The Medicaid Expansion & Initial Health Outcomes: An Inquiry from State to Subpopulation Levels

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Executive Summary

One the most significant changes of the Affordable Care Act, and of federal policy over the last few decades, was the Medicaid expansion. Beginning January 1, 2014, the federal government provided 100% of the revenue for states that set an income eligibility level for single adults to 138% of the federal poverty level, and will provide 90% of the funds beginning in 2020. With this financial responsibility looming for state governments, along with the change in leadership in the White House and efforts to repeal much of the ACA, it is critical for state and federal government decision makers to understand the effects of this policy when considering whether or not to continue this level of coverage.

This study seeks to understand the impact of the Medicaid expansion on the general public's health by comparing states that did and did not expand Medicaid eligibility in 2014 and 2015. According to the Kaiser Family Foundation, there have been over 150 studies analyzing the Medicaid expansion's impact on insurance coverage, access to care, and healthcare utilization, but there have been a limited number of studies on actual health outcomes, and none that analyze outcomes beyond specific subpopulations.

Our dataset is the Current Population Survey's Annual Social and Economic Supplement in the years 2013, 2014, and 2015. Our outcome variable is self-reported health status. Our analysis is composed of a series of seven models:

- Aggregate Level
    - No standard error clustering
    - With standard error clustering
    - With standard error clustering and inverse probability weighting
- Individual Level
    - Without standard error clustering
    - With standard error clustering
    - Sample Trimming
    - With standard error clustering and inverse probability weighting

We included each stage of our analysis for the sake of transparency and to demonstrate the sensitivity of our results.

Our analysis found no statistically significant results at the aggregate/state level. We believe this is due to:

- Dissimilarity between the treatment and control groups' pretreatment characteristics
- Significantly reduced sample size from using state-level averages/percentages
- The relatively small percentage of the general population newly eligible due to the expansion

After failing to find significant results and identifying flaws in the aggregate models, we performed analysis at the individual level, however we still did not detect a statistically significant change in health status.

The Medicaid Expansion & Health Outcomes - Conceptual Frameworks & Existing Research

Health care policy in the United States is a highly complex and controversial subject. Most post industrial countries provide a much greater level of government investment in health care than the U.S. For a variety of reasons, the U.S. has remained resistant to efforts to expand the government's role in providing health care for its citizens.

After litigation in the Supreme Court, states were given the option to set the income eligibility limit for single adults to receive Medicaid to 138% of the federal poverty level beginning January 1, 2014. The federal government paid 100% of the increased cost of providing this coverage, and will reduce their share to 90% in 2020.

One of the primary goals of the Medicaid expansion, as stated by the Obama Administration, was reducing the number of uninsured Americans and expanding access to healthcare. While access in and of itself may be a worthwhile goal, there is also an implicit assumption that providing access to healthcare will improve health outcomes.

The cost of the Medicaid expansion is one of its most controversial aspects.

Studies have found significant effects of the expansion for certain subpopulations of the U.S., such as children, pregnant women, the newly eligible population, or evaluating one or a small number of states. However, we have not found a published analysis of the impact of the expansion on the entire U.S. population. Analyzing a dataset representative of the entire U.S. population gives policymakers a big picture perspective on the effect of the Medicaid expansion. This is valuable because while a program or policy may have a strong impact on the target population, the total effect on society may be small, and therefore not as worthwhile as investing in other programs that may have a larger positive net effect.

Data

Data Source

Our study uses the Current Population Survey's Annual Social and Economic Supplement, which provides more detail than the standard CPS survey, and crucially, the self reported "health status" variable. This data was collected in April by the US Census Bureau in 2013, 2014, 2015. Our total number of observations at the individual level across all states and the three years is 601,214.

The CPS is widely accepted as a dataset representative of the US population for the purpose of statistical analysis. The health status variable within the Social and Economic Supplement provides the only variable that could indicate changes health outcomes that was readily accessible within the timeframe and resources of this analysis.

Identification Strategy: Timeframe, Outcome Variable, & Assumptions

For a variety of reasons, 18 state governments have chosen not to accept the federal funds that would fund this eligibility expansion. Our treatment group is states that received the federal funds for the expansion, and our control group is the 18 states that did not. We are including the state of Vermont in the control group because, while its medicaid eligibility rate is higher than federal policy requires, there was no change in its eligibility rate between 2013 and 2015.

Our pretreatment year is 2013. The policy went into effect January 1, 2014, but we will also use 2015 as an additional post-treatment year to allow for an implementation period, as people generally sign up or change their insurance only once over the course of the year. Additionally, using 2015 allows us to capture the change in Medicaid enrollment with the CPS' variable "enrolled in Medicaid last year".

Based on information accessed from the Kaiser Family Foundation, we have created the new "treated" variable, with states that changed their eligibility rate to 138% of the federal poverty line designated as "1", and those that did not as "0."

Measuring health outcomes is also a difficult endeavour. Previous studies have used specific medical measurements, such as blood pressure, cholesterol levels,

Explanation of health status variable

Explanation & Justification of Control Variables

Understanding what contributes to an individual's self-reported health status is a complicated endeavour. By including certain demographic characteristics as control variables, we hope to eliminate the effect of these confounding variables in our analysis.

- Age

Because older individuals enroll in Medicare, not Medicaid, we expect states with older populations to experience a smaller impact from the expansion.

- Percent Female

Because a major cost and component of Medicaid is providing maternity care, we have created the variable to account for the potential difference in effect the expansion would have on .

- Percent Non-White

There are major disparities across a wide variety of outcomes regarding race, even when controlling for many other societal variables. Most racial minority groups earn less than whites on average, and are therefore more likely to enroll in Medicaid. We therefore expect that states with larger non-white populations will experience a larger impact from the expansion.

- Percent Married

Numerous studies have found that marriage predicts better health outcomes. We include this control variable to account for the potential difference in marriage-rates between states.

- Percent Employed

Employed individuals experience better health outcomes than unemployed individuals. We include this control variable to account for the potential difference in employment rates between states.

- Percent with High School Diploma/GED or Higher

Educated individuals are more likely to experience better health outcomes.

- Average Income

We expect that wealthier individuals will experience better health status. We chose the variable "Total Family Income for Replicating Official Poverty Status", as that looked to be the variable most relevant to the poverty rate, which is what is used to determine Medicaid eligibility.

We have also included a suite of variables that will account for variation of enrollment in Medicaid and private health insurance between states:

- Percent Covered by Medicaid last year (we included HIMCAID, in which the Census Bureau imputes whether or not people were covered even if they didn't say they were)
- Percent Covered by Private Health Insurance Last Year
- Child Covered by Medicaid Last Year
- Dependent covered by privately-purchased insurance last year
- Covered by military health insurance last year

4) Methodology

4.1) The "Ideal" Experiment

For statistical inference, we need to have exogenous variation in the treatment variable. However, in most real-world problems, such exogenous variation is not possible. For example, local government does not flip a coin to pass a bill. As a result, we usually face some potential endogeneity biases while studying actual policies implemented by the states. This is particularly true for the case of Medicaid expansion policy. There are various ways to think about why some states have implemented the policy, while others have not. For example, states with majority of democratic voters tend to favor such policies more. In this paper, we do our best to control for such endogeneity issues and examine the effects of the Medicaid expansion policy. To do so, we can use our pre-treatment health status data on individuals in all states before the Medicaid expansion.

An ideal experiment would also have zero attrition and 100% compliance over time, which we would accomplish by mandating all selected states implement the Medicaid expansion simultaneously and identically and continue the policy until after the experimental period.

4.2) Aggregate-level Model

We start with an aggregate-level model of states in different years. As such, the number of observations will be the number of states multiplied by the number of time periods we want to study. An advantage of starting with this aggregate-level model is that it gives us a good overview of overall patterns in the data. We specify our aggregate-level model as follows:

$$Y_{st} = \gamma_s + \delta_t + \beta treated_{st} + \alpha X_{st} + \epsilon_{st}$$

where Y_{st} is the average health status for state s at time t, γ_s and δ_t are state and time fixed effects respectively, $treated_{st}$ indicates the policy is in the effect for state s at time t, and X_{st} refers to all other covariates we control for. We are interested in the coefficient β as our main variable of interest which indicates the effects of policy.

There are some general issues with the specification above that we try to control. First, we know that when the number of time periods exceed 2, we should cluster our standard errors by states as suggested by Bertrand et al. (2004). Second, we know that the population selected from each city is not necessary a random sample of the population. Fortunately, we have the propensity of each individual being interviewed in our data. Therefore, we can adjust for weights while running the model. Finally, we have the issue of the endogenous policy which we describe in greater detail later in this section.

4.3) Individual-level Model

Like our aggregate-level model, we can build our individual-level model since we have the individual-level data. We specify our individual-level model as follows:

$$Y_{ist} = \gamma_s + \delta_t + \beta treated_{st} + \alpha X_{ist} + \epsilon_{ist}$$

the only difference is that we now have the variables $Y_{ist}$and X_{ist} at the individual-level. As such, we can estimate the effect of policy using individual-level variation in our covariates as well as outcome. Again, we need to cluster our standard errors by states. Further, we must take into account the problem with the endogenous policy.

Another challenge that we face in our individual-level analysis is that not all individuals had the same probability to be interviewed in our sample. Fortunately, we have the propensity of each individual being represented by our sample. Therefore, we can control for weights to avoid selection problems caused by the sampling technique. Note that this selection problem is different from the one caused by the endogenous policy. In the next part, we discuss that problem and our approach to fix it in detail.

4.4) Endogenous Policy

As mentioned before, the assignment to the policy is not random across states. As a result, some states may have different propensities to get the treatment. If the underlying mechanism by which some states are more likely to get the treatment is correlated with the outcome, it will bias our estimates for β. For example, if states with more educated population pass this policy with a greater chance, our estimates for the effects of policy can be biased since the level of education is likely correlated with the health outcomes.

We start by empirically evaluating whether the assignment to the policy is random based on states' pre-treatment characteristics, such as average level of education, the percentage of black population and so on. We simply take the mean for each variable for observations in both treatment and control groups and see whether it is balanced across two groups. The results are presented in Table 1. By examining the p-values for the difference in means of each pre-treatment variable, we find that the assignment of the treatment is not random. For example, the average age in states that implemented the policy is higher than those that did not. This provides some preliminary evidence for the existence of endogenous policy, *i.e.*, the underlying mechanism by which the states have adopted the policy is not quite random. In the following parts, we discuss two different ways to control for this issue -- sample trimming and inverse probability weighting.

Table 1. Summary Statistics for Treatment & Control Groups Pretreatment(2013)

Pre treatment characteristics	Treatment	Control	p_value
Average Age	36.216 (22.495)	35.584 (22.386)	0.000
Percent Female	(0 .486 (0.001)	0.485 (0.000)	0.420
Percent Nonwhite	.228 (0.000)	0.218 (0.000)	0.000
Percent Married	0.403 (0.001)	.403 (0.000)	0.933
Percent Employed	0.459 (0.001)	0.456 (0.000)	0.021
Percent Enrolled in Medicaid Last Year	0.210 (0.000)	0.175 (0.000)	0.000
Percent With a High School Degree/GED or Higher	0.819 (0.000)	0.816 (0.000)	0.002
Covered by Military Health Insurance	0.040 (0.000)	0.048 0.000	0.000
Covered by Private Health Insurance	0.666 (0.001)	0.647 (0.001)	0.000
Child Covered by Medicaid Last Year	0.369 (0.002)	0.371 (0.001)	0.000

4.4.1) Sample Trimming Technique

We start with sample trimming technique. This technique is in line with our main identification strategy in Difference-in-Difference method: We are able to identify the effects of a policy in the treatment state, only if the control state has the same pre-treatment conditions. In other words, if we observe that the pre-treatment variables are the same across two groups, the differences in outcomes after the policy can be attributed to the policy.

In sample trimming technique, we try to find which groups have the similar patterns before treatment and compare those states. To do so, we first run a logit model with the treatment variable as outcome and all the pre-treatment variables as independent variables to estimate the probability that a particular state gets the treatment. We then compare those with the same propensity to get the treatment. For example, if Washington has 0.9 probability of implementing the policy and implemented the policy, while Georgia has 0.1 probability of getting the policy and did not implement the policy, we do not consider Georgia as a control group for Washington.

4.4.2) Inverse Probability Weighting

Another approach to fix the issue of endogenous policy is to use Inverse Probability Weighting, wherein we assign weight to the states based on their treatment outcome and their estimated propensity to get the treatment. Let p_s denote the estimated propensity to get the treatment for each state, we assign the weight w_s to the state as follows:

$$w_s = \frac{1}{treated_s\, p_s \;+\; (1 - treated_s)(1 - p_s)}$$

It means that we assign weight $1/p_s$if the state s gets the treatment, while we assign weight $1/(1 - p_s)$ if that state does not get the treatment. We can then give these weights to the estimator and it automatically picks the right controls to estimate the policy effects.

Section 5. Results

5.1) Aggregate-level analysis

We start with aggregate data to see if we can find any evidence regarding the effects of the policy. Please note that it is a form of preliminary result that we want to get with a smaller data. An advantage of starting with this smaller aggregate sample is that we can easily track all the observations. However, we know that it is subject to a substantial information loss, since we have individual records with variation at more granular levels.

To run our models on the aggregate data, we first build the aggregate-level data at the state-year level. As such, each unit refers to a specific state in a particular year. Since we have 51 states and 3 time periods, it translates to 153 observations. We exclude two states that have the policy implemented prior to our study (Washington DC and Vermont). So we exclude those states from our analysis which gives us a total of 147 observations.

Table 3 shows that results for our aggregate data. The first column represents the results for without the clustering of standard errors. As we can see in column 1, the effects of policy is insignificant, indicating that we cannot reject the hypothesis saying that the policy has null effect. Except for the average age of the subjects in a state-year cross section which has a

positive and significant effect, all the other covariates are also insignificant. This suggests that most of the variation is captured by state and year fixed effects.

In column 2, we cluster our standard errors by states. This is in light of the results in Bertrand et al. (2004) who show that not adjusting for the cluster structure of the standard errors would substantially bias the estimates for standard errors. Our results in column 2 show the same patterns as column 1.

In sum, our aggregate-level analysis does not provide any evidence for the effects of the policy. However, there are some problems with the use of aggregate data. First, we do not have variation at the individual-level. Incorporating more granular variation in the model would increase our statistical power for identification. Second, as we mentioned before, we might have the issue of endogenous policy. That is, states do not have the same propensity to get the treatment. While we can adjust for propensity scores and similar prior trends at the aggregate-level, it is not feasible with small number of observations. Therefore, we do this part in our individual-level analysis.

Table 3. Results for Aggregate State Level

	Column	Column
	(1)	(2)
treated	-0.00273	-0.00273
	(0.0113)	(0.0136)
age	0.0144	0.0144
	(0.00756)	(0.00776)
sex	0.278	0.278
	(0.747)	(0.890)
race	0.564	0.564
	(0.317)	(0.423)
Marital status	0.461	0.461
	(0.646)	(0.733)
employed status	0.580	0.580
	(0.402)	(0.483)
education	-0.394	-0.394
	(0.442)	(0.523)
average income	-0.000000973	-0.000000973

	(0.00000158)	(0.00000196)
Covered by Medicaid last year	0.518*	0.518*
	(0.229)	(0.239)
Covered by military insurance last year	-0.736	-0.736
	(0.475)	(0.582)
dependant covered by private insurance last year	0.856	0.856
	(0.434)	(0.529)
Covered by private insurance last year	-0.126	-0.126
	(0.274)	(0.310)

N	147	147
adj. R-sq	0.935	0.935
Standard errors in parentheses		
* p<0.1	** p<0.01	*** p<0.001"

5.2) Individual-level Analysis

Here we run the model on the individual-level data. It is worth mentioning that the high-level models are the same. Results are presented in Table 4. As shown in this table, again, we see no significant evidence for the effect of policy in any direction. However, we see some general effects from other covariates. For example, as we expected, older people have worse health status on average. On the other hand, employment, education and income are significantly linked with a better health outcome.

Models presented in column 1 and 2 refer to the models with or without clustering of the standard errors respectively. As we can see in Table 4, the estimated standard errors with clustering are generally higher than unclustered ones, confirming the concerns raised by Bertrand et al. (2004).

Table 4: Results for Individual Level

	Column	Column
	(1)	(2)
treated	0.000539	0.000539
	(0.00717)	(0.00864)
age	0.0209***	0.0209***
	(0.000118)	(0.000294)
sex	0.0124**	0.0124***
	(0.00391)	(0.00281)
race	0.0871***	0.0871***
	(0.00479)	(0.00811)
marst	-0.0512***	-0.0512***
	(0.00435)	(0.00684)
empstat	-0.270***	-0.270***
	(0.00451)	(0.00944)
education	-0.110***	-0.110***
	(0.00572)	(0.00910)
Average income	-0.00000103***	-0.00000103***
	(2.63e-08)	(6.75e-08)
receiving Medicaid last year	0.382***	0.382***
	(0.00703)	(0.0136)
covered by military health insurance last year	-0.0260**	-0.0260
	(0.0101)	(0.0221)
dependant covered by Private Health Insurance Last Year	0.00394	0.00394
	(0.0104)	(0.0140)

covered by private insurance last year	-0.256***	-0.256***
	(0.00500)	(0.00801)
N	451116	451116
adj. R-sq	0.199	0.199
*** p<0.01, ** p<0.05, * p<0.1		

5.3) Sample Trimming

As mentioned before, one major concern is the problem of endogenous policy. For example, one would say the states with more educated population tend to favor such policies more. In addition, these states have a better health outcome. As a result, our estimates for the effects of policy will be biased. We find such differences in our Table Table 1 that shows the assignment to treatment and control groups is not random.

In fact, the identification strategy for Difference-in-Difference model is that the patterns before the policy must be the same. Therefore, if the states are similar enough in their pre-treatment variables, we can attribute the changes after the policy to the policy and not the pre-treatment conditions. This is in line with the famous paper by Card and Krueger wherein they compare the outcomes for two very similar states -- New Jersey and Pennsylvania.

Here we implement a sample trimming approach based on the pre-treatment variables in the states and their propensity to get the treatment. To do so, we focus on the sample of 2013 for the states when no state has implemented the policy. We then run a logit model of the policy as the outcome (if the state will implement the policy it takes value 1) and states' pre-treatment characteristic as independent variables such as the average level of education, percent married and etc. We also add the latitude and longitude of the states to control for potential spatial patterns. We estimate the logit model and recover the propensities for all the states. That is, we estimate the probability with which the state is predicted to get the treatment based on their pre-treatment conditions. Now, if we can match states with similar propensities but different outcomes in terms of getting the treatment, out comparison is valid. We find two groups with similar range of propensities but different treatment outcomes. The propensity for the first group lies in the range [0.45,0.55], while the second group has the propensity range of [0.25,0.35].

Table 5. States with Similar Pre-treatment Patterns

Group 1: Probability to get the treatment with the range [0.25 - 0.35]		**Group 2: Probability to get the treatment within the range [0.45 - 0.55]**	
State	**Treatment Status**	**State**	**Treatment Status**
Kansas	Not Treated	Alaska	Not Treated
Kentaki	Treated	Arizona	Treated
Michigan	Treated	Delaware	Treated
North Dakota	Treated	Florida	Not Treated
South Dakota	Not Treated	Minnesota	Treated
Texas	Not Treated	Mississippi	Not Treated
Utah	Not Treated		

Inspired by these findings, we conduct our analysis on three different subsamples of the states. We first start with a very simple comparison -- North Dakota and South Dakota. It is interesting to see that one of these states got the treatment, while the other did not. The results for the model of North Dakota and South Dakota are presented in column 1 of Table. The results of this model show no significant effect for the policy.
In column 2 and 3, we focus on the sample of group 1 and 2 respectively. Again, we find no significant evidence for the effect of policy on these samples.

Table 6. Sample Trimming

	Column	Column	Column
	(1)	(2)	(3)
treated	-0.0260	0.0164	0.0372
	(0.0418)	(0.0211)	(0.0234)
age	0.0191***	0.0208***	0.0200***
	(0.000725)	(0.000315)	(0.000352)
sex	0.0225	0.0141	0.0182
	(0.0221)	(0.0103)	(0.0118)
race	0.158***	0.0928***	0.120***
	(0.0399)	(0.0135)	(0.0146)
marst	-0.106***	-0.0510***	-0.0455***

	(0.0264)	(0.0114)	(0.0131)
employed status	-0.198***	-0.253***	-0.251***
	(0.0311)	(0.0121)	(0.0138)
education	-0.117**	-0.128***	-0.0917***
	(0.0390)	(0.0147)	(0.0171)
avg income	-0.000000804***	-0.00000111***	-0.00000124***
	(0.000000159)	(8.62e-08)	(7.31e-08)
receiving Medicaid last year	0.327***	0.450***	0.358***
	(0.0562)	(0.0199)	(0.0209)
covered by military health insurance- last year	0.0824	-0.0794**	-0.0101
	(0.0504)	(0.0260)	(0.0272)
having private insurance	-0.0362	-0.00638	-0.00172
	(0.0466)	(0.0333)	(0.0306)
covered by private health insurance- last year	-0.235***	-0.261***	-0.228***
	(0.0327)	(0.0128)	(0.0144)
N	11366	67450	53896
adj. R-sq	0.182	0.218	0.214
*** p<0.01, ** p<0.05, * p<0.1			

5.4) Inverse Probability Weighting

Another approach similar to sample trimming is to use inverse probability weighting (IPW). An advantage of this approach is that we can automatically select the control groups. This is another attempt to fix the issue of endogenous policy. Here we use our estimated propensities for the states to get the treatment and invert them to construct IPWs. We can then estimate our model using these weights. Adjusting for IPWs, the results of both aggregate and individual-

level models are presented in Table 7 respectively in column 1 and 2. Again, we find no evidence for the effect of policy in those models.

	Column	Column
	(1)	(2)
treated	0.00586	0.00748
	(0.0153)	(0.00947)
age	0.0157	0.0208***
	(0.0106)	(0.000290)
sex	0.0786	0.00915*
	(0.970)	(0.00413)
race	0.317	0.0817***
	(0.340)	(0.0103)
marst	0.305	-0.0417***
	(0.673)	(0.00806)
empstat	-0.305	-0.279***
	(0.743)	(0.0118)
educ99	-0.133	-0.103***
	(0.533)	(0.00998)
offtotval	-0.00000239	-0.00000112***
	(0.00000173)	(7.56e-08)
Receiving Medicaid last year		0.390***
		(0.0155)
Covered by military health insurance last year		-0.0264
		(0.0309)
Dependant covered by Private Health Insurance Last Year		0.00567
		(0.0134)
Covered by private insurance last year		-0.259***

		(0.00795)
N	147	451116
adj. R-sq	0.926	0.201
*** p<0.01, ** p<0.05, * p<0.1		

In this step we splitted our data in smaller groups and estimate the effect of policy on sub samples of the covariates. As in table 4, we see that we have significant effect on women and percent non-white once we do the regression with clustering and including weight.

Conclusion

As of now, our results have shown no significant evidence from the effect of policy on health status. However, it is possible that the effect will express itself with delay in longer period of time.
In further studies we can collect more time periods in order to better track the effect of the policy. Moreover, we can look for the effects in different sub groups such as certain age period.

On the other hand, health status may not be a good measure to identify the effect of the policy. Since it is an ordinal variable and does not differentiate between different diseases. Also it may be affected by health cost and other outcomes. The other limitation is that CPS data does not let us to track single individual and change of their health status over time. Therefore it is hard to rely on the results from different individuals in different time. .

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