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Three Essays on the Impact of the Affordable Care Act Expansion of Dependent Coverage for Young Adults

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ABSTRACT

THREE ESSAYS ON THE IMPACT OF THE AFFORDABLE CARE ACT EXPANSION OF DEPENDENT COVERAGE FOR YOUNG ADULTS

By

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To achieve the goal of universal coverage of health insurance for the Americans, in March 2010, the Patient Protection and Affordable Care Act (ACA) was signed into law. The ACA targets at providing help to improve access to affordable health coverage for everyone and protect consumers from abusive insurance company practices. One of the precedent mandates, implemented in September 2010, is to expand coverage on young adults of age 19 to 26, who may lose insurance coverage due to the remove from their parents' plan after age 18 and lacking of productivity to bargain with employers in the labor market.

This dissertation looks into the impact of the ACA health insurance coverage expansion for young adults on the subsequent health outcomes, health care utilization, and further social impact on traffic fatalities. Difference-in-differences models are used with different treatment groups and corresponding control groups. Chapter I uses survey data (BRFSS) to evaluate health care access, health behavior and self-assessed health status. The results suggest an improvement in health care access and self-assessed health but more risky behavior. Chapter II uses hospital discharge data (NIS) to estimate avoidable hospitalization in order to assess primary care utilization. The result shows that less primary care was consumed, which leads to more avoidable hospitalization but health may have been improved by using more hospital care. The results from both chapters imply potential ex ante moral hazard among young adults in the policy targeting age group. Thus, chapter III uses accident records data (FARS) to examine the impact of the health insurance expansion on traffic fatality for young adults, to see whether young drivers perform ex ante moral hazard through risky behavior like drunk and/or reckless driving after they get covered by the health insurance expansion policy. Primary result shows that there is an increase in traffic accidents and fatalities for those younger adults as a result of the ACA dependent coverage expansion.

THREE ESSAYS ON THE IMPACT OF THE AFFORDABLE CARE ACT EXPANSION OF DEPENDENT COVERAGE FOR YOUNG ADULTS

BY

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ACCEPTANCE

This dissertation was prepared under the direction of the candidate's Dissertation Committee. It has been approved and accepted by all members of that committee, and it has been accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Economics in the Andrew Young School of Policy Studies of Georgia State University.

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DEDICATION

To Dr. James H. Marton for his unwavering support and encouragement over the years.

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INTRODUCTION

To achieve the goal of universal coverage of health insurance for the Americans, in March 2010, the Patient Protection and Affordable Care Act (ACA) was signed into law. The ACA targets at providing help to improve access to affordable health coverage for everyone and protect consumers from abusive insurance company practices. One of the precedent mandates, implemented in September 2010, is to expand coverage on young adults of age 19 to 26, who may lose insurance coverage due to the remove from their parents' plan after age 18 and lacking of productivity to bargain with employers in the labor market. The expansion shows an increase of 3.1 million in coverage for young adults by December 2011.

This dissertation looks into the impact of the ACA health insurance coverage for young adults on the subsequent health outcomes, health care utilization, and further social impact on traffic fatalities. The first essay uses survey data to evaluate health care access, health behavior and self-assessed health status. The results suggest an improvement in health care access and self-assessed health but more risky behavior. The second essay uses hospital discharge data to estimate avoidable hospitalization in order to assess primary care utilization. The primary result shows that less primary care was consumed, which leads to more avoidable hospitalization but health may have been improved by using more hospital care. The results from both essays imply potential ex ante moral hazard among young adults in the policy targeting age group. Thus, the third essay uses accident records data to examine the impact of the health insurance expansion on traffic fatality for young adults, to see whether young drivers perform ex ante moral hazard through risky behavior like drunk and/or reckless driving after they get covered by the health insurance expansion policy. Primary result shows that there is an increase in traffic accidents and fatalities for those younger adults as a result of the ACA dependent coverage expansion.

CHAPTER I

Impacts of the ACA Dependent Coverage Provision on Health-Related Outcomes of Young Adults[1]

I. Introduction

The Patient Protection and Affordable Care Act (ACA) of March 2010 aimed to achieve nearly universal coverage in the United States through a combination of mandates, subsidies, Medicaid expansions, and health insurance exchanges (Gruber, 2011). Although the majority of the ACA's provisions just took effect in 2014, one important component of the law – a dependent coverage provision – was implemented on September 23rd, 2010. This provision allows dependents to remain on a parent's private health insurance plan until the start of the first plan year after they turn 26 years old. Previously, private insurers often dropped non-student dependents at age 19 and student dependents at age 23 (Anderson et al., 2012 and 2014).

Many states already had some form of dependent coverage mandate before the ACA, but the state laws are typically weaker. Most state laws have an age threshold below 26 or require additional criteria, such as being a full-time student, living with one's parents, or not being married. Moreover, state laws do not apply to self-funded benefit programs, and more than half of private sector workers with employer-provided health insurance are in self-funded plans (Monheit et al., 2011). Perhaps because of these limitations, Monheit et al. (2011) and Levine et al. (2011) find that state dependent coverage mandates only lead to small increases in dependent coverage that are offset by a decline in young adults holding their own policies. In contrast, the ACA provision applies to all young adults under age 26 and all private plans. It therefore has the

[1] This chapter is coauthored with Silvia Barbaresco and Charles Courtemanche. Reprinted from *Journal of Health Economics*, 40, Impacts of the Affordable Care Act Dependent Coverage Provision on Health-Related Outcomes of Young Adults, 54-68, Copyright (2015), with permission from Elsevier.

potential to dramatically affect young adults across the country, including in states with a pre-existing dependent coverage provision.

The ACA dependent coverage expansion provides a unique opportunity to study the impacts of a health insurance intervention specific to young adults, the age group with the highest uninsured rate (Levine et al., 2011). Prior to the ACA, the uninsured rate was 29% among individuals ages 18-24 and 27% among those 25-34, compared to 19% for 35-44 year olds and 14% for 45-64 year olds (DeNavas-Walt et al., 2010). Since any attempt to obtain universal coverage necessarily involves large coverage expansions among young adults, it is important to understand the effects of insurance on this group. It is unclear the extent to which results from other contexts – such as Medicaid, Medicare, or the Massachusetts health care reform of 2006 – are applicable. Young adults are generally healthier than the populations covered by these programs, and therefore may experience smaller gains from health insurance. Alternatively, young adults may be relatively poor and therefore respond strongly to reduced out-of-pocket costs of medical care.[2]

Given the short amount of time since its implementation, researchers are only beginning to study the impacts of the ACA dependent coverage provision. Cantor et al. (2012) and Sommers and Kronick (2012) show that the mandate increased health insurance coverage for young adults across all racial groups and regardless of employment status. Sommers et al. (2013) find that the provision increased insurance coverage among young adults, while reducing delays in getting care and care foregone because of cost. Akosa Antwi et al. (2013) again find an increase in insurance coverage, but they also present evidence of labor market consequences such as young adults shifting from full-time to part-time jobs. Akosa Antwi et al. (2014) show

[2] Aside from age, the ACA dependent coverage mandate is also a unique coverage expansion in that it represents an expansion of private rather than public insurance, and that, since it only affects those whose parents have insurance, the treated population may be of higher socioeconomic status than that of other interventions.

that the mandate increased young adults' utilization of inpatient care, particularly for mental illness. Chua and Sommers (2014) do not find any evidence that the provision affected health care use, but they do find a reduction in out-of-pocket medical expenses and increases in excellent self-reported physical and mental health.

These papers all share a common general research design: comparing changes in outcomes among the treated age range 19-25 to those of other young adults. The age range used for the control group varies across these studies, with some including individuals up to 34 years old (Sommers and Kronick, 2012; Sommers et al., 2013; Chua and Sommers, 2014). Slusky (2013) questions the validity of this approach, arguing that different age groups are often subject to different economic shocks. He runs placebo tests using data from before the mandate and artificial "treatment" dates, finding that the same specification estimates significant "effects" more often than could be attributed to chance. He suggests narrowing the age bandwidths of the treatment and control groups as a possible solution.

We contribute to this literature on the ACA dependent coverage provision in four ways. First, we consider a number of new outcomes. Using data from the Behavioral Risk Factor Surveillance System (BRFSS), we investigate 18 outcomes related to health care access, utilization of preventive care, risky health behaviors, and self-assessed health. The health care access measures include having insurance, a primary care doctor, and any foregone care because of cost. Our preventive care measures are dummies for recent flu vaccinations, well-patient checkups, and pap tests. The health behavior outcomes reflect smoking, drinking, body mass index, exercise, and pregnancy. The self-assessed health variables relate to overall, mental, and physical health as well as health-related functional limitations. Of these outcomes, only insurance coverage, foregone care because of cost, and self-assessed physical and mental health are studied in other papers in the literature. To our knowledge we are the first to investigate the ACA dependent coverage provision's impact on preventive care or health behaviors. Moreover, although Chua and Sommers (2014) examine self-assessed physical and mental health, their measures and ours are meaningfully different. They use dummies for self-reporting excellent physical and mental health, so their estimates only capture changes at the upper end of the health distribution. In contrast, we utilize five measures that should together capture changes at various parts of the distribution. A dummy for excellent overall health reflects the high end, a dummy for very good or excellent health reflects a somewhat lower portion, and three more severe outcomes – number of days of the past 30 not in good physical health, not in good mental health, and with health-related limitations – reflect an even lower portion. This distinction will prove critical to the results.

Our second contribution is to push further than prior studies toward addressing the methodological concerns raised by Slusky (2013), both by using narrow age ranges for the treatment and control groups and by validating these selections through placebo testing. Our treatment group consists of individuals ages 23-25, slightly below the dependent coverage provision's age cutoff, and our control group consists of those slightly above the cutoff at ages 27-29. We run placebo tests checking for "effects" of artificial interventions in the pre-treatment period. Our classifications perform well in the placebo tests, whereas the wider age ranges commonly used in the literature prove more problematic.

Another contribution is that we use over three full years of post-treatment data (2011 through 2013, plus a few months after implementation at the end of 2010). To our knowledge, none of the prior papers in the ACA dependent coverage provision literature have used more than one full year of post-treatment data, which leaves the estimates susceptible to confounding from temporary age-specific shocks and fluctuations. If estimated effects persist with three years of post-treatment data, we can be more confident that they are not driven by transitory movements in unobserved characteristics.

Finally, we contribute to the literature by testing for heterogeneous effects. Of the outcomes included in our paper, heterogeneity in the effects of the ACA dependent coverage provision has only previously been evaluated for insurance coverage (Akosa Antwi et al., 2013; Sommers et al., 2013) and cost being a barrier to care (Sommers et al., 2013). We will find important heterogeneous effects on other outcomes as well, such as self-assessed health. Moreover, although Akosa Antwi et al. (2013) and Sommers et al. (2013) evaluate whether effects differ by certain demographic characteristics, neither paper tests for heterogeneous effects by socioeconomic status.[3] We will find that the effects of the dependent coverage provision vary considerably by education level.

Our difference-in-differences results from the full sample suggest that the ACA dependent coverage provision improved health care access for young adults, had little effect on preventive care use, had mixed effects on risky health behaviors, and improved self-assessed health at the high end of the distribution. Specifically, we document improvements in four of the eighteen outcomes: health insurance coverage, access to a primary care doctor, excellent self-assessed health, and body mass index. However, we find evidence of an increase in risky drinking, and no clear effects in either direction on the remaining thirteen outcomes.

We evaluate heterogeneity in the effects of the mandate through subsample analyses, finding the greatest improvements in outcomes for men and college graduates. The increase in

[3] Sommers et al. (2013) note that testing for heterogeneity by educational attainment is difficult because many individuals in their treatment group – 19 to 25 year olds – are still in the process of completing their education. Another advantage of using a narrow age range for the treatment group – 23 to 25 year olds – is that excluding the prime college ages largely ameliorates this concern.

health insurance coverage was greater for men than women, and only men experienced statistically significant gains in any outcomes beyond health insurance: primary care access, exercise, and overall self-assessed health. Stratifying by education reveals that the insurance expansions were similar for college graduates and non-college graduates. However, only college graduates experienced significant gains in any other outcomes besides insurance – specifically, primary care access, cost being a barrier to care, body mass index (BMI), obesity, and overall self-assessed health. Young adults with different education levels therefore appear to respond differently to exogenously obtaining health insurance.

II. Health Insurance and Health-Related Outcomes

The most obvious theoretical implication of health insurance is that by lowering the effective price of health care, health insurance should increase its utilization. However, increased health care utilization does not necessarily improve health. Diminishing marginal returns suggest that health care can only improve health up to a certain level (e.g. Grossman, 1972). Whether the additional consumption of medical care induced by insurance generates substantial gains in health therefore depends on the initial level of health capital. Since the uninsured can often obtain essential needs by paying directly or receiving charity care, these individuals need not have low baseline levels of health. Moreover, the marginal returns to health care differ for different outcomes. Risky health behaviors such as smoking, excessive drinking, and overeating might be particularly difficult to improve through health care, as they require lifestyle changes. Medical professionals' ability to influence health behaviors is generally limited to providing accountability, information, strategies, and sometimes drugs to make behavioral changes easier.

Another relevant issue when evaluating the impact of health insurance on health is that obtaining insurance could induce individuals to take more health risks, since the provision of health insurance decreases the financial losses associated with sickness. This concept is known as *ex ante* moral hazard (Ehrlick and Becker, 1972). Theoretically, *ex ante* moral hazard could both increase risky behaviors and reduce investments in preventive care.

Finally, exogenous provision of health insurance could lead to income effects for individuals who used to purchase their own insurance policy but now are able to receive free or subsidized coverage, or for the newly-insured if their out-of-pocket medical expenses drop. The available evidence from natural experiments suggests that additional income increases health care utilization (Acemoglu et al., 2013), either increases BMI or has no effect (Lindahl, 2005; Schmeiser, 2009; Cawley et al., 2010), increases smoking along the intensive but not extensive margin (Apouey and Clark, 2014), and increases drinking (Apouey and Clark, 2014). The income effect may therefore improve health via medical care but worsen health via risky behaviors. Accordingly, evidence of income's causal effect on overall health is mixed, with Lindahl (2005) and Frijters et al. (2005) finding that it improves self-assessed health, Apouey and Clark (2014) finding that it improves mental health but not overall health, and Snyder and Evans (2006) showing that it raises mortality risk among seniors.

In sum, the effects of insurance on preventive health care utilization, risky health behaviors, and overall health status are theoretically ambiguous. Insurance may improve these outcomes through direct price effects, worsen them through *ex ante* moral hazard, or affect them in either direction through income effects. The net effects could differ for different outcomes. For instance, direct price effects might dominate for primary care utilization but moral hazard might dominate for risky behaviors. Empirical analysis is necessary to resolve this ambiguity.

Causally interpretable evidence generally confirms the prediction that insurance increases health care utilization for U.S. adults. Manning et al. (1987) analyzed the randomized RAND

Health Insurance Experiment, finding that lower copayments increased doctor visits. Medicaid and Medicare expansions have been shown to increase utilization of primary and hospital care (Currie and Gruber, 1996a; Finkelstein et al., 2012; Taubman et al., 2014; Lichtenberg, 2002; Card et al., 2008). Other evidence suggests that the Massachusetts universal coverage initiative of 2006 increased preventive services while reducing emergency room utilization, avoidable hospitalizations, and medical needs unmet because of cost (Miller, 2011; Kolstad and Kowalski, 2012; Miller, 2012; Van der Wees et al., 2013). More directly relevant to our study population, Anderson et al. (2012 and 2014) exploit the sharp drops in coverage on parents' insurance at ages 19 and 23 to show that losing coverage reduced young adults' emergency room and hospital visits. Finally, as mentioned previously, Akosa Antwi et al. (2014) show that the ACA dependent coverage provision increased hospital admissions, although Chua and Sommers (2014) find no significant effects on survey measures of hospital, primary care, or prescription drug utilization.

The evidence of health insurance's effect on health is mixed. The RAND experiment only found that better insurance coverage improved health for certain subgroups (Brook et al., 1983). Medicaid expansions increase self-reported overall, physical, and mental health and reduce mortality, but have no statistically detectable effects on laboratory-measured health outcomes (Currie and Gruber, 1996b; Finkelstein et al., 2012; Sommers et al., 2012; Baicker et al., 2013). Card et al. (2009) find a reduction in the mortality rate among recently hospitalized Medicare recipients, but Finkelstein and McKnight (2008) find no significant effect of Medicare on the mortality rate of seniors in general. Evidence suggests that the Massachusetts reform improved self-assessed overall, physical, and mental health, while decreasing functional limitations, joint disorders, and mortality (Van der Wees et al., 2013; Courtemanche and Zapata, 2014; Sommers et al., 2014). As mentioned previously, Chua and Sommers (2014) find that the ACA dependent provision increased the probabilities of self-reporting excellent physical and mental health.

Evidence on the causal effects of health insurance on risky health behaviors is also mixed. Brook et al. (1983) find no evidence that insurance affected smoking or body weight in the RAND experiment. Dave and Kaestner (2009) report that Medicare decreased physical activity while increasing smoking and drinking. Finkelstein et al. (2012) do not find any significant impacts of Medicaid on smoking or BMI. Courtemanche and Zapata (2014) find that the Massachusetts reform reduced body mass index and did not affect smoking or physical activity.

In sum, there is little prior evidence on the effects of health insurance on young adults' access to care, preventive care utilization, risky health behaviors, or health. Given the theoretical ambiguities and variation in empirical findings discussed above, we cannot assume prior results from other contexts such as Medicaid and Medicare generalize. For instance, young adults' relatively high baseline levels of health might lead them to have relatively inelastic demand for health care or a low marginal effect of health care on health. On the other hand, young adults' demand for health care could be relatively elastic given their generally low income and wealth levels. Moreover, one might expect young adults to be the most susceptible to *ex ante* moral hazard since this is often the life stage in which opportunities to engage in particular risky behaviors (e.g. binge drinking) are introduced.

III. Data

Our main data source is the BRFSS, a telephone survey conducted by state health departments in conjunction with the U.S. Centers for Disease Control and Prevention to collect information on health and health behaviors. The survey is conducted monthly through a random digit dialing method that selects a representative sample of respondents from the noninstitutionalized population of adults at least 18 years old. The BRFSS provides several advantages for our analyses. First, it contains a wide range of appropriate outcome variables. Second, it includes demographic characteristics as well as state, month, and year identifiers that allow us to construct the treatment variable and jointly control for many different factors. Next, it contains a much larger number of observations than other datasets with the necessary variables. Finally, the BRFSS includes a number of pre-treatment waves that allow for detailed testing of differential trends in the outcomes between treatment and control groups.

Our primary analysis sample consists of the 2007-2013 waves, which include the year the ACA dependent coverage mandate took effect plus three years on both sides. One reason we exclude the years before 2007 is to limit our sample to years of relatively poor economic performance. This reduces the possibility of confounding from differential impacts of macroeconomic shocks on the health-related outcomes of different age groups. However, robustness checks and placebo tests will utilize data as far back as 2001. We do not use any waves before 2001 because the BRFSS made major changes to the survey in that year. Many of the questions used to construct our outcome variables are either not available in earlier years or differ in non-trivial ways.

Most of our analyses use ages 23-25 as the treatment group and ages 27-29 as the control group. Following much of the prior literature, 26 year olds are excluded because their treatment status is ambiguous: they may still be covered by the ACA mandate depending on their birthdate and the start date of their parents' insurance plan year (Akosa Antwi et al., 2013). Although the prior literature uses 19-25 as the treatment group, we prefer 23-25 for two reasons.[4] First, prior to the ACA, insurers most commonly dropped non-student dependents from parents' plans at age

[4] Studies in the literature utilize somewhat different control groups. Cantor et al. (2012) use 27-30 year olds; Sommers and Kronick (2012), Sommers et al. (2013), and Chua and Sommers (2014) use 26-34 year olds; Akosa Antwi et al. (2013) use 16-18 and 27-29 year olds; and Akosa Antwi et al. (2014) use 27-29 year olds.

19, but most commonly dropped student dependents at age 23. Excluding 19-22 year olds therefore results in a "cleaner" treatment group, i.e. a higher proportion of the treatment group actually being affected by the treatment. Accordingly, Akosa Antwi et al. (2014) show that the ACA dependent coverage provision's impact on having insurance was more than twice as large for 23-25 year olds as for 19-22 year olds. Second, Slusky (2013) shows that the models from prior papers with ages 19-25 as the treatment group lead to poor placebo test results for insurance and labor market outcomes. He suggests narrowing the age bandwidth as a potential solution. Indeed, we will show that wider age ranges lead to problematic placebo test results for our outcomes as well, and that our narrower age range performs better.

We utilize eighteen different health-related dependent variables. The first three relate to health care access: dummy variables reflecting whether the respondent has any health insurance, has a primary care physician, and had any medical care needed but not obtained because of cost in the previous year. Unfortunately, the BRFSS does not include more detailed questions on health insurance, such as the source of coverage. The next three outcomes – dummies for having a flu vaccination (shot or spray), a well-patient doctor check-up visit (e.g. physical), and a pap test (for women) in the previous year – reflect preventive care utilization.[5] The next category of variables relates to risky health behaviors: a dummy for whether the individual currently smokes, number of alcoholic drinks in the past 30 days, a dummy for being a risky drinker (more than 30 drinks total or at least one occasion with four or more drinks for women, more than 60 drinks total or at least one occasion with five or more drinks for men),[6] body mass index (BMI=weight

[5] Other preventive care variables typically studied in the literature, such as mammograms and prostate exams, are not relevant for our study population of young adults.
[6] The dummy for risky drinker is created to come as close as the BRFSS data will allow to the National Institute on Alcohol Abuse and Alcoholism's definition of at-risk drinking: more than 7 drinks per week total or at least one occasion with three or more drinks for women, and more than 14 drinks per week total or at least one occasion with four or more drinks for men. See http://pubs.niaaa.nih.gov/publications/womensfact/womensfact.htm.

in kg/height in m^2),[7] a dummy for obese (BMI≥30), a dummy for whether an unmarried female respondent is pregnant (the only proxy for risky sexual activity available in the BRFSS), and a dummy for obtaining any recreational exercise in the past 30 days.[8] Finally, we include several variables related to self-assessed health status: a dummy for whether overall health is very good or excellent, a dummy for whether overall health is excellent, and days of the last 30 not in good mental health, not in good physical health, and with health-related functional limitations. Although self-assessed health is subjective, research has repeatedly found it to be correlated with objective measures of health such as mortality (e.g. Idler and Benyamini, 1997; DeSalvo et al., 2006; Phillips, Der, and Carroll, 2010). Self-assessed health is also a global measure of health that captures the full range of possible diseases and limitations (Idler and Benyamini, 1997).[9]

We also utilize a wide array of control variables. These include dummy variables for each year of age, gender, race/ethnicity, marital status, education, household income category, number of children in the household, whether the respondent reports her primary occupation as student, and whether the respondent is unemployed. Additionally, we control for monthly state unemployment rate, obtained from the Bureau of Labor Statistics. As mentioned previously, we are concerned about different impacts of the recession on different age groups, so controlling for several variables related to economic conditions at both the individual and aggregate levels could potentially be important. We also control for whether the respondent's state had any dependent coverage mandate covering her age*marital status*student status group in the survey year based

[7] Body mass index is based on self-reported height and weight, which are prone to measurement error (Cawley, 2004). Researchers have repeatedly found that this measurement error does not affect the signs and significance of regression estimates with BMI as a dependent variable, though it may slightly attenuate the magnitude of the estimates (e.g. Lakdawalla et al., 2002; Courtemanche et al., 2014; Courtemanche et al., forthcoming).

[8] Unfortunately, the more detailed BRFSS questions on physical activity are only available in odd numbered survey years and changed dramatically in 2011, so they are not useful for our analyses.

[9] Moreover, other commonly-used measures of health are not practical in our context. Mortality rates are likely too low among young adults to estimate effects of coverage expansions with meaningful precision, while measures of avoidable hospitalizations confound insurance's impact on health with the reduction in effective prices.

on information from the National Conference of State Legislatures (2010).[10] Additionally, in the flu vaccination regressions we control for interactions of the age fixed effects with the number of positive influenza tests in the country during the particular flu season (a proxy for severity of the flu season). Flu seasons in the post-treatment years were much more severe than those in the pre-treatment years, so adding these interactions prevents the estimates from being confounded by differential responses to flu season severity by young adults of different ages.[11]

Finally, we include a dummy for whether the respondent is part of a "cell phone only" component of the sample, added in 2011 (this variable is 0 for all respondents before 2011). The fact that individuals who only used cell phones were not explicitly included in the sample until 2011 raises the question of whether our sample makeup meaningfully changed at about the same time the post-treatment period began. To address this issue, we not only control for "cell phone only" users but also utilize the BRFSS sampling weights in all analyses. We found that these weights eliminate any sharp changes in sample demographic characteristics in 2011. Additionally, this issue would only bias our regression estimates if the relationship between the outcomes of landline and cell phone users is different among 23-25 year olds than among 27-29 year olds, and in a way that is not captured by the controls. It is not obvious why this would be the case. Accordingly, we have verified (results available upon request) that dropping individuals who only use cell phones from our sample has very little effect on the coefficient estimates, though it does generally increase the standard errors due to the reduced sample size.

[10] Note that not everyone coded as a 1 for state mandate is actually "treated" by such a mandate. Additional qualifiers beyond age, student status, and marital status exist in some states, while young adults whose parents' employers self-insure are also not covered by state mandates.

[11] Specifically, for the pre-treatment years 2007, 2008, and 2009, there were 23,753, 39,827, and 27,682 positive influenza test results in the corresponding flu seasons 2006-2007, 2007-2008, and 2008-2009. For the post- or during-treatment years 2010, 2011, 2012, and 2013, there were 157,449, 55,403, 27,012, and 75,342 number of influenza test results in the corresponding flu seasons 2009-2010, 2010-2011, 2011-2012, and 2012-2013 (CDC, 2014). The large 2009-2010 flu season number largely reflects the swine flu pandemic, but two of the three subsequent seasons were still relatively strong. Our results suggest that younger young adults respond more strongly to flu season severity than older young adults; therefore, omitting these interactions would lead to biased estimates.

After excluding observations with missing data for any of the control variables, Table 1.1 reports the sample sizes for the regressions for each dependent variable, along with the numbers of individuals in the treatment and control groups. The sample sizes differ slightly across dependent variables for two reasons. First, each health-related variable is missing for a different number of respondents. Second, the health-related variables have different "reflection periods;" some apply to the present (e.g. current smoker), while others refer to a 30-day period (e.g. number of alcoholic drinks in the past 30 days) and others to a one-year period (e.g. any well-patient doctor visit in the past year). We are concerned that short-run estimates would be misleading for variables with a long reflection period.[12] We therefore drop respondents surveyed during this period of ambiguity; e.g. for well-patient doctor visit in the past year we drop October 2010 through September 2011, while for drinks in the past 30 days we drop only October 2010.[13]

Table 1.2 lists the control variables and compares the pre-treatment (January 2007 through September 2010) summary statistics of the treatment and control groups. Individuals in the treatment group are less likely to be married, have a college degree, earn a high income, and have children in the household, and they are more likely to be students or employed.

Table 1.3 reports the pre- and post-treatment sample means of the outcome variables for the treatment and control groups, and calculates the simple difference-in-difference of means. Prior to the ACA dependent coverage provision, the uninsured rate was higher for young adults in the treatment group than those in the control group. The treatment group had lower rates of health care utilization and health care access than the control group; higher drinking and

[12] For example, suppose a respondent is surveyed in November 2010, the second month of the post-implementation period. The respondent would be classified as post-treatment, but her answer about well-patient doctor visits in the past year would reflect only two months of the post-treatment period and ten months of the pre-treatment period.

[13] For flu vaccinations in the past year, we only drop October 2010 through December 2010, as opposed to dropping a full year. We feel a shorter reflection period is appropriate in this case because flu vaccinations are typically administered in the fall. For instance, if someone surveyed in March 2011 reports being vaccinated in the past year, that vaccine almost certainly occurred during the post-treatment period (October 2010 or later).

unmarried pregnancy rates but healthier levels of risky drinking, BMI, obesity, and exercise; and broadly similar levels of smoking and self-assessed health. Comparing changes in the post- and pre-treatment means for the treatment and control groups, the difference-in-differences are positive and significant for any insurance, primary care doctor, excellent health, and risky drinker; negative and significant for body mass index and obesity; and insignificant for the other outcomes – including all those in the preventive care category.

Simple difference-in-differences estimates account for fixed differences in unobservable characteristics between the treatment and control group, but are still susceptible to bias from time-varying observables and unobservables. Figures 1-3 show that at a first glance the pre-ACA trends for the treatment and control groups appear generally similar for most outcomes, providing preliminary evidence that changes over time in observables and unobservables may not be substantially different for 23-25 year olds and 27-29 year olds. We next turn to regression analyses that adjust for changes in observables. Later, we will also conduct more formal tests of the assumption of common trends in unobservables.

IV. Average Effects of the ACA Dependent Coverage Mandate

A. Baseline Model

We estimate the effects of the ACA dependent coverage provision on the eighteen health-related outcomes using reduced-form difference-in-differences regressions. While it is tempting to estimate instrumental variables models using the mandate as an instrument for having insurance coverage, we are not confident that the exclusion restriction would hold in such models because there are several other mechanisms through which the mandate could affect health-related outcomes besides the extensive margin of health insurance coverage. Other possible mechanisms include the intensive margin of coverage (switching from high deductible catastrophic coverage to more comprehensive coverage), income effects, and peer effects.

Our baseline regression is of the form

$$Y_{igst} = \beta_0 + \beta_1(Treat_g * Post_t) + \boldsymbol{\beta_2 X'_{igst}} + \alpha_g + \varphi_t + \sigma_s + \varepsilon_{igst} \quad (1)$$

where Y_{igst} is the health-related outcome for individual i of age g living in state s in time t, expressed in a month/year combination.[14] $Treat_g$ is a dummy variable for whether age g is in the treated age range 23-25 as opposed to the control age range 27-29. $Post_t$ indicates whether period t is after the implementation of the provision (October 2010 or later). β_1 is the difference-in-differences coefficient and it captures the difference between the effects of the mandate on the treatment and control groups. $\boldsymbol{X'_{igst}}$ is a vector of the aforementioned control variables for sex, race, marital status, education, income, children, cell phone survey, student status, individual and state unemployment, and state dependent coverage mandate. We also include fixed effects for each year of age, month/year of time (e.g. January of 2007), and state, denoted by α_g, φ_t, and σ_s, respectively. ε_{igst} is the error term.[15] We do not separately include $Treat_g$ and $Post_t$ in the model because $Treat_g$ is perfectly collinear with the age fixed effects while $Post_t$ is perfectly collinear with the month/year fixed effects.

We report heteroskedasticity-robust standard errors clustered at the level of treatment: age. Following convention when there are a small number of clusters (six in our case), for hypothesis testing we use a t-distribution with degrees of freedom equal to the number of clusters minus one. The critical values used in our hypothesis tests are therefore considerably more

[14] Even though most of our outcomes are binary or non-negative count, we estimate linear models because they typically give reliable estimates of average effects (Angrist and Pischke, 2008). In unreported regressions (available upon request), we verify that the average treatment effects are very similar using probit regressions for the binary outcomes and negative binomial regressions for the count outcomes.
[15] In unreported regressions (available upon request) we have verified the results remain virtually identical if we replace the state fixed effects with fixed effects for each state-by-year combination.

stringent than those using the standard normal distribution. It is possible that even using stringent critical values might not be sufficient to eliminate the tendency to over-reject when the number of clusters is small (Cameron et al., 2008). However, the placebo tests in the next section will reject the null hypothesis even fewer than the expected number of times, suggesting that our hypothesis tests are sufficiently conservative. One of our robustness checks will also address this issue.

The key identifying assumption in a difference-in-differences model is common counterfactual trends between the treatment and control groups; i.e. in the absence of the intervention the treatment and control groups would have experienced the same changes in outcomes. Slusky (2013) argues that this assumption is problematic when studying the impact of the ACA dependent coverage provision on labor market-related outcomes (e.g. sources of health insurance coverage, employment status, and work hours) since cyclical fluctuations in the economy have different effects on different age groups. Since economic fluctuations are related to health,[16] Slusky's concern could also apply to health-related outcomes. As discussed previously, this is one of our main reasons for using narrow age bandwidths of 23-25 and 27-29.

B. Robustness Checks

We also estimate several variations of (1) as robustness checks. First, we run regressions including only the demographic controls (the sex, age, race, children, and marital status dummies) and fixed effects, excluding the economic controls since they may be endogenous to the dependent coverage provision. Obtaining access to parents' insurance could potentially influence a young adult's decisions about employment and education, which would then affect income.

[16] Research generally shows that recessions are associated with improvements in health and health behaviors (e.g. Ruhm, 2000, 2002, 2005), although recent evidence suggests that the countercyclical nature of health observed in prior recessions may not have been present during our sample period (Ruhm, 2013; Tekin et al., 2013).

Including covariates related to employment, education, and income might therefore "control away" part of the causal effect of the policy.

Our next several robustness checks vary the time period included in the sample. In order to verify that the results are not driven by our chosen length of the pre-treatment period, we consider two alternatives: starting the sample in 2004 and 2001. Additionally, we run regressions dropping March 2010 through December 2010, as these months are somewhat ambiguous with respect to their treatment status. We drop March-September because the ACA was passed in March, so some insurance plans may have complied preemptively prior to the dependent coverage provision's official implementation in September. We drop October-December because, even though the mandate was implemented in September, insurers did not have to comply until the start of the next plan year, which is often January.[17]

Our final robustness check addresses the potential concern that standard errors may be understated because of autocorrelation given the small number of clusters. We collapse the data into one observation for each year of age in the pre-treatment period and one observation for each year of age in the post-treatment period, for a total of twelve observations. We then estimate

$$\bar{Y}_{gt} = \gamma_0 + \gamma_1 (Treat_g * Post_t) + \gamma_2 Treat_g + \gamma_3 Post_t + \gamma_4 \bar{X}_{gt} + \varepsilon_{gt} \qquad (2)$$

where the lines above variables indicate averages across all individuals of age g in time period (pre- or post-treatment) t, weighted by the individual BRFSS sampling weights. Since the small sample size prevents all the control variables from being separately included, $\bar{X}$ is a single variable that summarizes the influence of all the controls. $\bar{X}$ is computed by regressing outcome Y on the controls using the individual-level pre-treatment data, then predicting Y for the whole sample based on the coefficient estimates, then aggregating in the same manner described above.

[17] Akosa Antwi et al. (2013) include two treatment variables to separately model the effects of the mandate during the implementation period and after full implementation. We have considered this specification in unreported regressions and the estimated post-implementation effects remain very similar.

C. Results

Table 1.4 presents the results for the baseline model and robustness checks. In addition to reporting estimated treatment effects and standard errors, for the baseline regressions we also report (in brackets) the treatment effects expressed in standard deviations of the dependent variables to provide some comparability of effect sizes across the different outcomes.

The results suggest sizeable improvements in health care access along at least some dimensions. We estimate that the ACA dependent coverage provision statistically significantly increased the insurance coverage rate of 23-25 year olds by between 5.5-6.7 percentage points, depending on the model. This is somewhat larger than the around 3-5 percentage point increase estimated by previous studies that use the broader treated age range of 19-25 (Cantor et al., 2012; Sommers and Kronick, 2012; Akosa Antwi et al., 2013; Sommers et al., 2013).[18] Additionally, the mandate increased the probability of having a primary care doctor by 2.0-3.4 percentage points and decreased the probability of having any care needed but foregone because of cost by 1.6-2.3 percentage points. The effect on primary care doctor access is statistically significant in all specifications, but the effect on care foregone because of cost is never significant.

Despite this improved access, we do not find any evidence of increased preventive care utilization. We estimate a total of eighteen models across the three preventive care measures, and none of these models reveal a statistically significant positive effect of the dependent coverage provision. The estimated effects on flu vaccinations and pap tests are negative in most specifications and occasionally statistically significant. The estimates for well-patient checkup are all positive but never significant.

[18] This discrepancy is consistent with Akosa Antwi et al.'s (2014) finding that the mandate's impact on the probability of having any coverage was around twice as large for 23-25 year olds than 19-22 year olds (4 compared to 2 percentage points). Alternatively, estimates using the treated age range 19-25 could be biased downward given the problems documented in our placebo tests and those of Slusky (2013).

We find mixed evidence regarding the dependent coverage provision's impacts on risky health behaviors. No significant estimates are observed for smoking, pregnancy, or alcoholic drinks per month. However, the mandate statistically significantly increased the probability of risky drinking (excessive drinks per month or any binge drinking) in all specifications, with magnitudes ranging from 0.8-1.4 percentage points. The dependent coverage expansion therefore appears to affect drinking at only the high end of the distribution, which is consistent with an *ex ante* moral hazard explanation since mild to moderate drinking generally does not increase the need for medical services. In contrast, the dependent coverage provision appears to improve weight-related behaviors. The mandate reduces BMI in all six specifications, with magnitudes ranging from -0.098 to -0.175. All but one of the six estimates for BMI are significant, with the remaining one being nearly significant. The effect on obesity is also negative in all six models, though it is only significant in three. The effect on probability of having any exercise is positive in all specifications but only significant in one. It is possible that our inability to measure exercise in greater detail – e.g. calories burned per day from physical activity – prevents the emergence of further significant results. It is also possible that the reduction in BMI is coming via reduced caloric intake, which we are unable to measure in the BRFSS.

It is theoretically conceivable that insurance coverage could increase risky drinking but reduce weight. Health care access may be more helpful for losing weight than reducing drinking. Gains in information and accountability may both be greater for weight control than drinking: dieting strategies can be complicated and benefit greatly from professional advice, and accountability is greater for weight since patients are weighed at each visit. Additionally, the *ex ante* moral hazard effect could be stronger for risky drinking than weight-related behaviors. Binge drinking has a non-trivial chance of resulting in immediate medical needs, either from alcohol poisoning, drunk driving accidents, or other injuries.[19] In contrast, expenditures to treat diseases associated with obesity typically occur years down the road. Perhaps uninsured young adults assume that they will be insured by time these downside risks are realized, in which case *ex ante* moral hazard would not apply. In short, the direct price effect could dominate for BMI, while the *ex ante* moral hazard effect could dominate for drinking. Income effects may play a role as well, especially for alcohol consumption given the aforementioned evidence of a positive causal effect of income on drinking (Apouey and Clark, 2014).

Turning to the self-assessed health outcomes, the mandate increased the probability of young adults reporting excellent overall health by 1.3-1.5 percentage points and very good/excellent health by 1.1-1.8 percentage points. However, only the estimates for excellent health are significant, as the standard errors for very good/excellent health are larger. We do not find any evidence of effects on the variables representing more severe health problems: days not in good mental health, not in good physical health, and with health-related functional limitations. The lack of effects on our mental and physical health outcomes is particularly interesting in light of Chua and Sommers' (2014) finding that the ACA dependent coverage provision increased the probabilities of reporting excellent mental and physical health. Chua and Sommers' mental and physical health variables emphasize changes at the high end of the health distribution and may therefore correspond more closely to our variable for excellent overall health than our physical and mental health variables, which focus on "not good" health. In other words, both our results and those of Chua and Sommers are consistent with the provision's effects on mental and physical health being concentrated in the high end of the health distribution.

[19] In the US, approximately 80,000 cases of alcohol poisoning and 10,322 alcohol-impaired driving crashes occur annually, with these incidents disproportionately involving young adults (CDC, 2012; NHTSA, 2014). 599,000 alcohol-related injuries occur annually among 18-24 year old college students (NIAAA, 2013).

Finally, we provide a brief discussion of the relative magnitudes of the effects on different outcomes by comparing the treatment effects expressed in standard deviations of the dependent variables. Not surprisingly, the largest effect of 0.13 standard deviations is on the probability of having any health insurance coverage. The next largest statistically significant effect is on primary care doctor access (0.065 standard deviations), then excellent health (0.032 standard deviations), then risky drinker (0.026 standard deviations), then finally BMI (-0.017 standard deviations). The largest statistically insignificant effects are on flu vaccinations (-0.033 standard deviations) and very good/excellent health (0.031 standard deviations).

V. Placebo Tests

We next provide a series of placebo tests to evaluate whether the previous results can credibly be interpreted as causal effects of the ACA dependent coverage provision. Following Slusky (2013), we estimate variants of equation (1) that test for "effects" of artificially-timed "treatments" during pre-treatment years. We estimate models for three different seven-year windows of pre-treatment data (to match the seven years used in our main 2007-2013 analyses): 2003-2009, 2002-2008, and 2001-2007. Since the first month after the implementation of the actual dependent coverage mandate was the 46^{th} month (October 2010) of our 2007-2013 sample, in each placebo test sample we date the implementation of the artificial intervention to the 46^{th} month (e.g. October 2006 for the 2003-2009 sample). We estimate (1) for each of the eighteen dependent variables in each of the three placebo test samples.

Table 1.5 reports the coefficient estimates of interest from these placebo tests. We run three tests for each of the eighteen dependent variables, though a test is not possible for checkups using 2001-2007 data since the checkup question was not asked until 2005. This leaves a total of 53 regressions. Given the large number of estimates, we would expect some significant results even for valid models. Specifically, approximately 0-1 estimates should be significant at the 1% level, about 2-3 at the 5% level, and about 5 at the 10% level. We obtain numbers even smaller than these. No estimated "treatment effects" are significant at the 1% level, 2 (3.8%) are significant at the 5% level, and 3 (5.7%) are significant at the 10% level. Moreover, we do not obtain more than one placebo test rejection for any outcome. In other words, it is not clear that there are any outcomes for which our baseline difference-in-differences model is inappropriate.

In the interest of contributing to the broader debate in the literature about the appropriateness of different age bandwidths when using difference-in-differences models to estimate the effects of the ACA dependent coverage provision, we also run the same set of placebo tests for the most common age ranges used in the literature: treatment group 19-25 and control group 26-34 (Sommers and Kronick, 2012; Sommers et al., 2013; and Chua and Sommers, 2014). We obtain 4 placebo test rejections (7.5%) at the 1% level, 7 (13.2%) at the 5% level, and 11 (20.8%) at the 10% level. The full table of results is available upon request.

VI. Heterogeneity

Having established our baseline results and assessed the validity of our model, we next turn to an examination of heterogeneity in the treatment effects. We considered stratifications by sex, race/ethnicity, education, and state pre-ACA dependent coverage law status, but we did not observe any statistically significant differences in effects across the subgroups for race/ethnicity and pre-ACA law, so we only report the results for the stratifications by sex and education. For education, we stratify into two groups: college graduates and non-college graduates.[20]

Theoretically, the ACA dependent coverage provision could have heterogeneous effects on health-related outcomes for three reasons. First, there could be heterogeneous effects on the

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[20] Further stratification by education led to estimates that were too imprecise to be useful. Note that we do not include a separate category for current students because our sample only includes those 23 and older, so the proportion of our respondents reporting "student" as their primary occupation is low.

probability of having insurance coverage. In the pre-treatment portion of sample, females were more likely to have insurance than males (76% versus 67%), and college graduates were much more likely to have insurance than non-college graduates (88% versus 64%). One might therefore expect larger gains in coverage among males and non-college graduates. On the other hand, young adults of high socioeconomic status may be more likely to have parents with employer-provided coverage, so the gains in coverage could potentially be larger for college graduates.

A second possible source of heterogeneity is that, even if the gains in health insurance are the same among all groups, different groups could respond differently to receiving coverage. For instance, Grossman (1972) argues that education enables individuals to become more efficient producers of health. More education may therefore better equip individuals to make the most out of the newly-acquired insurance (e.g. more easily find providers who accept the insurance, ask better questions at doctor's appointments, or better follow medical advice). Alternatively, the price elasticity of medical care could be strongest among low-income individuals, in which case the effects of obtaining insurance on health care utilization and health could be largest for non-college graduates. The price elasticity of medical care could also differ by sex. For instance, evidence suggests that females are more risk averse than males (e.g. Jiankoplos and Bernasek, 1998). One might therefore expect females to be more likely to obtain medical care regardless of its price, whereas males might only utilize care if the cost is minimal; i.e. males might have stronger price elasticities. Indeed, in our pre-treatment data uninsured females had higher rates of primary care doctor access, flu vaccination, and well-patient checkups than uninsured males.

Third, as discussed at the beginning of Section IV, the dependent coverage provision could affect health-related outcomes through mechanisms besides the extensive margin of insurance coverage – particularly the intensive margin of coverage – and there could be heterogeneous effects along these dimensions. For instance, suppose part of the reason females and college graduates had lower pre-ACA uninsured rates was because they were more likely to privately purchase a bare-bones, catastrophic plan if they did not have access to employer-provided coverage. In that case, the ACA dependent coverage provision may lead to larger gains along the intensive margin of coverage for women and college graduates, leading to larger improvements in health-related outcomes among these groups.

The first two columns of Table 1.6 report the results for females and males. Males experienced a 2.9 percentage point larger gain in health insurance coverage than females, and the difference is significant at the 1% level. Moreover, only males experienced statistically significant favorable effects on any outcomes besides insurance coverage. Specifically, males' rates of primary care doctor access, having any exercise, reporting very good/excellent health, and reporting excellent health increased substantially – by 4.6, 1.9, 2.9, and 3.1 percentage points, respectively. These effects are all significantly different from zero, and three of the four (all but very good/excellent health) are also statistically different from the corresponding effects on females. The only statistically significant result for females (besides insurance coverage) is an *adverse* effect on days with health-related limitations. In sum, the results suggest that males experienced larger improvements in health-related outcomes from the ACA dependent coverage provision than females, and that there appear to be multiple reasons for this heterogeneity. Gains in insurance coverage were larger for males, consistent with them having a higher pre-ACA uninsured rate. Responses to obtaining insurance coverage also appear to have been stronger for males, perhaps indicating a larger price elasticity of demand for medical care.

The last two columns of Table 1.6 report the results stratifying by college degree attainment. Both groups experienced similar gains in insurance coverage as a result of the ACA dependent coverage provision. However, statistically significant improvements in outcomes besides health insurance are only observed for college graduates. The mandate led to large and significant gains for college graduates in the following outcomes: primary care doctor access (5.1 percentage points), cost being a barrier to care (reduction of 3.4 percentage points), BMI (reduction of 0.25 units), obesity (reduction of 1.7 percentage points), and excellent self-reported health (increase of 3.7 percentage points). Besides insurance, the only significant effects for non-college graduates are unfavorable: a 2.2 percentage point reduction in flu vaccinations and a 1.6 percentage point increase in risky drinking. In short, college graduates experienced greater improvements in health-related outcomes than non-college graduates, and this appears to be due to heterogeneous effects *of* coverage rather than heterogeneous effects *on* coverage. This is consistent with a Grossman-style story in which education enables individuals to better take advantage of their health care opportunities. However, the results could also be partly attributable to greater gains along the intensive margin of coverage for college graduates, which we cannot measure in our data. Regardless of the reason, these results suggest that the mandate increases SES-based disparities in health.

VII. Discussion

The first major insurance expansion under the ACA – a provision requiring insurers to allow young adults to remain on their parents' health insurance until turning 26 – was implemented in September 2010. This paper uses data from the BRFSS to examine the effects of this mandate on various outcomes related to health care access, preventive care utilization, risky health behaviors, and self-assessed health. We implement a difference-in-differences model with individuals slightly below the mandate's age cutoff (ages 23-25) as the treatment group and those slightly above the cutoff (ages 27-29) as the control group.

We first estimate average effects for the entire sample. The results suggest that the ACA dependent coverage provision increased health care access but not utilization of preventive care, had mixed effects on risky health behaviors, and improved health at the high end of the distribution. Specifically, we observe significant and robust favorable effects on health insurance, access to a primary care doctor, probability of having excellent self-assessed health, and BMI. However, we also find an adverse effect on risky drinking consistent with *ex ante* moral hazard and no clear effects on the other outcomes. We then validate our model through a series of placebo tests and show that our classifications of treatment and control groups perform better in these tests that the wider age bandwidths common in the literature. Finally, we conduct subsample analyses, finding particularly striking improvements in outcomes for men and college graduates. Men had larger gains in health insurance coverage than women, and only men experienced statistically significant gains in any outcomes beyond health insurance – specifically primary care access, exercise, and overall self-assessed health. Insurance expansions were similar for college graduates and non-college graduates, but only college graduates experienced significant gains in any other outcomes: primary care access, cost being a barrier to care, BMI, obesity, and overall self-assessed health.

The ACA dependent coverage mandate provides a unique opportunity to study a health insurance intervention specific to young adults as opposed to seniors (Medicare), the poor (Medicaid), or the uninsured population at large (the Massachusetts reform). In general, our results suggest that health insurance affects health-related outcomes of young adults more modestly than prior studies have observed for these other populations. First, we find no evidence of increased preventive care utilization, in contrast to prior results from both Medicaid (Finkelstein et al., 2012) and the Massachusetts reform (Kolstad and Kowalski, 2012). Second, we only find statistically significant improvements in overall self-assessed health at the top of the distribution, as reporting of excellent health increases but there is no clear evidence of an effect on reporting very good or excellent health. We do not observe any gains in the variables reflecting more severe health conditions: days not in good physical health, days not in good mental health, and days with functional limitations. This contrasts the clear gains in these same outcomes observed for both Medicaid (Finkelstein et al., 2012) and the Massachusetts reform (Van der Wees et al., 2013; Courtemanche and Zapata, 2014). Interestingly, Chua and Sommers (2014) find that the ACA dependent coverage provision increased the probabilities of self-reporting excellent physical and mental health. Combining their results with ours suggests that physical and mental health did improve, but only at the high end of the distribution.

While our results suggest that health insurance expansions for young adults are less impactful than those for other age groups, it is still important to emphasize that we *do* observe *some* improvements in important outcomes, including health care access, excellent self-assessed health, and BMI. One might have initially worried that a coverage expansion for young adults would not lead to any health improvements given the generally good baseline health of this age group.

An important contribution of our paper is that we provide, to our knowledge, the first empirical investigation of *ex ante* moral hazard that focuses specifically on young adults. We find evidence consistent with *ex ante* moral hazard in only one domain: risky drinking (binge drinking or excessive number of drinks per month). In contrast, we find evidence that the dependent coverage improved weight-related behaviors while not affecting smoking and pregnancies. Our results therefore suggest that *ex ante* moral hazard is domain-specific.

Another interesting result is that, since the improvement in health is concentrated among college graduates, the ACA dependent coverage provision appears to *increase* SES-based disparities in health. This is contrary to the usual impacts of public policies to expand health insurance. Medicaid has been shown to improve at least some health outcomes (Currie and Gruber 1996a and 1996b; Finkelstein et al., 2012; Sommers et al., 2012), implying reduced income-based disparities in health. The Massachusetts reform also appears to have reduced income-based disparities, as Courtemanche and Zapata (2014) found the largest gains in self-assessed health among low-income individuals.

Several caveats to our analyses provide directions for future research. First, since we study eighteen different dependent variables, we might expect one or two results to emerge as significant at conventional levels simply by chance. We did not employ multiple hypothesis test adjustments in this paper because, even though such adjustments control the Type I error rate (probability of falsely rejecting any null hypotheses), they do so at the cost of substantially increasing the Type II error rate (probability of failing to reject false null hypotheses).[21] However, future research should revisit our questions using different data to see if any of our findings could be attributable to chance rather than genuine causal effects of the mandate.

[21] For instance, the simple Bonferroni correction involves multiplying all p-values by the number of hypotheses being tested, which is eighteen in our case. This would make it virtually impossible to reject any null hypothesis in regressions that already demand quite a bit of the data by including fixed effects and clustering at an aggregated level. It is not clear to us that it would be preferable to, for example, fail to reject five false null hypotheses for the sake of not rejecting one true null hypothesis. This seems especially true in cases such as ours, where null results are an important part of the story. Moreover, we view our analyses as testing for eighteen distinct effects, some of which are more plausible theoretically than others, as opposed to testing for one effect that may manifest itself through eighteen different measures. It is not clear why, for instance, we should inflate the p-values in the health insurance regressions merely because we also study smoking, pregnancies, etc.

Next, we focus on estimating the ACA dependent coverage provision's effects on 23-25 year olds, ignoring possible effects on 19-22 year olds because of the greater difficulty in finding a suitable control group and the weaker *ex ante* expectations of significant effects. Further understanding whether benefits accrue to young adults besides 23-25 year olds is obviously important in order to fully evaluate the policy.

Further research is also necessary to understand the mechanisms through which the mandate improves health. Increased health care utilization is an obvious possibility, but early evidence on the ACA provision's impact on health care consumption is mixed. Akosa Antwi et al. (2014) report a rise in hospitalizations using administrative data, but Chua and Sommers (2014) find no evidence of changes in survey-based measures of hospital care, primary care, or prescription drug utilization, while we find no significant increases in preventive care. Another possible explanation is that self-assessments of health improve due to a "warm glow" from the peace of mind of having insurance. Finkelstein et al. (2012) proposed this as an explanation for their finding from the Oregon Medicaid experiment that most of the gains in self-assessed health appeared to occur *before* changes in utilization.

Finally, and critically, our results should not be interpreted as providing a full accounting of the benefits of expanding insurance coverage among young adults. The primary purpose of insurance is to protect individuals from financial risk, and gains along this dimension may be especially substantial for young adults given their relatively low income and wealth levels. Moreover, expanding coverage among young adults is an important component of the overall strategy behind the ACA since it is necessary to offset the additional costs of insuring older and sicker individuals under community rating. In other words, the costs and benefits of the different components of the ACA need to be evaluated together, as the different pieces of the reform are designed to work synergistically.

Table 1.1 – Sample Sizes for Different Outcomes

Outcome Variable	Total	Treatment (23-25)	Control (27-29)
Health care access			
Any health insurance coverage	126,702	53,057	73,645
Any primary care doctor	118,392	49,520	68,872
Cost prevented care in past year	107,831	45,041	62,790
Preventive care utilization			
Flu vaccination in past year	118,394	49,502	68,892
Well-patient checkup in past year	107,931	45,085	62,846
Pap test in past year (women only)+	26,919	10,799	16,120
Risky health behaviors			
Currently smokes cigarettes	125,616	52,607	73,009
Alcoholic drinks in past 30 days	120,958	50,521	70,437
Risky drinker in past 30 days	120,037	50,110	69,927
Body mass index	120,373	50,529	69,844
Obese	120,373	50,529	69,844
Any exercise in past 30 days	122,720	51,337	71,383
Pregnancy (unmarried women only)	39,499	19,610	19,889
Self-assessed health			
Overall health very good or excellent	126,662	53,102	73,560
Overall health excellent	126,662	53,102	73,560
Days of last 30 not in good mental health	124,773	52,386	72,387
Days of last 30 not in good physical health	124,861	52,387	72,474
Days of last 30 with health-related limitations	125,365	52,615	72,750

+The pap test variable is only available in even-numbered years, reducing the sample size for that outcome.

Table 1.2 – Pre-Treatment Means and Standard Deviations for Control Variables

Control Variable	Treatment (Ages 23-25)	Control (Ages 27-29)
Age dummies (age=23 is omitted)		
Age=24	0.349 (0.477)	--
Age=25	0.322 (0.467)	--
Age=27	--	0.310 (0.462)
Age=28	--	0.343 (0.475)
Age=29	--	0.347 (0.476)
Female	0.505 (0.500)	0.508 (0.500)
Race/ethnicity dummies (non-Hispanic white is omitted)		
Non-Hispanic black	0.112 (0.316)	0.116 (0.320)
Hispanic	0.224 (0.417)	0.209 (0.407)
Other than black, Hispanic, or white	0.087 (0.282)	0.077 (0.266)
Currently married	0.305 (0.460)	0.564 (0.496)
Education dummies (less than high school degree is omitted)		
High school degree but no further	0.283 (0.450)	0.257 (0.437)
Some college but no four-year degree	0.299 (0.458)	0.271 (0.444)
College graduate	0.303 (0.459)	0.364 (0.481)
Household income dummies (less than $10,000 is omitted)		
Between $10,000 and $15,000	0.068 (0.252)	0.049 (0.216)
Between $15,000 and $20,000	0.102 (0.303)	0.077 (0.267)
Between $20,000 and $25,000	0.116 (0.321)	0.097 (0.296)
Between $25,000 and $35,000	0.144 (0.351)	0.129 (0.335)
Between $35,000 and $50,000	0.166 (0.372)	0.165 (0.371)
Between $50,000 and $75,000	0.143 (0.350)	0.187 (0.390)
$75,000 and over	0.186 (0.389)	0.240 (0.427)
Number of children in household dummies (0 is omitted)		
One child	0.230 (0.421)	0.235 (0.424)
Two children	0.159 (0.366)	0.233 (0.423)
Three children	0.055 (0.229)	0.110 (0.313)
Four children	0.018 (0.133)	0.038 (0.192)
Five or more children	0.008 (0.090)	0.016 (0.124)
Cell phone only	0.703 (0.457)$^{+}$	0.678 (0.467)$^{+}$
Student	0.109 (0.312)	0.054 (0.226)
Unemployed	0.111 (0.314)	0.093 (0.290)
State unemployment rate	7.032 (2.615)	7.186 (2.666)
Pre-ACA state mandate	0.220 (0.415)	0.033 (0.179)

Notes: BRFSS sampling weights are used. Means are reported, with standard deviations in parentheses. $^{+}$ indicates the summary statistics are from 2011-2013, since the variable is 0 for all respondents in all prior years.

Table 1.3 – Means and Standard Deviations for Outcome Variables

Outcome Variable	Pre-Treatment Period		Post-Treatment Period		Difference-in-Differences
	Treatment (Ages 23-25)	Control (Ages 27-29)	Treatment (Ages 23-25)	Control (Ages 27-29)	
Health care access					
Any health insurance coverage	0.680 (0.466)	0.753 (0.431)	0.709 (0.454)	0.708 (0.455)	0.073 (0.018)***
Any primary care doctor	0.564 (0.496)	0.641 (0.480)	0.519 (0.500)	0.558 (0.497)	0.038 (0.010)**
Cost prevented care in past year	0.241 (0.427)	0.216 (0.411)	0.240 (0.427)	0.235 (0.424)	-0.020 (0.014)
Preventive care utilization					
Flu vaccination in past year	0.225 (0.418)	0.246 (0.431)	0.239 (0.426)	0.265 (0.441)	-0.006 (0.009)
Well-patient checkup in past year	0.521 (0.500)	0.545 (0.498)	0.524 (0.499)	0.529 (0.499)	0.019 (0.011)
Pap test in past year	0.693 (0.461)	0.724 (0.447)	0.614 (0.487)	0.647 (0.478)	-0.002 (0.013)
Risky health behaviors					
Currently smokes cigarettes	0.260 (0.432)	0.249 (0.432)	0.257 (0.437)	0.254 (0.435)	-0.009 (0.012)
Alcoholic drinks in past 30 days	17.359 (43.926)	13.883 (34.703)	19.481 (43.947)	16.841 (40.916)	-0.836 (0.889)
Risky drinker in past 30 days	0.775 (0.418)	0.807 (0.394)	0.749 (0.434)	0.769 (0.422)	0.013 (0.005)*
Body mass index	26.404 (5.807)	27.253 (6.031)	26.167 (6.019)	27.192 (6.142)	-0.177 (0.050)**
Obese	0.222 (0.415)	0.262 (0.440)	0.197 (0.398)	0.252 (0.434)	-0.014 (0.003)***
Any exercise in past 30 days	0.810 (0.392)	0.799 (0.401)	0.819 (0.385)	0.799 (0.401)	0.009 (0.005)
Pregnancy	0.048 (0.215)	0.043 (0.203)	0.044 (0.205)	0.040 (0.195)	-0.001 (0.004)
Self-assessed health					
Overall health very good/excellent	0.607 (0.488)	0.610 (0.488)	0.608 (0.488)	0.589 (0.492)	0.022 (0.011)
Overall health excellent	0.255 (0.436)	0.257 (0.437)	0.250 (0.433)	0.236 (0.425)	0.017 (0.003)***
Days not in good mental health	4.050 (7.638)	3.844 (7.680)	4.410 (8.067)	4.165 (8.063)	0.040 (0.162)
Days not in good physical health	2.240 (5.526)	2.303 (5.815)	2.446 (5.999)	2.484 (6.170)	0.025 (0.053)
Days with health-related limitations	1.589 (4.757)	1.664 (5.177)	1.727 (5.131)	1.739 (5.332)	0.063 (0.104)

Notes: Standard errors, heteroskedasticity-robust and clustered by age, are in parentheses. BRFSS sampling weights are used. Means are reported, with standard deviations in parentheses. *** indicates the difference-in-difference is significant at the 1% level; ** 5% level; * 10% level.

Table 1.4 – Difference-in-Difference Regression Estimates of Effects of ACA Dependent Coverage Mandate

Outcome Variable	Baseline Model	Demographic Controls Only	Start in 2004	Start in 2001	Drop 3/10-12/10	Collapsed Data
Health care access						
Any health insurance	0.061 (0.017)** [0.130]	0.067 (0.018)**	0.059 (0.013)***	0.055 (0.012)***	0.064 (0.016)***	0.061 (0.015)***
Any primary doctor	0.032 (0.010)** [0.065]	0.034 (0.010)**	0.020 (0.006)**	0.021 (0.006)**	0.033 (0.009)**	0.029 (0.011)**
Cost prevented care	-0.019 (0.014) [-0.044]	-0.019 (0.014)	-0.022 (0.015)	-0.023 (0.015)	-0.020 (0.015)	-0.016 (0.011)
Preventive care utilization						
Flu vaccination	-0.014 (0.007) [-0.033]	-0.011 (0.008)	-0.017 (0.009)	-0.018 (0.008)*	-0.014 (0.008)	-0.020 (0.006)**
Well-patient checkup	0.013 (0.011) [0.026]	0.015 (0.010)	0.011 (0.010)	0.011 (0.010)	0.017 (0.010)	0.011 (0.006)
Pap test	-0.004 (0.015) [-0.009]	-0.003 (0.014)	-0.019 (0.010)	-0.025 (0.015)**	-0.015 (0.015)	0.002 (0.008)
Risky health behaviors						
Currently smokes	0.003 (0.007) [0.007]	-0.006 (0.010)	-0.001 (0.004)	-0.008 (0.005)	-0.001 (0.006)	0.005 (0.007)
Drinks per month	0.120 (0.906) [0.003]	-0.468 (0.887)	-0.429 (0.604)	-0.597 (0.590)	0.083 (0.840)	0.011 (0.929)
Risky drinker	0.011 (0.003)** [0.026]	0.008 (0.004)*	0.009 (0.003)**	0.009 (0.007)**	0.014 (0.003)***	0.009 (0.003)**
Body mass index	-0.098 (0.029)** [-0.017]	-0.175 (0.045)**	-0.124 (0.062)	-0.169 (0.061)**	-0.173 (0.074)*	-0.118 (0.033)***
Obese	-0.009 (0.008) [-0.022]	-0.014 (0.005)**	-0.010 (0.007)	-0.011 (0.008)	-0.013 (0.006)*	-0.010 (0.004)**
Any exercise	0.003 (0.004) [0.008]	0.008 (0.007)	0.005 (0.003)	0.004 (0.004)	0.001 (0.005)	0.007 (0.003)**
Pregnancy	-0.003 (0.005) [-0.014]	-0.002 (0.005)	-0.004 (0.004)	-0.002 (0.004)	-0.003 (0.005)	-0.002 (0.004)
Self-assessed health						
Very good/exc. Health	0.015 (0.011) [0.031]	0.018 (0.010)	0.016 (0.011)	0.015 (0.008)	0.011 (0.010)	0.014 (0.009)
Excellent health	0.014 (0.005)** [0.032]	0.014 (0.003)***	0.013 (0.004)**	0.014 (0.005)**	0.014 (0.006)*	0.015 (0.004)***
Days not good mental	0.081 (0.158) [0.010]	0.064 (0.144)	0.050 (0.144)	0.036 (0.116)	0.156 (0.156)	0.084 (0.127)
Days not good phys.	0.059 (0.068) [0.011]	0.045 (0.046)	-0.022 (0.079)	-0.014 (0.076)	0.075 (0.028)**	0.028 (0.063)
Days health limitations	0.122 (0.099) [0.025]	0.102 (0.093)	0.073 (0.109)	0.065 (0.094)	0.201 (0.107)	0.101 (0.086)

Notes: *** indicates significant at the 1% level; ** 5% level; * 10% level. Standard errors, heteroskedasticity-robust and clustered by age, are in parentheses. All regressions include the controls plus age, state, and time fixed effects. BRFSS sampling weights are used. For the baseline regression, effect sizes in standard deviations of the dependent variable (for the treatment group in the pre-treatment period) are in brackets.

Table 1.5 – Placebo Regressions

Outcome Variable	2003-2009 Treatment 10/06	2002-2008 Treatment 10/05	2001-2007 Treatment 10/04
Health care access			
Any health insurance coverage	-0.002 (0.005)	0.002 (0.008)	-0.009 (0.007)
Any primary care doctor	-0.008 (0.014)	0.002 (0.007)	0.019 (0.011)
Cost prevented care in past year	-0.007 (0.011)	-0.017 (0.013)	-0.013 (0.007)
Preventive care utilization			
Flu vaccination in past year	-0.013 (0.016)	-0.007 (0.008)	-0.001 (0.007)
Well-patient checkup in past year	0.002 (0.014)	-0.008 (0.014)	--
Pap test in past year	-0.012 (0.014)	-0.024 (0.022)	-0.027 (0.025)
Risky health behaviors			
Currently smokes cigarettes	-0.019 (0.007)**	-0.006 (0.009)	-0.007 (0.008)
Alcoholic drinks in past 30 days	-1.648 (1.035)	-0.659 (0.584)	-1.146 (0.788)
Risky drinker in past 30 days	-0.001 (0.006)	-0.014 (0.007)*	-0.009 (0.011)
Body mass index	-0.001 (0.146)	-0.023 (0.196)	-0.082 (0.145)
Obese	0.002 (0.008)	0.005 (0.011)	0.0005 (0.009)
Any exercise in past 30 days	0.008 (0.008)	0.008 (0.005)	-0.004 (0.005)
Pregnancy	0.011 (0.010)	0.005 (0.012)	-0.002 (0.007)
Self-assessed health			
Overall health very good/excellent	0.011 (0.003)**	0.002 (0.009)	0.004 (0.009)
Overall health excellent	0.004 (0.005)	0.009 (0.005)	0.006 (0.008)
Days not in good mental health	-0.064 (0.174)	-0.005 (0.232)	-0.054 (0.143)
Days not in good physical health	-0.041 (0.109)	0.034 (0.121)	0.165 (0.107)
Days with health-related limitations	-0.039 (0.084)	-0.043 (0.051)	0.017 (0.060)

Notes: *** indicates significant at the 1% level; ** 5% level; * 10% level. Standard errors, heteroskedasticity-robust and clustered by age, are in parentheses. All regressions include the controls plus age, state, and time fixed effects. BRFSS sampling weights are used.

Table 1.6 – Heterogeneity by Sex and Education

	Sex		Education	
Outcome Variable	Female	Male	Not College Graduate	College Graduate
Health care access				
Any health insurance coverage	0.045 (0.017)**	0.074 (0.016)***+++	0.067 (0.019)**	0.061 (0.013)***
Any primary care doctor	0.016 (0.009)	0.046 (0.012)**+	0.025 (0.012)	0.051 (0.006)***++
Cost prevented care in past year	-0.019 (0.021)	-0.016 (0.013)	-0.014 (0.017)	-0.034 (0.009)**
Preventive care utilization				
Flu vaccination in past year	-0.020 (0.012)	-0.012 (0.010)	-0.022 (0.008)**	0.003 (0.009)
Well-patient checkup in past year	0.013 (0.014)	0.013 (0.016)	0.006 (0.019)	0.035 (0.016)
Pap test in past year	-0.004 (0.015)	--	-0.007 (0.021)	0.008 (0.028)
Risky health behaviors				
Currently smokes cigarettes	0.011 (0.011)	-0.004 (0.016)	0.001 (0.008)	0.002 (0.006)
Alcoholic drinks in past 30 days	-0.117 (0.441)	0.359 (1.559)	-0.068 (1.171)	0.398 (0.985)
Risky drinker	0.009 (0.012)	0.015 (0.014)	0.016 (0.004)***	-0.007 (0.007)
Body mass index	-0.133 (0.153)	0.018 (0.160)	0.001 (0.050)	-0.254 (0.096)**
Obese	-0.010 (0.010)	-0.005 (0.012)	-0.004 (0.009)	-0.017 (0.004)***
Any exercise in past 30 days	-0.010 (0.007)	0.019 (0.004)***++	0.001 (0.006)	0.010 (0.005)
Pregnancy	-0.003 (0.005)	--	-0.005 (0.006)	-0.001 (0.006)
Self-assessed health				
Overall health very good or excellent	0.001 (0.022)	0.029 (0.009)**	0.007 (0.009)	0.029 (0.017)
Overall health excellent	-0.003 (0.009)	0.031 (0.005)***++	0.002 (0.006)	0.037 (0.012)**
Days of last 30 not in good mental health	0.100 (0.196)	0.083 (0.160)	0.259 (0.154)	-0.323 (0.193)+++
Days of last 30 not in good physical health	0.109 (0.081)	-0.011 (0.167)	0.211 (0.145)	-0.262 (0.166)
Days of last 30 with health-related limitations	0.347 (0.110)**	-0.102 (0.206)	0.265 (0.161)	-0.149 (0.096)

Notes: +++ difference between effects on subgroups is significant at the 1% level; ++ 5% level; + 10% level. See other notes for Table 1.5.

Table 1.7 – Full Regression Output for Selected Dependent Variables

Control Variable	Insurance	Smoker	Excellent Health
Treated*Post	0.061 (0.017)*	0.003 (0.007)	0.014 (0.005)*
Age=24	-0.004 (0.002)*	0.007 (0.001)**	-0.002 (0.001)*
Age=25	-0.007 (0.003)	0.022 (0.001)**	-0.004 (0.001)**
Age=27	0.012 (0.010)	0.030 (0.002)**	-0.016 (0.002)**
Age=28	0.016 (0.011)	0.039 (0.002)**	-0.026 (0.002)**
Age=29	0.029 (0.011)*	0.032 (0.002)**	-0.018 (0.003)**
Female	0.070 (0.006)**	-0.056 (0.005)**	-0.020 (0.008)
Non-Hispanic black	-0.012 (0.010)	-0.126 (0.009)**	0.006 (0.007)
Hispanic	-0.117 (0.007)**	-0.183 (0.004)**	-0.019 (0.005)**
Other than black, Hispanic, or white	-0.011 (0.010)	-0.025 (0.013)	-0.010 (0.004)*
Currently married	0.069 (0.008)**	-0.110 (0.008)**	0.027 (0.005)**
High school degree but no further	0.110 (0.012)**	-0.091 (0.017)**	0.039 (0.009)**
Some college but no 4-year degree	0.171 (0.017)**	-0.161 (0.014)**	0.051 (0.008)**
College graduate	0.251 (0.019)**	-0.310 (0.020)**	0.111 (0.009)**
Between $10,000 and $15,000	-0.048 (0.014)*	0.007 (0.006)	-0.008 (0.018)
Between $15,000 and $20,000	-0.070 (0.018)*	0.021 (0.011)	-0.007 (0.013)
Between $20,000 and $25,000	-0.032 (0.009)*	0.007 (0.009)	0.0001 (0.009)
Between $25,000 and $35,000	0.051 (0.010)**	-0.020 (0.013)	0.026 (0.013)
Between $35,000 and $50,000	0.120 (0.009)**	-0.036 (0.014)	0.044 (0.010)**
Between $50,000 and $75,000	0.169 (0.011)**	-0.063 (0.016)**	0.058 (0.011)**
$75,000 and over	0.179 (0.012)**	-0.057 (0.015)*	0.108 (0.012)**
One child in household	0.021 (0.007)*	0.035 (0.009)**	-0.012 (0.003)**
Two children in household	0.031 (0.005)**	0.044 (0.012)*	-0.010 (0.008)
Three children in household	0.020 (0.008)	0.055 (0.010)**	-0.025 (0.011)
Four children in household	0.017 (0.021)	0.071 (0.023)*	-0.038 (0.016)
Five or more children in household	0.065 (0.023)*	0.071 (0.018)*	-0.007 (0.012)
Cell phone only	-0.013 (0.006)	0.007 (0.006)	0.011 (0.007)
Student	-0.006 (0.016)	-0.035 (0.008)**	0.013 (0.009)
Unemployed	-0.164 (0.017)**	0.100 (0.010)**	-0.027 (0.006)**
State unemployment rate	0.004 (0.003)	-0.009 (0.004)*	0.002 (0.002)
Pre-ACA state mandate	0.017 (0.010)	0.001 (0.010)	-0.015 (0.004)*

Notes: ** indicates significant at the 1% level; * 5% level. Standard errors, heteroskedasticity-robust and clustered by age, are in parentheses. All regressions also include the age, state, and time fixed effects. BRFSS sampling weights are used. Separate variables for "treated" and "post" are not included because they are subsumed by the age and time fixed effects.

Figure 1.1 -- Trends in Access to Care and Preventive Care Variables by Age Group

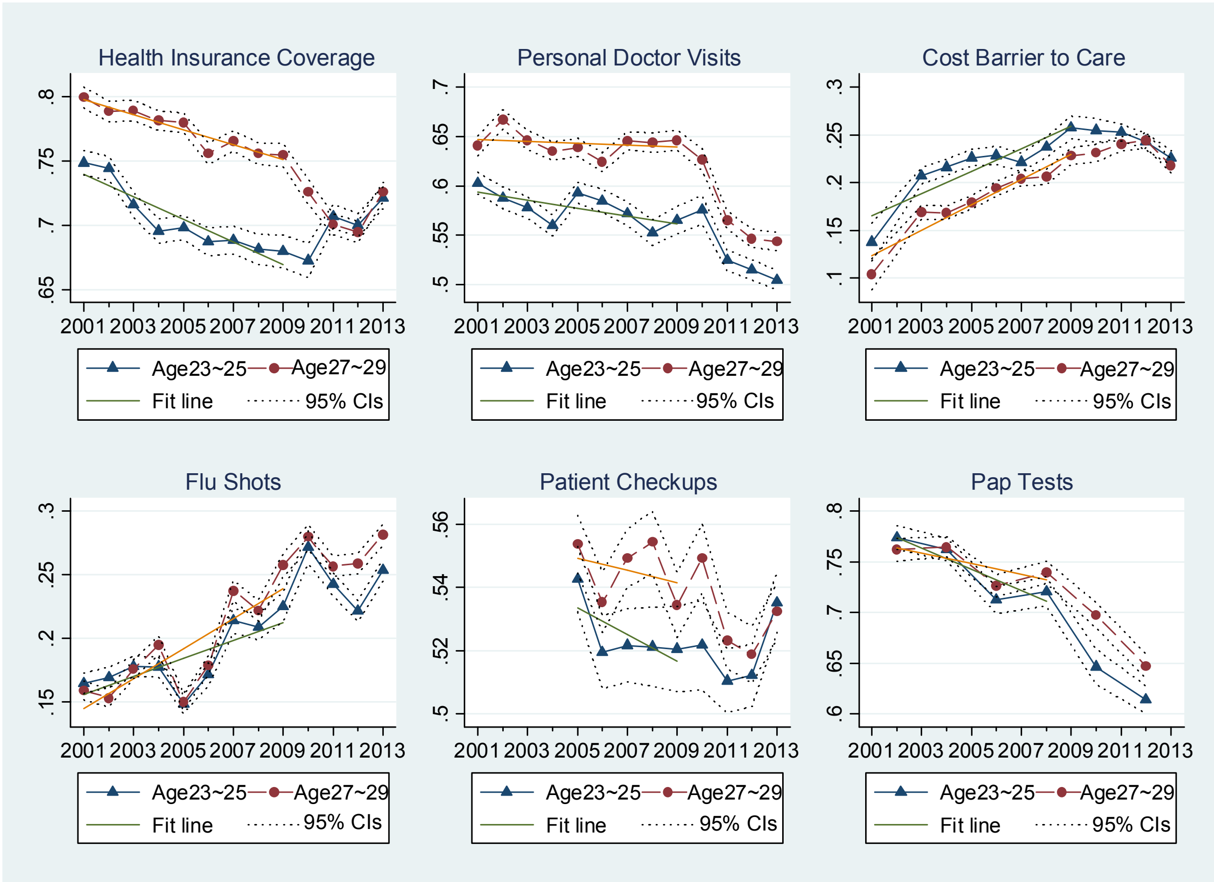


Figure 1.2 -- Trends in Health Behavior Variables by Age Group

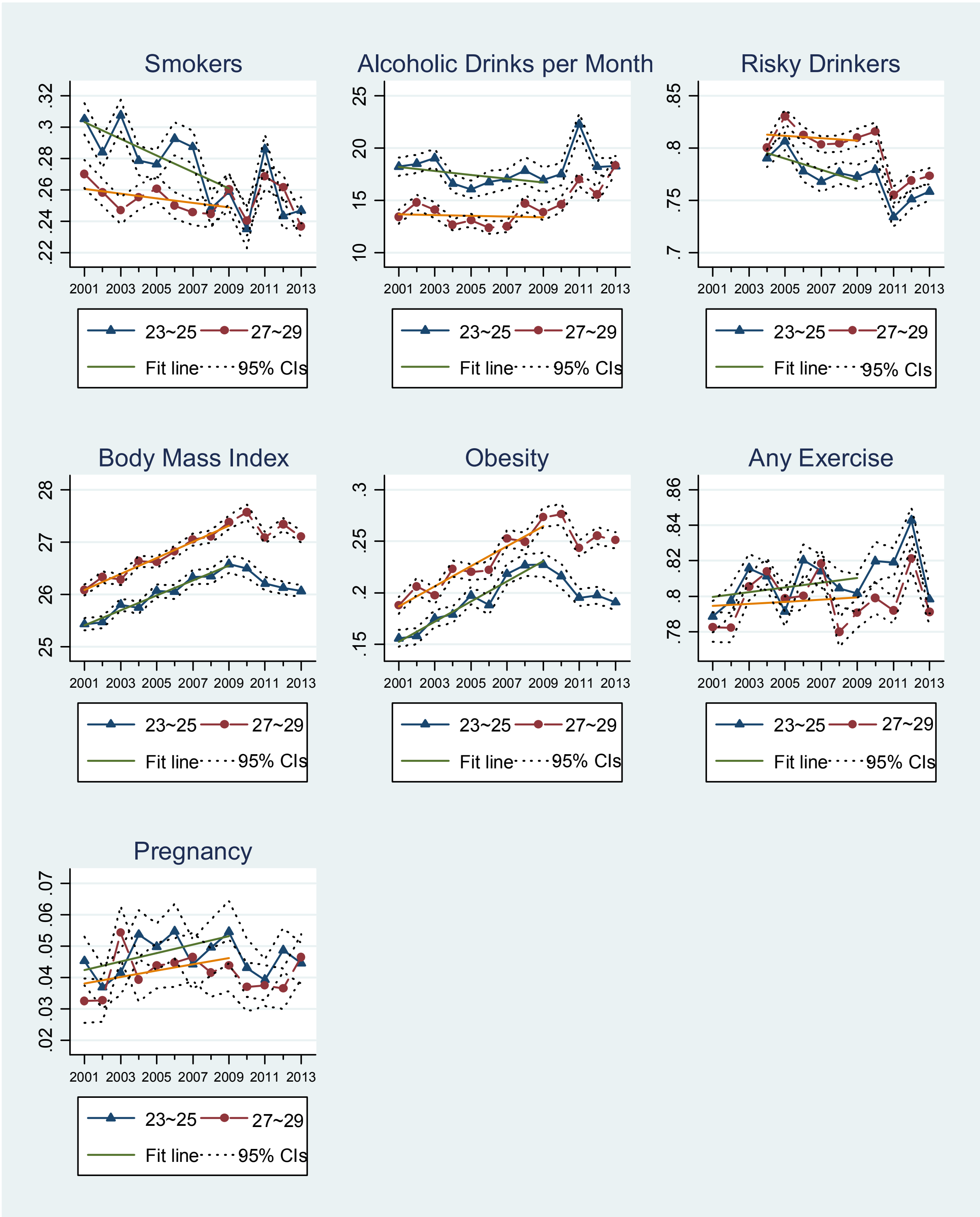
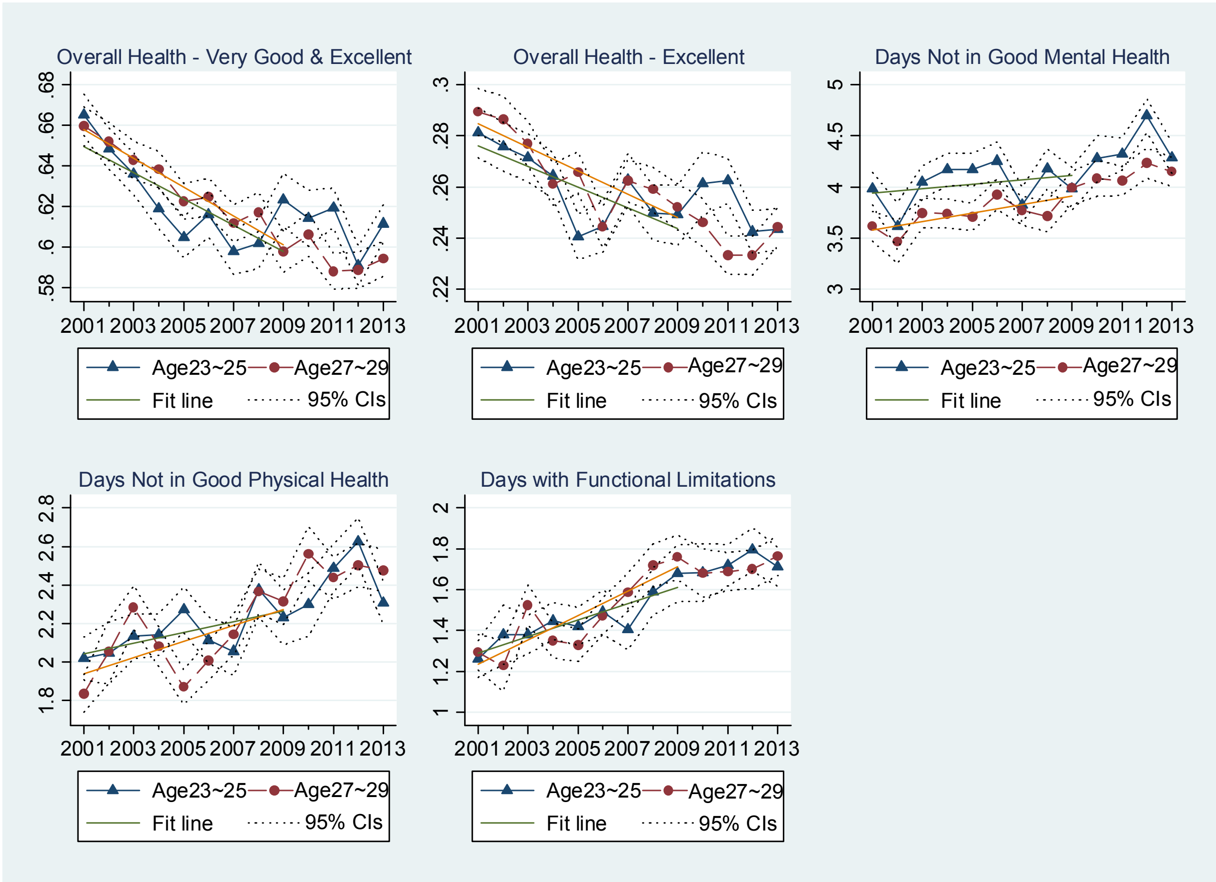


Figure 1.3 -- Trends in Self-Assessed Health Variables by Age Group



CHAPTER II

Health Insurance and Young Adults' Avoidable Hospitalizations

I. Introduction

On March 23, 2010, President Obama signed the Patient Protection and Affordable Care Act (ACA) into law.[22] One of the first implemented provisions of the ACA was targeted at young adults, who often face the risk of losing their health insurance coverage as early as age 19. Prior to the ACA, insurance companies typically removed enrolled children from their parents' plans at age 19 for non-students and 23 for full-time students (Anderson et al., 2012 and 2014).[23] Under the new law, starting in September 2010, young adults are allowed to stay on their parents' plan until they turn 26 years old, with the same benefits.[24] By allowing young adults to maintain coverage under their parents' health plan, the law makes it easier and more affordable for them to get health care.

Historically, the rate of insurance coverage for young Americans decreased at age 19, as these young adults may have lost their health insurance due to being ineligible to maintain coverage under their parents' plan or because of their employment status (unemployed, part-time employment, entry-level employment or small business employment without employer-sponsored coverage). For these reasons, young adults typically have the lowest rate of insurance coverage in comparison with other age groups. To be more specific, the rate of insurance coverage for young adults in the age group of 19-25 was only 68.6 percent in 2009, while the national rate was 83.9 percent (DeNavas-Walt et al., 2010).

[22] For more information one can visit the following websites:
http://www.whitehouse.gov/healthreform/healthcare-overview
http://www.hhs.gov/healthcare/rights/law/index.html
[23] There was a great deal of prior state-to-state variation in dependent coverage rules, including differences in age limits and marital status requirements.
[24] For more on this policy see: http://www.hhs.gov/healthcare/rights/youngadults/index.html; http://www.cms.gov/CCIIO/Resources/Files/adult_child_fact_sheet.html.

Contrary to the idea that young people do not "need" health insurance, one out of six young adults experiences a chronic illness like cancer, asthma or appendicitis (Centers for Disease Control and Prevention, 2009). Also, young adults often partake in behaviors such as overeating, sedentary lifestyles, smoking, excessive drinking, and unprotected sex that pose long-term risks. Additionally, compared to insured young adults, uninsured peers are two-to-four times more likely to delay healthcare due to costs (Cantor, 2010). Moreover, young adults are at risk for their health as well as their finances: nearly half of uninsured young adults report problems associated with paying medical bills (Collins, 2012). Lacking health insurance as a young adult tends to cause health and economic problems in later adulthood (Merluzzi, 1999; Callahan, 2005; Nicholson, 2009).

A recent literature has developed showing that the ACA expansion of dependent coverage increased the rate of insurance coverage among the targeted group of young adults (Cantor et al., 2012; Sommers and Kronick, 2012; Sommers et al., 2013; Akosa Antwi et al., 2013 and 2015; Chua and Sommers, 2014; Barbaresco et al., 2015). However, there is little, if any, evidence on the effect of this aspect of the ACA on the quality of care received by young adults. The purpose of this paper is to evaluate the impact of the ACA expansion of dependent coverage on primary care quality by examining changes over time in the probability of having an avoidable hospitalization among the targeted group of young adults as compared to young adults just outside this age range.

As in the Kolstad and Kowalski (2012) (hereafter as KK) study of the Massachusetts health care reform, I analyze the universe of hospital discharges from a nationally-representative sample of roughly 20 percent of all hospitals in the United States that is compiled by the Agency for Healthcare Research and Quality (AHRQ) Healthcare Cost and Utilization Project (HCUP).

This sample is known as the National Inpatient Sample (NIS). In addition, I also follow KK and use the AHRQ-provided methodology for identifying avoidable hospitalizations in the data. While conventional wisdom suggests that an increase in insurance coverage would result in more preventive care in an outpatient setting and thus fewer avoidable hospitalizations, I develop a conceptual model in which the impact of the expansion of dependent coverage on the probability of having an avoidable hospitalization is ambiguous and use that to motivate my empirical work. In contrast to KK's findings for the Massachusetts reform, my primary results suggest that the ACA expansion of dependent coverage for young adults aged 23-25 leads to an *increase* in overall avoidable hospitalizations, which is driven by a large increase in *chronic* avoidable hospitalizations. The effects are stronger for female, whites, and the middle income quartiles of patient's zip code.

Some would interpret this result as implying the ACA led a reduction in primary care quality. It may instead suggest a tradeoff between two forces likely to increase avoidable hospitalization rates, the moral hazard aspect of expanding insurance coverage as well as improved access to hospitals, and the efficiency effect associated with increasing access to primary care relative to hospital and emergency room care, which should reduce avoidable hospitalization rates. The size of this tradeoff is likely different for the young adults targeted by this reform as compared to older adults, children, or the elderly.

The rest of this paper is organized as follows: Section II provides an overview of the relevant literature, section III describes the conceptual relationship between the ACA expansion of dependent insurance coverage for young adults and the number of avoidable hospitalizations, section IV describes my methodology, and section V describes the data. Section VI presents my results and section VII provides a discussion of these results. Conclusions are given in section VIII.

II. Literature Review

In this section I review the literature on previous state policy as well as the ACA mandate with respect to dependent coverage and related types of coverage expansions. I focus on the literature dealing with the impacts of expansions of coverage on general health care utilization, risky behaviors, and health outcomes, as well as avoidable hospitalizations.

A. Dependent Coverage Policies and Insurance Coverage

Prior to the ACA, about two thirds of states implemented state-level policies allowing for some type of dependent coverage expansion. However, researchers found small or even no net impact of these policies on the number of uninsured young people (Levine et al., 2011; Monheit et al., 2011; Blum et al., 2012). This was due in part to the scope of these reforms being limited by the state definitions of a dependent, which could include restrictions related to student status, marital status, co-residence with parents and tax dependent status. Additionally, all state laws excluded self-funded benefit programs, which meant that they did not apply to around half of employer-provided plans. In addition, increases in dependent coverage could have been offset by reductions in other types of coverage.

In contrast, the ACA dependent coverage expansion aimed to improve net coverage among young adults by relaxing the eligibility requirements and extending the same requirements to employers who have self-insured plans. Recent studies have shown that the ACA dependent coverage expansion has significantly increased health insurance coverage levels for young adults across all racial groups and for both the employed and the unemployed (Cantor et al., 2012; Sommers and Kronick, 2012). Other research focused on health care utilization also shows significant increases in health insurance coverage (Sommers et al., 2013; Akosa Antwi et al., 2013 and 2015; Chua and Sommers, 2014; Barbaresco et al., 2015). This increase in coverage provides protection to young adults at risk of losing insurance in the absence of the law, especially for men, unmarried adults, non-students, and those with poor health (Cantor et al., 2012). Thus the ACA dependent coverage reform has successfully increased health insurance coverage, which decreases the price of medical care faced by young adults.

B. The Impact of the ACA Dependent Coverage Expansion on Utilization, Health Outcomes, and Risky Behaviors

One would predict that increases in coverage would lead to overall increases in medical care access and consumption as a result of reduced medical care prices. Sommers et al. (2013) show that the ACA dependent coverage expansion reduces delays in getting care and care foregone due to costs. Akosa Antwi et al. (2015) find that the number of overall (non-birth) hospital visits as well as inpatient visits associated with a mental health diagnosis increase as a result of the ACA dependent coverage expansion.[25] The authors do not find evidence of a noticeable impact on hospital length of stay or number of procedures. Barbaresco et al. (2015) find an increase in the probability of having a personal doctor as a result of the ACA dependent coverage expansion. However, they did not find any significant increase with respect to preventive care utilization. Chua and Sommers (2014) do not find any impact of the ACA dependent coverage expansion on inpatient or outpatient utilization.

There is less work considering the impact of dependent coverage expansions on health outcomes or risky behaviors. In terms of health outcomes, Chua and Sommers (2014) show significant increases in excellent self-reported mental and physical health as a result of the ACA

[25] Rather than focusing on gains in coverage, Anderson et al. (2012 and 2014) examine the consequences of young adults "aging off" (age 19 and age 23 for students) of their parents' insurance plans and find a 61 percent reduction in inpatient hospital admissions.

dependent coverage expansion. Barbaresco et al. (2015) find a statistically significant increase in self-reported excellent health, but no significant changes in mental health, physical health, or functional limitations. With respect to risky behaviors, Barbaresco et al. (2015) find mixed results, with increases in binge drinking, along with decreases in BMI (body mass index).

C. Other Types of Coverage Expansions and Avoidable Hospitalizations

Billings and Teicholz (1990) first developed the concept of using avoidable (or ambulatory care sensitive (ACS)) hospitalizations as an indirect indicator of problems associated with primary care quality and access to care. The idea here is that certain hospitalizations could be avoided if the patient has access to high quality primary care. Thus, this approach allows researchers to use hospital discharge data, which is readily available, to assess ambulatory care quality.

Dafny and Gruber (2005) use such an approach to investigate the impact of the Medicaid expansions of the 1980s and 1990s on low income children made newly eligible for public coverage using data from the National Hospital Discharge Survey. They find that total hospitalizations increase significantly as a result of these coverage expansions. A decomposition of all hospital stays into those that are avoidable versus those that are unavoidable suggests that the increase for unavoidable hospitalizations is much larger than that for avoidable hospitalizations. In addition, the increase in avoidable hospitalizations they estimate is not statistically significant. They take this as evidence that there is an "efficiency" effect associated with expanding coverage, but that this efficiency effect is dominated by the "access" effect.

In the study about the impact of a Medicaid outreach program in California the late 1990s, Aizer (2007) tests the hypothesis that families responding to outreach efforts will sign their children up before they get sick, improving their access to outpatient care, and reducing their number of avoidable hospitalizations. Using California Medicaid administrative enrollment and claims data, she finds that increases in Medicaid take up resulted in lower hospitalization rates for avoidable conditions, but not others.

Using the HCUP NIS hospital discharge data from 2004 to 2008, KK examine the impact of the 2006 Massachusetts health insurance reform on avoidable hospitalizations for non-elderly adults. After controlling for illness severity, the authors estimate a statistically significant, negative impact of the Massachusetts reform on avoidable hospitalizations. They attributed the reduction in such hospitalizations to patients with less severe medical problems. Unlike the previous papers described here, KK employ avoidable hospitalization definitions developed by the AHRQ specifically for this type of analysis.

Taken as a whole, evidence from the literature suggests that expanding insurance coverage leads to more primary care utilization. Dafny and Gruber (2005), Aizer (2007), and KK all hypothesize that this could lead to a reduction in the need for avoidable hospitalizations. Dafny and Gruber (2005) refer to this as the efficiency effect. On the other hand, an increase in insurance coverage could lead to more hospitalizations as the price of hospital care falls. Dafny and Gruber (2005) call this the access effect. Both Aizer (2007) and KK find reductions in avoidable hospitalizations among the different populations gaining coverage in their studies. Their findings suggest that the efficiency effect dominates the access effect. Conversely, Dafny and Gruber (2005) find an increase in avoidable hospitalizations, though not as large as for total hospitalizations among children gaining Medicaid coverage in the mid-1980s through mid-1990s. Therefore, it is not obvious which effect would dominate for young adults gaining coverage through the ACA dependent coverage expansion. The conceptual model described in the next section formalizes this discussion and introduces moral hazard as a third potential channel through which insurance expansions can impact avoidable hospitalization demand.

III. Conceptual Model

Here I derive a conceptual model of avoidable hospitalizations that will guide my empirical work. I posit that the probability of an avoidable hospitalization is a function f of the price of an avoidable hospital stay (P_{AV}) and the consumer's health status H, so $Prob(AV) = f(P_{AV}, H)$. Further, I assume that the price of an avoidable hospital stay is a function of the ACA mandate or law (L) and that the consumer's health status is a function of their primary care consumption (PC) and their engagement in risky behaviors (B):

$$Prob(AV) = f(P_{AV}(L), H(PC, B)) \tag{1}$$

Primary care consumption (PC) is going to depend on the price of primary care (P_{PC}), which itself is a function of the ACA (L), and risky behaviors (B) are going to depend on the price of avoidable hospitalizations (P_{AV}), which itself is also a function of the ACA (L). Putting this all together gives me the following equation:

$$Prob(AV) = f\{P_{AV}(L), H\left(PC\big(P_{PC}(L)\big), B(P_{AV}(L))\right)\} \tag{2}$$

Since I am interested in the impact of the ACA dependent care coverage expansion on avoidable hospitalizations, I take the derivative of this function with respect to L:

$$\frac{dAV}{dL} = \frac{df}{dP_{AV}} * \frac{dP_{AV}}{dL} + \frac{df}{dH} * \left(\frac{dH}{dPC} * \frac{dPC}{dP_{PC}} * \frac{dP_{PC}}{dL} + \frac{dH}{dB} * \frac{dB}{dP_{AV}} * \frac{dP_{AV}}{dL}\right) \tag{3}$$

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Below each term I include my assumption about its sign based largely on the literature described in the previous section.

Since the ACA dependent care expansion increased insurance coverage among young adults, the partial derivatives of the change in health care prices with respect to the law should all be negative, so $\frac{dP_{AV}}{dL}$ and $\frac{dP_{PC}}{dL} < 0$. The law of demand suggests that as health care prices fall, we would expect health care consumption to increase, implying that $\frac{df}{dP_{AV}}$ and $\frac{dPC}{dP_{PC}} < 0$. These assumptions imply that the first term on the right hand side of equation (3) is positive and can be thought of as the **access effect**. The access effect suggests that as young adults gain insurance coverage and face lower prices for avoidable hospitalizations, their demand for avoidable hospitalizations will increase.

The second term on the right hand side formalizes two additional channels through which a coverage expansion can influence avoidable hospitalization consumption. The first channel is the **efficiency effect**. It suggests that as the price of primary care falls, young adults will consume more primary care ($\frac{dPC}{dP_{PC}} < 0$).[26] This in turn is assumed to improve their health ($\frac{dH}{dPC} > 0$) and reduce their demand for avoidable hospitalizations ($\frac{df}{dH} < 0$).[27] As mentioned in the previous section, both Aizer (2007) and KK find that the efficiency effect dominates the access effect since they estimate overall reductions in avoidable hospitalizations among the populations they study.

[26] Several studies have found that insurance expansions increase primary care consumption, including Manning et al. (1987), Currie and Gruber (1996a), Lichtenberg (2002), Card et al. (2008), and Finkelstein et al. (2012).

[27] Does more primary care really improve health? This can be a difficult question to answer with respect to insurance expansions as such expansions may increase primary care consumption, while at the same time also potentially increasing risky behavior, and both will affect health outcomes. The literature on the ACA dependent coverage example discussed in the previous section of this paper suggests mixed findings with respect to health outcomes. Brook et al. (1983) find that free care improves cholesterol levels, mental and physical health in certain sub-groups in the RAND health insurance experiment. Several studies suggest that Medicaid expansions reduce mortality and increase self-assessed overall, mental and physical health, while having no statistically significant effects on laboratory-measured health outcomes (Currie and Gruber, 1996b; Finkelstein et al., 2012; Sommers et al., 2012; Baicker et al., 2013). The Medicare program has been estimated to decrease mortality rates for Medicare inpatients (Card et al., 2009), but no significant impact of Medicare on the mortality rate for the elderly in general has been found (Finkelstein and McKnight, 2008). Unanimous evidence from the 2006 Massachusetts health insurance reform shows increases in self-assessed overall, mental and physical health, and decreases in functional limitations, joint disorders and mortality (Van der Wees et al., 2013; Courtemanche and Zapata, 2014; Sommers et al., 2014).

The other channel captured by the second term on the right hand side of equation (3) can be thought of as representing ***ex ante* moral hazard** (Ehrlick and Becker, 1972) through the term B in two ways. First, a reduction in the price of avoidable hospitalizations could lead to an increase in risky behaviors such as drinking and smoking. Second, a reduction in the price of avoidable hospitalizations could lead to a reduction in the demand for health promoting activities, such as flu vaccinations or smoking cessation program participation. Such behavior suggests $\frac{dB}{dP_{AV}} < 0$ and I assume that an increase in risky behavior leads to a reduction in health $\frac{dH}{dB} < 0$. The moral hazard effect would thus be predicted to lead to an increase in avoidable hospitalizations.

As this channel was not explicitly mentioned in the previous literature on avoidable hospitalizations, more discussion is warranted. First, is there evidence that reductions in the price of avoidable hospitalizations lead to increases in risky behaviors and reductions in health promoting activities? The empirical literature on these topics is mixed. Neither the RAND health insurance experiment nor the Oregon Medicaid study found a significant impact of insurance coverage on smoking or body weight (Brook et al., 1983; Finkelstein et al., 2012). While Dave and Kaestner (2009) find increases in smoking and drinking, and decreasing physical activity, associated with enrolling in the Medicare program, none of the effects are significant. Courtemanche and Zapata (2014) find no evidence on smoking or physical activity as a result of the 2006 Massachusetts health insurance reform, though they do find a significant reduction in body mass index.[28] As mentioned, Barbaresco et al. (2015) find mixed results for risky behaviors with significant improvement in BMI, but increase in binge drinking.

[28] Body mass index is a proxy of poor diets and sedentary lifestyles, and has been broadly used as one of the risky behaviors in the literature. However, it might not fully satisfy the narrow definition here as it can be affected through

As for the impact of risky behaviors on health, Mcginnis and Foege (1993) find in their influential study that half of the deaths in the United States in 1990 are from external modifiable risk behaviors. More recent studies by Mokdad et al. (2004 and 2005) also show similar results for the U.S. in 2000: smoking, diet, physical activity, and drinking are the main risky behaviors leading to death. Danaei et al. (2009) break dietary behavior into detailed categories and find high body mass index, physical inactivity, and high blood glucose are the three main risk factors leading to death, followed by a list of dietary risk factors. In general, the literature supports the notion that risky behaviors have an impact on health. However, young adults maybe more immune to the health effects of risky behaviors as compared to the elderly or to children.

Taken together, the evidence on these terms suggests that there may be a moral hazard effect associated with increased insurance coverage, which would lead to a higher demand for avoidable hospitalizations. My conceptual model predicts that the efficiency effect would lead to a reduction in avoidable hospitalizations, while the access effect and the moral hazard effect lead to increases in avoidable hospitalizations. Thus the overall effect is ambiguous, reinforcing the need to analyze this issue empirically.

IV. Methodology

I use a difference-in-differences strategy to examine the impact of the ACA dependent coverage expansion on the prevalence of avoidable hospitalizations among the treatment group of young adults relative to the control group of slightly older young adults before and after the mandate's implementation in late September of 2010. Because the group targeted by the mandate is 19-to-25 year olds, most previous studies on the ACA dependent coverage mandate use an age

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other channels (such as being suggested or reminded by the doctor during each physician visit) than the pure price effect of avoidable hospitalizations.

range of 19-25 to define their treatment group and typically use older young adults (sometimes including those as old as 34) as their control group.

The key identifying assumption in any difference-in-differences model is the assumption that both the treatment and control groups would have experienced the same changes in outcomes in the absence of the intervention of interest. Slusky (2013) calls into question the validity of the "common trends" assumption with respect to labor market outcomes for young adults in the age rage typically used in the literature. He replicates previous studies with "placebo" treatment dates occurring several years prior to the implementation of the mandate and finds significant "effects". This suggests that previous studies may be mistakenly attributing changes in young adult insurance coverage to the ACA that are actually driven by dynamics in the age structure of insurance and labor markets. He finds more reliable estimates after reducing the age bandwidth associated with the treatment group.

Like Barbaresco et al. (2015), I address this concern by defining my treatment group as young adults aged 23 to 25 and the control group as young adults aged 27 to 29.[29] Slusky's concerns are arguably less important for avoidable hospitalizations than they are for labor market outcomes, since avoidable hospitalizations are likely less directly impacted by cyclical economic fluctuations. In addition, narrowing the age bandwidth associated with the treatment and control groups, as done here and in Barbaresco et al. (2015), should also reduce the impact of any differential economic shocks. Finally, relative to other studies, I use a longer pre-reform period (starting from 2002) in my analysis to better test for differences in pre-reform trends between the treatment and control groups.

Formally, I estimate the following equation:

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[29] I follow the previous literature and exclude young adults aged 26, as it is difficult to determine whether or not the mandate is binding for them. It would be a function of their birthdate and the start date of their parents' insurance plan for the year.

$$Y_{ight} = \beta_0 + \beta_1(Treat_g * After_t) + \boldsymbol{X}'_{ight}\beta_2 + \theta_g + \varphi_t + \sigma_h + \varepsilon_{ight}, \qquad (4)$$

where Y is a dummy variable equal to one if hospital discharge i is considered an avoidable hospitalization generated by a patient of age g in hospital h at time t. The primary parameter of interest is denoted by β_1. It measures the effect of the mandate after implementation on the targeted age group. $Treat_g$ is a dummy variable equal to one for any discharge generated by a patient in the age range of 23-25 (the treatment group). $After_t$ is a dummy variable equal to one for any discharge occurring in a time period t that is after the implementation of the ACA mandate (October 2010 or later). The vector X' includes a set of patient demographic characteristics and a set of risk adjusters to control for patient illness severity. The terms θ_g, φ_t and σ_h capture separately age, time, and the hospital fixed effects. Finally ε_{ight} represents the error term. In my estimation, I use heteroskedasticity-robust standard errors clustered at the treatment level of the interaction of age-by-time.[30] NIS sampling weights, discussed below, are used in the analysis.

To verify the validity of my findings, I perform several placebo regressions using treatment dates occurring several years prior to the implementation of the mandate as in Slusky (2013). I also perform multiple additional robustness checks. The first two checks re-estimate equation (4) with shorter pre-reform time frames (15 and 23 quarters versus 35) to verify that my results are not driven by my chosen length of the pre-reform period. The third check excludes the time period of April 2010 to September 2010 (Q2 2010 – Q3 2010), which is the time period between when the law passed and its effective date, to avoid ambiguity about the treatment status of hospitalized young adults during this period.

[30] The estimated standard errors are similar when they are clustered on age alone. I prefer using an interaction of age-by-time as it gives more clusters (Angrist and Pischke, Chapter 8).

V. Data

The dataset used for this analysis is the Nationwide Inpatient Sample (NIS), which is part of the Healthcare Cost and Utilization Project (HCUP) administered by the Agency for Healthcare Research and Quality (AHRQ). Each year of the NIS is a stratified sample of 20 percent of community hospitals in the U.S. and is nationally representative of all community hospitals.[31] If a hospital is sampled in a given year, it provides the universe of its discharges for that year, regardless of payer. As in KK, I take advantage of the fact that a large fraction of hospitals are sampled in each year to identify within hospital changes over time.

The NIS is a good data source to examine the impact of health insurance coverage reforms since it has complete payer information for each discharge. Detailed information on diagnoses and patients' point of admission (directly admitted or transferred from other facilities) allow me to create indicators for avoidable hospitalizations. One weakness of this data is that it only consists of hospitalized patients, which may introduce a selection problem with illness severity into the analysis. I use several patient-level risk adjusters to control for this problem.

The years I use for this analysis range from 2002 to 2011 (the most recent year available).[32] Since the mandate was implemented in late September 2010 and NIS is a quarterly data, I define the time from the first quarter of 2002 to the third quarter of 2010 as the pre-reform period, and from the fourth quarter of 2010 to the fourth quarter of 2011 as the post-reform period. My sample starts with 4,813,849 discharges from the NIS for young adults aged 23-29 over the 2002-2011 period of analysis. After excluding discharges with missing values for key

[31] One caveat to note is that not every state participates in this endeavor. By 2011, there are 46 states reporting data to the HCUP database. Data from Alabama, Delaware, Idaho, and New Hampshire are not available in any year because they did not provide data to the NIS. Other states report incomplete data. I exclude the states of California, Maine and Texas from the analysis because detailed age information for patients is not available.
[32] The AHRQ redesigned the NIS sampling strategy in 2012. The new NIS is a sample of discharges from all hospitals participating in HCUP, rather than all discharges from a sample of participating hospitals, as in previous years. A consistent hospital identifier, allowing researchers to control for hospital fixed effects, will no longer be available.

variables (age, gender, principal diagnosis, quarter, year, and hospital), my sample is reduced to 3,845,814 discharges. A total of 3,363,241 discharges occur in the pre-reform time period and 482,573 occur in the post-reform time period.

The main outcome I consider in this analysis is the classification of a given discharge as an avoidable hospitalization, which implies that it is a hospitalization for a condition or treatment which could have been potentially prevented by effective community outpatient / primary care or other early medical intervention. Thus avoidable hospitalizations serve as a proxy for primary care quality. Such a hospitalization is also referred to as an ambulatory care sensitive (ACS) hospital admission.

One issue associated with this literature is that the definition of an avoidable hospitalization is often ad hoc and can differ from study to study. Given that I am using data from the AHRQ, I follow KK and use the AHRQ methodology for identifying avoidable hospitalizations. This methodology identifies twelve separate conditions / treatments considered to be avoidable for adults, such as an inpatient stay due to dehydration or uncontrolled diabetes.[33] The AHRQ provides software that creates flags for each of the twelve conditions / treatments, which they call Prevention Quality Indicators (PQIs).[34]

Table 2.1 provides the summary statistics of the pre- and post-reform and corresponding difference-in-differences calculations for all of the PQI avoidable hospitalization indicators, as well as AHRQ generated composites for acute and chronic PQIs, and an overall composite. The definition of the acute composite indicator, PQI 91, is the union of PQI indicators 10, 11, and 12

[33] Actually, this methodology identifies fourteen conditions, but I am not considering COPD / asthma admissions among older adults or low birth weight admissions.

[34] The AHRQ software generates these PQIs based on hospital discharge data by using complex algorithms. Essentially, the indicators first look for specific principal diagnoses, then exclude certain discharges based on their secondary and tertiary diagnoses. Transfers from other facilities are excluded to avoid double-counting. A diagnosis of pregnancy, if necessary, is also excluded in certain PQIs. For more information on the AHRQ PQI methodology, see: http://www.qualityindicators.ahrq.gov/Modules/PQI_TechSpec.aspx

(dehydration, bacterial pneumonia, and urinary tract infections). Similarly, the definition of the chronic composite indicator, PQI 92, is the union of PQI indicators 1, 3, 7, 8, 13, 14, 15, and 16 (short-term and long-term diabetes complications, hypertension, congestive heart failure, angina, uncontrolled diabetes, adult asthma, and lower-extremity amputation). Thus it includes all PQIs except the previously defined acute indicators and PQI 2 (perforated appendix), because it has a different denominator. Finally, the overall PQI indicator (PQI 90) is defined as the union of all of the individual indicators except PQI 2.

The first row of table 2.1 suggests that in the pre-reform time period, the probability of a discharge among a young adult in the treated group being avoidable is 3.48 percent while the probability of a discharge among a young adult in the control group being avoidable is 3.55 percent. There is also a slightly lower probability of a discharge being chronic avoidable for young adults in the treated group than in the control group (1.81 vs 1.94 percent) in the pre-reform time period. For the acute PQI composite, the probability of a discharge being acute avoidable among the treated group is 1.67 percent, while it is 1.61 percent in the control group. For the twelve individual PQI indicators, most discharges have a slightly higher probability of being avoidable in the control group in the pre-reform time period, except short-term diabetes (PQI 1) and urinary tract infections (PQI 12).

The simple difference-in-differences calculations presented in the last column of table 2.1 compare the changes of the mean probability for the treatment relative to control group in the pre- and post-reform periods, showing statistically significant increases in the overall PQI composite, the chronic PQI composite, as well as the PQIs for short-term diabetes complications (PQI 1), congestive heart failure (PQI 8), dehydration (PQI 10), angina without a procedure (PQI 13), and uncontrolled diabetes (PQI 14). The calculations also show statistically significant decreases in PQI 7 and 16. This is suggestive evidence that the ACA dependent coverage expansion may have led to an increase in avoidable hospitalizations.

Figure 1 shows trends in the probability that a given discharge is an overall, acute or chronic avoidable hospitalization separately for the treatment and control groups. The figures show similar trends for both groups before mandate, indicating that time-variant changes in observables and unobservables may not differ substantially between the two groups. This provides further support for implementing a difference-in-differences analysis.

In order to isolate the impact of the ACA dependent coverage mandate, I include in my regression analysis a set of demographic control variables. These are dummy variables for each year of age, gender, race/ethnicity, and patient's zip code in income quartile. In addition, I include the quarterly state unemployment rate, from the Bureau of Labor Statistics, to control for state level economic conditions. Following KK, I also utilize a set of risk adjusters to control for patient disease severity. These risk adjusters include the number of diagnoses on the discharge record, AHRQ comorbidity dummies for different diseases, All-Patient Refined Diagnosis Related Groups (APR-DRGs) classification, the APR-DRG severity of illness score, and the APR-DRG risk of mortality score.[35] All the risk adjusters are designed to measure some level of illness severity and are included in my discharge level regression.

Table 2.2 shows the pre-reform means and standard deviations for the demographic controls for the young adult discharges in the sample. Within both the treatment and the control group, the discharges are evenly distributed across the age categories. A larger share of discharges is generated by females (81.3 percent) than males, with similar percentages in both

[35] The number of diagnoses is calculated by counting the number of diagnoses on each discharge record. The AHRQ comorbidity dummies provide 29 categories of disease comorbidity (i.e. for congestive heart failure: 1 represents comorbidity and 0 shows comorbidity is not present). The APR-DRG related measures, developed by 3M, are used to classify patients according to their degree of potential mortality and illness severity.

the treatment group and the control group. As for race and ethnicity, discharges from whites make up a slightly lower (40.9 percent vs. 43.5 percent) share for treatment group as compared to the control group. For the patient's zip code income quartile, discharges associated with the age group 23-25 have a higher share (33.5 percent) in the two lowest quartiles, as compared to 30.6 percent among discharges from the age group 27-29.

VI. Results

A. Average Effects of the ACA Dependent Coverage Expansion on the Probability of Avoidable Hospitalizations

Table 2.3 provides the results of difference-in-differences estimation of the baseline model representing equation (4) (left panel) and a similar model including patient risk adjusters (right panel). The baseline model suggests a 0.12 percentage point increase in the probability of a discharge being avoidable, which represents a 3.4 percent increase compared to the baseline rate of avoidable hospitalizations.[36] The composite indicator for chronic avoidable hospitalizations also shows a significant increase of 0.09 percentage points, which represents a 5 percent increase. The coefficient on the composite acute indicator, although not significant, is also positive.

Additionally, table 2.3 lists results for each individual PQI indicator. Among the twelve individual indicators in the baseline model, five of them suggest statistically significant increases in a discharge being associated with that particular avoidable admission (short-term diabetes complications, congestive heart failure, dehydration, angina, and uncontrolled diabetes); two exhibit statistically significant reductions (hypertension and lower-extremity amputation) and the remaining five have no statistical significance.

[36] Compared to the pre-reform treatment mean of 3.48 percent, the increase of 0.12 percentage point represents an increase of 3.4 percent.

To control for potential changes in the patient population in the post-reform time period, I estimate the same model with risk adjusters, where I use severity of disease to control for observable changes in the health status of the patient pool. These results are presented in the right panel of table 2.3. The estimates are similar to those generated by the baseline model, with slightly higher effects associated with overall (4.9 percent increase vs. 3.4 percent) and chronic avoidable hospitalizations (7.7 percent increase vs. 5 percent). This suggests that the illness severity of the inpatient population for young adults did not change much after the ACA mandate, which may due to the fact that young adults are relatively healthy in general.

B. Placebo Tests

In order to test the validity of the difference-in-differences results presented in the previous sub-section, I estimate a series of four placebo tests that use artificial effective dates within the pre-reform period as in Slusky (2013). Following previous studies (Antwi Akosa et al., 2014 and 2015; Barbaresco et al., 2015) which use a five-year period for their primary analyses, I use five-year windows pre-reform for my placebo tests spanning 2005-2009, 2004-2008, 2003-2007, and 2002-2006.[37] In my baseline model, there are five quarters in the post-reform time period, so I also use five quarters as the length of my artificial post-reform time period in each placebo test (e.g. the fourth quarter of 2008 is the start of the artificial post-reform time period for the 2005-2009 placebo test). I estimate a specification similar to my baseline model for all of the PQIs in each of the four placebo tests.

Table 2.4 reports the estimates from these tests. Fifteen PQI regressions in each of the four sets of placebo tests generate a total of 56 regressions. Theoretically, a small number of significant results are expected due to the large number of regressions. Around one estimate is

[37] In unreported placebo tests (available upon request), I estimate another five placebo tests with varying time windows of 2002-2009 (8 years), 2002-2008 (7 years), 2002-2007 (6 years), 2002-2006 (5 years), and 2002-2005 (4 years). The results are similar in terms of the number of significant estimates.

expected to be significant at the 1 percent level, three at the 5 percent level, and six at the 10 percent level by chance. The number of significant results reported in table 2.4 is 0 at the 1 percent level, four (6.7 percent) at the 5 percent level, and eight (13.3 percent) at the 10 percent level. Note that one particular PQI, PQI 13 (angina), accounted for three of the eight significant results. Dropping PQI 13 from the definitions of the overall PQI avoidable hospital indicator and the chronic composite indicator does not lead to major changes in my primary results. Overall, these placebo tests suggest that my primary difference-in-differences approach is sound and there does not appear to be any sustained differential pre-reform trends between the treatment and control groups. Moreover, these placebo test results also suggest that the standard errors, which are clustered at the age-by-time level, are not meaningfully understated.

C. Robustness Checks

Here I describe the results of multiple robustness checks that are presented in table 2.5. For ease of comparison, the first column of table 2.5 re-states my baseline results. Columns two and three restrict the period of analysis to 2007-2011 and 2005-2011 respectively. In each case the estimated impact of the ACA dependent coverage expansion on the likelihood that a young adult discharge is avoidable is very similar in terms of magnitude and statistical significance. The coefficient estimate in the baseline model suggests a 0.12 percentage point increase, while the coefficient estimate in the 2007-2011 (2005-2011) model suggests a 0.11 (0.12) percentage point increase. The results are similar for both the chronic and acute composite PQI indicators. This suggests my baseline results are not being driven by the length of the pre-reform period.

The next robustness check, presented in column four, drops the time period between the passage of the ACA and its dependent coverage expansion implementation date, which I define as the second and third quarters of 2010. As above, making this change does not impact the coefficient estimates in a major way.

D. Heterogeneity Tests

Having verified the validity of my empirical model and estimated the average effects of the ACA dependent coverage expansion, I next present the results of models that allow for heterogeneous effects for different sub-groups in my sample. There may be differences by gender or race in response to gaining insurance coverage. In addition, differences in socioeconomic status may also lead to different responses. Tables 6 and 7 present the results from heterogeneity regressions based on gender, race and patient's zip code income quartile.

The first two columns of table 2.6 illustrate differences by gender. These results suggest that the statistically significant increases in the probability of an overall avoidable hospitalization (PQI 90) or a chronic avoidable hospitalization (PQI 92) in my baseline model are being driven by young females, rather than young males. Young men do statistically significantly reduce their probability of a hospitalization for hypertension (PQI 7) and extremity amputation (PQI 16) after gaining coverage, but increase their probability of a hospitalization for heart failure (PQI 8).

The next three columns of table 2.6 present differences by race. Black, Hispanic, Asian, Native American and other races compose 28 percent of the sample and are grouped together as non-white. The remaining sample is classified as either unknown race (30 percent) or white (42 percent). These results suggest that the statistically significant increases in the probability of an overall avoidable hospitalization (PQI 90) or a chronic avoidable hospitalization (PQI 92) in my baseline model are being driven by whites, rather than non-whites or those with unknown race. Although I find no statistically significant impact on acute avoidable hospitalizations (PQI 91) in my baseline model, table 2.6 suggests that the ACA dependent coverage expansion lead to an increase in the probability of having such a hospitalization for non-whites.

Table 2.7 presents heterogeneity model results based on patient's zip code income quartile.

These results suggest that the statistically significant increase in the probability of an overall avoidable hospitalization (PQI 90) in the baseline model are being driven by patients coming from zip codes with income that fall in the second or third income quartile of the distribution. The increase in the probability of a chronic avoidable hospitalization (PQI 92) in the baseline model is being driven by patients coming from zip codes with income that fall in the third income quartile of the distribution. Taken together, this heterogeneity analysis suggests that there are important differences by gender, race, and income in response to gaining insurance through the ACA dependent coverage mandate.

VII. Discussion

The overall increase in the probability of avoidable hospitalizations suggested by my empirical analysis implies that the access effect and the moral hazard effect dominate the efficiency effect for young adults gaining coverage through the ACA dependent coverage expansion. This is broadly consistent with the finding in Antwi Akosa et al. (2015) that ACA dependent coverage expansion increases non-birth hospital admissions and admissions associated with a mental health diagnosis. I find some evidence of an efficiency effect for young adult avoidable hospitalizations as there are two individual indicators (hypertension and extremity amputation) that show reductions in probability after the ACA mandate. This echoes the results found in Dafny and Gruber (2005) for children gaining Medicaid coverage. Among those children there was some evidence of an efficiency effect, but this was dominated by the access effect. On the other hand, Aizer (2007) finds that when eligible, but not enrolled children formally enroll in Medicaid coverage in California they experience a reduction in avoidable hospitalizations. This would suggest the efficiency effect dominates.[38]

Relative to my results, studies from the Massachusetts health insurance expansion tell a different story for older (non-elderly) adults. KK find that the Massachusetts reform leads to a reduction in the probability of avoidable hospitalizations, which they implicitly attribute to the efficiency effect dominating the access effect. This suggests that the older adults targeted by the reform responded by increasing their primary care consumption, thus reducing their rate of avoidable hospital stays. The difference in findings for young adults from ACA expansion and older (non-elderly) adults from Massachusetts reform may due to several potential reasons:

- **Information or experience:** Gaining health insurance coverage may lead to reductions in avoidable hospitalizations (i.e. the efficiency effect dominates), but that requires the newly insured to seek out and receive appropriate primary care. Young adults gaining coverage through the ACA dependent coverage expansion may not have enough experience with the health care system to successfully find such primary care services. Older adults are more likely to have this needed experience.
- **Risk attitudes:** Additionally, these older adults may be more risk averse than young adults, as they may realize that their overall health is no longer as good as when they

[38] Why do the results from Dafny and Gruber (2005) and Aizer (2007) regarding avoidable hospitalizations for children newly enrolled in Medicaid vary? One possible explanation is that Dafny and Gruber (2005) focus on children made newly eligible for Medicaid, while Aizer (2007) focuses on already eligible children who are now formally taking up Medicaid coverage. Presumably, children made newly eligible for Medicaid did not have a previous source of coverage for hospital or primary care. On the other hand, families of children who are eligible, but not formally enrolled in Medicaid may understand that hospital care would still be covered by Medicaid, as the hospital likely has experience assisting such families in the Medicaid enrollment process. This is less likely to be true with respect to primary care. Therefore, one could consider eligible, but not formally enrolled children as having “conditional hospital coverage” but not “conditional primary care coverage”. Thus the children analyzed in Aizer (2007) experienced a greater increase in access to primary care as compared to hospital care. This increase in primary care access could explain why avoidable hospitalizations for this particular group of children fall.

were younger. Older adults may also need to protect themselves more diligently so that certain infectious disease (such as the flu) will not affect their family members. Therefore, even though the price of hospital care decreases due to expansions in insurance coverage, non-elderly adults do not want to face the risk of being hospitalized and so make sure they consume the necessary primary care.

- **Income constraints and Moral Hazard:** For financial reasons, young adults may be more likely than the older adults to forgo insurance coverage and instead focus on lower cost interventions such as flu vaccines and over-the-counter medications. However, receiving insurance coverage alleviates the financing constraint, and as a result, young adults may engage in more risky behavior or invest less in their health, such as increasing binge drinking (Barbaresco et al., 2015). In other words, the *ex ante* moral hazard effect of obtaining coverage may be stronger for young adults than other adults.

On the other hand, dependent health insurance coverage may also increase young adults' disposable income, as some of them may no longer have to pay their own insurance premium. They may use this "extra" income to consume goods with adverse health consequences, such as cigarettes and alcohol. Barbaresco et al. (2015) show an increase in risky drinking; increases in drinking may lead to heart disease and diabetes in the long-run.

This discussion illustrates the benefits of using a conceptual model to think about how the impact of gaining coverage might differ for individuals of different ages. While my results might seem at first glance to contradict the results from Massachusetts, there are several plausible reasons why we might expect young adults to respond differently to a gain in insurance coverage than older adults.

VIII. Conclusion

A typical hospitalization may be characterized as an unavoidable because there is nothing that could have been done medically to avoid the stay, such as suffering a major injury in a car accident. In this paper I investigate whether or not there were changes in the probability of having an avoidable hospitalization – one that could have been prevented by the receipt of timely and appropriate primary medical care – among young adults gaining health insurance coverage through ACA dependent coverage expansion which was implemented in September 2010. Though several previous studies have examined the impact of coverage expansions on hospital utilization, there are many reasons why we might expect young adults to potentially respond differently than older adults or children. To answer this question I use HCUP NIS hospital discharge data and AHRQ avoidable hospitalization definitions to estimate a difference-in-differences model with a narrow age bandwidth of age 23-25 as the treatment group and age 27-29 as the control group. The results shown in the baseline model for the entire sample indicate increases in the probability of having any avoidable hospitalization as well as the chronic composite, but no clear effects on the acute composite index.

Specifically, the ACA dependent coverage mandate leads to an increases in the probability of PQI 1 (short-term diabetes), PQI 8 (congestive heart failure), acute PQI 10 (dehydration), PQI 13 (angina), and PQI 14 (uncontrolled diabetes). At the same time, I estimate decreases in the probability PQI 7 (hypertension) and PQI 16 (lower-extremity amputation). Controlling for patient illness severity does not lead to major changes in these results. I then utilize several placebo regressions with pre-reform periods to validate the model with a narrow age range treatment group. Next I implement four robustness checks to confirm the effects shown in the baseline model are not driven by my choice of the length of the pre-reform period in my analysis. Finally, I estimate the model on sub-samples of different gender, race, and zip code income quartiles. There are important differences by gender, race, and income in response to the ACA dependent coverage mandate.

Table 2.1 – Means and Standard Deviations for Outcome Variables

Quality Indicators	Pre-reform Period Treatment (Ages 23-25)	Pre-reform Period Control (Ages 27-29)	Post-reform Period Treatment (Ages 23-25)	Post-reform Period Control (Ages 27-29)	Difference-in-Difference
Overall Prevention Quality Indicators					
PQI 90 Overall Composite	0.0348 (0.1832)	0.0355 (0.1851)	0.0381 (0.1916)	0.0368 (0.1883)	0.0021 (0.0012)*
PQI 91 Acute Composite	0.0167 (0.1281)	0.0161 (0.1260)	0.0165 (0.1274)	0.0154 (0.1230)	0.0006 (0.0006)
PQI 92 Chronic Composite	0.0181 (0.1332)	0.0194 (0.1379)	0.0216 (0.1455)	0.0214 (0.1448)	0.0015 (0.0008)*
Individual Component Measures of Prevention Quality Indicators					
PQI 01 Diabetes short-term comp.	0.0079 (0.0885)	0.0067 (0.0814)	0.0108 (0.1033)	0.0085 (0.0917)	0.0011 (0.0005)**
PQI 02 Perforated appendix	0.1717 (0.3771)	0.1748 (0.3798)	0.1763 (0.3812)	0.1853 (0.3887)	-0.0059 (0.0127)
PQI 03 Diabetes long-term comp.	0.0020 (0.0446)	0.0030 (0.0550)	0.0027 (0.0523)	0.0039 (0.0622)	-0.0001 (0.0002)
PQI 07 Hypertension	0.0005 (0.0222)	0.0009 (0.0305)	0.0005 (0.0234)	0.0012 (0.0348)	-0.0002 (0.0001)**
PQI 08 Congestive heart failure	0.0009 (0.0303)	0.0015 (0.0390)	0.0009 (0.0307)	0.0013 (0.0356)	0.0003 (0.0001)**
PQI 10 Dehydration	0.0041 (0.0642)	0.0043 (0.0655)	0.0034 (0.0585)	0.0032 (0.0561)	0.0005 (0.0002)**
PQI 11 Bacterial pneumonia	0.0053 (0.0728)	0.0058 (0.0757)	0.0059 (0.0752)	0.0060 (0.0772)	0.0001 (0.0005)
PQI 12 Urinary tract infection	0.0072 (0.0847)	0.0061 (0.0776)	0.0074 (0.0857)	0.0062 (0.0786)	0.00001 (0.0003)
PQI 13 Angina without procedure	0.0001 (0.0096)	0.0002 (0.0134)	0.0001 (0.0081)	0.0001 (0.0101)	0.0001 (0.00003)*
PQI 14 Uncontrolled diabetes	0.0008 (0.0277)	0.0009 (0.0299)	0.0008 (0.0280)	0.0007 (0.0272)	0.0002 (0.0001)**
PQI 15 Asthma in younger adults	0.0059 (0.0767)	0.0062 (0.0782)	0.0058 (0.0756)	0.0057 (0.0753)	0.0003 (0.0005)
PQI 16 Lower-extremity amputation	0.00003 (0.0053)	0.0001 (0.0082)	0.00002 (0.0042)	0.0001 (0.0113)	-0.0001 (0.00003)**
Sample Size	1,620,088	1,743,153	225,861	256,712	--

Notes: Means are reported, with standard deviations in parentheses. Standard errors, heteroskedasticity-robust and clustered at the age-by-time level, are in parentheses for difference-in-differences calculations. NIS sampling weights are used. *** indicates the difference-in-differences is significant at the 1% level; ** 5%; * 10%.

Table 2.2 – Pre-reform Means and Standard Deviations for Control Variables

Control Variables	Total (Ages 23-29)	Treatment (Ages 23-25)	Control (Ages 27-29)
Age dummies (age=23 is omitted)			
Age=24	0.161 (0.367)	0.334 (0.472)	--
Age=25	0.164 (0.371)	0.341 (0.474)	--
Age=27	0.173 (0.378)	--	0.333 (0.471)
Age=28	0.173 (0.378)	--	0.333 (0.471)
Age=29	0.173 (0.378)	--	0.334 (0.471)
Female	0.813 (0.390)	0.813 (0.390)	0.812 (0.391)
Race/ethnicity dummies (non-Hispanic white is omitted)			
Black	0.130 (0.336)	0.139 (0.346)	0.121 (0.327)
Hispanic	0.089 (0.285)	0.094 (0.291)	0.085 (0.279)
Asian	0.018 (0.131)	0.015 (0.121)	0.020 (0.140)
Native American	0.006 (0.079)	0.007 (0.081)	0.006 (0.077)
Other than black, Hispanic, Asian, Native, or white	0.036 (0.185)	0.035 (0.184)	0.036 (0.186)
Unknown Race	0.299 (0.458)	0.301 (0.459)	0.297 (0.457)
Patient's Zip Code in Income Quartile dummies (First (Lowest) is omitted)			
Second Income Quartile	0.154 (0.361)	0.158 (0.365)	0.150 (0.357)
Third Income Quartile	0.133 (0.340)	0.127 (0.333)	0.139 (0.346)
Fourth Income Quartile	0.099 (0.299)	0.084 (0.277)	0.113 (0.317)
Unknown Income	0.448 (0.497)	0.454 (0.498)	0.442 (0.497)
State Unemployment Rate	6.059 (2.056)	6.041 (2.048)	6.077 (2.064)

Notes: Means are reported, with standard deviations in parentheses. NIS sampling weights are used.

Table 2.3 – Difference-in-Differences Estimates of Effects of ACA Dependent Coverage Mandate on Quality Indicators

Quality Indicators	Baseline Model	with Risk Adjusters	Pre-reform mean (treated group)
Overall Prevention Quality Indicators			
PQI 90 Overall Composite	0.0012 (0.0005)**	0.0017 (0.0005)***	0.0348
PQI 91 Acute Composite	0.0004 (0.0004)	0.0003 (0.0004)	0.0167
PQI 92 Chronic Composite	0.0009 (0.0004)**	0.0014 (0.0004)***	0.0181
Individual Component Measures of Prevention Quality Indicators			
PQI 01 Diabetes short-term	0.0009 (0.0003)***	0.0010 (0.0003)***	0.0079
PQI 02 Perforated appendix	-0.0021 (0.0116)	0.0003 (0.0088)	0.1717
PQI 03 Diabetes long-term	-0.0003 (0.0002)	-0.00003 (0.0002)	0.0020
PQI 07 Hypertension	-0.0003 (0.0001)***	-0.0003 (0.0001)***	0.0005
PQI 08 Heart failure	0.0002 (0.0001)**	0.0003 (0.0001)***	0.0009
PQI 10 Dehydration	0.0004 (0.0002)**	0.0005 (0.0002)***	0.0041
PQI 11 Bacterial pneumonia	0.00002 (0.0002)	-0.0001 (0.0002)	0.0053
PQI 12 Urinary tract infection	-0.00004 (0.0002)	-0.0001 (0.0002)	0.0072
PQI 13 Angina	0.00005 (0.00002)*	0.0001 (0.00003)*	0.0001
PQI 14 Uncontrolled diabetes	0.0002 (0.0001)**	0.0002 (0.0001)***	0.0008
PQI 15 Asthma (younger)	0.0001 (0.0002)	0.0003 (0.0002)	0.0059
PQI 16 Extremity amputation	-0.0001 (0.00003)***	-0.0001 (0.0002)***	0.00003
Sample Size	3,845,814[a]	3,812,595[b]	1,620,088

Notes: [a] For PQI 2, the sample size is 48,748. [b] For PQI 2, the sample size is 48,275. *** indicates the difference-in-difference is significant at the 1% level; ** 5%; * 10%. Standard errors, heteroskedasticity-robust and clustered at the age-by-time level, are in parentheses. All regressions include the controls plus age, hospital and time fixed effects. NIS sampling weights are used.

Table 2.4 – Placebo Regressions

Quality Indicators	2005-2009 Treatment 2007 Q4	2004-2008 Treatment 2006 Q4	2003-2007 Treatment 2005 Q4	2002-2006 Treatment 2004 Q4
PQI 90	0.0007 (0.0006)	0.0004 (0.0004)	0.0004 (0.0006)	0.0003 (0.0006)
PQI 91	0.0002 (0.0004)	0.0001 (0.0004)	-0.0003 (0.0004)	-0.0003 (0.0004)
PQI 92	0.0006 (0.0005)	0.0003 (0.0004)	0.0007 (0.0004)*	0.0005 (0.0004)
PQI 01	0.0002 (0.0003)	-0.0001 (0.0003)	-0.00001 (0.0003)	-0.0001 (0.0002)
PQI 02	0.0128 (0.0120)	-0.0272 (0.0120)**	0.0007 (0.0113)	0.0054 (0.0118)
PQI 03	0.0002 (0.0002)	-0.0003 (0.0002)**	0.0002 (0.0002)	0.0001 (0.0001)
PQI 07	-0.0001 (0.0001)	-0.00002 (0.0001)	0.0001 (0.0001)	0.00003 (0.0001)
PQI 08	0.0001 (0.0001)	0.0002 (0.0001)	0.0001 (0.0001)	0.0002 (0.0001)
PQI 10	0.0001 (0.0002)	-0.0002 (0.0002)	-0.0002 (0.0002)	0.00004 (0.0002)
PQI 11	0.00002 (0.0003)	0.0003 (0.0002)	0.0001 (0.0002)	-0.0001 (0.0002)
PQI 12	0.00001 (0.0003)	0.0001 (0.0002)	-0.0003 (0.0003)	-0.0002 (0.0003)
PQI 13	0.0001 (0.00002)**	0.0001 (0.00003)*	-0.00001 (0.00003)	0.0001 (0.00004)*
PQI 14	-0.0001 (0.0001)	-0.0001 (0.0001)	0.0002 (0.0001)*	-0.000003 (0.0001)
PQI 15	0.0002 (0.0003)	0.0006 (0.0003)**	0.0002 (0.0002)	0.0003 (0.0003)
PQI 16	0.00000 (0.00002)	0.00003 (0.00002)	0.00001 (0.00002)	-0.00001 (0.00002)

Notes: *** indicates significant at the 1% level; ** 5%; * 10%. Standard errors, heteroskedasticity-robust and clustered at the age-by-time level, are in parentheses. All regressions include the controls plus age, hospital and time fixed effects. NIS sampling weights are used.

Table 2.5 – Robustness Checks

Quality Indicators	Baseline Model	2007-2011	2005-2011	Drop periods 2010 Q2 - 2010 Q3
PQI 90	0.0012 (0.0005)**	0.0011 (0.0006)**	0.0012 (0.0005)**	0.0012 (0.0005)**
PQI 91	0.0004 (0.0004)	0.0004 (0.0004)	0.0005 (0.0004)	0.0004 (0.0004)
PQI 92	0.0009 (0.0004)**	0.0007 (0.0004)*	0.0008 (0.0004)*	0.0008 (0.0004)**
PQI 01	0.0009 (0.0003)***	0.0009 (0.0003)***	0.0009 (0.0003)***	0.0009 (0.0003)***
PQI 02	-0.0021 (0.0116)	-0.0021 (0.0121)	-0.0027 (0.0118)	-0.0044 (0.0118)
PQI 03	-0.0003 (0.0002)	-0.0003 (0.0002)	-0.0003 (0.0002)*	-0.0003 (0.0002)
PQI 07	-0.0003 (0.0001)***	-0.0003 (0.0001)***	-0.0003 (0.0001)***	-0.0003 (0.0001)***
PQI 08	0.0002 (0.0001)**	0.0002 (0.0001)	0.0002 (0.0001)*	0.0002 (0.0001)**
PQI 10	0.0004 (0.0002)**	0.0005 (0.0002)***	0.0005 (0.0002)***	0.0004 (0.0002)**
PQI 11	0.00002 (0.0002)	-0.0001 (0.0002)	-0.0002 (0.0002)	0.00004 (0.0002)
PQI 12	-0.00004 (0.0002)	-0.00002 (0.0003)	-0.00000 (0.0002)	-0.00001 (0.0002)
PQI 13	0.00005 (0.00002)*	0.00001 (0.00003)	0.00002 (0.00003)	0.00005 (0.00002)*
PQI 14	0.0002 (0.0001)**	0.0002 (0.0001)**	0.0002 (0.0001)**	0.0002 (0.0001)**
PQI 15	0.0001 (0.0002)	0.0001 (0.0003)	0.0002 (0.0003)	0.0001 (0.0002)
PQI 16	-0.0001 (0.00003)***	-0.0001 (0.00003)***	-0.0001 (0.00003)***	-0.0001 (0.00003)***
Sample Size	3,845,814	1,975,809	2,749,374	3,645,578

Notes: *** indicates significant at the 1% level; ** 5%; * 10%. Standard errors, heteroskedasticity-robust and clustered at the age-by-time level, are in parentheses. All regressions include the controls plus age, hospital and time fixed effects. NIS sampling weights are used.

Table 2.6 – Heterogeneity by Gender and Race

Quality Indicators	Female	Male	White	Non-white	Unknown Race
PQI 90	0.0018 (0.0005)***	0.0017 (0.0015)	0.0020 (0.0007)***	0.0015 (0.0010)	0.0008 (0.0015)
PQI 91	0.0004 (0.0004)	0.0001 (0.0007)	-0.0003 (0.0005)	0.0011 (0.0005)**	-0.0001 (0.0011)
PQI 92	0.0014 (0.0004)***	0.0017 (0.0014)	0.0023 (0.0005)***	0.0004 (0.0008)	0.0009 (0.0009)
PQI 01	0.0011 (0.0003)***	0.0007 (0.0011)	0.0013 (0.0004)***	0.0007 (0.0005)	0.00001 (0.0008)
PQI 02	0.0071 (0.0121)	-0.0040 (0.0103)	0.0017 (0.0114)	0.0081 (0.0128)	-0.0128 (0.0245)
PQI 03	0.00001 (0.0002)	-0.0001 (0.0006)	0.0002 (0.0002)	-0.0006 (0.0004)*	0.0007 (0.0003)**
PQI 07	-0.0002 (0.0001)**	-0.0005 (0.0002)**	-0.0001 (0.0001)	-0.0004 (0.0002)**	-0.0003 (0.0002)
PQI 08	0.0001 (0.0001)	0.0013 (0.0004)***	0.0001 (0.0001)	0.0008 (0.0002)***	0.0001 (0.0002)
PQI 10	0.0006 (0.0001)***	-0.0005 (0.0005)	0.00002 (0.0002)	0.0007 (0.0003)***	0.0011 (0.0004)**
PQI 11	-0.0002 (0.0002)	0.0006 (0.0005)	-0.00001 (0.0002)	0.0003 (0.0003)	-0.0005 (0.0005)
PQI 12	0.00002 (0.0003)	-0.0001 (0.0004)	-0.0003 (0.0003)	0.0004 (0.0004)	-0.0007 (0.0006)
PQI 13	0.00004 (0.0000)**	0.0001 (0.0001)	0.00003 (0.0000)	0.0001 (0.0001)	0.0001 (0.0000)***
PQI 14	0.0002 (0.0001)***	0.0002 (0.0003)	0.0001 (0.0001)	0.0003 (0.0002)*	0.0003 (0.0001)**
PQI 15	0.0003 (0.0002)	0.0001 (0.0006)	0.0007 (0.0003)**	-0.0004 (0.0005)	0.0001 (0.0004)
PQI 16	-0.00003 (0.0000)	-0.0002 (0.0001)**	-0.00004 (0.0000)*	-0.0001 (0.0000)*	-0.0001 (0.0001)*
Sample Size	3,096,019	716,576	1,646,963	1,100,320	1,065,312

Notes: *** indicates significant at the 1% level; ** 5%; * 10%. Standard errors, heteroskedasticity-robust and clustered at the age-by-time level, are in parentheses. All regressions include the controls plus age, hospital and time fixed effects. NIS sampling weights are used.

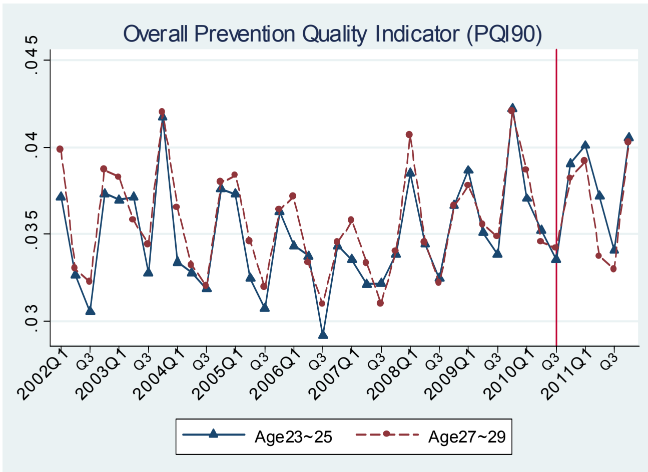
Table 2.7 – Heterogeneity by Patient's Zip Code Income Quartile

Quality Indicators	First (Lowest) Income Quartile	Second Income Quartile	Third Income Quartile	Fourth Income Quartile	Unknown Income Quartile
PQI 90	0.0001 (0.0010)	0.0022 (0.0011)**	0.0035 (0.0011)***	0.0009 (0.0015)	0.0008 (0.0034)
PQI 91	0.00003 (0.0008)	0.0014 (0.0008)*	0.0008 (0.0007)	-0.0002 (0.0009)	-0.0015 (0.0018)
PQI 92	0.0001 (0.0009)	0.0008 (0.0008)	0.0028 (0.0009)***	0.0011 (0.0009)	0.0023 (0.0026)
PQI 01	0.0012 (0.0006)*	-0.0006 (0.0006)	0.0013 (0.0007)*	0.0017 (0.0006)***	0.0022 (0.0017)
PQI 02	-0.0169 (0.0207)	0.0097 (0.0163)	0.0415 (0.0162)**	-0.0364 (0.0220)	0.0484 (0.0769)
PQI 03	-0.0008 (0.0004)**	-0.0002 (0.0004)	0.0011 (0.0003)***	-0.0003 (0.0004)	0.0003 (0.0008)
PQI 07	-0.0004 (0.0002)**	-0.0002 (0.0002)	-0.0003 (0.0001)**	0.0001 (0.0001)	-0.0005 (0.0004)
PQI 08	0.0008 (0.0002)***	0.0004 (0.0002)**	-0.0002 (0.0002)	-0.0002 (0.0002)	-0.0011 (0.0009)
PQI 10	0.0005 (0.0003)	0.0006 (0.0004)*	0.0005 (0.0003)	0.0001 (0.0004)	0.0009 (0.0009)
PQI 11	-0.0005 (0.0004)	0.0006 (0.0003)*	-0.00002 (0.0004)	0.0002 (0.0005)	-0.0013 (0.0014)
PQI 12	0.00003 (0.0005)	0.0002 (0.0005)	0.0003 (0.0004)	-0.0005 (0.0007)	-0.0011 (0.0016)
PQI 13	0.0001 (0.0001)	-0.0001 (0.0001)	0.00003 (0.00003)	-0.00004 (0.0001)	0.0003 (0.0001)**
PQI 14	-0.0001 (0.0002)	0.0004 (0.0002)**	0.0004 (0.0001)***	-0.0001 (0.0001)	0.0006 (0.0008)
PQI 15	-0.0007 (0.0005)	0.0011 (0.0005)**	0.0004 (0.0004)	-0.0001 (0.0005)	0.0004 (0.0015)
PQI 16	-0.0001 (0.0001)**	0.00000 (0.0000)	-0.00004 (0.0000)	-0.0001 (0.0000)	0.00001 (0.0000)
Sample Size	698,365	639,567	565,782	409,215	1,499,666

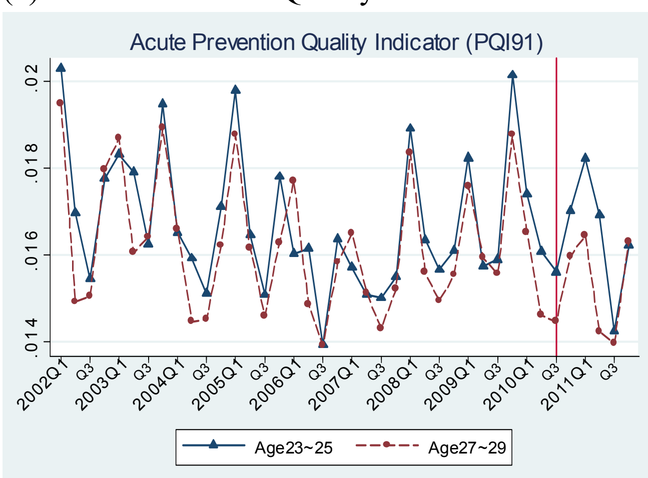
Notes: *** indicates significant at the 1% level; ** 5%; * 10%. Standard errors, heteroskedasticity-robust and clustered at the age-by-time level, are in parentheses. All regressions include the controls plus age, hospital and time fixed effects. NIS sampling weights are used.

Figure 2.1 – Trends in Prevention Quality Indicators by Age Group

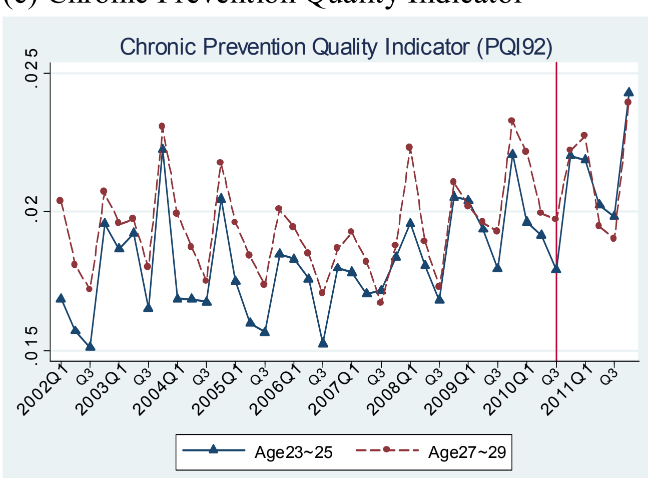
(a) Overall Prevention Quality Indicator



(b) Acute Prevention Quality Indicator



(c) Chronic Prevention Quality Indicator



CHAPTER III

Health Insurance and Traffic Fatalities for Young Adults

I. Introduction

On September 23rd, 2010, the dependent coverage expansion of the Patient Protection and Affordable Care Act (ACA) was implemented to increase health insurance coverage for young adults aged 19 to 25. Prior to this expansion, young adults in this age range would commonly age off of their parents' insurance plans and often become uninsured due to a lack of other sources of insurance.[39]

Several studies examine the impact of ACA dependent coverage expansion on health insurance coverage; these studies employ different datasets and consistently find a statistically significant increase in coverage for young adults (Cantor et al., 2012; Sommers and Kronick, 2012; Sommers et al., 2013; Akosa Antwi et al., 2013 and 2015; Chua and Sommers, 2014; Barbaresco et al., 2015). Less explored in the literature however is moral hazard, a potential unintended consequence associated with such an expansion in coverage. The purpose of insurance is to protect the insured from financial problems due to large losses (such as disease, accidents, loss of valuables etc.). The theory of moral hazard predicts that when the potential costs from these losses are borne, in whole or in part, by others, the insured have a tendency to take more risks. Increases in risk-taking behavior associated with a health insurance expansion could happen in two ways: one is through reductions in the consumption of preventive care that an individual might otherwise consume in order to avoid costly hospitalizations; the other is through increases in risky behaviors, such as smoking, binge drinking, or over eating. Excessive drinking may also be associated with drunk driving, which can lead to fatal traffic accidents.

[39] For more on this policy see: http://www.hhs.gov/healthcare/rights/youngadults/index.html

Concerns about drunk driving leading to fatal traffic accidents are particularly important for young adults. According to Insurance Institute for Highway Safety (IIHS), although young drivers are less likely to drink and drive than adults, their crash and fatality risks are higher if they do, due to their relative inexperience associated with both drinking and driving.[40] All fifty states and the District of Columbia have imposed Graduated Driver Licensing (GDL) programs in an attempt to reduce teen drivers' driving risk through enhancing driving restrictions. Of course, reckless driving behavior, especially drinking and driving, is not easily regulated.[41]

In the earlier study of the impact of the ACA young adult health insurance coverage expansion on health outcomes (Barbaresco, Courtemanche and Qi, 2015), we find some potential evidence of moral hazard among newly insured young adults, that an increase in health insurance coverage leads to an increase in risky drinking behavior. As described the above, another potential channel for moral hazard associated with new insurance coverage would be through increases in reckless, including alcohol-impaired driving. This, in turn, could lead to more traffic accidents and more traffic fatalities.

What do we know about the rate of fatal traffic accidents for young adults during our timeframe of interest? According to the National Vital Statistics Reports, young adults in the age group of 15 to 24 have the highest number of deaths (20 percent of all ages) caused by motor vehicle accidents. [42] Also, motor vehicle crashes are the leading cause of death (15.9 percent of all causes) for young adults in the same age group (Centers for Disease Control and Prevention, 2011). Among all the fatal accidents, alcohol related crashes make up 36 percent for all ages (National Highway Traffic Safety Administration, 2011).

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[40] For more information, see: http://www.iihs.org/iihs/topics/t/teenagers/topicoverview.
[41] For more information about statewide Graduated driver licensing program, see: http://www.iihs.org/iihs/topics/laws/graduatedlicenseintro?topicName=teenagers.
[42] The report is at: http://www.cdc.gov/nchs/data/nvsr/nvsr61/nvsr61_04.pdf, Table7.

In this paper, I estimate the causal relationship between health insurance coverage and traffic fatalities among young adults; that is, whether or not the increase in health insurance coverage for young adults through the ACA dependent coverage expansion leads to more overall traffic fatalities and more alcohol-related traffic fatalities. My primary results suggest that for young adults (aged 20) who just gained health insurance from the ACA dependent coverage expansion increased their risky driving behavior, leading to more traffic accidents and even more traffic fatalities. The magnitude associated with the increase in traffic accidents is smaller when I restrict attention to alcohol-related accidents. The rest of this chapter is organized as follows: Section II presents a literature review of traffic fatalities; section III describes the data used in this study; section IV illustrates the methodology employed in my empirical work; section V presents my primary results, and section VI concludes the chapter.

II. Literature Review

There is a broad literature investigating the impact of specific types of insurance on related types of injuries. Cohen and Dehejia (2004) find that an increase in the share of drivers with auto insurance increases traffic fatalities and Bolduc et al. (2002) find that increase of generosity in worker's compensation insurance increase injuries related to work. To the best of my knowledge, there is no previous research directly investigating the impact of health insurance coverage on traffic fatalities. As will be discussed in more detail below, there are, however, established strands of the literature exploring the impact of alcohol consumption, prices changes, and Body Mass Index (BMI) on fatal crashes. The ACA dependent coverage expansion has been shows to influence each of these factors, so it could in turn have an impact of fatal crashes.

One strand of the literature debates on the impact of beer taxes and minimum drinking age laws (MDAL) on traffic fatalities. Chaloupka et al. (1993) show that increased beer taxes has the largest impact on reducing youth fatality (an 11.5 percent reduction), followed by mandatory administrative license laws (a 9 percent reduction). The authors also indicate that MDAL reduces total fatalities by about 5 to 6 percent. Saffer and Grossman (1987) conclude that the elasticity of the motor vehicle fatality rate to the real beer tax is about 20 for young adults. This suggests that an increase in beer taxes could reduce the youth death rate. Similarly, Ruhm (1996) finds that, compared to relatively small impacts from other regulations (i.e. MDAL), increases in beer taxes cause larger reductions in youth fatalities. However, Dee (1999) suggests that beer taxes have a relatively small and statistically insignificant impact on teen drinking after controlling for cross-state heterogeneity; while the implementation of MDAL actually leads to reductions in heavy teen drinking by 8 percent and reductions in traffic fatalities by at least 9 percent. This debate suggests that in my analysis of the impact of expansions in health insurance coverage I should control for the money cost of alcohol, as well as any non-pecuniary costs faced by young adults when attempting to acquire alcohol. In addition, I should control for potential cross-state heterogeneity, as state laws prior to the ACA may weaken the impact of the federal policy changes.

Another strand of the literature looks at how income changes impact traffic fatalities among young adults. Adams et al. (2012) show that an increase of 10 percent in the minimum wage has a positive correlation of 5 to 10 percent with alcohol-related accidents for teen drivers. Grabowski and Morrisey (2004) find that a 10-cent decrease in gasoline prices leads to an increase in motor vehicle fatalities over a 2-year period. They also find that the effect is larger for higher-risk young adult drivers. By looking at the changes in state gasoline taxes, Grabowski and Morrisey (2006) suggest that plausibly exogenous increases in state gasoline taxes are related to fewer traffic fatalities. This strand of literature implies that income effects, such as an increase in income when a young adult substitutes insurance coverage they pay for with costless coverage through their parents' plan, are related to fatal traffic crashes.

A third strand of the literature considers the relationship between obesity and driving behavior. Anderson et al. (2012) show that commercial motor vehicle operators with higher BMI were more likely to be in a subsequent accident. Simmons and Zlatoper (2010) find that during 2005, accident fatalities per mile traveled was positively associated with a state's obesity prevalence. Dunn and Tefft (2013) investigate the relationship between BMI and traffic fatalities among young adults and find that obesity tends to make the body less inebriated and helps decrease traffic fatalities related to alcohol consumption. As in the earlier study of the impact of the ACA dependent coverage expansion on health outcomes (Barbaresco, Courtemanche, and Qi, 2015), we find that ACA dependent coverage expansion helped improve BMI among young adults, which may reduce traffic fatalities.

A final strand of the literature I consider focuses on the impact of statewide Graduated Driver Lisensing (GDL) programs on young adults' traffic fatalities. Dee et al. (2005) and Morrisey et al. (2006) find GDL regulations reduce traffic fatalities among 15-17 year olds. Morrisey and Grabowsky (2010) find that "good" GDL programs reduce overall traffic fatalities, as well as driver fatalities for the policy's targeted age groups (teenagers from 15-17 and 18-20 age groups). Therefore, controlling for the type of state GDL program is also important when evaluating other policies related to traffic fatalities of young adults.

There are also many studies that consider show a mix of factors mentioned above and tend to give us an ambiguous prediction for the impact of each factor. Gallet (2007) finds that higher income leads to greater alcohol consumption but lowers the risk of being overweight. Courtemanche (2010) finds a negative relationship between gasoline prices and BMI as higher gasoline prices encourage more walking and less dinning out. As the mechanism of maintaining sobriety by heavier weight may differ by personal physical body function, it may not be very efficient to be considered as a factor that can be easily influenced by policy.

III. Data

A. Fatality Analysis Reporting System

Data on fatal vehicle crashes are obtained from the Fatality Analysis Reporting System (FARS) of the National Highway Traffic Safety Administration (NHTSA). The FARS is a census of all motor vehicle traffic accidents that result in a fatality for either occupants or non-motorists. It includes detailed information on the characteristics of the vehicles, drivers, occupants and non-occupants involved in the crash. Because I am interested in the relationship between insurance coverage of young adults and traffic fatalities, I restrict attention to accidents resulting in a fatality caused by young drivers. As discussed in the following sub-sections, policies such as Minimum Drinking Age Laws and Zero Tolerance Laws associated with drinking, as well as Graduated Driver Licensing programs targeted at young adults below age 21, tried to reduce traffic fatalities caused by younger drivers. Thus, in this study, I mainly focus on young drivers aged 20 and below as the primary treatment group. Following Morissey and Grabowski (2010), by state, year, and age group of interest, I count the number of traffic accidents resulting in a fatality as well as the total number of fatalities associated with these accidents.[43] These counts serve as the primary dependent variables in my analysis, which is focused on 2008-2013 in order to have an equal number of years before and after the ACA expansion of dependent coverage.

[43] Following previous literature, I exclude Alaska, Hawaii and the District of Columbia due to the fewer observations.

The results of blood alcohol concentration (BAC) tests for those involved in traffic accidents are sometimes not reported. The FARS attempts to impute these missing values and adopted a new method for doing so in 2001.[44] Because I use imputed values of BAC test results in my analysis, I do not include any data prior to 2001. This insures the imputation method for the BAC test results is constant throughout every year in my sample. Since young adults may be more easily involved in an alcohol-related crash, any accident record with a BAC greater than 0 will be counted as alcohol-related accident.[45] This is consistent with the enforcement of Zero Tolerance Laws for young drivers below age 21.

B. Graduated Driver Licensing Program

Graduated driver licensing (GDL) laws have been established to help reduce traffic accidents among teen drivers. GDL regulation varies across states and across three distinct licensing stages. These three licensing stages are: learner stage (minimum entry age, mandatory permit holding period, and minimum amount of supervised driving), intermediate stage with unsupervised driving (unsupervised nighttime driving prohibition and restriction on passengers), and unrestricted stage (restrictions lifted age).[46]

By using a standardized classification system created by the Insurance Institute for Highway Safety (IIHS), one can characterize GDL laws into four groups (Good, Fair, Marginal, and Poor) to evaluate the restrictiveness of the laws. Those criteria include the required length of holding a learner's permit (usually six months), restrictions on unsupervised nighttime driving (usually 10pm to 5am), the number of teen passengers (usually no more than one) in the car, and

[44] For more discussion about new imputation methodology, see: http://www-nrd.nhtsa.dot.gov/Pubs/809-450.pdf.
[45] Zero Tolerance Laws in different states have different criteria regarding acceptable BAC levels, ranging from 0.00 to 0.02 percent. For more information, see: http://dui.findlaw.com/dui-laws-resources/underage-dui-zero-tolerance-laws.html.
[46] For detailed regulations by states, see: http://www.iihs.org/iihs/topics/laws/graduatedlicenseintro?topicName=teenagers.

the minimum age (usually age 17) until the restrictions are lifted. The most restrictive states in terms of teen driving are placed in the "good" category, while state with the least restrictions are placed in the "poor" category. Previous studies (Morrisey et al., 2006; Morrisey and Grabowski, 2010) find that states with GDL programs in the "good" and "fair" categories have fewer young adult motor vehicle fatalities. In this study, I classify state GDL programs by year according to the IIHS criteria and use this to control a state's young adult driving environment.

Following the literature, other control variables I employ include beer tax rates, gasoline prices, state unemployment rates, and state total population. The beer tax data were obtained from the Tax Foundation.[47] This source provides tax rates (dollars per gallon) for beer, as well as other alcohol, for each state in each year. Annual average regular grade gasoline wholesale/resale prices (dollars per gallon) by refiners were obtained from the U.S. Energy Information Administration.[48] Both the tax and price data were adjusted for inflation based on the 2013 annual CPI. Annual average state unemployment rates were obtained from the Bureau of Labor Statistics (BLS). Total population by state, year and age were constructed using data from Current Population Survey (CPS).[49]

IV. Methodology

I use a difference-in-differences approach to identify the impact of the ACA dependent coverage expansion on traffic accidents and fatalities. Equation (1) below described the

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[47] For further information, see: http://taxfoundation.org/article/state-sales-gasoline-cigarette-and-alcohol-tax-rates.

[48] I use annual average wholesale/resale gasoline prices since annual average retail gasoline prices were not available for years after 2010. Resale prices are usually slightly lower than retail prices as the intermediary businesses earn the price differences, and the price differences should stay stable over years. In the sample, wholesale/resale prices and retail prices for gasoline before 2011 have the same time trends. This suggests I can use wholesale/resale gasoline prices as a proxy for retail gasoline prices.

[49] Specific age information was not available from the Census Bureau. To validate the effectiveness of using the CPS, I constructed the total number of observation with weights from CPS in the corresponding age group from the ACS (American Community Survey). The CPS with weights has around 0.5% fewer total individuals in each age group as compared to the ACS. Since total population at each age serves as a state-year-age group level control, consistently lower number of population make it plausible to use the CPS to construct total population in each estimated cell.

empirical model I estimate with the data described in the previous section:

$$F_{gst} = \beta_0 + \beta_1(Treat_g * After_t) + \beta_2 Female_{gst} + \beta_3 GasPrice_{st} + \beta_4 BeerTax_{st} + \beta_5 GDL_{st} + \beta_6 Unem_{st} + \beta_7 Pop_{st} + Age_g + Time_t + State_s + \varepsilon_{gst}, \qquad (1)$$

where F is accident or fatality counts in age group g, state s, and year t; β_1 captures the effect of mandates on the treatment over control group; $Female$ is a dummy variable that indicates the gender of the driver, the pedestrian, or the bicyclist that caused the fatal accident; $GasPrice$ represents the gasoline price for each state in each year; $BeerTax$ represents the beer tax for each state in each year; GDL represents categories of restrictiveness of the Graduated Driver Licensing program in force in the state in the relevant year; $Unem$ is the annual average unemployment rate for each state in each year; Pop is the annual total population in each state in each year. Age, $Time$ and $State$ control for age, year and state fixed effects separately for the drivers and the accidents they caused, and ε_{gst} is the error term. Standard errors are clustered at the treatment level of the interaction of age-by-time.[50]

As mentioned above, for the main analysis, I use data from 2008 to 2013 to allow for three years of pre-reform data (2008-2010) and three years of post-reform data (2011-2013).[51] In order to cleanly estimate the causal impact of the ACA dependent coverage expansion, the treatment group I analyze consists of young drivers aged 20, and the control group is the young drivers aged 18. Age 19 was excluded from the analysis as it is hard to tell whether they have been dropped from their parents plan due to their birth date and the renewal dates of their parents' health insurance plan. Teenagers aged 15 to 17 were excluded for two reasons. First, they should

[50] If clustered by age alone, there are only two clusters in the regression. I prefer using an interaction of age-by-time as it gives more clusters (Angrist and Pischke, Chapter 8).

[51] I treat 2010 as a pre-treatment year since insurance companies may not expand the dependent coverage until parents for whom the reform is binding renew their health insurance coverage, normally the beginning of the next year.

not be impacted by the ACA dependent coverage expansion, since they were likely already eligible for insurance through their parents' employer or for public coverage through the CHIP program.[52] Second, most states set the minimum driving age to be no earlier than age 16, and some states don't lift supervised driving restrictions until age 17 or 18.[53] Since we cannot tell from the FARS which drivers are college students for whom the ACA dependent coverage mandate is not binding, I assume all drivers are not students. This should result in lower bound estimates of the impact of the reform.

The fundamental identifying assumption associated with difference-in-differences analysis is the parallel trends assumption for the treatment and control groups in the pre-treatment period. Figure 1 shows the trends for two outcomes – traffic accident counts and traffic fatality counts - over the entire period of study for both sets of treatment and control groups. Figure 2 is the corresponding figures for alcohol-related traffic accident counts and traffic fatality counts. Trends for the pre-treatment periods for each set of treatment and control groups are parallel to each other, which validates the use of the difference-in-differences approach in this study.

Table 3.1 shows means and standard errors for all traffic accidents and fatalities (panel I), as well as alcohol-related traffic accidents and fatalities (panel II) for both the treatment and control group. Panel I shows that both accident counts and fatality counts for treatment group (age 20) have similar means as in the control group (age 18) in the pre-treatment period. After the ACA dependent coverage mandate, means in treatment group are much higher than the

[52] See Marton (2007) and Marton and Talbert (2010) for more on the CHIP program.

[53] Eight states (Arkansas, Florida, Georgia, Missouri, New Jersey, Texas, Virginia, and Washington) plus Washington, D.C. set their full privilege minimum driving age at 18. Seven states (Connecticut, Illinois, Maryland, Massachusetts and Nevada; New York and Pennsylvania) have specific requirements regarding their full privilege minimum driving age, such as night driving restrictions or restrictions related to driver education completion, but also set the full privilege minimum driving age at 18. Thus age 17 will not be included in the control group of the main analysis, but will be included in the control groups of my robustness checks.

means in control group. This suggests significant net increases in accidents and fatalities associated with the ACA dependent coverage mandate according to the simple difference-in-differences calculations presented in the last column. Panel II shows similar patterns when we restrict attention to alcohol-related traffic accidents and fatalities only, though with smaller magnitudes. The results in Table 3.1 reflect the trends in Figures 1 and 2. The simple difference-in-differences calculation shows a simple, unadjusted pre and post comparison. My regression models will provide more precise estimates by controlling for other confounding factors that might influence traffic accidents and fatalities.

V. Results

Table 3.2 shows regression results for both the full sample of traffic accidents and the alcohol-related-only sub-sample. Results from panel I and II are for full sample, and panel III and panel IV are for alcohol-related-only sub-samples. The first column of each table is the baseline regression with years 2008-2013 for age group 20 vs age 18. Results from column 1 of panel I show that after the implementation of the ACA dependent coverage expansion, young adults aged 20 experience a 4.4 percentage point (17.0 percent) increase in traffic accidents and a 5.6 percentage point (19.4 percent) increase in traffic fatalities.

The next three columns are robustness checks. To show that the estimates are not affected by the chosen length of pre-treatment periods, column 2 and 3 of panel I are estimated with a longer pre-treatment period, one is from 2005, and the other is from 2001, the first available year in the sample. Column 4 excludes 2010, as the ACA dependent coverage mandate was implemented in late 2010 and some insurance companies may have enrolled dependents in the last three quarters of the year in compliance with the reform. In addition, 2007 was included to maintain the same number of year pre and post reform. Panel II is another set of robustness checks for the full sample analysis. Column 1 restates the baseline estimation from column 1 of panel I. Columns 2 to 4 are estimations with broader age groups. When these robustness checks are applied to the full sample of traffic accidents and fatalities, the results remain stable.

Panel III and IV present similar baseline models and robustness checks for the alcohol-related traffic accident sub-sample. Here I would expect a smaller impact of the ACA dependent coverage expansion among those aged 20 since the legal drinking age in the United States is 21. As expected, the coefficient estimates in panels III and IV are smaller in magnitude than the estimates given in panels I and II. Results from column 1 of panel III show that young adults aged 20 have a 1.3 percentage point (11.9 percent) increase in traffic accidents and a 1.6 percentage point (12.6 percent) increase in traffic fatalities. The corresponding figures for broader age groups are given in Figures 3.3-3.8.

VI. Conclusion

Young adults aged 20 who are newly insured by the ACA expansion of dependent coverage may be more likely to engage in risky behavior, such as reckless driving, and even drinking and driving, than those aged 18, who were already covered by other types of health insurance. This could be due to a reduction in their health insurance spending increasing their disposable income and allowing them to buy more alcohol and / or to drive more miles than before. Gaining dependent insurance coverage through a parent may also induce these young adults to drop out of college and/or be more willing to accept a part-time job that does not offer health insurance. This could potentially increase the amount of driving they do by providing them with more leisure time and thus increase the potential for traffic accidents.

One caveat of using FARS for younger adults who may not be affected by the ACA dependent coverage expansion due to their student status is that FARS does not have education information for drivers. Thus, the estimates provided in this study only show a lower bound of the impact of ACA dependent coverage mandate as I am assuming all the younger drivers in my sample are *not* college students.

This study focuses on the younger adults who just obtained health insurance coverage from ACA dependent coverage expansion and found an increase in traffic accidents and fatalities for them. Older young adults who finished their education (aged 23-25) may behave differently from those younger peers when gaining health insurance from their parents. Future study will examine the impact of ACA dependent coverage expansion on older young adults.

Table 3.1 – Unadjusted Difference-in-Differences Estimates of the Impact of the ACA Dependent Coverage Expansion on Traffic Accidents and Fatalities

Panel I: Treatment (age 20) VS Control (age 18) – All Traffic Accidents

Outcome Variables	Pre-treatment Periods (08-10)		Post-treat periods (11-13)		Difference-in-Differences
	Treat T1 (age 20)	Control C1 (age 18)	Treat T1 (age 20)	Control C1 (age 18)	
Accident Count	25.71	25.35	23.38	18.98	4.04*
	(1.62)	(1.45)	(0.17)	(0.42)	(2.22)
Fatality Count	28.83	29.38	26.08	21.42	5.20*
	(1.79)	(1.78)	(0.17)	(0.44)	(2.57)

Panel II: Treatment (age 20) VS Control (age 18) – Alcohol-Related Accidents Only

Outcome Variables	Pre-treatment Periods (08-10)		Post-treat periods (11-13)		Difference-in-Differences
	Treat T1 (age 20)	Control C1 (age 18)	Treat T1 (age 20)	Control C1 (age 18)	
Accident Count	10.97	8.89	10.44	7.17	1.19
(alcohol-related)	(0.57)	(0.34)	(0.23)	(0.16)	(0.72)
Fatality Count	12.38	10.24	11.69	8.08	1.47*
(alcohol-related)	(0.53)	(0.34)	(0.27)	(0.17)	(0.71)

Notes: Means are reported. Standard errors, heteroskedasticity-robust and clustered by age-by-time, are in parentheses. *** indicates the difference-in-differences is significant at the 1% level; ** 5% level; * 10% level.

Table 3.2 – Multivariate Difference-in-Differences Estimates of the Impact of the ACA Dependent Coverage Expansion on Traffic Accidents and Fatalities

Panel I: All Traffic Accidents - Treatment (age 20) VS Control (age 18)

Outcome Variables	2008-2010 VS 2011-2013	2005-2010 VS 2011-2013	2001-2010 VS 2011-2013	2007-2009 VS 2011-2013
Accident	4.368***	4.611***	5.425***	4.381***
Count	(0.278)	(0.281)	(0.389)	(0.276)
Fatality	5.586***	5.606***	6.219***	5.518***
Count	(0.276)	(0.375)	(0.457)	(0.278)
N	562	848	1,231	561

Panel II: All Traffic Accidents - Pre (years 2008-2010) VS Post (years 2011-2013)

Outcome Variables	Age 20 VS Age 18	Age 20 VS Age 17-18	Age 20-21 VS Age 17-18	Age 20-22 VS Age 16-18
Accident	4.368***	3.282***	3.466***	3.223***
Count	(0.278)	(0.552)	(0.534)	(0.605)
Fatality	5.586***	3.986***	4.332***	3.968***
Count	(0.276)	(0.685)	(0.695)	(0.729)
N	562	843	1,124	1,676

Panel III: Alcohol-Related Accidents Only - Treatment (age 20) VS Control (age 18)

Outcome Variables	2008-2010 VS 2011-2013	2005-2010 VS 2011-2013	2001-2010 VS 2011-2013	2007-2009 VS 2011-2013
Accident	1.302***	1.128***	1.459***	0.783*
Count	(0.340)	(0.356)	(0.398)	(0.371)
Fatality	1.560***	1.277***	1.640***	0.951*
Count	(0.361)	(0.394)	(0.475)	(0.458)
N	536	816	1,191	536

Panel IV: Alcohol-Related Accidents Only - Pre (years 2008-2010) VS Post (years 2011-2013)

Outcome Variables	Age 20 VS Age 18	Age 20 VS Age 17-18	Age 20-21 VS Age 17-18	Age 20-22 VS Age 16-18
Accident	1.302***	0.981**	0.873**	0.748**
Count	(0.340)	(0.391)	(0.352)	(0.342)
Fatality	1.560***	1.051**	1.041**	0.941**
Count	(0.361)	(0.426)	(0.431)	(0.419)
N	536	777	1,054	1,540

Notes: *** indicates significant at the 1% level; ** 5% level; * 10% level. Standard errors, heteroskedasticity-robust and clustered by age-by-time, are in parentheses. All regressions include the controls plus age, state, and time fixed effects.

Figure 3.1 – Traffic Accident / Fatality Counts for Young Adults Aged 20 VS 18

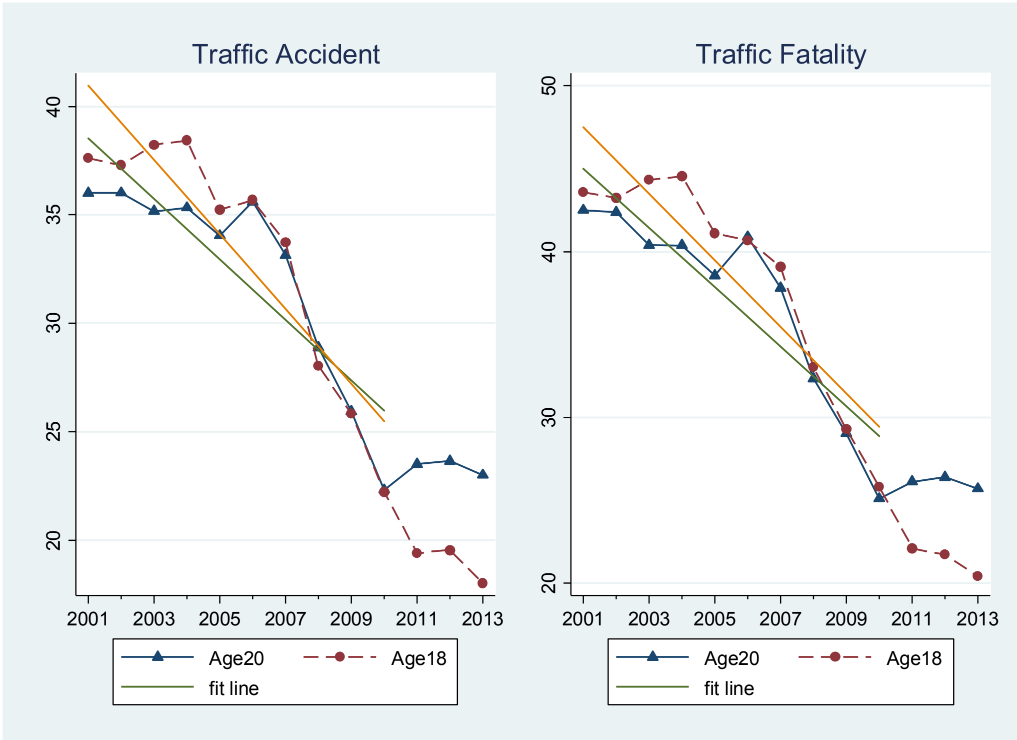


Figure 3.2 – Alcohol-Related Traffic Accident / Fatality Counts for Young Adults Aged 20 VS 18

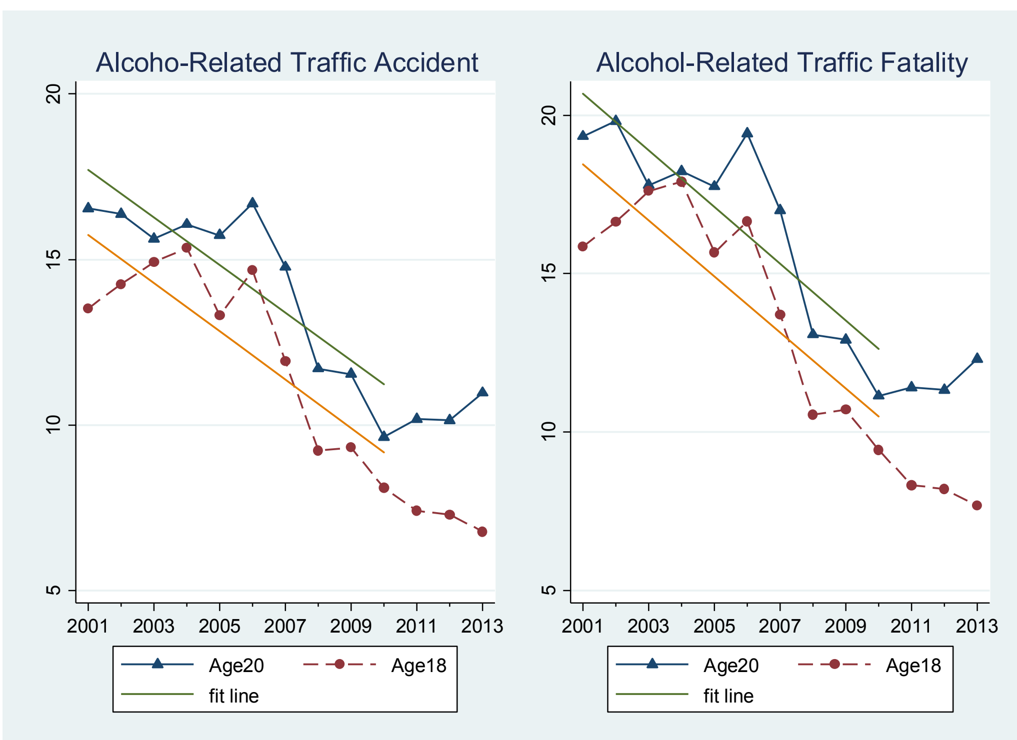


Figure 3.3 – Traffic Accident / Fatality Counts for Young Adults Aged 20 VS 17-18

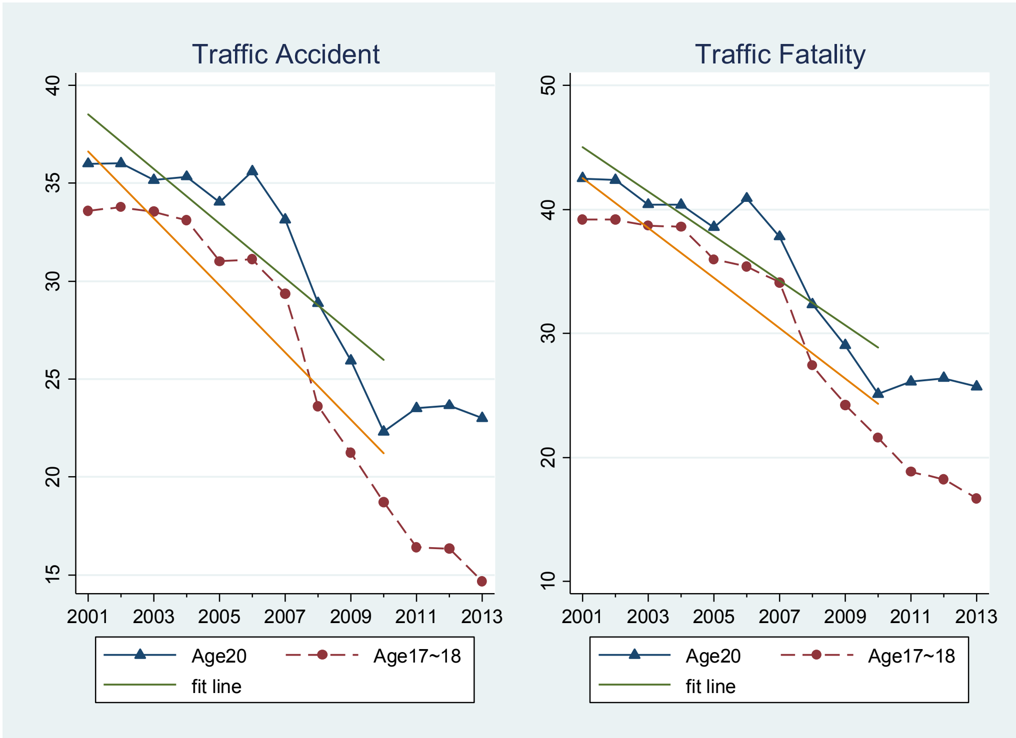


Figure 3.4 – Alcohol-Related Traffic Accident / Fatality Counts for Young Adults Aged 20 VS 17-18

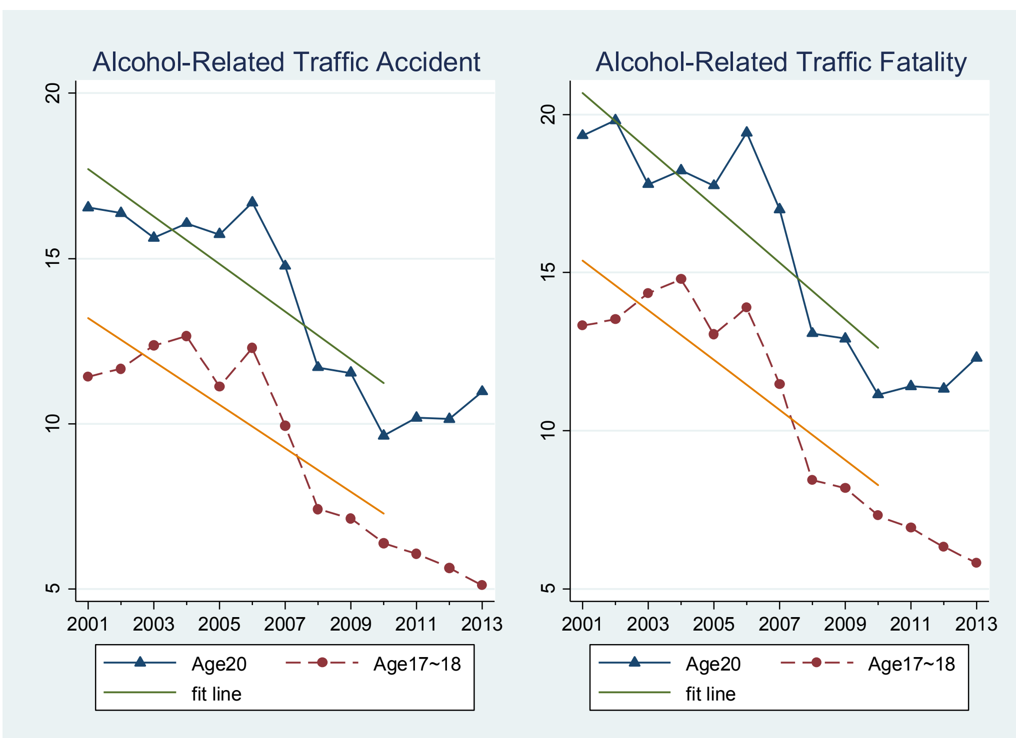


Figure 3.5 – Traffic Accident / Fatality Counts for Young Adults Aged 20-21 VS 17-18

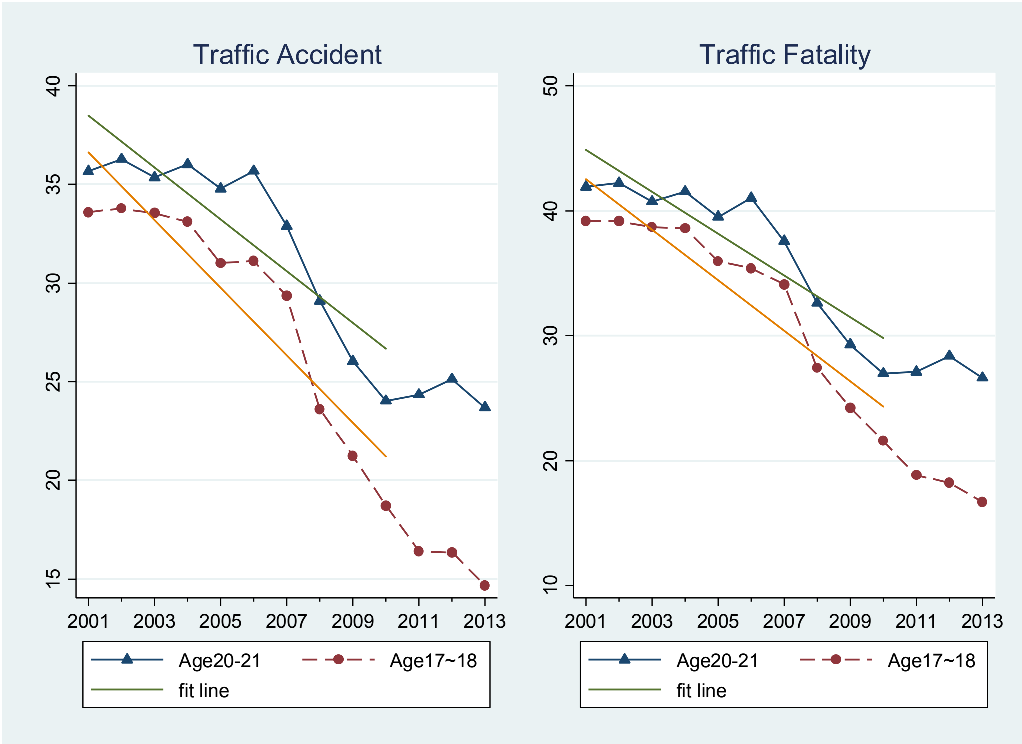


Figure 3.6 – Alcohol-Related Traffic Accident / Fatality Counts for Young Adults Aged 20-21 VS 17-18

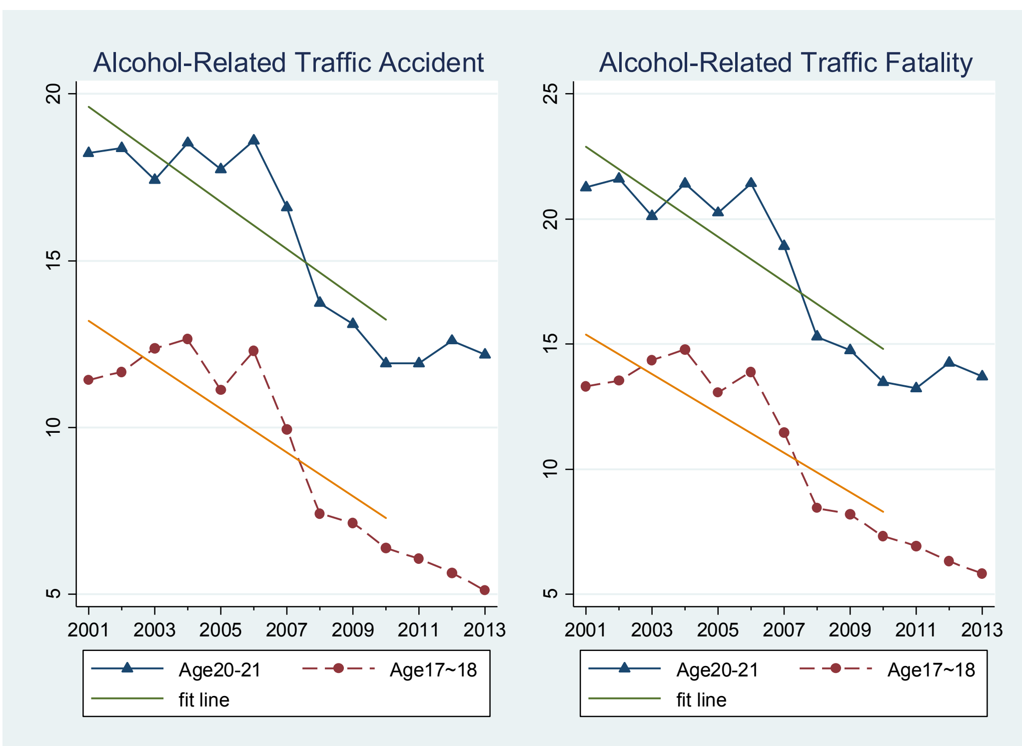


Figure 3.7 – Traffic Accident / Fatality Counts for Young Adults Aged 20-22 VS 16-18

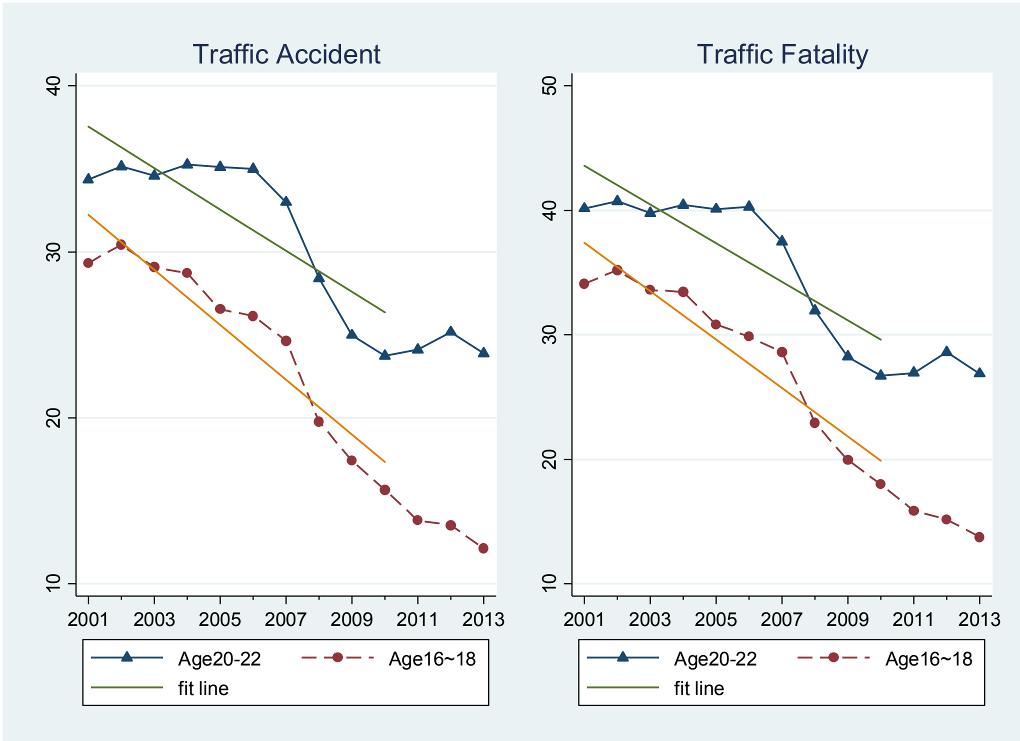
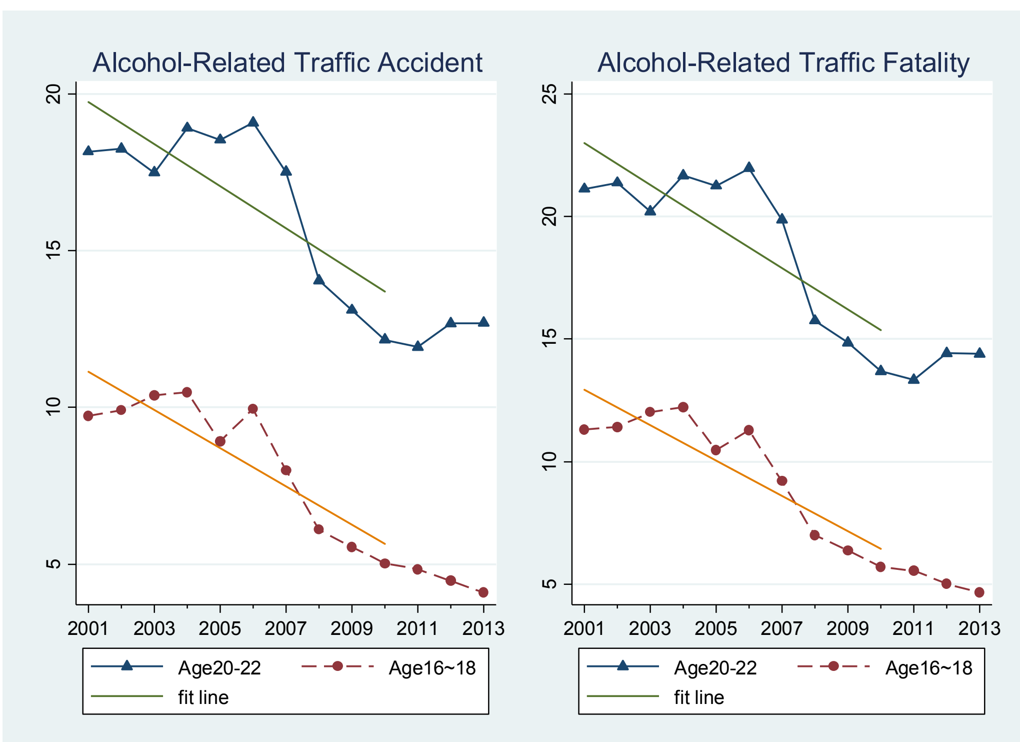


Figure 3.8 – Alcohol-Related Traffic Accident / Fatality Counts for Young Adults Aged 20-22 VS 16-18



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VITA

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