ESSAYS IN PUBLIC AND HEALTH ECONOMICS

By

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ABSTRACT

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This dissertation consists of three empirical studies in public and health economics. In the first chapter I use inpatient level hospital data to examine the effect of state tort reforms on physician behavior. For the second chapter, I evaluate the impact of a large expansion in public health insurance for children on the labor supply of single mothers. In the third chapter I describe trends in health insurance coverage during the Great Recession, and investigate the degree to which business cycle variation accounts for the large decline in insurance coverage among adults during the recession.

The first dissertation chapter explores the relationship between medical malpractice laws and the behavior of medical providers. Physician incentives in the current medical malpractice system may encourage socially wasteful behavior if doctors, out of liability concerns, provide medical services to patients of little benefit relative to the cost. Tort reform is often touted as a way to reduce liability induced provision of costly and unnecessary medical services. Empirical studies that assess the effect of tort reform on physician behavior offer mixed evidence of a behavioral response. Using a source of inpatient data not previously used in this literature, I investigate potential explanations for this lack of consensus. Results indicate some evidence that tort reforms influence physician behavior in a sample of heart attack patients, but not in a sample of stroke patients. These results are not robust to different specifications, however. I conclude that there is little evidence of a relationship between tort reform and hospital treatment for AMI or stroke.

In the second chapter, I evaluate the implementation of the State Children's Health Insurance Program (SCHIP) and its impact on the labor supply of single mothers. SCHIP, established in 1997, is an important source of health care access for children in near poor families. As with other means tested government programs, a worry is that program eligibility rules distort parental labor supply decisions. Because states have a large amount of flexibility in the design and administration of their SCHIP programs, there is considerable heterogeneity in state eligibility rules. Consequently, reduced form methods that associate SCHIP eligibility policies with labor supply must average outcomes over a diverse set of households with varying incentives for insurance coverage and employment. With data from the March Current Population Survey (CPS), I evaluate the program's effect on the labor supply using an instrumental variables estimation strategy that relates child insurance coverage with program eligibility rules. Results suggest that SCHIP led to an increase in public coverage and private insurance crowd-out, with no effects on maternal work behavior.

For the third chapter, I investigate adult health insurance coverage during the Great Recession (2007-2009). The Great Recession is associated with large reductions in employment and health insurance coverage. However, it is not clear that the decline in health insurance coverage can be completely attributed to the recession. Using data from the CPS, I relate health insurance coverage for working age adults with state level measures of employment. Estimates from this model are used to generate out of sample predictions of health insurance coverage during the Great Recession. Results show that the proportion of adults without insurance coverage during the recession is greater than the level predicted by the regression model. These results are sensitive to different specifications of the model, however.

To my parents, for their constant support and encouragement.
To Kristen, whom I love.

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CHAPTER 1
TORT REFORM, MEDICAL EXPENDITURES, AND PATIENT OUTCOMES

I. Introduction

In the United States, rising medical costs and large government budget deficits have renewed focus on reducing expenses associated with medical care provision. While there are many methods that may achieve this goal, it is often argued that the medical malpractice system is a large source of the cost growth and should be reformed. Expenses directly related to the medical malpractice industry (such as malpractice payments, liability insurance premiums, and legal expenses) are relatively small: Mello et al. (2010) report that existing research shows these direct costs to be less than 2% of total health care spending. Nevertheless, it is possible that indirect expenses associated with medical malpractice may potentially be much larger.[1] Indirect expenses of malpractice litigation include defensive medicine, a phenomenon that occurs when the threat of litigation causes doctors to prescribe medical services of relatively little benefit to patients (Kessler and McClellan 1996).

States adopt tort reforms in an effort to control expenses related to medical malpractice. Tort reform may lessen an individual's incentive to file a malpractice claim, primarily by reducing the amount of damages they can pursue. In response, physician perceptions of malpractice risk may decline, lowering the provision of malpractice induced medical services. This is a frequent justification for tort reform. Former United States Senator Judd Gregg argues, "We cannot possibly reduce the cost of health care in this country without reducing defensive medicine" (Langel, 2009). However, it is important to note that the liability system is intended to induce physicians to take appropriate precaution when treating patients. Studdert et al. (2004) state that "lawsuits deter physicians [from unsafe practice] by reminding those who wish to avoid

[1] Generally, indirect costs are the result of the malpractice system's effect on physician behavior.

the emotional and financial costs of litigation that they must take care" (p.283). Limits to liability may reduce malpractice induced behavior and health care costs, but may also harm patient outcomes if such services have some positive benefit. The critical question is whether costs from such behavior exceed the benefits.

In order to address this question, numerous studies have been conducted to evaluate the effect of tort reform on medical expenses, procedure choice, and patient outcomes. This literature has not reached a consensus regarding these relationships. There are two primary explanations for the lack of consensus. Researchers have analyzed different populations of patients. This inhibits comparison across studies if the effect of malpractice pressure on medical treatment varies across patient groups. In addition, the specification of the tort reform variable varies across studies. Some authors (Kessler and McClellan 1996, 2002; Sloan and Shadle 2009) group reforms into direct and indirect categories. Others (Currie and MacLeod, 2008) consider separately the effect of each individual reform. Critically, both tort variable specification and the population of interest vary across studies. This makes it difficult to isolate either factor as an explanation of the literature's mixed results.

This paper seeks to add to the literature in two important ways. First, I evaluate the effect of state tort reform laws on patient outcomes and measures of medical resource use.[2] Two samples, patients hospitalized with a primary diagnosis of either heart attack (acute myocardial infarction, or AMI) or stroke, are derived from a nationally representative data set of hospital inpatient stays unique to the literature. As opposed to Kessler and McClellan (1996 and 2002) and Sloan and Shadle (2009), who focus only on Medicare patients with an initial hospitalization, this is an all-payer data set that is representative of all hospital inpatient

[2] I use two measures of medical resource use: total charges and length of stay. There is good reason to think that charges are not well correlated with actual medical costs (Reinhardt, 2006). Consequently, I use length of stay as an additional measure of resource use and medical service intensity.

admissions in the country. Secondly, I seek to determine if either the specification of the tort variable or the patient population is important in accounting for the literature's results. I test for the existence of malpractice induced behavior using two specifications of the tort variable across similar disease groups – one similar to the indirect and direct classifications used by Kessler and McClellan (1996 and 2002) and Sloan and Shadle (2009), and another similar to the approach of Currie and MacLeod (2008) that considers individual tort reforms. The paper is thus an attempt to reconcile the different methods used to estimate the effect of tort reform on medical practice.

Results show that several reforms are associated with at least one measure of resource use in the AMI sample. Similar associations are found across both tort reform specifications, suggesting that estimates of tort reform are not sensitive to the specification of that variable. However, these results are not consistent across different measures of medical resource use and are not robust to standard sensitivity checks. In addition, I find almost no evidence of a statistically significant relationship between reform adoption and the treatment of stroke.

In section II, I discuss some of the important institutional details associated with tort reform and malpractice litigation, relevant academic literature, and predictions of the effect of tort reform on medical practice. The empirical strategy is explained in Section III, and the data are discussed in Section IV. Results are presented in Section V. Section VI concludes.

II. Background Information

II.A. Types of tort reforms

States adopted various tort reforms over the past several decades. Kessler and McClellan (1996) identify eight common reforms, of which I focus on the most popular between 1988 and 2008: caps on award size, collateral source rule reform, joint and several liability reform, and mandatory periodic payments. Caps on award size are any tort reform that limits the size of a malpractice award, whether punitive or compensatory. These include limits on non-economic damages, punitive damages, or total damages. Collateral source rule reform (CSR) reduces payments to a plaintiff by the dollar amount of payments from other sources, such as insurance. Reforms to joint and several liability (JSL) rules limit the ability of a plaintiff to seek damages from multiple defendants. Finally, mandatory periodic payments (PPA) require malpractice awards to be paid out in fixed amounts over time.

Direct tort reforms, as defined by Kessler and McClellan (1996), are reforms that directly reduce payment size and claim frequency. These include damage caps and CSR reforms. Indirect tort reforms are reforms which do not directly affect payment size and claim frequency. This category includes PPA and JSL reforms.

II.B. The medical malpractice system and predicted effects of tort reform

The medical malpractice system serves two purposes: provide compensation to patients who suffer losses as the result of negligent medical care and to discourage doctors from behaving carelessly (Congressional Budget Office [CBO], 2006). Medical malpractice cases are judged using the negligence rule, a common law legal standard. The rule holds that physicians are liable for malpractice if the plaintiff demonstrates they suffered an injury, the injury was caused by the physician, and the physician's level of care deviated from what is considered by the court to be due or customary care. Due care is associated with the practice of physicians in good standing; injuries and other adverse medical outcomes that are the result of due care are not considered negligent. It is often not clear exactly what customary practice should be in any given case. This standard must be established in every case, contributing to variance in legal outcomes (Danzon, 2000). Assuming physicians are risk averse, the ambiguity of the negligence rule may create incentives for doctors to behave defensively, since they cannot be sure of exactly what constitutes due care and whether a given level of care will protect them from a lawsuit.

It is not obvious that malpractice litigation should affect physician behavior, however. Physicians are insured from malpractice award and settlement payments, and their insurance plans generally do not contain experience rated premiums or cost-sharing. Instead, premiums vary by physician specialty and geographic location (Danzon, 2000). Consequently, physicians are well insulated from any of the pecuniary costs of a successful tort claim. Physicians do face significant lawsuit associated costs, however. These include mental, time, and reputational costs. To illustrate, Danzon (2000) cites a 1993 survey of physicians that finds physicians with a claims history are significantly more likely to discuss medical risks with patients and order additional procedures. These physicians indicate they incurred significant financial and non-financial costs of being sued, despite having insurance.

Litigation related costs imply that the malpractice system should affect physician behavior. Along with the uncertain nature of the negligence rule, these costs provide physicians with incentive to prescribe an inefficient amount of medical services in order to limit their exposure to litigation. Argues Danzon (2000), "Because liability is all-or-nothing, by incurring a small additional cost [the physician's] probability of a large penalty may be significantly reduced" (p.1348). Tort reform may affect this calculation in several ways. Before a liability reducing reform, a physician's private incentives are such that the marginal benefit of additional treatment is high relative to the costs. The introduction of a tort reform may reduce physician liability, decreasing the benefit of malpractice induced behavior to the physician.[3]

[3] A key assumption throughout the literature is that changes in tort law affect physician perceptions about the risk of malpractice litigation, and therefore affect practice behavior (Danzon, 2000).

Since physicians are fully insured against the size of an award, the primary way a tort reform may reduce physician liability is by lowering claim frequency. Direct reforms, including award caps, limit the maximum size of the malpractice award and reduce a potential plaintiff's incentive to file a malpractice claim. The relationship between indirect reforms and liability is less clear. JSL reform reduces the ability of a plaintiff to hold more than one defendant liable for damages, potentially lowering the expected award size. In states that also enacted award caps, however, the effect of JSL reform on claim frequency may be limited. PPA reform does not directly alter the award size, and will only reduce claim frequency if potential plaintiffs prefer lump sum payments.

A "first-stage" literature, meant to evaluate the link between tort reform and malpractice liability, consistently estimates a negative relationship between direct reforms and measures of malpractice liability. Dependent variables include the size of a malpractice claim payment, the likelihood of a malpractice claim being filed, and premium payments for liability insurance. In a survey of this literature, Mello (2006) finds that the strongest studies show a negative relationship between damage caps and malpractice claim payout size, as well as liability insurance premiums. She finds only mixed evidence for CSR reform, with equal number of studies indicating either no effect or a negative relationship with claim frequency and payout. Additional studies, including Kessler and McClellan (2002) and Avraham (2007), offer evidence that direct reforms reduce claim frequency and payment size.

Indirect reforms, however, are not consistently associated with measures of physician liability. Mello (2006) finds little evidence in the literature that JSL or PPA reforms are related to claim payouts, claim frequency, or liability premiums. However, Avarham (2007) estimates that both JSL and PPA reforms have a negative effect on claim payment size. Kessler and

McClellan (2002) find that indirect reforms, which include JSL and PPA, reduce claim frequency but have a positive effect on other forms of malpractice pressure.

II.C. Relevant literature

Authors of several previous studies use variation in tort reform adoption to test for a relationship between malpractice pressure and physician behavior. Kessler and McClellan (1996, 2002) estimate the effect of state level tort reforms on medical expenditures and outcomes one year after initial diagnosis for a sample of Medicare patients with AMI or ischemic heart disease. Their 1996 paper covers a period between 1984 and 1990, while the later paper extends this time frame to 1994. Kessler and McClellan (1996) argue that defensive medicine is present if tort reforms meant to reduce liability risk lower medical expenditures without harming patient outcomes. The authors generally find that direct tort reforms are associated with a decrease in expenditures, and indirect reforms have a small positive relationship with expenditures. Coefficients for medical outcomes are rarely significant and when significant are very small. Results from the CBO (2006) mirror these results. They estimate the effect of state level tort reform adoption on aggregate measures of Medicare expenditures using two decades of data, starting in 1980. Authors find that individual direct reforms and the direct reform variable decrease expenditures. JSL reforms increase expenditures, though the authors find no evidence of a relationship between the indirect reform variable and expenditures.

Currie and MacLeod (2008) also find an effect between tort reform and medical practice. Employing data from the National Vital Statistics birth dataset for 1989 to 2001, the authors use variation in state tort reform adoption to estimate the effect of malpractice pressure on usage rates of different birth procedures, as well as health outcomes for mother and baby. Using a theoretical model where the effect of JSL reform on physician liability is the opposite of award caps, the authors estimate the effect of each individual reform. They find that damage caps increase the incidence of a Caesarean section by 5%, whereas JSL reform decreases the probability of a Caesarean section 7%. In addition, JSL reforms reduce the likelihood of preventable labor complications by 13%, while caps on noneconomic damages increase these complications by 6%. These results show that reductions in physician liability due to tort reform may exacerbate socially wasteful behavior while worsening patient outcomes.

Finally, one study does not find evidence of a relationship between tort reform and physician behavior, despite using a methodology similar to previous studies. Sloan and Shadle (2009) extend Kessler and McClellan's analysis by examining all expenditures (from inpatient as well as outpatient services) and mortality outcomes one year after any hospital diagnosis, as well as separately for diagnoses of AMI, stroke, breast cancer, and diabetes. Using 1985 to 2000 data from the National Long Term Care Survey matched with Medicare information, they fail to replicate any of the results found by Kessler and McClellan (1996). One explanation for these results is that outpatient expenditures are not sensitive to changes in tort law. Given the results of Kessler and McClellan (1996) on inpatient expenditures for AMI treatment, this suggests that tort reform has a heterogeneous effect across inpatient and outpatient services. However, the authors do not investigate whether tort reform has a differential effect across these categories.

III. Empirical Methodology

To examine the relationship between tort reform and physician behavior, the following empirical specification is used (where *i* denotes hospital admission, *s* state, and *t* year):

$$Y_{ist} = \alpha_s + \delta_t + \lambda_s \cdot t + \gamma Tort_{st} + X_{ist}\beta + \varepsilon_{ist}$$

This specification is derived from natural experiment methods found in Kessler and McClellan (1996) and Currie and MacLeod (2008).[4] Here Y_{ist} is the outcome measure, indicating the natural log of real total charges, length of inpatient stay, or mortality. Total charges and length of stay model resource use per inpatient stay in a hospital, and mortality is an indicator that equals one if the patient died while hospitalized.[5] State and year indicator variables are indicated with α_s and δ_t, respectively. The specification also includes state specific time trends, indicated with $\lambda_s \cdot t$. X_{ist} is a vector of patient controls. This vector includes indicators for whether the patient is an elective admission, female, white, expected primary payer, age and age squared, and an indicator for whether the hospital associated with the inpatient record is a teaching hospital. Finally, standard errors are clustered by state to account for within state correlation.

$Tort_{st}$ is an indicator for the presence of a reform in state s during year t. For tort reforms with an effective date on or after July 1st of year t, the law change is coded as $Tort_{st} = 1$ in $t+1$. This variable is modeled in two ways: as a vector including indicator variables for each tort reform or as a vector including an indicator for direct and indirect reforms. This first specification is similar to the empirical strategy employed by Currie and MacLeod (2008).[6] The second specification is similar to the method first outlined in Kessler and McClellan (1996).

There are two main threats to the validity of this empirical approach. It is possible that tort reform adoption influences the likelihood of admission. If so, this will affect the mix of patients, so that any estimate of the effect of tort reform would reflect both the law change and the change in admission behavior. One way to address this issue is to select a subpopulation of

[4] There are some differences. Kessler and McClellan (1996) include a vector of state legal and political characteristics but do not estimate a time trend, and Currie and MacLeod (2008) use county fixed effects.
[5] Real total charges are measured in 1988 dollars. Length of stay is measured in days.
[6] I combine all damage caps – punitive, non-economic, and total – into one variable in this specification. An alternate specification is run with separate variables for each type of cap. Results are similar, though the result for AMI JSL on stay length is of greater magnitude and significance. These are found in Tables 1.17 and 1.18.

patients with limited discretion over admission. For this analysis, patients with a hospitalization for one of two medical emergencies are selected. These are patients with a primary diagnosis for AMI or stroke.

Unobserved trends correlated with reform adoption also threaten the above empirical strategy. Identification requires that trends in the dependent variable for both adopting states and non-adopting states are similar, in the absence of a treatment. To investigate this, leads and lags of the tort reform variables are estimated in an alternate specification to examine whether there are unobserved trends correlated with reform adoption. Estimated in this specification are one and two year leads, a year of adoption dummy, one and two year lags, and a lag variable for three years or longer after the policy change. Significant leads may indicate a differential trend for adopting states that is correlated with reform adoption, calling into question the causality of results in the main analysis. If, for instance, leads for the effect of direct reforms on charges are negative, then this may indicate that expenditure trends in direct reform adopting states began to decline before the law change. Any estimated effect would be biased, since it includes both the effect of the tort reform and the pre-adoption trend. Lags are included to investigate how quickly physicians respond to tort reform.

IV. Data

I use data from the 1988-2008 Healthcare Cost and Utilization Project (HCUP) Nationwide Inpatient Sample (NIS). The HCUP NIS is an all payer database of hospital inpatient stays from nearly 1,000 U.S. hospitals. This database uses a stratified sampling strategy to approximate a 20% sample of all U.S. community hospitals. Forty-two states are included in the 2008 release of the NIS, while only 8 states are included in the original 1988 release.[7] The NIS data are at the discharge level. The data include information on diagnoses, procedures, charges, outcomes, and other patient and hospital information related to the inpatient admission. Primary diagnosis information is used to create the two patient samples used in the analysis. A patient discharge record is included in the sample by selecting the relevant ICD-9-CM codes for either AMI or stroke.

Table 1.1 includes a summary of means for the key variables used in the analysis, organized by disease group and tort reform type. The first column indicates means for all states. For AMI patients, means for length of stay and mortality are 6.15 and 0.088, respectively. These means tend to be consistent across tort reform status. The mean total charge for an inpatient stay in the first column is $20,803.45. This is consistent across tort reform status as well, with the exception of CSR and PPA reforms which are lower and higher, respectively, than the overall average. These trends are generally true of stroke patients as well. Across disease samples, AMI treatment incurs greater charges but lower stay length than stroke. The incidence of mortality is greater in the stroke sample. Stroke patients are more likely to be female, and are several years older, than patients in the AMI sample.

Information on state tort laws is from data compiled by Ronen Avraham (2010). Included in this analysis are award caps, PPA mandates, reforms to JSL, and CSR reform. I include two other types of tort reforms, contingency fee caps and patient compensation fund requirements, as indirect reforms but not in the individual reform specification.[8] Only two states enacted these reforms, and these states contribute a small amount of observations. Table 1.2

[7] The data is unbalanced in states, potentially confounding estimates of tort reform if states are not randomly missing. To account for this, the regression model used in this analysis includes state fixed effects. Estimates using a 1988-2008 balanced panel, which includes only 8 states, are similar to main results from this paper but have larger standard errors.

[8] In an alternative specification, I estimate the effect of both contingency fee (adopted by Nevada) and patient compensation funds (adopted by West Virginia). Controlling for these laws does not affect the results. These additional estimates are presented in Tables 1.19 and 1.20.

Table 1.1: Means of key variables, specific reforms

Variable	All states	No tort reform	Awards cap	JSL	CSR	PPA
			Acute Myocardial Infarction (Heart Attack)			
Total charges	20,803.450	20,560.580	20,803.610	19,153.640	17,722.820	23,478.880
Length of Stay	6.150	5.959	6.299	6.368	6.650	5.861
Mortality	0.088	0.083	0.092	0.092	0.094	0.079
Age	67.695	67.638	67.786	67.724	68.029	66.685
Female	0.398	0.403	0.397	0.412	0.407	0.397
White	0.580	0.714	0.498	0.388	0.590	0.412
Medicare	0.581	0.581	0.583	0.585	0.597	0.543
Medicaid	0.042	0.052	0.033	0.038	0.034	0.044
Private Insurance	0.300	0.301	0.299	0.306	0.296	0.314
Elective Admission	0.075	0.079	0.073	0.059	0.052	0.086
Teaching Hospital	0.420	0.479	0.381	0.517	0.547	0.433
Observations	2,551,355	991,060	1,500,431	557,094	303,614	450,630
			Stroke			
Total charges	15,821.300	16,376.290	15,440.140	14,317.100	13,610.620	17,518.840
Length of Stay	7.869	7.868	7.925	8.043	8.321	7.419
Mortality	0.116	0.120	0.115	0.114	0.119	0.107
Age	71.014	70.941	71.113	71.171	71.988	70.068
Female	0.532	0.540	0.527	0.535	0.531	0.537
White	0.526	0.660	0.444	0.328	0.533	0.363
Medicare	0.680	0.678	0.683	0.682	0.696	0.635
Medicaid	0.053	0.068	0.042	0.047	0.035	0.051
Private Insurance	0.200	0.200	0.199	0.198	0.182	0.210
Elective Admission	0.085	0.079	0.089	0.078	0.083	0.083
Teaching Hospital	0.414	0.474	0.373	0.498	0.525	0.442
Observations	1,791,518	682,579	1,072,249	397,541	200,662	305,773

indicates year of both tort reform adoption and reforms turning off in relevant states. Several of the adopting states in the sample enacted multiple reforms at the same time. For instance, Wisconsin adopted damage caps, CSR, and JSL reform in the same year. This complicates the analysis, since it is harder to identify the effects of any one tort reform if states adopt several reforms simultaneously.

Table 1.2: Changes in state tort laws

Year	Any cap	JSL	CSR	PPA
1988				
1989	CO, WA*			CO
1990	WI*			
1991				
1992				
1993				
1994				AZ*
1995	IL, WI	IL, WI	WI	
1996	NJ			
1997	PA, IL*	IL*		
1998				
1999	OR*			
2000				
2001				
2002		PA	PA	PA
2003	NV, OH	OH	WV	OH
2004	FL, TX			TX
2005	GA			GA
2006	IL, SC, MO	SC		
2007				
2008				

Law change only included if state was in HCUP that year
**Indicates that law turned off, usually due to court ruling*

V. Results

Table 1.3 presents main results for the AMI sample. Estimates from this sample are consistent across specifications. Columns 1 and 2 indicate results on total hospital charges for individual and grouped reform categories, respectively. Award caps and CSR reform, as well as the direct reform category, do not have a statistically significant effect on hospital charges. In addition, results from columns 5 and 6 show no evidence that these reforms influence patient mortality. Indirect reforms, including JSL and PPA reform, appear to influence hospital charges. Coefficients for these reforms are positive and statistically significant for both PPA reform and the overall indirect reform category. These point estimates imply that PPA reform increases

Table 1.3: AMI results

Variable	Natural log charges		LOS		Mortality	
	1	*2*	*3*	*4*	*5*	*6*
Award cap	-0.013		-0.262**		0.000	
	(0.025)		(0.098)		(0.001)	
CSR reform	0.020		-0.153		-0.002	
	(0.040)		(0.145)		(0.002)	
Direct reforms		-0.021		-0.251**		0.001
		(0.024)		(0.105)		(0.001)
JSL reform	0.026		0.269*		-0.002	
	(0.034)		(0.151)		(0.002)	
Periodic payment	0.047**		0.128		-0.001	
	(0.022)		(0.107)		(0.002)	
Indirect reforms		0.052**		0.230**		-0.002
		(0.023)		(0.113)		(0.002)
Observations	2,551,355					

Cluster robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Not indicated are the following control variables: elective admission, female, white, Medicare, Medicaid, private insurance, age, age squared, teaching hospital, state and year dummies, state linear time trends

charges 4.8% and the indirect category raises total charges 5.3%.[9] As with direct reforms, neither PPA reform nor the indirect category has any effect on patient mortality. Overall, AMI results for direct reforms diverge from those of Kessler and McClellan (1996), who find a negative relationship between the direct reform category and expenditures. Indirect reform results for AMI are consistent with Kessler and McClellan (1996), however, implying that indirect reforms have a positive relationship with resource use but no relationship with outcomes.

Columns 3 and 4 of Table 1.3 present results on hospital stay length, which is meant as an additional measure of resource use. As with total hospital charges in columns 1 and 2, the length of stay results are consistent across both tort variable specifications. However, the types

[9] I exponentiate the point estimates here. For instance, PPA reform: 100*[e^(0.047)-1] = 4.81.

Table 1.4: Stroke results

Variable	Natural log charges		LOS		Mortality	
	1	*2*	*3*	*4*	*5*	*6*
Award cap	0.014		-0.518*		-0.005	
	(0.042)		(0.264)		(0.003)	
CSR reform	0.134**		0.092		0.008	
	(0.057)		(0.531)		(0.013)	
Direct reforms		0.002		-0.376		-0.004**
		(0.044)		(0.261)		(0.002)
JSL reform	-0.030		0.472		0.008	
	(0.052)		(0.444)		(0.005)	
Periodic payment	-0.025		-0.660		-0.014*	
	(0.045)		(0.395)		(0.007)	
Indirect reforms		0.012		-0.035		0.003
		(0.052)		(0.334)		(0.005)
Observations			1,791,518			

Cluster robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Not indicated are the following control variables: elective admission, female, white, Medicare, Medicaid, private insurance, age, age squared, teaching hospital, state and year dummies, state linear time trends

of reforms estimated to influence stay length are different than for total charges. Award caps have a negative effect on stay length with a coefficient of -0.262, or a 4.3% reduction relative to mean stay length. Direct reforms have a similar relationship with stay length, with the coefficient implying a 4.1% reduction relative to the mean. JSL reform, which does not have a significant effect on charges in column 1, has a positive effect on stay length in column 3. The opposite is true of PPA reform, which is not associated with length of stay. The only variable that is consistent across either measure of resource use is the overall indirect reform category in column 4, estimated to increase stay length 3.7%. These results suggest that tort reform estimates are sensitive to the choice of medical resource use variable.

Results are also sensitive to the use of state specific time trends. Table 1.5 presents AMI estimates without these trends. None of the tort reform variables have a statistically significant effect on either measure of resource use. The only significant relationship between tort reform

Table 1.5: AMI results, no state time trends

Variable	Natural log charges		LOS		Mortality	
	1	*2*	*3*	*4*	*5*	*6*
Award cap	0.099		-0.154		-0.003*	
	(0.081)		(0.272)		(0.002)	
CSR reform	-0.038		-0.154		0.001	
	(0.049)		(0.195)		(0.002)	
Direct reforms		0.106		-0.145		-0.003*
		(0.081)		(0.274)		(0.002)
JSL reform	-0.040		0.150		0.001	
	(0.064)		(0.257)		(0.002)	
Periodic payment	-0.024		0.078		0.000	
	(0.053)		(0.176)		(0.001)	
Indirect reforms		-0.068		0.086		0.002
		(0.061)		(0.232)		(0.002)
Observations			2,551,355			

Cluster robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Not indicated are the following control variables: elective admission, female, white, Medicare, Medicaid, private insurance, age, age squared, teaching hospital, state and year dummies, state linear time trends

and the dependent variables is with patient mortality. Award caps and the overall direct reform category reduce the likelihood of mortality 0.3% points. The results from specifications with and without state specific time trends imply that states have differential underlying trends in the dependent variables. Finally, lead and lag results in Tables 1.7 and 1.8 present additional evidence that the main results from Table 1.3 are sensitive to unobservable trends. Both tables show that direct and indirect reform categories, as well as award caps, have significant and positive 1 year leads for total charges. This implies that hospital charges in reform adopting states were increasing relative to the state specific trend prior to the law change.

Results for the stroke sample show almost no relationship between tort reform and medical resource, regardless of whether state time trends are included in the model. Table 1.4 and 1.6 present estimates for the stroke sample. The only significant effects on resource use are

Table 1.6: Stroke results, no state time trends

Variable	Natural log charges		LOS		Mortality	
	1	*2*	*3*	*4*	*5*	*6*
Award cap	0.114		-0.329		-0.007*	
	(0.095)		(0.743)		(0.004)	
CSR reform	0.072		0.694		0.015***	
	(0.068)		(0.570)		(0.004)	
Direct reforms		0.111		-0.372		-0.009**
		(0.095)		(0.763)		(0.003)
JSL reform	-0.093		-0.096		0.004	
	(0.075)		(0.687)		(0.004)	
Periodic payment	-0.094		-0.548		-0.004	
	(0.067)		(0.576)		(0.004)	
Indirect reforms		-0.087		-0.079		0.008*
		(0.076)		(0.687)		(0.004)
Observations			1,791,518			

Cluster robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Not indicated are the following control variables: elective admission, female, white, Medicare, Medicaid, private insurance, age, age squared, teaching hospital, state and year dummies, state linear time trends

associated with individual reforms. In Table 1.4, CSR reform and award caps affect hospital charges and stay length, respectively. CSR reform is associated with a large positive expenditure increase of 14.3%. For length of stay, the coefficient on award caps is -0.518, or a 6.6% reduction in hospital stay. Despite no evidence of a relationship with either measure of resource use, the direct reform category and PPA reform lower the likelihood of mortality during the inpatient stay. Tort reform is also associated with hospital mortality in specifications without state time trends. In Table 1.6, both the direct and indirect reform categories, as well as CSR and award caps, have a statistically significant relationship with mortality. This is despite no evidence of a relationship between resource use and any of the reform variables in Table 1.6. Estimates from an additional specification, found in Table 1.12, show that results are similar even when different definitions of the stroke sample are considered.[10]

Table 1.7: AMI lag and lead results, direct and indirect reform specification

Variables	Natural log charges	LOS	Mortality
Direct 2 year lead	-0.016	-0.068	-0.002
	(0.029)	(0.144)	(0.002)
Direct 1 year lead	0.075***	0.121	0.002
	(0.025)	(0.140)	(0.002)
Direct year of adoption	0.037*	-0.096	0.002
	(0.022)	(0.144)	(0.002)
Direct 1 year lag	-0.042	-0.271	-0.001
	(0.035)	(0.170)	(0.001)
Direct 2 year lag	0.026	-0.247	-0.002
	(0.043)	(0.195)	(0.002)
Direct 3 year plus lag	0.014	-0.394*	-0.001
	(0.048)	(0.211)	(0.002)
Indirect 2 year lead	0.052	0.243	-0.001
	(0.034)	(0.148)	(0.002)
Indirect 1 year lead	0.045*	0.014	-0.002
	(0.023)	(0.111)	(0.002)
Indirect year of adoption	0.071***	0.260**	-0.003**
	(0.025)	(0.113)	(0.001)
Indirect 1 year lag	0.140***	0.288*	-0.002
	(0.046)	(0.168)	(0.002)
Indirect 2 year lag	0.065	0.131	-0.003
	(0.046)	(0.180)	(0.004)
Indirect 3 year plus lag	0.125**	0.209	-0.002
	(0.052)	(0.212)	(0.003)
Observations		2,551,355	

Cluster robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Not indicated are the following control variables: elective admission, female, white, Medicare, Medicaid, private insurance, age, age squared, teaching hospital, state and year dummies, state linear time trends

[10] Specifically, two additional samples are created. These include a "wide" sample, which consists of those patients indicated as having some type of cerebrovascular disease. A "narrow" sample consists of only those patients reported to have a cerebral hemorrhage or infarction. The sample used in the main analysis is an intermediate form of these definitions, including all cerebral hemorrhages and infarctions as well as diagnoses where it is not indicated if the patient suffered an infarction.

Table 1.8: AMI lags and leads results, individual reform specification

Variables	Natural log charges	LOS	Mortality
Award cap 2 year lead	0.000	-0.065	-0.002
	(0.027)	(0.132)	(0.001)
Award cap 1 year lead	0.064***	0.026	0.000
	(0.023)	(0.137)	(0.001)
Award cap year adoption	0.043*	-0.149	0.001
	(0.022)	(0.146)	(0.002)
Award cap 1 year lag	-0.029	-0.305*	0.000
	(0.029)	(0.164)	(0.002)
Award cap 2 year lag	0.032	-0.356*	-0.002
	(0.044)	(0.190)	(0.002)
Award cap 3 year plus lag	0.023	-0.449**	-0.001
	(0.046)	(0.215)	(0.002)
CSR 2 year lead	-0.015	0.449**	0.001
	(0.039)	(0.217)	(0.002)
CSR 1 year lead	0.011	0.564**	0.005
	(0.069)	(0.229)	(0.003)
CSR year of adoption	-0.034	0.424**	0.004
	(0.047)	(0.201)	(0.002)
CSR 1 year lag	-0.028	0.199	0.000
	(0.055)	(0.344)	(0.003)
CSR 2 year lag	-0.025	0.748*	-0.002
	(0.104)	(0.377)	(0.004)
CSR 3 year plus lag	-0.040	0.365	-0.006*
	(0.065)	(0.427)	(0.004)
JSL 2 year lead	0.024	0.004	-0.004*
	(0.037)	(0.195)	(0.002)
JSL 1 year lead	0.015	-0.187	-0.004
	(0.041)	(0.189)	(0.003)
JSL year of adoption	0.055	0.122	-0.005**
	(0.043)	(0.168)	(0.002)
JSL 1 year lag	0.093**	0.070	-0.004**
	(0.039)	(0.178)	(0.002)
JSL 2 year lag	0.043	-0.170	0.002
	(0.062)	(0.293)	(0.002)
JSL 3 year plus lag	0.088*	-0.036	0.002
	(0.046)	(0.250)	(0.002)
PPA 2 year lead	0.042	-0.063	0.001
	(0.025)	(0.111)	(0.002)
PPA 1 year lead	0.065	-0.245	-0.003
	(0.044)	(0.149)	(0.003)

Table 1.8 (cont'd)

PPA year of adoption	0.086*	-0.116	-0.004
	(0.049)	(0.194)	(0.003)
PPA 1 year lag	0.167***	-0.045	-0.005
	(0.057)	(0.278)	(0.004)
PPA 2 year lag	0.092	-0.156	-0.012**
	(0.068)	(0.312)	(0.005)
PPA 3 year plus lag	0.184**	-0.109	-0.006
	(0.076)	(0.363)	(0.005)
Observations		2,551,355	

Cluster robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Not indicated are the following control variables: elective admission, female, white, Medicare, Medicaid, private insurance, age, age squared, teaching hospital, state and year dummies, state linear time trends

VI. Conclusion

This paper presents inconclusive results regarding the relationship between physician behavior and state tort reform adoption. For the treatment of AMI, aggregating individual law changes into direct and indirect categories of tort reform does not seem to be an important part of the story behind mixed evidence in the literature. Results on stay length, as well as the relationship between indirect reforms and hospital charges, are generally consistent with the empirical results of Kessler and McClellan (1996). They are also consistent with theoretical predictions of Currie and MacLeod (2008), who argue that the effect of direct reforms and JSL reform on physician behavior should be oppositely signed. However, the AMI results are sensitive to the choice of variable meant to measure medical resource use and the use of state time trends. Results from an event history analysis indicate significant leads in hospital charges prior to tort reform adoption, suggesting that results from the main analysis in Table 1.3 do not represent the true causal effect of the law change.

There is little evidence of a behavioral response in the stroke sample. The few significant associations between tort reform and resource use in Table 1.4 – the relationship between CSR reform and charges, as well as award caps and stay length – are sensitive to the use of state specific time trends. Because there is little evidence of a relationship between physician behavior and tort reform in stroke treatment, and the main results in the AMI sample are sensitive to several robustness checks, it is difficult to make any conclusions regarding the role of patient population in explaining the lack of consensus in the tort reform literature.

Finally, results from this paper do little to identify the presence of defensive medicine in medical practice. Authors from the tort reform literature analyze a more comprehensive set of patient outcome variables than presented in this paper. Notably, Kessler and McClellan (1996) examine both mortality and health status one year after initial diagnosis, and Currie and MacLeod (2008) explore the effect of tort reform on birth complications and APGAR scores. Information on outcomes beyond mortality is limited in the HCUP NIS, and observation of patient outcomes ends upon hospital discharge. Without better information on patient health status after treatment, it is impossible to obtain a good picture of the benefits, if any, of liability induced care. If incentives are appropriate in the current malpractice system, then reductions in liability may actually harm patient outcomes if physicians no longer take sufficient measures to avoid negligent injury. Future investigations of the effect of tort reform on physician behavior must take into account both benefits and costs generated by the malpractice system. Given mixed results on outcomes from the literature, whether tort reforms induce efficient or inefficient behavior is a critical point of clarification in the evaluation of these laws.

APPENDIX

Table 1.9: Stroke lags and leads results, direct and indirect reform specification

Variables	Natural log charges	LOS	Mortality
Direct 2 year lead	0.010	0.155	0.004
	(0.038)	(0.170)	(0.003)
Direct 1 year lead	0.062*	0.076	0.012***
	(0.037)	(0.278)	(0.004)
Direct year of adoption	0.033	-0.082	0.007
	(0.048)	(0.332)	(0.004)
Direct 1 year lag	-0.001	-0.319	0.000
	(0.065)	(0.379)	(0.006)
Direct 2 year lag	0.057	-0.377	-0.001
	(0.068)	(0.396)	(0.006)
Direct 3 year plus lag	-0.016	-0.711	0.000
	(0.076)	(0.576)	(0.007)
Indirect 2 year lead	0.018	0.269	0.001
	(0.038)	(0.227)	(0.004)
Indirect 1 year lead	0.099***	0.086	0.001*
	(0.034)	(0.281)	(0.005)
Indirect year of adoption	0.082**	0.257	0.005
	(0.037)	(0.317)	(0.006)
Indirect 1 year lag	0.128**	0.121	0.009*
	(0.061)	(0.424)	(0.004)
Indirect 2 year lag	0.072	0.135	0.014*
	(0.055)	(0.427)	(0.007)
Indirect 3 year plus lag	0.147**	0.355	0.014
	(0.072)	(0.627)	(0.009)
Observations		1,791,518	

Cluster robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Not indicated are the following control variables: elective admission, female, white, Medicare, Medicaid, private insurance, age, age squared, teaching hospital, state and year dummies, state linear time trends

Table 1.10: Stroke lags and leads results, individual reform specification

Variables	Natural log charges	LOS	Mortality
Award cap 2 year lead	0.017	0.044	0.004
	(0.040)	(0.164)	(0.003)
Award cap 1 year lead	0.069*	-0.102	0.013***
	(0.040)	(0.252)	(0.004)
Award cap year adoption	0.040	-0.298	0.006
	(0.052)	(0.267)	(0.004)
Award cap 1 year lag	0.005	-0.601*	0.000
	(0.062)	(0.337)	(0.005)
Award cap 2 year lag	0.047	-0.668*	-0.002
	(0.074)	(0.361)	(0.005)
Award cap 3 year plus lag	-0.020	-0.907*	-0.001
	(0.077)	(0.515)	(0.006)
CSR 2 year lead	0.008	0.657***	0.002
	(0.045)	(0.211)	(0.004)
CSR 1 year lead	0.045	1.364***	-0.001
	(0.066)	(0.343)	(0.009)
CSR year of adoption	0.098	1.446***	0.000
	(0.060)	(0.304)	(0.011)
CSR 1 year lag	0.167**	1.466***	0.015
	(0.082)	(0.517)	(0.010)
CSR 2 year lag	0.117	1.411**	0.022*
	(0.095)	(0.568)	(0.011)
CSR 3 year plus lag	0.305***	1.854**	0.022
	(0.105)	(0.906)	(0.015)
JSL 2 year lead	0.031	0.060	0.001
	(0.041)	(0.306)	(0.005)
JSL 1 year lead	0.080**	-0.317	0.009
	(0.034)	(0.349)	(0.005)
JSL year of adoption	0.018	-0.027	0.004
	(0.028)	(0.214)	(0.005)
JSL 1 year lag	0.028	-0.330	0.006**
	(0.033)	(0.350)	(0.003)
JSL 2 year lag	0.003	-0.212	0.015***
	(0.053)	(0.214)	(0.004)
JSL 3 year plus lag	0.019	-0.016	0.007**
	(0.038)	(0.233)	(0.003)
PPA 2 year lead	-0.051	-0.345*	-0.006
	(0.043)	(0.196)	(0.008)
PPA 1 year lead	0.015	-0.969***	0.000
	(0.059)	(0.303)	(0.011)

Table 1.10 (cont'd)

PPA year of adoption	-0.021	-1.252***	-0.007
	(0.073)	(0.442)	(0.011)
PPA 1 year lag	0.034	-1.372**	-0.012
	(0.088)	(0.656)	(0.011)
PPA 2 year lag	-0.031	-1.911***	-0.018
	(0.098)	(0.659)	(0.013)
PPA 3 year plus lag	-0.041	-2.563**	-0.018
	(0.139)	(1.033)	(0.018)
Observations		1,791,518	

Cluster robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Not indicated are the following control variables: elective admission, female, white, Medicare, Medicaid, private insurance, age, age squared, teaching hospital, state and year dummies, state linear time trends

Table 1.12: Stroke comparison results

Variables	*Narrow* Nat. log charges	*Main results* Nat. log charges	*Wide* Nat. log charges	*Narrow* LOS	*Main results* LOS	*Wide* LOS	*Narrow* Mortality	*Main results* Mortality	*Wide* Mortality
				Individual reform specification					
Award cap	0.024	0.014	0.005	-0.295	-0.518*	-0.562**	-0.004	-0.005	-0.005**
	(0.045)	(0.042)	(0.034)	(0.214)	(0.264)	(0.277)	(0.003)	(0.003)	(0.002)
CSR reform	0.142**	0.134**	0.140***	0.271	0.092	0.067	0.010	0.008	0.000
	(0.053)	(0.057)	(0.049)	(0.496)	(0.531)	(0.509)	(0.013)	(0.013)	(0.010)
JSL reform	-0.050	-0.030	-0.024	0.297	0.472	0.586	0.005	0.008	0.008*
	(0.052)	(0.052)	(0.038)	(0.380)	(0.444)	(0.423)	(0.006)	(0.005)	(0.004)
PPA	-0.027	-0.025	-0.028	-0.619*	-0.660	-0.637	-0.016**	-0.014*	-0.012*
	(0.040)	(0.045)	(0.048)	(0.339)	(0.395)	(0.436)	(0.007)	(0.007)	(0.006)
				Direct and indirect reform specification					
Direct ref.	0.008	0.002	-0.008	-0.188	-0.376	-0.416	-0.004*	-0.004**	-0.004**
	(0.049)	(0.044)	(0.038)	(0.206)	(0.261)	(0.282)	(0.002)	(0.002)	(0.002)
Indirect ref.	-0.003	0.012	0.019	-0.092	-0.035	0.065	0.001	0.003	0.001
	(0.054)	(0.052)	(0.049)	(0.289)	(0.334)	(0.352)	(0.006)	(0.005)	(0.004)
Observations	1,450,969	1,791,518	2,247,711	1,450,969	1,791,518	2,247,711	1,450,969	1,791,518	2,247,711

Cluster robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Not indicated are the following control variables: elective admission, female, white, Medicare, Medicaid, private insurance, age, age squared, teaching hospital, state and year dummies, state linear time trends

Table 1.13: AMI results, direct/indirect specification

Variable	Natural log charges	Natural log charges	LOS	LOS	Mortality	Mortality
Direct ref.	0.106	-0.021	-0.145	-0.251**	-0.003*	0.001
	(0.081)	(0.024)	(0.274)	(0.105)	(0.002)	(0.001)
Indirect ref.	-0.068	0.052**	0.086	0.230**	0.002	-0.002
	(0.061)	(0.023)	(0.232)	(0.113)	(0.002)	(0.002)
Female	-0.043***	-0.043***	0.260***	0.257***	0.007***	0.007***
	(0.004)	(0.004)	(0.020)	(0.020)	(0.001)	(0.001)
White	-0.047**	-0.037**	-0.209**	-0.327***	-0.004***	-0.004***
	(0.023)	(0.015)	(0.081)	(0.062)	(0.001)	(0.001)
Age	0.050***	0.050***	0.141***	0.141***	-0.006***	-0.006***
	(0.002)	(0.002)	(0.008)	(0.008)	(0.000)	(0.000)
Age squared	-0.000***	-0.000***	-0.001***	-0.001***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Medicare	0.057***	0.064***	0.581***	0.594***	0.000	0.000
	(0.014)	(0.013)	(0.089)	(0.086)	(0.001)	(0.001)
Medicaid	0.035***	0.042***	1.064***	1.054***	0.011***	0.011***
	(0.013)	(0.010)	(0.102)	(0.090)	(0.002)	(0.002)
Pri. insurance	0.049***	0.053***	-0.222***	-0.206***	-0.013***	-0.014***
	(0.013)	(0.012)	(0.052)	(0.050)	(0.001)	(0.001)
Elective	0.040**	0.035**	-0.041	-0.057	-0.019***	-0.019***
	(0.015)	(0.014)	(0.099)	(0.091)	(0.002)	(0.002)
Teaching hos.	0.473***	0.479***	1.537***	1.533***	0.003***	0.003**
	(0.042)	(0.041)	(0.122)	(0.121)	(0.001)	(0.001)
State, year FE	Yes	Yes	Yes	Yes	Yes	Yes
State time trends	No	Yes	No	Yes	No	Yes
Observations			2,551,355			

Cluster robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Not indicated are the following control variables: elective admission, female, white, Medicare, Medicaid, private insurance, age, age squared, teaching hospital, state and year dummies, state linear time trends

Table 1.14: AMI results, individual reform specification

Variable	Natural log charges	Natural log charges	LOS	LOS	Mortality	Mortality
Award cap	0.099	-0.013	-0.154	-0.262**	-0.003*	0.000
	(0.081)	(0.025)	(0.272)	(0.098)	(0.002)	(0.001)
CSR reform	-0.038	0.020	-0.154	-0.153	0.001	-0.002
	(0.049)	(0.040)	(0.195)	(0.145)	(0.002)	(0.002)
JSL reform	-0.040	0.026	0.150	0.269*	0.001	-0.002
	(0.064)	(0.034)	(0.257)	(0.151)	(0.002)	(0.002)
PPA	-0.024	0.047**	0.078	0.128	0.000	-0.001
	(0.053)	(0.022)	(0.176)	(0.107)	(0.001)	(0.002)
Female	-0.043***	-0.043***	0.260***	0.257***	0.007***	0.007***
	(0.004)	(0.004)	(0.020)	(0.020)	(0.001)	(0.001)
White	-0.046*	-0.038**	-0.207**	-0.330***	-0.004***	-0.004***
	(0.023)	(0.015)	(0.079)	(0.061)	(0.001)	(0.001)
Age	0.050***	0.050***	0.141***	0.141***	-0.006***	-0.006***
	(0.002)	(0.002)	(0.008)	(0.008)	(0.000)	(0.000)
Age squared	-0.000***	-0.000***	-0.001***	-0.001***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Medicare	0.057***	0.064***	0.581***	0.593***	0.000	0.000
	(0.014)	(0.013)	(0.089)	(0.086)	(0.001)	(0.001)
Medicaid	0.036***	0.043***	1.064***	1.053***	0.011***	0.011***
	(0.013)	(0.010)	(0.102)	(0.090)	(0.002)	(0.002)
Pri. insurance	0.049***	0.054***	-0.222***	-0.207***	-0.013***	-0.014***
	(0.013)	(0.012)	(0.052)	(0.050)	(0.001)	(0.001)
Elective	0.040**	0.035**	-0.0411	-0.057	-0.019***	-0.019***
	(0.015)	(0.014)	(0.099)	(0.091)	(0.002)	(0.002)
Teaching hos.	0.473***	0.479***	1.537***	1.533***	0.003***	0.003**
	(0.042)	(0.041)	(0.122)	(0.121)	(0.001)	(0.001)
State, year FE	Yes	Yes	Yes	Yes	Yes	Yes
State time trends	No	Yes	No	Yes	No	Yes
Observations			2,551,355			

Cluster robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Not indicated are the following control variables: elective admission, female, white, Medicare, Medicaid, private insurance, age, age squared, teaching hospital, state and year dummies, state linear time trends

Table 1.15: Stroke results, direct/indirect specification

Variable	Natural log charges	Natural log charges	LOS	LOS	Mortality	Mortality
Direct ref.	0.111	0.002	-0.372	-0.376	-0.009**	-0.004**
	(0.095)	(0.044)	(0.763)	(0.261)	(0.003)	(0.002)
Indirect ref.	-0.087	0.012	-0.079	-0.035	0.008*	0.003
	(0.076)	(0.052)	(0.687)	(0.334)	(0.004)	(0.005)
Female	0.000	0.001	0.211***	0.199***	0.002***	0.002***
	(0.003)	(0.003)	(0.049)	(0.046)	(0.001)	(0.001)
White	-0.109***	-0.098***	-0.715***	-0.985***	0.000	0.000
	(0.025)	(0.017)	(0.183)	(0.183)	(0.002)	(0.002)
Age	-0.004***	-0.004***	-0.173	-0.174	-0.004***	-0.004***
	(0.001)	(0.001)	(0.122)	(0.122)	(0.000)	(0.000)
Age squared	-0.000***	-0.000***	0.001	0.001	0.000***	0.000***
	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)
Medicare	0.066***	0.074***	-0.124	-0.067	-0.046***	-0.047***
	(0.015)	(0.013)	(0.470)	(0.458)	(0.006)	(0.006)
Medicaid	0.175***	0.183***	2.498***	2.456***	-0.025***	-0.026***
	(0.016)	(0.012)	(0.312)	(0.291)	(0.005)	(0.005)
Pri. insurance	0.043**	0.049***	-0.840***	-0.774**	-0.039***	-0.040***
	(0.017)	(0.017)	(0.296)	(0.290)	(0.005)	(0.004)
Elective	-0.151***	-0.150***	-1.437***	-1.399**	-0.057***	-0.056***
	(0.021)	(0.021)	(0.522)	(0.532)	(0.006)	(0.006)
Teaching hos.	0.311***	0.314***	1.417***	1.380***	0.019***	0.019***
	(0.033)	(0.033)	(0.105)	(0.103)	(0.003)	(0.003)
State, year FE	Yes	Yes	Yes	Yes	Yes	Yes
State time trends	No	Yes	No	Yes	No	Yes
Observations			1,791,518			

Cluster robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Not indicated are the following control variables: elective admission, female, white, Medicare, Medicaid, private insurance, age, age squared, teaching hospital, state and year dummies, state linear time trends

Table 1.16: Stroke results, individual reform specification

Variable	Natural log charges	Natural log charges	LOS	LOS	Mortality	Mortality
Award cap	0.114	0.014	-0.329	-0.518*	-0.007*	-0.005
	(0.095)	(0.042)	(0.743)	(0.264)	(0.004)	(0.003)
CSR reform	0.072	0.134**	0.694	0.092	0.015***	0.008
	(0.068)	(0.057)	(0.570)	(0.531)	(0.004)	(0.013)
JSL reform	-0.093	-0.030	-0.096	0.472	0.004	0.008
	(0.075)	(0.052)	(0.687)	(0.444)	(0.004)	(0.005)
PPA	-0.094	-0.025	-0.548	-0.660	-0.004	-0.014*
	(0.067)	(0.045)	(0.576)	(0.395)	(0.004)	(0.007)
Female	0.000	0.001	0.210***	0.199***	0.002***	0.002***
	(0.003)	(0.003)	(0.049)	(0.046)	(0.001)	(0.001)
White	-0.110***	-0.098***	-0.724***	-0.988***	0.000	0.000
	(0.024)	(0.017)	(0.183)	(0.182)	(0.002)	(0.002)
Age	-0.004***	-0.004***	-0.173	-0.174	-0.004***	-0.004***
	(0.001)	(0.001)	(0.122)	(0.122)	(0.000)	(0.000)
Age squared	-0.000***	-0.000***	0.001	0.001	0.000***	0.000***
	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)
Medicare	0.067***	0.075***	-0.125	-0.072	-0.046***	-0.047***
	(0.015)	(0.013)	(0.467)	(0.460)	(0.006)	(0.006)
Medicaid	0.175***	0.185***	2.497***	2.451***	-0.025***	-0.026***
	(0.016)	(0.012)	(0.310)	(0.287)	(0.005)	(0.005)
Pri. insurance	0.043**	0.050***	-0.840***	-0.779**	-0.039***	-0.040***
	(0.017)	(0.017)	(0.296)	(0.291)	(0.005)	(0.004)
Elective	-0.151***	-0.150***	-1.434***	-1.399**	-0.057***	-0.056***
	(0.021)	(0.021)	(0.521)	(0.532)	(0.006)	(0.006)
Teaching hos.	0.311***	0.314***	1.417***	1.378***	0.019***	0.019***
	(0.033)	(0.033)	(0.105)	(0.103)	(0.003)	(0.003)
State, year FE	Yes	Yes	Yes	Yes	Yes	Yes
State time trends	No	Yes	No	Yes	No	Yes
Observations			1,791,518			

Cluster robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Not indicated are the following control variables: elective admission, female, white, Medicare, Medicaid, private insurance, age, age squared, teaching hospital, state and year dummies, state linear time trends

Table 1.17: AMI results, all cap definitions

Variable	Natural log charges	LOS	Mortality
	Individual reform specification		
NE damage cap	-0.027	-0.319***	-0.001
	(0.028)	(0.111)	(0.001)
Punitive cap	0.020	-0.101	0.002
	(0.020)	(0.143)	(0.002)
Total damage cap	-0.182***	-0.725***	0.000
	(0.035)	(0.183)	(0.002)
CSR reform	0.010	-0.186	-0.002
	(0.036)	(0.145)	(0.002)
JSL reform	0.052	0.371**	-0.001
	(0.031)	(0.158)	(0.002)
Periodic payment	0.046**	0.121	-0.001
	(0.021)	(0.115)	(0.002)
Observations		2,551,355	

Cluster robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Not indicated are the following control variables: elective admission, female, white, Medicare, Medicaid, private insurance, age, age squared, teaching hospital, state and year dummies, state linear time trends

Table 1.18: Stroke results, all cap definitions

Variable	Natural log charges	LOS	Mortality
	Individual reform specification		
NE damage cap	0.007	-0.535	-0.001
	(0.041)	(0.344)	(0.004)
Punitive cap	0.036	-0.380	-0.012*
	(0.061)	(0.361)	(0.007)
Total damage cap	-0.162***	-0.489	0.004
	(0.049)	(0.501)	(0.004)
CSR reform	0.124**	0.078	0.008
	(0.055)	(0.539)	(0.012)
JSL reform	-0.008	0.529	0.004
	(0.053)	(0.532)	(0.006)
Periodic payment	-0.027	-0.670*	-0.015**
	(0.046)	(0.393)	(0.007)
Observations		1,791,518	

Cluster robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Not indicated are the following control variables: elective admission, female, white, Medicare, Medicaid, private insurance, age, age squared, teaching hospital, state and year dummies, state linear time trends

Table 1.19: AMI, patient compensation fund and contingency fee

Variable	Natural log total charges	LOS	Mortality
Award cap	-0.013	-0.262**	0.001
	(0.025)	(0.098)	(0.001)
CSR reform	0.022	-0.155	-0.001
	(0.040)	(0.145)	(0.002)
JSL reform	0.026	0.270*	-0.002
	(0.034)	(0.151)	(0.002)
Periodic payment	0.046**	0.129	-0.002
	(0.022)	(0.107)	(0.002)
Patient comp	-0.104***	0.057	-0.012***
	(0.026)	(0.082)	(0.001)
Contingency fee	-0.160***	0.014	0.015***
	(0.030)	(0.089)	(0.002)
Observations		2,551,355	

Cluster robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Not indicated are the following control variables: elective admission, female, white, Medicare, Medicaid, private insurance, age, age squared, teaching hospital, state and year dummies, state linear time trends

Table 1.20: Stroke, patient compensation fund and contingency fee

Variable	Natural log total charges	LOS	Mortality
Award cap	0.013	-0.520*	-0.005
	(0.042)	(0.264)	(0.003)
CSR reform	0.137**	0.117	0.009
	(0.056)	(0.528)	(0.012)
JSL reform	-0.030	0.468	0.007
	(0.052)	(0.443)	(0.005)
Periodic payment	-0.026	-0.669*	-0.015**
	(0.045)	(0.395)	(0.007)
Patient comp	-0.116***	-1.042***	-0.041***
	(0.032)	(0.218)	(0.006)
Contingency fee	-0.286***	-1.074***	-0.036***
	(0.037)	(0.163)	(0.004)
Observations		1,791,518	

Cluster robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Not indicated are the following control variables: elective admission, female, white, Medicare, Medicaid, private insurance, age, age squared, teaching hospital, state and year dummies, state linear time trends

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CHAPTER 2
THE STATE CHILDREN'S HEALTH INSURANCE PROGRAM AND MATERNAL LABOR SUPPLY INCENTIVES

I. Introduction

In recent decades, there have been numerous reforms to the public health insurance system in the United States. One of the largest is the State Children's Health Insurance Program (SCHIP), adopted in 1997. This program provides health insurance to children less than 19 years of age in families with incomes above existing Medicaid eligibility thresholds. SCHIP quickly became an important source of health care access for near poor children. Between 1999 and 2006 total enrollment grew from two million children to well over six and a half million (Congressional Budget Office [CBO], 2007).

Despite the benefits of health insurance, means-tested programs like SCHIP and Medicaid are often criticized for distorting labor supply behavior. Recent research focuses on the separation of Medicaid and AFDC eligibility rules during the late 1980s and early 1990s, and Medicaid's initial implementation period (Ham and Shore-Sheppard, 2005a; Strumpf, 2011). These authors find no evidence of a Medicaid labor supply effect among mothers.[11]

The effect of Medicaid on labor supply need not be comparable to the SCHIP program, however. The SCHIP expansion provides insurance coverage to children in families with higher incomes and, frequently, access to employer sponsored health plans. In addition, the SCHIP program is associated with greater variation in state level eligibility rules than traditional Medicaid. The federal government gives individual states a large degree of flexibility in the design and operation of their SCHIP programs. Some states established income eligibility thresholds well beyond the federal poverty limit (FPL). New Jersey, for instance, covered

[11] In an early paper, Yelowitz (1995) estimates reduced form relationships between Medicaid eligibility rules and labor supply behavior. He finds that reforms to Medicaid eligibility rules for children increase maternal labor force participation. These findings were questioned in later research (Ham and Shore-Sheppard, 2005a).

children living in families with incomes less than or equal to 350% of the federal poverty limit in 2007, whereas thresholds in North Dakota and Oregon were 140% and 185% FPL respectively. Because it is likely that access to private or employer sponsored insurance varies with these thresholds, the incentives to participate in SCHIP and to work could vary substantially. While such heterogeneity is useful from an empirical perspective, it complicates the interpretation of the program's relationship with labor supply.

In this paper, I study the role of SCHIP on maternal labor supply behavior in two steps. First, I estimate the effect of the eligibility expansion on health insurance coverage for a sample of children belonging to single mothers, similar to the insurance crowd-out literature. Second, I examine the effect of changes in income eligibility thresholds on the work effort of single mothers. Results indicate that SCHIP is associated with public insurance take-up among children, suggesting the program was successful in enrolling uninsured children. Corresponding estimates on private and any insurance coverage, however, imply that a large fraction of the take-up occurs among children who were previously privately insured. Finally, I find no evidence of a relationship between SCHIP and work behavior in the labor supply analysis.

In section II of this paper, I discuss important institutional details about SCHIP and review related academic literature. Theoretical labor supply predictions, data, and empirical methods are outlined in section III. Health insurance coverage results are found in section IV, and labor supply results are discussed in section V. Finally, section VI concludes.

II. Background Information

II.A. The State Children's Health Insurance Program

The federal government established the State Children's Health Insurance Program as part of the Balanced Budget Act of 1997. The program is intended to expand health insurance access to children less than 19 years in age in families earning too much income to qualify for Medicaid coverage. By the end of 2000, all states finished initial implementation of their SCHIP programs. Matching funds are provided to states from the federal government, but total funding is capped. Additionally, the law allows states considerable flexibility in the design and maintenance of their programs. To satisfy SCHIP requirements, states can implement an expansion of their existing Medicaid programs, create a stand-alone program, or enact some combination of the two options. As a consequence, eligibility and program rules vary considerably across states.

The discussion of SCHIP eligibility rules regarding family income can be separated into two parts – thresholds and disregards. Children are generally eligible for SCHIP coverage if their family's income is below the income limit, or threshold, for eligibility. Income disregards raise eligibility cutoffs by allowing families to earn an income in excess of a given threshold. This is done by exempting specific amounts of family income in order to reflect work status or child care expenses, as well as child support. As with eligibility thresholds, there is considerable variation in income disregards. These vary with expense type, such that some states only exempt work expenses and others discount all three types. The amount of income disregarded varies as well. In 2008, for instance, Alabama allowed families to disregard $90 of income per month for work expenses. The same disregard in Kansas was $200 monthly (Ross et al., 2008a). The size of child care disregards often vary with child age, with younger children eligible for larger income exemptions. All income eligibility threshold information in this analysis is calculated using work and child care disregards.[12]

SCHIP includes a number of additional provisions regarding cost-sharing and private insurance crowd-out. Under traditional Medicaid, families were not required to pay premiums or cost-sharing. The SCHIP expansion, however, does allow states to institute these policies. Generally states can charge both premiums and cost sharing if family income exceeds 100% FPL. Families earning below 100% FPL do not pay premiums, but can face cost sharing charges for non-preventative medical services (CBO, 2007). In 2006, 35 states charged premiums and roughly a third required cost-sharing payments (Ross et al., 2007). These payments vary across states and income level, but are usually low.[13] In addition, the total sum of premium and cost-sharing payments is capped at 5% of family income (CBO, 2007).

In addition, most states use enrollment waiting periods to inhibit private insurance crowd-out. Waiting periods require children in families be uninsured for a specific length of time before receiving coverage, though some states have exceptions for involuntary loss of private coverage (CBO, 2007). As of 2006, 35 states had some sort of waiting period, almost all of which are 6 months in length or less (Ross et al., 2007). For privately insured families, these policies raise the cost of leaving existing coverage for public insurance. However, this may also have the effect of reducing take-up among uninsured children.

The SCHIP program is also associated with a limited expansion of parental eligibility for public insurance. The Congressional Budget Office reports that 13 states used SCHIP funding to

[12] I do not apply disregards for child support expenses or income. March CPS data does not include information on child support expenses, though it does report information on child support income.

[13] Ross et al. (2007) report that for a family of three with two children and an income of 200% FPL in 2006, effective annual premium payments exceeded $500 in 9 states. For similar families earning 150% FPL, only 4 states exceeded this amount. At 100% FPL, no states charged more than $500 yearly. This is well below average yearly private insurance premium expenditures for families of 3 persons or more, which were $2,846 in 2006 (Bernard and Banthin, 2009).

expand parental eligibility by 2007 (CBO, 2007). In general, income eligibility is much lower for parents than children in either the Medicaid or SCHIP programs. Only 8 states had parental income eligibility thresholds for Medicaid or SCHIP above 200% FPL in 2006, with thresholds in a majority of states less than or equal to the poverty level (Ross et al., 2007). Large differences between child and parental eligibility for SCHIP may encourage public insurance take-up among children without changing labor supply among working mothers.

II.B. Relevant literature

Authors of recent papers find little evidence of a relationship between maternal work behavior and public health insurance programs for children.[14] Strumpf (2011) examines the initial implementation of Medicaid in the late 1960's and its effect on labor supply behavior. She uses both a difference-in-difference and triple difference specification to evaluate the effect of the program's introduction on labor force participation of single mothers. No evidence of a relationship between Medicaid eligibility and labor supply behavior is found. Ham and Shore-Sheppard (2005a) use variation in income eligibility thresholds to determine an effect on AFDC and labor force participation for a sample of single mothers during the middle 1980s to early 1990s. They estimate specifications with AFDC and Medicaid thresholds entered separately, finding no evidence that Medicaid reforms in this period increase labor force participation. The authors do find, however, that AFDC thresholds are consistently associated with both of the dependent variables.

[14] Older papers in the literature offer mixed evidence of a labor supply effect from Medicaid. Yelowitz (1995) finds evidence of an association between Medicaid and maternal labor supply. Others (Winkler, 1991; Moffitt and Wolfe, 1992; Montgomery and Navin, 2000; Meyer and Rosenbaum, 2001) conclude that the program has no effect on maternal labor supply.

There is a sizable literature devoted to evaluating the effects of public insurance expansions on both take-up and private insurance crowd-out. Many authors from this literature analyze the late 1980s to early 1990s Medicaid expansions. Cutler and Gruber (1996) were the first to assess the relationship between Medicaid eligibility and insurance coverage. Using Current Population Survey (CPS) data and a simulated eligibility measure to instrument for program eligibility, they estimate crowd out rates of 31-50%. Ham and Shore-Sheppard (2005b) also conduct a Cutler and Gruber style analysis, but with Survey of Income and Program Participation (SIPP) data. They find no evidence of crowd-out.

Researchers have also used SCHIP to investigate questions of public coverage take-up and private insurance crowd-out. Authors estimate high levels of crowd-out due to the SCHIP expansion. These researchers (Lo Sasso and Buchmueller, 2004; Hudson et al., 2005; Gruber and Simon, 2008) estimate crowd-out rates between 50% and 60%, though their estimates are sensitive to specification. Results are robust to the choice of data set as authors use the CPS, Medical Expenditure Panel Survey (MEPS), and SIPP. Estimates of SCHIP take-up among eligible children are low. Gruber and Simon (2008) and Lo Sasso and Buchmueller (2004) estimate only 5 – 16% of eligible children receive SCHIP coverage, though the latter pair of authors argue that the take-up rate for previously uninsured children is over 24%. Additionally, SCHIP legislation included several provisions designed to reduce crowd-out. Lo Sasso and Buchmueller (2004) find that waiting periods reduce both take-up of public insurance and crowd-out. Gruber and Simon (2008) conclude differently, arguing that waiting periods and cost sharing associated with SCHIP exacerbate crowd-out by limiting take-up of the uninsured more than they limit movement away from private insurance.

III. Methods

III.A. Predictions from a static labor supply model

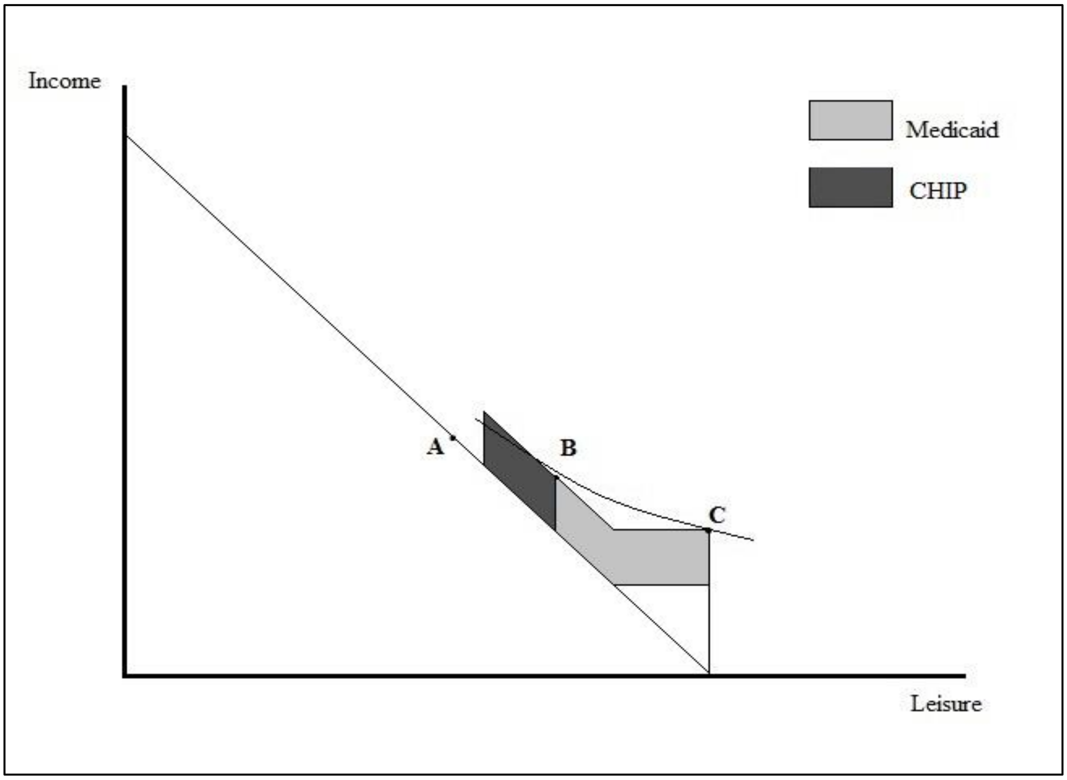
Standard static labor supply models imply specific predictions for both the extensive and intensive margin labor supply responses, depicted in Figure 2.1 below. For single mothers that value children's health insurance, SCHIP effectively extends children's insurance eligibility beyond existing coverage from the Medicaid program. This implies that the traditional Medicaid budget constraint notch exists at higher income levels under SCHIP.[15] Working mothers above this notch, with or without insurance for their children, may decrease earnings to become eligible for the program. These mothers are indicated with an *A* in Figure 2.1. Employed mothers at *B*, constrained to Medicaid coverage at the old notch, may increase their labor supply in response to eligibility expansion. Finally, those located between these two points – in the SCHIP expansion region of the budget constraint – may reduce labor supply because of the income effect generated by the program. This group includes mothers with employer sponsored insurance as well as those with uninsured children.[16]

The overall intensive margin response, then, is ambiguous. Families with existing public coverage through Medicaid may increase labor supply if the SCHIP expansion allows mothers to increase work effort and reach a higher indifference curve. Mothers of children without public insurance coverage, either uninsured or privately covered, face negative intensive margin incentives. For these families, the income effect generated by SCHIP allows them to reduce labor supply and move to a more desirable mix of income and leisure. The size of the income

[15] This is a simplification, since few states expanded SCHIP eligibility to parents. In states without expanded parental eligibility, there may be an additional notch between Medicaid and SCHIP. This generates additional discontinuities in the mother's budget set.

[16] These predictions do not account for any differences in the set of insurance coverage options for the mother as a result of changes in her labor supply behavior. A change in labor supply that results in the loss of employer sponsored coverage, or even a change in medical providers, may reduce the mother's incentive to alter employment behavior in response to SCHIP.

Figure 2.1: Effect of SCHIP on labor supply (intensive and extensive margin)



effect generated by SCHIP for working mothers will depend on how much they value public insurance coverage. Mothers will value public coverage more if they are financing the employer plan with premium contributions or if their employer plan does not provide a level of benefits comparable to SCHIP. Investigating employer-sponsored insurance trends between 2000 and 2008, Vistnes et al. (2010) find both an increase in premium contributions paid by employees and a decline in insurance offers from some employers. Both trends may encourage families to take up public coverage.

Figure 2.1 also depicts the extensive margin labor supply response. The static labor supply model predicts an extensive margin response only from unemployed single mothers with Medicaid coverage for their children. These mothers, depicted at point *C* on the budget constraint, may be induced to join if SCHIP allows them to work and retain insurance coverage for their children. This may be especially relevant for individuals with limited discretion over hours of work. It is not expected that SCHIP would result in a negative extensive margin response among working mothers with employer sponsored insurance or without coverage for their children. These families were already free to leave the labor force and obtain Medicaid coverage before the program was in effect.

Despite the extensive margin predictions, SCHIP may have a role in reducing labor force participation. This is because the program is associated with efforts to increase enrollment among low-income families eligible for traditional Medicaid. Selden et al. (2004) argue that, "concerns about Medicaid and SCHIP enrollment have led to unprecedented efforts to improve outreach, reduce stigma, simplify enrollment, and retain eligible enrollees since 1996" (p.40). They also note that SCHIP legislation requires applicants be screened for Medicaid eligibility, potentially increasing Medicaid enrollment (Selden et al., 2004). Consequently, SCHIP implementation may also increase public coverage among families already eligible for Medicaid. This can reduce labor force participation among low income mothers working to finance health care with employer coverage or their wages. A similar response can also occur among relatively high income eligible families with limited exposure to, or knowledge of, the public health insurance system. A final possibility for a negative participation effect is among families not previously eligible for traditional Medicaid even if they left work, possibly due to high levels of non-work related income.[17]

[17] Child support payments are an important source of income for some single mothers. Approximately one-third of the mothers in this analysis receive child support payments. Conditional on positive values, the median annual payment is just over $2,990 (in 1996 dollars). For comparison, the federal poverty limit in 1996 for a family of three was $12,980.

III.B. Data

This analysis uses data from the IPUMS CPS, which is an integrated version of the March Current Population Survey (CPS) managed by the University of Minnesota, survey years 1997 through 2007.[18] I chose this time period to provide sufficient policy variation before and after the implementation of SCHIP, and to avoid the Great Recession era and the introduction of the Affordable Care Act. I lower child and maternal age by one year to reflect the age concurrent with recorded labor market information. I draw two extracts from this data set. The first consists of 19-65 year old single mothers whose youngest child is less than or equal to 18 years of age. Attached to each observation is information regarding all related children living with the mother. The sample does not contain information about children greater than 18 years of age, or from children designated in the data as household heads. The second extract consists of 0-18 year old children belonging to the single mothers identified in the first sample. In both samples, I drop observations from Tennessee. Throughout much of the sample period, Tennessee's TennCare program extended Medicaid coverage to uninsured children not eligible for traditional Medicaid. Importantly, TennCare did not restrict eligibility based on income, implying that children did not face an income eligibility cutoff for coverage.

Four measures of labor supply are collected in the maternal sample: labor force participation, full-time work participation, usual weekly work hours, and annual weeks worked. Labor force participation is a binary indicator equal to one if the mother spends at least one week at work during the year. Full-time employment is a binary variable equal to one if the mother is both employed and works thirty-five or more hours a week, based on usual weekly work hours. Usual weekly hours is a self-reported measure of work hours the mother experiences in a typical

[18] Because the March CPS contains data on the previous year, this survey period reflects information from years 1996 through 2006.

week. Finally, the weeks worked variable counts the number of weeks the mother is employed over the course of the year.

One feature of the IPUMS CPS is the summary health insurance variables constructed by the State Health Access Data Assistance Center (SHADAC) at the University of Minnesota. These insurance variables are created to be consistent across survey years, and are distinct from health insurance variables reported in the March CPS supplement. IPUMS CPS documentation notes that health insurance information from the March supplement is vulnerable to changes in the CPS survey, primarily the introduction of an insurance coverage verification question and changes in editing procedures. To account for these changes, SHADAC releases an enhanced version of the March variables meant to be consistent across time.[19]

Policy data come from a variety of sources. Income eligibility thresholds for Medicaid and SCHIP, both before and after the introduction of SCHIP, are primarily from Rosenbach et al. (2001) and annual surveys of state SCHIP policies by the Kaiser Family Foundation.[20] For this analysis, the relevant eligibility threshold for each state and child age group is the Medicaid or SCHIP income cutoff during that year. Dates of state level SCHIP program implementation come from Rosenbach et al. (2001). Medicaid and SCHIP income disregard information are from Ku et al. (1999) and Ross et al. (2008b), as well as the Urban Institute's TRIM 3 database.[21] Ku et al. (1999) describe state level disregard information for Medicaid and SCHIP as of October 1998, and Ross et al. (2008b) define the same information as of January 2008. These are used as endpoints; if disregards are unchanged between the two data sources then that disregard

[19] In addition, the documentation also advises the use of SHADAC summary health insurance weights as opposed to standard CPS weights. These weights remove observations from survey respondents who do not answer questions from the March supplement. Responses for these individuals are normally imputed. Consequently, sample sizes using SHADAC weights are smaller than with standard weights.

[20] See works cited page for complete list of surveys.

[21] For my analysis, the relevant disregard information (Medicaid or SCHIP disregards) is the one that applies to the highest eligibility threshold in that state.

information is applied to all intervening years. When this is not the case, the disregard change is coded as occurring during the year of the SCHIP policy adoption. Disregard information for years before 1998 are from TRIM 3.

Sample means of key variables from both samples are found on Table 2.1. These means are calculated using SHADAC weights. The top panel presents mean and standard deviation information for policy and labor supply variables. The mean SCHIP or Medicaid income threshold in the sample is just under 2.3, implying the average eligibility cutoff for children is 230% of FPL. Three-quarters of all children in the child sample are imputed to be program eligible based on SCHIP and Medicaid eligibility rules. Maternal labor supply characteristics are also depicted in the top panel. Most mothers, 80.6%, are in the labor force. Those working are on the job nearly 40 hours a week and over 45 weeks out of the year, on average. The third panel indicates health insurance information in both samples. A majority of the children have health insurance, roughly 86% to 87% in either sample. Private insurance is more prevalent than public for these children. In the fourth panel, a majority of mothers are separated or divorced from their spouse, and roughly a quarter have at least a two year college degree.

Trends in income eligibility and insurance coverage rates are displayed in Figures 2.2 and 2.3. Figure 2.2 illustrates average income eligibility thresholds across years, by age group.[22] These trends demonstrate the variation present in the key policy variable used in this analysis. Before SCHIP was fully implemented, there was considerable age variation in eligibility rules. As SCHIP moved towards complete implementation, differences in age group eligibility

[22] Age cutoffs for the age groups presented on these figures are based on eligibility rules for traditional Medicaid. Infants (<1 years), on average, experience the highest levels of income eligibility. Federal law mandates that children less than six years in age (1-5 years) have income eligibility of at least 133% FPL. Children born after September 30, 1983, and who are six years of age or greater, have eligibility requirements of at least 100% FPL (6-16 year olds by the end of the SCHIP implementation period in 2000). The last group, 17 to 18 year olds, is never subject to increased eligibility under traditional Medicaid before the SCHIP expansion.

Table 2.1: Means of key variables

Variable	*Child sample*		*Maternal sample*	
	Mean	SD	Mean	SD
Policy and labor supply variables				
Medicaid/SCHIP income eligibility threshold	2.282	0.609	2.276	0.593
Imputed eligibility	0.751	0.432		
Imputed eligibility (share of children)			0.704	0.451
Imputed eligibility (all children)			0.694	0.461
Labor force participation (LFP)			0.806	0.396
Full-time work (FT)			0.650	0.477
Hours worked, positive values~			38.050	9.588
Weeks worked, positive values~			45.356	13.010
Health insurance coverage				
Public	0.398	0.489		
Private	0.467	0.499		
Group private	0.386	0.487		
Non-group private	0.081	0.272		
Any	0.865	0.342		
Public (share)			0.356	0.471
Private (share)			0.504	0.492
Group private (share)			0.417	0.483
Non-group private (share)			0.086	0.271
Any (share)			0.860	0.337
Public (all)			0.340	0.474
Private (all)			0.488	0.500
Group private (all)			0.397	0.489
Non-group private (all)			0.075	0.264
Any (all)			0.845	0.362
Sample characteristics				
Female	0.500	0.500		
Foreign born	0.039	0.193		
Share of family w/ some college	0.009	0.056		
Share of family employed	0.050	0.128		
Family size	3.770	1.569	3.279	1.374
White	0.608	0.488	0.649	0.477
Age	8.961	5.273	34.933	9.009
Child less than 5			0.306	0.461
Cohabitation			0.033	0.179
Separated or divorced			0.553	0.497
High school			0.356	0.479
Some college			0.234	0.423
Two year degree			0.096	0.295
Four year degree			0.097	0.296

Table 2.1 (cont'd)

Graduate degree			0.036	0.186
Observations	117,131		67,016	

~There are 54,513 observations for the conditional work variables

disappeared. This implies older age groups experienced relatively large increases in income eligibility. Figure 2.3 shows rates of insurance coverage, by type, across income. Income is measured in units of the FPL. At 200% FPL, roughly where SCHIP is designed to operate, a majority of children have private insurance coverage. Less than one fifth of children have public coverage at this income level. These insurance trends show that SCHIP extends public insurance eligibility to children living in families that are much more likely to have private coverage, suggesting the program's potential for private insurance crowd-out.

Figure 2.2: Child Income Eligibility Thresholds for Medicaid and SCHIP

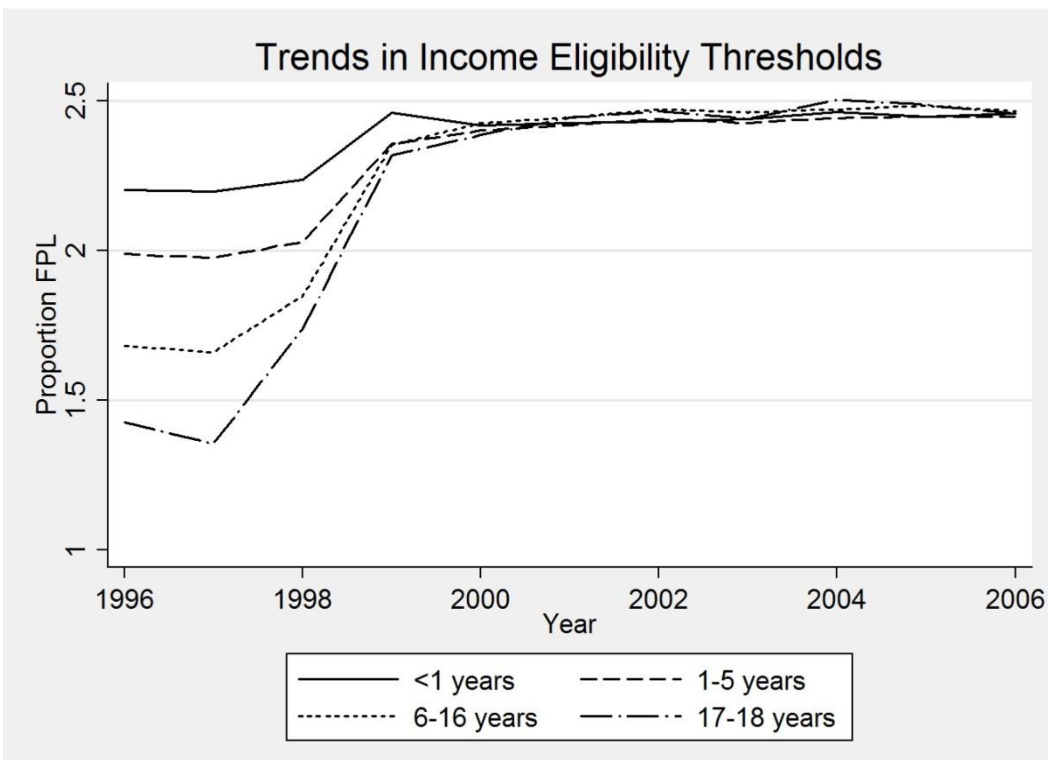
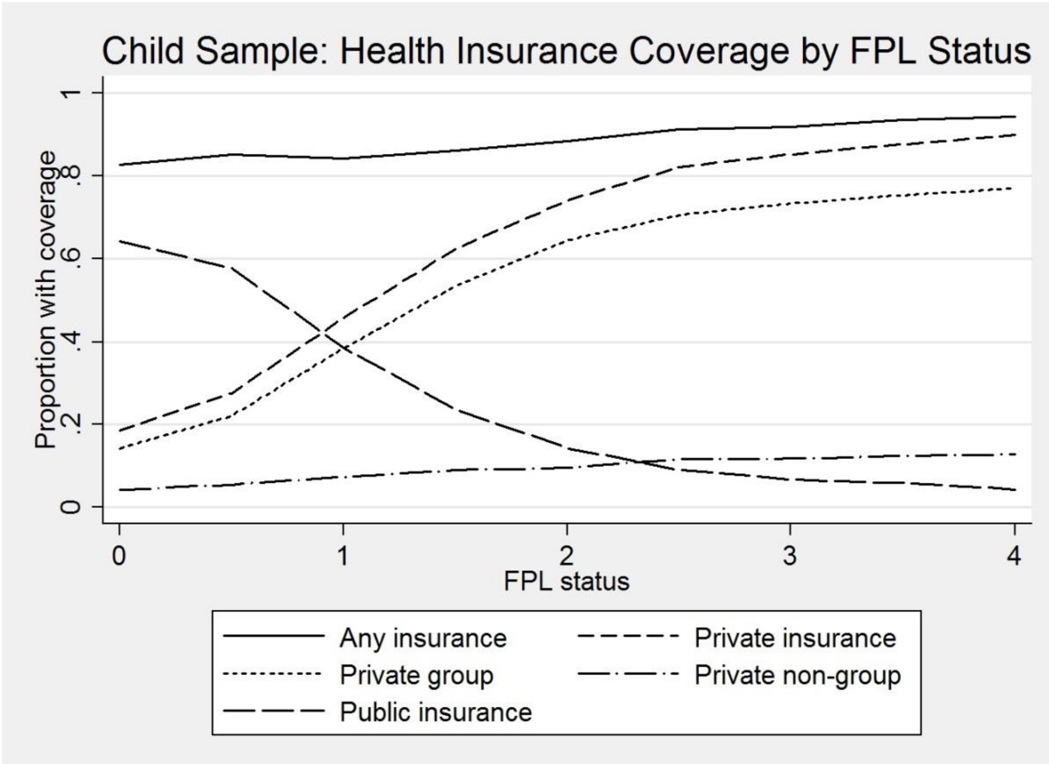


Figure 2.3: Child Health Insurance Coverage Trends, by Income Status



III.C. Empirical methodology

To organize my analysis, consider the following labor supply equation (where *i* denotes mother, *a* youngest child age, *s* state, and *t* year):

$$LS_{iast} = \alpha_s + \delta_t + \mu_a + \lambda_{st} + \gamma \text{ PublicCoverage}_{iast} + X_i\beta + \varepsilon_{iast} \qquad (1)$$

LS_{iast} is a maternal labor supply variable, meant to capture extensive margin (indicators for labor force participation and full-time work) or intensive margin effects (usual hours worked per week and number of weeks worked during the year). Specifications with an intensive margin variable are conditional on positive values. $PublicCoverage_{iast}$ reflects the public insurance coverage status of children attached to the mother's observation. I use two definitions of this variable in the analysis. The first is a share variable meant to indicate the fraction of children with public insurance. The second definition is an indicator that equals one if all children have coverage.[23] X_i is a vector with information on race, age, family size and composition, cohabitation status, separated or divorced status, and education level. Standard errors are clustered at the state level, and observations are weighted using SHADAC summary health insurance weights.

To control for unobserved effects, I include state and year indicators in the model. These allow the model to capture unobservable time and state specific variation that may be correlated with eligibility rules, and are denoted with α_s and δ_t. I also include youngest child age fixed effects, μ_a, to account for time invariant age group specific behavior. In an effort to control for year differences that affect certain states more or less than others, I include a fully interacted set of state and year dummies in all specifications. These are denoted with λ_{st}. Including these interactions imply that equation (1) achieves identification from changes in income eligibility thresholds across youngest child age groups within state and year combinations. This would include comparisons of similar families where some of those families, before the implementation of SCHIP, were ineligible for Medicaid because their children were older and faced a lower income threshold for coverage. SCHIP generally removes differences in income eligibility based on child age, which existed under traditional Medicaid.

Because individuals select into coverage, $PublicCoverage_{iast}$ should be regarded as endogenous in this specification. As a solution, I estimate equation (1) using an income eligibility threshold variable as an instrument for $PublicCoverage_{iast}$. Variation in the threshold variable is due solely to state, year, and child age group differences in eligibility policies for state Medicaid or SCHIP programs. This approach follows methods from the Medicaid labor supply

[23] Other definitions of insurance coverage were considered. These include an indicator for any (at least one) child in the family with coverage, and whether the youngest child in the family has coverage. Estimating these variables does not change the results substantially.

literature (Yelowitz, 1995; Ham and Shore-Sheppard, 2005a; Dave et al. 2013), where authors relate variation in program eligibility rules with labor supply.

A benefit of equation (1) is that it connects two related literatures. When estimating equation (1) by instrumental variables, the first stage is similar to health insurance models from the health insurance crowd-out and take-up literature:

$$PublicCoverage_{iast} = \alpha_s + \delta_t + \mu_a + \lambda_{st} + \gamma EligibilityThreshold_{iast} + X_i\beta + \varepsilon_{iast} \qquad (2)$$

Fundamentally, this literature seeks to understand the relationship between public health insurance eligibility and insurance coverage behavior among eligible individuals and families. Estimates of private insurance crowd-out would imply that privately insured families move to public coverage in response to SCHIP. The labor supply analysis helps determine whether insurance crowd-out is the result of a change in maternal labor supply behavior or decisions to enroll with employer sponsored coverage.

Equation (1) is also related to the Medicaid labor supply literature. The reduced form associated with estimating equation (1) by instrumental variables updates methods from Yelowitz (1995) and Ham and Shore-Sheppard (2005a), who associate labor supply with measures of program eligibility rules. The reduced form relationship between maternal labor supply and SCHIP eligibility is:

$$LS_{iast} = \alpha_s + \delta_t + \mu_a + \lambda_{st} + \gamma EligibilityThreshold_{iast} + X_i\beta + \varepsilon_{iast} \qquad (3)$$

Dave et al. (2013) use a similar reduced form approach, relating the work behavior of pregnant women with a simulated measure of Medicaid eligibility. A disadvantage of reduced form models like equation (3) is that they estimate the average effect of eligibility across households with varying incentives for program take-up and labor supply. As discussed in section III.A, SCHIP labor supply predictions can be either positive or negative depending on a child's initial insurance coverage status.

Instrumental variable estimation of equation (1) also shares this disadvantage. The instrumental variables strategy assumes that the threshold variable affects labor supply though only the public insurance take-up decision. Mothers with children covered under traditional Medicaid, however, may increase work effort with no change to their public insurance status (point *B* mothers in Figure 2.1). In this case, estimating equation (1) by instrumental variables will not deliver the causal labor supply effect of gaining public coverage. If the assumption necessary for instrumental variables estimation is true, however, instrumental variable estimation of equation (1) will reflect the causal effect on maternal labor supply of children gaining public insurance coverage as a result of the SCHIP eligibility expansion.

IV. SCHIP Eligibility and Children's Public Insurance Coverage

Equation (2) investigates the effect of program eligibility rules on public insurance take-up among children. This equation is estimated in both the child and maternal level samples. The purpose of the child level estimation is to benchmark results on insurance coverage to the crowd-out literature. The maternal level analysis serves as the first stage for instrumental variable estimation of equation (1). Insurance coverage variables are defined differently for each sample, with a binary coverage indicator in the child sample and either a share variable or indicator for all children with coverage in the maternal sample.[24]

Estimates of the relationship between SCHIP and public insurance coverage are presented in the first column of Table 2.2. In the child sample, the eligibility expansion is

[24] Other specification differences between the child and maternal level analyses are: different variables in the vector of controls, X_i; child age dummies are used in the child level sample, but are replaced with youngest child age dummies in the maternal level sample.

Table 2.2: Health insurance coverage, equation (2)

Variable	Public	Private	Any
		Child sample	
SCHIP threshold	0.130**	-0.055	0.075**
	(0.044)	(0.040)	(0.016)
	[2.955]	*[1.375]*	*[4.688]*
Mean value	*0.398*	*0.467*	*0.865*
Observations		117,131	
		Maternal sample: share of children	
SCHIP threshold	0.080*	-0.029	0.051**
	(0.031)	(0.032)	(0.015)
	[2.580]	*[0.906]*	*[3.400]*
Mean value	*0.356*	*0.504*	*0.860*
		Maternal sample: all children	
SCHIP threshold	0.070*	-0.037	0.040*
	(0.030)	(0.033)	(0.016)
	[2.333]	*[1.121]*	*[2.500]*
Mean value	*0.340*	*0.488*	*0.845*
Observations		67,016	

Cluster robust standard errors in parentheses, absolute value of t-statistic in brackets
** p<0.01, * p<0.05
Not indicated are estimates from the following control variables: family size, white racial status, black racial status, female, foreign born, share of family with some college education, share of family in labor force, age, age squared, child age dummies, year dummies, and state dummies.

associated with public insurance take-up. For a one unit increase in the eligibility threshold (one unit equals 100% FPL), the eligibility expansion is associated with a 13 percentage point increase in the likelihood of public insurance coverage. This represents an increase in the mean number of children with public insurance of almost 33%. Maternal level estimates, found in the middle and bottom panels, are similar. The threshold variable is associated with positive public insurance take-up, the middle panel showing an 8 percentage point increase in the share of children with coverage from a one unit increase in the eligibility threshold. For the bottom panel, the same threshold change is associated with a 7 percentage point increase in the likelihood of all children having public insurance coverage.

To better understand the public insurance take-up decision among children, dependent variables reflecting private and any insurance coverage are estimated in addition to the $PublicCoverage_{ist}$ variable. Insurance coverage variables are defined to be mutually exclusive; summing across public and private coverage equals the any insurance category.[25] Private coverage estimates are reported in the middle column of Table 2.2. In both the child and maternal samples, coefficients for private coverage are negative but never significant. Results for any insurance coverage are found in the last column of Table 2.2. In the child sample, the coefficient on any insurance coverage indicates a statistically significant 7.5 percentage point decline in the probability of a child being uninsured. Maternal sample estimates are similar, suggesting that the SCHIP expansion increased the likelihood of having any insurance coverage.

In both the child and maternal samples, the significant effect of the threshold variable on public insurance take-up is associated with a less than equivalent rise in the likelihood of any insurance coverage. This suggests that some fraction of public insurance take-up associated with SCHIP is due to children leaving private coverage. Public and any insurance coverage estimates imply a crowd-out rate of just over 42% in the child sample and roughly 36% in the maternal

[25] Individuals reporting both private and public insurance in a given year are recorded as having private insurance. Additionally, the all children insurance coverage variables in the maternal sample do not sum to the overall coverage category. Within some families, different children have different sources of coverage. For these families, the all insurance coverage definition records a 0 for any specific coverage type, but a 1 for having any insurance.

sample.[26] These are smaller than similar estimates from Lo Sasso and Buchmueller (2004) or Gruber and Simon (2008), whose preferred estimates range from nearly 50% to 60%.

The effect of the threshold variable on private insurance coverage is statistically insignificant in both samples. This result, however, masks variation across group and non-group private insurance coverage. Table 2.3 reports the effect of SCHIP on private insurance categories for the child and maternal samples. The effect of the eligibility expansion on group coverage is large – a one unit increase in the threshold is associated with a 10.9 percentage point decline in the probability of group coverage for the child sample. The same threshold change yields group coverage declines of 8.1 to 9.4 percentage points for the maternal sample. The effect of the threshold variable on non-group private coverage is smaller in magnitude, but show an increase of 3.8 to 5.2 percentage points in the maternal sample. Lo Sasso and Buchmueller (2004) find similar results on group and non-group private coverage. They argue this is evidence that parents are incorrectly reporting SCHIP coverage as private coverage, and that this is reflected in positive estimates on private non-group coverage and negative estimates on group insurance (Lo Sasso and Buchmueller, 2004). If such an assumption is true about non-group private coverage, then insurance coverage estimates from Table 2.2 understate both the true effect of SCHIP on public coverage and the extent of private insurance crowd-out.[27]

Finally, public insurance coverage results from the maternal level sample suggest that the eligibility threshold is a weak instrumental variable. From maternal sample results in Table 2.2, the t-statistic associated with the share of children coefficient is only 2.58. For the all children

[26] The crowd-out rate is the difference between the increase in public and any coverage, divided by the increase in public coverage. I use the following formula: 1 – (coefficient for any/coefficient for public).
[27] This can be seen from the expression used to calculate crowd-out, 1 – (coefficient for any/coefficient for public). If the increase in non-group private coverage actually reflects public coverage take-up, then the coefficient for public coverage is too small. The coefficient for any coverage is not affected. This decreases the size of the crowd-out expression above.

Table 2.3: Group and non-group private coverage, equation (2)

Variable	Group Private	Non-Group Private
	Child sample	
SCHIP threshold	-0.109**	0.054**
	(0.031)	(0.014)
	[3.516]	*[3.857]*
Mean value	*0.386*	*0.081*
Observations	117,131	
	Maternal sample: share of children	
SCHIP threshold	-0.081**	0.052**
	(0.024)	(0.014)
	[3.375]	*[3.714]*
Mean value	*0.417*	*0.086*
	Maternal sample: all children	
SCHIP threshold	-0.094**	0.038*
	(0.028)	(0.015)
	[3.357]	*[2.533]*
Mean value	*0.397*	*0.075*
Observations	67,016	

Cluster robust standard errors in parentheses, absolute value of t-statistic in brackets
** p<0.01, * p<0.05
Not indicated are estimates from the following control variables: family size, white racial status, black racial status, female, foreign born, share of family with some college education, share of family in labor force, age, age squared, child age dummies, year dummies, and state dummies.

public coverage coefficient, the t-statistic is 2.33. This shows that the threshold variable is only weakly correlated with public coverage, which is the key explanatory variable in the labor supply model.[28] A weak relationship between these variables may mean that instrumental variables

[28] Tables 2.6 through 2.12 present results from specifications including additional policy variables (parental eligibility thresholds for public insurance, waiting periods, premium payments, and cost-sharing payments) as first stage instruments. These policy variables do not strengthen the first stage relationship.

estimation of equation (1) will yield large standard errors and point estimates (Bound, Jaeger, and Baker, 1995). Consequently these estimates, which follow, should be interpreted cautiously.

V. The Effect of SCHIP Coverage on Maternal Labor Supply

Labor supply estimates from equation (3), which provide the direct effect of SCHIP on maternal work behavior, are reported in Table 2.4. This specification is meant to be similar to reduced form methods used in the literature, and serves as the starting point for the labor supply analysis. As with the analysis of Ham and Shore-Sheppard (2005a), estimates from Table 2.4 indicate almost no evidence that SCHIP influences the labor supply of single mothers. Coefficients generally show a negative extensive and intensive margin labor supply effect, though the coefficient on the threshold variable for hours is positive. These estimates are insignificant at the 5% level, with the exception of the threshold variable coefficient for annual weeks worked. This estimate, in the last column of Table 2.4, implies a reduction in work of over a week and a half. Relative to the sample mean, this represents a decline of nearly 3.5%. It is possible that the negative coefficient on weeks reflects labor force exits, since the variable includes mothers who work a fraction of the year before leaving work.

Estimates from equation (3) do not provide any evidence of a SCHIP labor supply effect. This leaves equation (1), which focuses on only the public insurance take-up decision, to indicate a relationship between the program and maternal work behavior. Table 2.5 reports ordinary least squares and two-stage least squares (2SLS) estimates of equation (1). The first four columns of Table 2.5 report results for both the labor force and full-time work participation variables. OLS estimates for both variables are negative, reflecting the fact that public coverage status is associated with lower rates of labor force attachment. 2SLS estimates are negative and

Table 2.4: Labor supply, equation (3)

Policy variable	LFP	FT	Hours~	Weeks~
SCHIP threshold	-0.026	-0.030	0.086	-1.566*
	(0.019)	(0.020)	(0.402)	(0.752)
	[1.368]	*[1.500]*	*[0.214]*	*[2.082]*
Mean value	*0.806*	*0.650*	*38.050*	*45.356*
Observations	67,016		54,513	

Cluster robust standard errors in parentheses, absolute value of t-statistic in brackets
** p<0.01, * p<0.05
~Estimates conditional on positive values
Not indicated are estimates from the following control variables: family size, child less than five years in age, white racial status, cohabitate with partner but not married, separated or divorced, high school education, some college education, two year college degree, four year college degree, graduate degree, age, age squared, youngest child age dummies, year dummies, and state dummies.

Table 2.5: Labor supply, equation (1)

Variable	LFP		FT		Hours		Weeks	
	(1) OLS	*(2) 2SLS*	*(3) OLS*	*(4) 2SLS*	*(5) OLS*	*(6) 2SLS*	*(7) OLS*	*(8) 2SLS*
	Share of children (first stage t-statistic: 2.580)							
Public coverage	-0.219**	-0.322	-0.302**	-0.381	-3.479**	1.346	-6.962**	-24.49**
	(0.014)	(0.202)	(0.011)	(0.199)	(0.153)	(6.391)	(0.232)	(8.365)
	All children (first stage t-statistic: 2.333)							
Public coverage	-0.214**	-0.370	-0.291**	-0.437	-3.350**	1.554	-6.780**	-28.27**
	(0.013)	(0.225)	(0.010)	(0.223)	(0.144)	(7.414)	(0.231)	(10.590)
Observations	67,016		67,016		54,513		54,513	

Cluster robust standard errors in parentheses
** p<0.01, * p<0.05
Not indicated are estimates from the following control variables: family size, child less than five years in age, white racial status, cohabitate with partner but not married, separated or divorced, high school education, some college education, two year college degree, four year college degree, graduate degree, age, age squared, youngest child age dummies, year dummies, and state dummies.

insignificant, though the full-time work participation results are significant below the 10% level. According to the point estimates, moving all children in a family to public coverage reduces a mother's likelihood of full-time work 38.1 to 43.7 percentage points. This reflects a decline of full-time work participation of at least 58.6% relative to the sample mean.

Columns 5-8 of Table 2.5 reports results for the hours and weeks dependent variables. 2SLS estimates for hours are never significant and have large standard errors. Estimates for weeks worked are significant and negative. The coefficients imply that when all children in a family are induced into public coverage, mothers work approximately 24 to 28 fewer weeks each year. Relative to the sample mean of just over 45 weeks, this reflects a 54% to 62% reduction in work weeks. The significant effect of SCHIP on weeks worked may reflect job exits among working mothers, though the large size of the coefficients and standard errors may be due to the weak first stage association between public coverage and the eligibility threshold (Bound, Jaeger, and Baker, 1995).

VI. Conclusion

The State Children's Health Insurance Program is one of the largest recent reforms to the public health insurance system in the United States, providing insurance to children living in families with incomes above Medicaid eligibility thresholds. Because eligibility is means-tested, some worry that SCHIP distorts the labor supply decisions of parents. In this paper, I examine the effect of the SCHIP expansion on public insurance coverage for children and the labor supply behavior of single mothers. First stage estimates imply that a significant fraction, nearly 40%, of public insurance take-up associated with SCHIP is the result of private insurance crowd-out. Labor supply estimates from equation (1) indicate little evidence of an extensive or intensive margin response, although the program is associated with a negative effect on annual weeks worked. These estimates should be interpreted with caution, however, as the first stage relationship between public coverage and the threshold variable is weak. In addition, the instrumental variables strategy used to estimate equation (1) relies on the assumption that the

SCHIP threshold variable affects labor supply only through the public insurance take-up decision. If the program also affects maternal work behavior through another channel, my instrumental variable estimates are difficult to interpret. Reduced form estimates from equation (3) also indicate little evidence of a labor supply response among single mothers. Only the negative coefficient on annual weeks worked is significant.

Overall, estimates from equations (1) and (3) show almost no evidence of a relationship between SCHIP and maternal labor supply. This conclusion may reflect changes in employer sponsored health insurance behavior among eligible families and their employers. Based on Figure 3, a large fraction of families made eligible for public coverage because of SCHIP were privately insured. This suggests that the potential for crowd-out with the program is higher than with previous Medicaid expansions. Vistnes et al. (2010) attribute decreasing rates of employer coverage between 2000 and 2008 to declines in both coverage offers from employers, which includes offers of dependent coverage, and take-up among employees. Importantly, the authors find that these trends correspond with large increases in employee premium costs.

There is also substantial heterogeneity in the coverage and design of employer insurance plans. So-called “mini-med” plans, for instance, offer limited benefits for covered employees (Levitt and Claxton, 2011). SCHIP insurance is less heterogeneous. States are required to cover the same services as Medicaid or offer a set of services actuarially equivalent to several benchmark plans, such as insurance offered to federal or state government employees (CBO, 2007). The CBO argues that states often elect to provide vision and dental services under SCHIP, services not often covered by private insurance (CBO, 2007). Limited benefit plans and increasing premium costs may have encouraged mothers to forgo employer coverage for their children in favor of public insurance, without changing work behavior.

These results point to future research topics. The availability of public child health insurance programs like SCHIP may increase the incidence of job turnover for eligible families. This can happen if, before program implementation, parents were constrained to stay in their current job to retain insurance coverage for their children. The availability of SCHIP may allow parents to transition to a more preferred job without loss of insurance benefits.[29] Additionally, methods from this analysis can be adapted to a broader population of households. Results from the health insurance literature (Lo Sasso and Buchmueller, 2004; Gruber and Simon, 2008) show that SCHIP is an important determinant of children's health insurance coverage for all household types. These authors estimate high rates of private insurance crowd-out, suggesting that families substitute employer insurance with public coverage. What these SCHIP induced insurance coverage effects imply for household labor supply are unknown.

[29] Bansak and Raphael (2008) investigate parental job lock in the context of SCHIP using 1996 – 2001 SIPP data. They find married fathers of SCHIP eligible children, whose wives did not have employer sponsored insurance, were 5-6% more likely to separate from their job after implementation of the program than married fathers whose wives did have their own insurance coverage.

APPENDICES

APPENDIX A

Analysis with Additional Medicaid and SCHIP Policy Variables

New policy variables:

1. Parent thresholds: This variable captures the income eligibility cutoff for parental public insurance. It is measured in terms of the income cutoff as a proportion of the FPL.

2. Waiting periods: This variable captures the length of time a child must be uninsured before receiving SCHIP coverage, and is measured in months.

3. Premium payments: This is a dummy variable that indicates if the SCHIP program in state *s* required families to pay a premium payment during year *t*.

4. Cost sharing payments: This is a dummy variable that indicates if the SCHIP program in state *s* required families to pay co-payment/co-insurance payments for any medical services during year *t.*

This analysis uses the same equations as the main analysis. In addition to the child threshold variable I add policy variables for parental public insurance thresholds, waiting periods, premium payments, and cost sharing payments. Since these variables vary at the state by year level, my estimates do not include state by year interaction terms. Consequently, some coefficients will vary from corresponding estimates in main analysis.

$$LS_{iast} = \alpha_s + \delta_t + \mu_a + \gamma\ PublicCoverage_{iast} + X_i\beta + \varepsilon_{iast} \qquad (1)$$

$$PublicCoverage_{iast} = \alpha_s + \delta_t + \mu_a + \gamma_1 EligibilityThreshold_{ast} + \gamma_2 ParentThreshold_{st} + \gamma_3 WaitPeriod_{st} + \gamma_4 Premium_{st} + \gamma_5 CostShare_{st} + X_i\beta + \varepsilon_{iast} \qquad (2)$$

$$LS_{iast} = \alpha_s + \delta_t + \mu_a + \gamma_1 EligibilityThreshold_{ast} + \gamma_2 ParentThreshold_{st} + \gamma_3 WaitPeriod_{st} + \gamma_4 Premium_{st} + \gamma_5 CostShare_{st} + X_i\beta + \varepsilon_{iast} \qquad (3)$$

Table 2.6: Means of additional policy variables

Variable	Mean	SD
Parental threshold (FPL)	0.781	0.512
Waiting period (dummy)	0.463	0.499
Waiting period (months)	2.005	2.558
Premium (dummy)	0.597	0.490
Cost sharing payments (dummy)	0.376	0.484
Observations	67,016	

Table 2.7: First stage (maternal sample), equation (2)

Variable	*Share of children with public coverage*									
	1	2	3	4	5	6	7	8	9	10
SCHIP Threshold	0.027*	0.028*	0.032*	0.035*	0.031*	0.027*	0.035*	0.036*	0.032*	0.033*
	(0.013)	(0.013)	(0.014)	(0.014)	(0.012)	(0.012)	(0.013)	(0.014)	(0.013)	(0.014)
Parental Threshold		0.004							0.003	0.002
		(0.011)							(0.011)	(0.011)
Waiting Period (months)			-0.003				-0.003		-0.003	
			(0.002)				(0.002)		(0.002)	
Waiting Period (dummy)				-0.021				-0.017		-0.019
				(0.015)				(0.016)		(0.016)
Premium					-0.020		-0.017	-0.013	-0.020	-0.016
					(0.016)		(0.016)	(0.016)	(0.016)	(0.017)
Cost sharing						0.003			0.011	0.014
						(0.017)			(0.017)	(0.017)
F-Statistic	4.796	2.920	2.950	2.980	3.830	2.490	2.620	2.380	1.890	1.880
R-Squared	0.1732	0.1732	0.1733	0.1733	0.1733	0.1732	0.1733	0.1733	0.1734	0.1734
Observations	67,016									

Cluster robust standard errors in parentheses

** p<0.01, * p<0.05

Table 2.8: Labor supply, equation (3)

Variable	Labor Force Participation									
	1	2	3	4	5	6	7	8	9	10
SCHIP Threshold	-0.025	-0.024	-0.023	-0.024*	-0.027*	-0.021	-0.025*	-0.025*	-0.021*	-0.022*
	(0.013)	(0.012)	(0.012)	(0.012)	(0.012)	(0.011)	(0.011)	(0.011)	(0.010)	(0.010)
Parental Threshold		0.016							0.015	0.015
		(0.012)							(0.011)	(0.011)
Waiting Period (months)			-0.001				-0.001		-0.001	
			(0.002)				(0.002)		(0.001)	
Waiting Period (dummy)				-0.002				-0.007		-0.002
				(0.013)				(0.014)		(0.013)
Premium					0.012		0.014	0.015	0.019	0.019
					(0.011)		(0.010)	(0.011)	(0.010)	(0.010)
Cost sharing						-0.013			-0.017	-0.017
						(0.012)			(0.012)	(0.012)
Observations	67,016									

Cluster robust standard errors in parentheses

** p<0.01, * p<0.05

Table 2.9: Labor supply, equation (3)

Variable	Full time work									
	1	2	3	4	5	6	7	8	9	10
SCHIP Threshold	-0.014	-0.014	-0.016	-0.022	-0.017	-0.014	-0.018	-0.022	-0.017	-0.020
	(0.011)	(0.010)	(0.011)	(0.012)	(0.011)	(0.010)	(0.011)	(0.012)	(0.010)	(0.011)
Parental Threshold		0.014							0.015	0.017
		(0.010)							(0.009)	(0.009)
Waiting Period (months)			0.001				0.001		0.001	
			(0.002)				(0.002)		(0.002)	
Waiting Period (dummy)				0.021				0.020		0.024
				(0.014)				(0.016)		(0.015)
Premium					0.012		0.011	0.004	0.013	0.005
					(0.010)		(0.010)	(0.011)	(0.011)	(0.011)
Cost sharing						-0.002			-0.005	-0.009
						(0.010)			(0.011)	(0.011)
Observations	67,016									

Cluster robust standard errors in parentheses
** p<0.01, * p<0.05

Table 2.10: Labor supply, equation (3)

Variable	Usual weekly work hours									
	1	2	3	4	5	6	7	8	9	10
SCHIP Threshold	0.276	0.287	0.189	0.082	0.226	0.236	0.159	0.082	0.142	0.084
	(0.182)	(0.178)	(0.222)	(0.234)	(0.205)	(0.209)	(0.233)	(0.240)	(0.243)	(0.248)
Parental Threshold		0.340*							0.390*	0.422*
		(0.155)							(0.147)	(0.164)
Waiting Period (months)			0.054				0.049		0.056	
			(0.038)				(0.036)		(0.037)	
Waiting Period (dummy)				0.583*				0.583**		0.637**
				(0.224)				(0.214)		(0.211)
Premium					0.258		0.198	-0.001	0.169	-0.013
					(0.284)		(0.267)	(0.251)	(0.261)	(0.250)
Cost sharing						0.138			0.083	-0.013
						(0.214)			(0.182)	(0.178)
Observations	54,513									

Cluster robust standard errors in parentheses

** p<0.01, * p<0.05

Table 2.11: Labor supply, equation (3)

Variable	*Weeks worked*									
	1	2	3	4	5	6	7	8	9	10
SCHIP Threshold	-0.438	-0.452*	-0.557*	-0.606*	-0.567*	-0.453	-0.647*	-0.653*	-0.605*	-0.607*
	(0.223)	(0.222)	(0.249)	(0.269)	(0.247)	(0.280)	(0.265)	(0.275)	(0.292)	(0.294)
Parental Threshold		-0.413							-0.380	-0.378
		(0.376)							(0.388)	(0.394)
Waiting Period (months)			0.074				0.058		0.053	
			(0.044)				(0.044)		(0.045)	
Waiting Period (dummy)				0.507				0.351		0.343
				(0.272)				(0.299)		(0.331)
Premium					0.662*		0.591	0.506	0.661	0.589
					(0.317)		(0.308)	(0.339)	(0.348)	(0.370)
Cost sharing						0.053			-0.206	-0.252
						(0.336)			(0.338)	(0.343)
Observations	54,513									

Cluster robust standard errors in parentheses
** p<0.01, * p<0.05

Table 2.12: Labor supply, equation (1)

Variable	1	2	3	4	5	6	7	8	9	10
					Labor force participation					
Share public coverage	-0.896*	-0.824*	-.602	-.579*	-.829*	-.922*	-.630*	-.619*	-.663*	-.654*
	(0.414)	(0.390)	(0.321)	(0.294)	(0.352)	(0.419)	(0.292)	(0.282)	(0.284)	(0.269)
					Full time work					
Share public coverage	-0.521	-0.464	-0.502	-0.680*	-0.545*	-0.522	-0.524*	-0.654*	-0.506*	-0.643*
	(0.323)	(0.314)	(0.278)	(0.289)	(0.274)	(0.323)	(0.255)	(0.270)	(0.248)	(0.258)
					Usual weekly work hours					
Share public coverage	20.038	22.718	-3.513	-11.182	2.470	18.034	-4.888	-10.873	-1.656	-7.569
	(23.113)	(18.875)	(7.960)	(8.836)	(12.064)	(21.084)	(8.080)	(8.843)	(7.567)	(7.739)
					Weeks worked					
Share public coverage	-31.834	-32.463	-20.345	-19.942*	-32.788	-31.385	-23.086*	-21.279*	-24.417*	-22.594*
	(32.573)	(24.891)	(12.676)	(8.645)	(18.480)	(30.527)	(11.679)	(8.788)	(12.058)	(9.305)
SCHIP Threshold	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Parental Threshold	X	Y	x	x	x	x	x	x	Y	Y
Waiting Period (months)	X	x	Y	x	x	x	Y	x	Y	x
Waiting Period (dummy)	X	x	x	Y	x	x	x	Y	x	Y
Premium	X	x	x	x	Y	x	Y	Y	Y	Y
Cost sharing	X	x	x	x	x	Y	x	x	Y	Y
Observations						67,016				

Cluster robust standard errors in parentheses

** p<0.01, * p<0.05

APPENDIX B

Additional Tables

Table 2.13: Health insurance coverage (maternal sample), equation (2), 1996-2002 sample years only

Variable	Public	Private	Any
		Share of children	
SCHIP threshold	0.069**	-0.027	0.042*
	(0.024)	(0.031)	(0.016)
	[2.875]	*[0.871]*	*[2.625]*
		All children	
SCHIP threshold	0.062*	-0.030	0.035
	(0.024)	(0.032)	(0.017)
	[2.583]	*[0.938]*	*[2.059]*
Observations		31,082	

Cluster robust standard errors in parentheses, absolute value of t-statistic in brackets
** p<0.01, * p<0.05

Table 2.14: Group and non-group private coverage (maternal sample), 1996-2002 sample years only

Variable	Group Private	Non-Group Private
	Share of children	
SCHIP threshold	-0.070**	0.043**
	(0.024)	(0.015)
	[2.917]	*[2.867]*
	All children	
SCHIP threshold	-0.077**	0.036*
	(0.026)	(0.015)
	[2.962]	*[2.400]*
Observations		31,082

Cluster robust standard errors in parentheses, absolute value of t-statistic in brackets
** p<0.01, * p<0.05

Table 2.15: Labor supply, equation (3), 1996-2002 sample years only

Policy variable	LFP	FT	Hours~	Weeks~
SCHIP threshold	-0.040*	-0.053**	-0.178	-1.461
	(0.018)	(0.018)	(0.371)	(0.770)
	[2.222]	*[2.944]*	*[0.480]*	*[1.897]*
Observations		31,082		25,434

Cluster robust standard errors in parentheses, absolute value of t-statistic in brackets
** p<0.01, * p<0.05
~Estimates conditional on positive values

Table 2.16: Labor supply 2SLS only, equation (1), 1996-2002 sample years only

Variable	LFP	FT	Hours~	Weeks~
		Share of children		
Public coverage	-0.583**	-0.770**	-3.030	-24.950**
	(0.212)	(0.218)	(6.048)	(9.011)
		All children		
Public coverage	-0.656**	-0.866**	-3.379	-27.820**
	(0.233)	(0.266)	(6.734)	(10.250)
Observations		31,082		25,434

Cluster robust standard errors in parentheses
** p<0.01, * p<0.05
~Estimates conditional on positive values

Table 2.17: Health insurance coverage (maternal sample), equation (2), no state-year interactions

Variable	Public	Private	Any
	Share of children		
SCHIP threshold	0.028*	-0.008	0.019**
	(0.013)	(0.014)	(0.007)
	[2.154]	*[0.571]*	*[2.714]*
Mean value	*0.356*	*0.504*	*0.860*
	All children		
SCHIP threshold	0.026*	-0.011	0.016*
	(0.012)	(0.014)	(0.007)
	[2.167]	*[0.786]*	*[2.286]*
Mean value	*0.340*	*0.488*	*0.845*
Observations		67,016	

Cluster robust standard errors in parentheses, absolute value of t-statistic in brackets
** p<0.01, * p<0.05

Table 2.18: Group and non-group private coverage (maternal sample), no state-year interactions

Variable	Group Private	Non-Group Private
	Share of children	
SCHIP threshold	-0.032**	0.024*
	(0.011)	(0.009)
	[2.909]	*[2.667]*
Mean value	*0.417*	*0.086*
	All children	
SCHIP threshold	-0.035**	0.018
	(0.011)	(0.009)
	[3.181]	*[2.000]*
Mean value	*0.397*	*0.075*
Observations	67,016	

Cluster robust standard errors in parentheses, absolute value of t-statistic in brackets
** p<0.01, * p<0.05

Table 2.19: Labor supply, equation (3), no state-year interactions

Policy variable	LFP	FT	Hours~	Weeks~
SCHIP threshold	-0.025	-0.014	0.276	-0.438
	(0.013)	(0.011)	(0.182)	(0.223)
	[1.923]	*[1.273]*	*[1.516]*	*[1.964]*
Mean value	*0.806*	*0.650*	*38.050*	*45.356*
Observations		67,016	54,513	

Cluster robust standard errors in parentheses, absolute value of t-statistic in brackets
** p<0.01, * p<0.05
~Estimates conditional on positive values

Table 2.20: Labor supply 2SLS only, equation (1), no state-year interactions

Variable	LFP	FT	Hours~	Weeks~
		Share of children		
Public coverage	-0.896*	-0.521	20.040	-31.830
	(0.414)	(0.323)	(23.110)	(32.570)
		All children		
Public coverage	-0.953*	-0.555	23.80	-37.810
	(0.429)	(0.332)	(30.020)	(43.020)
Observations		67,016	54,513	

Cluster robust standard errors in parentheses
** p<0.01, * p<0.05
~Estimates conditional on positive values

Table 2.21: Health insurance coverage (maternal sample), equation (2), results with overlap category

Variable	Public	Private	Overlap	Any
		Share of children		
SCHIP threshold	0.080*	-0.044	0.015*	0.051**
	(0.031)	(0.033)	(0.006)	(0.015)
	[2.580]	*[1.333]*	*[2.500]*	*[3.400]*
		All children		
SCHIP threshold	0.070*	-0.050	0.022**	0.040*
	(0.030)	(0.034)	(0.008)	(0.016)
	[2.333]	*[1.471]*	*[2.750]*	*[2.500]*
Observations			67,016	

Cluster robust standard errors in parentheses, absolute value of t-statistic in brackets
** p<0.01, * p<0.05

Table 2.22: Health insurance coverage (maternal sample), equation (2), no weights

Variable	Public	Private	Any
		Share of children	
SCHIP threshold	0.083*	-0.017	0.065**
	(0.033)	(0.034)	(0.012)
	[2.515]	*[0.500]*	*[5.417]*
		All children	
SCHIP threshold	0.073*	-0.026	0.054**
	(0.032)	(0.036)	(0.013)
	[2.281]	*[0.722]*	*[4.154]*
Observations		73,855	

Cluster robust standard errors in parentheses, absolute value of t-statistic in brackets
** p<0.01, * p<0.05

Table 2.23: Group and non-group private coverage (maternal sample), no weights

Variable	Group Private	Non-Group Private
	Share of children	
SCHIP threshold	-0.070*	0.053**
	(0.032)	(0.011)
	[2.188]	*[4.818]*
	All children	
SCHIP threshold	-0.087*	0.036**
	(0.036)	(0.013)
	[2.417]	*[2.769]*
Observations	73,855	

Cluster robust standard errors in parentheses, absolute value of t-statistic in brackets
** p<0.01, * p<0.05

Table 2.24: Labor supply, equation (3), no weights

Policy variable	LFP	FT	Hours~	Weeks~
SCHIP threshold	-0.017	-0.001	0.629	-0.753
	(0.020)	(0.028)	(0.582)	(0.850)
	[0.085]	*[0.036]*	*[1.081]*	*[0.886]*
Observations		73,855	60,032	

Cluster robust standard errors in parentheses, absolute value of t-statistic in brackets
** p<0.01, * p<0.05
~Estimates conditional on positive values

Table 2.25: Labor supply 2SLS only, equation (1), no weights

Variable	LFP	FT	Hours~	Weeks~
		Share of children		
Public	-0.200	-0.018	10.380	-12.410
coverage	(0.204)	(0.325)	(13.410)	(9.405)
		All children		
Public	-0.227	-0.020	12.250	-14.650
coverage	(0.227)	(0.367)	(16.320)	(11.040)
Observations				

Cluster robust standard errors in parentheses

** p<0.01, * p<0.05

~Estimates conditional on positive values

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CHAPTER 3
HEALTH INSURANCE COVERAGE DURING THE GREAT RECESSION

I. Introduction

The Great Recession is an extensive period of reduced economic activity in the United States, beginning in December 2007 and continuing until June 2009 (The National Bureau of Economic Research, 2010). The recession led to a large increase in the unemployment rate, peaking at 10% in October 2009 (United States Department of Labor, 2014). To date, unemployment is still well above pre-recession levels. The economic downturn was also associated with a large decrease in health insurance coverage among working age adults. Approximately 20.3% of 19 to 64 year old adults were without insurance in 2009, an increase of 2.6 percentage points from 2007.[30]

The recession worsened an existing insurance coverage decline among adults that began almost a decade earlier. Researchers attribute this decline to a large decrease in private coverage (Fronstin, 2013; Holahan and Chen, 2011; Vistnes, Zawacki, Simon, and Taylor, 2010). Existing research of the Great Recession, however, does not establish the extent to which the fall in coverage between 2007 and 2009 is the result of the recession itself or other factors, such as the existing secular decline in private coverage. In this paper, I investigate the degree to which business cycle variation accounts for the insurance coverage decline among adults during the Great Recession. I accomplish this by using state level employment information in a regression model of insurance coverage to predict insurance trends from the Great Recession. A deviation between predicted and actual insurance trends is evidence that insurance coverage either changed in a way that is not fully implied by the economic downturn, or that its relationship with the

[30] Based on March Current Population Survey data collected for this analysis. Adult insurance coverage trends are shown in Table 1.2.

economy is different than in previous downturns.[31] In a separate regression model, I directly test whether the Great Recession had a differential effect on insurance coverage relative to past recessions.

Results show evidence that the proportion of adults without health insurance during the Great Recession is greater than the level predicted by regression models using state level employment information. This finding is driven by a deviation between actual and predicted insurance coverage trends in the years leading up to the Great Recession, suggesting that factors other than the economic decline were important determinants of adult insurance coverage during the recession. These results, however, are sensitive to the choice of state level employment and time trend variables.

Empirical methods and data are discussed in section II. Results of the empirical analysis are presented in section III. Section IV concludes.

II. Methods

II.A. Data

This analysis uses data from the IPUMS CPS, a version of the March Current Population Survey (CPS) managed by the University of Minnesota. The extract drawn from this data set spans survey years 1988 to 2013, and includes information on working age (19-64 years) adults. Elderly adults are not included in this extract, since individuals in this group are likely to be Medicare eligible. Insurance coverage variables are defined to be mutually exclusive; summing

[31] In their paper, Cawley et al. (2011) seek to estimate the relationship between insurance coverage and the economy during the Great Recession. They do not establish whether the proportion of adults without insurance coverage by the end of the recession is due entirely to the economic downturn or some other factor.

across both private and public coverage equals the overall insurance category.[32] Because the March CPS collects coverage information from the year preceding the survey, these variables measure individual insurance coverage data from 1987 to 2012. Demographic variables such as age are lowered by one year to reflect the age concurrent with insurance information. Finally, I use state employment information from both the Bureau of Labor Statistics Local Area Unemployment Statistics (BLS LAUS) program and the IPUMS March CPS. BLS LAUS data reflects employment information from the civilian non-institutionalized population at least 16 years in age (Bureau of Labor Statistics, 2014). I include March CPS employment information in order to create a set of aggregate state employment variables based on the 19 to 64 year old working age population.[33]

Table 3.1 shows means of key variables used in this analysis. At the top of this table are state employment variables from the BLS LAUS and the working age (19 to 64 year old) adult extract from the March CPS. For the BLS LAUS data, the mean state unemployment rate is 5.9% and state employment to population (EP) ratio is 62.7%. The mean state unemployment rate among 19 to 64 year olds is approximately 5.8%, slightly lower than the BLS LAUS measure. The average state EP ratio for 19 to 64 year olds also differs from the BLS LAUS, indicating an employment rate of 74.3%. Table 3.1 also indicates health insurance coverage means. Among the adults in the sample, 73.6%, have private insurance coverage, 17.3% have no insurance coverage, and only 9.1% of have public insurance. A minority of the sample, 35.7%, have a two year college degree or above. Finally, 62.4% of working age adults indicate being currently married, and 52.2% are parents.

[32] Some individuals report both public and private sources of coverage during a year. For this analysis, these "overlap" individuals are coded as having private insurance coverage.
[33] Because labor force questions in the March CPS are based on current, rather than previous year, employment status, results that use age 19-64 state employment variables correspond to years 1988-2012.

Table 3.1: Means of key variables (1987-2012)

Variable	*Mean*
Unemployment rate (BLS LAUS)	5.940
EP ratio (BLS LAUS)	62.716
Unemployment rate (19-64 year olds)^	5.765
EP ratio (19-64 year olds)^	74.303
Private HI	0.736
Public HI	0.091
Uninsured	0.173
Family size	3.066
Female	0.520
Age	39.769
White	0.823
Black	0.106
Parent	0.522
Less than HS education	0.186
HS education	0.276
Some college education	0.182
2 year college degree	0.091
4 year college degree	0.179
Graduate degree	0.087
Married	0.624
Full-time employment	0.672
Self-employed	0.082
Private sector work	0.593
Public sector work	0.130
Observations	2,684,632

^ 1988-2012 data

Table 3.2 presents insurance coverage trends for working age adults during the Great Recession. The proportion of uninsured adults grew 2.6 percentage points between 2007 and 2009, a 14.7% increase relative to the 2007 mean. The loss in overall insurance coverage is driven by a 4.2 percentage point decline in private coverage over this time period. Private coverage losses are largest among adults with low educational attainment or who are not currently married. Gains in public coverage offset only a fraction of the decline in private coverage, with an overall increase of 1.6 percentage points. The largest gains occur among adults who are young and have low educational attainment. Consequently, declines in private coverage for adults translate to large increases in uninsured status. These results are similar to insurance coverage trends reported elsewhere in the health insurance literature (Fronstin, 2013; Holahan and Chen, 2011).

Table 3.2: Insurance coverage trends during the Great Recession, 19-64 adults

Group	2007 % of sample	2007 mean	2007 - 2009 change
		Uninsured	
Overall	1.000	0.177	0.026
Education			
Less than HS	0.115	0.406	0.027
HS/some college	0.496	0.197	0.034
2-4 year degree	0.292	0.094	0.018
Graduate degree	0.097	0.050	0.007
Age			
19-26	0.163	0.293	0.020
27-54	0.677	0.164	0.030
55-64	0.160	0.112	0.015
Marital status			
Married	0.614	0.122	0.017
Not married	0.386	0.264	0.034
		Private coverage	
Overall	1.000	0.727	-0.042
Education			
Less than HS	0.115	0.374	-0.048
HS/some college	0.496	0.694	-0.055
2-4 year degree	0.292	0.857	-0.029
Graduate degree	0.097	0.922	-0.013
Age			
19-26	0.163	0.598	-0.042
27-54	0.677	0.755	-0.045
55-64	0.160	0.741	-0.028
Marital status			
Married	0.614	0.810	-0.030
Not married	0.386	0.595	-0.050
		Public coverage	
Overall	1.000	0.096	0.016
Education			
Less than HS	0.115	0.220	0.021
HS/some college	0.496	0.109	0.021

Table 3.2 (cont'd)

2-4 year degree	0.292	0.048	0.010
Graduate degree	0.097	0.029	0.005
Age			
19-26	0.163	0.109	0.022
27-54	0.677	0.081	0.015
55-64	0.160	0.146	0.013
Marital status			
Married	0.614	0.068	0.014
Not married	0.386	0.141	0.016

II.B. Empirical methodology

The insurance coverage model is specified as follows (where *i* denotes an individual observation, *s* state, and *t* year):

$$Y_{ist} = \alpha_s + \delta_1 t + \delta_2 t^2 + X_{ist}\beta + \mathrm{Employment}_{st}\gamma + \varepsilon_{ist} \quad (1)$$

Y_{ist} is an indicator variable for one of three insurance coverage categories: uninsured status, public coverage, and private coverage. $Employment_{st}$ is a vector of employment variables meant to capture the effect of business cycle variation on insurance coverage. These variables reflect information on either the annual unemployment rate or EP ratio in state *s* during year *t*.[34] This approach follows methods from other researchers who estimate the effect of business cycle variation on insurance coverage (Cawley et al., 2011; Glied and Jack, 2003). These authors use only contemporaneous state unemployment rates. Equation (1) allows additional flexibility in modeling the effect of business cycle variation on insurance coverage, including a quadratic employment variable and employment lags.

Equation (1) includes right hand side variables in addition to the employment information. X_{ist} is a vector of demographic controls including information on parent status

[34] The unemployment rate does not account for discouraged workers who leave the labor force, perhaps as a result of long term unemployment. In case the unemployment rate understates the true effect of the recession on employment, I include state employment to population ratios in my analysis.

(whether the individual has a child living in the household), gender, age, family size, race, education, and marital status. Also included are state fixed effect indicators, α_s, and linear and quadratic time trends, t and t^2. In case the model is sensitive to the specification of the time trend, I present results from models with only a linear time trend or with a full set of state specific linear time trends. Finally, standard errors are clustered at the state level.

I also directly test whether the Great Recession has a differential effect on insurance coverage, relative to other years. I use the following equation:

$$Y_{ist} = \alpha_s + \delta_1 t + \delta_2 t^2 + X_{ist}\beta + \gamma_1 Employment_{st} + \lambda_1 GreatRecession_t + \lambda_2 Employment_{st} * GreatRecession_t + \varepsilon_{ist} \qquad (2)$$

Here, γ_1 captures the effect of the contemporaneous employment information in years outside of the Great Recession period of 2007 to 2009. $GreatRecession_t$ is an indicator variable equal to one for observations belonging to years 2007, 2008, or 2009. λ_2 is the main coefficient of interest, indicating whether the effect of the employment variable during the recent recession is consistent with other time periods. If the relationship between the economy and insurance coverage during the Great Recession is similar to past downturns, then λ_2 should not be statistically significant. The remaining variables in equation (2) are defined as in equation (1).

III. Results

In order to determine whether observed insurance trends during the Great Recession are consistent with past recessions, I estimate equation (1) using data in the years before 2007.[35] I

[35] Employment variable coefficients from equation (1) are shown in Table 3.8.

Figure 3.1: Proportion uninsured, unemployment rate (BLS LAUS)

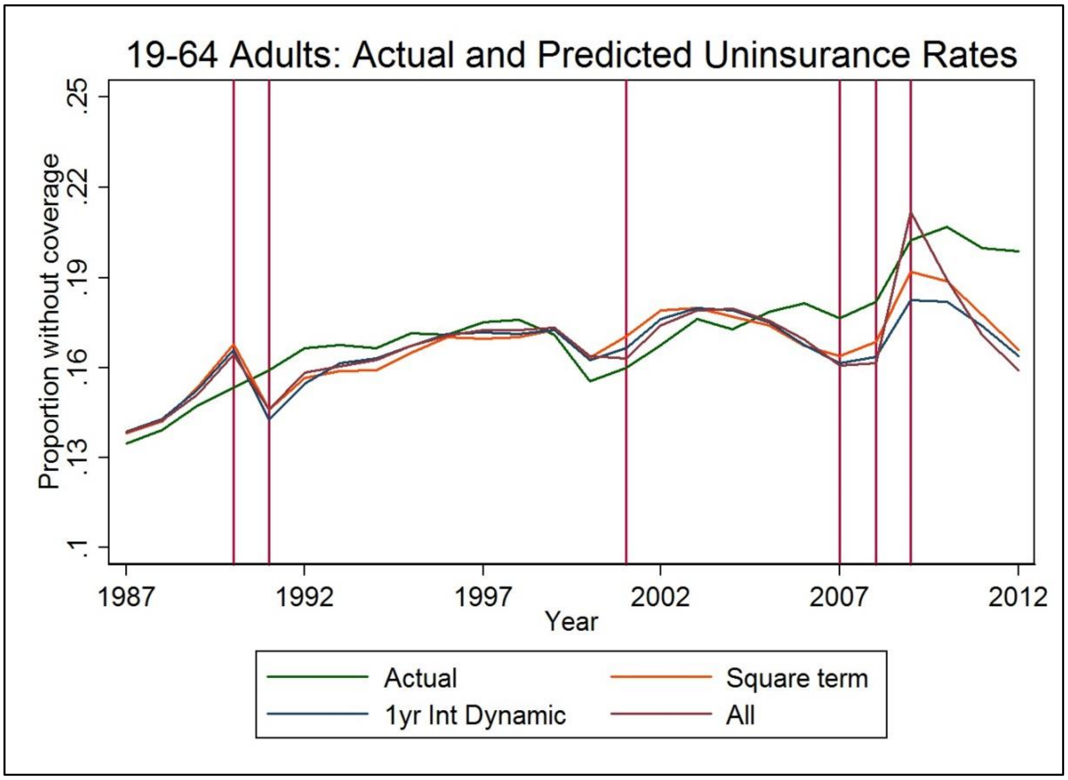
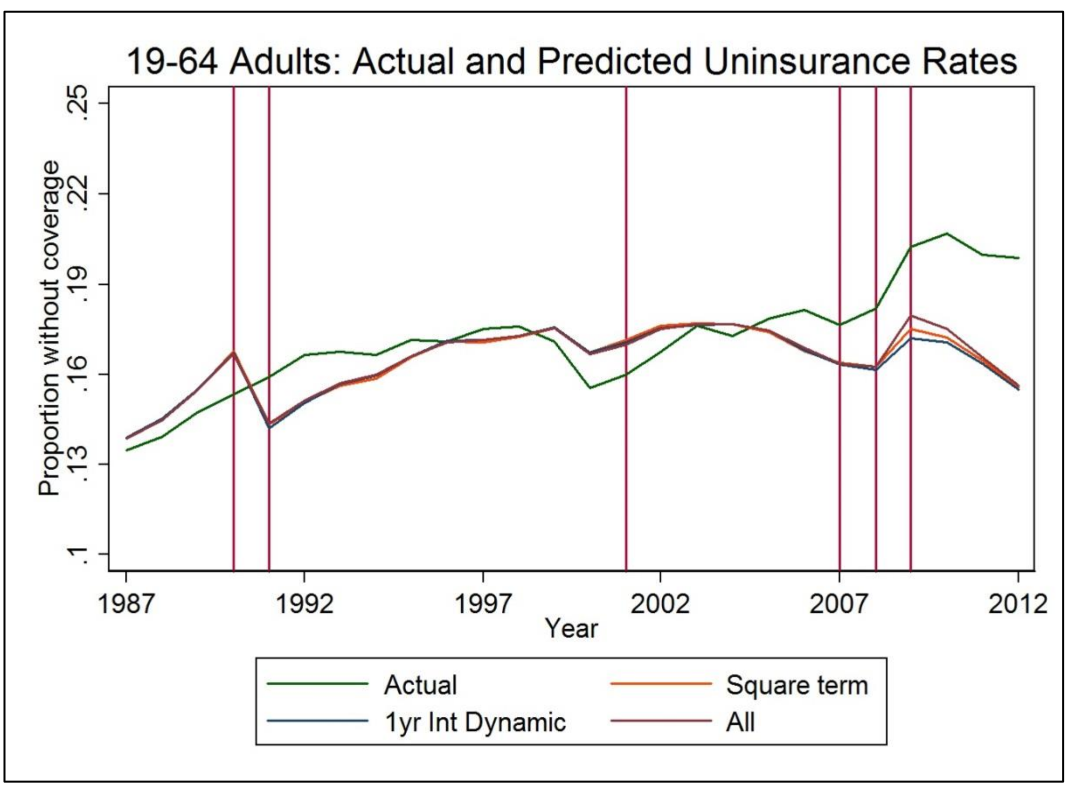


Figure 3.2: Proportion uninsured, EP ratio (BLS LAUS)



use the resulting estimates to create out of sample health insurance predictions from 2007 to 2012. Figures 3.1 and 3.2, which use BLS LAUS information on state unemployment rates and EP ratios, respectively, display trends in the actual and predicted uninsured rates among working age adults. Predicted trends in Figure 3.1 are sensitive to the specification of the employment variable.[36] The specification including both dynamic and quadratic terms, denoted "All", predicts a substantial increase in the proportion of uninsured adults during the Great Recession. This increase is large relative to the actual trend and remaining predicted trends, although the "Square term" trend also over-predicts the loss in insurance coverage. Results using state EP ratios, indicated in Figure 3.2, are more consistent across specifications. Predictions based on EP ratio information appear to accurately predict the magnitude of the increase in uninsured status during the Great Recession.

Both Figures 3.1 and 3.2 show a predicted proportion of uninsured adults that is less than the actual amount during the recession. This suggests that the regression model, which generally captures the change in insurance coverage during the Great Recession, under-predicts the level of adults without coverage. This result is due to a divergence between actual and predicted trends in the period before 2007. Throughout the decade, the actual uninsured rate among working age adults is increasing. Meanwhile, the predicted uninsured rate reverses its upward trend in the middle of the decade, in approximately 2004 or 2005. One possible reason for this result is the secular decline in private insurance coverage. Figures 3.3 and 3.4 indicate that private coverage is decreasing throughout the decade, despite the model predicting a large increase in private coverage in the years immediately prior to the Great Recession. This is unlike the previous

[36] Figures 1 to 4 contain 3 different predicted trend lines, each representing a different specification of the employment variable in equation (1). "Square term" uses both a contemporaneous and quadratic employment variable. "1yr Int Dynamic" includes a contemporaneous term, 1 year lag, and an interaction of both variables. Finally, "All" includes employment variables from the "1yr Int Dynamic" specification as well as a quadratic.

Figure 3.3: Private coverage, unemployment rate (BLS LAUS)

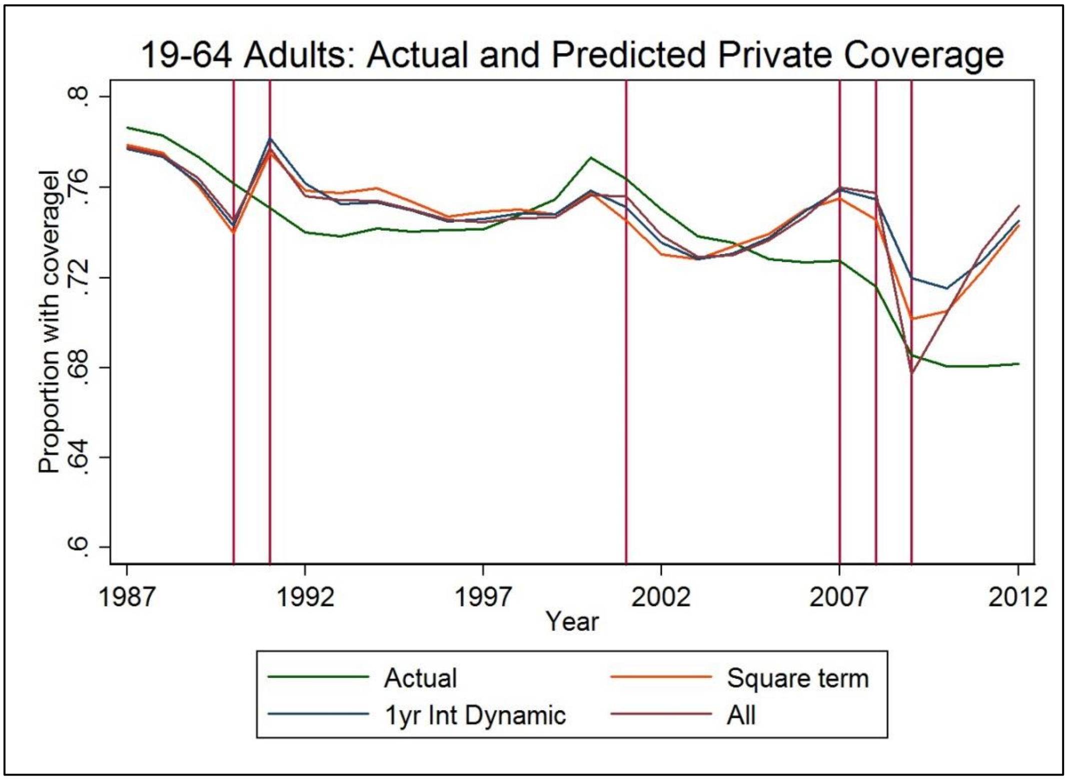


Figure 3.4: Private coverage, EP ratio (BLS LAUS)

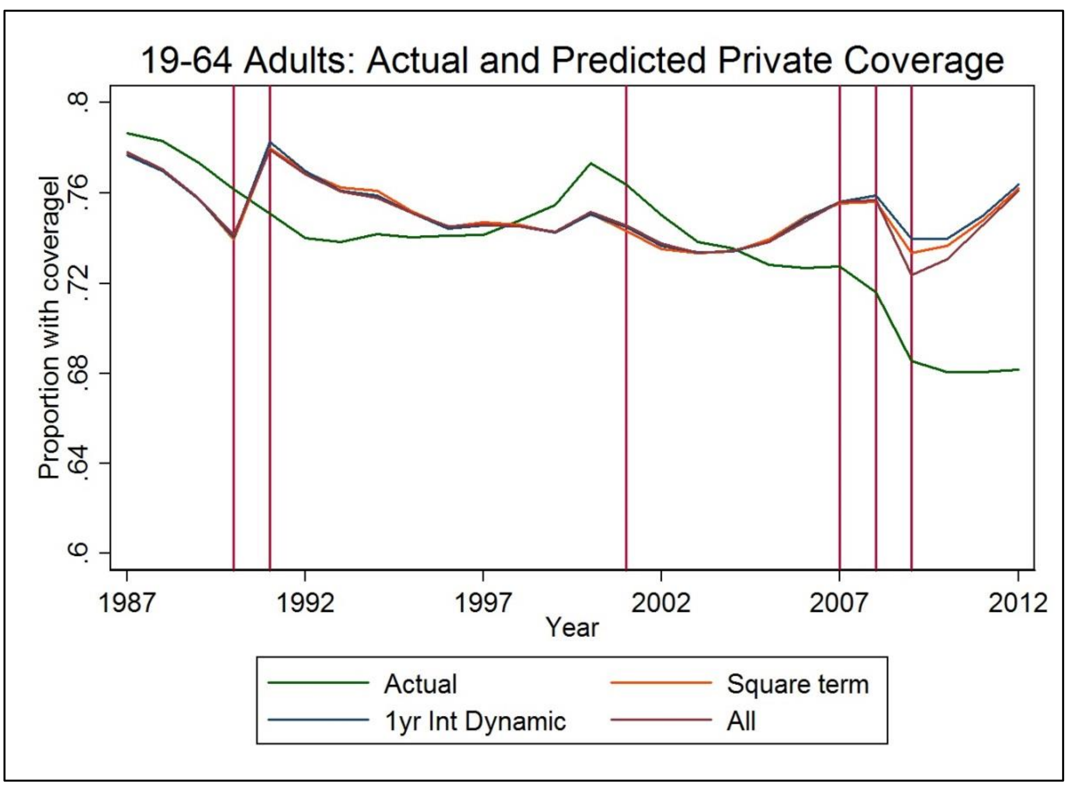


Figure 3.5: 1988-2012 Proportion uninsured, unemployment rate (19-64 year old employment variable)

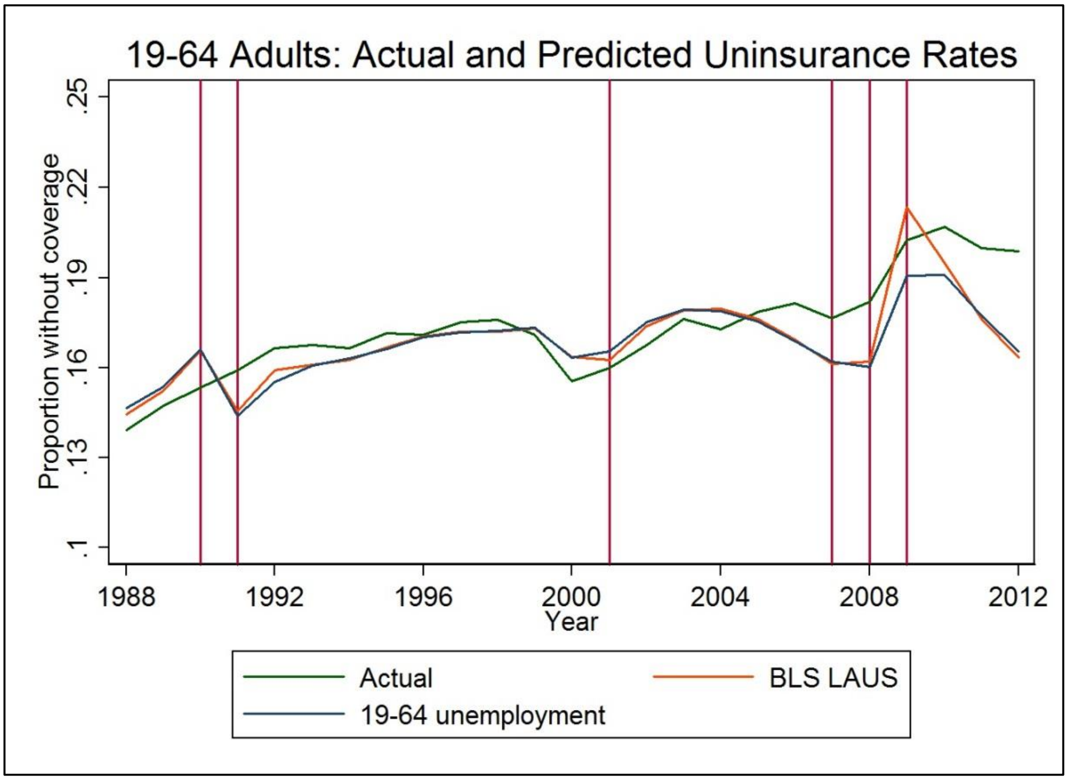
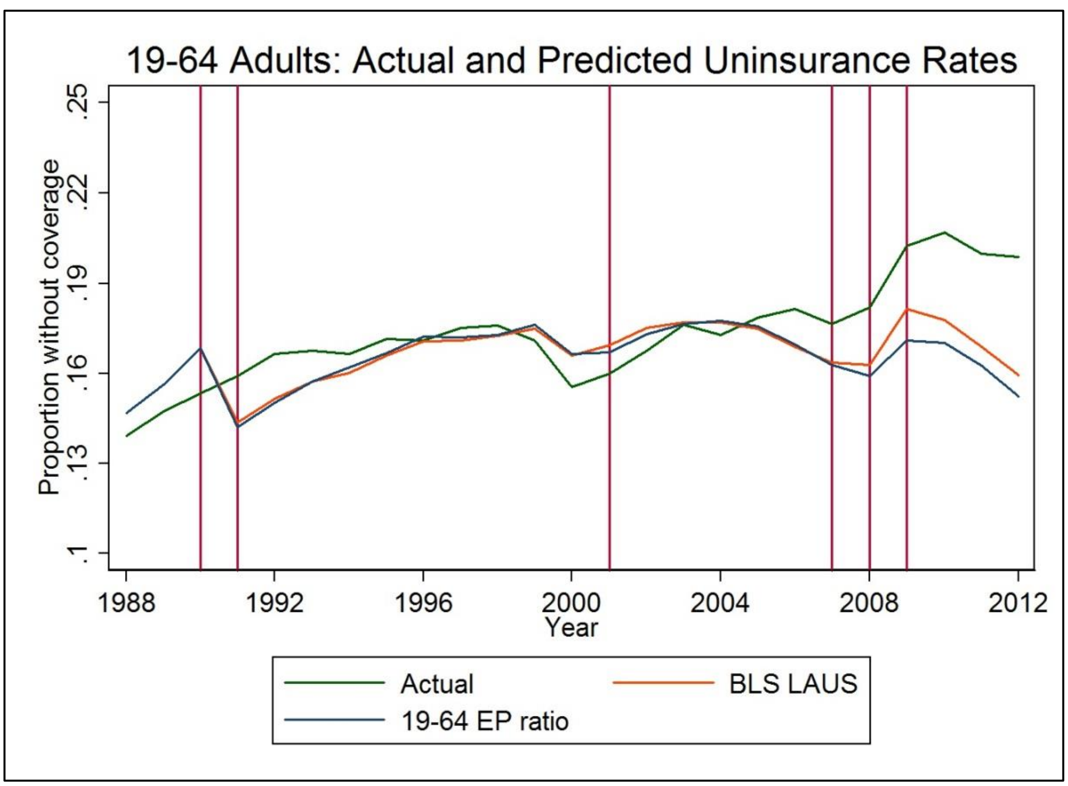


Figure 3.6: 1988-2012 Proportion uninsured, EP ratio (19-64 year old employment variable)



period of economic recovery during the middle and late 1990s, where private coverage shows a strong upward trend. Instead, private coverage during the middle 2000s declines despite the improvement in the economy.

Figures 3.5 and 3.6, which display results from equation (1) using 19 to 64 year old employment data, show only small differences with the BLS LAUS estimates. Predicted trends in these figures are based on the "All" specification of the employment variable. In Figure 3.5, the predicted increase in uninsured status during the Great Recession is smaller when using 19-64 year old state unemployment data than with the BLS LAUS information. This increase is now more in line with the actual uninsured trend. Figure 3.6 displays a similar result, with a slightly smaller predicted increase in the uninsured rate when using the 19 to 64 year old state EP ratio.

Figures 3.7 and 3.8 show results from equation (1) with varying specifications of the time trend variable.[37] Results from this exercise indicate that predictions from the main analysis are sensitive to the type of time trend included in the model. Results from the main analysis use both a linear and quadratic time trend. The two additional time trend specifications in Figures 3.7 and 3.8 are a linear and state specific linear time trend. In both figures, models using the linear or state specific trends produce similar predictions of the uninsured rate. However, these results show a predicted proportion of uninsured adults that is greater than the actual level during the Great Recession. This is the opposite of the finding from the main analysis. Unlike results from the main analysis, predicted rates of uninsurance in the years just before 2007 do not decline. Instead, the predicted rate from equation (1) using either of the alternative trends appears to increase throughout the decade. The sensitivity of the predictions to different time

[37] As with Figures 3.5 and 3.6, predicted trends in these figures are based on the "All" specification of the employment variable.

Figure 3.7: Proportion uninsured trend comparison, unemployment rate (BLS LAUS)

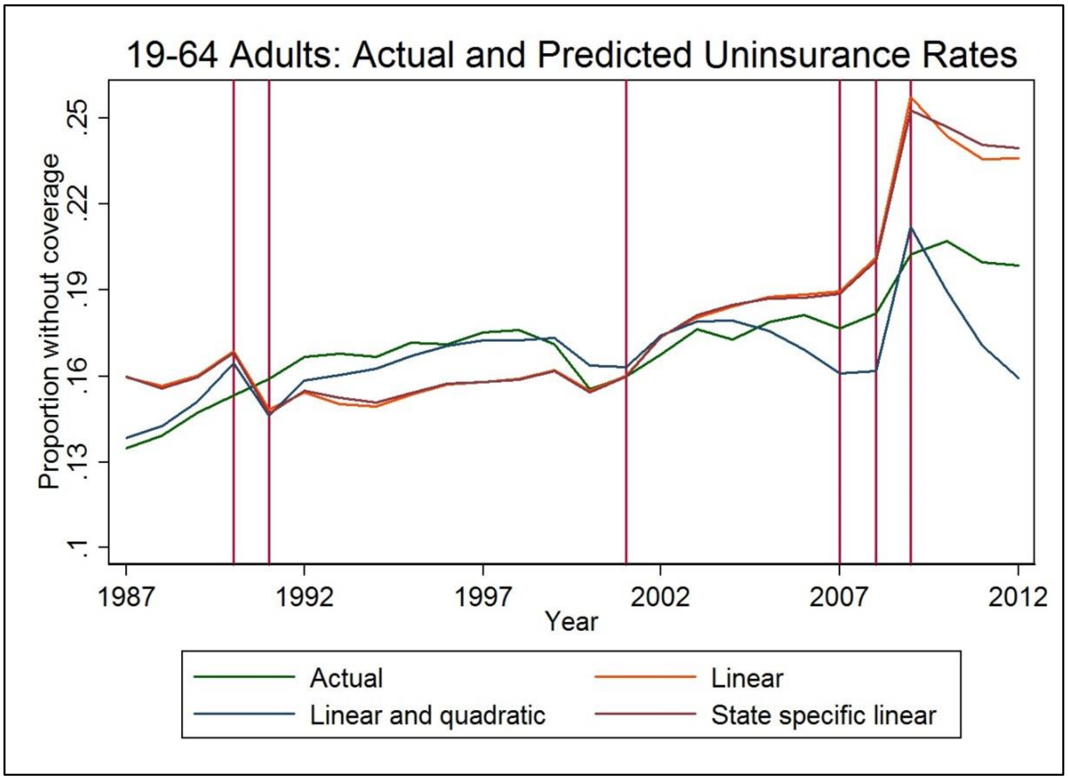
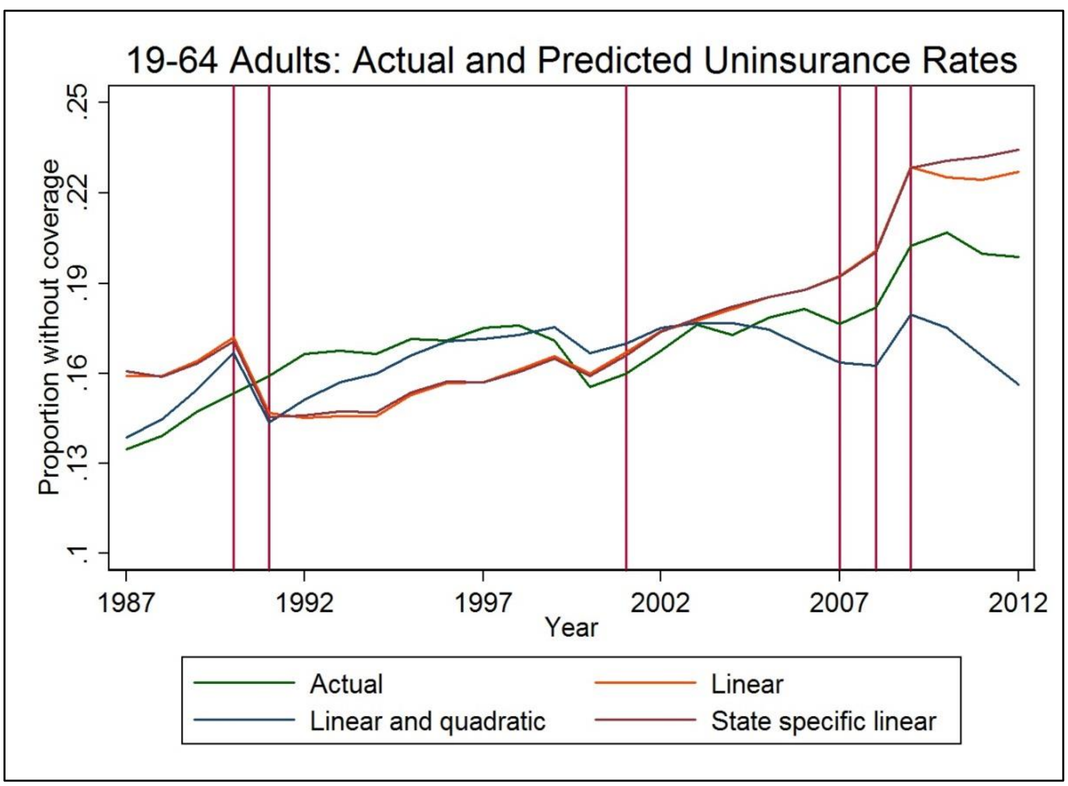


Figure 3.8: Proportion uninsured trend comparison, EP ratio (BLS LAUS)



trend specifications implies that the model fails to control for omitted variables which also affect insurance coverage.

Finally, Table 3.3 displays estimates from equation (2) using BLS LAUS employment data. This equation is intended to evaluate whether the relationship between the economy and insurance coverage is different during the Great Recession, relative to past recessions. As with the predicted insurance trends, results depend on the choice of employment variable. EP ratio results, presented in even numbered columns, show no evidence that the relationship between employment and insurance is different during the Great Recession. Results based on state unemployment information are shown in odd numbered columns. Uninsured status estimates, shown in column 1, indicate that the effect of the unemployment variable between 2007 and 2009 is smaller than in other years. This appears to match trends from Figure 3.1, where some of the specifications indicate that the actual increase in uninsured status during the Great Recession is smaller than predicted. Private and public coverage estimates show similar results. The coefficient on the interaction term in column 3, for instance, suggests that negative relationship between private coverage and unemployment is smaller in magnitude during the Great Recession. Results from Table 3.4, which uses 19 to 64 year old employment data, are similar.

IV. Conclusion

The Great Recession is associated with large declines in employment and insurance coverage. Between 2007 and 2009, working age adults experienced a 14.7% increase in uninsured status relative to the mean number of uninsured in 2007. In this paper, I evaluate the degree to which the increase in the number of uninsured working age adults is explained by the economic downturn. Using a regression model to estimate the relationship between employment

Table 3.3: Great Recession and insurance coverage (BLS LAUS employment variables)

Variable	*Uninsured*		*Private coverage*		*Public coverage*	
	1	2	3	4	5	6
07-09 indicator	0.0094*	0.0161	-0.0269***	-0.0243	0.0176***	0.0082
	(0.0048)	(0.0293)	(0.0069)	(0.0318)	(0.0035)	(0.0201)
Unemployment rate	0.0077***		-0.0135***		0.0058***	
	(0.0007)		(0.0009)		(0.0005)	
EP ratio		-0.0060***		0.0010***		-0.0040***
		(0.0008)		(0.0009)		(0.0005)
Interaction	-0.0016**	-0.0002	0.0040***	0.0003	-0.0024***	-0.0001
	(0.0006)	(0.0005)	(0.0008)	(0.0005)	(0.0005)	(0.0003)
R-squared	0.108	0.108	0.166	0.166	0.068	0.068
Observations			2,684,632			

Cluster robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 3.4: Great Recession and insurance coverage (1988-2012; 19-64 year old employment variables)

Variable	*Uninsured*		*Private coverage*		*Public coverage*	
	1	2	3	4	5	6
07-09 indicator	0.0108**	0.0143	-0.0264***	-0.0159	0.0156**	0.0016
	(0.0046)	(0.0257)	(0.0064)	(0.0327)	(0.0031)	(0.0237)
Unemployment rate	0.0070***		-0.0117***		0.0047***	
	(0.0006)		(0.0008)		(0.0004)	
EP ratio		-0.0050***		0.0086***		-0.0036***
		(0.0006)				(0.0004)
Interaction	-0.0013**	-0.0002	0.0033***	0.0001	-0.0019***	0.0000
	(0.0006)	(0.0004)	(0.0008)	(0.0004)	(0.0005)	(0.0003)
R-squared	0.110	0.109	0.168	0.168	0.069	0.069
Observations			2,593,396			

Cluster robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

and insurance coverage in previous recessions, I create out of sample predictions of insurance coverage between 2007 and 2012. I find evidence that the proportion of adults without insurance coverage during the recession is higher than implied by predictions from the regression model. This result is sensitive to the specification of the time trend variable, however. Estimates from regression models which include linear or state specific linear time trends suggest that level of uninsured adults during the Great Recession is lower than the predicted.

Overall, evidence from the main analysis indicates that at least some of the decline in insurance coverage during the Great Recession is due to factors other than the economic downturn. A possible explanation for this finding lies with the secular decline in private insurance coverage, which predates the Great Recession. In addition, I find little evidence for an alternative explanation of the main results discussed above. Predicted insurance trends from equation (1), as well as estimates from equation (2), suggest that the relationship between adult insurance coverage and the economy during the Great Recession is consistent with past downturns. This conclusion depends on the choice of employment variable. In both equations, state unemployment rate data results in an over-prediction of the increase in uninsured status. Unlike the EP ratio results, however, estimates based on unemployment rate data are sensitive to the specification of the employment variable.

Among adults, there are several factors contributing to the decline in private and overall insurance coverage levels. Vistnes et al. (2010) find that decreasing rates of employer coverage in the 2000s is the result of a decline in both coverage offers from employers and take-up among employees. The authors also find that these trends correspond with rising employee premiums. In addition, there were few policy changes during this time period to help preserve insurance coverage among working age adults. The State Children's Health Insurance Program (CHIP), implemented in all states by 2000, acted as an important health insurance safety net for children throughout the 2000s and in the Great Recession. For adults, however, CHIP is associated with only a limited expansion of parental public insurance eligibility in a small number of states. Subsequent health insurance reforms, such as the Affordable Care Act (ACA), should help preserve health insurance coverage among working age adults. The ACA expands adult eligibility for Medicaid and provides subsidized private insurance coverage. In the years to come, this should reduce the impact of declining employer insurance coverage and lessen the sensitivity of adult health insurance coverage to variations in the business cycle.

APPENDIX

Table 3.5: Great Recession, proportion of adults unemployed at least 1 week in year

Group	2007 % of sample	2007 mean	2007 - 2009 change
Overall	1.000	0.060	0.035
Education			
Less than HS	0.115	0.088	0.043
HS/some college	0.496	0.069	0.039
2-4 year degree	0.292	0.046	0.030
Graduate degree	0.097	0.030	0.019
Age			
19-26	0.163	0.105	0.039
27-54	0.677	0.056	0.037
55-64	0.160	0.033	0.020
Marital status			
Married	0.614	0.044	0.033
Not married	0.386	0.086	0.035

Table 3.6: Trends in main employment variables over past decade

Variable	2002	2007	2012
Unemployment rate (BLS LAUS)	5.567	4.477	7.728
Unemployment rate (19-64 year olds)	5.437	4.122	7.807
EP ratio (BLS LAUS)	63.589	63.915	59.633
EP ratio (19-64 year olds)	75.617	75.930	71.319

Table 3.7: Great Recession, parental insurance coverage trends

Group	2007 % of sample	2007 mean	2007 - 2009 change
		Uninsured	
Overall	1.000	0.150	0.016
Education			
Less than HS	0.118	0.402	0.017
HS/some college	0.467	0.166	0.025
2-4 year degree	0.310	0.069	0.010
Graduate degree	0.106	0.036	0.002
Age			
19-26	0.081	0.274	0.004
27-54	0.838	0.140	0.017
55-64	0.081	0.130	0.022
Marital status			
Married	0.808	0.126	0.013
Not married	0.192	0.250	0.022
		Private coverage	
Overall	1.000	0.761	-0.035
Education			
Less than HS	0.118	0.401	-0.053
HS/some college	0.467	0.729	-0.050
2-4 year degree	0.310	0.886	-0.018
Graduate degree	0.106	0.942	-0.008
Age			
19-26	0.081	0.524	-0.063
27-54	0.838	0.786	-0.034
55-64	0.081	0.739	-0.029
Marital status			
Married	0.808	0.813	-0.030
Not married	0.192	0.545	-0.044
		Public coverage	
Overall	1.000	0.089	0.019
Education			
Less than HS	0.118	0.197	0.036
HS/some college	0.467	0.105	0.026
2-4 year degree	0.310	0.045	0.008
Graduate degree	0.106	0.022	0.006
Age			
19-26	0.081	0.202	0.059
27-54	0.838	0.074	0.016
55-64	0.081	0.131	0.007
Marital status			
Married	0.808	0.061	0.017

Table 3.7 (cont'd)

Not married	0.192	0.205	0.021

Table 3.8: 19-64 Adults results from equation (1) used for prediction, uninsured (1987-2006)

Variable	Uninsured				
	1	2	3	4	5
	Employment variable: unemployment rate (BLS LAUS)				
Current year	0.0073***	0.0112***	0.0062***	0.0083***	0.0044
	(0.0006)	(0.0036)	(0.0008)	(0.0018)	(0.0035)
Current year, squared		-0.0003			0.0004*
		(0.0002)			(0.0002)
1 year lag			0.0011	0.0061***	0.0094***
			(0.0009)	(0.0017)	(0.0013)
2 year lag			0.0012		
			(0.0008)		
Interaction (current and 1 year lag)				-0.0005*	-0.0009***
				(0.0003)	(0.0002)
R-squared	0.108	0.108	0.108	0.108	0.108
	Employment variable: EP ratio (BLS LAUS)				
Current year	-0.0061***	-0.0114	-0.0054***	-0.0071	-0.0364***
	(0.0007)	(0.0101)	(0.0008)	(0.0052)	(0.0106)
Current year, squared		0.0000			0.0005***
		(0.0001)			(0.0002)
1 year lag			-0.0020**	-0.0005	0.0282***
			(0.0008)	(0.0053)	(0.0103)
2 year lag			0.0019**		
			(0.0008)		
Interaction (current and 1 year lag)				0.0000	-0.0005***
				(0.0001)	(0.0002)
R-squared	0.108	0.108	0.108	0.108	0.108
Observations			2,684,632		

Cluster robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 3.9: Great Recession and insurance coverage (full set of results from Table 3.3)

Variable	*Uninsured*		*Private coverage*		*Public coverage*	
	1	2	3	4	5	6
07-09 indicator	0.0094*	0.0161	-0.0269***	-0.0243	0.0176***	0.0082
	(0.0048)	(0.0293)	(0.0069)	(0.0318)	(0.0035)	(0.0201)
Unemployment rate	0.0077***		-0.0135***		0.0058***	
	(0.0007)		(0.0009)		(0.0005)	
EP ratio		-0.0060***		0.0010***		-0.0040***
		(0.0008)		(0.0009)		(0.0005)
Interaction	-0.0016**	-0.0002	0.0040***	0.0003	-0.0024***	-0.0001
	(0.0006)	(0.0005)	(0.0008)	(0.0005)	(0.0005)	(0.0003)
Parent	-0.0455***	-0.0456***	-0.0004	-0.0004	0.0460***	0.0460***
	(0.0039)	(0.0039)	(0.0030)	(0.0030)	(0.0035)	(0.0035)
Family size	0.0129***	0.0129***	-0.0136***	-0.0136***	0.0007	0.0007
	(0.0017)	(0.0017)	(0.0020)	(0.0020)	(0.0006)	(0.0006)
Female	-0.0264***	-0.0265***	0.0028**	0.0029**	0.0236***	0.0236***
	(0.0015)	(0.0015)	(0.0012)	(0.0012)	(0.0016)	(0.0016)
Age	-0.0005	-0.0005	0.0102***	0.0102***	-0.0097***	-0.0097***
	(0.0004)	(0.0004)	(0.0007)	(0.0007)	(0.0005)	(0.0005)
Age squared	-0.0000***	-0.0000***	-0.0001***	-0.0001***	0.0001***	0.0001***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
White	-0.0447***	-0.0447***	0.0663***	0.0663***	-0.0216***	-0.0216***
	(0.0126)	(0.0126)	(0.0176)	(0.0177)	(0.0073)	(0.0074)
Black	-0.0333***	-0.0334***	-0.0178	-0.0174	0.0510***	0.0509***
	(0.0095)	(0.0095)	(0.0163)	(0.0164)	(0.0088)	(0.0088)
HS education	-0.1140***	-0.1130***	0.1880***	0.1850***	-0.0738***	-0.0728***
	(0.0112)	(0.0113)	(0.0089)	(0.0090)	(0.0057)	(0.0056)
Some college	-0.1840***	-0.1830***	0.2830***	0.2810***	-0.0986***	-0.0978***
	(0.0148)	(0.0148)	(0.0132)	(0.0132)	(0.0068)	(0.0067)
2 year degree	-0.1930***	-0.1920***	0.3010***	0.2990***	-0.1080***	-0.1070***
	(0.0144)	(0.0144)	(0.0125)	(0.0126)	(0.0076)	(0.0075)
4 year degree	-0.2250***	-0.2250***	0.3600***	0.3580***	-0.1340***	-0.1340***
	(0.0163)	(0.0164)	(0.0143)	(0.0144)	(0.0076)	(0.0076)
Graduate degree	-0.2390***	-0.2380***	0.3780***	0.3770***	-0.1400***	-0.1390***
	(0.0167)	(0.0168)	(0.0151)	(0.0152)	(0.0080)	(0.0080)
Married	-0.1020***	-0.1020***	0.1810***	0.1820***	-0.0797***	-0.0798***
	(0.0034)	(0.0034)	(0.0038)	(0.0038)	(0.0051)	(0.0051)
Time trend	0.0118***	0.0119***	-0.0157***	-0.0156***	0.0039***	0.0037***
	(0.0007)	(0.0008)	(0.0008)	(0.0009)	(0.0005)	(0.0006)
Time trend squared	-0.0003***	-0.0003***	0.0004***	0.0004***	-0.0001***	-0.0001***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
R-squared	0.108	0.108	0.166	0.166	0.068	0.068
Observations	2,684,632					

Table 3.9 (cont'd)

Cluster robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Figure 3.9: Public coverage, unemployment rate (BLS LAUS)

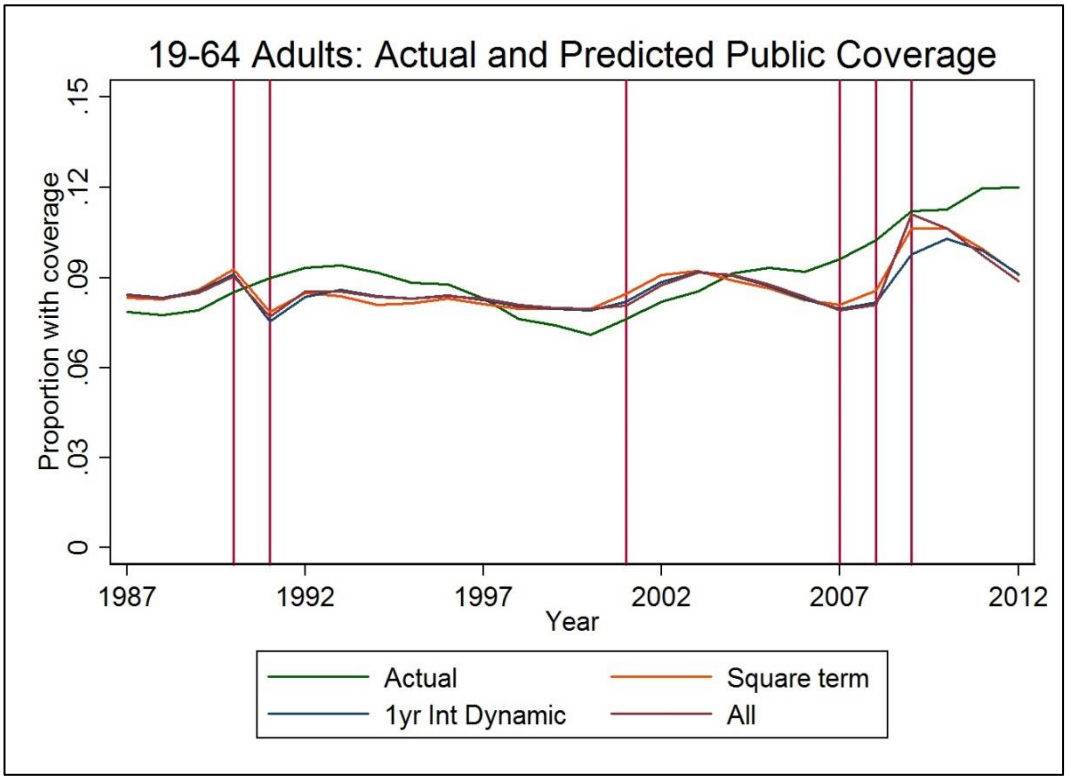
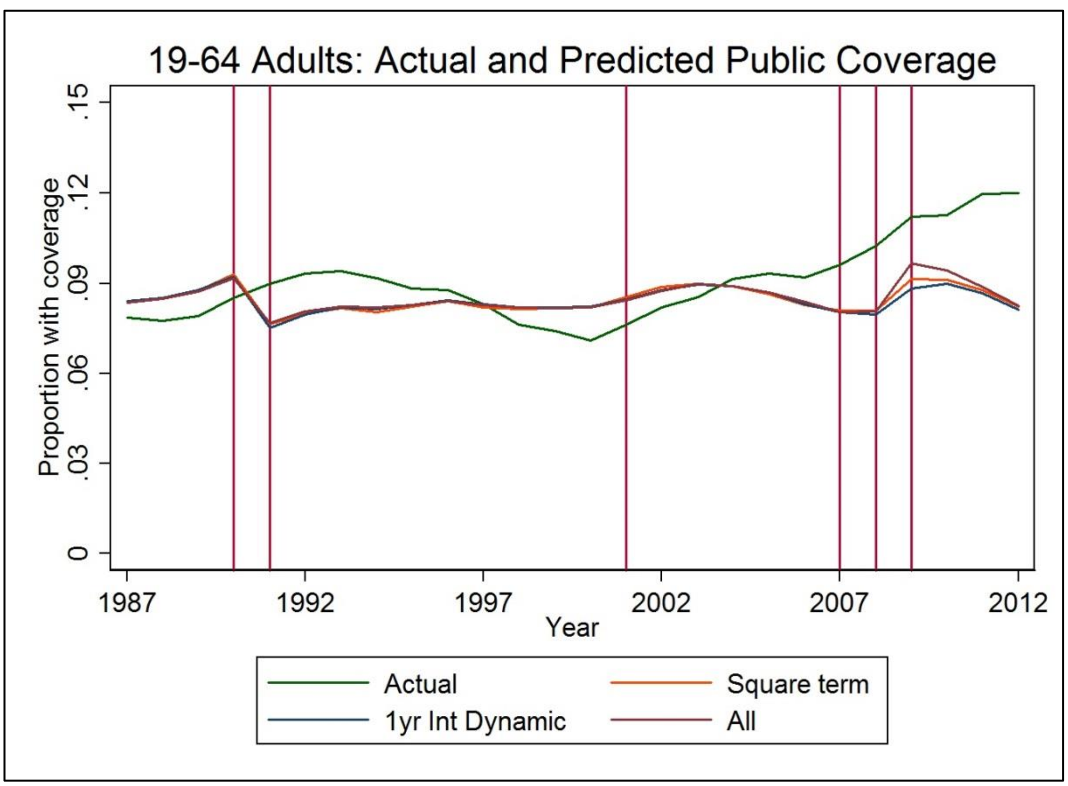


Figure 3.10: Public coverage, EP ratio (BLS LAUS)



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