Marijuana Legalization and Firms' Cost of Equity

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ABSTRACT

After medical marijuana legalization (MML) by U.S. states, firms' cost of equity (COE) decreases, especially for those with more growth opportunities, higher productivity, or a more skilled workforce. This policy change also reduces firm risk and leads to an increase in labor supply through increased labor force participation, employment, hours worked, and net migration. Further, home prices rise after MML, reflecting increased local housing demand due to a growing supply of workers. These findings align with theoretical models that link asset prices to labor markets and suggest that MML can lower firms' COE by mitigating labor search frictions.

Keywords: Marijuana Legalization; Cost of Equity; Labor Search Frictions; Adjustment Cost Shock; Labor Supply; Worker Skill

JEL Classifications: G12, G30, J21, I18

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I. Introduction

Marijuana is the most widely used controlled substance in the U.S., with nearly a quarter of adults reporting having used it in 2022.[1] For many years, enforcing marijuana prohibition in the U.S. has entailed substantial economic and societal costs, consuming billions of dollars in criminal justice expenditures and leading to millions of marijuana-related arrests.[2] Yet, in response to changing societal attitudes, marijuana's popularity, and growing recognition of the potential benefits of legalization, an increasing number of states have taken steps to decriminalize, regulate, and tax the use of marijuana for both medicinal and recreational purposes. This trend raises important questions about the financial implications of these policy changes, particularly for firms operating within this developing legal framework.

In this paper, we investigate a critical aspect of this evolving landscape: the impact of marijuana legalization on a firm's cost of equity (COE). On the one hand, legalizing marijuana could reduce financing costs by alleviating labor market frictions through an expanded labor supply. Specifically, MML may provide relief for health conditions that limit some individuals' participation in the workforce, reduce the effects of drug testing policies that keep individuals unemployed, or enhance a state's appeal to out-of-state workers (e.g., Nicholas and Maclean (2019), Chung and Partridge (2020), Kim, O'Connor, and Norwood (2020), Zambiasi and Stillman (2020), Pant, Wang, and Yao (2024)). Belo, Lin, and Bazdresch (2014) develop an investment-based asset pricing model showing that labor adjustment cost shocks carry a negative price of risk, i.e., shocks that decrease

[1] See, for example, the Substance Abuse and Mental Health Services Administration (https://www.samhsa.gov/marijuana) and Schaeffer (2024).

[2] Criminal justice expenditures for enforcing marijuana laws are estimated to be about $7.6 billion a year, and more than 14.2 million marijuana-related arrests were made between 2001 and 2018 (ACLU (2013, 2020)).

adjustment costs reduce a firm's required return. The intuition underlying this relation is that labor market frictions prevent firms from instantaneously adjusting their workforce and achieving full investment following productivity shocks. When a shock lowers these adjustment costs and makes finding workers easier and less costly, firms can expand and make profits more quickly, resulting in relatively lower risk and, hence, lower required returns in equilibrium.[3] Thus, to the extent that marijuana legalization eases labor market frictions by increasing worker supply, it can reduce a firm's COE.[4]

On the other hand, marijuana legalization might increase firms' financial costs by negatively affecting employee performance and safety, as well as raising potential legal liabilities stemming from worker impairment (Anderson and Rees (2023)). These concerns are especially pertinent in safety-sensitive industries, such as transportation and manufacturing, where increased marijuana use could severely adversely impact a firm's operations. Additionally, marijuana legalization may result in social costs that negatively affect firms and workers, such as increased substance abuse and homelessness (Brown, Cohen, and Felix (2025)). In light of these considerations, we focus on firms' financing costs and aim to explore whether the perceived risks and costs associated with legalizing marijuana are justified or if the benefits dominate. Our investigation is timely and consequential, as it brings attention to the intersection of public health policy and corporate finance and provides insights that can inform business decisions and public discourse in an era of changing marijuana legislation.

[3] Similarly, Kuehn, Simutin, and Wang (2017) propose a labor-based capital asset pricing model where reduced search frictions in labor markets decrease required returns.

[4] We follow the standard search-and-matching framework (e.g., Pissarides (2000)) in which a larger supply of labor is assumed to reduce adjustment costs by decreasing the time it takes for firms to find workers and increasing the probability of the quality of the worker-firm match. While this assumption may not be true in every context, it should hold, on average, for most markets most of the time. Nonetheless, we acknowledge that an increased labor supply could, under certain conditions, exacerbate frictions by increasing noise in the hiring process.

To identify the effect of legalizing marijuana on a firm's COE, we exploit the passage of U.S. state laws that legalize the use of medical marijuana. Currently, 40 states, together with the District of Columbia, have passed laws legalizing medical marijuana, with 24 states and the District of Columbia allowing the use of recreational marijuana. Importantly, medical marijuana legalization (MML) has substantially affected marijuana use. For instance, Wen, Hockenberry, and Cummings (2015) find that MML led to a 14% increase in the probability of adults aged 21 and older using marijuana in the previous month.

While there are many reasons states claim for legalizing medical marijuana, including economic and political considerations, these reasons are largely unrelated to individual firms and their financing costs. One key factor driving MML is shifting public opinion toward marijuana. Some states have also passed these laws to generate sales tax revenue and address budgetary challenges. Other states have done so in response to the opioid epidemic, seeking to provide alternative pain management options for patients. Political and lobbying groups and groups representing the interests of medical professionals, patients, and other stakeholders have also shaped the debate around marijuana and its legalization. To explore the importance of these stated reasons for MML, we estimate Weibull hazard models and find that the timing of MML is not systematically related to local economic conditions, the political leaning and other demographics of the state, and the level of a state's corporate tax rate, among other factors. Further supporting the notion that MML is independent of firms' financing costs, we find no relation between the timing of legalization and the average COE of firms in a state. We also find that across many characteristics, firms located in states that have legalized medical marijuana are statistically indistinguishable from firms in states that have not.

We measure a firm's *ex ante* COE as the discount rate that equates the present value of its future cash flows to its current stock price. We use the implied COE rather than realized stock returns because returns not only capture variation in a firm's COE but also embody critical factors such as perturbations in growth opportunities, disparities in anticipated growth rates, alterations in investors' risk aversion, and changes in sentiment (e.g., Stulz (1999), Hail and Leuz (2006, 2009), Chen, Kacperczyk, and Ortiz-Molina (2011a), Chen, Chen, and Wei (2011b)). Fama and French (1997) also assert that expected returns, when calculated from *ex post* data and asset pricing models, exhibit imprecision due to the uncertainty surrounding factor premiums and potential noise in factor loading estimates. In contrast, implied COE models disentangle a firm's COE from valuations while being able to directly control for changes in cash flows, growth effects, and stock price momentum (Hail and Leuz (2006, 2009)). We measure a firm's implied COE using market prices, analyst earnings forecasts, and several cash flow discount models (Claus and Thomas (2001), Gebhardt, Lee, and Swaminathan (2001), Ohlson and Juettner-Nauroth (2005)). We also show that the results are robust when using expected cash flows derived from cross-sectional earnings forecast models instead (Hou, Van Dijk, and Zhang (2012), Li and Mohanram (2014)).

We examine the effect of MML on a firm's COE in a difference-in-differences (DiD) research design. We address recent concerns about biases arising in two-way fixed effects (TWFE) staggered DiD settings due to treatment effect heterogeneity by using an imputation-based methodology (e.g., Baker, Larcker, and Wang (2022), Borusyak, Jaravel, and Spiess (2024)). We apply this approach by using observations from firms in states that never and have not yet legalized medical marijuana to impute the counterfactuals for the observations of firms in states that have. The average treatment effect on the treated (ATT) is then calculated as the weighted sum of the differences between the observations of firms in states that have legalized medical marijuana and their imputed counterfactuals, where weights depend on treatment assignment and timing. Our tests include firm and year fixed effects, and we estimate models with and without an extensive set of firm- and state-level controls that capture a state's economic conditions, tax environments, demographics, regulations related to substance use, and other socially liberal policies.

We find that relative to firms in states that have not legalized medical marijuana, a firm's COE decreases significantly following MML. When we measure the implied COE as the average estimate across three discounting models and only include firm and year fixed effects, a firm's COE decreases by 48.4 BPS per annum after legalization. This magnitude moderates to 34.7 BPS after controlling for firm- and state-level characteristics. Moreover, test statistics proposed by Oster (2019) suggest that our estimates are unlikely to be significantly biased by omitted variables. Our findings are similar when we use the individual COE estimates from each discounting model instead of the average across the models. Given a mean COE of 8.0%, the economic effect of MML on a firm's COE is significant, implying a reduction of 4.3% to 6.1%. The results are also similar when we (i) address concerns about biases in staggered DiD treatment effects using alternative estimators from Sun and Abraham (2021) and Wooldridge (2021), (ii) include industry-by-year fixed effects to control for time-varying industry heterogeneity, (iii) exclude firms with more geographically dispersed operations, and (iv) weight the regressions by the inverse probability of a firm being treated by MML to reduce potential biases arising from differences in firm and state characteristics between treated and untreated firms. As further evidence that our results are unlikely driven by differences in states' political leanings, we show that firms' COE decreases after MML in both Democrat- and Republican-leaning states.

Timing tests reveal insignificant differences in the COE of firms in states that legalize medical marijuana and those that do not in the years before legalization, consistent with satisfying the parallel trends assumption of the DiD methodology. We also find that, conditional on MML in a state, a firm's COE decreases by an additional 47.2 to 60.2 BPS after the state opens its first dispensary and by 23.8 to 27.7 BPS after legalizing recreational marijuana.

To test whether our findings are consistent with the proposed channel that MML lowers firms' COE by alleviating labor search frictions through increasing worker supply, we conduct three sets of analyses. First, as shown by Belo et al. (2014), shocks that reduce adjustment costs permit firms to respond to productivity shocks more efficiently by allowing them to expand faster and make profits more quickly, which lowers firm risk. Consistent with MML reducing search frictions and thus risk, we find that after MML, a firm's total stock return volatility, idiosyncratic return volatility, and exposure to systematic risk decrease.

Second, we explore heterogeneity in the effect of MML on a firm's COE. Belo et al. (2014) and Belo, Li, Lin, and Zhao (2017) generate the cross-sectional predictions that the effect of MML on a firm's COE should be stronger for growing and more productive firms, those that employ more skilled workers, and more labor-intensive firms. The idea is that a reduction in labor search costs will have a larger effect on firms that are expanding and need to hire workers, employ more highly skilled workers who are more costly to hire, and rely more on labor as an input factor. We create three measures of growth expectations related to employment growth, sales growth, and market-to-book ratios, as well as one measure that captures total factor productivity (TFP). We create two measures of labor skill based on the average number of years it takes workers in an industry to learn how to perform their jobs and the R&D intensity of the firm. We measure labor intensity using selling, general, and administrative (SG&A) expenses, which are largely comprised of wages and salaries. Consistent with the interpretation that MML lowers a firm's COE by easing labor search frictions, we find that the cost-reducing effect of legalization is economically and statistically stronger for firms with higher values of growth rates, growth opportunities, TFP, worker skill levels, and R&D and SG&A expenditures.

Third, we conduct outcome-based tests of the labor supply channel. Using worker-level data from the Current Population Survey (CPS) and Internal Revenue Service (IRS) data on net migration flows to a state, we find that after MML, individuals are more likely to participate in the labor force, and, conditional on being in the labor force, be employed and work more hours. Further, states that legalize medical marijuana experience a net inflow of workers. These labor market effects of MML are especially strong for more educated workers, who tend to be in higher demand and more geographically mobile (e.g., Greenwood (1997)), and generally stronger among younger individuals who tend to be more adversely affected by marijuana testing policies and willing to relocate for job opportunities (e.g., Topel and Ward (1992), ACLU (2013, 2020), Chung and Partridge (2020)). However, for labor force participation, the effect of MML is stronger among older individuals, who may be more likely to reap the health benefits of marijuana, allowing them to increase their participation in the workforce (Nicholas and Maclean (2019), Ghimire and Maclean (2020)). We also find that home prices rise after the legalization of medical marijuana, which is consistent with MML increasing housing demand due to a larger local labor supply.[5] Finally, we show that the legalization of medical marijuana is associated with improvements in self-reported health outcomes and fewer reports of poor health or disabilities limiting individuals' ability to work. These findings suggest that the increase in the labor supply after MML may partly result from individuals with pre-existing health conditions reentering the workforce.

[5]Additional analyses indicate that not all firms benefit from a lower COE after MML. We find that for firms where workplace safety is a top priority, as captured by a higher rate of Occupational Safety and Health Administration (OSHA) inspections, MML has a much smaller to no effect on a firm's COE.

II. Related Literature and Institutional Background

A. Related Literature

Our paper contributes to at least three strands of literature. First, we contribute to the literature identifying the costs and benefits of legalizing marijuana. Prior work has shown that legalizing medical and recreational marijuana can affect several economic and social outcomes, including regional employment growth rates, housing prices, local government debt costs, unemployment, workplace injuries, crime, the use of other drugs and tobacco, personal bankruptcy rates, pain management, and innovation and creativity.[6] However, for most of these outcomes, the evidence is mixed on the direction of the effect or if there is an effect at all. In our study, we focus on the costs and benefits of MML through the lens of a firm's financing costs. Our results show that MML is beneficial in terms of reducing a firm's COE. Therefore, we add to the debate on whether the net effect of MML is positive or negative by showing that an unexplored benefit of marijuana legalization is that it lowers a firm's COE through the channel of reducing labor search frictions.[7]

Second, we contribute to the literature that examines the relationship between labor markets and equity financing costs. For example, equity returns and prices are related to labor mobility, hiring rates, worker skill, labor market pooling, labor-technology substitution, and labor flexibility (e.g., Chen et al. (2011a), Belo et al. (2014), Donangelo

[6]See, for example, Anderson, Rees, and Tekin (2018), Sabia and Nguyen (2018), Brinkman and Mok-Lamme (2019), Choi, Dave, and Sabia (2019), Dougal and Hutton (2020), Kim et al. (2020), Sabia, Dave, Alotaibi, and Rees (2024), Wu, Wen, and Wilson (2021), Dong (2022), Anderson and Rees (2023), Cheng, De Franco, and Lin (2023), and Dave, Liang, Muratori, and Sabia (2025), among others.

[7]Pant et al. (2024) find that MML is associated with higher firm valuations. However, because firm value is determined by the cost of capital as well as expected future cash flows and growth rates, it is not clear *ex ante* whether the legalization of medical marijuana impacts a firm's COE and risk. We isolate the effect of MML on a firm's discount rate from its valuation by using the firm's implied COE while controlling for cash flows and expected growth rates. Thus, our findings suggest that one channel through which MML can increase firm value is by lowering a firm's COE and risk.

(2014), Belo et al. (2017), Gu and Huang (2017), Kuehn et al. (2017), Zhang (2019), Chino (2021), Fedyk and Hodson (2023), Ge, Qiao, and Zheng (2023)). We provide two primary innovations to this work. For one, we utilize a DiD framework that accounts for heterogeneous treatment effects and employ a plausibly exogenous measure of labor adjustment costs by exploiting the passage of state laws that legalize medical marijuana as a shock that increases worker supply. Most prior work tends to rely on endogenous firm- or industry-level measures to capture labor market frictions. Further, Pástor, Sinha, and Swaminathan (2008) show that a firm's implied COE is a good proxy for its conditional expected return. Thus, unlike most previous studies that rely on realized stock returns or specific asset pricing models, by using a firm's implied COE, our findings more precisely capture the effect of labor market frictions on equity financing costs (e.g., Hail and Leuz (2006, 2009)).

Last, we contribute to the literature on the financial effects of substance use and employee health. Opioid abuse in the U.S. is an epidemic that has resulted in increased municipal financing costs (Cornaggia, Hund, Nguyen, and Ye (2022)), reduced establishment growth and firm value (Ouimet, Simintzi, and Ye (2025)), increased likelihood of loan defaults (Jansen (2023)), fewer innovations (Louca, Michaely, and Petmezas (2024), Cohle and Ortega (2023)), lower house prices (Custodio, Cvijanovic, and Wiedemann (2025)), and reduced consumer and mortgage credit (Agarwal, Li, Roman, and Sorokina (2025), Law (2024)). The negative effects of tobacco use in the workplace on corporate innovation have also been explored (Gao, Hsu, Li, and Zhang (2020)). In addition to being one of the few papers that studies the effect of legalizing marijuana on firm outcomes, our study provides the first evidence of how substance use can affect a firm's equity financing costs.

B. Institutional Background on Marijuana Legalization

Between 1840 and 1900, marijuana was legally available and utilized for medicinal purposes in the U.S. (e.g., Dong (2022)). However, concerns about its potential adverse effects prompted the establishment of the first federal commission to study marijuana in 1860. By the 1890s, a significant portion of the medical community began to view marijuana as a narcotic requiring regulatory control.[8]

Recreational marijuana use in the U.S. emerged in the early 20th century, leading to a growing movement advocating for its regulation. In 1914, the Harrison Act was passed, marking a significant legislative milestone by declaring drug use a crime. However, marijuana was not classified as a major drug like opium and heroin, which were explicitly prohibited under this Act.[9] Soon after, in 1915, California became the first state to criminalize marijuana possession, setting a precedent for subsequent state-level interventions.

By the 1930s, the Federal Bureau of Narcotics warned of increasing marijuana abuse, prompting legislative action, with 23 states criminalizing its possession by 1937 (Dave et al. (2025)). Still, the federal government hesitated, partly due to the ongoing exploration of marijuana's therapeutic potential and its economic significance in industries utilizing hemp. However, pressure from state governments eventually led the U.S. Congress to pass the "Marihuana" Tax Act of 1937, effectively prohibiting marijuana through federal taxation powers (Cheng et al. (2023)). This Act imposed a tax on marijuana and introduced stringent regulatory measures governing its importation, cultivation, possession, and distribution, resulting in fines and imprisonment for violations (Sabia and Nguyen (2018)).

[8] See, for example, About Cannabis Policy, *Alcohol Policy Information Systems*, 2023, https://alcoholpolicy.niaaa.nih.gov/about/about-cannabis-policy.

[9] See, for example, Did You Know... Marijuana Was Once a Legal Cross-Border Import?, *U.S. Customs and Border Protection*, 2019, https://www.cbp.gov/about/history/did-you-know/marijuana.

While aimed primarily at curbing recreational marijuana use, the 1937 Marihuana Tax Act inadvertently impacted industrial hemp production, rendering its importation and commercial production economically unviable. The Act also led to a decline in scientific research and medical testing of marijuana. In 1956, marijuana was included in the Federal Narcotics Control Act, leading to strict federal penalties for its possession. Despite federal regulations, marijuana use remained prevalent during the 1960s.

In response to marijuana's enduring popularity, Congress enacted the Comprehensive Drug Abuse Prevention and Control Act (CDAPCA) of 1970, further solidifying federal regulations on marijuana by listing it as a controlled substance (e.g., Cheng et al. (2023)). The Act classified marijuana as a Schedule I substance along with drugs like heroin and cocaine, signifying its perceived potential for abuse and negligible medical value (e.g., Sabia and Nguyen (2018)). Moreover, the Controlled Substances Act (CSA), which was included as part of the CDAPCA, ushered in more stringent regulations, prohibiting the manufacture, importation, possession, and distribution of marijuana in the U.S., with violations resulting in criminal and civil penalties (e.g., Vakili and Zhang (2018)). The Drug Enforcement Administration was established in 1973 to enforce federal laws related to controlled substances.

Efforts at decriminalization emerged at both federal and state levels following the CSA. President Nixon appointed a commission to review marijuana laws; in 1972, the commission recommended decriminalizing marijuana. However, President Nixon declined to act on that recommendation. Subsequent initiatives and efforts arose to decriminalize and legalize marijuana in the late 1970s and early 1980s as attitudes started to shift culturally, especially among younger Americans (e.g., Vakili and Zhang (2018)).[10] A breakthrough came in 1996 when California passed Proposition 215, becoming the first state to legalize

[10]Groups advocating for marijuana legalization, such as the National Organization for the Reform of Marijuana Laws, have also been active in pushing for legislative change since the 1970s.

medical marijuana, with the first dispensary opening later that year (e.g., Nicholas and Maclean (2019)). This action sparked a state-by-state movement toward the legalization of marijuana for medical purposes. Currently, 40 states, together with the District of Columbia, have legalized some form of medical marijuana.

Another milestone was reached in 2012, when Colorado and Washington became the first states to legalize the use of marijuana for recreational purposes, with commercial sales beginning in 2014 (e.g., Brinkman and Mok-Lamme (2019)). Since then, 22 additional states and the District of Columbia have legalized recreational marijuana use, each with its own set of regulatory frameworks and restrictions. Table 1 summarizes the states and dates when marijuana use for medical and recreational purposes became legal.

[Insert Table 1 about here]

The current legal landscape concerning marijuana reflects a complex interplay between federal prohibition and state-level legalization efforts. While marijuana remains illegal at the federal level, recent presidential administrations have adopted varying degrees of leniency in enforcing federal marijuana laws. For instance, the Obama administration issued the Cole Memorandum in 2013, providing guidelines for federal prosecutors to deprioritize certain marijuana-related offenses (e.g., Cheng et al. (2023)). However, this approach was rescinded under the Trump administration in 2018.

Signaling a shift towards a less prohibitive policy at the federal level, the Marijuana Opportunity Reinvestment and Expungement (MORE) Act, which proposes to remove marijuana from the CSA and leave legalization decisions to the states, was passed by the U.S. House of Representatives in December 2020 (e.g., Andrews (2020)). However, the bill did not advance in the Senate.[11] In October 2022, President Biden initiated a review of federal marijuana law and pardoned thousands of U.S. citizens who were convicted federally of simple possession of marijuana (e.g., Dave et al. (2025)). The Biden administration further advocated for governors and local leaders to similarly clear prior convictions involving marijuana, acknowledging that criminal histories connected to marijuana consumption and possession have created undue obstacles, such as holding up individuals from obtaining jobs and finding adequate housing, among others. In May 2024, the Biden administration moved to downgrade marijuana from a Schedule I to Schedule III illegal drug, signifying that the federal government considered marijuana as having less potential for abuse and "a currently accepted medical use in treatment in the United States" (21 U.S.C. § 812(b)(3)) (e.g., Legare (2024), Sullivan (2024)).

The second Trump administration has yet to confirm its stance on federal marijuana prohibition, but a statement made during the presidential campaign suggests that President Trump supports the rescheduling of marijuana from a Schedule I to Schedule III illegal drug.[12] As it stands, however, marijuana use for both medical and recreational purposes remains prohibited at the federal level.

III. Data and Empirical Methodology

This section introduces our empirical measures of a firm's implied COE and describes our main sample and identification strategy.

[11] The MORE Act was reintroduced to Congress with some changes in May 2021 and was passed by the House for a second time in April 2022. The bill was subsequently referred to the Subcommittee on Health but failed to advance any further. At present, the MORE Act remains in the House after being introduced for a third time in September 2023. For the latest information on the MORE Act, see https://www.congress.gov/bill/118th-congress/house-bill/5601.

[12] See, for example, https://truthsocial.com/@realDonaldTrump/posts/113105431683796730.

A. Measuring the Implied Cost of Equity

A firm's implied COE is the discount rate that makes the present value of all expected future cash flows to equity equal to the firm's current stock price. Formally, a firm's current stock price P_t can be expressed as:

$$P_t = \sum_{i=1}^{\infty} \frac{E_t(FCFE_{t+i})}{(1+R)^i}, \tag{1}$$

where $FCFE_{t+i}$ is free cash flow to equity at time $t+i$, and R is the implied COE. We estimate a firm's implied COE using analysts' earnings forecasts from IBES.[13] Section IV B shows that our results are robust to using earnings forecasts derived from statistical models instead of analysts' forecasts.

We solve Eq. (1) for a firm's implied COE using three discount models introduced by Claus and Thomas (2001), Gebhardt et al. (2001), and Ohlson and Juettner-Nauroth (2005). The first two models are based on the residual income valuation model of Ohlson (1995), while the last is based on Ohlson and Juettner-Nauroth's abnormal earnings growth valuation model. Appendix A provides a description of the models. Given that there is little consensus as to which of these models performs best or how they should be evaluated (e.g., Gode and Mohanram (2003), Botosan and Plumlee (2005)), we follow prior work in its attempt to mitigate the effect of measurement errors associated with one particular model and use the mean estimate across all three to measure a firm's implied COE (e.g., Hail and Leuz (2006), Chen et al. (2011b), Dhaliwal, Judd, Serfling, and Shaikh (2016)).

[13] A benefit of using analyst forecasts of multiple horizons made at time t is that if MML changes a firm's expected cash flows, analysts will update their forecasts at all horizons after MML. Thus, forecasts and current prices will reflect all information known at time t.

B. Sample Selection

We construct our base sample using the CRSP-Compustat merged database from 1991 to 2019. We select 1991 as the starting year to capture the five years before the first state of California legalized medical marijuana in 1996, and we end the sample in 2019 to avoid the severe distortions in labor and capital markets caused by the COVID-19 pandemic.[14] We exclude utility (SIC 4900-4999), financial (SIC 6000-6999), and quasi-public firms (SIC greater than 9900). Analyst forecast information is obtained from IBES, financial statement data from Compustat, and stock return and price data from CRSP. We identify each firm's historical state of headquarters using two files from the CRSP/Compustat Merged databases (COMPHIST and CST_HIST) that provide historical headquarters locations back to 1994. We backfill headquarters information to 1991. Data for state-level controls are retrieved from several sources. We obtain data on corporate tax rates from the Book of the States, GDP from the U.S. Bureau of Economic Analysis, unemployment rates from the Bureau of Labor Statistics, presidential voting results by state from the Federal Elections Commission, unemployment benefits from the Department of Labor, and dates for various opioid, smoking ban, and LGBTQ nondiscrimination laws from prior studies and internet searches.

The final sample includes 44,188 firm-year observations. However, the number of observations can vary in the analyses due to using the DiD imputation method and the model specification. Appendix B provides variable definitions. We winsorize continuous variables at their 1st and 99th percentiles and express dollar values in 2022 dollars. Table 2 reports summary statistics. For example, the mean implied COE is 8.0%, and 25.9% of the firm-year observations come from states that have legalized medical marijuana.

[14]However, our main results are robust to using an extended sample through 2022.

[Insert Table 2 about here]

C. Identification Strategy

Our identification strategy exploits the staggered passage of state laws that legalize the use of medical marijuana in a DiD research design with TWFE. The traditional TWFE regression with staggered adoption of treatment takes the form of:

$$y_{it} = \alpha_i + \beta_t + \tau D_{it} + \gamma X_{it-1} + \varepsilon_{it}, \tag{2}$$

where y_{it} is the implied COE at firm i in year t or another outcome variable of interest. α_i and β_t are firm and year fixed effects, respectively. D_{it} is an indicator variable that equals one if a firm is headquartered in a state that has legalized medical marijuana by year t, and zero otherwise. X_{it-1} are lagged firm- and state-level controls (defined later in this section and Appendix B). We cluster standard errors by state to account for serial correlation in the standard errors within a firm over time and across firms within the same state.

Recent econometric advances raise concerns about the traditional TWFE model in a staggered DiD setting (e.g., Goodman-Bacon (2021), Baker et al. (2022), Borusyak et al. (2024)). Goodman-Bacon (2021) shows that the ATT obtained from TWFE DiD regressions are variance-weighted averages of many "2×2" DiD estimators from comparisons of treated and control groups in a window before and after treatment. Regarding a staggered DiD setting, the main concern is that some of these "2×2" DiD estimators are derived from comparing newly treated observations to already treated observations. These are typically referred to as "forbidden comparisons" because, in the presence of treatment effect heterogeneity, such as when the effects of treatment are realized over time, these DiD estimates are biased and can even produce the opposite sign of the true ATT. In our sample, only 22.0% of the observations come from states that never legalize medical marijuana, and using the Goodman-Bacon (2021) decomposition, we find that about 13.9% of the treatment effects of MML in our sample would be derived using these forbidden comparisons. Thus, to obtain unbiased DiD estimates using the MML setting, we require an alternative estimation technique that explicitly accounts for these concerns.

To estimate the treatment effect of MML on a firm's COE and other outcome variables while accounting for concerns with traditional staggered DiD regressions, we adopt an imputation-based estimation method following Borusyak et al. (2024). While several estimators have been proposed to account for the concerns mentioned above (e.g., De Chaisemartin and d'Haultfoeuille (2020), Callaway and Sant'Anna (2021), Sun and Abraham (2021), Wooldridge (2021)), the advantages of the imputation-based method are that it is transparent, possesses attractive efficiency properties, and permits the estimation of asymptotically conservative standard errors. The imputation estimator in our study is constructed in three steps. First, Eq. (2) is estimated on the subsample of firm-year observations from states where medical marijuana is illegal (i.e., when D_{it}=0) to obtain estimates of $\hat{\alpha}_i$, $\hat{\beta}_t$, and $\hat{\gamma}$. Second, estimates of the effect of MML for each firm-year observation from a state that has legalized medical marijuana are derived as $\hat{\tau}_{it} = Y_{it} - \hat{\alpha}_i - \hat{\beta}_t - \hat{\gamma} X_{it-1}$. Last, the ATT equals the weighted sum of these individual effects of MML, where weights depend on treatment assignment and timing.

Like any DiD analysis, the main identification assumption underlying the methodology is that firms located in states that have not legalized medical marijuana offer valid counterfactuals to the firms located in states that have legalized it. Controlling for firm fixed effects in the DiD imputation method ensures that static differences between firms in states that have and have not legalized medical marijuana do not drive our findings. Additionally, controlling for year fixed effects accounts for general trends in MML and firms' COE.

Table 3 examines the extent to which firms in states that have and have not legalized medical marijuana differ along several dimensions. Ideally, MML would be randomly assigned such that key characteristics of firms in states where medical marijuana use is legal would be indistinguishable from firms in states where it is illegal. To assess the comparability between these two groups of firms in our data, we test the covariate balance between them in the year before MML (i.e., t-1) across several firm- and state-level characteristics, including a firm's market value of equity, book-to-market ratio, book leverage, stock return over the fiscal year, cash flow, capital expenditures, analyst coverage, forecast dispersion, and forecasted long-term growth rate.[15] For state characteristics, we consider several variables that capture a state's economic condition, regulations related to substance use, propensity for socially liberal policies, and demographics. These variables include a state's per capita GDP, per capita GDP growth, highest marginal corporate tax rate, unemployment rate, fraction of voters who voted for the Democratic presidential candidate in the most recently held election, and maximum total unemployment benefit payouts. We also include variables for whether a state has passed legislation limiting opioid prescriptions, banning smoking in the workplace, and banning workplace LGBTQ discrimination as well as variables that capture the average age of the state's population and the fraction of its adults with a Bachelor's degree.

[Insert Table 3 about here]

Overall, firms in states that have and have not legalized medical marijuana display substantial covariate balance. Across these 20 characteristics, the only significant

[15] As mentioned in the introduction, a benefit of using a firm's implied COE is that it allows us to isolate discount rate effects from valuation effects by directly controlling for a firm's cash flows, recent stock price changes, and growth effects.

differences are that states that legalize medical marijuana in the following year have higher unemployment rates, have not passed opioid limits, have banned smoking in the workplace, are pro-LGBTQ, and have a more educated populace. Thus, there is some evidence that more socially liberal states are more likely to legalize medical marijuana. To account for these possibly confounding factors, we report results with and without these 20 control variables in all our tests. Section IV B further documents that our results are robust to weighting the regressions by the inverse probability of being in a state that legalizes medical marijuana. Further, as we will show shortly, the fact that including these controls has little effect on our results suggests that differences in these characteristics between states that do and do not recognize MML are not a large concern for our inferences.

Our identification strategy also assumes that a state's timing of MML is not directly related to the financing costs of firms in the state nor to local economic and political characteristics that could directly drive changes in a firm's COE. To assess the validity of this assumption, we estimate Weibull hazard models where a "failure event" is defined as the year when a state legalizes medical marijuana. The sample spans the years 1991 to 2019, and states are dropped from the sample after legalization. We lag all independent variables by one year. The key independent variable is STATE_AVGCOE, which equals the average COE across all firms in a state in a given year. We also examine whether MML is associated with the same state-level variables from Table 3.

Table 4 presents the findings. The results in columns 1-4 show a statistically insignificant coefficient on STATE_AVGCOE (t-stat = 0.95), indicating that a state's legalization of medical marijuana is unrelated to its local firms' prevailing COE. This result is also inconsistent with a potential reverse causality concern. For the state characteristics, all of the determinants are economically and statistically unrelated to MML, except for whether the state has pro-LGBTQ policies and a smoking ban, for which we occasionally find positive and marginally statistically significant associations (t-stats ranging from 1.69 to 2.06). In column 5, when we alternatively measure these state characteristics in changes, only a state's corporate tax rate (t-stat = 2.57) is related to MML. These findings generally support our assumption that state-level MML is largely exogenous to the financing costs of local firms. Additionally, we control for each of these economic and political characteristics in our fully specified regression models.

[Insert Table 4 about here]

IV. Results

This section begins by presenting our baseline and robustness analyses examining the relation between MML and firms' COE. We then explore a potential economic channel that could explain our findings by analyzing the effect of MML on firm risk, heterogeneity in its effect on firms' COE, and its impact on labor market, housing demand, and health outcomes.

A. MML and Firms' Cost of Equity

Table 5 presents results examining how MML affects a firm's COE using the DiD imputation method. Columns 1-3 report results in which the dependent variable is the average implied COE across the three most commonly used estimation models (Gebhardt et al. (2001), Claus and Thomas (2001), Ohlson and Juettner-Nauroth (2005)), while columns 4-6 tabulate results using the individual COE model estimates as the respective dependent variable.

Overall, the results show a negative ATT, implying that compared to firms headquartered in states that do not legalize medical marijuana, firms located in states that legalize it experience a decrease in their COE. When controlling for only firm and year fixed effects in column 1, the estimated ATT indicates that MML is associated with a decrease in a firm's COE of 48.4 BPS (t-stat = -5.34). Controlling for firm characteristics in column 2 slightly reduces the effect of MML on a firm's COE to 44.7 BPS (t-stat = -5.24), and further controlling for state characteristics in column 3 yields an ATT of 34.7 BPS (t-stat = -4.83). Relative to the mean COE of 8.0%, the estimates imply that MML not only has a statistically significant effect on a firm's COE but also an economically significant effect, with the corresponding reduction relative to the mean ranging from 4.3% to 6.1%. These magnitudes are comparable with those in prior work on the effects of cross-listing status, labor unions, governance, alternative work arrangements, and easier access to company filings on a firm's COE (Hail and Leuz (2009), Chen et al. (2011a), Chen et al. (2011b), Chino (2021), Lai, Lin, and Ma (2024)).

[Insert Table 5 about here]

Based on column 3, we also assess the extent to which omitted variables could bias our estimates using the method proposed by Oster (2019). This method calculates a "delta" statistic to measure how much the model's explanatory power and coefficients change when adding controls. Importantly, this statistic allows us to understand how large the influence of unobserved omitted variables compared to observed variables would have to be to eliminate the ATT. As a rule of thumb, a delta value greater than one is desirable, where a value of one indicates that unobserved and observed variables are equally important. We calculate a delta statistic of 1.75, implying that the variance explained by unobserved variables would have to be 1.75 times greater than the variance explained by the regressors in column 3 to negate the ATT. Thus, biases arising from omitted variables are likely negligible.

When we consider the individual COE model estimates in columns 4-6 as the respective dependent variable, we find a consistent decline in the COE for firms in states that have legalized medical marijuana. Across the models, MML is associated with decreases in a firm's COE of 24.7 to 48.2 BPS, with t-stats ranging from -3.32 to -5.95, respectively.

Figure 1 examines the timing of changes in firms' COE relative to the timing of states' legalization of medical marijuana. A causal interpretation of our finding that MML reduces a firm's COE using the DiD imputation method requires satisfying the parallel trends assumption. For this assumption to hold, there should be no differences in the trends in the COE of firms located in states that legalize medical marijuana and those that do not in the years before MML. We assess whether there are trend differences by plotting the DiD coefficients for up to ten years before and after legalization. The plotted results come from models that exclude or include firm- and state-level controls. Reassuringly, the figure shows no pre-trend differences in the COE of firms in states that legalize medical marijuana and those that do not; this is consistent with no anticipation by investors expecting a state to legalize medical marijuana. Moreover, beginning in the second year after MML, firms in states that legalize medical marijuana experience significant reductions in their COE.[16]

[Insert Figure 1 about here]

[16] In Table A1, we report the results from Table 5 but using the traditional OLS-based TWFE estimator. The findings indicate that when we fail to correctly implement the staggered DiD and instead use the traditional OLS approach, the negative relationship between MML and firms' COE becomes economically smaller and statistically insignificant. The difference in the results using the traditional TWFE estimator is consistent with Figure 1, which shows that the effect of MML on firms' COE becomes stronger over time. Thus, when the ATT is calculated in the traditional TWFE estimator by comparing the effect of recent MML on firms' COE to that of firms in states that had previously legalized medical marijuana (i.e., the "forbidden" comparisons), the ATT is biased toward zero and underestimated.

B. Robustness

We examine the robustness of our main finding that MML reduces a firm's COE along seven dimensions and report the results in the Online Appendix.

First, we estimate a firm's COE using EPS forecasts derived from statistical models instead of analyst forecasts. Using analyst EPS forecasts is potentially problematic for two reasons. For one, it restricts the sample to firms with sufficient analyst coverage and multi-horizon forecasts. Additionally, analyst forecasts have well-documented biases (e.g., Lim (2001), Hong and Kubik (2003)). We overcome these concerns by forecasting a firm's EPS using an earnings persistence model and a residual income model following Li and Mohanram (2014), and a third model following Hou et al. (2012). For each model, we estimate the EPS of a firm for the years t+1 to t+5 using ten years of historical data; thus, the forecasts are all made out-of-sample. We describe these models in detail in Appendix A. Table A2 shows that MML continues to be associated with a lower COE when using model-forecasted EPS.

Second, our baseline tests employ the DiD imputation method from Borusyak et al. (2024) to address concerns about heterogeneous treatment effects in TWFE DiD analyses. In Table A3, columns 1-4, we further investigate whether our results hold using the DiD estimators from Sun and Abraham (2021) and Wooldridge (2021). For both estimators, we continue to find that a firm's COE decreases after MML.

Third, instead of estimating the effect of MML on a firm's COE based solely on the date when a state legalizes medical marijuana, we consider two other important events in the MML process. For one, legalization may matter more if there is a physical place (e.g., a dispensary) to obtain medical marijuana legally. Therefore, we consider the impact of when a dispensary first opens in a state that legalizes medical marijuana on the local firms' COE.

Another potentially important event is when a state legalizes marijuana for recreational use. Again, in each of these tests, we specify whether a state has legalized medical marijuana; thus, the effects of introducing a first dispensary or legalizing recreational use are incremental to the effect of MML. Table A3 shows that there are additional reductions in a firm's COE after a dispensary opens and recreational use is legalized.

Fourth, there are a few cases of a delay between when medical marijuana is legalized and when a dispensary first opens. However, this does not mean that MML does not make marijuana more accessible upon legalization (e.g., Sabia and Nguyen (2018)).[17] Nevertheless, to ensure that our results are robust to addressing potential concerns related to the timing of MML in relation to when dispensaries open, Table A4 shows that our results hold after excluding states where there are at least five, three, and two years between the year of MML and the first dispensary opening.

Fifth, columns 1 and 2 of Panel A in Table A5 show that our results hold after controlling for the possibility that unobserved time-varying industry heterogeneity drives our findings by including the interaction of 2-digit SIC industry and year fixed effects in the regressions.

Sixth, while we match state-level marijuana legalization to a firm's headquarters state under the assumption that this location represents a substantial fraction of where its employees work, especially more skilled and higher-paid workers, this approach might create measurement error because most U.S. public firms have geographically dispersed

[17] For example, in the interim years between when Washington state legalized medical marijuana in 1998 and its first dispensary opened in 2009, patients had access to medical marijuana by means of growing their own via home or collective cultivation or through buying it from someone else who grows. Additionally, early MML also allowed caregivers to grow and supply marijuana to registered patients (commonly referred to as "caregiver cultivation"). This approach was especially common in states like California before dispensaries became widespread. Overall, these alternative channels meant that patients did not have to wait for dispensaries to open to have access to medical marijuana (e.g., Anderson et al. (2018), Sabia and Nguyen (2018), Nicholas and Maclean (2019), Ghimire and Maclean (2020)).

operations.[18] Columns 3-8 in Panel A of Table A5 show that our results are robust to excluding firms that are more likely to have dispersed operations, defined as those in industries that are more likely to have major operations in several states (retail, wholesale, and transportation), larger firms with above median values of PP&E, or firms with operations in an above median number of states reported in their 10-K filings following Garcia and Norli (2012). In Panel B, we utilize establishment-level employment data from Data Axle (formerly Infogroup) and link it to the parent firms in our main sample. We show that our results continue to hold when focusing on subsamples of firms that have at least 50%, 75%, 90%, or 100% of their employees located in their headquarters state. A caveat of this analysis is that our sample becomes more limited. Nevertheless, it is reassuring that even with the smaller samples, these robustness tests consistently indicate that firms with more of their operations in their headquarters state have a lower COE after MML.

Last, we examine the robustness of our findings to using alternative control samples and further addressing differences between firms in states that legalize medical marijuana and those in states that do not. While Table 4 shows no significant relation between political leaning and MML, the distribution of states legalizing medical marijuana (shown in Table 1) suggest Democratic-leaning states legalized earlier in our sample. To examine whether this creates a bias by invalidating the "clean control" assumption due to a large portion of control firms residing in Republican-leaning states for much of our sample, we estimate the effect of MML on firms' COE separately for Democrat- and Republican-leaning states. We create a state-level measure of political leaning as the first principal component of four variables: an indicator variable for whether a state's governor is a Democrat, the fraction of a state's House of Representatives that is Democratic, the

[18] We note that this assumption is consistent with previous work showing that a firm's main plants, operations, and R&D facilities are typically located within close proximity to the firm's headquarters (e.g., Howells (1990), Henderson and Ono (2008), Karlsson (2008)).

fraction of a state's Senate that is Democratic, and the fraction of a state that voted for the Democratic presidential nominee. Columns 1-2 of Table A6 shows that regardless of whether the treatment and control firms are restricted to be in both Democrat- or Republican-leaning states, firms' COE decreases after MML.

As shown in Table 3, there are some differences in characteristics between states that do and do not legalize medical marijuana. We examine the extent to which these differences may affect our findings in columns 3 and 4 of Table A6 by showing that the results are robust to estimating the effect of MML on firms' COE using inverse probability weighting following Austin (2011). This approach weights observations based on a firm's propensity to be in a treatment state, creating a pseudo-population where treatment assignment is independent of observed covariates. Thus, weighting helps balance the distribution of covariates between treatment and control groups, reducing potential bias from confounding factors. We estimate weights as the probability of a firm being in a state that will legalize medical marijuana in the following year using our full set of main regression controls. After applying the weights, we find no statistically significant differences in firm- and state-level characteristics between treatment and control states, and that a firm's COE continues to decrease after MML.

C. Channel Analysis

The negative relation between MML and a firm's COE appears consistent with theoretical models that analyze the relationship between labor market frictions and expected returns (Belo et al. (2014, 2017)). These models show that shocks that decrease adjustment costs reduce a firm's required return. The intuition is that if a shock lowers adjustment costs by increasing the labor supply and thus making it less costly to find new workers, a firm can expand and make profits more quickly following a productivity shock, which lowers its risk and hence required return in equilibrium. We next test whether our findings are consistent with this proposed labor channel in three ways.

1. MML and Firm Risk

We first examine the relation between MML and firm risk. To measure firm risk, we employ a firm's: (i) total stock return volatility using the standard deviation of its daily returns over the fiscal year; (ii) idiosyncratic volatility using the standard deviation of its daily idiosyncratic returns over the fiscal year; and (iii) its exposure to systematic risk, which equals the beta coefficient from a regression of its daily stock returns on CRSP value-weighted market portfolio returns over the fiscal year. Table 6 shows that firm risk decreases after MML. Columns 1 and 2 indicate that total stock return volatility declines by 4.4% to 8.7% (t-stats of -5.03 and -10.20) following MML, while columns 3 and 4 show that idiosyncratic volatility decreases by 4.4% to 9.1% (t-stats of -5.14 and -10.70). Columns 5 and 6 show that market betas fall by 9.1% to 10.1% (t-stats of -5.53 and -5.58) after legalization, translating to declines of 8.8% to 9.7% relative to the mean.

[Insert Table 6 about here]

2. Heterogeneity by Firm Characteristics

We next explore heterogeneity in the effect of MML on a firm's COE. Belo et al. (2014, 2017) generate the following key testable predictions for our study. First, from Belo et al. (2014), we hypothesize that the cost-reducing effect of MML should be more pronounced for firms with higher expected growth rates and productivity. Second, following the prediction in Belo et al. (2017), we anticipate that the negative association between MML and a firm's COE should be stronger for firms that employ more skilled workers. Third, the effect of MML and firms' COE should also be stronger for firms that rely more on labor as an input factor. The intuition is that a reduction in employee search costs will have a larger effect on firms that are expanding and need to hire workers to facilitate that growth, as well as on firms that utilize more labor, especially skilled workers who tend to be more costly to hire.

Table 7 examines the effect of MML on a firm's COE conditional on growth expectations and past productivity. The first two measures for growth expectations, employment growth and sales growth rates, capture historical growth with the expectation that firms that are growing will continue on this path. The third measure is the market-to-book ratio, which captures future growth opportunities. For productivity, we focus on total factor productivity (TFP), which measures the portion of a firm's output not accounted for by inputs such as capital, labor, and materials. We estimate TFP using a Cobb-Douglas production function following the methodology outlined in Wooldridge (2009). For all four measures, we form top and bottom terciles to define firms as facing either high or low growth expectations and TFP, respectively. We employ the DiD imputation method to estimate these cross-sectional regressions and continue to estimate models with and without firm- and state-level controls.

Overall, we find a differentially more negative effect of MML on the COE of firms with higher growth expectations and TFP. While the effect of MML is often negative and statistically significant for the COE of firms with lower growth rates and past productivity, its cost-reducing effect is statistically greater for the firms with higher growth expectations and TFP. In particular, besides column 3, which shows that, without conditioning on firm- and state-level controls, the negative effect of MML on a firm's COE is statistically similar for firms with lower and higher sales growth rates, columns 2 and 5-8 indicate that the larger effects for the high-growth and more productive firms are significant at the 1% level, and at the 2.4% and 6.1% levels in columns 1 and 4.

[Insert Table 7 about here]

Columns 1-4 of Table 8 present a similar analysis but split the firms by top and bottom terciles into those that employ more versus less skilled workers. We use two measures to capture worker skill. The first is based on the percentage of workers in occupations that require a high level of training and preparation (Belo et al. (2017)). This measure uses employment data from the Bureau of Labor Statistics (Occupation Employment Statistics program) and data from the Specific Vocational Preparation (SVP) index from the Dictionary of Occupational Titles available from the Department of Labor. The SVP index measures the amount of time a typical worker requires to learn the techniques, acquire the information, and develop the facility needed for average performance in a specific job-worker situation. SVP index values range from one to nine, with higher indicating a longer amount of preparation needed. High-skilled occupations are defined as those with a value of seven or more (i.e., an occupation that requires at least two years of preparation). Our second measure is a firm's R&D-to-sales ratio. Most R&D expenses consist of wage payments to highly skilled scientists, engineers, and other technology workers (e.g., Brown and Petersen (2011)), and thus, firms that expend more on R&D are more likely to have a higher concentration of skilled workers.

[Insert Table 8 about here]

In each specification, we find a more pronounced negative effect of MML on a firm's COE for firms with more skilled labor. Although legalization is negatively associated with the COE of firms with less skilled workers, the difference in its effect on firms with more skilled workers is statistically larger in nearly all specifications.

In columns 5 and 6, we examine the effect of MML on a firm's COE, conditional on the extent that the firm relies on labor as an input in its production process. To measure labor intensity, we follow the literature and use SG&A expenditures (e.g., Bates, Du, and Wang (2024)). Specifically, we employ lagged SG&A scaled by assets and divide firms based on these values into top and bottom terciles. The intuition behind this analysis is that a large part of SG&A expenses is comprised of salaries, wages, and bonuses to workers and managers (Eisfeldt and Papanikolaou (2013)), and thus, higher values of SG&A indicate that firms spend more on labor-related costs. Consistent with MML reducing labor adjustment costs, the effect of MML on firms' COE is statistically stronger, at more than the 1% significance level, for firms with labor intensity in the top tercile compared to those in the bottom.

In columns 7 and 8, we conduct an exploratory analysis examining how MML affects firms with more workplace safety concerns. While legalization can benefit firms via an increasing labor supply, the potential risks associated with impaired workers can create large operational and legal liabilities for safety-sensitive firms, which might dominate the benefits of increased worker supply. To explore this hypothesis, we obtain establishment-level OSHA inspection data and match the establishments to the parent firms in our sample. We then split firms into those in the top and bottom terciles of inspection rates (i.e., the number of inspections scaled by the number of employees). We assume that high inspection rates proxy for workplace safety concerns. In column 7, without firm- and state-level controls, the decrease in COE after MML is significant for firms with both high and low inspection intensities, but the effect is significantly smaller and differentially so at more than the 1% level for firms with higher OSHA inspection rates. After including controls in column 8, the cost-reducing effect of MML for safety-sensitive firms is economically smaller in magnitude and no longer statistically significant. These results suggest that the legalization of medical marijuana may not benefit firms where workplace safety is a top priority.

3. MML and Labor Market Outcomes

Based on the theoretical work of Belo et al. (2014, 2017), we hypothesize that the negative relation between MML and firms' COE is best explained by MML easing labor market search frictions and increasing worker supply. While our cross-sectional analyses are consistent with the predictions made by these models, we consider additional outcome-based support for the labor supply channel by employing the following measures based on Park and Powell (2021), Al-Sabah and Ouimet (2023), and Ouimet et al. (2025). First, we use worker-level data to examine the effect of MML on labor force participation, the likelihood of being employed, and the number of hours worked during a week. Second, we examine how MML affects net labor migration into a state. Third, we analyze home prices with the aim of capturing worker flows via their impact on local housing demand.

Table 9 explores the effect of MML on worker-level outcomes using data from the Integrated Public Use Microdata Series (IPUMS)-Current Population Survey (CPS) database. We utilize the Annual Social and Economic Supplement of the CPS, which is based on a nationally representative survey of more than 75,000 households. For our analyses, we focus on household heads in the civilian population that are between the ages of 16 and 65 and have valid family income information. With the aim of capturing changes in worker supply following MML, we consider: (i) an indicator variable equal to one if an individual is in the labor force or not, (ii) an indicator set to one if an individual, conditional on being in the labor force, is employed or not, and (iii) the usual number of hours an individual works during a week. We continue to use the DiD imputation method and cluster standard errors by state. Moreover, in addition to controlling for state and year fixed effects, we also control for the natural logarithm of total family income and include fixed effects for an individual's age, race, education level, marital status, and sex.[19]

[Insert Table 9 about here]

Columns 1 and 2 in Panel A indicate that the labor force participation rate increases by 0.40 to 1.26 percentage points (t-stats of 1.97 and 4.50, respectively) after the legalization of medical marijuana, while Panel B shows that the likelihood that an individual is employed increases by about 0.38 percentage points (t-stats of 3.11 and 3.58, respectively). Using our fully specified model, Panel C documents that the usual weekly hours an individual works increases by 0.46 hours (t-stat = 3.49). These estimates imply that, relative to their respective means, MML is associated with a 2.3% to 7.1% decrease in the fraction of workers not in the labor force, a 7.4% reduction in the unemployment rate, and a 1.1% increase in the number of weekly hours worked.

Next, in columns 3-6, we examine whether the labor market effects of MML vary based on an individual's education level, defined as having at least attained a bachelor's degree, and age. Due to higher demand for skills, better access to information, more financial resources, and greater transferability of skills, we hypothesize that more educated workers are better positioned to take advantage of employment opportunities in legalizing states (e.g., Greenwood (1997)). Additionally, the labor market effects of MML may also vary by age. For example, MML could increase the likelihood of younger individuals seeking out employment, as this group of workers tend to be more adversely affected by marijuana drug testing policies and more willing to relocate for work (e.g., Topel and Ward (1992), ACLU (2013, 2020), Chung and Partridge (2020)). Conversely, older individuals,

[19] Following Park and Powell (2021), the results in Tables 9-11 are robust to controlling for economic conditions and labor demand shocks using a Bartik-style instrument. This instrument predicts state-level employment growth by aggregating predicted industry-level growth by state, where these predictions are derived by interacting baseline (1990) state-level industry worker shares with national industry-level growth.

who might be more likely to benefit from improved health outcomes related to medical marijuana, may increase their workforce participation and work more hours following legalization (Nicholas and Maclean (2019), Ghimire and Maclean (2020)).

The findings in columns 3 and 4 indicate that the improvements in labor market outcomes after MML are all stronger for more educated individuals. However, consistent with our hypothesis discussion on age in the preceding paragraph, when we examine cross-sectional variation based on an individual's age in columns 5 and 6, we find more nuanced results. Specifically, MML is associated with an increase in labor force participation and usual number of hours worked among older individuals, whereas, conditional on being in the labor force, legalization results in increased employment rates among younger individuals.

Next, columns 1-2 of Panel A in Table 10 examine the effect of MML on state-level net migration flows using data from the IRS Statistics of Income Division, which are based on year-to-year address changes reported via individuals' income tax return filings.[20] The results show that MML is associated with an increase in net migration rates of 0.24 to 0.27 percentage points (t-stats of 3.91 and 4.34, respectively). Compared to the sample's standard deviation of net migration of 0.78 percentage points, the effects of MML on net migration are economically significant, consistent with an increasing labor supply after MML.

[Insert Table 10 about here]

In columns 3-8, we also examine the effect of MML on net migration using CPS data, primarily because it allows us to split the sample by education and age. Columns 3 and 4 show that, while the average effect of MML on net migration is positive in the full

[20] Because IRS migration data is derived from all filed tax returns, which is about 150 million annually, it provides broader coverage than the CPS data, reducing sampling error.

sample, the effect is statistically insignificant. Conversely, in columns 5 and 6, when we examine the effect of MML on net migration conditional on an individual having attained at least a bachelor's degree, we document significant increases in net migration rates of 0.41 to 0.44 percentage points (t-stats of 3.50 and 3.72, respectively). Further, we show that net migration rates increase by 0.30 to 0.34 percentage points (t-stats of 2.69 and 2.62, respectively) when focusing on the movement patterns of younger individuals in columns 7 and 8. These effects of MML on net migration are economically significant when compared to the sample's standard deviation of net migration of 1.96 percentage points.

Finally, Panel B examines how MML affects state-level home prices. The intention of this test is to provide additional evidence that worker inflows increase after MML by showing their impact on local housing demand. We measure home prices using the annualized state-level all-transaction house price index from the Federal Housing Finance Agency. The results are consistent with MML increasing local housing demand. The estimates indicate that home prices rise by 4.5% to 4.7% (t-stats of 3.24 and 2.56, respectively) following legalization.

4. MML and Health Outcomes

In our last analysis, we examine how MML affects reported health outcomes. Prior research indicates that the legalization of medical marijuana leads to reductions in prescription drug use for conditions such as pain, anxiety, and sleep disorders among elderly and disabled Medicare recipients (Bradford and Bradford (2016), Bradford, Bradford, Abraham, and Adams (2018)). Moreover, improvements in physical and mental health, increased physical activity, and reduced self-reported chronic pain (Sabia, Swigert, and Young (2017), Nicholas and Maclean (2019)) suggest that MML could enhance worker well-being. Thus, MML could increase the labor supply by improving the health conditions of individuals who would benefit from medical marijuana, allowing them to participate more in the workforce.

For these tests, shown in Table 11, we use the same CPS data and empirical specifications as those from our previous individual-level analyses. In Panel A, the dependent variable is an indicator equal to one if an individual reports having a health problem or a disability that prevents them from working or which limits the kind or amount of work they are able to perform. In Panel B, the dependent variable is an index ranging from one to five indicating an individual's perceived current level of health, with lower values representing better health.

[Insert Table 11 about here]

Columns 1 and 2 of Panel A show that MML is associated with a decrease of 0.35 to 0.38 percentage points (t-stats of -2.57 and -2.86, respectively) in the likelihood that an individual reports a disability or health condition limiting their ability to work. Relative to the 8.9% mean number of individuals reporting health and disability issues, these effects translate into decreases of 3.9% to 4.3%. The last four columns of Panel A document that these effects are differentially stronger at greater than the 1% significance level for better educated and older individuals. Panel B shows that in the fully specified model, MML is also associated with a 0.03 decrease in the health index, indicating better reported health after legalization. This translates to a decrease in the index of 1.2% relative to its mean. In columns 3-6, when we examine heterogeneity in the effects of MML on health outcomes, we find that the effects are especially pronounced for more educated and older individuals.

Overall, we interpret the evidence in Table 11 as suggesting that part of the increase in the labor supply following MML is due to the improved health outcomes of workers, especially those that are more educated and older, which enables them to rejoin the workforce.

V. Conclusion

Using the passage of state laws that legalize medical marijuana and an imputation-based DiD research design that corrects for treatment effect heterogeneity, we find that a firm's implied COE decreases after MML. As an explanation for this finding, we hypothesize that the COE-reducing effect of MML is driven by legalization easing labor search frictions and increasing worker supply. This interpretation is consistent with theoretical work that shows how reductions in labor adjustment costs can decrease firms' risk and required returns in equilibrium, especially for growing and more productive firms that need to hire workers and those that employ more skilled labor, who tend to be costlier to hire. Supporting this labor supply channel, we find that several measures of firm risk decrease after MML. Cross-sectional analyses are further consistent with this explanation, revealing that the COE-reducing effect of MML is more pronounced for firms expecting higher growth or past productivity, employing more skilled workers, or relying more on labor as a production factor.

Additional tests provide direct evidence that MML increases the labor supply. At the individual level, we find that after legalization, labor force participation, the likelihood of being employed, and the number of hours worked increase. At the state-level, MML is also associated with an increase in net migration rates and home prices. Finally, we show that the legalization of medical marijuana is associated with improvements in self-reported health outcomes and fewer reports of poor health or disabilities limiting individuals' ability to work.

Overall, our study contributes to a nascent but growing body of literature on the ramifications of marijuana legalization, offering insights into how MML can impact corporate financing costs and labor market outcomes. As marijuana policies continue to evolve, our findings provide a foundation for policymakers, researchers, and businesses to navigate the intricate landscape of marijuana legalization and its far-reaching implications for the economy and society at large.

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Appendix A. Estimation of the Implied Cost of Equity and Alternative Earnings Forecast Models

Implied Cost of Equity Models

Following previous research in this literature, we outline the most commonly used methodologies to estimate a firm's implied COE (e.g., Chen et al. (2011b), Li and Mohanram (2014), Brushwood, Dhaliwal, Fairhurst, and Serfling (2016), Dhaliwal et al. (2016)). We begin by defining the variables used in these models.

- P_t^* = Market price of a firm's common stock at time t. We use the price in June following the latest fiscal year-end to compute P_t^*.
- B_t = Book value of equity from the most recent financial statements at time t.
- $FEPS_{t+i}$ = Median forecasted earnings per share (EPS) from IBES or derived EPS forecasts for the next ith year at time t.
- $POUT$ = Forecasted dividend payout ratio. We use the ratio of the indicated annual dividends from IBES and $FEPS_{t+1}$ to measure the forecasted payout ratio. If $FEPS_{t+1}$ is negative, we assume a return on assets of 6% to calculate earnings. $POUT$ is winsorized to be within 0 and 1.

A.1 Gebhardt et al. (2001)

$$\text{(A-1)} \quad P_t^* = B_t + \sum_{i=1}^{T-1}\left[\frac{(FROE_{t+i} - R_{GLS}) \times B_{t+i-1}}{(1+R_{GLS})^i}\right] + \left[\frac{(FROE_{t+T} - R_{GLS}) \times B_{t+T-1}}{(1+R_{GLS})^{T-1}R_{GLS}}\right]$$

We employ IBES analysts' predictions as a measure of the market's anticipation of the firm's earnings in the upcoming three years. We measure market expectations for earnings by assuming a linear decrease in the future return on equity (FROE), converging to an equilibrium return on equity (ROE) from the fourth year to the Tth year. This equilibrium ROE is determined using a historical, 10-year, industry-specific median ROE. The computation of ROE involves scaling the income available for common shareholders (Compustat data item *IBC*) by the lagged total book value of equity (Compustat data item *CEQ*). We categorize all firms into Fama-French 48 industries, encompassing every company, even those with negative ROEs, to calculate the industry ROE. In instances where the industry ROE falls below the risk-free rate, we set the industry ROE equal to the risk-free rate. The future book value of equity is estimated by assuming the clean surplus relation (i.e., $B_{t+1} = B_t + EPS_{t+1} - DPS_{t+1}$). The future dividend, DPS_{t+i}, is calculated by multiplying EPS_{t+i} by $POUT$. We assume that $T = 12$. We use a numerical approximation program to solve for the R_{GLS} that equates the right- and left-hand sides of Eq. (A-1) within a difference of \$0.001.

A.2 Claus and Thomas (2001)

$$\text{(A-2)}\quad P_t^* = B_t + \sum_{i=1}^{5}\left[\frac{FEPS_{t+i} - R_{CT}\times B_{t+i-1}}{(1+R_{CT})^i}\right] + \left[\frac{(FEPS_{t+5} - R_{CT}\times B_{t+4})\times(1+g_{lt})}{(R_{CT}-g_{lt})\times(1+R_{CT})^5}\right]$$

We employ IBES earnings projections to compute abnormal earnings over the subsequent five years. Projections for earnings in the fourth and fifth years are derived from forecasts for the third year and the long-term earnings growth rate. In instances where the long-term earnings growth rate is unavailable in IBES, an implied earnings growth rate is derived from EPS_{t+2} and EPS_{t+3}. The long-term abnormal earnings growth rate is calculated by subtracting 3% from the contemporaneous risk-free rate (i.e., the yield on a 10-year Treasury bond). The estimation of the future book value of equity follows the assumption of the clean surplus relation. The subsequent dividend, DPS_{t+i}, is computed by multiplying EPS_{t+i} by the payout ratio, $POUT$. We utilize a numerical approximation program to determine R_{CT}, ensuring equality between the right- and left-hand sides of Eq. (A-2) within a difference of \$0.001.

A.3 Ohlson and Juettner-Nauroth (2005) and implemented by Gode and Mohanram (2003)

$$\text{(A-3)}\qquad R_{OJN} = A + \sqrt{A^2 + \frac{FEPS_{t+1}}{P_t^*}(g_2 - g_{lt})},$$

where

$$\text{(A-4)}\qquad A = 0.5\left(g_{lt} + \frac{DPS_{t+1}}{P_t^*}\right),$$

and where g_2 denotes the mean of the short-term earnings growth rate implied in EPS_{t+1} and EPS_{t+2}, and the long-term growth rate forecasted by analysts. The application of this model necessitates the conditions $EPS_{t+1} > 0$ and $EPS_{t+2} > 0$. The calculation of g_{lt} involves subtracting 3% from the contemporaneous risk-free rate (i.e., the yield on a 10-year Treasury bond).

Earnings Forecast Models

Instead of relying only on analyst forecasts as inputs for the aforementioned implied COE models, we also employ various model-based approaches following the methodologies proposed by Hou et al. (2012) and Li and Mohanram (2014).

For each of the three models, we conduct estimations to derive predicted earnings per share for the years $t+1$ to $t+5$. This involves estimating the models for each year between 1991 and 2019, and utilizing all available data from the preceding ten years. The independent variables in year t are multiplied by their corresponding coefficient estimates. This approach ensures that the earnings forecasts remain strictly out of sample. For instance, when forecasting earnings for year $t+1$ (e.g., the year 2001, with year t being 2000), we use data from the years 1990 to 1999. After obtaining the coefficient estimates, we multiply them by the values from the year 2000 to obtain forecasted earnings for the year 2001. Similarly, to forecast year $t+2$ (year 2002), we estimate the regressions using data from the years 1989 to 1998 and multiply the estimated coefficients by the year 2000 values.

A.4 Earnings Persistence Model

$$E_{i,t+\tau} = \beta_0 + \beta_1 NegE_{i,t} + \beta_2 E_{i,t} + \beta_3 NegE \times E_{i,t} + \varepsilon_{i,t}, \tag{A-5}$$

where $E_{i,t}$ represents the per-share income before extraordinary items, excluding special items, for firm i in year t. Additionally, $NegE_{i,t}$ is an indicator variable, taking the value of one if the firm reports negative earnings. The interaction of the two variables is denoted as $NegE \times E_{i,t}$.

A.5 Residual Income Model

$$E_{i,t+\tau} = \beta_0 + \beta_1 NegE_{i,t} + \beta_2 E_{i,t} + \beta_3 NegE \times E_{i,t} + \beta_4 B_{i,t} + \beta_5 TACC_{i,t} + \varepsilon_{i,t}, \tag{A-6}$$

where $E_{i,t}$, $NegE_{i,t}$, and $NegE \times E_{i,t}$ retain the same definitions as outlined in Eq. (A-5). Additionally, $B_{i,t}$ denotes the common shareholder equity per share, while $TACC_{i,t}$ represents total accruals. Total accruals are defined as the aggregate of changes in working capital, changes in non-current operating accruals, and changes in net financial assets, all divided by the number of shares outstanding.

A.6 Hou et al. (2012) Model

$$E_{i,t+\tau} = \beta_0 + \beta_1 A_{i,t} + \beta_2 E_{i,t} + \beta_3 NegE \times E_{i,t} + \beta_4 DIV_{i,t} + \beta_5 DD_{i,t} + \beta_6 TACC_{i,t} + \varepsilon_{i,t}, \tag{A-7}$$

where $E_{i,t}$, $NegE_{i,t}$, and $NegE \times E_{i,t}$ maintain the same definitions as specified in Eq (A-5). Additionally, $A_{i,t}$ represents the book value of assets per share, $DPS_{i,t}$ denotes common dividends per share, $DD_{i,t}$ is an indicator variable that equals one if a firm pays a common dividend and zero otherwise. Finally, $TACC_{i,t}$ stands for total accruals, which are calculated as the change in current assets less cash holdings minus the change in current liabilities less current debt and taxes paid minus depreciation, all divided by the number of shares outstanding.

Appendix B. Variable Definitions

This table provides variable definitions. Variables not included here are defined in the corresponding table captions. Compustat and CRSP variables are listed in italics when appropriate.

Variable	Definition
ΔGDP	One year percent change in per capita state-level gross domestic product.
%DEMOCRAT	The fraction of voters that vote for a Democrat during presidential elections. Voting outcomes for year t are matched to years t-1 to t+2.
AGE	The average age of a state's population. Data are from the Current Population Survey.
ANALYSTS	Number of analysts providing an annual earnings forecast.
AVGCOE	A firm's implied COE derived from analyst EPS forecasts calculated by taking the mean value across three implied COE models (Gebhardt et al. (2001), Claus and Thomas (2001), Ohlson and Juettner-Nauroth (2005)).
B/M	Book value of equity divided by market value of equity [*ceq*/(*prcc_f*× *csho*)].
BETA	The beta coefficient from a regression of a firm's daily stock returns on CRSP value-weighted market portfolio returns over the firm's fiscal year (i.e., a firm's exposure to systematic risk).
BLEV	Book value of debt scaled by book value of assets [(*dltt*+*dlc*)/*at*].
CAPEX	Capital expenditures scaled by book value of assets [(*capx*)/*at*].
CASHFLOW	Income before extraordinary items plus depreciation and amortization scaled by book value of assets [(*ib*+*dp*)/*at*].
CT	A firm's implied COE based on Claus and Thomas (2001).
DISABILITY	An indicator variable equal to one if an individual reports having a health problem or a disability which prevents them from working or which limits the kind or amount of work, and zero otherwise.
EDUC	The fraction of a state's population aged 25 or older that have a Bachelor's degree. Data are from the Current Population Survey.
EG	A firm's employment growth rate ($emp_t/emp_{t\text{-}1}$-1).
EMPLOYED	An indicator variable equal to one if, conditional on being in the labor force, an individual is employed, and zero otherwise.
FRCTSDISP	Standard deviation of analysts' annual earnings forecasts scaled by the firm's stock price. Missing values are set to zero, and we add one before taking the natural logarithm of the variable.
GDP	Per capita state-level gross domestic product in 2022 dollars.
GLS	A firm's implied COE based on the model from Gebhardt et al. (2001).
HEALTH	An index ranging from one to five indicating an individual's current health, with lower values representing excellent health.
HOURSWORKED	The usual number of hours an individual works during a week.
HPI	State-level House Price Index from the Federal Housing Finance Agency.
IDOVOL	Standard deviation of daily idiosyncratic stock returns over a firm's fiscal year. Idiosyncratic returns are the residuals from a regression of a firm's daily stock returns on CRSP value-weighted market portfolio returns over the firm's fiscal year.
LABPART	An indicator variable equal to one if an individual is in the labor force, and zero otherwise.
LGBTQ	An indicator variable that equals one if a firm is headquartered in a state that has passed laws preventing employment discrimination based on sexual orientation or gender identity by year t, and zero otherwise. Data are from https://www.lgbtmap.org.
LTGRATE	Analysts' consensus forecast of a firm's long-term earnings growth rate.
M/B	A firm's market-to-book ratio [(*prcc_f*× *csho*+*at*-*ceq*)/*at*].

MOM	Buy-and-hold stock return over a firm's fiscal year.
MVE	Market value of equity at the end of each fiscal year ($prcc\_f \times csho$) in millions and 2022 dollars.
NETMIG	The fraction of individuals moving to a state minus the fraction of individuals moving away from the state.
OJN	A firm's implied COE based on Ohlson and Juettner-Nauroth (2005).
OPIOIDLIMIT	An indicator variable that equals one if a firm is headquartered in a state that has passed laws and regulation limiting opioid prescriptions by year t, and zero otherwise. Data are from the "Report to Congress on Opioid Prescribing Limitations."
OSHA	A firm's OSHA inspection intensity, defined as the total number of inspections at a firm scaled by total employment. Inspection and employment data are from the Occupational Safety and Health Administration.
POST (D in Eq. 2)	An indicator variable that equals one if a firm is headquartered in a state that has legalized medical marijuana by year t, and zero otherwise.
R&D	A firm's research and development expenditures scaled by sales ($xrd/sale$).
SG	A firm's sales growth rate ($sale_t/sale_{t-1}$-1).
SG&A	A firm's selling, general, and administrative expenditures scaled by book value of assets ($xsga/at$) (Bates et al. (2024)).
SKILLED	Industry-level labor skill. Data are from Belo et al. (2017).
SMOKEBAN	An indicator variable that equals one if a firm is headquartered in a state that has banned smoking in the workplace by year t, and zero otherwise. Data are from Gao et al. (2020) and internet searches.
TAXRATE	State-level highest marginal corporate tax rate.
TFP	A firm's total factor productivity, which measures the portion of a firm's output not accounted for by inputs such as capital, labor, and materials (Wooldridge (2009)).
UNEMPRATE	State-level fraction of workers that are unemployed.
UNEMPBEN	State-level total unemployment benefits (max benefits times max number of weeks). Data are from the Department of Labor.
VOLATILITY	Standard deviation of daily stock returns over a firm's fiscal year.

FIGURE 1

Effect of MML on COE Over Time

This figure plots the effect of MML on a firm's implied COE (AVGCOE) over the period 1991 to 2019, estimated using the DiD imputation method. AVGCOE is derived from analyst EPS forecasts calculated by taking the mean value across three implied COE models. Lagged control variables include LN(MVE), B/M, BLEV, MOM, CASHFLOW, CAPEX, LN(ANALYSTS), LN(FRCSTDSIP), LTGRATE, LN(GDP), %ΔGDP, TAXRATE, UNEMPRATE, %DEMOCRAT, OPIOIDLIMIT, SMOKEBAN, LN(UNEMPBEN), LGBTQ, AGE, and EDUC. All variables are defined in Appendix B. 95% confidence intervals based on standard errors clustered by state are reported.

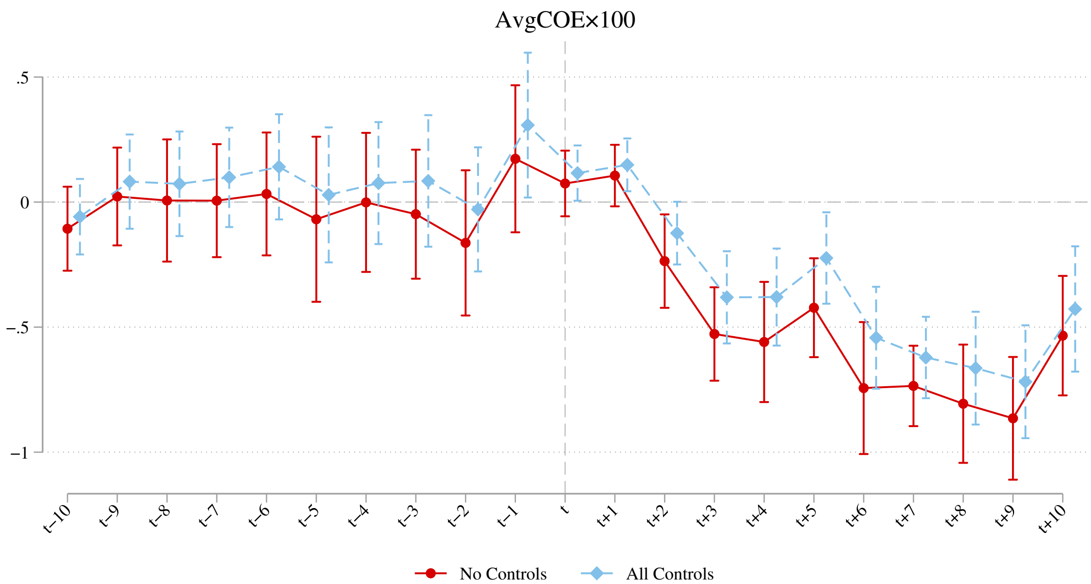

TABLE 1
Marijuana Legalization Dates

This table lists when and which states legalize medical marijuana, open their first dispensary, and legalize recreational marijuana. Dates are from Cheng et al. (2023), https://www.procon.org, and internet searches.

State	State Abbreviation	Medical	Dispensary	Recreational
Alabama	AL	2021	2022	
Alaska	AK	1998	2016	2014
Arizona	AZ	2010	2012	2020
Arkansas	AR	2016	2019	
California	CA	1996	1996	2016
Colorado	CO	2000	2005	2012
Connecticut	CT	2012	2014	2021
Delaware	DE	2011	2015	2023
District of Columbia	DC	2010	2013	2014
Florida	FL	2016	2016	
Georgia	GA			
Hawaii	HI	2000	2017	
Idaho	ID			
Illinois	IL	2013	2015	2019
Indiana	IN			
Iowa	IA			
Kansas	KS			
Kentucky	KY	2023		
Louisiana	LA	2016	2019	
Maine	ME	1999	2011	2016
Maryland	MD	2014	2017	2022
Massachusetts	MA	2012	2015	2016
Michigan	MI	2008	2009	2018
Minnesota	MN	2014	2015	2023
Mississippi	MS	2022	2023	
Missouri	MO	2018	2020	2022
Montana	MT	2004	2009	2020
Nebraska	NE	2024		
Nevada	NV	2000	2009	2016
New Hampshire	NH	2013	2016	
New Jersey	NJ	2010	2012	2020
New Mexico	NM	2007	2009	2021
New York	NY	2014	2016	2021
North Carolina	NC			
North Dakota	ND	2016	2019	
Ohio	OH	2016	2019	2023
Oklahoma	OK	2018	2018	
Oregon	OR	1998	2009	2014
Pennsylvania	PA	2016	2018	
Rhode Island	RI	2006	2013	2022
South Carolina	SC			
South Dakota	SD	2020	2021	
Tennessee	TN			
Texas	TX	2025		
Utah	UT	2018	2020	
Vermont	VT	2004	2013	2018
Virginia	VA	2021	2021	2021
Washington	WA	1998	2009	2012
West Virginia	WV	2017	2019	
Wisconsin	WI			
Wyoming	WY			

TABLE 2
Summary Statistics

This table reports summary statistics for the variables used in our main analyses over the period 1991 to 2019. There are 44,188 firm-year observations. All continuous variables are winsorized at their 1st and 99th percentiles and dollar values are expressed in 2022 dollars. Appendix B provides variable definitions.

	Mean	SD	P25	P50	P75
AVGCOE	0.080	0.029	0.060	0.076	0.095
GLS	0.060	0.033	0.037	0.063	0.082
CT	0.075	0.035	0.052	0.070	0.091
OJN	0.104	0.033	0.083	0.098	0.119
VOLATILITY	0.028	0.013	0.019	0.025	0.035
IDIOVOL	0.026	0.013	0.016	0.023	0.032
BETA	1.039	0.542	0.666	0.997	1.358
POST	0.259	0.438	0.000	0.000	1.000
LN(MVE)	7.250	1.731	6.030	7.167	8.378
B/M	0.466	0.373	0.240	0.395	0.611
BLEV	0.188	0.192	0.020	0.136	0.292
MOM	0.227	0.610	-0.122	0.122	0.420
CASHFLOW	0.092	0.107	0.060	0.097	0.140
CAPEX	0.062	0.062	0.023	0.043	0.077
LN(ANALYSTS)	2.041	0.769	1.386	2.079	2.639
LN(FRCSTDISP)	0.010	0.065	0.001	0.002	0.005
LTGRATE	0.171	0.085	0.115	0.150	0.200
Ln(GDP)	11.033	0.192	10.907	11.016	11.169
%ΔGDP	0.014	0.026	-0.000	0.015	0.030
TAXRATE	7.205	2.516	6.000	7.500	9.000
UNEMPRATE	5.978	1.865	4.700	5.600	6.900
%DEMOCRAT	0.497	0.078	0.438	0.493	0.543
OPIOIDLIMIT	0.033	0.179	0.000	0.000	0.000
SMOKEBAN	0.191	0.393	0.000	0.000	0.000
LN(UNEMPBEN)	9.579	0.311	9.402	9.574	9.755
LGBTQ	0.415	0.493	0.000	0.000	1.000
AGE	35.843	2.010	34.291	35.806	37.227
EDUC	0.280	0.061	0.239	0.272	0.320

TABLE 3
Covariate Balance

This table reports results from tests examining covariate balance between firms in states that have legalized medical marijuana and firms in states that have not in the year before legalization. MML firms are defined as those in states that will legalize medical marijuana in year *t*. The non-MML sample comprises firms in states that never or have not yet legalized medical marijuana in the year before a legalization event occurs. There are 1,060 observations in the MML sample and 18,062 observations in the non-MML sample. All variables are defined in Appendix B. *t*-statistics for tests of the differences in means are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	MML Mean	Non-MML Mean	Difference	*t*-statistic
LN(MVE)	7.232	7.266	-0.034	-0.13
B/M	0.456	0.475	-0.019	-0.88
BLEV	0.171	0.197	-0.026	-1.59
MOM	0.168	0.163	0.005	0.13
CASHFLOW	0.093	0.094	-0.001	-0.21
CAPEX	0.058	0.064	-0.006	-0.97
LN(ANALYSTS)	2.060	2.018	0.041	0.40
LN(FRCSTDISP)	0.013	0.011	0.002	0.77
LTGRATE	0.176	0.165	0.011	0.77
Ln(GDP)	11.070	11.029	0.041	0.77
%ΔGDP	0.016	0.018	-0.002	-0.38
TAXRATE	7.423	6.899	0.524	0.62
UNEMPRATE	7.318	5.636	1.682	3.34***
%DEMOCRAT	0.515	0.488	0.028	1.41
OPIOIDLIMIT	0.000	0.005	-0.005	-2.29**
SMOKEBAN	0.432	0.188	0.244	1.69*
LN(UNEMPBEN)	9.586	9.578	0.008	0.10
LGBTQ	0.657	0.285	0.372	2.98***
AGE	36.252	36.042	0.210	0.19
EDUC	0.302	0.274	0.028	1.73*

TABLE 4
Timing of MML: Duration Model

This table reports results from Weibull hazard models where a failure event is defined as the year when a state legalizes medical marijuana. The sample period is from 1991 to 2019, and states are dropped from the sample after legalizing medical marijuana. Lagged control variables are measured in year t-1. STATE_AVGCOE is the average implied COE across all firms in a state in a given year, where AVGCOE is derived from analyst EPS forecasts calculated by taking the mean value across three implied COE models. In column 5, all determinants are measured in changes. All variables are defined in Appendix B. t-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	MML				
	1	2	3	4	5
LN(GDP)	0.101	-0.112	-0.809	-0.365	-5.245
	(0.09)	(-0.10)	(-0.71)	(-0.29)	(-0.42)
%ΔGDP	-7.641	-7.021	-6.995	-6.398	-1.174
	(-1.28)	(-1.19)	(-1.05)	(-0.80)	(-0.14)
TAXRATE	-0.009	-0.097	-0.109	-0.154	0.484**
	(-0.10)	(-1.02)	(-1.08)	(-1.62)	(2.57)
UNEMPRATE	0.023	0.089	0.102	0.095	-0.031
	(0.25)	(1.00)	(1.20)	(1.13)	(-0.13)
%DEMOCRAT	3.431	0.666	-0.156	-0.385	-4.266
	(1.05)	(0.22)	(-0.05)	(-0.11)	(-0.37)
OPIOIDLIMIT		0.820	0.715	0.715	0.874
		(1.30)	(1.13)	(1.07)	(0.97)
SMOKEBAN		0.270	0.336	0.513*	0.188
		(0.87)	(1.08)	(1.71)	(0.18)
LN(UNEMPBEN)		0.557	0.436	0.405	1.730
		(0.88)	(0.70)	(0.66)	(0.77)
LGBTQ		1.240**	1.122*	1.019	1.449
		(2.06)	(1.69)	(1.54)	(1.36)
AGE			0.006	-0.004	-0.154
			(0.05)	(-0.03)	(-0.46)
EDUC			6.428	8.305	8.861
			(1.32)	(1.54)	(0.64)
STATE_AVGCOE				15.414	1.196
				(0.95)	(0.06)
X Vars in Changes					✓
N of Obs	1,151	1,151	1,151	1,053	987

TABLE 5
MML and COE

This table reports results from the DiD imputation method examining the effect of MML on a firm's implied COE over the period 1991 to 2019. The dependent variable in columns 1-3 is a firm's AVGCOE derived from analyst EPS forecasts calculated by taking the mean value across three implied COE models. In columns 4-6, a firm's implied COE is defined using the models of Gebhardt et al. (2001), Claus and Thomas (2001), and Ohlson and Juettner-Nauroth (2005), respectively. ATT measures the average treatment effect of MML on a firm's COE. Lagged firm control variables include LN(MVE), B/M, BLEV, MOM, CASHFLOW, CAPEX, LN(ANALYSTS), LN(FRCSTDSIP), and LTGRATE. Lagged state controls include LN(GDP), %ΔGDP, TAXRATE, UNEMPRATE, %DEMOCRAT, OPIOIDLIMIT, SMOKEBAN, LN(UNEMPBEN), LGBTQ, AGE, and EDUC. All variables are defined in Appendix B. t-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	AVGCOE×100			GLS×100	CT×100	OJN×100
	1	2	3	4	5	6
ATT	-0.484***	-0.447***	-0.347***	-0.482***	-0.247***	-0.371***
	(-5.34)	(-5.24)	(-4.83)	(-5.95)	(-3.32)	(-4.76)
Year FE	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓
Firm Controls		✓	✓	✓	✓	✓
State Controls			✓	✓	✓	✓
N of Obs	39,895	39,895	39,895	39,895	39,895	39,895

TABLE 6
MML and Firm Risk

This table reports results from the DiD imputation method examining the effect of MML on firm risk over the period 1991 to 2019. The dependent variables in columns 1-6 are the natural logarithm of a firm's stock return volatility, the natural logarithm of a firm's idiosyncratic stock return volatility, and a firm's market return beta. ATT measures the average treatment effect of MML on firm risk. Lagged control variables include LN(MVE), B/M, BLEV, MOM, CASHFLOW, CAPEX, LN(ANALYSTS), LN(FRCSTDSIP), LTGRATE, LN(GDP), %ΔGDP, TAXRATE, UNEMPRATE, %DEMOCRAT, OPIOIDLIMIT, SMOKEBAN, LN(UNEMPBEN), LGBTQ, AGE, and EDUC. All variables are defined in Appendix B. *t*-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	LN(VOLATILITY)		LN(IDIOVOL)		BETA	
	1	2	3	4	5	6
ATT	-0.087***	-0.044***	-0.091***	-0.044***	-0.101***	-0.091***
	(-10.20)	(-5.03)	(-10.70)	(-5.14)	(-5.58)	(-5.53)
Year FE	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓
N of Obs	39,892	39,892	39,892	39,892	39,892	39,892

TABLE 7
MML and COE: Effect of Growth and Past Productivity

This table reports results from the DiD imputation method examining the effect of MML on a firm's implied COE over the period 1991 to 2019. The dependent variable is a firm's AVGCOE derived from analyst EPS forecasts calculated by taking the mean value across three implied COE models. In columns 1-2, 3-4, 5-6, and 7-8, treatment effects are estimated for firms with low and high values of lagged employment growth, sales growth, market-to-book ratios, and total factor productivity, respectively. ATT_LOW and ATT_HIGH measure the average treatment effect of MML on a firm's COE for firms with bottom and top tercile values of the respective measure. Lagged control variables include LN(MVE), B/M, BLEV, MOM, CASHFLOW, CAPEX, LN(ANALYSTS), LN(FRCSTDSIP), LTGRATE, LN(GDP), %ΔGDP, TAXRATE, UNEMPRATE, %DEMOCRAT, OPIOIDLIMIT, SMOKEBAN, LN(UNEMPBEN), LGBTQ, AGE, and EDUC. All variables are defined in Appendix B. t-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Split by EG		Split by SG		Split by M/B		Split by TFP	
	1	2	3	4	5	6	7	8
ATT_LOW	-0.449***	-0.348***	-0.524***	-0.427***	0.107	0.224***	-0.472***	-0.326***
	(-5.68)	(-4.00)	(-5.39)	(-4.94)	(1.15)	(2.70)	(-3.90)	(-2.79)
ATT_HIGH	-0.548***	-0.493***	-0.578***	-0.547***	-0.890***	-0.649***	-0.703***	-0.596***
	(-7.77)	(-6.67)	(-7.21)	(-6.88)	(-8.53)	(-7.51)	(-6.49)	(-5.78)
Low = High p-val	0.024	0.005	0.344	0.061	<0.001	<0.001	<0.001	<0.001
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓		✓
N of Obs	26,661	26,661	26,255	26,255	26,137	26,137	27,810	27,810

TABLE 8
MML and COE: Effect of Labor Characteristics

This table reports results from the DiD imputation method examining the effect of MML on a firm's implied COE over the period 1991 to 2019. The dependent variable is a firm's AVGCOE derived from analyst EPS forecasts calculated by taking the mean value across three implied COE models. In columns 1-2, 3-4, 5-6, and 7-8, treatment effects are estimated for firms with low and high values of lagged worker skill (Belo et al. (2017)), R&D expenditures, SG&A expenditures, and OSHA inspection rates, respectively. ATT_LOW and ATT_HIGH measure the average treatment effect of MML on a firm's COE for firms with bottom and top tercile values of the respective measure. Lagged control variables include LN(MVE), B/M, BLEV, MOM, CASHFLOW, CAPEX, LN(ANALYSTS), LN(FRCSTDSIP), LTGRATE, LN(GDP), %ΔGDP, TAXRATE, UNEMPRATE, %DEMOCRAT, OPIOIDLIMIT, SMOKEBAN, LN(UNEMPBEN), LGBTQ, AGE, and EDUC. All variables are defined in Appendix B. *t*-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Split by SKILLED		Split by R&D		Split by SG&A		Split by OSHA	
	1	2	3	4	5	6	7	8
ATT_LOW	-0.359***	-0.173**	-0.151	-0.038	-0.201*	-0.097	-0.594***	-0.463***
	(-4.44)	(-2.16)	(-1.30)	(-0.34)	(-1.88)	(-0.97)	(-6.62)	(-6.36)
ATT_HIGH	-0.478***	-0.367***	-0.833***	-0.717***	-0.629***	-0.533***	-0.246**	-0.091
	(-4.87)	(-4.01)	(-8.35)	(-8.04)	(-5.95)	(-6.01)	(-2.54)	(-1.23)
Low = High *p*-val	0.072	0.004	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓		✓
N of Obs	25,702	25,702	33,073	33,073	27,029	27,029	39,864	39,864

TABLE 9
MML and Labor Market Outcomes

This table reports results examining the effect of MML on various labor market outcomes using CPS data from 1991 to 2019. The dependent variables in Panels A-C are: an indicator variable equal to one if an individual is in the labor force, and zero otherwise (LABPART); conditional on being in the labor force, an indicator variable equal to one if an individual is employed, and zero otherwise (EMPLOYED); and the usual number of hours an individual works during a week (HOURSWORKED), respectively. Columns 1-2 use the full sample, and ATT measures the average treatment effect of MML on labor outcomes. In columns 3-4 and 5-6, treatment effects (ATT_LOW and ATT_HIGH) are estimated for individuals without and with at least a bachelor's degree, and for younger and older individuals (based on median age). All models include the natural logarithm of family income and fixed effects for state, year, race, education level, age, sex, and marital status. Regressions are weighted using sample weights. Lagged control variables include LN(GDP), %ΔGDP, TAXRATE, UNEMPRATE, %DEMOCRAT, OPIOIDLIMIT, SMOKEBAN, LN(UNEMPBEN), LGBTQ, AGE, and EDUC. All variables are defined in Appendix B. t-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Dependent Variable is LABPART×100						
	Full Sample		Split by EDUC		Split by AGE	
	1	2	3	4	5	6
ATT	1.255***	0.401**				
	(4.50)	(1.97)				
ATT_LOW			0.624**	-0.133	-0.064	-0.894***
			(2.55)	(-0.63)	(-0.21)	(-3.85)
ATT_HIGH			2.248***	1.441***	2.416***	1.541***
			(6.93)	(6.21)	(7.28)	(5.62)
Low = High p-val			<0.001	<0.001	<0.001	<0.001
All FEs	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓
N of Obs	1,533,038	1,533,038	1,440,267	1,440,267	1,533,038	1,533,038
Panel B: Dependent Variable is EMPLOYED×100						
ATT	0.379***	0.377***				
	(3.58)	(3.11)				
ATT_LOW			0.206*	0.195	0.681***	0.671***
			(1.81)	(1.56)	(6.21)	(5.66)
ATT_HIGH			0.725***	0.719***	0.085	0.093
			(5.28)	(4.70)	(0.65)	(0.63)
Low = High p-val			<0.001	<0.001	<0.001	<0.001
All FEs	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓
N of Obs	1,261,178	1,261,178	1,188,032	1,188,032	1,261,178	1,261,178
Panel C: Dependent Variable is HOURSWORKED						
ATT	0.221	0.460***				
	(1.64)	(3.49)				
ATT_LOW			0.013	0.238*	0.042	0.269**
			(0.10)	(1.65)	(0.30)	(2.03)
ATT_HIGH			0.414***	0.693***	0.397***	0.647***
			(2.69)	(4.88)	(2.86)	(4.69)
Low = High p-val			<0.001	<0.001	<0.001	<0.001
All FEs	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓
N of Obs	1,028,651	1,028,651	971,306	971,306	1,028,651	1,028,651

TABLE 10
MML and Net Labor Migration and Housing Demand

This table reports results examining the effect of MML on net labor migration and home prices over the period 1991 to 2019. The dependent variables in Panels A and B are: percentage of individuals moving into a state minus the percentage of individuals leaving a state (NETMIG); and a state's House Price Index (HPI), respectively. Columns 1 and 2 in Panel A calculate net migration rates using IRS data, while columns 3-8 use CPS data. Home prices are from from the Federal Housing Finance Agency. Columns 1-4 in Panel A and all columns in Panel B use the full sample, and ATT measures the average treatment effect of MML on net migration and home prices. In columns 5-6 and 7-8 in Panel A, treatment effects (ATT_LOW and ATT_HIGH) are estimated for individuals without and with at least a bachelor's degree, and for younger and older individuals (based on median age). Regressions are weighted using population weights. Lagged control variables include LN(GDP), %ΔGDP, TAXRATE, UNEMPRATE, %DEMOCRAT, OPIOIDLIMIT, SMOKEBAN, LN(UNEMPBEN), LGBTQ, AGE, and EDUC. All variables are defined in Appendix B. t-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Dependent Variable is NETMIG×100

	Full Sample IRS		Full Sample CPS		Split by EDUC		Split by AGE	
	1	2	3	4	5	6	7	8
ATT	0.271***	0.240***	0.107	0.139				
	(4.34)	(3.91)	(0.95)	(1.16)				
ATT_LOW					-0.142	-0.094	0.312***	0.347***
					(-1.40)	(-0.91)	(2.76)	(2.69)
ATT_HIGH					0.412***	0.444***	-0.064	-0.020
					(3.50)	(3.75)	(-0.56)	(-0.17)
Low = High p-val					<0.001	<0.001	<0.001	<0.001
Year & State FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓		✓
N of Obs	1,479	1,479	1,479	1,479	2,958	2,958	2,958	2,958

Panel B: Dependent Variable is HPI

	Ln(HPI)	
	1	2
ATT	0.047**	0.045***
	(2.56)	(3.24)
Year & State FE	✓	✓
Controls		✓
N of Obs	1,479	1,479

TABLE 11
MML and Health Outcomes

This table reports results examining the effect of MML on health outcomes using CPS data over the period 1991 to 2019 in Panel A and 1996 to 2019 in Panel B. The dependent variables in Panels A and B are: an indicator variable equal to one if an individual reports having a health problem or a disability which prevents them from working or which limits the kind or amount of work, and zero otherwise (DISABILITY); and an index ranging from one to five indicating an individual's current health, with lower values representing excellent health (HEALTH), respectively. Columns 1-2 use the full sample, and ATT measures the average treatment effect of MML on health outcomes. In columns 3-4 and 5-6, treatment effects (ATT_LOW and ATT_HIGH) are estimated for individuals without and with at least a bachelor's degree, and for younger and older individuals (based on median age). All models include the natural logarithm of family income and fixed effects for state, year, race, education level, age, sex, and marital status. Regressions are weighted using sample weights. Lagged control variables include LN(GDP), %ΔGDP, TAXRATE, UNEMPRATE, %DEMOCRAT, OPIOIDLIMIT, SMOKEBAN, LN(UNEMPBEN), LGBTQ, AGE, and EDUC. All variables are defined in Appendix B. t-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Dependent Variable is DISABILITY×100						
	Full Sample		Split by EDUC		Split by AGE	
	1	2	3	4	5	6
ATT	-0.383***	-0.350**				
	(-2.86)	(-2.57)				
ATT_LOW			0.329*	0.337*	0.102	0.137
			(1.65)	(1.92)	(0.66)	(0.75)
ATT_HIGH			-1.690***	-1.687***	-0.811***	-0.779***
			(-13.15)	(-9.81)	(-4.00)	(-4.25)
Low = High p-val			<0.001	<0.001	<0.001	<0.001
All FEs	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓
N of Obs	1,533,038	1,533,038	1,440,267	1,440,267	1,533,038	1,533,038
Panel B: Dependent Variable is HEALTH						
ATT	-0.006	-0.026*				
	(-0.54)	(-1.88)				
ATT_LOW			0.066***	0.046***	0.018	-0.002
			(5.48)	(2.91)	(1.59)	(-0.17)
ATT_HIGH			-0.108***	-0.129***	-0.027**	-0.048***
			(-11.41)	(-9.48)	(-2.54)	(-3.31)
Low = High p-val			<0.001	<0.001	<0.001	<0.001
All FEs	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓
N of Obs	978,634	978,634	921,846	921,846	978,634	978,634

Marijuana Legalization and Firms' Cost of Equity

Online Appendix

TABLE A1
MML and COE: OLS

This table reports results from OLS regressions examining the effect of MML on a firm's implied COE over the period 1991 to 2019. The dependent variable in columns 1-3 is a firm's AVGCOE derived from analyst EPS forecasts calculated by taking the mean value across three implied COE models. In columns 4-6, a firm's implied COE is defined using the models of Gebhardt et al. (2001), Claus and Thomas (2001), and Ohlson and Juettner-Nauroth (2005), respectively. POST is an indicator variable equal to one if a state has legalized medical marijuana by year *t*, and zero otherwise. Lagged control variables include LN(MVE), B/M, BLEV, MOM, CASHFLOW, CAPEX, LN(ANALYSTS), LN(FRCSTDSIP), LTGRATE, LN(GDP), %ΔGDP, TAXRATE, UNEMPRATE, %DEMOCRAT, OPIOIDLIMIT, SMOKEBAN, LN(UNEMPBEN), LGBTQ, AGE, and EDUC. All variables are defined in Appendix B. *t*-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	AVGCOE×100			GLS×100	CT×100	OJN×100
	1	2	3	4	5	6
POST	-0.174	-0.156	-0.161	-0.290	-0.063	-0.124
	(-1.29)	(-1.27)	(-1.23)	(-1.46)	(-0.49)	(-1.21)
Year FE	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓
Firm Controls		✓	✓	✓	✓	✓
State Controls			✓	✓	✓	✓
N of Obs	43,247	43,247	43,247	43,247	43,247	43,247

TABLE A2

Alternative COE Measures

This table reports results from the DiD imputation method examining the effect of MML on a firm's implied COE over the period 1991 to 2019. The dependent variables in columns 1-2, 3-4, and 5-6 are a firm's AVGCOE estimated using EPS forecasts derived from an earnings persistence model (EARNPERSIST), residual income model (RESIDINC), and the model from Hou et al. (2012) (HVZ), respectively. ATT measures the average treatment effect of MML on a firm's COE. Lagged control variables include LN(MVE), B/M, BLEV, MOM, CASHFLOW, CAPEX, LN(ANALYSTS), LN(FRCSTDSIP), LTGRATE, LN(GDP), %ΔGDP, TAXRATE, UNEMPRATE, %DEMOCRAT, OPIOIDLIMIT, SMOKEBAN, LN(UNEMPBEN), LGBTQ, AGE, and EDUC. All variables are defined in Appendix B. *t*-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	EARNPERSIST×100		RESIDINC×100		HVZ×100	
	1	2	3	4	5	6
ATT	-0.409***	-0.333***	-0.335***	-0.125*	-0.329**	-0.260**
	(-3.31)	(-4.62)	(-2.62)	(-1.69)	(-2.38)	(-2.31)
Year FE	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓
N of Obs	58,107	58,107	58,656	58,656	57,710	57,710

TABLE A3
Alternative DiD Methods and Event Dates

This table reports results re-examining the effect of MML on a firm's implied COE over the period 1991 to 2019 with alternative DiD methods and other MML-related event dates. The dependent variable is a firm's AVGCOE derived from analyst EPS forecasts calculated by taking the mean value across three implied COE models. Columns 1-2 and 3-4 present results from DiD estimates of the effect of MML on a firm's COE using the method from Sun and Abraham (2021) and Wooldridge (2021), respectively. In columns 5-8, ATT_DISPENSARY and ATT_RECREATIONAL measure the average treatment effect of MML on a firm's COE using dates when a dispensary is first opened and recreational marijuana is legalized, respectively, using the DiD imputation method. These columns also control for whether a state has already legalized medical marijuana, and thus the ATTs represent the effects in addition to MML of a first dispensary opening or the legalization of recreational marijuana on firms' COE. Lagged control variables include LN(MVE), B/M, BLEV, MOM, CASHFLOW, CAPEX, LN(ANALYSTS), LN(FRCSTDSIP), LTGRATE, LN(GDP), %ΔGDP, TAXRATE, UNEMPRATE, %DEMOCRAT, OPIOIDLIMIT, SMOKEBAN, LN(UNEMPBEN), LGBTQ, AGE, and EDUC. All variables are defined in Appendix B. *t*-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	AVGCOE×100							
	1	2	3	4	5	6	7	8
ATT_SA	-0.617***	-0.519***						
	(-3.87)	(-3.42)						
ATT_WOOLDRIDGE			-0.477***	-0.331***				
			(-4.55)	(-3.65)				
ATT_DISPENSARY					-0.602***	-0.472***		
					(-8.18)	(-6.95)		
ATT_RECREATIONAL							-0.277***	-0.238***
							(-3.94)	(-3.13)
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓		✓
N of Obs	43,247	43,247	43,247	43,247	40,586	40,586	44,038	44,038

TABLE A4
Gaps Between MML and First Dispensary Dates

This table reports results examining the effect of MML on a firm's implied COE over the period 1991 to 2019. The dependent variable is a firm's AVGCOE derived from analyst EPS forecasts calculated by taking the mean value across three implied COE models. ATT measures the average treatment effect of MML on a firm's COE using the DiD imputation method. In columns 1-6, we exclude treatment states where the gap between when a state legalized marijuana and when the first dispensary was established is greater than or equal to five years, three years, and two years, respectively. Lagged control variables include LN(MVE), B/M, BLEV, MOM, CASHFLOW, CAPEX, LN(ANALYSTS), LN(FRCSTDSIP), LTGRATE, LN(GDP), %ΔGDP, TAXRATE, UNEMPRATE, %DEMOCRAT, OPIOIDLIMIT, SMOKEBAN, LN(UNEMPBEN), LGBTQ, AGE, and EDUC. All variables are defined in Appendix B. *t*-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Gap<5 Years		Gap<3 Years		Gap<2 Years	
	1	2	3	4	5	6
ATT	-0.511***	-0.361***	-0.571***	-0.454***	-0.921***	-0.775***
	(-5.83)	(-5.43)	(-6.06)	(-6.13)	(-8.39)	(-8.74)
Year FE	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓
N of Obs	37,818	37,818	31,946	31,946	21,478	21,478

TABLE A5
SIC2-Year FE and Non-Dispersed Operations

This table reports results examining the effect of MML on a firm's implied COE over the period 1991 to 2019. The dependent variable is a firm's AVGCOE derived from analyst EPS forecasts calculated by taking the mean value across three implied COE models. ATT measures the average treatment effect of MML on a firm's COE using the DiD imputation method. Columns 1 and 2 in Panel A control for two-digit SIC industry-year fixed effects. Columns 3 and 4 exclude firms in industries with dispersed operations, which include retail, wholesale, and transportation (two-digit SIC codes of 52-59, 50-51, and 40-48, respectively). Columns 5 and 6 exclude firms with values of PP&E above the median. Columns 7 and 8 exclude firms with operations in an above median number of states following Garcia and Norli (2012). In columns 1-8 of Panel B, we use establishment-level data and only keep firms with at least either 50%, 75%, 90%, or 100% of their employees located in the headquarters state. Lagged control variables include LN(MVE), B/M, BLEV, MOM, CASHFLOW, CAPEX, LN(ANALYSTS), LN(FRCSTDSIP), LTGRATE, LN(GDP), %ΔGDP, TAXRATE, UNEMPRATE, %DEMOCRAT, OPIOIDLIMIT, SMOKEBAN, LN(UNEMPBEN), LGBTQ, AGE, and EDUC. All variables are defined in Appendix B. *t*-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Include Industry-Year FE or Exclude Firms with Dispersed Operations using Compustat Data

	SIC2×Year FE		Non-Disp Industries		Smaller Firms		#States<Median	
	1	2	3	4	5	6	7	8
ATT	-0.346***	-0.347***	-0.568***	-0.395***	-0.530***	-0.407***	-0.596***	-0.431***
	(-7.13)	(-4.83)	(-5.93)	(-4.90)	(-6.63)	(-5.15)	(-5.66)	(-4.16)
SIC2×Year FE	✓	✓						
Year FE			✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓		✓
N of Obs	39,805	39,895	31,102	31,102	18,877	18,877	18,337	18,337

Panel B: Exclude Firms with Dispersed Operations using Establishment-Level Data

	InState>50%		InState>75%		InState>90%		InState=100%	
	1	2	3	4	5	6	7	8
ATT	-0.820***	-0.629***	-1.027***	-0.807***	-1.182***	-1.073***	-0.767**	-1.006**
	(-4.99)	(-3.93)	(-7.28)	(-4.58)	(-7.43)	(-4.63)	(-2.38)	(-2.34)
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓		✓
N of Obs	12,530	12,530	7,195	7,195	4,415	4,415	2,056	2,056

TABLE A6
Other Robustness

This table reports results from the DiD imputation method examining the effect of MML on a firm's implied COE over the period 1991 to 2019. The dependent variable is a firm's AVGCOE derived from analyst EPS forecasts calculated by taking the mean value across three implied COE models. In columns 1-2, treatment effects are estimated for firms in states with low and high values of lagged Democratic leaning. Democratic leaning (i.e., %DEM) is calculated as the first principal component of four variables: an indicator variable for whether a state's governor is a Democrat, the fraction of a state's House of Representatives that is Democratic, the fraction of a state's Senate that is Democratic, and the fraction of a state that voted for the Democratic presidential nominee during the general election. ATT_LOW and ATT_HIGH measure the average treatment effect of MML on a firm's COE for firms with below and above median values of the Democratic leaning measure. In columns 3-4, ATT measure the average treatment effect of MML on a firm's COE. Columns 3-4 weight the regressions by the inverse of the probability of treatment. We estimate weights using a logit regression as the probability of a firm being in a state that will legalize medical marijuana in the following year, using the same set of firm- and state-level controls as in our main regressions. For this regression, the sample is restricted to firms who are in states that will legalize medical marijuana in year t and control firms in states that never or have not yet legalized medical marijuana in the year before a legalization event occurs. These weights are then used for all the firm's observations. Lagged control variables include LN(MVE), B/M, BLEV, MOM, CASHFLOW, CAPEX, LN(ANALYSTS), LN(FRCSTDSIP), LTGRATE, LN(GDP), %ΔGDP, TAXRATE, UNEMPRATE, %DEMOCRAT, OPIOIDLIMIT, SMOKEBAN, LN(UNEMPBEN), LGBTQ, AGE, and EDUC. All variables are defined in Appendix B. t-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Split by %DEM		PS Weighted	
	1	2	3	4
ATT_LOW	-0.351***	-0.288***		
	(-3.82)	(-4.25)		
ATT_HIGH	-0.546***	-0.373***		
	(-6.05)	(-4.74)		
ATT			-0.597***	-0.549***
			(-6.77)	(-5.44)
Low = High p-val	<0.001	0.161		
Year FE	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓
Controls		✓		✓
N of Obs	39,862	39,862	38,250	38,250