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Policy impact evaluations on labour and health

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POLICY IMPACT EVALUATIONS ON LABOUR AND HEALTH

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2020

I hereby declare that this dissertation is my original work
and it has been written by me in its entirety.
I have duly acknowledged all the sources of information
which have been used in this dissertation.

This dissertation has also not been submitted for any
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Junxing Chay
9 April 2020

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Abstract

This dissertation consists of three chapters that evaluate the impacts of public policies on labour and health.

The first chapter studies a wage supplement scheme in Singapore, called the Workfare Income Supplement, which targets older low-income workers. I exploit differences in maximum benefits across age and over time to find that increasing benefits generosity encourages labour market participation and self-employment. I also find improved life satisfaction and happiness among those with low education, who are likely to be eligible for the scheme. These results suggest that wage supplements can ease some burdens of an ageing population.

The second chapter investigates the effects of raising a non-pension retirement age on labour market outcomes and subjective well-being in Singapore. Adopting a difference-in-differences identification strategy, I find an increase in employment and a decrease in retirement of older workers. Additional analyses suggest that mental anchors may be an important mechanism. I also find improved satisfaction with life as whole and with health, especially among those who are less educated, less prepared for retirement or dissatisfied with household income.

The third chapter examines heterogeneous health effects of medical marijuana legalization on young adults in the United States. Using a difference-

in-differences approach accounting for spatial spill-over, I find that states with stricter regulations generate health gains, but not states with lax access to marijuana. Subsample analysis reveal that subgroups such as Blacks, individuals from lower-income households and the uninsured experience larger gains under strict regulations. However, the low-educated, individuals from lower-income households and the uninsured are more likely to suffer worse health under lax regulations.

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Chapter 1: Does a Wage Supplement Program Work? Evidence from Older Singaporeans

1.1 Introduction

Wage supplement programs are important government transfer policies designed to encourage work among low-income individuals. In many developed countries, such programs are administered at the household level and usually take the form of refundable tax credits. The Earned Income Tax Credit (EITC) in the United States is a prominent example and extensively discussed in the literature (Hotz & Scholz, 2003).

When targeted at older low-income workers, wage supplements may lower burdens associated with population ageing such as a shrinking labour force and increased social spending. However, it is important to understand behavioural and welfare implications of a wage supplement policy for older workers. Unlike prime-age adults, they usually have fewer financial commitments, such as dependent children or home mortgage, and may even have access to partial or full financial support from government pensions and other family members. It is also important to distinguish the types of employment older workers engage in from a policy planning perspective. Apart from barriers like age discrimination and skill obsolescence, older adults may choose to work in a different capacity or environment as they did when younger. For instance, older Americans prefer a flexible working schedule and are willing to forego proportional earnings to do so (Ameriks, Briggs, Caplin, Lee, Shapiro, & Tonetti, 2020). Such information is crucial for designing policies that support the old in working longer, particularly the low-income who have more restricted work choices.

The Workfare Income Supplement (WIS) scheme in Singapore offers a unique setting to study the impact of wage supplements on older low-income workers. First introduced in 2007, it is a major welfare program which makes regular transfers that increase with age. Unlike refundable tax credit programs, WIS qualifying criteria and benefits are at the individual level, avoiding traditional welfare disincentives present in multi-person households. In this paper, I evaluate the impact of WIS benefits on labour market outcomes and welfare of older Singaporeans. To avoid simultaneity bias arising from endogenous WIS benefit amounts, I follow the literature on social insurance programs in the United States that use changes in maximum benefit levels as an exogenous source of variation (e.g. Hsu, Matsa, & Melzer, 2018; Maestas, Mullen, & Zamarro, 2012). Using the Singapore Life Panel, a nationally representative monthly panel survey of Singaporeans aged 50-70, I estimate the impact of increasing maximum WIS benefits on labour market outcomes, subjective well-being and household consumption.

My results show that WIS encourages work and work-seeking behaviour, especially among the low-educated who are likely to be low-income earners. A $100 increase in monthly maximum benefits increases labour force participation by 6.5% among those with primary education or lower. This increase translates to 6.1% increase probability of working, of which 3.9% comes from self-employment. In terms of subjective well-being, I find improved life satisfaction across various domains ranging from financial to social and family life, as well as improved feelings of happiness. The results suggest that increasing the generosity of wage supplements can improve labour outcomes and well-being of older low-income workers. More importantly, it shows that self-employment is a popular work arrangement among the old, possibly because of the flexibility it offers.

1.2 Literature Review

WIS's design is influenced by refundable tax credit programs, like the EITC, which reduce taxes owed below zero, resulting in a net payment to the household. Studies on the EITC show consistent evidence that tax credits increase labour force participation among single parent households (Eissa & Liebman, 1996; Meyer & Rosenbaum, 2001; Grogger 2003; Hotz & Scholz, 2006). Furthermore, the EITC expansions appears to improve health and subjective wellbeing (Evans & Garthwaite, 2014; Hoynes, Miller, & Simon, 2015; Boyd-Swan, Herbst, Ifcher, & Zarghamee, 2016). However, the EITC's effect on individual workers in multi-person households may vary in opposite directions. For instance, married women in couple households are less likely to work due to the tax credits, and overall family labour supply may even fall (Eissa & Hoynes, 2004).

Other programs aimed at older workers bear less resemblance to WIS. For instance, low-wage or hiring subsidies granted to firms aim to encourage hiring or retaining older employees. These demand-side subsidies are typically provided temporarily and appear to have limited overall effects on increasing employment rates (Boockmann, Zwick, Ammermüller, & Maier, 2012; Huttunen, Pirttilä, & Uusitalo, 2013; Albanese & Cockx, 2019). Tax reforms to reduce labour income taxes paid by older workers tend to be one-off, like the removal of the retirement earnings test in the United States (Friedberg, 2000; Song & Manchester, 2007), or are non-refundable, like age-targeted tax credits in Sweden (Laun, 2017). Furthermore, these tax reforms apply broadly to older workers, not just the low-income.

Several studies investigate the impact of WIS on labour outcomes. Two are by the Singapore Ministry of Trade and Industry and make use of individual-level administrative records which are publicly unavailable (Leong, Ong, Tan, & Harichandra, 2014; Lee, Leong, & Harichandra, 2014). Both adopt a difference-in-differences approach, comparing changes in probability of being an employee 3 years before and after the implementation of the scheme. Leong et al. (2014) find that WIS increased wage employment rates by up to 7.9% in the first 2 years of the scheme. They also find little effects on the intensive margins of labour supply and negative effects on gross wages. Lee et al. (2014) confirm that WIS eligibility did not adversely impact spousal labour market outcomes, which was expected since eligibility is primarily assessed at the individual level. However, effects identified in both studies may be influenced by selection as their samples were limited to those who ever worked as an employee during the analysis period. Freire (2015) used a triple-difference approach to examine WIS impact on labour force participation, with Hong Kong as a counterfactual. He finds labour participation rate of older women increase by up to 5.5% but did not detect a significant impact for men.

My study contributes to existing literature by considering WIS's impact on other important labour outcomes, such as self-employment and full-time/part-time status, as well as measures of subjective well-being and consumption, which are not available from administrative datasets.

1.3 Institutional Background

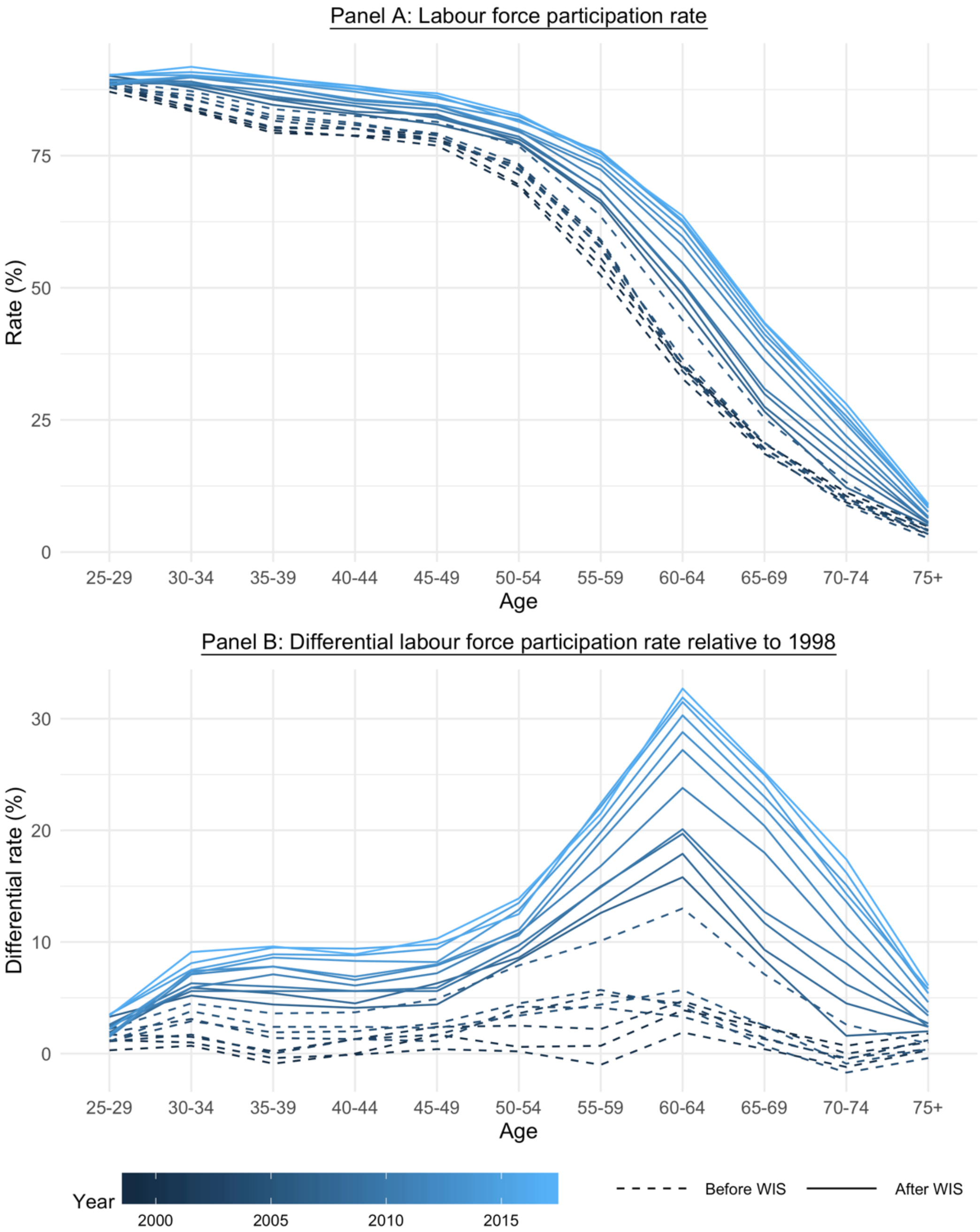
Unlike most countries, Singapore does not legislate minimum wage rates.[1] Instead, the government administers a wage supplement program called Workfare Income Supplement. It is a "permanent feature of Singapore's social security system" (Ministry of Manpower, 2017) and plays an important role in supporting low-income workers. First implemented in 2007, the scheme supports low-income workers with regular cash transfers and Central Provident Fund (CPF)[2] top-ups. Between 2007-2017, the scheme paid out about $5.5 billion to about 830,000 Singaporeans (Teo, 2018), which is an average of about $660 annually to recipients.

Figure 1 plots in the age profile of resident labour force participation for years 1999-2017. It provides preliminary evidence of WIS's impact on the age profile of labour participation. Each line in the figure represents a year, with more recent years having a darker shade. Dashed lines indicate years prior to WIS, and solid lines indicate years when WIS is in effect. Panel A shows little change in age-

[1] Workers in cleaning, security and landscaping services have a form of minimum wage through a Progressive Wage Model (https://www.mom.gov.sg/employment-practices/progressive-wage-model). This scheme differs from typical minimum wage systems in that wage requirements rise with pre-defined job role and responsibilities.

[2] Central Provident Fund is a compulsory individual savings plan administered by the Singapore government. Members contribute a portion of their monthly labour income to fund future retirement, healthcare and housing needs. (https://www.cpf.gov.sg/). CPF funds have restricted utilization and only a part of it can be withdrawn upon reaching 55 years of age.

Figure 1. Age profile of labour force participation, 1999-2017



Source: Singapore Department of Statistics specific labour participation rates between 1999 and 2005. Participation rates fall quickly from 75% to 10% between ages 50-70. In 2006, rates of the old surged substantially due to the Workfare Bonus Scheme, a one-off predecessor to WIS introduced a year before WIS. This is why we observe a gap between one dashed line and the others. After WIS was implemented in 2007, labour participation rates continue to climb, shifting the age-profile curve upwards. Panel B depicts the increases more clearly by plotting differences in labour participation rates relative to the year 1998. Although WIS targets workers aged 35 years and older, it is those aged 50-70 that experience the largest growth in labour participation. Steady growth among this age demographic over 2007-2016 matches the increase in maximum WIS benefits (See Table 1). More recently since 2016, the slowdown in labour participation growth coincides with smaller increases in generosity. Furthermore, difference in growths across age groups correspond to differences in age-specific maximum benefits. The growth in labour participation rates are greatest for those aged 60-69, followed by 55-59, then 45-54. Altogether, Figure 1 suggests that variation in maximum benefits, arising both from policy revisions and age groups, have an influence on labour market outcomes.

Table 1: WIS generosity, 2007-2019

Year	Annual maximum benefits by age group (SGD)				Income ceiling
	35-44	45-54	55-60	60+	
2007-2009	900	1,200	1,800	2,400	1,500
2010-2012	1,050	1,400	2,100	2,800	1,700
2013-2016	1,400	2,100	2,800	3,500	1,900
2017-2019	1,500	2,196	2,904	3,600	2,000

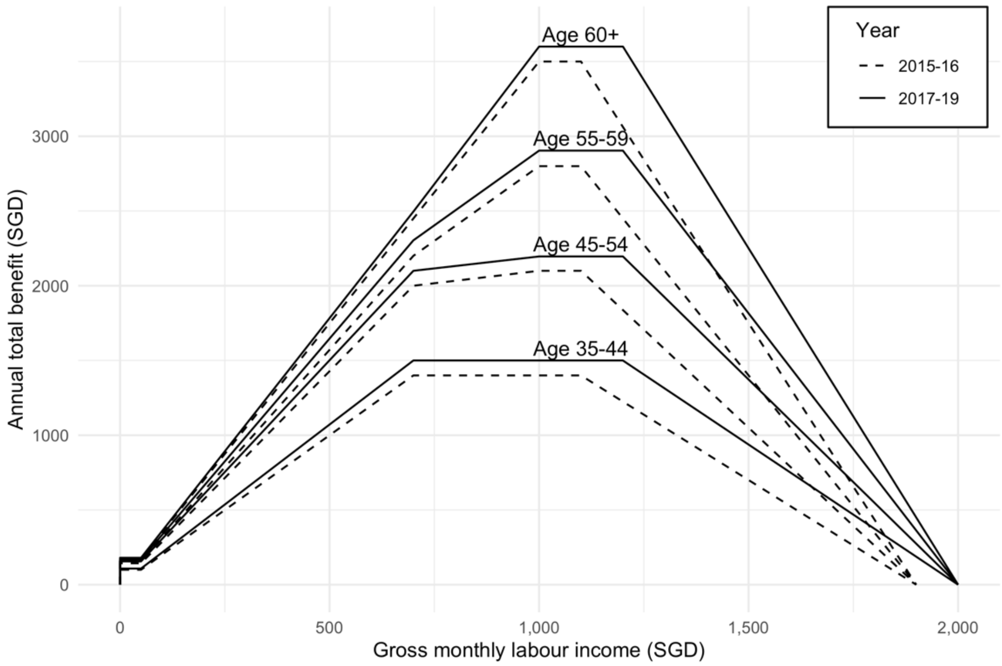
Source: Central Provident Fund Act (Chapter 36, 2018)

My study focuses on the period 2015-2019, during which the latest revision to WIS took place. The overall policy structure, however, remains the same. To qualify for benefits, workers must be Singapore citizens and meet certain income threshold and residential property ownership restrictions. In 2017, policymakers raised the income ceiling and increased benefits payable at every income level and age band. The frequency of payments to employees was also changed from quarterly to monthly. The payment frequency for self-employed remains annually. The rest of this section outlines WIS policy rules concerning eligibility and payment during the study period.

Eligible workers must be 35 years or older and have either worked for pay or for self during the assessment period. To qualify as an employee, a worker's gross monthly pay must not exceed $1,900 or $2,000 for the period under assessment in 2015-2016 or 2017-2018 respectively. To qualify as a self-employed person, a worker's annual net trade income[3] must not exceed $22,800 or $24,000 in 2015-2016 or 2017-2019, respectively. Despite meeting these criteria, an individual may still be excluded from the scheme if they and their spouse (if married) live in a property with annual value[4] exceeding $13,000, or own two or more residential properties, or if their spouse has an assessable income exceeding $70,000.

[3] Net trade income is the gross trade income less all allowable business expenses, capital allowances and trade losses.
[4] Annual value is the estimated gross annual rent of the property (excluding furniture, furnishings and maintenance fees). It is maintained by the Internal Revenue Authority of Singapore for the purpose of property tax calculations.

Figure 2: WIS schedule for employees



Source: Central Provident Fund Act (Chapter 36, 2018)

The total amount of WIS benefits depends on the year, individual's age at the end of the year, labour income and employment type. Figure 2 shows the benefit schedules for a hypothetical employee who worked throughout a given year. Benefit schedules vary by age bands and years, but they follow similar patterns. Benefits increase with labour income up to a certain point, then plateaus over a range before decreasing towards zero. The phase-in region is a monthly labour income of less than $700 for individuals aged 44 and below, and less than $1,000 for individuals aged 45 and above. The phase-out region is a monthly labour income of $1,100 to $1,900 (for 2015-2016) or $1,200 to $2,000 (for 2017-2018). The positive relationship between a schedule's maximum benefit and actual benefit allows us to loosely interpret an increase in maximum benefits as an increase in actual benefits received. The benefits schedule for self-employed persons is nearly identical to Figure 2, only that the level of annual benefit is a third lower everywhere. Total benefit amount includes both cash payments and CPF top-ups. Employees receive 40% of benefits in cash, whereas self-employed persons only receive 10% of benefits in cash.

1.4 Data

Singapore Life Panel is a monthly longitudinal survey of a nationally representative sample of Singaporeans aged 50 to 70 and their spouse (if married).[5] Beginning on July 2015, each survey wave collects individual and household information on economic, social, demographic and health outcomes. For this study, I restrict my analysis to individuals aged 50 and above and use information collected up to March 2019. While all respondents may not participate in every wave, there are approximately 8,000 respondents each wave on average.

Table 2 summarizes key demographic characteristics of survey respondents in column (1). Respondents are about 61 years old on average and are mostly ethnic Chinese. 23% have no formal schooling or only primary school level educational attainment. About 79% of them are married and they have about 2 children on average. I also document the proportion of respondents excluded from WIS based on the criteria discussed above. Since information on annual values of specific properties are not available, I determine a cut-off based

[5] The survey also includes a small proportion of Permanent Residents (4.7%) and non-residents (0.3%). I exclude these individuals in my analysis since they are ineligible for WIS. Although it is possible to include them as a control in the analysis by assigning zero to the maximum WIS benefit, substantial differences in characteristics between non-citizens and citizens will likely bias the analysis. For instance, Permanent Residents are almost twice as likely to have attained post-secondary qualification compared to Singapore Citizens.

Table 2. Summary of respondent characteristics

	(1) All	(2) Ever received WIS	(3) Never received WIS
Demographics:			
Age	61.0	61.5	60.8
	(5.99)	(5.94)	(5.99)
Male	0.476	0.451	0.489
	(0.499)	(0.498)	(0.500)
Chinese	0.869	0.867	0.870
	(0.337)	(0.340)	(0.336)
No formal schooling/Primary	0.234	0.341	0.180
	(0.423)	(0.474)	(0.385)
Secondary	0.424	0.471	0.400
	(0.494)	(0.499)	(0.490)
Post-secondary	0.343	0.188	0.420
	(0.475)	(0.391)	(0.493)
Married	0.794	0.755	0.814
	(0.404)	(0.430)	(0.389)
Number of children	1.95	1.94	1.96
	(1.12)	(1.16)	(1.10)
Number of other household members	2.49	2.37	2.56
	(1.37)	(1.37)	(1.37)
WIS exclusion criteria:			
Own secondary residential property	0.078	0.027	0.103
	(0.267)	(0.162)	(0.303)
Current residence is non-HDB	0.184	0.042	0.255
	(0.388)	(0.201)	(0.436)
Spouse's tax-assessable income exceeds $ 70,000	0.059	0.020	0.078
	(0.235)	(0.141)	(0.268)
WIS benefits:			
Received any benefit in a given year	0.217	0.632	-
	(0.412)	(0.482)	-
Annual benefit amount received conditional on receipt	753	753	-
	(1255)	(1255)	-
Number of persons	14,494	3,259	11,235
Number of observations	3365,59	111,808	224,751

Notes: Standard deviations are reported in parenthesis. Non-HDB properties are a proxy for residential property exceeding $13,000 annual valuation.

on property type using statistics published by the Inland Revenue Authority of Singapore. Housing and Development Board[6] (HDB) properties are a natural threshold because the median annual value of all types of HDB homes (1-room to executive apartments) were below $13,000 for the relevant years while the median annual value of non-HDB properties (both landed and non-landed) far surpassed $13,000.[7] Based on this conjecture, I approximate about 24% of respondents are ineligible for WIS benefits even if they were below the income thresholds.

Survey questions on WIS start from April 2016 and include whether the individual received a WIS in the previous quarter or month, and the amount. About 22% of respondents report receiving WIS benefits at least once over the study period. In columns (2) and (3), I present respondent characteristics conditional on recipient status. WIS recipients tend to be older, female and unmarried relative to non-recipients. Differences between the two groups is starker in terms of education. WIS recipients are almost twice as likely to have no formal schooling or only a primary education attainment. A small percentage of WIS recipients appear to meet one of the exclusion criteria. About half of these are explained by changes in exclusion status mid-way the study period. The other half may be attributed to erroneous responses or from using non-HDBs as a proxy for high value residential properties may. Among WIS

[6] HDB is the government organization responsible for providing public housing in Singapore. More than 80% of Singaporeans live in HDB flats.
[7] Executive apartments, which had the highest median annual value among HDB type properties, had a median annual value of $10,680 -$11,220 over the period 2015-2017.Non-landed residential properties, which had the lowest median annual value among non-HDB type properties, had a median annual value of $22,200-$25,200 over the period 2015-2017.

recipients, only about 63% received the benefits in a given year. Conditional on receipt, the average annual benefit amount received is $753, which is higher than the national average due to age of survey respondents. This implies about $300 in cash receipts for employees and $75 for self-employed persons.

The set of dependent variables include self-reported labour market outcomes, subjective well-being, and household consumption. Labour market outcomes include labour force participation, employee, self-employed, and gross monthly labour income (pre-tax and before deductions). I also differentiate employees by part-time and full-time using the 35-hour work week as the threshold.

For subjective well-being, I consider both evaluative and experiential measures (Dolan & Metcalfe, 2012). For evaluative measures, respondents were asked about their satisfaction with various aspects of their life on a monthly basis. An overall measure asks, “Taking all things together, how satisfied are you with your life as a whole these days?”. Domain-specific measures relate to satisfaction with total household income, overall economic situation, social and family life, job and health. Respondents select a response to each question from a 5-point scale, ranging from “Very satisfied” to “Very dissatisfied”. For experiential measures, respondents report their mental state on a quarterly basis. They were asked how often they felt worn out or happy in the past 30 days, with possible responses ranging from “None of the time” to “All of the time” on a 6-point scale”. They were also asked how much difficulty they had sleeping or feeling sad, with possible responses ranging from “None” to “Severe” on a 5-point scale. When analysing subjective well-being measures, I use dummy variable outcomes that indicate the two worse responses.

Monthly total household consumption spending includes expenditures on mortgage interest, rent, utilities, home furnishing and maintenance, food, tobacco, clothing and personal care, healthcare, leisure, transportation, household appliances, education, insurance, taxes. I consider total spending, as well as spending on durables and non-durables. Durable spending comprises furnishing/furniture, home maintenance, vehicle repair and maintenance and household appliances. Non-durable spending is total spending less durable spending. I only use responses of the household member most confident in answering these questions. For households with equally confident members, one respondent was randomly chosen.

1.5 Empirical Strategy

I define the WIS generosity as the maximum benefit an individual can receive and estimate the following reduced form relationship between an outcome of interest ($Y_{i,t}$) and the level of maximum WIS benefit ($Max_{i,t}$) applicable to an individual i in period t:

$$Y_{i,t} = \beta_0 + \beta_1 Max_{i,t} + \mathbf{\Gamma X}_{i,t} + w_t + \varepsilon_{i,t} \tag{1.1}$$

$\mathbf{X}_{i,t}$ is a vector of additional controls (age, gender, race, education, marital status, number of children, number of other household members), w_t is the wave fixed effects, and $\varepsilon_{i,t}$ captures all other factors that might influence $Y_{i,t}$. β_1 captures the marginal effect of increasing maximum benefits. Wave fixed effects capture common trends across time, which also include the effects of raising the income ceiling and increasing the frequency of WIS pay-outs.

The identifying assumption underpinning this approach is that variations in WIS generosity are independent of $\varepsilon_{i,t}$, after controlling for observable factors. A potential concern is that the policy revision in 2017 may be driven by unfavourable macroeconomic conditions or possible changes in social spending associated with electoral cycles (Brender & Drazen, 2013). This is unlikely. Prior to the announcement to revise WIS in February 2016, quarterly unemployment and GDP growth rates were steady at about 2% and 4% respectively. Furthermore, Singapore's general election took place 5 months prior to the announcement without any change in leadership. Even if the revision was influenced by aforementioned factors, variations in age-specific generosity account for a much larger share of the total variation. Annual maximum benefits differ by $700 between adjacent age bands, whereas it only increased by $100 under the 2017 policy revision (see Figure 2).

Since identification of β_1 comes primarily from comparing outcomes between age bands, it is important to control for age effects unrelated to WIS. As a baseline specification, I adopt a linear age specification. This implies that deviations from the cross-sectional linear relationship are attributable to differences in WIS generosity. Figure 1 Panel A provides some support, at least for labour force participation. Before WIS took effect, the decline in labour force participation was approximately linear over the ages 50-70. Subsequently, the age profile shifted upwards and became more concave because older workers received higher benefits and experienced greater increases in these benefits over the years.

Despite the overall linear trend, other age dependent policies may induce sharp discontinuities between age brackets which are not captured by Figure 1. This could potentially bias estimates of β_1 in various directions, depending on the outcome of interest. For example, Kim and Koh (2019) showed that a reduction in payroll tax had a positive effect on subjective well-being and consumption spending, but no change to employment. Hence, I include controls for several major age- and cohort-related social security policies in Equation (1.1).

The first two policies concern CPF contribution and withdrawal. Compulsory CPF contributions act as a payroll tax on employees. Contribution rates decrease discontinuously from 17% to 7.5% as individuals age and are fixed within age bands (55 and below, above 55 to 60, above 60 to 65, above 65). New rates apply in the month following an individual's birthday month. Upon turning 55, CPF members are also eligible to withdraw a portion of CPF savings in cash. The third policy is the statutory retirement age. Under the Retirement and Re-employment Act, employers are not allowed to dismiss workers below the age of 62 because of their age. They are also required to offer re-employment to eligible individuals until age 65, which was later raised to 67 from 1 July 2017. One may note that the age thresholds in the policies described above coincide with those in the WIS scheme. By using monthly data, I exploit within-year differences in timing. Increases in maximum WIS benefit occur at the start of the year. Whereas changes in CPF contribution rate, CPF withdrawal eligibility and minimum retirement age take effect on or immediately following the month of the individual's birthday. Finally, I also control for cohorts born in 1949 or earlier who qualify for the Pioneer Generation Package. These individuals enjoy annual CPF top-ups from the government, subsidized outpatient care and medical insurance, monthly cash transfers to those suffering from moderate to severe functional disabilities.

For the main analysis, I estimate the impact of WIS over the entire sample of individuals aged 50-70 years old. This includes those who do not qualify regardless of their labour supply decisions. I interpret these estimates as the intention-to-treat effect. I also estimate the impact of WIS on those with low potential income, and therefore more likely to receive WIS if they were working. This is defined as individuals who have primary school education or less.

1.6 Results

Table 3 presents estimated impacts on various labour market outcomes. To aid readability, I use monthly maximum benefits in hundred dollars (i.e. annual maximum benefits divided by 1,200) for the variable $Max_{i,t}$ in Equation (1.1). So marginal effects are interpreted in terms of $100 increase in monthly maximum benefits. Estimates in columns (1) and (2) differ in that the latter includes controls for age-related government policies outlined in the previous section. Obtaining different estimates between the two columns is indicative that substantial discontinuity effects arises from other age-related policies.

For Panels A and B, column (1) shows a $100 increase in monthly maximum benefits is estimated to increase the labour participation rate by 11.3% and the probability of work by 10.1%. When age-related government policies are included in column (2), the coefficient of benefit generosity becomes much smaller in magnitude and less significant. A $100 increase in monthly maximum benefits increase the probability of work by 2.4%. The magnitude of effects on labour participation and employment are similar due to consistently low unemployment rates.

Table 3. WIS impact on labour market outcomes.

	(1)	(2)	(3)
Panel A: Labour Force Participant			
Maximum WIS benefit	0.113***	0.021	0.065**
	(0.015)	(0.013)	(0.031)
R-squared	0.195	0.199	0.188
Mean outcome	0.642	0.642	0.513
Panel B: Working			
Maximum WIS benefit	0.101***	0.024*	0.061**
	(0.015)	(0.014)	(0.031)
R-squared	0.155	0.159	0.159
Mean outcome	0.589	0.589	0.465
Panel C: Employee			
Maximum WIS benefit	0.074***	0.004	0.026
	(0.015)	(0.015)	(0.033)
R-squared	0.109	0.112	0.097
Mean outcome	0.512	0.512	0.405
Panel D: Self-employed			
Maximum WIS benefit	0.029***	0.019**	0.039**
	(0.008)	(0.009)	(0.019)
R-squared	0.032	0.032	0.066
Mean outcome	0.084	0.084	0.063
Panel E: Monthly labour income conditional on working			
Maximum WIS benefit	378***	-30.4	230*
	(133)	(127)	(124)
R-squared	0.230	0.230	0.105
Mean outcome	3,733	3,733	1,620
Control for age-related policies	No	Yes	Yes
Subsample	All	All	Primary education or less
Observations	323,969	323,969	75,305

Notes: Cluster-robust standard errors at the household-level are reported in parentheses. Statistical significance denoted by * p<0.1, ** p<0.05, *** p<0.01. Additional controls include age, gender, race, education, marital status, number of children, number of other household members, and wave fixed effects. Age-related policies controlled for include CPF contribution rates, CPF withdrawal, retirement and re-employment age, and Pioneer Generation Package.

Panels C and D break down the increased likelihood of working by the type of employment. Comparing columns (1) and (2) shows a substantial reduction in the estimated effect on working for pay when age-related policies are included, but the effect on self-employment remains similar. This is expected since changes in mandatory CPF contribution rates and CPF withdrawal should have little effect on self-employed persons. CPF contribution rates of self-employed persons remain constant beyond age 50. They also typically have substantially less CPF funds to withdraw since contribution rates for the self-employed are lower. Furthermore, the Retirement and Re-employment Act does not apply to self-employed persons. When age-related policies are controlled for, self-employment is the main contributor to the increased likelihood of working and is statistically significant. The smaller and insignificant extensive margin effects for employees agree with Leong et al. (2014), who did not find any effects after the increase in maximum benefits in 2009[8] and 2010[9].

In Panel E, I estimate the impact of WIS on labour income conditional on working. The estimate reflects both intensive margin changes in labour supply as well as the incidence of benefits potentially captured by employers. I find a negative impact on income after controlling for age-related policies, but this is estimated imprecisely.

I restrict the same analyses to those with no formal or only primary level educational attainment and report the results in column (3). Estimated impacts

[8] The Workfare Special Payment was a one-off payment introduced in 2009 to help workers cope with the economic downturn. Up to $800 in benefits could be received for work done in 2009.

[9] The schedule of benefits was revised in 2010, increasing annual maximum benefits by $200 for ages 45-54, $300 for ages 55-60, and $400 for age 60 and over.

on extensive margins are about 2 to 3 times larger in magnitude than column (2) and more statistically significant. As these individuals are likely to be bottom earners, they are more likely to qualify for WIS if working and therefore more responsive to changes in benefits generosity. Finding a positive and marginally significant impact on conditional labour income is also an encouraging sign that increasing generosity does not discourage work on the intensive margin.

In Appendix Table 1, I report estimated impacts of WIS on full-and part-time paid work. After controlling for age-related policies in the full sample, I find positive and marginally significant effects for part-time work but negative and insignificant effects for full-time work. This finding suggests that extensive margin responses are primarily in the form of part-time work. Increases in part-time work is also larger than full-time work among the low-educated, although neither estimates are statistically significant.

In Appendix Table 2, I explore several alternative specifications to test the robustness of the estimates in Table 2. Replacing the linear age specification with a cubic function produced estimated impacts that are close to zero and statistically insignificant. This likely occurs because the specification is too flexible and is a competing source of variation for maximum benefits attributed to age. Probit and Logit regressions produce less flexible non-linear age function. Marginal effects obtained from these models are similar to the main results in magnitude and significance. When individual fixed effects are included to the linear probability model, estimates are smaller and insignificant due to a lack of within-individual variation. Only about 29% of respondents experienced an increase in benefits generosity from moving into a higher age band over the analysis period.

Table 4: WIS impact on labour market outcomes by gender

	(1)	(2)	(3)	(4)
Panel A: Labour Force Participant				
Maximum WIS benefit	0.025	0.008	0.096***	0.019
	(0.016)	(0.019)	(0.035)	(0.044)
R-squared	0.231	0.154	0.196	0.096
Mean outcome.	0.737	0.557	0.689	0.403
Panel B: Working				
Maximum WIS benefit	0.024	0.012	0.094**	0.013
	(0.018)	(0.020)	(0.038)	(0.044)
R-squared	0.184	0.132	0.166	0.085
Mean outcome.	0.672	0.515	0.626	0.365
Panel C: Employee				
Maximum WIS benefit	0.012	-0.007	0.078*	-0.015
	(0.021)	(0.020)	(0.047)	(0.043)
R-squared	0.125	0.114	0.094	0.078
Mean outcome	0.552	0.475	0.507	0.341
Panel D: Self-employed				
Maximum WIS benefit	0.009	0.020**	0.025	0.028
	(0.015)	(0.009)	(0.037)	(0.017)
R-squared	0.014	0.014	0.056	0.014
Mean outcome	0.130	0.043	0.126	0.024
Panel E: Monthly labour income conditional on working				
Maximum WIS benefit	15.6	-195	254	84.8
	(192)	(157)	(175)	(137)
R-squared	0.216	0.245	0.067	0.054
Mean outcome	4,226	3,155	20,27	1,184
Subsample	All		Primary education or less	
	Men	Women	Men	Women
Observations	153,381	170,588	28,943	46,362

Notes: Cluster-robust standard errors at the household-level are reported in parentheses. Statistical significance denoted by * p<0.1, ** p<0.05, *** p<0.01. Additional controls include age, age-related policies (CPF contribution rates, CPF withdrawal, retirement and re-employment age, and Pioneer Generation Package), race, education, marital status, number of children, number of other household members, and wave fixed effects.

In Table 4, I report results from the subsample analysis by gender. Comparing coefficient estimates of men and women reveal that men are more likely to work when potential benefits are increased. For every $100 increase in maximum monthly benefits, male employment increases by about 2.4% while female employment increases by 1.2%. Finding larger responses to benefits generosity among men is reasonable, given the traditional role of men as the main income earner in a household in this sample of older Singaporeans. Panels C and D suggests that men tend to engage in paid work, while women tend to engage in self-employment when benefits generosity are increased. Analysis on the sample of low education individuals show similar results, with greater differences in point estimates across gender. My finding contradicts those of Freire (2015), showing that men respond substantially more to WIS than women. This may be due to Hong Kong being an inappropriate control for men.

I also examine the effect of WIS on spouse's labour outcomes by restricting the sample to married couple households and controlling for maximum benefits receivable by the respondent and spouse, as well as the age and age-related policies applicable to the couple. The results are in Appendix Table 3. In all regressions, the coefficients of own benefit generosity remain similar in magnitude and significance as Table 4. The effect of spouse's benefit generosity on labour outcomes are mostly positive for husbands and negative for women, but neither are statistically significant. This confirms the finding of no adverse spousal effect of WIS documented by Lee et al. (2014).

Table 5. WIS impact on evaluative subjective well-being measures

	(1)	(2)	(3)
Panel A: Dissatisfied or very dissatisfied with life overall			
Maximum WIS benefit	-0.018**	-0.008	-0.035*
	(0.008)	(0.008)	(0.019)
R-squared	0.010	0.011	0.017
Mean outcome.	0.073	0.073	0.087
Panel B: Dissatisfied or very dissatisfied with household income			
Maximum WIS benefit	-0.016	-0.010	-0.053**
	(0.012)	(0.012)	(0.026)
R-squared	0.011	0.011	0.015
Mean outcome	0.163	0.163	0.186
Panel C: Dissatisfied or very dissatisfied with economic situation			
Maximum WIS benefit	-0.017	-0.013	-0.051**
	(0.012)	(0.011)	(0.026)
R-squared	0.010	0.011	0.016
Mean outcome	0.160	0.160	0.185
Panel D: Dissatisfied or very dissatisfied with social/family life			
Maximum WIS benefit	-0.012*	-0.004	-0.034**
	(0.007)	(0.007)	(0.016)
R-squared	0.010	0.010	0.015
Mean outcome	0.052	0.052	0.063
Panel E: Dissatisfied or very dissatisfied with job			
Maximum WIS benefit	-0.019**	-0.016*	-0.045**
	(0.009)	(0.009)	(0.020)
R-squared	0.011	0.012	0.017
Mean outcome	0.090	0.090	0.099
Control for age-related policies	No	Yes	Yes
Subsample	All	All	Primary education or less
Observations	335,462	335,462	78,319

Notes: Cluster-robust standard errors at the household-level are reported in parentheses. Statistical significance denoted by * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Additional controls include age, gender, race, education, marital status, number of children, number of other household members, and wave fixed effects. Age-related policies controlled for include CPF contribution rates, CPF withdrawal, retirement and re-employment age, and Pioneer Generation Package.

Table 5 presents results for evaluative well-being measures. Without controlling for age-related policies, column (1) of Panel A shows that WIS has a positive and significant effect on overall life-satisfaction. A $100 increase in monthly maximum benefits reduces the likelihood of being dissatisfied with life by 1.8%. However, once age-related policies were included in column (2), the coefficient becomes smaller and statistically insignificant. I make similar observations for other domains such as household income, economic situation, social/family life and job. Among the low-educated however, I find substantial and statistically significant impacts. A $100 increase in monthly maximum benefits reduces the chance of dissatisfaction without household income and economic situation by more than 5%. Dissatisfaction with social or family life and job were also significantly reduced but impacts were slightly smaller in magnitude. Not reported here, I find a statistically insignificant reduction in dissatisfaction with health.

Unlike evaluative measures, benefit generosity does not seem to improve experiential measures of well-being, apart from happiness (see Appendix Table 4). A $100 increase in WIS maximum benefits significantly reduces reported feelings of little or no happiness by 6.9% among individual with low education. Because experiential measures were collected less frequently, the smaller sample size may partially explain the lack of evidence in other measures. However, point estimate magnitudes are generally much smaller than those in Table 4.

I report results for household consumption in Appendix Table 5. In general, potential impacts on spending measures are estimated very imprecisely and not statistically different from zero across all specifications. This supports the prediction of standard life-cycle models that consumption behaviour is unaffected by an anticipated increase in income (Jappelli & Pistaferri, 2010). In the case of WIS, increases in benefits received are largely predictable since it is triggered by individual labour supply decision or by moving into a higher age bracket. Unanticipated increase in benefits due to policy reform are smaller in comparison. Earlier studies find that households with low income or liquid wealth were more sensitive to income shocks (Johnson, Parker, & Souleles, 2006; Jappelli & Pistaferri, 2010). Restricting the analysis to households where the main respondent has low education and to liquidity-constrained households[10] did not yield significant estimates. However, point estimates for the liquidity-constrained households are positive and marginally significant for spending on food, clothing and personal care. The overall lack of consumption response could also be due to the relatively small cash component of WIS benefits. Average cash benefits account for only 1.3% of what an average WIS-receiving household spends.

1.7 Concluding Remarks

This study evaluates a wage supplement program in Singapore, called Workfare Income Supplement, with respect to labour market outcomes and subjective well-being of older Singaporeans, as well as their household's consumption spending. I find that WIS benefit generosity has a positive impact on employment outcomes and subjective well-being of older Singaporeans. The impact is larger and significant among the low-educated, who are more likely

[10] Liquidity-constrained households are identified as those whose financial wealth (i.e. excluding housing and CPF savings) make up less than 5% of their total wealth, or those with negative wealth.

to qualify for WIS. These findings imply that wage supplements, targeted at older workers, potentially reduce the burden of population ageing. Nonetheless, the effectiveness of wage supplements also depends on other policies that influence the level of early pension support, retirement age, and opportunities to gain new skills.

Even though current WIS support for the self-employed is significantly less than employees, self-employment makes up a substantial part of the labour response to wage supplements. This phenomenon could be a driven by older workers' preferences, coupled with the availability of freelance jobs from a growing gig economy. Policies that foster the creation of such jobs and equip older workers with skills to participate can complement existing incentive structures, like WIS, in supporting older workers to remain in the labour force. Policy makers may also need to reassess the adequacy of benefits for self-employed persons.

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1.9 Appendix

Appendix Table 1: WIS impact on being full-/part-time employee

	(1)	(2)	(3)
Panel A: Full-time employee			
Maximum WIS benefit	0.049***	-0.014	0.007
	(0.015)	(0.015)	(0.028)
R-squared	0.124	0.127	0.109
Mean outcome	0.379	0.379	0.245
Panel B: Part-time employee			
Maximum WIS benefit	0.025**	0.018*	0.019
	(0.010)	(0.010)	(0.024)
R-squared	0.008	0.008	0.009
Mean outcome	0.133	0.133	0.160
Control for age-related policies	No	Yes	Yes
Subsample	All	All	Primary education or less
Observations	323,921	323,921	75,286

Notes: Cluster-robust standard errors at the household-level are reported in parentheses. Statistical significance denoted by * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Additional controls include age, gender, race, education, marital status, number of children, number of other household members, and wave fixed effects. Age-related policies controlled for include CPF contribution rates, CPF withdrawal, retirement and re-employment age, and Pioneer Generation Package.

Appendix Table 2. Alternative models for WIS impact on labour market outcomes

	(1)	(2)	(3)	(4)
Panel A: Labour Force Participant				
Maximum WIS benefit	-0.015	0.012	0.013	0.001
	(0.012)	(0.014)	(0.015)	(0.006)
R-squared	0.200	-	-	0.011
Pseudo R-squared	-	0.162	0.161	-
Panel B: Working				
Maximum WIS benefit	-0.012	0.023	0.024	0.005
	(0.014)	(0.015)	(0.015)	(0.007)
R-squared	0.160	-	-	0.011
Pseudo R-squared	-	0.123	0.123	-
Panel C: Employee				
Maximum WIS benefit	-0.014	0.012	0.013	0.009
	(0.015)	(0.015)	(0.015)	(0.007)
R-squared	0.112	-	-	0.007
Pseudo R-squared	-	0.084	0.084	-
Panel D: Self-employed				
Maximum WIS benefit	0.001	0.020**	0.019**	-0.005
	(0.009)	(0.008)	(0.008)	(0.004)
R-squared	0.033	-	-	0.002
Pseudo R-squared	-	0.058	0.058	-
Panel E: Monthly labour income conditional on working				
Maximum WIS benefit	-81.6	-	-	109***
	(130)	-	-	(39.6)
R-squared	0.231	-	-	0.024
Model specification	Linear probability with cubic age function	Probit	Logit	Individual fixed effects
Subsample	All	All	All	All
Observations	323,969	323,969	323,969	323,969

Notes: Cluster-robust standard errors at the household-level are reported in parentheses. Statistical significance denoted by * p<0.1, ** p<0.05, *** p<0.01. Additional controls include age, age-related policies (CPF contribution rates, CPF withdrawal, retirement and re-employment age, and Pioneer Generation Package), gender, race, education, marital status, number of children, number of other household members, and wave fixed effects. For Probit and Logit estimates, the estimates reported are marginal effects in terms of percentages to be comparable with estimates from linear models.

Appendix Table 3. WIS impact on spouse's labour market outcomes

	(1)	(2)	(3)	(4)
Panel A: Labour Force Participant				
Spouse's maximum WIS benefit	0.020	-0.029	0.024	-0.097
	(0.021)	(0.031)	(0.054)	(0.074)
R-squared	0.246	0.129	0.236	0.082
Mean outcome	0.743	0.546	0.695	0.396
Panel B: Working				
Spouse's maximum WIS benefit	0.008	-0.018	0.022	-0.051
	(0.023)	(0.030)	(0.059)	(0.067)
R-squared	0.193	0.118	0.192	0.079
Mean outcome	0.686	0.509	0.644	0.362
Panel C: Employee				
Spouse's maximum WIS benefit	-0.007	-0.010	0.016	-0.025
	(0.028)	(0.031)	(0.076)	(0.066)
R-squared	0.129	0.104	0.098	0.072
Mean outcome	0.565	0.473	0.515	0.341
Panel D: Self-employed				
Spouse's maximum WIS benefit	0.026	-0.006	0.026	-0.026*
	(0.022)	(0.011)	(0.060)	(0.014)
R-squared	0.016	0.017	0.073	0.024
Mean outcome	0.132	0.039	0.136	0.022
Panel E: Monthly work income conditional on working				
Spouse's maximum WIS benefit	36.3	-58.7	851	56.3
	(282)	(236)	(626)	(173)
R-squared	0.215	0.242	0.089	0.086
Mean outcome	4,424	3,248	2,064	1,210
Subsample	All		Primary education or less	
	Husbands	Wives	Husbands	Wives
Observations	124,244	108,838	21,935	27,400

Notes: Cluster-robust standard errors at the household-level are reported in parentheses. Statistical significance denoted by * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Additional controls include respondent's maximum WIS benefit, age and age-related policies of respondent and spouse (CPF contribution rates, CPF withdrawal, retirement and re-employment age, and Pioneer Generation Package), race, education, number of children, number of other household members, and wave fixed effects.

Appendix Table 4. WIS impact on experiential subjective well-being measures

	(1)	(2)	(3)
Panel A: Worn-out most or all of the time			
Maximum WIS benefit	-0.017*	-0.014	0.007
	(0.009)	(0.012)	(0.029)
R-squared	0.015	0.015	0.013
Mean outcome.	0.122	0.122	0.160
Panel B: Happy a little or none of the time			
Maximum WIS benefit	-0.019**	-0.012	-0.069***
	(0.009)	(0.011)	(0.023)
R-squared	0.010	0.011	0.016
Mean outcome	0.096	0.096	0.104
Panel C: Severe or extreme difficulty sleeping			
Maximum WIS benefit	0.016	0.002	-0.030
	(0.014)	(0.018)	(0.039)
R-squared	0.012	0.012	0.009
Mean outcome	0.579	0.579	0.511
Panel D: Severe or extreme sadness			
Maximum WIS benefit	0.026*	0.017	0.005
	(0.013)	(0.016)	(0.038)
R-squared	0.018	0.019	0.007
Mean outcome	0.701	0.701	0.616
Control for age-related policies	No	Yes	Yes
Subsample	All	All	Primary education or less
Observations	106,497	106,497	24,815

Notes: Cluster-robust standard errors at the household-level are reported in parentheses. Statistical significance denoted by * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Additional controls include age, gender, race, education, marital status, number of children, number of other household members, and wave fixed effects. Age-related policies controlled for include CPF contribution rates, CPF withdrawal, retirement and re-employment age, and Pioneer Generation Package.

Appendix Table 5. WIS impact on household-level spending

	(1)	(2)	(3)	(4)
Panel A: Total consumption				
Maximum WIS benefit	-189*	-164	-30.8	35.7
	(110)	(121)	(126)	(209)
R-squared	0.200	0.203	0.075	0.169
Mean outcome.	3,217	3,192	1,357	2,456
Panel B: Durables				
Maximum WIS benefit	0.446	11.6	6.37	17.6
	(11.6)	(19.4)	(29.4)	(34.2)
R-squared	0.025	0.025	0.007	0.021
Mean outcome	188	189	68.1	141
Panel C: Non-durables				
Maximum WIS benefit	-190*	-176	-37.2	18.2
	(102)	(113)	(119)	(196)
R-squared	0.199	0.202	0.079	0.170
Mean outcome	3,029	3,004	1,289	2,314
Panel D: Food				
Maximum WIS benefit	-8.59	1.62	-37.8	64.4*
	(17.7)	(18.4)	(28.1)	(35.8)
R-squared	0.194	0.199	0.083	0.158
Mean outcome.	628	619	377	534
Panel E: Clothing and personal care				
Maximum WIS benefit	-11.1*	-4.58	-4.92	23.1*
	(6.58)	(7.28)	(7.88)	(12.6)
R-squared	0.096	0.097	0.032	0.071
Mean outcome	132	133	50.5	97.6
Control for age-related policies	No	Yes	Yes	Yes
Subsample	All	All	Primary education or less	Liquidity constrained households
Observations	271,676	218,100	49,496	49,047

Notes: All samples were restricted to the household member who was most confident in answering household financial questions. Cluster-robust standard errors at the household-level are reported in parentheses. Statistical significance denoted by * p<0.1, ** p<0.05, *** p<0.01. Additional controls include age, gender, race, education, marital status, number of children, number of other household members, and wave fixed effects. Age-related policies controlled for include CPF contribution rates, CPF withdrawal, retirement and re-employment age, and Pioneer Generation Package. Liquidity constrained is defined as financial wealth less than 5% of total wealth or negative wealth.

Chapter 2: Can Non-Pension Retirement Age Influence Retirement Decisions? Evidence from Raising Singapore's Re-Employment Age

2.1 Introduction

Retirement age is a key policy lever to promote elderly employment and rein in burgeoning fiscal burdens associated with population ageing. When linked to pension benefits, retirement age strongly influences labour force participation of older workers (Gruber & Wise, 2004). However, delaying pension is challenging because of public backlash and spill-overs to other social security programs, such as disability and unemployment insurance (Staubli & Zweimüller, 2013). Apart from financial incentives, retirement ages can also shape social norms or be perceived as a government-sanctioned "recommendation" on the appropriate age to retire (Behaghel & Blau, 2012; Alonso-García, Bateman, Bonekamp, & Stevens, 2018). Can policymakers exploit these notions to encourage employment without postponing pension eligibility?

To answer this question, I investigate changes to a unique statutory retirement age in Singapore. In 2017, a reform of the Retirement and Re-employment Act (RRA) saw the re-employment age, which is the minimum age employers are required to offer older workers re-employment, increase from 65 to 67. Re-employment age is distinct from Central Provident Fund[11] (CPF)

[11] Central Provident Fund is a compulsory individual savings plan administered by the Singapore government. Members contribute a portion of their monthly labour income to fund

pension payout eligibility, which remained at age 65. The Act affords considerable flexibility and discretion to employers concerning re-employment and imposes relatively low cost of not offering one at all. This quasi-experimental setting presents an opportunity to investigate if a nominal retirement age, which does not impose binding constraints or distort incentives, can influence retirement decisions. I refer to the effects of a nominal retirement age as mental anchoring, which may be generated by a combination of endorsement effects, social norms, or reference-dependent preferences. Using data from the Singapore Life Panel and a difference-in-differences identification strategy, I estimate the impact of the RRA reform on labour market outcomes and subjective well-being of older Singaporeans.

The results show an increased likelihood of employment and a decreased likelihood of retirement for re-employment ages under the reformed Act (i.e. 62-66). I present evidence of mental anchoring by showing similar changes to labour outcomes of those aged 65-66 but whose re-employment age remained at 65 (i.e. born before July 1952). In a separate analysis of those aged 62-64, increased employment rates and decreased retirement rates provide further support for mental anchoring since the reform does not reduce future employment uncertainty via legislative channels. Lastly, I find improvements in subjective well-being measures relating to overall life satisfaction and health, particularly among those who are less educated, initially dissatisfied with household income, or feel unprepared for retirement. These individuals are most vulnerable to involuntary retirement.

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future retirement, healthcare and housing needs. (https://www.cpf.gov.sg/). CPF funds have restricted utilization and only a part of it can be withdrawn upon reaching 55 years of age.

My study contributes to the literature in two ways. First, it provides novel evidence of mental anchoring on statutory retirement ages using a unique policy in Singapore that is unrelated to pension eligibility. Early evidence for behavioural effects relied on rejecting other competing explanations for a spike in labour force exit at the Full Retirement Age (Lumsdaine, Stock, & Wise 1996). Behaghel and Blau (2012) found that staggered changes in Full Retirement Age were closely matched by shifts in benefit claiming hazard. Since this response is inconsistent with standard life-cycle models, their results support a behavioural explanation in the form of reference dependence with loss aversion. Stated preference studies also demonstrate that hypothetical pension-linked retirement ages influence retirement expectation (Vermeer, 2016) and planned pension drawdown (Alonso-García et al., 2018). However, stickiness to pension-linked retirement age may be also driven by a precautionary motive to replace uncertain labour income with a certain stream of pension benefits (Magnani, 2019). The RRA reform presents an opportunity to test for anchoring in a non-pension setting.

Second, this study contributes to understanding whether re-employment as a viable policy for improving elderly employment and welfare. To my best knowledge, only two studies examined the effects of mandating re-employment. The first study by Kondo and Shigeoka (2017) examines the 2006 reform of Japan's Elderly Employment Stabilization Law, compelling employers to offer continuous employment up to the pension eligibility age. The objective was to bridge an existing gap between the mandatory retirement age and pension eligibility age. A second study by Lee, Huang, and Guo (2017) examines the enactment of RRA in 2012. Like Japan, re-employment fills the gap between the minimum retirement age (the earliest age an employer can require Singapore resident[12] workers to retire at) and the CPF payout eligibility age, which was concurrently raised to 65 as well. Both studies found positive employment effects. I add to this discussion by examining whether a re-employment policy is effective at encouraging employment beyond pension eligibility and measuring its impact on subjective well-being.

The rest of this paper is organized as follows: Section 2 provides a brief background to the RRA; Section 3 describes the survey data used; In Section 4, I detail the empirical strategy; Section 5 reports the regression results; Section 6 concludes with a discussion of the findings.

2.2 Retirement and Re-employment Act

On 1 January 2012, the Retirement and Re-employment Act (RRA) came into effect, superseding the older Retirement Age Act which was in place since 1993. RRA maintained the minimum retirement age at 62. Furthermore, it introduced a new provision mandating employers to offer re-employment to eligible employees who reach the minimum retirement age, until they reach the re-employment age of 65. Employers who do not comply with the Act are liable for financial and criminal punishment. However as seen later, there are legitimate ways for employers to avoid re-employment.

To be eligible for re-employment, Singapore resident employees must meet all 4 eligibility criteria. First, the employee must attain the minimum retirement age on or after RRA took effect, i.e. born on or after 1 January 1950. Second, the employee must have worked for the employer for at least 3 years

[12] Singapore citizens and permanent residents.

prior to turning 62. Third, the employee must be medically fit for work. Fourth, the employee must have satisfactory work performance, as determined by the employer. The discretion and subjectivity of this last criteria allows employers to potentially discharge themselves from re-employment obligations.

Re-employment aims to support older workers in remaining employed, without compromising employability and imposing “excessive” burden on employers. Consequently, RRA affords ample flexibility to employers. Because re-employment contracts are deemed as new contracts of service, job responsibilities and working hours can be re-negotiated, and wages may be adjusted downwards to reflect changes. Furthermore, re-employment contracts can be as short as a year, renewable annually until the re-employment age. In the event the employer is unable to offer re-employment, he may either transfer re-employment obligations to another employer (with the agreement of both the employee and new employer) or make a one-off Employment Assistance Payment (EAP) to the employee. EAP quantum range from $3,500 to $13,000 (approximately 2 to 3.5 months’ salary), depending on how long the employee has been re-employed. Compared to prevailing norms for retrenchment benefits[13], EAP is less costly to employers. In view of these provisions, it is unlikely that RRA incentivises employers to retain older workers beyond what is already in their own interest. In fact, the vast majority of private-sector employers were already offering re-employment to older workers past the minimum retirement age before RRA enactment (Lee et al., 2017).

[13] According to the Ministry of Manpower, retrenchment benefits range between 2-weeks to 1-month salary per year of service, depending on the company’s financial position and the industry. See https://www.mom.gov.sg/employment-practices/retrenchment/responsible-retrenchment.

On 23 August 2015, the government announced that the re-employment age will be raised from 65 to 67, with effect from 1 July 2017. In other words, employers have to offer re-employment to eligible workers who attain age 65 on or after 1 July 2017 (i.e. born on or after 1 July 1952) for two additional years. If they are unable to do so, they are only liable for a lower EAP amount of $3,500 to $$7,500 (approximately 2 months' salary). News of the reform was not entirely unexpected as it had already been provided for in the 2012 Act and hinted at in Parliament as early as 2011 (Ministry of Manpower, 2011). My study focuses on investigating the effects of this recent increase in re-employment age.

2.3 Singapore Life Panel

The Singapore Life Panel is a monthly longitudinal survey of a nationally representative sample of Singaporean [14] aged 50-70 and their spouse (Vaithianathan, Hool, Hurd, & Rohwedder, 2018). Beginning July 2015, each wave collects individual- and household-level information on economic, social, demographic and health outcomes. While all respondents may not participate every month, there is a stable response rate of approximately 8,000 respondents each wave. For this study, I restrict my analysis to individuals aged 55-70 and use information collected up to December 2019. Appendix Table 1 provides a summary of respondent characteristics by applicable re-employment age.

Self-reported labour market outcomes and subjective well-being are outcomes of interest. I consider two extensive labour measures: whether an individual is currently working for pay (i.e. an employee) and whether he

[14] The survey also includes a small proportion of permanent residents (4.7%) and non-residents (0.3%). I exclude non-residents in my analysis since they are not covered by the RRA.

consider himself being retired. Both measures are not necessarily mutually exclusive because retirement is a subjective concept. For example, one person may regard himself retired if he leaves full-time employment for less demanding part-time work, while another may only deem herself retired if she completely withdraws from the labour force. I also consider two intensive labour measure, conditional on being employed: gross[15] monthly wage income and full-time[16] employment.

For subjective well-being, I focus on evaluative measures which ask respondents to assess various aspects of life (Dolan & Metcalfe, 2012). An overall measure asks, "Taking all things together, how satisfied are you with your life as a whole these days?". I also examine domain-specific measures relating to satisfaction with total household income, daily activities (or job if working) and health. Respondents select a response to each question from a 5-point Likert scale, ranging from "Very satisfied" to "Very dissatisfied". I construct dummy variables that indicate a dissatisfied response, i.e. "Very dissatisfied" and "Dissatisfied".

2.4 Empirical Strategy

The RRA reform is a supply-side intervention targeted at increasing demand for workers aged 65-66 who are born on or after 1 July 1952. In addition to extending the re-employment mandate, it could also influence employment decisions indirectly through raising mental anchors on retirement (Behaghel & Blau, 2012; Vermeer, 2016; Alonso-García et al., 2018). This mechanism has a wider reach than the reform's legal implications. For example, employers may

[15] Pre-tax and before deductions.
[16] Defined as usually working 35 or more hours a week.

be more willing to hire or re-hire older workers who are marginally ineligible for re-employment (e.g. born on 30 June 1952) but are still under 67. Likewise, workers younger than 67 may also revise retirement expectations upwards when faced with a higher age anchor, regardless of eligibility.

In view of these potential spill-overs, defining the treated as those falling under the extended re-employment mandate is likely to understate the full impact of the RRA reform. Instead, I define the treated group as those aged 62-66 and the control group as those aged 55-61 or 67-70. I assume that employment outcomes of individuals aged below 62 are not influenced by anchoring effects because the minimum retirement age is more salient for them. Similarly, those aged 67-70 are unlikely to be influenced by anchoring effects by virtue of being past the re-employment age.

Following Lee et al. (2017), I implement a difference-in-differences identification strategy with the basic form

$$y_{it} = \rho(T_{it} \times D_t) + T_{it} + D_t + \varepsilon_{ist} \tag{2.1}$$

y_{it} is an outcome of interest for individual i in period t. T_{it} is the treated indicator for whether individual i is aged between 62 to 66 in period t. D_t is the post-reform period indicator for whether period t is July 2017 or later. The coefficient ρ captures the impact of raising the re-employment age and $\varepsilon_{i,t}$ captures all other factors that might influence y_{it}.

The key identifying assumption is that counterfactual y_{it} trends of those aged 62-66 are parallel to those aged 55-61 and 67-70. Two potential sources of endogeneity arise because treatment is determined by age. First, cohort differences may also explain variations in y_{it} over time, confounding estimates of ρ. For instance, later birth cohorts who transition into the treated age group may be healthier or more educated than earlier birth cohorts who transition out. Hence, we may observe higher labour force attachment among the treated over time, even in the absence of the RRA reform. However, the relative short analysis period of 2 years before and after the reform suggests that changes to treated and control groups due to cohort differences are likely to be small. Controlling for observable cohort differences can further reduce this threat.

The second source of endogeneity is changes to other age-related policies which also influence y_{it}. I identify several policy changes that occurred during the study period. The first policy concerns the age at which CPF members can start receiving monthly payouts. Over the period 2012-2018, the government progressively raised the CPF payout eligibility age (previous known as the drawdown age) from age 62 to 65. The second policy is the Workfare Income Supplement scheme, a major social security program aimed at supporting low-income workers with regular cash and CPF transfers. In 2017, the scheme increased benefits quantum and raised the income ceiling for eligibility. The third policy is the Special Employment Credit which provides employers with wage offsets for hiring older workers. Subsidy rates have generally decreased throughout the study period for less elderly workers, which is in line with efforts to incentivise the hiring beyond the minimum retirement age and re-employment age. Appendix A detail changes to the above policies. While there are some overlaps in age bands and implementation year between the RRA and other policies, monthly observations allow identification of within-year differences in timing. Only changes to the RRA took effect on July 2017, while other policy changes occurred at the start of the calendar year.

In the full specification, I replace treated and post-reform indicators with respective fixed effects dummies in monthly intervals, and include controls for demographic characteristics and other age-related policies.

$$y_{it} = \rho(T_{it} \times D_t) + \text{age}_{it} + \text{yrmth}_t + \beta_1 \text{X}_{it} + \beta_2 \text{Z}_{it} + \varepsilon_{ist} \qquad (2.2)$$

age_{it} and yrmth_t are vector of dummies, representing age (in months) and year-month fixed effects respectively. X_{it} is a vector of individual and household characteristics which includes 5-year birth cohort, gender, race, education, marital status, number of children, number of household members. Z_{it} is a vector of dummies controlling for changes to other age-related policies such as Workfare Income Supplement, Special Employment Credit, and CPF payout eligibility.[17]

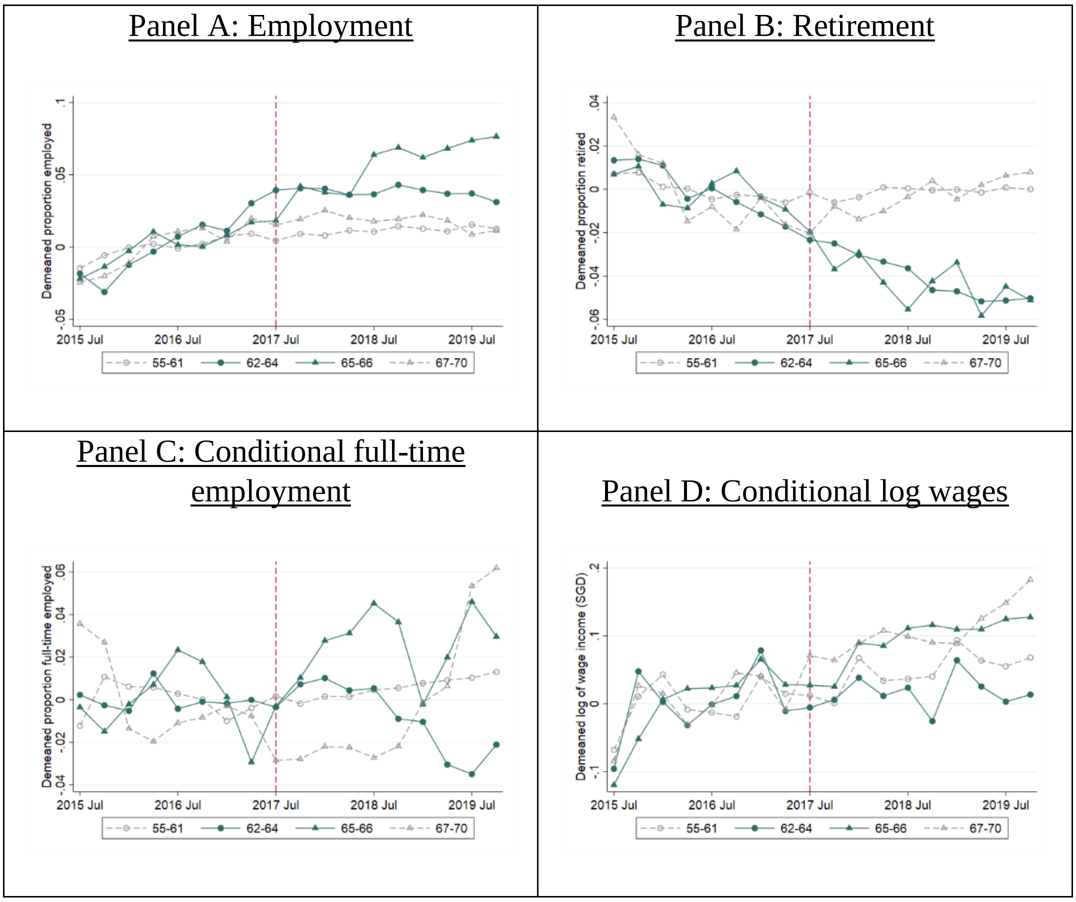
It is difficult to credibly estimate effects of announcing the RRA reform due to data limitations. Since the regression specifications assume no pre-implementation effects, the presence of announcement effects will invalidate the parallel trend assumption. However, any announcement effects are expected to be small since the reform was foreshadowed in the Act passed in 2012. As a robustness check, I estimate leading and lagging treatment effects.

Potential learning effects suggest that those aged 67-70 may not be an inappropriate control. As a result of a higher re-employment age, employers have more experience employing workers up to 67, affecting their willingness to re-hire beyond that. Therefore, labour market outcomes for the those aged

[17] Although CPF contribution rates and withdrawal eligibility are also determined by age, I do not explicitly control for them because there were no changes to these policies during the study period. Their effects are implicitly captured by age fixed effects.

67-70 may be also impacted by the RRA reform. To deal with this possibility, I re-estimate impacts using only those age 55-61 as control.

Figure 1: Trends in labour market outcomes by age



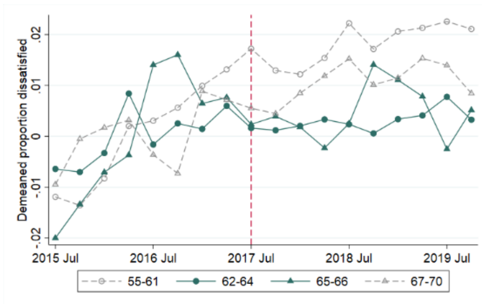
Notes: Outcomes are demeaned over pre-reform period values.

2.5 Results

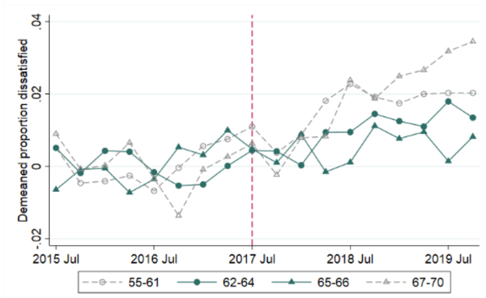
Figure 1 presents trends of average labour market outcomes as a "first cut" look at the effects of the RRA reform. I plot separate trends for the control group (ages 55-61 and 67-70) and the treated group (ages 62-64 and 65-66). Since labour market outcome levels vary substantially by age, I demean observations over their respective pre-reform period to improve comparison. Vertical dotted lines indicate when the re-employment age was raised from 65 to 67. Panel A show a separation of employment trends between treated and control ages after the RRA reform, with employment rates rising faster among treated ages. The opposite pattern for retirement is more distinct in panel B, with rates for treated ages falling steeply while rates for control ages remain flat. On the other hand, there is no clear indication of effects on intensive margins, as measured by full-time employment and wages in panels C and D respectively.

Figure 2: Trends in evaluative measures of well-being by age

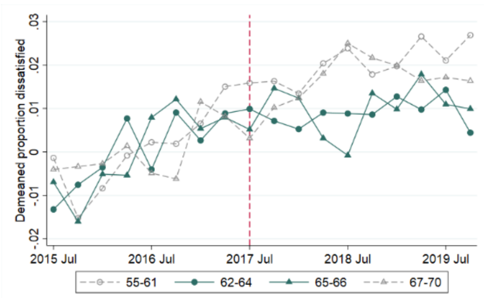
Panel A: Dissatisfied with life as a whole



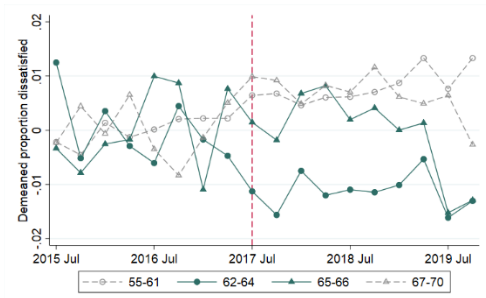
Panel B: Dissatisfied with household income



Panel C: Dissatisfied with daily activities / job



Panel D: Dissatisfied with health



Notes: Outcomes are demeaned over pre-reform period values.

Similar plots for evaluative measures of well-being are shown in Figure 2. Subjective well-being measures tend to be more variable, making it more difficult to discern any effects. However, subplots consistently show the likelihood of dissatisfaction for life as a whole and other domains tend to rise less for treated ages than control ages after the RRA reform. It is interesting to

Table 1. Overall impact of RRA reform on labour market outcomes

	(1)	(2)	(3)
Panel A: Employment			
$\hat{\rho}$	0.030**	0.033**	0.027**
	(0.012)	(0.013)	(0.013)
R-squared	0.062	0.077	0.077
Mean outcome	0.514	0.514	0.514
Observations	312,959	312,292	312,292
Panel B: Retirement			
$\hat{\rho}$	-0.037***	-0.029**	-0.023**
	(0.010)	(0.012)	(0.011)
R-squared	0.135	0.155	0.155
Mean outcome	0.194	0.194	0.194
Observations	312,959	312,292	312,292
Panel C: Conditional full-time employment			
$\hat{\rho}$	-0.003	0.014	0.006
	(0.015)	(0.017)	(0.016)
R-squared	0.031	0.069	0.070
Mean outcome	0.725	0.725	0.725
Observations	160,821	160,531	160,531
Panel D: Conditional log wages			
$\hat{\rho}$	-0.017	0.038	0.033
	(0.033)	(0.032)	(0.029)
R-squared	0.052	0.333	0.333
Mean outcome	7.83	7.83	7.83
Observations	154,283	154,023	154,023
Controls:			
Year-month and age fixed effects	Yes	Yes	Yes
Demographics	No	Yes	Yes
Age-related policies	No	No	Yes

Notes: Cluster-robust standard errors at the household level are reported in parentheses. Statistical significance denoted by * p<0.1, ** p<0.05, *** p<0.01. Demographics include 5-year birth cohort, gender, race, education, marital status, number of children, number of other household members. Age-related policies include Workfare Income Supplement, Special Employment Credit, CPF monthly payout eligibility.

note that for subplots of Figure 1 and 2 where there is a clear separation between treated and control post-reform, both treated ages 62-64 and 65-66 tend to have move in tandem. Since individuals aged 62-64 are not directly affected by the legislation change, this observation is consistent with mental anchoring.

Accounting for possible confounders, I calculate difference-in-differences estimates and progressively include controls described in the previous section. Table 1 reports the results for labour market outcomes. Estimated impacts on employment and retirement rates are in the expected direction and are statistically significant at the 5% level. Effect magnitudes remain similar after controlling for demographic characteristics and other age-related policies, which gives confidence that the key identification assumption is likely to hold. Estimates based on the full specification in equation (2.2) show that the RRA reform increased employment by 2.7pp or 5.3% relative to the mean, and decreased retirement by 2.3pp or 11.8% relative to the mean. Conditional on being employed, the RRA reform does not seem to have an impact on working hours or wage income. Although point estimates are positive, they are statistically insignificant.

To verify mental anchoring as a mechanism, I estimate the reform's effect on those whose re-employment age remain unchanged at 65 (i.e. born before July 1952). Because employers are not required to re-employ such individuals to age 67, a change to employment rates of ages 65-66 relative to ages 67-70 is evidence of mental anchoring. Column 1 of Table 2 present results for employment and retirement using the full regression specification in equation (2.2). The direction and magnitude of estimates are comparable to those in Table 1 but are statistically insignificant due to a substantially smaller sample.

Table 2. Impact of RRA reform on extensive labour market outcomes by birth cohort

	(1)	(2)
Panel A: Employment		
$\hat{\rho}$	0.026	0.032*
	(0.028)	(0.019)
R-squared	0.024	0.034
Mean outcome	0.315	0.582
Panel B: Retirement		
$\hat{\rho}$	-0.031	-0.022
	(0.029)	(0.015)
R-squared	0.069	0.059
Mean outcome	0.429	0.114
Sample	Born before July 1952	Born on or after July 1952
Treated	Ages 65-66	Ages 62-64
Control	Ages 67-70	Ages 55-61
Observations	67,188	225,720

Notes: Cluster-robust standard errors at the household level are reported in parentheses. Statistical significance denoted by * p<0.1, ** p<0.05, *** p<0.01. Additional controls are year-month and age fixed effects, demographics, and changes to age-related policies. Demographics include 5-year birth cohort, gender, race, education, marital status, number of children, number of other household members. Age-related policies include Workfare Income Supplement, Special Employment Credit, CPF monthly payout eligibility.

Increases in employment of ages 62-64 also provides additional support of mental anchoring if the reform has no effect on current labour decisions. From a life-cycle perspective, a higher re-employment age reduces uncertainty of future employment, which in turn influences current labour decisions. However, I claim any reduction is likely negligible because re-employment is still contingent on subjective work performance and employers have the option to make a relatively low one-off payment instead. Column 2 of Table 2 reports estimated impacts for ages 62-64 relative to ages 55-61. Like column 1, estimates are similar in direction and magnitude. Only the effect on employment is marginally significant at the 10% level.

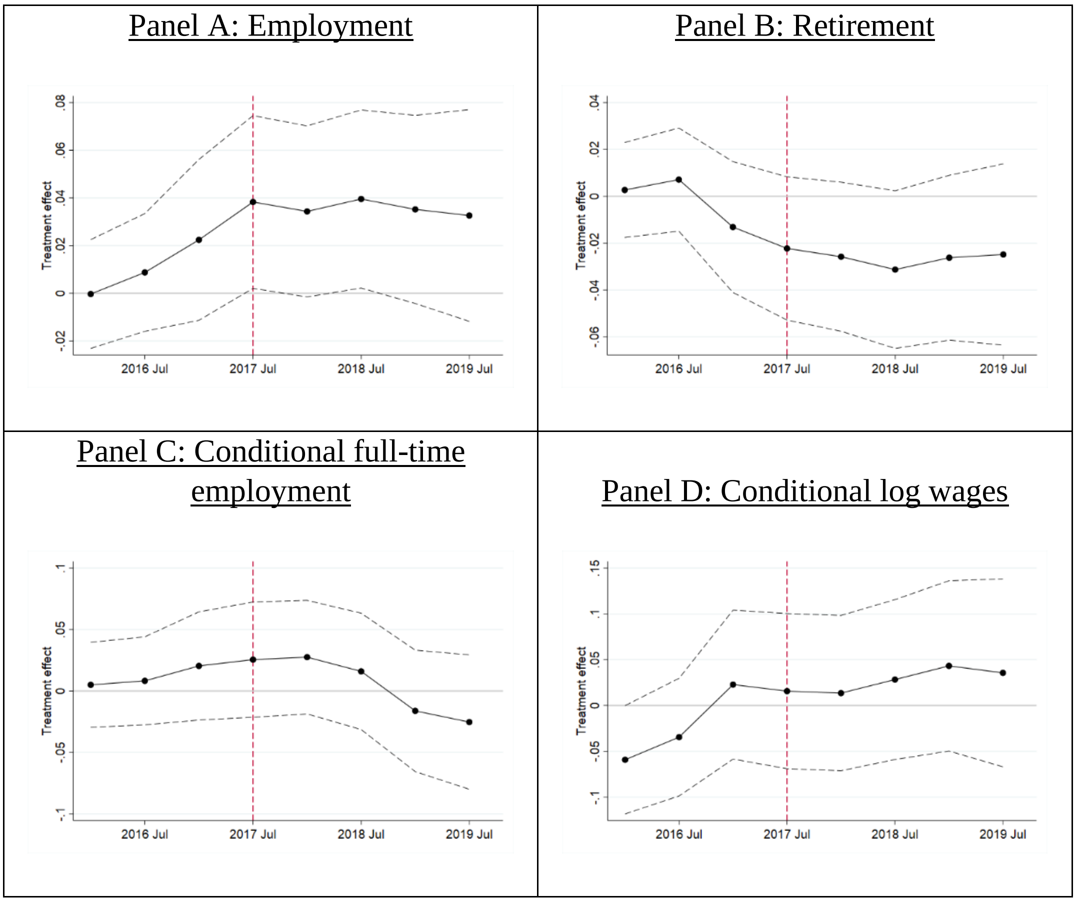
Table 3. Overall impact of RRA reform on evaluative measures of well-being

	(1)	(2)	(3)
Panel A: Dissatisfied with life overall			
$\hat{\rho}$	-0.013**	-0.019***	-0.015***
	(0.005)	(0.006)	(0.006)
R-squared	0.002	0.010	0.010
Mean outcome	0.073	0.073	0.073
Observations	313,649	312,975	312,975
Panel B: Dissatisfied with household income			
$\hat{\rho}$	-0.008	-0.015	-0.007
	(0.008)	(0.009)	(0.009)
R-squared	0.002	0.012	0.012
Mean outcome	0.162	0.162	0.162
Observations	313,381	312,712	312,712
Panel C: Dissatisfied with daily activities / job			
$\hat{\rho}$	-0.011*	-0.017**	-0.011*
	(0.006)	(0.007)	(0.006)
R-squared	0.003	0.012	0.012
Mean outcome	0.090	0.090	0.090
Observations	303,186	302,543	302,543
Panel D: Dissatisfied with health			
$\hat{\rho}$	-0.015**	-0.017**	-0.017**
	(0.007)	(0.009)	(0.008)
R-squared	0.001	0.008	0.008
Mean outcome	0.136	0.136	0.136
Observations	313,537	312,867	312,867
Controls:			
Year-month and age fixed effects	Yes	Yes	Yes
Demographics	No	Yes	Yes
Age-related policies	No	No	Yes

Notes: Cluster-robust standard errors at the household level are reported in parentheses. Statistical significance denoted by * p<0.1, ** p<0.05, *** p<0.01. Demographics include 5-year birth cohort, gender, race, education, marital status, number of children, number of other household members. Age-related policies include Workfare Income Supplement, Special Employment Credit, CPF monthly payout eligibility.

If changes to labour supply decisions are primarily due to mental anchoring, does increasing the re-employment age improve welfare? I explore this question by estimating effects on subjective well-being, a proxy for individual welfare. Table 3 reports the results on evaluative measures of well-being. Point estimates are negative across the board, suggesting there is no evidence that welfare has worsened on average. Like Table 1, the magnitude and statistical significance of estimated effects do not change substantially with the inclusion of controls for demographic characteristics and other age-related policies. Among the 4 measures, I find statistically significant reductions in dissatisfaction concerning life as a whole and health at the 5% level. Reduction in dissatisfaction with daily activities or job is also marginally significant and of a comparable magnitude. I validate significant results of Table 3 by examining impacts on similar welfare measures. I find statistically significant improvements in happiness, which is another popular measure of subjective well-being, and marginally significant improvements in self-reported health status (see Appendix Table 2 for details). I perform several robustness checks on potential weaknesses in my identification strategy. To verify that the parallel trend assumption holds, I estimate lead and lag treatment effects of 6-month intervals. This length was chosen instead of monthly intervals to improve statistical precision. July 2015 to December 2015, which includes the announcement event, form the reference period with its effect is set to zero. Results for labour market outcomes are graphed in Figure 3, and those for evaluative measures of well-being in Figure 4. Solid lines represent point estimates while dotted lines indicate the 95% confidence interval.

Figure 3: Impact of RRA reform on labour market outcomes

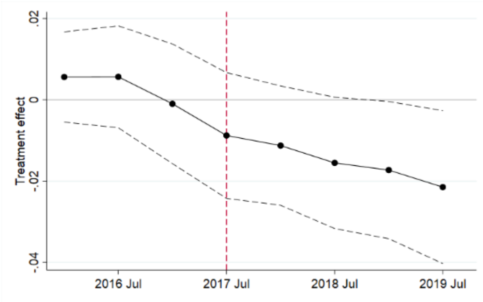


Notes: Solid circles represent point estimates while dotted lines indicate the 95% confidence interval based on household-clustered standard errors.

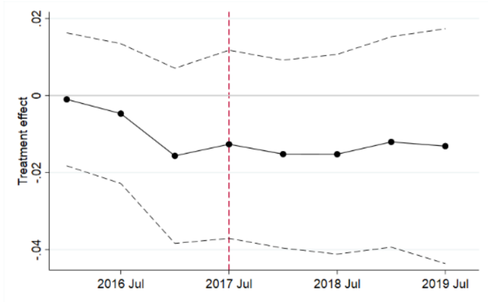
Parallel trends appear to hold in general because pre-reform differences are statistically insignificant. However, trending estimates 6 months prior to the reform, particularly for employment and retirement, hint at possible anticipation effects. When potential anticipation effects are included in regressions, impact estimates are slightly larger but less statistically significant as expected (see Appendix Table 3 and 4). This finding implies that results in Table 1 and 3 are likely attenuated and may represent a more conservative estimate. One concern is these patterns may also be due to unobserved cohort changes. However, this is unlikely given the short

Figure 4: Impact of RRA reform on evaluative measures of well-being

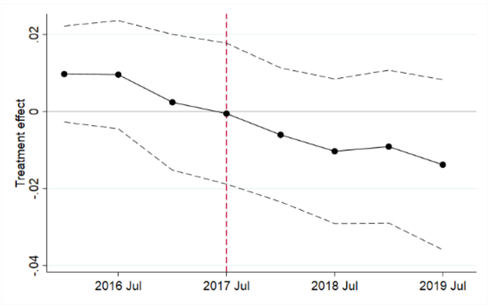
Panel A: Dissatisfied with life as a whole



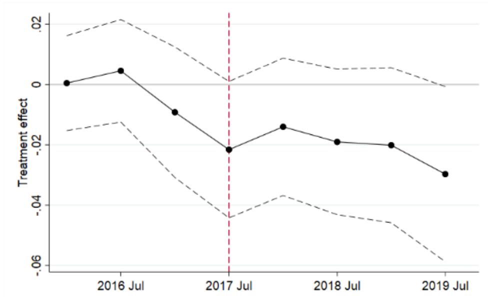
Panel B: Dissatisfied with household income



Panel C: Dissatisfied with daily activities / job



Panel D: Dissatisfied with health



Notes: Solid circles represent point estimates while dotted lines indicate the 95% confidence interval based on household-clustered standard errors.

analysis period of 4 years and robustness of estimates to the inclusion of demographic controls.

To address concerns of learning effects among those aged 67-70, I replicate the analyses in Tables 1 and 3 using only those aged 55-61 as controls (see Appendix Tables 5 and 6 for details). Estimated effects are very similar both in terms of magnitude and statistical significance. In fact, point estimates based on the new control group are slightly larger in all cases. These results suggest that learning effects, if any, are likely positive. Checks for parallel trend improved marginally using this new control group, with smaller pre-reform differences and larger post-reform differences (see Appendix Figure 1 and 2). However, there remains a clear anticipation effect at 6-months prior to reform.

Lastly, I separately estimate impacts for various subgroups of interest to explore possible heterogeneity. These subgroups vary by age, gender, educational attainment, dissatisfaction with household income at baseline (i.e. July 2015), and self-rated retirement preparedness at baseline. For this analysis, I retain ages 67-70 as controls for better statistical power. Estimates and their respective 95% confidence intervals are presented in Appendix Figure 3 and 4. In general, I fail to find convincing evidence of heterogeneity in labour market outcomes. Point estimates indicate that those initially dissatisfied with household income have somewhat larger employment effects, which is expected. There is also weak evidence suggesting that older or low-educated workers experience lower, and possibly negative, wage effects. Concerning subjective well-being, I find larger reductions in overall life dissatisfaction among those who were initially dissatisfied with household income and unprepared for retirement. This result is consistent with the objective of re-employment to protect against involuntary retirement. There is also evidence that more vulnerable groups, such as those with primary school or lower education, from poorer households, or unprepared for retirement, have greater reductions in dissatisfaction with health. Again, this finding consistent with the effects of involuntary retirement on health (Gallo, Bradley, Siegel, & Kasl, 2000; Rhee, Mor Barak, & Gallo, 2016).

2.6 Concluding Remarks

This study examines the effects of the Retirement and Re-employment Act (RRA) reform on labour market outcomes and subjective well-being. I provide novel evidence of mental anchoring on statutory retirement age in a non-pension setting, suggesting that statutory retirement ages may have powerful behavioural effects. My findings have important implications on the design of interventions to encourage elderly employment and deferring pension claims. I also show weak evidence that mandating re-employment beyond the pension eligibility age may increase employment. Future work should examine whether this also translates to deferred pension claims.

Potential negative wage impacts on more elderly or less educated workers highlights another issue that warrants further investigation: RRA may institutionalize annual wage bargaining in a manner that disadvantages vulnerable workers. Re-employed workers are in some sense “bonded” to their employer if they wish to remain under the protection of RRA. This restrains job mobility and undermines the worker’s bargaining power, while employers are free to adjust wages along with the job scope. Low-skilled workers, who have more difficulties remaining employed, are especially vulnerable. To improve job mobility, policymakers may consider relaxing the work history criteria for re-employment. Some possibilities include reducing the required period of past employment, or also taking into consideration past employment beyond age 62.

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2.8 Appendix

Appendix A: Information on changes to other age-related policies

CPF Payout Eligibility Age

Year of Birth	Payout Eligibility Age	Year announced
1944 and earlier	60	1986
1994 to 1949	62	1997
1950 to 1951	63	2007
1952 to 1953	64	2007
1954 and later	65	2007

Source: www.cpf.gov.sg

Workfare Income Supplement

Year	Annual maximum benefits by age group				Income ceiling
	35-44	45-54	55-60	60+	
2013-2016	1,400	2,100	2,800	3,500	1,900
2017-2019	1,500	2,196	2,904	3,600	2,000

Source: www.workfare.gov.sg

Special Employment Credit

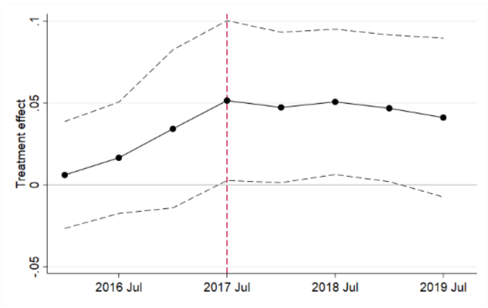
Age band	2015	2016	2017-2019	2020
50-54	8.5%	8%	0%	0%
55-59	8.5%	8%	3%	3%
60-64	8.5%	8%	5%	5%
65-66 (born on or after 1 July 1952)	11.5%	11%	11% → 8%*	8%
65+ (born before 1 July 1952)	11.5%	11%	11%	11%
67+ (born on or after 1 July 1952)	11.5%	11%	11%	11%

* Subsidy rate decreased on 1 July 2017 when the re-employment age increased from 65 to 67.

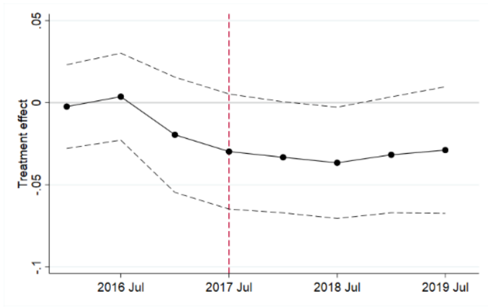
Source: www.sec.gov.sg

Appendix Figure 1: Impact of RRA reform on labour market outcomes, using ages 55-61 as the control only

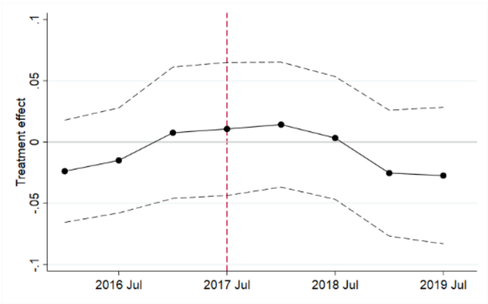
Panel A: Employment



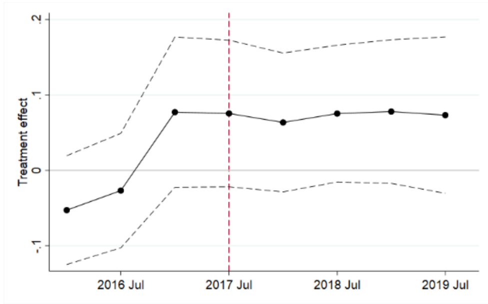
Panel B: Retirement



Panel C: Conditional full-time employment



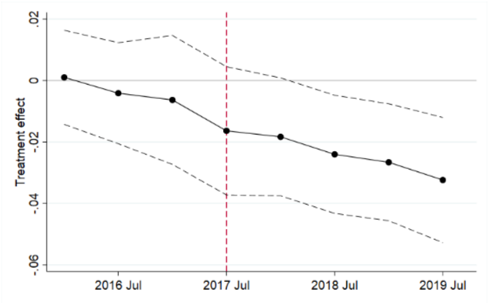
Panel D: Conditional log wages



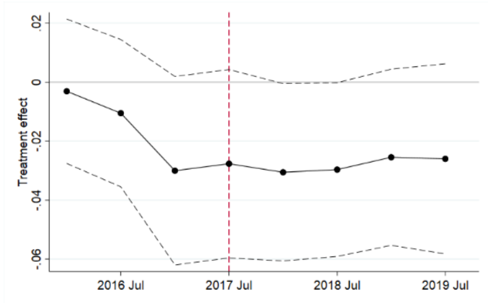
Notes: Solid circles represent point estimates while dotted lines indicate the 95% confidence interval based on household-clustered standard errors.

Appendix Figure 2: Impact of RRA reform on evaluative measures of well-being, using ages 55-61 as the control only

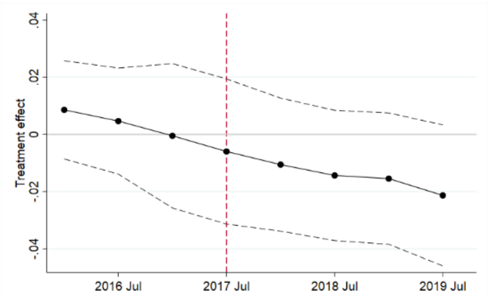
Panel A: Dissatisfied with life as a whole



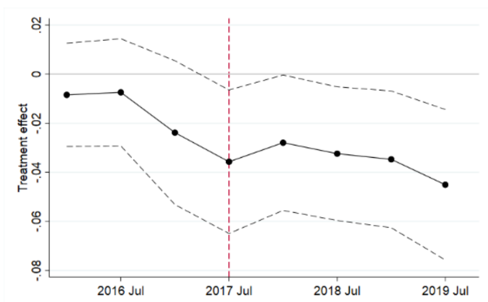
Panel B: Dissatisfied with household income



Panel C: Dissatisfied with daily activities / job

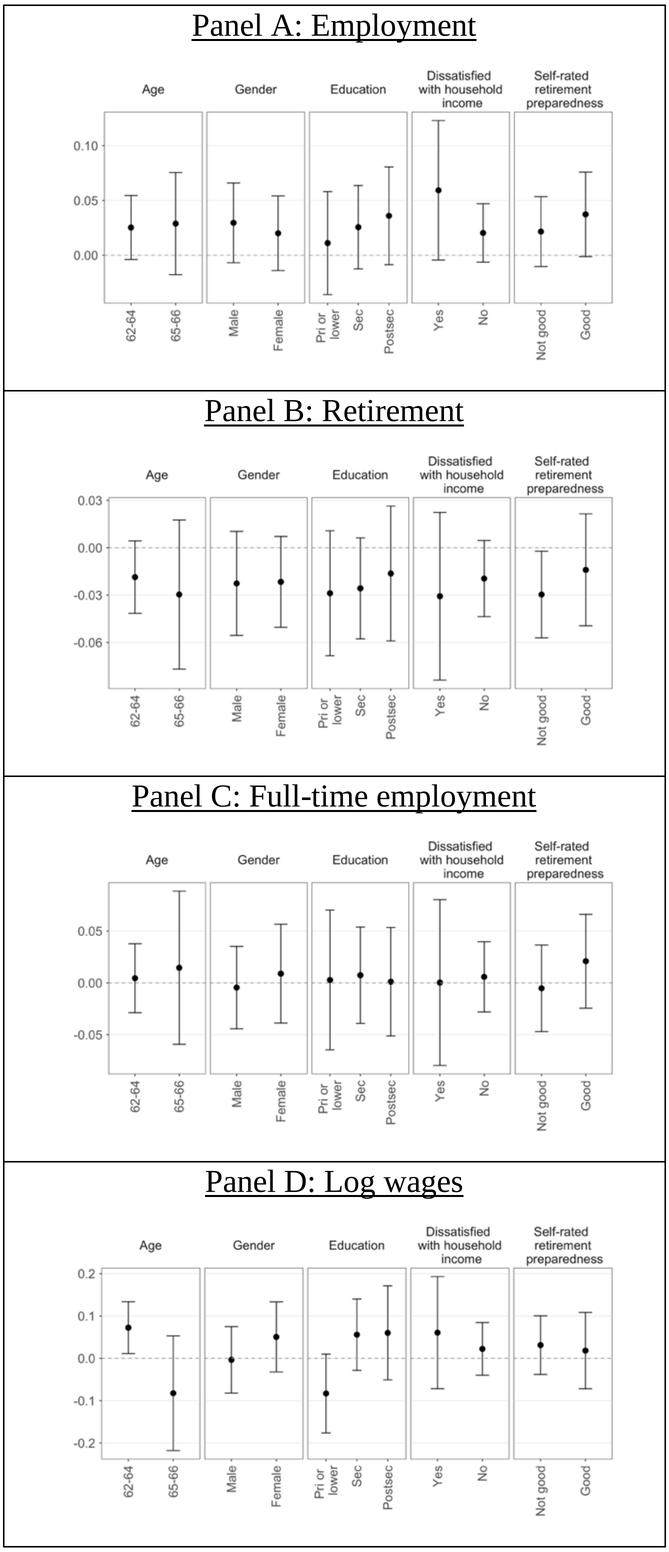


Panel D: Dissatisfied with health



Notes: Solid circles represent point estimates while dotted lines indicate the 95% confidence interval based on household-clustered standard errors.

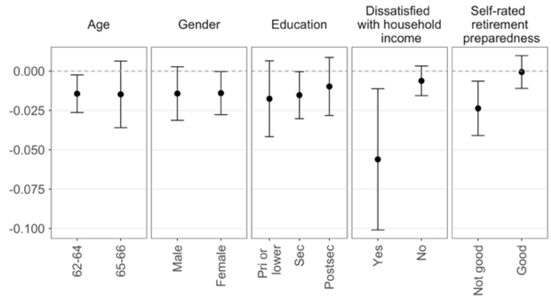
Appendix Figure 3: Heterogeneous impacts on labour market outcomes



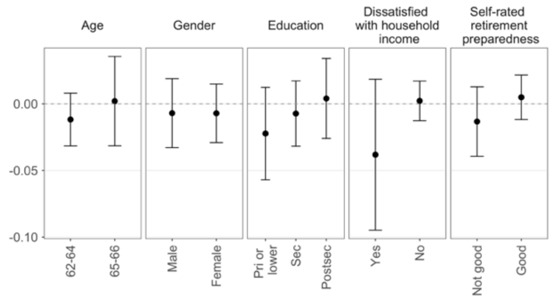
Notes: Point estimates are indicated by solid circles. 95% confidence intervals based on household-clustered errors are indicated by bars.

Appendix Figure 4: Heterogeneous impacts on evaluative measures of well-being

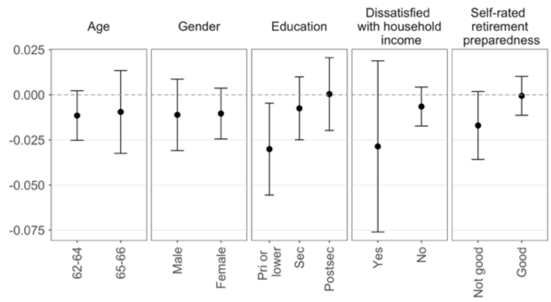
Panel A: Dissatisfied with life as a whole



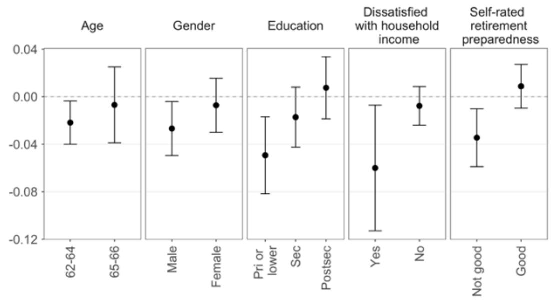
Panel B: Dissatisfied with household income



Panel C: Dissatisfied with daily activities / job



Panel D: Dissatisfied with health



Notes: Point estimates are indicated by solid circles. 95% confidence intervals based on household-clustered errors are indicated by bars.

Appendix Table 1. Summary of respondent characteristics aged 55-70, July 2015 to Dec 2019

	(1)	(2)	(3)
Age	68.4	66.5	59.9
	(1.09)	(1.50)	(3.01)
Male	0.506	0.498	0.480
	(0.500)	(0.500)	(0.500)
Chinese	0.882	0.888	0.863
	(0.322)	(0.315)	(0.344)
No formal schooling/Primary	0.362	0.267	0.200
	(0.481)	(0.442)	(0.400)
Secondary	0.378	0.440	0.426
	(0.485)	(0.496)	(0.494)
Post-secondary	0.260	0.293	0.375
	(0.439)	(0.455)	(0.484)
Married	0.751	0.778	0.802
	(0.433)	(0.416)	(0.398)
# of children	1.99	1.91	1.84
	(0.965)	(0.944)	(1.01)
# of other household members	2.06	2.12	2.44
	(1.23)	(1.23)	(1.20)
Number of persons	2,322	1,493	9,093
Number of observations	33,917	40,928	238,676
Birth cohort	Before 1950	1950 to June 1952	July 1952 or later
Re-employment age	-	65	67

Note: Standard deviations are reported in parenthesis. Individuals born 1945 to 1949 are not eligible for re-employment.

Appendix Table 2. Impact of RRA reform on other measures of well-being

	(1)	(2)	(3)
Panel A: Happy none or little or some of the time			
$\hat{\rho}$	-0.025**	-0.029**	-0.023**
	(0.010)	(0.012)	(0.012)
R-squared	0.003	0.016	0.016
Mean outcome	0.369	0.369	0.369
Observations	100,608	100,402	100,402
Panel B: Good health or better			
$\hat{\rho}$	0.030***	0.030**	0.021*
	(0.011)	(0.013)	(0.012)
R-squared	0.004	0.013	0.013
Mean outcome	0.622	0.622	0.622
Observations	313,503	312,831	312,831
Controls:			
Year-month and age fixed effects	Yes	Yes	Yes
Demographics	No	Yes	Yes
Age-related policies	No	No	Yes

Notes: Cluster-robust standard errors at the household level are reported in parentheses. Statistical significance denoted by * p<0.1, ** p<0.05, *** p<0.01. Demographics include 5-year birth cohort, gender, race, education, marital status, number of children, number of other household members. Age-related policies include Workfare Income Supplement, Special Employment Credit, CPF monthly payout eligibility.

Happiness measure is based on a quarterly question "During the past 30 days, how much of the time have you been a happy person?", with possible responses ranging from "None of the time" to "All of the time" on a 6-point scale".

General health measure is based on the monthly question "Would you say your health is excellent, very good, good, fair, or poor?", with respondents selecting only one option.

Appendix Table 3. Impact of RRA reform on labour market outcomes, allowing for anticipation effects

Outcomes:	Employment	Retirement	Conditional full-time employment	Conditional log wages
	(1)	(2)	(3)	(4)
Treated X (13-18 months prior)	-0.001	0.003	0.002	-0.058*
	(0.011)	(0.010)	(0.017)	(0.030)
Treated X (7-12 months prior)	0.008	0.007	0.005	-0.033
	(0.012)	(0.011)	(0.018)	(0.032)
Treated X (1-6 months prior)	0.022	-0.013	0.016	0.025
	(0.016)	(0.014)	(0.021)	(0.039)
Treated X Post-expansion	0.036**	-0.026*	0.014	0.023
	(0.017)	(0.015)	(0.022)	(0.040)
R-squared	0.077	0.155	0.070	0.333
Mean outcome	0.514	0.194	0.725	7.828
Observations	312,292	312,292	160,531	154,023

Notes: Cluster-robust standard errors at the household level are reported in parentheses. Statistical significance denoted by * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Estimates control for age (in months) and year-month fixed effects, demographic and age-related policies. Demographics include 5-year birth cohort, gender, race, education, marital status, number of children, number of other household members. Age-related policies include Workfare Income Supplement, Special Employment Credit, CPF monthly payout eligibility.

Appendix Table 4. Impact of RRA reform on evaluative measures of well-being, allowing for anticipation effects

Outcomes:	Dissatisfied with life overall	Dissatisfied with household income	Dissatisfied with daily activities / job	Dissatisfied with health
	(1)	(2)	(3)	(4)
Treated X (13-18 months prior)	0.005	-0.001	0.009	0.000
	(0.006)	(0.009)	(0.006)	(0.008)
Treated X (7-12 months prior)	0.005	-0.005	0.009	0.004
	(0.006)	(0.009)	(0.007)	(0.008)
Treated X (1-6 months prior)	-0.002	-0.016	0.001	-0.010
	(0.007)	(0.011)	(0.009)	(0.011)
Treated X Post-expansion	-0.013*	-0.014	-0.006	-0.020*
	(0.007)	(0.011)	(0.008)	(0.011)
R-squared	0.010	0.012	0.012	0.008
Mean outcome	0.073	0.162	0.090	0.136
Observations	312,975	312,712	302,543	312,867

Notes: Cluster-robust standard errors at the household level are reported in parentheses. Statistical significance denoted by * p<0.1, ** p<0.05, *** p<0.01. Estimates control for age (in months) and year-month fixed effects, demographic and age-related policies. Demographics include 5-year birth cohort, gender, race, education, marital status, number of children, number of other household members. Age-related policies include Workfare Income Supplement, Special Employment Credit, CPF monthly payout eligibility.

Appendix Table 5. Impact of RRA reform on labour market outcomes, using ages 55-61 only as the control group

	(1)	(2)	(3)
Panel A: Employment			
$\hat{\rho}$	0.031***	0.033**	0.028**
	(0.012)	(0.015)	(0.013)
R-squared	0.035	0.052	0.052
Mean outcome	0.551	0.551	0.551
Observations	267,137	266,598	266,598
Panel B: Retirement			
$\hat{\rho}$	-0.038***	-0.027**	-0.023**
	(0.009)	(0.012)	(0.011)
R-squared	0.090	0.105	0.105
Mean outcome	0.150	0.150	0.150
Observations	267,137	266,598	266,598
Panel C: Conditional full-time employment			
$\hat{\rho}$	-0.003	0.018	0.007
	(0.015)	(0.018)	(0.016)
R-squared	0.013	0.054	0.054
Mean outcome	0.743	0.744	0.744
Observations	147,139	146,901	146,901
Panel D: Conditional log wages			
$\hat{\rho}$	-0.010	0.038	0.050*
	(0.032)	(0.033)	(0.029)
R-squared	0.026	0.326	0.326
Mean outcome	7.88	7.88	7.88
Observations	141,258	141,045	141,045
Controls:			
Year-month and age fixed effects	Yes	Yes	Yes
Demographics	No	Yes	Yes
Age-related policies	No	No	Yes

Notes: Cluster-robust standard errors at the household level are reported in parentheses. Statistical significance denoted by * p<0.1, ** p<0.05, *** p<0.01. Demographics include 5-year birth cohort, gender, race, education, marital status, number of children, number of other household members. Age-related policies include Workfare Income Supplement, Special Employment Credit, CPF monthly payout eligibility.

Appendix Table 6. Impact of RRA reform on evaluative measures of well-being, using ages 55-61 only as the control group

	(1)	(2)	(3)
Panel A: Dissatisfied with life overall			
$\hat{\rho}$	-0.015***	-0.024***	-0.017***
	(0.005)	(0.007)	(0.006)
R-squared	0.002	0.010	0.011
Mean outcome	0.074	0.074	0.074
Observations	267,736	267,193	267,193
Panel B: Dissatisfied with household income			
$\hat{\rho}$	-0.008	-0.019*	-0.012
	(0.008)	(0.010)	(0.009)
R-squared	0.002	0.011	0.011
Mean outcome	0.165	0.165	0.165
Observations	267,549	267,011	267,011
Panel C: Dissatisfied with daily activities / job			
$\hat{\rho}$	-0.012*	-0.020***	-0.013**
	(0.006)	(0.007)	(0.006)
R-squared	0.003	0.012	0.012
Mean outcome	0.093	0.093	0.093
Observations	258,666	258,150	258,150
Panel D: Dissatisfied with health			
$\hat{\rho}$	-0.015**	-0.019**	-0.019**
	(0.007)	(0.009)	(0.008)
R-squared	0.001	0.007	0.007
Mean outcome	0.133	0.133	0.133
Observations	267,635	267,096	267,096
Controls:			
Year-month and age fixed effects	Yes	Yes	Yes
Demographics	No	Yes	Yes
Age-related policies	No	No	Yes

Notes: Cluster-robust standard errors at the household level are reported in parentheses. Statistical significance denoted by * p<0.1, ** p<0.05, *** p<0.01. Demographics include 5-year birth cohort, gender, race, education, marital status, number of children, number of other household members. Age-related policies include Workfare Income Supplement, Special Employment Credit, CPF monthly payout eligibility.

Chapter 3: Heterogeneous Health Effects of Medical Marijuana Legalization Among Young Adults in the United States

3.1 Introduction

Legalizing marijuana for medical purposes is controversial. Numerous marijuana products are being marketed and used as therapeutic drugs or health supplements despite not being approved by the Food and Drug Administration[18]. Not only is evidence of marijuana's health effects limited (NASEM, 2017), potential exploitation of medical marijuana laws (MML) for recreational use is also a concern because of associated harms to health and psychosocial functioning (WHO, 2016). Young adults aged 18 to 25 are especially vulnerable given that prevalence of recreational use is highest relative to other age groups (SAMHSA, 2017). They are also more susceptible to long-term damage (Hall, 2015) and less likely to perceive marijuana as harmful after medical marijuana legalization (Wen, Hockenberry, & Druss, 2019).

Can prospective health gains from legitimate medical use be diminished or even surpassed by losses from recreational use? If so, which groups are most at risk and what role do regulations play in mitigating this adverse impact? To address these questions, I examine the impact of MML on self-reported health measures of young adults. Using data from the Behavioral Risk Factors

[18] Marijuana products, which refer to the whole, unprocessed marijuana plant or its basic extracts, are not approved by the Food and Drug Administration. Only a few marijuana-derived and synthetic marijuana-related products, such as Epidiolex (cannabidiol), Marinol (dronabinol), Syndros (dronabinol), and Cesamet (nabilone), have been approved for treating specific forms of epilepsy and chemo-induced nausea.

Surveillance System (BRFSS), I adopt a difference-in-differences identification strategy which utilizes variations in the timing of MML implementation across states. In addition to controlling for state- and time-fixed effects, I also allow for spatial spill-overs from neighbouring state's MML following Kalbfuß, Odermatt, and Stutzer (2018).

The results show positive and significant health gains from implementing MMLs that adhere to universal medical and pharmaceutical standards imposed on other controlled substances (i.e. "medicalized"), but small and insignificant effects from MMLs with lax access rules (i.e. "non-medical"). Furthermore, heterogeneous responses across population subgroups may have implications on inequality in health outcomes. Subgroups usually associated with poorer health, such as those from low-income households and the uninsured, enjoy greater health gains under "medicalized" MML. However, under "non-medical" MML, these subgroups are more likely to report having bad health.

These findings contribute to the literature in three ways. To my best knowledge, this is the first study to examine how MML affects the overall health of young adults. Several studies examine MML's impact on mortality rates due to suicide (Grucza et al., 2015; Anderson, Rees, & Sabia, 2014), traffic accidents (Anderson, Hansen, & Rees, 2013) and drug overdose (Powell, Pacula, & Jacobson, 2018; Smart, 2015). While informative, these extreme outcomes miss subtler health changes which are also of interest. On the other hand, specific aspects of health considered such as body weight (Sabia, Swigert, & Young, 2017), mental health (Kalbfuß et al., 2018), and opioid addiction (Powell et al., 2018) are unlikely to reflect all potential avenues through which marijuana use can benefit or harm health. Using a broad health measure, such as general health status, is practical from a policy perspective because it summarizes net changes in health without requiring extensive knowledge about the underlying mechanisms.

Second, this study shows substantial differences in health impacts by the extent MML incorporates medical and pharmaceutical regulations. It also suggests that future research on health outcomes should consider a wider range of MML provisions when investigating heterogeneous effects. Dimensions along which MML differ range from legal definitions such as decriminalization, removal of state penalties for possession, legal protection of dispensaries, to implementation issues such as permitting home cultivation, treatment of unspecified pain, and regulating supply quality in a commercial market (Pacula & Smart, 2017). Due to its complexity, most studies account for MML heterogeneity by controlling for only a few provisions, e.g., those concerning supply like dispensary operation and home cultivation (e.g. Pacula, Powell, Heaton, & Sevigny, 2015; Kalbfuß et al., 2018; Powell et al., 2018; Sabia et al., 2017). However, there is no a priori reason to ignore other provisions, especially those that restricts non-medical use. Following Abouk and Adams (2018), I adopt a more comprehensive MML classification originally developed by Williams, Olfson, Kim, Martins, and Kleber (2016) to assess the medical orientation of MML as a whole.

Third, this study highlights how sensitive the health of lower socioeconomic subgroups are to MML, medicalized or otherwise. Existing studies have only investigated heterogeneity by age and gender (e.g. Abouk & Adams, 2018; Smart, 2015). However, racial and ethnic differences in marijuana use (Williams, Pacula, & Smart, 2019; Keyes, Wall, Feng, Cerdá, & Hasin, 2017) and the fact that marijuana is not covered by health insurance have important implications for the role MML plays on persistent health disparities in the United States (Zimmerman & Anderson, 2019; Singh et al., 2017). This paper is the first to document heterogeneous health effects of MML along socioeconomic characteristics. The results suggest a narrowing of health inequality under "medicalized" MML, but the reverse under "non-medicalized" MML.

The rest of this paper is organized as follows: Section 2 summarizes the background of MML in the U.S. and provides a brief literature review; Section 3 describes the BRFSS data used; In Section 4, I detail the empirical strategy; Section 5 reports the regression results; Section 6 concludes with a discussion of the findings.

3.2 Background and Literature Review

The use of marijuana was illegal in the United States before 1996 (Marijuana Policy Project, 2016) and remains classified as a Schedule 1 drug[19], along with heroin, ecstasy and LSD. Besides prohibiting marijuana prescriptions, federal laws restrict the supply of research-grade marijuana necessary for randomized controlled trials (NASEM, 2017). As a result, there are limited rigorous randomized trials which demonstrate the efficacy of medical marijuana. A comprehensive literature review undertaken by the National Academies of Sciences, Engineering, and Medicine (2017) found mostly inconclusive evidence for marijuana's therapeutic effects and associated health risks. While

[19] See https://www.dea.gov/drug-scheduling for more information.

strong empirical support exists for the use of marijuana to treat chemotherapy-induced nausea, chronic pain, and spasm caused by multiple sclerosis, there is also substantial evidence for risks such as bronchitis, cannabis use disorder, schizophrenia, motor vehicle crashes, and lowered birth weight.

Decisions to enact MML appear largely driven by public opinions and influential lobby groups (Cousijn, Núñez, & Filbey, 2018). California was the first to effectively remove criminal penalties for cultivation, possession and use of marijuana for qualifying patients with a doctor's recommendation. Many states followed shortly, each enacting their own MML with varying provisions and conditions for medical use. As of 2018, 31 states and the District of Columbia (D.C.) have effective MMLs that remove criminal penalties for possession and use of medical marijuana, and allow a realistic means of access (e.g. home cultivation, dispensaries). Appendix Table 1 details starting dates of effective legalization of medical and recreational marijuana. Louisiana and West Virginia are not classified as MML states because there was no legal means of accessing medical marijuana during the analysis period.

Temporal and geographical variations in MML implementation present an opportunity to investigate its consequences. Marijuana use is of first-order interest in the literature. Studies on adult use tend to have insignificant findings due to unaccounted heterogeneity in laws (Pacula & Smart, 2017), but more recent findings report positive effects on both extensive and intensive margins when narrowed to specific MML provisions or high risk groups (Pacula et al., 2015; Wen, Hockenberry, & Cummings, 2015; Smart, 2015). In particular, Chu (2014) finds sizeable increases in illegal use based on marijuana possession arrests and admissions to marijuana-abuse treatment. Also among young adult males, Williams et al. (2019) finds varied quitting responses by ethnicity and legal status of dispensaries. These findings reflect the concern that MML could encourage non-medical use through increased accessibility.

MML's influence on marijuana use can affect health through a variety of channels. In terms of its benefits, several studies find that MML leads to less pain among the elderly (Nicholas and Maclean 2019), improved mental health (Kalbfuß et al., 2018), decreased body weight and obesity rates (Sabia et al., 2017), and reduced sickness absence from work (Ullman, 2017). Also, a reduction in prescriptions filled for diagnoses such as nausea, pain, depression and seizures suggests that marijuana is used as an alternative (Bradford & Bradford, 2017a; Bradford & Bradford, 2018). In terms of adverse effects, there is no evidence that MML increases suicide rates (Bartos, Kubrin, Newark, & Mccleary, 2019; Grucza et al., 2015; Anderson et al., 2014) despite marijuana's association with depression, suicide ideation and attempt (WHO, 2016). However, MML may raise the risk of cardiac deaths among older adults (Abouk & Adams, 2018). In addition to increased marijuana consumption, MML can impact health through the degree harmful substances are substitutes or complements to marijuana. There is consistent evidence that MML decreases use of illegal opioids (Smith, 2020; Powell et al., 2018; Chu, 2015), as well as opioid-related deaths (Powell et al., 2018; Kim et al., 2016; Smart, 2015; Bachhuber, Saloner, Cunningham, & Barry, 2014) and hospitalizations (Shi, 2017). Effects of MML on alcohol consumption is mixed (Santaella-Tenorio, et al., 2017; Wen et al., 2015; Pacula, Powell, Heaton, & Sevigny, 2013) and the effects on smoking relatively unexplored (Choi, Dave, & Sabia, 2018).

Significant heterogeneity of health impacts across MML provisions likely reflects heterogeneity in use. Yet varying effects over a range of health outcomes makes it challenging to interpret and inform policy decisions. For instance, lax regulation of dispensaries results in greater reduction in opioid deaths (Powell et al., 2018) but the presence of dispensaries does not have an independent effect on opioid-related hospitalizations (Shi, 2017). Increased alcohol-related traffic fatalities are associated with the allowance of dispensaries (Pacula et al., 2013) but not the actual operation of dispensaries (Santaella-Tenorio, et al., 2017). There are several reasons for the lack of consistent results. First, dispensaries are associated with increased marijuana potency (Sevigny, Pacula, & Heaton, 2014) which could have offsetting health effects. Second, most studies use a simple binary variable for dispensary, which does not reflect the extent it is regulated or its population coverage within a state. Third, supply-side provisions like dispensaries and home cultivation do not work in isolation. Other provisions such as requirements to be a registered medical marijuana user, supply quality regulations, and physician training for recommending marijuana can also affect health outcomes.

To assess MML provisions as a whole, I investigate MML heterogeneity based on Williams et al.'s (2016) classification. MMLs in effect as of 2016 are identified as either "medicalized" or "non-medical" according to basic tenets of medical practice, Current Good Manufacturing Practices[20], and restrictions on controlled substances. On the other hand, states with "medicalized" MML vary Williams, Santaella-Tenorio, Mauro, Levin, & Martins (2017) find an increase

[20] The main regulatory standard set by the Food and Drug Administration (FDA) to certify pharmaceutical quality.

in adult marijuana use for states with “non-medicalized” MML, but not for states with “medicalized” MML. This classification also has strong agreement with another independently derived taxonomy to characterize MML restrictiveness (Chapman, Spetz, Lin, Chan, & Schmidt, 2016). “Medicalized” MML have much higher levels of restrictiveness than “non-medicalized” MML, with little overlap between the two types.

3.3 Behavioral Risk Factors Surveillance System

The BRFSS is a repeated cross-sectional and nationally representative survey of U.S. residents. It is conducted annually via telephone survey[21]. Detailed questions on health conditions, health-related behaviours, use of healthcare services and basic demographic characteristics are collected from adults aged 18 and above. Appendix Table 2 summarizes respondent characteristics by state’s MML status.

My study focuses on a self-assessed measure of general health of adults aged 18-29 from 50 states and D.C., over the years 1993- 2018. The survey asks respondents “Would you say that in general your health is excellent, very good, good, fair or poor?” I initially treat responses as cardinal, assigning the value 5 for the best possible health status, i.e. excellent, and the value 1 for the worst possible health status, i.e. poor. I call this variable the self-assessed health score. While this constructed variable is a useful summary of health status, it may not adequately reflect changes in the distribution of health. For example, MML may be beneficial for the relatively healthy but detrimental to the relatively unhealthy. These changes may cancel each other out when viewed through the lens of

[21] Cellular phone lines were included from 2011 onwards. All estimates use the sampling weights provided.

health scores, resulting in a null effect. Therefore, I also estimate MML impacts on reporting specific health states.

In addition to self-assessed health, I analyse impacts on the number of days in the past 30 days that a respondent's health is not good, differentiating between physical health (e.g. physical illness and injury) and mental health (e.g. stress, depression, emotional problems).

3.4 Empirical Strategy

I adopt a difference-in-differences identification strategy commonly used in the literature (e.g. Anderson et al., 2013; Wen et al., 2015). The regression specification is

$$\begin{aligned} y_{ist} = {} & \beta_1 MML_{st} + \beta_2 NMML_{st} + \beta_3 (NMML_{st} \times MML_{st}) \\ & + \boldsymbol{\beta}_4 \mathbf{X}_{it} + \boldsymbol{\beta}_5 \mathbf{Z}_{st} + \alpha_s + \delta_t + \lambda_s t + \varepsilon_{ist} \end{aligned} \tag{3.1}$$

y_{ist} is the health measure. MML_{st} is a dummy variable indicating that state s has an MML in effect at period t. The value of MML_{st} is determined daily using the exact interview date. β_1 is the causal effect of interest here, representing the overall average impact of MML when there is no adjacent MML state. α_s captures any time-invariant state characteristics. δ_t non-parametrically captures year-quarterly trends common across states, while $\lambda_s t$ captures any unobserved state heterogeneity that trend linearly. ε_{ist} is an error term.

Following Kalbfuß et al. (2018), I include spatial controls for the effect of neighbouring state's laws on medical marijuana. $NMML_{st}$ is a dummy variable indicating that at least one adjacent state has an MML. Like MML_{st}, its value is also determined daily. Recent studies suggest the importance of accounting for cross-border spill-overs (Hao & Cowan, 2019; Han, Compton, Blanco, & Jones, 2018; Hansen, Miller, & Weber, 2017). Furthermore, a neighbouring state's MML may have positive diffusion effect on a state's decision to implement MML (Bradford & Bradford, 2017b). In the presence of spill-overs, potentially different effects for states with or without neighbours with MML are captured by β_3.

$\mathbf{X}_{it}$ is a vector of individual and household characteristics such as age, ethnicity, gender, educational attainment, marital status, health insurance coverage, household income relative to the Federal Poverty Level (FPL), and the presence of any child in household. More importantly, the regression controls for time-varying health determinants at the state-level that potentially correlate with the adoption of MML. $\mathbf{Z}_{st}$ is a vector of state characteristics which include unemployment rate, beer and cigarette excise tax rate, Medicaid expansion status. I also control for whether recreational use is legalized to isolate the effects of MML only. Some of these controls may be affected by MML, resulting in potentially biased estimates. However, I show that including them do not materially change point estimates.

The key identification assumption is that the counterfactual trend in health outcomes of MML states are parallel to those of non-MML states. Therefore, I include annual lead effects of MML_{st} up to 5 years before MML implementation. The presence of pre-treatment effects indicates a violation of the common trend assumption. I also adopt a flexible specification for post-treatment effects in later analyses, replacing MML_{st} with a set of dummies to allow for annual effects up to 5 years after treatment.

In the initial analysis, I use the full sample to estimate the impact of MML. This approach unrealistically assumes homogeneous effects across states. Under heterogeneous effects, the estimate simply represents the average impact of different types of MML. To examine different impacts of strict and lax regulations, I conduct a series of subsample analyses. The analysis of "medicalized" MML restricts the sample to states with "medicalized" MML (Connecticut, Delaware, D.C., Illinois, Maryland, Massachusetts, Minnesota, New Hampshire, New Jersey, and New York) and non-MML states. Similarly, the analysis of "non-medical" MML restricts the sample to states with "non-medical" MML (Alaska, Arizona, California, Colorado, Hawaii, Maine, Michigan, Montana, New Mexico, Nevada, Oregon, Rhode Island, Vermont, Washington) and non-MML states. This approach implies that the classification of MML states do not change over time, which is true. Policy amendments over the analysis period mostly concern the allowance of dispensaries among "non-medical" MML states, which would have affected classification (Chapman, Spetz, Lin, Chan, & Schmidt, 2016). It also assumes that non-MML states are the appropriate control group, which is supported in the data. Retaining the same set of non-MML states in the sample allows a clearer comparison of impacts from the 2 types of MML. Other MML states that legalized medical marijuana after 2014 were not classified in Williams et al. (2016) and are excluded from subsequent analyses. I also investigate heterogeneous effects across key socioeconomic characteristics.

Table 1. Overall impact of any MML on self-assessed health score

	(1)	(2)	(3)	(4)
5 years prior	0.017	0.015	0.016	0.006
	(0.015)	(0.015)	(0.015)	(0.019)
4 years prior	0.014	0.012	0.008	-0.003
	(0.020)	(0.021)	(0.017)	(0.017)
3 years prior	0.004	0.002	-0.001	-0.013
	(0.012)	(0.012)	(0.012)	(0.014)
2 years prior	0.023	0.021	0.019	0.000
	(0.016)	(0.017)	(0.018)	(0.018)
1 years prior	-0.017	-0.020	-0.020	-0.032
	(0.021)	(0.022)	(0.017)	(0.020)
MML	0.027	0.043**	0.051**	0.048**
	(0.020)	(0.017)	(0.020)	(0.020)
MML X NMML		-0.027**	-0.032***	-0.033**
		(0.011)	(0.010)	(0.015)
NMML		0.005	0.012	0.010
		(0.009)	(0.012)	(0.012)
R-squared	0.007	0.007	0.031	0.080
Mean outcome	3.796	3.796	3.798	3.818
Demographic and state controls	No	No	Yes	Yes
Other controls	No	No	No	Yes
Observations	887,972	887,972	871,216	730,929

Notes: Cluster-robust standard errors at the state-level are reported in parentheses. Statistical significance denoted by * p<0.1, ** p<0.05, *** p<0.01. All regressions control for year-quarter and state fixed effects, and state-specific linear time trends. Demographic and state controls include age, race/ethnicity, gender, marital status, presence of children in household, Medicaid expansion status, cigarette and beer excise tax rate. Other controls include education, health insurance coverage, household income relative to FPL, unemployment rate, and whether state legalized recreational marijuana.

3.5 Results

Table 1 reports the overall effects of any MML on self-assessed health score. Column 1 begins with a basic difference-in-differences specification, controlling only for year-quarter fixed effects, state fixed effects, and state-specific linear time trends. The estimated impact is positive but statistically insignificant form zero. While there does not appear to be significant pre-treatment effects, coefficients of the 5-year lead MML indictors have comparable magnitudes as the estimated post-treatment effect. Controlling for cross-border spill-overs in column 2, the estimated MML effect increases in magnitude and statistical significance. The coefficient of MML_{st} is interpreted as the effect of MML when no adjacent states have implemented MML. For a state with at least one adjacent MML state, the marginal effect of implementing own MML is the sum of the coefficients of MML_{st} and $MML_{st} \times NMML_{st}$. The negative coefficient estimate of $MML_{st} \times NMML_{st}$ suggests that MML's impact is attenuated when there is at least one adjacent state with MML, and the positive coefficient of $NMML_{st}$ indicates potential cross-border spill-overs on health. These border effects on general health are consistent with those found for mental health (Kalbfuß et al., 2018).

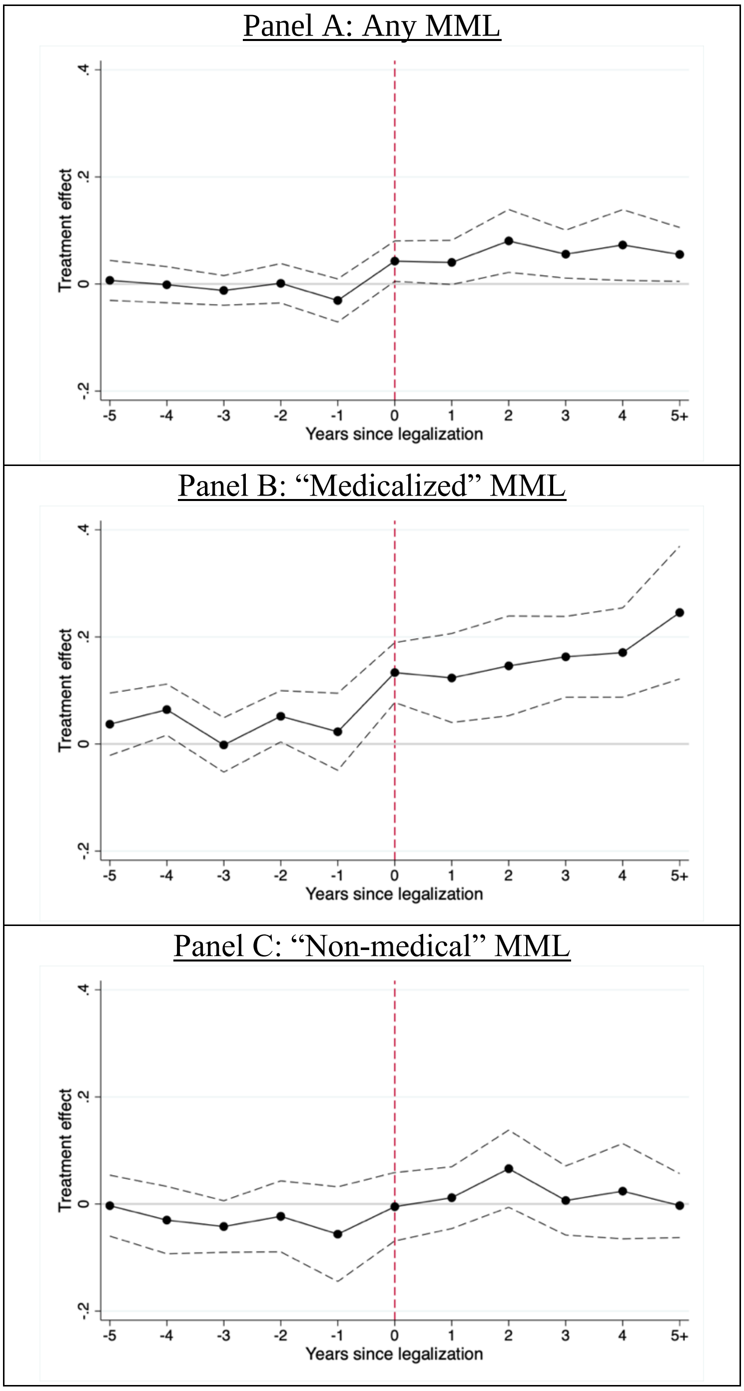
In column 3, I include demographic and state characteristics that are unlikely to be outcomes of MML. Subsequently, potential bad controls that may be directly affected by MML were added in column 4. Point estimates of both columns are similar, suggesting that MML is likely to be orthogonal to additional controls. In fact, estimates from columns 2 to 4 are not substantially different. This evidence, together with relatively smaller and insignificant pre-treatment effects, support the parallel trend assumption. The result from the full model implies that, in the absence MML in adjacent states, implementing own MML improves self-assessed health score by 0.048 (1.3% of the average score) and is statistically significant at the 5% level. If there is an adjacent state with MML, implementing own MML improves self-assessed health score by 0.015 (0.4% of the average score), but this is not statistically significant. There is also little evidence of spill-overs to adjacent states. The impact of at least one neighbouring state implementing MML are statistically insignificant: 0.010 (0.3% of the average score) for a non-MML state, and -0.023 (-0.6% of the average score) for an MML state.

I find similar impacts for days in bad physical health and days in bad mental health (see Appendix Tables 3 and 4). On average, MML reduces the number of days in bad physical health and bad mental health. Like Kalbfuß et al. (2018)., I find significant anticipatory effects on mental health up to 2 years prior to MML.

Estimates from the full model are robust to more flexible state-specific time trends and the exclusion of non-MML states. Regression results are reported in Appendix Table 5. Using state-specific quadratic trends produces a similar estimated effect: 0.061 increase in health score or 1.6% of the average score, suggesting that any residual unobserved heterogeneity is likely negligible. Omitting non-MML states is equivalent to an event study design. Regression on the restricted sample yield a similar increase of 0.039 or 1.0% of the average score. I also find estimated effects on days in bad physical health and days in bad mental health to be similarly robust (see Appendix Tables 6 and 7).

To more accurately characterize the impact of MML, I estimate dynamic treatment effects by replacing MML_{st} in the full regression specification with dummy variables for each year before and after the MML took effect. Effects for more than 5 years before MML form the baseline and are set to zero. Figure 1 plots the results by type of MML. Solid lines represent point estimates while dotted lines indicate the 95% confidence interval. Pre-MML effects are generally near zero for all panels, which supports causal interpretation. Panel A shows that the average health impact of any MML is slightly positive. Panels B and C reveal that this effect is largely driven by "medicalized" MML states. Health scores increase considerably following the implementation of "medicalized" MML and continue to rise up to 5 years post-MML. In contrast,

Figure 1: Dynamic impact of MML on self-assessed health score



Notes: Solid circles represent point estimates while dotted lines indicate the 95% confidence interval there is no discernible impact of “non-medical” MML with post-MML effects being much smaller in magnitude and statistically insignificant. Observed effects for “medicalized” MML do not appear to be predominantly driven by one state. Excluding any “medicalized” MML state from the analysis yields similar effects (see Appendix Figure 1). I also find similar results for days of bad physical and mental health (see Appendix Figures 2 and 3), but the difference between “medicalized” and “non-medical” MML are less obvious.

The absence of similar positive effects for “non-medical” MML may be due to offsetting effects between subgroups. For example, one group’s health score improves from access to medical marijuana, but another’s health score worsens due to non-medical use. Offsetting could also arise between reported health statuses. For example, lax regulations may improve the health status of relatively healthy individuals but worsen those who are relatively unhealthy. In both cases, average health scores can remain unchanged despite changes in the underlying distribution of health. In subsequent analyses, I explore potential impacts on the distribution of health.

Using the full model specification from Table 1, I analyse the impact of MML on 3 binary outcome variables which indicate whether an individual reported poor/fair, good, or very good/excellent health. Table 2 presents the results for different types of MML. Panel A show results of implementing any MML. A 2.6pp increase in the proportion with very good/excellent health matches a decrease in the proportion with good and poor or fair health. Panel B and C report the impact of “medicalized” and “non-medical” MML respectively. Like Figure 1, there is a stark difference between the 2 types of MML. “Medicalized” MML significantly reduces the proportion with poor/fair health

Table 2. Overall impact of MML on self-assessed health responses

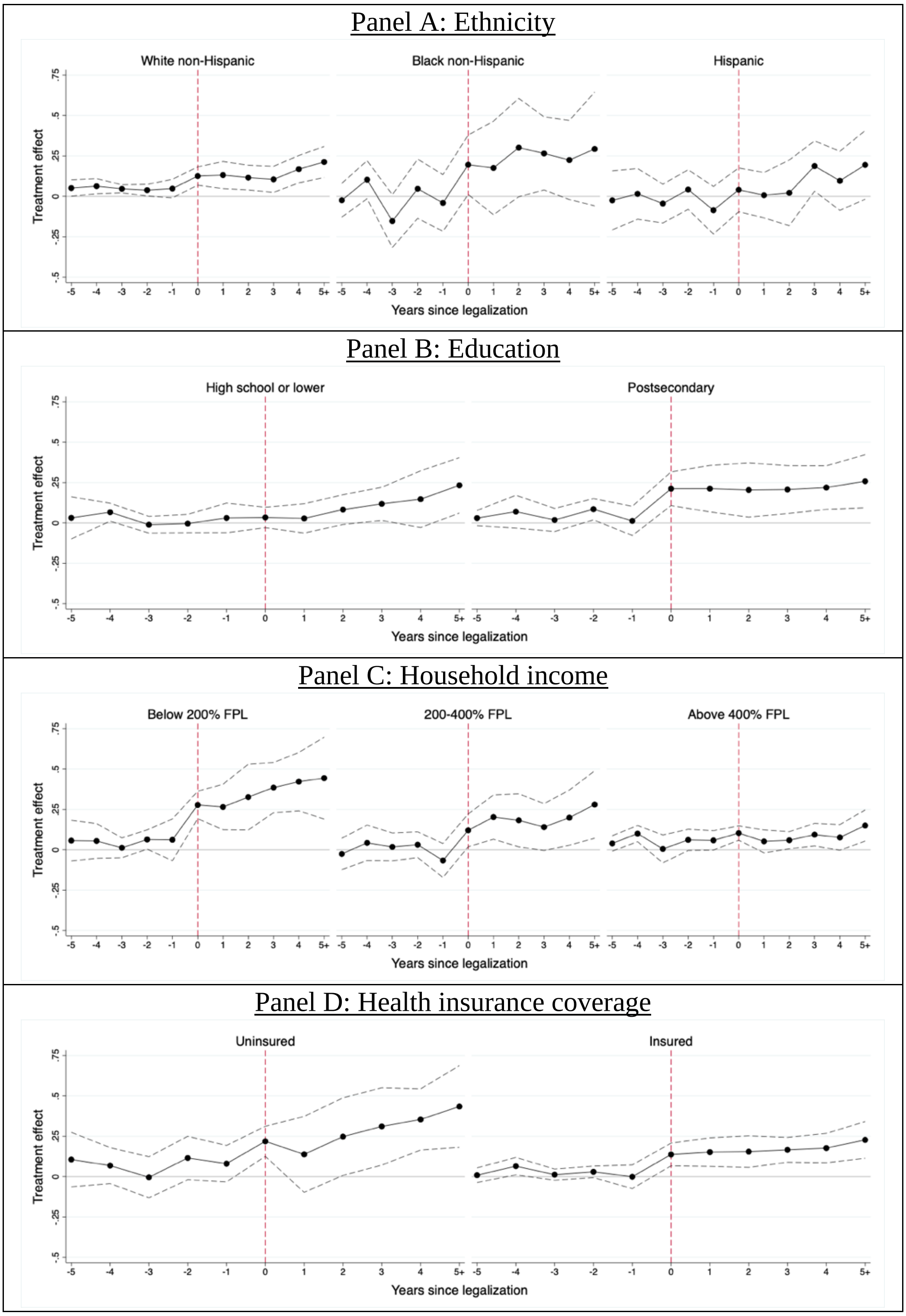
Dependent variable:	Poor/ Fair (1)	Good (2)	Very good/ Excellent (3)
Any MML	-0.010^{**}	-0.016^{*}	0.026^{**}
	(0.005)	(0.009)	(0.010)
Mean outcome	0.077	0.278	0.645
Observations	730929	730929	730929
Medicalized" MML	-0.032^{***}	0.020^{*}	0.013
	(0.008)	(0.010)	(0.010)
Mean outcome	0.075	0.273	0.652
Observations	415182	415182	415182
"Non-medical" MML	0.001	-0.026	0.026
	(0.007)	(0.021)	(0.023)
Mean outcome	0.079	0.285	0.636
Observations	461959	461959	461959

Notes: Cluster-robust standard errors at the state-level are reported in parentheses. Statistical significance denoted by * p<0.1, ** p<0.05, *** p<0.01. Estimates obtained from the model specification used in column 4 of Table 1.

by 3.2pp. In contrast, "non-medical" MML appears to have no significant impact on health. Both Table 2 and Figure 1 demonstrate the importance of accounting for differences in regulations when evaluating MMLs.

To investigate demographic heterogeneity of health impacts, I repeat the same analyses in Figure 1 for each subgroup of interest. Figure 2 displays the results for "medicalized" MML, with each subfigure generated from a separate regression. It is reassuring to find no significant pre-MML effects in all of them. Post-MML differences by ethnicity, education, household income and health insurance status are noteworthy. Point estimates for Blacks are relatively larger in magnitude than Whites and Hispanics but are less precise due to the smaller sample sizes. Individuals with postsecondary education experience a sharp improvement in health score immediately following MML implementation,

Figure 2: Impact of “medicalized” MML on self-assessed health score by demographic characteristics



Notes: Connected solid points represent point estimates while dotted lines indicate the 95% confidence interval.

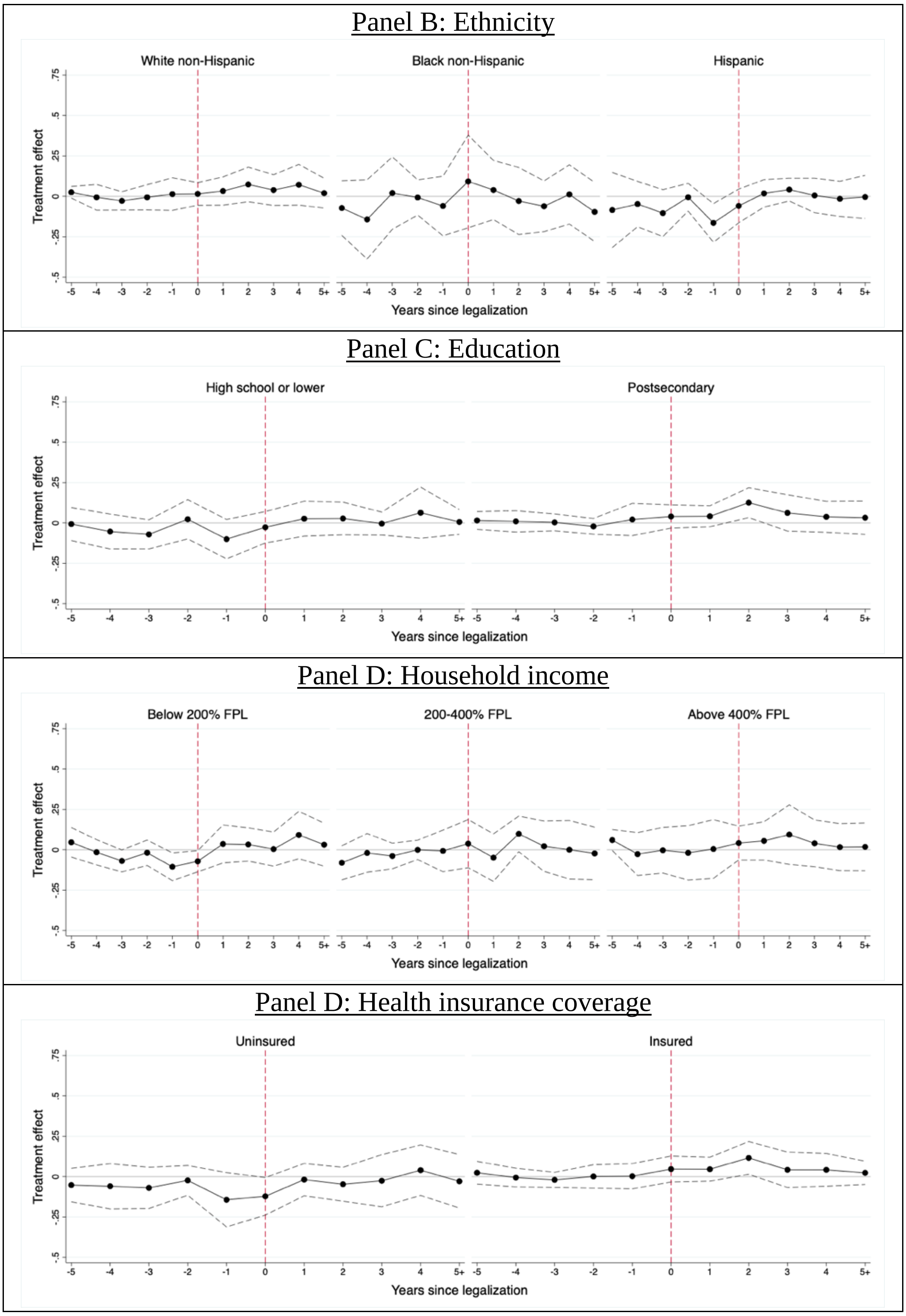
while improvements for those with high school or lower education are more gradual.

Individuals from the lowest-income households tend to have the largest improvements in health, while individuals from the top-income households exhibit relatively little effects on health. This pattern is consistent with the flat-of-the curve phenomenon found in healthcare expenditures. Wealthier and healthier individuals are likely to benefit less from medical marijuana compared to poorer and unhealthier individuals. It may also be because medical marijuana is an affordable substitute to prescription drugs (Bradford & Bradford, 2018), allowing low-income individuals who cannot afford prescription drugs to obtain some form of treatment. This mechanism is supported by Panel D, which shows that those without health insurance coverage experience larger health improvements than the insured.

Figure 3 displays the dynamic effects of "non-medical" MML. Apart from the subgroup with postsecondary education and the insured, health effects are virtually non-existent. Even for these exceptions, improvements in self-assessed health are much smaller than those found for "medicalized" MML. This comes as no surprise given findings from Figure 1. In fact, Figure 3 suggests that the absence of effects for "non-medical" MML is not a result of offsetting effects among subgroups.

Dynamic effects on days in bad physical health and days in bad mental health by demographic characteristics are also reported in Appendix Figures 4 to 7. Like overall health scores, improvements in physical health of Blacks, the low-income and the uninsured are relatively greater than other subgroups under

Figure 3: Impact of “non-medical” MML on self-assessed health score by demographic characteristics



Notes: Solid circles represent point estimates while dotted lines indicate the 95% confidence interval.

"medicalized" MML. Those with postsecondary education, the top income group, and the insured have relatively greater improvements in mental health under "medicalized" MML. On the other hand, there is no clear beneficial physical or mental health effects of "non-medical" MML. In fact, it may even worsen the physical health of Blacks and Hispanics.

Dynamic effects on days in bad physical health and days in bad mental health by demographic characteristics are also reported in Appendix Figures 4 to 7. Like overall health scores, improvements in physical health of Blacks, the low-income and the uninsured are relatively greater than other subgroups under "medicalized" MML. Those with postsecondary education, the top income group, and the insured have relatively greater improvements in mental health under "medicalized" MML. On the other hand, there is no clear beneficial physical or mental health effects of "non-medical" MML. In fact, it may even worsen the physical health of Blacks and Hispanics.

Lastly, I examine demographic heterogeneity over the distribution of health status following the implementation of different MML types. Table 3 reports the results by ethnicity, showing significant improvements under "medicalized" MML for Whites and Blacks, but not Hispanics. Relative to Whites, Blacks experience a larger reduction in poor or fair health, and a greater increase in very good or excellent health. Whites and Hispanics appear to be largely unaffected by "non-medical" MML, but Blacks are less likely to have good health (the middle response).

Table 3. Overall impact of MML on self-assessed health responses by ethnicity

Dependent variable:	Poor/ Fair	Good	Very good/ Excellent
	(1)	(2)	(3)
Panel A: White non-Hispanic			
"Medicalized" MML	-0.022***	0.012	0.010
	(0.007)	(0.011)	(0.012)
Mean outcome	0.061	0.250	0.689
Observations	294,318	294,318	294,318
"Non-medical" MML	-0.008	-0.021	0.028
	(0.007)	(0.022)	(0.024)
Mean outcome	0.064	0.259	0.677
Observations	321,267	321,267	321,267
Panel B: Black non-Hispanic			
"Medicalized" MML	-0.057***	-0.021	0.078*
	(0.020)	(0.035)	(0.039)
Mean outcome	0.101	0.315	0.584
Observations	56,249	56,249	56,249
"Non-medical" MML	0.007	-0.080***	0.073
	(0.036)	(0.025)	(0.053)
Mean outcome	0.105	0.320	0.575
Observations	44,647	44,647	44,647
Panel C: Hispanic			
"Medicalized" MML	-0.023	0.039	-0.017
	(0.024)	(0.026)	(0.030)
Mean outcome	0.137	0.362	0.501
Observations	42,301	42,301	42,301
"Non-medical" MML	0.022	-0.028	0.005
	(0.014)	(0.026)	(0.025)
Mean outcome	0.133	0.369	0.499
Observations	60,466	60,466	60,466

Notes: Cluster-robust standard errors at the state-level are reported in parentheses. Statistical significance denoted by * p<0.1, ** p<0.05, *** p<0.01. Estimates obtained from the model specification used in column 4 of Table 1.

Table 4. Overall impact of MML on self-assessed health responses by education

Dependent variable:	Poor/ Fair	Good	Very good/ Excellent
	(1)	(2)	(3)
Panel A: High school or lower			
"Medicalized" MML	-0.019*	0.094***	-0.075***
	(0.011)	(0.013)	(0.016)
Mean outcome	0.116	0.337	0.546
Observations	155,047	155,047	155,047
"Non-medical" MML	0.017**	-0.026	0.008
	(0.008)	(0.036)	(0.038)
Mean outcome	0.119	0.346	0.535
Observations	185,846	185,846	185,846
Panel B: Postsecondary			
"Medicalized" MML	-0.039***	-0.021*	0.060***
	(0.014)	(0.011)	(0.016)
Mean outcome	0.050	0.235	0.715
Observations	260,135	260,135	260,135
"Non-medical" MML	-0.015*	-0.028	0.042**
	(0.008)	(0.017)	(0.017)
Mean outcome	0.052	0.243	0.704
Observations	276,113	276,113	276,113

Notes: Cluster-robust standard errors at the state-level are reported in parentheses. Statistical significance denoted by * p<0.1, ** p<0.05, *** p<0.01. Estimates obtained from the model specification used in column 4 of Table 1.

Table 4 reports heterogeneity by educational attainment. Less educated individuals appear to be worse off under both types of MML. While "medicalized" MML increases the proportion in good health, it comes largely at the expense of a significant decrease in very good or excellent health. "Non-medical" MML is more detrimental, causing a significant increase in poor or fair health by 1.7pp or 14% relative to the average. In contrast, individuals with postsecondary education benefit from both types of MML, albeit more substantially under "medicalized" MML. This observation is consistent with the efficient producer hypothesis that more educated individuals are more efficient producers of health (Grossman, 1972).

Table 5. Overall impact of MML on self-assessed health responses by household income

Dependent variable:	Poor/ Fair	Good	Very good/ Excellent
	(1)	(2)	(3)
Panel A: < 200% FPL			
"Medicalized" MML	-0.042***	-0.001	0.043***
	(0.013)	(0.012)	(0.014)
Mean outcome	0.131	0.343	0.527
Observations	132,581	132,581	132,581
"Non-medical" MML	-0.006	0.035**	-0.029
	(0.012)	(0.016)	(0.019)
Mean outcome	0.130	0.349	0.521
Observations	164,471	164,471	164,471
Panel B: 200-400% FPL			
"Medicalized" MML	-0.042**	0.071***	-0.029
	(0.016)	(0.018)	(0.021)
Mean outcome	0.061	0.270	0.669
Observations	135,334	135,334	135,334
"Non-medical" MML	0.017**	-0.072**	0.055
	(0.008)	(0.033)	(0.038)
Mean outcome	0.061	0.275	0.664
Observations	156,090	156,090	156,090
Panel C: > 400% FPL			
"Medicalized" MML	-0.023**	-0.004	0.026*
	(0.008)	(0.016)	(0.014)
Mean outcome	0.038	0.213	0.749
Observations	147,267	147,267	147,267
"Non-medical" MML	-0.006	-0.030	0.036
	(0.011)	(0.027)	(0.030)
Mean outcome	0.040	0.221	0.739
Observations	141,398	141,398	141,398

Notes: Cluster-robust standard errors at the state-level are reported in parentheses. Statistical significance denoted by * p<0.1, ** p<0.05, *** p<0.01. Estimates obtained from the model specification used in column 4 of Table 1.

Table 5 reports heterogeneity by household income relative to the relevant FPL. Under “medicalized” MML, individuals from all income groups experience significant decline in poor or fair health, with larger effects for those below 400% FPL. For those in the bottom and top income groups, reductions in poor or fair health match similarly large and significant increases in very good or excellent health. But the reduction for those in the middle income group corresponds to increases in good health only. This distinction may stem from the fact that the middle income does not qualify for Medicaid and are more likely to be uninsured relative to the top and bottom income groups. Because it is difficult to obtain marijuana without a doctor’s recommendation under “medicalized” MML, this group has more restricted access to medical marijuana compared to the top and bottom income groups. Unfortunately, less restrictive regulation does not appear to be a solution due to potential negative consequences of self-medicating with marijuana. Under “non-medical” MML, the middle income group experiences a 1.7pp increase in poor or fair health and a 7.2pp decrease in good health. Only the top income group does not appear negatively affected by “non-medical” MML and, if anything, may still have some health gains.

Table 6 reports heterogeneity by health insurance status. Results for the uninsured are qualitatively similar to the middle income group in Table 5. “Medicalized” MML increases the proportion of the uninsured who are in very good or excellent health but “non-medical” MML increases the proportion of the uninsured in poor or fair health. Whereas, those with some form of insurance coverage benefit from “medicalized” MML and do not seem to be negatively

Table 6. Overall impact of MML on self-assessed health responses by health insurance coverage

Dependent variable:	Poor/ Fair	Good	Very good/ Excellent
	(1)	(2)	(3)
Panel A: Uninsured			
"Medicalized" MML	-0.040	-0.023	0.062**
	(0.024)	(0.015)	(0.023)
Mean outcome	0.125	0.345	0.530
Observations	85,612	85,612	85,612
"Non-medical" MML	0.025***	0.001	-0.026
	(0.009)	(0.027)	(0.030)
Mean outcome	0.125	0.351	0.524
Observations	104,442	104,442	104,442
Panel B: Insured			
"Medicalized" MML	-0.031**	0.027*	0.005
	(0.014)	(0.015)	(0.009)
Mean outcome	0.062	0.254	0.684
Observations	329,570	329,570	329,570
"Non-medical" MML	-0.008	-0.034*	0.042*
	(0.008)	(0.020)	(0.024)
Mean outcome	0.065	0.265	0.669
Observations	357,517	357,517	357,517

Notes: Cluster-robust standard errors at the state-level are reported in parentheses. Statistical significance denoted by * p<0.1, ** p<0.05, *** p<0.01. Estimates obtained from the model specification used in column 4 of Table 1.

impacted by "non-medical" MML. Estimates of Tables 2 to 6 are robust to a logit specification (see Appendix Tables 8 to 12).

3.6 Concluding Remarks

This study finds that medical marijuana laws (MMLs) which incorporate more medically oriented regulations improves self-assessed health of young adults relative to states with no MML. Subgroup analysis by demographic characteristics suggest these gains come mostly from Blacks, those from low-income households, postsecondary educated or the uninsured. However, states with less medically oriented regulations show no significant health gains in general. Additional analysis on the distribution of health statuses reveal that these lax laws may even be detrimental among those more likely to be in poor health, such as the less educated and the uninsured.

These findings highlight the important role of government regulations in containing the current opioid epidemic devasting the United States, and for avoiding a similar crisis with marijuana. Both opioids and marijuana offer potential therapeutic benefits when used in a controlled manner, consistent with established medical principles. Evidence from this study point towards the need for stricter regulations concerning supply and access. Drawing parallels to how the opioid crisis developed, state government may also do well to err on the side of caution when legalizing medical marijuana. We currently have little comprehension of medical marijuana's propensity for misuse and addiction, or its long-term effects. If the rate of marijuana liberalisation outpaces understanding its risks, the United States may be in danger of another costly health crisis.

3.7 References

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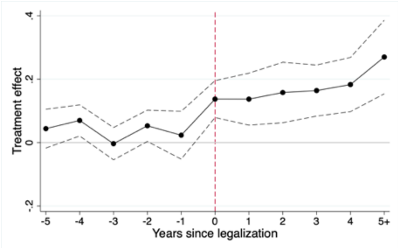
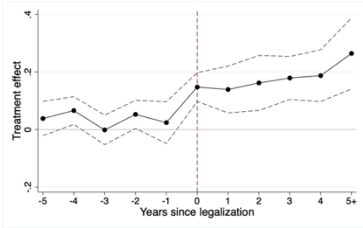
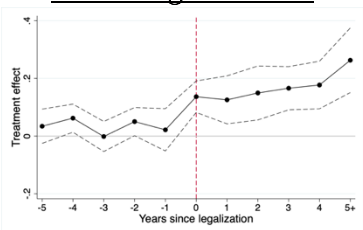
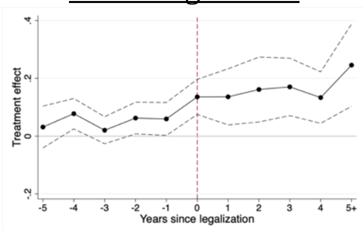
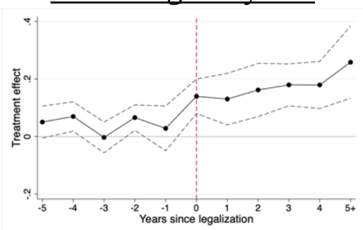
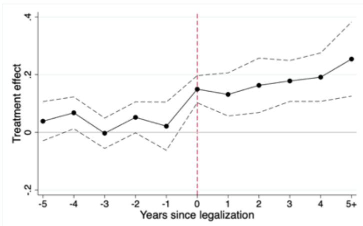
3.8 Appendix

Appendix Figure 1: Impact of "Medicalized" MML on self-assessed health score

Excluding Connecticut	Excluding D.C.
	
Excluding Delaware	Excluding Illinois
	
Excluding Maryland	Exclude Massachusetts
	
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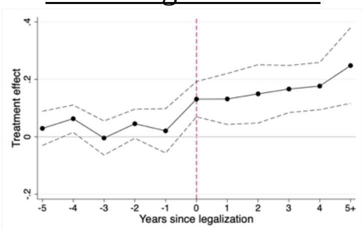
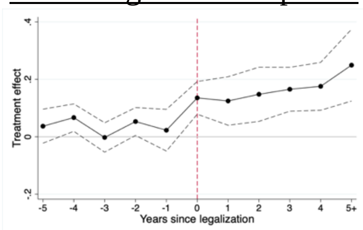
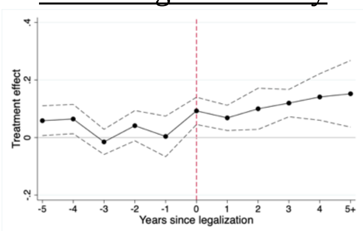
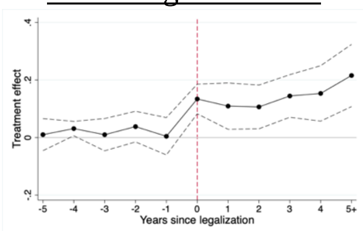
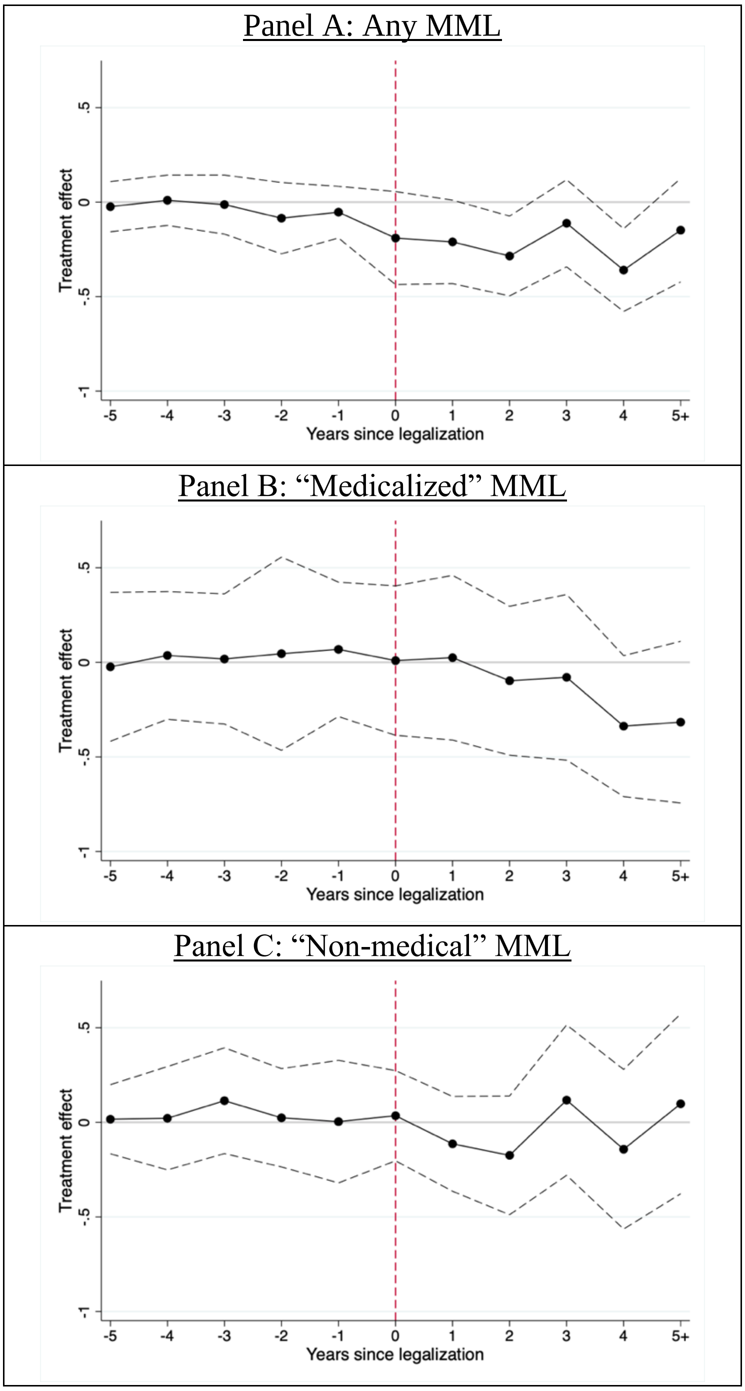
	
Excluding New Jersey	Excluding New York
	

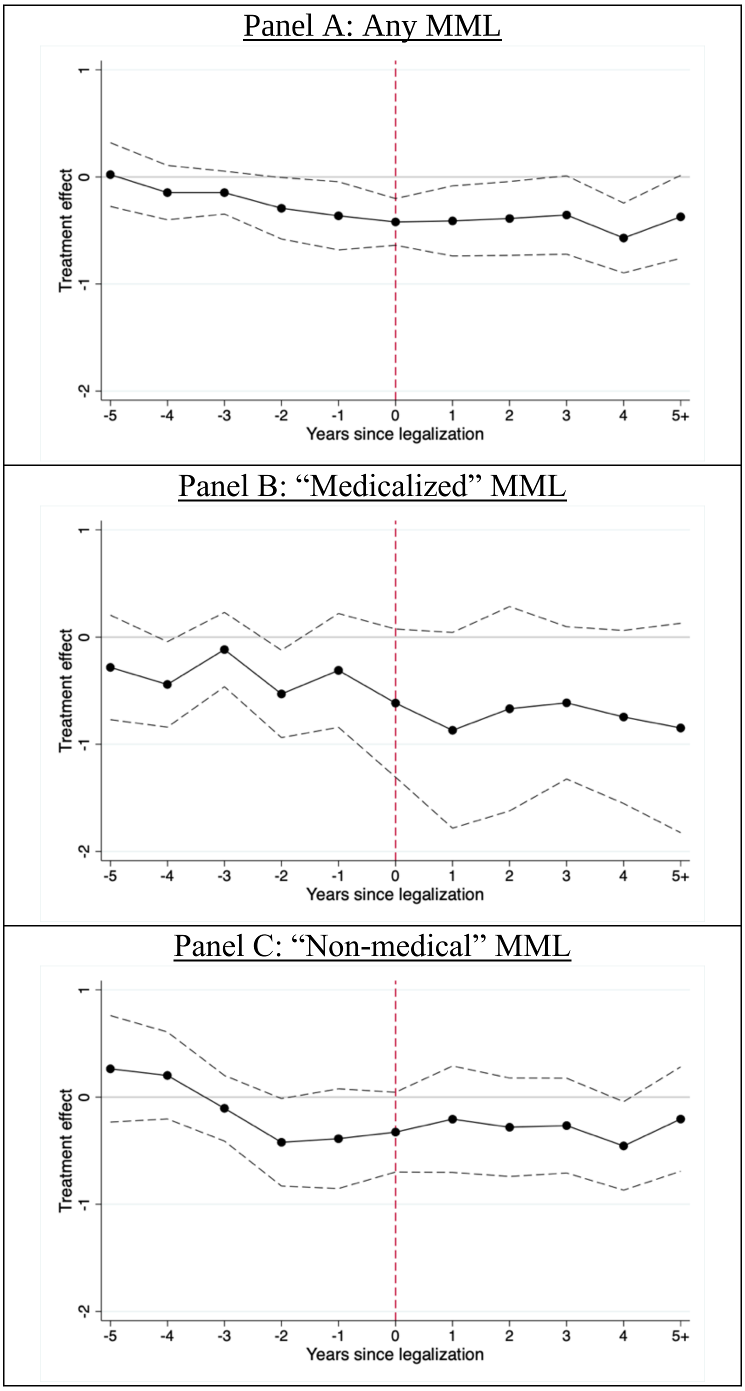
Notes: Solid circles represent point estimates while dotted lines indicate the 95% confidence interval.

Appendix Figure 2: Impact of MML on days in bad physical health



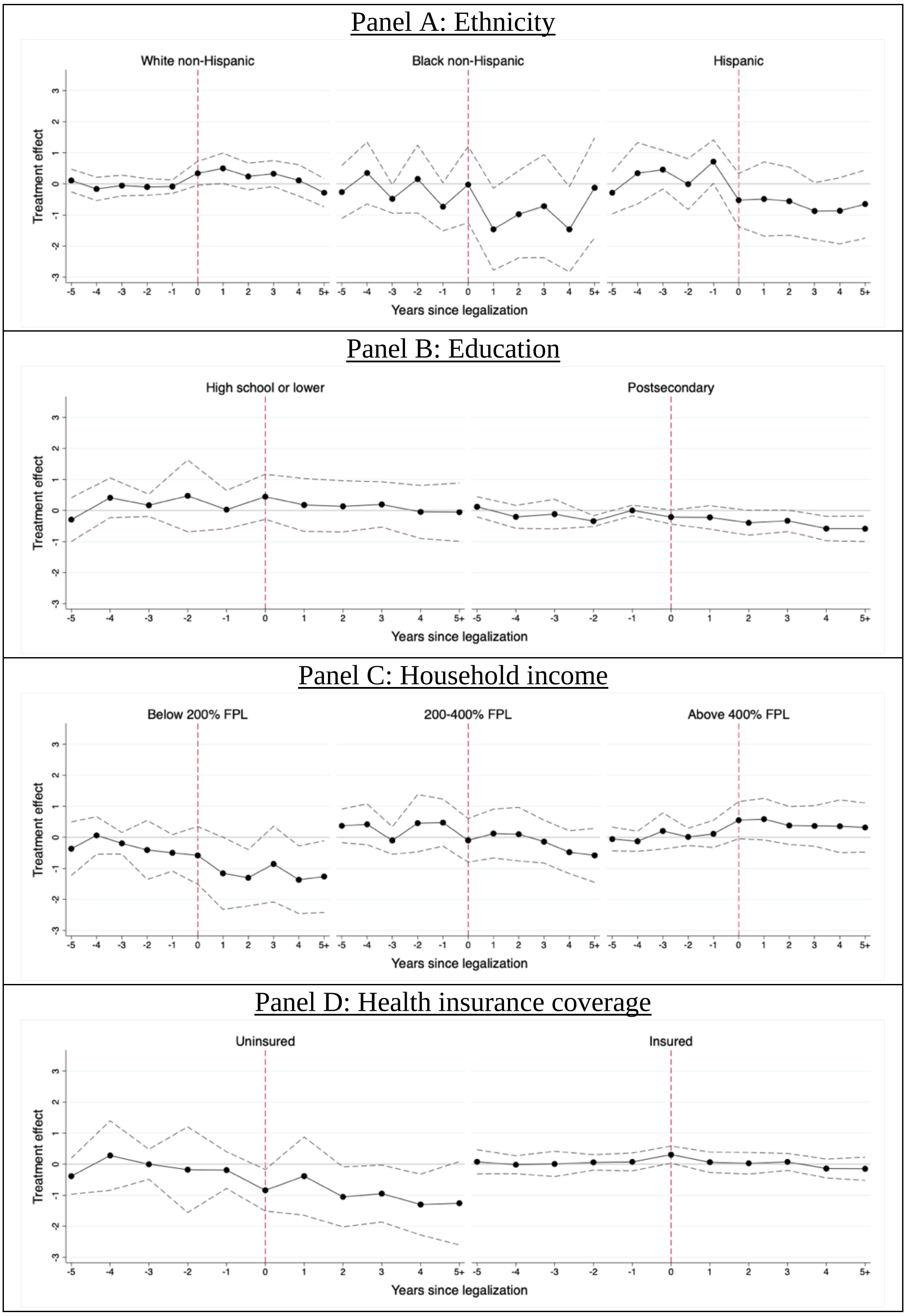
Notes: Solid circles represent point estimates while dotted lines indicate the 95% confidence interval.

Appendix Figure 3: Impact of MML on days in bad mental health



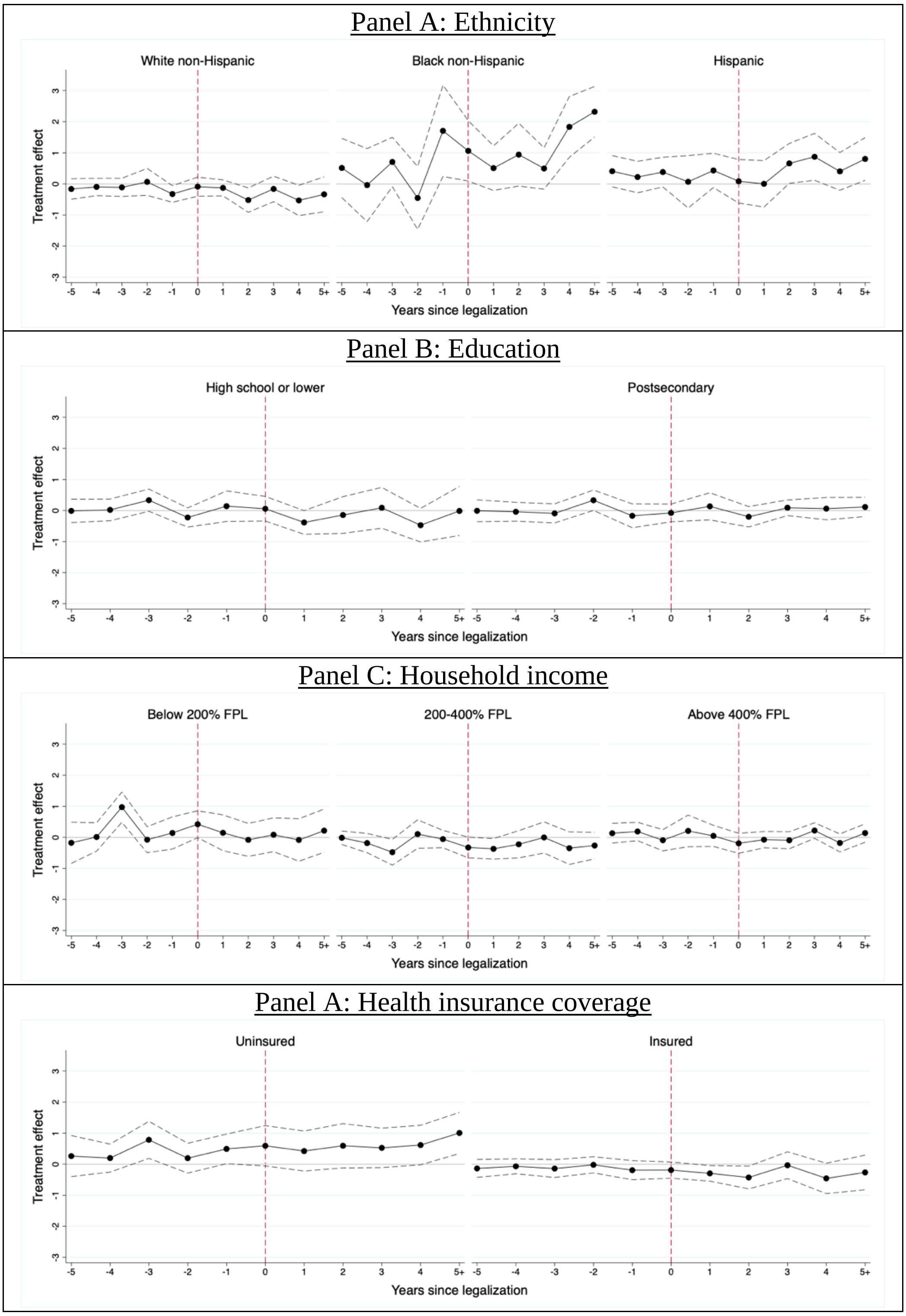
Notes: Solid circles represent point estimates while dotted lines indicate the 95% confidence interval.

Appendix Figure 4: Impact of "medicalized" MML on days in bad physical health by demographic characteristics



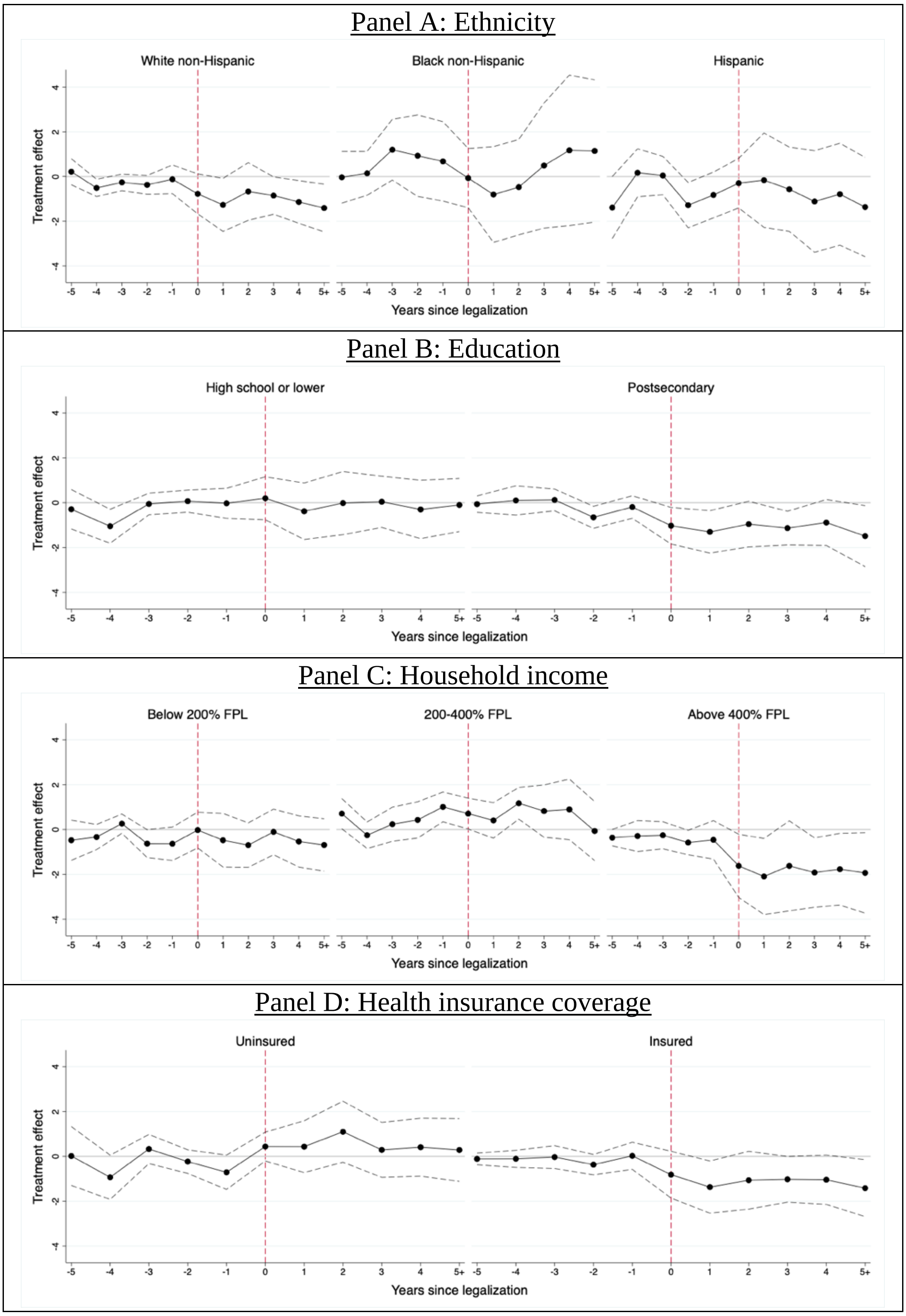
Notes: Solid circles represent point estimates while dotted lines indicate the 95% confidence interval.

Appendix Figure 5: Impact of “non-medical” MML on days in bad physical health by demographic characteristics



Notes: Solid circles represent point estimates while dotted lines indicate the 95% confidence interval.

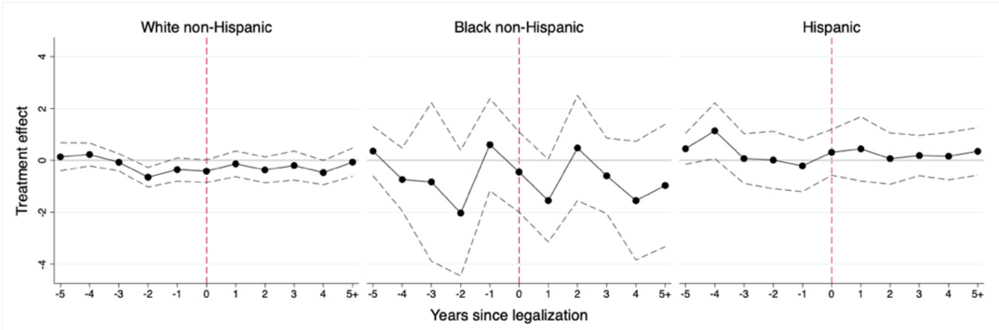
Appendix Figure 6: Impact of "medicalized" MML on days in bad mental health by demographic characteristics



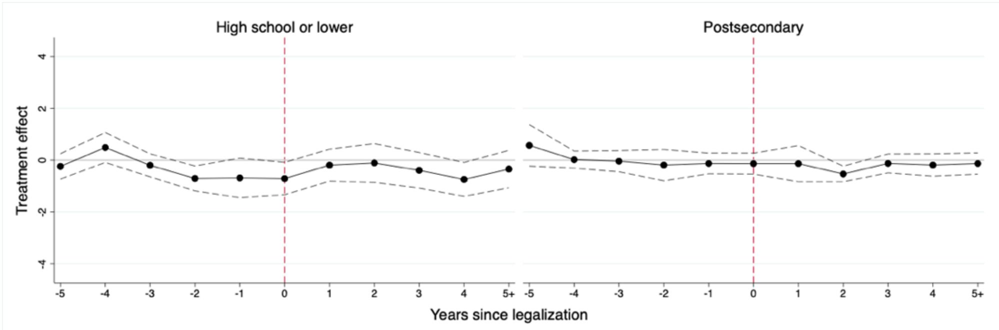
Notes: Solid circles represent point estimates while dotted lines indicate the 95% confidence interval.

Appendix Figure 7: Impact of “non-medical” MML on days in bad mental health by demographic characteristics

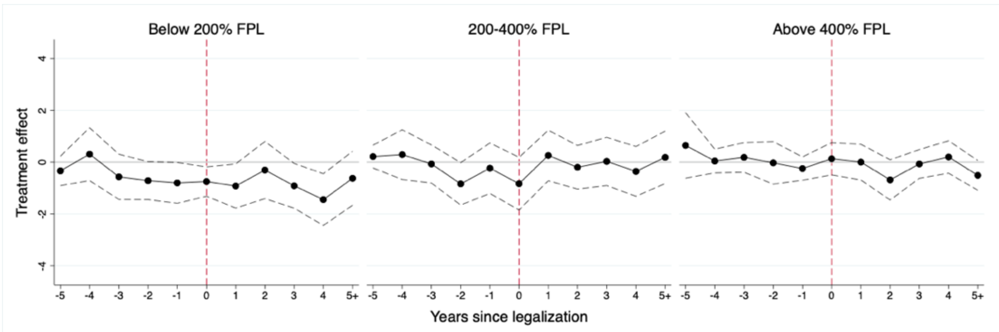
Panel A: Ethnicity



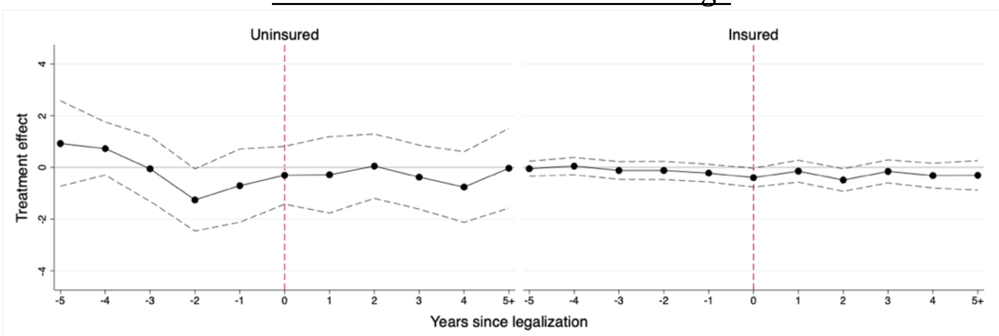
Panel B: Education



Panel C: Household income



Panel D: Health insurance coverage



Notes: Solid circles represent point estimates while dotted lines indicate the 95% confidence interval.

Appendix Table 1. Effective marijuana legalization dates

State	Date of medical marijuana legalization (1)	Date of recreational marijuana legalization (2)
Alaska	4 Mar 1999	24 Feb 2015
Arizona	10 Dec 2010	-
Arkansas	9 Nov 2016	-
California	6 Nov 1996	9 Nov 2016
Colorado	30 Jul 2001	10 Dec 2012
Connecticut	1 Oct 2012	-
Delaware	1 Jul 2011	-
District of Columbia	27 Jul 2010	26 Feb 2015
Florida	3 Jan 2017	-
Hawaii	14 Jul 2000	-
Illinois	1 Jan 2014	-
Maine	22 Dec 1999	30 Jan 2017
Maryland	1 Jun 2014	-
Massachusetts	1 Jan 2013	15 Dec 2016
Michigan	4 Dec 2008	6 Dec 2018
Minnesota	30 May 2014	-
Missouri	6 Dec 2018	-
Montana	2 Nov 2004	-
Nevada	1 Oct 2001	1 Jan 2017
New Hampshire	23 Jul 2013	-
New Jersey	10 Oct 2010	-
New Mexico	1 Jul 2007	-
New York	5 Jul 2014	-
North Dakota	8 Dec 2016	-
Ohio	8 Sep 2016	-
Oklahoma	25 Aug 2018	-
Oregon	3 Dec 1998	1 Jul 2015
Pennsylvania	17 May 2016	-
Rhode Island	3 Jan 2006	-
Utah	1 Dec 2018	-
Vermont	1 Jul 2004	1 Jul 2018
Washington	3 Nov 1998	9 Dec 2012

Sources: Marijuana Policy Project. (2015). State-by-State Medical Marijuana Laws; Marijuana laws in the United States. Ballotpedia: The encyclopedia of American Politics.

Appendix Table 2. Summary of respondent characteristics, 1993-2018

	No MML	Any MML	"Medicalized" MML	"Non-medical" MML
	(1)	(2)	(3)	(4)
Age	23.4	23.4	23.5	23.4
	(0.031)	(0.027)	(0.035)	(0.052)
Male	0.514	0.516	0.511	0.523
	(0.001)	(0.002)	(0.003)	(0.002)
White non-Hispanic	0.633	0.599	0.612	0.515
	(0.054)	(0.053)	(0.033)	(0.087)
Black non-Hispanic	0.166	0.099	0.136	0.056
	(0.029)	(0.015)	(0.019)	(0.013)
Hispanic	0.153	0.212	0.165	0.314
	(0.066)	(0.052)	(0.021)	(0.080)
Married	0.366	0.330	0.291	0.349
	(0.010)	(0.008)	(0.006)	(0.008)
Childless	0.524	0.547	0.587	0.518
	(0.013)	(0.012)	(0.010)	(0.014)
High school or lower	0.479	0.451	0.405	0.474
	(0.008)	(0.009)	(0.007)	(0.008)
Below 200% FPL	0.338	0.325	0.264	0.373
	(0.013)	(0.021)	(0.016)	(0.024)
200-400% FPL	0.333	0.291	0.275	0.278
	(0.007)	(0.013)	(0.006)	(0.023)
Above 400% FPL	0.329	0.385	0.460	0.349
	(0.010)	(0.013)	(0.018)	(0.007)
Health insurance coverage	0.722	0.761	0.793	0.747
	(0.026)	(0.011)	(0.014)	(0.009)
Number of states	19	32	10	14
Observations	327,532	560,451	178,481	231,826

Notes: Cluster-robust standard errors at the state-level are reported in parentheses. States are classified as “Any MML” if they have an effective MML as of 2018. States with an MML in effect as of 2015 are further classified as “medicalized” or “non-medical” are based on Williams et al. (2016).

Appendix Table 3. Overall impact of any MML on days in bad physical health

	(1)	(2)	(3)	(4)
5 years prior	-0.043	-0.027	-0.033	-0.028
	(0.061)	(0.060)	(0.062)	(0.068)
4 years prior	-0.028	-0.015	-0.022	0.008
	(0.068)	(0.070)	(0.074)	(0.067)
3 years prior	-0.085	-0.066	-0.096	-0.017
	(0.074)	(0.070)	(0.074)	(0.079)
2 years prior	-0.001	0.020	-0.010	-0.089
	(0.079)	(0.076)	(0.079)	(0.095)
1 years prior	-0.091	-0.066	-0.074	-0.058
	(0.062)	(0.061)	(0.067)	(0.071)
MML	-0.104	-0.234**	-0.226**	-0.231**
	(0.069)	(0.105)	(0.107)	(0.105)
MML X NMML		0.219*	0.199*	0.237**
		(0.112)	(0.108)	(0.114)
NMML		-0.033	-0.024	-0.032
		(0.057)	(0.056)	(0.057)
R-squared	0.003	0.003	0.007	0.015
Mean outcome	2.160	2.160	2.155	2.141
Demographic and state controls	No	No	Yes	Yes
Other controls	No	No	No	Yes
Observations	859,386	859,386	843,275	708,649

Notes: Cluster-robust standard errors at the state-level are reported in parentheses. Statistical significance denoted by * p<0.1, ** p<0.05, *** p<0.01. All regressions control for year-quarter and state fixed effects, and state-specific linear time trends. Demographic and state controls include age, race/ethnicity, gender, marital status, presence of children in household, Medicaid expansion status, cigarette and beer excise tax rate. Other controls include education, health insurance coverage, household income relative to FPL, unemployment rate, and whether state legalized recreational marijuana.

Appendix Table 4. Overall impact of any MML on days in bad mental health

	(1)	(2)	(3)	(4)
5 years prior	-0.072	-0.057	-0.074	0.020
	(0.132)	(0.140)	(0.148)	(0.153)
4 years prior	-0.152	-0.141	-0.203	-0.148
	(0.113)	(0.115)	(0.125)	(0.128)
3 years prior	-0.136	-0.125	-0.182	-0.149
	(0.122)	(0.126)	(0.132)	(0.102)
2 years prior	-0.310**	-0.300**	-0.322**	-0.295*
	(0.130)	(0.139)	(0.144)	(0.148)
1 years prior	-0.313**	-0.301**	-0.377***	-0.367**
	(0.123)	(0.129)	(0.136)	(0.165)
MML	-0.354**	-0.354***	-0.406***	-0.431***
	(0.135)	(0.129)	(0.133)	(0.131)
MML X NMML		0.031	0.052	0.027
		(0.117)	(0.115)	(0.102)
NMML		-0.062	-0.078	-0.094
		(0.131)	(0.119)	(0.101)
R-squared	0.008	0.008	0.019	0.030
Mean outcome	4.155	4.155	4.147	4.148
Demographic and state controls	No	No	Yes	Yes
Other controls	No	No	No	Yes
Observations	859,060	859,060	842,946	708,308

Notes: Cluster-robust standard errors at the state-level are reported in parentheses. Statistical significance denoted by * p<0.1, ** p<0.05, *** p<0.01. All regressions control for year-quarter and state fixed effects, and state-specific linear time trends. Demographic and state controls include age, race/ethnicity, gender, marital status, presence of children in household, Medicaid expansion status, cigarette and beer excise tax rate. Other controls include education, health insurance coverage, household income relative to FPL, unemployment rate, and whether state legalized recreational marijuana.

Appendix Table 5. Robustness checks for the impact of any MML on self-assessed health score

	(1)	(2)
5 years prior	0.009	-0.001
	(0.019)	(0.020)
4 years prior	0.006	-0.014
	(0.019)	(0.019)
3 years prior	-0.002	-0.023
	(0.022)	(0.019)
2 years prior	0.011	-0.012
	(0.024)	(0.022)
1 years prior	-0.020	-0.044*
	(0.027)	(0.024)
MML	0.061**	0.039*
	(0.030)	(0.019)
NMML	0.013	0.023
	(0.016)	(0.017)
MML X NMML	-0.040**	-0.045**
	(0.016)	(0.017)
R-squared	0.080	0.082
Mean outcome	3.818	3.821
State-specific trend	Quadratic	Linear
Sample	All states	MML states
Observations	730,929	447,078

Notes: Cluster-robust standard errors at the state-level are reported in parentheses. Statistical significance denoted by * p<0.1, ** p<0.05, *** p<0.01. Controls include state and year-quarter fixed effects, state-specific quadratic time trends, age, race/ethnicity, gender, education, marital status, presence of children in household, Medicaid expansion status, unemployment rate beer excise tax rate, health insurance coverage, household income relative to FPL, and whether state legalized recreational marijuana.

Appendix Table 6. Robustness checks for the impact of any MML on days in bad physical health

	(1)	(2)
5 years prior	-0.027	-0.008
	(0.073)	(0.070)
4 years prior	0.018	0.045
	(0.080)	(0.081)
3 years prior	-0.016	0.014
	(0.097)	(0.077)
2 years prior	-0.089	-0.039
	(0.127)	(0.099)
1 years prior	-0.055	0.005
	(0.116)	(0.081)
MML	-0.231	-0.180**
	(0.152)	(0.079)
NMML	-0.046	-0.089
	(0.067)	(0.081)
MML X NMML	0.256**	0.283**
	(0.113)	(0.108)
R-squared	0.015	0.014
Mean outcome	2.141	2.222
State-specific trend	Quadratic	Linear
Sample	All states	MML states
Observations	708,649	433,522

Notes: Cluster-robust standard errors at the state-level are reported in parentheses. Statistical significance denoted by * p<0.1, ** p<0.05, *** p<0.01. Controls include state and year-quarter fixed effects, state-specific quadratic time trends, age, race/ethnicity, gender, education, marital status, presence of children in household, Medicaid expansion status, unemployment rate beer excise tax rate, health insurance coverage, household income relative to FPL, and whether state legalized recreational marijuana.

Appendix Table 7. Robustness checks for the impact of any MML on days in bad mental health

	(1)	(2)
5 years prior	0.015	0.049
	(0.155)	(0.135)
4 years prior	-0.109	-0.135
	(0.132)	(0.113)
3 years prior	-0.113	-0.135
	(0.139)	(0.124)
2 years prior	-0.258	-0.226
	(0.168)	(0.135)
1 years prior	-0.315	-0.317*
	(0.219)	(0.179)
MML	-0.360**	-0.366**
	(0.177)	(0.153)
NMML	-0.051	-0.216*
	(0.116)	(0.123)
MML X NMML	0.043	0.091
	(0.127)	(0.112)
R-squared	0.030	0.028
Mean outcome	4.148	4.235
State-specific trend	Quadratic	Linear
Sample	All states	MML states
Observations	708,308	433,313

Notes: Cluster-robust standard errors at the state-level are reported in parentheses. Statistical significance denoted by * p<0.1, ** p<0.05, *** p<0.01. Controls include state and year-quarter fixed effects, state-specific quadratic time trends, age, race/ethnicity, gender, education, marital status, presence of children in household, Medicaid expansion status, unemployment rate beer excise tax rate, health insurance coverage, household income relative to FPL, and whether state legalized recreational marijuana.

Appendix Table 8. Logit marginal effects of MML on self-assessed health responses

Dependent variable:	Poor/ Fair	Good	Very good/ Excellent
	(1)	(2)	(3)
Panel A: Effect of any MML			
Any MML	-0.011**	-0.016*	0.026***
	(0.005)	(0.009)	(0.010)
Mean outcome	0.077	0.278	0.645
Observations	730,929	730,929	730,929
Panel B: Effect of "medicalized" MML			
Medicalized" MML	-0.034***	0.019*	0.014
	(0.007)	(0.010)	(0.010)
Mean outcome	0.075	0.273	0.651
Observations	415,182	415,182	415,182
Panel C: Effect of "non-medical" MML			
"Non-medical" MML	0.004	-0.027	0.023
	(0.006)	(0.019)	(0.021)
Mean outcome	0.079	0.285	0.636
Observations	461,959	461,959	461,959

Notes: Cluster-robust standard errors at the state-level are reported in parentheses. Statistical significance denoted by * p<0.1, ** p<0.05, *** p<0.01. Estimates obtained from the model specification used in column 4 of Table 1. Panel A uses all states. Panel B uses non-MML states and states with "medicalized" MML. Panel C uses non-MML states and states with "non-medical" MML.

Appendix Table 9. Logit marginal effects of MML on self-assessed health responses by ethnicity

	Poor/ Fair	Good	Very good/ Excellent
	(1)	(2)	(3)
Panel A: White non-Hispanic			
"Medicalized" MML	-0.027***	0.011	0.014
	(0.009)	(0.012)	(0.012)
Mean outcome	0.061	0.250	0.689
Observations	294,318	294,318	294,318
"Non-medical" MML	-0.005	-0.024	0.029
	(0.007)	(0.020)	(0.023)
Mean outcome	0.064	0.259	0.677
Observations	321,267	321,267	321,267
Panel B: Black non-Hispanic			
"Medicalized" MML	-0.067***	-0.012	0.079*
	(0.018)	(0.034)	(0.043)
Mean outcome	0.101	0.315	0.584
Observations	56,249	56,249	56,249
"Non-medical" MML	0.006	-0.087***	0.077
	(0.034)	(0.029)	(0.054)
Mean outcome	0.105	0.320	0.575
Observations	44,647	44,647	44,647
Panel C: Hispanic			
"Medicalized" MML	-0.015	0.049**	-0.033
	(0.027)	(0.022)	(0.031)
Mean outcome	0.137	0.362	0.501
Observations	42,301	42,301	42,301
"Non-medical" MML	0.015	-0.025	0.008
	(0.014)	(0.026)	(0.024)
Mean outcome	0.133	0.369	0.499
Observations	60,466	60,466	60,466

Notes: Cluster-robust standard errors at the state-level are reported in parentheses. Statistical significance denoted by * p<0.1, ** p<0.05, *** p<0.01. Estimates obtained from the model specification used in column 4 of Table 1.

Appendix Table 10. Logit marginal effects of MML on self-assessed health responses by education

	Poor/ Fair	Good	Very good/ Excellent
	(1)	(2)	(3)
Panel A: High school or lower			
“Medicalized” MML	-0.023**	0.086***	-0.067***
	(0.009)	(0.015)	(0.017)
Mean outcome	0.116	0.337	0.546
Observations	155,047	155,047	155,047
“Non-medical” MML	0.021**	-0.020	-0.001
	(0.008)	(0.032)	(0.033)
Mean outcome	0.119	0.346	0.535
Observations	185,846	185,846	185,846
Panel B: Postsecondary			
“Medicalized” MML	-0.044***	-0.022**	0.060***
	(0.014)	(0.010)	(0.015)
Mean outcome	0.050	0.235	0.715
Observations	260,135	260,135	260,135
“Non-medical” MML	-0.014*	-0.034**	0.047***
	(0.008)	(0.016)	(0.017)
Mean outcome	0.052	0.243	0.704
Observations	276,113	276,113	276,113

Notes: Cluster-robust standard errors at the state-level are reported in parentheses. Statistical significance denoted by * p<0.1, ** p<0.05, *** p<0.01. Estimates obtained from the model specification used in column 4 of Table 1.

Appendix Table 11. Logit marginal effects of MML on self-assessed health responses by household income

	Poor/ Fair	Good	Very good/ Excellent
	(1)	(2)	(3)
Panel A: < 200% FPL			
"Medicalized" MML	-0.036***	-0.002	0.043***
	(0.011)	(0.013)	(0.012)
Mean outcome	0.131	0.343	0.527
Observations	132,581	132,581	132,581
"Non-medical" MML	-0.004	0.036**	-0.034
	(0.014)	(0.017)	(0.021)
Mean outcome	0.130	0.349	0.521
Observations	164,471	164,471	164,471
Panel B: 200-400% FPL			
"Medicalized" MML	-0.044***	0.068***	-0.030
	(0.016)	(0.018)	(0.023)
Mean outcome	0.061	0.270	0.669
Observations	135,334	135,334	135,334
"Non-medical" MML	0.017**	-0.070**	0.054
	(0.008)	(0.030)	(0.035)
Mean outcome	0.061	0.275	0.664
Observations	156,090	156,090	156,090
Panel C: > 400% FPL			
"Medicalized" MML	-0.024***	-0.004	0.028**
	(0.007)	(0.016)	(0.014)
Mean outcome	0.038	0.213	0.749
Observations	147,267	147,267	147,267
"Non-medical" MML	-0.002	-0.031	0.035
	(0.012)	(0.025)	(0.027)
Mean outcome	0.040	0.221	0.739
Observations	141,398	141,398	141,398

Notes: Cluster-robust standard errors at the state-level are reported in parentheses. Statistical significance denoted by * p<0.1, ** p<0.05, *** p<0.01. Estimates obtained from the model specification used in column 4 of Table 1.

Appendix Table 12. Logit marginal effects of MML on self-assessed health responses by health insurance coverage

	Poor/ Fair (1)	Good (2)	Very good/ Excellent (3)
Panel A: Uninsured			
"Medicalized" MML	-0.041**	-0.030	0.065**
	(0.019)	(0.019)	(0.028)
Mean outcome	0.125	0.345	0.530
Observations	85,612	85,612	85,612
"Non-medical" MML	0.023**	-0.005	-0.018
	(0.009)	(0.028)	(0.030)
Mean outcome	0.125	0.351	0.524
Observations	104,442	104,442	104,442
Panel B: Insured			
"Medicalized" MML	-0.038**	0.027*	0.005
	(0.015)	(0.014)	(0.010)
Mean outcome	0.062	0.254	0.684
Observations	329,570	329,570	329,570
"Non-medical" MML	-0.003	-0.032*	0.036*
	(0.008)	(0.017)	(0.020)
Mean outcome	0.065	0.265	0.669
Observations	357,517	357,517	357,517

Notes: Cluster-robust standard errors at the state-level are reported in parentheses. Statistical significance denoted by * p<0.1, ** p<0.05, *** p<0.01. Estimates obtained from the model specification used in column 4 of Table 1.