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Abstract

This paper examines the influence of local alcohol prohibition on the prevalence of methamphetamine labs. Using multiple sources of data for counties in Kentucky, we compare various measures of meth manufacturing in wet, moist, and dry counties. Our preferred estimates address the endogeneity of local alcohol policies by exploiting differences in counties' religious compositions between the 1930s, when most local-option votes took place, and recent years. Even controlling for current religious affiliations, religious composition following the end of national Prohibition strongly predicts current alcohol restrictions. We carefully examine the validity of our identifying assumptions, and consider identification under alternative assumptions. Our results suggest that the number of meth lab seizures in Kentucky would decrease by 34.5 percent if all counties became wet.

Breaking Bad in Bourbon Country: Does Alcohol Prohibition Encourage Methamphetamine Production?

This paper examines the influence of alcohol prohibition on the number of methamphetamine (meth) labs in Kentucky. We carefully control for observable heterogeneity between counties, and then address the remaining endogeneity of local alcohol laws. Our preferred estimates exploit variation in religious affiliations in the 1930s, when most local-option votes took place, that is not explained by current religious affiliations; however, we also consider alternative identifying assumptions. In each case, our results suggest that local alcohol prohibition increases in the prevalence of meth labs.

After the Federal prohibition of alcohol sales and production was repealed in 1933, some states permitted localities to adopt local-option ordinances. Kentucky is one of 12 states that still contain jurisdictions that prohibit sales of alcohol. Over a fourth of the 120 counties in Kentucky are "dry", meaning that all sales of alcohol are banned in the county. In contrast, "wet" counties in Kentucky allow the sale of alcohol; "moist" counties contain wet jurisdictions, but are otherwise dry; and "limited" counties only allow the sale of alcohol by the drink in certain types of restaurants.

Miron and Zwiebel (1995) argue that alcohol bans flatten the punishment gradient for alcohol drinkers to engage in other illicit activities, encouraging illicit drug use by raising the relative price of a substitute. Consumers in dry jurisdictions can only purchase alcohol if they drive to a jurisdiction that allows alcohol sales, or if they make an illegal local purchase.[1] In

[1] Kentucky's alcohol control laws describe potentially severe penalties for violations of local alcohol prohibition. As described in "A Review of the Commonwealth of Kentucky Alcohol Control Laws, 2007", criminal penalties grow from a class B misdemeanor on the first offense to a felony on the third offense, with a fine of up to $10,000 and up to 10 years in jail. The costs of civil asset forfeiture can also be quite high, even for first-time offenders. The law

addition to introducing the risk of criminal penalties and asset forfeiture, alcohol transactions with illegal dealers may provide consumers with more information about illicit drugs than they would have otherwise.

Most previous empirical work is consistent with the idea that illegal drugs are substitutes for alcohol. Conlin, Dickert-Conlin, and Pepper (2005) find that a change in the status of Texas counties from dry to wet lowers drug-related mortality by approximately 14 percent. DiNardo and Lemieux (2001) find that higher minimum drinking ages reduce alcohol consumption by high school seniors, but increase marijuana consumption. Crost and Guerrero (2012) find that alcohol consumption increases and marijuana use falls at age 21, and Deza (2015) finds that hard drug use also decreases at 21. Anderson, Crost, and Rees (2014) find that marijuana is a substitute for alcohol, and that legalizing medical marijuana is associated with a decrease in traffic fatalities.

On the other hand, Pacula (1998) finds that increases in beer taxes are associated with less drinking and less marijuana use among young adults, consistent with the two goods being complements. Yörük and Yörük (2011) find that marijuana use increases at age 21; however, their results are strongest for a subsample that used alcohol, cigarettes or marijuana at least once since the previous interview. Crost and Rees (2013) examine the same data and find no evidence of complementarity between marijuana and alcohol.

Access to alcohol can also have indirect effects on other crimes. Carpenter (2005) finds that zero-tolerance drunk driving policies reduce property crime among 18-21 year old males by 3.4 percent and reduce the incidence of nuisance crimes. Anderson et al. (2014) find an association between establishments serving alcohol by the drink and violent crime. Other studies

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requires that any premises or vehicle involved in "unlawfully selling, transporting or possessing alcoholic beverages in dry territory" be seized and forfeited to the state, regardless of whether anyone is even charged with a crime.

find that higher alcohol excise taxes reduce alcohol consumption as well as certain types of property and violent crime (See Carpenter & Dobkin, forthcoming for a full survey).

We contribute to the literature on the consequences of prohibition by considering the effects of alcohol restrictions on meth lab seizures in Kentucky. On one level, our work can be seen as contributing to the discussion of whether alcohol and illegal drugs are substitutes or compliments. An advantage of our study is that we do not rely on self-reported drug use or counts of drug arrests reported by law enforcement. As we argue in the next section, the counts of meth labs we use should be less affected by potentially endogenous law enforcement behavior than arrest data.[2]

On a more direct level, our work focuses on an outcome that has important implications for public health, the production and use of methamphetamine. In a systematic review of the literature on health consequence of meth use, Marshall and Werb (2010) find that users are more likely to have suicidal ideation, eating disorders, paranoia, and mortality from overdose. The production of methamphetamine not only places the "cooks" in danger from explosions, burns and chemical injuries; but also creates an environmental hazard. Kentucky's Department for Environmental Protection reports that the average remediation cost is roughly $5,000 per lab, with some clean-ups costing over $20,000.[3]

Gonzales, Mooney, and Rawson (2010) report that meth use in the United States increased threefold between 1997 and 2007. Weisheit and Wells (2010) find that Kentucky has one of the highest rates of meth lab seizures in the country, with 15.24 labs seized per 100,000

[2] We also consider other outcomes, including various counts of drug arrests. We consider ER visits for burns as an alternative measure that is completely independent of law enforcement and illuminates a cost of local, amateur meth production. While our results using arrest data should be taken with a grain of salt, they are consistent with our results for meth labs, and they suggest that the negative effects of prohibition extend to other drugs.
[3] See, e.g., http://waste.ky.gov/SFB/MethLabCleanup/Pages/default.aspx.

residents between 2004 and 2008.[4] They suggest that meth labs may be as prevalent as they are in Midwestern and Southeastern states because distance from the Mexican border raises the costs of imported meth relative to locally produced products.[5] Cunningham et al. (2010) support this conclusion, reporting that methamphetamine purity falls with distance from the borders with Mexico and Canada, which is consistent with local demand being met by production in small local labs. Kentucky's location, therefore, suggests that its 120 counties are an excellent setting to study the effects of alcohol restrictions on meth use and production.

Of course, local alcohol laws are likely endogenous. Toma (1988) argues that local-options give voters an opportunity to increase the price of alcohol by increasing the cost of obtaining it. Yandle (1983) points out that both bootleggers and Baptists have historically supported alcohol bans: Baptists for religious reasons and bootleggers for economic reasons. In both cases, local alcohol laws would be affected by the religious, cultural, and economic characteristics of the area. Restrictions may also be enacted in response to local public health concerns or other problems related to alcohol such as the incidence of drunk driving.

Our analysis begins with a careful treatment of observed heterogeneity between counties. All of our estimates control for a wide array of geographic, demographic, economic and cultural characteristics. Furthermore, we consider the robustness of our results to additional control variables, functional form assumptions, and sample restrictions based on observable characteristics. If anything, we find that adjusting for observable differences strengthens the association between local-option status and the prevalence of meth labs.

[4] Between 2004 – 2008 the ten states with the highest meth lab seizure rates (from highest to lowest) were Missouri, Arkansas, Iowa, Tennessee, Indiana, Kentucky, Alabama, Oklahoma, Kansas, and Mississippi.

[5] Weisheit and Wells (2010) point out that methamphetamine *use* appears to be higher in Western states than the Midwest or Southeast, but labs are relatively rare in Western states.

We account for remaining unobserved heterogeneity using two different approaches, each of which relies on different identifying assumptions. First, our preferred approach exploits variation in religious composition shortly after the end of national prohibition that is independent of counties' current religious composition. We show that religious composition from the 1930s, when most local option laws were passed, strongly predicts current wet/dry status; and we perform multiple tests of the validity of our exclusion restrictions. Our second approach to unobservable differences follows Oster (Forthcoming) and Altonji, Elder, and Taber (2005b) (referred to as AET, 2005b), replacing exclusion restrictions with the assumption that selection on observables is informative about selection on unobservables.

I. Data

Our primary data are a panel of meth lab seizures and local-option ordinances for Kentucky counties from 2004 to 2010. The lab seizure counts are from the DEA's National Clandestine Laboratory Register.[6] The DEA provides the physical street addresses for all meth labs seized as a public service due to the public health risk from chemical contamination. An advantage of these data is that they do not depend solely on arrests or other law enforcement interventions. The DEA also lists labs that are accidentally discovered following a fire or explosion. Furthermore, the significant negative externalities associated with meth labs, especially the unpleasant chemical odors, result in additional discoveries that are likely independent of the intensity of local enforcement efforts.

In contrast, arrest data are known to suffer from potentially endogenous measurement error (Levitt, 1998; Lott Jr & Whitley, 2003; Pepper, Petrie, & Sullivan, 2010). Reported arrest

[6] These data do not include mobile labs or parephenalia. Dumpsites are counted among the lab seizures, but only when they are found within a residence or place of business. Including roadside dumpsites would result in higher counts, but we worry that such dumpsites are less likely to be discovered due to accidents or complaints.

rates are a function of the crime rates, the enforcement rates and the reporting behavior of local law enforcement. When we compare arrest data from the FBI's UCR and the Kentucky State Police, we find some evidence that errors in the data vary with local option status. For example, the correlation between the reported arrests for drug categories that are common to the two databases varies with wet/dry status, even when we control for other county characteristics.[7] Furthermore, the DEA data show at least one meth lab is discovered in over 18% of county/year observations in which the UCR records no arrests for manufacturing or sales of any "other nonnarcotic" drug, which includes methamphetamine.

County-level local-option ordinance data are provided by the Kentucky Department of Alcoholic Beverage Control.[8] In 2010, Kentucky had 32 wet counties, 39 dry counties, 20 moist counties and 29 counties with limited alcohol access. For the sake of simplicity, we treat counties with limited alcohol access as dry counties in our analysis, which should work against finding effects of local alcohol bans.[9]

As a robustness check, we also collect drug-related arrest data from the FBI Uniform Crime Reports (UCR) and the Kentucky State Police. Both sources include reported arrests related to the sales, manufacturing and possession of "other nonnarcotic" drugs, which include methamphetamine; and synthetic narcotics, which includes prescription opioids. The Kentucky State Police data also provide reported arrests for meth-related crimes, including manufacturing,

[7] Both datasets record arrests related to synthetic narcotics and "other nonnarcotic" drugs. Methamphetamine is in the "other" category. For both categories, the raw correlation between the reports in the UCR and the KSP databases is higher in wet counties than in dry, which is consistent with less accurate reporting in dry counties. Controlling for county characteristics explains much of this difference for "other" drugs; however, the differences in reporting accuracy for synthetic narcotics persist (and increase) when we control for county characteristics.

[8] http://www.abc.ky.gov/

[9] A few counties allow alcohol sales on vineyards, golf courses, or in two qualified historic Shaker districts; but are otherwise dry. We treat these counties as dry.

sales, possession, dump sites, and unlawful possession of precursors. We use the sum of reported meth-related incidents as a dependent variable in our robustness checks.

Similar to national trends, meth lab seizures in Kentucky fell by 50 percent between 2004 and 2007, but increased more than three-fold by 2010. As seen in Figure 1, the highest rates of meth lab seizures occur in the southern counties bordering Tennessee and in the center of the state. Comparing Figure 1 with Figure 2, which shows wet/dry status, there appears to be a relationship between dry status and meth lab seizures. The mean lab seizure rate is 2.17 per 100,000 residents in wet, 2.26 in moist, and 3.92 in dry counties (see Table 1). The means for moist counties are consistent with Campbell et al. (2009) who find that alcohol bans have smaller effects when a jurisdiction is less geographically isolated.

We use county-level demographic variables from the U.S. Census and American Community Survey. As suggested by Yandle (1983) and Strumpf and Oberholzer-Gee (2002), the demographic composition of voters influences local-option ordinances. Counties are more likely to adopt restrictive alcohol policies as population, income, percent black, and percent college educated decrease; or as poverty and unemployment increase.

Finally, we use data from Haines (2004) on religious membership in 1936 to capture religious attitudes at the time of the initial wet/dry votes following the end of Federal Prohibition. We control for current religious attitudes using data from the Association of Religion Data Archives (2000, 2010). As a robustness check, we examine the sensitivity of our results to the inclusion of controls for religious composition in 1990.

Table 1 shows the means of several key variables and how they vary by local-option status. Wet counties are more densely populated on average than dry counties. Wet counties also have higher average levels of education, higher household income, and more minorities. Given the observable differences between wet and dry counties, many of which are statisticaly significant at a 5 percent level, the adoption of local-option ordinances should not be treated as exogenous. Note also that religious participation and the share of Baptists, both of which are associated with restrictive alcohol policies, have increased across all county types since 1936.[10]

II. Estimation

To determine the robustness of our results we use four different estimation methods. The first two methods address observable heterogeneity between counties. The next two address unobservable differences that may be correlated with wet and dry status.[11]

Observable Heterogeneity

First, we consider an ordinary least squares model with year fixed effects and county-level demographics to estimate the treatment effect:

$$Meth\ Crime\ rate_{it} = \alpha_t + \gamma Status_{it} + X_{it}\beta + e_{it}$$

The year fixed effects, α_t, control for time-varying changes that affected the entire state. We cannot include county fixed effects because we only observe a few changes in wet/dry status during our sample period.[12]

The matrix X_{it} consists of controls for geographic, demographic, economic and religious characteristics. Following Strumpf and Oberholzer-Gee (2002), we include indicators for bordering surrounding states and having a wet neighbor. We also control for latitude, longitude

[10] Strumpf and Oberholzer-Gee (2002) study local prohibition ordinances between 1930 and 1940. They find Baptists, Presbyterians, and Methodists are more likely to support local prohibition; but Catholics, Episcopalians, and Lutherans are more likely to support legalized liquor.

[11] All of these methods use linear models for the incidence of meth labs and other outcome variables. Estimates from Poisson count models, which can be found in the appendix, are qualitatively similar to the simpler linear specifications we present in the text.

[12] Garrard, Johnson and Clay counties became moist in 2008, 2009 and 2011, respectively. Spencer and Trigg counties became wet in 2009. Strumpf and Oberholzer-Gee (2002) argued that the use of religious composition and other taste variables are better predictors of county status than county dummies are.

and their interaction; access to interstate highways; and the ratio of residents working in the county to total county employment.[13] The demographic characteristics in X_{it} are county population; population density; and the percentages of the population who are married, male, black, under age 21 and over 65. Economic controls include median household income; female labor-force participation; the poverty rate; the percent of children receiving TANF; and the percentages of acres dedicated to crops and pasture. Finally, we control for the percent of the population belonging to a religious congregation, that percent squared, the number of Baptists, and the interaction of the Baptist population with the percent black.[14]

The variables of interest in the regressions are the county alcohol $Status_{it}$ variables. We use three sets of measures for local-option status. The first are dummy variables indicating whether the county is wet, moist or dry, with dry being the omitted category. The second measure exploits variance between moist counties by measuring the percent of the county population that lives in a wet jurisdiction. This variable equals one in wet counties and zero in dry counties. Lastly, we use the number of liquor stores per 100,000 residents as an alternative measure of wetness that is not based on the state local-option data.[15]

Our second estimation method is propensity score matching, which controls for observed heterogeneity more flexibly than OLS. Propensity scores also allow us to identify and exclude observations that are not comparable to any observation in another treatment group. For example, the counties that contain Louisville and Lexington are wet, more densely populated and

[13] A higher ratio of residents to total county employment suggests more isolation. This variable is constructed using data on commuting patterns from the American Community Survey.
[14] Religion data from the 1930s suggest that black and white Baptists have different views on alcohol prohibition; however, the data from more recent years do not separate Baptists as clearly along racial lines.
[15] Our counts of liquors stores come from the Quarterly Census of Employment and Wages, which is collected by the Bureau of Labor Statistics with the cooperation of state agencies.

otherwise different from any dry county in Kentucky. Additionally, a few dry counties are so geographically isolated and sparsely populated that they are not comparable to any wet county.

The matching estimates we present only evaluate binary treatment variables. Specifically, we perform this analysis for two groupings: wet vs dry and moist vs dry. We also estimated multinomial treatment variables using inverse probability weighting instead of matching. The estimates based on inverse probability weighting, which we report in the appendix, are similar to those produced by the simpler matching estimates presented in the text.

Unobserved Heterogeneity

We use two approaches to address the likely unobserved heterogeneity among counties in our sample. First, we exploit variation in religious membership following Prohibition that is not correlated with current religious composition. A flurry of local-option votes occurred shortly after the repeal of Prohibition in 1933. Since 1940, local option votes have become less common. We find that per capita religious membership in 1936 and its square strongly predict current wet status. All of our regressions include the current religion variables described above to ensure that the instruments do not proxy for present day beliefs, which would compromise the credibility of our exclusion restrictions.

For the IV estimation with discrete treatment groups, we only consider wet versus dry, and group the moist counties into the dry category. Our instruments cannot identify wet and moist as separate treatments.[16] This grouping works against our finding an effect of alcohol bans as some counties in the dry group contain moist counties, which are dry, but have wet jurisdictions.

[16] Our instruments have similar effects on the moist probability as the wet probability, but with less precision.

Additionally, we continue to restrict the sample for our IV estimates, excluding counties with propensity scores that approach one or zero. We believe any attempt to identify exogenous variation is more plausible if it doesn't compare Louisville and the Cinncinnati suburbs to racially homogeneous, isolated counties with a predicted probability of dry greater than 0.999. For the sake of comparison, we also present analogous OLS results for the restricted sample.

Our identification strategy is unique in the literature on alcohol restrictions, which has previously been dominated by difference-in-difference estimation based on changes in local option laws (E.g., Baughman, Conlin, Dickert-Conlin, and Pepper (2001); Conlin et al. (2005)). The advantage of our IV approach is that it does not require us to assume that local policy changes are independent of time-varying unobservables. The disadvantage is that we must assume that religious composition in the 1930s is not correlated with current unobserved county characteristics, conditional on current religious composition and other observed characteristics. Our approach finds some support in the work of Strumpf and Oberholzer-Gee (2002), who not only use religion to capture local tastes for prohibition, but also argue that changes in local alcohol laws are endogenous.

The Validity of Exclusion Restrictions

We address the possibility that counties' religious compositions in the 1930s have persistent correlations with unobserved county characteristics that are not captured by current religion or other variables in a few ways. First, we consider the robustness of our results to the addition of controls for religious composition in the 1990s and polynomials in the estimated propensity scores, which should provide some reassurance that our results are not driven by omitted variables.[17] Motiviated by the possibility that counties with greater observable differences likely have more unobservable differences, we also consider robustness to changes in our sample restrictions based on extreme values of the propensity score.

We then examine the validity of our instruments using an approach discussed in Altonji, Elder and Taber (2005a; 2005b). Considering instruments for Catholic high school attendance, they regress various outcomes on proposed instruments using a sample of students who almost never attend Catholic high school (public school eighth graders), eliminating the possibility that these estimated coefficients are due to the instruments' effects on Catholic high school attendance. Non-zero coefficient estimates for instruments in that sample, therefore, reflect a covariance between the instruments and unobservables, suggesting the instruments are not valid.[18]

We execute the same test using a sample of counties that were dry as late as 1994, 10 years before our sample period began. These counties are still dry or moist in over 96 percent of our observations. Two previously dry counties became wet between 1994 and 2004, and another two became wet in 2009; however, these changes in treatment happened recently enough that they should not have been caused by religious composition in 1936.[19] Therefore, any apparent effect our instruments have in this subsample cannot operate through the instruments' effects on wet status, even in the few cases where wet status is not zero.

[17] We also considered potential bias due to enforcement efforts by adding the rate of property crime arrests as a regressor. The resulting changes in our coefficient estimates and the first-stage F statistics were neglible, even though property crime is potentially endogenous in our context. These results are available upon request.

[18] AET (2005b) reject the instruments used previously in the Catholic schooling literature, including the contemporary share of Catholics in the local population. Our identification strategy resembles that of Cohen-Zada and Elder (2009) who use the historic shares of Catholics as an instrument for Catholic high school attendance, controlling for contemporary Catholic shares. When they perform the test of AET (2005b), they fail to reject the validity of historic Catholic shares as an instrument.

[19] Fulton county, which became wet prior to our sample period, provides an interesting case study. Despite being dry as late as the mid 1990s, it has a predicted $\Pr(\text{wet}) > 0.99$ during our sample period, primarily due to changes in its racial composition. Fulton county has the highest percent black of any county in our sample, at roughly 25%; however, its population was less than 17% black in 1980.

Proportional Selection on Obsevables and Unobservables

Finally, we use the proportional selection approach of Oster (2016), who extends AET (2005b), to consider the amount of selection on unobservables that would be required to explain away the entire effect suggested by our OLS estimates. This approach avoids the use of exclusion restrictions; however, it does not provide point estimates. Instead, it simply tells us whether some part of the effect implied by our OLS coefficients is likely to be causal.

AET (2005b) argue that the amount of selection on observable variables is informative about the amount of selection on unobservables.[20] If observable and unobservable characteristics were drawn at random from the set of all relevant characteristics, then selection on observables and unobservables should be equal; however, AET (2005b) argue that selection on unobservables should be less than selection on observables when observable variables are chosen to capture relevant variation. Therefore, we compare the amount of selection on unobservables that would be needed to explain our OLS coefficients to the estimated selection on observables. As the ratio of selection on unobservables to observables approaches or exceeds one, it becomes increasingly likely that part of the covariance identified by our OLS coefficients reflects a causal effect.

One difference between AET (2005b) and Oster (2016) is that Oster (2016) allows the maximum possible R^2 to be bounded below 1. This feature is important in our context because counting the number of meth labs discovered in a calendar year necessarily introduces variance from any underlying, "true" rate of meth lab discovery. To see this point, note that counting from February to January would produce slightly different numbers than counting from January to December. Even when we regress the meth lab rates on year *and county* fixed effects, county

[20] Their arugment is based on the intuition that is commonly used when papers, including the current paper, discuss robustness of treatment effects to the inclusion of additional control variables.

time trends, and all of our time-varying control variables, we can't produce an R^2 over 0.764. Therefore, we set the maximum R^2 at 0.8 when performing the proportional selection test.[21]

III. Results

As described above, we examine the number of meth lab siezures per capita using three different measures of county wet/dry status and four estimation techniques. The three wet/dry measures are dummy variables for wet and moist counties, the percent of the population living in a wet jurisdiction, and the number of liquor stores per capita. The four estimation techniques are ordinary least squares, propensity score matching, instrumental variables, and the proportional selection approach of Oster (2016).

Table 2 presents the OLS and propensity score results for our primary outcome variable, DEA Meth Lab Seizures. Column 1 shows the OLS results using observations from all counties, while estimates in columns 2 through 4 exclude observations where the $\Pr(wet)$ approaches 0 or 1.[22] Column 4 presents OLS results that compare wet counties to dry and and moist counties, facilitating comparison to our IV results.

All models find that legal access to alcohol is associated with fewer meth labs per capita.[23] The OLS estimates using the full sample suggest that wet counties have 1.47 (0.61) fewer meth labs and moist counties have 1.03 (0.53) fewer meth labs than dry counties. In the middle panel, the coefficient estimate for the percent wet treatment variable is 1.17 (0.60). In the bottom panel, the point estimates for liquor stores per capita are also negative, but are not statistically significant.

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[21] We use psacalc.ado, which Oster wrote to accompany her paper, to perform this test in Stata.
[22] Specifically, we exclude observations where the $\Pr(wet)$ is above 0.99999 or below 0.00001.
[23] We present additional results using Poisson count models instead of OLS in Appendix Table 2.

Next, we use propensity score matching to estimate the treatment effects. The basic common support restriction applied in this table reduces the sample size from 840 to 800 observations.[24] As mentioned above, we estimate propensity score treatment effects for wet vs dry and moist vs dry separately, with samples of 656 and 369 observations.[25]

The propensity score matching results in the second column of Table 2 again suggest that allowing alcohol sales in a county reduces the prevalence of meth labs. Wet counties have 2.48 (0.34) fewer labs and moist counties have 2.10 (0.41) fewer labs than dry counties. Both point estimates are statistically significant at the 1 percent level. As seen in Appendix Table 3, these results are not sensitive to estimating separate binomial treatments. If anything, the estimated treatment effects increase in magnitude as we control for observable heterogeneity more carefully.

For the sake of comparison, the third and fourth columns in Table 2 present OLS results using the restricted sample that excludes counties that are off the common support. This results in slightly larger coefficient estimates compared to OLS using the full sample; however, the estimated coefficients in the first panel are still much smaller than the propensity score estimates. When wet counties are compared to both dry and moist counties, as they are by our instrumental variables estimates, the coefficient on the wet county indicator falls to -1.08 (0.57).

Table 3 presents estimates that use religious composition from the 1930s as instrumental variabels. The first column presents results using the same sample and control variables as the last three columns of Table 2. The other columns in Table 3 investigate the robustness of our results by adding control variables or varying the estimation sample. The smallest first-stage F

[24] These results are robust to applying a stricter common support restriction that limits attention to observations with a $\Pr(not\ wet)$ between 0.03 and 0.9999.

[25] Estimation results that use inverse probability weighting to estimate multinomial treatment effects are presented in Appendix Table 7. The inverse probability coefficient estimates are qualitatively similar to the simpler matching estimates present in Tables 2 through 7, but tend to be larger in magnitude.

statistic in the table is 51.5, consistent with religions composition in the 1930s being a strong predictor of current local option status, despite our controls for current religious composition.[26]

The results in Table 3 uniformly suggest that local alcohol access decreases the prevalence of meth labs in a county. Focusing on the main specification in the first column, wet counties are estimated to have 2.38 (1.14) fewer meth labs per 100,000 than moist or dry counties. The estimated effect of the percent wet treatment variable is -2.58 (1.28). Finally, the IV estimates suggest that liquor stores have a statistically significant, negative effect on the number of meth labs, with a coefficient of -0.092 (0.045). While the IV coefficients are all much larger than their OLS analogs in Table 2, the IV coefficient on the wet county treatment variable is only slightly larger than the propensity score estimate.[27]

Taken at face value, these estimates suggest that repealing all alcohol prohibition in Kentucky would decrease the total number of meth lab seizures in the Commonwealth by almost 43 labs per year. This translates to a 34.5 percent decrease in the prevalence of meth labs statewide, and a 59.8% percent decrease in moist and dry counties.

The Validity of our Instrumental Variables

The remaining columns of Table 3 consider the robustness of our IV estimates as an informal means of assessing the validity of our instruments. If religious composition in the 1930s is correlated with some aspect of current culture that is not captured by our controls for current religious composition, then adding controls for religion from the more recent past should affect our results; however, the second column shows that the estimated effects of alcohol access

[26] Using only the rate of religious membership in 1936 and its square as instruments means that tests of overidentifying restrictions are not useful in our case. In previous drafts, we also used the number of Baptists and black Baptists as instruments, which arguably made overidentification tests more meaningful; however, the historic Baptist variables were relatively weak instruments. In any case, none of our overidentification tests suggest a problem for our main results.

[27] Note that the propensity score estimate for the wet treatment effect in Table 2 compares wet counties to dry counties only. When we compare wet counties to both dry and moist (not shown), the propensity score treatment effect is -2.322 (0.309).

change very little when measures of religious composition in 1990 are added to the regressions.[28] As an additional test for correlation with omitted variables, the third column adds a cubic polynomial in the estimated propensity score. The results suggest that, if anything, controlling for observed differences in a more flexible manner increases the estimated effects of local alcohol access.

The final colums of Table 3 restrict the sample further, excluding wet counties with a predicted $\Pr(not\ wet) < 0.03$ and dry counties with a predicted $\Pr(not\ wet) > 0.9999$.[29] If our instruments are correlated with unobserved county characteristics, and unobservable differences are more pronounced in counties that differ more along observable dimensions, trimming counties based on extreme values of the propensity score should affect our estimates. But the coefficients in the fourth and fifth columns are again similar to those in the first.[30]

Table 4 presents a more formal evaluation of our instrumental variables. Following AET (2005b), we first present the reduced form coefficients for our instruments using the main estimate sample. We then present analogous coefficients estimated on a sample of counties that were dry in 1994. As discussed in Section II, the estimates from the counties that were dry in 1994 can only reflect covariance between our instruments and unobserved county characteristics.

As expected, the reduced form coefficients in the first two columns are statistically significant, suggesting that the number of meth labs increases with religious membership in 1936 at low levels of membership, but eventually decreases as membership rises.[31] Adding controls

[28] The variables added to the regressions in the second column are the percent of the population in 1990 that belonged to a religious congregation, that percent squared, and the number of Baptists in 1990.
[29] The minimum $\Pr(not\ wet)$ among dry counties is 0.034. The maximum among wet counties is 0.9998.
[30] The fifth column adds both the religion variables from the second column and the propensity score polynomial from the third.
[31] Religious membership in 1936 ranges from 6 percent of a county's population to over 60 percent, with a median around 32.5. The variable's partial derivative in the reduced form equation is 0.143 (0.084) at 6 percent, and -0.131 (0.056) at 60.

for religion in 1990, in addition to current religious composition, results in larger coefficients in the second column, but the increase is not statistically significant.

More importantly, the coefficients on the 1936 religion variables are uniformly smaller in the subsample that was dry in 1994, and they are neither individually nor jointly statistically significant. The reduced-form coefficients in the main specification (first column) are several times larger than the analogous estimates in the third column. Furthermore, we see a similar pattern when we consider effects of our instruments on alternative outcomes such as ER visits for burns or drug arrests (not shown). We find no evidence of covariance between our instruments and unobservable county characteristics, regardless of which outcomes we consider.

Proportion of Selection on Unobservables

Our final estimation method considers the potential influence of unobserved county characteristics without relying on exclusion restrictions. As discussed in Section II, we use the approach of Oster (2016) to estimate the proportion of influence the unobservables would need to have relative to observed characteristics to suggest that our OLS coefficients do not reflect any causal effect. As the proportion approaches or exceeds one, it becomes increasingly reasonable to believe we have identified some causal relationship, even if that relationship is smaller than our point estimates.

Table 5 presents the OLS coefficients and proportion required for zero causal effect for several regressions. The coefficients of proportionality are estimated assuming a maximum R^2 of 0.8, which is nearly four times larger than any estimated R^2 in the table.[32] The regressions in the first column were also presented in the last two columns of Table 2. The later columns add control variables and vary the sample restrictions.

[32] Even when we regressed meth labs on county fixed effects, county-specific time trends, and other control variables, we could not produce an R^2 as large as 0.8. See Section II for details.

The results in Table 5 again suggest a causal effect of local alcohol access on the prevalence of meth labs. Despite the coefficients in the first column being the most conservative estimates in the table, we find that selection on unobservables would need to be roughly the same as selection on observables before the entire OLS coefficient could be explained by unobservable factors.

The case for a causal effect only grows stronger as we attempt to reduce omitted variable bias or further restrict the sample based on observable differences. Adding controls for religious composition in 1990 raises the OLS coefficients and coefficients of proportionality, but the change is not dramatic. When we add a polynomial in the propensity score, we find that unobservable differences would need to have 2.7 times the influence of the observable variables in order to explain the coefficient on the wet dummy variable; and 1.5 times the influence of observables to explain the coefficient on the percent living in a wet jurisdiction. If we further restrict the sample to counties that are more observably similar, the coefficients of proportionality for the wet county treatment variable continue to exceed two; and the coefficients of proportionality for the percent treated suggest that selection on unobservables would need to be several times larger than selection on observables to explain our OLS coefficients.

It's worth noting that the negative coefficient in the lower panel of the fifth column suggests that our controls for observable characteristics *increase* the estimated treatment effect. This reflects the fact that our control variables do not uniformly mitigate the differences in meth production between wet and dry counties. For example, our controls for geographic coordinates and population reduce the coefficients on our treatment variables; however, adjusting for religion (current or 1990) or many of our economic controls increases the estimated treatment effects. If the unobserved county characteristics resemble our existing economic and cultural controls more than our basic controls for geography and demographics, our OLS coefficients understate the true causal effects, as suggested previously by our IV and propensity score estimates.

Alternative Outcome Measures

As a robustness check, we repeat our estimates using data on ER visits for burns, as well as arrest data from the UCR and the Kentucky State Police (KSP). Burns can be viewed as an alternative measure of meth production that is entirely independent of law enforcement reporting.[33] On the other hand, we view the arrest data with some suspicion; however, they do provide alternative measures of local drug crime.

The production of meth involves corrosive, combustible chemicals and a heating element. "Cooks" risk chemical and conventional burns. We obtained data on emergency room visits for burns from chemicals or hot substances from the Kentucky Injury Prevention and Research Center.[34] As shown in Table 6, there is a consistent pattern of fewer burns per capita in wet counties. The OLS estimates in both samples indicate roughly 20 fewer ER burn visits per 100,000 residents, which is similar to the propensity score estimate of 24.3 (9.2) fewer visits.

The magnitude of the reduction in burns increases dramatically when we use instrumental variables. The coefficient for the wet treatment in the first panel is -61.9 (23.2), and the coefficient on the percent wet is -70.2 (26.1). The IV coefficient on liquor stores per capita, -3.1 (1.1), also suggests that alcohol access reduces ER burn visits.

[33] We have also examined data on hospital admissions for drug overdoses; however, the overdose data we've acquired so far contain a large number of censored values in order to protect patient confidentiality in small cells. Burns, being more common, are censored less often.

[34] These data refer to emergency room visits listed under ICD-9 code E924, which are accidental burns caused by hot substance or object, caustic or corrosive material, and steam. These data are provided by Svetla Slavova at the Kentucky Injury Prevention and Research Center.

Table 7 presents results for three categories of drug arrests. The outcome in the first panel is the rate of all meth-related incidents, as reported by the KSP.[35] The outcome variables in the second and third panels are the arrest rates for synthetic narcotics, including Oxycodone and other prescription opiates, as reported by the UCR and the KSP.[36]

The results for total meth-related crimes in the first panel are very much consistent with our main results. Least squares estimates using either the full or restricted samples find a reduction in meth-related arrests of 20 to 22 per 100,000 residents in wet counties. The propensity score estimates find larger reductions, with wet counties having 32.03 (9.29) fewer meth-related arrests and moist counties having 27.63 (13.55) fewer arrests. The IV estimates find that wet counties have 47.65 (19.25) fewer meth-related arrests. The IV estimates for the percent wet and the liquor store treatment variables also find statistically significant reductions in meth-related arrests when alcohol sales are allowed.

Given the systematic measurement error noted above, it is not surprising to see the results for synthetic narcotic arrests vary somewhat between the UCR and the KSP data. That said, the IV results in the second and third panels still paint a consistent picture, suggesting that alcohol prohibition encourages the illegal use and distribution of prescription opiates, as well as crystal meth. The coefficients on the wet treatment dummy suggest reductions of 28 to 35 arrests per 100,000. The coefficients on the percent wet vary from -31.1 (11.6) to -35.8 (18.6).

Our final outcome measure addresses the possibility that unobserved health trends are associated with both the demand for illicit drugs and local alcohol policy. If poor population health is a motivation for local prohibition, then we should observe "effects" on other health measures. In Table 8, we report the effects of local-option status on childhood obesity as a

[35] The meth-related crimes include dumpsites, possession, sales, paraphanellia, and meth labs.
[36] We estimated similar results for "Other Nonnarcotic Drugs", which includes methamphetamine. The results, which are available on request, are much less precise than those for synthetic narcotics.

falsification test. All of the estimates are close to zero, they vary in sign, and none of them are statistically significant. Furthermore, we find similar results when using infant mortality as the dependent variable (not shown), despite the potential effects of alcohol consumption on fetal health.

Comments and Alternative Explanations

Although most of our results focus on supply-side measures, the observed effects could still reflect the local demand for methamphetamine. The geographic position of Kentucky far from the country's borders and its sparse population lower the (private) costs of local production, including production for personal use, relative to importation. According to the DEA, methamphetamine and marijuana are the only illegal drugs that are easily produced by the users: "A cocaine or heroin addict cannot make his own cocaine or heroin, but a methamphetamine addict only has to turn on his computer to find a recipe identifying the chemicals and process required for production of the drug." (Keafe, 2001). Furthermore, Cunningham et al. (2010) find that methamphetamine purity falls with distance from Mexico and Canada, which is consistent with demand being met by small, amateur labs.

An alternative supply-side story is that meth labs may be more prevalent in dry counties due to a longer history of illicit alcohol production. Experience producing "moonshine" may result in greater knowledge about hiding labs, more skilled production, greater ability to influence law enforcement, and more extensive networks for distributing illegal products. But it's not clear that these channels would result in a higher number of labs being discovered, even if they did result in a higher volume of illicit production.[37] If anything, greater experience with

[37] More sophisticated production and distribution by Mexican organized crime would explain the observation of Weisheit and Wells (2010) that meth lab siezures are less common in Western states than in the Midwest and Southeast, despite higher levels of self-reported meth use.

illicit production would suggest that the number of meth labs would be undercounted to a greater degree in dry counties, which would bias our estimates toward zero.

That said, we acknowledge that our estimated effects may be realized gradually following a change in policy. Legal liquor sales would make alcohol more readily available, reducing the benefits of illegal production and distribution of alcohol or other drugs. The relative costs of producing, distributing and using methamphetamine would rise immediately; however, amateur production by addicted users may change more slowly.

IV. Conclusion

We find strong evidence that local alcohol prohibition in Kentucky increases the prevalence of methamphetamine labs in dry jurisdictions. Our results suggest that, if all counties in Kentucky became wet, the number of meth labs statewide would be reduced by 34.5 percent. Although we consider data on arrests to be less reliable than the DEA's lab seizure data, our results using drug arrests not only support those based on meth labs, but also suggest that the negative consequences of alcohol prohibition extend to prescription optiates. Furthermore, we find that local alcohol prohibition increases the prevalence of ER visits for burns, which is consistent with local labs being run by poorly trained amateur "cooks."

We address the likely endogeneity of local-option status using a novel set of instrumental variables. There was a spate of votes in Kentucky following the end of national Prohibition with relatively few votes since the 1940s. We find that the religious composition of counties in 1936 strongly predicts current wet/dry status, even controling for current religious composition. We find support for our exclusion restrictions using the test of AET (2005b), as well less formal tests. Furthermore, we use the approach of Oster (2016) to provide evidence of causal effects that does not rely on exclusion restrictions.

Our work adds to the literature documenting unintended consequences of restricting access to alcohol. Our results are consistent with the work of Conlin et al. (2005), Dinardo and Lemieux (2001), and others who have found evidence of substitution between alcohol and other drugs. Our results support the idea that prohibiting the sale of alcohol lowers the relative cost of participating in the market for illegal drugs.

Finally, our work has implications for policy aimed at reducing the harm caused by the use and production of methamphetamine. The most notable policies intended to reduce meth production have been restrictions on precursors. Even though studies of earlier interventions (Cunningham & Liu, 2003, 2005; Dobkin & Nicosia, 2009) found that these policies had only temporary effects on the supply of meth, most states and the Federal government had placed restrictions on the purchase of pseudoephedrine (a common cold medicine) by 2006. The most careful study we have seen of precursor restrictions, Dobkin, Nicosia, and Weinberg (2014), estimates that the restrictions reduced the number of meth labs in a state by around 36 percent, which is comparable to our estimate of the effect of ending local alcohol prohibition. Although it's not clear how well our results would generalize to other states or to substances other than alcohol, our study provides an example in which liberalizing the treatment of one substance may be an effective policy tool for reducing harm associated with other substances.

Figure 1: Meth Lab Seizures per county (darker green higher values)

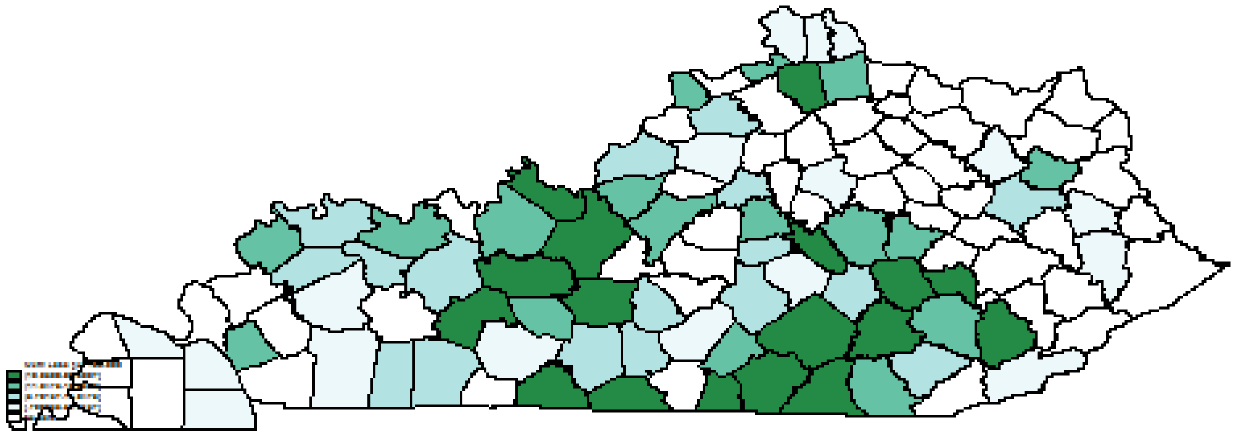

Figure 2: Wet (darkest, red), Moist, and Dry (lightest, yellow) County Status

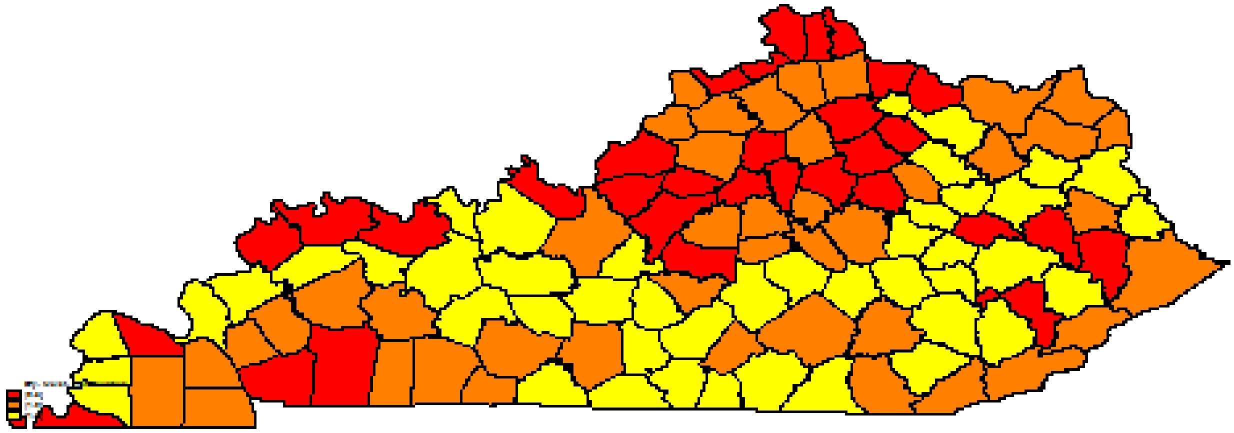

Table 1: Means of outcome and control variables

County Demographic Variables	Wet	Moist	Dry
Meth lab seizures rate (DEA)[a,b]	2.17	2.26	3.92
Non-narcotic Drug Possession rate (UCR)	98.8	95.9	90.8
Non-narcotic Drug Sale/Manufacture rate (UCR)	76.8	89.0	91.9
All Meth Related Incidences (KSP) rate [a,b]	42.2	55.5	81.2
Property Crime Rate [a,b,c]	451	358	267
Violent Crime Rate [a,b,c]	101	79.9	60.8
ER Burns rate[a]	132	137	149
Population (1000's) [a,b,c]	70.1	38.4	20.2
Population Density [a,b,c]	245	111	60.7
Median Household Income ($1000) [a,b,c]	40.4	37.2	32.5
Pct. Access to Interstate Highway [a,b]	40.1	43.0	21.6
Pct. Resident Workers/ Total Employment [a,b,c]	48.7	56.1	53.1
Pct. Black [a,b,c]	6.38	3.79	2.57
Pct. Children Obese	17.2	17.4	17.2
Pct. College [a,b]	16.3	15.6	11.5
Pct. Female Labor Force Participation [a,b]	34.1	32.4	30.0
Pct. Male [a,b]	49.0	49.0	49.6
Pct. Married [a,b,c]	54.0	55.5	56.5
Pct. Widowed [a,b]	7.13	7.28	8.16
Pct. Poverty [a,b,c]	17.7	19.3	21.4
Pct. Poverty under 18 years old [a,b]	24.3	25.2	27.9
Pct. Public Assistance[a]	2.64	2.72	2.93
Pct. Under 21 years old [a,b]	28.8	28.4	27.4
Pct. Over 65 years old [a,b]	12.6	12.9	14.4
Pct. Any Religion	53.1	50.8	50.5
Pct. Baptist[a,c]	30.0	32.8	35.2
Pct. Baptist of All Religion[a,c]	56.6	65.7	67.4
Pct. Any Religion in 1936 [a,b,c]	38.2	30.6	26.9
Pct. Baptist in 1936	12.8	12.0	13.5
Pct. Black Baptist in 1936 [a,b,c]	3.30	2.61	1.56
Pct. Baptist of All Religion in 1936 [a,b,c]	34.8	38.5	49.3
Population in 1936 (1000's)	37.1	27.7	15.9

Note: DEA = Drug Enforcement Agency, KSP = Kentucky State Police, and UCR = FBI Uniform Crime Report. County level demographics are collected from the American Community Survey. Religion characteristics in 1936 are collected from Hayes (2010) and contemporary religion data are collected from the Association of Statisticians of American Religious Bodies. All rates are calculated per 100,000 people in the county population. Equal means t-test at α=.05 are conducted for each pair of groups. Significant outcomes are indicated: a = wet vs dry, b = moist vs dry, c = wet vs dry.

Table 2: Effect of Alcohol Access on DEA Meth Lab Seizures per 100,000. Controlling for Observable Heterogeneity.

	Full Sample	Counties on Common Support		
	OLS	PS	OLS	OLS[††]
Wet	-1.469**	-2.482***	-1.505**	-1.082*
	(0.608)	(0.336)	(0.641)	(0.566)
Moist	-1.029*	-2.100***	-1.093**	…
	(0.535)	(0.413)	(0.537)	
R^2	0.20		0.21	0.21
Pct. Pop. Wet	-1.166*	…	-1.167*	…
	(0.599)		(0.635)	
R^2	0.20		0.21	
Liquor Stores per cap	-0.013	…	-0.038	…
	(0.025)		(0.033)	
R^2	0.19		0.21	
Observations	840	656/369[†]	800	800

Robust standard errors in parentheses. Propensity score estimates use Abadie–Imbens robust standard errors.
*** p<0.01, ** p<0.05, * p<0.1
All specifications use current county demographic information, current religious organization membership, county latitude and longitude, interstate highway access, Census commuting patterns, state and dry county border dummies, and year fixed effects.
Common Support limits the sample to observations with $\Pr(dry)$ between 0.00001 and 0.99999.
[†] Propensity score estimates are constructed by comparing wet vs dry (n=656) and moist vs dry (n=369) separately.
[††] Moist counties are included with dry counties in this regression.

Table 3: Effects of Alcohol Access on DEA Meth Lab Seizures per 100,000. Using Religious Composition in 1936 as Instrumental Variables.

		Added Controls		Strict Common Support†	
	Main IV	1990 Religion	Prop. Score Polynomial	Main IV	Added Controls
Wet††	-2.377**	-2.528**	-3.118**	-2.077*	-2.477**
	(1.136)	(1.152)	(1.270)	(1.077)	(1.190)
First-stage ***F*** stat	91.77	79.45	61.29	92.22	58.31
Pct. Pop. Wet	-2.580**	-2.274*	-3.476**	-2.348*	-2.716**
	(1.282)	(1.347)	(1.446)	(1.208)	(1.363)
First-stage ***F*** stat	83.29	71.75	53.03	78.08	51.52
Liquor Stores	-0.0917**	-0.0940	-0.112**	-0.0857*	-0.118*
	(0.0454)	(0.0578)	(0.0497)	(0.0443)	(0.0626)
First-s**tage *F*** stat	119.78	75.35	109.39	118.89	60.77
Observations	800	800	800	749	749

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

All specifications control for the same county characteristics as in Table 2, including current religious composition. The instrumental variables are the percent of the population in 1936 who belong to a religious congregation, and that percent squared. The 1990 religion variables added in the second and fifth columns are the percent belonging to a congregation, that percent squared, and the number of Baptist adherents in the county. The third and fifth columns add a cubic polynomial in the estimated $\Pr(not\ wet)$.

†The stricter common support restriction in the last two columns drops observations from wet counties with a predicted $\Pr(dry) < 0.03$, and from dry counties with $\Pr(dry) > 0.9999$. The sample in the first three columns only limits attention to observations with $\Pr(dry)$ between 0.00001 and 0.99999.

†† Moist counties are included with dry counties in this estimation

Table 4: Effect of Instruments on Meth Labs in Counties that were Dry in 1994, Compared to Reduced Form Coefficients for Estimation Sample.

	Reduced Form on Estimation Sample		***Subsample that was Dry in 1994***	
	Main IV	1990 Religion	Main IV	1990 Religion
1936 % Religious	0.172*	0.303**	-0.024	0.189
	(0.0972)	(0.137)	(0.187)	(0.255)
Religious Squared	-0.0024**	-0.0040**	-0.0005	-0.0037
	(0.0011)	(0.0016)	(0.0031)	(0.0038)
Joint $\boldsymbol{p}$-value	0.0596	0.0288	0.6554	0.4708
Observations	800	800	506	506

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1
All specifications control for the same county characteristics as in Tables 2 and 3, including current religious composition. The 1990 religion variables are the percent belonging to a congregation, that percent squared, and the number of Baptist adherents in the county.

Table 5: Proportion of Selection on Unobservables Needed for Zero Treatment Effect

	Primary Estimation Sample			**Strict Common Support**		
	Main	1990 Religion	Prop. Score Polynomial	Main	1990 Religion	Prop. Score Polynomial
Wet	-1.082*	-1.102*	-1.258**	-1.268**	-1.270**	-1.210**
	(0.566)	(0.573)	(0.590)	(0.559)	(0.571)	(0.583)
Estimated R^2	0.209	0.209	0.211	0.220	0.221	0.224
Coefficient of Proportionality for Zero Effect						
$R^2_{max} = 0.8$	1.106	1.195	2.703	3.085	3.163	2.178
Pct. Pop. Wet	-1.167*	-1.213*	-1.294*	-1.557**	-1.611**	-1.518**
	(0.635)	(0.649)	(0.670)	(0.632)	(0.661)	(0.650)
Estimated R^2	0.208	0.209	0.210	0.221	0.222	0.225
Coefficient of Proportionality for Zero Effect						
$R^2_{max} = 0.8$	0.946	1.102	1.492	22.399	-17.219	8.325
Observations	800	800	800	749	749	749

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1
Regressions in the first column are the same as OLS regressions presented in third and fourth columns of Table 2. All specifications control for the same county characteristics as in previous tables, including current religious composition. The 1990 religion variables added in the second and fifth columns are the percent belonging to a congregation, that percent squared, and the number of Baptist adherents in the county. The third and sixth columns add a cubic polynomial in the estimated Pr(*not wet*).
The "Primary Estimation Sample" limits attention to observations with Pr(*not wet*) between 0.00001 and 0.99999. The stricter common support restriction drops observations from wet counties with predicted Pr(*not wet*) < 0.03, and from dry counties with Pr(*not wet*) > 0.9999.

Table 6: Effect of ER visits for Burns per 100,000

		Counties on Common Support		
	OLS	PS	OLS	IV
Wet	-20.23**	-24.30***	-21.34**	-61.90***†
	(9.749)	(9.247)	(10.27)	(23.20)
Moist	-13.97	-45.22	-13.91	
	(9.981)	(40.66)	(9.980)	
First-stage ***F*** stat				31.86
Pct. Pop. Wet	-14.94		-16.57	-70.16***
	(9.687)		(10.30)	(26.06)
First-stage ***F*** stat				27.22
Liquor Stores	-0.505		-0.815*	-3.112***
	(0.365)		(0.488)	(1.119)
First-stage ***F*** stat				34.58
Observations	345	274/151††	327	327

Robust standard errors in parentheses. Propensity score uses Abadie–Imbens robust standard errors.
*** p<0.01, ** p<0.05, * p<0.1
All specifications include current county demographic information, religious membership, county latitude and longitude, interstate highway access, Census commuting patterns, as well as state border and dry county border dummies. The IV specifications use religious membership for 1936 as instruments. Full Sample results use all Kentucky counties between 2004 – 2010. Common Support restricts the sample to include counties with overlapping propensity scores of the Pr(dry).
† Moist counties are included with dry counties in this estimation.
†† Propensity score estimates compare wet vs dry (n=274) and moist vs dry (n=151) separately.

Table 7: Effects of Alcohol Access on Drug Arrests,

	Counties on Common Support			
	OLS	PS	OLS	IV
Total Meth-related Arrests (KSP)				
Wet	-22.41**	-32.03***	-20.58**	-47.65**
	(8.762)	(9.292)	(8.644)	(19.25)
Moist	-14.99	-27.63**	-14.42	
	(9.196)	(13.55)	(9.443)	
Pct. Pop. Wet	-17.62**		-15.54*	-49.00**
	(8.695)		(8.593)	(21.74)
Liquor Stores	-0.0510		-0.621	-1.741**
	(0.366)		(0.472)	(0.758)
Total Synthetic Drug Arrests (UCR)				
Wet	-5.623	-3.127	-5.552	-28.41***
	(4.182)	(2.846)	(4.294)	(10.34)
Moist	8.227	-23.70*	8.986	
	(6.197)	(12.90)	(6.187)	
Pct. Pop. Wet	-5.599		-5.615	-31.06***
	(4.155)		(4.333)	(11.59)
Liquor Stores	0.00698		-0.146	-1.104***
	(0.159)		(0.189)	(0.413)
Total Synthetic Drug Arrests (KSP)				
Wet	-3.952	-17.45***	-1.455	-34.80**
	(9.577)	(6.092)	(9.571)	(16.42)
Moist	-7.249	-26.41***	-6.883	
	(7.066)	(7.070)	(6.979)	
Pct. Pop. Wet	-1.456		1.352	-35.77*
	(9.544)		(9.626)	(18.60)
Liquor Stores	0.485		0.175	-1.271*
	(0.446)		(0.636)	(0.661)

Robust standard errors in parentheses. Propensity score uses Abadie–Imbens robust standard errors.
*** p<0.01, ** p<0.05, * p<0.1
All specifications include current county demographic information, religious membership, county latitude and longitude, interstate highway access, Census commuting patterns, as well as state border and dry county border dummies. The IV specifications use religious membership for 1936 as instruments. Full Sample results use all Kentucky counties between 2004 – 2010. Common Support restricts the sample to include counties with overlapping propensity scores of the Pr(dry).
† Propensity score estimates are constructed by comparing wet vs dry (n=655) and moist vs dry (n=445) separately.
†† Moist counties are included with dry counties in this estimation

Table 8: Falsification Test: "Effects" on the Percent of Children who are Obese

	Full Sample	Counties on Common Support		
	OLS	PS	OLS	IV
Wet	0.004	-0.004	0.006	-0.009†
	(0.005)	(0.004)	(0.005)	(0.009)
Moist	-0.0004	0.0019	-0.002	
	(0.004)	(0.0055)	(0.004)	
Pct. Pop. Wet	0.004		0.007	-0.0005
	(0.005)		(0.005)	(0.0095)
Liquor Stores per cap	0.0001		-0.0002	-0.00003
	(0.0002)		(0.0002)	(0.0003)
Observations	811	640/363††	776	776

Robust standard errors in parentheses, except for propensity score which uses Abadie–Imbens robust standard errors. *** p<0.01, ** p<0.05, * p<0.1

All specifications use current county demographic information, current religious organization membership, county latitude and longitude, interstate highway access, Census commuting patterns, as well as state border and dry county border dummies. The instrumental variable specifications use religious organization membership for 1936 as instruments. Full Sample results use the full sample of Kentucky counties between 2004 – 2010. Common Support restricts the sample to include counties with overlapping propensity scores of the Pr(dry).

† Moist counties are included with dry counties in this estimation.

†† Propensity score estimates are constructed by comparing wet vs dry (n=275) and moist vs dry (n=153) separately.

Appendix 1: Means of outcome and control variables for Counties on the Common Support

County Demographic Variables	Wet	Moist	Dry
Meth lab seizures Rate (DEA) [a,b]	2.34	2.26	3.79
Synthetic Narcotic Arrest Rate (KSP)	42.2	42.7	53.7
Synthetic Narcotic Possession Rate (UCR)[a,b]	29.9	36.5	23.4
Synthetic Narcotic Sale/Manufacture Rate (UCR)	18.5	25.2	21.1
Non-narcotic Drug Possession Rate (UCR)	98.0	95.9	90.8
Non-narcotic Drug Sale/Manufacture Rate (UCR)	77.2	89.0	91.3
All Meth Related Arrest Rate (KSP)[a]	44.9	55.5	76.3
Property Crime Arrest Rate [a,b]	390.1	385.3	272.0
Violent Crime Arrest Rate [a,b]	86.4	79.9	61.9
ER Burns Rate[a]	134.6	138.6	149.3
Population (1000's) [a,b]	34.1	38.6	21.6
Population Density [a,b]	122.3	109.7	61.0
Median Household Income ($1000) [a,b, c]	37.9	35.9	30.9
Pct. Access to Interstate Highway [a,b]	32.2	43.0	20.2
Pct. Resident Workers/ Total Employment [a,b, c]	49.4	56.1	53.4
Pct. Black [a,b, c]	5.10	3.84	2.57
Pct. College [a,b]	14.1	15.3	11.2
Pct. Children Obese	16.9	17.2	17.3
Pct. Female Labor Force Participation [a,b]	37.6	36.1	34.1
Pct. Male [a,b]	49.1	49.0	49.4
Pct. Married[a]	55.5	56.1	57.3
Pct. Widowed [a,b]	7.29	7.31	8.06
Pct. Poverty [a,b]	17.7	19.0	21.7
Pct. Poverty under 18 years old [a,b]	23.7	24.2	28.0
Pct. Public Assistance[a]	3.02	3.04	3.59
Pct. Under 21 years old[a,b]	29.1	28.6	27.9
Pct. Over 65 years old[a,b]	12.6	12.8	14.0
Pct. Any Religion	51.9	50.8	49.3
Pct. Baptist[a,c]	30.3	34.6	34.7
Pct. Baptist of All Religion[a]	58.9	65.7	66.7
Pct. Baptist in 1936	13.2	12.0	13.2
Pct. Black Baptist in 1936[a,b]	3.3	2.6	1.6
Pct. Any Religion in 1936[a,b,c]	37.4	30.4	26.5
Pct. Baptist of All Religion in 1936[a,b]	44.5	46.2	54.5
Population in 1936 (1000's)[a,b,c]	22.1	27.4	16.6

Note: DEA = Drug Enforcement Agency, KSP = Kentucky State Police, and UCR = FBI Uniform Crime Report. Common Support restricts the sample to include counties with overlapping propensity scores of the Pr(dry). County level demographics are collected from the American Community Survey. Religion characteristics in 1936 are collected from Hayes (2010) and contemporary religion data are collected from the Association of Statisticians of American Religious Bodies. All rates are calculated per 100,000 people in the county population. Equal means t-test at α=.05 are conducted for each pair of groups. Significant outcomes are indicated: a = wet vs dry, b = moist vs dry, c = wet vs dry.

Appendix 2: Poisson Count Models for Various Outcomes
Analogous to OLS Estimates on Common Support from Tables 2, 6 and 7

	Meth Labs (DEA)	ER Burn Visits	Meth Arrests (KSP)	Synth. Narcotic Arrests (UCR)	Synth. Narcotic Arrests (KSP)
Wet County	-0.591***	-0.160***	-0.490***	-0.105	-0.0489
	(0.202)	(0.0324)	(0.0651)	(0.0693)	(0.160)
Moist County	-0.430***	-0.113	-0.426***	0.156	-0.0857
	(0.0796)	(0.0752)	(0.117)	(0.112)	(0.161)
Pct. Pop. Wet	-0.528**	-0.124***	-0.441***	-0.117**	-0.0204
	(0.211)	(0.0347)	(0.0718)	(0.0522)	(0.160)
Liquor Stores	-0.0168	-0.00612***	-0.0173**	0.00430	0.00430
	(0.0125)	(0.000597)	(0.00868)	(0.0131)	(0.0131)
Observations	800	327	800	800	800

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1
All specifications use current county demographic information, current religious organization membership, county latitude and longitude, interstate highway access, Census commuting patterns, as well as state border and dry county border dummies. The sample size is restricted to include counties with overlapping propensity scores of the Pr(dry).

Appendix 3: Inverse Propensity Score Weighting Estimates for Various Outcomes
Analogous to Propensity Score Matching Estimates from Tables 2, 6 and 7

	Meth Labs (DEA)	ER Burn Visits	Meth Arrests (KSP) †	Synth. Narcotic Arrests† (KSP)	(UCR)
Wet County	-3.288***	-25.14	-39.13**	-29.59***	-1.701
	(0.544)	(15.50)	(16.22)	(6.187)	(4.960)
Moist County	-2.012***	-23.58**	-15.83	-16.79**	14.86*
	(0.727)	(10.50)	(13.29)	(7.268)	(8.672)
Observations	770	317	770	770	770

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1
All models control county demographic information, current religious organization membership, county latitude and longitude, interstate highway access, Census commuting patterns, as well as state border and dry county border dummies.The sample size is restricted to include counties with overlapping propensity scores.
†IPW models for arrest rates did not converge.

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