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By

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Medicaid as an Investment in Children: What is the Long-Term Impact on Tax Receipts?*

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Abstract

We use administrative data from the IRS to examine the long-term impact of childhood Medicaid expansions. We use eligibility variation by cohort and state that we can relate to outcomes graphically. We find that children with greater Medicaid eligibility paid more in cumulative taxes by age 28. They collected less in EITC payments, and the women had higher cumulative wages. Our estimates imply that the government will recoup 56 cents of each dollar spent on childhood Medicaid by the time these children reach age 60. This return does not include estimated private gains from increased college attendance and decreased mortality.

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1 Introduction

With the ongoing implementation of the Affordable Care Act (ACA) of 2010, the United States is poised to experience a large increase in health insurance coverage. The literature provides some evidence on the short term impacts of increases in health insurance coverage, but it is possible that some of the most important gains from health insurance coverage manifest themselves in the long term, after prolonged exposure. Although we will not know the realized long term impacts of the ACA for many years to come, we can examine the long term impact of previous health insurance expansions now.

Using data from the Internal Revenue Service (IRS), we examine the long term impact of expansions to Medicaid that occurred in the 1980's and 1990's. Medicaid, a public health insurance program for low income persons, began almost 50 years ago in 1965. It expanded dramatically in the 1980's and again in the 1990's with the establishment of the State Children's Health Insurance Program (SCHIP) in 1997. These combined "Medicaid" expansions resulted in a tremendous amount of variation in health insurance eligibility for similar children in different states and birth cohorts. Some children affected by these expansions have reached age 31 in our data, allowing us to examine outcomes for them as adults. Our main outcomes of interest include net tax receipts, Earned Income Tax Credit (EITC) receipts, wages, mortality, and college attendance.

One reason why we might expect to observe long term impacts of Medicaid expansions is that a very large literature demonstrates robust short-term impacts of health insurance expansions on coverage and on a small variety of other outcomes. Seminal papers by Janet Currie and Jonathan Gruber examine a doubling of childhood eligibility between 1984 and 1992 (Currie and Gruber, 1996b) and the inclusion of pregnant women in coverage beginning in 1985 (Currie and Gruber, 1996a). Pioneering the use of a simulated instrument methodology that isolates policy variation, they find that Medicaid eligibility increased utilization of medical care and reduced childhood and infant mortality. Card and Shore-Sheppard (2004) focus on children's eligibility increases induced by two federal expansions included in the Currie and Gruber (1996b) analysis and find that the eligibility increases had modest impacts on contemporaneous health insurance coverage, but they do not examine any other outcomes. Several other papers revisit these expansions and later SCHIP expansions, all finding impacts on coverage, generally from 5 to 24 percent.[1]

Beyond coverage, a small number of papers have found contemporaneous impacts on other outcomes that could be potential mechanisms for long term health impacts. For example,

[1] See Blumberg et al. (2000); Rosenbach et al. (2001); Zuckerman and Lutzky (2001); Cunningham et al. (2002a); Cunningham et al. (2002b); Lo Sasso and Buchmueller (2004); Ham and Shore-Sheppard (2005); Hudson et al. (2005); Bansak and Raphael (2006); Buchmueller et al. (2008); and Gruber and Simon (2008).

Lurie (2009) finds evidence of increased doctor visits as a result of Medicaid expansions, and Joyce and Racine (2003) find evidence of higher vaccination rates in response to the same expansions. Beyond direct impacts on health care utilization, Yelowitz (1998) finds that parents whose children who are exposed to Medicaid expansions are more likely to be married, and Gruber and Yelowitz (1999) find that parents with access to public health insurance save more, potentially making college more accessible to their children.

Bolstering the previous quasi-experimental literature, findings from recent Oregon Health Insurance Experiment of 2008, in which the state of Oregon expanded coverage to childless adults through a lottery, demonstrate short-term impacts of Medicaid expansions on a large variety of outcomes, also suggesting that long-term impacts could be possible in our setting.[2] However, the long term impact of the Oregon Health Insurance Experiment will not be known until more time has passed. Moreover, two years after the health insurance eligibility lottery took place, individuals who were randomized out of the lottery became eligible, so future estimates of the long term impact of the Oregon Health Insurance Experiment will only reflect up to two years of additional coverage for the lottery winners.

One advantage of the expansions that we study is that they resulted in several years of expanded eligibility for the same children. Therefore, our results can potentially shed light on the policy-relevant impact of expanding health insurance eligibility for all of childhood, as opposed to the impact of expanding health insurance eligibility for a single year of childhood. Given that our intervention occurred over a longer time period than other interventions examined in the literature, we are also potentially more likely to observe long-term impacts.

Although most existing literature examines the impact of Medicaid expansions contemporaneously or within two years of the expansion, we are aware of a small number of papers that examine the impact of health insurance expansions several years after the expansions occurred. These papers provide other potential mechanisms for our findings. Meyer and Wherry (2012) revisit one of the federal expansions examined by Card and Shore-Sheppard (2004) with Vital Statistics data, which allows them to examine childhood and teen mortality after the expansions. They find a decrease in mortality for black teens, but they cannot reject increases for white teens. Sommers et al. (2012), examine the impact of much more recent expansions in Medicaid eligibility in three states since the year 2000, and they find reductions in mortality up to five years after the expansion. Levine and Schanzenbach (2009) examine the impact of availability of SCHIP and Medicaid at birth on children's test scores

[2]Results from the first year after the experiment provide evidence that increased Medicaid eligibility led to increased health insurance coverage, increased medical utilization, increased emergency room visits, lower out-of-pocket medical expenditures, and better self-reported health (Finkelstein et al. (2012) and Taubman et al. (2014)). Results from two years after the experiment show decreases in depression, increased use of preventive services, and no impact on clinical measures of cholesterol and diabetes (Baicker et al., 2013b). There are no detectable impacts on labor market participation or earnings (Baicker et al., 2013a).

and find an impact on reading scores but not math scores. Recent work by Cohodes et al. (2014) finds that childhood exposure to Medicaid increases schooling, and other recent work by Miller and Wherry (2014) finds that *in utero* exposure to Medicaid decreases obesity and some types of hospitalizations in adulthood. However, impacts of Medicaid on adult economic outcomes are hard to find. Boudreaux (2014) examines the impact of the staggered adoption of Medicaid by states on later life economic outcomes for children exposed to Medicaid during childhood but does not find any statistically significant impacts, likely because the Panel Study of Income Dynamics (PSID) data that he uses is too small to detect changes based on the state-level variation that he examines.

We contribute to the literature on the long term impact of health insurance eligibility by using data from the IRS, a source of data that has never to our knowledge been used for this purpose. The main advantage of the tax data is that they allow us to follow individuals, as well as their parents and children, longitudinally for a long time period. One of the main difficulties in studying long term outcomes is the need for longitudinal data. Especially if the intervention occurs over several years, analysis requires data in all intervening years, not just the beginning and the end. Another advantage of the tax data is that it includes all individuals with any interaction with the US tax system from 1996 to the present, yielding a very large sample size. Using the tax data, we can move beyond traditional outcomes examined in the literature to include tax outcomes, labor market outcomes, and educational outcomes.

Although some studies have examined contemporaneous changes in labor market outcomes in response to changes in adult Medicaid coverage, we examine long term impacts on labor market outcomes for eligible children. For adults, expansions in health insurance coverage can have work disincentive effects because the provision of health insurance coverage might encourage the consumption of additional leisure. Indeed, the literature generally finds little impact on labor supply (see the review by Gruber and Madrian (2004)), and some recent papers show disincentive effects for adults: Dave et al. (2013) for pregnant women; Garthwaite et al. (2013) for childless adults; and Borjas (2003) for immigrants. However, as children generally do not work, the work disincentive mechanism is likely not as strong, and their labor force participation and earnings could actually increase because of improved health or increased parental resources.

Indeed, we find evidence of improved labor market outcomes for the children we study. These individuals paid more in cumulative taxes by age 28. They collected less from the government in cumulative EITC payments, and the females earned more in cumulative wages by age 28. Incorporating additional data from the Medicaid Statistical Information System (MSIS), we find that the government spent $872 in 2011 dollars for each additional year of

Medicaid eligibility induced by the expansions we study. Using this figure as well as the estimated increase in tax payments discounted at a 3% rate, assuming that tax impacts will persist in percentage terms, we estimate that the government will recoup 56 cents for each dollar invested by the time these children reach age 60. This return on investment ignores other benefits that accrue directly to the children, including estimated decreases in mortality and increased college attendance. Moreover, this return on investment is for eligibles, not beneficiaries. Using further data from MSIS, we find that each additional year of Medicaid eligibility from birth to age 18 results in approximately 0.58 additional years of Medicaid receipt. Therefore, if we scale our results by the ratio of beneficiaries to eligibles, the return on investment is about twice as large.

In the next section, we discuss data. We discuss our simulated instrument methodology in Section 3. We then present our results and evaluate the return on investing in Medicaid in Section 4. We then perform a novel exercise that graphically illustrates the variation that drives our results in Section 5 . We examine the robustness of our results in Section 6. Section 7 concludes.

2 Data

The main source for this project, the Internal Revenue Service (IRS) Compliance Data Warehouse (CDW), make our analysis possible because they allow us to link children to the their parents and follow them longitudinally through adulthood. Furthermore, they are broadly representative and they can allow us to detect impacts with a high level of precision because they include all individuals with an interaction with the tax system from 1996 to the present. The CDW data include most elements of the population of federal tax documents from tax year 1996 through 2012. The CDW data have been used in very few studies because of extremely limited accessibility due to the confidential nature of these data. (Exceptions include Chetty et al. (2013a), Chetty et al. (2011), and Yagan (2014)). Our project is the first to our knowledge to use these data to evaluate the intersection of health policy and long-term tax administration.

Because our goal is to examine long term impacts of Medicaid eligibility on outcomes, we want to use the oldest cohorts possible. The 1981 cohort is the oldest cohort that we are comfortable using because our data begin in 1996, allowing us to observe parental income to determine Medicaid eligibility beginning at age 15 for this cohort (we calculate age as tax year minus birth year so that age is the same for everyone born in the same year). The 1984 cohort is the youngest cohort that we are comfortable using because they are the youngest cohort for which we can observe outcomes at age 28. Our data include all individuals born in the calendar years 1981 to 1984, allowing us to observe the vast majority of all 14.6 million children born during that time period.

To determine Medicaid eligibility during childhood, we link adult children in our cohorts of interest to their parents using the Form 1040 from tax year 1997, the first year such data are broadly available. As soon as we have linked children to parents in 1997, we can follow the parents in all other years, even if the parent did not claim the child. We restrict our main sample to children whose parents filed in every tax year from 1996 (the first year of our data) through the variable year in which the child turned 18 (1999 for the 1981 birth year through 2002 for the 1984 birth year). In any given year, the vast majority of low income parents file the Form 1040 because the EITC and the child tax credit are refundable, providing an incentive to file even if the taxpayer faces no tax liability. Furthermore, federal income tax witholdings reported on the W-2 are forfeited if a 1040 is not filed. However, by requiring parents to file in *every* childhood year that we can observe, we lose about 20% of our sample.[3] We examine the robustness to imputing eligibility for children of non-filers in Section 6.

While Form 1040 is important for linking children to their parents, most of the outcomes that we observe do not require the adult children to file Form 1040 themselves, given the availability of a rich set of information returns. For example, the W2, which is filed by employers, gives information on wages, payroll taxes, and federal income tax withholdings. After other minor sample selection steps, we arrive at a final estimation sample of 10,040,234 children.[4]

Within the CDW, using parental income and household structure for each child at each age, we calculate Medicaid eligibility using a calculator that we compiled from many sources.[5] Because we only observe the information needed for the calculator once per year, we focus on the eligibility threshold in December of a given year for a child in a given state in given birth month cohort, as a function of the federal poverty level (the federal poverty level is a statutory function of household size, income, year, and state – all states except Alaska and Hawaii share

[3]Part of the reason why we lose sample size by requiring parents to file in every year is that there are 3,429,112 Form 1040 records missing from our CDW data in the state of Florida in 1999. Children whose parental records are missing will be excluded from our sample as non-filers.

[4]Census estimates show that approximately 14.6 million children were born in 1981-1984. Our CDW data starts with 13,471,359 dependents claimed the Form 1040 in 1997 that were born between 1981 and 1984 (we rely upon the DOB maintained by SSA linked to the dependent's SSN rather than taxpayer-provided DOB on the Form 1040). However, some of these dependents are duplicates claimed on more than one return. Addressing this issue and other minor issues, we arrive at 13,113,877 children in the 1997 matched dependent file. We lose additional children for whom we cannot identify a state of residence, arriving at 12,845,285 children before restricting the sample to children whose parents file in every tax year from 1996 through age 18.

[5]Several individuals contributed to the development of our calculator. Documentation with acknowledgments is available here: `http://www.econ.yale.edu/~ak669/Medicaid_Calculator_Documentation_BKL_latest.pdf`.

the same level). The focus on December eligibility should slightly overstate eligibility, which generally increased over time. Therefore, our estimates of Medicaid eligibility on outcomes will be conservative. Since our data begin in 1996, we calculate Medicaid eligibility based on contemporaneous parental income and family characteristics starting at age 15 for our oldest cohort and age 12 for our youngest cohort. To incorporate eligibility variation at earlier ages, we assume that family size and income as a percentage of the federal poverty line were the same in years prior to 1996 as they are in the year of parent-child linkage (1997). We then calculate Medicaid eligibility using our calculator in those years.

We examine the impact of Medicaid eligibility from birth through age 18 on several later life outcomes. In our preferred specifications, we specify these outcomes cumulatively instead of at a point in time so that we can capture a long-term view of the effect of Medicaid and so that we harness the longitudinal nature of our data to reduce measurement error. For example, examining cumulative wages allows us to observe whether wages ultimately go up for individuals who attend college, even though contemporaneous wages might be lower at some ages for individuals attending college. We now consider our five main outcomes in turn and describe how we derive them from the tax data.

Tax Payments. Our main outcome is cumulative income and payroll tax payments from age 18 to the age of interest, adjusted by the CPI-U to 2011 dollars.[6] Because payroll taxes are dependent on wages, and income taxes used to calculate Earned Income Tax Credit, this tax outcome is a broad measure that should reflect both our wage and EITC findings. When calculating the government return on investment from Medicaid spending, we prefer this measure, since it is very broad.

EITC Receipt. We examine EITC receipt directly since EITC is administered through the tax system. The response of EITC receipt to changes in Medicaid eligibility should shed light on whether Medicaid creates a culture of dependence on the government. EITC receipt is also an outcome relevant to the government budget. Even though EITC generosity expanded during our period of study, we do not adjust our estimates for increases in EITC generosity in our preferred specification because the actual spending is budget-relevant. All reported EITC amounts are in 2011 dollars and they represent EITC payments to the entire household filing unit.

Wages. To evaluate the impact of eligibility on wages, we consider cumulative wages unconditional on working, meaning that non-workers in a given year have zero wages according to our measure. For this measure, we only include wages as measured on the W-2, aggre-

[6]We start with the income tax after credit (L55 of Form 1040), then subtract refundable credits (EITC receipt, additional child tax credit, and the refundable portion of AOTC). We include all income taxes for the household filing unit. To calculate payroll tax payments, we use the employee portion of payroll taxes reported on the W-2 across employers, only for the individual of interest, and the payroll taxes reported on Schedule SE for the self employed.

gated across an employee's employers in a given tax year. In contrast to tax payments, we calculate wages for individuals, and not for households. To mitigate the influence of outliers, we censor wages above ten million dollars. All wages are in 2011 dollars.

Mortality. We consider the effect of Medicaid eligibility on health by examining mortality. Mortality is measured well in our data through Social Security Administration (SSA) death records. However, because our sample only includes children who are alive when our data begins in 1996, we only observe mortality starting at age 12 for children born in 1984 and age 15 for children born in 1981. Because we select the sample to include only those children for whom we can calculate eligibility from birth to age 18, we do not include children who die before age 18 in our analysis. Given that mortality is higher for younger children, only observing mortality for older children should bias us against finding mortality impacts and should make any impacts that we do find more striking.

College Attendance. We evaluate the impact of Medicaid eligibility on higher education by looking at likelihood of having ever attended college by a given age. We observe this outcome in the CDW data because colleges file 1098-T information returns to the IRS that indicate whether individuals are enrolled in college for the purpose of administering a variety of tax incentives, including the American Opportunity Tax Credit and Lifetime Learning Credit. We consider this metric in lieu of other metrics such as years enrolled in college, because increased years of college attendance is not an unambiguously positive outcome. Furthermore, our data only allow us to observe enrollment and not graduation rates.

Medicaid Spending. Even though we can calculate Medicaid eligibility using program rules applied to our data, we cannot observe Medicaid spending or Medicaid take-up directly. Most other studies of Medicaid eligibility face the same issue. To address it, we supplement our data with data on Medicaid spending from other sources. These data give broader context to our eligibility findings. To obtain an estimate of the return on investment that the government receives by providing Medicaid to children, we need data on how much the government spends to provide Medicaid to children, as well as an estimate of how much the government recoups in the long term through collection of higher tax payments. Tax payments are available in the tax data, but Medicaid spending is not.

We calculate Medicaid spending on children at the state-year level from 1981 to the present using administrative data from the Medicaid Statistical Information System (MSIS). Because these data are only available by state and year and not by individual or by birth cohort, we need to incorporate additional data to calculate per-eligible Medicaid spending on only those children in our birth cohorts of interest from 1981–1984.[7] We then run regressions

[7] We apply our calculator to the Current Population Survey (CPS), to determine the share of eligible children in each birth year for each enrollment year. We interact these fractions by the intercensal population estimates by birth year and enrollment year. We then aggregate eligible counts across all children ages 0 to

using this measure as a dependent variable, which gives us the change in spending per eligible associated with our expansions of interest.

Medicaid Take-up. Since policymakers can manipulate Medicaid eligibility directly, eligiblity is arguably more policy-relevant than take-up. Furthermore, since individuals who utilize services can be signed up for Medicaid coverage retroactively, focusing on eligibility eliminates the need to differentiate enrollment from conditional coverage. Although we prefer Medicaid eligibility measures to Medicaid take-up measures for several reasons, data on take-up provides a more complete picture of the impact of Medicaid on outcomes. If take-up rates are very low, we would not expect to find large impacts of Medicaid on outcomes. To calculate measures of Medicaid take-up by cohort, we use the MSIS data and the same methodology that we use to calculate Medicaid spending described above.

3 Methods

To examine the impact of increased Medicaid eligibility on outcomes, we exploit variation in total years of eligibility during childhood from birth to age 18. The impact of eligibility over the entire course of childhood is likely more policy-relevant than the impact of contemporaneous periods of eligibility because legislation generally extends Medicaid eligibility to children for the course of their entire childhoods rather than for just a single year. However, it is much more difficult to examine the impacts of longer periods of Medicaid eligibility because doing so requires richer longitudinal data.

Because we have longitudinal data on tax filers, our data allow us to calculate eligibility for the same individual across several years, which is not possible with other sources of data such as the Current Population Survey (CPS). We calculate eligibility at each age using our calculator. We then sum a child's eligibility over all ages from 0 to 18 to obtain the number of years of Medicaid eligibility for each child over his entire childhood, $\sum_{t=0}^{18} M_{i,a=t}$, our main explanatory variable of interest. This explanatory variable necessitates specifications that are different from the contemporaneous specifications typically used in the literature. We estimate instrumental variable specifications of the following form:

$$Y_{i,a=A} = \beta \sum_{t=0}^{18} M_{i,a=t} + \gamma_c + \gamma_{s,a=15} + X_{i,a=15} + \epsilon_{i,a=A} \quad (1)$$

18 in each enrollment year and determine the percent of that eligibility driven by each of our birth cohorts, 1981 through 1984. Then, under the (potentially strong) assumption that total spending on each cohort is proportional to the number of eligible children in each cohort, we calculate per eligible Medicaid spending by year of birth cohort for each enrollment year for each state in our data, indexed to 2011 dollars. We then calculate an aggregate measure of spending per child as the sum of per eligible spending for each year the child is eligible and adjust spending to 2011 dollars.

$$\sum_{t=0}^{18} M_{ics,a=t} = \delta \sum_{t=0}^{18} I_{cs,a=t} + \gamma_c + \gamma_{s,a=15} + X_{i,a=15} + \eta_i \qquad (2)$$

where Y measures an outcome for individual i at adult age A, which can take on any value from 19 to 31 in our data. We run these regressions for outcomes Y at all ages A from 19–31, covering all of the ages in which we can observe long term outcomes after childhood in our data for our cohorts of interest. We focus on results through age 28 because that is the last age for which the results are based on all four cohorts (at age 31, we only see individuals born in 1981, at age 30 we see individuals born in 1981 and 1982, ..., and at age 28 we at last see individuals born in 1981–1984). For completeness, we report results through age 31 in Section 5.

The coefficient β gives the impact of a year of Medicaid eligibility on the adult outcome of interest. Our specification includes fixed effects for each birth month cohort γ_c from January 1981 to December 1984 and each state at age 15 $\gamma_{s,a=15}$. We focus on controls at age 15 because age 15 is the first age at which we can observe all cohorts. We also include a vector of control variables $X_{i,a=15}$ at age 15, which includes fixed effects for the number of siblings, tax filing status of the parents (head of household, joint, married filing separately), a spline on total positive income on the parents' tax return (re-estimated for every sample), and a female dummy. We cluster our standard errors by state, defined as state at age 15, to account for arbitrary correlations within states over time.

We begin by estimating Equation (1) directly via OLS. Because low-income individuals are eligible for Medicaid, a simple OLS comparison of Medicaid eligibles to non-Medicaid eligibles will likely reflect individual characteristics instead of the impact of Medicaid policy. Therefore, we expect OLS estimates to be biased toward demonstrating deleterious impacts of Medicaid on outcomes. In our preferred specifications, we instrument Equation 1 using the first stage Equation 2. We are concerned that individual characteristics determine Medicaid eligibility and have an independent effect on outcomes. Therefore, we construct a simulated instrument that affects individual eligibility but only plausibly affects outcomes through individual eligibility.

Our instrument isolates variation based on program rules while eliminating variation based on individual characteristics. To construct the instrument, we run a national sample of 200,000 dependents in 1997 through the calculator, reassigning state to be each state s, and calculating the fraction of the sample eligible for Medicaid from each month of birth cohort c at each age a from age 0 to age 18. For each child i, we calculate total years of simulated eligibility during childhood by summing $I_{cs,a=t}$, the simulated eligible fraction of individuals in cohort c in residing in the state s in which the child is living at each age a from 0 to 18. Mean eligibility for individuals in our full sample from birth to age 18 is 2.76

years, with a standard deviation of 3.98 years, and mean simulated eligibility is 3.38 years, with a standard deviation of 1.69 years.

All children of the same birth month cohort c who remain in the same state for their entire childhood have the same value of the instrument $\sum_{t=0}^{18} I_{cs,a=t}$. For children who move, the instrument reflects the full amount of simulated eligibility to which they are exposed over the course of their entire childhoods. Therefore, the instrument varies across cohorts and the vector of states in which we observe the children.

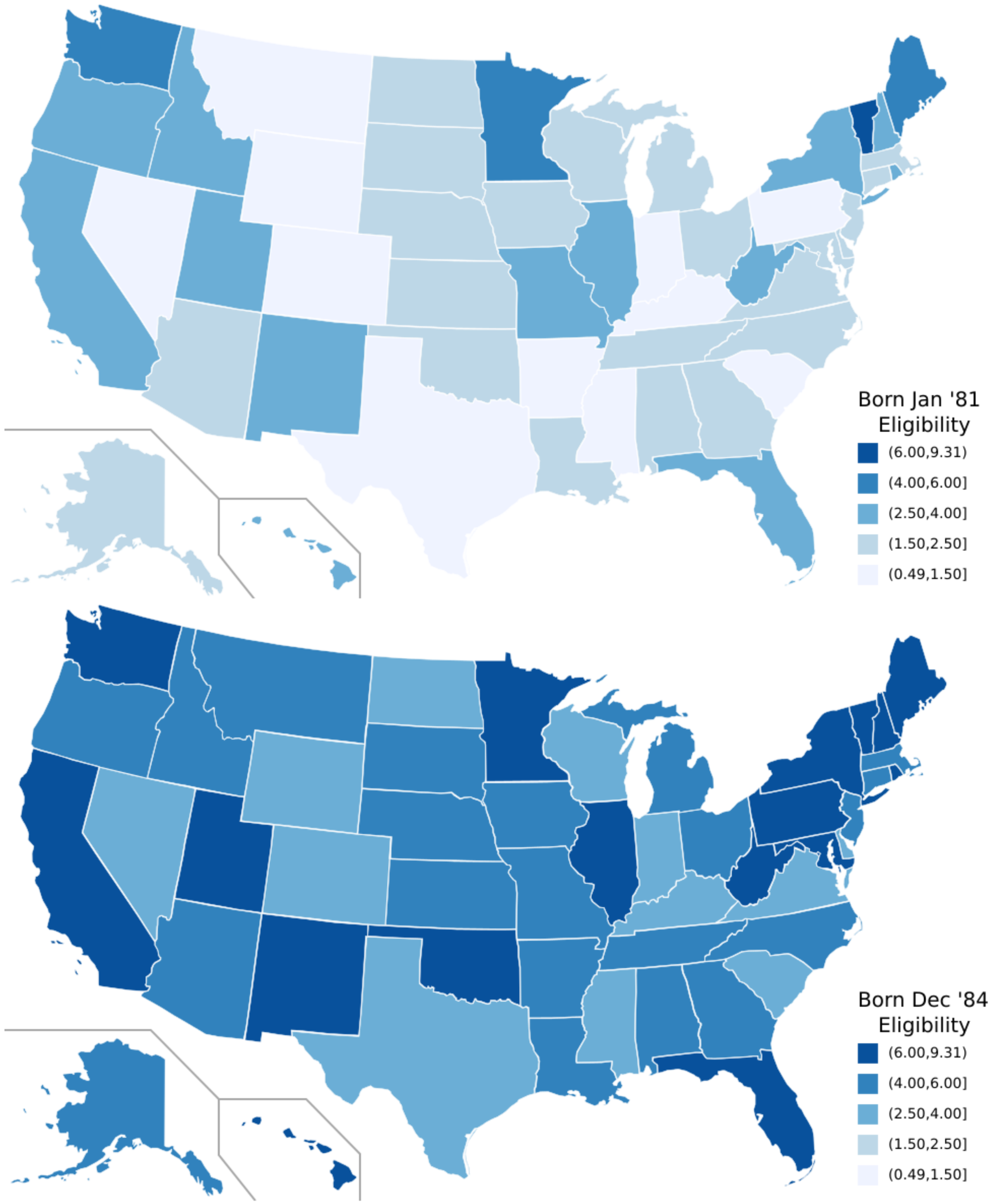
Abstracting away from variation that comes from children who move during childhood for now, we describe variation in the instrument across cohorts and states, assuming that children remain in the same state from birth to age 18. The top panel of Figure 1 shows the value of the instrument, average years of simulated Medicaid eligibility from birth to age 18, by state for individuals in our oldest cohort, born in January 1981. As shown, simulated Medicaid eligibility varies dramatically within a cohort, with Arkansas offering an average of less than half a year and Vermont offering an average of over six years of eligibility from birth to age 18.

For comparison, the bottom panel of Figure 1 shows the same variation for the youngest cohort in our data, born December 1984. As shown, there is still a considerable amount of variation across states within this cohort, with Wyoming offering an average of 3.3 years and Vermont offering an average of 9.3 years of eligibility from birth to age 18. However, individuals in this younger cohort have a population-weighted average of 2.23 additional years simulated eligibility relative to individuals in the oldest cohort.

There is also variation in simulated Medicaid eligibility across birth month cohorts born within the same calendar year, which we use for identification even though it is not visible in Figure 1. This variation arises because eligibility formulas actually set eligibility by birth month. Indeed, a 1990 Federal expansion in Medicaid eligibility resulted in greater Medicaid eligibility for all birth month cohorts born in October 1983 and later, relative to all previous birth month cohorts. This variation is arguably very exogenous, given that there would have been no reason for parents to manipulate birth timing in 1983 in anticipation of federal legislation that did not occur until 1990. Card and Shore-Sheppard (2004) focus on only this variation and find effects on contemporaneous insurance coverage, and Meyer and Wherry (2012) revisit this variation to investigate long term impacts on mortality.

In Section 5, we perform a novel exercise to show graphically which forms of variation drive our results. While we prefer to harness all variation to maximize external validity, we show that the federal variation plays a role in identifying our results. More generally, we conclude that variation across states and across birth cohorts within a given calendar birth year drives our results while variation across calendar years of birth attenuates them.

Figure 1: Cross Sectional and Time Series Variation in Simulated Medicaid Eligibility from Age 0-18 in years



By studying the impact of Medicaid eligibility from birth to age 18 on long term outcomes after age 18, we potentially mitigate concerns of legislative endogeneity relative to studies that examine the impact of Medicaid eligibility at a point in time on contemporaneous outcomes. The traditional legislative endogeneity concern is that states increase Medicaid eligibility in response to changes in the outcomes of interest such that the results do not reflect the causal policy impact. Similar concerns are still present in our analysis, but we expect that they are at least weakly less severe than they are in contemporaneous analysis. In contemporaneous analysis, there is a concern if states enact policy in response to outcomes at a single point in time. In our analysis, for the legislative endogeneity concern to be as severe as it is in contemporaneous specifications, states would have to enact policy in response to outcomes at *all* points in time from birth to age 18.

4 Results

We present results for each of our five main outcomes from the tax data and our two main outcomes from the MSIS data one at a time. For our tax outcome, we first consider OLS results with no controls other than state and cohort fixed effects, which should be biased toward showing deleterious impacts of Medicaid. We then present OLS results with the full set of richer controls, which should be biased, but less so. Finally, we consider our preferred instrumental variable estimates for all outcomes. We begin by presenting results pooled across genders, and we also present separate results for women and men to allow for differential impacts given differential means. Our sample includes 4,911,040 female observations and 5,129,194 male observations for outcomes through age 28.

4.1 Cumulative Income and Payroll Taxes

As shown by the means in the first row of Figure 2, men pay more in average cumulative taxes than women at every age from 19 to 28. The second row shows coefficients on Medicaid eligibility (specified as years of eligiblity from birth to age 18) from OLS models with no controls other than state and cohort fixed effects. As expected, based on selection into Medicaid, the results show that for both men and women, additional years of eligibility decrease tax contributions: for women, on a base of $18,114 average cumulative tax payments by age 28, an additional year of Medicaid eligibility decreases cumulative tax payments by $1,639. The effect size is smaller for men even though overall male tax contributions are larger. On a base of $23,025, an additional year of eligibility decreases cumulative tax payments by $1,284. As shown by the dashed lines that report the 95% confidence intervals, our OLS results show statistically significant decreases in cumulative taxes for both genders at all ages.

The coefficients are still negative, though the coefficients are smaller in magnitude, when we add our full set of controls to the OLS model. Even though we control very flexibly for income using splines, we still see negative coefficients. Using this specification, we find that an additional year of eligibility decreases average cumulative tax contributions at age 28 by \$292 among women and \$259 among men. We see statistically significant decreases in tax payments at all ages.

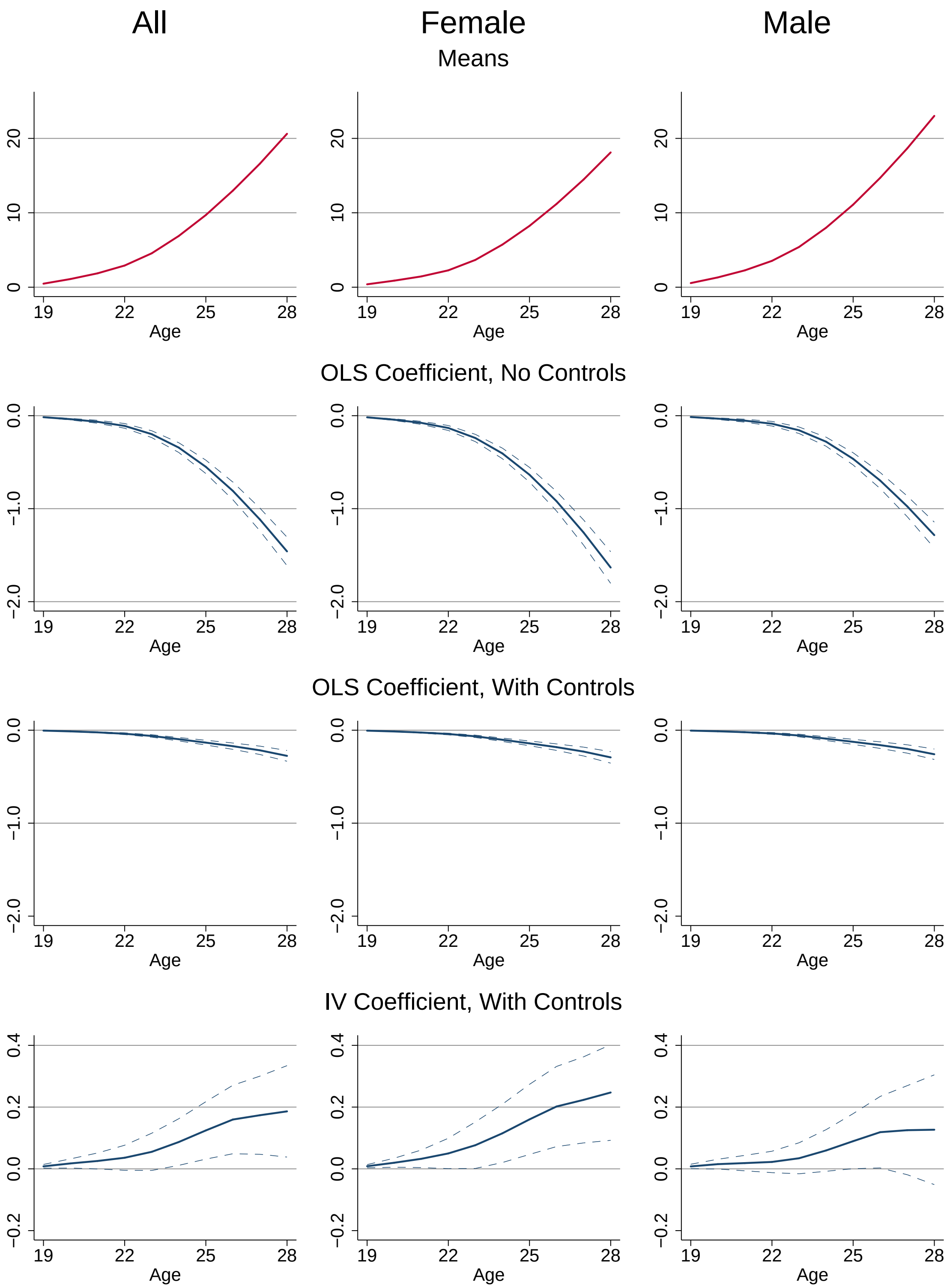
Turning to our simulated instrument specification in the last row of Figure 2, the coefficients change sign. We find that Medicaid eligibility *increases* tax contributions for both women and men, though the results are larger and more significant for women. The larger tax impacts for women are consistent with the larger EITC and wage impacts for women that we find below. By age 28, women have contributed an average of \$18,114 in taxes and each additional year of eligibility from birth to age 18 increases their cumulative tax payment by \$247. This result is statistically significant at the 1% level at ages 20-21 and 25-28 and at the 5% level at ages 21-24, as shown in the last row of Figure 2.

By age 28, men have contributed an average of \$23,025 in income and payroll taxes. The point estimate suggests that each year of eligibility increases their cumulative contributions by \$127. This increase is about half as much as the increase for women, even though men pay more in taxes overall. We observe positive impacts on cumulative taxes for men at all ages, though we see less statistical significance for men. We see increases in cumulative tax payments for men that are statistically significant at the 5% level at ages 19, 25, and 26. Pooling men and women, the IV coefficient shows that a one standard deviation increase in Medicaid eligibility from birth to age 18 (an increase of 3.98 years of eligibility) increases cumulative tax payments by age 28 by \$740 (=186*3.98), which represents a 3.6% (=740/20,623) increase.

4.2 Unconditional Cumulative EITC Receipts

On average, women receive about twice as much money from the EITC program as men, reflecting differential eligibility standards by family composition, as well as differences in wages by gender. Including zeros for individuals who do not receive EITC payments to capture an "unconditional" measure EITC receipts, women receive \$4,187 in cumulative EITC payments by age 28 and males receive \$1,948, as shown in the first row of Figure 3. We examine unconditional cumulative EITC receipts using our simulated instrument specification and find that Medicaid eligibility significantly *decreases* future EITC receipts for both sexes. Our results suggest that Medicaid eligibility during childhood does not create a culture of dependency that leads to increased EITC receipt later in life. This finding stands

Figure 2: Results – Cumulative Income and Payroll Taxes ($000's)



Coefficient on Medicaid Eligibility from Age 0-18 in years

in contrast to findings from recent work by Dahl et al. (2013).

For each additional year of Medicaid eligibility from birth to age 18, women receive $109 less in cumulative EITC payments by age 28, as shown in the second row of Figure 3. Males receive $41 less. These results are statistically significant for women at all ages and for men at all ages after age 19.

In results not shown, we see that Medicaid eligibility has an impact on EITC receipt through the participation margin. 51% of females participate in EITC at some point between age 19 and age 28, and a one standard deviation increase in their Medicaid eligibility (3.99 years) results in a statistically significant 1.7% (=(0.22*3.99)/51) decrease in EITC participation. Similarly, 44% of males participate in EITC at some point between age 19 and age 28, and a one standard deviation increase in their Medicaid eligibility (3.96 years) results in a 1.5% (=(0.17*3.96)/44) decrease in EITC particpation. However, the male participation result is not statistically different from zero.

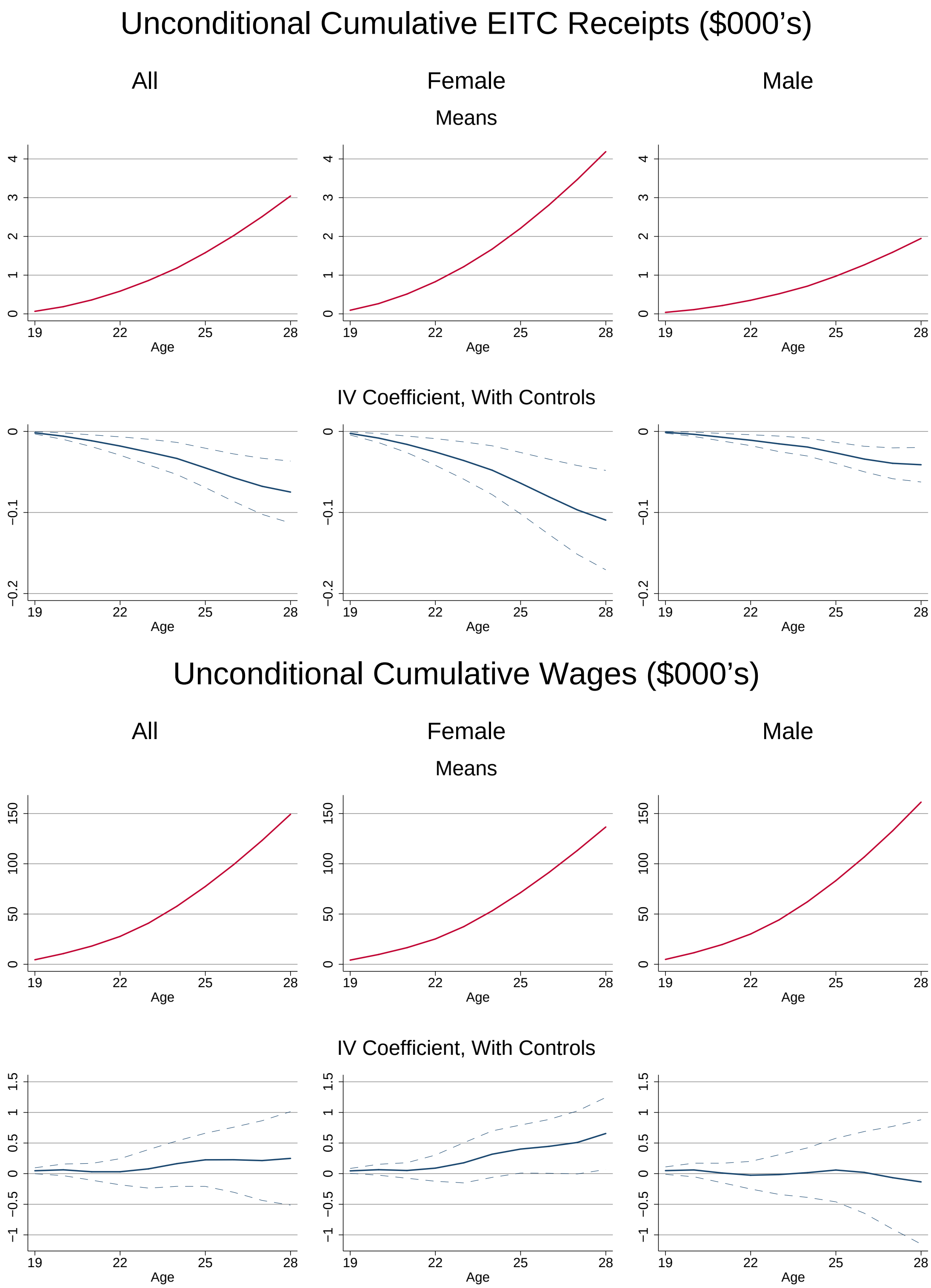
We can compare our EITC results to our tax results to explore what fraction of the increased taxes that we observe occurs as a result of decreased EITC payments. When considering both males and females at age 28, an additional year of eligibility from birth to age 18 increases cumulative tax payments by $186 and reduces cumulative EITC receipts by $75. Therefore, around 40% of the observed increased tax payments occur through reductions in EITC. The rest likely occurs because actual payments to the government increase when wages and income increase. We next examine changes in wages directly.

4.3 Unconditional Cumulative Wages

We examine a measure of cumulative wages that is "unconditional" on working because we include zero wages for individuals who do not work. Our simulated instrument coefficients, presented in the bottom row of Figure 3, provide evidence that Medicaid eligibility has a positive effect on future wages. On a base of $136,591 cumulative wages from age 19 to 28, the female point estimate suggests a $656 increase for each additional year of Medicaid eligibility from birth to age 18. This result is statistically significant at the 5% level at age 28 and at many earlier ages.

Men enjoy higher average wages at every age, but there is less compelling evidence that they experience increases in wages as a result of Medicaid eligibility. In fact, point estimates at some ages are negative, and we observe a slight downward trend in the point estimates as men age. On a base of $161,360 cumulative wages from age 19 to 28, the point estimate at age 28 suggests a $134 decrease for each additional year of Medicaid eligibility from birth to age 18. However, this decrease is not statistically significant. As shown by the dashed lines that report the upper and lower bounds of the 95% interval in the bottom row of Figure 3, the

Figure 3: Results – Unconditional Cumulative EITC Receipts & Unconditional Cumulative Wages ($000's)



Coefficient on Medicaid Eligibility from Age 0-18 in years

estimates for males are somewhat less precise than the estimates for females, likely driven by higher variability in wages for males. In unreported results that consider cumulative wages only for individuals with positive earnings, we draw very similar conclusions because individuals of both genders have high rates of labor force participation.

To put our wage results for women in the context of a finding from the small existing literature on long term wage impacts of interventions during childhood, Chetty et al. (2011) find that a one standard deviation increase in teacher value-added in a given grade increases earnings at age 28 by 1.3%. Our estimate of a one standard deviation increase in Medicaid eligibility is of a similar magnitude. A one standard deviation increase in female Medicaid eligibility (3.99 years) results in a 1.9% (=(656*3.99)/136,591) increase in cumulative earnings by age 28. Incidentally, Chetty et al. (2011) also show that wages at age 28 are a good predictor of future wages, which supports our focus on outcomes at age 28 in this paper.

4.4 Mortality

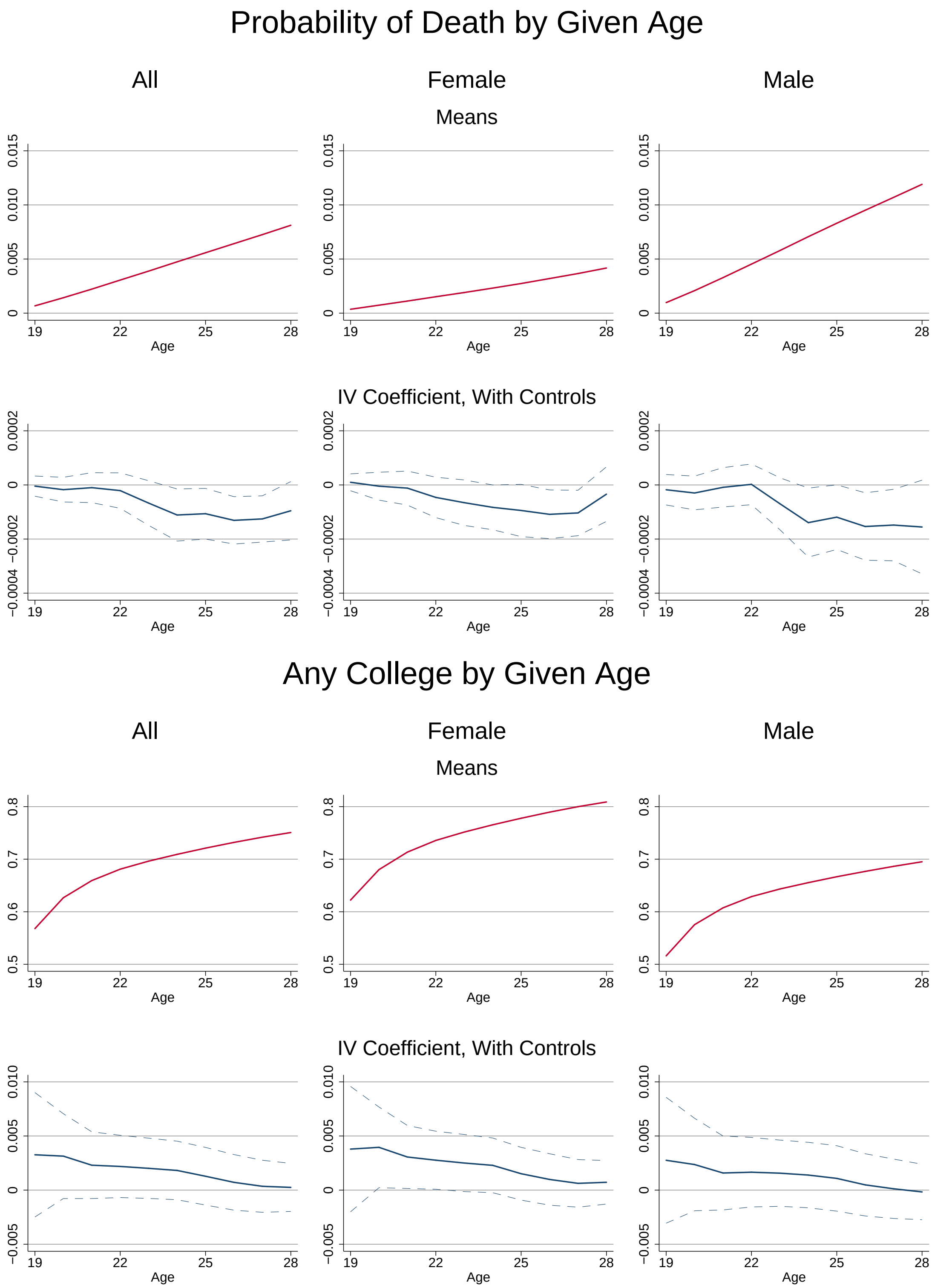
Next, we examine the impact of Medicaid on mortality. We only include individuals in our sample if they do not die before age 18, which biases us against finding mortality impacts, since we do not capture mortality during childhood. We examine the impact of Medicaid eligibility during childhood on cumulative adult mortality through age 28.

The simulated instrument mortality results, shown in the second row of Figure 4, show that male and female eligibles have lower adult mortality rates. In absolute terms, the mortality reduction is smaller among women, who die at lower rates at these ages. From age 18 to age 28, 0.42% of females die. A one standard deviation increase in female Medicaid eligibility (3.99 years) decreases mortality by 3.2% (=(0.0034*3.99)/0.42) at age 28, but this difference is not statistically different from zero. However, the effect is larger and statistically significant at ages 26 and 27. By age 27, 0.37% of females die, and a one standard deviation increase in Medicaid eligibility decreases mortality by 10.8%.

The impacts of Medicaid eligiblity on mortality are larger for men, which seems reasonable given that the male adult mortality rate is nearly three times the female adult mortality rate through age 28. From age 18 to age 28, 1.2% of adult males die. A one standard deviation increase in male Medicaid eligibility (3.96 years) decreases mortality by 5.3% (=(0.016*3.96)/1.2), and this effect is marginally statistically significant. The percent reduction in mortality for males is similar in magnitude and statistically significant at ages 24, 26, and 27.

These mortality decreases are striking, especially since they only reflect reductions in adult mortality. Relative to Meyer and Wherry (2012), who are not able to detect mortality decreases in the full population at any age, we gain additional power by incorporating vari-

Figure 4: Results – Probability of Death & Any College by Given Age



Coefficient on Medicaid Eligibility from Age 0-18 in years

ation in eligibility based on parental income and state of residence during childhood, which are not available in their data. The decreases in mortality that we find, which could reflect general improvements in health from childhood Medicaid eligibility, provide a suggestive mechanism for our wage and tax results.

4.5 College Attendance

As the final outcome within our tax data, we consider the relationship between Medicaid eligibility and college attendance. As shown in the first row of Figure 4, a larger percentage of women than men have ever attended college at every age from 18 to 28.

Using our simulated instrument specification, as shown in the last row of Figure 4, we find that both male and female Medicaid eligibles are more likely to have attended college. This effect is more pronounced for women. The coefficients plotted in the bottom row show that female eligibles are more likely to have attended college by any age, starting at age 19, through age 28. The increase in college attendance is statistically different from zero at ages 20-22, an important age for college attendance. On a base of 68% of the female population that has ever attended college by age 20, one additional year of eligibility from birth to age 18 increases the likelihood of having ever attended college by 0.40 percentage points. There is suggestive evidence of increased college attendance among male Medicaid eligibles at age 20. Only 58% of the male population have ever attended college by age 20. The coefficient suggests that one additional year of Medicaid eligibility increases the likelihood of having ever attended college by 0.24 percentage points. As a percentage of mean college attendance, the effect sizes for males are closer to the effect sizes for women, but they are still slightly smaller. Like decreased mortality, increased college attendance could also potentially explain some of the mechanism for our tax and wage outcomes.

4.6 Medicaid Spending and Return on Investment

Next, using data from the Medicaid Statistical Information System (MSIS), we calculate the increase in Medicaid spending induced by the expansions that we study. In contrast to the tax outcomes, we only run a single regression with spending as the dependent variable because spending from birth to age 18 does not change after age 18. Furthermore, Medicaid spending is not available separately for women and men, so we pool both genders.[8] We report the results in Table 1. When we apply a 3% discount rate, as shown in the top portion of the first column, each additional year of Medicaid eligibility from birth to age 18 increases

[8]The MSIS does not report spending or take-up for Arizona in some years, so we drop observations for children who lived in Arizona in those years.

Table 1: Medicaid Spending, Income and Payroll Taxes, and Medicaid Take-up: Pooled Female and Male

	(1) Medicaid Spending	(2) Income & Payroll Taxes by Age 26	(3) Income & Payroll Taxes by Age 27	(4) Income & Payroll Taxes by Age 28	(5) Medicaid Take-up
			With 3% Discount Rate		
Medicaid Eligibility	872***	133**	128*	119	
from Age 0 - 18 in years	(71.44)	(62.65)	(71.00)	(83.25)	
Mean	2,130	15,376	19,303	23,496	
Mean/Coeff Ratio (%)		0.87%	0.66%	0.51%	
			No Discount Rate		
Medicaid Eligibility	575***	160***	174***	186**	0.58***
from Age 0 - 18 in years	(45.53)	(55.19)	(62.99)	(73.69)	(0.04)
Mean	1,352	12,981	16,616	20,623	2.01
Mean/Coeff Ratio (%)		1.23%	1.05%	0.90%	
Observations	9,873,825	10,040,234	10,040,234	10,040,234	9,873,825

Medicaid spending by \$872 on a base of \$2,130 in 2011 dollars. In the results that do not impose a discount rate, shown just below, each additional year of Medicaid eligibility from birth to age 18 increases Medicaid spending by \$575 on a base of \$1,352 in 2011 dollars.

We can calculate the government return on the incremental Medicaid spending by combining our spending result with our cumulative income and payroll tax result. Since our spending result is pooled by gender, we report pooled cumulative income plus payroll tax for results for age 26 through age 28 in Table 1. Our preferred return on investment calculations assume a 3% discount rate. We also report the baseline results that do not impose a discount rate in the lower portion of Table 1.

We first perform the return on investment calculation at age 28, the latest age at which we can observe all cohorts in our data. Dividing the discounted estimated increase in Medicaid spending from birth to age 18 (\$872) by the discounted estimated increase in cumulative tax payments at age 28 (\$119), we conclude that the government recoups 14 cents for each dollar that it spent on Medicaid for children by the time they reach age 28. Ignoring the discount rate on the grounds that the federal government should not have a time preference, we conclude that the government recoups 32 cents on each dollar (186/575) by the time the children reach age 28. In addition to observing age 28 directly in our data, another advantage to focusing on age 28 is that it is ten years after the final childhood Medicaid spending occurs, and the Congressional Budget Office (CBO) often considers a 10 year investment return.

However, the return on investment is arguably higher as increased tax payments persist over time, and our preferred estimates take into account a longer time horizon. To forecast increased tax payments at older ages, we first observe in Table 1 that at each age from 26 to 28, cumulative tax payments increase between 0.87% and 0.51%. To be conservative, we focus on the 0.51% increase at age 28, which is the smallest reported increase in percentage terms. We assume that the increased tax payments persist as a percentage of mean tax payments at every age going forward. To estimate mean tax payments by age, we take a random sample of one in one thousand of tax returns in 2011 and calculate mean payments and filing probabilities by age. Tax year 2011 was anomalous because of the payroll tax holiday, so we calculate mean tax payments assuming that that there had been no payroll tax holiday. Based on this calculation, we find that the government recoups 56 cents on the dollar by age 60.

For comparison, we perform a similar return on investment calculation assuming a discount rate of zero. As shown in the lower portion of Table 1, across ages 26 to 28, the most conservative increase in cumulative tax payments is 0.90% on the age 28 base. Our results change dramatically if we do not impose a discount rate: the government recoups its investment by age 36 and by age 60, the government has earned a 550% return. We report our results that impose a 3% discount rate as our main results because they are much more conservative.

One caveat to our return on investment calculations is that our tax measure only includes federal taxes, but the Medicaid spending includes state and federal spending. In practice, the federal government paid for about 50% of Medicaid spending in the period that we study, making the return on investment for the *federal* government even larger. However, given that the federal government will pay almost the entire bill for Medicaid expansions under the ACA, our calculation is relevant to those expansions.

4.7 Medicaid Take-up

Finally, using other data from MSIS, we examine the impact of Medicaid eligibility on take-up of Medicaid. As shown in the last column of Table 1, Medicaid take-up increases by 0.58 years for each additional year of Medicaid eligibility from birth to age 18. We have previously argued that we prefer estimates based on Medicaid eligibility instead of take-up, however, it is interesting to examine Medicaid take-up to gauge the magnitudes of our results. Our results imply that if we want to report impacts on beneficiaries as opposed to eligibles, we should multiply all of our effect sizes by a factor of (1/0.5804), which is approximately 1.7. For example, our tax results imply that for each additional year of Medicaid *enrollment* from birth to age 18, cumulative tax payments at age 28 increase by $321.04 (=186.331*1.723).

5 Variation that Drives the Results

To illustrate the variation in simulated Medicaid eligibility that drives our results and its relationship with our outcomes of interest, we perform a novel exercise. The goal of this exercise is to demonstrate which cohorts and states give the most identifying variation and to show a graphical dose-response relationship between this identifying variation and our outcomes of interest. Identification in our main specifications exists at the cohort (month of birth from January 1981 to December 1984) by state level. For children who move between states during their childhoods, the instrument reflects the vector of state policies to which they are exposed, but we abstract away from that variation in this exercise.

For this exercise, we eliminate variation in our instrument, first by cohort, and then by state, to determine whether variation across states or variation across cohorts is empirically more relevant for our main IV results. Next, using variation only across states, we graph the first stage and reduced form for each cohort. Analogously, using variation only across cohorts, we graph the first stage and reduced form for each state. In these graphs, we look for a dose-response relationship that suggests that greater changes in medicaid eligibility lead to greater changes in adult outcomes, which we would expect to find in the case of homogeneous treatment effects.

First, we eliminate variation by cohort such that variation only exists across states. Using the entire estimation sample, we average the values of the instrument $\sum_{t=0}^{18} I_{cs,a=t}$ by state at age 15 such that we obtain a new instrument $\overline{\sum_{t=0}^{18} I_{s,a=t}}$ that only varies across states. Similarly, using the entire estimation sample, we average the values of the instrument $\sum_{t=0}^{18} I_{cs,a=t}$ by cohort such that we obtain a new instrument $\overline{\sum_{t=0}^{18} I_{c,a=t}}$ that only varies across cohorts. Next, we run our main IV specification given by Equations 1 and 2 with our new instruments (one at a time) in place of the full instrument. In the specification where the instrument only varies across states, we must drop the state fixed effects. Similarly, in the specification where the instrument only varies across cohorts, we must drop the fixed effects by cohort, but we can and do still include fixed effects by birth year. Below, we separately investigate cohort variation across and within birth years.

As shown in Table 1, our IV coefficient in our main specification is 186, which indicates that an additional year of Medicaid eligibility during childhood increases cumulative income and payroll tax payments at age 28 by \$186 on a base of \$20,623 in 2011 dollars. In the analogous specification in which we only use variation across states, we obtain a larger coefficient which indicates an increase in payroll and income payments by age 28 by \$915 on the same base. This coefficient is statistically different from zero at the 1% level. However, when we only use variation across cohorts, controlling for year of birth, we obtain a *negative* coefficient which indicates a *decrease* in cumulative income and payroll tax payments of \$2,432 on the same base, and this coefficient is statistically different from zero at the 1% level. This comparison tells us that variation across states drives our finding that Medicaid eligibility increases tax payments at age 28, and variation across cohorts works against it.

Next, we dig deeper, asking which cohorts experience the largest variation across states. We run a separate first stage regression using data from each cohort k separately, identified only by variation across states:

$$\sum_{t=0}^{18} M_{is,c=k,a=t} = \delta^k \overline{\sum_{t=0}^{18} I_{s,a=t}} + X_{i,a=15} + \eta_i. \tag{3}$$

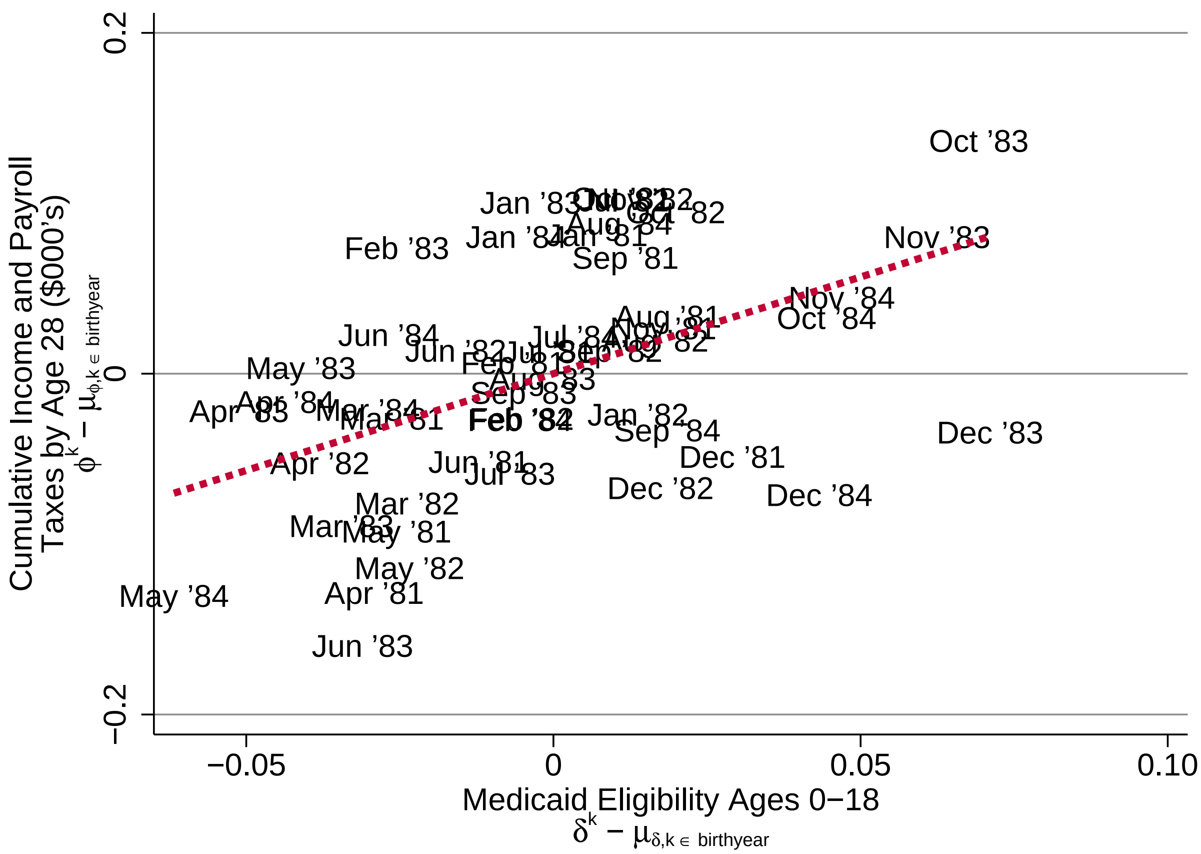
This equation is related to the baseline first stage equation given by Equation 2, but note that it no longer includes state fixed effects $\gamma_{s,a=15}$ because the variation now exists at the state level. It also no longer includes cohort fixed effects γ_c because each regression only includes data from a single cohort k. From these regressions, we obtain a coefficient δ^k for each cohort. This coefficient gives us the magnitude of the change in Medicaid eligibility identified only by variation across states within each cohort.

We can compare the magnitudes of these coefficients across cohorts to determine which cohorts exhibit the most variation in Medicaid eligibility across states through the simulated instrument. We find a coefficient δ^k of 0.9 for our oldest cohort, born January 1981, which indicates that each additional year of simulated eligibility translates into 0.9 years of eligibility for this cohort. In contrast, we find a larger coefficient δ^k of 1.4 for our youngest cohort, born December 1984, indicating that variation in generosity across states leads to more variation in Medicaid eligibility in the youngest cohort than it does in the oldest cohort. Averaging the first stage coefficients across all cohorts born in the same calendar year, weighting by the observation count, we find a general pattern that the first stage across states increases as birth year increases from $\mu_{\delta,k\in1981} = 0.88$ to $\mu_{\delta,k\in1982} = 1.03$ to $\mu_{\delta,k\in1983} = 1.16$ to $\mu_{\delta,k\in1984} = 1.32$. This pattern is not surprising, given that Medicaid eligibility increased over time.

The same pattern generally holds *within* each birth year, with children born in later months of the year experiencing greater first stage variation than children born in earlier months of the year, also because younger cohorts generally have greater Medicaid eligibility. The horizontal axis of Figure 5 depicts variation in the first stage coefficient *within* each birth year by normalizing the coefficient by the observation-weighted average coefficient from that calendar year. As shown, the first stage coefficient is strikingly larger for children born in October, November, and December of the 1983 cohort relative to children born in earlier months of the same year. This pattern reflects the federal legislation that differentially affected children born after September 30, 1983, discussed above.

Next, we examine whether larger first stage variation translates into larger reduced form variation in outcomes. Simulated Medicaid eligibility could have heterogeneous treatment effects on different cohorts, so there is no reason to expect an exact linear relationship

Figure 5: Relationship Between Reduced Form Coefficient ϕ^k for Changes in Taxes and First Stage Coefficient δ^k for Changes in Medicaid Eligibility for Each Cohort k



between the first stage coefficient and the reduced form coefficient across cohorts. However, it would be reassuring to see some amount of a dose-response relationship, indicating that larger increases in Medicaid eligibility translate into larger changes in later life outcomes. For each cohort, we run an analogous reduced form regression, identified only by the average variation across states:

$$Y_{i,a=A} = \phi^k \overline{\sum_{t=0}^{18} I_{s,a=t}} + X_{i,a=15} + \eta_i. \tag{4}$$

We can estimate this regression for any of our reduced form outcomes, $Y_{i,a=A}$. We begin by estimating it for our main outcome of interest, cumulative income and payroll taxes at age 28. We find a coefficient ϕ^k of 1,130 for our oldest cohort, born January 1981, which indicates that each additional year of simulated eligibility translates into $1,130 of cumulative income and payroll tax payments by age 28 for this cohort. In contrast, we find a smaller coefficient ϕ^k of 835 for our youngest cohort, born December 1984, indicating that variation in generosity across states increases tax payments in the oldest cohort more than it does in the youngest cohort. Indeed, averaging the reduced form coefficients across all cohorts born in the same calendar year, weighting by the observation count, we find a general pattern that the reduced form decreases as the birth year increases from $\mu_{\phi,k\in 1981} = 1,048$ to $\mu_{\phi,k\in 1982} = 1,032$ to $\mu_{\phi,k\in 1983} = 976$ to $\mu_{\phi,k\in 1984} = 907$.

If we simply relate the average first stage coefficient for each year of birth to the average reduced form coefficient for each year of birth, we find a negative relationship between Medicaid eligibility and cumulative income and payroll taxes. We also see a negative relationship if we relate the first stage to the reduced form by cohort without regard to birth year. These results are consistent with the negative coefficient that we obtain in the full specification above that only uses variation across cohorts. However, our attempt to eliminate time effects by examining all cohorts at the same age could still allow for time to have a differential impact on individuals born in different calendar years through mechanisms such as business cycles which affect different cohorts in the same year but at different ages. It is especially important to take calendar years into account given that we only observe adult outcomes once per calendar year at tax time.

The vertical axis of Figure 5 depicts variation in the reduced form coefficient *within* each birth year by demeaning the coefficient by the observation-weighted-average coefficient within each calendar year of birth. Each point gives $(\delta^k - \mu_{\delta,k\in \text{birthyear}}, \phi^k - \mu_{\phi,k\in \text{birthyear}})$ for a single cohort, identified only by variation across states. As shown, within each birth year, children born in later months have larger tax payments by age 28. Strikingly, children born in October 1983, the youngest affected by the federal Medicaid expansion, have the largest increases in cumulative income and payroll taxes, suggesting that increases in Medicaid eligibility are indeed the mechanism for larger income and payroll taxes.

The dashed line in Figure 5 gives the observation-weighted average relationship between the first stage and reduced form coefficients demeaned by year of birth. The upward slope shows that larger increases in Medicaid eligibility identified by average variation across states translate into larger increases in tax payments by age 28. The slope of the line, which is $1,135$, is itself an instrumental variable estimate because it gives the change in the reduced form in response to the change in the first stage. This slope is a difference-in-difference estimate of sorts, which uses variation across states to identify a coefficient within each cohort and then compares coefficients across cohorts. It indicates that if we increase Medicaid eligibility during childhood by one year, cumulative income and payroll taxes at age 28 increase by \$1,135. This slope is similar in magnitude to the \$915 coefficient that we obtained in the full instrumental variable specification that uses only variation across states.

We next perform the analogous exercise, using only variation across cohorts within states.

Using the entire estimation sample, we average the values of the instrument $\sum_{t=0}^{18} I_{cs,a=t}$ by state such that the new instrument $\overline{\sum_{t=0}^{18} I_{c,a=t}}$ only exists at the cohort level. Next, we run a separate first stage regression using data from state x separately, identified only by the average variation across cohorts:

$$\sum_{t=0}^{18} M_{ic,s=x,a=t} = \delta^x \overline{\sum_{t=0}^{18} I_{c,a=t}} + birthyear_c + X_{i,a=15} + \eta_i. \tag{5}$$

This first stage equation is related to the baseline stage equation given by Equation 2, but it no longer includes state fixed effects $\gamma_{s,a=15}$ because each regression only includes data from a single cohort. It also no longer includes cohort fixed effects γ_c because the variation now exists at the cohort level, so they would drop out of the regression. However, we can and do include fixed effects by calendar year of birth $birthyear_c$ so that we take into account that reduced form outcomes are measured in different tax years for children born in different calendar years.

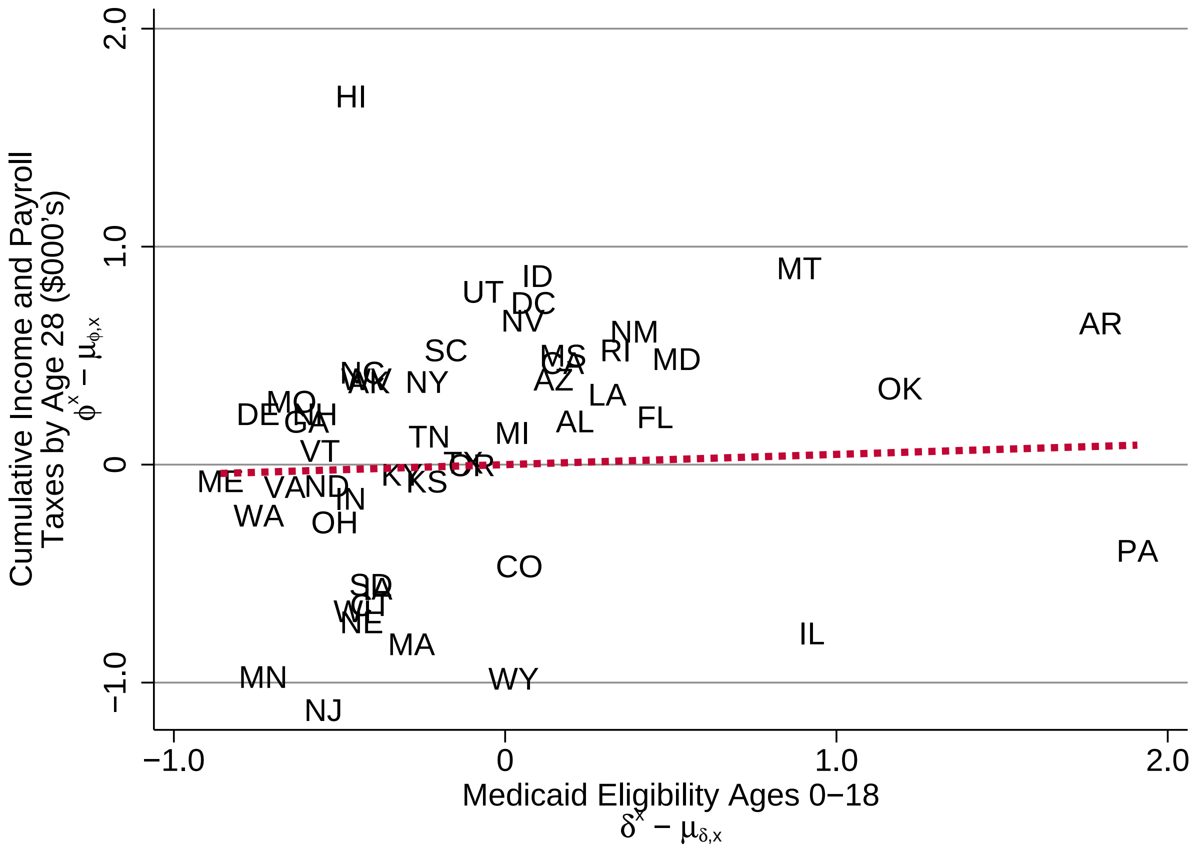
Next, we run a separate reduced form regression using data from state x separately, identified only by the average variation across cohorts:

$$Y_{i,a=A} = \phi^x \overline{\sum_{t=0}^{18} I_{c,a=t}} + birthyear_c + X_{i,a=15} + \eta_i. \tag{6}$$

Figure 6 plots the reduced form coefficients against the first stage coefficients, demeaned by the average coefficient, weighted by the observation count. Each point gives $(\delta^x - \mu_{\delta,x}, \phi^x - \mu_{\phi,x})$ for a single state, identified only by variation across cohorts. From the horizontal axis of this figure, we can see the that the states that have the largest relative variation in medicaid eligibility across cohorts are Pennsylvania, Arkansas, Oklahoma, Illinois, and Montana, with each year of simulated Medicaid eligibility leading to 1-2 years of Medicaid eligibility above the national average of $\mu_{\delta,x} = 0.77$. Along the vertical axis, we see that some states have much larger reduced form variation in tax payments than others. However, the coefficients within all states are *negative*, and the average observation-weighted coefficient is $\mu_{\phi,x} = -1,980$. The negative coefficients that we obtain within each state using only variation across cohorts are consistent with the negative coefficient of \$2,432 that we obtain in the full sample with the same instrument.

However, as we see in Figure 6, even though all of the reduced form coefficients are negative, when we demean by the national observation-weighted average, we see that states with greater increases in Medicaid eligibility experience greater increases in tax payments. The slope of the fitted line indicates that a one year increase in Medicaid eligibility during childhood translates into a \$47 increase in cumulative income and payroll tax payments by

Figure 6: Relationship Between Reduced Form Coefficient ϕ^x for Changes in Taxes and First Stage Coefficient δ^x for Changes in Medicaid Eligibility for Each State x

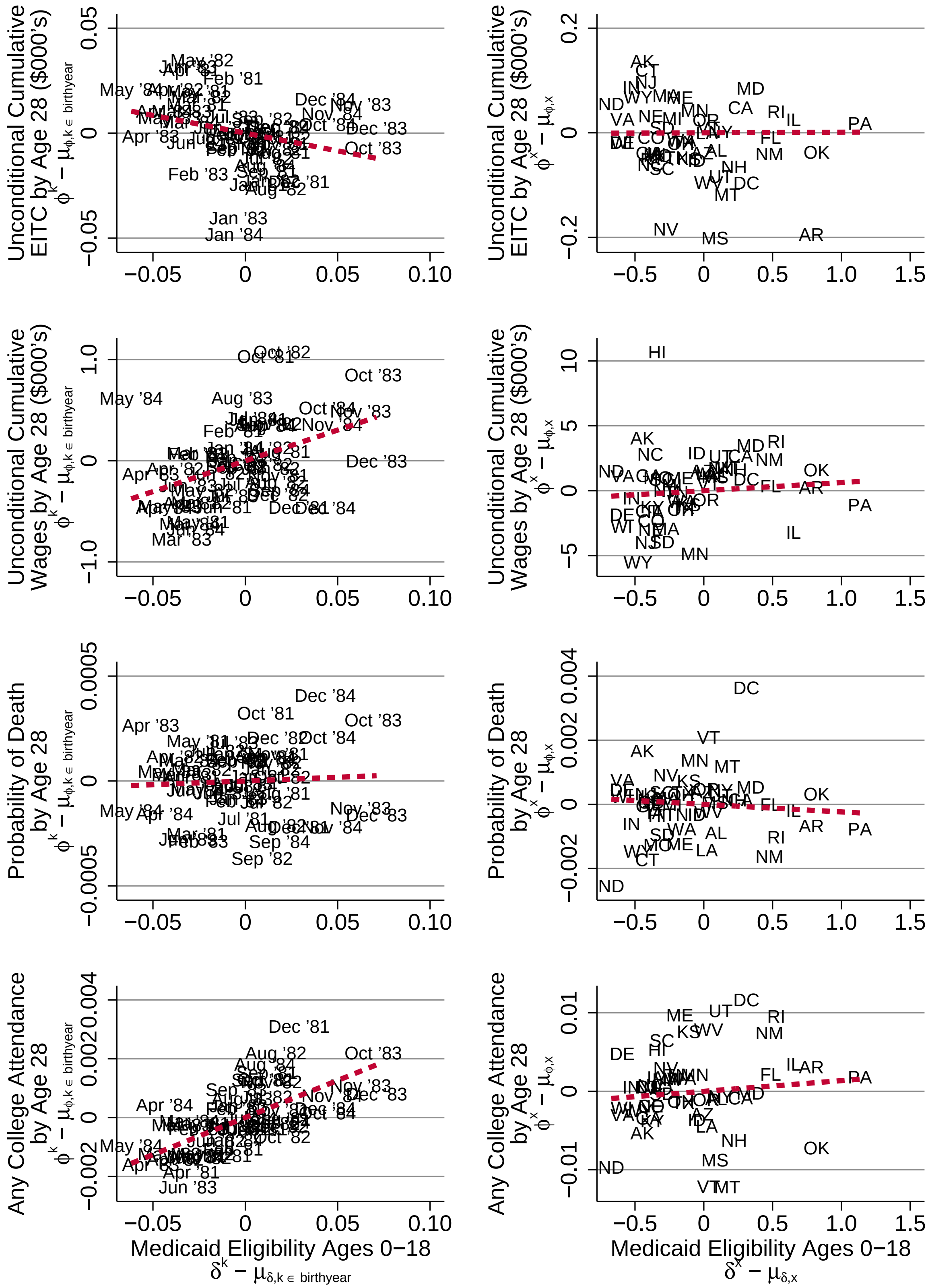


age 28. While still upward sloping, this slope is much less steep than the slope obtained in Figure 5. The smaller slope results because the first form of variation used for identification is across cohorts, and we have shown that variation across cohorts yields a negative relationship between Medicaid eligibility and tax payments by age 28.

Taken together, these exercises show that variation across states drives our main results. Variation across cohorts within a year of birth also yields a positive relationship between Medicaid eligibility and tax payments as adults. However, variation across years of birth shows a negative relationship. Figure 7 shows analogous graphs to 5 and 6 for our other main outcomes of interest. In general, we see that the patterns that we have found are not specific to our tax payment outcome – we find similar relationships across cohorts and states for all of our main outcomes, except mortality, which appears to be noisy.

Given what we have learned about how variation across states and cohorts drives our results, we retain our main specification given by Equations 1 and 2 on the grounds that it is conservative because it includes children born in four different calendar years, and variation

Figure 7: Relationship Between Reduced Form Coefficients ϕ^k, ϕ^x for Outcomes and First Stage Coefficients δ^k, δ^x for Changes in Medicaid Eligibilty for Each Cohort k and for Each State x



across calendar years severely attenuates our results. If we estimate our main specification using data from each calendar year separately, such that variation only comes from the vector of childhood states and the month of birth within a calendar year, we see a positive relationship between Medicaid eligibility and our main tax outcome of interest within each birth year, with an additional year of Medicaid eligibility leading to increased cumulative tax payments by age 28 of $871 for children born in 1981, $875 for children born in 1982, $82 for children born in 1983, and $533 for children born in 1984. Our main IV coefficient is $186, which is smaller than the coefficients for all but one of the birth years. Notably, the coefficient for the 1983 birth year is the smallest, even though the federal legislation induces the most variation across cohorts within the 1983 birth year. All-in-all, though variation across cohorts born in 1983 works in the same direction as our main results, our main results rely on variation across states. Variation across birth years attenuates our results.

Given what we have learned about variation across birth years, it is also conservative for us to focus on outcomes at age 28 as opposed to focusing on outcomes at later ages. Figure A1 shows IV coefficients from ages 19 to 31 for all of our main outcomes of interest, and Figure A2 shows the corresponding means. Although the corresponding means for all outcomes increase smoothly with age, the IV coefficients increase dramatically in absolute magnitude for ages 29 to 31 relative to earlier ages. Our analysis in this section explains the dramatic increase in magnitude. Because variation across years of birth attenuates our results, when we remove years of birth, our results increase in magnitude. While we can observe all four years of birth (1981-1984) at age 28, we can only observe three years of birth (1981-1983) at age 29, two years of birth (1981-1982) at age 30, and one year of birth (1981) at age 31. Not surprisingly, our results are much larger at older ages. Our main coefficient suggests that each year of Medicaid eligibility during childhood increases cumulative income and payroll taxes at age 31 by $1,561 on a base of $35,268, a 4.4% increase. Therefore, focusing on results at age 28 is conservative.

The exercise that we have performed here to explore variation in our instrument could be useful in other applications where the instrument varies along two dimensions. The key novel feature of this exercise is that we first eliminate variation along one dimension to focus only on variation along the other dimension. If we had not done so and had simply run our main specification restricting our sample to single states or single month of birth cohorts, it would not have been as informative to compare the magnitudes of the first stage and reduced form coefficients along the other dimension because the first stage coefficients would have approached one within each group. Using a fixed instrument for all groups allows us to compare first stage and reduced form magnitudes across groups and to conclude that variation across states drives our results much more than variation across cohorts.

6 Robustness

To examine the sensitivity of our results to our main sample selection restriction, we impute Medicaid eligibility for individuals in the tax years in which their parents do not file taxes and incorporate these "non-filers" into our regression sample. To ensure that our estimate of the impact of Medicaid is conservative, we assume that all children are eligible for Medicaid in the years when their parents do not file. We generally think of the filing threshold as around the poverty level, so non-filing occurs when parents fall below the poverty level, and many, but not all, children will be eligible for Medicaid if their parents do not file. For these non-filers, we set simulated eligibility equal to the average value of the instrument for filers in the same birth cohort and state at age 15 (which is itself sometimes imputed).[9] Using our main tax outcome, we find results that are similar in magnitude to our main results. As shown in Figure A3, by age 28, the average individual has contributed $19,037 in income and payroll taxes, and each additional year of eligibility increases the cumulative contribution by $157 (which is slightly larger than the $119 increase on a larger base in the main sample). This estimate and all other estimates from ages 19-28 are statistically significant at least the 10% level. As with our main results, the increase in cumulative tax payments is more pronounced for females and less pronounced for males, further demonstrating the robustness of our results to the inclusion of non-filers.

7 Conclusion

There has been very little work on the long-term impact of Medicaid expansions, or the long-term impact of most interventions, for that matter, because of data availability. However, beyond data availability, studying long-term impacts brings unique challenges. Perhaps the biggest challenge is that between the intervention and the long term, other interventions can occur, so it is difficult to assess whether the apparent impact is actually the result of the initial intervention or the impact of subsequent interventions. This issue is particularly prevalent in the case of Medicaid expansions because Medicaid expansions tend to occur by state, and states that expand Medicaid in one year could expand it further in future years. By focusing on Medicaid expansions in the 1980's and 1990's, we can follow the same individual in our longitudinal tax data to generate a measure of Medicaid eligibility in each year that is arguably one of the most accurate and representative ever generated.

Looking forward to the implementation of the Affordable Care Act, our research suggests

[9] We need to impute values for several of the control variables, including state at age 15, number of siblings at age 15, and family income at age 15. We impute these values using the values from the closest available filing year, and we also include a control for non-filing.

that Medicaid generally has favorable effects on eligible children. We find that by expanding Medicaid to children, the government recoups much of its investment over time in the form of higher future tax payments. Moreover, children exposed to Medicaid collect less money from the government in the form of the Earned Income Tax Credit, and the women have higher cumulative earnings by age 28. Aside from the positive return on the government investment, the eligible children themselves also experience decreases in mortality and increases in college attendance.

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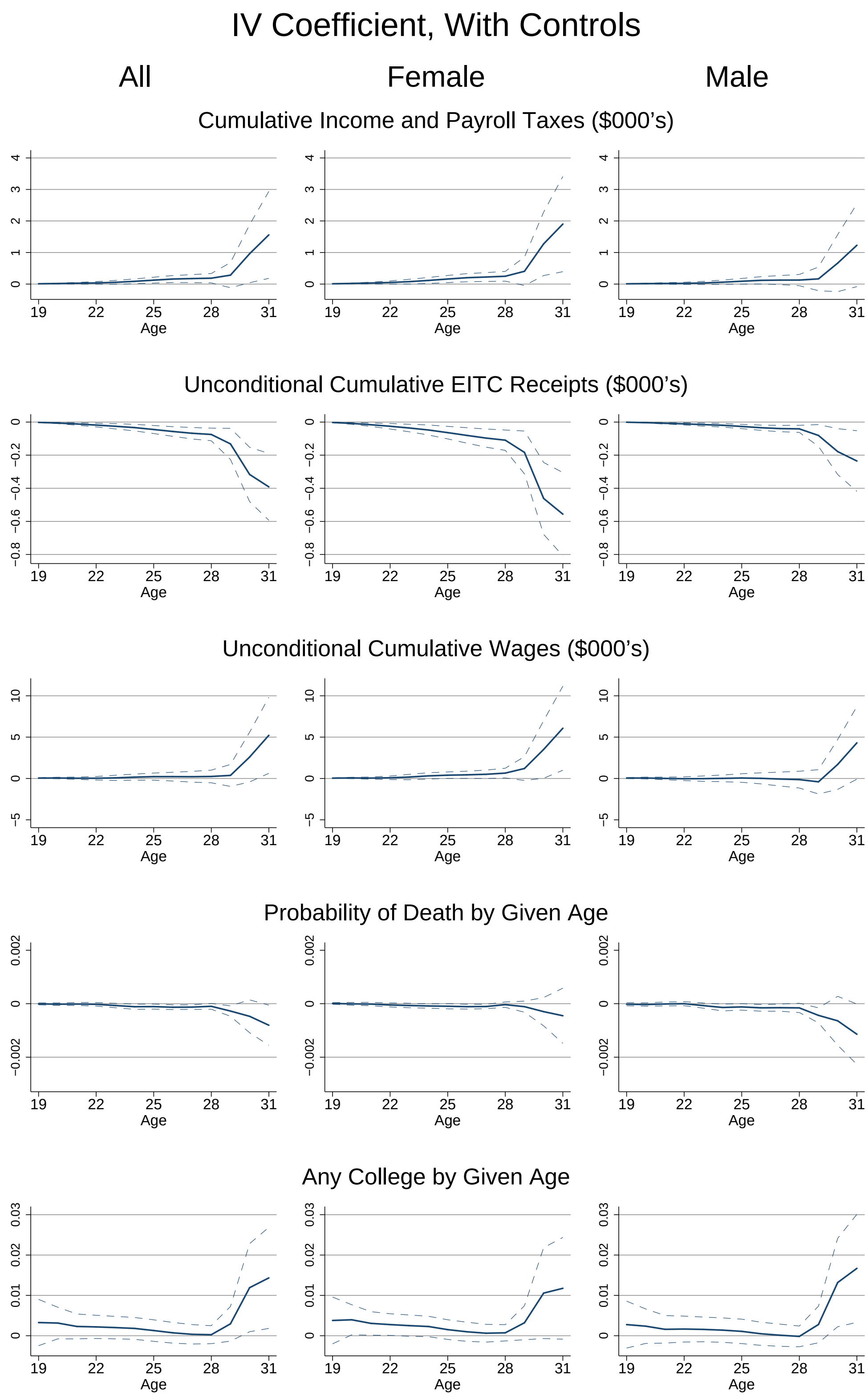
A Appendix

Figure A1: Primary Outcomes (Coefficients): Results through Age 31



Coefficient on Medicaid Eligibility from Age 0-18 in years

Figure A2: Primary Outcomes (Means): Results through Age 31

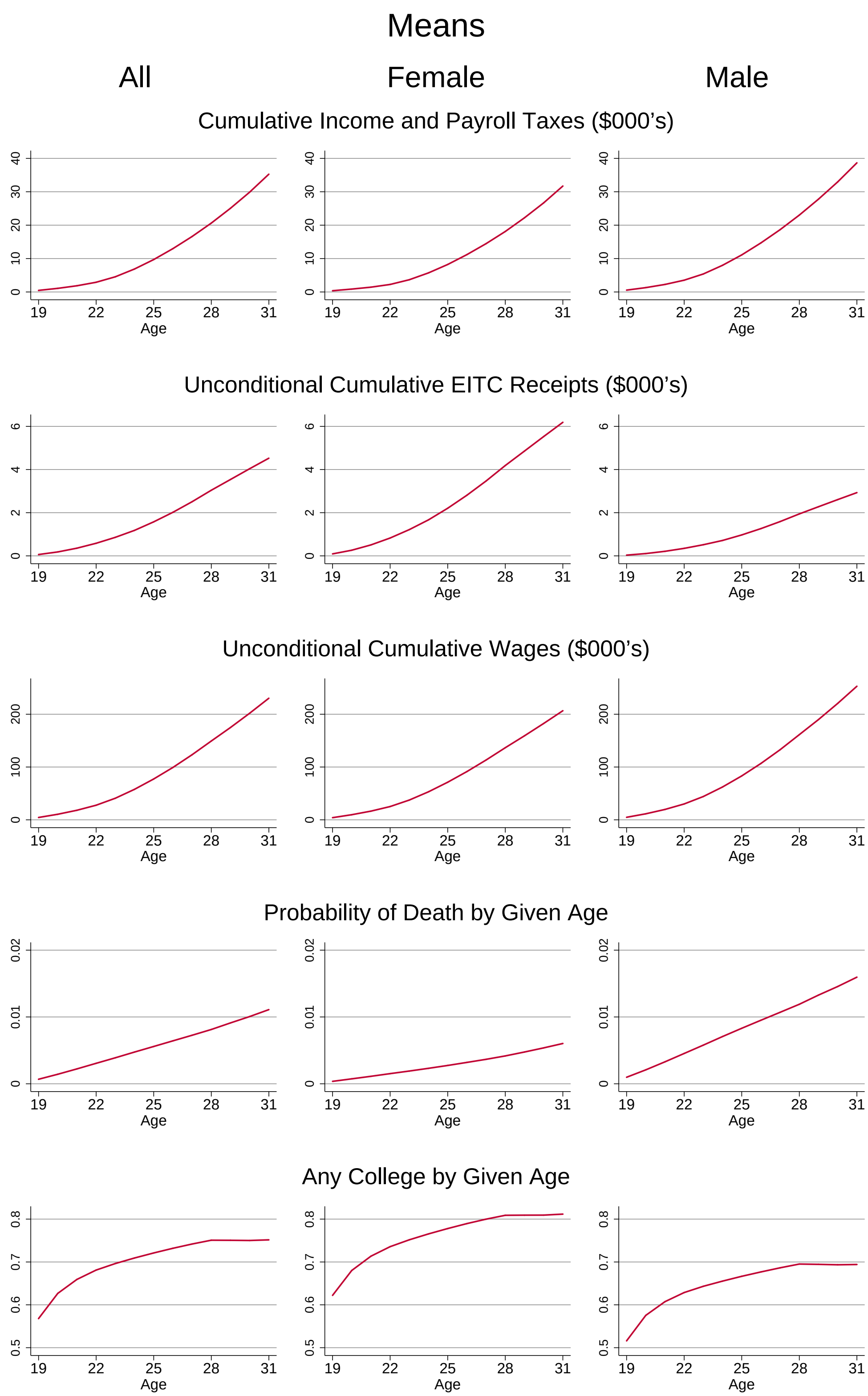
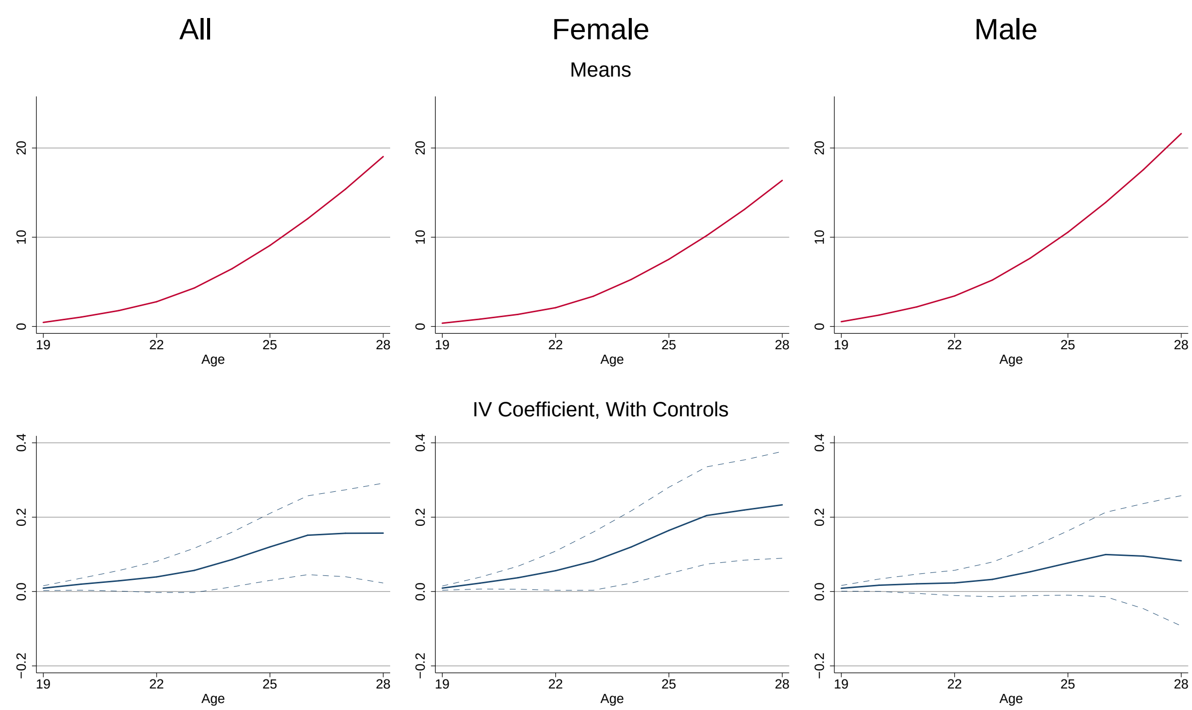


Figure A3: Results, Including "Non-Filers" - Cumulative Income and Payroll Taxes ($000's)



Coefficient on Medicaid Eligibility from Age 0-18 in years