ESSAYS ON HEALTH ECONOMICS AND HEALTH BEHAVIORS

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TO God, my family and Peru

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INTRODUCTION

At least half of personal health spending in the U.S. is related to behavior, lifestyle or other avoidable causes. It has become increasingly important for policy makers to understand what affects behavior and why certain policies work while others do not. Economist are interested in how these policies affect health behaviors and how these behaviors affect economic outcomes. In this dissertation I analyze two state level policies and their effects on health and health behaviors. In my last chapter I analyze the effect of a particular health behavior –obesity– on an economic outcome: wages.

In my first chapter, I study a reform that occurred in Tennessee in which the state disenrolled about 170,000 individuals from the Medicaid Program. The majority of these individuals were childless adults. I use this reform to investigate what happens to individuals when they lose public health insurance in terms of their health, health behaviors and health care utilization. Using data from the 2000-2010 Behavior Risk Factor Surveillance System (BRFSS) and restricted-use versions of the 2000-2010 National Health Interview Survey with state identifiers, I compare differences in outcomes between childless adults and other adults in Tennessee with the associated differential for these two groups across other Southern states, before and after the reform. I confirm that the 2005 TennCare disenrollment significantly decreased overall health insurance coverage, and I provide the first evidence that the disenrollment significantly decreased health care access, out of the people being dropped 70 percent of them reporting not being able to afford going to the doctor. I also document increases in the number of days with bad health. I do not find consistent evidence of effects for preventive care, although I do find suggestive evidence of increases in healthy behaviors. Overall the effects of the reform are concentrated among less educated childless non-elderly adults. These findings have potentially important implications for recent state public insurance expansions that are part of federal health care reform.

The second chapter, co-authored with Christopher S. Carpenter, analyses the enactment of a bill adopted in 26 states named Commonsense Consumption Acts (CCAs) – aka 'Cheeseburger Bills' –that greatly limit fast food companies' liability for weight-related harms. We provide the first evidence of the effects of CCAs using plausibly exogenous variation in the timing of CCA adoption across states. In two-way fixed effects models, we find that CCAs significantly increased stated attempts to lose weight and consumption of fruits and vegetables among heavy individuals. We also find some evidence that CCAs increased employment in fast food. Finally, we find that CCAs significantly increased the number of company-owned McDonald's restaurants and decreased the number of franchise-owned McDonald's restaurants in a state. Overall our results provide novel evidence supporting a key prediction of tort reform – that it should induce individuals to take more care – and show that industry-specific tort reforms can have meaningful effects on market outcomes.

In the third chapter, co-authored with Andrea Moro and Tommaso Tempesti we study how a health behavior can affect economic outcomes. We estimate the effects of being obese on wages accounting for the level of personal interactions required by the job and accounting for job selection. Using data from the National Longitudinal Survey of Youth 1979 (1982 - 2006) combined with detailed information about jobs from O*Net, our results show that the obesity penalty occurs mostly on white women and that this penalty is higher in jobs that require a higher level of social interaction. In addition, we find that accounting for selection increases the estimates of the wage-penalty by 50% compared to estimates that ignore job selection. This provides strong evidence that a health behavior can greatly affect the economic outcomes of an individual and in turn impose a cost for society.

These three chapters provide empirical evidence that policies have an effect on people's health behavior and that changes in health behavior can affect health and economic outcomes. These results show that losing public health insurance is detrimental for population health. Tort reform policies for fast food make the industry stronger and induce intentions to change health behaviors but these intentions do not translate to actual changes. White women receive a wage penalty for being obese, and this seems to be driven by women who work in jobs where social interactions matters more.

CHAPTER 1

EFFECTS OF LOSING PUBLIC HEALTH INSURANCE ON HEALTHCARE ACCESS, UTILIZATION AND HEALTH OUTCOMES: EVIDENCE FROM THE TENNCARE DISENROLLMENT

1. Introduction

There is an extensive literature in health economics that explores the effects of public health insurance eligibility on health outcomes and access to health care (Buchmueller et al. 2015; Finkelstein et al 2014; Currie and Gruber 1999; Kolstad and Kowalksi 2012). However, most of what we know of the relationship between health insurance and health comes from empirical investigations of people gaining public health insurance. There has been relatively less research done on the effects of *losing* public health insurance on health, mostly due to lack of exogenous events that cause people to lose public health insurance eligibility. This paper is the first comprehensive study of the effects of losing public health insurance on population health outcomes using a quasi-experimental design. Specifically, I consider the effects of one of the largest public health insurance disenrollments in the U.S.: the 2005 Tennessee disenrollment in which approximately 170,000 residents were dropped from the state's Medicaid program, TennCare. This reform targeted non-elderly childless adults, an understudied population in the health insurance literature. This population is of particular interest since most of the recent Affordable Care Act (ACA) expansions target childless adults.[1]

Theoretically, predictions regarding the effects of losing health insurance on health are not necessarily symmetric to the predictions regarding the effects of gaining health insurance. The main difference relies on the accumulation of health capital: individuals who have had health insurance for an extended period of time could have a greater level of health capital than a person who has not had health insurance. For instance, consider a diabetic woman who has had health insurance for an extended period of time. During this time she has been able to learn that she has a chronic condition, the degree of the problem, and how to handle it. She has received information about the importance of an adequate diet and she may have had access to prescription drugs. Once this person loses health insurance, even though her health care access is reduced, she does not lose the information she has on her health condition. In contrast, consider the same woman who starts out without health insurance. In that case, it is likely that she would not have been able to obtain as much information on her health condition during her uninsured spell. If she gains health insurance, not only will her health care access increase but she may also experience

[1] I define childless adults as adults who report having no children under 18 years-old living in their household. Using family relationships within the household I am also able to identify adults with dependents and adults without dependents.

large and immediate information gains. These and other examples illustrate the possibility of asymmetries in the effects of losing and gaining health insurance.

While the few investigations of the effects of losing health insurance have focused on one particular health related outcome, this paper studies a broad range of health outcomes. First I study people's decisions to go to the doctor and their rates of preventive care utilization.[2] I consider this to be the primary mechanism through which losing health insurance may affect health. Second, I study how losing health insurance affects self-rated health status and the number of reported sick days. I also consider the effects on where people choose to obtain medical care and their total demand for care. Finally, I am also able to study changes in risky and non-risky health behaviors to identify the presence of moral hazard.

In order to answer these questions, I use an exogenous reform that caused people to lose public health insurance. In 2005, Tennessee underwent a major Medicaid cutback, in which approximately 170,000 residents lost public health insurance eligibility. Recent research has examined the effects of this reform on labor supply (Garthwaite et al. 2014), hospital uncompensated care (Garthwaite et al. 2015)

[2] To this point there are two studies that examined the effect of the TennCare reform on a health related outcome: Hearvin et al. (2011) and Ghosh and Simon (2015). Heavrin et al. (2011) evaluate the effects of Tennessee's disenrollment on Emergency Department visits. Ghosh and Simon (2015) evaluate the effects of the disenrollment on hospitalizations. There have also been reports by the Robert Wood Johnson Foundation (Farrar et al. 2007) that describe through anecdotal evidence and interviews the effects of the disenrollment on an individual's heath status.

and inpatient hospitalizations (Ghosh and Simon, 2015).[3] The cutbacks were made on the 1994 TennCare Reform, which had expanded eligibility for public health insurance to non-traditional Medicaid beneficiaries. This expansion group was mostly composed of non-elderly childless adults and people who were considered "uninsurable."[4]

In doing so, the 2005 reform targeted a particular subpopulation that has been understudied in the health insurance literature: childless adults. At least half of the uninsured adult population in the United States is composed of childless adults. These individuals are 19 to 64-year-olds who are commonly lower income, less educated, and either work for an employer that does not provide health insurance or do not work enough hours to qualify for benefits (ASPE 2005). This population constitutes a large portion of the population that would be affected by numerous Medicaid expansions under the Affordable Care Act (ACA) that aim to close the health coverage gap between individuals who are not poor enough to qualify for Medicaid but not wealthy enough to purchase private health insurance.[5] Therefore, if any future cutbacks target the most recent expansions, childless adults may be the first group to lose coverage.[6]

[3] An inpatient is a patient that had a doctor recommend to stay at least one night in the hospital.
[4] This term refers to people who have been previously denied private health insurance.
[5] Estimates range from 15 to 20 million of individuals covered by the ACA Medicaid Expansions. (Kenney et al., 2012)
[6] In 2012, the U.S. Supreme Court (*Florida v. Department of Health and Human Services*) overturned the provision of the law requiring Medicaid expansions, leaving the decision up to each state. Since then, a considerable number of states have decided not to use federal money to expand Medicaid programs. As of March 2013, 17 states opposed Medicaid Expansion (Kaiser Report 2013).

My empirical strategy uses the sharp state-specific timing of the disenrollment combined with the fact that it mostly targeted childless adults to obtain inference on the effects of losing public health insurance eligibility on health care access, utilization of care and health outcomes. The first approach is a straightforward Difference-in-Differences (DD) model that compares residents of Tennessee to those of other southern states before and after the disenrollment. The second approach uses a triple difference (DDD) model to take advantage of the fact that the vast majority of individuals who lost eligibility during the reform were childless adults. Garthwaite et al. (2014) estimate that 91% of those affected by the disenrollment were adults without dependents under the age of 18. I compare the differential in outcomes of adults with and without children in Tennessee to the associated difference for the same groups in other southern states before and after the reform. In addition, given the single-state nature of my treatment, to account for state specific shocks I use synthetic control methods to corroborate my findings (Abadie et al. 2010).

I estimate that the TennCare disenrollment significantly decreased the likelihood of having health insurance between 2 and 5 percent. I provide evidence of decreases in health care access; specifically, I estimate an increase in the likelihood of forgone or delayed medical care due to cost of at least 10 percent and a decrease in the likelihood of seeing a general doctor of 4 percent. This serves as a mechanism to understand the decreases in health status. I estimate that the reported number of days with bad health over 12 months increased by 0.6 days (out of a mean of 5 days), and the number of days incapacitated increased by 0.84 days (out of a mean of 4.7 days).

In terms of demand for medical care, I provide evidence that the likelihood of people to change their place of care due to health insurance reasons increases by almost half out of a mean of 3 percent. This effect is larger for low educated individuals, who experience a 115 percent increase. Relatedly, I find that, after the reform, this group is less likely to report the doctor's office or HMO provider as their source of usual care and is more likely to report an Emergency Department (ED), hospital outpatient department or a clinic as their source of usual care. In terms of health care utilization, I show that the likelihood of going to an Emergency Department increases by 7 percent along the intensive and extensive margins. I also find a 20 percent decrease in the number of surgeries and the likelihood of having a surgery.

In terms of inpatient stays, using survey data I find a 10 percent decrease in the number of times a patient has stayed overnight in a hospital. Using administrative data I find a 40 percent decrease in the number of discharges per hospital quarter for the non-elderly. I also I find a significant 20 percent reduction in the payments coming from Medicaid and a 30 percent increase in the payments coming from the patient. These results are larger for individuals with a high school degree or less and they are robust to the choice of alternative control groups as well as inference adjustment that accounts for the single-state nature of the reform. I also find suggestive evidence of the presence of moral hazard: I estimate an 8 percent increase in the likelihood of getting a flu shot and engagement in healthier behaviors.

My paper contributes to the literature in the following ways: First, I provide the literature's first comprehensive evidence on the population health effects of losing public health insurance eligibility using a quasi-experimental design. Second, I investigate possible mechanisms of how changes in health insurance status can affect health, and in doing so I provide evidence of how people's decisions regarding health care and health behaviors changed after the disenrollment. Third, part of the mixed evidence of public health insurance eligibility effects on healthcare utilization comes from analyzing different types of data: survey data versus administrative data. In my paper, I use both types of data. I provide evidence from two population representative surveys and one administrative dataset on inpatient hospitalizations. Furthermore, having numerous datasets allows me to study the reform in a comprehensive way by investigating not only health care access but also changes in preventive care, health behaviors, health care utilization and health status.

In addition to these contributions, this paper is important for policy-makers since it provides evidence on a particular population of interest: childless adults. This population is the target of the recent ACA Medicaid expansions which have recently met significant opposition, and their future is highly contingent upon political and economic environments. Especially since a considerable number of states have opted to depend on state funding rather than federal funding to comply with the ACA. Even if most of the ACA mandates are not repealed, it is not unreasonable to expect that budget deficits could drive states to enact public health insurance cutbacks similar to the 2005 disenrollment in Tennessee.

The rest of the paper proceeds in the following manner. Section 2 describes the existing literature on the effects of changing public health insurance eligibility on health. Section 3 provides institutional background on the 2005 TennCare reform. Section 4 explains the empirical strategy. Section 5 describes the data. Section 6 presents the results, and Section 7 offers a discussion and conclusions.

2. Literature Review

In this section I review the literature on the effects of policy-induced changes in health insurance on health outcomes, with a focus on studies examining public health insurance eligibility.

2.1. Studies on the effects of gaining insurance coverage on health

A large literature in economics has examined the effects of obtaining public health insurance eligibility on health outcomes. I focus here on papers with populations similar to the one I study: namely, non-elderly childless adults.[7]

[7] I do not review a large literature that has studied policy induced changes in public health insurance eligibility for different target populations such as: Medicaid expansions for pregnant women (Currie and Gruber, 1996b) and infants (Currie and Gruber, 1996a; Dafny and Gruber, 2005), or obtaining coverage through Medicare for the elderly (Card, Dobkin and Maestas, 2004; 2009; Finkelstein and McKnight, 2008). Buchmueller et al (2015) summarizes the main findings from the extensive literature of the effects of the Medicaid program on a variety of economic and health outcomes.

Recently, two health care reforms have received a significant amount of attention: the 2008 Oregon Medicaid Lottery and the 2006 Massachusetts health insurance reform. Both of these reforms mostly affected non-elderly adults. In fact, it is estimated that around 56 percent of the people affected by Oregon Lottery were childless adults while 50 percent of people affected by the MA health reform were childless adults (Garthwaite et al., 2014).

There are three main papers that estimate the effects of the Oregon Medicaid Lottery on health outcomes: Finkelstein et al. (2012), Baicker et al. (2013) and Taubman et al. (2014). These studies provide evidence from survey data and administrative data on the effects of the Oregon Medicaid Lottery in which some individuals were randomly selected to gain Medicaid eligibility. From survey data the studies found that outpatient visits increased by 35 percent and the likelihood of having a prescription filled increased by 15 percent. They also document increases in preventive care: namely cholesterol tests, blood tests for diabetes, mammograms, and Pap tests. Nevertheless, they did not find changes in diagnoses for any of the conditions that were associated with the changes in preventive care. They also find increases in self-assessed measures of health but did not find evidence of changes in ER utilization or inpatient stays.[8] In contrast, using administrative data to study the intervention showed that inpatient

[8] The authors conjecture that the increases in self-reported ratings of health can be mostly explained by the reductions in financial distress.

admissions increased by 30 percent while ER visits increased by 40 percent over an 18 month period.[9]

The impact of the Massachusetts health reform of 2006 on adult health has been extensively studied. This reform expanded Medicaid while at the same time creating incentives to obtain private health insurance. Most of these papers use a Difference-in-Difference strategy to compare outcomes in Massachusetts before and after the reform with the associated changes in outcomes for individuals in other states. They find evidence that the Massachusetts reform increased health coverage by about 6 percent (Kolstad and Kowalski, 2012; Long et al., 2009), which consequently increased access to care (Long et al., 2014), breast and cervical cancer screenings three years after the implementation (Sabik et al., 2015) and self-assessed ratings of health (Courtemanche et al., 2014). Miller (2012) and Long et al. (2012) found a reduction in ED utilization between 5 and 8 percent. Finally, Kolstad and Kowalsi (2012) found no evidence of changes in inpatient admissions but they do document a decline in inpatient admissions originating from the ED.

There are other less studied Medicaid expansions from Wisconsin, New York, Maine and Arizona, each with different target populations and unique aspects of the expansions. DeLeire et al. (2013) and Burns et al. (2014) study the Wisconsin Medicaid expansion that occurred in 2003 and allowed approximately 9,000 residents to gain

[9] They report that the increase in inpatient stays is mostly not originating from the ED.

health insurance. This expansion was targeted at low-income, uninsured and non-elderly adults with chronic health conditions. Both studies used administrative claims data from 2008-2009. Burns et al. focus on the population of rural adults while DeLeire et al. (2013) focus on adults from all areas.[10] DeLeire et al. (2013) found that outpatient visits increased by 29 percent, emergency department visits increased by 46 percent, inpatient hospitalization decreased by 59 percent and preventable hospitalizations decreased by 48 percent. Burns et al. (2014) found that obtaining public health insurance eligibility increased the likelihood of outpatient visits by 39 percent, preventative services by 93 percent (i.e. physical check-ups, health education, and smoking cessation), and inpatient visits by 124 percent.[11] The expansions from New York, Maine and Arizona were studied by Sommers et al. (2012). They compared the expansions in these states to neighboring states and found that Medicaid coverage increased by 2.2 percentage points and that the expansions were associated with a reduction in all-cause mortality for older, non-white, lower income individuals. They also find reduced rates of delayed care and increases in "excellent" and "very good" ratings of self-assessed health.

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[10] DeLeire et al. (2013) use an individual fixed effect model to identify changes in outcomes within individuals over time, while Burns et al. use a regression discontinuity method to compare individuals who enrolled in the public health insurance program right before and after the date of last enrollment, which was an unforeseen date since the enrollment was supposed to continue after that date.

[11] In both cases, their sample is not representative of the average uninsured person. In DeLeire et al. (2013), the authors do not have a control group made of individuals who did not gain coverage. This means that part of their estimated effect might be driven by reasons unrelated to changes in health insurance coverage.

Another recent study examines the effects of an insurance expansion for childless adults, despite that it is not a public health insurance expansion per se. Barbaresco et al. (2014) use a provision from the ACA (in effect since September 2010) which extends the permissible age for individuals to remain under their parents' health insurance plan to age 26. They use a difference-in-difference approach in which the treatment group is composed of 23-25 year-olds (right below the age cutoff) and the control group is made up of 27-29 year-olds. The authors found that this mandate increased the likelihood of having health insurance, having a primary care doctor, and reporting excellent health. They also found that the provision decreased the likelihood of being unable to afford medical care and receiving a flu vaccine.

2.2. Studies on the effects of losing insurance coverage on health

In contrast to the numerous studies of gaining public insurance eligibility, I am aware of no published work in economics on the health effects of losing public health insurance eligibility.[12] In a recent working paper, Ghosh and Simon (2015) use the same TennCare reform I study here and investigate its effects on inpatient hospitalizations. They find that that the disenrollment decreased the share of

[12] Recently, Garthwaite et al. (2014) studied the effects of the 2005 TennCare disenrollment on employment and labor force participation. Using the Current Population Survey, they found that the reform was associated with a 4.6 percentage point increase in employment for childless adults. This effect was stronger for jobs providing employer health insurance and for individuals working more than 20 hours a week. Their results suggest that if individuals were able to obtain health insurance independently from their employers, some of them would leave their jobs, work less hours, or exit the labor force. In addition in Garthwaite et al. (2015), the authors used the Tennessee reform to study the effects on the disenrollment on uncompensated care provided by hospitals. They found that the disenrollment caused an increase of $138 million dollars in uncompensated care.

hospitalizations covered by Medicaid by 21 percent. They also find a 75 percent increase in the uninsured hospitalizations originating from emergency department visits. They report that uninsured hospitalizations increased for both avoidable and unavoidable conditions, which does not suggest lack of preventive care. They find suggestive evidence of decreases in inpatient stays. This research complements my findings on the effects of the disenrollment; I not only study the effects of the disenrollment on the sample of inpatient hospitalizations but also provide evidence of the effects for the overall population using two population-based representative datasets.

In the medical and health policy fields, there are a several additional studies on people losing health insurance. Hearvin et al. (2011) compared emergency department (ED) visits in Tennessee before and after the disenrollment controlling for state linear and non-linear trends. Using administrative data from hospitals, they found that the overall number of outpatient visits decreased while the share of uninsured individuals visiting EDs increased. In my paper, I find increases in the number of visits to the ED as opposed to decreases. Since they do not provide a control group to compare Tennessee's outcomes, it is possible that their estimated effect reflects both changes from the TennCare reform and the regional trend decline in ED visits that was occurring around the same time.

Lurie et al. (1984; 1986) explore the effects of a contraction of California's Medicaid expansion program in 1982. California cut public health insurance eligibility for 270,000 medically indigent residents and transferred the funds to subsidize the medically indigent's cost of care in county health care facilities'. However, counties were not obliged to provide free care. Lurie et al. (1986) perform a survey of 215 individuals, of which 186 were affected by the disenrollment and rest were part of a control group. They found that the population affected by the disenrollment had higher levels of uncontrolled hypertension and lower access to care six months after the disenrollment.

Oregon went through a reform in 2003 that was similar to the one in Tennessee. The reform included a stricter premium payment policy, cutbacks on benefits, increases in premiums, and the introduction of co-payments resulting in individuals losing their public coverage. Carlson, et al. (2011) studied the effects of this reform. They collected their own survey data, eight and ten months after the reform.[13] They found that 31 percent of respondents reported losing public coverage and remaining uninsured, while another 15 percent reported continued disrupted coverage. Those who remained uninsured were less likely to have a primary care visit and more likely to report unmet health care needs than those who had continuous coverage.[14] A potential concern with this study is that technically the state did not terminate eligibility. Individuals chose to leave the program, which implies that the comparison

[13] They used the data to compare three groups: those who were not affected by the reform, those who lost it but reacquired it, and those who lost it and remained uninsured through their period of analysis.
[14] Those with disrupted insurance coverage had similar effects which were smaller in magnitude.

groups could have unobserved characteristics that are correlated with the health outcomes under study, thus potentially biasing the estimated effects.

In 2005, Missouri also undertook a health reform that involved Medicaid cutbacks. This reform resulted in approximately 100,000 residents losing Medicaid coverage while others faced reduced benefits and higher cost-sharing. Zucherman et al. (2009) studied this reform using a combination of administrative data and interviews with providers and managers. Comparing outcomes before and after the reform (i.e. a single differences) they found an increase in the number of uninsured, an increase in uncompensated hospital care and a decrease of hospital revenues.[15]

There are also some relevant studies on the effects of losing health insurance that are not about losing public health insurance per se. For example, Anderson et al. (2012, 2014) have a two papers that studied individuals aging out of their parents' health insurance plans at the ages of 19 and 23. In both cases, they found a decrease in ED visits, with a larger effect on the older group. For the younger group, losing health coverage led to a 40 percent reduction in ED visits. For the older group, it led to an approximately 88 percent reduction. They explain that the disparity is due to the fact that individuals at age 19 have lower socioeconomic status which makes them more likely to be covered by a means-tested program while those at age 23 are typically not in school and are not working in jobs that provide health insurance.

[15] They also found that community health centers were "forced" to apply for larger state grants and increase their prices.

Overall, the existing literature on the health effects of public insurance eligibility expansions has found a positive relationship between health insurance and health care access as well as self-assessed health, although the mechanism for the latter outcome has not been clearly established.[16] There is mixed evidence on the effects for preventive care and ER visits.[17] This paper adds to this body of literature in economics and complements our understanding of the relationship between public health insurance and health.

3. Institutional Background

This section summarizes the context of the disenrollment that occurred in Tennessee. I describe a brief history of the program and the political context that led to the decision and timing of the reform. To an extent, people affected by the disenrollment were not necessarily aware if they would be disenrolled or when it would happen.

In the early 1990s a Tennessee state budget report projected a budget deficit of $250 million which was largely driven by increased Medicaid spending. In addition,

[16] For example, people could be stating they have better health because they are in a better financial status because of insurance rather than having improved clinical outcomes.

[17] Existing theory provides an ambiguous prediction on the effect of losing health insurance on health. On one hand, losing health insurance increases medical care costs and lowers demand for medical care could end up having a negative impact on health (Grossman, 1972). On the other hand, losing health insurance coverage can lead to changes in preventive care efforts and health behaviors that have positive effects on health (Ehrlich and Becker, 1972). Exactly the opposite effects are in place when an individual gains health insurance, but it is not clear that the magnitude of the effects needs to be symmetric. In terms of ED utilization, there is no clear ex-ante prediction on how losing health insurance would affect ED visits. It is possible that individuals who have had health insurance are more informed about how the system works and therefore would be less likely to use ED as their source of care. On the other hand it is possible that people who lose health insurance avoid going to the doctor long enough until it becomes an urgent enough situation for the patient to attend the ED.

a substantial part of the Medicaid funding (around $400 million) came from a special tax on hospitals and nursing homes and this provision was soon to end. This led Governor Ned McWherter to invoke a task force to identify three options for the state legislature. The three options were: 1) increase state taxes, 2) reduce health care or provider reimbursement rates, and 3) engage in a comprehensive restructuring of health care delivery and financial systems. Governor McWherter took this opportunity to push his vision of expanding Medicaid by pushing the third option to the state legislature. This third option would be a major overhaul of the way Medicaid was delivered and funded in Tennessee. This reform would become the beginning of TennCare.[18]

TennCare had two main goals: to control costs and to expand coverage. In order to control costs, the state decided to enroll its Medicaid recipients into managed care insurance plans. The idea was to transfer the federal and state payments for indigent care from hospitals to insurance coverage. In addition, new state taxes were created to help finance the expansion. The savings from transitioning enrollees to a managed care organization and the new tax income were then used to expand coverage

[18] The state legislature approved a federal waiver that authorized deviations from standard Medicaid rules. This waiver was part of a 5 year demonstration project. The credibility of Tennessee to have sustainable managed care depended on the participation of Blue Cross Blue Shield of Tennessee. The idea behind TennCare was two-fold: to control cost and expand Medicaid coverage. The first goal was to be achieved by enrolling all of their Medicaid recipients into a managed care insurance plans.

to uninsured individuals with incomes up to 400% of the federal poverty line and to those considered "uninsurable" by private insurance companies.[19]

Individuals who benefited from this expansion were mainly non-traditional Medicaid beneficiaries. Compared with traditional Medicaid recipients, the expansion group was more likely to be white, between the ages of 21 and 64 year old, and have higher income. This expansion allowed for childless adults, who had never been covered by Medicaid prior 1994, to be covered under TennCare. The enrollment into TennCare started in January of 1994. New enrollees had premiums based on their income level, though this did not deter applications.

By 2000, it was clear that the system was not sustainable, since health expenditures were rising faster than Tennessee's budget. Independent auditors recommended either reducing coverage, cutting benefits, or increasing taxes, but none of these suggestions were popular solutions.[20] In 2003, Democrat Phil Bredesen was elected as Tennessee's new governor. During his campaign, he promised to take care of TennCare's accrued debt. Although Bredesen assured Tennessee residents that he was going to work with the managed care organizations to find ways to cut costs

[19] To be considered "uninsured" in 1994, individuals had to be uninsured as of March 1, 1993; to be considered "uninsurable," individuals had to prove that they were denied private health insurance coverage (Moreno and Hoag, 2001).

[20] In 2002, a re-verification process started in which everyone under TennCare had to be re-verified for program eligibility. Most of the people who applied for re-verification continued to be covered under TennCare (Ruble, 2003). The information from the re-verification process was used to determine who was covered under the 1994 expansion and who was covered under traditional Medicaid. In addition, eligibility requirements were changed for the uninsurable category. A Medical review of "insurability" was required instead of the regular of denial of coverage from private insurers.

without dropping people from the program, in January 2005 Bredesen announced that a major disenrollment would happen that year, and that it would affect the people covered under the 1994 expansions.[21] By August 2005, individuals started receiving letters stating that their TennCare health insurance coverage was terminated. This disenrollment continued until May 2006; in total, about 170,000 residents were dropped from the program. Figure 1 shows the monthly TennCare enrollment and confirms there was a very large and sharp decrease in the TennCare enrollments during this time period.

4. Empirical Strategy

My research design compares changes in outcomes of interest between Tennessee and other Southern states before and after the reform. In addition, I use the fact that this reform targeted mostly childless adults to compare the differential in outcomes of adults with children and adults without children in Tennessee to the same differential in other Southern states before and after the reform.[22] These specifications allow me to interpret my results as the causal effects of the disenrollment on health outcomes.[23]

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[21] In fact, he told the press that people with disabilities and uninsurable status would still be covered.

[22] For comparison purposes, the percentage of adults with no dependents who were affected by the Massachusetts health care reform and the Oregon Health Experiment was around 50%. Kenny et al. (2012) predict that the ACA expansion group will be composed of 82.4% childless adults.

[23] I also explored as control groups states that border Tennessee and states selected by the standard synthetic control method (Abadie et al., 2010); both yielded similar results. I use the definition of southern states given by the U.S. Census; this contains the following states: Alabama, Arkansas, Delaware, the District of Columbia, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, North Carolina, Oklahoma, Tennessee, Texas, Virginia, South Carolina, and West Virginia.

The first approach makes use only of the relative change in outcomes in Tennessee versus other southern states in a Difference-in-Differences (DD) model. Specifically, I estimate the following equation:

$$(1)\ \mathrm{Y_{ist}} = \beta_0 + \beta_1\left(\text{Post July 2005} \times \text{ TN}\right)_{st} + \beta_2 \mathrm{X_{ist}} + \delta_t + \alpha_s + \epsilon_{ist}$$

Each outcome Y is measured for individual i in state s, at time $t.$ Here, time is a month-year combination. $Post\ July\ 2005 \times\ TN$ is a variable that takes the value of 1 for individuals in Tennessee who reported outcomes after July 2005, and 0 for everyone else. The coefficient on this variable, β_1, represents the Difference-in-Differences treatment estimate of interest. I control for state fixed effects (α_s) and for year and month fixed effects (δ_t) which include year dummies as well as month dummies to account for any seasonality in outcome responses (i.e. the possibility of responding more positively during the summer months).[24] X_{ist} is a vector of individual level controls such as education, race, age, gender, and marital status. I estimate this specification for the full-sample but also for the sample of adults with children (who were not targeted by the reform) and the sample of adults without children (who were targeted by the reform). My identifying assumption is that outcomes in Tennessee would have evolved in the same way as other Southern states in the absence of the disenrollment conditional on observable characteristics.

[24] This is true in the BRFSS specification. In the NHIS specification I do not have information of month of interview for all observations and so I do not include this variable.

My second specification is the triple difference model which uses the fact that the TennCare disenrollment targeted childless adults. This model takes the form:

$$(2)\ Y_{igst} = \beta_0 + (\gamma_g \times \alpha_s) + \left(\gamma_g \times \delta_t\right) + (\alpha_s \times \delta_t) + \beta_1 \left(Post \times\ TN \times No\ Kid\right)_{\mathrm{gst}} + \beta_2 X_{ist} + \epsilon_{igst}$$

As in the DD specification, I index individual i, in state s, at time t and group g which indicates if the individual is a childless adult.[25] In this DDD specification the estimate of interest is the coefficient on the triple interaction $Post \times TN \times No\ Kid$, β_1. This interaction terms takes the value of 1 if an individual does not have dependents under 18 in the household, lives in Tennessee and is reporting outcomes after July 2005, and 0 otherwise. γ_g is a dummy variable that indicates the childless status of the individual (i.e. if they have dependents in the household or not). Thus, I include state, year, and childless status fixed-effects in the model as well as any two-way interactions between these three sets of fixed effects. This makes my estimates robust to any state-year (e.g. a state program that does not differentially affect childless adults vs. adults with children), state-childless (e.g. a Tennessee specific outreach to childless adults that is constant over time), and year-childless status (e.g. any national outreach campaign that affects childless adults) specific effects. In this case, my identifying assumption is that the difference between the two demographic groups (adults with children and adults without children) would have evolved similarly in Tennessee to the differential in other

[25] I defined a childless adult as an adult who lives in a household with no other member under the age of 18

southern states in the absence of the disenrollment. In other words, the two demographic groups are allowed to evolved differently from each other, but the differential between these two groups would have evolved similarly in Tennessee to the rest of southern states in the absence of the disenrollment. For my estimates to be biased in the DDD, there has to be a trend or an event – around the time of Tennessee's disenrollment – that affects adults with children and adults without children differently and this pattern is not consistent across the control states. As an example, if we hypothesize that Medicaid premiums were changing in this period of time in southern states - with each state having different changes – then the effect of the premiums would also have to be different for adults with children and adults without children to bias my results.[26] I consider this specification to be more robust and have a weaker identifying assumption that the DD model; therefore, it is my preferred specification.[27]

To estimate appropriate standard errors, I use a modified version of block bootstrap developed by Garthwaite et al. (2014). Traditionally, I would need to account for serial correlation within states over time and this is usually done by clustering standard errors at the state level. However as MacKinnon et al. (2014) point out,

[26] As reviewed on the background of the reform, I am not aware of any other policies in Tennessee that affected childless adults and adults with children differentially around this time period.

[27] I also estimate models by changing the timing of the DDD variable to different starting points. For some outcome variables BRFSS asks if a procedure was done in the past 12 months. In these cases, I create a variable that represents the number of months an individual is exposed to the reform by accounting for the months lapsed between disenrollment and the interview. It takes a fractional value, from 0-1. Separately, I aggregate the data at the state-year level and re-run the main specifications. The different specifications provided similar results to the ones presented in this paper.

clustering relies on the number of clusters being large. In this study the number of clusters is 17, and therefore the main assumption for Cluster Robust Variance Estimation (CRVE) becomes hard to justify. In addition, the percent of treated units matters for the finite sample properties of CRVE to hold. In simulations Mackinnon et al. (2014) show that this could lead to an over-rejection of the null hypothesis. In order to account for this issue, in additions to CRVE, I use a modified version of block bootstrap which is composed of a two stage sampling across states and within states. In the appendix, I use Monte Carlo simulations to test the finite sample properties of this method and to perform comparison across other standard error adjustment. I conclude that the modified version of block bootstrap has rejection rates closer to the appropriate value (using a p-value of 0.05, we would want 5% rejection rates).

Additionally, as it is becoming popular with single state interventions (Courtemanche and Zapata, 2014; Shah and Cunningham, 2014) I also implement the synthetic control method. This method was developed by Abadie et al. (2010) and is a generalization of the DD framework, it addresses the possible bias in a DD framework that comes from potentially not having a correct control group. Essentially, even if the control groups have parallel trends, there could be something inherently different about the control group that we are not able to observe which could end up biasing the DD estimates. To account for this, synthetic control uses a weighted subset of all possible controls, which is selected by matching to the treated group on pre-treatment dynamics.

When using synthetic controls the estimated effect is the difference between the outcome for the treated unit and the synthetic unit. To measure the causal effect I estimate:

$$Y_{1t} - \Sigma_{s=2}^{S+1} \omega_s Y_{st}$$

Here Y_{1t} represents the outcome of the treated unit, at time t, while ω_s stands for weights for all control states. These weights represent how much of each state in the control pool is contributing to the creation of the counterfactual outcome. Weights are calculated using a set of matching covariates which help determine how similar states are in the pre-treatment dynamics. An important thing to notice about this framework is that all the matching is made on observables and not unobservables.[28] Intuitively, if we are able to match the dynamics before treatment between the treatment and control group, then we will be able to predict what would happen in the absence of treatment, because we are assuming that nothing else changes.

For the analysis using administrative data on inpatient hospitalizations, I use a DD approach similar to the one presented above. This specification compares outcomes before and after the reform in Tennessee to other Southern states. Since I do not observe if the individuals who come in have children or not I am not able to use the DDD specification I proposed for BRFSS and NHIS. Hence I use the following model:

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[28] However Abadie et al. (2010) mention that when the number of pre-treatment periods is large, matching on pre-treatment covariates helps control for any heterogeneity of unobserved and observed factors on the outcome in addition to accounting for the unobserved factors that affect the outcome.

$$Y_{dhts} = \beta_0 + \beta_1\,(TN \times Post) + \gamma * X_{dhts} + \delta_t + \alpha_s + \rho_h + \epsilon_{hts}$$

Where Y is an outcome for a hospital discharge d, in hospital h, at time t, in state s. The estimate of β_1 provides the impact of the reform on outcomes. X_{dhts} is a vector of covariates that contains characteristics of the inpatient discharge such as age, age squared, sex, race dummies, number of diagnoses, dummies for quartile zip income level of the place where the inpatient lives and a set of inpatient risk adjusters.[29] In my specification, they serve as way to control for patient's health composition. In addition I include year-quarter fixed effects (δ_t), state-fixed effects (α_s) and hospital fixed effects (ρ_h). I use hospital fixed effects to account for the unbalanced panel nature of NIS; without hospital fixed effects the estimator could be capturing changes in the sample of hospitals across years. This is something to be cautious about since the data is not state-representative.

Identification under hospital fixed effects comes from within hospital changes in discharge outcomes before and after the reform compared to hospitals in other Southern states, allowing for national and state-specific linear trends.[30] The identification assumption is that outcomes of inpatients and hospitals in Tennessee

[29] These include comorbidities, and All Patient Refined Diagnostic Related Group (APR-DRGs) as well as All Patient Severity Diagnostic Related Groups (APS-DRGs). These measures are developed by an external organization that helps evaluate the patient before procedures are done and assigns a payment category given their health status and conditions.
[30] In an alternative specification I can estimate the model controlling for seasonality and year-quarter time trends, however this implies dropping Florida from the control pool since observations in Florida do not provide month of quarter date of admission. My preferred specification opts for including Florida since it represents 20 percent of the total sample I use.

would not have evolved differently from those in other southern states in the absence of the reform. Since uninsured individuals might avoid going to the hospital until a serious health event occurs, it is plausible that the pool of inpatients after the reform are relatively in worse health than the pool of patients before the reform and this could be driving changes in outcomes. However, in my preferred specification I do not control for this selection mechanism, as I am interested in the effects in the presence of this selection, since this is a consequence of the reform.[31] For estimation of standard errors I also use a modified block bootstrap procedure.

For all of the analysis above I study the period of 2000-2010, which allows to have enough pre and post periods of the reform to credibly identify its effects. However, following Garthwaite et al. (2014) I also perform my analysis using the 2000-2007 to avoid potentially confounding effects from the Great Recession on health outcomes (e.g. Tekin et al. 2013; Ruhm, 2000, 2002, and 2005; Cotti et al. 2014). For the recession to bias my estimates, the recession would have had to affect the differential of childless adults and adults with children in Tennessee differently than it

[31] However, for robustness checks I propose two empirical alternatives to account for this selection. Ideally we would like to have information on the health status of the patient before any procedures. The NIS offer a set of measures of group risk-adjusters that aid in holding patient's health composition constant. I then compare results with and without risk adjuster to understand the degree of selection. An alternative to tackling selection is using ICD-9 codes to identify groups of diagnoses that should not be affected by health insurance status (urgent procedures) versus procedures that are more likely to be avoided if one does not have health insurance or procedures that the patient can have some control on the timing (elective procedures). The idea is to identify health shocks that one cannot wait for medical attention, and therefore would end up in a hospital admission regardless of health insurance status. NIS provides a classification for each discharge on the type of "Urgency", I used this classification to compare discharges that are elective and non-elective.

did in other Southern states. Most of the results are robust to this alternative sample period. In the results section I point out which outcomes have different implications using the shorter period of time.

5. Data

In this section, I describe the datasets I used in my analysis. For population level outcomes I use two major datasets: the 2000-2010 Behavioral Risk Factor Surveillance System (BRFSS), and restricted versions of the 2000-2010 National Health Interview Survey with state identifiers.[32] To study the effects of the disenrollment on inpatient care, I use the 2000-2010 Nationwide Inpatient Sample.

5.1. BRFSS and NHIS Survey Data

BRFSS is an telephone survey that started in 1984. The survey includes information on a variety of self-reported health status and health behaviors as a monthly repeated cross-section. It also contains standard demographic characteristics such as age, race, marital status, education, and – importantly for my study – the presence of children in the household. The survey is administered by each state in collaboration with the Centers for Disease Control and Prevention (CDC), which compiles information into an annual dataset at the state level.

[32] I use the restricted version of the NHIS because the public version does not contain information on state of residence and time of interview.

NHIS is a cross-sectional household interview survey in which sampling and interviewing are continuous throughout the year. The survey contains detailed information on health insurance, health access and utilization of medical care. The data is collected by interviewers trained at the U.S Bureau of the Census and the survey is administered by the National Center for Health Statistics (NCHS) and the CDC. This survey asks questions about members of the household but it also contains a section called "Sample Adult" which selects non-institutionalized individuals over the age of 18 and asks them more detailed information on their health and health care access. I use outcomes from the Household file and the Sample Adult file. For each sample file I use the NCHS provided weighing adjustments.[33]

These surveys complement each other well. On the one hand BRFSS contains a large number of observations which can be identified at the state-month level. Also, BRFSS contains several questions on health behaviors and preventive care and the questions are consistent over the sample period as opposed to NHIS which only asks about certain health behaviors and preventive care in some years. On the other hand, NHIS has detailed questions on the type of health insurance (which BRFSS does not provide) which is critical information since the reform should have induced predictable changes in different types of coverage. In addition NHIS contains questions on health access, utilization of medical care, and health spending that BRFSS does not offer.

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[33] In BRFSS and NHIS, I exclude from the sample individuals age 65 and older since they are eligible for Medicare, and I also require individuals to be at least 21 years old. Individuals under this age could be covered under their parent's health insurance and will be less likely to be treated.

Since both surveys have their advantages and disadvantages they serve as useful complements of each other in studying the effects of the disenrollment.

I study five categories of outcomes: health care access, preventive care, health behaviors, self-assessed health and utilization of medical care. For health care access, I study three types of variables. The first variable is health insurance. Using NHIS I observe: having any health coverage, Medicaid, Medicare, Private Insurance or other type of health insurance.[34] In BRFSS, I construct a health coverage variable based on the question "Do you have any kind of health care coverage, including health insurance, prepaid plans such as HMOs, or government plans such as Medicare?" reporting "Yes" to this question is coded as one and "No" is coded as zero. In addition in NHIS, for people who report having no health coverage, the surveyors ask individuals to give reasons for not having insurance and one of the options is "Losing Medicaid". I use this variable as direct evidence of the disenrollment. The second margin on health care access derives from a question that is similarly worded in BRFSS and NHIS: "Was there a time in the past 12 months when you needed to see a doctor but could not because of cost?" I assign the value of one if the response is yes and zero otherwise. In NHIS, I also use the question "During the past 12 months, has

[34] Although TennCare was considered to be an extension of Medicaid, it is possible that some people thought they had private health insurance even if they had TennCare. In the Appendix B I include an example of a TennCare report card, which illustrated how an individual could confuse their reporting of TennCare with private health insurance. Given the existing literature which raises the issue of misreporting on types of health insurance (Lynch et al. 2003), it is possible that a cleaner measure of the reform is a variable measuring having health insurance or not, which can be found both in NHIS and BRFSS.

medical care been delayed for {person} because of worry about the cost? (Do not include dental care)" as a measure of access to medical care. Intuitively, losing health insurance means higher costs for most medical care, and therefore I expect an increase in the number of occasions that individual decides to forgo or delay medical care due to cost. The third margin derives from questions about seeing a doctor. The NHIS asks questions regarding seeing a variety of specialist doctors (e.g. pediatrician, mental health professional, ophthalmologist, etc.) as well as a general doctor. In BRFSS, I use the question "Do you have one person you think of as your personal doctor or health care provider?" to study the effect of the disenrollment on seeing a doctor and reporting having a doctor as measures of health care access. I expect a decrease in this outcome as well.

For preventive care, I have a total of 8 outcomes, all of which derive from questions of the following kind: "In the past 12 months have you had a (Preventive Test) ?" I assign the value of one if individuals responded "Yes" and zero if individuals responded "No".[35] For questions about preventive care that are gender and age specific, I define the variables only for those that are recommended by the United States Preventive Services Task Force (USPSTF). These are having a mammogram for women over 50, having a breast exam for women over 21 and having

[35] I also create index variables that summarize the information from each separate question of preventive care. I construct the index using the method proposed by Anderson (2008).

a Pap test for women that are over 21. For men, I code having a PSA test for men over 40 and having a rectal exam for men over 40 as well.[36]

For health behaviors, I use BRFSS and NHIS questions on alcohol consumption, smoking, consumption of fruits and vegetables, and exercise. I create a variable named "Physical Activity" which takes the value of one if the individual answered "Yes" and zero if the individual answered "No" to questions of performing more than 10 minutes of vigorous physical activity.[37] There are also several questions regarding consumption of fruits and vegetables. I use the information from these variables to create a variable representing the average number of daily servings of fruits and vegetables. For drinking alcohol, I report three variables. "Binge Drinking" is coded one if the individual reported having 5 or more drinks in one occasion in the past 30 days. "Any drink in the past 30 days" takes the value of one if the individual reported having at least one drink of any alcoholic beverage in the past 30 days. Finally, I use a self-reported average of number of alcoholic drinks per occasion to create "Drinks per occasion in the past 30 days" variable. I also created a variable named "Currently a smoker" which takes the value of one if an individual reported that currently he is smoking either every day or some days, and it takes the value of zero otherwise.

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[36] I do not study colonoscopies because the question was introduced in 2004.
[37] In BRFSS "During the past month, other than your regular job, did you participate in any physical activities or exercises such as running, calisthenics, golf, gardening, or walking for exercise?" and in NHIS is "How often do you do vigorous leisure-time physical activities for at least 10 minutes that cause heavy sweating or large increases in breathing or heart rate?" and similarly for moderate activity.

For health outcomes I use questions in both BRFSS and NHIS regarding self-rated health and number of days being sick.[38] The first question asks individuals to rate their health from 1-5, 5 being excellent and 1 being a poor level of health. I use as outcomes the probabilities of reporting each level. Each variable takes the value of 1 if in the respective category and 0 if not.[39] Second, I use a question that asks individuals to report the number of days they had bad physical health (BRFSS), bad mental health (BRFSS), and any type of bad health that prevented them from performing daily tasks or made them miss work (BRFSS and NHIS). I use each outcome in two ways. First, I use the raw variable reporting the number of days. Second, I create a variable that takes the value of one if they reported a positive number of days and zero otherwise.

For utilization of medical care, I use questions from NHIS regarding changes in the usual place of medical care, place of care the respondent goes when sick, number of times in the Emergency Department, and number of times spent overnight in a hospital.

In Table 1, I present summary statistics for Tennessee and other Southern states using BRFSS. Most of the variables across both groups are similar and not statistically different from each other. Notably, the health insurance rate was 8 percentage points higher in Tennessee than in other southern states before the reform;

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[38] The advantage of using self-assessed health is that it encompasses all the potential health related problems, including those that a physician may not observe.

[39] I focus on the extreme ratings since recent evidence by Greene et al. (2015) mention that the middle ratings are usually inflated. In addition, there has been recent research that has suggested using ordered probit for this specific outcome (Contoyannis et al. 2004) I have estimated these models as well and the results are similar to the linear probability model.

this difference reduces to 4 percent after the reform. Tennessee has less reporting of forgone medical care and higher reporting of people having a personal doctor, which is consistent with the higher insurance rates.[40] In Table 2, I present summary statistics for Tennessee and other southern stats using NHIS. [Fill in]

5.2. NIS Inpatient Data

I expand my analysis of changes in inpatient stays by using the Nationwide Inpatient Sample (NIS). These data are a nationally representative database developed by the Healthcare Cost and Utilization Project (HCUP) that is the largest publicly available all-payer inpatient healthcare database in the U.S. These data contain information on inpatient discharges from community hospitals. Given the period used in this analysis (2000-2010) the design of NIS contains the universe of discharges from a sample of community hospitals. [41] The sample of hospitals aims at representing 20 percent of a stratified sample of all U.S community hospitals. This amounts to 5 to 8 million hospitals stays each year coming from about 1000 hospitals. The NIS provides information on both hospitals (location, teaching status, size, ownership type, number of discharges, etc.) and patient discharge characteristics (payment type, diagnosis,

[40] In Appendix Table 1 I show a table of summary statistics for demographic characteristics. Comparing pre-reform means across Tennessee and other southern states the difference that is most stark is the racial composition of Tennessee (much less Hispanic than other southern states) and the percent of high school graduates and some college is bigger in Tennessee than other southern states. I take into account this observables difference by controlling for race and levels of education for each individual in my regression specifications.

[41] Community hospitals are all non-federal, short-term general, and other specialty hospitals, excluding hospitals units of institutions. Ninety percent of all hospitals in the U.S are considered community hospitals. Examples of non-community hospitals are hospitals for prison inmates or veterans' hospitals.

length of stay, cost of stay, admission type, etc.). Notably it also contains month of admission, which is particularly useful since the reform was implemented over the course of 8 months (August 2005 to May 2006). Therefore given the relatively large sample sizes in each monthly bin, these data allow me to identify changes at the monthly level and to compare outcomes before the reform, during the reform and after its full implementation.

There are two major limitation with these data. First, they are designed to be nationally representative, but are not designed to be representative at the state-level. Second, NIS contains data on inpatient visits. Regarding these limitations, this paper offers analyses of the reform with other population-based outcomes, which complements the results found with the NIS data.

For this analysis, I use all discharges from hospitals for years 2000 through 2010 that are located in the South, as defined by the U.S Census. Since not all states report to the HCUP database, this pool of states is composed of Arkansas, Florida, Georgia, Kentucky, Maryland, North Carolina, South Carolina, Tennessee and West Virginia.[42] I exclude patients over the age of 65 and under the age of 20, since both of these populations were not directly targeted by the reform. In Table 3 I provide statistics that describe the NIS data. The final sample consist of 1583 hospitals over the span of 11 years (2000-2010), this equals a total of 3,155,042 of inpatient discharge

[42] I also estimated the analysis excluding Virginia, Oklahoma and Texas. The first two states come in and out in the sample over this period. Texas has non-trivial differences in the way they report it reports its outcomes compared to the rest of the states.

records. The average appearance per hospital is 3.8 times over the 11 years, hence it is an unbalanced panel. Comparing Medicaid rates before and after the reform in Tennessee I find a 5 percent decline, while in other Southern states I find a 10 percent increase. I also find there is a decrease in the percentage of admission coming from the ED in Tennessee of about 2 percent, which is much smaller than the 11 percent decrease experienced by other Southern states.

6. Results

In this section I describe the main findings of the effects of the disenrollment on health. I show that the disenrollment decreased overall health insurance rates, which in turn decreased access to care. I then show decreases in health status and how changes in the places where people obtain care.

6.1. How did the disenrollment affect health insurance rates?

I begin by providing evidence of the reform, namely that there is an increase in reporting having "lost Medicaid". I complement evidence from this outcomes by reporting the effects of the reform on having Medicaid and any health insurance.

To show graphically the effect of the reform, I use BRFSS data to plot a graph of health insurance rates.[43] Figure 2 illustrates that before the disenrollment the two demographic groups within each state move similarly, but once the reform occurs the group of childless adults in Tennessee majorly diverges from the group of adults with

[43] I use BRFSS since it has a larger sample size and reporting is in monthly bins

children in Tennessee. Notably, there is no divergence between childless adults and adults with children in other Southern states.[44] After 2009 there is another visible drop in the health coverage rate for childless adults possibly driven by the recession. In which case the $year \times childless\ status$ coefficient should take into account any national trend that affects childless adults and adults with children differently for each year. Relatedly, the results on health insurance rates are consistent when I restrict the sample to end in 2007.

In Table 4, I present the results from different specifications using data from BRFSS and NHIS to provide evidence of the disenrollment. The first three panels are specifications based on the DD models and the bottom panel is based on the DDD. The columns represent different outcomes: the first three columns are outcomes from NHIS while the last column is an outcome from BRFSS.

I first look at the effects of the reform on the likelihood of reporting having lost Medicaid. I take this as the most direct evidence of the reform. The estimate in the first row and column of Table 4 is the DD estimator for the full sample, which indicates that the TennCare reform increased the likelihood of reporting having lost Medicaid by 1.1 percentage points. I then proceed to estimate the DD by the sample of adult with children and adult without children. I expect the effect to be mostly driven by the sample of childless adults. When I estimate the same model using the sample of adults with children (second panel) and childless adults (third panel) it is

[44] Since the sample size for NHIS is smaller the data contains more noise.

noticeable that the DD effect for the full sample is driven by the sample of childless adults. In the DDD model, I estimate a significant 1.8 percentage point increase in the likelihood of report of losing Medicaid. This represents a 128 percent increase over the pre-reform mean.

Since not everyone gets asked the question on losing Medicaid, I use reporting on having Medicaid. The second column provides the estimates on reporting having any Medicaid as health coverage. In the DD full sample model, I estimate a 2.8 percentage point reduction in the probability of reporting having Medicaid. When comparing across sub-samples for the DD, the full sample effect is mainly driven by the sample of childless adults. The DDD specification estimated a significant 2.7 percentage point reduction in the probability of reporting Medicaid, which represents a 30 percent reduction over the mean. These findings confirm a strong treatment effect of the reform.

Since it is possible that people who were dropped could have been able to obtain other sources of insurance, in columns 3 and 4 I estimate the effects of the reform on overall health insurance rates. In both datasets, when comparing the DD estimates by sub-sample it is clear that the effect is mainly driven by the sample of adults without children. Focusing on the DDDs, using the NHIS I estimate a statistically significant 4.5 percentage point increase in the likelihood of reporting being uninsured, which represents a 32 percent increase over the mean. Using BRFSS, the DDD specification estimates a 1.7 percentage point reduction in the probability of having any type of insurance, which represents a 2 percent reduction over the mean. Both of these estimates are statistically significant.[45]

In 2004, childless adults represented 52% of all adults in Tennessee between the ages of 21 to 64. Using the estimates from the DDD models I find a decline in health insurance rates of 4.5 (NHIS) and 1.7 (BRFSS) percentage points. These effects translate into approximately 34,000 to 97,750 residents – about 20 to 57 percent of people losing eligibility - who did not get other types of coverage.[46]

Finally, I present evidence of the existence of the reform using the Synthetic Control Method for health insurance. I present the results using BRFSS since it has a larger sample size.[47] I graphically present the main results in Figure 3. For this

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[45] In the appendix I explore how the reform affected the probabilities of reporting other types of health insurance. Using NHIS I find evidence of increases in reporting private insurance for the DD, but this effect is small and statistically insignificant. Using the DDD I estimate a 3.21 percentage point decline in reporting private coverage. This effect is statistically significant using the clustered standard errors but is not statistically significant using the block bootstrap p-values. It is possible that since people under TennCare were covered by managed care it is possible that individuals were reporting losing private health insurance as opposed to Medicaid when ask the question about their insurance. In Appendix B I provide a TennCare card example which can illustrate the confusion when reporting health insurance. If this hypothesis is true I should find changes in private-payment types in NIS data, since these are records that come from the hospital administrative data and therefore are less likely to be contaminated the confusion in reporting. Using NIS data, I do not find a significant change in the rate of private payments. I also find a reduction of 0.01 percentage points in reporting having Medicare. In the NHIS results it is statistically significant at the 5 percent level. I also present results for other reasons for not having health coverage. Most of the effects are not statistically, significant and the largest coefficient is for the losing Medicaid outcome.

[46] These estimates are larger than those provided by Garthwaite et al. (2014): they estimated a decline in public coverage of 3.6 percentage points in their DDD specification and 4.6 percentage points in their DD model. However it is expected that their estimates are different since they are estimated from the differences between being publicly covered and having no coverage. They estimated the crowd-out effect, the ratio of the decrease in public coverage to the increase in private coverage, to be about 36.2%. Using Garthwaite et al. estimates on private coverage, their results imply a decline in overall health insurance coverage of 2.9 percentage points for the DD model and 1.4 percentage points for the DDD specification.

[47] The results for this method using NHIS can be found in the appendix, the results are similar.

estimation I use all states as my donor pool, however I have also restricted the donor pool to Southern states as I do in the DD framework. The results are similar and can be found in Appendix C. The synthetic Tennessee's outcomes diverge significantly from actual Tennessee's outcomes after 2005, the year of disenrollment. The estimated effect of disenrollment on health insurance coverage is a reduction of 3.48 percentage points. This estimate is higher than the one obtained from the DD analysis but strengthens the evidence on the effects of disenrollment.

For the rest of the paper I only show results from my preferred specification, the DDD. In addition I will estimate this specification using the full-sample and using a sample of low-educated individuals (high school degree or less) and high-educated individuals (above high school degree). The idea behind the sub-sampling is that the population of low educated individuals would be more likely be affected by the reform since they are more likely to have income under 400% of the federal poverty line. In Table 5, I estimate the results using the sub-samples of low and high educated. I estimate that the probability of having Medicaid falls by 4.8 percentage points among low educated while for the high educated sample it falls 1.5 percentage point. This corroborates the reform having a higher impact on the low educated sample than its counterpart.

6.2. Mechanism of the Losing Health Insurance of Health Status

This section studies the effects of the disenrollment on health care access, preventive care, and health behaviors. These are all potential mechanisms on how the disenrollment could affect health outcomes. I find evidence of decreases in health care access, increases in the likelihood of having a flu vaccine and suggestive evidence of reductions in risky health behaviors and increases in positive health behaviors. I do not find consistent evidence in changes in preventive care measures related to cancer detection.

6.2.1. Effects on Health Care Access

In order for losing health insurance to affect one's health care decisions and health it should be the case that losing health insurance reduces health care access as a result of increased cost. In this section I document that the reform increased reporting of forgone and delayed medical care specifically due to cost. In addition I also provide evidence of a reduction in the probability of reporting seeing a general care physician.

Table 6 explores the effects of the reform on health care access. Panel A presents results for health care access variables from NHIS while Panel B resents results for variables in BRFSS. The first outcome is forgone or delay medical care due to cost. It represents the probability of not going to see a doctor when needed because of cost, which is one of the main mechanisms through which lack of insurance affects health.[48] The full sample DDD estimates an effect of 3.2 (NHIS) and 1.3 (BRFSS) percentage points. This represents a 30 to 10 percent effect over the mean, respectively. The estimates for BRFSS are statistically significant only using the cluster-

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[48] There is a potential problem with the timing component of this question, but given the set-up of the DD, we should still be able to detect effects. I have also tried a specification in which the DD is not binary but a fraction representing the possible amount of months treated by the reform, the results from this variation are similar to this specification.

adjustment while the estimates from NHIS are significant under both standard error adjustments. In column 2 and 3, I re-estimate the model using the sample of people with a high school degree or less and people with more than a high school level of education. Comparing the estimates from the sample of less educated to more educated, I find that the low-educated sample has a larger increase on the probability of reporting forgone medical care then the high-educated sample in both datasets. The probability of reporting forgone medical care among the low educated group increases by 4.2 percentage points in NHIS and 3.8 percentage points in BRFSS. Both estimates are statistically significant for both inference methods. When looking at the higher educated sample, I find an increase of 2.3 percentage points in NHIS and a very small (0.7 percentage points) decline in BRFSS, in both cases this effect is significant under the cluster standard error adjustment. I next study the effects of the reform on the probability of reporting not being able to afford prescription drugs. I find a 1.9 percentage point increase for the full sample and a 4.4 percentage point increase for the low educated sample. These estimates represent a 17 percent and 40 percent for the low educated sample.

Finally, I can study the effects on seeing a general care physician (NHIS) or having a general check-up in the past 12 months (BRFSS). In both datasets I find negative coefficients across the full sample and the breakdown by education. The effect is also larger for the low-educated sample than the high-educated sample, following the pattern observed in the previous health access variable. All of this effects are statistically significant under the cluster procedure but not significant under the block bootstrap procedure. For the full-sample, these effects translate to a 2.8 to 4 percent decrease in the probability of seeing a general care physician.

6.2.2. Effects on Preventive Care

Delaying or forgoing medical care can be problematic for individuals with chronic health conditions since the lack of medical checks can cause delay in treatment and ultimately increase health risk. Another potential implication of avoiding medical care is that one can have fewer opportunities for getting preventive care. I find evidence of increases in the likelihood of getting a flu vaccine. I do not find consistent effects of preventive care related to cancer detections and cholesterol checks.

In Table 7, I use BRFSS to study the effects of the reform on preventive care. In Panel A, I report outcomes reflecting preventive care of interest to the whole population such as receiving flu vaccine and having a cholesterol check. In Panel B I report on preventive care for women and in Panel C I report on preventive care for men over the age of 40.

I find that the disenrollment is associated with a statistically significant 2.7 percentage point increase on likelihood of having a flu shot, which represents an 8 percent increase over the mean.[49] This effect is consistent over the low and high educated sample, however it is larger and statistically significant only for the high educated sample. Although this results might be counterintuitive, it could reflect moral

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[49] In a result not reported the same outcomes variable in NHIS had a coefficient of 2.9 percentage point increase with a 9 percent increase over the mean

hazard. Since people are losing health insurance, they are more likely to invest in low-cost prevention such as the flu shot since the expected cost of getting the flu are higher without health insurance. Barbaresco et al. (2015) found decreases in having flu shots when individuals gain insurance under the ACA dependent coverage provision. I also find negative effects on the probability of having a cholesterol check, although this effect is only statistically significant using the clustered standard errors in the full and high-educated sample.

In Panel B I study the effects on preventive care for women. The age reference in each outcomes follows the age recommendations for each preventive care measure provided by the USPSTF. Most of these coefficients are relatively small in size and not statistically significant. I only find statistical significance using the clustered standard error for the coefficient on breast exam, which represents a decrease of 2.3 percentage points or a 3 percent increase over the mean.

In Panel C, I study the effects on preventive care for men over 40.[50] I find that the disenrollment is associated with a 6.3 percentage point reduction in having a (Prostate-specific antigen) PSA exam for the low educated sample, which is significant using the cluster procedure. I also find a 4.1 percentage point increase in having a PSA exam for the high educated sample: again, this coefficient is only significant using the cluster procedure.

[50] The reference age was the only possible reference since that this is how it was asked in BRFSS.

6.2.3. Effect on Health Behaviors

The results provide mixed evidence on the effects of the disenrollment on preventive care. The only consistent significant result is the increase in having a flu shot which could be an indication of moral hazard. The idea is that when individuals lose health insurance they will be more likely to adopt behaviors to improve their health, since negative health shocks can induce costs that are no longer mitigated by health insurance. A possible way to investigate presence of moral hazard is to study health behaviors.[51] In Table 8, I study the effect of the disenrollment on health behaviors using BRFSS outcomes. The first panel shows a summary measure that includes information of risky and non-risky health behaviors. Following a methodology in Anderson (2008) that helps correct for multiple inference, I create an index which higher scores represent engagement in more positive health behaviors or less risky health behaviors. I study each behavior separately in Panel B and C. The estimates from panel A indicate that for the full sample there is an increase an overall improvement in health behaviors but this effect is not statistically significant. For the low educated sample it is also positive and statistically significant. For the high educated sample I find that the reform is associated with a statistically significant decline in the index. The size of this effect can be interpreted as changes in a z-score measure. Taken together, the results from Tables 7 and 8 are consistent with and suggestive of the presence of moral hazard for low educated individuals but not for

[51] Carpenter and Tello-Trillo (2015) provide evidence of moral hazard using health behaviors from the same data.

high educated individuals. It is important to note that changes in health behaviors need not necessarily be consistent with only a moral hazard argument since reduction in smoking and drinking could be driven by changes in the current budget constraint that were driven by lack of insurance.

6.3. Are people getting sicker?

In this section I study how the reform affects health status. In order to understand the effects of the disenrollment on population health I study two measures of population health: self-rated health and days with some sickness.

Arguably the main disadvantage of self-rated health is that it is a subjective measure and it could be representing changes in the individual's well-being rather than clinical health outcomes. However it also encompasses all potential health problems observed by individuals and not observed by a physician. Previous studies have shown that this measure correlates with objective measures of health outcomes (DeSalvo et al., 2005; Idler et al., 1997) and mortality (Bound, 1991; Burstrom and Fredlund, 2001; Mossey and Shapiro, 1982). In addition, this is a widely studied measure of health which helps compares estimates across policies. Table 9 presents the results on probability of reporting excellent health, and reporting fair or poor health.[52] In Panel A, I study the outcomes using NHIS. I find that the disenrollment is associated with an increase in

[52] The probit version of this specification as well as the ordered probit are available in the online appendix, all of the results presented in here have the same interpretation as those found in the non-linear models.

the likelihood of reporting excellent health of about 2.6 percentage points for the full sample and it is of similar size and sign for the low and high educated sample. These effects are statistically significant using the cluster procedure but not the bootstrap procedure. In addition, I estimate a decrease in the probability of reporting fair and poor health of about 1.4 percentage points for the full sample and more than double for the low educated sample. Similarly, these coefficients are only significant under the cluster procedure. In contrast to these results, using the BRFSS I estimate a decrease in the probability of reporting excellent health of 0.5 percentage points, and an increase in the probability of reporting fair and poor health of 0.9 percentage points. These estimates are significant using the cluster procedure but not the bootstrap procedure. Even though the effects do not seem to be significant, the magnitudes and signs of the coefficients across both surveys are puzzling, especially given the consistency across the first stage. As I do with all outcomes, I have estimated the specifications with the set of years 2000-2007 to account for any potential confounding from the recession. When I estimate these outcomes on those set of years, the coefficient on reporting excellent health for NHIS becomes negative (and remains insignificant) across all of the samples. However the coefficient in reporting fair or poor health remains negative. It is possible that the reporting of this measure is being affected by conditions of well-being that are not related to health.[53] Given the inconsistency of

[53] Another hypothesis could be that individual know less about their current health status since they have stopped going to the doctor and this is why we observe improvements in health. The problem with this hypothesis is that this doesn't explain the difference between BRFSS and NHIS.

these results across surveys, I move to analyze arguably more "objective" measures of health such as number of days sick.

In Table 10 Panel A, I report estimates for the number of days over the past 12 months in bed due to sickness. In Panel B, I study the number of days over the past 12 months with bad physical health, bad mental health and days when the respondent was incapacitated. In Panel A, I find that the reform is associated with an increase of 0.6 days in the number of days in bed for the full sample, a 13 percent increase. This effect is much larger - 1.6 days - for the sample of low educated individuals, a 30 percent increase over the mean. These results are statistically significant with the cluster procedure but not the block bootstrap adjustment. In Panel B, I find evidence of a small and insignificant decrease in bad physical health for the full sample but a 7 percent increase for the low educated sample, which is significant using the clustered standard errors. I also find positive coefficients on days of bad mental health. Finally, I find statistically significant increases in the number of days incapacitated: there is a 17 percent increase for the full sample, a statistically significant 25 percent increase for the low educated sample.

The evidence brought in Table 8 is much more consistent relative to the results on self-assessed health ratings and provides evidence of a decrease in health, especially for the low educated sample.

6.4. What kind of care do sick people use?

I now study how health care utilization changes after the disenrollment. I present evidence that people report changing their place of care because of their health insurance, as well as evidence that people increase reporting using the ED as their place of usual care. I then report increases in visits to the ED and decreases in the total number of nights stayed in the hospital. I further explore this decrease in number of nights in the hospital by studying administrative hospital data.

In Table 11 I use NHIS to study changes in place of usual medical care when sick. First I study if individuals change their health care place due to health insurance. I find that the disenrollment is associated with a 1.9 percentage point increase in the probability of changing their place of care due to health insurance. This effect is stronger for the low educated sample, I find a 3.9 percentage point increase. Both of these outcomes are statistically significant with any of the standard error adjustment. The second outcome is reporting having a usual place of care. I find a positive association across the three samples, however these effects are only statistical significant using the clustered standard errors. Given the results from these two outcomes, it is possible that individuals still consider they have a place of care, but that the place has changed. In Panel B, I investigate what type of places individuals report being their usual place of care. Focusing on the low-educated sample, I find that individuals are more likely to use clinics and Emergency Departments as their source of usual care and less likely to use the doctor's office. These effects are significant using the clustered standard errors. In contrast, for the high-educated sample, which may have more means of obtaining care, they are more likely to report clinics or the doctor's office as their source of usual care and less likely to report emergency departments as their source of usual care.

Changing place of care is a key part of the story on how health care utilization changes. In Table 12 I explore in more detail changes to health care utilization. In terms of Emergency Department use, I find consistent patterns with the previous findings. Low educated people are more likely to use the Emergency Department while high educated people are less likely to use it. For the low educated, I find a 6.8 percent increase in the probability of using the ED and a similar effect on the number of times in the ED. For the high educated I find a 9.5 percent decrease in the probability of using an ED and similar for the number of times in the ED.

Another margin to study health care utilization is to study procedures that are more complicated than just visiting the ED. These involve surgeries or procedures where the patient needs to stay overnight. The next outcomes in Table 12 focus on these outcomes related to more intensive procedures. I find significant effects under clustered standard errors for the full and low-educated sample, I find a 23 percent decrease in the likelihood of having a surgery for the full sample, and a 49 percent decrease for the low educated sample.

I then study the effect on the likelihood of having an inpatient admission. All the coefficients are positive but they are not statistically significant. I also study the outcomes of number of times being an inpatient in the past 12 months, I find for the full sample a reduction of about 10 percent, and a reduction of 44 percent for the low educated sample. Finally, I study the average number of nights spent overnight in a hospital. I find a statistically significant increase of almost 2 days for the low educated sample and a reduction of 2.8 days for the high educated sample. These results provide evidence that there are non-trivial differences across low and high educated samples, potentially since high educated individuals are more likely to have other means to get medical care.

6.4.1. Effects on Inpatient Visits

In order to complement the findings on utilization, I use the Nationwide Inpatient Sample to study the effects on the number of inpatient stays. The full sample from these regressions comes from discharges for individuals ages 21 to 64 in hospitals of Southern states for years 2000 to 2010.

I start by presenting evidence of the reform (fewer people reporting Medicaid) using NIS data. I can investigate the effects of the reform by looking at the type of payment the hospital received for each discharge. The six payment categories are: Medicaid, Medicare, private, self-pay, no charge and other types of payment. Since NIS provides information on primary and secondary payment information, I am able to analyze changes in payment composition as well as change in overall type of payments. The most direct prediction from the TennCare disenrollment would be a reduction in Medicaid payments for hospitals in Tennessee compared to hospitals in other southern states. In Table 13 I present the results of changes in payment structure using different payment measures. I estimate a significant 19 percent decrease in the likelihood of having providing Medicaid as the source of payment. For private coverage I also find a decrease in coverage of about 6 percent. In terms of self-payment, I see an increase in this category of about 30 percent. This provides further evidence that the composition of payments for inpatient stays was changing drastically after the disenrollment.

In Table 14, I present evidence of the effects of the reform on the number of discharges. These regressions come from analyzing the data at the hospital quarter level. I estimate that after the reform there are approximately 86 fewer discharges per hospital quarter, and this amounts to a 22 percent reduction over the mean. When dividing the effect into discharges by age groups, I estimate that 91 percent of this decrease is attributed to non-elderly adults. Using the sample of non-elderly discharges, I estimate a 43 percent decline in discharges. This complements the findings from NHIS that demonstrates decreases in the number of times a person has been an inpatient.

7. Discussion and Conclusion

In this paper I have provided the literature's first evidence on the effects of losing public health insurance eligibility on population health outcomes. I find that the 2005 Tennessee Medicaid disenrollment significantly decreased health care access by making people less likely to see a doctor. Since the doctor can recommend certain tests or check-ups, one would expect individuals to have less preventive care as a result, but

I do not find strong evidence for changes in preventive care with the exception of having a flu shot.[54] This could indicate that individuals are willing to invest in low-cost preventive care to avoid getting sick in the future since the expected cost of being sick has now increased. This indicates the presence of moral hazard and it is highlighted by the adoption of positive health behaviors (eating healthier and exercising) and reduction of harmful health behaviors (drinking and smoking). If individuals take better care of themselves this could be a channel through which the disenrollment can improve their health rather than decrease it.[55] In contrast, I also find evidence for decreases in health care access which can lead to negative health outcomes. Even with these two conflicting effects, I find evidence that the reform significantly decreased health. Since not everyone who lost public health insurance eligibility remained uninsured, the effects identified by the research design are average effects, which implies that there were some people for which the reform had a substantial negative impact on their health. To illustrate this possibility, we should think of an individual who had an episode of sickness. Their health will start to deteriorate, and the rate at which this happens can be accentuated by the fact that the individual avoids going to the doctor because of cost and not having access to prescription drugs. This potentially

[54] This is a mixed problem of precision and economic significance. For example for Mammograms in the full sample, the effect is about 1 percent decrease over the mean, but the confidence interval also include effects of about 10 percent. However for Pap Exams we can rule out effects bigger than 1.6 percent with the clustered standard errors

[55] Note that changes in health behaviors need not only to be explained by moral hazard argument, one could also hypothesize that losing health insurance could affect people's budget constraint which makes them less likely to drink or smoke. In either case, this is a mechanism of how losing health insurance could be affecting health behaviors and in turn health outcomes.

leads to a more severe decrease in his or her health. Once an individual decides to get medical care the place where he/she receives it may not be the same. I estimate that approximately 30 percent of individuals affected by the reform change their place of care specifically due to health insurance reasons. These individuals are less likely to report the doctor's office or HMO provider as their source of usual care and more likely to use either emergency departments, hospital outpatient department or clinics. This last finding refers to people reporting what is their source of usual care, but I also investigate if they actually use ED more. I find that the likelihood of going to the ED increases, about a 7 percent increase for the low-educated individuals. This increase of ED attendance occurs in the extensive and intensive margin. I also find evidence of decreases in the number of surgeries and the probability of having a surgery. This in accordance with the findings from the administrative data which I estimate to have a 40 percent reduction in non-elderly discharges. That is, individuals who have some leverage of choosing whether or not to get a procedure or the timing of the procedure would be less likely to get it because of the increased cost. This could imply larger negative health effects in the long-run or simply that the individuals could have had better health during the current period if they had health insurance.

These results are subject to some limitations. First, the survey outcomes are self-reported; however, there is substantial research that indicates self-reported health outcomes and objective health outcomes are strongly correlated. Also, I control for sources of reporting heterogeneity such as income, age and gender (Zierbarth, 2010).

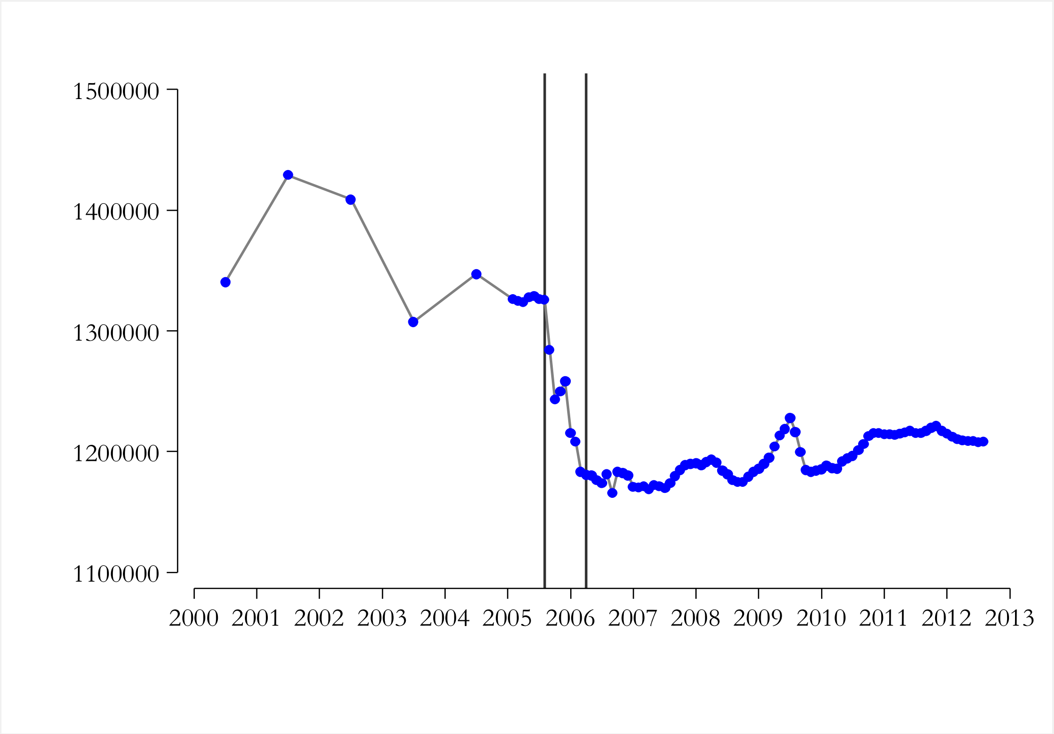
Second, I am not able to pin down all the mechanisms of how losing health insurance affects health other than forgone medical care and changes in health behaviors; it is possible that there are other mechanisms which I am not able to identify. For instance, given that the TennCare disenrollment increased employment (Garthwaite et al., 2014), it is possible that part of my effects can be driven by the gain in employment that the reform caused.[56]

Nevertheless, this is the first paper to study the effects of a sizeable public health insurance disenrollment on health care access, preventive care, health behaviors, health care utilization and self-assessed health. In doing so, I provide evidence of potential mechanism of how the disenrollment could affect health outcomes and subsequent health care utilization. Most of these results are consistent across two population representative surveys as well as administrative data on hospitalizations. In addition, my study focuses on a largely understudied population which ACA's Medicaid expansion explicitly targets: childless adults. My results provide evidence that losing health insurance is significantly detrimental for health care access and health. It also induces the use of ED and reduces the demand for surgeries. Further research should focus on other aspects of the effects of the disenrollment, such as the time they remain uninsured, more detailed information on the effects on prescription drugs and how losing health insurance affects the household consumption bundle. Finally, for

[56] For the purposes of policy-making it is important to highlight that the counterfactual world I am currently comparing is Tennessee without disenrollment and implicitly without its budget deficit. In a true counterfactual world, Tennessee would have taken an alternative action in order to deal with the budget deficit.

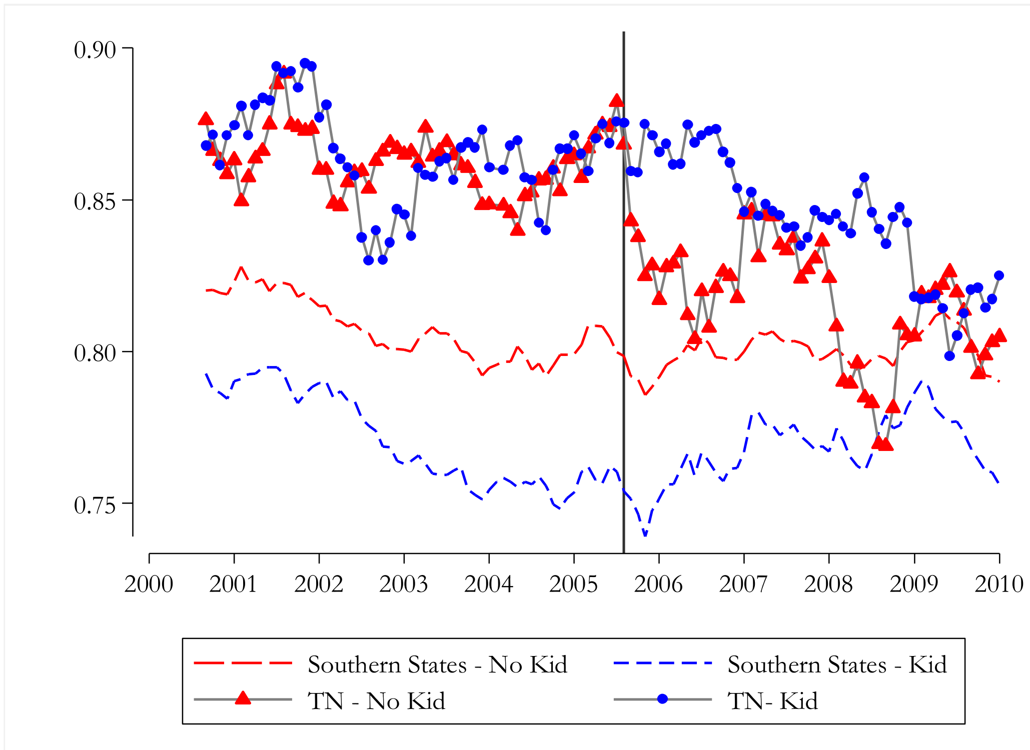
welfare analysis, another relevant set of outcomes to study would be the effects of the reform on the supply side of health (i.e. wages of health practitioners, their hours worked, etc.). This will help us have a more complete picture on the broad effects of this particular disenrollment, which eventually can help inform policy-makers when making choices about changes to public health insurance eligibility and other alternatives policies.

Figure 1: Number of People Enrolled in TennCare



Source: http://www.tn.gov/tenncare/news.shtml

Figure 2: Monthly Health Insurance rate



Notes: All lines are tailing 8-month moving averages, and for Tennessee-Nokid the trailing moving average is computed separately for the time periods after August 2005

Figure 3: Synthetic control method for health insurance rates using BRFSS 2000-2010

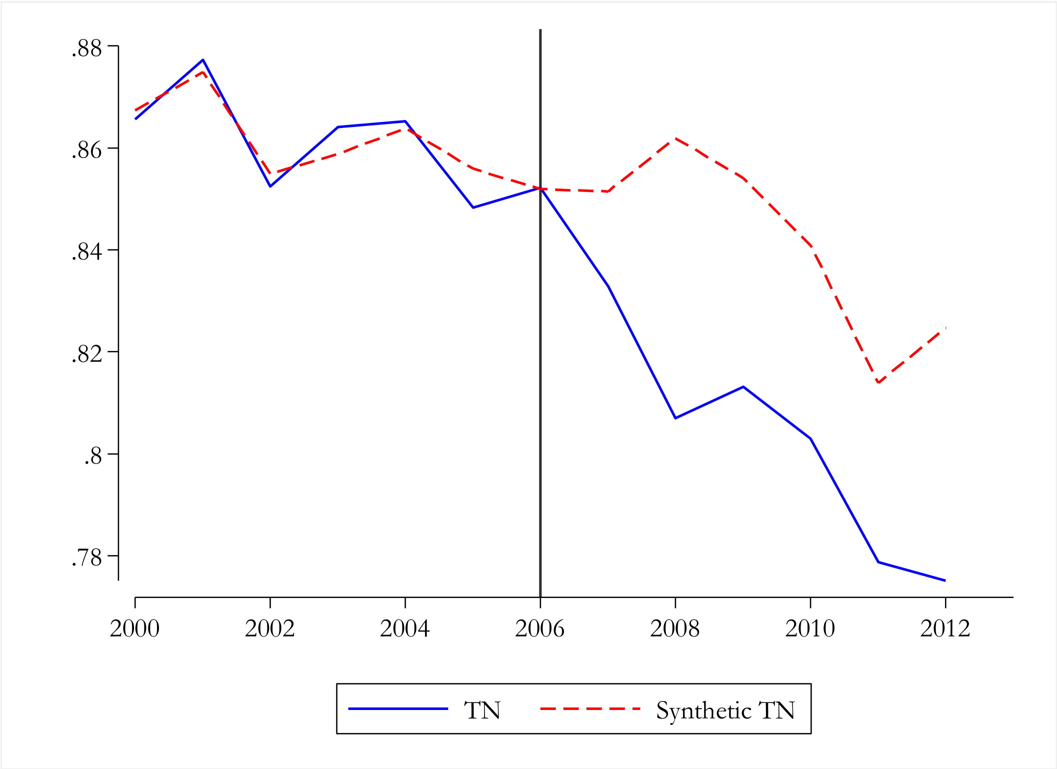


Table 1: Summary Statistics of Main Outcomes 2000-2010 BRFSS Data

	TN Before	Southern States Before	TN After	Southern States After	Total
Health Access					
Has Health Insurance	0.87	0.79	0.82	0.78	0.79
Forgone Medical Care in the past 12 months	0.13	0.17	0.19	0.19	0.18
Had a General Check-up in the past 12 months	0.76	0.69	0.74	0.67	0.68
Health Status					
Pr(Reporting Excellent Health)	0.21	0.23	0.19	0.22	0.22
Pr(Fair and Poor Self-reported Health)	0.17	0.15	0.18	0.15	0.15
Number of Days Physical Health Not Good	3.36	3.27	4.10	3.45	3.38
Number of Days Mental Health Not Good	3.42	3.63	3.66	3.82	3.72
Number of Days Incapacitated by Bad Health	4.34	4.03	5.90	4.51	4.32
Health Behaviors					
Participates in Physical Activity	0.70	0.74	0.72	0.75	0.74
Daily Servings of Fruits and Vegetables	4.21	3.61	3.74	3.67	3.66
Binge Drinking	0.09	0.16	0.10	0.15	0.15
Any Drink in past 30 Days	0.35	0.53	0.33	0.52	0.51
Avr. Drinks per Occassion is past 30 Days	0.81	1.32	0.76	1.29	1.28
Currently a Smoker	0.29	0.26	0.25	0.22	0.24
BMI Adjusted	27.73	27.51	28.69	28.25	27.88
Preventive Care					
Had Flu Shot in the past 12 months	0.28	0.24	0.34	0.31	0.28
Had a Blood Cholesterol check in the past 12 months	0.76	0.76	0.82	0.79	0.78
Had a Mammogram in the past 12 months for women over 50	0.68	0.65	0.65	0.64	0.64
Had a Breast Exam in the past 12 months for women over 21	0.74	0.68	0.67	0.65	0.67
Had a Pap Exam in the past 12 months for women over 21	0.75	0.69	0.66	0.64	0.67
Ever had a Prostate Specific Antigen Test for men over 40	0.53	0.55	0.52	0.59	0.57
Ever had a Rectal exam for men over 40	0.67	0.71	0.57	0.70	0.70
Chidless Status					
Currently Pregnant	0.04	0.04	0.03	0.04	0.04
No Children in the Household under age of 18	0.54	0.51	0.53	0.49	0.50
Number of Children	0.86	0.93	0.88	0.99	0.96

Table 2: Summary Statistics of Main Outcomes 2000-2010 NHIS Data

	TN Before	Southern States Before	TN After	Southern States After	Total
Health Access					
Uninsured Rate	0.14	0.23	0.20	0.25	0.23
Has Medicaid	0.09	0.04	0.08	0.05	0.05
Has Lost Medicaid in the past 12 months	0.01	0.01	0.03	0.02	0.02
Forgone Medical Care in the past 12 months	0.09	0.08	0.12	0.11	0.09
Has seen a doctor in the past 12 months	0.67	0.65	0.64	0.63	0.64
Health Status					
Pr(Reporting Excellent Health)	0.26	0.32	0.25	0.30	0.31
Pr(Fair and Poor Self-reported Health)	0.16	0.11	0.14	0.12	0.12
Number of Days Missed from Work	5.23	4.69	4.85	4.00	4.42
Number of Days Spent in Bed	7.11	4.86	6.98	5.26	5.15
Medical Care					
Change Health Care Place due to Health Insurance	0.03	0.03	0.03	0.02	0.03
Has an Usual Place of Care	0.86	0.82	0.84	0.79	0.81
Place of Usual Care					
Clinic	0.10	0.12	0.12	0.12	0.12
Doctor's Office or HMO	0.73	0.67	0.67	0.62	0.65
Emergency Department	0.01	0.01	0.01	0.02	0.01
Hospital Outpatient Department	0.00	0.01	0.01	0.01	0.01
Other Place	0.01	0.01	0.01	0.01	0.01
Health Care Utilization					
Visited the ED	0.23	0.21	0.22	0.21	0.21
Number of Times in ED in past 12 months | ED Visit==1	1.59	1.59	1.69	1.62	1.61
Had an Overnight Stay in the Hospital in the past 12 Months	0.09	0.08	0.08	0.08	0.08
Number of Overnights Stays	0.12	0.12	0.13	0.12	0.12
Average Number of Nights per Stay	3.45	4.11	3.96	4.28	4.16

Table 3: Summary Statistics of Main Outcomes 2000-2010 NIS Data

	TN Before	Southern States Before	TN After	Southern States After	Total
Payment Type					
Any Insurance	0.94	0.89	0.88	0.87	0.89
Medicaid	0.38	0.24	0.33	0.29	0.27
Medicare	0.20	0.16	0.24	0.18	0.17
Private	0.52	0.54	0.46	0.50	0.52
Self Pay	0.12	0.16	0.17	0.20	0.18
No charge	0.00	0.02	0.00	0.01	0.01
Other Type of Payment	0.02	0.05	0.02	0.05	0.05
Missing Information	0.01	0.00	0.01	0.00	0.00
Hospital Level					
Number of Hospitals	98	1060	91	1161	1583
Average Number of Discharges Per Hospital	23502	23815	20718	19983	22423
Average Total Charges in $millions	17,922	19,925	23,367	24,308	21,437
Number of diagnoses on this record	5.28	5.52	7.52	6.96	6.05
Number of procedures on this record	1.69	1.58	1.72	1.69	1.63
Discharge Type					
Emergency	0.44	0.47	0.47	0.45	0.46
Urgent	0.19	0.22	0.20	0.23	0.22
Elective	0.37	0.30	0.33	0.31	0.31
Discharge Admission					
ED Admission	0.47	0.45	0.45	0.34	0.43
Routine Admission	0.49	0.51	0.50	0.62	0.53
Transfer Admission	0.03	0.03	0.05	0.04	0.04

Table 4: Effects Disenrollment on Health Insurance Coverage Using BRFSS and NHIS 2000-2010

DD Model, All Adults	Lost Medicaid NHIS	Medicaid NHIS	Uninsured NHIS	Health Coverage BRFSS
TN X Post	0.011	-0.028	0.043	-0.031
	(0.002)	(0.002)	(0.004)	(0.002)
	{0.000}	{0.000}	{0.000}	{0.002}
	[0.012]	[0.000]	[0.000]	[0.000]
R-Square	0.03	0.07	0.17	0.16
N	193,086	193,086	193,086	841,757
DD model, sample with children < 18				
TN X Post	0.005	-0.009	0.013	-0.026
	(0.003)	(0.003)	(0.005)	(0.003)
	{0.115}	{0.008}	{0.019}	{0.003}
	[0.464]	[0.332]	[0.291]	[0.005]
R-Square	0.06	0.11	0.18	0.19
N	76,227	76,227	76,227	355,693
DD model, sample without children < 18				
TN X Post	0.014	-0.030	0.052	-0.036
	(0.001)	(0.002)	(0.005)	(0.003)
	{0.000}	{0.000}	{0.000}	{0.003}
	[0.001]	[0.000]	[0.000]	[0.000]
R-Square	0.01	0.07	0.15	0.14
N	113,610	113,610	113,610	485,175
DDD Model				
TN X Post X No Children under 18	0.018	-0.027	0.045	-0.017
	(0.003)	(0.003)	(0.003)	(0.002)
	{0.000}	{0.000}	{0.000}	{0.000}
	[0.012]	[0.015]	[0.003]	[0.089]
R-Square	0.04	0.08	0.16	0.17
N	189,837	189,837	189,837	840,868
Mean of Dependent	0.014	0.09	0.14	0.82

Notes: Each coefficient comes from a different specifications, all of them were estimated using OLS. All DD models include state and year fixed effects while DDD models include state, year, chidless status fixed effects, and any two way interaction between these set of fixed effects. In each model I control for race, gender, education, age and marital status. Standards error in parenthesis are obtained from cluster standard errors. The P-values from with cluster standard errors are in while the p-values in [] were obtained using the standard errors from a modified block-bootstrap procedure. The mean of the dependent is the pre-treatment in Tennessee for non-elderly adults.

Table 5: Effects of Disenrollment on Health Care Access Using NHIS and BRFSS 2000-2010

	Mean of Dependent	Full Sample	Sample: HS Degree or less	Sample More than HS Degree
Panel A: Using NHIS				
Medicaid	0.09	-0.027	-0.048	-0.015
		(0.003)	(0.014)	(0.007)
		{0.000}	{0.002}	{0.039}
		[0.015]	[0.014]	[0.154]
Losing Medicaid	0.014	0.018	0.036	0.002
		(0.003)	(0.008)	(0.005)
		{0.000}	{0.000}	{0.692}
		[0.012]	[0.012]	[0.687]
Uninsured	0.14	0.045	0.052	0.040
		(0.003)	(0.024)	(0.018)
		{0.000}	{0.036}	{0.031}
		[0.003]	[0.060]	[0.014]
Panel B: Using BRFSS				
Insured	0.82	-0.017	-0.026	-0.011
		(0.002)	(0.006)	(0.003)
		{0.000}	{0.000}	{0.003}
		[0.089]	[0.170]	[0.308]

Notes: Each coefficient comes from the DDD specification for different sub-samples estimated using OLS. All models include, state, year, chidless status fixed effects, and any two way interaction between these set of fixed effects. In this model I control for race, gender, education, age, and marital status. Clustered Standards error in parenthesis and associated p-values are in . The block-bootstrapped p-values are in [ ] The mean of the dependent is the pre-treatment in Tennessee mean for childless adults.

Table 6: Effects of Disenrollment on Health Care Access Using NHIS and BRFSS 2000-2010

	Mean of Dependent	Full Sample	Sample: HS Degree or less	Sample More than HS Degree
Panel A: Using NHIS				
Pr(Forgone or Delay Care due to Cost in past 12 months)	0.11	0.032	0.042	0.023
		(0.003)	(0.005)	(0.002)
		{0.000}	{0.000}	{0.001}
		[0.004]	[0.035]	[0.106]
Pr(Cannot Afford Prescription Drugs)	0.11	0.019	0.044	-0.001
		(0.004)	(0.008)	(0.005)
		{0.000}	{0.000}	{0.845}
		[0.290]	[0.205]	[0.966]
Pr(Seen/talk to a general doctor in past 12 months)	0.59	-0.024	-0.042	-0.003
		(0.007)	(0.017)	(0.008)
		{0.003}	{0.025}	{0.712}
		[0.442]	[0.393]	[0.934]
Panel B: Using BRFSS				
Pr(Forgone Care due to Cost in past 12 months)	0.12	0.013	0.038	-0.007
		(0.002)	(0.007)	(0.002)
		{0.000}	{0.000}	{0.014}
		[0.259]	[0.055]	[0.582]
Pr(Had a Dr Check-up in the past 12 months)	0.76	-0.022	-0.039	-0.013
		(0.003)	(0.008)	(0.004)
		{0.000}	{0.000}	{0.010}
		[0.246]	[0.157]	[0.585]

Notes: Each coefficient comes from the DDD specification for different sub-samples estimated using OLS. All models include, state, year, chidless status fixed effects, and any two way interaction between these set of fixed effects. In this model I control for race, gender, education, age, and marital status. Clustered Standards error in parenthesis and associated p-values are in . The block-bootstrapped p-values are in [ ]. The mean of the dependent is the pre-treatment in Tennessee mean for childless adults.

Table 7: Effects of Disenrollment on Preventive Care Using BRFSS 2000-2010

	Mean of Dependent	Full Sample	Sample HS Degree or Less	Sample More than a HS Degree
Panel A: General Population				
Had a Flu shot in the past 12 months?	0.32	0.027	0.024	0.028
		(0.003)	(0.003)	(0.004)
		{0.000}	{0.000}	{0.000}
		[0.025]	[0.152]	[0.091]
Had Cholesterol Check in the past 12 months?	0.61	-0.021	-0.012	-0.033
		(0.004)	(0.012)	(0.005)
		{0.074}	{0.487}	{0.007}
		[0.219]	[0.643]	[0.113]
Panel B: For Women				
Had a Mammogram in the past 12 months?	0.70	-0.007	0.011	-0.030
(Over 50)		(0.031)	(0.039)	(0.040)
		{0.369}	{0.409}	{0.464}
		[0.831]	[0.791]	[0.474]
Had a Breat Exam in the past 12 months?	0.75	-0.008	0.009	-0.023
(Over 21)		(0.006)	(0.006)	(0.010)
		{0.195}	{0.174}	{0.043}
		[0.644]	[0.726]	[0.307]
Had a Pap Exam in the past 12 months?	0.72	0.002	-0.003	0.004
(Over 21)		(0.005)	(0.008)	(0.004)
		{0.706}	{0.704}	{0.419}
		[0.903]	[0.907]	[0.852]
Panel C: For Men over 40				
Had a PSA Exam in the past 12 months?	0.43	-0.003	-0.063	0.041
		(0.009)	(0.021)	(0.007)
		{0.771}	{0.008}	{0.000}
		[0.945]	[0.250]	[0.430]
Had a Rectal Exam in the past 12 months?	0.39	0.001	-0.017	0.012
		(0.0045)	(0.011)	(0.008)
		{0.836}	{0.147}	{0.169}
		[0.977]	[0.758]	[0.829]

Notes: Each coefficient comes from the DDD specification for different sub-samples estimated using OLS. All models include, State, Year, Childless status Fixed Effects, and any two way interaction between these set of fixed effects. In this model I control for race, gender, education, age and marital status. Standards error in parenthesis and P-values are in brackets both obtained from a modified block-bootstrap procedure. The mean of the dependent is the pre-treatment in Tennessee mean for childless adults.

Table 8: Effects of Disenrollment on Health Behaviors Using BRFSS 2000-2010

	Mean of Dependent	Full Sample	Sample: HS Degree or Less	Sample: More than HS Degree
Panel A: Summary Measure for Health Behaviors				
Index "Taking Better Care	-0.04	0.011	0.091	-0.049
		(0.022)	(0.040)	(0.025)
		[0.608]	[0.022]	[0.044]
Panel B: BMI and Non-risky Health Behaviors				
Participates in Physical Activity	0.73	0.004	0.012	0.001
		(0.003)	(0.006)	(0.003)
		{0.248}	{0.086}	{0.623}
		[0.742]	[0.539]	[0.918]
Daily Servings of Fruits and Vegetables	3.63	-0.095	0.0140	-0.178
		(0.019)	(0.029)	(0.038)
		{0.000}	{0.633}	{0.000}
		[0.210]	[0.906]	[0.101]
Panel C: Risky Health Behaviors				
Bing Drinking	0.12	-0.002	-0.021	0.017
		(0.003)	(0.001)	(0.003)
		{0.448}	{0.000}	{0.000}
		[0.849]	[0.217]	[0.190]
Any Drink in Past 30 Days	0.47	-0.015	-0.026	-0.006
		(0.005)	(0.007)	(0.005)
		{0.010}	{0.002}	{0.261}
		[0.324]	[0.219]	[0.769]
Drinks per Occassion in the Past 30 Days	1.08	-0.074	-0.222	0.068
		(0.020)	(0.037)	(0.016)
		{0.002}	{0.000}	{0.001}
		[0.421]	[0.132]	[0.276]
Currently a Smoker	0.27	0.006	-0.001	0.007
		(0.002)	(0.003)	(0.003)
		{0.004}	{0.679}	{0.019}
		[0.587]	[0.942]	[0.589]

Notes: Each coefficient comes from the DDD specification for different sub-samples estimated using OLS. All models include, State, Year, Childless status Fixed Effects, and any two way interaction between these set of fixed effects. In this model I control for race, gender, education, age and marital status. Standards error in parenthesis and P-values are in brackets both obtained from a modified block-bootstrap procedure. The mean of the dependent is the pre-treatment in Tennessee mean for childless adults.

Table 9: Effects of Disenrollment on Self-Assesed Health Using NHIS and BRFSS 2000-2010

	Mean of Dependent	Full Sample	Sample: HS Degree or less	Sample More than HS Degree
Panel A: Using NHIS				
Pr(Reporting Excellent Health)	0.30	0.026	0.021	0.030
		(0.005)	(0.005)	(0.008)
		{0.000}	{0.001}	{0.001}
		[0.135]	[0.327]	[0.243]
Pr(Reporting Fair or Poor Health)	0.12	-0.014	-0.038	0.0003
		(0.002)	(0.003)	(0.002)
		{0.000}	{0.001}	{0.981}
		[0.296]	[0.093]	[0.982]
Panel B: Using BRFSS				
Pr(Reporting Excellent Health)	0.20	-0.005	0.002	-0.008
		(0.002)	(0.002)	(0.003)
		{0.023}	{0.360}	{0.007}
		[0.628]	[0.882]	[0.595]
Pr(Reporting Fair or Poor Health)	0.20	0.009	0.016	0.002
		(0.003)	(0.005)	(0.003)
		{0.005}	{0.005}	{0.638}
		[0.273]	[0.354]	[0.877]

Notes: Each coefficient comes from the DDD specification for different sub-samples estimated using OLS. All models include, state, year, chidless status fixed effects, and any two way interaction between these set of fixed effects. In this model I control for race, gender, education, age and marital status. Standards error in parenthesis and P-values are in brackets both obtained from a modified blockbootstrap procedure. The mean of the dependent is the pre-treatment in Tennessee mean for childless adults.

Table 10: Effects of Disenrollment on Having Bed Days Using NHIS 2000-2010

	Mean of Dependent	Full Sample	Sample: HS Degree or less	Sample More than HS Degree
Panel A: Using NHIS				
Number of Bed Days in past 12 months	5.1	0.666	1.592	0.875
		(0.330)	(0.619)	(0.328)
		{0.060}	{0.020}	{0.012}
		[0.723]	[0.574]	[0.724]
Panel B: Using BRFSS				
Number of Days with Bad Physical Health	3.9	-0.067	0.294	-0.413
		(0.049)	(0.102)	(0.064)
		{0.190}	{0.011}	{0.000}
		[0.749]	[0.429]	[0.092]
Number of Days with Bad Mental Health	3.3	0.132	0.229	0.026
		(0.055)	(0.069)	(0.092)
		{0.028}	{0.004}	{0.786}
		[0.554]	[0.575]	[0.914]
Number of Days of Incapacitation	4.7	0.836	1.213	0.437
		(0.064)	(0.114)	(0.064)
		{0.000}	{0.000}	{0.000}
		[0.018]	[0.027]	[0.260]

Notes: Each coefficient comes from the DDD specification for different sub-samples estimated using OLS. All models include, state, year, chidless status fixed effects, and any two way interaction between these set of fixed effects. In this model I control for race, gender, education, age and marital status. Standards error in parenthesis and P-values are in brackets both obtained from a modified block bootstrap procedure. The mean of the dependent is the pre-treatment mean in Tennessee for childless adults.

Table 11: Effects of Disenrollment on Place to go for Medical Care when Sick NHIS 2000-2010

	Mean of Dependent	Full Sample	Sample: HS Degree or less	Sample More than HS Degree
Panel A: Place of Usual Care when sick				
Pr(Change health care place due to health insurance)	0.03	0.019	0.039	0.002
		(0.002)	(0.003)	(0.003)
		{0.000}	{0.000}	{0.514}
		[0.075]	[0.019]	[0.916]
Pr(Has usual place of care)	0.81	0.033	0.039	0.033
		(0.006)	(0.012)	(0.004)
		{0.034}	{0.005}	{0.000}
		[0.149]	[0.291]	[0.218]
Panel B: Type of Usual Place Care when sick				
Pr(Usual place is Clinic)	0.12	0.022	0.021	0.025
		(0.004)	(0.005)	(0.005)
		{0.049}	{0.001}	{0.000}
		[0.270]	[0.512]	[0.301]
Pr(Usual place is Dr or HMO)	0.65	0.009	-0.015	0.033
		(0.007)	(0.012)	(0.007)
		{0.216}	{0.265}	{0.000}
		[0.748]	[0.738]	[0.366]
Pr(Usual place is ED)	0.01	0.004	0.021	-0.009
		(0.001)	(0.002)	(0.001)
		{0.001}	{0.000}	{0.000}
		[0.568]	[0.159]	[0.168]
Pr(Usual place is Hospital Outpatient Department)	0.01	0.002	0.006	-0.001
		(0.001)	(0.001)	(0.002)
		{0.062}	{0.000}	{0.624}
		[0.508]	[0.309]	[0.834]
Pr(Does not have a usual place of care)	0.19	-0.032	-0.037	-0.033
		(0.006)	(0.013)	(0.005)
		{0.000}	{0.012}	{0.000}
		[0.152]	[0.355]	[0.233]

Notes: Each coefficient comes from the DDD specification for different sub-samples estimated using OLS. All models include, state, year, childless status fixed effects, and any two way interaction between these set of fixed effects. In this model I control for race, gender, education, age and marital status. Standards error in parenthesis and P-values are in brackets both obtained from a modified block bootstrap procedure. The mean of the dependent is the pre-treatment in Tennessee mean for childless adults.

Table 12: Effects of Disenrollment on Hospital Health Care Using NHIS 2000-2010

	Mean of Dependent	Full Sample	Sample: HS Degree or less	Sample More than HS Degree
Pr(Going to the ED in the past 12 months)	0.22	-0.0003	0.015	-0.021
		(0.004)	(0.007)	(0.005)
		{0.941}	{0.048}	{0.000}
		[0.988]	[0.732]	[0.461]
Number of times in ED in the past 12 months	0.33	-0.008	0.026	-0.048
		(0.007)	(0.019)	(0.011)
		{0.269}	{0.190}	{0.000}
		[0.881]	[0.748]	[0.448]
Pr(Had surgery in the past 12 months)	0.12	-0.028	-0.062	-0.006
		(0.006)	(0.007)	(0.009)
		{0.000}	{0.000}	{0.509}
		[0.274]	[0.044]	[0.816]
Number of surgeries in the past 12 months	0.15	-0.034	-0.109	0.028
		(0.008)	(0.009)	(0.012)
		{0.001}	{0.000}	{0.033}
		[0.288]	[0.051]	[0.482]
Pr(Had any overnight hospital stay in the past 12 months)	0.08	0.008	0.003	0.010
		(0.003)	(0.004)	(0.003)
		{0.017}	{0.464}	{0.004}
		[0.474]	[0.845]	[0.474]
Number of times being an inpatient in the past 12 months	0.13	-0.013	-0.058	0.009
		(0.005)	(0.009)	(0.007)
		{0.019}	{0.000}	{0.217}
		[0.603]	[0.337]	[0.701]
Average number of nights per stayed if Overnight	3.55	-0.194	1.982	-2.841
		(0.413)	(0.653)	(0.186)
		{0.645}	{0.008}	{0.000}
		[0.883]	[0.244]	[0.206]

Notes: Each coefficient comes from the DDD specification for different sub-samples estimated using OLS. All models include, state, year, chidless status fixed effects, and any two way interaction between these set of fixed effects. In this model I control for race, gender, education, age and marital status. Standards error in parenthesis and P-values are in brackets both obtained from a modified block bootstrap procedure. The mean of the dependent is the pre-treatment in Tennessee mean for childless adults.

Table 13: Effects of the Reform on Payment Types Using NIS 2000 - 2010

	Medicaid	Self-Pay	Private	Medicare
TN X Post	-0.039	0.018	0.0280	-0.012
	(0.001)	(0.001)	(0.001)	(0.001)
	{0.000}	{0.000}	{0.000}	{0.000}
N	31,55,042	31,55,042	31,55,042	31,550,42
R-Squared	0.0612	0.0589	0.124	0.0337
Pre Mean in TN	0.27	0.11	0.43	0.21

Notes: Using 100 percent of Data. State, Year-Quarter, Hospital and Month FE Using Years 2000 thru 2010

Table 14: Effects of the Reform on Number of Discharges Using NIS 2000 - 2010

	Number of Discharges Total	Number of Discharges Non-Elderly Adults	Number of Discharges Elderly	Number of Discharges Under 18
TN X Post	-85.71	-78.42	-12.21	4.865
	(0.033)	(0.014)	(0.026)	(0.009)
	{0.000}	{0.000}	{0.000}	{0.000}
N	17,134	17,134	17,134	17,134
R-Squared	0.988	0.980	0.987	0.978
Pre Mean in TN	397.2	184.1	136.9	76.12

Notes: Each coefficient comes from a DD specifications for different sub-samples estimated using OLS. All models include state, year-quarter, state-year, and hospital fixed effects. I use 20 percent of the discharges per hospital. Standards error in parenthesis and P-values are in brackets both obtained from a modified block bootstrap procedure. The mean of the dependent is the pre-treatment in Tennessee.

Appendix A: Tables

Table A.1: Summary Statistics of Demographic Variables 2000-2010 BRFSS Data

	TN Before	Southern States Before	TN After	Southern States After	Total
Black	0.14	0.16	0.15	0.16	0.16
Hispanic	0.02	0.13	0.03	0.14	0.13
Other Races	0.03	0.06	0.04	0.07	0.06
Female	0.51	0.51	0.51	0.51	0.51
Kinder or never attended school	0.00	0.00	0.00	0.00	0.00
Grades 1 through 8	0.03	0.04	0.02	0.03	0.03
Grades 9 through 11	0.08	0.08	0.08	0.07	0.08
Grade 12 or GED	0.35	0.30	0.35	0.27	0.29
College 1 year to 3 year	0.29	0.26	0.30	0.27	0.27
Reported Age in years	41.45	40.92	42.98	42.12	41.54
Divorced	0.13	0.11	0.13	0.10	0.11
Widowed	0.02	0.02	0.02	0.02	0.02
Separated	0.03	0.03	0.03	0.03	0.03
Never Married	0.16	0.17	0.15	0.16	0.16
Unmarried Couple	0.01	0.03	0.01	0.04	0.03
Less than 10K	0.04	0.05	0.05	0.05	0.05
Less than 15K	0.04	0.05	0.05	0.04	0.05
Less than 20K	0.08	0.08	0.08	0.07	0.08
Less than 25K	0.12	0.10	0.13	0.08	0.09
Less than 35K	0.17	0.15	0.13	0.10	0.13
Less than 50K	0.21	0.18	0.16	0.15	0.17
Less than 75K	0.17	0.18	0.17	0.17	0.17
75K or more	0.18	0.22	0.22	0.34	0.27

Table A.2: Summary Statistics of Independent Variables 2000-2010 NHIS Data

	TN Before	Southern States Before	TN After	Southern States After	Total
Female	0.51	0.52	0.52	0.51	0.51
Age	41.28	41.10	41.63	41.81	41.43
Less than HS	0.17	0.16	0.12	0.14	0.15
HS or GED	0.34	0.30	0.32	0.28	0.29
Some College	0.28	0.29	0.31	0.29	0.29
White	0.82	0.66	0.75	0.62	0.65
Black	0.15	0.19	0.20	0.19	0.19
Hispanic	0.02	0.12	0.03	0.15	0.13
Asian	0.01	0.02	0.01	0.03	0.03
American Indian, Alaska Native	0.00	0.01	0.01	0.01	0.01
Other race	0.00	0.00	0.00	0.00	0.00
Multiple Race	0.00	0.00	0.00	0.00	0.00
Married	0.64	0.63	0.60	0.60	0.62
Divorced	0.02	0.02	0.02	0.02	0.02
Widowed	0.11	0.09	0.11	0.10	0.10
Separated	0.02	0.03	0.02	0.03	0.03
Never Married	0.15	0.16	0.17	0.18	0.17
Unmarried Couple	0.06	0.06	0.07	0.07	0.07

Table A.3: Effects Disenrollment on Health Insurance Coverage Using BRFSS and NHIS 2000-2010

	Uninsured	Medicaid	Private	Medicare	Lost Medicaid	Health Coverage
DD Model, All Adults	NHIS	NHIS	NHIS	NHIS	NHIS	BRFSS
TN X Post	0.043	-0.028	-0.006	-0.004	0.011	-0.031
	(0.009)	(0.005)	(0.009)	(0.004)	(0.004)	(0.007)
	[0.000]	[0.000]	[0.536]	[0.326]	[0.012]	[0.000]
R-Square	0.17	0.07	0.20	0.05	0.03	0.16
N	193086	193086	193086	193086	142192	572769
DD model, sample with children < 18						
TN X Post	0.013	-0.009	0.014	-0.001	0.005	-0.026
	(0.012)	(0.009)	(0.014)	(0.003)	(0.007)	(0.009)
	[0.291]	[0.332]	[0.345]	[0.790]	[0.464]	[0.005]
R-Square	0.18	0.11	0.23	0.02	0.06	0.18
N	76227	76227	76227	76227	57249	250077
DD model, sample without children < 18						
TN X Post	0.052	-0.030	-0.020	-0.007	0.014	-0.036
	(0.009)	(0.007)	(0.012)	(0.005)	(0.004)	(0.008)
	[0.000]	[0.000]	[0.118]	[0.225]	[0.001]	[0.000]
R-Square	0.15	0.07	0.18	0.05	0.01	0.13
N	113610	113610	113610	113610	82614	322151
DDD Model						
TN X Post X No Children under 18	0.045	-0.027	-0.032	-0.011	0.018	-0.017
	(0.012)	(0.010)	(0.016)	(0.006)	(0.006)	(0.010)
	[0.003]	[0.015]	[0.068]	[0.103]	[0.012]	[0.089]
R-Square	0.16	0.08	0.20	0.06	0.04	0.16
N	189837	189837	189837	189837	137962	572228
Mean of Dependent	0.14	0.09	0.71	0.04	0.014	0.82

Notes: Each coefficient comes from the DDD specification for different sub-samples estimated using OLS. All models include state, year, chidless status fixed effects , and any two way interaction between these set of fixed effects. In this model I control for race, gender, education, age and marital status. Modified block-bootStrap standards error in parenthesis and p-values are in brackets.The mean of the dependent is the pre-treatment in Tennessee mean for childless adults.

Table A.4: Effects of Disenrollment on Health Care Access Using NHIS 2000-2010

	Mean of Dependent	Full Sample	Sample: HS Degree or less	Sample More than HS Degree
Panel A: Using NHIS				
Uninsured	0.14	0.045	0.052	0.040
		(0.012)	(0.026)	(0.015)
		[0.003]	[0.060]	[0.014]
Medicaid	0.09	-0.027	-0.048	-0.015
		(0.010)	(0.018)	(0.010)
		[0.015	[0.014]	[0.154]
Private	0.71	-0.032	-0.009	-0.049
		(0.016)	(0.028)	(0.020)
		[0.068]	[0.741]	[0.027]
Medicare	0.04	-0.011	-0.016	-0.005
		(0.006)	(0.011)	(0.007)
		[0.103]	[0.161]	[0.455]

Notes: Each coefficient comes from the DDD specification for different sub-samples estimated using OLS. All models include state, year, chidless status fixed effects, and any two way interaction between these set of fixed effects. In this model I control for race, gender, education, age and marital status. Modified block-bootStrap standards error in parenthesis and p-values are in brackets The mean of the dependent is the pre-treatment in Tennessee

Table 5: Effects Disenrollment on Medicaid Coverage Using NHIS 2000-2010 - DD

	Sample: Full Sample			Sample: HS Degree or Less			Sample: More than HS Degree		
	Baseline	Childless Adults	Adults with Chilren	Baseline	Childless Adults	Adults with Chilren	Baseline	Childless Adults	Adults with Chilren
TN X Post	-0.028	-0.030	-0.009	-0.044	-0.048	-0.011	-0.012	-0.015	0.000
	(0.005)	(0.007)	(0.009)	(0.009)	(0.012)	(0.019)	(0.005)	(0.006)	(0.008)
	[0.000]	[0.000]	[0.332]	[0.000]	[0.001]	[0.550]	[0.029]	[0.030]	[0.980]
R-Squared	0.07	0.07	0.11	0.08	0.08	0.12	0.03	0.02	0.07
N	193086	113610	76227	92673	54292	36072	100413	59318	40155
Mean of Dependent	0.05	0.05	0.05	0.08	0.08	0.08	0.02	0.02	0.03
Pre Mean in TN	0.09	0.09	0.09	0.09	0.09	0.09	0.09	0.09	0.09

Notes: The sample includes individuals who live in southern states as defined by the U.S Census of ages 21 to 64 Each coefficient comes from the DD specification for different sub-samples estimated using OLS. All models include state and year fixed effects. In this model I control for race, gender, education, age and marital status. Modified block-bootStrap standards error in parenthesis and p-values are in brackets. The mean of the dependent is the pre-treatment in Tennessee mean.

Table 6: Effects Disenrollment on Uninsurance Rate Using NHIS 2000-2010 - DD

	Sample: Full Sample			Sample: HS Degree or Less			Sample: More than HS Degree		
	Baseline	Childless Adults	Adults with Chilren	Baseline	Childless Adults	Adults with Chilren	Baseline	Childless Adults	Adults with Chilren
TN X Post	0.043	0.052	0.013	0.061	0.068	0.021	0.024	0.036	-0.002
	(0.009)	(0.009)	(0.012)	(0.013)	(0.016)	(0.020)	(0.010)	(0.012)	(0.013)
	[0.000]	[0.000]	[0.291]	[0.000]	[0.000]	[0.309]	[0.026]	[0.008]	[0.891]
R-Squared	0.16	0.15	0.18	0.15	0.15	0.14	0.08	0.08	0.09
N	193086	113610	76227	92673	54292	36072	100413	59318	40155
Mean of Dependent	0.26	0.25	0.27	0.38	0.35	0.40	0.16	0.16	0.14
Pre Mean in TN	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.14

Notes: The sample includes individuals who live in southern states as defined by the U.S Census of ages 21 to 64 Each coefficient comes from the DD specification for different sub-samples estimated using OLS. All models include state and year fixed effects. In this model I control for race, gender, education, age and marital status. Modified block-bootStrap standards error in parenthesis and p-values are in brackets. The mean of the dependent is the pre-treatment in Tennessee mean.

Table 7: Effects Disenrollment on Private Coverage Rate Using NHIS 2000-2010 - DD

	Sample: Full Sample			Sample: HS Degree or Less			Sample: More than HS Degree		
	Baseline	Childless Adults	Adults with Chilren	Baseline	Childless Adults	Adults with Chilren	Baseline	Childless Adults	Adults with Chilren
TN X Post	-0.006	-0.020	0.014	0.016	0.006	0.026	-0.026	-0.044	0.001
	(0.009)	(0.012)	(0.014)	(0.015)	(0.018)	(0.024)	(0.012)	(0.014)	(0.015)
	[0.536]	[0.118]	[0.345]	[0.290]	[0.724]	[0.298]	[0.047]	[0.007]	[0.925]
R-Squared	0.20	0.18	0.23	0.17	0.17	0.18	0.09	0.08	0.12
N	193086	113610	76227	92673	54292	36072	100413	59318	40155
Mean of Dependent	0.64	0.64	0.64	0.49	0.50	0.48	0.78	0.77	0.79
Pre Mean in TN	0.70	0.70	0.70	0.70	0.70	0.70	0.70	0.70	0.70

Notes: The sample includes individuals who live in southern states as defined by the U.S Census of ages 21 to 64 Each coefficient comes from the DD specification for different sub-samples estimated using OLS. All models include state and year fixed effects. In this model I control for race, gender, education, age and marital status. Modified block-bootStrap standards error in parenthesis and p-values are in brackets. The mean of the dependent is the pre-treatment in Tennessee mean.

Table 8: Effects Disenrollment on Reporting Losing Medicaid Using NHIS 2000-2010 - DD

	Sample: Full Sample			Sample: HS Degree or Less			Sample: More than HS Degree		
	Baseline	Childless Adults	Adults with Chilren	Baseline	Childless Adults	Adults with Chilren	Baseline	Childless Adults	Adults with Chilren
TN X Post	0.011	0.014	0.005	0.024	0.030	0.008	0.001	0.001	0.001
	0.004	0.004	0.007	0.006	0.007	0.014	0.003	0.003	0.006
	0.012	0.001	0.464	0.002	0.000	0.579	0.809	0.733	0.831
R-Squared	0.03	0.01	0.06	0.04	0.01	0.06	0.02	0.01	0.04
N	193086	113610	76227	92673	54292	36072	100413	59318	40155
Mean of Dependent	0.02	0.01	0.04	0.03	0.01	0.06	0.01	0.00	0.02
Pre Mean in TN	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01

Notes: The sample includes individuals who live in southern states as defined by the U.S Census of ages 21 to 64 Each coefficient comes from the DD specification for different sub-samples estimated using OLS. All models include state and year fixed effects. In this model I control for race, gender, education, age and marital status. Modified block-bootStrap standards error in parenthesis and p-values are in brackets. The mean of the dependent is the pre-treatment in Tennessee mean.

Table 9: Effects Disenrollment on Medicare Rates Using NHIS 2000-2010 - DD

	Sample: Full Sample			Sample: HS Degree or Less			Sample: More than HS Degree		
	Baseline	Childless Adults	Adults with Chilren	Baseline	Childless Adults	Adults with Chilren	Baseline	Childless Adults	Adults with Chilren
TN X Post	-0.004	-0.007	-0.001	-0.013	-0.016	-0.009	0.002	0.000	0.005
	0.004	0.005	0.003	0.007	0.010	0.006	0.004	0.005	0.005
	0.326	0.225	0.790	0.088	0.106	0.147	0.631	0.991	0.335
R-Squared	0.05	0.05	0.02	0.06	0.06	0.02	0.03	0.03	0.01
N	193086	113610	76227	92673	54292	36072	100413	59318	40155
Mean of Dependent	0.03	0.05	0.01	0.05	0.07	0.02	0.02	0.03	0.01
Pre Mean in TN	0.04	0.04	0.04	0.04	0.04	0.04	0.04	0.04	0.04

Notes: The sample includes individuals who live in southern states as defined by the U.S Census of ages 21 to 64 Each coefficient comes from the DD specification for different sub-samples estimated using OLS. All models include state and year fixed effects. In this model I control for race, gender, education, age and marital status. Modified block-bootStrap standards error in parenthesis and p-values are in brackets. The mean of the dependent is the pre-treatment in Tennessee mean.

CHAPTER 2

DO 'CHEESEBURGER BILLS' WORK? EFFECTS OF TORT REFORM FOR FAST FOOD

With Christopher S. Carpenter

1. Introduction

In 2002 McDonald's corporation was the subject of two high profile lawsuits alleging liability for weight-related health claims. In the first, Caesar Barber, a severely obese 56 year old man from the Bronx, accused McDonald's and other popular fast food chains of intentionally withholding nutritional content information from customers, causing him to consume unhealthy food without his knowledge.[1] In the second, the parents of Ashley Pelman and Jazlen Bradley – two severely obese teenagers – sued McDonald's on the basis of deceptive marketing. Although both of these cases were ultimately dismissed, McDonald's and other fast food restaurants viewed the threat of lawsuits as non-trivial. Around this same time period, McDonald's eliminated its 'super-size' option, provided nutrition information on food packaging, and added healthier options such as salads and fruit to its menu, and other restaurants took similar actions.[2]

Regardless of the legal merits of claims such as those in the *Barber* and *Pelman* cases, there is strong popular belief that holding food companies legally responsible

[1] Barber also sued Burger King, Wendy's, and Kentucky Fried Chicken.

[2] McDonald's is no stranger to high profile lawsuits and has paid out very large jury awards and settlements in the 1994 hot coffee case, the 2005 acrylamide case, and others. More recently, in 2010 the Center for Science in the Public Interest sued McDonald's, alleging that toys in Happy Meals constituted deceptive marketing to children.

for obesity-related claims is unwarranted at best and ridiculous at worst. In response to the potentially costly effects of these and similar lawsuits, states quickly began to adopt laws that explicitly limited liability of food companies in weight-based claims. Termed 'Commonsense Consumption Acts' (henceforth CCAs) but more commonly known as 'Cheeseburger Bills', these laws were promoted by the National Restaurant Association and achieved a great deal of success beginning as early as 2003. In June of that year, Louisiana became the first state to adopt a CCA; by the end of 2004, 12 other states had followed suit. Today, 26 states have adopted some limit on legal liability for the food industry. A federal version titled 'American Personal Responsibility in Food Consumption Act' was introduced to Congress by Florida Republican and self-acknowledged fast-food lover Ric Keller in both 2004 (HR339) and 2005 (HR554). Both times it passed in the House but did not pass the Senate.

Commonsense Consumption Acts effectively serve as tort reform for the food industry (in particular, the fast-food industry, which is the subject of most of the legal attention). These laws share several common features, and in fact many share a nearly identical structure and language (see Appendix A for an example). Specifically, the modal CCA protects food manufacturers, packers, distributors, carriers, holders, sellers, marketers, and advertisers from civil actions for claims arising out of weight gain, obesity, health conditions associated with weight gain or obesity, or other known conditions likely to result from long-term consumption of food. CCAs also generally clarify the conditions under which protection is not extended: if there is a violation of the adulteration or misbranding requirement or if there is any other knowing and willful material violation of state or federal laws pertaining to manufacture, sale, distribution, marketing, or labeling of food. Most laws explicitly define 'food' using the Food and Drug Administration definition that includes beverages. Some CCAs also contain explicit exceptions for altered products, deceptively advertised products, or fraudulent claims. Thus, proponents of CCAs claim that they still provide adequate protection to consumers while at the same time limiting frivolous lawsuits.

Opponents of CCAs (including the Center for Science in the Public Interest) claim that the laws are simply designed to help the large and powerful food industry – particularly fast food and the National Restaurant Association, which was the primary supporter and financial sponsor of state and federal CCA lobbying activity – make more money and avoid their responsibilities to society. Opponents also claim that the fact there have been no successful obesity-related lawsuits suggests that, in fact, the legal system in this realm is not broken – and thus there is no need to fix it.[3]

Indeed, the success of any such weight-based claim hinges critically on a link that many legal experts agree is the fundamental problem with such cases: establishing

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[3] There are examples of successful obesity-related lawsuits against McDonald's in other countries. For example, in a 2010 case in Brazil, McDonald's was ordered to pay $17,500 to one of its managers who alleged the company contributed to his 65 pound weight gain over 12 years in part by supplying him free meals. The manager also claimed that 'mystery shoppers' sent by the firm to the store required him to eat the food to make sure it passed quality standards (Hartenstein 2010). Legal scholars have also identified other possible social benefits of nontrivial obesity-related legal threats to fast food companies. For example, such threats may increase consumer awareness of obesity and nutrition issues, and it also may incentivize fast food companies to voluntarily change menus or alter their marketing practices (Mello et al. 2003).

a causal connection between consumption at *a particular restaurant* (e.g., McDonald's) and weight-related harms. Despite this inherent legal challenge, two trends suggest that legal action against fast food companies is unlikely to go away. First, there has been a well-documented and high-profile dramatic rise in population rates of obesity. Second, there has been a concomitant reduction in the relative price of calories consumed away from home, particularly at fast food restaurants. These factors give many observers reason to believe that from a legal standpoint obesity could become the next tobacco: that is, just as the largest US tobacco companies were on the losing end of class-action lawsuits and huge jury awards for smoking-related illnesses two decades ago, experts have suggested that we might see similar obesity-related legal activity for economic and non-economic damages pertaining to weight-based health problems.[4]

Documenting the effects of Commonsense Consumption Acts is important for several reasons. First, our results help inform a large and growing literature in law and economics on the effects of other more traditionally studied tort reforms.

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[4] In the debates over obesity litigation, many parallels are made with tobacco litigation. In both cases the products in question are harmful to public health if consumed in excess quantity and are potentially addictive, and in both cases there are claims that companies may have deceived consumers about the content, safety, and addictiveness of the products. Moreover, in both cases the public thought it was implausible that plaintiffs would win large jury awards. The release of tobacco industry documents through the 1998 Master Settlement Agreement – which was negotiated after several state Attorneys General filed class action lawsuits against the largest tobacco companies in the United States (RJ Reynolds and Lorillard) – helped fuel big awards. Notably, Altria Group (formerly Phillip Morris), one of the largest tobacco companies worldwide, actively lobbied for adoption of the federal cheeseburger bill. As of 2014, lawyers in at least 16 states have lobbied state Attorneys General to require the food industry to pay for obesity-related health care costs (Evich 2014).

Specifically, our study provides direct evidence on whether parties take more care when they are less able to sue companies for the harmful effects of products. This is a central idea behind tort reform and has been studied extensively in the context of medical malpractice. For example, multiple studies have found that tort reforms such as caps on economic damages and establishment of joint and several liability rules are associated with significant changes in the way doctors practice medicine and in health outcomes (e.g. mortality and birth outcomes) (Currie and McLeod 2008). Second, our study provides direct evidence on whether the laws are having their intended effects, as one explicit goal of CCAs was to induce people to take more personal responsibility for their food consumption and weight. Debate over these regulations remains at the top of the legislative agenda in many states: North Carolina's CCA went into effect in October 2013, while Governor Mark Dayton of Minnesota vetoed his state's CCA in May 2011. There is substantial latitude for the remaining states to adopt their own CCAs or for a federal version of a CCA to be reintroduced.

In this paper we provide the first empirical evidence on the effects of CCAs on investments in weight-related health, population weight, fast-food prices, and fast-food market size. We use several datasets and a two-way fixed effects empirical framework that identifies the effects of CCAs on outcomes using quasi-random variation in the timing of policy adoption across states, net of area (city or state) and year fixed effects, linear area-specific time trends, state unemployment rates and other state characteristics, and other state policies related to tort-reform (e.g., damage caps) and obesity prevention (e.g., menu labelling laws).

To preview, we find that CCAs significantly increased the likelihood that individuals state they are currently trying to lose weight. We also find that CCAs significantly increased healthy food consumption as measured by the number of servings of fruits and vegetables consumed per day. In both cases, we find that these effects are concentrated among heavy individuals. We do not find any effects of CCAs on exercise, nor do we find that the CCA-induced changes in weight loss attempts and healthy food consumption translated into contemporaneous changes in body weight. These null weight effects are precise: our large samples of individual data allow us to rule out effects of CCAs on population weight larger than 0.2 percent. We find no consistent effects of CCAs on prices of fast food or other food items. Regarding the fast-food market, we find some evidence that CCAs increased employment in fast food restaurants within the state. Finally, we show that CCAs significantly increased the number of company-owned McDonald's restaurants and reduced the number of franchise-owned McDonald's restaurants in a state. Our results are important because they provide novel evidence supporting a key tenet of tort reform – that it should induce parties to take more care – and demonstrate that industry-specific tort reform can affect market-wide outcomes.

The paper proceeds as follows: Section 2 outlines the mechanisms through which CCAs could plausibly have affected health and market outcomes and provides a brief literature review. Section 3 describes the data and outlines the empirical approach. Section 4 presents the results, and Section 5 discusses and concludes.

2. Mechanisms and Literature Review

2.1 Mechanisms

How might Commonsense Consumption Acts affect individual and market outcomes in fast food? There are several possible mechanisms. First, for health outcomes, there is a straightforward moral hazard explanation. This mechanism predicts that because CCAs eliminate the 'insurance' function of litigation, heavy individuals should be more likely to invest in weight-related health. That is, individuals in states without CCAs (or prior to CCA adoption) could reason that if they became sick due to their food consumption, they could plausibly have legal recourse to recover health costs and personal damages by suing food manufacturers. CCAs make this avenue substantially less attractive by providing those manufacturers some degree of immunity from weight-based claims, and as such the laws should be expected to induce shifts toward healthier consumption and weight loss attempts.[5]

A challenge with the pure moral hazard mechanism is that it requires individuals to credibly believe that they might successfully sue fast-food companies for their weight-related health costs in the absence of CCAs and to be aware of CCA

[5] These effects should be concentrated among heavy individuals, as they are the most likely to suffer weight-related harms. People who are underweight may also experience increased risk for some health problems, but in those cases it is more difficult to imagine them suing food manufacturers in an attempt to recover health costs and noneconomic damages.

adoption in their state substantially eroding this possibility. While major national newspapers and television programs covered debates over the federal Cheeseburger bill and some of the state laws, it is difficult to know who saw and read those news stories. Google Trends data confirm that there was a spike in national searches for the phrase 'Cheeseburger Bill' in late 2005 when the federal law passed the House, as well as in 2011 (when Minnesota adopted a CCA before its governor vetoed it) and in late 2013 (when North Carolina adopted a CCA) (shown in Figure 1). Unfortunately, Google Trends does not provide comparable data for individual states due to insufficiently low search volume for the specific term.[6] We note that even if people did not know about CCAs in their state from local newspaper and television reports – which would be a problem for the moral hazard story – it could be that trial lawyers seek out clients for weight-based claims, thereby disseminating relevant information.[7]

Another mechanism for any health effects is information. Specifically, the debates about CCA could cause individuals (particularly heavy individuals) to learn or be reminded about society's views and expectations regarding personal responsibility and weight. Even if individuals had no intentions of ever suing fast-food companies, this information channel could have changed people's behavior.

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[6] Using Google Trends to provide complementary evidence for the results on weight loss intentions and healthy food consumption is complicated by two further challenges. First, Google Trends does not have any data before 2004; given that most states adopted in 2004, we cannot implement the quasi-experimental approach used in the main analyses. Second, it is not clear what the right search terms would be for each of the health outcomes for which we find significant associations. For example, for weight loss intentions one could imagine numerous possible search terms such as 'lose weight', 'diet', 'diet foods', and others.

[7] Both the *Barber* and the *Pelman* cases were brought by the same lawyer (Samuel Hirsch).

Regarding market size, CCAs should be expected to increase entry into and reduce exit from the fast food industry (thus increasing the overall fast-food market size) by lowering costs of operation, especially litigation costs and liability insurance costs, which prior work has shown is responsive to tort reform (Born et al. 2009, Viscusi and Born 2005). Prices should be plausibly affected by simple supply and demand forces: if CCAs reduce demand for fast food due to either of the channels described above for individual health outcomes and if CCAs weakly increase supply of fast food restaurants, then prices of fast food should decline.[8]

2.2 Literature Review

Regarding prior literature related to CCAs, we are not aware of any studies in economics that evaluate their effects. Despite this gap in the literature, scholars in health policy and law have studied the public health and legal issues related to obesity litigation in general (Mello et al. 2003, Courtney 2006) and CCAs in particular (Jones 2005, Wilking and Daynard 2013). And despite the lack of specific economics evaluations of CCAs, there is a substantial literature in empirical health economics that studies the causes of the obesity epidemic (see, for example, Courtemanche et al. 2014), including studies that specifically focus on the role of restaurants (e.g., Currie et al. 2010, Chou et al. 2004).

[8] A relevant issue is the degree of latitude franchisees have at setting prices. Our price data suggest there is substantial latitude for individual restaurants to set prices, as there is a great deal of variation across locations, even within the same quarter. This basic finding is confirmed in recent work using a panel of McDonald's prices in the Bay Area (Ater and Rigbi 2007).

Our work is also related to a well-developed literature in law and economics on the effects of policies such as caps on non-economic damages, caps on punitive damages, joint and several liability rules, collateral source rules, and others on outcomes such as insurance premiums (e.g. Viscusi et al 1993, Born et al 1998, Born et al. 2009, Avraham et al. 2012), defensive medicine (Kessler and McClellan 1996, Sloan and Shadle 2009), and health outcomes such as mortality and birth outcomes (Currie and McLeod 2008). More recent work has suggested that movements toward national standards of care are also important in this context (Frakes 2014).

3. Data Description and Empirical Approach

3.1 Data Description

We use multiple datasets to test for effects of CCAs on outcomes. Our primary outcome data for adult weight-related outcomes come from public use versions of the Behavioral Risk Factor Surveillance System (BRFSS). These telephone surveys are fielded every year by state health departments and are coordinated by the Centers for Disease Control who compiles them into an annual individual level dataset that is designed to be representative at the state level. As most of the laws we study were adopted in the mid-2000s, our primary analysis sample is 2000-2012.

Individuals in the BRFSS are asked to state their height and weight (weight without shoes on).[9] We use these responses to create body mass index (BMI) which

[9] We used adjustments for self-reported height and weight based on gender, race and age (Cawley 2004, Cawley and Burkhauser 2008, Courtemanche et al. 2014).

equals weight in kilograms divided by height in meters squared. In addition to height and weight, individuals are also asked about several other weight-related outcomes and behaviors. We examine BRFSS outcomes related to exercise and healthy food consumption as two possibilities. Regarding exercise, the BRFSS asks respondents: "During the past month, other than your regular job, did you participate in any physical activities or exercises such as running, calisthenics, golf, gardening, or walking for exercise?" We create a variable called ANY EXERCISE that equals one if the person reported any past month exercise. Regarding healthy food consumption, the BRFSS asks respondents in several years (2000-2003, 2005, 2007, 2009, 2011, and 2012) about the number of servings of fruits and vegetables the individual reports eating on a usual day.[10] We create a variable called TOTAL FRUITS AND VEGETABLES SERVINGS PER DAY that represents the number of total servings of fruits and vegetables the respondent reports eating per day. Finally, for a limited set of years (2000-2003, and 2005) and states we also observe responses to a question about weight intentions. Specifically, individuals are asked: "Are you now trying to lose weight?". We therefore examine an indicator variable for TRYING TO LOSE WEIGHT.

[10] Specifically, from 2000-2009 the BRFSS asks: "These next questions are about the foods you usually eat or drink. Please tell me how often you eat or drink each one. … Include all foods you eat, both at home and away from home." Respondents can report numbers of servings consumed per day, per week, per month, or per year; we convert all responses into average number of servings consumed per day. The specific foods asked include: fruit juices such as orange, grapefruit, or tomato; fruit not counting juice; green salad; potatoes not including French fries, fried potatoes, or potato chips; carrots; and vegetables not counting carrots, potatoes, or salad. For the years 2011 and 2012 the vegetables questions ask about beans, green vegetables, orange vegetables and other types of vegetables.

We also examine fast food prices using data from the ACCRA/C2ER database. ACCRA data were originally designed to provide cost of living estimates for urban professionals and have been collected quarterly since 1976. Typically volunteers from local Chambers of Commerce would collect information on prices of several local goods in multiple locations, and ACCRA/C2ER compiled these into a city-specific index. These data have been used extensively in the existing literature on obesity in part because of the large number of participating cities. From 2000-2012 we observe three relevant fast-food prices: a McDonald's Quarter Pounder with Cheese; an 11"-12" Pizza Hut or Pizza Inn thin crust cheese pizza; and a thigh and drumstick from either Kentucky Fried Chicken or Church's Chicken. We also observe prices of other food items which serve as placebo or falsification outcomes: a half gallon of whole milk, a loaf of bread, a ten pound sack of potatoes, and an overall grocery price index.[11] We measure all prices in real 2010 US dollars.

We use data from the Quarterly Census of Employment and Wages (QCEW) to study effects on market size. These data are published by the Bureau of Labor Statistics (BLS) and contain quarterly counts of employment and wages reported by employers who cover 98 percent of all U.S jobs. Importantly, these data also identify the relevant industry for each employer. We use data from 2000-2012 on the number of establishments and wages for different industries, including limited-service restaurants (i.e., fast food establishments who should be most directly affected by

[11] We note that the sampling of the ACCRA data has some limitations, most obviously that we do not observe prices in rural markets.

CCAs), full-service restaurants (generally establishments with 'sit down' table service that may also be partly affected by CCAs but which were not targeted in most of the lawsuits), and other industries such as gas stations with convenience stores and grocery stores which should have been unaffected by CCAs and therefore serve as controls or placebo outcomes.

We supplement the QCEW data with information on the number of McDonald's franchises and McDonald's company owned stores in each state and year from 2000-2012. These data are reported in annual Uniform Franchising Offering Circulars (UFOC, later called Franchise Disclosure Documents after 2008). These are legal documents that the Federal Trade Commission requires each parent company to provide to potential franchisees; recent years are available on various state commerce department websites, and we supplemented these by purchasing individual years from Frandata.com. We chose McDonald's given their involvement in the major court cases that led to CCA adoption.

3.2 Empirical Approach

To estimate the effect of the CCAs across all the datasets, we estimate standard difference-in-differences models that rely on plausibly exogenous variation in the timing of adoption of CCAs across states. For the BRFSS-based analyses of individual level data we estimate the following model:

$$(1)\quad Y_{ist} = \beta_0 + \beta_1 X_{ist} + \beta_2(\text{COMMONSENSE CONSUMPTION ACT})_{st} + \beta_3 Z_{st} + \beta_4 S_s + \beta_5 T_t + \beta_6 S_s{*}\text{TREND} + \varepsilon_{ist}$$

where Y are the weight-related health outcomes available in the BRFSS data. We estimate linear probability models on the dichotomous BRFSS outcomes for ease of interpretation. X_{ist} is a vector of individual characteristics available in the BRFSS, including: gender, age and its square, race/ethnicity (Hispanic, black, Asian, other), education (elementary, some high school, high school, some college, college or above), and marital status (divorced, widowed, separated, never married, member of an unmarried couple). COMMONSENSE CONSUMPTION ACT is an indicator variable equal to one in the states and periods when a CCA is in effect which we coded according to Wilking and Daynard (2013) and which is reported in Table 1.[12] The coefficient of interest is β_1 and in the presence of state and year dummies (described below) is identified from within-state changes in outcomes coincident with variation in the timing of CCA adoption across states. The key identifying assumption is that weight and health outcomes would have evolved identically in states with and without CCAs had the laws not been adopted.

Z_{st} is a vector of other potentially relevant state obesity-related public policies, some of which have been studied in prior work. These include: soda taxes (Fletcher, Frisvold, and Tefft 2008), complete streets laws, state and local menu labeling laws

[12] The coding of the CCA variable for the BRFSS outcomes takes the value of 1 if the individual's interview date is on or after the law's effective date in the state and 0 otherwise. For the UFOC outcomes, the CCA variable takes the value of 1 for all years after the law is in effect and 0 otherwise, with the exception that for the year of adoption we set the CCA variable equal to the fraction of the year the law was in effect in the state. For the ACCRA and QCEW outcomes, the CCA variable takes the value of 1 for all the quarters after the quarter the law was effective in the state and 0 otherwise, with the exception that if a law is effective in the middle of a quarter we set the CCA variable in that quarter equal to the fraction of the quarter the law was in effect in the state.

(Restrepo 2014), and cigarette taxes (Chou et al. 2004, Gruber and Frakes 2006, Courtemanche 2009).[13] The Z vector also includes controls for more traditionally studied tort reforms (Avraham 2014), state unemployment rates, and state demographic characteristics (fraction female; fraction black, Hispanic, and other non-white races; fraction of individuals with high school degrees and college or more, fraction of individuals under 21 and between 21-64; and fraction of individuals below the federal poverty line).[14] S_s and T_t are a full set of state and year dummies, respectively. We also control for state-specific linear time trends where we interact each state fixed effect with a variable TREND that equals 1 in 2000, 2 in 2001, and so forth. In these BRFSS models we also control for (but do not show in equation 1) month dummies to account for seasonality in the weight-related health investments (e.g., exercise). We use sample weights provided by BRFSS, and we cluster standard errors at the state level (Bertrand, Duflo, and Mullainathan 2004).[15]

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[13] Other state policies aimed at reducing obesity were also rolled out over this time period but were mainly focused on school environments and children as opposed to the adults we study here.

[14] State unemployment rates come from the Bureau of Labor Statistics. State demographic characteristics are from the Census Bureau.

[15] In results not reported but available upon request, we estimated models that predicted the number of years a CCA was in effect in a state as a function of state economic and demographic characteristics, as well as state policies related to obesity and tort reform (but excluding state fixed effects). We also controlled for the vote share to the Democratic candidate in the most recent Presidential election (to account for citizen ideology) and the share of the state legislature that is Democratic (since Democratic legislators might be friendlier to trial lawyers). Most state demographic characteristics were not significantly associated with the CCA variable except the fraction of individuals aged 65 or older in a state and the fraction of the state below the federal poverty level which were negatively and positively related to the number of years a CCA was in effect in a state, respectively. Obesity-related policies and tort reforms – either measured individually or as an index – were not meaningfully associated with the CCA variable. State unemployment rates were generally negatively related to the number of years a state had a CCA in effect. Political variables were significant predictors of CCAs, but there was a not a simple relationship: while the democratic share of the state legislature was negatively related to the number of years a CCA was in effect in a state (as expected), we also found that the vote share to the

For the analysis of fast food prices we account for the city-quarter nature of the observations and estimate the following model (estimated using OLS):

(2) $Y_{cst} = \beta_0 + \beta_1$ (COMMONSENSE CONSUMPTION ACT)$_{st}$ + $\beta_2 Z_{st} + \beta_3 C_c + \beta_4 T_t + \beta_5 C_c$*TREND + ε_{cst}

where Z is as previously defined and where Y_{cst} are the city-specific fast-food prices in city *c* in state *s* in quarter *t*. We replace the state dummies from equation (1) with city dummies in equation (2), and we also replace the year fixed effects with a full set of fixed effects for each unique quarter in the data. We also control for smooth linear city-specific time trends in equation (2) in place of the state-specific linear time trends in equation (1). We continue to cluster the standard errors at the state level for the prices analysis, and the key identifying assumption remains: that the evolution of local prices in states without CCAs represents what would have occurred to prices in CCA states in the absence of policy adoption.

For the QCEW data, we write the two-way fixed effects model (also estimated using OLS) as:

(3) $Y_{st} = \beta_0 + \beta_1$ (COMMONSENSE CONSUMPTION ACT)$_{st}$ + $\beta_2 Z_{st} + \beta_3 S_s + \beta_4 T_t + \beta_5 S_s$*TREND + ε_{st}

where Y_{st} are the outcomes of interest (e.g., number of establishments, number of employees, and average weekly wage). All other variables are as described above in equation (1). For analyses of the number of McDonald's restaurants using UFOC data

democratic candidate in the most recent Presidential election was positively related to the number of years a CCA was in effect in a state. Our main findings are robust to inclusion of these political variables.

we modify equation (3) by studying as our outcome variable the number of McDonald's restaurants per state (total and separately by franchise-owned and company-owned). For both the QCEW and UFOC analyses we continue to cluster standard errors at the state level, and we weight these regressions by annual state population counts.[16]

4. Results

4.1 Weight-Related Health Investments

We begin by presenting descriptive statistics for the weight-related health investments and individual demographics in the BRFSS data. Table 2 presents means of key variables relating to CCA prevalence, weight-related outcomes, and demographic characteristics from the BRFSS 2000-2012 sample. We report descriptive statistics for the full sample (column 1) and separately for individuals living in states that ever adopt CCAs (column 2) and individuals living in states that do not adopt CCAs (column 3). For the full sample, we find that about 41 percent of respondents report trying to lose weight.[17] Fully 75 percent of the sample reports any past month exercise, while individuals report consuming 3.67 servings of fruits and vegetables per day. Average BMI in the sample is 27.61 (i.e., overweight), and nearly 30 percent of the sample is

[16] Results were qualitatively similar when we estimated models in logs instead of levels or estimated outcomes on a per capita basis within each state (available upon request).

[17] In addition we find that 77 percent of the sample report trying to lose or maintain weight (not reported in the table). Of those trying to lose or maintain weight, 26 percent report eating less to lose weight, while 29 percent reporting exercising more.

obese. Notably, 3 percent of the sample satisfies the definition of Class-3 obesity (i.e., BMI>40). In general we find few meaningful differences in average outcomes and characteristics for people in states with CCAs compared to those in states without CCAs.

In Table 3 we present estimates of the effects of CCAs that take explicit advantage of the plausibly exogenous variation in the timing of policy adoption across states on three measures of weight-related investments in health: the likelihood that an individual reports trying to lose weight (top panel), the number of servings of fruits and vegetables per day the individual reports consuming (middle panel), and the likelihood the person reported vigorous exercise in the past month (bottom panel). Each estimate in the table is the coefficient on the CCA variable from a separate difference in differences model with linear state trends. The first column reports estimates for the full sample, and the subsequent columns report estimates for models that restrict attention to people in each of the various (mutually exclusive) weight categories. We also report in the row just above each panel of estimates the average for each outcome variable across the various weight categories (and for the full sample for column 1). Notably, stated weight loss attempts are increasing in objective weight category, while fruit and vegetable consumption and exercise are both decreasing in objective weight category conditional on being at least normal weight. These patterns lend face validity that the data are plausibly measuring meaningful underlying constructs.

The results in the top panel of Table 3 provide strong evidence that Commonsense Consumption Acts increased stated weight loss attempts among heavy individuals. For example, the estimate in the top panel of column 7 of Table 3 indicates that CCAs were associated with a 3.3 percentage point increase in the likelihood an individual with Class-3 obesity reports trying to lose weight. Relative to the average rate for this group, this represents about a 4.3 percent effect. For obese individuals in column 5 we find a 4 percentage point increase in weight loss intentions, or a 6.2 percent effect relative to the mean, and this estimate is statistically significant. Notably, we find that most of the estimated effects of CCAs on weight loss attempts for individuals in the lighter weight categories are smaller in magnitude and statistically insignificant.

The middle and bottom panels of Table 3 further investigate investments in weight-related health by examining healthy food consumption and exercise, respectively, from the preferred specification with state and year fixed effects and linear state trends. For fruit and vegetable consumption in the middle panel of Table 3, we find strong evidence that CCAs were associated with statistically significant increases in healthy food consumption, and these effects are particularly large in magnitude and statistically significant for the heaviest individuals. Among individuals with Class-3 obesity in column 7, we estimate that CCAs increased consumption of fruits and vegetables by a statistically significant 0.25 servings per day, or about 7.3 percent relative to the sample average for that group. We estimate smaller (in both absolute and relative terms) but statistically significant increases for overweight individuals in column 4 and for the full sample in column 1, while the estimates for underweight and normal weight individuals are both small and statistically insignificant.[18] Turning to exercise participation in the bottom panel, we find no effects of CCAs: none of the estimates in the bottom panel of Table 3 are economically or statistically significant, indicating that CCAs did not meaningfully affect exercise activity for any group, let alone those most likely to have been treated by CCAs (i.e., heavy individuals).[19]

In Table 4 we report results for the effects of CCAs on weight for the full sample period 2000-2012. All estimates are from the preferred specification with state and year fixed effects and linear state trends. Notably, we find no meaningful effects of CCAs on average BMI, and these null findings are precise: the estimate in column 1 can rule out reductions in BMI larger than about 0.05, or about 0.2 percent relative

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[18] Sample sizes differ across the panels of Table 3 because the weight intentions questions were only asked by a subset of states and were not asked in 2004 or after 2005. There is an emerging understanding of appropriate inference in settings with a small number of clusters; when we estimated p-values using the Wild-cluster bootstrap method (Cameron et al. 2008), the estimate in the top panel of Table 3 is no longer statistically significant at conventional levels. Note that the weight intention outcome is the only one for which we have a small number of clusters due to the BRFSS sampling structure.

[19] Results for the number of minutes of exercise similarly did not indicate any meaningful effects of CCAs on exercise. In results not reported but available upon request we performed many robustness exercises for the results in Table 3. Restricting the healthy food consumption analysis to the same years the weight loss attempts questions were asked (2000-2003 and 2005) did not change the main result on healthy food consumption. Those same results were also robust to excluding 2011 and 2012 to account for a change in the BRFSS sampling structure that accounts for cellphones. Results were also robust to the choice of included states; for example, dropping each CCA adopter one at a time did not change the results. Results were also similar when we excluded the linear state trends, as well as when we allowed for quadratic state trends in addition to linear trends. Adding a control for whether the state would adopt a CCA in the next year did not materially alter the significant coefficients in the top and middle panels of Table 3, and the coefficient on future CCA adoption itself was not statistically significant.

to the average BMI in the population (27.9).[20] We similarly find very small CCA estimates on the likelihood of being at or above any of the standard weight thresholds. Even when measured against the very low rate of Class-3 obesity in the population (3.81 percent), the estimate in column 7 of Table 4 can rule out reductions in the likelihood of Class-3 obesity larger than about 2.6 percent.[21]

4.2 Fast Food Prices

In Table 5 we turn to prices of fast-food and other food items from the ACCRA data. Table 5 shows the coefficient on the CCA indicator separately for several food-related prices in fully saturated models that include controls for city and year-quarter fixed effects and linear city-specific time trends as well as all the other policy controls and state level controls. Three prices are for fast-food items that should have been directly affected by CCAs (a McDonald's Quarter Pounder with Cheese, a Pizza Hut medium cheese pizza, and a Kentucky Fried Chicken thigh and drumstick), and we also show results for prices of three food items that should not have been directly affected by CCAs (a half gallon of milk, a loaf of bread, and a ten pound sack of potatoes) as well as a summary grocery price index.

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[20] Event study specifications – which replace the single CCA indicator variable with a series of variables representing months relative to CCA adoption in the state – confirm the null findings on population average BMI and are presented in Figure 2 for a two year period around CCA adoption. Event study figures for the probability of being at or above other weight thresholds (e.g., obese) were similar and are available upon request. Note that because height and weight were asked in every survey wave we have good coverage of data surrounding each state's CCA; most other outcomes that were not asked in every year do not support such detailed models.

[21] Controlling for fast food prices had no meaningful effect on any of the BRFSS-based outcomes.

The results in Table 5 provide little evidence that CCAs affected prices at fast food restaurants. Estimates on the CCA variable for the three fast-food prices are all small in magnitude. The estimate in column 1 for a hamburger indicates that CCAs were associated with decreases in prices of about 1 percent, and the standard errors can rule out reductions in prices of larger than about 3 percent.[22] Coefficient estimates on the other food prices and the grocery price index are also small and statistically insignificant with the exception of the price of a sack of potatoes which we estimate a statistically significant reduction in price of about 6.6 percent.[23]

4.3 Fast Food Market Size

In Table 6 we turn to the effects of CCAs on market size as measured by the QCEW data on the number of establishments, the number of employees, and average weekly wages across industries in each state and year from 2000-2012. If CCAs provide valuable protection to new entrepreneurs considering opening a restaurant or to existing firm owners in fast-food restaurants who are considering closing establishments, we might expect to see CCAs increasing the number of fast-food establishments. Moreover, we would expect weaker or no effects on establishments in other industries.

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[22] Demand effects could differ across restaurants for a number of reasons; for example, some restaurant chains are likely more able to substitute items across a healthy/unhealthy food spectrum than others. As such, the predictions on specific fast food prices need not be the same.

[23] ACCRA also consistently tracked prices of other food items over our sample period, including: one pound of Blue Bonnet or Parkay margarine, one pound of bananas, one head of lettuce, one 11.5 ounce can of Maxwell House, Hills Brothers, or Folgers coffee, and one 29 ounce can of Hunt's, Del Monte, Libby's, or Lady Alberta peaches. Results on those other items are not shown but are available upon request. None indicated a statistically or economically significant association with CCAs.

Table 6 presents estimates of the effects of CCAs on the number of establishments separately for fast-food restaurants, full service restaurants, grocery stores, and gas stations with convenience stores. Each entry in Table 6 presents the coefficient on the CCA indicator in a fully-saturated separate model with state and year fixed effects and linear state trends; the top panel reports estimates for the number of establishments, the middle panel reports estimates for the number of employees, and the bottom panel reports estimates for the average weekly wage. At the top of each panel we also report the average within each industry for the respective outcomes.

The results in Table 6 provide some evidence that CCAs increased employment in fast-food, consistent with our preferred supply-side mechanism. The point estimates on the CCA variable for number of establishments and number of employees suggest 2.2 percent and 4.5 percent increases, respectively, and the latter is statistically significant at the ten percent level.[24] We find no effects on average weekly wages in fast-food, nor do we find effects on outcomes in other industries. The patterns in Table 6 suggest that CCAs may have increased employment in fast-food, primarily by increasing employment per store (as opposed to increasing numbers of stores).

Finally, in Table 7 we perform the parallel exercise on the number of McDonald's restaurants in each state from 2000-2012, controlling for state and year fixed effects, linear state trends, and all the state/time varying controls include in the

[24] Although we do not observe specific restaurants in the QCEW data, it is worth noting that the supply side effects could also vary across restaurants.

QCEW models above. Column 1 reports the effect of CCAs on the total number of McDonald's restaurants in the state, while columns 2 and 3 separate company-owned from franchise-owned stores, respectively. The results in column 1 indicate that CCAs did not significantly change the total number of McDonald's stores in a state, but the results in columns 2 and 3 indicate that CCAs significantly increased the number of McDonald's restaurants within the state that are company owned and decreased the number of McDonald's restaurants within the state that are franchise-owned.[25]

5. Discussion and Conclusion

The results above provide the literature's first evidence on the effects of Commonsense Consumption Acts – more commonly known as 'Cheeseburger Bills' – which greatly limit food companies' liability in weight-based claims. An interesting example of tort reform for a single industry, CCAs were adopted with the explicit goal of inducing people to take more personal responsibility for their weight (a central tenet of tort reform more generally). We are the first to examine how CCAs affected weight-related investments in health, finding that the laws did induce heavy people to take more care by consuming healthy food and trying to lose weight. We also show CCAs had no effects on population weight or food prices. We provide some evidence that CCAs increased employment in fast-food restaurants in a state. Finally, we show that

[25] Unfortunately, we do not have data on the frequency of obesity-related lawsuits, nor do we know who is named in such lawsuits (i.e., the McDonald's corporation or the individual franchisee). We are not aware of data that would allow us to measure lawsuits against different types of named defendants, which would be useful for interpretation.

for one specific restaurant targeted by obesity-related lawsuits (McDonald's), CCAs were associated with significant increases in the likelihood that restaurants are company-owned as opposed to franchise-owned.

There are some important limitations to the current study that should be noted. First, all of the weight, consumption, and exercise data are self-reported, though we have adjusted the reports of weight using the method outlined in Cawley and Burkhauser (2008). It is unlikely that reporting biases or errors are systematically correlated with the timing of CCA adoption, however. Second, although we observe healthy food consumption as measured by the number of servings of fruits and vegetables consumed per day, we do not observe either total food consumption or unhealthy food consumption. We interpret increases in healthy food consumption as likely evidence of substitution from unhealthy to healthy foods given the findings on stated weight loss intentions, though we cannot directly show that unhealthy consumption falls following adoption of CCAs.[26] Third, we do not observe the activities of state public health departments to reduce adult obesity. While we have controlled flexibly for state trends and other state initiatives that have been previously studied in the literature on determinants of weight, we cannot rule out that there are

[26] While we cannot directly comment on unhealthy food consumption, we can disaggregate the components of fruit and vegetable consumption for the period 2000-2009 when the specific items were asked consistently. In results not reported but available upon request we find that the CCA-induced increases in fruits and vegetables consumption is concentrated among fruits, fruit juices, and green salads as opposed to potatoes and carrots. To the extent that the former are likely to be relatively healthier than the latter, this is suggestive evidence in favor of an overall improvement attributable to CCAs.

other state-specific factors (e.g., outreach) that are correlated both with CCA adoption and with the weight-related outcomes we study here.[27]

Fourth, we do not have data on the frequency and nature of obesity-related lawsuits. This means that we do not, for example, know how frequently individual franchisees are named in such lawsuits (as opposed to the larger corporate entity), and we similarly do not know how concentrated the legal activity is toward individual companies. This information would be useful for interpreting our findings on prices, market size, and McDonald's restaurants.[28]

Finally, we do not observe other firm responses to CCAs beyond the outcomes in the QCEW and UFOC for McDonald's. For example, it is plausible that McDonald's restaurants located in states with CCAs were slower to roll out 'healthier' menu options (such as salad, milk, and fruit), eliminate the 'Super Size' option, or adopt packaging with calories posted on the menu (to the extent that such decisions are not made nationally at the company level) following CCA adoption, but we do not observe these outcomes. We also do not know how seriously food companies take such weight-based claims (either in the presence or absence of CCAs), though we do know that there have been multiple high profile lawsuits filed and that the food industry has lobbied strenuously for CCAs, suggesting a nontrivial legal threat.

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[27] We did not, however, find significant associations between state expenditures on health or personal health care and the presence of a state CCA.

[28] The UFOC documents do list pending litigation against the company to inform potential franchisees about legal risk, but annual versions of these documents over our sample period were prohibitively expensive (approximately $270 per restaurant per year).

Relatedly, we do not observe how liability insurance premiums responded to CCAs. While the protection afforded by the laws should have reduced insurance premiums for fast food companies, we cannot directly demonstrate this channel. A better understanding as to whether and to what extent fast food companies and the insurance industry responded to CCAs is an important area for future research.

Despite these limitations, our results are the first in the literature to evaluate the effects of CCAs and should provide valuable information to policymakers considering similar adoptions (as occurred as recently as 2013 in North Carolina). In doing so, our results provide novel new empirical evidence supporting a central tenet of tort reform that parties take more care and also show that industry-specific tort reforms can affect market-wide outcomes.

Figure 1
Searches for 'Cheeseburger Bill' – Google Trends

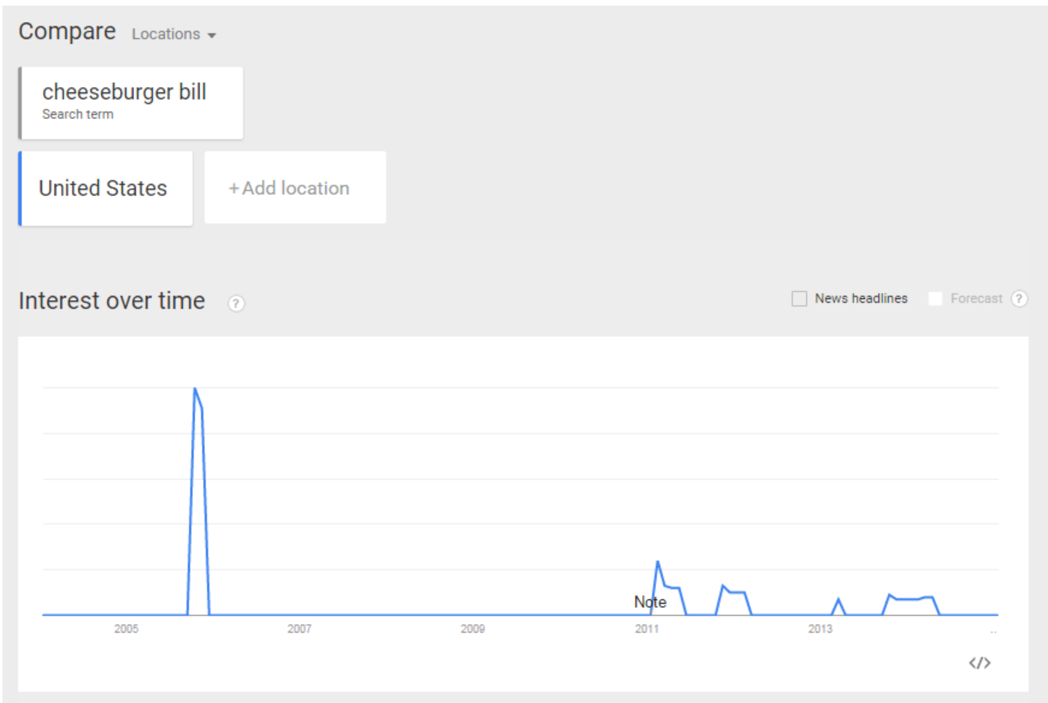

Figure 2
Event Study Estimates of CCAs on Population Average BMI

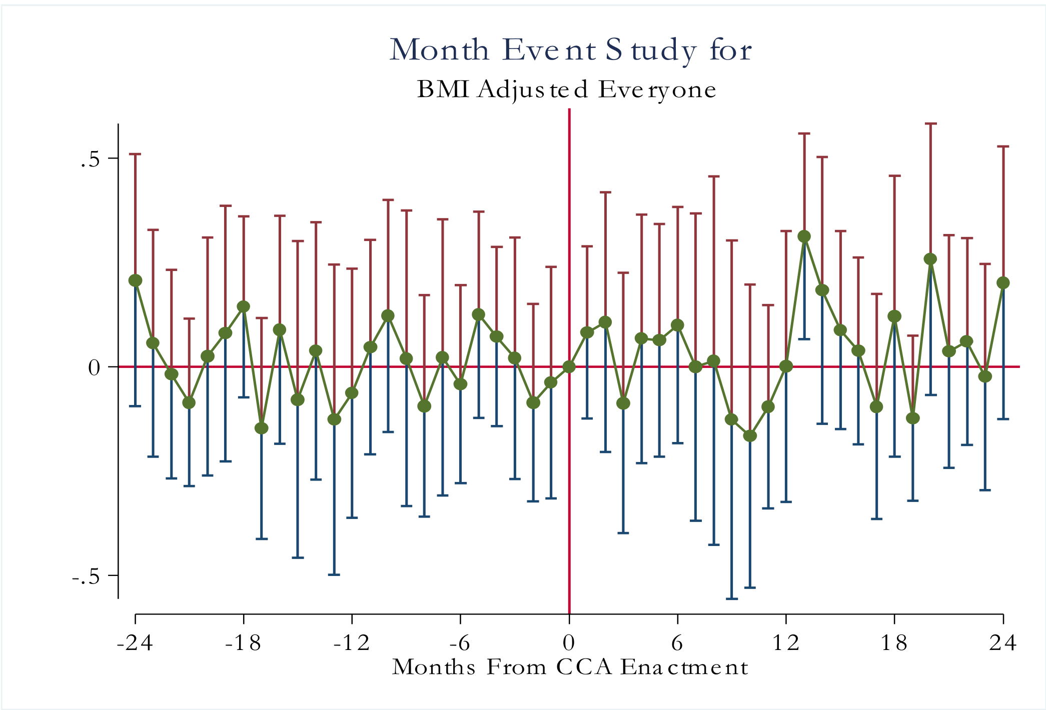

Table 1:
Timing of CCA Adoption

	Enacted	Effective	Citation
AL	--	5/23/20 12	ALA. CODE §§ 6-5-730 to 6-5-736 (2012)
AZ	4/8/2004	--	ARIZ. REV. STAT. ANN. §§ 12-683, 12-688 (2012)
CO	5/17/200 4	5/17/20 04	COLO. REV. STAT. §§ 13-21-1101 to 13-21-1106 (West 2012)
FL	5/21/200 4	5/21/20 04	FLA. STAT. ANN § 768.37 (West 2012)
GA	5/14/200 4	5/14/20 04	GA. CODE ANN. §§ 26-2-430 to 26-2-436 (West 2102)
ID	4/2/2004	7/1/200 4	IDAHO CODE ANN. §§ 39-8701 to 39-8706 (2012)
IL	7/30/200 4	1/1/200 5	ILL. COMP. STT. ANN 43/1 to 43/20 (West 2012)
IN	3/17/200 6	7/1/200 6	IND. CODE ANN. §§ 34-30-23-1 to 34-30-23-3 (West 2012)
KS	4/15/200 5	--	KAN. STAT. ANN. § 60-4801 (2012)
KY	3/8/2005	--	KY. REV. STAT. ANN §§ 411.600 to 411.640 (West 2012)
LA	6/2/2003	6/3/200 3	LA. REV. STAT. ANN § 9:2799.6 (2012)
ME	6/9/2005	--	ME. REV. STAT. ANN. tit. 14, § 170 (2012)
MI	10/7/200 4	10/7/20 04	MICH. COMP. LAWS ANN. § 600.2974 (West 2012)
MO	6/25/200 4	1/1/200 5	MO. ANN. STAT. § 537.595 (West 2012)

ND	3/3/1/2005	3/31/2005	N.D. CENT. CODE §§ 19-23-01 to 19-23-03 (2012)
OH	--	4/7/2005	OHIO REV. CODE ANN. § 2305.36 (West 2012)
OK	5/22/2009	11/1/2009	OKLA. STAT. ANN. tit. 76, §§ 34 to 37 (West 2012)
OR	7/27/2005	--	OR. REV. STAT. ANN. § 30.961 (West 2012)
SD	3/9/2004	--	S.D. CODIFIED LAWS §§ 21-61-1 to 21-61-4 (2012)
TN	4/30/2004	7/1/2004	TENN. CODE ANN. § 29-34-205 (West 2012)
TX	6/18/2005	6/18/2005	TEX. CIV. PRAC. & REM. CODE ANN. §§ 138.001 to 138.004 (Vernon 2012)
UT	3/19/2004	5/3/2004	UTAH CODE ANN §§78b-4-301 to 78B-4-306 (West 2012)
WA	3/26/2004	6/10/2004	WASH. REV. CODE ANN. § 7.72.070 (West 2012)
WI	--	3/28/2008	WIS. STAT. ANN. § 895.506 (West 2012)
WY	2/24/2005	--	WYO. STAT. ANN. §§ 11-47-101 to 11-47-103 (2012)

Source: Wilking & Daynard (2013). This list excludes Minnesota which passed a CCA that was subsequently vetoed by its Governor in 2011. It also excludes North Carolina which adopted a CCA that did not take effect until October 2013, after the end of our sample period.

Table 2:
Descriptive Statistics, 2000-2012 BRFSS Data

Variable	(1) Full sample	(2) Individuals in states that ever adopt a CCA	(3) Individuals in states that never adopt a CCA
Weight Intentions			
Trying to lose weight	0.41	0.40	0.42
# servings of fruits and vegetables per day	3.67	3.59	3.76
Any exercise last month	0.75	0.75	0.75
Weight Outcomes			
BMI	27.61	27.70	27.52
Underweight (BMI: less than 18.5)	0.02	0.02	0.02
Normal weight (BMI:18.5 - 24.9)	0.34	0.33	0.34
Overweight (BMI: 25.0 to 29.9)	0.36	0.36	0.37
Obese (BMI: 30.0 & above)	0.18	0.18	0.18
BMI > 35	0.07	0.07	0.07
Class-3 obesity (BMI > 40)	0.03	0.03	0.03
Demographics			
Female	0.51	0.51	0.51
Age	45.99	46.06	45.92
Less than high school degree	0.12	0.12	0.13
High school degree	0.30	0.31	0.28

Some college	0.27	0.28	0.26
BA or more			
Married	0.58	0.59	0.56
Black	0.10	0.10	0.10
Asian	0.03	0.02	0.04
Other race	0.04	0.03	0.04
Hispanic	0.14	0.11	0.18

Weighted means.

Table 3:
CCAs Induced Modest Improvements in Weight-Related Health Investments among Heavy Individuals
BRFSS 2000-2012

Sample is ➔	(1) Full sample	(2) Under weight BMI<18.5	(3) Normal Weight 18.5≤BMI< 25	(4) Overweight 25≤BMI<30	(5) Obese 30≤BMI<35	(6) Severe Obese 35≤BMI<40	(7) Class III Obese 40≤BMI
Trying to lose weight (avg ➔)	.42	.03	.20	.46	.65	.72	.77
CCA	0.009	0.008	-0.018	-0.001	0.040**	-0.001	0.033
	(0.019)	(0.023)	(0.013)	(0.013)	(0.013)	(0.014)	(0.025)
R squared	.05	.06	.08	.09	.05	.04	.04
N	590653	9669	191266	188779	87925	31517	15076
# fruit/veggie per day (avg ➔)	3.676	3.678	3.862	3.668	3.533	3.441	3.380
CCA	0.058**	0.113	0.061	0.130***	0.063	0.0014	0.246**
	(0.028)	(0.113)	(0.042)	(0.033)	(0.0654)	(0.051)	(0.105)
R squared	.05	.07	.05	.05	.05	.04	.05
N	2339201	33913	679503	752056	387990	149813	76680
Exercised past month (avg ➔)	.74	.66	.80	.77	.70	.63	.54
CCA	-0.005	-0.003	-0.008	-0.004	-0.001	0.010	-0.012
	(0.004)	(0.023)	(0.006)	(0.007)	(0.006)	(0.009)	(0.13)
R squared	.08	.09	.08	.08	.07	.08	.08
N	4404754	63149	1260866	1414696	737495	285528	146694

* significant at 10%; ** significant at 5%; *** significant at 1%. Results are from linear probability models and use BRFSS sampling weights. All models include controls for individual demographic characteristics (age, gender, race, education level, marital status); state, month, and year fixed effects; linear state-specific time trends; soda and cigarette taxes; menu labelling laws; complete streets laws; other tort-reforms; state unemployment rates; and state demographic characteristics (fraction female, fraction black, Hispanic, and other races, fraction of individuals of age 21 to 64 and 64 to 65, fraction of individuals with high school degree and with some college or more, and fraction below the federal poverty level). Standard errors clustered at the state level. All include weights provided by BRFSS.

Table 4:
CCAs Had No Effects on Population Weight
BRFSS 2000-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome is ➔	BMI	Pr(BMI≥18.5)	Pr(BMI≥20	Pr(BMI≥25)	Pr(BMI≥30)	Pr(BMI≥35)	Pr(BMI≥40)
Sample average ➔	27.88	.984	.954	.661	.299	.110	.038
CCA	0.029	-0.001	0.000	0.000	0.004	0.003	0.001
	(0.042)	(0.001)	(0.001)	(0.003)	(0.004)	(0.002)	(0.001)
R squared	.062	.009	.025	.065	.038	.026	.018
N	3,931,002	3,931,002	3,931,002	3,931,002	3,931,002	3,931,002	3,931,002

* significant at 10%; ** significant at 5%; *** significant at 1%. See notes to Table 3.

Table 5:
CCAs Had No Systematic Effects on Fast Food or Other Food Prices
ACCRA/C2ER 2000-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome is ➔	McDonald's quarter pounder with cheese	Pizza Hut medium cheese pizza	Kentucky Fried Chicken thigh and drumstick	Half gallon of milk	Loaf of bread	Ten pound sack of potatoes	Grocery price index
Average price ➔	2.52	11.01	1.18	2.16	1.28	3.81	99.95
CCA	-0.028	-0.007	0.024	0.039	-0.024	-0.252***	-0.767
	(0.027)	(0.086)	(0.017)	(0.031)	(0.018)	(0.060)	(0.839)
R-squared	.69	0.76	0.67	0.77	0.75	0.76	0.88
N	14938	14938	14938	14938	14938	14398	14398

* significant at 10%; ** significant at 5%; *** significant at 1%. Each entry is from a separate model and shows the coefficient on the CCA variable on the local price for the item at the top of each column. All models include controls for city and quarter fixed effects; linear city-specific time trends; soda and cigarette taxes; menu labelling laws; complete street laws; other tort-reforms; and state unemployment rates; and state demographic characteristics (fraction female, fraction black, Hispanic, and other races, fraction of individuals of age 21 to 64 and 64 to 65, fraction of individuals with high school degree and with some college or more, and fraction below the federal poverty level). Standard errors are clustered throughout at the state level. All include state population weights.

Table 6:
Some Evidence that CCAs Increased Employment in Fast Food
QCEW 2000-2012

Industry is ➔	(1) Limited-service restaurants (fast-food)	(2) Full-service restaurants	(3) Grocery stores	(4) Gas station with convenience store
Number of establishments (avg ➔)	3781.9	3983.6	689.7	1786.9
CCA	82.97	-20.19	11.01	-.33.01
	(79.18)	(85.73)	(24.68)	(46.43)
R squared	.99	.99	.99	.99
N	662	662	660	660
Number of Employees (avg ➔)	65316.5	84469.62	13763.27	146682.5
CCA	2908.49*	1655.95	321.87	-523.33
	(1584.87)	(1644.22)	(372.15)	(409.95)
R squared	.99	.99	.99	.99
N	662	662	660	660
Average weekly wage (avg ➔)	246.83	300.31	894.26	350.78
CCA	-2.29	-0.18	10.84	-1.46
	(1.89)	(2.00)	(7.37)	(4.33)
R squared	.98	.99	.97	.93
N	662	662	660	660

* significant at 10%; ** significant at 5%; *** significant at 1%. Each entry is from a separate model and shows the coefficient on the CCA variable on the outcome at the top of each panel (number of establishments, number of employees, and average weekly wage) for the industries in each column

(limited service restaurants, full service restaurants, grocery stores, and gas stations with convenience stores). All models include controls for state and year fixed effects; linear state-specific time trends; soda and cigarette taxes; menu labelling laws; complete street laws; other tort-reforms; and state unemployment rates; and state demographic characteristics (fraction female, fraction black, Hispanic, and other races, fraction of individuals of age 21 to 64 and 64 to 65, fraction of individuals with high school degree and with some college or more, and fraction below the federal poverty level). Standard errors are clustered throughout at the state level. All include state population weights.

Table 7:
CCAs Increased Company-Owned and Reduced Franchise-Owned McDonald's
UFOC/FDD 2000-2012

	(1)	(2)	(3)
Outcome is ➔	Total # McDonald's in the state	# Company-Owned McDonald's in the state	# Franchise-Owned McDonald's in the state
(avg ➔)	269.07	36.43	232.65
CCA	2.27	11.16***	-8.891**
	(2.64)	(3.62)	(3.86)
R squared	0.99	0.98	0.99
N	662	662	662

* significant at 10%; ** significant at 5%; *** significant at 1%. Each entry is from a separate model and shows the coefficient on the CCA variable. All models include controls for state and year fixed effects; linear state-specific time trends; soda and cigarette taxes; menu labelling laws; complete street laws; other tort-reforms; and state unemployment rates, fraction female, fraction black, Hispanic, and other races, fraction of individuals of age 21 to 64 and 64 to 65, fraction of individuals with high school degree and with some college or more, and fraction below the federal poverty level). Standard errors are clustered throughout at the state level. All include state population weights.

APPENDIX A
Sample Commonsense Consumption Act (Missouri)

TITLE 36. STATUTORY ACTIONS AND TORTS (Chs. 521-538)

CHAPTER 537. TORTS AND ACTIONS FOR DAMAGES

COMMONSENSE CONSUMPTION FUND

§ 537.595 R.S.Mo. (2014)

§ 537.595. Citation -- definitions -- immunity from liability for claims relating to weight gain or obesity, when, exceptions -- petition, contents -- effective date

1. This section may be known as the "Commonsense Consumption Act".

2. As used in this section, the following terms mean:

(1) "Claim", any claim by or on behalf of a natural person, as well as any derivative or other claim arising therefrom asserted by or on behalf of any other person;

(2) "Generally known condition allegedly caused by or allegedly likely to result from long-term consumption", a condition generally known to result or to likely result from the cumulative effect of consumption and not from a single instance of consumption;

(3) "Knowing or willful violation of federal or state law", that:

(a) The conduct constituting the violation was committed with the intent to deceive or injure consumers or with actual knowledge that such conduct was injurious to consumers; and

(b) The conduct constituting the violation was not required by regulations, orders, rules, or other pronouncements of, or statutes administered by, a federal, state, or local government agency;

(4) "Other person", any individual, corporation, company, association, firm, partnership, society, joint-stock company, or any other entity, including any governmental entity or private attorney general.

3. Except as exempted in subsection 4 of this section, a manufacturer, packer, distributor, carrier, holder, seller, marketer, retailer, or advertiser of a food, as defined in the Federal Food, Drug, and Cosmetic Act *(21 U.S.C. 321*(f)), as amended, but shall not include alcoholic beverages, or an association of one or more such entities shall not be subject to civil liability under any state law, including all statutes, regulations, rules, common law, public policies, court or administrative decisions or decrees, or other state actions having the effect of law, for any claim arising out of weight gain, obesity, or a health condition associated with weight gain or obesity.

4. The provisions of subsection 3 of this section shall not preclude civil liability where the claim of weight gain, obesity, health condition associated with weight gain or obesity, or other generally known condition allegedly caused by or allegedly likely to result from long-term consumption of food is based on:

(1) A material violation of an adulteration or misbranding requirement prescribed by statute or regulation of the state of Missouri or the United States and the claimed injury was proximately caused by such violation; or

(2) Any other material violation of federal or state law applicable to the manufacturing, marketing, distribution, advertising, labeling, or sale of food, provided that such violation is knowing and willful, and the claimed injury was proximately caused by such violation. The provisions of subsection 3 of this section shall not preclude civil liability for breach of express contract or express warranty in connection with the purchase of food.

5. In any action exempted under subdivision (1) or (2) of subsection 4 of this section, the petition initiating such action shall state with particularity the following: the statute, regulation, or other state or federal law that was allegedly violated, the facts that are alleged to constitute a material violation of such statute or regulation, and the facts alleged to demonstrate that such violation proximately caused actual injury to the plaintiff. In any action exempted under subdivision (2) of subsection 4 of this section, the petition initiating such action shall also state with particularity facts sufficient to support a reasonable inference that the violation occurred with the intent to deceive or injure consumers or with the actual knowledge that such violation was injurious to consumers. For purposes of applying this section the pleading requirements under this section are deemed part of state substantive law and not merely procedural provisions.

6. In any action exempted under subsection 4 of this section, all discovery and other proceedings shall be stayed during the pendency of any motion to dismiss

unless the court finds upon the motion of any party that particularized discovery is necessary to preserve evidence, resolve the motion to dismiss, or to prevent undue prejudice to that party. During the pendency of any stay of discovery under this subsection and unless otherwise ordered by the court, any party to the action with actual notice of the allegations contained in the petition shall treat all documents, data compilations, including electronically recorded or stored data, and tangible objects that are in the custody or control of such party that are relevant to the allegations as if they were the subject of a continuing request for production of documents from an opposing party under the Missouri rules of civil procedure.

7. The provisions of this section shall apply to all covered claims pending on or filed after January 1, 2005, regardless of when the claim arose.

HISTORY: L. 2004 H.B. 1115 § 537.900

NOTES:

EFFECTIVE 1-1-05

CHAPTER 3

THE IMPACT OF OBESITY ON WAGES: THE ROLE OF PERSONAL INTERACTIONS AND JOB SELECTION

with Andrea Moro and Tommaso Tempesti

1. Introduction

This paper investigates how the relationship between obesity and wages is affected by the level of inter-personal interactions required in the workplace. A vast literature studies how obesity affects labor market outcomes.[1] In an influential paper, Cawley (2004) finds a negative effect of obesity on wages for white women. One conjecture for this result is that obesity results in social stigma, affecting men and women differently (the so-called "beauty premium").[2] Undoubtedly, at least in modern western societies, body weight may affect a person's appearance and other people's perception of the person's market-relevant traits.[3] The effect of stigma or perceptions on wages is more prominent for workers employed in jobs requiring frequent inter-personal interactions. There may also be indirect effects if more obese workers are induced to self-select into jobs requiring fewer interactions, which in turn affects the observed job-specific relationship between obesity and wages.[4]

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[1] See Averett, Susan et al. (1996), Cawley (2004), Pagan et al. (1997), Register et al. (1990)

[2] A growing literature documents the relationship between wages and physical appearance, which is at the root of the "beauty premium" conjecture Hamermesh et al. (1994).

[3] See Baum et al. (2011) for an analysis of the socio-economic causes of obesity.

[4] Another explanation from the sociological literature is that obesity has a more adverse impact on the self-esteem of white female than on that of other groups, but Averett, Susan et al. (1996) do not find empirical support for such conjecture.

There exist at least two channels that may generate a higher "beauty premium" in jobs that require social interaction. One possibility is that overweight individuals, being perceived as less attractive in the workplace, may be discriminated against by co-workers, customers, or employers who have a taste against interacting with individuals with abnormal weight (Becker (1957)). Therefore, jobs that require more frequent interactions will display a stronger relationship between weight and labor market outcomes. A second theory is that co-workers, customers, or employers statistically discriminate against overweight individuals because they believe they are, on average, less productive (see Phelps (1972)). According to this theory, there are no preferences or perceptions against overweight individuals, but a (perhaps biased) belief that these workers, carrying a visible marker, are, on average, less productive. If the relevant outcome variable (such as productivity) is imperfectly observed, weight is used as a proxy in order to improve the decision-makers' choice. Therefore, otherwise-identical individuals, but carrying a different body weight, are treated differently. Both conjectures carry the same implication: workers in jobs that require more interactions with customers or co-workers will be more greatly affected by their appearance.

When these effect are economically relevant, workers may, depending on their body mass, self-select into jobs requiring different levels of interpersonal interactions. We investigate the effect of obesity on wages after accounting explicitly for this selection of workers into jobs. We adopt a job selection model to correct for the selection bias that occurs when selection is ignored.[5] To this end, we merge two sources of data. One is the National Longitudinal Survey of the Youth 1979, a nationally representative sample of men and women aged between 14 and 22 when first interviewed. This dataset contains detailed information about the respondents, including their weight, height, employment status, occupation and wages, and has been widely used to explore the connection between obesity and wages. The second source of data is the O*Net database, which contains information about occupations. O*Net classifies occupations according to hundreds of standardized descriptors illustrating each occupations' characteristics and the worker's required skills.[6] From this information we use factor analysis to construct a variable measuring the level of interpersonal interactions required by each occupation.

The literature on the effects of obesity on wages typically regresses wages on obesity status, sometimes using fixed effects or instrumental variables to account for the possibility of reverse causality (i.e poorer individuals have less time and monetary resources to eat healthy food, attend the gym, etc.). We complement this approach by

[5] Previous research has noted that people with different body mass choose different jobs. Han et al. (2011), Morris (2006) Our contribution is to account explicitly for the endogenous selection of people with different obesity into different types of jobs

[6] O*Net provides for each occupation, values to descriptors such as "Contact with Others" (How much does this job require the worker to be in contact with others, face-to-face, by telephone, or otherwise in order to perform it?), Face-to-Face Discussions (How often do you have to have face-to-face discussions with individuals or teams in this job?), or Work With Work Group or Team (How important is it to work with others in a group or team in this job?). The full set of descriptors used in our analysis is listed in Appendix.

adopting a Roy model of self-selection.[7] In this framework, a person chooses between two types of jobs; in our environment, jobs requiring a high or low level of interpersonal interactions. Each job-type requires specific skills, which are distributed differently among workers of different body mass. Workers of different body mass may have a comparative advantages in performing different types of jobs. Workers self-select the job that gives them the highest expected earnings. Ignoring this selection biases a standard regression of wages on Body Mass Index (BMI), the standard measure of obesity. For example, obese individuals may disproportionally find it more advantageous to seek employment in jobs requiring fewer interactions, where obesity has less impact. Because we do not observe the wages that obese workers would obtain in jobs requiring interpersonal interactions, a standard OLS regression of wages on BMI would return a biased coefficient.

To correct for the selection bias, we adopt the two-step approach introduced by Willis et al. (1979). A selection equation explains the discrete choice between the two job-types. A wage equation is estimated accounting for the selection bias recovered from the first equation. To improve the identification of the bias correction, we include in the first stage equation a variable that affects the relationship between obesity and wages only indirectly, through job choice. Because the NLSY has family information, we include in the selection equation information about family members' job

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[7] See Roy (1951); Heckman (1990). Two seminal papers of this empirical literature are Willis et al. (1979); Heckman et al. (1985); Borjas (1987).

characteristics.[8] The intuition for this exclusion restriction is that individual may find it easier, regardless of their obesity, to find jobs requiring skills that are similar to the jobs of their family members, either because of direct referrals, or because family members correlate on other required skills.

To account for reverse-causality between obesity and wages, that is, the possibility that low-wages cause obesity (perhaps because poorer families consume more fattening food), and the possibility that unobserved correlates affect both obesity and wages, our wage equation includes individual fixed effects in our benchmark specification.[9]

Our results confirm, using up-to-date information, the literature's result that the negative relationship between obesity and wages is significant only in some demographic groups, notably white women. However, accounting for selection, we see that the negative relationship between obesity and wages is mostly coming from the subset of workers in jobs needing a high level of social interactions. In such jobs, the relationship is stronger than average, and understated in a regression that does not correct for the selection bias. In jobs that require a lower level of social skills, the relationship between jobs and wages is smaller, not statistically significant, and overstated when not accounting for the selection bias. Our analysis is most related to the research in Cawley (2004), Han et al. (2009) and Shinall (2014). Cawley (2004) focuses on the effect of obesity on wages. To account for sources of bias, such as

[8] We include the "closest" sibling's job type. This is described in detail in the empirical approach section.
[9] The same approach is adopted in Cawley (2004)'s benchmark specification

omitted variables and reverse causality, he follows different strategies: controlling for lagged values of BMI, using siblings' weight as an instrument for the respondent's weight, and, in his preferred specification, including individual fixed effects. However, he does not control for job type. Han et al. (2009) introduce the use of job characteristics. They use data from the Dictionary of Occupational Titles (DOT - a precursor of O*Net, the database we use) to obtain information about the jobs the individuals are performing. They find that the association with BMI and wages is stronger for jobs that require more interpersonal skills. Finally, Shinall (2014) also uses job characteristics to compare the effects of obesity on wages for that require some physical skills versus jobs that require some social skills. However, in that paper the data does not offer the possibility of controlling for individual fixed effects and there is no specific accounting for selection bias in the wage-obesity estimation. Our paper's contribution is (i) to extend the analysis to data including the most recent years, (ii) to use a broader range of information on job characteristics, and, (iii) to explicitly model workers' job selection to verify if job sorting affects the magnitude of the effects of obesity on wages.

2. Data

The National Longitudinal Survey of the Youth is a nationally representative sample of the American youth. Respondents were sampled first in 1979 when they were between 14 and 22 years of age, every year until 1994, and every two years since. We use 12 years of data ranging from 1982 thru 2006.[10] This is a widely used survey in research on employment and obesity. The survey contains detailed questions about employment and tenure, household environment and structure, and personal health information like height and weight. For the empirical analysis we use information on gender, race, education status, marital status, number of children in the household, Armed Forces Qualification Test (AFQT), age of youngest child, highest grade achieved by mother and father, work tenure, years of experience, region dummies and create dummies representing "missing" values for each of the variables to account for different types of misreporting. The estimation sample included 97,092 person-years (46,180 person-years for women and 50,912 person-years for men) after excluding sample individuals who were pregnant at the time of interview, individuals under the age of 18, and individuals who did not have full employment or weight information. About 91 percent of our sample is employed and has tenure of 141 weeks at current job.

To construct the main dependent outcome we obtain information on the hourly wage of each individual and then, similar to Cawley (2004), we top-code the hourly wage at 500 such that anyone above a 500 hourly wage gets coded at 500. We normalize wages to 2010 using a consumer price index. Finally we take the log of real hourly wages and

[10] The years that we end up using for our sample are: 1982, 1985, 1986, 1988, 1989, 1990, 1992, 1993, 1994, 1996, 1998 and 2006. We cannot use some of the survey years because the employment status recode we use to match the data with the O*Net database was not created in 2000-2004, or in 2008-present because the CPS section on activity in the week before the survey was not included in those rounds. In addition, the survey does not include body weight information in some of the years between 1982 and 1998.

this is the variable we use on the left hand side to estimate the effects of obesity on wages. Our estimation sample shows that the average wage for men is $ 18.65 while the average wage for women is $ 13.94, when focusing on the difference between normal weight category and obese category, we estimate a wage differential of $ -2.33 for women and a $ 9.08 for men, that is obese men earn -on average- higher hourly wages than normal weight men.

In order to calculate body mass index we pool the responses for all years that recorded self-reported weight. Height was assumed to be equal to the height recorded in 1985, when respondents were between 20 and 27 years of age. We adopt the standard practice in this literature to correct weight and height for self-reporting error using the procedure proposed by Lee et al. (1995), exploiting the information on the relationship between true and reported height and weight collected in the National Health and Nutrition Survey. In our estimation sample the average BMI is around 25.92 for women and 26.65 for males. For women almost 22% of our sample falls in the obese or higher category while for males it is around 21%.

We identified the respondents' job in every year in which they are employed, as well as the associated hours worked and the hourly wage received at each job. If individuals are working multiple jobs, they report one of the jobs as their main job and we use that information as their main source of employment. The NLSY offers census occupation codes for all jobs. We use it to merge the NLSY data with data from the 2010 revision of the Occupational Information Network (O*Net) database, containing detailed information about job characteristics. For each job, the O*Net offers a description of tasks, tools and technologies, knowledge, skills, abilities, work activities, work context and education required. For each piece information, the database reports two numerical values, the "importance" and the "level". The "importance" is a numeric value ranging from 1 to 5 representing how important a specific skill to perform the job; the "level" is a numeric value ranging from 1 to 7 representing the expertise of the skill needed to perform the job. Essentially, the importance factor is a measure of how often the skilled is used, while the "level" measures the needed quality of the skill. For example, when comparing a job of a secretary versus a journalist it is possible that the "importance" of the skill "writing" is the same for both occupations but the "level" is higher for the journalist than the secretary. The database contains a total of 277 job descriptors. Figure 1 reports, as an example, one of the questions relevant to infer the importance and level of inter-personal interactions required to perform the respondent's job.

We use information from the categories "skills" and "abilities" in O*Net to calculate how important interpersonal interactions are to perform any given job and use factor analysis to create **a** univariate index. We selected 17 work activities and job skills that, to our judgement, are most important for discerning the important interpersonal interactions. These questions assess how relevant it is, to perform a job, to communicate with others (supervisors, co-workers, or customers), to resolve conflicts or coordinate other individuals**, to** speak, negotiate, coordinate others, etc. The full list of job descriptors we selected can be found in the Appendix B.

Using the "level" values for each task and skill, we reduce the dimensionality of the information and use factor analysis to extract a single variable measuring the degree of required social interactions on the job.[11] Once this index is normalized, it resembles a z-score variable. We use the median of this standardized score as a threshold to categorize jobs in two categories: those requiring high and low levels of interpersonal interaction. In our estimation sample we find that 71% of women are occupied in high social jobs, whereas only 60% of males are employed in these job. In Figure **2** we plot the predicted logarithm wage across the different levels of BMI for jobs requiring a high level of interactions versus jobs requiring low levels of social interactions for two groups white women and white males.[12]

These figures illustrate several facts. First, jobs requiring interactions usually pay higher wages than other jobs. Second, the relationship between BMI, wages, and job-type is different between males and women. The relationship between wages and BMI is close to linear for women, and decreasing in BMI, the same relationship for males looks closer to a quadratic (i.e. non-linear) relationship. Finally, the wage-gap between high and low-social jobs for white women is decreasing in BMI, suggesting that there is a

[11] We replicated the analysis using the "importance" indicators, which lead to similar results. In our main specifications we use the "level" measure since they display more variations than the "importance" measure.

[12] The other race-gender groups can be found in the appendix

higher penalty of BMI on wages for women in jobs requiring interactions, presumably because weight and appearance is more important in such jobs. The same penalty is not **as evident for males' wages as** for women' wages.

Figure 3, displays the distribution of BMI by type of job and gender. Even though there seems to be a higher accumulation of individuals with BMI around each **group's** average for the high-social job, for both genders, the average BMI between high and social jobs is not that different from each other.

3. Empirical Framework

In this section we describe the empirical strategy adopted and the structure behind the two-step estimation procedure to account for the selection into the different types of jobs. We start by assuming that individuals can be employed in two types of jobs $\boldsymbol{j}$, requiring $(\boldsymbol{j = 1})$ or not requiring $(\boldsymbol{j = 2})$ a high level of social interactions. Let $\boldsymbol{w_{itj}}$ be the log wage of individual $\boldsymbol{i}$ at time $\boldsymbol{t}$ employed in job $\boldsymbol{j} \in \{\boldsymbol{1, 2}\}$, which we assume to depend on a set of covariates $\boldsymbol{X_{it}}$, including her or his obesity at time $\boldsymbol{t}$, $\boldsymbol{BMI_{it}}$, and, depending on the adopted specification, individual and time fixed effects:

$$w_{itj} = \alpha_j X_{it} + \epsilon_{jit} \quad (1)$$

Let Z_{it} denote a set of observed variables that influence the job choice of individual i at time t without affecting wages, which may include a subset of variables in X_{it}, and let J_{it} be a latent variable determining the job-type choice of the individual:

$$J_{it} = \beta Z_{it} - \epsilon_{3it}$$

with job choice $j_{it} = 1$ if $J_{it} \geq 0$, and $j_{it} = 2$ otherwise. Hence,

$$Pr(j_{it} = 1) = Pr(\epsilon_{3it} \leq \beta Z_{it}) \quad (2)$$

Notice that in this approach one will not need to have elements in X_{it} that do not belong in Z_{it}, in other words the methodology can identify the parameters from its functional form without having any exclusion restrictions. In our case we do have one parameter that is included in Z_{it} and that is not included in X_{it}, this is the type of job the closest sibling has. This exclusion restriction will improve identification of the parameter of interest (namely α).[13]

The job choice selects people into different jobs according to preferences. Therefore, wages are not observed from a random sample of the population and an OLS regression of the wage equation delivers biased coefficients because:

$$E(w_{it1}|j_{it} = 1) = \alpha_1 X_{it} + E(\epsilon_{1it}|j_{it} = 1) = \alpha_1 X_{it} + E(\epsilon_{1i}|\epsilon_{3it} \leq \beta Z_{it})$$

$$E(w_{it2}|j_{it} = 2) = \alpha_2 X_{it} + E(\epsilon_{2it}|j_{it} = 2) = \alpha_1 X_{it} + E(\epsilon_{2it}|\epsilon_{3it} > \beta Z_{it})$$

[13] In order to identify the "closest" sibling of each individual we create a measure of distance between each individual and all of their siblings. This measure is the sum of the squared difference between the siblings' age, race and gender. This means that the "closest" possible sibling is one that have the same age, race and gender.

Assuming joint normality of the error term vector $[\epsilon_1, \epsilon_2, \epsilon_3]$, with zero means and variance-covariance matrix equal to $\Sigma = [\sigma_{ij}]$. Hence we can compute an analytical expression for the bias:

$$E(w_{it1}|j_{it} = 2) = \alpha_1 X_{it} + \frac{\sigma_{13}}{\sigma_{33}} \frac{\phi\left(\frac{\beta Z_{it}}{\sigma_{33}}\right)}{\Phi\left(\frac{\beta Z_{it}}{\sigma_{33}}\right)} \quad (3)$$

$$E\left(w_{it2} \,\middle|\, j_{it} = 2\right) = \alpha_2 X_{it} + \frac{\sigma_{23}}{\sigma_{33}} \frac{\phi\left(\frac{\beta Z_{it}}{\sigma_{33}}\right)}{1 - \Phi\left(\frac{\beta Z_{it}}{\sigma_{33}}\right)} \quad (4)$$

where φ is the PDF referring to (2) while Φ is the CDF. These equations can be estimated with the two-step procedure adopted in Willis et al. (1979): First, estimate the probit model (2) and use the estimates to compute the predicted values of the inverse mills ratios:

$$\widehat{\lambda_{1it}} = \frac{\varphi(\frac{\widehat{\beta Z_{it}}}{\sigma_{33}})}{\Phi\left(\frac{\widehat{\beta Z_{it}}}{\sigma_{33}}\right)} \quad (5)$$

$$\widehat{\lambda_{2it}} = \frac{\varphi(\frac{\widehat{\beta Z_{it}}}{\sigma_{33}})}{1 - \Phi\left(\frac{\widehat{\beta Z_{it}}}{\sigma_{33}}\right)} \quad (6)$$

Next, estimate the wage equations (1) by Ordinary Least Squares including $\widehat{\lambda_{1it}}$ and $\widehat{\lambda_{2it}}$ to correct for the selection bias. The second stage then becomes:

$$ln(w_{it1}) = \alpha_1 X_{it} + \gamma \widehat{\lambda_{1it}} + \epsilon_{1it}$$

$$ln(w_{it2}) = \alpha_2 X_{it} + \gamma \widehat{\lambda_{2it}} + \epsilon_{2it}$$

The procedure provides consistent estimates of the parameter vector α.

4. Results

In this section we present the results from the second-stage estimations. In Table 1, we present the results from the estimation for the sample of women.[14] The top panel shows the results for the sample of women in jobs that require high levels of social interactions while the bottom panel presents the results for the sample of women in jobs that don't require high level of social interactions. We report both the results from the "unadjusted" OLS regression, and those from the regression that accounts for the selection bias (in the "adjusted" columns). Focusing on the results of the unadjusted estimation, we find that for white women there is a wage penalty associated with being obese of 0.076 percent in jobs requiring high personal interactions, which is statistically significant, and consistent with previous literature. In jobs not requiring social interaction, the effect is also positive and significant, with a point estimate is 0.088. For Black and Hispanic women we find and positive effect of obesity on wages in both types of jobs, however these effect are not statistically significant except for the coefficient for black women in low social jobs. When comparing the coefficients on obese across high and low social jobs for women in the unadjusted regressions, the

[14] The results from the first-stage estimation which predicts the parameters needed to obtain the mills ratio are in the appendix (Table 7 and 8). The exclusion restriction that we include is statistically significant and positive (the expected direction) for all groups gender-race groups except for Hispanics.

coefficient on obesity in low social jobs is larger than in high social jobs. This would indicate that there is a larger effect from being obesity in low social jobs. For white women, this means a higher wage penalty, while for Black and Hispanic women this means a larger wage premium for being obese in low social jobs. This goes against the expected direction, given the results from Figure 2.

Once we adjust the estimates to account for job selection, we find that for white women in jobs requiring a high level of social interactions the effects of obesity on wages are about 50% higher than their unadjusted counterpart. This provides evidence that accounting for selection is non-trivial and that previous estimates are closer to a "lower" bound of the estimates of obesity on wages for white women. This supports the hypothesis that obese white women who are in this type of jobs have –on average-unobserved skills that would affect their performance on the job and subsequently their wages in a positive direction. Hence once we control for this selection, the effects of obesity on wages are larger than not accounting for selection. For Black and Hispanic women in high social jobs, the coefficients on obesity become larger but remain not statistically significant. When looking at the bottom panel we find that for white women, the effects of being obese on wages are still negative but the coefficient is approximately 24% smaller when adjusting for selection and it is not statistically significant. After adjusting for selection, the coefficient on obesity for low social jobs is now smaller than in high social jobs. This points out to evidence that white women in high social jobs have a 73% higher wage penalty than in low social jobs, which is consistent with the suggestive evidence from Figure 2. In Table 2 we present the results from the estimation for the sample of men. Focusing on the sample of men working in high social jobs, the unadjusted column we find that the coefficient on obese for males are positive and not statistically significant with the exception of black males. In the sample of men working in low social jobs, we also find positive coefficient associated with the obese dummy, but only statistical significance for Hispanic males. Once we account for selection into these types of jobs, we still find positive coefficients for the high-social jobs, but none of these effects are statistically significant. Similarly for the low-social jobs, the coefficients remains positive but only the coefficient for Hispanic in statistically significant. The mills ratio coefficient is not significant is any of the males regressions, not providing strong evidence of sorting from males into these types of jobs.

5. Discussion

Our results are consistent with the findings in Cawley (2004) and Han et al. (2009) (see Table 3 for a comparison of the main results).[15] Both of these papers find significant negative effects of obesity on wages for white women, and the magnitude of these estimates is within the confidence interval of our unadjusted estimates. Similarly these papers do not find strong evidence for the effects of obesity in males or Black and

[15] There are several differences across the different specifications: Cawley (2004) and Han et al. (2009) do not include year 2006. Han et al. (2009) wage equations are conditioned on people who are employed. Cawley (2004) and Han et al. (2009) use a blue collar dummy and several state and local economic characteristics.

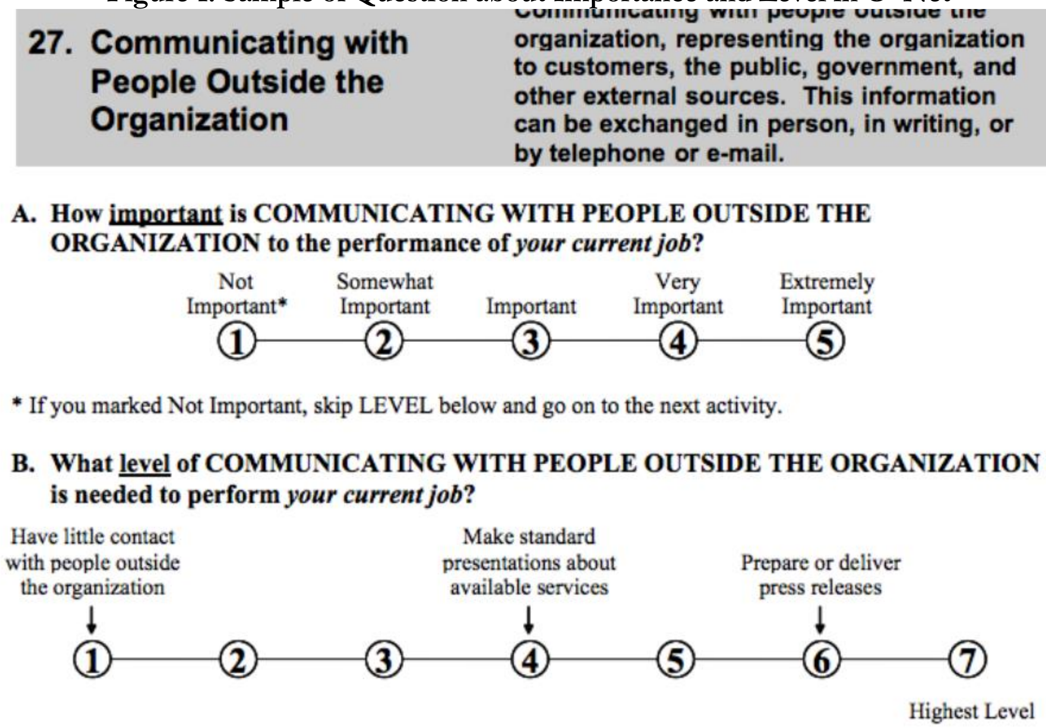
Hispanic minorities. This reinforces the evidence for an obesity wage penalty occurring for white women, but not other race-gender pair.

Focusing on white women our results with the estimations accounting for selection provide a larger effect of obesity on wages in jobs requiring a high level of social interactions, while a lower coefficient on obesity for the other jobs. This provides evidence of the prevalence of some characteristics character that makes them more suited for the job, and therefore more likely to obtain that job despite their weight status.[16]

We find that the effect of obesity on wages is larger for jobs that require high levels of social interaction, suggesting that the obesity wage penalty is more associated with a beauty premium rather than a productivity penalty. We also find that selection affects the magnitude of these effects.

[16] Example of these unobservable could include ability to interact with other individuals, and skills on social interactions.

Figure 1: Sample of Question about Importance and Level in O*Net



27. Communicating with People Outside the Organization

Communicating with people outside the organization, representing the organization to customers, the public, government, and other external sources. This information can be exchanged in person, in writing, or by telephone or e-mail.

A. How important is COMMUNICATING WITH PEOPLE OUTSIDE THE ORGANIZATION to the performance of *your current job*?

Not Important* — 1; Somewhat Important — 2; Important — 3; Very Important — 4; Extremely Important — 5

* If you marked Not Important, skip LEVEL below and go on to the next activity.

B. What level of COMMUNICATING WITH PEOPLE OUTSIDE THE ORGANIZATION is needed to perform *your current job*?

1 — Have little contact with people outside the organization; 2; 3; 4 — Make standard presentations about available services; 5; 6 — Prepare or deliver press releases; 7 — Highest Level

Figure 2: Wages and BMI for different groups and job types

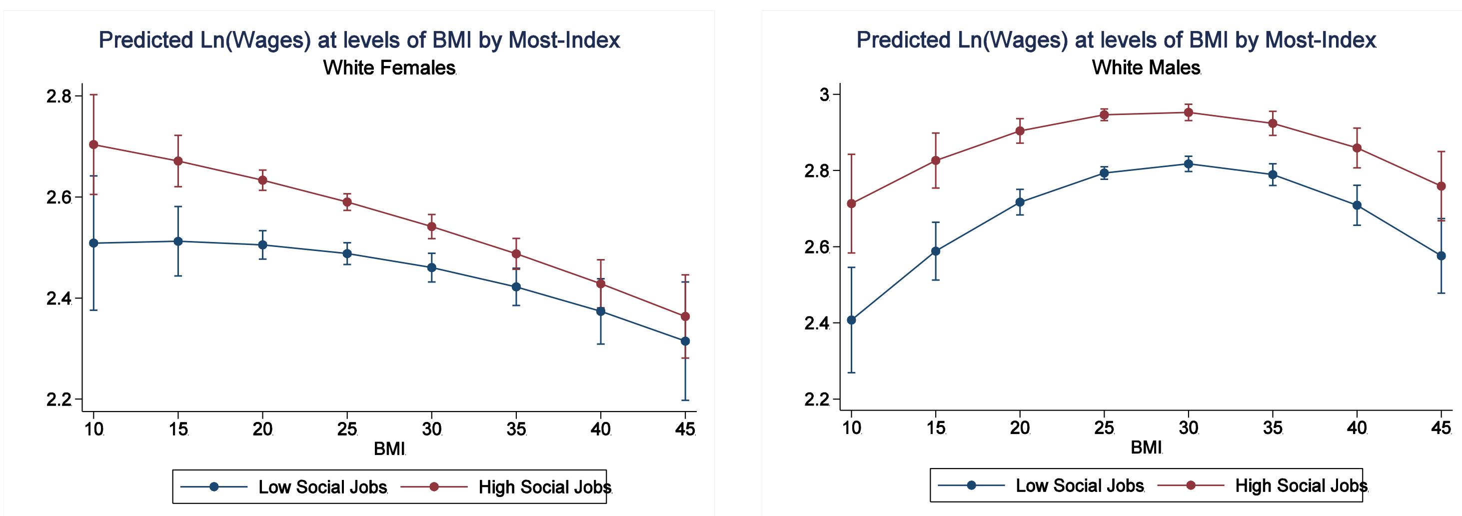


Figure 3: Distribution of BMI by Gender and Type of Job

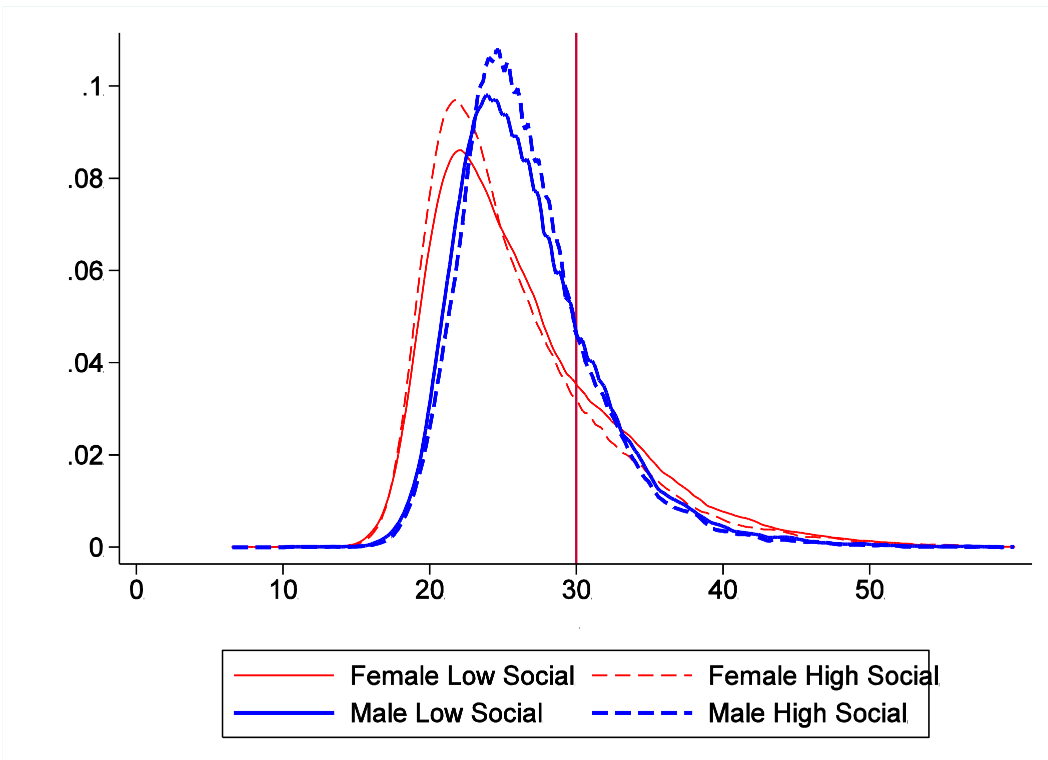


Table 1: Effects of BMI on Ln(Wages) for Women Using NLSY (1982-2006)

	Whites		Blacks		Hispanic	
	Adjusted	OLS	Adjusted	OLS	Adjusted	OLS
High Social						
Under weight (BMI: less than 18.5)	-0.083*	-0.053	-0.134	-0.152**	-0.124	-0.036
	(0.040)	(0.027)	(0.089)	(0.055)	(0.082)	(0.047)
Overweight (BMI: 25.0 to 29.9)	-0.067*	-0.035	0.082*	0.034	0.042	0.018
	(0.029)	(0.019)	(0.041)	(0.024)	(0.054)	(0.027)
Obese (BMI: 30 and Up)	-0.112*	-0.076**	0.092	0.022	0.058	0.019
	(0.047)	(0.029)	(0.058)	(0.034)	(0.086)	(0.046)
Mills Ratio	-0.416*		-0.314		-1.556*	
	(0.188)		(0.373)		(0.668)	
Low Social						
Under weight (BMI: less than 18.5)	-0.081	-0.016	-0.035	-0.110	-0.086	-0.033
	(0.050)	(0.035)	(0.068)	(0.057)	(0.066)	(0.056)
Overweight (BMI: 25.0 to 29.9)	0.001	0.004	0.054	0.051**	0.014	0.026
	(0.026)	(0.021)	(0.030)	(0.019)	(0.036)	(0.025)
Obese (BMI: 30 and Up)	-0.067	-0.089*	0.095	0.078**	-0.008	0.035
	(0.050)	(0.035)	(0.048)	(0.028)	(0.063)	(0.041)
Mills Ratio	0.000		0.000		0.000	
	(0.000)		(0.000)		(0.000)	
N: High Social	7041	16789	2396	5934	1780	4756
N : Low Social	4341	9657	2589	6033	1230	3011
Mean: High Social	2.667	2.596	2.515	2.510	2.601	2.584
Mean: Low Social	2.338	2.318	2.259	2.274	2.306	2.327

These estimations from from regressions where the dependent variable is Ln(Wage). The model includes individual fixed effects and it includes other demographics controls like: number of children, highest grade achieved of the individual and both parents, work tenure, marital status, region fixed effects and dummies for missing values of the main explanatory variables. We use clustered at the individual level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2: Effects of BMI on Ln(Wages) for Men Using NLSY (1982-2006)

	Whites		Blacks		Hispanic	
	Adjusted	OLS	Adjusted	OLS	Adjusted	OLS
High Social						
Under weight (BMI: less than 18.5)	-0.029	-0.003	0.024	-0.042	0.781	0.191
	(0.096)	(0.053)	(0.194)	(0.117)	(0.402)	(0.202)
Overweight (BMI: 25.0 to 29.9)	0.050*	0.033	0.031	0.090*	0.025	0.021
	(0.024)	(0.017)	(0.050)	(0.036)	(0.061)	(0.032)
Obese (BMI: 30 and Up)	0.050	0.024	0.051	0.120*	0.072	0.093
	(0.039)	(0.029)	(0.063)	(0.052)	(0.107)	(0.058)
Mills	0.077		0.199		0.087	
	(0.206)		(0.267)		(0.583)	
Low Social						
Under weight (BMI: less than 18.5)	-0.104	-0.179***	0.045	0.073	0.076	-0.072
	(0.099)	(0.054)	(0.061)	(0.053)	(0.186)	(0.098)
Overweight (BMI: 25.0 to 29.9)	0.044	0.035*	0.029	0.023	0.029	0.017
	(0.024)	(0.016)	(0.024)	(0.016)	(0.028)	(0.020)
Obese (BMI: 30 and Up)	0.017	0.010	0.060	0.020	0.111*	0.065*
	(0.035)	(0.023)	(0.040)	(0.026)	(0.045)	(0.030)
Mills	-0.000		0.000		0.000	
	(0.000)		(0.000)		(0.000)	
N: High Social	6541	15441	1697	4550	1630	3928
N : Low Social	6292	13071	4062	8799	2442	5123
Mean: High Social	2.954	2.900	2.769	2.721	2.853	2.802
Mean: Low Social	2.581	2.590	2.440	2.434	2.574	2.568

These estimations from from regressions where the dependent variable is Ln(Wage). The model includes individual fixed effects and it includes other demographics controls like: number of children, highest grade achieved of the individual and both parents, work tenure, marital status, region fixed effects and dummies for missing values of the main explanatory variables. We use clustered at the individual level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3: Comparison of our results with Cawley (2004) and Han et al. (2009)

			Our results			
	Cawley (2004)	Han et al. (2009)	High OLS	Low OLS	High Adjusted	Low Adjusted
White women	-0.087	-0.075	-0.076	-0.089	-0.112	-0.067
	(0.015)	(0.021)	(0.029)	(0.035)	(0.047)	(0.050)
Black women	0.002	-0.049	0.022	0.078	0.092	0.095
	(0.017)	(0.025)	(0.034)	(0.028)	(0.058)	(0.048)
Hispanic women	-0.020	0.032	0.019	0.035	0.058	-0.008
	(0.024)	(0.035)	(0.046)	(0.041)	(0.086)	(0.063)
White Males	0.013	-0.001	0.024	0.010	0.050	0.017
	(0.015)	(0.017)	(0.029)	(0.023)	(0.039)	(0.035)
Black Males	0.031	0.014	0.120	0.020	0.051	0.060
	(0.019)	(0.027)	(0.052)	(0.026)	(0.063)	(0.040)
Hispanic Males	0.023	0.008	0.093	0.065	0.072	0.111
	(0.025)	(0.032)	(0.058)	(0.030)	(0.107)	(0.045)

These are all coefficients on the category for "Obese" in each paper. The standard errors are in parentheses. The specification in Cawley (2004) and Han et al. (2009) both include individual fixed effects. All of these three specifications use NLSY data.

Appendix A : Descriptive Statistics

Table A.1: Summary Statistics of NLSY 1987-2006

	Women			Men			
	White	Black	Hispanic	White	Black	Hispanic	Total
Employment Variables							
Labor Force Participation	0.76	0.71	0.68	0.89	0.82	0.86	0.81
Employed	0.93	0.81	0.88	0.92	0.82	0.88	0.91
Hourly Wage in Dollars	13.36	9.45	12.60	16.23	12.99	20.47	14.50
Log(Wage) Top Coded - in 2010 $	2.53	2.43	2.50	2.80	2.54	2.67	2.64
Work in a High Social Job	0.78	0.70	0.77	0.71	0.61	0.64	0.73
Weeks worked in Current Job	132.74	124.79	115.40	158.78	113.75	138.40	141.14
Currently Enrolled in School	0.17	0.16	0.15	0.18	0.14	0.15	0.17
Work Experience (1000s hours)	15.84	13.92	13.97	20.77	16.45	19.40	17.82
Body Mass Index							
BMI Adjusted	25.29	28.68	27.42	26.57	26.61	27.61	26.28
Under weight (BMI: less than 18.5)	0.04	0.02	0.02	0.01	0.01	0.01	0.02
Overweight (BMI: 25.0 to 29.9)	0.23	0.28	0.31	0.39	0.34	0.39	0.31
Obese (BMI: 30 and Up)	0.18	0.36	0.28	0.19	0.21	0.26	0.20
Demographics							
Age at the Date of the interview (84-10)	30.08	30.42	30.25	29.70	29.93	29.80	29.94
Highest Grade as of May of Svy Year	10.91	10.98	10.00	10.28	9.97	9.62	10.52
Highest Grade Completed by Mother	11.44	9.98	7.46	11.44	9.89	7.37	10.98
Highest Grade Completed by Father	11.32	7.65	6.90	11.50	7.75	6.97	10.62
Number of Children Ever Had	0.89	1.27	1.25	0.63	0.90	0.93	0.83
Age of Youngest Child	2.68	3.80	3.37	1.61	1.38	1.80	2.22
Never Married	0.32	0.52	0.34	0.44	0.61	0.45	0.41
Married	0.55	0.28	0.48	0.46	0.27	0.42	0.47
Separated	0.03	0.09	0.06	0.02	0.05	0.04	0.03
Divorced	0.10	0.10	0.11	0.08	0.07	0.08	0.09
Widowed	0.00	0.01	0.01	0.00	0.01	0.00	0.00

Table A.2: Summary Statistics of NLSY 1987-2010 for women

	Underweight	Normal	Overweight	Obese or More	Total
BMI Adjusted	17.58	21.91	27.19	35.74	25.92
Employment Variables					
Labor Force Participation	0.69	0.77	0.76	0.74	0.76
Employed	0.88	0.93	0.93	0.92	0.92
Hourly Wage in Dollars	9.28	14.38	15.23	12.05	13.94
Log(Wage) Top Coded - in 2010 $	2.44	2.58	2.58	2.52	2.57
Work in a High Social Job	0.73	0.73	0.69	0.66	0.71
Weeks worked in Current Job	99.41	161.31	207.46	234.11	185.44
Currently Enrolled in School	0.20	0.15	0.08	0.06	0.12
Work Experience (1000s hours)	11.02	17.14	22.37	25.75	19.98
Demographics					
Age at the Date of the interview (84-10)	27.99	30.64	34.52	37.06	32.82
Highest Grade as of May of Svy Year	12.68	13.39	13.11	12.89	13.19
Highest Grade Completed by Mother	11.21	11.41	10.80	10.25	11.01
Highest Grade Completed by Father	10.84	11.18	10.18	9.43	10.56
Number of Children Ever Had	1.02	1.15	1.56	1.70	1.36
Age of Youngest Child	2.70	3.29	5.11	6.14	4.30
Never Married	0.36	0.30	0.22	0.24	0.27
Married	0.44	0.55	0.60	0.56	0.56
Separated	0.05	0.03	0.05	0.06	0.04
Divorced	0.15	0.12	0.13	0.13	0.12
Widowed	0.01	0.01	0.01	0.01	0.01

Table A.3: Summary Statistics of NLSY 1987-2010 for Males

	Underweight	Normal	Overweight	Obese or More	Total
BMI Adjusted	17.39	22.69	27.19	34.19	26.65
Employment Variables					
Labor Force Participation	0.70	0.89	0.92	0.93	0.90
Employed	0.79	0.91	0.94	0.94	0.93
Hourly Wage in Dollars	22.90	13.69	21.49	22.77	18.65
Log(Wage) Top Coded - in 2010 $	2.32	2.73	2.92	2.88	2.83
Work in a High Social Job	0.63	0.59	0.61	0.57	0.60
Weeks worked in Current Job	81.08	168.16	264.37	309.63	232.15
Currently Enrolled in School	0.38	0.16	0.07	0.04	0.11
Work Experience (1000s hours)	9.27	19.17	30.24	37.78	27.00
Demographics					
Age at the Date of the interview (84-10)	23.97	28.96	34.20	37.43	32.60
Highest Grade as of May of Svy Year	11.38	12.98	13.24	12.96	13.06
Highest Grade Completed by Mother	10.43	11.10	11.09	10.84	11.04
Highest Grade Completed by Father	10.66	10.88	10.91	10.38	10.79
Number of Children Ever Had	0.34	0.77	1.22	1.44	1.07
Age of Youngest Child	0.37	1.51	3.06	4.19	2.62
Never Married	0.77	0.51	0.28	0.25	0.37
Married	0.15	0.38	0.58	0.62	0.50
Separated	0.01	0.03	0.03	0.03	0.03
Divorced	0.06	0.08	0.11	0.11	0.10
Widowed	0.00	0.00	0.00	0.01	0.00

Appendix B: Job Descriptors

We report below the set of abilities, skills and work activities we used to construct the index that encompasses the level of social interaction required on the job.

- Work Activities:
  - Communicating with Supervisors, Peers, or Subordinates
  - Communicating with Persons Outside Organization
  - Establishing and Maintaining Interpersonal Relationships
  - Assisting and Caring for Others
  - Selling or Influencing Others
  - Resolving Conflicts and Negotiating with Others
  - Coordinating the Work and Activities of Others
- Skills:
  - Speaking
  - Social Perceptiveness
  - Coordination
  - Persuasion
  - Negotiation
  - Instructing
  - Service Orientation

Appendix C: First Stage Results

Table C.1: First Stage for Women: Pr(Work in a High Social Job)

	White	Black	Hispanic
BMI Adjusted	-0.00207	0.00377	-0.0118**
	(0.00233)	(0.00280)	(0.00435)
Closest Sibiling's Job level of social interaction	0.0915***	0.0553**	0.0397
	(0.0136)	(0.0190)	(0.0266)
Age	0.00326	-0.0156	0.00406
	(0.00660)	(0.00972)	(0.0126)
Number of Children Ever Had	-0.0212	0.0184	0.000668
	(0.0184)	(0.0219)	(0.0288)
Age of Youngest Child	-0.0171***	0.00867	-0.0103
	(0.00442)	(0.00533)	(0.00747)
Highest Grade as of May of Svy Year	0.147***	0.178***	0.144***
	(0.00765)	(0.0128)	(0.0136)
Highest Grade Completed by Mother	0.0238***	-0.0250**	0.0109
	(0.00666)	(0.00935)	(0.00820)
Highest Grade Completed by Father	0.0155**	0.0210**	-0.00638
	(0.00494)	(0.00650)	(0.00766)
Weeks worked in Current Job	0.0000646	0.00000701	0.000582***
	(0.0000755)	(0.000108)	(0.000157)
Currently Enrolled in School	-0.0760	-0.00655	0.0555
	(0.0427)	(0.0711)	(0.0893)
Work Experience (1000s hours)	0.00937***	0.0180***	0.00222
	(0.00216)	(0.00294)	(0.00376)
Year	0.00241	-0.000827	0.0177
	(0.00643)	(0.00952)	(0.0125)
Going to School	-0.234*	-0.0946	-0.223
	(0.0932)	(0.149)	(0.192)
Constant	-7.260	-0.738	-36.62
	(12.64)	(18.73)	(24.51)
N	13733	6442	3808
Pseudo R-Squared	0.193	0.268	0.209
Log-Likelihood	-7002.5	-3190.9	-1917.1

Standard errors in parentheses. These regressions also included dummies for missing observations of main co-variates, these are not output it here. The other omitted variable for output is armed forces, marital status dummies, qualification test percentile score and region dummies. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

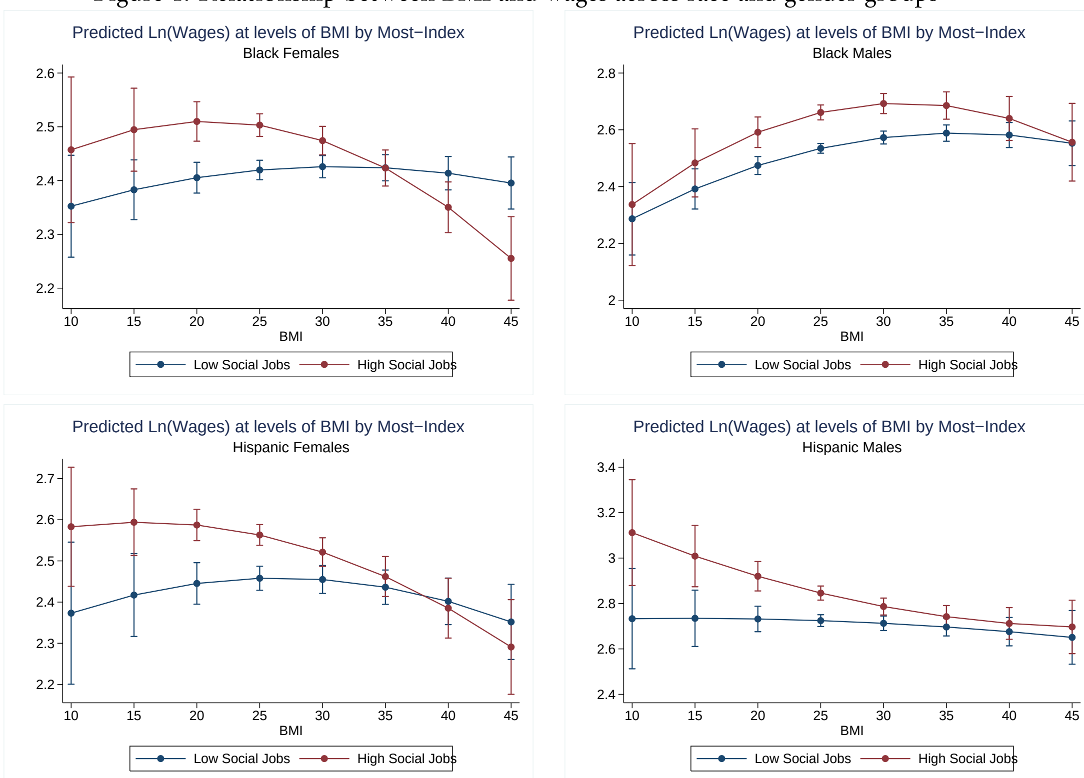
Table C.2: First Stage for Men: Pr(Work in a High Social Job)

	(1)	(2)	(3)
	White	Black	Hispanic
BMI Adjusted	-0.00341	0.00246	-0.00795
	(0.00289)	(0.00394)	(0.00449)
Closest Sibiling's Job level of social interaction	0.0758***	0.0697***	0.00237
	(0.0133)	(0.0193)	(0.0238)
Age	-0.00619	-0.0254**	-0.00844
	(0.00612)	(0.00921)	(0.0111)
Number of Children Ever Had	-0.0309*	-0.0188	-0.0374
	(0.0155)	(0.0157)	(0.0229)
Age of Youngest Child	-0.0147**	-0.00323	-0.00753
	(0.00470)	(0.00687)	(0.00780)
Highest Grade as of May of Svy Year	0.200***	0.196***	0.159***
	(0.00742)	(0.0116)	(0.0120)
Highest Grade Completed by Mother	0.000543	-0.00786	0.00282
	(0.00642)	(0.00900)	(0.00663)
Highest Grade Completed by Father	0.0156***	0.000860	0.0189**
	(0.00470)	(0.00669)	(0.00637)
Weeks worked in Current Job	0.000381***	0.0000353	0.000497***
	(0.0000631)	(0.000103)	(0.000119)
Currently Enrolled in School	-0.0925*	0.0830	-0.130
	(0.0461)	(0.0833)	(0.0857)
Work Experience (1000s hours)	0.0123***	0.00788**	0.00269
	(0.00183)	(0.00250)	(0.00326)
Year	0.00708	0.0247**	0.0259*
	(0.00580)	(0.00916)	(0.0107)
Married	0.135***	0.268***	0.0164
	(0.0348)	(0.0536)	(0.0615)
Separated	-0.0733	0.155	-0.0657
	(0.0820)	(0.0806)	(0.111)
Divorce	0.0383	0.0967	0.163
	(0.0495)	(0.0755)	(0.0892)
Widowed	-0.179	0.267	-0.428
	(0.373)	(0.260)	(0.502)
Going to School	-0.277**	-0.00463	0.0899
	(0.0847)	(0.162)	(0.176)
Constant	-17.16	-51.76**	-53.64*
	(11.42)	(18.01)	(20.99)
N	14147	6981	4538
Pseudo R-Squared	0.220	0.306	0.215
Log-Likelihood	-7614.1	-3248.8	-2445.6

Standard errors in parentheses. These regressions also included dummies for missing observations of main co-variates, these are not output it here. The other omitted variable for output is armed forces qualification test percentile score and region dummies. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix D: Relationship between BMI and Wages, other roups

Figure 1: Relationship between BMI and wages across race and gender groups



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