Religion and Depression in Adolescence*

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Abstract

Depression is the leading cause of illness and disability in adolescence. Many studies show a correlation between religiosity and mental health, yet the question remains whether the relationship is causal. We exploit within-school variation in adolescents' peers to deal with selection into religiosity. We find robust effects of religiosity on depression that are stronger for the most depressed. These effects are not driven by the school social context; depression spreads among close friends rather than through broader peer groups that affect religiosity. Exploration of mechanisms suggests that religiosity buffers against stressors in ways that school activities and friendships do not.

I. Introduction

Depression is the leading cause of illness and disability in adolescence worldwide. The World Health Organization lists mental health in adolescence as a key issue that needs to be addressed (WHO, 2014). In the US, the incidence of a major depressive episode in adolescence has risen by more than a third over the past decade to 12.5 percent of adolescents as of 2015 (CBHSQ, 2016). This is troubling for a number of reasons. First, depression during adolescence is correlated with a range of adverse outcomes, including lower academic achievement and non-cognitive development (Cook, Peterson, and Sheldon, 2009). Second, studies estimate that half of adults who suffer from mental health issues had symptoms that began in adolescence (WHO, 2014).[1] Third, the economic costs are substantial. Between 1996 and 2006, mental health expenditure rose rapidly from $35.2 to $57.5 billion and from the 5th to the 3rd most costly medical condition in the US (AHRQ, 2014).[2] In this paper, we examine the role of one important determinant of depression in adolescence—religiosity.

A contentious literature dating back to Freud in the early 1900s debates the role of religion in mental health and has been influential in the treatment of mental health problems (Levin, 2010).[3] Understanding the role of religion remains relevant today. More than 8 in 10 people identify with a religious group worldwide (PewForum, 2012). Sixty-five percent of Americans say religion plays an important part in their daily lives, and a majority of Americans claim religion could address most or all of today's problems (Crabtree, 2010; Newport, 2014). Among adolescents, 31 percent of twelfth graders attend church on a weekly basis, and 28 percent report that religion plays a *very* important part in their lives (CTD, 2014a,b).

[1] Williams, Holmbeck, and Greenley (2002) highlight adolescence as a key period of development that should be addressed due to its important consequences for mental health in adulthood.

[2] Langa et al. (2004) estimate a yearly cost of about $9 billion for caregiving associated with depressive symptoms in elderly Americans, many of whom experienced depression in adolescence.

[3] Discussion of these issues features in Freud (1927) and his other writings which examine religion and its effect on the human psyche.

Considerable scientific evidence suggests that religiosity is positively correlated with mental health, yet the meaning of this correlation remains a puzzle (Ellison and Henderson, 2011; Levin, 2010). We contribute to the debates about religion and mental health by first, exploring whether the link between religiosity and depression can be interpreted as causal. Second, we combine insight from economics and social psychology to explore how religiosity affects depression, focusing particularly on the role of social context and stressors. The National Longitudinal Study of Adolescent to Adult Health in the United States, a nationally representative sample of adolescents in grades 7 to 12 in 1995 provides an excellent context for studying these questions, as it includes measures of depression, religiosity, and detailed information about the home, the school environment and associated stressors.

The key challenge with establishing a causal effect of religiosity is the issue of selection into religiosity. In our context, it could be that religiosity simply proxies difficult-to-measure aspects of family background and that it is family background rather than religiosity that leads to lower depression. Further, it could be that people select into religiosity as a way of dealing with negative shocks to mental health (Ferraro and Kelley-Moore, 2000). To deal with selection into religiosity based on individual unobservables, we focus on an alternative determinant of religiosity—school peers. We exploit arguably exogenous within-school, cross-cohort variation in peers to shift religiosity independently of the individual-level unobservable determinants of depression. Robustness checks help alleviate concerns about key confounders commonly understood in the peer effects literature—selection into peer groups and shared correlated unobservables among the adolescent and her peers (Manski, 1993).

We then explore the determinants of the effect of religiosity on depression. The first channel we explore is the school social context, where we disentangle whether our estimated effect of religiosity is driven by an individual's religiosity or their school peers. Here, we benefit from observing friendship patterns in the data, which permit us to test a key theory that depression is spread among close friends rather than the broader peer group which we use to instrument for religiosity. We examine whether school clubs/sports participation and/or friendships sub-

stitute for religiosity. We also examine other key theories in the literature, including whether religiosity reduces exposure to or helps to buffer against stressful situations, and whether it improves self-esteem or coping skills.[4] This provides important insight for policy and helps to support our claim of a causal effect of religiosity by illustrating plausible channels.

Our paper contributes methodologically to the literature in economics that addresses the difficult problem of disentangling a causal effect of religiosity (Hungerman, 2011; Iannaccone, 1998; Iyer, 2016). The method we use is similar in spirit to Gruber (2005) and Mellor and Freeborn (2011), which use variation in religiosity at the county level to shift individual religiosity, relying on insight from the competition literature on how density of churches affects attendance. We build instead on the power of within-school peers to shift religiosity.[5] What has received less attention in the economics of religion literature is whether the effect of religiosity derives through having a more religious social context or a direct effect of an individual's religiosity, which is implicitly confounded by most instrumenting strategies in the literature.[6]

A broad literature in psychology and sociology studies the link between religiosity, depression and other indicators of mental health, but without establishing causality (Ellison and Henderson, 2011; Hackney and Sanders, 2003; Levin, 2010). Recent overviews of the literature on religion and mental health support a need to better understand why religion improves mental health (Ellison et al., 2001; Nooney, 2005). Chiswick and Mirtcheva (2013) is the only paper we are aware of that studies the effect of religiosity on mental health in youth and treats seriously the concerns about selection using matching methods, though they are not able to address selection on unobservables.[7] Our study is also related

[4] These theories are described in Ellison et al. (2001) and Ellison and Henderson (2011).

[5] That peers affect religiosity is explored in Cheadle and Schwadel (2012) and Desmond, Morgan, and Kikuchi (2010).

[6] Even the most convincing identification strategies, such as Gruber and Hungerman (2008), do not take the additional step of separating these two channels.

[7] Becker and Woessmann (2011) use a unique instrument for dealing with selection on unobservables, but in a very different context of 19th century Prussia and focusing on the question of Protestantism and suicide.

to the growing literature in economics that recognizes the importance of non-cognitive aspects of child development for determining outcomes (Cunha and Heckman, 2008; Cunha, Heckman, and Schennach, 2010; Heckman, Stixrud, and Urzua, 2006).

We find that religiosity has sizeable effects on depression in adolescence, which is understated by OLS estimates that do not deal with selection into religiosity. For example, a one standard deviation increase in religiosity decreases the probability of being depressed by 11 percent. By comparison, increasing mother's education from no high school degree to a high school degree or more only decreases the probability of being depressed by about 5 percent. We find evidence suggesting that the peers (at the school-cohort level) that are associated with religiosity are different than the peers (self-reported friends) that are associated with depression, suggesting our results are driven by individual religiosity rather than the social context at the school-cohort level. We further provide evidence on the types of stressors that religiosity helps to buffer against, providing useful insight for policy.

II. Data

We use data drawn from the restricted version of the National Longitudinal Study of Adolescent to Adult Health (Add Health). Add Health interviewed a representative sample of U.S. adolescents in grades 7–12 (primarily aged 13–18) during the 1994/95 academic year. A short in-school survey was conducted for every student in the sampled schools. Following the in-school survey, a random sample of students also participated in an in-home survey, which provides more detailed information about the adolescent, including our primary variables of interest, religiosity and depression.[8] This is supplemented with information about the child and his/her parent provided in the parent survey, and is based

[8] On average, there are 330 students per school who respond to the in-home survey. While this is a fairly large sample, we will also consider whether measurement error caused by not sampling the whole school biases our estimates among the specification checks in Online Appendix A.3. While Wave II also takes place in high school, we focus on Wave I because measurement error in the peer group becomes a larger issue in Wave II. That said, our results are similar and even slightly larger, if we include Wave II.

primarily on self-reports.

Depression is measured on the Center for Epidemiological Studies Depression (CES-D) scale, one of the most common screening tests for depression and depressive disorder developed by Radloff (1977). It has been validated in a number of clinical trials. The CES-D scale consists of a list of symptoms, to each of which respondents report how often they experience the feeling.[9] Responses are rated on a frequency scale ranging from 0 = never or rarely, to 3 = most or all the time. Response values are aggregated to create a point score, with higher scores indicating greater depressive symptoms. A score of 16 or above is considered to be indicative of moderate to severe depression (Radloff, 1977). Appendix Figure A1 shows the distribution of the depression scale. The distribution is skewed left with a long right tail; 24% show symptoms of depression (CES-D score $\geq$ 16). While we primarily focus on the effect of religiosity on the CES-D scale, we also consider effects on the indicator of whether an adolescent is depressed by this definition, in order to get a better sense of magnitudes. We examine how sensitive our estimates are to the choice of threshold and to alternative scales in Appendix A.3.

The data provide information on four aspects of religiosity: frequency of church attendance, importance of religion, frequency of praying, and frequency of attending youth religious activities. Each aspect is assessed on a scale of 0–3 or 0–4. We use the aggregate of these four aspects as our main measure of religiosity.[10] A limitation of the data is that only adolescents who report a religious affiliation were asked the more detailed religion-related questions.[11] Therefore, we are only able to study the effect of religiosity on mental health for those who report having a religion, which is 85.9 percent of the sample.[12] In principle, we expect this

[9] Appendix Table A1 lists the questions. The original CES-D scale lists 20 items, only 19 of which appear in Wave I of Add Health. Add Health substitutes the CES-D item "You felt life was not worth living" for two questions on sleeping and crying spells.

[10] The details are in Appendix Table A1. Principle component analysis based on polychoric correlations, which honor the ordinal nature of the measures, suggest that a single factor explains 77% of the variation. We find similar results if we use an extracted factor as our variable of interest rather than our index of religiosity; see Online Appendix A.3.

[11] Participants were asked "What is your religion?" and given a broad list of potential affiliations to choose from, as shown in Appendix Table A2.

[12] For the purposes of the social context calculations, individuals who report not

to understate the effect of religiosity, given that some people may be "religious" by the other measures but not report a religion. Sample means show that the non-affiliated are statistically significantly more depressed with a 12.3 average CES-D compared to 11.1 for the affiliated sample. We show robustness to including the non-religious in Section A.3.

Our identification strategy relies on defining a set of "similar" peers to which individuals are most likely to respond in choosing religiosity, based on students in the same school, grade, race, gender and denomination, as discussed further in Section III. This requires categorizing race and denominations. We categorize race as white, black, Hispanic or other. We group Christian faiths into Catholic, Liberal Protestant, Moderate Protestant, and Conservative Protestant.[13] We drop non-Christian affiliating (4.7 percent of the sample), as they are arguably not largely substitutable across belief systems and no single affiliation has enough of a presence to be considered separately.[14] Because peer religiosity is needed for identification, we also exclude those without a peer respondent from the main results, 14.9 percent of the sample. We show robustness to including the non-Christian and those with missing peer groups in Online Appendix A.3. The average peer group in our estimating sample has 11 students.

We control for a range of covariates in our specifications, taken primarily from the in-home and parent surveys: individual characteristics such as age, sex, race, grade, denomination, physical development, whether the respondent was interviewed during the school year session; parental background including whether mother or father was present, mother's education and household income; and school fixed effects. Removing those with missing data on religiosity, depression and covariates

having a religious affiliation are coded as having 0 religiosity rather than missing religiosity, which we think provides a better approximation of the average religiosity of peers.

[13] The details of the categorization are summarized in Appendix Table A2. The categorization is based on the Churches and Church Membership 1990 (CCM1990) data which collect county-level membership information on 133 Judeo-Christian church bodies in the US. Add Health categorizes these church bodies as Jewish, Catholic, Black Baptist, other liberal, other moderate and other conservative denominations in the Contextual Database.

[14] 2.7% report being affiliated with unspecified "other religion". The largest specified non-Christian religion, Jewish, is only 0.7 percent of the sample.

reduces the sample by about 3.8 percent.

Table A3 describes how the final estimating sample compares to the original sample. The final sample has marginally lower average CES-D (11.1 compared to 11.4), marginally higher religiosity (8.6 compared to 8.5), and is marginally more affluent by a number of metrics in the table.

III. Empirical Strategy

Let i index the individual student and s the school. Adolescent i's mental health (H_{is}) is determined by religiosity (R_{is}), observable background characteristics ($\boldsymbol{X}_{is}$), and unobservable factors (ε_{is}), i.e.,

$$H_{is} = \alpha_1 R_{is} + \boldsymbol{X}'_{is}\boldsymbol{\alpha}_2 + \alpha_s + \varepsilon_{is}, \tag{1}$$

where α_s captures fixed school factors that might affect mental health. The key concern with identifying an effect of religiosity is unobservable individual characteristics that affect mental health and make an individual more likely to be religious, such that $\mathbb{E}(\varepsilon_{is}|R_{is}, \boldsymbol{X}_{is}) \neq \mathbb{E}(\varepsilon_{is}|\boldsymbol{X}_{is})$. For instance, religiosity may signal something about the home environment that affects mental health. Similarly, a shock, like the death of a friend or family member, could lead an individual to become more religious and also suffer from mental health issues. Reverse causality could also be a concern if individuals go to church as a way of dealing with poor mental health. It is thus ambiguous whether OLS estimates of equation (1) would over- or under-state the effect of religiosity and depends on the type of selection that dominates.

To identify an effect of religiosity, we seek to isolate within-school variation in peers that shifts an individual's religiosity independently of ε_{is}. Let the subscript $g(i)s$ denote the relevant peer group of student i in school s, in a way that we will make specific below, and $\overline{R}_{g(i)s}$ denote the average of i's peers' religiosity, excluding i. Then the first stage equation is simply

$$R_{is} = \beta_1 \overline{R}_{g(i)s} + \boldsymbol{X}'_{is}\boldsymbol{\beta}_2 + \beta_s + u_{is}, \tag{2}$$

where β_s denotes the school fixed effects and u_{is} the residual. For α_1 to be identified, we need the following conditions to be satisfied:

Assumption A1 $\mathbb{E}(\varepsilon_{is}|\overline{R}_{g(i)s}, \boldsymbol{X}_{is}) = \mathbb{E}(\varepsilon_{is}|\boldsymbol{X}_{is})$,

Assumption A2 $\mathbb{E}(R_{is}|\overline{R}_{g(i)s}, \boldsymbol{X}_{is}, S_i)$ *is a non-degenerate function of* $\overline{R}_{g(i)s}$ *(*$\beta_1 \neq 0$*), where* S_i *is an indicator for the individual's school.*

An important question is how to define the peer group such that it meets the independence and relevance conditions. To begin with relevance (A2), we first consider the friendship sorting patterns, with the intuition that adolescents who have a higher probability of being friends are more likely to influence each other. Table 1 contrasts the proportion of a student's schoolmates (column 1) to the proportion of a student's friends (column 2) who share a given characteristic. Consistent with evidence of homophily in McPherson, Smith-Lovin, and Cook (2001) and elsewhere, students are more likely to form friendships with other students of the same school, grade, race, and gender. An average adolescent shares the same school, grade, race, and gender with 8% of the students in the school, but share these characteristics with 40% of her friends. Homophily by religious affiliation is less pronounced, but still present, with 3% of students in the school being of the same school-grade-race-gender-denomination group compared to 18% of friends.

A second way we determine relevance is by estimating the first stage equation (2) using different measures of peers' average religiosity.[15] Table 2 column (1) shows that average friends' religiosity is positively correlated with own religiosity, and column (2) shows that this correlation is stronger for friends of the same denomination.[16]

Because these correlations are likely to be driven at least in part by sorting into friendships, we do not expect average friends' religiosity to be independent of the individual's unobservable type, violating the key independence assumption (A1). Instead, we attempt to isolate plausibly random in peer religiosity by using variation in religiosity across cohorts

[15]While we control for selection into schools through school fixed effects, this regression has all the well-known identification problems defined in Manski (1993), but here we are attempting to establish correlation for our first stage regression rather than causation.

[16] We control for missing friendships and replace missing values of friends' religiosity with zero. 65 percent of the sample does not have friend's religiosity because this data is only available for the subsample of students who are in the in-home survey, which is just a subset of any given adolescent's friends.

within schools. Using the insights on sorting patterns and strong correlations with same-denomination friends to determine relevant cohorts, we define $\overline{R}_{g(i)s}$ as the average religiosity of peers in the same school-grade-race-gender-denomination group. Column (3) of Table 2 shows peers of the same school, grade, race, gender, and denomination have statistically significant effects on religiosity (satisfying A2), and stronger effects than same-school-grade-race-gender peers of *other* denominations, mirroring patterns we find in friendship correlations.[17]

In Section V we discuss further evidence that independence is satisfied, considering two key challenges: (1) potential selection into having higher-religiosity peers of the same school-grade-race-gender-denomination, and (2) the possibility that peer religiosity proxies for some shared unobservables that affect all students' religiosity and mental health. We further discuss mechanisms of this effect in Section VI, particularly considering whether the effects we find are driven by a student's own religiosity or by having peers who are more religious.

IV. Results

A. Baseline Results

In Table 3 we present the results for the OLS and IV estimation of the relationship between depression and religiosity. In all specifications, we control for individual characteristics, family background, and school fixed effects. We start with the OLS specification in column (1) which does not instrument for religiosity. These results suggest that religiosity decreases depression by -0.16. Controlling for school fixed effects helps eliminate concerns about fixed factors at the school or community level that might affect both religiosity and mental health, but results that do not control for school fixed effects (not reported) are surprisingly similar (estimated coefficient is -0.15), suggesting that the correlations are not mediated by school-level unobservables.

Column (2) presents results when we instrument for religiosity using

[17] Appendix Table A4 shows that there is considerable variation in peer religiosity both within and across schools, grades, races, genders, and denominations.

the average religiosity of same school-grade-race-gender-denomination peers, and column (3) shows the first stage results. First, note that peer religiosity is significant and positively predicts own religiosity, with an F -statistic of 30.44, suggesting that we do not have a weak instrument problem. The estimated effect of religiosity on depression using our IV estimator is -0.70, over four times as large as the OLS estimate of -0.16, and it is statistically significant at the 5% level. In standardized terms, this indicates that a one standard deviation increase in religiosity leads to a 0.31 standard deviation reduction in the depression score. That the IV estimates predict more negative effects of religiosity than OLS suggests there may be negative selection into religiosity, *i.e.*, more depressed adolescents participate in more religious activities, biasing OLS toward zero. One explanation for this selection is that adolescents may choose religion as a way of coping with depression or other difficult home circumstances that are correlated with depression. This is consistent with evidence in Ferraro and Kelley-Moore (2000), which show that some health problems lead to increased religiosity. An alternative interpretation is that religiosity is measured with error, and thus the OLS results understate the effect relative to IV. An additional interpretation is that IV and OLS results may not be directly comparable if there are heterogeneous effects, as OLS estimates the average treatment effect and IV a weighted local average effect for those adolescents whose religiosity is affected by their peers. We return to consider heterogeneity in treatment effects in Section IV.B.

To get an idea of the magnitude of these effects, we consider an indicator of whether the adolescent is depressed as an alternative dependent variable. [18] Columns (5) and (6) present IV results from the linear probability model and IV probit model respectively. [19] The estimates are similar across the two models, suggesting that being one unit more religious decreases the probability of being depressed by 3% on average. A one standard deviation (or 3.3 units) increase in religiosity decreases

[18] Recall that CES-D greater than or equal to 16 signals risk of moderate to severe depression (Radloff, 1977).

[19] In the probit model, we control for school fixed effects using school dummies, though there is a concern about consistency for smaller schools.

the probability of being depressed by 11%.[20] In terms of relative risks of being depressed, one unit (standard deviation) increase in religiosity leads to a relative risk ratio (RRR) of 0.87 (0.62).[21] Figure 1 presents the RRRs at each level of religiosity from 0 to 13.

B. Heterogeneity in Effects

The effects of religiosity may vary depending upon the individual's unobservable propensity for being depressed. This is particularly relevant given that psychotherapy, and particularly cognitive-based therapy (a primary method of treatment for depression in the United States) is generally accepted to be effective for mild to moderate depression and less so for the more severely depressed individuals (Gloaguen et al., 1998).[22] To explore how the effect of religiosity differs based on severity of depression, we use a two-step control function approach, as described in detail in Appendix A.1. Figure 2 shows that the effect of religiosity is higher for people who are conditionally more depressed—comparing the 0.05 quantile to the 0.95 quantile, we see that the estimated effect of religiosity increases from about -0.27 to -1.13. That psychotherapy alone is less effective for more depressed individuals then offers an interesting contrast to the role of religiosity in these contexts.

We also explore nonlinear effects of religiosity on mental health based on how religious the individual is. We test this using a control function approach and try a number of different specifications of polynomials in religiosity. We find little evidence of heterogeneity by degree of religiosity.[23] Though we cannot completely rule it out, these specifications sug-

[20] Appendix Table A7 shows that estimated effects of religiosity are similar at higher cutoffs for being depressed.

[21] RRRs are calculated as the probability of being depressed at a certain level of religiosity, to that at the mean religiosity. Probabilities of being depressed are predicted from the IV probit model.

[22] There seems to be a broad consensus that more severely depressed individuals may need a combination of psychotherapy and antidepressant medication (March et al., 2007), as suggested by the guidelines posted by the National Institute for Mental Health.

[23] One potential concern is whether this could be a result of the instrument we are using, in that peer religiosity does not shift over the full distribution of religiosity. To test this, we also estimate a quantile regression version of the first stage and find that peer religiosity has significant effects on all but the most religious (0.9 quantile of the con-

gest that heterogeneity in the effects of religiosity may not be a primary reason that IV estimates are higher than OLS.

V. Robustness

The key threats to identification are issues common in the peer effects literature—selection into peer groups and the presence of unobserved group level effects. To clarify these threats in our context, it helps to divide the residual from the mental health equation (1) into a group-specific component ($\eta_{g(i)s}$) and an individual-specific component (ν_{is}), i.e., $\varepsilon_{is} = \eta_{g(i)s} + \nu_{is}$. The group-specific component could be a direct effect of the peer group characteristics on mental health or other unobservable correlated factors. We discuss identification challenges associated with each of these components in turn.

A. Selection into Peer Groups

A primary channel that $\mathbb{E}(\nu_{is}|\overline{R}_{g(i)s}, \boldsymbol{X}_{is}) = \mathbb{E}(\nu_{is}|\boldsymbol{X}_{is})$ might be violated is through selection into peer groups based on unobservables that determine both mental health and religiosity. While school fixed effects control for selection into schools based on fixed characteristics at the school-level, there may be other channels through which selection occurs. One example is if students change their religious affiliation in response to their peers. While we believe this is not a concern in our context because of existing evidence that adolescents rarely deviate from the denomination of their parents (Smith and Denton, 2005), in column (1) of Table 4, we test robustness to replacing the adolescent's denomination with the parent's denomination as both a control variable and to define the relevant peer group for the instrument.[24] Given that parents are arguably less likely than adolescents to choose denomination based on the adolescent's peers, this provides a useful test for ruling out potential endogenous denomination choices. Results are robust, though a bit noisier.

ditional religiosity distribution), which is likely due to a ceiling effect. The estimated effects of peer religiosity are also fairly homogeneous across the conditional quantiles.

[24] 24% of our sample has a different denomination from their parents, though this could in part be a result of only observing one parent's denomination.

We next perform a series of robustness tests that relax our assumption of selection based only on fixed school factors. Column (2) shows that our estimates are robust to controlling for selection based on school-specific trends. Column (3) shows robustness to controlling for selection into a neighborhood (and hence school) based on an influential local church by controlling for average religiosity of peers in the same school-denomination. While average school-denomination peer religiosity is a significant predictor of religiosity, our instrument remains significant. Most importantly, estimated effects of religiosity are robust. Interestingly, average school-denomination religiosity does not have a statistically significant effect on depression, though point estimates are large.[25] We then expand this in column (4) to control for average religiosity of same-race-denomination peers. We believe this to be an important additional check given the racial segregation of churches in the US, even within denominations. We again see that while school-race-denomination average religiosity is a statistically significant predictor of religiosity, our instrument still has significant effects (though F-statistics are smaller at 7.5). Most importantly, estimated effects of religiosity are robust. Results are very similar in column (5) when we relax the assumption still further to allow for selection based on trends in average school-race-denomination religiosity. LIML estimates, which are more robust to the potential concern about weak instruments in this setting, provide almost identical results.[26]

Finally, column (6) considers a placebo test that helps to rule out selection based on time-varying shocks. Absent selection, we would expect that peers in the same time period but sufficiently far apart in school grades would not have an effect on each others' religiosity. We test whether this is the case considering peers that are two grades apart. The two-grade-apart peers have no effect on religiosity and we pass the test of overidentifying restrictions, suggesting that they have no separate effect on depression either.[27]

[25] Results (not reported here) remain very similar when we control for grade trends in school-denomination average religiosity.

[26] We also try removing private schools from our analysis, out of the concern that selection on religiosity is more prominent in these schools. Our results are very similar.

[27] Comparable to other studies that claim random variation in peer composition

B. Unobserved Group Effects

The second central identification concern is whether there is some unobserved peer group-specific factor that violates $\mathbb{E}(\eta_{g(i)s}|\overline{R}_{g(i)s}, \boldsymbol{X}_{is}) = \mathbb{E}(\eta_{g(i)s}|\boldsymbol{X}_{is})$. An example would be some shock that hits the peer group causing all of them to have lower religiosity and worse mental health. To be a threat to identification it would need to vary at the peer group level (so that it is not controlled by the school fixed effects) and be correlated with (but not determined by) peer religiosity.[28] This can be clarifed by rewriting equation (2) to solve for $\overline{R}_{g(i)s}$, which gives us $\overline{R}_{g(i)s} = \frac{1}{1-\beta_1}(\overline{\boldsymbol{X}}_{g(i)s}\boldsymbol{\beta}_2 + \beta_s + \overline{u}_{g(i)s})$. Assumption A1 then can be reinterpreted as

Assumption A1′

$$\begin{aligned}\mathbb{E}(\varepsilon_{is}|\overline{\boldsymbol{X}}_{g(i)s}, \overline{u}_{g(i)s}, \boldsymbol{X}_{is}) &= \mathbb{E}(\eta_{g(i)s} + \nu_{is}|\overline{\boldsymbol{X}}_{g(i)s}, \overline{u}_{g(i)s}, \boldsymbol{X}_{is}) \\ &= \mathbb{E}(\eta_{g(i)s} + \nu_{is}|\boldsymbol{X}_{is}).\end{aligned}$$

This reinterpretation highlights that independence could be violated either because observable ($\overline{\boldsymbol{X}}_{g(i)s}$) or unobservable ($\overline{u}_{g(i)s}$) determinants of peer religiosity are not conditionally mean independent of the mental health residual, particularly $\eta_{g(i)s}$. We can test this in part by using peer characteristics $\overline{\boldsymbol{X}}_{g(i)s}$ that predict peer religiosity and are predetermined (i.e., age, mother has a college degree, mother not present and father not present) as an alternative set of instruments, thus relaxing the assumption on $\overline{u}_{g(i)s}$. Column (1) of Table 5 shows that estimates of the effect of religiosity are not statistically significantly different from the baseline results, though the instruments are weaker.[29] Furthermore, these instruments pass the test of overidentifying restrictions, which would not hold if they were correlated with unobserved factors that affected depression.

We also directly test the role of observable peer characteristics by see-

within school, we confirm that peer religiosity does not significantly predict observable individual characteristics using balancing tests. See Appendix A.2.

[28] Note that if it is determined by peer religiosity it is part of the social context of having peers who are more religious.

[29] Because the model is overidentified in this case, we use efficient two-step GMM for estimation.

ing whether they matter for mental health after instrumenting for religiosity. Column (2) presents the results when we control for peer characteristics. None of the peer characteristics are individually or jointly significant and controlling for them does not change our estimates of the effect of religiosity. These results also control for peer depression which could be an important additional omitted variable. We expect the coefficient on peer depression to be biased upward due to simultaneity, but we find that it is close to 0. Furthermore, estimates of the effect of religiosity remain robust at -0.62. We find similar results if we control for either peer characteristics or peer depression in isolation.[30] These results also help to rule out an important additional concern that the findings are driven by reference group effects. We expect that if reference group effects at the peer group level were key determinants, then controlling for average peer depression would significantly affect our estimates of the effect of religiosity, which is not the case.[31]

Because of the various ways in which we could define the relevant peer group, we also consider some overidentified cases (such as same and cross-gender peers of the same school-grade-race-denomination group) to see whether we pass the test of overidentifying restrictions as an additional test on certain types of unobserved group effects. For instance, if there were important unobserved group effects at the level of same school-grade-race-denomination, we would expect to fail the test of overidentifying restrictions using same and cross-gender peers. The same logic can be applied to same and cross-denomination and same and cross-race peers.[32] Column (3) shows that own religiosity is affected by

[30] In unreported estimates, we also check that our results are not driven by school contextual variables that vary across grades and are used to define our subgroups, including the percentage female, the percentage belonging to different racial subgroups and the percentage belonging to different denominations. None of these are individually or jointly significant in determining mental health. Most importantly, our estimated effect of religiosity on mental health is robust.

[31] In results not reported, we find further that our estimates are robust to controlling for potential reference effects at all levels of potential references groups, including the school-grade, school-race, school-gender, school-denomination, school-grade-race, school-grade-gender, school-grade-denomination, school-race-gender, school-race-denomination and school-grade-race-gender average depression.

[32] This also helps with concerns about whether mismeasurement of peer groups could be biasing our estimated effects of religiosity, through correlation with peer religiosity and the residual from the depression equation.

both same-gender and cross-gender peers (of the same school, grade, race, and denomination), but relatively more by same-gender peers. Estimated effects of religiosity are robust and we pass the test of overidentifying restrictions. In column (4) we consider the influence of same-race versus cross-race peers (of the same school, grade, gender, denomination), and in column (5), we consider the effects of same- and cross-denomination peers (of the same school, grade, gender, race). We find that neither cross-race or cross-denomination peers affect religiosity. Most importantly, estimated effects of religiosity are similar across the different potential instrument sets, and we pass the test of overidentifying restrictions in all cases.

Finally, we provide further supportive evidence that the correlations of religiosity and depression are not driven by unobserved peer group-level factors by showing that non-instrumented estimated effects of religiosity on depression are similar whether we control for peer group fixed effects or not. Point estimates with peer group fixed effects are -0.14 (not reported) compared to -0.15 without (not reported), and a Hausman test supports that they are not statistically significantly different.[33]

C. Other Concerns

Combined, these results provide support that our estimated effects of religiosity are not driven by selection or unobserved group effects. Further robustness tests described in Appendix A.3 show our results are robust to a number of other important concerns. We show robustness to scale concerns, removing possible social components of our depression measure and testing sensitivity to using polychoric correlations rather than simple aggregates. We also show robustness to a number of sample selection concerns, including dropping the non-religious and non-Christian from the sample, along with those with missing peer groups. We also verify that our results are not driven by some unusual sampling

[33] Recall from the discussion of Table 1 that this was also true for the comparison of OLS to school fixed effects results. Note that we cannot control for peer group fixed effects and use peer group religiosity as an instrument as there is not enough variation in the data.

features, such as measurement error in peer religiosity resulting from the selection of the in-home sample, the size of the school and the number of peer groups.

VI. Mechanisms

The primary hypothesis we are interested in testing is whether religiosity has a direct effect on mental health or if our estimated effect is driven by being in a more religious group of peers. This provides evidence on a key potential mechanism for how religiosity affects mental health that is highlighted in the literature—social support (Ellison and Henderson, 2011). We then turn to other key mechanisms, whether religiosity reduces exposure to some types of stressors or buffers against these stressors. Finally, we consider whether there is evidence that the direct effects of religiosity on mental health operate through improved self-esteem and problem solving, key psychological resources and coping skills that have been identified in the literature as helpful for dealing with stress (Sherkat and Reed, 1992; Smith, Weigert, and Thomas, 1979).

A. Social Context

Because we rely on variation in peer group religiosity, we must be open to the interpretation of our estimated effect of religiosity as being inclusive of peer religiosity. As far as we know, this is a characteristic that is shared by all the instrumenting strategies used to identify the effect of religiosity, it is just made more explicit in our context. While the effect of religiosity inclusive of social context is arguably also of policy interest, we have a strong theoretical justification why peers (as we have defined them) would not directly affect depression. Theoretical and applied literature in psychology suggests that peers affect depression primarily through close relationships and not through the typical status-oriented processes that we often consider in peer effects models, such as for externalizing behaviors (Brechwald and Prinstein, 2011). For instance, Rose (2002) describes a process of *co-rumination* by which negative affect and hence depression spreads among close friends, through dwelling on and

re-enforcing each other's negative experiences. Consistent with this, any evidence of contagion in depression in the literature is among friends and spouses (Prinstein, 2007; Stevens and Prinstein, 2005; van Zalk et al., 2010).

Because we observe friends in our data, we can test directly the hypothesis that depression spreads among close friends rather than the school-cohort peer groups we have defined. We measure friends' depression as the average depression of any person whom i nominates to be her friend.[34] In column (1) of Table 6, we estimate the effects of religiosity controlling for average friends' depression. While we find that friends' depression matters, the estimated effects of religiosity are remarkably similar, which would not be the case if the effects of religiosity were driven by friends.[35] That said, these are biased estimates of the effect of friends' depression because of measurement error and/or selection into friendship. In column (2), we address this by instrumenting for average friends' depression with same school-grade-race-gender peers' average depression.[36] We find that though the estimated effects of friends' depression are larger after instrumenting, the effects of religiosity remain remarkably robust and if anything are higher. In column (3), we perform the same regression except controlling for friends' average religiosity in the first and second stages. In this case, average friends' religiosity is not statistically significantly correlated with depression and estimated effects of religiosity are again similar.[37] These combined results highlight the main reason that we believe we have identified an individual effect of religiosity rather than a social effect: the peer group

[34] As in Table 2, we set average friends' depression to 0 for the missing observations and include an indicator that the person is missing friends' depression. We also allow for the effect of religiosity to differ by whether the person is missing friendships.

[35] We also find that estimates of the effect of religiosity are not significantly different for the sample that is missing friends, which would not be the case if friends mattered.

[36] We choose average school-grade-race-gender peers' depression because this is a stronger predictor and gives better F-statistics than the same school-grade-race-gender-denomination peers. It also fits observed patterns of friendship homophily.

[37] In unreported results, we also find that friends' characteristics are statistically significant predictors of religiosity and depression, which offers an interesting contrast to our findings on peer characteristics in Table 5 and further corroborates our hypothesis. Estimates on religiosity are very similar when we control for peer and/or friend characteristics.

as we have defined it matters for religiosity but not for depression, because contagion in depression occurs only among close friends.

A further test relies on the idea that if estimated effects are driven by social influence, we would expect the effect to be larger with more peers. Thus, columns (4) and (5) interact religiosity with the number of peers of the same school-grade-race-gender-denomination and the number of peers of the same denomination in the school. Formally, these regressions take the form

$$H_{is} = \alpha_1 R_{is} + \boldsymbol{X}'_{is}\boldsymbol{\alpha}_2 + \alpha_3 R_{is} W_{is} + \alpha_4 W_{is} + \alpha_s + \varepsilon_{is}, \tag{3}$$

where W_{is} denotes the relevant peer group size, R_{is} is instrumented by $\overline{R}_{g(i)s}$ as before and $R_{is}W_{is}$ is instrumented by $\overline{R}_{g(i)s}W_{is}$.[38] We do not find evidence that effects vary based on the size of the peer group or the number of peers in the school of the same denomination.[39]

A related hypothesis is that if the effect of religion is driven through social support at school, we might expect other school activities (clubs or sports) to act as alternative social support structures, substituting for religiosity. In Table 7, we consider whether there is evidence of substitutability, in that more religious students participate less in school activities. Columns (1) to (3) of Panel A suggest that this is not the case. We also test whether religiosity matters less if the adolescent participates in school clubs or sports, following the model presented in equation (3), where W_{is} is now the number of clubs or sports or combined school activities. Columns (1) to (3) of Panel B show that religiosity does not matter statistically significantly less for adolescents participating in school activities. This is true even though school activities are statistically significantly negatively correlated with depression.[40] Finally, column (4)

[38] Note that this is easiest to interpret when W_{is} is exogenous, which may not be plausible here. Bun and Harrison (2014) describe conditions under which the interaction can be interpreted as exogenous even if the stressor itself is endogenous. In our context some reasonable sufficient conditions are that the covariance of W_{is} and the unobservable determinants of mental health do not vary systematically with peer religiosity and that peer religiosity is independent of W_{is} or a linear function of W_{is}.

[39] A number of other specifications (not reported) such as the percentage in the grade or percentage of the same-denomination in the county similarly show no statistically significant interactions with religiosity.

[40] We test robustness of these findings to a variety of functional form assumptions,

in both panels consider whether school friendships (measured by the in-degree, i.e., the number of schoolmates that nominate a given adolescent as a friend) substitute for religiosity. Again, we find that religiosity does not significantly affect school friendships and does not seem to matter less for individuals with more friends. Thus, the evidence does not support that school activities or friendships offer substitute support structures for religiosity in their effects on depression.

B. Stressors

The literature suggests that religiosity reduces exposure to stressors that may be correlated with mental health (Ellison and Henderson, 2011). We present in Table 8 evidence on this, selecting a set of stressors selected based on whether we find them to be correlated with depression—GPA, whether a family member or friend has committed suicide in the past 12 months, general health, and whether the adolescent is in a single parent family.[41] Panel A shows the instrumented effects of religiosity on each of these stressors. Religiosity does not reduce exposure to these types of stressors in statistically significant ways. Panel B then considers whether there is evidence of stress-buffering effects of religiosity, using the same model as in equation (3), where W_{is} is now defined as a different stressor of interest in each column. We find that the stress-buffering hypothesis does seem to hold for the suicide of someone close to the adolescent, general health and coming from a single parent family, but not for GPA.

C. Self-Esteem and Passive Problem Solving

Psychologists hypothesize that religiosity can support mental health through self-esteem if, for instance, relationship with a divine helps provide a

such as allowing both the decision to participate in sports and the number of sports to matter, as well as considering the log of the number of sports to deal with the skewed distribution. We also test sensitivity to outliers. In no case can we find evidence that these activities substitutes for religiosity.

[41] We also consider parental divorce, whether the parents fight, whether parents have other marriage difficulties or financial problems, but these are not significantly related to depression conditional on covariates.

sense of worth.[42] A second related theory is that religiosity affects mental health through how people cope with difficult situations or problems, by inspiring a more fatalistic perspective on life, leading one to engage in more passive problem-solving attitudes (Pargament and Brant, 1998).

Add Health includes questions that are intended to reflect the adolescent's self-esteem and approaches to problem solving, and we create an index of self-esteem and passive problem solving based on these questions.[43] Appendix Table A11 considers the effect of religiosity on self-esteem and passive problem solving. Consistent with the literature described in Ellison and Henderson (2011), OLS shows that religiosity is positively correlated with self-esteem. When we instrument for religiosity, the estimated effect of religiosity increases from 0.075 to 0.15 in the case of self-esteem and 0.02 to 0.11 for the case of passive problem solving. However, the standard errors are also large so that our IV results are not statistically significantly different from zero. This does not provide strong support that self-esteem and passive problem solving are key channels for the effect of religiosity, at best suggesting a degree of heterogeneity in the effects of religiosity on these potential mediators.[44]

VII. Conclusion

In this paper, we find that a one unit increase in religiosity decreases the probability of being depressed by 3% out of a probability of 24%. To put this estimate in context, an increase in mother's education from no high school degree to a high school degree or more is correlated with only a 5% reduction in the probability of being depressed. Our estimated effect of religiosity is bigger than what is found in OLS. This could be a result

[42] Importantly, the arguments for why religiosity could support self-esteem could also be turned to suggest reasons that religiosity could hurt self-esteem (Ellison and Henderson, 2011).

[43] See details in Appendix Table A1 and discussion of these measures in Rosenberg (1989) and Nooney (2005).

[44] Appendix Table A11 further shows evidence of a mediating effect of self-esteem and passive problem solving in that the coefficient on religiosity on depression is statistically significantly reduced when these are controlled for. That said, the evidence is not conclusive given the strong possibility of reverse causality from depression to self-esteem and passive problem solving.

of negative selection into religiosity, i.e., that individuals may select into religiosity to deal with depression or shocks associated with depression, as evidenced in the literature, or because of random measurement error in individuals' reported religiosity.

Interestingly, while the effects of religiosity on depression do not vary by how religious the individual is, more depressed individuals benefit significantly more from religiosity than the least depressed. This offers a striking contrast to evidence on the effectiveness of cognitive-based therapy, one of the most recommended forms of treatment, which is generally less effective for the most depressed individuals.

The method we use to identify a causal effect of religiosity relies on variation in peer composition within schools across time. Our results are robust to a large number of specification checks, helping us rule out potential confounders such as selection into peer groups and unobservable shocks that affect the group as a whole. We show that the reason the cross-cohort peer variation identifies an individual effect of religiosity rather than a social effect is that the peers that matter for depression appear to be different from the peers that matter for religiosity, which is consistent with theory and previous studies on depression. We find that school peers of the same denomination regardless of whether they are friends have a particularly strong association with adolescents' religiosity, whereas close friends are highly associated with mental health. While there is significant discussion of the complex nature of adolescent peer groups in the psychology literature (Brown, 2004), less is known about different realms of influence for peer groups in different aspects of adolescents' lives (Brechwald and Prinstein, 2011). We see this as an important avenue for further research in economics.

We consider potential mechanisms for why religiosity may affect depression. We show that the benefits of religiosity do not appear to derive from a more religious or less-depressed social context in the school. Furthermore, alternative forms of school social support, such as clubs, sports and the number of friends, do not appear to substitute for religiosity. We also do not find evidence that religiosity reduces exposure to stressors. We find instead that religiosity helps to buffer against some types of stressors, including poor health, the suicide of a friend or family

member, or coming from a single parent home. We find that while the hypothesis of religiosity operating through improved self-esteem and coping skills is supported by OLS, our instrumented estimates show larger but statistically insignificant effects of religiosity on these potential mediators, raising questions about their role.

Overall, our findings have important implications for policies related to improving mental health in adolescence. Given that clinically the effect of antidepressants on reducing depression is successful in about one-fifth of cases (IHN, 2015), our research suggests that counselors would be remiss to dismiss the potential beneficial effect of religiosity in treating clients, contributing to a vigorous debate initiated by Freud (1927). Future work would benefit from more detailed information on churches and other places of worship that adolescents attend to determine further the mechanisms driving these effects.

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TABLE 1
FRIENDS SORTING PATTERN

	(1) Proportion of students sharing certain characteristics in the school	(2) Proportion of friends sharing certain characteristics among all friends	(3) Difference = (1) − (2)
	mean (s.d.)	mean (s.d.)	mean (s.e.)
Of same school and grade	0.277 (0.109)	0.713 (0.397)	−0.437*** (0.005)
Of same school and race	0.617 (0.305)	0.793 (0.375)	−0.176*** (0.005)
Of same school and gender	0.501 (0.040)	0.645 (0.393)	−0.144*** (0.005)
Of same school and denomination	0.330 (0.224)	0.414 (0.442)	−0.084*** (0.005)
Of same school, race and denomination	0.219 (0.192)	0.352 (0.428)	−0.132*** (0.005)
Of same school, grade, race, and gender	0.083 (0.058)	0.400 (0.423)	−0.317*** (0.005)
Of same school, grade, race, gender, and denomination	0.030 (0.036)	0.182 (0.340)	−0.152*** (0.004)
Observations	6,342	6,342	6,342

NOTE.—This table reports the proportions of students and friends who share the same characteristics. Column (1) reports the share of students who share certain characteristics with the respondent in the school. Column (2) reports the share of the respondent's friends who share certain characteristics with the respondent among all his/her friends. Column (3) tests the difference between these two proportions using a t-test. Standard deviations or standard errors are in parentheses. ***, **, and * denote statistical significance at 0.01, 0.05, and 0.10 levels respectively.

TABLE 2
ASSOCIATIONS BETWEEN THE ADOLESCENT'S RELIGIOSITY AND THE RELIGIOSITY OF THEIR FRIENDS AND PEERS

	Dependent variable = religiosity		
	(1)	(2)	(3)
Friends' religiosity	0.126***		
	(0.015)		
Same-denomination friends' religiosity		0.164***	
		(0.022)	
Cross-denomination friends' religiosity		0.085***	
		(0.015)	
Same-denomination peer religiosity[a]			0.109***
			(0.021)
Cross-denomination peer religiosity[b]			0.022**
			(0.011)

NOTE.—This table reports the estimates for regressions of the adolescent's own religiosity on the religiosity of their friends or peers. All models control for covariates as in Table A5. The number of observations is 12,945 in all models. Clustered standard errors at the school level are in parentheses. ***, **, and * denote statistical significance at 0.01, 0.05, and 0.10 levels respectively. Not all observations have valid friends data. Missing values for friends' religiosity are coded as zeros, and dummy variables indicating these missings are included in the regressions where appropriate.

[a] This is calculated as the average religiosity of same-denomination peers in the same school and grade, of the same gender and race.

[b] This is calculated as the average religiosity of cross-denomination peers in the same school and grade, of the same gender and race.

TABLE 3
BASELINE RESULTS FOR THE EFFECT OF RELIGIOSITY ON DEPRESSION

	Dependent variable= depression			Dependent variable = depressed		
	(1) OLS	(2) IV	(3) First stage	(4) OLS	(5) IV	(6) IV probit
Religiosity	-0.163^{***}	-0.698^{**}		-0.006^{***}	-0.034^{**}	-0.034^{**}
	(0.024)	(0.289)		(0.001)	(0.016)	(0.016)
Peer religiosity			0.112^{***}			
			(0.020)			
Controls	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes
F-statistic			30.438			

NOTE.—This table reports the OLS and IV estimates of religiosity on the CES-D scale of depression and the probability of being depressed. The instrumental variable peer religiosity is calculated as the average religiosity of peers (excluding oneself) of the same school, grade, race, gender, and denomination. Columns (1)–(5) report the coefficients, whereas column (6) reports the marginal effects at the mean. Clustered standard errors at the school level are in parentheses. ***, **, and * denote statistical significance at 0.01, 0.05, and 0.10 levels respectively. *F*-statistic on the excluded instrument refers to the Wald version of Kleibergen and Paap (2006) *rk*-statistic on the excluded instrumental variables for non-i.i.d. errors (Baum, Schaffer, and Stillman, 2002). The number of observations is 12,945 in all models (31 observations are not used for identification in model (6) due to perfect prediction of school fix effects). Estimates for control variables are omitted here but reported in Table A5.

TABLE 4
ROBUSTNESS CHECKS ON SELECTION ISSUES

	(1) Substitute adolescent's denomination with parent's denomination		(2) Control for school-specific grade trends in depression		(3) Control for average religiosity of school-denomination peers		(4) Control for average religiosity of school-race-denomination peers		(5) Control for average religiosity of school-race-denomination peers and its grade trends		(6) Placebo test using average religiosity of two-grade-apart peers as an additional instrument	
	Second stage	First stage	Second stage	First stage	Second stage	First stage	Second stage	First stage	Second stage	First stage	Second stage	First stage
Religiosity	−0.736*		−0.855**		−0.859**		−1.255**		−1.261*		−0.883**	
	(0.435)		(0.434)		(0.401)		(0.628)		(0.658)		(0.377)	
Peer religiosity		0.096***		0.077***		0.095***		0.069***		0.067***		0.096***
		(0.028)		(0.021)		(0.023)		(0.025)		(0.026)		(0.023)
Average religiosity of school-denomination peers					0.173	0.164**					0.179	0.138*
					(0.172)	(0.083)					(0.166)	(0.080)
Average religiosity of school-race-denomination peers							0.305	0.208***	0.236	−0.244*		
							(0.209)	(0.059)	(0.405)	(0.138)		
Grade × average religiosity of school-race-denomination peers									0.008	0.048***		
									(0.053)	(0.015)		
Average religiosity of two-grade-apart peers												0.031
												(0.019)
F-statistic	12.114	12.114	13.387	13.387	16.721	16.721	7.501	7.501	6.812	6.812	11.733	11.733
J-test[a]											0.827	
Observations	9,972	9,972	12,945	12,945	12,945	12,945	12,945	12,945	12,945	12,945	12,945	12,945

NOTE.—All models report the first- and second-stage of the IV estimates, where religiosity is instrumented for with peer religiosity, and include control variables as in Table A5. Model (1) replaces the respondent's denomination with the parent's denomination. Models (2)–(5) add further controls as indicated in the column heading separately. Model (6) includes an additional instrument, average religiosity of two-grade-apart pees, and controls for a binary variable indicating if this variable is missing. Average religiosity of two-grade-apart peers is calculated as the average religiosity of peers who are of the same school, race, gender, and denomination, but two grades ahead of behind the respondent. Clustered standard errors at the school level are in parentheses. ***, **, and * denote statistical significance at 0.01, 0.05, and 0.10 levels respectively. *F*-statistic on the excluded instruments refers to the Wald version of the Kleibergen-Paap (2006) *rk*-statistic on the excluded instrumental variables for non-i.i.d. errors.

[a] This row reports the *p*-value of Hansen's *J*-statistic χ^2 test of the over-identification restrictions.

TABLE 5
CHECKS ON UNOBSERVED GROUP EFFECTS

	(1) Subset of peer characteristics as instruments[a]	(2) Control for peer depression and characteristics	(3) Same- and cross-gender peer religiosity as instruments[b]		(4) Same- and cross-race peer religiosity as instruments[c]		(5) Same- and cross-denomination peer religiosity as instruments[d]	
	2-step GMM	Second stage	Second stage	First stage	Second stage	First stage	Second stage	First stage
Religiosity	-1.254^{*}	-0.620^{**}	-0.744^{***}		-0.720^{**}		-0.711^{**}	
	(0.723)	(0.312)	(0.277)		(0.294)		(0.290)	
Peer depression		0.006						
		(0.024)						
Same-gender peer religiosity				0.108^{***}				
				(0.020)				
Cross-gender peer religiosity				0.065^{***}				
				(0.022)				
Same-race peer religiosity						0.112^{***}		
						(0.020)		
Cross-race peer religiosity						-0.015		
						(0.015)		
Same-denomination peer religiosity								0.112^{***}
								(0.020)
Cross-denomination peer religiosity								0.017
								(0.013)
F-statistic	3.046	28.467	19.204	19.204	15.521	15.521	15.580	15.580
Joint test[e]		0.886						
Over-identification test[f]	0.510		0.750	0.750	0.551	0.551	0.782	0.782

NOTE.—All models include control variables as in Table A5. Model (1) instruments for religiosity with peer characteristics. Model (2) adds controls for peer depression and characteristics on top of the baseline model. Models (3)–(5) instrument for religiosity with over-identifying instrumental variables indicated under each column heading. Models (3)–(5) also control for a dummy variable indicating missing values in cross-gender (8.6%), cross-race (31.9%), or cross-denomination peer religiosity (5.3%), respectively. Clustered standard levels at the school level are in parentheses. ***, **, and * denote statistical significance at 0.01, 0.05, and 0.10 levels respectively. *F*-statistic on the excluded instrument refers to the Wald version of the Kleibergen-Paap (2006) *rk*-statistic on the excluded instrumental variables for non-i.i.d. errors. The number of observations is 12,945 in all models.

[a] Instruments include peer age, peer mother not present, peer mother having a degree and its squared term, and peer father not present.

[b] The instruments are same- and cross-gender peer religiosity. Same-gender peer religiosity is calculated as the average religiosity of peers of the same school, grade, race, denomination and gender. Cross-gender peer religiosity is calculated as the average religiosity of peers of the same school, grade, race, denomination but different gender.

[c] Instruments and their calculations are similar as in [b] but for but for race.

[d] Instruments and their calculations are similar as in [b] but for but for denomination.

[e] This row reports the *p*-value for a joint significance test of all peer characteristics in the second stage. The joint test *p*-value for the first stage is 0.490.

[f] This row reports the *p*-value for Hansen's *J*-statistic χ^2 test of the over-identifying restrictions.

TABLE 6
INDIVIDUAL AND SOCIAL EFFECTS OF RELIGIOSITY ON DEPRESSION

	(1) Control for friends' depression	(2) Instrument for friends' depression	(3) Control for friends' religiosity	(4) Interact religiosity with peer group size	(5) Interact religiosity with number of school-denomination students
Religiosity	-0.737^{**}	-0.856^{***}	-0.788^{**}	-0.752^{**}	-0.665^{**}
	(0.307)	(0.294)	(0.385)	(0.309)	(0.311)
Friends' depression	0.076^{***}	0.172^{**}			
	(0.026)	(0.086)			
Friends' religiosity			0.068		
			(0.072)		
Religiosity × friends missing	0.046	0.058	0.093		
	(0.133)	(0.139)	(0.186)		
Friends missing	0.662	1.601	−0.077		
	(1.255)	(1.566)	(1.221)		
Religiosity × peer group size				0.008	
				(0.008)	
Peer group size				−0.064	
				(0.066)	
Religiosity × number of school-denomination students					0.000
					(0.001)
Number of school-denomination students					0.002
					(0.006)
F-statistic	14.707	8.215	11.006	15.301	15.256

NOTE.—All models include the covariates as in Table A5. To allow for differential effects for those who have no valid friends data (65%), models (1)–(3) include an binary indicator of friends missing, and appropriate interaction terms described as below. Model (1) controls for friends' depression and the friends missing indicator on top of the baseline model, and instruments for religiosity and its interaction with friends missing, with peer religiosity and its interaction with friends missing. Model (2) further treats friends's depression as endogenous and instruments for it, with average depression of school-grade-race-gender peers and its interaction with friends missing. Model (3) controls for friends' religiosity as well as the friends missing indicator on top of the baseline model, and instruments for religiosity and its interaction with friends missing, with peer religiosity and its interaction with friends missing. Model (4) (or (5)) instruments for religiosity and its interaction with peer group size (or the number of same-denomination students in the school) with peer religiosity and its interaction with peer group size (or the number of same-denomination students in the school), while controlling for peer group size (or the number of same-denomination students in the school). Peer group size refers to the number of peers in the same school-grade-race-gender-denomination group. Clustered standard levels at the school level are in parentheses. ***, **, and * denote statistical significance at 0.01, 0.05, and 0.10 levels respectively. *F*-statistic on the excluded instrument refers to the Wald version of the Kleibergen-Paap (2006) *rk*-statistic on the excluded instrumental variables for non-i.i.d. errors. The number of observations is 12,945 in all models.

TABLE 7
RELIGIOSITY, SCHOOL ACTIVITIES AND DEPRESSION

	School activities			
	(1) School club participation	(2) School sports participation	(3) School activity participation	(4) School friendships (in-degree)
	Panel A: dependent variable = school activity[a]			
Religiosity	0.016	−0.012	−0.017	0.025
	(0.017)	(0.021)	(0.020)	(0.199)
F-statistic	30.438	30.438	30.438	18.817
Observations	12,945	12,945	12,945	9,543
	Panel B: dependent variable = depression			
Religiosity	−0.670**	−0.748***	−0.740**	−0.644*
	(0.313)	(0.284)	(0.298)	(0.368)
Interaction[b]	−0.040	0.135	0.053	−0.025
	(0.138)	(0.144)	(0.154)	(0.019)
School activity[a]	−0.137	−1.708	−1.211	0.244
	(1.150)	(1.290)	(1.318)	(0.171)
Joint test[c]	0.112	0.021	0.005	0.329
F-statistic	14.821	15.177	15.721	9.450
Observations	12,945	12,945	12,945	9,543

NOTE.—Panel A reports the IV estimates for the effect of religiosity on school activities. Panel B reports the IV estimates for the main and interaction effect of religiosity on depression conditional on school activities. All models control for covariates as in Table A5. Clustered standard levels at the school level are in parentheses. ***, **, and * denote statistical significance at 0.01, 0.05, and 0.10 levels respectively. *F*-statistic refers to the Wald version of the Kleibergen-Paap (2006) *rk*-statistic on the excluded instrumental variables for non-i.i.d. errors.

[a] Each school activity refers to the one indicated in the column heading. Detailed definition for school activities can be found in Table A1.

[b] This is the interaction term between religiosity and school activities.

[c] This reports the *p*-value of a joint significance test of school activity and its interaction term with religiosity.

TABLE 8
STRESS-BUFFERING EFFECTS OF RELIGIOSITY ON DEPRESSION

	Stressor			
	(1) Most recent GPA	(2) Family or friends suicide	(3) General health	(4) Single parent
	Panel A: dependent variable = stressor[a]			
Religiosity	0.033	−0.006	−0.063	0.014
	(0.031)	(0.019)	(0.039)	(0.013)
F-statistic	30.425	30.284	30.416	28.102
Observations	12,838	12,888	12,944	10,504
	Panel B: dependent variable = depression			
Religiosity	−0.667*	−0.643**	−1.436***	−0.575*
	(0.349)	(0.293)	(0.389)	(0.320)
Interaction[b]	0.015	−0.598***	0.160**	−0.322*
	(0.088)	(0.197)	(0.072)	(0.177)
Stressor[a]	−1.747**	8.214***	−3.050***	2.630*
	(0.780)	(1.687)	(0.623)	(1.525)
F-statistic	14.615	14.914	16.010	14.120
Observations	12,838	12,888	12,944	10,504

NOTE.—Panel A reports the IV estimates for the effect of religiosity on exposure to stressors. Panel B reports the IV estimates for the main and interaction effect of religiosity on depression conditional on stressors. All models control for covariates as in Table A5. Clustered standard errors at the school level are in parentheses. ***, **, and * denote statistical significance at 0.01, 0.05, and 0.10 levels respectively. *F*-statistic refers to the Wald version of the Kleibergen-Paap (2006) *rk*-statistic on the excluded instrumental variables for non-i.i.d. errors.

[a] Each stressor refers to the one indicated in the column heading. Detailed definition for stressors can be found in Appendix Table A1.

[b] This is the interaction term between religiosity and stressor.

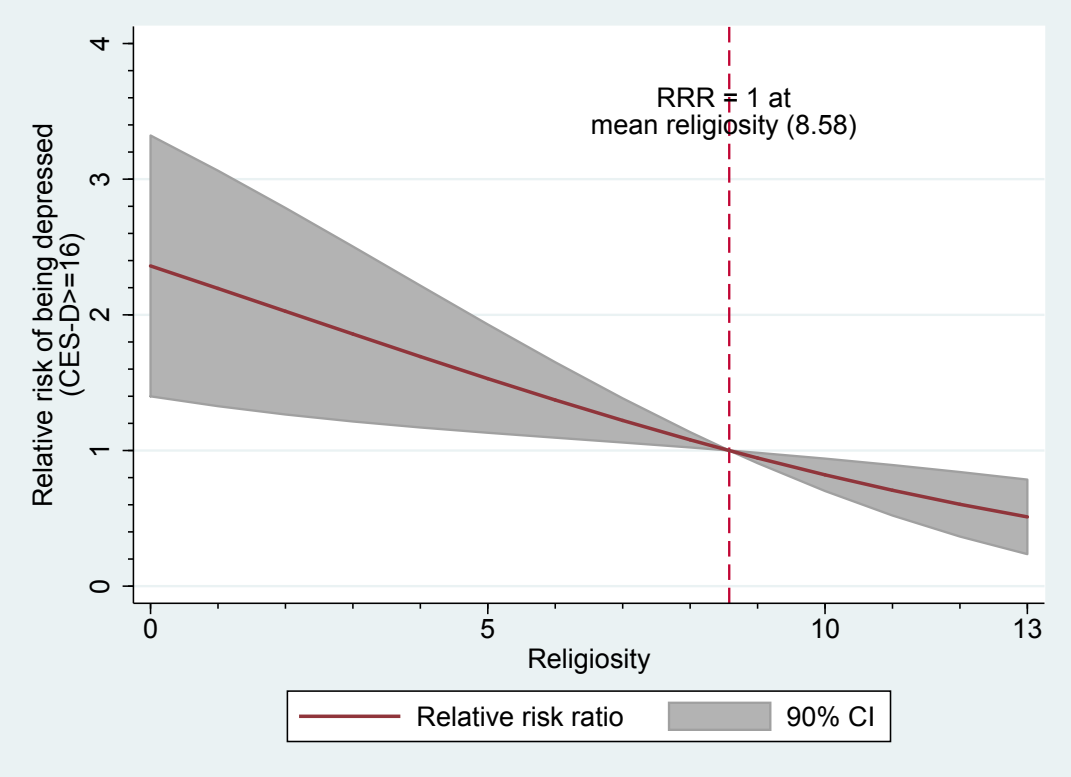

FIG. 1.—Predicted relative risk ratios (RRRs) at different levels of religiosity. Probabilities of being depressed (CES-D score $\geq$ 16) are predicted for each level of religiosity from 0 to 13 based on estimates from the IV probit model. RRRs are calculated as the ratios of the probability of being depressed for each level of religiosity, to the probability of being depressed at mean religiosity (8.58).

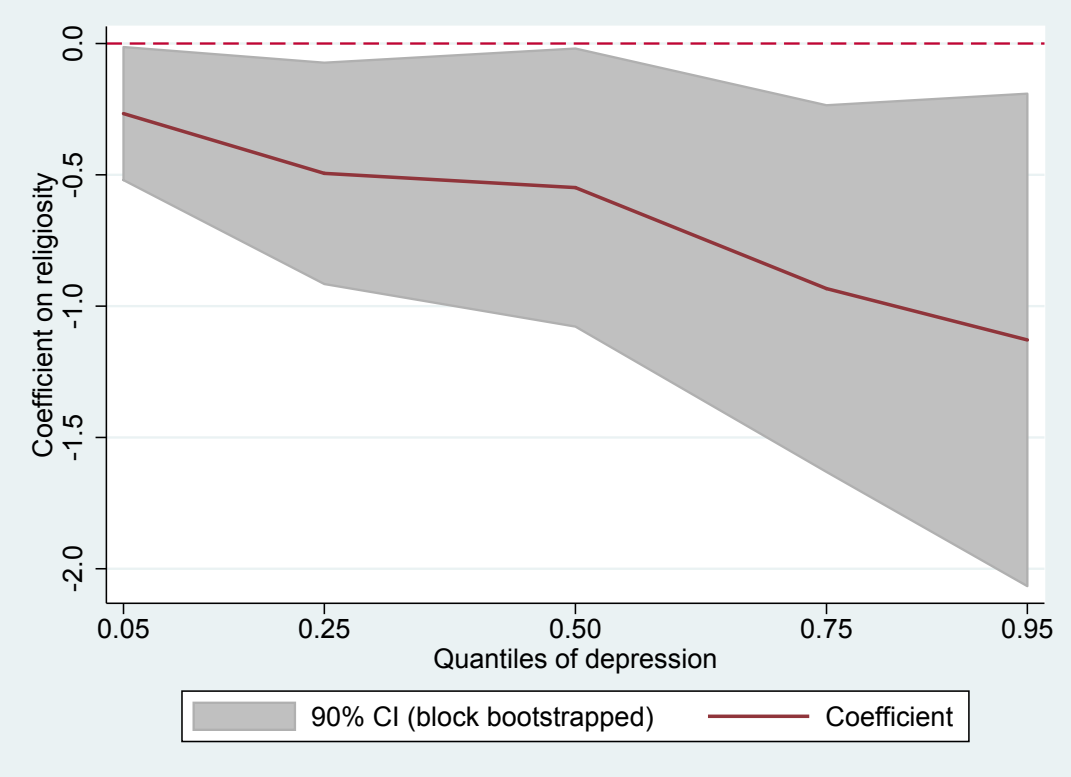

FIG. 2.—The effect of religiosity on different quantiles of the conditional depression distribution. Estimated quantile regression coefficients on religiosity and 90% confidence intervals are plotted for different quantiles of depression. Standard errors are obtained by block bootstrapping clustered at the school level with 500 replications. Estimation procedures are described in Online Appendix Section A.3.

Online Appendix A: Supplemental Material for "Religion and Depression in Adolescence"

A.1 Heterogeneity

To explore heterogeneity, we estimate how the effects of religiosity differ across the conditional quantiles of the depression index, using a version of the two-step control function approach, as developed in Imbens and Newey (2009). We estimate the first stage as described in equation (2) and recover the estimated residual $\hat{u}_{is}$ from this regression rather than the predicted value of religiosity. We then include the residual as an additional regressor in our second stage regression to control for the endogeneity of religiosity, and estimate the second stage by quantile regression, i.e.,

$$H_{is} = \alpha_1(q)R_{is} + \boldsymbol{X}'_{is}\boldsymbol{\alpha}_2(q) + \alpha_3(q)\hat{\alpha}_s + \alpha_4(q)\hat{u}_{is} + \varepsilon_{is}(q), \tag{4}$$

for each quantile q.[45] Note that this requires a stronger form of assumption A1, that the instrument satisfies full conditional independence.

A.2 Balancing Test

We perform balancing tests to determine if peer religiosity is correlated with observable individual characteristics, which would be evidence of selection on observables. The balancing tests should hold conditional on the full set of gender, race and denomination dummies that define the peer group and that we condition on in the main regressions. For instance, Hispanics are more religious, and they also have peers who are more religious by our definition. Hispanic is also correlated with lower income. Therefore a regression of income on average religiosity of same-race peers that did not control for individual race dummies would

[45] There is no accepted way in the literature for incorporating fixed effects into quantile models. We report results that predict the school fixed effects from the mean 2SLS regression and control for these in equation (4). Standard errors are block bootstrapped at the school level with 500 replications. Estimates are qualitatively similar if we instead include school dummies. There are on average 276 students per school in our final sample, which helps alleviate concerns about consistency in this case.

find (for the case of Hispanic students) that peer religiosity is negatively correlated with individual income by construction. The variation that we isolate by controlling for the full set of gender, race and denomination dummies is instead plausibly random variation in the average religiosity of "like" peers within schools across grades.[46]

Table A6 shows the results of these tests. Out of nine indicators for adolescent and family background characteristics, only one variable, mother not being present, seems to be correlated with peer religiosity and the size of the correlation is very small, at -0.002. Thus the observable covariates seem to be well balanced between adolescents facing peers who are more religious and those facing peers who are less religious, conditional on the group dummies. This provides supportive evidence that at least in terms of observables the assumption of random variation in peer religiosity is valid.

A.3 Additional Robustness Checks

Scale While the CES-D 20 is a well-recognized, validated scale, we remain concerned about the extent to which our results are robust to different measurement choices. In Table A7, we conduct sensitivity analysis with a series of different cutoffs on the CES-D scale for high depressive symptoms. Our baseline results adopt 16 as the threshold for being depressed (Radloff, 1977), and Table A7 shows that results are robust for higher thresholds. In Table A8, we compare estimates when we remove 3 questions from the CES-D that are more social in nature,[47] which serves as another check for social effects and reference effects. To provide a common metric we normalize both the CES-D and religiosity. Column (2) of Table A8 presents results with the reduced scale and column (1) with the original scale. Estimates are very similar. The choice to assign equal weights to the different questions was also arbitrary. Columns (3) and (4) include the same specifications as columns (1) and (2), except extracting a factor from the different questions included in our depression

[46] Note that results are also robust if we control for the interactions of gender, race and denomination at the individual level.

[47] These questions include "You felt that you were just as good as other people", "You felt that people disliked you", and "People were unfriendly to you".

and religiosity scales, using principal component analysis based on polychoric correlations, which respects the ordinality of the different components of the scales. Results again are similar.

Sample Selection In Table A9, we further test how sample selection affects our results. We control for same-denomination average religiosity to rule out associated concerns about selection into schools. Column (1) repeats the results in column (2) of Table 4 for comparison. Column (2) adds in the non-Christian-affiliated subgroup. Results are similar with an estimated effect of religiosity falling from -0.86 to -0.84. Column (3) attempts to deal with the problem of dropping observations for individuals due to missing peer groups. For these individuals, we assigned the peer religiosity at the school-grade-gender-denomination level, if available, and if not then at the school-grade-race-gender level.[48] These modifications incorporate most of the students who report a religious affiliation, 15,939 out of a total sample of 16,169 whose other relevant variables are not missing. In the specification, we also include a control for the students who are missing observations of school-grade-race-gender-denomination peer average religiosity and allow for the effect of the peer religiosity to be different for these students. The first stage (not reported) shows that the main effect of peer religiosity is 0.11, and this is reduced to about 0.04 for the subgroups where we do not observe peer religiosity at the school-grade-race-gender-denomination level, so our instrument is much weaker for this subgroup. That said, the estimated effect of religiosity with this bigger sample is still similar at -0.61. We also pass the test of over-identifying restrictions, which provides further support that unobservables about these students with missing peer groups do not present additional endogeneity concerns.

A final sample selection concern is the exclusion of the non-religious from the sample. Ideally, we would like to find an instrument that shifts whether a student reports a religious affiliation, the extensive margin, as well as religiosity so that we could jointly estimate the selection into religion and religiosity. We tried a number of instruments based on

[48] Results are comparable if we replace missings first with school-grade-race-gender average religiosity and then school-grade-gender-denomination average religiosity.

within-school peer variation, including the percentage of peers that are non-religious using different definitions of peer groups and allowing for higher order terms. We could not find a robust predictor of whether a student was religious or not. One interpretation of this is that peers do not directly affect the choice to be religious, which is in line with previously cited work by Smith and Denton (2005) showing that adolescents rarely deviate from the religious affiliation of their parents. Iannaccone (1990) also shows that religious conversions most frequently occur in young adulthood. Thus, instead we treat whether a student is religious as exogenous and include the non-religious in the regression, with a control for being non-religious and defining peer religiosity for these students at the school-grade-race-gender level.[49] This increases the sample to 18,104 out of a total possible sample of 18,420. The estimated effect of religiosity is robust at -0.56.

Table A10 considers whether measurement error in the peer groups or variation in the size of the peer groups may be biasing our results. Column (1) deals with measurement error by weighting peer religiosity by the percentage of school-grade-race-gender peers observed in the data (calculated from the in-school survey) and controlling for the percentage observed.[50] Estimated effects are similar with this alternative instrument, which helps account for differential sampling bias across subgroups. Second, column (2) shows that the estimated effects of peer religiosity are larger in the big schools (those with more than 1000 students). However, the estimated effects of religiosity remain similar when we allow the instrument to vary by the size of the school, and we continue to pass the test of over-identifying restrictions. In column (3), we allow the effect of peer religiosity to vary by the number of peer groups. The statistical significance of the first stage is driven by the schools with more peer groups. This is similar to the result for big schools, which have 40 peer groups on average compared to 19 on average in other schools. However, our estimated effects of religiosity remain similar in this case,

[49] Note that if we define religiosity at the denomination level, peer religiosity is 0 and perfectly predicts own religiosity.

[50] This follows the logic of Sojourner (2013) for dealing with measurement error in peer groups, with the exception that we do not observe religious affiliation in the school sample.

and we continue to pass the test of overidentifying restrictions. We then see whether the effects of peer religiosity are bigger with the size of the peer group or with the share of the peers in the grade (columns (4) and (5)). Here, we find no evidence of bigger effects of peer religiosity with larger peer groups. Again, estimated effect of religiosity are similar, and we pass the test of overidentifying restrictions. Combined this evidence suggests that while the size of the school and associatedly number of peer groups matter for identification, our estimated effects of religiosity are not biased by this.

A.4 Additional Tables and Figures

Additional tables and figures are presented at the end of this document.

Appendix References

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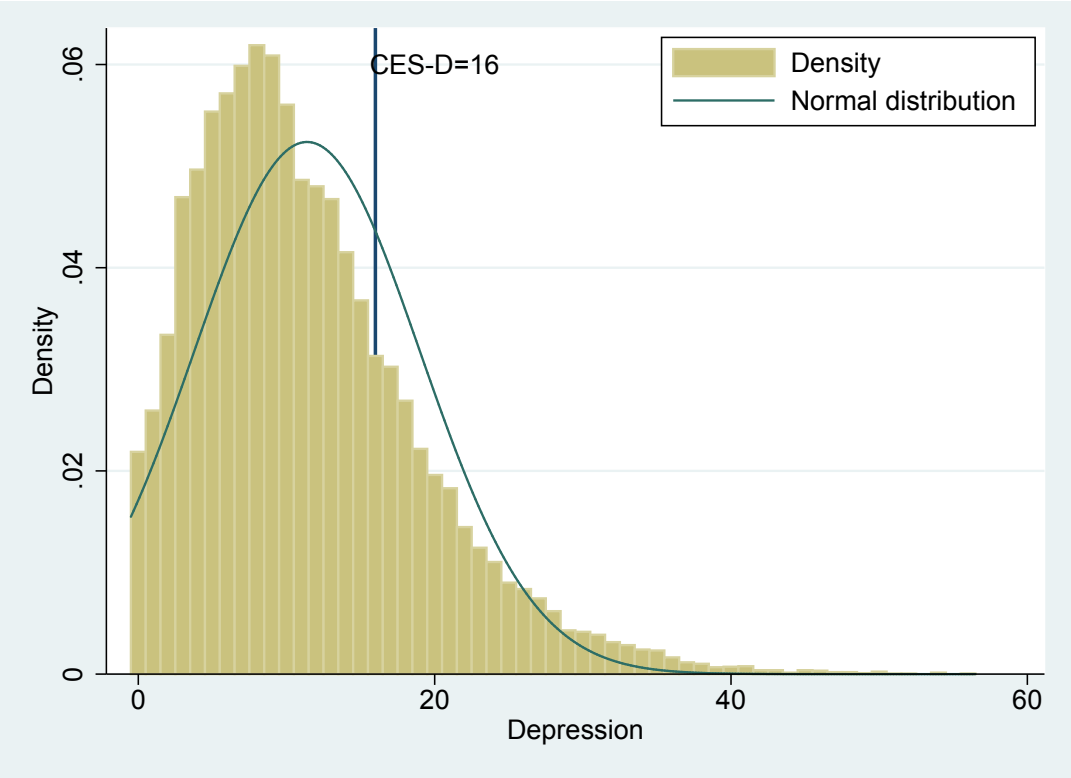

FIG. A1.—Distribution of the CES-D scale of depression

TABLE A1
DEFINITION OF KEY VARIABLES

Variable
Definition

Depression
Definition: sum over the following variables.
Coding of responses: 0 = never/rarely, 1 = sometimes, 2 = a lot of the time, 3 = most/all of the time.
How often was each of the following true during the last week?
(1) You were bothered by things that usually don't bother you.
(2) You didn't feel like eating, your appetite was poor.
(3) You felt that you could not shake off the blues, even with help from your family and your friends.
(4) You felt that you were just as good as other people.[a]
(5) You had trouble keeping your mind on what you were doing.
(6) You felt depressed.
(7) You felt that you were too tired to do things.
(8) You felt hopeful about the future.[a]
(9) You thought your life had been a failure.
(10) You felt fearful.
(11) You were happy.[a]
(12) You talked less than usual.
(13) You felt lonely.
(14) People were unfriendly to you.
(15) You enjoyed life.[a]
(16) You felt sad.
(17) You felt that people disliked you.
(18) It was hard to get started doing things.
(19) You felt life was not worth living.

Depressed
Definition: $= 1$ if depression ≥ 16, $= 0$ otherwise.

Religiosity
Definition: sum over the following variables.
(1) In the past 12 months, how often did you attend religious services?
Responses: 0 = never, 1 = less than once a month, 2 = less than once a week/at least once a month, 3 = once a week or more.
(2) Many churches, synagogues, and other places of worship have special activities for teenagers—such as youth groups, Bible classes, or choir. In the past 12 months, how often did you attend such youth activities?
Responses: coded same as question (1) above.

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Variable
Definition

(3) How important is religion to you?
Responses: 0 = not important at all, 1 = fairly unimportant, 2 = fairly important, 3 = very important.

(4) How often do you pray?
Responses: 0 = never, 1 = less than once a month, 2 = at least once a month, 3 = at least one a week, 4 = at least once a day.

Peer religiosity
Definition: The average religiosity of peers who are of the same school, grade, race, gender, and denomination.

Friends' religiosity
Definition: The average religiosity of students who the respondent nominated as friends in the same school.

School club participation
Definition: $= 1$ if the respondent answers "Yes" to currently participating or planning to participate later in the school year in the following listed clubs: French club, German club, Latin club, Spanish club, book club, computer club, debate team, drama club, Future Farmers of America, History club, Math club, Science club, band, cheerleading/dance team, chorus or choir, orchestra, other club or organization, newspaper, honor society, student council, and yearbook; $= 0$ otherwise.

School sports participation
Definition: $= 1$ if the respondent answers "Yes" to currently participating or planning to participate later in the school year in the following listed sport activities: baseball/softball, basketball, field hockey, football, ice hockey, soccer, swimming, tennis, track, volleyball, wrestling, and other sport; $= 0$ otherwise.

School activities participation
Definition: $= 1$ if school club participation $= 1$ or school sports participation $= 1$; $= 0$ otherwise.

Number of friends in school
Definition: The number of times the respondent is nominated as a friend by students in the school. This is also referred to as in-degree, and it is constructed from the friend network based on the in-school survey.

Most recent GPA
Definition: average across the following variables.
Coding of responses: 1 = D or lower, 2 = C, 3 = B, 4 = A.

(1) At the most recent grading period, what was your grade in each of the following subjects? English/Language Arts

(2) At the most recent grading period, what was your grade in each of the following subjects? Mathematics

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Variable
Definition

(3) At the most recent grading period, what was your grade in each of the following subjects? History/Social Studies
(4) At the most recent grading period, what was your grade in each of the following subjects? Science

Family/friends suicide
Definition: equals 1 if answer is "yes" to either question, and 0 otherwise.
Coding of responses: 1 = yes, 0 = no.
(1) Have any of your family tried to kill themselves during the past 12 months?
(2) Have any of your friends tried to kill themselves during the past 12 months?

General health
Definition: response to the following variable.
Coding of responses: 1 = poor, 2 = fair, 3 = good, 4 = very good, 5 = excellent.
(1) In general, how is your health?

Single parent
Definition: $= 1$ if parent is currently not in a marriage or marriage-like relationship; $= 0$ otherwise.

Self-esteem
Definition: sum over the following variables.
Coding of responses: 1 = strongly disagree, 2 = disagree, 3 = neither agree nor disagree, 4 = agree, 5 = strongly agree.
(1) You have a lot to be proud of.
(2) You like yourself just the way you are.
(3) You feel like you are doing everything just about right.
(4) You have a lot of good qualities.

Passive problem-solving
Definition: sum over the following variables.
Coding of responses: 1 = strongly disagree, 2 = disagree, 3 = neither agree nor disagree, 4 = agree, 5 = strongly agree.
(1) You usually go out of your way to avoid having to deal with problems in your life.
(2) Difficult problems make you very upset.
(3) When making decisions, you usually go with your "gut feeling" without thinking too much about the consequences of each alternative.

NOTE.—
[a] Responses to these questions are reverse coded, such that 3 = never/rarely, 2 = sometimes, 1 = a lot of the time, 0 = most/all of the time.
[b] Coded as: 1 = somewhat/quite a bit/very much, 0 = not at all/very little.

TABLE A2
CATEGORIZATION OF RELIGIOUS AFFILIATIONS

Categorized denomination	Percent[a]	Reported religious affiliation
No religion	12.18%	None
Catholic	25.79%	Catholic
Liberal Protestant	8.26%	Episcopal, Friends/Quaker, Methodist, Presbyterian, United Church of Christ, Unitarian
Moderate Protestant	16.66%	Christian Church (Disciples of Christ), Lutheran, National Baptist, other Protestant
Conservative Protestant	30.42%	Adventist, AME/AME Zion/CME, Assemblies of God, Baptist, Christian Science, Jehovah's Witness, Congregational, Holiness, Latter Day Saints (Mormon), Pentecostal
Other religion	4.73%	Baha'i, Buddhist, Eastern Orthodox, Hindu, Islam, Jewish, other religion
Missing	1.96%	Invalid responses

NOTE.—
[a] Percentage of each denomination out of 20,745 observations in the full Add Health Wave I in-home sample.

TABLE A3
SUMMARY STATISTICS

	Full sample (max. N=20,745)	Selected sample (N=12,945[a])	p-value for test of equality in
	mean (s.d.)	mean (s.d.)	mean s.d.
Depression			
Depression	11.390 (7.617)	11.099 (7.433)	0.001 0.002
Depressed	0.249 (0.433)	0.236 (0.424)	0.005 0.017
Religiosity			
Religiosity	8.493[b] (3.332)	8.578 (3.296)	0.026 0.186
Peer religiosity			
Peer religiosity	8.527[c] (2.299)	8.568 (2.235)	0.140 0.001
Individual characteristics			
Female	0.505 (0.500)	0.515 (0.500)	0.083 0.963
White	0.504 (0.500)	0.527 (0.499)	0.000 0.857
Black	0.209 (0.407)	0.218 (0.413)	0.060 0.061
Hispanic	0.170 (0.375)	0.173 (0.378)	0.397 0.295
Other ethnicity	0.117 (0.321)	0.082 (0.274)	0.000 0.000
Catholic	0.263 (0.440)	0.330 (0.470)	0.000 0.000
Liberal Protestant	0.084 (0.278)	0.087 (0.282)	0.331 0.041
Moderate Protestant	0.170 (0.376)	0.194 (0.395)	0.000 0.000

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	Full sample (max. N=20,745)	Selected sample (N=12,945[a])	p-value for test of equality in
	mean (s.d.)	mean (s.d.)	mean s.d.
Conservative Protestant	0.310 (0.463)	0.389 (0.488)	0.000 0.000
Age	16.162 (1.719)	16.173 (1.677)	0.550 0.002
School year in session	0.363 (0.481)	0.370 (0.483)	0.185 0.601
Puberty (male)	5.487 (6.029)	5.502 (6.038)	0.820 0.849
Puberty (female)	7.032 (7.581)	7.324 (7.592)	0.001 0.863
Grade 7	0.135 (0.341)	0.128 (0.334)	0.067 0.005
Grade 8	0.135 (0.341)	0.128 (0.334)	0.070 0.005
Grade 9	0.179 (0.384)	0.172 (0.377)	0.081 0.036
Grade 10	0.197 (0.397)	0.204 (0.403)	0.107 0.087
Grade 11	0.189 (0.391)	0.199 (0.399)	0.026 0.014
Grade 12	0.166 (0.372)	0.170 (0.376)	0.333 0.223
Parental background			
Mother not present	0.061 (0.239)	0.052 (0.222)	0.001 0.000
Mother high school or some college	0.553 (0.497)	0.565 (0.496)	0.023 0.704
Mother degree and above	0.224 (0.417)	0.225 (0.418)	0.739 0.753
Father not present	0.303 (0.460)	0.285 (0.451)	0.000 0.024

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	Full sample (max. N=20,745)	Selected sample (N=12,945[a])	p-value for test of equality in
	mean (s.d.)	mean (s.d.)	mean s.d.
Log household income	7.692 (4.638)	7.850 (4.566)	0.002 0.048
Log household income squared	80.680 (50.219)	82.476 (49.492)	0.001 0.067
Household income missing	0.260 (0.439)	0.248 (0.432)	0.017 0.057
Stressors			
Most recent GPA	2.750 (0.772)	2.762 (0.768)	0.156 0.510
Friends/Family suicide	0.195 (0.397)	0.188 (0.391)	0.092 0.063
General health	3.877 (0.914)	3.900 (0.901)	0.024 0.077
Single parent	0.240 (0.427)	0.232 (0.422)	0.147 0.204
Participation in school activities			
School club participation	0.411 (0.492)	0.444 (0.497)	0.000 0.227
School sports participation	0.394 (0.489)	0.423 (0.494)	0.000 0.172
School activity participation	0.566 (0.496)	0.609 (0.488)	0.000 0.051
Number of friends in school (in-degree)	4.339 (3.645)	4.524 (3.743)	0.000 (0.005)
Psychological resources			
Self-esteem	16.285 (2.566)	16.367 (2.534)	0.004 0.109
Passive problem-solving	8.268 (2.200)	8.259 (2.196)	0.710 0.860

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	Full sample (max. N=20,745)	Selected sample (N=12,945[a])	p-value for test of equality in
	mean (s.d.)	mean (s.d.)	mean s.d.

NOTE.—The table reports summary statistics for the original full sample (20,745 observations including missings) and selected sample (12,945 observations, or 62.40% of the full sample). Selected sample excludes respondents with missing and no religion (14.14% of the full sample), non-Christian religion (4.73%), missing valid peer group (14.93%), and missing values in own and peer depression, religiosity, individual characteristics and parental background (3.8%). Variable definitions are available in Table A1. Column (1) reports variable means for the variable-wise non-missing sample (that is, excluding missing values for each variable). Column (2) reports the means for the selected sample. Standard deviations are in parentheses. Column (3) reports the p-values for a t-test for equality of means and an F-test for equality of standard deviations between the original and selected samples.

[a] The selected sample has 12,945 observations for the main analysis. In extended analysis, the sample sizes are smaller due to missing values in most recent GPA (12,838 non-missing), family/friends suicide (12,888), general health (12,944), single parent (10,504), number of friends in school (9,543), self-esteem (12,931), and passive problem-solving (12,900).

[b] Note that respondents reporting no religion are not asked religiosity questions, thus their religiosity is treated as missing in this calculation. If the 2,526 respondents with no religion are coded as having zero religiosity, the mean of religiosity is 7.435, and the standard deviation 4.194.

[c] Note that respondents reporting no religion are not asked religiosity questions, thus peer religiosity is treated as missing for those with no religion in this calculation. If the 2,526 respondents with no religion are coded as having zero religiosity, the mean of peer religiosity is 7.572, and the standard deviation is 3.453.

SOURCE.—Add Health Wave I.

TABLE A4
DECOMPOSITION OF VARIATION IN PEER RELIGIOSITY

Grouping	Standard deviation		
	Within	Between	Total
School	1.841	1.488	2.235
Grade	2.212	0.380	2.235
Race	2.128	0.811	2.235
Gender	2.212	0.453	2.235
Denomination	2.090	0.785	2.235
School-grade	1.756	1.694	2.235
School-race	1.749	1.816	2.235
School-gender	1.780	1.554	2.235
School-denomination	1.672	1.889	2.235
School-race-denomination	1.566	2.055	2.235

NOTE.—This table reports the within-group, between-group, and total standard deviation of peer religiosity at various group levels. Peer religiosity is calculated as the average religiosity of peers who are in the same school and grade, of the same race, gender, and denomination.

TABLE A5
BASELINE RESULTS FOR THE EFFECT OF RELIGIOSITY ON DEPRESSION: FULL RESULTS

	Dependent variable = depression			Dependent variable = depressed		
	(1) OLS	(2) IV	(3) First stage	(4) OLS	(5) IV	(6) IV Probit
Religiosity	−0.163***	−0.698**		−0.006***	−0.034**	−0.034**
	(0.024)	(0.289)		(0.001)	(0.016)	(0.016)
Peer religiosity			0.112***			
			(0.020)			
Black	0.526	0.918**	0.660***	0.025	0.045*	0.048*
	(0.372)	(0.455)	(0.120)	(0.021)	(0.025)	(0.025)
Hispanic	1.165***	1.515***	0.600***	0.035*	0.053**	0.053**
	(0.287)	(0.365)	(0.133)	(0.020)	(0.023)	(0.022)
Other ethnicity	2.240***	2.766***	0.864***	0.100***	0.128***	0.124***
	(0.393)	(0.561)	(0.212)	(0.022)	(0.031)	(0.028)
Liberal Protestant	−0.616*	−0.466	0.242	−0.049***	−0.041**	−0.046**
	(0.325)	(0.342)	(0.195)	(0.017)	(0.018)	(0.022)
Moderate Protestant	0.074	0.436	0.604***	−0.010	0.009	0.013
	(0.253)	(0.303)	(0.116)	(0.013)	(0.017)	(0.018)
Conservative Protestant	0.155	0.757*	1.006***	−0.015	0.016	0.020
	(0.251)	(0.392)	(0.134)	(0.015)	(0.023)	(0.025)
Female	0.826	1.132**	0.505**	0.053	0.069*	0.068**
	(0.511)	(0.558)	(0.208)	(0.033)	(0.036)	(0.034)
Age	1.405***	1.276***	−0.235***	0.073***	0.066***	0.063***
	(0.105)	(0.135)	(0.048)	(0.007)	(0.008)	(0.008)
School year in session	1.092***	1.146***	0.100	0.052***	0.055***	0.055***
	(0.149)	(0.162)	(0.064)	(0.008)	(0.008)	(0.008)
Puberty (male)	−0.108***	−0.119***	−0.022	−0.006***	−0.007***	−0.008***
	(0.032)	(0.034)	(0.014)	(0.002)	(0.002)	(0.002)
Puberty (female)	0.015	0.008	−0.014	0.000	−0.001	−0.001
	(0.031)	(0.032)	(0.010)	(0.002)	(0.002)	(0.002)
Mother not present	−0.181	−0.302	−0.206	−0.001	−0.007	−0.005
	(0.339)	(0.347)	(0.136)	(0.019)	(0.018)	(0.016)
Mother high school	−1.100***	−1.035***	0.124	−0.051***	−0.048***	−0.042***

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	Dependent variable = depression			Dependent variable = depressed		
	(1) OLS	(2) IV	(3) First stage	(4) OLS	(5) IV	(6) IV Probit
or some college	(0.280)	(0.251)	(0.119)	(0.012)	(0.012)	(0.012)
Mother degree and above	−1.646*** (0.351)	−1.266*** (0.390)	0.718*** (0.157)	−0.072*** (0.017)	−0.053** (0.020)	−0.051** (0.022)
Father not present	0.591*** (0.163)	0.292 (0.228)	−0.555*** (0.069)	0.030*** (0.010)	0.014 (0.013)	0.013 (0.014)
Log household income	1.194 (1.500)	1.367 (1.451)	0.388 (0.662)	0.044 (0.078)	0.053 (0.081)	0.075 (0.082)
Log household income squared	−0.079 (0.073)	−0.087 (0.071)	−0.019 (0.033)	−0.003 (0.004)	−0.003 (0.004)	−0.005 (0.004)
Household income missing	3.843 (7.722)	4.812 (7.445)	2.123 (3.367)	0.144 (0.402)	0.194 (0.413)	0.296 (0.413)
Grade 8	−1.113*** (0.273)	−1.179*** (0.258)	−0.089 (0.104)	−0.049*** (0.015)	−0.052*** (0.014)	−0.044*** (0.016)
Grade 9	−2.058*** (0.443)	−2.060*** (0.420)	0.044 (0.163)	−0.107*** (0.024)	−0.107*** (0.023)	−0.093*** (0.025)
Grade 10	−3.092*** (0.521)	−3.070*** (0.503)	0.110 (0.177)	−0.161*** (0.029)	−0.160*** (0.029)	−0.141*** (0.029)
Grade 11	−4.522*** (0.601)	−4.432*** (0.597)	0.242 (0.213)	−0.226*** (0.034)	−0.221*** (0.034)	−0.197*** (0.033)
Grade 12	−6.310*** (0.705)	−6.198*** (0.696)	0.299 (0.256)	−0.329*** (0.039)	−0.323*** (0.038)	−0.295*** (0.039)
School FE	Yes	Yes	Yes	Yes	Yes	Yes
F-statistic			30.438			

NOTE.—This table reports the OLS and IV estimates of religiosity on CES-D scale of depression and the probability of being depressed. The instrumental variable peer religiosity is calculated as the average religiosity of peers (excluding oneself) of the same school, grade, race, gender, and denomination. Columns (1)–(5) report the coefficients, whereas column (6) reports the marginal effects at the mean. Clustered standard errors at the school level are in parentheses. ***, **, and * denote statistical significance at 0.01, 0.05, and 0.10 levels respectively. *F*-statistic on the excluded instrument refers to the Wald version of the Kleibergen-Paap (2006) *rk*-statistic on the excluded instrumental variables for non-i.i.d. errors (Baum, Schaffer, and Stillman, 2002). The number of observations is 12,945 in all models (32 observations are not used for identification in model (6) due to perfect prediction of school fix effects).

TABLE A6
BALANCING TEST

	(1) Mother not present	(2) Father not present	(3) Mother no high school	(4) Mother high school	(5) Mother degree and above	(6) Log household income	(7) School year in session	(8) Puberty (male)	(9) Puberty (female)
Peer religiosity	−0.002**	−0.003	0.001	0.002	0.000	−0.011	0.001	0.025	0.010
	(0.001)	(0.002)	(0.002)	(0.003)	(0.002)	(0.023)	(0.002)	(0.021)	(0.021)
Female	−0.006	0.038***	0.010	0.001	−0.005	−0.206**	−0.036***		
	(0.005)	(0.008)	(0.007)	(0.010)	(0.009)	(0.103)	(0.012)		
Black	−0.007	0.219***	−0.003	0.013	−0.003	−0.785***	0.068***	−1.311***	−0.375**
	(0.008)	(0.016)	(0.020)	(0.023)	(0.021)	(0.182)	(0.021)	(0.132)	(0.180)
Hispanic	−0.007	0.038**	0.352***	−0.180***	−0.165***	−1.544***	0.041*	−0.353**	−0.057
	(0.007)	(0.019)	(0.045)	(0.031)	(0.025)	(0.221)	(0.021)	(0.162)	(0.169)
Other ethnicity	−0.001	0.002	0.030	−0.143***	0.114**	−1.556***	0.003	−1.057***	−0.432**
	(0.012)	(0.018)	(0.045)	(0.026)	(0.049)	(0.279)	(0.027)	(0.169)	(0.173)
Liberal Protestant	−0.018*	0.001	−0.031**	−0.037	0.086***	0.377**	0.035	0.367***	0.060
	(0.009)	(0.015)	(0.013)	(0.027)	(0.025)	(0.178)	(0.023)	(0.131)	(0.166)
Moderate Protestant	0.004	0.037***	−0.013	0.019	−0.009	−0.080	−0.005	0.201*	0.297**
	(0.006)	(0.010)	(0.016)	(0.023)	(0.015)	(0.148)	(0.013)	(0.107)	(0.148)
Conservative	0.001	0.055***	0.022	0.013	−0.036***	−0.148	0.021	0.185	0.260*
Protestant	(0.007)	(0.013)	(0.016)	(0.019)	(0.012)	(0.129)	(0.015)	(0.112)	(0.147)
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Grade dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	12,945	12,945	12,945	12,945	12,945	12,945	12,945	6,279	6,666

NOTE.—Clustered standard errors at the school level are in parentheses. ***, **, and * denote significance at 0.01, 0.05, and 0.10 levels respectively.

TABLE A7
SENSITIVITY ANALYSIS WITH DIFFERENT CUTOFFS ON THE CES-D SCALE FOR HIGH DEPRESSIVE SYMPTOMS

	Cutoffs on the CES-D scale									
	(1) ≥ 15	(2) ≥ 16	(3) ≥ 17	(4) ≥ 18	(5) ≥ 19	(6) ≥ 20	(7) ≥ 21	(8) ≥ 22	(9) ≥ 23	(10) ≥ 24
	Panel A: 2SLS (linear probability model)									
Religiosity	−0.024	−0.034**	−0.041**	−0.051***	−0.045***	−0.040***	−0.039***	−0.030**	−0.027**	−0.027**
	(0.016)	(0.016)	(0.016)	(0.016)	(0.015)	(0.014)	(0.013)	(0.012)	(0.011)	(0.011)
F-statistic	30.438	30.438	30.438	30.438	30.438	30.438	30.438	30.438	30.438	30.438
% above cutoff	27.2	23.6	20.7	17.8	15.2	13.0	11.0	9.4	8.0	6.7
Observations	12,945	12,945	12,945	12,945	12,945	12,945	12,945	12,945	12,945	12,945
	Panel B: IV probit									
Religiosity	−0.024	−0.034**	−0.041**	−0.053***	−0.048***	−0.046***	−0.046***	−0.035**	−0.035**	−0.037*
	(0.017)	(0.016)	(0.017)	(0.016)	(0.016)	(0.017)	(0.017)	(0.016)	(0.017)	(0.019)
% above cutoff	27.3	23.6	20.7	17.9	15.3	13.1	11.1	9.4	8.2	6.9
Observations	12913	12913	12915	12872	12872	12831	12831	12831	12653	12517

NOTE.—This tables reports the IV estimates for the effect of religiosity on a series of binary variables indicating high depressive symptom using different cutoffs on the CES-D scale. The instrument is peer religiosity, where the peer group is defined as other students of the same grade, race, gender and denomination in the same school. Linear probability models (LPM) report the coefficients, whereas probit models report the marginal effects evaluated at the means. All models control for covariates and school fixed effects as in Table A5. School fixed effects in probit models are controlled for by including school dummies in the estimation. Clustered standard errors at the school level are in parentheses. *F*-statistic on the excluded instrument refers to the Wald version of the Kleibergen-Paap (2006) *rk*-statistic on the excluded instrumental variables for non-i.i.d. errors. ***, **, and * denote statistical significance at 0.01, 0.05, and 0.10 levels respectively.

TABLE A8
ROBUSTNESS CHECKS USING STANDARDIZED MEASURE OF DEPRESSION AND RELIGIOSITY

	Standardized depression		Principal component score (PCS) of depression	
	(1) Sum over all 19 questions[a]	(2) Remove 3 social questions[b]	(3) Based on all 19 questions[a]	(4) Remove 3 social questions[b]
Standardized religiosity	−0.305**	−0.307**		
	(0.127)	(0.128)		
PCS religiosity			−0.269**	−0.267**
			(0.129)	(0.129)
F-statistic	30.438	30.438	28.433	28.433
Observations	12,945	12,945	12,945	12,945

NOTE.—Columns (1)–(2) use standardized religiosity and depression measures. Standardized religiosity is instrumented for with its peer average of the same school, grade, race, gender, and denomination. Columns (3)–(4) use standardized predicted principal component scores (PCS) of religiosity and depression based on polychoric correlations. PCS religiosity is instrumented for with its peer average of the same school, grade, race, gender, and denomination. All models control for covariates as in Table A5. Clustered standard errors at the school level are in parentheses. *F*-statistic on the excluded instrument refers to the Wald version of the Kleibergen and Paap (2006) *rk*-statistic on the excluded instrumental variables for non-i.i.d. errors. ***, **, and * denote statistical significance at 0.01, 0.05, and 0.10 levels respectively.

[a] These questions refer to all 19 depression questions listed in Table A1.

[b] These questions refer to the depression questions listed in Table A1 excluding (4), (14), and (17).

TABLE A9
ADDITIONAL ROBUSTNESS CHECKS

	(1) Controlling for school-denomination average religiosity	(2) Including other religion	(3) Including those with missing peer religiosity	(4) Including non-religious
Religiosity	−0.859**	−0.837**	−0.608*	−0.557*
	(0.401)	(0.427)	(0.324)	(0.293)
Average religiosity of school-denomination peers	0.173	0.230	0.199*	0.213
	(0.172)	(0.177)	(0.117)	(0.133)
Peer religiosity missing			0.255	0.278
			(0.189)	(0.184)
F-statistic	16.721	19.692	14.579	18.372
J-test			0.841	0.834
N	12,945	13,475	15,939	18,104

NOTE.— This table reports the IV estimates of the effect of religiosity on depression with additional controls or larger samples. All models control for covariates as in Table A5, and school-denomination average religiosity (excluding the respondent). Column (1) controls for school-denomination average religiosity on top of the baseline model. Column (2) then includes individuals who report other affiliated religions. A dummy variable indicating other religion is also included as a covariate henceforth. Column (3) further includes those who do not have a valid school-grade-race-gender-denomination peer group, by replacing their peer religiosity with school-grade-gender-denomination average religiosity (excluding the respondent) first, and if still missing then with school-grade-race-gender average religiosity (excluding the respondent). The instruments in this model are the redefined peer religiosity, and its interaction with a dummy indicating missing peer religiosity (by original definition). The dummy variable is also included as a control variable. Column (4) further includes those who are not religious, by treating them as having zero religiosity. Peer religiosity for these individuals are redefined as school-grade-race-gender average religiosity (excluding the respondent). The instruments in this model are the redefined peer religiosity, and its interaction with a dummy variable indicating missing peer religiosity (by original definition). This dummy variable and another indicating no religion are also included as covariates. Clustered standard errors at the school level are in parentheses. ***, **, and * denote statistical significance at 0.01, 0.05, and 0.10 levels respectively. *F*-statistic refers to the Wald version of the Kleibergen-Paap (2006) *rk*-statistic on the excluded instrumental variables for non-i.i.d. errors. *J*-test reports the *p*-values of Hansen's *J*-test on overidentifying restrictions.

TABLE A10
ROBUSTNESS CHECKS ON SAMPLING ISSUES

	(1) Adjust for % observed in in-home survey		(2) Adjust for big schools		(3) Adjust for number of peer groups		(4) Adjust for size of peer group		(5) Adjust for share of peers in school-grade	
	Second stage	First stage	Second stage	First stage	Second stage	First stage	Second stage	First stage	Second stage	First stage
Religiosity	−0.751*		−0.541***		−0.709***		−0.611**		−0.694**	
	(0.443)		(0.205)		(0.241)		(0.287)		(0.288)	
Peer religiosity				0.026		0.002		0.095***		0.119***
				(0.030)		(0.050)		(0.024)		(0.028)
Interaction term[a]		0.214***		0.167***		0.002***		0.004		−0.074
		(0.058)		(0.036)		(0.001)		(0.002)		(0.210)
Share of observed peers	−0.365	−1.905***								
	(0.891)	(0.589)								
Peer group size							0.000	−0.035*		
							(0.008)	(0.021)		
Share of peers in school-grade									−1.260	1.062
									(0.954)	(2.203)
F-statistic	13.459		39.751		25.027		18.286		15.471	
J-test			0.401		0.928		0.364		0.828	
Observations	11,141	11,141	12,945	12,945	12,945	12,945	12,945	12,945	12,945	12,945

NOTE.—Column (1) weights religiosity with the proportion of peers who are of the same school, grade, race, and gender that are observed in the in-home survey out of those observed in the in-school survey. Column (2)–(5) instruments for religiosity with peer religiosity and an interaction term with each variable indicated in the column heading. All models include controls as in Table A5. Columns (1)–(5) each respectively further controls for the variable indicated in the column heading. Note that for columns (2) and (3) the variables big school and number of peer groups are absorbed in the school fixed effects. Column (1) share of observed peers refers to the proportion of students who are of the same school, grade, race, and gender that are observed in the in-home survey out of those with the same characteristics observed in the in-school survey. Denomination is not taken into account in the calculation as it is not available in the in-school survey. Column (2) big schools refers to those with more than 1,000 students. Column (3) number of peer groups refers to the number of unique groups of the same grade, race, gender, and denomination within the school. Column (4) size of peer group refers to the number of student in each peer group. Column (5) share of peers in school-grade refers to the proportion of peers out of all students within the same school and grade. Clustered standard errors at the school level are in parentheses. ***, **, and * denote statistical significance at 0.01, 0.05, and 0.10 levels respectively. *F*-statistic refers to the Wald version of the Kleibergen and Paap (2006) *rk*-statistic on the excluded instrumental variables for non-i.i.d. errors. *J*-test reports the *p*-values of Hansen's *J*-test on overidentifying restrictions.

[a] This refers to an interaction term between peer religiosity and the variable indicated under each column heading.

TABLE A11
RELIGIOSITY, PSYCHOLOGICAL RESOURCES AND DEPRESSION

	Dependent variable = psychological resources				Dependent variable = depression		
	Self-esteem	Self-esteem	Passive problem-solving	Passive problem-solving	Self-esteem	Passive problem-solving	Both
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) IV	(6) IV	(7) IV
Religiosity	0.075***	0.153	0.022***	0.113	−0.508*	−0.571**	−0.406
	(0.008)	(0.105)	(0.007)	(0.102)	(0.270)	(0.275)	(0.257)
Self-esteem					−1.234***		−1.228***
					(0.041)		(0.038)
Passive problem-solving						−0.725***	−0.689***
						(0.032)	(0.031)
F-statistic		30.399		31.916	30.117	31.644	31.331
Wald test[a]					0.184	0.114	0.042
Observations	12,931	12,931	12,900	12,900	12,931	12,900	12,889

NOTE.—Columns (1)–(4) report the IV estimates for the effect of religiosity on psychological resources. Columns (5)–(7) report the IV estimates for the effect of religiosity on depression conditional on psychological resources. All models control for covariates as in Table A5. Detailed definition for self-esteem and passive problem-solving can be found in Table A1. Clustered standard levels at the school level are in parentheses. ***, **, and * denote statistical significance at 0.01, 0.05, and 0.10 levels respectively. *F*-statistic refers to the Wald version of the Kleibergen and Paap (2006) *rk*-statistic on the excluded instrumental variables for non-i.i.d. errors.

[a] This row reports the *p*-value of a Wald test of equality of coefficients on religiosity between two models with and without controlling for psychological resources. Covariance matrix of the two coefficients is estimated from 1,000 replications of bootstrapping clustered at the school level.