Investigating Public Health Outcomes in the Context of 1332 State Innovation Waivers

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Abstract

State-based reinsurance programs under 1332 State Innovation Waivers offer opportunities for primary insurers in the private health care market to receive reimbursement from a state-established fund for incurred costs that exceed expectations. Initial effects from these programs include marginal benefits of decreased premium prices charged to policyholders and moderate expansions to insurance coverage, but downstream effects of individual health outcomes and health care utilization remain uninvestigated in this setting. Through a Difference-in-Differences analysis of the Behavioral Risk Factor Surveillance Survey, this paper examines whether reinsurance programs have any causal impacts on selected health outcomes, finding that the introduction of a reinsurance program through a 1332 waiver has only minimal and marginal effects, if any, on these health outcomes and that the effects of these programs do not translate far enough downstream to significantly alter American citizens' experiences and interactions with their health care system.

Section 1: Introduction

The general upheaval of the American health care throughout the 2010's was characterized by the passage of the Affordable Care Act [ACA] under the Obama administration, a wide-sweeping variety of reform measures that were implemented in efforts to expand coverage and improve the delivery of health-related services. Short-term results were mostly positive, with an estimated number of uninsured Americans who gained coverage ranging from approximately 7-16.5 million, and general overall satisfaction from these new enrollees (Blumenthal, 2015). One particular program initialized under the ACA, which this paper focuses on, that has recently seen expanded development and adoption at the state-level is that of reinsurance.

Reinsurance is a niche market concept, but one that continues to gain traction in the realm of health insurance to stabilize premium prices for individual market enrollees. In essence, reinsurance allows for primary health insurers in the private market to receive insurance on high-cost claims that would otherwise result in large net losses via negotiations with a reinsurer. In theory, reinsurance results in more underwriting of risk at a reduced premium, due to the insurer holding less financial culpability, and it can encourage stronger market competition that generates expanded coverage. As states continue to grapple with an inefficient and costly American health care system, they have begun to turn towards instituting these programs on their own to solve their health care crises.

Through 1332 waivers, states can institute their own independent reinsurance programs in private health insurance markets with some supplementary support coming from federal funds, akin to the ones that previously existed from 2014-2016 under the ACA's transitional reinsurance program (Uberoi, 2017). To be specific, Alaska, Minnesota, and Oregon had their 1332 waivers approved in 2017, while Wisconsin, New Jersey, Maryland, and Maine were approved in 2018-- more states have applied and been approved for 1332 waivers in 2019 and 2020, but these programs are too recent to examine, especially due to the COVID-19 global pandemic fundamentally altering everyday life within the same timespan (Howard, 2020). With more and more states throughout the nation joining the list of those applying for these waivers, it is becoming apparent that there exist legislators across both sides of the aisle who believe there is beneficial value in investment of these programs.

The purpose of this paper is to examine the downstream impacts of reinsurance implementation on citizens' health outcomes and utilization of health care services. Much of the preliminary research conducted with respect to 1332 waivers hones in on the supply-side, particularly the interactions existing between the health care insurer and their reinsurers [Further discussion of these impacts exists within section 2]. While this research is incredibly useful and valuable, there is a clear gap in the surrounding literature in terms of ascertaining the effects of instituting these programs on the outcomes of the people for whom it is intended to benefit. If these programs help to reduce the cost of having health insurance, then are individuals within states that have these programs using those offered services more?

The work done in this thesis attempts to close that existing literature gap. Utilizing survey data from the Behavioral Risk Factor Surveillance System [BRFSS], an annual nationwide telephone survey that collects information about Americans' health-related behaviors and their associated risks, we conducted regression analysis on the proportion of individuals within a single state who exhibited a particular health behavior as captured within the BRFSS. Through a difference-in-differences research design, we determined the extent to which individuals who received treatment [i.e., resided in a state that implemented reinsurance under 1332 waivers] altered their health-related behaviors and health-based outcomes when compared to individuals who did not receive treatment [i.e., resided in a state that did not implement a reinsurance program] specifically due to the treatment. The specific measures examined are covered thoroughly within section 3, and they are predicated on an individual's mental, physical, and overall health, their health insurance status, their ability to see a doctor without financial impediment, their frequency of visiting a doctor and whether or not they have a primary personal doctor.

The results of this study yielded inconclusive results about the casual effect of reinsurance on a state population's health status and engagement with health resources. While some larger trends and correlations were unveiled, our findings indicate that it is not possible to ascribe any causal effects of reinsurance on health outcomes, utilizing the stated data with the given methodology. These results give pause to the true beneficial impacts on the general population that could be derived through a reinsurance program. Limitations and confounding elements of this research necessitate further investigation into reinsurance through a variety of potential avenues, including, but not limited to: considering a different set of health outcomes and behaviors and/or a different data source; comparing and contrasting the effects of reinsurance on sub-populations as opposed to a wider general state population; analyzing whether reinsurance bears any impact on what services health insurers choose to cover [or to what extent they are covered], and how that trickles down to the insured consumer.

The rest of the paper is organized as follows: Section 2 provides a literature review, section 3 describes the data and variables of interest, section 4 explains the methodology of the research design, section 5 reports the results of the analysis, section 6 is a discussion of those results, and section 7 contains concluding remarks.

Section 2: Literature Review

Reinsurance in a General Framework

To begin, we look at what prior research has unveiled about how reinsurance works, generally, in an insurance market. In our context, reinsurance is a mechanism that affects the supply-side of the market, as reinsurers and policyholders do not directly interact with one another. Research of this market has revealed that optimal decisions, from the insurer's perspective, do exist in the presence of reinsurance with regards to premiums charged to policyholders and the amount of risk ceded to reinsurers; moreover, the existence of reinsurance implies that an insurer could feasibly reap more profit than without the reinsurer (Zhuang, 2016). The implication here, then, is that primary insurers could serve to benefit by offering more insurance, and then passing off some of the additionally incurred risk to the reinsurer.

Although the purchase of reinsurance is typically costly, it tends to be worthwhile. By purchasing reinsurance, the insurer does concede that they expect to have to pay higher costs in producing insurable services for the claims they choose to reinsure, but they do so to reduce the risk they are financially liable for (Cummins, 2008). This supports the findings by Zhuang, suggesting that higher costs do not directly lead to larger losses in a market that features reinsurance, which permits insurers to underwrite more risk for policyholders. Moreover, Zhuang's results also demonstrated the existence of an optimum premium in the presence of reinsurance. While not directly specified in that paper, a traditional economist perspective on these policies can be used to contemplate how that optimal premium differs from equilibrium; reinsurance is a policy which is shifting the supply curve to the right, hence the increased quantity supplied by the underwriting of additional insurance. From this perspective, we would expect to also see a decreased premium price charged to policyholders.

Reinsurance in Health Care Markets: Supply-Side Direct Impacts

Armed with the knowledge of how reinsurance affects insurer behavior generally, we turn towards research that is more focused on the specific impacts of reinsurance in health-care markets, as most of the early studies on this topic have examined outcomes from a primary insurer's perspective. To verify what we expect regarding premium prices, we note that former research has demonstrated how states that operate reinsurance programs see, on average, a 16.9% reduction in their individual market premiums, relative to estimated premiums without reinsurance (Sloan, 2019). This agrees with previously detailed research, and so do findings that reinsurance greatly decreases the variability of profits for private insurers (Ellis, 2017). Again, this makes sense as insurers no longer suffer the full brunt of any high-end losses, helping to drive down costs and stabilize expected profits, which in turn means that stabilization can be passed off to consumer prices. While some expected results of implementing reinsurance have been borne out, others remain somewhat unclear.

The expectation is for a reinsurance program to help provide cheaper health care coverage for more people, but the actualities of 1332 state innovation waivers have had mixed outcomes to hit on these goals thus far. For example, when Maryland initialized their reinsurance program in 2019, total enrollment in the insurance market rose by 2.2% from the prior year,

particularly boosted by increases to enrollment for African Americans, Hispanics, and young adults. This represented a 20% increase in predicted enrollment rates without the reinsurance program (Hoag, 2019). Juxtapose this with states like Minnesota, which did see a bump to its enrollment rate after establishing a reinsurance program, but fell short of their projected growth (Schwab, 2018), and we can begin to doubt the true effectiveness of reinsurance as a primary means to expand insurance coverage. While coverage has expanded, the inconsistency across states to reach their goals causes uncertainty as to how attributable coverage expansion is to the presence of reinsurance. Thus, while in theory reinsurance provides the means for insurers to offer additional coverage, reinsurance in practice does not necessarily guarantee that more individuals will be enrolled in the market.

Reinsurance in Health Care Markets: Supply-Side Second Order Impacts as Selection Incentives

One possible way to interpret the previous conundrum [or possibly provide some justification to it] is the notion of selection incentives for primary insurers, and how being in a reinsurance market affects that. At its core, selection incentives are a way to frame insurer behavior as being risk averse in a market with incomplete information. That is, insurers have a preference to sell insurance to low-risk enrollees, as they are more likely to incur lower costs, and deny coverage to high-risk clients who may be costly (Jack, 2001). Prior to the ACA, insurers typically denied coverage to those deemed to be high-risk according to their having a deniable pre-existing condition (Claxton, 2016), but the ACA made it illegal to deny, or charge more for, health insurance coverage based solely on a pre-existing condition. Despite however well-intentioned this component of the legislation is, it does nothing to alter the insurer mindset of desirability to avoid high-risk. In other words, while the ACA eliminated the most prominent form of selection incentives, it did not outright eliminate incentives for insurers to try and select their risk. To this end, if these remaining selection incentives are significant, it can help explain why coverage enrollment has fallen short of some expectations.

Bearing this in mind, one sub-field of research that has emerged recently has predicated upon how these remaining selection incentives continue to exist, from the insurer perspective, in the private health care market. In this context, selection incentives have instead shifted towards being based on the quality of plan offerings for enrollees. This is referred to as service-level selection, and typically involves alteration of the types of services, or the availability of providers, offered by a particular plan. The rationale behind this is to either deter or encourage enrollees towards certain plans—take, for instance, an insurer who covers both smokers and non-smokers. This insurer, hypothetically, could be incentivized to degrade the plan for non-smokers by removing offered services or providers due to having a lack of complete information [i.e., to discourage a smoker from lying about their smoking habit to receive the cheaper, non-smoker plan].

Research has shown that, under such circumstances, the potential exists for particular types of consumers to not be able to purchase insurance for any level of coverage (Hendren, 2017). This type of behavior by insurers is detrimental to market stability, serving the welfare of an insurer rather than the welfare of society; previous work has revealed that some sectors of the insurance market, such as drug pricing for certain therapeutic classes, do, in fact, suffer from

service-level selection which makes it harder for enrollees in the individual markets who need those drugs to find suitable healthcare plans (Rose, 2017). This implies that selection incentives can still motivate insurers to restrict plan access for specific demographics and populations.

With these basic logistics of selection incentives, we return to the question of how these incentives may be impacted in the presence of reinsurance. It should be stated that, in the subsequently mentioned studies, results indicate that reinsurance has the capacity to reduce selection incentives. It follows then that what is important is determining the extent to which reinsurance combats selection incentives; the findings here are much more nuanced. One study found that, compared to other chronic diseases like heart disease, cancer and diabetes, reinsurance only covered 80% of costs on average for individuals with mental illness, suggesting that mental health plans could still suffer from being intentionally degraded to deter those with mental illness from enrolling (Zhu, 2013). Moreover, recalling that reinsurance reduces the variation in the profitability of an insurer by limiting high-end losses, a logical extension from this fact is that predictive power of costs has increased due to the insurer only being completely financially liable to the lower-risk enrollees. Once insurers have this knowledge, their determination of optimal plans to offer can vary to maximize profitability—this assessment lends itself quite well to the discovery that a reinsurance program performs worse in terms of reducing selection incentives when compared to a form of risk adjustment (Ellis, 2017). Thus, it has been proven that reinsurance can mitigate selection incentives, but the extent to which it does so pales in comparison to other policies that could remedy inefficiencies of the health care market and may be insufficient in its own right to prevent detrimental plan degradation, specifically with regards to sub-populations that are characterized as high-risk.

Reinsurance in Health Care Markets: Demand-Side Direct Impacts

We now examine reinsurance on the demand side, as it pertains to market enrollees. We note that while reinsurance does not directly affect demand [as policyholders and reinsurers do not directly interact with one another], the direct market outcomes from these programs are still felt by consumers: premium prices are decreasing on average and enrollment has seen modest increases in these states, as previously stated, although causality in the latter case remains undetermined as of this point.

While these direct impacts are known and could still be worthwhile to research as a component of analysis for these programs, the real interest lies in determining if we can uncover significant differences of health care access and utilization between residents of states with reinsurance programs versus those without. Finding an answer to this question would help illuminate whether reinsurance has downstream health effects that could further affect [either positively or negatively] individuals beyond the scope of simply the price of their premium. It is worth noting that although reinsurance, by design, is more so intended to address high-cost enrollees, research of outcomes should not be limited to only high-cost enrollees. These programs operate state-wide, so research should investigate the outcomes for not only high-cost enrollees, but for varying socio-demographic and health characteristics.

Patient Health Outcomes and Health Care Utilization as Developed Under the ACA

Much of the recent research surrounding patient outcomes and health care utilization has, rightfully, revolved around the impacts of the ACA on these factors. We thus build our discussion around what we know about health care access and utilization, as it has developed under current legislation. One of the most general studies that was encountered on this subject was an analysis of the effects of the ACA on preventative care usage amongst the privately insured. Despite encouraging preventative care use being a deliberate intention of the ACA, results from this research showed that substantial increases to preventative care were relatively trivial, with only rates of routine checkups and the percentage of individuals receiving flu vaccinations having sizable boosts (Hong, 2017). On a related note, a separate study examined how preventative care was impacted by additional Medicaid expansions. In that study, results were quite a bit stronger: states that expanded Medicaid were found to have higher rates of insurance amongst low-income, childless adults and positive health access and utilization benefits, including the probability of having a personal doctor, receiving some forms of preventative care [in particular, HIV testing] and improved self-reported health (Simon, 2017). One takeaway which is interesting is that other studies on Medicaid expansions have found results inconsistent with Simon's findings—in particular, the inability to conclude that Medicaid expansions had any impact on self-reported health status (Wherry, 2016). Collectively, the variations in the results of the literature suggest that at least some forms of preventative care usage are positively correlated with having insurance coverage, but the extent to which health care access and utilization is altered depends on a case-by-case basis, where each case could feasibly warrant its own detailed research.

If at least some forms of preventative care are positively linked with expanded insurance coverage, then it is natural to wonder if some forms of preventative care are used less frequently when coverage is expanded. The literature itself is a bit of a mixed bag in this regard. Specifically, we have seen contradictory findings in Emergency Department [ED] visits and hospitalizations, with an Oregon study that found an increase in the usage of these services, while other studies of coverage expansions have uncovered reductions in ED visits and hospitalization usage (Sommers, 2017). It makes sense for fluctuations like these to exist, as services like these ones are prone to serving critical health needs, which can often fall outside of the domain of insurance coverage. Considering the discrepancies found in different settings that are examining the same basic question of how health insurance coverage impacts health outcomes, it seems reasonable to investigate characteristics that are of the same vein as these ones within the states that have reinsurance programs, and to be prepared for some mixed outcomes, depending on the natural state of usage of the service in question.

Concluding Thoughts on the Surrounding Literature

In summation of the literature review, we know that: reinsurance can drive down premium prices and result in modest enrollment increases; primary insurers remain risk-averse in spite of this, and will potentially continue to find ways to avoid selling insurance to high-risk individuals, commonly by degrading plan offerings; expanding insurance coverage is [usually] positively related with health care utilization and access, although the magnitudes of these effects

vary depending on the populations and settings we consider. We are interested in researching the effect [if one exists] of implementing reinsurance programs on health care market enrollees' health outcomes, as it relates to having primary care access, making use of available health services, reductions in cost limitations to receive care and self-reported health status. The prior heading into this research is that these reinsurance programs will not actually have a significant impact on individual health outcomes, even though they are reducing premium prices and resulting in modest enrollment increases. The simple intuition is that reinsurance is a relatively small measure affecting the supply-side of insurance that not a lot of people know about and, thus, should not have exceptionally large impacts on consumer behavior. The goal, then, is to determine if investment in these programs will yield any downstream benefits, from the demand-side, outside of premium prices and modest coverage boosts.

Section 3: Data

The data analyzed in this research paper is that of the BRFSS survey. One key feature of the BRFSS dataset which makes it enticing for analysis of the impact of reinsurance on health outcomes is the fact that the survey does not de-identify respondents at the state-level [as opposed to other health-outcomes focused datasets like the Michigan Retirement Survey or the National Health Interview Survey, which limit geographical identification to broader census regions of the United States]. Thus, the BRFSS represents a rich data source spanning multiple years that could be utilized to compare health outcomes at the state-level.

Having clear access to state-level data easily permits splitting the data up into states that instituted reinsurance programs, and those that did not. The actual number of responses per state and per year does vary, but it tends to range anywhere from a minimum of approximately 500 to a maximum of 6,000 respondents annually. It should be noted that the format of the BRFSS was permanently altered beginning with the 2012 survey, meaning that data from prior to 2012 does not include the survey items consistently found in each year henceforth. Survey data for 2020 was not publicly available at the time of this research, so the data collected and reported on in this paper exists from between 2012 to 2019. Additionally, New Jersey did not participate in the national BRFSS during 2019, the only year within our available data in which New Jersey's 1332 waiver was in effect—this renders analysis of New Jersey for this paper impossible as currently constructed, and all further analysis focuses only on Alaska, Minnesota, Oregon, Maine, Maryland, and Wisconsin as states which have implemented reinsurance.

The following gives basic observational counts for each state included in this study; this indicates counts after omission of observations which contained missing records for necessary variables of interest. All states which implemented reinsurance are present, as well as additional control states, to be explained more thoroughly and conceptually in section 4:

State	2012	2013	2014	2015	2016	2017	2018	2019	Total
Alaska	1,185	1,345	1,154	946	756	787	471	505	7,149
Minnesota	3,317	3,677	2,632	2,541	2,062	1,847	1,550	910	18,536
Oregon	1,564	1,572	1,108	902	625	529	346	333	6,979
Maine	3,259	2,504	2,518	2,557	2,430	2,507	2,085	2,043	19,903
Maryland	4,147	3,975	3,923	3,505	4,112	2,901	3,205	3,185	28,953
Wisconsin	1,454	1,862	1,705	1,242	1,017	840	708	837	9,665
Pennsylvania	5,895	3,162	2,820	1,008	1,018	768	473	594	15,738
Washington	5,130	3,234	2,737	3,679	2,775	1,817	1,835	1,414	22,621
Michigan	3,319	3,377	1,788	1,587	1,631	1,617	922	1,104	15,345
New York	1,771	2,432	1,696	3,089	6,850	2,289	4,487	2,075	24,689

State	2012	2013	2014	2015	2016	2017	2018	2019	Total
Arizona	2,112	1,042	3,984	1,692	2,516	1,935	955	1,074	15,310
Wyoming	1,839	1,784	1,745	1,329	1,091	789	799	865	10,241
South Dakota	1,743	1,286	1,186	1,286	915	1,095	1,013	842	9,366
Kansas	3,175	5,528	2,333	3,285	1,798	2,153	1,552	984	20,808
Total	39,910	36,780	31,329	28,648	29,596	21,874	20,401	16,765	225,303

With respect to the actual logistics of working with the data, we now focus on the variables of interest within the dataset. We can break down the variables into outcomes that we are looking to measure, and the features [or predictors] of those outcomes. Below is a list of both outcomes and predictors that were utilized when conducting this research. Survey respondents have a pre-determined set of possible responses [i.e., factor levels] to each question below—most of those factor levels have been re-coded for our purposes, particularly to condense multinomial variables into a more traditional binary form. Further explanation of the re-coding is present within Section 4, as this re-coding is primarily conducted to match our model's conceptual framework. For the sake of brevity and readability, these response levels will be omitted here, but one can find them, as well as each encoded variable name, at the BRFSS website under the "Codebook" section for each survey year post-2011. Portions of questions contained in brackets are additional needed clarifications provided by the author:

Outcomes:

- "In general, your health is [how good/poor?]:"
- "Now thinking about your physical health, for how many days during the past 30 days was your physical health not good?"
- "Now thinking about your mental health, for how many days during the past 30 days was your mental health not good?"
- "During the past 30 days, for about how many days did poor physical or mental health keep you from doing your usual activities?
- "Do you have any kind of health care coverage, including health insurance, prepaid plans such as HMOs, or government plans such as Medicare, or Indian Health Service?"
- "Do you have one person you think of as your personal doctor or health care provider?"
- "About how long has it been since you last visited a doctor for a routine checkup?"
- "Was there a time in the past 12 months when you needed to see a doctor but could not because of cost?"

Control features:

- Sex
- Race/Ethnicity
- Age
- Employment Status
- Marital status
- Income
- Number of adults present in the household [household size]

Despite the BRFSS containing survey data on both education and income, due to the well-known correlation that exists between the two variables, we opted to include only income as a control feature and omit education. Furthermore, although the BRFSS contains many questions about preventative care, such as the usage of cancer screenings, these questions were not consistently asked for all years in the available data—as such, it was not possible to consider them; had the questions regarding these preventative treatments been asked for each available year, they would have been researched as outcomes.

Having discussed the nature of the BRFSS dataset, and the variables within it that we are focusing in on as pertinent to our research question, we now want to develop a model that can make use of this data to answer whether reinsurance programs under 1332 waivers are causing changes in health care utilization and access.

Section 4: Methodology

As mentioned in section 1 of this paper, we employ a difference-in-differences [DID] research design to investigate our question. A DID method makes sense at the most basic level for a handful of reasons. Firstly, implementation of a reinsurance program can quite easily be interpreted as a treatment, where residents of states that are approved, or have been approved, for 1332 waivers are receiving treatment, and residents of states that have not been approved for 1332 waivers are in a control group. As previously articulated, the state-level nature of the BRFSS data also goes together with this mode of thought, making it easy to identify respondents within the treatment group and respondents within the control group. Another reason that a DID model is enticing is due to the ability to infer causality from the model; the interpretation of the DID estimator is the true effect of the treatment on the outcome that is being measured, which would permit investigation of the changes American citizens are experiencing in their utilization and access to health care that is attributable to reinsurance programs. When working with observational data, as is the case with the BRFSS survey, the non-experimental design greatly hinders the ability to determine causal impacts; a DID model is an excellent alternative to obtain a quasi-experimental design which alleviates those concerns.

In reference to the selection of treatment and control groups that was briefly mentioned in section 3, we then have 6 states that were approved for 1332 waivers which compose our treatment group: Alaska, Minnesota, Oregon, Maine, Maryland, and Wisconsin. For selection of the control states, we referenced the United Health Foundation's Annual Report (2018), specifically the component focusing on ranking U.S. states with respect to their health. The health ranking model used by the Foundation examined behavioral health, community and environment, policy, and clinical care to determine each state's health score, then ranked them accordingly. We selected Pennsylvania, Washington, Michigan, New York, Arizona, Wyoming, South Dakota, and Kansas due to their similar health scores and ranks in relation to the given treatment states.

It is imperative that treatment and control groups be as similar as possible to ideally capture only the effect of the treatment that is being investigated, rather than some other potential confounders. With this in mind, we must note that one potential roadblock in our design is that out of our treatment group, all states except for Wisconsin adopted Medicaid Expansion policies in the 2010's (Kaiser Family Foundation, 2021). Medicaid Expansions could certainly have impacts upon individual health outcomes, and we also deliberately selected Wyoming, South Dakota, and Kansas as control states since those three have also not adopted Medicaid Expansions. Sensitivity analysis was conducted during the research by omitting Wisconsin and the non-Expansion control states and focusing strictly in on treatment and control states that had adopted Medicaid Expansions. These results are presented alongside ones which included all of the above states within section 5.

To define the details of our DID model in the context of this research, consider a BRFSS survey respondent i at time t. This individual has a collection of sociodemographic characteristics at this time, as defined above in the controls of our features, and we can denote it by X_{it}. Now, we can let "treatment" be defined as residence within a state that implemented a

reinsurance program. In our case, we necessitate two treatment groups: one represented by individuals which implemented reinsurance in 2018 [those states being the ones that had their waivers approved in 2017, so Alaska, Minnesota, and Oregon], and another represented by individuals that implemented reinsurance in 2019 [these states are the ones which had their waivers approved in 2018, so Wisconsin, Maryland, and Maine]. We can denote these groups by:

$$T_{1i} = \begin{cases} 1, if\ individual\ i\ resides\ in\ a\ state\ that\ implemented\ reinsurance\ in\ 2018 \\ 0, otherwise \end{cases}$$

$$T_{2i} = \begin{cases} 1, if\ individual\ i\ resides\ in\ a\ state\ that\ implemented\ reinsurance\ in\ 2019 \\ 0, otherwise \end{cases}$$

To track when and if a reinsurance program has been implemented in the state, we can introduce the indicator variables:

$$t_{1i} = \begin{cases} 1, if\ time\ is\ after\ or\ during\ early\ treatment\ implementation\ (2018\ or\ later) \\ 0, if\ time\ is\ before\ early\ treatment\ implementation\ (2017\ or\ earlier) \end{cases}$$

$$t_{2i} = \begin{cases} 1, if\ time\ is\ after\ or\ during\ late\ treatment\ implementation\ (2019\ or\ later) \\ 0, if\ time\ is\ before\ late\ treatment\ implementation\ (2018\ or\ earlier) \end{cases}$$

We now must clarify one component of the DID model. The underlying assumption guiding the development of a DID model rests on the parallel trends assumption. The parallel trends assumption is predicated upon both control and treatment groups linearly trending in a similar way with respect to the measured outcome prior to treatment being implemented, allowing for differences in the outcome levels between both groups, but relying on them to be tracking in the same direction, at a relatively similar rate and manner. Then, post-treatment, any additional change to the treatment group's outcome which strays from the trendline that they were previously on can be chalked up to the intervention effect, captured by the DID estimator.

One complication with working with the BRFSS survey exists in reconciling the categorical nature of the data to fit in with the linearity of a DID model. Taking a cue from other studies in pertinent literature, one workaround is to instead determine the proportion of individuals in the treatment and control groups that exhibited the outcome behavior; one such example is a study that utilized BRFSS data to examine the effects of laws which permitted the denial of services to same-sex couples on sexual minority adults' mental health (Raifman, 2018). Doing so coerces the data to fit a linear form much more naturally than its true binomial/multinomial form, giving an avenue with which to pursue a DID research design.

In this vein, our research henceforth measures the previously identified outcomes in terms of the proportion of individuals in the treatment and control groups that demonstrated that outcome. For example, rather than looking at an individual respondent's self-reported health status, we instead look at the proportion of individuals per year, in each group, who reported their health to be good, very good, or excellent. This type of re-coding applies to other outcome variables in similar ways [Feature variables retain their categorical nature in the model]. Put another way, we now look at the proportion of individuals who:

- Have had poor physical health for 14 or more days in the previous month (referred to as being in physical distress).
- Had poor mental health for 14 or more days in the previous month (referred to as being in mental distress).
- Have had poor physical or mental health inhibit their usual activities for 14 or more days in the previous month (referred to as poor overall health).
- Have some form of health coverage.
- Have at least one personal doctor.
- Have had an annual checkup within the past 12 months.
- Have had to delay or forgo needed medical care due to excessive costs in the past 12 months.

Let any of these proportional outcomes listed above be given by Y_{it}, so that we can measure each outcome at separate times for differing individuals. One common method to accounting for omitted variable bias in a DID design is to introduce a vector of fixed effects which represents time-invariant characteristics that measure any potential heterogeneity that may exist between respondents and, in turn, impact research findings. In many instances with designs like ours, the fixed effects are simply dummy variables for each state, attributing any potential noise in the data to the nature of differing state conditions. However, in this case, by definition of our treatment variable as state residence, introduction of state dummy variables would subsequently invoke multicollinearity within our model. Due to this fact, a fixed effects vector is omitted from our analysis.

Finally, with the above variables defined, and the addition of an intercept, α, and an error term, ε_i, for each individually predicted outcome, we can now define our model to be:

$$Y_{it} = \alpha + \beta T_{1i} + \gamma t_{1i} + \delta T_{2i} + \tau t_{2i} + \rho(T_{1i} * t_{1i}) + \varphi(T_{2i} * t_{2i}) + \omega X_{it} + \theta fixed + \varepsilon_i$$

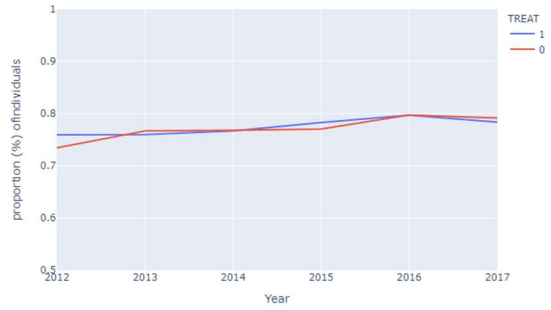
The main takeaway and major point of interest from this model lies in the ρ term and the φ term, which are the DID estimators, with the former representing the impact of reinsurance on the proportion of individuals who experience outcome Y_{it} in those states which implemented reinsurance in 2018, whereas the latter represents the impact of reinsurance on the proportion of individuals who experience outcome Y_{it} in states which implemented reinsurance in 2019. [noting, of course, that:

$$T_{1i} * t_{1i}, T_{2i} * t_{2i} > 0 \; if \; and \; only \; if \; T_{1i} = t_{1i} = 1 = T_{2i} = t_{2i},$$

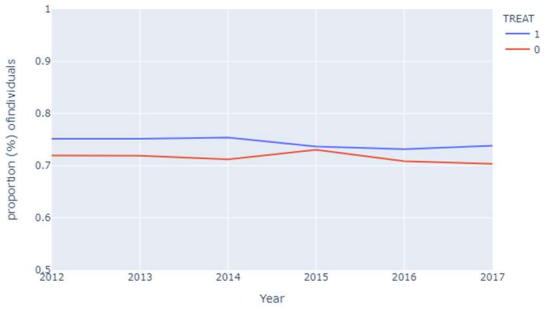
so that the individual in each treatment group resides in a state that has already implemented reinsurance].

We now explicitly check the parallel trends assumption for our analysis. 8 graphs are given below, each representing the trends of the treatment and control groups for each of the detailed proportional outcomes:

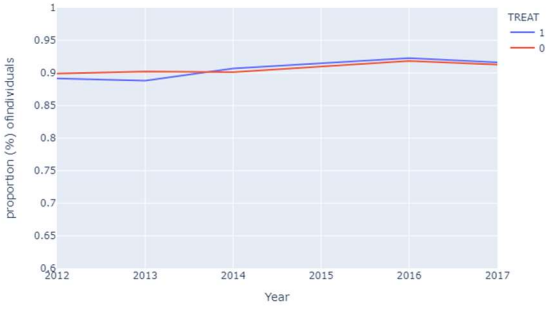
Proportion of individuals who had a checkup within the past year



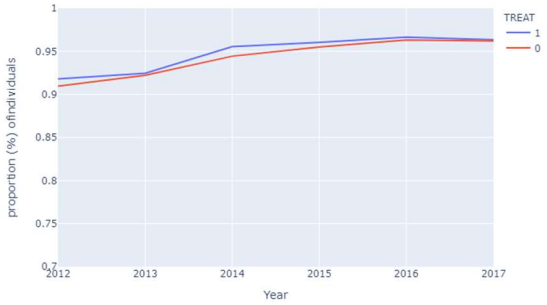
Proportion of individuals who reported good general health



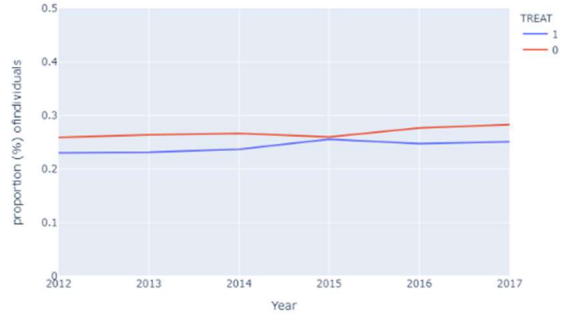
Proportion of individuals who had a personal doctor



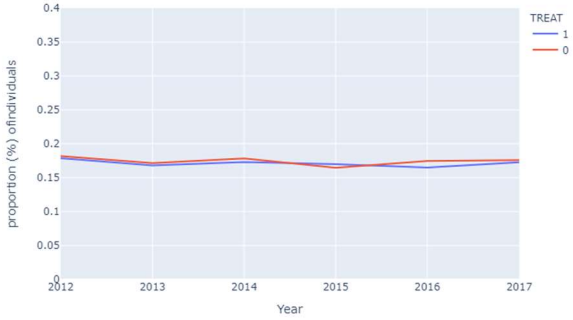
Proportion of individuals who had a health plan



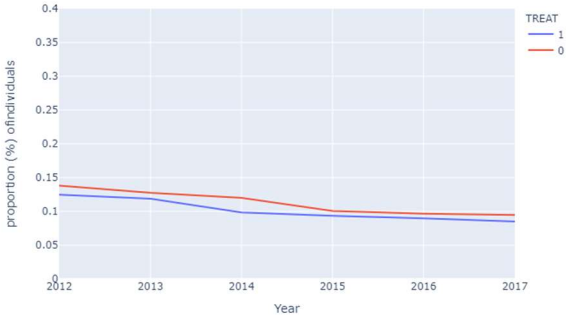
Proportion of individuals who reported physical distress



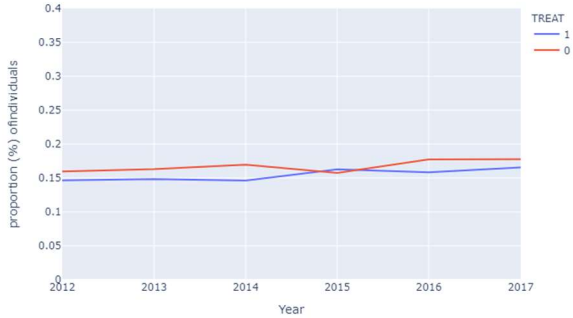
Proportion of individuals who reported mental distress



Proportion of individuals who could not see a doctor due to cost



Proportion of individuals who reported overall poor health



The graphing of the trendlines for each outcome shows, by and large, a violation of the parallel trends assumption. While linearity does appear to be present in most of the outcomes, in several instances there are points in time at which the trendlines between the treatment group [corresponding to treat being 1] and those of the control group [corresponding to treat being 0] overlap and seem to move in opposing directions. Even in outcomes where the trendlines are more parallel with one another, such as the proportion of individuals who did not see a doctor due to cost or the proportion of individuals who reported good general health, the differences between the outcomes for the two groups are relatively minor.

With the basic assumption of the DID model not being satisfied, how can we rectify the non-parallel nature of the trends to still conduct DID analysis? One of the recently emerging methods to combat this violation and still proceed with a DID model to ascertain causality is to use propensity score matching [PSM]; PSM is a way of reducing selection bias present within observational data by matching members of the control group and the treatment according to their covariate characteristics. With these covariates, our survey respondents can be given propensity scores that represent their probability of being assigned to the treatment group. We can then match participants in the control group to those in the treatment group by pairing together subjects with similar propensity scores, helping to balance the covariates throughout our data, thereby reducing the confounding nature of observational data, and providing a reasonable avenue by which to ascribe causality.

Relatively new research has begun to assess the impact of utilizing PSM within the context of DID analyses. One such study tested the performance of standard DID estimators, time-series analysis and DID with PSM estimators in a setting where the parallel trends assumption did not hold. Their findings demonstrated that the DID model with PSM yielded better mean squared error results than either of the other estimators and had a superior estimator coverage [i.e., the true treatment effect was contained within the confidence interval of the estimator] (Ryan, 2018). An interesting point of PSM is that, when running the model after performing matching, the covariates used to balance the groups can either be included in the model or not; the primary analysis of this paper did include those covariates in the final model, but sensitivity analysis without them was also ran and tables for those results are in the appendix. Considering this research, we can conduct analyses using both the standard DID approach and an approach utilizing PSM and have more confidence that the latter method will yield an estimator which is closer to the true effect of reinsurance implementation on individual health outcomes.

For this analysis, multiple linear regression models were tested. One point to note is that usage of weights within machine learning is a grey area in the field, with some recommendations to utilize them and some to stray from introducing weighting, instead letting the model determine the details on its own in an algorithmic manner. In accordance with the BRFSS outlines on utilizing the survey data for analytical purposes, since our dataset was comprised of several years' worth of BRFSS data, we constructed complex sampling weights by adjusting each annual observation's weight according to the proportion of its prevalence within the data. These weights were employed in the primary analysis, but sensitivity analysis was conducted by omitting any weighting, and the corresponding regression results are in the appendix. Furthermore, studies

have shown that using sampling weights for matching, and then using a product of the sampling weights and the propensity score weights as a final weight measure for modeling gives far more robust results (Ridgeway, 2015). Considering these findings, we conducted a standard DID approach and a DID approach utilizing PSM with a weighting scheme that follows the methodology outlined by Ridgeway. The models built were standard OLS, both by using the weights directly and by creating a survey design object based on the complex weights via the "Survey" package in R, LASSO linear models, and Elastic-net linear models. The benefit of using LASSO and Elastic-net regularization methods were to reduce overfitting of the model and identify the primary predictors of the proportional outcomes. The additional advantage to employing an Elastic-net model is that it combines the penalty terms of both Ridge and LASSO regression, where the former drives non-significant predictor coefficients towards 0, but never completely eliminates them, while the LASSO penalty completely crushes some coefficients to 0. The Elastic-net method thus offers a happy medium between the two, still providing a form of regularization, but not quite as aggressively as the LASSO does.

Section 5: Results

Below are the regression results that were derived from our analyses. Each table represents the results for a different proportional outcome, containing estimates for the standard DID weighted OLS coefficients and standard errors, the survey design's OLS coefficients and standard errors, the coefficients for the weighted Elastic-net and LASSO models, and the equivalent set of results for the DID with PSM [with the inclusion of covariates]. The estimator results given in the tables are for both the early treatment estimator [corresponding to the ρ term in our model's definition] and the late treatment estimator [corresponding to the φ term in our model's definition], for the analysis including all states and for the analysis consisting strictly of the Medicaid Expansion states, as previously detailed. All models were developed with the use of robust standard errors to circumnavigate any heteroskedasticity.

Table 1a. Regression Results for the proportion of individuals who reported good general health (weighted, PSM with covariates)

	Early treatment estimator (all states)	**Late treatment estimator (all states)**	**Early treatment estimator (expansion states)**	**Late treatment estimator (expansion states)**
Weighted OLS coef. (std. errors)	-0.00691712 (0.00058712)	-0.00383393 (0.00012987)	-0.007661 (0.00058962)	-0.0032476 (0.00013597)
Survey coef. (std. errors)	-0.00712586 (0.00053575)	-0.00381699 (0.00010988)	-0.00778503 (0.00052563)	-0.0031858 (0.00011567)
Weighted LASSO coef.	0	0	0	0
Weighted elastic-net coef.	-0.00611419	-0.00267067	-0.00681387	-0.00197765
PSM with covariates OLS coef. (std. errors)	-0.00443277 (0.00054108)	0.00417314 (0.00033849)	-0.00611152 (0.00056732)	0.00200334 (0.00028418)
PSM with covariates survey coef. (std. errors)	-0.00454168 (0.00048296)	0.00411324 (0.00028743)	-0.0063657 (0.0005086)	0.00214956 (0.00024569)
PSM with covariates LASSO coef.	0	0	0	0
PSM with covariates elastic-net coef.	-0.0031504	0	-0.00561153	0.00179358

Table 2a. Regression results for proportion of individuals that reported having poor overall health in the past month (weighted, PSM with covariates)

	Early treatment estimator (all states)	Late treatment estimator (all states)	Early treatment estimator (expansion states)	Late treatment estimator (expansion states)
Weighted OLS coef. (std. errors)	-0.00310876 (0.00011626)	0.00451106 (0.00008012)	-0.00314357 (0.00011793)	0.00417056 (0.00008538)
Survey coef. (std. errors)	-0.0030948 (0.00010092)	0.00450247 (0.0000659)	-0.00316981 (0.00010304)	0.0041699 (0.00007104)
Weighted LASSO coef.	0	0	0	0
Weighted elastic-net coef.	-0.0022573	0.00386659	-0.00296639	0.00384218
PSM with covariates OLS coef. (std. errors)	-0.0025324 (0.00011225)	0.00367353 (0.00008532)	-0.0027297 (0.00012068)	0.00355147 (0.00009324)
PSM with covariates survey coef. (std. errors)	-0.00254037 (0.00009677)	0.00368778 (0.00007025)	-0.00276235 (0.0001057)	0.0035244 (0.00007845)
PSM with covariates LASSO coef.	0	0	0	0
PSM with covariates elastic-net coef.	-0.00240539	0.0036094	-0.00256347	0.00330007

Table 3a. Regression results for the proportion of individuals that suffered from mental distress in the past month (weighted, PSM with covariates)

	Early treatment estimator (all states)	Late treatment estimator (all states)	Early treatment estimator (expansion states)	Late treatment estimator (expansion states)
Weighted OLS coef. (std. errors)	0.00057711 (0.00021231)	-0.00041485 (0.00006886)	0.00056776 (0.00022052)	-0.00030999 (0.00007187)
Survey coef. (std. errors)	0.00072803 (0.00019041)	-0.00040322 (0.00005376)	0.0005842 (0.00019278)	-0.00031836 (0.00005699)
Weighted LASSO coef.	0	0	0	0
Weighted elastic-net coef.	0.00049855	-0.00036579	0.00048626	-0.00025699
PSM with covariates OLS coef. (std. errors)	0.00114944 (0.00018428)	-0.00285518 (0.00011786)	0.00098013 (0.00020399)	-0.00206619 (0.00011003)
PSM with covariates survey coef. (std. errors)	0.00125241 (0.00016018)	-0.0028466 (0.00009835)	0.00103098 (0.00017896)	-0.00211231 (0.00009331)
PSM with covariates LASSO coef.	0	0	0	0
PSM with covariates elastic-net coef.	0.00011137	-0.00143512	0.000773	-0.00177292

Table 4a. Regression results for proportion of individuals that suffered from physical distress in the past month. (weighted, PSM with covariates)

	Early treatment estimator (all states)	Late treatment estimator (all states)	Early treatment estimator (expansion states)	Late treatment estimator (expansion states)
Weighted OLS coef. (std. errors)	0.01021917 (0.00021592)	0.0180987 (0.00010114)	0.01082123 (0.00020969)	0.01751457 (0.00010265)
Survey coef. (std. errors)	0.01025381 (0.00019097)	0.01807188 (0.00007973)	0.01078196 (0.00018406)	0.01748344 (0.00008225)
Weighted LASSO coef.	0	0	0	0
Weighted elastic-net coef.	0.00974468	0.01696582	0.01033458	0.01629675
PSM with covariates OLS coef. (std. errors)	0.00847811 (0.00022719)	0.01544981 (0.00014674)	0.00970496 (0.00022549)	0.01587144 (0.00013581)
PSM with covariates survey coef. (std. errors)	0.00848283 (0.00019895)	0.01546705 (0.00012015)	0.00970983 (0.00019879)	0.01580936 (0.00011227)
PSM with covariates LASSO coef.	0	0	0	0
PSM with covariates elastic-net coef.	0.00836079	0.01505263	0.00934871	0.01469292

Table 5a. Regression results for the proportion of individuals who have some form of health coverage. (weighted, PSM with covariates)

	Early treatment estimator (all states)	Late treatment estimator (all states)	Early treatment estimator (expansion states)	Late treatment estimator (expansion states)
Weighted OLS coef. (std. errors)	0.00025847 (0.00028766)	-0.00553098 (0.00024125)	0.00028561 (0.00029384)	-0.0065822 (0.0002424)
Survey coef. (std. errors)	0.00012492 (0.00023504)	-0.00556025 (0.00018144)	0.00016916 (0.00023879)	-0.00656162 (0.00018498)
Weighted LASSO coef.	0	0	0	0
Weighted elastic-net coef.	0.00019836	-0.00035117	0.00025186	-0.00128647
PSM with covariates OLS coef. (std. errors)	0.00089482 (0.0003108)	-0.00427586 (0.00024171)	0.00077117 (0.00032694)	-0.00579675 (0.00025299)
PSM with covariates survey coef. (std. errors)	0.00078587 (0.00025584)	-0.00427603 (0.00018527)	0.00060098 (0.0002707)	-0.00576753 (0.00019802)
PSM with covariates LASSO coef.	0	0	0	0
PSM with covariates elastic-net coef.	0.00210623	0	0.00090116	-0.00520588

Table 6a. Regression results for the proportion of individuals who have at least one (1) personal doctor. (weighted, PSM with covariates)

	Early treatment estimator (all states)	Late treatment estimator (all states)	Early treatment estimator (expansion states)	Late treatment estimator (expansion states)
Weighted OLS coef. (std. errors)	0.00573485 (0.00018049)	0.00261582 (0.00010974)	0.0061722 (0.00018514)	0.00175782 (0.00010726)
Survey coef. (std. errors)	0.00561996 (0.00015402)	0.00257701 (0.00009058)	0.00608037 (0.00015699)	0.00176214 (0.00008924)
Weighted LASSO coef.	0	0	0	0
Weighted elastic-net coef.	0.00536671	0.00247348	0.00576645	0.00160776
PSM with covariates OLS coef. (std. errors)	0.00467586 (0.00018313)	0.00386658 (0.00012048)	0.0055181 (0.00019434)	0.0027257 (0.00012049)
PSM with covariates survey coef. (std. errors)	0.00458906 (0.00015372)	0.00384765 (0.00010082)	0.0054058 (0.00016481)	0.00274798 (0.00010219)
PSM with covariates LASSO coef.	0	0	0	0
PSM with covariates elastic-net coef.	0.00420616	0.00284436	0.00521147	0.00211825

Table 7a. Regression results for the proportion of individuals who did not see a doctor when one was needed due to excessive costs. (weighted, PSM with covariates)

	Early treatment estimator (all states)	Late treatment estimator (all states)	Early treatment estimator (expansion states)	Late treatment estimator (expansion states)
Weighted OLS coef. (std. errors)	0.01042772 (0.0002173)	0.01522752 (0.00018345)	0.0107791 (0.00022006)	0.01594933 (0.00018883)
Survey coef. (std. errors)	0.01051761 (0.00017779)	0.01523317 (0.00013778)	0.01086045 (0.00017887)	0.01593947 (0.00014483)
Weighted LASSO coef.	0	0	0	0
Weighted elastic-net coef.	0.0094719	0.0141841	0.00986738	0.01495231
PSM with covariates OLS coef. (std. errors)	0.00861883 (0.00024427)	0.01433051 (0.00018461)	0.00950771 (0.00025203)	0.0155055 (0.00019589)
PSM with covariates survey coef. (std. errors)	0.00870012 (0.00020149)	0.01431624 (0.00014134)	0.00964094 (0.00020826)	0.01550226 (0.00015408)
PSM with covariates LASSO coef.	0	0	0	0
PSM with covariates elastic-net coef.	0.00596864	0.01044133	0.00857849	0.01432904

Table 8a. Regression results for the proportion of individuals who received a checkup within the past 12 months. (weighted, PSM with covariates)

	Early treatment estimator (all states)	Late treatment estimator (all states)	Early treatment estimator (expansion states)	Late treatment estimator (expansion states)
Weighted OLS coef. (std. errors)	0.00554795 (0.00026032)	-0.02096665 (0.00020068)	0.00458218 (0.00025835)	-0.02119157 (0.00021457)
Survey coef. (std. errors)	0.00545718 (0.00021606)	-0.02098755 (0.00015448)	0.00450096 (0.00021072)	-0.02117373 (0.00016724)
Weighted LASSO coef.	0	0	0	0
Weighted elastic-net coef.	0.00593109	-0.01797213	0.00501728	-0.0182508
PSM with covariates OLS coef. (std. errors)	0.00923743 (0.00030165)	-0.01897975 (0.000213)	0.00703648 (0.00029308)	-0.02019869 (0.00022105)
PSM with covariates survey coef. (std. errors)	0.00917421 (0.00025202)	-0.0190091 (0.00016861)	0.00686794 (0.0002438)	-0.02019006 (0.00017582)
PSM with covariates LASSO coef.	0	0	0	0
PSM with covariates elastic-net coef.	0.00889409	-0.01720262	0.00801501	-0.01707957

One of the more noticeable and easily interpretable results is that all estimator coefficients of the LASSO models for all outcomes are reduced to 0. That is, the LASSO models consistently determine that the reinsurance programs have no causal effects on any of the measured health outcomes. The same result also holds in the case of our sensitivity analyses. While discouraging in the sense of trying to demonstrate that the reinsurance programs do have a true effect on the observed outcomes, these results are not entirely surprising given the formerly described aggressive regularization nature of the LASSO model—most of the LASSO models instead selected covariate variables such as age and annual income to have non-trivial coefficients in the model, and PSM LASSO models ran without covariates selected the linear components from which the DID estimator is the product, rather than the estimator itself as the interaction term. The Elastic-net model provided a more reliable method of performing regularization for our purposes as most optimal alpha penalty parameters for each outcome tended to be between 0.1 and 0.3, meaning that the model was much more of a ridge regression than a LASSO one. Further discussion of these estimates will segment analysis based on whether the model was a standard DID approach or incorporated PSM, but, unless otherwise specified, will not draw distinctions between the estimates which included all states and the estimates which were derived from only examining the Medicaid expansion states.

With respect to the other models, some of the stronger and more consistent results that were derived come from a handful of the investigated outcomes. In table 6a, regression results demonstrated a positive relationship between reinsurance and the proportion of individuals who have at least one personal doctor, with the standard DID approach showing between a .46% to .62% proportional increase for the early treatment states and between a .16% to .385%

proportional increase for the late treatment states; DID with PSM yielded between a .42% to .55% proportional increase for the early treatment states and between a .21% to .386% proportional increase for the late treatment states. Where standard errors were applicable to the model, the errors tended to be quite smaller in magnitude than the estimates themselves, indicating relatively precise estimates—for instance, taking the early and late treatment estimators for the all states analysis from the survey design as an example, the 95% confidence interval widths were only .06% and .03%, respectively. Even with the estimated coefficients being relatively small in magnitude, these interval widths are incredibly narrow, implying a fair deal of precision is present in our estimates.

In an opposite manner to the previous result in the sense of the betterment of individuals' lives and ease of access to health care, our regression results similarly consistently showed a positive relationship between reinsurance and the proportion of individuals who suffered from physical distress in the past month, as well as the proportion of those who needed to delay or forgo medical care due to excessive costs. In the former outcome, table 4a shows that standard DID methods gave estimators ranging from a .97% to a 1.08% increase for early treatment states and from a 1.63% to 1.81% for the later treatment states whereas DID with PSM produced estimators ranging from a .83% to a .97% increase for the early treatment states and a 1.47% to 1.59% increase for the late treatment states. In the case of the latter outcome, table 7a exhibits standard DID results that ranged from a .95% to 1.08% increase for the early treatment estimator and between a 1.42% to 1.59% increase for the late treatment estimator. For this outcome, DID with PSM estimators were approximately between a .6% to .96% increase for the early treatment and between a 1.04% to 1.55% increase for the later treatment group. These estimator coefficients suggest an interesting dynamic between reinsurance implementation and health outcomes, as increases to these proportions represent less people receiving medical help, entirely due to excessive costs for receiving that service, and a greater proportion of people struggling with their physical health for half of the month or more. Like with the outcome measured in table 6a, the standard errors here, when applicable, are quite small in relation to the estimates themselves, again implying relatively precise estimates. It is worth noting that the DID with PSM estimates, which prior research has shown tend to be more reliable, are smaller on average than their standard DID counterparts, but that the difference is not monumental, and all estimates remain positive regardless of the model deployed.

Alternatively, some other results indicated varying directional impacts of reinsurance programs on health outcomes—that is, for several outcomes, the two DID estimator coefficients had opposing signs, implying differing treatment effects for each treatment group. In one instance in particular, the proportion of individuals who reported having good overall health in the past month, results for a given estimator conflicted with other results for that same estimator depending on the model developed—this is reflected in the late treatment estimator for this outcome, where the standard DID approach yielded negative estimates, yet the addition of PSM gave positive estimates. In other cases, like the proportion of individuals that reported having poor overall health last month, table 2a demonstrates that results for all models of the early treatment states yielded negative estimates, so decreases in the proportion, while the estimator for late treatment was consistently positive, so that reinsurance resulted in more individuals

having poor overall health. The inverse occurrence [i.e., the early treatment effect being positive and the late treatment effect being negative] existed with the regressions of the proportion of individuals who have received an annual checkup, the proportion of individuals who suffered from mental distress in the past month, and the proportion of individuals who had some form of health coverage, although it should be stated that these estimates trend towards a very minor magnitude, especially when compared to some other results such as the aforementioned one of the proportion of individuals who delayed medical care due to high costs.

In this last case of situations where the early treatment effect was routinely positive and the late treatment effect was negative, when we considered the proportion of individuals who had some form of health coverage, the result derived is relatively unexpected, particularly because states which have been approved for 1332 waivers have experienced modest increases to their private health care insurance market enrollment rates; our findings seem to run counter to this notion. They suggest that reinsurance programs in and of themselves do not directly cause a higher proportion of citizens to possess some form of health insurance coverage. Most outcomes researched except for this one and, arguably, the proportion of individuals who delayed or did not receive medical care due to excessive costs, are quite downstream from the immediate market impacts on reinsurance; however, these ones are directly connected to the premium pricing which reinsurance reduces. Thus, seeing these outcomes have mixed proportional shifts, or even net negative effects regarding the ease and affordability of health care access, proved to be a surprise.

In summation of section 5, our results varied moderately significantly depending on the outcome being investigated, and the exact model methodology that was utilized. As previously detailed, outcomes like the proportion of individuals who needed to delay or forgo medical care due to cost limitations were quite consistent across the board, and contained estimates that were, compared to other outcome measures, fairly large in their magnitudes. On the other hand, some outcomes yielded estimates that either were not consistent in their signage across all models, or that differed in their signage dependent on which treatment group was being considered—further comments on these results, their implications, and potential rationalizations for the estimates we have obtained are present in the proceeding section.

Section 6: Discussion

We now contextualize the findings of section 5 in the grander scheme of reinsurance implementation. As already stated, reinsurance is a market mechanism that directly affects the supply-side of health insurance. However, the end goal of these policy waivers is to provide consumers with a better healthcare product that they can access easily and utilize when needed to promote and preserve their health. While this research is by no means exhaustive of all potential ramifications for consumers in a state that introduces reinsurance, it does focus in on several components that are effectively measuring Americans' health status, health care access and health care utilization, and is justified to the extent of examining whether the equilibrium market shifts that reinsurance creates are being felt by consumers as well.

In that sense, our results are rather inconclusive as to answering whether reinsurance programs are helping or hurting individual health outcomes. As elaborated on in section 5, many outcomes had resulting DID estimators with differing signage, making it difficult to definitively draw conclusions about the true treatment effect in those instances. Moreover, many of the proportional shifts represented by the estimators, in either direction, are relatively minor in magnitude, typically being well under 1%, however, the ones that do approach the 1% threshold account for moderately non-trivial alterations. Although our model accounted for control variables as an attempt to reduce confounding in our study, it remains entirely possible that an omitted variable is playing a role in these changes, or that the BRFSS data is simply too noisy and these variations are being skewed by that, rather than a true treatment effect. When Maryland established their reinsurance program, they did so in conjunction with a boost to advertisements that encouraged citizens to acquire health insurance (Hoag, 2019). The joint nature of these strategies contributes to the difficulty in validating the extent to which reinsurance is solely responsible for our results, and is only one potential confounder to our results, amongst other unknowns that could feasibly modify individual behaviors in terms of health care.

Regardless, despite the volatile nature of some of our estimates, there are still some takeaways to glean from the results. Again, our results seem more consistent in a handful of outcomes like the proportion of individuals who have at least one personal doctor, leading us to be more confident in the notion that reinsurance implementation can have some positive causal effects on one's tendency to have a primary doctor. The same principle applies to the proportion of individuals who did not receive or delayed medical care due to high costs, although the positive relationship here actually carries negative implications for citizens. Harkening back to section 2, it was determined that while reinsurance can fight selection incentives, it typically is not the best approach. Especially when relating selection incentives in the form of plan degradation to these findings, our results could be tangentially connected to the cited prior research and follow as a result from it—insurer plan offerings may be hampered due to a desire to acquire reimbursement on their claims, limiting the tendency then for individuals to seek out care, even if they may need it. Although not verified by this study, it is one potential factor that cannot be ruled out, and this particular result could justify future research into this outcome, especially since, across all model trials, this estimate had some of the smallest variation amongst

any of our measures and was one of the consistently largest in terms of magnitude. Again, external factors could be playing a significant role in this finding, yet it is likely one of the most unexpected result of this analysis since its implications run directly counter to the intended effects of introducing a reinsurance program.

In the instances where treatment effects appeared to differ depending on the timing of the implementation of reinsurance, the interpretation is a bit less clear. While it is certainly possible that the variability of individual state conditions results in differing effects of reinsurance on those outcomes, it is also equally possible that reinsurance is having no causal effects on these outcomes, so that the variations exist as natural phenomena, and our model is being coerced to ascribe some non-zero coefficient to the DID estimator. Rather than asserting causality, it is better in these cases to err on the side of caution and fail to reject the null hypothesis that reinsurance has no definitive casual impacts on these outcomes. Specifically, findings on the proportion of individuals who had any form of health coverage fall under this type, and, like that of those who do not receive medical care because of costs, it is remarkably interesting to see a result that does not agree with the underlying motivations of implementing reinsurance—while we have stated that we cannot definitively conclude any causal implications for this outcome, that does not imply we should dismiss or ignore this result. Rather, the estimated causal effects for this outcome beckon even further analysis, particularly with respect to the fact that the estimators for the early treatment group are practically trivially small, while those of the late treatment group are consistently sizably negative. This dissonance between the two groups is peculiar in this instance because of the size of those differing magnitudes, whereas other results tend to live within a similar range of one another, regardless of whether they differ in size or not.

When considering the limitations of this study, we touch again on a couple of previously mentioned facts, specifically that the BRFSS is a sprawling dataset, which may have contained too much noise to accurately capture the effect that we were looking for, and, additionally, that the implementation of other programs in conjunction with reinsurance to encourage health insurance participation clouds the ability to speak in absolutes in terms of causality. Furthermore, all of these programs established under 1332 waivers are very new, with only a couple years, at most, of being in effect [with respect to the data that we operated with]—it remains possible that it is simply too early on for the larger, overarching impacts of these programs to extend as far down as to individual health outcomes, particularly when the programs have yet to fully experience increased insurer participation, one expected outcome of reinsurance implementation that should further improve health care insurance options for consumers (Schwab, 2018). It is also understood that our model setup did not fully satisfy the requirements of a DID model and, even when accounting for this violation via PSM, our results do remain qualified by this complication. Lastly, as explained in section 3, New Jersey was not included in this analysis due to data unavailability, thus limiting the scope of being able to completely assess the entirety of states which have implemented reinsurance programs.

Looking forward to potential future avenues of research that can follow up on this thesis, there exist a few different directions that one could head in. An independent examination of New Jersey utilizing similar methodologies and investigating the same types of health outcomes from

a different dataset could be fruitful and add additional weight to the findings stated here. In a separate vein, investigation of different health outcomes could also be useful; it is entirely plausible that other health outcomes [like the usage of preventative testing services] may have larger, more measurable alterations in response to reinsurance. Additionally, a similar approach could be employed to look at sub-populations rather than entire state populations. Reinsurance is designed to help on claims with unexpectedly high costs, such as someone in otherwise good health who is diagnosed with cancer, and it may be worthwhile to try and determine if certain sub-populations, like those with chronic diseases, experience differing outcomes in relation to others. Subsequently, tying back to the findings pertaining to proportions of individuals who had some form of health coverage, justification was provided in section 2 which reasoned that examining entire state populations was appropriate for the purposes of this thesis, yet our results indicate that further segmentation may unveil important additional information. Reinsurance operates within the sphere of the private health care market, so, along with stratifying populations based on health characteristics, it may be worthwhile to also do so based on where and how an individual receives their health coverage. Ideally, doing so could have the potential to shed light and provide clarity to why we found such stark discrepancies in that particular outcome amongst our treatment groups, perhaps revealing a more nuanced and concrete difference amongst varying narrower populations. Finally, in connection to our dialogue on insurer actions in the presence of reinsurance and our results on the proportion of individuals with health coverage and those who have forgone or delayed medical care due to costs, there is research potential in exploring the relationship between plan offerings by insurers in states that have adopted 1332 waivers, especially in the context of selection incentives; this could perhaps provide some justification to why these outcomes can seem to devolve with the implementation of reinsurance. If insurers are intentionally degrading plan offerings to dissuade individuals from some options, then perhaps those individuals are finding themselves with no course of action but to accept a plan that covers their worst-case scenario needs while leaving them in a position where they cannot afford to seek out care for more [subjectively] minor ailments or conditions.

Section 7: Conclusion

1332 waivers provide the means for states to limit increasing premium prices in private health insurance markets, with the hope of providing a more affordable healthcare option to citizens. In doing so, states are simultaneously hoping that lower prices can drive more people towards acquiring health insurance and protecting their health. We investigated whether these 1332 waivers are bearing out those results, as measured by individuals' health outcomes, in addition to a handful of other downstream effects, which tend to improve under boosted insurance enrollment rates. This investigation was done with the purpose of determining the efficiency and effectiveness of reinsurance to not only qualm market volatility, as it is directly intended to do, but to further refine the livelihood of Americans. Our research findings indicate minimal causal effects of reinsurance implementation on individual health outcomes currently. Any such effects that have seemed to emerge are relatively marginal, and some of our more consistent and stronger results even indicate poorer health outcomes for populations of states which have implemented reinsurance, suggesting that the immediate positive market effects of reinsurance on health insurance suppliers are not effectively working their way down to the consumers' experiences. As these waivers continue to pick up steam with bi-partisan support that makes them one of the few significant healthcare-related measures that are progressing into law in the current day and age, the financial and practical logistics of continuing to establish, develop and maintain reinsurance programs for the improvement of the American health care system warrants further examination. As a measure to improve insurer market conditions, reinsurance has been proven through research to succeed, but the research conducted here demonstrates that the average American's access and utilization of health care will remain mostly unchanged in response to this policy, beckoning the question of whether continued support of these programs can truly be justified as a policy that yields any substantial health enhancements for the American people.

Appendix

Table 9b. Regression Results for the proportion of individuals who reported good general health (unweighted)

	Early treatment estimator (all states)	Late treatment estimator (all states)	Early treatment estimator (expansion states)	Late treatment estimator (expansion states)
OLS coef. (std. errors)	-0.00809045 (0.00040074)	-0.00034249 (0.0001115)	-0.00755331 (0.00038825)	0.00152321 (0.00014982)
LASSO coef.	0	0	0	0
Elastic-net coef.	-0.00786429	-0.00008522	-0.00738618	0.0012666

Table 10c. Regression Results for the proportion of individuals who reported good general health (PSM with no covariates)

	Early treatment estimator (all states)	Late treatment estimator (all states)	Early treatment estimator (expansion states)	Late treatment estimator (expansion states)
PSM without covariates OLS coef. (std. errors)	-0.0044478 (0.00054348)	0.0041875 (0.00034021)	-0.00612429 (0.00057028)	0.00206045 (0.0002862)
PSM without covariates survey coef. (std. errors)	-0.00455876 (0.0004849)	0.0041455 (0.00028902)	-0.0063878 (0.00051055)	0.0021994 (0.00024769)
PSM without covariates LASSO coef.	0	0	0	0
PSM without covariates elastic-net coef.	-0.00316988	0	-0.00564286	0.00182674

Table 11b. Regression results for proportion of individuals that reported having poor overall health in the past month (unweighted)

	Early treatment estimator (all states)	Late treatment estimator (all states)	Early treatment estimator (expansion states)	Late treatment estimator (expansion states)
OLS coef. (std. errors)	-0.00309541 (0.00007659)	0.00401594 (0.00004502)	-0.00259464 (0.00007599)	0.0039823 (0.00005112)
LASSO coef.	0	0	0	0
Elastic-net coef.	-0.00308154	0.00394813	-0.0025838	0.00390895

Table 12c. Regression results for proportion of individuals that reported having poor overall health in the past month (PSM with no covariates)

	Early treatment estimator (all states)	Late treatment estimator (all states)	Early treatment estimator (expansion states)	Late treatment estimator (expansion states)
PSM without covariates OLS coef. (std. errors)	-0.00252779 (0.00011347)	0.00365425 (0.0000834)	-0.00272769 (0.00012054)	0.00347551 (0.00009149)
PSM without covariates survey coef. (std. errors)	-0.00253138 (0.00009736)	0.00365239 (0.00006831)	-0.00275523 (0.00010631)	0.00346051 (0.00007736)
PSM without covariates LASSO coef.	0	0	0	0
PSM without covariates elastic-net coef.	-0.00225128	0.0035364	-0.00186436	0.00287066

Table 13b. Regression results for the proportion of individuals that suffered from mental distress in the past month (unweighted)

	Early treatment estimator (all states)	Late treatment estimator (all states)	Early treatment estimator (expansion states)	Late treatment estimator (expansion states)
OLS coef. (std. errors)	0.00276652 (0.00013611)	-0.00125839 (0.00004448)	0.00300355 (0.0001301)	-0.00157019 (0.00005571)
LASSO coef.	0	0	0	0
Elastic-net coef.	0.00265408	-0.00119154	0.00289177	-0.00150061

Table 14c. Regression results for the proportion of individuals that suffered from mental distress in the past month (PSM with no covariates)

	Early treatment estimator (all states)	Late treatment estimator (all states)	Early treatment estimator (expansion states)	Late treatment estimator (expansion states)
PSM without covariates OLS coef. (std. errors)	0.00114541 (0.00018337)	-0.0028376 (0.00011484)	0.00100353 (0.00020369)	-0.00201343 (0.00010759)
PSM without covariates survey coef. (std. errors)	0.00125322 (0.00015931)	-0.00281546 (0.00009599)	0.00103617 (0.00017821)	-0.00207083 (0.00009094)
PSM without covariates LASSO coef.	0	0	0	0
PSM without covariates elastic-net coef.	0	-0.00017474	0.00078662	-0.00172959

Table 15b. Regression results for proportion of individuals that suffered from physical distress in the past month. (unweighted)

	Early treatment estimator (all states)	Late treatment estimator (all states)	Early treatment estimator (expansion states)	Late treatment estimator (expansion states)
OLS coef. (std. errors)	0.00931668 (0.00014914)	0.01662738 (0.00005946)	0.00913124 (0.00014881)	0.01600457 (0.00007179)
LASSO coef.	0	0	0	0
Elastic-net coef.	0.00917531	0.01634544	0.00898224	0.01569091

Table 16c. Regression results for proportion of individuals that suffered from physical distress in the past month. (PSM with no covariates)

	Early treatment estimator (all states)	Late treatment estimator (all states)	Early treatment estimator (expansion states)	Late treatment estimator (expansion states)
PSM without covariates OLS coef. (std. errors)	0.00848649 (0.00022995)	0.01543421 (0.00014571)	0.00972085 (0.00022788)	0.01578222 (0.00013473)
PSM without covariates survey coef. (std. errors)	0.00850052 (0.00020089)	0.01543145 (0.00011914)	0.00973022 (0.00020183)	0.01573921 (0.00011194)
PSM without covariates LASSO coef.	0	0	0	0
PSM without covariates elastic-net coef.	0.00835421	0.01496773	0.00938156	0.01462574

Table 17b. Regression results for the proportion of individuals who have some form of health coverage. (unweighted)

	Early treatment estimator (all states)	Late treatment estimator (all states)	Early treatment estimator (expansion states)	Late treatment estimator (expansion states)
OLS coef. (std. errors)	-0.0013286 (0.00016665)	-0.00415033 (0.00010963)	-0.00053786 (0.00017053)	-0.00368991 (0.00012275)
LASSO coef.	0	0	0	0
Elastic-net coef.	-0.00123754	-0.00405926	-0.00046616	-0.00359941

Table 18c. Regression results for the proportion of individuals who have some form of health coverage. (PSM with no covariates)

	Early treatment estimator (all states)	Late treatment estimator (all states)	Early treatment estimator (expansion states)	Late treatment estimator (expansion states)
PSM without covariates OLS coef. (std. errors)	0.00084709 (0.00031015)	-0.00437163 (0.00022425)	0.00072392 (0.00032706)	-0.0060749 (0.00023663)
PSM without covariates survey coef. (std. errors)	0.0007705 (0.00025476)	-0.00440906 (0.00016896)	0.00058245 (0.00026965)	-0.00599435 (0.00018115)
PSM without covariates LASSO coef.	0	0	0	0
PSM without covariates elastic-net coef.	0.0022051	0	0.00187219	-0.00052908

Table 19b. Regression results for the proportion of individuals who have at least one (1) personal doctor. (unweighted)

	Early treatment estimator (all states)	Late treatment estimator (all states)	Early treatment estimator (expansion states)	Late treatment estimator (expansion states)
OLS coef. (std. errors)	0.00319664 (0.00010765)	0.00185971 (0.00005555)	0.00337236 (0.00010573)	0.0020764 (0.0000628)
LASSO coef.	0	0	0	0
Elastic-net coef.	0.00283605	0.00173138	0.002956	0.00187188

Table 20c. Regression results for the proportion of individuals who have at least one (1) personal doctor. (PSM with no covariates)

	Early treatment estimator (all states)	Late treatment estimator (all states)	Early treatment estimator (expansion states)	Late treatment estimator (expansion states)
PSM without covariates OLS coef. (std. errors)	0.00467326 (0.00018483)	0.00384703 (0.00011313)	0.0055188 (0.00019651)	0.00263207 (0.00011484)
PSM without covariates survey coef. (std. errors)	0.00459884 (0.00015475)	0.00380903 (0.0000952)	0.00541793 (0.00016705)	0.0026728 (0.0000969)
PSM without covariates LASSO coef.	0	0	0	0
PSM without covariates elastic-net coef.	0.0040858	0.00249633	0.0049665	0.00152854

Table 21b. Regression results for the proportion of individuals who did not see a doctor when one was needed due to excessive costs. (unweighted)

	Early treatment estimator (all states)	Late treatment estimator (all states)	Early treatment estimator (expansion states)	Late treatment estimator (expansion states)
OLS coef. (std. errors)	0.01021232 (0.00012249)	0.0138283 (0.00008795)	0.0093576 (0.00012801)	0.0135492 (0.00009864)
LASSO coef.	0	0	0	0
Elastic-net coef.	0.01004803	0.01364442	0.00919714	0.01335796

Table 22c. Regression results for the proportion of individuals who did not see a doctor when one was needed due to excessive costs. (PSM with no covariates)

	Early treatment estimator (all states)	Late treatment estimator (all states)	Early treatment estimator (expansion states)	Late treatment estimator (expansion states)
PSM without covariates OLS coef. (std. errors)	0.00865173 (0.00024462)	0.01439903 (0.00017166)	0.00954293 (0.00025223)	0.01572048 (0.00018365)
PSM without covariates survey coef. (std. errors)	0.00870766 (0.00020146)	0.01441418 (0.00012889)	0.00965508 (0.00020723)	0.01567565 (0.00014153)
PSM without covariates LASSO coef.	0	0	0	0
PSM without covariates elastic-net coef.	0.00593348	0.01048717	0.0071022	0.01222205

Table 23b. Regression results for the proportion of individuals who received a checkup within the past 12 months. (unweighted)

	Early treatment estimator (all states)	Late treatment estimator (all states)	Early treatment estimator (expansion states)	Late treatment estimator (expansion states)
OLS coef. (std. errors)	0.00699615 (0.00013909)	-0.01736331 (0.00009357)	0.00828173 (0.00015087)	-0.01674114 (0.00010877)
LASSO coef.	0	0	0	0
Elastic-net coef.	0.00709829	-0.01676497	0.00842867	-0.01606843

Table 24c. Regression results for the proportion of individuals who received a checkup within the past 12 months. (PSM with no covariates)

	Early treatment estimator (all states)	**Late treatment estimator (all states)**	**Early treatment estimator (expansion states)**	**Late treatment estimator (expansion states)**
PSM without covariates OLS coef. (std. errors)	0.00920679 (0.00029735)	-0.01908114 (0.00020035)	0.00699281 (0.0002884)	-0.02044406 (0.00020476)
PSM without covariates survey coef. (std. errors)	0.00916386 (0.00024857)	-0.01913734 (0.00015589)	0.00684429 (0.00023596)	-0.02039445 (0.00015893)
PSM without covariates LASSO coef.	0	0	0	0
PSM without covariates elastic-net coef.	0.0088647	-0.01697549	0.00801948	-0.01725675

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