

Essays on Health Care Consumption and Household Finance

by  
Tal Gross

A.B., University of Chicago (2003)

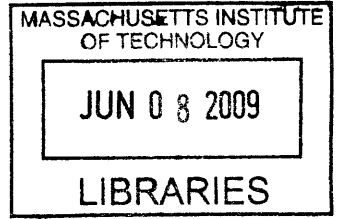
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Author ... ..

Department of Economics

May 18, 2009

Certified by .... ..

David Autor

Professor of Economics

Thesis Supervisor

Certified by ..... ..

Jonathan Gruber

Professor of Economics

Thesis Supervisor

Certified by ..... ..

Joshua Angrist

Ford Professor of Economics

Thesis Supervisor

Accepted by ..... ..

Esther Duflo

Abdul Latif Jameel Professor

of Poverty Alleviation and Development Economics

Chair, Departmental Committee on Graduate Studies



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## Abstract

This thesis explores how health insurance affects the decisions that individuals make. The first chapter studies the effect of insurance on health care consumption. Nearly 10 percent of teenagers become ineligible for their families' health insurance coverage on their nineteenth birthdays. Due to the federal Emergency Medical Treatment and Active Labor Act, however, they do not lose access to free emergency room care. I develop a straightforward theoretical framework to understand the implications of insurance transitions at age nineteen. I then develop an empirical framework that exploits the discontinuity in health insurance at age nineteen. Using a unique database of 15 million hospital discharge records, I find that Emergency Room (ER) usage rises discontinuously at age nineteen, particularly for minorities and residents of low-income zip codes. As predicted by the theoretical framework, the jump in ER utilization at age nineteen is disproportionately driven by ailments that physicians classify as inappropriate for ER care. I also find suggestive evidence that health care expenditures outside of the ER decline. A large share of the increase in ER utilization at age nineteen takes the form of uncompensated care, the cost of which is born by third parties. These findings constitute some of the first evidence on how the incentives faced by the uninsured affect medical expenditure.

The second chapter, written jointly with Matthew Notowidigdo, studies the contribution of medical costs in the decision to declare bankruptcy. Consumer bankruptcies increased eighty-seven percent in the 1990s. By the end of the decade, more than one percent of American households were declaring bankruptcy in any given year. Anecdotal evidence and several observational studies suggest that out-of-pocket medical costs are pivotal in a large fraction of consumer bankruptcy declarations. In this paper, we use variation in Medicaid eligibility to assess the contribution of medical costs to household bankruptcy risk. Using cross-state variation in Medicaid expansions from 1992 through 2002, we find that a 10 percentage point increase in Medicaid eligibility reduces the personal bankruptcy rate by 8.7 percent, with no evidence that business bankruptcies are similarly affected. We interpret our findings with a model in which health insurance substitutes for other forms of financial protection. We conclude with a calibration exercise that suggests that out-of-pocket medical costs are

pivotal in roughly 26 percent of personal bankruptcies among low-income households.

The third chapter studies how transitions in insurance status may affect the consumption of health care. Transitions from one insurance program to another—or from insured status to uninsured status—are common. How these transitions affect individuals depends, in part, on whether consumers anticipate the loss of insurance. Potentially, if consumers are sufficiently forward-looking, they may “stock up” on health care before losing coverage. This paper studies the transition in insurance status as teenagers move from their family’s coverage to uninsured status or other insurance plans. I find no evidence that teenagers stock up on medical care before coverage ends, but rather a general decrease in health care consumption in the last month of coverage.

Thesis Supervisor: David Autor

Title: Professor of Economics

Thesis Supervisor: Jonathan Gruber

Title: Professor of Economics

Thesis Supervisor: Joshua Angrist

Title: Ford Professor of Economics

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# Chapter 1

## The High Cost of Partial Insurance: Emergency Room Use and the Uninsured

### 1.1 Introduction

Fifteen percent of Americans have no health insurance. Those who do not have health insurance consume health care very differently from those who have it. The uninsured are less likely to demand preventative care, such as diagnostic exams and routine checkups (Ayanian et al., 2000). As a result, they are more likely to be hospitalized for conditions that—if treated promptly—do not require hospitalization (Weissman et al., 1992). Such correlations have led to much discussion of the impact of insurance on health, and to broader debates over policies that would cover the uninsured.

But would the uninsured behave any differently if they had health insurance? Those who do not have health insurance have different discount factors, risk tolerances, and medical risks than those who do have health insurance, which makes causal inference difficult. Little evidence exists that overcomes this empirical challenge. The evidence that does exist is based primarily on Medicaid and Medicare, and estimates

the behavioral response to the acquisition of health insurance, rather than the loss of health insurance.<sup>1</sup> This research is restricted to the health care consumption of the very young and the elderly, groups whose health care utilization may not be comparable to that of adults.

While Medicaid and Medicare provide health insurance for individuals at either end of the life-cycle, young adults make up a disproportionate share of the uninsured. Young adults, age nineteen to twenty-nine, comprise seventeen percent of the population under age sixty-five, but thirty percent of the non-elderly uninsured (Collins et al., 2003). In order to evaluate public policies that would insure the uninsured, one needs to understand how young adults react when they lose health insurance. Such research is also critical for understanding the basic incentives created by health insurance.

In this chapter, I study how young adults change their health care consumption once uninsured. To do so, I exploit quasi-experimental variation in insurance status that is discontinuous in age. Many private health insurance contracts cover dependents “eighteen and under” and only cover older dependents who are full-time students. As a result, ten percent of teenagers become uninsured on their nineteenth birthdays. I exploit this variation through a regression discontinuity (RD) design. I compare the health care consumption of teenagers who are almost nineteen to the health care consumption of those who just turned nineteen.

To understand how nineteen-year-olds change their behavior once uninsured, I focus on one measure of health care: emergency room (ER) visits. Emergency rooms are required to treat all patients by the Emergency Medical Treatment and Active Labor Act (hereafter, “EMTALA”).<sup>2</sup> The existence of convenient—and often free—care in the ER poses a moral hazard problem. The uninsured can shift their health care consumption towards the ER in response to the constrained prices they face, rather than the real cost of their actions. In this way, the uninsured effectively retain health insurance for the ER when they lose insurance for all other providers. That

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<sup>1</sup>See, for instance, papers by Dafny and Gruber (2005) and Card et al. (2008).

<sup>2</sup>This unfunded mandate requires hospitals to provide full medical screenings to all patients who enter their waiting rooms. It was passed by Congress in 1986.

is, they become nominally uninsured.

This chapter presents preliminary steps towards a full evaluation of health insurance. It does not study the effect of insurance on *all* health care expenditures, nor does it evaluate the long-term effects of being uninsured. The RD at age nineteen isolates the behavior of young adults who are insured one day and uninsured the next. As a result, this analysis shows how individuals immediately react to a loss of health insurance. It provides some of the first evidence on the financial incentives faced by the twenty percent of Americans who lose health insurance each year.

I find that teenagers are more likely to visit the emergency room (ER) once they lose health insurance. This effect is imprecisely estimated overall, but pronounced for minorities and the poor. These groups become more likely to visit the ER for conditions that are easily treated by a doctor or medication --conditions for which the ER is an expensive and probably inappropriate provider (Grumbach et al., 1983). I also find evidence that non-ER care falls when teenagers lose insurance. Nineteen-year-olds have fewer doctor visits than eighteen-year-olds. This suggests that lack of health insurance induces individuals to shift health care from doctor offices and pharmacies to the hospital, often at the cost of the public.

I offer a conceptual model to rationalize these results. Uninsured status has two distinct effects on health care consumption. On the *extensive* margin, the uninsured reduce all health care expenditures, because they face market prices rather than co-payments. On the *intensive* margin, the uninsured shift care to the ER, because the existence of free care lowers the relative price of an ER visit. In general, ER use may rise or fall when individuals become uninsured, depending on whether the extensive or intensive effect dominates. I find an increase in ER visits, which suggests that the intensive effect dominates and that the uninsured rely on ER's for conditions that could be handled by a doctor.

The chapter is organized as follows. Section 1.2 reviews the previous literature on insurance and health care consumption. Section 1.3 presents a simple model of moral hazard that generates predictions regarding insurance status and ER visits. Section 1.4 describes the data I use, and section 1.5 describes my econometric ap-

proach. I document the change in insurance coverage rates in section 1.6. Section 1.7 presents the core of my empirical results, the discontinuous change at age nineteen in the number and composition of ER visits. I discuss other changes in health care consumption in section 1.8. Section 1.9 speculates on the effect of lack of insurance on total financial costs, and section 1.10 concludes.

## 1.2 Prior Evidence on the Health Expenditure of the Uninsured

The uninsured tend to consume expensive health care treatments when cheaper options are available. Weissman et al. (1992) find that the uninsured are much more likely to be admitted to the hospital for a medical condition that could have been prevented with timely care. Similarly, Braveman et al. (1994) estimate that the uninsured are more likely to suffer a ruptured appendix. Appendicitis is not preventable, but it is easily treated when diagnosed early. For the uninsured, however, appendicitis is more likely to result in an expensive medical emergency. Dozens of similar studies are summarized in an Institute of Medicine (2002) report. Nearly all find a robust correlation between a lack of insurance and a reliance on expensive, avoidable medical treatments.

The medical literature generally finds that the uninsured are *not* more likely to visit the ER. Weber et al. (2005) estimate probit models of ER use on insurance status with a household survey. The authors calculate that the uninsured and insured are equally likely to have visited an ER in the past year. Such an estimation strategy may suffer from omitted variables bias, a shortcoming that does not affect the regression discontinuity framework presented below.

In general, such correlations do not establish a causal effect between uninsured status and a particular pattern of health care consumption. To my knowledge, only two sets of studies have explored whether insurance status has a causal effect on health care utilization.<sup>3</sup> The first of these evaluates Medicaid expansions. Dafny and

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<sup>3</sup>One exception is Meer and Rosen (2003), who use self-employment status as an instrumen-



Gruber (2005) estimate that Medicaid expansions lead to an increase in total inpatient hospitalizations, but not to a significant increase in avoidable hospitalizations. The authors conclude that being insured through Medicaid leads individuals to visit the hospital more often and, potentially, to consume health care more efficiently.

Other papers study the effect of Medicare on health care utilization. Finkelstein (2007) studies the aggregate spending effects of the introduction of Medicare and Card et al. (2008) study the effects of Medicare on individual health care consumption. Both papers conclude that Medicare leads to an increase in health care consumption. Card et al. (2008) find that Medicare leads to an increase in elective procedures performed at hospitals and a general narrowing of utilization rates across demographic groups.

One disadvantage with such studies is that individuals who gain health insurance through Medicaid and Medicare are not always uninsured beforehand. Cutler and Gruber (1996) demonstrate that fifty percent of new Medicaid enrollees were previously enrolled in employer-provided insurance. Similarly, Card et al. (2007) conclude that much of the effect of Medicare stems from its effect as supplementary insurance for those who already have private insurance. Consequently, these papers do not isolate the causal effect of uninsured status on health care consumption, which is the object of interest here.

This chapter contributes to the literature on health insurance and utilization in several respects. First, it studies the loss of health insurance when most previous research has studied the acquisition of health insurance. Second, this chapter focuses on the health care consumption of young adults rather than the very young and the elderly. Young adults are more representative of the broader population; they are healthier than the elderly and do not have the developmental issues of the very young. Finally, this research focuses on outpatient emergency room visits, an important measure of health care expenditure rarely studied in the health economics literature.

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tal variable for health insurance. Such a strategy is likely invalid if unobservable characteristics determine both self-employment and health insurance status.

### 1.3 Theoretical Framework

This section offers a theoretical framework to explain why the insured and uninsured may consume medical treatments differently. I first use a general theoretical result based on the work of Goldman and Philipson (2007) to explain why the insured are induced to consume the majority of their medical care outside of the hospital. I next add a functional form to the model to consider whether the uninsured will shift their care back into the hospital.

Consider a continuum of risk-averse agents. The agents choose between two substitutable medical treatments: emergency room care ( $E$ ) and doctor-based care ( $D$ ). They consume medical expenditure  $x \equiv (x_E \ x_D)'$  and face co-payments for each treatment  $p \equiv (p_E \ p_D)'$ . Suppose that the agents face probability  $q$  of becoming ill, have exogenous income  $z$ , and face actuarially-fair insurance premiums  $\pi$ . Denote the ex-ante utility function as

$$U(x, c) = q \cdot u_1(x, z - \pi - p'x) + (1 - q) \cdot u_0(z - \pi).$$

The functions  $u_1(x, z - \pi - p'x)$  and  $u_0(z - \pi)$  are ex-post utility functions once the state of the agent is realized, that is, whether the agent is healthy or ill.

Goldman and Philipson (2007) prove that insurance companies will set co-payments on doctor visits such that

$$\underbrace{\frac{\partial U}{\partial x_D} \cdot \frac{\partial x_D}{\partial p_D}}_{\text{moral hazard versus risk}} + \underbrace{\frac{\partial U}{\partial x_E} \cdot \frac{\partial x_E}{\partial p_D}}_{\text{offset effect}} = 0 \tag{1.1}$$

The first two terms in equation (1.1) represent the standard moral hazard trade-off. On the one hand, when insurers raise co-payments for doctor-based care, they increase the risk faced by the agent. On the other hand, when insurers raise co-payments, they reduce the incentive faced by the agent to over-consume medical care. As a result of this trade-off, insurers set positive co-payments.

With multiple medical technologies, insurers must also consider the “offset ef-

fect,” the third term in equation (1.1). When insurers raise co-payments for doctor visits, they induce more emergency room visits, because agents substitute between the two treatments. As a result, insurers may set lower co-payments for doctor visits to prevent customers from substituting the ER for a primary-care physician or the pharmacy.

Now consider the choice faced by the uninsured. They still choose between  $x_E$  and  $x_D$  when ill. But the uninsured do not face optimal co-payments  $p_E$  and  $p_D$ ; rather, they face a set of distorted market prices,  $\tilde{p}_E$  and  $\tilde{p}_D$ . It is how  $\tilde{p}_E$  and  $\tilde{p}_D$  compare to  $p_E$  and  $p_D$  that determines how the uninsured consume health care differently from the insured. On average, the uninsured face higher prices for all medical care, but institutional forces may distort the way that these prices rise.

The prices faced by the uninsured in emergency rooms are constrained by several forces. Federal regulations effectively lower  $\tilde{p}_E$  by mandating that ER’s treat all visitors. As a result, ER’s provide a great deal of uncompensated care, in part because many uninsured patients simply do not pay their hospital bills (Gross and Notowidigdo, 2008). In this way, many individuals who become uninsured do not experience a large increase in the full price of an ER visit. They may be aware that they can be treated in the ER and some may expect (correctly) to be treated for free.

Care is not subsidized outside hospitals, however. Pharmacies are not required to provide the uninsured with discounts for prescription medications, and doctors also provide few discounts. Gruber and Rodriguez (2007) find that, on net, private doctors provide zero uncompensated care. Consequently, many of the uninsured experience a larger increase in the price of a doctor visit than the price of an ER visit.

In order to examine this shift from co-payments to constrained prices, consider the ex-post (once sick) utility function:

$$u_1 = ((A_E x_E)^\rho + (A_D x_D)^\rho)^{\alpha/\rho} \cdot (z - p_E x_E - p_D x_D)^{1-\alpha}.$$

This utility function captures the substitutability between emergency room care and doctor-based care, with consumption, a third good, as the numeraire. It assumes a

constant elasticity of substitution,  $\frac{1}{1-\rho}$ , between the two types of medical care, with  $\rho \in (0, 1)$ .<sup>4</sup> Under this framework, the agent's demand for medical care satisfies

$$\frac{x_E}{x_D} = \left(\frac{p_D}{p_E}\right)^{\frac{1}{1-\rho}} \cdot \left(\frac{A_E}{A_D}\right)^{\frac{\rho}{1-\rho}},$$

and the agent's absolute demand for emergency room care is:

$$x_E = \frac{z\alpha/p_E}{1 + \left(\frac{p_E}{p_D}\right)^{\frac{\rho}{1-\rho}} \left(\frac{A_D}{A_E}\right)^{\frac{\rho}{1-\rho}}}. \quad (1.2)$$

This demand depends on two prices, the absolute price of emergency care in the numerator and the relative price of emergency care in the denominator.

When some individuals become uninsured, they face a larger increase in  $p_D$  than  $p_E$  because of EMTALA and the existence of free care. For those individuals, equation (1.2) predicts that emergency room visits will increase.<sup>5</sup> Figure 1-1 presents the simple intuition behind this prediction. Panel (a) graphs the agent's budget set over medical and non-medical consumption. Since the uninsured face an increase in the price of medical care, the budget constraint shifts in, and the uninsured will unambiguously consume less medical care. Panel (b) graphs the agent's budget set on the intensive margin, the choice among medical goods. For this sub-problem, the agent chooses between doctor visits and ER visits. Panel (b) demonstrates that, given the price change described above, the consumption of ER visits may increase. If this intensive effect outweighs the extensive effect shown in panel (a), then ER visits will become more common.

The model makes one other prediction. For certain medical conditions, the ER is vastly more effective than a doctor office. For those conditions  $A_E \gg A_D$ , it can be shown that the behavioral response into the ER will be much smaller.<sup>6</sup> Intuitively, agents with those conditions for which  $A_E \gg A_D$  will already have most of their expenditures spent in the ER. As such, the intensive effect does not overwhelm the

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<sup>4</sup>This assumption on  $\rho$  means that ER care and doctor-based care are imperfect substitutes.

<sup>5</sup>Please see appendix for a proof.

<sup>6</sup>This is also shown more rigorously in the appendix.

extensive effect, and the increase in  $x_E$  is necessarily smaller.

In what follows, I test these predictions. I use counts of ER visits to estimate the causal effect of insurance on the quantity and composition of ER visits. In general, I confirm the predictions of this model and find suggestive evidence that its predictions regarding doctor-based care also hold true.

## 1.4 Data

For the uninsured, the most relevant health care outcome is ER visits. The ER is the only source of care to which individuals do not lose unfettered access when they lose insurance. I use a unique data set of such visits: the universe of hospital discharges for Massachusetts from 2002 through 2006. Many economists have used inpatient hospital discharge records—Card et al. (2008) and Evans and Kim (2006) are two examples—but few have had access to outpatient emergency room visits. Only recently have such records become available.

The Massachusetts ER data provide a large sample and a rich set of covariates. For each visit, I observe the patient’s medical condition, along with demographic information. Additionally, the data contain encrypted social security numbers, which allow me to track individuals over time and to observe whether patients return to the ER.

The five years in my sample contain 15.7 million ER visits and hospitalizations. I select 12.2 million visits that are either outpatient ER visits or hospitalizations that began in the ER. I then restrict my sample to 389,966 visits that are within 350 days of the patient’s nineteenth birthday (this restriction is described below). Table 1.1 summarizes the basic demographics for the working sample.<sup>7</sup>

I supplement these data with several publicly-available data sets that contain information on both age and health care utilization. I use the 1997 through 2006 extracts of the National Health Interview Survey (NHIS), which provides a large,

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<sup>7</sup>To determine age in days, I restrict the sample to observations that can be linked to a date of birth and to an encrypted social security number. Roughly fifteen percent of hospital visits in the data are eliminated in this way.

nationally-representative sample of households with over 1,800 nineteen-year-olds.<sup>8</sup> I also use the 1993 through 2005 extracts of the National Ambulatory Medical Care Survey (NAMCS), a nationally-representative sample of doctor visits, described below.

## 1.5 Empirical Approach

I estimate the effect of health insurance on the probability of visiting the ER. For any group at age  $a$ , one can estimate this probability as  $\sum_i Y_{ia}/N_a$ , the count of all individuals of age  $a$  that visit the ER, divided by the precise number of individuals at risk of a visit. This is the dependent variable in the main regression of interest:

$$\log\left(\frac{\sum_i Y_{ia}}{N_a}\right) = \alpha_0 + \alpha_1 U_a + f_P(a) + \nu_a. \quad (1.3)$$

Here  $U_a$  denotes the share of individuals of age  $a$  that are uninsured, and  $f_P(a)$  parametrizes the effect of age on ER visits.

One problem with equation (1.3), however, is that I do not observe  $N_a$  directly. I observe counts of ER visits, but not the precise number of individuals that could have visited the ER. For that reason, I make my first identifying assumption:  $\log(N_a)$  can be approximated by a smooth function  $f_N(a)$ .<sup>9</sup> I assume that there exist no abrupt changes in the population at risk of an ER visit by age in days. Between ages eighteen and nineteen, many teenagers move into and out of the state, thereby changing  $N_a$ . But such fluctuations in population do not occur discontinuously by age in days. With this assumption, equation (1.3) can be re-written as

$$\log\left(\sum_i Y_{ia}\right) = \alpha_0 + \alpha_1 U_a + f_P(a) + f_N(a) + \nu_a. \quad (1.4)$$

Two remaining challenges make (1.4) infeasible to estimate directly in my data.

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<sup>8</sup>I restrict the NHIS sample to only those observations in the Northeast region in order to make the sample more comparable to the Massachusetts ER records.

<sup>9</sup>Card et al. (2008) originally employ this derivation in the case of Medicare.

First, I do not observe  $U_a$  at the population level. I only observe insurance status for the population that visits the ER. Secondly,  $U_a$  is not assigned at random. Unobservable variables may induce both lower insurance rates and lower health care consumption. Such unobservable variables would lead to inconsistent estimates of  $\alpha_1$ .

For those reasons, I exploit the exogenous variation in insurance status that occurs at age nineteen. When teenagers turn nineteen, they are more likely to lose insurance, but, aside from  $f_P(a)$ , no other variables in equation (1.4) are affected. Conditional on  $f_P(a)$ , the variable  $I\{a > 19\}$  is a valid instrument for  $U_a$ , and I can use this regression discontinuity to recover a consistent estimate of  $\alpha_1$ .

In this way, I assume that  $f_P(a)$  is continuous about age nineteen; no other determinants of health care demand change at age nineteen other than insurance status. This exclusion restriction would be violated if, for example, nineteen-year-olds celebrate their birthdays in ways that lead to medical emergencies. I argue below that this “birthday party effect” does not contaminate the estimates. The continuity of  $f_P(a)$  is the key assumption of the empirical framework.

Denote the effect of age nineteen on insurance status as:

$$U_a = \pi_0 + \pi_1 I\{a > 19\} + f_U(a) + \eta_a. \quad (1.5)$$

I substitute equation (1.5) into equation (1.4), yielding:

$$\begin{aligned} \log \left( \sum_i Y_{ia} \right) &= [\alpha_0 + \alpha_1 \pi_0] + \alpha_1 \pi_1 I\{a > 19\} \\ &\quad + f_P(a) + \alpha_1 f_U(a) + f_N(a) \\ &\quad + \alpha_1 \eta_a + \nu_a. \end{aligned} \quad (1.6)$$

Equation (1.6) is a reduced-form version of equation (1.3). It contains three smoothing functions:  $f_P(a)$ , the continuous effect of age on ER visits;  $f_U(a)$ , the continuous effect of age on insurance status; and  $f_N(a)$ , the continuous effect of age on the population at risk of an ER visit. These smoothing functions originate from the second stage, first stage, and parametrization of the underlying population.

Finally, I re-label  $f(a) \equiv f_P(a) + \alpha_1 f_U(a) + f_N(a)$ ,  $\beta_0 \equiv \alpha_0 + \alpha_1 \pi_0$ ,  $\beta_1 \equiv \alpha_1 \pi_1$ , and  $\varepsilon_a \equiv \alpha_1 \eta_a + \nu_a$ , which leads to a final, parsimonious equation that I take to the ER discharge records:

$$\log \left( \sum_i Y_{ia} \right) = \beta_0 + \beta_1 \cdot I\{a > 19\} + f(a) + \varepsilon_a. \quad (1.7)$$

With equation (1.7), I can indirectly estimate parameter  $\alpha_1$  in equation (1.3). The parameter  $\alpha_1$  is approximated by implied IV estimates: the ratio of reduced-form point estimates from equation (1.7) to first-stage point estimates of the effect of age nineteen on insurance status.

Around age nineteen, many teenagers enter the labor market and obtain health insurance through their new employers. Similarly, teenagers who enter college often retain health insurance.<sup>10</sup> This framework assumes that teenagers do not gain insurance precisely on their nineteenth birthdays. Under these assumptions, the regressions above will recover the local average treatment effect provided that the instrument acts monotonically (Imbens and Angrist, 1994). Monotonicity requires that no teenagers *gain* insurance precisely on their nineteenth birthdays. To my knowledge, there exist no institutions that use age nineteen as a milestone to begin providing health insurance. As a result, monotonicity is most likely satisfied.

One final concern is that some teenagers may wait until their nineteenth birthday to purchase new insurance plans in the individual market. This would confound my interpretation of the regression discontinuity, because the new insurance plans are likely to be less generous than the ones they replace. Nevertheless, this is unlikely to be a major source of bias. Only four percent of young adults purchase health insurance through the individual market. And, using the NHIS, I find no increase in this share at age nineteen (not shown).

A remaining question is how to model  $f(a)$ . I parametrize  $f(a)$  as a quadratic function with coefficients that are allowed to differ for those older and younger than age nineteen. The quadratic function captures the curvature that is apparent in many

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<sup>10</sup>Many private insurance plans offer coverage to older dependents who are still in school.



of the figures described below. As suggested by Hahn et al. (2001) and Imbens and Lemieux (2008), I tabulate results for different bandwidths in order to ensure that the results are robust to several alternatives. I allow a maximum bandwidth of 350 days before and after the patient's nineteenth birthday. I do so in order to avoid other birthdays, which may also induce discontinuities in health care coverage.<sup>11</sup> But I present results for many bandwidths smaller than this maximum to ensure that the results do not depend on a particular bandwidth.

## 1.6 The Change in Insurance Coverage Rates at Age Nineteen

I begin the empirical analysis by documenting the change in insurance coverage rates at age nineteen. I calculate rates of insurance coverage for individuals living in the northeast region and surveyed by the NHIS.<sup>12</sup> Figure 1-2 plots the life-cycle pattern of insurance coverage with these data. The figure demonstrates that insurance coverage rates change abruptly at two ages: nineteen and sixty-five. At age sixty-five, individuals gain insurance through Medicare. The discontinuity in insurance coverage rates that occurs at age nineteen, however, is larger in magnitude. Insurance coverage rates go up 5.9 percentage points at age sixty-five and down 9.4 percentage points at age nineteen. Figure 1-3 shows this pattern for a narrower range of ages and, using age in quarters, it demonstrates an abrupt change in coverage rates at age nineteen.

Table 1.2 shows how this discontinuity at age nineteen differs across sub-populations. The table estimates linear probability models of uninsured status using age in quarters in the NHIS. Column one reports a roughly nine percentage point increase in share uninsured at age nineteen. Columns two through four of the table suggest that this discontinuity is larger for minorities. This pattern—though not statisti-

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<sup>11</sup>Some insurance plans may stop covering dependent children at age eighteen and twenty-six. But these discontinuities are smaller than what occurs at age nineteen, and may be invalid instruments for health insurance. For instance, age eighteen is the legal age of majority in many states, and so other determinants of health care consumption may change discontinuously at age eighteen.

<sup>12</sup>For this section, I restrict the NHIS sample to only those observations living in the northeast in order to keep the sample comparable to the Massachusetts ER visits.

cally significant re-appears in the reduced-form results below. Columns five and six suggest that the change in coverage rates is much larger for males than for females, thirteen percent for males versus six percent for females. This may be explained by different college attendance rates by gender. Women are much more likely to attend college and thus less likely to become uninsured at age nineteen.<sup>13</sup>

Finally, it is important to note that this discontinuity isolates a transition from a broad cross-section of insurance plans—both private and public—towards zero traditional health insurance. Table 1.3 presents the change in insurance type at age nineteen. The nine percentage point increase in share uninsured is composed roughly equally between a decrease in private health insurance and a decrease in Medicaid. This pattern provides more evidence that the teenagers who lose health insurance at age nineteen are of lower socioeconomic status.<sup>14</sup> In the population as a whole, seventy percent of teenagers are enrolled in private health insurance programs. In the sub-population of teenagers that lose health insurance at age nineteen, public health insurance is much more prevalent.

## 1.7 The Impact of Insurance on ER Visits

In this section, I describe the change in ER visits at age nineteen. Section 1.7.1 demonstrates that ER visits become more common at age nineteen, especially for minorities and residents of low-income zip codes. Section 1.7.2 shows that two specific medical conditions become more common at age nineteen in the ER: pharyngitis (sore throat) and asthma. These conditions do not account for all of the increase in ER visits but suggest that patients—who could have easily sought treatment for such conditions outside of the hospital—are shifting towards the ER. Section 1.7.3 shows that the composition of ER visits changes at age nineteen as visits related to less severe conditions become more common. Finally, section 1.7.4 shows that the newly

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<sup>13</sup>Based on the 2000 decennial census, females in Massachusetts aged twenty-five to thirty are ten percentage points more likely to have gone to college than males.

<sup>14</sup>Still further evidence of this is that the discontinuity at age nineteen is much larger for high school dropouts than high school graduates, roughly thirty percent versus seven percent (not shown).

uninsured patients at age nineteen are more likely to return to the ER once treated.

### 1.7.1 The Impact of Insurance on Total ER Visits

Consider first the impact of insurance on total ER visits. Figure 1-4 plots RD results for total visits. The dashed line plots predicted values from equation (1.7), and the solid line plots ten-day cell means of the log of the number of ER visits. The figure demonstrates that ER visits become slightly more common after patients' nineteenth birthdays (statistically significant only at the ten percent level). This effect is much larger for certain groups. The number of visits by patients from low-income zip codes increases by five percent (figure 1-5).<sup>15</sup> In contrast, I find no major discontinuity for residents of other zip codes (not shown).

Table 1.4 reports the regressions behind these figures. Each row reports results of equation (1.7) for one outcome of interest: total visits, visits from low-income zip codes, and visits from all other zip codes. Columns one through five present results using several bandwidths. The first row reports a one to two percent increase in overall visits. The second row demonstrates a statistically significant increase in visits among low-income patients. This increase is relatively precise for most bandwidths. The third row demonstrates a statistically insignificant increase among patients from other zip codes. Overall, table 1.4 confirms the pattern evident in the figures: a relatively precise increase in ER visits for low-income patients, but an imprecise increase for all other patients.

One notable pattern in table 1.4 is that the point estimates are relatively robust to bandwidth. Specifically, the effect of age nineteen on visits from low-income zip codes is roughly four percent regardless of bandwidth, even when eliminating over half of the data. This suggests that the quadratic functional form fits the data well. For the remaining tables, I report only results for the 350 day bandwidth, but all results are robust to narrower bandwidths.

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<sup>15</sup>I label a zip code as low-income if more than three percent of the households have income under the poverty line, as measured in the 2000 census. This categorizes twenty percent of ER patients as living in a low-income zip code.

The results for low-income zip codes suggest that the effect of age nineteen may differ by demographic group. For that reason, I estimate equation (1.7) by race and gender. Figure 1-6a plots the discontinuity for White patients and finds no effect. In contrast, figures 1-6b through 1-6d suggest substantially larger discontinuities for minorities. Table 1.5 confirms this pattern. The first column demonstrates that White teenagers are *not* more likely to visit an ER at age nineteen. But the remaining columns show that patients of all other races (Blacks, Hispanics, Asians, and those with missing race information) do become more likely to visit the ER. This effect is statistically significant for Blacks and Hispanics.

The final two columns of table 1.5 suggest that the discontinuity in ER visits occurs for females but not for males. ER visits increase by 4.5 percent for females, but males exhibit no statistically significant increase in visits. I run these regressions separately by gender times minority/non-minority, and find that ER visits *decrease* for White males, but increase for White females, non-White males, and non-White females (not shown). I do not conclude from table 1.5 that there exists no discontinuity for males. Rather, I conclude that some groups, especially White males, exhibit a decrease in ER visits, but most groups exhibit an increase. I can only speculate as to why White males have fewer ER visits after age nineteen but all other groups have more ER visits. Billings et al. (2000) find that minorities and females are more likely to visit the ER for primary-care needs. If minorities and females attach less stigma to uncompensated ER visits, then the model in section 1.3 would predict an increase in ER visits for those groups, but not necessarily for White males.

In most cases, one would scale the reduced-form estimates above in order to calculate two-stage least squares estimates. It is difficult to do so here, because I observe insurance status in a separate data set from ER visits. The implied IV estimate for total visits is  $\frac{0.018}{0.093} = 0.19$ , a ten percentage point increase in share uninsured leads to a two percent increase in ER visits. For Hispanics— the sub-population with the largest first stage and reduced form— this implied IV estimate is much larger:  $\frac{0.063}{0.125} = 0.50$ , a ten percentage point increase in share uninsured leads to a five percent increase in

ER visits.<sup>16</sup>

One concern with these results is that they may be capturing the direct effect of the patient's birthday. If teenagers celebrate their birthdays in ways that could lead to a medical emergency, then they will be more likely to visit the ER, but not due to changes in health insurance. Carpenter and Dobkin (2009) find evidence for such a "birthday effect" for twenty-one-year-olds. But the estimates above, and throughout this chapter, do not appear to be driven by such birthday effects. One would expect the birthday effect to lead to a short-lived increase in visits, but most of the RD figures suggest a lasting increase.<sup>17</sup>

Additionally, one would expect just as much celebration on other birthdays, and yet there is no discontinuity in ER visits at age twenty. Very few teenagers lose their health insurance at age twenty. Overall, there is no change in private health insurance coverage, and only a small discontinuity for Medicaid (not shown). The latter is driven by state laws that provide Medicaid to small groups of teenage mothers and foster children but remove such eligibility at age twenty. Table 1.6 presents estimates of equation 1.7, but estimated at age twenty rather than nineteen. Most point estimates are much smaller than the four or five percent increase estimated for some groups at age nineteen, and nearly all point estimates are statistically indistinguishable from zero.

Finally, the results above raise the question: why does age nineteen have such different effects across demographic groups? To some extent, this pattern follows the college attendance rates by race.<sup>18</sup> Any remaining difference in the effect of age nineteen across demographic groups may be due to factors that are more difficult to quantify. The model in section 1.3 depends on changes in prices. But the model's prices may include the cost of stigma, and ER visits may be more stigmatized for

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<sup>16</sup>The NHIS contains age in quarters rather than age in days, which makes it difficult to calculate true two-sample instrumental variables estimates. These implied IV estimates can be considered a special case of two-sample IV in which I assume that age does not enter the first stage other than through age nineteen.

<sup>17</sup>Technically, RD only identifies the causal effect of health insurance at the threshold point. The figures suggest a lasting increase in visits but do not prove that one exists.

<sup>18</sup>From the 2000 census, forty-six percent of Whites in Massachusetts attend college versus thirty-one percent of Blacks and twenty percent of Hispanics.

some demographic groups. For instance, female patients are over-represented in the ER, with fifty-eight percent of visits in my sample made by women. Minorities are over-represented as well.

### 1.7.2 The Impact of Insurance on Case Composition

The model in section 1.3 predicts that the uninsured will shift care from doctor offices to the ER. In this section, I test whether nineteen-year-olds appear in the ER for conditions that can also be treated outside of the hospital. I do this in two ways. First, I select conditions that other researchers have classified as inappropriate for ER care and estimate equation (1.7) using visits related to those conditions. Second, I select the most common conditions among teenagers, and estimate equation (1.7) for those conditions.

The first of these methods proceeds as follows. Millman (1993) lists twenty-two conditions that a panel of doctors has designated as “ambulatory-care-sensitive conditions.” Such conditions—if treated in a hospital—are considered evidence of medical mismanagement. For instance, hospitalizations related to ear, nose, and throat (ENT) infections are included on the list because such conditions can be easily diagnosed and treated outside of the hospital. I study those conditions for which I have more than 4,000 cases within my 701-day window. Table 1.7 presents the results of equation (1.7) for six conditions that doctors have classified as avoidable hospitalizations or ambulatory-care sensitive conditions.

Three of the six common avoidable hospitalizations exhibit discontinuities in ER prevalence at age nineteen: ear, nose, and throat infections; gastroenteritis; and asthma. Of these three, the most precisely estimated is asthma: cases increase by 12.6 percent with a  $p$ -value of three percent. Asthma is easily treated by medication. The robust increase in asthma cases at age nineteen suggests that purchases of asthma medication decrease when teenagers become uninsured.

Visits for two other conditions exhibit statistically significant changes in table 1.7. Visits for ear, nose, and throat infections increase by seven percent, and visits for gastroenteritis *decrease* by sixteen percent. Both effects are statistically significant at the

ten-percent level, but not at the five-percent level. The drop in gastroenteritis-related visits is surprising given the overall increase in visits at age nineteen. Gastroenteritis is often referred to as stomach flu or food poisoning. While it is classified by doctors as an ambulatory-care sensitive condition, patients themselves may not view doctor visits and ER visits as substitutable treatments for gastroenteritis. In that case, the model predicts that the extensive margin will dominate. Individuals will consume less medical care overall, and will not consume care differently for gastroenteritis. This would lead to a pattern similar to that found in table 1.7.

One key drawback with this strategy is that I may be selecting conditions for which there are too few visits for adequate statistical power. As an alternative, I calculate RD point estimates for the five most common conditions among fifteen to eighteen year-olds. Table 1.8 presents the results of this procedure. Most of the conditions in table 1.8 are accidental injuries and exhibit no significant increase at age nineteen (as one would expect).<sup>19</sup> In fact, the only common condition that becomes more prevalent among nineteen-year-olds is the only common condition that is *not* accidental: pharyngitis. The condition pharyngitis is typically referred to as a “sore throat.”<sup>20</sup> It does not normally require an emergency room visit and can be easily treated by a doctor. The last row of table 1.8 documents that pharyngitis cases increase by nine percent after age nineteen. This increase is statistically significant at the five-percent level.

I do not have sufficient statistical power to estimate the regression discontinuity for all medical conditions. Nevertheless, the *only* medical conditions for which I observe a discontinuity are consistent with the predictions of the model. The only conditions that become more common after age nineteen are not severe, could be treated outside of the hospital, and therefore represent some discretion on the part of the patient. I do *not* observe a discontinuity for accidental or non-discretionary conditions, such as

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<sup>19</sup>One might expect upper limb contusions (bruised arms) to become less common if the teenagers partake in less risky behavior once uninsured. However, unless this behavioral effect were very large—or the risky behavior led to many ER visits—it seems unlikely that this effect would be precisely measured.

<sup>20</sup>Pharyngitis is one of the conditions included in the ENT avoidable hospitalizations category in table 1.7.

neck sprains.

### **1.7.3 The Impact of Insurance on the Composition of ER Visitors**

The previous section demonstrates that at least two medical conditions—sore throat and asthma—become more common in the ER at age nineteen. But these two conditions do not fully account for the increase in caseloads at age nineteen. In this section, I use other characteristics to measure changes in the composition of hospital visits.

Dobkin (2003) finds that ER cases admitted on weekdays tend to be less severe than those admitted on a weekend. The model above predicts an increase in less severe cases at age nineteen, but little change for severe cases. The first two columns of table 1.9 show that weekday visits become more common at age nineteen with no discontinuity for weekend visits.<sup>21</sup>

Table 1.9 also tests whether the composition of visits changes in one other way. It tests whether the composition of visits in the ER shifts towards new visitors, individuals who have not been observed previously in the ER.<sup>22</sup> If teenagers are shifting their consumption of health care from outside the hospital to inside the hospital, then much of the increase in visits at age nineteen should come from visitors who have not been to the ER before. Columns three and four of table 1.9 demonstrate that this is the case. Visits by previously-unobserved patients increase by five percent, while visits by previously-observed patients do not change.

### **1.7.4 The Impact of Insurance on Rates of ER Recidivism**

Teenagers who begin to visit the ER immediately after their nineteenth birthday are ones for whom the intensive margin dominates the extensive margin. One would

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<sup>21</sup>When these regressions are performed on counts by day, the point estimates for Monday through Thursday are much larger than those for Friday through Sunday (not shown). This suggests that the pattern in table 1.9 is not simply driven by the fact that there are more weekdays than weekends.

<sup>22</sup>I define a patient as being “new” if he or she has not been observed in the ER for the past year. This excludes the first year of data from the analysis.



expect those teenagers to consume more ER care in the future, as well. Table 1.10 presents evidence regarding this hypothesis. It presents estimates of equation (1.7) in which the left-hand side variable is log counts of visits that returned within ten, twenty, or thirty days. All point estimates are positive. Once they visit the ER, the marginal patient is likely to return.<sup>23</sup>

This finding suggests that for some uninsured teenagers, the ER has become the primary source of care. Of course, many of these return visits may be follow-up appointments for the same medical condition. In that case, recidivism alone does not demonstrate that long-term health care consumption has changed. But recidivism does suggest that the medical cost of the marginal ER visit is larger than what one would conclude from the basic RD results. Even once a newly-uninsured patient visits the ER once and receives the bill, the patient is still likely to return.

## 1.8 Other Measures of Health Care Access

The results above suggest that teenagers consume less health care outside of the ER after age nineteen, because they begin to visit the ER for conditions that could be handled by medication or a doctor visit. However, the ER records do not allow me to directly observe other measures of health care consumption. I turn to other data sets to estimate these effects.

The NHIS includes a handful of questions that begin to address how other measures of health care consumption change at age nineteen. For instance, the NHIS asks its respondents: “during the past twelve months, has medical care been delayed for you because of worry about the cost?” In figure 1-9, I graph the responses to this question. There is a discontinuity at age nineteen.<sup>24</sup> This suggests that nineteen-year-olds are concerned about the cost of medical care and changing their behavior accordingly.

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<sup>23</sup>Many teenagers visit the ER only several days before their nineteenth birthday. Those teenagers may become uninsured shortly afterwards, and thus their rates of recidivism are confounded by the treatment effect. For that reason, I have also run these regressions with no observations that are too close to the discontinuity to be identified. When I do so, the results become imprecise—unsurprisingly—but are all still positive.

<sup>24</sup>In figure 1-9, I do not include the data point at nineteen in either trend. Since I only have age in quarters, respondents turned nineteen uniformly across that quarter.

Similarly, the NHIS asks its respondents: “During the past twelve months, was there any time when you needed medical care, but did not get it because you couldn’t afford it?” Figure 1-10 presents the responses to this question. The graph seems to be continuous about age nineteen, suggesting that teenagers delay medical care once they become uninsured but never lose access completely.

This pattern in the NHIS provides more suggestive evidence that nineteen-year-olds reduce certain types of non-ER health expenditures. To pursue this further, I turn to the NAMCS. The NAMCS is designed to capture a nationally representative sample of doctor visits. As such, if nineteen-year-olds do indeed limit doctor visits, the NAMCS ought to provide evidence of that behavioral response.

Figure 1-11 presents the raw counts of doctor visits by age in the NAMCS. The figure demonstrates that nineteen-year-olds have fewer doctor visits than eighteen-year-olds, which would be consistent with the theoretical model presented in section 1.3. But the figure also demonstrates a large drop in doctor visits after age eighteen. This pattern suggests that doctor visits decrease primarily due to the transition into college, which affects both eighteen and nineteen year-olds. (The NAMCS does not collect visits from college infirmaries.)

But figure 1-12 presents some evidence that the decline in doctor visits is driven partly by insurance coverage rates. The NAMCS records the insurance status of each patient. Figure 1-12 plots the share uninsured by age. Only two percentage points more doctor visits are uninsured at age nineteen than at age eighteen—far less than the nine percentage point change in the population at large. Consequently, figure 1-12 suggests that newly-uninsured teenagers stop visiting the doctor. Of course, an alternative explanation is also plausible: the teenagers who lose insurance never went to the doctor in the first place. I view these figures as suggestive but not conclusive evidence that other measures of health care consumption decline at age nineteen. More research is needed to explore these other margins.

## 1.9 The Net Costs of Being Uninsured

The sections above demonstrate that, once uninsured, individuals shift medical consumption into the ER. They confirm the substitution pattern described in section 1.3. But they do not demonstrate that total health care costs rise when individuals become uninsured. This section presents a rough calculation of the net costs of becoming uninsured. I first discuss the economic costs of ER visits, and estimate the implied change in total health care costs. I then discuss who pays for these costs.

### 1.9.1 The Cost of Visits to the ER

The marginal cost of an ER visit has been the subject of debate. A minority view put forward by Williams (1996) is that the marginal cost of an ER visit is relatively low, and that ER's charge high prices mainly to transfer the costs of uncompensated care onto the insured. More recently, Bamezai et al. (2005) estimate the marginal cost of an ER visit to be much larger. The authors estimate that the marginal cost of a non-trauma ER visit is \$300, a number larger than the average price, let alone the marginal cost, of a doctor's visit.<sup>25</sup>

Several market-based tests also suggest that ER visits are indeed much more expensive than visits to a private doctor. Health maintenance organizations (HMO's) generally enjoy much bargaining power over hospitals, but still reimburse hospitals hundreds of dollars for each ER visit (Polsky and Nicholson, 2004). Additionally, some HMO's own hospitals, and therefore absorb the true marginal cost of an ER visit when their customers visit ER's. Were ER visits to cost less than doctor visits, one would expect such HMO's to shift their customers into the ER. But these HMO's—for example, Kaiser Permanente—still provide incentives for patients to use doctor offices rather than ER's.<sup>26</sup>

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<sup>25</sup>The average total payment for a doctor visit recorded in the Medical Expenditure Panel Survey is \$120.

<sup>26</sup>A representative health care plan from Kaiser Permanente for the individual market charges a \$150 co-payment for an ER visit, but a \$50 co-payment for a doctor visit.

## 1.9.2 The Impact of Insurance on Total Health Care Costs

The regressions in section 1.7.1 estimate the percent increase in probability of visiting an ER once uninsured. How does this increase in probability relate to net health expenditures? The total increase in ER-related costs can be decomposed into the following terms. First, equation (1.7) provides the percent change in the probability of visiting the ER at age nineteen. Second, I calculate ER visits per-capita using population estimates from the 2000 decennial census. Finally, I scale the change in visits per capita by the marginal cost of such visits. For the purpose of this rough calculation, I use the estimate from Bamezai et al. (2005). A rough estimate of the change in per-capita costs at age nineteen is then:

$$\beta_1 \times P(\text{visit}) \times \$300. \quad (1.8)$$

Table 1.11 presents estimates of expression (1.8) by race. The first column reproduces estimates of  $\beta_1$  from section 1.7.1, and the second column presents visits per-capita for each race. The third column presents expression (1.8) for each demographic group. Per-capita costs increase by \$1.69 overall, but the increase is much larger for minorities: more than nine dollars for Blacks and more than six dollars for Hispanics. This heterogeneity stems from the fact that minorities exhibit both a larger behavioral response to being uninsured and more visits per-capita.

The estimates in table 1.11 omit many changes in health care costs beyond the increase in ER visits. The estimates ignore the drop in doctor visits. In this sense, they over-estimate the increase in costs. Based on the model, each marginal ER visit is associated with a doctor visit that did not occur.

Additionally, the estimates ignore changes in health. Young adults are healthier than other sub-populations, and thus we would expect their health to be much less sensitive to changes in health care consumption. For instance, there is no discontinuity in death rates at age nineteen.<sup>27</sup> But the loss of insurance likely does not *improve*

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<sup>27</sup>I use the Social Security Administration Death File to test whether there is an increase in deaths after age nineteen. The Death File the Social Security Administration contains date of birth and date of death for every death that can be linked to a social security number. I find no such increase.

health. Therefore, the estimates in table 1.11 under-estimate the true increase in costs to the extent that they ignore potential long-term impacts of being uninsured on health.

While ER visits are expensive, they comprise a small share of total health care spending. The last column of table 1.11 presents per capita health expenditures for twenty to twenty-five-year-olds from the Medical Expenditure Panel Survey. Total expenditures are substantially larger than the per-capita changes: 890 dollars for Blacks and 840 dollars for Hispanics. I cannot measure how much these other expenditures change at age nineteen, nor can I estimate what share of these other expenditures represent social costs. Nevertheless, these other expenditures need only drop by less than one percent to compensate for the increase in ER costs. One cannot conclude from the evidence in section 1.7 that a loss of insurance leads to an increase in *total* health care costs. But one can conclude that being uninsured affects both the way in which individuals consume health care and who pays for that care. I consider the latter next.

### 1.9.3 Who Pays for the Marginal ER Visit?

The uninsured pay for a large share of ER visits themselves (Tyrance et al., 1996), but many visits are also classified as free care. Thirty-five percent of uninsured patients are given free care in the emergency room.<sup>28</sup> Table 1.12 documents that such visits become more frequent after age nineteen. It presents results from linear probability models of free-care status estimated exclusively on uninsured visits. For all races, the uninsured become roughly ten percentage points more likely to be granted free care after age nineteen. Moreover, Hispanics and Blacks experience a larger shift towards free care than Whites. This suggests that a large share of the marginal visits at age

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The regression I run in the SSA death file is equation (1.7) with log counts of deaths on the left-hand side:  $\beta = -0.0006$  with a standard error of 0.0225.

<sup>28</sup>This number calculated from the Massachusetts ER discharge records, based on expected payment status at date of admission. Hospital billing departments often classify uninsured visits as “self-pay” by default, and then later re-label them as free-care. Weeks or months after the date of admission, many more visits are re-classified as free-care once the hospital billing department realizes that the patient will not pay. Thus this number is actually an under-estimate.

nineteen are given free care. It is the hospital (and in the end, the public at large) that pays for these visits and not primarily the uninsured.

## 1.10 Conclusions

This chapter uses a discontinuous change in health care coverage at age nineteen to estimate the effects of health insurance on health care consumption. I find that ER visits rise at age nineteen, in particular visits related to conditions that could be handled by a private doctor or medication. I also find suggestive evidence that other types of health care consumption, such as doctor visits, become less common at age nineteen. Without knowing the full effects of such changes on health or on other measures of health care consumption, it is difficult to draw definitive conclusions regarding total costs. But I can conclude that as individuals become uninsured, costs shift to third parties.

This chapter makes three contributions to the literature on health insurance. First, it documents previously-unexamined exogenous variation in insurance coverage. Future research may apply this natural experiment to other questions, such as understanding how health insurance affects the purchase of medication. Second, it studies demand for ER visits, an important measure of health care consumption for which data are often not available. It demonstrates that the uninsured rely on ER's for health care that the insured receive outside of the hospital. Researchers have estimated such correlations, but few have demonstrated a causal relationship.

Finally, this chapter documents empirically how partial insurance can lead to moral hazard. The uninsured in the US effectively retain health insurance for ER care, but are not insured for other medical treatments. As such, they shift care towards the treatment for which they retain insurance. This phenomenon may be widespread; nearly one-fifth of Americans do not have health insurance. It may also re-appear in other contexts, whenever government intervention and medical ethics effectively subsidize certain medical technologies and not others. Such examples may grow in importance in the future, as costly medical technologies become more common

and the price of health insurance continues to rise.

# 1.A Theoretical Appendix

The conditions derived in section 1.3 are:

$$\frac{x_E}{x_D} = \left(\frac{p_D}{p_E}\right)^{\frac{1}{1-\rho}} \cdot \left(\frac{A_E}{A_D}\right)^{\frac{\rho}{1-\rho}}, \quad (1.A.1)$$

and

$$x_E = \frac{z\alpha}{1 + \left(\frac{p_E}{p_D}\right)^{\frac{\rho}{1-\rho}} \left(\frac{A_D}{A_E}\right)^{\frac{\rho}{1-\rho}}} \cdot \frac{1}{p_E}. \quad (1.A.2)$$

Suppose that the agent becomes uninsured and experiences an increase in both  $p_E$  and  $p_D$ . Specifically, suppose that she faces new prices  $\tilde{p}_E$  and  $\tilde{p}_D$  where  $\tilde{p}_E = \beta p_E$  and  $\tilde{p}_D = \beta \delta p_D$ , with  $\delta > \beta > 1$ .<sup>29</sup> The variable  $x_E$  will increase if and only if

$$1 + (p_E/p_D)^{\rho\sigma} (A_D/A_E)^{\rho\sigma} > \beta \cdot (1 + \delta^{-\rho\sigma} (p_E/p_D)^{\rho\sigma} (A_D/A_E)^{\rho\sigma}),$$

where  $\sigma \equiv \frac{1}{1-\rho}$ . This then simplifies to the condition

$$1 - \beta > (p_E/p_D)^{\rho\sigma} (A_D/A_E)^{\rho\sigma} \left[ \frac{\beta}{\delta^{\rho\sigma}} - 1 \right]. \quad (1.A.3)$$

And since the left-hand side of equation (1.A.3) is negative by assumption, it must be the case that  $\beta < \delta^{\rho\sigma}$ . That is, the scaled, relative increase in  $p_D$  must be larger than the absolute increase in both prices. Under that condition,  $x_E$  increases.

I further assert that this increase in  $x_E$  will be smaller whenever  $A_E \gg A_D$ . Specifically, denote the demand for ER visits when uninsured as  $x'_E$ . Then,

$$\frac{x'_E}{x_E} = \frac{1 + (p_E/p_D)^{\rho\sigma} (A_D/A_E)^{\rho\sigma}}{\beta + \beta \delta^{-\rho\sigma} (p_E/p_D)^{\rho\sigma} (A_D/A_E)^{\rho\sigma}},$$

and

$$\frac{\partial(x'_E/x_E)}{\partial(A_D/A_E)} = \frac{\sigma \frac{\beta}{\delta^{\rho\sigma}} \frac{\rho}{A_D} A_E \left(\frac{A_D}{A_E}\right)^{\sigma\rho} \left(\frac{p_E}{p_D}\right)^{\sigma\rho} (\delta^{\sigma\rho} - 1)}{\beta^2 (\beta + \beta \delta^{-\rho\sigma} (p_E/p_D)^{\rho\sigma} (A_D/A_E)^{\rho\sigma})^2}.$$

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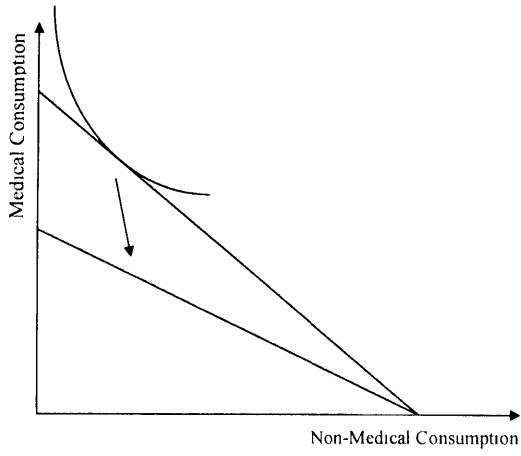
<sup>29</sup>For simplicity, I ignore the change in income due to the insurance premium and consider here only the changes in prices faced by the agent.



This derivative is positive so long as  $\rho \in (0, 1)$ . For very severe conditions,  $A_D/A_E \rightarrow 0$ , and then the change in ER visits is arbitrarily small.

Figure 1-1: Budget Constraints of the Uninsured Agent

(a) Extensive Margin



(b) Intensive Margin

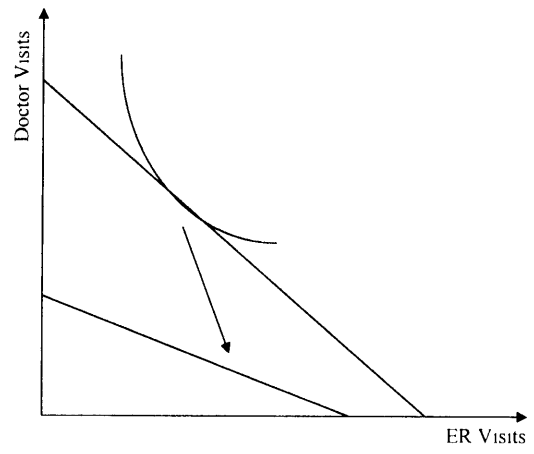


Figure 1-2: Share Uninsured by Age, NHIS

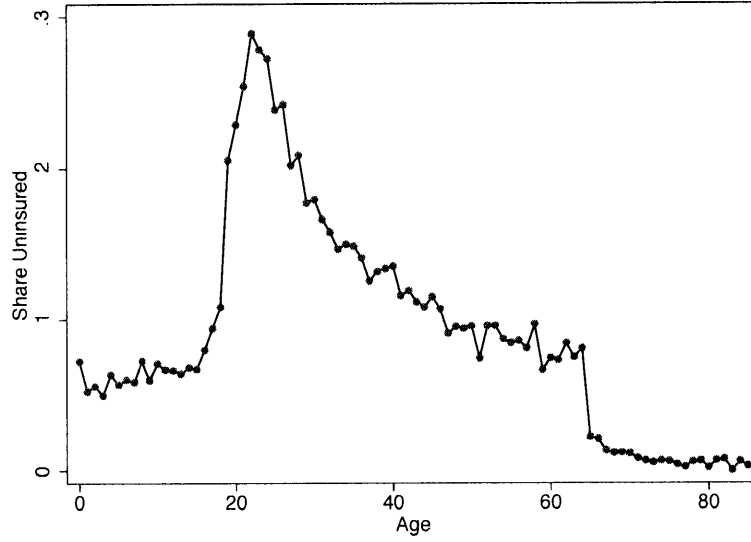


Figure 1-3: Share Uninsured by Age in Quarters, NHIS

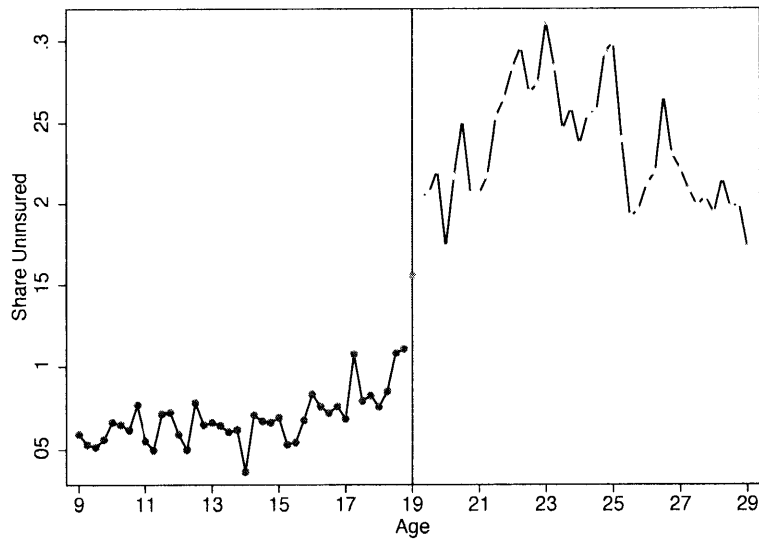


Figure 1-4: Total ER Visits, MA Data

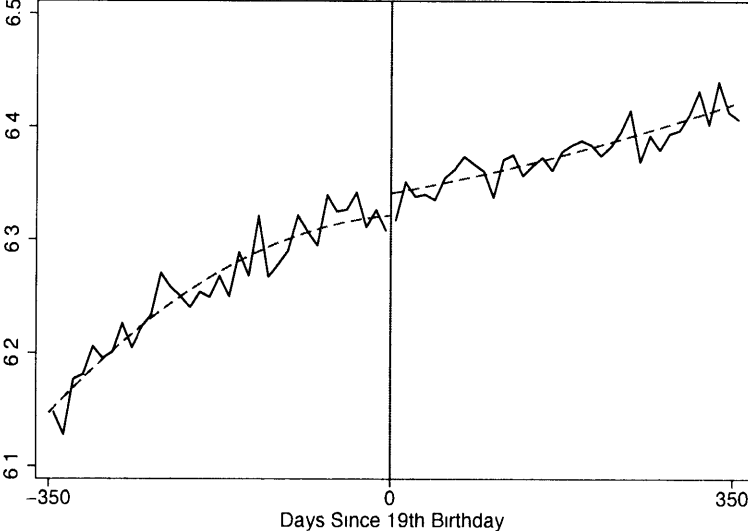


Figure 1-5: ER Visits from Low-Income Zip Codes, MA Data

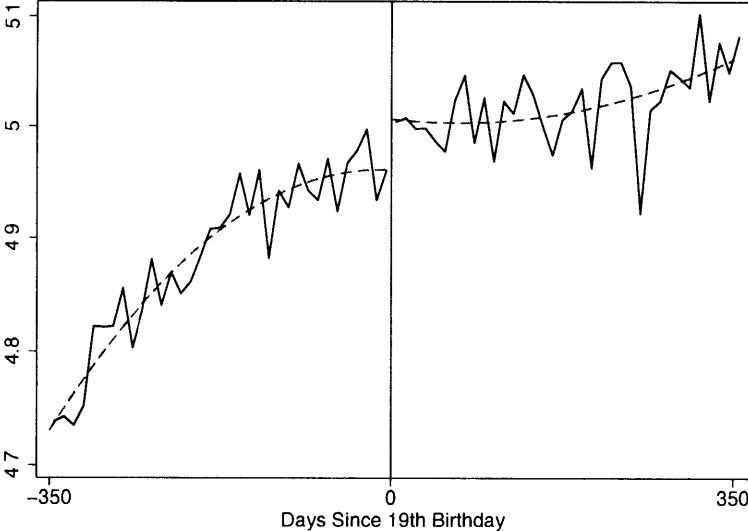


Figure 1-6: RD Estimates by Race, MA Data

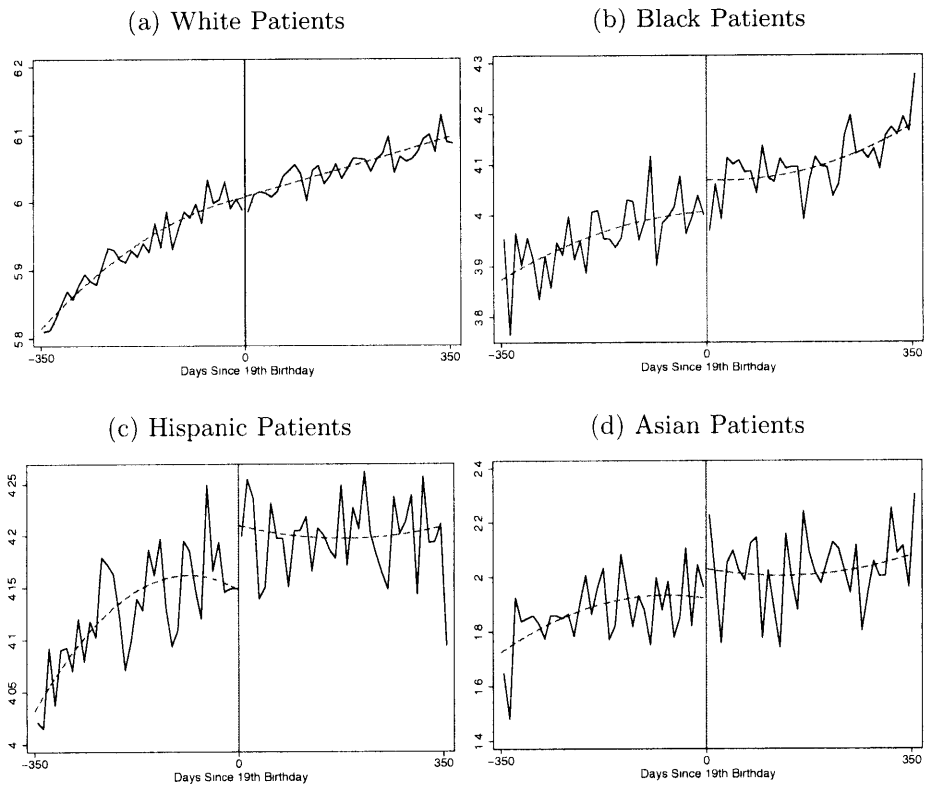


Figure 1-7: Asthma ER Visits, MA Data

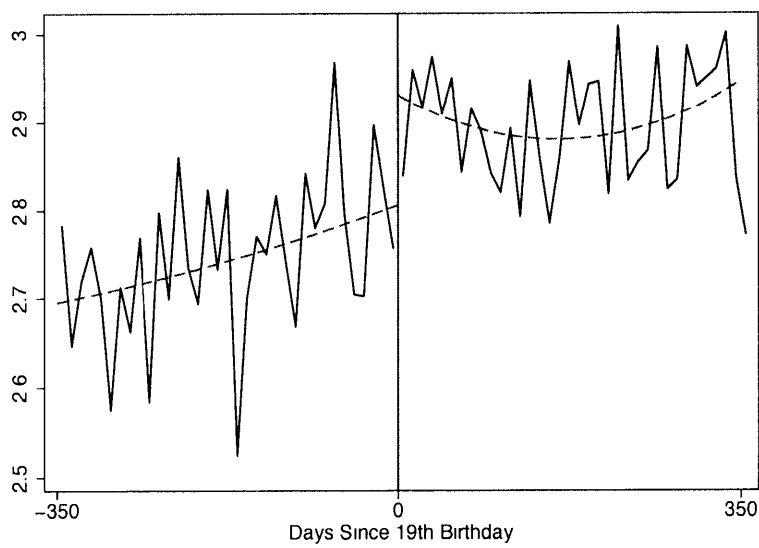


Figure 1-8: Pharyngitis ER Visits, MA Data

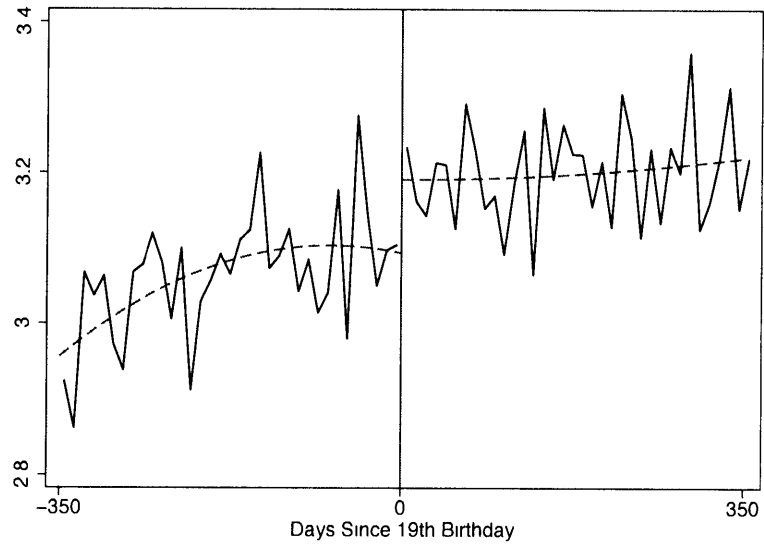


Figure 1-9: Delayed Medical Care because of Cost, NHIS

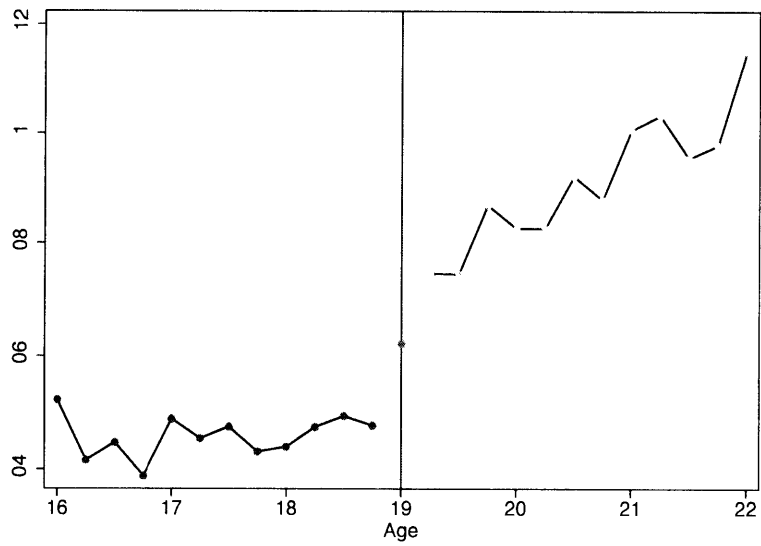


Figure 1-10: Could Not Afford Medical Care, NHIS

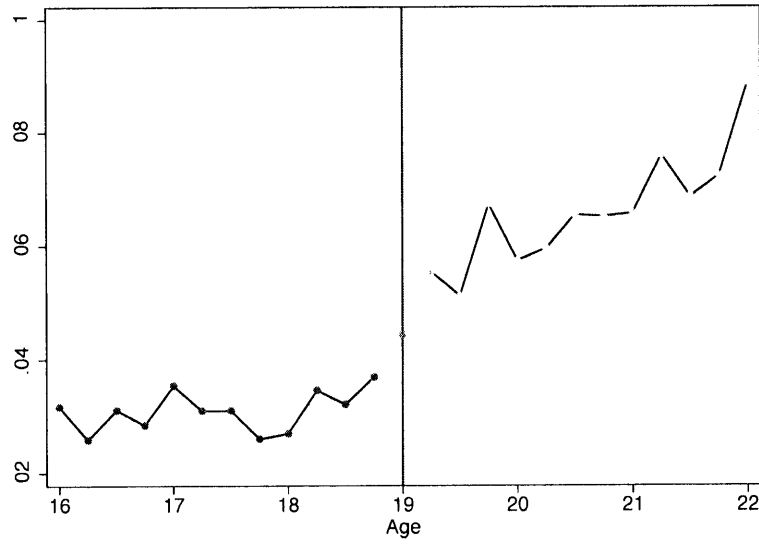


Figure 1-11: Doctors Visits by Age. NAMCS

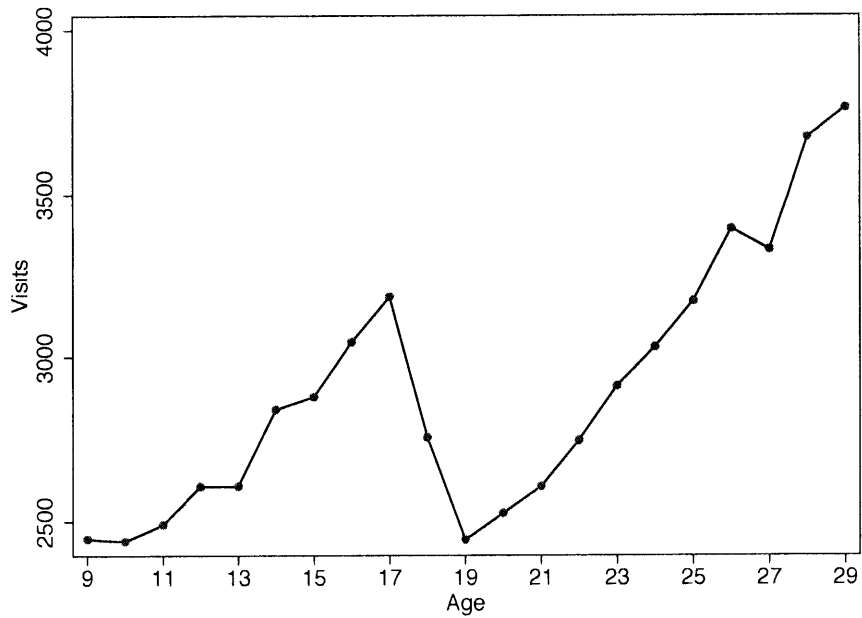


Figure 1-12: Share of Doctor Visits that are Uninsured, NAMCS

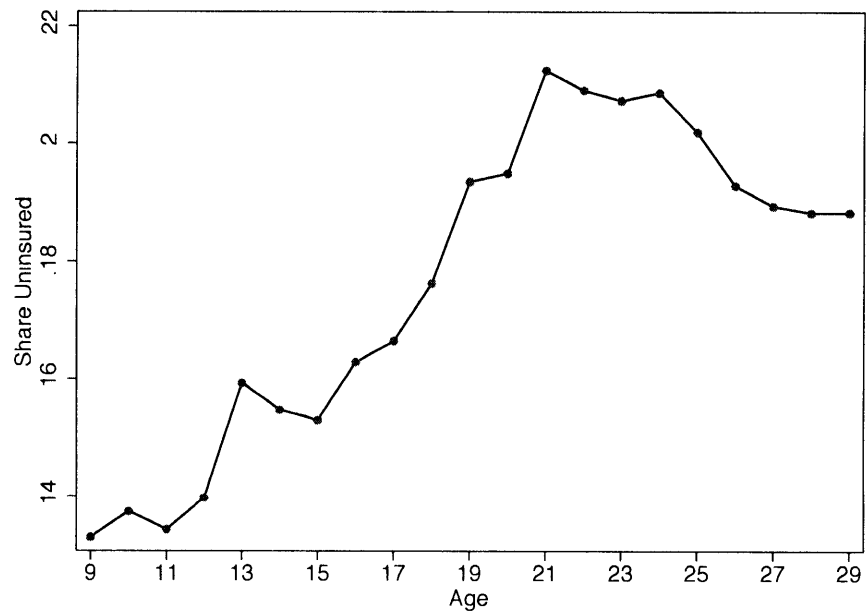




Table 1.1: Descriptive Statistics

	(1)	(2)	(3)
Sample	All	Before 19	After 19
Total Visits	389,966	183,901	206,065
% White	72.2	72.2	72.3
% Black	10.2	10.1	10.4
% Hispanic	28.5	28.3	28.8
% 1st Time Visits	37.6	39.6	35.7
% Return, 30 Days	19.2	18.4	19.9
% Uninsured	21.3	12.7	29.0

Note: Sample restricted to all MA ER visits within 350 days of the patient's nineteenth birthday.

Table 1.2: Effect of Age Nineteen on Insurance Status, NHIS

	Dependent Variable: Indicator Variable Equal to One (Zero) if Uninsured (Insured)					
	(1)	(2)	(3)	(4)	(5)	(6)
Sample	All	Whites	Blacks	Hispanics	Males	Females
Age > 19	0.093 (0.016)	0.088 (0.011)	0.104 (0.041)	0.125 (0.036)	0.126 (0.020)	0.059 (0.020)
<i>p</i> -value	0.00	0.00	0.01	0.00	0.00	0.01
R <sup>2</sup>	0.060	0.053	0.055	0.097	0.091	0.035
<i>N</i>	36,409	21,710	5,554	7,585	18,033	18,376

Note: Standard errors in parentheses clustered on age in quarters. Reported *p*-values correspond to a test that the coefficient of the indicator variable for over age nineteen is equal to zero. All regressions include a quadratic function of days since the patient's nineteenth birthday fully interacted with an indicator variable equal to one if the patient is nineteen. NHIS sample restricted to all NHIS respondents between ages nine and twenty-nine, living in the northeast region.

Table 1.3: Effect of Age Nineteen on Type of Insurance, NHIS

Dependent Variable: An Indicator Variable Equal to One if Patient has  
Given Insurance Status and Zero Otherwise

	(1)	(2)	(3)	(4)
Status	Uninsured	Private	Medicaid	Other Public
Age > 19	0.093 (0.016)	-0.043 (0.014)	-0.051 (0.013)	-0.005 (0.005)
<i>p</i> -value	0.00	0.00	0.00	0.31
R <sup>2</sup>	0.060	0.009	0.021	0.001
<i>N</i>	36,409	36,409	36,409	36,409

Note: Standard errors in parentheses clustered on days since the patient's nineteenth birthday. Reported *p*-values correspond to a test that the coefficient of the indicator variable for over age nineteen is equal to zero. All regressions include a quadratic function of days since the respondent's nineteenth birthday fully interacted with an indicator variable equal to one if the respondent is nineteen.

Table 1.4: Effect of Insurance Status on Total ER Visits, MA ER

	Dependent Variable: Log Count of Patients from Given Sample				
	(1)	(2)	(3)	(4)	(5)
Bandwidth	350	300	250	200	150
Total Visits	0.019 (0.010)	0.011 (0.011)	0.013 (0.012)	0.023 (0.013)	0.023 (0.015)
Low-Income Zip Codes	0.046 (0.019)	0.035 (0.020)	0.042 (0.022)	0.038 (0.024)	0.035 (0.028)
Other Zip Codes	0.012 (0.012)	0.004 (0.013)	0.003 (0.014)	0.017 (0.015)	0.021 (0.018)

Note: Each cell presents results from a regression of log counts of visits by patients of the given group using the given bandwidth. Standard errors in parentheses. All regressions include a quadratic function of days since the patient's nineteenth birthday fully interacted with an indicator variable equal to one if the patient is nineteen.

Table 1.5: Effect of Insurance Status on Total ER Visits by Race and Gender, MA ER

	Dependent Variable: Log Counts of Patients from Given Group						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sample	White	Black	Hispanic	Asian	Other	Male	Female
Age > 19	0.001 (0.012)	0.063 (0.033)	0.062 (0.026)	0.109 (0.088)	0.099 (0.051)	-0.014 (0.014)	0.045 (0.015)
<i>p</i> -value	0.910	0.058	0.018	0.215	0.052	0.318	0.002
R <sup>2</sup>	0.673	0.25	0.131	0.044	0.148	0.514	0.63
<i>N</i>	701	701	701	700	701	701	701

Note: Standard errors in parentheses. Reported *p*-values correspond to a test that the coefficient of the indicator variable for over age nineteen is equal to zero. All regressions include a quadratic function of days since the patient's nineteenth birthday fully interacted with an indicator variable equal to one if the patient is nineteen.

Table 1.6: Falsification Check: Effect of Age Twenty on Total ER Visits, MA ER

	Dependent Variable: Log Counts of Visits by Patients from Given Group							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample	All	White	Black	Hispanic	Asian	Other	Male	Female
Age > 19	-0.004	0.002	-0.044	-0.010	0.159	-0.049	-0.011	0.002
	(0.011)	(0.012)	(0.031)	(0.029)	(0.083)	(0.042)	(0.015)	(0.015)
<i>p</i> -value	0.710	0.889	0.159	0.723	0.056	0.243	0.471	0.910
R <sup>2</sup>	0.287	0.243	0.049	0.012	0.038	0.026	0.273	0.091
<i>N</i>	701	701	701	701	701	701	701	701

Note: Standard errors in parentheses. Reported *p*-values correspond to a test that the coefficient of the indicator variable for over age twenty is equal to zero. All regressions include a quadratic function of days since the patient's nineteenth birthday fully interacted with an indicator variable equal to one if the patient is twenty.

Table 1.7: Effect of Insurance Status on Avoidable Hospitalizations, MA ER

Dependent Variable: Log Counts of Patients with Given Medical Condition						
	(1)	(2)	(3)	(4)	(5)	(6)
Condition	ENT	UTI	Asthma	Cellulitis	Dehyd.	Gastro.
Age > 19	0.067 (0.038)	0.009 (0.053)	0.126 (0.056)	-0.068 (0.080)	-0.095 (0.100)	-0.165 (0.095)
<i>p</i> -value	0.082	0.869	0.025	0.396	0.344	0.082
R <sup>2</sup>	0.212	0.119	0.097	0.118	0.047	0.077
<i>N</i>	701	701	701	701	701	698

Note: Standard errors in parentheses. Reported *p*-values correspond to a test that the coefficient of the indicator variable for over age nineteen is equal to zero. All regressions include a quadratic function of days since the patient's nineteenth birthday fully interacted with an indicator variable equal to one if the patient is nineteen. Abbreviations: ENT, "ear nose and throat infections"; UTI, "urinary tract infection"; Dehyd, "dehydration"; Gastro, "gastroenteritis".

Table 1.8: Effect of Insurance Status on Most Common Conditions, MA ER

Dependent Variable: Log Counts of Patients with Given Medical Condition					
	(1)	(2)	(3)	(4)	(5)
Condition	Ankle Sprain	Back Sprain	Abdominal Symptoms	Upper Limb Contusion	Pharyngitis
Age > 19	-0.069 (0.061)	0.014 (0.040)	0.029 (0.041)	-0.056 (0.058)	0.097 (0.047)
<i>p</i> -value	0.261	0.717	0.484	0.336	0.040
R <sup>2</sup>	0.012	0.115	0.243	0.016	0.114
<i>N</i>	701	701	701	701	701

Note: Standard errors in parentheses. Reported *p*-values correspond to a test that the coefficient of the indicator variable for over age nineteen is equal to zero. All regressions include a quadratic function of days since the patient's nineteenth birthday fully interacted with an indicator variable equal to one if the patient is nineteen.



Table 1.9: Effect of Insurance Status on Composition of Visits, MA ER

Dependent Variable: Log counts of Patients with Given Characteristic				
	(1)	(2)	(3)	(4)
	Weekend Visits	Weekday Visits	New Patients	Old Patients
Age > 19	-0.013	0.035	0.051	0.001
	(0.024)	(0.014)	(0.015)	(0.014)
<i>p</i> -value	0.581	0.011	0.001	0.962
R <sup>2</sup>	0.337	0.642	0.077	0.791
<i>N</i>	701	701	701	701

Note: Standard errors in parentheses. Reported *p*-values correspond to a test that the coefficient of the indicator variable for over age nineteen is equal to zero. All regressions include a quadratic function of days since the patient's nineteenth birthday fully interacted with an indicator variable equal to one if the patient is nineteen.

Table 1.10: Effect of Insurance Status on Patient Recidivism, MA ER

Dependent Variable: Log Counts of Patients who Returned to ER in Given Number of Days			
	(1)	(2)	(3)
	10 Day Return	20 Day Return	30 Day Return
Age > 19	0.061 (0.028)	0.055 (0.025)	0.050 (0.023)
<i>p</i> -value	0.028	0.025	0.027
R <sup>2</sup>	0.521	0.562	0.577
<i>N</i>	701	701	701

Note: Standard errors in parentheses.  
Reported *p*-values correspond to a test  
that the coefficient of the indicator  
variable for over age nineteen is equal to  
zero. All regressions include a quadratic  
function of days since the patient's  
nineteenth birthday fully interacted with  
an indicator variable equal to one if the  
patient is nineteen.

Table 1.11: Benchmarks of Health Care Costs at Age Nineteen

	Percent Change in ER Visits	Visits Per- Capita	Estimated Change in Costs	Average Health Care Costs Per- Capita
All	0.019	0.288	\$1.69	\$1,216.37
White	0.001	0.269	\$0.10	\$1,357.43
Black	0.063	0.506	\$9.58	\$983.70
Hispanic	0.062	0.368	\$6.79	\$890.90

Table 1.12: Effect of Age Nineteen on Expected Payment Status Conditional on Being Uninsured, MA ER

Dependent Variable: An Indicator Variable Equal to One (Zero) if Patient is Expected to Receive Free-Care (Self-Pay)

	(1)	(2)	(3)	(4)	(5)
Sample	White	Black	Hispanic	Asian	Other
Age > 19	0.086 (0.013)	0.107 (0.030)	0.123 (0.028)	0.144 (0.093)	0.061 (0.049)
p-value	0.00	0.00	0.00	0.12	0.22
R <sup>2</sup>	0.023	0.013	0.017	0.035	0.012
N	50,182	11,452	11,059	933	3,884

Note: Sample restricted to uninsured patients. Standard errors in parentheses clustered on days since the patient's nineteenth birthday. Reported p-values correspond to a test that the coefficient of the indicator variable for over age nineteen is equal to zero. All regressions include a quadratic function of days since the patient's nineteenth birthday fully interacted with a dummy equal to one if the patient is nineteen.

# Chapter 2

## Medicaid and Consumer Bankruptcies

Joint with Matthew Notowidigdo

### 2.1 Introduction

Bankruptcy is a legal procedure designed to forgive debtors their debt. Originally bankruptcy was a rare occurrence, undertaken by few debtors (Zywicki, 2005). But over the past two decades, bankruptcy has become common. In the 1990s, the number of personal bankruptcies in the United States rose by more than 85 percent. By the end of the decade, more than one percent of American households were declaring bankruptcy in any given year. Stavins (2000), for instance, calculates that 8.5 percent of American households have ever filed for bankruptcy.

This trend has motivated research on factors that induce households to declare bankruptcy. A common conjecture is that a large fraction of consumer bankruptcies are driven by out-of-pocket medical costs. This hypothesis is supported by a wide variety of anecdotal evidence and is also forcefully argued in several widely-publicized observational studies.<sup>1</sup> In this chapter, we test that hypothesis. To do

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<sup>1</sup>For example, the American Association of Retired Persons has publicized anecdotal evidence on medical bankruptcy as part of its political campaign, “Divided We Fail” ([www.dividedwefail.org](http://www.dividedwefail.org)). A bill proposed in congress, “The Medical Bankruptcy Fairness Act of 2008,” would have raised the

so, we exploit plausibly exogenous variation in publicly-provided health insurance. In the 1990s, states expanded access to publicly-provided health insurance both by expanding eligibility for Medicaid and through the State Children's Health Insurance Program (SCHIP). Medicaid and health insurance through SCHIP drastically decrease the medical costs faced by households. Using cross-state variation in these expansions from 1992 to 2002, we find that Medicaid and SCHIP eligibility also reduce bankruptcy risk. In our preferred specification, we calculate that a ten percentage point increase in eligibility for publicly-provided insurance reduces the personal bankruptcy rate by roughly eight percent. Our robustness tests are generally consistent with this finding. As a falsification test, we find no evidence that business bankruptcies are similarly affected. We conclude that out-of-pocket medical costs play a role in consumer bankruptcy risk and we utilize several calibration exercises to quantify that role.

This chapter demonstrates an interaction between two types of social insurance. Bankruptcy is one form of social insurance, allowing households to smooth consumption despite shocks that lead to formal debt (White, 2005). Medicaid is a second form of social insurance, providing health insurance to eligible members of low-income households. This chapter demonstrates that the generosity of Medicaid affects the utilization of the bankruptcy system. We present a theoretical model that examines this interaction directly, and outlines implications for the joint optimality of both programs.

The chapter proceeds as follows. The subsequent section discusses the state of research on personal bankruptcy. Section 2.3 develops a simple model of the interaction between bankruptcy and Medicaid, and discusses the normative implication of such an interaction. Section 2.4 describes the state Medicaid expansions and discusses our empirical strategy. Section 2.5 presents our main results, and section 2.6 estimates the share of bankruptcies driven by medical costs. Section 2.7 concludes.

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homestead exemption and lowered penalties for debtors declaring bankruptcy for a medical cause.

## 2.2 Previous Research on the Determinants of Consumer Bankruptcy

A large literature has explored the determinants of consumer bankruptcy. The research generally falls into two categories. One strand of research emphasizes the strategic nature of the household bankruptcy decision. These papers document that households are forward-looking and optimally choose whether or not to file for bankruptcy based on expected financial benefit. Households take the generosity of the bankruptcy system into account in making savings and investment decisions; that is, the bankruptcy system creates an *ex ante* moral hazard problem. The second category of papers attempt to quantify the role of adverse, potentially-unforeseen shocks that lead to consumer bankruptcies.

An example of the second category is a study by Himmelstein et al. (2005), which estimates that medical costs are pivotal in more than half of all consumer bankruptcies. The authors interview bankruptcy filers and find that 54 percent of respondents list “any medical cause” when asked what led them to declare bankruptcy. The finding confirms several qualitative studies that point to adverse events as the primary driver of personal bankruptcy (Braucher 1993 and Sullivan et al. 1989).

A concern with such observational studies, however, is that the authors define medical costs very broadly. They include the birth or death of a family member, alcoholism, drug addiction, and uncontrolled gambling as “any medical cause.” Dranove and Millenson (2006) re-analyze the same survey data using a narrower definition of medical causes and attribute far fewer bankruptcies to medical causes. They estimate that 17 percent of bankruptcies are due to medical causes, most of which involve low-income households.

On the other hand, many researchers have documented evidence of strategic bankruptcy behavior. White (1987) studies how households respond to the generosity of the local homestead exemption.<sup>2</sup> She finds that households facing more generous

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<sup>2</sup>The homestead exemption is the amount of home equity that households are able to preserve following a bankruptcy filing. There is a federal minimum, but many states choose levels well above this minimum, and some states have chosen unlimited homestead exemptions.

exemptions are more likely to declare bankruptcy. Similarly, Fay et al. (2002) study a sample of respondents to the Panel Study of Income Dynamics (PSID) who have declared bankruptcy. They find that households are more likely to declare bankruptcy when the financial benefits of declaring bankruptcy are higher.

Lastly, several papers have focused on stigma and the availability of consumer credit. Gross and Souleles (2001) analyze individual credit card accounts and conclude that the stigma of declaring bankruptcy has diminished over time. Zywicki (2005) discusses trends related to personal bankruptcy and reaches a similar conclusion. Finally, Livshits et al. (2007) estimate a structural model of the household financial decisions to quantify the relative contributions of potential drivers of personal bankruptcy. The authors conclude that the rise in personal bankruptcy is driven mainly by the increasing availability of consumer credit and a decline in the cost of filing for bankruptcy (perhaps through changes in social stigma), rather than by uncertainty or medical shocks.

A main drawback to much of this previous literature is that the papers do not employ quasi-experimental variation in the determinants of bankruptcy they study. This empirical challenge cannot be overcome by exploiting changes in the laws that govern bankruptcy, as such laws rarely change. The state laws that govern homestead exemptions, for example, have rarely been modified since their inception (Gropp et al., 1997).<sup>3</sup> To our knowledge, this chapter is the first to document the relative importance of medical costs in the bankruptcy decision using plausibly-exogenous variation in medical costs.

## **2.3 Theoretical Background on the Intersection between Medicaid and Bankruptcy**

This chapter documents an interaction between two types of social insurance: the consumer bankruptcy system and Medicaid. Bankruptcy is one form of insurance, serving as an insurer-of-last-resort for all types of economic shocks that lead to formal

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<sup>3</sup>A recent exception is the Bankruptcy Abuse Prevention and Consumer Protection Act, passed by Congress in 2005.



debt. Medicaid provides insurance for health shocks occurring to eligible individuals. When Medicaid eligibility is expanded, newly eligible individuals may substitute away from bankruptcy to insure their health shocks. That interaction provides preliminary evidence on the joint optimality of the two programs.

This section offers a model of the interaction between bankruptcy and Medicaid. Specifically, it derives sufficient statistics (Chetty, 2008) which map reduced-form results to welfare. Our model is based on the simple one-period optimal insurance problem analyzed by Chetty (2006). The agent faces two types of shocks: health shocks and productivity shocks, receiving the former with fixed probability  $p_H$ . When sick the agent chooses  $m$  units of medical consumption at price  $1 - b_H$ , where  $b_H$  is the co-insurance rate provided by the government. The value of medical consumption is captured by a concave, increasing function,  $v(m)$ .

The agent suffers a productivity shock with probability  $p_B(e, m)$ , where  $e$  is effort exerted ex ante to avoid the productivity shock. This effort is costly, with convex cost  $f(e)$ . We assume a stylized version of the bankruptcy system that captures the nature of bankruptcy as social insurance, but for simplicity, we do not explicitly model the financial decision taken by the debtor. Specifically, we assume that if the agent suffers a productivity shock, the agent files for bankruptcy and must pay a fixed amount of debt,  $D$ , and that the bankruptcy system dissolves a share  $b_B$  of that debt.<sup>4</sup> Note that in the derivation to follow, the probability of a productivity shock may depend on whether the agent has suffered a health shock. This allows for out-of-pocket medical costs to increase bankruptcy risk, which in practice might be one mechanism through which health insurance benefits affect bankruptcy risk.

Suppose that the social planner imposes a lump-sum tax of  $\tau$  in each state of the world. Denote as  $c$  the agent's consumption in the case of no shocks. In that case,

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<sup>4</sup>The fixed amount of debt is a simplification. In reality, the debt level is likely affected by the level of ex ante moral hazard and the generosity of both insurance systems. One way to incorporate this is to make the choice of  $D$  endogenous, which results in an additional term in each of the optimal insurance formulas: the elasticities of  $D$  with respect to  $b_H$  and  $b_B$ . In practice, the level of borrowing does not appear to be substantially affected by Medicaid generosity (Gruber and Yelowitz, 1999), so we do not think this additional term is empirically important, while the moral hazard from health insurance and the interaction effect we estimate are both quantitatively important.

the agent's consumption is simply her wealth less taxes:

$$c = W - \tau.$$

If the agent suffers a health shock but no productivity shock, she chooses  $m$  units of medical care, but is partially compensated by the government, so that:

$$c_H = W - \tau - (1 - b_H) \cdot m.$$

When the agent suffers a productivity shock but no health shock, her consumption is:

$$c_B = W - \tau - (1 - b_B) \cdot D.$$

Finally, the agent may suffer both a productivity and health shock, in which case her consumption is:

$$c_{BH} = W - \tau - (1 - b_H)m - (1 - b_B)D.$$

Under these assumptions, the agent solves the following problem:

$$\begin{aligned} V^*(b_H, b_B, \tau) \equiv \max_{m, e} & \quad p_H p_B(e, m)(u(c_{BH}) + v(m)) + \\ & \quad (1 - p_H)p_B(e, m)u(c_B) + \\ & \quad p_H(1 - p_B(e, m))(u(c_H) + v(m)) + \\ & \quad (1 - p_H)(1 - p_B(e, m))u(c) - \\ & \quad f(e). \end{aligned}$$

The social planner takes the agent's actions as given and maximizes  $V^*$  subject to the following resource constraint:

$$\tau = p_H b_H m + p_B(e, m) b_B D.$$

By the envelope theorem, optimal health insurance benefits are given by:

$$\frac{p_B u'(c_{HB}) + (1 - p_B) u'(c_H)}{\bar{u}'} = 1 + \frac{d \log m}{d \log b_H} + \frac{p_B b_B D}{p_H b_H m} \cdot \frac{d \log p_B}{d \log b_H} \quad (2.1)$$

where  $\bar{u}'$  is the agent's expected marginal utility of consumption. Equation (2.1) is analogous to the formulas for optimal insurance derived in Baily (1978).<sup>5</sup> This expression shows that the social planner will provide full health insurance if both of the following two conditions hold: (1) medical consumption does not respond to the health insurance benefit rate and (2) the probability of bankruptcy does not respond to the health insurance benefit rate. If the right-side of equation (2.1) is greater than 1, then less than full insurance is optimal.

A long literature in health economics has estimated the moral hazard elasticity of health consumption (the first elasticity on the right-hand side of (2.1)), most notably in the RAND health insurance experiment (Manning et al., 1987). The literature generally suggests a positive but small moral hazard elasticity ( $\frac{d \log m}{d \log b_H} \approx 0.2$ ). To our knowledge, this paper is the first to estimate the second elasticity: the elasticity of the bankruptcy rate with respect to the generosity of health insurance. Below, we estimate that this elasticity is negative—Medicaid expansions reduce the bankruptcy rate. This model suggests that the reduction in bankruptcies stemming from Medicaid expansions justifies a more generous Medicaid expansion than would be implied by the standard calculation.

## 2.4 Empirical Strategy and Data

This section briefly describes the Medicaid expansions we study, the data we use, and our empirical framework.

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<sup>5</sup>If there is no bankruptcy system ( $p_B = 0$ ), then equation (2.1) simplifies to:

$$\frac{u'(c_H)}{\bar{u}'} = 1 + \frac{d \log m}{d \log b_H}$$

which is the expression derived in Baily (1978).

### 2.4.1 Background on Medicaid Expansions

In the mid-1990s, states expanded Medicaid eligibility to cover all young children below 133% of the federal poverty line. In 1997, the Medicaid program was augmented further with the introduction of the State Children's Health Insurance Program (SCHIP). This program expanded Medicaid eligibility for children and pregnant women. Many states went beyond the minimum federally-required extended eligibility. For example, New Jersey offered Medicaid to children under 350% of the federal poverty line (see Gruber and Simon 2007 and Gruber 2000 for more details on the Medicaid program). Many states also expanded eligibility for parents in conjunction with their SCHIP expansions. Crucially for our estimation strategy, states expanded Medicaid eligibility at different times, and states chose to expand eligibility by different amounts during this time period.

Figure 2-2 plots the overall increase in Medicaid eligibility from 1992 through 2002. Overall, roughly 10% of all U.S. households became eligible for Medicaid during this time. To demonstrate the potential effects of this expansion on consumer finances, we turn to the Medical Expenditure Panel Survey (MEPS). The MEPS collects detailed records on out-of-pocket medical costs for a nationally-representative sample of households. Figure 2-3 plots the distribution of out-of-pocket medical costs for two groups of households: those with at least one family member eligible for Medicaid and those with no family members eligible. For this cross-section, the figure demonstrates that Medicaid beneficiaries face a dramatically lower risk of large out-of-pocket medical costs. Roughly two percent of the uninsured spend more than \$5,000 in out-of-pocket medical costs, while less than 0.2 percent of Medicaid beneficiaries spend more than \$5,000 in out-of-pocket medical costs. Such a cross-sectional pattern does not conclusively demonstrate a causal relationship, and the MEPS sample is too small to employ an instrumental variable for Medicaid participation. Nevertheless, figure 2-3 provides suggestive evidence that Medicaid substantially reduces financial risk, especially in the right tail. Our regressions below will test whether this potential drop in financial risk lowers the risk of a personal bankruptcy.

## 2.4.2 Data

Our investigation into bankruptcy and public insurance requires accurate measures of both types of insurance. For the former, we rely on the publicly-available census of consumer and business bankruptcies. This census is published annually by the Administrative Office of the US Courts, and has been used in other, related papers.<sup>6</sup> The census is composed of simple counts of cases for each bankruptcy district since the 1980s. There are 94 bankruptcy districts, with one to four districts per state. We exclude bankruptcy districts in US territories and compile counts of bankruptcies by state and year.<sup>7</sup>

We construct measures of public insurance eligibility from the March Current Population Survey (CPS). First, we calculate whether each surveyed household is eligible for Medicaid in their state of residence and year given the household's income, number of children, and gender of the head of household. Denote that share as  $M_{it}$  for state  $i$  during year  $t$ . We also perform a similar procedure to calculate the state-year's simulated eligibility. We take a 20% national sample from the 1996 CPS and calculate the share of this fixed population that would be eligible for Medicaid in each state and year. Denote the share of such households as  $\tilde{M}_{it}$ .<sup>8</sup>

Table 2.1 presents descriptive statistics. On average, bankruptcy districts process roughly ten-thousand bankruptcies each year. During the 1990s, bankruptcy counts nearly doubled. The table presents descriptive statistics for the five states with the smallest expansion of Medicaid and the five states with the largest expansion of Medicaid during our sample period. The table demonstrates that the states with especially large Medicaid expansions experienced a smaller increase in the number of bankruptcies over the 1990s.<sup>9</sup>

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<sup>6</sup>See, for example, Fay et al. (2002).

<sup>7</sup>The excluded bankruptcy districts are those in the Virgin Islands, Puerto Rico, Northern Mariana Islands, and Guam.

<sup>8</sup>We are grateful to Kosali Simon for computer code that constructs these two variables.

<sup>9</sup>The large expansion states are California, Tennessee, Massachusetts, Minnesota, and Montana. The small expansion states are South Carolina, Texas, North Carolina, North Dakota, and Idaho.

### 2.4.3 Empirical Strategy

As a summary of our approach and main results, consider figure 2-4. The figure plots for each state the difference in log consumer bankruptcies between 1992 and 2002 against the change in simulated Medicaid eligibility over that time period. The figure demonstrates that states with larger Medicaid expansions experienced a smaller increase in bankruptcies over the 1990s.<sup>10</sup> Our empirical strategy is similar. We compare the change in the consumer bankruptcy rate across states with varying changes in Medicaid generosity. Figure 2-4 suggests that a ten percentage point increase in Medicaid eligibility reduces consumer bankruptcies by roughly ten percent. In what follows, we use a regression framework to rigorously test this pattern.

We model the relationship between Medicaid eligibility and the consumer bankruptcy rate as:

$$\log(c_{it}) = \alpha_i + \gamma_t + \beta M_{it} + X'_{it}\phi + \varepsilon_{it}, \quad (2.2)$$

where  $c_{it}$  denotes the number of consumer bankruptcies in state  $i$  and year  $t$ ,  $M_{it}$  denotes the fraction of the population eligible for Medicaid,  $X_{it}$  denotes log average earnings and the unemployment rate, and  $\varepsilon_{it}$  represents unobserved state-year shocks that affect the number of consumer bankruptcies.

Simply estimating equation (2.2) with ordinary least squares would lead to biased estimates of  $\beta$ . Adverse economic shocks will lead to both more consumer bankruptcies and to more households qualifying for Medicaid. For that reason, we use simulated Medicaid eligibility as an instrumental variable for actual Medicaid eligibility.<sup>11</sup> Simulated Medicaid eligibility is correlated with actual Medicaid eligibility (the  $t$  statistic for simulated eligibility from our first stage regression is 18.10), but is assumed not to be correlated with adverse economic shocks. This key, identifying assumption implies that absent changes in Medicaid eligibility, state bankruptcy rates would have evolved similarly over time. We begin by estimating equation (2.2) using

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<sup>10</sup>The slope of the regression line is -1.02 with a standard error of 0.34.

<sup>11</sup>Simulated instrumental variables for Medicaid eligibility were introduced by Currie and Gruber (1996). Simulated instruments for Medicaid have also been used by Gruber and Yelowitz (1999), Gruber and Cutler (1996), DeLeire et al. (2007) and Gruber and Simon (2007).

instrumental variables under this assumption. We then investigate the validity of this assumption in several ways. We explore our methodology's robustness to unique state trends in bankruptcy rates, to time-varying control variables, and to models with lagged-dependent variables.

## 2.5 The Aggregate Effect of Medicaid on Bankruptcies

Table 2.2 presents our main results. The first column shows the OLS relationship between Medicaid eligibility and state bankruptcy counts. This relationship is strongly negative. The second column reports the IV estimates; a ten percentage-point increase in Medicaid eligibility reduces consumer bankruptcies by 8.7 percent. Adverse economic shocks are positively correlated with bankruptcies along with actual Medicaid eligibility. As a result, the IV estimates are larger in absolute value than the OLS estimates.

The remainder of table 2.2 reports the results of a falsification test. One would expect Medicaid to have little impact on business bankruptcies; few businesses are both nearly bankrupt and have many employees eligible for Medicaid. The third and fourth columns of table 2.2 present OLS and IV results for business bankruptcies. Both point estimates are less than four percent the magnitude of the point estimates for consumer bankruptcy, and are statistically indistinguishable from zero. Table 2.2 thus demonstrates a strong negative relationship between Medicaid eligibility and personal bankruptcies.

We turn next to specification tests designed to explore the robustness of these findings. Table 2.3 reports results of several alternative specifications. The first column reproduces our preferred specification. The second column drops state unemployment rate and log average earnings, included as controls in our preferred specification. Without these controls, the magnitude of the coefficient on Medicaid eligibility increases slightly and is still strongly significant.

Column three presents reduced-form estimates in order to test whether a two-year lead or lag of simulated Medicaid eligibility is a potential confounder. The lagged effect of eligibility is marginally significant, but the lead of eligibility is statistically indistinguishable from zero. We find these results reassuring, as they suggest that the contemporaneous effect of eligibility on bankruptcy is not simply a proxy for future changes.

A remaining concern is that state bankruptcies may follow trends correlated with Medicaid expansions. The remainder of table 2.3 addresses that concern by testing whether the effect of Medicaid expansions can be distinguished from a linear time-trend. Column five presents results that include a linear time-trend for each of the nine census regions. Such region time-trends have little effect on our estimates or precision. Column six includes region-year fixed effects, with a similar result. Finally, column seven presents a more stringent test: including state-specific linear time-trends. Relative to the other columns, the results change dramatically. State trends lead to a much smaller point estimate and a slight decrease in the standard errors.

Strictly interpreted, column seven suggests little interaction between Medicaid and bankruptcy. The point estimate implies that a ten percentage point expansion of Medicaid would lead to a two percent decrease in bankruptcies, but this estimate is statistically indistinguishable from zero. While this result is disturbing, the data contain only eleven years of bankruptcy records per state. Thus adding state trends to our baseline two-stage least squares specification is demanding. Moreover, many states rolled out their Medicaid eligibility expansions over time, making eligibility well-approximated by a state-specific trend.

Some states, however, had either no Medicaid expansions or only one major expansion during this time period. We label these states “sharp expansion states,” because their Medicaid eligibility trends are much better approximated by a step function than by a single, positively-sloped line.<sup>12</sup> Column eight presents the baseline specifi-

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<sup>12</sup>Specifically, we categorize a state as a sharp expansion state if it expanded eligibility by more than two percent two or fewer times within the sample. The sharp expansion states are AK, AL, AZ, CO, IL, KY, LA, MI, MS, MT, NC, ND, NJ, NM, NY, OK, OR, RI, SC, SD, TN, TX, UT, VA, WI, WV, WY.



cation restricted to these 23 states. The coefficient on Medicaid eligibility drops by roughly 30% (from -0.870 to -0.581) but is marginally significant ( $p = 0.061$ ). Column nine adds state-specific linear trends to this specification. For these states, the point estimate is unchanged when state trends are added. We conclude that state-specific trends absorb the identifying variation for states that expanded Medicaid smoothly over time. For the sharp expansion states, however, state-specific linear trends do not substantially affect the results.

Another concern with our baseline specification is that Medicaid expansions may affect bankruptcy rates via more complex adjustment dynamics. If bankruptcy rates require several years to react to changes in public insurance, than the previous regressions, which focus only on a contemporaneous effect, would not capture the full effect. Table 2.4 explores alternative specifications designed to address that concern. The second column presents the results of a regression on three-year averages of all variables. The results are similar to the baseline estimates ( $-0.870$  versus  $-0.715$ ). The third column presents estimates when only three years of data are included (1992, 1997, and 2002) to measure longer-run responses to changes in eligibility. The point estimates remain similar to our preferred specification, suggesting that short-run and long-run responses to changes in Medicaid eligibility do not appear to be very different.

The last two columns of table 2.4 present specifications with lagged dependent variables. Column four presents results that include the consumer bankruptcy count of the previous year. Since the lagged bankruptcy count is endogenous, column five presents the same regression with a three-year lag of the dependent variable as an instrumental variable for the one-year lag. The results are somewhat imprecise, but the implied long-run effect is  $-0.332/(1 - 0.605) = -0.840$ , with a standard error (calculated using the delta method) of 0.472. This point estimate is very similar to our baseline result.

Lastly, if public insurance indeed reduces bankruptcy risk, we should be able to observe a reduction in the amount of uncompensated care provided by hospitals. To test this, we measure the amount of uncompensated care provided by hospitals using

records of hospital bad debt and charity care from the American Hospital Association's (AHA) annual census of all U.S. hospitals.<sup>13</sup> Unfortunately, we were only able to gather these data for 1994 through 1999.<sup>14</sup> Table 2.5 estimates the effect of Medicaid eligibility on aggregate hospital bad debt and charity care using the state-level AHA data. Column one reports results of our baseline specification using only the years for which we have AHA data. Reassuringly, the results are very similar to the baseline specification for our full sample (a 10 percentage point increase in Medicaid eligibility reduces consumer bankruptcies by 9.0 percent as compared to 8.7 percent in our baseline specification). Columns two through four replace the consumer bankruptcy dependent variable with total hospital bad debt, total hospital charity care, and total uncompensated care (the sum of bad debt and charity care). Although none of the results in these columns are significant at conventional levels, the point estimates are uniformly negative, and the magnitudes are economically large (in column four, the coefficient on Medicaid eligibility suggests that a 10 percentage point increase in Medicaid eligibility reduces hospital uncompensated care by 5.2 percent). We interpret these uncompensated care results as suggestive and broadly consistent with our baseline result that Medicaid eligibility reduces consumer bankruptcy risk.

## 2.6 The Share of Bankruptcies Driven by Medical Costs

Researchers have found that medical costs are pivotal in between 17 and 54 percent of bankruptcies (see Himmelstein et al. 2005 and Dranove and Millenson 2006), depending on the definition of qualifying medical costs. This section offers a simple framework that translates our regression results into estimates directly comparable to those previous observational studies.

Suppose that we decompose the overall bankruptcy rate,  $P(B)$ , into a conditional bankruptcy rate for the low-income population with health insurance,  $I$ , and without

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<sup>13</sup>We are grateful to Damon Seils and Kevin Schulman for assistance with these data.

<sup>14</sup>We have been unable to gain access to more years of uncompensated care data; the American Hospital Association no longer provides access to such numbers, even in aggregate form.

health insurance,  $\neg I$ :

$$P(B) = P(B|I)P(I) + P(B|\neg I)P(\neg I). \quad (2.3)$$

Suppose further that the expansion of Medicaid increases the fraction of the population with health insurance by 10 percentage points (from  $P(I)$  to  $0.10 + P(I)$ ), and that this leads to a new bankruptcy rate,  $\hat{\beta} \times P(B)$ .<sup>15</sup> This leads to the following equation:

$$\hat{\beta} \times P(B) = P(B|I) (P(I) + 0.10) + P(B|\neg I) ((P(\neg I) - 0.10)). \quad (2.4)$$

Equations (2.3) and (2.4) form a system of two linear equations with two unknowns,  $P(B|\neg I)$  and  $P(B|I)$ , given estimates of  $P(B)$ ,  $\beta$ ,  $P(I)$ , and  $P(\neg I)$ . We choose  $P(B) = 0.025$  based on our aggregate bankruptcy statistics and  $P(I) = 0.70$  based on tabulations from the CPS.<sup>16</sup> We use  $\beta = 0.913$  based on our regression results (8.7% decline in bankruptcy rate following 10 percentage point increase in Medicaid eligibility). From equations (2.3) and (2.4) we calculate that  $P(B|\neg I) = 0.040$  and  $P(B|I) = 0.018$ . This implies that—*ceteris paribus*—low-income households without health insurance are roughly two times more likely to file for bankruptcy than insured low-income households.

Universal health insurance for low-income families would simplify the overall bankruptcy rate in (2.3) to  $P(B) = P(B|I)$ . Consequently, the fraction of bankrupt-

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<sup>15</sup>It is well documented that an increase in Medicaid eligibility does not translate into a one-for-one increase in health insurance coverage. Like many social insurance programs, the overall take-up rate of Medicaid is low, so many newly-eligible households continue to remain uninsured. We consider nominally uninsured but Medicaid-eligible households “conditionally insured,” meaning that if such households found themselves in the hospital then the hospital would enroll them in Medicaid.

<sup>16</sup>Overall, roughly one percent of household file for bankruptcy in any given year, but bankruptcy risk is higher for low-income households; Warren (2003) suggests that bankruptcy risk is 2–3 times higher for low-income households.

To estimate the share of low-income households that are uninsured, we calculate the share of uninsured households among households between 100% and 200% of the federal poverty line using the 1996 CPS.

cies that can be attributed to a lack of health insurance is:

$$\frac{P(B) - P(B|I)}{P(B)} \approx 26\%.$$

This estimate is lower than the 54% reported by Himmelstein et al. (2005) and slightly larger than the 17% reported by Dranove and Millenson (2006).

A key issue in comparing this estimate to those calculated by observational studies is that our estimates are based on families affected by Medicaid expansions. Potentially, out-of-pocket medical costs are much less important to the bankruptcy decision of higher-income families. Dranove and Millenson (2006) argue that most “medical bankruptcies” are filed by low-income families. In that case, our estimates can be interpreted as providing an upper bound on the overall importance of out-of-pocket medical costs on consumer bankruptcy risk for the average family. These results, however, offer an upper-bound based on consistently estimated regressions, rather than qualitative interviews.

In particular, bankruptcy filers are more likely to be drawn from the lower half of the income distribution, meaning that Medicaid-eligible households are not atypical among bankruptcy filers. To confirm that this is the case, we collected data on self-reported household income in the bankruptcy filings of a random sample of recent filers in the Southern District of Ohio. Consistent with the work of Warren (2003), we find strong evidence that bankruptcy filers are much more likely to be drawn from the lower half of the income distribution. In figure 2-5, we present kernel density plots which compare the distribution of household income between all households and households filing for bankruptcy. The figure suggests that households on the margin of Medicaid eligibility have substantially higher bankruptcy risk.

Lastly, we can use the results of this section to estimate the elasticity parameter  $\frac{d \log p_B}{d \log b_H}$ , the elasticity of the probability of filing for bankruptcy with respect to the health insurance benefit rate. We find that Medicaid eligibility reduces the probability of filing for bankruptcy by about 55% ( $-0.55 = (P(B|\neg I) - P(B|I))/P(B|\neg I)$ ). This corresponds to an arc elasticity estimate of  $-0.28$ . As emphasized in section 2.3, this

negative elasticity will tend to raise the optimal level of health insurance benefits beyond what would be determined by focusing solely on the consumption smoothing benefits and moral hazard costs of Medicaid.

## 2.7 Conclusions

This chapter estimates the effect of Medicaid expansions on personal bankruptcies. The results demonstrate a large interaction between these two types of insurance; a ten percentage-point rise in Medicaid eligibility would decrease bankruptcies by almost nine percent. Upon close inspection, these point estimates are large, but not implausible. A ten percentage point increase in Medicaid eligibility is itself an enormous expansion of social insurance. But in the 1990s, bankruptcies increased by roughly five percent each year. Our results suggest that a massive expansion of Medicaid would prevent one year of 1990s-era growth in personal bankruptcies.

Our estimates do not suggest that medical costs are responsible for the massive rise in consumer bankruptcies. From 1994 to 1999, the share of uninsured Americans increased by seven percentage points (Short, 2001). Our regressions would predict a seven percent increase in the number of bankruptcies over this period. In reality, bankruptcies increased by 71 percent. Consequently, our estimates explain roughly ten percent of the overall increase in bankruptcies. As pointed out by Livshits et al. (2007), Canada also experienced an enormous increase in consumer bankruptcies over the 1980s and 1990s. But, during that time period, Canadians enjoyed universal access to health insurance. This suggests that medical costs cannot explain the large growth of consumer bankruptcies. We conclude that medical costs are an important driver of bankruptcies, especially among low-income families, but that medical costs are unlikely to be fundamental in the overall rise in bankruptcies.

Taken as a whole, our results suggest that there may be substantial interaction between the bankruptcy system and other forms of social insurance. Medicaid affects not only its beneficiaries, but also a disperse group of creditors. Medicaid expansions appear to lead to greater transfers from debtors to creditors. As bankruptcies become less common following Medicaid expansions, lenders may charge lower prices to all

other borrowers. The full extent of this pass-through remains an important area for future work.

Table 2.1: Summary Statistics

Year	Consumer Bankruptcies			Simulated Medicaid Eligibility		
	Median	Mean	Standard Deviation	5th percentile	50th percentile	95th percentile
<u>A. All States</u>						
All Years	14,276	22,163	26,699	0.344	0.450	0.666
1993	11,942	16,197	22,411	0.339	0.358	0.539
1996	14,995	20,565	25,757	0.344	0.450	0.578
1999	16,449	25,457	30,056	0.356	0.462	0.740
2002	21,804	29,307	29,007	0.380	0.483	0.738
<u>B. Small Expansion States</u>						
All Years	10,741	19,275	21,505	0.344	0.454	0.463
1993	6,447	12,981	16,670	0.342	0.453	0.457
1996	8,886	17,976	21,824	0.347	0.453	0.458
1999	11,276	22,059	25,203	0.355	0.459	0.461
2002	15,348	27,546	30,221	0.352	0.457	0.462
<u>C. Large Expansion States</u>						
All Years	24,399	51,582	55,966	0.353	0.662	0.792
1993	15,074	44,787	57,406	0.355	0.453	0.633
1996	20,162	51,711	63,092	0.446	0.451	0.662
1999	27,291	58,320	71,360	0.668	0.740	0.792
2002	32,366	54,692	53,502	0.666	0.738	0.791

Notes: The sample consists of bankruptcy counts for the 50 states and DC from 1992-2002; all observations are state-year. For the purposes of this table only, we define "small expansion states" as the five states with the smallest change in simulated eligibility between 1992 and 2002 (South Carolina, Texas, North Carolina, North Dakota, and Idaho). The "large expansion states" are defined similarly (and are California, Tennessee, Massachusetts, Minnesota, and Montana).

Table 2.2: The Effect of Medicaid on Bankruptcy Declarations

Dependent Variable: Regression:	(1)	(2)	(3)	(4)
	Personal Bankruptcies		Business Bankruptcies	
	OLS	IV	OLS	IV
Medicaid Eligibility, $M_{it}$	-0.564 (0.265) [0.038]	-0.870 (0.288) [0.004]	-0.018 (0.467) [0.969]	-0.012 (0.557) [0.983]
R <sup>2</sup>	0.993		0.931	
N	561		561	

Notes: The sample consists of bankruptcy counts for all 50 states and DC from 1992-2002; all observations are state-year. All dependent variables are in logs. All specifications include the state unemployment rate, the log of average earnings in the state as time-varying controls, state fixed effects, and year fixed effects. Standard errors, adjusted to allow for an arbitrary variance-covariance matrix within each state over time, are in parentheses and p-values are in brackets.



Table 2.3: Alternative Specifications Involving Time Trends

	Dependent Variable Consumer Bankruptcies							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Baseline	Drop $X_{i,t}$ s	Reduced Form with Lead and Lag	Region Trends	Region $\times$ Year Fixed Effects	State Trends	Sharp Expansion States	Sharp Expansion States w/ State Trends
Medicaid Eligibility, $M_{i,t}$	-0.870 (0.288) [0.004]	-1.033 (0.351) [0.005]		-0.815 (0.283) [0.006]	-0.824 (0.334) [0.017]	-0.174 (0.299) [0.563]	-0.686 (0.237) [0.007]	-0.612 (0.434) [0.170]
Simulated Medicaid Eligibility			-0.463 (0.229) [0.049]					
Simulated Medicaid Eligibility, 2 year Lead			-0.131 (0.163) [0.424]					
Simulated Medicaid Eligibility, 2 year lag			-0.252 (0.156) [0.113]					
N	561	561	357	561	561	561	297	297

Notes The sample consists of bankruptcy counts for all 50 states and DC from 1992-2002, all observations are state-year. All specifications include state fixed effects and year fixed effects. Dependent variable is always in logs. Standard errors, adjusted to allow for an arbitrary variance-covariance matrix within each state over time, are in parentheses and p-values are in brackets.

Table 2.4: Short-Run versus Long-Run Effects

Dependent Variable: Log Count of Consumer Bankruptcies					
	(1)	(2)	(3)	(4)	(5)
	Baseline	3-Year Averages	1992, 1997, 2002 Panel	Lagged Dependent Variable	IV Lagged Dependent Variable
Medicaid Eligibility, $M_{it}$	-0.870 (0.288) [0.004]	-0.715 (0.285) [0.015]	-0.834 (0.340) [0.018]	-0.099 (0.121) [0.415]	-0.332 (0.216) [0.131]
$\log(c_{i,t-1})$				0.844 (0.040) [0.000]	0.605 (0.097) [0.000]
N	561	204	153	510	408

Notes: The sample consists of bankruptcy counts for all 50 states and DC from 1992-2002; all observations are state-year. All specifications report instrumental variables estimates of variants of equation (2) in text. In all specifications, Medicaid Eligibility is instrumented by a simulated measure of Medicaid Eligibility (see text for details). All specifications include state fixed effects and year fixed effects. All specifications include the state unemployment rate and the log of average earnings in the state as time-varying controls. Standard errors, adjusted to allow for an arbitrary variance-covariance matrix within each state over time, are in parentheses and p-values are in brackets

Table 2.5: Medicaid and Uncompensated Care

	(1)	(2)	(3)	(4)
Dependent Variable:	Consumer Bankruptcies	Bad Debt	Charity Care	Total Un-compensated Care
Medicaid Eligibility, $M_{it}$	-0.901 (0.397) [0.028]	-0.137 (0.217) [0.531]	-0.940 (0.721) [0.199]	-0.515 (0.435) [0.232]
N	306	306	306	306

Notes: In all specifications, Medicaid Eligibility is instrumented by a simulated measure of Medicaid Eligibility (see text for details). All specifications include state fixed effects and year fixed effects. Standard errors, adjusted to allow for an arbitrary variance-covariance matrix within each state over time, are in parentheses and p-values are in brackets.

Figure 2-1: Bankruptcies per Household, 1983–2002

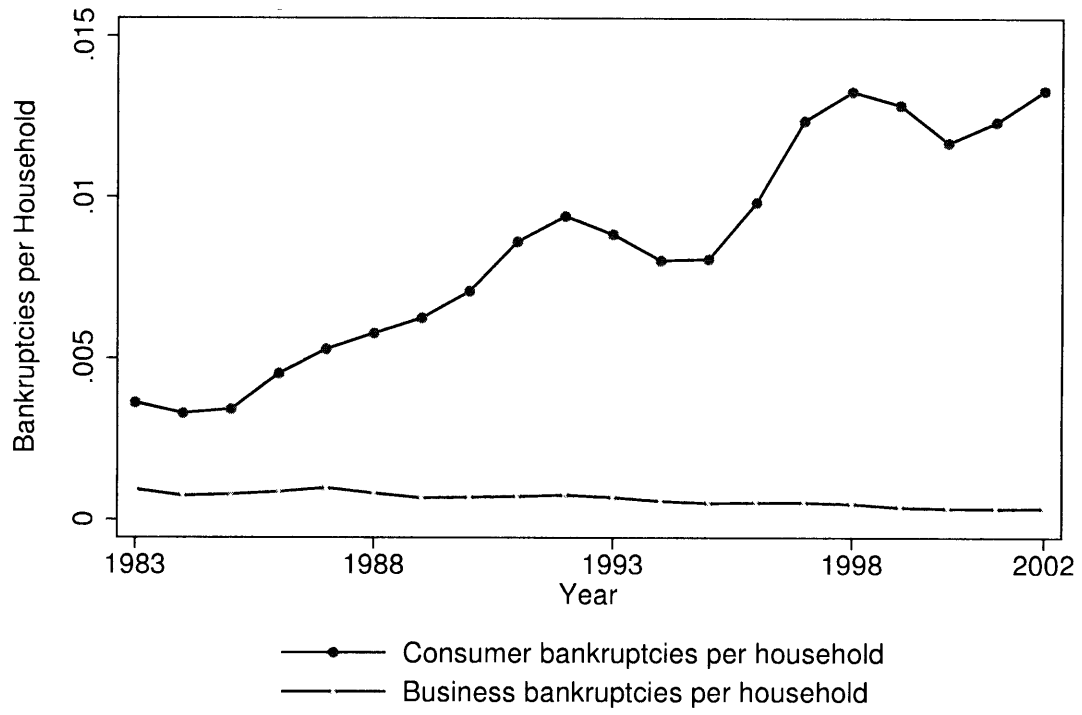


Figure 2-2: Growth in Medicaid Eligibility, 1992-2002

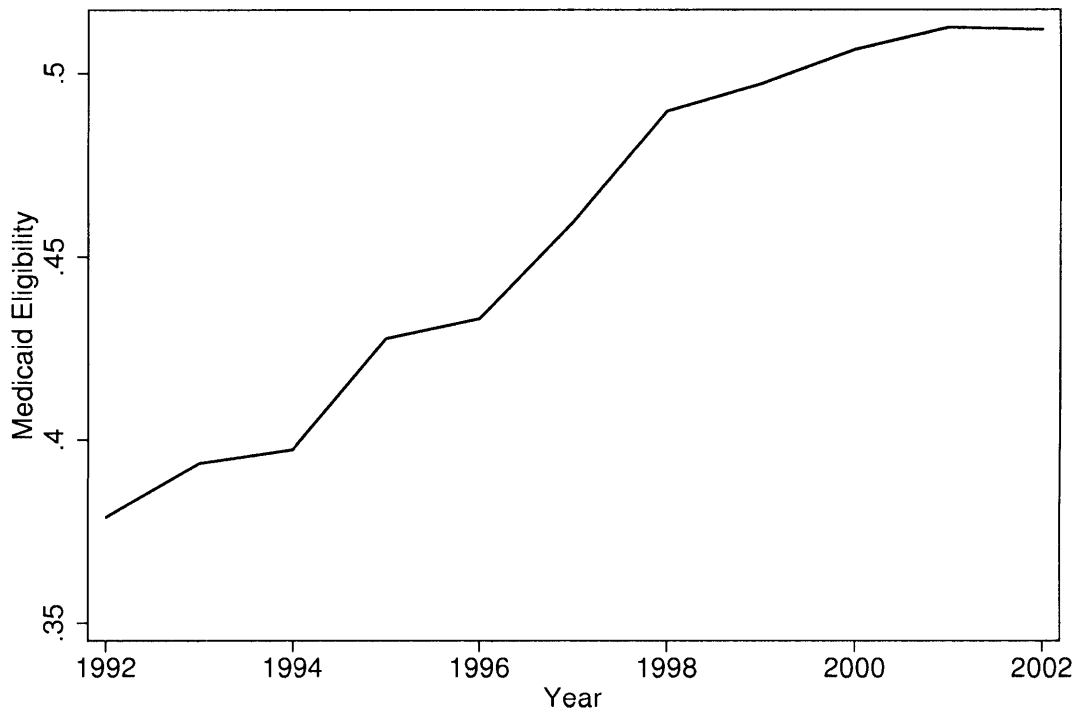


Figure 2-3: Histogram of Out of Pocket Medical Spending For Medicaid Recipients and the Uninsured

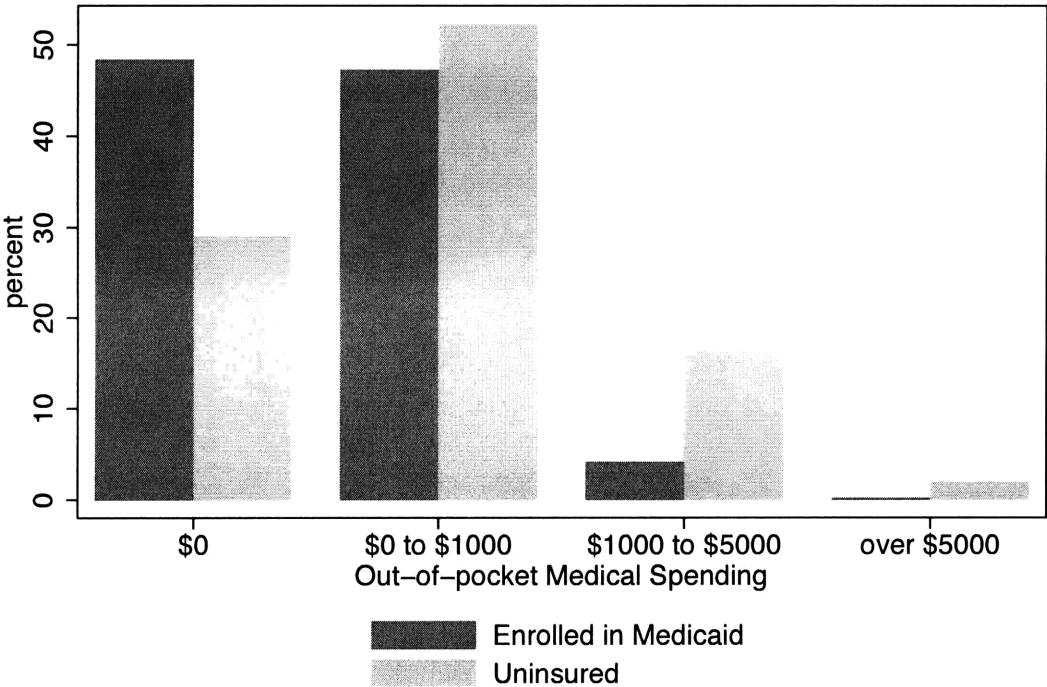


Figure 2-4: Bankruptcies and Medicaid Eligibility, 1992–2002

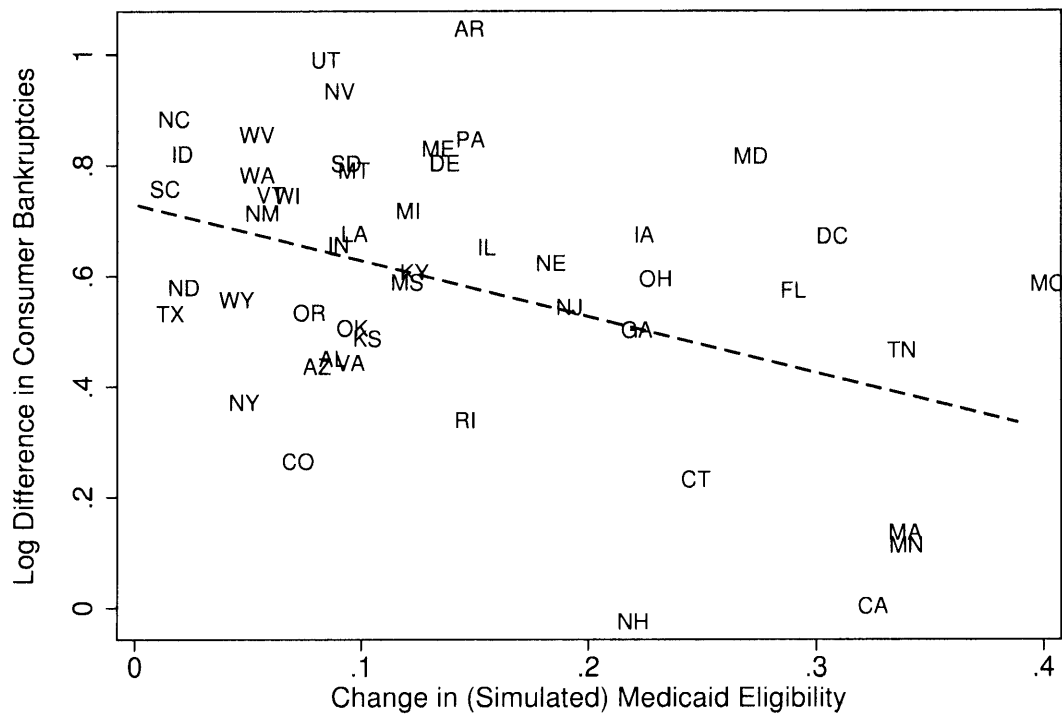
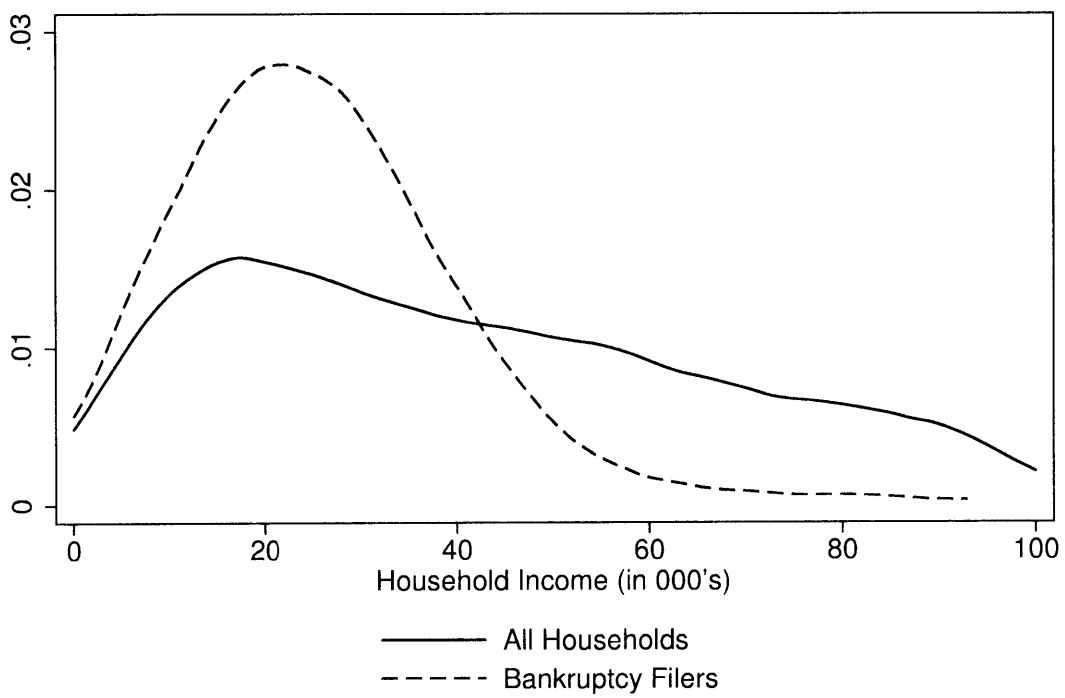


Figure 2-5: Estimated Density of Household Income for Bankruptcy Filers





## Chapter 3

# Using Insurance Before You Lose It: Health Care Consumption at the End of Coverage

### 3.1 Introduction

Insured Americans obtain health insurance through their employer, from a family member's employer, from public sources, or in the individual market. As a result, transitions in insurance coverage are common. Individuals may switch health insurance programs when they move from one employer to another, when they become unemployed, or when they become ineligible for their current health insurance plan. Insurance status, consequently, is not simply a cross-sectional issue but a longitudinal one; many more individuals are uninsured for a brief period of time than are uninsured at any one point in time. Levy (2007), for example, estimates that roughly 30% of young adult males are currently uninsured, but that 55% are uninsured within a two-year interval. Indeed, transitions into uninsured status have become more common, despite large expansions of publicly-provided health insurance (Cutler and Gelber, 2009).

This chapter analyzes one such transition in insurance status. It tests whether

individuals anticipate losing health insurance and stock up on care in the weeks before coverage is lost. When faced with a loss of coverage, a forward-looking consumer recognizes that the price she faces for health care will soon increase. Before coverage ends, she may consume elective care and medication in order to prepare for that increase.

There exists anecdotal evidence that individuals do indeed stock up on care before coverage ends. Health care professionals at one four-year-college have observed a 13% increase in visits to the pharmacy at the end of the academic year, and cite students graduating and becoming uninsured as the prime cause (Twiggs, 2009). Despite such anecdotal evidence, statistical evidence is limited. To my knowledge, Long et al. (1998) present the only statistical analysis of anticipatory consumption for individuals about to become uninsured. The authors examine the health care consumption of respondents to the Survey of Income and Program Participation who later lose insurance coverage. The authors find no evidence that those respondents consume more health care in the months before they lose coverage than respondents who retain coverage. But the authors cannot isolate households who lose coverage exogenously. Moreover, given the limited information on health care consumption in the household survey, the authors cannot measure the path of consumption in the weeks until coverage is lost. They rely instead on self-reported doctor visits and hospitalizations.

Whether individuals anticipate the loss of insurance and stock up on medical care in advance is an important question for at least three reasons. First, forward-looking behavior affects how economists evaluate insurance transitions. The first chapter of this thesis analyzes the transition from insured status to uninsured status at age nineteen, as teenagers lose coverage under their family's plans. Such a transition can be anticipated, and thus to fully understand the effects of the transition, one needs to evaluate whether health care consumption changes *before* the transition to uninsured status. The same may apply to other research on health insurance in which cross-sectional patterns are studied but "anticipatory" consumption is not.

Secondly, such transitions in insurance status may lead to inefficient consumption

of health care. Health is a stock variable with an optimal investment path (Grossman, 1972). Transitions from one insurance plan to another bring about changes in price that are neither based on the cost of production nor designed to prevent over-consumption. Those price changes can induce the agent to deviate from the efficient path of investment. For that reason, health insurance transitions present a potential efficiency loss, solely in their distortion of health care consumption.

Finally, behavior at the end of coverage can demonstrate whether individuals are forward-looking in their demand for medical care. The degree to which individuals are forward-looking is a pivotal assumption in research on the demand for health care. Consider, for instance, research on high-deductible health insurance plans. Such research is complicated by the issue of forward-looking behavior. Upon observing consumption of health care in a high-deductible plan, the analyst does not know which price consumers take as relevant. In theory, forward-looking agents will predict the probability that their annual spending will exceed the deductible, and choose their consumption based on the expected price. A woman who is six months pregnant at the start of the year will realize that expenses associated with childbirth will exceed the deductible. She will then consider health care at the beginning of the year to be free, even though, nominally, she is charged a co-payment. In reality, however, consumers may act myopically, and focus exclusively on the contemporaneous price of medical care.

Keeler and Rolph (1988) grapple with this issue when estimating the price elasticity of demand for medical care from the results of the RAND Health Insurance Experiment. They estimate a structural model and conclude that individuals react to deductibles myopically, responding only to the current price of medical care, rather than the expected, year-end price. They then assume myopic behavior in order to calculate the widely-cited elasticity of demand. This assumption of myopia is criticized by Kowalski (2009), who assumes perfect foresight. To my knowledge, though, no studies have conclusively demonstrated whether myopia is an appropriate assumption, or rather, whether individuals are forward-looking in their demand for care.

This chapter focuses on the loss of coverage in order to test for forward-looking behavior. Several empirical challenges complicate any analysis of individuals losing coverage. Insurance transitions are not assigned at random. Individuals may experience health shocks that end their employment and affect their demand for health care. Moreover, health insurance transitions are often voluntary. Economic theory suggests that agents will switch health care plans whenever faced with new information on the distribution of their potential health care costs.

For that reason, this chapter focuses on teenagers who lose coverage under their family's plans. Insurance contracts cover dependents "eighteen and under," and only cover older dependents who are full-time students. As a result, 5 to 10 percent of American teenagers lose health insurance coverage at age nineteen. This chapter exploits that transition in order to isolate changes in individual behavior as health insurance coverage comes to an end.

An advantage of the focus on nineteen-year-olds is that the population is likely to be transitioning to uninsured status or to less generous insurance coverage. A disadvantage is that teenagers do not represent the typical demand for health care. Teenagers who lose coverage at age nineteen are wealthy enough to have been privately insured, but not to have been enrolled in college by age nineteen. Nevertheless, the econometric framework described below captures a consistent estimate of the change in behavior towards the end of insurance coverage. I discuss how the findings may relate to the general population.

The chapter proceeds as follows. The subsequent section introduces the empirical framework and discusses the sample restrictions required. Section 3.3 reviews the main results on health care consumption. Section 3.4 extends the basic results to consider alternative explanations. Section 3.5 concludes.

## **3.2 Empirical Approach**

An examination of health care consumption towards the end of coverage requires detailed data on consumption for a sample of individuals who lose health insurance

exogenously. This section describes the data I use and the empirical framework designed to test such a hypothesis.

### 3.2.1 Data and Sample Restrictions

The 2003 Medstat MarketScan research database contains insurance claims for the universe of beneficiaries from a sample of organizations. The MarketScan database contains a record for each claim charged to the insurer for three types of expenditures: outpatient, inpatient, and drug expenditures. The date of each claim is recorded along with the monetary cost of the service. The MarketScan database is well-suited for this application. The 2003 database is large, containing the complete expenditure records of over six million individuals. It contains the precise date when individuals lose coverage, and there is likely little measurement error, as the data are critical to each insurance company's billing procedures.

At the same time, the Medstat data present several drawbacks. The data contain limited demographic information.<sup>1</sup> More importantly, the data do not indicate why individuals lose coverage. There exists no variable in the data set that categorizes individuals by whether their loss of insurance is exogenous and anticipated. Instead, I apply the following selection criteria to isolate a sample likely to be losing health insurance in that way.

1. The individual is listed as a dependent, rather than being the primary beneficiary or the spouse of the primary beneficiary.
2. The individual either loses coverage on the last day of a month (the treated sample) or does not lose coverage (the control group).<sup>2</sup>
3. If the individual loses coverage, the primary beneficiary and spouse maintain coverage.

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<sup>1</sup>Exceptions include age, gender, and geographic location.

<sup>2</sup>Of those dependents who lose coverage, more than 99% are listed as losing coverage on the last day of a month. I eliminate from the sample individuals that lose coverage during the month. The analysis below is based on cell means for individuals ending coverage on the same day. Retaining individuals who lose coverage during the month would lead to hundreds of additional cell means based on a small number of individuals for each cell.

4. The individual begins the year at age 18.

These criteria narrow the sample to young adults likely to be losing their family's coverage and becoming uninsured. A drawback to the MarketScan database is that I cannot isolate a sample in which all of the individuals become uninsured. Surely, a portion of the sample loses coverage but gains insurance through another provider. Nevertheless, Collins et al. (2003) estimate that the share of teenagers who are uninsured triples from age eighteen to age nineteen. Few eighteen-year-olds transition from their family's coverage to coverage that is *more* generous.

Table 3.1 offers evidence on how selection criteria 1 through 3 interact with age. The second and third columns display simple counts of observations for dependents that begin the year at the given age. The second column lists counts of dependents who either do not lose coverage or lose coverage with the primary beneficiary. These counts decrease with age; fewer young adults qualify as dependents as they grow older. The third column lists counts of observations who lose coverage during the year, while the primary beneficiaries do not (the treated sample). The number of treated individuals jumps to 17,145 for individuals who begin the year at age eighteen (as compared to only 2,309 at age 17 and 8,007 at age 19). This spike at age 18 suggests that the selection criteria are indeed isolating teenagers affected by the variation described in the first chapter of this thesis. Below, I isolate this sample and track their consumption before coverage ends.<sup>3</sup>

Table 3.2 presents descriptive statistics for those individuals who satisfy selection restrictions 1 through 3 and also begin the year at age 18. The sample consists of 100,882 teenagers, 17,001 who lose coverage during the year. Kowalski (2009) estimates that 40% of individuals consume zero health care in a given year. Table 3.2 demonstrates that this mass point becomes even more pronounced for teenagers. Nearly 30% of the sample consumes no health care at any point during the year. For the sample that loses coverage, 55% consume no health care while still eligible. This

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<sup>3</sup>Also of note, the number of treated individuals drastically increases at age 22. That pattern is likely driven by rates of four-year college attendance. Upon exiting college, young adults often lose eligibility under their parents' coverage, as they are no longer full-time students (Collins et al., 2003).

pronounced mass point at zero presents an empirical challenge. Small changes in the rate of consumption are difficult to isolate when so few individuals consume care. In what follows, I implement several complementary strategies as a result.

The last two columns of table 3.2 present average expenditures and consumption rates for the entire individual-day sample. The 17,001 individuals who lose coverage and 83,881 individuals who do not comprise a “micro” data set of over 33 million individual-day observations. Each individual in the control group is observed for 365 days, and each treated individual is observed for the days of the year until his or her coverage ends. Approximately one percent of the observations consume care on any given day, with an average expenditure of roughly forty dollars.

The dollars spent on care are a much less reliable outcome than the rate of consumption. First, the dollars spent are often not directly chosen by the consumer. The RAND health insurance experiment found that more generous insurance plans lead to more emergency department visits, for example, but that the dollars spent at the emergency department was not affected (Newhouse and Group, 1993). Once patients choose to consume a given treatment, the intensive margin is often not discretionary. Secondly, the dollars spent on care are extremely skewed. Only 366 observations in the treated group consumes an inpatient visit, but the median expenditure is \$59,568 and the mean is \$767,531. Since inpatient care is dramatically more expensive than other types of care, a very small share of individuals (less than one in ten thousand) are responsible for the average expenditures of roughly \$50. For that reason, I focus below on the share of individuals who consume care rather than the dollars spent.

### **3.2.2 Regression Framework**

Given this sample of consumers, I estimate the anticipatory effects of losing insurance coverage. To do so, I compare individuals who will lose coverage in several weeks to observations who lose coverage much later or do not lose coverage at all. Though the sample is selected, the comparison between such sub-groups captures the parameter of interest.

For each group of individuals who lose coverage on month  $e$ , I observe the percent

who consume care on day  $t$ . Denote this value as  $\bar{y}_{te} \in [0, 100]$ . I run simple difference-in-difference regressions of the form:

$$\bar{y}_{te} = \alpha_0 + \beta_1 \cdot I\{e = t + 1\} + \beta_2 \cdot I\{e = t + 2\} + \dots + \alpha_t + \alpha_e + \varepsilon_{te}.$$

In this regression equation, the indicator functions,  $I\{\cdot\}$ , measure how long till a group loses coverage. This regression captures any seasonal movement in medical consumption with the  $\alpha_t$  fixed effects. The  $\alpha_e$  fixed effects capture any fixed differences between groups that lose coverage early in the year versus late in the year. The hypothesis that individuals stock up on health care before coverage ends implies that  $\beta_j > 0$ .

### 3.3 Main Estimates of the Change in Behavior Towards the End of Coverage

Table 3.3 presents estimates of the regression equation above. The first column presents results for all types of health care and the point estimates are plotted in figure 3-1. The results indicate a drop in consumption in the last month of coverage. Individuals are roughly 0.07 percentage points less likely to consume medical care in the three weeks before the week that coverage is lost. In the last month of coverage, patients gradually cut back on care. But more than four weeks before losing coverage, there is no significant change in the probability of consuming care. These point estimates are not statistically significant at conventional levels. But the 95% confidence intervals rule out increases in the probability of consumption above 0.05 percentage points. The regressions provide little evidence of stocking up.

The remaining columns in table 3.3 present these results by type of care. It is difficult to judge the magnitude of these estimates, since so few individuals consume care on any given day. For instance, only 0.01 percent of individuals have an inpatient stay on any given day. Nevertheless, columns 2 and 3 suggest that in the last month



of coverage, the probability of consuming medication or an outpatient visit decrease by 0.05 percentage points, a roughly 10 percent change in the rates of those types of care.<sup>4</sup>

Consider next health care consumption in dollars. Table 3.4 presents estimates of the regression equation above for mean expenditures by category of care. As argued above, expenditures are a far less informative outcome than consumption rates. It is thus unsurprising that the estimates in table 3.4 are no more precisely estimated than those in table 3.3. Nevertheless, the table generally demonstrates a similar pattern. Mean expenditures seem to decrease in the last month of coverage. Expenditures in the penultimate month of coverage are imprecisely estimated and do not form a clear pattern.

### 3.4 Robustness Checks

This section offers several variations on the main specification above. A key drawback to the approach above is that so few individuals in the sample consume any care. Potentially, it is individuals with chronic conditions and a greater need for care who would consume care immediately before losing coverage. One approach to select that sample is to select individuals who consumed care at some point in the past. That selected sample would exhibit a higher rate of consumption, and potentially, lead to more precise estimates of the consumption path before coverage is lost.

To pursue that approach, I select only individuals who consume a positive amount of health care in January of 2003. I assume that health care consumption in January is not a reaction to the (then distant) loss of coverage. To make this assumption more palatable, I restrict the sample to individuals who lose coverage in May through November or who did not lose coverage. Thus, I assume that individuals may change their consumption in the 3–5 months before they lose coverage, but they do not consume differently over six months prior to losing coverage.

When I impose those restrictions on the sample, the average rate of consumption

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<sup>4</sup>One concern is that aggregated linear probability models may not perform well when the mean of the dependent variable is close to zero, as it is here. Conditional fixed effects Poisson models (not shown) also estimate a decrease in consumption at the end of coverage.

increases from 1.2 percent to 3.2 percent. This selected sample contains individuals much more likely to consume health care. Thus if the point estimates in table 3 are driven by the large share of zeroes, then this selected sample ought to make that mass point less prominent.

The second column of table 3.5 presents results for this selected sample. Nearly all point estimates for the last month of consumption are still negative. For instance, in the week before coverage is lost, individuals are half a percentage point less likely to consume medical care (and this point estimate is statistically significant). The magnitude of that decrease in consumption seems to be driven equally between medication and outpatient services. Restricting the sample by previous consumption, in this way, does not change the basic conclusion here.

The remaining columns of table 3.5 explore other, alternative explanations. Most importantly, I test whether the results above are driven by the socio-economic status of the dependents in the sample. A general concern with the results reported in section 3.3 is that I cannot isolate a sample of dependents that become uninsured. I have argued above that few dependents transition onto *more* generous insurance at age eighteen. But the specific nature of the transitions are difficult to ascertain. Potentially, the regressions above isolate a decrease in consumption during the last month of coverage solely because some individuals are indeed transitioning onto more generous coverage, and thus postponing consumption.

In order to evaluate this alternative explanation, I use the limited demographic information contained in the Medstat sample. I stratify dependents by a proxy for their probability of remaining insured. The Medstat sample lists the county of residence for the main beneficiary. I match each county to the share of the county's residents aged 25 and older who have attended at least some college, a statistic reported in the 2000 decennial census. I consider this statistic, "share college," an imprecise proxy for the probability that the given dependent will attend college.

The sample of dependents that lose coverage have a mean county share college of 48%. The control sample has a mean share college of 52%. This differential suggests that the proxy captures the significant socio-economic differences in between the

samples. The variation at age nineteen in insurance coverage is primarily driven by the requirement that the dependent be a full-time student (Collins et al., 2003). Thus it is re-assuring that dependents who actually lose coverage have a higher probability of losing coverage, as predicted by the demographics of their home county.

Based on this proxy, I split the dependents in the sample into those with high and low probability of attending college.<sup>5</sup> Columns 3 and 4 of table 3.5 present results for these two samples. The results do not differ qualitatively between the two groups; both samples exhibit similar decreases in consumption in the last month of coverage, and no significant increase in the penultimate month of coverage. I tentatively conclude that prospects after coverage ends are not responsible for the consumption patterns above.

Finally, the last two columns of table 3.5 present results for males and females separately. Health care consumption often differs by gender (see, for instance, Billings et al. (2000)), but these last two regressions suggest no significant difference by gender in consumption patterns at the end of coverage.

## 3.5 Conclusions

Forward-looking agents ought to increase consumption as the end of coverage approaches. This chapter, however, finds no evidence of such anticipatory consumption. It demonstrates a slight decrease in health care consumption in the last month of coverage, and no significant increase beforehand. The decrease in consumption may be driven by individuals awaiting a transition to more generous coverage. But stratifying the sample by a proxy for the probability that they maintain insured status does not significantly alter the results.

A more likely explanation for the pattern above is that in the last month of coverage, consumers are uncertain over when precisely they become uninsured. Consequently, consumption gradually decreases throughout the last month. Still, consumers could stock up on health care in the penultimate month of coverage, and this chapter

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<sup>5</sup>I categorize individuals as having a low probability of attending college if their primary beneficiary lives in a county in which less than 50% of its adult residents attended some college.

has found no evidence for that behavior.

Clearly, more research is necessary in order to understand the degree to which consumers of health care are forward-looking. Future research may focus on older populations that are more likely to consume care, and potentially more likely to exhibit anticipatory consumption. But the evidence presented above suggests that myopia may indeed be a reasonable assumption.

Table 3.1: Sample Restrictions by Age

Age	Do Not Lose Coverage, N	Loses Coverage, N	Percent Losing Coverage
15	109,426	861	0.8
16	110,275	968	0.9
17	113,019	2,309	2.0
18	93,297	17,145	15.5
19	80,823	8,007	9.0
20	74,326	7,170	8.8
21	62,459	9,258	12.9
22	32,730	26,655	44.9
23	15,126	7,337	32.7
24	7,081	4,840	40.6
25	1,253	948	43.1

Table 3.2: Descriptive Statistics

	Individuals	Percent with No Consumption	Average Expenditures, \$	Rates of Consumption, %
All	100,882	29.3	43.30	1.67
Loses Coverage	17,001	54.9	86.46	1.09
Does Not Lose	83,881	24.1	38.61	1.73
Loses in January	1,045	85.6	44.19	0.82
February	1,007	77.0	23.70	0.83
March	1,769	66.6	59.64	1.09
April	1,127	65.8	5.13	0.95
May	1,179	57.5	25.13	1.06
June	2,142	54.3	21.27	1.17
July	1,443	51.8	76.93	1.05
August	1,550	46.9	21.19	1.05
September	2,641	41.8	117.31	1.24
October	2,036	44.2	14.03	0.99
November	1,062	40.0	387.13	1.10

Table 3.3: Probability of Consumption at End of Coverage

Dependent Variable: Percent of observations consuming care in the given category of health care

	(1)	(2)	(3)	(4)
	Total	Drug	Outpatient	Inpatient
Mean	1.18	0.59	0.72	0.01
Week 0	-0.159 (0.066) 0.04	-0.060 (0.038) 0.14	-0.128 (0.046) 0.02	-0.004 (0.003) 0.19
Week 1	-0.075 (0.054) 0.20	-0.031 (0.031) 0.34	-0.049 (0.041) 0.26	0.000 (0.002) 0.97
Week 2	-0.084 (0.056) 0.17	-0.042 (0.032) 0.21	-0.071 (0.037) 0.08	0.000 (0.003) 0.86
Week 3	-0.060 (0.058) 0.32	-0.011 (0.033) 0.75	-0.055 (0.040) 0.19	-0.002 (0.003) 0.41
Week 4	0.010 (0.043) 0.81	0.011 (0.025) 0.66	0.002 (0.032) 0.95	-0.001 (0.002) 0.60
Week 5	0.010 (0.056) 0.86	0.013 (0.027) 0.65	-0.005 (0.045) 0.92	-0.004 (0.004) 0.25
Week 6	0.060 (0.063) 0.37	0.003 (0.038) 0.94	0.071 (0.044) 0.13	0.004 (0.004) 0.38
Week 7	0.034 (0.053) 0.53	0.008 (0.021) 0.70	0.034 (0.047) 0.49	-0.001 (0.003) 0.70
Week 8	0.018 (0.046) 0.71	-0.008 (0.025) 0.74	0.018 (0.035) 0.62	0.002 (0.002) 0.51
Week 9	-0.026 (0.064) 0.70	-0.011 (0.032) 0.73	-0.018 (0.051) 0.73	-0.005 (0.002) 0.09
R <sup>2</sup>	0.208	0.272	0.142	0.037

Note: The sample includes 2,363 daily cell means for individuals losing coverage on the same day. Standard errors in parentheses are robust to auto-correlation between observations ending coverage on the same day. All regressions include fixed effects for the week and last day of coverage (not shown).

Table 3.4: Consumption in Dollars at End of Coverage

Dependent Variable: Mean expenditures for the given category of health care  
in dollars

	(1)	(2)	(3)	(4)
	Total	Drug	Outpatient	Inpatient
Mean	92.06	0.37	1.81	89.88
Week 0	-151.333 (77.781) 0.08	-0.038 (0.026) 0.17	-0.446 (0.187) 0.04	-150.849 (77.738) 0.08
Week 1	-79.227 (90.865) 0.40	-0.017 (0.031) 0.59	-0.294 (0.158) 0.09	-78.916 (90.821) 0.40
Week 2	-203.802 (157.968) 0.22	-0.037 (0.029) 0.22	-0.132 (0.297) 0.66	-203.633 (157.943) 0.22
Week 3	-293.832 (225.983) 0.22	-0.012 (0.036) 0.74	-0.270 (0.218) 0.24	-293.549 (225.985) 0.22
Week 4	-69.259 (33.974) 0.07	-0.016 (0.021) 0.47	0.407 (0.360) 0.28	-69.650 (33.880) 0.06
Week 5	-138.433 (77.730) 0.10	0.002 (0.025) 0.94	-0.190 (0.343) 0.59	-138.246 (77.679) 0.10
Week 6	-117.689 (83.594) 0.19	-0.020 (0.029) 0.51	0.694 (0.501) 0.19	-118.364 (83.459) 0.18
Week 7	-360.266 (362.916) 0.34	0.025 (0.025) 0.35	0.174 (0.243) 0.49	-360.465 (362.864) 0.34
Week 8	-57.019 (34.147) 0.12	-0.033 (0.044) 0.47	0.076 (0.173) 0.67	-57.062 (34.144) 0.12
Week 9	-137.153 (74.048) 0.09	-0.009 (0.026) 0.74	-0.078 (0.256) 0.77	-137.066 (74.102) 0.09
R <sup>2</sup>	0.032	0.24	0.042	0.032

Note: The sample includes 2,363 daily cell means for individuals losing coverage on the same day. Standard errors in parentheses are robust to autocorrelation between observations ending coverage on the same day. All regressions include fixed effects for the week and last day of coverage (not shown).

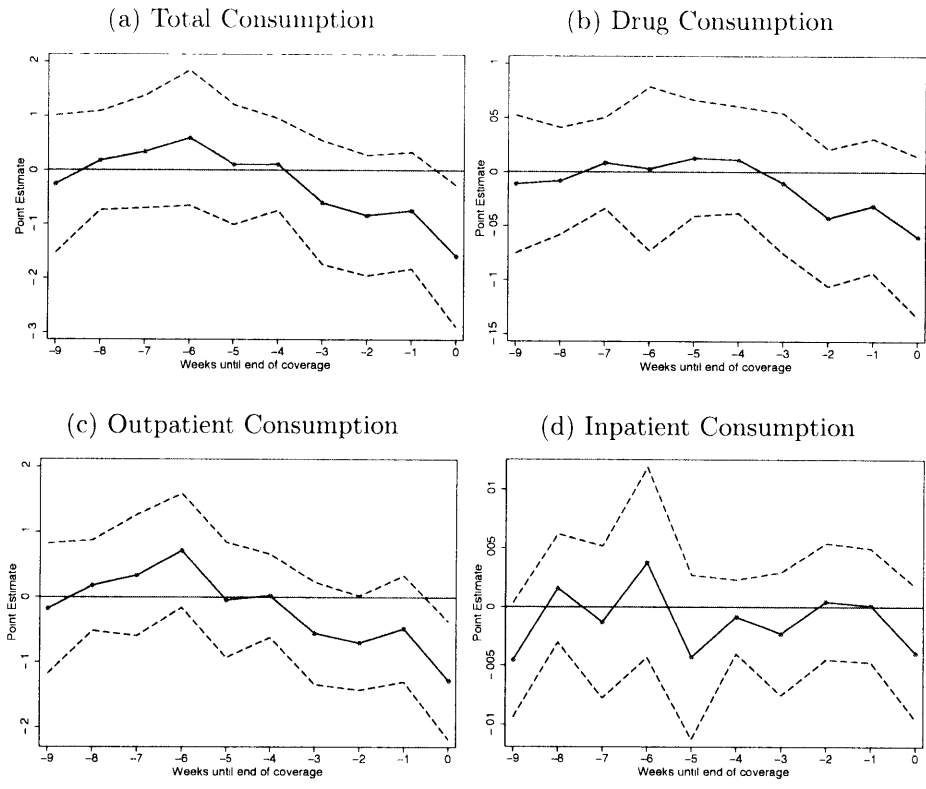


Table 3.5: Robustness Checks

Dependent Variable: Percent of observations consuming any care, for the given sub-population						
	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	Consumed in January	Low College Probability	High College Probability	Males Only	Females Only
Mean	1.18	3.23	1.62	1.70	0.81	1.61
Week 0	-0.159 (0.066) 0.04	-0.410 (0.262) 0.18	-0.170 (0.044) 0.00	-0.157 (0.077) 0.07	-0.114 (0.033) 0.01	-0.207 (0.113) 0.09
Week 1	-0.075 (0.054) 0.20	-0.506 (0.167) 0.03	-0.095 (0.057) 0.12	-0.069 (0.064) 0.31	-0.024 (0.040) 0.55	-0.134 (0.088) 0.15
Week 2	-0.084 (0.056) 0.17	-0.253 (0.174) 0.21	-0.095 (0.046) 0.06	-0.081 (0.062) 0.22	-0.067 (0.029) 0.04	-0.098 (0.104) 0.37
Week 3	-0.060 (0.058) 0.32	-0.522 (0.128) 0.01	-0.070 (0.071) 0.35	-0.060 (0.066) 0.38	-0.057 (0.044) 0.22	-0.060 (0.083) 0.48
Week 4	0.010 (0.043) 0.81	-0.010 (0.119) 0.94	0.010 (0.046) 0.84	0.011 (0.051) 0.84	0.014 (0.040) 0.73	0.015 (0.086) 0.87
Week 5	0.010 (0.056) 0.86	-0.243 (0.156) 0.18	0.033 (0.085) 0.70	0.005 (0.064) 0.94	-0.035 (0.044) 0.44	0.069 (0.089) 0.46
Week 6	0.060 (0.063) 0.37	0.065 (0.205) 0.76	0.155 (0.051) 0.01	0.041 (0.070) 0.57	0.060 (0.048) 0.24	0.064 (0.093) 0.50
Week 7	0.034 (0.053) 0.53	-0.287 (0.165) 0.14	0.031 (0.123) 0.80	0.037 (0.049) 0.47	0.008 (0.035) 0.81	0.074 (0.082) 0.39
Week 8	0.018 (0.046) 0.71	-0.055 (0.129) 0.69	-0.082 (0.058) 0.19	0.034 (0.058) 0.57	0.014 (0.050) 0.78	0.031 (0.083) 0.72
Week 9	-0.026 (0.064) 0.70	0.037 (0.219) 0.87	0.065 (0.110) 0.56	-0.043 (0.067) 0.53	-0.017 (0.059) 0.78	-0.032 (0.087) 0.72
R <sup>2</sup>	0.208	0.302	0.134	0.209	0.21	0.151
N	2,363	1731	2363	2363	2363	2363

Note: The sample includes the given number of daily cell means for individuals losing coverage on the same day. Standard errors in parentheses are robust to auto-correlation between observations ending coverage on the same day. All regressions include fixed effects for the week and last day of coverage (not shown).

Figure 3-1: Point Estimates from Table 3.3



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