Measuring Health Policy Effects During Implementation

Dissertation

Presented in Partial Fulfillment of the Requirements for the Degree Doctor of Philosophy in the Graduate School of The Ohio State University

By

David Boone Muhlestein, J.D., M.S., M.H.A., B.A.

Graduate Program in Public Health

The Ohio State University

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Dissertation Committee:

Thomas M. Wickizer, PhD, Advisor

Eric E. Seiber, PhD

Abigail B. Shoben, PhD
Abstract

The policy making and implementation process is poorly understood by many quantitative health services researchers leading to potential threats to the validity of some studies. There is an extensive qualitative literature on the policy making process and a distinction arises between those that decide on a policy (policy makers) and those that implement a policy (policy actors). As multiple actors implement a policy there are multiple concerns raised including heterogeneity of effect among actors, evolving policies as different policy actors interact amongst themselves, variable implementation times among actors and spillover effects as actors engage with other, non-intended groups. When measuring health policies, each of these factors needs to be considered when designing studies and drawing conclusions to properly inform policy makers of policy effects.

Crowd-out in health insurance occurs when individuals would have private insurance but for the existence of a public insurance program. Policy makers concerned with crowd-out will put barriers to enrollment in public plans which may discourage the neediest people, who the programs are intended to help, from enrolling. Past estimates of children’s crowd-out during expansions in eligibility to Medicaid and the Children’s Health Insurance Program have varied widely and are national in scope. I estimate state-specific levels of crowd-out using a regression discontinuity analysis to estimate crowd-
out independent of an expansion for children near the eligibility threshold. I find that among states with similar eligibility levels, there is significant variability among crowd-out levels.

To evaluate how crowd-out levels may change over time, I estimate crowd-out in Ohio from 2004-2012. I find that crowd-out levels were not constant over time and decreased during these years.

Caps on noneconomic damages are viewed by some as a means of lowering the cost of healthcare. A potential unintended effect of these caps is that people with meritorious claims may not be able to find appropriate legal representation and will not be compensated for their injury. A challenge in measuring the effect of these tort reforms is that different states implement the same general policy differently and have different statutes of limitations, meaning the effective date for a policy will vary. By applying state-level, interrupted time series and matched pair difference-in-difference designs, and adjusting for the variable implementation times, I estimate state-specific effects. I find that facially similar noneconomic damage cap statutes led to significantly different effects on the rate of malpractice settlements, with many states seeing no effect from the policy.

Changes in Medicare reimbursement policies are known to affect the behavior of providers serving the Medicare population, but less is known about how such policies may spillover onto the non-Medicare population. I evaluate the change in the rate of bariatric surgeries in the United States after the Centers for Medicare and Medicaid Services passed several National Coverage Decisions using an interrupted time series
design. I find a significant decrease in the rate of bariatric surgeries for the Medicare and non-Medicare populations. The timing and magnitude of these changes were nearly identical for both populations.
Dedication

To my beautiful wife Megan. You made this possible. I love you.
Acknowledgments

I would like to sincerely thank my committee for their help on this journey. Their guidance has made this work a reality.
Vita

2001.............. Woods Cross High School

2006.............. B.A., Brigham Young University

2010.............. J.D., The Ohio State University Moritz College of Law

2012.............. M.H.A., The Ohio State University College of Public Health

2012.............. M.S., The Ohio State University College of Public Health

Fields of Study

Major Field: Public Health

Minor Field: Health Services Management and Policy

Minor: Applied Statistical Analysis
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Chapter 1 – Introduction

Overview

When attempting to measure the effect of health policy decisions, researchers often ignore important components of the policy making process that could affect their research design. While the policy process literature contains many models and extensive insight into the creation of health policies, these contributions are often ignored when quantifying the effects of those policies. In this dissertation I identify some weaknesses in common approaches to evaluating health policy decisions and offer a list of factors that need to be considered by health services researchers when evaluating a study. I then address each of these issues as they are raised in studies designed to quantify the effects of three discrete policies: the crowd-out of children’s private health insurance, state medical tort reforms that impose caps on non-economic damages and Medicare reimbursement changes for bariatric surgery’s effect on the non-Medicare population.

Issues Addressed

Political scientists have long studied the process by which policies are created and have created numerous models to evaluate the creation and implementation of policies (Birkland 2010; Sutton 1999; McLaughlin 1987; Van Meter and Van Horn 1975; Sabatier and Mazmanian 1980). Such theories have been extensively reviewed and applied to the healthcare policy making process (For a brief overview, see Béland 2010). Many quantitative health services research studies, though, do not apply an explicit theory of
policy implementation when designing their studies (See, for example, Flum et al. 2011; Nguyen et al. 2010; Seiber and Sahr 2011; Gruber and Simon 2008; Avraham 2007; Durrance 2010). While researchers may discuss aspects of the implementation process, a common approach is that policy creation and implementation is somewhat of a black box where how a policy is implemented is not formally addressed. This approach evaluates health policies as a natural experiment where changes over time are evaluated or estimated results are compared to similar groups unaffected by the policy. An oversimplification of the implementation process, though, leads to threats to the validity of conclusions raised by researchers as potential confounders are ignored or important statistical assumptions are not met.

Though addressed in other disciplines (See O’Toole 1986) and in many qualitative studies (See, for example, Hanning and Spångberg 2000; Rochefort 1988), a major shortcoming in much quantitative health services research is a failure to differentiate between the policy making process and the policy implementation process. In particular, there is a distinction within many federal and state policies between those that choose the policy (the policy makers) and those that implement the policy (the policy actors) (See Sutton 1999; Thomas and Grindle 1990). To appropriately measure the effect or impact of such policies, it is necessary to consider how these policies are implemented. In this dissertation I propose four potential issues that require consideration when designing a study to estimate the effect of a policy: heterogeneity of implementation effects among actors, the changing nature of a policy over time, variable implementation dates and spillover effects on unintended groups.
Heterogeneity of policy effect refers to how the same policy will be implemented by different actors in different ways, leading to different effects on the target population. This can occur when, for example, the federal government mandates that states implement a new policy; with fifty different states implementing the policy in their own way, the results should be expected to be mixed. Whenever studying a policy, then, it should be recognized that the same policy may have different individual-level effects. While the heterogeneity of effect is heavily studied in other policy areas such as educational policy (Zimmer 2009; Belfield and Levin 2002), it is not explicitly evaluated in all health services research (See, for example, Avraham 2007, where tort reform policies were treated as being the same across states; Gruber and Simon 2008 which assumes similar crowd-out levels in states which have the same Medicaid eligibility levels). Such approaches allow researchers to calculate a national estimate of a policy, but such an estimate may be flawed if the underlying implicit assumptions of homogeneous implementation effects fail. A better approach is to calculate the effect of the policy at the level of the actors and then create a national estimate by summing or averaging the actor-level effects. I will look at heterogeneity of effect with state-level crowd-out estimates and state-level effects of caps on noneconomic damages.

The changing nature of policies arises from the continued didactic between the policy maker and the policy actor, or internal reevaluations by the actor alone, leading to changes of policy or changes in implementation approaches. This occurs commonly as the federal government makes a policy, such as a legislative change in Medicare reimbursement, and then providers interact with their legislators, leading to additional changes in the policy a short time later. When a policy is constantly in flux, it is
impossible to perform typical before and after analyses (with or without a comparison group) as the multiple iterations of the policy would confound the estimates of the result of any one of the policy decisions. Thus, when a policy is in flux it must be evaluated as the cumulative effect of all the policy iterations or, as I do in my study on crowd-out using regression discontinuity, to calculate the effect of the cumulative policies at individual points in time which allows a researcher to evaluate how incremental policy changes affected the policy outcomes at different points in time.

*Variable implementation times* refers to the different periods in which a policy is enacted or when it is acted upon by an actor. While most policies, particularly those enacted with legislation, have specific effective dates associated with them, actors may begin to act on the belief that a specific policy will be adopted long before the legislation is passed. Alternatively, due to the uncertainty of how specific regulations will be written, others do not begin implementing the policies until after the proposed or final regulations. Other actors simply choose not to participate in a particular policy option (such as the optional Medicare accountable care organization program) when they are first announced, but may, at any later point, decide to act on that policy. To estimate the effect of the policy the individual actors must be evaluated and the timing of their implementation must be assessed. The challenge for many health services researchers is that this requires first studying the legislative history and understanding the timing of the agency rule-making process before quantitatively studying a policy’s effect over time. I will look at variable timing by evaluating when state tort reforms went into effect, factoring in the delayed timing due to statutes of limitations.
Spillover occurs when a policy intended to affect one group also affects another group. Policy actors do not simply interact with the policy makers during the policy making process, but also interact with, and affect, other actors. The result is that a policy that was directed at a particular organization or group may indirectly affect another group via the policy actors. Additionally, many actors are influenced by governmental policies so, even if they are not required to adopt the policy, they may choose to conform to its tenets. To accurately measure the full effect of a policy, it is thus necessary to also measure the “spillover effect” on groups that were not intended to be directly affected by the policy.

1. Is the policy implemented differently among actors? If so, measure the individual effects among the actors and sum to create a group estimate.

2. Does the policy change over time? If so, either measure the cumulative effect of all the policy changes or measure the effects of individual policies at specific times to compare incremental changes.

3. Does the timing of the policy implementation vary among actors? If so, calculate the timing of the policy implementation process among different actors and account for these differences in the study.

4. Does the policy affect unintended groups? If so, identify the unintended group and how the group is engaged; study how the policy affects that group.

Table 1-1: Questions for Health Services Researchers
This dissertation studies three different topics in health services research where these questions (see Table 1-1) cannot all be answered negatively. In each case I structured the studies with this implementation challenge in mind and, to the extent possible, accounted for each of these concerns.

**Estimating State-Level Crowd-Out of Children’s Health Insurance**

**Implementation Concern**

This study addresses the heterogeneity of a policy’s effect among actors.

**Background**

Crowd-out occurs when individuals are insured by a public program and would be covered by private insurance but for the existence of the public program. This occurs both when individuals drop private coverage in exchange for public insurance and also when individuals remain on public insurance when private insurance becomes available. Concerns about crowd-out are particularly relevant to state policy makers deciding how to insure children in Medicaid or the Children’s Health Insurance Program (CHIP). It is the belief among some that states should not provide insurance for children who, but for the public plan, would have private insurance (Hogberg 2007) and policies are put in place to discourage such behavior, such as waiting periods and increased cost-sharing, which can lead to fewer needy children enrolling in the plans (Lo Sasso and Buchmueller 2004; Gruber and Simon 2008).

To effectively discuss the merits of policies intended to limit crowd-out, there first must be estimates of crowd-out to determine whether such policies are even necessary. Past work has focused on the amount of crowd-out that occurs when the
eligibility for Medicaid or CHIP is expanded and estimates of crowd-out are calculated by the number of children dropping private insurance divided by the number of children moving to public plans (Shore-Sheppard 2009; Lo Sasso and Buchmueller 2004a; Cutler and Gruber 1996).

An important issue for policy makers is that current estimates of crowd-out vary widely from detecting no crowd-out (Ham and Shore-Sheppard 2001) to estimates that over 50% of the children newly insured with public insurance were crowded-out of private insurance (Yazici and Kaestner 2000; Cutler and Gruber 1996). To exacerbate this problem, the econometric models often relied upon are very sensitive to assumptions within the model, where different ways of treating the same private insurance variable led to estimates of crowd-rate ranging from 10% to 47% (Lo Sasso and Buchmueller 2004a).

In addition to the variable estimates, previous studies are generally limited to national-level figures; Medicaid and CHIP, though, are state-run plans. Where different actors (states) implement a policy differently, there is expected to be heterogeneity in the effects of the policy and a national estimate may not reflect the actual level of crowd-out within an individual state.

Past approaches are further limited by only evaluating crowd-out in conjunction with an expansion of eligibility, disallowing estimates of how other policies, such as barriers to enrollment, affect crowd-out after the initial expansion. This focus on expansions limits crowd-out to children who drop private insurance to enroll in a public plan and ignores children who are enrolled in public insurances but fail to enroll in a private plan that becomes available. Further, Medicaid and CHIP are constantly altered
by states meaning that they are constantly being changed and the effects of an expansion years ago may not hold today.

My study provides an additional estimate of crowd-out by (1) recognizing that state implementations vary considerably and focus will be on the individual state actors (i.e., state-level crowd-out estimates, not national estimates) and (2) providing an estimate of crowd-out independent of an expansion in eligibility which allows for both a more complete estimate of crowd-out (including those that stay on a public plan after a private option is available) and allows for estimates of the effect of non-expansion policies at a specific point in time, which allows me to measure the effect of a policy that is constantly in flux. By recognizing and addressing these issues, a more accurate estimate of crowd-out can be provided to state-level policy makers regarding the crowd-out levels in their state which will more directly influence their decisions regarding Medicaid and CHIP.

Contributions

In this study I make three major additions to past work: (1) I evaluate states individually so that state-level estimates of crowd-out are the outcome of interest which will be more relevant to state-level policy makers; (2) I take advantage of a dataset – the American Community Survey (ACS) – that has not previously been used to estimate crowd-out and is sufficiently large to allow for state-level estimates; and (3) I apply a regression discontinuity (RD) design individually to each state that allows for crowd-out estimates independent of an expansion and allows for longitudinal estimates in crowd-out.
The most important innovation that this study provides is its focus on individual state actors and acknowledges that different states, despite having similar eligibility thresholds for Medicaid, may have significantly different levels of crowd-out.

To accomplish this state-level evaluation I rely on a survey, the ACS, that has not been generally used to estimate crowd-out (It was used previously to “create a crude measure” of crowd-out by Blumenthal and O’Hara 2011, but has not been used, to date, in a study focused on crowd-out). The ACS is a nationally representative, yearly study performed by the United States Census Bureau that samples approximately 3 million households (Griffin and Waite 2006). Beginning in 2008, the ACS began tracking health insurance status of surveyed individuals (Lynch, Boudreaux, and Davern 2010). Due to the increased sample size and the representative sampling of the entire nation, I have sufficient data to estimate the number of children crowded-out at the state level. The ACS is now the largest survey that tracks health insurance coverage trends and is significantly larger than previous datasets used to estimate crowd-out (For other data sources, see Gruber and Simon 2008, Table 1). I use the 2010 ACS survey to conduct this analysis.

The final addition that I make is that I apply a regression discontinuity (RD) design. The RD design is based on the concept of taking some arbitrarily determined cutoff point (in this case, the eligibility threshold for Medicaid/CHIP) and comparing the values immediately on either side of cutoff (the percent of children with private health insurance who are barely eligible for Medicaid/CHIP to the percent of children with private health insurance who are barely not eligible for Medicaid/CHIP). This approach will allow me to make estimates of crowd-out using cross-sectional data (Lee and
The RD design also allows me to infer causality as the results are interpretable as a randomized experiment (Lee and Lemieux 2010). Thus, I am able to estimate the level of crowd-out at a specific point of time, independent of any expansions in eligibility; this estimate includes the traditionally measured crowd-out (children dropping private coverage for public coverage) and the unmeasured variety (children remaining on public coverage when private coverage becomes available).

Findings

I find that crowd-out rates vary widely among states. In comparing states that have similar eligibility thresholds for Medicaid/CHIP, crowd-out can range from negligible or non-existent to over 18% of children in families whose income is near the income eligibility being crowded-out of private insurance by the public plan. Additionally, and surprisingly, I found that as the eligibility threshold for a state increased, the percent of children crowded out near the eligibility threshold tended to decrease.

Children’s Insurance Coverage and Crowd-Out through the Recession: Lessons from Ohio

Implementation Concern

This study addresses how the effect of a policy may change over time.

Background

As a companion evaluation to the interstate variation of crowd-out, I also estimate how crowd-out levels in a single state (Ohio) have changed over time. This study uses the Ohio Family Health Survey – a periodic state-level survey of the health insurance

Findings

I find that the percent of children being crowded out near the Ohio Medicaid eligibility level changed over time. I also found that the percent of children that were publicly insured increased due to more children living in families that earned less and from more children at lower income levels moving to public insurance.

Caps on Noneconomic Damages’ Effect on the Number of Paid Malpractice Claims

Implementation Concern

This study addresses the heterogeneity of a policy’s effect among actors and the variable implementation times of the same policy.

Background

Tort reform has been debated for many years as a means of lowering the cost of healthcare (Sloan 2005; Krauthammer 2009; Eviatar 2009; Simmons 2009; Keene 2011). Proponents believe that this will lower costs in two ways: by (1) lowering malpractice premiums (Morrisey, Kilgore, and Nelson 2008) and (2) decreasing defensive medicine (Kessler and McClellan 1996). The first suggestion infers that the high costs of insuring against malpractice lead to physicians transferring those costs to patients and insurers. The belief is that if physicians have lower premiums they will also be willing to lower their prices. The second suggestion is based on the belief that physicians, due to a desire to not be sued, will order excessive tests, procedures and care on the off-chance that the patient has a serious illness. This is particularly believed to result in higher service
utilization among high-risk specialties (such as emergency department physicians and obstetricians) (Studdert 2005) and higher rates of diagnostic imaging (Iglehart 2006).

One approach to tort reform is to place limits, or caps, on noneconomic damages. Within a tort claim there are two claims that a plaintiff may make: claims for compensatory or actual damages and claims for punitive or exemplary damages; the former type of damages intend to reimburse for objective sums such as lost wages, medical bills, etc., while the latter intend to reimburse for mental anguish or suffering and are inherently subjective (Dietz et al. 2012). The goal of a cap on noneconomic damages is to limit the potential liability for a medical malpractice claim. The argument is that if there is a limit to the total amount of noneconomic damages, then there will be a smaller chance of extremely high awards for victims as the award will primarily be limited to provable, economic damages. The end result, according to this argument, is that overall awards will decrease which will lead to lower premiums and, thus, lower prices charged by physicians.

A criticism of this approach is that caps will lead to the unintended consequence of reducing access to the system for those with legitimate claims. Critics argue such caps make lawyers less willing to take meritorious cases which will leave deserving patients – those that suffered a legitimate tort – without access to the legal process (Hyman 2008). For an average person to gain access to the system, he or she typically must rely on attorneys who believe that the individual has a valid case to agree to represent the client and also furnish much of the cost of the trial (Helland 2003). The attorney will often work on a contingency fee wherein, if the client’s claim is successful, the attorney will be paid a percentage of the final award. The attorney, then, must be willing to bear some
amount of risk to accept a case as, if they do not win, the attorney may not only be unpaid but may lose the expenses of litigation. If there are caps put on noneconomic damages, then some individuals, particularly those that have low economic losses (such as the elderly who are unemployed and thus do not suffer any lost earnings), then the potential award from the litigation decreases. By decreasing the maximum award for the litigation, attorneys have a smaller potential return on their investment of time and resources. Thus, the argument goes, with a smaller potential return with some cases, they will be less likely to take on those clients, leaving potentially deserving clients without access to the legal system.

National tort reform with caps on noneconomic damages has been proposed as a way of decreasing costs in health care (Tumulty 2005). In March of 2012, the United States House of Representatives passed a bill imposing national caps on noneconomic damages (Gringey 2012). While the bill did not pass the Senate, the passage of the bill by the House indicates it is a seriously considered policy. As some states have already enacted these laws, they serve as test cases to estimate the effect such a policy would have on the national level. By evaluating the effects on the number of paid malpractice claims in the states, an estimate of the effect of a national policy on the number of malpractice claims may be generated. Such an estimate, which this study provides, will help policy makers as they evaluate the wisdom of such a national policy.

A challenge with measuring whether there is an effect of such tort reforms on the number of paid malpractice claims relates to the timing of the implementation of the tort reforms. While tort reforms generally have a specific day in which they are passed and become effective, their effect on a specific case varies. For example, statutes of
limitations for claims may last for several years in states which affect whether the tort reforms apply to a specific case (in many states, the law at the time of the tort governs, so if the tort occurred the day before tort reform became effective, the tort reform would not apply to that case). Additionally, there are often constitutional challenges to tort reform bills which lead to uncertainty in whether caps will actually stand, leading some attorneys to continue to take cases and then to challenge the caps on noneconomic damages on state constitution grounds or to avoid cases while waiting for decisions about the constitutionality of the law (Avraham 2006). A third issue relates to the timing of when attorney behavior may change. For example, as soon as a bill is proposed some attorneys may be tempted to shift their practice away from medical malpractice cases in an effort to avoid the issue altogether; or, if attorneys decide to leave the state, it may take several years to arrange to move elsewhere due to licensure and employment. Such issues of timing must be accounted for when estimating the effects of state tort reform.

Few previous studies have investigated whether tort reform affects the number of paid settlements, as most focus on filed lawsuits, size of awards or cost of insurance (Congressional Budget Office 2004). One study, looking at data through 2005 and using a regression model to evaluate six different types of reforms found that tort reform was associated with a decrease in the number of paid claims (Avraham 2007). A second study using instrumental variables to control for policy endogeneity and evaluating tort reforms implemented between 1991 and 2001 found no effect of caps on noneconomic damages on the number of paid claims (Durrance 2010). Both previous studies ignored the issue that the state actors, though enacting a similar policy, did so in different ways. My study (1) expressly recognizes that different state actors implement tort reform
policies differently and (2) recognizes that implementation times are not definite and statutes of limitations, in particular, will affect when results should be expected.

Contributions

In this study I make four major additions to past work: (1) I evaluate states as individual actors and estimate the effect of implementing caps on noneconomic damages by state, rather than estimating an overall effect of such caps on all states; (2) I evaluate these policies with variable implementation times by specifically evaluating how statutes of limitations will effect when the policy should go into effect; (3) I apply a unique difference-in-difference (DiD) approach to compare states with a matched comparison state to evaluate changes over time and (4) I address more recent caps on noneconomic damages than have previously been studied.

A major departure from previous work is that I seek to estimate the effect of caps on noneconomic damages within individual states, rather than seeking to estimate the average effect of a single policy among different states (Durrance 2010; Avraham 2007). This is because I do not assume that the same policy (caps on noneconomic damages) will have the same effect in all states. I thus make state-specific estimates so caps on noneconomic damages may significantly affect the number of paid malpractice settlements in one state but will not in another.

The second innovation focuses on variable implementation times. In particular, I evaluate the state-specific statutes of limitations to these bills to estimate variable times when there may be some effect seen. I then apply a multi-stage interrupted time series to the rate of malpractice payments at these variable times.
The third addition to the literature is that I do a matched DiD evaluation of the states that passed tort reform. By matching states with “control” states that did not implement caps on noneconomic damages I am able to allay some concerns for historical threats to validity relating to noneconomic damage caps’ effect on paid malpractice claims.

The final innovation is that I address the most recent tort reforms that were passed from 2002-2006. Previous studies dealt with earlier reforms. Sufficient time has passed since these reforms were passed that relevant statutes of limitations have been met and any changes in the number of paid claims should now be detectable.

Findings

I find that among states are significant differences in the effect on paid claims of facially similar statutes. For most states, there was not a significant change in the number of paid claims, but for a few states, the historic trend of the number of paid changes did decrease.

The Spillover Effect of a Change in Medicare Reimbursements on Provider Behavior in the Non-Medicare Population for Bariatric Surgery

Implementation Concern

This study addresses the spillover effect of a policy from the intended population to another.

Significance

While Medicare policy changes are extensively studied as to how they affect the Medicare population, these policies may also affect how care is provided to non-Medicare enrollees; i.e., the “spillover effect.” As an example of a specific Medicare
policy change, I evaluate how changes in Medicare reimbursements for bariatric surgery affected provider behavior related to non-Medicare patient care.

The Centers for Medicare and Medicaid Services (CMS) is responsible to reimburse for reasonable care of their covered beneficiaries (*Social Security Act § 1862 2013*). These decisions, though, are not made inside a vacuum. Whenever Medicare seeks to change a payment method, particularly if it seeks to lower reimbursements, the providers (physicians and hospitals) that rely on these payments try to influence CMS’s payment decisions. While these providers that are directly paid by Medicare have a strong interest in the reimbursement decisions that CMS makes, others are also influenced by these decisions. Insurance companies, for example, often negotiate rates based on Medicare payment rates and adopt Medicare coverage policies (Cassidy and Dentzer 2010; Reinhardt 2010; Mathews and Mcginty 2010).

Bariatric surgery consists of a family of related procedures that are intended to treat morbidly obese people who have failed to lose weight using other methods (Buchwald 2004). Prior to 2006, Medicare would reimburse for gastric bypass procedures if performed on patients with extreme obesity, was medically appropriate and corrected an illness which caused or aggravated the obesity (Centers for Medicare & Medicaid Services 1979). Because of the uncertainty of whether the surgery was appropriate, there was significant variation in how and where bariatric decisions were performed. In 2006, Medicare provided a national coverage determination (NCD) as to how bariatric surgery must be performed to be eligible for Medicare reimbursements (Centers for Medicare & Medicaid Services 2006a). This NCD limited the number of facilities that could be reimbursed for providing bariatric surgery by requiring that they
be performed in facilities accredited as *Bariatric Surgery Centers of Excellence* or as *Level 1 Bariatric Surgery Centers*. Additionally, CMS clarified what were acceptable clinical indicators for bariatric surgery (such as requiring a body mass index (BMI) over 35 when formally no specific value was fixed) and agreed to cover additional procedures that had recently been approved by the FDA and specifically excluded other procedures from coverage (Centers for Medicare & Medicaid Services 2006) (Centers for Medicare & Medicaid Services 2006). Then, in 2009, CMS made an additional change wherein they expressly stated that Type 2 diabetes mellitus, for the purpose of coverage, is a comorbid condition which effectively increased the number of patients eligible for reimbursable bariatric surgery (Centers for Medicare & Medicaid Services 2009). The effect of the two changes was to first limit when and where services could be used in 2006 and then expand access in 2009.

Prior work has evaluated how these changes affected Medicare populations (Flum et al. 2011; Nguyen et al. 2010; Dimick JB 2013). In particular, the types of surgeries changed, the number of surgeries temporarily decreased and quality outcomes were suggested to improve. What is unknown, however, is what spillover effect, if any, there was on the non-Medicare population. One study did estimate how the policy may have affected the health outcomes of privately insured individuals, but no work has estimated how it affected the entire non-Medicare population (Kwon et al. 2012). This study evaluates how a Medicare reimbursement change for bariatric surgery affected the rate of bariatric surgery among the whole non-Medicare population.

This study, though limited to the effect of Medicare on bariatric surgery, will be useful to policy makers as it suggests whether Medicare policies affect non-Medicare
patients. In particular, if there is a significant effect on the non-Medicare population, then researchers and policy makers will need to be aware of these changes when designing or studying policies.

**Innovation**

This study provides two contributions to the literature: (1) I quantify the change in the non-Medicare population that mimicked the change within the Medicare population (spillover effect) and (2) I evaluate the timing differential of any change (lag time).

The most important feature of this study is that I expressly evaluate how a Medicare policy change affects the non-Medicare population. Previous studies have evaluated the NCD’s effect on Medicare patients, but no studies have evaluated its effect on the non-Medicare population. Additionally, there is dearth of literature on Medicare policies’ effect on the non-Medicare population, often because the data used to evaluate Medicare changes is limited to Medicare data.

The second addition to the literature is that I expressly identify any timing delay (or lag time) between the change in Medicare and any change within the non-Medicare population. The topic of bariatric surgeries is ideal because the timing between the NCD and the change in Medicare rates of bariatric surgery are very pronounced and any timing differential with changes in the non-Medicare population should be easy to identify.

**Findings**

Following the implementation of the 2006 National Coverage Decision, there was a significant drop in the rate of bariatric surgeries for both the Medicare and the non-Medicare population. The timing and magnitude of the effect was nearly identical for both populations.
Chapter References


Chapter 2 – Estimating State-Level Crowd-Out of Children’s Health Insurance

Note: Eric Seiber is co-author of this chapter.

Abstract

**Background.** Health insurance crowd-out occurs when individuals are enrolled in a public health insurance plan and, but for the public option, would have been enrolled in a private plan. The crowding-out of private insurance is often used to criticize state Medicaid and Children’s Health Insurance Program (CHIP) expansion as already insured children move their coverage to the states, at the public’s expense. A difficulty in discussing crowd-out comes from inconsistent estimates. Previous work focusing on the expansion of public programs has led to estimates ranging from 0% to 50% of the children newly insured on public plans being crowded-out.

**Methods.** We apply a regression discontinuity approach to estimate how many children near the state Medicaid/CHIP threshold are crowded-out of private insurance. This approach allows estimates of crowd-out near the eligibility threshold independent of any expansion. Data from the American Community Survey’s yearly survey of American households allows for state-level estimates of crowd-out.

**Results.** We find considerable heterogeneity in the crowd-out that occurs in each state, ranging from no crowd-out to over 18% in states with similar eligibility thresholds.
Additionally, we found that as state eligibility thresholds increase, children are less likely to be crowded-out.

**Discussion.** This research indicates that national estimates of crowd-out are inappropriate because state-specific Medicaid and CHIP programs have state-specific crowd-out. Additionally, it indicates that wealthier families that are eligible for public insurance are less likely to switch from private to public coverage than families earning less. Future work should identify reasons for the heterogeneity among states.
Background

The debate on expanding public health insurance coverage to adults has little consensus. Some support exists, though, to provide health insurance to low income children who would lack even basic coverage without government assistance. This coverage is accomplished primarily via the joint federal/state programs of Medicaid and the Children’s Health Insurance Program (CHIP)\(^1\). Since the expansion of Medicaid programs, there has been continual debate about the role of “crowd-out”, or the movement from private to public insurance coverage, when estimating the effects and costs of these publicly financed programs.

Current law requires states to introduce measures to limit crowd-out, which is primarily achieved by mandating waiting periods before enrollment, requiring asset tests, checking for insurance coverage elsewhere (database searches) and adding premiums and copays (Hoag et al. 2011). The challenge is that efforts to limit crowd-out will simultaneously discourage uninsured children, who in these programs are designed to help, from enrolling. An accurate measurement of crowd-out is thus needed to guide policymakers as they design methods to enroll children in public programs.

Crowd-out Explained

\(^1\) Throughout this chapter we refer to the Medicaid, SCHIP (State Children’s Health Insurance Program) and CHIP programs under the umbrella term “Medicaid.”
Crowd-out occurs when individuals are insured by a public program and would be covered by private insurance but for their enrollment in the public program. In recent years focus has been on estimating the degree to which children are crowded-out of private insurance because of the expansion of CHIP eligibility levels.

There are two distinct situations that can result in crowd-out, each of which leads to challenges in estimating total crowd-out. Crowd-out of existing insured children arises when a child who is privately insured switches to a public plan, which is known as substitution crowd-out (Davidson, Blewett, and Call 2004). The difficulty in estimating this arises because not every child who is privately insured and switches to public insurance would still be privately insured but for the existence of the public program. For example, many children who are privately insured and switch to public insurance do so because they lost private insurance, such as when their parent changes or loses employment. Retrospectively analyzing whether children would have maintained private insurance is difficult as methods used to estimate this (surveys, public reporting, enrollment rates and the like) often lack a definitive means of identifying whether a child would still be privately insured if they were not eligible for the public plan.

The second variety of crowd-out occurs when a child with public coverage remains on that public insurance when a private offer becomes available (L. Dubay 1999). We refer to this as “continuation crowd-out” because children have continued on
the public plan after a private option becomes available. With no evidence of past, private insurance, it is difficult to estimate whether the child would have become newly insured with private insurance but for the existence of the public plan. Further, it is difficult to estimate this second type of crowd-out using surveys and interviews because families of many children that are publicly covered cannot conclusively say that, but for the public insurance, they have would enrolled their children in a private plan.

Past Work

Crowd-out has been debated since Cutler and Gruber in 1996 first estimated that fifty percent of newly insured Medicaid children were crowded-out of private plans following an expansion of eligibility (Cutler and Gruber 1996). Subsequent analysis has suggested varying estimates ranging from values similar to Cutler and Gruber to values near zero (Gruber and Simon 2008). These studies primarily relied on three methods: an econometric instrumental variable (IV) approach (Cutler and Gruber 1996; Lo Sasso and Buchmueller 2004b), comparing children who gained access to expanded Medicaid/CHIP programs to control groups that did not (Blumberg, Dubay, and Norton 2000; Yazici and

2 An example would be a child who is enrolled in CHIP because her parents are employed in jobs that do not offer employer-sponsored insurance and, but for the public plan, would have been uninsured. Subsequently, a parent finds new employment that offers health insurance. If the child were uninsured, her parents would have enrolled her privately at this point, but because she is on an existing CHIP plan, they continue CHIP enrollment.
Kaestner 2000) and estimates of substitution using surveys (Sommers et al. 2007), all of which have weaknesses (E. E. Seiber and Sahr 2011, E5–6).

The IV approach of Cutler and Gruber analyzes eligibility variability across states and estimates substitution of private insurance following a change in public insurance eligibility levels. While this does allow for researchers to adjust for policy endogeneity, it does lead to several problems. The first issue is its results are very sensitive to assumptions in the model. Particularly, different ways of treating the same private insurance variable led to overall estimates of crowd-out ranging from 10% to 47% (Lo Sasso and Buchmueller 2004b, 1078). A second difficulty, which this paper intends to address, is that the IV approach is not able to estimate state-specific crowd-out due to its reliance on inter-state variations to arrive at a national estimate (E. E. Seiber and Sahr 2011, E5). Finally, this approach relies on expansions of the public program.

The second approach evaluates expansions of public programs by comparing children that are newly eligible to individuals that are not, such as non-expansion children or adults, using longitudinal data. These studies have not been able to identify state-specific estimates and are reliant on program expansions.

The last approach relies on primary data to estimate the number of individuals who dropped private insurance in exchange for public insurance. The general approach is to interview those that newly enroll in Medicaid and ask about former insurance coverage to gain an estimate of the number that dropped private coverage to move to a public plan. In addition to the cost and difficulty in acquiring such primary data, this method is limited because it only focuses on the substitution of private insurance for
public insurance and ignores the children who remain on public insurance plans when private plans become available. None of the approaches have been able to estimate continuation crowd-out.

This paper adds to the existing literature in two primary ways: (1) It demonstrates an additional method to estimate crowd-out independent of a policy change, capturing both substitution and continuation crowd-out, and (2) it produces an estimate of crowd-out levels in individual states. While previous studies focused on estimating the number of children crowded-out before and after a policy change (usually an expansion in Medicaid) we look at the steady-state of crowd-out independent of any policy change which allows us to estimate all children who were crowded-out, both by substituting private for public and continuing on public after private insurance became available. The large sample size of the American Community Survey (Davern et al. 2009), which recently began tracking health insurance status, allows us to estimate crowd-out at the state-level. The regression discontinuity method we employ should not be viewed as a replacement for existing crowd-out estimation methods, but as an additional estimation tool that can help triangulate crowd-out estimates.

Methods

In this analysis we expand on previously published crowd-out estimates based on a regression discontinuity design and data from the American Community Survey. Regression discontinuity (RD) is used to determine the effect of a treatment or policy which is applied at an arbitrary threshold or cutoff point. RD is applied in non-experimental settings where eligibility in a specific program is determined by an arbitrary
point along a continuum. The effect of the policy is estimated by comparing the values immediately on each side of the threshold and the difference between them is attributable to the policy (Lee and Lemieux 2010).

While the RD approach was developed for other disciplines (Thistlethwaite and Campbell 1960) (Schochet 2008), the externally determined threshold for Medicaid eligibility, where children whose family income is below the cutoff are eligible for Medicaid while those with family incomes are just above the threshold are not eligible, allows RD to be used to estimate effects of the program. Previous studies by Card and Shore-Sheppard (Card and Shore-Sheppard 2004), Koch (Koch 2010) and De La Mata (De La Mata 2012) have used this approach to estimate the crowding-out of private health insurance.

Card and Shore-Sheppard compared children whose eligibility was determined by their age and evaluated children who were eligible for an expansion (those born after September 1983) to those that were ineligible (those born September 1983 or earlier). The threshold, then, was the age of children who were eligible. Koch and De La Mata performed a similar analysis to ours, using the income eligibility as the threshold, but due to their smaller sample sizes (using Medical Expenditure Panel Survey, Current Population Survey and Panel Study of Income Dynamics data), they were forced to
combine states together to generate national estimates\(^3\). By taking advantage of the representative national sample of the American Community Survey data we are able to expand their work and apply this approach at the state level and compare differences in crowd-out between states.

We will illustrate the RD design by looking at two states: one with no estimated crowd-out and one with crowd-out. The basis for estimating crowd-out with RD is that the percent of children with private insurance generally increases as the child’s family’s income increases (DeNavas-Walt, Proctor, and Smith 2006, 26). Thus, it is expected that a higher proportion of children whose families earn 150% of the federal poverty level (FPL) will have private health insurance than children whose families earn only 100% of the FPL. When there is no crowd-out the proportion of children with health insurance is expected to increase gradually until it plateaus at a maximum level. Figure 2-1A shows an example of a state (Maryland) where we found negligible crowd-out as the regression lines on either side of the eligibility threshold both predict the same number of children having private health insurance. When there is crowd-out we see a break or “discontinuity” in the predicted values as is seen in Figure 2-1B (North Carolina). The

\(^3\) Since different states have different eligibility thresholds, they combined children based on distance from the eligibility threshold. For example, children whose families earn 290% of the FPL in a state with a 300% eligibility threshold would be combined with children whose families earn 190% of the FPL in a state with a 200% eligibility threshold.
expected crowd-out is the discontinuity between the two regression lines. In other words, but for the existence of Medicaid eligibility (for all children whose families earn less than FPL eligibility threshold), there would be no discontinuity in the predicted values.
Figure 2-1: Example of Regression Discontinuity
The RD approach has two significant advantages over other methods of estimating crowd-out. The first is that it does not focus on measuring the effect of a specific policy change such as the SCHIP expansions in the 1990s (See Table 1 of Gruber and Simon 2008). Past approaches relied on difference-in-difference analysis where children that became newly eligible for Medicaid were compared to groups that did not gain eligibility such as adult men (L. C. Dubay and Kenney 1996) or other children (Yazici and Kaestner 2000). Such difference-in-difference approaches raise concerns as the comparison group and the treatment group may be different in significant but unidentified ways (Bertrand, Duflo, and Mullainathan 2004). The RD approach allows us to look at the “steady-state” of Medicaid enrollment, long after a policy altered enrollment eligibility.

A second advantage of the RD design is that its results can be considered causal. As long as the variation of individuals assigned on each side of the threshold is approximately random, RD can be used to test the effects of the treatment as if it were a randomized experiment (Lee and Lemieux 2010, 283). In this case, the “treatment” is whether a child is eligible for Medicaid.

The RD approach also has some known weaknesses. First, it only allows us to evaluate the localized effect of crowd-out; specifically, we can only estimate the rate of children who were crowded out near the eligibility threshold. We are unable to estimate how many total children were crowded-out because the estimates do not necessarily hold.
for other income levels. While this makes it impossible to compare all states to each other, it does allow us to compare states that have the same Medicaid cutoff-level.

A second weakness is that RD requires those being measured to be assigned to a treatment independent of their choice. If those that are measured can manipulate whether they are eligible for the treatment, the eligibility threshold is no longer an arbitrary cutoff (Lee and Lemieux 2010, 284) and the family income is thus endogenous to the eligibility threshold and the results may not be valid (Lee and Lemieux 2010). We tested for potential endogeneity in two ways: (1) by evaluating whether the population density shifts near the eligibility threshold and (2) by conducting a sensitivity analysis of those near, but not directly next to the threshold.

McCrary (McCrary 2008) suggests that if individuals do, indeed, modify their income to qualify for the intervention (in this case, eligibility for Medicaid), the population density will change near the eligibility threshold. If families near, but above the threshold voluntarily choose to lower their incomes to the point of eligibility, there would be an increase in the total population of children immediately below the eligibility threshold and a decrease in children who are immediately above the threshold. To test whether there is a difference, we created bins representing the population density as a function of the FPL and regressed the height of the bins to estimate if the population, near the eligibility threshold, shifted towards the eligibility side. Only one state at p<.05 showed evidence of a population shift towards eligibility (Idaho, p=.039). Two other states were significant at p<.10 (Nevada, p=.078 and Texas, p=073). There is thus little evidence of a systematic movement of income towards eligibility.
Under the assumption that only individuals on the margins would alter their income to qualify their children for public insurance, we repeated our regression discontinuity performing a sensitivity analysis. In this case we excluded data from individuals within 10% of the FPL (those most likely to change their income to affect their eligibility) and found only a modest change in the estimated crowd-out: the median change was 0% (mean of 0.02%) with an interquartile range of -1.45% to 1.27%. To illustrate the magnitude of such a change, a family of four would have to reduce its monthly income by $220 to decrease its total income by 10% of the FPL. Absent some underlying health concern, it is unlikely that significant numbers of parents would intentionally lower their income to gain Medicaid for their children, particularly when the median price of insuring a child on an individual plan is less than $100 per month (eHealth 2011, 13). Additionally, there are significant process barriers to enrollment in Medicaid (Stuber and Bradley 2005) which make enrollment in the program an uncertainty; it is unlikely that a family would intentionally decrease their income if they were unsure whether, and when, they could enroll their children in Medicaid.

A final weakness of the RD design deals with the discontinuity itself where the eligibility threshold may not clearly divide those eligible from those ineligible for Medicaid. This may occur for multiple reasons. First, since income is self-reported based on memory of the preceding 12 months, there is likely some error with its measurement (U.S. Census Bureau 2009). Second, some states disregard some forms of income, meaning some children with incomes above the threshold may, in fact, be eligible for the program. Third, more than half of states have a continuous eligibility
policy where children who are enrolled in Medicaid can remain enrolled for up to 12 months following a change in income above the eligibility limit (Horner 2008).

For a sharp RD, the probability of being eligible for Medicaid must go from 1 to 0 immediately at the eligibility threshold as income increases. In a fuzzy RD, the probability of being eligible for Medicaid will decrease at the eligibility threshold by an amount less than 1 (for example, if only 80% of children lose eligibility at the threshold, then the probability of being eligible for Medicaid decreases from 1 to 0.2 as income increases). To interpret the fuzzy RD, the effect of the eligibility on private insurance must be divided by the change in the probability of being eligible for Medicaid (Lee and Lemieux 2010, 299). Since we do not know, with certainty, what the proportion of children at the threshold truly lose eligibility, we estimate this as a sharp RD (i.e., divide our estimates by 1 rather than a value less than 1). This biases our results towards zero, so our estimates should be viewed as conservative estimates of crowd-out at the eligibility threshold. If the probability of losing eligibility at the threshold is indeed less than 1, the actual estimates for crowd-out would be higher.

Data

All data came from the Public Use Microdata Sample (PUMS) from the 2010 American Community Survey (ACS). The ACS is a yearly survey performed by the US Census Bureau that collects a variety of demographic information from a randomized sample of households in the United States (Griffin and Waite 2006). Household information is broken down by individual within the home and then each individual is weighted to allow estimates for the entire population. The 2010 ACS PUMS file
contained information from 1,235,126 households and 3,013,142 individuals. After eliminating all individuals over the age of 18, our sample size was 732,906 unique records representing a weighted total of 78,661,704 children.

The large sample size allows for more accurate estimates of the percent of children who are insured in any income level compared to other datasets. Further, the large sample size allows us to look at the state-specific crowd-out levels. This state-by-state comparison, in particular, is a desirable approach as Medicaid and SCHIP are state-run programs and what is true in one state may not hold in another.

Data for the ACS is collected throughout the year with monthly surveys that are aggregated to create yearly estimates of the population (U.S. Census Bureau 2009). Each survey provides information on numerous demographic variables including, of interest in this study, income and insurance status. The insurance status variable refers to insurance status at the time of the survey. Income refers to income earned during the prior 12-month period (Noss 2011). The Census Bureau then creates an estimate of each individual’s poverty level based on family income from the previous 12 months, family size and age of family members (U.S. Census Bureau 2011). We use this income-to-poverty ratio recode variable as the basis for the federal poverty level of the children in our sample.

Medicaid or CHIP eligibility is determined by household income relative to the FPL established by the Department of Health and Human Services (HHS). In 2010, this was $10,830 for a single-person home and an additional $3,740 for each subsequent person in the household (Department of Health and Human Services 2010). To calculate
the HHS poverty level, we utilized the Census Bureau’s income-to-poverty ratio recode variable multiplied by the HHS poverty level and then divided by the Census Bureau’s income level. For example, with a family of two, we multiply the Census Bureau’s poverty level by $14,570 (100% of the HHS poverty income for a family of two) / $14,676 (100% of the Census Bureau’s weighted average poverty income for a family of two). This gives us the change between ACS poverty level and HHS poverty level. This makes a relatively small difference for most children in our sample, but makes a large difference for children in families with more than 9 people.

A second analysis that we performed was based on income disregards where some states allow families to exclude a portion of their income when determining eligibility for Medicaid. In early 2008, the Kaiser Family Foundation conducted a survey of all states and compiled their income disregard rules (Ross et al. 2008)\(^4\). We used the rules that were in place in early 2008 and applied them to our 2010 data and estimated the new income, given the disregards. There were two challenges with this. The first is that we are only able to apply the disregards that are applicable to everyone (those based on worker’s incomes) and we were not able to estimate other disregards (such as amount paid or received in child support). The second challenge is that some states significantly

\(^4\) The disregard rules were thus applicable, primarily, to 2007 year data. The ACS did not begin tracking health insurance until 2008.
changed their Medicaid/CHIP programs between early 2008 and 2010. Because we were unable to accurately estimate the income given the sometimes significant changes to the Medicaid/CHIP plans in some states, we primarily focus on the results that were derived without the disregards, but we do provide these estimates.

**Analytic Strategy**

For the RD estimates, we first grouped children, by state, into blocks based on their household income as a percent of the FPL. For example, children whose families earn from 0-10 percent of the FPL are in one block, children who earn from 11-20 percent are in another block, and so on. The width of the blocks was chosen based on the need to balance having sufficient observations within each block while maximizing the number of blocks to increase the power of our regression. Wider blocks result in a larger sample of children within each block but fewer total blocks to regress and smaller blocks result in the opposite. A block size of 10% of the FPL was chosen as a balance between these two competing interests.

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5 In particular, 11 states (Alabama, Colorado, Indiana, Iowa, Louisiana, Montana, New York, Pennsylvania, South Carolina, Washington and Wisconsin) increased their eligibility thresholds by at least 50% of the FPL. Additionally, 4 more states (Kansas, Nebraska, North Dakota and West Virginia) increased their eligibility threshold to a lesser extent. It is unknown how income disregard rules changed during this time.

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Within each block we then calculated the percent of children in each block who have private insurance using weights provided by the ACS\(^6\). We then plotted the blocks of data as function of the FPL. The result is that we have created blocks with a width of 10% of the FPL and we can plot the percentage of children with private insurance in each block as a function of the FPL value of that block closest to the eligibility threshold (i.e., for a block going from 20%-30% of the FPL, we regress the value at 20%). We created these blocks of children because the RD design depends on looking at the effect of the Medicaid eligibility threshold on the proportion of children with private insurance; the blocks allow us to estimate the proportion of children at a given FPL that have private health insurance. The unit of analysis, then, is the proportion of children with private insurance at different income levels, by state. There was one data point for each income block, with 29 income blocks for most of the states.

Using state Medicaid/CHIP income eligibility from the Kaiser Foundation (Heberlein et al. 2011, 29), we centered the blocks for each state around that state’s eligibility threshold and evaluated the blocks plus or minus 150% from the eligibility threshold.

\(^6\) The median weighting for each data point was 86 with an interquartile range of 63 to 127. The median subject from the ACS survey, then, represented 86 actual children.
threshold\textsuperscript{7,8}. In Figure 2-2, for example, of children in Oklahoma whose families earned from 125-135\% of the FPL (50\% of the FPL less than the cutoff level of 185\%), approximately 32\% had private insurance and of children whose families earned from 235-245\% of the FPL (50\% of the FPL more than the cutoff level), approximately 75\% had private insurance.

By regressing the blocks of the percent of children with private insurance who are eligible for Medicaid, we were able to predict the percent of children who are privately insured at the eligibility cutoff if the child is Medicaid eligible. Then, by regressing the blocks of the percent of children with private insurance who are not eligible for Medicaid, we are able to predict how many children are privately insured at the eligibility cutoff if they are not eligible for Medicaid. The general form of the regression model we estimated is as follows:

Equation 2-1:

\[ Y = \beta_0 + \beta_1 \text{Poverty Level} + \beta_2 \text{Ineligible} + \beta_3 (\text{Poverty Level} \times \text{Ineligible}) \]

\textsuperscript{7} This reduction in total blocks to those nearest the threshold allows us to linearly regress the data points closest to the threshold value.

\textsuperscript{8} We also excluded any blocks of data that were based on fewer than eight observations (16 total blocks, primarily in the District of Columbia).
The dependent variable, \( Y \), is the estimated percent of children at any \textit{Poverty Level} with private insurance. With the blocks of data centered around the eligibility threshold, \( \beta_0 \) is the expected percent of children at the threshold with private insurance based on those eligible for Medicaid. \( \beta_0 + \beta_2 \) equals the expected percent of children with private insurance at the threshold based on those ineligible for Medicaid. \( \beta_1 \) is the slope of the line predicting the percent of children with private insurance as a function of poverty level among those eligible for Medicaid. \( \beta_1 + \beta_3 \) equals the slope of the line predicting the percent of children with private insurance as a function of poverty level among those ineligible for Medicaid. \textit{Ineligible} is a dummy variable with 1=ineligible for Medicaid and 0 otherwise. For an example of this strategy applied to the state of Oklahoma, refer to Figure 2-2. Graphical depictions for each state are available in Appendix B.
Figure 2-2: Coefficients of Regression Discontinuity Model

The estimated number of children crowded out is represented by $\beta_2$ which is the difference between the estimated percent of children with private insurance at the threshold that are ineligible for Medicaid ($\beta_0 + \beta_2$) and the estimated percent of children at the threshold with private insurance that are eligible for Medicaid ($\beta_0$). P-values are calculated based on the significance of $\beta_2$.

We used linear regression to estimate crowd-out for each state. We experimented with non-linear regression but linear regression provided the most consistent fit of the data between states, particularly when considering the small number of blocks that we had to work with (approximately 15 blocks on each side of the eligibility threshold). We
alternatively also ran polynomial regressions (second and third order) that would capture non-linear relationships. Graphs of the third order polynomial fit for each state are available in Appendix C. As the polynomial fits did not significantly alter our findings, all results presented use the linear fit. The primary code used to generate these estimates is available in Appendix D.

Findings

Table 2-1 shows the estimated crowd-out for each of the states and the District of Columbia and Figure 2-3 includes this same information in graphical form with a linear trend line indicating the estimated localized crowding-out of children as a percent of the FPL.
<table>
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<th>State</th>
<th>Cutoff</th>
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<th>Estimated Crowd Out</th>
<th>Standard Error</th>
<th>P-Value</th>
<th>Estimated Crowd Out</th>
<th>Standard Error</th>
<th>P-Value</th>
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<td>0.765</td>
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<td>Colorado</td>
<td>250</td>
<td>5724</td>
<td>1.53%</td>
<td>3.54%</td>
<td>0.669</td>
<td>-0.44%</td>
<td>3.29%</td>
<td>0.893</td>
</tr>
<tr>
<td>Connecticut</td>
<td>300</td>
<td>3143</td>
<td>2.82%</td>
<td>4.82%</td>
<td>0.564</td>
<td>0.53%</td>
<td>4.82%</td>
<td>0.912</td>
</tr>
<tr>
<td>Delaware</td>
<td>200</td>
<td>974</td>
<td>16.94%</td>
<td>9.21%</td>
<td>0.077*</td>
<td>11.28%</td>
<td>10.18%</td>
<td>0.278</td>
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<tr>
<td>Washington, DC</td>
<td>300</td>
<td>204*</td>
<td>7.80%</td>
<td>26.98%</td>
<td>0.778</td>
<td>-24.99%</td>
<td>10.90%</td>
<td>0.042**</td>
</tr>
<tr>
<td>Florida</td>
<td>200</td>
<td>23309</td>
<td>6.19%</td>
<td>2.32%</td>
<td>0.013**</td>
<td>6.94%</td>
<td>2.14%</td>
<td>0.003***</td>
</tr>
<tr>
<td>Georgia</td>
<td>235</td>
<td>12491</td>
<td>3.86%</td>
<td>2.62%</td>
<td>0.152</td>
<td>3.36%</td>
<td>3.00%</td>
<td>0.273</td>
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Table 2-1: State-Level Estimates of Crowd-Out
Table 2-1 continued

<table>
<thead>
<tr>
<th>State</th>
<th>Cutoff</th>
<th>Surveyed Children</th>
<th>Estimated Crowd-Out</th>
<th>Standard Error</th>
<th>P-Value</th>
<th>Estimated Crowd-Out</th>
<th>Standard Error</th>
<th>P-Value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Hawaii</td>
<td>300</td>
<td>1394</td>
<td>-0.10%</td>
<td>8.75%</td>
<td>0.990</td>
<td>-0.51%</td>
<td>4.87%</td>
<td>0.917</td>
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<tr>
<td>Idaho</td>
<td>185</td>
<td>3092</td>
<td>7.84%</td>
<td>4.85%</td>
<td>0.117</td>
<td>7.09%</td>
<td>5.19%</td>
<td>0.183</td>
</tr>
<tr>
<td>Illinois</td>
<td>200</td>
<td>15873</td>
<td>4.92%</td>
<td>2.62%</td>
<td>0.071*</td>
<td>6.18%</td>
<td>2.81%</td>
<td>0.037**</td>
</tr>
<tr>
<td>Indiana</td>
<td>250</td>
<td>8868</td>
<td>-0.51%</td>
<td>3.42%</td>
<td>0.882</td>
<td>-2.86%</td>
<td>3.45%</td>
<td>0.414</td>
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<tr>
<td>Iowa</td>
<td>300</td>
<td>4082</td>
<td>-4.08%</td>
<td>3.41%</td>
<td>0.242</td>
<td>0.10%</td>
<td>3.23%</td>
<td>0.974</td>
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<tr>
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<td>241</td>
<td>3978</td>
<td>2.97%</td>
<td>3.92%</td>
<td>0.455</td>
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<td>3.69%</td>
<td>0.381</td>
</tr>
<tr>
<td>Kentucky</td>
<td>200</td>
<td>5559</td>
<td>2.71%</td>
<td>5.18%</td>
<td>0.604</td>
<td>5.43%</td>
<td>4.77%</td>
<td>0.265</td>
</tr>
<tr>
<td>Louisiana</td>
<td>250</td>
<td>5304</td>
<td>-2.61%</td>
<td>5.16%</td>
<td>0.618</td>
<td>-0.35%</td>
<td>3.27%</td>
<td>0.914</td>
</tr>
<tr>
<td>Maine</td>
<td>200</td>
<td>1572</td>
<td>14.26%</td>
<td>7.33%</td>
<td>0.062*</td>
<td>19.71%</td>
<td>7.19%</td>
<td>0.011**</td>
</tr>
<tr>
<td>Maryland</td>
<td>300</td>
<td>5425</td>
<td>0.29%</td>
<td>3.18%</td>
<td>0.927</td>
<td>0.37%</td>
<td>3.65%</td>
<td>0.920</td>
</tr>
<tr>
<td>Massachusetts</td>
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<td>5611</td>
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<td>3.08%</td>
<td>0.644</td>
<td>1.28%</td>
<td>3.60%</td>
<td>0.725</td>
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<table>
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<th>State</th>
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<th>Surveyed Children</th>
<th>Estimated Crowd-Out</th>
<th>Standard Error</th>
<th>P Value</th>
<th>Estimated Crowd-Out</th>
<th>Standard Error</th>
<th>P Value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Michigan</td>
<td>200</td>
<td>13221</td>
<td>5.89%</td>
<td>2.72%</td>
<td>0.039**</td>
<td>7.13%</td>
<td>2.62%</td>
<td>0.011**</td>
</tr>
<tr>
<td>Minnesota</td>
<td>275</td>
<td>6986</td>
<td>-0.17%</td>
<td>3.27%</td>
<td>0.958</td>
<td>-0.59%</td>
<td>3.11%</td>
<td>0.850</td>
</tr>
<tr>
<td>Mississippi</td>
<td>200</td>
<td>4349</td>
<td>-0.24%</td>
<td>5.10%</td>
<td>0.962</td>
<td>8.09%</td>
<td>6.32%</td>
<td>0.212</td>
</tr>
<tr>
<td>Missouri</td>
<td>300</td>
<td>6604</td>
<td>-0.82%</td>
<td>2.63%</td>
<td>0.756</td>
<td>2.35%</td>
<td>3.27%</td>
<td>0.478</td>
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<tr>
<td>Montana</td>
<td>250</td>
<td>1258</td>
<td>-6.72%</td>
<td>9.73%</td>
<td>0.495</td>
<td>-13.31%</td>
<td>7.62%</td>
<td>0.093*</td>
</tr>
<tr>
<td>Nebraska</td>
<td>200</td>
<td>2549</td>
<td>1.32%</td>
<td>5.26%</td>
<td>0.804</td>
<td>-1.11%</td>
<td>3.69%</td>
<td>0.766</td>
</tr>
<tr>
<td>Nevada</td>
<td>200</td>
<td>3892</td>
<td>9.22%</td>
<td>5.07%</td>
<td>0.080*</td>
<td>8.72%</td>
<td>3.92%</td>
<td>0.035**</td>
</tr>
<tr>
<td>New Hampshire</td>
<td>300</td>
<td>1260</td>
<td>7.60%</td>
<td>8.98%</td>
<td>0.405</td>
<td>6.00%</td>
<td>6.37%</td>
<td>0.355</td>
</tr>
<tr>
<td>New Jersey</td>
<td>350</td>
<td>13417</td>
<td>-3.36%</td>
<td>2.45%</td>
<td>0.182</td>
<td>-4.34%</td>
<td>2.75%</td>
<td>0.126</td>
</tr>
<tr>
<td>New Mexico</td>
<td>235</td>
<td>2580</td>
<td>11.71%</td>
<td>5.50%</td>
<td>0.043**</td>
<td>11.96%</td>
<td>5.32%</td>
<td>0.033**</td>
</tr>
<tr>
<td>New York</td>
<td>400</td>
<td>23428</td>
<td>0.70%</td>
<td>2.15%</td>
<td>0.746</td>
<td>2.56%</td>
<td>2.35%</td>
<td>0.288</td>
</tr>
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Continued
Table 2-1 continued

<table>
<thead>
<tr>
<th>State</th>
<th>Cutoff</th>
<th>Surveyed Children</th>
<th>Estimated Crowd-Out</th>
<th>Standard Error</th>
<th>State</th>
<th>Cutoff</th>
<th>Surveyed Children</th>
<th>Estimated Crowd-Out</th>
</tr>
</thead>
<tbody>
<tr>
<td>North Carolina</td>
<td>200</td>
<td>12589</td>
<td>9.91%</td>
<td>3.08%</td>
<td>0.003***</td>
<td>12.15%</td>
<td>2.82%</td>
<td>0.000***</td>
</tr>
<tr>
<td>North Dakota</td>
<td>160</td>
<td>836</td>
<td>7.98%</td>
<td>10.90%</td>
<td>0.471</td>
<td>2.16%</td>
<td>11.79%</td>
<td>0.856</td>
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<td>Ohio</td>
<td>200</td>
<td>15223</td>
<td>5.23%</td>
<td>2.70%</td>
<td>0.064*</td>
<td>4.53%</td>
<td>2.39%</td>
<td>0.069*</td>
</tr>
<tr>
<td>Oklahoma</td>
<td>185</td>
<td>5907</td>
<td>18.23%</td>
<td>3.10%</td>
<td>0.000***</td>
<td>11.93%</td>
<td>3.91%</td>
<td>0.005***</td>
</tr>
<tr>
<td>Oregon</td>
<td>300</td>
<td>3886</td>
<td>-0.66%</td>
<td>3.67%</td>
<td>0.858</td>
<td>-1.19%</td>
<td>3.44%</td>
<td>0.731</td>
</tr>
<tr>
<td>Pennsylvania</td>
<td>300</td>
<td>12990</td>
<td>0.61%</td>
<td>2.23%</td>
<td>0.787</td>
<td>0.07%</td>
<td>2.47%</td>
<td>0.976</td>
</tr>
<tr>
<td>Rhode Island</td>
<td>250</td>
<td>993</td>
<td>6.89%</td>
<td>8.05%</td>
<td>0.400</td>
<td>2.89%</td>
<td>8.00%</td>
<td>0.720</td>
</tr>
<tr>
<td>South Dakota</td>
<td>200</td>
<td>1236</td>
<td>3.74%</td>
<td>4.08%</td>
<td>0.368</td>
<td>-2.58%</td>
<td>4.60%</td>
<td>0.580</td>
</tr>
<tr>
<td>South Carolina</td>
<td>200</td>
<td>6140</td>
<td>8.55%</td>
<td>7.73%</td>
<td>0.278</td>
<td>11.14%</td>
<td>8.81%</td>
<td>0.217</td>
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<tr>
<td>Tennessee</td>
<td>250</td>
<td>7532</td>
<td>-2.13%</td>
<td>3.65%</td>
<td>0.563</td>
<td>-3.30%</td>
<td>3.39%</td>
<td>0.338</td>
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<tr>
<td>Texas</td>
<td>200</td>
<td>37133</td>
<td>7.44%</td>
<td>1.90%</td>
<td>0.000***</td>
<td>9.28%</td>
<td>2.37%</td>
<td>0.000***</td>
</tr>
</tbody>
</table>

Continued
Table 2-1 continued

<table>
<thead>
<tr>
<th>State</th>
<th>Cutoff</th>
<th>Surveyed Children</th>
<th>No Disregarded Income</th>
<th>Disregarded Income based on 2008 Rules</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td>Estimated Crowd-Out</td>
<td>Standard Error</td>
</tr>
<tr>
<td>Utah</td>
<td>200</td>
<td>6050</td>
<td>10.61%</td>
<td>2.92%</td>
</tr>
<tr>
<td>Vermont</td>
<td>300</td>
<td>659</td>
<td>16.20%</td>
<td>10.54%</td>
</tr>
<tr>
<td>Virginia</td>
<td>200</td>
<td>8273</td>
<td>1.51%</td>
<td>3.77%</td>
</tr>
<tr>
<td>Washington</td>
<td>300</td>
<td>6995</td>
<td>4.59%</td>
<td>3.56%</td>
</tr>
<tr>
<td>West Virginia</td>
<td>250</td>
<td>2001</td>
<td>-2.36%</td>
<td>6.62%</td>
</tr>
<tr>
<td>Wisconsin</td>
<td>300</td>
<td>6876</td>
<td>-4.51%</td>
<td>3.55%</td>
</tr>
<tr>
<td>Wyoming</td>
<td>200</td>
<td>793</td>
<td>-8.77%</td>
<td>11.26%</td>
</tr>
</tbody>
</table>

* p<.10
** p<.05
*** p<.01

*Due to the very small sample size used to calculate Washington, DCs estimated crowd-out, its estimates should be evaluated with caution.
The primary finding is that there is considerable variation between the states’ crowd-out at specific eligibility thresholds. Because the RD design does not permit us to estimate total crowd-out, we are limited to comparing the localized effect of crowd-out near the eligibility threshold. Nineteen states use 200% of the FPL as their threshold for Medicaid eligibility and the estimated crowd-out ranges from 6.9% to 10.61% (p<.05 n=5; range of -8.77% to 16.94% for all p-values). Of the eight states that use 250% of the FPL as their threshold, only one has a significant crowd-out estimate (California at...
3.27%; range of -6.2% to 6.89% for all p-values). Of the 14 states that use 300% of the FPL as their threshold, none had significant estimates of crowd-out (ranges of -4.51% to 16.20% for all p-values). The heterogeneity of estimated crowd-out is indicative of the heterogeneous approach to implementing Medicaid programs. Though jointly funded by the Federal and State governments, Medicaid and SCHIP programs are implemented individually by the states, with different approaches to managing the Medicaid population (Kaiser Family Foundation 2010, 1) and with different approaches to limiting crowd-out (Hoag et al. 2011).

While the large sample size of the ACS allowed us to estimate crowd-out in every state, the states with smaller populations generally had larger p-values and smaller adjusted R² values as more variability in the estimates for each block of data. We generally found more modest estimates of crowd-out from states with larger populations as seen in Figure 2-4 where only the states with more than 5000 observations were used to estimate the crowd-out.
Of the 50 states and the District of Columbia, seventeen were estimated to have negative crowd-out. Of these, most had smaller sample sizes which lead to more variability and more variability in the estimates for each block of data. For example, the state with the most negative estimated crowd-out, Wyoming, only had 793 observations with which to base our estimate. Of these seventeen with negative estimated crowd-out, all of had p-values greater than 0.18. It is unlikely that Medicaid eligibility would lead to “crowding-in” of private insurance and these data do not support that proposition.
The second finding is that the local crowd-out effects tend to decrease as the eligibility threshold increases. As viewed in Figure 2-4, the trend line predicts no crowd-out at 301% of the FPL\(^9\), indicating that there is no expected crowd-out with eligibility thresholds above this point. Similarly, if we only chart the states with significant levels of crowd-out (p<.10), then the expected point at which there is no crowd-out is 300%. Previous work has identified a decrease in crowd-out as income increases (Koch 2010, 11) and our work confirms this effect at the state eligibility threshold. This finding calls into question the common perception that crowd-out is higher among children with higher family incomes (Winfree and D’Angelo 2007). It is unclear if the type of crowd-out (switching from private insurance to public insurance or remaining on public insurance when private insurance becomes available) changes as income levels increase.

This does not support a finding that overall crowd-out is less in states with higher eligibility thresholds; the effect is local and only suggests that higher eligibility thresholds are associated with lower crowd-out at that income level. This finding indicates that proportionately fewer high income children are expected to be crowded out than low-income children. For example, we estimate that, overall, approximately 3% of all children who are eligible for Medicaid and whose families make 250% of the FPL

\[\text{\ldots}\]

\(^9\) Excluding New York and New Jersey, the two states with the highest income thresholds, changes our estimate of no crowd-out to 280% of the FPL, effectively making the slope steeper.
would be crowded out while 6% of all children who are eligible for Medicaid and whose families make 200% of the FPL would be crowded out and no children would be crowded-out if their families earn 300% of the FPL. This general finding does not account for program differences, such as copays or premiums in states with higher eligibility thresholds, or income volatility, which may vary by income level. These may reduce the comparability of these estimates.

**Discussion and Policy Implications**

The extreme variability in crowd-out estimates indicates substantive differences between states. While previous work has adjusted for state-specific differences (Cutler and Gruber 1996, 398) to estimate national crowd-out, this extreme heterogeneity indicates a need to evaluate total crowd-out on a state by state basis. It is not sufficient to simply adjust for state residency or use other statistical methods to estimate state-level effects because crowd-out is a function of the state of residency. The disparate state programs and their individual effect on crowd-out must be estimated and then national models can be developed based on these state-specific estimates.

The differences between how states implement their Medicaid programs is the variable that should be of interest to policy makers as crowd-out is a function of how these programs are implemented. Acting within the concept of states acting as laboratories of democracy, states that have successfully reduced the number of uninsured while limiting crowd-out should be evaluated and emulated by other states.

One reason that children may not move to public coverage is due to a preference for the private coverage. While state plans may be cheaper for some eligible children,
there are clear reasons that families might choose the private option such as, for example, a preference for providers that do not accept Medicaid or a perceived stigma associated with Medicaid (Stuber and Kronebusch 2004). When deciding which insurance plan to enroll children in more than just finances factor in, particularly as it relates to access to preferred providers and the effort required to disenroll from employer-sponsored coverage and to enroll in a public plan.

There may also be some state policies that effectively discourage crowd-out, such as by adding premiums and co-pays at higher income levels (Heberlein et al. 2011). There was no difference in crowd-out for states with Medicaid/CHIP premiums versus states without premiums (p=.49), but states with copays had significantly lower average crowd-out rates (2.3% < 5.9%, p=.019). Copays, then, may be a deterrent to dropping private insurance in exchange for public. Identifying which factors drive decisions to drop private insurance will aid policymakers’ decisions about structuring public insurance plans going forward.

Another policy issue involves the decreasing local crowd-out effect as the eligibility threshold increases. Critics of Medicaid expansion have claimed that higher eligibility thresholds will cause greater percentages of children to be crowded out at the higher eligibility levels (Winfree and D’Angelo 2007). This directly counters that assumption, implying that wealthier children are less likely to be crowded-out than ones from poorer families. This may be due to the premiums charged for wealthier families or because Medicaid is not a perfect substitute for private insurance.
From a policy perspective, concerns of families with higher incomes dropping private insurance and moving to public plans appear to be unfounded. Our results indicate that the majority of children who obtain Medicaid and come from families with higher incomes do so because they otherwise lacked private insurance.

From a state and national perspective, less concern of crowding-out at higher income levels should lead to a reevaluation of the eligibility threshold for Medicaid and SCHIP programs. States should compare the risk of crowd-out at higher eligibility levels to the transaction-costs of assuring the continued eligibility of children which may reduce take-up of public insurance by children that otherwise would be uninsured (See also Dick et al. 2002).

Finally, the relatively small crowd-out at all income levels suggests that the discourse on children’s health insurance programs should shift away from crowd-out towards the merits of public programs. Arguments for and against public children’s health insurance programs should be based on benefits of publicly insuring children that otherwise would be uninsured, not on whether previously insured children drop private insurance and move to the public’s payrolls.

From a research perspective, the RD approach should be viewed as one more tool to help triangulate estimates of crowd-out. It is limited to local estimates of crowd-out effects, but is relatively simple to calculate and can be combined with other approaches for a more robust estimate of total crowd-out. There is significant work still to be done to accurately calculate levels of crowd-out at the state level, but this does provide one more arrow in the quiver of researchers.
Future Work

There is no consensus as to the overall effect of public insurance plans crowding out private plans and further estimates of crowd-out should be pursued. While this work provides localized estimates of crowd-out, it does not provide estimates of total crowd-out within a state. Therefore, state-level estimates of total crowd-out still need to be determined. National estimates of crowd-out can then be calculated as the sum of the state-level estimates.

Along with estimating state-specific crowd-out levels, there is a need to determine why there are such differences in crowd-out between states. There is scant evidence as to which specific policies lead to increases or decreases in crowd-out and such research will be pivotal for policymakers’ decisions. Also, factors such as state economies, demand for insurance and the political environment within a state may affect the levels of crowd-out. These factors need to be identified and further explored. Additionally, different state approaches to Medicaid and CHIP should be evaluated on their effect on uninsured children taking up Medicaid.

To further understand the dynamics of the effect of state policies and external factors on crowd-out is to estimate how crowd-out changes over time. Repeated measures of crowd-out within a state, coupled with a close, qualitative analysis of state policies and external economic and political factors, may shed light into what drives crowd-out.

A second area of work is to learn why children who are eligible for public insurance, particularly at higher incomes, do not leave their private plans. There are
many potential reasons for lower levels of crowd-out at higher income levels and we suggest studying the relative desirability of private insurance at different income levels and the effect of premiums and copays on demand for public insurance.
Chapter References


Hoag, Sheila, Mary Harrington, Cara Orfield, Victoria Peebles, Kimberly Smith, Adam Swinburn, Matthew Hodges, Kenneth Finegold, Sean Orzol, and Wilma


———. 2011. “Poverty Thresholds for 2010 by Size of Family and Number of Related Children Under 18 Years.”


Chapter 3 – Children’s Insurance Coverage and Crowd-Out through the Recession: Lessons from Ohio

Note: Eric Seiber is co-author of this chapter.

Abstract

**Background.** The “Great Recession” that occurred from December 2007 through mid-2009 was associated with severe economic hardships for people throughout the United States with many losing jobs and their accompanying employer-sponsored health insurance. Children, who often depended on their parents’ employment for insurance, also lost insurance benefits and some became uninsured while others moved to the public insurance programs. It is unknown how many of these newly insured children were crowded-out of private insurance.

**Objective.** Evaluate how children’s insurance coverage changed over the course of the recession and estimate how crowd-out levels varied in the state of Ohio.

**Findings.** From the period of 2004 through 2012, the uninsurance rates for children in Ohio remained stable with approximately 5% of children lacking health insurance. Private insurance rates dropped from 67% of all children to 55% and public insurance grew from 28% to 40%. Across all income levels below approximately 400% of the FPL, fewer children had private insurance, and there was a population shift towards more children living in lower-income households. Crowd-out levels near the Medicaid eligibility threshold slightly dropped from 8% to 6%, indicating that children were not being crowded-out of private insurance.

**Conclusion.** Children, particularly those with incomes lower than 400% of the FPL, are increasingly moving from private to public insurance, but they are not being crowded-out of the private plans.
Background

The role of government in financing healthcare is hotly debated, but the current importance in establishing state budgets is undeniable as Medicaid now accounts for almost a quarter of total state spending (National Association of State Budget Officers 2012). Medicaid and the Children’s Health Insurance Program are also increasingly important for individuals as more than 62 million people in the United States are enrolled (Kaiser Family Foundation 2013). As legislatures grapple with how to fund current programs and potentially expand Medicaid enrollment under the Patient Protection and Affordable Care Act, legislatures frequently ask how many beneficiaries are “crowded-out” of private insurance and move to the public’s payroll arises. Estimating the impact of crowd-out will guide these policy makers as they design programs in their own state, particularly as they consider eligibility restrictions to lower crowd-out.

Just as enrollment in public programs is not static (Kaiser Commission on Medicaid Facts 2013), there is little reason to suggest that crowd-out levels would also be static. Thus, a comprehensive analysis of crowd-out should seek to identify how crowd-out changes over time. This study adds to extant literature by evaluating crowd-out levels over time. As Ohio, along with the rest of the country, suffered a severe recession, we evaluate how crowd-out levels changed through the course of the recession.
Crowd-out explained

Crowd-out refers to individuals who are insured by a public program and would, but for the existence of the public program, have private insurance. There are two general situations when crowd-out could occur. The first type, called substitution, is when someone drops private insurance that they could have maintained and moves to the public plan (Davidson, Blewett, and Call 2004). The second type, which we refer to as continuation, occurs when an individual is enrolled in a public plan and then, when the opportunity to move to private insurance becomes available, fails to make that transition.

Substitution crowd-out is estimated at the time of enrollment, but it is not sufficient to simply measure how many individuals were previously insured privately (E. Seiber and Sahr 2010). For example, a privately insured person who loses their job and moves to public insurance no longer had the option of maintaining it. Continuation crowd-out is more difficult to measure because it relies on tracking newly available insurance opportunities. As such, most crowd-out literature focuses on substitution (Gruber and Simon 2008). Continuation crowd-out is important, though, as insurance options change over time and environmental factors, such as the general state of the economy, may affect crowd-out over time.

Data

Our analysis is based on data from the Ohio Medicaid Assessment Survey, which prior to the 2012 iteration was called the Ohio Family Health Survey. This survey is
intended to examine Ohioan’s use of health services, insurance status and identify health status and other determinants of population health (“The Ohio Medicaid Assessment Survey” 2013). This periodic survey, performed in 1998, 2004, 2008, 2010 and 2012, is a computer-assisted telephone survey that uses telephone lists and random digit dialing to interview random Ohio residents, stratified at the county level. Beginning in 2008, the survey added a second sampling frame to use random digit dialing to call cellular phones, resulting in a single stratum of cellular phone surveys for the state. This dual-frame methodology (landline and cellular phone) insures a more complete survey of Ohio’s population, including groups such as younger adults and lower-income families that are more likely to only have a cellular phone and no landline. Data validation occurred by recalling a portion of surveys to confirm important variables. Survey collection was provided by outside vendors which varied by year.

One challenge with this survey involves the sample size of respondents. During the recession, funding for projects such as this one was cut throughout Ohio. Due to financial considerations, the sample size was lowered. The effect is that information was available on significantly fewer children in the 2010 and 2012, with 2010 having the fewest. This leads to decreased precision and increased standard errors for these years’ estimates.
Methods

The variable of interest is type of insurance that children have. We break down insurance status into three categories: public, private and uninsured. Public insurance includes Medicaid, Medicare and both Medicaid and Medicare. Private insurance is all other types of health insurance such as job-based coverage and directly purchased insurance. Among children, the most common reason for gaining public coverage is because household income is below a certain threshold, but individuals at higher incomes may also gain public coverage because of a disability or because they have a certain disease (chronic renal disease requiring a transplant or dialysis, or amyotrophic lateral sclerosis) (United States Social Security Administration 2012).

Our approach to estimating crowd-out is to apply a regression discontinuity with the discontinuity occurring at the eligibility threshold as has been done previously to estimate levels of crowd-out (De La Mata 2012; Card and Shore-Sheppard 2004; Koch 2010). The general approach is to estimate the percent of children who have private insurance at specific income levels (we used blocks of 10% of the FPL; i.e., household income from 0-10% of FPL, 10-20% of FPL, etc.) and then regress the percent of private insurance on either side of the eligibility threshold for Medicaid. The estimated crowd-out is the percent of children near the eligibility threshold that have private insurance and are ineligible for Medicaid minus the percent of children near the eligibility threshold who are eligible for Medicaid. Figure 3-1 shows a graphical representation of this with
children left of the vertical line being eligible for Medicaid and those to the right being ineligible. The difference between the intersections of the two regressed lines represent the estimated crowd-out. The model for our estimates is as follows:

Equation 3-1:

\[ Y = \beta_0 + \beta_1 \text{ (Poverty Level)} + \beta_2 \text{ (Ineligible)} + \beta_3 \text{ (Poverty Level*Ineligible)} \]

The dependent variable, \( Y \), is the estimated percent of children at any Poverty_Level with private insurance. By centering the data around the eligibility threshold, \( \beta_0 \) is the estimated percent of children at the eligibility threshold who have private insurance and are eligible for Medicaid while \( \beta_0 + \beta_2 \) is the estimated percent of children at the eligibility threshold who have private insurance and are not eligible (Ineligible) for Medicaid. \( \beta_2 \), then, is the estimated crowd-out at the eligibility threshold.

This analytic approach is suited for this analysis because it allows for estimates of crowd-out independent of any expansion of the Medicaid program which will capture both substitution and continuation crowd-out. Additionally, as long as the individuals on either side of the threshold is approximately random, the results of this can be treated as a random experiment (Lee and Lemieux 2010). A potentially endogenous threat would be if people adjust their incomes to qualify for Medicaid for their children. If this were to happen, we would see a population shift from those ineligible for Medicaid to those
eligible for Medicaid (McCrary 2008). By looking at the weighted total population and using 5% block sizes of the FPL, we found no evidence of a population shift for any of the years of the survey (at p<.20).

A significant weakness of the regression discontinuity is the limitation of its finding. Because the regression is around the eligibility threshold, the estimated crowd-out is localized to this population. Therefore, we are able to estimate the percent of children who are crowded-out near 200% of the FPL, but we are unable to estimate the total percent of children who are crowded-out because it is unlikely that crowd-out levels are the same at every income level.

Congress created the Children’s Health Insurance Program (CHIP) (“Originally Called the State Children’s Health Insurance Program (SCHIP).”) in 1997 which provided additional federal funding for states to provide insurance for children in families with income levels that disqualified them from Medicaid coverage, but with incomes too low to purchase private insurance. States were given the option of expanding their Medicaid coverage or creating a separate CHIP program; Ohio elected to expand their Medicaid program to insure children whose families earned up to 150% of the federal poverty level (FPL) in 1998 and up to 200% of the FPL in 2000 (Ohio Department of Job and Family Services 2013). Since that expansion, Ohio has made no significant changes to children’s eligibility requirements. This study focuses on crowd-out levels independent of expansions and thus excludes the 1998 survey data.
All population estimates were performed in Stata version 12.1 using the \textit{svy} command which accounts for the complex sampling design of the surveys. The primary code used to generate these estimates is available in Appendix E.

\textbf{Findings}

We found that from 2004 to 2012 proportionately more Ohio children became enrolled in public insurance at income levels below 400\% of the FPL and more children lived in families with lower incomes. We found that crowd-out near the eligibility threshold was relatively minor (accounting for 5.5-10\% of children) and varied by year.

Our estimates for the percent of total Ohio children with various types of insurance coverage is shown in Table 3-1. Additionally, Table 3-1 includes our estimates for the percent of children crowded-out of private insurance whose household income is near the 200\% eligibility threshold for Medicaid and the total rate of private insurance for children on both sides of that eligibility threshold.
<table>
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<tbody>
<tr>
<td><strong>Number of children in study with insurance information</strong></td>
<td>15,447</td>
<td>13,443</td>
<td>2,002</td>
<td>5,515</td>
</tr>
<tr>
<td><strong>Weighted number of children</strong></td>
<td>2,899,134</td>
<td>2,754,928</td>
<td>2,751,434</td>
<td>2,898,984</td>
</tr>
<tr>
<td><strong>Percent with private insurance [95% CI]</strong></td>
<td>66.8% [65.8%,67.8%]</td>
<td>62.5% [61.3%,63.6%]</td>
<td>57.7% [54.9%,60.4%]</td>
<td>54.8% [53.1%,56.4%]</td>
</tr>
<tr>
<td><strong>Percent with public insurance [95% CI]</strong></td>
<td>27.8% [26.9%,28.8%]</td>
<td>33.5% [32.4%,34.6%]</td>
<td>37.7% [35.0%,40.5%]</td>
<td>40.4% [38.8%,42.1%]</td>
</tr>
<tr>
<td><strong>Percent uninsured [95% CI]</strong></td>
<td>5.4% [4.9%,5.8%]</td>
<td>4.0% [3.6%,4.5%]</td>
<td>4.6% [3.4%,5.8%]</td>
<td>4.8% [4.2%,5.5%]</td>
</tr>
<tr>
<td><strong>Percent of children with Private Insurance immediately below 200% of FPL (from model)</strong></td>
<td>74.15%</td>
<td>65.02%</td>
<td>68.12%</td>
<td>62.29%</td>
</tr>
<tr>
<td><strong>Percent of children with Private Insurance immediately above 200% of FPL (from model)</strong></td>
<td>82.22%</td>
<td>75.05%</td>
<td>73.76%</td>
<td>67.84%</td>
</tr>
<tr>
<td><strong>Percent of Children Estimated to be Crowded-Out near 200% FPL</strong></td>
<td>8.07% (p=.018)</td>
<td>10.03% (p=.0075)</td>
<td>5.64% (p=.303)</td>
<td>5.55% (p=.091)</td>
</tr>
</tbody>
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Table 3-1: Ohio Children’s Insurance Rates and Estimated Crowd-Out, 2004-2012
By regressing the frequency of types of insurance over time, we were able to estimate the yearly change in insurance rates. Children had significantly lower rates of private insurance (p<.001) and higher rates of public insurance (p<.0001). The rate of uninsured children did not significantly change (p=.18). Thus, children were not significantly more likely to be uninsured, but they were more likely to have public health insurance.

Movement of children from private insurance to public insurance, though, only tells part of the story. A portion of the decrease can be accounted for by examining the income level of children over this time period. Figure 3-1 contains population density charts of Ohio children as a function of household income for each survey year. There is a noticeable shift in the population density of children towards the left (lower incomes), indicating that more children live in households with lower incomes. After adjusting for this shift in population towards lower incomes (by adding an individual income variable), there is still an average yearly decrease in private insurance rates among Ohio children of 1.3% (p<.0001) and an increase in private insurance of 1.4% (p<.0001). There is also a very small decrease in rates of uninsurance of 0.1% (p=.034).
This shift of children from private to public plans is not uniform across all household income levels. Figure 3-2 contains area charts of the percent of children with different types of insurance as a function of their household income for each of the survey years. Above approximately 400% of the FPL, there is little change in the rates of insurance types. Below 400% of the FPL, though, the area that represents children with
public insurance is expanding over time, indicating that each of the FPLs below 400% have relatively fewer privately insured children at the end of the study period.

Figure 3-2: Children’s Insurance Coverage Status by Household Income, 2004-2012

We also estimated the percent of children whose household incomes were near the eligibility threshold who were crowded out of private insurance by public plans for each
of the years. Table 3-1 contains the numeric estimates and Figure 3-3 has a graphical representation of the crowd-out estimates. Since crowd-out estimates rely on the percent of children at each income level with private insurance, with a smaller sample size we see more variability in our estimates. From 2004 to 2008 there was a slight increase in estimated crowd-out and then a decrease from 2008 to 2010 which held steady into 2012. Looking at the eligibility threshold for Medicaid (200% of the FPL), we also found that the rate of children with insurance dropped during this time. For those near the threshold and eligible for Medicaid, private insurance rates dropped from 74% to 62% and for those near the threshold but eligible, private insurance rates dropped from 82% to 68%. This mimics the visual changes seen in the area chart.

Continued

Figure 3-3: Estimated Crowd-Out Levels in Ohio Near 200% of the Federal Poverty Level, 2004-2012
Discussion

A positive finding is that uninsurance rates of children have not increased during the recent recession. For many policymakers and children advocates, this is important as the recession did not slow efforts to increase children’s access to insurance. From a policymaker’s perspective, though, the movement of children from public to private insurance can be seen as concerning because of the increased pressure on the state to fund the public insurance programs. Further, with the proposed expansion of Ohio’s Medicaid to 133% of the FPL for all people, including childless adults, the cost of continuing the program becomes very important.

A contributor to the increase in publicly-insured children is the decrease of children’s household income. From 2004 to 2012, the median child’s household income
has dropped from 239% of the FPL to 217%. Over the same period, the proportion of
children eligible for Medicaid based on a household income of 200% of the FPL has
increased from 36% of children to 40%. This indicates that more children are falling in
lower income brackets where there is more need for public insurance. A decrease in
household income, though, is largely a result of a stagnant economy. As the effects of
the recession dissipate, household incomes will return, and may exceed, pre-recession
values, and some children can return to private insurance.

A more concerning trend for policymakers is the rate of insurance uptake at
specific poverty levels. Children in households that earned more than 200% of the FPL
were ineligible for income-based Medicaid, indicating that insurance rates for these
children represent a theoretical “maximum” rate of private insurance, given the specific
FPL. During the 2004-2012 period, the rate of private insurance for those who are
ineligible for income-based Medicaid dropped from 84% to 74%. Children near the
eligibility threshold (just more than 200% of the FPL) dropped from 82% with private
insurance to 68%, but children earning from 400-1000% of the FPL remained constant
with 95% having private insurance. At the same income level, then, children were more
likely to be uninsured during this period. A decrease in private insurance rates at lower
income levels is not something that will right itself with a recovering economy, but is
more of a permanent trend that policymakers will have to deal with in an ongoing
manner.
Crowd-out, though, is something that it appears policymakers do not need to be overly concerned about, at least near the eligibility threshold. Not only did crowd-out decrease in absolute terms, but it decreased as a percentage of publicly insured children. While there was a small increase in crowd-out from 2004 to 2008, the increase (2%) was much less than the decrease of children on private insurance (9%). During this one time increase, then, only 22% of children who moved public insurance could have been classified as being crowded-out, which is much less than previous estimates (Gruber and Simon 2008; Cutler and Gruber 1996; Lo Sasso and Buchmueller 2004a). In subsequent years, crowd-out decreased, indicating that none of the newly publicly insured children were crowded-out.

There are many reasons that could explain the observed decrease in estimated crowd-out. One potential reason is that children who are more likely to be crowded out were also more likely to lose the possibility of private insurance. This could occur, for example, when an employer offered a plan with minimal coverage, parents may have been more likely to leave that private option for a public plan with more coverage; such a low-coverage plan may also have been one of the first plans to be dropped by an employer during the recession. A second potential reason is that the price of private insurance increased during this period to the point that households near the eligibility threshold who had previously barely been able to purchase private insurance (and thus
their children could have been crowded-out) were no longer able to purchase it, rendering it impossible for children to be crowded-out.

There is a major caveat to our crowd-out discussion and that is the limitation of our estimates to children near the eligibility threshold. There is no guarantee that children at other poverty levels will be crowded-out at the same rate. We hypothesize that there would be a similar relative trend at other eligibility levels (a relative decrease in crowd-out over time), but further work will be needed to test that empirically.

Another important consideration of this study is its generalizability to other states. While most states suffered a similar economic downturn during the years in this study, baseline uninsurance rates do vary by state and the rates of uninsured children did decrease in some states, while staying constant in Ohio (Alker, Mancini, and Heberlein 2012). The general trends, though, are likely consistent where more children have increasingly become insured by public programs, indicating that children throughout the country have moved from public to private plans (Kaiser Commission on Medicaid Facts 2013). It is unclear, though, whether this is primarily caused by more children living in households that earn less or because the percent of children with private insurance at a specific income level has decreased. Crowd-out levels, though, also very dependent on individual state policies, how individual Medicaid and CHIP programs are structured and what barriers to enrollment exist (Stuber and Bradley 2005; Stuber and Kronebusch 2004).
Conclusion

From the period of 2004 through 2012, the uninsurance rates for children in Ohio remained stable with approximately 1 out of 20 children lacking health insurance. During this same period, there was significant movement of children from private insurance to public insurance. Part of this movement can be explained by a population where more children live in households with lower incomes; across all income levels below approximately 400% of the FPL, fewer children had private insurance, independent of Medicaid eligibility. Simultaneous to this movement of children from private to public insurance, crowd-out levels near the Medicaid eligibility threshold slightly dropped, indicating that children were not being crowded-out of private insurance. Part of this can be explained by fewer children having private insurance to begin with as the rate of private insurance for those ineligible for Medicaid based on household income also dropped significantly during this period. Children, particularly those with incomes lower than 400% of the FPL, are increasingly moving from private to public insurance, but they are not being crowded-out of the public plans.
Chapter References


“Originally Called the State Children’s Health Insurance Program (SCHIP).”


Chapter 4 – Caps on Noneconomic Damages’ Effect on the Number of Paid Malpractice Claims

Abstract

Context. Tort reform with caps on noneconomic damages, such as pain and suffering, has been proposed as a way of decreasing the national cost of healthcare. Critics of this effort claim that caps will lead to the unintended consequence of reducing access to the system for those with legitimate claims because it dissuades attorneys from accepting meritorious, but less lucrative, cases. This study focuses on measuring the impact of caps on noneconomic damages on the rate of paid malpractice claims at the state level.

Methods. Changes in the rate of paid claims are estimated using an interrupted time series design which identifies changes in trends following the implementation of an intervention. Data from the National Practitioner’s Data Bank are used to create yearly trends in state malpractice claims using a linear spline model with a knot at the year that noneconomic caps were implemented to estimate the effect of the noneconomic caps. The effect of statutes of limitations are also modeled with a spline model. Finally, a difference-in-difference design matches states that instituted or significantly changed
noneconomic caps to states that did not. Subsequent rates of paid claims are then compared.

**Findings.** Of the fifteen states that implemented caps on noneconomic damages or significantly changed their caps since 2000, two had statistically significant differences in the absolute number of paid claims and six had significant changes to their trend of paid claims.

**Conclusions.** Tort reforms that address caps on noneconomic damages, though facially similar, have significantly different results when implemented in individual states. Qualitative studies of the individual state policies need to evaluate how the state policies differ and why they led to different results to direct other states and the federal government as they consider similar policies. The decrease in paid claims, independent of tort reform, may indicate that tort reform is not necessary to curb the number of paid claims, potentially because less medical malpractice is being committed.
Background

The cost of healthcare has become a significant financial burden for America (Auerbach and Kellermann 2011). Tort reform has been proposed as a means of lowering its growth (Sloan 2005; Krauthammer 2009; Eviatar 2009; Simmons 2009; Keene 2011). Proponents believe that this will lower costs in two ways: (1) by lowering malpractice premiums (Morrisey, Kilgore, and Nelson 2008) and (2) by decreasing defensive medicine (Kessler and McClellan 1996). The former concept infers that the high costs of insuring against malpractice lead to physicians transferring those costs to patients and insurers. The assumption is that if physicians have lower premiums they will also be willing to lower their prices. The latter way is based on the belief that physicians, due to a desire to not be sued, will order excessive tests, procedures and care on the off-chance that the patient has a serious illness. This is particularly believed to result in higher service utilization among high-risk specialties (such as emergency department physicians and obstetricians) (Studdert 2005) and higher rates of diagnostic imaging (Iglehart 2006).

One approach to tort reform is to place limits, or caps, on noneconomic damages. Within a tort claim there are two claims that a plaintiff may make: claims for compensatory or actual damages and claims for punitive or exemplary damages; the latter type of damages intend to punish the tortfeasor (the party that committed the tort) while the former intend to make the victim whole (Dietz et al. 2012, sec. 25). Within the
compensatory damages category there are two general types of damages: economic losses and noneconomic losses (Frumer and Friedman 2012, sec. 43.01). Economic losses include claims where a dollar value is clearly assignable such as the reasonable cost of necessary medical care (Frumer and Friedman 2012, sec. § 43.07) and the loss of income due to the injury (Frumer and Friedman 2012, sec. § 43.08). Noneconomic illnesses are more subjective and include claims for pain and mental suffering (Frumer and Friedman 2012, sec. 43.06). A common form of state tort reform addresses these noneconomic damages, usually relating to pain and suffering, and places an upper limit on the damages that can be awarded to the patient due to a malpractice claim (Thorpe 2004).

The goal of a cap on noneconomic damages is to limit the potential liability for a medical malpractice claim. The argument is that if there is a limit to the total amount of noneconomic damages, then there will be a smaller chance of extremely high awards for victims as the award will primarily be limited to objective, economic damages. The end result, according to this argument, is that overall awards will decrease which will lead to lower premiums and, thus, lower prices charged by physicians. Subsequently, if physicians have less fear of being sued, they will also change their practice habits and decrease the practice of defensive medicine. Caps, though, do not guarantee lower costs. There are often exceptions to the noneconomic caps, such as for the loss of a limb or permanent loss of function (Ohio 2005), and the actual damages may exceed any noneconomic damages.
A criticism of this approach is that caps will lead to the unintended consequence of reducing access to the system for those with legitimate claims. Critics argue such caps make lawyers less willing to take meritorious cases which will leave deserving patients – those that suffered a legitimate tort – without access to begin the legal process (Hyman 2008). In the United States, a civil tort claim may be brought by an individual without representation, but, in practice, without an attorney individuals will not successfully argue a claim (Landsman 2009; Haire, Hartley, and Lindquist 1999). Additionally, there is the issue of cost as legal cases are generally protracted, expensive affairs (Harris and Foran 2001). For an average person to gain access to the system, he or she typically must rely on attorneys who believe that the individual has a valid case and agree to represent the client and also furnish much of the cost of the trial (Helland 2003). The attorney will work on a contingency fee wherein, if the client’s claim is successful, the attorney will be paid a percentage of the final verdict. The attorney, then, must be willing to bear some amount of risk to accept a case as, if they do not win, they attorney may not only not be paid anything but may lose the expenses of litigation. If there are caps put on noneconomic damages, then some individuals, particularly those that have low economic losses (such as the elderly who are unemployed and thus do not suffer any lost earnings), will have smaller potential claims. By decreasing the maximum award for the litigation, attorneys have a smaller potential return on their investment of time and resources. Thus, the argument goes, with a smaller potential return with some cases, they will be less
likely to take on those clients, leaving potentially deserving clients without access to the legal system. The question that this study addresses is whether deserving patients are left without access to the legal system.

If deserving individuals are indeed left without access into the system, then there is an expectation that the number of malpractice claims (i.e., cases wherein a payment for malpractice is made, either as the result of a jury verdict or as the result of a pre-trial settlement) will decrease due to reforms that impose caps on noneconomic damages. This study does not seek to address whether the size of the average paid claim is affected by states putting caps on noneconomic damages nor does it address the effect on malpractice premiums and medical costs; it is limited to evaluating whether tort reforms limit access to the legal system for meritorious claims. The assumption is that claims that are paid are meritorious. While there are cases where individual claims may be paid as a result of settling nuisance lawsuits, the vast majority of paid settlements are a result of a medical error having, in fact, occurred (Studdert et al. 2006).

Previous studies evaluating tort reform have focused on the number of filed lawsuits, the size of awards and the cost of insurance (Congressional Budget Office 2004). Some studies, though, have investigated whether tort reform affects the number of paid settlements. One study, looking at data through 2005 and using a regression model evaluating six different types of reforms found that tort reform was associated with a decrease in the number of paid claims (Avraham 2007). A second study using
instrumental variables to control for policy endogeneity and evaluating tort reforms implemented between 1991 and 2001 found no effect of caps on noneconomic damages on the number of paid claims (Durrance 2010).

A challenge with measuring whether there is an effect of such tort reforms on the number of paid malpractice claims relates to the timing of the implementation of the tort reforms. While tort reforms generally have a specific day in which they were passed, signed and became effective, their effect on a specific case varies. Statutes of limitations for claims may last for several years in states which affect whether the tort reforms apply to a specific case. For example, an injury that occurred on 12/31/2005 may not be covered by a law that imposes a cap on noneconomic damages that is effective on 1/1/2006. If there is a three year statute of limitations to file a claim, then that claim may be filed until 12/31/2008 without the caps affecting it. Some states, though, will make new laws effective retrospectively, eliminating this possibility. Additionally, statutes of limitations may vary based on numerous factors including the age of the claimant (children usually have longer to file, sometimes after they turn 18) and when the adverse event was known. For example, some states do not start tolling the statute of limitations until a patient learns of alleged malpractice (such as learning of a sponge left in their body following a surgery several years after the surgery) but often do have maximum time periods following the initial action (such as the surgery) which are binding. For example, in Florida there is a two year statute of limitations to bring a claim, but the
statute does not begin to toll until the patient learns of the harm, but all claims must be brought within four years. That means that if a patient learns of the harm three years following an operation, they will only have one year to file the claim before the four year maximum is met.

Additionally, there are often constitutional challenges to tort reform bills which lead to uncertainty in whether caps will actually stand, leading some attorneys to continue to take cases and then to challenge the caps on noneconomic damages on state constitution grounds or to avoid cases while waiting for decisions about the constitutionality of the law (Avraham 2006). A third issue relates to the timing of when attorney behavior may change. For example, as soon as a bill is proposed some attorneys may be tempted to shift their practice away from medical malpractice cases in an effort to avoid the issue all together; or, if attorneys decide to leave the state, it may take several years to arrange to move elsewhere due to licensure and employment. Such issues of timing must be accounted for when estimating the effects of state tort reform.

**Significance**

Caps on noneconomic damages have been implemented by multiple states as one approach to mitigating healthcare cost growths. Of the 50 states and the District of Columbia, 27 have had caps on noneconomic damages at some point, and 24 had them in place as of 2010 (Avraham 2011) and they have been considered by others. Some states have implemented these caps multiple times after the original version of the law was
declared unconstitutional by the individual state’s Supreme Court. There have been four periods when these caps have been enacted: 1976 (2 states), 1986-1988 (13 states), 1995-1997 (6 states) and 2002-2006 (15 states), and more recently, states are considering new proposals (National Conference of State Legislatures 2012).

National tort reform with caps on noneconomic damages has been proposed as a way of decreasing costs in health care (Tumulty 2005). In March of 2012, the United States House of Representatives passed a bill imposing national caps on noneconomic damages (Gringey 2012). While the bill was never passed by the Senate, the passage of the bill by the House indicates it is a seriously considered policy. As some states have already enacted these laws, they serve as test cases to estimate the effect such a policy would have on the national level. By evaluating state-level effects of tort reform on the number of paid malpractice claims, if any, on the states, an estimate of the effect of a national policy may have on the number of malpractice claims may be created. This study seeks to gain insight into the potential effect of such a national policy and to guide policy makers as they evaluate the wisdom of such a policy.

The focus of this research will be limited to the impact of caps on noneconomic damages. This is because the proposed federal legislation’s tort reform’s primary focus is on caps and changes to joint and several liability (Gringey 2012). Joint and several liability reform is of less interest because only 11 states have not already adopted this reform and only five states did so during the past fifteen years (one of which,
Pennsylvania, had its reforms ruled unconstitutional). In this paper I evaluate states that implemented a cap on noneconomic damages where no cap existed before and states that significantly changed their economic cap (either by increasing or decreasing the cap).

**Methods**

The statistical analysis relies on two different approaches: interrupted time series and a difference-in-difference analysis. The former will be used to estimate any changes in trends of the number of paid claims while the latter will allow direct comparisons to similar states without tort reform. For all analyses, the unit of analysis is the rate of paid claims, by state, per 1,000 physicians within that state. The time period for these reforms is 2002-2006.

An interrupted time series design is used to estimate changes in trends following the implementation of some intervention (Shadish, Cook, and Campbell 2002). The general approach is to regress trends before and after the intervention and evaluate whether the slope or intercept of the regressed lines (centered at the time of the intervention) change. Such a design has been used previously to estimate the effects of policy changes such as the effect of quality reporting on mortality rates (Ryan, Nallamothu, and Dimick 2012). The approach requires the assumption of independence among yearly estimates. In this case, that requires a belief that the individual paid malpractice claims are independent of previous years’ claims. Since each instance of malpractice is unique (i.e., not driven by past instances), this assumption is met.
The challenge with using an interrupted time series to estimate such a change is the difficulty in establishing a clear time frame in which a cap on a noneconomic damage affects behavior. Due to issues with effective dates, statutes of limitations, constitutional challenges, and attorney behavior, there is not a discrete time when the reform goes into effect. To address this challenge, I evaluate states that implemented tort reform and model the time the law was passed and at the end of the statute of limitations for claims that occurred before the law was passed. The expected result, if there is any effect from the tort reform, is a gradual decrease in the number of paid claims from the time of the passage of the bill to the end of the statute of limitations and then a similar trend in the number of paid claims as there was prior to the change in the law. Alternatively, there could be a change in the intercept (a change in the absolute number of paid claims) either at the time of the bill’s passage or at the end of the statute of limitations.

A second tool to evaluate these claims relies on a difference-in-difference (DiD) design (Shadish, Cook, and Campbell 2002). In this approach, states which made a change to their caps on noneconomic damages are compared to states that did not have changes. To accomplish this, I first match states to a suitable control state by matching the intervention state (the state that changed its caps on noneconomic damages) to: (1) the state that, up to the year before the law was passed, had the most similar trend in the rate of paid claims, and (2) the state that had the most similar total number of paid claims. I
then evaluate whether there is a difference between the subsequent rates of paid claims between the state that changed its cap on noneconomic damages and the state that did not.

In other analysis of tort reform, a single reform, including caps on noneconomic damages, are evaluated in a fixed effects model (Paik et al. 2012). In these regression analyses, state effects are combined and an average effect is estimated for that particular reform (Avraham 2007). In my analysis, I do not identify an average effect from noneconomic caps, but instead focus on estimating a state-level impact of this reform. Due to state-specific factors and variations in the implementation of the tort reform, a statute that is facially similar may have different effects once put in effect, and I seek to identify these differences.

Data

The primary data set that I use to perform this study is the National Practitioner Data Bank (NPDB). Established by Congressional order, the NPDB contains information on malpractice payments made by, and adverse actions taken against, healthcare providers including physicians, dentists and other healthcare practitioners (Secretary of Health and Human Services 1989). By law, all paid malpractice claims must be reported to the NPDB within a month of the claim or settlement being paid (Secretary of Health and Human Services 1989, sec. 424). The NPDB, then, contains a census of malpractice claims paid since 1990. I use information on the number of settlements that occurred by year in individual states in this study. Since the first year’s reporting began midyear, I
use full year data beginning in 1991. Table 4-1 contains information on the total number of claims and the average, median and total malpractice payments from 1991-2010, measured in nominal dollars. Figure 4-1 shows the number of claims and average claims broken down by states that had a cap on noneconomic damages in place at some point and those that never had a cap.

<table>
<thead>
<tr>
<th>Year</th>
<th>Paid Claims</th>
<th>Average Claim</th>
<th>Median Claim</th>
<th>Total Payments</th>
</tr>
</thead>
<tbody>
<tr>
<td>1991</td>
<td>17921</td>
<td>$129,335</td>
<td>$42,500</td>
<td>$2,317,808,951</td>
</tr>
<tr>
<td>1992</td>
<td>19560</td>
<td>$140,848</td>
<td>$47,500</td>
<td>$2,754,988,836</td>
</tr>
<tr>
<td>1993</td>
<td>19325</td>
<td>$148,911</td>
<td>$47,500</td>
<td>$2,877,708,940</td>
</tr>
<tr>
<td>1994</td>
<td>19460</td>
<td>$153,841</td>
<td>$57,500</td>
<td>$2,993,745,860</td>
</tr>
<tr>
<td>1995</td>
<td>17754</td>
<td>$164,248</td>
<td>$65,000</td>
<td>$2,916,055,441</td>
</tr>
<tr>
<td>1996</td>
<td>19011</td>
<td>$179,278</td>
<td>$72,500</td>
<td>$3,408,246,454</td>
</tr>
<tr>
<td>1997</td>
<td>18004</td>
<td>$184,729</td>
<td>$72,500</td>
<td>$3,325,853,714</td>
</tr>
<tr>
<td>1998</td>
<td>17359</td>
<td>$189,901</td>
<td>$82,500</td>
<td>$3,296,487,987</td>
</tr>
<tr>
<td>1999</td>
<td>18563</td>
<td>$194,659</td>
<td>$87,500</td>
<td>$3,613,449,448</td>
</tr>
<tr>
<td>2000</td>
<td>19031</td>
<td>$215,481</td>
<td>$97,500</td>
<td>$4,100,826,523</td>
</tr>
<tr>
<td>2001</td>
<td>20160</td>
<td>$238,312</td>
<td>$97,500</td>
<td>$4,804,359,840</td>
</tr>
<tr>
<td>2002</td>
<td>18682</td>
<td>$239,388</td>
<td>$97,500</td>
<td>$4,472,248,484</td>
</tr>
<tr>
<td>2003</td>
<td>18706</td>
<td>$258,274</td>
<td>$125,000</td>
<td>$4,831,265,962</td>
</tr>
<tr>
<td>2004</td>
<td>17424</td>
<td>$263,307</td>
<td>$125,000</td>
<td>$4,587,866,395</td>
</tr>
<tr>
<td>2005</td>
<td>16984</td>
<td>$261,016</td>
<td>$135,000</td>
<td>$4,433,092,347</td>
</tr>
<tr>
<td>2006</td>
<td>15562</td>
<td>$272,437</td>
<td>$135,000</td>
<td>$4,239,659,925</td>
</tr>
<tr>
<td>2007</td>
<td>14236</td>
<td>$284,918</td>
<td>$145,000</td>
<td>$4,056,098,342</td>
</tr>
<tr>
<td>2008</td>
<td>13822</td>
<td>$291,472</td>
<td>$145,000</td>
<td>$4,028,719,073</td>
</tr>
<tr>
<td>2009</td>
<td>13575</td>
<td>$288,277</td>
<td>$145,000</td>
<td>$3,913,353,488</td>
</tr>
<tr>
<td>2010</td>
<td>12937</td>
<td>$285,601</td>
<td>$145,000</td>
<td>$3,694,813,669</td>
</tr>
</tbody>
</table>

Table 4-1: Number and Size of Malpractice Payments, 1991-2010
Figure 4-1: Malpractice Trends, 1991-2010
To obtain the rate of claims per 1,000 physicians I must estimate the number of physicians in each state. The Area Resource File (ARF) is a database compiled by the Health Resources and Services Administration that contains information on healthcare resources, including physicians, at the county level which I use to calculate state-level values (Health Resources and Services Administration 2012). While some previous research has used the number of claims divided by the population of the state (Durrance 2010), I use the number of claims per 1,000 physicians following others (Avraham 2007). This approach better reflects the volume of care delivered in a state as patients may travel across state lines to receive care but physicians rarely travel across state lines to provide care due to state-level medical licensing.

The total number of physicians is calculated by combining the number of non-federal, practicing MDs (allopathic physicians) and DOs (osteopathic physicians). The ARF does not contain estimates for all years of each type of physician, so when a year of data is missing I approximate the number of physicians by averaging the reported years’ values (i.e., if 2009 MDs is missing, I average 2008 MDs and 2010 MDs).

For information on the different reforms that have been implemented, I use the Database of State Tort Law Reforms, 4th Edition (DSTLR4) (Avraham 2011). This database is a compendium of state tort reform laws including information on when they were passed, modified and whether they were found to be unconstitutional. An accompanying analytic file contains information on the types of reforms in effect in states.
by year and the level of the caps. The analytic files assigns a year as having a tort reform in place if the reform was in effect for a majority of the year; for example, a law effective June 30, 2004 is coded as “in effect” for 2004, but a law with an effective date of July 1, 2004 is not coded as being in effect until 2005. The DSTL4 is current through 2010, which represents the final year of the study.

I focus on states that implemented or significantly changed caps on noneconomic damages after 2000. Table 4-2 includes general information on these states. Note that Utah and Wisconsin significantly raised their caps during this period and that Illinois’ caps were only in place for two years.
<table>
<thead>
<tr>
<th>State</th>
<th>Years in Effect</th>
<th>Cap ($1000s)</th>
<th>Previous Cap ($1000s)</th>
<th>Tort Statute of Limitations (Years)</th>
<th>Statute</th>
</tr>
</thead>
<tbody>
<tr>
<td>Alaska</td>
<td>2006-2010</td>
<td>250</td>
<td>400</td>
<td>2</td>
<td>Alaska Stat. § 09.55.549</td>
</tr>
<tr>
<td>Florida</td>
<td>2003-2010</td>
<td>500</td>
<td>No cap</td>
<td>2</td>
<td>Fla. Stat. § 766.118</td>
</tr>
<tr>
<td>Idaho</td>
<td>2004-2010</td>
<td>250-290</td>
<td>650</td>
<td>2</td>
<td>Id. Code § 6-1603</td>
</tr>
<tr>
<td>Illinois</td>
<td>2006-2007</td>
<td>500</td>
<td>No cap</td>
<td>2</td>
<td>735 Ill. Compiled Stat. 5/2-1706.5</td>
</tr>
<tr>
<td>Mississippi</td>
<td>2003-2010</td>
<td>500</td>
<td>No cap</td>
<td>2</td>
<td>Miss. Code § 11-1-60</td>
</tr>
<tr>
<td>Missouri</td>
<td>2006-2010</td>
<td>350</td>
<td>580</td>
<td>2</td>
<td>Mo. Stat. § 538.210</td>
</tr>
<tr>
<td>Nevada</td>
<td>2003-2010</td>
<td>350</td>
<td>No cap</td>
<td>1</td>
<td>Nv. Stat. 41A.035</td>
</tr>
<tr>
<td>Ohio</td>
<td>2003-2010</td>
<td>500</td>
<td>No cap</td>
<td>1</td>
<td>Ohio Rev. Code § 2315.18</td>
</tr>
<tr>
<td>Oklahoma</td>
<td>2004-2010</td>
<td>300</td>
<td>No cap</td>
<td>2</td>
<td>23 Okla. Stat. § 61.2</td>
</tr>
</tbody>
</table>

Continued

Table 4-2: States that Modified Caps on Noneconomic Damages, 2000-2010
Table 4-2 continued

<table>
<thead>
<tr>
<th>State</th>
<th>Years in Effect</th>
<th>Cap (1000s)</th>
<th>Previous Cap (1000s)</th>
<th>Tort Statute of Limitations (Years)</th>
<th>Statute</th>
</tr>
</thead>
<tbody>
<tr>
<td>South Carolina</td>
<td>2006-2010</td>
<td>350</td>
<td>No cap</td>
<td>3</td>
<td>S.C. Code 15-32-220</td>
</tr>
<tr>
<td>Texas</td>
<td>2004-2010</td>
<td>250</td>
<td>No cap</td>
<td>2</td>
<td>Tex. Civ. Prac. &amp; Rem. 74.301</td>
</tr>
<tr>
<td>Utah*</td>
<td>2002-2010</td>
<td>400-440</td>
<td>250</td>
<td>2</td>
<td>Ut. Code 1953 § 78B-3-410</td>
</tr>
<tr>
<td>West Virginia</td>
<td>2003-2010</td>
<td>250</td>
<td>1000</td>
<td>2</td>
<td>W. Va. Code § 55-7B-8</td>
</tr>
</tbody>
</table>

* Raised Cap

Statistical Models

The general statistical model for this analysis is based on changes in temporal trends in the rate of paid malpractice claims using a linear spline model with a knot at the year that noneconomic caps were enacted. Data run from 1991-2010 and represent 20 points in time. Each state is analyzed separately using the following model:
Equation 4-1:

\[ Y_{\text{year}} = \beta_0 + \beta_1 (\text{year}) + \beta_2 (\text{year} - \text{year}_\text{enacted})_{(\text{year}_\text{enacted}-0)} + \beta_3 \]

\text{(caps}_{\text{enacted})}

\( Y \) is the estimated number of malpractice payments per 1,000 physicians at time \( \text{year} \). \( \text{Year}_\text{enacted} \) is the year that the noneconomic caps were enacted (the knot). \( \text{Caps}_{\text{enacted}} \) is a dummy variable that equals 1 if the noneconomic caps were enacted in the state during time \( \text{year} \) (e.g., \( \text{caps}_{\text{enacted}} =1 \) if \( \text{year}_\text{enacted}>0 \)). \( \beta_0 \) represents the baseline rate of claims in 1991 (the first complete year of the data). \( \beta_1 \) is the slope of the trend of paid claims prior to the change in the noneconomic caps policy (either implementing a new cap or changing an existing cap). \( \beta_2 \) is the change in slope of the trend line following implementation of the noneconomic caps (following the knot), so \( \beta_1 + \beta_2 \) is the slope of trend line following implementation. \( \beta_3 \) is the change in the intercept at the time of implantation (the knot). If \( \beta_2 \) is significant, then there is evidence that following the implementation of noneconomic caps the trend in the rate of paid claims changed. If \( \beta_3 \) is significant, there is evidence that the absolute rate of paid claims changed at the implementation of noneconomic caps. Either a change in slope or intercept is indicative of a positive effect from the change in baseline (Shadish, Cook, and Campbell 2002).

To evaluate the effect of the statute of limitations, the model is expanded to:
Equation 4-2:

\[ Y_{\text{year}} = \beta_0 + \beta_1 (\text{year}) + \beta_2 (\text{year} - \text{year}_{\text{enacted}})(\text{year} - \text{year}_{\text{enacted}} > 0) + \beta_3 (\text{caps}_{\text{enacted}}) + \beta_4 (\text{year} - \text{year}_{\text{statute_limitations_end}})(\text{year} - \text{year}_{\text{statute_limitations_end}} > 0) + \beta_5 (\text{statute_limitations}_{\text{ended}}) \]

The effect of this model is to add a second knot at the time that the statute of limitations ended. \( Y_{\text{year}_{\text{statute_limitations_end}}} \) represents the year the statutes of limitations end and \( \text{statute_limitations}_{\text{ended}} \) is a dummy variable that is 1 for each year once the statute of limitations have been reached. \( \beta_4 \) is the change in the slope following the end of the statute of limitations and \( \beta_5 \) is the change in the intercept at that point. \( \beta_1 + \beta_2 + \beta_4 \) equals the slope following the end of the statute of limitations. If \( \beta_4 \) is significant, then there is evidence that following the end of the statute of limitations there was a change in the trend of the rate of paid claims. If \( \beta_5 \) is significant, there is evidence that the absolute rate of paid claims changed after the end of the statute of limitations.

To evaluate the effects of noneconomic caps using a DiD approach, I first match states that passed tort reform to control states that did not enact tort reform during this period (they may have implemented noneconomic caps prior to 1991, but did not do so during the period of the study). To match I regress all states up to the year immediately prior to the year that the state in question implemented noneconomic caps (such as regressing all states up to 2002 to compare to Florida, whose noneconomic caps went into effect in 2003). I then match the state to the closest control states based on the slope (the
temporal trend of the rate of paid claims) and the absolute rate of paid claims, meaning each state has two controls. Once each state is matched, I regress the state that implemented the noneconomic caps (the intervention state) and the states that did not (the control states) using Equation 1 and evaluating the control states as if they implemented noneconomic caps at the same time as the intervention state. I then compare the change in slope ($\beta_2$) between the intervention state and the trend control state and change in intercept ($\beta_3$) between the intervention state and the absolute rate control state.

All analysis was done using Stata version 12.1 and used user-written commands bigtab and estout (StataCorp 2012a; Bern 2003; Jann 2009). The primary code used to generate these results is available in Appendix G.

Limitations

A difficulty with estimating the effects of policies is the timing of these policies arises from not knowing when a policy may have affected individual behavior. As described in my methods, I model the end of the statutes of limitations to see if they affect estimates of the effects of the policy change, but I am unable to access individuals or person-specific information.

A second limitation relates to concurrent changes that could be causing a perceived change, independent of the policy change. Commonly, when one tort reform is passed, others are passed simultaneously. The challenge, then, is that the actual causal agent may not be the specific tort reform that I am investigating. To address this
challenge, in addition to the interrupted time series, which is able to account for existing
trends over time, I perform the matched pair difference-in-difference analysis. This
allows me to compare whether any similar change occurred in the control state. If there
is no significant change from the control state, there is evidence that noneconomic caps
did not lead to a change in the rate of paid claims. If there is a difference from the
control states and there is a significant temporal or absolute change in the rate of paid
claims, there is evidence of an association between noneconomic caps and an effect on
paid claims, but there is the possibility that another aspect of the state’s tort reform that
was the causal factor. A positive correlation, though, is evidence that potentially
deserving claimants did lose access to the legal system because the rate of paid claims
decreased differently than past trends would predict.

A third limitation arises from the dataset. In an effort to make identifying a
specific person reported on the NPDB difficult, data is not presented in the most granular
form possible. In particular, all data is based on full years, thus only 20 data points for
each state are available for analysis. This limits precision when estimating any effects of
a policy, particularly relating to any delayed implementation effects such as those that
may arise as a result of the various statutes of limitations.

**Results**

Since the early 1990s, the rate of paid medical malpractice settlements has been
dropping in the United States. Simultaneously, many states have implemented caps on
noneconomic damages. Figure 4-2 charts the total number of paid claims per 1,000 physicians in the United States, with the bar chart representing the number of states that had enacted caps on noneconomic damages during this period. The rate of paid claims went from a high of 32.2 per 1,000 physicians in 1992 to a low of 14.7 per 1,000 physicians in 2010 with an annual decrease of .90 settlements per 1000 physicians (p<.001). The number of states with caps on noneconomic damages also increased at an annual rate of .64 states per year (p<.001). Also, the total number of states with caps on noneconomic damages and the rate of paid claims is highly correlated (-.931, p<.001).

Figure 4-2: Paid Malpractice Claims per 1,000 Physicians in the United States
The strong correlation between caps on noneconomic damages and paid claims suggests that the caps may lead to a decrease in the rate of paid claims. When evaluating individual states, though, it is important to consider the timing of the caps in relation to the decrease in the rate of paid claims. Table 4-3 contains the estimated effect of the caps on noneconomic damages on the number of paid claims, calculated by state. Figure 4-3 contains the graphical representation of this effect for each of these states. Graphs for all states are shown in Appendix F. Only two states saw a significant change in the total number of paid claims (the intercept) while six states saw a significant change in the slope (the trend change). Wisconsin and Utah, which greatly raised their caps, did not see a significant increase in the rate of paid claims.
<table>
<thead>
<tr>
<th>State</th>
<th>Years in Effect</th>
<th>Cap ($1000s)</th>
<th>Previous Cap ($1000s)</th>
<th>Change in Intercept (Absolute Change)</th>
<th>Change in Slope (Trend Change)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Alaska</td>
<td>2006-2010</td>
<td>250</td>
<td>400</td>
<td>-0.623</td>
<td>-0.598</td>
</tr>
<tr>
<td>Florida</td>
<td>2003-2010</td>
<td>500</td>
<td>No cap</td>
<td>-2.849</td>
<td>-2.425***</td>
</tr>
<tr>
<td>Georgia</td>
<td>2005-2010</td>
<td>350</td>
<td>No cap</td>
<td>-2.714</td>
<td>-1.385**</td>
</tr>
<tr>
<td>Idaho</td>
<td>2004-2010</td>
<td>250-290</td>
<td>650</td>
<td>2.986</td>
<td>-0.978*</td>
</tr>
<tr>
<td>Illinois</td>
<td>2006-2007</td>
<td>500</td>
<td>No cap</td>
<td>0.874</td>
<td>0.479</td>
</tr>
<tr>
<td>Mississippi</td>
<td>2003-2010</td>
<td>500</td>
<td>No cap</td>
<td>-8.557*</td>
<td>-1.206</td>
</tr>
<tr>
<td>Missouri</td>
<td>2006-2010</td>
<td>350</td>
<td>580</td>
<td>2.643</td>
<td>-0.724</td>
</tr>
<tr>
<td>Nevada</td>
<td>2003-2010</td>
<td>350</td>
<td>No cap</td>
<td>3.685</td>
<td>-2.215*</td>
</tr>
<tr>
<td>Ohio</td>
<td>2003-2010</td>
<td>500</td>
<td>No cap</td>
<td>-4.402</td>
<td>-1.046</td>
</tr>
<tr>
<td>Oklahoma</td>
<td>2004-2010</td>
<td>300</td>
<td>No cap</td>
<td>6.382</td>
<td>-1.129</td>
</tr>
<tr>
<td>South Carolina</td>
<td>2006-2010</td>
<td>350</td>
<td>No cap</td>
<td>-1.466</td>
<td>-3.424***</td>
</tr>
<tr>
<td>Texas</td>
<td>2004-2010</td>
<td>250</td>
<td>No cap</td>
<td>0.182</td>
<td>-1.681**</td>
</tr>
<tr>
<td>Utah</td>
<td>2002-2010</td>
<td>400-440</td>
<td>250</td>
<td>6.822</td>
<td>1.139</td>
</tr>
<tr>
<td>West Virginia</td>
<td>2003-2010</td>
<td>250</td>
<td>1000</td>
<td>-18.808**</td>
<td>0.321</td>
</tr>
<tr>
<td>Wisconsin</td>
<td>2006-2010</td>
<td>750</td>
<td>350-450</td>
<td>2.208</td>
<td>0.773</td>
</tr>
</tbody>
</table>

**p<.01
*p<.05
***p<.001

Table 4-3: Effect of Caps on Noneconomic Damages on Paid Claims
Figure 4-3: Effect of Caps on Noneconomic Damages on Paid Claims, by State

Continued
Figure 4-3 continued

Continued
Table 4-4 shows the results when the statute of limitations was added as a second knot. The percent change in $R^2$ shows indicates improvement in fit of the model with this added data element. Most states saw very little improved fit by adding the second knot, but two states (Alaska and West Virginia) saw fits that improved by over 5%.
<table>
<thead>
<tr>
<th>State</th>
<th>Years in Effect</th>
<th>Cap (1000s)</th>
<th>Previous Cap (1000s)</th>
<th>Tort Statute of Limitations (Years)</th>
<th>Change in Slope at End of Statute of Limitations</th>
<th>Statute of Limitations Model R² - Base Model R²; (Higher = Better Fit)</th>
<th>% Improvement in R² Over Base Model</th>
</tr>
</thead>
<tbody>
<tr>
<td>Alaska</td>
<td>2006-2010</td>
<td>250</td>
<td>400</td>
<td>2</td>
<td>-4.219</td>
<td>7.242</td>
<td>0.03</td>
</tr>
<tr>
<td>Florida</td>
<td>2003-2010</td>
<td>500</td>
<td>No cap</td>
<td>2</td>
<td>-4.432</td>
<td>2.593</td>
<td>0.025</td>
</tr>
<tr>
<td>Georgia</td>
<td>2005-2010</td>
<td>350</td>
<td>No cap</td>
<td>2</td>
<td>-1.007</td>
<td>-0.602</td>
<td>0.001</td>
</tr>
<tr>
<td>Idaho</td>
<td>2004-2010</td>
<td>250-290</td>
<td>650</td>
<td>2</td>
<td>-0.239</td>
<td>-1.034</td>
<td>0.003</td>
</tr>
<tr>
<td>Illinois</td>
<td>2006-2007</td>
<td>500</td>
<td>No cap</td>
<td>2</td>
<td>0.479</td>
<td>Not in Effect</td>
<td>0</td>
</tr>
<tr>
<td>Mississippi</td>
<td>2003-2010</td>
<td>500</td>
<td>No cap</td>
<td>2</td>
<td>0.119</td>
<td>-1.713</td>
<td>0.006</td>
</tr>
<tr>
<td>Missouri</td>
<td>2006-2010</td>
<td>350</td>
<td>580</td>
<td>2</td>
<td>-2.291</td>
<td>3.134</td>
<td>0.008</td>
</tr>
<tr>
<td>Nevada</td>
<td>2003-2010</td>
<td>350</td>
<td>No cap</td>
<td>1</td>
<td>0.836</td>
<td>-3.328</td>
<td>0.005</td>
</tr>
</tbody>
</table>

Table 4-4: Effect of Caps on Noneconomic Damages on Paid Claims with Statute of Limitations
<table>
<thead>
<tr>
<th>State</th>
<th>Years in Effect</th>
<th>Cap (in $1000s)</th>
<th>Previous Cap (in $1000s)</th>
<th>Tort Statute of Limitations (Years)</th>
<th>Change in Slope at End of Statute of Limitations</th>
<th>Change in Slope at End of Statute of Limitations</th>
<th>Statute of Limitations Model $R^2$ – Base Model $R^2$; (Higher – Better Fit)</th>
<th>% Improvement in $R^2$ Over Base Model</th>
</tr>
</thead>
<tbody>
<tr>
<td>Ohio</td>
<td>2003-2010</td>
<td>500</td>
<td>No cap</td>
<td>1</td>
<td>-4.341</td>
<td>3.594</td>
<td>0.005</td>
<td>0.6%</td>
</tr>
<tr>
<td>Oklahoma</td>
<td>2004-2010</td>
<td>300</td>
<td>No cap</td>
<td>2</td>
<td>-1.663</td>
<td>0.748</td>
<td>0.002</td>
<td>0.9%</td>
</tr>
<tr>
<td>South Carolina</td>
<td>2006-2010</td>
<td>350</td>
<td>No cap</td>
<td>3</td>
<td>-3.815</td>
<td>1.955</td>
<td>0.004</td>
<td>0.5%</td>
</tr>
<tr>
<td>Texas</td>
<td>2004-2010</td>
<td>250</td>
<td>No cap</td>
<td>2</td>
<td>-4.581</td>
<td>4.06</td>
<td>0.015</td>
<td>1.5%</td>
</tr>
<tr>
<td>Utah</td>
<td>2002-2010</td>
<td>400-440</td>
<td>250</td>
<td>2</td>
<td>-2.283</td>
<td>4.189</td>
<td>0.008</td>
<td>1.7%</td>
</tr>
<tr>
<td>West Virginia</td>
<td>2003-2010</td>
<td>250</td>
<td>1000</td>
<td>2</td>
<td>-6.853</td>
<td>9.271</td>
<td>0.07</td>
<td>10.2%</td>
</tr>
<tr>
<td>Wisconsin</td>
<td>2006-2010</td>
<td>750</td>
<td>350-450</td>
<td>3</td>
<td>1.167</td>
<td>-1.968</td>
<td>0.002</td>
<td>0.3%</td>
</tr>
</tbody>
</table>
Table 4-5 shows the differences between states that changed caps on noneconomic damages compared to control states that did not. One state had a significantly different intercept compared to its control, and three states had significantly different slopes compared to their matched controls. Only two states, Nevada and Texas, had a significant change from both the control state and from their baseline trend.
<table>
<thead>
<tr>
<th>State</th>
<th>Years in Effect</th>
<th>Cap ($1000s)</th>
<th>Previous Cap ($1000s)</th>
<th>Matched State for Intercept (Absolute Change)</th>
<th>P-Value of Change in Intercept from Matched State</th>
<th>Matched State for Slope (Trend)</th>
<th>P-Value of Change in Slope from Matched State</th>
</tr>
</thead>
<tbody>
<tr>
<td>Alaska</td>
<td>2006-2010</td>
<td>250</td>
<td>400</td>
<td>Michigan</td>
<td>0.8521</td>
<td>Kansas</td>
<td>0.8969</td>
</tr>
<tr>
<td>Florida</td>
<td>2003-2010</td>
<td>500</td>
<td>No cap</td>
<td>Pennsylvania</td>
<td>0.7037</td>
<td>District of Columbia</td>
<td>0.3484</td>
</tr>
<tr>
<td>Georgia</td>
<td>2005-2010</td>
<td>350</td>
<td>No cap</td>
<td>Delaware</td>
<td>0.7335</td>
<td>District of Columbia</td>
<td>0.0588</td>
</tr>
<tr>
<td>Idaho</td>
<td>2004-2010</td>
<td>250-290</td>
<td>650</td>
<td>New Hampshire</td>
<td>0.3525</td>
<td>Louisiana</td>
<td>0.0747</td>
</tr>
<tr>
<td>Illinois</td>
<td>2006-2007</td>
<td>500</td>
<td>No cap</td>
<td>Maine</td>
<td>0.2405</td>
<td>New Hampshire</td>
<td>0.241</td>
</tr>
<tr>
<td>Mississippi</td>
<td>2003-2010</td>
<td>500</td>
<td>No cap</td>
<td>New Mexico</td>
<td>0.4786</td>
<td>District of Columbia</td>
<td>0.3825</td>
</tr>
<tr>
<td>Missouri</td>
<td>2006-2010</td>
<td>350</td>
<td>580</td>
<td>Connecticut</td>
<td>0.1912</td>
<td>Indiana</td>
<td>0.003**</td>
</tr>
<tr>
<td>Nevada</td>
<td>2003-2010</td>
<td>350</td>
<td>No cap</td>
<td>Kansas</td>
<td>0.6313</td>
<td>Massachusetts</td>
<td>0.0001***</td>
</tr>
</tbody>
</table>

Table 4-5: Differences Between States that Changed Caps on Noneconomic Damages and Those That Did Not
Table 4-5 continued

<table>
<thead>
<tr>
<th>State</th>
<th>Years in Effect</th>
<th>Cap ($1000s)</th>
<th>Previous Cap ($1000s)</th>
<th>Matched State for Intercept (Absolute Change)</th>
<th>P-Value of Change in Intercept from Matched State</th>
<th>Matched State for Slope (Trend)</th>
<th>P-Value of Change in Slope from Matched State</th>
</tr>
</thead>
<tbody>
<tr>
<td>Ohio</td>
<td>2003-2010</td>
<td>500</td>
<td>No cap</td>
<td>Washington</td>
<td>0.4031</td>
<td>Delaware</td>
<td>0.8407</td>
</tr>
<tr>
<td>Oklahoma</td>
<td>2004-2010</td>
<td>300</td>
<td>No cap</td>
<td>Delaware</td>
<td>0.015*</td>
<td>Hawaii</td>
<td>0.1798</td>
</tr>
<tr>
<td>South Carolina</td>
<td>2006-2010</td>
<td>350</td>
<td>No cap</td>
<td>Kentucky</td>
<td>0.4196</td>
<td>Nebraska</td>
<td>0.1764</td>
</tr>
<tr>
<td>Texas</td>
<td>2004-2010</td>
<td>250</td>
<td>No cap</td>
<td>Indiana</td>
<td>0.1767</td>
<td>Rhode Island</td>
<td>0.0001***</td>
</tr>
<tr>
<td>Utah</td>
<td>2002-2010</td>
<td>400-440</td>
<td>250</td>
<td>Washington</td>
<td>0.2789</td>
<td>Michigan</td>
<td>0.751</td>
</tr>
<tr>
<td>West Virginia</td>
<td>2003-2010</td>
<td>250</td>
<td>1000</td>
<td>Wyoming</td>
<td>0.4207</td>
<td>Alabama</td>
<td>0.5276</td>
</tr>
<tr>
<td>Wisconsin</td>
<td>2006-2010</td>
<td>750</td>
<td>350-450</td>
<td>Minnesota</td>
<td>0.8868</td>
<td>Vermont</td>
<td>0.4733</td>
</tr>
</tbody>
</table>

***p<.001  
**p<.01  
*p<.05
Discussion and Conclusion

This study has both important findings as well as implications. First, the effect of similar caps on noneconomic damages on the number of paid claims varies between states. A traditional approach to evaluating the effect of a tort reform is to put the tort reform into a regression along with all the other tort reforms and a state-specific variable and then estimate the average effect of the tort reform. Using 1991-2010 data, this combined approach estimates that caps on noneconomic damages, on average, decrease the rate of paid claims by about .31 per 1,000 physicians (p=.028). This approach, though, ignores the extreme heterogeneity among states (the state-specific variable) which is of great interest to policy makers as similar reforms in different states have very different outcomes (see Table 4-3). The strong policy implication of this study is that there is more to tort reform than just the statutory language. As other states consider implementing caps on noneconomic damages or Congress continues to pursue national caps, attention needs to focus on the specifics of how the policy is implemented. Qualitative evaluation of the statutes, the rule regulations and the implementation process may be needed.

Second, even when there are significant effects on the number of paid claims as a result of noneconomic caps, these effects are relatively minor. While some states had relatively significant changes in malpractice payment trends following implementation of noneconomic caps, when compared to a control state, these differences were quite small.
This is because, independent of state-level tort reform, the trend has been towards fewer malpractice payments over time.

There are several potential reasons some states had significant changes in the rate of paid claims. The first is regression towards the mean which implies that extreme observances will tend to be less extreme (closer to the mean) in subsequent measurements. The two states with the most significant change in rate of paid claims, Florida and South Carolina, were respectively the second most and most extreme states in terms of trend of paid claims prior to implementing noneconomic caps. Regression towards the mean suggests that, simply because of their extreme position, their rates of paid claims would naturally move toward the average, independent of any change in policy.

A second possible reason for the significant drop in paid claims may be a historical effect. A likely possibility is the National Institutes of Health report “To Err is Human: Building a Safer Health System” (Kohn, Corrigan, and Donaldson 1999). Following that report’s release, there began an intense national focus on improving patient safety and eliminating medical errors (Leape and Berwick 2005). The report was issued in November 1999 and beginning in 2001 there was a significant decrease in the national rate of paid claims (p<.0001). Looking at states individually with 2001 as a knot, 18 states saw a significant drop in their number of paid claims. Thus, while tort reform may have been pursued as a means of limiting frivolous lawsuits, the effects of
practitioners throughout the country limiting medical mistakes may have done more to reduce the number of paid malpractice claims.

From a policymaker’s perspective, these results suggest that caps on noneconomic damages are unlikely to severely limit access of meritorious claims to the legal system. Simultaneously, the general trend towards fewer malpractice payments, independent of noneconomic caps, should raise questions as to whether tort reform really has the potential to decrease the cost of healthcare. During the study period, healthcare costs did not significantly decrease, despite the decrease in paid claims. Advocates of tort reform, rather than focusing on its potential cost-benefit, should instead evaluate whether it provides other, noneconomic benefits, such as increased numbers of providers who practice in the state or an elevation of the overall satisfaction of the healthcare workforce.

In conclusion, there is evidence that state caps on noneconomic damages have decreased the number of paid malpractice claims in a small number of states that enact such policies. The change, though, when compared to historical trends in similar comparison states, is modest. It appears that few people with meritorious claims will lose access to the legal system when states implement caps on noneconomic damages.
Chapter References


StataCorp. 2012. Stata Statistical Software: Release 12. College Station, Texas: StataCorp LP.


Chapter 5 – The Spillover Effect of a Change in Medicare Reimbursements on Provider Behavior in the Non-Medicare Population for Bariatric Surgery

Abstract

**Background.** In 2006, the Centers for Medicare and Medicaid Services (CMS) released a national coverage determination (NCD) that created a national policy on Medicare reimbursement for bariatric surgery which defined the covered procedures and required that the procedures be performed in accredited centers of excellence. This led to a decrease in the rate of bariatric surgeries among the Medicare population.

**Objectives.** Determine whether the 2006 NCD had an effect on the non-Medicare population and, if so, estimate the magnitude and timing of that effect.

**Design.** Longitudinal study evaluating rates of bariatric surgery over time.

**Data.** Hospital care data from the Healthcare Cost and Utilization Project using inpatient records of 20% of American hospitals from 1998 to 2010.

**Results.** Decreases in rates of bariatric surgery occurred in both the Medicare and non-Medicare populations. Among patients eligible for bariatric surgery based on obesity and for procedures covered by Medicare, decreases in the rate of surgeries started simultaneously in both populations before the national coverage determination was finalized (July 2005), and rates continued to decrease for approximately 18 months (until
January 2007); Medicare patients saw an overall rate decrease of 34% (95% CI: -46%, -22%) and non-Medicare patients saw a rate decrease of 28% (95% CI: -37%, -20%).

**Conclusions.** The 2006 NCD led to significant decreases in the rates of bariatric surgery in both the Medicare and non-Medicare populations that were comparable in both magnitude and timing. A decrease in the non-Medicare population suggests significant Medicare spillover onto the non-Medicare population and indicates CMS has the ability to influence provider behavior beyond the Medicare population. For analyses of bariatric surgery, this indicates that the non-Medicare population is not an ideal control group to estimate effects of the 2006 NCD on the Medicare population.
Background

Governed by the Centers for Medicare and Medicaid Services (CMS), Medicare is a federally financed insurance program primarily for the elderly (Medicare.gov 2013). Due to increases in the number of covered lives and the cost of healthcare, the total spending through Medicare is expected to grow at an annual rate of up to 6.8% through 2021 (Keehan et al. 2012). Coupled with financial pressures, this expected growth has led to strong political pressures to lower the cost of spending through these programs (Serafini 2012).

CMS is responsible for reimbursing healthcare provided by physicians and other healthcare practitioners. Determination of which procedures are covered by the Medicare program is based on statute and covered procedures must be “reasonable and necessary for the prevention of illness” (Social Security Act § 1862 2013, sec. (1) (B)). These coverage decisions may (1) include defining types of procedures or treatments are reimbursable, and (2) mandate that certain criteria be met prior to reimbursement, e.g., mandating that certain procedures are performed in facilities accredited by a specific facility. These coverage decisions and reimbursement policies directly impact provider behavior (Mitchell, Hadley, and Gaskin 2002).

There are two approaches to deciding whether a procedure is covered by Medicare (Social Security Act § 1869 2013; Medicare Payment Advisory Commission 2003). First, CMS may make a National Coverage Determination (NCD) that specifics whether a specific procedures is covered for all of Medicare. Alternatively, if an NCD has not been released by Medicare, regional Medicare contractors may make a Local
Coverage Determination (LCD) which defines coverage decisions for the region covered by the Medicare contractor. The LCD may not go against an NCD, but when the NCD is silent on a matter, the LCD may fill the void. Each LCD, though, is not bound by precedent set by other LCDs which, in the absence of an NCD, results in variations of covered services across Medicare regions (Foote et al. 2004).

Periodically, CMS is required to make changes to its reimbursement policies as the practice of medicine evolves. Parties who have a financial interest in how Medicare reimburses certain forms of treatment have a vested interest in engaging with CMS as reimbursement changes are considered. This process can take many months and involves multiple steps (Centers for Medicare & Medicaid Services 2003; Medicare Payment Advisory Commission 2003; Centers for Medicare & Medicaid Services 2013). CMS may begin to reevaluate an existing NCD at its own discretion or upon the request of an outside party (known as “A Formal Request for Consideration”) (Centers for Medicare & Medicaid Services 2003, 55638). Following the initiation of a review, the CMS will evaluate the NCD and, if the issue is complex, will refer the issue to the Medicare Coverage Advisory Committee (MCAC) which provides independent recommendations regarding coverage decisions. The MCAC, composed of scientific and technical experts, publicly meets and then recommends to Medicare whether it should modify its coverage policy; CMS may or may not adopt this recommendation. Subsequently, CMS will make a proposed coverage determination which is followed by a period of public comment and a final coverage determination, and then determine an appropriate reimbursement level for the newly covered services.
Individuals and entities paid directly by Medicare have a strong interest in the reimbursement decisions that CMS makes, but others are also affected by these decisions. Insurance companies, for example, often negotiate rates based on Medicare payment rates and adopt Medicare coverage policies (Cassidy and Dentzer 2010; Reinhardt 2010; Mathews and Mcginty 2010). Thus, a change in Medicare reimbursement may also affect the type and volume of care delivered to the non-Medicare population via a spillover effect. Spillover from the private side to Medicare has already been shown as private health maintenance organization penetration rates have been shown to correlate with lower Medicare spending (Baker 1997). Estimating the degree to which the non-Medicare population is affected by a Medicare policy decision will guide policymakers as they seek to use Medicare to improve the whole health system. Medicare policy changes relating to bariatric surgery have been extensively studied as to how they affect the Medicare population, but less has been done to evaluate their spillover effect on the non-Medicare population (Flum et al. 2011; Nguyen et al. 2010). This study evaluates how a Medicare reimbursement change for bariatric surgery affected the rate of bariatric surgery among the non-Medicare population.

**Bariatric Surgery**

Bariatric surgery consists of a family of related procedures that are intended to treat morbidly obese people who have failed to lose weight using other methods (Buchwald 2004). These procedures generally involve constricting the stomach with staples or bands, or bypassing the stomach or portions of the intestine (Buchwald 2004). These procedures are intended to decrease the level at which the person feels satiated or...
limit the amount of calories absorbed by the intestines during digestion. While these procedures have generally been shown to be effective in helping the morbidly obese lose weight (Buchwald 2004), they are also associated with a high risk of mortality within the Medicare population of up to 2% for 30 days and 4.6% at one year (Flum 2005). However, there is evidence that among high-risk groups the death rate for bariatric surgical patients is lower than for patients who do not undergo the operation (Maciejewski 2011). As bariatric surgery techniques have evolved, the types and the volumes of those procedures have changed over time. Figure 5-1 shows the national rates of different types of bariatric surgeries performed in the United States from 1998 to 2010.
Figure 5-1: Rates of Bariatric Surgery by Type, 1998-2010

Note: Prior to October 2004, laparoscopic gastric bypass was not a separate ICD-9-CM code.

Prior to 2006, Medicare would reimburse for gastric bypass procedures if performed on patients with extreme obesity, was medically appropriate and corrected an illness which caused or aggravated the obesity (Centers for Medicare & Medicaid Services 1979). This policy, though, was not detailed enough to specify which types of bariatric procedures would be covered in which circumstances, instead relying on regional LCDs.
In 2006, Medicare provided an NCD dictating how bariatric surgery must be performed to be eligible for Medicare reimbursements (Centers for Medicare & Medicaid Services 2006a). This NCD specifically did three things: it (1) clarified the clinical indicators for bariatric surgery, (2) specified which procedures would be covered by Medicare; and (3) limited the number of facilities that could be reimbursed for providing bariatric surgery. The indicators for morbid obesity limited coverage to patients with a body mass index (BMI) greater than or equal to 35, have at least one comorbidity related to obesity and have unsuccessfully been medically treated for obesity. CMS also explicitly agreed to cover procedures that had recently been approved by the FDA and excluded other procedures from coverage. Finally, and most significantly, CMS required that reimbursable bariatric surgeries be performed in facilities accredited as Bariatric Surgery Centers of Excellence or as Level 1 Bariatric Surgery Centers. The effect of this final policy significantly decreased the number of providers performing these surgeries for the Medicare population (Flum et al. 2011).

While this NCD technically became official when the final rule was published, the process for its adoption was drawn out. Starting on August 1, 2004, the Medicare Coverage and Analysis Group (CAG) evaluated the scientific evidence on the quality outcomes of bariatric surgery and presented their findings in the form of a Summary of Evidence to the MCAC on November 4, 2004 (Brechner et al. 2004). CMS formally opened reconsideration on their bariatric surgery reimbursement policies on May 24, 2005, released a proposed decision memorandum on November 23, 2005 and implemented its final decision on February 21, 2006 (Centers for Medicare & Medicaid Services 2006b).
Services Coverage and Analysis Group 2005; Centers for Medicare & Medicaid Services 2006b). Then, on February 12, 2009, CMS made an additional change wherein they expressly stated that Type 2 diabetes mellitus, for the purpose of coverage, is a comorbid condition which effectively increased the number of patients eligible for reimbursable bariatric surgery (Centers for Medicare & Medicaid Services 2009). More recently, on June 27, 2012, CMS announced an additional expansion of coverage for a newer form of laparoscopic bariatric surgery (Centers for Medicare & Medicaid Services 2012b). The text of the original 1979 NCD, the 2006 NCD and the 2009 NCD are available in Appendix H.

Prior work has evaluated how these changes affected Medicare populations (Flum et al. 2011; Nguyen et al. 2010). In particular, they found that the types of surgeries changed, the number of surgeries temporarily decreased and quality outcomes were better post-implementation (Flum et al. 2011). One study, comparing the Medicare population to the non-Medicare population as a control, found that there were not significantly better improvements in outcomes for Medicare beneficiaries, but this assumes that non-Medicare beneficiaries are not affected by the NCD (Dimick JB 2013). There is anecdotal evidence that private insurers reevaluated their own coverage policies, such as requiring beneficiaries to use Centers of Excellence, in response to the NCD, but the cumulative spillover effect on the non-Medicare population, if any, has not been estimated (Blackstone 2006; BlueCross BlueShield of North Carolina 2012). One study, using commercial claims, suggested that the NCD lead to better outcomes for non-Medicare patients, but does not discuss the rate of procedures performed (Kwon et al.
The purpose of this paper is to evaluate how Medicare reimbursement decisions affected non-Medicare bariatric surgery rates and the timing of those changes.

There is the possibility that it was not Medicare’s NCD that drove the decrease in the rate of bariatric surgeries but some other unidentified factor, but this seems unlikely. In this study, I attempt to identify the nature and extent of the impact of Medicare’s NCD on provider behavior for the non-Medicare patient population; that is, examine the spillover effect of a policy change.

Methods

Data

Data for this study come from the Healthcare Cost and Utilization Project (HCUP) Nationwide Inpatient Sample (NIS) from 1998-2010 (Agency for Healthcare Research and Quality 1998). HCUP collects a representative sample of hospital care data including information on types of care received, cost of care, month of care and payer information (Agency for Healthcare Research and Quality 2009). The NIS contains inpatient data from a sample of more than 1,000 hospitals from 45 states\(^\text{10}\). Information from this sample of hospitals is used to make national estimates on the volume of services provided. The size of the NIS allows me to make monthly estimates of bariatric surgery rates, which improves precision in estimating the timing of changes from

\(^{10}\text{Ranged from 22 states in 1998 to 45 states in 2010.}\)
quarterly changes, as done in past work, to monthly changes. This leads to a sample size of 156 months. It also identifies the primary payer for procedures with Medicare including all fee-for-service Medicare and Medicare Advantage enrollees; non-Medicare includes all other payers. To estimate bariatric surgery rates per 100,000 people, annual Medicare enrollment numbers and national population estimates were used (Centers for Medicare & Medicaid Services 2012a; U.S. Census Bureau 2013).

Analytic Approach

This study evaluates how the rate of bariatric surgery procedures changed as a result of Medicare’s NCD. The rate of bariatric surgeries can vary depending on how procedures and patients are classified using the HCUP International Classification of Diseases, Ninth Revision, Clinical Modification (ICD-9-CM) procedure and International Classification of Diseases, Ninth Revision (ICD-9) diagnosis codes. Following past work, I first evaluated rates of bariatric surgery rates using Medicare diagnosis and procedure requirements (Nguyen et al. 2010). Based on the 2006 NCD, for a procedure to be reimbursable under Medicare, a patient must have a diagnosis of obesity with a body mass index (BMI) over 35; this includes ICD-9 codes V85.35, V85.36, V85.37, V85.38, V85.39, V85.4 and 278.01. In addition, the following ICD-9-CM procedure codes are covered: 44.39 (open roux-en-y gastric bypass), 44.38 (laparoscopic roux-en-y gastric bypass, 44.95 (laparoscopic adjustable gastric banding), or all three of 43.89, 45.51 and 45.91 (open or laparoscopic bileopancreatic diversion with a duodenal switch). There are several bariatric surgeries that are not covered by Medicare, including ICD-9-CM codes 44.31 (high gastric bypass) 44.68 (laparoscopic gastroplasty or vertical banded
gastroplasty), 44.69 (other inversion of gastric diverticulum) and 43.89 by itself (other partial gastrectomy). Despite Medicare not presently covering these procedures as part of its general rule, in prior years these procedures were reimbursed by some Medicare contractors and Medicare is still the primary payer for a small number of these procedures. To capture the widest range of possible populations affected by the NCD, I evaluated (1) patients that had an eligible obesity diagnosis for Medicare reimbursement and received an eligible Medicare procedure, (2) patients with any diagnosis who received an eligible Medicare procedure, (3) patients that had an eligible obesity diagnosis for Medicare reimbursement and received any bariatric surgery and (4) all patients who received any bariatric surgery, regardless of diagnosis.

To calculate bariatric surgery rates, I first summed the procedures performed in each month, using the HCUP admission month as a proxy for the month of the surgery. For Medicare rates I divided the number of procedures where Medicare was the primary payer by the annual population of Medicare enrollees (Centers for Medicare & Medicaid Services 2012a). I estimated the non-Medicare population by subtracting the annual Medicare population from the annual estimated population of the United States; I estimated the rate by dividing the number of procedures where Medicare is not the primary payer by this estimated population. All rate estimates are per 100,000 people.

An alternative approach to estimate changes over time would be to use logistic regression and evaluate the probability of an individual receiving bariatric surgery by year. Following past work, I will focus on rate of surgeries, which is the unit of analysis, using a time series approach (Flum et al. 2011; Nguyen et al. 2010).
Using the same underlying rates of bariatric surgery data, I use two different statistical models to evaluate (1) the timing of the beginning and ending of the changes and (2) the degree of the changes at the time the policy was officially implemented. The temporal rate trends were fit using a restricted cubic spline model\(^\text{11}\) (Harrell 2001). This spline model smooths the raw data while still capturing the increases and decreases that occurred around the NCD. Because I am primarily interested in what occurred near the NCD, I model the rate beginning in 2003; earlier years’ data is plotted to show prior trends. Images of these graphs are available in Figure 5-2. I used these fitted values to estimate temporal maxima and minima near the time of the NCD. The maxima represent the point in time when the rate of bariatric surgeries began to decrease for the Medicare and the non-Medicare population and the minima represent the end of the decrease. I estimated the size of the impact with \((\text{maximum} – \text{minimum}) / \text{maximum}\).

\[^{11}\] This was calculated using the \textit{mkspline} function in Stata 12.1 with default knots placed by the function.
Figure 5-2: Rate of Bariatric Surgery per Month by Payer, 1998-2010
Figure 5-2 continued

Rate of Bariatric Surgery per Month, by Payer
Patient with Obesity Diagnosis, All Bariatric Surgery Procedures

Rate of Bariatric Surgery per Month, by Payer
All Patients, All Bariatric Surgery Procedures
The second analysis estimates whether there was a significant change at the specific time that the NCD went into effect (Flum et al. 2011). This approach uses an interrupted time series that evaluates whether there was a change in the temporal trend or the absolute number of procedures at the point in time when the NCD went into effect (Shadish, Cook, and Campbell 2002). The model for this analysis is:

Equation 5-1:

\[ Y_{time} = \beta_0 + \beta_1 \text{(time)} + \beta_2 \text{(time-NCDtime)}_{\text{time-NCDtime} > 0} + \beta_3 \text{(NCD)} \]

\( Y \) represents the estimated rate of bariatric procedures at \( time \). \( \beta_0 \) is the baseline rate of surgeries at the start of the study and \( time \) is the time in years (including fractions for months). \( \beta_1 \) is a vector for a series of polynomial terms\(^{12}\) of the trend of bariatric procedures over time. \( NCDtime \) is the time since the NCD went into effect and \( \text{time-NCDtime} \) must be greater than zero. \( NCD \) is a dummy variable indicating whether the NCD is in effect at any month. \( \beta_2 \) represents any change in the slope of the trend line following the implementation of the NCD and \( \beta_3 \) represents whether there is a change in

\(^{12}\) I used a polynomial model, rather than a linear model, because the data was not linear. This was calculated using the \text{fracpoly} function in Stata 12.1 which chooses the polynomial terms which minimize the Akaike information criterion (AIC) meaning the specific polynomial terms varied during specifications of the model.
the intercept at the point in which the NCD is implemented. Statistical significance is determined by the p-values of the βs. Either a change in slope or intercept is indicative of a significant effect from the baseline, indicating that Medicare spillover at this point in time occurred (Shadish, Cook, and Campbell 2002). Graphical depictions of these fits are included in Figure 5-3 and their tabulated values are in Table 5-1. Graphical depictions of each individual procedure are available in Appendix I.

I additionally ran each of my models and broke patients into three categories: Medicare, Medicaid and all other payers. The focus of this paper is on evaluating the Medicare spillover effect onto all other payers, so I only used two categories (Medicare and non-Medicare) of patients. Graphs of bariatric surgery rates including the Medicaid classification are available in Appendix J. All analysis was done using Stata version 12.1 (StataCorp 2012b). The primary code used to generate these results is available in Appendix K.
Continued

Figure 5-3: Rate of Bariatric Surgery per Month by Payer, 1998-2010 (Spline Fit)
Figure 5-3 continued
Results

The estimated decrease in the rates of bariatric surgery during the period of the NCD for each of the four classifications is available in Table 5-1. For both the Medicare and non-Medicare population, the rate of bariatric surgeries decreased between the middle of 2005 to their low around the beginning of 2007 (p<.001 for all classifications). For each of the classifications of procedures, the size of the decrease was similar; within each classification there was a maximum absolute difference of 6.6% (22.4% relative difference) between the Medicare population and the non-Medicare population.

There was also very similar timing effects between both populations. For the two classifications that dealt with the procedures that are covered by Medicare (those most likely to be affected by the Medicare decision), the beginning and ending month of the decrease was the same for the Medicare and the non-Medicare population (July 2005 to January 2007). For the procedures that are specifically covered by Medicare, the decrease began shortly after CMS opened reconsideration of their reimbursement policies for bariatric surgery. The initial declines in the rate of surgeries were gradual, but became larger once the proposed NCD was released. The other classifications that included procedures not covered by Medicare also saw similar beginning and ending months to their decrease, though the non-Medicare population began a little earlier (December 2004 or January 2005 compared to April 2005); both Medicare and non-Medicare populations saw similar ends to their decrease (December 2006 or January 2007). Some of the decrease in non-covered procedures was likely due to the general shift away from open to laparoscopic procedures.
<table>
<thead>
<tr>
<th>Classification</th>
<th>Payer</th>
<th>Beginning of Decrease</th>
<th>End of Decrease</th>
<th>Rate at Beginning of Decrease (per 100,000)</th>
<th>Rate at NCD Effective Date (Feb 2006)</th>
<th>Rate at End of Decrease (per 100,000)</th>
<th>Overall Rate Change (95% CI)</th>
<th>% Rate Change (95% CI)</th>
<th>P-value of change</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Patient with Obesity Diagnosis, Procedure Covered by Medicare</strong></td>
<td>Medicare</td>
<td>July 2005</td>
<td>January 2007</td>
<td>1.63</td>
<td>1.44</td>
<td>1.08</td>
<td>-0.56 (-0.75, -0.36)</td>
<td>-34% (-45.9%, -22.2%)</td>
<td>0.000</td>
</tr>
<tr>
<td></td>
<td>Non-Medicare</td>
<td>July 2005</td>
<td>January 2007</td>
<td>3.02</td>
<td>2.71</td>
<td>2.17</td>
<td>-0.86 (-1.11, -0.61)</td>
<td>-28.4% (-36.5%, -20.2%)</td>
<td>0.000</td>
</tr>
<tr>
<td><strong>All Patients, Procedure Covered by Medicare</strong></td>
<td>Medicare</td>
<td>July 2005</td>
<td>January 2007</td>
<td>2.56</td>
<td>2.32</td>
<td>1.85</td>
<td>-0.71 (-0.94, -0.49)</td>
<td>-27.9% (-36.6%, -19.2%)</td>
<td>0.000</td>
</tr>
<tr>
<td></td>
<td>Non-Medicare</td>
<td>July 2005</td>
<td>January 2007</td>
<td>3.18</td>
<td>2.87</td>
<td>2.30</td>
<td>-0.88 (-1.13, -0.63)</td>
<td>-27.7% (-35.6%, -19.8%)</td>
<td>0.000</td>
</tr>
<tr>
<td><strong>Patient with Obesity Diagnosis, All Bariatric Surgery Procedures</strong></td>
<td>Medicare</td>
<td>April 2005</td>
<td>Decembe 2006</td>
<td>1.81</td>
<td>1.53</td>
<td>1.22</td>
<td>-0.59 (-0.78, -0.4)</td>
<td>-32.6% (-43.2%, -22%)</td>
<td>0.000</td>
</tr>
<tr>
<td></td>
<td>Non-Medicare</td>
<td>December 2004</td>
<td>Decembe 2006</td>
<td>3.45</td>
<td>2.88</td>
<td>2.38</td>
<td>-1.07 (-1.32, -0.83)</td>
<td>-31% (-38.1%, -23.9%)</td>
<td>0.000</td>
</tr>
</tbody>
</table>

Table 5-1: Timing and Magnitude of Decrease in Rate of Bariatric Surgeries
Table 5-1 continued

<table>
<thead>
<tr>
<th>Classification</th>
<th>Payer</th>
<th>Beginning of Decrease</th>
<th>End of Decrease</th>
<th>Rate at Beginning of Decrease (per 100,000)</th>
<th>Rate at NCD Effective Date (Feb 2006)</th>
<th>Rate at End of Decrease (per 100,000)</th>
<th>Overall Rate Change (95% CI)</th>
<th>% Rate Change (95% CI)</th>
<th>P-value of change</th>
</tr>
</thead>
<tbody>
<tr>
<td>All Patients, All Bariatric Surgery Procedures</td>
<td>Medicare</td>
<td>April 2005</td>
<td>January 2007</td>
<td>3.25</td>
<td>2.94</td>
<td>2.51</td>
<td>-0.74 (-0.98, -0.5)</td>
<td>-22.7% (-30.2%, -15.3%)</td>
<td>0.000</td>
</tr>
<tr>
<td></td>
<td>Non-Medicare</td>
<td>January 2005</td>
<td>December 2006</td>
<td>3.73</td>
<td>3.16</td>
<td>2.63</td>
<td>-1.09 (-1.34, -0.85)</td>
<td>-29.3% (-35.9%, -22.7%)</td>
<td>0.000</td>
</tr>
</tbody>
</table>
By the time the official NCD was released, the rates of bariatric surgeries had already been decreasing for at least six months as shown in Table 5-1, and the total period of decrease lasted approximately a year and a half. With a gradual decrease, rather than a sharp decrease at a point in time, an interrupted time series is unlikely to capture the entire effect of the NCD. Despite this limitation, there is some evidence of a decrease in the number of bariatric surgeries, but it is stronger for the Medicare population. Estimates for each of the classifications for both populations is in Table 5-2.

<table>
<thead>
<tr>
<th>Classification</th>
<th>Payer</th>
<th>Estimated Change (95% CI)</th>
<th>Standard Error</th>
<th>P-value</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Patient with Obesity Diagnosis, Procedure Covered by Medicare</strong></td>
<td>Medicare</td>
<td>-0.50 (-0.84, -0.16)</td>
<td>0.17</td>
<td>0.004</td>
</tr>
<tr>
<td></td>
<td>Non-Medicare</td>
<td>-0.28 (-0.71, 0.16)</td>
<td>0.22</td>
<td>0.210</td>
</tr>
<tr>
<td><strong>All Patients, Procedure Covered by Medicare</strong></td>
<td>Medicare</td>
<td>-0.66 (-1.05, -0.28)</td>
<td>0.19</td>
<td>0.001</td>
</tr>
<tr>
<td></td>
<td>Non-Medicare</td>
<td>-0.29 (-0.73, 0.15)</td>
<td>0.22</td>
<td>0.201</td>
</tr>
<tr>
<td><strong>Patient with Obesity Diagnosis, All Bariatric Surgery Procedures</strong></td>
<td>Medicare</td>
<td>-0.59 (-0.96, -0.23)</td>
<td>0.19</td>
<td>0.002</td>
</tr>
<tr>
<td></td>
<td>Non-Medicare</td>
<td>-0.64 (-1.12, -0.17)</td>
<td>0.24</td>
<td>0.009</td>
</tr>
<tr>
<td><strong>All Patients, All Bariatric Surgery Procedures</strong></td>
<td>Medicare</td>
<td>-0.77 (-1.22, -0.31)</td>
<td>0.23</td>
<td>0.001</td>
</tr>
<tr>
<td></td>
<td>Non-Medicare</td>
<td>-0.68 (-1.16, -0.19)</td>
<td>0.24</td>
<td>0.007</td>
</tr>
</tbody>
</table>

Table 5-2: Estimated Change in Rate of Bariatric Surgery At Implementation of National Coverage Decision
I also repeated both of these analyses around the time of the 2009 NCD that determined Type 2 diabetes mellitus qualified as a comorbid condition for purposes of reimbursement. I found no conclusive evidence that this lead to a significant increase or decrease in the rate of bariatric surgery for either the Medicare or non-Medicare population. A challenge with this was that the overall trend seemed to be plateauing prior to the 2009 NCD going into effect, making any effect difficult to identify. This was also a much smaller change in policy and there may not have been a significant effect on the overall rate of bariatric surgeries.

Discussion

Spillover Effect

This study suggests that the Medicare NCD for bariatric surgery lead to a significant decrease in the rate of bariatric surgeries in the Medicare population and a comparable spillover effect on non-Medicare population. Two reasons likely combined to create this result: provider behavior and insurance company mimicry. It is unlikely that providers will drastically change their practice patterns for different populations that they serve. If providers serve both the Medicare population and the non-Medicare population, then any change they make in response to one population will likely carry through and affect the other. For bariatric surgery, Medicare’s NCD lead to a decrease in the number of surgeons performing bariatric surgeries and a decrease in facilities where these took place, with a shift from low- to high-volume centers (Flum et al. 2011). If low-volume facilities and surgeons stopped serving the Medicare population, it is likely that they simply stopped performing bariatric surgeries all together.
Additionally, there is the role that private insurers play. Private insurers are likely to mimic Medicare’s coverage decision for multiple reasons. First, it simplifies their policies. Second, the NCD was initiated based on Medicare’s finding that it would likely improve outcomes, a fact of interest to private insurers. Third, it was expected that by limiting reimbursement to centers of excellence, Medicare would decrease the total volume of surgeries. The decreased rate of surgery is consistent with the interests of private insurers.

Given these reasons, it is not surprising that there was an effect on the non-Medicare population following the NCD. What was more surprising was the degree of the effect, and the similarity of effect occurring in both populations. Such a strong impact is indicative of the power that Medicare has to influence provider behavior among all populations.

A second surprising finding was the timing of the effect. For the procedures that were directly affected by the Medicare decision (those procedures covered by NCD), the impact started and ended at the same time for both populations, and the period of decline in the rate lasted for a year and a half. Further, it was surprising the initial decrease began sooner than the NCD went into effect, but corresponded with CMS officially opening reconsideration. At this point, recommendations that CMS adopt a policy requiring reimbursable procedures to be performed in centers of excellence had already been made, and it is likely that once CMS officially began reconsideration, interested parties predicted the likely outcome (Centers for Medicare & Medicaid Services 2004; Sugerman 2005). When the proposed NCD came out, there was little room to doubt what
CMS would do and a more significant decrease occurred in both populations. This indicates that the effect of Medicare policies are likely to develop over time, not instantaneously, and that there is no lag between the effect on the Medicare and the non-Medicare population.

Non-Medicare Population as a Control Group

To compare the effect of an intervention using a control group, the control group must be unexposed to the effects of the intervention. In this case, the intervention (the NCD which defined reimbursable procedures and required them to be performed at centers of excellence) was aimed at the Medicare population and lead to a significant decrease in the Medicare rate of bariatric surgeries. A comparable effect, though, occurred in the non-Medicare population, suggesting a strong spillover effect on this population. Given the similar observed effect on the Medicare and non-Medicare populations, in the context of bariatric surgery, the non-Medicare population is not a suitable control group for evaluating the effects of the NCD on the Medicare population.

Generalizability

The results of this study speak only to the spillover effect between the Medicare and the non-Medicare patient population with regard to bariatric surgery. This spillover effect, though, is likely to occur with other policies as well. Whenever Medicare makes a policy that changes provider behavior, that policy is also likely to change provider behavior for non-Medicare patients. This suggests that Medicare, due to its size and influence, has considerable power to influence how care is delivered to the entire population, not just those enrolled in the Medicare program.
Chapter References


Kwon, Steve, Bruce Wang, Edwin Wong, Rafael Alfonso-Cristancho, Sean D. Sullivan, and David R. Flum. 2012. “The Impact of Accreditation on Safety and Cost of


StataCorp. 2012. *Stata Statistical Software: Release 12*. College Station, Texas: StataCorp LP.


Chapter 6 – Conclusion

Researchers deal with many challenges when estimating the effects of health policies. The implementation of the policy, in particular, raises multiple challenges as a single policy can be implemented differently by different actors, the effect of a policy itself can change over time, different actors can implement the policy at different times and the policy may spillover and affect other groups. In this dissertation I have identified and addressed these concerns with disparate research topics that health services researchers may face.

I first identified the concern of different actors with the topic of degree of crowd-out of children from public health insurance. In this case, past work had attempted to estimate the cumulative effect of changes in Medicaid or Children’s Health Insurance Program (CHIP) eligibility. This approach, though, ignores the extreme variability in how individual states implement their Medicaid or CHIP programs. I addressed this concern by individually estimating the rates of crowd-out at the individual state level using a regression discontinuity design. By adjusting for this, I found that crowd-out rates varied among states, even when states had similar Medicaid or CHIP eligibility levels. This indicates that crowd-out needs to be evaluated at the state level, and not estimated nationally.

I then identified that the effect of a policy may change over time with children’s crowd-out levels in the state of Ohio. Crowd-out levels are partly a function of how
many children have private insurance, which in turn is dictated by the strength of the economy. From 2007-2009 the United States was in a recession, indicating decreases in the strength of the economy. I evaluated Ohio children’s crowd-out levels prior, during and following the economic recession. I found that the crowd-out levels did indeed change over time and that they decreased between 2004 and 2012. This indicates that crowd-out levels are not static and should be evaluated regularly, not at a single point in time.

I next identified a policy that may have delayed implementation with state tort reform laws that put caps on noneconomic damages for medical malpractice awards. The delay in the effect of a policy may arise as a result of delays between passing a law and its effective date, statutes of limitations and judicial review of the law. I estimated, at the state level, the effect of caps on noneconomic damages, controlling for the general state trend in terms of the rate of paid claims per physician within the state and explicitly estimating the effect of the law following the end of the statute of limitations. I also matched the states with comparable, control states so that unobserved factors with the state may also be controlled for. I found that after adjusting for the states historical trend in the rate of paid claims, states had variable effects from implementing noneconomic damage caps. For most states, there was no statistically significant effect. This indicates that caps on noneconomic damages, even though they may be very similar statutes, may have different effects depending on how they are implemented with an individual state.

Finally, I identified a policy where there is potentially a spillover effect from the targeted population onto another with the rates of bariatric surgery. When Medicare
changed how bariatric surgery would be reimbursed, there was a decrease in the rate of procedures among the Medicare population due to changes in provider behavior. Medical providers, though, generally do not only serve the Medicare population and the change in their behavior towards the Medicare population may spillover onto their behavior towards the non-Medicare population. I evaluated both the Medicare and non-Medicare population to identify whether Medicare saw a similar effect and, if so, what was the timing of that effect. I found that the effect on the non-Medicare population was comparable to the effect on the Medicare population and there was no lag time (i.e., the effect on both populations was simultaneous). This indicates that when studying the effect of a Medicare policy change there is spillover onto the non-Medicare population and that the non-Medicare population would thus not make a good control group to evaluate the effects on the Medicare population.

The cumulative effect of this dissertation is to raise these implementation concerns and to demonstrate with specific problems when they may arise and some methods for addressing those concerns. The methods used are unique to the specific problems addressed, but the concerns are universal. As researchers approach any health policy evaluation problem, particularly if the impact is to be evaluated while the policy is being implemented, they should ask, at a minimum, whether the concerns raised in this dissertation apply to them. If one or more concerns does apply, then they need to be acknowledged and then an appropriate study design needs to be chosen. In this way, researchers will be able to more accurately estimate the impact of a health policy.
References


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“Originally Called the State Children’s Health Insurance Program (SCHIP).”


StataCorp. 2012a. *Stata Statistical Software: Release 12.* College Station, Texas: StataCorp LP.

———. 2012b. *Stata Statistical Software: Release 12.* College Station, Texas: StataCorp LP.


Appendix A – Acronyms

ACS – American Community Survey
ARF – Area Resource File
BA – Bachelor of Arts
BMI – Body Mass Index
CAG – Medicare Coverage and Analysis Group
CHIP – Children’s Health Insurance Program
CI – Confidence Interval
CMS – Centers for Medicare and Medicaid Services
DO – Doctor of Osteopathic Medicine
DSTLR – Database of State Tort Law Reforms
FDA – Food and Drug Administration
FPL – Federal Poverty Level
HCUP – Healthcare Cost and Utilization Project
HHS – Department of Health and Human Services
ICD-9 – International Classification of Diseases, Ninth Revision
ICD-9-CM – International Classification of Diseases, Ninth Revision, Clinical Modification
IV – Instrumental Variable
JD – Juris Doctor
LCD – Local Coverage Determination
MCAC – Medicare Coverage Advisory Committee
MD – Medical Doctor
MHA – Master of Health Administration
MS – Master of Science
NCD – National Coverage Determination
NIS – National Inpatient Sample
NPDB – National Practitioner Data Bank
PhD – Doctor of Philosophy
PUMS – Public Use Microdata Sample
RD – Regression Discontinuity
SCHIP – State Children's Health Insurance Program
US – United States
Appendix B – State Graphs of Estimated Crowd-Out
Percent of Texas Children with Private Insurance as a Function of Federal Poverty Level

Percent of Utah Children with Private Insurance as a Function of Federal Poverty Level

Percent of Vermont Children with Private Insurance as a Function of Federal Poverty Level

Percent of Virginia Children with Private Insurance as a Function of Federal Poverty Level

Percent of Washington Children with Private Insurance as a Function of Federal Poverty Level

Percent of Washington, DC Children with Private Insurance as a Function of Federal Poverty Level
Appendix C – State Graphs of Estimated Crowd-Out, Polynomial Fit
/* David Muhlestein  
Started: October 25, 2011  
Updated  
October 27, 2011  
November 1, 2011  
November 8, 2011  
November 9, 2011  
November 10, 2011  
November 14, 2011  
December 6, 2011  
January 18, 2012 I quit dropping data for blocks less than 50% of the FPL (it only affected 4 states and it made minimal difference)  
2013-03-13 - Updated with new file locations  
2013-03-28 Updates based on feedback from MMRR  
This is taking the ACS data and calculates the number of children who have private insurance in different blocks of income levels  
*/

set more off

clear

use "D:\data\American Community Survey\Crowd-Out\ACS_10_18under.dta" //The file to use

//this merges the data with the crosswalk
//data comes from the 2010 Kaiser Medicaid survey and the 2008 survey of disregards; I used disregards for those that are expected to be near the eligibility threshold; I only did generally applicable disregards because I can't estimate income from child support or money spent on child care
merge m:1 st using "D:\Dropbox\My Research\ACS\Crosswalks\2010_Crosswalk.dta"
drop _merge
//this will update everything on the povlev to match Census Bureau's to HHS; Census Bureau differs from HHS a little, see:
http://www.census.gov/hhes/www/poverty/about/overview/measure.html and
http://aspe.hhs.gov/poverty/10poverty.shtml

/*

Census Bureau:
Table with row headings in column A and column headings in rows 4 to 8.

Poverty Thresholds for 2010 by Size of Family and Number of Related Children Under 18 Years

<table>
<thead>
<tr>
<th>Related children under 18 years</th>
</tr>
</thead>
<tbody>
<tr>
<td>Size of family unit</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>One person (unrelated individual)</td>
</tr>
<tr>
<td>Under 65 years</td>
</tr>
<tr>
<td>65 years and over</td>
</tr>
<tr>
<td>Two people</td>
</tr>
<tr>
<td>Householder under 65 years</td>
</tr>
<tr>
<td>Householder 65 years and over</td>
</tr>
<tr>
<td>Three people</td>
</tr>
<tr>
<td>Four people</td>
</tr>
<tr>
<td>Five people</td>
</tr>
<tr>
<td>Persons in family</td>
</tr>
<tr>
<td>------------------</td>
</tr>
<tr>
<td>1</td>
</tr>
<tr>
<td>2</td>
</tr>
<tr>
<td>3</td>
</tr>
<tr>
<td>4</td>
</tr>
<tr>
<td>5</td>
</tr>
<tr>
<td>6</td>
</tr>
<tr>
<td>7</td>
</tr>
<tr>
<td>8</td>
</tr>
</tbody>
</table>

For families with more than 8 persons, add $3,740 for each additional person.

**2010 Poverty Guidelines for Alaska**

<table>
<thead>
<tr>
<th>Persons in family</th>
<th>Poverty guideline</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>$13,530</td>
</tr>
<tr>
<td>2</td>
<td>18,210</td>
</tr>
<tr>
<td>3</td>
<td>22,890</td>
</tr>
<tr>
<td>4</td>
<td>27,570</td>
</tr>
<tr>
<td>5</td>
<td>32,250</td>
</tr>
<tr>
<td>6</td>
<td>36,930</td>
</tr>
<tr>
<td>7</td>
<td>41,610</td>
</tr>
<tr>
<td>8</td>
<td>46,290</td>
</tr>
</tbody>
</table>

For families with more than 8 persons, add $4,680 for each additional person.

**2010 Poverty Guidelines for Hawaii**

<table>
<thead>
<tr>
<th>Persons in family</th>
<th>Poverty guideline</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>$12,460</td>
</tr>
<tr>
<td>2</td>
<td>16,760</td>
</tr>
</tbody>
</table>
For families with more than 8 persons, add $4,300 for each additional person.

*/
// I will change each of the poverty level estimates to the equivalent HHS estimates, using the
weighted average Census value

matrix census =
(11344,14676,17374,22314,26439,30970,35019,39720,45220,45220,45220,45220,45220,45220,45220,45220,45220,45220,45220,45220)

matrix hhs =
(10830,14570,18310,22050,25790,29530,33270,37010,40750,44490,48230,51970,55710,59450,63190,66930,70670,74410,78150,81890)

matrix alaska =
(13530,18210,22890,27570,32250,36930,41610,46290,50970,55650,60330,65010,69690,74370,79050,83730,88410,93090,97770,102450)

matrix hawaii =
(12460,16760,21060,25360,29660,33960,38260,42560,46860,51160,55460,59760,64060,68360,72660,76960,81260,85560,89860,94160)

gen povlevcensus=povlev
gen estincome=. //the estimated income

// I use the number of people in the family as the count size (since that is what is used to
determine the poverty level), unless its missing, and I use the number of people in the
household
gen num=npf
replace num=np if num==.

// disregard starts at 0
gen disregard=. 
destring wif, replace  // convert workers in family to

forvalues i = 1/20 {
disp `i'
local census = census[1,`i']
disp `census'
local hhs = hhs[1,`i']
disp `hhs'
local alaska = alaska[1,`i']
local hawaii = hawaii[1,`i']

replace estincome = povlevcensus/100*`census' if num==`i'

//the disregard is the higher of the perworker monthly amount times 12 months times
//the number of workers in the family or a percent of the total income
replace disregard = max (percentearning*estincome,wif*perworker*12) if num==`i'

replace povlev = min (round ( (max (estincome-disregard,0))/`hhs'*100),501) if num==`i' & st==2
replace povlev = min (round ( (max (estincome-disregard,0))/`alaska'*100),501) if num==`i' & st==15
}

replace povlev=. if povlevcensus==.

sort povlev

gen eligible=0  //the child is eligible if they are below the cutoff (don't use the schip_mcd_elig
data
replace eligible=1 if povlev<cutoff

label variable eligible "Whether the child is eligible for Medicaid or SCHIP; 1=eligible,
0=ineligible"

gen ineligible=abs (eligible-1) //oposite of eligible
label variable ineligible "Whether the child is ineligible for Medicaid or SCHIP; 0=eligible,
1=ineligible"

//generating blocks of poverty levels

sort povlev

gen p1=povlev

gen p3=povlev-mod (povlev,3)
//generate whether they have private coverage
gen private=abs(real(privcov)-2)
label variable private "Whether the child has private insurance coverage; 0=no, 1=yes"

//For each level of blocks we create a stata log file with all the data stored and then convert it to a .log file
//These contain the weighted number of children who have private insurance in each poverty block
set more off //don't pause the output

drop if _foreign==1 & privcov<1 & privcov>0.95 & !wh
egen p5=povlev-mod(povlev,5)
gen p10=povlev-mod(povlev,10)

log using "D:\Dropbox\My Research\ACS\Logs\private10.smcl", replace
table p10 private eligible [fw=pwgtp], csep (5) contents (freq) format (%12.0g) by (st) missing
close log
translate "D:\Dropbox\My Research\ACS\Logs\private10.smcl" "D:\Dropbox\My Research\ACS\Logs\private10.log", replace
linesize (179) translator (smcl2log)

log using "D:\Dropbox\My Research\ACS\Logs\private5.smcl", replace
table p5 private eligible [fw=pwgtp], csep (5) contents (freq) format (%12.0g) by (st) missing
log close
translate "D:\Dropbox\My Research\ACS\Logs\private5.smcl" "D:\Dropbox\My Research\ACS\Logs\private5.log", replace
linesize (179) translator (smcl2log)

log using "D:\Dropbox\My Research\ACS\Logs\private3.smcl", replace
table p3 private eligible [fw=pwgtp], csep (5) contents (freq) format (%12.0g) by (st) missing
log close
translate "D:\Dropbox\My Research\ACS\Logs\private3.smcl" "D:\Dropbox\My Research\ACS\Logs\private3.log", replace
linesize (179) translator (smcl2log)

log using "D:\Dropbox\My Research\ACS\Logs\private1.smcl", replace
table p1 private eligible [fw=pwgtp], csep (5) contents (freq) format (%12.0g) by (st) missing
log close
translate "D:\Dropbox\My Research\ACS\Logs\private1.smcl" "D:\Dropbox\My Research\ACS\Logs\private1.log", replace
linesize (179) translator (smcl2log)

//For each level of blocks we create a stata log file with all the data stored and then convert it to a .log file
//These contain the raw (non-weighted) number of children who have private insurance in each poverty block
set more off //don't pause the output

log using "D:\Dropbox\My Research\ACS\Logs\private10raw.smcl", replace
table p10 private eligible, csep (5) contents (freq) format (%12.0g) by (st) missing
log close
translate "D:\Dropbox\My Research\ACS\Logs\private10raw.smcl" "D:\Dropbox\My
Research\ACS\Logs\private10raw.log", replace linesize (179) translator (smcl2log)

log using "D:\Dropbox\My Research\ACS\Logs\private5raw.smcl", replace
table p5 private eligible, csep (5) contents (freq) format (%12.0g) by (st) missing
log close
translate "D:\Dropbox\My Research\ACS\Logs\private5raw.smcl" "D:\Dropbox\My
Research\ACS\Logs\private5raw.log", replace linesize (179) translator (smcl2log)

log using "D:\Dropbox\My Research\ACS\Logs\private3raw.smcl", replace
table p3 private eligible, csep (5) contents (freq) format (%12.0g) by (st) missing
log close
translate "D:\Dropbox\My Research\ACS\Logs\private3raw.smcl" "D:\Dropbox\My
Research\ACS\Logs\private3raw.log", replace linesize (179) translator (smcl2log)

log using "D:\Dropbox\My Research\ACS\Logs\private1raw.smcl", replace
table p1 private eligible, csep (5) contents (freq) format (%12.0g) by (st) missing
log close
translate "D:\Dropbox\My Research\ACS\Logs\private1raw.smcl" "D:\Dropbox\My
Research\ACS\Logs\private1raw.log", replace linesize (179) translator (smcl2log)

clear //clear the memory to get the info we put in the log files

//Input the data back into stat using fixed values
//This is for blocks of 10

infix st 1-3 p10 6-10 inelig_no_priv 14-27 inelig_priv 30-45 elig_no_priv 47-59 elig_priv 62-75
using "D:\Dropbox\My Research\ACS\Logs\private10.log"

//This generates the state codes so that they exist on each line of the data and drops the
instances that don't have values

gen y=. forval i = 1/=_N' {
local j = 'i' - 1
quietly replace y=y[j] in `i' if st[i]!=.
quietly replace y=y[j] in `i' if st[i]==.
}

207
drop if p10==.
replace st=y
drop y
save "D:\Dropbox\My Research\ACS\Block Data\private10.dta", replace

clear //clear the memory to get the info we put in the log files

//Input the data back into stat using fixed values
//This is for blocks of 5

infix st 1-3 p5 6-10 ineligible_no_priv 14-27 ineligible_priv 30-45 eligible_no_priv 47-59 eligible_priv 62-75 using "D:\Dropbox\My Research\ACS\Logs\private5.log"

//This generates the state codes so that they exist on each line of the data and drops the instances that don’t have values

gen y=.
forval i = 1/`=_N' {
    local j = `i' - 1
    quietly replace y=st[`i'] in `i' if st[`i']!=.
    quietly replace y=y[`j'] in `i' if st[`i']==.
 }
drop if p5==.
replace st=y
drop y
save "D:\Dropbox\My Research\ACS\Block Data\private5.dta", replace

clear //clear the memory to get the info we put in the log files

//Input the data back into stat using fixed values
//This is for blocks of 3

infix st 1-3 p3 6-10 ineligible_no_priv 14-27 ineligible_priv 30-45 eligible_no_priv 47-59 eligible_priv 62-75 using "D:\Dropbox\My Research\ACS\Logs\private3.log"

//This generates the state codes so that they exist on each line of the data and drops the instances that don’t have values

gen y=.
forval i = 1/'=_N' {
local j = `i' - 1
quietly replace y=st[`i'] in `i' if st[`i']!=.
quietly replace y=y[`j'] in `i' if st[`i']==.
}

drop if p3==.
replace st=y
drop y
save "D:\Dropbox\My Research\ACS\Block Data\private3.dta", replace

clear //clear the memory to get the info we put in the log files

//Input the data back into stat using fixed values
//This is for blocks of 1
infix st 1-3 p1 6-10 inelig_no_priv 14-27 inelig_priv 30-45 elig_no_priv 47-59 elig_priv 62-75
using "D:\Dropbox\My Research\ACS\Logs\private1.log"

//This generates the state codes so that they exist on each line of the data and drops the
instances that don't have values

gen y=.
forval i = 1/'=_N' {
local j = `i' - 1
quietly replace y=st[`i'] in `i' if st[`i']!=.
quietly replace y=y[`j'] in `i' if st[`i']==.
}

drop if p1==.
replace st=y
drop y
save "D:\Dropbox\My Research\ACS\Block Data\private1.dta", replace

clear //clear the memory to get the info we put in the log files

//Input the data back into stat using fixed values
//This is for blocks of 10 non-weighted
infix st 1-3 p10 6-10 n_inelig_no_priv 14-27 n_inelig_priv 30-45 n_elig_no_priv 47-59 n_elig_priv 62-75
using "D:\Dropbox\My Research\ACS\Logs\private10raw.log"
//This generates the state codes so that they exist on each line of the data and drops the instances that don't have values

gen y=.  
forval i = 1/`=_N' {  
local j = `i' - 1  
quietly replace y=st[`i'] in `i' if st[`i']!=.  
quietly replace y=y[`j'] in `i' if st[`i']==.  
}
drop if p10==.  
replace st=y  
drop y  
save "D:\Dropbox\My Research\ACS\Block Data\private10raw.dta", replace

clear //clear the memory to get the info we put in the log files

//Input the data back into stat using fixed values  
//This is for blocks of 5 non-weighted

infix st 1-3 p5 6-10 n_inelig_no_priv 14-27 n_inelig_priv 30-45 n_elig_no_priv 47-59 n_elig_priv 62-75 using "D:\Dropbox\My Research\ACS\Logs\private5raw.log"

//This generates the state codes so that they exist on each line of the data and drops the instances that don't have values

gen y=.  
forval i = 1/`=_N' {  
local j = `i' - 1  
quietly replace y=st[`i'] in `i' if st[`i']!=.  
quietly replace y=y[`j'] in `i' if st[`i']==.  
}
drop if p5==.  
replace st=y  
drop y  
save "D:\Dropbox\My Research\ACS\Block Data\private5raw.dta", replace

clear //clear the memory to get the info we put in the log files

210
// Input the data back into stat using fixed values
// This is for blocks of 3 non-weighted

infix st 1-3 p3 6-10 n_inelig_no_priv 14-27 n_inelig_priv 30-45 n_elig_no_priv 47-59 n_elig_priv 62-75 using "D:\Dropbox\My Research\ACS\Logs\private3raw.log"

// This generates the state codes so that they exist on each line of the data and drops the instances that don't have values

gen y=.
forval i = 1/=_N' {
    local j = `i' - 1
    quietly replace y=st[`i'] in `i' if st[`i']!=.
    quietly replace y=y[`j'] in `i' if st[`i']==.
}
drop if p3==.
replace st=y
drop y
save "D:\Dropbox\My Research\ACS\Block Data\private3raw.dta", replace

clear // clear the memory to get the info we put in the log files

// Input the data back into stat using fixed values
// This is for blocks of 1 non-weighted

infix st 1-3 p1 6-10 n_inelig_no_priv 14-27 n_inelig_priv 30-45 n_elig_no_priv 47-59 n_elig_priv 62-75 using "D:\Dropbox\My Research\ACS\Logs\private1raw.log"

// This generates the state codes so that they exist on each line of the data and drops the instances that don't have values

gen y=.
forval i = 1/=_N' {
    local j = `i' - 1
    quietly replace y=st[`i'] in `i' if st[`i']!=.
    quietly replace y=y[`j'] in `i' if st[`i']==.
}
drop if p1==.
replace st=y
drop y
save "D:\Dropbox\My Research\ACS\Block Data\private1raw.dta", replace

clear //clear the memory

//merge the weighted and raw files together
use "D:\Dropbox\My Research\ACS\Block Data\private10.dta"
merge 1:1 st p10 using "D:\Dropbox\My Research\ACS\Block Data\private10raw.dta"
drop _merge
save "D:\Dropbox\My Research\ACS\Block Data\private10combined.dta", replace
clear
use "D:\Dropbox\My Research\ACS\Block Data\private5.dta"
merge 1:1 st p5 using "D:\Dropbox\My Research\ACS\Block Data\private5raw.dta"
drop _merge
save "D:\Dropbox\My Research\ACS\Block Data\private5combined.dta", replace
clear
use "D:\Dropbox\My Research\ACS\Block Data\private3.dta"
merge 1:1 st p3 using "D:\Dropbox\My Research\ACS\Block Data\private3raw.dta"
drop _merge
save "D:\Dropbox\My Research\ACS\Block Data\private3combined.dta", replace
clear
use "D:\Dropbox\My Research\ACS\Block Data\private1.dta"
merge 1:1 st p1 using "D:\Dropbox\My Research\ACS\Block Data\private1raw.dta"
drop _merge
save "D:\Dropbox\My Research\ACS\Block Data\private1combined.dta", replace
clear

//Here is where we merge the data with the crosswalk file, center it around the crosswalk and calculate the percentage of children insured (eligible and ineligible)

//here we will calculate the total number of children eligible with private insurance
clear
use "D:\Dropbox\My Research\ACS\Block Data\private10combined.dta"
append using "D:\Dropbox\My Research\ACS\Block Data\private5combined.dta"
append using "D:\Dropbox\My Research\ACS\Block Data\private3combined.dta"
append using "D:\Dropbox\My Research\ACS\Block Data\private1combined.dta"
egen total = rsum (inelig_no_priv inelig_priv elig_no_priv elig_priv)
egen total_priv = rsum (inelig_priv elig_priv)
gen per_total = total_priv / total
egen total_inelig = rsum (inelig_no_priv inelig_priv)
gen per_inelig = inelig_priv/total_inelig
egen total_elig = rsum (elig_no_priv elig_priv)
gen per_elig = elig_priv/total_elig
egen rawtotal = rsum (n_inelig_no_priv n_inelig_priv n_elig_no_priv n_elig_priv)
gen weightedtotal = total
drop total total_priv total_inelig total_elig

//this merges the data with the crosswalk
merge m:1 st using "D:\Dropbox\My Research\ACS\Crosswalks\2010_Crosswalk.dta"
drop _merge

//center around the cutoff
gen p10c = p10-cutoff
gen p5c = p5-cutoff
gen p3c = p3-cutoff
gen p1c = p1-cutoff

//percentile for each level (is equal to p1, p3, etc at each level)
egen p = rsum (p1 p3 p5 p10)

//centered for each level
gen pc = p - cutoff

//this moves the p up one level so, for example, that the level covering from -10 of the cutoff to 0 of the cutoff is centered at 0, not -10
replace p10c = p10c+10 if pc<0
replace p5c = p5c+5 if pc<0
replace p3c = p3c+3 if pc<0
replace p1c = p1c+1 if pc<0

//this centers everything around zero if the cutoff level is not a multiple of block (for example, Oklahoma cutoff is 185 and was centered around 5 before this centers it all around 0)
replace p10c = p10c-mod (p10c,10)
replace p5c = p5c-mod (p5c,5)
replace p3c = p3c-mod (p3c,3)

//whether the child is ineligible for public programs
gen ineligible=0
replace ineligible=1 if pc>=0
label variable ineligible "Estimated Crowd-Out" //this is for the regression output with estout

//whether child is eligible
gen eligible=abs (ineligible-1)

//generate ineligible*pc - the interaction term
gen interaction = ineligible*pc

//generate values for a quadratic fit
gen p10c2 = p10c*p10c
gen p5c2 = p5c*p5c
gen p3c2 = p3c*p3c
gen p1c2 = p1c*p1c
egen pc2 = rsum (p1c2 p3c2 p5c2 p10c2)
gen interaction2 = ineligible*pc2

save "D:\Dropbox\My Research\ACS\Block Data\private_per.dta", replace

//this generates the results for p10
eststo clear

quietly regress per_total p10c interaction ineligible if  pc>-150 & pc<150 & rawtotal>8 & st==1
eststo state1, title (Alabama)
quietly regress per_total p10c interaction ineligible if  pc>-150 & pc<150 & rawtotal>8 & st==2
eststo state2, title (Alaska)
quietly regress per_total p10c interaction ineligible if  pc>-150 & pc<150 & rawtotal>8 & st==4
eststo state4, title (Arizona)
quietly regress per_total p10c interaction ineligible if  pc>-150 & pc<150 & rawtotal>8 & st==5
eststo state5, title (Arkansas)
quietly regress per_total p10c interaction ineligible if  pc>-150 & pc<150 & rawtotal>8 & st==6
eststo state6, title (California)
quietly regress per_total p10c interaction ineligible if  pc>-150 & pc<150 & rawtotal>8 & st==8
eststo state8, title (Colorado)
quietly regress per_total p10c interaction ineligible if  pc>-150 & pc<150 & rawtotal>8 & st==9
eststo state9, title (Connecticut)
quietly regress per_total p10c interaction ineligible if  pc>-150 & pc<150 & rawtotal>8 & st==10
eststo state10, title (Delaware)
quietly regress per_total p10c interaction ineligible if  pc>-150 & pc<150 & rawtotal>8 & st==11
eststo state11, title (DC)
quietly regress per_total p10c interaction ineligible if pc>=150 & pc<150 & rawtotal>8 & st==12
eststo state12, title (Florida)
quietly regress per_total p10c interaction ineligible if pc>=150 & pc<150 & rawtotal>8 & st==13
eststo state13, title (Georgia)
quietly regress per_total p10c interaction ineligible if pc>=150 & pc<150 & rawtotal>8 & st==15
eststo state15, title (Hawaii)
quietly regress per_total p10c interaction ineligible if pc>=150 & pc<150 & rawtotal>8 & st==16
eststo state16, title (Idaho)
quietly regress per_total p10c interaction ineligible if pc>=150 & pc<150 & rawtotal>8 & st==17
eststo state17, title (Illinois)
quietly regress per_total p10c interaction ineligible if pc>=150 & pc<150 & rawtotal>8 & st==18
eststo state18, title (Indiana)
quietly regress per_total p10c interaction ineligible if pc>=150 & pc<150 & rawtotal>8 & st==19
eststo state19, title (Iowa)
quietly regress per_total p10c interaction ineligible if pc>=150 & pc<150 & rawtotal>8 & st==20
eststo state20, title (Kansas)
quietly regress per_total p10c interaction ineligible if pc>=150 & pc<150 & rawtotal>8 & st==21
eststo state21, title (Kentucky)
quietly regress per_total p10c interaction ineligible if pc>=150 & pc<150 & rawtotal>8 & st==22
eststo state22, title (Louisiana)
quietly regress per_total p10c interaction ineligible if pc>=150 & pc<150 & rawtotal>8 & st==23
eststo state23, title (Maine)
quietly regress per_total p10c interaction ineligible if pc>=150 & pc<150 & rawtotal>8 & st==24
eststo state24, title (Maryland)
quietly regress per_total p10c interaction ineligible if pc>=150 & pc<150 & rawtotal>8 & st==25
eststo state25, title (Massachusetts)
quietly regress per_total p10c interaction ineligible if pc>=150 & pc<150 & rawtotal>8 & st==26
eststo state26, title (Michigan)
quietly regress per_total p10c interaction ineligible if pc>=150 & pc<150 & rawtotal>8 & st==27
eststo state27, title (Minnesota)
quietly regress per_total p10c interaction ineligible if pc>=150 & pc<150 & rawtotal>8 & st==28
eststo state28, title (Mississippi)
quietly regress per_total p10c interaction ineligible if pc>=150 & pc<150 & rawtotal>8 & st==29
eststo state29, title (Missouri)
quietly regress per_total p10c interaction ineligible if pc>=150 & pc<150 & rawtotal>8 & st==30
eststo state30, title (Montana)
quietly regress per_total p10c interaction ineligible if pc>=150 & pc<150 & rawtotal>8 & st==31
eststo state31, title (Nebraska)
quietly regress per_total p10c interaction ineligible if pc>=150 & pc<150 & rawtotal>8 & st==32
eststo state32, title (Nevada)
quietly regress per_total p10c interaction ineligible if pc>=150 & pc<150 & rawtotal>8 & st==33
eststo state33, title (New Hampshire)
quietly regress per_total p10c interaction ineligible if pc>=150 & pc<150 & rawtotal>8 & st==34
eststo state34, title (New Jersey)
quietly regress per_total p10c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==35
eststo state35, title (New Mexico)
quietly regress per_total p10c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==36
eststo state36, title (New York)
quietly regress per_total p10c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==37
eststo state37, title (North Carolina)
quietly regress per_total p10c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==38
eststo state38, title (North Dakota)
quietly regress per_total p10c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==39
eststo state39, title (Ohio)
quietly regress per_total p10c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==40
eststo state40, title (Oklahoma)
quietly regress per_total p10c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==41
eststo state41, title (Oregon)
quietly regress per_total p10c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==42
eststo state42, title (Pennsylvania)
quietly regress per_total p10c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==44
eststo state44, title (Rhode Island)
quietly regress per_total p10c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==45
eststo state45, title (South Carolina)
quietly regress per_total p10c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==46
eststo state46, title (South Dakota)
quietly regress per_total p10c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==47
eststo state47, title (Tennessee)
quietly regress per_total p10c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==48
eststo state48, title (Texas)
quietly regress per_total p10c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==49
eststo state49, title (Utah)
quietly regress per_total p10c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==50
eststo state50, title (Vermont)
quietly regress per_total p10c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==51
eststo state51, title (Virginia)
quietly regress per_total p10c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==53
eststo state53, title (Washington)
quietly regress per_total p10c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==54
eststo state54, title (West Virginia)
quietly regress per_total p10c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==55
eststo state55, title (Wisconsin)
quietly regress per_total p10c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==56
eststo state56, title (Wyoming)
estout using "D:\Dropbox\My Research\ACS\Results\p10_disregard.tsv", replace label cells (b set p) stat (r2 r2_a N)
eststo clear

/*
//this generates the results for p5
eststo clear

quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==1
eststo state1, title (Alabama)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==2
eststo state2, title (Alaska)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==4
eststo state4, title (Arizona)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==5
eststo state5, title (Arkansas)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==6
eststo state6, title (California)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==8
eststo state8, title (Colorado)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==9
eststo state9, title (connecticut)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==10
eststo state10, title (Delaware)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==11
eststo state11, title (DC)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==12
eststo state12, title (Florida)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==13
eststo state13, title (Georgia)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==15
eststo state15, title (Hawaii)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==16
eststo state16, title (Idaho)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==17
eststo state17, title (Illinois)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==18
eststo state18, title (Indiana)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==19
eststo state19, title (Iowa)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==20
eststo state20, title (Kansas)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==21
eststo state21, title (Kentucky)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==22
eststo state22, title (Louisiana)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==23
eststo state23, title (Maine)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==24
eststo state24, title (Maryland)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==25
eststo state25, title (Massachusetts)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==26
eststo state26, title (Michigan)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==27
eststo state27, title (Minnesota)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==28
eststo state28, title (Mississippi)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==29
eststo state29, title (Missouri)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==30
eststo state30, title (Montana)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==31
eststo state31, title (Nebraska)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==32
eststo state32, title (Nevada)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==33
eststo state33, title (New Hampshire)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==34
eststo state34, title (New Jersey)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==35
eststo state35, title (New Mexico)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==36
eststo state36, title (New York)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==37
eststo state37, title (North Carolina)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==38
eststo state38, title (North Dakota)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==39
eststo state39, title (Ohio)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==40
eststo state40, title (Oklahoma)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==41
eststo state41, title (Oregon)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==42
eststo state42, title (Pennsylvania)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==44
eststo state44, title (Rhode Island)

218
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==45
eststo state45, title (South Carolina)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==46
eststo state46, title (South Dakota)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==47
eststo state47, title (Tennessee)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==48
eststo state48, title (Texas)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==49
eststo state49, title (Utah)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==50
eststo state50, title (Vermont)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==51
eststo state51, title (Virginia)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==53
eststo state53, title (Washington)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==54
eststo state54, title (West Virginia)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==55
eststo state55, title (Wisconsin)
quietly regress per_total p5c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==56
eststo state56, title (Wyoming)

estout using "D:\Dropbox\My Research\ACS\Results\p5.tsv", replace label cells (b se t p) stat (r2 r2_a N)
eststo clear

//this generates the results for p3
eststo clear

quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==1
eststo state1, title (Alabama)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==2
eststo state2, title (Alaska)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==4
eststo state4, title (Arizona)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==5
eststo state5, title (Arkansas)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==6
eststo state6, title (California)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==8
eststo state8, title (Colorado)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==9
eststo state9, title (Connecticut)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==10
eststo state10, title (Delaware)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==11
eststo state11, title (DC)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==12
eststo state12, title (Florida)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==13
eststo state13, title (Georgia)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==15
eststo state15, title (Hawai)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==16
eststo state16, title (Idaho)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==17
eststo state17, title (Illinois)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==18
eststo state18, title (Indiana)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==19
eststo state19, title (Iowa)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==20
eststo state20, title (Kansas)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==21
eststo state21, title (Kentucky)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==22
eststo state22, title (Louisiana)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==23
eststo state23, title (Maine)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==24
eststo state24, title (Maryland)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==25
eststo state25, title (Massachusetts)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==26
eststo state26, title (Michigan)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==27
eststo state27, title (Minnesota)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==28
eststo state28, title (Mississippi)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==29
eststo state29, title (Missouri)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==30
eststo state30, title (Montana)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==31
eststo state31, title (Nebraska)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==32
eststo state32, title (Nevada)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==33
eststo state33, title (New Hampshire)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==34
eststo state34, title (New Jersey)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==35
eststo state35, title (New Mexico)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==36
eststo state36, title (New York)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==37
eststo state37, title (North Carolina)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==38
eststo state38, title (North Dakota)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==39
eststo state39, title (Ohio)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==40
eststo state40, title (Oklahoma)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==41
eststo state41, title (Oregon)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==42
eststo state42, title (Pennsylvania)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==43
eststo state43, title (Rhode Island)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==44
eststo state44, title (South Carolina)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==45
eststo state45, title (South Dakota)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==46
eststo state46, title (Texas)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==47
eststo state47, title (Tennessee)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==48
eststo state48, title (Virginia)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==49
eststo state49, title (Washington)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==50
eststo state50, title (West Virginia)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==51
eststo state51, title (Wisconsin)
quietly regress per_total p3c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==52
eststo state52, title (Wyoming)
estout using "D:\Dropbox\My Research\ACS\Results\p3.tsv", replace label cells (b se t p) stat (r2 r2_a N)
eststo clear

//this generates the results for p1

eststo clear

quietly regress per_total p1c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==1
eststo state1, title (Alabama)
quietly regress per_total p1c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==2
eststo state2, title (Alaska)
quietly regress per_total p1c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==4
eststo state4, title (Arizona)
quietly regress per_total p1c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==5
eststo state5, title (Arkansas)
quietly regress per_total p1c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==6
eststo state6, title (California)
quietly regress per_total p1c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==8
eststo state8, title (Colorado)
quietly regress per_total p1c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==9
eststo state9, title (connecticut)
quietly regress per_total p1c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==10
eststo state10, title (Delaware)
//quietly regress per_total p1c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==11
//eststo state11, title (DC)
quietly regress per_total p1c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==12
eststo state12, title (Florida)
quietly regress per_total p1c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==13
eststo state13, title (Georgia)
quietly regress per_total p1c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==15
eststo state15, title (Hawaii)
quietly regress per_total p1c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==16
eststo state16, title (Idaho)
quietly regress per_total p1c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==17
eststo state17, title (Illinois)
quietly regress per_total p1c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==18
eststo state18, title (Indiana)
quietly regress per_total p1c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==19
eststo state19, title (Iowa)
quietly regress per_total p1c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==20
eststo state20, title (Kansas)
quietly regress per_total p1c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==21
eststo state21, title (Kentucky)
quietly regress per_total p1c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==22
eststo state22, title (Louisiana)
quietly regress per_total p1c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==23
eststo state23, title (Maine)
quietly regress per_total p1c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==24
eststo state24, title (Maryland)
quietly regress per_total p1c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==25
eststo state25, title (Massachusetts)
quietly regress per_total p1c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==26
eststo state26, title (Michigan)
quietly regress per_total p1c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==27
eststo state27, title (Minnesota)
quietly regress per_total p1c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==28
eststo state28, title (Mississippi)
quietly regress per_total p1c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==29
eststo state29, title (Missouri)
quietly regress per_total p1c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==30
eststo state30, title (Montana)
quietly regress per_total p1c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==31
eststo state31, title (Nebraska)
quietly regress per_total p1c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==32
eststo state32, title (Nevada)
quietly regress per_total p1c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==33
eststo state33, title (New Hampshire)
quietly regress per_total p1c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==34
eststo state34, title (New Jersey)
quietly regress per_total p1c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==35
eststo state35, title (New Mexico)
quietly regress per_total p1c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==36
eststo state36, title (New York)
quietly regress per_total p1c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==37
eststo state37, title (North Carolina)
quietly regress per_total p1c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==38
eststo state38, title (North Dakota)
quietly regress per_total p1c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==39
eststo state39, title (Ohio)
quietly regress per_total p1c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==40
eststo state40, title (Oklahoma)
quietly regress per_total p1c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==41
eststo state41, title (Oregon)
quietly regress per_total p1c interaction ineligible if pc>150 & pc<150 & rawtotal>8 & st==42
eststo state42, title (Pennsylvania)
quietly regress per_total p1c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==44
eststo state44, title (Rhode Island)
quietly regress per_total p1c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==45
eststo state45, title (South Carolina)
quietly regress per_total p1c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==46
eststo state46, title (South Dakota)
quietly regress per_total p1c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==47
eststo state47, title (Tennessee)
quietly regress per_total p1c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==48
eststo state48, title (Texas)
quietly regress per_total p1c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==49
eststo state49, title (Utah)
quietly regress per_total p1c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==50
eststo state50, title (Vermont)
quietly regress per_total p1c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==51
eststo state51, title (Virginia)
quietly regress per_total p1c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==53
eststo state53, title (Washington)
quietly regress per_total p1c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==54
eststo state54, title (West Virginia)
quietly regress per_total p1c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==55
eststo state55, title (Wisconsin)
quietly regress per_total p1c interaction ineligible if pc>-150 & pc<150 & rawtotal>8 & st==56
eststo state56, title (Wyoming)

estout using "D:\Dropbox\My Research\ACS\Results\p1.tsv", replace label cells (b se t p) stat (r2 r2_a N)
eststo clear
*/
//do "D:\Dropbox\My Research\ACS\Do Files\2011-11-9 - ACS_Crowd-Out_Regression_Discontinuity_quadratic.do"
//do "D:\Dropbox\My Research\ACS\Do Files\2011-11-9 - ACS_Crowd-Out_Regression_Discontinuity_quadratic_no_quad_interaction.do"
clear //clear the memory

set more on //pause the output again
Appendix E – Ohio Crowd-Out Code

/* David Muhlestein
   Started: October 25, 2011
   October 27, 2011
   November 1, 2011
   November 8, 2011
   November 9, 2011
   November 10, 2011
   November 14, 2011
   December 6, 2011
   January 18, 2012  I quit dropping data for blocks less than 50% of the FPL (it only affected 4
                     states and it made minimal difference)
   March 27, 2012  This is updated to use Ohio Family Health Survey Data and calculate this just
                   for Ohio and is for the year 2010
   September 20, 2012 This is updated for the new computer
   February 8, 2013 This is using the new 2012 survey data.
   February 14, 2013 - Updated to Include population shift test
   This takes data and calculates the number of children who have private insurance in different
   blocks of income levels
*/

clear

use "D:\data\Ohio Family Health Survey\2012 (OMAS)\2012 OMAS Data
Set\omas_finaldata_research_v11.dta" //The file to use

drop if i_type_c==. //Drop entries where there is not information about a child's insurance
            status (this drops all adults)
gen cutoff=200 //this is the SCHIP/Medicaid eligibility threshold
gen st=39 //this is only Ohio data
gen pwgtp=round (wt_c) //this is the weight for children; note: I round to the nearest 1

gen povlev=round (100* (h85_value_imp/ (11170+ ( (h84_imp-1)*3960)))) //divides the
income (imputed income) by the federal poverty level for the (imputed)
//family size (10830 + (x-1)*3740) and then multiplies by 100; this is based on 2012 FPL
guidelines: http://aspe.hhs.gov/poverty/12poverty.shtml

// drop if h84==. //This drops children where we don’t know the family size
// drop if h85y_1==. //This drops children whose income is unknown
// gen povlev=round (100* (h85y_1/ (10830+ (h84-1)*3740)))) //does the same as above but
uses actual, not imputed, values and drops the values with missing data

gen eligible=0
replace eligible=1 if povlev<=cutoff //the child is eligible if they are below the cutoff;

label variable eligible "Whether the child is eligible for Medicaid or SCHIP; 1=eligible,
0=ineligible"
gen ineligible=abs (eligible-1) //opposite of eligible
label variable ineligible "Whether the child is ineligible for Medicaid or SCHIP; 0=eligible,
1=ineligible"

//generating blocks of poverty levels

sort povlev
gen p1=povlev-mod (povlev,1)
gen p3=povlev-mod (povlev,3)
gen p5=povlev-mod (povlev,5)
gen p10=povlev-mod (povlev,10)
gen p20=povlev-mod (povlev,20)
gen p100=povlev-mod (povlev,100)

//generate whether they have private coverage
GEN private=0
replace private=1 if i_type_c==4 | i_type_c==5 | i_type_c==6 | i_type_c==7 //04-JOB-BASED
COVERAGE; 05-DIRECTLY PURCHASED; 06-OTHER; 07-INSURED TYPE UNKNOWN
//NOTE: I'M NOT SURE WHETHER WE SHOULD PUT 06-OTHER AS INSURED OR IF WE SHOULD
DROP THEM

226
gen public=0
replace public=1 if i_type_c==1 | i_type_c==2 | i_type_c==3 //01-Medicaid and Medicare; 02-Medicaid, no Medicare; 03-Medicare, no Medicaid

gen uninsured=0
replace uninsured=1 if i_type_c==8 //08-Uninsured

label variable private "Whether the child has any insurance coverage; 0=no, 1=yes"
label variable public "Whether the child has any public insurance coverage; 0=no, 1=yes"
label variable uninsured "Whether the child is uninsured; 0=no, 1=yes"

gen insurance = 1
replace insurance = 2 if public
replace insurance = 3 if uninsured

label variable insurance "The type of insurance the child has: 1=private; 2=public; 3=uninsured"

//For each level of blocks we create a stata log file with all the data stored and then convert it to a .log file
//These contain the weighted number of children who have private insurance in each poverty block
set more off //don't pause the output

log using "D:\Dropbox\My Research\OFHS\Logs\private10.smcl", replace
table p10 private eligible [fw=pwgtp], csep (5) contents (freq) format (%12.0g) by (st) missing
log close
translate "D:\Dropbox\My Research\OFHS\Logs\private10.smcl" "D:\Dropbox\My Research\OFHS\Logs\private10.log", replace linesize (179) translator (smcl2log)

log using "D:\Dropbox\My Research\OFHS\Logs\private5.smcl", replace
table p5 private eligible [fw=pwgtp], csep (5) contents (freq) format (%12.0g) by (st) missing
log close
translate "D:\Dropbox\My Research\OFHS\Logs\private5.smcl" "D:\Dropbox\My Research\OFHS\Logs\private5.log", replace linesize (179) translator (smcl2log)

log using "D:\Dropbox\My Research\OFHS\Logs\private3.smcl", replace
table p3 private eligible [fw=pwgtp], csep (5) contents (freq) format (%12.0g) by (st) missing
log close

227
//For each level of blocks we create a stata log file with all the data stored and then convert it to a .log file
//These contain the raw (non-weighted) number of children who have private insurance in each poverty block

set more off //don't pause the output

log using "D:\Dropbox\My Research\OFHS\Logs\private1raw.smcl", replace
table p1 private eligible [fw=pwgtp], csep (5) contents (freq) format (%12.0g) by (st) missing
log close
translate "D:\Dropbox\My Research\OFHS\Logs\private1raw.smcl" "D:\Dropbox\My Research\OFHS\Logs\private1raw.log", replace

log using "D:\Dropbox\My Research\OFHS\Logs\private5raw.smcl", replace
table p5 private eligible, csep (5) contents (freq) format (%12.0g) by (st) missing
log close
translate "D:\Dropbox\My Research\OFHS\Logs\private5raw.smcl" "D:\Dropbox\My Research\OFHS\Logs\private5raw.log", replace

log using "D:\Dropbox\My Research\OFHS\Logs\private3raw.smcl", replace
table p3 private eligible, csep (5) contents (freq) format (%12.0g) by (st) missing
log close
translate "D:\Dropbox\My Research\OFHS\Logs\private3raw.smcl" "D:\Dropbox\My Research\OFHS\Logs\private3raw.log", replace

log using "D:\Dropbox\My Research\OFHS\Logs\private1raw.smcl", replace
table p1 private eligible, csep (5) contents (freq) format (%12.0g) by (st) missing
log close
translate "D:\Dropbox\My Research\OFHS\Logs\private1raw.smcl" "D:\Dropbox\My Research\OFHS\Logs\private1raw.log", replace

clear //clear the memory to get the info we put in the log files

//Input the data back into stat using fixed values
//This is for blocks of 10

infix st 1-2 p10 3-10 ineligible_no_priv 12-25 ineligible_priv 26-43 ineligible_no_priv 44-59 ineligible_priv 60-75 using "D:\Dropbox\My Research\OFHS\Logs\private10.log"

//This generates the state codes so that they exist on each line of the data and drops the instances that don't have values

gen y=.  
forval i = 1/`=_N' {
    local j = `i' - 1
    quietly replace y=st[`i'] in `i' if st[`i']!=.
    quietly replace y=y[`j'] in `i' if st[`i']==.
}
drop if p10==.
replace st=y
drop y
save "D:\Dropbox\My Research\OFHS\Block Data\private10.dta", replace

clear //clear the memory to get the info we put in the log files

//Input the data back into mat using fixed values
//This is for blocks of 5

infix st 1-2 p5 3-10 ineligible_no_priv 12-25 ineligible_priv 26-43 ineligible_no_priv 44-59 ineligible_priv 60-75 using "D:\Dropbox\My Research\OFHS\Logs\private5.log"

//This generates the state codes so that they exist on each line of the data and drops the instances that don't have values

gen y=.  
forval i = 1/`=_N' {
    local j = `i' - 1
    quietly replace y=st[`i'] in `i' if st[`i']!=.
    quietly replace y=y[`j'] in `i' if st[`i']==.
}
drop if p5==.
replace st=y
drop y
save "D:\Dropbox\My Research\OFHS\Block Data\private5.dta", replace

clear //clear the memory to get the info we put in the log files

//Input the data back into stat using fixed values
//This is for blocks of 3

infix st 1-2 p3 3-10 ineligible_no_priv 12-25 ineligible_priv 26-43 ineligible_no_priv 44-59 ineligible_priv 60-75 using "D:\Dropbox\My Research\OFHS\Logs\private3.log"

//This generates the state codes so that they exist on each line of the data and drops the instances that don't have values

gen y=. forval i = 1/`=_N' { local j = `i' - 1 quietly replace y=st[`i'] in `i' if st[`i']!=. quietly replace y=y[`j'] in `i' if st[`i']==. }
drop if p3==. replace st=y drop y save "D:\Dropbox\My Research\OFHS\Block Data\private3.dta", replace

clear //clear the memory to get the info we put in the log files

//Input the data back into stat using fixed values
//This is for blocks of 1

infix st 1-2 p1 3-10 ineligible_no_priv 12-25 ineligible_priv 26-43 ineligible_no_priv 44-59 ineligible_priv 60-75 using "D:\Dropbox\My Research\OFHS\Logs\private1.log"

//This generates the state codes so that they exist on each line of the data and drops the instances that don't have values

gen y=. forval i = 1/`=_N' { local j = `i' - 1 quietly replace y=st[`i'] in `i' if st[`i']!=. quietly replace y=y[`j'] in `i' if st[`i']==. }
drop if p1==.
replace st=y
drop y
save "D:\Dropbox\My Research\OFHS\Block Data\private1.dta", replace

clear //clear the memory to get the info we put in the log files

//Input the data back into stat using fixed values
//This is for blocks of 10 non-weighted

infix st 1-2 p10 3-10 n_inelig_no_priv 12-25 n_inelig_priv 26-43 n_elig_no_priv 44-59 n_elig_priv 60-75 using "D:\Dropbox\My Research\OFHS\Logs\private10raw.log"

//This generates the state codes so that they exist on each line of the data and drops the instances that don't have values

gen y=.
forval i = 1/`=_N' {
local j = `i' - 1
quietly replace y=st[`i'] in `i' if st[`i']!=.
quietly replace y=y[`j'] in `i' if st[`i']==.
}

drop if p10==.
replace st=y
drop y
save "D:\Dropbox\My Research\OFHS\Block Data\private10raw.dta", replace

clear //clear the memory to get the info we put in the log files

//Input the data back into stat using fixed values
//This is for blocks of 5 non-weighted

infix st 1-2 p5 3-10 n_inelig_no_priv 12-25 n_inelig_priv 26-43 n_elig_no_priv 44-59 n_elig_priv 60-75 using "D:\Dropbox\My Research\OFHS\Logs\private5raw.log"

//This generates the state codes so that they exist on each line of the data and drops the instances that don't have values
gen y=.
forval i = 1/_N' {
local j = `i' - 1
quietly replace y=st[`i'] in `i' if st[`i']!=.
quietly replace y=y[`j'] in `i' if st[`i']==.
}
drop if p5==.
replace st=y
drop y
save "D:\Dropbox\My Research\OFHS\Block Data\private5raw.dta", replace

clear //clear the memory to get the info we put in the log files

//Input the data back into stat using fixed values
//This is for blocks of 3 non-weighted

infix st 1-2 p3 3-10 n_inelig_no_priv 12-25 n_inelig_priv 26-43 n_elig_no_priv 44-59 n_elig_priv 60-75 using "D:\Dropbox\My Research\OFHS\Logs\private3raw.log"

//This generates the state codes so that they exist on each line of the data and drops the instances that don't have values

gen y=.
forval i = 1/_N' {
local j = `i' - 1
quietly replace y=st[`i'] in `i' if st[`i']!=.
quietly replace y=y[`j'] in `i' if st[`i']==.
}
drop if p3==.
replace st=y
drop y
save "D:\Dropbox\My Research\OFHS\Block Data\private3raw.dta", replace

clear //clear the memory to get the info we put in the log files

//Input the data back into stat using fixed values
//This is for blocks of 1 non-weighted
infix st 1-2 p1 3-10 n_inelig_no_priv 12-25 n_inelig_priv 26-43 n_elig_no_priv 44-59 n_elig_priv 60-75 using "D:\Dropbox\My Research\OFHS\Logs\private1raw.log"

//This generates the state codes so that they exist on each line of the data and drops the instances that don't have values

gen y=.
forval i = 1/`=_N' {
local j = `i' - 1
quietly replace y=st[`i'] in `i' if st[`i']!=.
quietly replace y=y[`j'] in `i' if st[`i']==.
}
drop if p1==.
replace st=y
drop y
save "D:\Dropbox\My Research\OFHS\Block Data\private1raw.dta", replace

clear //clear the memory

//merge the weighted and raw files together
use "D:\Dropbox\My Research\OFHS\Block Data\private10.dta"
merge 1:1 st p10 using "D:\Dropbox\My Research\OFHS\Block Data\private10raw.dta"
drop _merge
save "D:\Dropbox\My Research\OFHS\Block Data\private10combined.dta", replace
clear
use "D:\Dropbox\My Research\OFHS\Block Data\private5.dta"
merge 1:1 st p5 using "D:\Dropbox\My Research\OFHS\Block Data\private5raw.dta"
drop _merge
save "D:\Dropbox\My Research\OFHS\Block Data\private5combined.dta", replace
clear
use "D:\Dropbox\My Research\OFHS\Block Data\private3.dta"
merge 1:1 st p3 using "D:\Dropbox\My Research\OFHS\Block Data\private3raw.dta"
drop _merge
save "D:\Dropbox\My Research\OFHS\Block Data\private3combined.dta", replace
clear
use "D:\Dropbox\My Research\OFHS\Block Data\private1.dta"
merge 1:1 st p1 using "D:\Dropbox\My Research\OFHS\Block Data\private1raw.dta"
drop _merge
save "D:\Dropbox\My Research\OFHS\Block Data\private1combined.dta", replace
clear
Here is where we merge the data with the crosswalk file, center it around the crosswalk and calculate the percentage of children insured (eligible and ineligible)

// here we will calculate the total number of children eligible with private insurance
clear
use "D:\Dropbox\My Research\OFHS\Block Data\private1combined.dta"
append using "D:\Dropbox\My Research\OFHS\Block Data\private5combined.dta"
append using "D:\Dropbox\My Research\OFHS\Block Data\private3combined.dta"
append using "D:\Dropbox\My Research\OFHS\Block Data\private1combined.dta"

egen total = rsum (inelig_no_priv inelig_priv elig_no_priv elig_priv)
egen total_priv = rsum (inelig_priv elig_priv)
gen per_total = total_priv / total
egen total_inelig = rsum (inelig_no_priv inelig_priv)
gen per_inelig = inelig_priv/total_inelig
egen total_elig = rsum (elig_no_priv elig_priv)
gen per_elig = elig_priv/total_elig
gen rawtotal = rsum (n_inelig_no_priv n_inelig_priv n_elig_no_priv n_elig_priv)
gen weightedtotal = total
drop total total_priv total_inelig total_elig

save "D:\Dropbox\My Research\OFHS\Block Data\private_per_12.dta", replace

//this merges the data with the crosswalk
//merge m:1 st using "C:\Users\David Muhlestein\Desktop\OFHS\2010_Crosswalk.dta"
//drop _merge

gen cutoff=200
//center around the cutoff
gen p10c = p10-cutoff
gen p5c = p5-cutoff
gen p3c = p3-cutoff
gen p1c = p1-cutoff

//percentile for each level (is equal to p1, p3, etc at each level)
egen p = rsum (p1 p3 p5 p10)
//centered for each level
gen pc = p - cutoff
//this moves the p up one level so, for example, that the level covering from -10 of the cutoff to 0 of the cutoff is centered at 0, not -10
replace p10c = p10c+10 if pc<0
replace p5c = p5c+5 if pc<0
replace p3c = p3c+3 if pc<0
replace p1c = p1c+1 if pc<0

//this centers everything around zero if the cutoff level is not a multiple of block (for example, Oklahoma cutoff is 185 and was centered around 5 before this centers it all around 0)
replace p10c = p10c-mod (p10c,10)
replace p5c = p5c-mod (p5c,5)

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replace p3c = p3c-mod (p3c,3)

//whether the child is ineligible for public programs
gen ineligible=0
replace ineligible=1 if pc>=0
label variable ineligible "Estimated Crowd-Out" //this is for the regression output with estout

//whether child is eligible
gen eligible=abs (ineligible-1)

//generate ineligible*pc - the interaction term
gen interaction = ineligible*pc

//generate values for a quadratic fit
gen p10c2 = p10c*p10c
gen p5c2 = p5c*p5c
gen p3c2 = p3c*p3c
gen p1c2 = p1c*p1c
egen pc2 = rsum (p1c2 p3c2 p5c2 p10c2)
gen interaction2 = ineligible*pc2

gen year=2012 //the year of the survey
gen percent=per_total*100 //so we can show the percent insured on a scale of 1-100
save "D:\Dropbox\My Research\OFHS\Block Data\private_per_12.dta", replace

eststo clear

quietly regress per_total p10c interaction ineligible if  pc>-150 & pc<150 & rawtotal>8 & st==39
eststo state39_10, title ("Ohio - 10")
quietly regress per_total p5c interaction ineligible if  pc>-150 & pc<150 & rawtotal>8 & st==39
eststo state39_5, title ("Ohio - 5")
quietly regress per_total p3c interaction ineligible if  pc>-150 & pc<150 & rawtotal>8 & st==39
eststo state39_3, title ("Ohio - 3")
quietly regress per_total p1c interaction ineligible if  pc>-150 & pc<150 & rawtotal>8 & st==39
eststo state39_1, title ("Ohio - 1")

estout using "D:\Dropbox\My Research\OFHS\Results\results_12 (all).tsv", replace label cells (b se t p ci) stat (r2 r2_a N)
eststo clear
eststo clear

quietly regress weightedtotal p10c interaction ineligible if pc>-50 & pc<50 & rawtotal>8 & st==39
eststo state39_10, title ("Ohio - 10")
quietly regress weightedtotal p5c interaction ineligible if pc>-50 & pc<50 & rawtotal>8 & st==39
eststo state39_5, title ("Ohio - 5")
quietly regress weightedtotal p3c interaction ineligible if pc>-50 & pc<50 & rawtotal>8 & st==39
eststo state39_3, title ("Ohio - 3")
quietly regress weightedtotal p1c interaction ineligible if pc>-50 & pc<50 & rawtotal>8 & st==39
eststo state39_1, title ("Ohio - 1")

estout using "D:\Dropbox\My Research\OFHS\Results\results_12 (population shift).tsv", replace label cells (b se t p ci) stat (r2 r2_a N)
eststo clear

clear //clear the memory

set more on //pause the output again
local size 1200 //this is the size of png graph to be exported
set more off

clear
use "D:\data\Ohio Family Health Survey\2004\ofhs0304.dta" //The file to use

drop if i_type_c==. //Drop entries where there is not information about a child's insurance status (this drops all adults)

gen cutoff=200 //this is the SCHIP/Medicaid eligibility threshold
gen st=39 //this is only Ohio data
gen pwgtp=round (wtf_chld) //this is the weight for children; note: I round to the nearest 1

// gen povlev=round (100* (h85y_imp/ (9310+ ( (h84_imp-1)*3180)))) //divides the income (imputed income) by the federal poverty level for the (imputed) NOTE: Doesn't work for 2004 data
// family size (9310 + (x-1)*3180) and then multiplies by 100; this is based on 2004 FPL Guidelines: http://aspe.hhs.gov/poverty/04poverty.shtml

// drop if h84_imp==. //This drops children where we don't know the family size
// drop if h85y==. //This drops children whose income is unknown
// gen povlev=round (100* (h85y/ (9310+ ( (h84_imp-1)*3180)))) //does the same as above but uses actual, not imputed, values for income and drops the values with missing data

/*
Generate histograms of children with public, private and no insurance
2012-09-20

2013-02-08 - Updated with 2012 data

2013-02-14 - Updated svyset (added in stratum)
*/
gen eligible=0
replace eligible=1 if povlev<=cutoff //the child is eligible if they are below the cutoff;

label variable eligible "Whether the child is eligible for Medicaid or SCHIP; 1=eligible, 0=ineligible"
gen ineligible=abs (eligible-1) //oposite of eligible
label variable ineligible "Whether the child is ineligible for Medicaid or SCHIP; 0=eligible, 1=ineligible"

//generating blocks of poverty levels

sort povlev
gen p1=povlev-mod (povlev,1)
gen p3=povlev-mod (povlev,3)
gen p5=povlev-mod (povlev,5)
gen p10=povlev-mod (povlev,10)
gen p20=povlev-mod (povlev,20)
gen p100=povlev-mod (povlev,100)

//generate whether they have private coverage

gen private=0
replace private=1 if i_type_c==4 | i_type_c==5 | i_type_c==6 | i_type_c==7 //04-JOB-BASED COVERAGE; 05-DIRECTLY PURCHASED; 06-OTHER; 07-INSURED TYPE UNKNOWN
//NOTE: I'M NOT SURE WHETHER WE SHOULD PUT 06-OTHER AS INSURED OR IF WE SHOULD DROP THEM

gen public=0
replace public=1 if i_type_c==1 | i_type_c==2 | i_type_c==3 //01-Medicaid and Medicare; 02-Medicaid, no Medicare; 03-Medicare, no Medicaid

gen uninsured=0
replace uninsured=1 if i_type_c==8 //08-Uninsured

label variable private "Whether the child has any insurance coverage; 0=no, 1=yes"
label variable public "Whether the child has any public insurance coverage; 0=no, 1=yes"
label variable uninsured "Whether the child is uninsured; 0=no, 1=yes"

gen insurance = 1
replace insurance = 2 if public
replace insurance = 3 if uninsured
label variable `insurance` "The type of insurance the child has: 1=private; 2=public; 3=uninsured"

//graph bar (sum) private public uninsured if povlev<500 [pweight=pwgtp], over (p10) stack
//graph bar (sum) private public uninsured if povlev<500 [pweight=pwgtp], over (p20, gap (0)) stack
//graph bar (sum) private public uninsured if povlev<500 [pweight=pwgtp], stack over (p20, gap (0) relabel (1 "0" 2 " 3 " 4 " 5 "100" 6 " 7 " 8 " 9 " 10 "200" 11 " 12 " 13 " 14 " 15 " 300" 16 " 17 " 18 " 19 " 20 "400" 21 " 22 " 23 " 24 " 25 "500" 26 " 27 " 28 " 29 " 30 "600" 31 " 32 " 33 " 34 " 35 "700") title ("Population Density of Children in Ohio, 2004") ytitle ("Number of Children") intensity (*.85) legend (label (1 "Privately Insured") label (2 "Publicly Insured") label (3 "Uninsured")). ylab (0 100000 200000, format (%9.0fc) grid gmax) bcolor (3, color (dknavy)) bcolor (1, color (dkgreen))

graph bar (sum) private public uninsured if povlev<700 [pweight=pwgtp], stack over (p20, gap (0) relabel (1 "0" 2 " 3 " 4 " 5 "100" 6 " 7 " 8 " 9 " 10 "200" 11 " 12 " 13 " 14 " 15 " 300" 16 " 17 " 18 " 19 " 20 "400" 21 " 22 " 23 " 24 " 25 "500" 26 " 27 " 28 " 29 " 30 "600" 31 " 32 " 33 " 34 " 35 "700") title ("Percent of Children by Insurance Coverage in Ohio, 2004") ytitle ("Percent of Children") intensity (*.85) legend (label (1 "Privately Insured") label (2 "Publicly Insured") label (3 "Uninsured"). ylab (0 100000 200000, format (%9.0fc) grid gmax) bcolor (3, color (dknavy)) bcolor (1, color (dkgreen))

graph save Graph "D:\Dropbox\My Research\OFHS\Graphs\Population Density - 2004.gph", replace
graph export "D:\Dropbox\My Research\OFHS\Graphs\Population Density - 2004.png", replace width (`size')

window manage close graph
replace p100=1500 if p100>1500
svyset [pweight=pwgtp], strata (stratum) vce (linearized)
tabout p100 insurance using "D:\Dropbox\My Research\OFHS\Results\Insurance Status - 2004.csv", style (csv) replace svy format (0) cells (freq ci)

240
tabout p100 insurance using "D:\Dropbox\My Research\OFHS\Results\Insurance Status - 2004.csv", style (csv) append svy format (1p) cells (row ci) percent clab (%) npos (lab)

/////////////////////////////////////////////////////////////////////////2008///////////////////////

clear

use "D:\data\Ohio Family Health Survey\2008\ofhs2008.dta" //The data file to use

drop if i_type_c==. //Drop entries where there is not information about a child's insurance status (this drops all adults)

gen cutoff=200 //this is the SCHIP/Medicaid eligibility threshold
gen st=39 //this is only Ohio data
gen pwgtp=round (wt_c) //this is the weight for children; note: I round to the nearest 1

// gen povlev=round (100* (inc_imp/ (10400+ ( (h84_imp-1)*3600)))) //divides the income (imputed income) by the federal poverty level for the (imputed) family size (10400 + (x-1)*3600) and then multiplied by 100; this is based on the 2008 FPL guidelines: http://aspe.hhs.gov/poverty/08poverty.shtml

// drop if h84_imp==. //This drops children where we don't know the family size
// drop if h85y==. //This drops children whose income is unknown

gen povlev=round (100* (h85y/ (10400+ ( (h84_imp-1)*3600)))) //does the same as above but uses actual, not imputed, income values and drops the values with missing data

gen eligible=0
replace eligible=1 if povlev<=cutoff //the child is eligible if they are below the cutoff;

label variable eligible "Whether the child is eligible for Medicaid or SCHIP; 1=eligible, 0=ineligible"
gen ineligible=abs (eligible-1) //oposite of eligible
label variable ineligible "Whether the child is ineligible for Medicaid or SCHIP; 0=eligible, 1=ineligible"

//generating blocks of poverty levels

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```
sort povlev
gen p1=povlev-mod (povlev,1)
gen p3=povlev-mod (povlev,3)
gen p5=povlev-mod (povlev,5)
gen p10=povlev-mod (povlev,10)
gen p20=povlev-mod (povlev,20)
gen p100=povlev-mod (povlev,100)

//generate whether they have private coverage
gen private=0
replace private=1 if i_type_c==4 | i_type_c==5 | i_type_c==6 | i_type_c==7 //04-JOB-BASED COVERAGE; 05-DIRECTLY PURCHASED; 06-OTHER; 07-INSURED TYPE UNKNOWN
//NOTE: I'M NOT SURE WHETHER WE SHOULD PUT 06-OTHER AS INSURED OR IF WE SHOULD DROP THEM

gen public=0
replace public=1 if i_type_c==1 | i_type_c==2 | i_type_c==3 //01-Medicaid and Medicare; 02-Medicaid, no Medicare; 03-Medicare, no Medicaid

gen uninsured=0
replace uninsured=1 if i_type_c==8 //08-Uninsured

label variable private "Whether the child has any insurance coverage; 0=no, 1=yes"
label variable public "Whether the child has any public insurance coverage; 0=no, 1=yes"
label variable uninsured "Whether the child is uninsured; 0=no, 1=yes"

gen insurance = 1
replace insurance = 2 if public
replace insurance = 3 if uninsured

label variable insurance "The type of insurance the child has: 1=private; 2=public; 3=uninsured"

graph bar (sum) private public uninsured if povlev<700 [pweight=pwgtp], stack over (p20, gap (0) relabel (1 "0" 2 "3" 4 "5" 6 "100" 7 "10" 8 "200" 9 "11" 10 "12" 11 "13" 12 "14" 13 "15" 14 "16" 15 "17" 16 "18" 17 "19" 18 "20" 19 "21" 20 "22" 21 "23" 22 "24" 23 "25" 24 "26" 25 "27" 26 "28" 27 "29" 28 "30" 29 "600" 30 "31" 31 "32" 32 "33" 33 "34" 34 "35" 35 "700")) title ("Population Density of Children in Ohio, 2008") ytitle ("Number of Children") intensity (*.85) legend (label (1 "Privately Insured") label (2 "Publicly Insured") label (3 "Uninsured")) ylabel (0 100000 200000,format (%9.0fc) grid gmax) bar (3, color (dknavy)) bar (1, color (dkgreen))
gr_edit .b1title.text.Arrpush Percent of Federal Poverty Level
```
graph save Graph "D:\Dropbox\My Research\OFHS\Graphs\Population Density - 2008.gph", replace
graph export "D:\Dropbox\My Research\OFHS\Graphs\Population Density - 2008.png", replace
width ('size')

graph bar (sum) private public uninsured if povlev<700 [pweight=pwgtp], stack over (p20, gap (0) relabel (1 "0" 2 "3" 4 "5" 5 "100" 6 "7" 8 "9" 9 "10 "200" 11 "12" 13 "14" 15 "300" 16 "17" 18 "19" 20 "400" 21 "22" 23 "24" 24 "25 "500" 26 "27" 28 "29 "30 "600" 31 "32" 33 "34" 35 "700") title ("Percent of Children by Insurance Coverage in Ohio, 2008") ytitle ("Percent of Children") intensity (*.85) legend (label (1 "Privately Insured") label (2 "Publicly Insured") label (3 "Uninsured") ylable (,format (%9.0fc)) bar (3, color (dknavy)) bar (1, color (dkgreen)) percentages
gr_edit .b1title.text.Arrpush Percent of Federal Poverty Level

window manage close graph

replace p100=1500 if p100>1500

svyset [pweight=pwgtp], strata (stratum) vce (linearized)
tabout p100 insurance using "D:\Dropbox\My Research\OFHS\Results\Insurance Status - 2008.csv", style (csv) replace svy format (0) cells (freq ci)
tabout p100 insurance using "D:\Dropbox\My Research\OFHS\Results\Insurance Status - 2008.csv", style (csv) append svy format (1p) cells (row ci) percent clab (%) npos (lab)

clear

use "D:\data\Ohio Family Health Survey\2010\ofhs2010_v6_public.dta" //The file to use
drop if i_type_c==. //Drop entries where there is not information about a child’s insurance status (this drops all adults)

gen cutoff=200 //this is the SCHIP/Medicaid eligibility threshold
gen st=39 //this is only Ohio data
gen pwgtp=round (wt_c) //this is the weight for children; note: I round to the nearest 1
// gen povlev=round (100* (inc_imp/ (10830+ ( (h84_imp-1)*3740)))) //divides the income (imputed income) by the federal poverty level for the (imputed)
   //family size (10830 + (x-1)*3740) and then multiplies by 100; this is based on 2009/2010 FPL
guidelines: http://aspe.hhs.gov/poverty/10poverty.shtml

// drop if h84==. //This drops children where we don't know the family size
// drop if h85y_1==. //This drops children whose income is unknown
   gen povlev=round (100* (h85y_1/ (10830+ ( (h84-1)*3740)))) //does the same as above but uses actual, not imputed, values and drops the values with missing data

   gen eligible=0
   replace eligible=1 if povlev<=cutoff //the child is eligible if they are below the cutoff;

   label variable eligible "Whether the child is eligible for Medicaid or SCHIP; 1=eligible, 0=ineligible"
   gen ineligible=abs (eligible-1) //opposite of eligible
   label variable ineligible "Whether the child is ineligible for Medicaid or SCHIP; 0=eligible, 1=ineligible"

//generating blocks of poverty levels

   sort povlev
   gen p1=povlev-mod (povlev,1)
   gen p3=povlev-mod (povlev,3)
   gen p5=povlev-mod (povlev,5)
   gen p10=povlev-mod (povlev,10)
   gen p20=povlev-mod (povlev,20)
   gen p100=povlev-mod (povlev,100)

   //generate whether they have private coverage
   gen private=0
   replace private=1 if i_type_c==4 | i_type_c==5 | i_type_c==6 | i_type_c==7 //04-JOB-BASED
   COVERAGE; 05-DIRECTLY PURCHASED; 06-OTHER; 07-INSURED TYPE UNKNOWN
   //NOTE: I'M NOT SURE WHETHER WE SHOULD PUT 06-OTHER AS INSURED OR IF WE SHOULD DROP THEM

   gen public=0
   replace public=1 if i_type_c==1 | i_type_c==2 | i_type_c==3 //01-Medicaid and Medicare; 02-
Medicaid, no Medicare; 03-Medicare, no Medicaid
gen uninsured=0
replace uninsured=1 if i_type_c==8 //08-Uninsured

label variable private "Whether the child has any insurance coverage; 0=no, 1=yes"
label variable public "Whether the child has any public insurance coverage; 0=no, 1=yes"
label variable uninsured "Whether the child is uninsured; 0=no, 1=yes"

gen insurance = 1
replace insurance = 2 if public
replace insurance = 3 if uninsured

label variable insurance "The type of insurance the child has: 1=private; 2=public; 3=uninsured"

graph bar (sum) private public uninsured if povlev<700 [pweight=pwgtp], stack over (p20, gap (0) relabel (1 "0" 2 "3" 3 "4" 4 "6" 5 "100" 6 "7" 7 "8" 8 "9" 9 "10" 10 "200" 11 "12" 12 "13" 13 "14" 14 "15" 15 "300" 16 "17" 17 "18" 18 "19" 19 "20" 20 "400" 21 "22" 22 "23" 23 "24" 24 "25" 25 "500" 26 "27" 27 "28" 28 "29" 29 "30" 30 "600" 31 "32" 32 "33" 33 "34" 34 "35" 35 "700")) title ("Population Density of Children in Ohio, 2010") ytitle ("Number of Children") intensity (*.85) legend (label (1 "Privately Insured") label (2 "Publicly Insured") label (3 "Uninsured")) ylabel (0 100000 200000,format (%9.0fc) grid gmax) bar (3, color (dknavy)) bar (1, color (dkgreen))
gr_edit .b1title.text.Arrpush Percent of Federal Poverty Level
graph save Graph "D:\Dropbox\My Research\OFHS\Graphs\Population Density - 2010.gph", replace

graph export "D:\Dropbox\My Research\OFHS\Graphs\Population Density - 2010.png", replace width (`size')

graph bar (sum) private public uninsured if povlev<700 [pweight=pwgtp], stack over (p20, gap (0) relabel (1 "0" 2 "3" 3 "4" 4 "6" 5 "100" 6 "7" 7 "8" 8 "9" 9 "10" 10 "200" 11 "12" 12 "13" 13 "14" 14 "15" 15 "300" 16 "17" 17 "18" 18 "19" 19 "20" 20 "400" 21 "22" 22 "23" 23 "24" 24 "25" 25 "500" 26 "27" 27 "28" 28 "29" 29 "30" 30 "600" 31 "32" 32 "33" 33 "34" 34 "35" 35 "700")) title ("Percent of Children by Insurance Coverage in Ohio, 2010") ytitle ("Percent of Children") intensity (*.85) legend (label (1 "Privately Insured") label (2 "Publicly Insured") label (3 "Uninsured")) ylabel (,format (%9.0fc)) bar (3, color (dknavy)) percentages
gr_edit .b1title.text.Arrpush Percent of Federal Poverty Level
graph save Graph "D:\Dropbox\My Research\OFHS\Graphs\Area Chart - 2010.gph", replace
graph export "D:\Dropbox\My Research\OFHS\Graphs\Area Chart - 2010.png", replace width (`size')

window manage close graph

replace p100=1500 if p100>1500
svyset [pweight=pwgtp], strata (stratum) vce (linearized)
tabout p100 insurance using "D:\Dropbox\My Research\OFHS\Results\Insurance Status - 2010.csv", style (csv) replace svy format (0) cells (freq ci)
tabout p100 insurance using "D:\Dropbox\My Research\OFHS\Results\Insurance Status - 2010.csv", style (csv) append svy format (1p) cells (row ci) percent clab (%) npos (lab)

<<<<<<<<<<<2012>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>>

clear
use "D:\data\Ohio Family Health Survey\2012 (OMAS)\2012 OMAS Data Set\omas_finaldata_research_v11.dta" //The file to use

drop if i_type_c==. //Drop entries where there is not information about a child's insurance status (this drops all adults)

gen cutoff=200 //this is the SCHIP/Medicaid eligibility threshold
gen st=39 //this is only Ohio data
gen pwgtp=round (wt_c) //this is the weight for children; note: I round to the nearest 1

// gen povlev=round (100* (h85_value_imp/ (11170+ ( (h84_imp-1)*3960)))) //divides the income (imputed income) by the federal poverty level for the (imputed)
// family size (10830 + (x-1)*3740) and then multiplies by 100; this is based on 2012 FPL guidelines: http://aspe.hhs.gov/poverty/12poverty.shtml

// drop if h84==. //This drops children where we don't know the family size
// drop if h85y_1==. //This drops children whose income is unknown
    gen povlev=round (100* (h85_value/ (11170+ ( (h84-1)*3960)))) //does the same as above but uses actual, not imputed, values and drops the values with missing data

    gen eligible=0
replace eligible=1 if povlev<=cutoff //the child is eligible if they are below the cutoff;
label variable eligible "Whether the child is eligible for Medicaid or SCHIP; 1=eligible, 0=ineligible"

gen ineligible=abs(eligible-1) //opposite of eligible
label variable ineligible "Whether the child is ineligible for Medicaid or SCHIP; 0=eligible, 1=ineligible"

//generating blocks of poverty levels

sort povlev
gen p1=povlev-mod(povlev,1)
gen p3=povlev-mod(povlev,3)
gen p5=povlev-mod(povlev,5)
gen p10=povlev-mod(povlev,10)
gen p20=povlev-mod(povlev,20)
gen p100=povlev-mod(povlev,100)

//generate whether they have private coverage
gen private=0
replace private=1 if i_type_c==4 | i_type_c==5 | i_type_c==6 | i_type_c==7 //04-JOB-BASED COVERAGE; 05-DIRECTLY PURCHASED; 06-OTHER; 07-INSURED TYPE UNKNOWN
//NOTE: I'M NOT SURE WHETHER WE SHOULD PUT 06-OTHER AS INSURED OR IF WE SHOULD DROP THEM

gen public=0
replace public=1 if i_type_c==1 | i_type_c==2 | i_type_c==3 //01-Medicaid and Medicare; 02-Medicaid, no Medicare; 03-Medicare, no Medicaid

gen uninsured=0
replace uninsured=1 if i_type_c==8 //08-Uninsured

label variable private "Whether the child has any insurance coverage; 0=no, 1=yes"
label variable public "Whether the child has any public insurance coverage; 0=no, 1=yes"
label variable uninsured "Whether the child is uninsured; 0=no, 1=yes"

gen insurance = 1
replace insurance = 2 if public
replace insurance = 3 if uninsured

label variable insurance "The type of insurance the child has: 1=private; 2=public; 3=uninsured"
graph bar (sum) private public uninsured if povlev<700 [pweight=pwgtp], stack over (p20, gap (0) relabel (1 "0" 2 "3" 3 "4" 4 "5" 5 "100" 6 "7" 7 "8" 8 "9" 9 "10" 10 "200" 11 "12" 12 "13" 13 "14" 14 "15 " 15 "300" 16 "17" 17 "18" 18 "19" 19 "20" 20 "400" 21 "22" 22 "23" 23 "24" 24 "25" 25 "500" 26 "27" 27 "28" 28 "29 " 29 "30" 30 "600" 31 "32" 32 "33" 33 "34" 34 "35" 35 "700") title ("Population Density of Children in Ohio, 2012") ytitle ("Number of Children") intensity (*.85) legend (label (1 "Privately Insured") label (2 "Publicly Insured") label (3 "Uninsured")) ylabel (0 100000 200000,format (%9.0fc) grid gmax) bar (3, color (dknavy)) bar (1, color (dkgreen))

gr_edit .b1title.text.Arrpush Percent of Federal Poverty Level
graph save Graph "D:\Dropbox\My Research\OFHS\Graphs\Population Density - 2012.gph", replace
graph export "D:\Dropbox\My Research\OFHS\Graphs\Population Density - 2012.png", replace width (`size')

graph bar (sum) private public uninsured if povlev<700 [pweight=pwgtp], stack over (p20, gap (0) relabel (1 "0" 2 "3" 3 "4" 4 "5" 5 "100" 6 "7" 7 "8" 8 "9" 9 "10" 10 "200" 11 "12" 12 "13" 13 "14" 14 "15 " 15 "300" 16 "17" 17 "18" 18 "19" 19 "20" 20 "400" 21 "22" 22 "23" 23 "24" 24 "25" 25 "500" 26 "27" 27 "28" 28 "29 " 29 "30" 30 "600" 31 "32" 32 "33" 33 "34" 34 "35" 35 "700") title ("Percent of Children by Insurance Coverage in Ohio, 2012") ytitle ("Percent of Children") intensity (*.85) legend (label (1 "Privately Insured") label (2 "Publicly Insured") label (3 "Uninsured")) ylabel (,format (%9.0fc)) bar (3, color (dknavy)) bar (1, color (dkgreen)) percentages

gr_edit .b1title.text.Arrpush Percent of Federal Poverty Level
graph save Graph "D:\Dropbox\My Research\OFHS\Graphs\Area Chart - 2012.gph", replace
graph export "D:\Dropbox\My Research\OFHS\Graphs\Area Chart - 2012.png", replace width (`size')

window manage close graph

replace p100=1500 if p100>1500

svyset [pweight=pwgtp], strata (stratum) vce (linearized)
tabout p100 insurance using "D:\Dropbox\My Research\OFHS\Results\Insurance Status - 2012.csv", style (csv) replace svy format (0) cells (freq ci)
tabout p100 insurance using "D:\Dropbox\My Research\OFHS\Results\Insurance Status - 2012.csv", style (csv) append svy format (1p) cells (row ci) percent clab (%) npos (lab)
Appendix F – State Graphs of Rates of Malpractice Payments

Malpractice Settlements in Alabama by Year

Malpractice Settlements in Alaska by Year

Malpractice Settlements in Arizona by Year

Malpractice Settlements in Arkansas by Year

Malpractice Settlements in California by Year

Malpractice Settlements in Colorado by Year
Appendix G – Caps on Noneconomic Damages Code

/*
David Muhlestein
Medical Malpractice Payments By State

This will read in data from the National Practitioner’s Data Bank and calculate the number of malpractice settlements (meaning there was a payment made to the plaintiff) by state.

2012-10-04 - Base Work
2012-10-11 - Link it with data from the area resource file so I have information on population and physician population
2013-02-04 - Continue to link data
2013-03 - Generate all the results and all the pictures
*/

clear

capture estimates drop _all

//this generates a time stamp for when the file is run
local c_date: display %tdCY-N-D date (c (current_date),"DMY")
local c_time = c (current_time)
local c_time_date = ""c_date"" + ""c_time"
local time_string = subinstr ("c_time_date", ":", ",", .)
use "D:\data\National Practitioner Data Bank\NPDB1207.DTA", clear //Note: I am using NPDB 12/07 which contains data through September 30, 2012, downloaded February 4, 2013 from http://www.npdb-hipdb.hrsa.gov/servlet/PublicUseFileServlet
//start with 898,575 observations

drop if rectype=="A" | rectype=="C" //this drops records that deal with adverse action reporting
(such as practitioners losing their license)
//down to 374,343 observations

//this will generate the year of the malpractice; it will first assign the malyear as malyear1
which is the start of the malpractice and will replace it with malyear2 if it exists which is the end
of the malpractice;
//I'm doing this because, based on liability, the end of the malpractice is generally the time that
influence statutes of limitations
gen malyear=malyear1
replace malyear=malyear2 if malyear2!=.
replace malyear=origyear if malyear>3000  //I also say that the year the malpractice was
committed (malyear) is the same as the year of payment (origyear) if there's not data on the
year of malpractice

//generate the state; I use the work state (workstat) unless it is missing, and then I defer to
home state and then to license state
gen st=workstat
replace st=homestat if st=="
replace st=licnstat if st=="
drop if st==""  //drop any variables where there is no state (it drops 33)
//down to 377,137 observations

drop if st=="AA" | st=="AE" | st=="AP" | st=="AS" | st=="FM" | st=="GU" | st=="MH" |
st=="MP" | st=="PR" | st=="PW" | st=="VI"
//this drops observations for Armed Forces America (AA), Armed Forces Europe (AE), Armed
Forces Pacific (AP), American Samoa (AS), Federated States of Micronesia (FM), Guam (GU),
Marshall Islands (MH), Northern Marianas (MP), Puerto Rico (PR), Palau (PW), Virgin Islands (VI)
//down to 369,928 observations

drop if origyear==1990 | origyear==2012 //this drops the two partial years (Sep-Dec of 1990 and
Jan-September of 2012)
//down to 360,702 observations

//merge the states with a crosswalk so that we can see the full name
sort st origyear
merge m:1 st using ""home\Crosswalk\state region fips.dta"

//changes the directories and creates a tabout file of the frequency of payments for each state and year
cd Results
capture erase "Payments by State and Year.dta"
bigtab state origyear, saving (Payments by State and Year) zero

//loads the outputted data set into memory, drops unneccessary variables and reshapes it to wide format
use "Payments by State and Year.dta" , clear
drop.cumfreq cumpct pct
rename freq n
label variable n "Number of paid malpractice claims"
save "Payments by State and Year.dta" , replace
rename n y //this is so when it goes to wide format it says y1990, y1991, etc
reshape wide y, i (state) j (origyear)
save "Payments by State and Year (wide).dta", replace

//this will now take all the data in long format and regress it for each state
use "home\Results\Payments by State and Year.dta" , clear
egen s = group (state) //this creates a categorical group variable for each state
tabulate st, generate (s) //this creates a dummy variable for each state and is 1 if it is that state

//take the Database of State Tort Law Reforms 4th Edition and input it and then merge in the data I generated above
use "home\Results\Payments by State and Year.dta" , clear
sort state
save "home\Results\Payments by State and Year.dta", replace //this is just to make sure the data is sorted

use "home\Crosswalk\DSTLR4.dta" , clear //to use the clever version of the database (which eliminates as many caps as makes sense) use DSTLR4-clever.dta
merge 1:1 state origyear using "home\Results\Payments by State and Year.dta"
sort state origyear

//label all the variables
label variable caps "Value of caps on noneconomic damages (in $1000s)"
label variable r cn "Caps Noneconomic Damages"
label variable r cp "Caps Punitive Damages"
label variable r ct "Caps Total Damages"
label variable r_sr "Split Recovery Reform"
label variable r_cs "Collateral Source Reform"
label variable r_pe "Punitive Evidence Reform"
label variable r_pp "Periodic Payments Reform"
label variable r_cf "Contingency Fee Reform"
label variable r_js "Joint and Several Liability Reform"
label variable r_pcf "Patient Compensation Fund Reform"

drop _merge

replace year=origyear if year==.
drop origyear

save ""home\Results\Payments by State and Year (merged DSTLR).dta", replace

//this will merge data with the area resource file
clear
//note: I'm using active, non-federal MDs; this is because active physicians are practicing and because federal MDs have less data (particularly with DOs) and I'm not convinced that they have the same malpractice risks
rename statename state
rename censuspopulation* pop*
rename populationestimate* pop*
rename mdsnonfederaltotalactive* md*
rename dononfederalactive* do*
rename donftotalactive* do*
drop pop100s* fips

//there is no data for MDs in 2009 and 1991, so I'm generating it by averaging 2008 and 2010;
gen md2009 = (md2010+md2008)/2
ngen md1991 = (md1990+md1992)/2

//DO data is missing for multiple years, so I use linear approximations of the number of DOs; since my first year of data is 1998, I use that for all years prior
gen do1997=do1998
gen do1996=do1998
gen do1995=do1998
gen do1994 = do1998
gen do1993 = do1998
gen do1992 = do1998
gen do1991 = do1998
gen do1990 = do1998

gen do2002 = (do2001 + do2003) / 2


// add MDs and DOs to get the number of docs
forvalues x = 1990/2010 {
    gen doc`x' = do`x' + md`x'
}

replace state = "District of Columbia" if stateabbrev == "DC"  // rename dc so it is consistent with other datasets
merge 1:m state using "\home\Results\Payments by State and Year (merged DSTLR).dta"
// merges with the payments dataset

drop _merge
drop st
rename stateabbrev st
rename fipsstatecode fips

sort state year

drop if year <= 1990 | year >= 2011  // this drops the data outside the range of the DSTLR4

// this will pull out just the population for year by matching the year to the variable pop[year]
gen popx = variable ("pop" + string (year))
gen pop = .

forvalues x = 1/ = _N' {
    quietly local pop = popx[ `x' ]
    quietly replace pop = `pop' in `x'
}
drop popx pop2* pop1*  // drops all the unneeded population variables
/this will pull out just the number of MDs for the year by matching the year to the variable doc[year]
gen docx = variable ("doc"+string (year))
gen doc=
forvalues x = 1/`=_N' {
    quietly local doc = docx[`x']
    quietly capture replace doc=`doc' in `x'
}
drop docx doc2* doc1* //drops all the unneeded MD variables

gen perpop = n/pop*500000 //number of payments per 500,000 population
ngen perdoc = n/doc*1000 //number of payments per 1,000 MDs

label variable perpop "Number of paid malpractice claims per 500,000 population"
label variable perdoc "Number of paid malpractice claims per 1,000 MDs"

//this will tabulate the rate of malpractice claims for each state
tabulate state, generate (s)  //this creates a dummy variable for each state and is 1 if it is that state

//This cleans up the dataset a little bit
replace cnflips = 1 in 2 //just for Alabama, I change how many flips there were in 1992; this is so there are sufficient data points to graph the results
replace enactment_cn = 1987 in 2 //same thing for enactment for Alabama
replace cnflips = 3 in 277 //just for Alabama, I change how many flips there were in 2007; this is so there are sufficient data points to graph the results
replace cnflips = 1 in 749 //just for Oklahoma, I change how many flips there were in 2007; this is so there are sufficient data points to graph the results
replace enactment_cn = 2004 in 254/260 //for Idaho it says there was a change beginning in 2004 when they dropped their cap
replace enactment_cn = 2006 in 516/520 //for Missouri it says there was a change beginning in 2004 when they dropped their cap
replace enactment_cn = 2002 in 892/900 //for Utah it says there was a change beginning in 2002 when they increased their cap

//creates a dummy variable where 1=has had caps on noneconomic damages at some point
egen evercaps = sum (r_cn), by (state)
replace evercaps = 1 if evercaps>1
label var evercaps "Whether the state ever had caps on noneconomic damages"

//creates dummy variable with the maximum number of flips (number of changes between not having caps and having caps) by state
egen maxflips = max (cnflips), by (state)

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//Regression variables
//I first generate the interaction term
//this makes it so I am only looking at the last reform
egen lastenactment = max(enactment_cn), by(state)
replace lastenactment = 1991 if lastenactment<1991 //this removes all the states with
tort reform implemented before the NPDB was collecting data
gen enactment = lastenactment if lastenactment == enactment_cn
gen interaction = max(0,year-enactment) //this creates a term that is equal to the year -
the year that tort reform was enacted if that term is greater than 0, otherwise it is 0
//If I want to look at all reforms I use the following
//
gen interaction = max(0,year-enactment_cn) //this creates a term that is equal to the
year - the year that tort reform was enacted if that term is greater than 0, otherwise it is 0
label variable interaction "Slope" //label for the interaction; it's called slope becaust that what it
represents in the model

//This makes it so I'm only looking at the last reform
gen r_cn2 = r_cn if lastenactment == enactment_cn
replace r_cn2=0 if r_cn2==.
//If I want to look at all reforms I use the following
//
gen r_cn2 = r_cn //I'm creating a duplicate variable with a different label
label variable r_cn2 "Intercept" //it's called the intercept because that's what it
represents in the model

gen ineffect = lastenactment==enactment_cn //dummy variable that is 1 if the last tort reform is
in effect

order state year n pop doc per* s1-s51 //just order the data set a little better

/*
//create trend charts for each of the states by total claims

forvalues x = 1/51 {
    local mylabel : variable label s’x’
    local mylabel2 = substr ("mylabel",8,.)
    graph twoway (scatter n year ) (lfit n year) if s’x’, title (Malpractice Settlements in
`mylabel2’ by Year) ytitle (Number of Paid Claims) xtitle (Year Claim Reported) legend (label (1
"Number of Claims") label (2 "Linear Fit"))

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graph save Graph "`home'\Graphs\Stata Graphs\Trend`x' - `mylabel2'.gph", replace
graph export "`home'\Graphs\Trend`x' - `mylabel2'.png", replace width (`size')
window manage close graph
}

//create trend charts for all states with regressions of caps and non-caps: per 1,000 physicians (MDs + DOs (non-federal and active))
forvalues x = 1/51 {
    local mylabel : variable label s`x'
    local mylabel2 = substr ("mylabel",8,.)
    //this will graph all of the times when tort reform was active
    //graph twoway (scatter perdoc year ) (lfit perdoc year ) (scatter perdoc year if r_cn==1) (lfit perdoc year if r_cn==0)
    if s`x', title (Malpractice Settlements in `mylabel2' by Year) ytitle ("Number of Paid Claims per 1,000 Physicians") xtitle ("Year Claim Reported") legend (label (1 "Number of Claims (Overall)") label (2 "Linear Fit") label (3 "Number of Claims (Caps in Place)") label (4 "Linear Fit") label (5 "Number of Claims (No Caps in Place)") label (6 "Linear Fit"))
    //this will graph only the last time tort reform was active
    graph twoway (scatter perdoc year ) (lfit perdoc year ) (scatter perdoc year if ineffect) (lfit perdoc year if ineffect) (scatter perdoc year if lineffect) (lfit perdoc year if lineffect)
    if s`x', title (Malpractice Settlements in `mylabel2' by Year) ytitle ("Number of Paid Claims per 1,000 Physicians") xtitle ("Year Claim Reported") legend (label (1 "Number of Claims (Overall)") label (2 "Linear Fit") label (3 "Number of Claims (Caps in Place)") label (4 "Linear Fit") label (5 "Number of Claims (No Caps Previous Caps in Place)") label (6 "Linear Fit"))
    graph save Graph "`home'\Graphs\Stata Graphs\Docs`x' - `mylabel2'.gph", replace
graph export "`home'\Graphs\Docs`x' - `mylabel2'.png", replace width (`size')
graph export "`home'\Graphs\Docs`x'\Docs - `mylabel2'.png", replace width (`size')
window manage close graph
}

//create trend charts for all states with regressions of caps and non-caps: per 500,000 population
forvalues x = 1/51 {
    local mylabel : variable label s`x'
    local mylabel2 = substr ("mylabel",8,.)
    //this will graph all of the times when tort reform was active
    //graph twoway (scatter perpop year ) (lfit perpop year ) (scatter perpop year if r_cn==1) (lfit perpop year if r_cn==0)
    if s`x', title (Malpractice Settlements in `mylabel2' by Year) ytitle ("Number of Paid Claims per 500,000 population") xtitle ("Year Claim Reported") legend (label (1 "Number of Claims (Overall)") label (2 "Linear Fit") label (3 "Number of Claims (Caps in Place)") label (4 "Linear Fit") label (5 "Number of Claims (No Caps Previous Caps in Place)") label (6 "Linear Fit"))
    //this will graph only the last time tort reform was active
    graph twoway (scatter perpop year ) (lfit perpop year ) (scatter perpop year if ineffect) (lfit perpop year if ineffect) (scatter perpop year if lineffect) (lfit perpop year if lineffect)
    if s`x', title (Malpractice Settlements in `mylabel2' by Year) ytitle ("Number of Paid Claims per 500,000 population") xtitle ("Year Claim Reported") legend (label (1 "Number of Claims (Overall)") label (2 "Linear Fit") label (3 "Number of Claims (Caps in Place)") label (4 "Linear Fit") label (5 "Number of Claims (No Caps Previous Caps in Place)") label (6 "Linear Fit"))
    graph save Graph "`home'\Graphs\Stata Graphs\Docs`x' - `mylabel2'.gph", replace
graph export "`home'\Graphs\Docs`x' - `mylabel2'.png", replace width (`size')
graph export "`home'\Graphs\Docs`x'\Docs - `mylabel2'.png", replace width (`size')
window manage close graph
r.cn==0) if s'x', title (Malpractice Settlements in 'mylabel2' by Year) ytitle ("Number of Paid Claims per 500,000 Population") xtitle ("Year Claim Reported") legend (label (1 "Number of Claims (Overall)") label (2 "Linear Fit") label (3 "Number of Claims (Caps in Place)") label (4 "Linear Fit") label (5 "Number of Claims (No Caps in Place)") label (6 "Linear Fit"))

//this will graph only the last time tort reform was active
graph twoway (scatter perpop year) if ineffect) (lfit perpop year if ineffect) (scatter perpop year if lineffect) (lfit perpop year if lineffect) if s'x', title (Malpractice Settlements in 'mylabel2' by Year) ytitle ("Number of Paid Claims per 500,000 Population") xtitle ("Year Claim Reported") legend (label (1 "Number of Claims (Overall)")) label (2 "Linear Fit") label (3 "Number of Claims (No Caps in Place)") label (4 "Linear Fit") label (5 "Number of Claims (No Caps in Place)") label (6 "Linear Fit")

graph save Graph "home\Graphs\Stata Graphs\Population\x' - `mylabel2'.gph", replace
graph export "home\Graphs\Population\x' - `mylabel2'.png", replace width (`size')
graph export "home\Graphs\\x'Population - `mylabel2'.png", replace width (`size')

window manage close graph}

//create trend charts for all states with regressions of caps and non-caps: total number
forvalues x = 1/51 {
    local mylabel : variable label s'x'
    local mylabel2 = substr ("mylabel",8,.)

    //this will graph all of the times when tort reform was active
    //graph twoway (scatter n year) if r.cn==1) (lfit n year if r.cn==1) (scatter n year if r.cn==0) (lfit n year if r.cn==0) if s'x', title (Malpractice Settlements in `mylabel2' by Year) ytitle ("Number of Paid Claims") xtitle ("Year Claim Reported") legend (label (1 "Number of Claims (Overall)") label (2 "Linear Fit") label (3 "Number of Claims (Caps in Place)") label (4 "Linear Fit") label (5 "Number of Claims (No Caps in Place)")) label (6 "Linear Fit")

    //this will graph only the last time tort reform was active
    graph twoway (scatter n year) if ineffect) (lfit n year if ineffect) (scatter n year if lineffect) (lfit n year if lineffect) if s'x', title (Malpractice Settlements in `mylabel2' by Year) ytitle ("Number of Paid Claims") xtitle ("Year Claim Reported") legend (label (1 "Number of Claims (Overall)") label (2 "Linear Fit") label (3 "Number of Claims (Caps in Place)")) label (4 "Linear Fit") label (5 "Number of Claims (No Caps in Place)")) label (6 "Linear Fit")

    graph save Graph "home\Graphs\Stata Graphs\Reform\x' - `mylabel2'.gph", replace
    graph export "home\Graphs\Reform\x' - `mylabel2'.png", replace width (`size')
    graph export "home\Graphs\\x'Reform - `mylabel2'.png", replace width (`size')
    window manage close graph
}
//this will generate the statistics for the significance of the change
//I'm using a linear spline model with the model yhat = B0 + B1*year + B2 (year-year_enacted)
//but only where (year-year_enacted > 0) + B3*tort_reform_enacted
*/
eststo clear

//this regresses for each state and stores to one file
forvalues x = 1/51 {
    local mylabel : variable label s`x'
    local mylabel2 = substr ("mylabel",8,)
    qui regress perdoc year r_cn2 interaction if s`x'
    qui eststo doc`x', title ("Docs `x' `mylabel2'")
    qui regress perpop year r_cn2 interaction if s`x'
    qui eststo pop`x', title ("Population `x' `mylabel2'")
    qui regress n year r_cn2 interaction if s`x'
    qui eststo total`x', title ("Total `x' `mylabel2'")
}
estout using "`home'\Results\Results (All).csv", delimiter (","), cells (b t p) label replace
estout doc* using "`home'\Results\Results (Doctors).csv", delimiter (","), cells (b t p) label replace
estout pop* using "`home'\Results\Results (Population).csv", delimiter (","), cells (b t p) label replace
estout total* using "`home'\Results\Results (Total Number).csv", delimiter ("",), cells (b t p) label replace
esttab using "`home'\Results\Results Table (All).csv", label nostar noobs mtitles replace plain

estout using "`home'\Results\Time Stamp\Results (All) - `time_string'.csv", delimiter (","), cells (b t p) label replace
esttab using "`home'\Results\Time Stamp\Results Table (All) - `time_string'.csv", label nostar noobs mtitles replace plain

//estout using "`home'\Results\Results Table (All).csv", cells ( (b (fmt (3)) se (par ("=" ("" "")"")) fmt (3))) delimiter (","), label replace //the publication table
//esttab using "`home'\Results\Results (All).csv", label nostar noobs mtitles replace plain

267
//estout using "'home'\Results\Time Stamp\Results Table (All) - 'time_string'.csv", cells ( (b (fmt (3)) se (par ("=" ("""""""")) fmt (3))) delimiter (".") label replace //the publication table
//esttab using "'home'\Results\Time Stamp\Results Statistics (All) - 'time_string'.csv", label se
noestar noobs mtitles replace plain
*/

//this regresses just on doctors for three different potential statute of limitations (1, 2 and 3 years)
//note I updated this on 3/11 to generate a different interaction term each time through, rather
than at the beginning
eststo clear

//gen interaction1 = max (0,year- (enactment+1))
gen interaction2 = max (0,year- (enactment+2))
gen interaction3 = max (0,year- (enactment+3))
//label variable interaction1 "Slope 1yr"
label variable interaction2 "Slope after Statute of Limitations"
//label variable interaction3 "Slope 3yr"
//gen r_cn2_1 = r_cn2+1
//gen r_cn2_2 = r_cn2+2
//gen r_cn2_3 = r_cn2+3
//label variable r_cn2_1 "Intercept 1yr"
label variable r_cn2_2 "Intercept after Statute of Limitations"
//label variable r_cn2_3 "Intercept 3yr"
log using "'home'\Results\Statute of Limitations.log", replace
log using "'home'\Results\Time Stamp\Statute of Limitations - 'time_string'.log", name (time)
replace forvalues x = 1/50 {
    local mylabel : variable label s`x'
    local mylabel2 = substr ("'mylabel'",8,.)
disp "="
========================================
        "mylabel2"
disp "="
========================================
        "mylabel2"
disp
    replace interaction2=0
    replace r_cn2_2 = 0
    qui regress perdoc year r_cn2 r_cn2_2 interaction interaction2 if s`x'
    qui eststo sol0`x', title ("x 'mylabel2' Docs No Sols")
    qui estimates store s0
replace interaction2 = max (0, year - enactment + 1)
replace r_cn2_2 = r_cn2 + 1
qui regress perdoc year r_cn2 r_cn2_2 interaction interaction2 if s'x'
qui eststo sol1'x', title ("'x' `mylabel2' Docs 1yr SoL")
qui estimates store s1
replace interaction2 = max (0, year - enactment + 2)
replace r_cn2_2 = r_cn2 + 2
qui regress perdoc year r_cn2 r_cn2_2 interaction interaction2 if s'x'
qui eststo sol2'x', title ("'x' `mylabel2' Docs 2yr SoL")
qui estimates store s2
replace interaction2 = max (0, year - enactment + 3)
replace r_cn2_2 = r_cn2 + 3
qui regress perdoc year r_cn2 r_cn2_2 interaction interaction2 if s'x'
qui eststo sol3'x', title ("'x' `mylabel2' Docs 3yr SoL")
qui estimates store s3
qui suest s0 s1 s2 s3
disp "`mylabel2' - Baseline to 1 Year Statute of Limitations"
disp
"==============================================================================
========================================="
test [s0_mean=s1_mean]
disp "`mylabel2' - Baseline to 2 Year Statute of Limitations"
disp
"==============================================================================
========================================="
test [s0_mean=s2_mean]
disp "`mylabel2' - Baseline to 3 Year Statute of Limitations"
disp
"==============================================================================
========================================="
test [s0_mean=s3_mean]
estimates drop _all
}
estout using "'home\Results\Results Docs Statute of Limitations.csv", delimiter ("," ) cells (b se t p) stats (r2 aic bic N p) label replace
estout using "'home\Results\Time Stamp\Results Docs Statute of Limitations - `time_string'.csv", delimiter ("," ) cells (b se t p) stats (r2 aic bic N p) label replace
eststo clear
log close _all
*/
/*
// this regresses for each state and stores to one file but only goes up to certain years; I will use
this to match states with a comparison state. I only regress for the yearly trend (no tort reform
adjustment)

log using "`home'
\results\State Comparisons.log", replace
log using "`home'
\results\Time Stamp\State Comparisons - `time_string'.log", name (time)
replace

foreach y in 1996 2002 2003 2004 2005 2006 {
eststo clear

forvalues x = 1/51 {
local mylabel : variable label s`x'
local mylabel2 = substr ("mylabel",8,.)
qui regress perdoc year /*r_cn2 interaction*/ if s`x' & year<`y'
qui eststo y`y'doc`x', title ("Docs `x' mylabel2")
qui estimates store y`y'doc`x'
}
estout y`y'doc* using "`home'
\results\Results (Doctors through `y').csv", delimiter (",")
cells (b se t p) label replace

// This regresses before and after a set year for each state; after matching based on the previous
file, I'll look for a difference with this file
gen intercept = (year>=`y')
gen slope = max (0,year-`y')

forvalues x = 1/51 {
local mylabel : variable label s`x'
local mylabel2 = substr ("mylabel",8,.)
qui regress perdoc year intercept slope if s`x'
qui eststo y`y'docknot`x', title ("Docs Knot `x' mylabel2")
qui estimates store y`y'docknot`x'

// graph the yearly values
// graph twoway (scatter perdoc year ) (lfit perdoc year ) (scatter perdoc
year if year<`y') (lfit perdoc year if year<`y') (scatter perdoc year if year>=`y') (lfit perdoc year if
year>=`y') if s`x', title (Malpractice Settlements in `mylabel2' by Year) ytitle ("Number of Paid
Claims per 1,000 Physicians") xtitle ("Year Claim Reported") legend (label (1 "Number of Claims
(Overall)") label (2 "Linear Fit") label (3 "Number of Claims (Before `y')") label (4 "Linear Fit")
label (5 "Number of Claims (After `y')") label (6 "Linear Fit"))
// graph export "`home'\graphs\cutoff`y' docs`x' - `mylabel2'.png",
replace width (`size')

270
```stata
// graph save Graph "home\Graphs\Stata Graphs\Cutoff`y' Docs`x' - `mylabel2'.gph", replace
}
estout `y'`y'docknot* using "home\Results\Results (Doctors knot at `y').csv", delimiter (",") cells (b se t p) label replace

//This section will compare the slopes and intercepts of states that I have matched manually
//Note: I match both the slope and the intercept of the states that implemented tort reform to the states closest to them both in terms of trendline and absolute value, but exclude those that also implemented caps on noneconomic damages within five years

if `y'==1996 {
  //Montana
  local state 27
  local mylabel : variable label s`state'
  local mylabel2 = substr ("mylabel",8,.)
  //Slope compared to Wisconsin and Louisiana
  foreach state2 in 50 19 {
    local mylabel3: variable label s`state2'
    local mylabel4 = substr ("mylabel3",8,.)
    disp "==============================================================================
    ==========================================
    Slope Docs `y' `mylabel2' versus `mylabel4'
    ==========================================
    ==============================================================
    qui suest y`y'docknot`state' y`y'docknot`state2' 
    test [y`y'docknot`state'_mean]slope - [y`y'docknot`state2'_mean]slope = 0 
    test [y`y'docknot`state'_mean]year - [y`y'docknot`state2'_mean]year = 0 
    test [y`y'docknot`state'_mean]intercept - [y`y'docknot`state2'_mean]intercept = 0
```
test [y'y'docknot'state'_mean]_cons - [y'y'docknot'state2'_mean]_cons = 0
test [y'y'docknot'state'_mean=y'y'docknot'state2'_mean] disp ***
disp "+++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++
+++++++++++++++++++++++++++++++++++++++++
" disp *** disp ***
}

//Absolute compared to Nevada and Kansas
foreach state2 in 29 17 {
    local mylabel3: variable label s`state2'
    local mylabel4 = substr ("`mylabel3'",8,.)
disp "==============================================================================
========================================="
disp "Absolute Docs `y' `mylabel2' versus `mylabel4'
" disp "==============================================================================
=========================================
qui suest y`y'docknot'state' y`y'docknot'state2'
    test [y'y'docknot'state'_mean]slope = 0
test [y'y'docknot'state'_mean]year = 0
test [y'y'docknot'state'_mean]intercept = 0
test [y'y'docknot'state'_mean]_cons = 0
    test [y'y'docknot'state'_mean=y'y'docknot'state2'_mean] disp ***
disp "+++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++
+++++++++++++++++++++++++++++++++++++++++
" disp *** disp ***
}

//North Dakota
local state 35
    local mylabel : variable label s`state'
    local mylabel2 = substr ("`mylabel'",8,.)
272
//Slope compared to Colorado and Arizona
foreach state2 in 6 3 {
    local mylabel3: variable label s`state2'
    local mylabel4 = substr ("mylabel3",8,.)
    disp "==============================================================================
    ==========================================
    Slope Docs `y' `mylabel2' versus `mylabel4'
    ==============================================================
    ==============================================================
    qui suest y`y'docknot`state' y`y'docknot`state2'
    test [y`y'docknot`state'_mean]slope = 0
    test [y`y'docknot`state'_mean]year = 0
    test [y`y'docknot`state'_mean]intercept = 0
    test [y`y'docknot`state'_mean]_cons = 0
}

//Absolute compared to Oklahoma and Georgia
foreach state2 in 37 11 {
    local mylabel3: variable label s`state2'
    local mylabel4 = substr ("mylabel3",8,.)
    disp "==============================================================================
    Absolute Docs `y' `mylabel2' versus `mylabel4'
    ==============================================================
    ==============================================================
    qui suest y`y'docknot`state' y`y'docknot`state2'
    test [y`y'docknot`state'_mean]slope = 0
    test [y`y'docknot`state'_mean]year = 0
    test [y`y'docknot`state'_mean]intercept = 0
    test [y`y'docknot`state'_mean]_cons = 0
}
test [y'y'docknot'state'_mean]intercept -
[y'y'docknot'state2'_mean]intercept = 0
test [y'y'docknot'state'_mean]_cons -
[y'y'docknot'state2'_mean]_cons = 0
test [y'y'docknot'state'_mean=y'y'docknot'state2'_mean]
disp ""
disp """"+++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++
+++++++++++++++++++++++++++++++++++++++++++++++
""
disp ""
disp ""
}

//South Dakota
local state 42
local mylabel : variable label s'state'
local mylabel2 = substr ("$mylabel",8,.)
//Slope compared to Michigan and Vermont
foreach state2 in 23 46 {
    local mylabel3: variable label s'state2'
    local mylabel4 = substr ("$mylabel3",8,.)
disp "="/=
"==============================================================================
=========================================
Slope Docs `y' `mylabel2' versus `mylabel4''
disp "="
disp "="/="
disp "="/="
qui suest y'y'docknot'state' y'y'docknot'state2'
[y'y'docknot'state2'_mean]slope = 0
test [y'y'docknot'state'_mean]slope -
[y'y'docknot'state2'_mean]year = 0
test [y'y'docknot'state'_mean]year -
[y'y'docknot'state2'_mean]intercept = 0
test [y'y'docknot'state'_mean]_cons -
[y'y'docknot'state2'_mean]_cons = 0
test [y'y'docknot'state'_mean=y'y'docknot'state2'_mean]
disp ""
disp """"+++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++
+++++++++++++++++++++++++++++++++++++++++++++++
""
disp ""
disp ""
""
//Absolute compared to Ohio and Louisiana
foreach state2 in 36 19 {
    local mylabel3: variable label s`state2'
    local mylabel4 = substr ("mylabel3",8,.)
    disp "==============================================================================
    ==========================================
    Absolute Docs `y' `mylabel2' versus `mylabel4'
    =============================================================================
    ==========================================
    qui suest y`y'docknot`state' y`y'docknot`state2'
    test [y`y'docknot`state'_mean]slope = 0
    test [y`y'docknot`state'_mean]year = 0
    test [y`y'docknot`state'_mean]intercept = 0
    test [y`y'docknot`state'_mean=_cons = 0
    disp "++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++
    ++++++++++++++++++++++++++++++++++++++++++
    disp "
    disp 
    disp "
    
    }
}

if `y'==2002 {
    //Utah
    local state 45
    local mylabel : variable label s`state'
    local mylabel2 = substr ("mylabel",8,.)
    //Slope compared to Michigan and Colorado
    foreach state2 in 23 6 {
        disp ""
local mylabel3: variable label s`state2'
local mylabel4 = substr ("mylabel3",8,.)
disp
"==============================================================================
========================================="
disp "Slope Docs `y' `mylabel2' versus `mylabel4''
disp
"==============================================================================
========================================="
qui suest y`y'docknot`state' y`y'docknot`state2'
test [y`y'docknot`state' mean]slope = 0
test [y`y'docknot`state' mean]year = 0
test [y`y'docknot`state' mean]intercept = 0
test [y`y'docknot`state' mean]_cons = 0
disp "-------------
"++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++
" Absolute compared to Washington and New Hampshire
foreach state2 in 48 30 {
    local mylabel3: variable label s`state2'
    local mylabel4 = substr ("mylabel3",8,.)
disp
"==============================================================================
========================================="
disp "Absolute Docs `y' `mylabel2' versus `mylabel4''
disp
"==============================================================================
========================================="
qui suest y`y'docknot`state' y`y'docknot`state2'
test [y`y'docknot`state' mean]slope = 0
test [y`y'docknot`state' mean]year = 0
test [y`y'docknot`state' mean]intercept = 0
}
test [y'y'docknot'state'_mean]_cons -
[y'y'docknot'state2'_mean]_cons = 0
    test [y'y'docknot'state'_mean=y'y'docknot'state2'_mean]
disp ***
disp
"++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++
+++++++++++++++++++++++++++++++++++++++++++++++
+++++++++++++++++++++++++++++++++++++++++
disp ***
disp ***
}

if `y'==2003 {
    //Florida
    local state 10
    local mylabel : variable label s`state'
    local mylabel2 = substr ("mylabel",8,.)
    //Slope compared to DC and Wyoming
    foreach state2 in 9 51 {
        local mylabel3: variable label s`state2'
        local mylabel4 = substr ("mylabel3",8,.)
        disp "==============================================================================
=========================================
Slope Docs `y' `mylabel2' versus `mylabel4"
        disp "==============================================================================
=========================================
        qui suest y'y'docknot'state' y'y'docknot'state2'
        [y'y'docknot'state2'_mean]slope = 0
        test [y'y'docknot'state'_mean]slope -
        [y'y'docknot'state2'_mean]year = 0
        test [y'y'docknot'state'_mean]year -
        [y'y'docknot'state2'_mean]year = 0
        test [y'y'docknot'state'_mean]year -
        [y'y'docknot'state2'_mean]intercept = 0
        test [y'y'docknot'state'_mean]intercept -
        [y'y'docknot'state2'_mean]intercept = 0
        test [y'y'docknot'state'_mean]intercept -
        [y'y'docknot'state2'_mean]_cons = 0
        test [y'y'docknot'state'_mean=y'y'docknot'state2'_mean]
disp ***
disp "++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++
+--------------------------------＋---------------------------------
"
disp "Absolute\ncompared to Montana and Pennsylvania\nforeach state2 in 27 39 {
    local mylabel3: variable label s\'state2\'
    local mylabel4 = substr ("mylabel3",8,.)
    disp "Absolute Docs \`y\' `mylabel2\' versus `mylabel4\''"
disp "==============================================================================
=========
================================="
qui suest y`y'docknot\'state\'
y`y'docknot\'state2'
[y`y'docknot\'state2\'_mean]slope = 0
test [y`y'docknot\'state\'_mean]slope -
[y`y'docknot\'state2\'_mean]year = 0
test [y`y'docknot\'state\'_mean]year -
[y`y'docknot\'state2\'_mean]intercept = 0
test [y`y'docknot\'state\'_mean]intercept -
[y`y'docknot\'state2\'_mean]_cons = 0
test [y`y'docknot\'state\'_mean=y`y'docknot\'state2\'_mean]
disp ""
disp "++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++
+--------------------------------＋---------------------------------
"
disp ""
disp "Slope Docs `y' `mylabel2' versus `mylabel4''

disp
"
qui suest y`y'docknot`state' y`y'docknot`state2'
[ y`y'docknot`state2'_mean]slope = 0
[ y`y'docknot`state2'_mean]year = 0
[ y`y'docknot`state2'_mean]intercept = 0
[ y`y'docknot`state2'_mean]_cons = 0

test [ y`y'docknot`state'_mean=y`y'docknot`state2'_mean]

="/Abs
olute compared to New Mexico and Kansas
foreach state2 in 32 17 {
    local mylabel3: variable label s`state2'
    local mylabel4 = substr ("mylabel3",8,.)

disp
"
qui suest y`y'docknot`state' y`y'docknot`state2'
[ y`y'docknot`state2'_mean]slope = 0
[ y`y'docknot`state2'_mean]year = 0
[ y`y'docknot`state2'_mean]intercept = 0
[ y`y'docknot`state2'_mean]_cons = 0

test [ y`y'docknot`state'_mean=y`y'docknot`state2'_mean]"
test [y'y'docknot'state'_mean=y'y'docknot'state2'_mean]
disp ""
disp
"+++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++
+++++++++++++++++++++
+++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++
"  disp ""
disp ""
}

//Nevada
local state 29

local mylabel : variable label s'state'
local mylabel2 = substr ("mylabel'",8,.)
//Slope compared to Massachusetts and New Mexico
foreach state2 in 22 32 {
    local mylabel3: variable label s'state2'
    local mylabel4 = substr ("mylabel3'",8,.)
disp "==============================================================================
=========================================
Slope Docs `y' `mylabel2' versus `mylabel4'
==============================================================================
=========================================
qui suest y'y'docknot'state' y'y'docknot'state2'
test [y'y'docknot'state'_mean]slope -
[y'y'docknot'state2'_mean]slope = 0
test [y'y'docknot'state'_mean]year -
[y'y'docknot'state2'_mean]year = 0
test [y'y'docknot'state'_mean]intercept -
[y'y'docknot'state2'_mean]intercept = 0
test [y'y'docknot'state'_mean]_cons -
[y'y'docknot'state2'_mean]_cons = 0
test [y'y'docknot'state'_mean=y'y'docknot'state2'_mean]
disp ""
disp
"+++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++
+++++++++++++++++++++
+++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++
"  disp ""
disp ""
}

//Absolute compared to Kansas and New York
foreach state2 in 17 33 {

280
local mylabel3: variable label s`state2'
local mylabel4 = substr (""mylabel3"",8,.)
disp

"=====================================================================================
=========================================

Absolute Docs `y' `mylabel2' versus `mylabel4''
disp

"=====================================================================================
=========================================

qui suest y`y'docknot`state' y`y'docknot`state2'
[y`y'docknot`state2'_mean]slope = 0

qui suest y`y'docknot`state' y`y'docknot`state2'
[y`y'docknot`state2'_mean]year = 0

qui suest y`y'docknot`state' y`y'docknot`state2'
[y`y'docknot`state2'_mean]intercept = 0

qui suest y`y'docknot`state' y`y'docknot`state2'
[y`y'docknot`state2'_mean]_cons = 0

disp """

disp """

disp """

}
test [y'y'docknot'state'_mean]slope - [y'y'docknot'state2'_mean]slope = 0
test [y'y'docknot'state'_mean]year - [y'y'docknot'state2'_mean]year = 0
test [y'y'docknot'state'_mean]intercept - [y'y'docknot'state2'_mean]intercept = 0
test [y'y'docknot'state'_mean]_cons - [y'y'docknot'state2'_mean]_cons = 0
test [y'y'docknot'state'_mean=y'y'docknot'state2'_mean]
disp ""
//West Virginia
local state 49

local mylabel : variable label s`state'
local mylabel2 = substr ("mylabel",8,)

//Slope compared to Alabama and Hawaii
foreach state2 in 1 12 {
    local mylabel3: variable label s`state2'
    local mylabel4 = substr ("mylabel3",8,)
    disp "Slope Docs `y' `mylabel2' versus `mylabel4''
}

qui suest y`y'docknot`state'_mean y`y'docknot`state2'_mean
test [y`y'docknot`state'_mean]slope = 0
    test [y`y'docknot`state'_mean]year = 0
    test [y`y'docknot`state'_mean]intercept = 0
    test [y`y'docknot`state'_mean]_cons = 0
}

//Absolute compared to Wyoming and Pennsylvania
foreach state2 in 51 39 {
    local mylabel3: variable label s`state2'
    local mylabel4 = substr ("mylabel3",8,)
    disp "Absolute Docs `y' `mylabel2' versus `mylabel4''
}
qui suest y'y'docknot state' y'y'docknot state2'
test [y'y'docknot state'_mean]slope -
[y'y'docknot state2'_mean]slope = 0
test [y'y'docknot state'_mean]year -
[y'y'docknot state2'_mean]year = 0
test [y'y'docknot state'_mean]intercept -
[y'y'docknot state2'_mean]intercept = 0
test [y'y'docknot state'_mean]_cons -
[y'y'docknot state2'_mean]_cons = 0
test [y'y'docknot state'_mean=y'y'docknot state2'_mean]
disp ""
disp""""""""""""""""""
"""""
""""
"
if `y'==2004 {

//Idaho
local state 13

count state13
local mylabel : variable label s`state'
local mylabel2 = substr (""""mylabel""",8,.)
//Slope compared to Louisiana and Oregon
foreach state2 in 19 38 {
local mylabel3: variable label s`state2'
local mylabel4 = substr (""""mylabel3""",8,.)
disp """"""""""""""""""""
disp """"Slope Docs `y' `mylabel2' versus `mylabel4''

disp """""""""""""""
qui suest y'y'docknot state' y'y'docknot state2'

}
test [y'y'docknot'state'_mean]slope -
[y'y'docknot'state2'_mean]slope = 0
test [y'y'docknot'state'_mean]year -
[y'y'docknot'state2'_mean]year = 0
test [y'y'docknot'state'_mean]intercept -
[y'y'docknot'state2'_mean]intercept = 0
test [y'y'docknot'state'_mean]_cons -
[y'y'docknot'state2'_mean]_cons = 0
test [y'y'docknot'state'_mean=y'y'docknot'state2'_mean]
disp ""'
disp
"+++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++
+++++++++++++++++
+++++++++++++++++++++++
"
disp ""
disp ""
}
//Absolute compared to New Hampshire and North Dakota
foreach state2 in 30 35 {
    local mylabel3: variable label s`state2'
    local mylabel4 = substr ("`mylabel3'",8,.)
disp
"+++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++
=================================================================
=========================================
"
disp "Absolute Docs `y' `mylabel2' versus `mylabel4'"
disp
"============================================================================
==
=========================================
"
qui suest y`y'docknot'state' y`y'docknot'state2'
test [y'y'docknot'state'_mean]slope -
[y'y'docknot'state2'_mean]slope = 0
test [y'y'docknot'state'_mean]year -
[y'y'docknot'state2'_mean]year = 0
test [y'y'docknot'state'_mean]intercept -
[y'y'docknot'state2'_mean]intercept = 0
test [y'y'docknot'state'_mean]_cons -
[y'y'docknot'state2'_mean]_cons = 0
test [y'y'docknot'state'_mean=y'y'docknot'state2'_mean]
disp ""'
disp
"+++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++
=========================================================================
"
disp ""
disp ""
}

285
//Oklahoma
local state 37

local mylabel : variable label s`state'
local mylabel2 = substr ("mylabel",8,.)
//Slope compared to Hawaii and Washington
foreach state2 in 12 48 {
    local mylabel3: variable label s`state2'
    local mylabel4 = substr ("mylabel3",8,.)
    disp "========================================================================
    ==
    "
    disp "Slope Docs `y' `mylabel2' versus `mylabel4''
    disp "==============================================================================
    =
    qui suest y`y'docknot`state' y`y'docknot`state2'
    [y`y'docknot`state2'_mean]slope = 0
    test [y`y'docknot`state' _mean]slope -
    [y`y'docknot`state2'_mean]slope
    test [y`y'docknot`state' _mean]year -
    [y`y'docknot`state2'_mean]year
    test [y`y'docknot`state' _mean]intercept -
    [y`y'docknot`state2'_mean]intercept
    test [y`y'docknot`state' _mean]_cons -
    [y`y'docknot`state2'_mean]_cons
    test [y`y'docknot`state' _mean=y`y'docknot`state2'_mean]
}
//Absolute compared to Alaska and Delaware
foreach state2 in 2 8 {
    local mylabel3: variable label s`state2'
    local mylabel4 = substr ("mylabel3",8,.)
    disp "Absolute Docs `y' `mylabel2' versus `mylabel4''"
disp "==========================================================================
=========================================
qui suest y'y'docknot'state' y'y'docknot'state2'
est [y'y'docknot'state'_mean]slope -
[y'y'docknot'state2'_mean]slope = 0
est [y'y'docknot'state'_mean]year -
[y'y'docknot'state2'_mean]year = 0
est [y'y'docknot'state'_mean]intercept -
[y'y'docknot'state2'_mean]intercept = 0
est [y'y'docknot'state'_mean]_cons -
[y'y'docknot'state2'_mean]_cons = 0
disp ""
disp "++++++++++++++++++++++++++++++++++
+++++++++++++++++++
""disp
""disp
""
}

//Texas
local state 44
local mylabel : variable label s`state'
local mylabel2 = substr ("`mylabel'",8,.)
//Slope compared to Rhode Island and Minnesota
foreach state2 in 40 24 {
    local mylabel3: variable label s`state2'
    local mylabel4 = substr ("`mylabel3'",8,.)
    disp "===============================================================
=========================================
Slope Docs `y' `mylabel2' versus `mylabel4'
==============================================================================
=========================================
qui suest y'y'docknot'state' y'y'docknot'state2'
est [y'y'docknot'state'_mean]slope -
[y'y'docknot'state2'_mean]slope = 0
est [y'y'docknot'state'_mean]year -
[y'y'docknot'state2'_mean]year = 0
est [y'y'docknot'state'_mean]intercept -
[y'y'docknot'state2'_mean]intercept = 0
test [y’y’docknot’state’_mean]_cons -
[y’y’docknot’state2’_mean]_cons = 0
test [y’y’docknot’state’_mean=y’y’docknot’state2’_mean]
disp ***
disp

"+++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++
+++++++  
+++++++++++++++++++++++++++++++++++++++++

disp ***
disp ***
}
//Absolute compared to Indiana and Kentucky
foreach state2 in 15 18 {
    local mylabel3: variable label s`state2'
    local mylabel4 = substr ("`mylabel3'",8,.)
disp

"==============================================================================
=========================================
Absolute Docs `y' `mylabel2' versus `mylabel4'"
disp

"==============================================================================
=========================================
qui suest y`y’docknot’state’ y`y’docknot’state2’
[y’y’docknot’state2’_mean]slope = 0
test [y’y’docknot’state’_mean]slope -
[y’y’docknot’state2’_mean]year = 0
test [y’y’docknot’state’_mean]year -
[y’y’docknot’state2’_mean]intercept = 0
test [y’y’docknot’state’_mean]_cons -
[y’y’docknot’state2’_mean]_cons = 0
test [y’y’docknot’state’_mean=y’y’docknot’state2’_mean]
disp ***
disp

"+++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++
+++++++++++++++++++++++++++++++++++++++++

disp ***
disp ***
}

if `y'==2005 {

}
//Georgia
local state 11

local mylabel : variable label s`state'
local mylabel2 = substr ("mylabel",8,.)
//Slope compared to DC and Hawaii
foreach state2 in 9 12 {
    local mylabel3: variable label s`state2'
    local mylabel4 = substr ("mylabel3",8,.)
    disp "=========================================
    disp "Slope Docs `y' `mylabel2' versus `mylabel4'
    disp "==============================
    qui suest y`y'docknot`state' y`y'docknot`state2'
    test [y`y'docknot`state'_mean]slope = 0
    test [y`y'docknot`state'_mean]year = 0
    test [y`y'docknot`state'_mean]intercept = 0
    test [y`y'docknot`state'_mean]_cons = 0
    disp """
    disp "+++++++++++++++++++++++++++++++++++++
    disp "+++++++++++++++++++++++++++++++++
    disp "++++++++++++++++++++++++++++++++++

    //Absolute compared to Delaware and Michigan
    foreach state2 in 8 23 {
        local mylabel3: variable label s`state2'
        local mylabel4 = substr ("mylabel3",8,.)
        disp "Absolute Docs `y' `mylabel2' versus `mylabel4"
        disp """
    }
}

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qui suest y'y'docknot`state' y'y'docknot`state2'
[ y'y'docknot`state2'_mean]slope = 0
[ y'y'docknot`state2'_mean]year = 0
[ y'y'docknot`state2'_mean]intercept = 0
[ y'y'docknot`state2'_mean]_cons = 0

doctype "++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++
                        +++++++++++++++++++++++++++++++++++++++++++"
doctype 

test [y'y'docknot'state'_mean]intercept -
[y'y'docknot'state2'_mean]intercept = 0

test [y'y'docknot'state'_mean]_cons -
[y'y'docknot'state2'_mean]_cons = 0

test [y'y'docknot'state'_mean]=y'y'docknot'state2'_mean]
disp ***
disp
"+++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++
+++++++test [y'y'docknot'state'_mean]slope -
[y'y'docknot'state2'_mean]slope = 0
test [y'y'docknot'state'_mean]year -
[y'y'docknot'state2'_mean]year = 0
test [y'y'docknot'state'_mean]intercept -
[y'y'docknot'state2'_mean]intercept = 0
test [y'y'docknot'state'_mean]_cons -
[y'y'docknot'state2'_mean]_cons = 0
test [y'y'docknot'state'_mean]=y'y'docknot'state2'_mean]
disp ***
disp
"+++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++
+++++++
local mylabel : variable label s`state'
local mylabel2 = substr ("mylabel",8,.)
//Slope compared to Montana and New Hampshire
foreach state2 in 27 30 {
    local mylabel3: variable label s`state2'
    local mylabel4 = substr ("mylabel",8,.)
    disp "==============================================================================
    ==========================================
    disp "Slope Docs `y' `mylabel2' versus `mylabel4'
    disp 
    "==============================================================================
    ===============================================================================
    qui suest y`y'docknot`state' y`y'docknot`state2'
    test [y`y'docknot`state'_mean]slope = 0
    [y`y'docknot`state2'_mean]slope = 0
    test [y`y'docknot`state'_mean]year = 0
    [y`y'docknot`state2'_mean]year = 0
    test [y`y'docknot`state'_mean]intercept = 0
    [y`y'docknot`state2'_mean]intercept = 0
    test [y`y'docknot`state'_mean]_cons = 0
    [y`y'docknot`state2'_mean]_cons = 0
    disp ***
    disp ***
    disp ***
    }

//Absolute compared to Maine and Hawaii
foreach state2 in 20 12 {
    local mylabel3: variable label s`state2'
    local mylabel4 = substr ("mylabel",8,.)
    disp "==============================================================================
    ===============================================================================
    qui suest y`y'docknot`state' y`y'docknot`state2'
    test [y`y'docknot`state'_mean]slope = 0
    [y`y'docknot`state2'_mean]slope = 0
test [y'y'docknot'state'_mean]year - 
[y'y'docknot'state2'_mean]year = 0

test [y'y'docknot'state'_mean]intercept - 
[y'y'docknot'state2'_mean]intercept = 0

test [y'y'docknot'state'_mean]_cons - 
[y'y'docknot'state2'_mean]_cons = 0

test [y'y'docknot'state'_mean=y'y'docknot'state2'_mean]
disp ""
disp
"++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++

+++++++++++++++++++++++++++++++++++" 
disp "" 
disp ""
}

//Missouri
local state 26

local mylabel : variable label s'state'
local mylabel2 = substr ("`mylabel'"",8,)

//Slope compared to Indiana and Vermont
foreach state2 in 15 46 {
    local mylabel3: variable label s'state2'
    local mylabel4 = substr ("`mylabel3'"",8,)

disp "" 
disp "Slope Docs `y' `mylabel2' versus `mylabel4'" 
disp "" 
disp ""
}
qui suest y'y'docknot'state' y'y'docknot'state2' 
[y'y'docknot'state2'_mean]slope = 0 

test [y'y'docknot'state'_mean]slope - 
[y'y'docknot'state2'_mean]slope = 0 

test [y'y'docknot'state'_mean]year - 
[y'y'docknot'state2'_mean]year = 0 

test [y'y'docknot'state'_mean]intercept - 
[y'y'docknot'state2'_mean]intercept = 0 

test [y'y'docknot'state'_mean]_cons - 
[y'y'docknot'state2'_mean]_cons = 0 

test [y'y'docknot'state'_mean=y'y'docknot'state2'_mean] 
disp ""
disp
"++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++
+++++++++++++++++++++++++++++++++++++++++
"disp ""
disp ""
}

//Absolute compared to Connecticut and New Hampshire
foreach state2 in 7 30 {
    local mylabel3: variable label s`state2'
    local mylabel4 = substr ("mylabel3",8,.)
    disp
    "==============================================================================
========================================="
    disp "Absolute Docs `y' `mylabel2' versus `mylabel4'
    "==============================================================================
========================================="
    qui suest y`y'docknot`state' y`y'docknot`state2'
    test [y`y'docknot`state'_mean]slope = 0
    test [y`y'docknot`state'_mean]year = 0
    test [y`y'docknot`state'_mean]intercept = 0
    test [y`y'docknot`state'_mean]_cons = 0
    disp ""
disp
"++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++
+++++++++++++++++++++++++++++++++++++++++
"
}

//South Carolina
local state 41

local mylabel : variable label s`state'
local mylabel2 = substr ("mylabel",8,.)
//Slope compared to Nebraska and DC
foreach state2 in 28 9 {
    local mylabel3: variable label s`state2'
    local mylabel4 = substr ("mylabel3",8,.)
    disp ""
disp "Slope Docs `y' `mylabel2' versus `mylabel4''

disp

"==============================================================================
=========================================

qui suest y`y'docknot`state' y`y'docknot`state2'

test [y`y'docknot`state'_mean]slope = 0

test [y`y'docknot`state'_mean]year = 0

test [y`y'docknot`state'_mean]intercept = 0

test [y`y'docknot`state'_mean]_cons = 0

{ //Absolute compared to Kentucky and Arizona

foreach state2 in 18 3 {

    local mylabel3: variable label s`state2'

    local mylabel4 = substr ("mylabel3"",8,.)

    disp "Absolute Docs `y' `mylabel2' versus `mylabel4''

    "==============================================================================
=========================================

qui suest y`y'docknot`state' y`y'docknot`state2'

test [y`y'docknot`state'_mean]slope = 0

test [y`y'docknot`state'_mean]year = 0

test [y`y'docknot`state'_mean]intercept = 0

test [y`y'docknot`state'_mean]_cons = 0

} 295
// Wisconsin
local state 50

local mylabel : variable label s`state'
local mylabel2 = substr ("mylabel",8,.)

//Slope compared to Vermont and Indiana
foreach state2 in 46 15 {
    local mylabel3: variable label s`state2'
    local mylabel4 = substr ("mylabel3",8,.)
    disp "Slope Docs \'y\' \`mylabel2\' versus \`mylabel4\'
    disp "==============================================================================
    disp "=========================================
    qui suest y`y'docknot`state'_mean=y`y'docknot`state2'_mean
    test [y`y'docknot`state'_mean]slope = 0
    test [y`y'docknot`state'_mean]year = 0
    test [y`y'docknot`state'_mean]intercept = 0
    test [y`y'docknot`state'_mean]_cons = 0
    disp ""
local mylabel3: variable label s`state2'
local mylabel4 = substr ("mylabel3","8,"

disp "==============================================================================
=========================================
Absolute Docs `y' `mylabel2' versus `mylabel4'
==============================================================================
=========================================
qui suest y`y'docknot`state' y`y'docknot`state2'
[ y`y'docknot`state2'_mean]slope = 0
  test [ y`y'docknot`state'_mean]slope - |
[ y`y'docknot`state2'_mean]year = 0
  test [ y`y'docknot`state'_mean]year - |
[ y`y'docknot`state2'_mean]intercept = 0
  test [ y`y'docknot`state'_mean]intercept - |
[ y`y'docknot`state2'_mean]_cons = 0
  test [ y`y'docknot`state'_mean=y`y'docknot`state2'_mean] |
  disp ""
  disp ""
  disp ""
incremental = 0
  disp ""
  disp ""
}
}
drop intercept slope
}
log close _all
*/

//This is for statute of limitations, I'm going to run

//
//Graphs for the paper

297
// Malpractice payments per 1,000 physicians and
egen totalstates = sum (r_cn), by (year)
label var totalstates "Total States with Caps on Noneconomic Damages"
egen totalpop = sum (pop), by (year)
label var totalpop "US Population"
egen totaldoc = sum (doc), by (year)
label var totaldoc "US Physicians"
egen totalpayments = sum (n), by (year)
label var totalpayments "Malpractice Payments"
gen totalperdoc = totalpayments/totaldoc*1000
label var totalperdoc "Malpractice Payments per 1,000 Physicians"
gen totalperpop = totalpayments/totalpop*500000
label var totalperpop "Malpractice Payments per 500,000 Population"

graph twoway (bar totalstates year) (scatter totalperdoc year), title ("Malpractice Settlements in United States" "1991-2010") ytitle () xtitle ("Year") xlabel (1991 1995 2000 2005 2010) ylabel (10 15 20 25 30 35) legend (col (1) label (1 "Number of States with Caps on Noneconomic Damages") label (2 "Number of Paid Claims per 1,000 Physicians"))
graph save Graph "`home`\Graphs\Stata Graphs\Paper - Per Physicians.gph", replace
graph export "`home`\Graphs\Paper\Paper - Per Physicians.png", replace width (`size')
graph twoway (bar totalstates year) (scatter totalperpop year), title ("Malpractice Settlements in United States" "1991-2010") ytitle () xtitle ("Year") xlabel (1991 1995 2000 2005 2010) ylabel (10 15 20 25 30 35 40) legend (col (1) label (1 "Number of States with Caps on Noneconomic Damages") label (2 "Number of Paid Claims per 500,000 People"))
graph save Graph "`home`\Graphs\Stata Graphs\Paper - Per Population.gph", replace
graph export "`home`\Graphs\Paper\Paper - Per Population.png", replace width (`size')

*

// Look at whether there was a significant impact after To Err is Human
gen past2001 = (year>2001)
gen past2001interaction = max (0, year-2001)
xi: regress perdoc year past2001 past2001interaction i.state
regress perdoc year past2001 past2001interaction
forvalues x = 1/51 {
    local mylabel : variable label s`x'
    local mylabel2 = substr ("mylabel",8,.)
    regress perdoc year past2001 past2001interaction if s`x'
    qui eststo sol3`x', title ("`x` `mylabel2' Docs 2001")
}
estout using "'home'\Results\Results Docs 2001.csv", delimiter (","), cells (b se t p) stats (r2 aic bic N p) label replace
estout using "'home'\Results\Time Stamp\Results Docs 2001 - 'time_string'.csv", delimiter (","), cells (b se t p) stats (r2 aic bic N p) label replace
eststo clear

//this throws everything into a big regression and sees what is significant (this is a combined model to see the average effect, not a state-specific analysis)
xi: regress perdoc year r_cn r_cp r_ct r_sr r_cs r_pe r_pp r_pcf interaction i.state
Appendix H – Medicare National Coverage Decisions on Bariatric Surgery

1979 National Coverage Determination for Gastric Bypass Surgery for Obesity

Benefit Category
Physicians' Services
Note: This may not be an exhaustive list of all applicable Medicare benefit categories for this item or service.

Item/Service Description

Gastric bypass surgery, which is a variation of the gastrojejunostomy, is performed for patients with extreme obesity.

Indications and Limitations of Coverage

Gastric bypass surgery for extreme obesity is covered under the program if (1) it is medically appropriate for the individual to have such surgery; and (2) the surgery is to correct an illness which caused the obesity or was aggravated by the obesity.

Cross Reference
See §§40.5 and 100.8 of the NCD Manual.
2006 National Coverage Determination for Bariatric Surgery for Treatment of Morbid Obesity

Benefit Category

Incident to a physician's professional Service
Inpatient Hospital Services
Physicians' Services

Note: This may not be an exhaustive list of all applicable Medicare benefit categories for this item or service.

Item/Service Description

A. General

Bariatric surgery procedures are performed to treat comorbid conditions associated with morbid obesity. Two types of surgical procedures are employed. Malabsorptive procedures divert food from the stomach to a lower part of the digestive tract where the normal mixing of digestive fluids and absorption of nutrients cannot occur. Restrictive procedures restrict the size of the stomach and decrease intake. Surgery can combine both types of procedures.

The following are descriptions of bariatric surgery procedures:

a. Roux-en-Y Gastric Bypass (RYGBP)

The RYGBP achieves weight loss by gastric restriction and malabsorption. Reduction of the stomach to a small gastric pouch (30 cc) results in feelings of satiety following even small meals. This small pouch is connected to a segment of the jejunum, bypassing the duodenum and very proximal small intestine, thereby reducing absorption. RYGBP procedures can be open or laparoscopic.

b. Biliopancreatic Diversion with Duodenal Switch (BPD/DS)
BPD achieves weight loss by gastric restriction and malabsorption. The stomach is partially resected, but the remaining capacity is generous compared to that achieved with RYGBP. As such, patients eat relatively normal-sized meals and do not need to restrict intake radically, since the most proximal areas of the small intestine (i.e., the duodenum and jejunum) are bypassed, and substantial malabsorption occurs. The partial BPD with duodenal switch is a variant of the BPD procedure. It involves resection of the greater curvature of the stomach, preservation of the pyloric sphincter, and transection of the duodenum above the ampulla of Vater with a duodeno-ileal anastomosis and a lower ileo-ileal anastomosis. BPD/DS procedures can be open or laparoscopic.

c. Adjustable Gastric Banding (AGB)

AGB achieves weight loss by gastric restriction only. A band creating a gastric pouch with a capacity of approximately 15 to 30 cc’s encircles the uppermost portion of the stomach. The band is an inflatable doughnut-shaped balloon, the diameter of which can be adjusted in the clinic by adding or removing saline via a port that is positioned beneath the skin. The bands are adjustable, allowing the size of the gastric outlet to be modified as needed, depending on the rate of a patient’s weight loss. AGB procedures are laparoscopic only.

d. Sleeve Gastrectomy

Sleeve gastrectomy is a 70%-80% greater curvature gastrectomy (sleeve resection of the stomach) with continuity of the gastric lesser curve being maintained while simultaneously reducing stomach volume. It may be the first step in a two-stage procedure when performing RYGBP. Sleeve gastrectomy procedures can be open or laparoscopic.

e. Vertical Gastric Banding (VGB)

VGB achieves weight loss by gastric restriction only. The upper part of the stomach is stapled, creating a narrow gastric inlet or pouch that remains connected with the remainder of the stomach. In addition, a non-adjustable band is placed around this new inlet in an attempt to prevent future enlargement of the stoma (opening). As a result, patients experience a sense of fullness after eating small meals. Weight loss from this procedure results entirely from eating less.

Indications and Limitations of Coverage
B. Nationally Covered Indications

Open and laparoscopic Roux-en-Y gastric bypass (RYGBP), open and laparoscopic Biliopancreatic Diversion with Duodenal Switch (BPD/DS), and laparoscopic adjustable gastric banding (LAGB) are covered for Medicare beneficiaries who have a body-mass index ≥ 35, have at least one co-morbidity related to obesity, and have been previously unsuccessful with medical treatment for obesity. These procedures are only covered when performed at facilities that are: (1) certified by the American College of Surgeons as a Level 1 Bariatric Surgery Center (program standards and requirements in effect on February 15, 2006); or (2) certified by the American Society for Bariatric Surgery as a Bariatric Surgery Center of Excellence (program standards and requirements in effect on February 15, 2006).

A list of approved facilities and their approval dates are listed and maintained on the CMS Coverage Web site at http://www.cms.hhs.gov/center/coverage.asp, and published in the Federal Register.

C. Nationally Non-covered Indications

The following bariatric surgery procedures are non-covered for all Medicare beneficiaries:

- Open adjustable gastric banding
- Open and laparoscopic sleeve gastrectomy; and
- Open and laparoscopic vertical banded gastroplasty.

The two previously non-coverage determinations remain unchanged - Gastric Balloon (Section 100.11) and Intestinal Bypass (Section 100.8).

D. Other

(This NCD last reviewed February 2006.)

Cross Reference

See §§40.5, 100.8, and 100.11.
2009 National Coverage Determination for Bariatric Surgery for Treatment of

Morbid Obesity

Benefit Category

Incident to a physician's professional Service

Inpatient Hospital Services

Physicians' Services

Note: This may not be an exhaustive list of all applicable Medicare benefit categories for this item or service.

Item/Service Description

A. General

Bariatric surgery procedures are performed to treat comorbid conditions associated with morbid obesity. Two types of surgical procedures are employed. Malabsorptive procedures divert food from the stomach to a lower part of the digestive tract where the normal mixing of digestive fluids and absorption of nutrients cannot occur. Restrictive procedures restrict the size of the stomach and decrease intake. Surgery can combine both types of procedures.

The following are descriptions of bariatric surgery procedures:

1. Roux-en-Y Gastric Bypass (RYGBP)

The RYGBP achieves weight loss by gastric restriction and malabsorption. Reduction of the stomach to a small gastric pouch (30 cc) results in feelings of satiety following even small meals. This small pouch is connected to a segment of the jejunum, bypassing the duodenum and very proximal small intestine, thereby reducing absorption. RYGBP procedures can be open or laparoscopic.

2. Biliopancreatic Diversion with Duodenal Switch (BPD/DS)
BPD achieves weight loss by gastric restriction and malabsorption. The stomach is partially resected, but the remaining capacity is generous compared to that achieved with RYGBP. As such, patients eat relatively normal-sized meals and do not need to restrict intake radically, since the most proximal areas of the small intestine (i.e., the duodenum and jejunum) are bypassed, and substantial malabsorption occurs. The partial BPD/DS is a variant of the BPD procedure. It involves resection of the greater curvature of the stomach, preservation of the pyloric sphincter, and transection of the duodenum above the ampulla of Vater with a duodeno-ileal anastomosis and a lower ileo-ileal anastomosis. BPD/DS procedures can be open or laparoscopic.

3. Adjustable Gastric Banding (AGB)

AGB achieves weight loss by gastric restriction only. A band creating a gastric pouch with a capacity of approximately 15 to 30 cc’s encircles the uppermost portion of the stomach. The band is an inflatable doughnut-shaped balloon, the diameter of which can be adjusted in the clinic by adding or removing saline via a port that is positioned beneath the skin. The bands are adjustable, allowing the size of the gastric outlet to be modified as needed, depending on the rate of a patient’s weight loss. AGB procedures are laparoscopic only.

4. Sleeve Gastrectomy

Sleeve gastrectomy is a 70%-80% greater curvature gastrectomy (sleeve resection of the stomach) with continuity of the gastric lesser curve being maintained while simultaneously reducing stomach volume. It may be the first step in a two-stage procedure when performing RYGBP. Sleeve gastrectomy procedures can be open or laparoscopic.

5. Vertical Gastric Banding (VGB)

The VGB achieves weight loss by gastric restriction only. The upper part of the stomach is stapled, creating a narrow gastric inlet or pouch that remains connected with the remainder of the stomach. In addition, a non-adjustable band is placed around this new inlet in an attempt to prevent future enlargement of the stoma (opening). As a result, patients experience a sense of fullness after eating small meals. Weight loss from this procedure results entirely from eating less. VGB procedures are essentially no longer performed.

Indications and Limitations of Coverage
B. Nationally Covered Indications

Effective for services performed on and after February 21, 2006, Open and laparoscopic Roux-en-Y gastric bypass (RYGBP), open and laparoscopic Biliopancreatic Diversion with Duodenal Switch (BPD/DS), and laparoscopic adjustable gastric banding (LAGB) are covered for Medicare beneficiaries who have a body-mass index \( \geq 35 \), have at least one co-morbidity related to obesity, and have been previously unsuccessful with medical treatment for obesity. These procedures are only covered when performed at facilities that are: (1) certified by the American College of Surgeons as a Level 1 Bariatric Surgery Center (program standards and requirements in effect on February 15, 2006); or (2) certified by the American Society for Bariatric Surgery as a Bariatric Surgery Center of Excellence (program standards and requirements in effect on February 15, 2006).

Effective for services performed on or after February 12, 2009, the Centers for Medicare & Medicaid Services (CMS) determines that Type 2 diabetes mellitus is a co-morbidity for purposes of this NCD.

A list of approved facilities and their approval dates are listed and maintained on the CMS Coverage Web site at [http://www.cms.gov/Medicare/Medicare-General-Information/MedicareApprovedFacilities/Bariatric-Surgery.html](http://www.cms.gov/Medicare/Medicare-General-Information/MedicareApprovedFacilities/Bariatric-Surgery.html), and published in the Federal Register.

C. Nationally Non-Covered Indications

The following bariatric surgery procedures are non-covered for all Medicare beneficiaries:

- Open adjustable gastric banding;
- Open and laparoscopic sleeve gastrectomy; and,
- Open and laparoscopic vertical banded gastroplasty.

The two previous non-coverage determinations remain unchanged - Gastric Balloon (Section 100.11) and Intestinal Bypass (Section 100.8).

D. Other

N/A

(This NCD last reviewed February 2009.)
Cross Reference

See §§40.5, 100.8, and 100.11.
Appendix I – Bariatric Surgery Rates by Individual Procedure
Appendix J – Bariatric Surgery Rates by Medicare, Medicaid and Other Payers
Appendix K – Medicare Reimbursement Change Code

//David Muhlestein
//This will take rates of bariatric surgeries and will estimate changes over time and compare Medicare to non-medicare types
//2013-04-02 - work on base file
//2013-04-03 - continue work on base file
//2013-04-04 - working more on testing hypotheses and graphing results
//2013-04-11 - I'm doing the linear spline with just 2003 and later results

//the directory where my results are stored
local dir "D:\Dropbox\My Research\Medicare Payment Changes\Results"
//where the statistical results are output
local dir2 "D:\Dropbox\My Research\Medicare Payment Changes\Results\Statistical Results"
//where the graphs are output
local dir3 "D:\Dropbox\My Research\Medicare Payment Changes\Results\Graphs"
//crosswalk do file
local crosswalk "D:\Dropbox\My Research\Medicare Payment Changes\Programming\Crosswalk - 2013-04-04.do"

local size 1200 //this size of the output graphs

local degree 7 //this is the degree I want to do with the fractional polynomial for the linear spline model
local degree2 2 //this is the degree I want to do with the fractional polynomial for the non-spline model

//crosswalk file

set more off
set matsize 800
//This will take all the files in the results directory, transpose them and save them to stata datasets
cd "dir"
local myfiles: dir . files ".csv"

//get the crosswalk ready
do "crosswalk"

local counter = 0
foreach file of local myfiles {
    local counter = `counter'+1
    display "file"
    qui insheet using "file", clear nonames
    local n = c(k) //how many variables there are
    foreach l of varlist v2-v`n'{
        qui replace `l' = v1 if `l'=""
    }
    qui sxpose, clear force firstnames //transposes the files
    local newfile = substr ("file",1,length ("file")-4) //cuts off the file extension to save as a stata dataset
    local resultsname = "Results - " + substr ("newfile",20,length ("newfile")) //the name of the results file
    local shortfile = substr ("file",21,length ("file")-25) //a shortened version of the file name without the initial summary statistics
    qui destring *, replace //convert all the numeric values from string to numbers
    //this will rename all the variables for consistency
    local j = 1
    foreach var of varlist * {
        rename `var' v`j'
        local j = `j'+1
    }
}

//this shows how many lines there are between new sets of data to analyze; it depends on the number of comparison groups; for example, if "other" is found in the file name, it means there are 7 variables in each set (i.e.: title, medicare, medicare SD, medicaid, medicaid SD, other, other SD), which is 3 different groups
local n = 1*strmatch ("newfile","*otal*") + 2*strmatch ("newfile","*on-*") + 3*strmatch ("newfile","*ther*")
//the r value is telling how many observations there are in a year (1 for year, 4 for quarter, 12 for month)
local r = 1*strmatch ("newfile", "ear") + 4*strmatch ("newfile", "uarter") + 12*strmatch ("newfile", "onth") //this shows how many lines there are between new sets of data to analyze; it depends on the number of
//the unit of analysis
if `r'==1 local unit "Year"
if `r'==4 local unit "Quarter"
if `r'==12 local unit "Month"
capture gen year = string (v1)
capture gen year = substr (v1,1,4)
destring year, replace
gen month = (mod (_n-1,12)/12)
replace month = (mod (_n-1,4)/4) if `r'==4
replace month = 0 if `r'==1
gen time = year+month
egen min_time = min (time)
egen max_time = max (time)
gen timec = (time-min_time)/(max_time-min_time)*100 //this generates a time variable from 0 to 100 based on the minimum and maximum of time
drop min_time max_time
merge m:1 year using crosswalk
drop _merge

//this will do all the analyses

//5/24/2005 - CMS opens consideration (day 144 of year: 2005.3945 year)
//02/21/2006 - CMS releases final decision (day 52 of year: 2006.1427 year)
gen consideration = 2005.3945
gen proposed = 2005.8959
gen final = 2006.1427
gen effect = round (2006.1427,1/`r')
gen ineffect = (time>=effect)
label variable ineffect "Intercept" //whether the change is in effect represents a change in intercept
//the interaction term which represents a change in slope
gen interaction = max (0,time-effect)
label variable interaction "Slope" //the interaction is the change in slope
eststo clear

forvalues k = 0/21 {
    //this pulls out the title
    local t = 3 + (`k' * ( (`n'*2)+1))
    local titlefull = v' t'[1]
    local title = subinstr ("titlefull", ",", "", )

    forvalues j = 1/`n' {
        //this is which value to regress
        local v = 2 + (`k' * ( (`n'*2)+1)) + 2*`j'

        //this labels everything
        local varname "Total"
        if `n'==2 & `j'==1 local varname "Medicare"
        if `n'==2 & `j'==2 local varname "Non-Medicare"
        if `n'==3 & `j'==1 local varname "Medicare"
        if `n'==3 & `j'==2 local varname "Medicaid"
        if `n'==3 & `j'==3 local varname "Non-Medicare and Non-Medicaid"
        label var v`v' "varname"
        local se = `v'+1
        local varnamese = "varname" + " - Standard Error"
        label var v`se' "varnamese"
        if `j'==1 local v1 = `v'
        if `j'==2 local v2 = `v'
        if `j'==3 local v3 = `v'

        //convert the following to title case
        local shortfile = proper ("shortfile")
        local title = proper ("title")
        local titlefull = proper ("titlefull")

        //this calculates the rate of procedures (adjusted for population)
        gen rate_v`v' = v`v'/totalpop
        if "varname"=="Medicare" replace rate_v`v' = v`v'/medicare
        if "varname"=="Medicaid" replace rate_v`v' = v`v'/medicaid
        if "varname"=="Non-Medicare" replace rate_v`v' = v`v'/ (totalpop-medicare)
        if "varname"=="Other" replace rate_v`v' = v`v'/ (totalpop-medicare-medicaid)
        //the rate is per 100,000 people
        replace rate_v`v' = rate_v`v' * 100000
//this calculates the rate of the standard error of the procedures (adjusted for population)

```
gen rate_v`v'_se = v`se'/totalpop
if "`varname'"=="Medicare" replace rate_v`v'_se = v`se'/medicare
if "`varname'"=="Medicaid" replace rate_v`v'_se = v`se'/medicaid
if "`varname'"=="Non-Medicare" replace rate_v`v'_se = v`se'/(totalpop-medicare)
if "`varname'"=="Other" replace rate_v`v'_se = v`se'/(totalpop-medicare-medicaid)
//the rate is per 100,000 people
replace rate_v`v'_se=rate_v`v'_se*100000
```

//This graphs the rate over time (no fit)
```
gen rate_v`v' ub = rate_v`v'+1.96*rate_v`v'_se //upper bound of the raw rate
gen rate_v`v' lb = rate_v`v' - 1.96*rate_v`v'_se //lower bound of the raw rate
```

//This is a linear spline model with the model yhat = B0 + B1*time + B2*(time-time_enacted) but only where (time-time_enacted > 0) + B3*change_is_in_effect
```
fracpoly degree 2 or maybe 3 fracpoly, degree (degree): regress rate_v`v' timec interaction ineffect if rate_v`v'>0 & time>=2003 predict spline_rate_v`v' //predicted value
predict spline_rate_v`v'_se, stdp //predicted value's standard error
```
```
gen spline_rate_v`v' ub = spline_rate_v`v' + 1.96*spline_rate_v`v'_se //upper bound
```
```
gen spline_rate_v`v' lb = spline_rate_v`v' - 1.96*spline_rate_v`v'_se //lower bound
```
label var spline_rate_v`v' ub "Upper Bound of v`v' estimate"
label var spline_rate_v`v' lb "Lower Bound of v`v' estimate"
qui eststo, title ("Rate Spline `title' - `varname'")
```
```
//This graphs the result and saves the graph
```
twoway line rate_v'v' final, lwidth (medthick) lcolor (green) || line rate_v'v' proposed, lwidth (medthick) lcolor (gold) || line rate_v'v' consideration, lwidth (medthick) lcolor (blue) || rcap rate_v'v'_ub rate_v'v'_lb time, color (eltblue) || scatter rate_v'v' time, color (navy) || line spline_rate_v'v' time if lineffect, lwidth (medthick) color (red) || line spline_rate_v'v' time if ineffect, lwidth (medthick) color (red) || , legend (textwidth (75) order (5 "Rate of Procedures" 4 "95% Confidence Interval" 6 "Degree Polynomial Fit" (Linear Spline) 3 "CMS Begins Consideration" 2 "Proposed Coverage" "Determination" 1 "Final Coverage" "Determination") ) ytitle ("Procedures per 100,000 People") xtitle ("Year") xlabel (1998 "1998" 1999 "1999" 2000 "2000" 2001 "2001" 2002 "2002" 2003 "2003" 2004 "2004" 2005 "2005" 2006 "2006" 2007 "2007" 2008 "2008" 2009 "2009" 2010 "2010" 2011 "2011") title ("Rate of Bariatric Surgery per `unit' `varname' Population") note ("titlefull", span) //this has 95% confidence intervals plotted for the trend line, too //twoway rarea spline_rate_v'v'_ub spline_rate_v'v'_lb time if ~ineffect, color (gs12) || rarea spline_rate_v'v'_ub spline_rate_v'v'_lb time if ineffect, color (gs12) || line rate_v'v' final, lwidth (medthick) lcolor (green) || line rate_v'v' proposed, lwidth (medthick) lcolor (gold) || line rate_v'v' consideration, lwidth (medthick) lcolor (blue) || rcap rate_v'v'_ub rate_v'v'_lb time, color (eltblue) || scatter rate_v'v' time, color (navy) || line spline_rate_v'v' time if lineffect, lwidth (medthick) color (red) || line spline_rate_v'v' time if ineffect, lwidth (medthick) color (red) || , legend (textwidth (75) order (7 "Rate of Procedures per `unit'") 6 "95% Confidence Interval" 8 "Degree Polynomial Fit" 1 "95% Confidence Interval" 5 "CMS Begins Consideration" 4 "Proposed Coverage" "Determination" 3 "Final Coverage" "Determination") ) ytitle ("Procedures per 100,000 People") xtitle ("Year") xlabel (1998 "1998" 1999 "1999" 2000 "2000" 2001 "2001" 2002 "2002" 2003 "2003" 2004 "2004" 2005 "2005" 2006 "2006" 2007 "2007" 2008 "2008" 2009 "2009" 2010 "2010" 2011 "2011") title ("Rate Nonspline `varname' Population") note ("titlefull",, span) graph save "dir3\Stata\Spline Rate 2003 (d'degree') - `shortfile' - `title' - `varname'.gph", replace graph export "dir3\Spline Rate 2003 (d'degree') - `shortfile' - `title' - `varname'.png", replace width ('size') //This is the non-spline model (will use higher degrees fracpoly) fracpoly, degree (degree2): regress rate_v'v' timec if rate_v'v'>0 predict nonspline_rate_v'v', //predicted value predict nonspline_rate_v'v'_se, stdp //predicted value's standard error gen nonspline_rate_v'v'_se = nonspline_rate_v'v' + 1.96*nonspline_rate_v'v'_se //upper bound gen nonspline_rate_v'v'_lb = nonspline_rate_v'v' - 1.96*nonspline_rate_v'v'_se //lower bound qui eststo, title ("Rate Nonspline `title' - `varname'") //This graphs the result and saves the graph twoway line rate_v'v' final, lwidth (medthick) lcolor (green) || line rate_v'v' proposed, lwidth (medthick) lcolor (gold) || line rate_v'v' consideration, lwidth (medthick) lcolor

//this has 95% confidence intervals plotted for the trend line

//this graphs the non-spline graphs on the same graph

//Medicare, Medicaid and Other
if (`n'==3 & `j'==3) {

twoway line rate_v'v' final, lwidth (medthick) lcolor (green) || line rate_v'v' proposed, lwidth (medthick) lcolor (gold) || line rate_v'v' consideration, lwidth (medthick) lcolor (blue) || scatter rate_v'v'1' time, color (navy) || scatter rate_v'v'2' time, color (maroon) || scatter rate_v'v'3' time, color (emerald) || line nonspline_rate_v'v'1' time, lwidth (medthick) color (red) || line nonspline_rate_v'v'2' time, lwidth (medthick) color (pink) || line nonspline_rate_v'v'3' time, lwidth (medthick) color (magenta) || , legend (textwidth (75) order (4 "Medicare" 7 "Polynomial Fit" 5 "Medicaid" 8 "Polynomial Fit" 6 "All Other Payers") ytitle ("Procedures per 100,000 People" "") xtitle ("Year") xlabel (1998 "1998" 1999 "1999" 2000 "2000" 2001 "2001" 2002 "2002" 2003 "2003" 2004 "2004" 2005 "2005" 2006 "2006" 2007 "2007" 2008 "2008" 2009 "2009" 2010 "2010" 2011 "2011") title ("Rate of Bariatric Surgery per `unit', by Payer") note ("titlefull'", span)

//graph save "dir3\Stata\Non-Spline Rate (d'degree2') - shortfile' - title' - varname'.gph", replace
//graph export "dir3\Non-Spline Rate (d'degree2') - shortfile' - title' - varname'.png", replace width ('size')
// Medicare and Other
//
if (`n' == 2 & `j' == 2) {
    twoway line rate_v'v' final, lwidth (medthick) lcolor (green) ||
           line rate_v'v' proposed, lwidth (medthick) lcolor (gold) ||
           line rate_v'v' consideration, lwidth (medthick) lcolor (blue) ||
           scatter rate_v'v1' time, color (navy) ||
           scatter rate_v'v2' time, color (maroon) ||
           line nonspline_rate_v'v1' time, lwidth (medthick) color (red) ||
           line nonspline_rate_v'v2' time, lwidth (medthick) color (magenta) ||
           legend (textwidth (75) order (4 "Medicare" 6 "Polynomial Fit" 5 "All Non-Medicare Payers" 7 "Polynomial Fit" 3 "CMS Begins Consideration" 2 "Proposed Coverage" "Determination" 1 "Final Coverage" "Determination"))
    ytitle ("Procedures per 100,000 People" " " " "")
    xtitle ("Year")
            2009 "2009" 2010 "2010" 2011 "2011")
    title ("Rate of Bariatric Surgery per 'unit', by Payer")
    note (""titlefull", span)
    graph save "dir3\Stata\Combined - Non-Spline Rate (d'degree2') -
               'shortfile' - 'title'.gph", replace
    graph export "dir3\Combined - Non-Spline Rate (d'degree2') -
               'shortfile' - 'title'.png", replace width ('size')
}
}

estout using "dir2'\resultsname'\.csv", delimiter ("" )
cells (b se t p) label replace
drop Itim*
save "newfile", replace
}
cap erase crosswalk.dta