DOCTORAL THESIS

Three Essays in Health Economics

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Abstract

Insurance and education are some of important determinants of health. To understand how these determinants affect health, we leverage multiple policies across the globe to estimate their impacts on various health and health behavior. The first part of the thesis leverages a natural experiment in the United States in which the oral anticancer drugs are offered at the same price as the intravenous anticancer drugs by law. The second chapter investigates the policy which offers free smoking-cessation aids in Canada and examines its effect on tobacco use. The final part examines the relationship between college education and health behavior in Japan using a mismatch between the Japanese school year and the Firehose calendar year.

The first chapter investigates the impacts of anticancer drug parity laws on mortality rates in the United States using a difference-in-differences approach. Using data from 2004 to 2017 Detailed Mortality Files, we show that the anticancer drug parity laws reduce the mortality rate for head/neck malignant cancers but have no impact on malignant cancers of other types. We also rule out an insurance expansion channel that may influence the relationship between anticancer drug parity laws and malignant cancer mortality. Our results are robust to various specifications and falsification tests. Our findings imply that providing equal access to oral anticancer drugs is an effective tool for the prevention of premature mortality.

The second chapter explores the impact of smoking-cessation aids (SCA) coverage on tobacco use outcomes in Canada. In clinical trials, SCAs have proven to be effective at improving the odds of smoking cessation. Because of the effectiveness of SCAs in these settings, many countries have adopted the coverage of SCAs to reduce tobacco use. However,

the effect of such coverage on tobacco use is ambiguous. On one hand, the coverage may have the intended effect and reduce tobacco use. On the other hand, the coverage may cause beneficiaries to participate in tobacco use more as the drug coverage protects beneficiaries from future costs associated with tobacco use. To understand the effect of SCA coverage, we examine it using 2008–2012 Canadian Tobacco Use Monitoring Survey and a difference-in-differences approach. We find that SCA coverage increases cigarette and cigarillo use. Moreover, the effect of SCA coverage on tobacco use is stronger in men, in those with at least a college education, and those who are younger. Our results point to the unintended consequences of the coverage of SCAs on tobacco use.

For the final chapter, we investigate the casual effect of college education on smoking, drinking, sleeping, and cancer screening behavior in Japan. To estimate the casual effect, we leverage a unique natural experiment that occurred in Japan in 1966, the Firehorse Superstition. Japanese believe women born under this superstition has a difficult personality, leading to parental child rearing avoidance. This results to a decline in number of children born, leading to declines in college competition and classroom size for earlier education for those born in the Firehorse year. To avoid selection bias, we leverage the educational institution setting in Japan, which the new school year begins at April of each year. This leads to a mismatch between Japanese calendar year and school year in 1967, which is used as an instrument for college education. Using 2013 and 2016 Comprehensive Living Condition Surveys, we find that a longer year of college education is associated with a reduction in smoking and drinking and an improvement in sleeping and using cancer screening. We also explore the heterogeneity across gender and find that women drive the casual relationship between college education and health behavior in Japan. Finally, we found that the causal relationship between college education and health behavior is driven by better contract and

promotion in the labor market. Our findings show that education policies may not only improve labor market outcomes but also health behavior outcomes.

The paper that make up each chapter is as follows: first chapter is published in *Social Science & Medicine*, which is entitled as "Impact of Anticancer Drug Parity on Mortality Rates". The second chapter is published in Health Economics, which is entitled as "The Effect of Coverage of Smoking Cessation Aids on Tobacco Use Outcomes: Evidence from Canada." And the final chapter is yet to be published, and it is entitled as "Does College Education Make Us Act Healthier? Evidence from a Japanese Superstition."

Preface

The first chapter was coauthored with Professor Haruko Noguchi and was published in *Social Science & Medicine*. Similarly, the second chapter was coauthored with Professor Haruko Noguchi but published in *Health Economics*. The final chapter was coauthored with Professor Haruko Nogichi and Rong Fu. I was responsible for analyses and drafting of the manuscript at all stages

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Introduction

Grossman (1972) first formulates the idea that health can be thought of as a durable capital good, which can be used to produce healthy time that can be allocated to market and non-market activities. Health naturally depreciates with age, but appreciates when one invests in it and vice versa. Examples of investment include but not limited to education, insurance, exercise, and smoking. A vast literature has explored these investment behaviors showing that these behaviors indeed have casual effect on health.

To explore how these investment behaviors can affect health through policies and/or natural experiments, my two first chapter examines how insurance can affect health directly and indirectly through health behavior. Chapter one investigates the effect of anticancer drug insurance parity laws on cancer mortality rates in the United States (US). Chapter two explores the impact of coverage of smoking cessation aids on tobacco use behavior in Canada. Following the first two chapters, we shifted my attention away from insurance to education and explore how it can affect health through health behavior. Chapter three examines the effect of college education on alcohol, tobacco, sleeping, and cancer screening use in Japan.

Chapter one investigates the effect of anticancer drug insurance parity laws on cancer mortality in the US. The anticancer drug insurance parity laws are laws which equalized the payment scheme for private insurance between anticancer drugs offered in medical setting and same drugs offered as prescription. In particular, there is a significant gap in insurance coverage between intravenous anticancer drugs (IADs) and oral anticancer drugs (OADs). This gap is defined by the higher copayment, coinsurance, or out-of-pocket payments associated with using OADs rather than IADs. This is due to OADs being covered under the pharmacy benefit, which covers any drugs not administered in a medical setting, whereas

IADs are covered under the medical benefit, which customarily covers any drugs administered in a medical setting (Dusetzina et al., 2014a; Fitch et al., 2010; Hede, 2009; Kircher et al., 2016; Shen et al., 2014). Generally, pharmacy benefits have a tier-based copayment structure, which increases copayments, coinsurance, or out-of-pocket payments with or without an annual maximum allowable fee for drugs in the higher tiers, while medical benefits only have a single fixed copayment and an annual maximum allowable fee across all drug types. Such a disparity in coverage would lead to patients unable to afford these drugs (Dusetzina et al., 2014b; Streeter et al., 2011).

Offering parity to coverage of OADs would significantly improve access to these drugs; thus, reduce cancer mortality rates through the use of combination therapies (combining OADs with other drug and treatments), use of higher efficiency treatment specific type of cancer, and increase adherence to the drugs. To understand the effect of anticancer drug parity laws on mortality rates, we leverage data from 2004–2017 Detailed Mortality Files from the US. We extract malignant cancer death by using International Classification of Diseases-10 Codes. We further extract demographic and population information from Census and Current Population Survey – Annual Social and Economics Supplement of the same year as my main data source. Given we are estimating the effect of anticancer drug parity laws, we use the state-level variation in implementation timing of the laws and a difference-in-difference approach to estimate the effect.

Overall, the results suggest that the laws significantly reduce cancer mortality rates, specifically the head/neck cancer. The laws have no effect on other subtypes of cancer. We also implement an event study model to examine the pretrends. We show that the estimates are statistically insignificant suggesting limited impact of pre-existing. Therefore, it suggests that the laws do indeed have significant impacts on mortality, not driven by the natural

decline in mortality rates. Finally, we explore an alternative hypothesis regarding the potential effect of insurance policy on insurance expansion. In this hypothesis, the parity laws could increase insurance coverage rate as people know that the insurance covered OADs. The mortality effect of laws is driven not by increase access to the drug but an increase in coverage of insurance. we find that the insurance coverage does not increase after the laws suggesting that it is the increase in access to the OADs, which lead to a decline in cancer mortality rates. The results suggest that offering better access to drugs could have potential live saving effect to policymakers.

Chapter two investigates the effect of insurance coverage of smoking cessation aids (SCAs) on tobacco use outcomes in Canada. SCAs are drugs that reduce withdrawal symptoms by moderating the symptoms of irritation and mood disorders (bupropion and varenicline). Several clinical trials have highlighted the effectiveness of these drugs in improving smoking cessation (Jorenby et al., 1999; Aubin et al., 2004; Wagena et al., 2005; Cinciripini et al., 2013). Specifically, Hughes et al. (2014) reviewed the existing evidence from clinical trials and found that treatment by bupropion significantly increases the six-month smoking abstinence by 62% more than placebo treatment. Due to the effectiveness of SCAs in clinical settings, many countries have begun to implement insurance coverage of SCAs.

To explore how the insurance coverage of SCA can affect tobacco use behavior, we use 2008–2012 Canadian Tobacco Use Monitoring Survey. We leverage the provincial-variation in coverage timing of SCA and a difference-in-difference approach to estimate the effect of SCA coverage on tobacco use outcomes. We generate both traditional and non-traditional tobacco use outcomes as binary variables. The traditional outcome is cigarette use, and non-traditional tobacco use outcomes are cigarillo, cigar, pipe, and tobacco chew uses. We also explore the intensive use of cigarette by defining a binary variable as a person using cigarette

occasionally versus daily. We control for demographic information and other provincial-level tobacco use policies in all my models.

Based on the results, we find that SCA coverage *increases* tobacco use, instead of reducing them. In particular, we find that SCA coverage increases cigarette and cigarillo uses after the implementation of the coverage. This is in contrast with most of the existing literature, which suggests that insurance coverage does not increase substance uses. Moreover, we explore the heterogeneity across different subpopulation. We find that SCA coverage increases tobacco use outcomes for men and college-educated people. Finally, we examine the heterogeneity in drug coverage. We find that the provinces with existing coverage of other types of SCA increase tobacco use. This suggests that knowledge is significant mediator in the relationship between SCA coverage and tobacco use, as our heterogeneity analysis on college-educated demonstrated. Overall, my results suggest that policy makers wishing to cover SCA to reduce tobacco use may need to consider complimentary policies to alleviate the potential adverse consequences associated with the coverage of these drugs.

Chapter three examines the effect of college education on health behavior using a mismatch from the Firehose Year. The Firehorse (FH) is a superstition in which Japanese people believe that women born under this sign has a difficult personality. As a result of a difficulty personality, Japanese men generally avoid marrying women born in this year, leading to marriage discrimination. Because of this, people generally avoid having children in the FH year. That is, there is a significant decline in fertility rates in 1966 due to parental childrearing avoidance behavior. This leads to a significant decline in classroom size and competition for college enrollment for those born in 1966. However, as mentioned previously, the superstition is associated with marriage-related discrimination, which may correlate with health behavior. The caveats using this superstition to estimate the causal

relationship between education and health behavior would introduce significant biases stemming discriminations. To alleviate the biases, we leverage the Japanese education institution in which school year begins at April of each year. Using this setting, a mismatch is generated in that those born between January and March of 1967 is sorted along with the FH school cohort but does not experience the same discrimination as those born in the FH. We use this mismatch as an instrument to estimate the casual relationship.

The data that we am using is 2013 and 2016 Comprehensive Living Condition Survey. To estimate the causal relationship, we estimate the model using two-stage least square (2SLS) approach. The instrument is an interaction term between a binary variable equals to one if a person is born in 1967 and zero otherwise and a binary variable equals to one if a person is born between January and March and zero otherwise. We *exclude* those born in 1966 due to the biases arising from the FH superstition. We also control for demographic information and birth year and month fixed effect to control unobserved characteristics. For the dependent variables, we use smoking, alcohol, sleeping, and cancer screening behavior. We generate all the variables as binary variables. The main independent (endogenous) variable of interest is a continuous variable indicating years of college education.

Overall, we find that a longer year of college education decreases smoking and alcohol use behavior. It has no effect on sleeping behavior. We also find that longer years of college education increases cancer screening use, especially breast and ovarian cancer screening. Exploring the heterogeneity across gender, we find that the results are primarily driven by women. This suggests that women may be exposed to health knowledge regarding the effect of their behavior on the health of their offspring in college, which leads them to reduce (and improve) their health behavior. Finally, we also conduct multiple tests to validate the validity of the instrument, suggesting that my instrument is plausibly exogenous. In sum, my results

suggest that higher education has significant health benefit implying that policymakers considering implementation of educational policies cannot ignore the additional benefits on health from additional education.

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Chapter 1. Impacts of Anticancer Drug Parity Laws on Mortality Rates

1. 1. Introduction

Cancer is the second leading cause of death in the United States (Siegel et al., 2018). In 2015, the total earnings lost due to cancer mortality were approximately USD 94.4 billion (Islami et al., 2019). Therefore, policies that improve access to anticancer therapies can potentially yield large socioeconomic benefits through the reduction of premature cancer mortality. For instance, improved insurance coverage could potentially reduce cancer mortality through increased access to anticancer therapies (Rosenberg et al., 2015). Drug therapy is one such approach that has garnered significant attention. It involves the utilization of intravenous anticancer drugs (IADs) and oral anticancer drugs (OADs) to treat cancer when a tumor has been removed by surgery or other therapies or has spread to other parts of the body.

However, previous literature on the impact of drug insurance on health has been empirically inconclusive. For example, Huh and Reif (2018), Dunn and Shapiro (2019), Diebold (2016), and Wang et al. (2015) highlighted the beneficial health impact of drug insurance (such as Medicare Part D, which covers prescription drugs) and found that such insurance improves health outcomes. Conversely, Liu et al. (2011), Kaestner et al. (2019), and Khan et al. (2007) found that drug insurance had no discernable impact of drug insurance on health outcomes. As such, the impact of drug insurance on health outcomes remains an open issue.

To address this issue, we exploit the state-level policies under which the insurance costs of OADs and IADs are equalized to improve access to OADs in the US. These policies are referred to as anticancer drug parity laws. Previous literature has shown that these laws have significant and modest impacts on out-of-pocket costs for patients using OADs (Dusetzina et

al., 2018). Specifically, Dusetzina et al. (2018) demonstrated that the parity laws reduce the costs of OADs for patients and double the probability of patients receiving OADs at no costs. In other words, the parity laws improve cancer patients' accessibility to OADs. This, in turn, may improve patients' chances of survival by providing them access to novel oral drug therapies, combination therapies with more efficient treatments, and better drug adherence (Batson et al., 2017; Hershman et al., 2011; Maemondo et al., 2010; Motzer et al., 2009; O'Shaughnessy et al., 2002; Vokes et al., 1989; Zhou et al., 2011). Through these channels, a state implementing anticancer drug parity laws may experience a reduction in cancer mortality rates.

Our study is related to two strands of literature concerning the impact of insurance coverage on health outcomes. The first strand concerns the impact of parity laws on treatment utilization and health outcomes (Buckles, 2013; Klick & Markowitz, 2006; Lang, 2013; Popovici et al., 2017; Schmidt, 2007), while the second strand refers to the impact of drug insurance on health outcomes (Dunn & Shapiro, 2019; Kaestner et al., 2019; Khan et al., 2007; Huh & Reif, 2017; Liu et al., 2011; Wang et al., 2015). Prior literature on parity laws—for example, Lang (2013) and Popovici et al. (2017) —shows that such laws improve access to treatment utilization, which spills over to the overall population health. However, previous studies on parity laws have focused on mental health and infertility. As a result, we know little about the impact of non-mental health and non-infertility parity laws. Furthermore, past studies on drug insurance are limited to public programs such as Medicare Part D, which means there is limited evidence on the impact of private drug insurance on health outcomes. Our study fills in these gaps in the literature and provides the first analysis of the causal impact of anticancer drug parity laws on health outcomes.

The paper is organized as follows. Section 2 discusses the background of OADs with regard to anticancer drug parity laws and reviews the literature on the parity laws. Section 3 describes the estimation strategies and data sources. Section 4 presents the estimation results. Section 5 concludes the paper and discusses policy implications.

1.2. Background and Previous Literature

1.2.1. Background

Anticancer drugs, regardless of whether they are oral anticancer drugs (OADs) or intravenous anticancer drugs (IADs), can be categorized into two types: chemotherapy and targeted therapy drugs. Chemotherapy drugs kill cancer cells by targeting their cell replicating mechanisms (Olsen et al., 2019). Although OADs can involve chemotherapy drugs, the majority of chemotherapies are IADs. The difference lies in from the method of administration (oral vs intravenous) with minor differences in functionality between the two. Targeted therapy drugs annihilate cancer cells by targeting a specific cellular mechanism such as a gene, a protein, or antigens (Carrington, 2015; Olsen et al., 2019). The targeted therapy drugs of OADs include but are not limited to small molecule drugs and endocrine therapy drugs. For IADs, targeted therapy drugs are generally monoclonal antibodies. The functionalities of targeted therapies of OADs and IADs may differ, as the mechanisms they affect differ significantly. Specifically, small molecule drugs generally inhibit certain cellular mechanisms such as protein signaling, whereas monoclonal antibodies bind to cancerous cells, causing the immune system to attack them. Unfortunately, these drugs have side effects resulting from the fact that non-cancer and cancer cells share similar cellular mechanisms in replication and function (Milata et al., 2019; Olsen et al., 2019). The drugs are thus highly regulated because of these side effects.

Oral anticancer drugs have several advantages IADs. First, OADs are significantly more convenient to administer than IADs (Aisner, 2007). The latter require an infusion pump to administer the drugs into the body through the bloodstream, while OADs are pills that are taken directly. Second, clinical trials have shown that some OADs have higher treatment efficiencies, which contributes to better patient survival rates. For example, OADs, such as Gefitinib, Erlotinib, and Sunitinib, have been shown to improve the overall survival rates of cancer patients compared to the effincacy of IADs such as Carboplatin–Paclitaxel and Interferon alfa, and other therapies for certain types of cancer (Maemondo et al., 2010; Motzer et al., 2009; Zhou et al., 2011). Moreover, multiple studies have also shown that combining of OADs with therapies such as surgery, radiotherapy, and IADs, can increase treatment efficiency and patient survival rates (O'Shaughnessy et al., 2002; Raafat et al., 2012; Vokes et al., 1989). For instance, a clinical trial has shown that Capecitabine, used in conjunction with radiotherapy, can significantly reduce mortality rates associated with head/neck cancers (Raafat et al., 2012).

However, the relatively high costs of OADs mean that access to them is limited due to a gap in insurance coverage in the USA. This gap is defined by the higher copayment, coinsurance, or out-of-pocket payments associated with using OADs rather than IADs. This is due to OADs being covered under the pharmacy benefit, which covers any drugs not administered in a medical setting, whereas IADs are covered under the medical benefit, which customarily covers any drugs administered in a medical setting (Dusetzina et al., 2014a; Fitch et al., 2010; Hede, 2009; Kircher et al., 2016; Shen et al., 2014). Generally, pharmacy benefits have a tier-based copayment structure, which increases copayments, coinsurance, or out-of-pocket payments with or without an annual maximum allowable fee for drugs in the higher tiers, while medical benefits only have a single fixed copayment and

an annual maximum allowable fee across all drug types. Such a disparity in coverage would lead to patients unable to afford these drugs (Dusetzina et al., 2014b; Streeter et al., 2011).

Consequently, cancer patient advocacy groups have called for parity between OADs and IADs (Printz, 2014). Anticancer drug parity laws are meant to lower the copayments, coinsurance, and other costs of OADs, to make them comparable with IAD costs and thus affordable to insured cancer patients. This can therefore reduce cancer mortality in the implementing states through access to certain OADs with higher treatment efficiencies than other therapies, an increased utilization of OAD-based combination therapies, and an increase in adherence to the OAD therapies. This is critical, as non-adherence to drugs is a major contributor to mortality (Hershman et al., 2011; Walsh et al., 2019).

Table 1.1 presents the timing of implementing anticancer drug parity laws from 2004 to 2017. Over this period, Oregon was the first state to adopt anticancer drug parity laws (in 2008), 29 states had adopted such laws by 2017. To obtain the details on the laws, we searched the state statutes containing the phrases "shall provide coverage for a prescribed, orally administered anticancer medication used to kill or slow the growth of cancerous cells and shall apply the lower cost-sharing of either anticancer medication under the prescription drug benefit or intravenous or injected anticancer medications" or "shall provide coverage for prescribed, orally administered anticancer medications used to kill or slow the growth of cancerous cells on a basis not less favorable than intravenously administered or injected cancer medications that are covered as medical benefits." If a state has a specific statute containing such wording, we consider it an anticancer drug parity state. For the details of state statutes, refer to Appendix Table 1.1 (see Supplementary Material).

[Table 1.1]

It is noteworthy that the anticancer cancer drug parity laws are similar to fertility and mental health parity laws regarding the applications to insure populations. Like fertility and mental health parity laws, the anticancer cancer drug parity laws are only applicable to privately insured population. Thus, individuals covered by Medicare or Medicaid would not be affected. This has a significant implication for our analysis in Section 3. Moreover, some private insurance plans, such as self-insured plans, are not mandated to follow the parity laws due to the Employee Retirement Income Security Act of 1974.

1.2.2. Previous Literature

1.2.2.1. Impact of Parity Laws on Treatment Utilization and Health

Prior literature has investigated the impact of parity laws on treatment utilization and health outcomes using quasi-experimental approaches, specifically difference-in-differences, triple differences, or instrumental variable approaches. Although the significance and size of impact vary across the types of parity laws being studied, parity laws often result in an increase in the treatment utilizations.

One strand of literature focuses on the impact of mental health parity laws that force insurers to include mental healthcare in the insurance coverage. Using the 2001–2003 National Household Survey of Drug Abuse, Harris et al. (2006) showed that the laws increase the utilization of mental health treatments and that individuals with mild mental disorders tend to benefit more than those with severe disorders. Busch and Barry (2008), using the 1997–2001 National Survey of America's Families, found that the impact of these laws is highly dependent on the size of firm. They revealed that the mental health parity laws tend to increase the utilization of treatments for those working in firms with fewer than 100

employees. Dave and Mukerjee (2011), using the 1992–2007 Treatment Episodes, established that the number of treatments increases in states applying comprehensive mental health parity laws where equal coverage for a broad range of mental health disorders, including substance abuse disorder, is required.

Regarding health outcomes, Klick and Markowitz (2006) examine the impact of mental health parity laws on state suicide rates. Using 1981–2000 state panel data derived from the Compressed Mortality Files, they demonstrated that the implementations of mental health parity laws has a negative but insignificant impact on state suicide rates. Conversely, Lang (2013), using the 1990–2004 Compressed Mortality Files, found that the laws would significantly reduce state suicide rates. He showed that when states have stringent parity laws, such as "pure" parity and mandated offering laws, which require insurers to include or offer mental healthcare coverage at parity with physical healthcare, state suicide rates are significantly reduced, whereas when states have lenient parity laws, such as mandated if offered and minimum mandated benefits, which do not require mental healthcare coverage at parity with physical healthcare, there is no impact on state suicide rates. Popovici et al. (2017) investigated the impact of substance-use treatment parity laws on traffic fatalities using the 1988–2010 Fatal Accident Reporting System. They found that these laws reduce traffic fatality rates and that the impact is largest for states with the highest share of severely intoxicated drivers.

The other strand of literature investigates the impact of infertility treatment parity laws, which require insurers to cover infertility treatments alongside physical healthcare. Bitler and Schmidt (2012), using 1982–2002 data from the National Survey of Family Growth, find that the parity laws increase the utilization of treatments for white and college-educated women. Moreover, the impact is largely driven by treatments used to induce pregnancy, instead of

those preventing miscarriages. Schmidt (2007) focused on the impacts of the laws on women's fertility rates using the 1981–1999 Vital Statistics Detailed Natality Data. She found that the laws increased the probability of having children for women over 35 but had no statistically significant impact on women under 35. Buckles (2013) further explored the impact of treatments on multiple birth rates. Using the 1980–2001 Vital Statistics Detailed Natality Data, she found that the laws increased the probability of having triplets for college-educated white women over 30.

1.2.2.2. Impact of Drug Insurance on Health

Regarding the health impact of drug insurance, their findings are inconsistent among studies using a difference-in-differences approach. Wang et al. (2015) find, referencing the 1994–2003 National Population Health Survey, that Quebec's Universal Drug Insurance was positively associated with the health utility index but it had no impact on the overall self-reported health (SRH). Using the 2000–2010 Medicare Beneficiary Survey, Diebold (2016) found that Medicare Part D had no impact on the overall SRH but significantly reduces the incidences of high blood pressure for Medicare beneficiaries. Huh and Reif (2017) used National Vital Statistics System data from 2001–2008 and found that Medicare Part D reduces all-cause mortality rates, which is largely driven by cardiovascular mortality rates. Using the 2000–2010 National Vital Statistics System, Dunn and Shapiro (2019), like Huh and Reif (2017), found Medicare Part D reduces cardiovascular mortality rates.

In contrast to these studies, other authors argued that drug insurance has no impact on health outcomes. For example, Khan et al. (2007) used the 1992–2000 Medicare Current Beneficiaries Survey and found that Medicare Part D having no impact on either overall SRH or disability among the elderly. Similarly, using 2005–2006 data from the Medicare

Expenditure Panel Survey, Liu et al. (2011) found that Medicare Part D does not significantly influence health. Finally, Kaestner et al. (2019) used the 2002–2009 Medicare Current Beneficiaries Survey and found that Medicare Part D has no impact on all-cause mortality rates among the elderly.

1.3. Estimation Strategies and Data Source

1.3.1. Estimation Strategies

To investigate the impact of anticancer drug parity laws on cancer mortality rates, we implemented a difference-in-differences approach:

$$Y_{st} = \beta_0 + \beta_1 ParityLaws_{st} + \tau_s + \gamma_t + \tau\gamma_{st} + Z_{st}\alpha' + \epsilon_{st}, \qquad (1)$$

where Y_{st} is defined as the natural logarithm of the cancer mortality or incidence rates for state *s* in year *t.* $ParityLaws_{st}$ is the main policy variable indicating whether the anticancer drug parity laws are implemented in state *s* in year *t*. For state *s* that implemented the laws in year *t*, it is equal to 1 for all years after year *t* in state *s* and 0 otherwise. For the laws implemented between February and November, we define the variable as decimals, which were calculated as the number of months left in the year after policy implementation divided by 12. For instance, if the laws were implemented in March, there would be 9 months left in a year (12-3=9), dividing 9 by 12 gives the decimal, 0.75. τ_s and γ_t are vectors of state and year fixed effects represented by vectors of state and year dummies. $\tau\gamma_{st}$ is a vector of state-specific trends represented by a vector of interaction terms between state dummies and year trend. Z_{st} is a vector of state-level socioeconomic variables, including average age, gender, race, marital status, education, private insurance coverage rate, annual average household income, percentage of workers working in firms with ≥ 500 employees, and number of

hospitals per 100,000 persons in state *s* in year *t*. ϵ_{st} is the error term. β_1 is the parameter of interest. If the parameter is negative and significant, the laws have beneficial impacts on cancer mortality.

We focused on the malignant cancer mortality rates, because only malignant cancer requires drug treatment of any kind. Furthermore, we stratified the malignant cancer types according to the site of occurrence. This was done for a total of 12 sites: breast, respiratory, head/neck, digestive, bone/skin/soft tissue, female genital, male genital, urinary, nervous, thyroid, lymphoid, and ill-defined/multiple. We expected each cancer type to respond differently to the parity laws due to each type having a different number of drugs available and differential treatment efficiency (when used as a solo or combination therapy).

The validity of the difference-in-differences approach hinges on the common trend assumption, which indicates that states that experience no policy shocks are valid counterfactual groups for treatment states. A violation of common trend assumption is policy endogeneity, which involves the correlation between policy and outcomes or unobserved state-level characteristics. For example, states with increasing cancer mortality rates may implement parity laws to reduce the OAD cost burden on the insured (e.g., reverse causality). If that is the case, our estimates from equation (1) may be biased. We conducted an event study to examine whether the common trend assumption is violated for pre-policy periods (Anderson et al., 2013; Autor, 2003; Baggio et al., 2020; Chang, 2016; Nicholas & Mclean, 2019). We thus implement the following equation:

$$Y_{st} = \mu_0 + \sum_{k=-4}^{4} \mu_k ParityLaws_{s(t+k)} + \rho_s + \nu_t + \rho\nu_{st} + Z_{st}\zeta' + \varepsilon_{st}, \qquad (2)$$

Equation (2) introduces three lead and four lagged policy terms. For example, $ParityLaws_{s(t-4)}$ is denoted as a binary variable that equals 1 for the four or more years

prior to the implementation of parity laws in parity states, and 0 otherwise. Each lead (lagged) policy term corresponds to the number of years before (after) the implementation of the parity laws for each parity state. The omitted category is one year prior to the implementation of parity laws in parity states. To include the four lagged terms, we exclude the treatment states implementing the parity laws after January 2015, as they did not have enough post-periods. If any lead terms are significant, the conclusions may be not valid as the impact may be driven by policy endogeneity or unobserved state-level characteristics. ρ_s, γ_t, and $\rho\nu_{st}$ are identical to τ_s, γ_t, and $\tau\gamma_{st}$ from equation (1); and ζ' is a vector of parameters. ϵ_{st} is the error terms.

To further strengthen the validity of the difference-in-differences estimates, we performed a number of falsification tests to rule out exogenous influences other than the laws. For instance, a decline in the cancer mortality rate may be the result of a decline in cancer incidence or a spurious relationship between unobserved state variables and cancer mortality. To examine these possibilities, we first replaced the dependent variable with the log cancer incidence rates. If the coefficient on the parity laws is significant and has the same sign in equation (1), the results may be driven by a change in the cancer incidence rate. To test whether a spurious relationship exists, we replace thed dependent variable in equation (1) with the mortality rates of causes other than cancer and non-malignant cancer mortality rates. If our estimates are significant and have the same signs in equation (1), the findings may be driven by unobserved state variables. We estimated all equations using a fixed effects model. All regressions were weighted by the squared root of the yearly state population and were clustered by state.

1.3.2. Data Source

We constructed a state panel dataset using various publicly available data sources. We drew the dependent variables from two sources. The first was the death counts from the 2004–2017 Detailed Mortality Files of the Center for Disease Control and Prevention (CDC). These are derived from the National Vital Statistics System, which contains information on all-cause death for every individual residing in the United States (CDC, 2018a). We isolated cancer death counts using ICD-10 codes C.00–D.49, which represent neoplasms in ICD categories. The second source was the 2004–2016 cancer incidence count from the United States Cancer Statistics, jointly collected by CDC and National Institute of Cancer (CDC, 2018b). These are extracted from the National Program of Cancer Registries and Surveillance, Epidemiology, and End Results, which contains all diagnosed cancer cases from the medical records of the cancer registries affiliated with the program.

Due to a lack of data on state-level socioeconomic variables for mortality and incidence rates, we augmented the data with the socioeconomic characteristics from the Annual Social and Economic Supplement of the Current Population Survey (ASEC), provided by the University of Minnesota (IPUMS-CPS, 2018). This survey supplement collects socioeconomic information, such as marital status and education, from sampled individuals in the Current Population Survey during February, March, and April. We extracted age, gender, racial, marital, educational, household income, and private and public insurance information from the database and aggregate the individual-level characteristics into state-level characteristics. For other data, the number of workers working in firms with 500 or more employees was extracted from the Census Bureau (Census, 2018), and, the number of hospitals was retrieved from the Kaiser Family Foundation (KFF, 2018). We normalized mortality, incidence counts, and other state-level variables (whenever possible) by the yearly state population, obtained from the Surveillance, Epidemiology, and End Results database.

Since the parity laws do not affect the publicly insured population, we limited the data extraction to individuals between age 25 and 64 as those below and above this age range are covered by Medicaid and/or Medicare.

1.4. Results

1.4.1. Summary Statistics

Table 1.2 reports the summary statistics. Column (1) reports the weighted means and standard deviations of the log cancer mortality rates by the sites of occurrence for all states and years. Columns (2) and (3) report the means and standard deviations for non-parity states for 2004-2007 and 2008-2017, respectively. Column (4) reports the differences and t-statistics between (2) and (3). Columns (5) and (6) report the means and standard deviations for parity states for 2004-2007 and 2008-2017, respectively. Column (7) reports the differences and t-statistics between (5) and (6). The total number of observations is 714. The number of observations is 308 and 406 for non-parity and parity states, respectively. From Table 2, it can be observed that there was a general decline in the malignant cancer mortality rates across most types of cancers for parity states after 2008. However, the decline differs across the types. For example, digestive cancer declined by 2.7% in parity states, whereas urinary cancer declined by 4.6% in parity states after 2008. This implies that there may be significant heterogeneity in the impacts of anticancer drug parity laws across cancer types.

[Table 1.2]

1.4.2. Main Results

Table 1.3 reports the estimated impact of anticancer drug parity laws on the log of malignant cancer mortality rates by the sites of occurrence from equation (1). Columns (1) – (6) of Panel A report the estimates for breast, respiratory, head/neck, digestive, bone/skin/soft tissue, and female genital cancer mortality rates, respectively. Columns (1) – (6) of Panel B report the estimates for male genital, urinary, nervous, thyroid, lymphoid, and ill-defined/multiple sites cancer mortality rates, respectively. The estimates show that the parity laws significantly reduce head/neck malignant cancer mortality rates by 9.0%. No statistically significant impacts were found for other types of cancer. Our estimates imply that the anticancer drug parity laws only significantly affect head/neck cancers, while the laws do not affect other types of cancer mortality.

[Table 1.3]

1.4.3. Event Study

Tables 1.4 and 1.5 present the estimates of the event study from equation (2) for all cancer sites. One year prior to the parity laws is the omitted (or baseline) category. The lead policy terms, such as $Parity\ Law_{t-1}$, are statistically insignificant for most of the cancer types. Specifically, the lead policy terms on head/neck cancers are also statistically insignificant as well. This suggests that common trend assumption is unlikely to be violated.

Examining the four lagged policy terms, we observe that the lagged policy estimates of the parity laws on head/neck cancers are statistically significant. According to the lagged policy terms on head/neck cancer, it can be observed that the impacts of parity laws on head/neck cancers are permanent rather than transient. In other words, the implementation of the laws seems to have longer lasting impacts on head/neck cancers. Moreover, the impact seems to

get stronger the longer the lag, from 9.7% to 13.3%. The stronger lagged impacts may be the results of more individuals taking up the treatments. To summarize, we do not find evidence that the common trend assumption is violated for head/neck cancers, and we find that the parity laws significantly reduce the mortality for head/neck cancers in the long-run. As a robustness check, we also regress the event study with only parity states, and the results are consistent to these results (see Appendix Tables 9 and 10 in the Supplementary Material).

[Table 1.4]

[Table 1.5]

1.4.4. Falsification Tests

Table 1.6 reports the estimates of the falsification tests using the log cancer incidence, non-cancer mortality, and non-malignant cancer mortality rates. Column (1) reports the estimates for the log cancer incidence rate, column (2) the estimates for log non-cancer mortality rates, and column (3) the estimates for log non-malignant cancer mortality rates. Both estimates for cancer incidence and non-malignant cancer mortality are statistically insignificant. The estimate for non-cancer mortality is positive but statistically significant. This seems to suggest that non-cancer mortality is increasing over time. However, it does not alter the conclusion as the signs are not consistent with our main results. These results do seem to indicate that the parity laws are not driven by a decline in log cancer incidence rates. Furthermore, no spurious relationship seems to exist between parity laws, non-cancer mortality, and non-malignant cancer mortality rates.

[Table 1.6]

1.4.5. Alternative Channels

While our specification controls for private insurance rates, it is possible that the parity laws may affect private insurance rates in the implemented states. Specifically, private insurance may become more attractive to cancer patients in the face of the parity laws since public insurance plans are not subject to parity laws and may not have the same benefits as private insurance. Thus, the implemented states may experience a significant increase in private insurance coverage if such a channel exists. This would change the interpretation of our results since we expect the parity laws to be driven by an increase in the access to treatments rather than the coverage. To test this channel, we regressed private and public insurance rates obtained from ASEC on the parity laws.

Table 1.7 shows the impacts of the parity laws on private and public insurance rates. Column (1) reports the estimate for the log of private insurance rates, and column (2) reports the estimate for the log of public insurance rates. The estimates on the parity laws are statistically insignificant on both private and public insurance rates. This implies that the parity laws do not cause an expansion in private insurance, nor a contraction in public insurance.

Finally, it is possible that our estimates may be picking up a decreasing trend in prices of cancer treatments, specifically for head/neck cancers. While we do not have access to the prices of cancer treatments from states, we searched the information on the prices of head/neck cancer treatments over the period 2010–2018. We found that the prices have increased over time from US$3635.7 million to US$4187.9 million (National Institute of Cancer, 2020). This implies that the parity laws are unlikely to be driven by a decreasing trend in cancer treatment costs.

[Table 1.7]

1.4.6. Robustness Checks

We conducted additional analyses to check the robustness of our main results (see Supplementary Material). First, we tested the sensitivity of our estimates to alternative inference by clustering by state and year. Appendix Table 1.4 reports the estimates when clustering by state and year. No significant differences were found between the estimates of the robustness check and the main results. To further check the common trend assumption, we excluded all the never-adopted-parity states from the regression, and Appendix Table 1.5 reports these estimates. The relationship between the parity laws and malignant cancer mortality by types is still consistent with the main results. To check the sensitivity of our estimates, we used alternative policy coding. We coded parity laws as missing if they were introduced between February and November, instead of using a decimal. Appendix Tables 1.6 are reported these results. The results are similar to the main results. Alternatively, we used a more precise method by exploiting the monthly changes in cancer mortality rates from CDC mortality files. That is, we aggregated the data by month of mortality, instead of year. This results in approximately 8,568 state-year-month cells in the data. We could then run the same regression when the data was aggregated by year. However, this method has one drawback. Some state-monthly cells report 0 deaths, and this would translate into missing when the numbers were transformed them into log mortality rates. Hence, the observations of each estimation result would always be less than 8,568 observations. Appendix Table 1.7 reports the estimates using data aggregated by state, year, and month. The results of robustness checks are similar to the main results. Finally, a paper by Solon et al. (2015)

indicated that it is not clear whether or not weighting the estimates is a good idea. To show that our estimates are not sensitive to weighting, we ran unweighted regression. Appendix Table 1.8 reports the unweighted estimates. No significant differences were detected between these results and the main results. As a final robustness check, we tested the common trend of all states by including the states that implemented parity laws since 2015 into the event study model. Appendix Tables 1.11 and 1.12 report the results. The estimates from these tables are similar to our estimates from the main results.

Finally, even though the lead estimates on head/neck cancer from our event study are statistically insignificant, the sizes of the estimates seem to suggest concerns over the presence of some declining trends. The declining trends may be the result of the Medicare Part D (which apply to those under 65 with disabilities and diseases) and the Deficient Reduction Act (DRA) of 2005 occurred between 2004 and 2006. To assess whether these laws has any impact on our estimates, we first progressively remove pre-2006 years from the estimations and compare them. If the estimate from 2006–2017 is significantly differ from the estimate from full sample, it may suggest that the program is driving our policy estimates, not the parity laws. In addition, we implement a lead plus baseline model (equation (1)) similar to Carpenter et al. (2011) and Nyugen (2014) on 2006–2017 sample to test whether the pre-trend is present or not. All regressions are reported in Columns (1)–(4) of Appendix Table 1.13. Overall, the table suggests that the Medicare Part D and the DRA has little impacts on our policy estimates. We observe that the magnitude of the coefficient decline in absolute terms when we drop 2004 and 2005; however, the significance still holds. It would suggest that these laws do decrease the mortality rates, albeit small. In contrast, the estimates using 2006–2017 is still negative and significant, suggesting that the parity laws do have an impact on mortality rates. Moreover, when we implement a lead plus baseline model from equation (1) in Column (4), we observe that the pre-trend estimate is insignificant and trivial

in magnitude. This suggests that there is little trend after the exclusion of the periods for these two laws. Overall, we observe that the pre-policy trends may be the result of the Medicare Part D and the DRA, but they have relatively little impacts on the estimate of our parity laws.

1.5. Discussion

Our findings shed light on the impact of anticancer drug parity laws on cancer mortality rates. We find that anticancer drug parity laws reduce mortality rates of head/neck cancers in the implementing states. Specifically, the anticancer drug parity laws reduce mortality rates for head/neck malignant cancers and have no impact on cancers of other types. We also rule out the alternative channels that may influence the parity laws and cancer mortality rates, such as insurance expansion and decreasing treatment costs. This implies that the parity laws providing access to more effective treatment are important in reducing cancer mortality. Our results suggest that anticancer parity drug laws have a substantial beneficial impact on population health.

It is notable that the parity laws seem to only affect head/neck cancers only and not the other types. There are several plausible explanations for why the parity laws only affect only one type? First, for some cancers, including head/neck, combination therapies increase the treatment efficiency when compared to solo therapies, and they translate into overall better patient survival (O'Shaughnessy et al., 2002; Raafat et al., 2012; Vokes et al., 1989). Second, the availability of drugs and other treatments across the various types of cancers will also affect how effective the parity laws are. Breast cancer has a higher number of drug therapies than most of the other types of cancer (Sun et al., 2017). It may be that OADs have substantial costs compared to IADs, but the sheer number of drugs may allow patients to

substitute them with combination (or solo) therapies that can yield similar treatment efficiency at a lower cost before the implementation of the parity laws. Thus, even after the implementation of the laws, we would not observe a significant and meaningful impact due to the substitution behaviors before the introduction of the parity laws. Finally, it has been shown that head/neck cancers are increasing for some populations (Ellington et al., 2020). Indeed, the populations that experience increasing head/neck cancers are predominately White or Asian males between 30 and 60. These populations are generally more affluent than other ethnic groups and other age groups, which means that they are more likely to be covered under private insurance schemes (as they have more income) that are affected by the parity laws.

To gauge the impacts of the parity laws, we calculated the implied percent impacts of the laws. We found that the parity laws reduce head/neck cancer mortality between 7.5% and 11.7% relative to the pre-2008 means for individuals residing in parity states (coefficients from Table 1.3 and Appendix Table 1.3). While there is no research that examines the impact of anticancer drug parity laws on mortality, literature from mental health parity laws can be used to see how plausible our estimates are. Lang's (2013) estimates point to a 2% to 5% reduction in suicide rates relative to the means of individuals living in pre-policy mental health parity states. It appears that our implied percentage impacts are much higher than the impacts of mental health parity laws, but they are still plausible. The higher impacts could be attributed to the urgency of the conditions—while an individual may be able to live with poor mental health for several years before committing suicide, an individual who lacks proper treatment can be killed by cancer within months (depending on stages and types). This implies that the anticancer drug parity laws could save those individuals who are dying from (more aggressive) cancers as they may need urgent treatment.

Our study has a limitation. Although we show that our policy estimates are not affected by the Medicare Part D or DRA, there may still exist some pre-existing declining trends that could contaminated our policy estimates. For instance, there may exist some additional policy shocks during the periods which affect public insured under 65 that we cannot eliminate or control. Future studies with access to more disaggregated data (or individual-based data, ideally) are necessary to evaluate the pure effect of the parity laws by focusing on a specific subgroup of insured population (i.e. privately insured).

Our findings are both timely and important. They suggest that parity laws have a significant beneficial impact on cancer mortality rates and are an effective tool for reducing premature cancer mortality if properly implemented. Furthermore, state parity laws have limited impacts in terms of the affected plans, since self-insured firms do not have to comply with these laws, and thus they would only affect a certain segment of the population. That is, only 36% of the total population (58,511,500 individuals) may be impacted (calculated by taking the total population between the ages of 25 and 64 during our observational period from 2004 to 2017—163,456,382 individuals—multiplied by the proportion of the population not working in firms with more than 500 employees—49.7%—and by the proportion of the population that was privately insured population—72.1%). Given that less than half of the population is affected, the adoption of a federal parity law could penetrate plans not influenced by the state parity laws and affect the populations in these plans. The adoption of such a law at the federal level would therefore results in greater benefits for cancer patients.

Cancer remains one of the leading health problems in the United States (CDC, 2016). Many current cancer policies focus on changing the health behaviors of individuals to reduce cancer mortality in the population (Brown et al., 2012; Henley et al., 2014). Given the beneficial impacts of anticancer drug parity laws, these laws can be effective tools for policy

makers seeking to reduce the costs of cancer care, and they represent a useful addition to current cancer policies.

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Table 1.1. Timing of implementation for anticancer drug parity laws

Law/State	Policy Implementation Dates
Oregon	January 2008
Iowa	January 2009
DC, Indiana, Hawaii	January 2010
Vermont	April 2010
Minnesota	May 2010
Kansas	July 2010
Colorado, Connecticut	January 2011
New Mexico	June 2011
Texas	October 2011
Washington, Illinois, New York	January 2012
Nebraska	April 2012
Virginia, New Jersey	July 2012
Maryland	October 2012
Delaware	January 2013
Massachusetts, Rhode Island	January 2014
Maine	January 2015
Mississippi, Wyoming	July 2015

Pennsylvania, Dakota, Arizona	West	Virginia,	January 2016

DC stands for District of Columbia

Table 1.2. Summary statistics

	All States	Non-Parity States			Parity States		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All Years	2004-2007	2008-2017	Diff = (2)–(3)	2004-2007	2008-2017	Diff = (5)–(6)
Log breast cancer mortality rate	2.340	2.418	2.347	-0.071***	2.399	2.283	-0.116***
	(0.148)	(0.143)	(0.130)	(-4.167)	(0.133)	(0.147)	(-7.322)
Log respiratory cancer mortality rate	3.317	3.464	3.343	-0.121**	3.369	3.222	-0.147***
	(0.333)	(0.351)	(0.402)	(-2.457)	(0.215)	(0.264)	(-5.295)
Log head/neck cancer mortality rate	0.768	0.788	0.846	0.059	0.667	0.731	0.064**
	(0.281)	(0.262)	(0.296)	(1.605)	(0.272)	(0.258)	(2.208)
Log digestive cancer mortality rate	3.334	3.268	3.396	0.128***	3.237	3.338	0.101***
	(0.149)	(0.147)	(0.159)	(6.451)	(0.117)	(0.120)	(7.666)
Log bone/skin/soft tissue cancer mortality rate	1.497	1.539	1.518	-0.022	1.512	1.457	-0.055***
	(0.163)	(0.137)	(0.146)	(-1.183)	(0.147)	(0.183)	(-2.864)
Log female genital cancer mortality rate	1.882	1.859	1.898	0.039**	1.877	1.876	-0.000
	(0.146)	(0.133)	(0.143)	(2.183)	(0.143)	(0.154)	(-0.003)
Log male genital cancer mortality rate	0.694	0.671	0.768	0.097***	0.595	0.675	0.080***
	(0.224)	(0.244)	(0.186)	(3.741)	(0.221)	(0.229)	(3.192)
Log urinary cancer mortality rate	1.417	1.421	1.454	0.033	1.402	1.389	-0.013
	(0.194)	(0.198)	(0.201)	(1.303)	(0.195)	(0.181)	(-0.654)

Log nervous system cancer mortality rate	1.426	1.392	1.465	0.073***	1.366	1.426	0.060***
	(0.182)	(0.140)	(0.142)	(4.040)	(0.183)	(0.215)	(2.642)
Log thyroid cancer mortality rate	-0.808	-0.877	-0.770	0.107***	-0.881	-0.791	0.090**
	(0.322)	(0.320)	(0.315)	(2.645)	(0.365)	(0.305)	(2.463)
Log lymphoid cancer mortality rate	2.075	2.175	2.068	-0.107***	2.138	2.021	-0.116***
	(0.157)	(0.125)	(0.158)	(-5.657)	(0.130)	(0.151)	(-7.256)
Log ill-defined/multiple sites' cancer mortality rate	1.934	2.036	1.965	-0.071**	1.914	1.879	-0.035
	(0.232)	(0.230)	(0.237)	(-2.362)	(0.236)	(0.210)	(-1.465)
N	714	80	220	308	116	290	406

Note: Columns (1)–(7) report weighted means of dependent variables for all states, non-parity states, and parity states, respectively. Column (2) and (3) and Column (5) and (6) report the means of pre-2008 and post-2008 for non-parity and parity states. Column (4) and (7) report the differences of means for non-parity and parity states, respectively. The unit of observation is state-year cell. The standard deviations are reported in parentheses in Column (1)–(3) and (5)–(6). The t-statistics are reported in parentheses in Column (4) and (7).

Table 1.3. Impacts of anticancer drug parity laws on cancer mortality rates by the site of cancer

Panel A: Breast to Female Genital						
	(1) Breast	(2) Respiratory	(3) Head/Neck	(4) Digestive	(5) Bone/Skin/Soft tissue	(6) Female Genital
Parity Laws	0.018	-0.005	-0.090***	-0.004	0.010	0.016
	(0.016)	(0.009)	(0.030)	(0.007)	(0.020)	(0.024)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Mean (pre-2008 parity states)	2.418	3.464	0.788	3.268	1.539	1.859
N	714	714	714	714	714	714
Panel B: Male Genital to Ill-defined/Multiple						
	(1) Male Genital	(2) Urinary	(3) Nervous	(4) Thyroid	(5) Lymphoid	(6) Ill-defined/Multiple
Parity Laws	-0.005	-0.021	0.006	0.026	-0.007	-0.012
	(0.030)	(0.021)	(0.024)	(0.050)	(0.019)	(0.019)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Mean (pre-2008 parity states)	0.671	1.421	1.392	-0.877	2.175	2.036
N	714	714	714	690	714	714

Note: All regressions are estimated using FE. Columns (1)–(6) of Panel A report the estimates for all, breast, respiratory, head/neck, digestive, bone/skin/soft tissue, and female genital cancer mortality, respectively. Columns (1)–(6) of Panel B report the estimates for male genital, urinary, nervous, thyroid, lymphoid, and ill-defined/multiple cancer mortality, respectively. Controls are age, gender, race, marital status, education,

other state socio-economic variables, state-and year-fixed effects, and state-specific trends. The standard errors in parentheses are clustered by state.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.4. Event study: Impacts of anticancer drug parity laws on cancer mortality rates by cancer types

	(1) Breast	(2) Respiratory	(3) Head/Neck	(4) Digestive	(5) Bone/Skin/Soft tissue	(6) Female Genital
Parity Laws t-4 and prior	0.031	0.011	0.072	-0.019	0.013	0.038
	(0.028)	(0.020)	(0.046)	(0.014)	(0.031)	(0.032)
Parity Laws t-3	0.012	0.012	0.035	0.003	-0.015	0.047
	(0.024)	(0.015)	(0.047)	(0.010)	(0.025)	(0.033)
Parity Laws t-2	-0.001	0.001	0.003	-0.000	-0.045*	0.013
	(0.017)	(0.011)	(0.040)	(0.010)	(0.026)	(0.028)
Parity Laws t-1	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline
Parity Laws t	0.000	0.010	-0.061	-0.004	-0.004	0.026
	(0.022)	(0.013)	(0.043)	(0.007)	(0.029)	(0.032)
Parity Laws t+2	0.001	-0.006	-0.097**	0.014	0.016	0.031
	(0.026)	(0.016)	(0.041)	(0.011)	(0.030)	(0.030)
Parity Laws t+3	-0.008	-0.019	-0.122**	0.020	-0.020	0.023
	(0.025)	(0.019)	(0.057)	(0.012)	(0.038)	(0.028)
Parity Laws t+4 and after	0.002	0.006	-0.133*	0.034**	-0.009	0.018

	(0.027)	(0.022)	(0.073)	(0.016)	(0.045)	(0.035)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	602	602	602	602	602	602

Note: All regressions are estimated using FE. Columns (1)–(6) report the estimates for all, breast, respiratory, head/neck, digestive, bone/skin/soft tissue, and female genital cancer mortality, respectively. Controls are age, gender, race, marital status, education, other state socio-economic controls, state-and year-fixed effects, and state-specific trends. The standard errors in parentheses are clustered by state.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.5. Event study: Impacts of anticancer drug parity laws on cancer mortality rates by cancer types

	(1) Male Genital	(2) Urinary	(3) Nervous	(4) Thyroid	(5) Lymphoid	(6) Ill-defined/Multiple
Parity Laws t-4 and prior	0.027	0.000	0.020	0.023	0.028	-0.055
	(0.064)	(0.041)	(0.034)	(0.086)	(0.030)	(0.038)
Parity Laws t-3	-0.032	0.007	-0.013	-0.009	0.018	-0.027
	(0.062)	(0.024)	(0.035)	(0.105)	(0.031)	(0.028)
Parity Laws t-2	-0.015	0.014	-0.034	0.063	-0.034	-0.023
	(0.048)	(0.026)	(0.022)	(0.094)	(0.021)	(0.027)
Parity Laws t-1	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline
Parity Laws t	-0.080*	-0.002	-0.005	0.136	-0.013	-0.024
	(0.047)	(0.028)	(0.036)	(0.088)	(0.027)	(0.027)
Parity Laws t+2	0.022	-0.063**	0.017	0.061	-0.045*	-0.012
	(0.056)	(0.032)	(0.027)	(0.092)	(0.027)	(0.034)
Parity Laws t+3	-0.007	-0.025	0.016	0.084	-0.044	0.003
	(0.058)	(0.041)	(0.043)	(0.099)	(0.027)	(0.041)
Parity Laws t+4 and after	0.037	-0.017	0.030	0.207*	-0.061*	-0.013

	(0.066)	(0.044)	(0.046)	(0.117)	(0.036)	(0.055)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	602	602	602	602	602	602

Note: All regressions are estimated using FE. Columns (1)–(6) report the estimates for male genital, urinary, nervous, thyroid, lymphoid, and ill-defined/multiple cancer mortality, respectively. Controls are age, gender, race, marital status, education, other state socio-economic controls, state-and year-fixed effects, and state-specific trends. The standard errors in parentheses are clustered by state.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.6. Event study: Impacts of anticancer drug parity laws on cancer mortality rates by cancer types

	(1) Incidence	(2) Non-cancer	(3) Non-malignant
Parity Laws	-0.000	0.013*	0.002
	(0.005)	(0.007)	(0.041)
Controls	Yes	Yes	Yes
N	663	714	714

Note: All regressions are estimated using FE. Columns (1) and (2) report the estimates for cancer incidence, non-cancer mortality rates, and non-malignant cancer rates, respectively. Controls are age, gender, race, marital status, education, other state socio-economic variables, state-and year-fixed effects, and state-specific trends. The standard errors in parentheses are clustered by state.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.7. Impacts on private and public insurance coverage

	(1) Private	(2) Public
Parity Laws	0.001	-0.033
	(0.001)	(0.030)
Controls	Yes	Yes
N	714	714

Note: All regressions are estimated using FE. Columns (1) and (2) report the estimates for log of private and public insurance coverage, respectively. Controls are age, gender, race, marital status, education, other state socio-economic variables, state-and year-fixed effects, and state-specific trends. The standard errors in parentheses are clustered by state.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix

Appendix Table 1.1.Name and number of the statute of anticancer drug parity laws

State	Statue Number
Arizona	HB2078
Colorado	HB1202
Connecticut	SB50
Delaware	HB265
District of Columbia	Bill18-278
Hawaii	HB1964
Illinois	HB1825
Indiana	SB437
Iowa	514C.24
Kansas	HB2160
Maine	4317-B
Maryland	SB179
Massachusetts	S2363
Minnesota	SF1761
Mississippi	83-9-24
Nebraska	LB882
New Jersey	SB1834
New Mexico	SB385
New York	SB450
Oregon	SB8
Pennsylvania	HB60
Rhode Island	SB428
South Dakota	SB101
Texas	HB438
Vermont	HB444
Virginia	SB450
Washington	HB1517
West Virginia	33-25A-8I
Wyoming	26-20-501

Appendix Table 1.2. Summary statistics

	All States	Non-Parity States			Parity States		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All Years	2004-2007	2008-2017	Diff = (2)–(3)	2004-2007	2008-2017	Diff = (5)–(6)
Age	43.670	43.012	43.852	-0.840***	43.271	43.891	-0.620***
	(0.799)	(0.637)	(0.792)	(-8.800)	(0.626)	(0.735)	(-7.938)
Female (%)	52.215	52.380	52.252	0.129	52.183	52.137	0.046
	(1.138)	(1.110)	(1.208)	(0.857)	(1.057)	(1.113)	(0.377)
White (%)	79.049	79.277	78.016	1.261	81.091	79.100	1.991
	(11.501)	(9.419)	(9.620)	(1.038)	(13.041)	(12.931)	(1.390)
Black (%)	12.590	13.856	14.170	-0.314	10.689	11.471	-0.781
	(10.173)	(10.382)	(10.420)	(-0.238)	(10.047)	(9.702)	(-0.721)
Asian (%)	4.963	3.368	4.349	-0.981*	4.883	6.100	-1.217*
	(5.413)	(3.563)	(4.204)	(-1.914)	(6.271)	(6.275)	(-1.755)
Married (%)	16.364	17.278	17.268	3.306***	15.598	15.533	3.735***
	(2.200)	(2.014)	(2.067)	(7.215)	(1.917)	(2.043)	(7.245)
Divorce (%)	65.227	67.590	64.284	0.010	67.932	64.197	0.064
	(4.483)	(3.223)	(3.748)	(0.040)	(4.789)	(4.618)	(0.290)
High School (%)	34.959	38.412	35.757	2.655***	35.747	32.733	3.013***
	(5.415)	(5.195)	(4.569)	(4.401)	(5.491)	(5.268)	(5.116)
College (%)	27.735	28.315	28.905	-0.590*	26.755	26.866	-0.111
	(3.830)	(2.684)	(2.470)	(-1.835)	(4.617)	(4.475)	(-0.223)

Bachelor's and above (%)	31.699	26.774	29.722	-2.948***	31.748	35.170	-3.422***
	(6.208)	(3.153)	(3.612)	(-6.653)	(6.131)	(6.927)	(-4.612)
Hospitals per 100000 persons	3.571	3.544	3.404	0.140	3.795	3.643	0.152
	(2.104)	(1.635)	(1.490)	(0.721)	(2.581)	(2.479)	(0.549)
Firm with 500+ employee (%)	50.329	49.741	52.014	-2.273***	47.932	49.957	-2.025***
	(4.691)	(4.420)	(4.464)	(-4.022)	(4.276)	(4.621)	(-4.048)
Annual Household Income in 2015 Dollars	45124.359	43135.524	42044.978	1090.546**	47998.933	47461.403	537.530
	(5969.776)	(3926.473)	(3997.355)	(2.160)	(6417.867)	(6314.068)	(0.767)
Private Insurance (%)	72.067	72.388	69.265	3.123***	75.626	73.083	2.543***
	(5.977)	(5.220)	(5.322)	(4.646)	(5.693)	(5.820)	(3.979)
N	714	80	200	280	124	310	434

Note: Columns (1)−(7) report the weighted means of independent variables for all states, non-parity states, and parity states, respectively. Column (2) and (3) and Column (5) and (6) report the means of pre-2008 and post-2008 for non-parity and parity states. Column (4) and (7) report the differences of means for non-parity and parity states, respectively. The unit of observation is state-year cell. The standard deviations are reported in parentheses in Column (1)–(3) and (5)–(6). The t-statistics are reported in parentheses in Column (4) and (7).

Appendix Table 1.3. Estimates without state-specific trends

Panel A: Breast to Female Genital						
	(1) Breast	(2) Respiratory	(3) Head/Neck	(4) Digestive	(5) Bone/Skin/Soft tissue	(6) Female Genital
Parity Laws	-0.014	-0.026**	-0.055**	-0.018**	-0.015	-0.022
	(0.010)	(0.012)	(0.022)	(0.008)	(0.015)	(0.019)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	714	714	714	714	714	714
Panel B: Male Genital to Ill-defined/Multiple						
	(1) Male Genital	(2) Urinary	(3) Nervous	(4) Thyroid	(5) Lymphoid	(6) Ill-defined/Multiple
Parity Laws	0.009	-0.039**	0.009	-0.007	-0.009	0.006
	(0.024)	(0.017)	(0.018)	(0.037)	(0.013)	(0.019)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	714	714	714	690	714	714

Note: All regressions are estimated using FE. Columns (1)–(6) of Panel A report the estimates for all, breast, respiratory, head/neck, digestive, bone/skin/soft tissue, and female genital cancer mortality, respectively. Columns (1)–(6) of Panel B report the estimates for male genital, urinary, nervous, thyroid, lymphoid, and ill-defined/multiple cancer mortality, respectively. Controls are age, gender, race, marital status, education, other state socio-economic variables, and state-and year-fixed effects. The standard errors in parentheses are clustered by state.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 1.4. Cluster by state and year

Panel A: Breast to Female Genital						
	(1)	(2)	(3)	(4)	(5)	(6)
	Breast	Respiratory	Head/Neck	Digestive	Bone/Skin/Soft tissue	Female Genital
Parity Laws	0.018	-0.005	-0.090**	-0.004	0.010	0.016
	(0.021)	(0.010)	(0.039)	(0.008)	(0.015)	(0.025)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	714	714	714	714	714	714
Panel B: Male Genital to Ill-defined/Multiple						
	(1)	(2)	(3)	(4)	(5)	(6)
	Male Genital	Urinary	Nervous	Thyroid	Lymphoid	Ill-defined/Multiple
Parity Laws	-0.005	-0.021	0.006	0.026	-0.007	-0.012
	(0.038)	(0.022)	(0.024)	(0.054)	(0.021)	(0.023)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	714	714	714	690	714	714

Note: All regressions are estimated using FE. Columns (1)–(6) of Panel A report the estimates for all, breast, respiratory, head/neck, digestive, bone/skin/soft tissue, and female genital cancer mortality, respectively. Columns (1)–(6) of Panel B report the estimates for male genital, urinary, nervous, thyroid, lymphoid, and ill-defined/multiple cancer mortality, respectively. Controls are age, gender, race, marital status, education, other state socio-economic variables, state-and year-fixed effects, and state-specific trends. The standard errors in parentheses are clustered by state and year.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 1.5. Exclude never adopted states

Panel A: Breast to Female Genital						
	(1) Breast	(2) Respiratory	(3) Head/Neck	(4) Digestive	(5) Bone/Skin/Soft tissue	(6) Female Genital
Parity Laws	0.023	-0.006	-0.111***	-0.002	0.006	0.025
	(0.018)	(0.010)	(0.030)	(0.008)	(0.021)	(0.028)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	406	406	406	406	406	406
Panel B: Male Genital to Ill-defined/Multiple						
	(1) Male Genital	(2) Urinary	(3) Nervous	(4) Thyroid	(5) Lymphoid	(6) Ill-defined/Multiple
Parity Laws	-0.017	-0.027	0.007	0.014	0.008	0.001
	(0.031)	(0.023)	(0.027)	(0.053)	(0.019)	(0.021)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	406	406	406	388	406	406

Note: All regressions are estimated using FE. Columns (1)–(6) of Panel A report the estimates for all, breast, respiratory, head/neck, digestive, bone/skin/soft tissue, and female genital cancer mortality, respectively. Columns (1)–(6) of Panel B report the estimates for male genital, urinary, nervous, thyroid, lymphoid, and ill-defined/multiple cancer mortality, respectively. Controls are age, gender, race, marital status, education, other state socio-economic variables, state-and year-fixed effects, and state-specific trends. The standard errors in parentheses are clustered by state.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 1.6. Alternative policy coding

Panel A: Breast to Female Genital						
	(1)	(2)	(3)	(4)	(5)	(6)
	Breast	Respiratory	Head/Neck	Digestive	Bone/Skin/Soft tissue	Female Genital
Parity Laws	0.020	-0.006	-0.092***	-0.003	0.011	0.017
	(0.017)	(0.009)	(0.031)	(0.007)	(0.020)	(0.025)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	702	702	702	702	702	702
Panel B: Male Genital to Ill-defined/Multiple						
	(1)	(2)	(3)	(4)	(5)	(6)
	Male Genital	Urinary	Nervous	Thyroid	Lymphoid	Ill-defined/Multiple
Parity Laws	-0.018	-0.023	0.001	0.038	-0.007	-0.011
	(0.030)	(0.022)	(0.023)	(0.052)	(0.019)	(0.020)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	702	702	702	679	702	702

Note: All regressions are estimated using FE. Columns (1)–(6) of Panel A report the estimates for all, breast, respiratory, head/neck, digestive, bone/skin/soft tissue, and female genital cancer mortality, respectively. Columns (1)–(6) of Panel B report the estimates for male genital, urinary, nervous, thyroid, lymphoid, and ill-defined/multiple cancer mortality, respectively. Controls are age, gender, race, marital status, education, other state socio-economic variables, state-and year-fixed effects, and state-specific trends. The standard errors in parentheses are clustered by state.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 1.7. Estimates using monthly mortality rates

Panel A: Breast to Female Genital						
	(1) Breast	(2) Respiratory	(3) Head/Neck	(4) Digestive	(5) Bone/Skin/Soft tissue	(6) Female Genital
Parity Laws	0.013	0.000	-0.041*	-0.008	0.009	0.006
	(0.014)	(0.009)	(0.023)	(0.009)	(0.020)	(0.021)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	8482	8564	7406	8568	8181	8350
Panel B: Male Genital to Ill-defined/Multiple						
	(1) Male Genital	(2) Urinary	(3) Nervous	(4) Thyroid	(5) Lymphoid	(6) Ill-defined/Multiple
Parity Laws	0.000	-0.008	0.014	0.023	-0.014	-0.008
	(0.037)	(0.017)	(0.022)	(0.031)	(0.017)	(0.020)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	7382	8117	8134	4690	8419	8390

Note: All regressions are estimated using FE. Columns (1)–(6) of Panel A report the estimates for all, breast, respiratory, head/neck, digestive, bone/skin/soft tissue, and female genital cancer mortality, respectively. Columns (1)–(6) of Panel B report the estimates for male genital, urinary, nervous, thyroid, lymphoid, and ill-defined/multiple cancer mortality, respectively. Controls are age, gender, race, marital status, education, other state socio-economic variables, state-and year-fixed effects, and state-specific trends. The standard errors in parentheses are clustered by state.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 1.8. Unweighted estimates

Panel A: Breast to Female Genital						
	(1)	(2)	(3)	(4)	(5)	(6)
	Breast	Respiratory	Head/Neck	Digestive	Bone/Skin/Soft tissue	Female Genital
Parity Laws	0.036	-0.004	-0.148***	-0.007	0.000	0.047
	(0.025)	(0.012)	(0.037)	(0.011)	(0.024)	(0.038)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	714	714	714	714	714	714
Panel B: Male Genital to Ill-defined/Multiple						
	(1)	(2)	(3)	(4)	(5)	(6)
	Male Genital	Urinary	Nervous	Thyroid	Lymphoid	Ill-defined/Multiple
Parity Laws	0.009	-0.057*	0.002	0.010	-0.005	-0.008
	(0.035)	(0.034)	(0.038)	(0.063)	(0.028)	(0.026)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	714	714	714	690	714	714

Note: All regressions are estimated using FE. Columns (1)–(6) of Panel A report the estimates for all, breast, respiratory, head/neck, digestive, bone/skin/soft tissue, and female genital cancer mortality, respectively. Columns (1)–(6) of Panel B report the estimates for male genital, urinary, nervous, thyroid, lymphoid, and ill-defined/multiple cancer mortality, respectively. Controls are age, gender, race, marital status, education, other state socio-economic variables, state-and year-fixed effects, and state-specific trends. The standard errors in parentheses are clustered by state.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 1.9. Event study with treated states only: Impacts of anticancer drug parity laws on cancer mortality rates by cancer types

	(1) Breast	(2) Respiratory	(3) Head/Neck	(4) Digestive	(5) Bone/Skin/Soft tissue	(6) Female Genital
Parity Laws t-4 and prior	0.003	-0.000	0.083	-0.008	0.043	0.063**
	(0.037)	(0.022)	(0.052)	(0.015)	(0.032)	(0.032)
Parity Laws t-3	0.001	0.007	0.038	0.003	0.001	0.064*
	(0.031)	(0.015)	(0.049)	(0.011)	(0.029)	(0.033)
Parity Laws t-2	0.000	-0.007	-0.008	-0.001	-0.040	0.005
	(0.020)	(0.010)	(0.041)	(0.009)	(0.026)	(0.028)
Parity Laws t-1	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline
Parity Laws t	0.011	0.011	-0.093**	-0.001	-0.010	0.023
	(0.023)	(0.013)	(0.043)	(0.008)	(0.029)	(0.030)
Parity Laws t+2	0.015	-0.003	-0.132***	0.024**	0.022	0.036
	(0.026)	(0.016)	(0.045)	(0.012)	(0.026)	(0.036)
Parity Laws t+3	0.007	-0.018	-0.144**	0.038***	-0.006	0.029
	(0.028)	(0.021)	(0.066)	(0.015)	(0.040)	(0.034)
Parity Laws t+4 and after	0.016	0.009	-0.130	0.055***	0.017	0.031

	(0.028)	(0.025)	(0.091)	(0.019)	(0.049)	(0.048)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	308	308	308	308	308	308

Note: All regressions are estimated using FE. Columns (1)–(6) report the estimates for all, breast, respiratory, head/neck, digestive, bone/skin/soft tissue, and female genital cancer mortality, respectively. Controls are age, gender, race, marital status, education, other state socio-economic controls, state-and year-fixed effects, and state-specific trends. The standard errors in parentheses are clustered by state.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 1.10. Event study with treated states only: Impacts of anticancer drug parity laws on cancer mortality rates by cancer types

	(1) Male Genital	(2) Urinary	(3) Nervous	(4) Thyroid	(5) Lymphoid	(6) Ill-defined/Multiple
Parity Laws t-4 and prior	-0.004	-0.027	0.011	0.059	0.016	-0.044
	(0.076)	(0.047)	(0.050)	(0.102)	(0.040)	(0.035)
Parity Laws t-3	-0.040	0.002	-0.023	0.020	0.002	-0.046
	(0.070)	(0.028)	(0.048)	(0.121)	(0.037)	(0.030)
Parity Laws t-2	-0.031	0.014	-0.042*	0.053	-0.041*	-0.026
	(0.052)	(0.030)	(0.021)	(0.094)	(0.023)	(0.025)
Parity Laws t-1	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline
Parity Laws t	-0.075*	0.012	0.007	0.121	-0.009	-0.006
	(0.045)	(0.031)	(0.032)	(0.086)	(0.025)	(0.029)
Parity Laws t+2	0.032	-0.053	0.024	0.036	-0.031	0.014
	(0.062)	(0.038)	(0.028)	(0.089)	(0.027)	(0.034)
Parity Laws t+3	-0.006	-0.021	0.009	0.053	-0.021	0.041
	(0.055)	(0.051)	(0.041)	(0.093)	(0.027)	(0.043)
Parity Laws t+4 and after	0.043	-0.003	0.009	0.245**	-0.042	0.052

	(0.068)	(0.054)	(0.048)	(0.108)	(0.037)	(0.061)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	308	308	308	298	308	308

Note: All regressions are estimated using FE. Columns (1)–(6) report the estimates for male genital, urinary, nervous, thyroid, lymphoid, and ill-defined/multiple cancer mortality, respectively. Controls are age, gender, race, marital status, education, other state socio-economic controls, state-and year-fixed effects, and state-specific trends. The standard errors in parentheses are clustered by state.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 1.11. Event study with all states: Impacts of anticancer drug parity laws on cancer mortality rates by cancer types

	(1) Breast	(2) Respiratory	(3) Mouth/Neck	(4) Digestive	(5) Bone/Skin/Soft tissue	(6) Female Genital
Parity Laws t-4 and prior	0.028	-0.003	0.065	-0.014	0.030	-0.001
	(0.022)	(0.014)	(0.042)	(0.011)	(0.024)	(0.028)
Parity Law t-3	0.023	0.003	0.026	0.008	0.001	0.027
	(0.019)	(0.011)	(0.042)	(0.012)	(0.023)	(0.028)
Parity Law t-2	0.008	0.005	0.002	0.003	-0.021	0.005
	(0.017)	(0.009)	(0.039)	(0.010)	(0.025)	(0.026)
Parity Law t-1	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline
Parity Law t	0.021	0.002	-0.069**	-0.005	-0.000	0.018
	(0.022)	(0.010)	(0.034)	(0.007)	(0.027)	(0.030)
Parity Law t+1	0.026	-0.007	-0.124***	0.008	-0.003	0.035
	(0.023)	(0.012)	(0.039)	(0.010)	(0.026)	(0.030)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	714	714	714	714	714	714

Note: All regressions are estimated using FE. Columns (1)–(6) report the estimates for male genital, urinary, nervous, thyroid, lymphoid, and ill-defined/multiple cancer mortality, respectively. Controls are age, gender, race, marital status, education, other state socio-economic controls, state-and year-fixed effects, and state-specific trends. The standard errors in parentheses are clustered by state.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0$.

Appendix Table 1.12. Event study with all states: Impacts of anticancer drug parity laws on cancer mortality rates by cancer types

	(1) Male Genital	(2) Urinary	(3) Nervous	(4) Thyroid	(5) Lymphoid	(6) Ill-defined/Multiple
Parity Laws t-4 and prior	0.005	-0.009	0.015	0.042	0.038	-0.049**
	(0.047)	(0.029)	(0.027)	(0.072)	(0.024)	(0.024)
Parity Law t-3	-0.018	0.002	-0.015	-0.003	0.021	-0.029
	(0.054)	(0.022)	(0.029)	(0.088)	(0.025)	(0.024)
Parity Law t-2	-0.019	-0.010	-0.039*	0.018	-0.018	-0.027
	(0.040)	(0.025)	(0.020)	(0.080)	(0.019)	(0.022)
Parity Law t-1	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline
Parity Law t	-0.052	-0.018	-0.018	0.040	0.009	-0.030
	(0.043)	(0.023)	(0.030)	(0.081)	(0.024)	(0.023)
Parity Law t+1	0.025	-0.029	-0.011	0.010	-0.040	-0.013
	(0.047)	(0.029)	(0.029)	(0.082)	(0.025)	(0.027)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	714	714	714	690	714	714

Note: All regressions are estimated using FE. Columns (1)–(6) report the estimates for male genital, urinary, nervous, thyroid, lymphoid, and ill-defined/multiple cancer mortality, respectively. Controls are age, gender, race, marital status, education, other state socio-economic controls, state-and year-fixed effects, and state-specific trends. The standard errors in parentheses are clustered by state.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 1.13. Impacts of anticancer drug parity laws on head/neck cancer mortality rates and event study by sample years

	(1)	(2)	(3)	(4)
	2004–2017	2005–2017	2006–2017	2006–2017
Parity Laws t+2 and Prior				0.007
				(0.036)
Parity Law t-1				Baseline
Parity Laws	-0.090***	-0.085***	-0.073***	-0.070**
	(0.030)	(0.029)	(0.028)	(0.031)
Controls	Yes	Yes	Yes	Yes
N	714	663	612	612

Note: All regressions are estimated using FE. Columns (1)–(4) report the estimates for head/neck cancers using progressively smaller sample of pre-trend years. Controls are age, gender, race, marital status, education, other state socio-economic controls, state-and year-fixed effects, and state-specific trends. The standard errors in parentheses are clustered by state.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Chapter 2. The Effect of Coverage of Smoking-Cessation Aids on Tobacco Use Outcomes: Evidence from Canada

1. Introduction

Approximately 20% of the world's population smokes cigarettes (World Health Organization [WHO], 2018) and seven million deaths annually are attributed to smoking worldwide (WHO, 2017). Goodchild et al. (2012) estimated the total economic loss from smoking was US$1436 billion, or approximately 1.8% of the world's annual gross domestic product in 2012. Consequently, many governments have implemented various measures to reduce tobacco use through price-related and non-price-related policies such as taxation and public smoking bans (Peterson et al., 1992; Gallus et al., 2006; Hansen et al., 2017; Bitler et al., 2010; Carpenter, 2009; Carpenter et al., 2011). A particular non-price related policy that has gained considerable attention relates to smoking-cessation aids (SCAs).

SCAs are drugs that reduce withdrawal symptoms by moderating the symptoms of irritation and mood disorders (bupropion and varenicline). Several clinical trials have highlighted the effectiveness of these drugs in improving smoking cessation (Jorenby et al., 1999; Aubin et al., 2004; Wagena et al., 2005; Cinciripini et al., 2013). Specifically, Hughes et al. (2014) reviewed the existing evidence from clinical trials and found that treatment by bupropion significantly increases the six-month smoking abstinence by 62% more than placebo treatment. Due to the effectiveness of SCAs in clinical settings, many countries have begun to implement insurance coverage of SCAs. In particular, SCAs are covered in the United States (henceforth, coverage of SCAs will be referred to as SCA coverage) as a tobacco cessation program of Medicaid in an effort to promote smoking cessation among Medicaid beneficiaries.

A large volume of literature investigating the effect of insurance coverage on tobacco use has used Medicaid as a natural experiment with a difference-in-difference (DD)

framework (Courtemanche et al., 2018; Cawley et al., 2018; Koma et al., 2018; Simon et al., 2017; Cotti et al., 2019; Soni, 2020). The majority of these studies have pointed to Medicaid reducing cigarette use, though the significance of estimates varies. More specifically, much of the existing literature on SCA coverage has used Medicaid's coverage of SCA to examine its effect on cigarette use (Liu, 2009; Liu, 2010; Brantley et al., 2019; Kostova et al., 2018).[1] For instance, Liu (2009) examined the effect of Medicaid's SCA coverage with an index of SCA and related treatments on smoking cessation and initiation using 1996–2007 Tobacco Use Supplement to the Current Population Survey. He found that a higher index increases cessation and decreases initiation in women but not in men. Liu (2010) also examined the effect of different copayment requirements of Medicaid's SCA coverage on intention to quit using the same data as Liu (2010). He found that the states without copayment requirements had higher intention to quit among beneficiaries. Brantley et al. (2019) investigated the effect of Medicaid's different types of coverage requirements (copayment, counselling, or prior authorization) on current cigarette use using the 2010 and 2015 National Health Interview Survey. They found that the states covering counselling as a requirement to gain SCA in Medicaid had lower current cigarette use.

However, evaluating the effect of SCA coverage on tobacco use outcomes using Medicaid as a natural experiment is problematic (Liu, 2009; Liu, 2010; Brantley et al., 2019; Kostova et al., 2018). Medicaid is complicated by many health insurance coverage reforms ranging from the state-level Medicaid expansion for adults with children in the 1990s and 2000s to the Affordable Care Act (ACA).[2] Many of these policies aimed at expanding the health insurance coverage rate by changing the eligibility criteria. For instance, in 1996, Medicaid was delinked from Temporary Assistance for Needy Families, leading to many

[1] White et al. (2015) examined the effect of SCA coverage in the Canadian setting similar to our study using the same dataset. However, White et al. (2015) did not include geographic and time fixed effects to control for pre-existing differences over locations and time, which makes their study incomparable to studies that used a DD framework, including our study.
[2] These are by no mean the only reforms between 1990 and 2014. There were other health insurance expansions of Medicaid in the early 1990s. For example, a Medicaid expansion covered youth in the 1990s. See Leininger (2009).

states expanding the income eligibility of adults with children since then (McMorrow et al., 2016). More recently, the ACA expanded the coverage rate by changing the income eligibility of Medicaid enrollment to 133% below the federal poverty line for all individuals in the states that have adopted the expansion since 2014.[3] These changes in eligibility may have led to significant behavioral changes in Medicaid's beneficiaries through improved access to doctors and information. Because of these health insurance coverage reforms, studies using Medicaid's SCA coverage, such as Liu (2009) and Brantley et al. (2019), would not have been able to separate the effect of SCA drug coverage from that of changes in health insurance coverage, such as reforms from the state-level expansions of Medicaid to adults with children or the ACA. Therefore, the behavioral changes they observed would not reflect the change in drug insurance coverage; rather, the behavioral changes would reflect the changes in both drug and health insurance coverage.

In contrast, our study sets in Canada have an advantage over studies using Medicaid because Canada has provided universal health insurance since 1984. Universal health insurance occurred decades before the SCA coverage, which implemented in 2011 and 2012. Using Canada as a setting would allow the effect of SCA drug insurance coverage to be isolated without influence from health insurance coverage reforms. Thus, any behavioral change observed would reflect the change in the coverage of SCAs as a part of drug insurance coverage. We contribute to the existing literature by isolating the effect of SCA drug coverage from health insurance coverage and investigating its effect on tobacco use.

In addition, we also provide the first evidence of the effect of SCA coverage on a comprehensive set of non-cigarette tobacco use outcomes using a quasi-experiment from Canada. Previous literature focused exclusively on the effect of SCAs on cigarette cessation outcomes and ignored their potential effects on non-cigarette tobacco use outcomes (Liu,

[3] Note that some states have opted out of this expansion, because the Supreme Court of the United States ruled the mandatory expansion of Medicaid by the ACA to be unconstitutional (Rosenabum & Westmoreland, 2012). Thus, states are not mandated to adopt this expansion.

2009: Liu, 2010; Kostova et al., 2018; Brantley et al., 2019). Our Canadian dataset has a rich set of tobacco use outcomes including both cigarette and non-cigarette tobacco use, such as cigar and pipe use, which allows us to examine the effect of SCA coverage on non-cigarette tobacco use outcomes. Finally, we examine the heterogeneous effect of SCA coverage across gender and education, which previous literature has not adequately investigated.

Theoretically, the effect of SCA coverage on tobacco use behavior is ambiguous. SCA coverage would increase access to these drugs and reduce tobacco use as expected from the benefits of SCA, while SCA coverage would not necessarily increase its utilization. Rather, the coverage could promote beneficiaries to consume tobacco due to ex-ante moral hazard (Ehrlich & Becker, 1972). According to Suranovic et al. (1999), individuals' utility of tobacco use is composed of current benefit derived from tobacco use, future losses associated with health, and future and current costs associated with withdrawal. If the SCA coverage reduces the future withdrawal costs associated with tobacco addiction, it would increase the current utility of tobacco use and/or the current disutility of smoking abstinence, which may incentivize an individuals' current smoking behavior (Suranovic et al., 1999). As such, SCA coverage would increase the probability of beneficiaries engaging in tobacco use. Because of these opposite effects, the net effect of SCA coverage on tobacco use is not immediately clear.

To empirically investigate the effect of SCA coverage on tobacco use outcomes, we use variations in the timing of SCA coverage implementation across Canadian provinces since 2011, when several provinces began including SCA coverage in their insurance schemes. Using a DD approach and 2008−2012 Canadian Tobacco Use Monitoring Survey (CTUMS), we find that SCA coverage increases tobacco use, specifically cigarette and cigarillo use. Stratifying the analysis by gender and college education, we find that the ex-ante moral

hazard effect is stronger in men, in those who are college-educated, and those who are younger.

Section 2 introduces the institutional and theoretical background. Sections 3 and 4 describe the data and empirical strategy, respectively, while section 5 presents the results. Finally, section 6 concludes the paper with a discussion.

2. Institutional background

Although Canada has provided universal healthcare since 1984, it does not provide universal prescription drug coverage. Provincial and territorial governments are responsible for providing access to prescription drugs for their respective residents. Drug types that are covered, coverage timing, the price of drugs covered, and coverage length differ across provinces and territories (Anis et al., 2001; Daw & Morgan, 2012; Grégoire et al., 2001). The differences in coverage are primarily due to differences in the evaluation processes of drugs, such as evaluations of drug safety, efficacy, and cost, across provincial and territorial governments (Anis et al., 2001).

Table 2.1 lists the characteristics of provincial SCA coverage. Alberta was the first province to cover SCAs, in October 1998, while Manitoba was the last province to cover SCAs, in November 2011. For Nova Scotia, the date of SCA coverage in 2004 was dependent on funding to the subregions of the province (White et al., 2015). The provincial government of Nova Scotia provided funding to the health authorities of each subregion to combat tobacco use, and authorities could opt to use the funding to cover SCA sometime in 2004 (or later). Therefore, the date of coverage varies across the subregions of Nova Scotia. The SCAs commonly covered by the provinces are varenicline and bupropion. The length of coverage is similar across provinces—12 continuous weeks of coverage annually, except for Nova Scotia

and Prince Edward Island. The length of coverage in Prince Edward Island is much shorter, with approximately four weeks of treatment annually, as it provides only 75 Canadian dollars annually as a reimbursement. The length of coverage for Nova Scotia is dependent on the funding to each subregion and how much is devoted to the coverage, if any. Counseling is not mandatory in exchange for receiving SCAs in all provinces except for Prince Edward Island.

[Table 2.1]

Table 2.1 illustrates our research design, in which a DD approach was used. Given the difference in the timing of SCA coverage across provinces, provinces that never implemented SCA coverage or did not add additional SCAs into the existing coverage between 2008 and 2012 served as control provinces, while provinces that began SCA coverage or added additional SCA into the existing coverage between 2008 and 2012 served as treated provinces. However, there were two challenging issues that affected our setting. First, our natural experiment was confounded by policies that influenced access to (or prices of) prescription drugs. Specifically, three provinces have enacted price controls on prescription drugs: British Columbia, Alberta, and Ontario. In particular, British Columbia implemented the Full Payment Policy and Generic Drug Price Reform before the implementation of SCA coverage.[4] These policies could have significantly increased access to SCA drugs before the drug coverage. This in turn may have diminished the effect of SCA coverage in the said province. More importantly, some of these policies (i.e., Generic Drug Price Reform) were also phased in over three years in British Columbia, which made it difficult to disentangle the effect of SCA coverage from other drug policies. Second, only some subregions of Nova Scotia have implemented SCA coverage, and our data lacked the precise locations of subregions for Nova Scotia to allow for the proper identification of SCA coverage at

[4] For information on the policies, see https://www2.gov.bc.ca/gov/content/health/health-drug-coverage/pharmacare-for-bc-residents/what-we-cover/general-coverage-policies and https://www.claimsecure.com/content/pdfs/en-CA/eNewsBulletins/BC%20Pharmacare%20Reform%20-%20July%202010.pdf.

subregion levels. We excluded British Columbia and Nova Scotia and focused on analyzing the effect of SCA coverage on provinces not affected by these issues.

3. Data and measurements

We used the CTUMS, a nationally representative repeated cross-sectional survey sampled monthly between February and December by Statistics Canada since 1999. The CTUMS uses a two-phase stratified random telephone number sampling. Phase one involves selecting households by random digit dialing, while phase two involves selecting respondents from households based on household composition. The survey asks respondents aged 15 and older detailed questions regarding their smoking habits and household characteristics. We used five cycles of the CTUMS in our analysis: 2008, 2009, 2010, 2011, and 2012.

3.1. Treatment variables

To clarify the treatment variable, we generated a binary variable for SCA coverage. The variable was equal to one if a respondent lived in a province(s) where governments began SCA coverage or added additional SCAs into the existing coverage between 2008 and 2012 and zero if a respondent lived in a province(s) where governments never covered SCAs or did not add additional SCAs into existing coverage between 2008 and 2012 (See Table 2.1).[5] For example, if a respondent lived in Alberta, SCA coverage was equal to one if the respondent was sampled after June 2011. If a respondent was sampled before June 2011 (in the same province), SCA coverage was equal to zero. For a respondent living in a province(s) that never covered SCAs or did not add additional SCAs into the existing coverage between 2008 and 2012, such as New Brunswick or Quebec, the variable was always zero.

[5] Note that Manitoba implemented SCA coverage in the middle of November; therefore, we coded November to be a decimal in 2011 for Manitoba.

3.2. Outcome variables

The primary dependent variables of interest are tobacco use outcomes. We constructed six variables from tobacco-related questions:

- *"At the present time, do you smoke cigarettes every day, occasionally, or not at all?"*
- *"In the past 30 days, did you smoke any cigars not including little cigars or cigarillos?"*
- *"In the past 30 days, did you smoke any little cigars or cigarillos?"*
- *"In the past 30 days, did you smoke a pipe?"*
- *"In the past 30 days, did you use any chewing tobacco, pinch, or snuff?"*

We generated a binary variable for *any cigarette use* that equaled one if a respondent was either a daily current smoker or an occasional current smoker and zero otherwise. To investigate the intensive margin of cigarette use, we also generated a binary variable for *occasionally versus daily* that equaled one if a respondent was a daily current smoker and zero if a respondent was an occasional current smoker.[6] For *any cigar use*, we generated a binary variable equaling one if a respondent smoked at least one cigar in the past 30 days and zero otherwise. For other non-traditional tobacco use outcomes (cigar, pipe, and tobacco chew), we generated a binary variable that equaled one if a respondent used the corresponding tobacco product in the past 30 days and zero otherwise. Finally, to summarize all the tobacco use outcomes, we generated a binary variable for *any tobacco use* that equaled one if a respondent was use any of the above tobacco products and zero if a respondent never use any of these products.

[6] Note that a respondent who never smoked would be missing for this variable.

3.3. Socioeconomic characteristics

For socioeconomic characteristics, *Age* was a continuous variable between 15 and 85, where 85 included respondents aged 85 and older. *Women* equaled one if a respondent was a woman and zero otherwise. For current marital status, *W/S/D* equaled one if a respondent was widowed, separated, or divorced and zero otherwise. *Single* equaled one if a respondent was unmarried and zero otherwise. The omitted category was married respondents. For education, we generated nine binary variables: *Completed elementary*, *Some high school*, *Completed high school*, *Some college, Completed college, Some university, Completed university, Other education,* and *Education—unknown*. These variables equaled one if a respondent attended or completed the respective education level and zero otherwise. The omitted category was respondents who had no education. For language spoken at home, we generated three binary variables: *French only*, *English and French*, and *Languages—unknown*. These variables equaled one if a respondent could speak the respective language and zero otherwise. The omitted category was respondents who spoke English only. For residential area, we generated two binary variables: *Rural* and *Residential area—unknown*. These two variables equaled one if a respondent lived in the corresponding type of residential area and zero otherwise. The omitted category was those who lived in urban areas. For household size, we generated a continuous variable that equaled the number of members within a respondent's household. We also controlled for real cigarette taxes that were obtained from Finances of the Nation and deflated with the consumer price index.[7] Finally, we controlled for generic drugs and smoke-free car policies, which equaled one if a respondent resided in a province that implemented the policies and zero otherwise for each respective policy binary variable.

[7] The data can be obtained from https://www.ctf.ca/ctfweb/en/publications/finances_of_the_nation.aspx.

4. Empirical strategies

To investigate the effect of drug insurance coverage on tobacco use outcomes, we implemented a DD approach for SCA coverage as follows:

$$Y_{ipmt} = \beta SCA\ coverage_{pmt} + \theta_p + \pi_m + \omega_t + x_{ipmt}\alpha' + \eta_{ipmt}, \quad (1)$$

where Y_{ipmt} was the dependent variable of interest, such as tobacco use outcomes, for respondents *i* residing in province *p* in month *m* and year *t*. $SCA\ Coverage_{pmt}$ was a binary variable, our treatment variable, for province *p* that covers SCAs in month *m* and year *t*. θ_p was a vector of provincial binary variables. π_m and ω_t were vectors of month and year binary variables, respectively. x_{ipmt} was a vector of socioeconomic variables, including age, age squared, gender, marital status, education, language spoken at home, residential area, household size, drug policies, smoke-free car laws, and real cigarette taxes for respondents *i*. η_{ipmt} was the error term. We clustered standard errors at province levels and adjusted inferences using t-distribution with modified *G* degrees of freedom, where *G* was the optimal number of cluster groups based on the method proposed by Carter et al. (2017). Cameron et al. (2015) have shown that such a method would significantly improve inferences, even when the cluster group was as low as 10. We adjusted our inferences using t-distribution with five degrees of freedom calculated based on the algorithm of Carter et al. (2017). To make the inferences more conservative, we rejected the null hypothesis at the 5% level as the baseline. To make the sample representative of the Canadian population, we weighted the estimates using normalized sampling weights provided by Statistics Canada (Bataineh et al., 2019). Note that the normalized sampling weights were calculated by dividing the non-normalized

sampling weights by the sum of non-normalized sampling weights in a survey year. Therefore, in a given survey year, normalized sampling weights should sum up to one.

The main parameter of interest was β, which estimated the effect of SCA coverage on tobacco use outcomes. The identification of the parameter relied on the variation in the timing of provincial coverage of SCAs between 2008 and 2012. Credibly identifying these parameters hinged on the assumption that the trends of dependent variables in control provinces were a good counterfactual for the trends in treatment provinces in the absence of SCA coverage, commonly known as the common trend assumption. We used two methods to assess the validity of the common trend assumption. First, we implemented an event study model proposed by Autor (2003):

$$Y_{ipmt} = \sum_{k=-3}^{0} \tau_k SCA\ coverage_{p(t+k)} + \theta_p + \pi_m + \omega_t + x_{ipmt}\zeta' + \eta_{ipmt}, \qquad (2)$$

where $\sum_{k=-3}^{0} \tau_k SCA\ coverage_{p(t+k)}$ corresponded to vectors of binary variables that corresponded to k year(s) prior to the implementation of SCA coverage in treated provinces. For instance, Manitoba began to cover SCAs in November 2011. Therefore, $SCA\ coverage_{pt}$ was a binary variable that equaled one if a respondent lived in Manitoba after November 2011 and zero otherwise. Similarly, $SCA\ coverage_{p(t-2)}$ equaled one if a respondent lived in Manitoba between November 2009 and October 2010 and zero otherwise. The omitted category was one year prior to the implementation of SCA coverage. If the lead terms were insignificant, we could conclude that the common trend assumption was plausible. Although an event study model using yearly periods is standard, it did limit our ability to observe pre-trends, since there were only two lead policy terms, $SCA\ coverage_{p(t-2)}$ and $SCA\ coverage_{p(t-1)}$. A more sophisticated approach would be to

use six-month periods to increase the clarity of the trends over a much finer window of time, but at the cost of diminished power due to smaller sample size in each period for post treatment periods. We also implemented an event study model using six-month periods to complement our baseline event study model.

Finally, in addition to an event study model, we also included province-specific trends into equation (1) to control for unobserved province time-varying confounders. That is, we estimated the following equation:

$$Y_{ipmt} = \delta SCA\ coverage_{pmt} + \theta_p + \pi_m + \omega_t + \sigma_{pm} + \phi_{pt} + x_{ipmt}\gamma' + \eta_{ipmt}, \quad (3)$$

where σ_{pm} was a vector of interaction terms between a vector of province binary variables and a month trend variable and ϕ_{pt} was a vector of interaction terms between a vector of province binary variables and a year trend variable. Given the trends are linear, we also include nonlinear trends in order to mitigate concerns associated with nonlinear unobserved province time-varying confounders. All dependent variables were regressed using ordinary least squares.

5. Results

5.1. Summary statistics

Columns (1)–(5) of Table 2.2 report the weighted summary statistics on outcomes and socioeconomic variables by all, control, and treated provinces across the periods 2008–2010 and 2011–2012, respectively. Comparing dependent variables in control and treated provinces, tobacco use outcomes decreased in both the treated and control provinces over time. Specifically, we observed that *any tobacco use* decreased from 22.1% to 20.5% in

control provinces, and *any cigarette use* decreased from 19.6% to 18.7% in treated provinces. For specific tobacco use outcomes, *any cigarette use* decreased from 19.3% to 18.5% in control provinces, and *any cigarette use* decreased from 17.0% to 16.6% in treated provinces. Similar patterns were also found on *occasionally versus daily*, *any cigar use*, and *any cigarillo use*, though the sizes of the decrease in use differed. Taking a simple unadjusted DD between the two periods and two provinces, cigarette, occasionally versus daily, cigar, and cigarillo use exhibited trends of increasing in the proportions of use in treated provinces between 2011 and 2012. That is, *any cigarette use* increased by 0.4%, *occasionally versus daily* increased by 6.3%, *any cigar use* increased by 0.1%, and *any cigarillo use* increased by 1.6% in the treated provinces after SCA coverage. Other tobacco use outcomes, such as cigar, pipe, and tobacco chew, had no increases. Overall, a simple DD with summary statistics seemed to suggest that both cessation and moral hazard effects were somewhat plausible, but the ex-ante moral hazard channel seemed to dominate. A detailed analysis is required.

[Table 2.2]

5.2. Main results

Table 2.3 reports the effect of SCA coverage on SCA and tobacco use outcomes from Equation (1). Columns (1)−(7) present the estimates for *any tobacco use*, *any cigarette use*, *occasionally versus daily*, *any cigar use*, *any pipe use*, and *any tobacco chew use*, respectively. Based on Table 3, SCA coverage increased *any tobacco use* by 2.5 percentage points. Specifically, our estimates suggest that SCA coverage significantly increased both *any cigarette use* and *any cigarillo use* by 1.7 percentage points. Similar to these two outcomes, SCA coverage increased *any cigar use* by 0.3 percentage points, though it was insignificant. SCA coverage did not affect the outcomes for occasionally versus daily, pipe, or tobacco chew uses, since the magnitudes of the estimates were close to zero and statistically insignificant. This implies that SCA coverage induced the ex-ante moral hazard response for

some tobacco outcomes among the beneficiaries. In other words, SCA coverage made beneficiaries more prone to consume tobacco due to fewer costs associated with future cessation.

[Table 2.3]

Figure 2.1 shows the coefficient plot of the event study model from Equation (2).[8] The omitted level was one year prior to the implementation of SCA coverage. Each panel reports a different dependent variable. From the figure, we did not observe significant pre-trends for *any tobacco use*, *any cigarette use*, or *any cigarillo use*, since the estimates from the lead policy terms, -3+ and -2, were statistically insignificant, and the magnitudes of the estimates for the lead policy terms were trivial compared to the estimate for the current policy term. There appeared to be small increasing pre-trends for *any cigarillo use*, but the increase was trivial compared to the increase for the current policy term. However, we observed significant pre-trends for *any cigar use* for the lead policy term, -2, and the magnitude of the estimate for the lead policy term was similar to the magnitude of the estimate for the current policy term. This implies that the ex-ante moral hazard for *any cigar use* was driven by pre-existing trends. More importantly, we did not observe significant pre-trends for *any tobacco use*, *any cigarette use*, or *any cigarillo use*, suggesting the common trend assumption was plausible for these outcomes.

[Figure 2.1]

Figure 2.2 shows the coefficient plot of the event study model using six-month periods instead of yearly periods.[9] The model can provide a clearer picture of pre-trends before the coverage. The omitted level was six months prior to the implementation of SCA coverage. Each panel reports a different dependent variable. Similar to Figure 2.1, we did not observe strong pre-trends for *any tobacco use*, *any cigarette use*, or *any cigarillo use*. From panels A

[8] The full results of Figure 2.1 are reported in Appendix Table B.2.1.
[9] The full results of Figure 2.2 are reported in Appendix Table B.2.2.

and V, we can see that the magnitudes of the lead policy terms were close to zero, and the estimates were insignificant. In addition, we observed a significant increase in *any tobacco use* and *any cigarette use* in the estimate for the current policy term, but the estimate for the post-policy term was trivial in magnitude and insignificant. Based on panel D, we observed that the estimates for the lead policy terms of *any cigarillo use* were trivial in magnitudes, and they were all statistically insignificant. The estimates for the lead policy terms were increasing between -5+ and -3, but the increases were trivial compared to the significant increase for the current policy term, 0, for *any cigarillo use*. For *any cigar use*, we can also see that there were significant lead policy terms in -3 and -2 similar to Figure 2.1. This suggests significant pre-trends were driving *any cigar use*. Based on the two event studies, we may conclude that the common trend assumption was plausible for *any tobacco use*, *any cigarette use*, and *any cigarillo use*, and the estimates were not driven by pre-trends.

[Figure 2.2]

Figure 2.3 represents the coefficient plot of the specifications including province-specific linear and nonlinear trends from Equation (3).[10] Each panel reports a different dependent variable. Each dot represents a different specification. Based on the panels A and D of Figure 3, the magnitudes and significance of the estimates for *any tobacco use*, *any cigarette use*, and *any cigarillo use* were relatively stable across specifications. That is, the inclusion of province-specific linear and nonlinear trends did not much affect our estimates. This implies that our estimates for *any tobacco use*, *any cigarette use*, and *any cigarillo use* were unlikely to be a product of unobserved province-specific trends.[11] Reassuringly, this suggests that the common trend assumption was plausible for these outcomes.

[Figure 2.3]

[10] The full results of Figure 2.3 are reported in Appendix Table B.2.3.
[11] We also tested the robustness of our results by including region-specific linear and nonlinear trends. The full results are reported in Appendix Table B4 and Appendix Figure B.2.1.

Table 2.4 reports the estimates across subpopulations. Panels A and B report the estimates for men and women, panels C and D report the estimates for < college-educated and ≥ college-educated, and panels E and F the estimates for ≤ 40 years-old and > 40 years-old. Based on panel A, the estimates suggest that SCA coverage significantly increased by 3.6, 2.6, and 2.2 percentage points for men for *any tobacco use*, *any cigarette use*, and *any cigarillo use*, respectively. Based on panel B, the estimates suggest that SCA coverage increased 1.5, 0.9, and 1.2 percentage points for women for *any tobacco use*, *any cigarette use*, and *any cigarillo use*, respectively, but only the estimate for *any cigarillo use* was significant. This indicates that the moral hazard effect of SCA coverage affected men more than women. In panels C and D, we find that SCA coverage increased *any tobacco use*, *any cigarette use*, and *any cigarillo use* by 1.6, 0.8, and 0.6 percentage points, respectively, but all estimates were insignificant for < college-educated. In contrast, SCA coverage significantly increased *any tobacco use*, *any cigarette use*, and *any cigarillo use* by 2.2, 1.4, and 2.5 percentage points, respectively for ≥ college-educated. It seems to suggest the moral hazard affected ≥ college-educated more than < college-educated. Finally, in panels E and F, we observed that SCA coverage significantly increased any *tobacco use*, *any cigarette use*, and *any cigarillo use* by 3.9, 3.1, and 2.8 percentage points, respectively for less than and equal to 40 years-old, while SCA coverage only significantly increased *any cigarillo use* by 0.9 percentage points for greater than 40 years-old. This suggests that SCA coverage is driven mainly by younger population.[12]

Based on main results and event study for age stratification, we observed that the effect of SCA on short-term smoking behavior was present only for those who are younger than 40 years-old. According to the Grossman (1972), the discount rate for one's own health is higher when one is younger. Therefore, if SCA, which lowers the future withdrawal cost of

[12] Note that we present the event study for those younger and older than 40 years-old in Appendix Figures A.2.2 and A.2.3. The effect is mainly driven by younger population as expected with tobacco experimentation.

smoking, is covered by insurance, there will be a difference in the future costs of smoking between youth and elderly. That is, because the discount rate is higher for youth, the current utility of smoking may be higher than for youth. In short, the short-term increase in the probability of smoking due to the insurance coverage of SCA may be the result of the increased probability that young people will become new smokers. On the other hand, the effect is only short-term because nicotine dependence has a minimum threshold of smoking, which is determined by smoking history and other physiological factors (Suranovic et al., 1999), but insurance coverage for SCA does not affect this threshold. Thereby, the probability of smoking increases, especially among young people, because of the lower future cost of smoking and the higher discount rate for health, while this effect lasts only in the short term because it has no effect on the number of people who cross the threshold and become nicotine dependent.

[Table 2.4]

We also stratified the SCA coverage by the type of SCAs covered. That is, we stratified the SCA coverage into three binary variables: *SCA coverage adding varenicline with existing bupropion coverage*, *SCA coverage adding varenicline without existing bupropion coverage*, and *SCA coverage adding both drugs*. Table 5 reports the estimates of SCA coverage by the type of SCA covered. Interestingly, we observed that the provinces covering varenicline with existing bupropion coverage, Alberta and Ontario, had increased cigarette and cigarillo use. Given that these provinces covered bupropion almost a decade ago, it is possible that more people in these provinces had knowledge about these drugs and their effects. This could lead to more people abusing substances (i.e., tobacco) when they realized that the provinces were covering these drugs (or treatments) for free. This may also explain why our heterogeneity analysis shows that highly educated people were more likely to suffer from moral hazard than those who were less educated. It would imply that

knowledge played a role in mediating the ex-ante moral hazard response. It is also pointing out that provinces without any pre-existing coverage seems to have some reduction effect as intended by SCA coverage. One plausible explanation is that people in these provinces do not have the knowledge as the provinces with pre-existing coverage of SCA have. Therefore, these people are more likely to behave as the SCA coverage intended, leading a decline in tobacco use.

[Table 2.5]

There are two plausible mechanisms for the ex-ante moral hazard response. Our ex-ante moral hazard could be explained by either an increase in initiation or a decrease in cessation. To investigate the mechanisms, we generated two dependent variables from the questions, "*Are you seriously considering quitting within the next six months?*" and "*Are you seriously considering quitting within the next 30 days?*" to gauge a respondent's intention to quit.[13] Table 2.6 reports the estimates of the effect of SCA coverage on quitting intention outcomes. The estimates suggest that SCA coverage increased *the intention to quit in the next six months* and *the intention to quit in the next 30 days* by 0.4 and 0.8 percentage points, respectively, but both estimates were statistically insignificant. This implies that SCA coverage had little effect on intention to quit, given that the estimates were insignificant, and the magnitudes of the estimates were small. In other words, it is plausible that the ex-ante moral hazard was mainly driven by an increase in initiation, not a decrease in cessation.

[Table 2.6]

[13] Note that the questions were asked only to current smokers. Therefore, the sample sizes of these dependent variables were smaller than other dependent variables.

5.3. Additional robustness check

We assessed the robustness of our results by performing a leave-out analysis. For the leave-out analysis, we sequentially excluded treated provinces from our baseline estimation. This was to address the concern that only a single treated province was driving our results. Figure 2.4 shows the coefficient plot of the leave-out analysis. Each panel reports a different dependent variable.[14] Overall, we observed that the magnitudes of the estimates were relatively stable across the specifications that excluded different treated provinces, although we lost some precision when we excluded Ontario, since it was a major population center that consisted of nearly 50% of the Canadian population. This suggests that our results were unlikely to be a product of a single treated province.

[Figure 2.4]

To further check the plausibility of the common trend assumption, we used alternative control provinces. That is, according to Table 2.1, our control provinces were divided into two types: *never adopted provinces* versus *always adopted provinces*. *Never adopted provinces* were provinces that never covered SCAs during our data period. *Always adopted provinces* were provinces that covered SCAs prior to our data period (2008). Excluding one type of these provinces from the analysis could remove some concerns about the unobserved contaminations affecting the assumption. Appendix Table A.2.1 reports the estimates excluding *never adopted provinces*. The results of this analysis were similar to our main results. For Appendix Table A.2.2, we excluded the *always adopted provinces* from the analysis. We also did not find any significant difference between these estimates and those from the main results. We also assessed the sensitivity of our estimates to alternative inference procedures. Baker et al. (2008) have shown that clustering by the province and month levels can yield a more conservative inference. We clustered by the province and

[14] The full results of Figure 4 are reported in Appendix Table B.2.5.

month levels for all the estimates. Panel A of Appendix Table A.2.3 reports the estimates clustering by the province and month levels. We also assessed the robustness of our inference clustering by the province and month levels using wild bootstrapping. Panel B Appendix Table A.2.3 reports the estimates clustering by the province and month levels using wild bootstrapping. The significance using alternative cluster methods did not vary significantly from the main results. We also weighted the estimates using non-normalized sampling weights, and we ran the same model with Probit. Appendix Tables A.2.4 and A.2.5 report the estimates weighted with non-normalized weights and the estimates estimated using Probit, respectively. The magnitudes and significance of these estimates did not differ significantly from the main estimates. Finally, a concern was raised that our provincial policies seemed to cluster near 2011.[15] It is plausible that some unobserved trends may have been correlated with tobacco use outcomes in 2011. To address this, we included other province(s) that covered SCAs before 2011 as treated provinces. That is, we included Quebec, which covered SCAs in 2007. To include Quebec, we used pre-2008 CTUMSs.[16] Appendix Table A.2.6 and Appendix Figure A.2.1 report the estimates of baseline results and event studies using pre-2008 CTUMSs. The results did not vary significantly from our main results.

6. Discussion

In this paper, we examined the effect of SCA coverage on tobacco use outcomes. Using provincial variations in the timing of SCA coverage with a DD approach, we found that SCA coverage increased *any tobacco use*, *any cigarette use*, and *any cigarillo use* but did not

[15] We thank the anonymous reviewer for this point.
[16] In our main analysis, we only limited data up to 2008 for the sake of longer pre-trends and consistency across all outcomes in event study models. This is because of how data was collected and the dates of SCA coverage for Quebec. Cigar and cigarillo outcomes were only collected separately since 2007, and Quebec implemented SCA coverage close to the beginning of 2007. The inclusion of data before 2008 prevented us from implementation of the event study models using yearly periods for these outcomes, and it would also limit our ability to observe pre-trends in the event study models using six-month periods for these outcomes.

affect the intensive margin of cigarette use or other non-cigarette tobacco uses, such as pipe and tobacco chew. Specifically, we found that SCA coverage increased *any tobacco use* by 2.5 percentage points and *any cigarette use* and *any cigarillo use* both by 1.7 percentage points.[17] We further checked the validity of the common trend assumption by performing an event study model and including province-specific linear and nonlinear trends into the baseline specification. These analyses suggested that our estimates were not driven by pre-existing trends for *any tobacco use*, *any cigarette use*, and *any cigarillo use*. We also explored heterogeneity across gender, college education, and age. Across gender, we found that SCA coverage induced a stronger ex-ante moral hazard response in men than in women. For college education, we found that SCA coverage led to a stronger moral hazard response in ≥ college-educated than in < college-educated. By age, we found that SCA coverage led to a much stronger moral hazard response in those ≤ 40 years-old than > 40 years-old. Our results suggest that SCA coverage may have unintended consequences on tobacco use behavior.

Our findings stand in contrast to the findings from the literature about Medicaid's SCA coverage and most of the literature on the effect of insurance coverage of Medicaid on the ex-ante moral hazard (Liu, 2009; Liu, 2010; Brantley et al., 2019; Kostova et al., 2018; Koma et al., 2018; Cawley et al., 2018; Courtemanche et al., 2018; Simon et al., 2017; Cotti et al., 2019; Soni, 2020). Courtemanche et al. (2018) and Courtemanche et al. (2019) found no moral hazard effect on cigarette use using Medicaid's expansion from the ACA. As a matter of fact, most literature has shown that insurance coverage, including Medicaid's SCA coverage, has negative effects on cigarette use. For example, Soni (2020) showed that the

[17] Our estimate on cigarette use translated to about a 10% change in prevalence of cigarette use. The magnitude of change seems large but is plausible given the characteristics of treated provinces and the relative amounts of smokers in the total populations of these provinces. Our treated provinces were more urbanized (i.e., Ontario). Therefore, the treatment may have been taken up more readily by the population due to information travelling faster and easier access to treatment because of the close proximity to pharmacies in these provinces. Moreover, the number of smokers in these (treated) provinces was about 2.89 million in 2010 (total population divided by two multiplied by cigarette prevalence, or 34/2×0.17). This means only about 0.3 million people needed to take up the treatment to produce 10%, which was approximately 0.88% of the entire Canadian population in 2010. That is, given the small size of smoker population, only a small number of people needed to take up treatment to produce a modest effect.

ACA's Medicaid expansion significantly reduces cigarette use and consumption using Nielsen Consumer Panel from 2011–2015. Why do our findings differ? Given many reforms in health insurance coverage within Medicaid, it is plausible that improved access to doctors due to an expansion of health insurance coverage may be driving the negative effects of Medicaid's SCA coverage. The U.S. Preventive Services Task Force has created a guideline to ask doctors (or primary healthcare providers) to screen for risky health behavior in Medicaid and provide counselling (USPSTF, 2007). It may be that people accessing counselling or being directed to use SCAs (as SCAs are covered) is driving the observed effect in the case of Medicaid. This suggests that SCA coverage without any overlapping health insurance coverage reform may have unintended consequences on tobacco use outcomes.

In addition, our findings have other implications as well. Given that our findings not only show ex-ante moral hazard but also are driven by an increase in initiation, governments wishing to cover these drugs in an effort to reduce tobacco use may need to implement complementary policy interventions to prevent undue risky behaviors from occurring when such coverage is implemented. For instance, governments may be able to complement SCA coverage with reducing benefits or requiring counseling for beneficiaries that start consuming tobacco after the coverage. Such complements may be able to reduce the ex-ante moral hazard response. Furthermore, the moral hazard effect is much stronger in men than in women. The policy interventions should target some subpopulations over others.

Our study had limitations. Our dependent variable data were self-reported. Self-reported data is likely to suffer from desirability bias; respondents are likely to under-report their number of cigarettes smoked daily in order to appear more desirable. Using objective measurements such as blood-nicotine levels would alleviate this concern. Nonetheless, levels could not be measured across the entire population, making generalizability difficult. Using

self-reported data was a reasonable trade-off given this difficulty. In addition, the short time duration of our dataset limited our ability to examine the common trend assumption to only two years prior to the implementation of the coverage, though our results from the event study and the inclusion of trends suggested the assumption is unlikely to be violated. Finally, the effects of the coverage we evaluated should be considered as short-term effects rather than long-term effects. As our policy are evaluated with data near the end of our dataset, our implications can only be applicable to similar policies implemented with short time periods.

Recently, the provincial governments of New Brunswick and Newfoundland and Labrador began covering SCAs as part of their drug insurance plans in 2014. While policymakers intended to reduce tobacco use among the populations in these provinces, our study points to the unintended consequences of such coverage. The policymakers from these provinces may need to consider additional complementary policies in conjunction with SCA coverage to avoid the ex-ante moral hazard response. Moreover, in August 2017, Health Canada approved cytisine for sale in Canada (Karnieg & Wang, 2018). Cytisine, like varenicline, mimics nicotine and binds the receptors of nicotine to reduce withdrawal symptoms associated with smoking cessation (Jeong et al., 2015). Similar to all other SCAs, reviews have shown cytisine's effectiveness in reducing tobacco use (Hajek et al., 2013). Although cytisine has not been covered under any provincial or territorial insurance plans, it does raise concerns about the possibility of the ex-ante moral hazard response when provincial and territorial governments decide to cover the said drug in their drug insurance plans. Policymakers who intend to cover this drug need to consider the potential ex-ante moral hazard responses among beneficiaries when weighing the costs and benefits of such coverage.

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Table 2.1.Timing and characteristics of provincial coverage of SCA

Province	Date	Drugs covered	Length of coverage	Mandatory Counselling
Alberta	10/1998: bupropion; 06/2011: varenicline	Bupropion and varenicline	12 continuous weeks/year	No*
British Columbia	10/2011	Bupropion and varenicline	12 continuous weeks/year	No
Manitoba	11/2011	Varenicline	12 continuous weeks/year	No
New Brunswick	None			
Newfoundland & Labrador	None			
Nova Scotia	2004 (dates vary by sub-regions and depend on funding)	Bupropion and varenicline	Varies by subregion	No
Ontario	04/2000: bupropion; 08/2011: varenicline;	Bupropion and varenicline	12 continuous weeks/year	No
Prince Edward Island	01/2001	Bupropion and varenicline	~ 4 weeks/year (or CAN$75/year)	Yes
Saskatchewan	01/2011	Bupropion and varenicline	12 continuous weeks/year	No
Quebec	10/2000: bupropion; 10/2007: varenicline	Bupropion and varenicline	12 continuous weeks/year	No

Source: White et al. (2015)
* Alberta does not require any counselling for 12 weeks of varenicline. However, a beneficiary can participate in counselling which allows the benefit to be extended to 24 weeks (first 12 weeks still do not require any counselling).

Table 2.2. Summary Statistics

	All	Control		Treated	
	All	2008–2010	2011–2012	2008–2010	2011–2012
	(1)	(2)	(3)	(4)	(5)
Outcome Variables:					
Any Tobacco Use	0.199	0.221	0.205	0.196	0.187
	(0.400)	(0.415)	(0.403)	(0.397)	(0.390)
Any Cigarette Use	0.175	0.193	0.185	0.170	0.166
	(0.380)	(0.395)	(0.388)	(0.375)	(0.372)
Occasionally versus Daily	0.767	0.812	0.768	0.743	0.762
	(0.423)	(0.391)	(0.422)	(0.437)	(0.426)
Any Cigar Use	0.016	0.019	0.015	0.017	0.014
	(0.127)	(0.136)	(0.122)	(0.131)	(0.117)
Any Cigarillo Use	0.033	0.045	0.028	0.031	0.030
	(0.178)	(0.208)	(0.165)	(0.173)	(0.170)
Any Pipe Use	0.004	0.004	0.003	0.004	0.003
	(0.060)	(0.063)	(0.052)	(0.065)	(0.053)
Any Tobacco Chew	0.005	0.002	0.002	0.006	0.006
	(0.068)	(0.045)	(0.040)	(0.079)	(0.076)
Socioeconomic Variables:					
Age	44.856	45.710	46.104	44.160	44.651
	(18.160)	(18.163)	(18.198)	(18.057)	(18.235)
Women	0.506	0.508	0.506	0.506	0.505
	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)
S/W/D	0.110	0.124	0.125	0.102	0.106
	(0.313)	(0.329)	(0.331)	(0.303)	(0.308)
Single	0.263	0.261	0.250	0.266	0.267
	(0.441)	(0.439)	(0.433)	(0.442)	(0.443)
Marital status–unknown	0.008	0.011	0.006	0.008	0.007

	(0.090)	(0.106)	(0.077)	(0.087)	(0.086)
Completed elementary	0.017	0.025	0.017	0.014	0.015
	(0.129)	(0.157)	(0.130)	(0.117)	(0.121)
Some high school	0.138	0.152	0.157	0.133	0.126
	(0.345)	(0.359)	(0.364)	(0.339)	(0.332)
Completed high school	0.217	0.221	0.205	0.219	0.217
	(0.412)	(0.415)	(0.404)	(0.414)	(0.412)
Some college	0.049	0.056	0.055	0.045	0.047
	(0.216)	(0.231)	(0.227)	(0.207)	(0.211)
Completed college	0.203	0.195	0.207	0.212	0.194
	(0.402)	(0.396)	(0.405)	(0.409)	(0.396)
Some university	0.064	0.054	0.048	0.065	0.078
	(0.245)	(0.226)	(0.214)	(0.247)	(0.268)
Completed university	0.270	0.247	0.256	0.272	0.293
	(0.444)	(0.431)	(0.436)	(0.445)	(0.455)
Education–Other	0.011	0.009	0.019	0.014	0.005
	(0.106)	(0.096)	(0.137)	(0.118)	(0.071)
Education–unknown	0.019	0.015	0.020	0.019	0.020
	(0.136)	(0.123)	(0.142)	(0.137)	(0.142)
French only	0.247	0.720	0.705	0.016	0.015
	(0.431)	(0.449)	(0.456)	(0.127)	(0.120)
English and French	0.008	0.017	0.013	0.006	0.003
	(0.090)	(0.129)	(0.115)	(0.077)	(0.052)
Languages–unknown	0.108	0.054	0.068	0.127	0.140
	(0.311)	(0.225)	(0.252)	(0.333)	(0.347)
Rural	0.192	0.232	0.231	0.176	0.168
	(0.394)	(0.422)	(0.421)	(0.381)	(0.374)
Residential area–unknown	0.026	0.023	0.023	0.024	0.031
	(0.158)	(0.151)	(0.151)	(0.153)	(0.172)
Household size	2.913	2.712	2.720	2.993	3.038
	(1.261)	(1.217)	(1.204)	(1.271)	(1.275)
Real cigarette taxes (cents)	37.676	33.988	33.826	39.947	38.906

	(6.187)	(3.976)	(4.909)	(5.629)	(6.721)
N	81173	24096	16309	24816	15952

Note: There are four control provinces and four treated provinces. The standard deviations are reported in round brackets. All statistics are weighted by normalized sampling weights provided by Statistics Canada.

Table 2.3. The effect of SCA coverage on tobacco use outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Any tobacco use	Any cigarette use	Occasion versus Daily	Any cigar use	Any cigarillo use	Any pipe use	Any tobacco chew
SCA Coverage	0.025**	0.017**	-0.001	0.003	0.017*	0.000	-0.000
	(0.006)	(0.004)	(0.011)	(0.002)	(0.004)	(0.001)	(0.001)
Socioeconomic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Drug Policies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Smoke-Free Car Laws	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	81173	81173	15073	80872	80876	80880	80882

Note: Columns (1)−(7) report estimates for any tobacco use, any cigarette use, occasionally versus daily use, any cigar use, any cigarillo use, any pipe use, and any tobacco chew, respectively. Socioeconomic controls are age, age squared, gender, marital status, educational attainment, household size, languages, and other provincial-level variables. Province fixed effects are a vector of province-binary variables. Time fixed effects are vectors of month-and year-binary variables. Standard errors are clustered at the province levels. Inferences are adjusted using t-distribution with 5 degrees of freedom. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.4. Heterogeneity across subpopulation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Any tobacco use	Any cigarette use	Occasionally versus Daily	Any cigar use	Any cigarillo use	Any pipe use	Any tobacco chew
Panel A:Men							
SCA Coverage	0.036**	0.026**	0.019	0.004	0.022**	0.001	0.000
	(0.008)	(0.006)	(0.018)	(0.003)	(0.005)	(0.001)	(0.001)
N	37260	37260	7728	37082	37092	37088	37092
Panel B: Women							
SCA Coverage	0.015	0.009	-0.026	0.003*	0.012*	-0.001	-0.001
	(0.006)	(0.004)	(0.011)	(0.001)	(0.004)	(0.000)	(0.000)
N	43913	43913	7345	43790	43784	43792	43790
Panel C: < College							
SCA Coverage	0.016	0.008	0.015	0.008*	0.006	0.001	-0.003*
	(0.011)	(0.008)	(0.025)	(0.003)	(0.003)	(0.002)	(0.001)
N	42117	42117	8960	42082	42087	42104	42103
Panel D: ≥ College							
SCA Coverage	0.022**	0.014***	-0.018	0.000	0.025*	-0.001	0.002
	(0.004)	(0.003)	(0.009)	(0.001)	(0.008)	(0.000)	(0.001)
N	37254	37254	5623	37235	37221	37239	37249
Panel E: ≤40							
SCA Coverage	0.039*	0.031*	-0.013	0.002	0.028*	0.001	-0.000
	(0.011)	(0.011)	(0.008)	(0.004)	(0.008)	(0.000)	(0.001)
N	47948	47948	9478	47803	47808	47805	47804
Panel E: >40							
SCA Coverage	0.006	-0.002	0.015	0.004	0.009*	-0.000	-0.000
	(0.009)	(0.010)	(0.029)	(0.002)	(0.002)	(0.001)	(0.001)
N	33225	33225	5595	33069	33068	33075	33078

Note: Columns (1)−(7) report estimates for any tobacco use, any cigarette use, occasionally versus daily use, any cigar use, any cigarillo use, any pipe use, and any tobacco chew, respectively. Socioeconomic controls are age, age squared, gender, marital status, educational attainment,

household size, languages, and other provincial-level variables. Province fixed effects are a vector of province-binary variables. Time fixed effects are vectors of month-and year-binary variables. Standard errors are clustered at the province levels. Inferences are adjusted using t-distribution with 5 degrees of freedom. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.5. The effect of SCA coverage on tobacco use outcomes by types of drugs covered

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Any tobacco use	Any cigarette use	Occasionally versus Daily	Any cigar use	Any cigarillo use	Any pipe use	Any tobacco chew
SCA Coverage Adding Varenicline with Existing Bupropion Coverage	0.031***	0.021***	-0.006	0.006**	0.021**	0.000	-0.001
	(0.003)	(0.002)	(0.011)	(0.002)	(0.004)	(0.001)	(0.001)
SCA Coverage Adding Varenicline without Existing Bupropion Coverage	0.000	0.011	0.044	-0.015*	-0.003	-0.002*	0.002
	(0.008)	(0.006)	(0.030)	(0.005)	(0.006)	(0.001)	(0.003)
SCA Coverage Adding Both Drugs	-0.008*	-0.010	0.006	-0.002	0.002	0.001*	0.003**
	(0.003)	(0.004)	(0.015)	(0.001)	(0.003)	(0.000)	(0.001)
Socioeconomic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Drug Policies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Smoke-Free Car Laws	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	81173	81173	15073	80872	80876	80880	80882

Note: Columns (1)−(7) report estimates for any tobacco use, any cigarette use, occasion versus daily use, any cigar use, any cigarillo use, any pipe use, and any tobacco chew, respectively. SCA Coverage Adding Varenicline with Existing Bupropion Coverage is provinces covering varenicline with existing bupropion coverage (Alberta and Ontario); SCA Coverage Adding Varenicline without Existing Bupropion Coverage is province covering varenicline without existing coverage of any SCA (Manitoba); and SCA Coverage Adding Both Drugs is province covering both drugs at the same time (Saskatchewan). Socioeconomic controls are age, age squared, gender, marital status, educational attainment, household size, languages, and other provincial-level variables. Province fixed effects are a vector of province-binary variables. Time fixed effects are vectors of month-and year-binary variables. Standard errors are clustered at the province levels. Inferences are adjusted using t-distribution with 5 degrees of freedom. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.6. The effect of SCA coverage on cessation intention

	(1)	(2)
	Quit in Next 6 Months	Quit in 30 Days
SCA Coverage	0.004	0.008
	(0.034)	(0.022)
Socioeconomic controls	Yes	Yes
Province Fixed Effects	Yes	Yes
Time Fixed Effects	Yes	Yes
Drug Policies	Yes	Yes
Smoke-Free Car Laws	Yes	Yes
N	14400	14400

Note: Columns (1) and (2) report estimates for Quit in 6 Next Months and Quit in 30 Days, respectively. Socioeconomic controls are age, age squared, gender, marital status, educational attainment, household size, languages, and other provincial-level variables. Province fixed effects are a vector of province-binary variables. Time fixed effects are vectors of month-and year-binary variables. Standard errors are clustered at the province levels. Inferences are adjusted using t-distribution with 5 degrees of freedom. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

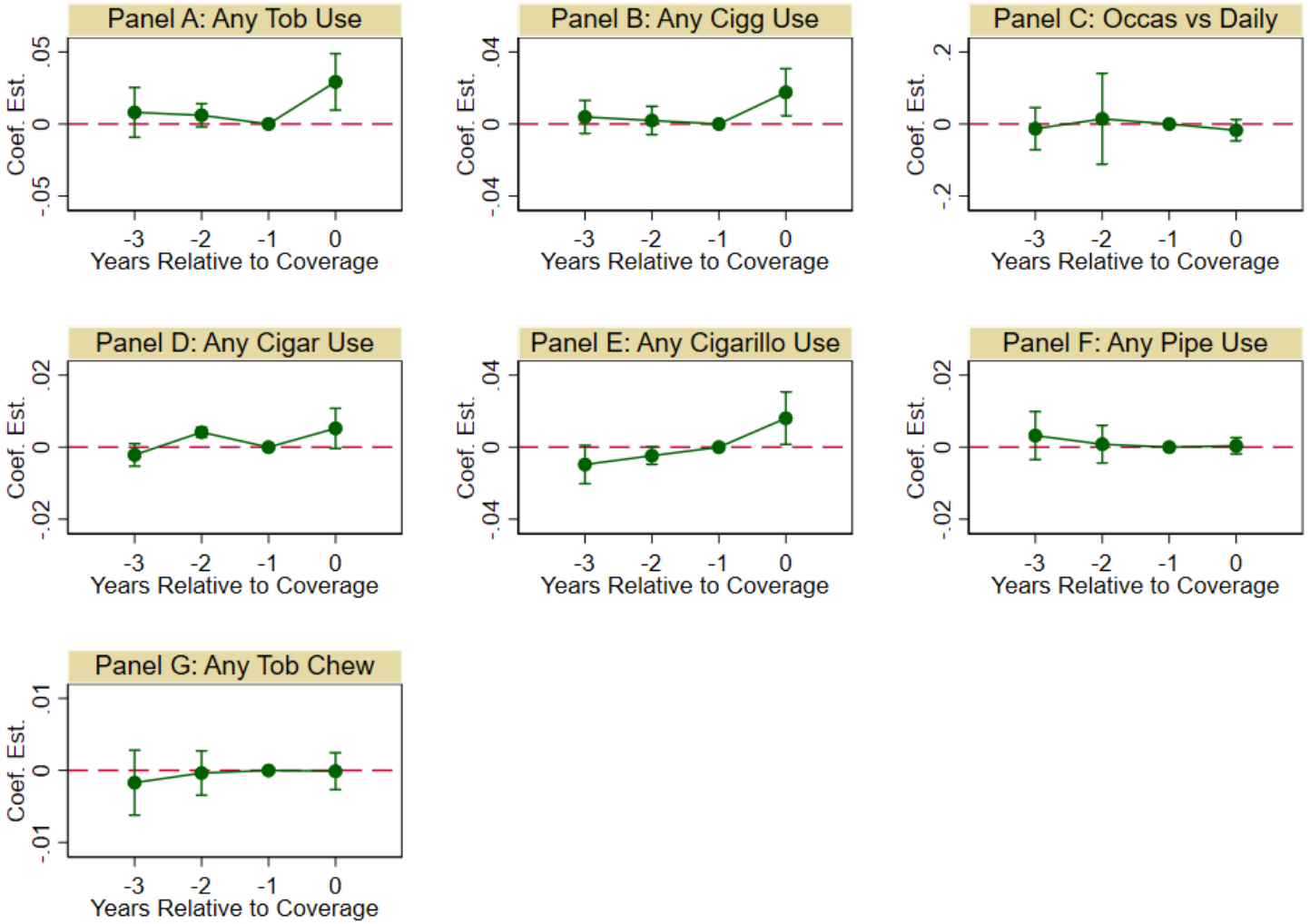

Panel A: Any Tob Use
Coef. Est.
-.05
0
.05
-3
-2
-1
0
Years Relative to Coverage
Panel B: Any Cigg Use
Coef. Est.
-.04
0
.04
-3
-2
-1
0
Years Relative to Coverage
Panel C: Occas vs Daily
Coef. Est.
-.2
0
.2
-3
-2
-1
0
Years Relative to Coverage
Panel D: Any Cigar Use
Coef. Est.
-.02
0
.02
-3
-2
-1
0
Years Relative to Coverage
Panel E: Any Cigarillo Use
Coef. Est.
-.04
0
.04
-3
-2
-1
0
Years Relative to Coverage
Panel F: Any Pipe Use
Coef. Est.
-.02
0
.02
-3
-2
-1
0
Years Relative to Coverage
Panel G: Any Tob Chew
Coef. Est.
-.01
0
.01
-3
-2
-1
0
Years Relative to Coverage

Figure 2.1. Event study coefficient plot.

Note: Each panel reports the estimates of SCA coverage from equation (2) and reports the estimate for a different dependent variable. The omitted category is one year prior to SCA coverage. Control variables include age, age squared, gender, marital status, educational attainment, household size, languages, other provincial-level variables, and all the fixed effects. Standard errors are clustered at the province levels. Confidence intervals are adjusted using t-distribution with 5 degrees of freedom. The dots represent point estimates, and the caps represent 95% confidence intervals.

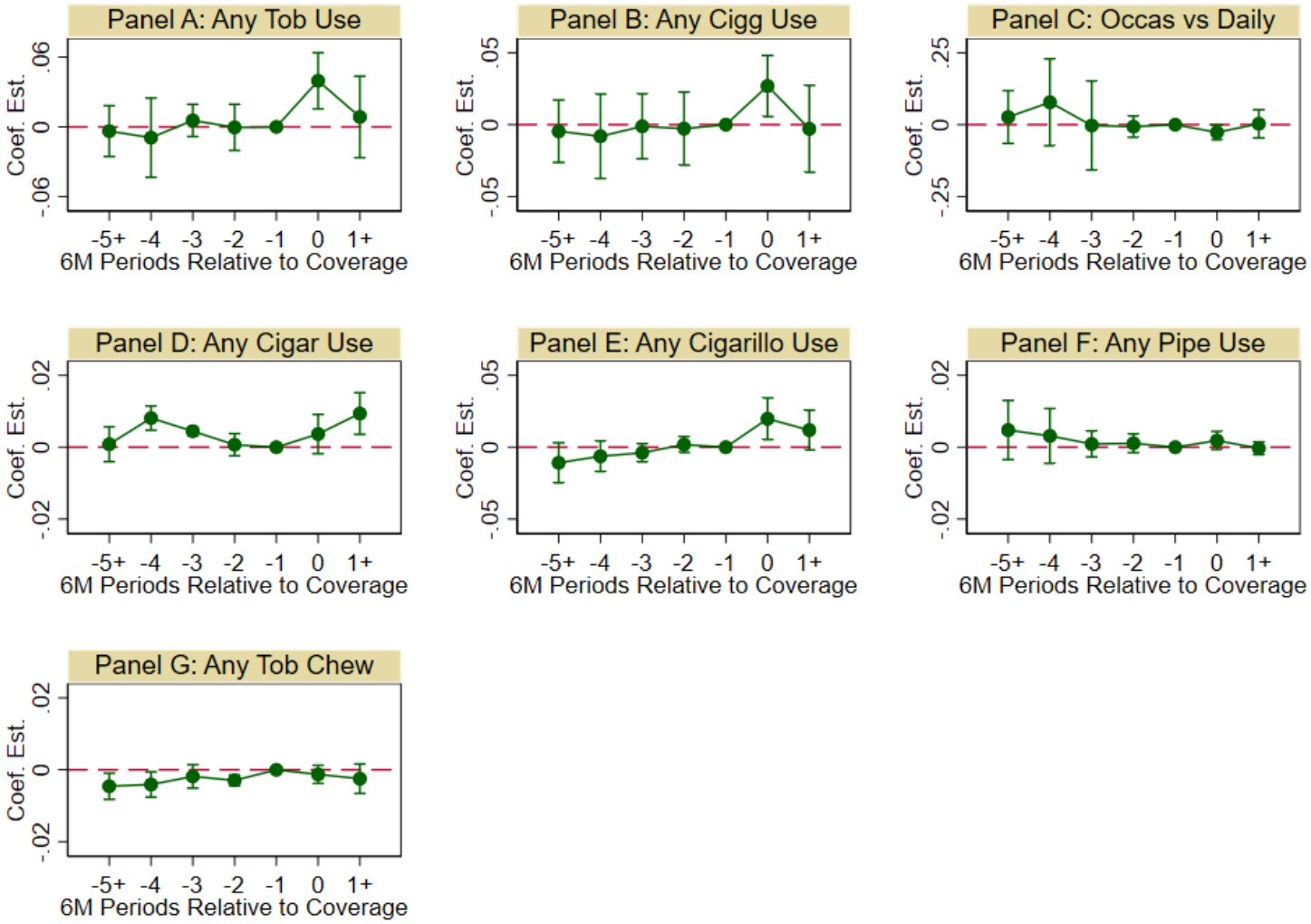


Figure 2.2. Event study coefficient plot using six-month periods.

Note: Each panel reports the estimates of SCA coverage similar to equation (2) but with six-month periods, instead of one year periods. Each panel reports the estimate for a different dependent variable. The omitted category is six months prior to SCA coverage. Control variables include age, age squared, gender, marital status, educational attainment, household size, languages, other provincial-level variables, and all the fixed effects. Standard errors are clustered at the province levels. Confidence intervals are adjusted using t-distribution with 5 degrees of freedom. The dots represent point estimates, and the caps represent 95% confidence intervals.

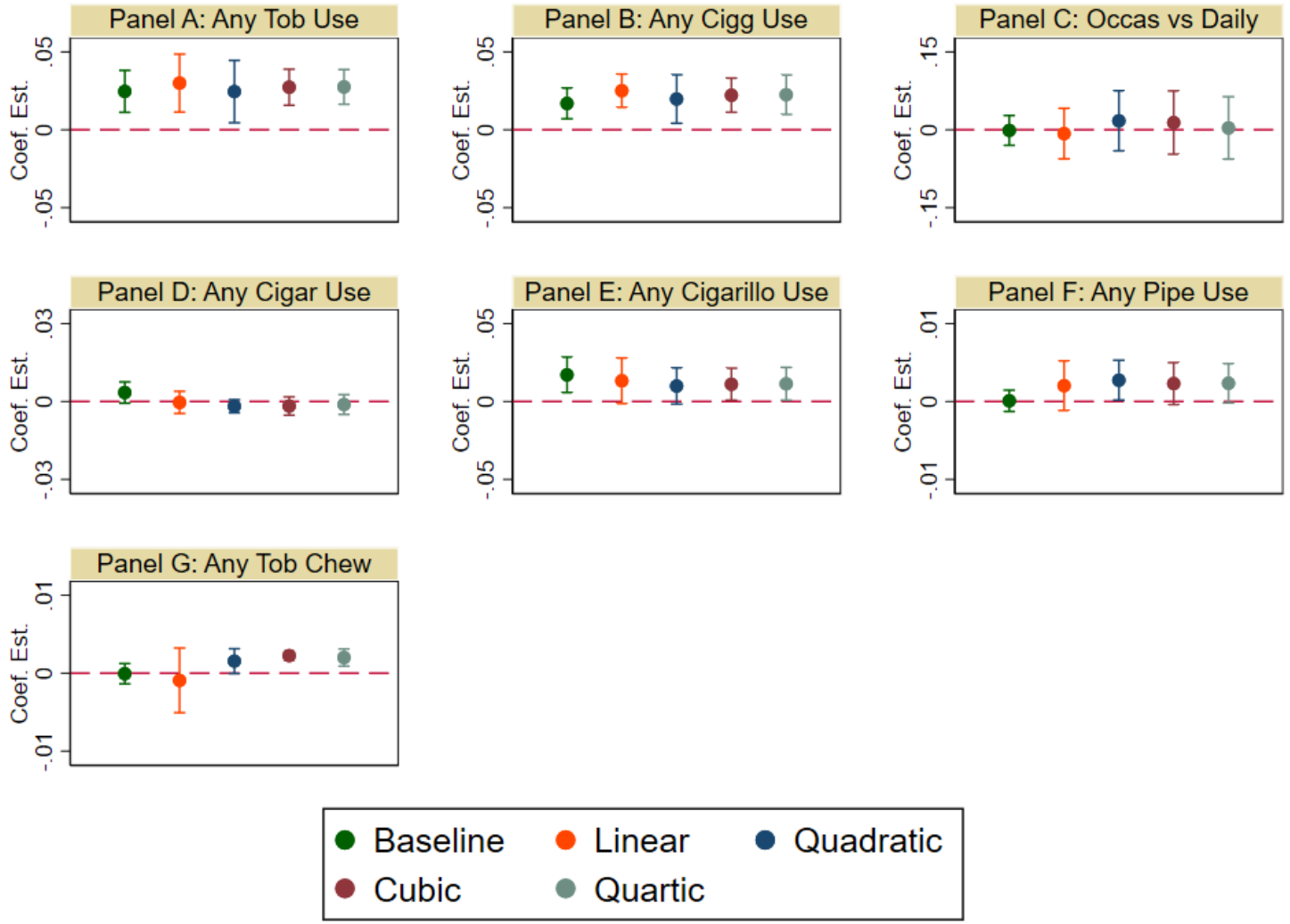


Figure 2.3. The coefficient plot for the inclusion of province-specific linear and nonlinear trends.

Note: Each panel reports the SCA coverage estimate from equation (3) for a different dependent variable. Each color represents a different specification: base is a specification without any province-specific trends; linear is a specification includes province-specific linear trends; quadratic is a specification includes province-specific quadratic trends; cubic is a specification includes province-specific cubic trends; and quartic is a specification includes province-specific quartic trends. Control variables include age, age squared, gender, marital status, educational attainment, household size, languages, other provincial-level variables, and all the fixed effects. Standard errors are clustered at the province levels. Confidence intervals are adjusted using t-distribution with 5 degrees of freedom. The dots represent point estimates, and the caps represent 95% confidence intervals.

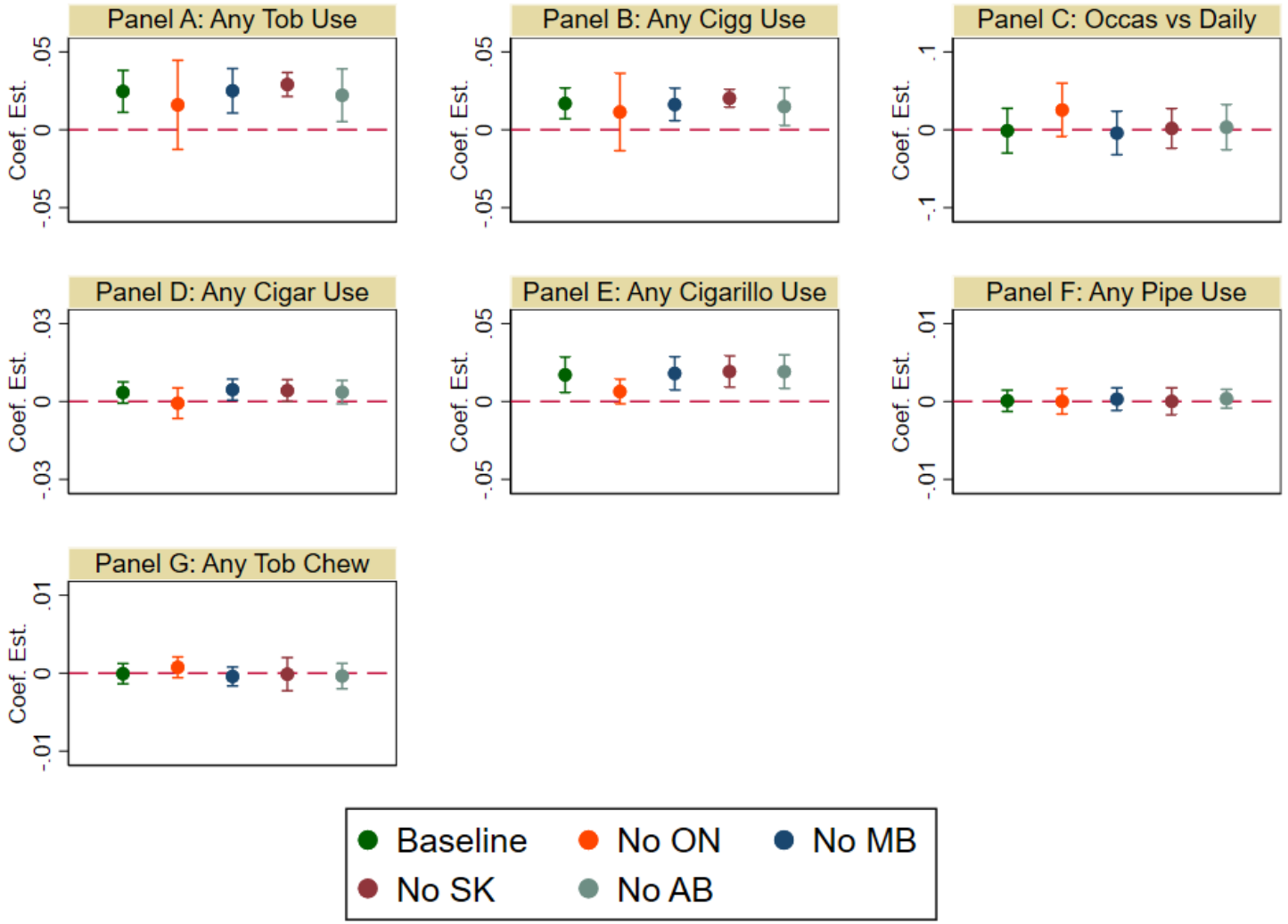


Figure 2.4.The coefficient plot for the “Leave-Out” analysis.

Note: Each panel reports the SCA coverage estimate for a different dependent variable. Each color represents a different specification: baseline is a specification includes all treated provinces; no ON is a specification excludes Ontario; no MB is a specification excludes Manitoba; no SK is a specification excludes Saskatchewan; and no AB is a specification excludes Alberta. Control variables include age, age squared, gender, marital status, educational attainment, household size, languages, other provincial-level variables, and all the fixed effects. Standard errors are clustered at the province levels. Confidence intervals are adjusted using t-distribution with 5 degrees of freedom. The dots represent point estimates, and the caps represent 95% confidence intervals.

Appendix

Appendix A
Appendix Table A.2.1. Exclude never adopted provinces

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Any tobacco use	Any cigarette use	Occasionally versus Daily	Any cigar use	Any cigarillo use	Any pipe use	Any tobacco chew
SCA Coverage	0.023*	0.018*	-0.026	0.000	0.010	0.001	0.000
	(0.007)	(0.005)	(0.027)	(0.002)	(0.005)	(0.001)	(0.001)
Socioeconomic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Drug Policies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Smoke Free Car Laws	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	60946	60946	11106	60717	60716	60727	60730

Note: Columns (1)−(7) report estimates for any tobacco use, any cigarette use, occasionally versus daily, any cigar use, any cigarillo use, any pipe use, and any tobacco chew, respectively. Socioeconomic controls are age, age squared, gender, marital status, educational attainment, household size, languages, and other provincial-level variables. Province fixed effects are a vector of province-binary variables. Time fixed effects are vectors of month-and year-binary variables. Standard errors are clustered at the province levels. Inferences are adjusted using t-distribution with 5 degrees of freedom. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix Table A.2.2. Exclude always adopted provinces

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Any tobacco use	Any cigarette use	Occasionally versus Daily	Any cigar use	Any cigarillo use	Any pipe use	Any tobacco chew
SCA Coverage	0.026**	0.018**	0.005	0.003	0.017*	0.000	0.000
	(0.005)	(0.004)	(0.009)	(0.002)	(0.005)	(0.001)	(0.001)
Socioeconomic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Drug Policies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Smoke Free Car Laws	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	60995	60995	11419	60759	60766	60766	60766

Note: Columns (1)−(7) report estimates for any tobacco use, any cigarette use, occasionally versus daily, any cigar use, any cigarillo use, any pipe use, and any tobacco chew, respectively. Socioeconomic controls are age, age squared, gender, marital status, educational attainment, household size, languages, and other provincial-level variables. Province fixed effects are a vector of province-binary variables. Time fixed effects are vectors of month-and year-binary variables. Standard errors are clustered at the province levels. Inferences are adjusted using t-distribution with 5 degrees of freedom. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix Table A.2.3. Alternative Inference

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Any tobacco use	Any cigarette use	Occasionally versus Daily	Any cigar use	Any cigarillo use	Any pipe use	Any tobacco chew
Panel A: Wild bootstrapping Two-Way Clustering							
SCA Coverage	0.025*	0.017*	-0.001	0.003	0.017*	0.000	-0.000
	[0.072]	[0.069]	(0.025)	[0.150]	[0.089]	[0.898]	(0.001)
Panel B: Two-Way Clustering							
SCA Coverage	0.025**	0.017**	-0.001	0.003*	0.017**	0.000	-0.000
	(0.009)	(0.004)	(0.011)	(0.002)	(0.004)	(0.001)	(0.001)
Socioeconomic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Drug Policies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Smoke Free Car Laws	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	81173	81173	15073	80872	80876	80880	80882

Note: Columns (1)−(7) report estimates for any tobacco use, any cigarette use, occasionally versus daily, any cigar use, any cigarillo use, any pipe use, and any tobacco chew, respectively. Socioeconomic controls are age, age squared, gender, marital status, educational attainment, household size, languages, and other provincial-level variables. Province fixed effects are a vector of province-binary variables. Time fixed effects are vectors of month-and year-binary variables. Standard errors are clustered at the province and month levels. For panel A, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. For panel B, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix Table A.2.4. Non-normalized weights

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Any tobacco use	Any cigarette use	Occasionally versus Daily	Any cigar use	Any cigarillo use	Any pipe use	Any tobacco chew
SCA Coverage	0.025***	0.017**	-0.001	0.003	0.017*	0.000	-0.000
	(0.005)	(0.004)	(0.011)	(0.002)	(0.004)	(0.001)	(0.001)
Socioeconomic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Drug Policies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Smoke Free Car Laws	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	81173	81173	15073	80872	80876	80880	80882

Note: Columns (1)−(7) report estimates for any tobacco use, any cigarette use, occasionally versus daily, any cigar use, any cigarillo use, any pipe use, and any tobacco chew, respectively. Socioeconomic controls are age, age squared, gender, marital status, educational attainment, household size, languages, and other provincial-level variables. Province fixed effects are a vector of province-binary variables. Time fixed effects are vectors of month-and year-binary variables. Standard errors are clustered at the province levels. Inferences are adjusted using t-distribution with 5 degrees of freedom. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix Table A. 2.5. Probit

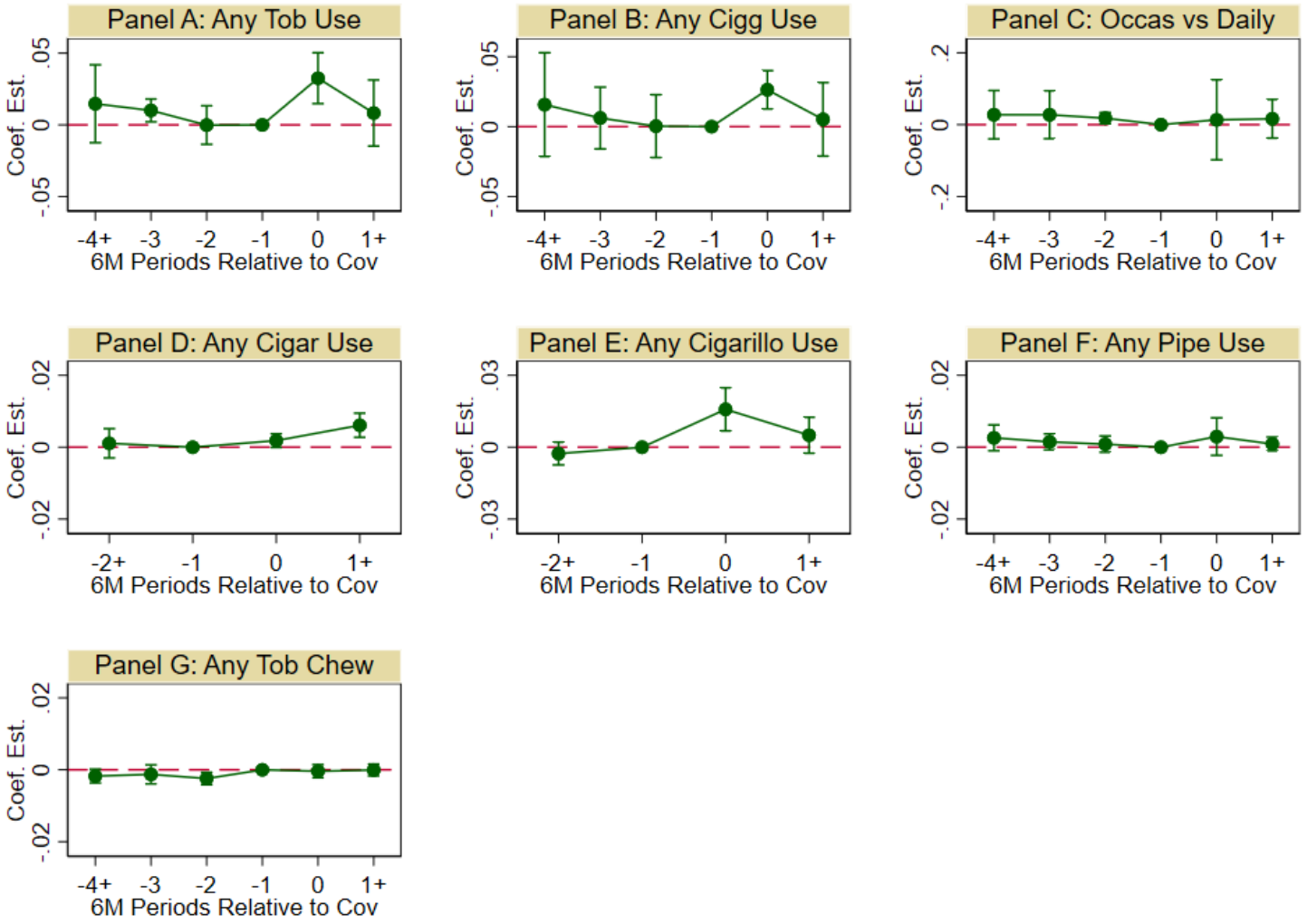
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Any tobacco use	Any cigarette use	Occasionally versus Daily	Any cigar use	Any cigarillo use	Any pipe use	Any tobacco chew
SCA Coverage	0.023**	0.015*	0.005	0.003	0.012*	-0.000	-0.000
	(0.005)	(0.004)	(0.015)	(0.001)	(0.004)	(0.000)	(0.000)
Socioeconomic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Drug Policies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Smoke Free Car Laws	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	81173	81173	15073	80872	80876	80880	80882

Note: Columns (1)−(7) report estimates for any tobacco use, any cigarette use, occasionally versus daily, any cigar use, any cigarillo use, any pipe use, and any tobacco chew, respectively. Socioeconomic controls are age, age squared, gender, marital status, educational attainment, household size, languages, and other provincial-level variables. Province fixed effects are a vector of province-binary variables. Time fixed effects are vectors of month-and year-binary variables. Standard errors are clustered at the province levels. Inferences are adjusted using t-distribution with 5 degrees of freedom. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix Table A.2.6. The effect of SCA coverage on tobacco use outcomes with pre-2008 CTUMS

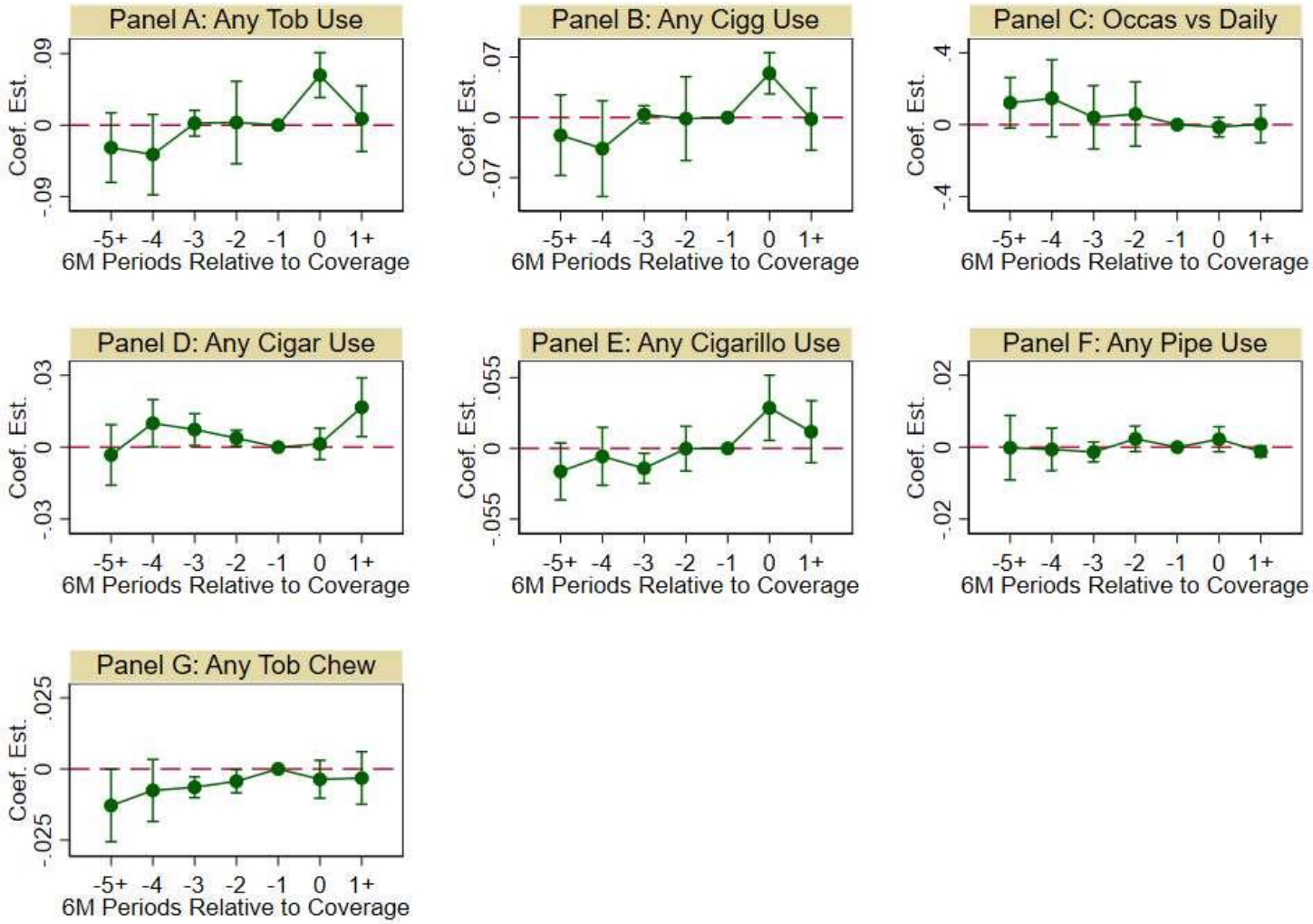
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Any tobacco use	Any cigarette use	Occasionally versus Daily	Any cigar use	Any cigarillo use	Any pipe use	Any tobacco chew
SCA Coverage	0.023**	0.017**	0.018	0.001	0.020*	0.002	-0.001*
	(0.005)	(0.004)	(0.032)	(0.002)	(0.005)	(0.002)	(0.000)
Socioeconomic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Drug Policies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Smoke-Free Car Laws	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	133085	133085	26103	97750	97753	132586	132583

Note: Columns (1)−(7) report estimates for any tobacco use, any cigarette use, occasionally versus daily use, any cigar use, any cigarillo use, any pipe use, and any tobacco chew, respectively. Columns (1), (2), (5), and (6) use 2005–2012 CTUMS, while columns (3) and (4) use 2007–2012 CTUMS. Socioeconomic controls are age, age squared, gender, marital status, educational attainment, household size, languages, and other provincial-level variables. Province fixed effects are a vector of province-binary variables. Time fixed effects are vectors of month-and year-binary variables. Standard errors are clustered at the province levels. Inferences are adjusted using t-distribution with 5 degrees of freedom. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$



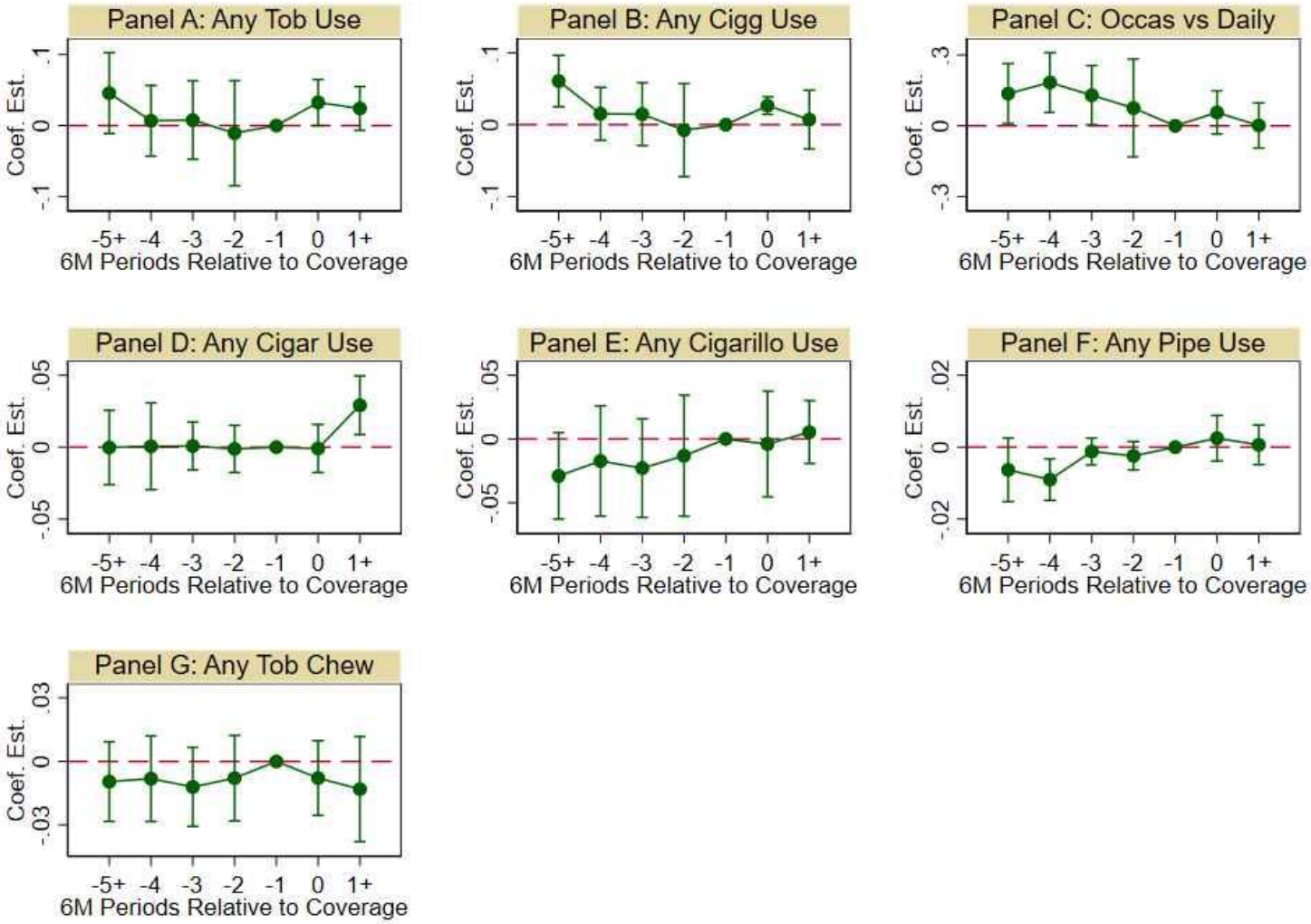
Appendix Figure A.2.1. Event study coefficient plot using pre-2008 CTUMS.

Note: Each panel reports the estimates of SCA coverage and reports a different dependent variable. The omitted category is six months prior to SCA coverage. Panels A, B, C, F, and G use 2005–2012 CTUMS, while panels D and E use 2007–2012 CTUMS. The control variables include age, age squared, gender, marital status, educational attainment, household size, languages, other provincial-level variables, and all the fixed effects. Standard errors are clustered at the province levels. Confidence intervals are adjusted using t-distribution with 5 degrees of freedom. The dots represent point estimates, and the caps represent 95% confidence intervals.



Appendix Figure A.2.2. Event study coefficient plot using six-month periods for ≤ 40 years-old.

Note: Each panel reports the estimates of SCA coverage similar to equation (2) but with six-month periods, instead of one year periods. Each panel reports the estimate for a different dependent variable. The omitted category is six months prior to SCA coverage. Control variables include age, age squared, gender, marital status, educational attainment, household size, languages, other provincial-level variables, and all the fixed effects. Standard errors are clustered at the province levels. Confidence intervals are adjusted using t-distribution with 5 degrees of freedom. The dots represent point estimates, and the caps represent 95% confidence intervals.


Panel A: Any Tob Use
Coef. Est.
-.1
0
.1
-5+ -4 -3 -2 -1 0 1+
6M Periods Relative to Coverage
Panel B: Any Cigg Use
Coef. Est.
-.1
0
.1
-5+ -4 -3 -2 -1 0 1+
6M Periods Relative to Coverage
Panel C: Occas vs Daily
Coef. Est.
-.3
0
.3
-5+ -4 -3 -2 -1 0 1+
6M Periods Relative to Coverage
Panel D: Any Cigar Use
Coef. Est.
-.05
0
.05
-5+ -4 -3 -2 -1 0 1+
6M Periods Relative to Coverage
Panel E: Any Cigarillo Use
Coef. Est.
-.05
0
.05
-5+ -4 -3 -2 -1 0 1+
6M Periods Relative to Coverage
Panel F: Any Pipe Use
Coef. Est.
-.02
0
.02
-5+ -4 -3 -2 -1 0 1+
6M Periods Relative to Coverage
Panel G: Any Tob Chew
Coef. Est.
-.03
0
.03
-5+ -4 -3 -2 -1 0 1+
6M Periods Relative to Coverage

Appendix Figure A.2.3. Event study coefficient plot using six-month periods for > 40 years-old.

Note: Each panel reports the estimates of SCA coverage similar to equation (2) but with six-month periods, instead of one year periods. Each panel reports the estimate for a different dependent variable. The omitted category is six months prior to SCA coverage. Control variables include age, age squared, gender, marital status, educational attainment, household size, languages, other provincial-level variables, and all the fixed effects. Standard errors are clustered at the province levels. Confidence intervals are adjusted using t-distribution with 5 degrees of freedom. The dots represent point estimates, and the caps represent 95% confidence intervals.

Appendix B
Appendix Table B.2.7. Event study

	(1) Any tobacco use	(2) Any cigarette use	(3) Occasionally versus Daily	(4) Any cigar use	(5) Any cigarillo use	(6) Any pipe use	(7) Any tobacco chew
SCA Coverage $_{t-3}$	0.008	0.004	-0.013	-0.002	-0.010	0.003	-0.002
	(0.007)	(0.004)	(0.023)	(0.001)	(0.004)	(0.003)	(0.002)
SCA Coverage $_{t-2}$	0.006	0.002	0.014	0.004***	-0.005	0.001	-0.000
	(0.003)	(0.003)	(0.049)	(0.001)	(0.002)	(0.002)	(0.001)
SCA Coverage $_{t-1}$	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline
SCA Coverage	0.029**	0.018*	-0.017	0.005	0.016*	0.000	-0.000
	(0.008)	(0.005)	(0.012)	(0.002)	(0.006)	(0.001)	(0.001)
Socioeconomic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Drug Policy	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Smoke-Free Car Laws	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	81173	81173	15073	80872	80876	80880	80882

Note: Columns (1)−(7) report estimates for any tobacco use, any cigarette use, occasionally versus daily, any cigar use, any cigarillo use, any pipe use, and any tobacco chew, respectively. Socioeconomic controls are age, age squared, gender, marital status, educational attainment, household size, languages, and other provincial-level variables. Province fixed effects are a vector of province-binary variables. Time fixed

effects are vectors of month-and year-binary variables. Standard errors are clustered at the province levels. Inferences are adjusted using t-distribution with 5 degrees of freedom. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix Table B.2.8 Event study using six-month periods

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Any tobacco use	Any cigarette use	Occasionally versus Daily	Any cigar use	Any cigarillo use	Any pipe use	Any tobacco chew
SCA Coverage $_{t-5}$	-0.004	-0.005	0.026	0.001	-0.011	0.005	-0.005*
	(0.009)	(0.008)	(0.036)	(0.002)	(0.005)	(0.003)	(0.001)
SCA Coverage $_{t-4}$	-0.009	-0.008	0.078	0.008**	-0.006	0.003	-0.004*
	(0.013)	(0.011)	(0.059)	(0.001)	(0.004)	(0.003)	(0.001)
SCA Coverage $_{t-3}$	0.006	-0.001	-0.003	0.004***	-0.004	0.001	-0.002
	(0.005)	(0.009)	(0.060)	(0.000)	(0.002)	(0.001)	(0.001)
SCA Coverage $_{t-2}$	-0.000	-0.003	-0.007	0.001	0.002	0.001	-0.003**
	(0.008)	(0.010)	(0.014)	(0.001)	(0.002)	(0.001)	(0.001)
SCA Coverage $_{t-1}$	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline
SCA Coverage $_{t}$	0.040**	0.027*	-0.027*	0.004	0.020*	0.002	-0.001
	(0.009)	(0.008)	(0.010)	(0.002)	(0.006)	(0.001)	(0.001)

SCA Coverage $_{t+1}$	0.009	-0.003	0.003	0.009**	0.012	-0.000	-0.002
	(0.014)	(0.012)	(0.019)	(0.002)	(0.005)	(0.001)	(0.002)
Socioeconomic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Drug Policy	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Smoke-Free Car Laws	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	81173	81173	15073	80872	80876	80880	80882

Note: Columns (1)−(7) report estimates for any tobacco use, any cigarette use, occasionally versus daily, any cigar use, any cigarillo use, any pipe use, and any tobacco chew, respectively. Socioeconomic controls are age, age squared, gender, marital status, educational attainment, household size, languages, and other provincial-level variables. Province fixed effects are a vector of province-binary variables. Time fixed effects are vectors of month-and year-binary variables. Standard errors are clustered at the province levels. Inferences are adjusted using t-distribution with 5 degrees of freedom. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix Table B.2.9. Inclusion of province-specific linear and nonlinear trends

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Any tobacco use	Any cigarette use	Occasionally versus Daily	Any cigar use	Any cigarillo use	Any pipe use	Any tobacco chew
Panel A: Control for province-specific linear trends							
SCA Coverage	0.030**	0.025**	-0.007	-0.000	0.013	0.002	-0.001
	(0.007)	(0.004)	(0.019)	(0.002)	(0.006)	(0.001)	(0.002)
Panel B: Control for province-specific quadratic trends							
SCA Coverage	0.025*	0.020*	0.017	-0.002	0.010	0.003*	0.002
	(0.008)	(0.006)	(0.022)	(0.001)	(0.005)	(0.001)	(0.001)
Panel C: Control for province-specific cubic trends							
SCA Coverage	0.027***	0.022**	0.014	-0.002	0.011*	0.002	0.002***
	(0.005)	(0.004)	(0.024)	(0.001)	(0.004)	(0.001)	(0.000)
Panel D: Control for province-specific quartic trends							
SCA Coverage	0.028**	0.023**	0.004	-0.001	0.011*	0.002*	0.002**
	(0.004)	(0.005)	(0.023)	(0.001)	(0.004)	(0.001)	(0.000)
Socioeconomic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Drug Policies	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Smoke-Free Car Laws	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	81173	81173	15073	80872	80876	80880	80882

Note: Columns (1)−(7) report estimates for any tobacco use, any cigarette use, occasionally versus daily, any cigar use, any cigarillo use, any pipe use, and any tobacco chew, respectively. Socioeconomic controls are age, age squared, gender, marital status, educational attainment, household size, languages, and other provincial-level variables. Province fixed effects are a vector of province-binary variables. Time fixed effects are vectors of month-and year-binary variables. Standard errors are clustered at the province levels. Inferences are adjusted using t-distribution with 5 degrees of freedom. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix Table B.2.10 Inclusion of region-specific linear and nonlinear trends

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Any tobacco use	Any cigarette use	Occasionally versus Daily	Any cigar use	Any cigarillo use	Any pipe use	Any tobacco chew
Panel A: Control for region-specific linear trends							
SCA Coverage	0.029**	0.022***	0.003	0.003	0.020**	0.000	0.000
	(0.004)	(0.003)	(0.011)	(0.002)	(0.004)	(0.001)	(0.001)
Panel B: Control for region-specific quadratic trends							
SCA Coverage	0.029**	0.021**	-0.000	0.003	0.020**	0.000	0.000
	(0.004)	(0.004)	(0.011)	(0.002)	(0.004)	(0.000)	(0.001)
Panel C: Control for region-specific cubic trends							
SCA Coverage	0.025**	0.017**	-0.001	0.003	0.017*	0.000	-0.000
	(0.005)	(0.004)	(0.011)	(0.002)	(0.004)	(0.001)	(0.001)
Panel D: Control for region-specific quartic trends							
SCA Coverage	0.027**	0.021***	-0.002	0.003	0.019**	0.000	0.000
	(0.004)	(0.003)	(0.011)	(0.002)	(0.004)	(0.001)	(0.001)
Socioeconomic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Drug Policies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Smoke-Free Car Laws	Yes	Yes	Yes	Yes	Yes	Yes	Yes

N	81173	81173	15073	80872	80876	80880	80882

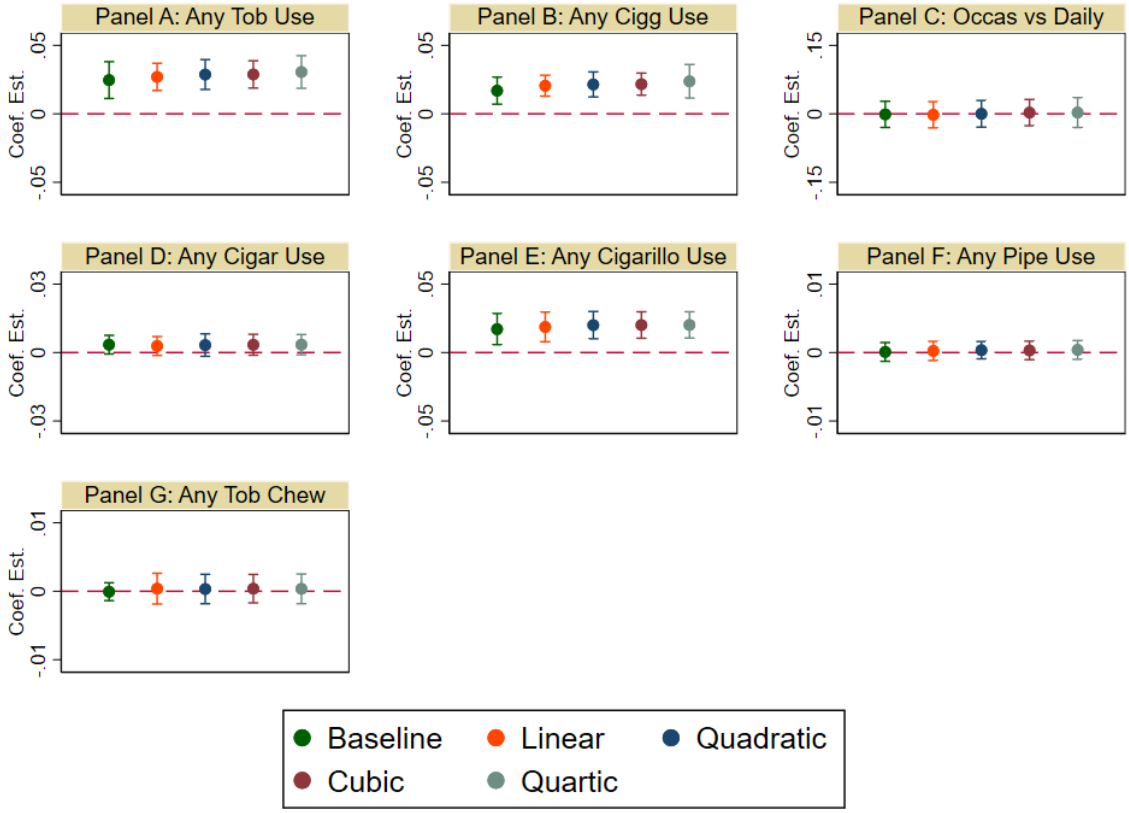
Note: Columns (1)−(7) report estimates for any tobacco use, any cigarette use, occasionally versus daily, any cigar use, any cigarillo use, any pipe use, and any tobacco chew, respectively. Socioeconomic controls are age, age squared, gender, marital status, educational attainment, household size, languages, and other provincial-level variables. Province fixed effects are a vector of province-binary variables. Time fixed effects are vectors of month-and year-binary variables. Standard errors are clustered at the province levels. Inferences are adjusted using t-distribution with 5 degrees of freedom. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix Table B.2.11. Leave-out analysis

	(1)	(2)	(3)	(4)	(5)
	Baseline	Exclude Ontario	Exclude Manitoba	Exclude Saskatchewan	Exclude Alberta
Panel A: Any Tobacco Use					
SCA Coverage	0.025**	0.016	0.025**	0.029***	0.022*
	(0.005)	(0.011)	(0.006)	(0.003)	(0.007)
N	81173	71545	70195	70729	71455
Panel B: Any Cigarette Use					
SCA Coverage	0.017**	0.011	0.016*	0.020***	0.015*
	(0.004)	(0.010)	(0.004)	(0.002)	(0.005)
N	81173	71545	70195	70729	71455
Panel B: Occasionally versus Daily					
SCA Coverage	-0.001	0.025	-0.004	0.002	0.003
	(0.011)	(0.013)	(0.011)	(0.010)	(0.011)
N	15073	13588	13017	12941	13294
Panel C: Any Cigar Use					
SCA Coverage	0.003	-0.001	0.005*	0.004	0.004
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
N	80872	71275	69935	70487	71187
Panel D: Any Cigarillo Use					

SCA Coverage	0.017*	0.006	0.018**	0.019**	0.019**
	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)
N	80876	71277	69938	70489	71194
Panel E: Any Pipe Use					
SCA Coverage	0.000	0.000	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)
N	80880	71282	69941	70492	71192
Panel F: Any Tobacco Chew Use					
SCA Coverage	-0.000	0.001	-0.000	-0.000	-0.000
	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)
N	80882	71287	69942	70493	71192

Note: Each column reports a different specification (baseline without any exclusion, exclude Ontario, exclude Manitoba, and so on). Panels A–F report estimates for any cigarette use, occasionally versus daily, any cigar use, any cigarillo use, any pipe use, and any tobacco chew, respectively. Each cell is a separate regression that controls for socioeconomic controls, province fixed effects, and time fixed effects. Standard errors are clustered at the province levels. Inferences are adjusted using t-distribution with 5 degrees of freedom. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$



Appendix Figure B.2.4. The coefficient plot for the inclusion of region-specific linear and nonlinear trends.

Note: Each panel reports the SCA coverage estimate for a different dependent variable. Each color represents a different specification: base is a specification without any region-specific trends; linear is a specification includes region-specific linear trends; quadratic is a specification includes region-specific quadratic trends; cubic is a specification includes region-specific cubic trends; and quartic is a specification includes region-specific quartic trends. The control variables include age, age squared, gender, marital status, educational attainment, household size, languages, other provincial-level variables, and all the fixed effects. Standard errors are clustered at the province levels. Confidence intervals are adjusted using t-distribution with 5 degrees of freedom. The dots represent point estimates, and the caps represent 95% confidence intervals.

Chapter 3. Does College Education Make Us Act Healthier? Evidence from a Japanese Superstition

1. Introduction

In 2015, across 25 Organization for Economic Co-operation and Development countries, a college-educated person aged 30 years could be expected to live for another 53.4 years, whereas a non-college-educated person of the same age could only be expected to live for another 47.8 years, translating to a 5.6-year difference (Organisation for Economic Co-operation and Development, 2017). This difference in life expectancy is consistent with research from past decades, in which higher education has been found to be causally associated with lower mortality (Lleras-Muney, 2005; Clark & Royer, 2013; Fischer et al., 2013; Buckles et al., 2016). Cutler and Lleras-Muney (2010) attributed these differences in health to the differences in health behavior between less educated and educated people. However, a substantive debate is ongoing regarding the extent to which higher education can causally affect health behavior. Specifically, the literature offers little consensus on this relationship. For instance, some studies have found that higher education reduces risky behaviors such as smoking (Kenkel et al., 2006; de Walque, 2007; Grimard & Parent, 2007; Kemptner et al., 2011; Jürges et al., 2011), whereas others have found that higher education does not affect health behavior (Reinhold & Jürges, 2010; Li & Powdthavee, 2015; Silles, 2015; Dursun et al., 2018).

To further our understanding, we investigated the causal effect of college education on health behavior in Japan. However, education is associated with unobserved cofounders that can correlate with health behavior, and thus, causality is difficult to establish. To address the endogeneity of education, we pursued an instrumental variable (IV) approach similar to that of previous studies. Relevant studies have leveraged natural experiments, such as compulsory schooling laws, expansionary schooling policy, and Vietnam War draft

avoidance, as instruments for education. Compulsory schooling laws exploit the geographic- and time-variation of the minimum age at which individuals must stay in school, which increases an individual's exposure to educational systems; thus, an individual who has experienced a change in compulsory schooling laws would have a higher educational attainment than one who has not (Amin et al., 2013; Dursun et al., 2018; Kemptner et al., 2011; Kenkel et al., 2006; Li & Powdthavee, 2015; Silles, 2015; Xie & Mo, 2014). Another related instrument is expansionary schooling policy (Jürges et al., 2011; Park & Kang, 2008), which mandates an increase in the number of schools operating across geographic locations and time. An increase in the number of schools would allow more individuals to attain higher education; thus, it would effectively increase access to higher education through reduced competition. Regarding draft avoidance, relevant studies have employed Vietnam War draft avoidance as an instrument for college education (Buckles et al., 2013; de Walque, 2007; Grimard & Parent, 2007). Specifically, individuals were able to defer entry into the army to fight in the Vietnam War if they entered college or institutions of higher education (Card & Lemieux, 2001). Therefore, men under the age of 24 years had a strong incentive to attend colleges to avoid the draft, which led to an increase in college educational attainment among American men born in the 1940s and 1950s.

We departed from existing literature by examining a unique natural experiment in Japan that has not been studied before, namely Japanese people's superstitious belief in the zodiac signs. They believe that children born under a specific combination of zodiac signs will have undesirable personality traits that are difficult to deal with. Notably, a specific combination of zodiac signs known as "Firehorse" (FH) occurs every 60 years. Japanese people believe that women born under the FH sign will have a domineering personality that can affect their relationships throughout their lives. This induces parents to avoid having children in the year of the FH, which resulted in a notably lower birthrate in 1966, the last FH

year. This lower birthrate significantly reduced competition for college enrollment and improved the learning environment during early education due to smaller classroom sizes, leading to a higher enrollment rate for college among individuals born in 1966, ceteris paribus. However, such a superstition may correlate with unobserved health-related confounders, which in turn may correlate with health behavior. To avoid this selection issue, we took advantage of a mismatch between the Japanese school year – which starts in April each year and ends the following March – and the calendar year. Specifically, we focused on a cohort of individuals born from January to March 1967, who enrolled in college together with those born in 1966 but were not affected by the FH superstition of 1966. Doing so minimized the effect of unobserved confounders associated with the superstition on education and health behavior.

Exploring the relationship between college education and health behavior, we focused on behaviors that are proven to have significant impacts on the health of the population. Specifically, we examined the effect of college education on smoking, drinking, sleeping, and cancer screening behavior. Smoking kills more than 7 million people annually worldwide (World Health Organization, 2017). Furthermore, poor sleep significantly increases traffic-related mortality (Gottlieb et al., 2018), depression (Tsuno et al., 2005), and cardiovascular diseases (Kronholm et al., 2011; Tobaldini et al., 2017). Moreover, cancer is the second-leading cause of death worldwide (World Health Organization, 2021). The early detection of cancer through screening is beneficial because it significantly improves the survival of cancer patients (Hugosson et al., 2010; Kalager et al., 2010; Olsen et al., 2005).

As a preview, we found that 1 additional year of college education reduces the probabilities of having ever smoked and of being a current smoker by 14.7 and 11.3 percentage points, respectively. Regarding alcohol use, we found that 1 additional year of college education reduces the probabilities of having ever been a drinker and of being a

current drinker by 19.1 and 18.2 percentage points, respectively. Moreover, we found that more years of college education have no effect on the probability of having good sleep. Regarding cancer screening behavior, college education was found to have no effects on the probabilities of getting stomach, lung, and ovarian cancer screenings; however, it was found to increase the probability of getting a breast cancer screening by 35.8 percentage points and a colon cancer screening by 23.3 percentage points. We also found that the causal relationship is mainly driven by women.

Our contribution to the literature is twofold. First, we depart from the existing literature by using a mismatch between the school year and a superstition as an instrument for our IV approach, whereas previous studies have used compulsory schooling laws or Vietnam War drafting as an instrument for their IV approach. This is the first study to leverage this strategy.[1] Second, this is the first study to examine the effect of education on sleeping and cancer screening behavior. We are not aware of any study having examined these behaviors previously. Finally, we also show that college education predominately increases the probability of being a fulltime worker and a civil servant by 17.0 and 12.0 percentage points, suggesting the stable employment is an important meditator between college education and health behavior.

The remainder of this paper is organized as follows. Section 2 introduces the background; Section 3 describes the data source; Section 4 presents the empirical strategies; Section 5 presents the estimation results; and finally, Section 6 discusses the policy implications and limitations.

[1] The construction of the instrument is similar in spirit to a strategy pioneered by Lau (2019). However, his study differed from ours in that the superstition shock being studied was different and the main focus of the study.

2. Background

2.1 The Firehorse Superstition

In Japan, the roots of many superstitions can be traced back to Chinese superstitions. A particular Japanese superstition is the belief in Chinese zodiac signs. Japanese people assign a specific zodiac to a year based on the five Chinese elements (i.e., Water, Metal, Earth, Fire, and Wood) and 12 Chinese zodiac signs (i.e., Mouse, Ox, Tiger, Rabbit, Dragon, Snake, Horse, Sheep, Monkey, Rooster, Dog, and Pig). By combining the two, a total of 60 zodiac signs can be assigned across years. A specific combination, namely the Fire element and the Horse zodiac sign, occurs every 60 years; in Japanese, this is called *Hinoeuma*, or FH. Women born in the FH year are believed to be particularly stubborn, headstrong, and independent (Azumi, 1968; Hashimoto, 1974). Consequently, people believe the FH women will bring misfortune and shorten their husbands' lives (Azumi, 1968; Hashimoto, 1974). Due to these beliefs, parents tend to avoid having children in the year of the FH.

[Figure 3.1]

The last FH year was 1966. Figure 3.1 presents the total fertility rates from 1947 to 1980 based on Japan's Vital Statistics. It demonstrates a significant decline in the number of individuals born from January to December 1966.[2,3] Moreover, the surrounding years exhibit stable, flat patterns for the number of individuals being born. Figure 3.1 suggests significant childbearing avoidance in the last FH year.

2.2. A Mismatched Cohort, Education Shock, and the Control Group

[Figure 3.2]

[2] Selection based on birth using ultrasound is unlikely. Fetal ultrasound was first developed in the late 1950s in Glasgow. However, the potential of fetal ultrasound was not realized until the development of real-time imaging in the 1970s (Campbell, 2013). Therefore, the widespread use of ultrasound would not have begun until the 1970s.
[3] Additionally, we plotted the proportion of women by birth year in Appendix Figure 3.1. We did not observe any abnormal peak in proportions during the period.

The FH year represents a natural experiment in which a cohort experienced a significant decline in population. A significant decline in the number of peers would effectively reduce the competition for colleges and improve the learning of this cohort due to a smaller classroom size in elementary and secondary education; thus, college enrollment would increase.[4] Therefore, college enrollment rates should have increased for those born in 1966, which corresponds to the 1985 school year. To demonstrate that this was the case, we plotted college enrollment rates from 1965 to 1997 in Figure 3.2 using the Basic Education Survey. As expected, we observed a sharp rise in college enrollment rates in 1985. This illustrates that the FH year may serve as an instrument for college education.

Unfortunately, there are two caveats associated with using 1966 as an instrument for college education. First, a person's education may affect whether he (or she) would have had a child in 1966. Mocan and Pogorelova (2017) showed that higher education significantly reduces superstitious beliefs. Given this evidence, it is not difficult to imagine that less-educated parents may be more inclined to believe in the FH superstition. This may indirectly affect parents' decision to invest in the education of individuals born in 1966. Second, individuals born in 1966 may experience discrimination in marriage markets, which might affect their health behavior. Akabayashi (2008) reported that women born into the FH year experienced a significant decline in marriage compared with those born in non-FH years. This suggests that FH individuals may experience significant marriage-related discrimination,

[4] According to the Ministry of Education, Culture, Sports, Science and Technology, the Japanese government regulated class sizes as follows: 50 students from 1959 to 1963, 45 students from 1964 to 1968, 40 students from 1969 to the present (https://www.mext.go.jp/a_menu/shotou/hensei/005/1295041.htm). Now, let us think about the case of 40 students. Normally, if a school has 120 students, the school would divide the students into three classes with 40 students in each. Suppose that only 100 students newly entered the school in 1967. If that was the case, the school would have divided them into three classes, such as 33 in one, 33 in another, and 32 in the other, to make all the class sizes as equal as possible, instead of sticking to 40 students per class. Thus, it is likely that FH individuals would have experienced a decline in classroom size in their elementary and secondary educations.

which can indirectly affect their education decisions and health behavior in unobservable ways.

[Figure 3.3]

To alleviate these concerns, we leveraged the institutional setting of the Japanese education system. In Japan, each school year starts in April of each calendar year. This results in a mismatch between the superstition and school year; specifically, the mismatch comes from the time lag between the Japanese school year and the calendar year. Figure 3.3 presents the identification strategy of our instrument. It illustrates that the FH year started in January of the 1966 calendar year and ended in December of the same year. Concurrently, the Japanese school year starts on April and ends on March of the following year. Specifically, the 1966 school year started on April 1, 1966 and ended on March 31, 1967. We denoted this period as the "FH school year." The mismatch appears for individuals who were born between January and March of 1967. This cohort was born after the FH year but was sorted into the FH school year. We named this cohort the "1967-mismatched cohort." The 1967-mismatched cohort was sorted into the FH school year and may have experienced a significant increase in enrollment into colleges due to the lower competition and smaller classroom sizes in the FH calendar year. The educational attainment of this cohort would have been positively affected by the superstition. More critically, *only* the educational attainment of the 1967-mismatched cohort was affected by the superstition, which makes the cohort a plausible IV.

To isolate the education effect of superstition shock, we required a control cohort that was similar to the mismatched cohort but was not affected by the superstition. The most obvious choice was those born from April to December of the same year (1967), who were born in the same year as the 1967-mismatched cohort but did not experience the same

educational shock (see Figure 3.3). We denoted this group as the "1967-control cohort." However, the 1967-control cohort may differ from the 1967-mismatched cohort in unobserved characteristics. Previous literature has indicated that those born before the school entry date have a lower college educational attainment than those born after the school entry date, because they have lower cognitive abilities due to being enrolled in educational institutions at a much younger age (Crawford et al., 2011). To adjust for the unobserved differences between these cohorts, we extended the mismatched-control comparison to 1968. For those born in 1968, none of the mismatched (i.e., born from January to March) and control (i.e., born from April to December) cohorts experienced any educational shock. We named them the "non-1967-mismatched cohort" and the "non-1967-control cohort." While we used 1968 as the control year in the figure as an example, we did not have to limit the control year to 1968. For non-1967 years, we used those born between 1947 to 1980 as our baseline birth year range to minimize the effects of parental selection and society discrimination on our estimates. Note that we *excluded* those born in 1966 from the baseline.

3. Data and Measurements

3.1. Data Description

We used the Comprehensive Survey of Living Conditions (CSLC), a triennial, nationally representative, repeated, cross-sectional survey that has been conducted by Japan's Ministry of Health, Labour, and Welfare since 1986. The CSLC is the largest individual collection of information on demographic, socioeconomic, and health status in Japan. Approximately, 60,000–80,000 respondents are randomly drawn from 30,000 households across Japan. The CSLC contains five components: household, health, caregiving, income, and saving. We employed the 2013 and 2016 household and health components of the CSLC

to examine the effect of college education on health behavior, since only the data from this period collected the education and dependent variables that we were using.

3.2. Health Behavior

We generated the following five binary variables for smoking, drinking, and sleeping behavior: “ever smoker,” “current smoker,” “ever drinker,” “current drinker,” and “good sleep.” To determine smoking behavior, the survey asked the question “Do you currently smoke?” and offered four responses: (a) smoke daily, (b) smoke occasionally, (c) used to smoke but quit for more than 1 month, and (d) never smoked. “Ever smoker” took a value of 1 if the respondent selected (a), (b), or (c), and 0 otherwise. “Current smoker” took a value of 1 if the respondent selected (a) or (b) and 0 if he or she selected (d). Those who selected (c) were excluded from “current smoker.” Similarly, for drinking behavior, we generated two binary variables from the alcohol use-related question “Do you currently drink?” The response categories were the same as those for smoking behavior. “Ever drinker” took a value of 1 if the respondent selected (a), (b), or (c) and 0 otherwise. “Current drinker” took a value of 1 if the respondent selected (a) or (b) and 0 for (d). For sleeping behavior, we asked the question “How is the quality of your sleep normally?” with the following four choices: (a) good, (b) average, (c) not so good, and (d) poor. “Good sleep” took a value of 1 if the respondent selected (a) or (b) and 0 otherwise.

For cancer screening behavior, we generated five binary variables: “stomach cancer screening,” “lung cancer screening,” “ovarian cancer screening,” “breast cancer screening,” and “colon cancer screening.” To generate these variables, we used the question “Have you been screened for stomach/lung/ovarian/breast/colon cancer in the past 12 months?” The respondents could choose either (a) yes or (b) no. We defined each respective binary variable as 1 if the respondent selected (a) and 0 otherwise.

3.3. College Education

To construct college education, we used the question "What is your highest level of educational attainment?" with the following six choices: (a) elementary/junior high school, (b) senior high school, (c) technical college, (d) short-term college, (e) bachelor's, and (f) master's or above. We decategorized the six categories into a continuous variable to facilitate ease of interpretation and comparison with previous studies. Specifically, we generated a continuous variable, namely "years of college education," which equaled 0 if the respondent selected (a) or (b) as his or her highest educational attainment, up to 2 if the respondent selected (c) or (d), up to 4 if he or she selected (e), and to 9 if he or she selected (f).[5] Therefore, a respondent had 0 years of college education if he or she had graduated from high school, 2 years of college education if he or she had graduated from technical or short-term colleges, 4 years if he or she had graduated with a bachelor's degree, and 9 years if he or she had graduated with a master's degree or above.

3.4. Socioeconomic Variables

We controlled for socioeconomic variables of a respondent, including sex, household structure, marital status, whether children lived together, type of house owned, prefecture of residence,[6] and survey year. All variables were generated as binary variables, which equaled 1 if corresponding to respondents' status and 0 otherwise.

[5] We collapsed "Technical College" and "Short-term College" into one category because their length of study and curricula are extremely similar.
[6] Prefectures in Japan are analogous to states in the United States.

4. Empirical Strategies

4.1. Instrumental Variable Approach

To examine the effect of college education on health behavior, we implemented two-stage least squares (2SLS) regression analysis. We estimated the following regression as the first-stage equation:

$$College_i = \gamma_t + \gamma_m + \gamma_s + \gamma_p + \beta Mismatch_c \times 1967_t + \boldsymbol{X_i'\zeta} + \varepsilon_i, \qquad (1)$$

where $Mismatch_c$ is a binary variable that equaled 1 if a respondent was born between January and March, and 0 otherwise; 1967_t is also a binary variable that equals 1 if a respondent was born in 1967, and 0 if he or she was born in other non-1967 years, namely 1947 to 1965 and 1968 to 1980 (for baseline). Note that we *excluded* 1966 from our estimation due to parental selection and discrimination, which could have introduced bias if we were to include it. The instrument was essentially an interaction term between $Mismatch_c$ and 1967_t. Furthermore, β represents the competition and classroom size shocks of the FH superstition on college education. $College_i$ is the years of college education, a continuous variable. γ_t, γ_m, γ_s, and γ_p controlled for the birth year, birth month, prefecture, and survey-year fixed effects (FEs), respectively. ε_i is the error terms. $\boldsymbol{X_i'}$ is a set of binary socioeconomic variables including sex, household structure, marital status, children living together, and type of house owned.

For the second-stage equation, we estimated the following regression for our model:

$$HB_i = \lambda_t + \lambda_m + \lambda_s + \lambda_p + \alpha College_i + \boldsymbol{X_i'\delta} + \eta_i, \qquad (2)$$

where HB_i is the binary dependent variable, including ever smoker, current smoker, ever drinker, current drinker, good sleep, stomach cancer, lung cancer, breast cancer, ovarian cancer, and colon cancer screening for respondent i. γ_t, γ_m, γ_s, γ_p, and $\boldsymbol{X_i'}$ are the same as in

the first-stage regression. α is the main parameter of interest, which captures the effect of college education on health behavior. η_i is the error terms. We clustered the standard errors at birth-month and birth-year levels.

As mentioned previously, we limited the birth year range to 1947–1980 and *excluded* those born in 1966 from our baseline specification. We did this because exogenous shocks exist that could affect the relationship between college education and health behavior for respondents born in or before 1947 and after 1980. For instance, there were two exogenous shocks that could affect the respondents born before 1947. First, before 1947, Japan was devastated by World War 2. This event would have severely affected access to education and health resources for children for parents born before 1947, since food and many other supplies were still in short supply during and after the war. Second, in 1946, Japan also passed a compulsory schooling law that mandated junior high school for all individuals who attended elementary school. This would also have affected respondents' educational attainment if they were attaining school after 1946. Similarly, after 1980, Japan experienced an economic boom. This economic boom is likely to have increased parental access to education and health resources due to increased income, which would have affected the behavior of children born in this period. However, it is possible that our results were driven by the nonrandom nature of the birth year range. To this end, we tested the sensitivity of our results by increasing the birth year range to 1938–1990.

It should also be noted that there were several nation-wide educational reforms in 1990s in Japan. These nation-wide reforms could also increase the education attainment of population and affect our estimates. To alleviate the issue, we can, instead, restrict the birth year range to 1954–1973 to test the sensitivity of our estimates to these reforms. Relating to the education reforms are the changes in college admission quota. In particular, these policies expand the number of seats available to all students which can influence college attainment of

those entering college in 1990s. To check whether there is a change in quota, we check whether there is a change in quota in years before and year 1985. We do not observe any significant changes in admission quota during the period at which the FH attain the college. This implies that it is unlikely that the expansion of college education quota is driving our results.[7,8]

4.2. Threats to Identification

[Figure 3.4]

Given the novelty of our identification strategy, its validity warrants additional discussion. For an instrument to be valid, its exclusion restrictions must be satisfied. For our instrument, there were several threats to this assumption. First, the misidentification of the mismatched cohort as FH might have affected the relationship. Specifically, others may have identified the respondents who were born between January and March 1967 as FH women, since these respondents were sorted together with those from the FH year in a school setting. These respondents would have experienced the marriage/dating discriminatory effect in school, which would have affected both their education and health behavior, rendering the assumption invalid. For this channel to be plausible, a rigorous division such as school year must exist, and such a case only exists in school systems such as junior high and high schools. Based on the reports of the 14th Japanese National Fertility Survey in 2010, Japanese people often find their marriage partners through their workplace, acquaintances, and other non-school places.[9] Approximately 90–95% of individuals meet their spouses through non-school-related places. Given the high prevalence of finding spouses through non-school

[7] The information on college admission quota can be found on http://www.crepe.e.u-tokyo.ac.jp/material/uil1.html (in Japanese). The first excel in the download section shows the college admission quota by universities in each prefecture.
[8] Another way of dealing with these reforms is to restrict the birth year range from our baseline to assess whatever our estimates are sensitive to smaller range.
[9] See http://www.ipss.go.jp/site-ad/index_Japanese/shussho-index.html for the report (Japanese). For a translated version, see Appendix Figure 3.2.

places and methods, it would be difficult for men to specifically identify FH women because the rigorous division would have disappeared in these instances. An alternative – albeit more crude – way of checking whether FH women in 1967 were discriminated against is to plot dating experience at different stages (ages) of their life for different birth years. This would only be a crude check since four different birth years were aggregated into a single category in this survey. Figure 3.4 depicts the prevalence of women who were not dating at each age for each birth year. Those born between 1963 and 1967 were quite similar to those of other birth years, such as 1973–1977. This suggests that they did not suffer dating discrimination, as one would expect if one were to find a significantly higher level of respondents who were not dating. Finally, given that we controlled for marriage and other household-related characteristics, the effect of this channel would have been absorbed by these variables.

[Figure 3.5]

Second, one would be concerned that there is a selection due to maternal education. Specifically, less educated women would be more likely to hold superstitious beliefs (Torgler, 2007); therefore, such women would have been more likely to delay their pregnancies until 1967. If this was indeed the case, we should observe a significant decline in maternal educational level in 1967. To assess this, we leveraged the 2005–2011 Osaka Preference Parameter Survey.[10] We generated a binary variable that indicated whether a mother possessed a college education. We regressed maternal college education on a binary variable, namely the Year 1967, which equaled 1 if a respondent was born in 1967 and 0 otherwise. We also controlled for survey-year FEs. Figure 3.5 reports the estimates of the variable Year 1967 across different birth year ranges.[11] Based on Figure 3.5, we found that the estimates were small in magnitude and the confidence levels were exceedingly large,

[10] It is a panel data collected by Osaka University. Particularly, the data focus on collection of information on risk preferences and behavior across waves. See https://www.iser.osaka-u.ac.jp/survey_data/eng_panelsummary.html.
[11] Appendix Table 3.1 reports the corresponding estimates.

which suggested that the estimates were insignificant in both size and statistical significance. These results suggested that selection due to maternal education level was unlikely to affect our results.

Third, a smaller classroom size may also correlate with other noneducation factors that can affect health behavior, such as bullying in class and teachers' ability to monitor. For instance, the 1967-mismatched cohort might be less likely to have been bullied because of the small class size, allowing for a stronger peer effect and teacher oversight. Less bullying may in turn reduce victimization and improve mental health, which are correlated with smoking and sleep behavior (Hertz et al., 2015). That being said, the effect of this channel was unlikely given that most studies suggest that class size has little to no effect on bullying or mental health (Coelho & Sousa, 2018; Jakobsson et al., 2013; Persson & Svensson, 2013).

4.3. Additional Tests of Instrument Validity

Even when we controlled for marriage- and household-related variables, there was still a chance that some unobserved marriage- or household-related variables might have correlated with our instrument. In other words, the estimates between college education and health behavior may be a product of these unobserved variables. Generally, although it is difficult to assess how omitted unobservable variables affect one's estimates, it is possible to simulate omitted variables using observed variables to infer the omitted variable bias, similar to the study of Maruyama and Heinesen (2020). That is, if we assume that observed variables are sufficient proxies for related unobserved variables, we could use observed variables to simulate omitted variable bias by excluding said variable from the estimation. For example, dating experience would be an unobserved variable that is significantly correlated with a respondent's observed marital status. Given that dating and marriage are relatively similar and related, one could use observed marital status to infer the bias of our IV estimates to omit dating variables under the assumption that dating and marriage have an equal relationship

with the outcome and instrument variables. To simulate omitted variables, we sequentially excluded each socioeconomic variable from our baseline estimation. If the IV estimates were insensitive to the exclusion, we could conclude that there was only limited bias stemming from unobserved omitted variables.[12]

In addition to simulating omitted variable bias, we performed additional tests to validate our instrument. First, although we demonstrated that parental education was unlikely to affect our relationship through a delay in the timing of children's birth, one of the most conventional (albeit easiest) ways of delaying a birth from the FH year to the subsequent non-FH year for less-educated parents would be to delay it from December 31, 1966 to January 1, 1967. To assess whether parental education could affect our estimates, we removed those born in January from our estimations. Second, an advantage of our instrument was that we could overidentify our model by interacting FH with each month of the mismatched cohort, from January to March. Specifically, we performed Hansen-J over-identification tests to assess whether our instrument was correlated with the error terms. If the over-identification tests were passed, then this would certainly increase the confidence in our instrument's exogeneity. Finally, we may take advantage of the fact that 1965 school year experience a similar situation as 1967 school year. That is, those born between April and December of 1965 would experience a decline in competition for college and a classroom size similar to those born between January and March of 1965. We could construct an additional instrument based on the cohort born between April and December of 1965 to overidentify our model and perform Hansen-J over-identification tests similar to the previous test.

[12] Alternatively, one can address the unobserved variables correlated with 1967 by interacting the year 1967 with the gender variable. This would allow us to absorb any time-invariant unobserved variables correlated with 1967, since the FH superstition is related to gender. The results are reported in Appendix Table 3.2.

5. Results

5.1. Summary Statistics

[Table 3.1]

Table 3.1 reports the summary statistics for both the independent and dependent variables. Column (1) presents the means and standard deviations of all data used in the estimation. Columns (2) and (3) present the means and standard deviations for the control and mismatched cohorts for those born in 1947 to 1965 and 1968 to 1980 (non-1967 years), respectively. Similarly, columns (4) and (5) present the means and standard deviations for the control and mismatched groups for those born in 1967, respectively. First, for years of college education, the difference between the control and mismatched groups in 1967 was notably smaller than that of those in non-1967 years. That is, the difference for those in non-1967 years was 0.136, whereas that for 1967 was 0.023. This suggests that the FH effectively increased college attainment for the 1967-mismatched cohort. Corresponding to an increase in years of college education, we also observed a decline in tobacco and alcohol use and an increase in cancer screening for the 1967-mismatched cohort. Without a loss of generality, we observed that the difference in ever smokers between the control and mismatched cohorts for those from non-1967 years was 0.6%, whereas the difference for 1967 was −1.2%. In other words, the mismatched cohort exhibited a significant improvement in health behavior. Note that some behaviors, such as stomach cancer screening, exhibited no difference among the four cohorts, suggesting that college education had a heterogeneous effect on different behaviors.

5.2. Main Results

[Table 3.2]

Table 3.2 reports the ordinary least squares (OLS), first-stage 2SLS, and second-stage 2SLS estimates of years of college education on health behavior. Each column reports a different dependent variable. Panel A reports the OLS estimates, panel B the first-stage 2SLS estimates, and panel C the second-stage 2SLS estimates. The OLS estimates revealed that one additional year of college education significantly reduced the probability of being an ever smoker and a current smoker by 3.2 and 3.5 percentage points, respectively. Furthermore, 1 additional year of college significantly increased the probability of being an ever drinker and a current drinker by 1.0 percentage point each. For sleeping behavior, 1 additional year of college education increased the probability of having a good sleep by 0.2 percentage points. For cancer screening behavior, 1 additional year of college education significantly increased the probability of being screened for stomach cancer by 2.9 percentage points; being screened for lung cancer by 2.5 percentage points; being screened for ovarian cancer by 2.7 percentage points; being screened for breast cancer by 2.8 percentage points; and being screened for colon cancer by 2.5 percentage points. All estimates were statistically significant at the 1% level. Overall, the OLS estimates suggested that more years of college education were associated with better health behavior. The OLS estimates, however, were biased and could not convey the causal relationship between college education and health behavior.

Examining panels B and C, we first checked the direction, magnitude, and significance of the first-stage estimates for our instrument. The first-stage 2SLS estimates suggested that the instrument increased the year of college education for ever smokers by 0.094; for current smokers by 0.098; for ever drinkers by 0.091; for current drinkers by 0.098; for good sleep by 0.085; for stomach cancer screening by 0.088; for lung cancer screening by 0.092; for ovarian cancer screening by 0.083; for breast cancer screening by 0.085; and for colon cancer screening by 0.093, respectively. All estimates were significant at the 1% level. In sum, our estimates revealed that the 1967-mismatched cohort experienced

competition and classroom size shocks, which is consistent with our identification strategy. Moreover, the first-stage F-statistics were over 10 for all first-stage estimates, suggesting that the estimates were not likely to be biased by a weak instrument (Andrews & Stock, 2005).

In addition, the second-stage estimates indicated that more years of college education had a negative effect on substance abuse, such as smoking and drinking, as well as a positive effect on cancer screening behavior. We found that 1 additional year of college education reduced the probability of being an ever smoker and a current smoker by 14.7 and 11.3 percentage points, respectively. Similarly, 1 additional year of college education reduced the probability of being an ever drinker and a current drinker by 19.1 and 18.2 percentage points, respectively. Moreover, 1 additional year of college education reduced the probability of having good sleep by 3.0 percentage points, although the estimate was statistically insignificant. Finally, 1 additional year of college increased the probabilities of having a stomach cancer screening by 0.1 percentage points; a lung cancer screening by 6.4 percentage points; an ovarian cancer screening by 0.9 percentage points; a breast cancer screening by 35.8 percentage points; and a colon cancer screening by 23.3 percentage points. Only the estimates on breast and colon cancer screenings were significant. Our second-stage estimates suggested that college education indeed had a *causal* effect on health behavior, which was much greater than one would initially have thought if based on OLS estimates. Overall, our results suggested that college education has a strong positive effect on health behavior.

[Figure 3.6]

Figure 3.6 reports the effect of years of college education on health behavior by gender. Each panel reports a different dependent variable. All reported estimates are the 2SLS second-stage estimates.[13] According to the graph, women are the primary drivers of the

[13] Appendix Table 3.2 reports the corresponding estimates.

causal effect. In panel A, for instance, the magnitude of the estimate for men is relatively smaller than the estimate for women; moreover, the confidence interval for men crosses zero, suggesting that the estimate was statistically insignificant. By contrast, the confidence interval for women is narrower and does not cross zero, indicating that it is statistically significant. Similar patterns could be observed for all dependent variables, except for good sleep. This suggests that women, rather than men, drive the causal relationship between college education and health behavior.

[Figure 3.7]

[Figure 3.8]

In addition, we tested the sensitivity of our estimates to alternative birth year ranges. We expanded the range to include those born before 1947 and after 1980. Figure 3.7 reports the estimates of these alternative ranges.[14] Each panel represents a different dependent variable, and each line and dot represent an estimate of years of college education on health behavior using a different birth year range. Overall, we observed that the magnitude and confidence interval of the estimates were almost the same across alternative birth year ranges. This was reassuring as it suggested that our estimates were not a product of an arbitrary cutoff of a birth year range. Second, we restrict the range to those born after 1947 and before 1980 to limit the effect of educational reforms in 1990s. Figure 3.8 shows the estimates of these alternative ranges.[15]

5.3. Additional Results on Instrument Validity

[Figure 3.9]

[14] Appendix Table 3.3 reports the corresponding estimates.
[15] Appendix Table 3.4 reports the corresponding estimates.

Figure 3.9 reports the estimates from simulating omitted variable bias through progressively removing each socioeconomic variable.[16] Each color represents a specification excluding a specific socioeconomic variable. Gray represents a specification excluding all socioeconomic variables from the baseline. According to the figure, the estimates were similar in magnitude and confidence intervals when each socioeconomic variable was sequentially excluded from our baseline estimation. The figure suggests that the IV estimates were extremely insensitive to any type of exclusion. In other words, under the assumption that both observed and unobserved marriage- and household-related variables had similar relationships with outcome and instrument variables, our estimates were insensitive to the presence of unobserved omitted marriage- and household-related variables. Therefore, unobserved omitted marriage- and household-related factors were unlikely to have influenced our findings.

[Table 3.3]

In Table 3.3, panels A, B, and C report the second-stage 2SLS estimates without the January cohort and overidentified with multiple instruments. Panel A indicates that the direction and significance of the estimates were similar to our baseline estimates. The magnitudes of the estimates were generally similar to our baseline estimates, although the estimate on current smokers was diminished more than other estimates. In sum, our estimates were robust to the exclusion of the January cohort. Examining panels B and C, the estimates overidentified with two instruments were similar in direction, magnitude, and significance with our baseline estimates with the just-identified instrument. More crucially, the Hansen-J *p* values for all dependent variables were greater than 0.250, indicating that the instrument

[16] Appendix Table 3.5 reports the corresponding estimates.

failed to reject the null hypothesis. This suggested that the instrument was not likely to be correlated with the error terms.

5.4. Mechanism: Labor Market Outcomes

Buckles et al. (2018) and Cutler and Lleras-Muney (2010) have shown that income is one potential way that higher education influencing health behavior. While our data lacked the income data, we did possess an expanded set of labor market outcomes that are related to income but also contribute to better understanding of labor market mechanism of higher education. We generated four binary and one continuous variables corresponding to labor market: Employed, Log(Hour Worked), Fulltime, Large Corporations, Civil Servants, and White Collar.

Table 3.4 presents the 2SLS estimates of college education on labor market outcome. We observed that all our 2SLS estimates on college education are positive, but only two estimates, Fulltime and Civil Servants, are statistically significant. Based on the estimates, 1 additional year of college education increases the probability of being employed by 8.5 percentage points; being a fulltime worker by 17.0 percentage points; being employed by large corporations by 15.3 percentage points; being a civil servant 12.0 percentage points; and being employed as a white collar worker by 4.3 percentage points. This results imply that the college education significantly increases the stability of employment, leading to better health behavior.

[Table 3.4]

5.4. Robustness Check

[Table 3.5]

As presented in Table 3.5, we further tested the robustness of our results by using an alternative definition of education, alternative college education variables, and alternative

imputation of education. Panel A of Table 3.5 reports 2SLS estimates using years of schooling; panel B reports 2SLS estimates using a binary college education, college or above; panel C reports 2SLS estimates using bachelor's or above; panel D reports 2SLS estimates where master's or above was defined as six instead of nine, panel E reports 2SLS estimates master's or above was defined as four instead of nine. And, finally, panel F reports 2SLS estimates excluding those respondents with less than junior high school education, since IV only estimate the local average treatment effect. In other words, the treatment only affects those individuals who attended college due to a decline in competition and classroom size. These are compliers. However, those noncomplier individuals should not affected by the treatment, for example, those with less junior high school education. Removing these individuals may further validate our results by showing that it is compliers who are driving our estimates. Overall, our estimates from the robustness checks did not differ significantly from our baseline estimates in terms of direction, magnitude, or significance.

6. Discussion

In this study, we investigated the causal effect of college education on smoking, drinking, sleeping, and cancer screening behavior in Japan. As an instrument for college education, we exploited college competition and classroom size shocks driven by a Japanese superstition, namely FH. Moreover, to avoid the issue of selection, we leveraged a mismatch between the calendar year and Japanese school year as an instrument. Using 2013 and 2016 CLSC data, we found that 1 additional year of college education significantly reduced smoking and drinking; significantly increased breast and colon cancer screening; but had no effect on sleep or stomach, lung, and ovarian cancer screening. We also explored the causal effect by gender. We observed that women were more affected by college education

compared with men. Finally, we show that labor market outcomes are important mechanism behind the relationship between college education and health behavior.

As with any IV, we estimating the treatment effect on compilers. Compilers may or may not represent the whole population. Therefore, it is difficult to generalize our estimates to different subpopulation of the whole. However, what we could suggest is that our compilers are individuals who experience a decline in competition and classroom size. We could generalize that to individuals who experience similar situations, such as schooling expansion and mandating classroom size cut in countries like Germany and South Korea. Overall, those compilers may be more similar to these individuals who experience these similar policies.

Our study had some limitations. First, our measurements were self-reported. This means that health behavior may have been underreported since respondents might have concealed their smoking and drinking behaviors to appear desirable (i.e., desirability bias). Such a bias generally cannot be eliminated unless one has access to objective data, such as blood nicotine and ethanol levels measured in a laboratory setting. However, this type of data comes at the cost of generalizability to the entire population. Given this tradeoff, it was sufficient to use self-reported data. Second, our data were cross-sectional. We were unable to track an individual over time, which would have allowed us to understand whether the effect was a short- or long-term one. Having access to panel data could further the understanding of the temporal effect of higher education on health behavior. Finally, our IV estimates captured the *whole* effect of college education on health behavior. In other words, we did not differentiate the effect by different channels regarding how college education affects health behavior, such as income, risk preference, and other channels. Given that our data set contained a limited number of these variables, we were unable to disentangle the channels

through which college education affects health behavior. Future research could focus on the channels that mediating this causal relationship.

Our findings have several implications. First, our results point to the fact that college-education experience significantly improves health-investment behaviors and reduces health-disinvestment behaviors. Given the beneficial effect of higher education, public policies aimed at improving the population's health can certainly be intervened in through the provision of better access to higher education, such as financial supports for students (e.g., student loans). Moreover, educational policies may generate many more advantages than one might expect, as one could overlook the fact that education would also affect health behavior and health status in later life. Second, our findings revealed that women are more affected. This is in contrast to findings from the UK and Germany, where men have been found to be more affected by higher education (Kemptner et al., 2011; Silles, 2015). One plausible explanation is that higher education may have offered more health knowledge to women. In Japan, traditional gender roles are still relatively rigid and prevalent, and women tend to be become mothers after marriage. With additional health knowledge gained from higher education, these individuals may change their health behavior in response to such knowledge, since mothers' health influence infants' health. This may partly have explained why we only observed women being affected by college education, as they are concerned about how their health could affect the health of their children in the future.

Recently, the public have begun to question the value of college education. Our findings indicate that college education has beneficial effects beyond the immediate economic returns to college, such as higher wages and greater job security. Instead, our study demonstrated that college education can significantly benefit one's health through a reduction in health-disinvestment behaviors and an improvement in health-investment behaviors. Policymakers who are considering reducing funding for higher education may need to

consider the associated costs and benefits of higher education – not only the economic benefits but also the health benefits.

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Table 3.1. Summary Statistics

	All years	1947–1965 and 1968–1980		1967	
	All	Control	Mismatched	Control	Mismatched
	(1)	(2)	(3)	(4)	(5)
Years of College	1.419	1.454	1.318	1.530	1.507
	(1.898)	(1.913)	(1.850)	(1.907)	(1.932)
Ever Smokers	0.262	0.263	0.257	0.285	0.273
	(0.440)	(0.440)	(0.437)	(0.451)	(0.446)
Current Smokers	0.687	0.688	0.682	0.713	0.692
	(0.464)	(0.463)	(0.466)	(0.452)	(0.462)
Ever Drinkers	0.682	0.683	0.676	0.710	0.687
	(0.466)	(0.465)	(0.468)	(0.454)	(0.464)
Current Smokers	0.402	0.403	0.390	0.479	0.465
	(0.490)	(0.491)	(0.488)	(0.500)	(0.499)
Good Sleep	0.418	0.415	0.423	0.443	0.446
	(0.493)	(0.493)	(0.494)	(0.497)	(0.497)
Stomach Cancer Screening	0.472	0.468	0.479	0.501	0.512
	(0.499)	(0.499)	(0.500)	(0.500)	(0.500)
Lung Cancer Screening	0.384	0.384	0.378	0.444	0.443
	(0.486)	(0.486)	(0.485)	(0.497)	(0.497)
Ovarian Cancer Screening	0.366	0.364	0.365	0.419	0.447
	(0.482)	(0.481)	(0.481)	(0.493)	(0.497)

Breast Cancer Screening	0.401 (0.490)	0.396 (0.489)	0.409 (0.492)	0.420 (0.494)	0.447 (0.497)
Colon Cancer Screening	0.367 (0.482)	0.361 (0.480)	0.377 (0.485)	0.420 (0.494)	0.447 (0.497)
Women	0.511 (0.500)	0.510 (0.500)	0.515 (0.500)	0.517 (0.500)	0.521 (0.500)
HH Structure – Couple	0.190 (0.393)	0.189 (0.391)	0.206 (0.404)	0.090 (0.287)	0.105 (0.307)
HH Structure – Couple with Children	0.419 (0.493)	0.423 (0.494)	0.398 (0.490)	0.519 (0.500)	0.497 (0.500)
HH Structure – Single with Children	0.065 (0.246)	0.064 (0.246)	0.063 (0.243)	0.087 (0.282)	0.079 (0.269)
HH Structure – 3 Generations	0.145 (0.352)	0.143 (0.350)	0.147 (0.354)	0.154 (0.361)	0.166 (0.372)
HH Structure – Other	0.096 (0.295)	0.095 (0.293)	0.101 (0.302)	0.075 (0.263)	0.077 (0.266)
Single	0.145 (0.353)	0.149 (0.356)	0.135 (0.341)	0.171 (0.376)	0.162 (0.369)
Widowed	0.029	0.029	0.032	0.008	0.009

	(0.168)	(0.167)	(0.177)	(0.091)	(0.096)
Divorced	0.067	0.067	0.067	0.077	0.077
	(0.250)	(0.250)	(0.251)	(0.267)	(0.267)
Children Do Not Live Together	0.178	0.175	0.196	0.070	0.086
	(0.382)	(0.380)	(0.397)	(0.255)	(0.281)
Children Live Together	0.559	0.558	0.547	0.664	0.653
	(0.497)	(0.497)	(0.498)	(0.472)	(0.476)
Children in Home – Unknown	0.016	0.016	0.017	0.014	0.013
	(0.126)	(0.125)	(0.129)	(0.119)	(0.111)
Shared House	0.216	0.220	0.204	0.249	0.237
	(0.412)	(0.414)	(0.403)	(0.432)	(0.425)
Observations	474872	332531	127490	10540	4311

Column (1) reports the means and standard deviations of the whole sample. Columns (2) and (3) report the means and standard deviations of the non-1967 years, which were between 1947 and 1980, excluding 1966. Columns (4) and (5) report the means and standard deviations of the sample in 1967. Control refers to individuals born between January and March, and mismatched refers to individuals born between April and December.

Table 3.2. Effect of Years of College Education on Health Behavior for OLS and 2SLS

	(1) Ever Smoker	(2) Current Smoker	(3) Ever Drinker	(4) Current Drinker	(5) Good Sleep	(6) Stomach Cancer Screening	(7) Lung Cancer Screening	(8) Ovarian Cancer Screening	(9) Breast Cancer Screening	(10) Colon Cancer Screening
Panel A: OLS										
Years of College	−0.032***	−0.035***	0.010***	0.010***	−0.002**	0.029***	0.025***	0.027***	0.028***	0.025***
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Panel B: 2SLS first-stage										
Mismatch × 1967	0.094***	0.098***	0.091***	0.098***	0.085***	0.088***	0.092***	0.083***	0.085***	0.093***
	(0.023)	(0.021)	(0.023)	(0.025)	(0.022)	(0.025)	(0.024)	(0.022)	(0.022)	(0.026)
Panel C: 2SLS second-stage										
Years of College	−0.147**	−0.113*	−0.191**	−0.182**	−0.030	0.001	0.064	0.009	0.358**	0.233**
	(0.074)	(0.068)	(0.083)	(0.078)	(0.071)	(0.076)	(0.071)	(0.141)	(0.156)	(0.092)
Socioeconomic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth-month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prefectural FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First-stage F-statistics	135.501	156.999	128.592	121.084	115.288	103.337	115.598	140.257	138.035	106.282
Observations	460,888	434,013	461,631	454,185	460,917	445,306	443,738	227,297	227,337	442,666

Note: Panel A reports OLS estimates, and panels B and C report 2SLS estimates. Panel B reports the first-stage estimates, and panel C reports the second-stage estimates. Each column reports a different dependent variable. Years of college education is a continuous variable that equals 0 if respondents graduate with an education level less than high school; 2 if vocational school or short-term college; 4 if bachelor's degree; and 9 if a master's degree or above. Socioeconomic controls include sex, marital status, household type, whether children lived together, and type of house owned. Prefectural FE is a vector of prefecture dummies where surveys were sampled, and survey-year FE is a vector of year dummies when surveys were sampled. Birth-month FE is a vector of month binary variables corresponding to January to December. Birth-year FE is a

vector of year binary variables corresponding to the year an individual was born from 1947 to 1980, excluding 1966. The standard errors are clustered at birth month and year levels. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.3. Additional Tests for Instrumental Validity

	(1) Ever Smoker	(2) Current Smoker	(3) Ever Drinker	(4) Current Drinker	(5) Good Sleep	(6) Stomach Cancer Screening	(7) Lung Cancer Screening	(8) Ovarian Cancer Screening	(9) Breast Cancer Screening	(10) Colon Cancer Screening
Panel A: Without January cohort										
Years of College	−0.100*	−0.077	−0.185**	−0.171**	−0.047	0.004	0.095	0.095	0.388**	0.238***
	(0.054)	(0.062)	(0.094)	(0.086)	(0.066)	(0.081)	(0.069)	(0.163)	(0.175)	(0.089)
Observations	414,451	390,234	415,158	408,468	414,510	400,439	399,064	204,090	204,094	398,071
Panel B: Overidentified with multiple instruments I										
Years of College	−0.147***	−0.112***	−0.187***	−0.173***	−0.029	0.008	0.064***	−0.007	0.364***	0.230***
	(0.020)	(0.017)	(0.022)	(0.021)	(0.021)	(0.026)	(0.023)	(0.045)	(0.062)	(0.037)
First-stage F-stats	91.339	100.596	92.466	119.872	78.569	55.142	66.577	67.560	78.891	55.177
Hansen-J P-values	0.922	0.343	0.315	0.315	0.342	0.312	0.361	0.308	0.302	0.257
Observations	460,888	434,013	461,631	454,185	460,917	434,765	433,190	227,297	227,337	442,666
Panel C: Overidentified with multiple instruments II										
Years of College	-0.124***	-0.100***	-0.178***	-0.169***	-0.065	-0.046	0.048*	0.105	0.333***	0.233***
	(0.043)	(0.023)	(0.021)	(0.020)	(0.045)	(0.046)	(0.026)	(0.088)	(0.007)	(0.037)
Hansen-J P-values	0.298	0.309	0.338	0.335	0.298	0.303	0.314	0.234		0.993
Observations	460,888	434,013	461,631	454,185	460,917	445,306	443,738	227,297	227,,337	442,666

Note: Panel A reports the second-stage 2SLS estimates without the January cohort, and panel B reports the second-stage 2SLS estimates with two instruments. Each column reports a different dependent variable. Years of college education is a continuous variable that equals 0 if respondents graduated with an education level less than high school; 2 if vocational school or short-term college; 4 if bachelor's degree; and 9 if a master's degree or above. Socioeconomic controls include sex, marital status, household type, whether children lived together, and type of house owned. Prefectural FE is a vector of prefecture dummies where surveys were sampled and survey-year FE is a vector of year dummies when surveys were sampled. Birth-month FE is a vector of month binary variables corresponding to January to December. Birth-year FE is a

vector of year binary variables corresponding to the year an individual was born from 1947 to 1980, excluding 1966. The standard errors are clustered at birth month and year levels. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.4. Effect of Years of College Education on Labor Market Outcomes

	(1) Employed	(2) Log(Hours Worked)	(3) Fulltime	(4) Large Corporations	(5) Civil Servant	(6) White Collar
Years of College	0.085	0.056	0.170*	0.153	0.120*	0.043
	(0.052)	(0.066)	(0.093)	(0.227)	(0.069)	(0.063)
Socioeconomic controls	Yes	Yes	Yes	Yes	Yes	Yes
Birth-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Survey-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Prefectural FE	Yes	Yes	Yes	Yes	Yes	Yes
First-stage F-statistics	111.738	95.707	78.067	34.686	69.237	138.204
Observations	473846	344818	254559	257095	281307	332004

Note: All columns report the second-stage estimates. Each column reports a different dependent variable. Years of college education is a continuous variable that equals 0 if respondents graduated with an education level less than high school; 2 if vocational school or short-term college; 4 if bachelor's degree; and 9 if a master's degree or above. Socioeconomic controls include sex, marital status, household type, whether children lived together, and type of house owned. Prefectural FE is a vector of prefecture dummies where surveys were sampled and survey-year FE is a vector of year dummies when surveys were sampled. Birth-month FE is a vector of month binary variables corresponding to January to December. Birth-year FE is a vector of year binary variables corresponding to the year an individual was born from 1947 to 1980, excluding 1966. The standard errors are clustered at birth month and year levels. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.5. Additional Robustness Check

	(1) Ever Smoker	(2) Current Smoker	(3) Ever Drinker	(4) Current Drinker	(5) Good Sleep	(6) Stomach Cancer Screening	(7) Lung Cancer Screening	(8) Ovarian Cancer Screening	(9) Breast Cancer Screening	(10) Colon Cancer Screening
Panel A: Alternative definition of schooling										
Years of Schooling	−0.096**	−0.076	−0.124**	−0.120***	−0.019	0.001	0.041	0.006	0.240***	0.151***
	(0.048)	(0.047)	(0.048)	(0.045)	(0.044)	(0.048)	(0.046)	(0.094)	(0.087)	(0.053)
Observations	460,888	434,013	461,631	454,185	460,917	445,306	443,738	227,297	227,337	442,666
Panel B: Binary variable of college I										
College or Above	−0.720***	−0.544***	−0.944***	−0.929***	−0.143	0.006	0.309***	0.039	1.520***	1.116***
	(0.095)	(0.077)	(0.117)	(0.116)	(0.100)	(0.128)	(0.107)	(0.198)	(0.332)	(0.167)
Observations	460,888	434,013	461,631	454,185	460,917	445,306	443,738	227,297	227,337	442,666
Panel C: Binary variable of college II										
Bachelor's or Above	−0.763	−0.545	−0.979**	−0.927**	−0.154	0.006	0.330	0.064	2.454	1.253**
	(0.531)	(0.423)	(0.431)	(0.424)	(0.342)	(0.400)	(0.316)	(0.953)	(2.134)	(0.591)
Observations	460,888	434,013	461,631	454,185	460,917	445,306	443,738	227,297	227,337	442,666
Panel D: Alternative imputation of graduate school (imputing graduate school to be 6 years)										
Years of College	−0.168**	−0.126*	−0.218**	−0.209**	−0.034	0.001	0.073	0.011	0.418**	0.266***
	(0.079)	(0.071)	(0.101)	(0.096)	(0.081)	(0.086)	(0.083)	(0.163)	(0.185)	(0.101)
Observations	460,888	434,013	461,631	454,185	460,917	445,306	443,738	227,297	227,337	442,666
Panel E: Alternative imputation of graduate school (imputing graduate school to be 4 years)										

Years of College	-0.185**	-0.136*	-0.240**	-0.232**	-0.037	0.002	0.080	0.012	0.469**	0.295***
	(0.084)	(0.074)	(0.119)	(0.113)	(0.090)	(0.095)	(0.094)	(0.181)	(0.213)	(0.109)
Observations	460,888	434,013	461,631	454,185	460,917	445,306	443,738	227,297	227,337	442,666
Panel F: Excluding respondents with less than junior high school										
Years of College	-0.172**	-0.128*	-0.260**	-0.247**	-0.033	-0.016	0.077	-0.003	0.397*	0.245*
	(0.083)	(0.067)	(0.118)	(0.115)	(0.102)	(0.084)	(0.086)	(0.182)	(0.206)	(0.136)
Observations	423492	398519	424072	417690	423525	409740	408278	211382	211388	407352

Note: Panel A reports the estimates using years of schooling, panel B the estimates using college or above, panel C the estimates using bachelor or above, and panel D the estimates using six as the final year of college education. All panels report the second-stage estimates. Each column reports a different dependent variable. Years of college education is a continuous variable that equals 0 if respondents graduated with an education level less than high school; 2 if vocational school or short-term college; 4 if bachelor's degree; and 9 if a master's degree or above. Socioeconomic controls include sex, marital status, household type, whether children lived together, and type of house owned. Prefectural FE is a vector of prefecture dummies where surveys were sampled and survey-year FE is a vector of year dummies when surveys were sampled. Birth-month FE is a vector of month binary variables corresponding to January to December. Birth-year FE is a vector of year binary variables corresponding to the year an individual was born from 1947 to 1980, excluding 1966. The standard errors are clustered at birth month and year levels. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

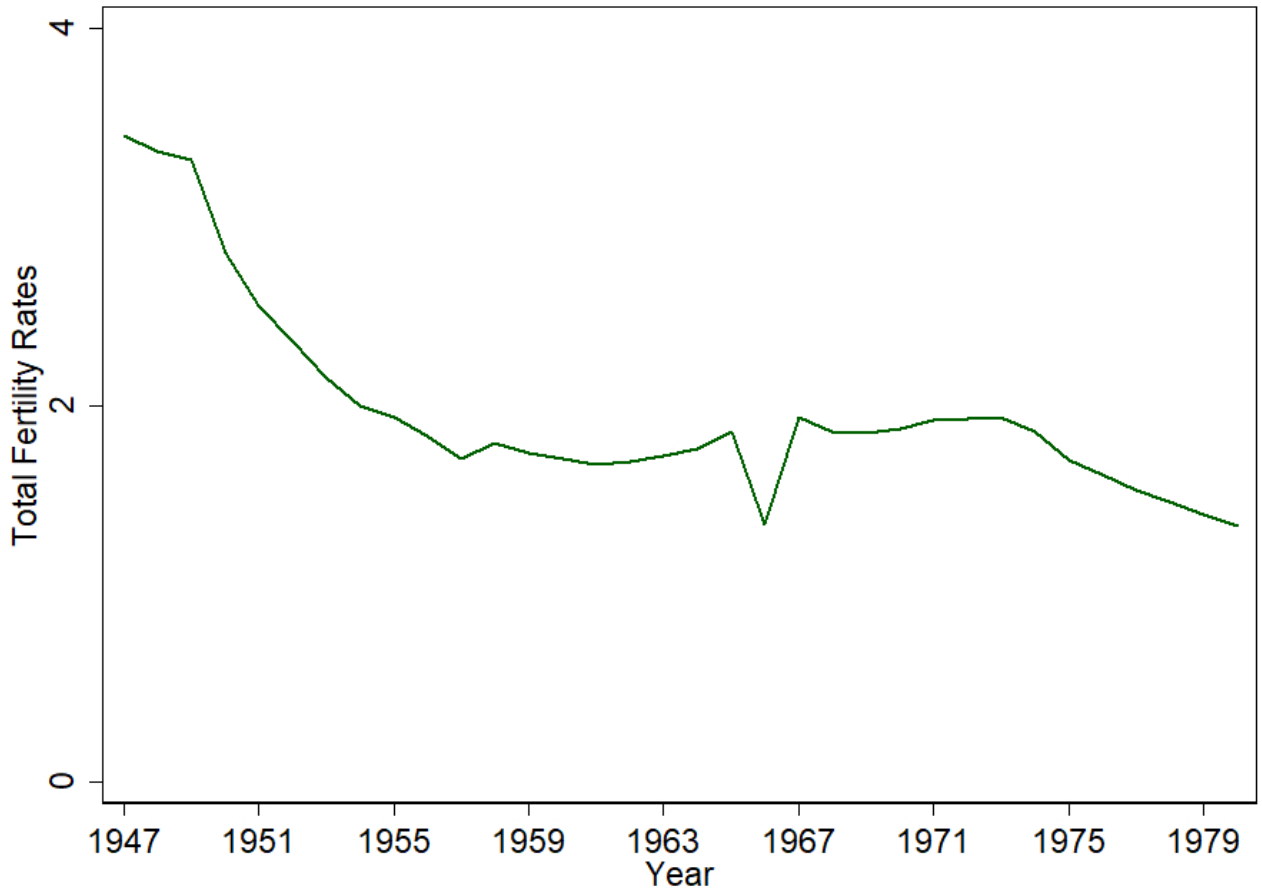


Figure 3.1. Total fertility rates from 1947 to 1980.

Source: Vital Statistics.

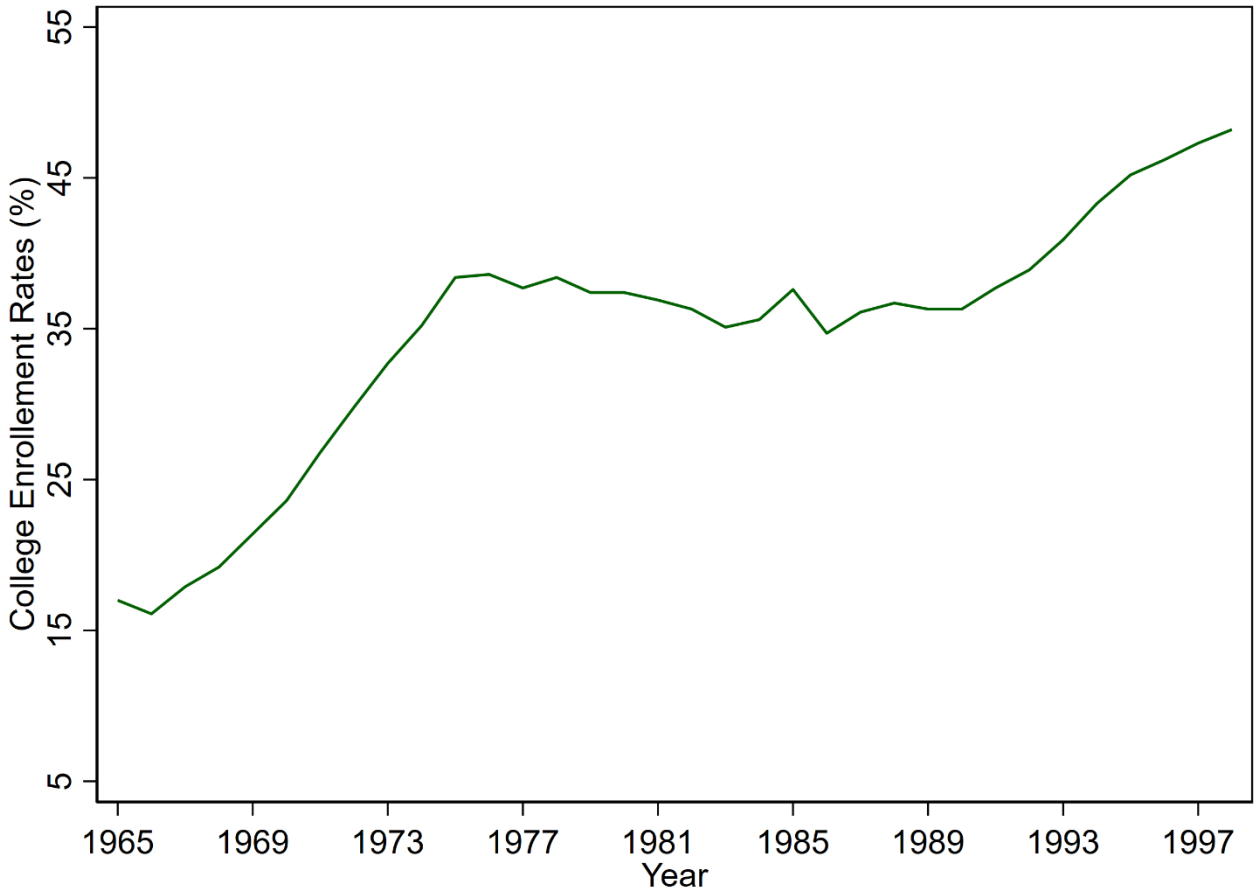


Figure 3.2. College enrollment rates from 1965 to 1998.

Source: 1954–2008 Basic Education Survey .

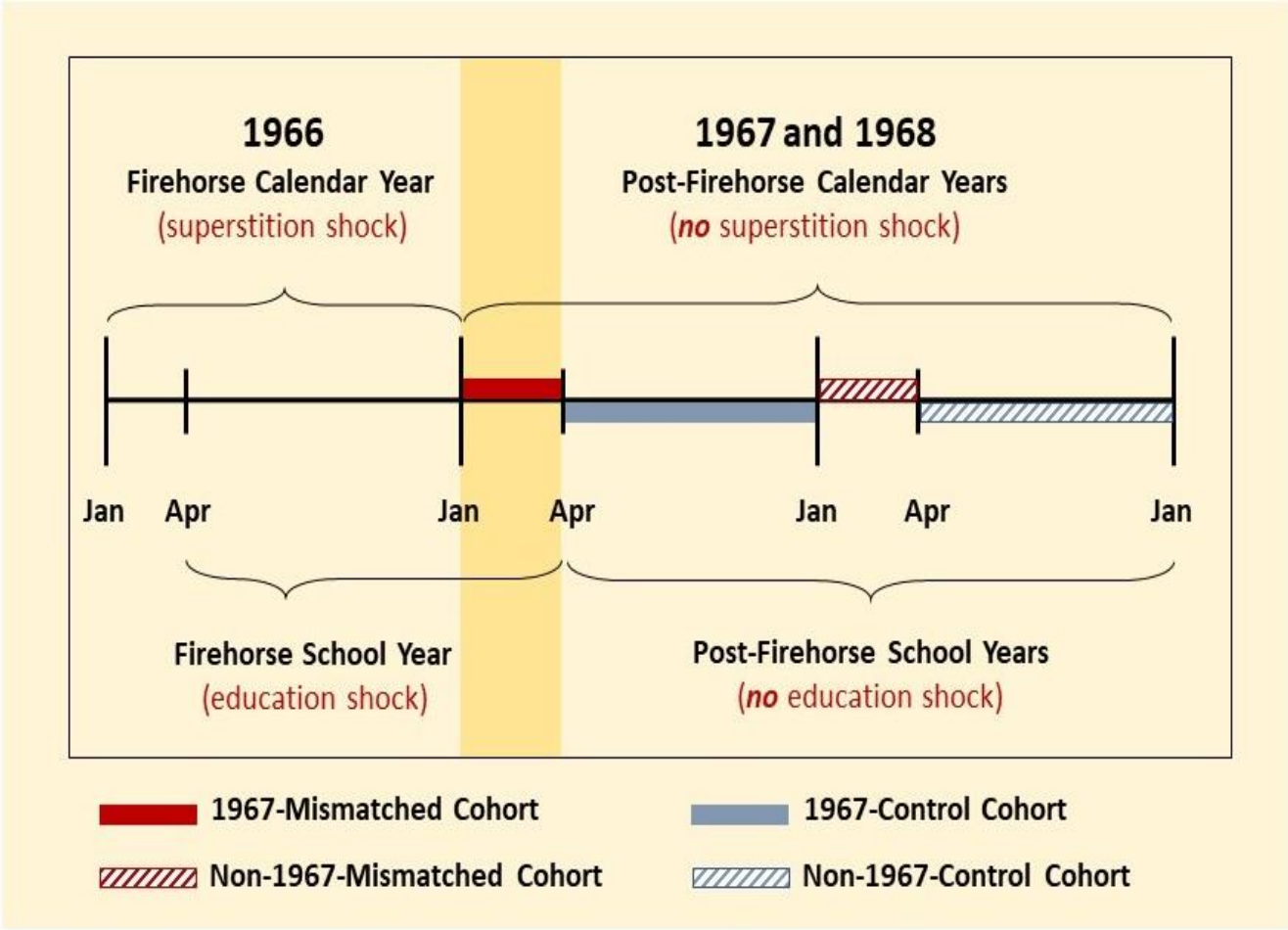


Figure 3.3 Mismatch between the calendar year and school year in Japan.

Source: Authors.

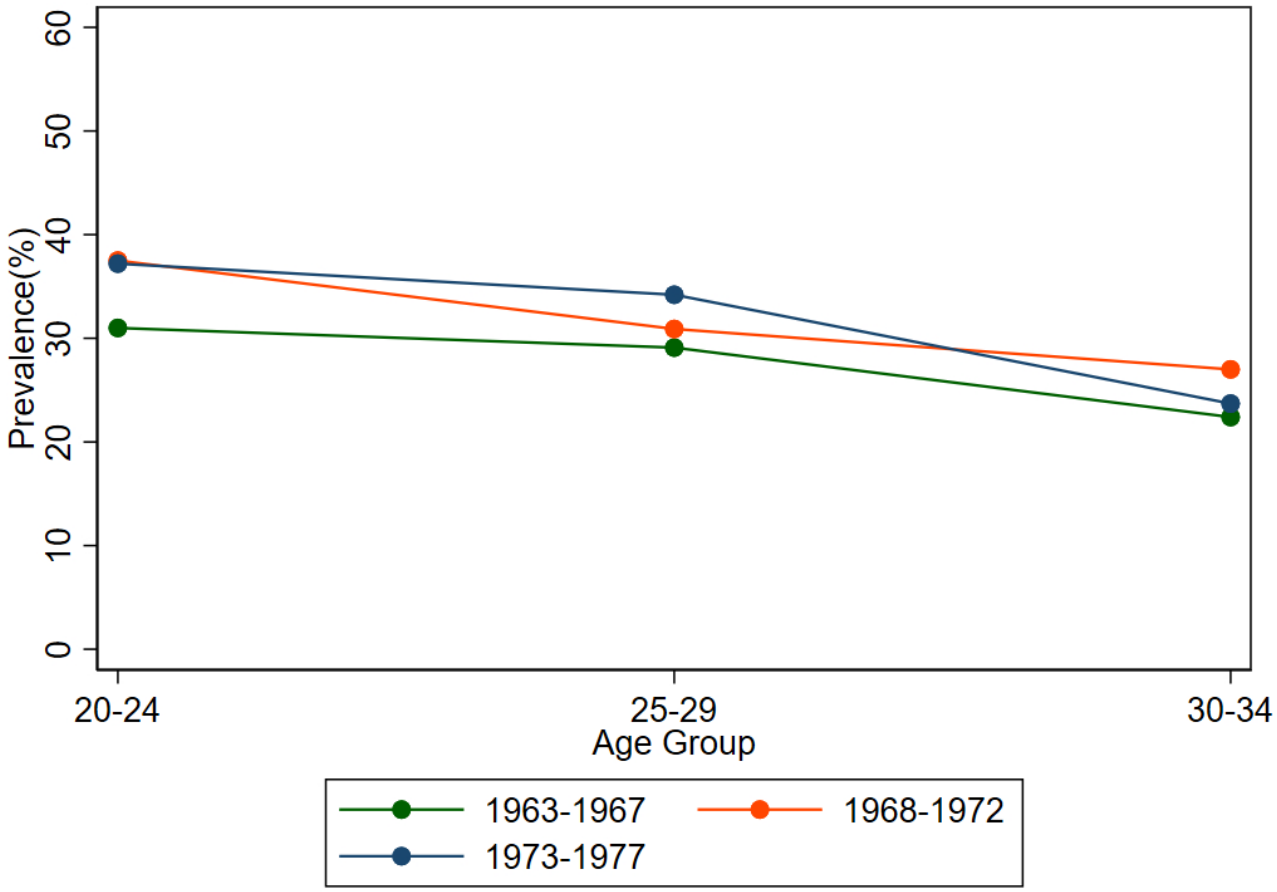


Figure 3.4. Prevalence of women who are not dating at each age by birth year.

Source: Reports on Japanese National Fertility Survey from 1987–2005.

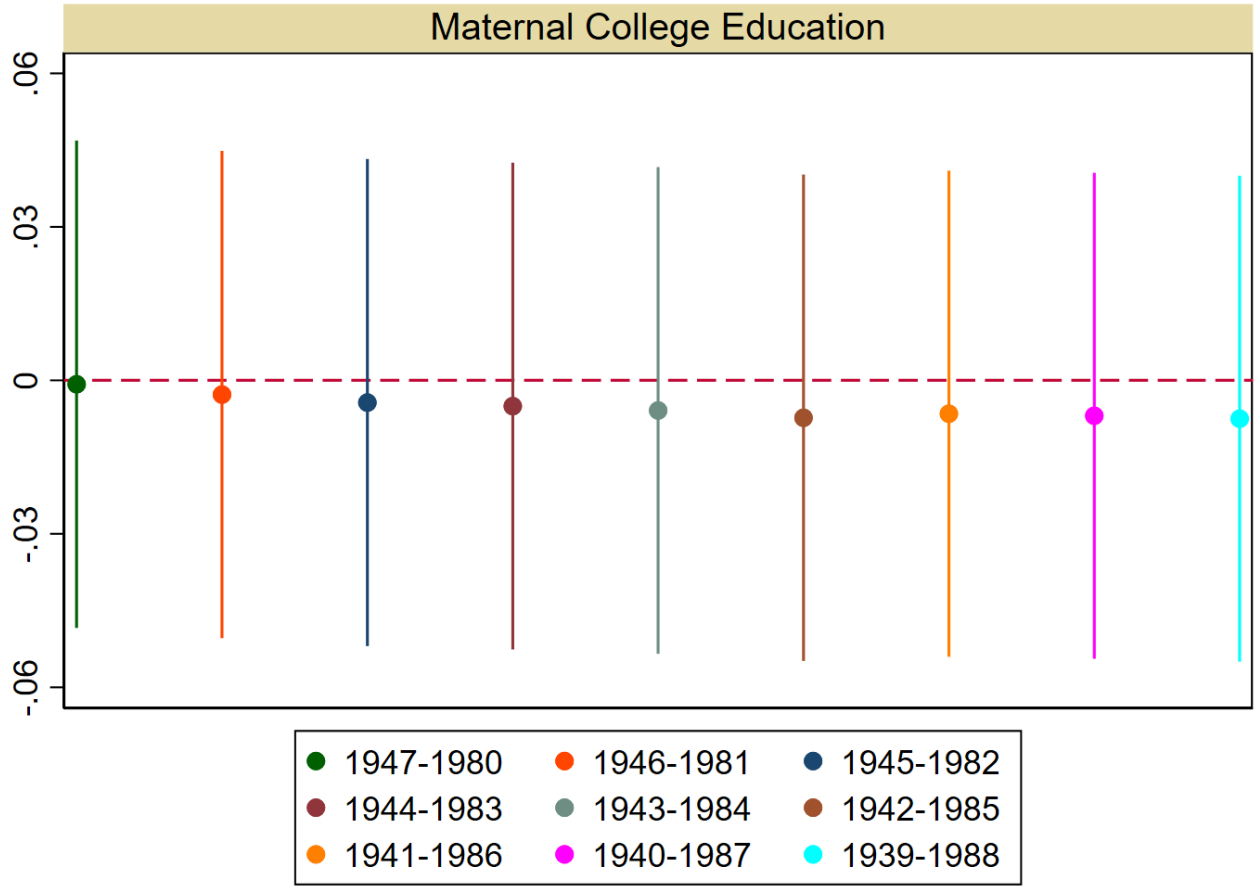


Figure 3.5. Effect of year 1967 on maternal college education.

Note: The dependent variable is a binary variable indicating whether an individual's mother has a college degree or not. The independent variable reported is a binary variable that indicates whether an individual was born in 1967 or not. Each controls for survey fixed effects and clusters the standard errors at the individual level. Each line represents a separate regression result using a different birth year range. Each line increases the birth year range by 1 year either side of the range. For example, the red line uses 1946–1981, whereas the dark blue line uses 1945–

1982.

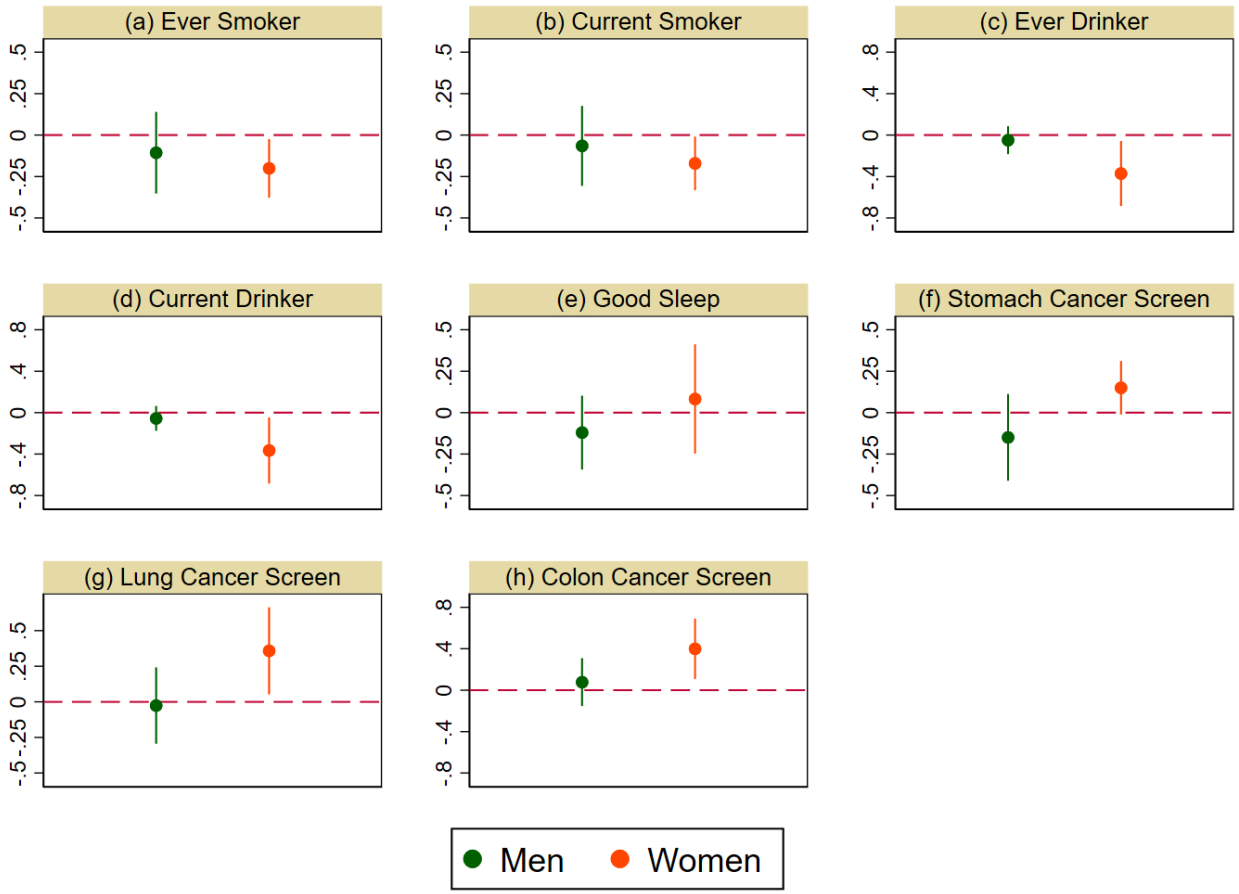


Figure 3.6. Effect of years of college education on health behavior by gender.

Note: Each panel represents a different dependent variable. Each line represents estimates for each gender. All estimates are the second-stage 2SLS estimates. All regression estimates control for socioeconomic variables, such as marital status and household type, as well as fixed effects. The standard errors are clustered at birth month and year levels.

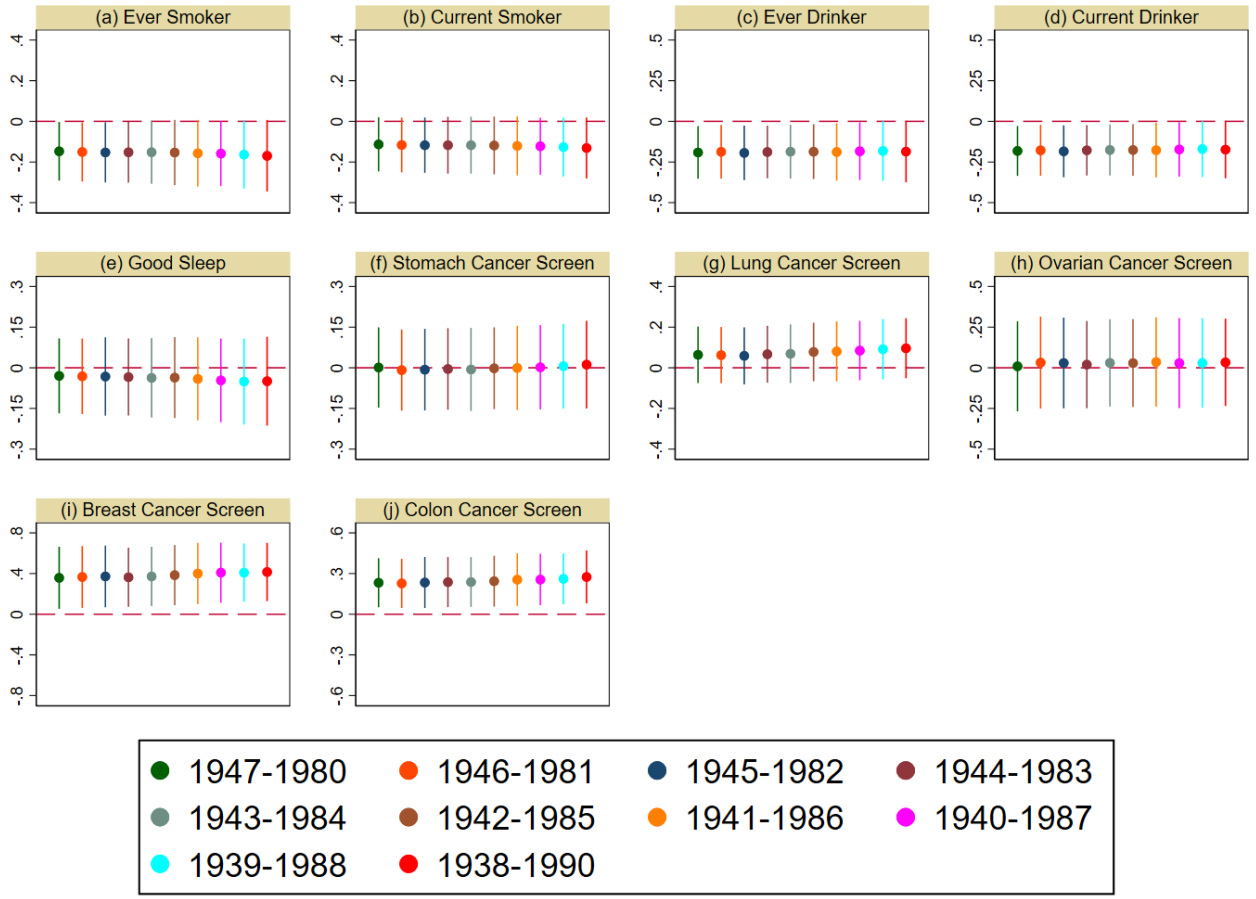


Figure 3.7. Alternative year ranges: expanding birth year range.

Note: Each panel represents a different dependent variable. Each line represents estimates for a different birth year range. All estimates are the second-stage 2SLS estimates. All regression estimates control for socioeconomic variables, such as marital status and household type, as well as fixed effects. The standard errors are clustered at birth month and year levels.

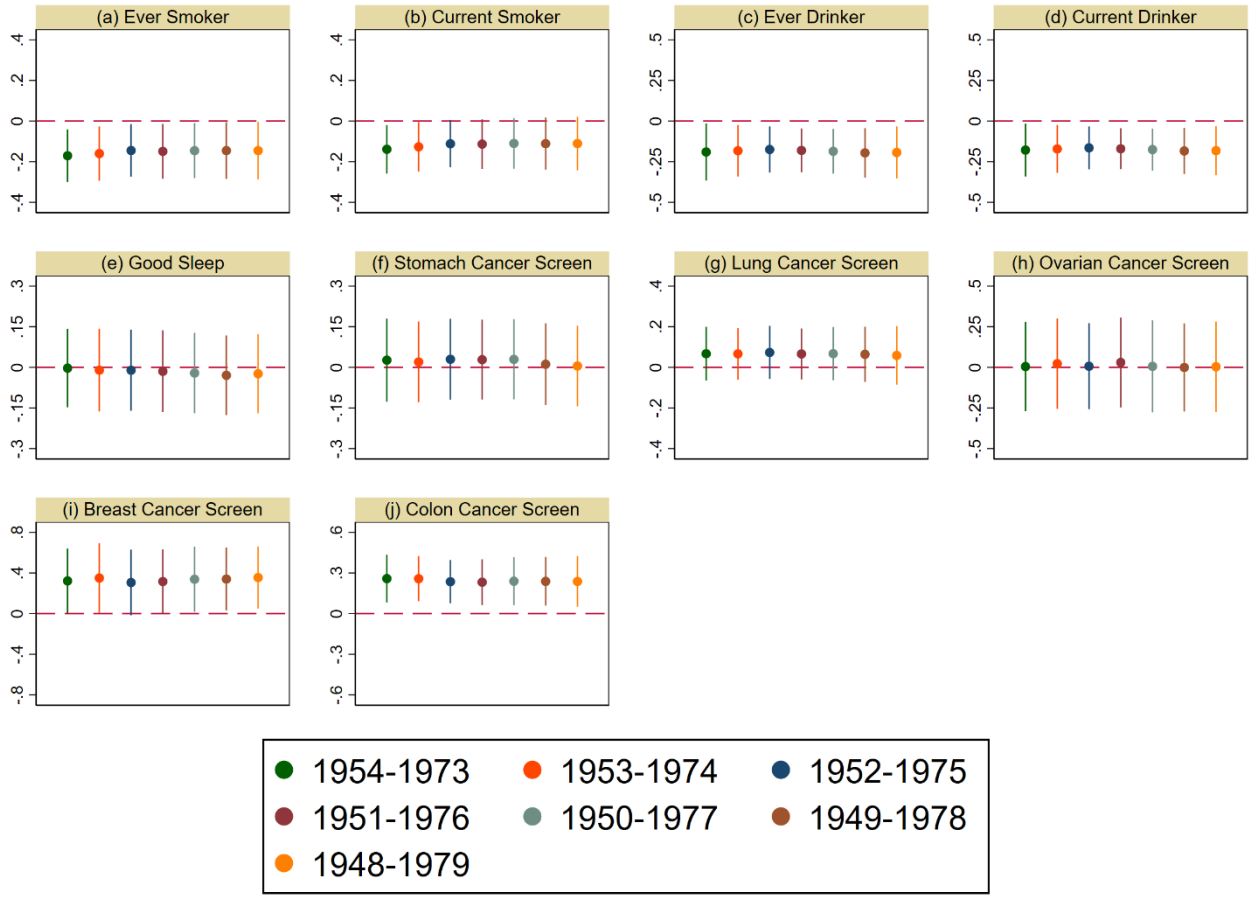


Figure 3.8. Alternative year ranges: restricting birth year range.

Note: Each panel represents a different dependent variable. Each line represents estimates for a different birth year range. All estimates are the second-stage 2SLS estimates. All regression estimates control for socioeconomic variables, such as marital status and household type, as well as fixed effects. The standard errors are clustered at birth month and year levels.

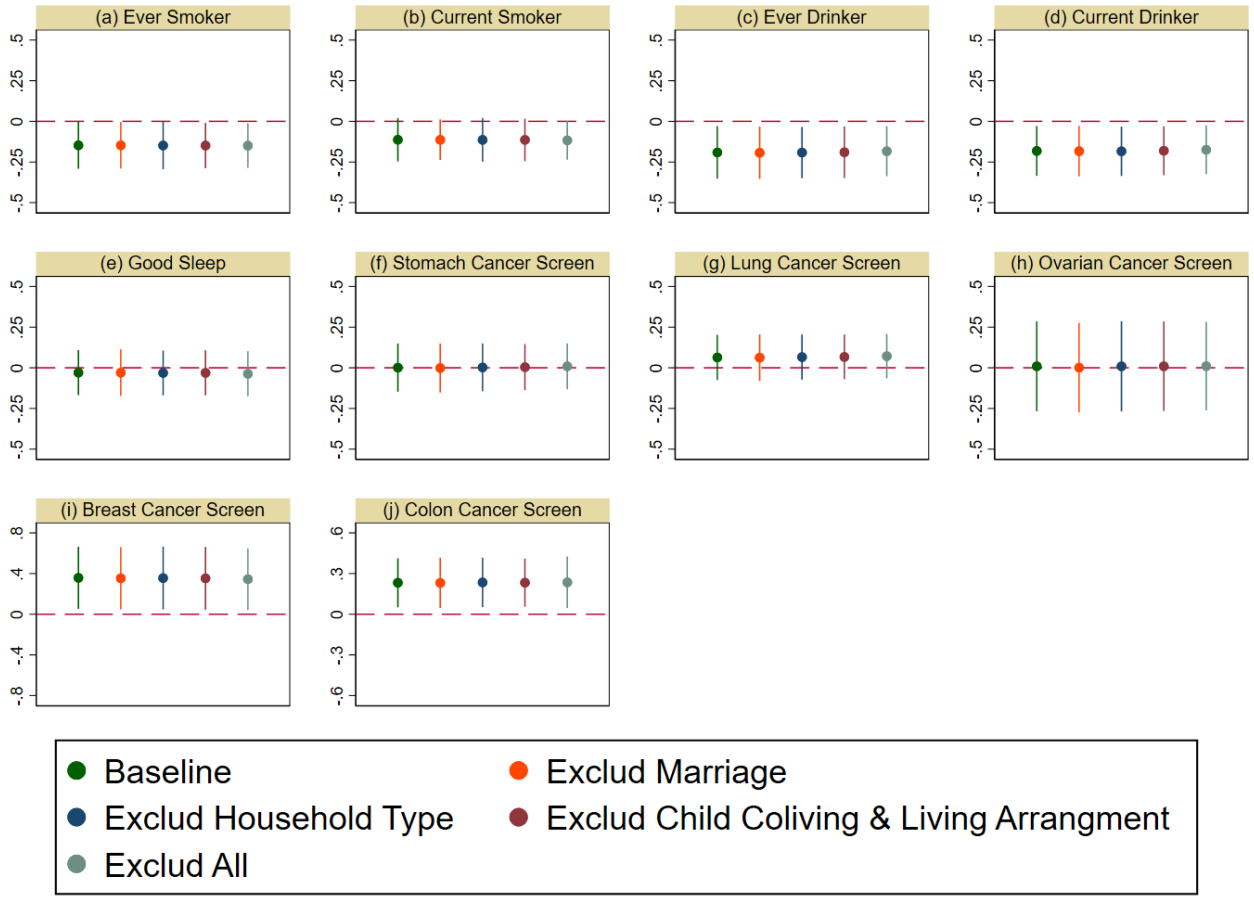


Figure 3.9. Inferring bias from unobserved omitted variables using observed variables.

Note: Each panel represents a different dependent variable. Each line excludes a different set of variables. All estimates are the second-stage 2SLS estimates. All regression estimates control for socioeconomic variables, such as marital status and household type, as well as fixed effects. The standard errors are clustered at birth month and year levels.

Appendix

Appendix Table 3.1. Maternal Education Level for Those Born in 1967

	Maternal College Education								
	(1) 1947–1980	(2) 1946–1981	(3) 1945–1982	(4) 1944–1983	(5) 1943–1984	(6) 1942–1985	(7) 1941–1986	(8) 1940–1987	(9) 1939–1988
Year 1967	−0.001	−0.003	−0.004	−0.005	−0.006	−0.007	−0.007	−0.007	−0.007
	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)
Observations	18,681	19,475	20,202	20,963	21,844	22,584	23,317	24,107	24,674

Note: Columns (1)–(9) report OLS estimates of the year 1967 on maternal college education using the 2005–2011 Osaka Preference Parameter Study. Each column reports the estimate using a different birth year range. Year 1967 is a binary variable that equals 1 if an individual is born in 1967, and 0 otherwise. The standard errors are clustered at individual levels. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.0$

Appendix Table 3.2. Adding an interaction term between Year 1967 and gender

	(1) Ever Smoker	(2) Current Smoker	(3) Ever Drinker	(4) Current Drinker	(5) Good Sleep	(6) Stomach Cancer Screening	(7) Lung Cancer Screening	(8) Ovarian Cancer Screening	(9) Breast Cancer Screening	(10) Colon Cancer Screening
Years of College	-0.147**	-0.113*	-0.192**	-0.183**	-0.030	0.002	0.064	0.009	0.358**	0.234**
	(0.072)	(0.067)	(0.083)	(0.079)	(0.071)	(0.075)	(0.071)	(0.141)	(0.156)	(0.092)
First-stage F-statistics	134.884	156.970	127.913	119.848	114.605	102.601	115.156	140.257	138.035	105.613
Observations	460888	434013	461631	454185	460917	445306	443738	227297	227337	442666

Note: Columns (1)–(9) report OLS estimates of the year 1967 on maternal college education using the 2005–2011 Osaka Preference Parameter Study. Each column reports the estimate using a different birth year range. Year 1967 is a binary variable that equals 1 if an individual is born in 1967, and 0 otherwise. The standard errors are clustered at individual levels. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 3.3. Effect of Years of College Education on Health Behavior by Gender

	(1) Ever Smoker	(2) Current Smoker	(3) Ever Drinker	(4) Current Drinker	(5) Good Sleep	(6) Stomach Cancer Screening	(7) Lung Cancer Screening	(8) Ovarian Cancer Screening	(9) Breast Cancer Screening	(10) Colon Cancer Screening
Panel A: Men										
Years of College	−0.106	−0.065	−0.049	−0.055	−0.120	−0.149	−0.026	n/a	n/a	0.078
	(0.126)	(0.123)	(0.069)	(0.061)	(0.114)	(0.133)	(0.137)	n/a	n/a	(0.118)
First-stage F-statistics	62.985	69.006	58.290	67.909	55.488	42.721	49.067	n/a	n/a	45.350
Observations	224,650	203,768	225,166	220,696	224,751	218,147	217,014	n/a	n/a	215,551
Panel B: Women										
Years of College	−0.201**	−0.171**	−0.371**	−0.365**	0.082	0.150*	0.156*	0.009	0.358**	0.400***
	(0.090)	(0.082)	(0.160)	(0.163)	(0.168)	(0.082)	(0.083)	(0.141)	(0.156)	(0.149)
First-stage F-statistics	125.790	144.274	115.362	108.100	108.214	153.648	168.666	140.257	138.035	139.013
Observations	236,238	230,245	236,465	233,489	236,166	227,159	226,724	227,297	227,337	227,115

Note: Panel A reports the second-stage 2SLS estimates for men, and Panel A reports the second-stage 2SLS estimates for women. Each column reports a different dependent variable. Years of college education is a continuous variable that equals 0 if respondents graduated with an education less than high school; 2 if vocational school or short-term college; 4 if a bachelor's degree; and 9 if a master's degree or above. Socioeconomic controls include marital status, household structure, whether children lived together, and type of house owned. Prefectural FE is a vector of prefecture dummies where surveys were sampled and survey-year FE is a vector of year dummies when surveys were sampled. Birth-month FE is a vector of month binary variables corresponding to January to December. Birth-year FE is a vector of year binary variables corresponding to the year an individual was born from 1947 to 1980, excluding 1966. The standard errors are clustered at birth month and year levels. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 3.4. Alternative Year Ranges: Expanding birth year range

	(1) Ever Smoker	(2) Current Smoker	(3) Ever Drinker	(4) Current Drinker	(5) Good Sleep	(6) Stomach Cancer Screening	(7) Lung Cancer Screening	(8) Ovarian Cancer Screening	(9) Breast Cancer Screening	(10) Colon Cancer Screening
Panel A: 1946–1981										
Years of College	−0.150**	−0.116*	−0.187**	−0.178**	−0.031	−0.008	0.062	0.032	0.367**	0.227**
	(0.074)	(0.069)	(0.084)	(0.080)	(0.071)	(0.076)	(0.070)	(0.144)	(0.155)	(0.092)
Observations	482,686	454,620	483,453	475,552	482,698	466,133	464,543	238,011	237,998	463,390
Panel B: 1945–1982										
Years of College	−0.153**	−0.117*	−0.194**	−0.184**	−0.032	−0.007	0.059	0.030	0.372**	0.234**
	(0.075)	(0.069)	(0.086)	(0.081)	(0.074)	(0.077)	(0.072)	(0.142)	(0.155)	(0.096)
Observations	502,937	473,713	503,734	495,389	502,902	485,509	483,886	248,006	247,961	482,664
Panel C: 1944–1983										
Years of College	−0.152**	−0.117	−0.188**	−0.178**	−0.034	−0.004	0.067	0.020	0.363**	0.237**
	(0.076)	(0.071)	(0.083)	(0.079)	(0.072)	(0.077)	(0.071)	(0.137)	(0.148)	(0.094)
Observations	525,560	495,128	526,379	517,519	525,510	507,150	505,442	259,343	259,260	504,186
Panel D: 1943–1984										
Years of College	−0.152*	−0.117	−0.186**	−0.176**	−0.037	−0.006	0.069	0.030	0.372**	0.238**
	(0.079)	(0.072)	(0.085)	(0.080)	(0.075)	(0.078)	(0.073)	(0.137)	(0.148)	(0.094)
Observations	548,020	516,407	548,882	539,460	547,898	528,588	526,809	270,379	270,285	525,517
Panel E: 1942–1985										

Years of College	−0.153*	−0.119	−0.187**	−0.176**	−0.036	−0.002	0.078	0.029	0.385**	0.244**
	(0.082)	(0.072)	(0.086)	(0.081)	(0.076)	(0.077)	(0.073)	(0.138)	(0.151)	(0.096)
Observations	569,487	536,848	570,389	560,455	569,298	549,067	547,275	281,040	280,945	545,906
Panel F: 1941–1986										
Years of College										
	−0.157*	−0.121	−0.188**	v0.177**	−0.041	−0.001	0.081	0.036	0.401***	0.256***
	(0.084)	(0.074)	(0.090)	(0.086)	(0.078)	(0.079)	(0.075)	(0.140)	(0.154)	(0.099)
Observations										
	590,849	557,252	591,814	581,333	590,638	569,393	567,560	291,531	291,378	566,153
Panel G: 1940–1987										
Years of College	−0.158*	−0.122*	−0.184**	−0.173**	−0.046	0.002	0.085	0.029	0.410***	0.256***
	(0.082)	(0.071)	(0.090)	(0.085)	(0.079)	(0.079)	(0.075)	(0.141)	(0.151)	(0.097)
Observations	610,817	576,413	611,825	600,887	610,560	588,465	586,576	301,397	301,226	585,131
Panel H: 1939–1988										
Years of College	−0.164*	−0.126*	−0.182*	−0.170*	−0.051	0.006	0.092	0.030	0.409***	0.261***
	(0.085)	(0.074)	(0.094)	(0.088)	(0.081)	(0.080)	(0.075)	(0.141)	(0.146)	(0.096)
Observations	629,250	594,098	630,291	618,911	628,935	605,988	604,047	310,397	310,209	602,587
Panel I: 1938–1990										
Years of College	−0.169*	−0.131*	−0.186*	−0.173*	−0.049	0.012	0.096	0.034	0.416***	0.275***
	(0.090)	(0.076)	(0.097)	(0.090)	(0.084)	(0.082)	(0.075)	(0.137)	(0.146)	(0.100)
Observations	646,530	610,807	647,628	635,849	646,196	622,407	620,435	318,992	318,805	618,938

Note: All columns report 2SLS estimates. Columns (1) and (2) report the estimates for ever and current smoker; columns (3) and (4) report the estimates for ever and current drinker; column (5) reports the estimates for good sleep; and columns (6)–(10) report the estimates for five types of cancer screening. Panel A reports the first-stage estimates and panel B reports the second-stage estimates. Years of college education is a

continuous variable that equals 0 if respondents graduate with an education level less than high school; 2 if vocational school or short-term college; 4 if a bachelor's degree; and 9 if a master's degree or above. Socioeconomic controls include sex, marital status, household generation, whether children lived together, and type of house owned. Prefectural FE is a vector of prefecture dummies where surveys were sampled and survey-year FE is a vector of year dummies when surveys were sampled. Birth-month FE is a vector of month binary variables corresponding to January to December. Birth-year FE is a vector of year binary variables corresponding to the year an individual was born from 1963 to 1971, excluding 1966 and 1970. The standard errors are clustered at birth year and month levels. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 3.5. Alternative Year Ranges: Restricting birth year range

	(1) Ever Smoker	(2) Current Smoker	(3) Ever Drinker	(4) Current Drinker	(5) Good Sleep	(6) Stomach Cancer Screening	(7) Lung Cancer Screening	(8) Ovarian Cancer Screening	(9) Breast Cancer Screening	(10) Colon Cancer Screening
Panel A: 1954–1973										
Years of College	-0.170***	-0.139**	-0.190**	-0.178**	-0.003	0.027	0.067	0.005	0.322**	0.259***
	(0.066)	(0.061)	(0.090)	(0.083)	(0.074)	(0.078)	(0.068)	(0.140)	(0.163)	(0.090)
Observations	256731	241703	257110	253481	256814	248906	248057	127462	127557	247428
Panel B: 1953–1974										
Years of College	-0.160**	-0.127**	-0.182**	-0.171**	-0.010	0.020	0.067	0.023	0.350**	0.258***
	(0.068)	(0.062)	(0.081)	(0.075)	(0.078)	(0.076)	(0.065)	(0.142)	(0.175)	(0.085)
Observations	285533	268790	285960	281865	285633	276746	275771	141607	141694	275046
Panel C: 1952–1975										
Years of College	-0.145**	-0.112*	-0.175**	-0.165**	-0.011	0.030	0.073	0.007	0.307*	0.236***
	(0.066)	(0.059)	(0.072)	(0.067)	(0.076)	(0.076)	(0.067)	(0.135)	(0.165)	(0.081)
Observations	314210	295796	314681	310087	314298	304424	303346	155783	155850	302551
Panel D: 1951–1976										
Years of College	-0.149**	-0.113*	-0.181***	-0.170***	-0.015	0.028	0.066	0.030	0.316**	0.232***
	(0.069)	(0.062)	(0.069)	(0.064)	(0.077)	(0.075)	(0.064)	(0.141)	(0.161)	(0.086)
Observations	343090	322996	343630	338468	343185	332265	331035	169899	169957	330263
Panel E: 1950–1977										

Years of College	-0.145**	-0.110*	-0.186***	-0.175***	-0.021	0.030	0.067	0.006	0.338**	0.240***
	(0.069)	(0.064)	(0.070)	(0.066)	(0.076)	(0.075)	(0.067)	(0.145)	(0.164)	(0.090)
Observations	372176	350367	372758	367074	372224	360230	358909	184054	184114	358082
Panel F: 1949–1978										
Years of College										
	-0.145**	-0.111*	-0.196**	-0.184**	-0.029	0.011	0.064	-0.001	0.341**	0.238***
	(0.071)	(0.065)	(0.077)	(0.073)	(0.075)	(0.077)	(0.069)	(0.138)	(0.157)	(0.092)
Observations	402765	379232	403408	397183	402843	389615	388233	198925	198998	387294
Panel G:1948–1979										
Years of College	-0.145**	-0.110	-0.193**	-0.182**	-0.023	0.005	0.059	0.004	0.355**	0.238**
	(0.072)	(0.067)	(0.082)	(0.077)	(0.074)	(0.076)	(0.073)	(0.142)	(0.156)	(0.095)
Observations	432562	407353	433263	426412	432617	418172	416678	213474	213517	415678

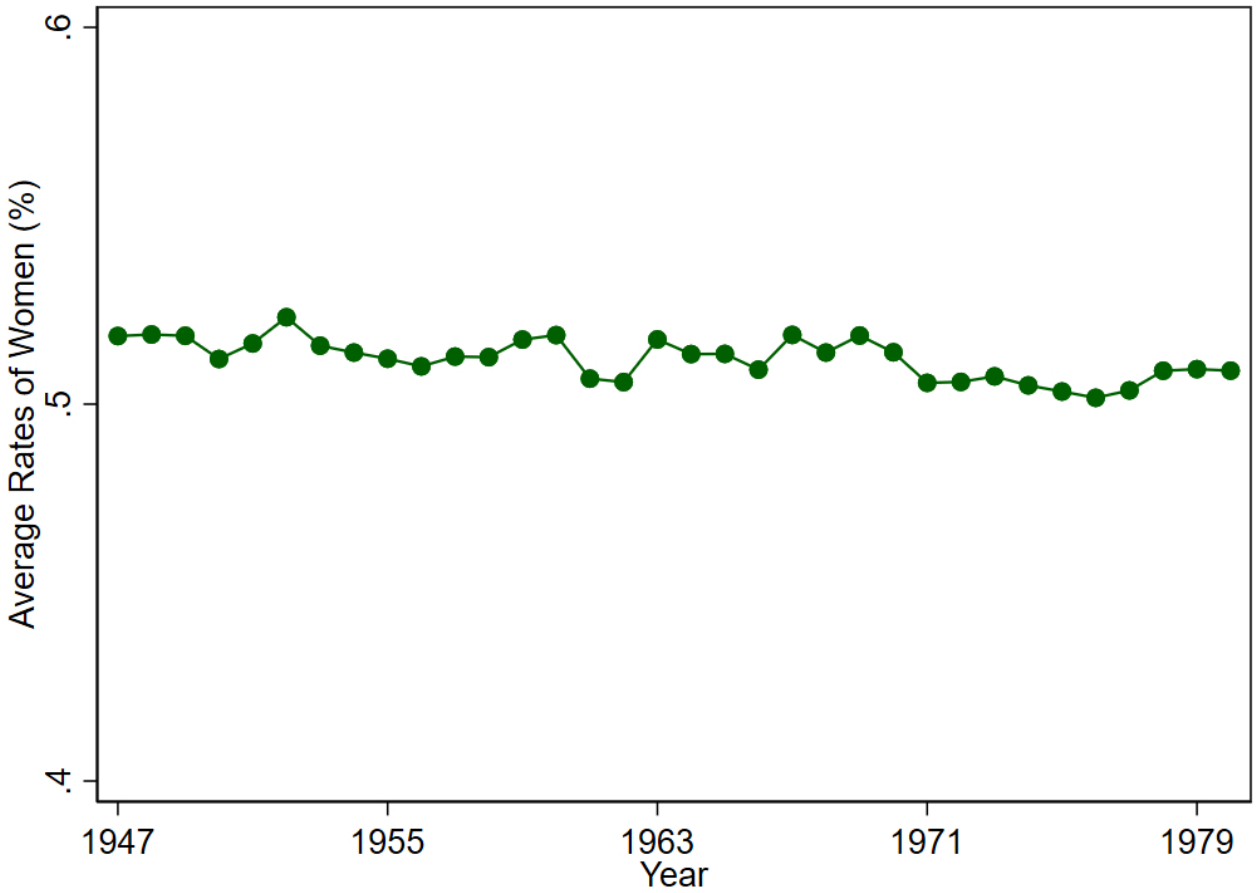
Note: All columns report 2SLS estimates. Columns (1) and (2) report the estimates for ever and current smoker; columns (3) and (4) report the estimates for ever and current drinker; column (5) reports the estimates for good sleep; and columns (6)–(10) report the estimates for five types of cancer screening. Panel A reports the first-stage estimates and panel B reports the second-stage estimates. Years of college education is a continuous variable that equals 0 if respondents graduate with an education level less than high school; 2 if vocational school or short-term college; 4 if a bachelor's degree; and 9 if a master's degree or above. Socioeconomic controls include sex, marital status, household generation, whether children lived together, and type of house owned. Prefectural FE is a vector of prefecture dummies where surveys were sampled and survey-year FE is a vector of year dummies when surveys were sampled. Birth-month FE is a vector of month binary variables corresponding to January to December. Birth-year FE is a vector of year binary variables corresponding to the year an individual was born from 1963 to 1971, excluding 1966 and 1970. The standard errors are clustered at birth year and month levels. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 3.6. Testing Sensitivity of Estimates to Unobservable Variables Using Observable Variables

	(1) Ever Smoker	(2) Current Smoker	(3) Ever Drinker	(4) Current Drinker	(5) Good Sleep	(6) Stomach Cancer Screening	(7) Lung Cancer Screening	(8) Ovarian Cancer Screening	(9) Breast Cancer Screening	(10) Colon Cancer Screening
Panel A: Exclude marital status variables										
Years of College	−0.147**	−0.113*	−0.193**	−0.183**	−0.030	−0.001	0.063	0.001	0.354**	0.232**
	(0.073)	(0.064)	(0.082)	(0.079)	(0.073)	(0.077)	(0.073)	(0.141)	(0.156)	(0.095)
Panel B: Exclude household type variables										
Years of College	−0.148**	−0.114*	−0.192**	−0.184**	−0.031	0.002	0.066	0.009	0.356**	0.235**
	(0.075)	(0.069)	(0.080)	(0.077)	(0.070)	(0.075)	(0.071)	(0.142)	(0.158)	(0.093)
Panel C: Exclude children status variables										
Years of College	−0.148**	−0.114*	−0.190**	−0.181**	−0.031	0.004	0.067	0.009	0.354**	0.233**
	(0.071)	(0.067)	(0.081)	(0.077)	(0.071)	(0.072)	(0.070)	(0.141)	(0.158)	(0.091)
Panel D: Exclude all socioeconomic variables										
Years of College	−0.149**	−0.117*	−0.183**	−0.175**	−0.037	0.009	0.072	0.010	0.345**	0.236**
	(0.070)	(0.061)	(0.078)	(0.076)	(0.071)	(0.072)	(0.070)	(0.139)	(0.155)	(0.097)
Observations	460,888	434,013	461,631	454,185	460,917	445,306	443,738	227,297	227,337	442,666

Note: All columns report 2SLS estimates. Columns (1) and (2) report the estimates for ever and current smoker; columns (3) and (4) report the estimates for ever and current drinker; column (5) reports the estimates for good sleep; and columns (6)–(10) report the estimates for five types of cancer screening. Each panel reports the estimates excluding respective variables from the estimation. Years of college education is a continuous variable that equals 0 if respondents graduated with an education level less than high school; 2 if vocational school or short-term college; 4 if a bachelor's degree; and 9 if a master's degree or above. Socioeconomic controls include sex, marital status, household generation, whether children lived together, and type of house owned. Prefectural FE is a vector of prefecture dummies where surveys were sampled and survey-year FE is a vector of year dummies when surveys were sampled. Birth-month FE is a vector of month binary variables corresponding to January to December. Birth-year FE is a vector of year binary variables corresponding to the year an individual was born from 1963 to 1971,

excluding 1966 and 1970. The standard errors are clustered at birth year and month levels. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$



Appendix Figure 3.1. Male-to-female ratio between 1938 and 1989.

Source: 2013 and 2016 Comprehensive Surveys of Living Conditions ,

Table 1-3. The Methods and/or Location at which Couple Met

Survey name (Surey Year)	Total	Romance Marriage							Match--Making Marriage	Unknown Method /Location
		Work	Friends & Family	School	Street & Travelling	Circle, club, & other	Part-time	Childhood Friend		
8th Survey (1982)	100.0 %	25.3 %	20.5	6.1	8.2	5.8	－	2.2	29.4 %	2.5 %
9th Survey (1987)	100.0	31.5	22.4	7.0	6.3	5.3	－	1.5	23.3	2.7
10th Survey (1992)	100.0	35.0	22.3	7.7	6.2	5.5	4.2	1.8	15.2	2.0
11th Survey (1997)	100.0	33.5	27.0	10.4	5.2	4.8	4.7	1.5	9.7	3.1
12th Survey (2002)	100.0	32.9	29.2	9.3	5.4	5.1	4.8	1.1	6.9	5.2
13th Survey (2005)	100.0	29.9	30.9	11.1	4.5	5.2	4.3	1.0	6.4	6.8
14th Survey (2010)	100.0	29.3	29.7	11.9	5.1	5.5	4.2	2.4	5.2	6.8

Note: This is based on Reports on 14th Japanese National Fertility Survey in 2010. The targets of the surveys are those who married in past 5 years. "Matchmaking Marriage" is for those who married through matchmaking method or through Japanese Marriage Counselling Office. For 8th and 9th Surveys, no data were collected for "part-time." The observation for each survey is 1298 for 8th, 1421 for 9th, 1526 for 10th, 1304 for 11th, 1488 for 12th, 1076 for 13th, and 1136 for 14th.

Appendix Figure 3.2. Translated version of Table 1-3 from the Report on 14th Japanese National Fertility Survey in 2010.

Source: Report on 14th Japanese National Fertility Survey in 2010.

Conclusion

This thesis is comprised of three chapters that address important issues regarding health insurance and education. Health insurance and education are some of the most important determinants of health. The first chapter explores how insurance policy which offering the same price between two types of drug, oral and intravenous anticancer drugs, can reduce cancer mortality rates of the patients. However, it is possible that insurance coverage of drug can also have unintended consequences, such as deteriorating health. The second chapter examines the unintended consequence of coverage of smoking-cessation on tobacco use in Canada. The insurance coverage of smoking-cessation aids could potentially make people more vulnerable to abusing tobacco due to protection from future costs as a result of the coverage. Finally, education may make people act healthier. The third chapter investigates the casual relationship between college education and health behavior in Japan using a mismatch in superstition.

The first chapter investigates the impacts of anticancer drug parity laws on mortality rates in the United States using a difference-in-differences approach. The anticancer drug parity laws are laws that mandate all insured to cover the oral anticancer drugs at the same price as intravenous oral anticancer drugs leading to better access to more efficient therapies and better drug adherence. The results show that the anticancer drug parity laws *only* reduce the mortality rate for head/neck malignant cancers in the implemented states. However, an improvement in coverage of specific drugs could also lead to an expansion in coverage rates. We further explore this point and find that there is little to no expansion in private or public insurance after the coverage of oral anticancer drugs. The findings imply that providing better access to drugs can be an effective tool for the prevention of premature mortality.

In the second chapter, we explore the effect of smoking-cessation aids coverage on tobacco use outcomes in Canada. Based on the results, we do not find significant benefit from the coverage of smoking-cessation aids. Instead, the coverage of such drugs lead to unintended consequence. In other words, we find that SCA coverage increases cigarette and cigarillo use. This implies that SCA coverage by itself does not lead to significant benefit on population, instead lead to deterioration in health. Furthermore, the effect of the coverage on tobacco use behavior is stronger in those with at least a college education and in provinces with pre-existing coverage of other types of smoking-cessation aids. This suggests that knowledge is a significant mediator in the relationship between smoking-cessation aids coverage and tobacco use behavior. Our findings show that the coverage of such drugs alone is not sufficient to induce tobacco cessation among smokers and additional incentives (or policies) are needed to motivate these smokers mitigating unintended abuse of such drug coverage.

For the final chapter, we investigate the effect of college education on health behavior using a mismatch between Japanese school year and superstition as an instrument for college education. The superstition is the Firehorse Year in 1966. To instrument college education, we leverage the fact that school year in Japan begins at April each year and the Firehore ends at December of 1966. The mismatch is generated because the school year does not precisely start at the same time as the Firehose ends. Instead, people who are not born in 1966, the Firehorse, are being sorted together with the Firehorse people in the same class. This leads to overall better education attainment for those born between January and March of 1967. Using this mismatch as an instrument, we find that longer years of college education benefit health behavior by reducing smoking and alcohol use and increasing cancer screening use. Overall, the results imply that any education policies that can provide more education to people will have spillover effect on health and health behavior. Moreover, we find that the causal

relationship is driven by women, not men. This suggests that college education may have mediated through health knowledge as women may gain access to knowledge that their behavior can potentially affect the health of their offspring leading to them behave healthier. In sum, our results show that policymakers considering adopting educational policies need to also weigh the potential benefits derived from health effect of higher education.