[.5]	[.3]	[.84]	[.97]
Credit Score	-2.2	-1.8	1.5	1.8
	(2.6)	(2.9)	(1.2)	(2.3)
	[.4]	[.53]	[.23]	[.43]
Credit Card Balances	599	1,156	535	851
	(976)	(867)	(668)	(1,219)
	[.54]	[.18]	[.42]	[.49]
Automobile Loan Balance	56	-29	34	-143
	(180)	(212)	(209)	(330)
	[.75]	[.89]	[.87]	[.66]
N (Unique Admissions through the ER)	131,446	97,997	131,446	97,997

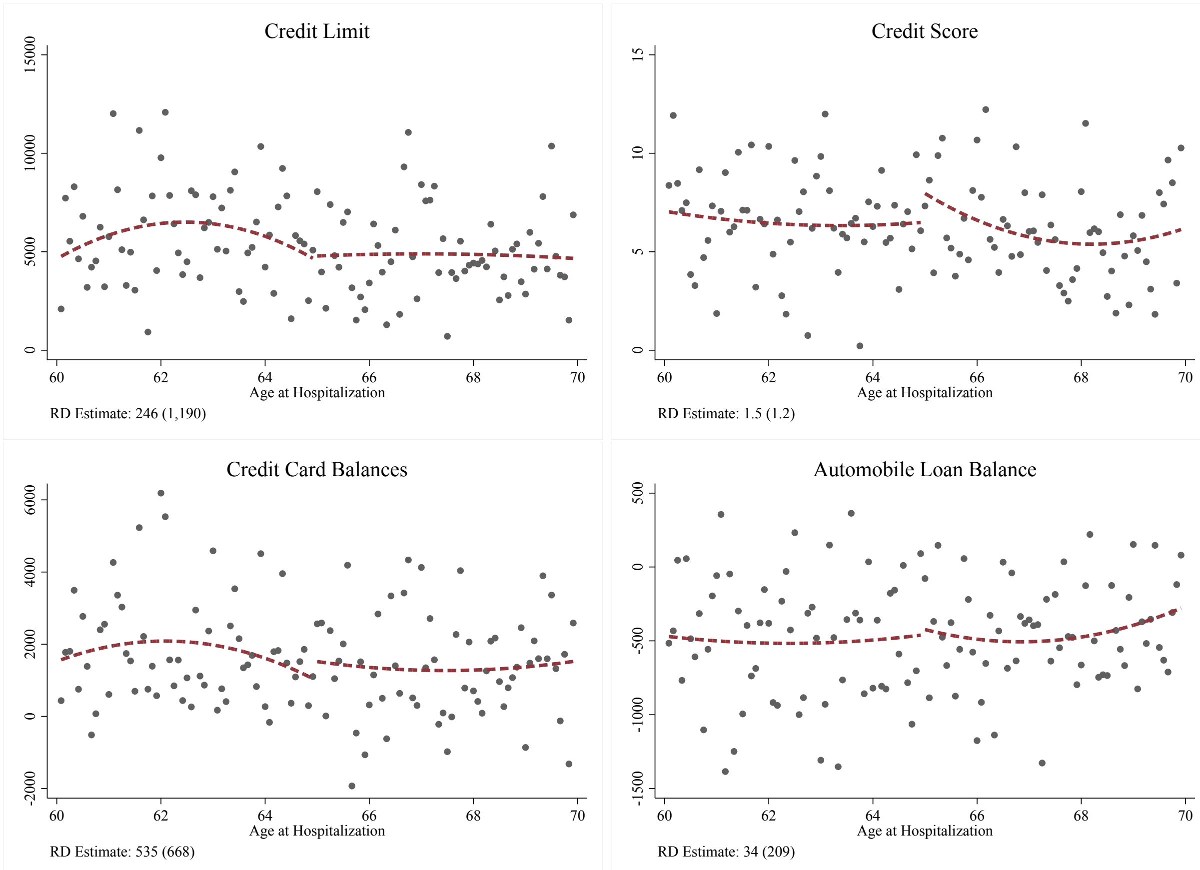
Notes: All columns report the coefficient on the dummy indicating an individual was over the age of 65 at the time of hospitalization based on estimating the regression discontinuity specification in equation (18). The sample is limited to individuals who are admitted to the hospital between ages 60 and 70; in all the analyses but the first row, we further limit the sample to admissions that occur from the ER. The first three rows - "All Admissions", "Admissions through the ER", and "Probability that Admission through ER is Uninsured" - are estimated on collapsed data which sums the number of hospitalizations by age in months. All other outcomes are estimated on the individual-level credit report data from January of the calendar year following the hospitalization (Columns 1 and 3) or the fourth January following the hospitalization (Columns 2 and 4). For brevity, refer to the former as the "one year" impact of the hospital admission, although in practice it measures outcomes 1 to 12 months after admission; likewise the "4-year" impact measures outcomes 37-48 months post admission. In the differenced specifications (columns 3 and 4), the dependent variable is differenced; specifically, we subtract from the dependent variable in column 1 or 2, respectively, an individual's observation for the outcome in the calendar year preceding the hospitalization. Standard errors (clustered at the age-month level) are in parentheses and p-values are in brackets. All estimates are weighted to account for individuals' sampling probabilities.

Appendix Figure 29. Regression Discontinuity



Notes: The points are given by the mean value of the variable for the given x-axis age (measured in months), except the top two figures which are sums of hospitalizations for that age. Means are taken from the credit report from the January following the hospitalization. The dashed line plots the regression discontinuity specification following equation (14); it allows for a separate quadratic in age (measured in months) on either side of the discontinuity at age 65 when individuals become eligible for Medicare. The sample is comprised of hospitalizations for individuals who are admitted through the ER between the ages of 60 and 70 (except for the top left figure which features all hospitalizations in this age range, regardless of the source of admission). All estimates are weighted to account for individuals' sampling probabilities.

Appendix Figure 30. Regression Discontinuity



Notes: The points are given by the mean value of the variable for the given x-axis age (measured in months). Means are taken from the credit report from the January following the hospitalization. The dashed line plots the regression discontinuity specification following equation (18); it allows for a separate quadratic in age (measured in months) on either side of the discontinuity at age 65 when individuals become eligible for Medicare. The sample is comprised of hospitalizations for individuals who are admitted through the ER between the ages of 60 and 70 (except for the top left figure which features all hospitalizations in this age range, regardless of the source of admission). All estimates are weighted to account for individuals' sampling probabilities.

G.3 Medical Expenditure Survey

Appendix Table 37. Summary Statistics for the Medical Expenditure Survey

	Non-Elderly Insured (1)	Non-Elderly Privately Insured (2)	Non-Elderly Medicaid (3)	Non-Elderly Uninsured (4)	Elderly (5)
Annual Share of Individuls with Non-Childbirth Hospitalization	5.7%	4.9%	13.8%	2.9%	17.3%
	Panel A. Index Event				
Total Charges for Index Event	25,267	25,255	26,064	24,349	31,459
Total Payments for Index Event	10,839	11,585	8,448	6,938	11,182
Average Share of Index Event Payments Out-of-Pocket	.055	.06	.034	.49	.025
Total Out-of-Pocket Payments for Index Event	362	427	85	1,363	212
	Panel B. Index Event Plus Next 12 Months				
Total Charges	40,420	38,366	48,978	35,381	51,307
Total Spending	18,660	18,916	18,292	11,131	19,920
	(28,435)	(29,389)	(27,126)	(25,845)	(21,911)
Median	10,779	10,941	10,528	3,085	13,502
75th Percentile	21,463	21,800	20,637	9,797	24,985
90th Percentile	42,573	42,838	41,776	23,212	41,999
95th Percentile	58,583	58,987	57,767	52,973	58,060
Average Share of Total Spending Out-of-Pocket	.084	.088	.067	.53	.08
Total Out-of-Pocket Spending	865	930	582	2,682	1,001
	(1,558)	(1,501)	(1,676)	(7,125)	(1,966)
Median	372	442	124	680	529
75th Percentile	979	1,088	496	2,411	1,194
90th Percentile	2,105	2,237	1,363	6,884	2,358
95th Percentile	3,314	3,488	2,496	9,862	3,274
Number of Inpatient Discharges	1.4	1.4	1.6	1.4	1.6
Total IP Spending	15,451	15,864	14,439	9,583	16,726
	(27,064)	(27,945)	(26,203)	(24,755)	(20,980)
Median	7,976	8,294	6,851	2,104	10,309
75th Percentile	17,787	18,280	15,260	7,488	20,798
90th Percentile	36,572	36,743	36,478	21,152	37,521
95th Percentile	52,100	52,100	52,732	43,541	52,507
Average Share of IP Spending Out-of-Pocket	.053	.058	.03	.44	.023
Total Out-of-Pocket IP Spending	414	485	113	1,781	266
	(1,181)	(1,237)	(796)	(6,422)	(1,639)
Median	8	48	0	59	0
75th Percentile	325	455	0	1,027	122
90th Percentile	1,140	1,293	100	3,627	585
95th Percentile	1,934	2,113	500	7,961	1,035
Total ER, OP, and Rx Spending	3,209	3,052	3,854	1,548	3,194
	(5,346)	(5,270)	(5,605)	(3,053)	(4,756)
Median	1,394	1,222	2,129	484	1,858
75th Percentile	3,769	3,529	4,596	1,609	3,738
90th Percentile	7,830	7,487	9,191	3,861	7,160
95th Percentile	12,551	12,148	13,410	7,045	10,116
Average Share of ER, OP, and Rx Spending Out-of-Pocket	.26	.29	.17	.67	.34
Total Out-of-Pocket ER, OP, and Rx Spending	451	445	469	901	735
	(975)	(788)	(1,462)	(2,290)	(1,086)
Median	175	195	98	243	379
75th Percentile	479	495	386	841	862
90th Percentile	1,090	1,097	1,026	2,439	1,874
95th Percentile	1,689	1,636	2,105	4,139	2,813
Unique Individuals	3,683	2,695	1,078	371	2,897

Notes: All estimates are the sample of individuals in the Medical Expenditure Panel Survey (MEPS) with an inpatient claim record in their first year of the two year survey. This claim (summing multiple claims when a single hospitalization is spread across more than one unit in a hospital or more than one hospital resulting in mulitple claims) provides the "Index Event" estimates in Panel A. Panel B provides statistics for the index event and all medical expenses accrued in the following 12 months. Standard deviations are in parentheses. See Appendix B.3 for details.