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DOES SCHOOLING PREVENT TEEN SMOKING AND DRINKING?*

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Abstract: A number of studies have explored the link between education and health or health-related behaviors like drinking and smoking. However, most suffer from concerns of omitted variables bias and reverse causality that make it difficult to argue for a causal relationship from schooling to these two behaviors. In this paper, we exploit a randomized trial from the Dominican Republic that provided information on the returns to schooling to 8th grade students, which resulted in an increase in schooling. We find that the treatment had a significant impact in reducing smoking and heavy drinking. We also find evidence that the effects may be due to changes in peer networks and increases in disposable income, but are unlikely to be caused by any direct impact of schooling on rates of time preference, attitudes towards risk or perceptions that drinking or smoking are harmful to health.

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I. INTRODUCTION

Schooling is a strong predictor of health, in both developed and developing countries. These associations are large: for example in the U.S. in 2000, one more year of schooling was associated with approximately one more year of life expectancy. More educated individuals are in turn less likely to smoke or drink excessively, and in general have better health behaviors than their less educated counterparts. (See Cutler and Lleras-Muney 2007 for a review.) However there is considerable debate about whether these associations reflect causal effects of schooling on health behaviors and health. The correlation between education and health behaviors for example may be driven by omitted variables bias; for example, high discount rates would be likely to influence both schooling and health-related behaviors, since both require forgoing utility today in favor of future benefits (higher wages or better health). Alternatively, there may be reverse causality; for example, students who regularly drink may not perform well in school and therefore may not pass to the next grade, or may be expelled from school. Previous research has relied on natural experiments to establish causality, but not all studies find evidence of causal effects.¹ Furthermore there is little evidence on the mechanisms that could explain why schooling may indeed improve health behaviors and lead to better health.

In this paper, we exploit a randomized trial in the Dominican Republic explored in Jensen (2010). That study finds that 8th grade male students in the Dominican Republic had dramatically lower estimates of the returns to schooling than those measured in a household survey. Students at a randomly selected subset of schools were provided information on the returns estimated from earnings data and followed for a six year period. Relative to those not provided with information, these students on average completed 0.20 more years of schooling.

We use this experiment to look at whether, in addition to increasing schooling, the treated students also changed their smoking and drinking habits. Smoking and excessive drinking are two of the most important risk factors in explaining early mortality, accounting for about 12 percent of deaths worldwide (WHO 2006). These behaviors typically start in adolescence, the

¹ Using instrumental variables, Lleras-Muney (2005) and Deschesnes (2007) find that education lowers mortality, but Albouy and Lequien (2008) and Clark and Royer (2008) do not. Sander (1995a, b), De Walque (2004), Kenkel, Lillard and Mathios (2006) and Grimard and Parent (2007) find that schooling lowers smoking rates but Grimard and Parent (2007) find no evidence that schooling increases quitting rates. Park and Kang (2008) find no effect of education on smoking or heavy drinking.

period under study; most individuals have tried alcohol in their early teens² and typically most smokers start smoking before the age of 18.³ Drinking and smoking are a large policy concern because of the externalities associated with their consumption, such as second hand smoke. Excessive drinking among adolescents particularly is associated with increases in deaths from accidents (such as motor vehicle injuries) and crime. Further, smoking and excessive drinking among teens is of particular concern for several reasons. First, the health consequences of drinking and smoking are a function of exposure, so delaying initiation of these behaviors will also delay the onset of the adverse health consequences (and thus extend life expectancy). Further, delaying initiation to smoking does reduce the rate of eventual smoking (Gruber and Zinman 2001, Auld 2005⁴). Finally, recent evidence from the medical and biology literature suggests that brain development is not complete during the teen years and is also sensitive to alcohol and nicotine, and thus drinking and smoking among teens may have more severe long-term impacts, even compared to such behaviors among adults.⁵ To the extent that adolescents aren't fully informed about these costs or are unable to make fully rational decisions,⁶ preventing adolescents from engaging in these behaviors is a worthwhile public policy goal.

Our study provides two key advantages over previous studies. First, we exploit the random assignment of information on the returns to schooling to identify the effects of schooling (and work) on drinking and smoking. Second, our survey gathered (albeit, imperfect) measures of a range of intermediate outcomes that can be used to explore the channels through which education might influence drinking and smoking, including peer networks, discretionary income, rates of time preference, attitudes towards risk, and perceptions of the health consequences of these behaviors. Thus we can provide some evidence on the possible mechanisms through which schooling may affect risky behavior. We find that in addition to increasing schooling and decreasing work, treated boys were less likely to smoke and started drinking heavily at a later

² For example in the U.S. the average age of first alcohol use is 13.1, and in a survey of 23 European countries more than 50% of 11 year olds reported having tried alcohol, and a few existing surveys suggests that alcohol intake begins even earlier in developing countries (WHO 2001).

³ In the U.S., 80% of all smokers have their first cigarette before age 18 and the mean age of starting smoking is about 15.5 (U.S. Department of Health and Human Services, 1998). In a tobacco use survey of 43 countries around the world, the median country had 33% of students ages 13-15 smoke (Global Youth Tobacco survey collaborative Group 2002).

⁴ Though Auld also finds that smoking is perhaps less addictive for early initiators than for later ones.

⁵ A 2004 volume (1021) of the *Annals of the New York Academy of Sciences* on adolescent brain development contains several examples.

⁶ Gruber and Zinman (2001) for example report that adolescents under-estimate the extent to which smoking is addictive.

age. We also find evidence that the effects may be due to changes in the fraction of peers that drink and smoke, and to increases in disposable income, but are unlikely to be caused by any direct impact of schooling on rates of time preference, attitudes towards risk or perceptions of harm.

The remainder of this paper proceeds as follows. In section II, we discuss the data and experiment, and for comparison purposes explore the OLS estimates of the impact of schooling on drinking and smoking. Section III shows the results from the experimental variation in schooling, and explores the mechanisms that may explain the impact of schooling on drinking and smoking. Finally, section IV discusses the results and the limitations of our study, and concludes.

II. DATA

A. Survey Information

Students were drawn from a school-based survey in January, 2001 conducted nationwide, but only in non-rural areas (comprising about two-thirds of the population). The sample was drawn in two stages. First, from the 30 largest cities and towns, we chose 150 sampling clusters at random,⁷ with the number of clusters chosen in each town approximately proportional to that town's share of the combined population of the 30 cities/towns.⁸ For each of the 150 household sample clusters, we selected the school where students from that cluster attend 8th grade.⁹ From each school, during April and May of 2001, we interviewed 15 randomly selected boys¹⁰ enrolled in 8th grade, the final year of primary school and therefore the point right before large declines in enrollment.¹¹ All 2,250 students in the study were administered a survey gathering information on a variety of individual and household characteristics, as well as some simple questions on expected earnings by education.

⁷ Cities and towns were divided into a set of clusters with the help of community leaders and government officials.

⁸ Though for greater geographic variation, we undersampled the capital, Santo Domingo. The city contains roughly 45 percent of the total population of the 30 cities/towns, but is only about 25 percent of our sample.

⁹ In 6 cases, two clusters primarily used the same school; for these cases, we also chose the nearest alternate school.

¹⁰ We did not interview girls because of difficulties in eliciting expected earnings. Due to a low female labor force participation rate in the Dominican Republic (about 40 percent), in focus groups most girls were unwilling to estimate their expected earnings because they felt they would never work.

¹¹ Students were randomly selected from a list of currently enrolled students, and interviewed individually at the school. If a student was not present on the day of the interview, enumerators returned to the school the following day, and then contacted the student at home if they were still not available. 58 students were interviewed in their homes, primarily due to extended illness. Students were not compensated for their participation.

A second survey of the students was conducted after the beginning of the next academic term (October, 2001), with respondents interviewed again (at home, school or work) about perceived returns to education and current enrolment status, and health-related behaviors. A third round, follow-up survey on schooling was also conducted in May and June of 2005, by which time students should have been finishing their last year of secondary school; for the approximately 120 students who were still enrolled in 2005 but not yet through their final year of school (due primarily to grade repetition), we conducted follow-ups for each of the next two years. More details on data are available in Jensen (2010). Note that health measures are not available at baseline

B. Descriptive Statistics

Table 1 reports the summary statistics, for the full sample and separately by treatment status. Socio-economic characteristics, collected at baseline, appear balanced between treatment and controls. Formal tests suggest that randomization was indeed successful. The p-value for the *F*-test that baseline characteristics jointly predict treatment is 0.89. Additional individual tests cannot reject that the means of these covariates are the same for treatment and control groups. Though not reported, the majority of boys were ages 14 (80%) and 15 (11%) in the first round.¹²

Some features of the data are worth commenting on. There is a large drop-out rate (42%) at the end of primary school (between baseline and round 2) and only 30% of the students attending school in 8th grade eventually graduate from high school. About 30% of the sample is working in round 2. Interestingly, this is not exclusively driven by those that are not in school in round 2: 22% of boys are idle, 19% work exclusively, 47% attend school only, and 10% both attend school and work. Earnings from work are low—about 4.2 U.S. dollars per week on average (the exchange rate in May 2001 was US\$1=RD\$16.2), and about 16 U.S. dollars per week for those that are working. These earnings increase by a factor of four by round 3, although the fraction working only increases to 36%.¹³

Our two behaviors of interest are smoking and alcohol consumption. Only 5% of boys reported smoking in round 2, but this increases to 13% by round 3.¹⁴ Drinking alcohol is

¹² Students ranged in age from 14 to 18, with variation due to late starts and grade repetitions.

¹³ For comparison, in the U.S. about 39% of those aged 17 worked during the school year in 1996-1998 (BLS 2000).

¹⁴ Smoking rates are in line with those reported in other studies of Dominican youths in school (Dornitzer et al 2004) but higher than those in the Dominican Republic Demographic and Health Survey (DHS) of 2005, which

extremely common: by round 2, when most of the sample is 14 years old, 75% of boys already report drinking. Although that fraction rises only to 80% in round 3, the frequency of drinking increases substantially; the fraction reporting they drink every week increases from 19% to 46%, and the fraction reporting they drink every day (which we will refer to as "heavy drinking") increases from 2% to 13%.¹⁵ By comparison in 2000, 40% of males aged 18-19 in the U.S. report smoking, 50% report drinking alcohol at all, and about 6% report drinking 15 days a month or more.¹⁶

The survey gathered data on several mechanisms that could underlie the relationship between schooling and health behaviors: disposable income, patience, risk aversion, peer behavior and perceptions of harm associated with smoking and drinking. Boys were asked to report the amount of money that they have in a typical week to spend on themselves for whatever they want. In round 2, they report having about RD\$56 (3.5 U.S. dollars) a month (about the same as average earnings for youths in the sample) and by round 3, they have about 8 (about half of earnings). To understand drinking and smoking behavior, it should be noted that the costs of alcohol and tobacco are relatively low; the average cost of a pack of cigarettes is about RD\$30-45, though they are also commonly purchased individually for closer to \$RD2-4, and a bottle of beer, the most common drink among youths, costs about RD\$30-35 (other common drinks are more expensive, such as vodka (RD\$150-300)). Thus, although disposable income may be low, alcohol and tobacco are certainly affordable to youths.

The survey elicited a measure of patience in both rounds using the following question: "Some people like to have everything now, other people are willing to wait. On a scale of 1 to 5, where 1 is not very patient (you almost always want to have things now) and 5 is the most patient (you are almost always willing to wait), where would you rank yourself?"¹⁷ On average boys report being somewhat patient, but patience decreases substantially by round 3. Attitudes towards risk were only measured in round 3 using the following question: "Some people like to

reports a smoking rate of about 4% (among 18-19 year old boys in urban areas). However, it is worth noting that the DHS and the school-based samples differ. Our survey consists of youths enrolled in 8th grade (which omits the roughly 10 percent of boys not enrolled in school), while the DHS of boys is only those living with a never-married woman aged 15-49.

¹⁵ Drinking rates are extremely high but they are identical to those found in the DHS, and to those reported elsewhere for youths in the Dominican Republic (Dornitzer et al 2004).

¹⁶ Percentages are computed using the 2000 wave of the NLSY97 survey. Sample is restricted to males.

¹⁷ Our measure captures overall patience, though it is possible that individuals' patience or discounting differs across domains, for example individuals may discount money differently from health.

take risks, while others don't. On a scale of 1 to 5, where 1 means you usually like to take risks and 5 means you almost always avoid risk, where would you rank yourself?" Risk taking in this sample is high: about 50% report they usually take risks (category 1), while only 10% choose categories 4 or 5.

The survey also asked about the drinking and smoking behavior of peers in both rounds, using questions very similar to those asked in the NLSY97 in the U.S. In particular, boys were asked: "Now, I would like you to think about the people you spend your time with on a typical day. About what fraction of them would you say [are smokers/drink alcohol]?" The possible responses were: 1. zero (none); 2. more than zero, but less than half (just a few; about a quarter); 3. about half; 4. more than a half, but not all of them (many of them; about three-quarters) ; 5. all of them or almost all of them. In round 2, just over half (53%) of boys report that at least one-half of their peers smoke, and 61% report that at least one-half of their peers drink. These fractions get even larger by round 3 (62% and 94%, respectively).

Finally, the survey asked about the perceived health consequences of smoking and drinking, although only in round 3. In particular, students were asked: "On a scale of 1 to 5, where 1 means very bad and 5 means not at all bad, how bad do you think [smoking/drinking] is for a person's health?" Overall, smoking and drinking are not perceived as very harmful, and in fact drinking is perceived to be more harmful than smoking.¹⁸

Appendix Table A shows the correlations between education and work and our proxies for mechanisms. As expected, those who work have fewer years of schooling, and have more disposable income. Education and work status are also highly correlated with reported peer behavior: work appears to be associated with a larger share of peers that smoke and drink. Patience and risk aversion are associated with more school, as one would expect, but these correlations are small (and they are even smaller for work status).

Correlations with perceptions of harm are very small. The more educated are slightly more likely to report that alcohol is harmful, and surprisingly they are less likely to report smoking is harmful. Perhaps this is due to the fact that in the Dominican Republic there had been

¹⁸ Consistent with our data Dozier et al (2006) in their small survey of smoking among adults in the Dominican Republic, not a single respondent reported that negative health consequences of smoking as a reasons why they never started smoking. Both smokers and non-smokers reported that smoking was harmful, but neither group could identify the specific risks associated with smoking. Vincent et al (1993) also report that in the Dominican Republic respondents were unable to properly answer questions on the harms of smoking, though the vast majority reported that smoking was bad. They also report that a large fraction of physicians smoke (about 35%).

very few campaigns against smoking as of 2005. Other studies suggest that although individuals report smoking is “bad” when prodded, they are very poorly informed about the specific harms associated with smoking (Dozier et al 2006, Vincent et al 1993). The same is not true in the U.S.

C. OLS Results

In order to establish the commonly found relationship between education, work and health behaviors in our data set, and for comparison with the experimental results, we start by estimating OLS regressions of smoking and heavy drinking on education and work status using individuals in the control group only. We include some basic baseline controls: father's education, log of family income and teacher's reports of student performance in school. For each outcome, we estimate three regressions: including education only, work only, and then including them both in addition to our measures of possible mechanisms.

Table 2 shows the results. In round 2, education appears to lower the likelihood of smoking, whereas working increases it. Although neither coefficient is significant, the magnitudes are large: returning to school is associated with a 30% decline in smoking; working is associated with a 50% increase in smoking. These patterns are similar for heavy drinking in round 2. For both behaviors, adding controls for the mechanisms has a large impact, with the coefficients on both school and work falling to close to zero, suggesting these mechanisms can “explain” the effects of education and work on drinking and smoking.

For round 3, we also find that education is associated with lower likelihood of smoking (one more year of schooling lowers smoking by 18%), and that work results in a higher likelihood of smoking (increasing it by almost 80%). But now these effects are statistically significant (when included individually). Adding controls reduces both coefficients by about one-half, but they remain large in magnitude. Working is associated with a greater likelihood of heavy drinking (72% increase), and education appears to *increase* heavy drinking (one more years of school increases heavy drinking by about 10%). Furthermore additional controls increase the magnitude of the coefficients on education and work.

In both rounds and for both smoking and heavy drinking, the fraction of peers that smoke appears to matter. For smoking, patience and risk aversion also matter in round 3, but risk aversion has the “wrong” coefficient, with youths who report being less willing to take risks also being more likely to smoke. In all cases, disposable income and knowledge are not significant.

To see whether the associations between education and health behaviors we estimate are particular to the sample used here, we estimated the same regressions using young, urban males in the 2007 Dominican Republic Demographic and Health Survey (DHS) and young males in the U.S. in the National Longitudinal Survey of Youth 1997 (NLSY97). Work status is not available in the DHS, so we only compare the coefficients on years of schooling across data sets. The DHS also only asks about drinking, not heavy drinking.¹⁹

Appendix Table B reports the results. In spite of the differences in sampling, levels of education and prevalence rates of smoking and drinking, the results show remarkable consistency. In all three data sets, years of schooling lowers smoking but increases the likelihood of drinking, including heavy drinking. The magnitudes of the associations are also quite similar, when measured in terms of percentage changes relative to the sample mean. These results suggest that the patterns for young males in the experimental data are similar to those one would obtain using alternative data sets in the Dominican Republic or in the U.S. However, the main concern, and the motivation for this paper, is that these OLS patterns reflect only associations and cannot be interpreted as causal effects. We now turn to analysis based on the information experiment, and later comment on the extent to which these OLS associations appear to be biased.

D. The Experiment

As discussed in Jensen (2010), both qualitative and quantitative evidence suggests that on average, 8th grade male students in the Dominican Republic had very low perceptions of the returns to finishing secondary schooling. The mean expected return to secondary school was about RD\$330-340 (about US\$21) per month, or about 9.5 percent greater than earnings with only 8 years of schooling (completing primary). This figure is only about one-quarter the size of the actual difference in earnings estimated from a labor force survey conducted by the author. (Estimates using a limited instrumental variables strategy, to address as best as possible potential omitted variables bias, yield even larger estimates of the returns.) Assuming a constant annual increment, student's perceptions represent a 2.3 percent return to an additional year of schooling, which is far below what has been found not just in the labor force survey for the Dominican

¹⁹ Our set of background characteristics is different across data sets—we attempted to control for background as well as we could, but there is no common set of controls available. There are also some differences in how the variables are defined—these are noted in the table notes.

Republic, but also what has been found in almost every other setting, even when possible omitted variables bias has been addressed. (Estimates typically range from 5-10 percent, and are in fact often higher in poorer countries like the Dominican Republic. See Psacharopoulos and Patrinos 2004.)

Jensen (2010) also finds suggestive evidence that the underestimate of the returns may be attributable to students not having information on workers outside of their own communities. As a result, at the end of the survey, each respondent at a randomly selected subset of schools was given information on earnings by education from the household survey, and the absolute and percent return implied by those values:

“Before we end, I would like to provide you with some information from our study. In January, we interviewed adults living in this community and all over the country. We asked them about many things, including their earnings and education. We found that the average earnings of a man 30 to 40 years old with only a primary school education was about 3,200 pesos per month. And the average income of a man the same age who completed secondary school, but did not attend university, was about 4,500 pesos per month. So the difference between workers with and without secondary school is about 1,300 pesos per month; workers who finish secondary school earn about 41 percent more than those who don’t. And people who go to university earn about 5,900 pesos per month, which is about 85 percent more than those who only finish primary school.”

This statement alone constituted the experiment. In this spirit, the experiment is consistent with a number of recent studies which find that providing information can influence actual behavior.²⁰

III. EMPIRICAL STRATEGY AND RESULTS

A. Empirical Strategy

In order to explore the impacts of the intervention on drinking and smoking, we estimate the following model:

$$Y_i = \beta_0 + \beta_1 * Treatment_i + X_i \alpha + \varepsilon_i$$

where Y is an outcome of interest for individual i , and $Treatment$ is a dummy equal to one if the individual was treated. Standard errors are adjusted for clustering at the school level. We present

²⁰ These include: Dupas (2008), who examines risky sexual behavior among Kenyan girls after they are given information on HIV prevalence rates by age; Duflo and Saez (2003), who explore retirement plan decisions when individuals are given incentives to attend a session providing benefit information; Hastings and Weinstein (2008) who study school choice decisions when parents are given simplified information on tests scores and admissions probabilities that was already available; and, mostly closely related to the present case, Nguyen (2008) who studies how school attendance and performance respond to information on the returns to schooling in Madagascar.

regressions with and without additional controls. (Following Jensen 2010, the additional baseline controls we add are father's education, school performance and log of family income.)²¹ Since treatment is orthogonal to these baseline covariates, in principle, their addition should increase precision, but have no effect on the estimated coefficients. All tables report results from OLS regressions regardless of whether the outcomes are continuous or discrete, but non-linear models yield nearly identical conclusions in terms of magnitudes and significance (results available upon request). To gauge the magnitudes of the treatment effect, we compare coefficients to the mean of the control group reported in Table 1.

Because we study a large number of outcomes, in addition to presenting the results for each individual outcome, we present two other statistics. The first is the mean effect of the treatment across outcomes within a similar domain, computed using the methodology described in Kling, Liebman, and Katz (2007): all variables are standardized to have mean 0 and standard deviation of 1, and all outcomes within a domain are redefined so that a higher outcome constitutes an improvement. The average effect is then computed as the unweighted average of the coefficient on treatment on each of the standardized outcomes. The aggregation will improve power if the effect of treatment within a domain goes in the same direction for all outcomes in that domain, and it provides a useful summary statistic. We also provide the p-values of standard *F*-tests: we test the null that the effect of treatment is 0 for all outcomes within a domain.

B. Results: Work and Schooling

Table 3 presents the results for schooling outcomes and for labor force outcomes. Panel A essentially reproduces the results in Jensen (2010) and shows that the intervention was successful in increasing the perceived returns to a secondary degree. Moreover the treated group was about 4 percentage points (7.4%) more likely to return for the next school year after the intervention, and obtained on average about 0.2 more years of school (a 2% increase relative to the control mean). We cannot reject, however, that the treatment did not increase the likelihood of completing secondary school. Overall, the average education effect was large in round 2 (an increase of about one-third of a standard deviation in the index) and somewhat smaller by round 3 (about one-tenth of a standard deviation). For both rounds we reject the null that the treatment had no effect across education outcomes.

²¹ Results are not sensitive to the addition of other baseline controls.

Panel B presents the results for labor market outcomes. The treated were significantly less likely to work in both round 2 and round 3. The effects are large, with an approximately 6 to 7 percentage point (about 18%) reduction in both rounds in the share that report they work. This reduction in work is larger than the increase in the fraction that are in school in round 2, or than the increase in high school graduation in round 3. Thus, the intervention appears to have had an effect on the work decisions of students who would otherwise have stayed in school anyway. (Recall that a non-trivial share of students both attended school and worked.) While we have no direct evidence, it is possible that these students worked less as a result of the intervention because they wanted to increase their schooling effort, either by increasing attendance or devoting more non-school time to studying. This hypothesis is consistent with the results of Nguyen (2008), who finds that school attendance and test scores within a school year increased as a result of a similar information intervention in Madagascar.

The next two outcomes we consider are hours worked per week and total weekly earnings. For those who are not working, hours and earnings are set to zero. The treatment lowered total hours and earnings in both rounds. In round 2, the magnitude of the declines (about 20% for both hours and wages) suggests that the effects are driven mostly by the fact that fewer students are working at all; conditional on working there appears to be only a small decline in hours and wages. (If we run regressions conditional on working, we find small negative and statistically insignificant effects of treatment on hours and wages.)²² In round 3 the reductions are larger than what is suggested by the change in labor supply: hours and wages are lower conditional on working. (Regressions conditional on working suggest as much.) These results are consistent with several explanations however, and could be entirely explained by selection: those who are induced to stay in school and work less are the higher earners. Since we have no way of disentangling these explanations here, we do not pursue them further. But we note that these effects are larger than in the second round, thus the estimated average labor market effect is about half a standard deviation larger in round 3 compared to round 2. At the 10% level, we reject that the effects on labor market outcomes are jointly zero in both rounds and for both specifications. Overall results in this table suggest that the intervention increased schooling and lowered the fraction of boys at work by a significant amount.

²² We do not report these regressions since they condition on an outcome of the experiment, namely working, and thus are difficult to interpret. The results are available upon request.

The fact that our experiment both increased schooling and decreased working means that, without strong assumptions, we cannot isolate the pure effects of schooling on health behaviors, which has been the focus of almost all of the literature, from the pure effects of working on these same behaviors (especially in our case since the intervention had a large effect on work than on education). However, we believe that this is not a unique feature of our data or experiment. Any factor that improves schooling is also likely to reduce working (though of course, the relationship need not be one-for-one). Thus in general, it will almost never be possible to isolate the pure effect of one, since it is difficult to imagine a policy or factor that increases schooling, holding work constant, or vice-versa. So, for our empirical analysis, we will discuss the joint effect of these changes. While this limits our ability to make more precise statements about schooling and these behaviors as others have, that parameter is most likely not as relevant for policy makers, and studies that do report the pure effect of schooling are likely to have inappropriately attributed all the effects exclusively to schooling.

C. Results: Drinking and Smoking

We now turn to our outcomes of interest, smoking and drinking. The results are in Table 4. In round 2 we find a somewhat small (about 8%) and statistically insignificant decline in smoking. On the other hand frequency of alcohol intake significantly declined. We estimate the effects of treatment separately for each level of drinking to see which margin of alcohol intake is driving the reductions—in principle moderate alcohol consumption need not be harmful whereas heavy alcohol consumption is potentially associated with large health effects later in life and large contemporaneous externalities. The largest effects in round 2 are concentrated among the heavy drinkers—the fraction drinking at all decreases by 3%, the fraction drinking at least every week decreases by 13% and the fraction reporting they drink every day falls by about 50%.

In round 3, we find different results. There is a large (27% decline) and statistically significant effect of the treatment on smoking, but the effect on drinking, although negative, is not significant. The effects on drinking are in fact small at every margin: no effect on drinking at all, a 6% decline on drinking at least every week, and a 5% decline on heavy drinking. Together these results suggest that the initial reduction in alcohol intake observed in round 2 is mostly temporary (with perhaps a small but not statistically significant longer-term effect)—thus our results suggest that the intervention increased the age at which boys started drinking more

intensively, not whether they do so by age 18 or above. The results also suggest that OLS estimates presented earlier are biased, in particular the positive association between education and drinking (or heavy drinking) is reversed in the experimental data.

Overall, we find a decline in smoking and drinking in both rounds associated with the intervention, however for smoking the effects are small and not statistically significant in round 2, but large and significant in round 3, while the reverse is true of drinking.

D. Results: Possible Mechanisms

Next we turn our attention to the possible mechanisms that might explain the changes in risky behaviors we observe as well as the time pattern of those changes noted above. Table 5 presents the results. Although we present these as mechanisms, by its nature the experimental evidence cannot tell us which outcomes are intermediate outcomes or mechanisms and which are directly affected by the intervention and cause other subsequent changes in behavior (all outcomes are potentially begin affected simultaneously by the intervention). We return to this issues below.

We start by discussing mechanisms for which we find no effects. Patience, risk and perceptions of the harms of drinking and smoking are not affected by the intervention—the treatment effects for these outcomes are small and statistically insignificant. Figure 1 plots the distributions of these outcomes in Round 3. Patience decreases, though the magnitudes are very small and the effects are not statistically significant. The treatment tended to decrease risk aversion overall, although detailed examination of Panel B in Figure 1 suggests that in fact the intervention pushes youths more towards the tails, but more so in the direction of increasing risk taking. Overall, this evidence does not support Becker and Mulligan (1997)’s hypothesis that schooling affects behavioral parameters. There were also no effects of the intervention on perceptions of health consequences of these two behaviors. The intervention does make boys more likely to think smoking is harmful, but less likely to think drinking is harmful, however in both cases the effects are small and insignificant. Though we find no effects on patience, risk and perceptions of harms, it is worth re-iterating that our measures are imperfect.

Two of the possible mechanisms we examine do in fact change very significantly: disposable income and the behavior of peers. Disposable income falls only slightly in round 2 (7%, but not statistically significant), but a great deal in round 3 (38%). There are also significant

effects on both the fraction of peers that smoke and drink in round 2. But by round 3, only the fraction of peers that smoke is different for the treatment and control groups. Figure 2 shows more clearly the changes in the distributions of peer behaviors. In round 2, the treated are much less likely to report that more than half of their peers drink. But by round 3, both treated and control report almost all of their peers drink. For smoking, the distribution is shifted by the treatment in both rounds, but the effects in round 3 are concentrated in the upper tail: the treated are less likely to report that more than half of their peers smoke.

Jointly, these results suggest that peer effects may be the most relevant mechanism explaining how drinking is influenced by school and work. In round 2, alongside the increase in drinking we observe increases in the fraction of peers that drink, particularly increases in the fraction for whom at least half their friends drink, but no changes in any of the other mechanisms. By contrast, in round 3, there is no change in the fraction of peers that drink, and accordingly we do not observe increases in drinking, despite the large increase in disposable income. These results are consistent with Kremer and Levy (2008), who find that students who are randomly assigned to a roommate who drank prior to college were more likely to drink and had lower GPA than those whose roommate did not drink.

For smoking, there are at least two mechanisms that appear to matter: both the fraction of one's peers that smoke and disposable income. In round 3, there are large increases in disposable income and declines in the fraction of peers who smoke, especially declines in youths who say at least half their friends smoke, and accordingly a large decline in smoking. By contrast, in round 2, there were no effects on disposable income and small effects on the fraction of peers that smoke (in particular, no change in the fraction for whom at least half their peers smoke), and accordingly no (small) change in smoking behavior.

Together these results suggest that education and work matter in part because of peer effects. They also suggest that there may be a threshold effect in peer influences on these behaviors: what matters is whether the majority of peers is engaged in the behavior, which could suggest that perhaps effects such as peer pressure become more relevant when a non-drinker or smoker moves from the majority to the minority among their peers. Although in other context peer effects might operate in different directions (school could result in higher fraction of peers behaving poorly), the results do suggest a mechanism that is likely to operate elsewhere.

For smoking the results also suggest that disposable income is important, and that in fact it is harmful (at least in the short run): having more money leads you to smoke more (previous literature in the US using regressions finds similar results, Gruber and Zinman 2001 find similar results among high school seniors in the US). This is consistent with smoking being a normal good (if we assign all of the changes in smoking to the effects of income the implied elasticity of smoking with respect to income is around 0.4),²³ and suggests why in other contexts income is sometimes associated with worse rather than better health, in particular once education is accounted for (Ruhm, 2002, Ruhm and Black, 2005, Grossman 2005). Interestingly, because in this context we are studying short-term effects, more education comes with smaller (not larger) incomes (and lower smoking rates). This suggests that at least some of the association between education and health (or health behaviors) in other contexts is not driven by income.

Interestingly income does not appear to affect drinking, although presumably drinking is also a normal good.²⁴ It is not entirely clear why this is the case. We hypothesize that because drinking is so common among both teenagers and adults, it is often shared and thus it is more easily available in social contexts where individuals can drink and do not actually have to pay for alcohol. By contrast, cigarettes are perhaps less likely to be provided or distributed in these same social contexts.

It is worth mentioning that we cannot rule out that there are additional mechanisms at play, for example education has been hypothesized to also affect self-esteem, self-control and other personality traits (e.g. Ross and Mirosvky 1999). In our experiment in particular, given that school attendance and perhaps studying intensity are increasing, there could be increases in cognition that also explain the observed changes in risky behavior. Also since the intervention very significantly affected the perceived returns to school, it is possible that many of the changes

²³ Indeed in most countries the income elasticity of smoking is positive (for example see Selvanathan and Selvanathan 2005). Among youths in the US Chaloupka and Grossman (1996) estimated the elasticity of smoking participation with respect to income to be around 0.14, and more recently Markovitz and Tauras (2009) report an elasticity of smoking participation with respect to parental allowances (which they consider less likely to be endogenous than other income sources for youth) between 0.015 and 0.26. Our estimates are larger, but they are likely to be over-estimates of the true elasticities, given that other factors (not just income) are changed by the experiment.

²⁴ Again alcohol consumption is a normal good in most countries; see Selvanathan and Selvanathan (2005), Ruhm (1995)—though we know of no estimates for youths' heavy drinking. Markovitz and Tauras (2009) report an elasticity of drinking participation (not heavy drinking) with respect to parental allowances between 0.13 and 0.26. Our estimates imply an income elasticity of everyday drinking with respect to income of about 0.2 (if we ascribe all of the changes in drinking to changes in income), consistent with these previous findings (although we are looking at heavy drinking), though our coefficients for drinking in the second period are not statistically significant. Also, as is the case with smoking, our elasticity estimate is most likely upwards biased.

we observe are driven by the fact that the expected present discounted value of earnings has increased. So in principle, behaviors may have changed even among those whose schooling and work behaviors were unaffected. However given that information about smoking and drinking appears to be so minimal, it is not clear why increases in cognition or in future earnings would lead to changes in behavior if these behaviors are not perceived as harmful. So in this context these explanations are less convincing, but we cannot rule them out.

Finally, for peer effects, we should point out potential limitations to our interpretation. First, while we have interpreted changes in peers as having caused the changes in drinking and smoking, it may well be that drinking or smoking changed for other reasons and that people who drink or smoke seek out peers who do likewise. Second, the adolescent alcohol and tobacco use literature finds that adolescents who smoke or drink overestimate smoking and drinking by their peers compared to adolescents who do not smoke or drink. (See Norton, Lindrooth and Ennett 2002 for citations.) In this case, we again would not be able to causally attribute the changes in drinking and smoking to changes in peer group behaviors.

IV. DISCUSSION AND CONCLUSION

We find that the intervention, which increased schooling and decreased work, lead to a reduced likelihood of smoking, and a later onset of heavy drinking. These effects, both the delayed onset as well as the reduced overall likelihood, suggest important private and social gains both directly in terms of health, as well as indirect gains from externalities associated with these behaviors (such as second-hand smoke or drunk driving).

We also find evidence that suggests these effects may come from the effect of schooling and work on peers, as well as disposable income, but are unlikely to arise from changes in attitudes towards risk, patience or perceptions of the potential harms of drinking or smoking. The value of our study is that the intervention that increased schooling was randomized, and thus common concerns about omitted variables bias or reverse causality are not relevant.

However, there are three key limitations to our study. First, like virtually all other studies of drinking and smoking or other health-related behaviors, we rely on self-reports. Students may intentionally misreport such behaviors. However in our study the key issue is whether the treatment changes the likelihood of underreporting. For example youths that remain in school could be more likely to underreport smoking and drinking because they are more aware that

these are considered social ills, especially for youths. However, given that the treatment did not change reported perceptions of the harmful health effects of these behaviors, this possibility seems less likely.

Second, the variation in schooling we observe is driven by changes in the perceived returns to schooling. Higher perceived returns to schooling may impact health-related behaviors not just because of its effects on schooling, but because higher expected earnings in the future increase the returns to good health practices that extend the number of healthy working years an individual can expect. Although we cannot rule out this mechanism, this seems unlikely to be a first order concern, especially in our setting, because in our setting individuals do not seem to perceive these behaviors as particularly harmful and furthermore these perceptions of harm were not affected by the intervention. Raising the present discounted value of earnings could affect also smoking and drinking because these are normal goods. Thus our experiment in some sense underestimates the effects of changing current income and peers (if these are indeed the mechanisms at play) because we are not holding future income constant.²⁵ However, this concern is not unique to our study; the few other randomized interventions that have successfully increased schooling have done so by providing incentives (such as financial aid or mentoring) which are likely to have large direct effects on smoking and drinking. In addition, it is likely that gains in education are in fact often driven by increases in the returns, so in terms of external validity, our experiment may not be quite as exceptional and can thus provide valuable insight into the impact of schooling on drinking and smoking (since it may not be possible or realistic to discuss an increase in schooling, holding the returns constant). However, we cannot state definitively that increases in schooling driven by, say, enforcement of compulsory schooling laws or relaxing of credit constraints, will have the same impact.

Third, our intervention specifically attempted to increase secondary schooling and students were not followed beyond age 18. It took place in schools in the Dominican Republic between 2001 and 2005 and we only included boys in our sample. Thus we cannot generalize about the effects of post-secondary schooling, the effects of schooling in other countries, nor can we be sure that the patterns we observe for boys would hold among girls. However this is compensated by the strength of the causal evidence we can provide, and by the fact that we

²⁵ If this mechanism is at play then one would also expect that increases in schooling driven by for example compulsory legislation would have larger effects on smoking and drinking.

attempt to explore mechanisms which in theory are likely to operate in many settings as well. We simply note here that the few existing studies of drinking and smoking among youth, across countries, suggest more similarities than differences across countries and across genders (WHO 2001, Global Youth Tobacco Survey Collaborative Group 2002, 2003). Our own OLS estimations suggested that the associations we observed in the Dominican Republic were in fact very similar to those one would see in the U.S.

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Table 1: Summary Statistics—Dominican Republic survey of young males

	ALL			Control		Treated	
	Obs	Mean	s.d	Mean	s.d.	Mean	s.d.
<u>Socioeconomic characteristics</u>							
Dad's education	2250	0.37	0.48	0.37	0.48	0.37	0.48
log(family income—round 2)	2250	8.15	0.30	8.15	0.30	8.14	0.30
log(family income—round 2) missing	2250	0.06	0.24	0.06	0.24	0.06	0.24
Performance at school	2250	1.36	1.32	1.37	1.33	1.35	1.31
<u>Outcomes Round 2</u>							
Δ return to secondary education	1859	272	616	88	423	448	713
Returned to school	2250	0.58	0.49	0.56	0.50	0.60	0.49
Currently works	2250	0.30	0.46	0.33	0.47	0.27	0.44
Number of hours works per week, all	2111	4.06	8.93	4.61	9.94	3.5	7.7
Total earnings per week, all	2111	67.7	172.5	77.6	195	58	147
Currently smokes	2111	0.05	0.21	0.05	0.22	0.04	0.20
Drink frequency (1=never, 4=every day)	2111	1.96	0.69	1.99	0.71	1.93	0.69
Currently drinks alcohol	2111	0.75	0.43	0.76	0.42	0.74	0.44
Drinks at least once a week, all	2111	0.19	0.40	0.20	0.40	0.18	0.39
Drinks every day, all	2111	0.02	0.13	0.02	0.14	0.01	0.11
Disposable income per week	2111	56.4	71.2	58.9	73.6	53.9	68.6
Patient (1=not patient, 5=most patient)	2250	3.10	1.14	3.14	1.14	3.05	1.13
Around smokers (1=0, 5=1)	2111	2.68	1.13	2.76	1.14	2.61	1.13
Around drinkers (1=0, 5=1)	2111	3.32	1.27	3.39	1.28	3.26	1.27
<u>Outcomes Round 3</u>							
Finished High School=1	2250	0.31	0.46	0.30	0.46	0.32	0.47
Completed years of school	2074	9.85	1.77	9.76	1.78	9.95	1.77
Currently works=1	2250	0.36	0.48	0.40	0.49	0.32	0.47
Number of hours works per week, all	2011	5.95	11.59	7.01	13.08	4.9	9.8
Total earnings per week, all	1826	276	871	337	988	216	735
Currently smokes=1	2011	0.13	0.34	0.14	0.35	0.12	0.32
Drink frequency (1=never, 4=every day)	2011	2.37	0.95	2.40	0.96	2.35	0.95
Currently drinks alcohol=1	2011	0.79	0.41	0.79	0.41	0.79	0.41
Drinks at least once a week=1, all	2011	0.46	0.50	0.47	0.50	0.44	0.50
Drinks everyday=1, all	2011	0.13	0.33	0.13	0.34	0.12	0.33
Disposable income per week	1832	133.1	408.4	162.5	472.7	104.3	332
Patient (1=not patient, 5=most patient)	2011	2.06	1.14	2.09	1.14	2.03	1.13
Risk averse (1=always take risks, 5=never)	2011	1.98	1.20	1.98	1.19	1.99	1.21
Around smokers (1=0, 5=1)	2011	3.31	1.24	3.43	1.21	3.20	1.25
Around drinkers (1=0, 5=1)	2011	4.48	1.00	4.50	1.00	4.46	1.00
Smoking is bad (1=very bad, 5=not bad)	2250	3.67	0.68	3.68	0.68	3.67	0.68
Drinking is bad (1=very bad, 5=not bad)	2250	2.50	0.63	2.49	0.63	2.50	0.64

Sample consist of males enrolled in 8th grade that were in school in April-May of 2001. Table reports unweighted means.

Table 2: OLS Regressions (control group only)

Outcome	Smokes=1 Round 2			Drinks everyday=1 Round 2			Smokes=1 Round 3			Drinks everyday=1 Round 3		
Years of School, round 3							-0.031**	-0.017*	0.012	0.033**		
							[0.007]	[0.010]	[0.008]	[0.007]		
Return to school, round 2	-0.016	0	-0.014	0.011								
	[0.015]	[0.018]	[0.011]	[0.014]								
Works=1	0.025	0.005	0.040**	0.019	0.110**	0.047	0.094**	0.122**				
	[0.015]	[0.021]	[0.015]	[0.018]	[0.033]	[0.040]	[0.028]	[0.042]				
Kid's disposable income		0.008		-0.002		0.008		0.004				
		[0.016]		[0.014]		[0.005]		[0.004]				
Patient		-0.004		0.002		0.021**		0.005				
		[0.006]		[0.004]		[0.010]		[0.011]				
Risk Aversion						-0.022**		0.004				
						[0.010]		[0.008]				
% peers smoke round 2		0.011		0.027**		0.054**		0.046**				
		[0.011]		[0.010]		[0.022]		[0.018]				
% peers smoke round 3						-0.032		0.003				
						[0.026]		[0.016]				
% peers drink round 2		0.003		-0.004		0.009		-0.008				
		[0.009]		[0.004]		[0.014]		[0.010]				
% peers drink round 3						0		0.012				
						[0.013]		[0.009]				
Smoking bad (1: bad, 5: no)						-0.008		0.014				
						[0.017]		[0.018]				
Drinking bad (1:bad, 5: no)						0.014		0.017				
						[0.021]		[0.016]				
Dad's education	0.006	0.005	0.006	-0.016**	-0.017**	-0.016**	-0.008	-0.012	-0.011	-0.018	-0.016	-0.009
	[0.018]	[0.018]	[0.017]	[0.007]	[0.008]	[0.008]	[0.032]	[0.031]	[0.031]	[0.029]	[0.028]	[0.026]
log(family income)	-0.042	-0.038	-0.039	0.017	0.023	0.03	-0.015	-0.015	0	0.082*	0.113**	0.101**
	[0.031]	[0.031]	[0.030]	[0.014]	[0.016]	[0.021]	[0.054]	[0.052]	[0.051]	[0.046]	[0.047]	[0.045]
Performance in school	0.008*	0.008*	0.008*	-0.002	-0.003	-0.002	-0.016	-0.019*	-0.017	0.008	0.008	0.005
	[0.005]	[0.005]	[0.004]	[0.003]	[0.003]	[0.003]	[0.011]	[0.011]	[0.011]	[0.008]	[0.008]	[0.008]

Notes: Heteroskedasticity-consistent standard errors accounting for clustering at the school-level in parentheses.

*Significant at 10 percent level. **Significant at 5 percent level. ***Significant at 1 percent level.

Table 3: Effect of intervention on education and labor market outcomes

Specification	Round 2		Round 3	
	Basic	SES controls	Basic	SES controls
<u>Panel A: Effects on schooling</u>				
Δ returns to sec. deg.	366** [30.4]	366*** [30.5]		
Returned to school	0.039 [0.025]	0.041* [0.023]		
Finished High School=1			0.02 [0.025]	0.023 [0.021]
Completed years of school			0.180* [0.102]	0.202** [0.085]
<i>Average education effect</i>	<i>0.336**</i> <i>[0.038]</i>	<i>0.338**</i> <i>[0.037]</i>	<i>0.072</i> <i>[0.055]</i>	<i>0.082*</i> <i>[0.045]</i>
<i>p-value (joint F-test, treatment =0)</i>	<i>0.000</i>	<i>0.000</i>	<i>0.035</i>	<i>0.014</i>
<u>Panel B: Effects on labor market outcomes (all)</u>				
Currently works=1	-0.059** [0.023]	-0.062** [0.021]	-0.067** [0.024]	-0.071*** [0.022]
Hours works per week, all	-0.878** [0.432]	-0.931** [0.414]	-1.77** [0.610]	-1.92** [0.577]
Total earnings per week, all	-15.940* [8.134]	-16.833** [7.873]	-118.2** [45.007]	-126.49** [43.548]
<i>Average labor market effect</i>	<i>-0.106**</i> <i>[0.045]</i>	<i>-0.112**</i> <i>[0.042]</i>	<i>-0.143 **</i> <i>[0.045]</i>	<i>-0.153**</i> <i>[0.041]</i>
<i>p-value (joint F-test, treatment=0)</i>	<i>0.097</i>	<i>0.042</i>	<i>0.022</i>	<i>0.005</i>

Notes: Heteroskedasticity-consistent standard errors accounting for clustering at the school-level in parentheses. "SES control" regressions include: age, SES of family, dummy for SES missing, father's education and school performance.

*Significant at 10 percent level. **Significant at 5 percent level. ***Significant at 1 percent level.

Table 4: Effect of Intervention on smoking and heavy drinking

Specification	Round 2		Round 3	
	Basic	SES controls	Basic	SES controls
Currently smokes=1	-0.003 [0.011]	-0.004 [0.011]	-0.037** [0.018]	-0.039** [0.018]
Drink frequency (1=don't drink, 4=drink every day)	-0.063** [0.031]	-0.063** [0.031]	-0.035 [0.040]	-0.032 [0.041]
Currently drinks alcohol	-0.025 [0.020]	-0.025 [0.020]	0.003 [0.018]	0.004 [0.018]
Drinks at least once a week	-0.028* [0.017]	-0.027 [0.017]	-0.031 [0.023]	-0.03 [0.023]
Drinks everyday	-0.010* [0.006]	-0.010* [0.006]	-0.007 [0.016]	-0.006 [0.016]
<i>Average behavior effect (smoking and drinking)</i>	<i>-0.053 [0.033]</i>	<i>-0.027 [0.017]</i>	<i>-0.076** [0.031]</i>	<i>-0.075** [0.032]</i>
<i>p-value (joint F-test, treatment=0)</i>	<i>0.12</i>	<i>0.12</i>	<i>0.065</i>	<i>0.055</i>
<i>Average smoking and heavy drinking effect</i>	<i>-0.047 [0.034]</i>	<i>-0.048 [0.034]</i>	<i>-0.066** [0.030]</i>	<i>-0.068** [0.031]</i>
<i>p-value (joint F-test, treatment=0)</i>	<i>0.224</i>	<i>0.223</i>	<i>0.067</i>	<i>0.055</i>

Notes: Heteroskedasticity-consistent standard errors accounting for clustering at the school-level in parentheses. "SES control" regressions include: age, SES of family, dummy for SES missing, father's education and school performance. *Significant at 10 percent level. **Significant at 5 percent level. ***Significant at 1 percent level.

Table 5: Effect of intervention on possible mechanisms

Specification	Round 2		Round 3	
	Basic	SES controls	Basic	SES controls
Disposable income	-4.012 [3.289]	-3.73 [3.206]	-59.229** [21.898]	-61.047*** [21.861]
Patient (1=not patient, 5=most patient)	-0.053 [0.048]	-0.057 [0.047]	-0.025 [0.050]	-0.033 [0.048]
Risk (1=always takes risks, 5=never takes risks)			-0.011 [0.056]	-0.003 [0.056]
% peers smoke (1=0, 2=[0-0.49], 3=0.5, 4=[0.51-.99] 5=1)	-0.166** [0.061]	-0.173** [0.056]	-0.211*** [0.072]	-0.231*** [0.062]
% peers drink (1=0, 2=[0-0.49], 3=0.5, 4=[0.51-.99] 5=1)	-0.144** [0.066]	-0.151** [0.062]	-0.016 [0.043]	-0.022 [0.043]
Smoking is bad (1=very bad, 5=not bad at all)			-0.03 [0.026]	-0.031 [0.026]
Drinking is bad (1=very bad, 5=not bad at all)			0.009 [0.027]	0.011 [0.027]
<i>Average effect on mechanisms</i>	<i>0.039</i> <i>[0.025]</i>	<i>0.042*</i> <i>[0.023]</i>	<i>0.001</i> <i>[0.015]</i>	<i>0.002</i> <i>[0.015]</i>
<i>p-value (joint F-test, treatment=0)</i>	<i>0.095</i>	<i>0.032</i>	<i>0.108</i>	<i>0.014</i>
<u><i>Average effect overall all outcomes</i></u>	<i>0.077**</i> <i>[0.020]</i>	<i>0.080**</i> <i>[0.017]</i>	<i>0.030*</i> <i>[0.017]</i>	<i>0.034**</i> <i>[0.015]</i>
<u><i>p-value (joint F-test, treatment=0)</i></u>	<i>0.0000</i>	<i>0.0000</i>	<i>0.357</i>	<i>0.100</i>
<u><i>All outcomes, all rounds</i></u>	<i>0.0000</i>	<i>0.0000</i>		

Notes: Heteroskedasticity-consistent standard errors accounting for clustering at the school-level in parentheses. "SES control" regressions include: School performance, father's education and income *Significant at 10 percent level. **Significant at 5 percent level. ***Significant at 1 percent level.

Figure 1: Effects of intervention on patience, drinking and perceptions, Round 3

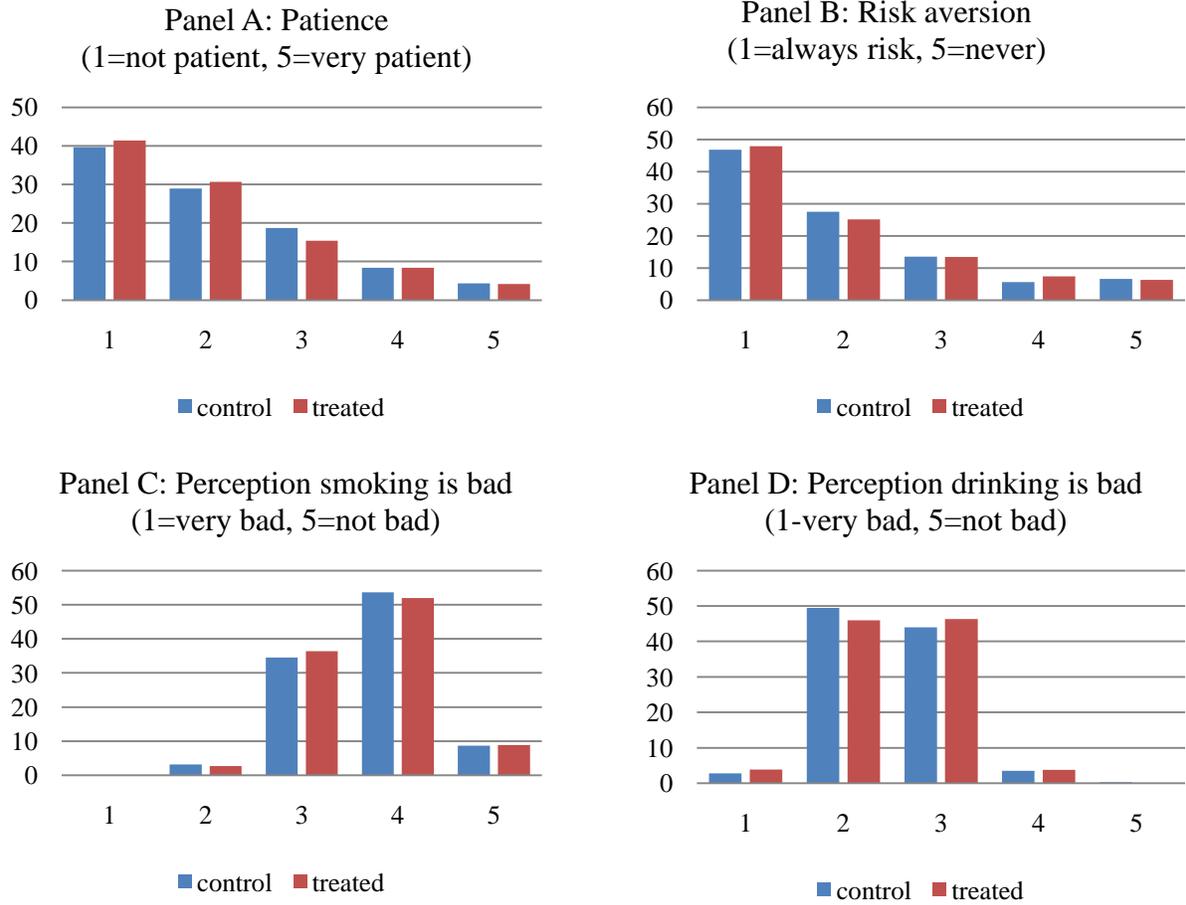
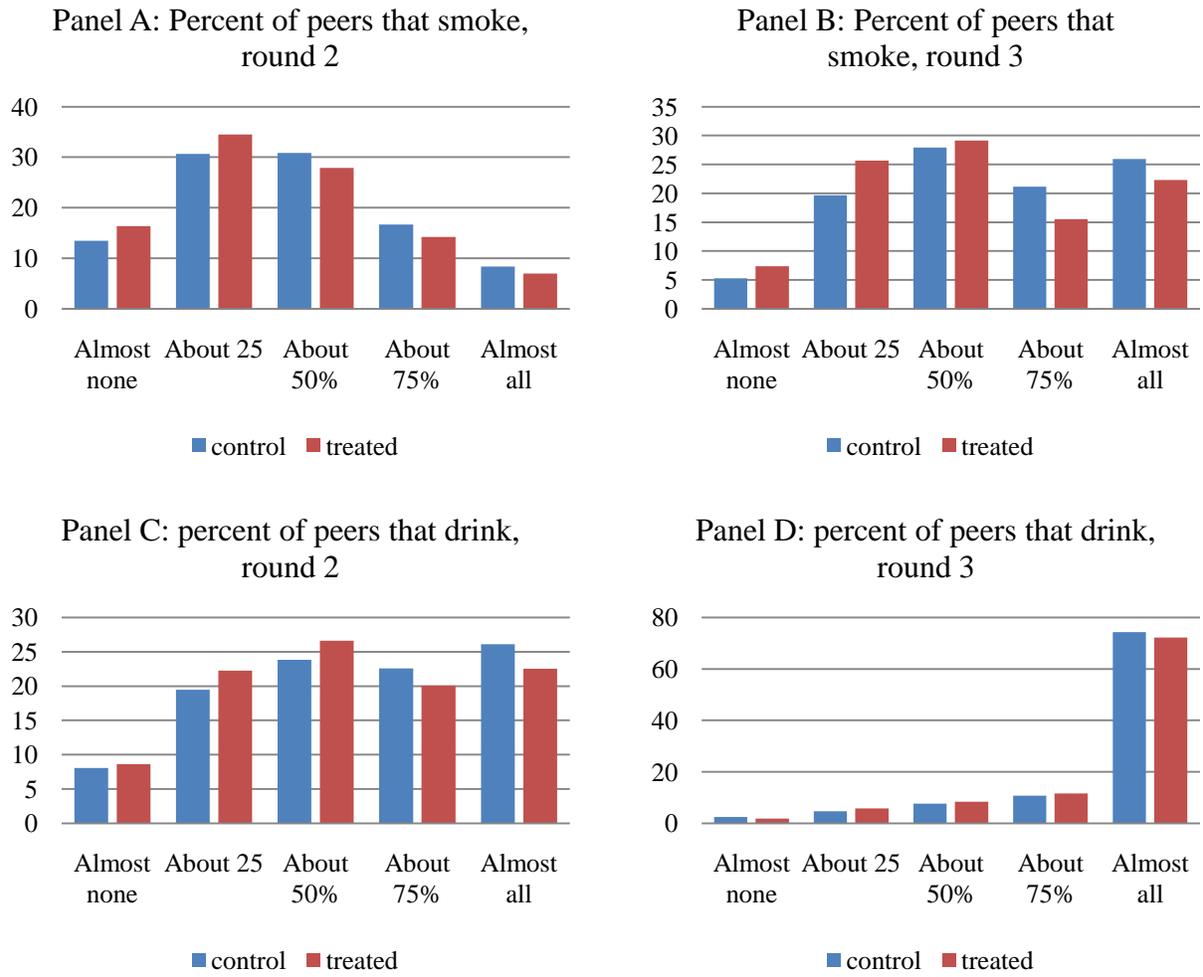


Figure 2: Effect of the intervention on fraction of peers that smoke and drink.



Appendix Table A: Correlation between schooling, work and possible mechanisms in Round 3. Control group only.

	Years of school	Works
Works	-0.2681	1
Disposable income	-0.2121	0.4691
Patient (1=not patient, 5=most patient)	0.1015	-0.0081
Risk aversion (1=always takes risks, 5=never takes risks)	0.0762	-0.0359
Around smokers (1=0, 2=[0-0.49], 3=0.5, 4=[0.51-.99] 5=1)	-0.5237	0.6383
Around drinkers (1=0, 2=[0-0.49], 3=0.5, 4=[0.51-.99] 5=1)	-0.131	0.1623
Knows smoking is bad (1=very bad, 5=not bad at all)	0.066	-0.0324
Knows drinking is bad (1=very bad, 5=not bad at all)	-0.0432	-0.0068

Sample corresponds to all non-treated individuals interviewed in round 3 with non-missing observations for the variables of interest. There are 906 observations.

Appendix Table B: Effect of education on health behaviors, experimental data, DHS and NLSY97 compared

	years of education		N	Mean	s.d	% effect
	coeff	s.e				
<u>Panel A: Experimental data (control group, R3)</u>						
(mean years of education 9.76, s.d 1.78)						
Currently smokes	-0.031***	[0.007]	1006	0.14	0.35	24%
Current drinker*	0.034**	[0.007]	1006	0.79	0.41	2%
Heavy drinker (drinks everyday)	0.012	[0.008]	1006	0.13	0.34	9%
<u>Panel B: young males in urban areas in the DHS</u>						
(mean years of education 9.79, s.d. 1.76)						
Currently smokes	-0.003***	[0.001]	3795	0.026	0.158	12%
Current drinker*	0.003	[0.003]	3148	0.770	0.421	0%
Heavy drinker (drank 45+ days last 3 months)	-0.001	[0.001]	3148	0.025	0.157	4%
<u>Panel C: Young males in the US, NLSY97</u>						
(mean years of education 10.74, s.d. 1.5)						
Currently smokes	-0.082***	[0.006]	4041	0.339	0.458	24%
Current drinker*	0.008	[0.007]	4069	0.458	0.498	2%
Heavy drinker (drank 15+ days last month)	0.004	[0.003]	4069	0.045	0.206	9%

Experimental regressions include age, SES of family, dummy for SES missing, father's education and school performance. Sample: males in control group, third round of survey data. Coefficients were estimated using linear regression. Sampling and clustering accounted for.

DHS regressions includes age and gender of household head, number of individuals in household, dummies for year of birth (6), month of birth (11), interview month (5), relationship to household head (9), region (31), wealth index (4), and urban status. Sample: males ages 15-20 from the Demographic and Health Survey 2007. We dropped individuals with missing education, missing information on age of household head's age or relationship to household head.

NLSY97 regressions are run using smoking and drinking in the 2000 wave of the survey. Controls include year and month of birth dummies, ethnicity and race dummies, mom and dad's education and dummies for citizenship status.

*questions not exactly comparable: in DHS current drinker equals one if the individual drank any day in the last 3 months. Heavy drinking was defined as drinking 45 days or more in the last 3 months, to make results comparable to those in the NLSY97. In the NLSY97 current drinking is set to one if individual reports having drunk in the last 30 days of the survey. Heavy drinking is defined in the NLSY97 as drinking 15 days or more in the last month because there are only 34 individuals in the NLS97 that report drinking every day.