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RELIGIOUS PARTICIPATION AND RISKY HEALTH BEHAVIORS AMONG ADOLESCENTS

JENNIFER M. MELLOR[a,*] and BETH A. FREEBORN[b]

[a]*Department of Economics and Thomas Jefferson Program in Public Policy, College of William and Mary, Williamsburg, VA, USA*
[b]*Department of Economics College of William and Mary Williamsburg, VA, USA*

SUMMARY

Previous studies have shown that adolescent religious participation is negatively associated with risky health behaviors such as cigarette smoking, alcohol consumption, and illicit drug use. One explanation for these findings is that religion directly reduces risky behaviors because churches provide youths with moral guidance or with strong social networks that reinforce social norms. An alternative explanation is that both religious participation and risky health behaviors are driven by some common unobserved individual trait. We use data from the National Longitudinal Study of Adolescent Health and implement an instrumental variables approach to identify the effect of religious participation on smoking, binge drinking, and marijuana use. Following Gruber (2005), we use a county-level measure of religious market density as an instrument. We find that religious market density has a strong positive association with adolescent religious participation, but not with secular measures of social capital. Upon accounting for unobserved heterogeneity, we find that religious participation continues to have a significant negative effect on illicit drug use. On the contrary, the estimated effects of attendance in instrumental variables models of binge drinking and smoking are statistically imprecise. Copyright © 2010 John Wiley & Sons, Ltd.

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1. INTRODUCTION

Religious participation has been linked to several beneficial outcomes such as increased educational attainment, reduced divorce rates, and lower mortality. In addition, many studies report that religious participation is associated with a reduced propensity to engage in risky health behaviors such as smoking, drinking, and illicit drug use. Findings from the adolescent population are of special interest, given the consequences of risky behaviors for both long-term health and educational attainment. It may be that religion directly deters adolescents from engaging in risky behaviors by instilling moral values and self-control skills. Alternatively, religious participation may deter risky behaviors by helping adolescents develop social networks, which provide social support and reinforce widely accepted social norms.

Many scholars, however, have expressed concerns regarding the causal nature of the association between religion and adolescent risky health behaviors. Iannaccone (1998) notes that unobservable characteristics may be correlated with both religious participation and individual behavior. If "'good'' kids …avoid drugs, stay in school *and* go to church,' then the statistical relationship may represent correlation rather than causation (p. 1475). Yet, few studies in this area have addressed the problem of

*Correspondence to: Department of Economics, College of William and Mary, P.O. Box 8795, Williamsburg, VA 23187-8795, USA. E-mail: jmmell@wm.edu

unobserved heterogeneity directly. A handful of studies on adult religious participation have employed instrumental variables (IV) approaches (e.g. Gruber, 2005; Gruber and Hungerman, 2008; and Borgonovi, 2008). The only such study to do so in an adolescent sample is Lillard and Price (2007), which finds mixed support for the causal effects of religion on youth smoking, drinking, and drug use.

This study adds to the small literature on the causal effects of religious participation on adolescent risky behaviors. We employ data from the National Longitudinal Study of Adolescent Health (Add Health). Following Gruber (2005), we use a county-level measure of religious market density as an IV for religious participation. We first demonstrate that religious market density has a strong positive effect on the frequency of religious participation among adolescents. Further, we find that even upon accounting for the endogeneity of religious participation, adolescents who attend services more frequently are less likely to use marijuana. On the contrary, the IV estimates of attendance in models of drinking and smoking are not statistically precise, and definitive conclusions about these behaviors are difficult to reach.

2. PREVIOUS LITERATURE

The literature on the association between religion and risky health behaviors is vast. Rew and Wong (2006) describe more than 43 empirical studies of adolescent health behaviors and religion, and Johnson *et al.* (2002) review over 150 studies on religiosity and substance abuse. Despite the large number of studies, however, only a few attempt to identify a causal effect of religion on behaviors or outcomes.

The small number of such studies highlights the challenge of identifying a factor that affects religious participation, but has no effect on behaviors. Although no study has identified a valid individual-level instrument, several have employed area-level IVs building on prior economic research on religious markets. As Iannaccone (1998) describes, this research dates to the observation by Adam Smith (1776) in *The Wealth of Nations*, and includes several, more recent, empirical analyses of religious participation and religious market traits. As described below, the studies using market traits to identify religious participation effects differ in the specific instrument used, the age of the population studied, and the outcomes examined.

Focusing on adults and young adults, Gruber and Hungerman (2008) employ a novel identification strategy based on the repeal of 'blue laws' across the US states largely in the 1970s and 1980s. They posit that blue laws, which ban commercial activities such as shopping and purchasing alcohol on Sundays, insulate religious activity from secular competition. Thus, the repeal of these laws should expand alternatives to religious participation and decrease service attendance. Further, if attendance has a deterrent effect on risky health behaviors, then blue law repeals should increase heavy drinking and drug use. Using data from the General Social Survey (GSS), Gruber and Hungerman first show that the repeal of blue laws led to declines in both religious participation and religious donations by adults. They then demonstrate that among young adults in the National Longitudinal Survey of Youth (NLSY-79), blue law repeal led to significant increases in marijuana and cocaine consumption.

Lillard and Price (2007) follow Gruber and Hungerman's blue law approach to study youth risky behaviors. Using data from the Panel Study of Income Dynamics, and focusing on respondents between ages 5 and 30, the authors examine the onset of smoking behavior. Interestingly, respondents living in states that repealed blue laws were significantly less likely to start smoking. However, the interaction of living in a state that repealed a blue law and belonging to a religion for which Sunday is the day of obligation had a positive effect on smoking onset (albeit somewhat smaller than the negative coefficient on the 'no blue law' dummy). Differences between these findings and Gruber and Hungerman's may be due to differences in the age group and the outcome studied (illicit drug use versus smoking onset), or differences in the states included the sample. Gruber and Hungerman restrict their analysis to 16 states, whereas Lillard and Price use a larger group.

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Lillard and Price (2007) also use propensity score matching to examine the effect of religious participation on youth behaviors. Using data from the Monitoring the Future surveys, they find that adolescents who participate in religious services monthly or more often are less likely to smoke, drink, and use drugs than those who go to church less often. Since matching estimators do not account for selection on unobservables, the authors follow Altonji *et al.* (2005) in calculating the amount of selection on unobservables that could explain their results. With the exception of current smoking, most findings from the propensity score matching exercise could be explained if the selection on unobservables was just a small fraction of the selection on observables.

Two additional studies use identification strategies based on the characteristics of the religious market. Using data from the GSS, Gruber (2005) examines the link between religious participation and religious market density. Religious market density, or the share of the local population that is of an individual's own religion, may increase religious participation by reducing the distance to religious services and increasing access to social interactions linked to religion. Prior empirical research by Phillips (1998) and Olson (1998) provides evidence that church attendance increases with religious density. Although density can be calculated from the GSS by aggregating respondents by area, the survey's relatively small sample makes this prone to measurement error. Partly out of concern for this, Gruber also uses a measure of 'predicted religious density' based on the share of the area population that is of an ancestry that shares the individual's religion and calculated from the much larger Census public use microdata sample. Results show that the GSS-based measure of religious market density has a positive effect on GSS measures of attendance. Predicted religious density has a positive and significant effect on outcomes such as incomes and educational attainment, and a negative association with divorce rates. These results are consistent with the notion that religious density increases attendance which in turn leads to better economic outcomes.[1]

Finally, Borgonovi (2008) uses data on adult respondents to the Social Capital Community Benchmark Survey (SCCBS), and focuses on self-rated health and happiness as measures of individual well-being. Drawing on past findings that church attendance is greater in areas characterized by more competition among denominations, Borgonovi constructs a Herfindahl index of religious market concentration using county-level data on the shares of adherents bellowing to various denominations. Values closer to 1 indicate more concentration (less competition) and values closer to 0 indicate less concentration (more competition). She finds that volunteering decreases with greater religious concentration, and that upon instrumenting for volunteering with concentration, religious volunteering has a positive and significant effect on happiness but an insignificant effect on health.

Our research builds on this small group of studies. Like Lillard and Price (2007), we focus exclusively on youths; like Gruber and Hungerman (2008), we look at heavy drinking and illicit drug use in addition to smoking. We use a measure of religious market density as an instrument, making our identification strategy most comparable to Gruber (2005).

3. DATA AND ESTIMATION

We use data from Wave I of the National Longitudinal Study of Adolescent Health, or Add Health, a nationally representative study of roughly 20 000 adolescents in grades 7 through 12. Between April and December of 1995, respondents completed in-home interviews with detailed questions on tobacco, drug, and alcohol use, as well as their religious affiliation and attendance patterns, household composition, and demographic traits. Our analysis also uses data from interviews of the respondent's parent or guardian, surveys the adolescents completed in school, and surveys completed by administrators of the schools.

[1]Dehejia *et al.* (2007) consider a similar strategy for identifying the effect of parental religious involvement on children's outcomes. However, the instrument yielded imprecise estimates attributed to the small sample size (1125 observations).

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One advantage of Add Health is that it employs specific fielding procedures for data on sensitive topics. Most questions on the survey were posed directly to respondents by an interviewer who entered the answers into a computer. However, for the sensitive topics on which we focus (smoking, drinking, and drug use), adolescents listened to the questions on headphones and entered their answers on a laptop themselves. This approach has been shown to increase the validity of adolescent responses (e.g. Turner *et al*., 1998). Procedures were used to ensure that no one other than the adolescent heard the sensitive questions or answers given.

A limitation of Add Health is that geographic identifiers are unavailable in either the public or the restricted use data, thus preventing researchers from merging individual records to local characteristics. This restriction protects the confidentiality of these highly sensitive data (thus, it may have the advantage of improving response rates and minimizing response bias). To define area-level variables including our instrument, we obtained the restricted use version of the Add Health data which includes access to a database of over 2600 'contextual variables' defined at the level of the state, county, census tract, or block level and prepared by the Add Health survey staff.

Our instrument for religious participation is religious market density, which is defined for each respondent as the proportion of the county population belonging to the same denomination as the respondent. We construct this with several variables in the contextual database taken from the 1990 survey of Churches and Church Membership (CCM), a survey conducted every 10 years by the Association of Statisticians of American Religious Bodies (ASARB). In 1990, the ASARB survey included 33 Judeo-Christian church/congregational groupings. Congregations were asked to provide data on the number of adherents including full members of the congregation, children of full members, and persons who participate in services regardless of membership status. From the 1990 ASARB survey, the Add Health contextual database includes the proportion of the county population belonging to several broadly defined denominations (Catholics, Jews, Moderate Protestants, Liberal Protestants, and Conservative Protestants) and the total proportion of religious adherents belonging to these five groups.

Our measure of religious market density differs from the one used in Gruber (2005). First, Gruber's market density measure is based on seven classifications – the five we use plus 'other' and 'no religion.' Because the contextual database does not include either the proportion of the population belonging to another religion or the proportion adhering to no religion, these groups cannot be distinguished in our data. A second difference is that Gruber instruments for market density using the ancestral density of other ancestries with shared religious preferences as an instrument. His IV approach is partly motivated by concerns for measurement error since religious density is calculated from GSS responses aggregated by primary sampling unit. Our measure of density is not calculated from the Add Health microdata, but it may, however, pick up some majority status effects, i.e., that religious participation is driven by membership in a large group, but not necessarily a large *religious* group. Unfortunately, the contextual database does not include the area-level ancestry proportions required to execute Gruber's exact approach. We return to this issue in the *Results* section.

The data and identification strategy limit our sample in two ways. First, our sample excludes respondents who did not report a religious affiliation. Not only can religious market density not be defined for this group, but respondents who did not report an affiliation were not asked about religious attendance. This exclusion is made in prior studies of religious participation using Add Health data (e.g. Rostosky *et al*., 2007; Nonnemaker *et al*., 2003). As the GSS asks about religious attendance for those who do not report an affiliation, Gruber (2005) is able to examine whether excluding nonaffiliates makes a difference. He finds no substantive difference in the effects of density on attendance whether nonaffiliates are included or excluded.

Second, we lose respondents in the 'other' category, that is, respondents whose religious affiliation does not fit within the five groups for which we have religious density information. Specific Protestant denominations were classified into one of three groups following the construction of the Add Health

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contextual variables for the proportions of Moderate, Conservative, and Liberal Protestants.[2] Because we are not able to assign religious market density to adolescents from various 'other' Protestant sects and from non-Judeo-Christian faiths, our sample excludes Christian Scientists, Jehovah's Witnesses, and other undefined Protestants, as well as Baha'i, Buddhists, Hindus, Muslims, Eastern Orthodox, and 'other' unspecified religious affiliations.

These restrictions affect our sample size, but not as much as missing data on other required variables. Although the full Wave I sample contains 20 745 observations, about 1800 are missing sampling weights and almost 400 respondents did not provide a response to the religious affiliation question. Of the remaining, about 3500 adolescents were missing parent interview data on age, education, and income (used as explanatory variables in our models).[3] Missing data on adolescent and school-level explanatory variables brought the sample size to about 13 978 individuals. Excluding the nonreligious drops 1714 observations, and excluding those with denominations outside of the five-category scheme drops 1188 observations. The means in Table I are based on the resulting 11 077 observations; missing data on the risky behaviors reduce the estimation sample sizes slightly.

The descriptive statistics in Table I are calculated with the survey-provided sampling weights. The first portion of the table reports summary statistics for adolescent characteristics, starting with the three behaviors on which we focus. Smoking is represented by an indicator variable equal to 1 if the youth reports smoking at least one cigarette in the past 30 days. Our measure of alcohol use is binge drinking, which equals 1 if the adolescent consumed five or more drinks in a row during the past 12 months. For drug use, we define a variable equal to 1 if the respondent used marijuana within the past 30 days.[4] About one-fourth of the sample reported smoking and binge-drinking, and 13% used marijuana. The smoking participation rate is consistent with several estimates from prior studies (e.g., Ross and Chaloupka, 2003; Powell *et al*., 2005). The marijuana and alcohol consumption rates are consistent with rates reported by Nonnemaker *et al*. (2003) and Clarke and Lohéac (2007). To measure religious participation, we use the survey question: 'In the past 12 months, how often did you attend religious services?' We recode the four possible responses to this question to create a scale that equals 1 for never attends, 2 for less than once a month, 3 for once a month or more but less than once a week, and 4 for weekly or more. The mean response is 2.958, with a standard deviation of 1.09.

Table I also presents means for the explanatory variables in models of religious participation and risky behaviors. We control for the adolescent's religious affiliation, sex, age, race and ethnicity, and for parent age, educational attainment, residence in the household, and mother's employment status. We include controls for household size and family income; given the number of refusals by parents to the income question (even for those who completed the parent questionnaire), we created an indicator variable for refusals and a set of indicator variables based on the ranges of parent income, coding these to zero for the refusals.

To account for as many influences on religious participation and risky behaviors as possible, we also control for a large number of area-level and school-level characteristics. Using data from the contextual database, we control for median household income in the county, county-level race density (percentage of the population that is of the same race as the respondent), county-level age and sex density

[2]See the documentation from Parts 1 and 2 of the Add Health Wave I Contextual Database. Specifically, Liberal Protestants include Episcopalians, Methodists, Presbyterians, United Church of Christ, Friends/Quaker, and Unitarians. Moderate Protestants include Lutherans, National Baptists, Disciples of Christ, and Black Baptists. Conservative Protestants include the Adventists, AME, Assemblies of God, Baptists, Congregationalists, Pentecostals, Holiness, and Latter Day Saints. This breakdown is similar to that followed by Gruber (2005) and Roof and McKinney (1987). One difference is that these two sources classify Methodists as moderate Protestants.

[3]Cases where parents were asked the income question, but refused to answer, are included in the sample; their treatment is described below.

[4]We focus on marijuana consumption because the rates of other drug usage were very low. Only 1% report use of cocaine, 2% use inhalants, and 4% use a combined listing of 'other' drugs including LSD, ecstasy, PCP, mushrooms, speed, heroin, and pills without prescriptions.

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Table I. Descriptive statistics

Variable	Definition	Mean
Adolescent risk behaviors (*samples vary*)		
Smoke	Equals 1 if adolescent smoked at least 1 day in the past 30 days; 0 otherwise ($n = 10\,948$)	0.257
Binge drink	Equals 1 if adolescent drank five or more drinks in a row at least 1 day in past 12 months; 0 otherwise ($n = 10\,972$)	0.254
Marijuana use	Equals 1 if adolescent smoked marijuana at least once in past 30 days; 0 otherwise ($n = 10\,926$)	0.130
Adolescent-level explanatory variables ($n = 11\,077$)		
Religious attendance	Equals 1 if never attends, 2 if attends less than once a month; 3 if attends at least once a month but not weekly; 4 if attends weekly or more	2.958
Catholic	Equal to 1 if adolescent is Catholic; 0 otherwise	0.318
Mod. Protestant	Equal to 1 if adolescent is Moderate Protestant; 0 otherwise	0.166
Lib. Protestant	Equal to 1 if adolescent is Liberal Protestant; 0 otherwise	0.117
Con. Protestant	Equal to 1 if adolescent is Conservative Protestant; 0 otherwise	0.393
Jewish	Equal to 1 if adolescent is Jewish; 0 otherwise	0.007
Age	Adolescent's age in years	15.78
Female	Equal to 1 if adolescent is female; 0 otherwise	0.500
Hispanic	Equal to 1 if adolescent is Hispanic; 0 otherwise	0.120
Black	Equal to 1 if adolescent is Black; 0 otherwise	0.163
Asian	Equal to 1 if adolescent is Asian; 0 otherwise	0.030
Other race	Equal to 1 if adolescent is another race; 0 otherwise	0.077
Parent and household-level explanatory variables ($n = 11\,077$)		
Parent age	Age of primary parent, in years	41.48
High school	Equal to 1 if primary parent's highest level of education is high school or GED; 0 otherwise	0.335
Some college	Equal to 1 if primary parent's highest level of education is some college; 0 otherwise	0.292
College grad	Equal to 1 if primary parent's highest level of education is college degree; 0 otherwise	0.131
Graduate degree	Equal to 1 if primary parent's highest level of education is graduate degree; 0 otherwise	0.079
Income 2	Equal to 1 if household income $\geq$ 10th and $<$25th percentile; 0 otherwise	0.115
Income 3	Equal to 1 if household income $\geq$ 25th and $<$50th percentile; 0 otherwise	0.250
Income 4	Equal to 1 if household income $\geq$ 50th and $<$75th percentile; 0 otherwise	0.237
Income 5	Equal to 1 if household income $\geq$ 75th and $<$90th percentile; 0 otherwise	0.126
Income 6	Equal to 1 if household income $\geq$ 90th percentile; 0 otherwise	0.105
Income refused	Equal to 1 if parent refused to answer household income question	0.094
Household size	Number of persons in household roster	3.48
Mother present	Equal to 1 if biological mother is listed in household roster; 0 otherwise	0.890
Father present	Equal to 1 if biological father is listed in household roster; 0 otherwise	0.613
Mother works	Equal to 1 if resident/biological mother works; 0 otherwise	0.743
Mother works missing	Equal to 1 if adolescent does not know or refuses to answer mother's work status; 0 otherwise	0.042
Area-level explanatory variables and IV ($n = 11\,077$)		
Cigarette tax	State tax on pack of cigarettes (cents)	32.19
Median income	Median household income in county	29 416
Religious density	Proportion own religious group in county of residence	0.216
Area density	Population density (persons/sq. km.) in county	0.468
Race density	Proportion own race in county of residence	0.686
Pr (Hispanic)	Proportion Hispanic in county of residence	0.059
Age–sex density	Proportion same-sex 14–21 years old in county	0.059
South	Equals 1 if state is in South; 0 otherwise	0.419
Midwest	Equals 1 if state is in Midwest; 0 otherwise	0.288

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Table I. *Continued*

Variable	Definition	Mean
West	Equals 1 if state is in West; 0 otherwise	0.143
Northeast	Equals 1 if state is in Northeast; 0 otherwise	0.150
School-level explanatory variables ($n = 11\,077$)		
Religious school	Equal to 1 if school has a Catholic or other religious affiliation; 0 otherwise	0.073
Small school	Equal to 1 if school size is from 1–400 students; 0 otherwise	0.197
Medium school	Equal to 1 if school size is from 401–1000 students; 0 otherwise	0.450
Large school	Equal to 1 if school size is more than 1000 students; 0 otherwise	0.354
Pr (Smokers)	Proportion of students at school who reported smoking cigarettes in past year	0.355
Drug expulsion	Equal to 1 is school has expulsion policy for illegal drug use at school; 0 otherwise	0.356
Alcohol expulsion	Equal to 1 is school has expulsion policy for drinking alcohol at school; 0 otherwise	0.211
Drug abuse program	Equal to 1 if school has a drug abuse program on premises; 0 otherwise	0.460

Note: Descriptive statistics are weighted by survey-provided sampling weights.

(percentage of the population that is of the same age and sex as the respondent), and county population density.[5] We include proportion Hispanic, indicators for region of residence (South, Midwest, and West, relative to Northeast), and the state excise tax per pack of cigarettes. From the school administrator survey, we construct indicator variables for religious schools, large, or medium school size, the presence of school policies related to alcohol and drug expulsion, and the availability of drug abuse programs in the school. From the in-school questionnaire (completed by nearly 90 000 students), we construct the proportion of smokers at each school.

4. RESULTS

We first demonstrate that the proposed instrument, religious market density, meets some of the criteria for valid instruments. In Table II, we report the results from models of participation in which religious market density is included as an explanatory variable. All models were estimated using the survey-provided sampling weights, and robust standard errors were calculated to account for the clustering of observations at the respondent's county of residence.[6] All models include the full set of explanatory variables listed in Table I; the complete results are available upon request. There is some concern that the CCM survey undercounts Black churches, which might translate into measurement error in the density measure. For this reason, we demonstrate the strength of our instrument in the full sample and a sample of non-Black respondents. Overall, results are very similar in both groups.

In Table IIA, we report models where the dependent variable is some measure of religious participation, either our preferred four-category measure or one of the two binary attendance variables representing any attendance and weekly attendance. The results are very supportive of our identification strategy. In all models, the positive coefficients suggest a direct relationship between attendance and religious density. In terms of significance, a common rule-of-thumb for a valid instrument is an *F*-statistic greater than 10; the *F*-statistics for religious density in the full sample models range from 10.5 to nearly 18. In terms of size, the estimates suggest that a one-standard deviation increase in density (equal to 0.16) would increase attendance frequency by 0.084 units, increase the probability of attendance by 2.1% points, and raise the likelihood of weekly attendance by 2.9% points, amounts that are roughly 2–6% increases from the means. Thus, our findings show that religious market density is a significant determinant of adolescent religious participation, building on the Gruber (2005) finding for adults in the GSS.

[5]Gruber (2005) includes similar controls in a model of religious participation.
[6]Clustering observations by school, instead of county, results in slightly smaller standard errors.

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Table II. Effects of religious density on religious participation and other activities

Dependent variable	Full sample		Excluding Blacks	
	Coefficient on religious density (std. error)	*F*-statistic for religious density (*p*-value)	Coefficient on religious density (std. error)	*F*-statistic for religious density (*p*-value)
A: *Participation in religious services*				
Frequency of attendance	0.518***	17.99	0.462***	10.52
	(0.122)	(0.0000)	(0.143)	(0.001)
Ever attends services	0.132***	14.50	0.146***	12.59
	(0.035)	(0.0002)	(0.041)	(0.0005)
Attends services weekly	0.183***	10.54	0.144**	4.99
	(0.056)	(0.001)	(0.064)	(0.027)
B: *Participation in secular activities, by adolescents*				
Participates in any school activities	−0.032	0.70	−0.059	1.89
	(0.038)	(0.403)	(0.043)	(0.171)
Number of school activities	0.170	0.76	0.141	0.41
	(0.87)	(0.383)	(0.219)	(0.522)
Participates in any school sports	−0.106*	2.88	−0.151**	4.27
	(0.062)	(0.092)	(0.073)	(0.041)
Number of school sports	−0.132	0.36	−0.285	1.25
	(0.222)	(0.552)	(0.255)	(0.265)
C: *Participation in secular activities, by parents*				
Parent participates in civic organizations	−0.004	0.03	−0.009	0.09
	(0.027)	(0.870)	(0.031)	(0.765)
Parent participates in hobby clubs	−0.078**	6.79	−0.093**	6.52
	(0.030)	(0.010)	(0.036)	(0.012)
Parent participates in PTO	−0.015	0.10	−0.026	0.30
	(0.047)	(0.750)	(0.049)	(0.587)
D: *Participation in religious activities*				
Frequency of prayer	0.449***	9.79	0.452***	7.43
	(0.143)	(0.002)	(0.166)	(0.007)
Frequency of attendance	0.526***	19.51	0.460***	11.14
Nonreligious schools only	(0.119)	(0.0000)	(0.138)	(0.001)

Note: Standard errors are in parentheses. Each row shows a different dependent variable used in a regression on religious market density. Sample sizes vary depending on missing values for dependent variables. All models are estimated as OLS models using the sampling weights and clustering observations at the county level. Models also include the full set of adolescent, parent, household, area, and school-level explanatory variables included in Table I. Statistical significance is indicated by ***, **, and * for the 1, 5, and 10% levels, respectively.

It is possible that the association between religious density and participation is picking up some county-level trait associated with increased secular involvement or nonreligious social capital. To see if this is the case, we define several measures of secular involvement for adolescents from the in-school interview and for parents from the parent interview. As shown in Table IIB, there is no evidence of a significant positive association between religious market density and whether or not a student participates in clubs or sports, or the number of clubs and sports in which a student participates. Three of the four coefficients are negative, and in the case of sports participation, the negative effect of religious market density is statistically significant. In Table IIC, we estimate models in which parent measures of secular participation are used as dependent variables. Again, there is no evidence that parents in areas with higher religious market density are more likely to belong to clubs, civic associations, or parent–teacher organizations.[7] Similarly, Gruber (2005) reports that predicted religious

[7]The parent interview also includes questions on membership in labor unions and veterans organizations. We do not report results using these variables as dependent variables since both are closely tied to parent work history. Regardless, religious market density is not positively associated with those measures either.

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density has negative coefficients in models of nonreligious group participation among adults in the GSS (p. 20). Thus, these findings offer further support for our identification strategy.

In Table IID, we return to the association between religious participation and religious market density, this time using the Add Health data to explore some suggested causal mechanisms for the link between religious density and beneficial outcomes. In particular, Gruber (2005) notes that the link between religious density and favorable outcomes could work through various channels: (1) religious attendance may promote well-being because social interaction has positive effects; (2) religious faith may reduce stress thus improving outcomes and behaviors; and (3) religious density may increase religious education, which in turn improves well-being. Although attendance at church services is the most widely used measure of religiosity, several studies on religion and youth behaviors show that prayer frequency and adherence to specific beliefs (e.g. God, the afterlife, etc.) are also associated with improved outcomes and behaviors (Rew and Wong, 2006). We find that religious market density is not only significantly and positively associated with attendance, but it is also significantly associated with adolescents' self-reported prayer frequency, and that its association with attendance holds in samples of students who do not attend religious schools. These findings offer support for the first and second mechanisms, but not the third.

In Table III, we examine the link between religious participation and health risk behaviors. Given that religious market density has strong influences on both attendance and prayer frequency, we examine the link between both aspects of religious activity and health behaviors. We first report single equation linear probability models for comparison, followed by regressions using religious market density as an instrument for religious activity.

In the first set of single equation models, the estimated coefficient on religious attendance is negative and significant for smoking, binge drinking, and marijuana use. This is expected, given the ample evidence from prior studies that adolescents who participate in religious services more frequently are less likely to engage in risky behaviors. The IV models show a different pattern of results. For example, both the IV estimates of the effects of religious participation on smoking and drinking are not statistically different from zero. The coefficients are large in absolute value, but estimated too imprecisely to reach definitive conclusions about the effect of attendance. As Table II suggests, the imprecision is not due to the weakness of the instrument in the first stage. On the contrary, for marijuana use, the IV estimate remains negative and statistically significant (at the 0.10 level) and is larger in magnitude than the OLS estimate. Thus, accounting for unobserved heterogeneity increases the deterrent effect of religious participation on illicit drug use. This result is similar to the findings reported by Gruber and Hungerman (2008) in which blue law repeals led to an increase in marijuana and cocaine use.[8]

The IV estimate of attendance frequency is almost 3.5 times the size of the OLS estimate, which is the opposite of what one might expect if 'good' kids go to church *and* avoid drugs, the point Iannaccone (1998) makes. Instead, this pattern suggests that the same unobservable factors that are positively associated with drug use are also positively associated with religious participation. Perhaps, youths increase their religious participation to help deal with the same pressures and problems that lead them

[8]The effects of other explanatory variables on risk behaviors and religious participation are similar to those reported in the prior literature. For example, age has a positive and significant effect on all three risk behaviors. Adolescents who live with their biological father are less likely to engage in risky behaviors. Black respondents are less likely to smoke, consistent with Powell *et al.* (2005), DeCicca *et al.* (2002), and Ross and Chaloupka (2003). Binge drinking is greater among Catholics, and less likely for female or black respondents (Bartkowski and Xu, 2007). Religious participation increases with parent's age and educational attainment and family income. The coefficients on female gender, Hispanic ethnicity, and Black race are all positive and significant in the participation models (consistent with Gruber and Hungerman, 2008; Brown and Taylor, 2007). Household size, maternal employment, residing with one's biological father, and school size are also positively associated with attendance frequency.

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Table III. Effects of religious activity on risk behaviors

	Dependent mean (SD)	OLS	IV	Underidentification test	Weak identification test
A: *Frequency of religious attendance*					
Current smoker ($n = 10\,948$)	0.257	−0.049***	−0.064	12.96	17.99
	(0.437)	(0.005)	(0.081)	(0.0003)	
Binge drinking ($n = 10\,972$)	0.254)	−0.038***	−0.036	13.75	19.84
	(0.436)	(0.005)	(0.084)	(0.0002)	
Marijuana use ($n = 10\,926$)	0.130	−0.033***	−0.114*	13.53	19.20
	(0.336)	(0.005)	(0.066)	(0.0002)	
B: *Frequency of religious prayer*					
Current smoker ($n = 10\,939$)	0.244	−0.036***	−0.072	7.38	9.79
	(0.430)	(0.004)	(0.099)	(0.007)	
Binge drinking ($n = 10\,963$)	0.249	−0.028***	−0.042	7.51	9.68
	(0.433)	(0.004)	(0.098)	(0.006)	
Marijuana use ($n = 10\,917$)	0.133	−0.025***	−0.139*	7.27	9.25
	(0.340)	(0.003)	(0.072)	(0.007)	
C: *Frequency of religious attendance*, *nonreligious schools only*					
Current smoker ($n = 10\,214$)	0.260	−0.048***	−0.024	14.87	19.51
	(0.438)	(0.005)	(0.084)	(0.0001)	
Binge drinking ($n = 10\,236$)	0.257	−0.037***	0.029	15.67	21.32
	(0.437)	(0.005)	(0.079)	(0.0001)	
Marijuana use ($n = 10\,191$)	0.129	-0.031***	-0.108*	15.45	20.58
	(0.335)	(0.005)	(0.066)	(0.0001)	

Note: Standard errors are in parentheses. Regressions are estimated as linear probability models using the sampling weights provided in the survey, and accounting for the clustering of the observations at the county level. Models also include the full set of adolescent, parent, household, area, and school-level explanatory variables included in Table I. The critical values of the Stock and Yogo (2005) weak identification tests are for 10% maximal IV size, 16.38; for 15% maximal IV size, 8.96; for 20% maximal IV size, 6.66; and for 25% maximal IV size, 5.53. Statistical significance is indicated by ***, **, and * for the 1, 5, and 10% levels, respectively.

to engage in drug use. Another explanation is the religious youths who use drugs are more honest in their survey reports, and this masks the true deterrent effect of religious participation.[9]

In the next set of models, we examine the association between prayer frequency and health behaviors. In the single-equation models, results suggest that adolescents who pray more frequently are less likely to smoke, drink, and use marijuana. The IV estimates of prayer frequency are imprecise for smoking and binge drinking but statistically significant for marijuana use. Finally, the bottom third of Table III reports OLS and IV estimates of the effect of religious attendance on health behaviors among adolescents who do not attend religious schools. These results are similar to the full-sample results. Thus, like the first-stage results, these results shed some light onto the causal pathways linking religion and health. That both attendance (by definition a social activity) and prayer (which may or may not be social) influence illicit drug use suggests that the benefits of religion may work through the first and second mechanisms noted above. The full-sample results are comparable to the results for students who do not attend religious schools, which suggests that the religious schooling mechanism is less important.[10]

[9]Aside from these reasons, two other explanations are possible. First, measurement error in the attendance variable would lead to attenuation bias in the OLS estimates. Second, there may be some unknown relationship in the data that leads to overly large IV estimates. When we use an alternate identification strategy based on a measure of religious market concentration similar to the one used in Borgonovi (2008), we find a similar pattern in which the IV estimates are larger than the OLS estimates.

[10]Also summarized in Table III are the results of tests of underidentification and weak identification that offer further support for our instrument. In all models, we can reject the null hypothesis that the model is underidentified, and we are able to reject the null hypothesis that the model is weakly identified using the critical values reported by Stock and Yogo (2005). The test statistic for underidentification is the LM version of the Kleibergen–Paap *rk* statistic and the test statistic for weak identification is a Wald *F*-statistic based on the Kleibergen–Paap *rk* statistic. Both tests are appropriate for clustered standard errors and are described in detail by Baum *et al*. (2007).

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Table IV. Effects of religious density on health risk behaviors

	Current smoker	Binge drinking	Marijuana use
Model 1			
Density	−0.033	−0.019	−0.061*
	(0.044)	(0.046)	(0.037)
F-statistic	0.56	0.18	2.69
(*p*-Value)	(0.455)	(0.676)	(0.103)
N	10 948	10 972	10 926
Model 2			
Density	0.006	0.003	−0.007
	(0.053)	(0.055)	(0.054)
Density*residence 1990	−0.067	−0.039	−0.095*
	(0.049)	(0.057)	(0.051)
F-statistic	1.24	0.31	4.71
(*p*-Value)	(0.292)	(0.732)	(0.010)
N	10 937	10 961	10 917

Note: Standard errors are in parentheses. Regressions are estimated as linear probability models using the sampling weights provided in the survey, and accounting for the clustering of the observations at the county level. Models also include the full set of adolescent, parent, household, area, and school-level explanatory variables included in Table I. Model 2 includes an indicator variable equal to 1 if the respondent lives at the same residence as he or she did in 1990 and 0 otherwise. In Model 1, the *F*-statistic is from a test of the null hypothesis that the religious density coefficient is equal to 0; in Model 2, the *F*-statistic is from a test of the joint significance of the coefficients on religious density and its interaction with residence. Statistical significance is indicated by ***, **, and * for the 1, 5, and 10% levels, respectively.

The Table III results suggest that health behaviors may be influenced by multiple aspects of religiosity. Because we lack a sufficient number of instruments to estimate a structural model that includes all possible measures of religiosity, we next examine a reduced form model in which religious density is used as an explanatory variable in the models of risky behaviors. Results are summarized in Table IV. Religious market density has the expected negative effect on marijuana use, and is significant at the 0.10 level. In terms of magnitude, the coefficient suggests that a one-standard deviation increase in religious market density (0.16 units) reduces the likelihood of marijuana use by just under 1% point from the sample mean of 13.4% (a reduction of about 7%). Direct comparisons of this estimate to other studies are not possible, but Gruber and Hungerman (2008) report very large effects of blue law repeals in their models of cocaine and marijuana use (pp. 853–855).

In the bottom panel of Table IV, we interact religious density with a dummy variable equal to 1 if the adolescent lived at the same residence during the survey year (1995) as he or she did in 1990, the year to which the CCM data pertain. Here, we find that religious market density has a statistically significant effect on marijuana use only for those adolescents who still lived at their 1990 residence. Because the accuracy of the instrument is best for this group, we take this as further support for our identification strategy.

Our results are robust to a number of specification changes. In the first stage, we obtain substantively similar results for the effect of religious market density when we include controls for parental attendance in the child participation models. We obtain similar results in both first-stage and IV models when we define religious attendance as either an indicator variable for any attendance (relative to none) or weekly attendance (relative to less frequent or none). We obtain the same pattern of results in the IV and reduced form models of drinking when the dependent variable is defined as drinking any alcohol within the past 12 months. When the dependent variable is 'heavy' smoking (defined as smoking every day in the past 30 days), we actually find that religious attendance has a *statistically significant* negative effect in IV models of smoking behavior. Our IV models and reduced form models yield similar results when estimated as IV probit or probit models.[11]

[11]We estimate the IV models using *ivreg2* in Stata, which includes more enhanced tests of identification than *ivprobit*.

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One potential limitation of our identification strategy is that variation in county-level religious density may be related to the migration decisions of households, which are endogenous. Prior studies have linked migration decisions to individual characteristics like education (Malamud and Wozniak, 2008) and characteristics of the county of birth (Munshi and Wilson, 2008). In our case, a concern arises from a particular type of migration – the decision to migrate *to an area with a higher density of one's own religion*. If this type of migration is driven by the same unobservables that are associated with a lower likelihood of engaging in risky health behaviors, then our results are biased toward finding a negative association between density and smoking, binge-drinking, or drug use. Gruber (2005) notes that the bias could also work in the opposite direction, if for example individuals with 'poor' unobservables are the same individuals who migrate to areas with higher densities of their own religion, and those who subsequently engage in health risks.

Evidence on the presence and direction of this type of bias is hard to obtain, but Gruber (2005) deals with this problem by estimating models of various outcome measures in a subset of respondents who moved from their state of birth. He finds that moving to an area that has a higher density of one's same religion is associated with lower incomes, lower educational attainment, lower rates of employment, and higher likelihoods of welfare receipt and divorce. This pattern of results is consistent with the finding that migrants to areas with a high density of coreligionists have worse unobservables, not better. Although Gruber does not examine health behaviors, an extension of this pattern would suggest that religious density effects are actually biased toward zero in models of smoking, drinking, and drug use. Unfortunately, we are unable to use our data to conduct a similar test. Between Add Health Wave I in 1995 and Wave II in 1996, there were only 160 cross-county movers. Wave III was conducted in 2002, and likely has a larger share of movers, but the contextual database for Wave III does not contain the religious density measures.

A second potential limitation of our identification strategy is the popularity phenomenon mentioned earlier. Perhaps, members of majority groups simply act differently because they are in the majority, whether the majority is defined by religious affiliation or some other trait. To examine support for this phenomenon, we re-estimated our models in Tables II and IV, but rather than using the religious market density variable, we used an alternative density measure defined from the subject's race, ethnicity (Hispanic or not), parental education, or household income.[12] Table V summarizes the estimated coefficients on the alternate density measures. In the case of racial, ethnic, and educational density, the pattern of results is quite different from what we observe with religious density. For example, we find that of these three, only educational density has a significant (negative) influence on attendance, but it has no significant association with any of the three health risk behaviors. The proportion of the county that is the same race as the respondent is close to having a significant negative association with marijuana use ($p = 0.104$), but is it not significantly associated with attendance. Thus, the pattern of results obtained in Tables II and IV cannot be explained by these types of majority effects. In the case of income density, results are more consistent with what we observe for religious market density (albeit with a smaller sample, since we lose more than 1000 observations with missing data on household income). Although we cannot entirely rule out the possibility that our religious density findings are picking up majority group effects, the bulk of results in Table V are not supportive of widespread majority influences on these behaviors.

5. DISCUSSION AND CONCLUSIONS

A large literature shows religious participation to be associated with various beneficial outcomes for adolescents, including higher educational attainment, reduced criminal activity, and better health and

[12]To construct these measures, we used Add Health contextual data on the proportion of the county by race, Hispanic ethnicity, education (less than high school, college or more, high school) and annual household income (less than $15 000, $15 000–$25 000, $25 000–$50 000, $50 000–$75 000 and more than $75 000).

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Table V. Effects of alternate densities on religious participation and health risk behaviors

	Dependent variable			
Density measure	Frequency of attendance	Current smoker	Binge drinking	Marijuana use
Proportion county population of same race	−0.058 (0.168) *F*-stat = 0.12 *p* = 0.73	−0.112* (0.058) *F*-stat = 3.79 *p* = 0.053	−0.021 (0.054) *F*-stat = 0.15 *p* = 0.702	−0.070* (0.043) *F*-stat = 2.67 *p* = 0.104
Proportion county population of same ethnicity	−0.213 (0.271) *F*-stat = 0.62 *p* = 0.43	−0.039 (0.058) *F*-stat = 0.46 *p* = 0.500	0.138*** (0.053) *F*-stat = 6.83 *p* = 0.010	0.024 (0.047) *F*-stat = 0.26 *p* = 0.609
Proportion of county population with parent's same educational attainment	−0.463* (0.249) *F*-stat = 3.46 *p* = 0.065	0.022 (0.067) *F*-stat = 0.11 *p* = 0.739	−0.024 (0.066) *F*-stat = 0.13 *p* = 0.721	0.004 (0.052) *F*-stat = 0.00 *p* = 0.946
Proportion of county population in household's same income bracket	0.264** (0.134) *F*-stat = 3.86 *p* = 0.051	−0.029 (0.080) *F*-stat = 0.13 *p* = 0.715	−0.049 (0.060) *F*-stat = 0.69 *p* = 0.409	−0.112** (0.054) *F*-stat = 4.69 *p* = 0.032

Note: Standard errors are in parentheses. Each cell shows the coefficient of a density measure used as an explanatory variable in a regression model of either attendance frequency or some risk behavior. Sample sizes vary depending on missing values for dependent variables. All models are estimated as OLS models using the sampling weights and clustering observations at the county level. Models also include the full set of adolescent, parent, household, and school-level explanatory variables included in Table I, and control for area population density, age–sex density, proportion Hispanic, county median income, and state cigarette tax. Each *F*-statistic is from a test of the null hypothesis that the coefficient of the density measure is equal to 0. Statistical significance of the coefficients is indicated by ***, **, and * for the 1, 5, and 10% levels, respectively.

health behaviors. On the contrary, a much smaller group of studies has attempted to address the potential endogeneity of religious participation. That group consists of a few studies using IV estimation in adult or young adult populations, and one study focusing on youths. Although the studies on adults are generally supportive of a causal effect of religious participation on outcomes such as happiness, illicit drug use, and income, the only known prior work on youth reports mixed support for the effects of attendance on smoking, drinking, and drug use.

This study adds to this small literature by applying IV estimation to data from the National Longitudinal Survey of Adolescent Health. We replicate past findings of single-equation analysis to show that adolescents who attend religious services more frequently are less likely to smoke, binge drink, or use marijuana. To deal with the potential endogeneity of attendance, we employ a county-level measure of religious density as an IV. We find that religious density is significantly associated with religious attendance. Adolescents who live in areas where greater proportions of the population adhere to their own faith have a higher frequency of attendance. When we use this variable as an instrument in IV models of smoking, drinking, and drug use, we find evidence that religious participation has a significant negative effect on marijuana use, even after accounting for its potential endogeneity.

Several limitations of our IV analysis should be noted. First, our IV estimate of religious attendance is a local average treatment effect, or the average effect of attendance for adolescents whose behavior was changed by the instrument. It may be the case that there are heterogeneous treatment effects across different types of youths in our sample, for example, between marginally adherent youths who are responsive to the behavior of others in the community and other more devout youths who are less responsive to others. Our strategy does not identify these differences, and as such our estimated effect may differ from the effects of policy experiments applied to entire groups of adolescents. A second limitation pertains to another form of heterogeneity–heterogeneous partial effects. Dunning (2008) makes the point that IV models such as the one estimated here make the assumption 'that variation in

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the endogenous regressor related to IV must have the same causal effect as variation unrelated to the instrument' (p. 2). In other words, the attendance variations driven by density and other factors are assumed to have the same effects on health behaviors. If, however, heterogeneous effects exist, then Dunning shows that the IV estimate asymptotically estimates the effect of the exogenous portion of the treatment, not the combined effect, and he suggests a useful specification test for this.

A third limitation is that we cannot conduct either overidentification tests or Dunning's specification test, given that we have only one instrument. In other analysis not reported here, we used the CCM shares in the contextual database to construct a religious Herfindahl index, similar to Borgonovi (2008). When entered as a quadratic into our first stage and IV models, we found both coefficients to be statistically different from zero, but tests of redundancy in the IV models showed the terms added little to the model identification when religious density was also included. Finally, we use only one cross-section, and are unable to include controls for youth fixed effects. The Add Health survey has multiple waves, but there are too few cross-county moves between Wave I and Wave II to generate within-panel variation in the instrument.[13]

Despite these limitations, our results are quite similar to those estimated by Gruber and Hungerman (2008), who use an older sample of young adults and employ an alternative estimation strategy. Like that study, we find evidence that religious participation has a large negative effect on illicit drug use. Given the significant consequences of adolescent drug use, our results provide important motivation for researchers attempting to identify the causal determinants of adolescent risky behaviors.

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[13]Another possibility is to control for a state fixed effect, using within-state variation in county-level religious density to identify the model. This approach, too, has its drawbacks in the Add Health survey. Given the school-based sampling design of the survey, a large share of the respondents from any given state resides in a single county. In our estimation sample, an average of 70% of the respondents in a state is drawn from one county, and for nearly one-third of the states represented in the survey, one county accounted for 95% or more of the state's observations. Thus, there is relatively little within-state variation in the survey. There is even less variation in county-level variables within school. On average, more than 95% of a school's respondents reside in one county.

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