

How Magnetic can Welfare be?

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Abstract

Twenty states expanded Medicaid eligibility to low-income childless adults in 2014. Did this large and costly expansion of welfare attract these newly-eligible adults to expansion states? By merging administrative tax records, Medicaid enrollment records and survey data, I find that 4.7% of these adults move interstate annually, over twice the rate reported in the Current Population Survey. Nevertheless, both state-level and border-county difference-in-differences designs detect no statistically significant impact of Medicaid on migration over the first five years. These estimates are precise enough to reject substantial budgetary or welfare costs or benefits from migration. In contrast, I find that the same subpopulation migrated substantially in response to Great Recession local shocks. This appears difficult to reconcile with the value of Medicaid. However, it may be explained by the fact that a newly-eligible adult gains less than 2 years of Medicaid enrollment in the 5 years after moving to an expansion state, or by the confusion about enrollment that I document in survey data. Regardless of the explanation, the welfare magnet effect of Medicaid expansion was negligible.

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1 Introduction

Policymakers have been concerned about migration responses to local welfare programs for centuries and remain so today. Economists are likewise interested in so-called “welfare magnet” effects because they help quantify the deadweight loss from place-based policies, the local government incentives to race-to-the-bottom, and the potential benefits to eligible individuals through migration. Not only the existence of welfare magnet effects, but their size and deterrents are important to understand so long as government transfer programs are implemented at the sub-global level.

The most expensive means-tested transfer program that differs from state-to-state in the United States (U.S.) is Medicaid, a government-sponsored health insurance program. It is an order of magnitude more expensive than programs such as the Earned Income Tax Credit (EITC), food stamps (SNAP) or cash welfare (TANF). Prior to the passage of the Affordable Care Act (ACA), able-bodied prime-age childless adults were not eligible for Medicaid at any income level in almost every state. I will refer to adults with these characteristics and incomes below the eligibility threshold as “newly-eligible” (regardless of whether they live in an expansion state). In 2014, the primary year of ACA expansion, 20 states allowed Medicaid enrollment for these newly-eligible adults for the first time, thereby creating substantial state-level differences in welfare that have persisted for a decade. This variation is much larger than that induced by reforms to Aid for Families with Dependent Children (AFDC), the program most frequently used to study welfare magnetism in the U.S. (McKinnish, 2005).

My paper also improves previous work by constructing better data. Most existing research on welfare magnets either make clever use of cross-sectional survey data (e.g. Goodman (2017)) or structural models on small panels (e.g. Blank (1988)). I combine administrative data from the Internal Revenue Service (IRS) and Centers for Medicare and Medicaid Services (CMS) to create a long panel of individual-level data that captures address, eligibility and Medicaid enrollment information for the universe of adults. This new dataset allows me to avoid the complications caused by income-adjustment for eligibility and other potential compositional confounds in cross-sectional data, and also gives me the ability to examine geographies and subsamples unavailable in smaller panels.

Basic descriptive facts from the administrative data suggest that the Current Population Survey (CPS) and the American Community Survey (ACS) under-report migration, and thus studies that use these data and migration as an outcome may suffer from attenuation bias. In the panel I construct using federal tax information (FTI), the average migration rate for adults from 2010-2019 is 3%, more similar to what was reported by the Current Population (CPS) in the 1970s than what is reported to the CPS today (Kaplan and Schulhofer-Wohl, 2017). This is because fewer of the moves identified in administrative datasets, such as tax data and the postal service, are reflected in the ACS and CPS over time, and because response rates are falling for each survey (Foster et al., 2023). I show that the average interstate migration rate for those newly-eligible for Medicaid is 4.7%, over twice the rate reported in the CPS, and roughly 75% greater than what is reported in the ACS. It is also much higher than the migration rate for higher-income households, despite their presumptive greater ability to weather moving costs.

Having established that low-income households are highly-mobile, I next turn to estimation

of the migration response to Medicaid. A simple way to examine this would be by comparing the eligible population in expansion to non-expansion states. Unfortunately, the non-expansion states (largely in the Sun Belt) have long been growing faster than expansion states (including the Rust Belt), so they do not evolve similarly in the pre-period. Looking at contemporaneous eligibility may also muddle conclusions, because people may adjust their income when Medicaid becomes available. I therefore define my sample prior to expansion (in 2012), and I begin by using a difference-in-difference design on the number of newly-eligible adults residing in contiguous border counties. The population of people in border counties with newly-eligible characteristics did evolve similarly in the before expansion, and their proximity makes them a plausible counterfactual afterwards.

Consider, for instance, St. Louis, Missouri (non-expansion) and East St. Louis, Illinois (expansion), which are across the river from one another, and are connected by several bridges as well as bus service. They have similar average tax income rates for low-income childless adults, EITC benefits, and sales taxes. They are presumably similar in terms of proximity to family, favorite restaurants etc. For the past 10 years, a poor household, which I show relocates every four years on average, could move across the bridge and get healthcare that costs approximately the same to provide as their annual income. I show that take up of Medicaid for this population in expansion-side border counties is much relatively high (about 26% annually), suggesting it has some value. Nevertheless, when I compare the population of newly-eligible adults within contiguous border-county pairs, I can reject any meaningful population increase in the first five years after expansion. This null result holds for both in- and out-migration, restricting my analysis to border-crossing Commuting Zones, and including late-expanding states. A triple-difference with people who are ineligible due to a higher income, and a regression border-discontinuity design also affirm these results.

To supplement this model, I also estimate a difference-in-difference regression on the state-to-state flows of newly-eligible adults. A standard location-choice model predicts that expansion would cause the share of people moving to treatment states to increase, and the share moving to control states to decrease. This motivates a market-share-style model that is frequently used in migration studies (e.g. [Moretti and Wilson, 2017](#); [Muñoz, 2021](#)). It has a direct utility interpretation, and can capture effects in non-border counties. When I estimate it, however, I see no change in the direction of interstate flows, and can reject any population increase with similar precision to my primary design. This confirms my border county results and demonstrates that the null is not a peculiar feature of border areas.

The null result also holds across a variety of subsamples, including the particularly low-income (who may benefit more from Medicaid) or the particularly mobile (who may face lower costs). I cut my data not only by characteristics observable in my full sample using federal tax information (FTI), but also the characteristics that can be observed by merging to the ACS and the CPS Annual Economic Supplement. The CPS, while small, allows me to look at indicators of health. In general, these results are similarly null, however, my results for those reporting “poor health” suggest the possibility of an effect, which I am not powered to reject outright.

It is enough, however, to answer the primary policy question of this paper: welfare-induced migration is of little concern in the case of Medicaid. Based on the upperbound of the 95%

confidence interval, I can reject increases in population of more the 0.56% in border counties, where previous research suggests migration would be highest (McKinnish, 2007). This represents a substantial increase in precision over the nearest-approximation of this regression in the ACS, which has standard errors over twice as large (despite clustering), and can only reject state-level population increases of 2.71%. It strengthens the null results found in previous work based on survey data (Schwartz and Sommers, 2014; Goodman, 2017) because it does not suffer from measurement error, and is over a longer time period. If (say) Texas expanded Medicaid to low-income childless adults, then I can reject increases in enrollment due to migration greater than about 900 adults, or a 0.01% increase in the current Medicaid budget for Texas five years after expansion. This increase would be 3 times larger if all of the new migrants had Medicaid expenses similar to those who report “poor health” (Cox et al., 2024), though it would remain small.

This null result may be surprising given that researchers have found population responds to local economic conditions, such as recessions (Yagan, 2019; Cadena and Kovak, 2016; Blanchard and Katz, 1992) or trade shocks (Autor et al., 2021). One potential difference between welfare magnets and these other shocks is that welfare magnets effect low-income individuals, who may have different migration elasticities. I test this using an difference-in-differences design similar to my primary Medicaid specification. In particular, I show that people who match the characteristics of newly-eligible adults in 2006 respond significantly to heterogeneity in statewide employment shocks from the Great Recession. In the style of Blanchard and Katz (1992), I use the state-level employment deviations estimated by Yagan (2019) to show that states that faced a 1 p.p. employment rate decrease from the Great Recession saw a net decrease of 0.31% (s.e. 0.10) at its nadir of the pre-period newly-eligible adult population. This corresponds with a \$752 (s.e. 177) decrease in cumulative earnings. This shows that this group does respond to economic incentives—about a 0.4% population increase for every \$1,000 increase—as well as providing a point of reference for the migration response to any potential government transfer program¹.

Suppose one assumes that migration ensures enrollment in Medicaid in all future years, information is perfect, and Medicaid shocks are valued similarly to Great Recession shocks relative to moving costs, then my estimates could reject an annual value of Medicaid greater than \$579. This upperbound is significantly less than just the reduction in out-of-pocket expenditures from Medicaid estimated from the Oregon Health Experiment (ignoring any improved care or insurance value) or 8% of the cost to the government². However, this value assumes that migrants to a new state would remain continuously enrolled in Medicaid.

I next measure the explicit enrollment payoff to migrants. In particular, I consider the people best situated to take maximum advantage of expansion: those newly-eligible adults who move from non-expansion to expansion states in 2014 and remain in expansion states through the end of my analysis period. Rather than gaining 5 years of eligibility and enrollment, these individuals gain 1.8 years of enrollment, slightly less than people who simply remained in expansion states the whole time. Thus, assuming that migrants get permanent access to welfare leads to an over-

¹Dollar values are inflated to 2023 dollars using the CPI-U unless otherwise noted

²See Section 7 for the details of these calculations

estimation of the incentives. Accounting for this churn implies an annualized Medicaid value of about \$1,736 at the upperbound of the 95% confidence interval. This is roughly consistent with other estimates of the value of Medicaid for this population (Finkelstein et al., 2019). Of course, this is an upperbound, and relies on several assumptions, nevertheless, it supports accounting for eligibility churn and imperfect take-up in considering the incentives for welfare magnets.

Information costs may be another reason for my null. To quantify the difficulties of understanding Medicaid eligibility, I merge the set of newly-eligible adults who respond to the ACS to those who appear in the Medicaid enrollment records in the same month. Of those who are verified to be enrolled in Medicaid in the administrative data, 30% incorrectly report to the ACS that they are not on Medicaid and 14% claim to have no health insurance at all. This reflects considerable confusion about Medicaid enrollment, even among its recipients.

In summary, I find no meaningful impacts of Medicaid expansion on the migration of newly-eligible adults, and that Medicaid magnetism is not a relevant concern for budget projections. When previous papers have found null results, they typically appeal to moving costs. Yet, the fact that low-income people move more than high-income people, and respond strongly to the Great Recession, makes moving costs less satisfying as an explanation. My paper suggests two others, (1) eligibility churn and imperfect take up reduce incentives for migration, and (2) information frictions make incentives unclear. Thus, though this paper affirms that people respond to incentives, it also suggests that, to the extent that future welfare expansions are limited by stringent means-testing, or not very salient, migration responses will be muted.

This paper contributes to several strands of economic research. First, and most straightforwardly, it adds new data, evidence and stylized facts to the long literature on welfare magnets. In the U.S., many papers focus on Aid for Families with Dependent Children (AFDC) reform (McKinnish, 2005; Bailey, 2005; Kaestner et al., 2003; Meyer, 2000; Moffitt, 1992), and yield conflicting results.³ Closest to my paper are Schwartz and Sommers (2014), who study early individual state-Medicaid expansions in the CPS ASEC, and Goodman (2017), who studies migration responses in the early months of the ACA in the ACS (observing his median respondent around June 2014). In contrast to these studies, my work considers a broader set of subsamples, a longer post-treatment period, and does not rely on surveys that under-report migration. My considerations of mechanisms might also provide evidence on why these estimates are lower than recent work on international migration (Agersnap et al., 2020), which I discuss in greater detail in Section 8.

My findings on welfare magnets naturally connect to broader concerns in the public, labor and urban economics discourse concerning fiscal federalism and place-based economic policies (Gaubert et al., 2021; Bartik et al., 2019; Kline and Moretti, 2014; Roback, 1982; Tiebout, 1956). My results imply that dead-weight loss due to migration induced by local programs is low, as are any gains to would-be recipients. My findings also add detail to what frictions might preclude the common assumption that utility is equalized across places, at least in the medium-term. They add to the limited set of studies on in-kind transfers, rather than the more prevalent studies on economic shocks or taxes.

³In his early review, Meyer (2000) notes that the range is from roughly no impact on migration (Levine and Zimmerman, 1999) to a near-doubling of out-migration in response to a roughly \$1,200/year difference in benefits (Enchautegui, 1997).

In service of its main findings, this paper produces valuable statistics for the ongoing debate concerning the apparent decline in internal migration, and its patterns across demographics (Olney and Thompson (2024); Jia et al. (2023); Basso and Peri (2020); Kaplan and Schulhofer-Wohl (2017); Molloy et al. (2011)). Most research in this area focuses on validating or ruling out trends such as demographic shifts or the dispersion in housing prices, whereas this paper joins a smaller body of work outside of economics emphasizing measurement error (Foster et al. (2023); Hyatt et al. (2018)).

The following section of this paper provides some brief background on Medicaid expansion. Section 3 describes my data sources, samples and the construction of key variables. Section 4 relates novel descriptive statistics on migration by demographic characteristics. Section 5 presents my primary results on Medicaid expansion. To benchmark these results, I estimate a response to the Great Recession in Section 6. I detail the incentives for migration and the budgetary consequences of migration in Section 7. Section 8 discusses the place of this paper in the literature, and Section 9 concludes.

2 Background

Prior to the passage of the Affordable Care Act, most means-tested healthcare in the U.S. was geared exclusively towards families, and largely towards children. Entering 2004, when the analysis period of this paper begins, states varied widely in terms of eligibility for Medicaid. Income cutoffs for parents ranged from 13% to 200% FPL, while those for children ranged from 100% to 300% FPL, although some states had enrollment freezes for each group (Ross and Cox, 2004). In every state except Vermont, there was no level of income, however low, under which childless able-bodied adults could qualify for Medicaid (Burns et al., 2017).⁴

The Affordable Care Act, passed in March 2010, was designed to change this. Congress mandated that each state expand Medicaid coverage to all people—regardless of their disability, assets, or parental status—who earned under 138% of the Federal Poverty Line (FPL). People who earned between 100% to 400% FPL could purchase subsidized private insurance, and people without insurance would be fined⁵. These changes were intended to take effect in 2014. In June 2012, however, the United States Supreme Court issued a ruling that allowed states to elect not to expand Medicaid. In states that chose not to expand, childless adults remained without publicly-provided healthcare. Those who earned *over* 100% FPL could theoretically purchase insurance on the new subsidized exchange, but navigating the process to apply was and is difficult, and healthcare is still not free.

The result was that only 20 states expanded coverage in 2014. This split several areas (e.g. the Washington DC, El Paso and St. Louis commuting zones) such that only some of their resident low-income childless adults became eligible for Medicaid. As stated above, Medicaid is

⁴There were several Waiver 1115 programs, which were capped and offered various benefits. Other states developed Medicaid-like programs for childless adults between 2004 and the ACA expansion.

⁵This fine, known as the “individual shared responsibility provision” or “mandate,” phased in at a nominal \$95 in 2014 and \$325 in 2015 before reaching \$695 in 2016 (Eibner and Nowak, 2018). The federal fine was set to \$0 in 2019 (after my analysis period), although several states enforce their own fines. I present the fine as a dollar amount because that is binding for low-income individuals. The fines could be a fraction of income for higher-income individuals. This fine and similar ones at the state level seem to be effective in encouraging enrollment (Fiedler, 2020).

the largest means tested program in the United States, so the difference in government transfers is large no matter how one describes it. The average annual cost to the government is \$7,047, and the willingness-to-pay (WTP) (although lower) is still sizable. I will discuss the WTP more in Section 7, but to mention just one value now, [Finkelstein et al. \(2019\)](#) estimate that it approximately \$2,379 a year for individuals earning under 100% FPL. This value is roughly 33% of income for my sample. Compared to both the decrease in variance of AFDC benefits ([McKinnish, 2005](#)) used in older studies on welfare magnets, and as a percentage of income relative to many of the studies on high-earner migration ([Kleven et al., 2020](#); [Young and Lurie, 2022](#)).

The initial plan for covering the cost of Medicaid expansion was that the federal government would reimburse 100% of costs of expansion for 3 years. The reimbursement rate would then decline until it leveled out at 90% of the costs ([MACPAC, 2022](#)). Thus every state that expands Medicaid can expect to pay some of the cost, and this cost increases in the number of people enrolled. A decade later, Medicaid expansion and its costs are still being actively debated around the country. In non-expansion states, the debate concerns ACA expansion, with a measure almost passing in Mississippi this year ([Hawkins, 2024](#)). In expansion states, the discourse centers on whether to expand coverage to new groups not covered by the ACA. In both cases the threat of welfare migration looms large for opponents, as it has for opponents of welfare since at least the 1600s.⁶ For instance, when California voted to expand coverage to undocumented immigrants in 2019, State Senator Jeff Stone warned, “We are going to be a magnet” ([Allyn, 2019](#)). The remainder of this paper concerns whether Medicaid expansion did induce migration, and to what extent we might expect similar policies to do so in the future. With variation this large, one just needs the appropriate data to study it.

3 Data

3.1 Data Sources

The data for this project come primarily from anonymized tax records given to the Census by the Internal Revenue Service. These records include some fields from the basic individual tax form in the U.S., the Form 1040, as well as several information returns. Information returns are sent to IRS automatically by third parties for things such as being employed or working as an independent contractor, having an interest-yielding bank account, or receiving unemployment insurance. The breadth of information returns means that they capture most adults living in the United States at any given time. In 2012, for instance, individuals appearing in the combined forms represented 97.5% of the Census estimate of the total adult population. I refer to the Form 1040 data and all the information returns collectively as “FTI” (Federal Tax Information). The full set of tax forms is available from 2005 to 2018, although certain variables are available for longer time periods, which I discuss below.

⁶For instance, the 1662 revisions to the English Poor laws begin: “[By] Defects in the [previous] Law, poor People are not restrained from going from one Parish to another, and therefore do endeavour to settle themselves in those Parishes where there is the best Stock..., and when they have consumed it, then to another Parish, and at last become Rogues and Vagabonds, to the great Discouragement of Parishes to provide Stocks, where it is liable to be devoured by Strangers”

The person associated with each anonymous tax record is assigned a random unique number, called a personal identification key (PIK) that consistently identifies that person across all Census datasets. I can therefore use this PIK to link the tax data to birth and death records from the Social Security Administration (SSA), the Decennial Census, the American Community Survey (ACS), and the Consumer Population Survey Annual Social and Economic Supplements (CPS). I use this same PIK to merge to the CMS MSIS and T-MSIS.

Appendix B discusses harmonizing all of these variables over time in detail, but I outline the process for a few key variables below. The most important variables to observe for a study of welfare magnets are location and eligibility. In the case of Medicaid, eligibility is determined by income relative to the FPL, and household structure.

3.2 Construction of Key Variables

Location: Tax returns for income in year t are typically filed in year $t + 1$. On the Form 1040, the tax filer puts their own mailing address, and information returns are sent by the third-party reporter to whatever address they have on file. For instance, the W2 (reporting salary and wages to the IRS) is sent by an employer to their employee to the same address they send their paychecks. Most tax filers receive information returns in January or February of year $t + 1$, and file in March or April of year $t + 1$. Thus, when I refer to someone’s location in year t , I glean this information from their tax return on income earned in year $t - 1$.

On each of these forms, I can see the state and zip code of this address, as well a Census-assigned random number associated with their unique mailing address. This number is known as a Master Address File Identification Number (MAFID). I use this MAFID or the zip code to assign each address to a county.

If a filing address is missing, I use the modal address from that same individual’s information returns, breaking ties randomly. If that address is missing, I use information from their spouse’s information return. In this way, I can match 91% of people in the tax data the average year to a valid state and zip code and 75% to a valid MAFID.

If I cannot determine an individual’s address in a given year, I use other years of tax data. If their most recent and next non-missing address are the same, I set the intervening missing years to that address. If the address has changed, I assume that that change happened at the midpoint of the spell of missing years, breaking ties randomly if the spell lasts an even number of years.

Household Income: If an individual or a spouse has filed a Form 1040, I set their income equal to their Adjusted Gross Income (AGI). If neither filed a Form 1040, I set their income equal to the sum of all W2 wages from both spouses. Similar to other papers using FTI (e.g. Wyse and Meyer (2023)), if an individual has no 1040 or W2, I assign her an income of zero.⁷ I winsorize income at the 99th percentile within year. For individuals with negative AGIs, which typically result from tax strategies used by the wealthy, I topcode their income.

Household Structure: I determine household structure using an individual’s most recently filed Form 1040. The 1040 lists spouses and dependents. I then merge these records to birth

⁷Wyse and Meyer (2023) also use income from the 1099-R (for retirement income), but this is not available in my project space at Census. Retirement income seems unlikely to be important with my age restrictions.

year records from the SSA. If an individual does not file in a given year, I use information from the most recent previous year available. I assume this person remains married and use the birth year information to infer future age, and thereby determine the number of children under 19 in the household.⁸

Relative Poverty: The Federal Poverty Line is published each year by the U.S. Department of Health and Human Services (not to be confused with the definition used by the U.S. Census Bureau). The line is uniform across the continental U.S., with slight modifications for Alaska and Hawaii. In 2012, it was a nominal \$11,170 for a one-person household, with \$3,960 added for each additional person in their household. It increases every year with inflation.

Eligibility for Medicaid is measured in terms of household income relative the FPL. The income definition used is called “Modified AGI.” Modified AGI is similar to the AGI variable I observe, but it adds back in certain forms of income that the U.S. government does not tax, such as money earned abroad. Since I cannot observe Modified AGI, I simply use the household income concept I describe above. The two will be very similar for most people. Another caveat is that someone who becomes unemployed will also become eligible for Medicaid, even if they have earned enough in that year to exceed 138% FPL. This is only a Type 2 error, so almost everyone I flag as earning under 138% FPL should be eligible.

Medicaid Enrollment: I have access to monthly enrollment data from the Medicaid Statistical Information System (MSIS) and the Transformed MSIS (T-MSIS). These include the number of days that a person is enrolled in a given month, and their basis of eligibility, allowing me to identify disabled adults. Unfortunately, they do not contain information on claims or healthcare spending. Years earlier than 2009 are unreliable for the MSIS, and so I do not use these data⁹.

Each of these datasets is collated by the CMS from quarterly reports from each state. Between 2014-2016, states gradually transitioned from the MSIS to the T-MSIS. The T-MSIS data is higher quality. The T-MSIS is reported based on calendar quarters rather than quarters of the federal fiscal year used by the M-SIS. Thus, I occasionally do not observe a quarter of data during the transition. For simplicity, I avoid the issue of dropped quarters by making my outcome variable “any enrollment” during the calendar year. [Miller et al. \(2021\)](#) carefully impute values for missing quarters, and it does not alter their results on Medicaid enrollment.

3.3 Sample

Descriptive Statistics Sample: This sample mimics the CPS and ACS in that it is primarily concerned with migration rates over the previous 12 months. It includes a 10% sample of the full population with a tax record in that year, without further restrictions.

Medicaid Sample: This full population sample includes everybody who appears in the tax data and is alive in 2012. The panel is strongly-balanced and based only on information known in 2012. This construction has the important advantage over the previous cross-sectional literature in that my results cannot be driven by any compositional changes, or endogenous shifting of

⁸If a person has never filed in a previous year, I assume they have no spouse or children for poverty calculations and that they have children for the purposes of my “childless adults.” Both assumptions are conservative in the context of my design, since they effectively exclude this negligible number of never-filers from being in my primary analysis sample.

⁹I understand this from conversations with experts at the Census Bureau.

income to participate in the program.

I make a number of restrictions so that these individuals are precisely the ones most affected by the spatial heterogeneity in Medicaid eligibility. I will describe this expansion in the following section, but it pertains mostly to childless adults, so I begin with this restriction. Non-citizen residents have different and less coverage than citizens, so I restrict to people who were citizens in 2012. I restrict my main analysis to people from the 1953 to 1984 birth cohorts, who are ages 28-59 by the end of 2012. This ensures that, for several years around expansion in 2014, these people will be eligible neither for their parents' insurance coverage (which is limited to children 26 and younger), nor Medicare, a type of government-provided health insurance that is available to people 65 and older in all states.¹⁰ I exclude people who are categorically eligible for Medicaid due to disability (as reported in the CMS data in 2012), since they are also eligible in all states. Finally, I restrict to people who earn under 138% FPL in 2012. This does not guarantee that they will remain under this income cutoff in 2014, and thus become eligible for Medicaid in some states, but defining the sample in 2012 prevents confusing effects from people who alter their income to become eligible with those who move to become eligible. The effects of each of these sample restrictions are shown in Appendix Table B.1. My final analysis sample contains approximately 12 million people.

Great Recession Sample: This sample includes everybody who appears in the tax data and is alive in 2006. To mimic my Medicaid sample, subsamples based on individual characteristics use only information known in 2006. The sample is strongly balanced over time.

4 Descriptive Statistics

The demands on the ideal dataset for studying welfare magnets are substantial. In particular, one needs a panel of addresses and of all criteria for eligibility and enrollment. For each of these variables in the context of Medicaid expansion, I find important measurement issues.

I focus particularly on issues in reporting migration. Both CPS and ACS show issues of under-reporting that are getting worse over the analysis period. I also emphasize that the group that becomes eligible for Medicaid is highly mobile, and more so than their higher-income counterparts.

4.1 Measurement Issues in Public Data

4.1.1 Migration

The three main publicly-available datasets for studying migration in the United States are the CPS, the ACS, and the county-to-county flows released by the IRS. These result in very different measurements of interstate migration. Table 1 shows the average interstate migration rate from

¹⁰Younger and older adults are eligible for Medicaid. However, for those under 26, it is common to be on parental insurance, and I cannot observe whether they receive this insurance. Seventeen percent of people on Medicare were also on Medicaid in 2023, with Medicaid acting as a backstop for when Medicare benefits run out, or there are gaps in coverage (Peña et al., 2023). However, Medicare also has relief provisions for low-income beneficiaries. The rules for Medicaid eligibility do not match those for younger low-income adults, and the elderly also often qualify through SSI. In general, eligibility for seniors is more geographically uniform, and the incentives for migration are less clear (Musumeci et al., 2019).

2010-2019 for each of these datasets. The estimate from the CPS is 1.5% whereas it is 2.4% in the ACS.¹¹ (Although it is not shown on the graph, the county-to-county flow rate is 2.42%.¹²) I estimate that interstate migration is 3.0%, higher than any of these, and closer to the CPS migration rate in the 1970s (Kaplan and Schulhofer-Wohl, 2017). I will discuss the problems with each of the public datasets in turn, and why my data is preferable for studying low-income migration.

CPS: Researchers have long understood that the CPS undercounts migration rates, although the reason is not entirely clear. The discrepancy is especially puzzling in comparison to the ACS, which asks nearly the same question (Molloy et al., 2011).¹³ Unfortunately, this undercount is getting worse over time. One reason seems to be response rates. Hyatt et al. (2018) merge CPS records to the Longitudinal Employer-Household Dynamics data and demonstrate that people who move in the administrative data are becoming increasingly less likely to respond to the CPS. Foster et al. (2023) show that only 70% of people responded to the CPS in 2018, and only 52% responded to the migration question—in contrast to 73% a decade earlier. In other words, migration is missing for roughly half of all individuals. Appendix Figure A.1 shows that the imputation rate (conditional on responding to some questions) has more than doubled in the past twenty years, and the pattern has been similar across income groups.

ACS: The ACS, to which sampled households are legally required to respond, enjoys a much higher overall response rate. However, the trends of decreasing response rates and capturing fewer of the moves reported in administrative data are the same as in the CPS (Foster et al., 2023).

In the early years of the ACS, it appeared that it tracked the IRS county-to-county flow files fairly well, despite measuring migration differently.¹⁴ However, the two datasets have diverged over the past two decades. Foster et al. (2023) argue that this increasing difference reflects declining number of moves captured in the ACS. They show that the response rate for ACS migration question has fallen roughly 12% since its inception, and that a decreasing fraction of moves registered with the U.S. postal service or IRS are reflected in the ACS. Figure A.1 shows imputation rates increasing steadily over the analysis time period.

IRS County-to-county flows: While this paper and the IRS county-to-county flows both ultimately derive their data from federal tax records, there are several important differences in

¹¹These are averages for individuals 18 or older. Following convention (e.g. Molloy et al. (2011)) these calculations exclude group quarters and imputed values. In general, residents of group quarters have slightly higher migration rates, but neither this nor the imputation reconciles these estimates.

¹²This rate is calculated using the methodology from Foster et al. (2023), which uses exemptions, and excludes 2015 and 2017 as aberrations.

¹³Ihrke et al. (2015) comprehensively catalog the differences. First, there are differences in sampled population, such as the exclusion of the armed forces from the CPS. Second, prior to 2016, they differed in sampling frame. Both begin with the most recent decennial census as a sampling frame, the ACS adds households using new postal addresses whereas the CPS uses building permits. The CPS now used the same address file as the ACS. Third, since all data presented here is from the ASEC, all households have previously been contacted as part of the CPS Basic, whereas the ACS is contacting households for the first time. As Molloy et al. (2011) note, however, the CPS marks a household as vacant after a single visit, while the ACS follows revisits apparently vacant housing for up to three months. Fourth, there are some differences in when the moves are counted, although this matters less for my 10-year average.

¹⁴Specifically, the ACS asks people if they moved over the previous 12 months, surveys an equal number of people in each month, and defines residence as a place one plans to reside for at least two contiguous months. The county-to-county flows use the address each household files their taxes from. Most taxes are filed in March in April, and does not include non-filers.

data construction. The most important in the context of welfare magnets is that the county-to-county flows file excludes non-filers. Low-income individuals are not legally required to file taxes, and often do not. In my sample, the mean household income for Medicaid-eligible individuals was a nominal \$5,381 in 2012, below the cutoff of \$9,750 over which filing taxes is required for singles, and well below the \$19,500 cutoff for joint-filers (IRS, 2012). Another subtle difference is that the county-to-county flows pulls the most recent filing address, whereas I use the original¹⁵. This difference may lead to sizeable differences in years with lots of amended filings, or years in which the flows data was built later after tax season. Certain years of the county-to-county flows files (e.g. 2015) exhibit unlikely spikes in migration that might be caused by this or other issues. In any case, the county-to-county flows data is ill-suited to studying welfare magnets because it does not contain information on migrant income.

Other administrative datasets: I am not the only person to calculate migration rates in administrative data sources. Young and Lurie (2022), working directly with the IRS data at the U.S. Treasury, construct an address panel using a more extensive set of information returns, and estimate average annual interstate migration rates between 4% and 6% for low-income groups. Estimates from credit report panels are less consistent over time, and perhaps less reliable. DeWaard et al. (2019) estimate that interstate migration rates are similar to those calculated with IRS data in the mid-2000s, but drop below estimates in the ACS in mid-2010s. Credit report estimates are likely lower bounds regardless, because the population with credit history tends to be older and wealthier (Holmes, 2022).

Individual Tax Records: As described in Section 3, my data is constructed from a combination of filings and information returns. In comparison to even the county-to-county flows, it is more comprehensive. Importantly for welfare research, it captures even low-income non-filers. My sample is 100 times larger than the ACS and 3,000 times larger than the CPS, allowing finer subsample and spatial analysis. Return issuers, such as employers, have an incentive to ensure the address is correct, as do states for filers. Filing rates and the likelihood of appearing on an information return are consistent over the analysis period. The panel nature of my data also allow me to avoid several of the issues prevalent in earlier papers using cross-sectional data, such as not being able to distinguish whether increases in the eligible population come from income adjustment or migration responses (Meyer, 2000).

My data is not perfect. My estimates may undercount migration, because I do not capture dependents moving out of their homes. This difference is unlikely to be important in my analysis sample, which excludes people under 28. Furthermore, brief, intra-annual moves, such as living in a dorm for a semester before returning home, will not be captured in my data because taxes are only filed annually. These moves seem unlikely to be welfare-motivated. Lastly, tax-filers may try to skirt the law and report their address in strategically lower-tax state. To the extent that this happens, however, it seems much more likely among high-earners. Misreporting an address on a tax form is unlikely to be sufficient to gain access to Medicaid because residence must be proved independently as part of the application process.

Thus, my data represents an important improvement over datasets used in previous research. In the next section, I demonstrate that it is substantially better at capturing low-income moves.

¹⁵I understand this from conversations with experts at IRS

4.1.2 Income Eligibility

Both the CPS and ACS rely on recall of income over the past 12 months at the time of the survey, and there are no incentives for accuracy. Tax records, in contrast, rely much less on human memory, and there are strong incentives for accuracy.

Furthermore, it has long been understood that surveys of income suffer from high non-response rates the tails of the income distribution (Lillard et al., 1986; Bollinger et al., 2019) and overestimate poverty rates (Meyer et al., 2021). Meyer et al. (2020) estimate AGI from CPS responses and compare that to reported AGI on merged tax records. They find that households in the bottom income decile in the CPS underestimate their income by almost 80%.

Moreover, by observing tax records, I plausibly observe the exact same information as Medicaid enrollment administrators. Many states explicitly suggest providing tax forms in their recommendations for applying for Medicaid.¹⁶ In my data, only 58% of people who match the eligibility criteria in the ACS also match it in the tax data, mostly due to misreported income.

4.2 Migration Patterns across Demographics

4.2.1 Trends in Surveys and FTI

Table 1 shows that insights into demographic patterns in interstate migration hold for the CPS (Column 1), ACS (Column 2) and FTI (Column 3). People with more education move more than people with less (Notowidigdo, 2020). Renters move more than homeowners, younger people move more than older people, white people move more than black or Hispanic people, and women and men move about the same amount (Molloy et al., 2011).

Comparing Columns 1 and 2, the public data, with Column 3, the FTI, a pattern in the measurement differences appears. In each demographic cut, the lower income group sees a larger increase in migration rates in the FTI relative to the ACS. For people with less than a high school education, the migration rate increases 0.6 p.p., whereas the migration rate increases by only 0.2 p.p. for people with a bachelor’s degree or above. A similar pattern holds true for age and homeownership. In general, however, the tax data and the ACS follow similar trends with only a difference in magnitude.

One final caveat in comparison to previous work is that, occasionally, these migration rates are presented across differences in individual income. This can flatten the relationship between migration and household income (the income concept used in this paper). This is exactly what one would expect, for instance, if married couples with a single wage-earner make their migration decisions jointly.

4.2.2 Migration by Income

In Figure 1, I plot the average estimated annual interstate migration rate from 2010-2019 by different income categories. It is split into four household income quartiles. I also show the Top 5% highest-income households in order to demonstrate that the relationship is not quite linear for interstate migration. Finally, I show those earning under 138% FPL, the cutoff for Medicaid

¹⁶See, for instance, California (2024) or New York (2024).

eligibility for childless adults (which accounts for household size as well as income) because that it the most relevant for my analysis.

The general decline in interstate migration rates in income, until the very highest earning households, is visible even in the public ACS data (indicated by the blue bars). Moving to my data, in red and labeled FTI, we can see that the ACS captures nearly all the moves in the top quartile, but only about 75% of those in the bottom quartile. The FTI data also does a better job at identifying the highest-earning households, leading to a greater increase in migration rates relative to the ACS than the top quartile as a whole.

Finally I show the same information for prime-age childless adults, the newly-eligible population under Medicaid expansion, with the green bars. The average annual interstate migration rate for the subset of this group earning under 138% FPL is 4.7%. This migration rate is high enough that, if directed, could result in substantial population increases in expansion states.

Note that I am not claiming that there is any causal relationship between income and migration. Indeed, reweighting each income bin such that they share the same age and education distribution (using the methodology in (DiNardo et al., 1995)) results in nearly equal migration rates across income bins. Rather, I aim to show that the group that is most likely to benefit from welfare programs is also the most likely to move.

Table 1 shows additional data on intercounty (Column 4) and any (defined as all changes of address) migration (Column 5) by income. For interstate migration, the bottom quartile of income moves about 60% more than the top quartile. For intercounty, they move 86% more, and for any move 118% more. One way to rationalize this is that moving long distances is indeed costly, but so is paying for stable, secure housing. Low-income individuals may enter into less stable rental situations, and therefore be forced to make frequent short-distance moves. This could be across state lines, as roughly a third of all Americans live in border counties, but conditional on moving, low-income people are much less likely to move far. Interstate migration constitutes a higher share of all moves for top quartile individuals than bottom quartile individuals. This would be consistent with evidence in Sprung-Keyser et al. (2022) that shows children born to low-income adults tend to live closer to home. Regardless of the underlying theory, however, newly-eligible adults are mobile enough to react strongly to Medicaid expansion.

5 Migration in Response to Medicaid Expansion

5.1 Simple Model of Welfare Migration

Consider an individual i choosing a local area ℓ and state s , in which to live in year t . The locale ℓ , perhaps a labor market, is an area with similar proximity to family or other amenities and may cross state borders. Her utility depends on individual private consumption C_{ilst} (including wages, rents and local amenities), the net-of-taxes consumption value of local means-tested government transfers G_{ist} , and time-varying idiosyncratic preferences for location e_{ilst} . Assuming additive separability between these components, we can express her utility as:

$$U_{ilst} = C_{ilst} + G_{ist} + e_{ilst} \quad (1)$$

If migration is frictionless and information is perfect, each individual will choose the location bundle ℓ and s where their utility is highest in year t . Even if consumption is constant across location, individuals move around in this model as they realize different values of $e_{\ell st}$, which could include things like family shocks or changes in hobbies or tastes.

Now consider a government policy shock at time t that increases G in some “treated” states, and leaves it untouched in other “control” states. What is the migration response to local welfare variation? Intuitively, in-migration to treated locations increases as increased G makes those locations more attractive and in-migration to control locations falls as they become less attractive. The reverse is true for out-migration, and by these two effects, the population of treated locations increases.

Perhaps less intuitive is the fact that this model predicts higher within-state flows for treatment states. To see this, imagine a couple living in San Francisco, California that is considering moving nearer to family in San Diego, California or Austin, Texas. San Diego would presumably become more attractive after California expanded Medicaid and Texas did not.

Thus the cleanest predictions of this model are about the relative changes in population between treated and control states within a particular locale ℓ . I will focus on this in my empirical strategy. In particular, I will restrict to contiguous border counties in my main specification, but will supplement these by looking within commuting zones (Figure 5) and a border regression discontinuity design (Figure A.4).

However, a large number of locales do not cross state boundaries, and I want to at least consider these in my analysis. I therefore rely on the strong predictions that this model implies about the direction of the flows to complement my main strategy. I discuss this explicitly in Section 5.6.

One thing that is implicit in this model, and indeed many models of spatial choice is that individuals re-optimize in each period, and that they receive the full value of G in “treated” locations. These assumptions are more convenient than plausible, and I revisit them in Section 7.

5.2 Empirical Strategy

In 2014, only 19 states expanded Medicaid coverage to childless adults earning under 138% FPL. Wisconsin also expanded Medicaid up to 100% FPL, and so, following Dague et al. (2022), I treat Wisconsin as an expansion state as well. These 20 states constitute are my treated group. My control group is the 18 states that chose not to expand Medicaid between 2014 and 2018. This cut-off was selected so that I can study the medium-term effects of expansion differences up to 5 years later. I choose to focus on these persistent differences because interstate migration is potentially a long-term decision.

I exclude Alaska and Hawaii because migration patterns into and out of those states are likely different from migration in the continental U.S. Four states, in addition to Alaska, expanded Medicaid coverage in 2015 and 2016, so I do not consider them in my main specification. Six states and the District of Colombia had some level of healthcare coverage for childless adults prior to 2014, and are therefore also excluded.¹⁷ Figure 2 summarizes the treatment and control

¹⁷DE, MA and VT expanded before 2010. CA, CT and NY expanded piecemeal before 2014. NY and CA both

states.

Ultimately, my results are robust to a variety of selections for treatment and control states, staggered adoption, as well as income-eligibility cutoffs, as shown in Figures 5 and 7, described later. However, my main specification includes these states as the simplest and most long-term differences in Medicaid-eligibility.

A straightforward place to begin my analysis would be by estimating the two-way fixed effects model on the newly-eligible population over time at the state-level. As in the following equation for state s in year t :

$$\text{Log}(\text{EligiblePopulation})_{st} = \sum_{t \neq 2012} \beta_t 1\{t = T\} \times \text{EXPANSION}_s + \theta_t + \gamma_s + \nu_{st} \quad (2)$$

This specification is desirable for a number of reasons. First, because I defined my sample in 2012, it is not affected by changes in composition or any factor endogenous to treatment. Second, because my sample is strongly balanced at the individual level, it is inherently accounts for both in- and out-migration and is cumulative.

Unfortunately, as an examination of the treated and control states shows in Figure 2, control states in the Sun Belt are growing faster than the expansion states in the Rust Belt. This is shown more explicitly in Appendix Figure A.2, which further shows that the impact of Great Recession also correlates with treatment status. Control states grew about 3 p.p. faster from 2004 through 2007 than treatment states. Treatment states were also hit harder on average than control states by the Great Recession, suffering a 1 p.p. greater loss in employment. Thus, when I plot the average growth rate in treatment and control states as shown in Figure 3, it is not surprising to see that these control states do not make a good counterfactual for the treatment states, and thus estimating Equation 2 does not yield parallel pre-trends. Control states grow much faster earlier in the analysis period, and while that trend starts to bend a little in the years after the Great Recession, they continue to grow faster even in the post-period¹⁸.

In some ways, this already demonstrates my main finding, that there were no large migration effects in response to Medicaid. However, the simple model of welfare migration espoused above highlights the need for comparison among places with similar amenities outside of Medicaid. Consider, as mentioned in the introduction, St. Louis, MO and East St. Louis, IL, which are across the river from one another, and are connected by several bridges as well as bus service. They have similar average tax income rates for low-income childless adults, as well sales taxes. They are presumably similar in terms of proximity to family, favorite restaurants etc. For the past 10 years (2014-2024), a poor household, which relocates on average every four years, could move across the bridge and get healthcare worth a huge fraction of their income. Whether or not they move is a strong test of the welfare magnet hypothesis. To operationalize this test, I

had Medicaid-like programs available in populous counties before 2012. For a comprehensive accounting of state coverage over time see Burns et al. (2017), and the annual surveys of the Kaiser Family Foundation (e.g. Brooks et al. (2020)).

¹⁸Additional caution in interpreting Figure 3 is advised due to the category of states that are neither treatment, nor control. They were hit by the Recession with a strength in between treatment and control states. Perhaps relatedly, they had middling growth in post-Recession period of 2007-2014. Despite all gaining Medicaid before 2018, they grew the slowest of all three groups in the post-treatment period.

estimate the following equation:

$$\text{Log}(\text{Population})_{s\text{c}pt} = \sum_{t \neq 2012} \beta_t 1\{t = T\} \times \text{Expansion}_s + \alpha_t \text{MinWage}_{st} + \theta_{pt} + \gamma_c + \nu_{s\text{c}pt} \quad (3)$$

for state s , county c and year t . Similar to [Dube et al. \(2010\)](#), I assign contiguous border counties to pairs p . Every two counties that touch each other from states with differing expansion status are assigned to a pair, and counties can be assigned to multiple pairs. To account for this, I include county fixed effects, and cluster my standard errors at both the state and pair levels. I also include interacted pair-year fixed effects, so that identifying variation is within county-pairs in a given year. I weight by 2012 population to avoid heteroskedasticity concerns.

Thus the β_t are the causal effect of Medicaid expansion on the change in the potentially-eligible population since 2012 so long as the non-Medicaid shocks on counties on both sides of the border are similar. It seems plausible that the economic fate on the both the St. Louis and East St. Louis side of the border are similar, and thus the main threat to identification is that there are additional government policy differences enacted in the post-period. I have researched this carefully, and the principle concern I have is that state-level minimum wage are more likely to increase in expansion states starting around 2015. Although much of the newly-eligible population is only weakly attached to the labor force and border-county residents could commute to gain higher wages without moving, this is still at least a theoretical concern for a low-income group and I therefore control for it in my main specification. As we will see, however, it turns out that this control does not meaningfully impact my results.

Table 2 shows that, at the individual-level, the newly-eligible adults living border counties are comparable on both the treatment and control sides. While this it is not necessary that they be similar at any point in time, and I do not use individual-level controls, it is nonetheless comforting that they are similar across a variety of characteristics. Their gender, age, education-levels are similar. They receive employer-sponsored health insurance at roughly the same rates. Income earned and mortality, while not identical, are not so different as to be concerning. Intercounty migration rates are also similar, both in they year 2012, and in the 5 years prior.

Table 2, like Table 1, demonstrates that this population is highly-mobile. Migration rates in 2012 were also incidentally higher than for the decade as a whole, as can be inferred from the intercounty migration rates. Lastly, the 5-year pre-period migration rates contextualize what fraction of the population we might expect to move in the post-period. The control-county in-migration rate is 27.8%, a number which is large enough to matter for any local budget.

Now all that remains is to check, first, whether Medicaid expansion did, in fact, increase enrollment and then, whether this increase incentivized migration.

5.3 Medicaid Enrollment

I begin by showing that Medicaid expansion did, in fact, increase Medicaid enrollment for childless adults. To do this, I estimate my border-county regression in Equation 3 except with “any enrollment in the year” as the outcome variable. From 2009-2013, Medicaid enrollment is similar on both sides of the border. As previously noted, childless adults earning under

138% of FPL were ineligible for Medicaid. However, I can estimate coefficients for this because there is some measurement error in the matching process. There are also a small number of parents whose children had left the house by 2012. The fact that these pretrends are flat and insignificant shows that these phenomena are uncorrelated with future expansion. Recall that I cannot estimate enrollment all the way back to 2004 due to data limitations.

Immediately after expansion, as shown in 4 Panel A, the enrollment rate for these newly-eligible childless adults jumps up to 21.5% (s.e. 1.6%). It is higher in later years, for an average of 26% from 2014-2018. These results are in-line with results from a variety of other papers on other papers examining childless adults, and slightly larger than papers that consider all adults (Miller et al., 2021; Wyse and Meyer, 2023), since some parents were eligible prior to expansion. This jump is also visible in the public ACS data, as shown in Appendix Figure A.3, although the effect is slightly more muted. Appendix Figure A.3 also shows that Medicaid expansion did more than simply crowd-out other insurance types, as the rate of people having “any insurance” also increased on the expansion side of the border.

Note that in comparison to many other papers on the impact of Medicaid itself, these results do not constitute a first stage. This is because in the general conception of “welfare magnets,” anybody with the potential to gain welfare might be considered treated, thus actual enrollment does not distinguish a treatment-on-the-treated effect. Rather, these results serve to show that expansion did meaningfully alter health insurance status for the people in expansion states, and therefore migrants might reasonably be expected to benefit. This regression is also important in estimating the budgetary impact of Medicaid expansion. Migration effects would not matter if there were no increase in enrollment.

5.4 Primary Results

With these benefits clear, I now consider the actual impact of expansion on migration, and estimate Equation 3. The results are shown in Figure 4 Panel B. There are no statistically significant effects of treatment in any year. Had there been a strong welfare magnet effect in the expected direction, then the coefficients in the post-period would all be positive and significant.

Instead, coefficients in both the pre- and post-period are slightly negative, indicating that the relative level of newly-eligible adults on the expansion side of the border peaked around 2012. The typical magnitude of coefficient in both the pre- and post-period is a little greater than half and percentage point change in the population relative to 2012. There is also no evidence of an anticipation effect in 2013, after the Supreme Court decision, but before expansion.

The estimates are also precise. After 5 years, I estimate a net cumulative population change of -1.2% (s.e. 0.9%) in the Medicaid eligible population due to expansion, and I can therefore reject a increase of greater than 0.56% with 95% confidence. This is a much tighter bound than the best-approximation of this regression done on cross-sectional state-level data in the ACS, which can reject a 2.7% increase with equivalent confidence, as shown in Appendix Figure A.3. This is also a presumably conservative estimate of the total potential population increase because this analysis is confined to border counties. Moreover, this result is consistent across a variety of alternative specifications.

5.5 Robustness Checks

Figure 5 summarizes a several plausible alternative specifications and shows that they all imply the same thing as my preferred specification. For parsimony, I plot the pooled post-treatment coefficient on $Post_t \times Expansion_s$, which averages effects over the years 2014-2018.

As stated before, controlling for the minimum wage turns out not to be important. I show my estimates with and without the control, as well as restricting to states with no changes just in case the minimum wage has dynamic effects. This effect is obviously noisier, as it retains a much smaller fraction of counties, but it is nonetheless consistent with my primary estimate.

Another worry might be that my use of all contiguous border counties includes some pairs that may not have much in common. I therefore estimate a version of my main regression considering only counties which are in the same commuting zone, but in different states. In this case, the point estimate becomes negative, but remains insignificant.

In my main specification, I do not require that people live for the duration of my analysis period because that may be endogenous, and instead code them as remaining in the last place they lived. This may attenuate my estimates, and so I next restrict the sample to people who are alive (and thus able to migrate) throughout my analysis period. Again, it does not change my result.

Next, I consider alternate definitions of treatment and expansion. One may wish to consider, for instance, the states which expanded after 2014 or exclude Wisconsin. This definition of expansion is that used in [Miller et al. \(2021\)](#), and one common to many studies of Medicaid expansion. It is also in-line with my main result.

If one remains worried about post-period economic shocks, then one strategy is compare migration flows for eligible individuals to those who earn income above, but are not too distant from, the eligibility threshold. I next estimate a triple difference, plotting the coefficient on $Post_t \times Expansion_s \times IncomeEligible_i$, where the third difference compares prime-aged childless adults with income under the 138% FPL eligibility cutoff with those earning 250 – 400% FPL.

Finally, I show the outcomes for out- and in-migration, because one might be more directed then the other. These both have the opposite sign of what the welfare magnet hypothesis would predict—although signs are consistent with the estimated effect on net population.

In Appendix Figure A.4, I plot the regression discontinuity equivalent of my border-county design. This uses the same variation as my primary result, but rather than use the politically-appointed bandwidth of counties, I let the data choose the bandwidth according to the methods used in [Calonico et al. \(2017\)](#). As Panel A shows, out-migration rates are symmetric around the border, increasing slightly as it approaches the border from either the expansion or non-expansion side. This is expected, as out-of-state migration is easier neared the border. Panel B shows that the border-discontinuity in out-migration rates is insignificant, and consistent over time, contrary to what would expect if there were a magnet effect, and consistent with my border-county results.

5.6 State-to-state flows

In addition to analysis on the stocks, I run an analysis of flows that is more directly related to the utility function defined by Equation 1. Here I follow [Moretti and Wilson \(2017\)](#). Using

the typical [McFadden \(1978\)](#) assumption that the idiosyncratic component of utility is i.i.d. Extreme Value Type 1 distributed, and the [Berry \(1994\)](#) trick of using the market share to get the utility parameters, I get the following equation:

$$\log \left(\frac{P_{odt}}{P_{oot}} \right) = \beta_1 \{t = T\} (Expansion_d - Expansion_o) + \gamma_d + \gamma_o + \gamma_t + \theta_{od} + \nu_{iodt} \quad (4)$$

where P_{odt} is the number of newly-eligible adults moving from origin state o to destination state d in year t , P_{oot} is the number of people who remain in o , and $o \neq d$. The θ_{od} are origin-destination pair fixed effects and can account for all moving costs. I use states here because estimating a model with roughly 10 million county pairs is intractable. This design has the advantage of including internal counties, ensuring that part of my analysis will capture those migration flows.

The basic equation above can be converted into a difference-in-difference-style equation with controls for the change in state-level minimum wage, as follows:

$$\log \left(\frac{P_{odt}}{P_{oot}} \right) = \sum_{t \neq 2012} \beta_t \{t = T\} (Expansion_d - Expansion_o) + \eta (MW_{dt} - MW_{ot}) + \gamma_d + \gamma_o + \gamma_t + \theta_{od} + \nu_{iodt} \quad (5)$$

with standard errors are three-way clustered by origin \times year, destination \times year, and origin-destination pair, again following [Moretti and Wilson \(2017\)](#).

Thus we have a framework for estimating interstate flows¹⁹. Figure 6 shows this analysis, beginning with a simple “eye test” on the raw migration flows between treated and control states in Panel A. If the effect of Medicaid were large, flows into treatment states from both treatment and control states should increase. Instead, they appear to decline. In fact, interstate migration in general peaks in the pre-period. This fact is inconsistent with a disequilibrating event, but the broad decline makes the relative decline more difficult to distinguish, motivating actual estimation.

This appears in Figure 6 Panel B, which shows the estimates corresponding to Equation 5. If the control-to-control and treatment-to-treatment flows serve as a good control, then the coefficients should be statistically insignificant and without a trend in the pre-period. Empirically, all coefficients are indeed insignificant and small. Note that a coefficient of -4 here indicates a roughly 4% decrease in the *rate* of migration. For reference, the left-hand y-axis scales all these values by 4.7%, the mean rate of migration for newly-eligible adults, to provide a sense of how these changes in flows might alter the stock of newly-eligible adults. If anything is happening in the pre-period, it is a slight upward trend, which would make the post-period more negative if adjusted for. There is a slight spike in the point estimates in 2013, which could be construed as a

¹⁹Note that β_t is technically an approximation of the elasticity, which can be derived by scaling β_t by the one less the weighted average of the log-odds ratio. Because this scaling factor is close to 1, this is a good approximation.

small anticipation effect, yet Panel A suggests that this is driven by a drop in control-to-control flows, which could not plausibly be motivated by Medicaid expansion.

One limitation of this analysis so far is that it assumes symmetry between gaining and losing Medicaid, as well as between treatment-to-treatment and control-to-control flows. The first assumption might be violated in the case of loss aversion, and the second is potentially violated if moving costs are large relative to the value of Medicaid. I relax both assumptions in Table 3. Column 1 simply repeats the estimation from Panel B Figure 6, pooling post-period coefficients for simplicity. Column 2 is the saturated version of this regression, where each type of flow is estimated relative to control-to-control flows. As might be expected from Panel A of Figure 6, all are slightly positive and none are significant. That positive value is predicted in the case of Medicaid magnetism for treatment-to-treatment and control-to-treatment flows, but not in the case of treatment-to-control flows. Taken together, these results provide another piece of evidence that there was no magnet effect of Medicaid.

5.7 Heterogeneity

Having verified my border county results with the state flows design, I return to my main border county design to conduct subsample analysis. Particularly in my setting, where we anticipate that the value of Medicaid might be very different across the distribution of newly-eligible adults, it is important to confirm that the null average treatment effect is not masking a significant effect for some subsample. To pick subsamples, I considered people that may have a high benefit to migrating for Medicaid (either because they are more likely to remain eligible, or because they are more likely to benefit from health insurance), or if they have a low cost of migration. For each subsample, I estimate a version of my standard contiguous border county difference-in-difference estimator (Equation 3), pooling all estimates for the post-period. I plot the results in Figure 7. All subsets are defined using only pre-period characteristics. I subset based, not only on variables observable in FTI (Panel A), but also the ACS (Panel B) and the CPS (Panel C). Because the ACS is roughly 100 times smaller than my FTI data, I include individuals if I can match them to the 2012, 2011 or 2010 surveys. Because the CPS is roughly 3,000 times small, I use any individual I can match in the previous 10 years. Nevertheless, the FTI sample remains larger than the ACS, and the ACS remains larger than the CPS. The standard errors reflect that.

For my low-cost of migration subsamples, I consider younger adults, those without a spouse, those who were born out-of-state (FTI), people with a Bachelors degree of higher and renters (ACS). These are not markedly different from my main estimates. For the high-benefit due to persistent eligibility (i.e. poverty), I consider “Permanently Low-income” adults (those who earned under 138% FPL in 3 consecutive years from 2010-2012), those earning under 100% FPL (and thus ineligible for ACA Marketplace insurance) and those without income (defined as having no 1040 or W2) in the FTI data. In the ACS, they are those with no college education. I also include people without employer-sponsored health insurance, and self-employed people (in the ACS), because they are less likely to have an alternative to Medicaid.

Lastly, I consider those who might be sicker and more in need of health insurance. For the FTI data, I consider older people (aged 50-59 in 2012). I cannot consider people older than 59

in 2012 because then the end of my analysis period would be polluted by Medicare enrollment. I also consider the group that died in 2019 or 2020. (I do not have mortality data beyond 2020.) While death is likely endogenous to Medicaid availability, it is nonetheless interesting to consider this group, which seems likely to be sicker. This is a small group, and so the standard error is large, but the point estimate is negative, the opposite of what one would expect if Medicaid were a magnet. The rest of my estimates come from the CPS, and so are quite noisy. The subsets of people who left their job for health reasons and who have top quartile out-of-pocket medical expenses are both insignificant. In the first significant migration result of the paper, however, those who self-reported as having “poor health” appear to responded strongly to Medicaid expansion, with an apparent 19% (s.e. 8.0) increase in population in expansion states. I report this for transparency, and acknowledging that my analysis would benefit from being able to better observe need for insurance, but I believe this is noise.

Because I have tested so many subsamples, it is possible that one is significant by chance. My other unhealthy subsamples show no effect. Indeed, if I apply a Bonferroni correction based on the subsamples in Panel C alone, the lower bound for the poor health estimate becomes -1.3%. Moreover, Appendix Figure A.5 shows that migration to expansion states for this subsample peaked in 2016, but there was not significant difference between treatment and control states by 2018. Why this result would only be significant in some years is unclear. The estimate is also incompatible with my estimates on the universe of newly-eligible adults. Poor health is the lowest possible rating for health, with “fair”, “good”, “very good”, and “excellent” following in ascending order. In the overall population, “excellent” is the modal answer in public 2012 CPS ASEC, with about 3.5% reporting poor health. In this population that is poor despite being prime-aged, however, 14.6% report being in poor health. This is too large for my average treatment effect, as it implies 2.8% of newly-eligible adults move. Lastly, if I pool “poor health” with people in “fair health,” the next most unhealthy group, the sign on the point estimate flips.

In conclusion, there is no evidence that the poorest or most mobile people respond to Medicaid expansion. There is no strong evidence that the unhealthiest do, but my lack of power here suggests avenues for future research.

6 Migration in Response to Great Recession Local Shocks

Parallel to the literature on welfare migration, there is a long literature on migration responses to recessions (Blanchard and Katz, 1992). Many papers have shown, for instance, that the Great Recession lead to population increases in the least-affected states and declines in the most-affected states (Yagan, 2014; Cadena and Kovak, 2016; Sprung-Keyser et al., 2022; Finkelstein et al., 2024). In this section, I aim to add to these results by showing that specifically the type of person who would later become eligible for Medicaid also reacts strongly to the Great Recession. This is valuable both because it shows that this population will move in response to their economic incentives and because it shows them moving despite interstate migration costs that are presumably similar to the ones they will face just a few years later.

To show this, I run a simple two-way fixed effects regression by state and year similar to

Yagan (2019). My estimating equation is:

$$\text{Log}(\text{Population})_{st} = \sum_{t \neq 2006} \beta_t 1\{t = T\} \times \text{SHOCK}_s + \theta_t + \gamma_s + \nu_{st} \quad (6)$$

for state s in year t . SHOCK_s is time-invariant, and defined as the state-level deviation from employment rate trends in 2008 and 2009 as calculated in Yagan (2019). The sign on SHOCK_s implies that the post-Recession β_t will be negative if the states hit harder by the Great Recession see greater declines in population. States are weighted by their 2006 population and standard errors are clustered at the state-level. As will always be the case for regressions run in this paper, my sample is strongly balanced at the individual level.

I begin by running this specification on all adults who appear in the 2006 tax data are alive in that year. The results are shown by the blue line in Figure 8. The interpretation of these coefficients is the impact of a 1% increase forecasting error in employment rates from 2008-2009 on the net change in state population relative to 2006. Because the panel is strongly-balanced, these changes must come from in- and out-migration, and cannot be caused by immigration, birth or death. For instance, the 2010 coefficient implies that, 5 years after the Great Recession began, states that saw a 1 p.p. drop in forecast employment saw a population loss of roughly 0.33% (s.e. 0.09%). This is similar to the estimate in Finkelstein et al. (2024), and serves as a validation of my data and design.

Next, I run the regression on a subsample of people who would qualify as “newly-eligible” in 2006—ie. they are childless adults, aged 28-59 earning under 138% FPL (shown in red). The population changes for this group closely track the adult population as a whole through 2010, before bouncing back up. Interpreting what this means for the relative mobility of this newly-eligible group depends what one’s model of the underlying incentives are. In a sense, it shows that they are more mobile, because they move out at the same rate and return more quickly. In another sense, their migration may be less directed, since employment declines were concentrated among the low-skilled (Notowidigdo, 2020), and the disadvantages of the Great Recession persisted beyond 2010 (Yagan, 2019). The proximity of the point estimates, and additional noise inherent in this smaller subsample cautions against over-interpretation. Furthermore, understanding the relative mobility of this group is less important in the context of welfare magnets than establishing that this group does indeed move in response to economic shocks, and that that migration is statistically significant in an empirical design similar to the one that I use for Medicaid. This provides a point of reference to gauge the my null result on Medicaid.

7 The Costs and Benefits of Migration

7.1 The Annual Value of Medicaid

As discussed in Section 2, the annual expense of Medicaid to the government for a newly-eligible adult is roughly similar to their annual income. Its benefit to the recipients has been quantified in many ways, including reduced mortality, increased medical care, and better reported physical and mental health (Finkelstein et al., 2012; Miller et al., 2021; Wyse and Meyer, 2023).

Of course, the recipients may still value health care at less than its average cost, which has generally been the finding for these low-income groups (Tebaldi, 2024; Finkelstein et al., 2019). Finkelstein et al. (2019) estimate the value of Medicaid to recipients earning under 100% of the FPL using evidence from the Oregon Health Experiment three different ways, and arrive at values between \$2,379 and \$1,126 a year—well below average cost, but still a huge fraction compared to the average annual income of \$7,142. Moreover, the wide difference in these estimates is primarily driven by the uncertainty around valuing the insurance component. A more straightforward minimum value might be the out-of-pocket medical expenses avoided, which was \$808 for compilers. Alternatively, one could use the fine for not having insurance, which averaged \$637 a year between 2014 and 2018. With amounts this large, the apparent indifference of newly-eligible adults may seem surprising.

However, these values do not take into account things such as moving costs, uncertainty about enrollment after the move, the fact that the average WTP might be raised by a few enrollees who value it highly, the difficulty of ascertaining where to move, or the hassle of enrolling. I address these each in turn.

7.2 The Benefits of Migration

Comparison of the reaction to Medicaid expansion to that from Great Recession local shocks is instructive for understanding what costs might be preventing migration in the former case. The population of newly-eligible adults fell by 0.31 percent from a -1 p.p. deviation in the employment rate, at its nadir in 2010. Table 4 shows that, over this same period, newly-eligible adults suffered losses in cumulative income equal to \$752 from the same shock, implying that a \$1,000 loss in income would result in a 0.4% decline in population after 3 years. As stated in Section 6, this shows that this group is capable of directed interstate migration in spite of any interstate migration costs. The fact established in Figure 1 that low-income people more also runs counter to a story where migration costs are the dominant factor.

After 3 years, I estimate a point estimate of -0.85 for the change in population due to Medicaid expansion, and the upperbound of my 95% confidence interval is 0.71. This upperbound is roughly 2.3 times the point estimate on the Great Recession (0.31), thus if we take the Great Recession semi-elasticity of migration as the true value, then this bounds my estimate of the value of Medicaid over 3 years at about 2.3 times the cumulative income impact of the Great Recession, or \$1,736. If the migrants received Medicaid in each year, then the annualized value (with no temporal discounting) would be divided by three, or about \$578. Thus my upperbound is about half the lowest WTP estimate from Finkelstein et al. (2019). This, however, assumes that migrants receive Medicaid in each year after migration.

Interstate migration, however, is plausibly a multi-year decision. To understand the value of migration over time, I consider the best-case for scenario for taking advantage of Medicaid expansion, and look at the people who move from a non-expansion state in 2013 to an expansion state in 2014, and remain there for all 5 years of my analysis post-period. Table 5 shows these outcomes in Column 2. Column 1 shows the same outcomes, except for stayers—that is people who were living in expansion states in 2014, and then remain there for the next 5 years. Far from being being enrolled for 5 years, these movers actually spend only 1.8 years enrolled—actually

less than stayers. After 3 years, the typical mover is enrolled for a about 1 year. If I use this to annualize my value over 3 years, then the upperbound of my WTP estimate returns to \$1,736, which is actually within the range of estimates of WTP for Medicaid.

There are many possible objections to this comparison. First, there is no positive value of Medicaid which would rationalize my point estimate. Second, this estimate is sensitive to timing. If I apply the semi-elasticity to my 5-year estimates for the change in population and 5-year enrollment rates, the annualized value drops to \$787. Third, there are obviously many differences between a Great Recession local shock to employment and Medicaid expansion. Both ATEs on consumption are skewed, the first by averaging large drops in income due to job loss with many job retainers, and the second by the unhealthy deriving most of the benefits. However, losing a job and looking for a new one might feel more salient and urgent than a government program expanding in a different state.

7.3 Information Costs

Understanding cross-state differences in Medicaid eligibility is difficult. Perhaps the best evidence of this comes from the research community itself. Many good papers use different specifications for the appropriate treatment and control group for their studies (c.f. [Miller et al. \(2021\)](#) and [Wyse and Meyer \(2023\)](#)). [Dague et al. \(2022\)](#) is an entire paper about which states we should consider expansion states and when, and the Kaiser Family Foundation releases annual reports on changes in eligibility guidelines. People who intend to move to obtain Medicaid must do some research.

Medicaid expansion may also reduce out-migration, but understanding ones benefits might change if they leave their state is also confusing. Medicaid is often named different things in different states. Some are recognizably state-provided healthcare, like “TennCare” in Tennessee, while others like “MediQuest” in Hawaii, seem to obscure this fact. Furthermore, hospitals may enroll patients in Medicaid if they are uninsured and meet eligibility criteria. They are incentivized to do so in order to ensure that they are reimbursed. Thus, people can end up enrolled in Medicaid without proactive action on their part, and may not recognize the service for the residence-based program it is. Several studies have documented confusion about Medicaid. I use a strategy similar to [Boudreaux et al. \(2015\)](#) to document a similar problem in my subpopulation, comparing ACS responses to administrative data.

The ACS asks:

Is this person CURRENTLY covered by any of the following types of health insurance or health coverage plans? ... Medicaid, Medical Assistance, or any kind of government- assistance plan for those with low incomes or a disability: [] Yes [] No

I take people who responded to this question in a given month in 2018 (when the T-MSIS data is cleanest) and merge it to administrative Medicaid enrollment records for that month. Of ACS respondents who are identified in the administrative data, 30.8% claim not to have Medicaid, and 14.3% claim to have no insurance at all.

7.4 Budgetary Consequences

Because the federal government covers 90% or more of the cost of expansion, the budgetary consequences at the state-level are unclear, no matter what the effect on migration is. The U.S. Comptroller Office estimated that in 2017, the average cost of a newly-enrolled adult was around \$7,048, of which the state would be responsible for at most \$705 (MACPAC, 2022). Most of this money goes not to the new enrollees, but rather to healthcare providers (Finkelstein et al., 2019). Without Medicaid, healthcare expenditures are reduced, but the remaining costs would likely fall on the patchwork charity care system, which may be funded in-part by the state.

The secondary effects of Medicaid further complicate the issue. For instance, there are effects that change state expenditures, such as a reduction in criminal activity for those newly covered (Deza et al., 2024). There may also be incentive effects that alter tax revenues. For instance, new enrollees may reduce their labor supply due to job “unlock.” At a high level, Gruber and Sommers (2020) conduct an event study around expansion and find that there is no change, positive or negative, on state spending.

It is nonetheless interesting to consider how much migration I can rule out. It is possible, for instance, that states will be asked to bear more of the cost in the future, and the issue of migration is sure to be raised in any discussion of other state-level program expansions. My 2018 estimate of the change in the newly-eligible population (-1.24%, s.e. 0.92) is perhaps the simplest way to estimate this. The upperbound of the 95% confidence interval is 0.56%. Thus, if movers and incumbents enroll at a similar rate, a fact supported by Table 5, then 0.56% of the new enrollees will come from out of state.

To put concrete numbers on this, I make the following calculation for a particular state:

$$\begin{aligned} \text{Enrollment Increase} &= \text{Upperbound on Net Population Increase} \\ &\times \text{Enrollment Rate} \times \text{Newly-eligible population} \end{aligned} \tag{7}$$

This equation is correct only in a partial-equilibrium. If every state expanded Medicaid, then the incentive to migrate would disappear. Therefore, consider what would happen if just Texas, the largest non-expansion state, expanded Medicaid. The upperbound on enrollment multiplied by the enrollment rate is the same for every state, and yields an estimate of 0.15% in 2018. In the case of Texas, this would result in approximately 900 additional enrollees compared 5 years after implementation.

Suppose that this population is roughly similar to the average population of expansion adults, then the average cost will be \$7,047, and the total cost from migrants will be about \$6.8 million (about 0.01% of the current budget for Medicaid in Texas). If the entire population is in poor health, then Cox et al. (2024) projects that their medical expenses will be 3 times greater (about 0.03%).

8 Discussion

My paper affirms the small effects found by several recent papers in the United States (e.g. Goodman (2017)), but contrasts with recent work by Agersnap et al. (2020) that shows that a

reduction in welfare benefits offered to immigrants in Denmark resulted in a significant drop in the stock of immigrants in Denmark. The population studied in [Agersnap et al. \(2020\)](#) is quite different from the group affected by Medicaid, as it is made up of immigrants who have already decided to move to the European Union. Immigrants are more mobile than the mostly native-born population that I study ([Basso and Peri, 2020](#)). Moreover, the Danish government exerted substantial effort to disseminate information about the change. For example, it sponsored advertisements in Lebanese newspapers to inform Syrian refugees that it was cutting benefits to deter immigration. The result is a semi-elasticity closer to my estimates from the Great Recession, than Medicaid. [Agersnap et al. \(2020\)](#) estimate that a \$100 (2018 dollars) drop in monthly benefits resulted in a 0.4% drop in population, or about 0.27% drop for every annualized \$1,000 (2023 dollars), compared to a 0.4% drop for every \$1,000 lost from the Great Recession. Taken together, these estimates suggest that when changes are large, salient, persistent, and urgent in the sense of job loss or asylum, welfare may be magnetic. I show in this paper that the population likely to be eligible for any welfare expansion is highly mobile, and responds to economic shocks.

I also show that, in the case of Medicaid, there was no meaningful migration. The differences seem to include that individuals are unlikely to be on Medicaid continuously. There is imperfect take up and significant churn. Furthermore, Medicaid is confusing, with enrollees often unaware that they are enrolled. In this case, welfare does not seem to be magnetic. I believe that issues of churn and information costs apply broadly in the context of local government transfers in the U.S., and thus my paper suggests we may see little effect from state-level expansions of SNAP, the EITC or various TANF programs.

In the case of in-kind transfers such as Medicaid, it is possible that some subpopulation that values it uniquely highly may be more responsive to welfare magnets. I try various ways of identifying this subpopulation in my data, and though most subsamples seem to be similarly indifferent, my results for those reporting poor health indicate that this may be an avenue for future research. This would be possible with richer data on individual-level expenses or health conditions. A paper that can simultaneously estimate migration and willingness-to-pay may sharpen our understanding of the response to in-kind transfers.

9 Conclusion

In 2014, 20 states expanded Medicaid eligibility to include able-bodied prime-aged childless adults for the first time. How magnetic was this expansion? To study this, I combine administrative data from the IRS and the CMS, along with survey data from the ACS and CPS. Using both a border county and an interstate flows difference-in-difference design, I show that there is no meaningful effect on migration, and I can rule out a cumulative impact on the newly-eligible of more than 0.56% after 5 years. This implies that if Texas expanded eligibility to newly-eligible adults in 2019, the Texas Medicaid budget would have grown by 0.01% by 2023.

I also provide new insight into why there is no effect. Two pieces of evidence point against moving costs, which though they undoubtedly exist, do not seem prohibitive. First, low-income adults move interstate frequently, and more often than their higher-income peers. Second, the

same newly-eligible population responded significantly to the economic incentives induced by Great Recession local shocks. Instead, my results point towards the reduction in incentives caused by imperfect take-up and churn in eligibility. Accounting for these facts, brings the values of Medicaid at the upperbound of my confidence interval into line with some estimates of the willingness-to-pay for Medicaid in this population. My results also support information frictions as a potential source of inaction, as 30% of newly-eligible adults enrolled in Medicaid reported to the ACS that they were not. Regardless of the explanation, it is clear that Medicaid is not significant a welfare magnet.

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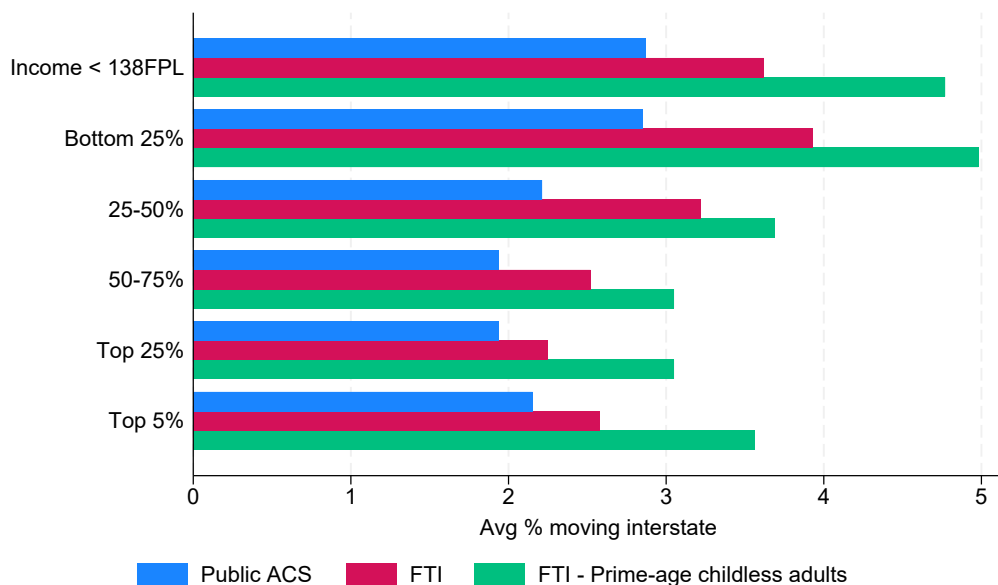
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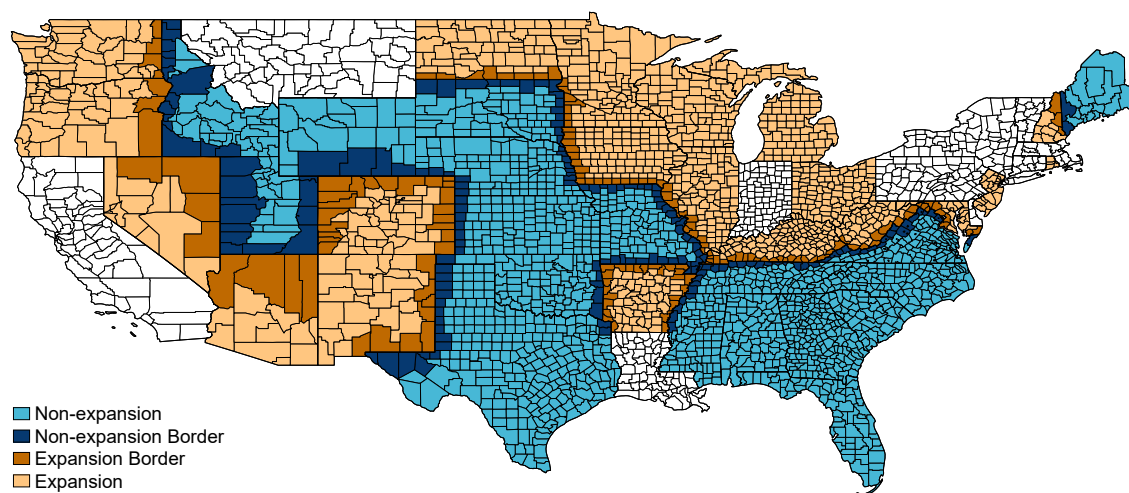
Figures

Figure 1: Comparison of Interstate Migration Rates across Datasets (2010-2019)



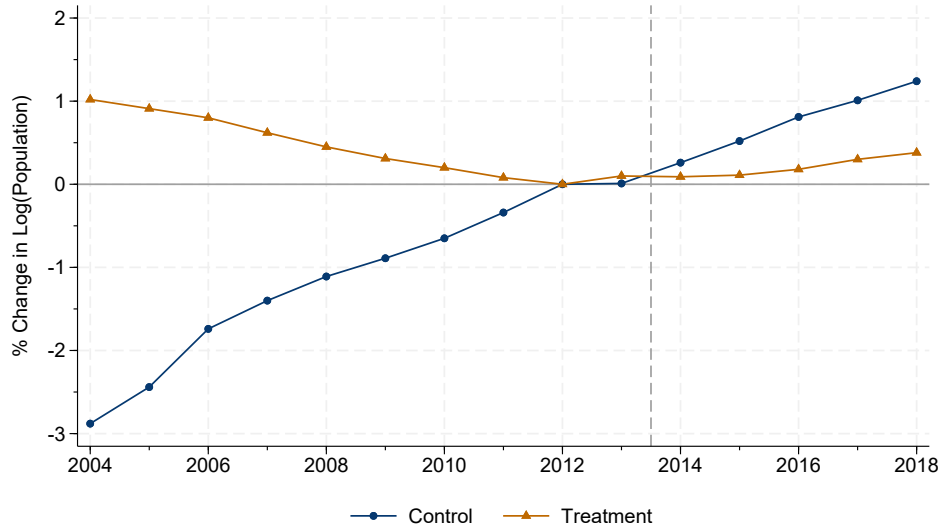
Notes: This figure compares average interstate migration rates from the public ACS data (blue) and Federal Tax Information (FTI) data (red) across different income bins. The green bars represent FTI data restricted to prime-age (28-59), childless adults. The top green bar highlights newly-eligible adults under Medicaid expansion—those who are childless, prime-age, and have household incomes below 138% of the Federal Poverty Line, and would therefore be eligible for Medicaid in expansion states. The figure shows that low-income adults move more frequently than high income adults, that this relationship sharpens in the FTI, and that being prime-aged and childless also makes individuals more mobile. The ACS data exclude imputed values and individuals in group quarters.

Figure 2: Medicaid Expansion Map



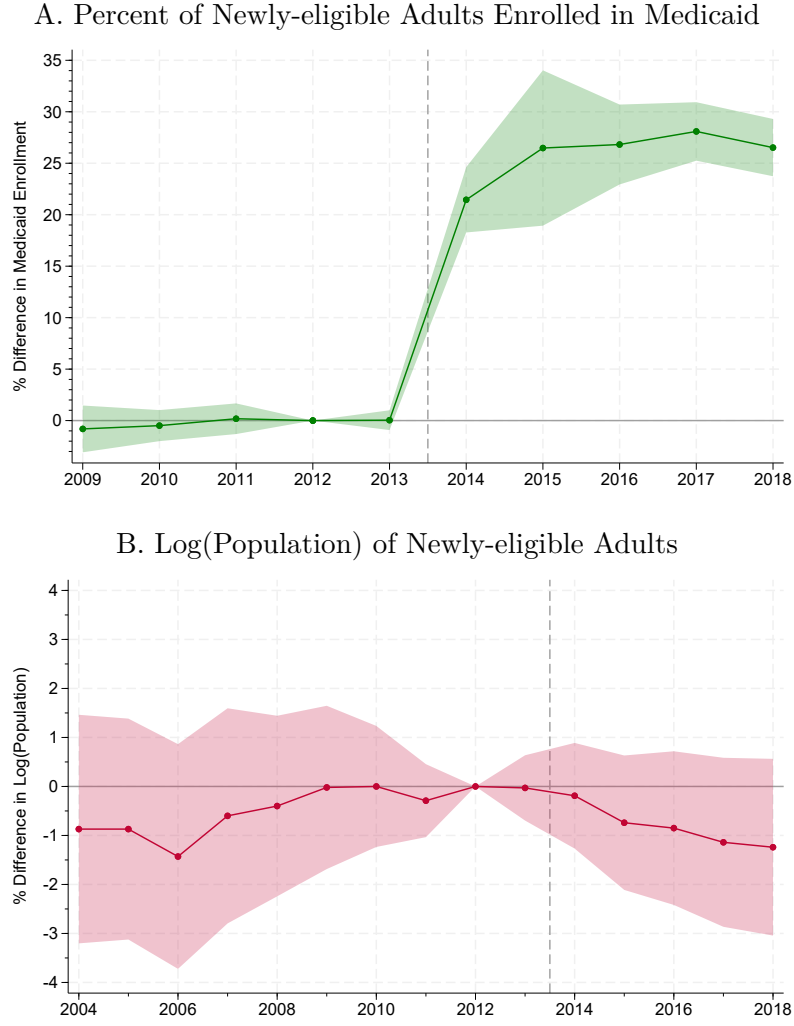
Notes: This figure shows states which expanded Medicaid in 2014 (treatment states) in brown and states which had not expanded Medicaid through 2018 (control states). Contiguous border counties are emphasized in darker shades of each color, and constitute my primary analysis sample. Uncolored states either expanded prior to 2014 or between 2014 and 2018.

Figure 3: Percent Statewide Change in Stock of Newly-eligible Adults



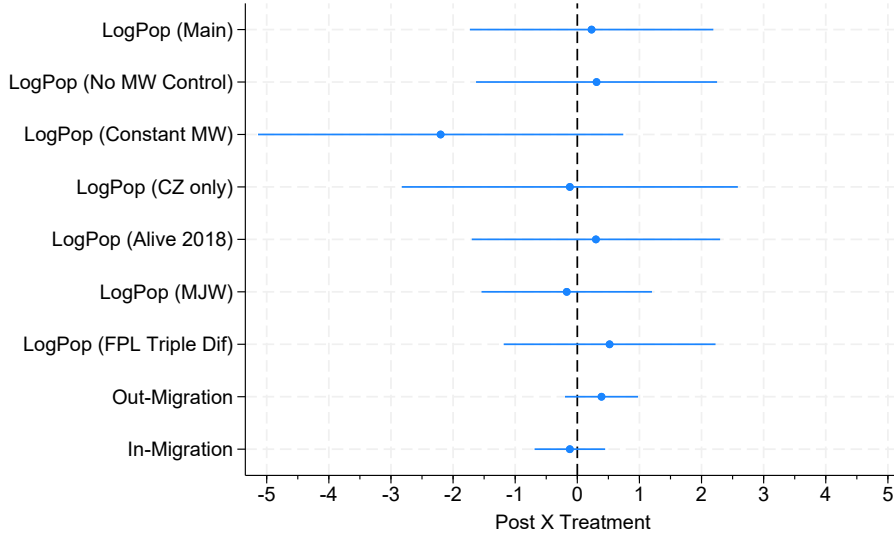
Notes: This figure shows the percent change in the Log(Population) of newly-eligible adults compared to 2012. It is intended to show that there are secular differences between the growth rates of non-expansion (control) states in blue circles and the expansion (treatment) states in brown triangles, such that the control group does not satisfy the parallel trends assumption necessary for differences-in-differences. Newly-eligible adults have characteristics that would make them eligible for Medicaid in the post-period in expansion states, but not in non-expansion states. They are defined in the pre-period (2012) as childless, aged 28-59, and earning income below 138% of the Federal Poverty Line. The sample is strongly balanced, so the difference in trends is enabled by differences in population levels in 2012, and the existence of states which are not treatment or control. The dashed gray vertical bar separates the pre- and post-treatment periods.

Figure 4: Contiguous Border County Pair Difference-in-differences



Notes: This figure shows how Medicaid expansion affected newly-eligible adults in expansion states relative to control states using a border-county difference-in-differences design. Newly-eligible adults have characteristics that would make them eligible for Medicaid in the post-period in expansion states, but not in non-expansion states. They are defined in the pre-period (2012) as childless, aged 28-59, and earning income below 138% of the Federal Poverty Line. Panel A plots the coefficients on $Expansion_s \times 1\{Year = t\}$ in Equation 3, but the outcome is any Medicaid enrollment in the year as measured in the by merging the administrative Medicaid data (MSIS and T-MSIS) to FTI data. These data are not available before 2009. Panel B is similar except the outcome is $\text{Log}(\text{Population of Newly-Eligible adults})$. Each panel is strongly-balanced at the individual-level. Point estimates are indicated by the connected dots. Shaded areas show the 95% confidence interval, with standard errors clustered at the state- and pair-level. The dashed gray vertical bars separate the pre- and post-treatment periods.

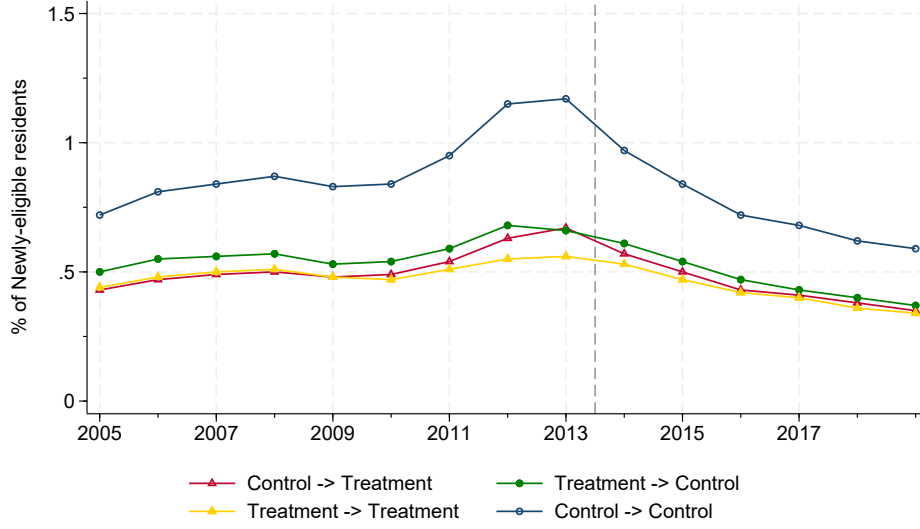
Figure 5: Alternative Border County Specifications



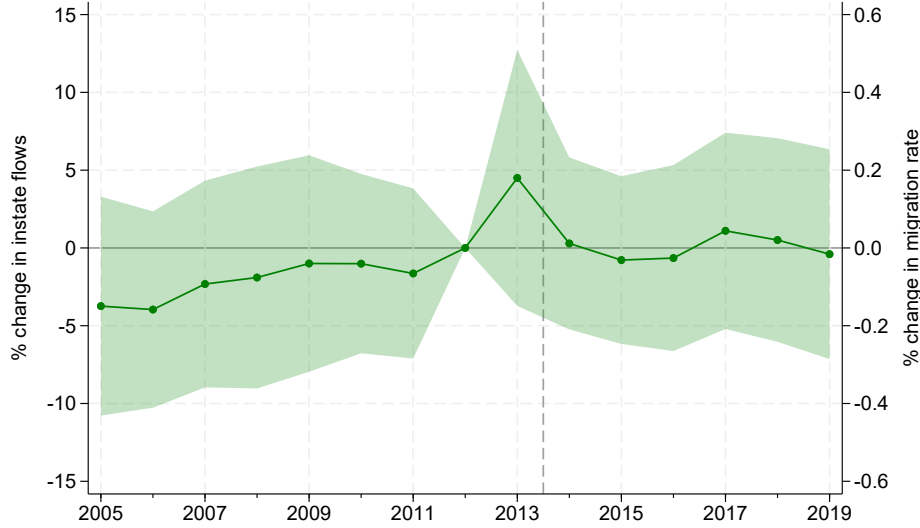
Notes: This figure re-estimates the contiguous border-county pair design described by Equation 3 and shown in Figure 4, except that it plots $Post_t \times Expansion_s$, where $Post_t$ is an indicator for being after Medicaid expansion (2014-2018). The point estimates are represented by dots. The lines show the 95% confidence interval, with standard errors clustered at the state- and pair-level. “LogPop (Main)” denotes my preferred specification where the outcome is the Log(Population of Newly-eligible adults), which controls for state-level minimum wages. “LogPop (No MW Control)” omits this control. “LogPop (Constant MW)” restricts to counties that saw no minimum wage changes between 2014 and 2018, “LogPop (MJW)” is a staggered difference-in-difference including the late-expanding states, following the treatment definition in Miller et al. (2021). “LogPop (FPL Triple Dif)” plots $Post_t \times Expansion_s \times IncomeEligible_i$, where the third difference compares prime-aged childless adults with income under the 138% FPL eligibility cutoff to those earning 250 – 400% FPL. Out- and In-Migration show the outcome for cumulative out- and in-migration, respectively.

Figure 6: State-to-state Flows Analyses

A. Average Annual State-to-state Flows

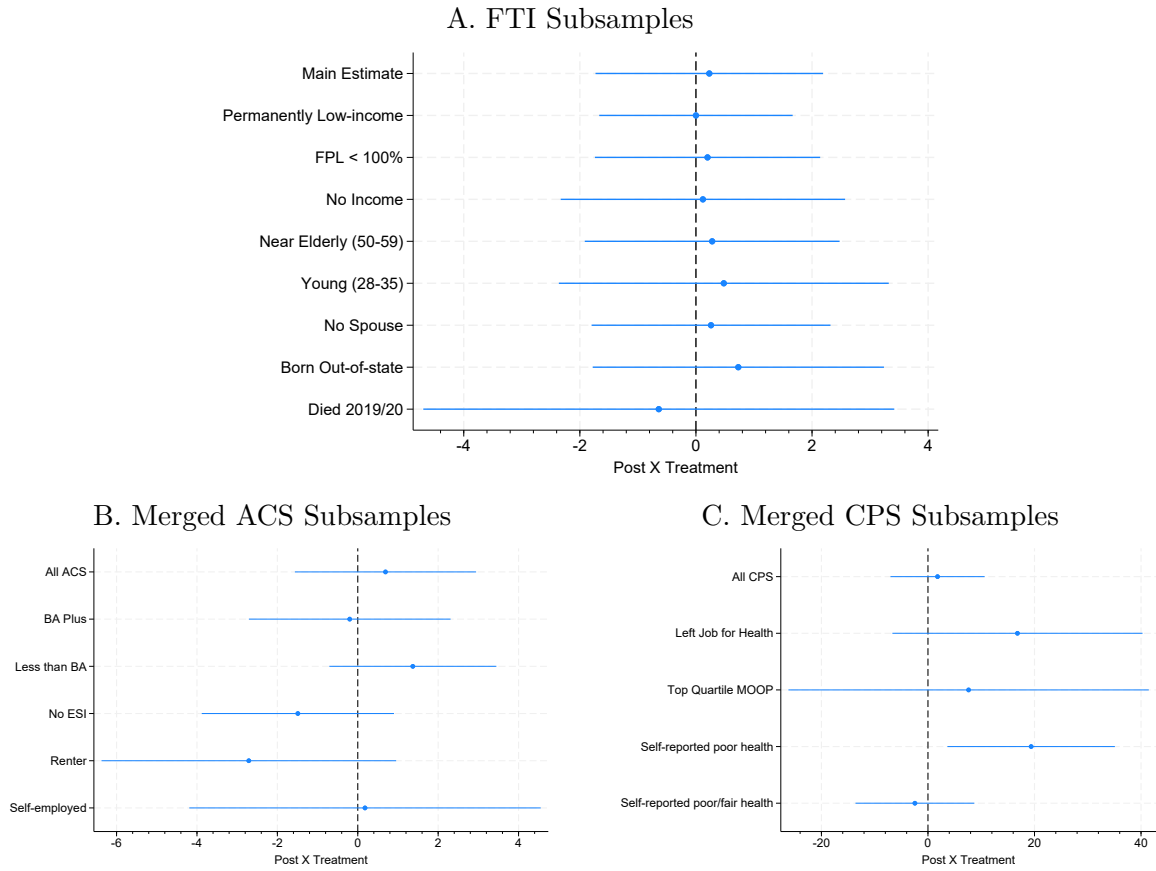


B. Interstate Flows Regression



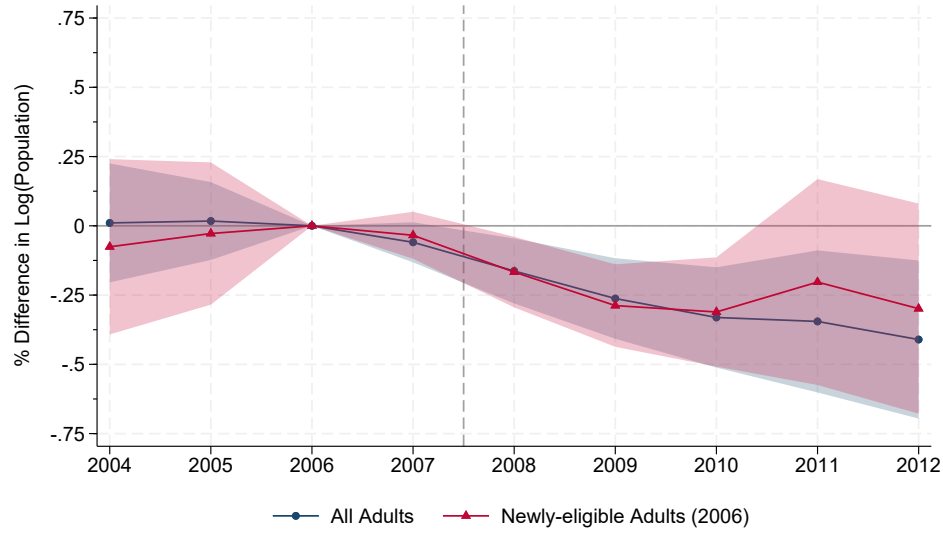
Notes: This figure summarizes how Medicaid expansion impacted the flow of newly-eligible adults. It is intended to show that there is no effect. Newly-eligible adults have characteristics that would make them eligible for Medicaid in the post-period in expansion states, but not in non-expansion states. They are defined in the pre-period (2012) as childless, aged 28-59, and earning income below 138% of the Federal Poverty Line. The sample is strongly-balanced at the individual level. Panel A show the fraction of newly-eligible adults that flow between states. The hollow red triangles for instance, show the fraction of people out-migrating to a treatment (expansion) state divided by the total number of residents in the control (non-expansion) state in a given year. Panel B plots the regression described by Equation 5. The dots represent the coefficient on $1\{Year = t\} \times [Expansion_d - Expansion_o]$, where d and o distinguish indicators for whether the destination or origin states are expansion states, respectively, in a regression on the log-odds ratio of moving between o and d . The right-hand y-axis scales the left-hand one by the mean migration rate to approximate how the change in flows impacts the overall migration rate. The sample is strongly-balanced at the individual-level. The shaded area shows the 95% confidence interval, with standard errors three-way clustered by origin \times year, destination \times year, and origin-destination pair. The dashed gray vertical bars separate the pre- and post-treatment periods.

Figure 7: Subsamples of Border County Difference-in-Difference in Log Population



Notes: This figure shows that my null results broadly hold, even for more mobile, lower-income and less-healthy subsamples. The dots indicate the coefficient on $Post_t \times Treatment_s$ for the border-county difference-in-difference analysis on the $\text{Log}(\text{Newly-Eligible Population})$, a pooled version of Equation 3 where $Post_t$ is an indicator for being after Medicaid expansion (2014-2018). Newly-eligible adults have characteristics that would make them eligible for Medicaid in the post-period in expansion states, but not in non-expansion states. They are defined in the pre-period (2012) as childless, aged 28-59, and earning income below 138% of the Federal Poverty Line. The lines indicate the 95% confidence interval with standard errors clustered at the state- and pair-level. Panel A shows subsamples that can be defined in my full sample of newly-eligible adults based on characteristics defined in 2012 or before. Panels B and C show the same, except that they are limited to newly-eligible adults that can merged to the ACS and CPS, respectively. “Permanently Low-Income” refers to newly-eligible adults that earn under 138% FPL in 2010, 2011 and 2012 consecutively. “No income” means lacking a 1040 and W2. “BA” refers to a bachelor’s degree. “ESI” refers to employer-sponsored health insurance. “MOOP” refers to out-of-pocket medical expenses. The subsamples are further explained in Section 5.7.

Figure 8: Population Response to State-level Great Recession Employment Shocks



Notes: This figure shows the impact of a -1 p.p. deviation in the state employment rate due to the Great Recession local shocks on all adults (blue circles) and newly-eligible adults (red triangles). Newly-eligible adults are defined in 2006, rather than 2012. Newly-eligible adults have characteristics that would make them eligible for Medicaid in expansion in the post-period in expansion states, but not in non-expansion states. They are defined in the pre-period (2006) as childless, aged 28-59, and earning income below 138% of the Federal Poverty Line. The dots represent the coefficients on $SHOCK_s \times 1\{Year = t\}$ for a difference-in-differences regression on $\text{Log(Population of All Adults)}$ as in Equation 6. The triangles are the coefficients for a similar regression on $\text{Log(Population of Newly-Eligible Adults)}$. Each regression is on a strongly-balanced panel at the individual-level. The shaded areas show the 95% confidence intervals, with standard errors clustered at the state-level. The standard deviation of $SHOCK_s$ is 2.9. The dashed gray vertical bars separate the pre- and post-treatment periods.

Tables

Table 1: Average Annual Migration (2010-2019)

| Subgroup | Public CPS | Public ACS | Federal Tax Information | | |
|-----------------------|-------------------|-------------------|-------------------------|--------------------|------------|
| | Interstate (1) | Interstate (2) | Interstate (3) | Intercounty (4) | Any (5) |
| All | 1.5 | 2.4 | 3.0 | 7.3 | 17.7 |
| Newly-eligible Adults | 1.8 | 2.7 | 4.7 | 10.5 | 25.6 |
| Sex | | | | | |
| Female | 1.4 | 2.2 | 2.8 | 7.0 | 17.4 |
| Male | 1.5 | 2.3 | 3.2 | 7.6 | 18.0 |
| Age | | | | | |
| 18-26 | 2.8 | 4.2 | 5.6 | 13.3 | 30.4 |
| 27-64 | 1.4 | 2.1 | 2.7 | 6.6 | 16.7 |
| 65 or above | 0.5 | 1.2 | 1.5 | 3.4 | 7.4 |
| Income | | | | | |
| No 1040 or W2 | - | - | 3.1 | 6.4 | 15.2 |
| Bottom Quartile | 1.8 | 2.9 | 3.9 | 9.5 | 24.2 |
| 25-50% | 1.5 | 2.2 | 3.2 | 8.4 | 21.4 |
| 50-75% | 1.3 | 1.9 | 2.5 | 6.5 | 15.0 |
| Top Quartile | 1.2 | 1.9 | 2.3 | 5.1 | 11.1 |
| Top 5% | 1.3 | 2.2 | 2.6 | 5.3 | 11.5 |
| Education | | | | | |
| Less Than High School | 0.8 | 1.3 | 1.9 | 5.1 | 15.6 |
| High School | 1.1 | 1.7 | 2.0 | 5.6 | 15.4 |
| Some College | 1.4 | 2.2 | 2.5 | 6.7 | 17.0 |
| College or more | 2.1 | 3.2 | 3.4 | 7.3 | 15.5 |
| Race/Ethnicity | | | | | |
| Asian Alone | 1.8 | 2.7 | 3.1 | 6.5 | 16.2 |
| Black Alone | 1.3 | 2.0 | 2.9 | 7.1 | 21.0 |
| Hispanic | 1.1 | 1.7 | 2.1 | 5.6 | 18.5 |
| White Alone | 1.5 | 2.3 | 2.6 | 6.5 | 14.5 |
| Homeownership | | | | | |
| Renter | 3.2 | 4.5 | 5.3 | 12.3 | 32.5 |
| Owner | 0.7 | 1.2 | 1.5 | 4.2 | 9.5 |

Note: This table compares demographic trends in migration as measured in different datasets by different subgroups. It is intended to show that these datasets agree on the trends by subgroup, but not on the level of migration. Columns 1 and 2 are calculated using public data, and show interstate migration in the CPS and ACS, respectively. In both cases, imputed values and individuals in group quarters are excluded. Column 3 shows the comparable value calculated from individual tax records. Column 4 shows the intercounty migration rate and Column 5 shows any change in address, as indicated by the anonymized MAFID generated by Census. Newly-eligible adults are the group maximally affected by Medicaid expansion, and are defined in 2012 as having no children and being between the ages of 28-59 and earning under 138% of the Federal Poverty Line.

Table 2: Comparison of Contiguous Border Counties

| | Non-expansion | Expansion |
|---|---------------|-----------|
| A. Mean 2012 Characteristics | (1) | (2) |
| Female (%) | 44.6 | 44.1 |
| Age | 44.1 | 44.5 |
| Household Size | 1.12 | 1.13 |
| High School or Less (%) | 48.5 | 49.0 |
| Household Income (\$) | 7,385 | 6,871 |
| Employer-sponsored Insurance (%) | 20.3 | 19.8 |
| Annual Mortality (%) | 0.70 | 0.74 |
| B. Intercounty Migration Rates (%) | | |
| Annual In-migration (2012) | 13.2 | 13.3 |
| Annual Out-migration (2012) | 13.3 | 12.9 |
| Cumulative In-migration (2008-2012) (%) | 27.8 | 28.6 |
| C. Counts | | |
| States | 18 | 20 |
| Counties | 169 | 158 |
| Newly-eligible Adults (2012) | 370,000 | 330,000 |

Note: This table compares descriptive statistics for newly-eligible adults residing the contiguous border counties used in the primary differences-in-differences analysis in 2012, as shown in Figure 2. It is intended to show their similarity, and provide reference statistics for interpreting the regression results. Panel A compares select descriptive statistics. Panel B shows intercounty migration rates, which are the relevant value for changes in county population. Panel C provides counts at different levels of observation. The number of newly-eligible adults has been rounded to protect privacy. The 327 counties shown form 307 contiguous border county pairs.

Table 3: State-to-State Flows Event Study

| | $\text{Log}(P_{odt}/P_{oot})$ (1) | $\text{Log}(P_{odt}/P_{oot})$ (2) |
|--|--------------------------------------|--------------------------------------|
| Post X [$\text{Expansion}_d - \text{Expansion}_o$] | 0.41 (1.84) | |
| Post X [Treatment \rightarrow Treatment] | | 2.32 (2.42) |
| Post X [Treatment \rightarrow Control] | | 1.11 (1.95) |
| Post X [Control \rightarrow Treatment] | | 3.61 (1.98) |

Note: This table shows the coefficients on Post_t (indicating after Medicaid expansion from 2014-2018) on the log odds ratio $\text{Log}(P_{odt}/P_{oot})$, where P_{odt} is the number of newly-eligible adults moving from origin state o to destination state d in year t , and P_{oot} is the number of these adults who remain in o . It is intended to show that there was no change in the direction of state-to-state flows after Medicaid expansion. Column 1 shows the pooled coefficient from Equation 5 and Figure 6 Panel B, where it is assumed that losing Medicaid eligibility—i.e. moving from an origin state o that has expanded to a d that has not ($\text{Post} \times -1$) is symmetric to gaining it ($\text{Post} \times 1$). Column 2 relaxes this assumption and shows each type of flow relative to Control \rightarrow Control. A Control \rightarrow Treatment flow indicates that the origin state o has not expanded, but that the destination state d has. The sample constitutes all newly-eligible adults (as defined in 2012) who reside in or move between the 20 treatment and 18 control states in a given year. Newly-eligible adults have characteristics that would make them eligible for Medicaid in the post-period in expansion states, but not in non-expansion states. They are defined in the pre-period (2012) as childless, aged 28-59, and earning income below 138% of the Federal Poverty Line. Standard errors are three-way clustered by origin \times year, destination \times year, and origin-destination pair. * indicates that $p < 0.05$.

Table 4: Medicaid Expansion v. Great Recession Employment Shock Impacts

| | Medicaid Expansion | | Great Recession | |
|---|--------------------|---------------------|------------------|---------------------|
| | Log(Pop) (1) | Cum. HH Inc. (2) | Log(Pop) (3) | Cum. HH Inc. (4) |
| Medicaid Expansion X (t=+3) | -0.85 (0.80) | 1,449 (3,054) | | |
| Great Recession Employment Deviation (-1 p.p.) X (t=+3) | | | -0.31* (0.10) | -752* (177) |
| Cumulative 3-year Out-migration (%) | 8.9 | | 12.1 | |
| Cumulative 3-year Household Income (\$) | | 27,232 | | 24,169 |

Note: This table compares the impacts of Medicaid expansion to Great Recession employment shocks on population and household income. Column 1 shows the coefficient on *Expansion_s* in 2016 (3 years after expansion) for the border-county difference-in-difference design described in Equation 3. It uses the sample of newly-eligible adults as defined in 2012, and estimates are weighted by their population in 2012. Standard errors are clustered at the contiguous border-county pair- and state-levels. There are 307 pairs for 327 counties across 28 states. Cumulative 3-year Out-migration reports the total out-of-state migration rate for the first 3 years after treatment. Column 2 runs the same regression with the outcome replaced by cumulative household income. Cumulative 3-year household income reports the total value for the first 3 years after treatment. Results are in 2023 dollars. Column 3 is the coefficient on a -1 p.p. deviation in employment in 2010, as shown in red in Figure 8. It uses the sample of newly-eligible adults as defined in 2006, and estimates are weighted by their population in 2006. Standard errors are clustered at the state-level. They include all 50 states and DC. The standard deviation in the the employment shock is 2.9 p.p. Column 4 shows the same regression as Column 3, except that the outcome is cumulative household income. * indicates that $p < 0.05$.

Table 5: Comparison of Stayers and Movers over 5 Years

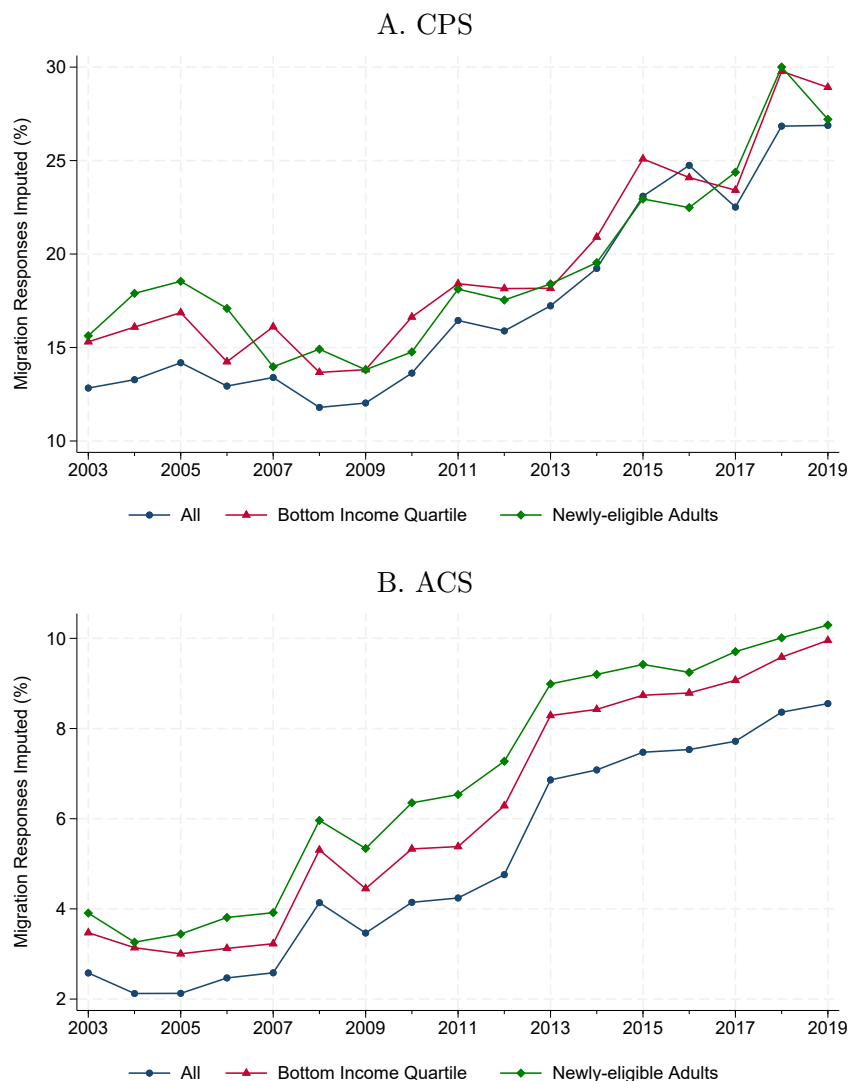
| | Stayers in Expansion States (1) | Arrivals to Expansion States (2014) (2) |
|-------------------------------|---------------------------------------|---|
| Outcomes (2014-2018) | | |
| Years Eligible | 3.4 | 2.9 |
| Years Enrolled | 1.9 | 1.8 |
| Average Household Income (\$) | 27,970 | 35,457 |
| Died 2019/2020 (%) | 2.2 | 1.9 |
| Characteristics (2012) | | |
| Age | 50.0 | 48.7 |

Note: This table compares newly-eligible adults who live in expansion states in 2014, and stayed there through 2018 (Column 1) to those living in non-expansion states in 2013, who moved to expansion states in 2014 and remained there through 2018 (Column 2). Thus, individuals in both columns were in expansion states for 5 years. Newly-eligible adults are defined in the pre-period (2012) as childless, aged 28-59, and earning income below 138% of the Federal Poverty Line. This table is intended to show the maximum gains for welfare migration, and compare those to incumbents in those states to understand selection.

A Supplementary Results

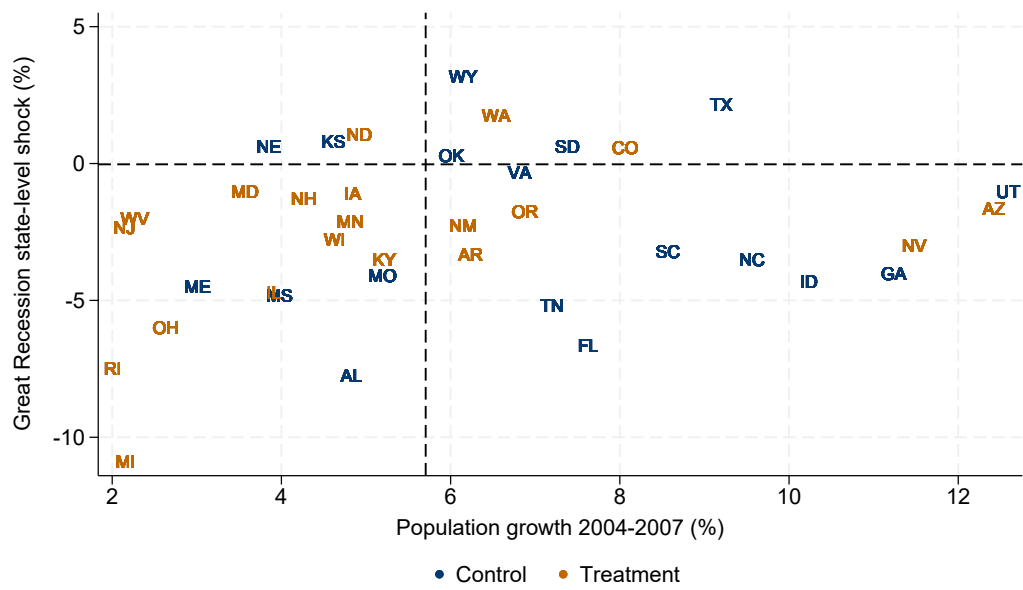
A.1 Figures

Figure A.1: Percent of Migration Rates Imputed (2003-2019)



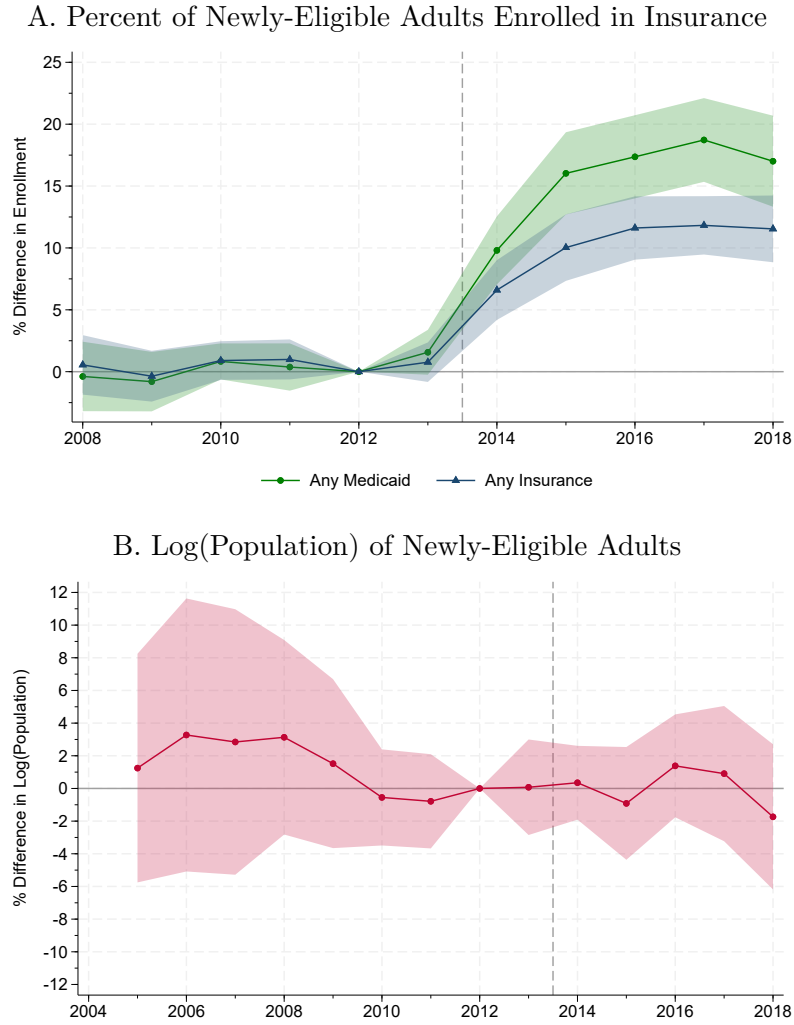
Notes: This figure shows the imputation rate for migration questions from 2003-2019 for the Current Population Survey March Supplement (Panel A) and American Community Survey (Panel B). Note that imputation is only one component of non-response, because it is conditional on getting a response at the surveyed address. The blue dots show the rate for all adults, the red triangle show the rate for the bottom income quintile, and the green diamonds show the rate for newly-eligible adults. Newly-eligible adults are defined as childless, aged 28-59, and earning income below 138% of the Federal Poverty Line.

Figure A.2: Correlation between Treatment and Pre-period Trends



Notes: This figure shows the percent change in the Log(Population) of newly-eligible adults compared to 2012. It is intended to show that there are secular differences between the growth rates of non-expansion (control) states in blue and the expansion (treatment) states in brown, such that the control group does not satisfy the parallel trends assumption necessary for differences-in-differences. Newly-eligible adults have characteristics that would make them eligible for Medicaid in expansion in the post-period in expansion states, but not in non-expansion states. They are defined in the pre-period (2012) as childless, aged 28-59, and earning income below 138% of the Federal Poverty Line. The sample is strongly balanced, so the difference in trends is enabled by differences in population levels in 2012, and the existence of states which are not treatment or control. The dashed gray vertical bar separates the pre- and post-treatment periods.

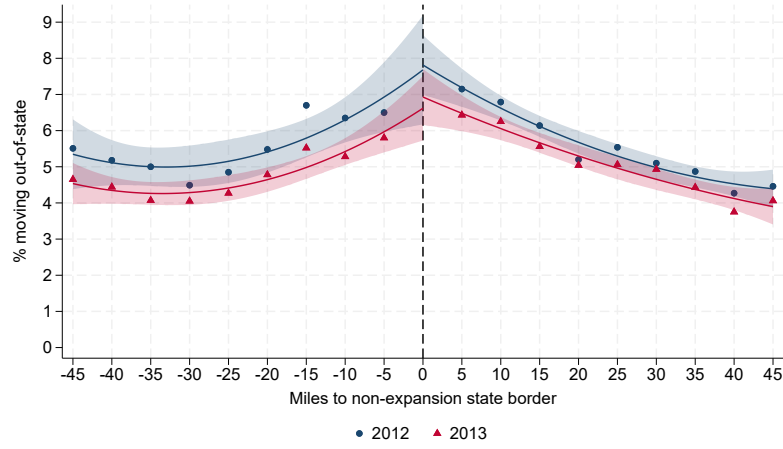
Figure A.3: Cross-sectional State-level Difference-in-differences in Public Data



Notes: This figure shows how Medicaid expansion affected newly-eligible adults in expansion states relative to control states using a state-level difference-in-differences design using public ACS data. Newly-eligible adults are defined as being childless, ages 27-64 and earning below 138% of the Federal Poverty Line. Panel A plots the coefficients on $Expansion_s \times 1\{Year = t\}$ in a regression with year and state fixed effects on “any Medicaid” (green) and “any insurance” (blue). The outcome variables are not available before 2009. Panel B is similar except the outcome is $\text{Log(Population of Newly-Eligible Adults)}$. Point estimates are indicated by the connected dots and the shaded areas show the 95% confidence interval, with standard errors clustered at the state-level. The dashed gray vertical bars separate the pre- and post-treatment periods.

Figure A.4: Difference in Discontinuities in Out-of-state Migration

A. Out-migration Rates by Distance to the Border and Year



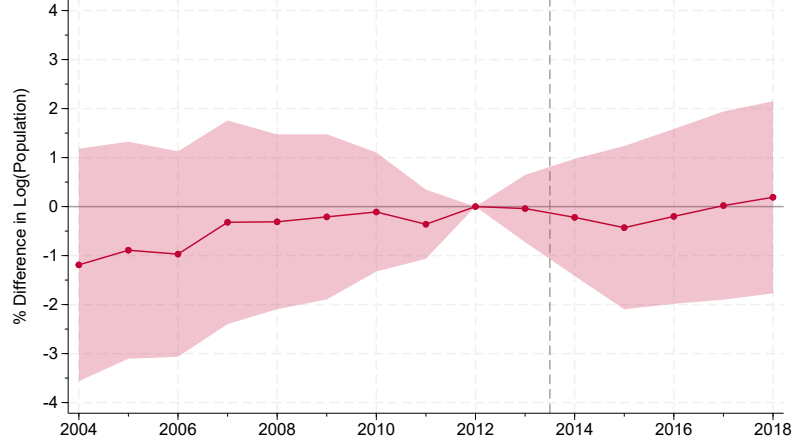
B. Regression Discontinuity Estimates relative to 2012



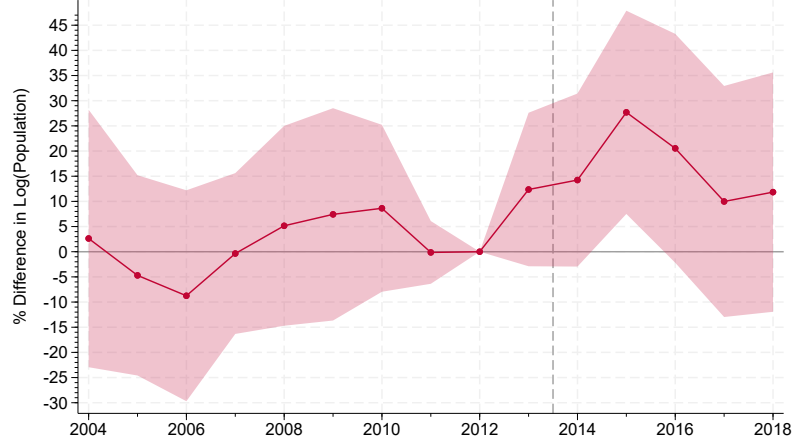
Notes: This figure shows how Medicaid expansion affected newly-eligible adults in expansion states relative to control states using a distance-to-the-border regression discontinuity design. Newly-eligible adults have characteristics that would make them eligible for Medicaid in expansion in the post-period in expansion states, but not in non-expansion states. They are defined in the pre-period (2012) as childless, aged 28-59, and earning income below 138% of the Federal Poverty Line. Individuals are assigned a distance based on their tract or, secondarily, zip code centroid. Panel A serves as an eye-test where each dot represents the out-of-state migration at that distance from the border in 2012 (blue) or 2013 (red). The lines and shaded areas represent the line of best fit and the 95% confidence interval. Panel B plots the size of the discontinuity as estimated using [Calonico et al. \(2017\)](#) and a triangular kernel. Dots show the estimate relative to the 2012 estimate of 0.47% (s.e. 1.12), and the shaded area represents the 95% confidence interval.

Figure A.5: Additional Border-County Difference-in-differences

A. Log(Newly-Eligible Population), No Minimum Wage Controls

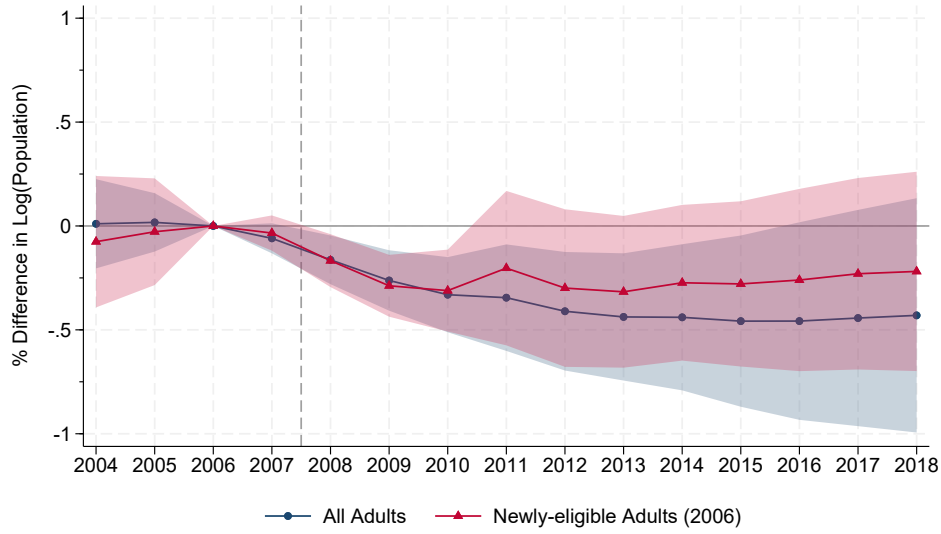


B. Log(Newly-Eligible Population), Poor Health



Notes: This figure shows how Medicaid expansion affected newly-eligible adults in expansion states relative to control states using a border-county difference-in-differences design. Newly-eligible adults have characteristics that would make them eligible for Medicaid in expansion in the post-period in expansion states, but not in non-expansion states. They are defined in the pre-period (2012) as childless, aged 28-59, and earning income below 138% of the Federal Poverty Line. Panel A plots the coefficients on $Expansion_s \times 1\{Year = t\}$ in Equation 3, but without controlling for the minimum wage. Panel B controls for the minimum wage, but subsets to people reporting poor health to the CPS. The panel is strongly-balanced at the individual-level. Point estimates are indicated by the connected dots and the shaded areas show the 95% confidence interval, with standard errors clustered at the state- and pair-level. The dashed gray vertical bars separate the pre- and post-treatment periods.

Figure A.6: Population Response to Great Recession Employment Shocks (Extended Post-Period)



Notes: This figure shows the impact of a -1 p.p. deviation in the state employment rate due to the Great Recession local shocks on all adults (blue) and newly-eligible adults (red). Newly-eligible adults are defined in 2006, rather than 2012. Newly-eligible adults have characteristics that would make them eligible for Medicaid in expansion in the post-period in expansion states, but not in non-expansion states. They are defined in the pre-period (2006) as childless, aged 28-59, and earning income below 138% of the Federal Poverty Line. The dots represent the coefficients on $SHOCK_s \times 1\{Year = t\}$ for a difference-in-differences regression on $\text{Log}(\text{Population of All Adults})$ as in Equation 6. The triangles are the coefficients for a similar regression on $\text{Log}(\text{Population of Newly-Eligible Adults})$. Each regression is on a strongly-balanced panel at the individual-level. The shaded areas show the 95% confidence intervals, with standard errors clustered at the state-level. The standard deviation of $SHOCK_s$ is 2.9. The dashed gray vertical bars separate the pre- and post-treatment periods.

B Data Construction

B.1 Sample Construction

My primary analysis sample is newly-eligible childless adults. Newly-eligible requires subsetting to prime-age low-income citizens without disabilities. Table B.1 shows how each of these choices impacts my sample size, and how the number of individuals in my data compare to those of similar description in the ACS. Note that, wherever comparable, my numbers are similar to those in the ACS.

B.2 Additional Details on Variable Definitions

This section is meant to be read in conjunction with Section 3.2. It provides additional details and statistics, but does not restate details from the main paper.

Location: Using the modal address from the information return is a fairly innocuous assumption, since only few individuals have conflicting addresses across returns. Unfortunately, because of how my data is commingled in my Census project space, I cannot test alternate assumptions—such as using a particular ranking of information returns—without requesting additional data.

Most people are observable in most years. Foster et al. (2018) show high “survival” rates in the tax data across of variety of subgroups.

Household Income: There is no entirely satisfying way to deal with the income of non-filers who lack a W2, but have other information returns. If I exclude them entirely, then I have no individuals with \$0 income in my dataset, and thereby exclude a large fraction of the Medicaid-eligible. Including them as having \$0 is also likely incorrect, as the existence of these information returns points towards some kind of income. However, as a practical matter of determining Medicaid eligibility, this appears not to matter. If I merge these individuals to the CPS in order to see their self-reported income, most people with neither a Form 1040 nor a W2 report an income that would make them Medicaid-eligible. Also note that my subsample of individuals with positive income, reported in Figure 5, shows no discrepancy with my primary result.

Medicaid Enrollment: My primary measure of “any Medicaid” is maximally inclusive. It codes anyone who appears in the Medicaid data in any state who has any number of days enrolled in a month as having Medicaid. This includes people who match to records in multiple states, and does not require that the enrollee match to records in the state in which they appear to reside.

The exception to this is when I check for information frictions. In this case, I require that respondents spend either the entire month on Medicaid or 0 days, so as to know perfectly whether they are on Medicaid at the time they respond to the ACS.

B.3 Tables

Table B.1: Impact of Sample Restrictions

| Sample | Number of Individuals (millions) | |
|-----------------------------------|----------------------------------|-----|
| | FTI | ACS |
| In 2012 tax data | 241 | - |
| + alive in 2012 | 238 | - |
| + resident in the 50 states or DC | 234 | 240 |
| + citizens | 214 | 220 |
| + birth cohort from 1953 to 1984 | 111 | 121 |
| + childless | 60 | 58 |
| + under 138% FPL | 15 | - |
| + able-bodied | 12 | - |

Note: This table compares sample counts in the Federal Tax Information at Census to the nearest-equivalent numbers in the ACS for the year 2012