DOES STAYING IN SCHOOL (AND NOT WORKING) PREVENT TEEN SMOKING AND DRINKING?[*]

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Abstract: Previous work suggests but cannot prove that education improves health behaviors. We exploit a randomized intervention that increased schooling (and reduced working) among male students in the Dominican Republic, by providing information on the returns to schooling. We find that treated youths were much less likely to smoke at age 18 and had delayed onset of daily or regular drinking. The effects appear to be due to changes in peer networks and disposable income. We find no evidence of a direct impact of schooling on rates of time preference, attitudes towards risk or perceptions that drinking or smoking are harmful to health, though our measures of these factors are more limited.

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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 year 2000, one more year of schooling was associated with approximately one more year of life expectancy in the U.S. More educated individuals are also less likely to smoke or drink excessively, and in general have better health-related behaviors (see Cutler and Lleras-Muney 2008 for a review). However, there is considerable debate about whether these associations reflect causal effects. The correlation may instead be driven by omitted variables bias; for example, high discount rates can influence both schooling and health-related behaviors, since both require forgoing utility today in return for future benefits (higher wages or better health). Alternatively, causality may be reversed; for example, students who drink regularly may not perform well in school and therefore drop out, be held back or expelled. A number of recent studies have addressed these concerns using instrumental variables and/or natural experiments, such as changes in compulsory schooling laws, but they have yielded mixed evidence on the health-education relationship.[1] However, these studies generally suffer from two primary concerns. First, they rely on difficult to test identifying assumptions. Second, most do not explore the mechanisms that explain *why* schooling affects health behaviors.

In this paper, we take advantage of a unique panel data set and a randomized intervention in the Dominican Republic to overcome these two challenges. The intervention provided students with information on the returns to schooling. While the standard model of human capital suggests that education responds to the returns to schooling, it is the returns *perceived* by decision-makers that matter, not the returns measured by economists (Manski 1993). And there are many reasons to believe that students, particularly in developing countries, may not be well-informed of the true returns.[2] For example, youths in rural communities or small towns where

[1] Using instrumental variables, Lleras-Muney (2005) and Deschesnes (2007) find that education lowers mortality, but Albouy and Lequien (2009) and Clark and Royer (2009) 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.

[2] The fact that decision-makers may not be well-informed has been explored in several contexts. For example, in exploring the choice of sexual partner in Kenya, Dupas (2010) finds that teenage boys and girls are not well-informed about the difference in HIV prevalence rates between younger and older men. Viscusi (1990) finds that individuals overstate the risks of lung cancer from smoking, and that these misperceptions reduce smoking behavior.

few adults have any education will have little information from which to infer the returns, including the returns in the urban sector. If students in particular underestimate the returns to schooling, the provision of information on the true returns alone may affect schooling. Consistent with this hypothesis, Jensen (2010) finds that 8th grade male students in the Dominican Republic significantly underestimate the returns to schooling. And students at randomly selected schools who were provided with information on the measured returns completed on average 0.20 years more schooling over the next four years than those who were not given this information. To the extent that this intervention affects drinking and smoking only through the impact on schooling (a point we discuss below), the random assignment in this experiment provides an exogenous shock to schooling, uncorrelated with omitted variables and with a clear direction of causality, with which to identify the relationship between schooling and alcohol and tobacco use.

The survey also collected data on a number of potential determinants of drinking and smoking that will allow us to understand the mechanisms through which schooling affects these behaviors. The mechanisms we explore are the behavior of peer networks, discretionary income, rates of time preference, attitudes towards risk, and perceptions of the health consequences of these behaviors. Although not an exhaustive list of mechanisms, these are some of the most commonly cited in the literature (Cutler and Lleras-Muney 2008). We briefly explain how schooling would affect behaviors though each. First, youths who drop out of school and enter the labor market will have more income at their disposal, which makes it easier to afford alcohol and tobacco. Second, schooling may change the youths' peer sets. Youths who stay in school will spend a significant fraction of their time with peers who are also in school, and thus for example of a similar age. By contrast, those who drop out may spend more time with older people, such as in the workplace. Third, schooling may directly affect knowledge of the health risks of drinking and smoking. For example, high schools may have required "health" classes that provide information on the health risks. Health knowledge could also be indirectly affected, if more schooling results in exposure to more and/or different sources of health information. Fourth, schooling may affect an individual's rate of time preference or attitude towards risk (Fuchs 1982), because for example schooling instills particular values, and it requires discipline

And several studies find that individuals are poorly informed of their own pension or social security benefits (Mitchell 1988; Gustman and Steinmeier 2005; Chan and Stevens 2008).

and patience. In light of this discussion, we note that it is not likely to be possible or even meaningful to talk about the pure effects of schooling on health behaviors, even though most of the literature does not account for this distinction; leaving school in some but not all cases means entering the labor force, and it may be that it is what happens in the workplace, rather than what happens in school, that affects whether a teen drinks or smokes. Thus, our analysis will focus on the combined effects of work and school.

Our study focuses on smoking and drinking (especially daily or regular drinking). These behaviors are two of the most important risk factors in explaining early mortality, accounting for about 12 percent of deaths worldwide (WHO 2006). Additionally, drinking and smoking are significant policy concerns because of the externalities associated with their consumption, such as second-hand smoke. Excessive drinking is also associated with increases in deaths from accidents (such as motor vehicle injuries) and crime, especially among adolescents. Our data focuses on the period of adolescence, the period during which both drinking and smoking typically start; most individuals have already tried alcohol by their early teens,[3] and most adult smokers begin smoking before the age of 18.[4] Thus, this age range is a particularly important one to study. And there are several other reasons why smoking and excessive drinking among teens is of particular concern. First, the health consequences of these behaviors are a function of exposure, so even just delaying initiation will also delay the onset of the adverse health consequences, and thus increase life expectancy. Second, delaying initiation of smoking reduces the likelihood of ever smoking (Gruber and Zinman 2001, Auld 2005).[5] Finally, recent evidence from the medical and biology literature suggests that brain development, which is not complete during the teen years, is sensitive to alcohol and nicotine; thus drinking and smoking among teens may have more severe long-term impacts, even compared to such behaviors among adults.[6] To the extent that adolescents aren't fully informed of these costs or are unable to make fully

[3] In the U.S. the average age of first alcohol use is 13.1, and in a survey of 23 European countries more than half of 11 year olds reported having tried alcohol, and a few existing surveys suggests that alcohol consumption begins even earlier in developing countries (WHO 2001).

[4] 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, the median country had 33% of students ages 13-15 smoke (Global Youth Tobacco survey collaborative Group 2002).

[5] Longitudinal studies have also found links between age of drinking initiation and adult alcohol dependence and drinking behavior (see WHO 2001 for a summary).

[6] A 2004 volume (1021) of the *Annals of the New York Academy of Sciences* contains several examples.

rational decisions,[7] preventing adolescents from engaging in these behaviors is a worthwhile public policy goal.

Our results show that in addition to increasing schooling and decreasing work, youths who received the treatment were significantly less likely to smoke four years later, and experienced daily or weekly drinking at a later age. These changes appear to be due to the effects of school and work on exposure to peers that drink and smoke as well as the amount of disposable income the youth has. The changes do not appear to be driven by any direct impact of school or work on rates of time preference, attitudes towards risk or perceptions of the adverse health consequences of these behaviors; however, our measures of these factors are limited and imperfect, so we cannot conclusively rule out such effects.

The remainder of this paper proceeds as follows. In section II, we discuss the data and experimental design. Section III presents the results and section IV discusses the limitations of our study and concludes.

II. DATA AND EXPERIMENTAL DESIGN

A. Survey Information

The sample was drawn in two stages. First, from the 30 largest cities and towns (representing about two-thirds of the population), we chose 150 sampling clusters at random, with the number of clusters chosen in each city or town approximately proportional to its share of the combined population of the 30 cities/towns. For each of the 150 clusters, we selected the school where students from that cluster attend 8th grade, the final year of primary school. From each school we selected 15 boys[8] at random from a list of all currently enrolled 8th grade students.[9] All 2,250 students were administered a survey between April and May 2001, close to the end of the academic year, gathering information on a variety of individual and household characteristics, as well as some simple questions on expected earnings by education.

A second survey was conducted shortly after the beginning of the next academic term (October, 2001), with respondents interviewed again (at home, school or work) about perceived

[7] For example, Gruber and Zinman (2001) find that adolescents under-estimate how addictive smoking is.
[8] 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.
[9] Schooling is compulsory through 8th grade, and 90 - 95 percent of all youths today complete 8th grade; thus, our sample of currently enrolled students is representative of a significant portion of youths of the relevant age.

returns to education and current enrolment status, as well as health-related behaviors. A third round, follow-up survey was 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 still enrolled in 2005 but not yet in their final year of school (due primarily to grade repetition), we conducted follow-ups for the next two years. We were able to re-interview 94% of youths in round 2 and 89% in round 3 (though for the educational outcomes, we were able to track students administratively even if we did not interview them, which reduced attrition to 3 and 9%, respectively). Attrition rates were very similar for treatment and control youths; in round 2, we were able to re-interview 1,053 of the 1,125 control students and 1,057 of the 1,125 treatment students, and in round 3, we were able to re-interview 1,006 of the controls and 1,005 of the treatment sample. Below, we will discuss the sensitivity of the results to differing assumptions on the unobserved outcomes of attriters. More details on the data are available in Jensen (2010).

B. Descriptive Statistics

Table 1 reports summary statistics for the full sample and 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. Tests for each individual baseline covariate also do not reject equality of means for treatment and control groups.

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 32% of the students attending school in 8th grade eventually graduate from high school. About 30% of the sample works in round 2. However, work is not exclusive to those who have dropped out: 19% of boys work and do not attend school, while 10% both attend school and work (47% attend school only and 22% are idle). Earnings from work are low—about 68 Dominican Pesos (RD$) per week ($4.2 U.S.; in May 2001, $1 U.S.≈RD$16.2), and about RD$260 ($16 U.S.) per week among those that work. These earnings increase by a factor of four by round 3 (by which time the youths are 18 years old on average), although the fraction working only increases to 36%.[10]

Our two health behaviors of interest are smoking and alcohol consumption, for which data were gathered in rounds 2 and 3, but not at baseline. Only 5% of boys reported smoking in

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

round 2, but this increases to 13% by round 3.[11] By contrast, drinking alcohol is more widespread: in round 2, when most of the sample is 14 years old, 61% of boys already report drinking. Although that fraction rises only to 73% 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 increases from 2% to 13%.[12] By comparison, data from the 2000 wave of the NLSY indicate that 40% of males aged 18-19 in the U.S. report smoking, 50% report drinking alcohol, and about 6% report drinking 15 days a month or more. A limitation of our alcohol data is that we only know the frequency of consumption, not the amount consumed (over some period or at any given sitting). For adults, moderate amounts of alcohol may not have adverse health consequences; the greater concern is over excessive or binge drinking. However, for the young adolescents we focus on, alcohol consumption may have important consequences in even smaller amounts, as noted in the introduction. And in particular, our analysis will focus primarily on daily drinking, which is likely to constitute a greater concern.

As noted above, the survey gathered data on several of the mechanisms through which schooling and work may affect drinking and smoking: 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 (disposable income), which can include earnings net of the amount given to their families, or any spending money given to them by parents. In round 2, youths report having about RD$56 (about $U.S. 3.50) a month in disposable income (which is just slightly less than their average earnings) and by round 3, they have about RD$121 (about $U.S. 7.50, or roughly half of earnings). Though disposable income is quite modest, 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 cigarettes are also commonly purchased individually for RD$2-4, and a bottle of beer, the most common drink among youths, costs about RD$30-35.

[11] Smoking rates are in line with those reported in other studies of Dominican youths in school (Dormitzer et al. 2004) but higher than those in the Dominican Republic Demographic and Health Survey (DHS) of 2005, which reports a smoking rate of about 4% (among 18-19 year old boys in urban areas). However, it is worth noting that the samples are not perfectly comparable. For example ours omits the roughly 5-10 percent not enrolled in school, and the DHS includes only boys currently living with an ever-married woman aged 15-49.

[12] Though these drinking rates appear extremely high, they are very close to those found in the 2005 Dominican DHS, and to those reported elsewhere for youths in the Dominican Republic (Dormitzer et al. 2004).

The survey elicited a measure of patience in rounds 2 and 3 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?"[13] On average, boys report being somewhat patient (3.09), but patience decreases substantially by round 3 to 2.06.

In round 3, we also measured time preferences using an approach that is more standard in the literature, namely the willingness to trade off a current payout in exchange for some larger future payout (Tversky and Kahneman 1986, Benzion, Rapoport and Yagil 1989, Coller and Williams 1999), which has been shown to have strong predictive power for a range of behaviors (Chabris et al. 2008). In particular, youths were asked, "Suppose you had the choice between receiving 100 pesos today, or 100 pesos in one month. Which would you choose?" Youths who would take 100 pesos today were asked the same question about 100 pesos today vs. 110 in one month, and so on up to 150 pesos. We define the discount rate as the midpoint between the interval where they first say yes to the future sum, and the previous value; thus, someone who prefers 100 pesos today to 110 pesos in one month but 120 pesos in one month to 100 pesos today is assigned a discount rate of 15 percent. For those who say no to 150 pesos, we top code the discount rate at 60 percent.[14] Overall, the mean implied monthly discount rate is 32 percent. Importantly, the two measures in round 3 are highly correlated (-0.86); thus, the analysis below is robust to using either. To an extent, this also validates the use of the 1 to 5 patience measure in round 2, where it is the only measure we have.

Attitudes towards risk were measured in round 3 only, using the following question: "Some people like to 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?" Self-assessed risk taking in this sample is high: about 50% report they usually take risks (category 1), while only 10% choose categories 4 or 5. In contrast to our measure of time preferences, this measure of attitudes towards risk is much more limited relative to the literature. It is more common to present a series of lotteries with tradeoffs between risk and payout

[13] 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.

[14] Only about 6 percent of boys still continued up to 150 pesos, so the results in our regressions below are not very sensitive to the treatment of top coding.

(Binswanger 1980, Holt and Laury 2002; see Cardenas and Carpenter 2008 for further discussion).

The survey also asked about the drinking and smoking behavior of peers in rounds 2 and 3: "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.

These measures are of course limited, and we do not take them as perfect measures of the underlying characteristic of interest. Other studies have measured these quantities using a longer battery of questions or via experimental games; further, more recent advances focus for example on the joint estimation of risk and time preferences (Anderson et al. 2008). However, given the young age of our sample, field tests with more sophisticated or complex questions yielded low response rates, which could have lead to selection problems. And though these measures are imperfect, we show in regressions below that these measures do have some predictive power for drinking and smoking.

Appendix Table A shows the correlations between education, work and our proxies for mechanisms in the control group in round 3. As expected, those who work have more disposable income. Education and work are highly correlated with reported peer behavior: working is associated with a larger share of peers that smoke and drink, while the opposite holds for years of schooling. Patience and risk aversion are correlated with schooling, as one would expect, but these correlations are small. Correlations with perceptions of harm are very small. The more educated are slightly more likely to report that smoking is harmful, but surprisingly, they are less likely to report drinking is harmful. While the weak correlation between belief that smoking is

bad for health and education is perhaps surprising, as of 2005 there had been very few campaigns against smoking in the Dominican Republic. Other studies for the country suggest that although adults often 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); further, Dozier et al. (2006) report that none of the non-smoking Dominican adults in their survey reported negative health consequences as a reason for why they never started smoking.

C. OLS Results

In order to establish the commonly found relationship between education and health behaviors in our data set, and for comparison with the experimental results, we start by estimating least squares regressions of smoking and daily drinking on education and work status, using individuals in the control group only. To match the specification used by most studies, we use round 3 data only, for which we have years of schooling. For each outcome, we present three separate regressions: first including years of school only, then only with an indicator for work status, and finally with both work and years of schooling, and our measures of possible mechanisms. All regressions also include the following baseline covariates: father's education, log of family income and teacher's reports of student performance in school.[15]

Table 2 shows the results. We find that education is associated with a lower likelihood of smoking (one more year of schooling lowers smoking by 21% relative to the control group mean), and that work results in a higher likelihood of smoking (increasing it by almost 80%), and these effects are statistically significant when included individually or jointly. Education and work both appear to *increase* daily drinking—and the effect of education actually increases when we control for work status and for mechanisms (one more year of education increases daily drinking by 27% relative to the control group mean).[16] Of course, as per the motivation for our study, these effects may not reflect causal relationships.

For both smoking and daily drinking, the fraction of peers that smoke appears to matter. For smoking, patience and risk aversion also matter, but the sign on patience is the opposite of

[15] School performance is teacher assessment of the student's performance, on a scale of 1 to 5 (much worse than average, worse than average, average, above average, much better than average).

[16] The correlation between education and daily drinking is also positive (but not statistically significant) among U.S. teens in the NLSY 1997 (results available upon request).

what is expected; youths who report being more patient are more likely to smoke.[17] For daily drinking disposable income seems to matter. None of the other possible mechanisms appear to be statistically significant.

D. The Experiment

As discussed in Jensen (2010), on average the youths in our sample had very low perceptions of the returns to finishing secondary schooling.[18] The baseline survey revealed that mean expected return to secondary school was about RD$330 (about US$21) per month, or about 9.5 percent greater than earnings with only completed primary schooling (see Appendix Table B). This figure is only about one-quarter the size of the actual returns estimated from a labor force survey conducted by the author. 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 in almost every other study, even where omitted variables bias concerns have been addressed. In addition, at baseline about 40% of students reported that there was no difference in earnings between those with only primary school and those with a secondary school degree.

The fact that perceived returns are so much lower than measured returns raises the possibility that providing information on the true returns could affect schooling. There is also suggestive evidence that the reason perceived returns are so much lower than measured returns is that students may only have information on the very local returns to education within their community only. Therefore, at the end of the first round survey each respondent at a randomly selected subset of schools was given information on measured earnings by education and the

[17] The same general conclusions hold if we use the implied discount rate measure instead of patience.

[18] To elicit perceived returns, the survey asked questions based on Dominitz and Manski (1996), though much more limited. Students were asked to estimate what they expected they might earn under alternative education scenarios: *"Suppose, hypothetically, you were to complete [this school year/ secondary school/ university], and then stop attending school. Think about the kinds of jobs you might be offered and that you might accept. How much do you think you will earn in a typical week, month or year when you are about 30 to 40 years old?"* They were also asked to estimate the earnings of current 30-40 year old workers: *"Now, we would like you to think about adult men who are about 30 to 40 years old and who have completed only [primary school/ secondary school/ university]. Think not just about the ones you know personally, but all men like this throughout the country. How much do you think they earn in a typical week, month or year?"* Although the former are likely to be the relevant criteria for decision-making, this second set of questions was included to measure perceptions of earnings that are purged of any beliefs students may have about themselves, their household or their community, such as the quality of their school or their own ability, or beliefs about factors such as race in determining earnings. These questions are limited, though Jensen (2010) finds they are good predictors of actual schooling. Manski (2004) and Delavande, Giné and McKenzie (2010) provide summaries of methods for eliciting expectations.

absolute and percent return implied by those values. In particular, immediately upon completing the baseline survey, students were read the following paragraph:

> "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 were 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."

The provision of information alone constituted the experiment.[19] In this spirit, the experiment is consistent with a growing literature that finds that providing information can influence behavior. For example, Dupas (2009) finds that providing age-disaggregated information on HIV prevalence rates affects the incidence of risky sexual behavior among girls in Kenya. Duflo and Saez (2003) find that retirement plan decisions respond to being given incentives to attend a session providing benefits information, and Hastings and Weinstein (2008) find that providing parents with simpler, more transparent and relevant information such as average test scores and admissions probabilities can affect school choice. Jalan and Somanathan (2008) find that households in India that are given information on the contamination of their water spend considerably more on water purification, and Madajewicz et al. (2007) similarly show households in Bangladesh are more likely to switch wells in response to contamination information. Finally, applying a similar strategy to that used in the present paper, Nguyen (2008) finds that providing parents in Madagascar with information on the returns to schooling improves their children's school performance and attendance in the first few months following the intervention.

[19] The interviewers were asked to repeat the statement a second time to make sure students understood the findings, and then invited them to ask any questions about the findings or the data.

III. EMPIRICAL STRATEGY AND RESULTS

A. Empirical Strategy

In order to explore the impacts of the intervention on drinking and smoking, we estimate regressions of the form,

$$Y_i = \beta_0 + \beta_1 * Treatment_i + \mathbf{X_i}\,\alpha + \varepsilon_i,, \qquad (1)$$

where Y is the outcome of interest for individual i, and *Treatment* is an indicator equal to one if the individual received the treatment. Standard errors are adjusted for clustering at the school level—the level of randomization. We present regressions with and without additional controls (father's education, school performance and log of family income). Since treatment is orthogonal to these baseline covariates, their inclusion 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).

Because we study a 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). In effect, 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 *F*-tests of the null hypothesis that the effect of treatment is jointly zero for all outcomes within a domain.

B. Results: Work and Schooling

Table 3 presents the results of the treatment on schooling and labor force outcomes.[20] Panel A reproduces the results in Jensen (2010). The intervention was successful in increasing the perceived returns to a secondary degree substantially. Students in treatment schools that received information on the measured returns to schooling increased their perceptions of the

[20] Though we treat the school and work outcomes as independent, the results are robust to alternative specifications of the school-work outcomes, including bivariate probits.

returns by a statistically significant RD$366, which more than doubles the baseline value. Correspondingly, 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 schooling by round 3. However, we cannot reject the hypothesis that the treatment had no effect on the likelihood of completing secondary school. The Kling-Liebman-Katz average overall 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 hypothesis that the treatment had no effect across education outcomes.

Panel B presents the results for labor market outcomes. The treated youths were significantly less likely to work in both rounds. The effects are large, with an approximately 6 to 7 percentage point (about 18%) reduction in both rounds in the share that report working. This reduction in work is greater than the increase in the fraction that are in school in round 2, or than the increase in high school graduation at 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.[21] 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 time to studying. This hypothesis is consistent with the results of Nguyen (2008), who finds that school attendance and test scores increased in response to an intervention in Madagascar that also provided information on the returns to schooling.

The treatment also lowered total hours and earnings in both rounds (both are set to zero for those who do not work). 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. 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.[22] For both rounds, we can reject that the average overall effect of the treatment is zero at the 10 percent level or better.

The fact that our experiment both increased schooling and decreased working means that without strong assumptions, we cannot isolate the effects of schooling on health behaviors,

[21] Recall that a non-trivial share of students both attend school and work; Appendix Table C shows the impact of the treatment on the 4 possible work/school combinations

[22] 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. Unfortunately, it isn't possible to disentangle these explanations.

which has been the focus of almost all of the previous literature, from the effects of working on these behaviors. 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 the relationship need not be one-for-one). Thus in general, it will almost never be possible to isolate the pure effect of either schooling or work, since it is difficult to imagine a policy or factor that increases schooling, holding work constant, or vice-versa. Thus, 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 report the pure effect of schooling on teen drinking or smoking are likely to have inappropriately attributed all of the effects exclusively to schooling.

C. Results: Drinking and Smoking

Table 4 provides results for our outcomes of interest, smoking and drinking. In round 2, the effect of the treatment on smoking is negative, but very small and not statistically significant. Table 1 shows that very few young adolescents have started smoking by round 2 (where the average age is around 14), so it is perhaps not surprising that we do not detect any effects.

However, the treatment did result in a significant decline in alcohol intake.[23] We estimate the effects of treatment separately for each level of drinking to see which margin of alcohol intake is driving the reductions—moderate alcohol consumption is not believed to be harmful, whereas heavy alcohol consumption is associated with large health effects later in life and large contemporaneous externalities. We find large declines in daily drinking among adolescents; the fraction reporting they drink every day is cut by 1 percentage point (halving the rate relative to the control group mean). We also find large declines in drinking at least once a week (2.8 percentage points, or 14%); again, the effect on less extreme drinking like this may tell us less about the link between education and health in adulthood, but it is still an important outcome to consider given the concerns about any alcohol intake for adolescents specifically.

The results for round 3 are different than those in round 2. Now, the treatment results in a statistically significant decline in smoking. The effect is large, with a 4 percentage point reduction, or 27% relative to the control group mean. However, the effects on drinking, although negative, are neither economically nor statistically significant. The point estimates for drinking

[23] The results on drinking frequency are very similar when using ordered logit models (available from authors).

are small at every margin: drinking at all, at least every week and daily drinking. Taken together, the drinking results suggest that the reduction in alcohol intake observed in round 2 is mostly temporary, and that the intervention increased the age at which boys start drinking more intensively, not whether they do so by age 18. The fact that we find a positive association between education and drinking in the OLS results in Table 2, which is reversed when using the data from the experiment, suggests the OLS estimates are biased.

The results are fairly robust to the treatment of attrition. Since the difference in attrition between treatment and control groups is small (of 2,250 baseline students, we lose 71 control and 68 treatment youths by round 2, and 119 control and 120 treatment youths by round 3), the point estimates are changed only slightly if we assume either all attriters have the best outcomes (don't drink, don't smoke) or have the worst outcomes (smoke, and drink heavily). The same holds for any adjustment that assumes smoking and drinking among attriters is uncorrelated with whether they were in the treatment group. Although we have no reason to believe treatment youths who leave our sample are more likely to drink or smoke than youths from the control sample who leave, we can compute the minimum differences in drinking and smoking for the two attriter-groups that would cause us to overturn our conclusions. For smoking in round 2, the smoking rates among treatment attriters would have to be at least 35-45 percent higher (5-6 percentage points) than the control attriters for the effect of the treatment to not be significant at the 10 percent level (and the point estimate would only decline from .037-.039 to .031-.033). For drink frequency, the mean would have to be at least 10 percent greater for treatment than control attriters for the effects to no longer be significant at the 10 percent level (the coefficient would decline to .047 from .063). These rough computations show that any differences in outcomes for treatment and control attriters would have to be fairly large in order to weaken our primary conclusions. Of course, greater differences would lead to weaker treatment effects, and in the extreme case where we assume all control attriters have the best outcomes and all treatment attriters have the worst outcomes, the treatment effects would be reversed (results available from the authors). However, this is an extreme assumption, and as noted, we have no reason to believe treatment attriters are more likely to smoke or drink heavily than control attriters.

D. Results: Possible Mechanisms

We next turn our attention to the possible mechanisms that might explain the changes in behaviors. For this analysis, we simply estimate regressions like *(1)* where now *Y* is one of the potential mechanisms linking school or work to drinking and smoking. Our maintained assumption is that the intervention changed work and schooling, which influenced mechanisms such as disposable income or peer groups, and that these in turn influenced drinking and smoking. However, we cannot definitively test this causal chain. It is possible that the treatment simultaneously caused changes in both behaviors and our indicators of potential mechanisms, without the latter causing the former, or that some of the behaviors changed first and then caused changes in the proposed mechanisms (e.g., a youth may take more risks when they drink alcohol, rather than the reverse).

Table 5 presents the results. We start by discussing mechanisms for which we find no effects. Patience, risk and perceptions of adverse health consequences of drinking and smoking are not affected by the intervention—the point estimates for these measures are all extremely small and not statistically significant.[24] Figure 1 plots the distributions of these outcomes in Round 3. Patience is perhaps slightly lower in the treatment group, though the magnitudes are very small and the effects are not statistically significant. The treatment tended to decrease risk aversion slightly 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 the health consequences of these two behaviors. The intervention does make boys slightly more likely to think smoking is harmful, but less likely to think drinking is harmful, however in both cases the effects are small and not statistically significant. However, for all these measures, it is worth reiterating that our measures are imperfect, so we cannot definitively rule out changes that cannot be detected in our data.

By contrast, two mechanisms do in fact change significantly in response to the treatment: disposable income and the behavior of the youths' peer groups. In round 2, disposable income is only slightly lower on average for the treatment group (7%, but not statistically significant), since hours worked fall only slightly and earnings per hour are fairly low. By contrast, the disposable income of the treatment group falls much farther below the control group's disposable

[24] The results are very similar if we estimate ordered logit models instead of linear regressions for these variables.

income in round 3 (38%). Thus, the treatment significantly reduced the ability of youths to purchase alcohol and tobacco in round 3 (but not in round 2).

The treatment also causes significant declines in both the fraction of peers that smoke and peers that 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 changes in the distributions of peer behaviors more clearly. 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 that 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.

Taken together, these results suggest that peer effects may be the most important mechanism explaining how early adolescent drinking is influenced by school and work, with disposable income having much less or no effect at all. In round 2, we observed no change in any of the mechanisms for the treatment group that increased schooling and decreased work, except for increases in the fraction of peers who drink--particularly increases in the fraction for whom at least half their friends drink. As mentioned above, leaving school and/or entering the labor force is likely to expose the youth to an older set of peers, among whom drinking is more common,[25] even though it does not increase their disposable income much because hours and earnings are low at this age (14-15). By contrast, in round 3 when the youths are 18 or older, there is little difference in the fraction of peers who drink (since drinking is already very high at age 18, so even those who remain in school are among peers who overwhelmingly drink). Thus overall, in round 2 we observe decreases in drinking alongside big reductions in the fraction of peers who drink (but no changes in disposable income or other mechanisms), while in round 3 we observe no difference in drinking, alongside little to no change in the fraction of peers who drink, despite large differences in disposable income. It is possible that disposable income matters less because alcohol is often available to youths in social settings such as parties without having to pay for it (or, purchased alcohol can easily be shared among peers) whereas what matters more is how exposed the youth is to drinking or how accessible alcohol is, i.e., what fraction of the people

[25] DHS data for the Dominican Republic shows large increases in drinking and smoking with age.

they spend most of their time with drink.[26] These results are consistent with Kremer and Levy (2008), who find that students 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, the interpretation is slightly less clear, and both disposable income and the fraction of one's peers that smoke may matter. In round 2, the treatment lead to a large reduction in the fraction of peers that smoke, but no change in smoking behavior; however, again, we might not expect any corresponding changes in smoking behavior in round 2 because so few adolescents have started smoking by then. But in round 3, the treatment results in decreases in both disposable income and the fraction of peers who smoke and correspondingly a large decline in smoking. Overall, we cannot untangle the relative impacts of these two factors, and it is possible the impact of the two factors varies with age (i.e., younger adolescents are more susceptible to the behavior of their peers than older adolescents).

We should also point out potential limitations to our interpretation for peer effects. First, while we have interpreted changes in peers as having caused the changes in drinking (and perhaps smoking in round 3), it may well be that these behaviors changed for other reasons and that people who drink or smoke seek out peers who do likewise. Second, there is evidence that adolescents who smoke or drink tend to overestimate the smoking and drinking behavior of their peers (see Norton, Lindrooth and Ennett 2002). In both cases, we would not be able to causally attribute the changes in drinking and smoking to changes in peer group behaviors.

For smoking the results also suggest that disposable income may be important, and that in fact it is harmful (at least in the short run): having more money leads to increased smoking (Gruber and Zinman 2001 find similar results among high school seniors in the U.S.). Disposable income may be more likely to matter for smoking than for drinking because cigarettes are perhaps not as often provided freely at parties, and are less likely to be shared among friends. 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),[27]

[26] Among adults, alcohol consumption is a normal good in most countries; see Selvanathan and Selvanathan (2005) and Ruhm (2005). We know of no estimates for youths' heavy drinking. Markovitz and Tauras (2009) report an elasticity of drinking (at all) with respect to parental allowances of $0.13 - 0.26$. Our (insignificant) 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), though this is most likely an overestimate given that other factors also changed.

[27] In most countries the income elasticity of smoking is positive (for example see Selvanathan and Selvanathan 2005). Among youths in the U.S., Chaloupka and Grossman (1996) find an elasticity of smoking with respect to income of 0.14. Markovitz and Tauras (2009) report an elasticity of smoking with respect to parental allowances of

and suggests why in other contexts income is sometimes associated with worse rather than better health, particularly once education is controlled for (Ruhm 2005, Ruhm and Black 2005, Grossman 2005). It is also worth noting that in contrast to studies of adult behaviors, in the case of adolescents, more education comes with smaller (not larger) current period income. While education still increases permanent income, we believe teens are likely to be credit-constrained, and thus unable to adjust their current smoking and drinking to expected future income.

Finally, we note that we cannot rule out that other mechanisms beyond those explored here may explain the link between work/schooling and drinking or smoking. For example, education may affect self-esteem, self-control or other personality traits (e.g. Ross and Mirosvky 1999). In our experiment, 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, as youths are able to more effectively absorb health knowledge. However, given that there was no difference in perceptions of health harms of drinking and smoking by treatment status, it seems unlikely this effect could explain the results found here. Alternatively, since the intervention affected the perceived returns to school, it is possible that changes in drinking and smoking are driven by changes in the value of the future or costs of these behaviors (lost years of healthy life expectancy carries a greater income loss), as opposed to any direct effects of work or school. While we cannot rule this out, given that smoking and drinking are not viewed as being particularly bad for health, it is unclear that increases in future earnings would lead to changes in behavior. Additionally, altering current behavior in response to greater potential earnings many years in the future when smoking might reduce healthy life expectancy would require teens to be extremely forward looking, which seems unlikely. Though the evidence does not suggest a role for either changes in rates of time preference, attitudes towards risk, or increases in expected future income in explaining our results, we of course cannot definitively rule them out with our data.

IV. DISCUSSION AND CONCLUSION

We find that an intervention providing information on the market returns to schooling, which increased schooling and decreased work, lead to a reduced incidence of smoking at age 18

0.015 − 0.26. Our estimates are larger, but are likely to over-estimate the true elasticities, given that other factors (not just income) are changed by the experiment.

and a later onset of daily drinking. Our results confirm that at least part of the gradient between education and health-related behaviors among teens is indeed causal. These behavioral changes are likely to carry important private and social gains both directly in terms of health, as well as indirectly via externalities associated with these behaviors (such as second-hand smoke or drunk driving). The results also show that the returns to schooling are much greater than what is implied by wages alone (e.g., Oreopolous and Salvanes 2009).

Our study provides two key advantages over previous studies. First, we exploit the random assignment of information to identify the effects of schooling (and work) on drinking and smoking. This allows us to overcome concerns about possible omitted variables bias or reverse causality. Second, our survey gathered (albeit, imperfect) measures of a range of intermediate outcomes that can be used to explore the channels through which education and work might influence drinking and smoking; we find evidence that both peers and disposable income are likely be key explanatory mechanisms, while changes in attitudes towards risk, patience or perceptions of the potential harms of drinking or smoking are unlikely to play a significant role in our setting (though again, we cannot rule out that our measures are too limited to detect such changes).

There are however several limitations to our study. First, like most other studies, we rely on self-reports of drinking and smoking, and students may intentionally misreport such behaviors. However, for our study the key issue is whether the treatment changed the likelihood of underreporting. This may happen for example if youths that remain in school are more likely to underreport smoking and drinking because they are more aware that these are considered social "bads" (particularly for youths). However, since the treatment did not change reported perceptions of the harmful health effects of these behaviors, this seems less likely.

A second limitation is that the variation in schooling we exploit is driven by changes in perceived returns to schooling. Higher expected earnings in the future may affect smoking and drinking now because the returns to good health practices that extend the number of healthy working years an individual can expect (though we find it unlikely because youths do not view drinking and smoking as unhealthy and because they are unlikely to be so forward looking). However, real world gains in education are often likely to be driven by increases in the returns, so in terms of external validity, our experiment may not be such an exceptional case, i.e., it may often not be meaningful to discuss the impacts of education on an outcome while holding the

returns to education constant. However, we cannot state definitively that increases in schooling driven by factors such as enforcement of compulsory schooling laws or conditional cash transfers will have the same impact on drinking or smoking. Our results are however consistent with Galárraga and Gertler (2009), who find broadly similar effects of a conditional cash transfer program on drinking and smoking in Mexico; this randomized intervention provided cash conditional on attending school, health workshops and regularly visiting health clinics—while separately identifying the effects of each component is difficult, they report that education appears to be associated with improved healthy behaviors whereas the increases in disposable income increase the consumption of harmful goods.

A final limitation is that we only observe the medium-run effects of schooling on health-related behaviors. While there is considerable concern about these behaviors among adolescents, due to the potential greater harms of these behaviors for this younger group, spillover effects, and possible persistent effects of delaying smoking, we cannot determine whether these effects will persist, weaken, or perhaps even reverse in adulthood. Finally, in terms of external validity, our intervention focused on secondary schooling among boys in the Dominican Republic. We cannot generalize about the effects of changes in primary or post-secondary schooling, the effects of schooling in other countries, nor can we be sure that the patterns we observe for boys would hold for girls. However we do note that the few existing studies of youth drinking and smoking across countries suggest more similarities than differences across countries or gender (WHO 2001, Global Youth Tobacco Survey Collaborative Group 2002, 2003).

Appendix Table A: Correlation between Schooling, Work and Possible Mechanisms. (Round 3 survey, control group only).

	Years of school	Works
Works	-0.26	1.00
Disposable income	-0.20	0.38
Patient (1=not patient, 5=most patient)	0.11	-0.010
Discount Rate	-0.088	0.001
Risk aversion (1=always takes risks, 5=never takes risks)	0.083	-0.014
% peers smoke (1=0, 2=[0-0.49], 3=0.5, 4=[0.51-.99] 5=1)	-0.52	0.60
% peers drink (1=0, 2=[0-0.49], 3=0.5, 4=[0.51-.99] 5=1)	-0.13	0.18
Knows smoking is bad (1=very bad, 5=not bad at all)	0.055	-0.038
Knows drinking is bad (1=very bad, 5=not bad at all)	-0.039	-0.040

Sample corresponds to all non-treated individuals interviewed in round 3 with non-missing observations for the variables of interest. There are 865 observations.

Appendix Table B. Measured and Perceived Monthly Earnings, Males Aged 30-40

	(1) Measured Mean	(2) Perceived (self)	(3) Perceived (others)
Primary	3,180	3,516	3,478
	[1,400]	[884]	[863]
Secondary	4,479	3,845	3,765
	[1,432]	[1,044]	[997]
Tertiary	9,681	5,127	5,099
	[3,107]	[1,629]	[1,588]
Secondary – Primary	1,299	329	287
		[403]	[373]
Tertiary – Secondary	5,202	1,282	1,334
		[1,341]	[1,272]

All figures in 2001 Dominican Pesos (RD$). Standard deviations in bracket. Column (1) provides the mean earnings among men aged 30-40 from a household survey conducted by the author in January 2001. The number of observations is: 1,278 primary, 339 secondary and 83 tertiary. Columns (2) and (3) provide data from the Round 1 survey of 8th grade male students, conducted by the author in April/May 2001. Column (2) refers to what current students expect to earn themselves under different education scenarios when they are 30 − 40. Column (3) refers to what current students believe current 30 − 40 year old workers with different education levels earn. For both columns, there are 2,025 observations with responses for primary and secondary, and 1,847 responses for tertiary.

Appendix Table C: Effect of the Treatment on Work/School Combinations

	Round 2		Round 3	
Dependent Variable:	Basic	+ SES controls	Basic	+ SES controls
School Only	0.102***	0.104***	0.035	0.042**
	[0.026]	[0.025]	[0.025]	[0.021]
School and Work	-0.064***	-0.064***	-0.021*	-0.022**
	[0.013]	[0.013]	[0.011]	[0.011]
Work Only	0.005	0.002	-0.049*	-0.056**
	[0.021]	[0.019]	[0.026]	[0.024]
Neither	-0.042**	-0.042**	0.036	0.036
	[0.020]	[0.020]	[0.023]	[0.023]

Notes: Heteroskedasticity-consistent standard errors accounting for clustering at the school-level in parentheses. Sample consists of non-rural males throughout the Dominican Republic enrolled in 8th grade in April-May of 2001. Round 2 data were collected in October 2001 and Round 3 data were collected May-June 2005. The number of observations is 2111 in columns 1 and 2 and 2011 in columns 3 and 4. "SES control" regressions include: age, SES of family, dummy for SES missing, father's education and school performance. All variables as defined in table 1. *Significant at 10 percent level. **Significant at 5 percent level. ***Significant at 1 percent level.

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Table 1: Summary Statistics

	Full Sample			Control			Treatment		
	Obs.	Mean	S.D.	Obs.	Mean	S.D.	Obs.	Mean	S.D.
Socioeconomic characteristics									
Father finished secondary	2250	0.37	0.48	1125	0.37	0.48	1125	0.37	0.48
log(family income, round 2)	2250	8.15	0.3	1125	8.15	0.3	1125	8.14	0.3
Family income missing	2250	0.06	0.24	1125	0.06	0.24	1125	0.06	0.24
Performance at school	2250	1.36	1.32	1125	1.37	1.33	1125	1.35	1.31
Outcomes: Round 2									
Δ returns to secondary education	1859	272	616	910	88	423	949	448	713
Returned to school	2241	0.58	0.49	1054	0.56	0.50	1057	0.61	0.49
Currently works	2111	0.30	0.46	1054	0.33	0.47	1057	0.27	0.45
Hours worked per week	2111	4.10	8.90	1054	4.60	9.90	1057	3.50	7.70
Earnings per week	2111	67.7	172.6	1054	77.6	194.6	1057	57.80	146.7
Disposable income per week	2111	56.4	71.1	1054	58.9	73.6	1057	53.90	68.60
Currently smokes	2111	0.05	0.21	1054	0.05	0.22	1057	0.04	0.20
Currently drinks alcohol	2111	0.61	0.49	1054	0.61	0.49	1057	0.61	0.49
Drink frequency (1=never, 4=every day)	2111	1.96	0.70	1054	1.99	0.71	1057	1.93	0.69
Drinks at least once a week	2111	0.19	0.40	1054	0.21	0.40	1057	0.18	0.39
Drinks every day	2111	0.02	0.13	1054	0.02	0.14	1057	0.01	0.11
Patient (1=not patient, 5=most patient)	2111	3.09	1.14	1054	3.14	1.14	1057	3.04	1.14
Fraction peers smoke (1=none, 5=all)	2111	2.68	1.13	1054	2.76	1.14	1057	2.61	1.13
Fraction peers drink (1=none, 5=all)	2111	3.32	1.27	1054	3.39	1.28	1057	3.26	1.27
Outcomes: Round 3									
Finished High School=1	2063	0.32	0.47	1031	0.31	0.46	1032	0.32	0.47
Completed years of school	2074	9.86	1.77	1033	9.76	1.78	1041	9.95	1.77
Currently works=1	2011	0.36	0.48	1006	0.40	0.49	1005	0.33	0.47
Hours worked per week	2011	6.00	11.60	1006	7.00	13.10	1005	4.90	9.80
Earnings per week	2011	250.6	833.4	1006	302.0	940.4	1005	199.1	707.2
Disposable income per week	2011	121.2	391.6	1006	146.4	451.2	1005	96.10	319.3
Currently smokes=1	2011	0.13	0.34	1006	0.14	0.35	1005	0.12	0.32
Currently drinks alcohol=1	2011	0.73	0.44	1006	0.74	0.44	1005	0.72	0.45
Drink frequency (1=never, 4=every day)	2011	2.37	0.95	1006	2.40	0.96	1005	2.35	0.95
Drinks at least once a week	2011	0.46	0.50	1006	0.47	0.50	1005	0.44	0.50
Drinks every day	2011	0.13	0.33	1006	0.13	0.34	1005	0.12	0.33
Patient (1=not patient, 5=most patient)	2011	2.06	1.14	1006	2.09	1.14	1005	2.03	1.13
Implied Discount Rate	2011	0.32	0.17	1006	0.32	0.17	1005	0.33	0.17
Risk (1=always take risks, 5=never)	2011	1.98	1.20	1006	1.98	1.19	1005	1.99	1.21
Fraction peers smoke (1=none, 5=all)	2011	3.31	1.24	1006	3.43	1.21	1005	3.20	1.25
Fraction peers drink (1=none, 5=all)	2011	4.48	1.00	1006	4.50	1.00	1005	4.46	1.00
Smoking bad for health? (1=very bad, 5=not bad)	2011	3.67	0.68	1006	3.67	0.68	1005	3.66	0.68
Drinking bad for health? (1=very bad, 5=not bad)	2011	2.49	0.63	1006	2.49	0.62	1005	2.50	0.64

Notes: Sample consists of non-rural males throughout the Dominican Republic enrolled in 8th grade in April-May of 2001. Round 2 data were collected in October 2001 and Round 3 data were collected May-June 2005. Table reports un-weighted means. All monetary figures reported in 2001 Dominican Pesos (RD$). Smoking/Drinking is bad for health: "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?" % peers who smoke/drink are categorical variables: "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." Patient is coded as: "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?" Implied discount rate: "Suppose you had the choice between receiving 100 pesos today, or 100[110/20/30/40/50] pesos in one month. Which would you choose?" We define the discount rate as the midpoint between the interval they first say yes and the previous value. Those who say no to 150 pesos are coded as a 60 percent rate. Attitudes towards risk is coded as: "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?" Drink frequency is coded as: "1. Never; 2. Less than once a week, 3. At least once a week; 4. Every day." School performance is teacher assessment of the student's performance, on a scale of 1 to 5 (much worse than average, worse than average, average, above average, much better than average).

Table 2: OLS Regressions of Smoking and Drinking on Schooling and Work in Round 3 (Control group only)

	Smokes=1			Drinks everyday=1		
Years of School	-0.031***		-0.018*	0.012		0.035***
	[0.007]		[0.009]	[0.008]		[0.007]
Works=1		0.110***	0.075**		0.094***	0.046
		[0.033]	[0.037]		[0.028]	[0.030]
Kid's disposable income			0.006			0.007*
			[0.005]			[0.004]
Patient (1=not patient,			0.021*			0.005
5=most patient)			[0.011]			[0.012]
Risk Aversion			-0.021**			0.003
			[0.010]			[0.008]
% peers smoke round 2			0.052**			0.051***
			[0.022]			[0.018]
% peers smoke round 3			-0.035			0.012
			[0.025]			[0.017]
% peers drink round 2			0.009			-0.009
			[0.014]			[0.010]
% peers drink round 3			0			0.01
			[0.013]			[0.009]
Smoking bad (1: bad, 5: no)			-0.007			0.013
			[0.017]			[0.018]
Drinking bad (1:bad, 5: no)			0.015			0.014
			[0.021]			[0.016]
Dad's education	-0.008	-0.012	-0.009	-0.018	-0.016	-0.014
	[0.032]	[0.031]	[0.031]	[0.029]	[0.028]	[0.027]
log(family income)	-0.015	-0.015	-0.001	0.082*	0.113**	0.104**
	[0.054]	[0.052]	[0.051]	[0.046]	[0.047]	[0.045]
Performance in school	-0.016	-0.019*	-0.018*	0.008	0.008	0.007
	[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. Sample consists of non-rural males throughout the Dominican Republic enrolled in 8th grade in April-May of 2001, in control group villages only. Round 2 data were collected in October 2001 and Round 3 data were collected May-June 2005. All regressions also include indicators for missing values of family income disposable income, % peers who drink and % peers who smoke. All variables as defined in table 1.
*Significant at 10 percent level. **Significant at 5 percent level. ***Significant at 1 percent level.

Table 3: Effect of the Intervention on Education and Labor Market Outcomes

	Round 2		Round 3	
Dependent Variable:	Basic	+ SES controls	Basic	+ SES controls
Panel A: Effects on schooling				
Δ returns to finish secondary	366***	366***		
	[30.4]	[30.5]		
Returned to school	0.042*	0.045*		
	[0.025]	[0.023]		
Finished high school			0.020	0.023
			[0.025]	[0.021]
Completed years of school			0.180*	0.202**
			[0.102]	[0.085]
Average education effect	*0.340****	*0.342****	*0.062*	*0.073*
	[0.038]	*[0.037]*	*[0.055]*	*[0.046]*
F-stat, p-value (joint test treatment =0)	*72.0, 0.000*	*71.6, 0.000*	*4.63, 0.011*	*5.35, 0.006*
Panel B: Effects on labor market outcomes				
Currently works	-0.059**	-0.063**	-0.073**	-0.079***
	[0.023]	[0.022]	[0.026]	[0.024]
Hours works per week	-0.88**	-0.93**	-1.77**	-1.92**
	[0.43]	[0.41]	[0.61]	[0.58]
Earnings per week	-15.94*	-16.83**	-98.57**	-106.10**
	[8.13]	[7.87]	[45.15]	[40.15]
Average labor market effect	*-0.106***	*-0.112***	*-0.143***	*-0.153****
	[0.045]	*[0.043]*	*[0.045]*	*[0.042]*
F-stat, p-value (joint test treatment =0)	*2.15, 0.097*	*2.74, 0.046*	*3.20, 0.025*	*4.53, 0.005*

Notes: Heteroskedasticity-consistent standard errors accounting for clustering at the school-level in parentheses. Sample consists of non-rural males throughout the Dominican Republic enrolled in 8th grade in April-May of 2001. Round 2 data were collected in October 2001 and Round 3 data were collected May-June 2005. The number of observations is 2111 in columns 1 and 2 and 2011 in columns 3 and 4. "SES control" regressions include: age, SES of family, dummy for SES missing, father's education and school performance. All variables as defined in table 1. "Average education/labor market effect" is the mean effect of the treatment across outcomes within the specified domain, computed using the methodology described in Kling, Liebman, and Katz (2007). *Significant at 10 percent level. **Significant at 5 percent level. ***Significant at 1 percent level.

Table 4: Effect of the Intervention on Smoking and Drinking

	Round 2		Round 3	
Dependent Variable:	Basic	+ SES controls	Basic	+ SES controls
Currently smokes=1	-0.004	-0.004	-0.037**	-0.039**
	[0.011]	[0.011]	[0.018]	[0.018]
Drink frequency (1=don't, 4= every day)	-0.063**	-0.063**	-0.035	-0.032
	[0.031]	[0.031]	[0.040]	[0.041]
Currently drinks alcohol	-0.014	-0.014	-0.002	-0.005
	[0.023]	[0.023]	[0.022]	[0.021]
Drinks at least once a week	-0.028*	-0.027	-0.031	-0.03
	[0.017]	[0.017]	[0.023]	[0.023]
Drinks everyday	-0.010*	-0.010*	-0.007	-0.006
	[0.006]	[0.006]	[0.016]	[0.016]
Average behavior effect (smoking + drinking)	*-0.053*	*-0.054*	*-0.074***	*-0.075***
	[0.033]	*[0.33]*	*[0.032]*	*[0.032]*
F-stat, p-value (joint test treatment =0)	*2.15, 0.12*	*2.12, 0.12*	*2.78, 0.065*	*2.95, 0.055*
Average smoking & daily drinking effect	*-0.047*	*-0.048*	*-0.066***	*-0.068***
	[0.034]	*[0.034]*	*[0.030]*	*[0.031]*
F-stat, p-value (joint test treatment =0)	*1.51,0.224*	*1.52, 0.223*	*2.75, 0.067*	*2.95, 0.055*

Notes: Heteroskedasticity-consistent standard errors accounting for clustering at the school-level in parentheses. Sample consists of non-rural males throughout the Dominican Republic enrolled in 8th grade in April-May of 2001. Round 2 data were collected in October 2001 and Round 3 data were collected May-June 2005. The number of observations is 2111 in columns 1 and 2 and 2011 in columns 3 and 4. "SES control" regressions include: age, SES of family, dummy for SES missing, father's education and school performance. All variables as defined in table 1. "Average behavior/smoking & daily drinking effect" is the mean effect of the treatment across outcomes within the specified domain, computed using the methodology described in Kling, Liebman, and Katz (2007). *Significant at 10 percent level. **Significant at 5 percent level. ***Significant at 1 percent level.

Table 5: Effect of the Intervention on Possible Mechanisms

	Round 2		Round 3	
Dependent Variable:	Basic	+ SES controls	Basic	+ SES controls
Disposable income	-4.01	-3.73	-49.98**	-51.97***
	[3.23]	[3.21]	[20.17]	[20.32]
Patient	-0.049	-0.053	-0.025	-0.033
(1=not patient, 5=most patient)	[0.049]	[0.048]	[0.050]	[0.048]
Implied Discount Rate			0.15	0.29
			[0.78]	[0.73]
Risk			-0.011	-0.003
(1=always take risks, 5=never)			[0.056]	[0.056]
% peers smoke	-0.166**	-0.173**	-0.211***	-0.231***
(1=none, 5=all)	[0.061]	[0.056]	[0.072]	[0.062]
% peers drink	-0.144**	-0.151**	-0.016	-0.022
(1=none, 5=all)	[0.066]	[0.062]	[0.043]	[0.043]
Smoking is bad for health?			-0.035	-0.036
(1=very bad, 5=not bad at all)			[0.027]	[0.027]
Drinking is bad for health?			0.009	0.015
(1=very bad, 5=not bad at all)			[0.029]	[0.029]
Average effect on mechanisms	*0.026*	*0.032**	*0.004*	*0.003*
	[0.024]	*[0.020]*	*[0.015]*	*[0.015]*
F-stat, p-value	*1.57,*	*2.73,*	*1.55,*	*2.31,*
(joint test treatment =0)	*0.185*	*0.031*	*0.146*	*0.023*
Average effect overall all outcomes	*0.072***	*0.077***	*0.030**	*0.033***
	[0.019]	*[0.016]*	*[0.017]*	*[0.016]*
F-stat, p-value	*9.26,*	*13.2,*	*1.20,*	*1.60,*
(joint test treatment =0)	*0.000*	*0.000*	*0.281*	*0.080*

	Round 2 & Round 3 Combined	
	Basic	+ SES controls
Average effect overall all outcomes	*0.050****	*0.055****
	[0.017]	*[0.015]*
All outcomes, all rounds	*5.43, 0.000*	*7.12, 0.000*

Notes: Heteroskedasticity-consistent standard errors accounting for clustering at the school-level in parentheses. Sample consists of non-rural males throughout the Dominican Republic enrolled in 8th grade in April-May of 2001. Round 2 data were collected in October 2001 and Round 3 data were collected May-June 2005. The number of observations is 2111 in columns 1 and 2 and 2011 in columns 3 and 4. "SES control" regressions include: age, SES of family, dummy for SES missing, father's education and school performance. All variables as defined in table 1. "Average effect on mechanisms/overall outcomes" is the mean effect of the treatment across outcomes within the specified domain, computed using the methodology described in Kling, Liebman, and Katz (2007). *Significant at 10 percent level. **Significant at 5 percent level. ***Significant at 1 percent level.

Figure 1: Effects of Intervention on Patience, Risk Aversion and Perceptions, Round 3

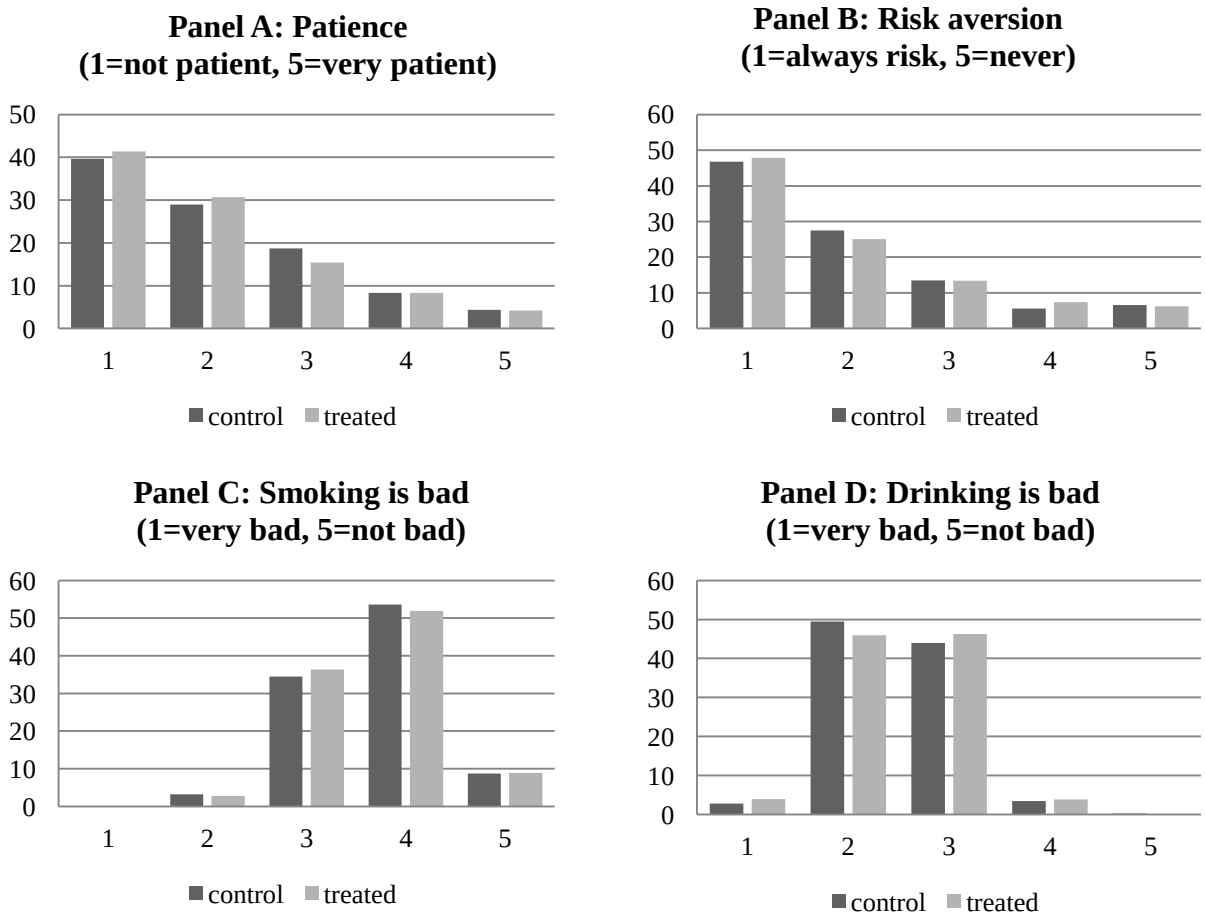
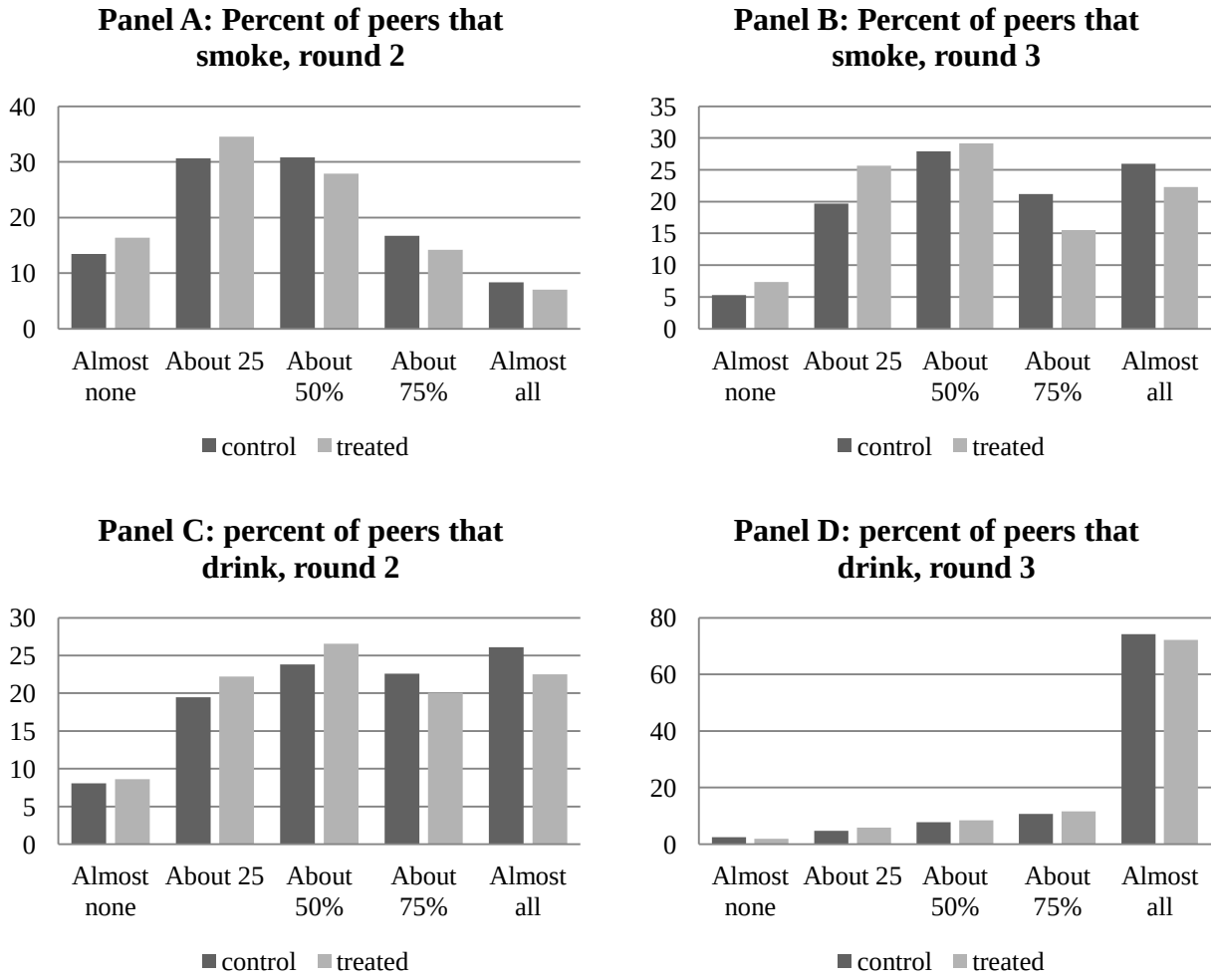


Figure 2: Effect of the Intervention on Fraction of Peers that Smoke and Drink.



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