High Times: The Effect of Medical Marijuana Laws on Student Time Use

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Abstract

Medical marijuana laws (MMLs) represent a major change of marijuana policy in the U.S. Previous research shows that these laws increase marijuana use among adults. In this paper, we estimate the effects of MMLs on secondary and post-secondary students' time use using data from the American Time Use Survey. We apply a difference-in-differences research design and estimate flexible fixed effects models that condition on state fixed effects and state-specific time trends. We find that on average, part-time college students in MML states spend 42 fewer minutes on homework, 37 fewer minutes attending class, and 60 more minutes watching television per day than their counterparts in non-MML states. However, we find no effects of MMLs on secondary or full-time college students' time use. These results provide evidence on the mechanisms through which marijuana use affects educational outcomes, and that the impacts of MMLs on student outcomes are heterogeneous and stronger among relatively disadvantaged students.

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"I now have absolute proof that smoking even one marijuana cigarette is equal in brain damage to being on Bikini Island during an H-bomb blast."

- Ronald Reagan, 40th U.S. President.

1. Introduction

It is a popular belief that marijuana use harms educational performance and predicts school dropout and truancy. Not only is there a strong correlation between marijuana use and low educational attainment, but medical research suggests that marijuana can affect motivation, attention, and cognition. However, empirical evidence on the causal link between marijuana use and educational outcomes is limited outside the laboratory. Despite significant policy implications, there are relatively few economic studies on the topic. Moreover, much of the extant literature relies on strong identification assumptions, and it is unclear to what extent the findings in prior research are driven by unobserved heterogeneity. A key difficulty in identifying the causal effects of marijuana use on educational outcomes is identifying arguably exogenous variation in marijuana consumption.

Over the past two decades, more than twenty U.S. states passed medical marijuana laws (MMLs) that effectively allow patients to legally possess and use marijuana. Such laws are naturally controversial, as they have the potential to increase illicit marijuana use among non-patients. Large surveys such as the National Survey on Drug Use and Health (NSDUH) document a strong, positive correlation between MMLs and marijuana use (Cerdá et al. 2012; Wall et al. 2011). This correlation could be causal as MMLs lower non-patients' marginal cost of marijuana use through several channels. First, MMLs might increase non-patients' access to marijuana and lower the drug's real and/or shadow price (Anderson, Hansen, and Rees 2013; Salomonsen-Sautel et al. 2012; Thurstone, Lieberman, and Schmiege 2011). Second, MMLs could shift social attitudes toward marijuana use and decrease stigma or the perceived harm associated with its use (Khatapoush and Hallfors 2004). Finally, MMLs could cause law enforcement and the judicial system to take a more lenient approach to illegal marijuana use (Eddy 2010; GAO 2002).[1]

Several recent studies provide evidence of a causal relationship and show that MMLs cause a 10–20% increase in marijuana use (Chu 2014, 2015; Wen, Hockenberry, and Cummings 2015). The effects of MMLs are largely on the intensive margin and on heavy marijuana use. For example, Wen, Hockenberry, and Cummings (2015) find that 44% of the

[1] In fact, cities like Denver, San Francisco, and Seattle, passed initiatives that either legalize marijuana or require authorities to make marijuana offenses 'the lowest law enforcement priority' (Eddy 2010).

increase in past-month marijuana use is due to increase in daily marijuana use. Somewhat surprisingly, the increase in marijuana use is concentrated among adults, as MMLs do not affect marijuana usage among juveniles (Anderson, Hansen, and Rees 2015; Choo et al. 2014; Lynne-Landsman, Livingston, and Wagenaar 2013; Wen, Hockenberry, and Cummings 2015).[2] A small but growing literature utilizes MMLs as a policy shock to study the effects of marijuana use on relevant health and social outcomes. For example, Anderson, Hansen, and Rees (2013) find that MMLs reduce alcohol consumption and traffic fatalities involving alcohol, suggesting that marijuana and alcohol are substitutes. Chu (2015) finds that marijuana is not a complement to cocaine and heroin, as the use of these drugs does not increase after the passage of MMLs. Powell, Pacula, and Jacobs (2015) show that medical marijuana states experience a relative decrease in opioid addictions and opioid overdose deaths. MMLs may affect behaviors as well, perhaps because marijuana use crowds out other activities or consumption. Anderson, Rees, and Sabia (2014) find that MMLs reduce suicide rates among younger men, and Sabia, Swigert, and Young (2015) find that MMLs reduce the prevalence of obesity.

The general finding that MMLs affect non-patients' marijuana use suggests that MMLs might affect educational attainment because students may smoke more marijuana and therefore change their motivation and behaviors. In this paper, we provide novel evidence on the impact of MMLs on student behaviors both in and out of the classroom and contribute to our understanding of the causal relationship between marijuana use and behaviors associated with educational success. We focus on time spent on studying and leisure, both of which may influence educational achievement and attainment (Jacob 2002; Kalenkoski and Pabilonia 2014), as studying is an input in the education production function while leisure might crowd out other more educationally productive activities. Specifically, we estimate the effects of MMLs on students' time spent on attending class, doing homework, and watching television using time diary data from the nationally representative American Time Use Survey (ATUS) for the years 2003–2013. Television in particular is thought to crowd out time spent in more educationally productive activities (Schmidt and Anderson 2007). We estimate difference-in-differences time-use regressions that control for state fixed effects, state-specific time trends, and a variety of student and time-diary covariates.

Consistent with extant evidence that MMLs do not affect juvenile drug use, we find no effect of MMLs on high-school students' time use. Effects on postsecondary students appear

[2] Wen, Hockenberry, and Cummings (2015) find that MMLs increase first time marijuana use but not regular use among ages 12–20. In the online appendix, Chu (2014) finds that MMLs increase marijuana use among treatment patients aged 15–17.

to be heterogeneous: MMLs affect part-time college students but not fulltime college students. On an average day, after the passage of MMLs, part-time college students spend 42 fewer minutes doing homework and 37 fewer minutes attending class. Interestingly, these decreases are offset by increased time in an educationally unproductive activity: part-time college students' time spent watching television increased by about 60 minutes per day. Changes on the extensive margin contribute to the reduction in homework and in-class time, as students are around 0.2 percentage points less likely to spend any time on doing homework or attending class. However, the increase in television time occurs entirely on the intensive margin. Our findings suggest that marijuana use may harm educational outcomes among less connected students and are consistent with the results in Marie and Zölitz (2015), who find that university students' academic performance increased at Maastricht University after legal access to marijuana was removed, particularly for low-performing students. Moreover, our findings provide evidence on the mechanisms through which these effects may operate.

This research makes several contributions. First, this paper leverages a new identification strategy—the exogenous shock of MMLs—for detecting the effects of marijuana use on intermediate educational outcomes. Previous studies either use instrumental variables that are largely based on cross-sectional variation, or try to model individual heterogeneity econometrically, with Marie and Zölitz (2015) being a notable exception. In contrast, the current study exploits a more plausible source of exogenous variation in marijuana consumption. Second, students' behavioral responses to MMLs are of policy interest in their own right. The finding of stronger negative effects on the educationally productive activities of potentially disadvantaged groups is particularly relevant to discussions of inequality and the design of future education and health policies. Finally, while MMLs provide numerous benefits to patients, unintended negative externalities associated with increased access to marijuana exist. Identifying and quantifying unintended consequences of public policy is paramount to conducting careful cost-benefit analyses and to improving future iterations of policies.

The paper proceeds as follows: Section 2 briefly describes the history of MMLs and what is known about the relationship between marijuana use and educational outcomes. Sections 3 and 4 describe the data and empirical strategy, respectively. Section 5 presents the results and section 6 concludes.

2. Background and Literature Review

2.1 History of Medical Marijuana Laws

In the late 1970s, many states began passing legislation that allowed the use of medical marijuana through research programs, but only a handful of states' research programs became operable due to federal restrictions (Pacula et al. 2002). In 1986, the Food and Drug Administration (FDA) approved Marinol, a prescription drug containing the same active ingredient, Delta-9-THC, as marijuana. However, taking oral medications could be difficult for patients suffering from severe nausea, a common symptom among AIDS and cancer patients. In the late 1980s and early 1990s, smokable marijuana was discovered to benefit growing populations of AIDS and cancer patients. In 1996, California became the first state to pass a medical marijuana law allowing patients to legally use and possess marijuana. With growing positive medical evidence and lobbying by marijuana legalization advocacy groups, such as the National Organization for the Reform of Marijuana Laws (NORML), many states have since joined California in passing a new wave of medical marijuana legislation. As of 2015, 23 states and the District of Columbia have passed similar medical marijuana laws. States with effective medical marijuana laws, and the years they passed, are summarized in Table 1.

These laws permit patients with legally designated diseases and syndromes to use marijuana as a means of treatment. The designated symptoms and conditions typically include AIDS, anorexia, arthritis, cachexia, cancer, chronic pain, glaucoma, migraines, persistent muscle spasms, severe nausea, seizures, and sclerosis. Patients can legally possess marijuana up to a fixed amount that varies by state. Since MMLs do not change the criminal status of marijuana, only legal patients can be exempted from state penalties. To become a legal patient, individuals need a recommendation from a physician.[3] In most states, legal patients also need to register with the state medical marijuana program and obtain a medical marijuana card.[4] The number of registered patients was quite small before 2009, but has increased dramatically in recent years. For example, Colorado passed its medical marijuana law in 2001 and had only 5,051 registered patients in January 2009. By January 2010, however, the number of registered patients had increased more than 2,000 percent in just one year to 118,895. An estimate from ProCon.org (2016) suggests that there are about 1.2 million legal patients in 2016, or about 0.8% of the total population of medical marijuana states.

[3] Federal law does not allow physicians to 'prescribe' marijuana.

[4] California created a registration program in 2004, but registration is voluntary. Maine passed an amendment in November 2009 that created a registration program, but it remains voluntary. Washington does not have a registration program. In some states, such as Colorado and Nevada, patients who do not join the registry and are arrested for marijuana charges may argue an "affirmative defense of medical necessity."

Medical marijuana laws passed prior to the Obama administration generally do not authorize marijuana dispensaries in order to conform to the federal classification of marijuana as a Schedule I drug. To overcome the supply issue, these laws allow for home cultivation. Patients can grow for their own and for other patients' use on a not-for-profit basis. Marijuana dispensaries with grey legal status still exist, however. The prevalence of dispensaries largely depends on the attitude of local governments and the actions of local law enforcement, which could change from time to time. In 2007, New Mexico became the first state to pass a medical marijuana law with a provision to license production and distribution at the state level, but the first state-licensed marijuana provider in New Mexico was not approved until March 2009.[5] In 2009, the Obama administration announced that the federal government would no longer seek to arrest medical marijuana users and suppliers so long as they conformed to state laws (Mikos 2011). This statement largely resolved the legal dispute between state and federal governments. As a result, laws and amendments now regulate dispensaries, and both the numbers of registered patients and dispensaries have increased significantly since then.

2.2 Marijuana Use and Educational Outcomes

Strong correlations between marijuana use and educational outcomes such as school dropout and truancy are well documented (Lynskey and Hall 2000; Macleod et al. 2004). Such associations may be causal, owing to changes in motivation, cognitive ability, and other psychological effects of marijuana use. Numerous laboratory experiments find that marijuana users exhibit lower motivation and cognitive impairment (Foltin et al. 1989; Griffith-Lendering et al. 2012; Lane et al. 2005; Musty and Kaback 1995; Randolph et al. 2013; Shannon et al. 2010; Verdejo-Garcia et al. 2007; Wetherell et al. 2012; Whitlow et al. 2004). Heavy marijuana use is also associated with depression (Degenhardt, Hall, and Lynskey 2003). Moreover, many recent studies utilize magnetic resonance imaging (MRI) to show brain abnormalities among marijuana users (Batalla et al. 2014; Bolla et al. 2005; Filbey et al. 2014; Gilman et al. 2014; Price et al. 2015; Raver, Haughwout, and Keller 2013; Vaidya et al. 2012; Wesley, Hanlon, and Porrino 2011). Some evidence suggests that the impacts of marijuana use on the brain could be permanent, as brain abnormities can be found even among casual and abstinent users (Bolla et al. 2005; Gilman et al. 2014; Raver, Haughwout, and Keller 2013).

[5] Dispensaries are considered to be legally protected in California (Senate Bill 420, 2003) and Colorado; their laws do recognize the existence of dispensaries even though they are silent as to their legality (Pacula, Boustead, and Hunt 2014; Pacula et al. 2015).

However, the correlation might be spurious and driven by individual heterogeneity. Most of the laboratory experiments mentioned above are not randomized control experiments. Due to ethical issues, researchers cannot randomly assign marijuana use to experiment subjects, and instead recruit volunteer marijuana users as experiment subjects. It is unclear to what extent the psychological effects found in the medical literature are indeed causal. For example, Denson and Earleywine (2006) suggest that some marijuana use may be self-medication, and marijuana users generally have less depressed moods than nonusers. Weiland et al. (2015) point out that in many previous studies marijuana users are observably different from nonusers in terms of alcohol use and along several other dimensions, and the researchers find no association between marijuana use and standard MRI brain shape measurements after carefully controlling for alcohol use, gender, age, and other variables. Filbey et al. (2014) also point out that current MRI evidence is largely correlational, and longitudinal studies that can trace changes over time are needed to determine causality. In addition, peer behaviors appear to affect marijuana use (Duarte, Escario, and Molina 2009; Gaviria and Raphael 2001). Thus, poor academic performance among marijuana users could be a result of social interaction and self-selection into low-achieving peer groups. A final concern is reverse causality, as Sander (1998) and Zimmerman and Schmeelk-Cone (2003) suggest that school performance may affect marijuana initiation and usage.

Perhaps due to a general lack of plausible exogenous variation, only a handful of studies have attempted to determine the causal association between marijuana use and educational attainment. In addition to directly controlling for individual and family characteristics (Cobb-Clark et al. 2015), most research uses instrumental variables and simultaneous equation models to address endogeneity problems (Bray et al. 2000; Chatterji 2006; Duarte, Escario, and Molina 2006; Roebuck, French, and Dennis 2004; Yamada, Kendix, and Yamada 1996). For example, state beer taxes, marijuana and cocaine prices, marijuana penalties, and de-penalization status have been used as instruments for marijuana use. Some research also relies on econometric modelling to control for individual heterogeneity (Duarte, Escario, and Molina 2006; Roebuck, French, and Dennis 2004; van Ours and Williams 2009). The literature finds evidence of a negative causal link between marijuana use and educational attainment. However, these conclusions should be interpreted cautiously, as the identification in these studies tends to rely on strong assumptions. For example, the instruments are generally weak and arguably endogenous (French and Popovici 2011). McCaffrey et al. (2010) show that estimates are sensitive to the specification of the conditioning set and propensity score weights, suggesting that unobserved heterogeneity remains an important factor. Moreover, medical research utilizing data on twins finds no causal effects of marijuana use on educational attainment (Bergen et al. 2008; Grant et al. 2012; Verweij et al. 2013). A recent exception to this critique is Marie and Zölitz (2015), who use a difference-in-differences strategy to estimate the effect of a policy that removed college students' legal access to marijuana. The authors find that student performance at Maastricht University increased after legal access was removed, that this effect was driven by younger university students, and provide suggestive evidence that these improvements were due to increased understanding of material rather than changes in students' effort provision.

We contribute to this literature by investigating the channels through which access to marijuana might affect educational achievement and attainment. Specifically, we provide reduced-form evidence on the causal effects of MMLs on student behaviors associated with educational success. Intuitively, MMLs might increase marijuana use among secondary and post-secondary students by reducing the costs associated with procuring and using marijuana. We therefore estimate MMLs' effects on time spent doing homework, attending class, and watching television, all of which might influence educational achievement and attainment (Jacob 2002; Kalenkoski and Pabilonia 2014).

3. ATUS Data

We examine the effects of medical marijuana laws on secondary and postsecondary students' time use. Behavioral responses to MMLs are of interest in their own right, but also a potential mechanism through which marijuana use might affect educational achievement and attainment. Prevailing social attitudes towards how individuals spend their time likely influence responses to traditional survey questions, a phenomenon known as *social desirability bias* (Grimm 2010). Accordingly, retrospective time diaries likely yield more accurate measures of students' non-school time use (Juster and Stafford 1991). We therefore analyze time diaries collected by the American Time Use Survey (ATUS) for the years 2003–2013.

The ATUS is a nationally representative survey that has been administered annually since 2003 by the Bureau of Labor Statistics. The ATUS collects a 24-hour retrospective time diary from one individual aged 15 or over per household from a subset of the Current Population Survey (CPS) sampling frame and links each diary to socio-demographic household data from the CPS. The analytic sample of almost 15,000 time diaries is restricted to respondents who self-reported being enrolled in high school or college at the time of completing the time diary, for whom basic demographic variables are observed.[6] Because weekends and certain demographic groups and months are oversampled by the ATUS, all subsequent analyses are weighted by person-day weights that account for unequal probabilities of selection across households, months, and days of the week. The person-day nature of the sampling weights reinforces the fact that time diary surveys sample both individuals and calendar days.

Table 2 summarizes the students who comprise the three analytic samples of the three populations of interest: high school students, full-time college students, and part-time college students. The first set of statistics summarize students' participation in three activities, homework, classes, and television viewing, on both the intensive and extensive margins. On average, full-time college students perform about 1.5 hours of homework per day, which is about twice as much time spent by either high school or part-time college students. Daily average homework time is significantly larger for students who engage in at least some homework on the diary day for all three types of students, and this increase is reflected in daily participation rates: only about 40 percent of high school and full-time college students, and 27 percent of part-time college students, performed any homework on the diary day. On both the intensive and extensive margins, full-time college students spend more time on homework than the other types of students. However, while high school students are more likely to participate in homework than part-time college students on a given day, conditional on participating in homework, part-time college students spend about 50 more minutes per day than high school students. This could be because part-time college students have more compartmentalized schedules, where some days are allocated to classes and coursework while other days are allocated to childcare or participation in the labor market.

High school students spend significantly more time in class, and are more likely to spend any time in class, than college students. Similarly, full-time college students spend more time in class, and are more likely to spend any time in class, than part-time college students. Intuitively, these differences are due to systematic differences in the school schedules of different types of students.

Television viewing habits are similar across student groups. Television viewing is common, as 70 to 76 percent of students watch at least some television on any given day and conditional on doing so, they watched more than two hours of television.

[6] We use diaries from the full calendar year, including summer months. However, as shown in Table 6, the main results are robust to using only diaries completed during the traditional academic year (September – May).

The remainder of table 2 summarizes the demographic composition of the analytic samples. The sexes are evenly represented in high school, but females outnumber males in college. The latter is consistent with the emerging gender gaps in college enrollment and completion that favor females (Bailey and Dynarski 2011). There is also an intuitive age gradient, as college students are older than high school students and part-time college students are older than full-time college students. Again, the latter is consistent with nationwide trends in college going, as are the subtle differences by race. Household income is also reported in table 2. However, the income distribution is not necessarily representative for college students' economic background because the reported income could be students' own income or their parents' income depending on whether or not the college student is a household head.

4. Econometric Model

We implement a difference-in-differences style identification strategy by estimating the following linear model by OLS: [7]

$$Time_{ist} = \tau MML_{st} + \beta X_{ist} + \delta_t + \theta_s + \theta_s t + \varepsilon_{ist}, \tag{1}$$

where *Time* is a measure (in minutes) of the time that respondent *i* spent on a particular activity on the diary day; *MML* is a binary indicator equal to one if state *s* had a medical marijuana law in effect in year *t*, and zero otherwise; *X* is a vector of observed respondent, diary, and time-varying state characteristics and dummy variables for marijuana decriminalization and marijuana legalization;[8] θ and δ are state and year fixed effects (FE), respectively; and ε is an idiosyncratic error term. Because MMLs are not uniformly enacted on January 1st and the ATUS records the specific date of the time diary, we adjust the MML indicator to equal one if and only if the MML was in effect at the time of respondent *i*'s time diary. For example, an individual surveyed in January of year *t* would not be considered treated if the MML was enacted in May of year *t*. The parameter of interest is τ, which represents the impact of state MMLs on average daily student time use.[9] Standard errors are clustered by state, to make

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[7] OLS estimates of linear time-use regressions are preferred despite the "pile-up" at zero inherent in daily time use data for two reasons. First, Stewart (2013) shows that OLS estimates are more robust than Tobit estimates when daily non-participation is caused by measurement error attributable to time diary surveys' sampling of days. Second, the linear model facilitates the inclusion of state fixed effects and state-specific linear time trends that aid in identification.

[8] In addition to the variables listed in Table 1, we also control for diary month and days of week fixed effects and a dummy variable indicating holiday. In our sample period, states that remove criminal status of marijuana possession are California (2011), Connecticut (2012), and Massachusetts (2009). States that legalize marijuana are Colorado (2013) and Washington (2013). (The years that these laws become effective are in the parenthesis.)

[9] One potential concern with the binary MML indicator is that there is potential heterogeneity across state legislations (e.g., whether or not the state allows dispensaries or home cultivation) (Pacula et al. 2015). However, it is difficult to distinguish differences in laws from regime/time effects as most MMLs allowing for dispensaries

statistical inference robust to heteroskedasticity and serial correlation within states over time, as the treatment occurs at the state level.

The critical identifying assumption necessary to give OLS estimates from a difference-in-difference research design a causal interpretation is the parallel trends (i.e., common slopes) assumption. Intuitively, this means that after controlling for level differences between states with state FE, time use in both treated (MML) and control (non-MML) states was trending similarly. However, if unobserved within-state secular trends jointly determined both time use and states' adoptions of MMLs, the OLS estimates from a fixed effects regression will be biased. Accordingly, equation (1) conditions on state-specific linear time trends ($\theta_s t$). This explicitly relaxes the parallel trends assumption by allowing each state, regardless of treatment status, to follow its own linear time trend [e.g., Wooldridge (2010)]. The resulting estimator identifies state-specific departures from trend in response to the passage of MMLs. We also estimate an event-study version of equation (1) that includes binary indicators for policy leads and lags. Intuitively, this allows us to test for "effects" of MMLs before they were passed. Should such "effects" be observed, this is evidence of differential, pre-existing trends in MML states. Together, the state time trend and event study sensitivity analysis provide evidence of the credibility of our estimates of the causal effect of MMLs on students' time use.

5. Results

5.1 Main Results

Table 3 presents baseline estimates of τ, which represent MMLs' impacts on daily time use, from equation (1). We estimate the model separately for three types of students: high school students, full-time college students, and part-time college students. Each panel of table 3 reports the estimated impacts of MMLs on daily minutes spent in one of three activities: studying and homework outside of school (upper panel), time in class or at school (middle panel), and television viewing (lower panel). For each student category, we present estimates from two specifications: First, we estimate a parsimonious specification that only includes indicators for marijuana legalization and decriminalization, state fixed effects, state-specific linear time trends, and controls for the date of the time diary (i.e., year, month, and day of week fixed effects). Second, we estimate an extended specification that also controls for students'

(home cultivation) were passed after (before) 2009 and only a handful of states enacted any specific type of MML at a given time. Therefore, as our sample sizes are not particularly large, we maintain the conventional assumption of a homogenous treatment to maintain power and avoid bias in the standard errors due to a small number of treatment groups (Conley and Taber 2011).

socio-demographic backgrounds. Thus each cell of table 3 reports the estimated effect of MMLs from a unique regression.

Columns 1 and 2 of table 3 show that there is no significant effect of MMLs on high school students' time use, as the estimates are relatively small in magnitude and not statistically significant at traditional confidence levels. This is unsurprising, given that the extant literature finds no evidence that MMLs affect high school students' marijuana use. However, the impact of MMLs on college students' time use varies by students' enrollment status. Like the case of high school students, the estimates in columns 3 and 4 provide no evidence of an effect of MMLs on full-time college students' time use. Indeed, the point estimates are relatively small in magnitude and statistically indistinguishable from zero.[10]

In contrast, columns 5 and 6 show large, statistically significant effects of MMLs on part-time college students' time use. The point estimates suggest that on an average day, after the passage of MMLs, part-time college students spend about 42 fewer minutes on homework and 37 fewer minutes attending class. These estimates are strongly statistically significant and represent increases of more than 100% relative to the average time spent in these activities prior to the passage of MMLs.[11] Interestingly, decreases in time spent in educationally productive activities are almost entirely offset by statistically significant increases in time spent watching television. This is consistent with the hypothesis that television viewing displaces time spent reading (Schmidt and Anderson 2007). The implicit assumption that television viewing is not an educationally productive activity is based on the fact that entertainment programming constitutes the majority of young adults' television viewing and most educational programming is aimed at children less than 5 years of age (Huston et al. 2007; Schmidt and Anderson 2007). The estimates suggest that part-time college students spend about 60 more minutes watching television and are robust to the inclusion of a rich set of student-level socio-demographic control variables.

The differences between full-time and part-time college students documented in table 3 are intuitive. Marijuana use rates, especially heavy usage rates, are lower among full-time

[10] Student migration is a potential concern, especially for full-time college students who leave their home state to attend college. For example, college students in nonmedical marijuana states may be from medical marijuana states and therefore were affected by MMLs when they were in high school. However, the literature finds that MMLs do not affect high school student marijuana use, so student migration should not play an important role in determining the results here.

[11] It might be counterintuitive to have estimates larger than the pre-law averages as the amount of time must be nonnegative. The main reason lies in the difference-in-difference research design. There is a secular increasing trend in time use, and the homework time and class time have increased in the control group. In other words, the counterfactual implied by the regression model is a positive increase – without effective MMLs, the average time spent on homework and class in medical marijuana states would be higher than its pre-law averages.

college students than among their similarly aged peers.[12] As Chu (2014, 2015) suggests, the impacts of MMLs are largely on the intensive margin and on heavy users. MMLs probably only affect a small proportion of full-time college students (compliers) who are perhaps struggling academically, but are not average students. The estimates in columns 3 and 4 may be insufficiently powered to detect such heterogeneous policy effects. Compared to full-time college students, part-time college students tend to be lower performing and of lower socioeconomic status. Data from the National Center for Education Statistics (NCES) suggest that part-time college students are more likely to be Hispanic, first-generation college goers, and to come from low-income families (Chen 2007). If marijuana usage indeed has negative effects on educational outcomes that are larger for lower-performing students, as found in Marie and Zölitz (2015), we would expect that the academically weaker, socioeconomically disadvantaged, first generation, and historically underrepresented students who are more likely to be part-time college students are more susceptible to the harmful effects of marijuana and increased access to marijuana. This hypothesis is consistent with the finding that MMLs affect time use among part-time but not full-time college students.

We therefore restrict attention to part-time college students in all subsequent analyses. Appendix Table 1 shows results of estimating equation (1) separately by student race and ethnicity. The effects of MMLs on black students' time use are larger in magnitude than those on white students' time use. However, as the sample sizes for black and Hispanic students are small, these estimates are noisy and not significantly different from those for white students.

In Table 4, we estimate the effects of MMLs on time use using different definitions of time use. A large proportion of part-time college students in the survey report zero time in homework (73%), in-class (83%), and television (29%).[13] One natural question is whether changes in time use are due to changes at the extensive margin or the intensive margin. To address this question, we create dummy variables indicating any time spent in each activity, and then estimate the effects of MMLs on the likelihood that a part-time college student participated in the activity on the diary day. If there is no effect of MMLs on participation, then the results presented in Table 3 suggest that MMLs primarily change people's time use on the intensive margin; otherwise, the extensive margin plays an important role. These results are

[12] For example, the Monitoring the Future survey shows that in 2014, among people who were one to four years beyond high school, the rate of daily marijuana use was about twice as high for non-college goers (10.8%) as for full-time college students (5.9%) (Johnston et al. 2015).

[13] One might be worried about that linear models provide a poor fit for the data. We also estimate the effects of MMLs by fixed effects Poisson models, and the results are quantitatively similar. These results are available upon request.

reported in the upper panel of Table 4. We find that the extensive margin is an important channel for explaining changes in homework and class time. In columns 1 and 2, MMLs decrease the likelihood of doing homework by 21 percentage points and the likelihood of attending class by 16 percentage points. However, in column 3, the estimate is very small and insignificant, so the increase in television time shown in Table 3 is primarily driven by behavioral changes on the intensive margin.

In the bottom panel of Table 4, we estimate the effects of MMLs strictly on the intensive margin and restrict the samples to be students reporting positive time on each activity. The estimates suggest that MMLs decrease homework time by 67 minutes and class time by 42 minutes. These magnitudes are similar to those in Table 3. Together with the results in the top panel of Table 4, this suggests that the main results in Table 3 are driven by changes on both the extensive and intensive margins. However, as the sample sizes are quite small for positive homework and class time and are selected samples that omit valid zeros, these estimates are merely suggestive in nature and do not precisely represent causal impacts. The estimate in column 3 shows an increase of 80 minutes in television time and is significant at 5% level, which together with the null effect on the extensive margin strongly suggests that the effect on television time in Table 3 is indeed the result of behavioral changes on the intensive margin.

5.2 Sensitivity Analyses

Table 5 presents event-study estimates. We replace the MML indicator in equation (1) with a series of binary indicators for the first partial year (Year 0) and each full year (and a dummy for year 5 and above) after the MML was passed to estimate the dynamic effects of MMLs on student time use. To test for policy endogeneity, that is, whether there were pre-policy changes in the states that eventually implemented MMLs, we also estimate specifications with indicators for years prior to the MML. To be clear, this exercise asks a lot of the data, and is underpowered in the sense that splitting the pre- and post- periods into year-specific cells yields some small state-by-year cells. Nonetheless, the estimates in Table 5 are broadly consistent with a causal interpretation of the main estimates reported in Table 3 and provide no evidence of pre-existing differential trends in treated states.

In column 1, the magnitudes of estimated effects of MMLs on time spent doing homework increase over time. We find no evidence of policy endogeneity, as the estimates for Years -1 and -2 in column 2 are small and statistically insignificant. The estimates for post-law dummies in column 2 display the same patterns and magnitudes observed in column 1, although they are less precisely estimated. For in-class time in column 3, the estimates for post-MML dummies are similar to one another, indicating that the effects of MMLs on in-class time were roughly constant over time. In column 4, the estimates for Year -1 and Year -2 are positive but imprecisely estimated, once again providing no evidence of a pre-existing differential trend in treated states. While the post-MML point estimates in column 4 are smaller, the pre-MML point estimates were positive, and the differences between pre- and post-MML point estimates tend towards -30 to -40 minutes, again consistent with the effects reported in column 3 and in the middle panel of columns 5 and 6 of Table 3.[14] Finally, the estimates in columns 5 and 6 suggest positive effects of MMLs on television time and no evidence of pre-existing differential trends, but are once again imprecisely estimated. Overall, except for homework time, we find little evidence of dynamic responses of time use to MMLs and no evidence of pre-existing differential trends that might bias the main estimates reported in Table 3. Given the relatively small number of observations in our part-time college student sample, these estimates for dynamic effects are a bit noisy, and a single post-treatment dummy that averages MMLs' effects arguably approximates the true policy effects.[15]

In Table 6, we further probe the robustness of the main results. Since the ATUS asks respondents how much they sleep, we can also measure a student's time spent doing homework, attending class, and watching television as proportions of non-sleep time. These ratios capture the allocation of available time across activities and may reduce some measurement error from individual reporting behaviors. Column 1 reports estimates of the baseline model (equation 1) in which the dependent variable is the fraction of non-sleep time spent in a given activity. These results are qualitatively similar to the main results reported in Table 3: part-time-college students spend 5% less non-sleep time doing homework and 4% less non-sleep time attending class after the passage of MMLs. Both reductions are statistically significant. As the average non-sleep time among part time college students is 15.5 hours, these estimates suggest a 47-minute decrease in homework time and a 37-minute decrease in class time, which are quantitatively similar to the point estimates reported in Table 3. The estimate for television time is large and suggests that part-time-college students spend 5% more non-sleep time on watching television, though this is imprecisely estimated.

[14] This anomaly is due to in-class time in Year -3, the reference year, being quite low. This is probably a sampling quirk: there are 42 observations in Year -3 in the treatment group, and 40 of them report zero in-class time. If we drop these 42 observations, the pre-law estimates in column 4 are close to zero while the post-law estimates in column 4 are quantitatively similar to those in column 3 and highly significant.

[15] We also estimate the dynamic effects of MMLs for high school students and college full-time students. The results are consistent with the estimates in Table 3 and do not show any significant effect of MMLs on time use.

The ATUS was conducted at the individual level, while MMLs vary only at the state level. Roughly, the MML estimates from individual-level regressions are the weighted average of the policy effects in each state weighted by state populations. One potential concern is that the difference-in-differences estimates are driven by one or two states with large populations if there is substantial heterogeneity in the policy effects (Solon, Haider, and Wooldridge 2015). To address this, we average the individual-level data to the state-level, including the dependent time-use variables and control variables, and re-run the regressions at the state-level. The estimates are shown in column 2. As expected, the estimated standard errors become larger: the estimates for homework time and television time remain significant at the 10% level, while the estimate for in-class time loses its significance. Nevertheless, the point estimates are remarkably similar to those in Table 3. This suggests that the baseline estimates of the effects of MMLs are not driven by larger states, and that the policy effects are relatively homogenous across states.

In addition, we examine the sensitivity of the results to different sample restrictions. In column 3, we restrict the sample to diaries recorded during the academic year (i.e., September to May). Once again, the point estimates remain qualitatively similar to the preferred baseline estimates shown in Table 3. In column 4, we exclude states that passed MMLs prior to 2003 from the sample. Because our data are available since 2003, these medical marijuana states do not directly contribute to identifying the estimates of MMLs. One may be concerned with treating these medical marijuana states as control groups. The estimates in column 4 remain qualitatively similar to the baseline estimates reported in Table 3 and continue to show negative impacts of MMLs on homework time and in-class time and positive impacts on television time.

Finally, we separately estimate the effects only for MMLs that were passed before 2009. The immediate impacts of MMLs on marijuana use and therefore students' time use are probably small because the implementation of a medical marijuana program can take a few years after a MML becomes legally effective. For example, New Jersey passed its MML in 2010 but the patient registration system and dispensaries did not open until the end of 2012. If the main results were driven by MMLs that only existed for a few years, the results might be spurious or attenuated towards zero. In column 5, except for in-class time, the estimated effects on homework time and television time for MMLs passed before 2009 are larger than the baseline estimates shown in Table 3.[16] Note that all of the MMLs passed before 2009 allow

[16] The estimates (standard errors) for MMLs passed after 2008 are -30.4 (14.1) for homework time, -66.1 (12.7) for in-class time, and 29.4 (38.5) for television time.

home cultivation.[17] The larger effects in column 5 are indeed consistent with the finding in Pacula et al. (2015) that the increase in marijuana use is larger in states allowing home cultivation. In sum, the results presented in Table 6 confirm that the main finding of significant, arguably causal effects of MMLs on part-time college students' time use are robust to a variety of modeling choices.

7. Conclusion

The current study uses a difference-in-differences strategy to estimate the effect of medical marijuana laws (MMLs) on students' time use. We find robust, arguably causal evidence that the passage of MMLs affected part-time postsecondary students' time use, but not that of secondary or full-time postsecondary students. Specifically, part-time postsecondary students spent about 42 fewer minutes per day on homework, 37 fewer minutes per day attending class, and about one more hour per day watching television than their part-time postsecondary counterparts in non-MML states. Changes on both the extensive and intensive margins contribute to the decrease in homework and in-class time, while the increase in television time occurs entirely on the intensive margin.

These results provide indirect evidence that marijuana use induced by increased access to marijuana affects relatively disadvantaged students' educational outcomes. They also provide suggestive evidence on the mechanisms through which marijuana use affects academic achievement. Still, a caveat of the current study is that we lack data on individual students' drug use, so these estimates represent reduced-form impact of policies that affect access to marijuana. Similarly, the ATUS data do not contain direct measures of academic achievement or persistence in postsecondary education, due to the cross-sectional nature of the survey. Still, these results identify a potentially important, and costly, unintended consequence of MML that might perpetuate socioeconomic inequities by delaying, or limiting, the postsecondary educational success of students from certain socio-demographic backgrounds and of nontraditional students who enroll part time or not immediately after high school completion.

[17] MMLs passed after 2008 try to regulate marijuana supply through state licensed dispensaries and therefore most of them do not allow home cultivation. Only Arizona and Massachusetts allow limited home cultivation for patients without access to dispensaries.

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Table 1: State Medical Marijuana Laws as of July 2015

State	Date Effective	State	Date Effective
Alaska	03/04/1999	Michigan	12/04/2008
Arizona	04/14/2011	Minnesota	05/30/2014
California	11/06/1996	Montana	11/02/2004
Colorado	06/01/2001	Nevada	10/01/2001
Connecticut	05/31/2012	New Hampshire	07/23/2013
D.C	07/27/2010	New Jersey	01/18/2010
Delaware	07/01/2011	New Mexico	07/01/2007
Hawaii	12/28/2000	New York	07/05/2014
Illinois	01/01/2014	Oregon	12/03/1998
Maine	12/22/1999	Rhode Island	01/03/2006
Maryland	06/01/2014	Vermont	07/01/2004
Massachusetts	01/01/2013	Washington	11/03/1998

Notes: Only states that passed laws before 2014 are coded as medical marijuana states in the paper. See ProCon.org (2016) for legal documents and details of laws.

Table 2: Summary Statistics

	High School		Full-Time College		Part-Time College	
	Mean	SD	Mean	SD	Mean	SD
Homework	47.8	1.3	92.4	3.1	43.4	2.3
HW (if > 0)	113.4	2.3	214.2	5.2	161.0	5.4
Pr(HW > 0)	0.42		0.43		0.27	
Class	206.0	3.1	70.7	2.9	31.9	2.3
Class (if > 0)	375.9	2.1	230.3	5.6	188.4	7.0
Pr(Class > 0)	0.55		0.31		0.17	
Television	124.9	2.0	116.8	2.5	108.7	2.8
TV (if > 0)	164.2	2.2	165.7	2.9	153.0	3.3
Pr(TV > 0)	0.76		0.70		0.71	
Male	0.51		0.45		0.41	
Age	16.32	0.016	24.14	0.11	30.47	0.21
Metro Locale	0.84		0.87		0.89	
White	0.78		0.77		0.80	
Black	0.15		0.14		0.13	
Hispanic	0.22		0.13		0.18	
Asian	0.04		0.07		0.04	
$0-$20k	0.21		0.28		0.19	
$20k-$40k	0.19		0.18		0.20	
$40k-$60k	0.16		0.14		0.17	
$60k-$75k	0.10		0.08		0.12	
$75k-$100k	0.14		0.14		0.16	
$100k-$150k	0.11		0.11		0.11	
$150k+	0.09		0.08		0.05	
N	6,313		5,090		3,276	

Notes: Time doing homework (HW), in class, and watching television (TV) is measured in daily minutes. Means and standard deviations (SD) are weighted by person-day weights that adjust for the unequal probability of sample selection across both households and days.

Table 3: Baseline Estimates of MMLs' Impact on Students' Daily Time Use

	High School		Full-Time College		Part-Time College	
	(1)	(2)	(3)	(4)	(5)	(6)
Homework	11.1	13.6	35.9	32.8	-42.6	-41.7
	(15.3)	(13.5)	(37.3)	(37.0)	(11.8)***	(12.8)***
Pre-law Avg.	48.7		83.3		37.4	
Class	5.5	1.9	16.4	13.1	-37.3	-36.5
	(11.5)	(12.4)	(17.6)	(16.6)	(15.9)**	(16.7)**
Pre-law Avg.	183.9		69.4		35.8	
Television	-18.2	-22.0	-28.2	-23.6	63.7	60.4
	(11.0)	(11.2)*	(24.6)	(23.1)	(29.8)**	(31.8)*
Pre-law Avg.	134.1		106.5		96.1	
N	6,313		5,090		3,276	
Student Controls	No	Yes	No	Yes	No	Yes

Notes: Time use is measured in daily minutes. Each cell reports the coefficient estimate on the MML indicator from a unique regression. All models contain state, year, diary month, and diary day fixed effects, state-specific linear time trends, and indicators for marijuana decriminalization and marijuana legalization. All estimates are weighted by person-day weights that adjust for the unequal probability of sample selection across both households and days. Standard errors are robust to clustering by state.
*** $p < 0.01$. ** $p < 0.05$. * $p < 0.10$.

Table 4: Estimates of MMLs' Impact on the Extensive and Intensive Margins of Part-Time College Students' Daily Time Use

	Homework (1)	Class (2)	Television (3)
Extensive margin	-0.21 (0.09)**	-0.16 (0.09)*	-0.01 (0.14)
Pre-law Avg.	0.30	0.20	0.76
N	3,276	3,276	3,276
Intensive margin	-67.5 (49.5)	-28.8 (79.4)	79.5 (35.3)**
Pre-law Avg.	123.9	179.8	125.8
N	829	376	2321
Student controls	Yes	Yes	Yes

Notes: All models contain state, year, diary month, and diary day fixed effects, state-specific linear time trends, and indicators for marijuana decriminalization and marijuana legalization. All estimates are weighted by person-day weights that adjust for the unequal probability of sample selection across both households and days. Standard errors are robust to clustering by state. *** $p < 0.01$. ** $p < 0.05$. * $p < 0.10$.

Table 5: Estimates of Dynamic Effects of MML on Part-Time College Students' Daily Time Use

	Homework		Class		Television	
	(1)	(2)	(3)	(4)	(5)	(6)
Year -2		-12.0		31.7		-0.9
		(29.6)		(41.3)		(18.5)
Year -1		10.8		33.5		27.7
		(17.6)		(25.4)		(32.3)
Year 0	-32.2	-29.8	-43.7	-9.4	10.8	28.5
	(20.8)	(27.9)	(18.9)**	(38.0)	(39.5)	(57.6)
Year 1	-49.6	-45.5	-34.7	5.0	93.0	115.1
	(13.8)***	(31.0)	(17.8)*	(35.7)	(43.0)**	(43.3)**
Year 2	-66.4	-59.8	-48.2	2.0	8.9	38.7
	(21.8)***	(37.0)	(12.6)***	(38.5)	(24.8)	(45.0)
Year 3	-85.2	-77.1	-58.8	-2.4	39.0	73.3
	(31.1)***	(53.6)	(15.9)***	(47.2)	(25.5)	(49.7)
Year 4	-120.7	-111.3	-58.3	8.7	89.8	130.3
	(31.9)***	(59.6)*	(21.9)**	(53.3)	(37.9)**	(60.8)**
Year 5+	-149.0	-138.4	-79.3	-4.0	62.9	108.4
	(33.3)***	(64.2)**	(24.0)***	(60.4)	(41.5)	(66.3)
Pre-law Avg.	41.3		22.5		108.3	
N	3,276	3,276	3,276	3,276	3,276	3,276
Student controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: All models contain state, year, diary month, and diary day fixed effects, state-specific linear time trends, and indicators for marijuana decriminalization and marijuana legalization. All estimates are weighted by person-day weights that adjust for the unequal probability of sample selection across both households and days. Standard errors are robust to clustering by state. *** $p < 0.01$. ** $p < 0.05$. * $p < 0.10$.

Table 6: Robustness Checks

	Non-sleep time	State-level regression	Academic year (Sep. - May)	No MML states before 2003	MMLs before 2009
	(1)	(2)	(3)	(4)	(5)
Homework	-0.05 (0.01)***	-46.3 (26.4)*	-23.097 (12.2)*	-42.1 (14.9)***	-60.8 (20.0)***
Pre-law Avg.	0.04	38.2	37.2	37.4	36.5
Class	-0.04 (0.02)**	-30.8 (21.3)	-45.448 (14.1)***	-38.1 (14.4)**	13.2 (36.0)
Pre-law Avg.	0.04	32.5	39.3	35.8	32.4
Television	0.05 (0.04)	72.4 (38.9)*	75.195 (30.6)**	64.4 (32.0)*	112. 6 (11.5)***
Pre-law Avg.	0.11	97.8	94.0	96.1	96.1
Obs.	3,276	486	2,562	2,604	3,276
Student controls	Yes	Yes	Yes	Yes	Yes

Notes: All models contain state, year, diary month, and diary day fixed effects, state-specific linear time trends, and indicators for marijuana decriminalization and marijuana legalization. All estimates are weighted by person-day weights that adjust for the unequal probability of sample selection across both households and days. Standard errors are robust to clustering by state. *** p < 0.01. ** p < 0.05. * p < 0.10.

Appendix Table A1: Estimates of MMLs' Impact on Part-Time College Students' Daily Time Use, by Races

	White (1)	Black (2)	Hispanic (3)
Homework	-42.7 (16.4)**	-107.5 (35.1)***	21.1 (38.0)
Pre-law Avg.	33.2	38.6	30.5
Class	-41.2 (21.9)*	-134.4 (85.1)	9.8 (46.8)
Pre-law Avg.	32.7	41.1	51.9
Television	64.6 (24.8)**	132.3 (151.1)	138.8 (93.9)
Pre-law Avg.	92.5	115.4	106.5
N	2,559	499	506
Student controls	Yes	Yes	Yes

Notes: Time use is measured in daily minutes. Each cell reports the coefficient estimate on the MML indicator from a unique regression. All models contain state, year, diary month, and diary day fixed effects, state-specific linear time trends, and indicators for marijuana decriminalization and marijuana legalization. All estimates are weighted by person-day weights that adjust for the unequal probability of sample selection across both households and days. Standard errors are robust to clustering by state. *** $p < 0.01$. ** $p < 0.05$. * $p < 0.10$.