The Effect of Health Insurance Coverage on the Use of Medical Services*

Michael Anderson
UC Berkeley
mlanderson@berkeley.edu

Carlos Dobkin
UC Santa Cruz & NBER
cdobkin@ucsc.edu

Tal Gross
Columbia University
tg2370@columbia.edu

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Abstract

Substantial uncertainty exists regarding the causal effect of health insurance on the utilization of care. Most studies cannot determine whether the large differences in healthcare utilization between the insured and the uninsured are due to insurance status or to other unobserved differences between the two groups. In this paper, we exploit a sharp change in insurance coverage rates that results from young adults "aging out" of their parents' insurance plans to estimate the effect of insurance coverage on the utilization of emergency department (ED) and inpatient services. Using a census of emergency department records and hospital discharge records from seven states, we find that aging out results in an abrupt 5 to 8 percentage point reduction in the probability of having health insurance. We find that not having insurance leads to a 40 percent reduction in ED visits and a 61 percent reduction in inpatient hospital admissions. The drop in ED visits and inpatient admissions is due entirely to reductions in the care provided by privately owned hospitals, with particularly large reductions at for profit hospitals. The results indicate that recently enacted health insurance coverage expansions may result in a substantial increase in ED visits and hospital inpatient visits for currently uninsured young adults.

JEL Codes: I11, I18, G22

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1. INTRODUCTION

Over one-quarter of nonelderly adults in the United States lacked health insurance at some point in 2007 (Schoen et al. 2008). A large body of research documents a strong association between insurance status and particular patterns of health care utilization. The uninsured are less likely to consume preventative care such as diagnostic exams and routine checkups (Ayanian et al. 2000). They are more likely to be hospitalized for conditions that – if treated promptly – do not require hospitalization (Weissman et al. 1992). Such correlations suggest that when individuals lose health insurance, they alter their consumption of health care and their health suffers as a result.

But would the uninsured behave differently if they had health insurance? Individuals without health insurance have different discount rates, risk tolerances, and medical risks than those with health insurance, making causal inference difficult. Little evidence exists that overcomes this empirical challenge. Several studies leverage quasi-experimental variation to measure the impacts of Medicare and Medicaid, the two largest public insurance programs in the United States.[1] There are two reasons why these studies provide little insight about the likely effects of coverage expansions on those currently uninsured, however. First, they focus only on the near-elderly or the very young, both of whom are at low risk of being uninsured. Most of the uninsured are non-elderly adults, particularly young adults. Estimates of this population's reaction to changes in health insurance status are essential to evaluate public policies that would expand access to health insurance. Second, studies that focus on Medicare or Medicaid often have difficulty separating the effects of gaining health insurance from the effects of a large-scale substitution from private to public insurance.

In this paper, we overcome these challenges by exploiting quasi-experimental variation in insurance status that results from the rules insurers use to establish the eligibility of dependents. Many private health insurance contracts cover dependents "eighteen and under" and only cover older dependents who are full-time students. As a result, five to eight percent of teenagers become uninsured shortly after their nineteenth birthdays. We exploit this variation through a regression discontinuity (RD) design and compare the health care

[1] See, for instance, papers by Dafny and Gruber (2005), Card et al. (2008, 2009), and Currie et al. (2008).

consumption of teenagers who are just younger than nineteen to the health care consumption of those who are just older than nineteen.

We examine the impact of this sharp change in coverage using emergency department administrative records from Arizona, California, Iowa, New Jersey, and Wisconsin and hospital admission records from Arizona, California, Iowa, New York, Texas and Wisconsin. We estimate sizable reductions in emergency department (ED) visits, contradicting the conventional wisdom that the uninsured are more likely to visit the ED. We also find substantial reductions in non-urgent hospital admissions. Overall, these results suggest that recently enacted health insurance coverage expansions could substantially increase the amount of care that currently uninsured individuals receive and require an increase in net expenditures.

The paper proceeds as follows. The following section describes previous research on insurance and utilization. Section 3 describes the data we use, while Section 4 outlines our econometric framework. Sections 5 and 6 present results for ED visits and inpatient hospitalizations respectively. Section 7 discusses the potential generalizability of our results and their relevance to recently enacted policy. Section 8 concludes.

2. Prior Evidence on the Health Care Consumption 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, an outcome that can be avoided with timely care. Dozens of similar studies are summarized in an Institute of Medicine (2002) report, and nearly all find a robust correlation between a lack of insurance and reliance on expensive, avoidable medical treatments. Some evidence also suggests that the uninsured are more likely to seek care in the ED than the insured (Kwack et al. 2004), and it is commonly assumed that uninsured patients visit the ED for non-urgent problems and contribute to ED crowding (Abelson

2008, Newton et al. 2008).[2] Simulation models, however, suggest that insurance coverage expansions could significantly increase overall medical spending (Hadley and Holahan 2003).

Given the substantial underlying differences between the insured and the uninsured, the correlations documented in these studies may not represent causal effects. To our knowledge, only a small number of studies have used credible research designs to determine the causal effect of insurance status on health care utilization. One group of studies evaluates Medicaid expansions. Dafny and Gruber (2005), for example, estimate that Medicaid expansions led 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, while McWilliams et al. (2003) and Card et al. (2008, 2009) study the effects of Medicare on individual health care consumption. All of these papers conclude that Medicare leads to a substantial increase in health care consumption.

One limitation of such studies is that individuals who gain health insurance through Medicaid and Medicare are often insured beforehand. For example, the study most similar to our own, Card et al. (2008), finds that the number of individuals transitioning from private coverage to Medicare at age 65 is six times larger than the number of individuals gaining health insurance at age 65. Card et al. conclude that much of the increase in hospitalizations that occurs after people become eligible for Medicare is likely due to transitions from private to public insurance rather than due to gaining health insurance. Consequently, they cannot isolate the causal effect of being uninsured on health care consumption, which is the object of interest here.[3]

[2] In spite of the positive cross-sectional correlation between uninsured status and ED utilization, however, Kwack et al. (2004) find no significant effect of the implementation of a managed care program on ED use patterns for formerly uninsured patients.
[3] Interestingly, Levine et al. (2011) examine the impact of the State Children's Health Insurance Program (SCHIP) around the age 19 discontinuity and find no evidence that SCHIP crowds out private health insurance.

The other limitation of studies focused on Medicare and Medicaid is that their estimates are based on the demographic groups at lowest risk of being uninsured. Medicare ensures that only a small fraction of the elderly lack health insurance, and Medicaid, combined with the passage and expansion of the State Children's Health Insurance Program (SCHIP), has reduced uninsurance rates among children to below 10 percent as of 2007 (Levine et al. 2011). Most of the uninsured are now non-elderly adults, and over half of uninsured non-elderly adults are between the ages of 19 and 35 (Kriss et al. 2008). Projecting the effects of new insurance coverage expansions using results from Medicare and Medicaid studies is therefore difficult, as new expansions will disproportionately affect those between ages 19 and 35.

A large literature also exists that studies the relationship between utilization and coinsurance rates. Most prominently, the RAND Health Insurance experiment, conducted in the 1970s, demonstrated that a high-deductible health plan reduced hospital admissions by approximately 20 percent relative to a free plan (Brook et al. 1984; Newhouse 2004). But such results are not directly relevant to the present study, as the Health Insurance experiment focused on the effect of coinsurance rates, not insurance itself. Variations in the coinsurance rate (from 25 to 95 percent) had no effect on hospital admissions, suggesting that the relationship between price and utilization may be highly non-linear.[4]

This study contributes to the literature on health insurance in several respects. First, it isolates the effects of uninsured status, avoiding contamination from simultaneous, large-scale transitions between private and public insurance. Second, it focuses on young adults, a group that is more representative of the uninsured population than either children or the elderly. Third, it identifies effects that correspond to a mixture of private and public insurance plans, rather than identifying effects for public insurance plans only.

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[4] Several other studies examine the relationship between insurance status and utilization using a variety of outcomes and research designs. Meer and Rosen (2004) instrument for health insurance status using self-employment status and find a positive, significant relationship between insurance status and office-based health care provider visits. Doyle (2005) compares auto accident victims with no health insurance to auto accident victims with no car insurance and concludes that health insurance increases hospital length of stay (conditional on being admitted to the hospital).

3. DATA

We rely on the following sources of data: the National Health Interview Survey (NHIS) 1997-2007, administrative records of Emergency Department visits, and administrative records of inpatient hospitalizations. We use the NHIS to test whether potential confounders such as behaviors that might affect health change discontinuously after people turn 19. For this analysis, we calculate each respondent's approximate age in days and restrict the NHIS sample to teenagers who are within one year of their 19th birthday at the time of the survey. This leaves us with a sample of 24,155 young adults for most of the behaviors we examine. However, the questions on alcohol consumption, smoking, and vaccinations are only asked to a subsample of 8,121 young adults.

To estimate the effect of insurance coverage on emergency department visits we use ED visit records from Arizona, California, Iowa, New Jersey, and Wisconsin. The records for Arizona and California span the 2005 to 2007 calendar years, the records for Iowa and New Jersey span the 2004 to 2007 calendar years, and the records for Wisconsin span the 2004 to 2006 calendar years. In each of these states we drop visits that occur in the last month of the sample period as some of the people who seek care in December are included in the following year's file. These datasets constitute a near-census of visits; the only ED visits not observed are those that occur at hospitals regulated by the federal government, such as Veterans Affairs hospitals. We restrict these data sets to visits by patients who are 18 or 19 years old at the time they seek care. For all exercises below, we refer to such samples – in which all individuals are within one year of their 19th birthday – as being composed of "young adults." In total, we observe 1,744,367 ED visits. For each visit, we observe basic demographic information including race, ethnicity, gender, type of health insurance, and age in months. In addition the dataset includes detailed information on the cause of the visit to the ED and the treatment received. Regressions are estimated on month level means for each of the 12 months before and after the 19th birthday rather than the individual level records.

To examine the impact of insurance coverage on hospital admissions, we use a near census of hospital discharges from six states: Arizona, California, Iowa, New York, Texas, and

Wisconsin. The hospital records include discharges occurring in the following time periods and states: 2000–2007 in Arizona, 1990–2006 in California, 1990–2006 in New York, 2004–2007 in Iowa, 1999–2003 in Texas and 2004–2006 in Wisconsin. Discharges from hospitals that are not regulated by the states' departments of health services are not included amongst these records. Between the six states we observe a total of 849,636 hospital stays among 18 and 19 year olds that are not pregnancy related. These records contain the same demographic variables available in the ED data along with detailed information on the cause of admission and treatment received in the hospital. We use the same approach as with the emergency department records of dropping the last month in each state and conducting the analysis using month level means at each month of age rather than the individual level data.

4. EMPIRICAL FRAMEWORK

Consider the following reduced-form model of the effects of health insurance coverage on health care utilization:

$$Y_i = \gamma_0 + \gamma_1 D_i + \varepsilon_i \quad (1)$$

In this model, Y_i represents the utilization of care of individual *i*, and D_i is an indicator variable equal to unity if individual *i* has health insurance. The error term, ε_i, corresponds to all other determinants of the outcome Y_i. The coefficient γ_1 represents the causal effect of health insurance on utilization.

It is difficult to obtain consistent estimates of γ_1 because health insurance status, D_i, is correlated with unobserved determinants of utilization. An individual chooses to acquire health insurance based on characteristics that affect both the choice to be insured and her use of health care services. Some of these characteristics are observable to researchers but many are not; uninsured individuals likely have different discount factors, risk tolerances, and medical risks than those with health insurance. In the first two columns of Table 1 we present summary statistics by health insurance status for young adults from the NHIS. Insured young adults are less likely to be married, less likely to smoke, and more likely to be attending school. These differences are highly significant. Since observable characteristics are correlated with insurance status, it is likely that unobservable characteristics are also correlated with insurance status. Consequently, we rely on an instrumental variables strategy, and identify the causal effect of health insurance via the sharp discontinuity in insurance coverage rates at age 19.

Let $Z_i = 1\{A_i > 19\}$ be an indicator variable equal to unity if individual i is older than 19.[5] When young adults turn 19, they become less likely to be insured. Figure 1 plots the age profile of insurance coverage, as well as ED visits and inpatient hospital stays, from NHIS data. The solid line plots the share uninsured by age. It demonstrates a sharp increase at age 19, one that is larger than the decrease in share uninsured at age 65 due to Medicare.

To identify γ_1, we assume that no other variables in equation (1) change discontinuously at the age 19 threshold. In particular, we assume that $\mathrm{E}[\varepsilon_i \mid A_i = a]$ is continuous at $a = 19$. This assumption would be violated if other factors affecting health care – such as employment, school attendance, or risky behaviors – change discontinuously when young adults turn 19. We discuss this assumption below and present empirical evidence that it holds.

Since age is not the sole determinant of insurance coverage, the RD design that we implement is a "fuzzy" RD (Campbell 1969). We estimate the reduced form effect of age 19 on each outcome of interest Y_i using local linear regressions. Specifically, we limit the sample to a bandwidth of one year around the age 19 threshold and estimate regressions of the form:

(2) $$Y_i = \alpha_0 + \alpha_1 Z_i + \alpha_2 (A_i - 19) + \alpha_3 Z_i (A_i - 19) + v_i$$

We estimate both the first stage – the share of young adults who lose insurance coverage at age 19 – and the reduced form – the change in the number of visits at age 19 – using hospital records. This poses an additional econometric challenge for the first stage, however,

[5] Many private and some public health plans only cover dependents through the last day of the month in which the dependent turns 19 (Kriss et al 2008). In the regressions that follow, we code Z_i accordingly. The abrupt decrease in private coverage documented in Figures 2 and 5 is further evidence that this coding is correct. However, to simplify the discussion we describe people as aging out when they turn 19.

as individuals are only observed when they visit the hospital. In Section 5 we describe how this issue affects our first stage estimates and develop a method for consistently estimating the first stage using hospital records.

We can identify γ_1, the causal effect of health insurance coverage (D_i) on outcome Y_i, by combining the first stage and reduced form results. We identify this parameter by dividing the effect of turning 19 on outcome Y_i by the effect of turning 19 on health insurance coverage, D_i. This strategy is analogous to using the age 19 discontinuity as an instrument to identify the causal effect of health insurance (Hahn et al. 2001).

The primary concern in our research design is that factors other than insurance coverage may change discontinuously at age 19. Because we measure age at the monthly level in our analyses, only factors that change sharply within one to two months of the age 19 threshold will bias our estimates. This fact implies that most obvious confounders should not bias our estimates. In particular, factors related to high school graduation, college start date, or commencement of employment should not bias our estimates. High school graduations, for example, occur in June, but 19th birthdays are distributed throughout the year. Thus the high school graduation rate should not change discontinuously in the month following an individual's 19th birthday.

We confirm that observable characteristics do not change sharply at age 19 in the last two columns of Table 1. These columns report regression coefficients from models of the form presented in equation (2). Each row corresponds to a separate regression, with the reported characteristic as the dependent variable. In these regressions we keep only respondents 18 or 19 years of age. There are slight decreases in employment and marital rates at age 19, and slight increases in school attendance at the same age. However, none of the changes are statistically significant. There are also no significant changes in drinking, flu shot receipt, or smoking at age 19, suggesting that young adults do not make an effort to reduce their exposure to risk when faced with the prospect of losing insurance coverage. Supplementary results (not reported in Table 1) demonstrate no significant changes in frequency of heavy drinking, intensity of smoking, or frequency of exercise. The last row of Table 1 reveals that there is no evidence that people aging out of their parents' insurance replace it with their own insurance.

In a previous version of this paper, Anderson et al. (2010), we document the change in insurance coverage at age 19 using the NHIS. We find a 4 to 8 percentage point increase in the share uninsured immediately at age 19, driven almost entirely by teenagers who are not in school. We omit this analysis here to save space and for the following reasons. First, the NHIS data are self-reported and may lead to biased estimates of the first stage if some 19 year olds do not immediately realize that they have become uninsured. Second, due to the smaller sample size, estimates from the NHIS are much less precise than estimates from the ED and hospital datasets.

5. THE EFFECTS OF HEALTH INSURANCE ON EMERGENCY DEPARTMENT VISITS

Many young adults receive health care at hospital emergency departments. For example, Figure 1 demonstrates that almost 30 percent of 18 and 19 year old NHIS respondents reported receiving treatment in an emergency department in the prior 12 months. Figure 1 also reveals that the probability of an ED visit is somewhat higher among young adults than middle-aged adults, suggesting that young adults may be more representative of the typical ED visitor.

ED utilization is of substantial policy interest for two reasons. First, ED crowding is a serious public health issue (Fatovich 2002; Trzeciak and Rivers 2003; Kellermann 2006). Whether insurance coverage expansions will alleviate or exacerbate ED crowding depends on how insurance coverage affects ED utilization. Second, the ED is an expensive location to receive care. Bamezai et al. (2005) estimate that the marginal cost of a non-trauma ED visit is $300, a number that exceeds the average price, let alone the marginal cost, of a doctor's visit.[6,7] Whether insurance coverage increases or decreases net ED usage thus affects the net cost of insurance coverage expansions.

[6] The average total payment for a doctor visit recorded in the Medical Expenditure Panel Survey is $120.
[7] A minority view, put forward by Williams (1996), posits that the marginal cost of an ED visit is relatively low, and that EDs charge high prices to transfer the costs of uncompensated care onto the insured. Market-based tests suggest, however, that ED visits are indeed more expensive than visits to a private doctor. Health

Figure 2 presents the age profile of insurance status for visitors to the emergency department.[8] Specifically, we plot the proportion with each type of insurance coverage for non-overlapping cells of one month of age and superimpose the fitted values from equation (2). We fit the regressions on the proportions from the 24 age cells. For most of the states in our sample we are only able to compute age in months as the exact date of treatment is not available due to confidentiality concerns. However, this does not result in any attenuation bias as the indicator variable Z_i is measured without error.[9] The figure reveals that the proportion of individuals with private coverage drops steadily with age, while the proportion that is uninsured increases with age.

Figure 2 reveals that there is a discrete reduction in private insurance coverage immediately after teenagers turn 19 and a corresponding increase in the proportion uninsured. The proportion privately insured decreases by 5.0 percentage points, and the proportion insured by Medicaid decreases by 0.8 percentage points as well. The proportion uninsured increases by 5.7 percentage points.[10] A similar change in the proportion uninsured does not appear at other nearby ages.[11]

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maintenance organizations (HMOs) generally enjoy bargaining power over hospitals but still reimburse hospitals hundreds of dollars for each ED visit (Polsky and Nicholson 2004). Additionally, some HMOs own hospitals and therefore absorb the true marginal cost of an ED visit when their customers visit EDs. Were ED visits less costly than doctor visits, one would expect such HMOs to shift their customers into the ED. But these HMOs still provide incentives for patients to use doctor offices rather than EDs. A representative plan for the individual market from HMO Kaiser Permanente, for example, charges a $150 copayment for an ED visit but a $50 copayment for a doctor visit.

[8] The expected payer is reported on the medical records.

[9] In all the datasets we observe both the month of birth and the month in which treatment is received. Since people age out of their parents insurance at the end of the month in which their birthday falls, we can correctly code the instrument Z_i using only these two variables. In addition, the coarse age variable does not substantially bias or reduce the precision of our estimates. This can be seen when estimating the results at different levels of coarseness in the age variable using data from California, the one state for which the records include exact date of birth (see online Appendix Figure 1).

[10] These estimates are robust to choice of bandwidth. This can be seen when plotting how the coefficient estimates change across a wide range of potential bandwidths (see online Appendix Figure 2).

[11] We examine the changes in insurance coverage at ages 19, 20, 21, 22, 23, 24, 25, 26 and 65. As expected, no changes appear at ages 20, 22, 24, 25, or 26. There is a large change at age 65 due to Medicare and a notable change at age 21 due to Medicaid. There is a small change at age 23 due to students aging out of their parents' insurance (see online Appendix Figure 3).

Regression estimates of the change in the proportion uninsured at age 19 understate the true size of this change. This bias stems from sample selection: we only observe insurance status for individuals that visit the ED. We find that losing insurance reduces the likelihood of an ED visit and thus affects the probability of appearing in the sample. This selection mechanism leads to attenuation bias when estimating the change in insurance coverage as newly uninsured individuals are likely to drop out of the sample. The following methodology adjusts our estimates for that bias.

The estimates in Figure 2 come from the sample analog of the following equation:

(3)
$$\pi_1 = \lim_{a \downarrow 19} \mathrm{E}\left[D_i \middle| A_i = a\right] - \lim_{a \uparrow 19} \mathrm{E}\left[D_i \middle| A_i = a\right]$$

where D_i is an insurance coverage indicator and A_i is age. The quantity π_1 represents the discrete change in the proportion insured that occurs at age 19 among people visiting the emergency department. However, π_1 is a biased estimate of the true reduction in insurance coverage because $\lim_{a \downarrow 19} \mathrm{E}\left[D_i \middle| A_i = a\right]$ is estimated from a population that is more likely to be uninsured and thus less likely to visit the ED. The population of ED visitors older than 19 is therefore not comparable to the population of ED visitors younger than 19.[12]

Under standard RD assumptions we can adjust our estimates of the first stage to estimate population-level parameters of interest. Suppose that $D_i(1)$ and $Y_i(1)$ indicate whether an individual is insured and whether they visit the ED, respectively, when they are older than 19. The indicator functions $D_i(0)$ and $Y_i(0)$ are defined similarly for individuals younger than 19. We would like to estimate:

(4)
$$\mathrm{E}\left[D_i(1) \mid Y_i(0) = 1\right] - \mathrm{E}\left[D_i(0) \mid Y_i(0) = 1\right].$$

[12] These issues would not affect our estimates if we had population-level estimates of the first-stage equation. The sample size of the NHIS, however, is far too small to generate a precise estimate of the first-stage effect of age 19 on insurance coverage when restricted to the states for which we have ED visit data.

That is, we wish to measure the change in the probability of being insured at age 19 conditional on visiting the ED before age 19. Instead, what we observe in the data is:

(5) $$\mathrm{E}\left[D_i(1) \mid Y_i(1)=1\right]-\mathrm{E}\left[D_i(0) \mid Y_i(0)=1\right].$$

We observe the share insured, but for two distinct groups: those who visit the ED after they turn 19 and those who visit the ED before they turn 19. These two groups are not directly comparable because, as we document below, insurance coverage affects the probability that a person receives treatment in the ED. We correct for the bias in our first-stage estimates under the assumption that the net change in observed ED visits at age 19 is driven only by individuals who lose insurance coverage. This assumption is implied by the standard IV exclusion restriction.

We adopt the following notation for counts of visits and insured patients: y_0 indicates visits made before age 19, d_0 indicates number of insured patients younger than 19, and y_1 and d_1 are defined similarly for patients older than age 19. The ratios $\frac{d_0}{y_0}$ and $\frac{d_1}{y_1}$ thus represent the fraction of insured ED patients before and after 19 respectively. We show in the online technical appendix that the following bias-corrected estimator converges to the quantity of interest:

(6) $$\frac{d_1}{y_1+(y_0-y_1)}-\frac{d_0}{y_0}=\frac{d_1-d_0}{y_0}\xrightarrow{p}\mathrm{E}\left[D_i(1)-D_i(0) \mid Y_i(0)=1\right].$$

Intuitively, the term (y_0-y_1) "adds back in" the individuals who stop visiting the ED because they lose insurance coverage. We thus consistently estimate the average change in insurance coverage for individuals who visit the ED prior to turning 19. Translating equation (6) into RD quantities yields a bias-corrected first-stage equation of:

(7) $$\frac{\lim_{a\downarrow 19}\mathrm{E}\left[D_i \middle| A_i=a\right]\cdot\lim_{a\downarrow 19}\mathrm{E}\left[Y_i \middle| A_i=a\right]}{\lim_{a\uparrow 19}\mathrm{E}\left[Y_i \middle| A_i=a\right]}-\lim_{a\uparrow 19}\mathrm{E}\left[D_i \middle| A_i=a\right]$$

In practice, these quantities are estimated via local linear regressions in which the dependent variables are observed insurance status or ED visit rates. The samples for these regressions are limited to be either one year less than age 19 (for $a \uparrow 19$) or one year greater than age 19 (for $a \downarrow 19$). We estimate the sample analogs of the elements of this equation along with the corresponding variance-covariance matrix via Seemingly Unrelated Regression.[13] We then estimate the standard errors via the Delta Method.

In Table 2 we present estimates of the change in insurance coverage at age 19, adjusting for the bias described above. As in the figures, the regressions in the tables are estimated using the proportions from the 24 one-month age cells. We estimate a 3.3 percent reduction in admissions at age 19 (see Table 3), and this effect shifts the estimated change in the proportion privately insured from –5.0 percentage points to –6.3 percentage points. It also shifts the estimated change in the proportion uninsured from 5.7 percentage points to 8.1 percentage points. The drop in private insurance coverage is complemented by a 1.7 percentage point reduction in the proportion of people covered by Medicaid. This reduction occurs because individuals in some states can age out of Medicaid at age 19.[14] This result implies that the newly uninsured transition from a mixture of private and public insurance. We discuss how this mixture of insurance impacts the generalizability of our results in Section 7.2. The table also presents estimates for men and women separately and reveals that men and women experience similarly sized reductions in insurance coverage.

Figure 3 presents the age profile of the rate of emergency department visits per 10,000 person years.[15] The figure reveals that the rates are increasing throughout this age range for both men and women. The figure also reveals evidence of a discrete reduction in treatment

[13] The corresponding bias-corrected first stage estimator for the increase in the proportion uninsured at age 19 is $\frac{\lim_{a\downarrow 19} \mathrm{E}[U_i|A_i=a] \cdot \lim_{a\downarrow 19} \mathrm{E}[Y_i|A_i=a]}{\lim_{a\uparrow 19} \mathrm{E}[Y_i|A_i=a]} + \left(1 - \frac{\lim_{a\downarrow 19} \mathrm{E}[Y_i|A_i=a]}{\lim_{a\uparrow 19} \mathrm{E}[Y_i|A_i=a]}\right) - \lim_{a\uparrow 19} \mathrm{E}[U_i|A_i=a]$, where U_i equals one if individual i is uninsured and zero otherwise.

[14] The Medicaid age out is very pronounced in Wisconsin due to the BadgerCare program, which is available to all individuals below 19 regardless of income. The Medicaid age out is notable in Iowa and New Jersey as well (see online Appendix Table 1). In contrast, the estimated magnitudes of the changes in private insurance coverage are similar across the five states included in the sample.

[15] The numerator for the rates comes from a near census of ED visits from Arizona, California, Iowa, New Jersey, and Wisconsin. The denominator is from the annual estimates of the resident populations of states by gender and age which are published annually by the US Census.

at age 19.[16] In the first column of Table 3 we present the regression estimate of the discrete change in the natural log of admissions at age 19 for the entire young adult sample and separately for males, females, and non-pregnant females. The regressions reveal that men and women experience a 3.3 percent decrease in visits. [17] Non-pregnant women experience a slightly higher 3.6 percent decrease in visits.

Figure 4 presents the age profile of emergency department visits by hospital type. The figure shows substantial decreases in the number of people treated in emergency departments in non-profit hospitals and for-profit hospitals but no evidence of any decrease in the number of people treated in public hospitals. This pattern is consistent with the differential incentives that patients face at private and public hospitals. Public hospitals are often designated as "core safety net providers" that provide medical services to the indigent at little or no cost. Loss of insurance therefore has a larger impact on the price of care at private hospitals than at public hospitals. The pattern is also consistent with the likelihood that privately insured individuals avoid public hospitals when possible due to long wait times (Weiner et al. 2006). Most of the newly uninsured are thus leaving private hospitals rather than public hospitals. However, the decision to visit private hospitals may persist over time. Thus, when they do seek service, the newly uninsured may continue to visit private hospitals rather than substituting to public hospitals. This possibility is consistent with the fact that visits to public hospitals do not rise at age 19. The regression estimates corresponding to Figure 4 are in the second through fourth columns of Table 3. The two classes of privately controlled hospitals account for almost the entire reduction in the number of people treated.[18]

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[16] As a falsification test, we run similar specifications for ED visits at age 20 and age 22. We find no evidence of either a break in insurance coverage or a substantial change in admissions at either age (see online Appendix Figure 4). We do not perform similar tests at ages 18 or 21 because they are the age of majority and the age at which people are allowed to start purchasing alcohol.

[17] These estimates are fairly robust to bandwidth choice (see online Appendix Figure 5). In addition the estimates for each of the five states in the sample are not significantly different than the overall estimate of –3.3 (see online Appendix Table 1).

[18] This is not necessarily evidence of a violation of the Federal Emergency Medical Treatment and Active Labor Act, a federal law that mandates that EDs treat all individuals needing emergency treatment, regardless of ability to pay. It may be that that people choose not to go to the emergency department, decline treatment when they are informed that they lack insurance, or present with conditions that are not emergencies.

The reduced-form estimates in Table 3 measure the average change in the probability of visiting the ED at age 19. Using the methods described above and detailed in the online technical appendix, we assume that losing insurance weakly affects individuals' propensity to visit the ED in one direction.[19] Then the reduced-form coefficients estimate the average causal effect of insurance (D_i) for individuals that visit the ED before age 19 and are "compliers" (i.e., lose insurance when turning 19), multiplied by the first-stage estimand:

$$(8)\quad \hat{\alpha}_1 \xrightarrow{p} \mathrm{E}\left[Y_i(D_i=1)-Y_i(D_i=0)\mid\ Y_i(0)=1,\ D_i(1)-D_i(0)=-1\right]\cdot \mathrm{E}\left[D_i(1)-D_i(0)\mid Y_i(0)=1\right].$$

We can thus estimate the impact of insurance coverage on the use of emergency department services by dividing the estimates of the percent change in admissions from Table 3 by the estimates of the percentage point change in insurance coverage rates from Table 2. This ratio estimates the expected reduction in ED utilization for individuals that visit the ED before age 19 and are compliers. These estimates are presented in Table 4. The estimate for the overall young adult sample is –0.404, implying that individuals that lose their insurance coverage reduce their emergency department visits by 40 percent.[20] The reductions for men and women are very similar.

6. THE EFFECTS OF HEALTH INSURANCE ON INPATIENT ADMISSIONS

Inpatient visits to the hospital are less common than ED visits. Among young adults, approximately 6 percent have had an inpatient admission in the past year.[21] Nevertheless, such visits are expensive; approximately 34 percent of total health care spending is driven by

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[19] The additional "monotonicity" assumption that losing insurance weakly affects individuals' propensity to visit the ED in one direction is not guaranteed to hold. It is possible that losing insurance induces some people to stop visiting the ED but induces others to start. Our reduced-form estimates indicate that the former group dominates the latter group, but the latter group may nevertheless exist. Relaxing the additional monotonicity assumption (referred to as "Extended Monotonicity" in the online Technical Appendix), we show that the reduced form estimates a weighted average causal effect for two groups: compliers that visit the ED before age 19 and compliers that visit the ED after age 19 (see the online Technical Appendix). We derive a modified first-stage estimator that converges to the sum of the reduced-form weights. Under reasonable assumptions, we establish a lower bound on the magnitude of the average effect of losing insurance on ED visits for compliers that could potentially visit the ED. This lower bound is 0.341, as compared to the estimate of 0.404 reported in this section (an absolute lower bound, which is downwardly biased, is 0.254). Relaxing the Extended Monotonicity assumption thus does not qualitatively change our conclusions.

[20] The estimates of the elasticity across the five states in the sample range from -0.586 to -0.191 (see online Appendix Table 1).

[21] Authors' calculations from the NHIS.

inpatient admissions.[22] As such, the effect of insurance coverage on inpatient visits is a critical object of interest.

We analyze changes in inpatient visits separately for men, pregnant women, and women who are not pregnant. In our sample of 18–19-year-olds, approximately 9.1 percent of women and 2.4 percent of men have an inpatient hospitalization in any given year. The gender difference is almost entirely due to admissions of pregnant women. Women who are pregnant are generally provided with public insurance through Medicaid and thus have a different insurance-age profile than the other two groups. Since pregnant women experience a substantial transition from private to public coverage and no change in the proportion uninsured at age 19 (see Table 5), we separate female observations by pregnancy status.

Figure 5 presents the age profile of insurance coverage for males and non-pregnant females admitted to a hospital. The figure reveals that the proportion of young adults with private insurance drops with age, while the proportion uninsured or covered by Medicaid increases with age. Overall, the proportion uninsured is far lower than the proportion observed in the ED. The figure also reveals a decline in private coverage at exactly age 19. This decline is matched by an increase in the proportion uninsured or covered by Medicaid at the same age.[23] Note, however, that the increase in the proportion of people covered by Medicaid is due in part to the decrease in the total number of inpatient admissions at age 19.

These estimates of the change in insurance coverage at age 19 are biased by a change in composition similar to the one that affects the ED estimates. The first row of Table 5 presents estimates of the discrete change in insurance coverage that occurs at age 19 for the entire inpatient young adult sample (including pregnant women), corrected for bias in the manner described in the prior section. The estimates reveal that among all admissions, approximately 41 percent of the loss in private coverage is offset by increases in Medicaid coverage, so that the proportion uninsured increases by only 2.7 percentage points. Most of the increase in Medicaid coverage, however, is concentrated among pregnant women. The other rows of Table 5 present estimates with pregnant women removed and by gender,

[22] Authors' calculations from the Medical Expenditure Panel Survey.
[23] As suggested by the linear age profiles in Figure 5, the estimates of the change in insurance coverage are robust to the choice of bandwidth (see online Appendix Figure 6).

separating women into pregnant and non-pregnant. These estimates reveal that pregnant women drive much of the increase in Medicaid coverage. Among men and women that are not pregnant there is a 6.6 percentage point drop in private coverage at age 19, and 87 percent of those that lost private coverage end up uninsured.[24] The small increase in the proportion covered by Medicaid is evidence of a modest transition from private to public insurance that is probably due to people being enrolled in Medicaid at the hospital. However, the magnitude of this transition is small in comparison to both the decline in share with private coverage and the size of the private to public transition in previous studies.[25] Pregnant women experience little change in the proportion uninsured.[26] For them, Medicaid absorbs most of the loss in private insurance coverage. In short, there is no first stage effect for pregnant females.

In Figure 6 we present the age profile of hospital admissions for men and non-pregnant women by the route through which they are admitted to the hospital.[27] The figure reveals only a small decline in admissions through the emergency department after people lose their insurance coverage. Many of these admissions are for medical conditions that are emergent and may be less sensitive to price. It is also likely that many of these admissions are subject to the Federal Emergency Medical Treatment and Active Labor Act. We see more substantial drops in admissions directly to the hospital. These admissions are typically planned admissions and many are elective. In Table 6 we present estimates of the change in the natural log of admissions at age 19, estimated from equation (2). The table reveals that inpatient admissions through the emergency department drop by about 2.0 percent for men

[24] The decrease in private coverage is between 5 and 10 percentage points in five of the six states in the sample (see online Appendix Table 2). The change in insurance coverage is imprecisely estimated for the one anomalous state (Iowa).

[25] One of the primary contributions of this paper is that it isolates the effects of uninsured status, avoiding substantial contamination by transitions from private to public insurance. It is thus instructive to compare these "first-stage" results to the "first-stage" results in Card et al. (2008). Among males, the change in uninsured individuals at age 19 is 8.7 times *larger* than the change in Medicaid-covered individuals. Among non-pregnant females, the change in uninsured individuals at age 19 is 4.1 times *larger* than the change in Medicaid-covered individuals. In Card et al. (2008), the change in uninsured individuals at age 65 is 6.3 times *smaller* than the change in Medicare-covered individuals. Thus the private-to-public "contamination problem" is one to two orders of magnitude smaller in this paper than it is in Card et al. (2008).

[26] Most hospitals try to enroll people that are uninsured when they present at the hospital in Medicaid so that they can recover the cost of treating them. Pregnant women are much more likely to qualify for Medicaid than men or non-pregnant women.

[27] Unlike the age profile of emergency department visits which were presented in rates the age profiles of inpatient hospital stays are presented in counts. This is because there is no precise way to estimate the number of women at a given age that are not pregnant.

and 1.3 percent for non-pregnant women. Inpatient admissions directly to the hospital drop by 6.7 percent for men and 6.0 percent for women.[28] Pregnant women exhibit no statistically significant change in hospital admissions. In the bottom three rows of the table we present the estimates of the change in hospital admissions by ownership type. There is a 1.5 percent decrease in admissions to non-profit hospitals and a 3.9 percent decrease in admissions to for profit-hospitals. There is no evidence, however, of a change in overall admissions to hospitals under public control. As in the ED results, this pattern is consistent with uninsured patients facing differential costs at private and public hospitals.

In Table 7 we present the instrumental variables estimates of the impact of insurance coverage on the probability of an inpatient admission. With the inpatient data, the implicit assumption for calculating the instrumental variables estimates – that losing insurance weakly affects individuals' propensity to visit the hospital in one direction – likely holds.[29] The estimate for men is –0.61 and for non-pregnant women is –0.66, implying that losing insurance coverage reduces the probability of an inpatient admission by 61 percent for men and 66 percent for non-pregnant women.[30] These estimates are even larger than the estimates for emergency department visits and suggest that insurance coverage is an important determinant of whether people will receive inpatient treatment. When we examine the results by route into the hospital, it is clear that the overall drop in admissions is due largely to the decline in admissions directly to the hospital, which are typically elective admissions.

7. DISCUSSION

[28] As suggested by Figure 6, these estimates are robust to bandwidth choice (see online Appendix Figures 7 and 8). The estimates vary somewhat across states. The largest reduction in visits is observed in Texas, which also has the largest reduction in insurance coverage at age 19 (see online Appendix Table 2).

[29] Almost all of the reduction in inpatient admissions comes through scheduled admissions, which suggests that the Extended Monotonicity assumption is unlikely to be violated in the inpatient analysis. The Extended Monotonicity assumption could plausibly be violated in the ED data because a lack of primary care might cause a non-serious condition to develop into an emergent condition, necessitating a visit to the ED. However, most of the reduction in inpatient admissions comes through scheduled admissions, which are unlikely to result from emergent conditions. We thus conclude that there is no substantial violation of the Extended Monotonicity assumption in the inpatient data.

[30] The elasticity estimates vary somewhat across states. However, all state estimates fall within one standard error of the overall estimate of –0.64, and the estimates for the three large states that are precisely estimated fall between -0.74 and -0.57 (see online Appendix Table 2).

The recently enacted Patient Protection and Affordable Care Act (PPACA) mandates that dependents – including dependents that are married, do not live with their parents, or are not dependents for tax purposes – may remain on their parents' insurance plans through age 26. Our results imply that this provision should, over time, increase both the insurance coverage rate and the ED and hospital visit rates of individuals in their early twenties. Modest increases should occur even in states that have already enacted private coverage extension laws because state insurance regulations do not apply to larger companies that self-insure (Levine et al. 2011).

Our results are somewhat less informative regarding provisions that expand coverage via other mechanisms or to other age groups. Three issues affect the generalizability of our estimates. First, the estimates are local average treatment effects based on the response of "compliers," individuals who become uninsured upon turning 19. Second, the estimates represent the effects of losing insurance relative to a specific mixture of private and public insurance. Third, the estimates represent the short-run response to uninsured status rather than the long-run response. We examine each of these issues below.

7.1 LOCAL AVERAGE TREATMENT EFFECTS

For policy purposes, the parameter of interest is the average effect of insurance coverage for the presently uninsured; current policies focus on expanding, rather than withdrawing, health insurance coverage. All regression discontinuity designs estimate treatment effects at a specific threshold. In this case, our estimates apply to individuals close to their 19th birthdays. The estimates are likely to generalize to individuals in their late teens or early twenties, as health care consumption patterns remain relatively stable through this age range.[31] Older individuals may react differently to a loss of health insurance, however, as they are susceptible to different medical conditions and may have greater financial resources than uninsured young adults. Nevertheless, adults under age 30 – the age group to which our estimates likely apply – represent a substantial share of the uninsured population. For

[31] We also estimate the discrete changes in insurance coverage and ED treatment rates that occurs at age 23 when individuals that are still in school age out of their parents' insurance. The change in insurance coverage at age 23 is smaller than the one at age 19, but the difference between the elasticity estimated at age 19 and the one estimated at age 23 is not statistically significant (see online Appendix Table 3).

example, 21 percent of all uninsured non-elderly adults were between the ages of 18 and 24 in 2008 (DeNavas-Walt et al. 2009). Furthermore, among non-elderly adults, 19 to 29 year olds comprise 42 percent of all uninsured ED visits and 25 percent of all uninsured inpatient visits.[32]

In addition to being focused on a particular age group, the regression discontinuity estimates are also specific to individuals who lose coverage because they age out of their parents insurance. These individuals differ from the typical young adult in numerous ways. For example, they are much less likely to attend college. Nevertheless, the estimates recover information that is useful for policy makers because compliers make up a substantial fraction of uninsured young adults. In our data, the fraction of uninsured 19-year-old ED and hospital visitors who are compliers is roughly 25 to 30 percent. These are the individuals that we expect will gain coverage under the new age 26 provision.

Of course, a portion of uninsured 19-year-olds do not lose insurance through the age out mechanism, and our estimates do not apply directly to them. These chronically uninsured individuals are, in the language of Angrist, Imbens, and Rubin (1996), "never-takers." In most health insurance contexts, a central concern is that insurance coverage choice is intimately related to underlying health; the chronically uninsured (never-takers) may therefore be significantly healthier than the recently uninsured (compliers). Such a relationship would diminish the magnitude of the response of never-takers to insurance coverage relative to compliers. In this case, however, it is unlikely that adverse selection causes a significant divergence in the mean health of never-takers and compliers. This is because the compliers' pre-19 insurance coverage is an artifact of their parents' insurance plans rather than a reflection of their own poor health (if it were not, they would be unlikely to drop coverage immediately after turning 19). The typical adverse selection mechanism thus does not apply in this context.

Moreover, we find no evidence that never-takers are significantly less healthy or consume less health care services than uninsured compliers. To test for any differences, we use the

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[32] Authors' calculations from 2005 National Hospital Ambulatory Medical Care Survey and 2007 National Hospital Discharge Survey.

Medical Expenditure Panel Survey (MEPS), a two-year panel survey of health care consumption. We isolate respondents who enter the survey at age 18 with insurance and then lose insurance during the second year of the survey. Such respondents are likely to be "compliers." We compare these respondents to respondents likely to be never-takers.[33] We find no significant differences in either self-reported health or total expenditures in the second year of the survey (when both compliers and never-takers are uninsured).[34] These results suggest that the impacts of insurance on never-takers may not be dramatically different than the impacts on compliers.

7.2 Private versus Public Insurance Effects

Most individuals who lose insurance at age 19 come off of private plans. However, Medicaid eligibility rules also change at age 19, generating an age out effect for Medicaid in some states. Table 2 reveals that, among ED visitors losing insurance at age 19, 78 percent lose private coverage while 20 percent lose Medicaid coverage. We thus interpret our estimates as representing a weighted average of the effects of private and public insurance, with a higher weight on private insurance. This interpretation affects the generalizability of our results only if private and public insurance plans have substantially different impacts on utilization.[35] Nevertheless, we believe that our estimates are relevant to proposed coverage expansions for two reasons.

First, our results constitute, to the best of our knowledge, the first quasi-experimental estimates that primarily identify the effect of private insurance. Previous quasi-experimental

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[33] Such respondents are 18 years old at the end of the first survey year, and uninsured during both the first and second years of the survey.

[34] Specifically, we find 318 respondents who are "likely compliers" and compare them to 1,070 respondents who are consistently uninsured (never-takers). In a comparison of means, likely compliers are 5.1 percentage points less likely to report being in good health. This difference is statistically insignificant (*t*-statistic of 1.06) and small relative to the proportion of consistently uninsured 18–20 year olds that report being in good health (48.0 percent). Likely compliers also consume 43.61 dollars per year more in health care once uninsured. This difference is again statistically insignificant (*t*-statistic of 0.24) and small relative to the mean health care consumption of consistently uninsured 18–20 year olds (681.46 dollars per year).

[35] State specific estimates suggest that the relative mixture of private and public plans does not have very large impacts on the estimated elasticity. Texas has an 11 percentage point drop in insurance coverage at 19, half of which is due to people aging out of Medicaid and half of which is due to people aging out of private insurance. Texas's estimated elasticity is -0.57, which is close to the elasticities of California (-0.72) and New York (-0.59), neither of which have people aging out of Medicaid at 19 (see online Appendix Table 2).

papers have focused solely on public insurance.[36] Our estimates therefore represent a substantial contribution to the existing literature even with the mixture of private and public coverage.

Second, most coverage expansions will likely involve a mixture of private and public insurance coverage. PPACA, for example, expands coverage to young adults through both private and public channels. The requirement that private plans cover dependents through age 26 should increase private insurance coverage, while expanded Medicaid eligibility rules should increase public insurance coverage. Nevertheless, the precise mix of private and public coverage may not match the mix in our sample. The Congressional Budget Office, for instance, projects that newly insured individuals will transition to a 50/50 mixture of private and public insurance under PPACA, while our estimates pertain to a higher share of private insurance and a lower share of public insurance (Elmendorf 2010).

7.3 SHORT RUN AND LONG RUN EFFECTS

Our results represent the short-run response to a change in health insurance coverage. The short-run response, however, may differ from the long-run response for three reasons. First, individuals may shift the timing of health care visits across the age 19 threshold. Second, individuals may be able to postpone consumption in the short run but not in the long run. Third, a reduction in preventative care visits may have no impact in the short run but could increase demand for health care in the long run.

The short time horizon in our study may allow individuals to shift the timing of health care visits from the uninsured period to the insured period. When losing insurance, individuals may "stockpile" health care shortly before coverage expires. When gaining insurance, individuals may postpone health care until shortly after coverage begins. In either case, the regression discontinuity we document would be confounded by such behavior. The estimates would reflect the inter-temporal substitution response to a sharp, anticipated change in health care prices and would overstate the net change in health care consumption.

[36] The most relevant study focusing primarily on private insurance is Doyle (2005). However, Doyle focuses on the intensive margin of care (i.e., utilization of care conditional on visiting the hospital) for a specific set of patients (auto accident victims). Our results focus on the extensive margin of care for a broader set of patients.

However, there is little evidence that individuals shift the timing of health care visits in anticipation of gaining or losing insurance coverage. Gross (2010) analyzes the health care consumption of teenagers in private insurance claims data. Teenagers who age out during the sample period are compared to teenagers who never age out or who age out later in the year. The paper finds no evidence that teenagers who lose coverage at age 19 increase their hospital visits or consume more prescription medications in the weeks before they lose coverage. Card et al. (2008) find no evidence that individuals nearing age 65 postpone inpatient care in significant numbers until they qualify for Medicare, and Long et al. (1998) find little evidence of health care stockpiling for the general population. Additionally, the age profiles of ED visits and hospital stays in this paper do not exhibit an increase in consumption in the months immediately before people turn 19. It is also worth noting that the conditions for which most young adults are seeking treatment do not lend themselves to stockpiling.[37]

A similar estimation problem may arise if individuals postpone care in the hopes of regaining coverage. If newly uninsured 19-year-olds expect to regain insurance coverage within the next six months, for example, they may postpone care until that point. The empirical evidence, however, suggests that this dynamic is not present. The age profiles in ED and inpatient care utilization (Figures 3, 4, and 6) show no evidence of postponement. If individuals were postponing care immediately after losing coverage, then we would expect the slope of the age profile to become steeper after age 19. Instead, in every case the slope of the age profile becomes less steep after age 19.

Finally, the RD approach isolates individuals who are insured one day and uninsured the next. As a result, it provides estimates of the effect of health insurance on demand for services independent from the effect of insurance on health itself. In the long run, though, a lack of preventive care – such as cardiovascular disease management or cancer screening – may lead to worse health, increasing demand for medical services. This mechanism could affect approximately half of all uninsured individuals – among the currently uninsured, 55

[37] Young adults seen in the Emergency Department are most commonly seeking treatment for sprains, contusions, lacerations, or infections. For inpatient admissions in this age group, hospital stays for elective procedures that are potentially storable are very rare.

percent have been uninsured for three years or more.[38] For those individuals, our estimates could overstate the long-run increase in care that would ensue from an expansion of health insurance coverage. This effect is unlikely to be large among young adults, however, as they receive minimal preventative care. Nevertheless, it could be important for older age groups to whom our estimates are not directly applicable.

8. Conclusion

We leverage a sharp discontinuity in health insurance coverage that occurs when dependents age out of their parents' insurance plans at age 19. By exploiting this discontinuity, we estimate the effects of health insurance coverage on utilization of care. We find that losing health insurance coverage reduces utilization of both emergency department care and inpatient care. The estimated responses are large – a 10 percentage point decrease in the insurance coverage rate among ED patients reduces ED visits by 4.0 percent, and a 10 percentage point decrease in the insurance coverage rate among hospital patients reduces hospital visits by 6.1 percent. The reduction in hospital visits is stronger for non-urgent admissions, and is concentrated among for-profit and non-profit hospitals, as opposed to public hospitals.

The net effect of losing health insurance on utilization of care is unambiguously negative for our study population. The results clarify several uncertainties about the impacts of insurance coverage on utilization of care. First, losing insurance coverage results in a net decrease in emergency department care. This suggests that newly uninsured patients do not substitute emergency department care for primary care (or, if they do substitute care towards the emergency department, the substituted care is swamped by a reduction in their normal emergency department visits). Second, any increase in uncompensated charity care is insufficient to offset the decrease in paid care, as total ED and inpatient care both fall. Finally, losing insurance does increase the proportion of care that individuals receive at public hospitals. However, this increase is solely due to a decrease in care received at for-

[38] These calculations are based on the NHIS. Note that the proportion of uninsured spells that are short-term is even larger than the proportion of currently uninsured individuals who will be short-term uninsured. Cutler and Gelber (2009) find, for example, that from 2001 to 2004, 76 percent of uninsured spells lasted less than two years among 18 to 61 year olds.

profit and non-profit hospitals. The total amount of care at public hospitals does not increase.

Our results apply specifically to young adults that lose insurance coverage by aging out of their parents' insurance plans. Nevertheless, evidence suggests that the coefficients may generalize to the greater population of uninsured young adults, and over half of uninsured non-elderly adults are under the age of 35. Recently enacted coverage expansions should thus increase the amount of ED and hospital care received by a large group of currently uninsured individuals and require a substantial increase in expenditures.

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NOT FOR PUBLICATION

FOLLOWING APPENDIX WILL BE POSTED ONLINE

9. TECHNICAL APPENDIX: ECONOMETRIC THEORY

In this appendix, we present the derivation of the empirical methods that we rely on above. First, recall that our reduced-form regressions involve the logarithm of counts of hospital visits on the left-hand side, and a first-order polynomial of age on the right-hand side (with the sample restricted to individuals within one year of their 19th birthdays). This specification recovers the population-level change in the probability of visiting the hospital at age 19. In particular, note that the structural relationship of interest is:

$$\text{(A1)} \qquad \log\left(\frac{\sum_i y_{ia}}{N_a}\right) = \alpha_0 + \alpha_1 1\{a > 19\} + f(a) + \varepsilon_a .$$

The left-hand side of equation (A1) represents the probability of having a hospital visit for age group *a* (that is, the total number of visits divided by the total number of individuals in the population of age *a*). Since we rely on administrative records, we do not observe the size of each age group in the underlying population. Instead, we assume that the underlying population at risk for a hospital visit evolves smoothly with age. Under this assumption, we can subtract $\log(N_a)$ from each side of the equation and allow the polynomial $f(a)$ to "absorb" changes in the size of the underlying population. In this way, our primary reduced-form estimating equation involves only simple counts of hospital visits but still captures the change in the *unconditional* probability of a hospital visit at age 19.

The description above justifies our reduced-form approach. But, as described in the main text, a remaining challenge is to consistently estimate both the first stage and the instrumental variables relationships using hospital administrative data. To do so, we rely on a bias correction in the first stage and an additional monotonicity assumption when interpreting the instrumental variables relationship. We demonstrate that, when applied to the ED data, the bias-corrected instrumental variables estimator converges to the average effect of insurance for individuals that lose their insurance coverage at age 19 and visit the ED shortly before age 19.

9.1 NOTATION AND ASSUMPTIONS

Define the instrument Z_i such that $Z_i = 1$ if individual *i* is encouraged to be uninsured (i.e., is older than the age cutoff threshold) and $Z_i = 0$ if individual *i* is encouraged to be insured (i.e., is younger than the age cutoff threshold). Define the insurance indicator D_i such that $D_i = 1$ if individual *i* is insured and $D_i = 0$ if individual *i* is uninsured. Define the outcome Y_i such that $Y_i = 1$ if individual *i* visits the ED and $Y_i = 0$ if individual *i* does not visit the ED.[39] Using the potential outcomes notation from Angrist, Imbens, and Rubin (1996), define $D_i(Z_i)$ such that $D_i(1)$ represents the insurance status of individual *i* when encouraged to be uninsured and $D_i(0)$ represents the insurance status of individual *i* when encouraged to be insured. Note that the relationship between D_i and Z_i is *negative*. Define potential outcomes $Y_i(Z_i)$ such that $Y_i(1)$ represents the ED visit indicator for individual *i* when encouraged to be uninsured and $Y_i(0)$ represents the ED visit indicator for individual *i* when encouraged to be insured. To represent potential outcomes under different insurance regimes, $Y_i(D_i)$, let $Y_i(D_i = 1)$ represent the ED visit indicator for individual *i* when insured and $Y_i(D_i = 0)$ represent the ED visit indicator for individual *i* when uninsured. Finally, define y_0 to be the total number of ED visits pre-19 (i.e., for individuals with $Z_i = 0$), y_1 to be the total number of ED visits post-19 (i.e., for individuals with $Z_i = 1$), d_0 to be the total number of insured ED visits pre-19 (i.e., for individuals with $Z_i = 0$), and d_1 to be the total number of insured ED visits post-19 (i.e., for individuals with $Z_i = 1$). Let N be the total population of individuals (both those that visit the ED and those that do not visit the ED).

We impose the standard LATE monotonicity assumption:

LATE Monotonicity: If $D_i(1) = 1$, then $D_i(0) = 1$.

[39] For the following derivations, we assume that the RD bandwidth is small enough that the probability of any individual visiting the ED twice is effectively zero.

In other words, if individual *i* is insured when encouraged to be uninsured, then individual *i* would also be insured when encouraged to be insured. We define the four potential types of individuals under the LATE Monotonicity assumption as:

LATE Always-takers (LAT): $D_i(0)=1$ and $D_i(1)=1$
LATE Never-takers (LNT): $D_i(0)=0$ and $D_i(1)=0$
LATE Compliers (LC): $D_i(0)=1$ and $D_i(1)=0$
LATE Defiers: $D_i(0)=0$ and $D_i(1)=1$ (ruled out by LATE Monotonicity)

We also impose an Extended Monotonicity assumption that we later relax:

Extended Monotonicity: If $Y_i(1)=1$, then $Y_i(0)=1$.

In other words, if individual *i* visits the ED when encouraged to be uninsured, then individual *i* would also visit the ED when encouraged to be insured. Given the LATE Monotonicity assumption, this assumption is equivalent to assuming that if individual *i* visits the ED when uninsured, then individual *i* would also visit the ED when insured. We define the four potential types of individuals under the Extended Monotonicity assumption as:

Extended Always-takers (EAT): $Y_i(0)=1$ and $Y_i(1)=1$
Extended Never-takers (ENT): $Y_i(0)=0$ and $Y_i(1)=0$
Extended Compliers (EC): $Y_i(0)=1$ and $Y_i(1)=0$
Extended Defiers (EDF): $Y_i(0)=0$ and $Y_i(1)=1$ (ruled out by Extended Monotonicity)

9.1.1 BIAS-CORRECTED FIRST STAGE

We first derive the bias-corrected first stage. Ideally we would estimate $\mathrm{E}[D_i(1)-D_i(0)]$, or the unconditional change in the probability of insurance coverage. However, it is impossible to estimate this quantity using ED data alone, since individuals only appear in these data if they visit the ED. We instead estimate $\mathrm{E}[D_i(1)-D_i(0) \mid Y_i(0)=1]$, or the change in the probability of insurance coverage for individuals that visit the ED when encouraged to be insured (i.e., pre-19). Under the LATE Monotonicity assumption, $\mathrm{E}\left[D_i(1) - D_i(0) \mid Y_i(0) = 1\right] = -\mathrm{P}(D_i(1) = 0 \cap D_i(0) = 1 \mid Y_i(0) = 1)$; the decrease in the probability of insurance coverage is equal to the proportion of LATE compliers. To estimate $\mathrm{E}\left[D_i(1) - D_i(0) \mid Y_i(0) = 1\right]$, we implement the bias-corrected first stage:

(A2)
$$\frac{d_1}{y_1 + (y_0 - y_1)} - \frac{d_0}{y_0} = \frac{d_1 - d_0}{y_0}$$

We show that this estimator converges to $\mathrm{E}\left[D_i(1) - D_i(0) \mid Y_i(0) = 1\right]$.

$$\operatorname{plim}\left(\frac{d_1 - d_0}{y_0}\right) = \operatorname{plim}\left(\frac{d_1/N - d_0/N}{y_0/N}\right)$$
$$= \frac{\mathrm{P}(D_i(1) = 1 \cap Y_i(1) = 1) - \mathrm{P}(D_i(0) = 1 \cap Y_i(0) = 1)}{\mathrm{P}(Y_i(0) = 1)}$$

By Law of Total Probability and LATE Monotonicity:

(A3)
$$= \frac{\mathrm{P}(D_i(1) = 1 \cap Y_i(1) = 1 \cap i \text{ is LAT}) + \mathrm{P}(D_i(1) = 1 \cap Y_i(1) = 1 \cap i \text{ is LNT}) + \mathrm{P}(D_i(1) = 1 \cap Y_i(1) = 1 \cap i \text{ is LC})}{\mathrm{P}(Y_i(0) = 1)}$$
$$- \frac{\mathrm{P}(D_i(0) = 1 \cap Y_i(0) = 1 \cap i \text{ is LAT}) + \mathrm{P}(D_i(0) = 1 \cap Y_i(0) = 1 \cap i \text{ is LNT}) + \mathrm{P}(D_i(0) = 1 \cap Y_i(0) = 1 \cap i \text{ is LC})}{\mathrm{P}(Y_i(0) = 1)}$$

By the IV exclusion restriction and the definitions of LATE always-takers, LATE never-takers, and LATE compliers:

i is LAT implies: $D_i(1) = D_i(0) = 1$ and $Y_i(1) = Y_i(0)$
i is LNT implies: $D_i(1) = D_i(0) = 0$ and $Y_i(1) = Y_i(0)$
i is LC implies: $D_i(1) = 0$ and $D_i(0) = 1$

Thus equation (A3) equals:

$$= \frac{P(Y_i(1)=1 \cap i \text{ is LAT})}{P(Y_i(0)=1)} - \frac{P(Y_i(0)=1 \cap i \text{ is LAT}) + P(Y_i(0)=1 \cap i \text{ is LC})}{P(Y_i(0)=1)}$$

$$\text{(A4)} \qquad = -\frac{P(Y_i(0)=1 \cap i \text{ is LC})}{P(Y_i(0)=1)}$$

$$= -P(i \text{ is LC} \mid Y_i(0)=1)$$

$$= E\left[D_i(1) - D_i(0) \mid Y_i(0)=1\right]$$

The bias-corrected first stage therefore estimates the probability that an individual is a LATE complier conditional on that individual visiting the ED when encouraged to be insured (i.e., pre-19). Equivalently, it represents a weighted average effect of the age 19 threshold on insurance coverage rates, where the weight for individual *i* is proportional to that individual's probability of visiting the ED just before turning 19. Note that the Extended Monotonicity assumption is not necessary to derive the bias-corrected first stage estimand.

9.1.2 REDUCED FORM

We estimate the percentage decline in visits induced by the instrument (i.e., crossing the "age out" threshold). The reduced form is:

$$\text{(A5)} \qquad \frac{y_1 - y_0}{y_0}$$

We show that this estimator converges to

$E\left[Y_i(D_i=1) - Y_i(D_i=0) \mid i \text{ is LC}, Y_i(0)=1\right] \cdot -P(i \text{ is LC} \mid Y_i(0)=1)$.

$$\text{plim}\left(\frac{y_1 - y_0}{y_0}\right) = \text{plim}\left(\frac{y_1/N - y_0/N}{y_0/N}\right)$$

$$= \frac{P(Y_i(1)=1) - P(Y_i(0)=1)}{P(Y_i(0)=1)}$$

By Law of Total Probability:

$$= \frac{\mathrm{P}(Y_i(1)=1 \cap Y_i(0)=0) + \mathrm{P}(Y_i(1)=1 \cap Y_i(0)=1)}{\mathrm{P}(Y_i(0)=1)} - \frac{\mathrm{P}(Y_i(0)=1 \cap Y_i(1)=0) + \mathrm{P}(Y_i(0)=1 \cap Y_i(1)=1)}{\mathrm{P}(Y_i(0)=1)}$$

By LATE Monotonicity (which implies that $Y_i(1) \neq Y_i(0)$ only for LATE compliers):

$$= \frac{\mathrm{P}(Y_i(1)=1 \cap Y_i(0)=0 \cap i \text{ is LC}) - \mathrm{P}(Y_i(0)=1 \cap Y_i(1)=0 \cap i \text{ is LC})}{\mathrm{P}(Y_i(0)=1)}$$

By Bayes' Thoerem:

$$= \frac{\mathrm{P}(Y_i(0)=0 \mid i \text{ is LC} \cap Y_i(1)=1) \cdot \mathrm{P}(i \text{ is LC} \cap Y_i(1)=1) - \mathrm{P}(Y_i(1)=0 \mid i \text{ is LC} \cap Y_i(0)=1) \cdot \mathrm{P}(i \text{ is LC} \cap Y_i(0)=1)}{\mathrm{P}(Y_i(0)=1)}$$

$$= \frac{\mathrm{E}\left[1-Y_i(0) \mid i \text{ is LC},\ Y_i(1)=1\right] \cdot \mathrm{P}(i \text{ is LC} \cap Y_i(1)=1) - \mathrm{E}\left[1-Y_i(1) \mid i \text{ is LC},\ Y_i(0)=1\right] \cdot \mathrm{P}(i \text{ is LC} \cap Y_i(0)=1)}{\mathrm{P}(Y_i(0)=1)}$$

$$= \frac{\mathrm{E}\left[Y_i(1)-Y_i(0) \mid i \text{ is LC},\ Y_i(1)=1\right] \cdot \mathrm{P}(i \text{ is LC} \cap Y_i(1)=1) - \mathrm{E}\left[Y_i(0)-Y_i(1) \mid i \text{ is LC},\ Y_i(0)=1\right] \cdot \mathrm{P}(i \text{ is LC} \cap Y_i(0)=1)}{\mathrm{P}(Y_i(0)=1)}$$

(A6)

$$= \mathrm{E}\left[Y_i(1)-Y_i(0) \mid i \text{ is LC},\ Y_i(1)=1\right] \cdot \frac{\mathrm{P}(i \text{ is LC} \cap Y_i(1)=1)}{\mathrm{P}(Y_i(0)=1)} - \mathrm{E}\left[Y_i(0)-Y_i(1) \mid i \text{ is LC},\ Y_i(0)=1\right] \cdot \frac{\mathrm{P}(i \text{ is LC} \cap Y_i(0)=1)}{\mathrm{P}(Y_i(0)=1)}$$

By Extended Monotonicity, $Y_i(1)=1$ implies $Y_i(0)=1$, so $\mathrm{E}\left[Y_i(1)-Y_i(0) \mid i \text{ is LC},\ Y_i(1)=1\right]=0$. Thus equation (A6) equals:

$$= -\mathrm{E}\left[Y_i(0)-Y_i(1) \mid i \text{ is LC},\ Y_i(0)=1\right] \cdot \frac{\mathrm{P}(i \text{ is LC} \cap Y_i(0)=1)}{\mathrm{P}(Y_i(0)=1)}$$

(A7) $$= -\mathrm{E}\left[Y_i(0)-Y_i(1) \mid i \text{ is LC},\ Y_i(0)=1\right] \cdot \mathrm{P}(i \text{ is LC} \mid Y_i(0)=1)$$

By definition of LATE compliers, $Z_i=0$ implies $D_i=1$, and $Z_i=1$ implies $D_i=0$. Thus equation (A7) equals:

$$= -\mathrm{E}\left[Y_i(D_i=1)-Y_i(D_i=0) \mid i \text{ is LC},\ Y_i(0)=1\right] \cdot \mathrm{P}(i \text{ is LC} \mid Y_i(0)=1)$$

Under the Extended Monotonicity assumption, the reduced form thus estimates the average causal effect of losing insurance on ED visits for LATE compliers that visit the ED pre-19 (i.e., with $Y_i(0)=1$) times the probability of being a LATE complier conditional on visiting the ED pre-19. For completeness, note that $-\text{P}(i \text{ is LC } \mid Y_i(0)=1)$ equals $\text{E}\left[D_i(1)-D_i(0) \mid Y_i(0)=1\right]$.

9.1.3 INSTRUMENTAL VARIABLES ESTIMATOR

The instrumental variables estimator (of which the fuzzy RD is a special case) equals the reduced-form estimator shown in equation (A5) divided by the bias-corrected first-stage estimator shown in equation (A2). It thus converges to:

$$\frac{-\text{E}\left[Y_i(D_i=1)-Y_i(D_i=0) \mid i \text{ is LC}, \; Y_i(0)=1\right] \cdot \text{P}(i \text{ is LC } \mid Y_i(0)=1)}{-\text{P}(i \text{ is LC } \mid Y_i(0)=1)}$$
$$=\text{E}\left[Y_i(D_i=1)-Y_i(D_i=0) \mid i \text{ is LC}, \; Y_i(0)=1\right]$$

Thus, under the Extended Monotonicity assumption, the IV coefficient estimates the average effect of D_i on Y_i for the subset of LATE compliers that visit the ED when $Z_i=0$ (i.e., that visit the ED pre-19). This is equivalent to a weighted average effect for the entire population of compliers, where the weights are proportional to the probability of visiting the ED pre-19.

9.2 RELAXING THE EXTENDED MONOTONICITY ASSUMPTION

The Extended Monotonicity assumption implies that losing insurance weakly affects individuals' propensity to visit the ED in one direction. This assumption is not guaranteed to hold in the ED data; it is possible that losing insurance induces some people to stop visiting the ED but induces others to start. (The Extended Monotonicity assumption more plausibly holds in the inpatient data used in Section 6; see footnote 29.) We now derive the reduced-form estimand while relaxing the Extended Monotonicity assumption. We then derive the modified first stage that is necessary to rescale the reduced-form estimand.

9.2.1 Reduced Form

The reduced form is $\frac{y_1 - y_0}{y_0}$, or the percentage decline in visits induced by the instrument. As shown in equation (A6) above, under LATE Monotonocity $\frac{y_1 - y_0}{y_0}$ converges to:

$$\text{(A8)} \quad E[Y_i(1) - Y_i(0) \mid i \text{ is LC, } Y_i(1) = 1] \cdot \frac{P(i \text{ is LC} \cap Y_i(1) = 1)}{P(Y_i(0) = 1)}$$
$$- E[Y_i(0) - Y_i(1) \mid i \text{ is LC, } Y_i(0) = 1] \cdot \frac{P(i \text{ is LC} \cap Y_i(0) = 1)}{P(Y_i(0) = 1)}$$

Note the convergence of the reduced form to equation (A8) does not depend on the Extended Monotonicity assumption. By the definition of LATE complier, equation (A8) equals:

$$= E[Y_i(D_i = 0) - Y_i(D_i = 1) \mid i \text{ is LC, } Y_i(1) = 1] \cdot \frac{P(i \text{ is LC} \cap Y_i(1) = 1)}{P(Y_i(0) = 1)}$$
$$- E[Y_i(D_i = 1) - Y_i(D_i = 0) \mid i \text{ is LC, } Y_i(0) = 1] \cdot \frac{P(i \text{ is LC} \cap Y_i(0) = 1)}{P(Y_i(0) = 1)}$$

$$\text{(A9)} \quad = E[Y_i(D_i = 1) - Y_i(D_i = 0) \mid i \text{ is LC, } Y_i(0) = 1] \cdot \frac{-P(i \text{ is LC} \cap Y_i(0) = 1)}{P(Y_i(0) = 1)}$$
$$+ E[Y_i(D_i = 1) - Y_i(D_i = 0) \mid i \text{ is LC, } Y_i(1) = 1] \cdot \frac{-P(i \text{ is LC} \cap Y_i(1) = 1)}{P(Y_i(0) = 1)}$$

Under LATE Monotonicity, the reduced form estimates a weighted sum of two average causal effects of D_i on Y_i. The first is the average causal effect of losing insurance for LATE compliers that visit the ED pre-19 (i.e., that have $Y_i(0) = 1$). The second is the average causal effect of losing insurance for LATE compliers that visit the ED post-19 (i.e., that have $Y_i(1) = 1$). Note that these two groups are not mutually exclusive; individuals that are "extended always-takers" appear in both groups.

9.2.2 Modified First Stage

The goal of the modified first stage is to recover the weights in the reduced form above. The original bias-adjusted first stage converged to $-\mathrm{P}(i \text{ is LC} \mid Y_i(0)=1)$ (which is identical to the first of the two weights above). We modify the first stage so that it now estimates the sum of the two weights above. The modified first stage is:

(A10) $$\frac{2(d_1-d_0)}{y_0}+\frac{(y_0-y_1)}{y_0}$$

From the derivation of the original bias-adjusted first stage, the first term of equation (A10) converges to twice the quantity shown in equation (A4):

$$\operatorname{plim}\left(\frac{2(d_1-d_0)}{y_0}\right)=-\frac{2\cdot \mathrm{P}(Y_i(0)=1\cap i \text{ is LC})}{\mathrm{P}(Y_i(0)=1)}$$

The last term of equation (A10) converges to:

$$\operatorname{plim}\left(\frac{y_0-y_1}{y_0}\right)=\operatorname{plim}\left(\frac{y_0/N-y_1/N}{y_0/N}\right)$$
$$=\frac{\mathrm{P}(Y_i(0)=1)-\mathrm{P}(Y_i(1)=1)}{\mathrm{P}(Y_i(0)=1)}$$
(A11) $$=\frac{\mathrm{P}(Y_i(0)=1\cap i \text{ is LAT})+\mathrm{P}(Y_i(0)=1\cap i \text{ is LNT})+\mathrm{P}(Y_i(0)=1\cap i \text{ is LC})}{\mathrm{P}(Y_i(0)=1)}$$
$$-\frac{\mathrm{P}(Y_i(1)=1\cap i \text{ is LAT})+\mathrm{P}(Y_i(1)=1\cap i \text{ is LNT})+\mathrm{P}(Y_i(1)=1\cap i \text{ is LC})}{\mathrm{P}(Y_i(0)=1)}$$

By the IV exclusion restriction and the definitions of LATE always-takers and LATE never-takers, "*i* is LAT" or "*i* is LNT" imply that $Y_i(0)=Y_i(1)$. Thus equation (A11) equals:

$$=\frac{\mathrm{P}(Y_i(0)=1\cap i \text{ is LC})-\mathrm{P}(Y_i(1)=1\cap i \text{ is LC})}{\mathrm{P}(Y_i(0)=1)}$$

The modified first stage shown in equation (A10) thus converges to:

$$\text{plim}\left(\frac{2(d_1-d_0)}{y_0}+\frac{y_0-y_1}{y_0}\right)=-\frac{2\cdot \text{P}(Y_i(0)=1\cap i \text{ is LC})}{\text{P}(Y_i(0)=1)}+\frac{\text{P}(Y_i(0)=1\cap i \text{ is LC})-\text{P}(Y_i(1)=1\cap i \text{ is LC})}{\text{P}(Y_i(0)=1)}$$

$$=-\frac{\text{P}(Y_i(0)=1\cap i \text{ is LC})+\text{P}(Y_i(1)=1\cap i \text{ is LC})}{\text{P}(Y_i(0)=1)} \tag{A12}$$

The modified first stage, equation (A10), therefore estimates the sum of the weights from the reduced form.

9.2.3 Modified Instrumental Variables Estimator

The modified instrumental variables estimator equals the reduced form estimator shown in equation (A5) divided by the modified first-stage estimator shown in equation (A10). It thus converges to:

$$\text{E}\left[Y_i(D_i=1)-Y_i(D_i=0)\mid i \text{ is LC},\ Y_i(0)=1\right]\cdot\frac{\text{P}(i \text{ is LC}\cap Y_i(0)=1)}{\text{P}(Y_i(0)=1\cap i \text{ is LC})+\text{P}(Y_i(1)=1\cap i \text{ is LC})}$$
$$+\text{E}\left[Y_i(D_i=1)-Y_i(D_i=0)\mid i \text{ is LC},\ Y_i(1)=1\right]\cdot\frac{\text{P}(i \text{ is LC}\cap Y_i(1)=1)}{\text{P}(Y_i(0)=1\cap i \text{ is LC})+\text{P}(Y_i(1)=1\cap i \text{ is LC})}$$

Thus, when relaxing the Extended Monotonicity assumption, the modified instrumental variables estimator converges to a weighted average of two average causal effects of D_i on Y_i. The first is the average causal effect of losing insurance for LATE compliers that visit the ED pre-19 (i.e., that have $Y_i(0)=1$). The second is the average causal effect of losing insurance for LATE compliers that visit the ED post-19 (i.e., that have $Y_i(1)=1$). Note that these two groups are not mutually exclusive. In particular, both groups contain LATE compliers that would visit the ED regardless of insurance status. Thus the average is skewed towards this group, but for this group insurance status has no causal effect on ED visits. The modified instrumental variables estimand is thus attenuated relative to the expected effect of increasing health insurance coverage for all LATE compliers.

9.2.4 Estimates From Emergency Department Data

The modified first-stage, equation (A10), is equal to –0.129 in the ED data.[40] The modified first stage thus generates a modified IV estimate of 0.254, as compared to the original IV estimate of 0.404. However, as noted in Section 9.2.3, this estimate is attenuated in the sense that it places double weight on individuals that visit the ED regardless of insurance status ("extended always-takers"), because for these individuals $Y_i(0)=1$ and $Y_i(1)=1$. To see that these individuals receive double weight, note that the reduced form estimand, equation (A9), can be rewritten as:

$$\mathrm{E}\left[Y_i(D_i=1)-Y_i(D_i=0)\mid i \text{ is LC, } Y_i(0)=1\right]\cdot\frac{-\mathrm{P}(i \text{ is LC}\cap Y_i(0)=1)}{\mathrm{P}(Y_i(0)=1)}$$
$$+\mathrm{E}\left[Y_i(D_i=1)-Y_i(D_i=0)\mid i \text{ is LC, } Y_i(1)=1\right]\cdot\frac{-\mathrm{P}(i \text{ is LC}\cap Y_i(1)=1)}{\mathrm{P}(Y_i(0)=1)}$$

$$=\Big\{\mathrm{E}\left[Y_i(D_i=1)-Y_i(D_i=0)\mid i \text{ is LC, } i \text{ is EAT}\right]\cdot\mathrm{P}(i \text{ is EAT}\mid i \text{ is LC, } Y_i(0)=1)$$
$$+\mathrm{E}\left[Y_i(D_i=1)-Y_i(D_i=0)\mid i \text{ is LC, } i \text{ is EC}\right]\cdot\mathrm{P}(i \text{ is EC}\mid i \text{ is LC, } Y_i(0)=1)\Big\}$$
$$\cdot\frac{-\mathrm{P}(i \text{ is LC}\cap Y_i(0)=1)}{\mathrm{P}(Y_i(0)=1)}$$
$$+\Big\{\mathrm{E}\left[Y_i(D_i=1)-Y_i(D_i=0)\mid i \text{ is LC, } i \text{ is EAT}\right]\cdot\mathrm{P}(i \text{ is EAT}\mid i \text{ is LC, } Y_i(1)=1)$$
$$+\mathrm{E}\left[Y_i(D_i=1)-Y_i(D_i=0)\mid i \text{ is LC, } i \text{ is EDF}\right]\cdot\mathrm{P}(i \text{ is EDF}\mid i \text{ is LC, } Y_i(1)=1)\Big\}$$
$$\cdot\frac{-\mathrm{P}(i \text{ is LC}\cap Y_i(1)=1)}{\mathrm{P}(Y_i(0)=1)}$$

$$=\mathrm{E}\left[Y_i(D_i=1)-Y_i(D_i=0)\mid i \text{ is LC, } i \text{ is EAT}\right]\cdot\frac{-\mathrm{P}(i \text{ is EAT}\cap i \text{ is LC}\cap Y_i(0)=1)}{\mathrm{P}(Y_i(0)=1)}$$
$$+\mathrm{E}\left[Y_i(D_i=1)-Y_i(D_i=0)\mid i \text{ is LC, } i \text{ is EC}\right]\cdot\frac{-\mathrm{P}(i \text{ is EC}\cap i \text{ is LC}\cap Y_i(0)=1)}{\mathrm{P}(Y_i(0)=1)}$$
$$+\mathrm{E}\left[Y_i(D_i=1)-Y_i(D_i=0)\mid i \text{ is LC, } i \text{ is EAT}\right]\cdot\frac{-\mathrm{P}(i \text{ is EAT}\cap i \text{ is LC}\cap Y_i(1)=1)}{\mathrm{P}(Y_i(0)=1)}$$
$$+\mathrm{E}\left[Y_i(D_i=1)-Y_i(D_i=0)\mid i \text{ is LC, } i \text{ is EDF}\right]\cdot\frac{-\mathrm{P}(i \text{ is EDF}\cap i \text{ is LC}\cap Y_i(1)=1)}{\mathrm{P}(Y_i(0)=1)}$$

[40] We count privately insured patients, Medicaid patients, and "other insurance" patients as insured. Taking the estimates from Tables 2 and 3, equation (A10) thus equals 2*(–0.0629 – 0.0166 –0.0015) + 0.033 = –0.129.

The extended always-takers (EAT) appear twice because they visit the ED both when insured and uninsured. By definition, however, $Y_i(D_i=1)=Y_i(D_i=0)=1$ for extended always-takers, so either of the conditional expectations involving extended always-takers can be eliminated. Thus the reduced form also converges to:

$$
\begin{aligned}
\text{(A13)}\quad & \mathrm{E}\left[Y_i(D_i=1)-Y_i(D_i=0)\mid i \text{ is LC},\ i \text{ is EAT}\right]\cdot\frac{-\mathrm{P}(i \text{ is EAT}\cap i \text{ is LC})}{\mathrm{P}(Y_i(0)=1)} \\
& +\mathrm{E}\left[Y_i(D_i=1)-Y_i(D_i=0)\mid i \text{ is LC},\ i \text{ is EC}\right]\cdot\frac{-\mathrm{P}(i \text{ is EC}\cap i \text{ is LC})}{\mathrm{P}(Y_i(0)=1)} \\
& +\mathrm{E}\left[Y_i(D_i=1)-Y_i(D_i=0)\mid i \text{ is LC},\ i \text{ is EDF}\right]\cdot\frac{-\mathrm{P}(i \text{ is EDF}\cap i \text{ is LC})}{\mathrm{P}(Y_i(0)=1)}
\end{aligned}
$$

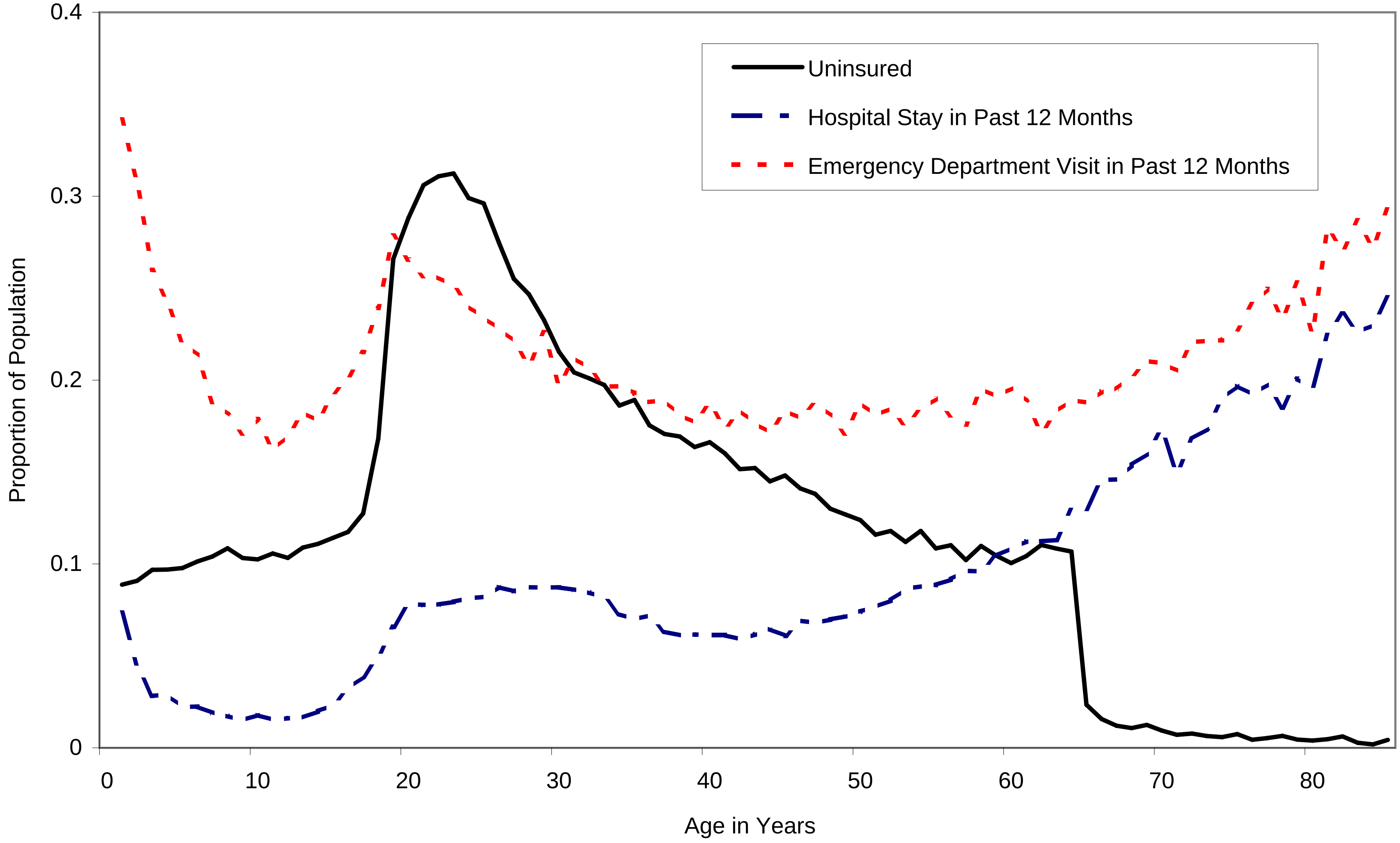
The reduced form therefore estimates a weighted average of three average causal effects: the average causal effect for LATE compliers who are extended always-takers, the average causal effect for LATE compliers who are "extended compliers" (individuals that visit the ED only when insured), and the average causal effect for LATE compliers who are "extended defiers" (individuals that visit the ED only when uninsured). These three mutually exclusive groups exhaust the population of LATE compliers that visit the ED. Each group's weight is proportional to its share of LATE compliers that visit the ED either before or after age 19 (i.e., LATE compliers who are not extended never-takers). With estimates of the weights in equation (A13), we can recover the average causal effect of insurance for LATE compliers that visit the ED before or after age 19.

It is impossible, however, to identify exactly what portion of LATE compliers are extended always-takers versus extended compliers or extended defiers. But note that from equation (A4), the original bias-corrected first stage (equation (A2)) estimates $-\frac{\mathrm{P}(Y_i(0)=1\cap i \text{ is LC})}{\mathrm{P}(Y_i(0)=1)}=-\left(\frac{\mathrm{P}(i \text{ is EAT}\cap i \text{ is LC})}{\mathrm{P}(Y_i(0)=1)}+\frac{\mathrm{P}(i \text{ is EC}\cap i \text{ is LC})}{\mathrm{P}(Y_i(0)=1)}\right)$, or the sum of the first two weights in equation (A13). As reported in Table 2, this quantity equals 0.081. Likewise, equations (A12) and (A4) imply that the difference between the modified bias-corrected first stage (equation (A10)) and the original bias-corrected first stage (equation (A2)) estimates

$-\frac{P(Y_i(1)=1 \cap i \text{ is LC})}{P(Y_i(0)=1)} = -\left(\frac{P(i \text{ is EAT} \cap i \text{ is LC})}{P(Y_i(0)=1)} + \frac{P(i \text{ is EDF} \cap i \text{ is LC})}{P(Y_i(0)=1)}\right)$, or the sum of the first and third weights in equation (A13). This quantity is 0.048 (given by 0.129 – 0.081 = 0.048). However, equation (A13) has three unknown quantities, and we have only two linearly independent estimates, equations (A2) and (A10). We must therefore make an additional assumption to derive a bound on the sum of the weights in equation (A13).

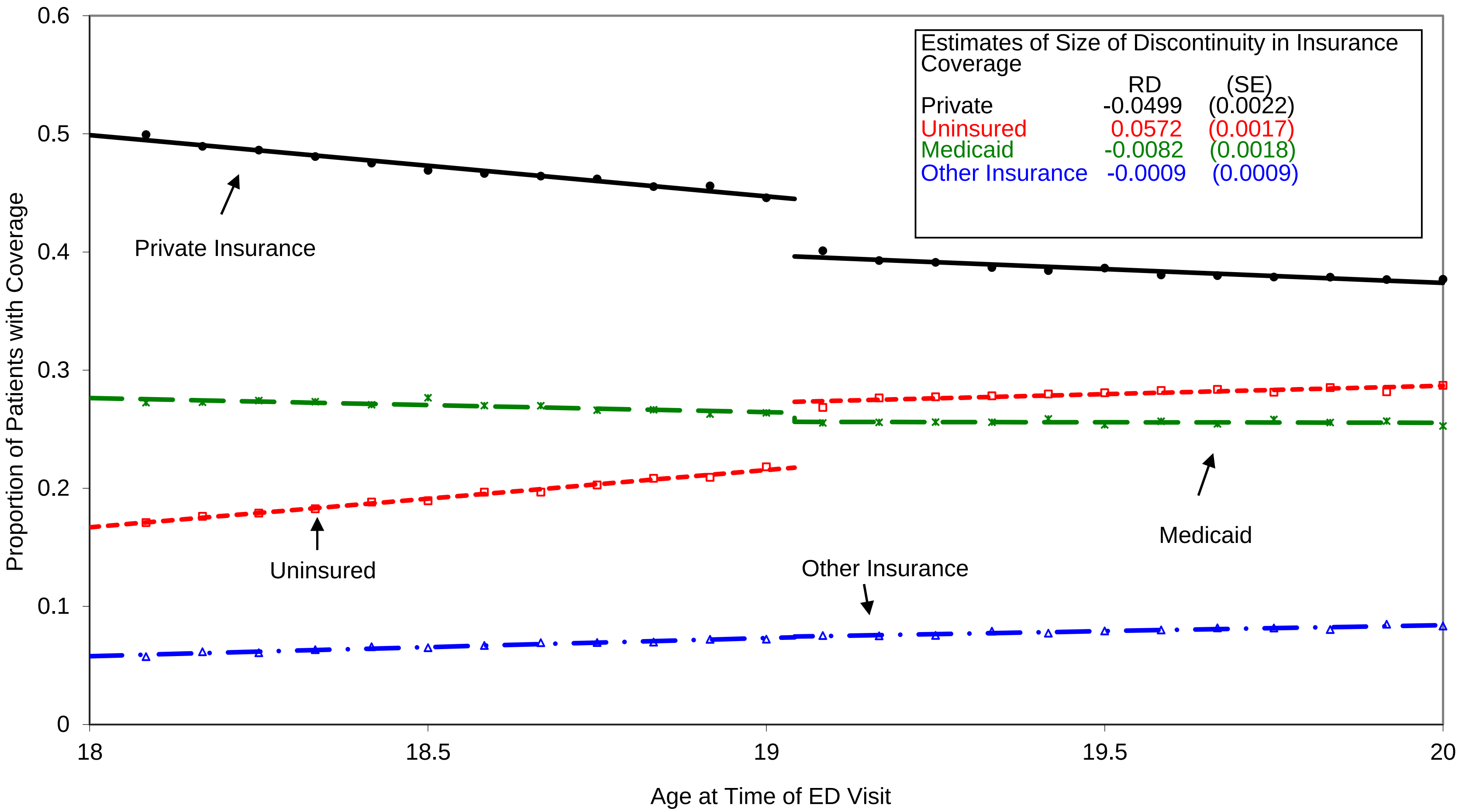
To establish an upper bound (in magnitude) on the sum of the three weights in equation (A13), we make the reasonable assumption that the number of LATE compliers that stop visiting the ED when becoming uninsured (extended compliers) is no greater than the number of LATE compliers that either continue to visit the ED when becoming uninsured or begin visiting the ED when becoming uninsured (extended always-takers plus extended defiers). In other words, we assume that the number of newly uninsured that stop visiting the ED is no greater than the number of newly uninsured that continue visiting the ED plus the number of newly uninsured that begin visiting the ED. Under this assumption, $\frac{P(i \text{ is EC} \cap i \text{ is LC})}{P(Y_i(0)=1)}$ is at most 0.048, and thus $\frac{P(i \text{ is EAT} \cap i \text{ is LC})}{P(Y_i(0)=1)}$ is at least 0.033 (given by 0.081 – 0.048 = 0.033). We therefore adjust the modified first-stage estimator for double counting of extended always-takers by subtracting at least 0.033, and find that the modified first-stage estimator has an upper bound (in magnitude) of 0.096 (given by 0.129 – 0.033 = 0.096). A modified first-stage of –0.096 generates a modified IV estimate of 0.341. We thus conclude that losing insurance coverage reduces the probability of an ED visit for LATE compliers that could potentially visit the ED by at least 34 percent.

Figure 1: Age Profile of Proportion Uninsured, ED visits and Hospital Stays



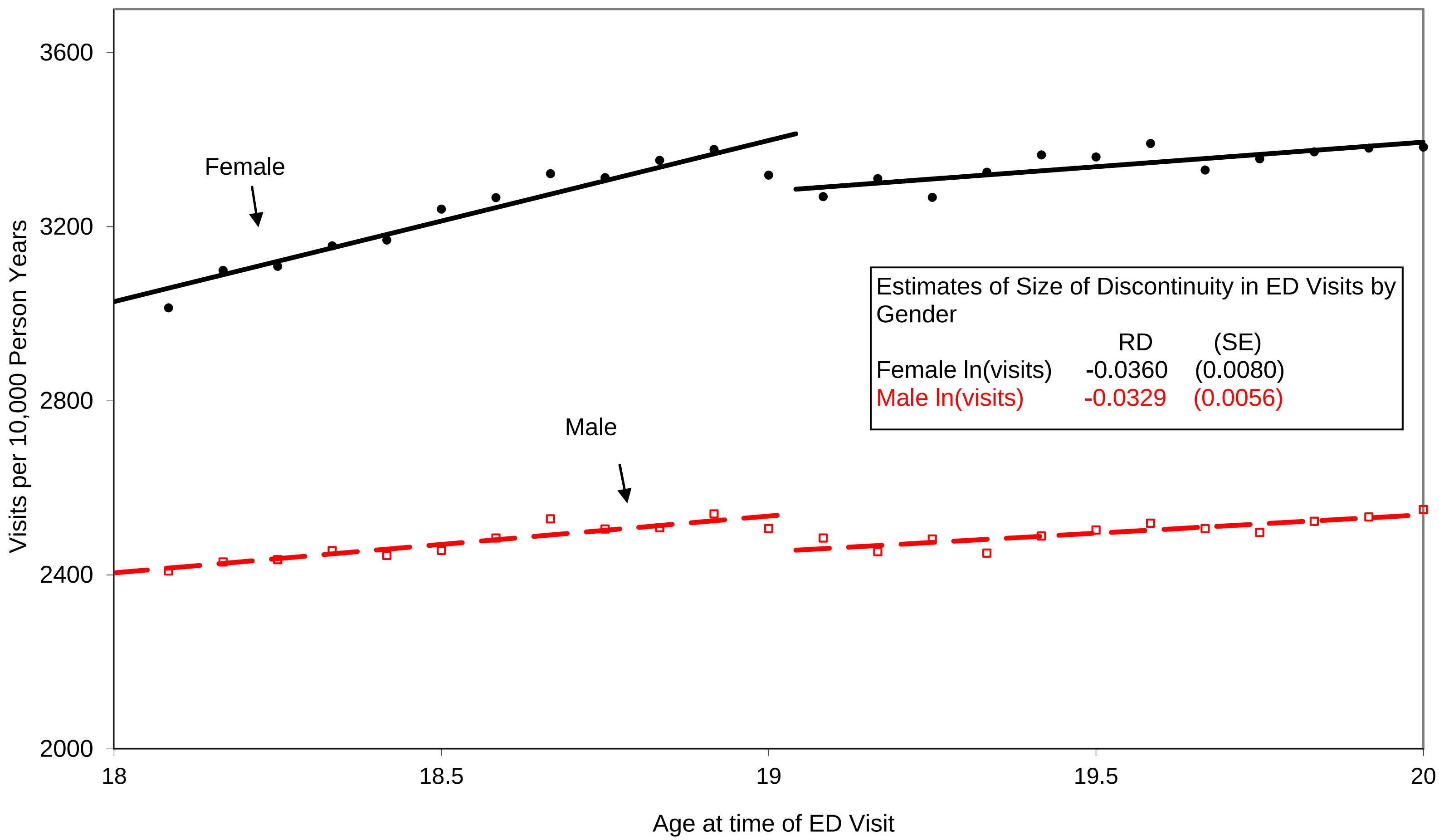
Notes: These estimates are derived from the NHIS 1997-2007.

Figure 2: Age Profile of Insurance Coverage for People Entering the Emergency Department



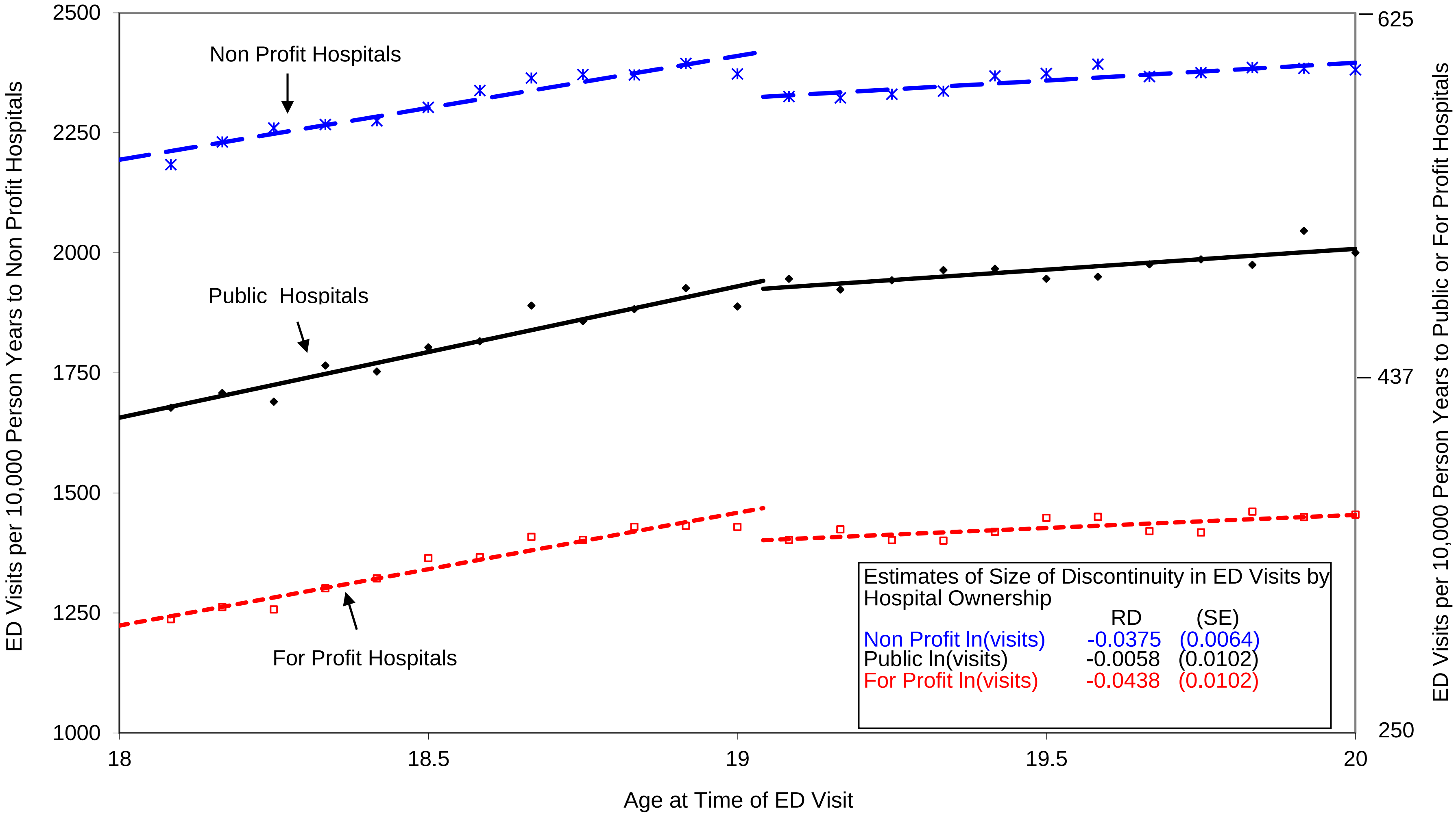
Notes: The Emergency Department datasets used to make the age profiles above are a near census of ED visits in Arizona (2005-2007), California (2005-2007), Iowa (2004-2007), New Jersey (2004-2007) and Wisconsin (2004-2006). Only hospitals that are not under state oversight do not contribute data. The sample includes 1,744,367 ED visits by individuals either 18 or 19 years old. The dependent variable in the regressions is the proportion of individuals that lack health insurance.

Figure 3: Age Profile of Emergency Department Visits by Gender



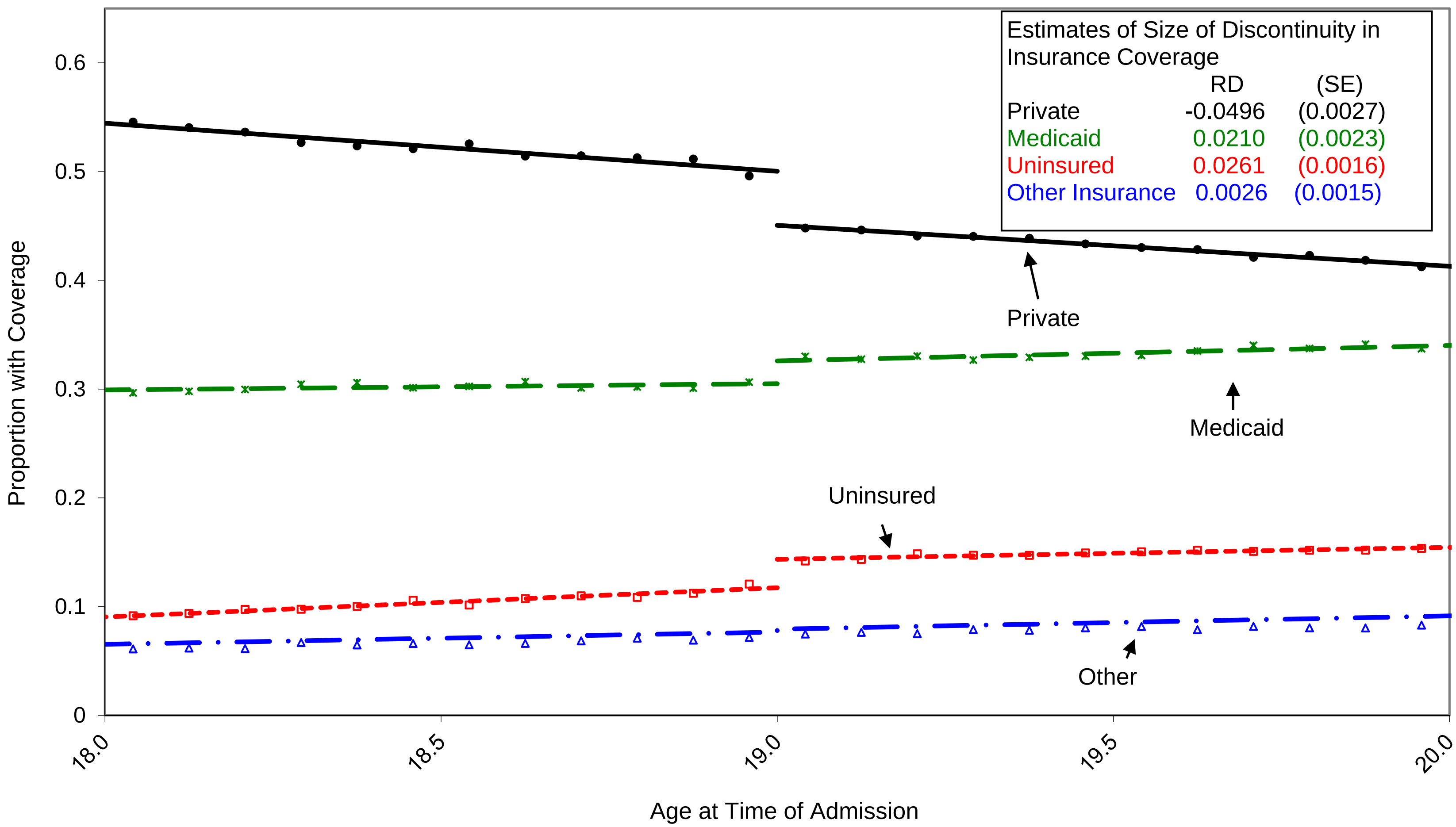
Notes: See notes from Figure 2. The age profiles are in rates per 10,000 person years. The dependent variable for the regression estimates is the natural log of the admission counts. The female category does not include pregnant women (7.8% of ED visits). Patients that present at the ED and are admitted to the hospital are drawn from hospital discharge records and included in the analysis.

Figure 4: Age Profile of Emergency Department Visits By Ownership of Hospital



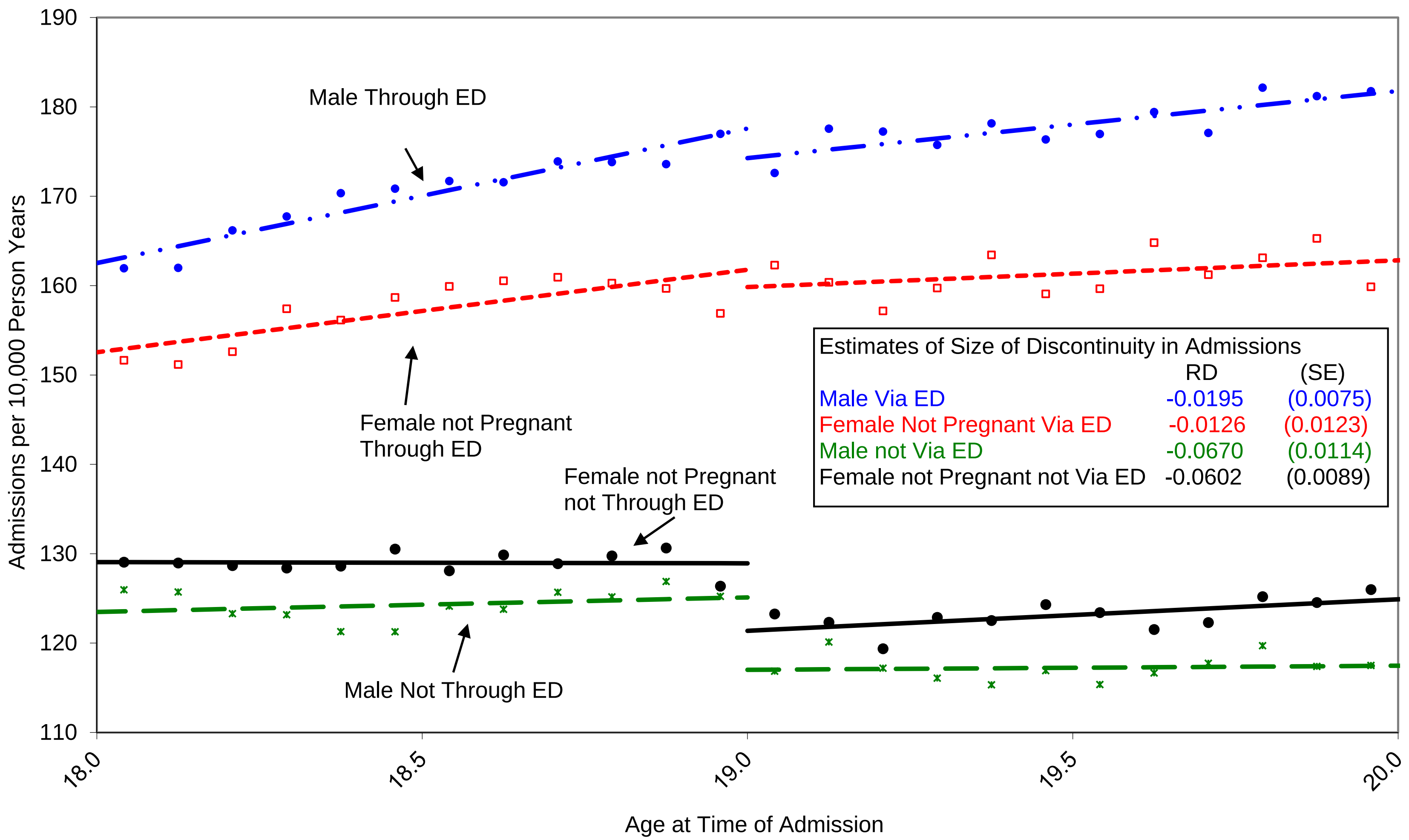
Notes: See notes from Figure 3. Approximately 1.4 percent of people are admitted to hospitals of unknown ownership type.

Figure 5: Age Profile of Insurance Coverage for Hospital Admissions



Notes: The hospital discharge datasets used to make the age profiles include a near census of hospital stays in Arizona (2000-2007), California (1990-2006), Iowa (2004-2007), New York (1990-2006), Texas (1999-2003) and Wisconsin (2004-2006). Women that are pregnant have been dropped from the sample. Combining the data from the six states gives a sample of 849,636 hospital stays by 18 and 19 year olds that are not pregnancy related. Each of the points plotted above is the proportion of people with a particular type of coverage. The lines are the fitted values from a linear regression fitted to the points on either side of the age 19 cut off.

Figure 6: Hospital Admissions by Gender and Route into Hospital



Notes: See notes from Figure 5. Each of the points plotted above is the number of people admitted to the hospital at a particular month in age per 10,000 person years. The line laying over the points is the fitted values from a linear regression estimated from the observations on either side of the age 19 cut off. The point estimate in the box is the estimate from a regression with the same specification but the dependent variable is the natural log of the counts.

Table 1: Differences Between Insured and Uninsured Young Adults
(National Health Interview Survey 1997-2007)

	Insured	Uninsured	Difference Between Insured and Uninsured	P-value for Difference in Means	Regression Estimates of Discrete Jump at 19 (1 year Bandwidth)	P-value for Difference in RD
	(1)	(2)	(3)	(4)	(5)	(6)
Employed	52.1	55.0	3.0	0.001	-2.4	0.090
In School	33.3	17.9	-15.4	0.000	1.0	0.458
Percent Days Drinking	5.9	5.9	0.0	0.955	0.2	0.707
Smoker	20.5	33.8	13.3	0.000	-0.3	0.875
Flu Shot Last 12 Months	15.5	10.2	-5.4	0.000	-2.2	0.244
Married	3.5	7.7	4.2	0.000	-0.5	0.333
Insurance in Own Name	6.9	0.0	-6.9	0.000	-0.7	0.318

Notes: All the estimates in the table are based on a dataset created by stacking the NHIS Person Files and Sample Adult Files for the 1997-2007 survey years. All the estimates are presented in percents. The estimates are weighted and the standard errors are adjusted to account for the stratified sampling frame. The outcomes Flu Shot, Smoker and Percent Days Drinking a coded from the NHIS Sample Adult files 1997-2007 which include 8,121 respondents surveyed within 12 months of their 19th birthday. The remaining variables are coded from the NHIS Person file 1997-2007 which includes 24,155 respondents survey within 12 months of their 19th birthday. The regression discontinuity estimates in the column 5 and its p-values in column 6 are from a linear polynomial interacted with an indicator variable for over 19 estimated from all respondents surveyed within 12 months of their 19th birthday.

Table 2: Change at Age 19 in Insurance Coverage of Emergency Department Visits

	Private	Uninsured	Medicaid	Other Insurance
	(1)	(2)	(3)	(4)
All	-0.0629	0.0810	-0.0166	-0.0015
	[0.0026]	[0.0046]	[0.0025]	[0.0010]
	0.4471	0.2154	0.2644	0.0731
All (Except Pregnant)	-0.0628	0.0843	-0.0201	-0.0014
	[0.0027]	[0.0048]	[0.0024]	[0.0011]
	0.4649	0.2178	0.2415	0.0758
Male	-0.0657	0.0831	-0.0191	0.0017
	[0.0035]	[0.0049]	[0.0026]	[0.0015]
	0.4632	0.2479	0.1949	0.0941
Female	-0.0605	0.0791	-0.0156	-0.0030
	[0.0030]	[0.0056]	[0.0032]	[0.0012]
	0.4336	0.1942	0.3158	0.0563
Female Not Pregnant	-0.0597	0.0844	-0.0216	-0.0031
	[0.0033]	[0.0059]	[0.0029]	[0.0014]
	0.4639	0.1953	0.2820	0.0588

Notes: The Emergency Department visits used to estimate the regressions are a near census of ED visits in Arizona (2005-2007), California (2005-2007), Iowa (2004-2007), New Jersey (2004-2007) and Wisconsin (2004-2006).The parameter estimates in the table above are the percentage point change in insurance coverage when people age out of their insurance coverage on the last day of the month in which they turn 19. The standard errors are in brackets directly below the parameter estimates. Below the SE we have included the estimated level of the dependent variable immediately before people age out. The parameter estimates are adjusted for the decline in admissions under the assumption that the decline in admission is due entirely to people losing their insurance coverage. The adjustment is made by estimating the insurance coverage regression and the log(admissions) regressions via seemingly unrelated regression then using the estimated percent drop in admissions to adjust the coverage estimates. The regressions are run on the averages for one month cells as this is the most refined version of the age variable available. The regressions include all Emergency Department visits by individuals age 18 or 19. There are 1,744,367 visits in this age range, of these 998,745 are by females, 695,012 are by males and the remainder are of unknown gender.

Table 3: Change at Age 19 in Volume of Emergency Department Visits

	All Visits	Public Hospitals	Non Profit Hospitals	For Profit Hospitals
	(1)	(2)	(3)	(4)
All	-0.0333	-0.0058	-0.0375	-0.0438
	[0.0060]	[0.0102]	[0.0064]	[0.0102]
All (Except Pregnant)	-0.0351	-0.0091	-0.0387	-0.0472
	[0.0061]	[0.0111]	[0.0063]	[0.0114]
Male	-0.0329	0.0076	-0.0390	-0.0506
	[0.0056]	[0.0135]	[0.0054]	[0.0138]
Female	-0.0330	-0.0170	-0.0353	-0.0403
	[0.0080]	[0.0143]	[0.0084]	[0.0132]
Female Not Pregnant	-0.0360	-0.0264	-0.0366	-0.0456
	[0.0080]	[0.0166]	[0.0085]	[0.0154]

Notes: See notes from Table 2. The dependent variable in all the regressions above is the log of visits at each age in months. Of the 1,744,367 total visits among people age 18 and 19: 255,715 are to public hospitals, 1,276,045 are to non profit hospitals, 188,409 are to for profit hospitals and the remaining admissions are to hospitals of unknown ownership type.

Table 4: Impact of Losing Insurance Coverage on Emergency Department Visits

	All Visits	Public Hospitals	Non Profit Hospitals	For Profit Hospitals
	(1)	(2)	(3)	(4)
All	-0.4041	-0.0709	-0.4540	-0.5289
	[0.0776]	[0.1259]	[0.0832]	[0.1296]
All (Except Pregnant)	-0.4102	-0.1074	-0.4509	-0.5473
	[0.0761]	[0.1318]	[0.0791]	[0.1389]
Male	-0.3891	0.0914	-0.4602	-0.5935
	[0.0713]	[0.1625]	[0.0706]	[0.1698]
Female	-0.4099	-0.2127	-0.4382	-0.4999
	[0.1053]	[0.1814]	[0.1108]	[0.1707]
Female Not Pregnant	-0.4190	-0.3088	-0.4262	-0.5288
	[0.0994]	[0.1979]	[0.1052]	[0.1864]

Notes: See notes from Table 3. The estimates above are the ratio of the change in admissions to the overall change in insurance coverage. The standard errors are in brackets below the estimates. The ratios and their standard errors are computed by estimating the relevant regressions via seemingly unrelated regression.

Table 5: Change at 19 in Insurance Coverage of People Admitted to the Hospital

	Private	Uninsured	Medicaid	Other Insurance
	(1)	(2)	(3)	(4)
All	-0.0458	0.0271	0.0187	0.0000
	[0.0027]	[0.0052]	[0.0033]	[0.0006]
	0.3393	0.0762	0.5396	0.0446
All (Except Pregnant)	-0.0664	0.0579	0.0088	-0.0003
	[0.0036]	[0.0048]	[0.0026]	[0.0013]
	0.5003	0.1174	0.3050	0.0766
Male	-0.0692	0.0626	0.0072	-0.0006
	[0.0043]	[0.0049]	[0.0034]	[0.0017]
	0.4888	0.1313	0.2896	0.0898
Female not Pregnant	-0.0621	0.0496	0.0121	0.0006
	[0.0043]	[0.0075]	[0.0044]	[0.0018]
	0.5137	0.0993	0.3266	0.0595
Female Pregnant	-0.0332	0.0091	0.0239	0.0002
	[0.0029]	[0.0078]	[0.0056]	[0.0006]
	0.2311	0.0476	0.6986	0.0226

Notes: The estimates above are from a near census of hospital stays in Arizona (2000-2007), California (1990-2006), Iowa (2004-2007), New York (1990-2006), Texas (1999-2003) and Wisconsin (2004-2006). Combining the data from the six states gives a sample of 2,067,996 inpatient stays for 18 and 19 year olds of which 849,636 are not pregnancy related. This table presents estimates of the change in insurance coverage (among people admitted to the hospital) that occurs on the first day of the month after people turn 19. Directly below the estimates are the standard errors of the estimates and below the standard errors are the proportion of the population with this type of coverage immediately before people age out at 19. The estimates are made using a linear polynomial in age for estimated using admissions among people age 18 to age 20. The estimates of the change in insurance are adjusted for the effect of insurance status on the probability of getting treated.

Table 6: Change at Age 19 in Admissions to the Hospital

	All Visits	Via Emergency Department	Not Via Emergency Department
	(1)	(2)	(3)
All	-0.0168	-0.0096	-0.0202
	[0.0056]	[0.0057]	[0.0082]
All (Except Pregnant)	-0.0379	-0.0174	-0.0663
	[0.0050]	[0.0077]	[0.0079]
Male	-0.0386	-0.0195	-0.0670
	[0.0057]	[0.0075]	[0.0114]
Female not Pregnant	-0.0333	-0.0126	-0.0602
	[0.0086]	[0.0123]	[0.0089]
Female Pregnant	-0.0053	0.0116	-0.0079
	[0.0086]	[0.0099]	[0.0091]
Public	0.0026	0.0181	-0.0122
	[0.0070]	[0.0087]	[0.0117]
Private Non Profit	-0.0145	-0.0193	-0.0127
	[0.0060]	[0.0074]	[0.0087]
Private For Profit	-0.0394	-0.0342	-0.0405
	[0.0104]	[0.0189]	[0.0117]

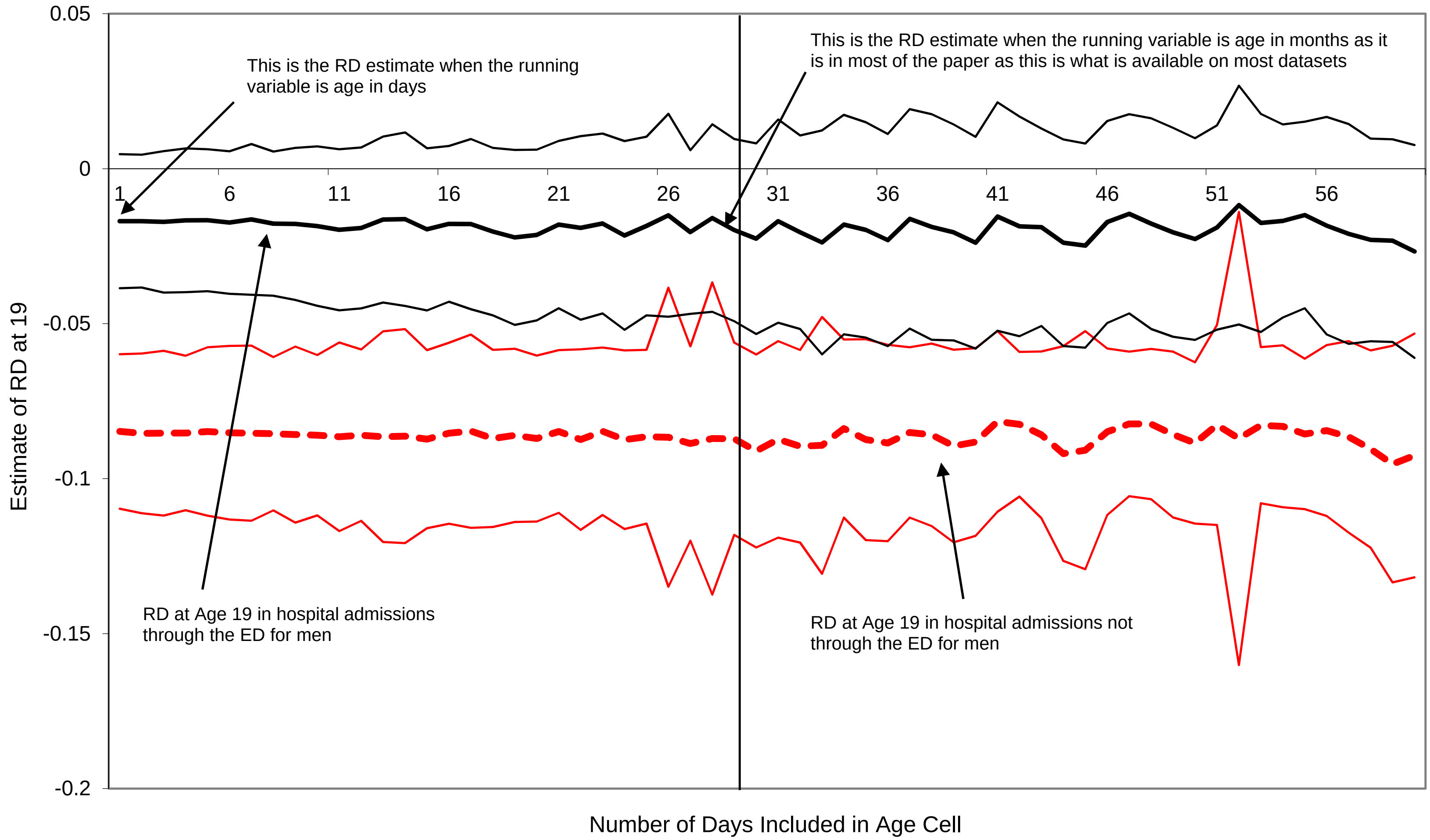
Notes: See notes from Table 5. The dependent variable is the log of admissions and the results are for overall admissions and split by route into the hospital.

Table 7: Impact of Losing Insurance on Admissions to the Hospital

	All Visits	Via Emergency Department	Not Via Emergency Department
	(1)	(2)	(3)
All	-0.6135	-0.3517	-0.7373
	[0.2382]	[0.2208]	[0.3343]
All (Except Pregnant)	-0.6411	-0.2969	-1.1073
	[0.1018]	[0.1351]	[0.1660]
Male	-0.6053	-0.3092	-1.0349
	[0.1030]	[0.1222]	[0.2004]
Female not Pregnant	-0.6601	-0.2516	-1.1779
	[0.2009]	[0.2509]	[0.2567]
Female Pregnant	-0.5853	1.2829	-0.8668
	[1.0731]	[1.5496]	[1.2521]
Public	0.0953	0.6730	-0.4467
	[0.2587]	[0.3453]	[0.4399]
Private Non Profit	-0.5298	-0.7039	-0.4647
	[0.2438]	[0.3050]	[0.3331]
Private For Profit	-1.4238	-1.2410	-1.4625
	[0.4739]	[0.7376]	[0.5178]

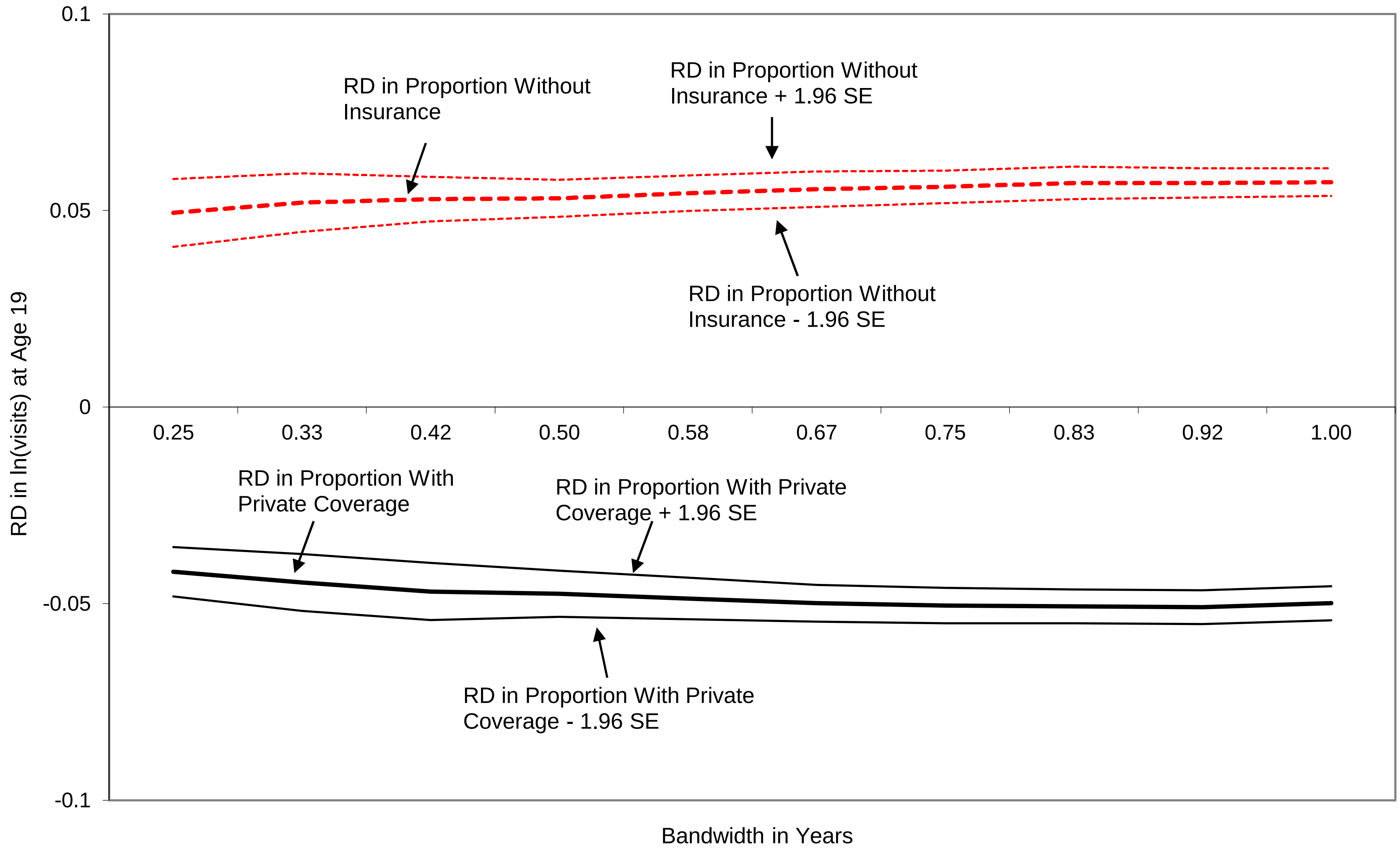
Notes: See notes from Table 6. The elasticities above are the impact of losing insurance on hospital admissions. They are computed by dividing the percent change in admissions by the percent change in the population that is uninsured.

Appendix Figure 1: Impact on RD Estimates of Coarsening the Age Variable from Age in Days to 60 Day Cells



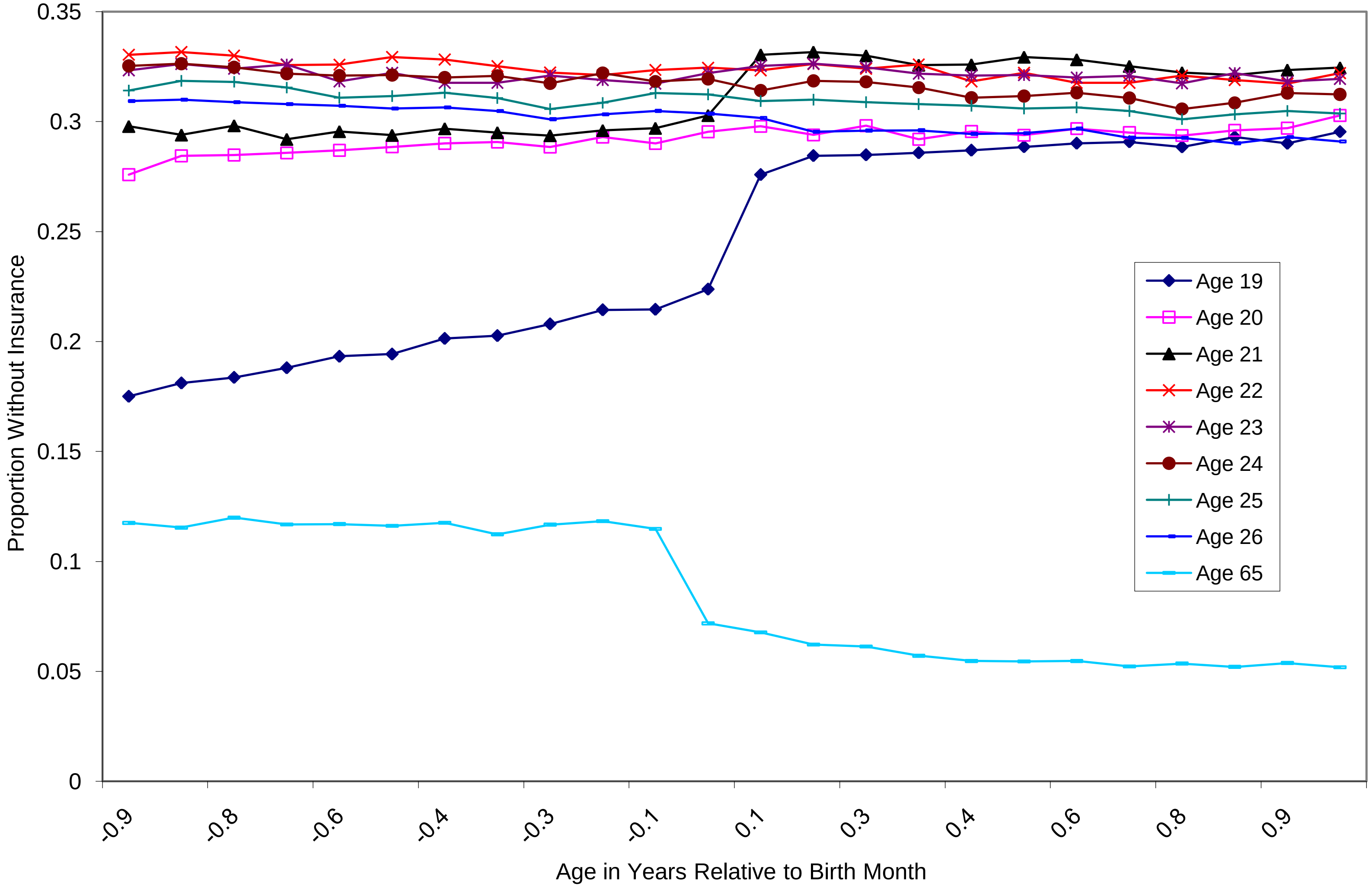
Notes: The regressions in the paper all have age cells of approximately 30 because age is typically only available in months. The heavy lines are the estimates of the RD and the lighter lines are the confidence intervals.

Appendix Figure 2: Assessing Sensitivity to Bandwidth Choice of Estimate of Change in Insurance Among People Treated in the Emergency Department

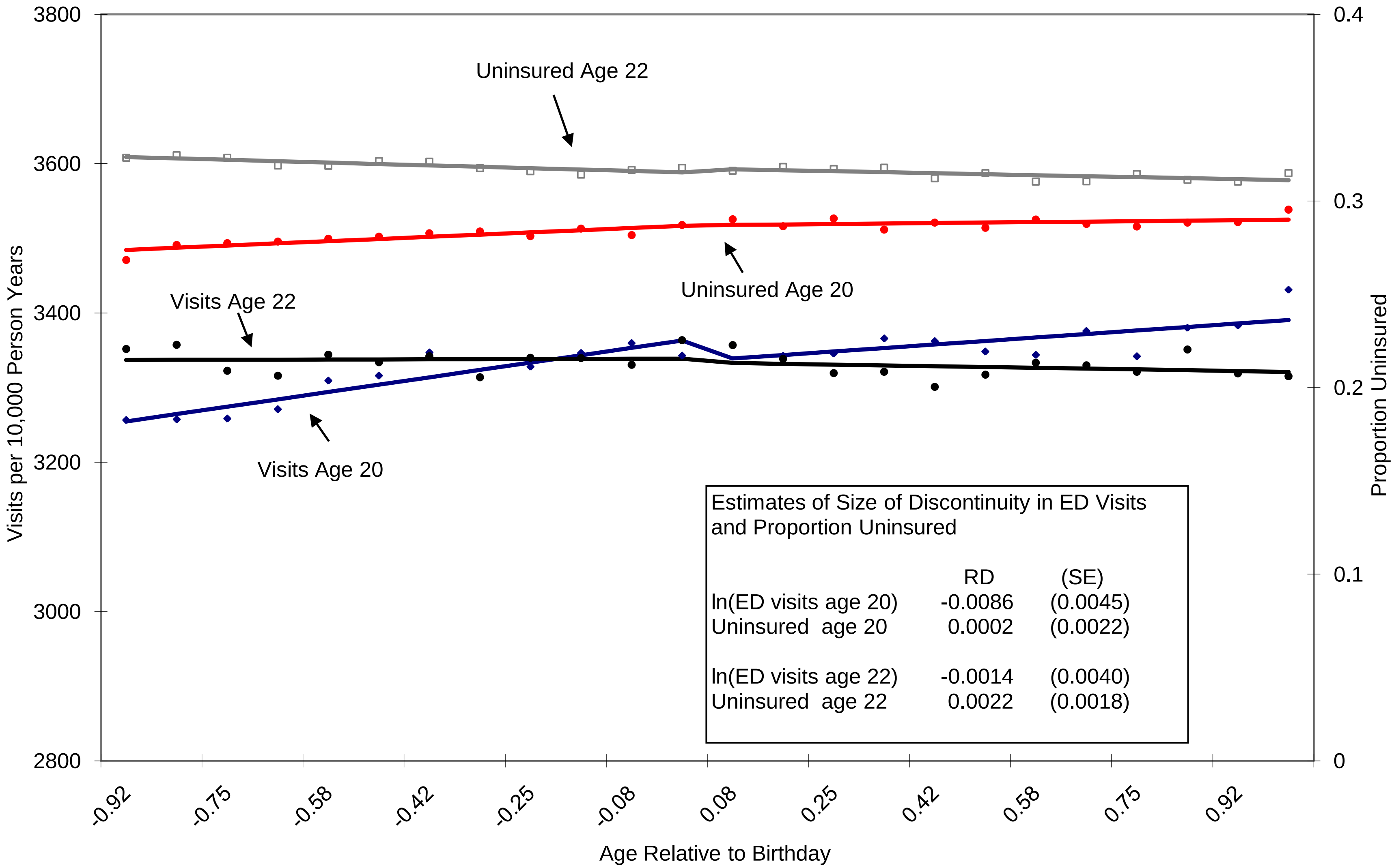


Notes: The estimates above are the discrete change at age 19 from a local linear regression with a symmetric bandwidth. The heavy line is the point estimate and the lighter lines are the confidence intervals.

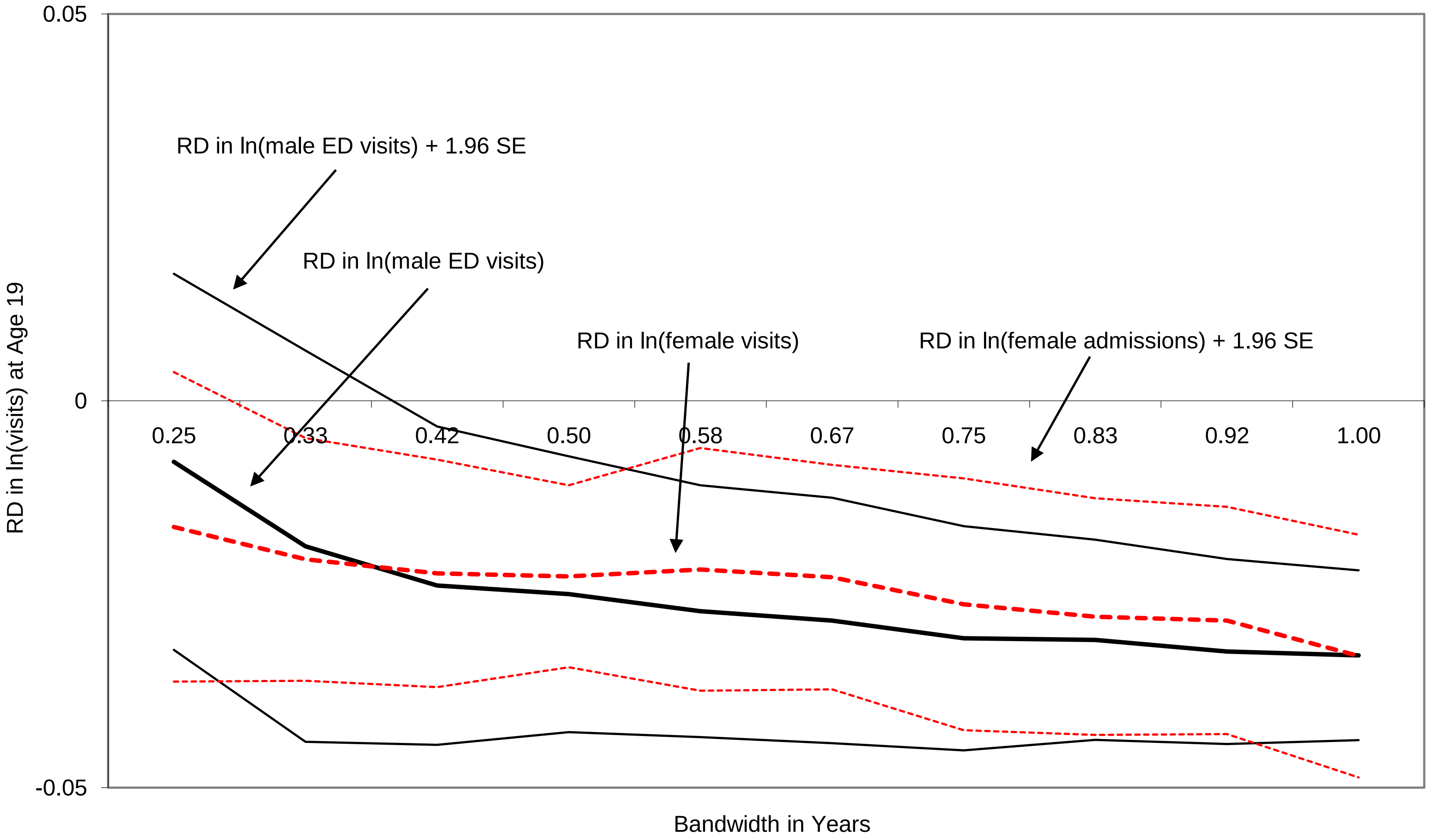
Appendix Figure 3: Age Profile of Proportion ED Visits Without Insurance Coverage Centered at Ages 19-26 and 65



Appendix Figure 4: Age Profile of Insurance and Emergency Department Visits
at Age 20 and Age 22

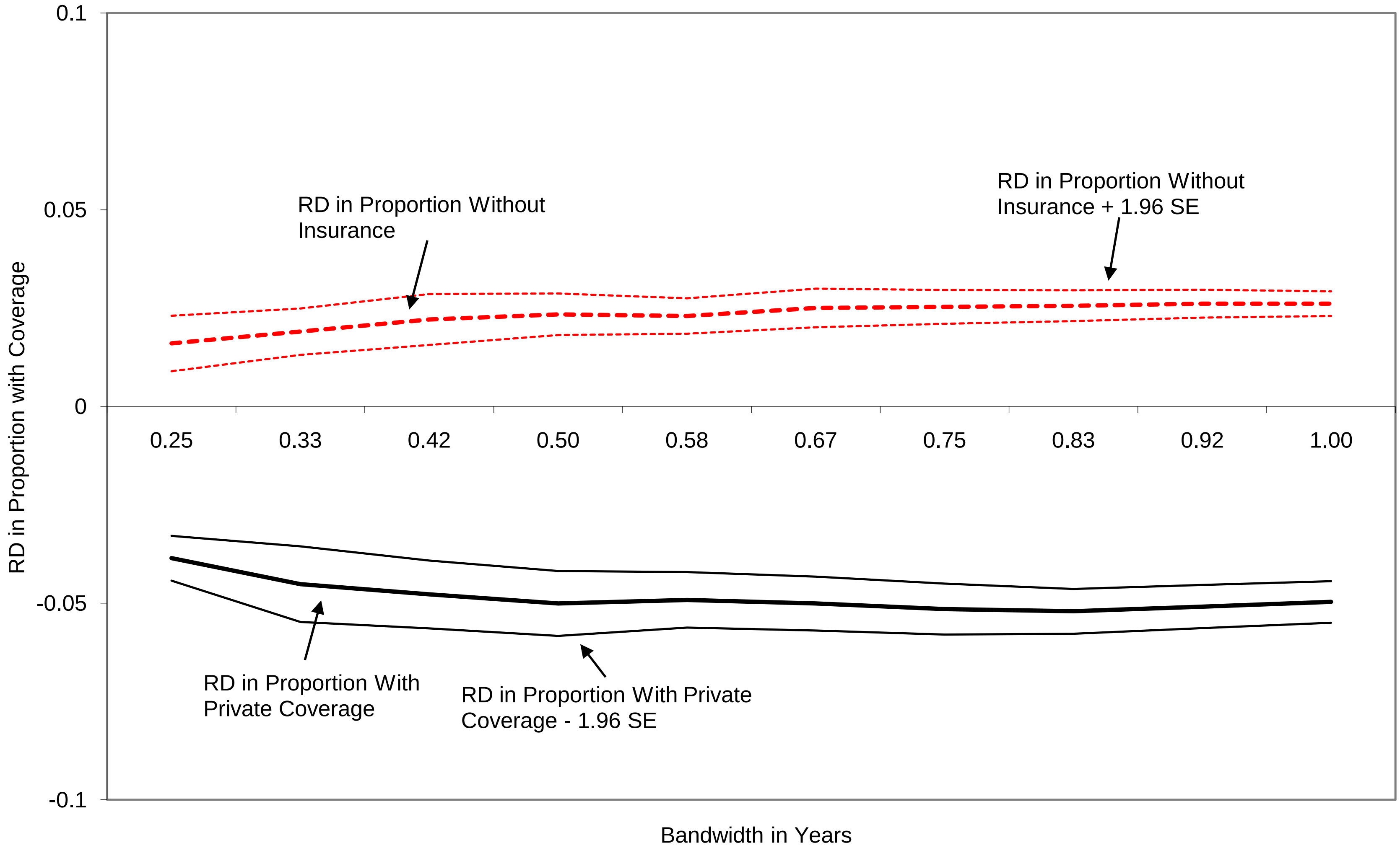


Appendix Figure 5: Assessing Sensitivity to Bandwidth Choice of Estimate of Change in Number of People Treated in the Emergency Department



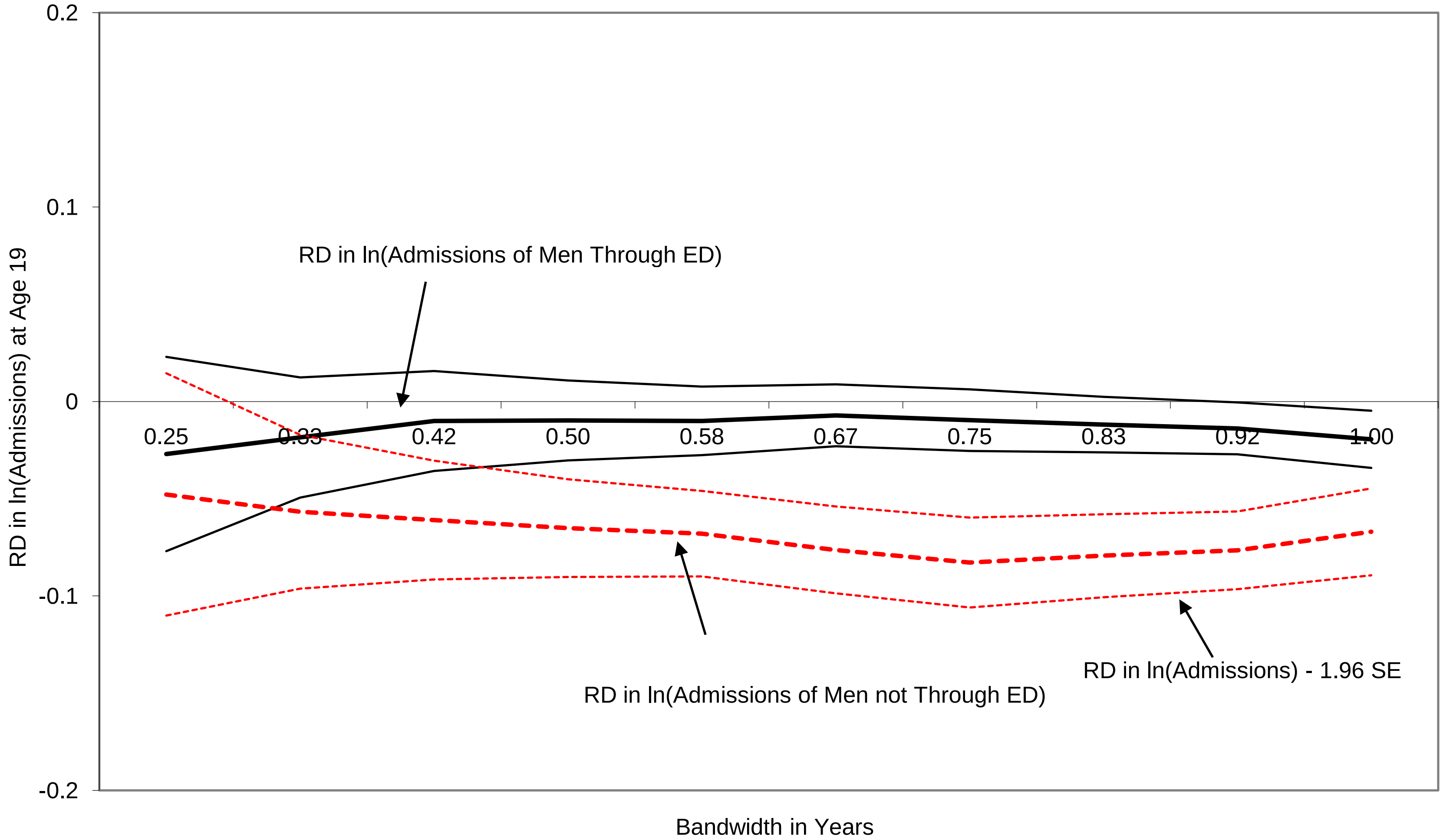
Notes: The estimates above are the discrete change at age 19 from a local linear regression with a symmetric bandwidth. The heavy line is the point estimate and the lighter lines are the confidence intervals.

Appendix Figure 6: Assessing Sensitivity to Bandwidth Choice of Estimate of Change in Insurance Among People Admitted to the Hospital



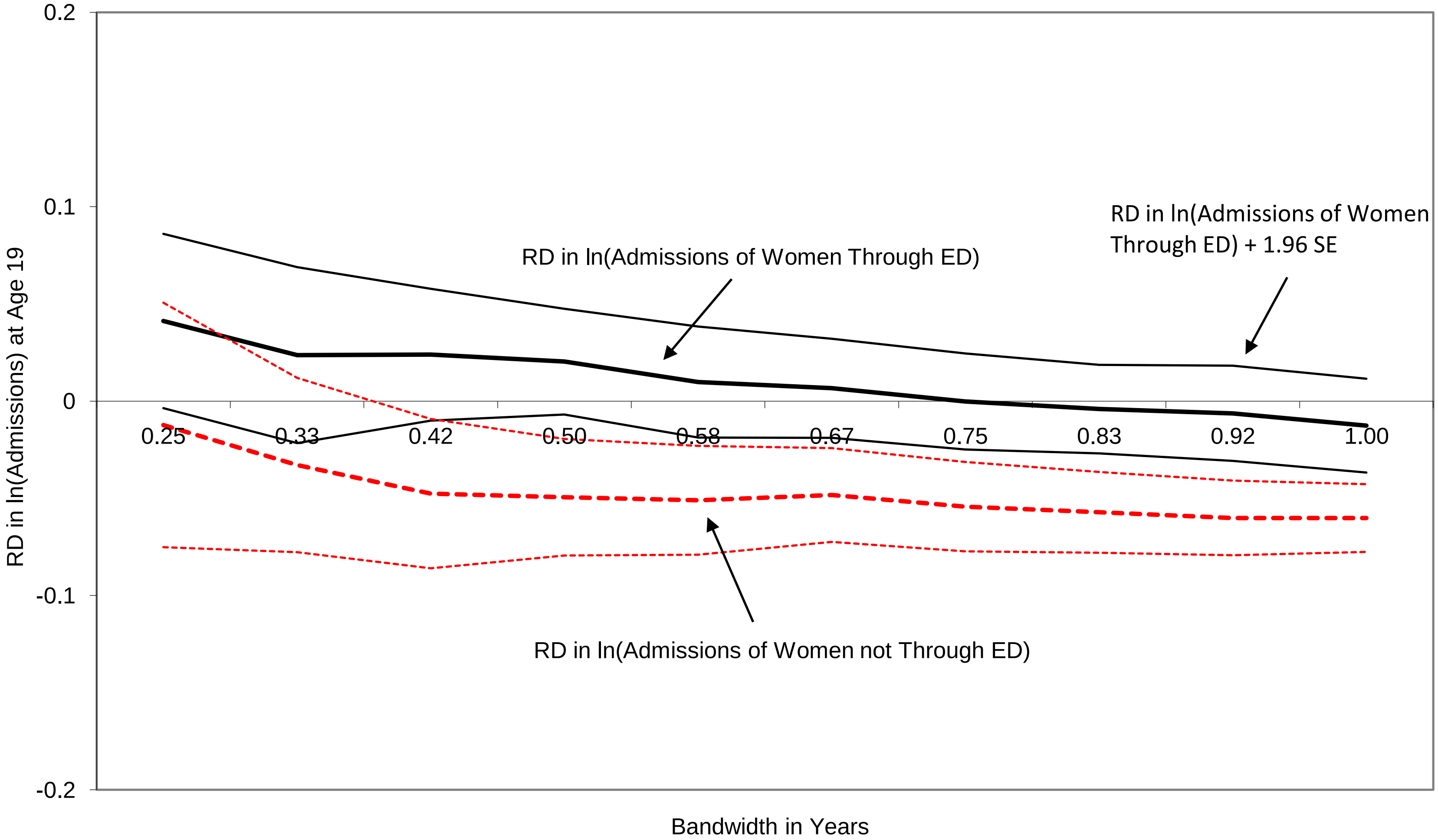
Notes: The estimates above are the discrete change at age 19 from a local linear regression with a symmetric bandwidth. The heavy line is the point estimate and the lighter lines are the confidence intervals. This figure only includes hospital stays that are not pregnancy related.

Appendix Figure 7: Assessing Sensitivity to Bandwidth Choice of the Estimate of the Change in Number of Men Admitted to the hospital at Age 19



Notes: The estimates above are the discrete change at age 19 from a local linear regression with a symmetric bandwidth. The heavy line is the point estimate and the lighter lines are the confidence intervals.

Appendix Figure 8: Assessing Sensitivity to Bandwidth Choice of the Estimate of the Change in Number of Women Admitted to the hospital at Age 19



Notes: The estimates above are the discrete change at age 19 from a local linear regression with a symmetric bandwidth. The heavy line is the point estimate and the lighter lines are the confidence intervals. Pregnant women are not included in the analysis.

Appendix Table 1: Change at Age 19 in Insurance Coverage and Emergency Department Visits by State

	Private	Uninsured	Medicaid	Change in ED Visits	Impact of Losing Insurance on ED Visits
	(1)	(2)	(3)	(4)	(5)
All States	-0.0629	0.0810	-0.0166	-0.0333	-0.4041
	[0.0026]	[0.0046]	[0.0025]	[0.0060]	[0.0776]
	0.4471	0.2154	0.2644		
Arizona	-0.0459	0.0536	-0.0035	-0.0286	-0.5258
	[0.0052]	[0.0103]	[0.0059]	[0.0128]	[0.2600]
	0.3480	0.2063	0.3580		
California	-0.0659	0.0760	-0.0059	-0.0368	-0.4755
	[0.0036]	[0.0057]	[0.0029]	[0.0074]	[0.1038]
	0.4073	0.2141	0.2944		
Iowa	-0.0514	0.0740	-0.0209	-0.0444	-0.5862
	[0.0072]	[0.0105]	[0.0059]	[0.0136]	[0.2024]
	0.4935	0.1818	0.2654		
New Jersey	-0.0722	0.0851	-0.0144	-0.0164	-0.1908
	[0.0038]	[0.0044]	[0.0029]	[0.0080]	[0.0945]
	0.5637	0.2751	0.1180		
Wisconsin	-0.0613	0.1331	-0.0799	-0.0464	-0.3407
	[0.0068]	[0.0100]	[0.0060]	[0.0126]	[0.0982]
	0.4937	0.1490	0.2872		

Notes: The Emergency Department visits used to estimate the regressions are a near census of ED visits in Arizona (2005-2007), California (2005-2007), Iowa (2004-2007), New Jersey (2004-2007) and Wisconsin (2004-2006). The parameter estimates in the columns 1-3 are the percentage point change in insurance coverage when people age out of their insurance coverage on the last day of the month in which they turn 19. The standard errors are in brackets directly below the parameter estimates. Below the SE we have included the estimated level of the dependent variable immediately before people age out. The parameter estimates are adjusted for the decline in visits under the assumption that the decline in visits is due entirely to people losing their insurance coverage. The adjustment is made by estimating the insurance coverage regression and the log(visits) regressions via seemingly unrelated regression. The dependent variable in column 4 is the log of visits at each age in months. The estimates in column 5 are the ratio of the change in visits to the overall change in insurance coverage.

Appendix Table 2: Change at 19 in Insurance Coverage and Hospital Admissions for People that are Not Pregnant

	Private	Uninsured	Medicaid	Change in Hospital Stays	Impact of Losing Insurance on Hospital Stays
	(1)	(2)	(3)	(4)	(5)
All	-0.0664	0.0579	0.0088	-0.0379	-0.6411
	[0.0036]	[0.0048]	[0.0026]	[0.0050]	[0.1018]
	0.5003	0.1174	0.3050		
Arizona	-0.0665	0.0615	0.0081	-0.0477	-0.7583
	[0.0113]	[0.0179]	[0.0110]	[0.0213]	[0.4137]
	0.4646	0.0782	0.3380		
California	-0.0702	0.0558	0.0141	-0.0409	-0.7179
	[0.0034]	[0.0052]	[0.0037]	[0.0069]	[0.1412]
	0.4925	0.1095	0.3180		
Iowa	-0.0047	-0.0117	0.0269	0.0504	-4.4170
	[0.0438]	[0.0473]	[0.0254]	[0.0542]	[18.001]
	0.6423	0.0805	0.2367		
New York	-0.0645	0.0438	0.0231	-0.0260	-0.5857
	[0.0060]	[0.0075]	[0.0052]	[0.0079]	[0.2070]
	0.5115	0.1274	0.3163		
Texas	-0.0522	0.1090	-0.0562	-0.0638	-0.5670
	[0.0087]	[0.0142]	[0.0062]	[0.0176]	[0.1785]
	0.4888	0.1553	0.2034		
Wisconsin	-0.0953	0.1078	-0.0385	-0.0399	-0.3631
	[0.0183]	[0.0238]	[0.0135]	[0.0256]	[0.2512]
	0.6089	0.0756	0.2471		

Notes: The estimates above are from a near census of hospital stays in Arizona (2000-2007), California (1990-2006), Iowa (2004-2007), New York (1990-2006), Texas (1999-2003) and Wisconsin (2004-2006). Combining the data from the six states gives a sample of 849,636 18 and 19 year olds. In columns 1-3 we present estimates of the change in insurance coverage (among people admitted to the hospital) that occurs on the first day of the month after people turn 19. Directly below the estimates are the standard errors of the estimates and below the standard errors are the proportion of the population with this type of coverage immediately before people age out at 19. The estimates are made using a linear polynomial in age for estimated using admissions among people age 18 to age 20. The estimates of the change in insurance are adjusted for the effect of insurance status on the probability of getting treated. In column 4 we present the change in ln(hospital stays) and in column 5 we present the impact of insurance coverage on hospital stays.

Appendix Table 3: Change at Age 23 in Insurance Coverage and Emergency Department Visits

	Private	Uninsured	Medicaid	ln(Visits)	Instrumental Variables Estimate
	(1)	(2)	(3)	(4)	(5)
All	-0.0154	0.0170	-0.0013	-0.0142	-0.8269
	[0.0018]	[0.0028]	[0.0013]	[0.0036]	[0.2517]
	0.3555	0.3111	0.2214		
All (Except Pregnant)	-0.0170	0.0195	-0.0023	-0.0167	-0.8496
	[0.0020]	[0.0028]	[0.0012]	[0.0037]	[0.2517]
	0.3651	0.3219	0.1952		
Male	-0.0195	0.0230	-0.0028	-0.0212	-0.9111
	[0.0031]	[0.0041]	[0.0013]	[0.0056]	[0.2517]
	0.3406	0.3995	0.1075		
Female	-0.0117	0.0117	-0.0004	-0.0082	-0.7010
	[0.0018]	[0.0034]	[0.0023]	[0.0055]	[0.2517]
	0.3640	0.2503	0.3048		
Female Not Pregnant	-0.0140	0.0150	-0.0016	-0.0115	-0.7625
	[0.0020]	[0.0031]	[0.0021]	[0.0053]	[0.2517]
	0.3833	0.2593	0.2710		

Notes: The Emergency Department visits used to estimate the regressions are a near census of ED visits in Arizona (2005-2007), California (2005-2007), Iowa (2004-2007), New Jersey (2004-2007) and Wisconsin (2004-2007).The parameter estimates in the table above are the percentage point change in insurance coverage when people age out of their insurance coverage on the last day of the month in which they turn 23. The standard errors are in brackets directly below the parameter estimates. Below the SE we have included the estimated level of the dependent variable immediately before people age out. The parameter estimates are adjusted for the decline in visits under the assumption that the decline in visits is due entirely to people losing their insurance coverage. The adjustment is made by estimating the insurance coverage regression and the ln(visits) regressions via seemingly unrelated regression.