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HEALTH INSURANCE FOR WHOM? THE 'SPILL-UP' EFFECTS OF CHILDREN'S HEALTH INSURANCE ON MOTHERS

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ABSTRACT

A rich literature documents the benefits of social safety net programs for children. This paper focuses on an unexplored margin: how children’s programs impact parents’ well-being. We explore changes in children’s public health insurance and its effects on parents’ economic and behavioral outcomes. Using a simulated eligibility for Medicaid eligibility expansions in the 1980s and 1990s, we isolate variation in children’s Medicaid eligibility due to changes in government policies. We find that increases in children’s Medicaid eligibility increases the likelihood a mother is married, decreases her labor market participation, and reduces her smoking and alcohol consumption. Our findings suggest improved maternal well-being as measured by the Center for Epidemiological Studies-Depression score, a proxy for mental health. These results uncover a new link that provides an important mechanism, parental well-being, for interpreting the literature’s findings on the long-term, short-term, and intergenerational effects of Medicaid coverage.

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1. Introduction

Social safety net programs are designed to serve as a backstop for families at the lower end of the income distribution (H. W. Hoynes and Shanzenbach 2018; H. W. Hoynes, Schanzenbach, and Almond 2016). In the United States, Medicaid is one of the largest social safety net programs, currently covering almost 40% of children (KFF 2020). A large literature shows that Medicaid improves a wide range of outcomes for recipients, including health and human capital outcomes. However, the exact pathways through which these programs affect children long-term are unclear. Examining these dynamics, and identifying potential mechanisms, is vital for understanding the overall impact of the social safety net and informing public program development to maximize their benefits.

We identify an important household spillover, that of children on parents due to their role as the main caretakers of children, including having financial responsibility for them. We call this upward intergenerational pathway "spill-up effects."[1] Children's Medicaid coverage can affect parents through multiple pathways including reduced financial burden of health insurance and health care. Less financial stress and improved intra-household relationships can also spillover back onto children through better marriage outcomes, pure income effects due to less medical and insurance spending, more time spent at home with children, and improved parental mental health and overall household environment.

In this paper, we document the existence of "spill-up" effects in the context of children's Medicaid eligibility expansions, which is a contribution in and of itself. Additionally, our work uncovers another novel contribution to the literature: We document a new mechanism through

[1] De Neve and Kawachi (2017), which review the literature of spillovers of social programs, found only 5 out of a total of 567 studies investigate spillovers from children to parents.

which these programs may affect children in the short- and long-run. Improving parental well-being, including maternal mental health, is a vital contribution to understanding intergenerational effects of, and returns to, public programs.

How does public health insurance for children affect parental outcomes? First, there has been substantial evidence that Medicaid improves children's health (Goodman-Bacon 2018; Currie and Gruber 1996a); we hypothesize that improvements in a child's mental and physical health can affect a parent's mental health and economic outcomes, independent of a parent's financial status. Second, Medicaid provides parents financial protection. We hypothesize that having an uninsured child or out-of-pocket spending on private insurance can be a large financial burden and source of stress for parents, particularly low-income parents. Public insurance can help protect parents from worrying about covering the burden of expected and unexpected medical costs, their children's health, and the financial cost of private insurance. The reduction in stress and lowered financial burden could impact labor market decisions, marriage market outcomes, and stress-related health behaviors. This hypothesis is based on the large literature on how public health insurance affects health and human capital,[2] as well as strong evidence that health insurance reduces one's own financial and mental health distress (Gross and Notowidigdo 2011; Finkelstein et al. 2012).[3] Expanding Medicaid can result in significant savings even for households with access to employer-sponsored health insurance.[4]

To determine if children's access to public insurance affects parents, we exploit variation in eligibility criteria for Medicaid and State Children's Health Insurance Program (SCHIP) over

[2] See for example (Goodman-Bacon 2018; Cohodes et al. 2016; Wherry et al. 2018; Wherry and Meyer 2016; Miller and Wherry 2019; Finkelstein et al. 2012; Currie and Gruber 1996a; 1996b).
[3] Financial disagreements are a predictor of divorce (Britt and Huston 2012; Dew, Britt, and Huston 2012).
[4] In 2000, expanding private insurance coverage from covering just the worker to covering a family increased the worker's premium contribution from $54.50 to $179.75 in nominal dollars (US Department of Labor 2003).

time (1980s-2000s) and across states for different age groups.[5] Essentially, children who live in states that have more generous eligibility rules and allow older children to be on public insurance are more likely 1) to be insured by Medicaid as they age, and 2) to have spent a larger fraction of their childhood covered by public insurance. To focus on variation due to these policy changes rather than changes due to demographic changes within a state, we use a measure of simulated eligibility that assigns the Medicaid eligibility of a fixed population using the Medicaid eligibility rules for each state in each year (Currie and Gruber 1996b; 1996a). Using this simulated eligibility, we study the effect of Medicaid expansions on mothers' outcomes measured from 1979-2010.[6] We focus on mothers because our data only have information on all children of women and not of men.[7]

We use data from the National Longitudinal Survey of Youth 1979 (NLSY79) to measure decision-making and well-being by focusing on family dynamics including marital status, divorce, labor force participation and alcohol consumption.[8] We use the longitudinal aspect of the data by including individual fixed effects to account for individual, time-invariant confounders, such as the mother's childhood experiences, baseline demographics, and other fixed unobservable characteristics. This model specification allows us to use within-mother changes in children's eligibility to identify the effects of Medicaid expansions on maternal outcomes. We also use several other controls including state-by-year fixed effects which accounts for other state-level policy changes over time. Finally, the NLSY79 has a somewhat small sample size; to address this,

[5] For the remainder of the paper, we will use "Medicaid" to encompass public insurance provided to children by both programs.

[6] We note that previous studies have documented a strong first-stage association between simulated and actual Medicaid eligibility (Cohodes et al. 2016). We use 2010 as the end of our sample because of large changes in Medicaid eligibility for adults due to the Affordable Care Act.

[7] In a supplementary analysis using a secondary dataset, we investigate the effects of changes in Medicaid on fathers.

[8] Financial distress is a leading contributor to divorce, and access to public insurance greatly improves recipients' financial situations (Gross and Notowidigdo, 2011).

we use repeated cross-sectional data from the larger, state-representative Current Population Survey Annual Social and Economic Supplement (CPS-ASEC) to augment our analysis in section 5.5 (Ruggles et al. 2020).

Many studies have investigated the validity of public insurance expansions as instruments for insurance coverage dating back to the innovative Currie and Gruber works (1996a; 1996b).[9] The main identifying assumption of the simulated eligibility is that changes in public insurance eligibility are not related to parental decision-making and well-being except through increased access to and use of public insurance by their children. We confirm the validity of the assumptions by performing a novel placebo analysis. We focus on a sample of non-mothers who have similar characteristic to the women on our sample. We assign the non-mothers a placebo eligibility from a similar mother, and as expected we find null effects on the sample of non-mothers. However, there may be additional steps on the causal pathway, such as children's use of public insurance reducing financial stress. We investigate these mediating pathways, such as ruling out mothers enrolling in Medicaid or mothers using more health care.

A main reason for the relative lack of research exploring the effect of children's public insurance eligibility on parents is data limitations. One needs a dataset that links children to parents and has information about *all* children. These data also must contain detailed outcome information on parents, preferably with repeated measures, which is rare. We use the NLSY79 Child and Young Adult dataset, which tracks all children born to *women* from the main NLSY79 sample, thus we cannot link fathers to their children. For this reason, our analysis focuses on mothers. In supplementary analysis, we use the CPS-ASEC to provide suggestive evidence for fathers, generally finding smaller effects.

[9] More recent examples include Cohodes et al. (2016), Miller and Wherry (2019), Brown, Kowalski, and Lurie (2020), and Jackson, Agbai, and Rauscher (2021).

Our main results show that a 10-percentage point increase in children's simulated Medicaid eligibility increases a mother's likelihood of being married by 3.4 percentage points or 5%. We decompose this effect and find that the marriage effect is mostly (80%) driven by women staying married (less divorce). This is consistent with literature on financial strain and relationship quality (Dew, Britt, and Huston 2012). We find that a 10-percentage point higher children's simulated Medicaid eligibility decreases the likelihood of mothers being in the labor force by 1.3 percentage points or 4%. The increase in mothers exiting the labor force comes from both the employed and the unemployed category. Analyses from the CPS-ASEC confirm these results. Effects on labor market outcomes could be from several mechanisms, such as job lock to provide insurance to children or from increased marriage stability resulting in more maternal home production. Combined with long-run effects on children, our findings suggest that a large fraction of the cost of Medicaid expansions are offset through reduced use of social safety net programs such as unemployment insurance. In Section 5.6, we interpret the size of our effects relative to other social programs.

To understand how these changes impact women's overall well-being, we explore effects on mental health. We find a substantial and robust improvement in maternal mental health in the form of a decrease in the Center for Epidemiological Studies-Depression score (CES-D), a valid proxy for mental health measures (Radloff 1977) (see Appendix C for details about CES-D). A 10-percentage point increase in children's simulated Medicaid eligibility is associated with a 2.5 percentage point decrease in maternal CES-D.

This paper makes contributions to three strands of literature. First, it contributes to our understanding of intergenerational spillovers. Several articles focus on spillovers of parental insurance coverage on children's health insurance and annual wellness visits (Sacarny, Baicker, and Finkelstein 2020; Hamersma, Kim, and Timpe 2019; Venkataramani, Pollack, and Roberts 2017; Sommers 2006; Aizer and Grogger 2003; Dubay and Kenney 2003; DeVoe et al. 2015). However, there is little research focusing on children's health insurance spillovers on adults (De Neve and Kawachi 2017), a gap in the literature we fill with our estimates of increases in Medicaid eligibility for children on their parents. Our estimates inform our understanding not only of Medicaid, but any other social program that provides an in-kind transfer to children.

Our findings help illustrate another mechanism for how children's health insurance affects their own outcomes: through parental responses to children's insurance. Children having Medicaid may increase mothers' likelihood of staying at home to spend more time with their children as well as reduce maternal stress.[10] Parental interactions with children, including having a less-stressed parent at home, can improve children's long-term health and human capital.

The relatively few papers that do focus on spillovers from children to parents mainly focus on adult children's educational attainment on elderly parents' health and mortality (Ma 2019; De Neve and Fink 2018). Koch (2015) is a notable exception and the closest paper to ours, which uses an income discontinuity to investigate the spillover effects of children's Medicaid eligibility on parental health insurance coverage. The author finds that Medicaid generosity crowds out private insurance for parents, which suggests that a reason parents seek private health insurance for themselves is to gain coverage for their children.[11] We however find no evidence that increased access to public health insurance for children affects mother's health insurance status.

We also contribute to the literature focusing on the effects of Medicaid on maternal labor supply. Results from this literature vary depending on context and target of the expansions. The

[10] An additional strain of literature focuses on the effects of adult mental health on children's well-being and participation in public programs (Kahn, Brandt, and Whitaker 2004; Noonan, Corman, and Reichman 2016).
[11] Hamersma and Ye (2021) find a similar result of private insurance crowd-out for parents.

introduction of Medicaid reportedly had no effect on labor supply (Strumpf 2011). The decoupling of cash welfare and Medicaid in the early 1980s had ambiguous impacts on married women's labor supply, and analyses of this period are sensitive to model specification (Yelowitz 1995; Montgomery and Navin 2000; Ham and Shore-Sheppard 2005). Dave et al. (2015) find expansions targeting pregnant women decreased labor supply of this group, especially for unmarried women. The novelty of our paper is based on the unexplored margin of *children's* Medicaid on mothers' labor outcomes. We think this is important not only for Medicaid, but any other social program that provides an in-kind transfer to children such as school-based nutrition programs and Head Start.

We also contribute to the broader literatures on the effects of Medicaid and on the determinants of mental health. Mental health improvements for mothers in our sample do not come from improvements in physical health of the parent but the "peace of mind" from reduced financial risk due to children having health insurance or the improvements in health of and treatment availability for their children.[12] Much of the existing research on Medicaid spillovers focuses on mothers' access to public insurance. Maternal access to Medicaid improves mothers' mental health measured by CES-D scores and Kessler scales (Guldi and Hamersma 2021; McMorrow et al. 2016). Another complementary recent working paper investigates how the aggregate social safety net affects maternal mental health and health behaviors (Schmidt, Shore-Sheppard, and Watson

[12] Generally, Medicaid is found to increase access to and use of health care (Finkelstein et al. 2012; Baicker et al. 2013; Currie and Gruber 1996a) including for mental health (McMorrow et al. 2016; Frank, Goldman, and Hogan 2003); improve health of young children (Goodman-Bacon 2018; Baicker et al. 2013; Currie, Decker, and Lin 2008); reduce mortality for near elderly adults (Miller, Johnson, and Wherry 2021); and reduce financial burden including bankruptcy (Gross and Notowidigdo 2011), although the harm from losing coverage may be larger than the benefit of gaining coverage (Argys et al. 2020). Additionally, Medicaid is associated with higher levels of family wealth (Jackson, Agbai, and Rauscher 2021). For mental health, Medicaid reduces out-of-pocket expense for mental health visits and pharmaceuticals (Ghosh, Simon, and Sommers 2019; Golberstein and Gonzales 2015), decreases psychological distress among low-income parents, reduces perceived unmet needs, and increases number of days with good mental health (Finkelstein et al. 2012; McMorrow et al. 2016; Wen, Druss, and Cummings 2015; Hampton and Lenhart 2021).

2021), finding moderate effects on mental health and mixed results on health behaviors.[13] Our work demonstrates that children's Medicaid spills over onto improvements in maternal mental health as well, an additional parameter to consider when calculating the benefits of children's public health insurance coverage.

The remainder of the paper continues with the following sections. Section 2 discusses Medicaid expansions and simulated eligibility. Section 3 presents our data. In section 4 we present our methods and identification strategy. We discuss our results in Section 5. We conclude in Section 6.

2. Medicaid Background

Medicaid is the largest provider of public insurance to children and non-elderly adults. The program covers nearly 20% of Americans and cost $557 billion in 2017 (Rudowitz, Hinton, and Antonisse 2018). Medicaid has grown rapidly given the program's modest voluntary introduction in 1965. Between 1966 and 1970 nearly all states implemented a Medicaid program. However, the generosity of these programs varied greatly, with Medicaid originally tied to cash welfare eligibility.[14] At the time, Medicaid also covered the medically needy[15] as well as children who were not categorically welfare-eligible[16] but whose family income would have qualified them.[17]

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[13] Our studies differ in important ways. While Schmidt, Shore-Sheppard, and Watson (2021) incorporate Medicaid into their study, they focus on food and cash benefits. We focus on labor and marriage market outcomes and include mental health and health behavior outcomes as secondary analyses. The panel nature of the NLSY allows us to include individual mother fixed effects in many of our analyses meaning that we exploit variation in Medicaid eligibility due to spatial, temporal, and children's age-related insurance rules.
[14] At the time cash welfare was provided through Aid to Families with Dependent Children (AFDC), the precursor to the current Temporary Assistance for Needy Families (TANF) program.
[15] https://www.kff.org/other/state-indicator/medicaid-eligibility-through-the-medically-needy-pathway/
[16] Two-parent households were not eligible for cash welfare at the time.
[17] See Gruber (2000) for a more detailed description of Medicaid policies and history.

Beginning with the Deficit Reduction Act of 1984, the federal government expanded Medicaid by increasing eligibility for pregnant women. Additional state and federal policies decoupled Medicaid from cash welfare and expanded eligibility. By the late 1980s states varied considerably in eligibility based on income and children's age. States could choose to provide Medicaid coverage to pregnant women and infants earning up to 185% of the federal poverty level (FPL).

Several federal expansions occurred in the early 1990s. First, the federal government extended coverage to all pregnant women and children up to age six in families below 133% of the FPL. Second, federal policy allowed all children born after September 30, 1983 and living below 100% of the FPL to enroll in Medicaid up to age 19. Finally, in 1997, Congress created the State Children's Health Insurance Program (SCHIP) which provided insurance to children whose parents earned too much to meet traditional Medicaid cutoffs. SCHIP eligibility thresholds vary by state and over time, and SCHIP provides matching funds for states to cover children under the age of 19 whose parents earned under 200% of the FPL.[18] Figure 1 portrays the variation over time and across states for our sample. The dotted line represents the average across all of our sample individuals.

3. Data

Our main sample data come from the National Longitudinal Study of Youth 1979 (NLSY79) from 1979 to 2010. We only use samples up to 2010 given the major reforms from the Affordable Care Act starting in 2010 that could affect mothers. NLSY79 is a nationally

[18] States are free to expand coverage to children whose parents earn above 200% of the FPL, and many have done so.

representative study of youth aged 14 to 22 in 1979. Participants were surveyed annually from 1979 to 1994 and biennially thereafter. We use a restricted version of the data which provides the state of residence of individuals at each survey. We link these data to the NLSY79 Children and Young Adults survey, which follows all biological children born to women of the NLSY79 cohort, to obtain accurate information on all children's year of birth. Women who do not yet have children do not have any measure of simulated eligibility and are thus not included in the analysis. Our research design requires information on all children for each mother, which few datasets have. The ability to link children and their mothers' responses is a major strength of the NLSY, despite a relatively small sample size of women. To address potential issues of sample size, we supplement our main analysis using repeated cross-section data from the larger CPS-ASEC for outcomes available in both data sets.

Additionally, we use detailed information on educational attainment, race/ethnicity, marital status, labor market outcomes, CES-D score, and risky health behaviors. The CES-D score is a seven-item measure of how often over the past week the respondent experienced depressive symptoms. Values vary from 0 (rarely or none) to 3 (most or all the time). CES-D scores therefore range from 0 to 21 (see Appendix C for a complete list of questions asked in the CES-D). While the CES-D is a short assessment, it has good internal consistency and test-retest repeatability as well as being correlated with other measures of mental health (Radloff 1977). Combining information on marriage and labor markets outcomes with measures of mental health is pivotal in understanding if individuals perceive the effects on work and marriages as "positive" (e.g. better mental health) or "negative" (e.g. worse mental health), something that other data sources do not provide.

Summary statistics for our analytic sample are in Table 1. Our main sample consists of approximately 4,700 women who had at least one child and were interviewed multiple times in the NLSY79, with the median respondent included in 14 waves of the data. Not all participants were interviewed in every survey wave. For time-varying outcomes, 70% of the sample were married,[19] and 19% were divorced. The average woman in our sample was employed in 61% and out of the labor force in 33% of survey periods.

Construction of Simulated Eligibility

Our simulated eligibility is constructed using data from the CPS-ASEC (Annual Social and Economic Supplement). We use the full national CPS sample of children aged 0 to 17 in 1986 as our fixed sample. Following Cohodes et al. (2016), Gross and Notowidigdo (2011), and Gruber and Simon (2008), we calculate annual state-level Medicaid eligibility for each age-by-birth cohort based on household income, accounting for household size, sex and unemployment status of the household head. Additionally, we use several other controls, such as state-by-year fixed effects, that control for other state-level policies or economic conditions that do not vary by age.

The simulated eligibility for Medicaid is the proportion of a fixed nationally representative sample of children who qualify for Medicaid applying the state-level Medicaid rules in a given year to individuals' household income and other characteristics listed above. By applying each state's eligibility rules to a fixed sample, our simulated eligibility exploits only variation in Medicaid state laws and not changes in demographic characteristics over time and across states or

[19] These are mutually exclusive measures of marital status. We use married as a dominating state, so that if a woman was divorced and then remarried, she will be included as married for all survey periods in which she responds married even though she is also divorced. If a woman is divorced but not remarried, she is coded as divorced.

economic characteristics of the household.[20] This addresses biases that may arise due to economic recessions or demographic trends across states affecting both Medicaid eligibility and coverage.

The simulated eligibility is the fraction of the fixed sample that would be eligible for Medicaid if the policies in state s when a child is age a in a given in year t were applied ($elig_{sat}$). We link the simulated eligibility to the children in the NLSY79 Children and Young Adults survey based on the state of residence, the year of the survey, and the birth year of the child.[21]

We construct two separate mother-level measures of simulated eligibility, one for our time-varying analyses on outcomes with several repeated measures over time and a second measure of simulated eligibility for our cross-sectional analyses on outcomes with one or few observations per mother. To derive the time-varying, mother-level measure of simulated eligibility, we aggregate the simulated eligibility of all of a woman's children in a given year to the mother-year level and divide by her total number of children:

$$Eligibility_{mt} = \frac{1}{J}\left(\sum_{j=1}^{J} elig_{sat}^{j}\right) \tag{1}$$

where $j \in J$ indexes the mother's jth child and $elig_{sat}$ refers to that child's eligibility in interview year t when the child is age a given their current state of residence s. We average the children's

[20] All household income measures are Consumer Price Index (CPI) corrected to account for changes in purchasing power. Groves (2020) argues the fixed year CPI correction contains a bias due to its assumption that low-wage worker incomes rise by exactly the CPI and argues this is potentially an invalid assumption during the 1970s and possibly the early 1980s when inflation was very high. However, his analyses show results were not sensitive to this potential source of bias.

[21] It is important to highlight that all children of the same age in the same year in the same state have the same simulated eligibility regardless of their household characteristics (e.g. income). This creates an advantage of simulated eligibility over actual eligibility, since actual eligibility is endogenous due to its relationship to income and family size. On the other hand, simulated eligibility is based only on policy changes. For instance, all children who are 5 years old in year 1996 living in state X have a simulated eligibility of 0.20. This means 20% of the *fixed nationally representative* sample of 5-year-olds in CPS would have been eligible for Medicaid if they lived in state X in 1996; the simulated eligibility is not based on the actual population of 5-year-olds living in state X in 1996.

eligibility by summing eligibility of all children and dividing by J, her total number of children.[22] Once children turn 18 and are no longer minors, we no longer consider their Medicaid eligibility, so we are only averaging across children below age 18. In other words, J is the number of children under 18 in year t.[23] This provides us with a dataset at the mother-year level, with each mother m in year t having an $Eligibility_{mt}$. A mother may have a different measure of simulated eligibility value in each year due to changes in the age of her children and/or state-level policies. Since women who do not yet have children do not have any measure of simulated eligibility, they are not included in the analysis. A woman whose children are all older than 18 years old would similarly be excluded from our analysis.

We also construct a measure of aggregate simulated eligibility, which can be thought of as the average eligibility of a woman's children over their life up to the time an outcome is measured. We use this measure in cross-sectional analyses:

$$AggregateEligibility_{mt} = \frac{1}{\sum_{j=1}^{J} A_j}\left(\sum_{j=1}^{J}\left(\sum_{a=1}^{A_j} elig_{sat}\right)\right) \quad (2)$$

We separately calculate total eligibility for each child j up to the time when a mother completed the CES-D scale or reported other cross-sectional outcomes by summing $elig_{sat}$ for the child at

[22] We believe averaging is more appropriate than other functions such as summing across children. First, averaging maintains the range of potential eligibility values, and effects still reflect percentage point changes in simulated eligibility. Second, and related, summing can result in a measure of simulated eligibility that can be greater than one. Third, a woman with more children will by construction have a higher simulated eligibility; it is more appropriate to treat a mother with three children each with $elig_{sat}^{j} = 0.5$ the same as a mother with one child with $elig_{sat}^{j} = 0.5$. If summing, the first mother would have a $Eligibility_{mt}$ three times as large as the second mother.

[23] As a robustness check, we calculate simulated eligibility including children over age 18 as having an eligibility of 0. This does not materially affect our simulated eligibility measure or our results (available upon request).

every age. We sum total eligibility for all children, then divide by$\sum_{j=1}^{J} A_j$, where A_j is the total number of years we have observed the *j*th child at year t.[24]

Figure 1 provides visual evidence of the variation in eligibility which we exploit in our analyses. This figure shows simulated eligibility of our entire sample and separately by state, which provides additional information of when children's Medicaid eligibility increased. This figure shows that there is substantial variation in simulated eligibility, both over time and between states.[25]

4. Methods and Identification Strategy

First, we perform time-varying analyses for mothers focusing on marital status, family size, labor force outcomes, and health behaviors. For these analyses, we estimate reduced form regressions of the form:

$$Outcome_{mt} = a + \beta_1 Eligibility_{mt} + \beta_2 Age_{mt} + \alpha_s \times Y_t + YOB_m + \gamma_m + \varepsilon_{mt} \quad (3)$$

Where outcomes are listed above and *m* indexes the individual mother at time *t*. *Eligibility* is the simulated eligibility defined in equation (1).

We also include age of the mother in a given year (Age_{mt}), current state-by-year of interview ($\alpha_s \times Y_t$), and individual mother (γ_m) fixed effects as well as a series of binary variables

[24] For example, consider a woman living in Florida interviewed in 1992, with children born in 1988 and 1990. For the older child we calculate the simulated eligibility of our CPS sample applying Florida's eligibility rules for children aged 0 in 1988, children aged 1 in 1989, children aged 2 in 1990, and children aged 3 in 1991. For the younger child, we apply Florida's eligibility rules for children aged 0 in 1990 and aged 1 in 1991. We then divide by the total number of years both children are in the sample (6 years).
[25] See Section 2 for more information on these expansions.

for children's year of birth ($\boldsymbol{YOB}_m$).[26] These fixed effects account for a large amount of potential confounders. For instance, mother fixed effects account for all time-invariant characteristics of the mother. This includes demographic characteristics like race or other unobservable characteristics like family background. The state-by-year fixed effects account for other state-level policies that do not vary with the age of children (e.g. EITC, AFDC/TANF, or mother's Medicaid eligibility) or state-level economic conditions. Additionally, state-by-year effects address the same variation as state fixed effects and year fixed effects, such as overall temporal trends in the United States. Finally, the children year of birth dummies are netting out the effects from having a particular composition of children of a given number and age, including variation that comes from fertility timing.[27] These controls allow us to isolate the exogenous variation in Medicaid eligibility that is based only on changes in Medicaid policies. We cluster our standard errors at the state level to allow for serial correlation, which is more conservative than the level of treatment assignment state-by-year-by-age of children.

As a follow-up analysis, we focus on measures of maternal well-being, primarily the CES-D score. This measure was captured at most at four points in time, in the 1992 and 1994 interviews and when a mother reached age 40 and 50. It thus does not provide enough variation to include maternal fixed effects as in equation (3). Instead, we estimate:

$$Outcome_{mt} = a + \beta_1 AggregateEligibility_{mt} + \boldsymbol{X'_{mt}}\boldsymbol{\beta_2} + \alpha_s + Y_t + \boldsymbol{YOB}_m + \varepsilon_{mt} \quad (4)$$

[26] The binary variables for children's year of birth are not strictly fixed effects since they are not mutually exclusive. For instance, a woman with children born in 1986 and 1988 will receive "1" for both year of birth variables. Additionally, the YOB_{1988} variable will only turn on for years post-1988, e.g. only when the child is alive.
[27] We note that our model is not suited to study fertility decisions as our sample only includes women who have had children. If women do not have children, then they would not receive a simulated Medicaid eligibility.

This is a similar model to equation (3), except we omit γ_m, and include additional covariates ($\boldsymbol{X}$ includes mother age and race), and use the simulated eligibility $AggregateEligibility_{mt}$ defined in equation (2).

5. **Results**

We first discuss our main results on marriage, labor markets, and health using NLSY data in subsection 5.1. In subsection 5.2 we provide support for the identifying assumptions of our empirical strategy. We then show that our results are robust to many additional specifications in subsection 5.3. We investigate heterogeneity in subsection 5.4. In subsection 5.5 we show that our results are robust to using another larger data set, the CPS-ASEC, and provide suggestive evidence of potential mechanisms for our main results. Finally, in subsection 5.6 we interpret the magnitude of our effects by scaling our estimates based on our first stage, the effect of simulated eligibility on actual eligibility at the mother level.

5.1 Main NLSY Results

In this section, we present our main estimates for the effect of Medicaid expansions. The point estimates in the tables are for a 0-to-1 or 100 percentage point (ppt) increase in simulated eligibility. When interpreting these results, we will primarily discuss 10 ppt changes in simulated eligibility, dividing our main results by 10, which is the increase over the first 10 years of our analysis (see Figure 1). We also show the effect of a 10 ppt change in simulated eligibility as a percent of the mean. In section 5.6 we provide an estimate of the first stage relationship to scale the size of the effect.

In Table 2, we use equation (3) to estimate the effect of childhood Medicaid expansions on family dynamics and maternal labor market outcomes using an individual fixed effects model. We find increasing simulated eligibility for a mother's children by 10 ppt is associated with a 3.4 ppt increase in the likelihood of a mother being married at the time of interview. This is equivalent to a 5% increase, and this result is statistically significant at the 0.1% level.[28] Next, we decompose this effect into changes in mothers never being married versus mothers getting divorced. We find the effect on being married is primarily driven by reductions in divorce (80%). Medicaid expansion could decrease divorce by reducing financial distress, which is a major predictor of divorce (Britt and Huston 2012; Dew, Britt, and Huston 2012).[29]

In terms of labor force effects,[30] a 10 ppt increase in Medicaid eligibility for one's children increases the likelihood of being out of the labor force by 1.3 ppt (4.1% decrease). We decompose this effect to determine the source of mothers exiting the labor force; were employed mothers leaving jobs, or were they unemployed and ceased their job search? We find the effect is driven by both exit out of employment and unemployment. However, the effect on employed is not statistically significant at conventional levels, and the smaller estimate on unemployed is significant at the 5% level. For a comparison, Bastian (2020) finds that the introduction of the EITC *increased* maternal employment by 3.5-5 ppt.

[28] Unlike our finding that Medicaid increases marriage, regulations requiring parents' insurance to cover adult children reduced marriage. The mechanism for those regulations is that the requirement only applies to unmarried adult children, which discourages marriage (Barkowski and McLaughlin, Forthcoming). However, this is not a plausible mechanism in our setting, especially as our effects are primarily on divorce and not on never married, and our analyses focus on adults with underage children rather than adult children.

[29]Another feasible mechanism for expanding Medicaid eligibility to reduce divorce is that Medicaid expansion lowers the incentive to gain access to Medicaid by using divorce to lower overall household income. In other words, parents do not need to lower household income for their children to become eligible for public insurance.

[30] The employment status variable we use is not available for all years (National Longitudinal Survey of Youth 1979 n.d.). We also use a binary employed/not employed variable, and find that a 10 ppt increase in simulated eligibility reduces this binary measure of employed by 1.8 ppt.

Combined with long-run effects on children, our results suggest that a large fraction of Medicaid costs is counteracted through lower spending on public programs.[31] For instance, a reduction in unemployment suggests savings from lower payouts of unemployment insurance. Additionally, since households headed by single mothers are the primary beneficiaries of cash welfare (AFDC/TANF), increases in marriage and reductions in divorce likely reduce spending on these programs (Moffitt, Reville, and Winkler 1998; Office of Family Assistance, US Department of Health and Human Services 2012). In fact, we find that a 10 ppt increase in simulated eligibility reduces the probability of receiving cash welfare by 0.6 ppt (p-value<0.10), a 4% effect (Table 5, column (6)).

Given that children's Medicaid is changing employment and marital outcomes, we want to understand if the changes are internalizing as positive for the mothers, hence we explore changes in mental health. We present results in Table 3 for maternal CES-D scores, which are based on respondents' response to 7 statements relating to the frequency with which they felt these depressive symptoms over the past week. Each response varies from 0 "Rarely or none of the time" to 3 "Most of the time," providing a maximum score of 21 with a higher score representing worse mental health.[32]

We estimate models pooling all observations and at four separate time periods or ages: in 1992 and 1994, when mothers are between 27 and 36 years old; in the first survey in which a woman participates after she turns 40; and in the first survey in which a woman participates after she turns 50. These are the only times in which respondents were given the 7-item CES-D scale. Since data on CES-D are only collected four times, this analysis uses a substantially smaller sample

[31] Additionally, research suggests that Medicaid expansions reduced mortality, increased the tax base, and decreased government transfer payments (Goodman-Bacon 2018; Brown, Kowalski, and Lurie 2020; Goodman-Bacon 2021)
[32] A full list of questions and ratings can be found in Online Appendix C.

size than the time-varying analysis in Table 2, and we do not have enough variation over time to estimate these analyses using an individual fixed effects estimator. Instead, we estimate equation (4), focusing on cross-sectional variation in aggregate simulated eligibility.

Column (1) provides estimates of CES-D scores pooled over all periods. A 10 ppt increase in aggregate eligibility from equation (4) is associated with a 0.25 point (6%) decrease in CES-D score. Effects are of a comparable size when using CES-D data from only one year or age. The results consistently point to a decrease in CES-D scores of between 4.7 and 8.6% from a 10 ppt increase in eligibility. This effect is economically meaningful and statistically significant for four out of five estimates, indicating that increased child Medicaid eligibility improves maternal mental health.

In Table 4, we explore additional outcomes focusing on stress-related health behaviors. We use a time-varying measure of alcohol consumption. Because drinking variables are not captured as often as the other sociodemographic variables these analyses have smaller sample sizes, but still include maternal fixed effects. A 10 ppt increase in Medicaid eligibility for children reduces alcohol consumption of mothers by 7.7 ppt or 13.6% decrease. Smoking is asked only periodically and does not allow for an individual fixed effect analysis.[33] However, we find strong evidence in all four periods in which cigarette smoking data are available that simulated eligibility is associated with substantial reductions in the likelihood of smoking.

In Table 5, we test for the effect of Medicaid expansions for children on other aspects of maternal socioeconomic status and access to health care.[34] Overall, we do not find much evidence

[33] We do not directly control for other policies that could impact smoking such as cigarette taxes, but we do account for cross-state variation in these policies using state fixed effects. There is no time-varying dimension in this analysis because each estimate only uses one year of data.

[34] During our sample period, NLSY consistently asks about any health insurance or not. There are more detailed questions in years outside of our sample. Questions about Medicaid specifically or sources of insurance are poorly reported.

for meaningful effects on these outcomes. Medicaid eligibility has a negative, but statistically insignificant effect on maternal income and household income. The effect sizes are modest and vary from 0.5% for household income to 0.7% for maternal income, each with large bounds.[35] It is worth noting that the direction of the effect on own income is consistent with women leaving the labor force (negative), and the direction of the effect on family income is consistent with remaining married (positive). While the estimate for highest grade is statistically significant at the 5% level, the outcome increases by 0.03 years from a 10 ppt increase in simulated eligibility. This represents a 0.21% effect, a small change.

Finally, a potential mechanism of how changes in children's eligibility could affect mothers' outcomes is by making mothers more likely to sign-up for Medicaid if they are eligible (welcome mat effect) or going to the doctor more now that they interact with the healthcare system more through their newly eligible children. We explore this directly by looking at changes in health insurance and check-ups. We estimate a 0.09 ppt increase in maternal health insurance that is marginally significant, but represents a small effect (1%). Similarly, we find a 0.08 ppt decrease in check-up, but this effect is not statistically significant. Additionally, when using the CPS-ASEC, we find that any increase in maternal insurance is through private insurance, and not Medicaid (see Table 9 columns (2) and (3)). The increase in maternal insurance is plausibly due to the increase in marriage, and not through increases in access to public insurance for mothers. These results do not provide evidence for the mechanisms of mothers' outcomes being affected by changes in mothers' own insurance status or changes in use of health care.

[35] We also test if children's Medicaid eligibility impacts characteristics of mothers' spouses. In Appendix Table A1, we show that expanding public insurance for children reduces the employment and age of mother's spouses, while having little-to-no effect on spouses' education. This is likely due to women with lower "quality" spouses staying married, but it could also be due to women initiating marriages with different types of spouses.

5.2 Support for Causal Interpretation: Placebo Test

A main concern of our identification strategy is that our simulated eligibility might be correlated with other changes that affect our main outcomes through a non-health insurance mechanism. To empirically explore this issue, we construct a placebo sample of women without children who are matched on baseline characteristics to mothers in our sample.[36] If the only way our main eligibility measure is affecting outcomes is through children's Medicaid, then we can test our measure of simulated eligibility on a sample of women without children who should not be affected. Additionally, since our model has similarities with a difference-in-differences empirical strategy, this analysis helps address the assumptions of difference-in-differences. For instance, if women are not on parallel trends before treatment (assignment of simulated eligibility once they have children), we would find non-null effects in this placebo analysis.

Using propensity score matching,[37] we create a sample of women without children who are similar to our main sample. We assign these women (non-mothers) the simulated eligibility of the mothers they are matched with, although they actually have no simulated eligibility because they are childless. We then estimate models using equation (3) or equation (4) on this matched sample of childless women. The results of this analysis are presented in Table 6. Overall, we show that simulated eligibility in this childless women sample is not associated with any of our dependent variables from Table 2 and Table 3; the point estimates are smaller and not statistically significant. This increases our confidence that our measure of simulated eligibility is only working through

[36] We match mothers on baseline characteristics including childhood poverty, number of siblings, educational attainment of parents, armed forces qualification test in 1981, family size in 1980, and highest grade in 1980 using propensity score matching.
[37] Propensity score is based on time-invariant *mothers'* characteristics: low childhood SES, number of siblings, parents' education, childhood family size, baseline education, and Armed Forces Qualification Test score.

children's Medicaid rather than capturing other types of changes that are correlated with our sample of mothers.

5.3 Robustness Checks

First, we test the sensitivity of our results to different model specifications. We do this by incrementally adding additional covariates to our model to show they are not materially affected by the specification we use. In Appendix Figure A1 we present the results for the time-varying outcomes in Table 2, including point estimates and 90% confidence interval whiskers. The first point estimate uses our baseline specification. Estimate two uses a model that removes sample weights from the main model. The third estimate uses the baseline model plus number of children fixed effects.[38] The fourth point estimate uses the baseline model plus fixed effects for current state-by-mother year of birth. The fifth point estimate uses the baseline model plus fixed effects for number of children-by-current year. Finally, the sixth point estimate uses a model that combines the fourth and the fifth estimates. Appendix Figure A2 provides a similar analysis for our cross-sectional outcomes in Table 3. In both figures, our estimates are quite stable regardless of the covariates included in this specification.

An alternative explanation for our results is that maternal health is improved at the time of birth and all benefits that we find from that point on are actually a reflection of that improved health, rather than children's eligibility expansions (Guldi and Hamersma 2021). This could be the case if expansions of maternal health or health insurance coverage are highly collinear with child expansions. We argue this is not the case for several reasons. First, we do not find evidence that

[38] This is not perfectly collinear with the children birth year dummies due to multiple births.

Medicaid expansions for children are strongly correlated with maternal health insurance or use of care (see Table 5 and Table 9).

Second, we directly test whether maternal Medicaid eligibility, rather than children's eligibility, is driving our results in two ways. First, we drop mothers who just recently gave birth because infant eligibility and maternal eligibility are highly collinear at this time. Results from these analyses, presented in Appendix Table A2 and Appendix Table A3, are consistent with our main results. Second, we include a control for simulated eligibility for prenatal Medicaid in Appendix Table A4 and Appendix Table A5. Again, our results for the effect of children's Medicaid are robust to this additional control, indicating that maternal access to Medicaid is not driving our results.

Another concern could be that women under 20 may not have entered the labor or marriage market yet, and so our results are capturing changes over time in age that are correlated with eligibility. We do not think this is driving our results as we are controlling for age and we find the same effects on cross-section data using the CPS-ASEC. We go even further with NLSY by checking for robustness if we drop women who are under age 20. In Appendix Table A6 we reproduce the analysis in Table 2 excluding these women and find similar results.

Another potential concern is that since children under five-years old are more likely to require at-home supervision and are eligible for a wider range of social programs. While our state-by-year fixed-effects likely address this concern, we also estimate the effect of simulated eligibility on maternal outcomes from Table 2 excluding mothers with children under five in Appendix Table A7 to properly adjust for these age dynamics. The effects on marriage are fairly stable, but the effects on employment are larger. The means that effects on labor force participation and employment are larger for women with older children. The fact that women with older children are more responsive to changes in Medicaid eligibility speaks to them having more options than women in states with less generous children's Medicaid eligibility.

Since NLSY is a longitudinal dataset, there may be states in which we have a small number of observations. Due to privacy regulations from BLS we cannot show how many observations per state and year there are. In order to explore the concern about low sample size per state, we exclude states that have few observations per wave (less than 20 women on average; this is the cut-off used by (Kondo 2015). These results are in Appendix Table A8 and Appendix Table A9. The estimates are robust for both time-varying outcomes from Table 2 and cross sectional CES-D measures from Table 3. Following the literature that uses state-by-year-by-race cells, we also use race-specific measures of simulated eligibility. These results are in Appendix Table A10 and Appendix Table A11, and these estimates are quite similar to the main estimates in Table 2 and Table 3.

Another potential concern with our simulated eligibility measure is that individuals endogenously move to states with more generous Medicaid eligibility rules. While moving within-state is common for lower-income families, inter-state mobility is relatively low for lower-SES mothers. For instance, people with a high school diploma or less are half as likely to move between states as people with college degrees, and parents are 25% less likely to move than people without children (Molloy, Smith, and Wozniak 2011). We also empirically test whether increased simulated eligibility causes between-state relocation, finding little evidence; a 10 ppt increase in simulated eligibility has a point estimate of 0.14 ppt for moving to another state (p-value=0.65).

Lastly, we use randomized inference to test if our results might be driven by random noise. It is possible that our specification is capturing another component that is correlated at the state level with eligibility and outcomes. Hence, we test the robustness of our main model in a specification in which we randomly assign each child a placebo state of residence, a year of interview, and a year of birth. Then given this random assignment we merge their simulated eligibility and re-run our analysis. We repeated this process 300 times. We report the results from this exercise in the form of "Randomized inference p-values", which are based on how many placebo point estimates are larger in magnitude than the main point estimate. To address the clustering nature of treatment assignment, each child born in the same year is assigned the same placebo birth year; each child living in the same state is assigned the same placebo state of residence; and each interview year is assigned the same placebo interview year. This is more conservative than only assigning each birth year-by-state of residence-by-interview year the same placebo combination.

Appendix Table A12 and Appendix Table A13 present results for these analyses. The top row reproduces point estimates from Table 2 and Table 3. The second row provides original p-values from standard errors clustered at the state level, and the third row shows randomized inference p-values.[39] All statistically significant results are still significant when using randomized inference, with the lone exception of "Never Married," consistent with our findings not being driven by random noise.

5.4 Heterogeneity Analysis

We next discuss how effects of Medicaid eligibility vary by race.[40] Black and Hispanic individuals are more likely to be enrolled in Medicaid ("U.S. Census Bureau QuickFacts: United States" 2019; "Medicaid Enrollment by Race/Ethnicity" 2017). We analyze our effects separately

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[39] Since we are using 300 iterations, we can only say that p-values are less than 0.003 when no placebo estimates are larger in magnitude than the main point estimate.

[40] In Appendix B, we additionally stratify results by mother's childhood socioeconomic status (SES) (Appendix Tables B1 and B2), and number of children (Appendix Tables B3 and B4). Please see this appendix for discussion of these results.

by race and ethnicity the outcomes in Appendix Table A14 (time-varying outcomes) and Appendix Table A15 (cross-sectional outcome CES-D). Because of the relatively small number of respondents by state and racial/ethnic group, we focus on two large groups: 1) Black and Hispanic mothers combined in Panel A, and 2) all non-Black non-Hispanic mothers in Panel B, a group which mostly consists of White mothers.

Results in Appendix Table A14 for both groups move in the same direction. However, there are some notable differences in the magnitudes of these effects. Despite differences in coefficient, the effect size on being married as a percent of the group-specific mean is quite similar for both groups (4.6% vs. 5.0% per 10 ppt increase in simulated eligibility). The effects on labor market outcomes are generally larger for non-Black non-Hispanic mothers, including the fact that non-Black non-Hispanic mothers have a 1.7 ppt increase (5.3%) in being out of the labor force, which is nearly twice as large as the 1.0 ppt increase (3.0%) for Black and Hispanic mothers.

In Appendix Table A15, we perform similar subsample analyses by race and ethnicity for CES-D. Results are quite similar across both groups for the pooled ages/years sample, indicating that public insurance eligibility for children improves mental health for mothers across these racial/ethnic groups.

5.5 CPS-ASEC Analyses: Larger Sample Size, Father's Outcomes, Mechanisms

To address potential issues of sample size in the NLSY, we supplement our main analysis using repeated cross-section data from the larger, state-representative CPS-ASEC for outcomes available in both datasets. The repeated cross-sectional nature of the CPS-ASEC prevents the use of individual mother fixed effects.[41] Thus, we estimate equation (3) without mother fixed effects, and include race/ethnicity controls.

In Table 7, we show the estimated effect of simulated eligibility on our primary labor market and marriage market outcomes using the CPS-ASEC. Comparing these estimates to the estimates in Table 2 using the NLSY, we can see that the effects are qualitatively similar but generally larger when using the CPS-ASEC. Additionally, to better compare similar models across the CPS-ASEC and NLSY, we estimate models without mother fixed effects using the NLSY data in Appendix Table A16. Overall, the main interpretation is robust to model specification and data source: Medicaid expansions for children increase marriage stability for mothers and increase the probability the mother is out of the labor force.

Next, we examine the effect of Medicaid expansions for children on fathers' outcomes using the CPS-ASEC in Table 8.[42] Based on previous research, we expect that fathers may be less impacted than mothers (Willage and Willen 2020; Lundberg and Rose 2000; Andresen and Nix Forthcoming). Generally, the effects on fathers are much smaller than on mothers, less than half the size for married. For labor force outcomes, fathers' results are not statistically significant and very small.

We also use the CPS-ASEC to investigate potential mechanisms between public insurance for children and the effects on their mothers in Table 9. In column (1), we investigate if women who leave the labor force increase home production. We find that increased Medicaid eligibility is associated with women leaving the labor force to engage in homemaking. This suggests that

[41] While the CPS-ASEC can provide up to two consecutive years of data for a subset of mothers, this follow-up does not provide enough variation to perform a longitudinal analysis. For this analysis, we only keep the first observation of a woman or man in the CPS-ASEC.
[42] This analysis is not possible with the NLSY analysis since we cannot track all fathers' children over time. Additionally, the CPS sample only includes fathers who are living in the same household as their children (the same requirements for mothers in the CPS). This creates a select sample of fathers if fathers are much less likely to be single parents.

Medicaid's impact on labor and marriage markets reduces financial need for women to work and instead increases home production, including non-financial investment in children.

In columns (2) and (3) of Table 9, we further investigate the relationship between children's public insurance and mothers' insurance, which we examined using the NLSY in Table 5. We find a very similar effect size of approximately 1% for any insurance in the CPS-ASEC sample. Using the CPS-ASEC, we can further examine if this is due to mothers' access to Medicaid increasing at the same time as children's access. We do not find evidence that expanding insurance for children increases Medicaid take-up for mothers. This suggests that mothers gain more access to private insurance, plausibly due to staying married and access to a spouse's employer-sponsored health insurance.

Finally, using the CPS-ASEC in Appendix Table A17, we confirm our findings on smoking from the NLSY in Table 4. We find that increasing simulated eligibility for children by 10 ppt reduced the probability that a mother in the CPS-ASEC is a current smoker or daily smoker by about 4.5 ppt, which is of a similar magnitude to the effect when using the NLSY sample.[43]

5.6 Interpretations of Effect Magnitude

To understand the magnitude of the effects, we perform several exercises. The coefficients on our primary explanatory variable represent a 100 ppt change in the children's eligibility. We note that using 100 ppt simulated eligibility for interpretation is not adequate because (1) this is an out of sample prediction and (2) that would be more similar to a policy of universal children coverage regardless of income. This is an out of sample prediction as the maximum simulated eligibility in our sample is 88.2%, the mean is 27%, and median is 23%. Since 100 ppt is an

[43] Cigarette smoking questions come from the CPS Tobacco Use Supplement.

irrelevantly large change in simulated eligibility, we have focused on a 10 ppt change. This scaling comes from an approximation of our sample's average absolute difference over the first 10 years of our sample period.

The estimates presented above should be interpreted as intent-to-treat (ITT) estimates. They represent changes in simulated Medicaid eligibility rather than changes in actual eligibility. Estimating actual eligibility is difficult because there are also several rules and exceptions across states and years that impact eligibility. We follow an approach from the literature (Currie and Gruber 1996b; Gruber and Simon 2008; Cohodes et al. 2016) that uses detailed family income and other familial characteristics to estimate actual eligibility for each child. With this measure of eligibility, we can estimate the local average treatment effect (LATE) of changes in actual eligibility rather than simulated eligibility. As the literature suggests, focusing on actual eligibility income thresholds is an important parameter of interest since this is what the policymaker can control, rather than focusing on take-up, which the policymaker cannot enforce.

Many papers have already explored the "first stage" effect of the Medicaid simulated eligibility. Our setting is closest to Cohodes et al. (2016), which estimates that a 10 ppt change in simulated eligibility leads to an 8.5 to 9.5 ppt increase in actual eligibility. We can use these estimates of the first stage to divide our ITT estimates to obtain a LATE. However, the differences between our approach and the rest of the literature include that (1) our level of observation is at the mother level, not the child level, and (2) we use an aging fixed panel rather than a cross-section. We expect our estimates of the first stage to be smaller for these reasons.

To illustrate the first point, in Panel A of Appendix Table A18 we use the simulated eligibility derived from the CPS-ASEC and true Medicaid eligibility using our main NLSY data as well as true eligibility from CPS-ASEC. Column (1) uses NLSY data and the same model as our main time-varying analysis in Table 2; column (2) uses CPS-ASEC mother-level data and the model we use for our CPS-ASEC analysis in Table 7-Table 9; and column (3) uses CPS-ASEC child-level data.

We find large and highly statistically significant effects across the data sources in Panel A of Appendix Table A18, providing evidence of a first stage effect. Using the NLSY mother-level data, we find that a 10 ppt increase in simulated eligibility has a first stage of 4.6 ppt in column (1). Even though smaller than the child-level first stage found in literature, our estimates for the mother-level approach with our specification are still relatively sizeable. We then estimate the first stage using CPS-ASEC at the mother's level (column (2)) and using CPS-ASEC at the children's level (column (3)). In column (2), we estimate an effect of 1.6 ppt at the mother-level, and in column (3), we estimate an effect of 7.1 ppt at the child-level, which is more consistent with previous findings using similar data structures.

We use our primary first stage estimate of 0.459 to calculate the LATE of actual eligibility for all of our main outcomes. The results from this exercise are in Panel B of Appendix Table A18. Column (1) of Panel B is the 10 ppt effect from Table 2 and the pooled estimate from Table 3; column (2) scales those estimates by the NLSY first stage from the first column in Panel A; column (3) shows the outcome mean; and column (4) shows the scaled estimate as a percent of the mean.

For example, we find that a 10 ppt increase in actual eligibility leads to a 7.4 ppt increase in marriage rates, representing a 11% effect. To understand the size of this effect, we can compare it to estimates of other social programs in the literature, such as the EITC. The EITC is a program that provides tax credits that phase in and out based on a household's wage earnings and number of children. Bastian (2017) reports that 10 ppt increase in state-EITC rate increases marriage rates by 1.5 ppt or about 3%.

Many factors can drive the difference between our estimate and the ones from the EITC. We think an important difference is the type of transfer; the EITC is a cash transfer that goes directly to the parents and is managed and spent by the parents. On the other hand, Medicaid is an in-kind transfer that is not fungible to other household members. Hence, even though these programs have similarities, they are also very different. It is important to highlight that since this is the first estimate of children's Medicaid on parents, there is not a directly comparable estimate in the literature.

Another way to understand the size of this effect is to compare how changes in children's Medicaid eligibility explain changes in outcome variables in our sample. For example, the average change in marriage over a ten-year span is 42.7 ppt. This means that our estimates explain about 8.75% of the overall increase in marriage rates over a ten-year span.[44] We perform this exercise for all the main outcome variables, and the results are in Panel B, column 6. Overall we find modest to large scaled effects as a fraction of the ten-year change. For the large effect on divorce, we find a high percentage mainly because the mean is very low during the 10-year range we explore.

6. Discussion and Conclusion

Our results indicate that increases in children's Medicaid eligibility lead to mothers being more likely to remain married, less likely to work outside the home, and less likely to consume alcohol or smoke. We also find evidence of an improvement in maternal mental health, as captured by CES-D scores.

Taken together, these results suggest an improvement in overall maternal well-being. However, higher rates of marriage and lower labor force participation for women may not be

[44] Calculation: (Change in simulated elig * Effect of simulated elig) / Change in marriage = (0.11*0.0034) / 0.42

universally welfare-improving. For instance, if Medicaid eligibility increases the likelihood of a woman remaining in an unhappy marriage and/or reduces her labor force participation and thus her professional capital and outside options, these women could be worse off. If on the other hand, the effects on labor and marriage reflect reduced financial constraints and better intra-household division of labor, then many women would be better off.

To unpack this more, we consider our how our effects may impact maternal welfare. Our marriage results are consistent with that of Yelowitz (1998) who finds that 1980s and 1990s child expansions increased marriage.[45] Marital disruption can harm children and adults including decreasing health insurance coverage of both mothers and children (Peters, Simon, and Taber 2014) and increasing financial strain (Finkelstein et al. 2012; Gross and Notowidigdo 2011).[46] While the direction of causality is unclear as child health problems increase both financial strain and the likelihood of a break-up (Reichman, Corman, and Noonan 2004), the positive benefits of increasing Medicaid eligibility are clear.

The relatively large effects we find on labor market outcomes suggest that lack of public insurance for children leads to maternal job-lock. That mothers are participating in the labor force to provide health insurance for their children and when Medicaid eligibility for children is increased, they are able to leave the market without negative consequences for their children's access to health care.[47] Additionally, and consistent with our findings, maternal labor supply

[45] More recent Medicaid expansions provide contradictory evidence on marriage effects. Slusky and Ginther provide evidence of fewer medical divorces among those aged 50-64 with a college degree to protect the assets of the healthy spouse (Slusky and Ginther 2017), while Hampton and Lenhart (2019) find evidence of lower marriage rates following the most recent Medicaid expansions.
[46] Others argue Medicaid expansions actually decreased savings (Gruber and Yelowitz 1999), although recent research find expansions increase family wealth (Jackson, Agbai, and Rauscher 2021).
[47] A large literature on job lock and Medicaid exists, but generally focuses on adult expansions. See e.g. (Hamersma and Kim 2009; Garthwaite, Gross, and Notowidigdo 2014; Argys et al. 2020)

responds to children's health (Corman, Noonan, and Reichman 2005; Gould 2004; Eriksen et al. 2021).

Recent work suggests large benefits of Medicaid expansion on children's future health and human capital outcomes (see e.g. Cohodes et al. 2016; East et al. 2017; Miller and Wherry 2019). Improvements in maternal well-being and higher rates of parents remaining married may provide a potential mechanism for improved children's outcomes. Related to our finding strong improvements in maternal mental health associated with children's Medicaid expansions at several different ages, Guldi and Hamersma (2021) find improvements in maternal mental health caused by Medicaid expansions for pregnant women; this effect persists through age 3 of the child.[48] Additionally, Reichman et al. (2015) provide evidence that higher rates of post-partum depression are associated with reduced likelihood of a couple remaining together after a birth, as well as worse maternal mental health and infant health post birth (Slomian et al. 2019). These effects suggest the strong interconnectedness of children's health insurance and maternal marriage market decisions, labor market decisions, and depressive symptoms.

Combined with long-run effects on children, our results suggest that a large fraction of Medicaid costs may be recouped through lower spending on public programs (Brown, Kowalski, and Lurie 2020; Goodman-Bacon 2021). For instance, a reduction in unemployment suggests savings from lower payouts of unemployment insurance. Additionally, since households headed by single mothers are the primary beneficiaries of cash welfare (AFDC/TANF), increases in marriage and reductions in divorce likely reduce spending.

[48] While this study uses a different source of variation, that of maternal Medicaid expansions, the results complement those of our own, using child Medicaid expansions, in finding improved maternal mental health from Medicaid expansions. However, these need not be mutually exclusive, and the effects may in fact build on each other.

Using a longitudinal panel of mothers followed for nearly 30 years, we find evidence that Medicaid eligibility increases the probability of a mother marrying, remaining married, and decreases the labor force participation of these women. We provide strong evidence of a positive effect of this increased eligibility on maternal health behaviors in terms of reduced drinking and smoking, and improvements in maternal mental health as measured by CES-D. Our results point to an additional positive spillover of children's Medicaid eligibility: improvements in maternal health. They also provide evidence of a potential mechanism through which long-term benefits of Medicaid coverage in childhood works. Future research should investigate whether these effects persist into old age.

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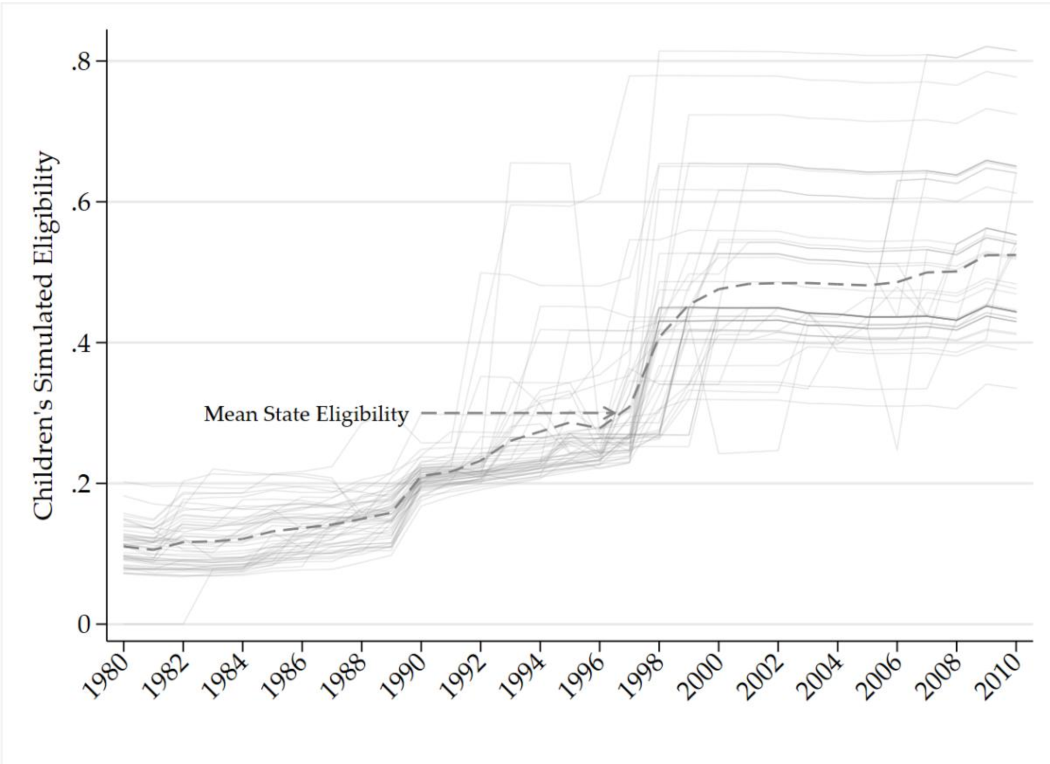
Figures

Figure 1: Variation in Treatment Variable Over Time, Overall and by State



Notes: The y-axis is the simulated eligibility over time. The black, dashed line is for the full sample, and the gray lines are for each state.

Tables

Table 1: Summary Statistics

Panel A: Main Time-Varying Simulated Eligibility and Outcomes	Mean	SD	N
Simulated Elig. - Time-Varying	0.298	0.154	62,550
Married	0.694	0.461	62,545
Divorced	0.187	0.390	62,545
Never Married	0.111	0.314	62,545
Out of Labor Force	0.326	0.469	51,174
Employed	0.612	0.487	51,174
Unemployed	0.062	0.240	51,174

Panel B: Main Cross-Sectional Simulated Eligibility and Outcomes	Mean	SD	N
Simulated Elig. - Year 1992	0.246	0.098	3,299
Simulated Elig. - Year 1994	0.262	0.107	3,448
Simulated Elig .- Age 40	0.306	0.133	3,649
Simulated Elig. - Age 50	0.318	0.133	3,339
Simulated Elig. - All Ages and Years	0.286	0.124	13,714
CES-D - Year 1992	4.630	4.281	3,299
CES-D - Year 1994	4.380	4.455	3,448
CES-D - Age 40	3.659	4.395	3,649
CES-D - Age 50	4.500	4.737	3,339
CES-D - All Ages and Years	4.279	4.496	13,714

Panel C: Baseline Characteristics	Mean	SD	N
Childhood Poverty Freq. Before 1985	1.431	1.812	4,695
Number of Siblings in 1979	4.040	2.687	4,689
Mother's Highest Grade in 1979	10.621	3.143	4,427
Father's Highest Grade in 1979	10.625	3.948	3,982
Armed Forces Qualification Test in 1981	37.898	27.259	4,539
Family Size in 1980	4.253	2.235	4,595
Highest Grade in 1980	11.066	1.919	4,595

Table 2: Regression on Main Time-Varying Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Married	Divorced	Never Married	Out of Labor Force	Employed	Unemployed
Simulated Elig.	0.345***	-0.276**	-0.126**	0.134+	-0.0720	-0.0603*
	(0.0866)	(0.0834)	(0.0374)	(0.0781)	(0.0763)	(0.0296)
N	62,545	62,545	62,545	51,174	51,174	51,174
Dep. Var. Mean	0.694	0.187	0.111	0.326	0.612	0.062
10 PPT Effect	0.034	-0.028	-0.013	0.013	-0.007	-0.006
10 PPT Effect as %	4.963	-14.804	-11.337	4.102	-1.177	-9.788

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Standard errors are in parentheses and are clustered at the state level.

Interpretation: Regressions are run at the mother-by-year level. The main explanatory variable (Simulated eligibility) has a range of 0-to-1 so the estimated β represents a 100 ppt change in eligibility.

10 PPT Effect rescales the main estimate by 1/10 and represents a 10 ppt change in eligibility. 10 PPT Effect as % is the 10 PPT divided by the dependent variable mean.

Model: Model controls include individual FE, state-by-year FE, age of mother at interview, and a series of binary variables for children's years of birth.

Regressions weighted by sample weight provided by NLSY.

Sample code: reghdfe unemployed Zany_time AGEATINT cyob* [pw=SAMPWEIGHT] if fips>0 & AGEATINT>0 & numkid>0, vce(cluster fips) a(id i.fips#i.year)

Table 3: Regression on Main Cross-Sectional Outcomes; CES-Depression Scale

	(1)	(2)	(3)	(4)	(5)
	All Years/Ages	Year 1992	Year 1994	Age 40	Age 50
Simulated Elig.	-2.468**	-3.232*	-2.040	-3.126*	-2.450*
	(0.771)	(1.527)	(1.467)	(1.330)	(1.166)
N	13,714	3,299	3,448	3,649	3,339
Dep. Var. Mean	4.28	4.63	4.38	3.66	4.50
10 PPT Effect	-0.25	-0.32	-0.20	-0.31	-0.24
10 PPT Effect as %	-5.77	-6.98	-4.66	-8.55	-5.44

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Standard errors are in parentheses and are clustered at the state level.

Interpretation: Regressions are run at the mother level. The main explanatory variable (Simulated eligibility) has a range of 0-to-1 so β represents a 100 ppt change in eligibility.

10 PPT Effect rescales the main estimate by 1/10 and represents a 10 ppt change in eligibility. 10 PPT Effect as % is the 10 PPT divided by the dependent variable mean.

Model: Model controls include state FE, year FE, race FE, age of mother at interview, and a series of binary variables for children's years of birth.

Regressions weighted by sample weight provided by NLSY.

Sample code: reghdfe cesd_7item_92 Zany1_1992 AGEATINT cyob* [pw=SAMPWEIGHT] if fips>0 & year==1992, vce(cluster fips) a(i.fips i.year i.SAMPLE_RACE_78SCRN)

Table 4: Regression on Secondary Outcomes, Health Behaviors

	(1)	(2)	(3)	(4)	(5)
	Any Alcohol - Time-Varying	Smoking - Year 1992	Smoking - Year 1994	Smoking - Year 1998	Smoking - Year 2008
Simulated Elig.	-0.772***	-0.628**	-0.844***	-0.863***	-0.677***
	(0.140)	(0.190)	(0.217)	(0.158)	(0.118)
N	25,155	3,328	3,454	3,489	3,321
Dep. Var. Mean	0.566	0.288	0.291	0.279	0.224
10 PPT Effect	-0.077	-0.063	-0.084	-0.086	-0.068
10 PPT Effect as %	-13.626	-21.831	-29.016	-30.889	-30.247

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Standard errors are in parentheses and are clustered at the state level.

Interpretation: Regressions are run at the mother-by-year level. The main explanatory variable (Simulated eligibility) has a range of 0-to-1 so the estimated β represents a 100 ppt change in eligibility.

10 PPT Effect rescales the main estimate by 1/10 and represents a 10 ppt change in eligibility. 10 PPT Effect as % is the 10 PPT divided by the dependent variable mean.

Model: Model controls in column (1) time-varying outcomes include individual FE, state-by-year FE, age of mother at interview, and a series of binary variables for children's years of birth.

Model controls in columns (2)-(5) cross-sectional outcomes include state FE, year FE, race FE, age of mother at interview, and a series of binary variables for children's years of birth.

Regressions weighted by sample weight provided by NLSY.

Sample code: reghdfe alcohol Zany_time AGEATINT cyob* [pw=SAMPWEIGHT] if fips>0 & AGEATINT>0 & numkid>0, vce(cluster fips) a(id i.fips#i.year)

reghdfe current_smoker98 Zany1_1998 AGEATINT AGEATINT cyob* [pw=SAMPWEIGHT] if fips>0 & year==1992, vce(cluster fips) a(i.fips i.year i.SAMPLE_RACE_78SCRN)

Table 5: Regression on Secondary Outcomes, Health Care Access and Socio-economic Status

	(1)	(2)	(3)	(4)	(5)	(6)
	Income - Time-Varying	Family Income - Time-Varying	Highest Grade - Time-Varying	Health Ins. - Time-Varying	Check-Up - Cross Section	Receiving Welfare - Time-Varying
Simulated Elig.	-564.4	1018.8	0.273*	0.0895+	-0.0796	-0.0651+
	(4289.5)	(23873.5)	(0.111)	(0.0449)	(0.0828)	(0.0384)
N	60,482	52,384	62,458	36,385	16,455	61,771
Dep. Var. Mean	22299.55	81871.95	12.84	0.87	0.728	0.164
10 PPT Effect	-56.44	101.88	0.03	0.01	-0.01	-0.007
10 PPT Effect as %	-0.25	0.12	0.21	1.02	-1.09	-3.980

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Standard errors are in parentheses and are clustered at the state level.

Interpretation: Regressions are run at the mother-by-year level. The main explanatory variable (Simulated eligibility) has a range of 0-to-1 so the estimated β represents a 100 ppt change in eligibility.

10 PPT Effect rescales the main estimate by 1/10 and represents a 10 ppt change in eligibility. 10 PPT Effect as % is the 10 PPT divided by the dependent variable mean.

Model: Model controls in columns (1)-(4) time-varying outcomes include individual FE, state-by-year FE, age of mother at interview, and a series of binary variables for children's years of birth.

Model controls in column (5) cross-sectional outcomes include state FE, year FE, race FE, age of mother at interview, and a series of binary variables for children's years of birth.

Regressions weighted by sample weight provided by NLSY. Income measured in real 2020 dollars.

Sample code: reghdfe income Zany_time AGEATINT cyob* [pw=SAMPWEIGHT] if fips>0 & AGEATINT>0 & numkid>0, vce(cluster fips) a(id i.fips#i.year)

reghdfe check_up Zany1_1998 AGEATINT AGEATINT cyob* [pw=SAMPWEIGHT] if fips>0 & year==1992, vce(cluster fips) a(i.fips i.year i.SAMPLE_RACE_78SCRN)

Table 6: Placebo Test, Matched Non-Mother Women Unaffected by Policy

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Married	Divorced	Never Married	OOLF	Employ	Unemp.	CES-D 1992	CES-D 1994
Simulated Elig.	0.0596	0.00534	-0.0504	0.0129	-0.0160	0.00262	0.864	-0.0789
	(0.0419)	(0.0361)	(0.0257)	(0.0305)	(0.0380)	(0.0256)	(1.268)	(1.610)
N	19,901	19,901	19,901	17,754	17,754	17,754	996	836
Dep. Var. Mean	0.376	0.117	0.502	0.089	0.861	0.049	4.188	3.832
10 PPT Effect	0.006	0.001	-0.005	0.001	-0.002	0.000	0.086	-0.008
10 PPT Effect as %	1.586	0.456	-1.002	1.446	-0.186	0.533	2.063	-0.206

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Standard errors are in parentheses and are clustered at the state level.

Interpretation: Regressions are run at the mother-by-year level. The main explanatory variable (Simulated eligibility) has a range of 0-to-1 so the estimated β represents a 100 ppt change in eligibility.

10 PPT Effect rescales the main estimate by 1/10 and represents a 10 ppt change in eligibility. 10 PPT Effect as % is the 10 PPT divided by the dependent variable mean.

Model: Model controls for time-varying outcomes include individual FE, state-by-year FE, age of mother at interview, and a series of binary variables for children's years of birth.

Model controls for cross-sectional outcomes include state FE, year FE, race FE, age of mother at interview, and a series of binary variables for children's years of birth.

Regressions weighted by sample weight provided by NLSY.

Sample code: reghdfe married Zany_time AGEATINT cyob* [pw=SAMPWEIGHT] if fips>0 & AGEATINT>0 & numkid>0, vce(cluster fips) a(id i.fips#i.year)

reghdfe current_ cesd_7item Zany1_1998 AGEATINT AGEATINT cyob* [pw=SAMPWEIGHT] if fips>0 & year==1992, vce(cluster fips) a(i.fips i.year i.SAMPLE_RACE_78SCRN)

Table 7: Regression on Main Time-Varying Outcomes, CPS-ASEC Data

	(1)	(2)	(3)	(4)	(5)	(6)
	Married	Divorced	Never Married	Out of Labor Force	Employed	Unemployed
Simulated Elig.	0.463***	-0.355***	-0.0935***	0.435***	-0.401***	-0.0334**
	(0.0339)	(0.0222)	(0.0228)	(0.0308)	(0.0314)	(0.0120)
N	157,873	157,873	157,873	157,713	157,713	157,713
Dep. Var. Mean	0.786	0.139	0.066	0.302	0.655	0.043
10 PPT Effect	0.046	-0.036	-0.009	0.043	-0.040	-0.003
10 PPT Effect as %	5.891	-25.593	-14.257	14.396	-6.130	-7.718

Notes: + p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001
Standard errors are in parentheses and are clustered at the state level.
Interpretation: Regressions are run at the mother-by-year level. The main explanatory variable (Simulated eligibility) has a range of 0-to-1 so the estimated β represents a 100 ppt change in eligibility.
10 PPT Effect rescales the main estimate by 1/10 and represents a 10 ppt change in eligibility. 10 PPT Effect as % is the 10 PPT divided by the dependent variable mean.
Model: Model includes age of mother at interview, state-by-year FE, race FE and a series of binary variables for children's years of birth.
Regressions weighted by sample weight provided by CPS-ASEC. Sample restricted to parents born 1957-1965 to be consistent with NLSY sample.
Sample code: reghdfe married sim_all hispanic blackNH otherrace age y19* y20* [pw=asec], absorb(i.state##i.year)vce(cluster fips)

Table 8: Regression on Main Time-Varying Outcomes, CPS-ASEC Data, Fathers

	(1)	(2)	(3)	(4)	(5)	(6)
	Married	Divorced	Never Married	Out of Labor Force	Employed	Unemployed
Simulated Elig.	0.209***	-0.217***	0.00471	-0.00687	0.0119	-0.00500
	(0.0255)	(0.0255)	(0.0125)	(0.0159)	(0.0175)	(0.0185)
N	123,172	123,172	123,172	119,884	119,884	119,884
Dep. Var. Mean	0.946	0.037	0.014	0.041	0.915	0.044
10 PPT Effect	0.021	-0.022	0.000	-0.001	0.001	-0.001
10 PPT Effect as %	2.212	-58.074	3.297	-1.665	0.130	-1.133

Notes: + p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001
Standard errors are in parentheses and are clustered at the state level.
Interpretation: Regressions are run at the mother-by-year level. The main explanatory variable (Simulated eligibility) has a range of 0-to-1 so the estimated β represents a 100 ppt change in eligibility.
10 PPT Effect rescales the main estimate by 1/10 and represents a 10 ppt change in eligibility. 10 PPT Effect as % is the 10 PPT divided by the dependent variable mean.
Model: Model includes age of mother at interview, state-by-year FE, race FE and a series of binary variables for children's years of birth.
Regressions weighted by sample weight provided by CPS-ASEC. Sample restricted to parents born 1957-1965 to be consistent with NLSY sample.
Sample code: reghdfe married sim_all hispanic blackNH otherrace age y19* y20* [pw=asec], absorb(i.state##i.year)vce(cluster fips)

Table 9: Regression on Potential Mechanisms, CPS-ASEC Data

	(1)	(2)	(3)
	Homemaking	Any Health Ins, Mother	Medicaid, Mother
Simulated Elig.	0.463***	0.119***	0.00796
	(0.0777)	(0.0203)	(0.0228)
N	70,578	157,873	157,873
Dep. Var. Mean	0.746	0.827	0.093
10 PPT Effect	0.046	0.012	0.001
10 PPT Effect as %	6.207	1.444	0.852

Notes: + p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001

Standard errors are in parentheses and are clustered at the state level.

Interpretation: Regressions are run at the mother-by-year level. The main explanatory variable (Simulated eligibility) has a range of 0-to-1 so the estimated β represents a 100 ppt change in eligibility.

10 PPT Effect rescales the main estimate by 1/10 and represents a 10 ppt change in eligibility. 10 PPT Effect as % is the 10 PPT divided by the dependent variable mean.

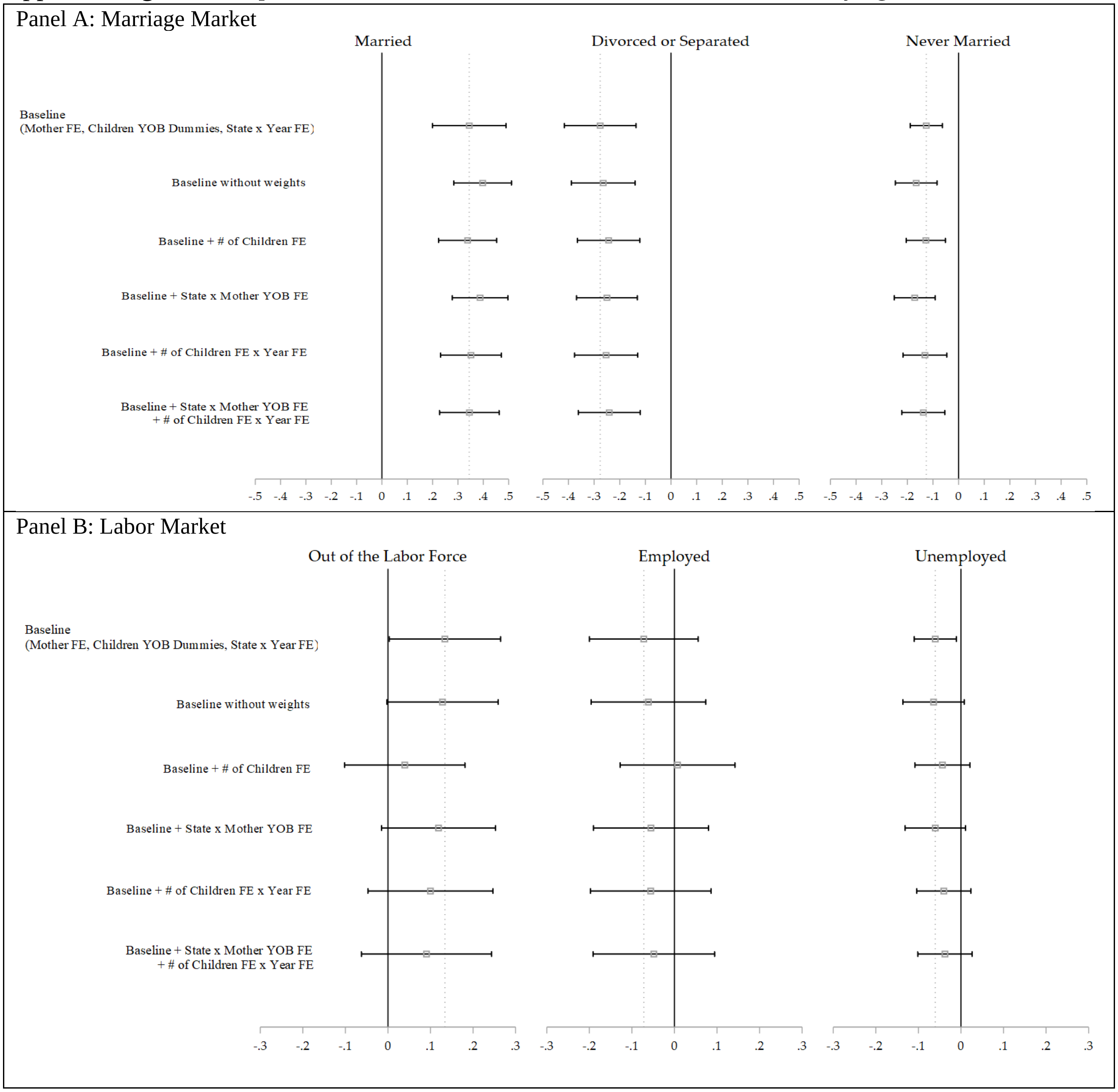
Model: Model includes age of mother at interview, state-by-year FE, race FE and a series of binary variables for children's years of birth.

Regressions weighted by sample weight provided by CPS-ASEC. Sample restricted to parents born 1957-1965 to be consistent with NLSY sample. Columns (1)-(3) conditional on not working or working part time.

Sample code: reghdfe married sim_all hispanic blackNH otherrace age y19* y20* [pw=asec], absorb(i.state##i.year)vce(cluster fips)

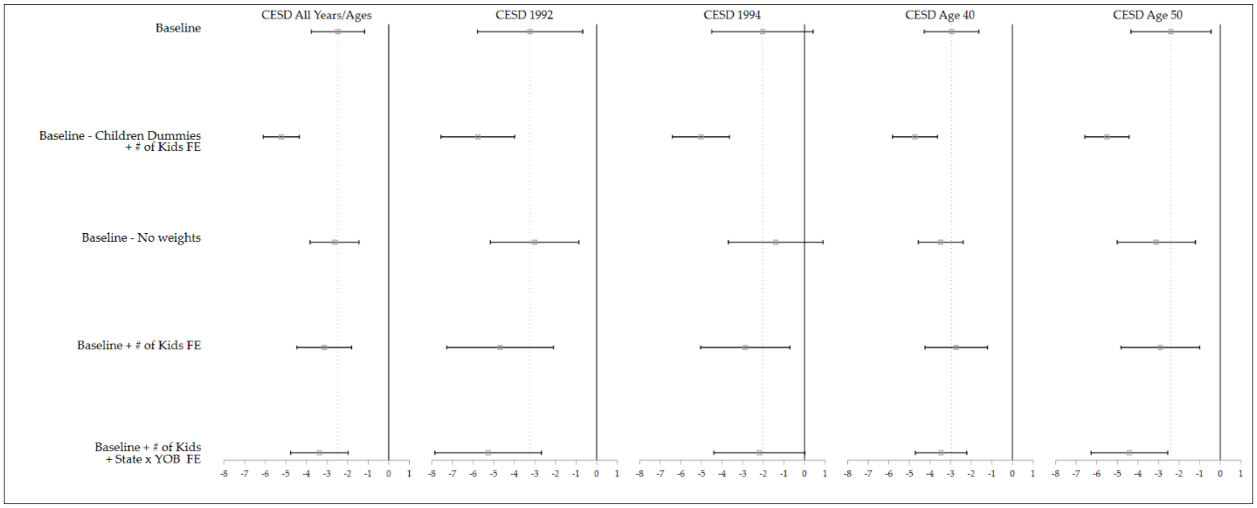
Appendix Figures

Appendix Figure A1: Specification Robustness to Additional Controls; Time-Varying Outcomes



Whiskers are 90% confidence intervals based on standard errors clustered at the state level.
Interpretation: Regressions are run at the mother-by-year level. The main X variable has a range of 0-to-1 so changes represent a 100 ppt change in eligibility affects Y by beta.
Model: The first specification is the main model, which includes mother FE, children year of birth dummy variables, and state-by-year FE. The second specification is the main model but does not include sample weights. The third specification is the main model plus number of children FE. The fourth specification is the main model plus current state-by-mother year of birth FE. The fifth specification is the main model plus number of children-by-year FE. The sixth specification is the main model plus current state-by-mother year of birth FE and number of children-by-year FE.
Regressions weighted by sample weight provided by NLSY unless otherwise noted.

Appendix Figure A2: Specification Robustness to Additional Controls; CES-Depression Scale



Whiskers are 90% confidence intervals based on standard errors clustered at the state level.

Interpretation: Regressions are run at the mother-by-year level. The main X variable has a range of 0-to-1 so changes represent a 100 ppt change in eligibility affects Y by beta.

Model: The first specification is the main model, which includes state FE, year FE, race FE, age of mother at interview, and a series of binary variables for children's years of birth. The second specification is the main model but adds number of children FE and removes binary variables for children's years of birth. The third specification is the main model without sample weights. The fourth specification is the main model plus number of children FE. The fifth specification is the main model plus number of children FE and state-by-year of birth FE.

Regressions weighted by sample weight provided by NLSY unless otherwise noted.

Appendix Tables

Appendix Table A1: Regression on Spouse Characteristics, Conditional on Married

	(1)	(2)	(3)
	Employed	Age	Highest Grade
Simulated Elig.	-0.114**	-2.368***	0.286
	(0.0416)	(0.667)	(0.239)
N	36,532	36,520	36,578
Dep. Var. Mean	0.961	35.835	13.277
10 PPT Effect	-0.011	-0.237	0.029
10 PPT Effect as %	-1.183	-0.661	0.216

Notes: + p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001
Standard errors are in parentheses and are clustered at the state level.
Interpretation: Regressions are run at the mother-by-year level. The main explanatory variable (Simulated eligibility) has a range of 0-to-1 so the estimated β represents a 100 ppt change in eligibility.
10 PPT Effect rescales the main estimate by 1/10 and represents a 10 ppt change in eligibility. 10 PPT Effect as % is the 10 PPT divided by the dependent variable mean.
Model: Model controls include individual FE, state-by-year FE, age of mother at interview, and a series of binary variables for children's years of birth.
Regressions weighted by sample weight provided by NLSY.
Sample code: reghdfe unemployed Zany_time AGEATINT cyob* [pw=SAMPWEIGHT] if fips>0 & AGEATINT>0 & numkid>0, vce(cluster fips) a(id i.fips#i.year)

Appendix Table A2: Regression on Main Time-Varying Outcomes, Addressing Maternal Medicaid Eligibility (Dropping mothers with children age 0)

	(1)	(2)	(3)	(4)	(5)	(6)
	Married	Divorced	Never Married	Out of Labor Force	Employed	Unemployed
Simulated Elig.	0.336**	-0.282**	-0.116**	0.163+	-0.120	-0.0408
	(0.0970)	(0.0924)	(0.0401)	(0.0865)	(0.0809)	(0.0322)
N	55,207	55,207	55,207	43,951	43,951	43,951
Dep. Var. Mean	0.682	0.201	0.109	0.303	0.636	0.062
10 PPT Effect	0.034	-0.028	-0.012	0.016	-0.012	-0.004
10 PPT Effect as %	4.929	-14.056	-10.607	5.396	-1.887	-6.628

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Standard errors are in parentheses and are clustered at the state level.
Interpretation: Regressions are run at the mother-by-year level. The main explanatory variable (Simulated eligibility) has a range of 0-to-1 so the estimated β represents a 100 ppt change in eligibility.
10 PPT Effect rescales the main estimate by 1/10 and represents a 10 ppt change in eligibility. 10 PPT Effect as % is the 10 PPT divided by the dependent variable mean.
Model: Model controls include individual FE, state-by-year FE, age of mother at interview, and a series of binary variables for children's years of birth.
Regressions weighted by sample weight provided by NLSY.
Sample code: reghdfe unemployed Zany_time AGEATINT cyob* [pw=SAMPWEIGHT] if fips>0 & AGEATINT>0 & numkid>0, vce(cluster fips) a(id i.fips#i.year)

Appendix Table A3: Regression on Main Cross-Sectional Outcomes; CES-Depression Scale, Addressing Maternal Medicaid Eligibility (Dropping mothers with children age 0)

	(1)	(2)	(3)	(4)	(5)
	All Years/Ages	Year 1992	Year 1994	Age 40	Age 50
Simulated Elig.	-2.670**	-4.473**	-2.599	-2.890*	-2.444*
	(0.792)	(1.438)	(1.699)	(1.342)	(1.165)
N	13,042	2,945	3,177	3,602	3,338
Dep. Var. Mean	4.29	4.69	4.43	3.65	4.50
10 PPT Effect	-0.27	-0.45	-0.26	-0.29	-0.24
10 PPT Effect as %	-6.22	-9.54	-5.86	-7.93	-5.43

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Standard errors are in parentheses and are clustered at the state level.
Interpretation: Regressions are run at the mother level. The main explanatory variable (Simulated eligibility) has a range of 0-to-1 so β represents a 100 ppt change in eligibility.
10 PPT Effect rescales the main estimate by 1/10 and represents a 10 ppt change in eligibility. 10 PPT Effect as % is the 10 PPT divided by the dependent variable mean.
Model: Model controls include state FE, year FE, race FE, age of mother at interview, and a series of binary variables for children's years of birth.
Regressions weighted by sample weight provided by NLSY.
Sample code: reghdfe cesd_7item_92 Zany1_1992 AGEATINT cyob* [pw=SAMPWEIGHT] if fips>0 & year==1992, vce(cluster fips) a(i.fips i.year i.SAMPLE_RACE_78SCRN)

Appendix Table A4: Regression on Main Time-Varying Outcomes, Addressing Maternal Medicaid Eligibility (Adding prenatal simulated eligibility for mothers with children age 0)

	(1)	(2)	(3)	(4)	(5)	(6)
	Married	Divorced	Never Married	Out of Labor Force	Employed	Unemployed
Simulated Elig.	0.282**	-0.222**	-0.120**	0.110	-0.0367	-0.0716*
	(0.0821)	(0.0815)	(0.0417)	(0.0767)	(0.0782)	(0.0300)
N	59,487	59,487	59,487	48,610	48,610	48,610
Dep. Var. Mean	0.711	0.176	0.105	0.327	0.613	0.060
10 PPT Effect	0.028	-0.022	-0.012	0.011	-0.004	-0.007
10 PPT Effect as %	3.963	-12.618	-11.347	3.363	-0.599	-12.021

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Standard errors are in parentheses and are clustered at the state level.

Interpretation: Regressions are run at the mother-by-year level. The main explanatory variable (Simulated eligibility) has a range of 0-to-1 so the estimated β represents a 100 ppt change in eligibility.

10 PPT Effect rescales the main estimate by 1/10 and represents a 10 ppt change in eligibility. 10 PPT Effect as % is the 10 PPT divided by the dependent variable mean.

Model: Model controls include individual FE, state-by-year FE, age of mother at interview, and a series of binary variables for children's years of birth.

Regressions weighted by sample weight provided by NLSY.

Sample code: reghdfe unemployed Zany_time AGEATINT cyob* prenatal_elig [pw=SAMPWEIGHT] if fips>0 & AGEATINT>0 & numkid>0, vce(cluster fips) a(id i.fips#i.year)

Appendix Table A5: Regression on Main Cross-Sectional Outcomes; CES-Depression Scale, Addressing Maternal Medicaid Eligibility (Adding prenatal simulated eligibility for mothers with children age 0)

	(1)	(2)	(3)	(4)	(5)
	All Years/Ages	Year 1992	Year 1994	Age 40	Age 50
Simulated Elig.	-2.702***	-3.622*	-2.115	-3.238*	-2.831*
	(0.755)	(1.557)	(1.452)	(1.317)	(1.191)
N	13,714	3,299	3,448	3,649	3,339
Dep. Var. Mean	4.279	4.630	4.380	3.659	4.500
10 PPT Effect	-0.270	-0.362	-0.211	-0.324	-0.283
10 PPT Effect as %	-6.316	-7.823	-4.828	-8.851	-6.290

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Standard errors are in parentheses and are clustered at the state level.

Interpretation: Regressions are run at the mother level. The main explanatory variable (Simulated eligibility) has a range of 0-to-1 so β represents a 100 ppt change in eligibility.

10 PPT Effect rescales the main estimate by 1/10 and represents a 10 ppt change in eligibility. 10 PPT Effect as % is the 10 PPT divided by the dependent variable mean.

Model: Model controls include state FE, year FE, race FE, age of mother at interview, and a series of binary variables for children's years of birth.

Regressions weighted by sample weight provided by NLSY.

Sample code: reghdfe cesd_7item_92 Zany1_1992 AGEATINT cyob* prenatal_elig [pw=SAMPWEIGHT] if fips>0 & year==1992, vce(cluster fips) a(i.fips i.year i.SAMPLE_RACE_78SCRN)

Appendix Table A6: Regression on Main Time-Varying Outcomes, Drop Women Under 20 Years Old

	(1)	(2)	(3)	(4)	(5)	(6)
	Married	Divorced	Never Married	Out of Labor Force	Employed	Unemployed
Simulated Elig.	0.348***	-0.282**	-0.123**	0.141+	-0.0825	-0.0566+
	(0.0873)	(0.0843)	(0.0375)	(0.0800)	(0.0773)	(0.0287)
N	60,739	60,739	60,739	49,369	49,369	49,369
Dep. Var. Mean	0.699	0.189	0.104	0.321	0.620	0.059
10 PPT Effect	0.035	-0.028	-0.012	0.014	-0.008	-0.006
10 PPT Effect as %	4.975	-14.922	-11.809	4.385	-1.330	-9.629

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Standard errors are in parentheses and are clustered at the state level.
Interpretation: Regressions are run at the mother-by-year level. The main explanatory variable (Simulated eligibility) has a range of 0-to-1 so the estimated β represents a 100 ppt change in eligibility.
10 PPT Effect rescales the main estimate by 1/10 and represents a 10 ppt change in eligibility. 10 PPT Effect as % is the 10 PPT divided by the dependent variable mean.
Model: Model controls include individual FE, state-by-year FE, age of mother at interview, and a series of binary variables for children's years of birth.
Regressions weighted by sample weight provided by NLSY.
Sample code: reghdfe unemployed Zany_time AGEATINT cyob* prenatal_elig [pw=SAMPWEIGHT] if fips>0 & AGEATINT>0 & numkid>0, vce(cluster fips) a(id i.fips#i.year)

Appendix Table A7: Regression on Main Time-Varying Outcomes, Drop Women with Children Under 5 Years Old

	(1)	(2)	(3)	(4)	(5)	(6)
	Married	Divorced	Never Married	Out of Labor Force	Employed	Unemployed
Simulated Elig.	0.318*	-0.400**	-0.00168	0.261*	-0.275+	0.0226
	(0.138)	(0.145)	(0.0617)	(0.119)	(0.144)	(0.0721)
N	26,818	26,818	26,818	16,570	16,570	16,570
Dep. Var. Mean	0.640	0.256	0.092	0.217	0.721	0.061
10 PPT Effect	0.032	-0.040	-0.000	0.026	-0.027	0.002
10 PPT Effect as %	4.964	-15.592	-0.183	12.029	-3.810	3.681

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Standard errors are in parentheses and are clustered at the state level.
Interpretation: Regressions are run at the mother-by-year level. The main explanatory variable (Simulated eligibility) has a range of 0-to-1 so the estimated β represents a 100 ppt change in eligibility.
10 PPT Effect rescales the main estimate by 1/10 and represents a 10 ppt change in eligibility. 10 PPT Effect as % is the 10 PPT divided by the dependent variable mean.
Model: Model controls include individual FE, state-by-year FE, age of mother at interview, and a series of binary variables for children's years of birth.
Regressions weighted by sample weight provided by NLSY.
Sample code: reghdfe unemployed Zany_time AGEATINT cyob* prenatal_elig [pw=SAMPWEIGHT] if fips>0 & AGEATINT>0 & numkid>0, vce(cluster fips) a(id i.fips#i.year)

Appendix Table A8: Regression on Main Time-Varying Outcomes, Drop States with Few Observations (Below 20 on average)

	(1)	(2)	(3)	(4)	(5)	(6)
	Married	Divorced	Never Married	Out of Labor Force	Employed	Unemployed
Simulated Elig.	0.328***	-0.263**	-0.127**	0.142+	-0.0766	-0.0634*
	(0.0889)	(0.0863)	(0.0379)	(0.0809)	(0.0793)	(0.0297)
N	58,578	58,578	58,578	47,856	47,856	47,856
Dep. Var. Mean	0.695	0.186	0.111	0.326	0.612	0.062
10 PPT Effect	0.033	-0.026	-0.013	0.014	-0.008	-0.006
10 PPT Effect as %	4.724	-14.189	-11.460	4.357	-1.251	-10.284

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Standard errors are in parentheses and are clustered at the state level.
Interpretation: Regressions are run at the mother-by-year level. The main explanatory variable (Simulated eligibility) has a range of 0-to-1 so the estimated β represents a 100 ppt change in eligibility.
10 PPT Effect rescales the main estimate by 1/10 and represents a 10 ppt change in eligibility. 10 PPT Effect as % is the 10 PPT divided by the dependent variable mean.
Model: Model controls include individual FE, state-by-year FE, age of mother at interview, and a series of binary variables for children's years of birth.
Regressions weighted by sample weight provided by NLSY.
Sample code: reghdfe unemployed Zany_time AGEATINT cyob* prenatal_elig [pw=SAMPWEIGHT] if fips>0 & AGEATINT>0 & numkid>0, vce(cluster fips) a(id i.fips#i.year)

Appendix Table A9: Regression on Main Cross-Sectional Outcomes; CES-Depression Scale, Drop States with Few Observations (Below 20 on average)

	(1)	(2)	(3)	(4)	(5)
	All Years/Ages	Year 1992	Year 1994	Age 40	Age 50
Simulated Elig.	-2.743**	-2.927+	-1.663	-3.547*	-3.346**
	(0.784)	(1.597)	(1.527)	(1.394)	(1.077)
N	12,926	3,108	3,264	3,438	3,139
Dep. Var. Mean	4.26	4.62	4.38	3.61	4.48
10 PPT Effect	-0.27	-0.29	-0.17	-0.35	-0.33
10 PPT Effect as %	-6.44	-6.34	-3.80	-9.83	-7.47

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Standard errors are in parentheses and are clustered at the state level.
Interpretation: Regressions are run at the mother level. The main explanatory variable (Simulated eligibility) has a range of 0-to-1 so β represents a 100 ppt change in eligibility.
10 PPT Effect rescales the main estimate by 1/10 and represents a 10 ppt change in eligibility. 10 PPT Effect as % is the 10 PPT divided by the dependent variable mean.
Model: Model controls include state FE, year FE, race FE, age of mother at interview, and a series of binary variables for children's years of birth.
Regressions weighted by sample weight provided by NLSY.
Sample code: reghdfe cesd_7item_92 Zany1_1992 AGEATINT cyob* prenatal_elig [pw=SAMPWEIGHT] if fips>0 & year==1992, vce(cluster fips) a(i.fips i.year i.SAMPLE_RACE_78SCRN)

Appendix Table A10: Regression on Main Time-Varying Outcomes, Race-Specific eligibility

	(1)	(2)	(3)	(4)	(5)	(6)
	Married	Divorced	Never Married	Out of Labor Force	Employed	Unemployed
Simulated Elig.	0.300***	-0.160*	-0.193***	0.120^{+}	-0.0424	-0.0765*
	(0.0754)	(0.0712)	(0.0378)	(0.0646)	(0.0624)	(0.0308)
N	62,545	62,545	62,545	51,174	51,174	51,174
Dep. Var. Mean	0.694	0.187	0.111	0.326	0.612	0.062
10 PPT Effect	0.030	-0.016	-0.019	0.012	-0.004	-0.008
10 PPT Effect as %	4.326	-8.570	-17.392	3.689	-0.693	-12.411

Notes: + p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001
Standard errors are in parentheses and are clustered at the state level.
Interpretation: Regressions are run at the mother-by-year level. The main explanatory variable (Simulated eligibility) has a range of 0-to-1 so the estimated β represents a 100 ppt change in eligibility.
10 PPT Effect rescales the main estimate by 1/10 and represents a 10 ppt change in eligibility. 10 PPT Effect as % is the 10 PPT divided by the dependent variable mean.
Model: Model controls include individual FE, state-by-year FE, age of mother at interview, and a series of binary variables for children's years of birth.
Regressions weighted by sample weight provided by NLSY.
Sample code: reghdfe unemployed Zany_time_race AGEATINT cyob* prenatal_elig [pw=SAMPWEIGHT] if fips>0 & AGEATINT>0 & numkid>0, vce(cluster fips) a(id i.fips#i.year)

Appendix Table A11: Regression on Main Cross-Sectional Outcomes; CES-Depression Scale, Race-Specific eligibility

	(1)	(2)	(3)	(4)	(5)
	All Years/Ages	Year 1992	Year 1994	Age 40	Age 50
Simulated Elig.	-2.174**	-2.640*	-1.284	-3.108*	-2.139^{+}
	(0.701)	(1.291)	(1.200)	(1.250)	(1.162)
N	13,714	3,299	3,448	3,649	3,339
Dep. Var. Mean	4.279	4.630	4.380	3.659	4.500
10 PPT Effect	-0.217	-0.264	-0.128	-0.311	-0.214
10 PPT Effect as %	-5.080	-5.703	-2.932	-8.494	-4.752

Notes: + p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001
Standard errors are in parentheses and are clustered at the state level.
Interpretation: Regressions are run at the mother level. The main explanatory variable (Simulated eligibility) has a range of 0-to-1 so β represents a 100 ppt change in eligibility.
10 PPT Effect rescales the main estimate by 1/10 and represents a 10 ppt change in eligibility. 10 PPT Effect as % is the 10 PPT divided by the dependent variable mean.
Model: Model controls include state FE, year FE, race FE, age of mother at interview, and a series of binary variables for children's years of birth.
Regressions weighted by sample weight provided by NLSY.
Sample code: reghdfe cesd_7item_92 Zany1_1992_race AGEATINT cyob* prenatal_elig [pw=SAMPWEIGHT] if fips>0 & year==1992, vce(cluster fips) a(i.fips i.year i.SAMPLE_RACE_78SCRN)

Appendix Table A12: Regression on Main Time-Varying Outcomes, Random Inference P-values

	(1)	(2)	(3)	(4)	(5)	(6)
	Married	Divorced	Never Married	Out of Labor Force	Employed	Unemployed
Simulated Elig.	0.345	-0.276	-0.126	0.134	-0.0720	-0.0603
Original P-value	<0.001	0.002	0.001	0.093	0.350	0.047
Randomized Inference P-value	<0.003	<0.003	0.107	0.050	0.230	0.020
N	62,545	62,545	62,545	51,174	51,174	51,174
Dep. Var. Mean	0.694	0.187	0.111	0.326	0.612	0.062

Notes: * p < 0.05, ** p < 0.01, *** p < 0.001.
Original p-values based on standard errors clustered at state level. Random inference p-values based on 300 iterations.
Interpretation: Regressions are run at the mother-by-year level. The main explanatory variable (Simulated eligibility) has a range of 0-to-1 so the estimated β represents a 100 ppt change in eligibility.
Model: Model includes Age, Current state-by-year of birth FE, Current number of children-by-current year FE, and Individual FE.
Regressions weighted by sample weight provided by NLSY.
Sample code: reghdfe yvar Zany_time AGEATINT [aw=SAMPWEIGHT] if fips>0 & AGEATINT>0 & numkid>0, vce(cluster fips) a(id i.fips##i.yob i.numkid##i.year)

Appendix Table A13: Regression on Main Cross-Sectional Outcomes; CES-Depression Scale, Random Inference P-values

	(1)	(2)	(3)	(4)
	Year 1992	Year 1994	Age 40	Age 50
Simulated Elig.	-3.232	-2.040	-3.126	-2.450
Original P-value	0.040	0.171	0.023	0.042
Randomized Inference P-value	<0.003	0.027	<0.003	0.027
N	3,299	3,448	3,649	3,339
Dep. Var. Mean	4.63	4.38	3.66	4.50

Notes: * p < 0.05, ** p < 0.01, *** p < 0.001.
Original p-values based on standard errors clustered at state level. Random inference p-values based on 300 iterations.
Interpretation: Regressions are run at the mother level. The main explanatory variable (Simulated eligibility) has a range of 0-to-1 so β represents a 100 ppt change in eligibility.
Model: Model includes Age, Current state-by-year of birth FE, Current number of children FE and Race FE.
Regressions weighted by sample weight provided by NLSY.
Sample code: reghdfe cesd_7item_40 Zany1_40 i.year AGEATINT [aw=SAMPWEIGHT] if firstyearabove40==1 & fips>0, vce(cluster fips) a(i.fips##i.yob i.numkid i.SAMPLE_RACE_78SCRN)

Appendix Table A14: Main Results by Race/Ethnicity; Time-Varying Outcomes, Race-Specific eligibility

Panel A: Black or Hispanic Women

	(1)	(2)	(3)	(4)	(5)	(6)
	Married	Divorced	Never Married	Out of Labor Force	Employed	Unemployed
Simulated Elig.	0.219***	-0.132+	-0.149**	0.0994	-0.0295	-0.0646
	(0.0612)	(0.0738)	(0.0507)	(0.0742)	(0.0803)	(0.0562)
N	30,718	30,718	30,718	25,227	25,227	25,227
Dep. Var. Mean	0.436	0.227	0.324	0.332	0.556	0.111
10 PPT Effect	0.022	-0.013	-0.015	0.010	-0.003	-0.006
10 PPT Effect as %	5.026	-5.834	-4.598	2.992	-0.531	-5.829

Panel B: Not Black and Not Hispanic Women

	(1)	(2)	(3)	(4)	(5)	(6)
	Married	Divorced	Never Married	Out of Labor Force	Employed	Unemployed
Simulated Elig.	0.354*	-0.359**	-0.0865+	0.172+	-0.128	-0.0448
	(0.141)	(0.122)	(0.0464)	(0.0909)	(0.0833)	(0.0366)
N	31,664	31,664	31,664	25,819	25,819	25,819
Dep. Var. Mean	0.772	0.175	0.047	0.324	0.630	0.046
10 PPT Effect	0.035	-0.036	-0.009	0.017	-0.013	-0.004
10 PPT Effect as %	4.591	-20.554	-18.356	5.291	-2.026	-9.777

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Standard errors are in parentheses and are clustered at the state level.

Interpretation: Regressions are run at the mother-by-year level. The main explanatory variable (Simulated eligibility) has a range of 0-to-1 so the estimated β represents a 100 ppt change in eligibility.

10 PPT Effect rescales the main estimate by 1/10 and represents a 10 ppt change in eligibility. 10 PPT Effect as % is the 10 PPT divided by the dependent variable mean.

Model: Model controls include individual FE, state-by-year FE, age of mother at interview, and a series of binary variables for children's years of birth.

Regressions weighted by sample weight provided by NLSY.

Sample code: reghdfe unemployed Zany_time AGEATINT cyob* [pw=SAMPWEIGHT] if fips>0 & AGEATINT>0 & numkid>0, vce(cluster fips) a(id i.fips#i.year)

Appendix Table A15: Main Results by Race/Ethnicity, Cross-Sectional Outcomes; CES-Depression Scale; Race-Specific eligibility

Panel A: Black or Hispanic Women

	(1)	(2)	(3)	(4)	(5)
	All Years/Ages	Year 1992	Year 1994	Age 40	Age 50
Simulated Elig.	-2.331*	-2.041	-0.529	-3.679**	-3.630+
	(0.919)	(1.774)	(1.541)	(1.120)	(1.899)
N	7,071	1,712	1,769	1,865	1,724
Dep. Var. Mean	4.673	5.187	4.923	4.063	4.588
10 PPT Effect	-0.233	-0.204	-0.053	-0.368	-0.363
10 PPT Effect as %	-4.987	-3.935	-0.108	-9.056	-7.913

Panel B: Not Black and Not Hispanic Women

	(1)	(2)	(3)	(4)	(5)
	All Years/Ages	Year 1992	Year 1994	Age 40	Age 50
Simulated Elig.	-2.670*	-4.051+	-3.552+	-3.100+	-2.153
	(1.043)	(2.289)	(2.050)	(1.743)	(1.801)
N	6,642	1,578	1,670	1,777	1,608
Dep. Var. Mean	4.171	4.457	4.235	3.547	4.480
10 PPT Effect	-0.267	-0.405	-0.355	-0.310	-0.215
10 PPT Effect as %	-6.401	-9.089	-0.839	-8.738	-4.806

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Standard errors are in parentheses and are clustered at the state level.

Interpretation: Regressions are run at the mother level. The main explanatory variable (Simulated eligibility) has a range of 0-to-1 so β represents a 100 ppt change in eligibility.

10 PPT Effect rescales the main estimate by 1/10 and represents a 10 ppt change in eligibility. 10 PPT Effect as % is the 10 PPT divided by the dependent variable mean.

Model: Model controls include state FE, year FE, race FE, age of mother at interview, and a series of binary variables for children's years of birth.

Regressions weighted by sample weight provided by NLSY.

Sample code: reghdfe cesd_7item_92 Zany1_1992 AGEATINT cyob* [pw=SAMPWEIGHT] if fips>0 & year==1992, vce(cluster fips) a(i.fips i.year i.SAMPLE_RACE_78SCRN)

Appendix Table A16: Regression on Main Time-Varying Outcomes, No Mother Fixed Effects

	(1)	(2)	(3)	(4)	(5)	(6)
	Married	Divorced	Never Married	Out of Labor Force	Employed	Unemployed
Simulated Elig.	0.342***	-0.279***	-0.0736+	0.129*	-0.0765	-0.0533*
	(0.0857)	(0.0642)	(0.0420)	(0.0613)	(0.0589)	(0.0199)
N	62,676	62,676	62,676	51,402	51,402	51,402
Dep. Var. Mean	0.694	0.187	0.111	0.326	0.612	0.062
10 PPT Effect	0.034	-0.028	-0.007	0.013	-0.008	-0.005
10 PPT Effect as %	4.920	-14.943	-6.632	3.956	-1.250	-8.664

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Standard errors are in parentheses and are clustered at the state level.

Interpretation: Regressions are run at the mother-by-year level. The main explanatory variable (Simulated eligibility) has a range of 0-to-1 so the estimated β represents a 100 ppt change in eligibility.

10 PPT Effect rescales the main estimate by 1/10 and represents a 10 ppt change in eligibility. 10 PPT Effect as % is the 10 PPT divided by the dependent variable mean.

Model: Model controls include individual FE, state-by-year FE, age of mother at interview, and a series of binary variables for children's years of birth.

Regressions weighted by sample weight provided by NLSY.

Sample code: `reghdfe unemployed Zany_time AGEATINT cyob* [pw=SAMPWEIGHT] if fips>0 & AGEATINT>0 & numkid>0, vce(cluster fips) a(i.fips#i.year)`

Appendix Table A17: Regression on Smoking Behaviors, CPS Tobacco Use Supplement

	(1)	(2)
	Current Smoker	Daily Smoker
Simulated Elig.	-0.451***	-0.419***
	(0.0820)	(0.0625)
N	22,992	22,992
Dep. Var. Mean	0.227	0.188
10 PPT Effect	-0.045	-0.042
10 PPT Effect as %	-19.843	-22.355

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Standard errors are in parentheses and are clustered at the state level.

Interpretation: Regressions are run at the mother-by-year level. The main explanatory variable (Simulated eligibility) has a range of 0-to-1 so the estimated β represents a 100 ppt change in eligibility.

10 PPT Effect rescales the main estimate by 1/10 and represents a 10 ppt change in eligibility. 10 PPT Effect as % is the 10 PPT divided by the dependent variable mean.

Model: Model includes age of mother at interview, state-by-year FE, current number of children FE, race FE and a series of binary variables for children's years of birth.

Regressions weighted by sample weight provided by CPS. Sample restricted to parents born 1957-1965 to be consistent with NLSY sample.

Sample code: `reghdfe married sim_all hispanic blackNH otherrace age y19* y20* [pw=asec], absorb(i.state##i.year i.nchild)vce(cluster fips)`

Appendix Table A18: First Stage and Scaling Main Estimates

Panel A: First Stage Estimates

	(1)	(2)	(3)
	NSLY, Mother-Level	CPS-ASEC, Mother-Level NLSY-Cohorts	CPS-ASEC, Child-Level NLSY-Cohorts
Simulated Elig.	0.459***	0.158***	0.705***
	(0.0621)	(0.0269)	(0.0817)
N	62,679	238,838	459,501
Dep. Var. Mean	0.172	0.264	0.297

Panel B: Main Estimates Scaled by NLSY First Stage (column (1) in Panel A)

	(1)	(2)	(3)	(4)	(5)	(6)
	10pp Estimate	Scaled Estimate	Outcome Mean	Scaled % Effect	Growth in outcome variable in 10 years (1980-1990)	How much change in Z affects growth in outcome (10 yrs)
Married	0.034	0.074	0.694	10.7	0.427	8.75%
Divorced	-0.028	-0.061	0.187	-32.6	0.163	-18.92%
Never Married	-0.013	-0.028	0.111	-25.5	-0.596	2.40%
Out of Labor Force	0.013	0.028	0.326	8.7	-0.034	-41.74%
Employed	-0.007	-0.015	0.612	-2.5	0.163	-4.73%
Unemployed	-0.006	-0.013	0.062	-21.1	-0.076	8.71%
CES-D, All Years/Ages	-0.25	-0.545	4.28	-12.7	0.260	18.21%

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$; Standard errors are in parentheses and are clustered at the state level.

Interpretation: Regressions are run at the mother-by-year level. The main explanatory variable (Simulated eligibility) has a range of 0-to-1 so the estimated β represents a 100 ppt change in eligibility. 10 PPT Effect rescales the main estimate by 1/10 and represents a 10 ppt change in eligibility. 10 PPT Effect as % is the 10 PPT divided by the dependent variable mean.

Model: Column (1): Model controls include individual FE, state-by-year FE, age of mother at interview, and series of binary variables for children's years of birth. Column (2): Model includes age of mother at interview, state-by-year FE, current number of children FE, race FE and series of binary variables for children's years of birth. Column (3): Model includes individual FE, age of mother at interview, state-by-year FE, current number of children FE, race FE and a series of binary variables for children's years of birth.

Regressions weighted by sample weight provided by NLSY or CPS-ASEC.

Sample code: Column (1): reghdfe totel Zany_time AGEATINT cyob* [pw=SAMPWEIGHT] if fips>0 & AGEATINT>0 & numkid>0, vce(cluster fips) a(id i.fips#i.year)

Column (2): reghdfe totel sim_all hispanic blackNH otherrace age y19* y20* [pw=asec], absorb(i.state##i.year i.nchild) vce(cluster fips)

Column (3): reghdfe totel sim_all hispanic blackNH otherrace age y19* y20* [pw=asec], absorb(i.state##i.year i.nchild i.id) vce(cluster fips)

Online Appendix B: Additional Heterogeneity Analyses

This section adds to Section 5.4 and includes stratified results by childhood socioeconomic status of mothers and number of children. Low SES is defined here as the mother's childhood household reported being in poverty at least once before 1985. We might expect larger effects for women with lower childhood SES if the mother's parents provide support, and because those with lower SES as children are more likely to be eligible for Medicaid as adults. For time-varying outcomes in Appendix Table B1, results differ slightly in terms of statistical significance, but overall are qualitatively similar for marital outcomes. However, the effects as a percent of the group-specific mean is larger for lower SES women, because their mean is lower. For labor force outcomes, simulated eligibility is associated with substantially less employment among high SES women, while it is associated with much lower rates of unemployment among low SES women.

In Appendix Table B2, we report CES-D results by mothers' childhood SES. This table has several interesting results. First, the low SES sample has a substantially higher mean of depressive symptoms as measured by the CES-D. Second, the effect is larger for lower SES women across ages and years. For instance, the pooled ages/years effect in column (1) is twice as big for lower SES women. This suggests that our results for low SES reflect the higher likelihood of this group of women receiving Medicaid coverage for their children, a test of the mechanism of our effect.

Appendix Table B3 and Appendix Table B4 present results separately by number of children a mother has and provides qualitatively similar results. However, the effects on labor market in Appendix Table B3 are larger for mothers with more children. For instance, simulated eligibility has economically meaningful effects on labor market outcomes for women with three or more children, but small and statistically insignificant effects for women with few children. However, in Appendix Table B4, the effects on mental health are larger for mothers with few children. For instance, when pooling all ages/years, the effect is twice as large for women with fewer than 3 children; a 10 ppt increase in simulated eligibility is associated with an 8.3% reduction in CES-D score for women with few children, but only a 4.0% decrease for women with more children.

Appendix Table B1: Main Results by Childhood SES, Time-Varying Outcomes

Panel A: High SES						
	(1)	(2)	(3)	(4)	(5)	(6)
	Married	Divorced	Never Married	Out of Labor Force	Employed	Unemployed
Simulated Elig.	0.312**	-0.310**	-0.0635	0.155+	-0.129	-0.0264
	(0.116)	(0.109)	(0.0403)	(0.0909)	(0.0943)	(0.0488)
N	26,840	26,840	26,840	21,191	21,191	21,191
Dep. Var. Mean	0.809	0.143	0.041	0.285	0.677	0.037
10 PPT Effect	0.031	-0.031	-0.006	0.016	-0.013	-0.003
10 PPT Effect as %	3.862	-21.695	-15.384	5.450	-1.906	-7.051

Panel B: Low SES						
	(1)	(2)	(3)	(4)	(5)	(6)
	Married	Divorced	Never Married	Out of Labor Force	Employed	Unemployed
Simulated Elig.	0.393***	-0.262*	-0.171**	0.0870	0.0321	-0.115*
	(0.0884)	(0.102)	(0.0609)	(0.113)	(0.116)	(0.0483)
N	35,581	35,581	35,581	29,885	29,885	29,885
Dep. Var. Mean	0.555	0.240	0.197	0.375	0.535	0.090
10 PPT Effect	0.039	-0.026	-0.017	0.009	0.003	-0.012
10 PPT Effect as %	7.085	-10.906	-8.662	2.322	0.600	-12.837

Notes: + p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001
Standard errors are in parentheses and are clustered at the state level.
Interpretation: Regressions are run at the mother-by-year level. The main explanatory variable (Simulated Eligibility) has a range of 0-to-1 so the estimated β represents a 100 ppt change in eligibility.
10 PPT Effect rescales the main estimate by 1/10 and represents a 10 ppt change in eligibility. 10 PPT Effect as % is the 10 PPT divided by the dependent variable mean.
Model: Model controls include individual FE, state-by-year FE, age of mother at interview, and a series of binary variables for children's years of birth.
Regressions weighted by sample weight provided by NLSY.
Sample code: reghdfe unemployed Zany_time AGEATINT cyob* [pw=SAMPWEIGHT] if fips>0 & AGEATINT>0 & numkid>0, vce(cluster fips) a(id i.fips#i.year)

Appendix Table B2: Main Results by Childhood SES, Cross-Sectional Outcomes; CES-Depression Scale

Panel A: High SES					
	(1)	(2)	(3)	(4)	(5)
	All Years/Ages	Year 1992	Year 1994	Age 40	Age 50
Simulated Elig.	-1.520	-2.040	0.656	-2.072	-1.799
	(1.386)	(1.600)	(2.306)	(1.974)	(2.319)
N	6,302	1,488	1,575	1,697	1,538
Dep. Var. Mean	3.735	4.055	3.814	3.127	3.987
10 PPT Effect	-0.015	-0.204	0.066	-0.021	-0.018
10 PPT Effect as %	-4.070	-5.032	1.720	-6.627	-4.512
Panel B: Low SES					
	(1)	(2)	(3)	(4)	(5)
	All Years/Ages	Year 1992	Year 1994	Age 40	Age 50
Simulated Elig.	-3.809***	-4.736+	-3.755*	-5.270***	-3.431+
	(0.870)	(2.544)	(1.411)	(1.411)	(1.724)
N	7,412	1,803	1,866	1,947	1,796
Dep. Var. Mean	4.973	5.335	5.106	4.347	5.150
10 PPT Effect	-0.381	-0.474	-0.376	-0.053	-0.034
10 PPT Effect as %	-7.658	-8.876	-7.354	-12.124	-6.662

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Standard errors are in parentheses and are clustered at the state level.

Interpretation: Regressions are run at the mother level. The main explanatory variable (Simulated Eligibility) has a range of 0-to-1 so β represents a 100 ppt change in eligibility.

10 PPT Effect rescales the main estimate by 1/10 and represents a 10 ppt change in eligibility. 10 PPT Effect as % is the 10 PPT divided by the dependent variable mean.

Model: Model controls include state FE, year FE, race FE, age of mother at interview, and a series of binary variables for children's years of birth.

Regressions weighted by sample weight provided by NLSY.

Sample code: reghdfe cesd_7item_92 Zany1_1992 AGEATINT cyob* [pw=SAMPWEIGHT] if fips>0 & year==1992, vce(cluster fips) a(i.fips i.year i.SAMPLE_RACE_78SCRN)

Appendix Table B3: Main Results by Number of Children, Time-Varying Outcomes

Panel A: Less than 3 children

	(1)	(2)	(3)	(4)	(5)	(6)
	Married	Divorced	Never Married	Out of Labor Force	Employed	Unemployed
Simulated Elig.	0.137	-0.110	-0.128^{*}	-0.0186	0.0362	-0.0158
	(0.102)	(0.0977)	(0.0600)	(0.117)	(0.122)	(0.0317)
N	38,477	38,477	38,477	32,069	32,069	32,069
Dep. Var. Mean	0.702	0.181	0.110	0.291	0.652	0.056
10 PPT Effect	0.014	-0.011	-0.013	-0.002	0.004	-0.002
10 PPT Effect as %	1.953	-6.061	-11.638	-0.640	0.555	-2.802

Panel B: 3 or more Children

	(1)	(2)	(3)	(4)	(5)	(6)
	Married	Divorced	Never Married	Out of Labor Force	Employed	Unemployed
Simulated Elig.	0.478^{***}	-0.370^{**}	-0.0915^{*}	0.562^{***}	-0.417^{**}	-0.145^{*}
	(0.125)	(0.122)	(0.0424)	(0.150)	(0.141)	(0.0591)
N	22,486	22,486	22,486	17,482	17,482	17,482
Dep. Var. Mean	0.674	0.202	0.114	0.401	0.524	0.075
10 PPT Effect	0.048	-0.037	-0.009	0.056	-0.042	-0.015
10 PPT Effect as %	7.083	-18.357	-8.033	14.032	-7.950	-19.438

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Standard errors are in parentheses and are clustered at the state level.

Interpretation: Regressions are run at the mother-by-year level. The main explanatory variable (Simulated Eligibility) has a range of 0-to-1 so the estimated β represents a 100 ppt change in eligibility.

10 PPT Effect rescales the main estimate by 1/10 and represents a 10 ppt change in eligibility. 10 PPT Effect as % is the 10 PPT divided by the dependent variable mean.

Model: Model controls include individual FE, state-by-year FE, age of mother at interview, and a series of binary variables for children's years of birth.

Regressions weighted by sample weight provided by NLSY.

Sample code: reghdfe unemployed Zany_time AGEATINT cyob* [pw=SAMPWEIGHT] if fips>0 & AGEATINT>0 & numkid>0, vce(cluster fips) a(id i.fips#i.year)

Appendix Table B4: Main Results by Number of Children, Cross-Sectional Outcomes; CES-Depression Scale

Panel A: Less than 3 children					
	(1)	(2)	(3)	(4)	(5)
	All Years/Ages	Year 1992	Year 1994	Age 40	Age 50
Simulated Elig.	-3.472*	-5.527*	-2.191	-3.344	-3.716
	(1.447)	(2.129)	(2.092)	(2.054)	(2.266)
N	8,712	2,263	2,262	2,203	1,990
Dep. Var. Mean	4.190	4.492	4.233	3.602	4.426
10 PPT Effect	-0.347	-0.553	-0.219	-0.334	-0.372
10 PPT Effect as %	-8.287	-12.304	-5.175	-9.285	-8.395
Panel B: 3 or more Children					
	(1)	(2)	(3)	(4)	(5)
	All Years/Ages	Year 1992	Year 1994	Age 40	Age 50
Simulated Elig.	-1.856	2.903	-8.786+	-2.443	-2.606
	(1.624)	(5.601)	(4.856)	(2.357)	(2.589)
N	5,001	1,031	1,181	1,443	1,347
Dep. Var. Mean	4.462	4.985	4.731	3.773	4.616
10 PPT Effect	-0.186	0.290	-0.879	-0.244	-0.261
10 PPT Effect as %	-4.159	5.824	-18.570	-6.474	-5.646

Notes: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Standard errors are in parentheses and are clustered at the state level.
Interpretation: Regressions are run at the mother level. The main explanatory variable (Simulated eligibility) has a range of 0-to-1 so β represents a 100 ppt change in eligibility.
10 PPT Effect rescales the main estimate by 1/10 and represents a 10 ppt change in eligibility. 10 PPT Effect as % is the 10 PPT divided by the dependent variable mean.
Model: Model controls include state FE, year FE, race FE, age of mother at interview, and a series of binary variables for children's years of birth.
Regressions weighted by sample weight provided by NLSY.
Sample code: reghdfe cesd_7item_92 Zany1_1992 AGEATINT cyob* [pw=SAMPWEIGHT] if fips>0 & year==1992, vce(cluster fips) a(i.fips i.year i.SAMPLE_RACE_78SCRN)

Online Appendix C: Full list of CES-D- Questions

In order to measure changes in mental health we use the Center for Epidemiologic Studies Depression (CES-D) 7-item Scale. The documentation from the NLSY website states that "*The CES-D is a self-report scale that measures the current prevalence of depression symptoms. Respondents rate a series of statements regarding how they felt during the week prior to the interview.*" The questions asked were the following:

1. I did not feel like eating; my appetite was poor
2. I felt that I could not shake off the blues even with the help from my family and friends
3. I had trouble keeping my mind on what I was doing
4. I felt depressed
5. I felt that everything I did was an effort
6. My sleep was restless
7. I felt lonely

Respondents would answer each question and score it from 0 to 3, the score represents the following:

0 point: Rarely or none of the time (< 1 day)

1 point: Some or a little of the time (1-2 days)

2 points: Occasionally or a moderate amount of the time (3-4 days)

3 points: Most or all of the time (5-7 days).

Therefore, the lowest score is 0 and highest is 21.