

The Labor Market Effects of Medicaid Expansion: An Analysis Based on CPS Data

Abstract

This project examines how Medicaid expansion under the Affordable Care Act affected people's income in the United States of America. Using 2012–2016 IPUMS-CPS data (University of Minnesota, 2025b), it compares expansion and non-expansion states and tracks income changes over time. While the initial model suggests a significant income gain, after controlling demographics differences and fixed differences, the income becomes smaller and statistically insignificant. However, a triple-differences model finds that low- to-middle-income individuals experienced a significant increase, suggesting heterogeneous impacts. These results suggest that Medicaid expansion may have had the greatest income benefits for economically vulnerable groups.

Introduction

In January 2014, the United States implemented Medicaid expansion under the Affordable Care Act (ACA), allowing states to extend eligibility to adults earning up to 138 % of the federal poverty level. This marked a major shift in public health policy, expanding access to coverage for low-income, childless, and working-age adults. Although the federal government initially funded the expansion in full, a 2012 Supreme Court ruling (*National Federation of Independent Business v. Sebelius*, 2012) made the implementation optional, leading to variation across states. As of 2025, 41 states have adopted the policy, while only 10 have not (Figure 3).

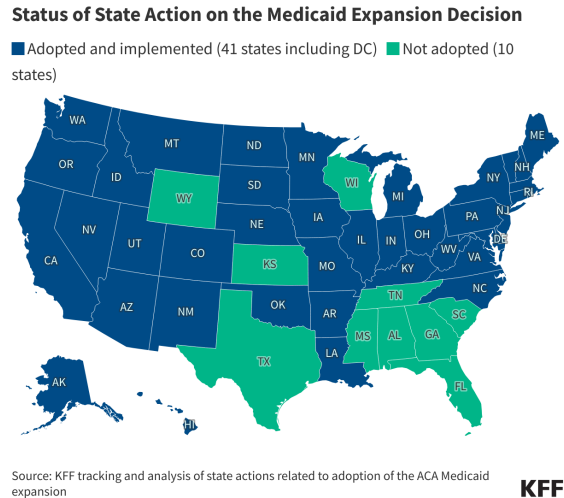


Figure 1

(Kaiser Family Foundation, 2025)

Currently, scholars are split up in regards to the labor market implications of the expansion. Supporters argue that it improves financial stability and workforce readiness by reducing economic burdens related to health (Lyon et al., 2014). Critics suggest that it may discourage work by reducing reliance on employer-provided insurance (Baicker et al., 2014). Given this debate, this essay will examine the following question:

What is the effect of the expansion of Medicaid on income?

Data

This study uses data from the IPUMS-CPS, a nationally representative survey conducted by the U.S. Census Bureau and Bureau of Labor Statistics (University of Minnesota, 2025b). The dataset spans 2012–2016, chosen to avoid COVID-related distortions and to capture short-term effects of the 2014 Medicaid expansion, which began with full federal funding. Meanwhile, to better approximate the parallel trends assumption, pre-expansion data (2012–2013) may be used to select states with similar income or labor market trajectories for comparison.

Table 1

Selected CPS Variables and Descriptions

No.	Variable	Description
1	YEAR	Survey year
2	STATEFIP	State (FIPS code)
3	ASECWT	Annual Social and Economic Supplement Weight
4	AGE	Age
5	SEX	Sex
6	RACE	Race
7	EMPSTAT	Employment status
8	LABFORCE	Labor force status
9	EDUC	Educational attainment recode
10	INCTOT	Total personal income
11	INCWAGE	Wage and salary income
12	HIUFPGBASE	Federal poverty guidelines (base)

The sample includes income, employment, demographic, and geographic variables, enabling identification of expansion status by state and year (Table 1). It is limited to working-age adults (18–64). The cleared sample consists of 591,642 individuals. Treatment states adopted Medicaid expansion on January 1, 2014; control states did not (for example TX, FL, GA). State identifiers use FIPS codes, and variable definitions follow IPUMS CPS documentation (University of Minnesota, 2025a).

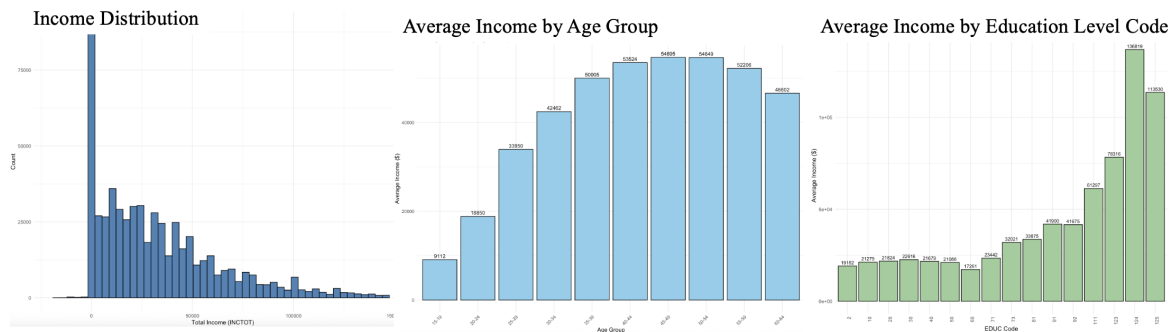


Figure 2

Income Patterns by Distribution, Age, and Education (University of Minnesota, 2025b)

To better understand the structure of the dataset, descriptive patterns of personal income among U.S. adults aged 18–64 using IPUMS-CPS data (2012–2016) are presented. Income is highly right-skewed, with a notable spike at zero—likely reflecting unemployment, part-time work, or underreporting. Average income rises with age, peaking at 45–54 before declining, consistent with career progression followed by retirement transition. Income also increases sharply with educational attainment, especially at the graduate and professional levels, suggesting that higher education leads to better-paying jobs.

Methodology

The primary source of endogeneity is states self-selection into Medicaid Expansion, which means the expansion is not random. For example, states with more generous welfare policies might be likely to expand, and also states with better economic conditions may already have faster income growth. In liberal states, the political leaning could correlate with both expansion and social support systems. These introduce correlation between the treatment indicator and the error term, which violates the key assumption of causal inference.

Estimation Strategy: From Pooled OLS to Difference-in-Differences

To answer the research question and also mitigate the effect of endogeneity, this essay estimates the causal effect of Medicaid expansion on personal income, using a series of econometric models with increasing robustness.

This project begins with a simple pooled Ordinary Least Squares (OLS) model, where Medicaid is a

binary indicator equal to 1 if the individual resides in a state that has expanded Medicaid in that year (i.e. from 2014 to 2016), and 0 otherwise. This model does not control for time and state effects, and is likely biased due to unobserved heterogeneity and omitted variable bias across states and over time.

$$Income_{it} = \beta_0 + \beta_1 * Medicaid_{it} + u_{it}$$

To address this concern, this essay adopts a Difference-in-Differences (DiD) approach. The DiD design compares the change in income before and after Medicaid expansion in treatment states (Illinois and Oregon) to corresponding change in control states (Missouri and Florida). This strategy relies on the parallel trends assumption—that in the absence of Medicaid expansion, both groups would have experienced similar income trends.

$$Income_{ist} = \beta_0 + \beta_1 * Post_t + \beta_2 * Treatment_s + \delta(Post_t \times Treatment_s) + \varepsilon_{ist}$$

- δ is the estimator of Difference-in-Differences.

To strengthen identification, this essay further implements a Two-Way Fixed Effects (TWFE) model, that includes both state and year fixed effects. The state fixed effects account for time-invariant characteristics specific to each state, such as industrial structure, historical income levels, while year fixed effects capture nationwide shocks, including federal policy changes or macroeconomic fluctuations). This report also includes individual-level covariates such as age, sex, and educational attainment to reduce omitted variable bias. Standard errors are clustered at the state level to correct for potential serial correlation and heteroskedasticity within clusters.

$$Income_{ist} = \beta_0 + \delta * (Treat_s \times Post_t) + \theta_1 * AGE_{ist} + \theta_2 * SEX_{ist} + \theta_3 * EDUC_{ist} + \lambda_s + \tau_t + \varepsilon_{ist}$$

- λ_s is the fixed effect of State
- τ_t is the fixed effect of year
- ε_{ist} is the unobservable error term

Further, this project estimates a Difference-in-Difference-in-Differences (DDD) model to examine whether the effect of Medicaid expansion on individual income varies by income level. Specifically, this report interacts the policy treatment indicator ($Treat_{st}$) with a binary income group indicator ($LowMid_i$), where: $LowMid = 1$ indicates low-income individuals whose income below \$11,170, and $LowMid = 0$ indicates middle-income individuals, whose annual income between \$34,418 and \$102,742 (ASPE Office of the Assistant Secretary for Planning and Evaluation, 2012).

$$Income_{ist} = \beta_0 + \delta_1 * Treat_s + \delta_2 * Post_t + \delta_3 * LowMid_i + \delta_4 * (Treat_s \times Post_t) + \delta_5 * (Treat_s \times LowMid_i) + \delta_6 * (Post_t \times LowMid_i) + \delta_7 * (Treat_s \times Post_t \times LowMid_i) + \theta_1 * AGE_{ist} + \theta_2 * SEX_{ist} + \theta_3 * EDUC_{ist} + \lambda_s + \tau_t + \epsilon_{ist}$$

- λ_s is the fixed effect of State
- τ_t is the fixed effect of year
- ϵ_{ist} is the unobservable error term

The regression specification includes all two-way and three-way interaction terms, as well as controls for age, sex, and educational attainment. State and year fixed effects (s and t) are included to absorb time-invariant state characteristics and common temporal shocks, respectively. The coefficient on the triple interaction term δ_7 captures the differential effect of Medicaid expansion on low-income individuals in treated states after the policy implementation, relative to their middle-income counterparts and relative to similar groups in non-expansion states.

Sample Construction and Treatment–Control Group Selection

To begin defining treatment and control groups for the analysis, this essay references Medicaid expansion implementation dates from Kaiser Family Foundation (2025). States that adopted Medicaid expansion effective on January 1, 2014 are considered candidates for the treatment group, subject to confirmation of actual rollout timelines and exclusion of early and later adopters. For states that had not adopted Medicaid expansion by the end of 2016, or did so well beyond our study window, are considered eligible for the control group. This includes states such

as Texas, Florida, Georgia, Alabama, Mississippi, Tennessee, Kansas, South Carolina, Wisconsin, and Wyoming.

Based on Sommers et al. (2013), this report excludes a number of early adopter states — such as California, Connecticut, Minnesota, New Jersey, Washington, and the District of Columbia—that expanded the Medicaid Expansion prior to 2014. These treatments are applied to preserve the integrity of the Difference-in-Differences design and to ensure the comparability of pre-treatment trends.

State Name	Expansion Start Date	STATEFIP
Virginia	1/1/19	51
Maine	1/10/19	23
Idaho	1/1/20	16
Utah	1/1/20	49
Nebraska	10/1/20	31
Oklahoma	7/1/21	40
Missouri	10/1/21	29
South Dakota	7/1/23	46
North Carolina	12/1/23	37

Table 2

States Applied Medicaid Expansion Between 2017 to 2025

To avoid bias from staggered implementation, this report excludes states that expanded Medicaid during the study period (2014–2016), including Michigan, New Hampshire, Pennsylvania, Indiana, Alaska, Montana, and Louisiana. These states adopted expansion partway through the analysis window, which could confound the estimation of the treatment effect and undermine the parallel trends assumption central to the Difference-in-Differences framework

(Abadie, 2005).

State Name	Expansion Start Date	STATEFIP
Michigan	4/1/14	26
New Hampshire	8/15/14	33
Pennsylvania	1/1/15	42
Indiana	2/1/15	18
Alaska	9/1/15	2
Montana	1/1/16	30
Louisiana	7/1/16	22

Table 3

Excluded States that Applied Medicaid Expansion Between 2/1/2014 to 12/31/2016

To strengthen the parallel trends assumption and improve comparability, this report selects two matched treatments–control state pairs based on pre-expansion income trends observed from 2012 to 2013. States were paired if their income slopes differed by less than \$300 and their baseline income levels (in 2012) were within a \$2,000–3,000 range. This matching rule follows the standard practice in DiD applications, where alignment in trends in pre-treatment time period and level helps mitigate bias from heterogeneous outcome trajectories (Abadie, 2005).

Specifically, this report retains Illinois (treatment) and Missouri (control), and Oregon (treatment) and Florida (control) as matched pairs. These states exhibit closely aligned income trajectories and similar slopes in average income growth prior to the Medicaid expansion, thereby minimizing baseline imbalances and enhancing the credibility of the DiD design.

Treatment State (FIPS)	Control State (FIPS)	slope diff	level diff
Illinois (17)	Missouri (29)	164	2482
Oregon (41)	Florida (12)	0	349

Table 4

State Pairs Selected Based on Parallel Trend Analysis

To visually assess the plausibility of the parallel trends assumption, this essay plots average income over 2012–2013 for each of the two matched treatment–control state pairs.

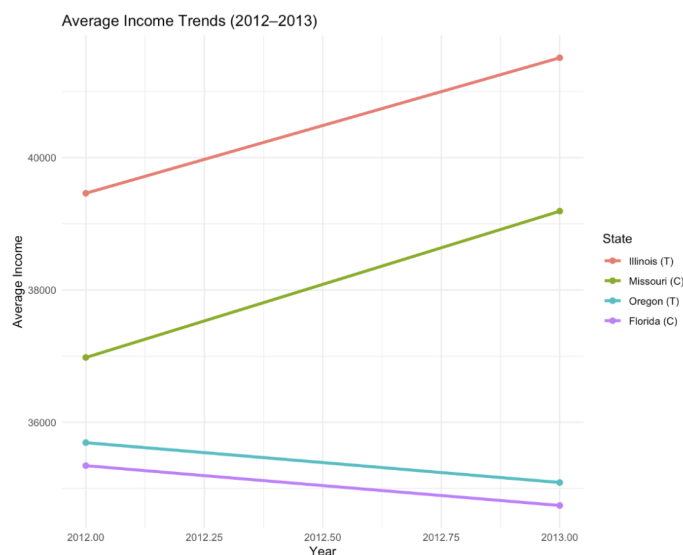


Figure 3

Parallel Trend Comparison

Remaining Identification Challenges

Despite the careful design, several empirical challenges remain that may limit the causal interpretation of our results.

Residual endogeneity: While the DiD and TWFE models help mitigate policy selection bias, they cannot fully eliminate it. States that expanded Medicaid may still differ from non-expansion states in unobserved, time-varying ways—such as local labor market fluctuations, political dynamics, or the adoption of concurrent policies.

Omitted variable bias: Although this report controls for a rich set of individual-level covariates, unobserved factors—such as health status, informal employment, or heterogeneity in how Medicaid was implemented within states—may still confound the estimated effects.

Measurement error: Misclassification of treatment status may arise due to interstate mobility, cross-state healthcare coverage, or inaccurate reporting of state of residence. Such errors could attenuate estimated treatment effects.

Parallel trends: The parallel trends assumption remains only partially testable. While the report matches treatment and control states on pre-policy income slopes and visually inspect trend similarity, unobservable divergences (deviations) in income trajectories prior to expansion may still bias the estimates.

Ideal Experiment

The ideal design would involve random assignment of Medicaid expansion across states, ensuring baseline equivalence and eliminating concerns about endogeneity or omitted variables. In reality, however, policy adoption was driven by political and economic forces, precluding true randomization. To approximate causal inference under these constraints, this report adopts a matched Difference-in-Differences framework with Two-Way Fixed Effects, leveraging pre-policy income trends to identify comparable treatment–control pairs.

Result

Table 5

Regression Results

	(1) Naïve OLS	(2) DiD (No FE)	(3) DiD + Control + TWFE	(4) DDD + Control + TWFE
Medicaid	4718.6*** (555.9)			
Post × Treat		1259.2** (220.0)	1104.4 (885.3)	-226.7** (36.8)
Post		2110.0*** (197.9)		-3.9 (29.4)
Treat		3055.0 (2007.6)		160.3** (13.4)
Age			651.9*** (66.7)	
Sex			-21737.5*** (1286.6)	
Education			773.3*** (—)	
LowMid				-32611.5*** (238.2)
Post × LowMid				-284.3* (74.2)
Treat × LowMid				-319.0 (269.2)
Post × Treat × LowMid				566.5** (92.1)
Constant	37574.8*** (284.0)	35869.3*** (986.6)	—	35893.6*** (8.3)
N	59,593	59,593	59,593	19,879
Adj. R-squared	0.0012	0.0014	0.149	0.895
State FE	NO	NO	YES	YES
Year FE	NO	NO	YES	YES
Controls (AGE, SEX, EDUC)	NO	NO	YES	YES
Clustered SE (STATEFIP)	NO	YES	YES	YES

Notes: Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

The simple pooled OLS regression indicates a \$4,719 higher annual income in Medicaid expansion states ($p < 0.001$). However, this estimate likely reflects upward bias due to omitted variables and baseline differences in income across states.

By comparing both across time and between groups, the Difference-in-Differences (DiD) design reduces this estimate to \$1,259 ($p < 0.05$), providing a more credible though modest policy effect.

With Two-Way Fixed Effects (TWFE), the estimated effect further drops to \$1,104 and becomes statistically insignificant ($p = 0.30$), suggesting that demographic differences may drive much of the initial result.

Finally, a Difference-in-Differences-in-Differences (DDD) model tests whether Medicaid expansion had different effects across income groups. The triple interaction term shows a

significant gain of \$567 ($p < 0.05$) for low-to-middle income individuals, suggesting they benefited more than others despite a modest overall effect.

Across models—OLS, DiD, TWFE, and DDD—the estimated income effect consistently declines as more controls are added: from \$4,719 in OLS to \$1,259 in DiD, and \$1,104 (insignificant) in TWFE. This downward trend suggests that earlier estimates may be biased by unobserved factors or group composition. However, the DDD model reveals a significant income gain of \$567 for low-to-middle income individuals, highlighting potential heterogeneity in the policy’s impact.

Conclusion

The results suggest that Medicaid expansion might have helped increase people’s income, but the effect becomes smaller and statistically insignificant once the report controls for fixed differences across states and individual characteristics. This implies that earlier strong effects may have reflected other factors, not just the policy itself.

However, while focusing on low- to middle-income individuals, a meaningful and statistically significant income gain was found. This suggests the policy may have had a stronger impact on more economically vulnerable groups. Our analysis depends on the assumption that treatment and control states would have followed similar income trends without the policy. If that assumption doesn’t hold, the estimates could still be biased. Future work should incorporate placebo tests and explore heterogeneous effects to better understand the income implications of Medicaid expansion.

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