Adolescent Depression and Adult Earnings: The Roles of Direct and Indirect Effects [*]

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Job Market Paper

December 2021

Abstract

Youth with depression earn less as adults, but we understand little about the pathways of effects that lead to this gap. I use data from a nationally representative survey of youth to estimate the direct and indirect effects of adolescent depression on adult earnings with a mediation analysis framework. I use several identification strategies to address issues of omitted variables bias. I find that more than half of the total effect of adolescent depression on earnings is mediated by educational attainment, while the rest of the effect is mediated by adult depression. The direct effect of adolescent depression on earnings is not robust to identification strategy. Results highlight the large economic benefits that could still be achieved by better treating adolescent depression or targeting gaps in mediating variables.

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[*] I thank Jeremy Bray, Martijn van Hasselt, Dora Gicheva, Kelsi Hobbs, and Martin Andersen for their helpful comments.
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1. Introduction

Youth with depression earn less as adults (J. Fletcher, 2013; Johar & Truong, 2014; Smith & Smith, 2010), but we understand little about the pathways of effects that lead to this gap. There is seldom reason to think that adolescent depression has a direct effect on earnings – several decades separate adolescent health conditions from adult labor market outcomes. Instead, adolescent depression may have impacts on intermediate outcomes that will ultimately affect earnings, such as educational attainment or adult depression (J. M. Fletcher, 2008; J. D. McLeod & Kaiser, 2004; Kessler et al., 2007; G. F. H. McLeod et al., 2016). However, estimates of the direct effect could be non-zero if there is omitted variables bias, measurement error, or misspecification in the earnings equation. For example, if influential family characteristics relate to both adolescent depression and earnings, then the direct effect may be biased away from zero. If education is mismeasured, then the direct effect could include part of the effect mediated by education. The potential endogeneity of adult depression and education makes the identification of mediated effects difficult as well. Earnings could impact the probability of depression in adulthood, and educational attainment is commonly endogenous in an earnings equation due to its relation to unobserved ability. Identification strategies that do not address these sources of endogeneity could result in biased estimates of effects.

Most previous literature estimates an earnings equation and focuses on the coefficient on adolescent depression, which varies significantly in its magnitude and interpretation depending on specification. Effects mediated by intermediate outcomes are only estimated in two studies. One study estimates a mediated effect through years of education but omits adult depression from the wage equation (Johar & Truong, 2014). Another study finds that adult depression mediates half of the total effect, yet excludes educational attainment (Philipson et al., 2020).

Omitting a mediator confounds the coefficient on adolescent depression with the omitted mediated effect and overestimates the direct effect. Between 2010 and 2020, the percentage of adolescents in the U.S. with a past-year major depressive episode more than doubled from 8% to 17% (SAMHSA, 2020), implying that an increasing number of youth will experience the long-term repercussions of depression. To mitigate the labor market consequences of adolescent depression, we must improve our understanding of the underlying pathways of effects.

This paper uses data from a longitudinal survey of youth in the United States and a mediation analysis framework to study why youth with depression earn less later in life. I treat adult depression and educational attainment as mediating variables due to their impact on earnings and responsiveness to adolescent depression. First, I use ordinary least squares (OLS) and a school fixed effect to estimate the direct effect of adolescent depression, which is the effect that is 'left-over' after conditioning on mediating and confounding variables. I then estimate specifications without one or both mediators to calculate indirect effects and contextualize the previous literature.

Second, I estimate a system of equations with maximum likelihood and use instrumental variables to address issues of omitted variables bias. Within-school variation in peer religiosity is used to identify the effect of adolescent depression on adult depression and earnings. Measures of tract-level educational attainment and traumatic events in the past year provide identifying variation in educational attainment and adult depression, respectively. I use parameter estimates to bootstrap direct, indirect, and total effects.

I find that youth with depression earn about 5% less than youth without depression. This effect is driven by two important mediated effects – adult depression mediates an average drop in earnings of more than 2%, while years of education mediates an average drop of nearly 3%.

These mediated effects are robust to identification strategy. In contrast, when instruments are used for identification, the direct effect is cut in half and becomes statistically insignificant. This supports the intuition that adolescent conditions do not have direct impacts on adult outcomes – instead, indirect pathways are most substantial.

This paper contributes to the literature in two ways. First, it addresses omitted variables bias in the literature using a mediation analysis framework. Defining the direct and indirect effects, as well as including important confounding variables, clarifies conflicting results in previous literature and makes results more readily interpretable. Estimating mediated effects through both years of education and adult depression at once is essential to understanding the total effect of adolescent depression on earnings. Second, this is the first paper to use instrumental variables to estimate the several effects of adolescent depression on earnings, which results in new and improved estimates of effects. Using creative identification strategies to address bias is an important step towards better understanding pathways of effects.

2. Background

Depression (i.e., major depressive disorder) is a mood disorder with symptoms including feelings of worthlessness, irritability, depressed mood, anxiety, poor concentration, and decreased interest in regular activities (Tylee, 2005). Depression is one of the most common mental disorders in the United States and is becoming more prevalent among adolescents. Between 2004 and 2019, the proportion of youth aged 12-17 in the U.S. with a past-year major depressive episode increased by 75%, while U.S. adults aged 26-49 only saw a modest 17% increase (Substance Abuse and Mental Health Services Administration, 2020).

Depression in adolescence has lasting consequences on health, education, and quality of life. Symptoms of depression can adversely affect cognition, hinder participation in school, and lower the overall quality of learning in school (Roeser et al., 1998). Adolescent depression leads to a lower grade-point average and lower rates of high school graduation, college enrollment, and college graduation (Berndt, 2000; Eisenberg et al., 2009; J. M. Fletcher, 2008; J. D. McLeod & Kaiser, 2004). Symptoms can lead to isolation, resulting in worsened relationships, poor health behaviors, and a perpetuation of depressive symptoms. Youth with depression are more likely to have a comorbid mental disorder or substance use disorders (Substance Abuse and Mental Health Services Administration, 2020), which can have lasting impacts on physical and mental health (*Adolescent Substance Use*, 2011). Youth who experience depression are up to two to three times more likely to experience symptoms in adulthood, all else constant (Fergusson et al., 2007; G. F. H. McLeod et al., 2016; Pine et al., 1999).

Altogether, these consequences of adolescent depression can have long-lasting effects on labor market outcomes. Research suggests that adolescents with depression earn less in adulthood. Using a nationally representative sample of young adults in New Zealand, Fergusson et al. (2007) finds that earnings decrease with the number of depressive episodes in adolescence. Depressive episodes in adolescence are strongly predictive of the likelihood of depression, anxiety, suicidal ideation, and employment in adulthood. Once confounding variables and cooccurring disorders are held constant, depressive episodes do not significantly predict earnings. The authors suggest that adolescent depression may affect earnings indirectly via its effect on adult psychological disorders.

Smith & Smith (2010) uses the Panel Study of Income Dynamics and recall data on whether respondents had depression in adolescence. They estimate an earnings equation where measures of adult depression and education are excluded. Controlling for family background and comorbid conditions, they find adolescent depression is associated with average drops in yearly earnings of about four to five thousand dollars. To address the potential bias of unobserved family-level heterogeneity, they estimate regressions with a family fixed effect, which cuts this effect in half and leaves it statistically insignificant. While the effect of adolescent depression on earnings is sensitive to specification, its effect on weeks worked is statistically significant and robust to family fixed effects.

Fletcher (2013) finds that adolescent depression has a large direct effect on earnings. Using data from the Add Health to estimate an earnings equation with OLS and a family fixed effect, a within-family difference in adolescent depression leads to a statistically significant 21% average decrease in yearly earnings. This is a reduced form estimate that does not include measures of adult depression or delinquency – once adult depression is added to the model, the effect decreases to about -16% and becomes statistically insignificant. In agreement with previous literature, adolescent depression appears to explain earnings in part through its relationship with adult depression, although a large portion of its effect remains direct. However, the robustness of these effects to family-level heterogeneity is still unclear. Neither Smith & Smith (2010) or Fletcher (2013) compare family fixed effects estimates to OLS models run on the identifying sample (i.e., youth with within-family variation in adolescent depression), leaving the possibility that results are being driven in part by sample selection (Miller et al., 2019).

Johar & Truong (2014) were the first to estimate how adolescent depression affects a labor market outcome through mediating variables. They posit that adolescent depression affects hourly wages through educational attainment, years of experience, and occupation. Using the National Longitudinal Study of Youth 97, they find that 30% and 66% of the total effect of adolescent depression on wages (for males and females, respectively) flows through these indirect channels. They do not control for adult depression, so estimates of the direct effect are confounded with any effect mediated by adult depression. Their estimates of the total effect range from a 10-15% drop, with about half of it being called direct. Although the measure of depression used is not well-tested, the authors highlight the significant mediating role of human capital accumulation, which was hypothesized as early as Mullahy & Sindelar (1993).

Philipson et al. (2020) is the only study to estimate the mediated effect of adolescent depression on earnings through adult depression. They use the Uppsala Longitudinal Adolescent Depression study to track how different types of depressive disorders relate to earnings later in life. In contrast to the rest of the literature, they use assessments done by healthcare professionals to measure depression, which are more reliable than the self-reported measures generally used. They find that youth with persistent depressive disorder earn about 17% less than youth without a depressive disorder. About half of this relationship is mediated by depression in early adulthood, a mediated effect that is especially prevalent for women. However, the study does not adjust for educational attainment, leaving open the possibility that the remaining direct effect could be mediated by education or other measures of human capital.

The literature varies notably in the types of effects that it estimates and the implications of its results. While it is generally agreed that adult mental health and human capital accumulation may act as mediators, estimated effects vary in their interpretation. Only two papers estimate mediated effects and they each omit a potentially important mediator. There is ample evidence that adult depression and educational attainment are both influenced by adolescent depression (Fergusson et al., 2007; J. M. Fletcher, 2008; Kessler et al., 2007; J. D. McLeod & Kaiser, 2004) and that they impact several labor market outcomes, making them ideal candidates for mediating variables. Although there is little reason to think that adolescent depression has a direct effect on earnings, several papers conclude this to varying degrees.

Approaching this topic with a mediation analysis framework will contextualize the previous literature's seemingly conflicting results, while also estimating important mediated effects.

While some studies have used school and family fixed effects to address unobserved heterogeneity, this may not be enough to address issues of endogeneity. Adolescent depression, adult depression, and education are all potentially endogenous in an earnings equation. Influential social characteristics relate to both adolescent depression and earnings, biasing the coefficient on adolescent depression away from zero. Earnings could impact the probability of depression in adulthood, and educational attainment is commonly endogenous in an earnings equation due to its relation to unobserved ability. While causal effects are difficult to establish in observational health studies, using instrumental variables for identification can reduce bias and improve our understanding of the pathways of effects in ways that have not yet been explored by the literature. It may also reveal the robustness of previous findings to an alternative identification strategy.

3. Data

This paper uses data from the National Longitudinal Study of Adolescent to Adult Health (Add Health). Add Health is a school-based study that tracks a nationally representative sample of youth in the United States through adulthood. An in-school questionnaire is administered to students in grades 7-12 in 1994-1995; a representative subsample participates in a follow-up in-home interview, which comprises of wave 1 of the survey (mean age of 15.59). Follow-up in-home interviews are given again in 1996 (wave 2), 2000 (wave 3), and 2008 (wave 4, mean age 28.48). Add Health collects a wide array of data from the respondents themselves, family members, peers, and school administrators. Youth can often be linked to other youth in the survey within their school, family, peer group, or community, allowing important within-group comparisons. Data at the census-block, census-tract, school, county, and state levels are also available. I use data from wave 1 for adolescent measures and wave 4 for adult measures.

Wave 1 contains 19 of the 20 items of the Center for Epidemiologic Studies Depression Scale (CES-D), a measure of depressive symptoms that is commonly used as a screening tool (Hann et al., n.d.; Radloff, 1977). The respondent is asked, “How often was each of the following things true during the past week?” and then presented with each item. Respondents choose from the responses of “never or rarely,” “sometimes,” “a lot of the time,” and “most of the time or all of the time.” Values of 0-3 are assigned to each response and are summed to calculate a “CES-D Score” with a possible range of 0-57. A higher CES-D score indicates more frequent symptoms of depression. Following the recommendation of the CES-D, I use the cutoff score of 16 and above to create a discrete measure of depression. Adult depression is measured using the abbreviated 10-item CES-D included in wave 4, where a cutoff score of 10 is used to create a discrete measure of depression (Kilburn et al., 2016; Oppong Asante & Andoh-Arthur, 2015).[1]

I measure adult earnings using the natural log of the respondent’s gross yearly earnings from the previous calendar year. If the respondent does not know their income, I use the midpoint of their reported “best guess” of their income.[2] Respondents with zero reported income are excluded from earnings analyses, which amounts to about 7% of the sample. To measure educational attainment, I use respondents’ highest level of educational attainment in wave 4 to extrapolate years of education.

[1] Table A.1 includes details on the items included in each measure of depression.
[2] If the respondent does not know their income, they are asked to give their ‘best guess’ of their gross income by picking from 1 of 12 categories. Categories are less than $5,000, $5,000-10,000, etc.

I create several measures of other important variables in adolescence, including grade, gender, race/ethnicity, log family income, health behaviors (e.g., alcohol use, marijuana use, smoking), anxiety, mothers' highest education, age-standardized picture vocabular score, and body mass index (BMI). Using wave 4 data, I create measures of marital status, school enrollment status, number of children, region, urban/rural status, and whether the respondent has been to jail. There are 15,690 respondents who participated in both the wave 1 and wave 4 interviews. To avoid significant attrition, I mean impute missing values for BMI and family income and median impute maternal education. I use a probit model to impute missing values for marital status.[3] I remove respondents who have any missing data for measures of yearly earnings, education, depression,[4] and the list of confounding variables above. In total, attrition brings the final sample size to 14,561.

4. Methods

4.1 Approach

Adolescent depression is related to a variety of adverse outcomes. Table 1 motivates my approach by presenting descriptive statistics by adolescent depression. Youth with depression earn about 20% less than those without depression. Youth with depression also receive about 0.74 fewer years of education and are more than twice as likely to have depression in adulthood, which could lead to lower earnings. Adolescent depression is associated with an increased likelihood of drinking, cigarette use, marijuana use, and involvement with the criminal justice system. Youth with depression are also disproportionately older, female, black, and Hispanic.

[3] Confounding variables in the probit model include race, ethnicity, gender, and a quadratic for age.
[4] Those with more than 4 missing items from the CES-D are marked as missing for the adolescent depression measure (Value Options Depression Screening).

Table 1 – Descriptive Statistics

	Full Sample		Adolescent Depression No	Adolescent Depression Yes	T-Test
	Mean	Std. Dev.	Mean	Mean	P-Value
Wave 4 - Adulthood					
Yearly Earnings	35,132	(44,583)	37,059	29,661	(0.00)
Years of Education	14.27	(2.20)	14.45	13.74	(0.00)
Depressed in Adulthood	0.20	(0.40)	0.15	0.34	(0.00)
Adult CES-D Score	6.07	(4.67)	5.33	8.17	(0.00)
Been to Jail or Prison	0.15	(0.35)	0.14	0.16	(0.00)
Age	28.48	(1.76)	28.37	28.78	(0.00)
Wave 1 - Adolescence					
Adolescent Depression	0.26	(0.44)			
CES-D Score	12.29	(6.67)	9.12	21.28	(0.00)
Drank Alcohol Past Year	0.47	(0.50)	0.43	0.58	(0.00)
Binged Past Year	0.26	(0.44)	0.23	0.35	(0.00)
Used Marijuana Past Month	0.14	(0.35)	0.11	0.21	(0.00)
Smokes Cigarettes	0.19	(0.39)	0.16	0.28	(0.00)
Age	15.58	(1.72)	15.48	15.88	(0.00)
Grade	9.65	(1.63)	9.58	9.84	(0.00)
Family Income ($1,000s)	44.22	(45.12)	45.36	41.01	(0.00)
Mother has High School Degree	0.86	(0.35)	0.88	0.80	(0.00)
Mother has College Degree	0.26	(0.44)	0.28	0.20	(0.00)
Demographics					
Female	0.53	(0.50)	0.50	0.63	(0.00)
White	0.60	(0.49)	0.63	0.52	(0.00)
Black	0.21	(0.40)	0.20	0.24	(0.02)
Non-White Hispanic	0.09	(0.28)	0.08	0.11	(0.00)
Other	0.11	(0.31)	0.10	0.13	(0.02)
Instrumental Variables					
Cohort Religiosity (0-13)	0.06	(0.24)	0.06	0.07	(0.04)
Friend Attempt Suicide Past Year	0.03	(0.16)	0.02	0.03	(0.26)
Family Passed Away Past Year	6.55	(3.29)	6.59	6.45	(0.00)
Adults Aged 25+ w/ Bachelor's	0.23	(0.13)	0.23	0.22	(0.00)
Youth Aged 16-19 in School	0.77	(0.11)	0.77	0.76	(0.00)
Observations	14,561		10,760	3,801	14,561

Note: Columns 1 and 2 report the means and standard deviations for the full sample. Columns 3 and 4 report means by adolescent depression status. Column 5 reports the p-value of a two-sample t-test of the equality of means in columns 3 and 4. Note that the race/ethnicity categories match up with the categories used to define cohorts for cohort religiosity.

Each of these differences could confound the relationship between adolescent depression and earnings.

Important differences at the family, school, and community levels could also conflate the earnings gap. In Table 1, youth with depression come from families with significantly lower income and lower educated mothers. Youth from families with higher incomes and highly educated parents could have the resources to prevent and treat depression effectively, as well as the social capital to better succeed in the labor market later. Schools with better teachers and more funding may positively impact both education and mental health outcomes. Countless other environmental and community characteristics may shape how youth act and flourish. These characteristics must be considered to understand the relationship between adolescent depression and earnings. However, accounting for important differences could crowd out parts of the relationship if the differences themselves are driven by adolescent depression. For example, youth with symptoms of depression may have a more difficult time focusing on school and receive less education in the long run (J. M. Fletcher, 2008), leading to lower average earnings in adulthood. Treating educational attainment as a confounding variable would waive away an important part of the relationship between adolescent depression and earnings.

I model the total effect of adolescent depression on earnings using a mediation analysis framework. I split determinants of earnings into two categories: mediators and confounders. Mediators are functions of adolescent depression that *mediate* part of the effect of adolescent depression on earnings. I treat adult depression and years of education as mediators due to the strong evidence that they are affected by adolescent depression and are determinants of earnings. I treat all other important factors as confounders – exogenous factors that are correlated with adolescent depression and earnings, obscuring their relationship.

To formalize this approach, consider the following set of recursive equations, where Y is log earnings, D_1 is adolescent depression, D_2 is adult depression, E is educational attainment, C contains confounding variables, s is a school-level effect, and ϵ is an error term.

$$Y = y(D_1, D_2, E, C_Y, s, \epsilon_Y) \tag{1}$$

$$D_2 = d_2(D_1, C_{D_2}, s, \epsilon_{D_2}) \tag{2}$$

$$E = e(D_1, C_E, s, \epsilon_E) \tag{3}$$

The *total effect* of adolescent depression on earnings can be written as the total derivative of earnings with respect to adolescent depression, $\frac{dY}{dD_1}$. If we only consider equation 1 and assume that all variables confound the relationship between adolescent depression and earnings, then the total effect is equal to the direct effect, $\frac{dY}{dD_1} = \frac{\partial Y}{\partial D_1}$. When we treat adult depression and education as mediators and consider equations 2-3, the expression becomes:

$$\frac{dY}{dD_1} = \underbrace{\frac{\partial Y}{\partial D_1}}_{\substack{Direct \\ Effect}} + \underbrace{\frac{\partial Y}{\partial D_2}\frac{\partial D_2}{\partial D_1}}_{\substack{Mediated\ by \\ Adult\ Depression}} + \underbrace{\frac{\partial Y}{\partial E}\frac{\partial E}{\partial D_1}}_{\substack{Mediated\ by \\ Education}} \tag{4}$$

The total effect is comprised of three terms: a *direct effect*, an *indirect effect* via adult depression, and an *indirect effect* via education. The direct effect is the effect of adolescent depression on earnings, holding mediators and confounding variables constant. An indirect effect is an effect on earnings due to how adolescent depression changes the value of the mediating variable.[5]

If some confounding variables are actually mediators – for example, involvement with the criminal justice system – then this approach underestimates the total effect. However,

[5] The terms *indirect effect* and *mediated effect* are used interchangeably.

addressing endogeneity in mediated effects is difficult. Increasing the number of mediators only introduces more challenges and increases the difficulty of identification. While only using two mediators could underestimate the total effect, it makes identification of those mediated effects easier. Adult depression and education have stronger relationships with adolescent depression and earnings than any other potential mediators. Estimating their mediated effects will shed light on the most economically significant effects of adolescent depression.

This approach also pays special attention to contextualizing the direct effect. There is little reason to think that adolescent depression has a direct impact on adult earnings – any estimated direct effect is likely due to omitted variables bias. If we hesitate to add confounding and mediating variables to the earnings equation, then the coefficient on adolescent depression becomes a ‘catch-all’ for omitted mediated effects, omitted variables bias, and other sources of bias, rendering it uninterpretable. Omitting potential mediators from an earnings equation also does *not* produce an upper bound on the total effect of adolescent depression. Since treatment for adolescent depression is omitted from the regression, the effect of untreated depression on earnings is likely much larger. Including all potential mediating and confounding variables on the right-hand side addresses omitted variables bias as much as possible with observed data and reveals what association is left over. Instrumental variables techniques can then be used to better understand the extent to which this effect is driven unobservables.

4.2 Empirical Model and Identification

I use two approaches to identify and estimate direct and indirect effects. First, consider the following earnings equation with a school fixed effect, s_i:

$$Y_i = \beta_0 + D_{1i}\beta_1 + D_{2i}\beta_2 + E_i\beta_3 + C_{Yi}\beta_4 + s_i + \epsilon_{Yi} \tag{5}$$

The first approach uses a single earnings equation and achieves identification using the standard OLS assumptions, namely that right-hand side variables are conditionally independent of the error term. I control for a wide range of confounding variables and proxy for unobserved heterogeneity to make this assumption more likely to hold. I use a school fixed effect (s_i) to address the biasing role of school-level heterogeneity; in the appendices, I use family and state fixed effects to address family-level and geographic-level heterogeneity that could be biasing coefficients. In specifications with fixed effects, the identifying assumption is that all right-hand side regressors are exogenous conditional on the fixed effect.

Next, consider the following system of equations, where $\{Z_{D_2}, Z_E, Z_{D_1}\}$ include instrumental variables:

$$Y_i = \beta_0 + D_{1i}\beta_1 + D_{2i}\beta_2 + E_i\beta_3 + C_{Yi}\beta_4 + s_i + \epsilon_{Yi} \tag{6}$$

$$D_{2i}^* = \gamma_0 + D_{1i}\gamma_1 + C_{D_2 i}\gamma_2 + Z_{D_2 i}\gamma_3 + s_i + \epsilon_{D_2 i} \tag{7}$$

$$E_i = \alpha_0 + D_{1i}\alpha_1 + C_{Ei}\alpha_2 + Z_{Ei}\alpha_3 + s_i + \epsilon_{Ei} \tag{8}$$

$$D_{1i}^* = \phi_0 + C_{D_1 i}\phi_1 + Z_{D_1 i}\phi_2 + s_i + \epsilon_{D_1 i} \tag{9}$$

The second approach estimates the system of equations 6-9 to obtain parameters needed for direct and indirect effects. Assume that the error terms in equations 6-9 are distributed joint normal as follows:

$$\begin{matrix} \epsilon_Y \\ \epsilon_{D_2} \\ \epsilon_E \\ \epsilon_{D_1} \end{matrix} \sim N \begin{bmatrix} 0 \\ 0 \\ 0 \\ 0 \end{bmatrix}, \begin{bmatrix} \sigma_Y^2 & \sigma_{Y,D_2} & \sigma_{Y,E} & \sigma_{Y,D_1} \\ & 1 & \sigma_{D_2,E} & \sigma_{D_2,D_1} \\ & & \sigma_E^2 & \sigma_{E,D_1} \\ & & & 1 \end{bmatrix} \tag{10}$$

I identify parameters using the conditional mean assumption and instrumental variables. First, for equations 6-9, all right-hand side variables must be conditionally independent of the error terms (e.g., $E\left(C_{D_1}\epsilon_{D_1}\right) = 0$). While the system of equations is technically identified based on the conditional mean assumption and distributional assumptions alone (Roodman, 2011; Wilde, 2000), instruments can make identification more robust. An instrumental variable is added to the right-hand side of the equation for an endogenous variable but excluded from the right-hand side of the equation for an outcome variable. Instrumental variables address endogeneity by providing variation in the endogenous variable that is plausibly random with respect to the outcome conditional on right-hand side variables. Instruments must satisfy two assumptions.

Assumption 1 (A1) – The instruments are orthogonal to all error terms. E.g., $E[Z\epsilon] = 0$, where $Z = \{Z_1, Z_2, Z_3\}$ and $\epsilon = \{\epsilon_Y, \epsilon_{D_2}, \epsilon_E, \epsilon_{D_1}\}$.

Assumption 2 (A2) – The instruments are relevant in the endogenous variable's equation (i.e., the rank condition holds). E.g., $\phi_2 \neq 0$.

Considered together, instruments produce consistent estimates if they are relevant predictors of the endogenous variable and as good as random in the outcome equation conditional on covariates.

4.3 Instrumental Variables

4.3.1 Instrumenting for Adolescent Depression

I use average cohort religiosity as an instrument for adolescent depression (Fruehwirth et al., 2019), where cohorts are defined within schools by grade, gender, and race. Religiosity is a positive variable with a range of 0-13 that is the sum of scores from four questions about the religiousness of the respondent.[6] Someone who reports no religious affiliation is not asked about their religiosity and is assigned a score of zero. All religious respondents are questioned about how important religion is to them, how often they pray, how frequently they attend church, and how frequently they attend religious events designed for youth. Average cohort religiosity is calculated by averaging up the religiosity score for all other respondents in the same school, grade, gender, and race/ethnicity category as the youth.[7] For example, a respondent who is a white male in 8th grade is assigned the average religiosity score of all other white male 8th graders at their school.

Youth are significantly more likely to be friends with those who are similar to them, known as homophily. Youth consider observable characteristics when selecting friends, which makes it more likely that they are the same age, gender, and race/ethnicity as their friends (McPherson et al., 2001; Shrum et al., 1988). Youth behavior is also substantially influenced by friends after selecting into friendships (Kandel, 2021; Prinstein, 2007). The cohorts I define make up likely candidates for friends of each respondent, implying that cohort characteristics may have significant impacts on own behaviors.

With this in mind, average cohort religiosity may be a relevant predictor of adolescent depression for three reasons. First, changes in peer religiosity motivate changes in own religious behavior (Cheadle & Schwadel, 2012). Increased religious involvement can play a protective effect against mental illness, including depression (Levin, 2010; McCullough & Larson, 1999).

[6] Youth who indicate having a religion are asked the following four questions: (1) “In the past 12 months, how often did you attend religious services?” (2) “How important is religion to you?” (3) “How often do you pray?” (4) “In the past 12 months, how often did you attend such youth (religious) activities?” The respondents choose from a Likert scale of responses (e.g., very important, fairly important, fairly unimportant, not important at all). Responses are assigned integer values and summed up, with a higher sore indicating being more religious.

[7] Race/ethnicity categories are as follows: white, black, non-white Hispanic, and other.

Second, peers who are more religious are also less likely to be depressed (Fruehwirth et al., 2018), and symptoms of depression spread between close friends (Conway et al., 2011; Giletta et al., 2012; Prinstein, 2007). This decreases the likelihood of own depression. Third, religiosity includes questions regarding attending religious events. Increased attendance to these events could represent increased social cohesion of the peers the youth are likely friends with. Increased social cohesion can lower the likelihood of depressive symptoms among youth.

Within-school variation in cohort religiosity is plausibly random in the earnings and adult depression equations after conditioning on the variables used to make the cohorts (i.e., grade, gender, race) and potential mediating variables (e.g., adult religiosity, educational attainment). Identifying variation comes from variation in depressive symptoms across cohorts within schools, satisfying A1. However, variation in cohort religiosity could be associated with unobserved peer characteristics that affect educational attainment, violating A1 when it is used to identify the effect of adolescent depression on years of education. For this reason, I only use cohort religiosity as an instrument to identify the effect of adolescent depression on earnings and adult depression. I do not address the potential endogeneity of adolescent depression in the years of education equation, because estimates using an invalid instrument could be more biased than OLS estimates.

4.3.2 Instrumenting for Adult Depression

I use two measures of potentially traumatic events in the past 12 months as instruments for adult depression: whether a family member or close friend has attempted suicide, and whether a parent or sibling has passed away. Traumatic and stressful life events are an exogenous shock to one’s mental health and can increase symptoms of depression (Ettner et al., 1997; Hammen, 2005). Therefore, these events may increase the respondent's symptoms of depression, making A2 likely to hold.

To ensure that A1 holds, the within-school variation in traumatic events must be conditionally random with respect to earnings. These events could impact the respondent's labor supply, ultimately affecting earnings and violating A1. To address this, I control for two measures of how family influences work. First, I control for the extent to which the respondent reports cutting back on hours due to family responsibilities in the past 12 months. Second, I control for how strongly the respondent agrees/disagrees that family responsibilities interfered with their ability to work in the past 12 months. Conditional on these measures and variables related to adverse health or mental health (e.g., family income, race/ethnicity), variation in traumatic events is plausibly random with respect to earnings. These instruments are used to identify the effect of adult depression on earnings.

4.3.3 Instrumenting for Education

I use two census tract-level variables as instruments for years of education in the earnings equation. First, I use the tract-level proportion of those aged 16-19 currently enrolled in school. The decisions youth make about their education are influenced by the paths taken by their peers and role-models. Peer networks have significant impacts on the decision to enroll and stay enrolled in school, and students are more likely to conform to their peers' decisions about college applications (Bobonis & Finan, 2009; Rosenqvist, 2018). As a result, local school enrollment is a relevant predictor of own education and A2 holds. Second, I use the tract-level proportion of those aged 25+ who have a bachelor's degree. Evidence suggests that the educational attainment of youth is influenced by the proportion of neighbors with high educational attainment or high-status jobs (Ainsworth, 2002; Ginther et al., 2000). Neighborhood characteristics affect the types of role models that youth are shaped by, ultimately affecting their own educational attainment and making A2 likely to hold.

To satisfy A1, within-school variation in tract-level characteristics must be conditionally random with respect to earnings. It is possible that families select into neighborhoods within school districts based on their own earnings, education, or race/ethnicity, which are all potentially related to the adolescent's educational attainment. I address these backdoor pathways by controlling for log family income, mother's education, and race/ethnicity. Neighborhood education characteristics could also be correlated with the propensity to commit crime, ultimately affecting earnings. I address this by controlling for whether the respondent has ever been to jail. Within-school variation in urban/rural status could also be driving long-term earnings differences, so I also control for Rural-Urban Commuting Area Codes. Conditional on the aforementioned variables, within-school variation in tract-level education characteristics is likely random with respect to earnings.

4.4 Estimation

In the single equation approach, I estimate equation 5 with OLS. The resulting estimate of $\hat{\beta}_1$ is the direct effect of adolescent depression on earnings. To calculate indirect effects with an earnings equation, I use the difference method (VanderWeele, 2016). First, I estimate equation 5 and save $\hat{\beta}_1$. Next, I remove a mediator (e.g., E_i) from the regression and re-estimate equation 5, saving the new coefficient on adolescent depression, $\hat{\beta}_1^*$. The difference between $\hat{\beta}_1$ and $\hat{\beta}_1^*$ is the estimated mediated effect. To estimate the total effect, I remove both E_i and D_{2i} and compare the coefficient on adolescent depression to $\hat{\beta}_1$.

In the system approach, I estimate equations 6-9 as a recursive system using limited information maximum likelihood (LIML) via the Conditional Mixed-Process Models (CMP) package in Stata (Roodman, 2011). This method allows parameters across equations to be related through the jointly distributed error structure; all variables show up on the right-hand side as observed, not as predicted values. Although I use discrete measures of adolescent and adult depression, I model their latent variables D_1^* and D_2^* and use a probit model for their parts of the likelihood function.

The likelihood function for individual i is expressed as follows, where $f(\cdot)$ is the multivariate normal probability density function (PDF), $\theta_1 - \theta_4$ are the parameters and variables on the right-hand sides of equations 6 – 9, $Q_2 = (2D_{2i} - 1)\theta_2$, and $Q_4 = (2D_{1i} - 1)\theta_4$:

$$L_i = \int_{-\infty}^{-\theta_1} \int_{-\infty}^{Q_2} \int_{-\infty}^{-\theta_3} \int_{-\infty}^{Q_4} f(\epsilon_{1i}, \epsilon_{2i}, \epsilon_{3i}, \epsilon_{4i})\, d\epsilon_{4i}\, d\epsilon_{3i}\, d\epsilon_{2i}\, d\epsilon_{1i} \tag{11}$$

When I restrict all covariance terms to zero, the PDF in equation 11 simplifies to $f(\epsilon_{1i})f(\epsilon_{2i})f(\epsilon_{3i})f(\epsilon_{4i})$ and produces estimates equivalent to estimating each equation separately with maximum likelihood. This approach assumes away potential issues of endogeneity. When I unrestrict covariance terms but do not add instruments, the system is identified only by functional form assumptions. Estimated covariance terms represent endogeneity in a recursive system. If the covariance is different than zero, this indicates that the conditional mean assumption is likely violated. For example, the consistency of β_1 relies on the assumption that $E(D_1\epsilon_y) = 0$. Expanding and substituting for D_1, we get:

$$E(D_1\epsilon_y) = E(D_1)E(\epsilon_y) + Cov(C_{D_1}\phi_1, \epsilon_y) + Cov(Z_{D_1}\phi_2, \epsilon_y) + Cov(\epsilon_{D_1}, \epsilon_y) \tag{12}$$

The first three terms converge to zero by assumption. The term $Cov(\epsilon_{D_1}, \epsilon_Y)$ is estimated by $\hat{\sigma}_{Y,D_1}$ in the system. If $\hat{\sigma}_{Y,D_1} \neq 0$, then we have evidence to reject the null hypothesis that $\hat{\beta}_1$ is consistent. Since identification based on parametric assumptions could be weak and parameters likely suffer from omitted variables bias, I use instrumental variables to better identify parameters.

When instruments are added, I estimate the covariance terms between the equations with instruments and the equations for endogenous variables, which allows for the instruments to inform relevant parameter estimates. For example, when I use cohort religiosity to instrument for adolescent depression and identify its effects on earnings (β_1) and adult depression (γ_1), I estimate σ_{Y,D_1} and σ_{D_2,D_1}. When I add instruments to the adult depression and education equations to identify their effects on earnings (β_2 and β_3, respectively), I also estimate σ_{Y,D_2} and $\sigma_{Y,E}$. Note that instruments do not address endogeneity by bringing the covariance terms to zero. Instead, instruments provide identifying variation in the endogenous variable that is conditionally uncorrelated with confounding unobservables in the error term. In fact, testing whether a covariance term is unchanged after adding instruments is a test of the validity of the instruments, similar to a Sargan test.[8]

I always restrict $\sigma_{E,D_1} = 0$ to prevent Z_{D_1} from identifying the effect of adolescent depression on years of education (α_1). This is done because Z_{D_1} may be related to the error term in the years of education equation, violating A1. While this assumes away a potentially important endogeneity issue for the effect of adolescent depression on education (α_1), it avoids an even

[8] This test is presented in appendix E. I report this test statistic in result tables as a test of instrument validity.

larger bias that could arise from using an invalid instrument. When $\sigma_{E,D_1} = 0$, we can rewrite the PDF as:

$$f(\epsilon_1, \epsilon_2, \epsilon_3, \epsilon_4) = f(\epsilon_1, \epsilon_2 | \epsilon_3, \epsilon_4) f(\epsilon_3 | \epsilon_4) f(\epsilon_4) = f(\epsilon_1, \epsilon_2 | \epsilon_3, \epsilon_4) f(\epsilon_3) f(\epsilon_4) \tag{13}$$

Since ϵ_3 is no longer conditional on ϵ_4, the estimate of α_1 (contained in ϵ_3) is no longer informed by the parameter on the instrument in the adolescent depression equation (contained in ϵ_4). This prevents the instrument from being used to identify the effect of adolescent depression on education (α_1), while still being used to identify its effect on earnings and adult depression (β_1 and γ_1, respectively).

Across all equations, I control for measures of gender, race/ethnicity, grade, adolescent health behaviors (cigarette use, marijuana use, alcohol use), adolescent anxiety, age-standardized vocabulary test score, log of family income in adolescence, mothers' highest education, adolescent BMI, and indicators for imputed values. In equations 6 – 8, I also control for whether the respondent has been to jail. In equations 6 and 7, I also control for whether the respondent is enrolled in school in wave 4, the number of children they have, rural/urban status, region, and adult religiosity.[9] The total derivative of earnings with respect to adolescent depression can be written as the following, where γ_1 is an average marginal effect:

$$\frac{dE(Y_i)}{dD_{1i}} = \underbrace{\beta_1}_{\substack{Direct \\ Effect}} + \underbrace{\beta_2 \gamma_1}_{\substack{Mediated\ by \\ Adult\ Depression}} + \underbrace{\beta_3 \alpha_1}_{\substack{Mediated\ by \\ Education}} \tag{14}$$

[9] I control for adult religiosity to shut down any backdoor pathway connecting adolescent cohort religiosity to adult earnings when it is used as an instrument.

I use a parametric bootstrap routine with 500 replications to estimate the standard errors of the direct, indirect, and total effects in equation 14.

5. Results

5.1 Single Equation

Table 2 presents coefficients for the log earnings regression estimated with OLS. [10] I convert coefficients to percentage change effects using the following equation for each coefficient: $\%\Delta y = e^{\Delta x \beta} - 1$. Column 1 regresses log earnings on adolescent depression and finds an average drop in earnings of about -22.8%, similar to the gap in means found in Table 1. Adding adult depression and years of education to the model in column 2 reduces the coefficient estimate to about -14.2%, revealing that more than a third of the association in column 1 was due to their omission. Including adolescent and adult controls in columns 3 and 4 reduces the estimate to -6.6%. If we assume that all confounding and mediating variables are on the right-hand side, then the coefficient in column 4 is an estimate of the direct effect. Although this estimate is only about one-fourth the size of the association estimated in column 1, it remains statistically significant at the 1% level and equates to about a $2,200 annual drop in earnings.

Columns 5-7 estimate the fully specified model in column 4 without one or both mediators. This allows the calculation of indirect and total effects using the difference method [11] and presents specifications similar to the previous literature. When adult depression is removed in column 5, the coefficient on adolescent depression increases to -0.087, suggesting that adult

[10] See Table A.2 in Appendix A for estimates without a school fixed effect, which are similar.
[11] The difference method is commonly in mediation analysis to estimate indirect effects. First, the direct effect is estimated. Next, the mediator of interest is removed from the specification and the model is re-estimated. The difference between the new coefficient and the direct effect is the estimated mediated effect. This method requires that (1) all confounding variables are on the right-hand side, (2) no confounding variable is a mediator, and (3) no mediator affects another mediator.

Table 2 - Earnings OLS Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Adolescent Depression	-0.259***	-0.153***	-0.082***	-0.068**	-0.087***	-0.087***	-0.111***
	(0.026)	(0.023)	(0.022)	(0.021)	(0.021)	(0.021)	(0.022)
Mediated Effect					-1.8%	-1.8%	-3.9%
Adult Depression	No	Yes	Yes	Yes	No	Yes	No
Years of Education	No	Yes	Yes	Yes	Yes	No	No
Adolescent Controls	No	No	Yes	Yes	Yes	Yes	Yes
Adult Controls	No	No	No	Yes	Yes	Yes	Yes
Fixed Effect	School	School	School	School	School	School	School
Observations	13,298	13,298	13,298	13,174	13,174	13,174	13,174

**** for p < 0.001, ** for p < 0.01, * for p < 0.05*
Note: Coefficients are reported with standard errors in parentheses. Standard errors are clustered at the school level. All columns are estimated with OLS and include a school fixed effect. Column 1 regresses log earnings on adolescent depression and column 2 adds adult depression and years of education. Column 3 adds adolescent controls, while column 4 adds adult controls. Columns 5-7 remove one or more mediators, allowing the use of the difference method to calculate indirect effects, as well as producing estimates similar to previous literature.

depression mediates an effect of about -1.9 percentage points. The estimated coefficient is slightly larger than that found in Johar & Truong (2014), which excludes adult depression from their wage equation and estimates the effect of adolescent depression to be about 9-10% for males and 4-5% for females, averaging about -7%. Column 6 removes years of education and finds a coefficient on adolescent depression of -0.087, suggesting that years of education also mediates an effect of about -1.9%. The coefficient is approximately the size of the 8-9% effect found by Philipson et al. (2020), which omits educational attainment from their earnings equation. While Johar & Truong (2014) and Philipson et al. (2020) referred to their estimates as direct effects, the estimates in columns 5 and 6 are combinations of the direct effect and the indirect effect through the omitted mediator.

When both adult depression and years of education are removed in column 7, the coefficient grows to -0.111, or about -10.5%. This is an estimate of the total effect, as all remaining right-hand side variables are treated as confounders and not mediators. While this is smaller than the coefficient of -0.23 found by Fletcher (2013) when these variables are omitted, this difference is due to the larger number of confounding variables included in this paper.[12]

While we have not yet addressed issues of endogeneity, Table 2 suggests that previous literature has overestimated the direct effect by omitting important mediators or confounding variables. However, these estimates are consistent with each other when put into the context of mediation analysis. Specifications without mediating variables produce estimates that are the sum of direct and indirect effects. These estimates (columns 5 and 6) line up with estimates of the direct effect from two studies in the literature that omit a mediator. Specifications without necessary confounding variables can produce estimates of the total effect that are biased upward. Results from using the difference method are also internally consistent, as the direct and indirect effects approximately sum up to the total effect estimate in column 7 of about -10.5%.

The column 4 coefficient of -0.068 suggests that adolescent depression has a -6.6% average direct effect on adult earnings. If there are omitted mediators, then some or all of this estimate could be capturing unobserved mediated effects. If there are unobserved confounding factors, then the direct effect may be biased away from zero. For example, there may be important characteristics at the family or environmental level that are related to adolescent depression and earnings. Similar issues of endogeneity with adult depression or years of education could also be biasing estimates of coefficients in columns 4-7, thus biasing estimates of indirect and total effects. Measurement error of a mediator is a common cause of an overstated

[12] When I replicate Table 5 of Fletcher (2013), I obtain results that are similar but slightly smaller for most columns (note: Fletcher uses Add Health but uses an additional wave of data and a smaller sample than this paper). When I add dummy variables for some college and graduate school (in addition to high school and college degree dummies that are already present), the direct effect drops to about -7-8%. Adding controls for going to jail, being married, having children, and region drop the direct effect to about -6%, consistent with my results.

direct effect (VanderWeele, 2016). In addition to controlling for relevant mediators and confounding variables, addressing potential endogeneity is important to improving estimates.

5.2 System of Equations

Table 3 presents estimation results from the full system of equations.[13] For convenience, average marginal effects are presented in lieu of coefficients for the adult depression and adolescent equations. Column 1 estimates each equation independently, which is identical to column 4 in Table 2 and provides initial evidence that both adult depression and years of education are mediators. Youth with depression are about 13.7 percentage points more likely to be depressed in adulthood, all else constant. Those with depression in adulthood make about 13.1% less than those without depression, which is expected to come through lower labor supply. This estimate is similar in magnitude to Ettner (1997) but a bit larger than some other estimates in the literature (e.g., Baldwin & Marcus, 2007; Cseh, 2008). Youth with depression also receive about one-third fewer years of education than those without depression, all else constant. The average return to a year of education is about 9.6%, which is on the high end of estimates in the literature.[14] Identical to column 4 of Table 2, column 1 estimates an average direct effect of about -6.6%, suggesting that there is a an economically significant immediate effect on earnings.

Column 2 is identical to column 1 but estimates the σ_{Y,D_1} and σ_{D_2,D_1} covariance terms. This opens up the covariance terms needed to identify β_1 and γ_1 with instruments in column 3. While the estimates of β_1 and γ_1 are identified in column 2, they are not well identified – both

[13] Full estimation results, including instrument tests, can be found in Table A.5.
[14] The literature generally finds an average wage premium of 6-8% (Harmon et al., 2003). The high return found in this paper could be driven by three factors. First, I use yearly earnings instead of hourly wages, so the return to a year of education includes effects on both wages and labor supply. Second, zero-earners are excluded from the earnings regression, plausibly excluding those who have a lower return to education, on average. Third, unobserved ability could be biasing the return to a year of education upward.

Table 3 - System of Equations Results

	(1)	(2)	(3)	(4)	(5)
Ln(earnings)					
Adolescent Depression ($\hat{\beta}_1$)	-0.068**	-0.017	-0.036	-0.033	-0.032
	(0.021)	(0.039)	(0.052)	(0.052)	(0.052)
Adult Depression ($\hat{\beta}_2$)	-0.140***	-0.143***	-0.140***	-0.155**	-0.164**
	(0.022)	(0.022)	(0.022)	(0.052)	(0.062)
Years of Education ($\hat{\beta}_3$)	0.092***	0.092***	0.092***	0.092***	0.091***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.026)
Adult Depression					
Adolescent Depression ($\hat{\gamma}_1$)	0.137***	0.080	0.128***	0.128***	0.126**
	(0.009)	(0.086)	(0.042)	(0.042)	(0.042)
Friend/Family Suicide Attempt in Past 12 Months					0.051*** (0.013)
Years of Education					
Adolescent Depression ($\hat{\alpha}_1$)	-0.320***	-0.320***	-0.320***	-0.320***	-0.315***
	(0.041)	(0.041)	(0.041)	(0.041)	(0.042)
Tract-level Proportion Age 25+ With College Degree					1.575*** (0.206)
Adolescent Depression					
Cohort Religiosity			-0.005***	-0.005***	-0.005***
			(0.001)	(0.001)	(0.001)
Covariance Terms Estimated	None	$\{\sigma_{Y,D_1}, \sigma_{D_2,D_1}\}$		$\{\sigma_{Y,D_1}, \sigma_{D_2,D_1}, \sigma_{Y,D_2}, \sigma_{Y,E}\}$	
Z-Test Statistic: σ_{Y,D_1}			-0.291		0.000
Z-Test Statistic: σ_{D_2,D_1}			0.487		0.035
Z-Test Statistic: σ_{Y,D_2}					0.107
Z-Test Statistic: $\sigma_{Y,E}$					0.002
Observations	14,561	14,561	14,561	14,561	14,561

**** for p < 0.001, ** for p < 0.01, * for p < 0.05*

Note: For the Ln(Earnings) and Years of Education equations, coefficients are reported with standard errors in parentheses. For the adult depression and adolescent depression equations, average marginal effects are reported. Standard errors are clustered at the school level. All columns are estimated with LIML and include a school fixed effect is in every equation. All columns include fully specified models. Column 1 presents results from when all covariance terms are restricted to zero and no instruments are included. Column 2 estimates σ_{Y,D_1} and σ_{D_2,D_1}, while column 3 instruments for adolescent depression. Column 4 also estimates σ_{Y,D_2} and $\sigma_{Y,E}$, while column 5 adds instruments for adult depression and years of education. The Z-Test Statistics are the test statistics for the Z-test of the difference between the covariance terms with and without an instrument added. Some instruments are excluded from specifications due to their insignificance. Full results can be found in Table A.5 and Figure A.8.

parameters drop in magnitude and are noisily estimated.

Column 3 adds cohort religiosity to the adolescent depression equation, which better identifies $\hat{\beta}_1$ and $\hat{\gamma}_1$. Cohort religiosity is a strong predictor of adolescent depression – a one standard deviation increase in cohort religiosity leads to about a 1.7% decrease in the probability of having depression in adolescence. Instrumenting for adolescent depression drops the direct effect to -0.036, or about -3.5%. This effect is statistically insignificant and about half the size of the effect in column 1. This reveals that about half of the direct effect in column 1 was driven by the correlation between the error terms in the earnings and adolescent depression equations – in other words, endogeneity. The Z-Test Statistic indicates that we fail to reject the null hypothesis that σ_{Y,D_1} is unchanged between columns 2 and 3, failing to find evidence that the instrument is invalid. Instrumenting for adolescent depression also slightly drops its average marginal effect on adult depression– youth with depression are about 12.8 percentage points more likely to suffer from depression in adulthood. In contrast to column 2, this estimate is significant at the 0.1% level. We also fail to reject the null hypothesis that σ_{D_2,D_1} is unchanged between columns 2 and 3.

Column 4 re-estimates column 3 but unrestricts σ_{Y,D_2} and $\sigma_{Y,E}$. The estimate of a return to a year of education is unchanged, while the effect of adult depression on adult earnings grows in magnitude slightly to about -14.4%. In contrast to column 2, estimates remain precisely estimated.

Column 5 adds instrumental variables to the adult depression and years of education equations.[15] Using instruments for identification grows the effect of adult depression on earnings

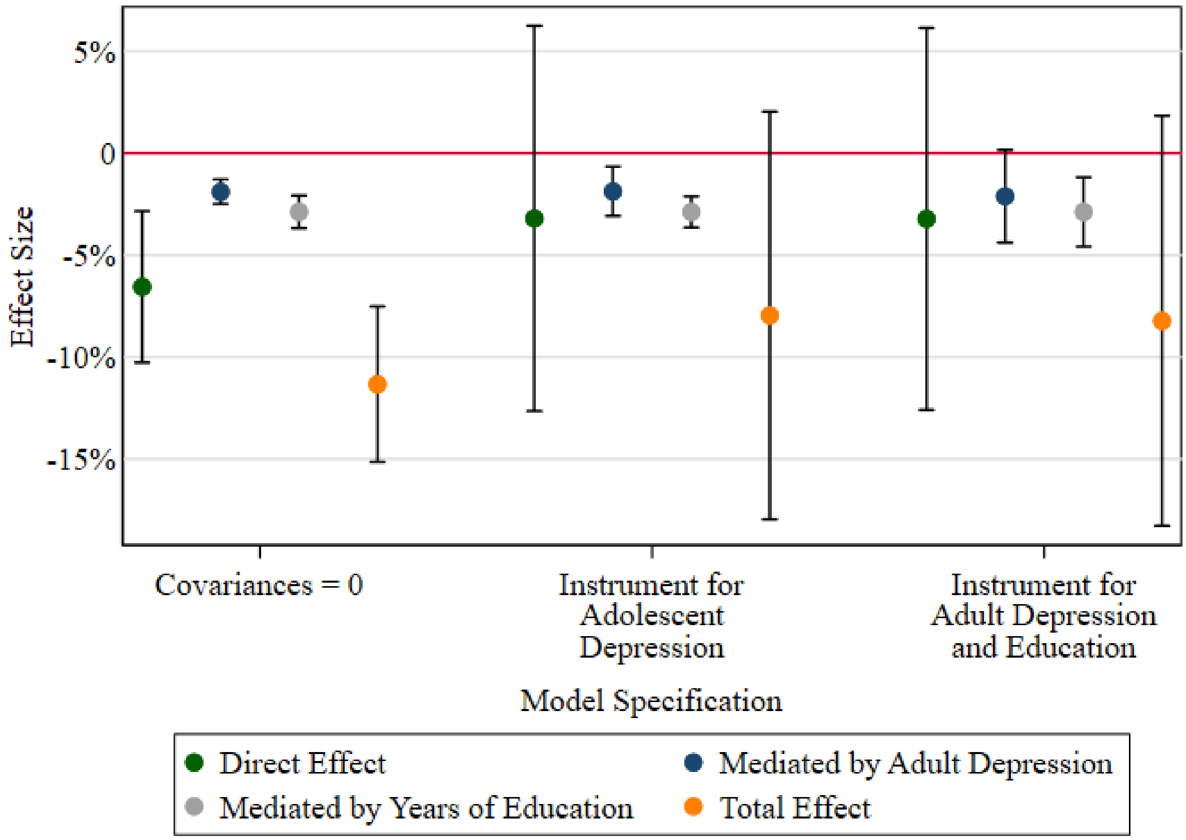
[15] See Table A.5 for full results, including coefficients on instrumental variables and other specifications.

to about -15.1% in column 5, larger than any previous specification. Whether a friend or family member attempted suicide in the past year is a relatively strong instrument. Having a death in the family is a weaker instrument only statistically significant at the 5% level, so it is excluded from column 5. The Z-Test Statistic suggests that we fail to reject the null hypothesis that σ_{Y,D_2} is unchanged between columns 4 and 5, failing to find evidence that the included instrument is invalid. Taken in context, results suggest that adult depression may play a slightly larger mediating role than implied by column 1.

Using instruments for identification has little effect on the mediating role of years of education. Youth with depression receive about one third fewer years of education – this estimate stays relatively constant across all specifications because instruments are not used to identify the effect of adolescent depression on years of education. In column 5, the return to a year of education is about 9.5%, similar to all previous estimates. The tract-level proportion of adults with a bachelor's degree is a strong and meaningful instrument – a 0.10 increase in the proportion increases years of education by about 0.16. However, tract-level school enrollment is only significant at the 5% level and is excluded from column 5. Finally, the direct effect of adolescent depression in column 3 drops slightly to about -3.1% and remains statistically insignificant. This maintains the result that column 1 significantly overstates the importance of the direct effect.

Figure 1 plots the direct, indirect, and total effects of adolescent depression on earnings as percent changes. All effects are calculated using the estimates from columns 1, 3, and 5 of Table 3 (see Table A.6 and Table A.7 for point estimates and standard errors). When each equation in the system is estimated separately, the average direct effect is -6.56% and is clearly the pathway with the most significant magnitude. Adult depression mediates a -1.90% effect,

Figure 1 – Direct, Indirect, and Total Mediated Effects



Note: Effect sizes are reported as percentage terms with 95% confidence intervals bands. Confidence intervals are calculated using a parametric bootstrap with 500 replications. Results match up with estimates in Tables A.6 and A.7. The first set of results presents results from when covariance terms are restricted to zero and no instruments are included. The second set of results estimates σ_{Y,D_1}. *The third set of results adds instrumental variables and estimates all covariance terms except for* $\sigma_{D_2,E}$ *and* σ_{E,D_1}. *School fixed effects are included in all specifications.*

while years of education mediates about a -2.88% effect. Mediated effects make up a notable portion of the total effect of adolescent depression on earnings – treating adult depression and education as confounders would underestimate the total effect about 42%.

When cohort religiosity is used as an instrumental variable in the adolescent depression equation, the direct effect falls to -3.21% and is imprecisely estimated. The effect mediated by adult depression drops slightly to -1.88% due to a drop in the average marginal effect of adolescent depression on adult depression. The drop in the direct effect drives a large drop in the total effect as well from -11.33% to -7.06%.

When instruments are also added to the adult depression and years of education equations, the direct effect moves to -3.23% and remains statistically insignificant. An increase in the marginal effect of adult depression on earnings leads the indirect effect through adult depression to grow to -2.12%. Little changes for the indirect effect through education – lower educational attainment resulting from adolescent depression results in a 2.89% drop in average earnings. Compared to estimating equations separately, the total effect drops from -11.33% to -8.23% and is statistically insignificant due to the drop in the direct effect and its noisy estimation. As displayed in Figure A.8, the total *mediated* effect is about -5% and statistically significant at the 1% level.

Using instrumental variables to address endogeneity flips the importance of the pathways of effects – the direct effect is cut in half and statistically insignificant, while the mediated effects remain meaningful and relatively precisely estimated. The effects mediated by adult depression and years of education are driving the total effect of adolescent depression on earnings. The drop in the direct effect implies that some of the direct effect was driven by omitted variables bias, measurement error, or misspecification. This could include omitted mediating variables or poor measurement of mediators. The effects mediated by adult depression and education, while economically meaningful, may be an underestimate of the total effect of adolescent depression on earnings if there are more complex mediating pathways. Still, the total effect estimated in this paper represents an economically significant drop in earnings, equivalent to about $1,750 annually.

In appendices B and C, I provide evidence that these results are robust to different measures of education, adult depression, and adolescent depression. Unobserved family-level heterogeneity is a common concern in the literature. In appendix D, I present evidence that family-level heterogeneity does not qualitatively change results.

7. Discussion

In contrast to previous literature, I find that the direct effect of adolescent depression on earnings is not robust to identification strategy. This is evident after endogenizing adolescent depression, adult depression, and years of education in an earnings equation and using instrumental variables for identification. The importance of the direct effect is sometimes driven by the omission of important mediating or confounding variables. At other times, unobserved heterogeneity biases the direct effect away from zero. Although the finding of a null direct effect is not common, it is intuitive – adolescent health conditions have little way of immediately impacting outcomes in adulthood. Instead, they can influence a variety of intermediate outcomes that end up affecting labor market outcomes down the road. If mediators and influential confounding variables are included in the model and well-measured, then a direct effect is not expected to show up.

These results suggest an added economic benefit to treating adolescent depression. Since this paper does not account for differences in receiving treatment for depression, estimated effects are in spite of rates of treatment over the life course. This highlights the potentially large remaining economic benefits that could be achieved from better identifying and treating adolescent depression. All else constant, decreasing symptoms of depression in adolescence will increase educational attainment, decrease the likelihood of depression in adulthood, and lead to increased earnings in the long run.

The mediating roles of adult depression and education suggest there may also be additional ways of avoiding lost earnings due to adolescent depression. Holding depressive symptoms constant, interventions that prevent those with depression from dropping out of school prematurely may avoid a significant portion of the earnings gap. For example, Supported Education interventions hire staff at educational institutions to identify and work with young adults with psychological disorders to help achieve their educational goals (Ringeisen et al., 2017). Interventions at the school level that simultaneously support education and refer participants to mental health services could address several indirect pathways at once. Part of the earnings gap could also be addressed by identifying and treating depression in adulthood, weakening the link between adolescent and adult depression. Interventions that help adults with mental illness find and maintain gainful employment could also be worthwhile, especially if the earnings gap is driven by labor supply. One such type of policies are Supported Employment policies (Marshall et al., 2014). If youth suffer from depression for an extended period before receiving treatment, it is possible that some these policies would be the most effective at preventing lower earnings in the long run.

The results of this paper complement several findings and predictions in previous literature that adult depression and educational attainment may be mediators in this context (Fergusson et al., 2007; J. Fletcher, 2013; Johar & Truong, 2014; Smith & Smith, 2010). For example, results are consistent with Philipson et al. (2020), which finds that about half of the total effect is mediated by adult depression. I also corroborate the evidence presented by Johar & Truong (2014) that educational attainment plays an important mediating role, as well as broad evidence in Lundborg et al. (2014) that adolescent health and mental health problems have labor market consequences.

This paper has several drawbacks. First, establishing causality without experimental data is difficult. Although the baseline results of this paper are consistent with previous literature, small violations in identifying assumptions could result in bias that alters results. Future research that uses data from randomized, experimental contexts would create the strongest case for identifying causal effects of adolescent depression on outcomes. Second, self-reported measures of mental health and labor market outcomes, while generally reliable, often suffer from measurement error that could bias effects towards zero. While the CES-D is a tested measure of symptoms of depression, it does not provide the same level of detail that would be obtained from a diagnosis or in-person assessment. Longitudinal surveys that include interactions with healthcare professionals would allow researchers to pinpoint exactly what mental health disorders are responsible for long-term consequences.

Finally, this paper is a snapshot connecting data from one point in adolescence to data from one point in adulthood. I am unable to construct long-term histories of labor market outcomes and mental health status that could provide a more detailed look at long-term effects of adolescent depression. The evidence presented in this paper should be taken in context of other work done with longer histories of outcomes (e.g., Philipson et al., 2020).

Despite these concerns, the results of this paper provide important new estimates of the various effects of adolescent depression on adult earnings. These estimates improve our understanding of the consequences of adolescent depression and highlight pathways that can be targeted by policies to avoid its adverse impacts on earnings.

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Appendix A – Descriptives and Model Building

Table A.1 – Measures of Depression in the Add Health

	CES-D	Add Health	Feelings Scale	CES-D 20-item	CES-D 10-item
1	I was bothered by things that usually don't bother me	You were bothered by things that usually don't bother you	Y	Y	Y
2	I did not feel like eating; my appetite was poor	You didn't feel like eating, your appetite was poor	Y	Y	N
3	I had trouble keeping my mind on what I was doing	You had trouble keeping your mind on what you were doing	Y	Y	Y
4	I felt that everything I did was an effort	You felt that you were too tired to do things	Y	Y	Y
5	I talked less than usual	You talked less than usual	Y	Y	N
6	I could not get "going"	It was hard to get started doing things	Y	Y	N
7	My sleep was restless	Trouble falling asleep or staying asleep	N	Y	N
8	I felt that I could not shake off the blues even with help from my family or friends	You felt that you could not shake off the blues, even with help from your family and your friends	Y	Y	Y
9	I felt depressed	You felt depressed	Y	Y	Y
10	I thought my life had been a failure	You thought your life had been a failure	Y	Y	N
11	I felt fearful	You felt fearful	Y	Y	N
12	I felt lonely	You felt lonely	Y	Y	N
13	n/a	You felt life was not worth living	Y	N	N
14	I felt sad	You felt sad	Y	Y	Y
15	I had crying spells	Frequent crying	N	Y	N
16	I felt that people dislike me	You felt that people disliked you	Y	Y	Y
17	People were unfriendly	People were unfriendly to you	Y	Y	N
18	I felt that I was just as good as other people	You felt that you were just as good as other people	Y	Y	Y
19	I felt hopeful about the future	You felt hopeful about the future	Y	Y	N
20	I was happy	You were happy	Y	Y	Y
21	I enjoyed life	You enjoyed life	Y	Y	Y

Note: The Feelings Scale is administered in wave 1 and is used to measure adolescent depression. The 10-item CES-D is administered in wave 4 and is used to measure adult depression. For the Feelings Scale, respondents are asked, "How often was each of the following things true during the past week?" and then presented with each item. Respondents choose from the responses of "never or rarely," "sometimes," "a lot of the time," and "most of the time or all of the time." Values of 0-3 are assigned to each response and are summed to calculate a "CES-D Score".

Table A.2 – Log Earnings OLS Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Adolescent Depression	-0.255***	-0.131***	-0.077***	-0.067**	-0.068**	-0.067**	-0.068**
	(0.032)	(0.026)	(0.022)	(0.021)	(0.021)	(0.021)	(0.021)
Years of Education		0.118***	0.111***	0.094***	0.092***	0.091***	0.091***
		(0.005)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)
Adult Depression		-0.244***	-0.185***	-0.142***	-0.140***	-0.139***	-0.137***
		(0.022)	(0.021)	(0.022)	(0.022)	(0.022)	(0.022)
Adolescent Controls	No	No	Yes	Yes	Yes	Yes	Yes
Adult Controls	No	No	No	Yes	Yes	Yes	Yes
Fixed Effects	None	None	None	None	School	W4 State	Comm.
Observations	13,298	13,298	13,298	13,174	13,174	13,174	12,911

**** for p < 0.001, ** for p < 0.01, * for p < 0.05*

Note: Coefficients are reported with standard errors in parentheses. Standard errors are clustered at the school level. Column 1 includes just adolescent depression, adult depression, years of education, and age on the right-hand side. Column 2 adds dummies for gender, race, and ethnicity. Column 3 adds wave 1 controls, including adolescent health behaviors, health measures, vocabulary score, parent education, and family income. Column 4 adds wave 4 controls, including number of kids, region, marital status, urban/rural status, whether they have been to jail, and school enrollment status. Columns 5-8 include different fixed effects, where "Comm." stands for community fixed effects.

Table A.3 – Years of Education Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Adolescent Depression	-0.716***	-0.584***	-0.419***	-0.312***	-0.320***	-0.320***	-0.317***
	(0.056)	(0.054)	(0.050)	(0.040)	(0.041)	(0.041)	(0.040)
Observations	14,561	14,561	14,561	14,561	14,561	14,277	14,420
Fixed Effect	None	None	None	None	School	Comm.	W4 State

**** for p < 0.001, ** for p < 0.01, * for p < 0.05*

Note: Coefficients are reported with standard errors in parentheses. Standard errors are clustered at the school level. Years of education is the dependent variable in every column and a linear model is estimated with OLS. Column 1 includes only adolescent depression on the right-hand side. Columns 2 adds gender, race/ethnicity, grade, and vocabular test score. Column 3 adds adolescent smoking, marijuana use, alcohol use, anxiety, and BMI. Column 4 adds log of family income, maternal education, and whether the respondent has been to jail before. Columns 5-7 include all controls in column 4 and include different fixed effects.

Table A.4 – Adult Depression Results

	Depressed in Adulthood						Adult CES-D Score		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Adolescent Depression	0.637***	0.619***	0.516***	0.485***	0.492***	0.486***	2.106***	2.103***	2.106***
	(0.028)	(0.028)	(0.030)	(0.030)	(0.031)	(0.030)	(0.112)	(0.113)	(0.112)
Observations	14,561	14,561	14,561	14,349	14,349	14,319	14,349	14,349	14,349
Fixed Effect	None	None	None	None	School	W4 State	None	School	W4 State

**** for p < 0.001, ** for p < 0.01, * for p < 0.05*

Note: Coefficients are reported with standard errors in parentheses. Standard errors are clustered at the school level. Columns 1-6 estimate a probit model with adult depression as the dependent variable. Columns 7-9 use CES-D score and estimate a linear model with OLS. Column 1 only includes adolescent depression on the right-hand side. Column 2 adds controls for gender, race, ethnicity, and grade. Column 3 adds measures of adolescent health behaviors (smoking status, marijuana use, alcohol use), adolescent anxiety, vocabulary test score, log of family income, maternal education, and BMI. Column 4 adds adult controls, including marital status, adult BMI, whether they have been to jail, school enrollment status, number of children, and adult religiosity. Columns 5-9 include all controls in column 4.

Table A.5 – System of Equations Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Ln(earnings)								
Adolescent Depression	-0.067**	-0.024	-0.068**	-0.017	-0.036	-0.033	-0.023	-0.032
	(0.021)	(0.057)	(0.021)	(0.039)	(0.052)	(0.052)	(0.053)	(0.052)
Adult Depression	-0.142***	-0.175**	-0.140***	-0.143***	-0.140***	-0.155**	-0.184*	-0.164**
	(0.022)	(0.062)	(0.022)	(0.022)	(0.022)	(0.052)	(0.090)	(0.062)
Years of Education	0.094***	0.119***	0.092***	0.092***	0.092***	0.092***	0.110*	0.091***
	(0.005)	(0.033)	(0.005)	(0.005)	(0.005)	(0.005)	(0.051)	(0.026)
Adult Depression								
Adolescent Depression	0.137***	0.125**	0.137***	0.080	0.128**	0.128**	0.126**	0.126**
	(0.009)	(0.040)	(0.009)	(0.086)	(0.042)	(0.042)	(0.042)	(0.042)
Family Member Passed		0.050					0.049	
Away in Past 12 Months		(0.040)					(0.042)	
Family/Friend Attempted		0.051***					0.051***	0.051***
Suicide in Past 12 Months		(0.012)					(0.013)	(0.013)
Years of Education								
Adolescent Depression	-0.312***	-0.306***	-0.320***	-0.320***	-0.320***	-0.320***	-0.313***	-0.315***
	(0.039)	(0.040)	(0.041)	(0.041)	(0.041)	(0.041)	(0.041)	(0.042)
Proportion 25+ with		1.613***					1.285***	1.575***
Bachelor's Degree		(0.185)					(0.235)	(0.206)
Proportion 16-19 Enrolled		0.792***					0.478*	
in School		(0.233)					(0.214)	
Adolescent Depression								
Religiosity Score		-0.003**			-0.005***	-0.005***	-0.005***	-0.005***
		(0.001)			(0.001)	(0.001)	(0.001)	(0.001)
$\sigma_{Y,E}$	= 0	-0.050	= 0	= 0	= 0	0.000	-0.036	0.002
		(0.067)				(0.001)	(0.100)	(0.050)
σ_{Y,D_4}	= 0	0.019	= 0	= 0	= 0	0.009	0.027	0.014
		(0.035)				(0.030)	(0.055)	(0.036)

σ_{Y,D_1}	= 0	-0.020	= 0	-0.031	-0.021	-0.021	-0.021	-0.021
		(0.030)		(0.020)	(0.028)	(0.028)	(0.028)	(0.028)
σ_{D_2,D_1}	= 0	0.024	= 0	0.114	0.018	0.017	0.022	0.021
		(0.077)		(0.180)	(0.080)	(0.080)	(0.081)	(0.082)
Z-Test Statistic: σ_{Y,D_1}					-0.291		0.000	0.000
Z-Test Statistic: σ_{D_2,D_1}					0.487		0.044	0.035
Z-Test Statistic: σ_{Y,D_2}							0.287	0.107
Z-Test Statistic: $\sigma_{Y,E}$							-0.036	0.002
Observations	14,561	14,561	14,561	14,561	14,561	14,561	14,561	14,561
Fixed Effects	None	None	School	School	School	School	School	School

**** for $p < 0.001$, ** for $p < 0.01$, * for $p < 0.05$*

Note: This table estimates the system of equations in equations 6-9 using limited information maximum likelihood. For the adolescent and adult depression equations, average marginal effects are presented with standard errors in parentheses. For the earnings and years of education equations, coefficients are reported. Standard errors in columns 3-8 are clustered at the school level. In column 1, all covariance terms are restricted to zero. In column 2, instruments are added to each equation, where appropriate, and the covariance terms displayed are estimated by maximum likelihood. The covariance terms σ_{E,D_2} and σ_{E,D_1} are restricted to zero in all specifications. Columns 3-8 include a school fixed effect. Column 3 estimates the equations independently, column 4 estimates σ_{Y,D_1} σ_{D_2,D_1}, and column 5 instruments for adolescent depression. Column 6 then opens up two more covariance terms: σ_{Y,D_2} and $\sigma_{Y,E}$. Column 7 adds instrumental variables to the adult depression and years of education equations. Column 8 removes two weak instruments from the system. The Z-Test Statistics are the test statistics for the Z-test of the difference between the covariance terms with and without an instrument added.

Table A.6 – Direct, Mediated, and Total Effect Sizes (% Change)

	(1)	(2)	(3)
Direct	-6.56	-3.21	-3.23
	(1.90)	(4.82)	(4.78)
Mediated by Adult Depression	-1.90	-1.88	-2.12
	(0.31)	(0.62)	(1.16)
Mediated by Years of Education	-2.88	-2.88	-2.89
	(0.41)	(0.39)	(0.87)
Total Mediated Effect	-4.78	-4.76	-5.00
	(0.53)	(0.69)	(1.48)
Total Effect	-11.33	-7.96	-8.23
	(1.95)	(5.10)	(5.13)
Instrument for Adolescent	No	Yes	Yes
Instrument for Adult Depression and	No	No	Yes

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$

Note: Effect sizes are reported as percentage changes with standard errors in parentheses. Standard errors are calculated using a parametric bootstrap with 500 replications. Effects are calculated using results in Table 3 and are graphed in Figure 1 – all include a school fixed effect. Column 1 presents results from when covariance terms are restricted to zero and no exclusion restrictions are included. Column 2 estimates σ_{Y,D_1}. *Column 3 includes instruments and estimates all covariance terms except for* $\sigma_{D_2,E}$ and σ_{E,D_1}.

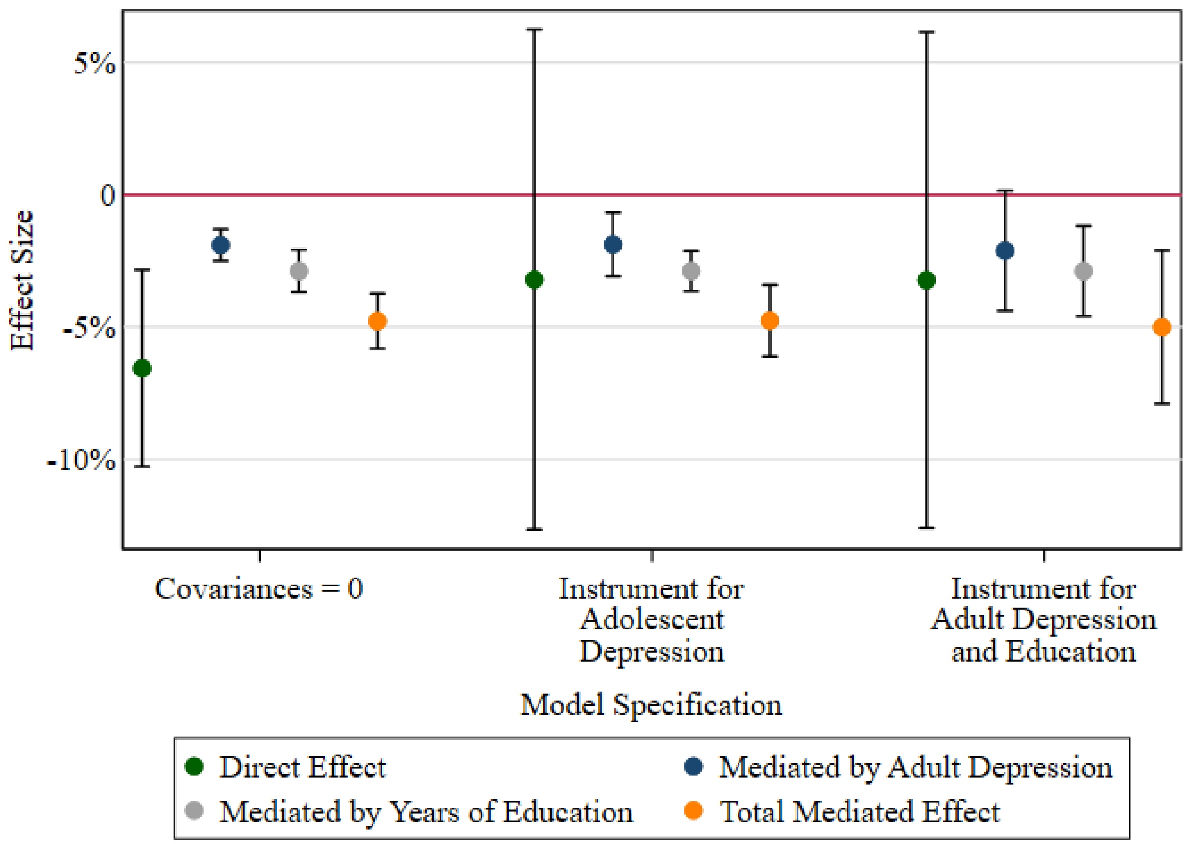
Table A.7 – Direct, Indirect, and Total Effects of Adolescent Depression on Earnings

	(1)	(2)	(3)
Direct	-0.068	-0.034	-0.034
	(0.020)	(0.050)	(0.049)
Indirect – Adult Depression	-0.019	-0.019	-0.021
	(0.003)	(0.006)	(0.012)
Indirect – Years of Education	-0.029	-0.029	-0.029
	(0.004)	(0.004)	(0.009)
Total Mediated Effects	-0.048	-0.048	-0.051
	(0.005)	(0.007)	(0.015)
Total Effect	-0.116	-0.082	-0.085
	(0.021)	(0.052)	(0.053)
Instrument for Adolescent Depression	No	Yes	Yes
Instrument for Adult Depression and	No	No	Yes

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$

Note: Coefficients are reported with standard errors in parentheses. Standard errors are calculated using a parametric bootstrap with 500 replications. Effects are calculated using results in Table 3 and are graphed in Figure 1 – all include a school fixed effect. Column 1 presents results from when covariance terms are restricted to zero and no exclusion restrictions are included. Column 2 estimates σ_{Y,D_1}. *Column 3 includes instruments and estimates all covariance terms except for* $\sigma_{D_2,E}$ and σ_{E,D_1}.

Figure A.8 - Direct, Indirect, and Total Mediated Effects of Adolescent Depression on Earnings



**** for $p < 0.001$, ** for $p < 0.01$, * for $p < 0.05$*
Note: Direct, indirect, and total mediated effects are reported. 95% confidence intervals are calculated using a parametric bootstrap with 500 replications. Results are from models in Table 3 and are presented in Table A.6. The first set of results are from when covariance terms are restricted to zero and no exclusion restrictions are included. The second set is from when σ_{Y,D_1} is estimated. The last set includes instruments and estimates all covariance terms except for $\sigma_{D_2,E}$ and σ_{E,D_1}. All specifications include a school fixed effect.

Appendix B – Categorical Measures of Education

Results suggest that adolescent depression has a significant indirect effect on earnings through years of education. Understanding whether depression affects each level of education differently could provide insight into where this indirect effect is the most important and have important policy implications.

I split reported educational attainment into four categories: less than high school, high school diploma, some college, and bachelor's degree or higher. I use three linear probability models[16] to estimate the effect of adolescent depression on the probability of moving from one group to another. Each model only uses respondents who report to be in at least the lower of the two categories (e.g., the model estimating the effect on college graduation uses those with some college or greater). I then estimate the earnings equation where I replace years of education with the dummies for each level of educational attainment. Indirect effects are calculated by multiplying the marginal effect of adolescent depression on the probability of an educational outcome by the change in earnings with respect to having that level of education.

Table B.1 presents marginal effects of the education and earnings models and finds evidence that adolescent depression is important across several margins of educational attainment. Column 1 finds that adolescent depression leads to about a 3.3% drop in the probability of graduating high school, all else constant. Among those who have a high school

Table B.1 – Discrete Measures of Educational Attainment

	(1) *High School*	(2) *Some College*	(3) *Bachelor's*	(4) *Ln(Earnings)*
Adolescent Depression	-0.034***	-0.057***	-0.036**	-0.069**
	(0.006)	(0.009)	(0.012)	(0.021)
High School Diploma				0.262***
				(0.049)
Some College				0.379***
				(0.051)
Bachelor's Degree				0.678***
				(0.055)
Observations	14,561	13,505	9,775	13,174
Fixed Effects	School	School	School	School

**** p < 0.001, ** p < 0.01, * p < 0.05*

Note: Coefficients are reported with standard errors in parentheses. Standard errors are clustered at the school level. Columns 1-3 use high school completion, some college, and bachelor's degree or more as dependent variables, respectively. Columns 1-3 estimate a linear probability model and only the youth on or above the margin of the outcome are included in the regression. Column 4 presents earnings results when the ordered measure of education is included instead of years of education. All specifications include a school fixed effect.

[16] A linear probability model is used instead of a probit so that observations from small schools without much variation in educational attainment can be used in estimation.

degree, column 2 suggests that youth with adolescent depression are about 5.5% less likely to enroll in college. Finally, among youth who enroll in college, column 3 shows that those with adolescent depression are about 3.5% less likely to finish college. Column 4 presents marginal effects of educational attainment on log earnings, where less than a high school diploma is the base category. Each level of education is an important determinant of earnings, with those with a bachelor's degree earning nearly double than those without a high school degree.

Table B.2 presents estimates of the indirect effects through each margin of education. When scaled by the returns to education, adolescent depression leads to a 0.90% drop in earnings through the margin of graduating high school, a 0.66% drop through the probability of enrolling in college, and a 1.07% drop via whether they finish college. This highlights that an additional year of education that leads to a degree is more meaningful than one that does not. However, the total indirect effect of -2.63% is similar and statistically indistinguishable from the estimated effect of -2.88% in column 1 of Table A.6 when years of education is used. This suggests that accounting for the nonlinear returns to education does not qualitatively change results.

Table B.2 - Indirect Effects of Adolescent Depression on Earnings (%) by Margin of Education

	(1) High School	(2) Some College	(3) Bachelor's	(4) Total
Adolescent Depression	-0.90%**	-0.66%***	-1.07%***	-2.60%***
	(0.19)	(0.19)	(0.34)	(0.46)

**** p < 0.001, ** p < 0.01, * p < 0.05*

Note: Indirect effects are presented as percent changes with standard errors in parentheses. Standard errors are non-parametrically bootstrapped with 100 replications. Effect sizes are calculated using results from Table . For example, the estimated difference in log earnings in column 2 is calculated by $(-0.057)(0.379-0.262)$. *Then the result is transformed to a percent change using* $e^{x}-1$.

Although I do not explore the several unobserved mechanisms that could be driving differences in earnings (e.g., quality of education, occupation), results are suggestive that adolescent depression has broad negative effects on earnings throughout the educational career of an adolescent. These effects are more likely to be direct in the case of high school completion, as symptoms of depression in adolescence may immediately affect the youth's ability to perform well in school. However, the effects on enrolling in college and/or finishing college could be mediated by several pathways, including changing expectations of the youth's career path, their social network, or their symptoms of depression in early adulthood.

Appendix C – Robustness to the Measure of Depression

To assess whether the measures of depression have arbitrary impacts on results, I consider three alternative measures of adolescent depression and one alternative measure of adult depression. First, I use the adolescent CES-D score, which provides an average effect of a marginal increase in symptoms when used in a regression. Second, I use a dichotomous measure with a lower CES-D cutoff of 14+. Third, I use a dichotomous measure with a higher CES-D cutoff of 18+. For adult depression, I use the CES-D score as an alternative to the usual dichotomous measure.

Table C.1 presents system of equations results with each alternative measure. All specifications include school fixed effects. Each pair of columns (e.g., columns 1-2) compares results with covariances restricted to zero to results when instruments are used. Table C.1 reveals three points about robustness to the measures of depression.

First, coefficient estimates without instruments (odd columns) are not dependent on the measures of depression used. In column 3, a lower cutoff for adolescent depression results in an estimate of the direct effect coefficient of -0.059; in column 5, a higher cutoff results in a coefficient of -0.048. Both are statistically different than zero but indistinguishable from the estimate of -0.068 in column 1. In column 7, using CES-D score produces a coefficient of -0.004. When scaled by the average difference in CES-D scores between youth with and without adolescent depression (see Table 1), this coefficient is about -0.049, comparable to estimates in columns 1, 3, and 5.

Second, using instruments for identification in each specification results in similar changes in results despite the measure of depression. The takeaways are the same. Compared to column 1, the direct effect in column 2 drops in magnitude and is imprecisely estimated. Using lower or higher cutoff scores in columns 4 and 6 leads to similar changes in the direct effect – a large drop in magnitude and statistical insignificance. When CES-D score is used for adolescent and adult depression in column 8, the direct effect grows slightly but the standard errors are very inflated. Overall, the impact that instrumental variables has on the results does not appear to be driven by the measures of depression used. Third, the measures of depression used do not affect instrument relevance. Peer religiosity remains a strong instrument across all even columns, and the instruments used in the adult depression equation have similar levels of relevance.

Altogether, Table C.1 suggests that the measures of adolescent and adult depression do not have a significant impact on results, which remain qualitatively similar.

Table C.1 – System Robustness to Different Measures of Depression

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Ln(Earnings)								
Adolescent Depression	-0.068**	-0.032						
	(0.021)	(0.052)						
Adolescent CES-D Score							-0.004**	-0.008
							(0.002)	(0.010)
Adolescent Depression Low Cutoff			-0.059**	-0.039				
			(0.019)	(0.086)				
Adolescent Depression High Cutoff					-0.048*	-0.008		
					(0.024)	(0.031)		
Adult Depression	-0.140***	-0.164**	-0.141***	-0.176*	-0.144***	-0.177*		
	(0.022)	(0.062)	(0.022)	(0.070)	(0.022)	(0.072)		
Adult CES-D Score							-0.018***	-0.033
							(0.002)	(0.034)
Years of Education	0.092***	0.091***	0.092***	0.092***	0.092***	0.091***	0.090***	0.091***
	(0.005)	(0.026)	(0.005)	(0.026)	(0.005)	(0.026)	(0.005)	(0.026)
Years of Education								
Adolescent Depression	-0.320***	-0.315***						
	(0.041)	(0.042)						
CES-D Score							-0.029***	-0.029***
							(0.003)	(0.003)
Adolescent Depression Low Cutoff			-0.329***	-0.326***				
			(0.037)	(0.037)				
Adolescent Depression High Cutoff					-0.323***	-0.314***		
					(0.041)	(0.042)		
Proportion 25+ with Bachelor's Degree		1.575***		1.577***		1.573***		1.555***
		(0.206)		(0.205)		(0.206)		(0.208)
Adult Depression								
Adolescent Depression	0.492***	0.458**						
	(0.031)	(0.142)						

Adolescent CES-D Score							0.185***	0.142***
							(0.007)	(0.032)
Adolescent Depression Low Cutoff			0.463***	0.342*				
			(0.029)	(0.158)				
Adolescent Depression High Cutoff					0.513***	0.306***		
					(0.032)	(0.038)		
Family/Friend Attempted Suicide in Past 12 Months		0.192***		0.186***		0.186***		0.658***
		(0.045)		(0.045)		(0.045)		(0.144)
Adolescent Depression								
Religiosity Score		-0.018***		-0.018***		-0.017***		-0.101***
		(0.005)		(0.005)		(0.005)		(0.021)
$\sigma_{Y,E}$	=0	0.002	=0	0.000	=0	0.001	=0	-0.002
		(0.050)		(0.050)		(0.050)		(0.050)
σ_{Y,D_4}	=0	0.014	=0	0.021	=0	0.019	=0	0.072
		(0.036)		(0.041)		(0.042)		(0.152)
σ_{Y,D_1}	=0	-0.021	=0	-0.010	=0	-0.033	=0	0.044
		(0.028)		(0.053)		(0.015)		(0.055)
σ_{D_2,D_1}	=0	0.021	=0	0.073	=0	0.184	=0	0.059
		(0.082)		(0.095)		(0.021)		(0.043)
Observations	14,561	14,561	14,561	14,561	14,561	14,561	14,561	14,561

**** for $p < 0.001$, ** for $p < 0.01$, * for $p < 0.05$*

Note: Coefficients are reported with standard errors in parentheses. Every column includes a school fixed effect in each equation, and standard errors are clustered at the school level. Each pair of columns uses a different measure of depression and compares estimation with covariances restricted to zero to estimation with instruments. Columns 1-2 use the regular depression measures. Columns 3-4 use a low cut-off (14+) for adolescent depression, while columns 5-6 use a high cutoff (18+). Columns 7-8 use CES-D score for both adolescent and adult depression. When the CES-D score is used, the variable is modeled as continuous; when a cut-off is used, the variable is treated as binary and a probit part is used in the likelihood function.

Appendix D – Family-Level Analysis

To examine the robustness of results to family-level heterogeneity, I estimate the earnings, adult depression, and years of education equations using the family sample and assess whether including a family fixed effect significantly alters coefficient estimates. The Add Health surveyed 5,512 youth from 2,633 families. The family sample in this paper consists of all respondents in the full sample who have at least one other family member in the full sample, resulting in 2,593 youth from 1,261 families. To appropriately assess the impact of a family fixed effect, I focus on the 'identifying sample' for each coefficient (Miller et al., 2019), which is the sample of respondents with within-family variation in the variable of interest. I compare the coefficient produced by OLS with this sample to the coefficient produced when a family fixed effect is included. Since there are potential sample selection issues into the family sample and the identifying sample, I focus on the change in estimates when family fixed effects are included, not how they differ from estimates using the full sample.

Table D.1 presents earnings results for the family sample. In column 1, adolescent depression has about a -5.6% direct effect on earnings that is statistically insignificant. Column 2 estimates the same model for the identifying sample for adolescent depression, where the direct effect drops slightly to -5.4%. When a family fixed effect is added in column 5, the direct effect grows to -7.6% but remains noisy and statistically insignificant. Column 3 estimates the model using the identifying sample for adult depression, finding about a -20% average drop in earnings. This result is unchanged when a family fixed effect is added in column 5. Column 4 estimates the model using the identifying sample for years of education, which increases only slightly in column 5.

Table D.1 – Earnings Family Results

	(1)	(2)	(3)	(4)	(5)
Adolescent Depression	-0.058	**-0.055**	-0.059	-0.013	**-0.079**
	(0.045)	**(0.071)**	(0.079)	(0.058)	**(0.065)**
Adult Depression	-0.224***	-0.382***	**-0.224****	-0.263***	**-0.229*****
	(0.051)	(0.085)	**(0.070)**	(0.070)	**(0.066)**
Years of Education	0.103***	0.113***	0.115***	**0.091*****	**0.096*****
	(0.010)	(0.018)	(0.020)	**(0.012)**	**(0.016)**
Sample	Family	ID	ID	ID	Family
Observations	2,593	931	828	1,640	2,593
Fixed Effect	None	None	None	None	Family

**** for p < 0.001, ** for p < 0.01, * for p < 0.05*

Note: Coefficients are reported with standard errors in parentheses. Standard errors are clustered at the family level. Ln(Earnings) is the dependent variable in all columns. Column 1 runs the full model on the family sample using OLS. Columns 2-4 run the full model on their respective 'identifying samples' – the samples which have within-family variation in the variable of interest. This is done so that the presented coefficients in columns 2-4 are identified by the same sample as the coefficients in column 5. In column 5, a family fixed effect is added, addressing within-family heterogeneity. All confounding variables are included in all columns, except for family characteristics that are excluded from column 5. I also control for whether a youth was the first born in their family.

Table D.2 presents regression results for adult CES-D score and years of education with the family sample. In columns 1-3, I estimate an equation for adult CES-D score using OLS. I use adult CES-D score as the dependent variable to avoid issues of incidental parameters bias in a probit with a large number of fixed effects. Column 1 runs OLS on the whole family sample. When only the identifying sample is used in column 2, the marginal effect of adolescent depression drops notably. Compared to column 2, adding a family fixed effect in column 3 has a marginal impact on the estimated coefficient, bringing it from 2.047 to 1.907. In columns 4-6, I estimate the years of education equation with OLS. When the whole family sample is used, the coefficient is -0.30 and statistically significant, similar to main results in Table A.5. When only the identifying sample is used, the coefficient drops to -0.046 and is statistically insignificant, indicating that the coefficient is sensitive to sample selection. When a family fixed effect is added, the effect of adolescent depression on years of education decreases slightly from -0.046 to -0.044, suggesting that family-level heterogeneity has little effect on the coefficient estimate. If we had not used the identifying sample results as a baseline, we would incorrectly infer that family-level heterogeneity plays a large role.

Time-invariant, family-level heterogeneity appears to have minimal impacts on coefficient estimates. While estimates of adolescent depression's impacts on adult depression, years of education, and earnings change slightly, they are statistically indistinguishable from estimates without fixed effects and are qualitatively similar.

Table D.2 – Adult Depression and Years of Education Family Results

	Adult CES-D Score			Years of Education		
	(1)	(2)	(3)	(4)	(5)	(6)
Adolescent Depression	2.341***	2.047***	1.907***	-0.300***	-0.046	-0.044
	(0.234)	(0.317)	(0.318)	(0.088)	(0.113)	(0.108)
Sample	Family	ID	Family	Family	ID	Family
Observations	2,593	931	2,593	2,593	931	2,593
Fixed Effect	None	None	Family	None	None	Family

**** for p < 0.001, ** for p < 0.01, * for p < 0.05*

Note: Coefficients are reported with standard errors in parentheses. Standard errors are clustered at the family level. In column 1, adult CES-D score is regressed on adolescent depression and a full set of covariates with OLS. Column 2 runs column one with the identifying sample, and column 3 adds a family fixed effect. Column 4 regresses years of education on adolescent depression and a full set of covariates using OLS. Column 5 runs column 4 with the identifying sample and column 6 adds a family fixed effect. In columns 3 and 6, only the 'identifying sample' is used – the sample of youth from a family with within-family variation in adolescent depression. While the whole family sample is being used in columns 2 and 4, only the identifying sample is identifying the coefficient on adolescent depression. Therefore, the same sample identifies the coefficient on adolescent depression across columns 2-3 and 5-6.

Appendix E – A Test for Instrument Validity

This appendix briefly outlines how I test for instrument validity when estimating a system of equations (i.e., equations 6-9). First, consider the covariance term $\sigma_{D_1,Y}$ when no instrument is included in the adolescent depression equation:

$$Cov\left(\epsilon_{D_1}, \epsilon_Y\right)^1 = Cov(D_1, \epsilon_Y) + Cov\left(C_{D_1}\phi_1, \epsilon_Y\right) + Cov(s, \epsilon_Y) \tag{E.1}$$

Second, consider the covariance term $\sigma_{D_1,Y}$ when Z_{D_1} is included as an instrument in the adolescent depression equation:

$$Cov\left(\epsilon_{D_1}, \epsilon_Y\right)^2 = Cov(D_1, \epsilon_Y) + Cov\left(C_{D_1}\phi_1, \epsilon_Y\right) + Cov(s, \epsilon_Y) + Cov(Z_{D_1}, \epsilon_{Yi}) \tag{E.2}$$

Taking the difference between the two terms, we get:

$$Cov\left(\epsilon_{D_1}, \epsilon_Y\right)^2 - Cov\left(\epsilon_{D_1}, \epsilon_Y\right)^1 = Cov(Z_{D_1}, \epsilon_{Yi}) \tag{E.3}$$

The validity assumption (A1) for Z_{D_1} is that equation E.3 equals zero. Therefore, we can test the validity of an instrument by testing whether the difference between $Cov\left(\epsilon_{D_1}, \epsilon_Y\right)^1$ and $Cov\left(\epsilon_{D_1}, \epsilon_Y\right)^2$ is zero. I conduct a Z-test for the difference between parameters, where the null hypothesis is that $Cov\left(\epsilon_{D_1}, \epsilon_Y\right)^2 - Cov\left(\epsilon_{D_1}, \epsilon_Y\right)^1 = 0$. If the test statistic is sufficiently large, then we can reject the null hypothesis that the instrument is valid.