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Dissertation Author: Leong Mun Yee, Elizabeth (Christ's College, University of Cambridge, m. 2019)

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An empirical study on the effects of a health insurance mandate

Abstract: This dissertation studies the effects of the repeal of the individual mandate provision of the USA's Affordable Care Act. It makes two key contributions. Firstly, using a difference-in-differences model, it shows that the mandate's repeal reduced the probability of an individual being insured by 0.9 percentage points, demonstrating the effectiveness of insurance mandate policies. Secondly, using a triple-difference model to explore the differential responses of healthy and non-healthy subgroups, it finds evidence of adverse selection in the health insurance market.

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1 Introduction

Government intervention in health insurance markets is common, with policies ranging from premium subsidies to universal coverage (The Economist, 2018). These are typically implemented on the basis of efficiency and equity. One common source of inefficiency in the health insurance space is market failure arising from adverse selection. This has a further equity implication, as adverse selection is associated with rising premiums that can make insurance prohibitively expensive to the poor. Unsurprisingly, this causes the welfare losses arising from adverse selection to be large in practice (Cutler and Zeckhauser, 2000).

Adverse selection models predict that healthier individuals are less likely to be insured. However, such models do not account for income differences. In reality, poor health is correlated with low income and a lower probability of being insured (Chokshi and Khullar, 2018). Consequently, we might instead observe "favourable selection", with the healthy being more likely to be insured. The question of which of these mechanisms dominates is important in determining which groups are more responsive to intervention in the health insurance market, thereby helping to clarify the welfare effects of such interventions.

To this end, I analyse the repeal of the USA Affordable Care Act's (ACA's) Individual Shared Responsibility Provision (the "individual mandate" or "mandate"). Implemented in January 2014, the individual mandate subjected individuals without health insurance coverage to a federal tax penalty, essentially making insurance compulsory. The mandate was repealed at the federal level from January 2019 following intense controversy (Kliff, 2015), although Washington D.C. and New Jersey (N.J.) immediately implemented similar state-level mandates (Government of the District of Columbia, 2020; New Jersey Legislature, 2018).

Exploiting this natural experiment, I investigate the following research questions with a difference-in-differences approach:

1. **The effectiveness question**: Did the repeal of the ACA's individual mandate negatively affect health insurance rates?

2. **The adverse selection question**: Are people with lower health risks more likely to be uninsured upon the mandate's repeal?

These questions are hereafter referred to by their "short names" in bold. Investigating the effectiveness question will show if the policy achieved its aims and will more generally show the usefulness of insurance mandates. Investigating the adverse selection question will clarify which aforementioned mechanism of uninsurance dominates.

I use annual data from the USA's Current Population Survey (CPS) to construct a panel sample and a repeated cross-sectional sample, to allow for a comparison of results across the two. The latter gives a bigger sample size while the former gives more robust results by avoiding issues with the sample composition changing over time. I find that the repeal reduced the probability of an individual being insured by 0.9 percentage points, demonstrating the effectiveness of insurance mandate policies. I also find that healthier individuals were more likely to lose insurance, providing support for the adverse selection mechanism.

The paper proceeds as follows. Section 2 reviews existing literature. Section 3 introduces the key dataset and variables, and describes the samples I construct. Section 4 conducts a preliminary data analysis. Sections 5 and 6 present the strategies used to investigate the two key research questions, and the corresponding results. Section 7 deals with robustness checks. Section 8 concludes.

2 Background and Literature Review

2.1 Mechanisms of Uninsurance

In examining health insurance patterns, it is crucial to understand the role of insurance. In the standard model (Gruber, 2008), individuals are risk-averse, with concave utility functions. They face a non-zero probability of an income-reducing catastrophe in the future (for example in the form of high medical costs). Due to their risk-aversion, they wish to smooth their consumption across the two possible states ("catastrophe" and "no catastrophe"). Hence individuals purchase insurance, paying a premium to insurance companies, which in return offer payouts in the event of the catastrophe. Individuals will buy full insurance if premiums are actuarially fair: that is, if premiums are equal to the expected compensation.

One major theory of uninsurance is that of adverse selection, as per Rothschild and Stiglitz (1976). In this model, individuals' health risks are private information and markets are competitive. Some individuals have higher health risks than others. There exists no pooling equilibrium in which all individuals are fully insured, as insurers can offer more limited coverage (that is not desired by higher-risk consumers) at a lower price to lower-risk consumers. This results in limited insurance coverage for lower-risk consumers, and lower-risk consumers being less likely to purchase insurance. A vicious cycle can arise: insurers face higher expected payouts due to a higher-risk consumer pool, prompting a rise in premiums. This further discourages the healthy from purchasing insurance, resulting in an even higher-risk consumer pool, and eventually an upward premium "death spiral" (Gruber, 2008) .

The adverse selection hypothesis unambiguously predicts that healthier people are more likely to lose their insurance after the mandate's repeal. However, the adverse selection model does not account for income differences. In practice, liquidity constraints (among other issues) might make the poor less likely to insure (Gruber, 2008). Additionally, being poor is associated with poorer health outcomes (Chokshi and Khullar, 2018). Under this mechanism, we could instead observe favourable selection, with healthier people being less likely to lose their insurance after the mandate's repeal.

It is not immediately clear which of these mechanisms will dominate. On the one hand, many studies find evidence of adverse selection in the health insurance space: Cutler and Zeckhauser (2000) summarise thirty studies investigating adverse selection, of which twenty-seven find evidence in its favour. On the other hand, in the USA, the poor and unemployed face significant barriers in obtaining insurance due to affordability issues, particularly because health insurance has traditionally been employer-based (Frank, 2013). The strong correlation between insurance status and income in the USA might cause the other mechanism to dominate instead.

This dissertation will show which of these mechanisms is stronger. This helps clarify the welfare effects of the repeal: for example, if unhealthier people (who benefit more from insurance) are more likely to lose insurance, there will be bigger short-term welfare losses compared to the case where healthier people were more likely to lose insurance. More broadly, uncovering the dominant mechanism of uninsurance could help in identifying the policy tools best suited to addressing the issue.

This paper also adds to the body of literature that investigates adverse selection in health insurance markets, allowing conclusions to be made about the presence of this form of market failure.

2.2 Literature on the Individual Mandate

Existing literature on the effectiveness of the individual mandate largely focuses on the consequences of its *implementation* in 2014. As many other ACA provisions came into effect the same year (such as guaranteed issue requirements and premium subsidies), researchers have adopted various strategies to isolate the effect of the mandate itself.

Jung and Tran (2016) designed a stochastic general equilibrium model with endogenous health capital accumulation, and calibrated the model with data on health spending and insurance in the USA. They found that virtually universal coverage was attainable with a mandate penalty of around 10%.

Individuals above 400% of the federal poverty level were ineligible for ACA subsidies.

Jacobs (2018) used this group to isolate the effect of the mandate's implementation, and found that the insurance rate in his study population was far higher after 2013. Lurie et al. (2021) estimated the effect of the mandate on insurance coverage using regression discontinuity and regression kink designs with 2015-16 tax returns data. Consistent with adverse selection, they found that those with indications of poor health responded less to the mandate penalty.

Limited research examines the impact of the mandate's *repeal*. Kamal et al. (2018) examine the 2019 rate filings of insurers and found that premiums for all ACA-compliant plans would be 6% higher on average compared to the case where the mandate was retained. This is indicative of the beginnings of a premium "death spiral", consistent with the adverse selection hypothesis. Fung et al. (2019) surveyed Californian residents on how they would respond to the repeal, but this is neither empirically rigorous nor necessarily indicative of actual behaviour.

At this juncture, it is to be noted that after this topic was chosen, I found the abstract of an undergraduate thesis by a student from another university with a similar focus (Zhang, 2021). The full paper was not available then, and I have not checked if it has since been made available.

This dissertation aims to extend the literature on the mandate itself by directly studying the effects of the individual mandate's repeal on insurance-consuming behaviour. As the other ACA provisions remain post-2019, I avoid the issue of confounding the effects of another provision with the effect of the mandate, which was present in earlier research surrounding the mandate's implementation. I examine various categories of health insurance coverage, and the responses of those who are policy owners versus policy dependents, to determine who might be vulnerable to insurance loss.

3 Data Sources and Variables

I use individual-level microdata from the Annual Social and Economic Supplement (ASEC) of the USA's Current Population Survey (CPS), from a database maintained by Flood et al. (2021). From this, I construct the following samples:

- **The cross-sectional sample**, comprising repeated cross-sectional data from the 2014-2021 surveys, as the mandate came into effect in 2014.
- **The panel sample**, comprising 2019 CPS ASEC data. I noticed that the 2019 survey recorded an individual's health insurance status in both 2018 and 2019, across the policy change investigated in this dissertation. I exploit this unique opportunity to create a panel sample on which fixed-effects estimations can be conducted, to obtain better identification than with the cross-sectional sample.

As each sample has its benefits and downsides (discussed in greater detail in Section 4.3), I chose to work with both at first.

Both samples are limited to those aged 15 to 64, inclusive. There is limited data availability for under-15s in the source dataset. Over-64s are excluded due to their eligibility for Medicare.

3.1 Main Approach

I investigate the effectiveness question with a difference-in-differences (DID) framework that isolates the effect of the mandate's repeal. The adverse selection question is investigated with a triple-difference (DDD) framework, identifying the differential impact of the mandate's repeal on groups with different health risk levels.

Knowledge of this main approach is required to describe and discuss key aspects of the data. Sections 5 and 6 establish the regression specifications in detail before displaying and discussing their respective results.

The "treatment" is defined as the repeal of the mandate. Hence N.J. and D.C. form the control group.

However, not all the other states form the treatment group. I omit states implementing individual mandates after 2019, as the anticipation of such mandates may influence insurance-purchasing behaviour. States that did not expand Medicaid to those under 138% of the federal poverty level in 2014 are also omitted, as their residents may be exempt from the mandate. This leaves 19 states in the treatment group (Kaiser Family Foundation, 2022): Arizona, Arkansas, Colorado, Connecticut, Delaware, Hawaii, Illinois, Iowa, Kentucky, Maryland, Minnesota, Nevada, New Mexico, New York, North Dakota, Ohio, Oregon, Washington, and West Virginia.

3.2 Covariates

I include covariates in the DID regressions on the cross-sectional sample. This is to attempt to control for the possibility that the populations sampled differ systematically between the "before" and "after" periods, in a way that is correlated with insurance take-up or loss (Wooldridge, 2009). Individual-level covariates can also increase the precision of the DID estimates (Angrist and Pischke, 2009). Existing literature on the impact of the individual mandate and other ACA provisions were consulted to identify the covariates used in this dissertation. This includes Buchmueller et al. (2011)'s work on Hawaii's employer health insurance mandates; Antwi et al. (2012)'s and Depew and Bailey (2015)'s work on the ACA's dependent coverage mandate; and the work of Lurie et al. (2021) and Fung et al. (2019) on the individual mandate.

The final list of covariates are: age, sex, high school completion, veteran status, marital status, race, nativity, Medicaid eligibility (based on an individual's poverty status, with those below 138% of the federal poverty line being eligible for Medicaid), disability status, and employment status. Disability status indicates if an individual has any physical or cognitive difficulty. Race is recoded into five categories: Black, Asian or Pacific Islander (PI), Native, White, and "Others".

3.3 Dependent Variables

The dependent variables used are binary indicator variables regarding an individual's health insurance status. These are described in Table 1. Some variables are only available for the panel sample.

Table 1: Description of the dependent variables available in each sample.

Available in	**Dependent variable name**	**Description: Whether an individual...**	**Abbreviation**
Both samples	Any coverage	Has any insurance	anycov
	Private ownership	Owns a private insurance policy	private_own
	Employer-based ownership	Owns an employer-based insurance policy	emp_own
	Direct-purchase ownership	Owns a direct-purchase insurance policy	direct_own
The panel sample only	Private coverage	Has private insurance coverage	private_cov
	Employer-based coverage	Has employer-based insurance coverage	emp_cov
	Direct-purchase coverage	Has direct-purchase insurance coverage	direct_cov

Note that employer-based and direct-purchase insurance are subsets of private insurance, and that private insurance is of course a subset of having any insurance at all. The difference between private and "any" insurance would be public insurance. Individuals typically have more choice over purchasing private insurance, as public insurance in the USA is largely provided by programmes that have specific eligibility requirements (Keisler-Starkey and Bunch, 2021). It is thus expected that if the mandate had a negative effect on insurance rates in the USA, private coverage should drive the results. For this reason, the private subsets of health insurance are examined in this dissertation.

The ownership variables are a subset of the coverage variables: for instance, someone who owns a private insurance policy will have private insurance coverage.

If the DID estimates of the percentage point decreases in coverage due to the repeal are similar across the ownership and coverage variables, then it is likely that decreases in private insurance coverage are being driven solely by policy-owners dropping their policies. However, if the DID estimates are larger in magnitude for the coverage variables than the ownership variables, then decreases in private insurance coverage are likely also driven by policy-owners taking dependents off their policies. Examining both ownership and coverage variables provides additional insights into patterns of insurance-dropping among different groups.

4 Preliminary Data Analysis

4.1 Sample Weights

The source dataset (the ASEC) relies on a "a complex stratified sampling scheme". Hence, the regressions are conducted using sample weights provided by the ASEC, which are "based on the inverse probability of [the individual's] selection into the sample" (Flood et al., 2021). This ensures that the results have external validity. Adjustments were also made for non-response bias related to the COVID-19 pandemic.

4.2 Defining "Before" and "After"

In the cross-sectional sample, the "any coverage" variable in the annual survey captures if an individual had any insurance in that survey year. However, private_own, emp_own, and direct_own capture if an individual owned these policies in the *previous year*. The mandate was repealed in January 2019 and the survey is conducted in March each year. Hence, for the anycov variable, 2014-2018 is "before" the mandate repeal and 2019-2021 is "after". For the private_own, emp_own and direct_own variables, "before" and "after" are 2015-2019 and 2020-2021 respectively.

4.3 Comparing the Panel and Cross-sectional Samples

One obvious advantage of the cross-sectional sample over the panel sample is its larger total sample size. It stands at 339,314 (for the survey years 2014-2021) or 288,734 (for 2015-2021), compared to 41,200 for the panel sample. However, there are numerous advantages that the panel sample has over the cross-sectional one.

Obtaining unbiased DID estimates of the treatment effect requires the Stable Unit Treatment Value Assumption (SUTVA) (Gertler et al., 2016). This involves the composition of the treatment and control groups being stable across the policy change.

The panel sample consists of the same individuals across the policy change; it avoids violation of the SUTVA by construction. It is however necessary to test for potential violations of the SUTVA in the cross-sectional sample. I hence conduct a difference-in-proportions test for the binary covariates of the sample. Table 2 presents these results.

Identification is threatened if the composition of each group changes in different ways over time (Stuart et al., 2014). Table 2 shows that this is the case. For example, for the treatment group, from 2015-2019 to 2020-2021, there is a change in the percentage of employed individuals significant at the 1% significance level (column 4). Conversely, there is no significant change for the control group (column 3).

Table 2: Results of the difference-in-proportions test for the control and treatment groups across the policy change, **for the cross-sectional sample**.

	Change from 2014-18 to 2019-21 (Relevant to anycov)		Change from 2015-19 to 2020-21 (Relevant to private_own, emp_own, direct_own)	
	(1) Control	(2) Treatment	(3) Control	(4) Treatment
Female	.0012	-.00017	-.0034	-.00010
	(0.16)	(-0.07)	(-0.38)	(-0.04)
High school	.019	0.0081	.015	0.0090
completion	(4.00)***	(5.36)***	(2.67)**	(5.12)***
Veteran	-.00021	-.0037	.00091	-.0034
	(-0.10)	(-4.14)***	(0.35)	(-3.24)***
Married	-.012	-.00039	-.0071	-.0033
	(-1.58)	(-0.17)	(-0.79)	(-1.26)
Born overseas	.0010	.0052	-.0033	.0037
	(0.15)	(3.13)**	(-0.41)	(1.88)
Medicaid eligible	-0.041	-0.029	-0.037	-0.022
	(-8.41)***	(-17.81)***	(-6.83)***	(-11.55)***
Disabled	-.0061	-.0018	-0.0093	-.0023
	(-1.79)+	(-1.49)	(-2.38)	(-1.63)
Employed	0.012	.0068	.0068	-.0094
	(1.71)+	(3.27)***	(0.81)	(-3.82)***
N	33684	305630	29162	259572

t statistics in parentheses
+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.01$

As explained in Section 3.2, the covariates listed in Table 2 are controlled for in regressions involving the cross-sectional sample, in an attempt to address the SUTVA. However, these differential changes in the compositions of the control and treatment groups might still be of concern if they capture unobserved heterogeneities between the groups. These unobserved heterogeneities might be correlated with both insurance and treatment status, possibly biasing the DID estimator. Hence the SUTVA is possibly violated in the cross-sectional sample.

The panel sample also allows for fixed-effects regressions, which control for unobserved constant individual-level characteristics. This is a more demanding test than what can be conducted on repeated cross-sectional data, and can aid identification. One likely source of bias is that an individual's level of risk-aversion is unobserved. Risk-aversion is correlated to an individual's decision to purchase insurance, as explained in the standard model (Gruber, 2008). Risk-aversion is also likely correlated with health outcomes, as a more risk-averse person may be likelier to adopt health-improving behaviours. If not differenced away, this omitted variable could then threaten identification in a triple-difference regression that uses health status as an explanatory variable for an individual's insurance status.

A final benefit of the panel sample is greater data availability for the dependent variables.

For these reasons, the panel sample is preferred. Due to space constraints, I examine the effectiveness question with both samples for comparison purposes, but investigate the adverse selection question only with the panel sample.

Another crucial assumption for the DID framework, regardless of the use of either sample, is the parallel trends assumption. In Section 7.1 I explain why this assumption is likely to hold and interpret my results in the event that it does not hold.

4.4 Panel Sample Summary Statistics

As the panel sample is the preferred one, key summary statistics for this sample are displayed in Table 3.

Table 3: Summary statistics for the panel sample, including standard errors. Figures are displayed in percentages.

Coverage type	Control	Treatment
Variables available in both samples		
	Control	Treatment
anycov, 2019	92.58	91.0
	(0.54)	(0.18)
anycov, 2018	92.63	91.9
	(0.54)	(0.17)
private_own, 2019	46.0	46.5
	(1.01)	(0.31)
private_own, 2018	46.4	47.6
	(1.01)	(0.31)
emp_own, 2019	40.2	40.3
	(0.99)	(0.30)
emp_own, 2018	40.8	41.4
	(0.99)	(0.30)
direct_own, 2019	5.76	5.55
	(0.46)	(0.14)
direct_own, 2018	5.94	5.95
	(0.46)	(0.15)
Variables only in the panel sample		
private_cov, 2019	78.3	73.2
	(0.84)	(0.27)
private_cov, 2018	78.7	74.3
	(0.84)	(0.27)
emp_cov, 2019	69.4	64.1
	(0.94)	(0.29)
emp_cov, 2018	69.9	65.3
	(0.94)	(0.29)
direct_cov, 2019	8.96	7.89
	(0.58)	(0.17)
direct_cov, 2018	9.19	8.49
	(0.58)	(0.17)
N	4198	37002

	Control	Treatment
Female	51.4	50.4
	(0.010)	(0.0030)
High school Completion	88.1	85.3
	(0.0063)	(0.0021)
Veteran	1.83	4.21
	(0.0028)	(0.0012)
Married	46.7	48.5
	(0.010)	(0.0031)
Born Overseas	29.1	17.6
	(0.0090)	(0.0023)
Medicaid Eligible	11.4	15.3
	(0.0063)	(0.0022)
Disabled	5.77	7.54
	(0.0047)	(0.0016)
Employed	69.	70.5
	(0.0095)	(0.0028)
Age groups		
Adolescent	8.35	8.14
	(0.55)	(0.15)
Adult	51.3	52.3
	(1.01)	(0.31)
Middle age	40.3	39.6
	(0.99)	(0.30)
Race		
Black	18.4	11.7
	(0.81)	(0.21)
Asian or PI	10.1	7.3
	(0.56)	(0.15)
Native	0.129	01.41
	(0.060)	(0.068)
White	70.4	77.2
	(0.92)	(0.26)
Others	0.959	2.47
	(0.18)	(0.091)

Both the control and treatment groups show a decline in all the insurance-related dependent variables from 2018 to 2019. Furthermore, for all of the dependent variables, the percentage point decline is greater for the treatment group than the control group. This initial observation is in line with the hypothesis that the mandate repeal negatively affected insurance rates. Formal DID regressions will investigate this suspicion.

The control and treatment groups differ in composition in some ways. For example, a difference-in-proportions test reveals statistically significant differences in racial composition, with the control group having a significantly higher proportion of Black and Asian/PI individuals, and a lower proportion of Native and White individuals. These differences can be observed from the "Race" section in Table 3.

However, the groups differ in the *levels* of the covariates. This poses no threat to identification as I will conduct individual-level fixed-effects regressions on this sample.

5 The Effectiveness Question

5.1 Empirical Strategy

Following programme evaluation methodology described by Imbens and Wooldridge (2009), I use the following linear probability model (LPM) difference-in-differences (DID) regression to examine the effectiveness question:

$$Y_{it} = \alpha + \beta_1 After_t + \beta_2 Treatment_i + \beta_3 After_t {*} Treatment_i + \gamma \boldsymbol{X_i} + \epsilon_{it} \qquad (1)$$

Notation. Y: Binary variable indicating insurance status. α: an intercept term.
After: Binary variable, set to 0 for before the mandate repeal, 1 for after.
Treatment: Binary variable, set to 0 for individuals from the control states N.J. and D.C., 1 for individuals from the treatment states.
X: Vector of covariates. i, t: Indices for individual and time (year) respectively.

The regression described by Equation 1 is run for the cross-sectional sample.

Exploiting the panel structure of the dependent (insurance) variables, the following fixed-effects regression is run for the panel sample. The covariates are only known for one point in time, 2019, and are hence differenced away:

$$Y_{i,2019} - Y_{i,2018} = \beta_1 + \beta_3 Treatment_i + \epsilon_{i,2019} - \epsilon_{i,2018} \qquad (2)$$

The DID estimator β_3 gives the treatment effect of the individual mandate's repeal, and is thus expected to have a negative sign.

The probability of an individual in a certain state being insured is unlikely to be independent of the probability of another individual in that state being insured, so the regressions are run with standard errors clustered on the individual's state of residence.

5.2 Results and Discussion

The dependent variables that are common to both samples (anycov, prvt_own, emp_own and direct_own) are first analysed. Using these variables, I run the regressions corresponding to equation 2 on the panel sample, with results displayed in Table 4. I run the regressions corresponding to equation 1 on the cross-sectional sample, with results in Table 5.

Although dependent binary variables often invite the use of the probit or logit models, only the results of the LPM regressions are shown and discussed in this paper. This is partly for the ease of interpretation: the use of linear probability models for policy evaluation with a binary dependent variable is well-documented, and coefficients may be interpreted as percentage point (pp) changes in insurance coverage outcomes (Cantor et al., 2012). More importantly, however, this is to avoid problems associated with applying DID frameworks to non-linear models like probit and logit: in such models, the magnitude of the interaction effect does not necessarily equal the marginal effect of the interaction term (Ai and Norton, 2003), raising concerns about how such results should be interpreted. As a robustness check, however, DID logit fixed-effects regressions were also run. Reassuringly, these results gave DID estimators of similar signs and significance as the LPM DID estimators.

As an additional reassurance, for every fixed-effects regression run on the panel sample, an F-test rejects the null hypothesis that the individual-level fixed-effects are the same across all individuals. Hence the fixed-effects are jointly significant, and the fixed-effects specification is justified.

Table 4: Results of the fixed-effects DID LPM regressions on the panel sample, for the dependent variables common to both samples.

	(1) anycov	(2) private_own	(3) emp_own	(4) direct_own
After	-0.00047	-0.0043***	-0.0055***	-0.0018+
	(-1.12)	(-14.54)	(-183.89)	(-2.01)
DID estimator	-0.0091***	-0.0068***	-0.0054***	-0.0023*
	(-4.68)	(-4.97)	(-4.63)	(-2.20)
N	41200	41200	41200	41200

t statistics in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The results of the fixed-effects estimations on the panel sample (Table 4) align with the hypothesis that the individual mandate was effective: all the DID estimators displayed are significantly negative

At the 1% significance level, there is a 0.9pp decrease in the probability of having any insurance upon the mandate's repeal. The figures for the decline in probability of owning a private insurance policy and an employer-based policy are 0.7pp and 0.5pp respectively. At the 5% significance level, the DID estimator on direct_own is also significantly negative: there is a 0.2pp decrease in the probability of owning a direct-purchase policy upon the mandate's repeal.

However, these results are not particularly large in magnitude. The rates of having or owning "any insurance", private coverage, and employer-based coverage all exceed 40% in both the treatment and control groups (Table 3); decreases in these coverage rates of less than 1pp are not necessarily alarming.

Even so, the impact of the mandate's repeal on insurance rates, as studied in this dissertation, are not as significant as the impact of its implementation as studied by other papers. For instance, Jacobs (2018) finds that the mandate's implementation was "associated with 7–12 percentage points of the 13-percentage-point increase in coverage for higher-income adults in the non-group market". Inertia might be an intuitive explanation for this: the mandate might have incentivised many people to take up insurance, but its repeal may not have warranted the effort of dropping existing insurance plans.

Regardless, the DID estimators are all significantly negative; there is still evidence of the negative impact of the individual mandate's repeal on insurance rates.

Table 5: Results of the DID regressions on the cross-sectional sample.

	(1) anycov LPM	(2) anycov Logit	(3) anycov Probit	(4) private_own	(5) emp_own	(6) direct_own
After	0.033***	0.39***	0.21***	-0.0029+	0.015***	-0.0156***
	(22.11)	(141.22)	(129.82)	(-1.99)	(4.22)	(-4.92)
DID estimator	-0.018***	-0.21***	-0.11***	-0.0037	-0.0025	-0.0038
	(-7.89)	(-7.70)	(-7.80)	(-0.92)	(-0.41)	(-1.02)
N	339314	339314	339314	288734	288734	288734

t statistics in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5 summarises the results of the DID regressions on the cross-sectional sample, with only the coefficients on "After" and the DID estimators displayed. For the sake of brevity, the logit, probit and LPM results are presented only for the anycov variables, with only the LPM results displayed for the other variables. In line with the what is expected, DID estimators across all the available variables and specifications (logit, probit and LPM) are negative.

The "any coverage" coefficient is highly significant: upon the mandate's repeal, there is a 1.8pp decrease in the probability of having any insurance at the 1% significance level. However, none of the other DID estimators displayed in Table 5 are significantly different from 0. It is possible that the violation of the SUTVA DID assumption as discussed in section 4.3 is a reason for the difference in results between the cross-sectional and the panel samples. The presence of unobserved individual-level heterogeneities may also have affected these results.

Table 6: Results of the fixed-effects DID LPM regressions on the panel sample, including the dependent variables unique to the panel sample.

	(1) private_own	(2) private_cov	(3) emp_own	(4) emp_cov	(5) direct_own	(6) direct_cov
After	-0.0043***	-0.0033***	-0.0055***	-0.0053***	-0.0018+	-0.0023**
	(-14.54)	(-10.89)	(-183.89)	(-12.80)	(-2.01)	(-2.92)
DID estimator	-0.0068***	-0.0083***	-0.0054***	-0.0074***	-0.0023*	-0.0036**
	(-4.97)	(-5.16)	(-4.63)	(-6.48)	(-2.20)	(-3.23)
N	41200	41200	41200	41200	41200	41200

t statistics in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 6 shows the DID estimators for the dependent variables that are unique to the panel sample: private, employer-based, and direct-purchase *coverage*. The ownership variables are re-displayed for comparison. As with the results for private and employer-based policy ownership, the DID estimators for private and employer-based coverage are also significantly negative. The results for the coverage variables are greater in magnitude: at the 1% significance level, there is a 0.8pp and a 0.7pp decrease in the probability of having private and employer-based coverage respectively. This is compared to 0.7pp and 0.5pp for the respective ownership variables. Direct-purchase coverage also has a DID estimator of a greater magnitude than direct-purchase policy ownership.

From the summary statistics on the panel sample displayed in Table 3, a higher percentage of the population had private, employer-based, and direct-purchase *coverage* compared to those who owned these policies. Table 6 shows that the DID estimators are greater in magnitude for the coverage variables compared to the ownership variables. This suggests that private insurance policy *dependents* are also at risk of losing insurance upon the repeal of the mandate.

Additionally, the magnitude of the result for private coverage (0.8pp) is very similar to that of "any coverage" (0.9pp), suggesting that private coverage is driving these results, as opposed to public coverage.

Overall, examining the panel sample shows evidence of the negative effect of the mandate's repeal, albeit one that is relatively small in magnitude. The cross-sectional sample's results also provide evidence in this regard, for the anycov variable.

6 The Adverse Selection Question

6.1 Empirical Strategy

Recent programme evaluation literature has used triple-difference (DDD) methodologies to estimate the differential effect of a treatment on sub-groups in a sample (Olden and Møen, 2020). I hence use the following LPM DDD regression to examine the adverse selection question with the panel sample:

$$\begin{aligned} Y_{it} = \alpha_i + \mu_1 After_t + \mu_2 Treatment_i + \mu_3 H_i + \mu_4 After_t{*}Treatment_i + \mu_5 After_t{*}H_i \\ + \mu_6 H_i{*}Treatment_t + \mu_7 After_t{*}Treatment_i{*}H_i + \gamma \boldsymbol{X_i} + \epsilon_{it} \quad (3) \end{aligned}$$

Notation. $\boldsymbol{H}$: A binary measure of health.
Y: Binary variable indicating insurance status. α_i: Individual fixed-effects term.
After: Binary variable, set to 1 for after the repeal.
Treatment: Binary variable, set to 1 for those in the treatment group.
i, t: Indices for individual and time respectively.

Its fixed-effects equivalent is:

$$Y_{i,2019} - Y_{i,2018} = \mu_1 + \mu_4 Treatment_i + \mu_5 H_i + \mu_7 Treatment_i H_i + \epsilon_{i,2019} - \epsilon_{i,2018} \quad (4)$$

since After=1 in 2019 and After=0 in 2018.

The DDD estimator, μ_7, gives the difference between the average mandate repeal effect on the H=1 subgroup and that on the H=0 subgroup. Per the adverse selection hypothesis, those with higher health risks are more likely to remain insured after the policy change; that is, an unhealthier individual is more likely to have a binary insurance variable be equal to 1. Thus, the treatment effect should be more positive for the higher-risk group, and more negative for the lower-risk group.

Two variables, "non-disability" and "self-reported health", are used as H, the "measure of health" variable. H is set to 1 when an individual is in the lower-risk group (that is, when an individual is not disabled, or is considered healthy). Therefore, the DDD estimator is expected to be negative.

6.2 Results and Discussion

Regressions corresponding to equation 4 were run on the panel sample, for all the dependent variables described in Table 1.

Table 7: Results of the fixed-effects DDD LPM regressions, where H=1 indicates that an individual is not disabled.

H = Non-disability	(1) anycov	(2) private_own	(3) private_cov	(4) emp_own	(5) emp_cov	(6) direct_own	(7) direct_cov
After	-0.014***	-0.022***	-0.024***	-0.017***	-0.027***	-0.014***	-0.014***
	(-13.13)	(-8.22)	(-8.22)	(-8.22)	(-8.22)	(-13.38)	(-13.38)
After x Treatment	0.0019	0.0078*	0.0038	0.0051+	0.0089*	0.012***	0.01***
	(0.66)	(2.54)	(1.06)	(1.89)	(2.23)	(7.37)	(6.23)
After x Non-disability	0.015***	0.018***	0.022***	0.012***	0.023***	0.013***	0.013***
	(9.16)	(5.91)	(7.94)	(5.43)	(7.58)	(6.43)	(6.42)
DDD Estimator	-0.012**	-0.016***	-0.013**	-0.011***	-0.017***	-0.015***	-0.014***
	(-3.68)	(-4.49)	(-3.78)	(-3.91)	(-4.72)	(-6.21)	(-5.72)
N	41200	41200	41200	41200	41200	41200	41200

t statistics in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 7 shows the results of the DDD regressions for the non-disability variable. As expected, all the DDD estimators negative, with all being significant at the 1% significance level.

Interpreting the DDD estimator for the "any coverage" variable, on average, a non-disabled person is 1.2pp less likely to still have any insurance after the repeal of the mandate, compared to a disabled person. Overall, these results are in line with the adverse selection hypothesis, where a person with lower health risks is more likely to lose their insurance upon the mandate's repeal.

Table 8 shows the results of the DDD regressions for the "self-reported health" variable. The original variable in the ASEC dataset involves respondents rating their current health status on a five-point scale, with 1 meaning "excellent" health and 5 meaning "poor". The values 1, 2 and 3 were recoded as H=1, with 4 and 5 ("fair" and"poor" health) recoded as H=0.

Table 8: Results of the fixed-effects DDD LPM regressions, where H=1 indicates that an individual has rated themself highly for "self-reported health".

H= Health	(1) anycov	(2) private_own	(3) private_cov	(4) emp_own	(5) emp_cov	(6) direct_own	(7) direct_cov
After	-0.027***	-0.029***	-0.031***	-0.017***	-0.020***	-0.026***	-0.020***
	(-13.51)	(-23.82)	(-31.91)	(-8.49)	(-5.37)	(-8.49)	(-5.37)
After x	0.010+	0.0084*	0.0076	0.00020	0.015**	0.0060	0.014**
Treatment	(2.08)	(2.14)	(1.59)	(0.05)	(3.54)	(1.24)	(3.38)
After x	0.028***	0.026***	0.029***	0.013***	0.019***	0.022***	0.019***
Health	(16.84)	(26.74)	(21.63)	(5.69)	(6.43)	(7.82)	(6.05)
DDD	-0.020***	-0.016***	-0.017**	-0.0058	-0.018***	-0.014**	-0.019***
Estimator	(-4.77)	(-4.20)	(-3.45)	(-1.55)	(-4.99)	(-3.10)	(-4.90)
N	41200	41200	41200	41200	41200	41200	41200

t statistics in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

For the self-reported health variable, all DDD estimators are of the correct sign (negative). All are significant at the 1% significance level except for the result on employer-based policy ownership. Interpreting the DDD estimator for anycov, on average, a healthy person is 2pp more likely to stop having any health insurance coverage after the repeal of the mandate, compared to a less healthy person.

Overall, the fixed-effects DDD regression results for both measures of health offer support for the adverse selection hypothesis: all DDD estimators (as displayed in Tables 7 and 8) have the expected sign, with most being significant at the 1% significance level. One caveat, however, is that the magnitude of these effects are not large.

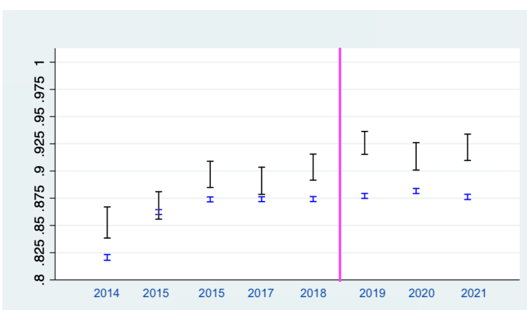
7 Robustness Checks

7.1 Investigating the Parallel Trends Assumption

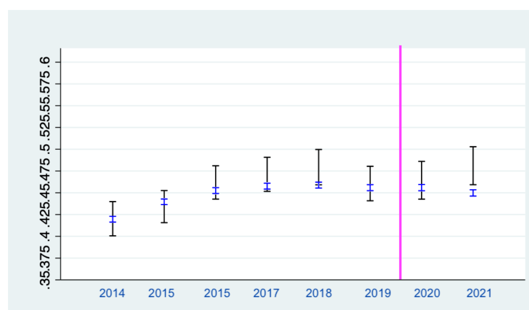
A critical threat to identification in the difference-in-differences framework used in this dissertation is the possibility that the repeal or retention of the mandate was not random. It might be possible that states select into the control or treatment groups because of different trends in insurance coverage in each state, possibly biasing the results.

It is difficult to conclusively prove that selection into or out of the mandate's repeal was random. However, if parallel trends are observed between the control and treatment groups in the pre-treatment time periods, then it is less likely that trends in insurance coverage were correlated with selection into the treatment group. Then endogenous selection into the treatment group is unlikely to be a significant issue; the DID regressions may then proceed (Angrist and Pischke, 2009).

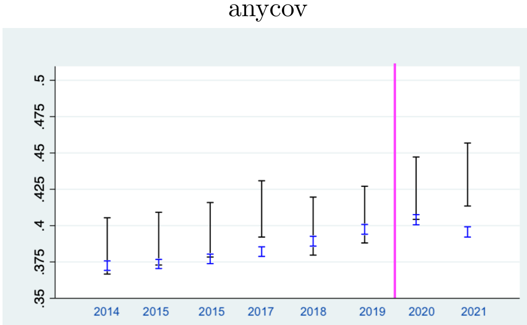
Figure 1: Visual depiction of parallel trends. The error bars show the 95% confidence interval about the mean of the variable for each year.
Blue: treatment group. Black: control group. A pink line divides the before and after periods.



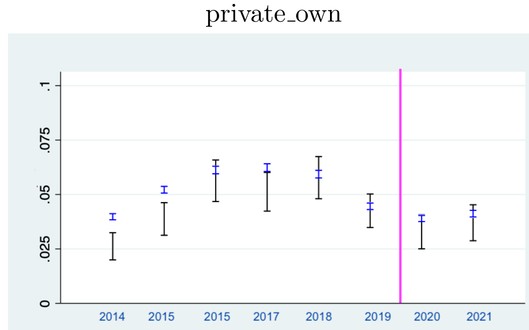
anycov



private_own



emp_own



direct_own

The parallel trends assumption is visually inspected in Figure 1, which plots the four dependent variables used for the cross-sectional sample over time, from 2014-2021. Taking the standard errors into account, Figure 1 does not show violation of the parallel trends assumption. Thus the use of the DID framework in this dissertation was likely valid.

Now consider the *hypothetical* case in which the parallel trends assumption did not hold and there is serious concern that the mandate's repeal was indeed endogenous. The mandate was repealed federally; states would have had to exert effort into retaining the mandate. Hence, the most realistic concern would be that N.J. and D.C. self-selected into the mandate's retention because insurance coverage in these two states was trending downward faster than in other states. In this hypothetical situation, regressions would fail to account for the control group's insurance rates systematically falling faster than the treatment group's insurance rates: the DID estimator would be biased *upwards*.

Thus the results of this dissertation are robust to this particular form of endogenous selection: the DID estimators were found to be consistently and significantly negative despite possibly being biased upwards. In other words, these results form a lower bound on the magnitude of the effect of the mandate's repeal.

7.2 Sample Restriction

Both samples were initially restricted to 15-64 year olds. However, under the ACA, plans and issuers that offer dependent child coverage must make this coverage available until a child turns 26 (U.S. Department of Labor, 2022). Due to this dependent coverage mandate, it is less likely that someone under the age of 26 will lose insurance upon the repeal of the individual mandate. To account for this, the fixed-effects regressions are re-run on the panel sample, restricted to 26-64 year old individuals.

Table 9 presents the DID and DDD estimators for this sub-sample. Reassuringly, all the estimators are significant and of the right sign (negative).

The DID estimators for the direct-purchase variables are bigger in magnitude for this sub-sample compared to the full sample (see Table 6). Intuitively, those who are dropped from the sample (15-25 year olds) are less likely to be driving the results for direct-purchase insurance, as they are eligible for dependent coverage.

Table 9: The DID and DDD estimates on the panel sample **for those aged 26-64**.

	(1) anycovnw	(2) private_own	(3) private_cov	(4) emp_own	(5) emp_cov	(6) direct_own	(7) direct_cov
DID estimator	-0.0056* (-2.74)	-0.0063** (-3.66)	-0.0070** (-3.73)	-0.0045** (-3.10)	-0.0050* (-2.83)	-0.0026* (-2.31)	-0.0044*** (-4.08)
DDD estimators							
H = not disabled	-0.013** (-3.83)	-0.020*** (-4.25)	-0.017** (-3.52)	-0.013** (-3.83)	-0.020*** (-4.14)	-0.018*** (-4.86)	-0.017*** (-4.74)
H = healthy	-0.021*** (-4.51)	-0.020*** (-4.26)	-0.020** (-3.74)	-0.0071+ (-1.83)	-0.022*** (-5.79)	-0.015** (-3.27)	-0.023*** (-5.68)
N	64496	64496	64496	64496	64496	64496	64496

t statistics in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

7.3 Recoding "Health"

"Self-reported health" was reported on a five-point scale (**1 being the healthiest and 5 being the least healthy**) in the ASEC dataset. This was arbitrarily recoded into a binary variable H for the DDD regressions in Section 6. In this section, as a check, I run the same regressions for different recodes of this "self-reported health" variable.

Initially, respondents who chose 1, 2, or 3 as their health score were coded as being healthy (H=1). Consider a **Recode B**, where those who report scores of 1, 4, and 5 are considered the H=0 group. People who were previously considered the "healthiest" are now considered "unhealthy", and insurance-dropping behaviour previously observed as belonging to the "healthiest" group are now being observed as belonging to the "unhealthy" group. From the Section 6 estimates, being healthy causes an increased likelihood of dropping insurance over the policy change. Hence, a less negative coefficient should be observed for Recode B compared to the original recode.

Another **Recode A** considers those who score their health as 1 or 2 as the H=1 group, and those who score 3-5 as the H=0 group. Then the DDD estimator for the original recode should be the most negative, and that associated with Recode B should be the least negative. The results of the fixed-effects LPM DDD regressions run with Recodes A and B are shown in Table 10 next to the results with the original recode, for "anycov". The relative values of the estimates are as expected, and do not contradict the adverse selection hypothesis. Similar regressions, whose results are not displayed, showed that this also held true for all the other dependent variables.

Table 10: Anycov: results of the DDD fixed-effects LPM regressions with various health recodes. As expected, the DDD estimator is most negative for the original recode and least negative for Recode B.

Variable: anycov	(1) Original	(2) Recode A	(3) Recode B
DDD Estimator	-0.020***	-0.011***	0.0083**
	(-4.77)	(-4.18)	(2.99)

t statistics in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

8 Conclusion

This dissertation's findings are in line with expectations.

In response to the **effectiveness question**: there is evidence of a significantly negative, albeit small in magnitude, effect of the repeal of the individual mandate on overall health insurance coverage, overall private coverage, and on subsets of private coverage (direct-purchase and employer-based coverage). From the magnitude of the DID estimators, I additionally find that private health insurance is driving the results, and that insurance policy dependents may be more vulnerable to coverage losses than policy owners. These differential responses are important when measuring welfare effects.

Econometrically, my findings highlight the importance of controlling for unobserved individual-level heterogeneities. Using the repeated cross-sectional estimates alone would have led one to somewhat different conclusions about the effectiveness of the mandate.

The small magnitude of the linear probability model coefficients throughout the dissertation do not necessarily imply that the mandate was responsible for only a small portion of insurance *uptake* when it was first implemented. Comparison to results of other studies suggests an asymmetry in responses to the implementation versus the repeal of the mandate, perhaps due to inertia. Policy-wise, this could imply that even a temporary mandate works by "getting people on board".

In response to the **adverse selection question**: healthier subgroups are indeed more likely to lose insurance over the policy change compared to unhealthier subgroups, providing evidence of adverse selection. The results are consistent across two measures of health: disability and self-reported health. This shows that the adverse selection mechanism dominates the alternative mechanism of uninsurance (based on the correlation between income and insurance) explained in Section 2.1. This also provides evidence of market failure in the health insurance market, which could justify government intervention. Another implication of this finding is that since healthier people (who are less at risk) are losing insurance at higher rates, the short-term welfare losses of the repeal are less serious. However, in the long run, premium "death spirals" could arise, potentially inducing long-term falls in social welfare.

Taken altogether, these results have the health policy implication that individual mandates could be an effective tool of addressing uninsurance and adverse selection.

The question of external validity then arises. Where else are these policy implications applicable? Virtually all high-income countries other than the USA have some form of universal health insurance (The Economist, 2018), so countries that may be considering an individual mandate as a health policy option are likely to be lower-income countries. Banerjee et al. (2019) cite various challenges to increasing health insurance enrollment in developing countries, such as weak administrative infrastructure, which must be accounted for.

Ultimately, this dissertation finds that the individual mandate's repeal had a significant negative effect on insurance rates and that responses to the repeal showed significant evidence of adverse selection. This shows that not only was adverse selection a dominant mechanism of uninsurance in the USA health insurance market, but also that individual mandates are potentially an effective tool of addressing it. This dissertation contributes to the body of evidence that the ACA's individual mandate was indeed, ex-post, justified. Whether a country is able to add such a mandate to its policy toolbox, however, depends on many factors: such as political will (in the USA) or administrative issues (in lower-income countries without universal health coverage).

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