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doi: https://doi.org/10.57709/22796645

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ABSTRACT

ESSAYS ON HEALTH ECONOMICS AND HUMAN CAPITAL

By

SAMUEL ASARE

MAY, 2021

Dissertation Chair: Dr. Shiferaw Gurmu

Major Department: Economics

This dissertation consists of three essays. The overarching objectives are to provide causal evidence of the effects of health insurance program from Ghana, charter school in the United States (U.S.), and economic conditions in the U.S. on healthcare utilization, health behaviors and human capital development.

Chapter 1 uses the Demographic and Health Survey to study the healthcare utilization effects of Ghana's 2004 adoption of a national health insurance scheme (NHIS), covering over 95% of medical expenditures. First, we find that self-reported participation in the NHIS increases twelve-month healthcare visits by 32 percentage points using the timing of the rollouts across districts as an instrument. We also show that the positive effect is larger for less-educated, poor, and rural women. Second, we find that the NHIS increases deliveries in health facilities and prenatal care visits by 6 and 7 percentage points, respectively, using a difference-in-differences strategy and women from rural Nigeria as the control group. Together, these findings are consistent with evidence from similar programs in developed countries despite numerous implementation challenges and relatively low take-up of the NHIS.

In Chapter 2, we examine the impacts of exposure to charter schools on students' long-term outcomes using the restricted, geocoded National Longitudinal Survey of

Youth data and school information from the National Center for Education Statistics. We use an instrumental variable method by constructing charter school exposure in the county of birth as an instrument for actual exposure in the county of residence. Our results suggest strong evidence of charter schools increasing four-year college completion and reducing adverse health behaviors. We also show evidence of heterogeneity. While college graduations are more pronounced among females and minorities, the reduction in binge drinking and cigarette smoking is higher among minorities and high-educated individuals. Overall, our results demonstrate that charter schools improve students' long-term outcomes.

Finally, Chapter 3 uses individual-level data from the 1987–2019 Behavioral Risk Factor Surveillance System and state-level employment data from the Bureau of Labor Statistics to estimate the effects of macroeconomic conditions on cigarette smoking. We find that a one-point increase in the employment rate raises the current cigarette smoking rate by 0.4%. We also show heterogeneity among males, Blacks and low-educated individuals having larger impacts but no differential effects by age. A dynamic treatment effect analysis demonstrates that the procyclical relationship is declining and unstable over time, with a sharp temporal decrease during the Great Recession period and a weak countercyclical effect in 2019. Overall, the results suggest that future adverse macroeconomic shocks will increase cigarette smoking, contrary to studies that demonstrate healthy living in bad economic times.

ESSAYS ON HEALTH ECONOMICS AND HUMAN CAPITAL

BY

SAMUEL ASARE

A Dissertation Submitted in Partial Fulfillment
of the Requirements for the Degree
of
Doctor of Philosophy
in the
Andrew Young School of Policy Studies
of
Georgia State University

GEORGIA STATE UNIVERSITY
2021

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2021

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.

Dissertation Chair:	Dr. Shiferaw Gurmu
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May, 2021

DEDICATION

To my father, George K. Asare, mother, Augustina Asare, and bothers and sisters for their unconditional love, encouragement, prayer, advice, and unending support for my educational career & To my wife, Veronica Asare, son, George K. Asare Jr., and daughter, Kayleigh B. Asare, who later joined the supporters' team to make this dissertation successful.

ACKNOWLEDGMENTS

Many great people guided this dissertation. My adviser, committee members, family, friends, classmates, seminar participants, conference participants, and generous financial support through several fellowships contributed to the success. I am grateful to my adviser, Dr. Shiferaw Gurmu, for his patience and advice. I appreciate his guidance and support in securing a fellowship to fund my sixth year to complete this dissertation. Without this funding, it would be difficult to complete it, as I would have spent most of my time working to fund my final years, which required the most attention.

To Dr. James Marton and Charles Courtemanche, I owe you thanks for your support and guidance in passing my comprehensive field exam and choosing my job market paper. Your constant feedback and suggestions made this dissertation successful. Also, I cannot forget the contributions of Dr. Michael Pesko when he joined my committee. His idea of finding a control group from West Africa for my job market paper gave me hope and changed it drastically. His support for my job market applications helped me get my first dream job. Dr. Mike Pesko, I am very thankful for your help and support.

Thanks to Dr. Pierre Nguimkeu for giving me confidence and coaching me on the job market. Though he was not a committee member on my dissertation research, our extensive discussion of econometrics methods, feedback and suggestions, and possible extensions for Chapter 1 is appreciated. My one-year personal encounter with him before my job market applications is unforgettable. I also want to thank Dr. Augustine Denteh for his availability anytime to answer my research questions, coaching, and even helping me in personal and family matters. You have been more than a brother to me. Similarly, I cannot forget about Dr. Michael Dzodzomenyoh and Isaac Opoku Agyeman for their encouragement and prayers for my success.

The suggestions and contributions of Dr. Thomas Mroz, Dr. Jonathan Smith, and all the seminar participants at the Department of Economics, Georgia State University, are much appreciated. I also presented Chapter 1 at the Southern Economic Association annual meeting, the Missouri Valley Economic Association annual meeting, and the yearly Southeastern International Economic Development Workshop meeting. I am grateful to all the participants who made suggestions.

I will be very ungrateful if I ignore my wife, Veronica Asare, mother, father, brothers, and sister, without showing my appreciation. They were the fallback of my educational career anytime. They spent time listening to my complaints and encourage me. Their financial, emotional, and spiritual support gave me peace and hope for success. Surprisingly, I got married and had two children before completing this dissertation. This amazing family gave me joy and helped me prove that with God, all things are possible. Thanks for their blessings.

Finally, Chapter 2 of this dissertation research was conducted with restricted access to Bureau of Labor Statistics (BLS) data. The views expressed here do not necessarily reflect the views of the BLS.

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INTRODUCTION

This dissertation is in three parts intended to understand how health system interventions, education policies, and the changing macroeconomic conditions affect medical care use, education as a component of human capital, and health behaviors. Policymakers worldwide make policy interventions to influence people's choices to improve their welfare. Understanding how the relevant populations respond or are affected by such policies requires rigorous empirical analyses to show their effects convincingly due to data limitations and several methodological challenges. This dissertation evaluates a government-subsidized health insurance scheme from a developing country on healthcare use. The program was intended to correct market failures in the healthcare delivery system. We also estimate the impacts of charter school laws on students' long-term education and health behaviors. The state-level policy intervention makes approved K-12 schools operate independently without interference from local and state education boards. Besides these policy interventions, this dissertation also explores how individuals' health behaviors change dues to the changing macroeconomic conditions.

The first essay (i.e., Chapter 1) estimates the causal effect of Ghana's National Health Insurance Scheme (NHIS), covering over 95% of medical costs,[1] on healthcare utilization among women of childbearing ages 15–49. Ghana implemented the NHIS nationwide with a district-level staggered rollout from 2004–2007. Estimating the causal effects of the NHIS on health outcomes is interesting for the following reasons. First, the purpose of the NHIS was to increase access to healthcare by removing the "cash and carry" system to achieve the Millennium Development Goals of reducing child mor-

[1]See more from http://www.nhis.gov.gh/benefits.aspx.

tality and improving maternal health. Under the "cash and carry" system,[2] individuals needed to pay for their healthcare utilization's full user-cost. Second, the government sold the insurance below its actuarially fair prices, subsidizing it for participants and making it free for several vulnerable groups, including pregnant women and children, through a 2.5% NHIS tax on goods and services, funding about 70% of the program's costs. Therefore, it is interesting to examine how people respond to health insurance in developing countries. Third, implementation issues in the NHIS create methodological challenges in teasing out its causal effects on different outcomes. Characterized by national coverage, universal eligibility, voluntary participation, and sharp rollouts across districts, the design of the NHIS creates endogeneity issues. Another identification challenge is that a pre-post comparison using non-experimental data is ineffectual in isolating its causal effect from the general time trend due to the universal eligibility and national rollout. Finally, although there is a vast body of literature on similar programs from developed countries, only a few studies have attempted to estimate the causal impact of Ghana's NHIS on healthcare utilization. Studying health insurance programs from developing countries is interesting because they can be different from those in developed countries due to several factors, including political environments, institutions, beliefs, and culture, affecting take-up decisions and healthcare utilization behavior. This chapter of the dissertation fills the gap in the literature by focusing on the NHIS from Ghana.

We present evidence on the causal impact of the NHIS on a variety of outcomes using the Standard Demographic and Health Survey and quasi-experimental methods that overcome prior methodological challenges. Our outcomes are twelve-month health-

[2] See more from http://www.nhis.gov.gh/nhisreview.aspx.

care use, prenatal care visits, and births in health facilities in this chapter. We leverage the district-level staggered rollout of the NHIS coverage as an exogenous source of variation to address the potential endogeneity in the NHIS take-up decisions. For the twelve-month care-seeking outcome observed only in the survey years, we employ an instrumental variable (IV) estimation strategy. We use the years of eligibility as an instrument for actual insurance participation. Using rural Nigeria as one control group, we use a difference-in-differences estimation framework for the births in health facilities and prenatal visits that are available retrospectively in all years. Our findings are that the NHIS increases twelve months healthcare utilization, institutional births, and prenatal care visits among women of childbearing ages by about 32, 6, and 7 percentage points, respectively. They correspond to about 66%, 18%, and 19% increase at their baseline means, respectively. We also demonstrate that the NHIS has heterogeneous impacts in favor of poor, low-educated, and rural women.

In Chapter 2, we study the long-term effects of charter school exposure on education and health outcomes in the United States. Excessive alcohol consumption and cigarette smoking are two health behaviors we consider. Two decades after the inception of charter schools in the U.S., we know little about their long-term impacts. Additionally, few studies have examined charter school exposure effects on students' educational outcomes using nationwide data. We use the National Center for Education Statistics and the restricted, geocoded data from the National Longitudinal Survey of Youth (NLYS) to fill this gap. Because the NLSY data did not have charter school information before 2003 but has state and county of births and residence as geographic identifiers, we link the school- and individual-level information at the county level. By exploiting variation in birth cohort, county of birth, and county of residence, we construct and use charter school exposure in the county of birth as an instrument for actual exposure in the county of residence. This enables us to address endogeneity bias since students could be making location choices based on charter school opening and attenuation bias since people do not live in their county of birth perpetually. We find strong evidence of charter school exposure increasing four-year college completion. Our results are more pronounced when we restrict our sample to include only those who already graduated from high school. We also find that charter school exposure reduces excessive alcohol consumption (binge drinking) and cigarette smoking. Finally, we find evidence of heterogeneity in the charter schools' impacts. While four-year college graduations are larger among females and minorities (i.e., Black and Hispanics), the reduction in binge drinking and cigarette smoking is higher among minorities and high-educated individuals. Our findings from this chapter demonstrate that charter school policies have long-term impacts on students' outcomes.

Chapter 3 estimates the impact of economic conditions on cigarette smoking. Although some previous literature reports substantial procyclical effects, suggesting lifestyles get healthier during bad economic times (Ruhm, 2000, 2005; Xu, 2013), updated results with long-run data, including the Great Recession period, are sparse. Using data from the Behavioral Risk Factor Surveillance System from 1987–2017 and employment data from the Bureau of Labor Statistics, we leverage variation in overtime state-level employment rates to estimate its effects on cigarette smoking. Though we find that similar estimates shown in Ruhm (2005), we also show that the impacts are widely driven by the older cohorts in our data and demonstrate evidence of attenuation bias toward countercyclical effects in the recent data. We present evidence of an imprecise zero estimate in 2019, just before the famous coronavirus pandemic, despite the large sample of individuals used in our estimations. We also show that the procyclical relationship between smoking and economic conditions declined drastically during the Great Recession period. Therefore, we expect a countercyclical relationship during the coronavirus pandemic season, suggesting that cigarette smoking will increase as the economy deteriorates. Finally, we find evidence of differential effects by race, gender, and education.

The rest of this dissertation is organized to provide detailed information on the backgrounds of policies, existing studies, conceptual frameworks, empirical strategies, data, results, and conclusions of each chapter.

Chapter 1

Health Insurance Provision and Women's Healthcare Utilization: Evidence from the National Health Insurance Scheme in Ghana

1.1 Introduction

Low healthcare utilization is an issue in developing countries. It likely leads to high maternal and infant mortality. Several countries implemented health insurance programs between 2001 and 2015, under the guidance and support of organizations such as the United Nations, United States Agency for International Development, and the World Health Organization. One such country was Ghana, which suffered from market failures under the traditional "cash-and-carry" system.[1] The government of Ghana, in late 2003, passed into law a National Health Insurance Scheme (NHIS) and started district-level rollouts in early 2004.[2] Before the NHIS, there were growing concerns regarding healthcare access because of the high incidence of home deliveries, low prenatal and postnatal care visits, self-medication, and the substitution of formal healthcare utilization for visits to traditional doctors and pastors as cheaper alternatives.

This study estimates the causal effect of the NHIS on healthcare utilization among women of childbearing ages ($15-49$). We limit our outcomes to any healthcare visits in the last twelve months, any births in health facilities (or institutional births), and any prenatal care visits in the first four months of pregnancy. Studying the impact of the

[1] Under the "cash-and-carry" system, consumers pay the full user cost of healthcare utilization. We discuss more of this system in Section 1.3.

[2] Districts, which are the third administrative division of Ghana, made rollout decisions during the rollout period. Therefore, even though the first district implemented the NHIS in March 2004, some district delayed rollout until April 2007.

NHIS on women's utilization behavior is essential because it provides evidence on the extent to which the NHIS could help Ghana achieve the MDG goals of reducing child mortality (Cesur et al., 2017; Currie and Gruber, 1996) and improving maternal health.

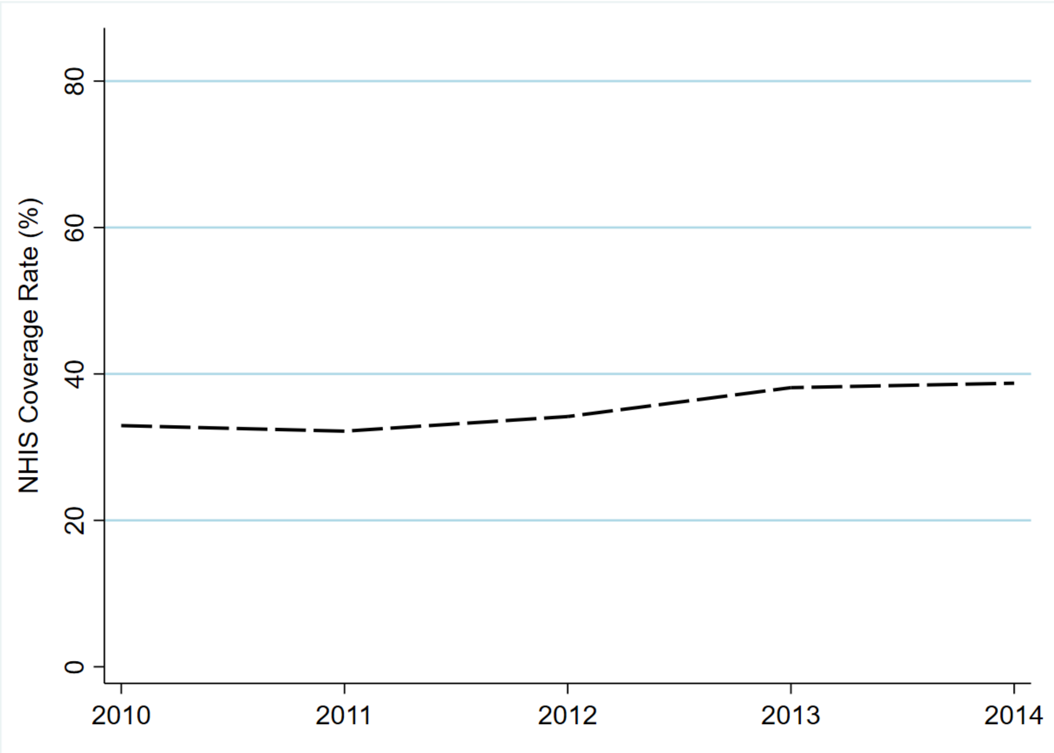

Figure 1.1. Trend of NHIS Coverage Rate in Ghana, 2010 – 2014

Source: Nsiah-Boateng and Aikins (2018)

Another important reason for studying the causal impact of the NHIS on healthcare utilization is that instituting health insurance in a developing country is challenging and sometimes unsuccessful irrespective of the program's level of generosity. Grossman's demand for health model suggests that as the price of health investment input falls, their demands rise, increasing health investments (Grossman, 1972). It indicates that government provision of cheap health insurance to places with limited health inputs and high healthcare costs reduces medical care's expected costs. Therefore, it is reasonable to expect a massive take-up of health insurance as a form of investment.

However, this has not always been the case in many developing countries. Sometimes insurance programs don't get implemented in African countries, and if they are, they are mostly not administered in a way that makes them work.[3] In the case of Ghana, even though the government sells the NHIS below the actuarially fair price, we observe low insurance coverage rates, as demonstrated in Figure 1.1. In this paper, we document the reasons for the low take-up of the NHIS and show how the NHIS induces healthcare utilization among women of childbearing ages in the presence of the numerous implementation challenges.[4]

The study uses instrumental variable (IV) and difference-in-differences (DID) methodologies. For the IV strategy, we use the timing of the rollouts across districts in Ghana to construct an instrument for NHIS participation to address the endogeneity concerns. Because districts independently made the participation decisions, there is variation in the timing of implementation (i.e., district-level staggered rollouts).[5] Using the instrument, we jointly model the decision to enroll in the NHIS and the outcome of any twelve-month healthcare visits. For the DID research design, we use rural Nigeria as a control group and the variation in the district-staggered rollouts to estimate the causal effect of the NHIS on institutional births and prenatal care visits. For most children in our sample, we do not have information on their mothers' insurance participation in utero and at birth. Since the individuals from rural Nigeria were less likely to be affected by a similar health insurance program, we use them to complement the Ghanaian children to allow us to use the DID strategy.

[3] For example, Nigeria introduced the NHIS in 1999 but was unsuccessful (Monye, 2006).

[4] The NHIS faces several challenges, which affect the participation rate. We discuss in Section 1.3 that factors that lead to low enrollment rates include household credit constraints, availability of informal healthcare and social networks, scarce supply-side factors, and cultural practices.

[5] We provide a detailed discussion of the NHIS and the rollout processes in Section 1.3 and demonstrate that the rollout of the NHIS across districts and over time are exogenous to the districts' pre-treatment outcomes and characteristics in Section 1.6.

We use the Demographic and Health Survey (DHS), available in several developing countries, as an individual-level data source. We pool the 2003, 2008, and 2014 survey waves for Ghana and 2003, 2008, and 2013 survey waves for Nigeria. Our samples consist of women of ages $15-49$ and under five-year-old children born from $1999-2013$, covering several periods of economic cycles, including the Great Recession. We also utilize administrative information on the dates of certificates of commencement of the NHIS in each district. We link the district-level information on policy dates to the individual-level data sets to define the instrument as years of exposure to the NHIS in their residential district at the survey interview date. Our final samples range from $15,000-47,000$, depending on the outcome.

Our results are as follows. From our IV model, we find that the NHIS increases women's probability of utilizing healthcare within twelve months by approximately 32 percentage points. Relative to a baseline mean of 47.7%, the estimate corresponds to 66% increase. Using the IV strategy, we also demonstrate that the NHIS has heterogeneous impacts on women with different demographic characteristics and locations. We find that the NHIS has differential effects in favor of poor women, rural women, and women with lower educational attainments.

The second set of results, which come from the DID models, suggests that the NHIS increases institutional births and prenatal care visits by about 5.7 and 6.6 percentage points, corresponding to 18.8% and 18.2% increase relative to the baseline means of 30.6% and 36.4%, respectively. We also provide event study analysis to show how the timing of the rollouts of the NHIS across districts leads to differential effects over time. We find that the NHIS has statistically significant impacts after three years of national coverage. Even though all areas in our sample implemented the NHIS before April 2007, we still do not find consistent increases in healthcare utilization until 2010. Thus, unlike similar programs from advanced countries that experience early impacts on healthcare utilization, our results are consistent with those from developing countries with little to no effect on healthcare utilization in their early stages.[6]

Our final set of results shows the impact of the free maternal care policy on institutional births and prenatal care visits. We find that the law increases deliveries in health facilities and prenatal care visits by approximately 26%. But we argue that the intent-to-treat effect could be larger if all the eligible women obtained NHIS coverage. Although our DID strategy assumes that all the qualified women received treatment, less than one-half of those who were pregnant at the interview date reported having NHIS coverage post the free maternal care policy.

This study makes four contributions to the literature. Since its inception, many studies have analyzed the impacts of the NHIS on different outcomes.[7] Some of these studies failed to address potential endogeneity concerns in the NHIS. Consequently, they do not make any causal interpretations of their findings. Similar to other health insurance programs elsewhere with endogeneity issues (Einav et al., 2013; Simon, 2005; Sapelli and Vial, 2003), Yilma et al. (2012) document evidence of moral hazards in the NHIS. Other studies that examined the causal effect of the NHIS have either methodological challenges, data limitations with external validity concerns, or focused on different populations of interest (e.g., under five-year-old children).

As our first contribution, we address critical methodological issues regarding the

[6]For example, in the US, the Oregon health insurance experiment (Finkelstein et al., 2012) and the Affordable care Act (Courtemanche et al., 2017) affected healthcare utilization in their early stages. In developing countries [for example, the case of the Chinese cooperative medical scheme (Wagstaff and Yu, 2007)], universal healthcare coverage had little impact on the use of services in their initial implementation stages. But the NHIS in Taiwan is an exceptional program. Studies show that the take-up was high. It also affected healthcare utilization a few years after its implementation (Chen et al., 2007).

[7]Examples of these studies include Dzakpasu et al. (2012); Ansah et al. (2009); Brugiavini and Pace (2016); Abrokwah et al. (2019); Mensah et al. (2010); Abrokwah et al. (2014); Powell-Jackson et al. (2014); Bonfrer et al. (2016); Agbanyo (2020).

endogeneity of health insurance choice and the design of the NHIS to interpret our estimates causally. By design, the NHIS participation is endogenous. Because of universal eligibility and voluntary participation, individuals with poor health expected to be sicker are more likely to enroll in the NHIS. Such people are more likely to self-select to obtain coverage and utilize healthcare excessively. Besides, behavioral responses in the form of ex-ante and ex-post moral hazards can be additional issues in the NHIS. In the absence of cost-sharing measures and cap on healthcare utilization after gaining coverage, participants of the NHIS can engage in risky behaviors, underinvest in other health inputs, or use healthcare excessively. Another issue from the design of the NHIS is that since everyone is eligible and rollouts across districts were sharp, it renders any pre-post comparison using non-experimental data ineffectual in isolating the causal effect of the NHIS from the general time trend.[8] Without accounting for these concerns, we cannot interpret our results causally. Our empirical strategies and data allow us to address the endogeneity concerns and disentangle the causal effect of the NHIS from time trends.

Second, we contribute by studying how people respond to health insurance policies in developing countries. Contrary to the vast body of literature on public health insurance programs in developed countries, we do not have enough evidence from different programs from developing countries to make conclusions about the impacts of health insurance on utilization (Abrokwah et al., 2019; Chen et al., 2007). In underdeveloped nations, several factors, including lack of health facilities, political and institutional settings, and culture, can influence people to make sub-optimal choices. This study provides evidence from Ghana's adoption of the NHIS, which faces most of these

[8]Several surveys in Ghana occurred either before or after the rollout period. The implication is that all the people had no coverage in the pre-NHIS period, and every individual in Ghana was eligible in the post-NHIS period. Therefore, there is no variation in the treatment of the NHIS across districts over time in these surveys.

challenges.[9] Additionally, our results from the NHIS in Ghana can be useful to policymakers from other developing countries without health insurance programs and those with but are unsuccessful. Since many African countries have no health insurance programs, the NHIS in Ghana can guide them.[10] Our causal estimates from the NHIS in Ghana can help policymakers in these countries to make informed decisions, minimizing uncertainties, if they decide to implement a similar healthcare policy.

Another important aspect of the study is that we provide the most credible causal estimates on the NHIS by overcoming possible misreporting in self-reported insurance participation.[11] In the absence of administrative data, studies rely on self-reported NHIS participation information in surveys subject to possible misreporting. Because the literature has demonstrated that the bias in a misreported-binary program participation variable is nontrivial (Wossen et al., 2019; Nguimkeu et al., 2019; Battistin and Sianesi, 2011), we take measures to overcome it. For the instrument used in the IV model, our data sets allow us to define NHIS participation to include only the women with verified valid NHIS cards. We argue in Section 1.6 that, unlike similar studies on the NHIS with misreporting concerns (Mensah et al., 2010; Bonfrer et al., 2016; Abrokwah et al., 2014, 2019), our definition of NHIS participation enables us to replicate the trends in national enrollment rates shown in Figure 1.1. It suggests that our estimates would be biased if we included women without verified NHIS cards.

Finally, we focus on the healthcare utilization outcomes of women and children

[9] Although there are many studies on social health insurance programs from developed countries, Ghana cannot adapt their estimates due to the differences in the levels of need and other factors. Moreover, this study is also relevant in developed countries due to disparities in geographic characteristics, demography, and economic conditions. In developed countries, programs like the NHIS could have differential treatments across different demographic groups. If the targeted population is similar to the Ghanaian people, then the causal estimates from the NHIS can be reasonably inferred. Therefore, policymakers in developed countries can also learn from programs in underdeveloped nations if they have a similar targeted population.

[10] For example, Burkina Faso, Cameroon, Lesotho, Malawi, Mali, and Zambia do not have any health insurance programs.

[11] A misclassification of NHIS participation occurs when some individuals report treatment status, that is, report being covered by the NHIS when they are not ("false positives") or report as uninsured when they have NHIS coverage ("false negatives").

since insurance is particularly for these groups for the following reasons. One reason is that they were the sub-groups of the population in Ghana who were most vulnerable before implementing the NHIS. Hence, we expect that the highest impacts of the NHIS would be among such sub-population. Because infant and maternal mortality were high and maternal healthcare utilization was extremely low, we focus on these groups to understand how insurance changed their utilization behavior. Consequently, the NHIS was designed with different generosity levels, which favored most women and children as described in detail in Section 1.3. Another reason why focusing on child and maternal healthcare use is important is that because they have long-run consequences on health, as demonstrated in the case of other health insurance programs such as the Medicaid in the US (Miller and Wherry, 2019; Boudreaux et al., 2016). We also argue that because household assets and decision-making power are controlled mostly by men, putting women and children at a disadvantage, the NHIS is expected to improve their health outcomes significantly. Even with some women's healthcare utilization decisions, some households still have men making choices. On the economic side, the unemployment rate among women is high compared to men in Ghana, putting them at a disadvantage to afford the NHIS. For example, in 2014, the DHS survey estimated that past twelve months unemployment rate among women was about 23%, while that of men was approximately 14%. Lastly, the data used in this study does not provide healthcare utilization information for men, limiting us to focus on only women and children.

We structure the article as follows. Section 1.2 provides a brief literature review on the effects of the NHIS on healthcare utilization. We provide background information of the NHIS and discuss the conceptual framework for understanding its impact as well as possible factors that hinder take-up in Section 1.3. In Section 1.4, we describe the data for the study, followed by a full description of the empirical strategies used to identify the causal impact of the NHIS on the outcomes in Section 1.5. We present our results in Section 1.6 and conclude in Section 1.7.

1.2 Literature Review

Some studies show correlations between the NHIS and healthcare utilization (Chankova et al., 2009; Dzakpasu et al., 2012; Brugiavini and Pace, 2016; Blanchet et al., 2012; Agbanyo, 2020). Since these studies do not make any causal claims due to the endogeneity concerns in the NHIS that they fail to address, we do not discuss them in detail. As evidence of endogeneity in the NHIS, Yilma et al. (2012) find that households with NHIS are less likely to invest in malaria preventative inputs. By obtaining full NHIS coverage that makes malaria treatment free, they are less likely to own mosquito bed nets and, even more so, are less likely to sleep under the nets. Also, the fact that individuals voluntarily select into the NHIS, unobserved heterogeneity, including health status, preferences, and risk behaviors, affect the decision to enroll in the NHIS and health-seeking behaviors (Abrokwah et al., 2014).

To the best of our knowledge, we find only two articles on NHIS that relate to our study in part. Both studies use district-level variation in the timing of NHIS rollout to tackle endogeneity in NHIS participation.[12] Abrokwah et al. (2014) estimate the effects of the NHIS on prenatal care visits using two-parts models and finds that the NHIS increases prenatal care visits. A limitation of their study is that they use data from

[12]We also use a similar identification strategy. However, while these studies use a 0/1 IV for NHIS participation, we improve by using the number of years of exposure to the NHIS before the interview dates. Several studies have used staggered rollouts of programs as exogenous sources of variation to evaluate the program's impact on outcomes of interest (Gruber and Hanratty, 1995; Ater et al., 2017).

only the 2005/6 wave of the Ghana Living Standard Survey and restrict their sample to a few women who were pregnant within one year before the survey interview date. We overcome the external validity concerns of their study by using a large sample of children from several districts in Ghana, born from $1999 - 2013$. The second study, Abrokwah et al. (2019), finds strong evidence that participation in the NHIS increases formal and informal care use. Our study differs from Abrokwah et al. (2019) since we estimate the impact of the NHIS on different and specific utilization outcomes.

Two studies use propensity score matching methodology but fail to convincingly estimate the causal impact of the NHIS on healthcare utilization. Bonfrer et al. (2016) study maternal healthcare utilization using the 2008 wave of the DHS survey data and find that NHIS participation increases prenatal and postnatal care visits, but decreases the number of unwanted pregnancies, and has no effect on child vaccination. A drawback in their study is the assumption that the mother's enrollment status at the interview date is representative of their enrollment status during pregnancy among under two-year-old children. They possibly misclassified NHIS participation and reported that 39.8% of the women in their sample had NHIS coverage. However, we use the same data source to demonstrate that only 25% of the women had NHIS coverage in 2008. The severity of their assumption is that standard errors-in-variable methods do not easily overcome non-classical measurement error that their misclassified binary endogenous NHIS variable introduces into the models. We define insurance participation to include only the women with verified valid NHIS cards to avoid measurement error concerns.[13]

One other study, Mensah et al. (2010), administered a survey to recruit $2,000$ women from two administrative regions of Ghana, but could only match a sample of

[13] We describe the data in detail in Section 1.4.

625 of them to evaluate the impact of gaining health insurance on healthcare utilization. They find that the women who enroll in the NHIS are more likely to utilize prenatal care services and deliver in health facilities. Because the women recruited in the survey come from only two of the ten administrative regions of Ghana, their sample may not represent the Ghanaian population. Therefore, the external validity of their estimates is a concern.

Using randomized control trials (RCTs), Ansah et al. (2009) and Powell-Jackson et al. (2014) study the effects of free NHIS enrollment on healthcare utilization among under five-year-old children.[14] Powell-Jackson et al. (2014) find that the provision of free NHIS increases the number of annual visits to clinics but reduces informal care use and financial stress, including out-of-pocket spending and borrowing, but do not affect the number of annual hospital visits and health outcomes of children. Ansah et al. (2009) find similar results that providing children with free access to healthcare through the NHIS increases formal healthcare utilization and decreases informal healthcare use. Our study differs from these studies in two ways. First, although both articles use experimental data that may be preferred to survey data, the individuals from the few poor rural districts are not representative of the Ghanaian population. Therefore, external validity is a concern in their study. We use data from several districts, which overcome the external validity concern. Second, these studies focus on the outcomes of under five-year-old children. But we consider the utilization behavior of women age $15-49$, which we expect the NHIS to affect differently compared to the under five-year-old children.

[14] In an experiment, Powell-Jackson et al. (2014) provided randomly assigned treated households in one poor rural district in Southern Ghana, with free healthcare by paying their enrollment fees for the NHIS. They then study the children's outcomes, including the use of healthcare, health status, and financial strain. Similarly, Ansah et al. (2009) provided free health insurance to some children in two districts to compare their outcomes to those of the control group. They then study the effects of free health insurance on anemia, healthcare utilization, and mortality.

1.3 Program Overview and Conceptual Framework

1.3.1 The Political Economy of Ghana and the NHIS

Ghana is a West African country that shares a border with Burkina Faso to the North, the Gulf of Guinea to the South, Togo to the East, and Cote d'Ivoire to the West. It gained independence from the British in 1957 and faced a lot of political unrest until 1992. Since then, it has consistently and democratically voted every four years and changed the ruling government every eight years.[15] The estimated population of Ghana in 2018 was approximately 29.8 million, with an annual growth rate of 2.2%. Timber, gold, diamond, bauxite, oil, etc., are some of the natural resources endowed in Ghana, but more than 70% of its economically active population engages in agriculture, forestry, and fishing (Ghana Statistical Service, 2014). A significant part of Ghana's export commodities comes from cash crops, including cocoa, oil palm, and cashew. Post-independence until the late 1980s, agricultural raw materials accounted for more than 50% of its Gross Domestic Product (GDP),[16] but the service and industry sectors have become popular recently (Aryeetey and Fosu, 2003).

Financed by tax, the Ghanaian government, as part of its 10-year development plan after independence in 1957, included an expansion of existing public health facilities, reduction of healthcare user fees, and in most cases, completely free healthcare (Arhinful, 2003). But the private healthcare centers charged unsubsidized user fees. Because of a series of political unrest between 1965 and 1982[17] and worsening economic

[15]Starting from 1992, only two political parties have governed the country. The National Democratic Congress ruled the country from 1993 - 2000, and 2009 - 2016, while the New Patriotic Party (NPP) ruled from 2001 - 2008, and 2017 - date.

[16]We demonstrate later that during the post-independent period until 2005, Ghana did not experience any significant growth in its GDP per capita.

[17]During this period, military men overthrew governments, and successive governments killed prominent people.

conditions, subsidizing the healthcare industry was not a sustainable policy option for the government (Yevutsey and Aikins, 2010; Fusheini et al., 2012). After the period of political instability, prolonged drought, widespread bush fires, declining agriculture, and famine further worsened economic conditions. It forced Ghana to adopt the International Monetary Fund and World Bank-sponsored structural adjustment program in 1983. The government of Ghana then cut down fiscal expenditure and abolished all healthcare subsidies through the adjustment program (Ankomah, 2004; Fusheini et al., 2012). The Ghanaian government instituted user fees of healthcare utilization to generate revenue to finance the industry. The charging of user fees continued through 1992 when another restructuring of the healthcare industry occurred to impose the full costs of using public health services on consumers, a system popularly known as the "cash and carry." The cost of medical care upsurged and resulted in low participation in formal healthcare utilization. The majority of the Ghanaian residents substituted health facility visits for alternatives, including self-medication, traditional medicine, and spiritual healing (Fusheini et al., 2012).[18]

With support from the United States Agency for International Development, the Government of Ghana enacted and implemented the NHIS in early 2004 to address the several issues that the "cash and carry" system created. Before instituting the NHIS, private, employer-based, and community-based health insurance schemes existed but were limited to a few areas.[19] Approximately 3% of the Ghanaian population participated in all insurance programs, according to the DHS 2003 survey report. Over time, enrollment in the NHIS has been increasing. Figure 1.1, which plots the trend of NHIS

[18] Because of these issues, maternal and infant healthcare utilization services were low and maternal, and infant mortality was high. For example, only 25% of pregnant women had at least one antenatal care visit in 1998, and under five-year-old mortality in Ghana is 108 deaths per $1,000$ live births (Ghana Statistical Service, 1999).

[19] Atim et al. (2001) provide a list of all the communities with healthcare financing schemes before the implementation of the NHIS.

coverage from 2010 – 2014, shows an increasing NHIS coverage over time.[20] Part of the reason is that the NHIS covers about 95% of all health issues and drugs in Ghana.[21]

The rollout of the NHIS occurred at the district level, which is the third administrative division of Ghana. Districts that wanted to participate in the NHIS needed at least 2,000 individuals to register initially, subject to review every six months. The NHIS by design have two types of costs (i.e., renewable registration fee and annual premium). All districts were initially required to charge premium based on the consumer's "ability to pay" with the yearly registration fee and premium ranging from ₵7,000 – ₵50,000 [i.e., ¢77 – $5.52 in 2005 U.S. dollars][22] and ₵72,000 – ₵480,000 [i.e., $7.95 – $53.03 in 2005 U.S. dollars] (Abrokwah et al., 2019; Blanchet et al., 2012). Because there were no formal ways of determining the income of participants, districts decided to charge flat rates for both the premium and registration fees.[23] Because districts made the participation decisions, rollouts were staggered from 2004 – 2007. While some areas began to roll out the NHIS in 2004, others delayed until 2007.

Through the NHIS, the government of Ghana also tries to extend healthcare access to impoverished individuals. The policymakers use a non-linear subsidy structure to divide the Ghanaian population into three groups. Individuals with mental disorders, indigents, and those on government cash transfer programs form one group and are eligible for free NHIS coverage. As a complementary policy to achieve the Millennium Development Goals, the government of Ghana enacted a free maternal health

[20] We computed the annual coverage rate as the fraction of the population who were active participants.

[21] Among the items covered by the NHIS are malaria, asthma, diabetes, hypertension, free maternal care for pregnant women, out-patient, in-patient services, oral health, eye care, healthy delivery, and complicated delivery. See more from http://www.nhis.gov.gh/benefits.aspx

[22] We used the 2005 average cedi (i.e., the old Ghanaian currency) to the U.S. dollar exchange rate of ₵9,051.95 = $1. Ghana redenominated its cedi currency in July 2007 at a rate of ₵10,000=GH₵1.

[23] Because many people in Ghana work in the informal sector and do not file taxes annually, it is almost impossible to verify their self-reported income. Most districts charged a fixed premium of ₵72,000 despite the income disparities among participants (Abrokwah et al., 2014).

policy in July 2008, which provides pregnant women free NHIS coverage to cover their pregnancies, childbirths, and three months postpartum (Dalinjong et al., 2018). Second, the formal sector employees, Social Security and National Insurance Trust (SSNIT) pensioners, adults over 70 years, and children under 18 years whose parents have NHIS coverage form another group and receive a partial subsidy. They pay only the registration fee and no premium for the NHIS. The rest of the population (i.e., informal sector employees) form the last group and face the full price of the NHIS with no subsidy. Without any restrictions, all Ghanaian citizens are eligible for the NHIS so far as it is available in their residential districts, and enrollment occurs throughout the year with a waiting period of three months. Unlike the Affordable Care Act in the U.S., the mandate arm of the NHIS requiring every Ghanaian resident to enroll in insurance is ineffective since there is no penalty for those without coverage.

The formal sector employees pay only registration fees because the government of Ghana deducts 2.5 percentage points of their SSNIT monthly contribution to fund the NHIS. Aside from these sources of funds, the primary source of revenues for the NHIS is taxation of goods and services. A national health insurance levy (NHIL) of 2.5% on all goods and services under the Value Added Tax (VAT) funds the NHIS. Relative to the total costs of implementing the NHIS, premium collection constitutes a small fraction of revenue. A large proportion of the NHIS revenue comes from the NHIL levy and SSNIT contributions. For example, the total premium accounted for only 3.4%, while the SSNIT contributions and NHIL levy generated 20.4% and 73.8%, respectively, of the total revenue in 2014.[24]

[24]The National Health Insurance Authority (NHIA) is responsible for managing all the funds of the insurance program. The NHIA compiles registration lists from districts, receives funds for the NHIS, and disburses funds to health care providers. Also, the NHIA deposits all the funds into a central National Health Insurance Fund (NHIF) and disburses proportional to each district's contribution (Alhassan et al., 2016). See more from http://www.nhis.gov.gh/nhisreview.aspx and http://www.nhis.gov.gh/about.aspx

1.3.2 Conceptual Framework

Grossman (1972) provides a basic framework for understanding the demand for health. In his model, people demand "good health" for two reasons. First, in the form of sick-free days, they seek "good health" as a consumption commodity since they become happy when healthy. To produce the "good health," they inherit initial stock of health capital (i.e., their human capital that depreciate with age and increase through investments) and die when their stocks of health fall below certain levels. Second, they demand "good health" as an investment commodity that determines the time available for market and non-market activities. They can work for wages or engage in home productions during sick-free days. One conclusion from the model is that a ceteris paribus reduction in the price of a health input reduces the "shadow price" of health, and the amount of health demanded increases. The "shadow price" of health depends on the cost of medical care and other factors; a shift in any of these factors changes the amount of health demanded and the derived demand for health investment.

The provision of the NHIS in Ghana is similar to reducing the price of health inputs in Grossman's model except for those in perfect health who would spend nothing even without the insurance program. Before the NHIS, the Ghanaian health industry had a limited number of health inputs and high medical costs. There was almost no health insurance in many areas before the NHIS became available. The implementation of the NHIS reduced the expected cost of medical care.[25] Therefore, we expect the majority of Ghanaian residents to enroll in the NHIS. Surprisingly, the annual NHIS participation rate has always been lower than one-half of the Ghanaian population, even

[25] The cost per bed day and outpatient visit in a hospital were about ₵60,000 and ₵18,000, respectively, in 2005. For more information, see from https://www.who.int/choice/country/gha/cost/en/.

after a decade of national coverage (see Figure 1.1). In part, a theoretical argument for the low take-up is that healthcare utilization may be exhibiting diminishing marginal returns (Folland et al., 2016). However, since healthcare utilization was low before providing the NHIS, we rule out this explanation. We devote the rest of the section to discuss the potential reasons for the low NHIS participation rate.

The household's budget constraint is one reason why some individuals cannot enroll in the NHIS. Although the expected cost of obtaining coverage in the NHIS is lower than the expected expenditure on healthcare utilization, some individuals are unable to enroll due to credit constraints. Given that the majority of the Ghanaian population lives under \$1 every day, the cost of obtaining the NHIS represents a significant expenditure on the budgets of households with low socioeconomic status and large family sizes (Kusi et al., 2015). A survey of households by Kusi et al. (2015) show that about 29% of individuals without NHIS coverage face credit constraints. In our data, about 25% of the women without NHIS coverage in 2014 believes that the NHIS is expensive.

Another reason is that most Ghanaian people patronize the informal healthcare industry, which serves as substitutes or complements to the formal healthcare sector. The government permits the use of traditional, complementary, and alternative medicines as forms of informal healthcare.[26] Evidence shows that about 70% of the Ghanaian population depends exclusively on traditional medicine for their healthcare (WHO, 2001). A recent survey by Gyasi (2015) shows that approximately 87% of their sample uses traditional and alternative medicines.

We also argue that risk-sharing opportunities and social networks serve as al-

[26]Complimentary or alternative medicine refers to other traditional medicines imported from other countries, but not part of Ghana's traditions. In Ghana, complementary or alternative medicines are highly patronized and usually advertised on the media.

ternative insurance for some people in Ghana. Because well-designed insurance programs were initially not available in Ghana, some extended-families, communities, or villages often provided mutual insurance to mitigate impacts of shocks. There is evidence on the existence of risk-sharing behaviors of group members of organizations and the availability of financial assistance in the event of shock (Goldstein et al., 2002; Fenenga et al., 2015). We suspect that sometimes, joining informal organizations crowd out NHIS coverage due to budget constraints.[27]

Supply-side factors, including access to health facilities and trust in healthcare and NHIS employees, potentially affect participation in the NHIS. Individuals who trust workers in the healthcare industry are more likely to obtain coverage (Fenenga et al., 2015). Kusi et al. (2015) find that most of the individuals in their sample who complained about the poor services from the formal healthcare industry were less likely to participate in the NHIS.[28]

Finally, we argue that religion and culture can affect people's participation in the NHIS. Religious and cultural norms play essential roles in Ghana's healthcare industry. Prayer for healing is a common practice in Ghana, where people seek divine healing from pastors and spiritual superiors. Some people also practice self-medication using their experiences and knowledge in drugs and herbal medicines.[29] A qualitative evidence on the negative relationship between religious and cultural norms and participation in the NHIS exists (Fenny et al., 2016).

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[27]Usually, the expected benefits of becoming a group member of an organization expand beyond just mitigating health shocks even though the evidence is weak in the literature (Fenenga et al., 2015).

[28]Examples of the claims are perceived poor quality of health services, lacked trust in scheme officials, lacked health facilities in their area, experienced negative attitudes from providers, etc.

[29]People visit chemical or drug stores to purchase medications without doctor's prescriptions, except a few, in Ghana.

1.4 Data

We use the restricted geocoded Standard Demographic and Health Survey (DHS) for Ghana and Nigeria as our primary data source. Supported by the U.S. Agency for International Development, the DHS program has assisted over 400 surveys in about 90 developing countries to conduct irregular, but high-frequently in-depth household-level surveys of health since the late 1970s (Young, 2013).[30] Ghana and Nigeria have benefited from the DHS program since 1988 and 1990, respectively. The surveys collect, analyze, and distribute accurately, a wide range of standard information across countries.[31] At the country-level, they provide nationally representative data on fertility, family planning, maternal healthcare utilization, gender, HIV/AIDS, malaria, and nutrition. All data sets from the DHS surveys are publicly available except for information on HIV and residential location. Countries restrict these sensitive data for confidentiality. We pool the 2003, 2008, and 2014 survey waves from Ghana as well as 2003, 2008, and 2013 survey waves from Nigeria.[32] DHS surveys cover many families and have high response rates, as the Ghanaian waves include $6,200$, $12,000$, and $11,800$ households interviewed in 2003, 2008, and 2014, respectively. Their response rates are at least 95.7%. Similarly, the DHS survey in Nigeria recruited $7,225$, $34,070$, and $38,522$ households in 2003, 2008, and 2013, respectively, with a minimum response rate of 98.3%.

They administer three main questionnaires (i.e., household, women, and the men

[30] For detailed information, see https://dhsprogram.com/Who-We-Are/About-Us.cfm

[31] Although they also collect country-specific information, most of the questions and their unit of measurement are standard across countries.

[32] Although there are three other earlier survey waves available for both countries, they do not have all the information needed for this study, including twelve-month healthcare visits. Also, the 1999 survey wave for Nigeria is not publicly available.

questionnaires) with different eligibility criteria. Eligibility for the men's sample is ages 15–59, while that of the women is ages 15–49. In the women questionnaire, the survey collects a broad set of questions on health insurance and healthcare utilization. We construct a binary indicator variable for past twelve-month healthcare visits as one outcome.[33] For under five-year-old children, detailed information on their places delivered is available. The surveys also provide a history of the mother's last birth prenatal care visits for under five-year-old children.[34] We link the women and child's samples to create an indicator variable for antenatal care visits and births in health facilities as two other outcomes.

Our variable of interest in this study is the woman's insurance participation status when utilizing healthcare. In the DHS survey, the women provide information on their health insurance coverage at the interview date. The surveys also ask additional questions to identify the type of health insurance plan that respondents patronize. Insurance plans available in the DHS surveys from Ghana are the NHIS, employer-based health insurance, mutual health organization (or community-based insurance), private health insurance, and commercial health insurance. However, over 98% of insurance participants obtain NHIS coverage. Consequently, we drop the women in the 2008 and 2014 surveys with insurance coverage from other types to focus on just the NHIS.[35] One advantage of the DHS survey is that it verifies the validity of the responses to the NHIS participation questions. They ask the women to provide their valid NHIS cards if they claim to have coverage.[36] We construct the NHIS participation variable to include

[33]We are unable to use the men's sample in this study because there is no information on their healthcare utilization. Additionally, information on postnatal care visits is available for only under-two-year old children.

[34]We use the date of birth to create outcomes from 1999 - 2013. However, we do not have enough samples in 2004 and 2019 for individuals from Ghana. Since rollouts of the NHIS began in 2004 and a few districts enrolled, we add the few individuals from 2004 to the 2003 samples, and 2009 to 2008 for the prenatal care visits outcome.

[35]However, we use private health insurance enrollment before the NHIS for identification purposes.

[36]In the questionnaires, respondents answer questions on health insurance participation status. The follow-up ques-

only the women whose NHIS cards are verified.

Our second source of data comes from the National Health Insurance Authority (NHIA), who provided information on the dates of commencement of NHIS in districts. We do not have full details on the rollout dates for all areas in Ghana. However, we have information for 111 of the 130 districts in Ghana. From the NHIA data, we construct our instrument, the years of exposure to the NHIS in the district of residence at the interview date and link it to the individual data sets from the DHS at the district level. Therefore, districts are the level of treatment in all our analyses.

A limitation of the DHS data is that information on women's insurance participation outcomes is known only in the survey year. For under five-year-old children whose data are available in off-survey years, we do not have information on the mother's insurance participation status at the time of conception or childbirth. Unfortunately, we lose most of the children if we use only the under one-year-old children whose mother's NHIS participation outcomes are available. Another challenge from restricting the data is that we would not have enough observations for estimation. The consequence of using a small sample is that we would lose statistical power and would be unable to detect statistically significant effects of the NHIS on the outcomes. Consequently, we are unable to use the DHS data from Ghana to estimate the impacts of the NHIS on prenatal care visits and institutional births unless we assume that the mother's enrollment status at the time of the interview is a representative for the enrollment status during pregnancy or delivery. We avoid such an assumption because it can lead to non-

tion is to identify the type of insurance that they purchase if the answer to the previous questions is "Yes." The next question probes to verify from those who claim to be NHIS participants if they have valid NHIS cards. Because the interviewers want to confirm the validity of their responses, they check the NHIS cards and categorize them as follows: "Yes, card seen," "Yes, card not seen," and "No." The last question that validates these answers is to determine why some of the NHIS participants did not hold valid NHIS cards. In our sample, most respondents either do not renew their insurance status on time or are new participants and in the three-month waiting period.

classical measurement errors in our variable of interest.[37] Therefore, we use children from Nigeria as one comparable group and define our treatment group as all children from Ghana in the samples.

1.5 Empirical Strategies

Consider the structural equation for binary choice healthcare utilization:

$$Y_{idt} = \mathbf{1}(\beta_0 + \beta_1 I_{idt} + \mathbf{\Lambda X_{idt}} + \boldsymbol{\gamma_d} + \boldsymbol{\tau_t} + \epsilon_{idt} > 0), \tag{1.1}$$

where $\mathbf{1}(\bullet)$ is the indicator function taking the value one if its argument is true and zero otherwise, Y_{idt} represents the outcomes, any healthcare visits in the past twelve months, births in health facilities, and any prenatal care visits, for individual i in district d at time t. The variables, I_{idt} and ϵ_{idt}, represent NHIS participation outcome and the error term, respectively, for individual i in district d at time t. Notice that the observed insurance participation, I_{idt}, takes the value one if the individual purchases the NHIS and zero otherwise. The vector $\mathbf{X_{idt}}$ represents the set of individual and household characteristics such as age, gender, occupation, education, wealth index, a dummy for residential locations (rural or urban), etc. We include a vector of district and year fixed effects (i.e., γ_d and τ_t, respectively) to account for district-level and time-invariant characteristics that contribute to changes in Y_{idt} other than the NHIS.

For each outcome, our econometric objective is to estimate causally the value of β_1, which represents the impact of the NHIS. However, an identification challenge is the issue of endogeneity in the NHIS participation outcome. As discussed earlier, I_{idt} has at least two significant sources of endogeneity. Adverse selection stems from the fact

[37]We discuss the consequences of the misclassification of the NHIS participation variable later.

that some individuals make participation decisions based on their type (i.e., healthy or unhealthy). Because everyone in Ghana is eligible for the NHIS, sicker individuals are more likely to participate. Also, given that there is open enrollment throughout the year with only a three-month waiting period, individuals can strategically make participation decisions. The issue of "ex-ante moral hazard," which means lower investments in health and behavioral changes in anticipation of using health insurance to access healthcare, is documented in the literature as another issue in the NHIS (Yilma et al., 2012). Therefore, for a causal interpretation of $\hat{\beta}_1$, we address these problems. We use different empirical strategies to address the endogeneity, depending on data availability.

1.5.1 Instrumental Variable Strategy

For the twelve-month healthcare visits outcome, which we observe only in the year of the survey, we use an instrumental variable (IV) strategy to address the endogeneity issues in NHIS participation. We specify the insurance participation equation as follows.

$$I_{idt} = \mathbf{1}(\alpha_0 + \alpha_1 \mathrm{Z_{dt}} + \boldsymbol{\theta}\mathbf{X_{idt}} + \eta_{idt} > 0), \tag{1.2}$$

where the variable η_{idt} represents an independently and identically distributed error term for individual i in district d at time t. The rest of the variables and indicator function, $1(\bullet)$, have their usual interpretations.

In equation (2), the variable Z_{dt} denotes our instrument, varying across districts and over time. We use the timing of the rollout of the NHIS across districts and the survey interview dates to construct the IV. Specifically, we define the IV as the number of years that the individuals become exposed to the NHIS in their residential districts.

The IV has no variation within districts surveyed in the same year because they survey individuals in the same area in the same month. Although Abrokwah et al. (2019) define their IV as a binary indicator of the presence of the NHIS in the district of residence, we improve it by allowing for variations in the years of exposure. Our intuition for using the years of NHIS exposure as an IV is that the women at the margin of signing up in the NHIS are more likely to obtain coverage as they get more years of exposure. We argue that some individuals require more time to understand the NHIS, trust it, or save money to purchase coverage. Others may need advertisements, testimonies, or an awareness of its importance in enrolling in the NHIS.

Angrist and Pischke (2008) review the assumptions needed to interpret our estimates causally. The first identifying assumption for a valid instrument is exclusion restriction requiring Z_{dt} to be uncorrelated with ϵ_{idt} [i.e., $Cov(Z_{dt}, \epsilon_{idt}) = 0$], enabling us to interpret the IV estimates as local average treatment effects (LATE). It imposes a restriction that NHIS participation is the only channel through which years of insurance eligibility affect the healthcare utilization outcomes, ignoring all other potential channels. We argue that this assumption is likely satisfied based on our knowledge of the institutional details. Since the NHIS rollout decision and premium choices were made by district executives, presided over by the chief executive officer appointed by the president of Ghana, they were influenced politically rather than need. We demonstrate that the years of eligibility are uncorrelated with the outcomes and pre-treatment characteristics of the districts in Section 1.6. Stating the exclusion restriction assumption differently, it says that the only channel through which Y_{idt} and Z_{dt} are related is through I_{idt}. This assumption is likely to be satisfied because districts don't have control over resource allocations, including the construction of healthcare centers and the posting of health workers. The government of Ghana determines how and where to allocate resources, likely uncorrelated with years of NHIS eligibility. In Section 1.6, we demonstrate that including supply factors in the models does not affect our estimates.

In the case of heterogeneous treatment effects, likely the case of the NHIS based on institutional details and results presented in Section 1.6, a second part of the identifying assumption required is independence, the NHIS rollout as good as random (or independent of potential outcomes, conditional on the observable characteristics). Satisfying the independence assumption would not require the exclusion restriction for identifying the reduced form estimates. In other words, the reduced form estimates would capture all the possible channels through which the NHIS affects the utilization outcomes.

Another identifying assumption that we discuss is the monotonicity of the instrument. Although years of NHIS eligibility may not affect some people, the impact must be in the same way (direction) among those affected. In other words, eligibility should not increase the NHIS take-up among a group of people, decreasing the NHIS participation among another sub-population. The final identifying assumption is the relevance of the instrument or first stage. The estimate of α_1 should be different from zero with a high F-statistic not less than 10 in practice. In Section 1.6, we show that the F-statistic is approximately 30, suggesting a strong first stage.

To understand the IV better to interpret the first stage appropriately, we briefly discuss it as follows. First, we show that the rollout of the NHIS across districts and over time is staggered. Figure 1.2 shows a plot of the count of districts that adopted the NHIS over time. We observe that there is a significant variation in the dates of NHIS adoption. Second, we show the distribution of the IV in our data in Figure 1.3.

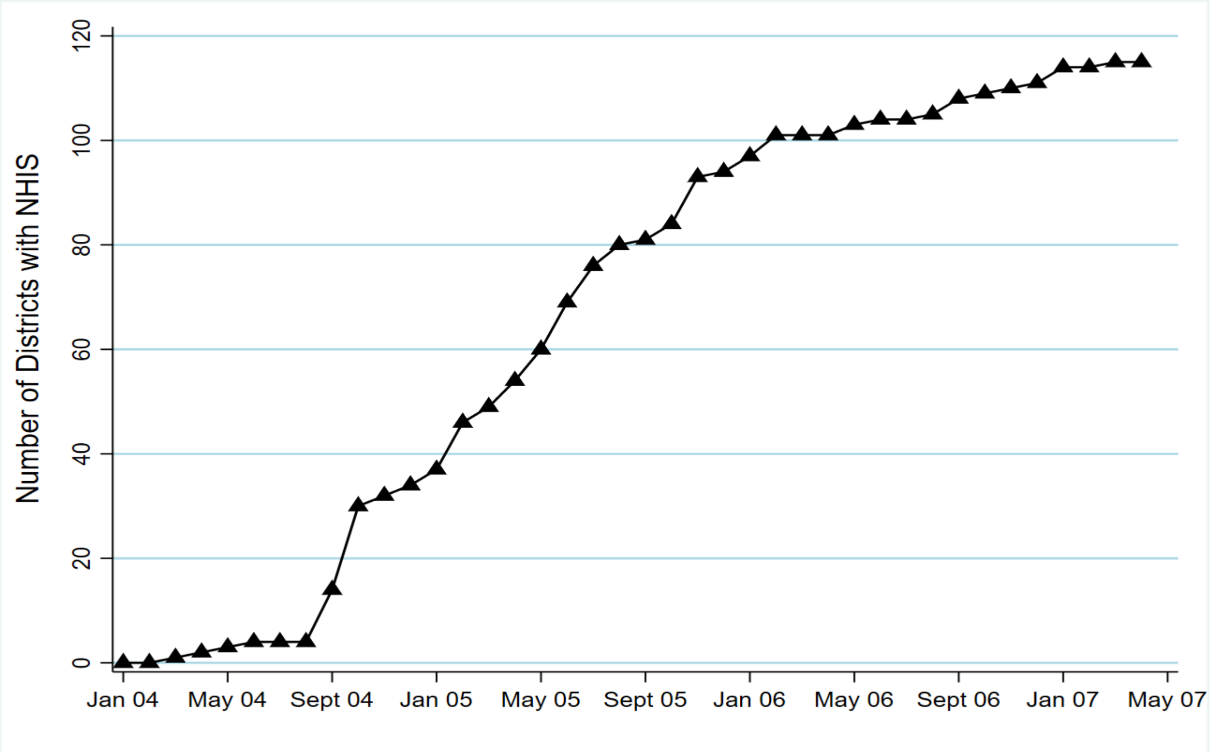

Figure 1.2. Timing of Rollout of NHIS, 2004 – 2007

Among the women exposed to the NHIS, the IV values range from approximately one year and five months to nine years and six months.

Because the outcome, Y_{idt}, and the endogenous regressor, I_{idt}, are binaries, we do not interpret our main results from the linear IV estimator. With a binary outcome and a binary treatment variable, the econometrics literature has documented important problems using a linear IV estimator (or linear probability models). At best, the linear IV estimator may only approximate the average marginal effects, which sometimes turns out to be poor in practice (Wooldridge, 2010). The reason is that the conditional mean functions in equations (1) and (2) can be highly nonlinear such that their "derivatives can be quite far from the derivatives of their linear approximations" (Lewbel et al., 2012).[38] The poor approximations of the nonlinear models from the linear

[38]We can think of approximating an S-shaped cumulative distributive function with a straight line. Higher nonlinear functions are poorly approximated with linear curves.

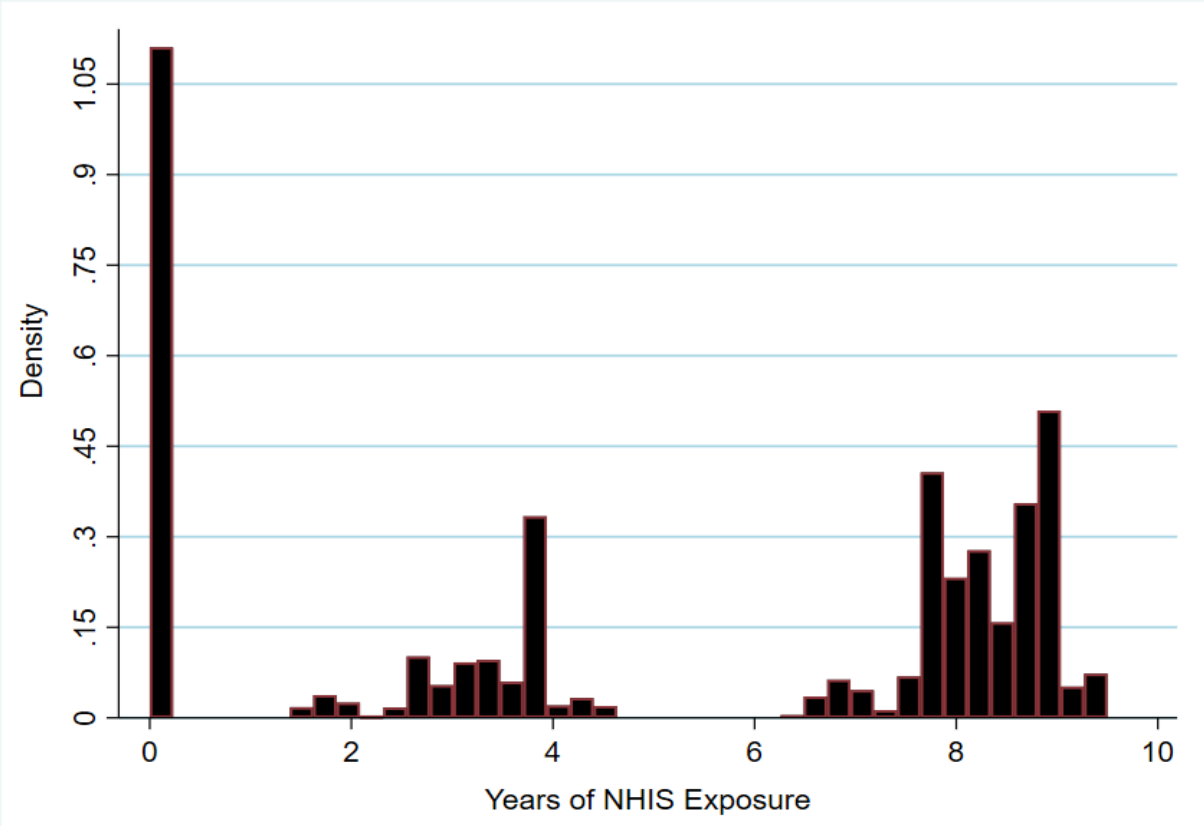

Figure 1.3. Histogram of Years of NHIS Exposure among Women in the Twelve-Months Healthcare Visits Sample

models can lead to marginal effects that are inconsistent and inefficient estimates of the treatment effects (see Appendix A for a comprehensive discussion of this issue). We use a bivariate probit model that is widely suggested in the literature. While it makes a distributional assumption of joint normality of the error terms in equations (1) and (2), allowing the identification of the parameters to come from the functional form restriction and the excluded instrument, it consistently estimates the average marginal effects. Altonji et al. (2005a) propose this strategy in their catholic schooling study. Aside from the benefit of getting consistent and efficient marginal effects, the bivariate model allows us to jointly model the effect of the exogenously staggered rollout of the NHIS on insurance participation and the impact of NHIS on healthcare utilization. This joint estimation approach also allows us to avoid potential biases from any unobserved factors common to the NHIS take-up and healthcare utilization decisions (Abrokwah et al., 2019).

1.5.2 Difference-in-Differences Strategy

As indicated earlier, for outcomes that we observe in all years (i.e., $1999 - 2013$), we are unable to use the IV strategy. For the children born in the off-survey years, we do not have their mothers' insurance participation outcomes in utero or at birth. We lose most of them if we drop those whose mothers' NHIS participation outcomes are not available. Since we do not want to assume that the enrollment status of insurance in the survey year and the time of utilization are the same, we use similar individuals from Nigeria as one control group. Combining the children born in Ghana and Nigeria allows us to use difference-in-differences (DID) methodology to estimate the intent-to-treat effect of the NHIS on births in health facilities and prenatal care visits. Consider the baseline specification below:

$$Y_{idct} = \mathbf{1}(\delta_0 + \delta_1 \mathrm{POST}_{dct} + \delta_2(\mathrm{TREAT}_{dc} \times \mathrm{POST}_{dct}) + \mathbf{\Omega X_{idct}} + \boldsymbol{\zeta_d} + \boldsymbol{\Psi_t} + \xi_{idct} > 0), \quad (1.3)$$

where Y_{idct} represents the outcome of individual i living in district d of country c at year t and POST_{dct} is an indicator for whether district d of country c implemented the NHIS at year t when the outcome was realized. The vector $\mathbf{X_{idct}}$ represents a set of characteristics of individual i living in district d of country c at time t, while ξ_{idct} captures the corresponding unobserved components. The specification also includes a vector of year fixed effect, $\boldsymbol{\Psi_t}$, and district fixed effect, $\boldsymbol{\zeta_d}$, to account for the impacts of time-invariant and district-level characteristics that can cause changes in the outcomes rather than the NHIS. Since only the individuals from districts in Ghana are exposed to the NHIS and no district from Nigeria implemented the NHIS during our study period, we exclude TREAT_{dc} from equation (3) to avoid perfect collinearity.[39]

In equation (3), the parameter of interest is δ_2. Its estimate, $\hat{\delta}_2$, has a causal interpretation of identifying the intent-to-treat effect of the NHIS on the outcomes if our data satisfy two assumptions. First, we need to ensure that the individuals in our data fulfill the assumption of perfect compliance. (All the individuals must have remained in their assigned group throughout the study period.) If some individuals switched groups, our estimate can be susceptible to non-compliance and be biased.[40] An issue equivalent to the switching of treatment assignment is when the control group receives similar treatment from other programs. In this case, it would be challenging to isolate that program's impact from the causal effect of NHIS that we evaluate. One control group that potentially satisfies this assumption is the under five-year-old children from Nigeria. In our study period, Nigeria's government did not implement any significant health insurance program or policy that affected these children.[41] Even though the federal government of Nigeria established a similar NHIS program in 1999 (Monye, 2006), it was unsuccessful,[42] and it relaunched a new health insurance program called "Formal Sector Social Health Insurance Program (FSSHIP)" in 2005 (Onoka et al., 2014).[43]

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[39] TREAT_{dc} is perfectly collinear with the district fixed effect.

[40] If some of the individuals in the control group defected to the treatment group, we would overestimate the effect of the NHIS. We would find an underestimated intent-to-treat effect if some individuals also switched from the treatment to the control group.

[41] Nevertheless, if there was any insurance program, we can construct a random sample such that the women in this sub-sample would not be affected significantly.

[42] Anarado (2001) discusses the various reason why the NHIS in Nigeria was unsuccessful. In summary, the author claimed that the NHIS faced implementation issues such as changes in political regimes, poor designs, etc. For example, the NHIS was designed to be compulsory for only a few of the population working in formal sector organizations with ten or more employees and voluntarily for everyone else. Given that a majority of the labor force worked in the agriculture sector, which was predominantly subsistence, the design of the NHIS automatically led to low participation.

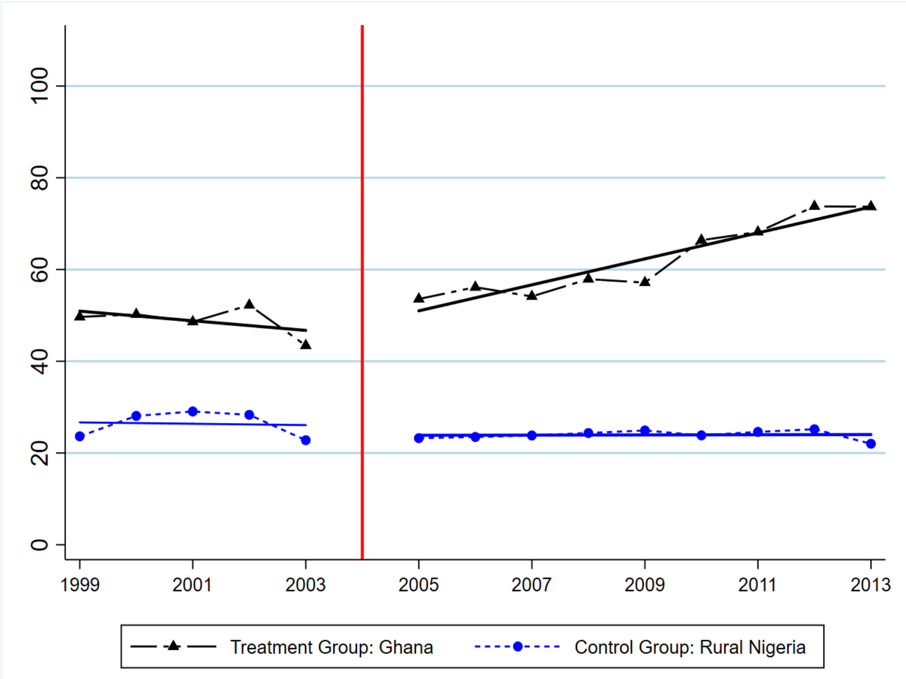
[43] The FSSHIP covers only formal sector employees (public and organized private-sector firms employing ten or more people), the informal sector (urban self-employed and rural community user-groups), and the vulnerable groups.

Despite the implementation of NHIS and FSSHIP in Nigeria, most mothers of the children in our sample have no insurance coverage. The DHS survey reports indicate that about 98% of women had no health insurance coverage in 2008 and 2013. Also, since most of the population employed in the formal sector live in urban areas (Ibiwoye and Adeleke, 2008), we eliminate the women and their children living in urban areas to use only those residing in rural areas as our control group. By removing the children living in Nigeria's urban areas, we minimize the possibility of including children whose mothers had health insurance coverage in the control group. Using the 2008 and 2013 DHS survey reports, we realize that less than 1% of rural Nigeria women had health insurance coverage. Additionally, our data shows that the mothers of only 0.58% of the children in our sample have health insurance coverage.[44]

The second condition for identifying δ_2 in equation (3) is that our data must satisfy the parallel trends assumption. In the absence of the NHIS, any changes in the outcomes that would have occurred in the post-NHIS period would not vary differentially between the treatment and control groups. Figure 1.4 shows the parallel trends of the mean outcomes in the pre- and post-NHIS periods. We show that the pre-trends of the births in health facilities (in Panel A) and prenatal care visits (in Panel B) are identical for the treatment and control groups. After the implementation of the NHIS in 2004, the treatment and control groups' trends for both outcomes diverged. By including district and birth year fixed effects, we hope to account for any unobserved heterogeneity affecting the outcomes of our treatment and control groups differently.

[44] A significant issue that we cannot address with our data is the problem of endogenous migration. If some mothers and their children migrated from urban areas to rural areas after childbirth, we do not observe this information. Additionally, we cannot address women's endogenous migration from rural areas to urban areas but resettle in rural areas after childbirth.

Panel A: Births in Health Facilities (or Institutional Births)



Panel B: Prenatal Care Visits in the First Four Months of Pregnancy

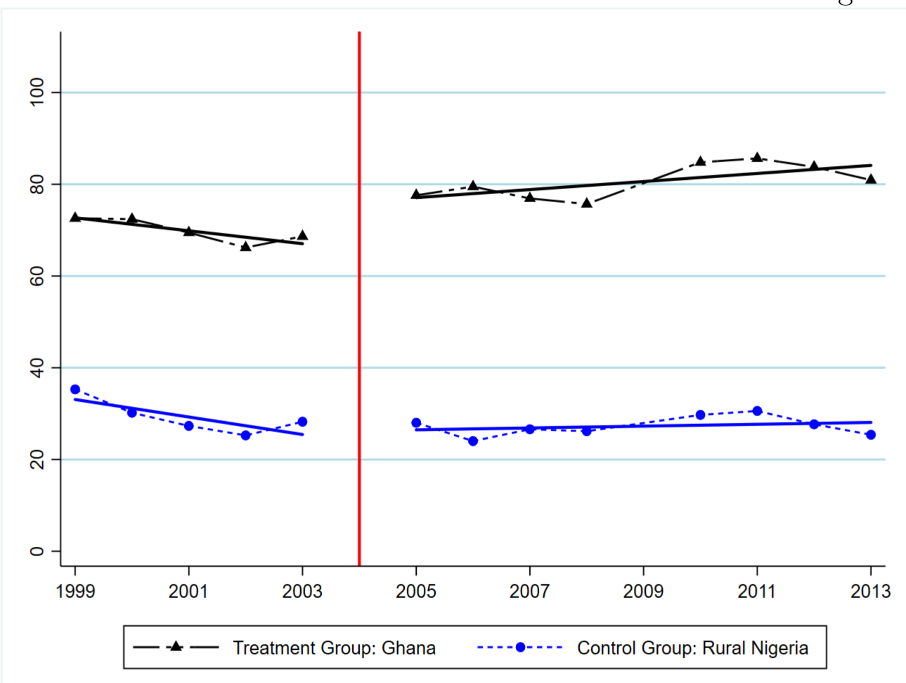


Figure 1.4. Parallel Trends of the Outcomes (%) for Treatment and Control Groups

1.5.3 Event Studies

While we cannot formally test the parallel trend assumption since we do not observe the counterfactual, we evaluate the chances of satisfying it through event study models. The event study models allow us to interact with our treatment variable, the full set of pre-NHIS year fixed effect. Similar to the approach in Pesko (2018) and Courtemanche et al. (2017), we estimate the parameters using data from the pre-NHIS period and omit the last year as the baseline group. We specify the pre-NHIS period event study model as below:

$$Y_{idct} = \lambda_0 + \lambda_k \sum_{k=1999}^{2002} \text{YEAR}_k \times \text{TREAT}_{dc} + \mathbf{\Omega X_{idct}} + \boldsymbol{\Pi_{cd}} + \boldsymbol{\Psi_t} + \xi_{idct}. \tag{1.4}$$

In equation (4), we would be concerned if the estimated coefficients, $\hat{\lambda}_k$, for $k = 1999 - 2002$, are statistically significant and different from zero. However, imprecise estimates or precise zero estimates suggest that there are no observed differences between the treatment and control groups in the pre-NHIS period, conditional on the observable characteristics.

We also take advantage of the event study model to test for differential effects of the NHIS on the outcomes over time. To do this, we use the individuals in the pre-NHIS period (i.e., $1999 - 2003$) as the reference group, and interact with our treatment variable, a full set of post-NHIS year fixed effect. We specify the post-NHIS period event study model as below:

$$Y_{idct} = \lambda_0 + \lambda_k \sum_{k=2004}^{2013} \text{YEAR}_k \times \text{TREAT}_{dc} + \mathbf{\Omega X_{idct}} + \boldsymbol{\Pi_{cd}} + \boldsymbol{\Psi_t} + \xi_{idct}. \tag{1.5}$$

In equation (5), the parameters of interest are λ_k, for $k = 2004 - 2013$. We expect their magnitudes and precision to improve as k rises. Achieving such results will provide suggested evidence that the NHIS has differential effects over time.

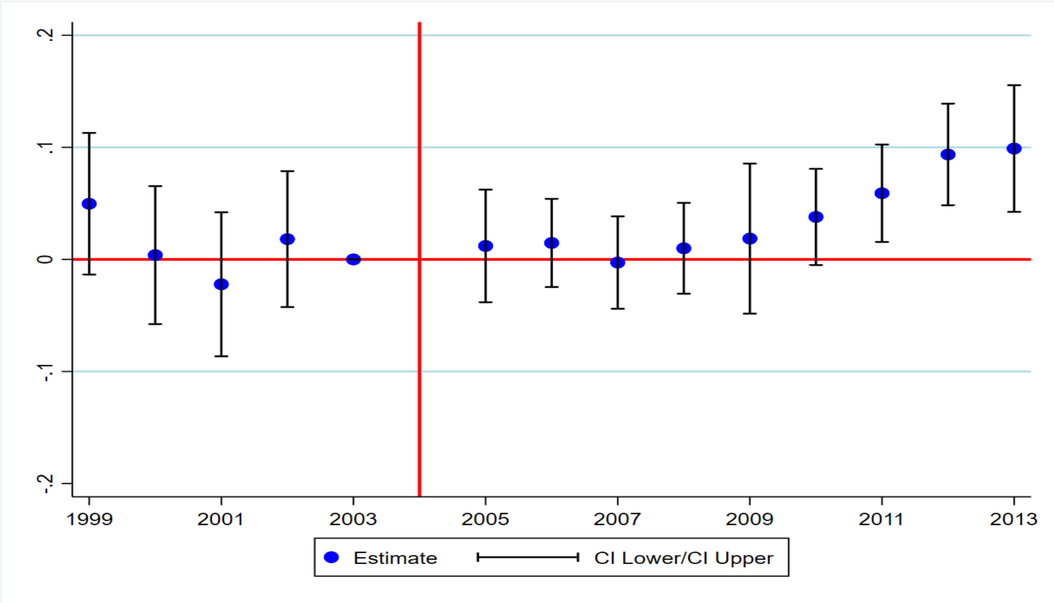
We plot the estimates from the event study models in equations (4) and (5) in Figure 1.5. We show their precision graphically by plotting their 95% confidence intervals. For both outcomes, our findings suggest that there are no economically and statistically significant differences between the outcomes of the treatment and control groups in the pre-NHIS period. All the estimates are statistically insignificant for both outcomes. In the post-NHIS period, we find that the estimates remain statistically insignificant until 2011 for births in health facilities, but irregular for prenatal care visits.

1.6 Results

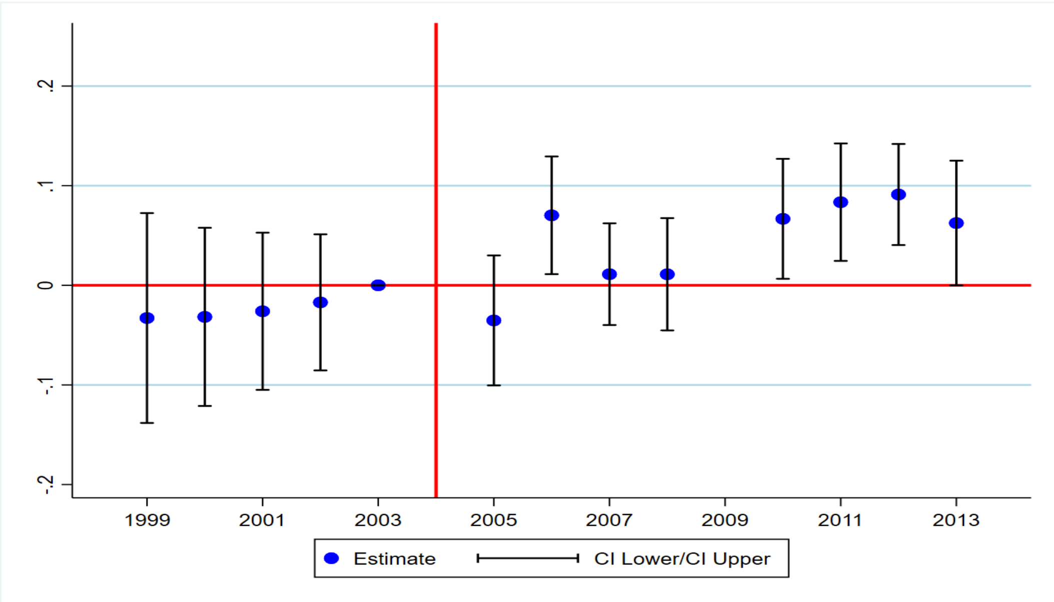
1.6.1 Descriptive Statistics

We first describe the differences between the characteristics of women who gain NHIS coverage and those without any insurance in Ghana. Table 1.1 reports the means and standard deviations of the variables we use in the IV methods. We employ approximately $15,100$ women aged $15 - 49$ to study the causal effect of the NHIS on twelve-month healthcare visits. In this sample, about $3,700$ (24.4%) have NHIS coverage, while the remaining $11,400$ (75.6%) women do not have health insurance coverage. About 11% of the women with NHIS coverage are pregnant compared to 8% of the uninsured women who are also pregnant. Healthcare visits vary tremendously by health insurance coverage. Our sample shows that the women who have NHIS coverage are 19 percentage points more likely to visit healthcare than those without health insurance. The NHIS participants are slightly older and are more likely to be married

Panel A: Births in Health Facilities (or Institutional Births)



Panel B: Prenatal Care Visits in the First Four Months of Pregnancy



Notes: In each figure, two separate linear regression models were used to calculate the estimates. The first regression used only data from the pre-NHIS period (1999 - 2003) to estimate the pre-NHIS coefficients (with 2003, before the NHIS, serving as the reference). The second regression used all data from 1999 to 2013 to estimate the post-NHIS coefficients, using the entire pre-NHIS period (1999 - 2003) as the reference. Results are conditional on the characteristics: sex of child, indicator for twins, birth order, place of resident (rural/urban), household wealth index, mother's age, marital status, education, occupation, literacy status, ethnicity, and religion.

Figure 1.5. Event Study of the Effects of the NHIS on the Outcomes

than non-participants of the NHIS. We find a somewhat small difference (2 percentage points) between the fraction of the uninsured and the insured who live in rural areas, suggesting an approximately equal distribution of NHIS coverage across locations. The fraction of women who experience at least one birth in five years differs between the NHIS participants and the women without any health insurance coverage. However, the number of children born by women with and without NHIS coverage does not differ.

Table 1.1. Means and Standard Deviations (in Parenthesis) of Last Twelve Months Healthcare Visits and Characteristics of Women (ages 15 – 49) with and without NHIS Coverage in Ghana

	Insured	Uninsured	All Women
Outcome and Instrument			
Any 12-Month Healthcare Visits	0.62 (0.49)	0.43 (0.50)	0.48 (0.50)
Years of NHIS Exposure	7.10 (2.28)	4.47 (3.74)	5.11 (3.62)
Characteristics of Women			
Currently Married	0.52 (0.50)	0.45 (0.50)	0.47 (0.50)
Currently Pregnant	0.11 (0.31)	0.08 (0.28)	0.09 (0.28)
Rural Residence	0.50 (0.50)	0.52 (0.50)	0.51 (0.50)
Age of Woman			
15 – 26	0.40 (0.49)	0.46 (0.50)	0.45 (0.50)
27 – 34	0.29 (0.45)	0.25 (0.43)	0.26 (0.44)
35 – 40	0.18 (0.38)	0.15 (0.36)	0.16 (0.37)
41 – 49	0.17 (0.37)	0.17 (0.38)	0.17 (0.38)
Number of Children Born			
No Child	0.40 (0.49)	0.43 (0.50)	0.42 (0.49)
One Child	0.36 (0.48)	0.34 (0.47)	0.34 (0.47)
Two Children	0.19 (0.39)	0.17 (0.38)	0.18 (0.38)
$\geq$ Three Children	0.05 (0.22)	0.06 (0.24)	0.06 (0.23)
Births in the Past 5 Years			
No Birth	0.53 (0.50)	0.61 (0.49)	0.59 (0.49)
One Birth	0.31 (0.47)	0.27 (0.44)	0.28 (0.45)
$\geq$ Two Births	0.16 (0.37)	0.13 (0.33)	0.13 (0.34)
Wealth Index			
1^{st} Quartile (poorest)	0.21 (0.41)	0.23 (0.42)	0.23 (0.42)
2^{nd} Quartile	0.18 (0.39)	0.18 (0.38)	0.18 (0.38)
3^{rd} Quartile	0.20 (0.40)	0.18 (0.39)	0.19 (0.39)
4^{th} Quartile	0.20 (0.40)	0.20 (0.40)	0.20 (0.40)
5^{th} Quartile (richest)	0.20 (0.40)	0.21 (0.41)	0.21 (0.41)

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Table 1.1 – Continued from the previous page

	Insured	Uninsured	All Women
Education			
No Education	0.23 (0.42)	0.27 (0.44)	0.26 (0.44)
$1-9$	0.50 (0.50)	0.50 (0.50)	0.50 (0.50)
$10-12$	0.19 (0.39)	0.18 (0.38)	0.18 (0.38)
≥ 13	0.08 (0.27)	0.06 (0.23)	0.06 (0.24)
Literate	0.53 (0.50)	0.48 (0.50)	0.49 (0.50)
Occupation			
Not Working	0.25 (0.43)	0.25 (0.43)	0.25 (0.43)
Professionals, Tech., Mgt., Clerks	0.08 (0.26)	0.04 (0.21)	0.05 (0.22)
Sales & Services	0.35 (0.48)	0.33 (0.47)	0.34 (0.47)
Agric. Sector & Self-employed	0.21 (0.40)	0.25 (0.43)	0.24 (0.43)
Manual Work	0.12 (0.32)	0.12 (0.33)	0.12 (0.32)
Ethnicity			
Akan	0.41 (0.49)	0.43 (0.50)	0.43 (0.50)
Ga-Dangme, Ewe & Guan	0.21 (0.41)	0.20 (0.40)	0.21 (0.40)
Mole-Dagbani	0.26 (0.44)	0.23 (0.42)	0.23 (0.42)
Hausa	0.10 (0.30)	0.10 (0.30)	0.10 (0.30)
Others	0.02 (0.15)	0.04 (0.20)	0.04 (0.19)
Religion			
Catholic	0.19 (0.39)	0.16 (0.37)	0.17 (0.37)
Christian	0.58 (0.49)	0.60 (0.49)	0.59 (0.49)
Muslim	0.19 (0.39)	0.17 (0.38)	0.18 (0.38)
Traditional	0.02 (0.13)	0.04 (0.19)	0.03 (0.17)
No Religion	0.03 (0.16)	0.04 (0.18)	0.03 (0.18)
Year of Survey			
2003	0.01 (0.11)	0.34 (0.47)	0.26 (0.44)
2008	0.22 (0.41)	0.21 (0.41)	0.21 (0.41)
2014	0.77 (0.42)	0.46 (0.50)	0.53 (0.50)
Observations	3,683	11,429	15,112

The DHS data includes a computed wealth index using household assets. Since there is no measure of income in our data, we substitute it with the wealth index categorized into quartiles. The distribution of wealth index between the NHIS participants and non-participants are generally not different. The variable completed years of schooling, categorized into four groups (i.e., 0, $1-9$, $10-12$, and ≥ 13), shows that approximately 27% of the women who do not have NHIS coverage have zero years of education. On the other hand, about 23% of women with health insurance have no formal education. A slightly higher fraction of the women with NHIS coverage is more likely to attend senior high school or college than those without coverage. The literacy rate (ability to write and read English) differ by NHIS participation status, as about 53% of women with and 48% of those without health insurance are literates.

The distribution of occupation between the women with and those without NHIS coverage differ. The fractions of women who work in the professional environment or sales and service are higher among the NHIS participants than the non-participants. While 25% of the women with NHIS coverage work in the agricultural industry or are self-employed, 21% of the women with insurance coverage work in these areas. We also find a pattern in the distribution of ethnicity and religious affiliations. We observe that more significant fractions of women in some ethnic groups are more likely to participate in the NHIS than women from other tribes in Ghana. Likewise, some religious segments show a strong preference for the NHIS than others. In summary, Table 1.1 shows that age, years of schooling, marital and pregnancy status, past five-year birth history, wealth index, occupation, ethnicity, and religious affiliation differ between the women with and those without NHIS coverage.

In Table 1.2, we also summarize the characteristics of the women stratified by those who do not attend health facilities (in Column 1) and the women who seek healthcare (in Column 2). Our sample shows that about $7,200$ (47.7%) of the women visit healthcare, while the remaining $7,900$ (52.3%) do not seek healthcare in twelve months. Similar to the distributions of characteristics described in Table 1.1, in Table 1.2, we show that they differ between the women who visit and those who do not attend healthcare in twelve months. Therefore, we include all of them in our regressions to account for the observed differences.

Table 1.2. Means and Standard Deviations (in Parenthesis) of the Characteristics of Women (ages 15 − 49) With and Without Healthcare Visits in the Last Twelve Months in Ghana

	With Visits	Without Visits	All Women
Insurance and Instrument			
NHIS Coverage	0.32 (0.47)	0.18 (0.38)	0.24 (0.43)
Years of NHIS Exposure	5.41 (3.53)	4.84 (3.68)	5.11 (3.62)
Characteristics of Women			
Currently Married	0.57 (0.49)	0.37 (0.48)	0.47 (0.50)
Currently Pregnant	0.13 (0.34)	0.05 (0.22)	0.09 (0.28)
Rural Residence	0.48 (0.50)	0.54 (0.50)	0.51 (0.50)
Age of Woman			
15 − 26	0.37 (0.48)	0.52 (0.50)	0.45 (0.50)
27 − 34	0.32 (0.47)	0.20 (0.40)	0.26 (0.44)
35 − 40	0.19 (0.39)	0.14 (0.34)	0.16 (0.37)
41 − 49	0.16 (0.37)	0.18 (0.38)	0.17 (0.38)
Number of Children Born			
No Child	0.35 (0.48)	0.49 (0.50)	0.42 (0.49)
One Child	0.37 (0.48)	0.31 (0.46)	0.34 (0.47)
Two Children	0.21 (0.41)	0.14 (0.35)	0.18 (0.38)
$\geq$ Three Children	0.06 (0.25)	0.05 (0.21)	0.06 (0.23)
Births in the Past 5 Years			
No Birth	0.44 (0.50)	0.72 (0.45)	0.59 (0.49)
One Birth	0.36 (0.48)	0.20 (0.40)	0.28 (0.50)
$\geq$ Two Births	0.19 (0.40)	0.08 (0.27)	0.13 (0.34)
Wealth Index			
1^{st} Quartile (poorest)	0.22 (0.41)	0.24 (0.43)	0.23 (0.42)
2^{nd} Quartile	0.17 (0.37)	0.19 (0.39)	0.18 (0.38)
3^{rd} Quartile	0.18 (0.39)	0.19 (0.40)	0.19 (0.39)
4^{th} Quartile	0.20 (0.40)	0.19 (0.39)	0.20 (0.40)
5^{th} Quartile (richest)	0.23 (0.42)	0.19 (0.39)	0.21 (0.41)
Years of Education of Woman			
No Education	0.26 (0.44)	0.25 (0.44)	0.26 (0.44)
1 − 9	0.47 (0.50)	0.53 (0.50)	0.50 (0.50)
10 − 12	0.19 (0.39)	0.17 (0.38)	0.18 (0.38)
$\geq$ 13+	0.08 (0.27)	0.05 (0.21)	0.06 (0.24)
Literate	0.48 (0.50)	0.50 (0.50)	0.49 (0.50)
Occupation of Woman			
Not Working	0.19 (0.39)	0.30 (0.46)	0.25 (0.43)
Professionals, Tech., Mgt., Clerks	0.07 (0.25)	0.04 (0.19)	0.05 (0.22)

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Table 1.2 – Continued from the previous page

	With Visits	Without Visits	All Women
Sales & Services	0.37 (0.48)	0.31 (0.46)	0.34 (0.47)
Agric. Sector & Self-employed	0.24 (0.42)	0.25 (0.43)	0.24 (0.43)
Manual Work	0.13 (0.34)	0.11 (0.31)	0.12 (0.32)
Ethnicity			
Akan	0.41 (0.49)	0.44 (0.50)	0.43 (0.50)
Ga-Dangme, Ewe & Guan	0.21 (0.41)	0.20 (0.40)	0.21 (0.40)
Mole-Dagbani	0.25 (0.43)	0.22 (0.41)	0.23 (0.42)
Hausa	0.10 (0.30)	0.09 (0.29)	0.10 (0.30)
Others	0.04 (0.18)	0.04 (0.20)	0.04 (0.19)
Religion			
Catholic	0.17 (0.38)	0.17 (0.37)	0.17 (0.37)
Christian	0.59 (0.49)	0.60 (0.49)	0.59 (0.49)
Muslim	0.19 (0.39)	0.16 (0.37)	0.18 (0.38)
Traditional	0.03 (0.17)	0.03 (0.18)	0.03 (0.17)
No Religion	0.03 (0.17)	0.04 (0.19)	0.03 (0.18)
Survey Year			
2003	0.23 (0.42)	0.29 (0.45)	0.26 (0.44)
2008	0.20 (0.40)	0.22 (0.41)	0.21 (0.41)
2014	0.57 (0.50)	0.50 (0.50)	0.53 (0.50)
Observations	7,213	7,899	15,112

From Table 1.1 and 1.2, we are yet to describe two variables of interest in detail: the NHIS participation and years of NHIS exposure variables. At the time of the interview, about 24.4% of the women in the sample had verified valid NHIS cards.[45] We observe differences in NHIS coverage between the women who visit healthcare and those who do not. The women who attend health facilities for care are 14% more likely to be NHIS participants than those who do not visit any healthcare in twelve months. The years of NHIS exposure also differ by both NHIS coverage status and healthcare

[45] By defining the NHIS participants to include only women with valid NHIS cards, we replicate the statistics in Figure 1.1, constructed from the National Health Insurance Authority's administrative data. For example, Figure 1.1 shows that about 39% of the Ghanaian population participated in the NHIS in 2014. We take several steps to verify the validity of the statistics presented in Figure 1.1. For example, the NHIA reported that only 10.5 million of the Ghanaian population were active subscribers of the NHIS in 2014, and 69% were from the premium exempt category. Since the number of people in Ghana was 27.22 million in 2014, it suggests that 38.6% of the Ghanaian population had NHIS coverage. We find from our data that about 35.2% of the women had NHIS coverage in 2014. It suggests that our data is representative of the Ghanaian population. Other studies of the NHIS, including Abrokwah et al. (2019), report that more than 60% of women in Ghana had NHIS coverage in 2014.

utilization behavior. The average span of NHIS exposure in the full sample is approximately five years and one month. However, the women who enroll in the NHIS have about seven years and one month of NHIS exposure, compared to four years and six months among the women who do not have health insurance coverage (see Table 1.1). Similarly, the women who visit health facilities for care have five years and five months of NHIS exposure, compared to those who do not attend any health facilities for care with four years and ten months (see Table 1.2).

For the samples used in the DID framework, Table 1.3 reports the pre- and post-NHIS treatment means and standard deviations (in parenthesis) of institutional births and prenatal care visits outcomes stratified by the treatment and control groups. Because the surveys gather information on the places of births of all under five-year-old eligible children but ask questions on a history of mother's prenatal care visits for the last child, the sample sizes differ by outcomes. For the institutional birth outcome, we obtain information on about $9,000$ and $37,900$ under five-year-old children from Ghana and rural Nigeria (see Panel A of Table 1.3). Similarly, we have details on $6,200$ and $27,200$ under five-year-old children from Ghana and rural Nigeria for the outcome of prenatal care visits (see Panel B of Table 1.3).

The first panel of Table 1.3 shows that, on average, approximately 48% and 25% of under five-year-old children in Ghana and rural Nigeria were born in health facilities in the pre-NHIS period. The statistic in Ghana increased to 66% but decreased in rural Nigeria to 24% in the post-NHIS period. In the full sample, about 33% and 30% of children were born in health facilities in the pre- and post-NHIS periods, respectively. We observe that the fraction of institutional births in the pre-NHIS period is more significant than that of the post-NHIS period in the full sample. The reason is that the

Table 1.3. Means and Standard Deviations (in parenthesis) of Births in Health Facilities and Prenatal Care Visits by Pre– & Post–Period for the Treatment, Control & the Full-Sample

Panel A: Births in Health Facilities

	Treatment Group		
	Pre-NHIS	Post-NHIS	Total
Ghana	0.48 (0.50)	0.66 (0.48)	0.58 (0.49)
Observations	3,852	5,104	8,956

	Control Group		
	Pre-NHIS	Post-NHIS	Total
Rural Nigeria	0.25 (0.43)	0.24 (0.43)	0.24 (0.43)
Observations	7,179	30,722	37,901

	Full Sample		
	Pre-NHIS	Post-NHIS	Total
All Children	0.33 (0.47)	0.30 (0.46)	0.31 (0.46)
Observations	11,031	35,826	46,857

Panel B: Any Prenatal Care Visits (Pregnancy $\leq$ 4 Months)

	Treatment Group		
	Pre-NHIS	Post-NHIS	Total
Ghana	0.70 (0.46)	0.81 (0.39)	0.76 (0.43)
Observations	2,601	3,590	6,191

	Control Group		
	Pre-NHIS	Post-NHIS	Total
Rural Nigeria	0.28 (0.45)	0.27 (0.45)	0.27 (0.45)
Observations	3,286	23,924	27,210

	Full Sample		
	Pre-NHIS	Post-NHIS	Total
All Children	0.46 (0.50)	0.34 (0.48)	0.36 (0.48)
Observations	5,887	27,514	33,401

relative share of the control group increased in the post-NHIS period drastically. In the pre-NHIS period, individuals from rural Nigeria account for approximately 65% of the sample, while in the post-NHIS period, they constitute 86%. Because the outcome improved only in the treatment group and worsened in the control group, we expect the control group's impact to drive the average outcome in the post-NHIS period. Overall, about 31% of the children in the full sample were born in health facilities.

Likewise, Panel B of Table 1.3 reports that, on average, the mothers of 70% and 28% of the children in Ghana and rural Nigeria received prenatal care services within the first four months of pregnancies in the pre-NHIS period. In the post-NHIS period, these statistics increased to 81% for the treatment group and decreased to 27% for the control group. In the full sample, mothers of 36% of under five-year-old children had prenatal checkups in the pre-NHIS period. But in the post-NHIS period, the statistic reduced to 34% due to the control group's dominance. On average, prenatal care checkups did not change in rural Nigeria. Approximately the mothers of 36% of the children in the full sample attended prenatal care checkups in the first four months in utero.

Table 1.4. Means and Standard Deviations (in parenthesis) of Household and Parents' Characteristics by Treatment Group, Control Group & the Full-Sample − Births in Health Facilities

	Treatment Group	Control Group	All Children
Post-NHIS Period	0.57 (0.50)	0.81 (0.39)	0.77 (0.42)
Treatment (Ghana)			0.19 (0.39)
Treatment × Post	0.57 (0.50)		0.11 (0.31)
Child's Characteristics			
Male	0.51 (0.50)	0.51 (0.50)	0.51 (0.50)
Twin	0.02 (0.13)	0.02 (0.13)	0.02 (0.13)
Birth Order			
First	0.23 (0.42)	0.18 (0.38)	0.19 (0.39)
Second	0.20 (0.40)	0.16 (0.37)	0.17 (0.37)

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Table 1.4 – Continued from the previous page

	Treatment Group	Control Group	All Children
Third	0.16 (0.37)	0.15 (0.35)	0.15 (0.36)
Fourth	0.14 (0.34)	0.13 (0.33)	0.13 (0.33)
$\geq$ Fifth	0.28 (0.45)	0.39 (0.49)	0.37 (0.48)
Household Characteristics			
Rural Residence	0.65 (0.48)		0.93 (0.25)
Wealth Index			
1^{st} Quartile (poorest)	0.33 (0.47)	0.32 (0.47)	0.32 (0.47)
2^{nd} Quartile	0.22 (0.42)	0.30 (0.46)	0.29 (0.45)
3^{rd} Quartile	0.17 (0.38)	0.21 (0.41)	0.20 (0.40)
4^{th} Quartile	0.15 (0.36)	0.12 (0.32)	0.13 (0.33)
5^{th} Quartile (richest)	0.13 (0.33)	0.04 (0.20)	0.06 (0.235)
Mother Currently Married	0.74 (0.44)	0.93 (0.25)	0.90 (0.31)
Mother's Age			
15 – 26	0.32 (0.47)	0.43 (0.49)	0.41 (0.49)
27 – 34	0.42 (0.49)	0.37 (0.48)	0.38 (0.49)
35 – 40	0.21 (0.41)	0.19 (0.39)	0.19 (0.39)
41 – 49	0.09 (0.29)	0.06 (0.24)	0.07 (0.25)
Mother's Education			
No Education	0.41 (0.49)	0.57 (0.50)	0.54 (0.50)
1 – 9	0.47 (0.50)	0.29 (0.46)	0.33 (0.47)
10 – 12	0.10 (0.30)	0.11 (0.32)	0.11 (0.31)
$\geq$ 13	0.03 (0.17)	0.02 (0.14)	0.02 (0.15)
Literate	0.28 (0.45)	0.29 (0.45)	0.29 (0.45)
Mother's Occupation			
Not Working	0.13 (0.34)	0.32 (0.47)	0.29 (0.45)
Manual Work	0.12 (0.33)	0.10 (0.30)	0.11 (0.31)
Professionals, Tech., Mgt., Clerks	0.03 (0.17)	0.02 (0.12)	0.02 (0.14)
Sales & Services	0.32 (0.47)	0.35 (0.48)	0.35 (0.48)
Agric. Sector & Self-employed	0.40 (0.49)	0.21 (0.40)	0.24 (0.43)
Ethnicity			
Hausa	0.13 (0.34)	0.32 (0.47)	0.28 (0.45)
Ghana: Akan	0.38 (0.49)		0.07 (0.26)
Ghana: Ga-Dangme, Ewe & Guan	0.18 (0.39)		0.04 (0.18)
Ghana: Mole-Dagbani	0.26 (0.44)		0.05 (0.22)
Ghana: Others	0.05 (0.21)		0.01 (0.09)
Nigeria: Fulani		0.11 (0.31)	0.09 (0.28)
Nigeria: Igbo		0.07 (0.25)	0.05 (0.22)
Nigeria: Yoruba		0.06 (0.24)	0.05 (0.21)
Nigeria: Others 1		0.14 (0.35)	0.11 (0.32)
Nigeria: Others 2		0.33 (0.47)	0.27 (0.44)
Religion			
No Religion	0.06 (0.24)		0.01 (0.11)
Catholic	0.15 (0.35)	0.08 (0.26)	0.09 (0.29)

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Table 1.4 – Continued from the previous page

	Treatment Group	Control Group	All Children
Christian	0.53 (0.50)	0.28 (0.45)	0.33 (0.47)
Muslim	0.20 (0.40)	0.61 (0.49)	0.53 (0.50)
Traditional	0.06 (0.24)	0.02 (0.13)	0.03 (0.16)
Survey Year			
2003	0.35 (0.48)	0.09 (0.29)	0.14 (0.35)
2008	0.26 (0.44)	0.48 (0.50)	0.44 (0.50)
2013		0.43 (0.50)	0.35 (0.48)
2014	0.39 (0.49)		0.08 (0.26)
Observations	8,956	37,901	46,857

The statistics in Table 1.3 suggest that the simple DID estimates without independent variables are 0.17 and 0.12 for births in health facilities and prenatal care visits, respectively. However, other factors could affect the outcomes, which the simple DID estimates would not capture. We summarize some of these characteristics in Tables 1.4 and 1.5. In general, the distribution of the observable characteristics of under five-year-old children in the control and treatment groups differ significantly for both outcomes. For example, the mothers of about 32% of the children in the control group have no occupation when we consider births in health facilities outcome (see Table 1.4). In the treatment group, the mothers of only 13% of the children are unemployed. Also, mothers of 57% of children in the control group have no formal education. On the other hand, the mothers of about 41% of the children from Ghana reported having no years of schooling. Our DID estimates will account for these differences by including the observable characteristics in Tables 1.4 and 1.5. However, after controlling these differences, other unobserved characteristics could have caused the differences in the treatment and control groups' utilization outcomes. By including the year of birth and district fixed effects in the models, we hope to account for the unobserved heterogeneity to obtain unbiased and precise estimates.

Table 1.5. Means and Standard Deviations (in parenthesis) of Household and Parents' Characteristics by Treatment Group, Control Group & the Full-Sample − Prenatal Care Visits

	Treatment Group	Control Group	All Children
Post-NHIS Period	0.58 (0.49)	0.88 (0.33)	0.82 (0.38)
Treatment (Ghana)			0.19 (0.39)
Treatment × Post	0.58 (0.49)		0.11 (0.31)
Child's Characteristics			
Male	0.51 (0.50)	0.51 (0.50)	0.51 (0.50)
Twin	0.02 (0.13)	0.02 (0.13)	0.02 (0.13)
Birth Order			
First	0.22 (0.41)	0.17 (0.37)	0.18 (0.38)
Second	0.19 (0.40)	0.15 (0.36)	0.16 (0.37)
Third	0.16 (0.37)	0.14 (0.35)	0.14 (0.35)
Fourth	0.14 (0.35)	0.13 (0.33)	0.13 (0.34)
$\geq$ Fifth	0.29 (0.45)	0.41 (0.49)	0.39 (0.49)
Mother and Household Characteristics			
Rural Residence	0.63 (0.48)		0.93 (0.25)
Wealth Index			
1^{st} Quartile (poorest)	0.30 (0.46)	0.32 (0.47)	0.32 (0.47)
2^{nd} Quartile	0.22 (0.41)	0.30 (0.46)	0.28 (0.45)
3^{rd} Quartile	0.18 (0.39)	0.21 (0.41)	0.21 (0.41)
4^{th} Quartile	0.17 (0.37)	0.12 (0.33)	0.13 (0.34)
5^{th} Quartile (Richest)	0.14 (0.34)	0.05 (0.21)	0.06 (0.24)
Mother Currently Married	0.73 (0.45)	0.92 (0.27)	0.89 (0.32)
Mother's Age			
15 − 26	0.33 (0.47)	0.42 (0.49)	0.40 (0.49)
27 − 34	0.40 (0.49)	0.35 (0.48)	0.36 (0.48)
35 − 40	0.21 (0.41)	0.19 (0.40)	0.20 (0.40)
41 − 49	0.11 (0.31)	0.08 (0.26)	0.08 (0.27)
Mother's Education			
No Education	0.39 (0.49)	0.56 (0.50)	0.53 (0.50)
1 − 9	0.47 (0.50)	0.29 (0.46)	0.33 (0.47)
10 − 12	0.11 (0.31)	0.12 (0.33)	0.12 (0.32)
$\geq$ 13	0.03 (0.18)	0.02 (0.15)	0.03 (0.16)
Literate	0.29 (0.46)	0.30 (0.46)	0.30 (0.46)
Mother's Occupation			
Not Working	0.13 (0.33)	0.32 (0.47)	0.28 (0.45)
Manual Work	0.13 (0.33)	0.10 (0.30)	0.11 (0.31)
Professionals, Tech., Mgt., Clerks	0.03 (0.17)	0.02 (0.13)	0.02 (0.14)
Sales & Services	0.34 (0.47)	0.35 (0.48)	0.35 (0.48)
Agric. Sector & Self-employed	0.38 (0.49)	0.20 (0.40)	0.24 (0.42)
Ethnicity			
Hausa	0.12 (0.33)	0.31 (0.46)	0.28 (0.45)

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Table 1.5 – Continued from the previous page

	Treatment Group	Control Group	All Children
Ghana: Akan	0.39 (0.49)		0.07 (0.26)
Ghana: Ga-Dangme, Ewe & Guan	0.19 (0.39)		0.04 (0.18)
Ghana: Mole-Dagbani	0.25 (0.44)		0.05 (0.21)
Ghana: Others	0.05 (0.21)		0.01 (0.093)
Nigeria: Fulani		0.11 (0.31)	0.09 (0.28)
Nigeria: Igbo		0.07 (0.25)	0.06 (0.23)
Nigeria: Yoruba		0.06 (0.24)	0.05 (0.22)
Nigeria: Others 1		0.14 (0.34)	0.11 (0.31)
Nigeria: Others 2		0.34 (0.47)	0.27 (0.45)
Religion			
No Religion	0.05 (0.23)		0.01 (0.10)
Catholic	0.15 (0.36)	0.08 (0.27)	0.09 (0.29)
Christian	0.54 (0.50)	0.29 (0.45)	0.33 (0.47)
Muslim	0.20 (0.40)	0.59 (0.49)	0.52 (0.50)
Traditional	0.06 (0.23)	0.02 (0.13)	0.02 (0.15)
Survey Year			
2003	0.37 (0.48)	0.08 (0.27)	0.13 (0.34)
2008	0.28 (0.45)	0.45 (0.50)	0.42 (0.49)
2013		0.47 (0.50)	0.38 (0.49)
2014	0.36 (0.48)		0.07 (0.25)
Observations	6,191	27,210	33,401

1.6.2 First Stage Estimates for the Effects of the NHIS on Twelve-Month Healthcare Visits

The statistics in Table 1.2 suggest that the unconditional correlation between NHIS participation and the years of exposure is positive. On average, women who have more years of exposure are more likely to enroll in the NHIS. We formally test this correlation and present the results in Table 1.6.[46] We report results from linear probability models and include district fixed effects in all specifications, allowing us to remove the impacts of district-level time-invariant characteristics on NHIS participation. The spec-

[46]Note that the first stage estimates show NHIS exposure's effects on the NHIS participation conditional on the women's observable characteristics.

ification in the first column excludes all control variables and time components in the model. However, because the distributions of the uninsured and insured women's characteristics differ significantly, as demonstrated in Table 1.1, we include the controls in the specifications in Columns (2) – (4). In the last two columns, we either add survey year fixed effects or post-NHIS dummy variable to account for the impacts of economic growth on the outcome.

Table 1.6. First-Stage Estimates: Effects of Years of NHIS Exposure on NHIS Participation among Women (ages 15 – 49) in Ghana using a Linear Probability Model

	(1)	(2)	(3)	(4)
Years of NHIS Exposure	0.036***	0.036***	-0.001	0.016***
	(0.003)	(0.003)	(0.028)	(0.003)
Controls	N	Y	Y	Y
Post-NHIS Indicator	N	N	N	Y
Survey Fixed-Effects	N	N	Y	N
F-Statistic	198	199	0	30
Observations	15,112	15,112	15,112	15,112

Notes: We include the woman's age, place of resident (rural/urban), marital status of woman, pregnancy status, number of births in the last five years, birth history, wealth index, woman's education, woman's occupation, literacy status of woman, ethnicity, religion, and district fixed effects as the controls in each specification. We report heteroscedastic robust-standard errors clustered within the district in the parentheses. *p<.1, **p<.05, ***p<.01

Our first stage estimates with or without controls, but excluding time variables, show a strong correlation between NHIS participation and years of exposure. In both cases, we find that a ceteris paribus increase in NHIS exposure by one year is associated with a 3.6 percentage points increase in insurance enrollments and is statistically significant at 1%. Their corresponding F-statistics are not less than 198. By including individual and household characteristics, our IV estimate does not change in magnitude and precision. It suggests that the individual and household characteristics are uncorrelated with the years of NHIS exposure in the first stage models. (We test this hypothesis later). By adding both district and survey year fixed effects to the model, we obtain a weak IV estimate [see Column (3)]. We explain that since the time frame for the DHS survey is typically three months, the years of NHIS exposure does not vary among women surveyed in the same year within a district. In fact, the instrument is perfectly collinear with the fixed effects of the survey years with an adjusted R-squared of 0.976.

To overcome the challenge of perfect collinearity, while controlling for time component, we replace the survey fixed effects with a post-NHIS dummy variable and report our estimate in Column (4) of Table 1.6. Our new first stage estimate still shows a strong correlation between the years of NHIS exposure and health insurance coverage, conditional on the women's observed characteristics, district fixed effects, and post-NHIS dummy variable. Notice that adding the post-NHIS dummy variable to the model reduces the size of our IV estimate by more than 50%, suggesting that the time variable is essential in the first stage equation [see Column (2) and (4)]. We find that a ceteris paribus additional year of NHIS exposure is associated with a 1.6 percentage points increase in NHIS participation and is statistically significant at 1%. The corresponding F-statistic is 30. Since the NHIS enrollment rate in the sample is 24.4%, the estimate corresponds to a 6.7% increase in NHIS take-up for every additional year of exposure.

Before closing this subsection, we return to the exclusion restriction assumption to rule out some possibilities of violating it. If districts that adopted the NHIS early decided based on need (i.e., pre-determined outcomes) or observed and unobserved characteristics, our estimates would not be causal. The excluded instrument would be endogenous if the rollouts were not random or conditionally random. Although we cannot formally test the exogeneity of our excluded instrument, we do some diagnostic checks to rule out some of the concerns of endogeneity. We regress the timing of rollouts (i.e., number of months before districts adopted the NHIS) on the pre-treatment outcome and observable characteristics and report the results in Table 1.7. In the first column, we use only the women surveyed in the pre-NHIS period (i.e., 2003). We also use all the women in the sample and include survey year fixed effects and report the estimates in the second column. Our results from linear regressions of the number of months before districts adopted the NHIS on the care-seeking outcome and the observable characteristics rule out our concerns that rollouts were pre-determined based on need or observable characteristics. Only a few of the variables are statistically significant in both specifications. Since we include these observable characteristics in the models, we conclude that NHIS adoptions across districts were conditionally random.

Table 1.7. Determinants of District NHIS Adoption. Dependent Variable - Number of Months

	Linear Regression using Women from Pre-NHIS Period (1)	Linear Regression using All Women or Full-Sample (2)
Last 12-Month Healthcare Visits	-0.537 (0.457)	0.233 (0.254)
Age of Woman		
27 – 34	0.234 (0.293)	-0.005 (0.187)
35 – 40	0.211 (0.422)	0.132 (0.225)
41 – 49	0.030 (0.341)	0.129 (0.214)
Currently Married	0.196 (0.359)	0.142 (0.237)
Currently Pregnant	-0.423 (0.342)	0.083 (0.224)
Births in the Last 5 Years		

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Table 1.7 – Continued from the previous page

	Women from Pre-NHIS	Full-Sample Women
One Birth	0.334 (0.296)	0.155 (0.184)
$\geq$ 2 Births	-0.070 (0.435)	-0.440 (0.268)
Number of Children Born		
One Child	-0.146 (0.310)	-0.064 (0.174)
Two Children	0.258 (0.614)	0.184 (0.332)
$\geq$ Three Children	0.023 (0.733)	-0.396 (0.546)
Wealth Index		
2^{nd} Quartile	1.020 (0.695)	0.910 (0.775)
3^{rd} Quartile	2.278** (0.979)	0.099 (1.018)
4^{th} Quartile	0.214 (1.096)	-1.867 (1.163)
5^{th} Quartile	-2.313* (1.361)	-4.000*** (1.361)
Education		
$1-9$	0.676 (0.417)	0.306 (0.344)
$10-12$	1.122** (0.541)	0.482 (0.378)
≥ 13	1.462* (0.760)	0.816* (0.452)
Literate	-0.546 (0.336)	0.088 (0.264)
Employment		
Professionals, Tech., Mgt., Clerks	-0.958* (0.566)	-0.195 (0.338)
Sales & Services	-0.239 (0.299)	-0.365 (0.304)
Agric. Sector & Self-employed	-0.150 (0.656)	-0.496 (0.414)
Manual Work	-0.436 (0.582)	-0.918*** (0.338)
Ethnicity		
Akan	-3.422 (2.461)	-2.531 (1.718)
Ga-Dangme, Ewe & Guan	-2.093 (2.493)	-0.869 (1.779)
Mole-Dagbani	-3.025 (2.774)	-0.630 (2.180)
Hausa	-4.775** (2.385)	-1.161 (1.874)
Religion		
Catholic	-0.926 (0.698)	-1.019 (0.732)
Christian	-0.601 (0.693)	-0.225 (0.541)
Muslim	0.545 (1.199)	0.128 (1.389)
Traditional	-0.713 (0.981)	-0.988 (0.881)
Rural Residence	1.674 (1.154)	1.415 (0.943)
Survey Year Fixed-Effect	N	Y
Observations	3,890	15,112

1.6.3 Second Stage Estimates for the Effects of the NHIS on Twelve-Month Healthcare Visits

Table 1.8 presents our results from the naive ordinary least squares (OLS), IV, and reduced-form models. For the naive OLS, we show estimates for two specifications. The specification in Column (1) includes survey fixed effects, while in Column (2), we replace the survey fixed effects with the post-NHIS dummy variable. Since survey year fixed effects are perfectly collinear with the instrument, we report our second-best results from the specification that includes post-NHIS dummy variable as our second stage IV and reduced-form estimates. The naive OLS estimates suggest that NHIS participation is associated with about 13.2 percentage points increase in the likelihood of visiting healthcare in twelve months, conditional on the women's observable characteristics, time variable, and district fixed effects, and is statistically significant at 1%. In Column (3) and (4), we report the IV estimates.

Our 2SLS estimate is statistically significant when we exclude time fixed effects, but larger in magnitude and precision compared to the naive OLS estimates. Since time variables are essential in our models, we include the post-NHIS dummy variable in the next specification. Including the post-NHIS dummy variable makes the estimate inefficient. At best, we find that the women induced by the instrument to obtain NHIS coverage are 30 percentage points more likely to visit healthcare, conditional on their observable characteristics, post-NHIS dummy variable, and district fixed effects but is statistically insignificant. Given that the average healthcare visit is 47.7%, our estimate in Column (4) represents a 63% increase. Our second stage estimates from the linear IV estimators are inconsistent and inefficient because the outcome and the endoge-

Table 1.8. Naive OLS, Second Stage and Reduced Form Estimates: Effects of NHIS on Twelve Months Healthcare Visits using Years of NHIS Exposure as Instrumental Variable

	Naive OLS		2SLS		Reduced-Form
	(1)	(2)	(3)	(4)	(5)
NHIS Coverage	0.131***	0.133***	0.246***	0.300	
	(0.012)	(0.012)	(0.079)	(0.226)	
Yrs. of NHIS Exposure					0.005
					(0.004)
Controls	Y	Y	Y	Y	Y
Post-NHIS Indicator	N	Y	N	Y	Y
Survey Fixed-Effects	Y	N	N	N	N
Observations	15,112	15,112	15,112	15,112	15,112

Notes: We include the woman's age, place of resident (rural/urban), marital status of woman, pregnancy status, number of births in the last five years, birth history, wealth index, woman's education, woman's occupation, literacy status of woman, ethnicity, religion, and district fixed effects as the controls in each specification. In Column (3) and (4), we use the years of NHIS exposure as an instrumental variable for the NHIS participation. We report heteroscedastic robust-standard errors clustered within the district in the parentheses. *p<.1, **p<.05, ***p<.01

nous variables are binary, as demonstrated in Section 1.11. The reduced-form estimate shows that a ceteris paribus additional year of NHIS exposure increases a twelve-month healthcare utilization by about 0.5 percentage points and is statistically insignificant. At the mean of 47.7% of the outcome, it suggests that a ceteris paribus one additional year of NHIS exposure increases healthcare utilization by 1%.

The 2SLS estimates are almost twice as large as the OLS estimates and are also noisy after including a time fixed effect. This finding is surprising because the nature of endogeneity reasonably points to an upward bias in the OLS. There are possible reasons for the large 2SLS estimates. One reason is that the instrument can be endogenous. However, since we have already demonstrated that early NHIS rollout is uncorrelated with the district-level pre-treatment characteristics and outcomes, we rule out this possibility. Another reason is that the 2SLS estimates which, are the LATE, are larger for the compliers. The final reason is that, as discussed in Section 1.4, the linear IV models can be approximating the marginal effects of the nonlinear models poorly. Altonji et al. (2005a) documents similar findings for the 2SLS in their binary response models with binary endogenous treatment when evaluating instrument strategies for estimating the effects of Catholic schooling. Consequently, we provide results from the bivariate probit estimator below to investigate this concern.

Table 1.9. Marginal Effect Estimates of the Impact of NHIS on Twelve Months Healthcare Visits using Bivariate Probit Models and Years of NHIS Exposure as an Excluded Instrument

	(1)	(2)	(3)	(4)
NHIS Coverage	0.266***	0.320***	0.320***	0.315***
	(0.070)	(0.106)	(0.096)	(0.112)
Controls	Y/Y	Y/Y	Y/Y	Y/Y
Post-NHIS Indicator	N/N	N/N	Y/Y	Y/N
Survey Fixed-Effects	N/N	Y/Y	N/N	N/Y
Observations	15,112	15,112	15,112	15,112

Notes: We include the woman's age, place of resident (rural/urban), marital status of woman, pregnancy status, number of births in the last five years, birth history, wealth index, woman's education, woman's occupation, literacy status of woman, ethnicity, religion, and district fixed effects as the controls in each specification. We report heteroscedastic robust-standard errors clustered within the district in parentheses. The coefficient of the excluded instrument is statistically significant at 1%, except the specification in Column (2). The notation "N/N" denotes that both the first and second equations of the bivariate model exclude the variable X. *p<.1, **p<.05, ***p<.01

Table 1.9 presents our main results on the twelve-month healthcare visits from the bivariate probit models. We provide estimates for several specifications due to the flexibility of this estimator in its arguments. In Column (1), we exclude the women's observable characteristics and time fixed effects from the first and second stage equations.[47] We include all the control variables in the first and second stage equations in specifications (2) − (4) but vary both post-NHIS dummy variable and survey year fixed effects in the first stage and outcome equations. In Column (2), the specification includes survey year fixed effects in both equations. The specification in Column (3) substitutes the survey year fixed effects with the post-NHIS dummy variable in both equations. For the specification in the last column, we include the post-NHIS dummy only in the first stage equation and survey fixed effects in the outcome equation. The bivariate model allows for this flexibility since some variables in the first equation can be excluded from the second equation and vice versa.

The bivariate probit estimates are economically and statistically significant and larger than the naive OLS results but similar to the linear IV estimates, regardless of the specification. The estimate for the specification in Column (1) is biased downwards. By adding time variables, we obtain consistently larger coefficients but less efficient, similar to the linear IV estimate. Focusing on our preferred specification in Column (4), we find that a ceteris paribus NHIS coverage increases twelve-month healthcare utilization by approximately 31.5 percentage points and is statistically significant at 1%. Since the estimates from the linear and nonlinear IV estimates are similar, it demonstrates that we only lose precision and not consistency in the linear models. Since, on average, 47.7% of women visit health facilities for care in twelve months, the estimate corresponds to a 66% increase. Compared to the naive OLS estimate, our IV results suggest that addressing the endogeneity in the NHIS increases the size of the effect by more than twice (i.e., from 13.1 to 31.5 percentage points). It indicates that endogeneity is a more significant concern in the NHIS, and not handling it well can bias the estimates.

[47] The first stage equation estimates the effect of the years of NHIS exposure on NHIS participation. The second stage equation estimates the impact of the NHIS on the outcome conditional on the predicted variables from the first stage equation that are included in the second stage equation [See equations (1) and (2)].

1.6.4 Heterogeneity

We also examine how the effect of the NHIS on any twelve-month healthcare visits is distributed along the dimensions of socioeconomic characteristics and the household location of residence. Estimating the causal impact of the NHIS on healthcare utilization along these dimensions is interesting because we expect the effect to vary by the place of residence (rural or urban), household wealth, and woman's education. Importantly, we are not sure of the demographic group within which the result will be more substantial. Since most of the NHIS participants are from low-income households and are more likely to be subsidized, we expect the impact to vary by household wealth and women's education. Also, because rural areas in Ghana are less likely to have healthcare centers, we expect the women in such places to be less likely to visit healthcare, holding other things constant.[48] At the same time, we expect women from rural communities to utilize the NHIS more than those in urban areas since they are more likely to have poorer health conditions, ceteris paribus. Table 1.10 presents the NHIS treatment effects across various sub-samples with twelve-month healthcare visits as the dependent variable. We only show the results from the preferred specification.[49] The first two columns show the analysis by location, followed by household wealth in the third and fourth columns, and women's education status in the last two columns.

From our sample, NHIS coverage does not vary much by location, but healthcare utilization differs on average.[50] We suspect that the take-up of the NHIS does not vary by location because the women from rural areas are more likely to be subsidized due to their low socio-economic status and are catching up with the urban women. The first two columns of Table 1.10 show that our estimate from the urban communities is smaller and less precise than the rural areas' result. We find that the NHIS increases twelve-month healthcare visits by about 15.7 percentage points among women from

[48] The monetary and time cost of traveling to healthcare centers may be too high due to poor road network.

[49] The specification that includes post-NHIS dummy variable in the first stage equation and survey year fixed effects in the outcome equation.

[50] Our sample shows 23.7% and 25.0% of the women from the rural and urban communities had NHIS coverage. We also observe from the sample that 44.9% and 50.7% of the women from rural and urban areas visited healthcare.

Table 1.10. Heterogeneity in the effect of the NHIS on Twelve Months Healthcare Visits using Bivariate Probit Models and Years of NHIS Exposure as an Excluded Instrument

	Location of Residence		Wealth of Household		Education of Woman	
	Rural	Urban	Poorest 60th Percentile	Richest 40th Percentile	Years of Schooling ≤ 6	Years of Schooling > 6
	(1)	(2)	(3)	(4)	(5)	(6)
NHIS Coverage	0.440***	0.157	0.421***	0.250**	0.436***	0.256*
	(0.037)	(0.159)	(0.049)	(0.114)	(0.051)	(0.148)
Controls	Y/Y	Y/Y	Y/Y	Y/Y	Y/Y	Y/Y
Post-NHIS Indicator	Y/N	Y/N	Y/N	Y/N	Y/N	Y/N
Survey Fixed-Effects	N/Y	N/Y	N/Y	N/Y	N/Y	N/Y
Observations	7,720	7,392	8,975	6,137	6,874	8,238

Notes: We include the woman's age, marital status of woman, pregnancy status, number of births in the last five years, birth history, wealth index, woman's education, woman's occupation, literacy status of woman, ethnicity, religion, and district fixed effects as the controls in each specification. We report heteroscedastic robust-standard errors clustered within the district in parentheses. The notation "Y/N" for a variable X denotes that the first equation of the bivariate probit model includes variable X, whiles the second equation excludes it.
*p<.1, **p<.05, ***p<.01

urban areas and is statistically insignificant. On the other hand, the estimate is more substantial and statistically significant when we consider rural women. Conditional on the control variables, NHIS coverage increases twelve-month healthcare visits by approximately 44 percentage points among the rural women and is statistically significant at 1%. At the mean of 44.9% of healthcare visits among women in the rural communities, the estimate corresponds to approximately 98% increase.

In Column (3) and (4), we divide the sample into two sub-samples based on low- and high-income household women. The women in households with wealth from the 1^{st} to 60^{th} percentile are in Column (3), while the remaining women constitute the group in column (4). On average, 45.7% and 50.7% of the women in the poor and rich households, respectively, visit healthcare, but NHIS enrollment does not vary between the two groups. Our results show that the NHIS has a more substantial effect on the healthcare-seeking behavior among women in the poorest 60^{th} percentile than the women in the upper 40^{th} percentile. We find a difference of approximately 17 percentage points and is statistically significant at 1%. Given that the average healthcare utilization among the women in poor households is 45.7%, our estimates suggest that the NHIS increases healthcare utilization among poor women by 42.8% more than the rich women.

As our final analysis to prove the heterogeneous effect of the health insurance on healthcare utilization, we show the causal impact of the NHIS on women with low (≤ 6) and high (> 6) years of schooling. A quarter of the women in our sample have no formal education, and 45.5% of them have up to six years of education. While NHIS enrollment among women with more years of education is about 3.8% higher than women with a few years of training, the effects of the NHIS on twelve-month healthcare visits differ substantially between the two groups. The last two columns of Table 1.10 show the results from the education sub-sample analysis. We find that the NHIS has a substantial effect on the women with lower years of schooling than those with higher education by approximately 18 percentage points and is statistically significant at 1%. At the mean utilization level of 47% among the low educated women, the difference corresponds to a 38.3% increase.

1.6.5 NHIS' Effects on Institutional Births and Prenatal Care

We report our DID results in Table 1.11 and 1.12 for the births in health facilities and prenatal care outcomes, respectively. We provide estimates for five different specifications by varying the type of controls that we include in the models. While some specifications exclude individual and household characteristics, others alternate a linear time trend, birth year fixed effects, and post-NHIS dummy variable. In Panel A, we present our results from the linear probability model (LPM). We also report the marginal effects of the probit models in Panel B. In all the specifications, we find that the estimates from the linear models are different from the marginal effects of the probit models in magnitude and precision for the outcome of the institutional birth. In the case of prenatal care visits, we do not find any differences in the estimates between the two estimators.

The magnitude of our estimates in Panel B of Table 1.11 shrinks when we include observable characteristics and a time variable. It decreases from 8.5 to 3.4 percentage points, depending on the control variables that we include. Focusing on our preferred specification in Column (4), we find that exposure to the NHIS increases the likelihood of delivering in health facilities by 5.7 percentage points and is statistically significant at 1%. Given that the baseline mean of births in health facilities is 30.6% (see Panel A of Table 1.3), the estimate corresponds to an 18.8% increase.

In the case of our results from prenatal care visits, reported in Table 1.12, our estimates are smaller when we exclude the observable characteristics. By including the controls and time component, they become larger in magnitudes, but with stable precision. The estimate from our preferred specification in Column (4) suggests that the NHIS in Ghana increases prenatal care visits by 6.6 percentage points and is statistically significant at 1%. Since 36.4% of the mothers attended prenatal care visits in the first four months of pregnancies (see Panel B of Table 1.3), the estimate corresponds to a 18.2% increase.

1.6.6 Effects of Free Maternal Healthcare on Maternal Healthcare Use

As already indicated, some pregnant women in our sample were eligible for free NHIS coverage after introducing the free maternal healthcare policy in July 2008. However, each mother needed to send a proof of pregnancy from an obstetrician to the district NHIS office to obtain a free NHIS card for free maternal services. We suspect that not all eligible pregnant women gained NHIS coverage because of the initial monetary costs of getting the proof from an obstetrician, the cost of traveling to healthcare centers, and the queues at the centers. We would think of our DID estimates as the effects of free maternal healthcare policy on prenatal care visits and institutional births if the law was passed at the initial rollout period. Since the new policy was later added, we re-estimate our DID models to include only the children born after July 2008 in the post-NHIS period and report our results in Table 1.13 and 1.14 for births in health facilities and prenatal care visits, respectively. We find a ceteris paribus exposure to the law increases institutional births and prenatal care visits by 8.7 and 10.5 percentage points, corresponding to 26.2% and 26% increase at the mean of 0.332 and 0.403, respectively.

Table 1.11. Difference-in-Differences Estimates: Effects of the NHIS in Ghana on Children Born in Health Facilities Outcome using Children from Rural Nigeria as a Control Group

	(1)	(2)	(3)	(4)	(5)
	Panel A: Coefficients from Linear Probability Models				
Treatment × Post	0.125***	0.114***	0.091***	0.088***	0.067***
	(0.033)	(0.033)	(0.022)	(0.022)	(0.022)
	Panel B: Marginal Effects from Probit Models				
Treatment × Post	0.085***	0.075***	0.061***	0.057***	0.034*
	(0.028)	(0.029)	(0.018)	(0.018)	(0.017)
Controls	N	N	Y	Y	Y
Post-NHIS Dummy	Y	N	Y	N	Y
Birth Year Fixed Effect	N	Y	N	Y	N
Linear Time Trend	N	N	N	N	Y
Observations	46,857	46,857	46,857	46,857	46,857

Notes: The specifications in Column (3) - (5) include mother, child, and household characteristics that may affect the outcome. They are mother's age at the time of childbirth categorized into four groups, ethnicity, place of resident (rural/urban), religious beliefs, marital status, education and, literacy status, occupation in the survey year, the gender of the child, birth order, and household wealth index. We cluster standard errors at the district and Local Government Agency to account for the overtime correlation in unobserved factors that affect the outcome. The DHS cluster is the same the enumeration area similar to census block (in the U.S.A. context).

Table 1.12. Difference-in-Differences Estimates: Effects of the NHIS in Ghana on Prenatal Care Visits using Children from Rural Nigeria as a Control Group

	(1)	(2)	(3)	(4)	(5)
	Panel A: Coefficients from Linear Probability Models				
Treatment × Post	0.072***	0.062**	0.074***	0.066***	0.067***
	(0.025)	(0.025)	(0.022)	(0.023)	(0.022)
	Panel B: Marginal Effects from Probit Models				
Treatment × Post	0.073***	0.062**	0.074***	0.066***	0.065***
	(0.026)	(0.025)	(0.023)	(0.023)	(0.022)
Controls	N	N	Y	Y	Y
Post-NHIS Dummy	Y	N	Y	N	Y
Birth Year Fixed Effect	N	Y	N	Y	N
Linear Time Trend	N	N	N	N	Y
Observations	33,401	33,401	33,401	33,401	33,401

Notes: The specifications in Column (3) - (5) include mother, child, and household characteristics that may affect the outcome. They are mother's age at the time of childbirth categorized into four groups, ethnicity, place of resident (rural/urban), religious beliefs, marital status, education and, literacy status, occupation in the survey year, the gender of the child, birth order, and household wealth index. We cluster standard errors at the district and Local Government Agency to account for the overtime correlation in unobserved factors that affect the outcome. The DHS cluster is the same the enumeration area similar to census block (in the U.S.A. context).

Table 1.13. Difference-in-Differences Estimates: Effects of Free Maternal Healthcare Policy in Ghana on Children Born in Health Facilities Outcome using Children from Rural Nigeria as a Control Group

	(1)	(2)	(3)	(4)	(5)
	Panel A: Coefficients from Linear Probability Models				
Treatment × Post	0.112***	0.129***	0.108***	0.118***	0.106***
	(0.039)	(0.038)	(0.027)	(0.027)	(0.026)
	Panel B: Marginal Effects from Probit Models				
Treatment × Post	0.072**	0.090**	0.076***	0.087***	0.074***
	(0.036)	(0.035)	(0.024)	(0.024)	(0.022)
Controls	N	N	Y	Y	Y
Post-NHIS Dummy	Y	N	Y	N	Y
Birth Year Fixed Effect	N	Y	N	Y	N
Linear Time Trend	N	N	N	N	Y
Observations	29,518	29,518	29,518	29,518	29,518

Notes: The mean of the outcome is 0.332. The specifications in Column (3) - (5) include mother, child, and household characteristics that may affect the outcome. They are mother's age at the time of childbirth categorized into four groups, ethnicity, place of resident (rural/urban), religious beliefs, marital status, education and, literacy status, occupation in the survey year, the gender of the child, birth order, and household wealth index. We cluster standard errors at the district and Local Government Agency to account for the overtime correlation in unobserved factors that affect the outcome. The DHS cluster is the same the enumeration area similar to census block (in the U.S.A. context).

Table 1.14. Difference-in-Differences Estimates: Effects of Free Maternal Healthcare Policy in Ghana on Any Prenatal Care Visits using Children from Rural Nigeria as a Control Group

	(1)	(2)	(3)	(4)	(5)
	Panel A: Coefficients from Linear Probability Models				
Treatment × Post	0.085***	0.085***	0.095***	0.096***	0.101***
	(0.031)	(0.031)	(0.027)	(0.027)	(0.026)
	Panel B: Marginal Effects from Probit Models				
Treatment × Post	0.093***	0.093***	0.104***	0.105***	0.108***
	(0.031)	(0.031)	(0.027)	(0.027)	(0.026)
Controls	N	N	Y	Y	Y
Post-NHIS Dummy	Y	N	Y	N	Y
Birth Year Fixed Effect	N	Y	N	Y	N
Linear Time Trend	N	N	N	N	Y
Observations	19,900	19,900	19,900	19,900	19,900

Notes: The mean of the outcome is 0.403. The specifications in Column (3) - (5) include mother, child, and household characteristics that may affect the outcome. They are mother's age at the time of childbirth categorized into four groups, ethnicity, place of resident (rural/urban), religious beliefs, marital status, education and, literacy status, occupation in the survey year, the gender of the child, birth order, and household wealth index. We cluster standard errors at the district and Local Government Agency to account for the overtime correlation in unobserved factors that affect the outcome. The DHS cluster is the same the enumeration area similar to census block (in the U.S.A. context).

The DID estimates in Table 1.13 and 1.14 would be the potential effects of free maternal healthcare policy on prenatal care visits and institutional births if every pregnant woman obtained NHIS coverage after July 2008. Given that the 2008 DHS survey interviewed the respondents in August − November 2008, all the pregnant women in our sample in the post-NHIS period were eligible for free maternal healthcare. But our data summarized in Table 1.1 shows that only 45% of the pregnant women obtained NHIS coverage, compared to 31.4% of the non-pregnant women with coverage in the post-NHIS period.[51] To show suggestive evidence of the free maternal healthcare policy's potential causal effect, we weight the DID estimates in Table 1.13 and 1.14 by the inverse probability that a pregnant woman gained NHIS coverage after the free maternal care policy. After adjusting the results, we find that they increase to 19.3 and 23.3 percentage points for birth in health facilities and prenatal care visits, respectively. Our interpretation is that if all pregnant women gain NHIS coverage, institutional births and prenatal care visits will increase as much, ceteris paribus. They are the full potential of the causal effect of the free maternal healthcare policy.

1.6.7 Robustness Checks

One threat to identifying the parameters in IV models is whether growth in the healthcare sector induces participation in the NHIS. Hospital openings after the rollout of the NHIS could lead to more enrollments in health insurance due to improvement in healthcare access. Although we consider these supply-side factors as mechanisms, we provide results to show that they do not fully influence healthcare utilization after the implementation of the NHIS. We include the per $1,000$ people regional-level number of hospitals, hospital beds, doctors, and nurses to capture the supply-side effects on healthcare utilization. Table 1.A1 in Appendix B reports the results from our re-estimated bivariate probit models when we include the supply-side factors as additional

[51] Although we do not report these statistics in the tables, we can calculate them indirectly. NHIS coverage differed by year and pregnancy status. About 25% and 34% of the non-pregnant women had NHIS coverage in 2008 and 2014, respectively. On the other hand, approximately 34% and 52% of the pregnant women had NHIS coverage in 2008 and 2014, respectively.

controls. We find that our results do not substantially differ from the main estimates reported in Table 1.9 when we add the supply-side factors separately or jointly.

We also show how our results change with additional controls. We include other observable characteristics that we believe in influencing the NHIS take-up decisions. Dummies for frequent radio listeners, television viewers, long-distance to healthcare centers, and the education and occupation of the women's partners at the time of the survey are the set of additional controls that we also include in the models. For the IV estimates, our results reported in Table 1.A2 in Appendix C are robust to these supplementary control variables compared to the main results shown in Table 1.9. Similarly, for the DID estimates, our results, reported in Table 1.B1 in Appendix D and 1.B2 in Appendix E, are not different from the main results shown in Tables 1.11 and 1.12.

The main concern in identifying the parameters in the DID models is whether the children from rural Nigeria are similar to the Ghanaian children in observable and unobservable characteristics. Since Nigeria implemented the NHIS in 1999 and introduced the FSSHIP in 2005, some individuals in the control group could have treatment. Our next robustness check shows the results when we use the full sample of children from Nigeria, including those from the urban areas. Figure 1.B1 in Appendix F and 1.B2 in Appendix G show the parallel trends and the events study graphs. Even though the treatment and control groups' pre-trends are parallel, the post-NHIS period slope of births in health facilities outcome for the control group increased drastically, contrary to what we observe when we use rural Nigeria as our control group. In the case of prenatal care visits, we do not see any significant change in the slope and is reasonably equivalent to the case of using rural Nigeria as a control group. Our estimates reported in Table 1.B3 in Appendix H and 1.B4 in Appendix I are consistently higher than those shown in Tables 1.11 and 1.12. They suggest that we would overestimate the causal effect of the NHIS if we used the full sample of children from Nigeria as the control group.

1.7 Discussion and Conclusion

Unlike developed economies, evidence on the causal effects of health insurance programs on healthcare utilization from developing countries, where several factors affect take-up decisions, is limited. Ghana is one of the first African countries to implement a public health insurance program successfully. Before the NHIS, there was limited access to the few insurance programs that existed in a few areas, and almost all the population had no insurance coverage. The districts, which are the third-level administrative divisions in Ghana, staggered the rollout of the NHIS from $2004 - 2007$ and set the price of the NHIS coverage; most of the districts chose the minimum premium enforced by the government of Ghana. Individuals voluntarily enrolled anytime once the district of residence adopted the NHIS, but with a three-month waiting period. Some other essential features of the NHIS include universal eligibility, national coverage with a short rollout period, subsidies for some eligible groups, and free insurance for pregnant women. Additionally, the government of Ghana imposes a 2.5% tax on goods and services, funding approximately 70% of the cost of the NHIS annually. Despite the fact that the districts sell the NHIS below its actuarially fair price, over 60% of the Ghanaian population still have no health insurance coverage.

Because the design of the NHIS introduces endogeneity and other issues, it is challenging to identify its causal impact. Consequently, evidence on the causal effects of the NHIS on a variety of outcomes is sparse. This paper estimates the causal impact of the NHIS on healthcare utilization among women aged $15 - 49$ using empirical strategies that overcome prior methodological issues. We organize data from Ghana and Nigeria Demographic and Health Surveys and administrative data on district rollout dates. Our outcomes of interest are twelve-month healthcare visits, births in health facilities (or institutional births), and prenatal care visits. We use two different empirical strategies to model the outcomes appropriately, depending on the availability of data. In both approaches, we exploit districts' staggered rollouts as an exogenous source of variation to identify the causal effect of the NHIS on the outcomes. For the twelve-month healthcare visits, which we observe only in the years that surveys occurred, we jointly model enrollment in the NHIS and healthcare utilization decisions using the years of NHIS exposure as an excluded instrument in an IV framework.[52] After constructing data for years 1999 − 2013 using dates of births of under five-year-old children, we apply a DID estimation strategy to identify the impact of the NHIS on prenatal care visits and institutional births using rural Nigeria as a control group.

Our results provide strong evidence that NHIS substantially increases healthcare utilization among women of childbearing ages. In all cases of healthcare utilization outcomes, we obtain statistically and economically significant effects. We find that NHIS participation increases twelve-month healthcare visits among women by about 32 percentage points (i.e., 66% relative to the baseline mean of approximately 47.7%). Our results also show that the NHIS increases the likelihood of mothers delivering in health facilities by 5.7 percentage points (18.8%) and prenatal care visits by 6.6 percentage points (18.2%). From the IV analysis of the NHIS's impact on twelve-month healthcare visits, we stratify our analysis by household wealth, residence, and education. In all cases, we find a more substantial effect of the NHIS on the outcome among the disadvantaged women (i.e., poor, rural, and uneducated women). We also provide an event study analysis to explore the effect of the timing of district staggered rollout on the outcomes. We find evidence of significant impacts of the NHIS only after three years of national coverage. Also, we show from the event study models that the NHIS in Ghana possibly has differential effects with an increasing impact over time. Finally, we characterize the potential effects of the free maternal care (i.e., a law passed in July 2008 to give pregnant women free NHIS). We find higher estimates for births in health facilities and prenatal care visits and show that they would double if all the eligible women enrolled in the NHIS.

We discuss a few implications of our results. First, they imply that the NHIS re-

[52]We construct the years of NHIS exposure in the district of residence using the dates of NHIS rollouts in districts and the survey dates.

duces home deliveries, which was a significant concern before implementing the program. Second, the NHIS induces women to utilize prenatal care services that have several effects on maternal and child health. Therefore, we believe that the NHIS helps Ghana achieve the Millennium Development Goals of improving maternal health and decreasing infant mortalities. Our third finding that the NHIS increases twelve-month healthcare utilization among women is evidence of increasing healthcare visits. This is important because Ghana as tropical country, where malaria and cholera are common, women must visit healthcare regularly. Our results also imply a declining social gradient in health since the NHIS has more significant effects on poor, rural, and low-educated women's outcomes.[53] Finally, our findings of NHIS increasing utilization motivates us to consider other outcomes in future studies. In one study, we intend to use DHS data from seven African countries in a synthetic control methodology to estimate the impact of the NHIS on infant mortality. In another study, we intend to evaluate the effects of the NHIS on the child's later-in-life outcomes, including cognitive skills and school-related consequences.

This study has several strengths and provides the most credible causal estimate of the effect of the NHIS on healthcare utilization using the best available data sets and empirical strategies to overcome methodological challenges in prior studies. First, we recognize the endogeneity caused by the nature of the design of the NHIS. We use districts' staggered rollout as an exogenous source of variation to address the endogeneity concerns in the NHIS participation. The staggered rollout is a common natural experiment often used to tackle endogeneity issues (Fink et al., 2013; Gruber and Hanratty, 1995; Cesur et al., 2017; Ater et al., 2017). We argue that the timing of NHIS rollouts across districts is uncorrelated with pre-existing healthcare utilization behaviors and the individual observed characteristics after ruling out some of the concerns about the excluded instrument being endogenous.

Second, our methodologies utilize all the women and under five-year-old children

[53] The social gradient in health indicates that the poorest of the poor have the worst health.

in our data sets, including those with incomplete information on NHIS enrollments. Studies either limit their samples to individuals with full details to use fewer observations or assume that the NHIS enrollment status of mothers observed in the year of the survey is representative of their enrollment status at the time of utilizing healthcare. Given that our study period overlapped the phase-in stage of the NHIS when rollouts were still increasing over time, we think that such a strong assumption can lead to an overclassification of NHIS enrollments. Because the NHIS participation variable takes a zero or one value, it makes measurement errors from misclassification non-classical.[54] Such errors negatively correlate with the actual underlying values of NHIS participation.[55] The severity of the non-classical measurement error is that since the bias can assume any sign (i.e., positive or negative), its effect can extend beyond just an attenuated coefficient to assume a wrong sign (Nguimkeu et al., 2019; Wossen et al., 2019). Additionally, non-classical measurement errors cannot be easily overcome with standard errors-in-variables methods. Therefore, regardless of the source of misclassification, causal identification in the presence of a non-classical measurement error is nontrivial. Failure to account for such issues will lead to biased estimates with misleading policy implications.

Third, our data sets allow us to overcome the concerns of measurement errors and endogenous misreporting. The interviewers used several approaches to verify respondents' answers to the survey questions. We define our health insurance participation variable to include only the women with verified NHIS cards. By this definition, we mitigate the concerns of measurement errors in the NHIS participation variable. Also, since we define our IV from administrative data on the dates of certificates of commencements of the NHIS across districts, we mitigate any measurement error concerns on our excluded instrument.

The final and probably the most subtle strength of our study is that we use es-

[54] If the measurement error is classical, it will not be a significant concern since we can sign the bias.

[55] If NHIS participation is one, measurement error can only be negative, and if the NHIS participation is zero, it can only be positive. So, the measurement error is negatively correlated with the actual values of NHIS participation.

timation techniques that consistently identify the causal impact of our binary endogenous NHIS participation variable on the binary outcomes. The majority of the causal studies on the NHIS use estimation techniques that literature has shown to inconsistently estimate the effect of a binary endogenous variable on binary outcomes of interest. We use a bivariate probit estimation technique that produces consistent and efficient estimates, unlike the linear models that sometimes give poor approximations of marginal effects of highly nonlinear models (Altonji et al., 2005a,b; Lewbel et al., 2012; Wooldridge, 2010, pp. 596-597).

Three caveats to our study are as follows. First, given that we observe one of the outcomes in our data only in 2003, 2008, and 2014, there could be many changes in the aggregate economy that the post-NHIS dummy variables cannot capture. It would be useful if data are available in all years to account for the trends in the growth of the Ghanaian economy and the health sector. Second, one challenge in our estimation procedure is the data limitation of not obtaining variations in the NHIS participation before 2003. Before implementing the NHIS in 2003, there was almost no health insurance. Everyone in our sample from the post-NHIS period, 2008 and 2014, was eligible for insurance. We overcome this challenge by relying on 1% women with private insurance from the pre-NHIS period for identification. It allowed us to implement the IV strategy that relies on the assumption of independence and exclusion restriction. We would focus on the reduced form strategy that makes a minimal assumption of independence and not exclusion restriction if data was available. Therefore, our IV estimate of 32 percentage points needs to be interpreted with caution due to these challenges. Third, a threat to the validity of the DID estimates is that our control group, rural Nigeria, could be different from the treatment group, Ghana, in terms of unobservable characteristics that could affect the outcomes differently. For example, we do not have information on household income. Since Nigeria has benefited from crude oil for many years, and Ghana only discovered oil in 2007, the individuals in Nigeria may be wealthier than the women from Ghana. We provide Figure 1.6 to compare the GDP per capita for Ghana and Nigeria. In the years that we draw our samples (1999 – 2013), the GDP per capita gap between Ghana and Nigeria widened, with Nigeria leading in all years. However, we assume that the women from rural Nigeria, used as the control group, are as poor as the Ghanaian women, making them comparable.

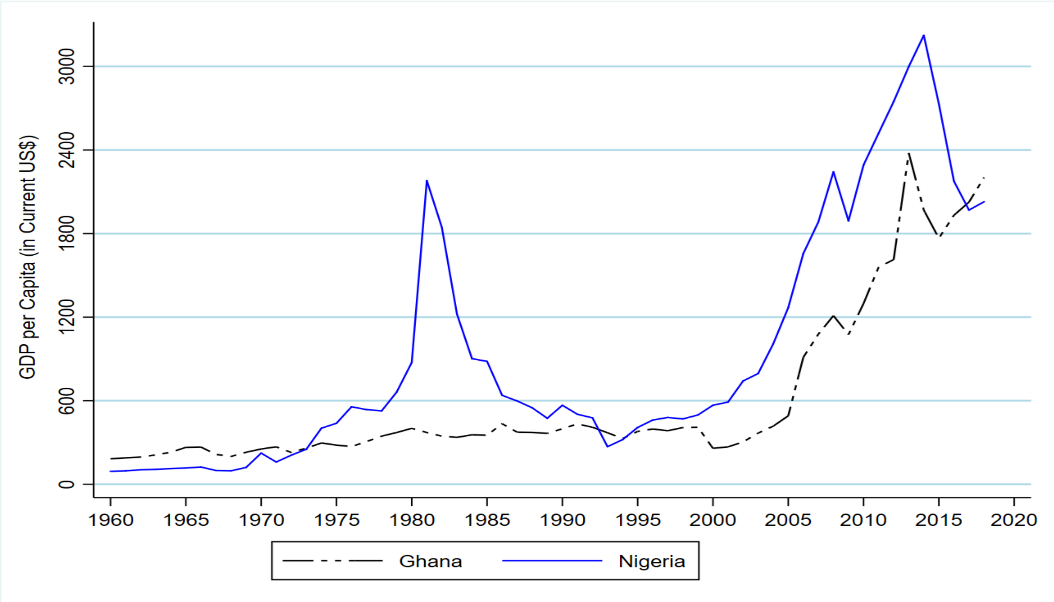


Figure 1.6. GDP per Capita (in Current US$) for Ghana and Nigeria, 1960 – 2018

To close, we provide three policy recommendations. First, from our results, we believe that the policymakers of the NHIS can influence the healthcare utilization of women by increasing coverage. Even with the low NHIS coverage (i.e., only 24.4% of the women in our sample), we find a substantial causal impact of the insurance on our outcomes of interest. It suggests that implementing a mandatory health insurance policy that increases NHIS coverage will induce more healthcare utilization. However, we also recommend that the supply-side factors should be available to meet the increasing demand to prevent overburden on the existing resources. Since the mandate cannot be effective without supporting low-income families, we recommend that the policymakers subsidize more women with low socioeconomic status who cannot afford the NHIS. Our finding of heterogeneous impacts of the NHIS in favor of the poor, low-educated, and rural women suggests that these women are benefiting more, reducing health inequality. Finally, we recommend that the policymakers implement a complementary policy that can ensure a 100% take-up of the free maternal healthcare policy. Our results suggest that if all pregnant women gain coverage, the NHIS's impact on deliveries in health facilities and prenatal care visits will double.

Chapter 2

The Long-term Effects of Charter School Exposure on Education and Health Behaviors

2.1 Introduction

Over the years, policymakers have devoted efforts to increase the quantity and improve the quality of human capital investments.[1] Increasing the quality of human capital investment can induce further investment among individuals with sub-optimal levels (Becker, 1967; Card et al., 2012; Aaronson and Mazumder, 2011). After Becker (1964) established that investment in human capital improves health, raises earning, and increases the person's knowledge about own lifetime, with more evidence from Schultz (1961, 1967) that investment in human capital positively affects aggregate income and economic growth, several studies have focused on it. In this paper, we present new evidence on how efforts to improve the quality of K-12 education in the U.S. influences human capital development and other outcomes.

A recent state-level policy intended to improve the quality of human capital investments in the U.S. was the state charter laws, which allow public schools to operate independently with less supervision from state and local school authorities. Minnesota was the first state to pass the law in December 1991 and opened its first charter school in 1992. Many states passed the laws subsequently and opened charter schools, and as of 2020, 44 states and the District of Columbia had charter schools. Currently, about 7500 charter schools exist, serving 3.3 million students, about 5% of the population of

[1] Policies such as state compulsory school attendance laws (CSLs), which started in Massachusetts in the late 19th century, were implemented to increase human capital investment. Other policies such as classroom size regulations, school lunch, anti-bullying regulations, etc., seek to improve the rate at which human capital investments convert to the desired outcomes. Improving the efficiency of the investment in human capital is analogous to increasing the rate at which every dollar invested in human capital translates into the desired outcome.

public schools.[2]

Since its inception in 1991, several studies have examined the impacts of charter schools on students' outcomes. However, these studies have primarily focused on immediate and short-term outcomes such as students' performance and school competition (Sass et al., 2016; Booker et al., 2011; Angrist et al., 2016; Ni, 2009), with a few considering medium-term impacts, including health behaviors and earnings.[3] Also, each study focused on a few jurisdictions, starting from one city to a maximum of sixteen states.[4] Therefore, a study that uses national data to assess the charter schools' long-term impacts on students' later-in-life outcomes is important. With many states opening charter schools by 2003, it is worth focusing on their impacts on students' later-in-life outcomes.

This paper estimates the long-term effects of exposure to charter schools on later-in-life education outcomes and health behaviors. Specifically, we answer the following questions. (1) What are the long-term impacts of charter school exposure on educational outcomes such as completed years of schooling and college completion? (2) What are the long-term effects of charter school exposure on health behaviors, including cigarette smoking and excessive alcohol consumption? We define charter school exposure from two different dimensions. First, we construct charter school exposure as the number of years students were exposed to charter schools in their resident counties before graduating from secondary schools. Second, we define a comprehensive measure of exposure analogous to those used in Aaronson and Mazumder (2011), who estimated

[2] See from data.publiccharters.org/

[3] Most of these studies consider outcomes, including college enrollment and a transition from 2- to a 4-year institution. For example, Sass et al. (2016) and (Booker et al., 2011) has college enrollment as the main outcome in their Florida and Chicago studies.

[4] Notably, studies have focused on Texas, Chicago, Michigan Denver, New York, Boston, Florida, North Carolina, and a few others ((Sass et al., 2016; Booker et al., 2011; Dobbie and Fryer, 2020; Bettinger, 2005; Booker et al., 2007; Ni, 2009; Hanushek et al., 2007; Zimmer et al., 2012; Jinnai, 2014; Hoxby, 2004; Davis and Raymond, 2012).

Rosenwald school exposure on black achievements and Miller and Wherry (2019) that assessed the long-term effects of early Medicaid coverage, respectively. Our measure calculates the intensity of charter schools coverage before graduating from high schools.

We use an instrumental variable approach as our empirical strategy since the ordinary least squares has two primary limitations. First, there might be endogenous migration, which would bias the estimates if we use the exposure at the county of residence. Charter school opening could induce students to migrate across counties. Second, even if we define charter school exposure at birth counties, we still face attenuation bias concerns since people do not stay in their county of birth forever. To address these issues, we compute the two measures, exposure from the county of residence and at the county of birth. Then we use the birth county exposure as an instrument for that of the county of residence. Therefore, the first-stage model regresses the county of residence's exposure on exposure from the birth county and controls for individual characteristics, county, and birth year fixed effects. The second stage then estimates the impacts of charter school exposure from the county of residence on the outcomes. Using this strategy overcomes the attenuation and endogeneity concerns.

We link potentially exposed individuals from the restricted, geocoded National Longitudinal Survey of Youth to charter schools at the county level using the National Center for Education Statistics Common Core Data, providing the universe of public elementary and secondary schools. A summary of our findings is as follows. We find a local average treatment effect estimate such that an additional year of charter school exposure increases the chances of completing a four-year college or better by 3%. When we use charter school coverage as the measure, our results are that a 1-point increase in the exposure increases the probability of completing a four-year university education or better by 0.4% among those induced. We also obtain larger estimates (i.e., 4% for every additional year and 0.5% for a 1-point increase in charter school coverage) when we restrict our sample to include only those who completed high schools. We also demonstrate that charter schools favor females more than males and Blacks and Hispanics than Whites on four-year college completion. Our local average treatment effects for alcohol consumption and cigarette smoking are small but not negligible and more pronounced among high-educated individuals and Blacks and Hispanics.

The study makes four substantial contributions to the literature. First, we are the first to provide national estimates on charter schools' impacts on students' outcomes to the best of our knowledge. Second, because the law started in 1991 and by 2003, more than 40 states had opened charter schools, there is a need to consider their long-term impacts on students' outcomes. Importantly, the inconclusive evidence of their short- and medium-term effects makes it impossible to predict their long-term effects. Third, a few studies have considered the impacts of charter schools on health behaviors. Although the policymakers intend to improve education outcomes, we demonstrate that charter schools also reduce adverse health behaviors. Finally, rather than focusing on only the direct effects (i.e., charter school attendance), our strategy estimates the spillover effects of charter schools, adjusting for their impacts on those who were indirectly affected.

We organize the rest of the paper as follows. In Section 2.2, we provide brief institutional details of charter school establishment. Section 2.3 summarizes the previous studies on charter schools, while Section 2.4 discusses the conceptual framework and mechanisms through which charter school exposure affect students' long-term outcomes. The empirical models and charter school exposure measures and data sources for this study follow in Section 2.5. We present the results in Section 2.6 and discuss the results, provide policy recommendations, and conclude the paper in Section 2.7.

2.2 Institutional Details

Charter laws are state-level legislation that permits K-12 schools to operate independent of local authorities. A state that legislates a charter school law provides guidelines for writing charter contracts. Charter authorizers are responsible for managing charter schools through the contracts. The authorizers can be the local school board, the state education board, or any independent organization. Charter laws differ by state. For example, several states place caps on the number or percent of charter schools the state (or a school district) can establish at any period. Mississippi passed charter law in 1996 but started with only one pilot charter school until 2013. California restricted authorizations to 250 schools in 1998/99 but allows a successive increase of 100 annually. Table 2.A1 in Appendix K presents states' charter law and regulations on the number of charter schools that the school districts permit. By 2018, about sixteen states and the District of Columbia imposed caps on charter schools.

Within school districts, charter authorizers are responsible for managing charter contracts. A traditional public school (henceforth, TPS) can convert into a charter school, while new schools can also begin as start-up charter schools. Charter schools can also reverse to TPS at any time. Non-profit organizations, people, or communities who wish to start charter schools apply for approval from the charter authorizers. In the application, the prospective school provides a comprehensive description of the school, including the attendance zone, the number of proposed students and teachers, available facilities, food and health service available, and students' background information such as students' age and grade levels. If the authorizer approves the application, they write the charter contract, and the two parties sign it.[5] The contract specifies essential clauses, including the duration of the charter status, the minimum academic performance, and a periodic reporting of the school's progress.[6] The school keeps the conditions in the contract; otherwise, the authorizers revoke the contract. A charter contract is also not renewed automatically. Instead, continuation depends on how well the school performs in upholding the standards specified in the contract.

After gaining the charter school status, it operates independently as a public school. Charter schools are semi-autonomous. Like any TPS, charter schools are publicly funded, have more freedom over their budgets, staffing, curricula, and other operations. They cannot charge tuition or demand extra fees from students and must hold the same academic accountability measures as TPSs and private schools (Yilan and Berger, 2011). Meeting the accountability standards outlined in the contracts are some of the ways that charter schools use to maintain their charter status so that they get exempted from a particular state or local rules and regulations accompanied by freedom, flexibility, and autonomy.[7]

Unlike the TPSs, charter schools are open to all students within the school district or attendance zone. Students within the school district are given enrollment opportunities before allowing those outside the school district to sign up if seats are still available.[8] In this regard, parents freely choose to enroll (or disenroll) their children in (or from) any charter school. A charter school cannot discriminate on demographic

[5] See more from https://www.dekalbschoolsga.org/charter-schools/.

[6] An example of a charter contract is available at https://www.gadoe.org/External-Affairs-and-Policy/Charter-Schools/Documents/Atlanta%20Public%20Schools.pdf.

[7] See more from "The Condition of Education 2018", a report on education from the National Center for Education Statistics.

[8] See more from https://www.gadoe.org/External-Affairs-and-Policy/Charter-Schools/Pages/General-Frequently-Asked-Questions.aspx.

characteristics or medical conditions. A student who transfers from a TPS to a charter school moves with the previously provided funds. If a charter school receives more application than its capacity, it uses a lottery strategy to allocate students. Also, charter schools have periods when students can apply for consideration. Therefore, charter schools are alternative (i.e., substitutes) to TPS and private schools since parents do not incur additional direct costs to transfer their children. In localities where all public schools are charter are called charter districts or system. A few charter districts exist.[9] The Decatur City Schools in Georgia is an example of a charter school system.

2.3 Previous Studies

Many studies have estimated the short-term impacts of charter school attendance on several outcomes, predominantly test scores but find mixed evidence with no consensus. Some evidence of no impact exists (Dobbie and Fryer, 2020; Zimmer et al., 2012; Booker et al., 2007; Hanushek et al., 2007; Bettinger, 2005). However, there is both positive and negative evidence as well. One group of studies find positive effects (Abdulkadiroğlu et al., 2017; Angrist et al., 2016; Dobbie and Fryer Jr, 2015; Deming et al., 2014; Jinnai, 2014; Abdulkadiroğlu et al., 2011; Hoxby, 2009; Booker et al., 2008; Holmes et al., 2006; Hoxby, 2004). On the other hand, some studies also find negative effects (Imberman, 2011; Winters, 2012; Ni, 2009; Sass, 2006; Bifulco and Ladd, 2006).[10] Other nonacademic outcomes of students such as health behaviors are rarely analyzed in the literature. Dobbie and Fryer Jr (2015) find that charter school atten-

[9] See more from https://www.crpe.org/publications/charter-school-districts.

[10] Hoxby (2004) surveyed 36 charter schools in 15 states and found that charter school attendance increases math and reading proficiency except that of targeted and at-risk students. The study included these states: Alaska, Arizona, California, Colorado, DC, Florida, Georgia, Hawaii, Illinois, Louisiana, Massachusetts, Michigan, New Jersey, New York, Ohio, Oregon, Pennsylvania, Texas, and Wisconsin.

dance reduces pregnancy for girls and incarceration for boys using New York data. One caveat is that studies that find positive impacts on short-run outcomes also find that students perform poorly in the few years following enrolling in charter schools (Booker et al., 2007; Hanushek et al., 2007).[11]

A few studies have considered medium-term outcomes, including college enrollment and shifts from 2- to 4-year institutions. Generally, they find positive effects (Sass et al., 2016; Angrist et al., 2016; Dobbie and Fryer Jr, 2015; Booker et al., 2011).[12] Deming et al. (2014) is the only study that has studied four-year college degree completion. They use Charlotte-Mecklenburg schools data to find that girls who attend their first-choice schools are 14 percentage points more likely to complete four-year degree colleges, but no effects on boys. To the best of my knowledge, only two studies have analyzed labor-market outcomes. However, these two papers find contradictory evidence. Sass et al. (2016) find that charter school attendance has positive effects on students earnings in their early 20s, while Dobbie and Fryer (2020) find adverse effects.[13] Therefore, the medium-term effects are also inconclusive. Besides, other studies have considered the effect of charter school competition on students' outcomes. In general, there is mixed evidence in the literature as well. While studies such as Ni (2009) find less competitive effects, Zimmer et al. (2009) finds no impact, as Holmes et al. (2006) demonstrates positive effects on competition among schools.

The studies discussed above focused on a few states or cities, usually from one state or city to a maximum of sixteen states or cities. However, as of 2017, all but six

[11] When students transfer from TPSs to charter schools, they experience lower outcomes than their counterparts who remain in TPSs in the first two years. They equalize in the third and fourth years before experiencing a positive impact afterward.

[12] Dobbie and Fryer (2020) analyzed administrative data from Texas and found that at the mean, charter school attendance has no impact on test scores, and the best case is increasing both test scores and college enrollment.

[13] Sass et al. (2016) study students from Florida and Chicago using administrative data find that charter school students experience higher earnings in their mid-20s. (Dobbie and Fryer Jr, 2015) also analyzed the impact of charter school attendance on students' outcomes six years after enrollment using Texas data.

states had opened charter schools. Since the school systems in states operate differently based on their educational policies, state-level estimates in the literature may lack external validity. Therefore, a study that uses national data is important. Additionally, charter schools' long-term impacts are relevant since states and the federal government expenditure on welfare programs correlate with the later-in-life outcomes. For example, states spent about $31.7 billion on Temporary Assistant for Needy Families (TANF) in 2015,[14] $592.75 billion on Medicaid in 2017,[15] and $66.54 billion on Supplemental Nutrition Assistance Program (SNAP) in 2016.[16] Understanding how charter school exposure affects these outcomes is useful because policymakers will understand how they possibly change these expenditures through education.

2.4 Conceptual Framework

Card and Krueger (1996) developments on Becker (1967) model provides a basis for understanding how charter schools affect students' future outcomes. Additionally, Aaronson and Mazumder (2011) framework demonstrates how changing schools' availability and quality affect the socially optimal choice. One precise prediction of the latter study's model was that Rosenwald's school construction, increasing the number and quality of schools in rural counties, increased the time spent in school among treated students. Since charter school opening is an expansion of school choice, we develop a similar framework for understanding the model's prediction as charter schools opening varied over time. By equating the marginal cost to the marginal benefit of schooling,

[14] See from https://www.acf.hhs.gov/ofa/resource/tanf-and-moe-spending-and-transfers-by-activity-fy-2015.

[15] See from https://www.medicaid.gov/state-overviews/scorecard/national-context/annual-expenditures/index.html.

[16] See from https://fns-prod.azureedge.net/sites/default/files/snap/FY16-State-Activity-Report.pdf.

students can choose their optimal education. Since the marginal benefit is a positive function of the school quality, improving the school quality increases the marginal benefit. Hence, the optimal level of schooling among affected school children rises as the school quality improves.

Suppose there are individuals with a suboptimal level of schooling due to some reasons, including low quality of schools and lack of competition. Then any policy that improves the quality of education leads to an increment in human capital investments. If opening charter schools enhances school quality within that neighborhood, students whose marginal benefits rise will invest more in their education. It implies that the theoretical prediction is an increment in schooling years among students with suboptimal levels due to charter school openings. Of course, one can argue with many reasons why charter school openings may improve school quality. States passing charter laws permit the penetration of charter schools, which serve as alternatives to TPSs to drive competition. Therefore, every new charter school potentially improves the average school quality in its neighborhood. Also, the fact that parents and students searched for alternative schools suggests that they were not satisfied with the quality of TPSs and wanted better alternatives.

Contrarily, suppose charter schools opening decreases the quality of schools in its neighborhood. In that case, the marginal benefit of schooling falls, and the optimal choice of years of education decreases as well. The optimal levels of investments in human capital will fall among affected school children. However, the argument that charter schools can reduce the quality of schools in their neighborhoods is debatable. On the one hand, proponents argue that charter schools are under close supervision and close if they cannot satisfy their charter contracts. The authorities' close monitoring ensures that charter schools are accountable to the charter contract. Moreover, parents possibly migrate or move their children to places with relatively good schools if schools' quality falls in their neighborhood. However, critics of charter schools argue that since a few charter schools closed for many reasons, including mismanagement, inadequate enrollment, and non-compliance of contract, they could have detrimental effects on students. Therefore, one cannot undermine the possibility that some charter schools negatively affect quality in their neighborhoods. In summary, the theoretical prediction of a charter school opening on human capital investment is ambiguous.

Aside from educational outcomes, there is no direct connection between charter school attendance or the state's charter law and health behaviors. However, there are so many mechanisms through which charter school exposure can potentially affect health behaviors in the long-term. Earnings and education are two apparent mechanisms. If charter schools impact earning and education, then we expect health outcomes to be affected. Starting from Grossman (1972), a body of literature has established the effects of earnings and education on health behaviors and outcomes. In his model, education increases the productivity of medical services, which predicts that any exogenous shock that shifts the education level increases the demand for health. Therefore, higher education leads to improved health outcomes. Also, in the model, higher earnings increase the opportunity cost of sick days. People cannot afford to stay home when their earnings are high. Therefore, they invest more in their health. The higher earnings translate into health investment to get more healthy days to work.

Many empirical studies have established these predictions of the Grossman (1972) model. For example, studies including but not limited to Gerdtham et al. (1999); Apouey and Clark (2015); Lindahl (2005) find that income improves health while Lleras-Muney

(2005); Silles (2009); Kenkel et al. (2006) find that higher education causes less risky behaviors and improve health outcomes. Using charter school exposure as an exogenous source of variation for education to estimate their impacts on health behaviors and outcomes is a useful exercise.

Because Sass et al. (2016) find that charter school attendance increases students' earnings in their mid-20s, it is exciting to explore how these students' health behaviors and outcomes are affected. Also, since Dobbie and Fryer Jr (2015) demonstrate that Texas' charter school attendance reduces teenage pregnancy and male incarceration, we further explore these outcomes using national data. Only these studies have analyzed the charter school effects on earnings and health behaviors to the best of our knowledge.

Aside from the two mechanisms discussed above, Dobbie and Fryer Jr (2015) identified other channels through which charter school attendance can affect health outcomes and behaviors. They argued that noncognitive skills, social networks, and economic preference parameters are other mechanisms. They also find that charter school attendance negatively impacts noncognitive skills, including self-esteem and persistence. Also, they find that attending charter schools has no effect on the discount rate but increases risk-aversion. The incremental change in risk aversion implies that charter school students are less likely to take risky behaviors, including smoking, drinking, and drug abuse, but are more likely to invest in their health, including purchasing insurance, exercising, and eating healthy food. Finally, they find that charter school attendance does not cause changes in peer quality. Because the evidence suggests that charter schools affect health behaviors, it is interesting to use nationally representative data.

2.5 Methods

2.5.1 Empirical Strategy

To address endogeneity in this study, we abstract away from models that compare the outcomes of students who attend charter schools to students who attend TPSs. We discuss the various method used in the literature in Appendix J. Our method relies on a few assumptions. First, we assume that an opening of charter schools potentially affects all elementary and secondary school students at all grade levels in the county. Because every student at the level of implementation of the law in the county with a charter school is potentially exposed, it is not feasible to use a pre-post approach to identify the effects. Second, we assume that there is no heterogeneous effect across grades. In other words, the impact of exposure in elementary level 6 is not different from that of grade 12. From these assumptions, we estimate a model that compares the outcome of students with varying levels of exposure based on year, cohort, and the county of residence (i.e., cohort-by-year-by-county analysis).

We begin with a basic specification of an intent-to-treat model below:

$$Y_{ibct} = \gamma_0 + \gamma_1 \text{ACSExposure}_{ibc} + \boldsymbol{\beta}\mathbf{X_{ibct}} + \boldsymbol{\lambda_c} + \boldsymbol{\lambda_b} + \boldsymbol{\lambda_t} + \xi_{ibct}. \tag{2.1}$$

In equation (1), the variable Y_{ibct} represents the outcome of the individual i, born in birth cohort b at county c, whose outcome was observed at year t. The outcomes are years of schooling, college attendance, cigarette smoking, and alcohol consumption. The variable ACSExposure_{ibc} represents a measure of charter school exposure for individuals i, among the birth cohort b, and in county c. The vector X_{ibct} represents a set of individual basic characteristics. To overcome potential endogeneity, we only include age, race, gender, and education. Also, $\boldsymbol{\lambda_c}$, $\boldsymbol{\lambda_b}$, and $\boldsymbol{\lambda_t}$ represent vector of county, birth cohort, and year fixed effects, respectively. Finally, the variable ξ_{ibct} captures the random unobserved component of equation (1).

The argument for specifying the model of this form is that the outcomes of the student within a county with charter schools may vary by birth cohort. In equation (1), the coefficient of interest is γ_1, capturing the effect of charter school exposure at the county level. By including the county, birth cohort and year of outcome fixed effects in the model, we compare individuals born in the same county across periods, while those in counties without charter schools serve as controls for those in areas with charter schools. The model exploits the cross-cohort variation in the timing of the opening of charter schools in counties.

In the next few paragraphs, we provide a detailed account of the strategies used to generate various measures of charter exposure. These measures rely on a few assumptions as follows. First, each cohort begins their first grade at age 6 and are expected to graduate from high school at age 18, irrespective of the state of residence. Although the school starting age in states in the U.S. are 5, 6 or 7, the mean and median age of starting school is 6. Second, every child in ages 6–18 in counties where charter schools opened was exposed. Third, all children of ages 6–8 enrolled in schools.

As our first measure of the charter school exposure, we aggregate the number of years that each birth cohort were exposed based on their county of residence. This variable is discrete and takes values 0 to 12. Using this definition, we assume that all states use a 12-year system where students begin first grade at age 6 and are expected to graduate at age 18. Therefore, by observing the grade of the student at the time the county of residence first opened a charter school and the years expected to graduate, we calculate the maximum years of exposure.

Our second measure of charter school exposure is like the definition of Rosenwald school exposure in Aaronson and Mazumder (2011). In their study, they constructed the "Rosenwald Coverage" that each student born in year b in county c experienced over ages 7–13 as the average probability of enrolling in the Rosenwald school.[17] Additionally, Miller and Wherry (2019) used a similar strategy in their long-term effect of early life Medicaid study. In their study, they constructed a cumulative measure of public health insurance eligibility at ages 1-18 for each birth year and the state as the fraction of children eligible for coverage at each age during childhood in each state and summed across ages. We construct our exposure variable as the average charter school coverage that each student born in year b in county c experienced between ages 6–18 based on counties ever resided before graduating from high school. That is, we compute the second measure of exposure ("actual exposure") based on all counties that the student lived before graduating from secondary school. Specifically, for all individuals in my sample who began school after their counties opened charter schools, we define exposure as:

$$\text{ACSExposure}_{ibc} = \frac{1}{T_{ib}} \sum_{\tau=b+6}^{b+12} \frac{\text{CSStud}_{c\tau}}{N_{c\tau,6-18}}, \tag{2.2}$$

where T_{ib} denotes the number of years student i in birth cohort c was exposed to charter school, which depends on the counties lived. We also use $CSStu_{c\tau}$ to represent the number of charter school students in county c at year τ and $N_{c\tau,6-18}$ for the number of school-going children of ages 6-18 in county c in year τ.

[17] Aaronson and Mazumder defined their measure as $\text{E}_{bc} = \frac{1}{7}\sum_{\tau=b+6}^{b+13} \frac{\text{T}_{ct}\times 45}{N_{ct}}$, where E_{bc} represents Rosenwald exposure for individuals born in year b in county c, T_{ct} represents the number of Rosenwald teachers and N_{ct} represents the number of black populations in county c.

This measure of charter school exposure calculates the cumulative probability of enrolling in a charter school at the county level and normalizes it by the expected years of exposure. It measures the average probability of enrolling in charter schools for all individuals throughout their elementary and secondary school years. The variation comes from the fact that individuals in counties without charter schools get zero exposure, while those in counties with charter schools get value ranging from 0 to 1, depending on the years of exposure and available seats. Therefore, two individuals born in the same county can have different exposures due to differences in the year of birth and places stayed.

Notice that exposure to charter schools depends on the counties that the person lived during his elementary and high school period. A major drawback of this approach is endogenous migration. Students could move due to charter school opening. This will bias the estimates in equation (2.1) if estimated by OLS. Even if we calculate the exposure at the county of birth, we would attenuate γ_1 since people do not stay in their county of births forever. To address these issues, we calculate the exposure from the county of birth and follow Miller and Wherry (2019) to use an instrumental variable approach. The first stage model is as follows:

$$\text{ACSExposure}_{ibc} = \alpha_0 + \alpha_1 \text{CSExposure}_{bc} + \mathbf{\Phi X_{ibct}} + \boldsymbol{\lambda_c} + \boldsymbol{\lambda_b} + \boldsymbol{\lambda_t} + \nu_{ibct}. \tag{2.3}$$

In equation (2.3), the dependent variable represents the "actual exposure," which depends on the year born and county of residence before graduating from high school, is regressed on charter school exposure in the county of birth, CSExposure_{bc}. We also include a vector of covariates that we use in equation (2.1). The parameter of interest is α_1, which shows the correlation between charter school exposure at the county of birth and residence.

We discuss the identifying assumptions as follows. First, the instrument needs to be relevant. We test this assumption from the data by looking at the first stage estimates, capturing the correlation between exposure in the county of birth and residence. A high positive α_1 and F-statistic would imply a robust first stage estimate and suggest exposure in the county of birth highly predicts exposure in the county of residence. The second identifying assumption required for a causal interpretation of our estimates is the exclusion restriction. This assumption requires that the charter school exposure be uncorrelated with omitted and unobserved variables that affect education outcomes and health behaviors. It also implies that the only mechanism through which the instrument affects the outcomes is exposure in the resident counties. The extent to which this assumption is satisfied depends on the randomness of charter school opening in birth counties. If charter school opening correlates with county characteristics, including wealth, educational resources, and educational outcomes, our IV estimates would be biased. We minimize this possibility by including county-level time-varying factors in our models. Finally, we discuss the assumption of monotonicity, which requires that the instrument must affect subjects in the same direction. In other words, the exposure in the birth county must increase exposure in the county of residence and not decrease it. Since this assumption is less likely to be satisfied among some children (i.e., some non-compliers exist), our estimates come from only compliers (local average treatment effects or simply LATE), children whose charter school exposure positively correlates with exposure in their birth counties.

2.5.2 Student-Level Data

The primary source of individual-level data comes from the publicly available and restricted, geocoded National Longitudinal Survey of Youth (NLSY), organized by the Bureau of Labor Statistics (BLS). The BLS follows different groups of individuals in the NLSY surveys. One cohort is a sample of youth born from 1957 through 1964, which included about 12,700 individuals surveyed in 1979 (i.e., NLSY79). Since charter school openings started in 1992, none of the youth in this sample potentially attended one. Fortunately, the BLS began to follow the children of all females in the NLSY79 cohort as well. This series tracks all children under the age of 14 born to women in the NLSY79 cohort annually beginning from 1986. In the NLSY79, the mothers provide information on their children. Starting in 1994, the BLS followed the young adults aged 14 or more born to all women in the NLSY79 cohort. These two series were combined and name NLSY79 Children Survey and Young Adult Survey.

We include all individuals in the sample potentially exposed to charter schools born from 1974–1995 in the NLSY79 Child and Young Adult Survey. The sample exclude all children who had completed high school before 1992, when charter school opening started. We supplement these individuals with the NLSY97 cohort, born from 1980-1984, due to sample size concerns. One advantage of using the NLSY data is that it collects information on respondent's state and county of residence. In the children's sample, because we can identify where their mothers lived when they were born or before charter school opening began, their county of birth and residence are available.[18] We obtained the respondents' geographic data and linked it to the publicly available

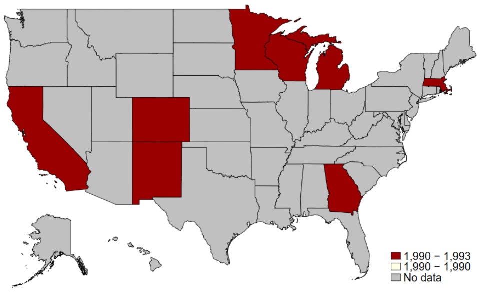
[18]Unfortunately, the NLSY data does not include census tracks, residential address, or school district of residence. Therefore, the smallest geographic identifier is county-level information.

information. The publicly available data has all other data, including demographics, including family income, age, sex, educational attainment, years of schooling, economic status such as employment status, type of employment, and wages, etc., and health behaviors, including smoking, alcohol consumption, and incarceration status.

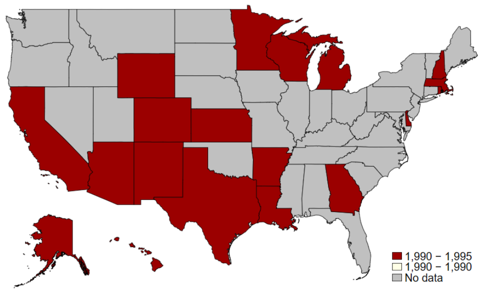
2.5.3 Charter School Data

One limitation of the NLSY data is that the BLS did not collect charter school information until 2000. Consequently, using the actual charter school attendance to estimate the long-term impacts can bias the estimates due to the missing information. We use the National Center for Education Statistics (NCES) Common Core Data (CCD)—data on the universe of public elementary and secondary schools. Every year, the NCES gathers information on all primary and secondary schools. Relevant information needed is the number of full-time teachers and county of residence of all charter schools. Therefore, the unit of analysis for the variable of interest, charter school exposure, can only be aggregated within the residence county.

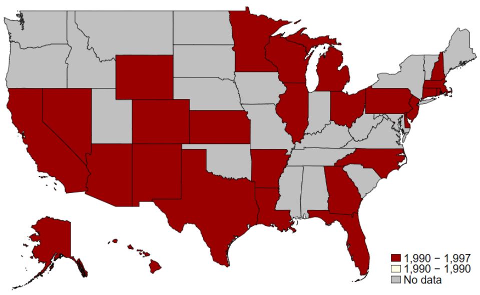
A limitation to the NCES-CCD data is that they included charter schools' identifiers starting from 1998, even though charter school opening began in 1992. To overcome missing data concerns, we supplement the NCES-CCD data with two additional sources to identify all charter schools. First, we contacted the twenty-four states that opened charter schools before 1998, but only eight states have made the data available. Unfortunately, some states do not keep information on charter schools at all. Second, we scrape charter school data from the U.S. Department of Education (ED) National Charter School Resource Center (NCSRC) website. The ED-NCSRC keeps names of all charter schools it has funded in the past. Importantly, for all active charter schools



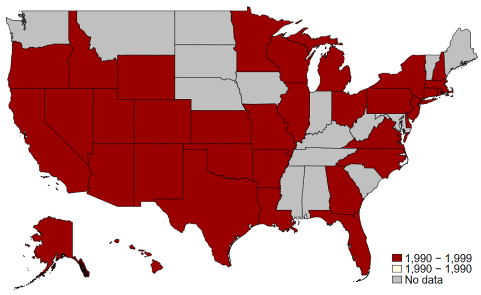
(a) 1992 - 1993



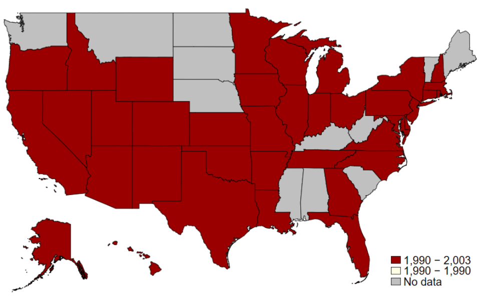
(b) 1992 - 1995



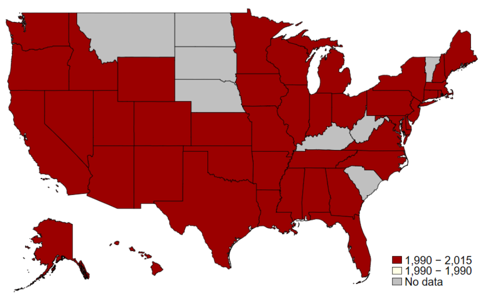
(c) 1992 - 1997



(d) 1992 - 1999

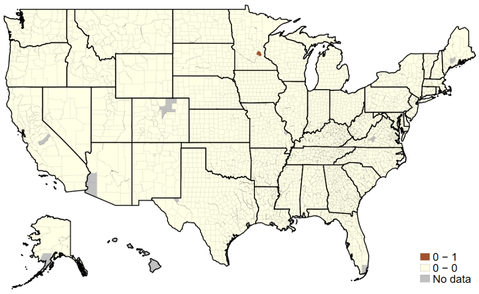


(e) 1992 - 2003

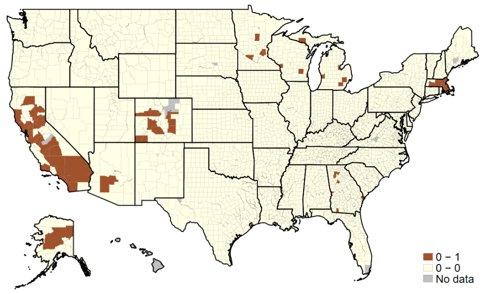


(f) 1992 - 2015

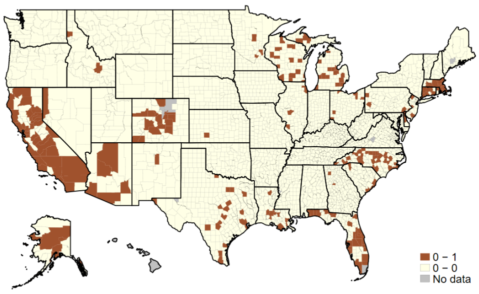
Figure 2.1. Timing of States Charter School Laws (1992 - 2015).



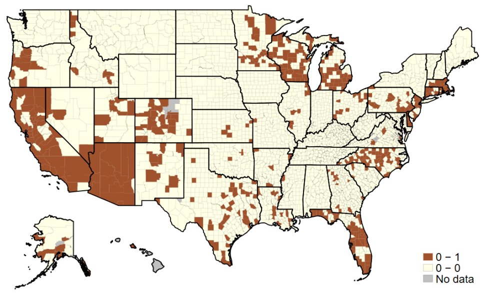
(a) 1992



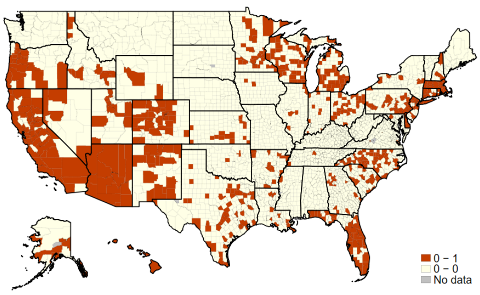
(b) 1995



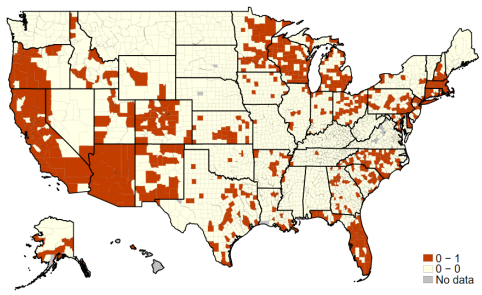
(c) 1998



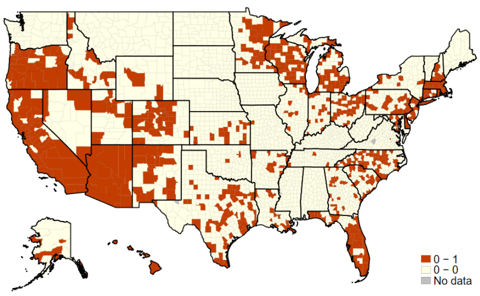
(d) 2001



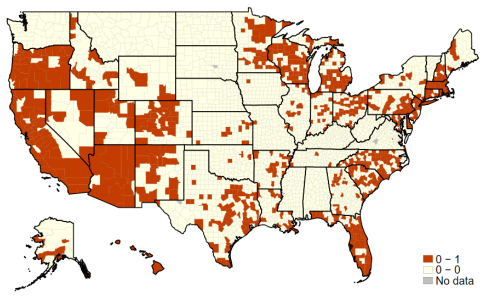
(e) 2004



(f) 2007

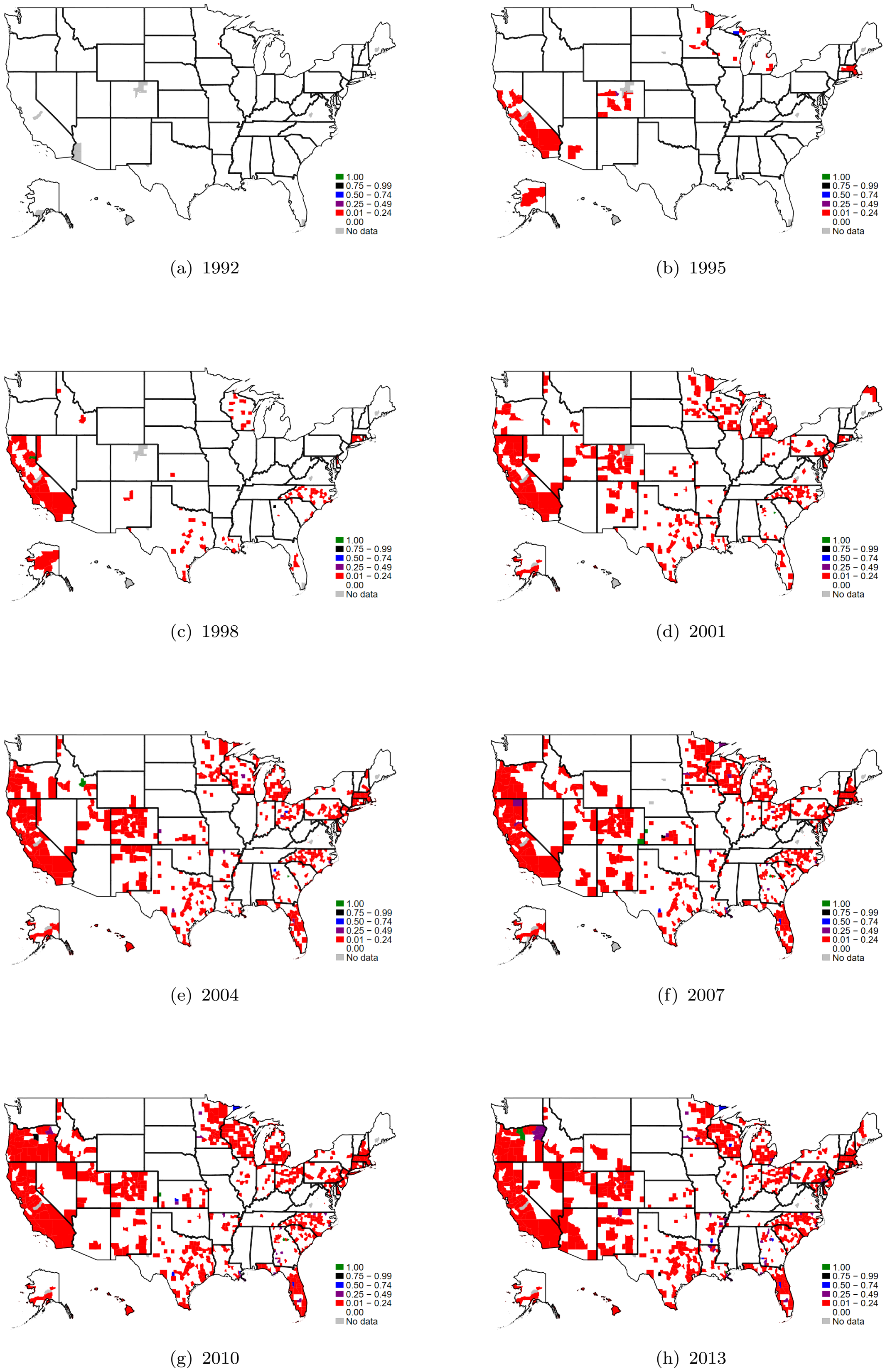


(g) 2010



(h) 2013

Figure 2.2. Charter School Presence in Counties for Some Selected Years (1992 - 2013).



(a) 1992

(b) 1995

(c) 1998

(d) 2001

(e) 2004

(f) 2007

(g) 2010

(h) 2013

Figure 2.3. Charter School Coverage in Counties for Some Selected Years (1992 - 2013).

under their umbrella, it keeps their information, including address, year of opening, number of students in the current year, and grades offered in the current year. Unfortunately, it does not include information on closed charter schools. Nevertheless, it publishes a complete list of all closed charter schools every year. Their publication consists of the school name, state and school district of residence, and the year opened or closed. Hence, we identify a complete list of all charter schools that opened and is still in operation or closed. We match all charter schools that existed before 1998 to the NCES data using school names and addresses.

2.5.4 Descriptive Statistics

Figure 2.1 shows variations ins states' adoptions of charter school laws over time. Seven states had not adopted the policy by 2015. Among states that passed the law, Figure 2.2 shows the timing of charter school openings within counties. Our definition for presence is all counties that had at least one charter school. We also calculate charter school coverage in each county and year, shown in Figure 2.3. Despite the fact that several counties opened charter schools, their students' populations were mostly low and below 25%. Therefore, the average exposure across all periods is expected to be small.

In Table 2.1, we summarize the variables. We obtained about $13,000$ individuals born in 518 counties. About 56% of them were exposed to charter schools in their county of residence. The average years of charter school exposure were 3.1 years, and their chances of getting admissions in charter schools were about 2.8%. Among those exposed to charter schools, the average length of exposure was about 5.6 years, and they had a 5.1% chance of enrolling in a charter school. To get enough variation in the variable of interest (i.e., exposure to charter schools), we exclude all counties with less than five births throughout the study period. Of the sample, 7,348 lived in counties with charter schools before graduating from secondary schools, while the remaining 5,817 lived in counties that never opened charter schools before graduating from high schools. The sample consists of 60% of individuals drawn from the NLSY97 cohort (the cohort born from 1980 to 1984). The remaining 40 percent comes from the NLSY79 Child and Young Adult Survey, born in 1971 to 1995. The NLSY oversampled the minority (i.e., the Blacks and Hispanics). Whites, Blacks, and Hispanics in our sample are 49, 31, and 20%, respectively. To mitigate the impacts of oversampling in our regressions, we use sampling weights and report the weighted means in the last two columns of Table 2.1. The weighted means are closer to national estimates. Males constitute about 51% of the sample. The ages of individuals in the sample ranged from 15 to 42, with an average of 30.

Table 2.1. Means and Standard Deviations (in Parenthesis) of Outcomes and Characteristics of Potentially Exposed Individuals

	Unweighted Means			Weighted Means	
	Full Sample	Not Exposed	Exposed	Not Exposed	Exposed
Outcomes					
Years of Schooling	13.25	13.27	13.24	13.37	13.45
	(2.589)	(2.651)	(2.539)	(2.524)	(2.352)
No High School Diploma	0.188	0.192	0.186	0.165	0.138
	(0.391)	(0.394)	(0.389)	(0.371)	(0.345)
High School Diploma	0.260	0.276	0.247	0.267	0.217
	(0.438)	(0.447)	(0.431)	(0.443)	(0.412)
Some College	0.295	0.278	0.309	0.299	0.331
	(0.456)	(0.448)	(0.462)	(0.458)	(0.471)
College or Better	0.256	0.254	0.259	0.268	0.313
	(0.437)	(0.435)	(0.438)	(0.443)	(0.464)
Current smoking	0.342	0.332	0.307	0.350	0.335

Continued on the next page

Table 2.1 – Continued from the previous page

	Unweighted Means			Weighted Means	
	Full Sample	Not Exposed	Exposed	Not Exposed	Exposed
	(0.475)	(0.471)	(0.461)	(0.477)	(0.472)
Binge Drinking	0.170	0.151	0.153	0.165	0.174
	(0.375)	(0.358)	(0.360)	(0.371)	(0.379)
Charter School Exposure					
Binary (0/1)	0.558				
	(0.497)				
Years [0,12]	3.116		5.583		6.022
	(3.754)		(3.388)		(3.613)
Coverage [0,1]	0.028		0.051		0.065
	(0.125)		(0.164)		(0.185)
Controls					
Black	0.308	0.329	0.292	0.169	0.164
	(0.462)	(0.470)	(0.455)	(0.374)	(0.370)
White	0.490	0.579	0.419	0.784	0.701
	(0.500)	(0.494)	(0.493)	(0.412)	(0.458)
Hispanic	0.202	0.092	0.289	0.048	0.135
	(0.401)	(0.289)	(0.453)	(0.213)	(0.342)
Male	0.509	0.510	0.507	0.527	0.525
	(0.500)	(0.500)	(0.500)	(0.499)	(0.499)
Birth County Known	0.608	0.569	0.638	0.670	0.752
	(0.488)	(0.495)	(0.481)	(0.470)	(0.432)
NLSY97 Cohort	0.601	0.627	0.580	0.475	0.398
	(0.490)	(0.484)	(0.494)	(0.499)	(0.490)
Observations	13,006	7,348	5,817	7,348	5,817

Since we do not have information on the county of birth for the individuals in the NLSY97 cohort but only know their counties resided on their 12th birthday, we use them as proxies for their birth counties. With charter schools opening beginning from 1992, we include a dummy variable in the regressions to distinguish between all individuals whose county of births are available or those in states that later adopted charter school law. About 60% of the individuals have information of their county of birth or residence at age 12 available.

We also summarize the outcomes in Table 2.1. The schooling outcome represents the highest grade as of the date of the last interview. The survey asks respondents to select one for the following categories: 8th grade or less, some high school, high school graduate, some vocational or technical after high school, completed some vocational or technical after high school, some college, completed an associate degree, completed bachelor's degree, some graduate, completed a Master's degree, some graduate beyond a Master's degree, Ph.D., some professional education such as Law, Medical School, Nursing, etc., and completed a professional education. We coded these categories into a continuous education outcome. For individuals with years of schooling above 20, we top-coded to 20.[19] The data shows that, on average, students in the NLSY attended some college (i.e., 13 years of schooling). We do not find any statistically significant difference between the years of education of those exposed to charter schools and those that were not. Since our continuous education measure is more likely to be inaccurate, we also create discrete education outcomes as follows: some high school, high school graduate, some college, and a college degree or better. About 19% had no high school diploma in the sample, while 26% had completed only high school. Also, about 30% of the individuals had between 13 and 15 years of education (some college). The remaining 34% had college degrees.

Two other outcomes that we consider in this study are cigarette smoking and alcohol consumption. Prevalence of current smoking (i.e., past 30-days smoking) was 2% lower among those exposed to charter schools than their counterparts that did not have any exposure. However, binge drinking (i.e., drinking more than 5 bottles a day) did not differ by charter school exposure status.

[19] We do not distinguish between the number of years required to complete different post-graduate programs, especially professional programs such as law school, medical school, doctoral programs.

2.6 Results

2.6.1 First Stage Estimates

A set of summary statistics that we do not report in Table 2.1 is information on exposure when individuals are treated as if they lived in the same counties forever (i.e., exposure at the county of birth). Among those born in counties that did not open charter schools in their elementary and secondary school years, only 10% were exposed to charter schools and only had about 4 months of exposure, on average. In contrast, among those born in counties that opened charter schools during their school ages, they were about 96% more likely to have some exposure to charter schools, and their average years of exposure were about 5.6. Notice that some individuals moved from their birth counties before charter schools opened. Therefore, not everyone whose birth counties opened charter schools had actual exposure.

The description above suggests that the unconditional correlation between charter school exposure in the birth county and residence is positive. On average, individuals who have more years of exposure at their birth counties are more likely to have more years of exposure in their county of residence. Before discussing the main results, we formally test this correlation to assess the strength of our instrument by estimating equation (4) and presenting the results in Table 2.2. The results in columns (1)–(4) show the case where we construct our variable of interest and instrument as years of charter school exposure. We use the years of exposure in the birth counties as an instrument for the years of exposure in the county of residence.

The estimates in the last four columns are for the case where we define our variable of interest as charter school coverage, the chance of getting a seat in charter schools

Table 2.2. First Stage Estimates for Education Outcomes - Effects of Charter School Exposure in the County of Birth on Exposure in the County of Residence

	Years of Charter School Exposure in County of Residence [0,12]				Proportion of Students in Charter School in County of Residence [0,1]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years [0,12]	0.908***	0.888***	0.819***	0.818***				
	(0.010)	(0.013)	(0.026)	(0.026)				
Coverage [0,1]					0.755***	0.730***	0.707***	0.700***
					(0.037)	(0.043)	(0.046)	(0.041)
F-Statistic	7,704	4,490	995	990	422	287	238	290
Observations	13,006	13,006	13,006	13,006	13,006	13,006	13,006	13,006

Notes: Each estimate comes from a separate regression. The regressions include gender (male/female), race (Black, White, and Hispanic), a dummy for birth county availability, and NLSY cohort fixed effect. Additionally, for the specifications in columns (2) and (6), we add the year of survey and birth cohort fixed effects. In columns (3) and (7), we also add county fixed effects. Finally, for the results in columns (4) and (8), the specifications control county time-varying characteristics by including poverty rate and unemployment rate. *p<.1, **p<.05, ***p<.01

in the county of residence, using the variable calculated at the birth county as an instrument. We provide alternative specifications to show how the results are robust to controls, year and county fixed effects and county-level time-varying characteristics, including unemployment and poverty rate. In all cases, the estimates are statistically significant at 1%, with F-statistics above 100, suggesting we have a strong instrument (Lee et al., 2020). From the results in the first four columns, we show that every year of exposure in the birth county is associated with approximately 10 months of actual exposure (i.e., exposure in the county of residence). Intuitively, it means that for individuals whose birth counties opened charter schools, they got approximately 10 months of exposure. Similarly, we interpret the results in the last four columns as follows. A 1-

point increase in charter school exposure in the birth county is associated with at least a 0.7 percentage point increase in the likelihood of getting actual exposure

2.6.2 Charter School Effects on Education Outcomes

We first show the effects of charter school exposure on education since it is the only channel for affecting the two other outcomes that we discuss later. In Table 2.3, we summarize our results when we measure education as a continuous variable. All our estimates are small and statistically insignificant, except the estimates in the first two columns. However, the estimates when we measure the charter school exposure in years are similar across different specifications, but the standard error blows up after including the county of birth fixed effects. In the first and fifth columns, we include only the basic controls (i.e., gender [male/female], a dummy for Hispanics, a dummy for birth county availability, and NLSY cohort fixed effect).

From column (1), we find that every additional year of charter school exposure increases schooling years by one week among those induced by the instrument (i.e., the local average treatment effect [LATE]). By including survey year and birth cohort fixed effects, which also control the individual's age, the estimate rises to approximately 10 days. Both estimates are statistically significant at 5%. After including the county fixed effects, the estimate decreases, and the standard error doubles; however, including the county time-varying characteristics increases the estimate. Therefore, the estimate from our preferred specification, where we include all the controls, is imprecise. When we define charter school exposure as coverage rates [see equation (2) and the results in columns (5) – (8)], our estimates are smaller and statistically insignificant across all specifications. We find imprecise estimates due, in part, our inability to calculate the

Table 2.3. Second Stage Estimates for Years of Schooling - Effects of Charter School Exposure on Years of Schooling using Exposure at the County of Birth as an Instrument

	Years of Charter School Exposure in County of Residence [0,12]				Proportion of Students in Charter School in County of Residence [0,1]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years [0,12]	0.021**	0.028**	0.019	0.026				
	(0.011)	(0.013)	(0.025)	(0.025)				
Coverage [0,1]					-0.031	0.113	0.093	0.135
					(0.154)	(0.180)	(0.295)	(0.294)
Observations	13,006	13,006	13,006	13,006	13,006	13,006	13,006	13,006

Notes: Each estimate comes from a separate regression. The regressions include gender (male/female), race (Black, White, and Hispanic), a dummy for birth county availability, and NLSY cohort fixed effect. Additionally, for the specifications in columns (2) and (6), we add the year of survey and birth cohort fixed effects. In columns (3) and (7), we also add county fixed effects. Finally, for the results in columns (4) and (8), the specifications control county time-varying characteristics by including poverty rate and unemployment rate. *p<.1, **p<.05, ***p<.01

students' actual schooling years precisely. For example, we do not know the exact years of schooling for those with some high school education.

Because the completed years of schooling results are less attractive, we measure education in discrete terms. Specifically, we focus on completed bachelor's degrees or better against everyone else and report the results in Table 2.4. We focus on our preferred specifications in columns (4) and (8). We find that every additional year of charter school exposure increases the probability of completing a four-year university education or better by 0.8 percentage points (i.e., 3% increase) among those induced by the instrument and is statistically significant at 10%. When we use charter school coverage, the LATE is such that a 1-point increase in exposure increases the chances of completing a four-year degree college by a 0.1 percentage point (0.4%) and is statistically

Table 2.4. Second Stage Estimates for Four-Year College Graduation - Effects of Charter School Exposure on Four-Year College Graduation using Exposure at the County of Birth as an Instrument

	Years of Charter School Exposure in County of Residence [0,12]				Proportion of Students in Charter School in County of Residence [0,1]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years [0,12]	0.002	0.002	0.007*	0.008*				
	(0.002)	(0.002)	(0.004)	(0.004)				
Coverage [0,1]					0.053	0.069	0.101**	0.101**
					(0.042)	(0.046)	(0.048)	(0.051)
Observations	13,006	13,006	13,006	13,006	13,006	13,006	13,006	13,006

Notes: Each estimate comes from a separate regression. The regressions include gender (male/female), race (Black, White, and Hispanic), a dummy for birth county availability, and NLSY cohort fixed effect. Additionally, for the specifications in columns (2) and (6), we add the year of survey and birth cohort fixed effects. In columns (3) and (7), we also add county fixed effects. Finally, for the results in columns (4) and (8), the specifications control county time-varying characteristics by including poverty rate and unemployment rate. *p<.1, **p<.05, ***p<.01

significant at 5%.

The results become stronger when we focus on those with high school diploma or better. We find that a 1-point increase in charter school coverage increases the likelihood of completing a four-year degree college by 0.15 percentage points (0.5%) among those induced by our instrument and is statistically significant at 1% (see Table 2.5). Our more substantial estimates suggest that charter schools move people from just a high school education or some college into four-year degree programs. We also demonstrate that charter schools significantly impact females than males (see Tables 2.6 and 2.7). Males experience smaller effects and are statistically insignificant at all conventional levels (see Table 2.6). When we consider the impact of charter schools on females, we find a large effect, but only significant at 10%. We also find that charter schools improve the educational outcomes of minorities (Blacks and Hispanics) than Whites (see Table 2.8 and Table 2.9).

Table 2.5. Second Stage Estimates for Four-Year College Graduation - Effects of Charter School Exposure on Four-Year College Graduation among High School Graduates using Exposure at the County of Birth as an Instrument

	Years of Charter School Exposure in County of Residence [0,12]				Proportion of Students in Charter School in County of Residence [0,1]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years [0,12]	0.001	0.001	0.008*	0.010**				
	(0.002)	(0.002)	(0.005)	(0.005)				
Coverage [0,1]					0.045	0.100*	0.152***	0.148***
					(0.053)	(0.054)	(0.052)	(0.055)
Observations	10,674	10,674	10,674	10,674	10,674	10,674	10,674	10,674

Notes: Mean of outcome is 0.326. Each estimate comes from a separate regression. The regressions include gender (male/female), race (Black, White, and Hispanic), a dummy for birth county availability, and NLSY cohort fixed effect. Additionally, for the specifications in columns (2) and (6), we add the year of survey and birth cohort fixed effects. In columns (3) and (7), we also add county fixed effects. Finally, for the results in columns (4) and (8), the specifications control county time-varying characteristics by including poverty rate and unemployment rate. *p<.1, **p<.05, ***p<.01

2.6.3 Charter School Effects on Later-in-Life Health Behaviors

We also show how charter school exposure possibly affects later-in-life health behaviors. One outcome we consider is excessive alcohol consumption. It is responsible for over 95,000 deaths annually in the U.S. and is associated with poor pregnancy outcomes and several chronic health effects, including heart attack, high blood pressure, heart disease, stroke, liver disease, and cancer.[20] We obtained 12,480 individuals who reported their alcohol consumption behavior (see Table 2.10). As already described in Table 2.1, about 17% of these individuals are binge drinkers (i.e., consuming five or

[20] See from https://www.cdc.gov/chronicdisease/resources/publications/factsheets/alcohol.htm

Table 2.6. Second Stage Estimates for College Attendance - Effects of Charter School Exposure on Four-Year College Graduation among Males using Exposure at the County of Birth as an Instrument

	Years of Charter School Exposure in County of Residence [0,12]				Proportion of Students in Charter School in County of Residence [0,1]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years [0,12]	0.003	0.002	0.008*	0.008				
	(0.002)	(0.002)	(0.005)	(0.005)				
Coverage [0,1]					0.051	0.042	0.067	0.072
					(0.058)	(0.066)	(0.093)	(0.089)
Observations	6,696	6,696	6,696	6,696	6,696	6,696	6,696	6,696

Notes: Each estimate comes from a separate regression. The regressions include race (Black, White, and Hispanic), a dummy for birth county availability, and NLSY cohort fixed effect. Additionally, for the specifications in columns (2) and (6), we add the year of survey and birth cohort fixed effects. In columns (3) and (7), we also add county fixed effects. Finally, for the results in columns (4) and (8), the specifications control county time-varying characteristics by including poverty rate and unemployment rate. *p<.1, **p<.05, ***p<.01

more bottles per day). We demonstrate the effects of charter school exposure on binge drinking in Table 2.10 - 2.17. We show the first stage estimates are in Table 2.A2 in Appendix L. The are strong, statistically significant at 1% across all specifications, and have F-statistics above 180.

From the full-sample results in Table 2.10, we find that charter school exposure has small local average treatment effects on binge drinking. In some specifications, we obtained inconsistent signs (i.e., positive effects instead of negative). Specifications that exclude the county of birth fixed effects reveal that each additional year of charter school exposure reduces binge drinking by about 0.4 percentage points (i.e., 2.4%) and is statistically significant at 1%. Our regression results from the charter school coverage show that a 1-point increase in exposure decreases binge drinking by approximately 0.08 percentage points (i.e., 0.4%), but only statistically significant at 10%.

Table 2.7. Second Stage Estimates for Females - Effects of Charter School Exposure on Four-Year College Graduation among Females using Exposure at the County of Birth as an Instrument

	Years of Charter School Exposure in County of Residence [0,12]				Proportion of Students in Charter School in County of Residence [0,1]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years [0,12]	0.002	0.001	0.008	0.009				
	(0.003)	(0.003)	(0.006)	(0.007)				
Coverage [0,1]					0.063	0.108	0.186*	0.178*
					(0.083)	(0.085)	(0.099)	(0.101)
Observations	6,469	6,469	6,469	6,469	6,469	6,469	6,469	6,469

Notes: Each estimate comes from a separate regression. The regressions include race (Black, White, and Hispanic), a dummy for birth county availability, and NLSY cohort fixed effect. Additionally, for the specifications in columns (2) and (6), we add the year of survey and birth cohort fixed effects. In columns (3) and (7), we also add county fixed effects. Finally, for the results in columns (4) and (8), the specifications control county time-varying characteristics by including poverty rate and unemployment rate. *p<.1, **p<.05, ***p<.01

Because the minimum drinking age was 21 during the years that the NLSY measured the outcomes, we restrict our sample to demonstrate the effects of the charter school exposure on those who could drink legally.[21] We find consistently similar results when we use the years of exposure but slightly high estimates in the case of the charter school coverage (see Table 2.11). A 1-point increase in charter school exposure decreases binge drinking by 0.1 percentage points (i.e., 0.6%), but only statistically significant at 5%. Again, by including the county of birth fixed effects in the specifications, we find only imprecise LATE estimates.

The final set of alcohol consumption results demonstrates heterogeneity in the effects of charter schools on binge drinking, analyzed by gender, race, and education.

[21]See more of the National Minimum Drinking Age Act of 1984 from the following link https://www.cdc.gov/alcohol/fact-sheets/minimum-legal-drinking-age.htm.

Table 2.8. Second Stage Estimates for Whites - Effects of Charter School Exposure on Four-Year College Graduation among Whites using Exposure at the County of Birth as an Instrument

	Years of Charter School Exposure in County of Residence [0,12]				Proportion of Students in Charter School in County of Residence [0,1]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years [0,12]	0.009***	0.009***	0.003	0.000				
	(0.003)	(0.003)	(0.006)	(0.006)				
Coverage [0,1]					0.219*	0.172	0.191	0.182
					(0.127)	(0.123)	(0.143)	(0.151)
Observations	6,450	6,450	6,450	6,450	6,450	6,450	6,450	6,450

Notes: Each estimate comes from a separate regression. The regressions include gender (male/female), a dummy for birth county availability, and NLSY cohort fixed effect. Additionally, for the specifications in columns (2) and (6), we add the year of survey and birth cohort fixed effects. In columns (3) and (7), we also add county fixed effects. Finally, for the results in columns (4) and (8), the specifications control county time-varying characteristics by including poverty rate and unemployment rate. *p<.1, **p<.05, ***p<.01

Table 2.9. Second Stage Estimates for Blacks - Effects of Charter School Exposure on Four-Year College Graduation among Blacks and Hispanics using Exposure at the County of Birth as an Instrument

	Years of Charter School Exposure in County of Residence [0,12]				Proportion of Students in Charter School in County of Residence [0,1]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years [0,12]	0.000	-0.001	0.006	0.009*				
	(0.002)	(0.002)	(0.004)	(0.005)				
Coverage [0,1]					0.035	0.059	0.096*	0.101*
					(0.046)	(0.050)	(0.054)	(0.057)
Observations	6,715	6,715	6,715	6,715	6,715	6,715	6,715	6,715

Notes: Each estimate comes from a separate regression. The regressions include gender (male/female), a dummy for Hispanics, a dummy for birth county availability, and NLSY cohort fixed effect. Additionally, for the specifications in columns (2) and (6), we add the year of survey and birth cohort fixed effects. In columns (3) and (7), we also add county fixed effects. Finally, for the results in columns (4) and (8), the specifications control county time-varying characteristics by including poverty rate and unemployment rate. *p<.1, **p<.05, ***p<.01

Table 2.10. Second Stage Estimates for Binge Drinking - Effects of Charter School Exposure on Alcohol Consumption ($\geq$ 5 bottles daily) using Exposure at the County of Birth as an Instrument

	Years of Charter School Exposure in County of Residence [0,12]				Proportion of Students in Charter School in County of Residence [0,1]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years [0,12]	-0.003***	-0.004***	0.002	0.002				
	(0.001)	(0.001)	(0.004)	(0.004)				
Coverage [0,1]					-0.075*	-0.075*	-0.05	-0.046
					(0.044)	(0.044)	(0.051)	(0.054)
Observations	12,482	12,482	12,482	12,482	12,482	12,482	12,482	12,482

Notes: Each estimate comes from a separate regression. The regressions include gender (male/female), dummies for Whites and Blacks, education (high school graduates, some college, and college graduates or better), a dummy for birth county availability, and NLSY cohort fixed effect. Additionally, for the specifications in columns (2) and (6), we add the year of survey and birth cohort fixed effects. In columns (3) and (7), we also add county fixed effects. Finally, for the results in columns (4) and (8), the specifications control county time-varying characteristics by including poverty rate and unemployment rate. *p<.1, **p<.05, ***p<.01

We find statistically significant estimates in some specifications for the female sample (see Table 2.12) but only noisy estimates among males (see Table 2.13). The results for Whites versus Non-Whites show a similar pattern. At least we find two consistent and statistically significant estimates among Whites (see Table 2.14), while the estimates for Non-Whites are just noisy (see Table 2.15). The results are most substantial among those with college degrees or better compared to those without college degrees. A 1-point increase in charter school exposure decreases binge drinking by 0.2 percentage points (i.e., 1.0%) and is statistically significant at 5% even after including the county of birth fixed effects (see Table 2.16). On the other hand, we find that, without county of birth fixed effects, one additional year of charter school exposure reduces binge drinking by 0.6 percentage points (i.e., 3.5%) among individuals without college

Table 2.11. Second Stage Estimates for Binge Drinking - Effects of Charter School Exposure on Alcohol Consumption ($\geq$ 5 bottles per day) among Individuals of Ages 21+ using Exposure at the County of Birth as an Instrument

	Years of Charter School Exposure in County of Residence [0,12]				Proportion of Students in Charter School in County of Residence [0,1]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years [0,12]	-0.003**	-0.004***	0.001	0.001				
	(0.001)	(0.001)	(0.004)	(0.004)				
Coverage [0,1]					-0.101**	-0.101**	-0.078	-0.073
					(0.043)	(0.043)	(0.051)	(0.054)
Observations	11,852	11,852	11,852	11,852	11,852	11,852	11,852	11852

Notes: Mean of outcome = 0.145. Each estimate comes from a separate regression. The regressions include gender (male/female), dummies for Whites and Blacks, education (high school graduates, some college, and college graduates or better), a dummy for birth county availability, and NLSY cohort fixed effect. Additionally, for the specifications in columns (2) and (6), we add the year of survey and birth cohort fixed effects. In columns (3) and (7), we also add county fixed effects. Finally, for the results in columns (4) and (8), the specifications control county time-varying characteristics by including poverty rate and unemployment rate. *p<.1, **p<.05, ***p<.01

Table 2.12. Second Stage Estimates for Binge Drinking - Effects of Charter School Exposure on Alcohol Consumption ($\geq$ 5 bottles per day) among Females using Exposure at the County of Birth as an Instrument

	Years of Charter School Exposure in County of Residence [0,12]				Proportion of Students in Charter School in County of Residence [0,1]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years [0,12]	-0.004***	-0.006***	0.002	0.002				
	(0.002)	(0.002)	(0.007)	(0.007)				
Coverage [0,1]					-0.001	-0.043	-0.037	-0.035
					(0.046)	(0.045)	(0.051)	(0.054)
Observations	6,085	6,085	6,085	6,085	6,085	6,085	6,085	6,085

Notes: Each estimate comes from a separate regression. The regressions include dummies for Whites and Blacks, education (high school graduates, some college, and college graduates or better), a dummy for birth county availability, and NLSY cohort fixed effect. Additionally, for the specifications in columns (2) and (6), we add the year of survey and birth cohort fixed effects. In columns (3) and (7), we also add county fixed effects. Finally, for the results in columns (4) and (8), the specifications control county time-varying characteristics by including poverty rate and unemployment rate. *p<.1, **p<.05, ***p<.01

Table 2.13. Second Stage Estimates for Binge Drinking - Effects of Charter School Exposure on Alcohol Consumption ($\geq$ 5 bottles per day) among Males using Exposure at the County of Birth as an Instrument

	Years of Charter School Exposure in County of Residence [0,12]				Proportion of Students in Charter School in County of Residence [0,1]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years [0,12]	-0.001	-0.002	0.004	0.004				
	(0.001)	(0.001)	(0.004)	(0.004)				
Coverage [0,1]					-0.035	-0.096	-0.049	-0.046
					(0.049)	(0.072)	(0.081)	(0.083)
Observations	6,391	6,391	6,391	6,391	6,391	6,391	6,391	6,391

Notes: Each estimate comes from a separate regression. The regressions include dummies for Whites and Blacks, education (high school graduates, some college, and college graduates or better), a dummy for birth county availability, and NLSY cohort fixed effect. Additionally, for the specifications in columns (2) and (6), we add the year of survey and birth cohort fixed effects. In columns (3) and (7), we also add county fixed effects. Finally, for the results in columns (4) and (8), the specifications control county time-varying characteristics by including poverty rate and unemployment rate. *p<.1, **p<.05, ***p<.01

Table 2.14. Second Stage Estimates for Binge Drinking - Effects of Charter School Exposure on Alcohol Consumption ($\geq$ 5 bottles per day) among Whites using Exposure at the County of Birth as an Instrument

	Years of Charter School Exposure in County of Residence [0,12]				Proportion of Students in Charter School in County of Residence [0,1]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years [0,12]	-0.004***	-0.005***	0.001	0.001				
	(0.001)	(0.002)	(0.005)	(0.005)				
Coverage [0,1]					0.066	-0.034	0.042	0.037
					(0.066)	(0.076)	(0.082)	(0.082)
Observations	6,218	6,218	6,218	6,218	6,218	6,218	6,218	6,218

Notes: Each estimate comes from a separate regression. The regressions include gender (male/female), education (high school graduates, some college, and college graduates or better), a dummy for birth county availability, and NLSY cohort fixed effect. Additionally, for the specifications in columns (2) and (6), we add the year of survey and birth cohort fixed effects. In columns (3) and (7), we also add county fixed effects. Finally, for the results in columns (4) and (8), the specifications control county time-varying characteristics by including poverty rate and unemployment rate. *p<.1, **p<.05, ***p<.01

Table 2.15. Second Stage Estimates for Binge Drinking - Effects of Charter School Exposure on Alcohol Consumption ($\geq$ 5 bottles per day) among Blacks and Hispanics using Exposure at the County of Birth as an Instrument

	Years of Charter School Exposure in County of Residence [0,12]				Proportion of Students in Charter School in County of Residence [0,1]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years [0,12]	-0.000	-0.002	0.009*	0.008				
	(0.002)	(0.002)	(0.005)	(0.005)				
Coverage [0,1]					-0.039	-0.075	-0.04	-0.034
					(0.038)	(0.050)	(0.059)	(0.063)
Observations	6,264	6,264	6,264	6,264	6,264	6,264	6,264	6,264

Notes: Each estimate comes from a separate regression. The regressions include gender (male/female), a dummy for Hispanics, education (high school graduates, some college, and college graduates or better), a dummy for birth county availability, and NLSY cohort fixed effect. Additionally, for the specifications in columns (2) and (6), we add the year of survey and birth cohort fixed effects. In columns (3) and (7), we also add county fixed effects. Finally, for the results in columns (4) and (8), the specifications control county time-varying characteristics by including poverty rate and unemployment rate. *p<.1, **p<.05, ***p<.01

Table 2.16. Second Stage Estimates for Binge Drinking - Effects of Charter School Exposure on Alcohol Consumption ($\geq$ 5 bottles per day) among Individuals with College Degree or Better using Exposure at the County of Birth as an Instrument

	Years of Charter School Exposure in County of Residence [0,12]				Proportion of Students in Charter School in County of Residence [0,1]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years [0,12]	0.002	-0.002	0.009*	0.007				
	(0.002)	(0.002)	(0.005)	(0.005)				
Coverage [0,1]					-0.043	-0.213***	-0.220***	-0.174**
					(0.038)	(0.067)	(0.077)	(0.089)
Observations	3,380	3,380	3,380	3,380	3,380	3,380	3,380	3,380

Notes: Each estimate comes from a separate regression. The regressions include gender (male/female), a dummy for Hispanics, a dummy for birth county availability, and NLSY cohort fixed effect. Additionally, for the specifications in columns (2) and (6), we add the year of survey and birth cohort fixed effects. In columns (3) and (7), we also add county fixed effects. Finally, for the results in columns (4) and (8), the specifications control county time-varying characteristics by including poverty rate and unemployment rate. *p<.1, **p<.05, ***p<.01

Table 2.17. Second Stage Estimates for Binge Drinking - Effects of Charter School Exposure on Alcohol Consumption ($\geq$ 5 bottles per day) among Individuals without College Degrees using Exposure at the County of Birth as an Instrument

	Years of Charter School Exposure in County of Residence [0,12]				Proportion of Students in Charter School in County of Residence [0,1]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years [0,12]	-0.004***	-0.006***	-0.000	-0.000				
	(0.001)	(0.001)	(0.005)	(0.005)				
Coverage [0,1]					-0.014	-0.022	-0.000	-0.001
					(0.044)	(0.051)	(0.059)	(0.062)
Observations	9,102	9,102	9,102	9,102	9,102	9,102	9,102	9,102

Notes: Each estimate comes from a separate regression. The regressions include gender (male/female), a dummy for Hispanics, a dummy for birth county availability, and NLSY cohort fixed effect. Additionally, for the specifications in columns (2) and (6), we add the year of survey and birth cohort fixed effects. In columns (3) and (7), we also add county fixed effects. Finally, for the results in columns (4) and (8), the specifications control county time-varying characteristics by including poverty rate and unemployment rate. *p<.1, **p<.05, ***p<.01

degrees and is statistically significant at 1% (see Table 2.17).

Table 2.18. Second Stage Estimates for Current Smoking - Effects of Charter School Exposure on 30-Day Smoking using Exposure at the County of Birth as an Instrument

	Years of Charter School Exposure in County of Residence [0,12]				Proportion of Students in Charter School in County of Residence [0,1]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years [0,12]	-0.003*	-0.006***	-0.009*	-0.009*				
	(0.002)	(0.002)	(0.005)	(0.005)				
Coverage [0,1]					0.029	-0.045	0.037	0.057
					(0.067)	(0.088)	(0.103)	(0.111)
Observations	11,156	11,156	11,156	11,156	11,156	11,156	11,156	11,156

Notes: Each estimate comes from a separate regression. The regressions include gender (male/female), dummies for Whites and Blacks, education (high school graduates, some college, and college graduates or better), a dummy for birth county availability, and NLSY cohort fixed effect. Additionally, for the specifications in columns (2) and (6), we add the year of survey and birth cohort fixed effects. In columns (3) and (7), we also add county fixed effects. Finally, for the results in columns (4) and (8), the specifications control county and state time-varying characteristics by including poverty rate, unemployment rate, and cigarette prices, taxes, and revenues. *p<.1, **p<.05, ***p<.01

Our second and last outcome that we consider in this study is current cigarette smoking. Cigarette smoking remains the leading cause of preventable death. It is responsible for more than $480,000$ deaths annually.[22] Among adults of ages 18 and above, about 14% of the U.S. population are daily smokers. We analyze past 30-day cigarette smoking behavior since daily smoking is not available in our sample. We organized information on about $11,156$ individuals who responded to the survey questions on their smoking behavior (see Table 2.18). Among these individuals, about 34% are current smokers (see Table 2.1). Our first stage results in Table 2.A3 in Appendix M are strong, statistically significant at 1% across all specifications, and extremely large F-statistics. The final set of tables present results to show how charter school exposure affects future cigarette smoking outcomes.

Table 2.19. Second Stage Estimates for Current Smoking - Effects of Charter School Exposure on 30-Day Smoking among Males using Exposure at the County of Birth as an Instrument

	Years of Charter School Exposure in County of Residence [0,12]				Proportion of Students in Charter School in County of Residence [0,1]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years [0,12]	-0.003	-0.007***	-0.006	-0.005				
	(0.002)	(0.003)	(0.008)	(0.008)				
Coverage [0,1]					0.046	-0.022	0.141	0.161
					(0.096)	(0.139)	(0.122)	(0.130)
Observations	5,846	5,846	5,846	5,846	5,846	5,846	5,846	5,846

Notes: Each estimate comes from a separate regression. The regressions include, dummies for Whites and Blacks, education (high school graduates, some college, and college graduates or better), a dummy for birth county availability, and NLSY cohort fixed effect. Additionally, for the specifications in columns (2) and (6), we add the year of survey and birth cohort fixed effects. In columns (3) and (7), we also add county fixed effects. Finally, for the results in columns (4) and (8), the specifications control county and state time-varying characteristics by including poverty rate, unemployment rate, and cigarette prices, taxes, and revenues. *p<.1, **p<.05, ***p<.01

[22] See more from https://www.cdc.gov/tobacco/campaign/tips/resources/data/cigarette-smoking-in-united-states.html.

Table 2.20. Second Stage Estimates for Current Smoking - Effects of Charter School Exposure on 30-Day Smoking among Females using Exposure at the County of Birth as an Instrument

	Years of Charter School Exposure in County of Residence [0,12]				Proportion of Students in Charter School in County of Residence [0,1]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years [0,12]	-0.003	-0.004	-0.009	-0.009				
	(0.003)	(0.003)	(0.007)	(0.007)				
Coverage [0,1]					0.031	-0.045	-0.063	-0.045
					(0.071)	(0.091)	(0.119)	(0.127)
Observations	5,304	5,304	5,304	5,304	5,304	5,304	5,304	5,304

Notes: Each estimate comes from a separate regression. The regressions include, dummies for Whites and Blacks, education (high school graduates, some college, and college graduates or better), a dummy for birth county availability, and NLSY cohort fixed effect. Additionally, for the specifications in columns (2) and (6), we add the year of survey and birth cohort fixed effects. In columns (3) and (7), we also add county fixed effects. Finally, for the results in columns (4) and (8), the specifications control county and state time-varying characteristics by including poverty rate, unemployment rate, and cigarette prices, taxes, and revenues. *p<.1, **p<.05, ***p<.01

Table 2.21. Second Stage Estimates for Current Smoking - Effects of Charter School Exposure on 30-Day Smoking among Blacks and Hispanics using Exposure at the County of Birth as an Instrument

	Years of Charter School Exposure in County of Residence [0,12]				Proportion of Students in Charter School in County of Residence [0,1]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years [0,12]	-0.002	-0.006**	-0.012*	-0.012*				
	(0.002)	(0.003)	(0.007)	(0.007)				
Coverage [0,1]					0.025	-0.065	0.042	0.065
					(0.066)	(0.094)	(0.106)	(0.115)
Observations	5,500	5,500	5,500	5,500	5,500	5,500	5,500	5,500

Notes: Each estimate comes from a separate regression. The regressions include gender (male/female), education (high school graduates, some college, and college graduates or better), a dummy for birth county availability, and NLSY cohort fixed effect. Additionally, for the specifications in columns (2) and (6), we add the year of survey and birth cohort fixed effects. In columns (3) and (7), we also add county fixed effects. Finally, for the results in columns (4) and (8), the specifications control county and state time-varying characteristics by including poverty rate, unemployment rate, and cigarette prices, taxes, and revenues. *p<.1, **p<.05, ***p<.01

Table 2.22. Second Stage Estimates for Current Smoking - Effects of Charter School Exposure on 30-Day Smoking among Whites using Exposure at the County of Birth as an Instrument

	Years of Charter School Exposure in County of Residence [0,12]				Proportion of Students in Charter School in County of Residence [0,1]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years [0,12]	-0.003	-0.003	-0.006	-0.005				
	(0.002)	(0.003)	(0.007)	(0.008)				
Coverage [0,1]					0.060	-0.005	-0.049	-0.032
					(0.150)	(0.161)	(0.217)	(0.217)
Observations	5,650	5,650	5,650	5,650	5,650	5,650	5,650	5,650

Notes: Each estimate comes from a separate regression. The regressions include gender (male/female), education (high school graduates, some college, and college graduates or better), a dummy for birth county availability, and NLSY cohort fixed effect. Additionally, for the specifications in columns (2) and (6), we add the year of survey and birth cohort fixed effects. In columns (3) and (7), we also add county fixed effects. Finally, for the results in columns (4) and (8), the specifications control county and state time-varying characteristics by including poverty rate, unemployment rate, and cigarette prices, taxes, and revenues. *p<.1, **p<.05, ***p<.01

Table 2.23. Second Stage Estimates for Current Smoking - Effects of Charter School Exposure on 30-Day Smoking among High-Educated Individuals using Exposure at the County of Birth as an Instrument

	Years of Charter School Exposure in County of Residence [0,12]				Proportion of Students in Charter School in County of Residence [0,1]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years [0,12]	-0.003	-0.005*	-0.014**	-0.014**				
	(0.002)	(0.002)	(0.007)	(0.007)				
Coverage [0,1]					-0.076	-0.144**	-0.098	-0.082
					(0.053)	(0.068)	(0.089)	(0.095)
Observations	5,843	5,843	5,843	5,843	5,843	5,843	5,843	5,843

Notes: Mean of outcome is 0.248. Each estimate comes from a separate regression. The regressions include gender (male/female), dummies for Whites and Blacks, dummy for college completion or better, a dummy for birth county availability, and NLSY cohort fixed effect. Additionally, for the specifications in columns (2) and (6), we add the year of survey and birth cohort fixed effects. In columns (3) and (7), we also add county fixed effects. Finally, for the results in columns (4) and (8), the specifications control county and state time-varying characteristics by including poverty rate, unemployment rate, and cigarette prices, taxes, and revenues. *p<.1, **p<.05, ***p<.01

Table 2.24. Second Stage Estimates for Current Smoking - Effects of Charter School Exposure on 30-Day Smoking among Low-Educated Individuals using Exposure at the County of Birth as an Instrument

	Years of Charter School Exposure in County of Residence [0,12]				Proportion of Students in Charter School in County of Residence [0,1]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years [0,12]	-0.003	-0.007***	-0.000	0.000				
	(0.003)	(0.003)	(0.008)	(0.008)				
Coverage [0,1]					0.212	0.121	0.229	0.255
					(0.142)	(0.203)	(0.201)	(0.212)
Observations	5,307	5,307	5,307	5,307	5,307	5,307	5,307	5,307

Notes: Each estimate comes from a separate regression. The regressions include gender (male/female), dummies for Whites and Blacks, dummy for high school graduates, a dummy for birth county availability, and NLSY cohort fixed effect. Additionally, for the specifications in columns (2) and (6), we add the year of survey and birth cohort fixed effects. In columns (3) and (7), we also add county fixed effects. Finally, for the results in columns (4) and (8), the specifications control county and state time-varying characteristics by including poverty rate, unemployment rate, and cigarette prices, taxes, and tax revenues. *p<.1, **p<.05, ***p<.01

From our full-sample estimates in Table 2.18, we find that charter school exposure has small local average treatment effects on current smoking. We find that every additional year of charter school exposure decreases current cigarette smoking by 1.8 and 2.6% at the mean with and without 1% and 10%, respectively. Because cigarette taxes could fund charter schools, we include state-level cigarette prices, taxes, and revenues as additional time-varying controls, aside from county-level poverty and unemployment rates. However, our estimate does not differ from that of the model in which we include county-fixed effects. It suggests that cigarette tax revenues do not correlate with charter school law and opening. We find similar estimates among males but noisy estimates among females (see Table 2.19 and 2.20). However, we find twice and robust evidence among Blacks and Hispanics, as reported in Table 2.21, but no effect among Whites (see Table 2.22). Even though all the estimates among Whites have correct signs, as expected, they are very imprecise. By considering heterogeneity by education, we find that charter school exposure effectively reduces cigarette smoking among the most educated individuals. From our preferred specification, we find that one additional year of exposure decreases cigarette smoking by 1.4 percentage point among individuals with some college or better education and is approximately 4.1% at the mean, with statistical significance at 5% (see Table 2.23). We only find half of this estimate among those with high school diploma or lower but only statistically significant in the specification that does not control for location fixed effects and time-varying area-level characteristics (see Table 2.24).

2.7 Discussion and Conclusion

States adopted charter school laws starting from 1991, and by 2003, more than 40 states had opened charter schools. Charter schools are public educational institutions that operate without state and local school boards' interference over specified years. Since states opened charter schools, several studies have analyzed their impacts on students, predominantly contemporaneous schooling outcomes such as test scores and high school completion. However, there are gaps in the literature, and other important questions remain unanswered. First, there is no consensus on the direction of their impacts since studies find negative, null, and positive results from different states. Second, no study has used national data to estimate charter school effects on students' outcomes. Finally, a few of the available articles have assessed their impacts on students' long-term outcomes. This study fills the gap by using national data to estimate the effects of charter schools on students' later-in-life education and health behaviors.

We use student-level data from the National Longitudinal Survey of Youth (NLSY) by combining the children of the 1979 cohort with the youth from the 1997 cohort. Some of the children of the NLSY79 cohort in the Child and Young Adult Survey were born when states passed the laws and opened charter schools. Also, the NLSY97 cohorts born from 1980 - 1984 were in elementary and high schools when several states opened charter schools. We obtained county geocoded, restricted students' information from the Bureau of Labor Statistics. Because the NLSY excluded students' information on the type of school they attended until 2003, we organized charter school information from the National Center for Education Statistics Common Core Data (NCES-CDD), which provides information on all K-12 schools. We supplement NCES-CDD data with charter school information from two other sources. First, we contacted and obtained data from the state's department of education. Second, we use charter school opening and closure information from the U.S. Department of Education National Charter School Resource Center since some states did not respond to our request or have the data available. We link students from the NLSY samples to the universe of elementary and high schools at the county level, which is the lowest geographic identifier in the NLSY.

Because we cannot identify students who attended charter schools in the NLSY, we calculate two charter school exposure measures at the county-level. First, we calculate the number of years that students were potentially exposed to charter schools. Since the years of exposure do not have information on their chances of getting seat assignments, we also calculate a second measure, the intensity and coverage of charter schools (i.e., number of charter school students in the relevant population). We calculated the weighted probability of charter school coverage for every student in the sample.

Our study uses instrumental variable (IV) strategy, which addresses two key methodological concerns. First, regressing the outcomes on the charter school exposure, calculated at the county of residents may bias the estimates due to endogenous migration. Because school choice is an individual decision, rather than random assignment, students could move around counties in response to charter school openings. Second, attenuation bias becomes another challenge when we calculate the charter school exposure at the county of birth rather than the county of residence. We address these concerns by using the county of birth exposure as an instrument for the exposure at the counties resided throughout elementary and high school years in the IV framework.

The findings of this study are as follows. First, we show that charter school exposure affects four-year college completion. Our local average treatment effect (LATE) estimate is that an additional year of charter school exposure increases college graduation by 3%. Given that students in the sample were exposed to charter schools for three years on average, we think the charter schools have substantial and desirable effects on students' long-term education outcomes. It also suggests that students with more years of exposure have significant chances of completing four-year degree programs than those born in the same county without any exposure. In terms of the charter school coverage, we find that a 1-point increase in exposure increases the probability of completing a four-year college by 0.4% among those induced by the instrument. Again, our estimate implies that students in counties with more charter schools have higher chances of completing a four-year college than those born in the same county but with fewer charter schools. We also demonstrate that the effects are large for Blacks and Hispanics than Whites and females than males.

Aside from education outcomes, we also consider two health behaviors. Excessive alcohol consumption (or binge drinking) and cigarette smoking are the outcomes we analyze. Our results suggest that charter school exposure reduces binge drinking. We find that a 1-point increase in exposure decreases binge drinking by 0.4% and 0.6% among those in the legal drinking age of 21 and above. We also find some evidence of heterogeneity by the level of education. A 1-point increase in exposure decreases binge drinking by 1% among individuals with four-year degrees or better. We do not find any statistically significant evidence among those without college degrees. While we do not find robust evidence from the full sample on cigarette smoking, we show some evidence from our sub-sample analysis. We find that every additional year of charter school exposure decreases cigarette smoking by about 3.6% among Blacks and Hispanics and 4.1% among individuals with more than twelve years of education.

This study has several strengths. We highlight a few as follows. First, we are the first to estimate both the direct and indirect effects of charter schools using exposure. With several studies focusing on actual attendance, they ignore the spillover effects of charter schools. By following individuals in the NLSY over time, our charter school exposure credibly estimates the spillover effects. Second, no study has used national data to characterize the impacts of charter schools on students' outcomes to the best of our knowledge. Our analysis uses data that includes students born in over 500 counties and currently live in over 44 states. Third, we demonstrate the long-term effects of charter schools by including individuals in the mid-careers. About 70% of our sample is 30 years or above, enabling us to estimate charter schools' long-term impacts. Finally, we show evidence of the positive effects of charter schools on adverse health behaviors, which the literature has ignored.

Two limitations to our analysis are as follows. First, we do not know the actual county of birth for individuals born from 1980-84. We only know the counties they resided at their 12^{th} birthday. Although we can identify their locations before states passed the laws, they could move earlier in anticipation of the policy and opening of charter schools. Second, our study covers about 25% of all counties and has a small sample size, making it computationally difficult to find statistically significant effects.

Overall, our results demonstrate that charter schools positively affect long-term education and health behaviors. Therefore, we recommend that states and local school boards should allow more charter schools to operate.

Chapter 3

Revisiting the Effects of Economic Conditions on Cigarette Smoking

3.1 Introduction

Tobacco use is still the leading cause of preventable death, claiming about 7 million lives worldwide annually, with only cigarette smoking responsible for 480,000 deaths per year in the United States. Additionally, about 16 million Americans have smoking-caused diseases currently.[1] Although the long-run trend in the prevalence of current (i.e., past 30-day) cigarette smoking among adults shows a steady decline from 42.4% in 1965 to 13.7% in 2018, over 34 million Americans were smokers in 2018.[2] Moreover, the associated economic costs of cigarette smoking are over $300 billion a year.[3]

A possible contributing factor to the steady decline in smoking prevalence is changing macroeconomic conditions. During the Great Recession, occurring from December 2007 to June 2009, and the COVID-19 pandemic season, which started in February 2020 in the U.S., leading to lockdown and social distancing policies, several individuals and households were impacted, which could affect their lifestyles. The daily and current smoking increased in several heavy-smoking countries during the Great Recession. Data show spikes in the current smoking in the Great Recession period.[4] Gallus et al. (2015) estimate that the number of current smokers in the U.S. increased by ap-

[1] See more from https://www.cdc.gov/tobacco/data_statistics/fact_sheets/fast_facts/index.htm.

[2] See from https://www.lung.org/research/trends-in-lung-disease/tobacco-trends-brief/overall-tobacco-trends.

[3] See more from https://www.cdc.gov/tobacco/data_statistics/fact_sheets/fast_facts/index.htm

[4] See more from https://www.cdc.gov/mmwr/preview/mmwrhtml/mm6444a2.htm.

proximately 0.6 million during the Great Recession period.[5] In addition to the long-lasting and high-intensive downturn, its aftermath was so devastating that recovery was slowest in history (Boen and Yang, 2016; Currie et al., 2015) and output and unemployment rates in the U.S. returned to their normal levels after several months (Cunningham, 2018).[6] It takes longer time for lifestyles to return to normal after a severe macroeconomy shock.

Lifestyle changes caused by macroeconomic fluctuations occur because victims suffer from involuntary job loss, longer unemployment spells, and stress from constant job searches, affecting their mental and physical health (Golden and Perreira, 2015; Catalano et al., 2011). During downturns, individuals experiencing financial loss, psychological stress, and physical strain might engage in unhealthy lifestyles, including smoking and excessive alcohol consumption, to mitigate the stress and its related consequences (Charles and DeCicca, 2008; Catalano and Dooley, 1983; Catalano, 1991). Besides stress, unemployed individuals spend more time at social gatherings during bad economic times, increasing their chances of engaging in risky behaviors. However, one plausible argument for a decrease in cigarette smoking is an income effect that shifts household budgets inwards during downturns. Nevertheless, in periods when involuntary unemployment becomes more prevalent, affecting most low-income households, government cash transfers, including unemployment insurance and stimulus packages, and automatic stabilizers, such as tax reduction, offset income loss.

[5]During the Great Recession, the U.S. economy was affected to the extent that the unemployment rate increased and peaked at 10 percent by October 2009. Recently, the COVID-19 pandemic affected the U.S. macroeconomy, such that the unemployment rate rose to about 15%, the highest since 1932. See from the following link https://www.bls.gov/opub/ted/2020/unemployment-rate-rises-to-record-high-14-point-7-percent-in-april-2020.htm?view_full.

[6]In the U.S., the unemployment rate remained at higher levels for several months before returning to its usual long-run trend. By 2012, the unemployment rate was still as high as 8 percent. One out of ten people in the labor market could not find a job, and involuntary unemployment formed a more significant proportion of the unemployment rate during the Great Recessionary period (Golden and Perreira, 2015; Theodossiou and Hipple, 2011). The U.S. unemployment duration between 2000 and 2020 was longer during the Great Recession and the COVID-19 season. That is, the fraction of people who were unemployed for at least 27 weeks and over increased drastically and remained high during and after the Great Recession (see https://www.bls.gov/charts/employment-situation/duration-of-unemployment.htm).

Studying the effects of macroeconomic conditions on cigarette smoking started back in the early 2000s. Yet, the evidence is inconclusive. Using data from the U.S., some studies find that smoking prevalence falls as the economy contracts (Ruhm, 2000, 2005; Xu, 2013). However, Goel (2008) finds that income and unemployment do not significantly affect cigarette smoking. Other studies find contrasting evidence of cigarette smoking increasing during bad economic times (Kalousova and Burgard, 2014; Barnes et al., 2009; Dehejia and Lleras-Muney, 2004). Besides, there is also evidence of heterogeneity in the economic conditions and cigarette smoking relationship in the U.S. context. Currie et al. (2015) finds that high-educated women are more likely to smoke during economic downturns. Charles and DeCicca (2008) demonstrate that cigarette smoking increases among minorities and less-educated individuals least likely to be employed but decreases for those with higher chances of getting jobs. While Falba et al. (2005) find that high-smoking levels persist even after re-employment, Golden and Perreira (2015) show that the effect becomes highest after re-employment and reverses when out of the labor market.[7]

Similar to the findings from the U.S., the effects are also inconclusive across different countries. De Vogli and Santinello (2005) find that higher unemployment is associated with a higher risk of cigarette smoking and stress using data from Italy. Montgomery et al. (1998) demonstrate that young British men who are unemployed are more likely to engage in life-long patterns of dangerous behaviors. Similarly, Novo et al. (2000) followed young men and women from Sweden during a recession and boom and found that unemployment is associated with lower daily smoking levels. McClure et al. (2012) show that the risk of cigarette smoking reduced among males whose income fell

[7]The most recent working paper, Peng et al. (2020), also finds some evidence of counter-cyclicality in cigarette smoking, suggesting that it increases during downturns. However, the study only uses MSA-level data, which may not provide evidence for the U.S. population.

in Iceland shortly after the Great Recession. Jung et al. (2013) demonstrate that job loss in Korea during the Great Recession was associated with a higher probability of becoming heavy smokers and job losers were more likely to smoke than their counterparts who remained employed. In China, Wang et al. (2016) find that an increase in the unemployment rate increases cigarette smoking. Finally, Kaiser et al. (2017) use German Socio-Economic Panel data to find that the propensity of becoming a smoker increase in downturns, but conditional on being a smoker, the number of cigarettes smoked decreases in recessions.

Although several studies have examined the effects of economic conditions on cigarette smoking behavior, the evidence is far from the conclusions. This study contributes to the literature in several ways. First, we use long-run data from 1987-2019, identifying the dynamics in smoking behavior. The over three decades of data allow us to include information on several significant macroeconomic shocks, including the September 11, 2001, terrorist attack, and the Great Recession. Including data from these dates when shocks heavily impacted the U.S. macroeconomy and worldwide can provide more insight into how lifestyle changes in such periods and help plan for similar future occurrences. The Ruhm (2005) study that we follow closely used data up to 2000. However, the introduction of electronic cigarettes without taxes after 2000 that are substitutes for regular cigarettes and cigars might make cigarettes more responsive to macroeconomic conditions. Additionally, the Food and Drug's Board ban on flavoring in cigarettes in 2009, other than menthol, might also affect the relationship we study. Using data covering several periods capturing these supply-side changes in product availability and tobacco control policies might help demonstrate how the effect has changed over time. Another important reason for studying the relationship between economic conditions and cigarette smoking using more recent data is differences in the marginal smokers likely to be affected by macroeconomic conditions. Because smoking intensities in the 1990s were higher than 2000s and 2010, it is crucial to study how sensitive the two groups were to economic conditions. Importantly, people from the 1990s who were most likely to be high-intensive smokers might be less susceptible to prices and other factors likely affected by economic conditions.

We use the 1987–2019 Behavioral Risk Factor Surveillance System (BRFSS) and employment data from the Bureau of Labor Statistics. We first replicate the results in Ruhm (2005), which uses the 1987–2000 BRFSS data. The results from our replication exercise are similar to Ruhm's findings. Using civilian employment rate as a measure of economic condition, Ruhm finds that a one-point increase in employment rate increases current smoking by 0.6%, while we find 0.7%. The author also estimated the effects of economic conditions on two categories of heavy smoking. He considered those who smoke more than 20 cigarettes a day and found an estimate of 0.9%. We estimate the effect to be 1%. Finally, he also demonstrated the cigarette smoking effects of employment among extremely heavy smokers who consume 40 or more sticks of cigarettes per day. Even though his estimate was not different from those who smoke 20 or more sticks of cigarettes daily, we estimate a slightly higher effect. We find a 1.2%. We attribute the difference in the estimates to the disparities in his and our sample. Our data for the replication is 212 above the 1,490,249 individuals in Ruhm's sample.

We extend the analysis for current smoking to include data from the 1987–2019. However, since the survey discontinued asking questions about the intensity of cigarette smoking per day, we cannot consider the heavy and extremely heavy smoking outcomes. We find that a one-point increase in employment rate raises current cigarette smoking by 0.4%, a slightly lower estimate. However, a sub-sample analysis provides more insights into our new estimate. We show evidence of attenuation bias toward a null effect and demonstrate that the older cohorts drive the current study results. We also demonstrate a drastic temporal decline in employment and smoking's procyclical relationship during the Great Recession period. Our final analysis considers heterogeneity by different demographic groups. We only find differential effects such that the positive impact is larger among males and low-educated individuals.

The rest of the paper is organized as follows. In Section 3.2, we describe the method used for this study. We present the estimates from the replication exercise in Section 3.3, while we devote Section 3.4 to our results. Section 3.5 concludes and provides some policy recommendations.

3.2 Methods

3.2.1 Data

Our data on cigarette smoking come from the 1987–2019 Behavioral Risk Factor Surveillance System (BRFSS), an annual telephone survey of the adult population administered by the Center for Disease Control and Prevention. The BRFSS, which started in 1984, collects information on health-related risk behaviors, chronic health conditions, and the use of preventive services from all 50 states and the District of Columbia. It consists of a repeated cross-section of randomly selected individuals but does not track them over time.

Information on individuals' smoking habits is available, allowing us to define binary variables for daily and current smoking outcomes. Before 1996, the BRFSS questionnaire included only one question asking for the number of cigarettes smoked per day. On the other hand, starting from 1996, they expanded the questionnaire to ask only those who smoke daily to indicate the number of cigarettes smoked every day. We classify a respondent as a current smoker if he smokes daily or some days. The BRFSS data also includes individual-level characteristics, which we control in the regressions. Specifically, we include the age, sex, race/ethnicity, education, and marital status of respondents in all regressions.

The second source of data comes from the Bureau of Labor Statistics (BLS) Local Area Unemployment Statistics (LAUS).[8] Every month, the BLS publishes state-level, including the District of Columbia, information on the number of adults (age 16 and above) employed, unemployed, and in the labor force. We calculate the percentage of those in the labor force employed in each month for each state.

3.2.2 Econometric Model

The individual smokes a positive amount of cigarettes when his unobserved latent utility from smoking is above a certain threshold and zero cigarettes when below. Because we only observe his behavior in the data, but not the latent utility, we model his observed cigarette smoking outcome in a binary choice model as follows:

$$Y_{ijmt} = \mathbf{1}(\beta + \lambda Emp_{mjt} + \mathbf{\Pi} \boldsymbol{X_{ijmt}} + \boldsymbol{\gamma_j} + \boldsymbol{\tau_m} + \boldsymbol{\delta_t} + \xi_{ijmt} > 0). \tag{3.1}$$

In equation (1), $\mathbf{1}(\bullet)$ is the indicator function taking the value one if its argument is true and zero if false, Y_{ijmt} represents a smoking outcome for individual i, living in

[8] The website, http://stats.bls.gov/lau/home.htm, provides information on state-level employment and unemployment rates.

state j, and interviewed in calendar month m of survey year t. The variable X_{ijmt} represents its corresponding vector of individual-level characteristics, Emp_{mjt} denotes employment rate, which is our measure of economic conditions, and ξ_{imjt} represents the disturbance term, indicating the effects of all unobserved and random factors that affect smoking. In the original study, the author estimated the effects of employment on cigarette use at different intensities by categorizing it into current smoking and smoking ≥ 20 and ≥ 40 sticks per day.

The vector of parameters $\boldsymbol{\gamma_j}$ removes time-invariant state-level characteristics correlated with both economic conditions and changes in cigarette smoking. Because smoking behaviors also depend on weather, seasons, and events, we also include $\boldsymbol{\tau_m}$ as the calendar-month fixed effects. In this case, our analysis compares individuals surveyed in the same calendar month. Even after including the calendar-month fixed effects, other fiscal year characteristics might be similar across states. For example, the 2009 Great Recession uniformly affected the entire U.S. economy. Therefore, we include a vector of survey-year fixed effects, $\boldsymbol{\delta_t}$, to remove such impacts. By including state, calendar month, and survey year fixed effects, we cannot add month-by-year dummies since they are perfectly collinear with Emp_{mjt}. Since Y_{ijmt} is a binary variable, we estimate probit models and report their marginal effects for interpretations.

Following Ruhm (2005), we use the average percent of the civilian non-institutionalized state population (aged 16 and over) employed during the three months ending with the survey month called "employment rate" as the primary measure of economic condition. The coefficient of interest in equation (1) is λ. Its estimate, $\hat{\lambda}$, measures the impact of employment rate, Emp_{mjt}, on the outcome, Y_{ijmt}. Identifying the parameter λ comes from the fact that individuals are affected by exogenous macroeconomic conditions, determining employment levels in the state of residence, affecting smoking behaviors.

3.2.3 Ordered Probit Estimator

In addition to the probit models used in Ruhm (2005), we also use an ordered probit estimator to simultaneously analyze the three smoking outcomes. Because current cigarette smoking and smoking more than ≥ 20 and ≥ 40 sticks per day are all defined from the same survey question "On the average, about how many cigarettes a day do you now smoke?" are mutually exclusive, we model the responses together as a discrete ranked-ordered variable. We do not observe the latent utility y^*_{ijmt}, but only the smoking outcome y_{ijmt} such that

$$y_{ijmt} \begin{cases} 0 & \text{if} \quad y^*_{ijmt} \leq \kappa_1 \\ 1 & \text{if} \quad \kappa_1 < y^*_{ijmt} \leq \kappa_2 \\ 2 & \text{if} \quad \kappa_2 < y^*_{ijmt} \leq \kappa_3 \\ 3 & \text{if} \quad y^*_{ijmt} \geq \kappa_3 \end{cases} \tag{3.2}$$

where κ_1, κ_2, and κ_3 are constants that represent the cutoff points. Also, the outcome categories are defined as follows; 0 represents non-smoking, 1 denotes smoking up to 19 cigarettes per day, 2 is for consuming 20 or more cigarettes per day up to 29, and 3 represents 40 or more cigarettes per day. An ordered probit regression of y_{ijmt} on the employment rate and the control variables estimates the probabilities in each of the categories 0-3 as follows:

$$\Pr(Y_{ijmt} = 0) = \mathbf{\Phi}(\kappa_1 - \beta - \lambda Emp_{mjt} - \mathbf{\Pi} \boldsymbol{X_{ijmt}} - \boldsymbol{\gamma_j} - \boldsymbol{\tau_m} - \boldsymbol{\delta_t} - \xi_{ijmt}) \tag{3.3}$$

$$
\begin{aligned}
\Pr(Y_{ijmt} = s) =& \boldsymbol{\Phi}(\kappa_s - \beta - \lambda Emp_{mjt} - \boldsymbol{\Pi X_{ijmt}} - \boldsymbol{\gamma_j} - \boldsymbol{\tau_m} - \boldsymbol{\delta_t} - \xi_{ijmt}) \\
& - \boldsymbol{\Phi}(\kappa_{s-1} - \beta - \lambda Emp_{mjt} - \boldsymbol{\Pi X_{ijmt}} - \boldsymbol{\gamma_j} - \boldsymbol{\tau_m} - \boldsymbol{\delta_t} - \xi_{ijmt}) \qquad (3.4) \\
& \forall s \in (1, 2)
\end{aligned}
$$

$$
\Pr(Y_{ijmt} = 3) = 1 - \boldsymbol{\Phi}(\kappa_3 - \beta - \lambda Emp_{mjt} - \boldsymbol{\Pi X_{ijmt}} - \boldsymbol{\gamma_j} - \boldsymbol{\tau_m} - \boldsymbol{\delta_t} - \xi_{ijmt}) \qquad (3.5)
$$

where $\boldsymbol{\Phi}$ represents a cumulative standard normal distribution function, and the parameter of interest is λ. Since the probability functions are not linear, we compute the marginal effect of the estimate for interpretation rather than the coefficient itself. In each equation, the parameter estimate $\hat{\lambda}$ represents the effect of a one-point increase in the employment rate on the probability of choosing the outcome.

3.3 Replication of the Results in Ruhm (2005)

We first show the results from the replication exercise and focus on only tobacco use outcomes.[9] Table 3.1 shows the author's summary statistics reported in the first two columns and our replication in the last two columns. While the author's final data consists of 1,490,249 individuals, our sample has 212 additional people. For the three outcomes we consider, the means are approximately equal, irrespective of being weighted or not. We find only one significant difference between our unweighted sample averages and those in the original paper for the explanatory variables. While the author reported that 49.3% of his sample were females, we find approximately 58.5% instead. To verify that our reported statistic for the percent of females in the data is accurate, we also compute the summaries for each survey year. Though we do not report them in the paper, our observation is that the percentage of females in the BRFSS survey data

[9] Ruhm (2005) considered body weight and physical activity as additional outcomes.

ranges from approximately 57–59% each year, suggesting the average from the pooled sample cannot be below.

Table 3.2 replicates the regression estimates of the tobacco outcomes. We first present the original results from Ruhm (2005) in Panel A, while Panel B shows our replicated estimates. For each outcome, the first column displays its weighted mean. The second column of Table 3.2 shows the predicted effect of a one-point increase in employment rate on the percentage point change in the probability of smoking evaluated at the sample means of the explanatory variable reported in Table 3.1. Each row corresponds to one regression estimate with two standard errors beneath it. A robust-standard error calculated assuming that observations are independent across months and states but not within states each month are reported in parentheses. Its corresponding robust-standard errors, assuming independence across but not within states, are shown in brackets.

Table 3.1. Replicated Means of Ruhm (2005) Study Sample

	Ruhm Sample Means		Replicated Sample Means	
	Unweighted Means	Weighted Means	Unweighted Means	Weighted Means
Smoking Outcomes				
Current smoking	23.4%	23.4%	23.4%	23.3%
Smokes $\geq$ 20 cigarettes per day	11.6%	11.4%	11.6%	11.4%
Smokes $\geq$ 40 cigarettes per day	1.7%	1.7%	1.7%	1.7%
Age (in years)	46.3	44.3	46.3	44.3
Female (%)	49.3%	52%	58.5%	52%
Race/ethnicity				
Non-Hispanic Black	8.4%	9.4%	8.4%	9.4%
Other non-Hispanic non-White	3.8%	3.6%	3.9%	3.6%
Hispanic origin	5.5%	9.2%	5.5%	9.1%

Continued on the next page

Table 3.1 – Continued from the previous page

	Ruhm (2005) Sample		Replicated Sample	
	Unweighted Means	Weighted Means	Unweighted Means	Weighted Means
White only	82.3%	77.8%	82.3%	77.9%
Education				
High school dropout	14.2%	15.1%	14.2%	15.1%
High School Graduate	33.2%	33%	33.5%	33.6%
Some college	26.1%	25.8%	25.6%	25.2%
College graduate	26.3%	25.9%	26.5%	25.9%
Education not reported	0.2%	0.2%	0.2%	0.2%
Current marital status				
Never married	17.1%	19.1%	17.1%	19.1%
Married/cohabiting	56.9%	62.5%	56.9%	62.5%
Divorced/separated	14.9%	10.9%	14.9%	10.9%
Widowed	10.9%	7.3%	10.9%	7.3%
Marital status not reported	0.2%	0.2%	0.2%	0.2%
State-level variables				
% Employed	64.1%	62.9%	64.1%	62.9%

The results in Panel B of Table 3.2 show that our sample generates quantitatively and qualitatively similar estimates reported by the author. While Ruhm (2005) found that a one-point increase in state-level employment rate increases current cigarette smoking by 0.13 percentage points, we find 0.16 percentage points. The author also estimated the effects for daily smoking ≥ 20 and ≥ 40 sticks of cigarettes to be 0.10 and 0.02 percentage points, respectively. Our corresponding estimates are 0.11 and 0.02 percentage points. The last two columns of Table 3.2 show estimates regarding the percentage change in the outcomes. The third column estimates are calculated by dividing the marginal effects in the second column by the sample averages in the first column. For those in the last column, we re-estimate the model and compute the average

Table 3.2. Replicated Predicted effect of a one-point increase in the percent employed on lifestyle behaviors for Tobacco use in Ruhm (2005)

	Sample Mean	Predicted Effect	Percent Change	
			a	b
Panel A: Original Results of Ruhm (2005)				
Current smoking	0.2336	0.1317 (0.0287) [0.0489]	0.6	0.6
Smokes $\geq$ 20 cigarettes daily	0.1144	0.1044 (0.0194) [0.0349]	0.9	1.0
Smokes $\geq$ 40 cigarettes daily	0.0174	0.0155 (0.0055) [0.0065]	0.9	1.4
Panel B: Replicated Results in Ruhm (2005)				
Current smoking	0.2338	0.1586 (0.0328) [0.0453]	0.7	0.7
Smokes $\geq$ 20 cigarettes daily	0.1156	0.111 (0.0227) [0.0350]	1.0	1.0
Smokes $\geq$ 40 cigarettes daily	0.0173	0.0202 (0.0074) [0.0081]	1.2	1.5

Note: This table is the replicated results of Table 2 in Ruhm (2005). Panel A corresponds to the original results from Ruhm (2005), whiles the replicated results are in Panel B. The table shows the predicted effects of a one-point increase in the state percentage of the population employed from binary probit models using data from BRFSS 1987–2000. The dependent variable means were calculated by incorporating sampling weights. The probit models also include month, year, and state dummy variables and controls for age, sex, race/ethnicity, education, and marital status. Sample size is 1,490,461. Predicted effects indicate the estimated percentage point change in the dependent variable, with other regressors evaluated at the sample means. Robust standard errors calculated assuming that observations are independent across months and states but not within states in a given month are reported in parentheses. Corresponding standard errors that assume independence across but not within states are shown in brackets. Percentage changes are computed by dividing the predicted effect by the dependent variable mean. In the third column, predicted effects are evaluated at the regressor means. In the fourth, these are calculated for each individual and then averaged across all sample members.

marginal effects using respondent values reported for each of the independent variables and average across all sample members before dividing it by the means in the first column.

Regardless of how we compute the marginal effects, we find 0.7% for the current smoking outcome, while the author found 0.6%. For the daily cigarette smoking ≥ 20 sticks of cigarettes, the author found 0.9% and 1.0%, depending on how he computed the marginal effect, but our replication leads to 1.0% in both cases. On the last outcome, smoking ≥ 40 sticks of cigarettes daily, the author found 0.9%, while we find 1.2% when we calculate the marginal effect at the means of the control variables. The corresponding estimate when we evaluate the marginal effect at the control variables' actual values for each person and average it across all individuals in the sample, we find a 1.5%. In contrast, the original paper estimated it to be 1.4%. Therefore, our results are similar to those we replicate, suggesting that our estimates from the extension will be comparable.

We also show results for the three smoking outcomes when we use an ordered probit estimator, described in Section 3.2. We cannot show ordered probit results in the extension because the survey discontinued asking questions about smoking intensities after 2000. Table 3.3 presents the estimates from the ordered probit models. The sample shows that 77% were non-smokers, 12% smoked at least 19 cigarettes per day, 10% consumed 20-39 sticks of cigarettes daily, while the remaining 2% smoked 40 or more cigarettes every day. Our results from the ordered probit models are that a one-point increase in employment rate decreases the probability of being a non-smoker by 0.2% and increases the chances of smoking 1-19, 20-39, and 40+ sticks of cigarettes by 0.4-0.5%, 0.7%, and 0.9-1.2%, respectively. These estimates provide better information than those presented in Ruhm (2005) since he combined smokers and non-smokers as a comparable group when estimating at different intensities.

Table 3.3. Ordered Probit Estimates: Predicted effect of a one-point increase in the percent employed on lifestyle behaviors for Tobacco use in Ruhm (2005)

	Sample Mean	Predicted Effect	Percent Change	
			a	b
Pr(Smokes = 0)	0.7662	-0.1460 (0.0314) [0.0441]	-0.2	-0.2
Pr(0 < Smokes < 20)	0.1182	0.0587 (0.0126) [0.0177]	0.5	0.4
Pr(20 ≤ Smokes < 40)	0.0984	0.0715 (0.0154) [0.0216]	0.7	0.7
Pr(Smokes ≥ 40)	0.0173	0.0158 (0.0034) [0.0048]	0.9	1.2

Note: The table shows the predicted effects of a one-point increase in the state percentage of the population employed from binary probit models using data from BRFSS 1987–2000. The ordered probit models also include month, year, and state dummy variables and controls for age, sex, race/ethnicity, education, and marital status. Sample size is 1,490,461. Predicted effects indicate the estimated percentage point change in the dependent variable, with other regressors evaluated at the sample means. Robust standard errors calculated assuming that observations are independent across months and states but not within states in a given month are reported in parentheses. Corresponding standard errors that assume independence across but not within states are shown in brackets. Percentage changes are computed by dividing the predicted effect by the dependent variable mean. In the third column, predicted effects are evaluated at the regressor means. In the fourth, these are calculated for each individual and then averaged across all sample members.

Table 3.4. Sample means (standard deviations in parenthesis) using BRFSS 1987–2019 (aged 18+)

	Unweighted means	Weighted means
Current smoking	0.177	0.201
	(0.382)	(0.401)
Age (in years)	52.880	45.734
	(17.567)	(17.764)
Female	0.592	0.516
	(0.492)	(0.500)
Race/ethnicity		
Non-Hispanic Black	0.087	0.114
	(0.281)	(0.318)
Other non-Hispanic non-White	0.067	0.068
	(0.250)	(0.251)
Hispanic	0.057	0.113
	(0.231)	(0.316)
White only	0.789	0.706
	(0.408)	(0.456)
Education		
High school dropout	0.098	0.137
	(0.297)	(0.344)
High school graduate	0.300	0.305
	(0.448)	(0.461)
Some college	0.268	0.274
	(0.443)	(0.446)
College graduate	0.331	0.281
	(0.471)	(0.449)
Education not reported	0.003	0.004
	(0.057)	(0.059)
Current marital status		
Never married	0.148	0.206
	(0.355)	(0.404)
Married/cohabiting	0.564	0.604
	(0.496)	(0.489)
Divorced/separated	0.158	0.117
	(0.365)	(0.322)
Widowed	0.125	0.069
	(0.331)	(0.253)
Marital status not reported	0.005	0.004
	(0.067)	(0.060)
State-level variables		
% Employed	0.621	0.615
	(0.045)	(0.038)
Observations	8,950,116	8,950,116

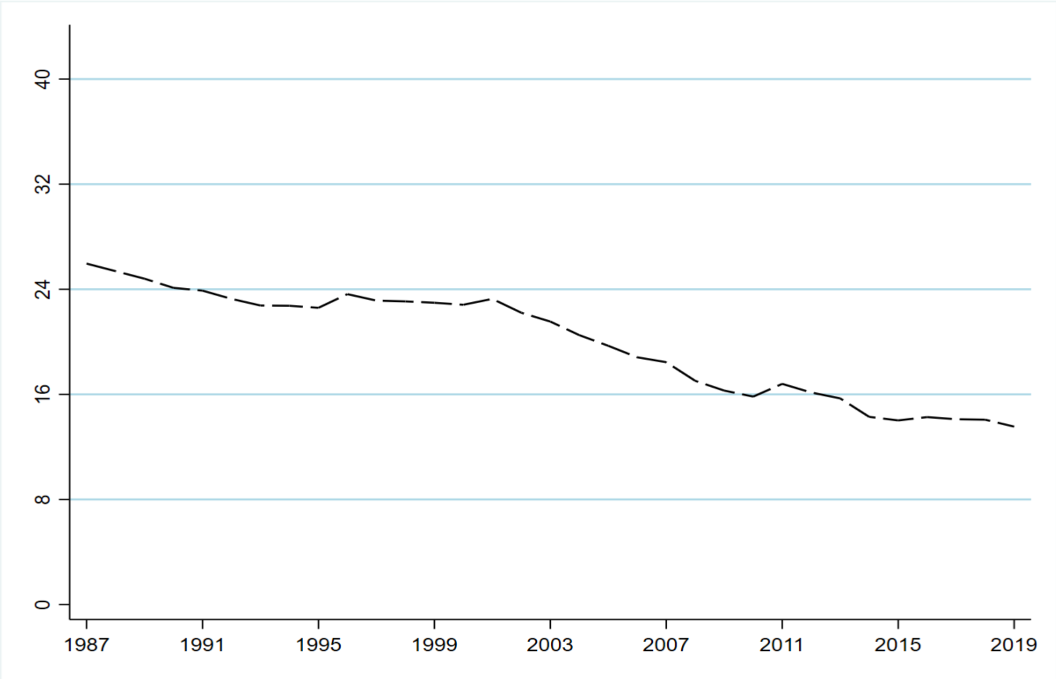


Figure 3.1. Prevalence of Current Cigarette Smoking among Adults 1987-2019 BRFSS

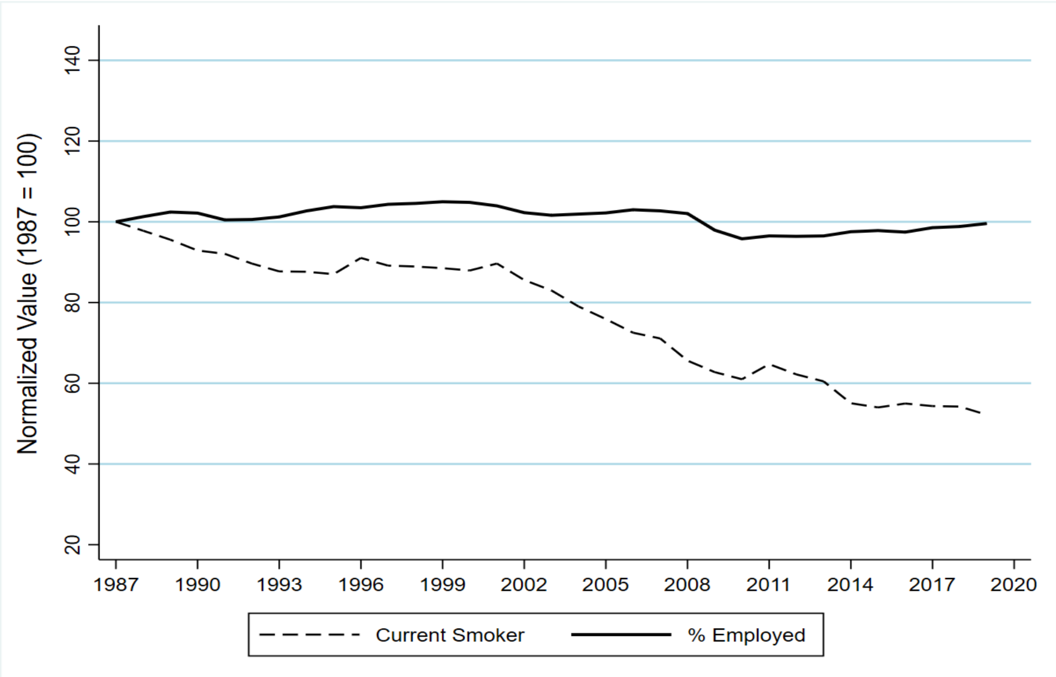


Figure 3.2. Trends in normalized current cigarette smoking and employment rate (1987-2019)

3.4 Main Results

We extend the analysis to include the cohorts surveyed from 2001–2019. We begin by describing the full sample data. In addition to the data we use for the replication exercise, we organize 7,459,655 respondents surveyed from 2001–2019. Therefore, the sample used for the final analysis increased to 8,950,116. Table 3.4 reports the summary statistics of the new sample. The statistics in the first column do not account for sampling weights, while those in the second column are weighted. Our new sample confirms that the prevalence of current smoking has declined drastically from about 25% in 1987 to 14% in 2019 (see Figure 3.1), which is approximately 48% decrease (see from Figure 3.2). On average, current smokers are about 18% of our full sample. Using sampling weights increases it to 20%.

The individuals in the recent BRFSS cohorts are relatively older than those in the earlier surveys since the average age in the full sample is 53 years, compared to 46 years in the data used in Ruhm (2005). The proportion of non-Whites non-Hispanic race also increased, while that of Whites declined. On average, approximately 6.7 percent of the entire sample is non-White non-Hispanic. Over the years, the proportion of high school dropouts has fallen in the BRFSS survey, while college graduates have increased. About 14 percent were high school dropouts in the older cohorts, but only 10 percent do not have high school diplomas in our new sample. Compared to the individuals in the data used in Ruhm (2005), our new data contains a relatively smaller proportion of people who have never married, declining from 17.1 to 14.8%.

Our measure of economic conditions, the percent of state civilian non-institutionalized monthly employment (or simply “employment rate”), is relatively smaller in the newer sample compared to the those used in Ruhm (2005). The average employment rate from 1987–2000 was 64.1% but has declined to 62.1% after including the data from 2000–2019. It is not surprising because of the 2001 and 2009 recessions. During these periods, the employment rates fell drastically. Figure 3.2 shows trends in employment rate and current smoking, plotting annual averages of employment rates and current smoking, normalized to 100 in 1987 values. The period from 1987-2000 mimics the graph presented in Ruhm (2005). While the employment rate rose mildly and was reasonably stable during this period, the percent of current smoking fell slowly, as shown in Figure 3.2. After 2000, current smoking fell drastically even though the employment rate continued its usual trend. The sharp decline in current smoking relative to the employment rate suggests that the findings in Ruhm (2005) might not be the same in the newer data. In the rest of the study, we demonstrate how the relationship between economic conditions and cigarette smoking has changed over time.

Table 3.5 shows the regression results from our full sample. In Panel A, we estimate the models without sample weights, while Panel B shows the results for the case where sample weights are included. The models also include calendar month, year, and state fixed effects. Robust standard errors, calculated assuming that observations are independent across months and states but not within states and each month, are reported in parentheses. Their corresponding standard errors that assume independence across but not within states are shown in brackets. From the unweighted regressions, we find that a one-point increase in employment rate on current cigarette smoking is 0.076 percentage points and is statistically significant at 1%. This estimate is lower than those reported in Ruhm (2005) and our replication results, which use data from 1987–2000. When we use the sample weights, the estimate decreases to 0.030 and is very imprecise.

Table 3.5. Percentage change in current smoking due to a one-point change in the employment rate using BRFSS 1987–2019 (aged 18+)

	Sample Mean	Predicted Effect	Percent Change	
			a	b
Panel A: Predicted effect without sampling weights				
Current smoking	0.1774	0.0763 (0.0145) [0.0358]	0.4	0.4
Panel B: Predicted effects with sampling weight				
Current smoking	0.2012	0.0300 (0.0299) [0.0380]	0.2	0.2

Note: The table shows the predicted effects of a one-point increase in the state percentage of the population employed, from binary probit models using data from BRFSS 1987–2019. The probit models also include month, year and state dummy variables and controls for age, sex, race/ethnicity, education, and marital status. Sample size is 8,950,116. Robust standard errors calculated assuming that observations are independent across months and states but not within states in each month, are reported in parentheses. Corresponding standard errors that assume independence across but not within states are shown in brackets. Predicted effects indicate the estimated percentage point change in the dependent variable, with other regressors evaluated at the sample means. Percentage changes are computed by dividing the predicted effect by the dependent variable mean. In the third column, predicted effects are evaluated at the regressor means. In the fourth, these are calculated for each individual and then averaged across all sample members.

The columns marked "a" and "b" show the percent change in current smoking when predicted effects are evaluated at the sample means of the regressors or calculated for each and then averaged across all sample members, respectively. Regardless of how we calculate the marginal effects, we find that the impact of a one-point increase in the employment rate on current cigarette smoking is 0.4%. As discussed in

Section 3.3, our estimate from the replication exercise that uses individuals from the older cohort surveyed from 1987-2000 is 0.7%, while Ruhm (2005) found an estimate of 0.6%. The drastic decline in the positive relationship between economic conditions and cigarette smoking suggests attenuation bias toward zero effect. Despite the fact that sample weights are necessary for the estimations to ensure our results are externally valid, we find a weighted least squares estimate of 0.2%, which is relatively smaller and statistically insignificant.

3.4.1 Dynamic Effects

We demonstrate more evidence on the attenuation bias towards a null relationship between economic conditions and cigarette smoking by allowing the effect to vary over time (see Sun and Abraham (2020) for more discussion). We interact employment rate with a full set of year dummies as specified below:

$$Y_{ijmt} = \mathbf{1}(\beta + \sum_{t=1987}^{2019} \lambda_t YEAR_t \times Emp_{mjt} + \boldsymbol{\Phi X_{ijmt}} + \boldsymbol{\gamma_j} + \boldsymbol{\tau_m} + \boldsymbol{\delta_t} + \xi_{ijmt} > 0), \quad (3.6)$$

where $YEAR_t$ represents a dummy for year t, and the rest of the variables follow their usual definitions in equation (1). Additionally, the parameters of interest are λ_t, for $t = 1987, 1988, ..., 2019$. By including the interaction between employment rate and a full set of year dummies, we omit E_{mjt} from the model to avoid a perfect collinearity.

Because there are 31 coefficients of interests from equation (2), we summarize them by providing their margins plot, with their 95% confidence intervals, in Figure 3.3. The black dots represent the marginal effect estimates for all the years, while the lines denote the 95% confidence intervals. The plots confirm that the impact of a one-point increase in employment rate on current smoking attenuates over time. The figure provides three insights. First, the effects of economic conditions on cigarette smoking have downward trends and are expected to switch from procyclical to a countercyclical relationship, especially after 2019. Since the estimate from 2019 shows approximately zero effect, we anticipate a countercyclical estimate in 2020. Second, there was a significant temporal drastic decline in the impact during the Great Recession. They reversed to their normal trajectory after recovery from the recession. Our last revelation, implied from the first and second insights, is that we should expect another sharp decline in the estimate during the coronavirus pandemic in 2020 that worsened the U.S. macroeconomic conditions. Because the coronavirus pandemic's economic impacts are similar to that of the Great Recession, we expect a similar sharp decline again.

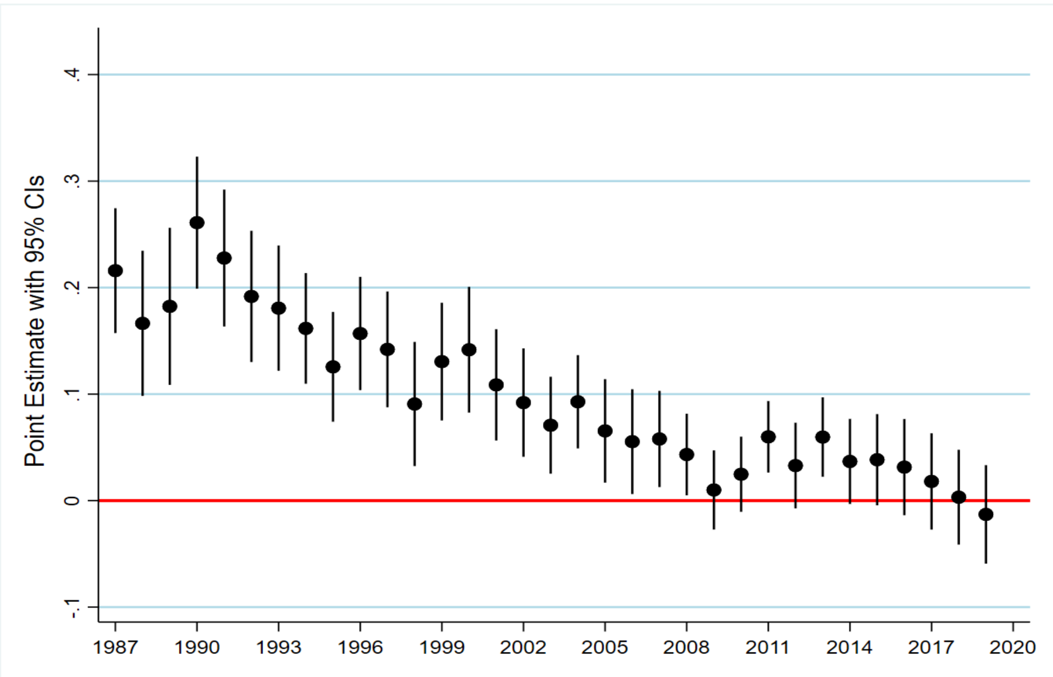


Figure 3.3. Dynamic Estimates - Effects of a one point increase in employment rate on current cigarette smoking, with their 95% confidence intervals (1987-2019)

The attenuation bias toward zero estimates in the smoking-employment relationship shown in Figure 3.3 suggests some underlying unobserved structural or behavioral changes are driving the continuous decline. Figure 3.2 shows that current cigarette smoking decreased drastically by about 40 percentage points from 2001–2019, but the employment rate changed only marginally, creating an impression of a structural break. Therefore, it is interesting to re-estimate the models with the newer cohorts. Table 3.6 shows the results for the analysis that uses only 2001–2019 survey data. Our estimates are negative in signs, contrary to our findings from the full sample. We find that the predicted effect of a one-point increase in the employment rate does not affect current cigarette smoking. The estimate is small and statistically insignificant, regardless of whether the samples are weighted or standard errors are clustered with assumptions. Also, the event-study graph in Figure 3.4 gives a consistent picture of the trends in the effects of economic conditions on cigarette smoking. In contrast, when we use data from 1987–2000, we find slightly decreasing impacts over time, but with significant dips in 1988–1989 and 1998 (see Figure 3.5).

3.4.2 Robustness Checks

One concern is whether state tobacco control policies can explain the estimates in Table 3.5 and the structural change in the relationship between cigarette smoking and economic conditions demonstrated in Figure 3.3. In recent years, policymakers have enacted several regulations to reduce cigarette smoking, including federal, state, and local cigarette excise taxes, youth access policies, flavor bans, and other tobacco control policies (Farrelly et al., 2017), expenditure (Huang and Chaloupka, 2014; Ciecierski et al., 2011), and State Medicaid smoking cessation programs (Greene et al., 2014). Since the models include the year, state, and calendar month fixed effects, we do not think tobacco control policies are the underlying cause of the structural break in cur-

Table 3.6. Percentage change in current smoking due to a one-point change in the employment rate using BRFSS 2001–2019 (aged 18+)

	Sample Mean	Predicted Effect	Percent Change	
			a	b
Panel A: Predicted effect without sampling weights				
Current smoking	0.1662	-0.0117 (0.0152) [0.0288]	-0.1	-0.1
Panel B: Predicted effects with sampling weight				
Current smoking	0.1829	-0.0057 (0.0312) [0.0343]	-0.0	-0.0

Note: The table shows the predicted effects of a one-point increase in the state percentage of the population employed, from binary probit models using data from BRFSS 2001–2019. The probit models also include month, year and state dummy variables and controls for age, sex, race/ethnicity, education, and marital status. Sample size is 7,459,306. Robust standard errors calculated assuming that observations are independent across months and states but not within states in each month, are reported in parentheses. Corresponding standard errors that assume independence across but not within states are shown in brackets. Predicted effects indicate the estimated percentage point change in the dependent variable, with other regressors evaluated at the sample means. Percentage changes are computed by dividing the predicted effect by the dependent variable mean. In the third column, predicted effects are evaluated at the regressor means. In the fourth, these are calculated for each individual and then averaged across all sample members.

rent cigarette smoking trends unless they vary within states over time. However, if state tobacco control policies change over time, then our states and year fixed effects in the models would not account for them. As the first robustness check, we include state-level cigarette sales taxes in the models. Cigarette excise taxes are shown to be a significant predictor of smoking and correlate with economic conditions (Charles and DeCicca, 2008). We use cigarette sales tax data from the Center for Disease Control

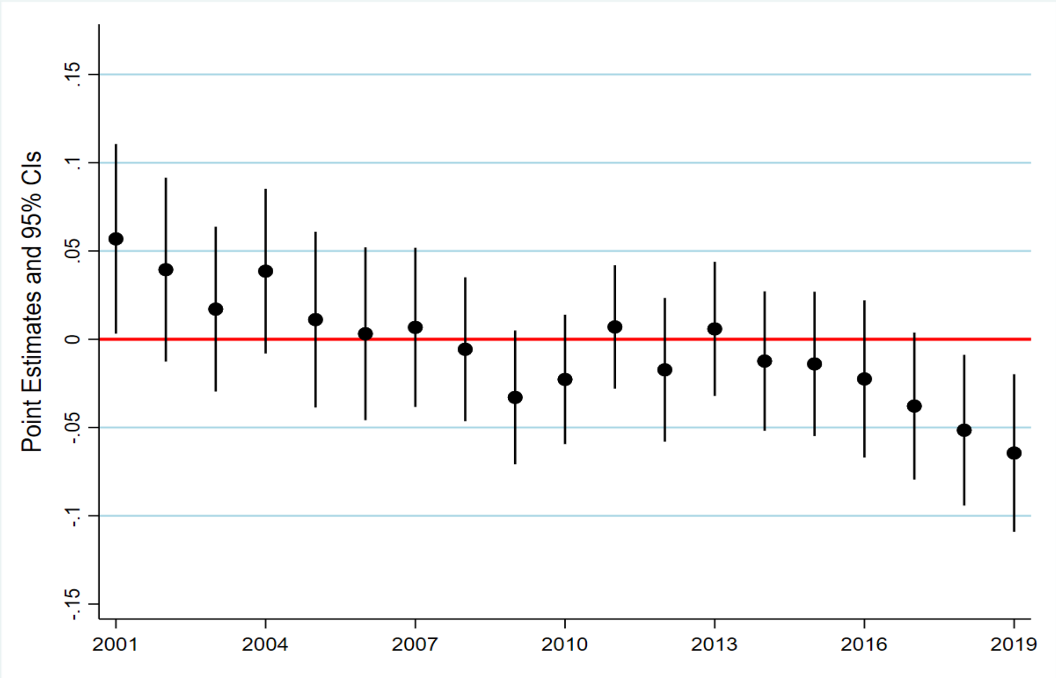


Figure 3.4. Dynamic Estimates - Effects of a one point increase in employment rate on current cigarette smoking, with their 95% confidence intervals (2001-2019)

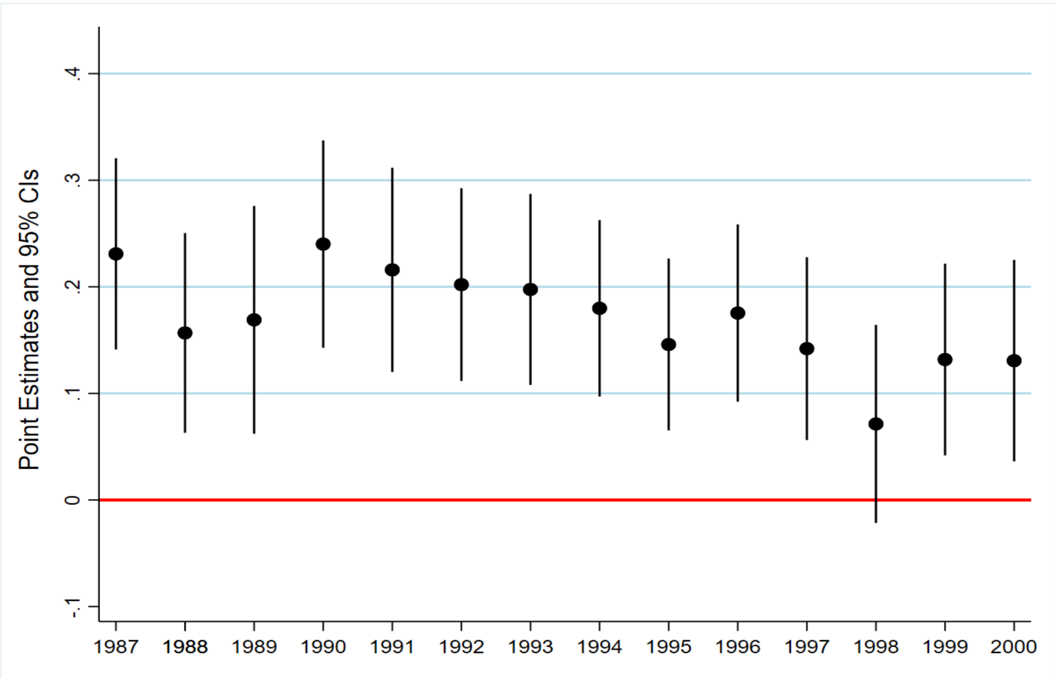


Figure 3.5. Dynamic Estimates - Effects of a one point increase in employment rate on current cigarette smoking, with their 95% confidence intervals (1987-2000)

and Prevention (Orzechowski and Walker, 2019). Table 3.7 re-estimates the models in equation (1) with state cigarette taxes. The results show that the increase in cigarette smoking prevalence due to a one-point increase in employment rate does not differ significantly with cigarette sales taxes in the models. While there is a slight increase in the estimate when we exclude sample weights in the models, the weight least squares estimates are not different from those in Table 3.5. Therefore, tobacco control policies seem not to explain the structural change in the effects.

Since it is reasonable to expect that immediate past economic conditions affect current cigarette smoking, the second set of robustness checks uses previous economic conditions in the specifications. We use the average three months employment rate, the average two-year employment rate (both ending in the survey month), and the previous month's employment rate in separate regressions. Table 3.8, 3.9, and 3.10, show the results from these specifications, respectively. Except for the case where we use the lagged employment rate (see Table 3.10), the estimates are consistently similar to the main results in Table 3.5. Even the weighted least squares estimates are sometimes statistically significant at 5%, depending on how we calculate the standard errors. If we use the lagged employment rate, the estimate is still positive and statistically significant at convention levels but economically insignificant. It is not surprising because we do not expect the previous month's economic conditions to have a similar effect as that of the current month. Importantly, since the estimate is consistently positive and statistically significant, it supports the findings of the procyclical relationship between economic conditions and cigarette smoking.

It could be possible that changes in macroeconomic conditions have differential impacts on individuals with different levels of demographic characteristics. In an-

Table 3.7. Percentage change in current smoking due to a one-point change in the employment rate using BRFSS 1987–2019 (aged 18+) with cigarette taxes

	Sample Mean	Predicted Effect	Percent Change	
			a	b
Panel A: Predicted effect without sampling weights				
Current smoking	0.1774	0.0991 (0.0137) [0.0320]	0.6	0.6
Panel B: Predicted effects with sampling weight				
Current smoking	0.2012	0.0306 (0.0290) [0.0378]	0.2	0.2

Note: The table shows the predicted effects of a one-point increase in the state percentage of the population employed from binary probit models using BRFSS 1987–2019. The probit models also include month, year, and state dummy variables and controls for age, sex, race/ethnicity, education, marital status, and state cigarette taxes. Robust standard errors calculated assuming that observations are independent across months and states but not within states each month are reported in parentheses. Corresponding standard errors that assume independence across but not within states are shown in brackets. Predicted effects indicate the estimated percentage point change in the dependent variable, with other regressors evaluated at the sample means. Percentage changes are computed by dividing the predicted effect by the dependent variable mean. In the third column, predicted effects are evaluated at the regressor means. In the fourth, these are calculated for everyone and then averaged across all sample members.

other set of robustness checks, we consider specifications that include interactions between age and sex (one variable), age and race/ethnicity (three variables), sex and race/ethnicity (three variables), sex and marital status (three variables), and sex and education (three variables). Table 3.11 presents the results from these specifications. The estimates compared to the main results in Table 3.5 are not different in any respect.

Table 3.8. Percentage change in current smoking due to a one-point change in the employment rate using BRFSS 1987–2019 (aged 18+) using average employment over three months

	Sample Mean	Predicted Effect	Percent Change	
			a	b
Panel A: Predicted effect without sampling weights				
Current smoking	0.1774	0.1140 (0.0227) [0.0582]	0.6	0.6
Panel B: Predicted effects with sampling weight				
Current smoking	0.2012	0.0856 (0.0423) [0.0668]	0.4	0.4

Note: The table shows the predicted effects of a one-point increase in the state percentage of the population employed from binary probit models for females using data from BRFSS 1987–2019. Sample size is 8,950,116. Different employment definitions are used for alternative specifications. The probit models also include month, year, and state dummy variables and controls for age, sex, gender, race/ethnicity, education, and marital status. Robust standard errors calculated assuming that observations are independent across months and states but not within states each month are reported in parentheses. Corresponding standard errors that assume independence across but not within states are shown in brackets. Predicted effects indicate the estimated percentage point change in the dependent variable, with other regressors evaluated at the sample means. Percentage changes are computed by dividing the predicted effect by the dependent variable mean. In the third column, predicted effects are evaluated at the regressor means. In the fourth, these are calculated for everyone and then averaged across all sample members.

In my next robustness checks, we include state-specific linear time trends in the models as a way of accounting for unobserved factors that vary within-states and across time. For example, social norms, natural disasters, and state-specific tobacco control policies can change over time. Table Table 3.12 shows the results from these specifications. Doing so does not significantly affect the predicted macroeconomic effects on

Table 3.9. Percentage change in current smoking due to a one-point change in the employment rate using BRFSS 1987–2019 (aged 18+) using average employment over 2 years

	Sample Mean	Predicted Effect	Percent Change	
			a	b
Panel A: Predicted effect without sampling weights				
Current smoking	0.1774	0.1152 (0.0175) [0.0435]	0.6	0.6
Panel B: Predicted effects with sampling weight				
Current smoking	0.2012	0.0696 (0.0344) [0.0516]	0.3	0.3

Note: The table shows the predicted effects of a one-point increase in the state percentage of the population employed from binary probit models for females using data from BRFSS 1987–2019. Sample size is 8,950,116. Different employment definitions are used for alternative specifications. The probit models also include month, year, and state dummy variables and controls for age, sex, gender, race/ethnicity, education, and marital status. Robust standard errors calculated assuming that observations are independent across months and states but not within states each month are reported in parentheses. Corresponding standard errors that assume independence across but not within states are shown in brackets. Predicted effects indicate the estimated percentage point change in the dependent variable, with other regressors evaluated at the sample means. Percentage changes are computed by dividing the predicted effect by the dependent variable mean. In the third column, predicted effects are evaluated at the regressor means. In the fourth, these are calculated for everyone and then averaged across all sample members.

current cigarette smoking. Even after controlling for the state, month, and year fixed effects and the trend absorbing some of the variations in the employment rates, we still find robust estimates.

One possible omitted bias to the main results Table 3.5 is our failure to control for household incomes, which correlates with macroeconomic conditions and affects

Table 3.10. Percentage change in current smoking due to a one-point change in the lagged employment rate using BRFSS 1987–2019 (aged 18+)

	Sample Mean	Predicted Effect	Percent Change	
			a	b
Panel A: Predicted effect without sampling weights				
Current smoking	0.1774	0.0008 (0.0001) [0.0004]	0.005	0.005
Panel B: Predicted effects with sampling weight				
Current smoking	0.2012	0.0004 (0.0003) [0.0004]	0.002	0.002

Note: The table shows the predicted effects of a one-point increase in the state percentage of the population employed from binary probit models for females using data from BRFSS 1987–2019. Sample size is 8,950,116. Different employment definitions are used for alternative specifications. The probit models also include month, year, and state dummy variables and controls for age, sex, gender, race/ethnicity, education, and marital status. Robust standard errors calculated assuming that observations are independent across months and states but not within states each month are reported in parentheses. Corresponding standard errors that assume independence across but not within states are shown in brackets. Predicted effects indicate the estimated percentage point change in the dependent variable, with other regressors evaluated at the sample means. Percentage changes are computed by dividing the predicted effect by the dependent variable mean. In the third column, predicted effects are evaluated at the regressor means. In the fourth, these are calculated for everyone and then averaged across all sample members.

cigarette smoking. As the final set of robustness checks, we include a measure of household income in the models. Before 1994, BRFSS categorized household income information into < US$ 10,000, US$ 10,000–14,999, US$ 15,000–19,999, . . . , and US$ 50,000+. Starting from 1994, they included US$ 50,000 – 74,999 and 75,000+ as additional categories. Since unobserved factors that affect cigarette smoking could also determine income, creating endogeneity concerns, we use the weighted averages for BRFSS residents

Table 3.11. Percentage change in current smoking due to a one-point change in the employment rate using BRFSS 1987–2019 (aged 18+) with additional controls

	Sample Mean	Predicted Effect	Percent Change	
			a	b
Panel A: Predicted effect without sampling weights				
Current smoking	0.1774	0.0834 (0.0143) [0.0342]	0.5	0.5
Panel B: Predicted effects with sampling weight				
Current smoking	0.2012	0.0291 (0.0284) [0.0320]	0.1	0.1

Note: The table shows the predicted effects of a one-point increase in the state percentage of the population employed, from binary probit models using data from BRFSS 1987–2019. The probit models also include month, year and state dummy variables and controls for age, sex, race/ethnicity, education, marital status, and interactions between age and sex (one variable), age and race/ethnicity (three variables), sex and race/ethnicity (three variables), sex and marital status (three variables), and sex and education (three variables). Sample size is 8,950,116. Robust standard errors calculated assuming that observations are independent across months and states but not within states in each month, are reported in parentheses. Corresponding standard errors that assume independence across but not within states are shown in brackets. Predicted effects indicate the estimated percentage point change in the dependent variable, with other regressors evaluated at the sample means. Percentage changes are computed by dividing the predicted effect by the dependent variable mean. In the third column, predicted effects are evaluated at the regressor means. In the fourth, these are calculated for each individual and then averaged across all sample members.

in the state with the same sex, age, and education, converted to 2019 constant dollars using the all-items consumer price index. Within each state, we categorize the real average incomes for the 16 groups stratified by sex (male versus female), age (18–34, 25–54, 55–64, 65 and over), and education (no college versus college graduate). By including household incomes in the models (see Table 3.13), the results do not differ

Table 3.12. Percentage change in current smoking due to a one-point change in the employment rate using BRFSS 1987–2019 (aged 18+) with state-specific linear time trends

	Sample Mean	Predicted Effect	Percent Change	
			a	b
Panel A: Predicted effect without sampling weights				
Current smoking	0.1774	0.1302 (0.0096) [0.0224]	0.7	0.7
Panel B: Predicted effects with sampling weight				
Current smoking	0.2012	0.0696 (0.0252) [0.0289]	0.3	0.3

Note: The table shows the predicted effects of a one-point increase in the state percentage of the population employed, from binary probit models using data from BRFSS 1987–2019. The probit models also include linear time trend, month and state dummy variables, and controls for age, sex, race/ethnicity, education, and marital status. Sample size is 8,950,116. Robust standard errors calculated assuming that observations are independent across months and states but not within states in each month, are reported in parentheses. Corresponding standard errors that assume independence across but not within states are shown in brackets. Predicted effects indicate the estimated percentage point change in the dependent variable, with other regressors evaluated at the sample means. Percentage changes are computed by dividing the predicted effect by the dependent variable mean. In the third column, predicted effects are evaluated at the regressor means. In the fourth, these are calculated for each individual and then averaged across all sample members.

from the main estimates in Table 3.5, providing strong evidence of the procyclical relationship between macroeconomic economic conditions and cigarette smoking. Also, although we do not report it in the table, we find that household income and cigarette smoking positively correlate.

Table 3.13. Percentage change in current smoking due to a one-point change in the employment rate using BRFSS 1987–2019 (aged 18+) with household income

	Sample Mean	Predicted Effect	Percent Change	
			a	b
Panel A: Predicted effect without sampling weights				
Current smoking	0.1774	0.0678 (0.0139) [0.0328]	0.4	0.4
Panel B: Predicted effects with sampling weight				
Current smoking	0.2012	0.0198 (0.0284) [0.0335]	0.1	0.1

Note: The table shows the predicted effects of a one-point increase in the state percentage of the population employed, from binary probit models using data from BRFSS 1987–2019. The probit models also include linear time trend, month and state dummy variables, and controls for age, sex, race/ethnicity, education, marital status, and state–age–sex–education group average household incomes. Sample size is 8,950,116. Robust standard errors calculated assuming that observations are independent across months and states but not within states in each month, are reported in parentheses. Corresponding standard errors that assume independence across but not within states are shown in brackets. Predicted effects indicate the estimated percentage point change in the dependent variable, with other regressors evaluated at the sample means. Percentage changes are computed by dividing the predicted effect by the dependent variable mean. In the third column, predicted effects are evaluated at the regressor means. In the fourth, these are calculated for each individual and then averaged across all sample members.

3.4.3 Heterogeneity

Since the data may not be nationally representative, the external validity of the results is a possible concern. As already pointed out, the BRFSS oversamples females. Therefore, the results could be disproportionately driven by females. To verify whether the results are heterogeneous across different demographic groups, we present sub-sample analyses by gender, age, race, and education. For the analysis by gender, we stratify the sample into only males and females. Table 3.14 shows the results for males, while those of the females are in Table 3.15. Regardless of whether the data is weighted or not, males are at least 2.6% more likely to be smokers than females. Focusing on the estimates from the unweighted regressions, we find that the predicted effect of a one-point increase in employment rate raises current cigarette smoking by 0.6 for males and 0.3 for females and is statistically significant at 1%. With sample weights in the models, the estimates are less precise and smaller. Therefore, economic conditions possibly affect cigarette smoking for males more than females.

Next, we present the results by age categories in Table 3.16 - 3.18. Even though the sample means differ significantly across different age groups, there are no disparities between their weighted and unweighted means. Also, smoking behavior does not vary so much between young adults aged 18–34 (see Table 3.16) and adults aged 35–64 (see Table 3.17). As expected, smoking prevalence among those above 64 years is lower than in the younger generations (see Table 3.18). Focusing on the estimates from the unweighted least squares, we find that the effect of a one-point increase in employment on current cigarette smoking does not differ among the three age groups. A one-point increase in employment rate increases cigarette smoking by about 0.5% if weighted or unweighted, except the weighted least squares results among individuals of ages 65+. The effect among those of ages 65+ when weighted is negative but statistically insignificant.

Another interesting demographic category that we consider is race. We study the effect of economic conditions on cigarette smoking among Whites, Blacks, Hispanics, and all other racial groups combined. The unweighted means suggest that smoking

Table 3.14. Heterogeneous Effect Among Males – Percentage change in current smoking due to a one-point change in the employment rate using BRFSS 1987–2019 (aged 18+)

	Sample Mean	Predicted Effect	Percent Change	
			a	b
Panel A: Predicted effect without sampling weights				
Current smoking	0.1922	0.1195 (0.0180) [0.0308]	0.6	0.6
Panel B: Predicted effects with sampling weight				
Current smoking	0.2222	0.0535 (0.0399) [0.0459]	0.2	0.2

Note: The table shows the predicted effects of a one-point increase in the state percentage of the population employed, from binary probit models for females using data from BRFSS 1987–2019. The probit models also include month, year and state dummy variables and controls for age, sex, race/ethnicity, education, and marital status. The sample size for the males is 3,653,696. Robust standard errors calculated assuming that observations are independent across months and states but not within states in each month, are reported in parentheses. Corresponding standard errors that assume independence across but not within states are shown in brackets. Predicted effects indicate the estimated percentage point change in the dependent variable, with other regressors evaluated at the sample means. Percentage changes are computed by dividing the predicted effect by the dependent variable mean. In the third column, predicted effects are evaluated at the regressor means. In the fourth, these are calculated for each individual and then averaged across all sample members.

prevalence is higher among Blacks and the other racial groups than Whites and Hispanics (see from Table 3.19 - 3.22). Our results show that a one-point increase in employment rate increases current smoking by 0.4% among Whites (see Table 3.19), 0.6% among the other racial groups (see Table 3.22), but imprecise among Blacks (see Table 3.20) and Hispanics (see Table 3.21) when we exclude sample weights in the re-

Table 3.15. Heterogeneous Effect Among Females – Percentage change in current smoking due to a one-point change in the employment rate using BRFSS 1987–2019 (aged 18+)

	Sample Mean	Predicted Effect	Percent Change	
			a	b
Panel A: Predicted effect without sampling weights				
Current smoking	0.1673	0.0456 (0.0175) [0.0432]	0.3	0.3
Panel B: Predicted effects with sampling weight				
Current smoking	0.1815	0.0089 (0.0359) [0.0458]	0.1	0.1

Note: The table shows the predicted effects of a one-point increase in the state percentage of the population employed, from binary probit models for females using data from BRFSS 1987–2019. The probit models also include month, year and state dummy variables and controls for age, sex, race/ethnicity, education, and marital status. The sample size for the males is 5,296,420. Robust standard errors calculated assuming that observations are independent across months and states but not within states in each month, are reported in parentheses. Corresponding standard errors that assume independence across but not within states are shown in brackets. Predicted effects indicate the estimated percentage point change in the dependent variable, with other regressors evaluated at the sample means. Percentage changes are computed by dividing the predicted effect by the dependent variable mean. In the third column, predicted effects are evaluated at the regressor means. In the fourth, these are calculated for each individual and then averaged across all sample members.

gressions. Including sampling weights in the estimation leads to different results. The estimates are that a one-point increase in employment rate increases current cigarette smoking by 0.8% among Blacks and at least 1.1% among the other racial groups, but we do not find any economic and statistically significant impacts among Whites and Hispanics.

Table 3.16. Heterogeneous Effect among Adults of Ages 18-34 – Percentage change in current smoking due to a one-point change in the employment rate using BRFSS 1987–2019 (aged 18+)

	Sample Mean	Predicted Effect	Percent Change	
			a	b
Panel A: Predicted effect without sampling weights				
Current smoking	0.2313	0.1145 (0.0346) [0.0819]	0.5	0.5
Panel B: Predicted effects with sampling weight				
Current smoking	0.2333	0.1100 (0.0828) [0.0940]	0.5	0.5

The table shows the predicted effects of a one-point increase in the state percentage of the population employed from binary probit models for females using data from BRFSS 1987–2019. The probit models also include month, year, and state dummy variables and controls for age, sex, gender, race/ethnicity, education, and marital status. The sample size is 1,637,595. Robust standard errors calculated assuming that observations are independent across months and states but not within states each month are reported in parentheses. Corresponding standard errors that assume independence across but not within states are shown in brackets. Predicted effects indicate the estimated percentage point change in the dependent variable, with other regressors evaluated at the sample means. Percentage changes are computed by dividing the predicted effect by the dependent variable mean. In the third column, predicted effects are evaluated at the regressor means. In the fourth, these are calculated for everyone and then averaged across all sample members.

Our final analysis demonstrates heterogeneity in the effects of economic conditions on cigarette smoking by education attainment. We categorize our sample into those without four-year college degrees (or the low-educated) and those with college degrees (or the high-educated). The unweighted means show that highly educated individuals are approximately two times less likely to smoke than those with fewer schooling years, as shown in Table 3.23 and 3.24, respectively. Regarding the impact of eco-

Table 3.17. Heterogeneous Effect among Adults of Ages 35-64 – Percentage change in current smoking due to a one-point change in the employment rate using BRFSS 1987–2019 (aged 18+)

	Sample Mean	Predicted Effect	Percent Change	
			a	b
Panel A: Predicted effect without sampling weights				
Current smoking	0.2065	0.0953 (0.0185) [0.0393]	0.5	0.5
Panel B: Predicted effects with sampling weight				
Current smoking	0.2175	0.1097 (0.0394) [0.0534]	0.5	0.5

The table shows the predicted effects of a one-point increase in the state percentage of the population employed from binary probit models for females using data from BRFSS 1987–2019. The probit models also include month, year, and state dummy variables and controls for age, sex, gender, race/ethnicity, education, and marital status. The sample size is 4,704,627. Robust standard errors calculated assuming that observations are independent across months and states but not within states each month are reported in parentheses. Corresponding standard errors that assume independence across but not within states are shown in brackets. Predicted effects indicate the estimated percentage point change in the dependent variable, with other regressors evaluated at the sample means. Percentage changes are computed by dividing the predicted effect by the dependent variable mean. In the third column, predicted effects are evaluated at the regressor means. In the fourth, these are calculated for everyone and then averaged across all sample members.

nomic conditions on cigarette smoking, the estimates suggest that a one-point increase in employment rate increases current cigarette smoking by 0.5% among the low-educated individuals and imprecise effect among highly educated people when we do not include sample weights. By including sampling weights in the regressions, the estimate for the low-educated individuals declines to 0.3%, but negative and statistically insignificant among the high-educated individuals. These results are not surprising since we expect

Table 3.18. Heterogeneous Effect among Adults of Ages 65+ – Percentage change in current smoking due to a one-point change in the employment rate using BRFSS 1987–2019 (aged 18+)

	Sample Mean	Predicted Effect	Percent Change	
			a	b
Panel A: Predicted effect without sampling weights				
Current smoking	0.0911	0.0535 (0.0155) [0.0286]	0.6	0.6
Panel B: Predicted effects with sampling weight				
Current smoking	0.0979	-0.0284 (0.0373) [0.0361]	-0.3	-0.3

The table shows the predicted effects of a one-point increase in the state percentage of the population employed from binary probit models for females using data from BRFSS 1987–2019. The probit models also include month, year, and state dummy variables and controls for age, sex, gender, race/ethnicity, education, and marital status. The sample size is 2,607,894. Robust standard errors calculated assuming that observations are independent across months and states but not within states each month are reported in parentheses. Corresponding standard errors that assume independence across but not within states are shown in brackets. Predicted effects indicate the estimated percentage point change in the dependent variable, with other regressors evaluated at the sample means. Percentage changes are computed by dividing the predicted effect by the dependent variable mean. In the third column, predicted effects are evaluated at the regressor means. In the fourth, these are calculated for everyone and then averaged across all sample members.

individuals with lower education levels to be affected by changes in economic conditions. Since high-educated people have stable employment and income, low-educated individuals are more likely to gain employment and income during good economic conditions. On the other hand, less-educated people are more likely to lose their jobs during economic downturns. Therefore, we expected the effect to be larger among the low-educated people, as confirmed in the data.

Table 3.19. Heterogeneous Effect among Whites – Percentage change in current smoking due to a one-point change in the employment rate using BRFSS 1987–2019 (aged 18+)

	Sample Mean	Predicted Effect	Percent Change	
			a	b
Panel A: Predicted effect without sampling weights				
Current smoking	0.1752	0.0636 (0.0148) [0.0336]	0.4	0.4
Panel B: Predicted effects with sampling weight				
Current smoking	0.2097	-0.0052 (0.0828) [0.0940]	-0.0	-0.0

Note: The table shows the predicted effects of a one-point increase in the state percentage of the population employed from binary probit models for females using data from BRFSS 1987–2019. The probit models also include month, year, and state dummy variables and controls for age, sex, gender, race/ethnicity, education, and marital status. Sample size is 7,065,604. Robust standard errors calculated assuming that observations are independent across months and states but not within states each month are reported in parentheses. Corresponding standard errors that assume independence across but not within states are shown in brackets. Predicted effects indicate the estimated percentage point change in the dependent variable, with other regressors evaluated at the sample means. Percentage changes are computed by dividing the predicted effect by the dependent variable mean. In the third column, predicted effects are evaluated at the regressor means. In the fourth, these are calculated for everyone and then averaged across all sample members.

3.5 Conclusion

Since cigarette smoking is one of the leading causes of preventable death, we document how macroeconomic conditions affect it. This paper uses the 1987–2019 Behavioral Risk Surveillance System and the Bureau of Statistics employment dataset to estimate the effects of economic conditions on cigarette smoking. Our findings support

Table 3.20. Heterogeneous Effect among Blacks – Percentage change in current smoking due to a one-point change in the employment rate using BRFSS 1987–2019 (aged 18+)

	Sample Mean	Predicted Effect	Percent Change	
			a	b
Panel A: Predicted effect without sampling weights				
Current smoking	0.1957	0.0693 (0.0458) [0.1116]	0.4	0.4
Panel B: Predicted effects with sampling weight				
Current smoking	0.2097	0.1688 (0.0739) [0.0493]	0.8	0.8

Note: The table shows the predicted effects of a one-point increase in the state percentage of the population employed from binary probit models for females using data from BRFSS 1987–2019. The probit models also include month, year, and state dummy variables and controls for age, sex, gender, race/ethnicity, education, and marital status. Sample size is 713,333. Robust standard errors calculated assuming that observations are independent across months and states but not within states each month are reported in parentheses. Corresponding standard errors that assume independence across but not within states are shown in brackets. Predicted effects indicate the estimated percentage point change in the dependent variable, with other regressors evaluated at the sample means. Percentage changes are computed by dividing the predicted effect by the dependent variable mean. In the third column, predicted effects are evaluated at the regressor means. In the fourth, these are calculated for everyone and then averaged across all sample members.

the evidence that cigarette smoking increases when the economy improves. Specifically, we find that a one-point increase in employment rate increases current cigarette smoking prevalence by 0.4%. We also show evidence of a declining procyclical relationship between cigarette smoking and employment towards a zero effect, and the attenuation is larger during severe economic downturns. The declining procyclical relationship, accompanied by a sharp decline during the Great Recession period, suggests that esti-

Table 3.21. Heterogeneous Effect among Hispanics – Percentage change in current smoking due to a one-point change in the employment rate using BRFSS 1987–2019 (aged 18+)

	Sample Mean	Predicted Effect	Percent Change	
			a	b
Panel A: Predicted effect without sampling weights				
Current smoking	0.1569	0.0417 (0.0525) [0.0881]	0.3	0.3
Panel B: Predicted effects with sampling weight				
Current smoking	0.1597	-0.0100 (0.1230) [0.1014]	-0.1	-0.1

Note: The table shows the predicted effects of a one-point increase in the state percentage of the population employed from binary probit models for females using data from BRFSS 1987–2019. The probit models also include month, year, and state dummy variables and controls for age, sex, gender, race/ethnicity, education, and marital status. Sample size is 569,367. Robust standard errors calculated assuming that observations are independent across months and states but not within states each month are reported in parentheses. Corresponding standard errors that assume independence across but not within states are shown in brackets. Predicted effects indicate the estimated percentage point change in the dependent variable, with other regressors evaluated at the sample means. Percentage changes are computed by dividing the predicted effect by the dependent variable mean. In the third column, predicted effects are evaluated at the regressor means. In the fourth, these are calculated for everyone and then averaged across all sample members.

mates with data from 2020 will be more likely to be negative (i.e., countercyclical) due to the coronavirus pandemic. Finally, we find evidence of heterogeneous impacts. The effects are larger among males, Blacks, and low-educated individuals than their counterparts, but no differential impacts by age.

From the "rational addiction" framework, we anticipated a relatively small response to transitory price variations for heavy smoking "addicted" individuals (Becker

Table 3.22. Heterogeneous Effect among Other Races – Percentage change in current smoking due to a one-point change in the employment rate using BRFSS 1987–2019 (aged 18+)

	Sample Mean	Predicted Effect	Percent Change	
			a	b
Panel A: Predicted effect without sampling weights				
Current smoking	0.1992	0.1559 (0.0614) [0.1063]	0.8	0.8
Panel B: Predicted effects with sampling weight				
Current smoking	0.1623	0.3590 (0.1759) [0.2261]	2.2	1.1

Note: The table shows the predicted effects of a one-point increase in the state percentage of the population employed from binary probit models for females using data from BRFSS 1987–2019. The probit models also include month, year, and state dummy variables and controls for age, sex, gender, race/ethnicity, education, and marital status. Sample size is 601,012. Robust standard errors calculated assuming that observations are independent across months and states but not within states each month are reported in parentheses. Corresponding standard errors that assume independence across but not within states are shown in brackets. Predicted effects indicate the estimated percentage point change in the dependent variable, with other regressors evaluated at the sample means. Percentage changes are computed by dividing the predicted effect by the dependent variable mean. In the third column, predicted effects are evaluated at the regressor means. In the fourth, these are calculated for everyone and then averaged across all sample members.

and Murphy, 1988; Ruhm, 2005). Consequently, our finding of the substantial macroeconomic effect of current cigarette smoking is contrary to my expectation. Nevertheless, the main finding is consistent with previous estimates demonstrating that improvement in macroeconomic conditions leads to unhealthy lifestyles (Ruhm, 2000, 2005; Xu, 2013) and contrast with studies that find no or countercyclical relationship (Goel, 2008; Currie et al., 2015; Kalousova and Burgard, 2014; Barnes et al., 2009; Dehejia

Table 3.23. Heterogeneous Effect among Low-Educated Individuals – Percentage change in current smoking due to a one-point change in the employment rate using BRFSS 1987–2019 (aged 18+)

	Sample Mean	Predicted Effect	Percent Change	
			a	b
Panel A: Predicted effect without sampling weights				
Current smoking	0.2209	0.1127 (0.0199) [0.0508]	0.5	0.5
Panel B: Predicted effects with sampling weight				
Current smoking	0.2394	0.0695 (0.0387) [0.0532]	0.3	0.3

Note: The table shows the predicted effects of a one-point increase in the state percentage of the population employed from binary probit models for females using data from BRFSS 1987–2019. The probit models also include month, year, and state dummy variables and controls for age, sex, race/ethnicity, education, and marital status. Sample size for the low educated individuals are 5,983,292. Robust standard errors calculated assuming that observations are independent across months and states but not within states each month are reported in parentheses. Corresponding standard errors that assume independence across but not within states are shown in brackets. Predicted effects indicate the estimated percentage point change in the dependent variable, with other regressors evaluated at the sample means. Percentage changes are computed by dividing the predicted effect by the dependent variable mean. In the third column, predicted effects are evaluated at the regressor means. In the fourth, these are calculated for everyone and then averaged across all sample members.

and Lleras-Muney, 2004). Following Ruhm (2000, 2005) that used the BRFSS microdata from 1987–1995 and 1987–2000, respectively, this study is the first to use long-run data covering major macroeconomic shocks, including the 2001 economic downturn and the 2009 Great Recession. Using the 1987–2019 data allows us to capture the changing cigarette smoking responses to macroeconomic shocks and recoveries.

Table 3.24. Heterogeneous Effect among High-Educated Individuals – Percentage change in current smoking due to a one-point change in the employment rate using BRFSS 1987–2019 (aged 18+)

	Sample Mean	Predicted Effect	Percent Change	
			a	b
Panel A: Predicted effect without sampling weights				
Current smoking	0.0898	0.0180 (0.0139) [0.0212]	0.2	0.2
Panel B: Predicted effects with sampling weight				
Current smoking	0.1034	-0.0181 (0.0369) [0.0377]	-0.2	-0.2

Note: The table shows the predicted effects of a one-point increase in the state percentage of the population employed from binary probit models for females using data from BRFSS 1987–2019. The probit models also include month, year, and state dummy variables and controls for age, sex, race/ethnicity, education, and marital status. Sample size for the high educated individuals is 2,966,824. Robust standard errors calculated assuming that observations are independent across months and states but not within states each month are reported in parentheses. Corresponding standard errors that assume independence across but not within states are shown in brackets. Predicted effects indicate the estimated percentage point change in the dependent variable, with other regressors evaluated at the sample means. Percentage changes are computed by dividing the predicted effect by the dependent variable mean. In the third column, predicted effects are evaluated at the regressor means. In the fourth, these are calculated for everyone and then averaged across all sample members.

Two policy recommendations are as follows. Currently, tobacco control policies are designed to be fixed irrespective of the economic conditions. However, the results suggest that individuals myopically change their smoking behavior during downturns, leading to multiple unstable equilibria, which reverse to their usual pattern after temporal shocks. Therefore, we recommendation that policymakers create programs and policies that embed features of automatic stabilizers to absorb temporal economic shocks on cigarette smoking. During economic downturns, these policies should mitigate the magnified impacts of the economic shocks on smoking behaviors. Secondly, by demonstrating that the procyclical relationship between economic conditions and cigarette smoking has diminished towards countercyclical effects, especially in recent years, it implies that current policies need to tackle the countercyclical impacts instead of the procyclical relationships found in previous studies.

Appendix A

An Appropriate Estimator - A Recursive Bivariate Probit

An important econometrics issue that several studies overlook but still a debate in the literature is how to estimate binary choice models with endogenous regressors (Lewbel et al., 2012). Our goal is to use an estimation technique that can efficiently and consistently determine the parameters in equations (1) and (2). We would rely on the linear IV [i.e., two-stage least squares (2SLS)] if the outcome in equation (1), Y_{idt}, is continuous, even if the endogenous variable, I_{idt} [i.e., the outcome in equation (2)], is binary. However, since the dependent variable and our variable of interest are both binary, we use nonlinear models since the linear IV models cab be bad approximations of highly nonlinear models and lead to inconsistencies marginal effects (Altonji et al., 2005a; Lewbel et al., 2012; Angrist and Pischke, 2008, pp. 80).

Below, we show that the linear models may not be good. First, we rewrite the binary choice outcomes in equations (1) and (2) as below:

$$\mathbb{P}(Y_{idt} = 1 | \mathbf{X_{idt}}, \mathbf{I_{idt}}, \mathbf{H}) = \boldsymbol{F}(\beta_0 + \beta_1 I_{idt} + \boldsymbol{\Lambda}\mathbf{X_{idt}} + \boldsymbol{\gamma_d} + \boldsymbol{\tau_t}), \tag{A.1}$$

$$\mathbb{P}(I_{idt} = 1 | \mathbf{X_{idt}}, \mathrm{Z}_{dt}, \mathbf{H}) = \boldsymbol{G}(\alpha_0 + \alpha_1 \mathrm{Z_{dt}} + \boldsymbol{\theta}\mathbf{X_{idt}} + \boldsymbol{\lambda_d} + \boldsymbol{\pi_t}), \tag{A.2}$$

where $\boldsymbol{F}(\bullet)$ and $\boldsymbol{G}(\bullet)$ are nonlinear functions in their arguments, $\mathbf{H}$ represents a vector of the instrument, district and year fixed effect. The 2SLS procedure requires the substitution of the $\boldsymbol{G}$ into the $\boldsymbol{F}$ function to get the conditional expectation function. By substituting equation (7) into (6), we obtain (8) below:

$$\mathbb{P}(Y_{idt} = 1|\mathbf{X_{idt}}, \mathbf{I_{idt}}, \mathbf{H}) = \boldsymbol{F}\{\beta_0 + \beta_1 \boldsymbol{G}(\alpha_0 + \alpha_1 \mathrm{Z_{dt}} + \boldsymbol{\theta}\mathbf{X_{idt}} + \boldsymbol{\lambda_d} + \boldsymbol{\pi_t}) + \boldsymbol{\Lambda}\mathbf{X_{idt}} + \boldsymbol{\gamma_d} + \boldsymbol{\tau_t}\}. \tag{A.3}$$

The conditional expectation function, $\mathbb{E}$, which we estimate empirically, is given by:

$$\mathbb{E}[Y_{idt} = 1|\mathbf{X_{idt}}, \mathbf{I_{idt}}, \mathbf{H}] = \mathbb{E}[\boldsymbol{F}\{\beta_0 + \beta_1 \boldsymbol{G}(\alpha_0 + \alpha_1 \mathrm{Z_{dt}} + \boldsymbol{\theta}\mathbf{X_{idt}} + \boldsymbol{\lambda_d} + \boldsymbol{\pi_t}) + \boldsymbol{\Lambda}\mathbf{X_{idt}} + \boldsymbol{\gamma_d} + \boldsymbol{\tau_t}\}]. \tag{A.4}$$

However, because the $\boldsymbol{F}$ and $\boldsymbol{G}$ are nonlinear functions, we cannot pass the expected value through the composite functions, unless we approximate them with linear functions. If they are highly nonlinear, then the linear approximations will be bad. Therefore, using 2SLS can lead to marginal effects that are far from the parameters we are trying to estimate. Another fruitless technique is to use the two-step procedure that mimics the 2SLS. It is tempting to estimate equation (7) to obtain the predicted values (i.e., the predicted values from the first-stage equation) and substitute into the outcome equation in (6) before estimating it. However, with the same reasons why the 2SLS fail, Wooldridge (2010) and Green (1998) argue that substituting first stage fitted values into the outcome equation is inappropriate (i.e., known in the literature as "forbidden regression") and cannot produce consistent and efficient estimates.

Although there is no consensus on the best technique to use, most studies in the literature argue for the bivariate probit estimator. The main weakness is the assumption of joint normality on the error terms, ϵ_{idt} and η_{idt}, as specified below:

$$\begin{pmatrix} \epsilon_{idt} \\ \eta_{idt} \end{pmatrix} \sim \mathbb{N}\left(\begin{bmatrix} 0 \\ 0 \end{bmatrix}, \begin{bmatrix} 1 & \rho \\ \rho & 1 \end{bmatrix} \right). \tag{A.5}$$

The matrices in equation (10) indicate that ϵ_{idt} and η_{idt} are jointly normal, each with mean zero, unit variances, but with an unknown correlation, $\rho \neq 0$.[1] With the assumption of joint normality of the error terms, our $\boldsymbol{G}(\bullet)$ and $\boldsymbol{F}(\bullet)$ functions become $\boldsymbol{\Phi}(\bullet)$, the cumulative normal distribution function. The identification in the bivariate probit framework comes from both the instrument and the functional form restriction. Although we can identify the parameters without the instrument, we include it to allow for a semiparametric identification (Altonji et al., 2005a). Unfortunately, there is no econometric theory or formal test to show the relative contribution of the functional form and excluded instrument to the identification of the parameters.

[1]Check Wooldridge (2010, pp. 596 - 597) for the full derivation of the maximum likelihood estimator.

Appendix B

Table 1.A1. Robustness to Supply-side Factors on the Impact of NHIS on Twelve Months Healthcare Visits using Bivariate Probit Models and Years of NHIS Exposure as an Excluded Instrument

	(1)	(2)	(3)	(4)	(5)
NHIS Coverage	0.276**	0.307***	0.291**	0.314***	0.289***
	(0.1285)	(0.1037)	(0.1224)	(0.0970)	(0.1038)
Number of Hospitals (per 1000)	0.559*				0.724*
	(0.3090)				(0.4034)
Number of Hospital Beds (per 1000)		-0.00294			-0.0193
		(0.0294)			(0.1078)
Number of Nurses (per 1000)			0.0726		0.0867
			(0.0695)		(0.0660)
Number of Doctors (per 1000)				0.194	0.478
				(0.1367)	(0.4621)
Controls	Y/Y	Y/Y	Y/Y	Y/Y	Y/Y
Post-NHIS Indicator	Y/N	Y/N	Y/N	Y/N	Y/N
Survey Fixed-Effects	N/Y	N/Y	N/Y	N/Y	N/Y
Observations	15,112	15,112	15,112	15,112	15,112

Notes: We include the woman's age, place of resident (rural/urban), marital status of woman, pregnancy status, number of births in the last five years, birth history, wealth index, woman's education, woman's occupation, literacy status of woman, ethnicity, religion, and district fixed effects as the controls in each specification. Additionally, we include the number of hospital beds to capture supply-side effects. We report heteroscedastic robust-standard errors clustered within the district in the parentheses. The notation "N/N" for a variable X denotes that both the first and second equations of the bivariate model exclude the variable X. *p<.1, **p<.05, ***p<.01

Appendix C

Table 1.A2. Robustness to Additional Control Variables on the Impact of NHIS on Twelve Months Healthcare Visits using Bivariate Probit Models and Years of NHIS Exposure as an Excluded Instrument

	(1)	(2)	(3)	(4)
NHIS Coverage	0.270***	0.314***	0.302***	0.309***
	(0.066)	(0.091)	(0.111)	(0.104)
Controls	Y/Y	Y/Y	Y/Y	Y/Y
Post-NHIS Indicator	N/N	N/N	Y/Y	Y/N
Survey Fixed-Effects	N/N	Y/Y	N/N	N/Y
Observations	15,112	15,112	15,112	15,112

Notes: We include the woman's age, place of resident (rural/urban), marital status of woman, pregnancy status, number of births in the last five years, birth history, wealth index, woman's education, woman's occupation, literacy status of woman, ethnicity, religion, and district fixed effects as the controls in each specification. Additionally, we include dummies for frequent television viewer and radio listener and education and occupation of the mother's partner at the time of the survey. We report heteroscedastic robust-standard errors clustered within the district in the parentheses. The notation "N/N" for a variable X denotes that both the first and second equations of the bivariate model exclude the variable X. *p<.1, **p<.05, ***p<.01

Appendix D

Table 1.B1. Robustness to Additional Control Variables on the Difference-in-Differences Estimates of NHIS on Children Born in Health Facilities using Children from Rural Nigeria as a Control Group

	(1)	(2)	(3)	(4)	(5)
	Panel A: Coefficients from Linear Probability Models				
Treatment × Post	0.095***	0.088***	0.086***	0.083***	0.062***
	(0.024)	(0.025)	(0.021)	(0.022)	(0.021)
	Panel B: Marginal Effects from Probit Models				
Treatment × Post	0.054***	0.047**	0.054***	0.051***	0.028*
	(0.019)	(0.020)	(0.017)	(0.018)	(0.017)
Controls	N	N	Y	Y	Y
Post-NHIS Dummy	Y	N	Y	N	Y
Birth Year Fixed Effect	N	Y	N	Y	N
Linear Time Trend	N	N	N	N	Y
Observations	46,857	46,857	46,857	46,857	46,857

Notes: The specifications in Column (3) - (5) include mother, child, and household characteristics that may affect the outcome. They are mother's age at the time of childbirth categorized into four groups, ethnicity, place of resident (rural/urban), religious beliefs, marital status, education and, literacy status, occupation in the survey year, the gender of the child, birth order, and household wealth index. Some of the models also include year of birth fixed effects to account for changes in the national trends in the healthcare sector, income, and other factors that may increase growth. We cluster standard errors at the district and Local Government Agency to account for the over-time correlation in unobserved factors that affect the outcome. The DHS cluster is the same the enumeration area similar to census block (in the U.S.A. context).

Appendix E

Table 1.B2. Robustness to Additional Control Variables on the Difference-in-Differences Estimates of NHIS on Prenatal care Visits using Children from Rural Nigeria as a Control Group

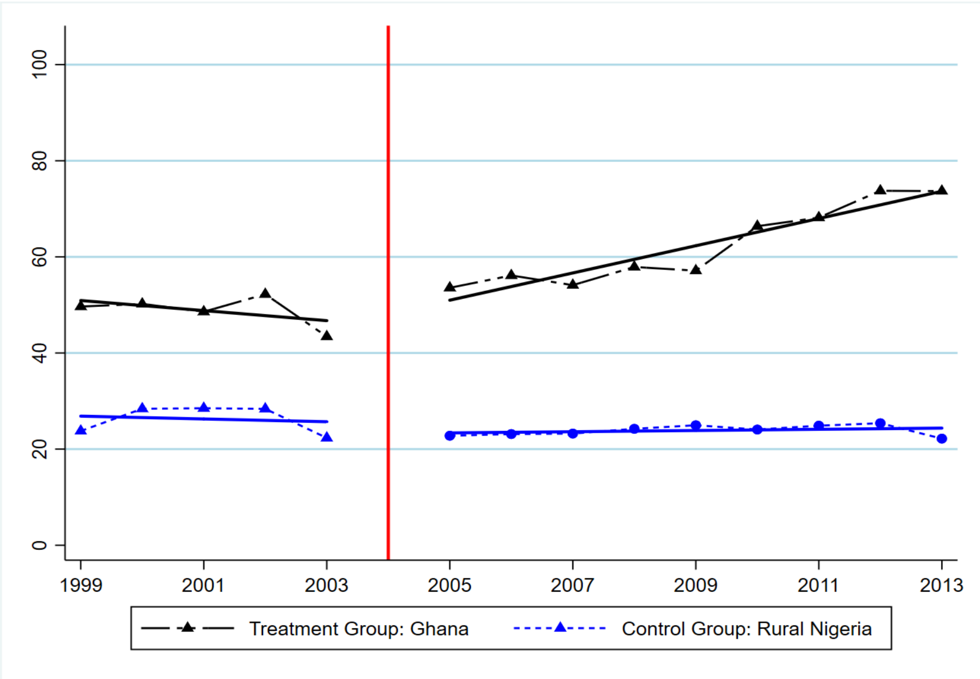
	(1)	(2)	(3)	(4)	(5)
	Panel A: Coefficients from Linear Probability Models				
Treatment × Post	0.047**	0.039*	0.063***	0.056**	0.057***
	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)
	Panel B: Marginal Effects from Probit Models				
Treatment × Post	0.045**	0.037*	0.061***	0.054**	0.054**
	(0.022)	(0.022)	(0.022)	(0.022)	(0.021)
Controls	N	N	Y	Y	Y
Post-NHIS Dummy	Y	N	Y	N	Y
Birth Year Fixed Effect	N	Y	N	Y	N
Linear Time Trend	N	N	N	N	Y
Observations	33,401	33,401	33,401	33,401	33,401

Notes: The specifications in Column (3) - (5) include mother, child, and household characteristics that may affect the outcome. They are mother's age at the time of childbirth categorized into four groups, ethnicity, place of resident (rural/urban), religious beliefs, marital status, education and, literacy status, occupation in the survey year, the gender of the child, birth order, and household wealth index. Some of the models also include year of birth fixed effects to account for changes in the national trends in the healthcare sector, income, and other factors that may increase growth. We cluster standard errors at the district and Local Government Agency to account for the over-time correlation in unobserved factors that affect the outcome. The DHS cluster is the same the enumeration area similar to census block (in the U.S.A. context).

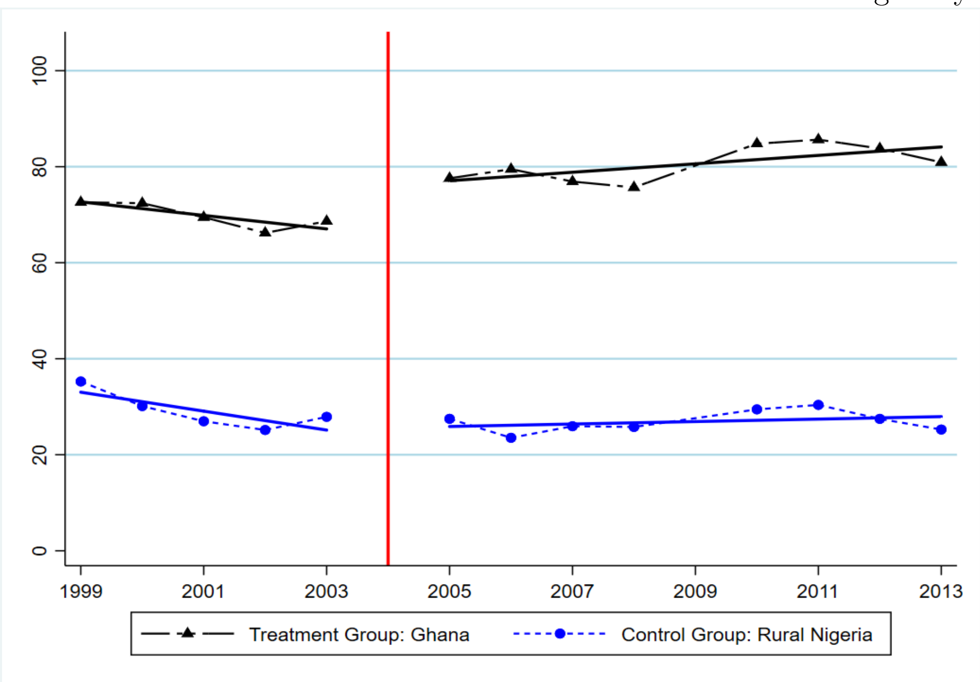
Appendix F

Figure 1.B1. Parallel Trends with Nigeria (i.e., Rural and Urban) as a Control Group

Panel A: Births in Health Facilities (or Institutional Births)



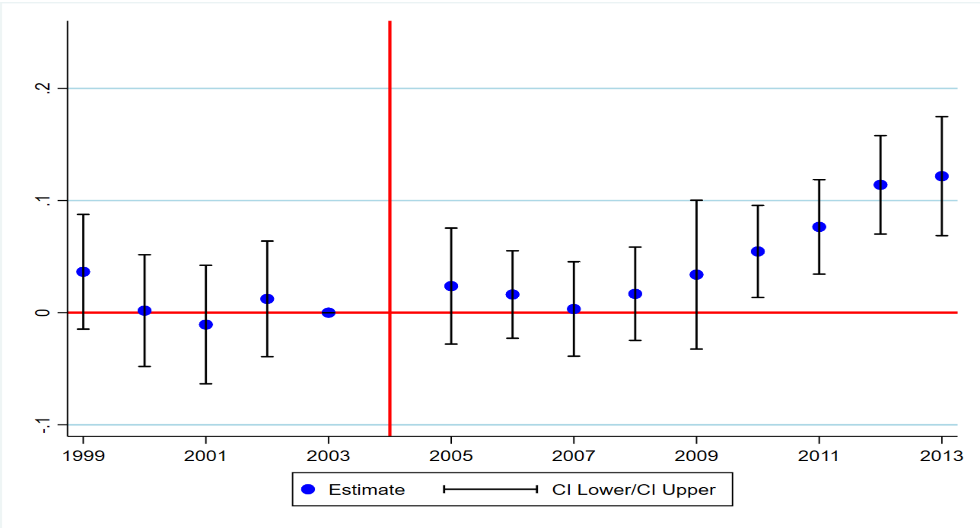
Panel B: Prenatal Care Visits in the First Four Months of Pregnancy



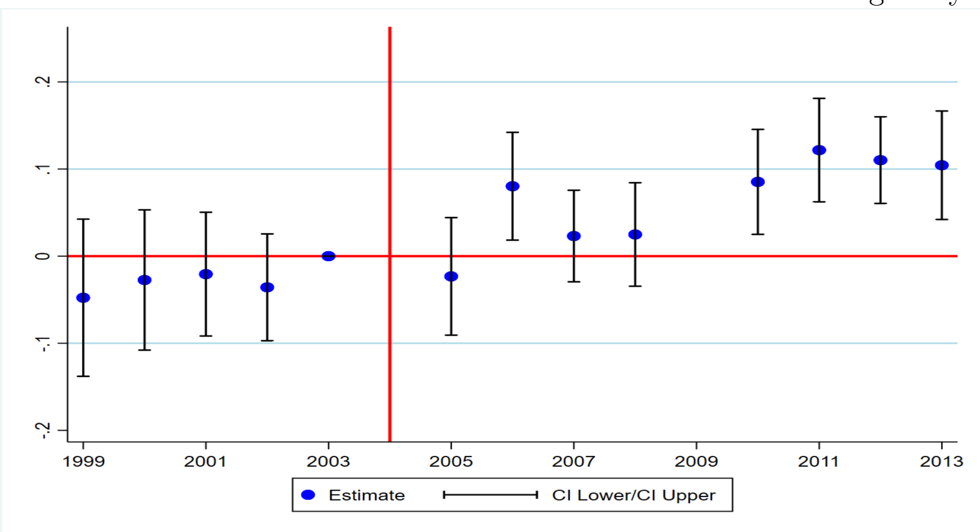
Appendix G

Figure 1.B2. Event Study with Nigeria (i.e., Rural and Urban) as a Control Group

Panel A: Births in Health Facilities (or Institutional Births)



Panel B: Prenatal Care Visits in the First Four Months of Pregnancy



Notes: In each figure, two separate linear regression models were used to calculate the estimates. The first regression used only data from the pre-NHIS period (1999 - 2003) to estimate the pre-NHIS coefficients (with 2003, before the NHIS, serving as the reference). The second regression used all data from 1999 to 2013 to estimate the post-NHIS coefficients, using the entire pre-NHIS period (1999 - 2003) as the reference. Results are conditional on the characteristics: sex of child, indicator for twins, birth order, place of resident (rural/urban), household wealth index, mother's age, marital status, education, occupation, literacy status, ethnicity, and religion.

Appendix H

Table 1.B3. Difference-in-Difference Estimates: Effects of the NHIS in Ghana on Children Born in Health Facilities Outcome using Children from Nigeria as a Control Group

	(1)	(2)	(3)	(4)	(5)
			Panel A: Coefficients from Linear Probability Models		
Treatment × Post	0.116***	0.101***	0.105***	0.102***	0.084***
	(0.033)	(0.033)	(0.021)	(0.022)	(0.022)
			Panel B: Marginal Effects from Probit Models		
Treatment × Post	0.088***	0.074**	0.074***	0.071***	0.052***
	(0.028)	(0.029)	(0.017)	(0.018)	(0.017)
Controls	N	N	Y	Y	Y
Birth Year Fixed Effect	N	Y	N	Y	N
Linear Time Trend	N	N	N	N	Y
Observations	65,032	65,032	65,032	65,032	65,032

Notes: The mean of the outcome is 0.378. The specifications in Column (3) - (5) include mother, child, and household characteristics that may affect the outcome. They are mother's age at the time of childbirth categorized into four groups, ethnicity, place of resident (rural/urban), religious beliefs, marital status, education and, literacy status, occupation in the survey year, the gender of the child, birth order, and household wealth index. Some of the models also include year of birth fixed effects to account for changes in the national trends in the healthcare sector, income, and other factors that may increase growth. We cluster standard errors at the district and Local Government Agency to account for the overtime correlation in unobserved factors that affect the outcome. The DHS cluster is the same the enumeration area similar to census block (in the U.S.A. context).

Appendix I

Table 1.B4. Difference-in-Difference Estimates: Effects of the NHIS in Ghana on Any Prenatal Care Visits Outcome using Children from Nigeria as a Control Group

	(1)	(2)	(3)	(4)	(5)
	Panel A: Coefficients from Linear Probability Models				
Treatment × Post	0.076***	0.071***	0.083***	0.082***	0.081***
	(0.023)	(0.023)	(0.021)	(0.021)	(0.021)
	Panel B: Marginal Effects from Probit Models				
Treatment × Post	0.084***	0.079***	0.090***	0.089***	0.086***
	(0.025)	(0.025)	(0.022)	(0.022)	(0.022)
Controls	N	N	Y	Y	Y
Post-NHIS Dummy	Y	N	Y	N	Y
Birth Year Fixed Effect	N	Y	N	Y	N
Linear Time Trend	N	N	N	N	Y
Observations	46,271	46,271	46,271	46,271	46,271

Notes: The mean of the outcome is 0.569. The specifications in Column (3) - (5) include mother, child, and household characteristics that may affect the outcome. They are mother's age at the time of childbirth categorized into four groups, ethnicity, place of resident (rural/urban), religious beliefs, marital status, education and, literacy status, occupation in the survey year, the gender of the child, birth order, and household wealth index. Some of the models also include year of birth fixed effects to account for changes in the national trends in the healthcare sector, income, and other factors that may increase growth. We cluster standard errors at the district and Local Government Agency to account for the overtime correlation in unobserved factors that affect the outcome. The DHS cluster is the same the enumeration area similar to census block (in the U.S.A. context).

Appendix J

Challenges in Identifying Charter School Effects

The positive effects of charter schools on school competition may imply they are "skimming the cream," taking only the best students from the TPSs. They could also signal parents looking for alternatives. On the other hand, a negative effect could suggest that poor-performing students transfer to charter schools, searching for better schools. Suppose charter schools increase competition, which eventually increases the outcomes of the students in their jurisdiction. In that case, comparing charter schools and TPSs students will not capture the charter schools' full effects if the initial distribution of performance is unknown. The presence of school competition could shift the distribution of students' performance. Therefore, understanding students' performance distribution for charter schools and TPSs students is essential for a valid comparison.[1] However, a common concern in most studies in the literature is that they do not have data on pre-treatment outcomes. Without such data, it is challenging to disentangle the charter schools impacts on students' achievements. Therefore, the mixed evidence of the impact of attending charter schools on students' outcomes can mislead policymakers because the results across different studies may be incomparable due to data limitations. Our study uses empirical strategy that does not require students' pre-treatment outcomes, as discussed in Section 2.5.

Identifying the impact of charter schools on the students' outcomes is not an easy task due to possible endogeneity. First, charter school attendance is not by chance. Parents seek a better alternative for their children, especially those who are already

[1] The reason is that charter schools may be attracting students from one side of the ability distribution. If only students struggling in the TPSs transfer to the charter schools, we would expect students' average quality in charter schools to be lower than those who remain in the TPSs. Making a comparison based on average performance leads to an underestimation of charter school effects. The converse is also true.

struggling in the TPSs. The fact that students and parents choose charter schools over TPSs suggests that there are inherent reasons which make them seek for alternatives. Also, students who choose charter schools may be having difficulties regarding their performances in the TPSs. Without data on students' initial performance before transferring into charter schools, it is challenging to identify charter schools' effects. Hence, unobserved characteristics, including students' ability and preferences, can lead to self-selection into charter schools. Second, the decision by a state to pass a charter school law may be endogenous since states with good and highly competitive school systems are less likely to implement the charter law, whereas states whose school system is less competitive would be willing to pass the law. Even if states do not endogenously pass the laws,[2] school districts and local school boards may endogenously open charter schools. Third, different charter schools may face different rules depending on the state, county, or school district's preferences. Fourth, charter schools may differ from each other based on curricular, teaching styles, teachers, and resources available. Finally, different charter schools may serve different communities or students, which can lead to different outcomes.

Studies that focus on charter school attendance and students' outcomes use different methods to address endogeneity issues. They use both experimental and quasi-experimental methods. The ideal scenario is to conduct an experiment where one-half of the student is randomly assigned to charter schools. The random assignment can identify the average treatment effect of charter school attendance. Unfortunately, conducting such an experiment is not feasible because of ethical concerns. (School choice

[2] This reason is that charter schools were established to provide alternatives to the TPSs and create competition. For example, Hoxby (2004) examines the impact of charter schools by examining the changes in mean test scores before and after introducing charter schools. The findings are that schools exposed to charter school competition have more enormous improvements in an average performance in terms of test scores than schools not exposed to a significant charter school competition.

is voluntary, and it is illegal to force students to attend schools unwillingly). One natural experiment similar to random assignment is using a lottery to assign students in oversubscribed charter schools. Students who lose in the lottery and remain in TPSs or private schools become the control group, and those who win the lottery form the treatment group. Focusing on students who subscribe to charter schools only becomes analogous to the experiment described earlier. These are known in the literature as "lottery-based" studies.

Dobbie and Fryer Jr (2015) is a lottery-based study that followed applicants to a charter middle school in the Harlem Children's Zone (New York). Similarly, Angrist et al. (2016) lottery-based study considered only oversubscribed Boston (Massachusetts) charters. Despite the experimental nature of these studies, they have limitations. First, conditions that lead to oversubscription is neither universal nor random since only older schools and academically better schools get oversubscription (Davis and Raymond, 2012). Second, the rate of oversubscription differs across charter schools. Whereas one school may have a larger subscription rate, others may have a lower subscription rate. Third, students may enroll in a non-TPS because of losing the lottery. If the data for these studies do not reflect students who are home-schooled or move to private schools after losing the lottery, the results can be biased. Fourth, these studies could include only oversubscribed schools. But there are many charter schools without oversubscription. Hence, a generalization of findings from lottery-based studies is problematic, and their findings' external validity is concerning.

An alternative to experimentally designed (or "lottery-based") studies is the quasi-experimental studies. These studies choose methods that can solve the endogeneity issues associated with survey data. Among these studies, the predominantly used method is the student-level fixed effects model. They use panel data to account for the students' pre-treatment outcomes and remove time-invariant observed and unobserved characteristics. The net effect is the "value-added" from attending charter schools. Studies such as Imberman (2011); Booker et al. (2007, 2008); Ni (2009); Zimmer et al. (2012); Jinnai (2014); Winters (2012); Bifulco and Ladd (2006); Zimmer et al. (2009), have implemented this approach. A few of their limitations are as follows. First, there may be demographically identifiable differences between students who always remain in charter schools and TPSs as noted in Davis and Raymond (2012). Second, the research design requires students to move from TPSs to charter schools and eliminates a sample of students who never switched schools. Third, these studies may have smaller sample sizes due to the nature of the design.

Other methods that are predominantly found in the literature are the matching method (Sass et al., 2016; Dobbie and Fryer, 2020; Hoxby, 2004), instrumental variable (IV) approach (Imberman, 2011; Bettinger, 2005), and difference-in-difference (Bettinger, 2005). However, all these methods have issues as well.

Appendix K

Table 2.A1. Dates of State Charter Law and Regulations on the Number of Charter Schools Permitted

State	Law Year	Cap	Description of Cap
Alabama	2015	Yes	10 until 2022
Alaska	1995	No	
Arizona	1994	No	
Arkansas	1995	Yes	Approve up to 24 per year
California	1992	Yes	250 in 1998/1999 and increased by 100 annually
Colorado	1993	No	
Connecticut	1996	No	
Delaware	1995	No	
DC	1996	Yes	10 per year per authorizer. If an authorizer has not reached 10, another authorizer can grant up to 20
Florida	1996	No	
Georgia	1993	No	
Hawaii	1994	No	
Idaho	1998	No	
Illinois	1996	Yes	120 but 70 in Chicago with at least 5 for students from low-performing or overcrowded schools. No more than 45 will operate in the rest of the state
Indiana	2001	No	
Iowa	2002	No	But not more than 10 innovation zone applications
Kansas	1994	No	
Kentucky	NA	No	
Louisiana	1995	No	
Maine	2011	Yes	10 until 2022
Maryland	2003	No	
Massachusetts	1993	Yes	120. Up to 48 reserved for Horace Mann charter & schools up to 72 reserved for commonwealth charter school not including charter schools in low-performing school districts. At least 2 charter schools must be in the lowest 10%.
Michigan	1993	Yes	15
Minnesota	1991	No	
Mississippi	2010	Yes	15 per year but expires in 5 years
Missouri	1998	No	But limited to certain areas
Nevada	1997	No	
New Hampshire	1995	No	
New Jersey	1996	No	
New Mexico	1993	Yes	75 schools in any 5-year period. Not more than 15 opened per year
New York	1998	Yes	460 and not more than 50 charters issued after July 1, 2015 can be granted for schools located in a city with a population of 1 million or more.
Ohio	1997	Yes	100 schools per authorizer and its DOE approving up to 20 schools per year
Oklahoma	1999	Yes	Up to 5 if county pop $<$ 500K and no more than 1 per year in single school district.
Oregon	1999	No	
Pennsylvania	1997	No	
Rhode Island	1995	Yes	35
South Carolina	1996	No	
Tennessee	2002	No	

Continued on the next page

Table 2.A1 – Continued from the previous page

State	Law Year	Cap	Description of Cap
Texas	1996	Yes	305 new per year starting 2019
Utah	1998	No	
Virginia	1998	No	
Washington	2012	Yes	A maximum of 40 charter schools may be established over a 5-year period, starting in 2016. Not more than 8 per year
Wisconsin	1993	Yes	Cap on the number that an authorizer may oversee
Montana	NA		
North Dakota	NA		
South Dakota	NA		
Nebraska	NA		
Kentucky	NA		
West Virginia	NA		
Vermont	NA		

Appendix L

Table 2.A2. First Stage Estimates for Drinking - Effects of Charter School Exposure in the County of Birth on Exposure in the County of Residence

	Years of Charter School Exposure in County of Residence [0,12]				Proportion of Students in Charter School in County of Residence [0,1]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years [0,12]	0.908***	0.890***	0.816***	0.816***				
	(0.010)	(0.013)	(0.028)	(0.029)				
Coverage [0,1]					0.734***	0.706***	0.687***	0.682***
					(0.041)	(0.047)	(0.051)	(0.047)
F-Statistic	7,724	4,568	822	809	317	221	182	203
Observations	12,482	12,482	12,482	12,482	12,482	12,482	12,482	12,482

Notes: Each estimate comes from a separate regression. The regressions include gender (male/female), race (Black, White, and Hispanic), education (high school graduates, some college, and college graduates or better), a dummy for birth county availability, and NLSY cohort fixed effect. Additionally, for the specifications in columns (2) and (6), we add the year of survey and birth cohort fixed effects. In columns (3) and (7), we also add county fixed effects. Finally, for the results in columns (4) and (8), the specifications control county time-varying characteristics by including poverty rate and unemployment rate. *p<.1, **p<.05, ***p<.01

Appendix M

Table 2.A3. First Stage Estimates for Smoking - Effects of Charter School Exposure in the County of Birth on Exposure in the County of Residence

	Years of Charter School Exposure in County of Residence [0,12]				Proportion of Students in Charter School in County of Residence [0,1]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years [0,12]	0.919***	0.907***	0.819***	0.819***				
	-0.01	(0.011)	(0.030)	(0.030)				
Coverage [0,1]					0.758***	0.715***	0.703***	0.696***
					(0.037)	(0.044)	(0.047)	(0.044)
F-Statistic	9,347	6,440	768	753	413	262	220	251
Observations	11,156	11,156	11,156	11,156	11,156	11,156	11,156	11,156

Notes: Each estimate comes from a separate regression. The regressions include gender (male/female), race (Black, White, and Hispanic), education (high school graduates, some college, and college graduates or better), a dummy for birth county availability, and NLSY cohort fixed effect. Additionally, for the specifications in columns (2) and (6), we add the year of survey and birth cohort fixed effects. In columns (3) and (7), we also add county fixed effects. Finally, for the results in columns (4) and (8), the specifications control county time-varying characteristics by including poverty rate and unemployment rate. *p<.1, **p<.05, ***p<.01

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Vita

Samuel Asare was born in Kumasi in the Ashanti Region of Ghana. He attended Adisadel College (i.e., high school), where he gained a keen interest in Economics. His undergraduate studies at the University of Ghana were in Economics, Mathematics, and Statistics, which he received a BA in Economics and Mathematics in May 2012. In the 2012/13 academic year, Samuel was a teaching assistant to Dr. Willim Bekoe and Dr. Festus Ebo Turkson in the Department of Economics at the University of Ghana. In Fall 2013, Samuel moved to the United States to pursue his master's degree in Economics at the University of Akron, Ohio. Before earning the MA in Spring 2014, he expressed a desire for research, which his graduate adviser, Dr. Sucharita Ghosh, recommended Ph.D. as the best next career option. In August 2015, he enrolled in a Ph.D. program in Economics at the Andrew Young School of Policy Studies at Georgia State University (GSU). Samuel's research interests are in applied econometrics, health economics, development economics, and education economics.

Throughout his graduate studies in the Andrew Young School of Policy Studies, Samuel worked as a Graduate Research Assistant for Dr. Shiferaw Gurmu, who was also his dissertation adviser. After passing a teaching qualification exam, he taught intermediate microeconomics twice and undergraduate econometrics once at GSU. He also worked as an intern at the American Cancer Society, where he published his first research paper.

Samuel received several awards throughout his graduate studies at Georgia State University, including the Provost's Dissertation Fellowship, Center for the Economic Analysis of Risk Fellowship, and the AYSPS Excellence in Teaching Economics Award.

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