Essays on labor economics and political economy

by

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Abstract

One explanation for why conscription may raise wages is that conscripts accumulate valuable human capital during their service in the military. An alternative theory posits that conscription affects labor market outcomes through the extensive margin. The first chapter examines those possible mechanisms by estimating the impact of the duration of military service on income, employment, and educational attainment with data from South Korea and an IV approach. While the amount of human capital increases with the duration of service, there is no effect on the extensive margin. Although I find a conscription premium: a positive relationship between conscription and income, the length of compulsory military service does not affect a worker's income or employment. On the contrary, I find that longer service length lowers conscripts' chances of obtaining post-graduate degrees suggesting a net human capital depreciation. My findings support the case that conscription primarily affects labor market outcomes through the extensive margin.

In the second chapter, I use the different timing of the adoption of anti-discrimination legislation in Japan, which bans the identification of the Burakumins, to examine the effect of uncertainty on divorces with a difference-in-differences approach. I show that an increase in such uncertainty has heterogeneous effects on divorces. I find that an increase in uncertainty increases divorces in places where the information is less important and decreases divorces when that information is more important.

The third chapter examines whether pharmaceutical companies directly lobbied U.S. state lawmakers for their votes on COVID vaccine legislations. I find that state legislators who received campaign contributions from pharmaceutical companies are not more likely to vote in favor of those companies on the vaccine mandates. Prior field experiments have shown that campaign contributions buy access, and the U.S. pharmaceutical industry has directly influenced key state referenda on drug pricing and regulation reforms with its political contributions. While Democrats have mainly voted for vaccine mandates, it can be quite costly for Republicans to do the same. Journalists find that the industry remained largely in the background of the controversial vaccine mandate policies and relied primarily on third-party organizations to advance its agenda. The findings of this paper seem to provide evidence for this observation.

Keywords: Conscription premium; extensive margin; Divorce; Uncertainty; Legislative voting; COVID

Dedication

In loving memory of my father, whose wisdom, strength, and unwavering support continue to guide me even though he is no longer physically present. His enduring legacy lives on in every accomplishment, and I am forever grateful for the values he instilled in me.

To my dearest mother, whose boundless love, sacrifice, and encouragement have been my rock. Her unwavering belief in my potential has fueled my journey, and I dedicate this work to her. Her strength and support have been the driving force behind my accomplishments, and I am profoundly grateful for her endless love.

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I am profoundly grateful to my parents for their enduring encouragement, love, and belief in my abilities. Their unwavering support has been my anchor, providing strength and motivation during the challenging moments of this academic pursuit.

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This accomplishment is a reflection not only of my efforts but of the guidance, encouragement, and support I have received from my supervisory committee and family. Thank you for being the pillars of strength on this academic journey.

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Chapter 1

Conscription: Extensive vs Intensive Margin?

1.1 Introduction

A large body of literature, such as Lim (2018) and Lee and Jung (2020), studies the effect of conscription on labor market outcomes and educational attainment. They find conscription positively affects those outcomes, but few examine the mechanism behind such an effect. Similar to a college degree, on the intensive margin, finishing compulsory military service (CMS) can help conscripts accumulate valuable human capital. On the extensive margin, conscription can be a signaling or networking device, it can also confer a sense of belonging among other functions. There seems to be no consensus on the nature of higher education (Huntington-Klein, 2021). In this paper, I study which mechanism is mainly behind conscription's effect on a veteran's labor market outcomes and educational attainment. To study the issue, I use an IV approach and a rich dataset from South Korea to determine if the duration of CMS affects an individual's income, employment, and educational attainment. I identify the causal impact of conscription length on those outcomes by exploiting the exogenous variation in the duration due to North Korean provocation events. These events increase the duration of service but are plausibly unrelated to other unobserved determinants of outcomes. By focusing only on those who served as a conscript and examining the impact of the duration of CMS, I am able to control for the singling effect and study only the channel affecting human capital accumulation.

Many countries such as Russia and South Korea still rely on conscription for military service. Understanding the effect of conscription on the labor market and education outcomes is still important because it will help those countries form relevant policies. Existing literature such as Bauer et al. (2012) and Bingley et al. (2022) disagreed on whether conscription offers a wage premium to veterans. The paucity of literature examining the mechanism behind conscription makes it even more difficult to determine if conscription does come with a wage premium. More importantly, taking a break during work or study to serve as a conscript can be detrimental to one's labor market outcomes and educational attainment. If little or no positive human capital accumulation occurs during conscription, governments extending the length of conscription will be even more harmful for marginalized men.

As mentioned earlier, conscription may lead to human capital accumulation on the intensive margin. It may also affect labor market outcomes on the extensive margin. If the effects of conscription are primarily on the extensive margin, then the length of Conscription, which only affects the intensive margin, should have no positive impact on the labor market outcomes of conscripts. As a country with one of the longest duration of CMS, South Korea has frequently changed the lengths of its conscription over the years. However, different male cohorts with different duration of service may face unique economic shocks that are correlated with individuals' labor market and education outcomes, which can be a potential source of bias. To examine how the length of the conscription in South Korea affects conscripts' income, education attainment, and employment status, I use the moving averages of North Korean (NK) provocation events when conscription started for each cohort as the instrument variables and the individual-level data from the Korean Labor and Income Panel Study (KLIPS).

I start by replicating the results in Lim (2018). Similar to their findings, I find being a conscript is associated with a higher income and this positive effect can only be observed in males with a low level of education. I then use the IV approach to examine the impact of the duration of CMS. I find that the length of conscription appears to have no effect on respondents' income or employment status. However, a longer conscription length does lower male conscripts' chances of acquiring graduate degrees, suggesting human capital depreciation. The evidence suggests that conscription likely affects labor market outcomes on the extensive margin.

1.2 Background

1.2.1 Military Service in South Korea

The South Korean Military Service Law stipulates that all men between the ages of 18 and 38, except those who have a serious physical or mental injury, such as a torn anterior cruciate ligament (ACL), a missing index finger or a serious mental illness, must serve a mandatory military duty term. Exemptions are extremely hard to acquire for healthy males. Only athletes who won international awards and artists who won exceptional awards can obtain an exemption. Healthy males must serve the full length and cannot quit early (Toyryla, 2023).

An enlisted man can choose a sector of service in the military. He can select among the Army, the Navy (including the Marine Corps), and the Air Force. Once he chooses the branch of the military, he can also choose what rank to apply to: enlisted soldier, non-commissioned officer, or commissioned officer. If he makes no choice, he must perform his service as an enlisted soldier in the Army. All the other options to perform his service must be made during the registration process, and are subject to qualification tests, which are different for each option (Hofverberg, 2022).

The duration of service also varies slightly between different branches over the years by about 2 months. The duration of the CMS in all branches changes in the same direction and by roughly the same length at the same time. Generally, the service length in the Army and Marine Corps is the shortest. The duration of conscription in the Navy and the Air Force is slightly longer. In some years, conscripts serve a longer term in the Air Force (Ari, 2019).

The active period of military service has generally varied in South Korea, depending on the geopolitical situation on the peninsula, ranging from 18 to 39 months. North Korean provocations play a major role in determining South Korea's duration of conscription. For instance, the South Korean government extended the duration of conscription for all branches in 1968 when a group of North Korean commandos infiltrated the South and attempted to assassinate the South Korean president (Ari, 2019).

Lim (2018) compares the wages of male conscripts to those who have never served in South Korea with an IV approach. They find conscripts enjoy a wage premium and it is the less educated men who receive such wage premium. They hypothesize that the wage premium can be from either more human capital accumulation or a positive signal to employers that veterans are potentially good workers. The findings of Lee and Jung (2020) further confirm the existence of the conscription premium in South Korea.

1.2.2 Analytical Framework

Conscription can affect an individual's income through both the intensive and the extensive margins. On the intensive margin, conscription can lead to human capital accumulation. Bingley et al. (2022) find compulsory military service can lead to skill acquisition in Denmark. They find male conscripts' numeracy and literacy have significantly improved after conscription. The positive effect of conscription is the strongest for men with low scores in the Armed Forces Qualification Tests.

Savcic et al. (2023) find male conscripts who have served a longer term immediately after high school obtain a higher GPA in university. They believe conscripts acquire non-cognitive skills when they serve a longer term, which improves academic performance. Non-cognitive skills like responsibility, self-discipline, teamwork, perseverance, leadership, self-assurance, and other social skills can also improve one's labor outcomes such as income. As Lim (2018) points out, South Korean conscripts may also obtain work-specific skills. And longer service length makes it possible for conscripts to hone those skills. Lastly, there is also human capital depreciation in skills such as math and biology when conscripts take a break from work or study.[1]

On the extensive margin, conscription can serve multiple functions. It can send a positive signal about the quality of a worker to potential employers. It may be a networking device that offers better career opportunities. Those who served in the military can also share a common sense of belonging so that they favor one another in the civilian workplace. Lastly, conscription can filter out those who are not mentally and physically healthy.

In all, conscripts can acquire experience or technical know-how during their service through the intensive margin. However, there may also be human capital depreciation. On the extensive margin, conscription can be a signaling or networking device, it can also confer a sense of belonging. To determine through which margin conscription affects labor market outcomes, we can examine how the length of conscription affects labor market outcomes and educational attainment. Conscription length can only affect labor market outcomes and educational attainment through human capital accumulation: the intensive margin. And if the conscription wage premium mainly comes from the extensive margin of conscription, the duration of conscription should have no positive effects on those outcomes.[2]

1.3 Methodology

1.3.1 Data

The bulk of the panel dataset used in the analysis comes from the Korean Labor & Income Panel Study (KLIPS). The individual-level dataset contains detailed information about a respondent's date of birth, income, education, region of residence, employment status, occupation, age, military service record, and marriage status from 1998 to 2019. The survey relies on questionnaires filled out by 5,000 urban households in cities nationwide (city sub-districts, towns, and sub-counties) in South Korea. Only those who reside in the Jeju province and those who are serving in the military or institutionalized are excluded from the survey. To overcome the representation problems due to sample attrition, an additional sampling of 1,415 households was conducted in 2009. In 2018, an additional 5,044 households were sampled (KLI, 2022).

For data on North Korean provocations, I use data from the Center for Strategic and International Studies. It includes missile launches, nuclear tests, incursions, infiltration, hijacking, and other provocations. The center keeps track of all provocation events initiated by North Korea regardless of the number of casualties since 1958 (CSIS, 2019). As will be shown later, physical attacks such as incursions and hijacking primarily affect South Korea's

[1] Hjalmarsson and Lindquist (2019) find conscripts are more prone to committing crimes suggesting a loss in human capital.

[2] Puhani and Sterrenberg (2021) find no significant impact of the service length on labor market outcomes.

conscription length compared to missile and nuclear tests[3]. The South Korean military expenditure to GDP ratio is obtained from the World Bank (WB, 2022).

1.3.2 Empirical Strategy

Since only men are subject to mandatory military service and this paper examines the effect of service length, only males are included. I keep only people aged 59 or less because the legal retirement age is 60 years and the decision to retire is endogenous. According to the South Korean Defence White Papers, the military service length ranges from 18 to 39 months over the years. When the South Korean government decides to change the duration of the CMS, they change it for all branches of the military by roughly the same length (Ari, 2019). I keep only respondents whose service length is between 18 and 39 months to minimize the impact of measurement errors. Furthermore, anyone who served before the age of 18 or after the age of 38 is also dropped to minimize measurement errors. Professional military personnel who work in the defense sector are also dropped from the samples.

The duration of conscription varies primarily at the cohort level, and a cohort may be subject to the same socioeconomic shocks that are correlated with the length of conscription and the outcome variables. For example, the government may decide to shorten the duration of the service to deal with a labor shortage. And a labor shortage may drive up the income. So not controlling for such shocks may introduce a downward bias. Another confounding factor may come from the universities. Typically, South Korean men first graduate high school, complete one or two years of university studies, and then start their military service (Toyryla, 2023). As service length is shortened, universities may decide to cover more materials in the first two years of universities due to less depreciation of human capital. And more materials covered may improve students' chances of better job placement, which may also bias the results.

The duration also varies slightly at the individual level due to conscripts serving in different branches. It is plausible that some individual characteristics may be correlated with the duration of the CMS. On the other hand, serving in the Air Force or the Navy can mean better living conditions and more specialized knowledge that can potentially raise conscripts' wages. The data set does not have information on the branch in which a respondent served.

To deal with the potential endogeneity, I use the 6-year, 5-year, and 4-year moving averages of all provocation events by North Korea before conscription started for each

[3]Indeed, according to Lee (2022), a 2022 survey of 1,006 adults by the Korea Society Opinion Institute (KSOI) and the Kukmin Ilbo newspaper said the North Korean missile launches over the previous month would not influence their choice for the country's next president.

individual as the instruments. The idea is that a higher level of threat from North Korea may force the South to extend its conscription length to increase its military preparedness.

The two neighboring countries share one of the most heavily defended borders in the world. Since the Korean War, the two countries have had little economic interaction. Wallace (2016) concludes that North Korea engages in provocations for internal reasons. The list of reasons for provocations ranges from domestic economic pressure to garnering domestic support during regime succession. Provocations by the authoritarian regime ultimately maximize the interest of the Kim dynasty. As long as a full-scale war does not break out, provocations from North Korea should be exogenous to other cohort-level and individual confounding factors in South Korea due to the long-term division between the two countries.

Yoo and Kim (2017) find that provocations are mostly driven by internal causes rather than external stimuli, and that North Korean leaders are more likely to utilize provocations in order to demonstrate power when they are politically strong and may have an incentive to divert internal economic discontent when the country is economically weak. They also find South Korean socioeconomic factors such as the presidential election and the ruling party of the country are not related to the North Korean provocation. North Korea may also attack when they perceive its relative military strength to be strong. By controlling for the defense budget to GDP ratio in South Korea, I can also control for this potential bias.

As shown below, equation 1.1 represents the second stage.

$$Y_{ict} = \alpha_0 + \alpha_1 Conscription\_Length_{ic} + \gamma X_{ict} + Region_j + Year_t + \epsilon_{ict} \tag{1.1}$$

Subscripts i, c, j, and t refer to the individual, the year when conscription . started, the region, and the year of the survey, respectively. The instruments are the 6-year, 5-year, or 4-year moving averages (MA) of North Korean provocation events before conscription started for each individual. $Conscription\_Length_{ic}$ is the length of conscription for each individual in months. Y_{ict} is the dependent variable. It can be the log inflation-adjusted pre-tax income, whether a respondent holds a post-secondary or post-graduate degree, and whether a respondent is working. $Region_j$ and $Year_t$ are the region and year fixed effects, respectively. X_{ict} include the education level, age, the age when conscription started and log defense to GDP ratio when conscription started, the class, and the type of the job of a respondent. ϵ_{ict} is the error term.

The error terms for those in the same conscription cohort may be correlated. Similarly, the error terms in every survey year may also be correlated. Because of that, two-way clustering by survey year and year when conscription started (cohort) is also implemented. Clustering at the cohort level means there is no need to cluster at the lower individual level. Since the survey only has information on respondents' province of residence and South Korea has only 16 provinces, there are too few clusters for clustering by region.

Service length is generated using the dates when conscription started and ended. The age when conscription started is generated with the date of birth and the beginning date of conscription. Log military spending to GDP ratio in South Korea is matched with every cohort as control. A cohort is a group of males who started conscription in the same year. Log income is obtained by taking the log of the inflation-adjusted income of each individual. There are two measures of educational attainment: whether a respondent holds a post-secondary degree or above and whether a respondent holds a post-graduate degree. These dummy variables are equal to one if a respondent holds the respective degree.

Figure 1.1 shows the first stage results of different moving averages of all North Korean provocation events in the previous years. Starting with the 4-year moving average, North Korean provocations have a positive and significant effect on conscription length. Regardless of the order of the moving average, provocations mostly have a positive and significant effect on service length.

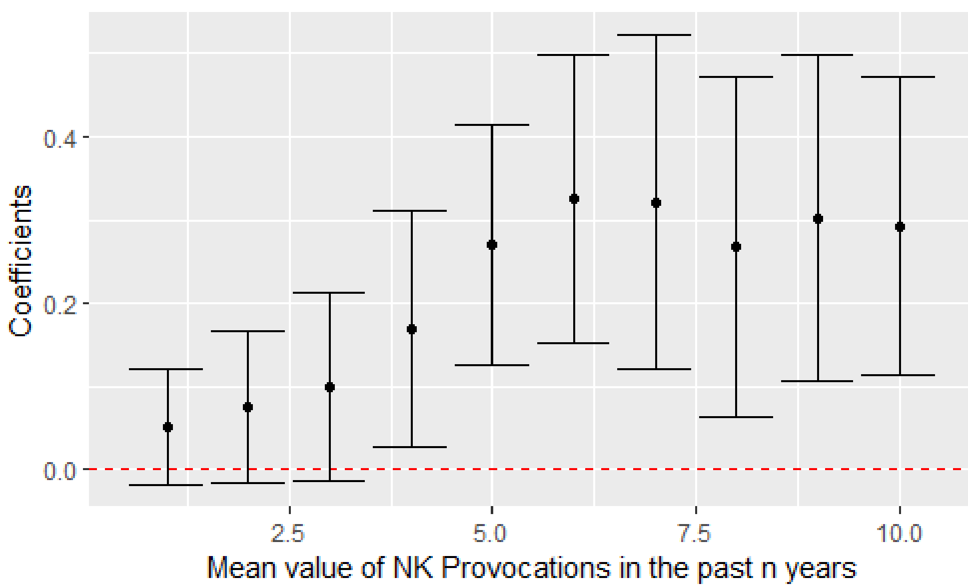


Figure 1.1: Effects of NK Provocations On SK Conscription Lengths (1)

Next, I include only incursions, physical attacks, and other events that led to actual damages in provocations. Figure 1.2 (a) contains the first-stage results of this measure of North Korean provocations. The new measure here appears to have a more significant effect than before. It is clear that more provocations from the North in prior years will lead to longer conscription periods even with a different measure of provocations.

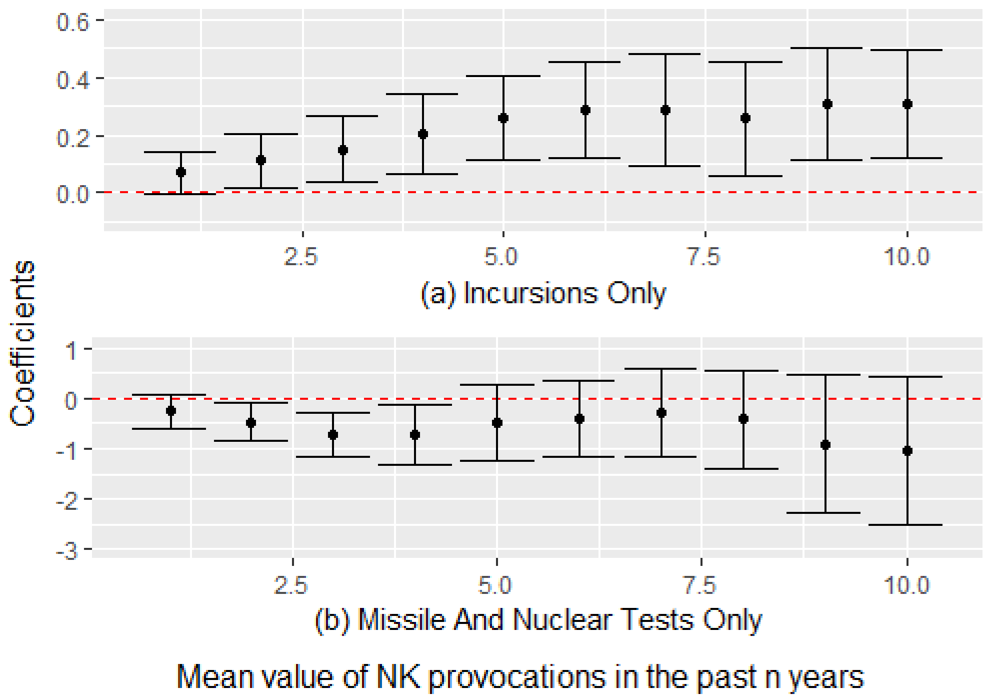


Figure 1.2: Effects of NK Provocations On SK Conscription Lengths (2)

Lastly, I include only nuclear and missile tests for provocation events. See Figure 1.2 (b) for the first-stage results of this measure of provocation events. As Lee (2022) reported, South Koreans do not seem too concerned about those missile and nuclear tests. The mostly insignificant effects here corroborate that claim. However, if there are attacks that lead to actual damages, then the situation may escalate.

Figure 1.3 shows the distribution of the length of the conscription in the sample. Table 1.1 contains the mean of all important dependent and independent variables by the median length of the service. Conscription length is measured in months. The older generation tends to serve a longer term in the military. As reported by Toyryla (2023), South Korean males finish their compulsory military service during university. The respondents in the sample also started their service around 21 years old, one or two years after starting university around age 19. A longer service length also comes with more military spending. Indeed, higher military preparedness also requires higher military expenditures. The younger generation who serves a shorter military service is also more educated holding more post-secondary and post-graduate degrees. As shown in Figure 1.4, the length of the conscription appears to have little effect on income.

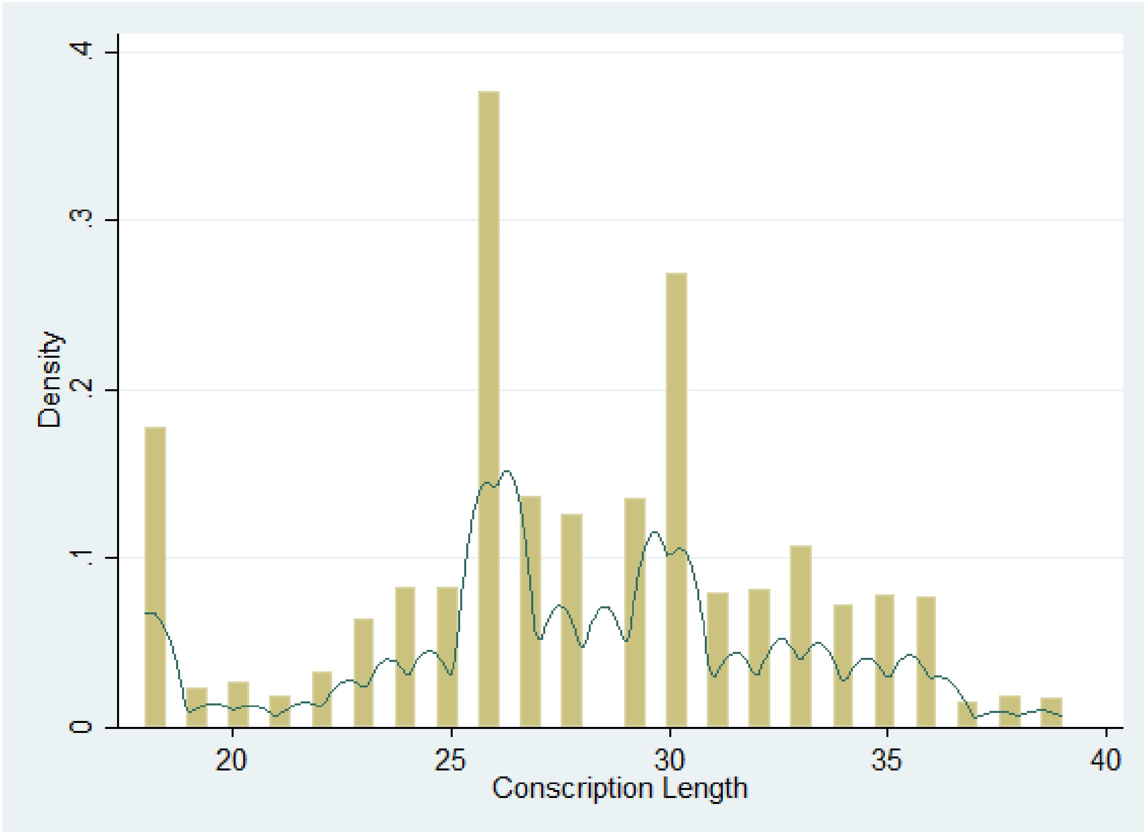


Figure 1.3: Distribution of Service length

Table 1.1: Summary Statistics

(1) Variables	(2) Full sample	(3) Short service length (< 28 mos)	(4) Long service length (> 28 mos)	(5) P-value of mean-comparison test (col 3&4)
Age	43.87	41.22	47.06	0
Age conscripted	20.78	20.82	20.74	0
Length of conscription (months)	27.79	24.24	32.08	0
Military spending as a percentage of GDP (%)	4.20	3.75	4.74	0
Inflation-adjusted pre-tax income (krw 10,000)	4,275	4,209	4,354	0
Post-secondary Or Post-graduate degree holders (%)	55.30	65.03	43.57	0
Post-graduate degree holders (%)	6.2	7.9	4	0
Unemployment rate (%)	3.49	3.42	3.56	0.512
Obs	31,231	17,081	14,151	-

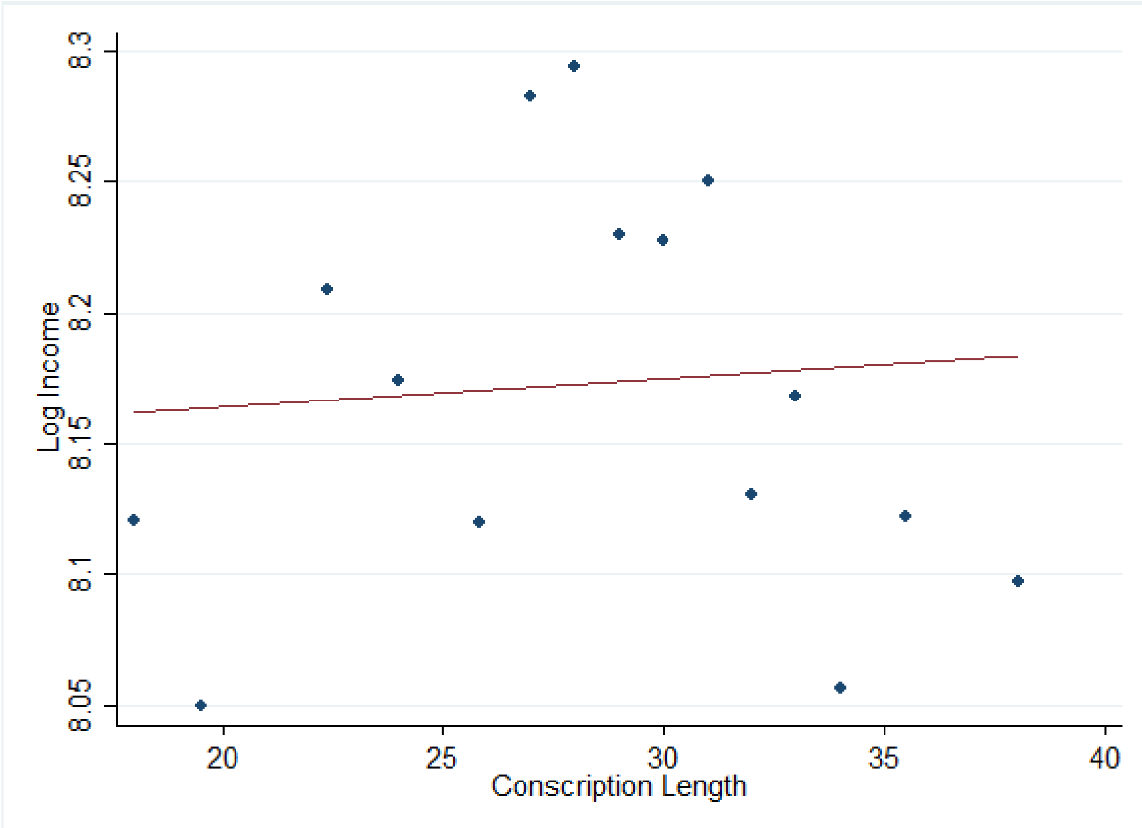


Figure 1.4: Conscription Length and Income

Note: the graph is a bin scatter plot of log inflation-adjusted income and the duration of conscription generated with the sample.

1.4 Results

1.4.1 Main Results

I start by replicating the results in Lim (2018). The paper compares the labor market outcomes between males who have served in the military and those who never have with OLS and IV models. I regress log income on whether the respondent served in the military and other control variables using an OLS model. All respondents in the sample are males who have at least a high school education. Table 1.2 has those results. As shown in column 1 of the table, being a veteran is associated with a 4.22% increase in annual income. Columns 2, 3, and 4 include only respondents with the respective degrees. Only column 2 which includes only high school graduates or lower has a positive and significant result. Similar to what Lim (2018) finds in their OLS and IV models, the wage premium can only be observed for men with a low education level.

Table 1.2: Replication of Lim (2018): Military Service and Income

Dependent variable: Log inflation-adjusted income				
	(1)	(2)	(3)	(4)
Variables	Full sample	High school or lower	Post secondary degree	Post graduate degree
Military service	0.042***	0.042***	0.019	0.005
	(0.011)	(0.016)	(0.015)	(0.038)
Observations	62,495	30,511	28,254	3,730
R-squared	0.451	0.436	0.435	0.593

Note: The full sample only includes males who have at least a middle school education. Military service is equal to 1 if a respondent served as a conscript and 0 otherwise. All models controlled for education, age, the age square, health condition, family economic status, industry, and job type. Year and region fixed effects were also included. The standard errors are clustered at the individual level.
*** p<0.01, ** p<0.05, * p<0.1

Table 1.3 examines the effect of conscription length on income. The instruments are the 6-year, 5-year, and 4-year moving averages of North Korean provocations prior to conscription. Service length is measured in months. The first stage results can be seen in Table A.1. It is clear that there are no significant effects in both the OLS and IV models. I find service length has no effect on income.

There are a few potential identification concerns here. The regression may lack power. Factors such as marriage status and the industry respondents work in may be relevant omitted variables. To address those concerns, I will use different specifications and samples in the following robustness checks.

Table 1.3: Conscription Lengths and Income

Dependent variable: Log inflation-adjusted income				
	OLS	IV		
	(1)	(2)	(3)	(4)
Variables		6-year MA	5-year MA	4-year MA
Service length	0.003	0.018	0.006	-0.007
	(0.002)	(0.029)	(0.026)	(0.035)
Weak identification test		12.05	11.29	4.964
Observations	31,230	31,226	31,230	31,230
R-squared	0.309	0.299	0.310	0.306

Note: The instruments are the 6-year, 5-year, and 4-year moving averages (MA) of North Korean provocations prior to conscription. Service length is measured in months. All models controlled for education, age, the age at conscription, military spending for each cohort, class, and type of a respondent's job. Year and region fixed effects were also included. The standard errors are clustered by the survey years and the years when conscription started. See the first stage results in Table A.1.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

1.4.2 Robustness Checks

Although there are multiple observations for each individual, the variable of interest only exhibits cross-sectional variation. Repeated measures for the same individuals can bias the standard errors. By collapsing the data to one observation per individual, I can more accurately reflect the cross-sectional variation. Therefore, I consolidate the data by the individual, retaining only age, age at conscription, defense budget to GDP ratio, and the first non-missing value of education level. The education level is retained because it barely varies. As shown in Table 1.4, both the OLS and the IV models find no significant effects. The results here also confirm that service length has no effects on income. The first stage results can be found in Table A.2.

Table 1.4: Mean Income and the Duration of Conscription

Dependent variable:Log mean inflation-adjusted income				
	OLS	IV		
	(1)	(2)	(3)	(4)
Variables		6-year MA	5-year MA	4-year MA
Service length	-0.001	-0.019	-0.017	-0.019
	(0.002)	(0.014)	(0.015)	(0.017)
Weak Identification Test		39.55	36.90	25.95
Observations	4,300	4,297	4,300	4,300
R-squared	0.155	0.133	0.136	0.131

Note: The dependent variable is log of mean inflation-adjusted income. The instruments are the 6-year, 5-year, and 4-year moving averages (MA) of North Korean provocations prior to conscription. Service length is measured in months. All models control for education, age, the age at conscription, and military spending for each cohort. The standard errors are clustered by the years when conscription started. See the first stage results in Table A.2.
*** p<0.01, ** p<0.05, * p<0.1

Next, I trim off only the top and bottom 5% of the sample by service length. Dropping those observations is necessary because lengths such as 6 and 50 months have never existed after the war according to Ari (2019). The sample now contains respondents with conscription lengths between 14 and 39 months. Fewer control variables are included and the errors are clustered at the cohort level only. As shown in Table 1.5, the results from all models contain no significant effects.

Table 1.5: Parsimonious Models

Dependent variable: Log inflation-adjusted income				
	OLS	IV		
	(1)	(2)	(3)	(4)
Variables		6-year MA	5-year MA	4-year MA
Service length	0.001	0.008	0.001	-0.020
	(0.001)	(0.024)	(0.029)	(0.041)
Weak identification test		15.59	13.95	7.547
Observations	32,734	32,729	32,733	32,734
R-squared	0.337	0.334	0.337	0.302

Note: The instruments are the 6-year, 5-year, and 4-year moving averages (MA) of North Korean provocations prior to conscription. Service length is measured in months. All models controlled for education, age, the age at conscription, military spending for each cohort, and type of a respondent's job. Individual, year, and region fixed effects were also included. The standard errors are clustered by the years when conscription started. See the first stage results in Table A.3. *** p<0.01, ** p<0.05, * p<0.1

The results appear to be robust to different specifications. Since everyone in the dataset served in the military, the effects on the extensive margin has been controlled for. Any significant effects of conscription lengths on labor market outcomes should be mainly a result of changes in human capital accumulation on the intensive margin. And no significant effects are observed here. The lengths of conscription do not have an effect on income. And that means conscription affects labor market outcomes primarily through the extensive margin.

1.4.3 The Heterogeneous Effects of Conscription

Lim (2018) finds that the conscription wage premium can only be observed in respondents holding a high school degree or lower. They explain the phenomenon as conscription helping low-skilled workers accumulate human capital that workers can later use in their jobs. To examine if that claim is true, I split the sample by whether respondents hold at least a high school degree. The results of the analysis can be found in Table 1.6 and the first stage results are in Table A.4. Only the 5-year moving average is used as the instrument for its high weak identification test value.

As shown in Table 1.6, both the OLS and the IV models find insignificant results for both post-secondary degree holders or above and high school diploma holders or below. The insignificant results reject the claim that low-skilled workers gain much experience during conscription. The wage premiums enjoyed by low-skilled workers are mainly a result of participation in military service instead of serving a longer term. The hardships that conscripts have to endure during military service and the good health that is needed to be in the service are certainly what employers of manual labor are looking for. However, the insignificant results indicate the possibility that there can be a greater impact for individuals with a lower education.

Table 1.6: Results by Education Level

Dependent variable: Log inflation-adjusted income				
	OLS		IV	
Variables	(1) High school or below	(2) Post-secondary or above	(3) High school or below (5-year MA)	(4) Post-secondary or above (5-year MA)
Service length	0.003	0.002	0.048	-0.002
	(0.003)	(0.003)	(0.038)	(0.019)
Weak identification test			6.117	13.54
Observations	13,959	17,271	13,959	17,271
R-squared	0.280	0.314	0.165	0.317

Note: The instrument is the 5-year moving average (MA) of North Korean provocations prior to conscription. The full sample is split by education level. Service length is measured in months. All models controlled for education, age, the age at conscription, military spending for each cohort, class, and type of a respondent's job. Year and region fixed effects were also included. The standard errors are clustered by the survey years and the years when conscription started. See the first stage results in Table A.4.
*** p<0.01, ** p<0.05, * p<0.1

There appears to be little change to the conscripts' level of human capital due to changes in the length of service. Existing literature finds that less educated men tend to benefit from conscription. While my findings do not contradict that of existing literature, I find that such a benefit is not primarily driven by positive human capital accumulation.

The lengths of conscription may also have different effects on people with different types of jobs. In Table 1.7, I examine if the service length may have different effects for males with office and non-office (manual) jobs. As shown in the table below, I split the sample by whether a respondent has an office job. I find no significant results in both the OLS and the IV models. It seems like the type of job that a male holds is irrelevant to the effect of conscription length on income.

In all, a longer conscription length does not lead to a higher wage for people of different educational backgrounds or different types of jobs. That suggests there is little net human capital gain for conscripts. Conscription affects labor market outcomes primarily through the extensive margin.

Table 1.7: Results by Type of Jobs

Dependent variable: Log inflation-adjusted income				
	OLS		IV	
Variables	(1) Office	(2) Non-office	(3) Office (5-year MA)	(4) Non-office (5-year MA)
Service length	0.001	0.003	0.024	-0.012
	(0.002)	(0.002)	(0.028)	(0.040)
Weak identification test			13.76	9.904
Observations	14,296	15,733	14,296	15,733
R-squared	0.461	0.333	0.408	0.279

Note: The instrument is the 5-year moving average (MA) of North Korean provocations prior to conscription. The full sample is split by whether a respondent holds an office job or not. Service length is measured in months. All models controlled for education, age, the age at conscription, military spending for each cohort, class, and type of a respondent's job. Year and region fixed effects were also included. The standard errors are clustered by the survey years and the years when conscription started. See the first stage results in Table A.5.
*** p<0.01, ** p<0.05, * p<0.1

1.5 Employment and Educational Attainment

Hjalmarsson and Lindquist (2019) find conscripts are more likely to commit crimes. That suggests conscription may have an effect on a conscript's chance of employment. I study the effect of conscription length on an individual's chance of finding work here. Table 1.8 has the results of the analysis. See the first stage results in Table A.6. The dependent variable is equal to 1 if a respondent is working and 0 otherwise. The rest of the specifications remain the same as before. In all columns, I find no significant results. Conscription lengths have no effect on the unemployment rate.

Table 1.8: Conscription Lengths and Employment Status

Dependent variable: Respondent is employed				
	OLS	IV		
	(1)	(2)	(3)	(4)
Variables	OLS	6-year MA	5-year MA	4-year MA
Service length	0.000	-0.001	-0.001	-0.002
	(0.000)	(0.001)	(0.001)	(0.001)
Weak identification test		12.33	11.44	5.070
Observations	31,230	31,226	31,230	31,230
R-squared	0.931	0.930	0.930	0.928

Note: The dependent variable is equal to 1 if a respondent is working and 0 otherwise. The instruments are the 6-year, 5-year, and 4-year moving averages (MA) of North Korean provocations prior to conscription. The dependent variable is equal to 1 if a respondent is employed. All models controlled for education, age, age of conscription, and military spending for each cohort. Year and region fixed effects were also included. The standard errors are clustered by the survey years and the years when conscription started. See the first stage results in Table A.6.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Taking breaks during one's education can lead to human capital depreciation. It is therefore possible that those who serve a longer term in the military may suffer more human capital depreciation. And more human capital depreciation can certainly in some cases lead to lower educational attainment. I study how conscription length can affect an individual's chance of obtaining an advanced degree in this section.

I examine the effect of service lengths on an individual's chance of obtaining post-secondary or post-graduate degrees and report the results in the first two columns of Table 1.9. The dependent variable in those columns is equal to 1 if a respondent holds a post-secondary or post-graduate degree and 0 otherwise. Only the 6-year and 5-year moving averages of North Korean provocation events are used as instruments. See the first stage results in table A.7. As shown in the table, both the OLS and the IV models find no significant results. That means shorter conscription length does not affect an individual's chance of obtaining a post-secondary degree. As Toyryla (2023) points out, most Korean males complete their military service 1 or 2 years after they start university. Korean males rarely complete their military service after university graduation. According to Enthoven (2013), even those who go to universities overseas complete their mandatory military service during their university years.

Table 1.9: Conscription Lengths and Educational Attainment

Dependent variable: Respondent holds the following degrees				
	Post-secondary degree or above		Post-graduate degree	
	(1)	(2)	(3)	(4)
Variables	6-year MA	5-year MA	6-year MA	5-year MA
Service length	-0.001	0.014	-0.031***	-0.030***
	(0.020)	(0.021)	(0.010)	(0.009)
Weak identification test	13.25	12.78	13.25	12.78
Observations	30,026	30,030	30,026	30,030
R-squared	0.415	0.403	0.070	0.091

Note: The dependent variable in the first two columns is equal to 1 if a respondent holds a post-secondary or above and 0 otherwise. The dependent variable in the last two columns is equal to 1 if a respondent holds a post-graduate degree and 0 otherwise. The instruments are the 6-year and 5-year moving averages (MA) of North Korean provocations prior to conscription. Service length is measured in months. All models controlled for age, age of conscription, military spending for each cohort, class, and type of a respondent's job. Year and region fixed effects were also included. The standard errors are clustered by the survey years and the years when conscription started. See the first stage results in A.7.
*** p<0.01, ** p<0.05, * p<0.1

Although South Korean males' chance of going to college is not affected by the lengths of their military service, it is still possible that their chance of obtaining a post-graduate degree may be affected. Since most Korean men finish their CMS during university, a longer duration of conscription can lead to more human capital depreciation on knowledge acquired from university courses. Such loss of human capital during university may seriously lower their grades, desire for a post-graduate degree, and chance of being admitted into a graduate school. I study whether longer conscription length affects people's chance of finishing graduate school and report the results in columns 3 and 4 of Table 1.9. I find that service length has a negative and significant effect on a conscript's chance of obtaining a post-graduate degree. An additional month of military service lowers an individual's chance of obtaining a post-graduate degree by about 3.1 percentage points. The unsurprising results confirm that longer breaks during one's education may lead to human capital depreciation.

Serving a longer term in the military does not hurt a conscript's chance of finishing college or chance of employment. However, a longer service in the military does lower his chance of obtaining a post-graduate degree. Ultimately, an individual has to do well in their undergraduate career to enter graduate school. The human capital depreciation may severely lower their GPA and prevent them from entering and finishing graduate school. In all, longer breaks only appear to affect an individual's chance of finishing graduate school because most people complete their military service during university.

1.6 Conclusion

There is a paucity of literature on the mechanism through which conscription affects labor market outcomes and educational attainment. By studying how conscription length affects those outcomes, I am able to control for the effects on the extensive margin and focus on the changes through the intensive margin due to changes in conscription length. Using North Korean provocation events as an instrumental variable and data from South Korea, I find the length of conscription does not have an effect on income, the chance of obtaining post-secondary degrees, or the employment status of conscripts. However, conscripts' chance of obtaining a post-graduate degree is lower when the conscription length is longer.

However, this paper is not without caveats. Since the South Korean government always changes the duration of conscription by no more than 3 months, there may not be sufficient variation in the independent variable. Therefore, it is possible that there is not enough power to identify the effects of longer duration on labor market outcomes.

The results suggest that conscription affects labor market outcomes primarily through the extensive margin. Since longer breaks from work or study will cause human capital depreciation, a longer mandatory military service can certainly be costly and has few benefits to conscripts even during peacetime. However, the results also suggest that such costs hardly impact labor market outcomes and are primarily borne by those who seek to pursue post-graduate degrees. For countries that already have conscription and are suffering from a shortage of military personnel, the findings of this paper suggest that those countries can slightly increase the lengths of their compulsory military service without ruining conscripts' labor market prospects. Additionally, exceptions should be made for those pursuing post-graduate degrees.

In the Russo-Ukrainian War, both sides relied heavily on conscripts. On the other side of the world, the U.S. and Canadian armed forces are beginning to have recruitment shortages in 2023. While conscription may be on its way out, recruitment shortages and possible future high-intensity armed conflicts can force countries to reconsider their positions on conscription. The findings of this paper can help governments with their cost-benefit analysis of the adoption or abolishment of the institution.

Chapter 2

The Heterogeneous Effects of Uncertainty on Divorces

2.1 Introduction

It is said that transparency is crucial to a stable marriage. Naturally, knowing more about the other partner introduces less uncertainty into the marriage and results in better matches being formed. As demonstrated by Becker et al. (1977), more uncertainty leads to more divorces. Existing empirical work, such as Charles and Stephens (2004) and Booth and Edwards (1985), confirm that result. In this paper, I present new empirical evidence using data from Japan and a difference-in-differences approach to show that an increase in uncertainty can lead to both a decrease and an increase in divorces.

In addition to reporting new findings on the effects of uncertainty, this paper also examines discrimination in the marriage market. It is hard to determine what qualities an individual values in a potential partner. Cerroni-Long (1985) proposes an exchange theory that suggests some look for higher income, while others prefer a higher social status. Gordon (1964) explains in detail how third-party preferences for certain values play an important role in marriages. Lastly, the availability of potential candidates may also affect individuals' preferences. The inability to determine what singles value when they search for marriage candidates makes it hard to determine what factors should be controlled for to obtain unbiased results of discrimination in the marriage market.

The case of the Burakumins in Japan presents an opportunity to study the effect of uncertainty on divorces and discrimination in the marriage market. As a minority group with phenotypes identical to those of the average Japanese, the Japanese have discriminated against them for centuries. Even after the emancipation during the Meiji Restoration, the non-Burakumin Japanese still want to identify the Burakumins to avoid marrying or hiring them. Many rely on private investigators to determine if their future spouses or in-laws are Burakumins.

Starting with the Osaka Prefecture in 1985, four other prefectures have adopted ordinances that make it more costly to identify the Burakumins using private investigators. Governments in those prefectures ban background checks aimed at discriminating the Burakumins. In addition to suspension of business, perpetrators can face months of imprisonment and thousands of dollars of fines. The exogenous decrease in the information that partners have about each other in those five prefectures allows me to conduct a diff-in-diff analysis on marriages and divorces. With the prefecture-level panel data compiled by the Statistics Bureau of Japan and the distribution of Burakumins from a government study, I can also conduct a triple diff-in-diff analysis in this paper.

I introduce a simple model to illustrate the mechanism involved. When the cost of marrying a Burakumin is high enough, interracial marriages that accidentally occur because of a lack of information will dissolve. In regions with only a small percentage of Burakumins, it is those new interracial marriages that drive up the overall divorce rates. On the other hand, partners in non-Burakumin marriages will be discouraged from divorce in regions with a large proportion of Burakumins. This is because they will have a higher chance of matching with a Burakumin after divorce, which lowers their remarriage value. It is this unwillingness to divorce among partners in non-Burakumin marriages that brings down the overall divorce rate.

I start with the standard difference-in-differences (DID) model and find no significant changes in either marriages or divorces. I then conduct a triple difference-in-differences analysis to determine if the effect of the policy on marriages and divorces depends on the percentage of Burakumins in each prefecture. While the ordinances have no effects on the number of marriages per thousand, I find an increase in divorces per thousand in places where Burakumins account for a smaller percentage of the total population and a decrease in the measure when there are relatively more Burakumins. The results suggest that an increase in uncertainty can have heterogeneous effects on divorces. Recent literature has shown that when treatment occurs at different time periods, the estimates obtained by staggered DID models may be biased. To alleviate such concern, I employ a new method called the two-stage difference-in-differences to confirm the results along with the synthetic control method.

2.2 Background

2.2.1 Related Literature

Becker et al. (1977) finds that uncertainty in a marriage will result in more divorces. Empirical works, such as Charles and Stephens (2004) and Booth and Edwards (1985), provide evidence for that result. Partners with less information about each other can also have more uncertainty in their marriage. In this paper, I set out to study how this type of uncertainty affects marriage.

On the other hand, this paper also touches on discrimination in the marriage market. Pager and Shepherd (2008) reviews the issue of racial inequality in many social domains and finds that little work is done on the issue in marriage markets. Hitsch et al. (2010) shows that racial preference exists in dating using micro-level data from online dating websites. And that both men and women exhibit same-race preference. Fisman et al. (2008) employs a speed dating experiment to study subjects' revealed preferences. The author also finds a same-race preference in matching. It is the women who tend to have a stronger same-race preference than men.

More recent works, such as Fryer Jr (2007) and Qian and Lichter (2011), focus on documenting the trend in interracial marriages and seeking to explain those trends. Another strand of literature studies divorces among interracial marriages. Bratter and King (2008) shows that interracial couples are more prone to divorce, while Zhang and Van Hook (2009) draws the opposite conclusion. To sum up, existing literature finds uncertainty has a homogeneous effect on divorces. And there is a paucity of empirical literature on discrimination in the marriage market.

2.2.2 The Burakumins

For centuries, the group of people known as the Burakumins were shunned by their fellow Japanese because of their low caste status during the Tokugawa Period. The discrimination against members of the group did not disappear with the abolishment of the caste system in the late 19th Century. Many were still impoverished and had to live in their segregated and dilapidated neighborhoods known as Burakus.

From 1965 to 2002, the Japanese government implemented the Dowa Project to improve the living conditions of those Burakus. However, the additional investment in education, infrastructure, and payments of subsidies did little to curb the discrimination. On the contrary, it made it easier to identify those neighborhoods and gave the general public the impression that the Burakumins lived off handouts (Ramseyer and Rasmusen, 2018).

Although the situation has improved since the 1960s, Burakumins still face discrimination in the areas of employment and marriage. However, the fact that they are identical in phenotype to the average Japanese makes it hard to discriminate against them. With stricter control of access to the Koseki (Household Registry), it becomes increasingly harder to identify the Burakumins. While certain Burakumins have specific Kanjis in their last names, many can be identified by their addresses. In 1975, a list of burakus was found by the police. The list was held and published by a private investigative service. Based on the 1936 survey, the list contained the location of Buraku communities. Just like how major corporations would check their prospective employees' backgrounds, parents would also look into their future in-laws' family origin to screen out the Burakumins. Many turned to private investigators for such background investigations since addresses and last names can be deceiving.

As a result of the severe discrimination, many Burakumins fared poorly in marriage and labor markets. Burakumins tend to marry within their neighborhood and divorce easily for the lack of a quality partner (Gordon, 2006). Being denied opportunities to work in big companies, many either worked in small family businesses or joined the Yakuza. It is estimated that about 60% of the members in Yakuza are Burakumins. In the Yamaguchi-Gumi, the largest organized crime syndicate in Japan, Burakumins account for 70% of the members (Kaplan and Dubro, 1986).

2.2.3 The Anti-Discrimination Ordinances

In 1985, the Osaka Prefecture adopted an ordinance that outlawed the investigation into individuals' backgrounds. Four other prefectures adopted similar ordinances shortly after. Kumamoto followed suit in 1995. A year later, Fukuoka and Kagawa adopted the ordinance. Tokushima passed the same law in 1997. In Figure 2.1, the proportions of Burakumins as a percentage of the total population in every prefecture are shown.

The ordinances punish transgressors by possible suspension of business, fines, criminal penalties, and public denunciation. Private investigators who continue to provide the services may be forced to suspend their businesses. Perpetrators in Osaka can face up to 3 months of imprisonment and 1000 dollars worth of fines. For instance, a business consulting firm and its investigating arm in Osaka were charged by the Osaka District Public Prosecutor for doing such background checks in 1999 (Buraku Liberation and Human Rights Research Institute, 1999).

On the other hand, public denunciation is a very effective measure often used by the Burakumin Liberation League (BLL), a Burakumin human rights activist group. The BLL is said to have used the threat of public denunciation to extort businesses for economic and political gains. The implementation of the policy raised the cost of such background research. Combined with the identical phenotype, the Burakumins can exit their neighborhoods and marry members of the majority when the conditions are favorable.

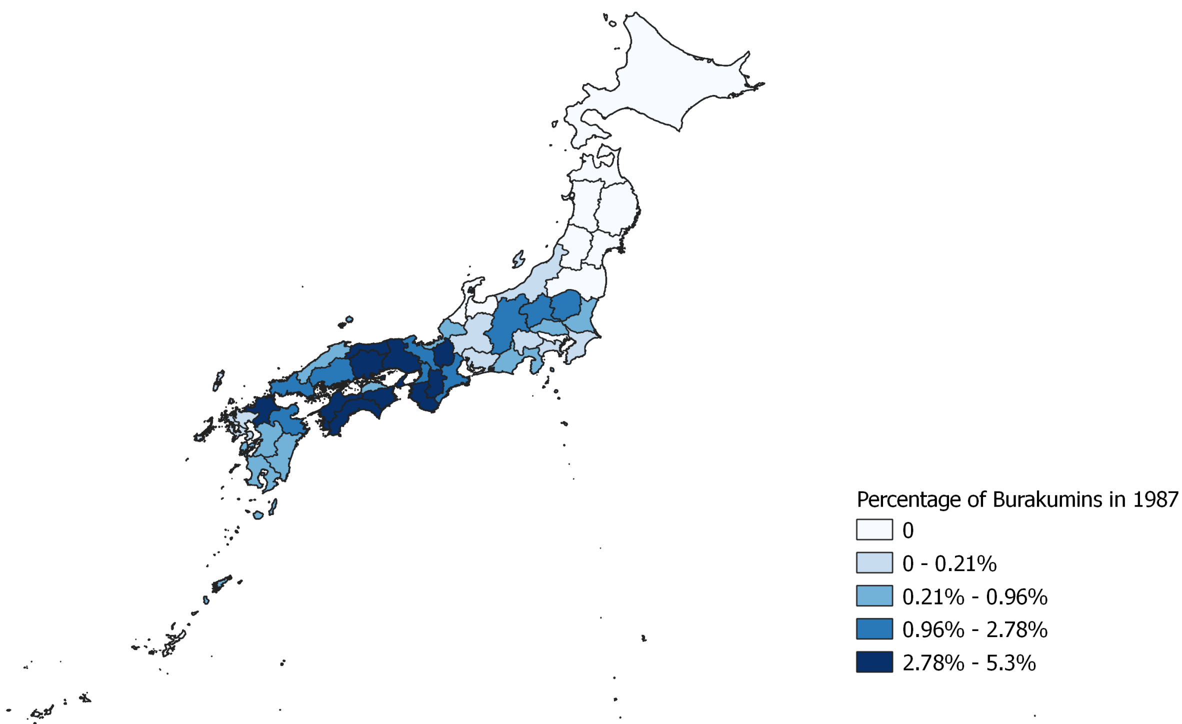


Figure 2.1: The Distribution of Burakumins

2.2.4 Theoretical Framework

This model is based on Browning et al. (2014). I extend the model by allowing different distributions of marriage quality for different ethnic groups and introducing a cost for interracial marriages. In this model, there are two groups of people-the majorities and the Burakumins. Each group has an equal number of men and women. And they live for two periods. Each consumes his or her income Y when they are single. If they get married, each spouse consumes $2Y$. Marriage also entails a non-monetary return θ that both partners share. This marriage quality is randomly distributed. Different couples will draw different values of θ when they get married. And it is not known until after the marriage at the end of the period. Let $F(\theta)$ and $G(\theta)$ be the CDFs of θ when a marriage is between two majorities and when one of the partners is a Burakumin, respectively. Assume further that the mean of θ is zero for both distributions. Additionally, if individuals choose to divorce, then they will have to draw another θ when they remarry.

The meetings occur randomly. An individual meets a person of the opposite sex at the beginning of the period. Assume further that the Burakumins account for R proportion of the population. While it is costless to marry within one's group, intermarriage will lower the consumption of a majority by C. While C can be interpreted as the cost of discrimination, the disutility of marrying a Burakumin may also stem from a lack of trust. A majority will only find out whether their partner is a Burakumin or not at the end of the period. A marriage lasts for at least 1 period. Divorce can occur at the end of the first period. However, remarriage is only possible with unattached individuals who are either never married or divorced. An individual will meet a mate in the first period with certainty. In the second period, the probability of meeting an eligible partner is equal to the proportion of the population that is unattached to the opposite sex.

At the end of the first period, the quality of the match and the identity of one's partner are revealed. Everyone will get married in the first period as consumption is always higher when married and the commitment is only for one period. Before the policy is implemented, the background check is rather cheap and everyone purchases the service before marriage. This implies segregation in the marriage market. The following should occur in both groups. Let α be the remarriage rate. So the value of being unattached at the beginning of the second period is

$$V(\alpha) = Y + \alpha Y \tag{2.1}$$

At the end of the first period, a person will choose to divorce if

$$2Y + \theta < Y + \alpha Y \tag{2.2}$$

At the end of the first period, when

$$\theta < \alpha Y - Y \tag{2.3}$$

divorces occur. An equilibrium is reached when the expected remarriage rate is equal to the actual divorce rate. We have,

$$\alpha_M^* = F[\alpha_M^* Y - Y] \;\; \alpha_B^* = G[\alpha_B^* Y - Y] \tag{2.4}$$

The Burakumins have a higher rate of divorce. To capture this feature in the model, I assume that θ is uniformly distributed with a support of $[-b_M, b_M]$ if the marriage is between two majorities. And if either one of the partners is a Burakumin, then θ will be uniformly distributed with a support of $[-b_B, b_B]$. Furthermore, b_B is greater than b_M. So the equilibrium divorce rate is the weighted average of the two divorce rates.

$$\alpha^* = (1-R)\frac{b_M - Y}{2b_M - Y} + R\frac{b_B - Y}{2b_B - Y} \tag{2.5}$$

After the anti-discrimination ordinance is implemented, the service will be too expensive to purchase. As a result, there are three types of marriages: Burakumin within-race marriages, majority within-race marriages, and interracial marriages. The expected value of being unattached for a Burakumin at the beginning of the second period is different from that of a majority. Let π be the proportion of Burakumins among divorcees. See Appendix B for the detailed derivation of the model.

In equilibrium, the expected remarriage rate is equal to the actual divorce rate, this gives,

$$\alpha = R^2 G[\alpha Y - Y] + 2R(1-R)G[\alpha Y - Y + (1-\alpha\pi)C] + (1-R)^2 F[\alpha Y - Y - \alpha\pi C] \tag{2.6}$$

The proportion of divorcees who are Burakumins at the beginning of the second period can be represented by the following equation,

$$\alpha\pi = R^2 G[\alpha Y - Y] + R(1-R)G[\alpha Y - Y + (1-\alpha\pi)C] \tag{2.7}$$

In Browning et al. (2014), the authors claim that solving for both α and π analytically in their original model is rather difficult, so they simulate the model. I follow their advice and also simulate the model. I simulate the divorce rates before and after the policy is implemented for regions with different levels of Burakumin presence and report the difference between the sets of divorce rates in Figure 2.2. A higher percentage of Burakumins implies a higher chance of meeting a Burakumin in the second period. And that will discourage a majority from exiting a majority-majority marriage. On the other hand, an increase in the proportion of the Burakumin population will also increase the proportion of marriages that are prone to dissolve[1] (i.e. Burakumin-Burakumin and interracial marriages).

[1] $\alpha Y - Y - \alpha\pi C < \alpha Y - Y < \alpha Y - Y + (1-\alpha\pi)C$

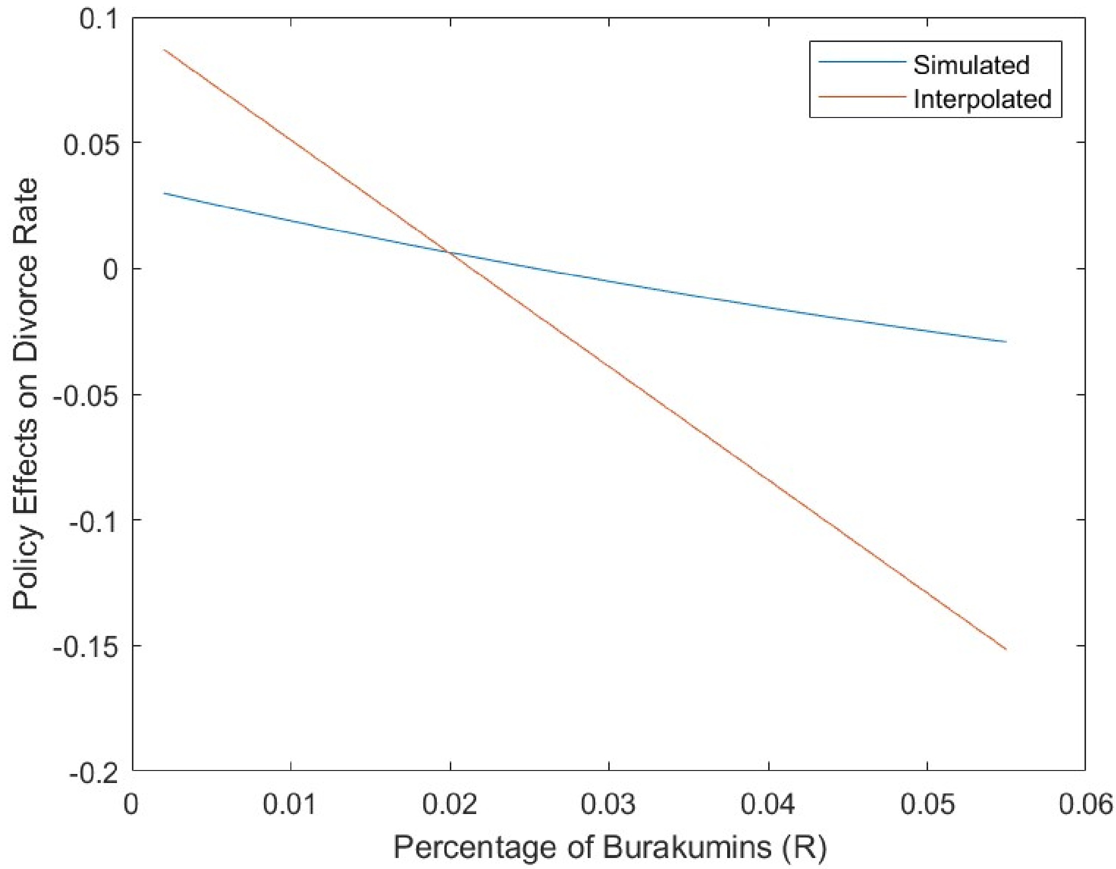


Figure 2.2: Relative Burakumin Presence and the Effect of the Policy

Note: I estimate the divorce rates before and after implementing the policy for various levels of Burakumin presence through simulation. The blue line represents the changes in divorce rates resulting from the policy, as generated by the simulation. The red line ($0.087 - 4.170 \times R$) is derived using linear interpolation with the results in Column 4, Table 2.2.

When the cost of marrying a Burakumin is high enough, all interracial marriages will result in divorce. The divorce rate of Burakumin-Burakumin marriages remains constant. When the Burakumins account for only a small proportion of the total population, it is the interracial marriages that drive up the total divorces. Partners in majority-majority marriages are discouraged from divorce when the Burakumins account for a higher proportion of the population, which lowers the overall divorce rate. As shown in Figure 2.2, the simulated changes in the divorce rate match the results generated by linear interpolation with data qualitatively.

2.3 Methodology

2.3.1 Data

For the empirical analysis, I use prefecture-level data from the Statistics Bureau of Japan. The Statistics Bureau of Japan is a government organization operating under the Ministry of Internal Affairs and Communications. The Bureau compiles administrative and survey data from various sources including population census to create the aggregate data at municipal, prefecture, regional, and national levels. The panel dataset contains data on the number of divorces and marriages, prefecture-level population, education, crime, local economy, and other relevant control variables from 1975 to 2018 (Statistics Bureau of Japan, 2019).

The data on the distribution of Burakumin comes from Smith (1995). According to the author, the data was originally collected by the Japanese government. With household registration records and population census, the government of Japan conducts studies specifically targeting Burakumins from time to time.

2.3.2 Empirical Strategy

Although Japan has 47 prefectures in total, I only keep the prefectures that have nonzero percentages of Burakumins (36 prefectures). That is because all the treated prefectures have Burakumins and I am concerned that prefectures without Burakumins may be fundamentally different from the treated prefectures. I drop the highest and lowest 1% of the data based on the outcome variables to avoid outliers driving the results.

The dependent variables are divorces per thousand and marriages per thousand. The list of covariates includes the unemployment rate, crimes per thousand, log GDP, percentage enrolled in universities and colleges, percentage of net immigration, percentage of land cultivated, and the labor force participation rate. Due to the fact that the data for covariates comes from the census, I use the nearest neighborhood extrapolation to fill in the gaps.

The standard difference-in-difference and the triple diff-in-diff approaches are chosen to study the effect of the policy. While the standard diff-in-diff setup is simple, the triple diff-in-diff involves interacting the diff-in-diff interaction term with the percentage of the Burakumin population.

For each dependent variable, I use a few types of models. The standard diff-in-diff model at the prefecture level is as follows

$$Y_{pt} = \alpha + \delta D_{pt} + \eta R + \beta X_{pt} + \gamma_p + \sigma_t + \epsilon_{pt} \tag{2.8}$$

Y_{pt} is the dependent variable and X_{pt} refers to the list of covariates mentioned earlier. γ_p and σ_t are the prefecture and year fixed effects, respectively. α is a constant. ϵ_{pt} is the error term. Subscripts p and t represent the prefecture and year, respectively. D_{pt} is the interaction term and δ captures the effect of the policy. The first triple diff-in-diff model used is given as

$$Y_{pt} = \alpha + \delta D_{pt} + \lambda D_{pt} \times R + \eta R + \beta X_{pt} + \gamma_p + \sigma_t + \epsilon_{pt} \tag{2.9}$$

In this model, I interact the relative presence R with the diff-in-diff interaction term D_{pt}. So the total effect on a treated prefecture is $\delta + \lambda R$. Everything else stays the same as before. For a clearer interpretation, I convert R into a dummy: large Burakumin presence. It is equal to 1 if R is greater than its median in the sample. So we have the following model

$$Y_{pt} = \alpha + \delta D_{pt} + \lambda D_{pt} \times LBP + \eta LBP + \beta X_{pt} + \gamma_p + \sigma_t + \epsilon_{pt} \tag{2.10}$$

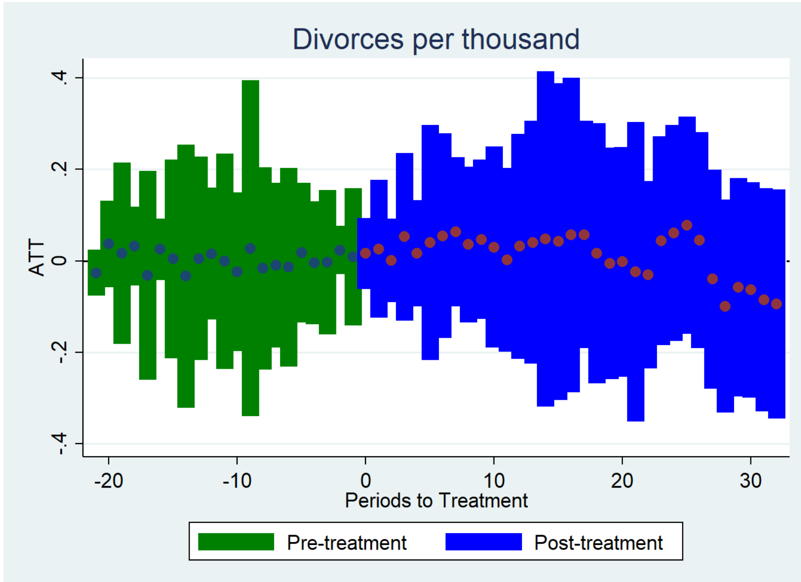
LBP is equal to one if the region has a large Burakumin presence. Therefore, in regions with a small proportion of Burakumins, δ captures the effect of the policy. And $\delta + \lambda$ gives the effect of the policy in places with a large Burakumin presence. To further confirm my results and address the potential bias that recent literature has found in staggered OLS DID models, I will implement a two-stage DID and the synthetic control method.

Table 2.1: Summary Statistics

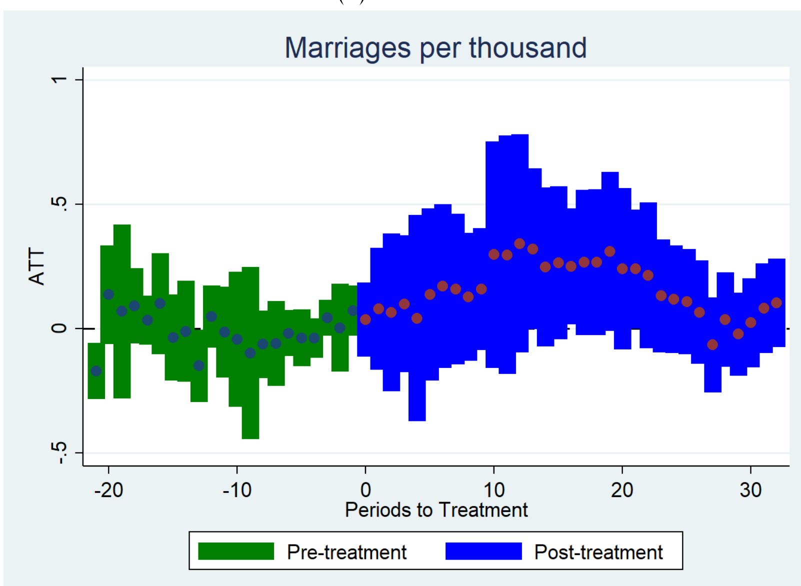
Variables (Mean)	No ordinance	Ordinance passed	Sample
Marriages per thousand	5.657	5.487	5.645
Divorces per thousand	1.508	1.992	1.547
Percentage of Burakumins (%)	1.56	1.94	1.59
Unemployment rate(%)	3.85	5.90	4.00
Crimes per thousand	1.18	1.59	1.21
Log GDP	15.754	16.096	15.781
Percentage enrolled in Uni/Coll (%)	2.13	2.81	2.18
Percentage of net immigration (%)	-2.14	1.41	-1.86
Percentage of arable land (%)	13.4	13.2	13.3
Labor force participation rate (%)	50.7	49.4	50.6
Observations	1,427	121	1,584

Table 2.1 holds the summary statistics for all the outcome variables and the covariates. The sample includes only the prefectures that have non-zero Burakumins. I then divide the sample by whether the prefectures passed the ordinance. Except for the amount of immigration, prefectures with and without the ordinance appear to be somewhat similar in other aspects. The DID approach relies on the parallel trend assumption between the treated and control groups. I conduct event studies on the outcome variables using the methodology proposed by Callaway and Sant'Anna (2021). This method addresses the bias inherent in the staggered OLS DID approach. As shown in Figures 2.3 and 2.4, the

coefficients before the treatment in all graphs are insignificant, indicating that the parallel trend assumption holds.

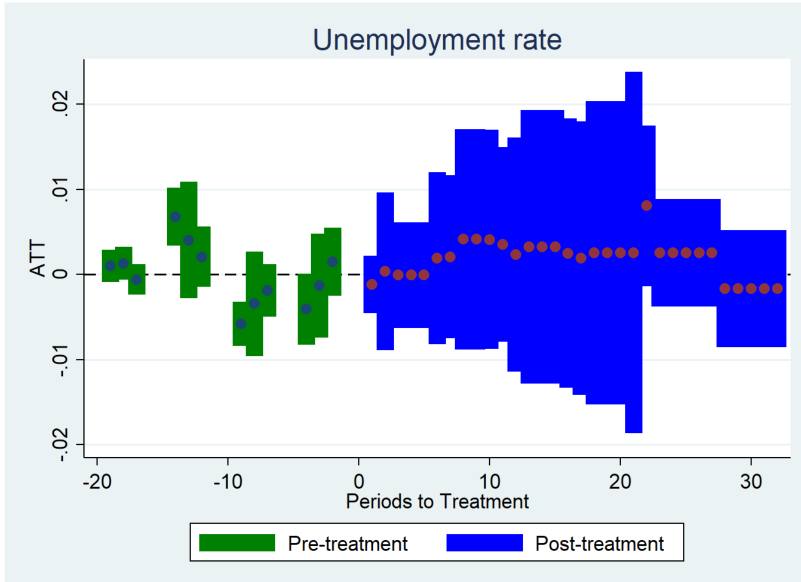


(a) Divorces

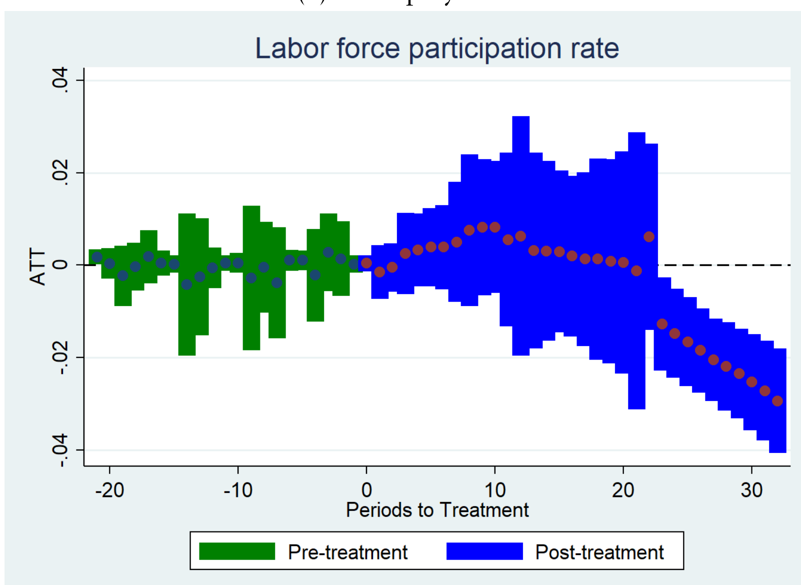


(b) Marriages

Figure 2.3: Event Studies for Marriages and Divorces



(a) Unemployment



(b) Labor Force Participation

Figure 2.4: Event Studies for Unemployment and Labor Force Participation

2.4 Results

2.4.1 Main Results

As shown in Table 2.2, I evaluate the effect of the policy with two different models and two different samples. Columns 1 and 3 report the results of model 2.8. Columns 2 and 4 analyze the data using model 2.9. For all columns, the sample includes prefectures with non-zero Burakumins with the highest and lowest 1% of the data dropped as mentioned earlier. Since I have to interpolate some of the data by prefecture to fill in the gaps, the errors may be correlated at the prefecture level. That is why I also cluster the errors by prefectures to make the results more robust in every column. The sample that I use for the analysis contains 36 prefectures, which is not an extremely small number of clusters. In the robustness checks, I will use the full sample which contains 47 prefectures to show that the small sample bias is not a concern.

Table 2.2: Effects of the Policy on Marriage and Divorce Rates

	Marriages per thousand		Divorces per thousand	
Variables	(1)	(2)	(3)	(4)
Ordinance implemented	0.083	0.242	0.005	0.096***
	(0.092)	(0.199)	(0.036)	(0.032)
Ordinance implemented × Burakumins (%)		-7.237		-4.505**
		(6.246)		(2.143)
Burakumins (%)	1,706.4	1,492	-502.3	-667.8
	(1,107.5)	(1,139.2)	(593.6)	(591.1)
Observations	1,489	1,489	1,480	1,480
R-squared	0.946	0.947	0.973	0.974

Note: Ordinance implemented is the diff-in-diff interaction term. It is equal to 1 if it is in a treated prefecture after the policy is implemented. All models include prefecture and year fixed effects. Covariates include the unemployment rate, crimes per thousand, log GDP, percentage enrolled in universities and colleges, percentage of net immigration, percentage of land cultivated, and the labor force participation rate. Errors are clustered by prefecture.
*** p<0.01, ** p<0.05, * p<0.1

The interaction term in the standard diff-in-diff regression is not significantly different from zero in the first column. The coefficients of the interaction terms in the second column are both insignificant. Moving on to column 3, the standard diff-in-diff model gives no significant results even when the divorce rate is the dependent variable. In column 4, the coefficient on the interaction term is positive and significant while the coefficient on the triple diff-in-diff interaction term is negative and significant. In all, I find little evidence that the policy has any effect on the marriage rate. However, it is clear that the policy increases the divorce rate in areas with a small Burakumin presence and lowers the number of divorces when Burakumins account for a larger percentage of the population. As mentioned earlier, with model 2.9, the effect on each treated prefecture can be calculated as $0.096 - 4.505 \times R$, where R is the proportion of Burakumins in a prefecture. The policy increases divorces per thousand in Kagawa, Kumamoto, and Osaka by 0.059, 0.065, and 0.022, respectively. In Fukuoka and Tokushima, the policy lowers divorces per thousand by 0.033 and 0.083, respectively. Table 2.6 reports the mean of divorces per thousand in each treated prefecture. To understand the magnitude of the effects, I will report the effects as a percentage of the mean in each prefecture. The policy increases divorces per thousand in Kagawa, Kumamoto, and Osaka by 3%, 3.45%, and 1.04%, respectively. In Fukuoka and Tokushima, the policy lowers divorces per thousand by 1.13% and 4.6%, respectively.

2.4.2 Robustness Checks

In this section, I check the robustness of the baseline results by including no covariates, using the full sample, clustering the errors by both year and prefecture, and using the binary measure of relative Burakumin presence, respectively. In Table 2.3, I use a set of different specifications to confirm the policy's effect on marriage per thousand. Columns 1 to 4 report the results using model 2.9, while the last column uses model 2.10 for the analysis. Additionally, all columns use the sample as that of the main results, except for column 3 which uses the full sample. Errors are clustered by prefecture in columns 1, 2, 3, and 5. In column 4, two-way clustering by prefecture and year is used.

I start by reporting the baseline result in column 1. This column obviously has no significant results. In column 2, I include no covariates or prefecture-fixed effects. There are still no significant results. Even with the full sample in column 3, the results are insignificant as well. The two-way clustering model in column 4 also finds no significant effects. In the last column, model 2.10 continues to find insignificant results[2]. Similar to the main results, I find the policy has no significant effects on the marriage rate. The main results for marriages appear robust to different specifications.

[2]The median of relative Burakumin presence is 0.011. The proportions of Burakumins in 2 out of 5 treated prefectures are below the median. 16 out of 31 control prefectures have proportions of Burakumins below the median.

Table 2.3: Robustness Checks for the Effect on Marriages

Dependent variable: Marriages per thousand					
	(1)	(2)	(3)	(4)	(5)
Variables	Baseline Results	No covariates	Full sample	Two-way clustering	Large Burakumin presence
Ordinance implemented	0.242	0.377	0.207	0.242	0.158
	(0.199)	(0.279)	(0.152)	(0.195)	(0.171)
Ordinance implemented × Burakumins (%)	-7.237	-7.697	-9.000*	-7.237	
	(6.246)	(9.287)	(4.689)	(6.021)	
Burakumins (%)	1,492.1	-3.3	784.9		
	(1,139.2)	(3.4)	(1,058.0)		
Ordinance implemented × Burakumin (LBP)					-0.119
					(0.195)
Burakumin (LBP)					-1.613*
					(0.926)
Observations	1,489	1,517	1,974	1,489	1,489
R-squared	0.947	0.681	0.949	0.947	0.946

Note: Ordinance implemented is the diff-in-diff interaction term. It is equal to 1 if it is in a treated prefecture after the policy is implemented. Large Burakumin presence (LBP) is equal to 1 if the proportion of Burakumins is greater than its median (0.011). Column 2 contains no covariates or prefecture-fixed effects. Models in columns 1, 3, 4, and 5 include prefecture and year fixed effects. Covariates in columns 1, 3, 4, and 5 include the unemployment rate, crimes per thousand, log GDP, percentage enrolled in universities and colleges, percentage of net immigration, percentage of land cultivated, and the labor force participation rate. Errors are clustered by prefecture in columns 1, 2, 3, and 5. Errors are clustered by prefecture and year in column 4.
*** p<0.01, ** p<0.05, * p<0.1

Next, I use the same specifications from the previous table to check the effect of the policy on divorces per thousand and report the results in Table 2.4. In all columns, I find positive effects on the diff-in-diff interaction terms and negative effects on the triple interaction terms. All results are quantitatively similar and significant. The main results for the divorce rate are also robust to different specifications.

Table 2.4: Robustness Checks for the Effect on Divorces

Dependent variable: Divorces per thousand					
	(1)	(2)	(3)	(4)	(5)
Variables	Baseline Results	No covariates	Full sample	Two-way clustering	Large Burakumin presence
Ordinance implemented	0.096***	0.140**	0.101***	0.096***	0.082***
	(0.032)	(0.067)	(0.035)	(0.032)	(0.021)
Ordinance implemented × Burakumins (%)	-4.505**	-4.457*	-5.091**	-4.505**	
	(2.143)	(2.306)	(2.513)	(2.108)	
Burakumins (%)	-667.8	3.3	-747.0		
	(591.1)	(2.6)	(539.7)		
Ordinance implemented × Burakumin (LBP)					-0.129***
					(0.043)
Burakumin (LBP)					0.679
					(0.529)
Observations	1,480	1,516	1,974	1,480	1,480
R-squared	0.974	0.789	0.970	0.974	0.974

Note: Ordinance implemented is the diff-in-diff interaction term. It is equal to 1 if it is in a treated prefecture after the policy is implemented. Large Burakumin presence (LBP) is equal to 1 if the proportion of Burakumins is greater than its median (0.011). Column 2 contains no covariates or prefecture-fixed effects. Models in columns 1, 3, 4, and 5 include prefecture and year fixed effects. Covariates in columns 1, 3, 4, and 5 include the unemployment rate, crimes per thousand, log GDP, percentage enrolled in universities and colleges, percentage of net immigration, percentage of land cultivated, and the labor force participation rate. Errors are clustered by prefecture in columns 1, 2, 3, and 5. Errors are clustered by prefecture and year in column 4.
*** p<0.01, ** p<0.05, * p<0.1

2.4.3 Alternative DID Methods

Recent work, such as Baker et al. (2022) and Callaway and Sant'Anna (2021), show that the staggered OLS difference-in-differences (DID) estimators can produce biased results. The result of a staggered DID analysis is essentially a weighted average of the treatment effects from various treated groups. With a staggered OLS DID setup, early-treated groups are used as the control group for late-treated groups. Effects estimated from such comparison will be given negative weights. Therefore, the estimates from such analysis may be smaller in magnitude than the actual effects or even have the wrong sign. The cause of this problem lies in the fact that a staggered OLS DID assumes the treatment effects are the same across groups and periods.

To deal with the potential bias, I implement a new method developed by Gardner (2022), known as the two-stage difference-in-differences. Borusyak et al. (2021) complete the mathematical derivation of the model. As one of the very few models that allows a triple difference-in-differences setup, the two-stage DID model allows me to check the robustness of the main results. This method computes the heterogeneous treatment effects across groups and periods to estimate the overall effect. To that end, group and period effects are identified in the first stage from the sample of untreated observations, and average treatment effects are identified in the second stage by comparing treated and untreated outcomes, after removing the group and period effects.

Table 2.5: Two-Stage Difference-in-Differences

	Marriages per thousand		Divorces per thousand	
Variables	(1)	(2)	(3)	(4)
Ordinance implemented	0.117	0.285	0.006	0.087***
	(0.088)	(0.178)	(0.034)	(0.033)
Ordinance implemented × Burakumins (%)		-8.635		-4.170**
		(5.706)		(2.072)
Observations	1,489	1,489	1,480	1,480

Note: Results in this table are generated using the two-stage DID estimator developed by Gardner (2022). Ordinance implemented is the diff-in-diff interaction term. It is equal to 1 if it is in a treated prefecture after the policy is implemented. In the first stage, prefecture and year fixed effects are included, and covariates in all columns are the unemployment rate, crimes per thousand, log GDP, percentage enrolled in universities and colleges, percentage of net immigration, percentage of land cultivated, and the labor force participation rate. Errors are clustered by prefecture.
*** p<0.01, ** p<0.05, * p<0.1

As shown in Table 2.5, I estimate the effect of the policy on the marriage and divorce rates again with the two-stage DID method. The covariates and sample remain the same as those of the main results. The results in all columns are roughly the same as before. I still find no evidence that the policy has any impact on marriages, and the heterogeneous effects of the policy on divorces can be observed. As shown in model 2.9, the effect of the policy on each treated prefecture can be computed as $\delta + \lambda R$. I can compute the effect of the policy on each prefecture using the coefficients of the interaction and the triple interaction terms estimated in both DID methods along with the percentage of Burakumins in each treated prefecture. Table 2.6 has the effects of the policy on each treated prefecture's divorce rate using the coefficients estimated by the two different estimators. The results are quantitatively similar.

Table 2.6: Impact on Divorces per Thousand by Prefecture

Prefecture	(1) Proportion of Burakumins	(2) OLS DID	(3) Two-Stage DID	(4) Divorces per thousand (mean)
Kagawa	0.008	0.059	0.052	1.907
Kumamoto	0.007	0.065	0.058	1.885
Osaka	0.016	0.022	0.019	2.107
Fukuoka	0.029	-0.033	-0.033	2.195
Tokushima	0.040	-0.083	-0.079	1.805

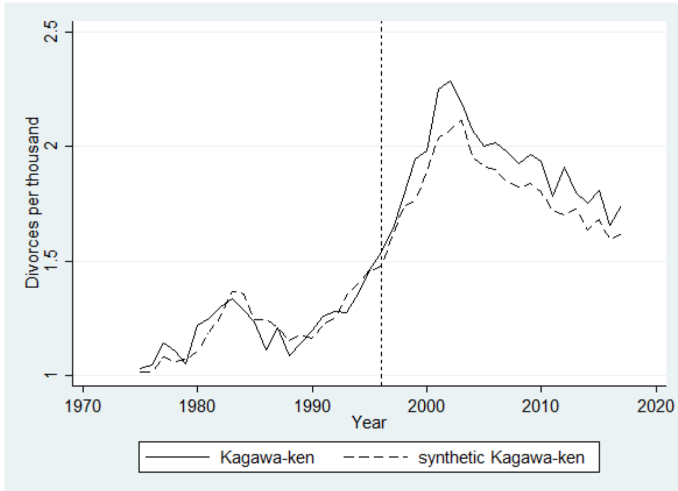
Note: Results in the table are computed using the interaction and the triple terms estimated by each estimator, and the percentage of Burakumins. R is the percentage of Burakumin in each prefecture. The effects of the policy in column 2 are equal to $0.096 - 4.505 \times R$. The results in column 3 are equal to $0.087 - 4.170 \times R$.

Estimates obtained from the DID approaches that have only one treatment period can still be trusted. As an alternative approach to further ascertain the effect of the policy on divorces, I keep only one treated prefecture in each sample and generate synthetic control graphs for each of the treated prefectures with divorces per thousand as the dependent variable. To synthesize the control group in all graphs, I use the following covariates: the unemployment rate, crimes per thousand, log GDP, percentage enrolled in universities and colleges, percentage of net immigration, percentage of land cultivated, the labor force participation rate, and the percentage of Burakumins.[3] I use the full sample for each graph and keep only one treated prefecture in each sample. Figures 2.5 and 2.6 explore the impact of the policy on the divorce rate in the five treated prefectures.

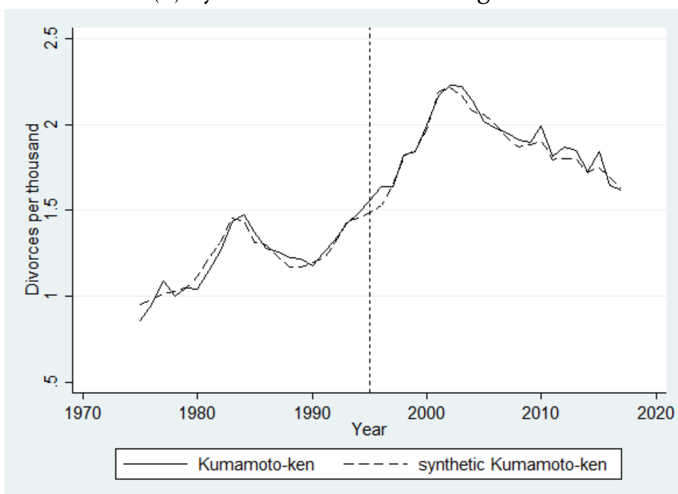
Results for prefectures with a small relative presence of Burakumins can be found in Figure 2.5, and Figure 2.6 has the results for prefectures with a large Burakumin presence. In Figures 2.5 (a) and 2.5 (c), delayed positive effects can be seen in Kagawa and Osaka. However, no effects of the policy can be observed in Figure 2.5 (b) for Kumamoto. In Figures 2.6 (a) and 2.6 (b), I observe delayed negative effects in Fukuoka and Tokushima, respectively. The delayed effects are plausible because it takes time for the effects to occur in the marriage market. With the exception of Kumamoto in Figure 2.5, which shows no effect from the policy, other prefectures in the figure with a small Burakumin presence see an increase in their divorce rate. On the other hand, all prefectures experience a decrease in divorces when Burakumins account for a large proportion of the population in Figure

[3]The nested and allopt options in Stata are selected to generate robust results.

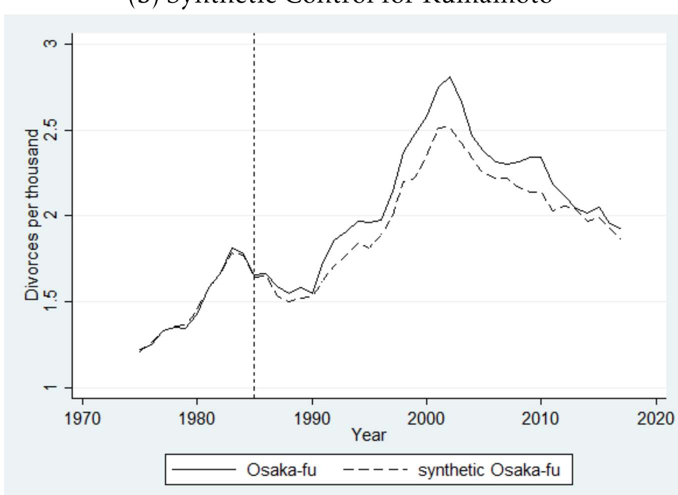
2.6. Results in the graphs for most prefectures qualitatively match that of the main results. While it is possible that the ordinance may be somewhat ceremonial in Kumamoto, the results here confirm that uncertainty created by the policy has heterogeneous effects on divorces. I also conduct placebo tests for all five treated prefectures. As shown in Appendix C, the effects in all prefectures are greater than most placebos except Kumamoto.



(a) Synthetic Control for Kagawa

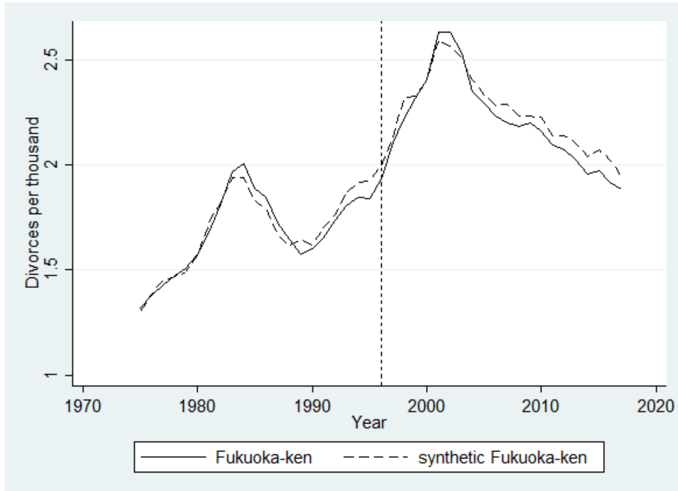


(b) Synthetic Control for Kumamoto

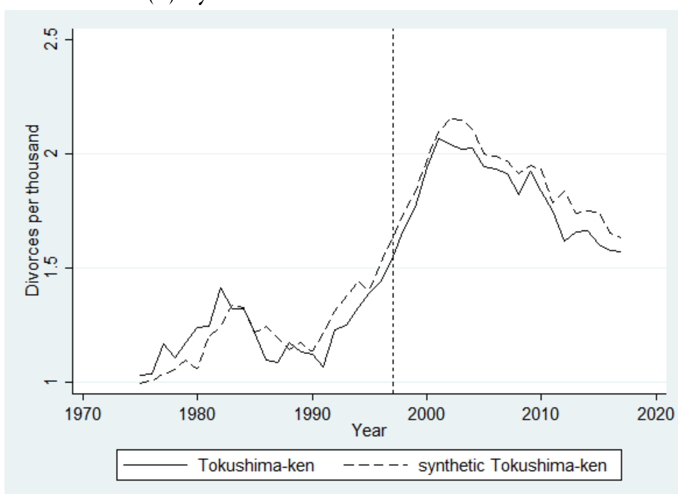


(c) Synthetic Control for Osaka

Figure 2.5: Small Burakumin Presence



(a) Synthetic Control for Fukuoka



(b) Synthetic Control for Tokushima

Figure 2.6: Large Burakumin Presence

2.4.4 Impact on Employment

A major concern is that the changes in the divorce rate may be driven by changes in people's employment status. The policy also bans firms from such background checks against Burakumins. And if they become more involved in the formal labor market, their marriage quality may change as a result of better employment opportunities. However, in all the previous analyses, I included the unemployment rate and the labor participation rate as controls. Therefore, it is unlikely that the effects of the policy on divorces are driven by changes in labor market outcomes.

Although changes in divorces are not related to changes in labor market outcomes, the policy can still have a separate effect on the Burakumins' labor market outcomes. The Burakumins can now work outside of their small family businesses as corporations can no longer screen out the Burakumins with background checks. As shown in Table 2.7, I use the same setup as Table 2.2 to examine the policy's effect on the unemployment rate and the labor force participation rate. Columns 1 and 3 report the results of model 2.8. Columns 2 and 4 analyze the data using model 2.9. The policy has no significant effect on labor outcomes. The insignificant results here also confirm that changes in the divorce rate are not driven by changes in labor market outcomes.

Table 2.7: Effects of the Policy on Labor Outcomes

	Unemployment rate		Labor force participation rate	
Variables	(1)	(2)	(3)	(4)
Ordinance implemented	-0.000 (0.002)	-0.001 (0.002)	-0.006 (0.004)	-0.005 (0.007)
Ordinance implemented × Burakumins (%)		0.028 (0.099)		0.004 (0.269)
Burakumins (%)	37.7* (20.2)	38.9** (19.0)	-85.9* (50.2)	-82.5* (46.5)
Observations	1,475	1,475	1,480	1,480
R-squared	0.962	0.963	0.881	0.885

Note: Ordinance implemented is the diff-in-diff interaction term. It is equal to 1 if it is in a treated prefecture after the policy is implemented. All models include prefecture and year fixed effects, prefecture Burakumin presence interaction, and year Burakumin interaction. Covariates include crimes per thousand, log GDP, percentage enrolled in universities and colleges, percentage of net immigration, and percentage of land cultivated. Errors are clustered by prefecture.
*** p<0.01, ** p<0.05, * p<0.1

2.5 Conclusion

I examine the effect of uncertainty on divorce in this paper. By exploiting the prefecture ordinances that prevent the identification of a group of heavily discriminated people known as the Burakumins in Japan, I am able to show with a triple diff-in-diff approach that uncertainty has heterogeneous effects on divorces. In regions with a small percentage of Burakumins where the information is less important, introducing uncertainty increases the divorce rate. When the information is more important in places where Burakumins account for a larger share of the population, uncertainty decreases the total number of divorces.

This paper offers a possible explanation for the heterogeneous effects. In regions where the information is less relevant, many new interracial marriages occur and dissolve, which drives up the overall divorce rate. When the information is more important, the low remarriage value discourages non-Burakumin marriages from dissolving. That lowers the overall divorce rate. Evidence provided by this article shows that uncertainty can increase and decrease marriage dissolution depending on the importance of the information. In addition, the paper also touches on discrimination in the marriage market. Unlike discrimination in the labor market, the aversion to particular ethnic groups in the marriage market is hardly seen as discrimination. The unique characteristics of Burakumins offer us a glimpse of discrimination in the marriage market.

Governments around the world can decide to restrict access to certain information to protect vulnerable groups. While existing literature argues that such action will no doubt increase the number of divorces, my findings suggest it will depend on how important and relevant the information is. Perhaps the price to pay for equality is not as high as officials originally thought. Lastly, individuals who desire a stable marriage obviously should disclose important and relevant information to their partners.

Chapter 3

Did Big Pharma Influence the Adoption of COVID Vaccine Mandates?

3.1 Introduction

Existing work that examines the influence of campaign contributions on roll call votes finds mixed results. Ansolabehere et al. (2003) find scant evidence that campaign contributions affect roll call voting at the federal level, while Bonica (2018) finds a positive relationship between campaign contributions and roll call votes. By controlling for state lawmakers' characteristics and those of their electoral districts in a micro dataset, I study the issue by examining the relationship between pharmaceutical companies' campaign contributions and state legislators' votes on COVID vaccine legislations. While the lack of positive results in much of the existing work may be due to how costly it is to lobby lawmakers for roll call votes, the potential revenue from the COVID vaccines may provide enough incentive for pharmaceutical companies to do so. On the other hand, the strong politicization of COVID vaccines can further increase the cost of influencing lawmakers. I find no evidence supporting the argument that there is a positive relationship between campaign contributions and roll call votes.

In addition to providing new evidence on the effect of political contributions, understanding how political spending by pharmaceutical companies shapes health policy can also inform discussions on how to keep the influence of industry on U.S. health policy in check. Corporations are known to influence legislation with campaign contributions and lobbying in the U.S. This is especially true for the pharmaceutical industry. From 1999 to 2018, pharmaceutical companies in the country spent \$4.7 billion on those activities. They target senior lawmakers in Congress involved in drafting health care laws and state committees that opposed or supported key referenda on drug pricing and regulation (Wouters, 2020).

State-level COVID vaccine legislation presents an opportunity to study the relationship at a different level of government. Merck had a huge influence on the adoption of the HPV vaccine mandates (Mello et al., 2012). COVID vaccine mandates can lead to more vaccine uptake (Karaivanov et al., 2022). As a result, pharmaceutical companies have an incentive to encourage the adoption of vaccine mandates and to oppose bans on vaccine mandates to increase their sales. With data from Vote Smart, the U.S. Census Bureau, the National Public Service for Legislative Tracking, and the National Institute on Money in State Politics, I construct a cross-sectional microdata set that has information on campaign contributions that state lawmakers received, their roll call votes on COVID vaccine legislations, and their individual and constituent characteristics. Following Gokcekus et al. (2006), I control lawmakers' legislative preferences to study the relationship between pharmaceutical companies' campaign contributions and state legislators' votes on COVID vaccine bills.

I find no significant correlation between receiving contributions from pharmaceutical companies and voting for those companies. This shows that state lawmakers are not more likely to vote in favor of pharmaceutical companies if they receive contributions from those companies. Existing work on information lobbying finds that interest groups use a low amount of political contributions to buy access to lawmakers to lobby them later (Stratmann, 2017). The public discourse on the COVID vaccine mandates is heavily politicized. While it may be common for Democrats to vote for vaccine mandates, it can be extremely costly for Republicans to do the same. Fang (2023) documented how pharmaceutical companies paid third-party groups to advocate for them on vaccine mandates while remaining largely in the background. The insignificant results suggest that pharmaceutical companies likely did not use their lobbyists to change lawmakers' votes on the bills passed in those legislatures. Instead, those companies may have relied more on third-party organizations to push their agenda.

3.2 Background

3.2.1 The COVID Vaccine Mandates in the U.S.

In the US, only state governments have the power to issue vaccine mandates or bans. Bans or mandates can be issued by governors through executive orders. Public health officers, which are usually appointed by governors, also have the authority to issue mandates. State lawmakers can pass legislations to restrict or mandate vaccines as well. A list of bills in all states with such legislations can be found in Table 3.1.

For adults, there are two broad types of mandates: employer mandates (private or public) and proof of vaccination (regulations regarding a centralized system to show proof of vaccination). In November 2021, the administration put a federal private employer mandate in place through OSHA. Described as a workaround, the order was withdrawn in February 2022.

Only Pfizer, Moderna, and Johnson&Johnson manufacture FDA-approved COVID vaccines. Since Moderna and its subsidiaries did not make contributions at the state level, I include only campaign contributions from Pfizer, Johnson&Johnson, and the Biotechnology Innovation Organization (BIO), a trade group for pharmaceutical companies.

Table 3.1: States and Bills

	Year passed	
	(1)	(2)
States&Bills	2021	2022
Alabama	SB267, SB9	
Arizona	SB1824	HB2498, SB1346
Arkansas	HB1547, SB615, SB739	
Connecticut		HB5047
Florida	HB1B, SB2006	
Georgia		SB345
Indiana	HB1405	HB1001
Iowa	HF889	
Kansas	HB2001, SB159	
Michigan	SB82	HB5783
Mississippi		HB1509
Missouri	HB271	HB1606
Montana	HB702	
Nebraska		LB906
New Hampshire	HB220	HB1455, HB1495, HB1604
North Dakota	HB1465, HB1511	
South Carolina		H3126, H5150
Tennessee	HB13, SB858, SB9014	SB1823, SB1982
Texas	SB968	
Utah	HB308, SB2004	HB63
West Virginia	HB335, HB4012	

3.2.2 Theoretical Framework

Ansolabehere et al. (2003) point out that the relatively small amount of campaign contributions may be used to buy access to lawmakers for information lobbying. Kalla and Broockman (2016) find senior lawmakers in the U.S. Congress are 3 to 4 times more likely to make themselves available to their donors with a field experiment. A large body of literature examines how campaign contributions are used to secure access for lobbying. According to the access theory, interest groups make a small amount of campaign contributions to buy access to politicians so that those interest groups can lobby them later. Lohmann (1995) theorize that interest groups pay a strictly positive monetary contribution to a policymaker who has conflicting interests to make their messages credible. Austen-Smith (1995) presents a mechanism in which interest groups make fewer campaign contributions to lawmakers with similar preferences for policy in the lobbying process.

Stratmann (2017) suggests that political contributions mainly influence the early stages of the legislative process rather than roll call votes in the final stage. Because of the publicity revolving around the process of roll call votes, publicly voting for special interest groups against the wishes of constituents can seriously harm lawmakers' chances of reelection. Yet, on certain issues, studies have found campaign contributions to be positively correlated with roll call votes. For instance, Gokcekus et al. (2006) find that federal lawmakers who received campaign contributions from the pharmaceutical industry tend to vote for the industry on the drug re-importation bill. Although most of the lobbying activities likely occur at the early stages of the legislative process, interest groups may also lobby lawmakers whom they have access to for their roll call votes. With hundreds of billions of dollars at stake, it is plausible that pharmaceutical companies may try to influence how state lawmakers vote.

Although Ansolabehere et al. (2003) rejects the idea that campaign contributions may affect lawmakers' roll call votes using data from the federal level, more recent work such as Mian et al. (2010) and Bonica (2018) does find a positive relationship between campaign contributions and roll call votes. Despite a relatively small body of literature examining the relationship at the state level, there is still disagreement among existing work studying the issue. Cann (2007) find that elected judges tend to rule in favor of their donors, while Dow and Endersby (1994) show that lawmakers do not vote in favor of special interest groups that contribute more funding to them.

3.3 Data and Methodology

3.3.1 Data

The cross-sectional micro dataset contains information about each state legislator's vote on COVID vaccine bills, their campaign finances in their most recent election, and their individual and electoral district characteristics in 21 U.S. states. To construct the dataset, I obtain data from multiple different sources. Roth (2022) compiled a list of COVID vaccine legislations in all U.S. states. Legislations on COVID vaccine mandates are divided into two categories: employer mandates (private and state) and proof of vaccination by the author. Based on that list, I retrieve the voting records on those bills from the National Public Service for Legislative Tracking and Data API (Legiscan, 2022). The database has all legislators' voting records on bills in 50 U.S. state legislatures. Data on the campaign financing of lawmakers are retrieved from National Institute on Money in State Politics (2022). The non-profit organization maintains a database of campaign finance data at the state level compiled from candidates' campaign filing records. The data contains legislators' campaign finance information in their most recent election between 2017 and 2020 in 21 U.S. states.

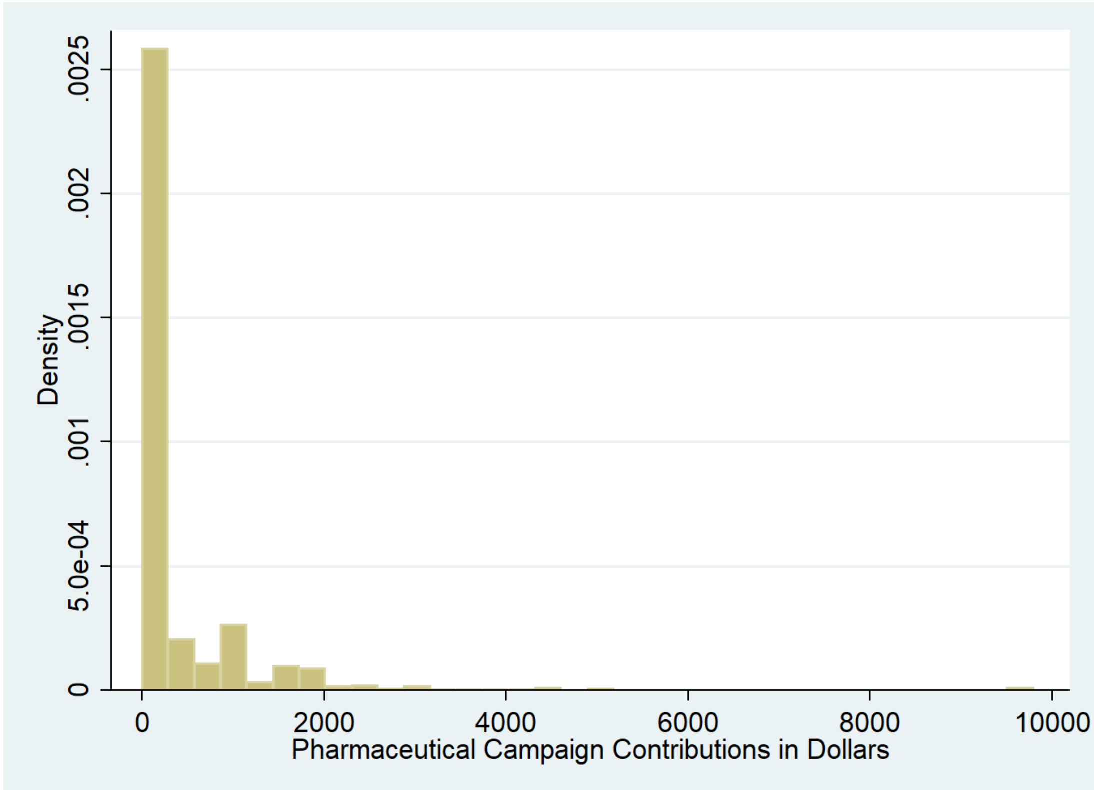
Data on the most recent legislative elections are retrieved from Ballotpedia (2022). The online political encyclopedia covers federal, state, and local politics, elections, and public policy in the United States. For lawmakers' characteristics, I scrap data from Vote Smart (2022). The research organization collects and distributes information on candidates for public office in the United States. Data on the characteristics of electoral districts are retrieved from U.S. Census Bureau (2023). The dataset is composed using data from the American Community Survey. The response rate of the survey is about 80%.

I only include bills that were passed in the legislature. And those bills are mostly bans. Many COVID vaccine mandates or bans were issued via executive orders or public health orders. Some COVID vaccine legislation was killed in the committee or in one chamber of the legislature, while other COVID vaccine mandates or bans were shelved. Contributions made by C4 or C5 non-profit organizations, known as dark money, cannot be tracked. Fang (2021) reported that dark money was being funneled through the Biotechnology Innovation Group in the 2020 election cycle by Pfizer, Moderna, Johnson&Johnson, and other pharmaceutical companies.

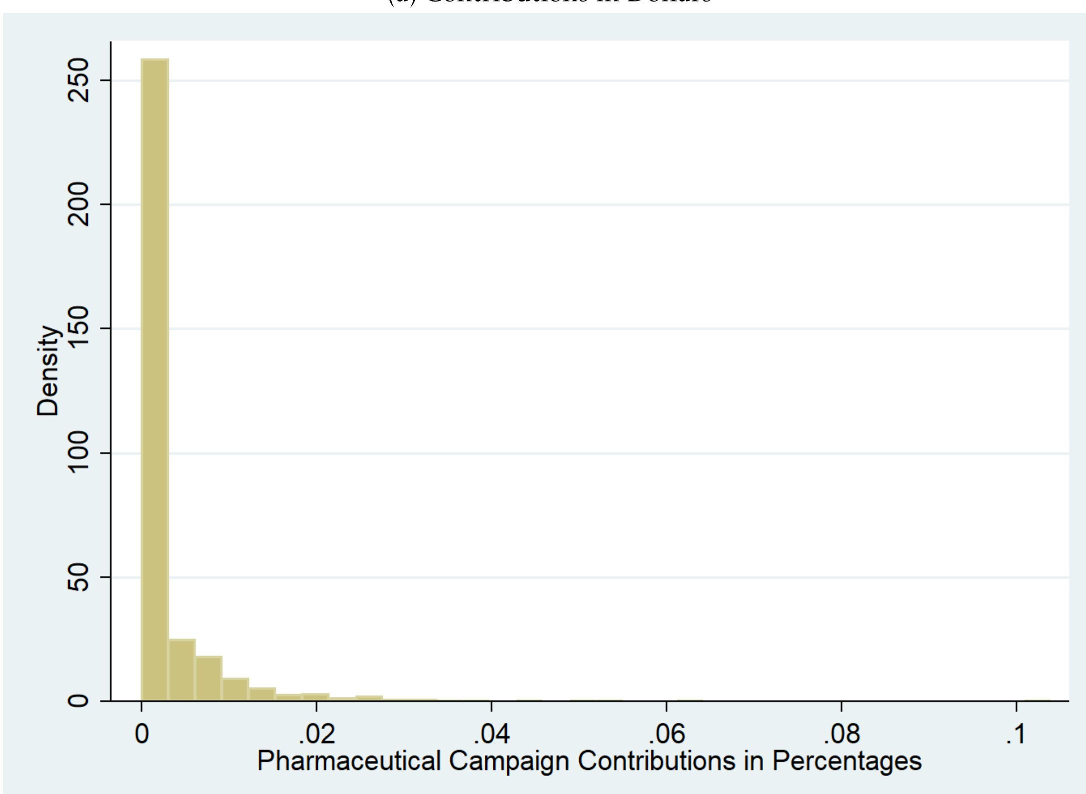
3.3.2 Summary Statistics

Table 3.2 summarizes the variables in the dataset. The state legislators in those 21 states are mostly Republicans and most lawmakers voted against pharmaceutical companies. A higher proportion of those who received campaign contributions from pharmaceutical companies voted in the companies' favor. All but one legislation are bans or restrictions on vaccine mandates. Most legislators were incumbents. Only the state houses of Arizona, New Hampshire, North Dakota, and West Virginia are made up of multiple-seat districts. The numbers of state representatives and senators who received contributions from the pharmaceutical companies were close.

Lawmakers who received campaign funds from pharmaceutical companies also received higher total contributions overall. Additionally, their opponents in the preceding election raised more money. This suggests that pharmaceutical companies likely bought access to lawmakers with relatively small contributions. Figure 3.1 shows the distribution of campaign contributions from pharmaceutical companies. While most lawmakers received no contributions from these companies, up to 10% of some lawmakers' total contributions came from them. Lastly, the characteristics of the legislators and their constituents are similar across both groups.



(a) Contributions in Dollars



(b) Contributions in Percentages

Figure 3.1: Distribution of Campaign Contributions from Pharmaceutical Companies

Table 3.2: Summary Statistics

Variables	(1) No pharma contribution	(2) Received pharma contribution	(3) Total
% voted against mandates	82.7	87.8	83.4
% of Republicans	70.9	80.8	73.7
% of state representatives	72.4	43.5	63.5
% of incumbents	74.8	87.3	78.3
% of male	77.1	77.8	77.3
% married	97.2	96.7	97.1
% with bachelor's degree or above	85.3	89.0	86.4
% with no committee assignment	25.1	30.4	26.6
% with no children	15.0	11.2	13.9
% in single-seat districts	94.2	92.9	93.8
% of male constituents	49.0	49.2	49.1
% of married household among constituents	49.7	51.5	50.2
% Constituents with (mean) bachelor's degree or above	29.3	30.2	29.5
Median age (mean) of constituents	39.6	39.5	39.6
Income of constituents (mean)	$81078	$84134	$81936
Labor force participation (mean) rate among constituents	61.2%	62.1%	61.5%
Unemployment rate (mean) among constituents	3.1%	2.9%	3.0%
Campaign contributions (Mean) from individuals	$45,648	$84,003	$56,413
Pharma (Mean) campaign contributions	$0	$1,196	$336
Total (Mean) contribution received	$147,869	$291,976	$188,315
Spending (Mean) by opponents	$85,445	$111,568	$ 92,776
Obs	**2,240**	**874**	**3,114**

3.3.3 Methodology

Denzau and Munger (1986) find that interest groups generally seek out legislators whose voters are indifferent to the policy that interest groups seek. Austen-Smith (1995) proposes that legislators will be willing to grant access to interest groups whose preferences over consequences are sufficiently close to theirs independent of financial incentives. Lohmann (1995) demonstrates a mechanism in which interest groups are forced to make a strictly positive contribution to buy access from legislators who have different policy preferences. Pharmaceutical companies may likely not need to buy access from lawmakers who want the same policy. The legislators who have the same policy preference as the pharmaceutical companies would have voted for mandates regardless of contributions. So not controlling for lawmakers' preferences can potentially bias the results.

To address the concern that lawmakers' legislative preferences may be correlated with contributions from pharmaceutical companies, I control the characteristics of lawmakers and those of their constituents. In addition to a legislator's ideology, which can be approximated by their political affiliation and personal characteristics, a lawmaker also needs to cater to the preferences of their constituents. As shown in the following equation,

$$Y_{isl} = \alpha_0 + \alpha_1 Contribution_{is} + \gamma X_{is} + State_s + \epsilon_{isl} \tag{3.1}$$

Subscripts i, s, and l represent the legislator, the state, and the legislation, respectively. Y_{isl} is equal to 1 if a legislator voted for the pharmaceutical companies (voting nay for a ban or voting yea for a mandate) on COVID vaccine legislations and 0 in other cases. $Contribution_{is}$ is a dummy variable that is equal to 1 if a state lawmaker received positive contributions from pharmaceutical companies and 0 otherwise. X_{is} is a vector of control variables. It includes the number of seats, average income, the percentage of people holding a bachelor's degree or higher, the percentage of married households, the percentage of males, the labor force participation rate, the unemployment rate, and the median age in a district. It also includes proportions of individual contributions, total contributions received by the officeholder, total contributions received by opponents, the year when a bill is passed, the year of the preceding election, political affiliation, the gender and education level of lawmakers, their committee assignment, whether they have children, and the chamber of the legislature in which they serve. $State_s$ is the state-fixed effect. ϵ_{isl} is the error term.

3.4 Results

3.4.1 Main Results

I examine the effect of pharmaceutical companies' campaign contributions on lawmakers' roll call votes and report the results in Table 3.3. The sample in column 1 contains all legislations related to COVID vaccine mandates, while the samples in columns 2 and 3 contain only legislations related to employer mandates and proof of vaccination respectively. No significant results can be observed in any column.

The relatively small amount of contributions from pharmaceutical companies is not enough to buy lawmakers' votes. However, as shown in Kalla and Broockman (2016), the companies' contributions can certainly buy access. Journalists find that the pharmaceutical industry remained largely in the background on these controversial mandate policies, which faced opposition from a broad array of civil libertarians, labor unions, and community-based groups. Instead, the industry mobilized support for the mandates through third-party organizations to which it typically provided financial support. For instance, Pfizer quietly financed consumer, medical, and civil rights groups to create the appearance of broad support for the mandate (Fang, 2023). The industry likely noticed that showing support for vaccine mandates in roll call votes publicly is costly for Republican lawmakers. The lack of positive results suggests that pharmaceutical companies likely did not directly send their lobbyists after state lawmakers to affect their roll call votes.

Table 3.3: Main Results

Dependent variable: Votes on mandates			
	(1)	(2)	(3)
Variables	All bills	Employer mandates	Proof of vaccination
Received pharmaceutical contributions	-0.011	0.012	-0.030*
	(0.012)	(0.016)	(0.017)
Observations	3,114	1,937	1,757
R-squared	0.453	0.524	0.381
No. of States	21	18	19

Note: The dependent variable is equal to 1 if a lawmaker voted nay for a ban or yea for a mandate and 0 otherwise. The measure of pharmaceutical contributions is a dummy that is equal to 1 if a lawmaker received funds from those companies and 0 otherwise. Results in all columns control for political affiliation, chamber of the legislature sex, education level, marriage status, current role in the legislature, the number of children of a lawmaker, no. of seats, median income, education level, the proportion of males, the proportion of married households, the labor force participation rate, and the unemployment rate in a district, individual contributions, the year when a bill is passed, the total contribution received by the officeholder, election year, and the total spending by opponents. State-fixed effects are also included in all columns.
***p<0.01, ** p<0.05, * p<0.1

3.4.2 Robustness Checks

There are some identification concerns. OLS models may yield less accurate estimates than logit models. To control for district and lawmaker characteristics, I have to discard up to 50% of the observations. In this section, I will use the logit models, use the full sample, and try different continuous measures of contributions from pharmaceutical companies. The results can be found in Table 3.4.

In the second column, I run the regression with a logit model. In column 3, I included no district or lawmaker characteristics to make use of the full sample. In column 4, I measure the contributions from pharmaceutical companies as percentages of total contributions. In the last column, pharmaceutical companies' contribution is measured in thousands of dollars.

As shown in column 2 of Table 3.4, I still find contributions from pharmaceutical companies to have no positive and significant relationship with state lawmakers' roll call votes with the logit model. The result remains unchanged when I drop all the district and lawmaker controls and use the full sample. Lastly, with different measures of campaign contributions in the last two columns, I still find no positive and significant results. In all, the results are robust to various specifications.

The lack of significant results may be due to the large number of small contributions from pharmaceutical companies. To explore this further, I analyze how lawmakers who received sizable contributions from pharmaceutical companies voted on vaccine mandates, as shown in Table 3.5. In the first column, I include only lawmakers who received at least \$1500 from pharmaceutical companies, representing the 75th percentile of non-zero contributions. In the second column, I restrict the sample to lawmakers whose contributions from pharmaceutical companies account for at least 1% of their total campaign contributions, also representing the 75th percentile of non-zero contributions. Despite these restrictions, I still find no significant results in the first two columns.

In the last two columns, I code contributions from pharmaceutical companies as categorical variables. Based on the measure of campaign contributions from pharmaceutical companies, in dollars or as a percentage of the total contribution, I categorize the contributions as follows: 0 for no contributions, contributions between 0 and the 25th percentile (\$500 or 0.3%) of nonzero pharmaceutical contributions are coded as 1 (low); contributions between the 25th and 75th percentiles (\$1500 or 1%) are coded as 2 (medium); and contributions greater than the 75th percentile are coded as 3 (high). Despite this categorization, the results in the last two columns remain insignificant, except for the medium contribution in column 4. However, the negative and significant result disappears when using the Logit model in Table D.3. These results suggest that receiving a significantly larger amount of campaign contributions does not make a recipient more likely to vote in favor of those companies.

Table 3.4: Robustness Checks

Dependent variable: Votes on mandates					
	(1)	(2)	(3)	(4)	(5)
Variables	Baseline	Logit model	No covariates	Relative contribution	Contribution in thousands of $
Received pharmaceutical contributions	-0.011 (0.012)	0.041 (0.189)	0.005 (0.011)		
Pharmaceutical contribution in percentage				-0.122 (0.876)	
Pharmaceutical contributions in thousand dollars					-0.010 (0.007)
Observations	3,114	3,105	6,209	3,114	3,114
R-squared	0.453		0.525	0.439	0.454

Note: The dependent variable is equal to 1 if a lawmaker voted nay for a ban or yea for a mandate and 0 otherwise. In columns 1 to 3, the measure of pharmaceutical contributions is a dummy that is equal to 1 if a lawmaker received funds from those companies and 0 otherwise. In columns 4 and 5, pharmaceutical contributions are measured in percentages and thousands of dollars respectively. Results in all columns except column 3 control for political affiliation, chamber of the legislature, sex, education level, marriage status, current role in the legislature, the number of children of a lawmaker, no. of seats, median income, education level, the proportion of males, the proportion of married households, the labor force participation rate, and the unemployment rate in a district, election year, the year when a bill is passed, individual contributions, and the total contribution received by the officeholder in the previous elections. State-fixed effects are also controlled in all columns.
***p<0.01, ** p<0.05, * p<0.1

Table 3.5: Lawmakers with Sizable Pharmaceutical Contributions

Dependent variable: Votes on mandates				
	Large pharmaceutical contribution		Categorical pharmaceutical contributions	
	(1)	(2)	(3)	(4)
Variables	Received > $1.5k from pharmaceutical companies	> 1% campaign funds from pharmaceutical companies	Converted from dollar contributions	Converted from percentage contributions
Dollar pharmaceutical contributions	-0.004			
	(0.014)			
Percentage pharmaceutical contributions		-1.278		
		(1.978)		
Low (dollars)			0.007	
			(0.020)	
Medium (dollars)			-0.021	
			(0.015)	
High (dollars)			-0.010	
			(0.023)	
Low (percentages)				-0.002
				(0.021)
Medium (percentages)				-0.034**
				(0.016)
High (percentages)				0.018
				(0.021)
Observations	182	215	3,114	3,114
R-squared	0.500	0.581	0.454	0.454

Note: The dependent variable is equal to 1 if a lawmaker voted nay for a ban or yea for a mandate and 0 otherwise. The measure of pharmaceutical company contributions is in thousands of dollars in column 1 and in percentages in column 2. The sample in the first two columns includes only lawmakers who received pharmaceutical company contributions greater than the 75th percentile of nonzero contributions from pharmaceutical companies. In the last two columns, pharmaceutical company contributions are coded as categorical variables: 0 for no contributions, 1 (low) for contributions between 0 and the 25th percentile ($500 or 0.3%), 2 (medium) for contributions between the 25th and 75th percentiles ($1500 or 1%), and 3 (high) for contributions above the 75th percentile. Results in all columns control for the same covariates as in Table 3. State-fixed effects are also controlled in all columns.
*** p<0.01, ** p<0.05, * p<0.1

3.5 The Heterogeneous Effects of Contributions

3.5.1 Political Affiliations and Chambers of Legislatures

The COVID vaccines and the COVID vaccine mandates have been highly politicized in the U.S. While President Trump took credit for the COVID vaccine rollout and rejected vaccine mandates, the Democrats first questioned the safety and efficacy of the vaccines before the election only to actively push for vaccine mandates when President Biden took office. It is rather obvious that the Democrats are largely and strongly for vaccine mandates. Therefore, according to the information lobbying model, pharmaceutical companies should not exert much effort to lobby Democrats who have similar preferences for policy. Splitting the sample by party affiliations allows me to examine the effects of pharmaceutical companies' contributions on lawmakers with different preferences. I split the full sample containing all bills by political affiliations, and report the results of the analysis in Table 3.6. As shown in columns 1 and 2 of Table 3.6, no positive and significant results can be observed in either column.

Since the introduction of term limits in state legislatures, Kousser (2005) have found that representatives of state houses tend to rely more on lobbyists for information compared to their state senate counterparts. According to Kousser (2005), the reason behind the phenomenon is the difference in term lengths between the two chambers. Generally, state senates have a 4-year term and state houses have a 2-year term. State houses are generally filled with newly elected lawmakers and state representatives tend to move into state senates as they become more experienced and hit their term limits. As a result, state representatives and their staff are usually less experienced than their senate counterparts in acquiring information for legislation. The result is state representatives rely on lobbyists more for information than senators. That means state representatives should be more responsive to lobbying than their senate counterparts who have more alternative channels to acquire information. In columns 3 and 4 of Table 3.6, I split the sample by the chamber of the legislature. I still observe no significant results in any of the columns.

Table 3.6: Political Affiliation and Chambers of the Legislature

Dependent variable: Votes on mandates				
	Party		Chamber	
	(1)	(2)	(3)	(4)
Variables	Democrat	Republican	House	Senate
Received pharmaceutical contributions	0.030	0.003	-0.012	-0.023
	(0.037)	(0.009)	(0.017)	(0.019)
Observations	816	2,299	1,978	1,140
R-squared	0.492	0.082	0.434	0.515

Note: The full sample includes all bills and is split by party lines and chamber of the legislature. The dependent variable is equal to 1 if a lawmaker voted nay for a ban or yea for a mandate and 0 otherwise. The measure of Big Pharma contributions is a dummy that is equal to 1 if a lawmaker received funds from those companies and 0 otherwise. Results in all columns control for the same covariates as in Table 3. State-fixed effects are also controlled in all columns.
*** p<0.01, ** p<0.05, * p<0.1

3.5.2 Chronic Conditions and Aging Society

Gallo Marin et al. (2021) find that preexisting comorbidities and old age are strongly correlated with the severity of COVID. The U.S. population is neither healthy nor young. That is especially true for the states with COVID vaccine legislations. See Table 3.7 for the percentage of the population with multiple chronic conditions (MCC)[1] and the percentage of the population aged 65 or above in those states. It is possible that lawmakers in states with a larger proportion of at-risk populations may be more interested in information about the vaccines' efficacy and respond more positively to pharmaceutical companies' pro-mandate messaging.

To determine if legislators are susceptible to pharmaceutical companies' lobbying, I split the sample by the percentage of the population with chronic conditions and by the percentage of the population aged 65 or above. Information on chronic conditions comes from Newman et al. (2020). Data on the proportion of seniors is obtained from the Population Reference Bureau. I report the results in Table 3.8. The sample in column 1 is made up of states where less than 50.8% (50th percentile) of the population have MCC,

[1] An individual considered to have MCC if they reported having 2 or more of the 12 chronic conditions: arthritis, asthma, cancer, chronic obstructive pulmonary disease (COPD), depression, diabetes, heart disease, high blood pressure, high cholesterol, kidney disease, obesity, stroke

and no significant effects can be observed. The result for the states where over 50.8% of the population have MCC is shown in column 2, I still observe no significant effects.

I then split the full sample by the median of the percentage of the population aged 65 or above (17.7%) and report the results in columns 3 and 4 respectively. I find no significant effects in either sample. Results in the table indicate no sign of pharmaceutical companies sending their lobbyists directly to lawmakers to change their votes.

Table 3.7: Chronic Conditions and Seniors

	(1)	(2)
States	% Population with multiple chronic conditions	% Population aged 65 or cbove
Alabama	60.1	17.8
Arizona	50.3	18.5
Arkansas	60.5	17.7
Connecticut	47.7	18.2
Florida	50.4	21.3
Georgia	48	14.7
Indiana	55.7	16.5
Iowa	51.8	17.9
Kansas	50.8	16.8
Michigan	56.7	18.2
Mississippi	57.1	16.9
Missouri	52.9	17.7
Montana	48	19.7
Nebraska	50.1	16.5
New Hampshire	49.4	19.3
North Dakota	50.2	16.1
South Carolina	54.4	18.7
Tennessee	54.9	17.1
Texas	48.5	13.2
Utah	43.7	11.7
West Virginia	64.4	20.9

Table 3.8: Chronic Conditions and Old Age

Dependent variable: Votes on mandates				
	% Population with multiple chronic conditions		% Population aged 65 or above	
	(1)	(2)	(3)	(4)
Variables	$\leq 50th$ Percentile ($\leq$ 50.8%)	$> 50th$ Percentile (> 50.8%)	$\leq 50th$ Percentile ($\leq$ 17.7%)	$> 50th$ Percentile (> 17.7%)
Received pharmaceutical contributions	-0.009 (0.019)	-0.015 (0.016)	-0.030* (0.016)	0.021 (0.019)
Observations	1,037	2,077	1,761	1,353
R-squared	0.599	0.360	0.409	0.539
No. of States	17	4	11	10

Note: The full sample includes all bills. The dependent variable is equal to 1 if a lawmaker voted nay for a ban or yea for a mandate and 0 otherwise. The measure of pharmaceutical contributions is a dummy that is equal to 1 if a lawmaker received funds from those companies and 0 otherwise. Results in all columns control for the same covariates as in Table 3. State-fixed effects are controlled in all columns.
*** p<0.01, ** p<0.05, * p<0.1

Lastly, I also examine how a larger proportion of unhealthy and senior populations, and more COVID deaths are related to lawmakers' votes on COVID-19 vaccine legislation. The data on the COVID death rate by states is retrieved from CDC (2023). As shown in Table D.2 in the appendix, there is no significant relationship between a large proportion of residents with MCC and votes for mandates. A higher percentage of the senior population is positively and significantly related to votes for mandates. A one percentage point increase in the percentage of the population aged 65 or above in a state is associated with a 1.2 percentage point increase in a lawmaker's chance of voting for mandates. Furthermore, 1 additional death per 100,000 people in 2020 is associated with a 0.1 percentage point lower chance for lawmakers to vote for mandates. While having a senior population who are at a higher risk of dying from COVID may put pressure on lawmakers to vote for mandates, a higher death rate in 2020 can translate into fewer at-risk populations which allows lawmakers to be less concerned about mandates.

3.6 Conclusion

This paper examines the relationship between pharmaceutical companies' campaign contributions and U.S. state lawmakers' votes on COVID vaccine mandates. I find that receiving pharmaceutical companies' campaign contributions appears to have no relationship with state legislators voting for COVID vaccine mandates. Furthermore, even when restricting the sample to include only Republicans who control the legislatures or state representatives who rely more on lobbyists for information, I still find no significant effects. Lawmakers who received funding from pharmaceutical companies in states with a larger at-risk population are also not more likely to vote in pharmaceutical companies' favor. Since these companies only contributed a relatively small amount to lawmakers, pharmaceutical companies' political contributions were likely used to buy access to state lawmakers, as documented by existing literature. The politicization of vaccine mandates likely made it costly for Republicans to publicly vote for pharmaceutical companies and forced Democrats to do the opposite. Given this, the pharmaceutical industry likely did not exert much effort to directly lobby for state legislators' roll call votes. Instead, the industry stayed in the background and used third-party organizations to advance its agenda (Fang, 2023). The absence of a positive relationship serves as evidence supporting this theory. Lastly, despite the limitations of this paper, it appears that politicization can make roll call votes costly enough for interest groups to influence.

Mandating the new medical product may have unintended consequences. It may erode vaccine confidence and civil liberties (Bardosh et al., 2022a). We now know that COVID booster mandates can cause a net increase in hospitalization in healthy young adults (Bardosh et al., 2022b). It is not surprising that the J&J COVID vaccine was pulled off the market and that the mRNA vaccines may be associated with more harm than initially estimated at the time of emergency authorization (Fraiman et al., 2022). The potentially serious consequences make it important to understand the extent of pharmaceutical companies' influence on the adoption of those mandates. My work adds to the large body of literature that seeks to understand how the industry operates in the political realm.

While this paper finds scant evidence of pharmaceutical companies directly influencing state lawmakers' roll call votes on COVID vaccine mandates with their lobbyists, those corporations may have engaged in other forms of influence campaigns. Perhaps the pharmaceutical companies engaged in a hybrid type of lobbying using multiple channels, including third-party organizations. The mostly Republican-controlled legislatures should have enough votes to pass bans on private employer mandates, which are likely to be the most effective policy at increasing vaccine uptake, none of the states passed such bills. It is possible that the industry focused primarily on defeating bans on employer mandates while allowing less restrictive bills to pass in the legislatures at the state level. More work should be done to study pharmaceutical companies' influence campaigns during the COVID era.

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Appendix A

First Stage Results Tables for Chapter 1

Table A.1: Conscription Lengths and Income

	Instruments		
	(1)	(2)	(3)
Variables	No. of NK provocation events		
6-year moving average	0.309***		
	(0.089)		
5-year moving average		0.257***	
		(0.076)	
4-year moving average			0.165**
			(0.074)
Age	0.248***	0.264***	0.291***
	(0.029)	(0.024)	(0.023)
Log military spending as % of GDP	-0.525	-0.838	-1.515**
	(0.840)	(0.703)	(0.672)
Level of education (schooling)	0.213*	0.210*	0.205*
	(0.116)	(0.117)	(0.117)
Work status (wage/ non-wage)	0.088	0.091	0.085
	(0.197)	(0.196)	(0.196)
Age conscripted	-0.424***	-0.437***	-0.457***
	(0.073)	(0.073)	(0.073)
F-statistic	12.05	11.29	4.964
Observations	31,226	31,230	31,230

Note: region, year, and all job fixed effects are omitted. *** p<0.01, ** p<0.05, * p<0.1

Table A.2: Mean Income and Duration of Conscription

	Instruments		
	(1)	(2)	(3)
Variables	No. of NK provocation events		
6-year moving average	0.525***		
	(0.084)		
5-year moving average		0.463***	
		(0.077)	
4-year moving average			0.379***
			(0.075)
Age	0.133***	0.140***	0.150***
	(0.021)	(0.021)	(0.022)
Log military spending as % of GDP	2.655***	2.554***	2.287***
	(0.559)	(0.521)	(0.551)
Age conscripted	-0.142**	-0.144**	-0.142**
	(0.057)	(0.057)	(0.057)
F-statistic	39.47	36.61	25.68
Observations	4,297	4,300	4,300

Note: education-level fixed effects are omitted. *** p<0.01, ** p<0.05, * p<0.1

Table A.3: Parsimonious Models

	Instruments		
	(1)	(2)	(3)
Variables	No. of NK provocation events		
6-year moving average	0.309***		
	(0.078)		
5-year moving average		0.239***	
		(0.064)	
4-year moving average			0.161***
			(0.059)
Age	0.311***	0.332***	0.356***
	(0.029)	(0.024)	(0.023)
Log military spending as % of GDP	-3.579***	-4.025***	-4.619***
	(0.812)	(0.683)	(0.657)
Level of education (schooling)	0.581***	0.577***	0.573***
	(0.151)	(0.151)	(0.151)
Work status (wage/ non-wage)	1.028	1.032	1.028
	(0.693)	(0.683)	(0.681)
Age conscripted	-0.701***	-0.719***	-0.739***
	(0.092)	(0.091)	(0.091)
F-statistic	15.59	13.95	7.547
Observations	32,729	32,733	32,734

Note: region, year, industry, and job fixed effects are omitted.
*** p<0.01, ** p<0.05, * p<0.1

Table A.4: Results by Education Level

	High school or below	Post-secondary or above
	(1)	(2)
Variables	No. of NK provocation events	
5-year moving average	0.220**	0.329***
	(0.0890)	(0.0891)
Log military spending as % of GDP	-1.900*	0.201
	(1.142)	(1.007)
Level of education (schooling)	1.691***	-0.111
	(0.457)	(0.224)
Work status (wage/ non-wage)	0.249	-0.217
	(0.329)	(0.220)
Age conscripted	-0.441***	-0.377***
	(0.107)	(0.0972)
Age	0.331***	0.222***
	(0.0392)	(0.0364)
F-statistic	6.117	13.54
Observations	13,959	17,271

Note: region, year, and all job fixed effects are omitted.
*** p<0.01, ** p<0.05, * p<0.1

Table A.5: Results by Type of Jobs

	Office	Non-office
	(1)	(2)
Variables	No. of NK provocation events	
5-year moving average	0.315***	0.238***
	(0.085)	(0.076)
Log military spending as % of GDP	0.877	-1.991**
	(0.864)	(0.825)
Level of education (schooling)	-0.121	0.567***
	(0.126)	(0.177)
Work status (wage/ non-wage)	-0.028	0.557*
	(0.379)	(0.303)
Age conscripted	-0.383***	-0.450***
	(0.094)	(0.099)
Age	0.194***	0.315***
	(0.029)	(0.033)
F-statistic	13.76	9.904
Observations	14,296	15,733

Note: region, year, and all job fixed effects are omitted.
*** p<0.01, ** p<0.05, * p<0.1

Table A.6: Conscription Lengths and Employment Status

	Instruments		
	(1)	(2)	(3)
Variables	No. of NK provocation events		
6-year moving average	0.311***		
	(0.089)		
5-year moving average		0.259***	
		(0.077)	
4-year moving average			0.167**
			(0.074)
Log military spending as % of GDP	-0.488	-0.808	-1.488**
	(0.842)	(0.703)	(0.673)
Level of education (schooling)	0.214*	0.211*	0.207*
	(0.116)	(0.117)	(0.116)
Age conscripted	-0.422***	-0.435***	-0.456***
	(0.073)	(0.073)	(0.073)
Age	0.247***	0.263***	0.291***
	(0.029)	(0.024)	(0.023)
F-statistic	12.33	11.44	5.070
Observations	31,226	31,230	31,230

Note: region, year, and all job fixed effects are omitted. *** p<0.01, ** p<0.05, * p<0.1

Table A.7: Conscription Lengths and Educational Attainment

	Post-secondary degree or above		Post-graduate degree	
	(1)	(2)	(3)	(4)
Variables	No. of NK provocations events			
6-year moving average	0.304***		0.304***	
	(0.088)		(0.088)	
5-year moving average		0.254***		0.254***
		(0.077)		(0.077)
			(0.074)	
Log military spending as % of GDP	-0.428	-0.736	-0.428	-0.736
	(0.858)	(0.720)	(0.858)	(0.720)
Work status (wage/ non-wage)	0.095	0.098	0.095	0.098
	(0.196)	(0.195)	(0.196)	(0.195)
Age conscripted	-0.400***	-0.413***	-0.400***	-0.413***
	(0.073)	(0.073)	(0.073)	(0.073)
Age	0.237***	0.253***	0.237***	0.253***
	(0.029)	(0.024)	(0.029)	(0.024)
F-statistic	13.25	12.78	13.25	12.78
Observations	31,228	31,232	31,228	31,232

Note: region, year, and all job fixed effects are omitted. *** p<0.01, ** p<0.05, * p<0.1

Appendix B

Model Derivation for Chapter 2

For a majority, the value of being unattached at the beginning of the second period is,

$$V_M(\alpha,\pi) = Y + \alpha(Y - \pi C) \tag{B.1}$$

While for a Burakumin [1], the value is,

$$V_B(\alpha) = Y + \alpha Y \tag{B.2}$$

At the end of the first period, a majority-majority marriage will dissolve if,

$$2Y + \theta < V_M \Rightarrow \theta < \alpha Y - Y - \alpha\pi C \tag{B.3}$$

A Burakumin-Burakumin marriage will dissolve if,

$$2Y + \theta < V_B \Rightarrow \theta < \alpha Y - Y \tag{B.4}$$

For intermarriages, the Burakumin partner will wish to divorce if,

$$2Y + \theta < V_B \Rightarrow \theta < \alpha Y - Y \tag{B.5}$$

the majority partner will find out about their partner's identity and wish to divorce if,

$$2Y + \theta - C < V_M \Rightarrow \theta < \alpha Y - Y + (1 - \alpha\pi)C \tag{B.6}$$

Since

$$\alpha Y - Y < \alpha Y - Y + (1 - \alpha\pi)C \tag{B.7}$$

when

$$\theta < \alpha Y - Y \tag{B.8}$$

both partners will want to divorce. If

$$\theta > \alpha Y - Y + (1 - \alpha\pi)C \tag{B.9}$$

[1] Recall that the interracial marriage is only costly for the majority partner

then no partner will wish to divorce.

The majority partners wish to divorce if

$$\alpha Y - Y < \theta < \alpha Y - Y + (1 - \alpha\pi)C \tag{B.10}$$

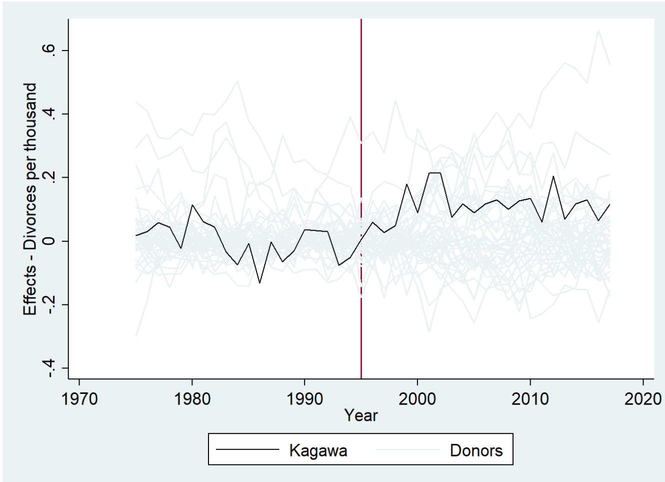
And thus, when

$$\theta < \alpha Y - Y + (1 - \alpha\pi)C \tag{B.11}$$

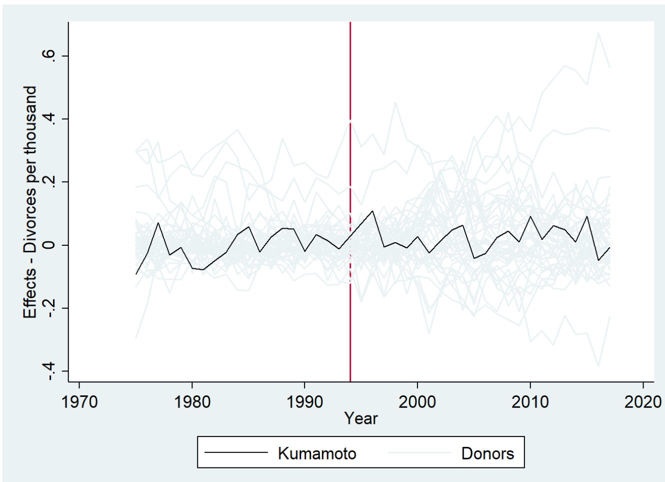
intermarriages will dissolve as the majority dominates in this case.

Appendix C

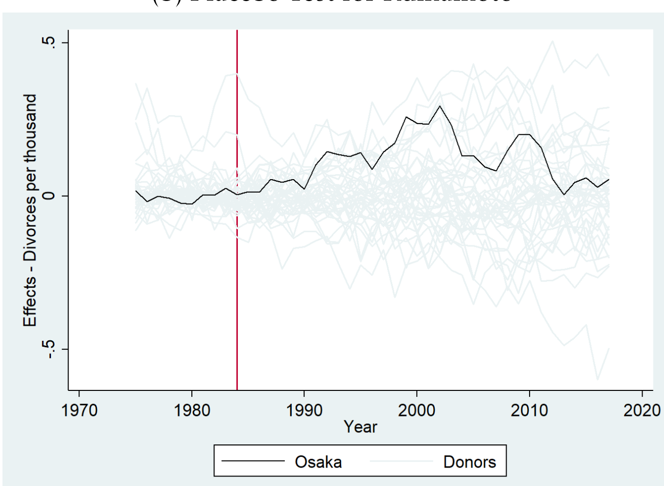
Placebo Tests for Synthetic Control for Chapter 2



(a) Placebo Test for Kagawa

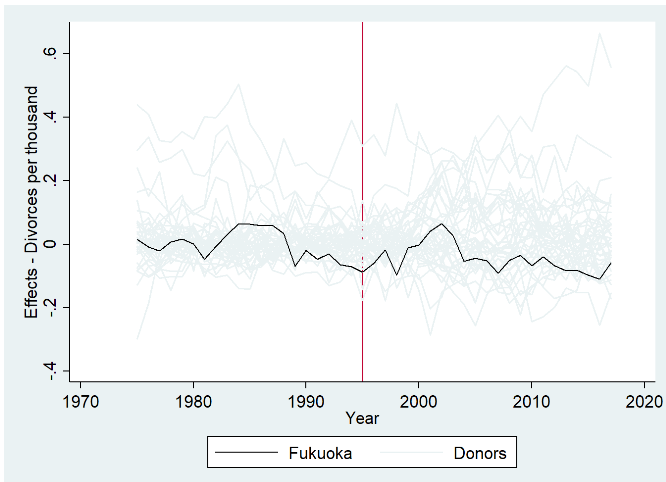


(b) Placebo Test for Kumamoto

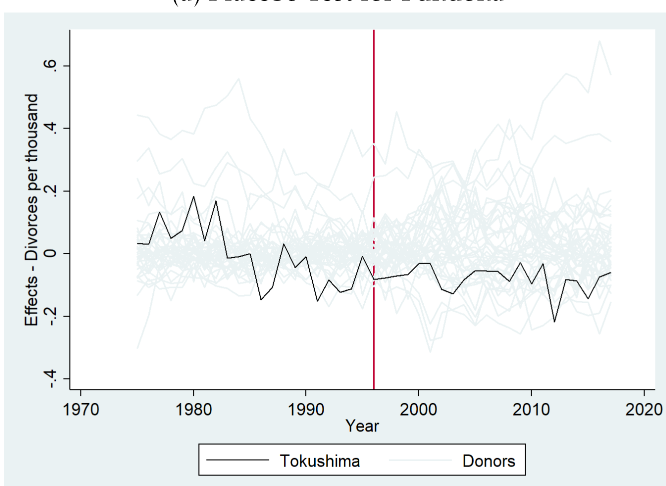


(c) Placebo Test for Osaka

Figure C.1: Placebo Tests for Small Burakumin Presence



(a) Placebo Test for Fukuoka



(b) Placebo Test for Tokushima

Figure C.2: Placebo Tests for Large Burakumin Presence

Appendix D

Additional Results for Chapter 3

Table D.1: COVID Mortality by State

States	(1) Age-adjusted COVID death per 100,000
Alabama	152.8
Arizona	139.5
Arkansas	127.7
Connecticut	56.7
Florida	111.7
Georgia	135.9
Indiana	106.8
Iowa	75.9
Kansas	103.1
Michigan	107.1
Mississippi	146.3
Missouri	100.5
Montana	108.8
Nebraska	69
New Hampshire	60.2
North Dakota	70.9
South Carolina	71.2
Tennessee	142.5
Texas	151.4
Utah	78.2
West Virginia	146.8

Table D.2: Old age, chronic diseases, and COVID deaths

Dependent variable: Votes on mandates			
	(1)	(2)	(3)
Variables	Chronic conditions	Old age	COVID deaths
% with MCC	-0.215		
	(0.136)		
% aged 65 or above		0.012***	
		(0.003)	
Age-adjusted deaths per 100,000			-0.001***
			(0.000)
Observations	3,114	3,114	3,114
R-squared	0.399	0.401	0.402

Note: The dependent variable is equal to 1 if a lawmaker voted nay for a ban or yea for a mandate and 0 otherwise. The measure of pharmaceutical contributions is a dummy that is equal to 1 if a lawmaker received funds from those companies and 0 otherwise. All columns contain 21 states. The control variables are the same as Table 3.3.
***p<0.01, ** p<0.05, * p<0.1

Table D.3: Categorical Campaign Contributions with Logit Model

Variables	(1) Categorical variable (Dollars)	(2) Categorical variable (Percentage)
Low (dollars)	0.105	
	(0.295)	
Medium (dollars)	-0.177	
	(0.230)	
High (dollars)	0.249	
	(0.348)	
Low (percentages)		-0.022
		(0.319)
Medium (percentages)		-0.256
		(0.241)
Higj (percentages)		0.373
		(0.294)
Observations	3,105	3,105

Note: The dependent variable is equal to 1 if a lawmaker voted nay for a ban or yea for a mandate and 0 otherwise. Only the Logit model is used in this table. In both columns, pharmaceutical company contributions are coded as categorical variables: 0 for no contributions, 1 (low) for contributions between 0 and the 25th percentile ($500 or 0.3%), 2 (medium) for contributions between the 25th and 75th percentiles ($1500 or 1%), and 3 (high) for contributions above the 75th percentile. Results in all columns control for the same covariates as in Table 3. State-fixed effects are also controlled in all columns.