Economic Evaluations of State Policies

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ABSTRACT

Economic Evaluations of State Policies

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Alcohol consumption and access to care are important measures of individual and public health. The goal of this dissertation is to examine these outcomes within the context of state-level policy evaluations and via an exploratory methods study.

In the first aim of the dissertation, we examine the effect of privatizing retail liquor sales and distribution on household-level alcohol purchases in Washington State by type of alcohol, using data from the Nielsen Consumer Panel Dataset. We use the differences-in-differences approach, with all other U.S. states serving as controls, to estimate the effect of privatization on household alcohol purchases. We find that privatization of liquor is associated with modest increases in liquor, beer, and ethanol purchases in the overall sample. Our subgroup analysis finds that there was variability in responsiveness to the policy. Privatization was associated with increases in liquor and ethanol purchases among low purchasers, an increase in liquor purchases among moderate purchasers, and a decrease in ethanol among high purchasers.

In the second aim of the dissertation, we examine whether Massachusetts health reform reduced the overall financial burden of accessing health services, using data from the Behavioral Risk Factor Surveillance System. We use the instrumental variables approach, with the

Massachusetts health reform as the instrument for health insurance and, with three other New England states serving as controls, to estimate the effect of gaining health insurance on forgoing care because of cost. We also investigate whether there was a differential response to the policy change by income levels. We find that gaining health insurance is associated with forgoing care because of cost in the overall sample. In the subgroup analysis, we found a differential effect of gaining health insurance, with health insurance benefiting those likely eligible for subsidies and likely not eligible for assistance. There was not a significant effect of gaining health insurance on forgoing care among those likely eligible for Medicaid.

In the third aim of the dissertation, we compare two broad instrumental variables approaches, two-stage least squares (2SLS) and two-stage residual inclusion (2SRI), within the setting of a binary endogenous treatment variable. We estimate the local average treatment effect of gaining health insurance on forgoing care, using Massachusetts health reform as an instrument for multiple models. Then we explore whether goodness-of-fit visualizations can assist in model selection. We conclude that the 2SLS model is biased and does not fit the data well. Goodness-of-fit visualizations are useful in distinguishing between choosing 2SLS and 2SRI.

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DEDICATION

For my Dad, Shelby Link, in loving memory

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CHAPTER 1

Introduction

This dissertation examines two state-level policy changes in order to investigate the intended and unintended effects of the policies on health outcomes. Furthermore, we explore the application of instrumental variables approaches using a policy change as our illustrative case study. We use quasi-experimental approaches to estimate treatment effects and conduct subgroup analyses to explore heterogeneity. The two policy topics we study are alcohol purchases and forgoing care because of cost; both topics are meaningful to individual and public health. Below, we introduce these two topics, our specific aims, and the guiding conceptual model.

Excessive alcohol consumption has detrimental consequences and is a common ongoing public health issue. In 2014, 24.7% of adults aged 18 and over reported engaging in binge drinking within the past month, and 6.7% reported heavy drinking in the past month.[1] Including binge drinking and underage drinking, excessive alcohol consumption is the third leading underlying cause of death in the United States, behind tobacco use and poor diet and physical inactivity.[2] It is responsible for 79,000 deaths per year.[3] The cost of excessive alcohol consumption was estimated at $223.5 billion ($746 per capita) in 2006. The cost to the government corresponded to $0.80 per alcoholic drink consumed in 2006.[4]

Specific aim 1: This study evaluates the effect of privatization of liquor distribution and retail sales in Washington State in 2012 on alcohol purchases by household. The policy change resulted in a five-fold increase in the number of off-premise liquor retail sales locations, a substantial increase in access to alcohol and reduction in the non-financial costs of purchasing liquor. Privatization also included new fees on distributors and retailers, increasing the price of

liquor in the first years following privatization. We investigate the overall effect of privatization on alcohol purchases by type of alcohol.

Prior to the implementation of the Affordable Care Act (ACA), 13.4% of the U.S. population did not have health insurance.[5] Lack of health insurance can lead to diminished access to care, unmet medical needs, worse health outcomes,[6] and ultimately a shorter lifespan. An estimated 18,000 deaths occur annually due to lack of health insurance.[7] The ACA and its predecessor, the Massachusetts health reform, sought to drastically reduce the number of uninsured individuals by increasing access to health insurance and instituting an individual mandate.

Specific aim 2: This study seeks to examine whether Massachusetts health reform reduced the overall financial burden of accessing health services. Massachusetts health reform expanded health insurance coverage after opening a state-based insurance exchange called The Connector and expanded Medicaid to cover all people below 150 percent of the federal poverty level. We examine whether Massachusetts health reform reduced forgoing care because of the cost, a meaningful measure of the overall affordability of health care. This study also examines whether there is a differential effect of health reform by income group corresponding to assistance level of the reform.

Specific aim 3: This study is a comparative analysis of instrumental variables methods in the specific application where the endogenous treatment variable of interest is binary. There are two broad approaches used to implement instrumental variables with a binary treatment variable: a linear approach (two-stage least squares (2SLS)), and a non-linear approach (two-stage residual inclusion (2SRI)). Using Massachusetts health reform as a case study, we first estimate the effect of gaining insurance on forgoing care using instrumental variables approaches, including 2SLS

and several models within 2SRI. We first determine whether the local average treatment effect varies by model. Then we use goodness-of-fit visualization techniques to evaluate models for the purpose of model selection.

Contribution

The proposed research will contribute to our understanding of exogenous factors that influence alcohol purchases and the financial burden of seeking care. The state-level policies that will be evaluated are hypothesized to directly affect access to alcohol use and health services. We use a modified Andersen behavioral model[10] as our guiding framework for understanding the ways in which these policies have multiple effects.

Significance

Theory and prior evidence suggest that the effects of the policies we examine on our outcomes of interest, alcohol purchases *(aim 1)* and forgoing care *(aims 2 & 3)*, may be positive or negative. Therefore, we have genuine uncertainty or equipoise about the outcomes of our studies. In *aim 1 (WA alcohol privatization)*, the privatization of alcohol in Washington has been reported to have a five-fold increase in the number of outlets that sell alcohol, greatly increasing access. However, privatization also increased the price of liquor, which would have countervailing effects on access. Therefore, it is unclear whether the policy has positive, negative, or equally opposing effects on alcohol purchases. In *aim 2 (MA health reform)*, gaining health insurance through the individual mandate could decrease the overall financial burden of seeking health services. However, assistance with the cost of health insurance may not provide enough assistance for people who have financial stress or strain or are burdened by paying for any portion of insurance premiums and copays. Finally, with *aim 3 (IV methods for binary*

treatment), although not a policy evaluation, we are unsure whether the criteria we use will provide us enough information for model selection.

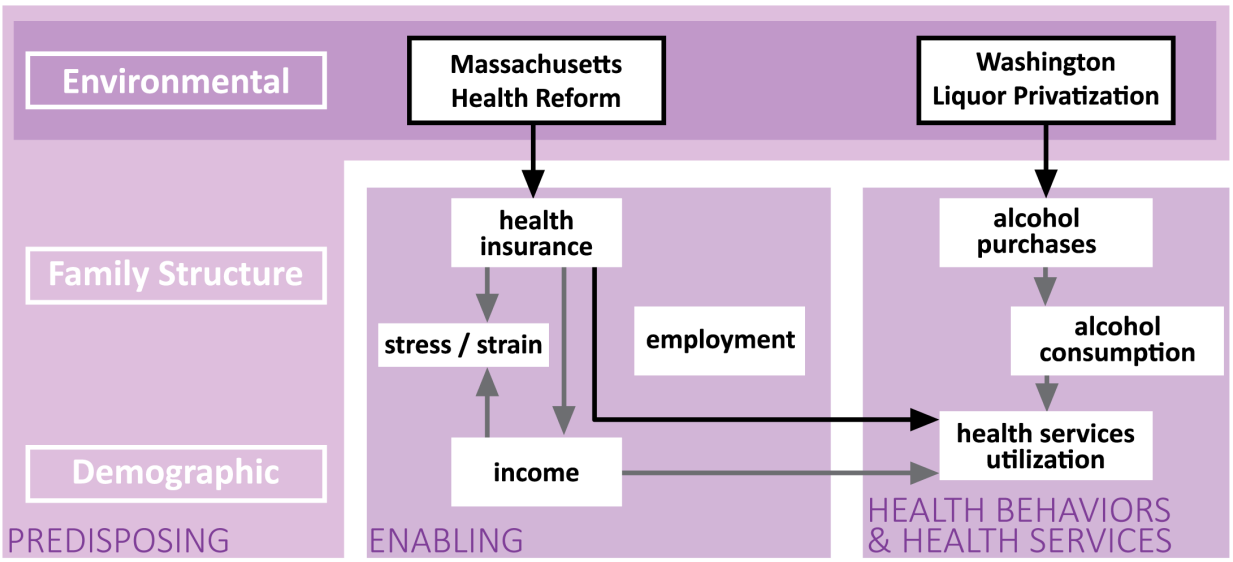
Both of these policies have meaningful implications to state and federal agencies and policymakers. Regarding *aim 1 (WA liquor privatization)*, Washington State's experience with privatizing alcohol will be an example to other states considering privatization, including Oregon, Virginia, and Pennsylvania. Although the move from public to private control of alcohol is rarely reversed, the effects of Washington's policy change are useful for Washington's public health surveillance and planning as well as to other states considering liquor privatization. Since the ACA was modeled after Massachusetts health reform, the findings of *aim 2 (MA health reform)* provide relevant information on the effect of gaining health insurance on the overall financial burden of seeking health care.

Quasi-experimental methods

These studies employ methods designed to yield empirically valid findings in social settings and in the absence of randomization. Specifically, we use the difference-in-difference and instrumental variables methods to identify the effect of policies on the health outcomes of interest. Pre-post study designs do not account for underlying time-dependent trends in the outcome of interest. Difference-in-difference includes a pre-post difference but overcomes the major flaw of the pre-post design by adding a second difference with a comparison group that is not exposed to the policy change but has similar trends in the outcome of interest.[8] Difference-in-difference methods will be used in *aim 1 (WA liquor privatization)*. Instrumental variables methods are used when the variable of interest is endogenous to the outcome, and an exogenous variable can be identified that affects the variable of interest directly but does not effect the outcome. Instrumental variables can "solve" the problem of omitted variable bias.[9] In *aim 2 (MA*

health reform) the policy change is exogenous to individual behavior and is used as an instrument for the variable of interest, health insurance, given that the policy is a strong predictor of the variable of interest. Lastly, we contribute to instrumental variables methods in *aim 3 (IV methods for binary treatment)* by reporting the effect of model choice on the treatment effect and exploring whether goodness-of-fit can assist researchers in choosing among the instrumental variable models.

Figure 1. Conceptual model



Conceptual model

The conceptual model for this dissertation draws upon Andersen's behavioral model as a guiding framework for explaining health services utilization and health outcomes.[10] Health behaviors and health services utilization are related to both predisposing and enabling factors. Enabling factors are types of resources that increase or decrease one's access and utilization of health services. Stress and strain are included as enabling factors because they either promote or deplete internal resources. The two state-level policies are conceptualized as environmental factors that are exogenous to individual-level behavior.

Washington State's privatization of alcohol *(aim 1 WA liquor privatization)* is expected to directly affect alcohol purchases. The black arrow representing this effect is what we estimate in *aim 1*. We believe alcohol purchases are a meaningful public health measure because they precede alcohol consumption. Alcohol use may result in acute injury and harm or long-term morbidity, leading to changes in health services utilization both in the short-term and over the lifetime.

Massachusetts health reform *(aim 2 MA health reform)* directly affects health insurance coverage, which provides access to health services. We estimate the first arrow leading from Massachusetts health reform to health insurance in the first stage of the instrumental variables approach. We then also investigate the effect of gaining health insurance on forgoing care due to the cost. We acknowledge that, for those who gain insurance, some may gain peace of mind due to the coverage, while others may experience stress or strain because of having to pay for insurance.

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CHAPTER 2

The effect of privatization of liquor distribution and retail sales on alcohol purchases: Evidence from Washington

Abstract

Objectives To investigate the effect of the 2012 privatization of liquor retail sales and distribution in Washington on household-level alcohol purchases. Privatization in Washington had two opposing effects on liquor purchases: It increased access to liquor and also included new fees on retailers and distributors, which increased the price of liquor in the first years following privatization. *Methods* Using the Nielsen Consumer Panel Dataset, we use differences-in-differences to estimate the effect of privatization of liquor in Washington on alcohol purchases by type of alcohol (ounces of liquor, wine, beer and total alcohol or ethanol) in the post-privatization period. *Results* Privatization is associated with an 8.36-ounce ($p<0.001$) and 3.7-ounce ($p<0.001$) increase in liquor and ethanol purchases, respectively, per household in the post-policy period spanning 31 months. In a longitudinal subgroup analysis, we find low alcohol purchasers and moderate alcohol purchasers increased purchases of liquor whereas high purchasers decreased ethanol purchases. *Conclusions* Privatization is associated with modest increases in liquor and ethanol purchases in the overall sample. Comparing the difference-in-difference effects to the baseline mean, privatization is associated with an approximate 3% increase in monthly liquor purchases and 1.6% increase in monthly ethanol purchases.

Introduction

Excessive alcohol consumption is the third leading underlying cause of death in the United States (U.S.),[1] preceded first by tobacco use and then poor diet and physical inactivity combined. It is responsible for 79,000 deaths per year.[2] Despite its detrimental consequences, alcohol consumption is ubiquitous in American culture and misuse is common. In 2014, 24.7% of adults ages 18 and over reported engaging in binge drinking within the past month, and 6.7% reported heavy drinking in the past month.[3]

Despite public health concerns with alcohol use, state and local governments have largely loosened their regulatory control of alcohol for decades. Following the end of prohibition in the U.S. in 1933, states created monopoly "control" or "license" systems to regulate the production, distribution, and sale of alcohol. Some states established partial monopolies for the retail sale of one type of alcohol. Other states commercialized alcohol through licenses. From the 1960s through the 1980s, several states privatized the retail sale of wine, with studies reporting subsequent increases in wine and total alcohol sales.[4-6] Overall, each state created a unique regulatory system, resulting in a national patchwork of differing policy systems across the U.S.

To decrease excessive alcohol consumption, the Task Force on Community Preventive Services, convened by the Centers for Disease Control and Prevention, recommends increasing alcohol taxes,[7] maintaining limits on days of sale[8] and hours of sale,[9] and regulating alcohol outlet density,[10] and recommends *against* privatization of alcohol sales.[11] Their 2011 recommendation against privatization is based on evidence that privatization of alcohol is associated with increased per capita alcohol sales, a proxy for excessive alcohol consumption.[11]

The public health effects of privatization of liquor in the U.S. have not yet been explored in recent history. In March 1987, Iowa was the first state with a monopoly on retail sales of

liquor to privatize retail liquor sales, resulting in a 9.5% increase in liquor consumption.[12] Twenty-four years later, Washington privatized liquor by popular vote in 2011, becoming the second state to privatize the retail sale of liquor. Washington also privatized the distribution of liquor. With this unprecedented change, Washington's privatization of liquor has been called the most significant change in alcohol regulation by a state since the repeal of prohibition.[13] Both the timing and the extent of the privatization efforts make studying the public health consequences of this policy change of particular interest.

The Washington State Liquor Control Board ceased state liquor store and liquor distribution operations on June 1, 2012, impacting the overall demand for liquor through changes in both price and access. The ending of the state's monopoly on liquor would be expected to decrease the price of liquor; however, the initiative included a 17% fee from retailers on all liquor sales as well as other fees for distributors that change over time.[14] Liquor prices rose by an average of 15.5% for a standard bottle of liquor (750 ml) post-privatization.[15] Two other studies report higher liquor sales in bordering states, Idaho[16,17] and Oregon,[17] providing additional evidence of increased liquor prices in Washington. As expected, the policy change greatly increased the number of off-premise alcohol sales locations. The number of locations licensed to sell liquor increased almost five-fold (from 328 to 1,600).[18] This substantial increase in access decreases the indirect costs of purchasing liquor.

Currently, only one published report examines the effects of privatization on state-level total purchases and individual-level consumption in Washington. The authors found a 1.2 million liter increase in liquor sold in the 16 months after privatization. Using self-reported alcohol consumption data from the Behavioral Risk Factor Surveillance System, the study found that there was an 0.9% increase in any alcohol use and no change in binge or heavy drinking.[19]

Because that report uses data from within Washington, the causes of the changes reported are unknown, and could reflect changing trends in alcohol purchases and consumption in the region or the U.S.

We examine the effect of privatization in Washington on household-level monthly alcohol purchases, a metric more relevant to public health than aggregate state-level sales volume. By employing differences-in-differences method, the present analysis improves on the pre-post design by adding a control group not exposed to the policy change. Our study contributes to the literature by examining the direct effect of the policy change on liquor purchases as well as purchases of wine and beer to examine cross-effects. We also examine a summary alcohol variable, *total ethanol*, to examine the overall effect on alcohol purchases. Given the opposing effects of the increased price of liquor (at least in the short term) and increased access to liquor, examining whether the policy change has had an overall increase, decrease, or non-significant change in alcohol purchases is an empirical research question with public health implications.

Methods

Data

We use data from the Nielsen Consumer Panel Dataset which tracks household purchases from various retailers. Panelists are provided an in-home bar code scanner to record each product purchased from any retailer. The scanner records the Universal Product Code (UPC), and panelists enter additional information including the quantity and price as well as when and where they made their purchases. These data include detailed information on the type and quantity of alcohol purchased.[20] Approximately 60,000 households have participated in the panel annually since 2007. Our study uses data from calendar years 2010 to 2014 to capture data before the

Washington State Liquor Control Board ceased state liquor store and liquor distribution (January 2010-May 2012) and after privatization of liquor sales (June 2012-December 2014). Many households participate in the panel for several years, though some households only participate for one year. Thus, the panel of households is an overlapping unbalanced panel. Nielsen Consumer Panel data are representative at the national level and at the level of "scantrack markets," which are large metropolitan and surrounding areas within a state. Our analysis is conducted at the state level.

Outcome variables

Our outcome variable of interest is *total ounces of liquor purchases* by households per month across all shopping trips. Since we are interested in whether privatization affected households' purchasing of other types of alcoholic beverage, other variables of interest include *total ounces of wine* and *total ounces of beer purchases* by households per month across all shopping trips. We computed a variable called *total ethanol ounces* by multiplying each type of alcoholic beverage by its estimated proportion of ethanol content (0.411 for spirits, 0.129 for wine, and 0.045 for beer) and summing across the types.[21]

Independent variables

Each analysis controls for the following binary demographic variables: income level, hours of employment, age group, marital status, whether there are children living in the home, racial group and Hispanic ethnicity. Indicators for each state are entered into the model to control for time-invariant confounders at the state level. Year and month indicators are included to control for time trends and seasonality in alcohol purchasing. We include two state-level variables, annual unemployment rate and annual beer taxes rates, to control for factors at the state level that could affect trends in alcohol purchases. To control for the potential confounding

effects of marijuana laws, an indicator for the decriminalization and an indicator for the legal retail sale of marijuana in Washington and Colorado are entered into the main analyses.

Analytic approach

To test whether privatization of liquor in Washington had a significant effect on alcohol purchases by households, we employ differences-in-differences. The analytic model can be written as:

$$Y_{it} = \beta_0 + \beta_1 X_{it} + \beta_2 S_t + \beta_3 T + \beta_4 WA_{it} + \beta_5 Privatization_{it} + \beta_6 WA_{it} * Privatization_{it} + \varepsilon_{it} \quad (1)$$

where the data vary by household(i), state(s) and time(t). Y_{it} is the outcome of interest: ounces of liquor, wine, beer, or ethanol purchases by households per month. X_{it} is a vector of household demographic variables. S_t is a vector of state-level indictor variables and state-level unemployment and beer tax rates. T is a vector of time variables, including year and month indicators. The variable, WA_{it}, represents households residing in Washington and β_4 captures differences in alcohol purchases in Washington, the treatment state, versus other states in the continental U.S., which serve as controls. A variable representing the timing of the dissolution of state control of liquor stores and distribution, $Privatization_{it}$, is equal to one in the post-privatization period (June 2012 onward) and zero beforehand. We are interested in the coefficient on the variable, $WA_{it} * Privatization_{it}$, an interaction between $Washington_{it}$ and $privatization_{it}$, equaling one for households residing in Washington in the post-privatization period. The difference-in-difference estimate is the predicted change in volume of alcohol purchases by households per month among people living in Washington minus the predicted change in volume of alcohol purchases by households per month among people living in other states. Predicted differences are estimated using non-linear post-estimation techniques to obtain

average treatment effects of the policy and standard errors.[22] For ease of interpretation, we also report the difference-in-difference effect in equivalent monthly drinks.[1]

The dependent variables are continuous. A large fraction of the population has no purchases of alcohol within a given month, and the data are skewed. We estimate the models using the negative binomial because, unlike Poisson, negative binomial accommodates over-dispersion. The error terms are clustered at the state level. All analyses are conducted using Nielsen-projection factor weights.

The difference-in-difference approach relies on the assumption that the treatment group and the control group have parallel trends in the outcomes of interest in the pre-policy period. To examine whether Washington and other states had parallel trends in liquor purchases in the pre-policy period, we first conduct a visual inspection of the unadjusted average volume of liquor purchases by households per month in Washington compared to all other states. We also formally test whether Washington has differential trends in liquor purchasing in the pre-policy period.

Subgroup analysis: the effect of privatization by pre-policy levels of alcohol purchasing

We explore whether there was a heterogeneous response to privatization by baseline level of alcohol purchases. We construct a longitudinal panel of households that participated in the Nielsen Consumer Panel for three continuous years surrounding privatization (2011-2013). The National Institute on Alcohol Abuse and Alcoholism defines low-risk drinking as no more than 4 drinks for men and 3 drinks for women on any day and no more than a total of 14 drinks a week for men and a total of 7 drinks a week for women.[23]Applying these weekly guidelines to the entire pre-policy period (17 months or 517 days), we created household-specific total ethanol

1 Equivalent monthly drinks = DID effect in ounces/(ounces in a standard drink by type*31 months in the post-policy period). A standard drink is defined as 1.5 ounces of liquor (40% alcohol), 5 ounces of wine (12% alcohol), or 12 ounces of beer (5% alcohol), all of which have about 0.6 ounces of ethanol.

purchase thresholds based on the number of adult men and women in each household (i.e., a household with one adult male and one adult female would have a threshold of 1,551 drinks). Households were labeled high alcohol purchasers (>threshold), moderate alcohol purchasers (<threshold & >0.5*threshold) and low alcohol purchasers (>0 & <0.5*threshold). We repeated the difference-in-difference estimations separately for each of these subgroups.

Accounting for changes in marijuana access

Both Washington and Colorado approved marijuana legalization for personal use by popular vote on November 6, 2012. Retail stores opened January 1, 2014 in Colorado and July 8, 2014 in Washington. In the intervening time periods, marijuana was decriminalized. If alcohol and marijuana were used independently, this would not be a concern. However, if they are substitutes, ignoring these changes would underestimate an anticipated overall positive effect of privatization on purchases. We have two ways to address these concerns in our estimation strategy. First, due the similarity in the timing of the marijuana legalization, we can use Colorado as a control group to help isolate the effect of privatization of alcohol on alcohol purchases. A second approach is to conduct the main analysis, excluding the time period in the data when marijuana was legally available to purchase in retail stores in Washington (July through December 2014).

Results

Table 1 reports the pre-policy means of households in the outcomes and covariates across Washington and all other states in the continental U.S. Washington households earn slightly higher incomes and are more likely to be white than households in other states. Notably, Washington households purchased much more wine than households in other states.

Figure 1 plots the average unadjusted values of ounces of liquor purchases by households in Washington and the control states every month from January 2010 through December 2014. Washington households had higher liquor purchases than people living in the control states even prior to privatization of liquor. Overall, there is seasonal variation, but trends are relatively stable from 2010 to May 2012. Despite the differences in levels, trends in Washington and the rest of the U.S. are approximately parallel. In the formal test of parallel trends in the pre-policy period, the variable representing Washington's trend is statistically insignificantly different from the control states (p=0.898), lending further support that using all other states is an appropriate control group for conducting difference-in-difference analysis. Post-privatization, Washington's trend remains insignificant (p=0.469) and we do not need to control for differential trends in the model. Colorado is used as a control state in additional analyses. Similarly, Washington's pre-period trend is statistically insignificantly different from Colorado's (p=0.133).

Difference-in-difference estimates of household-level purchases are presented in Table 2 with the "DID effect" representing the total change in purchases at the household level over the post-policy period (31 months). Using other states as controls, privatization is significantly associated with an 8.36-ounce increase in liquor purchases and a 20.13-ounce increase in beer purchases, but volume of wine purchases did not significantly change. Privatization is significantly related to an overall 3.7-ounce increase in ethanol purchases, representing a 1.6% increase in mean monthly ethanol purchases in comparison to baseline monthly purchases before privatization.

The subgroup analyses are presented in Table 3. The top panel repeats the main analysis using the longitudinal subsample of households. The results are slightly smaller for this longitudinal subsample: Privatization is associated with an increase in liquor purchases by 6.05

ounces and 1.59 ounces of ethanol. Among low alcohol purchasers, privatization is associated with an increase in liquor purchases by 7.42 ounces and purchases of ethanol by 3.15 ounces. Among moderate alcohol purchasers, privatization is similarly associated with an increase their liquor purchases by 25.41 ounces but there was not a corresponding increase in ethanol purchases. Privatization is associated with a decrease in ethanol purchases by 7.78 ounces for high alcohol purchasers. There were no significant changes in wine or beer purchases in the overall longitudinal sample or in the subgroups.

Using only Colorado as a control state to account for the decriminalization and legal retail sale of marijuana did not affect our main findings. Privatization is significantly associated with an increase in liquor purchases, though the effect is slightly smaller than was found in the main analysis, an increase in beer purchases and increase in ethanol purchases. Similarly, excluding the time period when marijuana was available for legal sale in retail stores, the effects of privatization are equivalent to our main analysis.

Discussion

We find that privatization of liquor sales and distribution, combined with a 15.5% price increase,[15] in Washington is related to statistically significant but modest increases in household purchases of liquor and ethanol. However, our subgroup analysis highlights that there was variability in responsiveness to the policy. Privatization was associated with increases in liquor and ethanol for low purchasers, increases in liquor purchases for moderate purchasers and was associated with a decrease in ethanol for high purchasers. We hypothesize that low and moderate alcohol purchasers may be more sensitive to liquor access and high alcohol purchasers may be more sensitive to the price of liquor.

Our study is the first to examine the effect of privatization of liquor sales and distribution in Washington on household-level purchases. Since participants in the Nielsen Consumer Panel scan all their groceries regardless of location of purchase, our study includes cross-border sales, giving a more complete measure of alcohol purchases than previous work that reports total volume of liquor sales in Washington. We examined cross-effects and found the privatization was associated with a slight increase in purchases of beer in the overall sample. There were no significant changes in purchases of beer or wine in the 3-year longitudinal panel.

Studies of the elasticity of alcohol purchases, or responsiveness to changes in price, have consistently found that alcohol purchases decrease when price increases, confirming a downward sloping demand for alcohol even among people characterized as addicted. A meta-analysis leads to the conclusion that liquor is the most responsive to price among the different types of alcohol.[24] Wagenaar and colleagues found the mean elasticities were -0.46 for beer based on 40 studies, -0.69 for wine based on 32 studies, and -0.80 for liquor based on 45 studies.[24] There has only been one study to date estimating the price change of liquor post-privatization in Washington, reporting a 15.5% increase in the price of a standard bottle of liquor (750ml) from June 2012 through March 2014.[15] In the absence of other changes, this increase in the price of liquor would be expected to *decrease* liquor purchases by about 12% (0.8*the percentage price change or 15.5%).

Yet, we found an *increase* in liquor purchases, suggesting that increased access to liquor substantially decreased the indirect costs of purchasing, more than offsetting the effect of the price increase. Comparing the difference-in-difference effects to the baseline mean, privatization is associated with an approximate 3% increase in monthly liquor purchases and 1.6% increase in monthly ethanol purchases. For most residents in Washington, the distance to a store that sells

liquor decreased. Counter to the Task Force on Community Preventive Services recommendations,[9] Washington's privatization also allowed for longer and later hours of sales. Post-privatization, stores selling alcohol can be open 20 hours a day, 7 days a week, almost doubling the hours of access prior to the policy change. Grocery stores and other retailers may now sell liquor, making its purchase more convenient with other types of shopping.

In the subgroup analysis where households were separated by pre-policy level of alcohol purchases, we found that there was a differential response to privatization, which is expected and calls into question what aspects of alcohol behavior are mutable by privatization. We found that low purchasers increase their liquor purchases and overall ethanol purchases, though the magnitude was modest. While moderate purchasers increase their liquor purchases, their overall ethanol purchases did not significantly change. High purchasers decrease their overall ethanol purchases. Given the opposing effects of increased price and increased access, taken together, a potential explanation for the subgroup findings is that access may have a more dominant effect for low and moderate purchasers, and price may have a more dominate effect for high purchasers may be more responsive to the change in the price of liquor.

The overall effect of privatization on liquor purchases is modest at the individual household level and is not expected to carry health effects if the alcohol purchased is consumed while adhering to the healthy drinking guidelines, including the maximum daily intake to avoiding binge drinking. However, for people with chronic conditions or taking medications, pregnant women, or those at the margin of developing an alcohol use disorder, even a modest increase in consumption could have negative health consequences.

Additionally, the protective effect of alcohol has been challenged in recent years.[25] A large-scale study concluded that reductions in alcohol consumption, even for light and moderate

drinkers, is beneficial to cardiovascular health.[26] Experts now agree that any level of regular drinking increases health risks including having a causal link to many cancers,[27] and has led Canada, Australia and UK to change their recommendations on alcohol consumption over the past few years.

Limitations

Our study has limitations. We examine the overall effect of privatization on alcohol purchases, encompassing both a change in access to liquor and prices of liquor, which change over time and cannot be disentangled by the current study design. Future work should attempt to tease out these effects. Using data through 2014, our study can only report on the short-term impacts of privatization. We use household-level purchasing data, which is not the equivalent to consumption. We do not know whether the incremental increases in alcohol purchases are consumed in a manner that is consistent with drinking guidelines. Since our analysis is conducted at the household-level, we implicitly assume that alcohol purchases are distributed evenly among all adults in the household, which may underestimate of the effect of privatization.

The Nielsen Consumer Panel dataset relies on households to scan alcohol and all items purchased with a scanner. Nielsen uses data from select retailers to validate purchases. Panel participants' purchases may not be generalizable if they do not scan all of the alcohol they purchase or if people who are heavy users of alcohol are less likely to participate in the panel. Both of these potential concerns would bias our results downward. It is important to stress that the Nielsen Consumer Panel dataset is representative of the U.S. and "scantrack markets," which are large urban areas but is not representative at the state level. However, we conducted a sensitivity analysis (results not shown) restricting the analysis to "scantrack markets," and the results do not change.

Policy implications

Both Oregon and Pennsylvania have considered legislation and ballot measures to privatize their liquor sales in the past year, and the issue may come up again in these states or in one of the other remaining 12 monopoly "control" states. Although the movement from public to private sale and/or distribution of alcohol is rarely reversed, the effects of Washington's policy change are useful for Washington's policymakers and to other states considering such a policy change.

Table 1. Pre-privatization (January 2010-May 2012) means by household in WA and U.S.

	U.S.	WA	Adj. Wald		U.S.	WA	Adj. Wald
Dependent variables			*Prob > F*	**Dependent variables cont.**			*Prob > F*
Monthly oz liquor purchases	8.41	9.81	0.00	Monthly oz beer purchases	55.56	59.40	0.04
Monthly oz wine purchases	12.35	24.96	0.00	Monthly oz ethanol purchases	7.54	9.92	0.00
Covariates (annual)				**Covariates (annual) cont.**			
Income				Household composition			
Under $25,000 (base)	0.23	0.20	0.00	Any children in the home	0.34	0.33	0.01
$25,000-34,000	0.11	0.09	0.00	Number of adults	1.97	1.92	0.00
$35,000-49,000	0.15	0.15	0.27	Household size	2.56	2.53	0.01
$50,000-69,000	0.16	0.18	0.00	Race and ethnicity			
$70,000-99,000	0.16	0.17	0.00	White (base)	0.77	0.83	0.00
$100,000 & up	0.20	0.21	0.05	Black	0.12	0.03	0.00
Marital status				Asian	0.03	0.06	0.00
Married (base)	0.47	0.47	0.12	Other race	0.08	0.08	0.78
Widowed	0.10	0.11	0.00	Hispanic	0.12	0.07	0.00
Divorced	0.21	0.22	0.00	State level covariates			
Single	0.22	0.21	0.00	State beer tax	0.27	0.47	0.00
				State unemployment	9.08	9.37	0.00
Covariates by male head of household (annual)				**Covariates by female head of household (annual)**			
Male head age cateogories				Female head age categories			
Age 18-24 (base)	0.00	0.01	0.01	Age 18-24 (base)	0.01	0.01	0.00
Age 25-44	0.17	0.20	0.00	Age 25-44	0.22	0.24	0.00
Age 45-64	0.34	0.33	0.33	Age 45-64	0.36	0.33	0.00
Age 65 & up	0.12	0.11	0.17	Age 65 & up	0.13	0.12	0.00
Male head employment status				Female head employment status			
Not employed (base)	0.21	0.20	0.00	Not employed (base)	0.34	0.33	0.01
Employed 30 hours	0.03	0.03	0.81	Employed 30 hours	0.09	0.10	0.03
Employed 30-34 hours	0.02	0.02	0.05	Employed 30-34 hours	0.04	0.04	0.04
Employed 35 hours & up	0.44	0.47	0.00	Employed 35 hours & up	0.32	0.31	0.01
Male head education				Female head education			
< high school grad (base)	0.05	0.04	0.00	< high school grad (base)	0.03	0.02	0.00
High school grad	0.23	0.19	0.00	High school grad	0.28	0.22	0.00
Some college	0.20	0.25	0.00	Some college	0.25	0.28	0.00
College graduate	0.22	0.23	0.00	College graduate	0.23	0.25	0.00

Notes: N in US=1,731,742. N in WA=43,948. Observations are household-months from January 2010-May 2012. Means are calculated using sampling weights. Male head and female head percentages are unconditional means and do not sum to one. U.S. includes all U.S. states with the exception of Washington, Alaska and Hawaii.

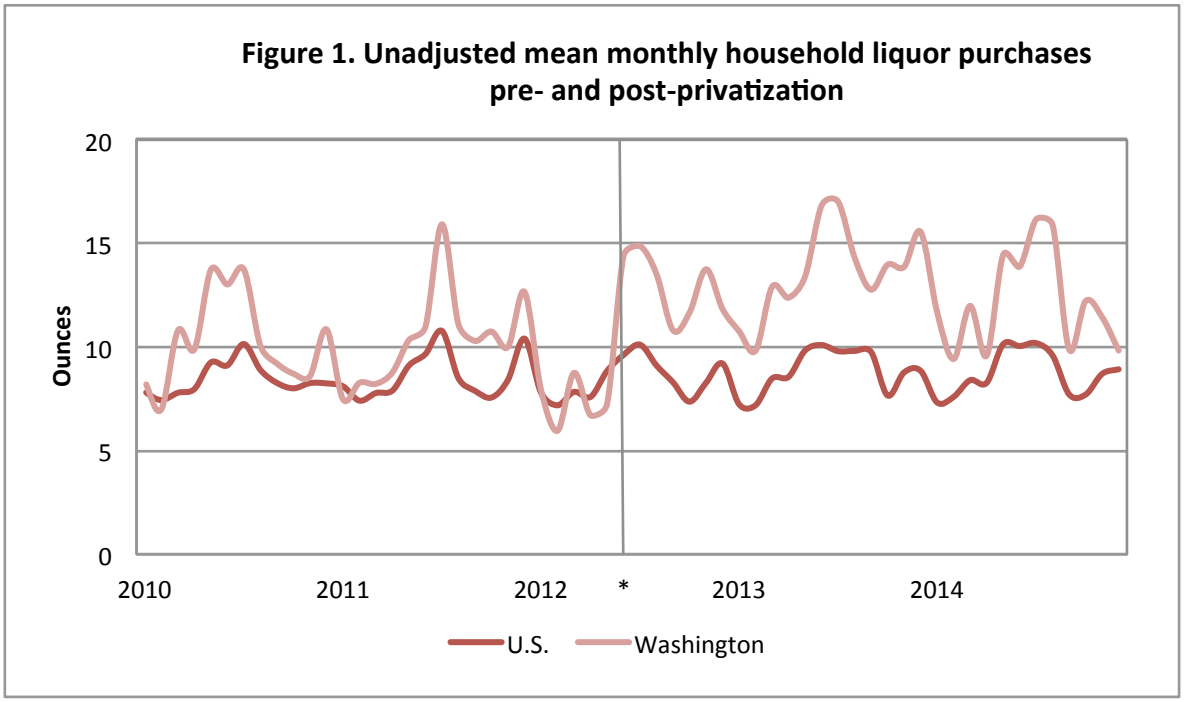

Table 2. Difference-in-Difference Estimates: Effect of privatization on alcohol purchases in the post-policy period (June 2012-December 2014)

	oz liquor	oz wine	oz beer	oz ethanol
DID effect	8.36 ***	-2.55	20.13 ***	3.7 ***
Analytical SE	[1.69]	[1.97]	[8.09]	[1.02]
p-value	0.00	0.20	0.01	0.00
Equivalent monthly drinks	0.18	-0.02	0.05	0.20
Baseline mean monthly drinks	5.63	2.53	4.64	12.66
N=3,671,304				

Notes: Observations are household-months. All specifications are estimated using sampling weights. Difference-in-difference (DID) effect is estimated using -predictnl- in stata. Analytic standard errors (SE) are clustered at the state level and are calculated using the delta method. Equivalent mean monthly drinks=DID effect/(ounces in a standard drink*31 months). Baseline mean monthly drinks are means in the pre-policy period. *Significantly different at the **5% level; ***1% level.

Table 3. 2011-2013 Longitudinal Panel DID Estimates: Effect of privatization on alcohol purchases in the post-policy period (June 2012-December 2013)

Full longitudinal panel		**oz liquor**		**oz wine**	**oz beer**	**oz ethanol**	
N=1,577,808	DID effect	6.05	***	-4.17	1.97	1.59	**
100% longitudinal sample	Analytical SE	[1.42]		[3.13]	[3.10]	[0.59]	
	P-value	0.00		0.18	0.53	0.01	
	Equivalent monthly drinks	0.21		-0.04	0.01	0.14	
	Baseline mean monthly drinks	6.23		2.93	5.12	14.15	
Low alcohol purchasers		**oz liquor**		**oz wine**	**oz beer**	**oz ethanol**	
N=878,220	DID effect	7.42	***	0.75	5.99	3.15	***
56% longitudinal sample	Analytical SE	[1.12]		[0.82]	[1.88]	[0.32]	
	P-value	0.00		0.36	0.06	0.00	
	Equivalent monthly drinks	0.26		0.01	0.03	0.28	
	Baseline mean monthly drinks	3.80		2.03	3.08	8.83	
Moderate alcohol purchasers		**oz liquor**		**oz wine**	**oz beer**	**oz ethanol**	
N=89,748	DID effect	25.41	**	0.19	-9.55	2.37	
5.7% longitudinal sample	Analytical SE	[9.52]		[16.78]	[21.68]	[2.87]	
	P-value	0.01		0.01	0.66	0.41	
	Equivalent monthly drinks	0.89		0.00	-0.04	0.21	
	Baseline mean monthly drinks	22.27		13.07	23.19	57.75	
High alcohol purchasers		**oz liquor**		**oz wine**	**oz beer**	**oz ethanol**	
N=78,120	DID effect	-10.73		-33.49	27.43	-7.78	**
5.0% of longitudinal sample	Analytical SE	[7.73]		[67.19]	[56.62]	[3.06]	
	P-value	0.16		0.62	0.63	0.01	
	Equivalent monthly drinks	-0.38		-0.35	0.12	-0.68	
	Baseline mean monthly drinks	63.49		23.98	46.89	133.18	

Notes: Observations are household-months. All specifications are estimated using sampling weights. Difference-in-difference (DID) effect is estimated using -predictnl- in stata. Analytic standard errors (SE) are clustered at the state level and are calculated using the delta method. Equivalent mean monthly drinks=DID effect/(ounces in a standard drink*19 months). Baseline mean monthly drinks are means in the pre-policy period. Households that did not purchase any alcohol in the pre-policy period are included in full longitudinal panel estimation. *Significantly different at the **5% level; ***1% level.

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CHAPTER 3

Is providing health insurance sufficient to remove financial barriers to health care? Evidence from the Massachusetts health reform

Abstract

One of the primary goals of recent health care reform legislation is to increase health insurance coverage and reduce the overall financial burden of accessing health care; however, little is known about whether cost remains a prohibitive barrier for those who gain insurance. Using data from the Behavioral Risk Factor Surveillance System, we examine whether gaining health insurance through the Massachusetts health reform, the precursor to the Affordable Care Act, affected forgoing care because of the cost among those who gained insurance. We also investigate whether there was a differential response to the policy change by income levels. By examining Massachusetts health reform, we identify the effect of the reform on those likely eligible for subsidies and likely not eligible for assistance, two groups which are difficult to study with the Affordable Care Act. We find gaining health insurance reduces forgoing care because of cost by 11.4 percent in the overall sample. In the subgroup analysis, we found the largest decrease in forgoing care, 15.5 percent, in the likely subsidy eligible group. We also found a 10 percent decrease in forgoing care in the group likely not eligible for assistance. However gaining health insurance did not significantly reduce forgoing care among those likely eligible for Medicaid.

Introduction

The health care system in the U.S. is a patchwork of private and public systems, historically relying on employers offering health insurance to their employees. In the decade leading up to the recent legislative reforms, however, employer-sponsored health insurance declined covering 69.7 percent of working aged adults in 1999-2000 to only 59.5 percent in 2010-2011 (Sonier, 2013). Concurrently, public insurance was only available to certain segments of the population, and both the individual and small group markets remained small. In 2013, the year before major components of the Affordable Care Act (ACA) were implemented, 13.4 percent of the U.S. population did not have health insurance (DeNavas-Walt, Proctor, & Smith, 2013). Lack of health insurance can lead to diminished access to care, unmet medical needs, worse health outcomes (Hadley, 2003), and ultimately to a shorter lifespan. An estimated 18,000 deaths occur annually due to lack of health insurance (Institute of Medicine, 2004). The ACA and its predecessor, the Massachusetts health reform, sought to drastically reduce the number of uninsured individuals by increasing access to health insurance and instituting an individual mandate.

The ACA expanded access to both privately- and publicly-funded health insurance. Previously, Medicaid provided health coverage to children, parents of young children, pregnant women, seniors, and people with disability, although eligibility rules varied state-to-state. Under the ACA, these eligibility rules became simplified for states that expanded Medicaid, such that Medicaid now covers all people under age 65 with incomes less than 138 percent of the federal poverty level (FPL), including low-income adults without children. The ACA also created state-based health insurance exchanges to improve the functioning of the individual and small group health insurance markets, as well as financial subsidies to increase the affordability of insurance

coverage. The ACA provides premium credits and cost-sharing subsidies for people at 139 through 400 percent of the federal poverty level, and caps the percentage of income people pay toward premiums and out-of-pocket expenses.

While the bulk of the research about the early effects of the ACA has focused on health insurance coverage, concern remains about whether the reform has done enough to make health care affordable. While the financial barriers to accessing health *insurance* are reduced under health reform, there are many other reasons people are unable to obtain *health care*. Having health insurance alone does not guarantee that health services can be utilized. People with and without health insurance have difficulty paying medical bills. In 2012, 20 percent of non-elderly insured adults and 45 percent of non-elderly uninsured adults reported having difficulty paying medical bills (Pollitz et al., 2014). Treating an illness or injury without adequate health insurance can lead to enormous medical debt. Due to cost concerns, people may delay or forgo care, even if they have health insurance coverage.

Previous evidence about the relationship between health insurance expansion and the overall financial burden of obtaining care comes primarily from public insurance program expansions. The Oregon Health Insurance Experiment, which expanded Medicaid coverage for adults through a lottery, found that average out-of-pocket expenditures decreased by $215.35. Medical debt decreased by 13 percentage points. Borrowing money to pay for bills or skipping payments decreased by 14 percentage points. Catastrophic expenditures were nearly eliminated (Baicker et al., 2013). Ayyagari & Shane (2015) examined the effect of Part D prescription drug coverage as part of Medicare expansion. The study reported a 2.9 percent reduction in people incurring more than $2,000 in out-of-pocket expenditures. In addition, there was an average decrease of $153 in out-of-pocket spending for those under 200 percent of the federal poverty

level. Although these studies are of different programs (Medicaid vs. prescription coverage) affecting different populations (low income people meeting specific criteria versus older adults), they both demonstrate that health insurance reduces the financial burden of medical care.

There is little research estimating the effect of gaining health insurance on delaying or forgoing care because of cost (Pande et al., 2011; Sommers, Baicker, & Epstein 2012; Van Der Wees, Zaslavsky, & Ayanian 2013). This is a key, but often omitted, component of the overall financial burden of medical care. The Rand Health Insurance Experiment, conducted in the 1970's and 1980's, randomized families into varying co-insurance and maximum out-of-pocket dollar amounts. The experiment found that when people faced any co-insurance, some people did not seek any care. Of those who were in the free care plan, 86.8 percent used care. That percentage dropped to 78.8 percent for the group of people who faced a 25 percent co-insurance rate (Gruber, 2006). This finding highlights an important point– any cost is a strong enough deterrent for some people to not seek care. Another study of Medicaid expansions in three states, New York, Maine and Arizona, examined whether obtaining health insurance reduced delays in seeking care. The study found a 2.9 percentage point decrease in delaying care because of cost at the population level (Sommers et al., 2012).

Massachusetts health reform

In 2006, Massachusetts enacted health insurance reform (referred to as "health reform" or "the reform" hereafter), which included an individual mandate for insurance coverage. In its first year, the reform was successful at reducing the percentage of working aged adults who were uninsured from 13 percent in 2006 to 7 percent (Long, 2008). The uninsured rate continued to decline to 4.8 percent as of 2012 (Cohen & Martinez, 2013), which corresponds to the end of the present study and demonstrates the effect of Massachusetts health reform in the absence of the

ACA. The state-based exchange, called the Connector, allowed individuals and small businesses with fewer than 50 employees the opportunity to purchase affordable health insurance. The reform also placed requirements on the quality of provided financial coverage by setting requirements like maximum out-of-pocket costs.

Massachusetts health reform expanded Medicaid to fully cover individuals with incomes up to 150 percent federal poverty level, which corresponded to an income of $25,755 for a family of three in 2007. Prior to this expansion, Medicaid covered low-income children, their parents, pregnant women, and people with disabilities. The expansion of Medicaid provided low-income working-aged adults with insurance without any premium and minimum out-of-pocket costs. The Commonwealth Health Insurance Program also provided subsidized insurance for individuals with incomes between 150 percent and 300 percent of the federal poverty level. People with incomes above 300 percent of the federal poverty level could purchase non-subsidized health insurance.

There are several studies on the impact of Massachusetts health reform on health services utilization and health outcomes using difference-in-differences, an analytic approach that produces population-level estimates. One study reported fewer hospitalizations originating from the emergency room (ER) (Miller, 2012) while another study found a large reduction in non-urgent ER usage (Kolstad & Kowalski, 2012). The Massachusetts health reform has also been found to decrease all-cause mortality; one life was saved for every 800 insured (Sommers, Long & Baicker 2014). Researchers have found that the reform significantly increased overall self-rated health, for both physical and mental health (Courtemanche & Zapata, 2014). A recent evaluation of the impact of the reform on financial outcomes found that the reform reduced the

total amount of debt that was past due, improved credit scores, and reduced personal bankruptcies (Mazumder & Miller, 2014).

Two studies, similar to this one, have examined the effect of Massachusetts health reform on *not going to the doctor because of cost* – the outcome of interest for this study – at the population level. Pande et al. (2011) found a 4.8 percent decrease in the proportion of adults forgoing care because of cost for all people living in Massachusetts from 2007-2009. Similarly, Van Der Wees et al. (2013) using difference-in-differences found a 2 percent decrease in forgoing care because of cost for all people living in Massachusetts. However, these studies did not examine the effect of Massachusetts health reform on forgoing care because of cost *among people who obtained insurance through health reform,* which is achieved in this study by using instrumental variable methods. Further, we are unaware of any studies that have tested whether there is a differential effect of health reform by income group corresponding to subsidy assistance level on forgoing care because of cost.

There is a dearth of information regarding the effects of health insurance on the financial burden of obtaining care among lower-income working aged adults. A key measure of the affordability of health care is self-reported, non-utilized health services because of cost. Therefore, a critical evaluation of health reform is whether those who gained health insurance were able to seek the medical care they needed without cost being a prohibitive concern. The literature has not addressed this question. This study seeks to fill these gaps by examining whether there was a change in the percentage of people forgoing care because of cost after gaining health insurance through the Massachusetts health reform. We estimate the overall effect and look for heterogeneity in the effect by income groups that correspond with the parameters of the policy expansions (<150 percent of the FPL, 150-300 percent FPL, >300 percent FPL).

There are several advantages to focusing on the Massachusetts reform. We can use several years of post-reform data to measure the impacts of the reform, since the Massachusetts reform occurred in 2007. We have good identification for both the Medicaid expansion and the health insurance exchange market expansions, which is methodologically more challenging when studying the ACA due to the simultaneity of the changes across the states and the potential legislative endogeneity of the decision to expand Medicaid. Finally, our findings from Massachusetts health reform foreshadow what we expect to experience in the broader US context under the ACA.

Methods

Data source

This study uses data from calendar years 2003-2012 from the Behavioral Risk Factor Surveillance System (BRFSS), a large state representative cross-sectional survey of non-institutionalized adults. We focus on a time period of data before the implementation of the health reform law in Massachusetts (January 2003-June 2007) and after (July 2007-Decemer 2012). BRFSS is an annual health survey coordinated by the Centers for Disease Control and Prevention (CDC) and conducted by individual states. BRFSS was a landline-based survey through 2010 and then in 2011 and 2012, BRFSS included cellular based surveys. The survey asks participants about their self-rated health, health-related quality of life, access to health care, health behaviors, as well as chronic conditions. We merged in two external data sources, state-level housing prices (Federal Housing Finance Agency, 2016), and state-level unemployment (Bureau of Labor Statistics, 2016) into the data to adjust for state-level conditions.

Study population and analytic sample

The study population includes all non-elderly adults, ages 18-64, who participated in the survey and had complete data for dependent variables and all covariates with the exception of income. All variables had less than 2 percent missing observations with the exception of income, which we account for by creating a separate indicator variable. We restrict the sample to people living in Massachusetts and three New England states acting as control states: Connecticut, New Hampshire, and Rhode Island. Vermont and Maine are not included because those states also had health policy changes enacted during the time period of interest. The complex survey design of BRFSS was accounted for using survey weights following BRFSS' recommendation (Centers for Disease Control and Prevention, 2013).

Outcome variable

The dependent variable *forgoing care* is binary, equaling one (and zero otherwise) if participants answered affirmatively to the question, "Was there a time in the past 12 months when you needed to see a doctor but could not because of cost?".

Independent variables

We include binary variables for sex, age categories, race and ethnicity, marital status, employment status, and educational levels, as well as binary variables for income categories in the overall estimation. State, year, and month binary variables were included in all analyses to control for state specific, time invariant effects and to control for time varying effects. We control for three time periods relating to the Massachusetts health insurance reform: the pre-reform period (January 2003-February 2006); the period after passing the law but before it was implemented (March 2006-June 2007), when the health insurance exchange, the Connector began and Medicaid eligibility was expanded; and after full implementation (July 2007-December 2012), when the individual mandate went into effect. The latter two periods were

interacted with a binary variable indicating that the individual lives in Massachusetts. The variable, *MA*Reform*, is used as an instrument for health insurance coverage. We also include a measure of state-level housing prices and state-level unemployment to control for the impact of the recession by state. The variable of interest, *health insurance*, is a binary variable, equal to one if people answered affirmatively to the question, "Do you have any kind of health care coverage, including health insurance, prepaid plans such as HMOs, or government plans such as Medicare, or Indian Health Service?".

Statistical analysis: Instrumental variables approach

Many unobserved variables are related to both having health insurance and forgoing care because of cost such as education level, employment, income, and age. The estimated effect of health insurance on forgoing care is biased because of cost that does not account for this endogeneity. This approach is called the naive approach because health insurance coverage is a choice and is endogenous to the decision of forgoing care because of cost. Because of this endogeneity, we use an instrumental variables approach. An instrument acts as a pseudo-randomization, and sorts people across the treatment in order to achieve a more equal distribution of both observed and unobserved confounders across health insurance status. The instrumental variable addresses, at least in part, the overt and hidden bias in estimating the average treatment effect. We begin by reporting the results of the naive approach, which is a correlation between health insurance and forgoing care because of cost. We estimate the naive model using probit regression.

We use an instrumental variable to test the extent to which the Massachusetts health reform had a causal effect on forgoing care because of cost *among working aged adults who gained health insurance* after the policy was implemented. The variable representing health

reform, an interaction term between living in Massachusetts and the post-policy period (*MA*Reform*), is used as instrument for health insurance status and is a binary variable equal to one for those reporting having any kind of health care coverage. The instrumental variables approach assumes that the Massachusetts health reform has a significant effect on health insurance coverage, which is a testable assumption.

The instrumental variables approach also assumes that health reform only affects forgoing care because of cost though gaining health insurance or along the intensive margin. This assumption would be violated if, for example, cost barriers were relaxed for other reasons not related to gaining coverage. The Massachusetts health reform provided full or partial health insurance premium subsidies to low- and middle-income individuals and families, but it did not attempt to change the amount billed for health services nor participate in negotiated rates which determine how much people pay out-of-pocket.[2]

Another potential concern for our identification strategy is legislative endogeneity. The reform legislation was proposed by Governor Mitt Romney and received overwhelming support by the legislature. If Massachusetts residents who supported the passage of reform were more likely to gain coverage under the reform, then the state policy change would be endogenous and would violate the assumptions of instrumental variables. However, a poll conducted just after health reform found that 64 percent of people supported the law after hearing a description of it and support did not significantly vary across people most likely to be impacted by the law, including the uninsured, poor, young adults, and minorities (Blendon et al., 2006). We assume that the enactment of Massachusetts health reform is exogenous to individual-level behavior.

[2] One potential way this assumption could be violated is if insured, high health-care utilizers changed their decision to seek care after being protected by maximum out-of-pocket limits, which were also part of the health care reform. Only a small fraction of people would fall into this category so we don't think it would undermine our approach.

The analytic model is estimated in two stages:

$$T_{it} = \alpha_0 + \alpha X_{it} + \alpha_1 MA_{it} + \alpha_2 Law_{it} + \alpha_3 Reform_{it} + \alpha_4 (MA_{it} * Law_{it}) + \alpha_5 (MA_{it} * Reform_{it}) + \phi_{it} \quad (1)$$

$$Y_{it} = \beta_0 + \beta_1 X_{it} + \beta_2 T_{it} + \beta_3 \hat{\varphi}_{it} + \beta_4 MA_{it} + \beta_5 Law_{it} + \beta_6 Reform_{it} + \beta_7 (MA_{it} * Law_{it}) + \varepsilon_{it} \quad (2)$$

The data varies by state, individual(i), and time(t). $\mathbf{T_{it}}$ is the variable health insurance and is equal to one for people who report having any kind of health insurance coverage. Y_{it} is the outcome of interest, forgoing care because of cost. MA_{it} is a binary variable equal to one for people living in Massachusetts. Law_{it} is a binary variable equal to one during the period that the health reform became law but was not implemented (April 2006-June 2007). $Reform_{it}$ is a binary variable equal to one during the period that heath reform was implemented (July 2007-2011). The interaction term $MA_{it}*Law_{it}$ equals one for people living in the state if Massachusetts during the time period the law was passed but not implemented; this term may capture anticipation effects of the law. $MA_{it}*Reform_{it}$ is the instrument and equals one for observations in Massachusetts in the post-policy time period.

Because we are estimating a non-linear model, we employ a two-stage residual inclusion approach where the endogenous regressor and the residual from the first stage (equation 1), $\hat{\boldsymbol{\varphi}}_{\mathbf{it}}$, are included in the second-stage probit regression (Terza, Basu, & Rathouz, 2008). We estimate Anscombe residuals from the first stage instead of raw scale residuals to reduce bias in the treatment effect estimates, given that the treatment mean and outcome mean are both relatively rare at approximately 10 percent (Basu & Coe, 2016). The coefficient of interest is β_2, the local average treatment effect, which is the effect of the reform among those who gained insurance due to health reform. Both the first and second stages are estimated using probit regression based on Hosmer-Lemeshow goodness-of-fit tests on the second stages, comparing logistic, probit and cloglog.

Subgroup analysis

The Massachusetts health reform affects individuals in different portions of the income distribution differentially due to the type of insurance and the amount of subsidy available. Thus we test for differential effects on who obtains health insurance based on income level. Using the reported household size and self-reported income categories, we created three broad assistance categories to correspond to the three levels of assistance provided by the Massachusetts exchange: likely Medicaid eligible (<150 percent FPL), likely subsidy eligible (150-300 percent FPL) and likely no assistance (>300 percent FPL). First and second stages were estimated for each of these income categories.

In order to create these three groups, we assume people's incomes were at the midpoint of the reported income category, with the exception of the highest income category, $75,000 and up. We computed total household size as the sum of number of children in the household and the sum of the number of adults in the household, assuming only one adult if the number of adults was missing and zero children if the number of children was missing. We used the incomes associated with 150 percent of the federal poverty level and 300 percent of the federal poverty level each year in the post-policy period (2007-2012) as cut offs for determining likely assistance levels (Health and Human Services, 2015). For example, households with three or more members reporting a household income of $20,000-$25,000 ($22,000 midpoint) would be categorized as *likely eligible for Medicaid* because their incomes were less than 150 percent of the federal poverty level ($25,755 in 2007 and higher in other years). Whereas households with three or more members reporting an income of $25,000-$35,000 ($30,000 midpoint) or $35,000-$50,000 ($42,500 midpoint) would *likely be eligible for a partial subsidy* because their incomes fall between 150 percent of the federal poverty level and 300 percent of the federal poverty level.

We assume people with incomes of $75,000 and up were *likely not eligible for subsidies*, given that: 1) only households with 5 or more members would be eligible for subsidies with a household income of $75,000, and 2) the mean household size in our sample is 2.7 members.

Results

The final analytical sample includes 229,491 participants. Table 1 presents variable means. Table 1 also presents variables means over the variable of interest, *health insurance*, and the instrument, *MA*Reform*. On average, people without health insurance are younger, male, unmarried, have lower incomes, are less educated, and are more likely to be unemployed. The instrument greatly improves imbalance, and hence, significantly reduces confounding. Table 2 presents the naive approach of estimating the relationship between health insurance on not seeking care because of cost by income group. In the full sample, health insurance is correlated with a 20.8 percent lower likelihood of not seeking care because of cost.

The effect of Massachusetts health reform on health insurance coverage (first stage estimates) overall and by subgroup are presented in the top panel of Table 3. The first stage results for the overall sample and by income category indicate that *MA*reform* has a positive and significant (at the 0.01-level) relationship with health insurance coverage, with an F-statistic (558 to 4,359) well over the recommended thresholds for each income category (Staiger & Stock, 1997; Stock & Yogo, 2005). We conclude that the instrument is statistically strong. Overall, Massachusetts health reform caused a 5.7 percent increase in health insurance. Furthermore, health reform increased health insurance by 15.1 percent for people likely eligible for Medicaid, 8.9 percent for people likely eligible for subsidies and 3.1 percent for people likely not eligible for premium subsidies.

The results for the effect of health insurance on *not seeking care because of cost*, using Massachusetts health reform as an instrument for health insurance (second stage estimates), are presented in the bottom panel of Table 3. Gaining health insurance through health reform decreased forgoing care due to cost by 11.4 percent for the full sample. Health insurance was associated with a 15 percent decrease in forgoing care because of cost for those likely eligible for subsidies and a 9.9 percent decrease among people likely not eligible for assistance. Using a Hausman test, the estimated coefficient on health insurance was not significantly different across the two groups. Health insurance did not significantly decrease not seeking care because of cost for those likely eligible for Medicaid.

In order to compare these results with the previous literature, we need to turn the estimates for the particular sub-sample gaining insurance into population-level estimates. An 11.4 percent reduction in forgoing care among the 5.7 percent of the population who gained insurance suggests a population-level impact of less than 1 percent decrease in forging care because of cost.

Robustness checks

We pool data from the BRFSS across years in which the survey was a landline-based survey only (2003-2010) and across years in which the survey was a combined landline and cellular phone based survey (2011-2012). The CDC cautions users that the BRFSS data from years with cell phones may not be directly comparable to years without the inclusion of cell phone survey members. We conduct analyses only on data prior to 2010 as a check for robustness. This did not substantively alter the main overall result, a finding that prior research has found (Van Der Wees et al., 2013).

The outcome we examine, forgoing care because of cost, has a 12-month look back period, which means some people may have obtained health insurance through the reform but responded to the question based on a period when they did not have health insurance. We conduct an additional analysis excluding the year after the reform and the overall result remain consistent.

Discussion

This study contributes to the literature about the extent to which health insurance removes financial barriers to accessing health care. Using the Massachusetts health reform for identification, we estimate the effect of gaining health insurance on an important, but often neglected, measure of health care affordability – whether insured individuals forgo care due to cost. We find significant decreases in forgoing care because of cost *among people who gained health insurance* through the Massachusetts health reform. In the overall sample, gaining health insurance was associated with an 11.4 percent decrease in forgoing care because of cost in the overall sample.

Previous studies that examined forgoing care because of cost have looked at the effect of Massachusetts health reform and the ACA on the *entire population*, not just those who gained insurance due to the reforms. Converting the local average treatment effect into a population-level effect, the converted result is a less than 1 percent decrease in forgoing care because of cost at the population level. Although the direction and significance is consistent with the earlier studies, our estimated magnitude is smaller. Previous work has estimated the population effect to be between a 2-4.8 percent decrease, depending on the population and health insurance expansion studied (Pande et al. 2011; Van Der Wees et al., 2013; Sommers et al., 2012; Sommers et al., 2014) .

As expected, the Massachusetts health reform is a strong predictor of gaining health insurance overall and across all income groups. The magnitude of the increase in health insurance coverage overall is 5.9 percent, which is similar to estimates previously reported by surveillance studies and provides external validity to our first stage findings. People *likely eligible for Medicaid* and *likely eligible for subsidies* experienced larger gains (15.5 percent and 9 percent increases) in health insurance compared to people *not likely eligible for assistance* who experienced only a modest increase in health insurance coverage (3.1 percent).

Our study is the first to examine forgoing care across income groups that correspond to assistance levels. When we examined the effect of heath insurance by income groups corresponding to assistance levels, we found the largest decrease in forgoing care, 15.5 percent, in the *likely subsidy eligible* group. We also found a 10 percent decrease in forgoing care in the group *likely not eligible for assistance*. These findings suggest that people who gained insurance were more likely to seek care when they felt they needed it. Another benefit of removing financial barriers to seeking care is that people may be more likely to get appropriate care. When people experience cost-sharing, they cut back on all care and do not discriminate between necessary or high value care and unnecessary or low value care (Manning, Newhouse, & Duan, 1987). The total welfare effects of reducing forgoing care because of cost may also include timely and appropriate medical care in addition to the financial benefits and peace of mind that comes from having coverage.

Despite the high level of financial assistance available for individuals with low incomes, there was not a statistically significant change in forgoing care because of cost among people who gained health insurance through the reform. These findings suggest that the provision of health insurance, even very generous coverage, may not be enough to eliminate financial barriers

to accessing care among people with very low incomes. While the BRFSS data limits our ability to discern the precise mechanism for the inability to help the very low income, we hypothesize these findings could be explained by five potential factors: (1) Medicaid churning; (2) remaining cost barriers in the design of the Medicaid program; (3) remaining access barriers with Medicaid coverage; (4) misunderstanding of Medicaid benefits among beneficiaries; and (5) defining "costs" to include non-financial barriers.

First, low income individuals and families often have volatile incomes over the course of the year (Acs, Loprest, & Nichols, 2009), leading to potentially inconsistent coverage during the year as they cycle in and out Medicaid eligibility. Simulations based on data from 2004 to 2008 demonstrate that about half of people with incomes below 200 percent of the poverty level will experience a shift in eligibility within a 12 month time period (Sommers & Rosenbaum, 2011). Churning has been recognized as a barrier to continuity of coverage and it carries high administrative costs and continues to be an issue under the ACA (Klein, Glied, & Ferry, 2005; Saunders & Alexander, 2009). Although the exchanges are meant to be a one-stop shop for gaining coverage, according to a news report, only a handful of states not including Massachusetts have implemented fully integrated systems to reduce churning (Andrews, 2015).

Second, in Massachusetts, Medicaid has no premiums and only small co-payments for prescriptions. However, results from the Rand Health Insurance Experiment demonstrate that incurring any cost associated with seeking care is a strong enough deterrent for some people to forgo care (Manning et al., 1987). Oregon's Medicaid program increased premiums ($6-$20 per month) and copays ($5 outpatient, $50 emergency department and $250 for an inpatient hospital admission) in 2003. One study examining the effects of the cost-sharing changes found no significant change in total expenditures per person, but there were shifts in treatment patterns

(Wallace et al., 2008). Administrative data showed a 46 percent decrease in enrollment in the 6 months after the cost-sharing changes. Beneficiaries reported leaving the program due to cost sharing and most of those who left did not have health insurance (Wright et al., 2005). Other studies suggest that copays reduce appropriate use of health services, particularly among people with low income (Solanki, Schauffler, & Miller, 2000; Solanki, Schauffler, & States, 1999; Wong et al., 2001)

Thirdly, following health reform in Massachusetts, 54 percent of family medicine physicians were not accepting new patients and the average wait time for new patients to see a family physician was 29 days in 2010 (Massachusetts Medical Society, 2010). Additionally, only a subset of primary care physicians accept Medicaid beneficiaries (Bodenheimer & Pham, 2010). With shortages and reduced options, people with Medicaid may have had to forgo care or pay for services from a provider who did not accept Medicaid.

A fourth potential explanation for the insignificant reduction in forgone care among the very-low income population is the complexity of learning and understanding insurance products. According to a nationally representative survey of American adults, only 12 percent of American adults had proficient health literacy, defined in part by the ability to calculate an employee's share of health insurance costs for a year based on a table of information (Kutner, et al., 2006). Several studies report that people from a variety of backgrounds have limited understanding health insurance concepts such as deductibles, copays, coinsurance, and maximum out-of-pocket costs (Loewenstein et al., 2013; Politi et al., 2014; Wong et al., 2015). People also tend to overestimate how well they understand these concepts (Long & Dahlen, 2014). New Medicaid beneficiaries may have forgone care to avoid cost because they were not aware that out-of-pocket costs with Medicaid are minimal in comparison to private insurance.

Lastly, when people answered the question, "Was there a time in the past 12 months when you needed to see a doctor but could not because of cost?" they may have responded to the question by interpreting costs broadly. As they navigate the health care system, people experience continual stigmatization for needing public assistance (DeVoe et al., 2012). The doctor shortages may have led to extremely long wait-periods, and the time cost that was too high for beneficiaries to seek care.

Our analysis has limitations. We use data from 2011 and 2012, which are years that included cell phone surveys. The CDC cautions against pooling BRFSS data across landline only years and years with cell phone surveys. We conducted a robustness check to examine this potential issue and we find that our overall result does not change. Our quasi-experimental approach addresses some of the endogeneity in the relationship between health insurance and forgoing care because of cost as demonstrated by the instrument improving balance in health insurance status. We are confident that the instrumental variables estimates are less biased than the naive estimation. If the reform impacted the intensive margin or if health system changes from the reform that influenced forgoing care because of cost, the instrumental variables estimates are biased upward.

In conclusion, we find a significant reduction in forgoing care because of cost among people who gained health insurance due to Massachusetts health reform. There is substantial heterogeneity in the reduction in forgoing care due to the cost, illustrating that that Massachusetts health reform benefited segments of the population but did not alleviate perceived costs of care for people with the lowest incomes. Future work should pay attention to this heterogeneity in effects based on income, as well as identify why forgone care remains among these individuals with the lowest incomes.

Table 1. Variable means, means by health insurance status and by the IV, MA*Reform

	Overall	No HI	HI	Adj. Wald	MA*Reform =0	MA*Reform =1	Adj. Wald
Outcome				Prob > F			Prob > F
forgoing care	0.100	0.397	0.067	0.00	0.106	0.086	0.00
Covariates							
age 18-24	0.136	0.237	0.125	0.00	0.139	0.130	0.01
age 25-29	0.086	0.140	0.080	0.00	0.087	0.083	0.07
age 30-34	0.113	0.129	0.112	0.00	0.115	0.111	0.07
age 35-39	0.112	0.092	0.114	0.00	0.113	0.109	0.11
age 40-44	0.137	0.112	0.140	0.00	0.134	0.144	0.00
age 45-49	0.116	0.088	0.120	0.00	0.117	0.115	0.35
age 50-54	0.122	0.083	0.127	0.00	0.121	0.126	0.00
age 55-59	0.096	0.064	0.100	0.00	0.096	0.097	0.31
age 60-64	0.080	0.054	0.083	0.00	0.079	0.083	0.00
female	0.508	0.410	0.520	0.00	0.507	0.511	0.35
married	0.586	0.322	0.616	0.00	0.591	0.575	0.00
under $10,000	0.029	0.071	0.024	0.00	0.028	0.030	0.22
$10,000-15,000	0.025	0.063	0.021	0.00	0.025	0.027	0.10
$15,000-20,000	0.038	0.107	0.031	0.00	0.037	0.043	0.00
$20,000-25,000	0.051	0.126	0.042	0.00	0.049	0.056	0.00
$25,000-35,000	0.069	0.136	0.062	0.00	0.071	0.066	0.00
$35,000-50,000	0.111	0.134	0.108	0.00	0.116	0.099	0.00
$50,000-75,000	0.157	0.092	0.164	0.00	0.164	0.140	0.00
$75,000 & up	0.403	0.097	0.438	0.00	0.395	0.424	0.00
income missing	0.117	0.174	0.110	0.00	0.117	0.117	0.92
no degree	0.069	0.191	0.055	0.00	0.067	0.074	0.00
high school grad	0.243	0.372	0.228	0.00	0.247	0.231	0.00
some college	0.251	0.253	0.250	0.56	0.252	0.248	0.17
college degree	0.438	0.183	0.466	0.00	0.433	0.448	0.00
employed	0.645	0.482	0.664	0.00	0.643	0.650	0.04
self-employed	0.093	0.157	0.085	0.00	0.095	0.088	0.00
unemployed	0.071	0.196	0.057	0.00	0.068	0.078	0.00
homemaker	0.057	0.050	0.058	0.00	0.061	0.049	0.00
student	0.057	0.057	0.057	0.86	0.058	0.056	0.62
retired	0.032	0.016	0.034	0.00	0.033	0.027	0.00
unable to work	0.045	0.042	0.045	0.10	0.042	0.051	0.00
white	0.810	0.627	0.832	0.00	0.824	0.778	0.00
black	0.046	0.076	0.042	0.00	0.043	0.053	0.00
Hispanic	0.086	0.235	0.069	0.00	0.084	0.090	0.00
other race	0.058	0.063	0.058	0.08	0.050	0.078	0.00
Observations	229,491	21,358	208,133		156,530	72,961	

Note: Means are weighted by BRFSS person-level weights

Table 2: Naïve probit regression of health insurance on not seeking care because of the cost by income group

	Full sample	Likely medicaid eligible (income <150% FPL)	Likely subsidy eligible (150% FPL<income<300% FPL)	Likely no assistance (income>300% FPL)
Incremental effect of health insurance	-0.208***	-0.304***	-0.278***	-0.181***
Analytic S.E.	0.010	0.017	0.019	0.010
p-value	0.000	0.000	0.000	0.000
Observations	229,491	33,941	41,117	154,433

Notes: All specifications include binary controls for sex, age groups, married, education levels, employment categories, race and ethnicity, state, year and month fixed effects, state level housing index and unemployment and are estimated using population weights. Standard errors clustered at the state level. * Significantly different at the 10% level; ** at the 5% level; *** at the 1% level.

Table 3: Instrumental variables estimates of health insurance (HI) on not seeking care because of the cost by income

	Full sample	Likely medicaid eligible (income <150% FPL)	Likely subsidy eligible (150% FPL<income<300% FPL)	Likely no assistance (income>300% FPL)
First stage incremental effect IV (MA*Reform) on HI	0.057***	0.151***	0.089***	0.031***
Analytical S.E.	0.000	0.008	0.002	0.001
p-value	0.000	0.000	0.000	0.000
F-stat	4358.71	313.02	2,418.79	557.91
Mean of not seeking care	0.104	0.241	0.168	0.057
Incremental effect of HI	-0.114***	-0.030	-0.150**	-0.099***
Bootstrap S.E.	0.023	0.087	0.075	0.029
p-value	0.000	0.731	0.045	0.001
Observations	229,491	33,941	41,117	154,433

Notes: All specifications include binary controls for sex, age groups, married, education levels, employement categories, race and ethnicity, state, year and month fixed effects, Law, MA*Law, Reform, state level housing index and unemployment and are estimated using population weights. The full sample includes income categories. Standard errors clustered at the state level. * Significantly different at the 10% level; ** at the 5% level; *** at the 1% level.

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CHAPTER 4

Instrumental variable model selection with a binary treatment variable

Abstract

There are two broad approaches to implementing instrumental variables, two-stage least squares (2SLS) and two-stage residual inclusion (2SRI). Although linear models do not typically fit binary data well, some researchers are hesitant to make functional form and distributional assumptions. Choosing between these approaches in an applied setting is a relatively underexplored area. We report the local average treatment effect (*LATE*) from several different instrumental variables models and we explore whether goodness-of-fit can assist the researcher in identifying an appropriate model for the data. Within our case study, the results demonstrate that the *LATE* is sensitive to model choice. We conclude that goodness-of-fit visualizations are useful in choosing between the 2SLS and 2SRI approaches.

Introduction

Instrumental variables approach is the primary statistical application that addresses endogeneity, also called unmeasured confounding or hidden bias. Instrumental variables are used when the covariate of interest is endogenous to the relationship with the outcome variable. Endogeneity occurs when the variable of interest is correlated with the error term for any reason; examples include omitted variables, reverse causality, and misspecification. A model that does not address the endogeneity will produce biased estimates of the effect of the treatment on the outcome. When the assumptions of the instrumental variable models are met, instrumental variables can produce an unbiased, causal estimate of the effect of a treatment on an outcome using observational data.

Instrumental variables use a set of two equations or stages. The causal pathway of instrumental variables is that an exogenous instrument predicts the endogenous variable, which then predicts the outcome. In order for the instrument to produce a causal effect, the instrument must meet two criteria. First, it must be strongly predictive of the endogenous treatment variable, which is testable. Second, it must be completely independent of the error terms, which is not statistically testable, leaving researchers to use logical arguments to defend this assumption. Conceptually, instrumental variables approach divides the endogenous variable into two parts: a part that can be explained with an exogenous instrumental variable and the endogenous part. Then only the uncorrupted part of the variable is used to estimate the relationship between the treatment and the outcome.

The two basic approaches for implementing instrumental variables with binary variables include two-stage least squares (2SLS) and two-stage residual inclusion (2SRI). Within the 2SRI approach, there are several different types of first-stage residuals that can be estimated and then

used in the second-stage. Moreover, within a given residual type, the residual can be entered into the second stage a variety of ways (as calculated, squared or another polynomial, or interacted with the treatment variable). This leaves the applied researcher with a host of modeling options.

Both of 2SLS and 2SRI approaches were developed in the linear setting with continuous endogenous treatment variables and continuous outcomes. Within the public health and medical fields, treatments and outcomes are often binary. Questions arise in the application of these methods to non-linear settings, including the situation with a binary endogenous treatment variables and/or binary outcomes. When the outcome is binary, the analogous approach to 2SLS, called two-stage predictor substitution, is to substitute the fitted values from the first stage into the binary second state. There is evidence in the literature that two-stage predictor substitution produces biased estimates and 2SRI is preferred (Blundell and Powell, 2003, 2004; Terza et al., 2008).

The case for which approach to use for a binary endogenous treatment is unfolding in the literature. There remains debate regarding which modeling approach is more appropriate. The challenges arise because both modeling approaches have drawbacks. There are problems of fitting linear models (as in 2SLS) to binary data. Conversely, with 2SRI approaches, the treatment effects are not non-parametrically identified, meaning that counterfactuals are estimated through extrapolation. Those in favor of the linear approach, 2SLS, cite that ordinary least squares (OLS) gives a minimum mean squared error to the conditional expectation function and are opposed to distributional assumptions and committing to a functional form (Angrist and Pischke, 2009). Others favor the non-linear approach, noting that 2SRI linear probability models also impose functional forms on the data when there is more than one covariate and that linear models might not provide good fit to the data (Basu and Coe, 2016).

Recently, Basu and Coe (2016), examine the specific case where the local average treatment effect (LATE) is identified using a binary treatment and provides some guidance based on the rarity of the binary treatment variable and binary outcome. The authors vary the rarity of the treatment and outcome to perform simulations to determine which models have the least bias in estimating the LATE. Based on their simulations, the authors recommend against the use of 2SLS for studies where the binary treatment mean deviates from 50%. When the treatment mean is 15 percent or higher and the outcome mean is 20 percent or higher, simulations support the choice of 2SRI with raw scale residuals. In the circumstances of a relatively rare treatment (less than 15 percent) or outcome (less than 20 percent), simulations support the choice of 2SRI with anscombe residuals. However, the underlying phenomenon that leads one residual to perform better than the other remain underexplored.

The consequences of choosing a particular model from among the instrumental variables approaches are not clear. One study has estimated the impact of model choice on treatment effects. Using health care cost data, Garrido et al. (2012) compared results from 2SLS and 2SRI models with different versions of the residuals. They report that both the local average treatment effect and treatment effect calculated at the median varied by the type of residual used in the second stage, pointing out that the results vary drastically across models. However, the authors do not provide an explanation for the variation in the estimates nor make clear recommendations for determining model selection.

In this paper, building on Basu and Coe (2016), we study whether assessing goodness-of-fit to data can explain the simulation results found in Basu and Coe (2016) and help guide researchers in choosing between alternatives methods. We use a case study to illustrate these methods and accompanying goodness-of-fit visualizations. We examine the effect of health

insurance on forgoing care because of cost, using Massachusetts health reform as an instrument. We estimate the local average treatment effect for a variety of models. Then we consider whether goodness of fit visualizations can provide enough distinguishing information across models to motivate model selection.

Methods

Theoretical model

Let us begin with y_i, a binary response model. One or more covariates within xi, called di, are jointly determined by y_i. Therefore, di is not independent of yi and is endogenous to the relationship between xi and y_i.

$$X_i = (d_i, w_i)$$

$$yi = 1\{d_iB_i + w_iB_2 + u_i > 0\}$$

The reduced form of the endogenous treatment variable, di, can be given as:

$$d_i = E(d_i| w_i, z_i) + v_i$$

$$d_i = \lambda (w_i, z_i) + v_i$$

The expected value of v_i conditional on w_i and z_i is equal to zero by construction. Z_i is a vector of instrumental variables. Typically, researchers are interested in estimating the average treatment effect (ATE) given by:

$$ATE (w_i) = G(B_1 + w_iB_2) – G(w_iB_2)$$

The ATE is the mean change in the outcome by subtracting the mean of assigning everyone in the population no treatment from the mean of assigning everyone in the population the treatment. The treatment effect of w_i varies over the error term, u_i. In order to estimate the ATE, the full parametric distribution of u_i is necessary. However, instrumental variables can only identify the effect of u_i given v_i. In the special case we are considering, when the instrumental

variable is binary, u_i given v_i is identified non-parametrically. This is the case because there are only two outcomes, zero and one, and there is no overlap in the residual distribution for those who receive treatment and those who do not. In the binary case only two points are used to estimate the distribution of $u_i|v_i$. In the binary case, the instrumental variable effect gives rise to the local average treatment effect (Imbens and Angrist, 1994), which can be written as:

$$LATE(w_i) = (dE(y_i|w_i)/dz_i) / (dE(d_i|w_i,z_i)/dz_i)$$

The LATE is the effect of the treatment, di, on the probability of y_i but only for individuals who are induced to change their treatment status due to a change in the level of the instrumental variable. If the instrument is a binary variable that represents a specific policy change, then the LATE can be interpreted as the effect of the policy on the outcome among people affected by the policy change (Heckman et al., 2006).

Next we will discuss how these two approaches are implemented to estimate the effect on the change in probability of the outcome due to an exogenous change in the treatment, di, due to a change in the instrumental variable.

Two-Stage Least Squares (2SLS)

In 2SLS, the first stage is a linear model estimating the effect of the instrument on the endogenous treatment variable. The set of covariates that are included in the outcome model are also included in the treatment model. Then the predicted probabilities from the first stage are entered into the estimation of the outcome model in place of the endogenous variable. The coefficient on the fitted values is the *LATE* and is interpreted as the effect of the endogenous variable on the outcome. The 2SLS approach can be implemented in two separate steps using ordinary least squares (OLS), however; the standard errors are only correct when the procedure is estimated simultaneously using statistical software packages.

Two-Stage Residual Inclusion (2SRI)

In 2SRI, the first stage is the same as in 2SLS. In the second stage, the residual from the first stage is included as a covariate in the second stage alongside the endogenous variable and other covariates. The coefficient on the endogenous treatment variables is interpreted as the *LATE*.

2SRI approach relies on the theory of control functions. The control function approach is a broad approach for addressing endogeniety. Control function theory states that a function of the residuals from the treatment equation exists, called the control function that can adjust for endogeniety in the outcome equation. It should be noted that the theory does not specify which function of the residuals, as there are many, achieves this result. The 2RSI method is one application of the control function theory. However the 2SRI approach was developed in the setting of a continuous endogeneous treatment variable (Blundell and Powell, 2003). The 2SRI approach within the context of a binary endogenous variable could be considered a misapplication of the theory because the treatment effect is not identified non-parametrically; the residuals are all positive for observations with treatment equal to one and all negative for observations with treatment equal to zero. There is no overlap in the distribution of the residuals. The counterfactuals are obtained though extrapolation are based on the functional form and the level of the residuals. Since treatment effects are estimated through extrapolation, we argue that the data ought to fit the observed data well in order to predict counterfactual outcomes and generate reliable treatment effects.

Empirical application

In order to compare the influence of the choice of the estimation method, we examine whether gaining health insurance through Massachusetts health reform effects not going to the

doctor because of the cost. The population level effect of Massachusetts health reform on forgoing care has been previously studied using difference-in-differences (Pande et al., Van der Wees, Zaslavsky, & Ayanian 2013). One study examines the effect of health insurance on forgoing care due to the cost using instrumental variables (Barnett et al). The study uses the policy change, Massachusetts health reform, as an instrument for health insurance. This case study has the benefit of having a relatively rare outcome, forgoing care because of the cost and a common treatment, having health insurance. Even prior to Massachusetts health reform, Massachusetts and surrounding New England states had relatively low rates of uninsurance.

Data

We use data from the Behavioral Risk Factor Surveillance System, a large nationally and state representative cross-sectional annual survey of population health. We restrict the sample to people living in Massachusetts and three New England states, Connecticut, New Hampshire and Rhode Island. We further restrict the sample to non-elderly adults, ages 18-64, the population targeted by health reform. The study uses data from before the reform was implemented (January 2003-June 2007) and after the reform was implemented (July 2007-2012). Means for the data are presented in Table 1.

Measures

The outcome variable, *forgoing care*, equals one if survey respondents answered affirmatively to the question, "Was there a time in the past 12 months when you needed to see a doctor but could not because of cost?". Health insurance is the variable of interest and is binary. Health insurance equals one if survey respondents answered affirmatively to the question, "Do you have any kind of health care coverage, including health insurance, prepaid plans such as HMOs, or government plays such as Medicare, or Indian Health Service?". Independent

variables include binary variables for sex, age categories, race and ethnicity, marital status, employment status, educational levels and income categories. State, year and month fixed effects, and two state-level measures, housing prices and unemployment are included. A binary variable, *law*, representing the time period after passing the law but before it was implemented (March 2006-June 2007) is included. A binary variable for the implementation of health reform (July 2007-2013), *reform*, which is when the individual mandate went into effect is also included. These time period variables are interacted with a binary variable for Massachusetts to create *MA*law* and *Ma*reform*.

Instrumental variable

The variable representing health reform, an interaction term between living in Massachusetts and the post-policy period, *MA*reform*, is used as instrument for health insurance status, which is a binary variable equal to one for those reporting having any kind of health care coverage. Instrumental variables approach has two requirements in order for the instrument to be considered strong and valid. The first is that the instrument, *MA*reform*, is a strong predictor of health insurance coverage, a testable assumption. The instrument is statistically strong. The first stage results indicate that *MA*reform* has a positive and significant ($p<0.01$) relationship with health insurance coverage with an F-statistic 4,359 (result not shown), well over the recommended threshold (Staiger & Stock, 1997; Stock & Yogo, 2005). The second requirement is that the instrument should be exogenous or unrelated to the outcome, *forgoing care,* other than through the endogenous variable. The validity of the instrument would be violated if the reform impacted forgoing care through the extensive margin. This would occur if, for example, the cost barriers for receiving care other than health insurance were changed. The main provision of the reform provided sliding-scale subsidies for health insurance but did not address the overall cost

of care for people that already had insurance through their employer or another government program. Thus we feel that the second assumption likely holds.

Model comparisons

In order to compare the results of estimates from 2SLS and 2SRI with different versions of the residuals, we present the *local average treatment effect* and visualizations of goodness of fit. As mentioned earlier, the *LATE* estimated through an instrumental variable in the binary setting has an intuitive interpretation when the instrument is a policy change. The *LATE* is the effect of the variable of interest on the outcome among people induced by the instrument to change levels of the treatment variable. The *LATE* in this application is the incremental effect of gaining health insurance on forgoing care because of the cost *among people who gained health insurance due to Massachusetts health reform and the individual mandate*. Standard errors for the incremental effects of health insurance on forgoing care are calculated using 1,000 bootstrap replicates.

We also compare the results of estimations from differing models using visualizations of goodness of fit. We first conduct a Hosmer-Lemeshow goodness of fit visualization (results not shown). After the estimation, we obtain the linear predictor, xb, for each observation. The data are then ordered according to their linear predicted means and partitioned into groups, often 10 equally sized groups or deciles. Since we have a relatively large number of observations, we choose to use 100 quantiles or percentiles. The first quantile of the data will include the observations with the lowest linear predictor of forgoing care. The last quantile of the data will include the observations with the highest linear predictor of forgoing care. We then plot the mean residuals within each quantile of the predicted mean. If the estimation fits the data well, then the mean residuals will be hug to the zero line, indicating that the predicted mean within a given

quantile is close to the observed mean. Another indication that the estimation fits the data well is if the residuals have a zig-zag pattern, demonstrating that there is no systematic over-prediction or under-prediction. If the residuals trend up or trend down across percentiles of the predicted means, that is an indication of a curvilinear pattern which is not accommodated by the model, suggesting a lack of goodness of fit.

The second visualization technique we use a plot of the percent bias across percentiles of the predicted means. Within each quantile of the linear predictor, the percent bias is calculated by taking the mean residual, multiplying by 100, and dividing by the mean of the linear predictor. The percent bias allows the researcher to compare the percentages, or magnitudes, of bias across models. The expectations for how the plot pattern will look are the same as discussed above.

The third visualization technique plots the predicted means versus the observed means across quantiles of the residual from the first state, separately for negative and positive residuals. The figure also includes a 45-degree line. Since the residuals are all positive for observations where health insurance is equal to one and all negative for observations where health insurance is equal to zero, we plot these two scenarios separately. Better predictions will be close to the 45-degree line, representing perfect prediction, and there should not be any systematic pattern in the residuals. In each of the visualizations, we overlay formulations of the residual, including first-degree polynomial, second-degree polynomial, and first-degree polynomial interacted with the treatment, health insurance, on one figure within a type of control function.

Results

Table 2 presents the *LATE* or incremental treatment effect of health insurance on forgoing care because of the cost among people who gained health insurance through health

reform. Some of the treatment effects are not significant while others are highly significant and magnitudes of the incremental treatment effects vary substantially. The effect of health insurance on forgoing care is not significant using the raw scale residual, regardless of how the residual enters the model. Similarly, the treatment effect is not significant using the second-degree polynomial using anscombe exposure residuals and the first and second-degree polynomial using deviance exposure residuals. The treatment effect is significant but smallest in magnitude using the deviance exposure residual interacted with the treatment variable. Anscombe exposure residuals, generalized exposure residuals and pearson residuals produce statistically significant estimates of the treatment effect ranging from -0.114 to -0.237.

The two-stage least squares yields a significant estimate of -0.578, which is more than twice the size of the largest estimate among the two-stage residual inclusion approaches. The interpretation of this LATE is that gaining health insurance reduces forgoing care because of cost by 57.8 percentage points. The mean of forgoing care in the overall sample is 10 percent and is 39.7 percent among those that do not have health insurance. Thus the 2SLS treatment effect appears implausible.

Next we turn to visualizations of goodness of fit to see whether differences in goodness of fit across these models can explain some of the differences we see in the magnitudes of the treatment effects with the ultimate goal of informing model selection. The first plots of the mean residuals across quantiles of the predicted means, which is a classic Hosemer-Lemeshow visualization. Several of the plots appear to be very similar so we do not present them. Instead we present the percentage bias figures, which are more informative. We present the percentage bias figures for the overall sample (Figures 1.1 through 1.6). Then we separate the estimates of

bias by treatment, health insurance equal to zero (Figures 2.1 through 2.6) and health insurance equal to one (Figures 3.1 through 3.6).

The percent bias figures of the 2SLS model demonstrate that 2SLS not fit the data (see Figures 1.1, 2.1 and 3.1). In the overall figure (Figure 1.1), the percent bias is almost 300% at low quantiles of the linear predictor. 2SLS under-predicts at lower levels of the linear predictor and then over-predicts at higher levels of the linear predictor. There is a clear curvilinear pattern.

Within the 2SRI approaches, overall fit is similar whether the residual is first-order polynomial, second-order polynomial or first-order polynomial interacted with the treatment, health insurance. Looking at the overall percent bias figures (Figures 1.2 through 1.6), there appears to be a slight inverted u-shape in higher levels of the linear predictor. In the bias figure of the raw residual (Figure 1.2), there appears to be a slight under-prediction at the highest levels of the linear predictor. We look at the bias figures by treatment for further investigation. On the whole, there does not appear to be meaningful differences between the 2SRI models by type of residual. Each has a zig-zag pattern, though they each have some curvilinear pattern in upper quanitiles of the linear predictor.

In the final set of figures, we examine the predicted means versus the observed means by quantiles of the residual, separately for health insurance equal to zero (Figures 4.2 through 4.6) and health insurance equal to one (Figures 5.2 through 5.6). The predicted mean versus observed mean look very similar across levels of the residual for health insurance equal to zero. The 2SRI models continue to look qualitatively similar. The plot of the generalize exposure residual for health insurance equal to zero shows that there is a cluster of predictions; however, a curvilinear pattern is not present.

Discussion

The use of instrumental variables approach with binary treatments and outcomes are common across many fields, and are particularly common in the public health and medicine. The current study confirms, within the binary treatment and binary outcome context, the main finding by Garrido et al. (2012) that treatment effects vary across models selection. The significance and magnitude of the *LATE* varies substantially across models. This sensitivity to model selection illuminates the challenge of choosing the "right" model and also calls into question the results of studies that use instrumental variables methods with a binary treatment and binary outcomes that do not consider more than one modeling approach.

We use visualizations of goodness of fit to evaluate the models. The goal of using goodness of fit visualizations is to explore whether these figures can assist in model selection. In the 2SRI approach, it is important that the model fit the data well because counterfactual outcomes are extrapolated based on level of the residual. Therefore, the behavior of the residuals is key to the extrapolation and the estimated treatment effect. If the predicted means and residuals do not fit the data well, then they will not provide appropriate information for the extrapolation of counterfactual outcomes.

Based on the figures we generate, we conclude with confidence that the 2SLS model has considerable bias, as expected. The treatment effect is very large and we believe it is implausible. The goodness-of-fit visualizations confirm that the 2SLS does not fit the data well and is very biased. Both the treatment effect and goodness-of-fit visualizations indicate that the 2SLS is not appropriate in this application.

We saw differences in the treatment effects produced by the 2SRI. Unfortunately we do not see qualitative differences in the goodness-of-fit visualizations that distinguish between the set of models reporting small and statistically insignificant treatment effects (raw and deviance

residuals) versus the models reporting moderate and significant treatment effects (pearson, genearalized and anscombe). We concluded that the 2SRI approaches provide better fit to the data. One type of residual's performance was not overtly superior to each other. However, goodness-of-fit measures were very informative for comparing the 2SLS and 2SRI models and and driving selection between these two choices.

The goodness-of-fit visualizations we implement do not produce a definitive answer for which model is best within the present example. Future work should consider alternative goodness-of-fit measures and consider whether slight differences in the magnitude of residuals are driving differences in treatment effects. In the cases where there is still uncertainty regarding which model best fits the data, researchers should consider averaging across models.

Table 1. Variable means

	Mean	SE
Covariates		
age 18-24	0.136	(0.002)
age 25-29	0.086	(0.001)
age 30-34	0.113	(0.001)
age 35-39	0.112	(0.001)
age 40-44	0.137	(0.001)
age 45-49	0.116	(0.001)
age 50-54	0.122	(0.001)
age 55-59	0.096	(0.001)
age 60-64	0.080	(0.001)
female	0.508	(0.002)
married	0.586	(0.002)
under $10,000	0.029	(0.001)
$10,000-15,000	0.025	(0.001)
$15,000-20,000	0.038	(0.001)
$20,000-25,000	0.051	(0.001)
$25,000-35,000	0.069	(0.001)
$35,000-50,000	0.111	(0.001)
$50,000-75,000	0.157	(0.001)
$75,000 & up	0.403	(0.002)
income missing	0.117	(0.001)
no degree	0.069	(0.001)
high school grad	0.243	(0.001)
some college	0.251	(0.001)
college degree	0.438	(0.002)
employed	0.645	(0.002)
self-employed	0.093	(0.001)
unemployed	0.071	(0.001)
homemaker	0.057	(0.001)
student	0.057	(0.001)
retired	0.032	(0.000)
unable to work	0.045	(0.001)
white	0.810	(0.001)
black	0.046	(0.001)
Hispanic	0.086	(0.001)
other race	0.058	(0.001)
Outcome		
forgoing care	0.100	(0.001)
Treatment		
health insurance	0.898	(0.001)
Observations	229,491	

Table 2. Local average treatment effects from different estimators

Estimators	LATE	Bootstrapped standard errors
2SLS	-0.578***	0.007
2SRI with the following CF:		
Raw scale exposure residual		
First-degree polynomial	-0.021	0.023
Second-degree polynomial	-0.022	0.023
First-degree polynomial#health insurance	-0.019	0.211
Pearson exposure residual		
First-degree polynomial	-0.206***	0.009
Second-degree polynomial	-0.160***	0.019
First-degree polynomial#health insurance	-0.237***	0.014
Deviance exposure residual		
First-degree polynomial	-0.041	0.028
Second-degree polynomial	-0.040	0.038
First-degree polynomial#health insurance	-0.081**	0.038
Generalized exposure residual		
First-degree polynomial	-0.181***	0.019
Second-degree polynomial	-0.147***	0.021
First-degree polynomial#health insurance	-0.106***	0.022
Anscombe exposure residual		
First-degree polynomial	-0.114***	0.023
Second-degree polynomial	-0.047	0.036
First-degree polynomial#health insurance	-0.156***	0.034

2SRI conducted with probit regression in first-stage and probit regression in second stage. CF: Control function. All CF are constructed based on first-stage (exposure/treatment) residuals. #health insurance indicates interaction with exposure/treatment variable in the second stage. **signficant at p-value<0.05, ***significant at p-value<0.01

Figure 1.1 2SLS res: % bias

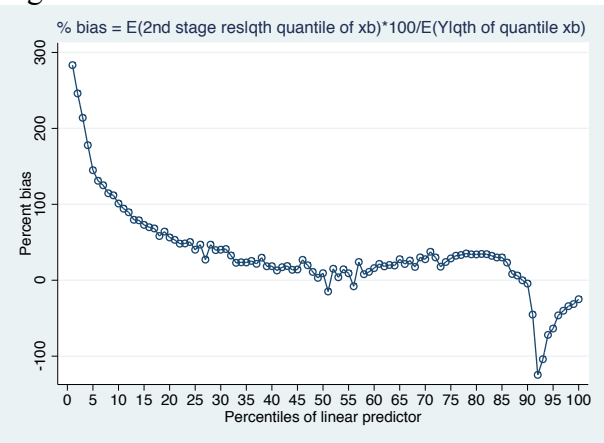


Figure 1.2. 2SRI raw res: % bias

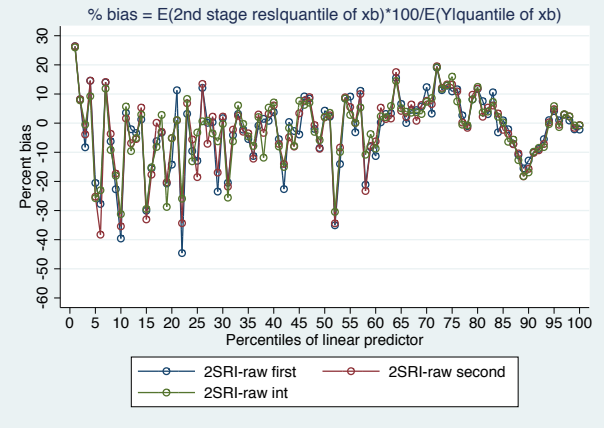


Figure 1.3. 2SRI pearson res: % bias

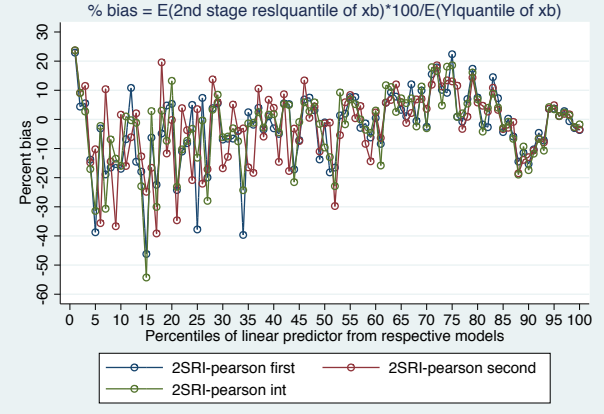


Figure 1.4. 2SRI deviance res: % bias

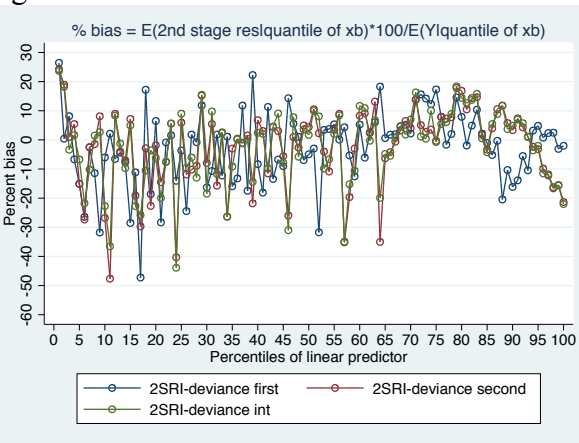


Figure 1.5. 2SRI gen res: % bias

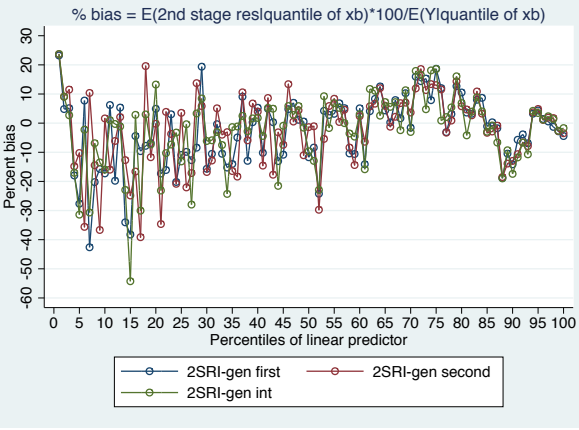


Figure 1.6. 2SRI ans res: % bias

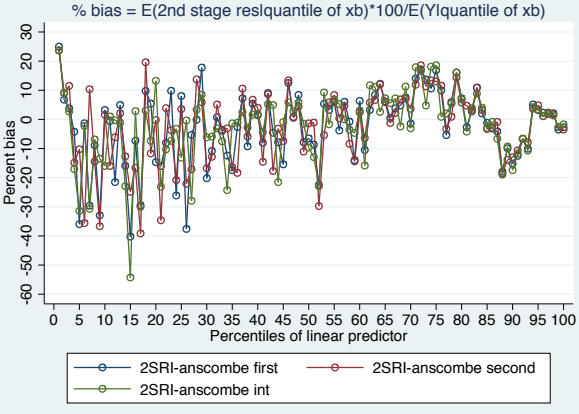
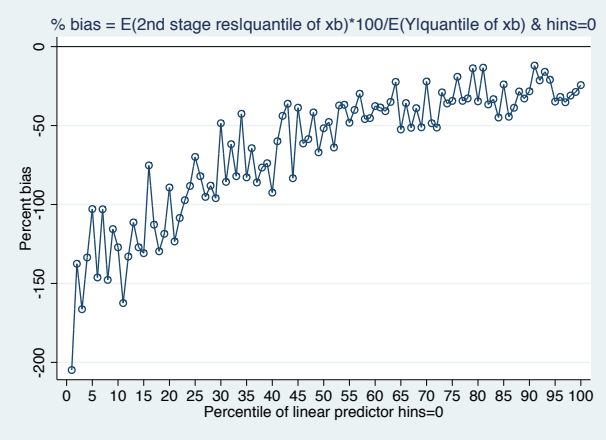


Figure 2.1. 2SLS res: % bias for hins=0



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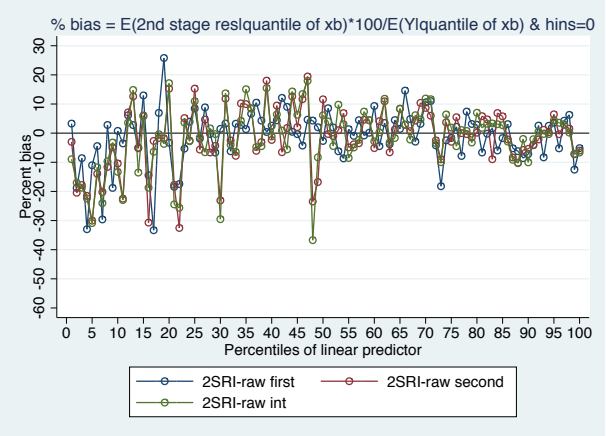

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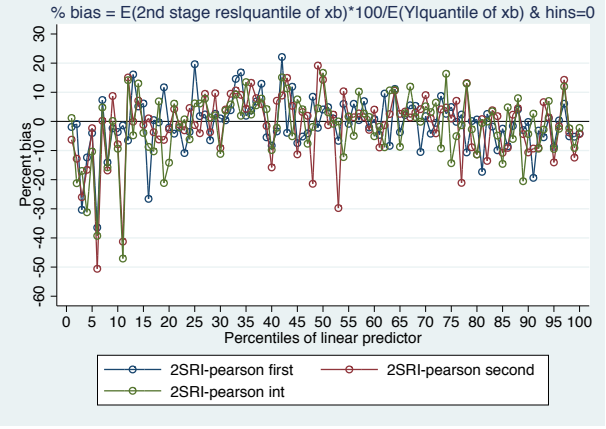

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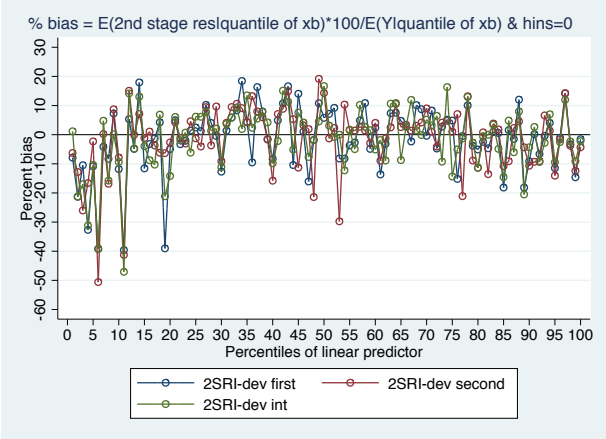

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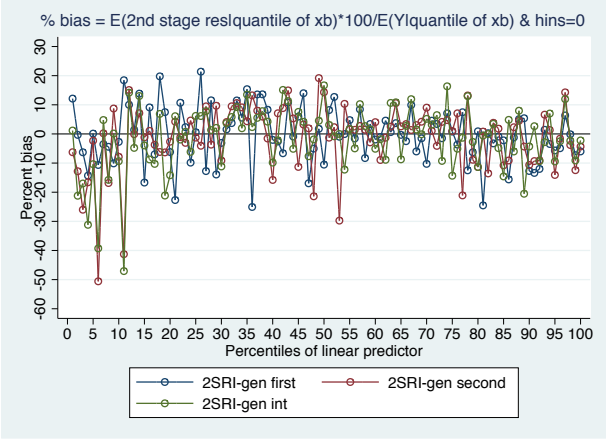


Figure 2.6. 2SRI ans res: % bias for hins=0

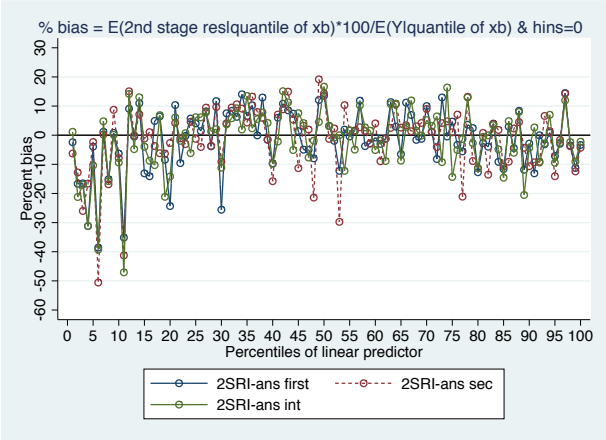
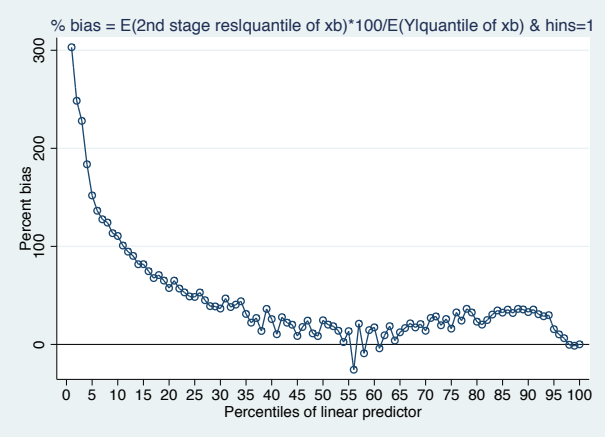


Figure 3.1. 2SLS res: % bias for hins=1



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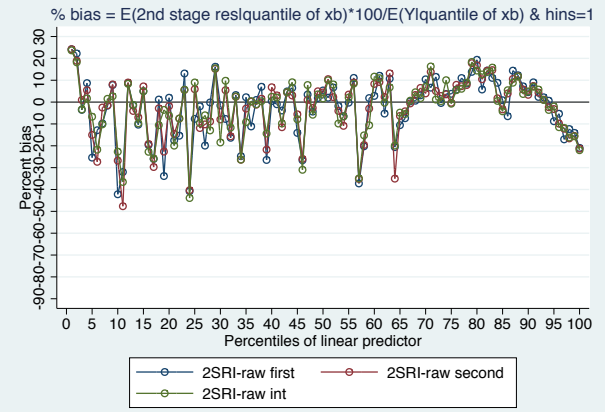
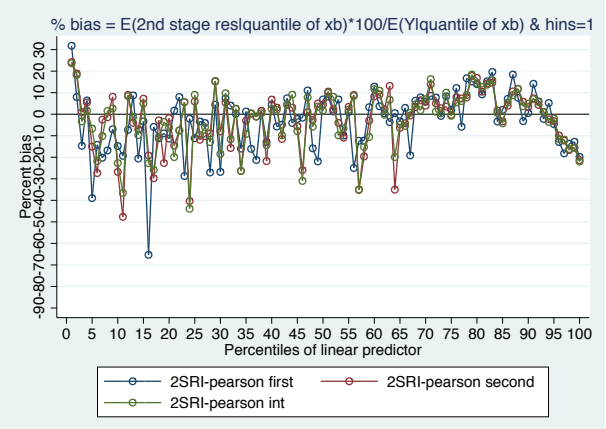


Figure 3.3. 2SRI pearson res: % bias for hins=1



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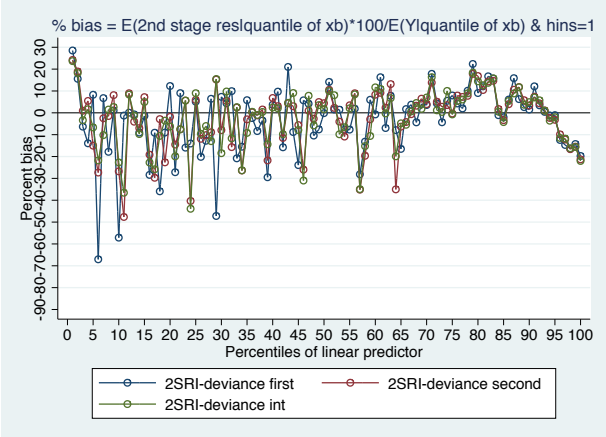

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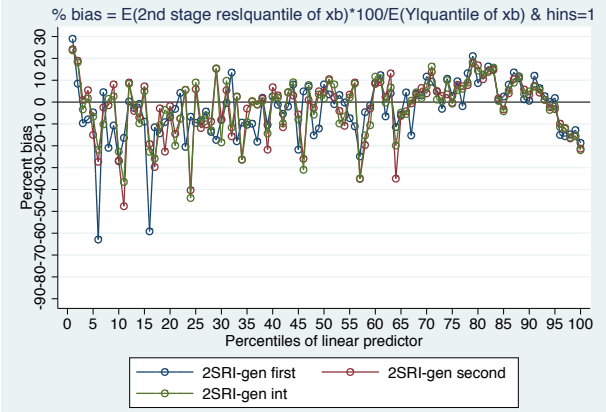

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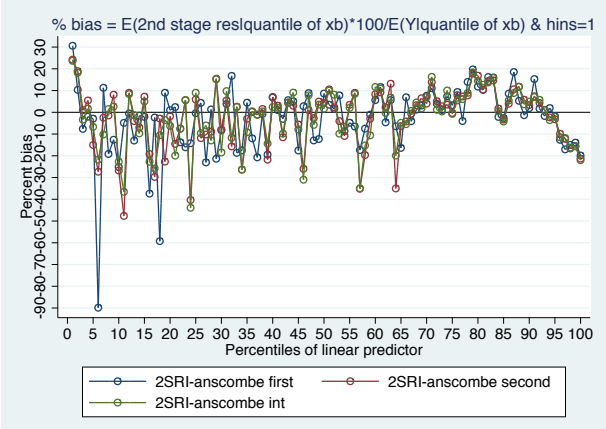
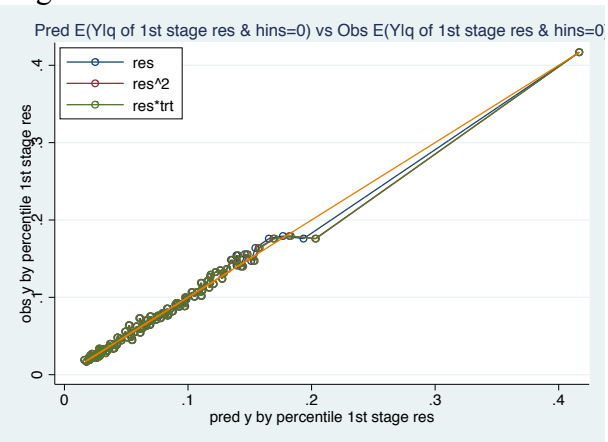


Figure 4.2. 2SRI raw res: pred vs. obs by 1st stage res & hins=0



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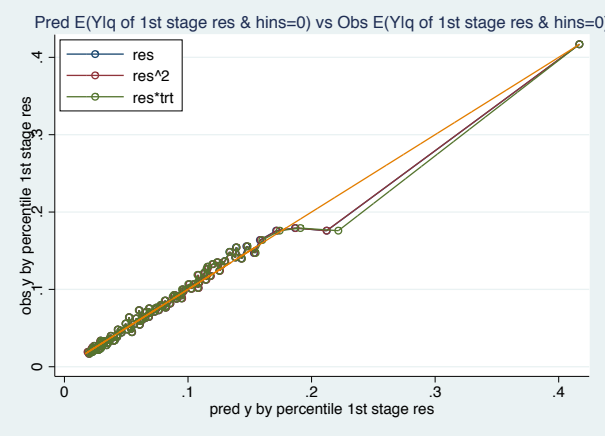


Figure 4.4. 2SRI deviance residual: pred vs. obs by 1st stage res & hins=0

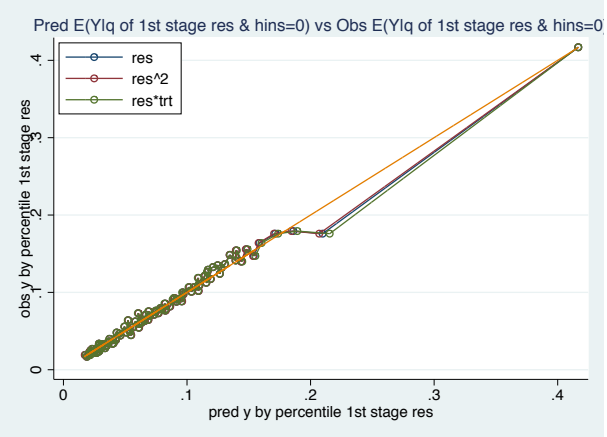


Figure 4.5. 2SRI gen residual: pred vs. obs by 1st stage res & hins=0

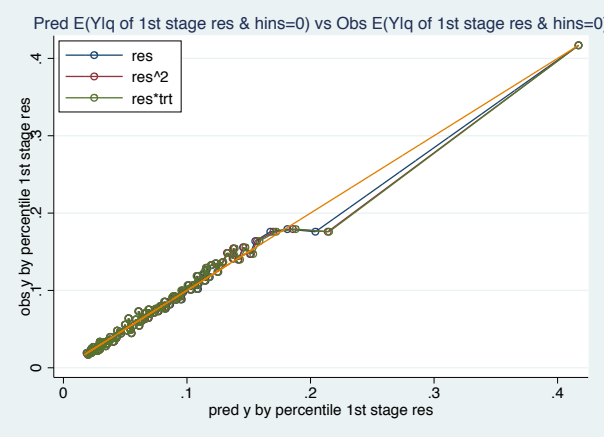


Figure 4.6. 2SRI ans res: pred vs. obs by 1st stage res & hins=0

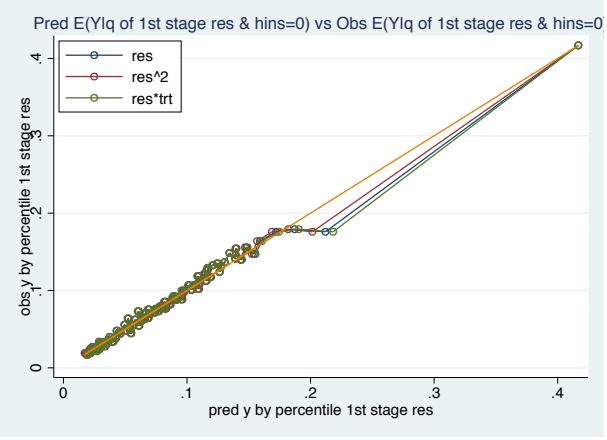
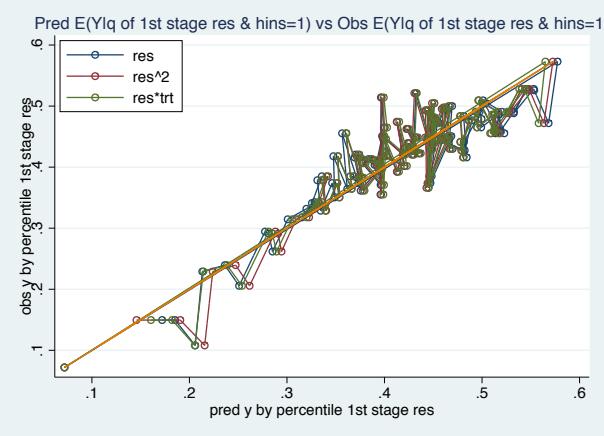


Figure 5.2. 2SRI raw res: pred vs. obs by 1st stage res & hins=1



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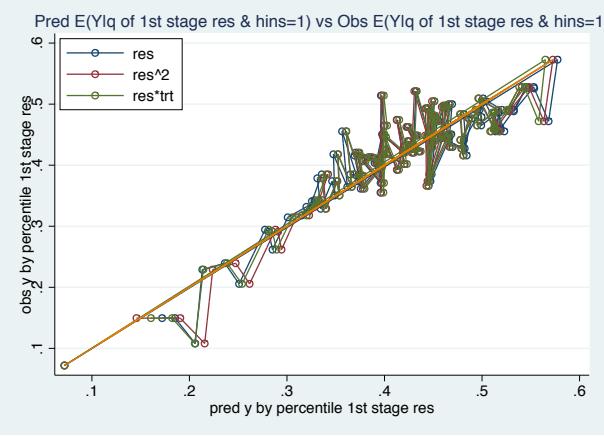


Figure 5.4. 2SRI deviance residual: pred vs. obs by 1st stage res & hins=1

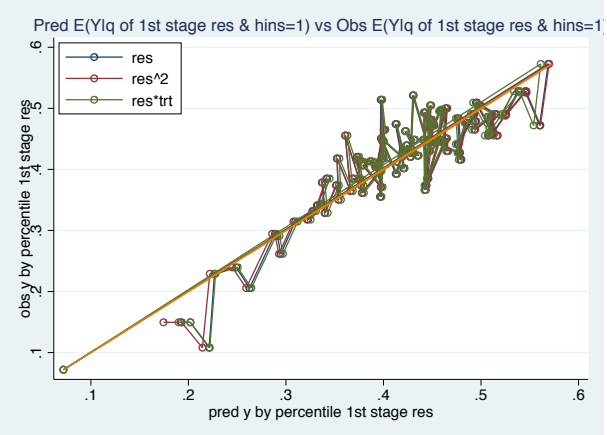


Figure 5.5. 2SRI gen residual: pred vs. obs by 1st stage res & hins=1

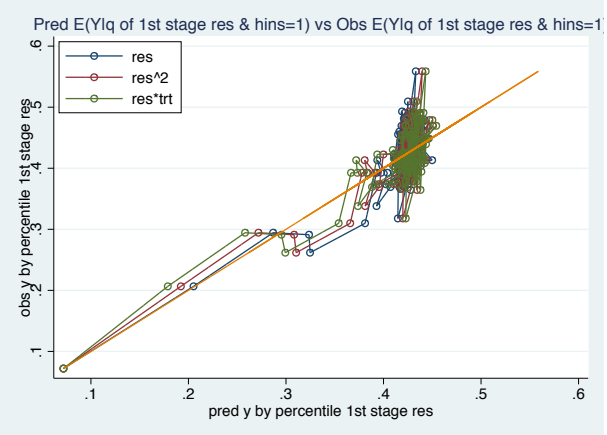
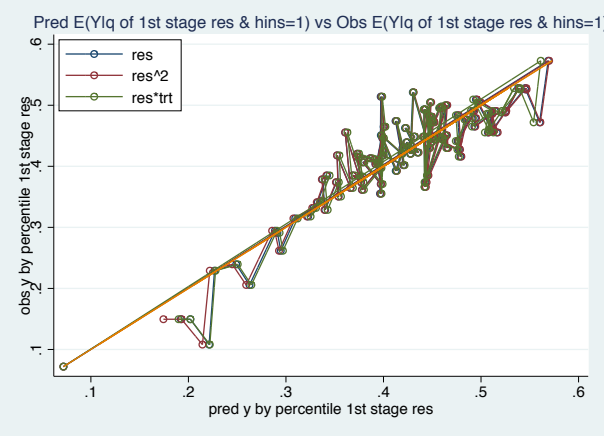


Figure 5.6. 2SRI ans res: pred vs. obs by 1st stage res & hins=1



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CHAPTER 5

Conclusion

The dissertation draws on the Andersen behavioral model as a guiding framework for understanding health services use and health behavior.[1] The overall goals of this dissertation are to evaluate two state-level policies, Washington's privatization of liquor *(aim 1)* and Massachusetts health reform *(aim 2)*, and to contribute to the literature on the application of instrumental variables methods in the situation of an endogenous binary treatment variable *(aim 3)*. This dissertation uses two quasi-experimental approaches, differences-in-differences[2] and instrumental variables,[3] to address, at least in part, hidden bias and unmeasured confounding. For each aim, a summary of the findings, future research, and policy implications are presented.

Summary of findings and future directions

Washington's privatization of liquor

Washington's privatization of retail liquor sales and distribution in 2012 had two major opposing effects: increased access and increased price. Using theory, we could not predict whether the policy would have an overall positive, negative, or non-significant effect on alcohol purchases by type of alcohol. In addition, the public health effects of the privatization in Washington are largely unknown. In our study, we find that privatization is associated with significant but modest increases in liquor, beer, and ethanol purchases in the full sample. Using a longitudinal 3-year panel and conditioning on pre-privatization level of alcohol purchases, we find that there was a differential response to the policy change. Low and moderate alcohol purchasers increased liquor purchases while high alcohol purchasers decreased ethanol purchases.

Future purchasing studies should consider controlling for the price of liquor. By controlling for prices in the model, the elasticity (i.e., the responsiveness to price changes) can be estimated. A meta-analysis found the mean elasticity of liquor to be -0.8.[5] A follow-on study to this dissertation aim would be to examine whether the elasticity of liquor varies by the subgroups we examined. Also, given that Washington's privatization included two components acting in opposite directions on purchases, future research should attempt to disentangle these effects. In order to isolate the effect of privatization, researchers would need to use an increase in liquor taxes in another state as a control. Alternatively, to isolate the effect of the price increase, researchers would need to use another state's privatization as a control. Presently, however, the only other liquor privatization in the U.S. occurred in Iowa over twenty years ago.[6] With a careful study design, liquor privatization within a state in Canada may be a comparable option for use as a control.

Massachusetts health reform and forgone care

Massachusetts health reform is the predecessor to the Affordable Care Act. Health insurance coverage was increased through Medicaid and through the state-based exchanged called The Connector. There is a lack of evidence of the effects of gaining health insurance on the financial burden of obtaining care among low income, working aged adults. A crucial measure of the affordability of health care is self-reported forgone care because of cost. We found that gaining health insurance reduced forgoing care due to the cost in the overall sample. When we explored for heterogeneity in the effect by income groups that correspond with the parameters of the policy expansions, we found that there was a differential response to gaining health insurance. People *likely eligible for subsidies* and *not likely eligible for assistance* experienced reductions in forgoing care. However, the reform did not have a significant effect on

those *likely eligible for Medicaid.* We found that Massachusetts health reform increased insurance coverage by 5.9 percent in the overall sample.

We postulate that people may be considering many types of cost when they answer the question, "Was there a time in the past 12 months when you needed to see a doctor but could not because of cost?" Extensions to this research could replicate this study with a dataset that has richer questions about reasons for forgone care and more detailed information about the continuity and type of health insurance coverage. For example, the National Health Interview Survey includes questions on forgoing care,[7] though researchers would need additional clearances to access state-based identifiers. Other studies should replicate this analysis with the Affordable Care Act. However, only the subgroup with incomes *likely eligible for Medicaid* can be identified since not all states chose to expand Medicaid.

Instrumental variables methods with binary treatment

There are two broad approaches, two-stage least squares (2SLS) and two-stage residual inclusion (2SRI), for implementing instrumental variables in the setting where the endogenous treatment variable of interest is binary. Despite concerns that the linear model does not fit non-linear binary data, many researchers choose 2SLS over the non-linear approach, 2SRI. We use the case study of the effect of gaining health insurance on forgoing care because of cost, and we use the implementation of Massachusetts health reform as the exogenous instrument to explore the impacts of using different instrumental variables approaches. We find that the treatment effect varies by type of model used. Using goodness-of-fit visualizations, we also find that the 2SLS approach is very biased.

Although we were able to conclude that the use of 2SLS was inappropriate in our case study and other studies with a binary endogenous treatment variable, we could not determine a

preferred 2SRI model based on the goodness-of-fit visualizations we used. Based on our visualizations, we could not distinguish between models that produced small and insignificant treatment effects versus models that produced significant treatment effects of similar magnitude. Extensions to this study should attempt to find or generate other goodness-of-fit visualizations that may illuminate the differences between these models that generate differing treatment effects.

Policy implications

Other monopoly "control" states have recently considered privatizing liquor. Although each state has a unique regulatory environment, previous privatizations have led the Task Force on Community Preventive Services, convened by the Centers for Disease Control and Prevention, to recommend against privatization.[8] In the case of Washington's privatization of retail liquor sales and distribution, we found a modest but significant increase in liquor, beer, and ethanol purchases. We found this increase despite increases in distributor and retail fees, which increases the price of liquor. If other states choose to move forward with privatization, policymakers should consider using fees and taxes to counteract the effect of increased access.

Massachusetts health reform and the Affordable Care Act have increased health insurance coverage. Both reforms include premium assistance ranging from partial subsidies to full subsidies, but there are many other reasons people are unable to obtain health care. We find that gaining health insurance through the Massachusetts health reform reduces forgoing care because of the cost. Yet, we found that those *likely eligible for Medicaid* did not have significant reductions in forgoing care. In other words, a segment of the population still experiences some sort of barrier to obtaining health care, despite getting improved access to care via health insurance. If this finding is replicated by future research and those primary barriers are identified,

then current programs, such as Medicaid, and new programs need to be modified or designed to address those barriers in order for low income people to access the care they need.

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