Online Appendix

A Child Education

A.I Sample Frames

We form our sample frame for child education policies prior to age 5 (Pre-K) using the survey article by Duncan and Magnuson (2013). We restrict our baseline sample to policies that employ experimental or quasi-experimental policy variation.[1] We require that all policies in our sample have estimated impacts on long-run earnings. We also include policies in our extended sample when we are able to observe impacts on college outcomes. (The inclusion of college outcomes allows us to forecast the impacts on children, but we do not include these policies in our baseline sample because this projection approach may miss impacts on income that do not operate through college enrollment.) We do not require that research has evaluated the impacts of these policies on crime. Where evidence on crime is available we add those estimates to alternate specifications and discuss how they impact our results. (They tend to simply reinforce our finding of high returns to investment in low-income children.)

For K-12 schooling, we form our analysis sample using the survey article by Jackson (2018). We once again require that all policies in our sample have estimated impacts on long-run earnings. We do not require the measurement of any other specific outcomes. While there are many papers looking at the impact on test scores, to the best of our knowledge there is only one paper looking at the long-run effects of school spending on earnings and only one looking at the impact on college attendance. Jackson et al. (2016) exploits variation from school finance equalizations on long-run earnings and Hyman (2017) uses variation from local house prices to study the impact of school finances (funded by property taxes) on college enrollment.

In many instances, existing work has conducted benefit-cost analyses of these policies, and in those instances our primary aim is to translate those estimates into the MVPF framework by isolating the incidence of the policy on beneficiaries and the government budget. We also attempt to harmonize the set of assumptions and outcomes used across studies. For example, we harmonize discount rate and tax rate assumptions.

We begin with a discussion of our MVPF estimates for preschool policies, followed by K-12 schooling policies.

A.II Preschool Results

A.II.1 Perry Preschool

The Perry Preschool Project was an intensive preschool program for low-income at-risk children for 3- to 5-year-olds in Ypsilanti, Michigan. Between 1962- and 1967, it was evaluated using a randomized control trial that allocated 123 children into a treatment and control group. While initial results suggested improvements in test scores, these effects faded after 10 years. However, later analysis showed improvements in earnings and other young adult outcomes.

We form our MVPF estimates from the Perry Pre-School experiment from the 1960s using the results from the cost-benefit analysis provided in Heckman et al. (2010). In particular, we use the estimates of the impacts on earnings and other outcomes provided in Heckman et al. (2010). But, we translate these estimates into their implied MVPF using our standardized projection method (we discuss how the MVPF would be similar if we use the projections provided in Heckman et al. (2010).) In addition, we separate the incidence of these outcomes on the government to construct costs.[2]

Heckman et al. (2010) construct estimates of the impact of the program on the cost and benefits of crime reduction. In particular, they estimate the cost of the crime/criminal justice system on government expenditures, and the willingness to pay by victims (and their families) to prevent crime. Both of these measures are not straightforward to value, and are often excluded in other papers. We exclude these components in our baseline

[1] It is worth mentioning that this leads us to exclude a couple of prominent papers in our baseline analysis, including Deming (2009) who uses a sibling design to estimate the impact of Head Start and also the Chicago Child-Parent-Center evaluations which use controls for area-level observables to estimate their impacts. However, we do construct the MVPF for several of these cases; and we note that our results would be quite similar (if not stronger) if these policies were included in our analysis.

[2] Standard errors are not reported for all estimates in Heckman et al. (2010). This means that we cannot obtain confidence intervals on our MVPF by aggregating the uncertainty in individual estimates as is our normal method. Instead, we take the t-stat for their CBA reported in their table 1, then assume that the WTP and fiscal externality we calculate both have the same t-stat, implying standard errors given our point estimates constructed from Heckman et al. (2010)'s estimates, allowing us to proceed with our normal parametric bootstrap procedure from this point.

MVPF construction, but we discuss how one can incorporate them into the MVPF below. As noted, including these effects only reinforces our main conclusions.

Cost The upfront costs of Perry Pre-School were $17,759 in 2006 USD. However, we estimate that the long-run reductions in transfer payments and increases in tax revenue offset roughly 92% of these upfront costs. Heckman et al. (2010) estimate significant earnings increases from ages 19-40, and an increase in earnings of 26% at age 40. We combine their estimated earnings effects with a forecast to age 65 this into a lifetime earnings impact of $70,535. Using a state and federal combined tax rate of 12.9%[3], this implies an increase in tax revenue of $9,607. Heckman et al. (2010) also estimate that the policy led to a reduction of payments on welfare programs of $3,941. In addition, there are also induced costs of college attendance and vocational training whose incidence falls on the government. Heckman et al. (2010)'s estimates actually imply a fall in such costs, saving the government $2,805. This suggests $16,353 is repaid to the government, implying a net cost of $1,406 (95% CI of [-9,494, 11,412]). Roughly 92% of the upfront spending is repaid to the government.

To harmonize across policies, these baseline cost measures exclude other cost components that are included in the analysis of Heckman et al. (2010). In particular, we do not include their estimated $4,287 marginal cost increases from K-12 education (costs which arise because of increased special education or GED attainment), nor do we include any costs of crime. Heckman et al. (2010) find reductions in crime, which they estimate reduced the cost of policing by $19,894 and correctional costs by $8,281. On net, including these additional cost reductions implies a net cost of $-22,483 (95% CI of [-49,305, 2,139]). Incorporating the cost savings from crime yields a point estimate that suggests the policy pays for itself.

WTP From the WTP perspective, we assume in our baseline specification that WTP is from the post-tax earnings impacts of $60,928 plus the estimated reduction in private outlays on college tuition and vocational training of $393.[4] This implies an MVPF of 43.6 with a 95% CI of [2.08, ∞]. If one includes the additional government cost reductions detailed above this moves to an MVPF of ∞ (95% CI of [11.13, ∞]). One can also follow Heckman et al. (2010) and include WTP externalities from reduced victimization which Heckman et al. (2010) suggests are equal to $172,384.[5] Including the effect of impacts on crime on both costs and WTP implies an MVPF of ∞ (95% CI of [42.4, ∞]).

A.II.2 Abecedarian

The Abecedarian Project is an intensive early childhood intervention conducted in the 1970s. It provided child care for 6-8 hours a day, 5 days a week, from infancy through to age 5. This child care revolved around game-based education, aimed particularly at developing language skills, but also other social, emotional and cognitive skills. We form MVPF estimates for the Carolina Abecedarian Project using the cost-benefit analysis of Barnett and Masse (2007) combined with later follow-up analysis in Campbell et al. (2012) that provides direct measurement of the impact of the policy on children's earnings.

The initial cost of the program was $63,467 per child. The policy led to increases in parental labor earnings of $68,728, which we estimate to have generated $13,746 in tax revenue using our (conservative) CBO tax and transfer rate of 20%. Our results would imply an even higher MVPF if we used the 35% assumption from Barnett and Masse (2007). In addition, the program partially crowded out previous attendance at publicly-funded preschools, which Barnett and Masse (2007) suggests reduced government costs by $3,165. In adulthood, the Barnett and Masse (2007) estimate an increase in college attendance of 23 percentage points, which we estimate to have cost the government $7,428. Campbell et al. (2012) estimate a $12,730 impact on earnings at age 30, which we project across the lifecycle using our forecasting methods to give a $191,990 increase in earnings, implying a $38,398 increase in

[3]This combined tax rate is based on estimates of tax rates by income from Congressional Budget Office (2016). We use the estimate of control group earnings at age 40 of $22,912, to estimate the tax rate, though the estimated tax rate is identical if we add the treatment effect on earnings to this number.

[4]Heckman et al. (2010) find that the Perry Preschool Program actually led to a reduction in college enrollment of 11 percentage points in the treatment group relative to the control (Appendix table D.1). In order to be consistent with our treatment of college costs in other policies, we take Heckman et al. (2010)'s estimates of the impact on total costs of college and vocational training, then estimate that 12.3% of these costs are privately borne. This 12.3% share is the average proportion of tuition in college costs at 2 and 4 year colleges, estimated from the Delta Cost Project (American Institutes for Research, 2017), weighted by the shares of individuals in Heckman et al. (2010)'s completing degrees at 2 and 4 year colleges respectively.

[5]Conceptually, this term enters the MVPF because there are potential victims of crime that are beneficiaries of the policy, and the MVPF framework suggests their WTP should be included (and their welfare be included in thinking about the incidence of the policy, $\bar{\eta}$).

tax revenues at our baseline 3% discount rate. On net, we estimate the total cost of the program to be $13,990, roughly 22% of its upfront cost.

From a WTP perspective, we measure WTP as the sum of after tax earnings on the children of $167,223, net of the $700 increased private college costs of children, and benefits to parents and relatives from reduced spending on child care of $15,052. This implies a sum WTP of $166,349, and an MVPF of 11.89 (95% CI of [-0.18, ∞]).

A.II.3 Chicago Child-Parent Center

CPC is an early education program aimed at children aged 3-9 aimed at children from low income neighborhoods (Reynolds et al., 2002). The program has a focus parent involvement alongside providing educational inputs.

We use estimates from an evaluation of Chicago's Child-Parent Center (CPC) beginning in 1985 (Reynolds et al., 2002, 2011) who compare children in these programs to observationally similar children not in the programs. Because these analyses do not employ an experimental or quasi-experimental design, we include the results in the extended but not the baseline sample. Following their analyses, we separately analyze three levels of involvement with the program: preschool (ages 3-5), school age (ages 6-9), and extended (ages 3-9). In this explanation we focus on the numbers for the preschool variant of the program, but our methodology is the same across the three programs.

The upfront cost of the preschool program is $6,692. This cost is then partially offset by fiscal externalities through lower grade retention by age 15, fewer years of special education and lower incidence of child maltreatment. We take Reynolds et al. (2002)'s estimates in order to convert these impacts into cost savings to the government. Summing these components the initial cost of the program is offset by $5,642. (In our baseline estimates we exclude cost reductions driven by fewer petitions to juvenile court, to be consistent with our exclusion of costs associated with crime across programs. Including these effects would offset costs by a further $4,518.) We use Reynolds et al. (2011)'s direct estimates of a $786 yearly earnings increase between the ages of 24 and 27.[6] We project this earnings gain across the lifecycle using our usual method described in Appendix I, estimating a $18,860 discounted sum of increased pre-tax earnings. We apply a 19.9% CBO tax rate[7], which implies a $3,753 increase in taxes paid. Reynolds et al. (2002) argue that as CPC increases high school completion there is a knock-on effect on college attendance, we incorporate their estimate that government college costs increase by $371 as a result of the program. Summing all of these components the net cost of the program is estimated to be $-2,332 (95% CI of [-4,117, -658]). The point estimates suggest the program pays for itself. Following these same calculations for the school age and extended programs, we find net costs of -413 (95% CI of [-3,791, 2,933]) and -4,937 (95% CI of [-24,850, 17,687]), respectively.

The WTP for the program in our baseline specification is driven solely by the post-tax earnings impact of the program, net of privately incurred college costs. The estimated post-tax earnings impact of the program is $15,107, attenuated by a $185 increase in college costs. There are additional components that could be considered as part of participants' WTP that we exclude in our baseline analysis. From Reynolds et al. (2002)'s estimates one could additionally include reduced childcare costs to parents ($1,657) or willingness to pay from individuals exposed to fewer crimes in the future ($6,127). We find a WTP for the program of $14,922 (95% CI of [1,614, 28,185]). Combining our cost and WTP estimates we find an MVPF for the preschool program of ∞ (95% CI of [∞, ∞]) . For the extended and school age programs there is significantly more uncertainty in our estimated MVPFs: the MVPF estimate for the school age program is 1.32 (95% CI of [$-\infty$, ∞]), for the extended program it is ∞ (95% CI of [$-\infty$, ∞]). In sum, the estimates suggest these programs provide large MVPFs – although sometimes imprecise – and are broadly consistent with the patterns in our baseline sample.

A.II.4 Head Start Impact Study

The Head Start Impact Study was a randomized control trial taking place in 2002-2006 that assigned a random sample of roughly 5,000 three and four year old children into Head Start programs across the country. While the program is too recent to observe impacts on earnings, Kline and Walters (2016) evaluate the Head Start program by translating impacts on test scores into long-run outcomes. They also conduct an analysis that incorporates substitution away from other preschool programs. Because child outcomes are not directly observed, we include this policy in our extended but not baseline sample.

Kline and Walters (2016) also estimate MVPFs for Head Start based on a menu of different assumptions. Their preferred MVPF estimate is 1.84 and this is the specification that most closely aligns with our baseline assumptions,

[6]We use this in place of the projections provided in Reynolds et al. (2002) who forecast future wage increases using high school completion rates and the cross-sectional differences in earnings between those with and without high school degrees.

[7]This is based on the observation that children are expected to earn roughly 128% of the FPL in their later lives.

with one exception: their baseline specification does not include any willingness to pay component from the fact that parents no longer expend private resources on preschool. We take the rest of their analysis as given, and arrive at an MVPF of 2.41 when incorporating this additional WTP by the parents.

To arrive at this estimate, we begin with costs. Kline and Walters (2016) report the upfront cost of the Head Start program at $8,000 in 2013 dollars, which we convert to $6,171 in 2002 dollars for alignment across components. However, this upfront cost does not incorporate substitution of children out of other pre-school programs in order to participate in Head Start. Kline and Walters (2016) estimate that 33.8% of Head Start enrollees substitute away from other programs, and we take their preferred assumption that these other programs have a marginal enrollment cost that is 75% of Head Start's. Together these imply $1,564 in savings to the government from lower enrollment in other childcare programs. Kline and Walters (2016) use their estimates of impacts on test scores to estimate lifetime earnings impacts and we try to follow their methodology as closely as possible. We take their assumption, derived form a review of the relevant literature, that a one standard deviation increase in test scores leads to a 10% increase in earnings. We also follow Kline and Walters (2016) in estimating lifetime earnings via an intergenerational income elasticity of 0.4 and an estimate of Head Start parents earning at 46% of the US average, instead of our normal forecasting methods. In this way we estimate an increase in future earnings of $6,464, implying a $1,286 in tax revenue at an estimated 19.9% tax rate. As a robustness exercise we calculate these impacts using the forecasting methods we use throughout the rest of the paper. We find very similar results: the earnings and tax revenue impacts are $7,023 and $1,398 respectively. Combining these substitution and earnings estimates, in our baseline specification we find Head Start to have a net cost of $3,320 (95% CI of [2,999, 3,612]).

The first component of the WTP for Head Start in our baseline is that we assume that children value their future post-tax earnings increases, estimated at $5,178. The second component is that parents value the reduction in care that they have to provide themselves. To estimate this we assume that the 45.5% of parents who when offered Head Start switch to it from no preschool value this reduction in parental care at the upfront cost of Head Start, implying a WTP of $2,808. Combining these two components we estimate a WTP of $7,986 (95% CI of [$6,811, 9,280]). Combining the WTP and cost components we estimate an MVPF of 2.41 (95% CI of [1.90, 3.15]).

A.II.5 Head Start Applications

Ludwig and Miller (2007) exploit a discontinuity in availability of head start facilities based on the county-level poverty rate. They estimate the impact of head start on outcomes across mortality and educational attainment. They do not observe direct impacts on earnings; we therefore exclude it from our baseline sample. But, we illustrate how the estimates can be translated into their implied MVPF if one is willing to forecast the long-run impacts through college attendance.

The average upfront costs of providing three years of head start in this sample were $506. Ludwig and Miller (2007) estimate a 3.7 percentage point increase in college attendance and a mortality reduction of 1.9 per 100,000. We use the college attendance impact as an intermediate outcome in order to project future earnings gains, using Zimmerman (2014)'s estimates of the impact of college attendance on earnings. We estimate a $455 impact on lifetime earnings, implying an $90 increase in tax revenues at a tax rate of 19.9%. This 3.7 percentage point increase in college attendance also implies a $94 cost to the government. Combining, these estimates imply a net cost of the policy of $509 (95% CI of [-348, 1,400]).

Turning to the WTP for these head start expansions, in our baseline this has two components: the after-tax earnings impact on children and willingness to pay for mortality reductions. The after-tax earnings impact is $364, which we subtract the increased private cost of college attendance at $20 from to get $344 on net. For the valuation of mortality reductions we assume a $1 million value of a statistical life in 2012 dollars to be consistent across programs. Combining this with Ludwig and Miller (2007)'s estimated yearly mortality reduction of 0.0019% over the ages 5-9, this implies a WTP for this reduction in mortality of $17. Summing these components we find a WTP of $362 (95% CI of [-30, 943]). This implies the MVPF for this policy is 0.71 (95% CI of [-0.02, ∞]).

The MVPF is quite low relative to other pre-school MVPFs (although we cannot reject the hypothesis that the policy pays for itself). However, we also note that if we were to forecast the long-run impacts of policies like the Perry Preschool Program, we would find a very low MVPF as well (the Perry program has a point estimate suggesting a decrease in college enrollment - which implies the effects on income operate through a channel other than college enrollment). This suggests one should interpret this MVPF as more illustrative of the method as opposed to drawing precise conclusions about the desirability of head start spending. And, it highlights the value of future analyses of these policies using long-run administrative earnings data.

A.III K-12 Spending Results

A.III.1 School Finance Equalization

Jackson et al. (2016) use variation from finance equalization reforms to estimate the impact of school funding on children's wages between ages 20-45. We begin with our estimates of costs, which suggest that the increased future tax revenue paid by children in adulthood fully covered the upfront cost of the increased spending.

We arrive at a cost estimate by first showing that the estimated results in Jackson et al. (2016) imply that every $1 of spending leads to a present discounted value increase in earnings of $4.49. To see this, note that Jackson et al. (2016) estimate that a 1% increase in spending is leads to a 0.77% increase in wages in later life. Average family income at age 30 in Jackson et al. (2016)'s sample is $49,308; to get an estimate of individual income we assume there are 1.302 earners per family[8], implying an individual income of $37,871. This implies that a 1% increase in school funding would increase yearly income by $293 on average. As schools in Jackson et al. (2016)'s sample spend on average $4,800 per year per student, a $1 increase in spending implies a 0.0017% increase in total educational spending over the ages 5-17. Combining these numbers, we estimate that $1 of additional spending increase incomes by $0.51 in a given year between the ages of 20 and 45. We then project this yearly increase up to age 65 using our standard forecasting methods, estimating a discounted sum of earnings increases of $10.81 for every $1 of upfront spending (at a 3% discount rate).

Given the $10.81, we apply a CBO tax and transfer rate of 18.8%. This suggests an increase in government revenues of $2.03 and a net cost of the policy of $-1.03 (95% CI of [-2.02, -0.06]).

To measure WTP, we assume in our baseline specification that children value the spending at its impact on their after-tax earnings. We estimate this to be $8.78 (95% CI of [4.58, 13.03]). Combining our cost and WTP estimates we find an infinite MVPF (95% CI of [∞, ∞]).

A.III.2 Local Spending Variation

Hyman (2017) estimates the impact of school funding on educational outcomes using variation induced by local house prices. Because the paper does not directly observe earnings outcomes, we include it in our extended but not our baseline sample.

We begin with an estimate of costs. Hyman (2017) finds that an increase of $4,000 of spending yields a 3 percentage point increase in college enrollment. This spending occurs during grades 4-7. Discounting the spending back to age 11 yields spending of $3,829. We then use our projection method combined with Zimmerman (2014)'s estimates of the impact of college attendance on earnings in order to convert Hyman's 3 percentage point increase in college enrollment into earnings. This suggests the spending yields an increase in earnings of $3,278 increase in future earnings. Applying a tax and transfer rate of 20%, it implies an increase in government revenue of $656. However, the increased college enrollment also imposes a cost on the government of $475 in public spending on college. Summing these components together, we estimate a $3,647 total cost of the policy (95% CI of [2,923, 4,126]).

We estimate the WTP for the policy as the post-tax earnings impacts on children, $2,623, net of their increased private costs of college attendance, $245. This gives a total WTP of $2,377, (95% CI of [25.74, 6,092]). Combining our WTP and cost estimates we get an MVPF of 0.65 (95% CI of [0.10, 2.48]).

Because earnings are not directly observed, the method is potentially severely understating the benefits and over-stating the net costs of the policy. To see this, note that the estimates effectively assume that the only children who value the increased educational spending are the 3% of compliers induced to enroll at college. It is reasonable to expect that the other 97% of children who experienced increased spending in college also place some value on additional educational spending, and that perhaps that spending increases their earnings, which further reduces government costs. We leave these explorations as important directions for future work.

B College

B.I Sample Frame

We begin the construction of our sample frame for college policies using the Page and Scott-Clayton (2016)review of the literature. We focus on our attention on traditional student aid, merit-based aid programs, and tax cred-

[8] Our finding of an infinite MVPF for this policy is not sensitive to the assumed number of earners per family. Assuming 2 earners per family the MVPF is infinite (95% CI of [9.41, ∞]) and the point estimate for the MVPF remains infinite up until 2.8 earners per family. We choose 1.302 in our baseline by estimating the average earners per household in the US using data from http://www.aei.org/publication/explaining-us-income-inequality-by-household-demographics-2017-edition-2/.

its/deductions for college spending. (These papers are found in Sections 2.1 - 2.4 of the review.) We supplement these papers with more recent policy reforms cited in Zimmerman (2014)and Denning et al. (2019). We also supplement with other recent research using quasi-experimental variation, such as Acton (2018). Although we do not include these programs in our baseline sample, we also present results for several "information treatment" reforms designed to increase college attendance. These include Bettinger et al. (2012) and Dynarski et al. (2018). As is the case in all our sample frames, we restrict our attention to studies with sufficient information to construct net costs and willingness to pay. In particular, we require that papers estimate impacts on earnings or impacts on college attainment. We use various measures of college attainment from college enrollment to degree receipt to credits completed. In cases where our calculations require estimates of the returns to college attendance, we use figures from Zimmerman (2014). We arrived at that reference using the Oreopoulos and Petronijevic (2013) review of the evidence on the returns to college.

B.II Outline of MVPF Derivation

We begin by outlining the theory of how to construct the MVPF for expenditures to support college students. This covers a wide range of policies such as supplemental financial aid, college tax credits and informational treatments to encourage college enrollment. As with other policies, MVPF is given by $WTP/Cost$. We discuss the numerator and denominator in turn, beginning with willingness to pay.

Willingness to Pay In cases where researchers have estimated the earnings impact of a college expenditure policy, our primary specification measures willingness to pay based both on the value of the expenditure and on the post-tax and post-transfer increase in earnings. Any individual who does not change their behavior in response to the expenditure values it based on the size of the transfer. For example, if a student receiving grant aid would have attended and graduated college without the additional aid, they value that aid as a transfer. By contrast, any individual who changes their behavior in response to the expenditure values it based on the increase in total income it provides. This implicitly assumes that increases in income amongst the college educated stem from returns to human capital, not from higher levels of effort, which would require an adjustment for the disutility of labor. This also requires an assumption that individuals induced to attend college were not engaging in perfect optimization at the time of the expenditure. We modify this assumption in an alternate specification below.

Performing this calculation of willingness to pay requires an estimate of total earnings gains due to college enrollment. In many instances, researchers have only calculated short- or medium-run earnings gains, and so we project out those gains over the course of the lifecycle. Appendix I explains how we take the percentage earnings gains observed and project out those gains using a trajectory of lifecycle earnings derived from the American Community Survey. We calculate lifetime earnings gains in constant dollars and discount all earnings gains by 3% back to the time of initial expenditure. We assess robustness to alternative discount rates.

In some cases researchers have not directly observed changes in earnings, but rather observe outcome variables such as college enrollment or college credits, which are correlated with earnings. In those instances, our approach to calculating willingness to pay must change. We once again assume that non-marginal beneficiaries of college expenditures value those expenditures dollar for dollar. We also assume that marginal beneficiaries, those induced to attend college due to the expenditure, value the expenditure based on the earnings gain it induces. We determine the earnings gains amongst these marginal beneficiaries by using estimates of the impact of an expenditure on college enrollment or college credits or college graduation. In the cases where we observe changes in college enrollment or graduation rather than years attended, we assume those variables translate into a fixed number of additional years of schooling. (We allow that number to vary in robustness checks.) We then use estimates from Zimmerman (2014), which suggest that an additional year of schooling results in 11.3% higher earnings (with a confidence interval of [3.79,18.72]). We project those earnings gains out over the full lifecycle and determine the discounted value of increased earnings. Appendix I outlines this method in detail.

Once we have calculated total earnings gains via either method described, we must account for changes in taxes paid and transfers received. If researchers have calculated the impact of college expenditures on taxes or transfers, we use those estimates directly. In all other cases, we use a measure of the effective marginal tax rate for recipient, as estimated by the Congressional Budget Office. Appendix G outlines the details of this method. That post-tax change in earnings provides information on willingness to pay for all individuals induced to change their behavior in response to the program being implemented. We calculate total willingness to pay by summing those earnings gains with the simple value of the transfer for the fraction of individuals not induced to change their behavior.

Cost The cost of these policies is the observed causal effect of the policy on the government budget. This begins with the initial program costs and any additional college costs that are "crowded in" due to the initial expenditure. For example, if assistance filling out the FAFSA form induces students from low-income families individuals to enroll college, that will induce a substantial state expenditure to help pay for the yearly cost of college enrollment. We incorporate these expenditures. In cases where researchers report the total government expenditures on a full-time student at the colleges in question, we use those estimates directly. In all other cases, we follow the approach of Zimmerman (2014), and calculate costs based on data from the Delta Cost Project. Appendix B.VIII outlines the details of this method. Finally, we incorporate the fiscal externality due to an increase in taxes paid or reductions in transfers received. Those calculations are made using the same method described in the willingness to pay section above.

Alternate Specifications In order to test the robustness of our MVPF estimates, we make several modifications to our primary specification. For example, in Section IV.C we show how all of our results vary under different discount rates (Appendix Figure IV) or assumed tax rates (Appendix Figure V). In this Appendix we explore how our results change with modifications to our willingness to pay assumptions. In particular, we implement an alternate specification where individuals do not value college expenditures based on their impact on college earnings. Instead, individuals value these expenditures base on the size of the initial transfer they receive.[9] We consider generally consider this approach to be a conservative measure of individual willingness to pay.

In this specification, we continue to assume that any individual who doesn't change their behavior in response to the transfer values the expenditure dollar for dollar. However, for those induced to attend college as a result of the policy, we no longer assume they value college by its impact on their after-tax earnings. Instead, we assume they value the transfer at some fraction of it's initial value. This approach stems from the logic of the envelope theorem. These individuals were offered a transfer that was conditional on enrollment and they consequently re-optimized their behavior in order to receive that transfer. To first order, those individuals were indifferent between their initial behavior and their re-optimized behavior. As a result, we set the willingness to pay equal to zero for those who change their behavior. We also allow this assumption to be modified so that individuals induced to change their behavior value the expenditure at some fraction of its initial value.

In cases where programs where programs eliminate barriers to college entry (Appendix B.VII), we also introduce an alternate conservative measure of willingness to pay. These programs generally provide students with information about college enrollment options or ease the college application process. Given that there is no explicit monetary transfer as part of the program (the program provides information not money), we consider the possibility that students do not value the expenditures. In that scenario, students are essentially indifferent between their options and so we set willingness to pay equal to just 1% of program costs. In each section below we show how the MVPF varies with these willingness to pay assumptions.

B.III College Financial Support with Observed Earnings Changes

B.III.1 Cal Grants

Our baseline specification for California's Cal Grant financial aid has two distinct MVPFs. We find an MVPF of ∞ with a 95% confidence interval of $[10.72, \infty]$ for students at the GPA eligibility threshold. We find an MVPF of -0.69 with a 95% confidence interval of $[-2.36, 7.41]$ for students at the income eligibility threshold. This Appendix walks through the construction of these estimates.

The Cal Grant provides full tuition support to students who meet certain eligibility requirements such receiving a sufficiently high GPA and having family income that falls below a given threshold. Work by Bettinger et al. (2019) provides the primary causal estimates for our analysis of the Cal Grant program. They examine the impact of Cal Grant eligibility using eligibility discontinuities based on both high school GPA and family income. During the late 1990s, when the students analyzed by Bettinger et al. (2019)were enrolled, the grant was worth between $1,500 and $3,500 for students enrolled in four-year public colleges and between $9,000 and $9,700 for students enrolled at private colleges (Bettinger et al., 2019). Bettinger et al. (2019) calculate the impact of Cal Grant eligibility on Cal Grant benefits, college attendance and long-term earnings.

GPA Threshold Result First we explain how we calculate the MVPF for those who receive a Cal Grant because they receive a high school GPA that is just above the eligibility threshold. In our primary estimate we calculate willingness to pay for Cal Grant aid based on the increase in post-tax earnings amongst those who receive the aid.

[9] In our .do files we refer to this method as valuation "at cost".

We consider the impact of ~$4,311 in provided aid due to Cal Grant eligibility. Bettinger et al. (2019) observe earnings gains over the course of 10-14 years and find that post-tax earnings, discounted by 3% back to the time of initial expenditure, rise by $7,387. (We conservatively assume that there is no net earnings impact in years 1-9 after the grant is received. Figure 7 in Bettinger et al. 2019 shows positive but insignificant point estimates in years 5-9, but those point estimates and standard errors are not reported in the paper. Given that additional college attendance may produce earnings declines in years 1-4 our assumption implicitly requires that the earnings declines in years 1-4 offset the unreported earnings gains in years 5-9.) Next we use the income projection method discussed above and outlined in detail in Appendix I. We project the percentage earnings gains observed in years 10-14 and find a lifetime earnings impact of $42,353. We calculate post-tax and post-transfer earnings by assuming a tax and transfer rate of 18.6%. This figure comes from our calculation of the effective marginal tax rates based on estimates from the Congressional Budget Office. Details of our approach can be found in Appendix G. The observed earnings gains provide the willingness to pay for all individuals induced to change their behavior and receive more education as a result of the grants. We calculate total willingness to pay by summing those earnings gains with the simple value of the transfer for the fraction of individuals not induced to change their behavior. We get a willingness to pay of $38,575.

We calculate net costs by starting with $4,311 in initial program costs and accounting for any relevant fiscal externalities. First, we account for the additional costs due to increased educational attainment on the part of those eligible for Cal Grant. We use estimates from Bettinger et al. (2019) on the number of additional bachelors degrees received by Cal Grant recipients and translate that into a fixed number of years of additional schooling. (In our baseline, we assume that number is two.) We also use estimates from Bettinger et al. 2019 on the number of individuals who switch between four-year public institutions and two-year public institutions. In each case, we follow the approach of Zimmerman (2014), and calculate government costs per full time enrollee based on data from the Delta Cost Project. Appendix B.VIII outlines the details of this method. This results in additional government costs of $1,002. We also account for the increase in costs as those who would have enrolled in the absence of the Cal Grant are induced to attend more expensive schools. This adds $210 to total costs. Next, we account for the changes in taxes paid and transfers received based on the earnings gains calculated in the willingness to pay section. That results in a fiscal externality of -$7,878 and a total net cost of -$2,355.

Combining these estimates, we get an MVPF of ∞. To obtain the confidence intervals, we bootstrap the enrollment, earning and tax revenue outcomes. We assume (conservatively) these estimates are perfectly correlated across bootstrap iterations. This leads to a confidence interval of $[10.72, \infty]$.

We also consider a specification where the Cal Grant is valued at the cost of the transfer rather than based on the change in long-term earnings. This conservative willingness to pay method ignore any effect from increases in earnings. Instead, it applies the envelope theorem to those induced to get more schooling and assumes they are indifferent to the expenditure. All those do not change their behavior as a result of the scholarship value it as a dollar-for-dollar transfer. The resulting MVPF for this alternate specification is ∞ with a 95% confidence interval of $[3.15, \infty]$.

Income Threshold Result Now we explain how we calculate the MVPF for those who receive a Cal Grant because they have a family income that is just below the eligibility threshold. Our methodology is nearly identical to the GPA threshold place above. In our primary estimate we calculate willingness to pay for the Cal Grant aid based on the increase in post-tax earnings amongst those who receive the aid. We consider the impact of ~$8,115 in provided aid due to Cal Grant eligibility. Bettinger et al. (2019) observe earnings changes over the course of 10-14 years and find that post-tax earnings, discounted by 3% back to the time of initial expenditure, decline by $3,297. (We conservatively assume that there is no net earnings impact in years 1-9 after the grant is received. Figure 7 in Bettinger et al. (2019) shows positive but insignificant point estimates in years 5-9, but those point estimates and standard errors are not reported in the paper. Given that additional college attendance may produce earnings declines in years 1-4 our assumption implicitly requires that the earnings declines in years 1-4 offset the unreported earnings gains in years 5-9.) Next we use the income projection method discussed above and outlined in detail in Appendix I. We project the percentage earnings decline observed in years 10-14 and find a lifetime earnings decline of $16,241. We calculate post-tax and post-transfer earnings by assuming a tax rate of 18.7%. This figure comes from our calculation of the effective marginal tax rates based on estimates from the Congressional Budget Office. Details of our approach can be found in Appendix G. The observed earnings change provide the willingness to pay for all individuals induced to change their educational attainment as the result of the grant. We calculate total willingness to pay by summing those earnings gains with the simple value of the transfer for the fraction of individuals not induced to change their behavior. We get a willingness to pay of -$8,094.

We calculate net costs by starting with $8,115 in initial program costs and accounting for any relevant fiscal externalities. First, we account for the additional costs due to increased educational attainment on the part of those eligible for Cal Grant. We use estimates from Bettinger et al. 2019 on the number of additional bachelors degrees received by Cal Grant recipients and translate that into a fixed number of years of additional schooling. (In our baseline, we assume that number is two.) We also use estimates from Bettinger et al. (2019)on the number of individuals who switch between four-year public institutions and two-year public institutions. In each case, we follow the approach of Zimmerman (2014), and calculate government costs per full time enrollee based on data from the Delta Cost Project. Appendix B.VIII outlines the details of this method. This results in additional government costs of \$148. We also account for the increase in costs as those who would have enrolled in the absence of the Cal Grant are induced to attend less expensive schools. This subtracts \$191 from total costs. Next, we account for the changes in taxes paid and transfers received based on the earnings gains calculated in the willingness to pay section. That results in a fiscal externality of \$3,654 in additional costs to the government and a total net cost of \$11,726.

Combining these estimates, we get an MVPF of -0.69. To obtain the confidence intervals, we bootstrap the enrollment, earning and tax revenue outcomes. We assume (conservatively) these estimates are perfectly correlated across bootstrap iterations. This leads to a confidence interval of $[-2.36, 7.41]$.

We also consider a specification where the Cal Grant is valued at the cost of the transfer rather than based on the change in long-term earnings. This conservative willingness to pay method ignore any effect from increases in earnings. Instead, it applies the envelope theorem to those induced to get more schooling and assumes they are indifferent to the expenditure. All those do not change their behavior as a result of the scholarship value it as a dollar-for-dollar transfer. The resulting MVPF for this alternate specification is 0.66 with a 95% confidence interval of $[0.46, 1.66]$.

B.III.2 Florida International University Admissions

Our baseline specification for admission to Florida International University (FIU) has MVPF of ∞ with a 95% confidence interval of $[\infty, \infty]$. This Appendix walks through the construction of this estimate.

Work by Zimmerman (2014) provides the primary causal estimates for this analysis. They examine the impact of admission to FIU using a fuzzy regression discontinuity design that examines students just above and just below FIU's academic performance cutoff for admissions. They calculate the impact of admissions on years of school attended and future earnings.

In our primary estimate we calculate willingness to pay for FIU admission. Zimmerman (2014) observes earnings changes over the course of 14 years, distinguishing between earnings declines in years 1-7 and earnings gains in years 8-14. We sum those earnings changes, discounting by 3% back to the time of initial expenditure, and find that earnings rise by \$25,427. Next we use the income projection method discussed above and outlined in detail in Appendix I. We project the percentage earnings gains observed in years 8-14 and find a lifetime earnings impact of \$142,757. We calculate post-tax and post-transfer earnings by incorporating a tax rate in each period, with a long-run tax and transfer rate of 18.6%. This figure comes from our calculation of the effective marginal tax rates based on estimates from the Congressional Budget Office. Details of our approach can be found in Appendix G. We also subtract out individual contributions to tuition in future years, which reduce willingness to pay by \$2,851. This results in a total willingness to pay of \$112,844.

The treatment in this case is admission to FIU, so the initial program costs are determined by federal expenditures on students in the state university system. Zimmerman (2014) provides estimates of the total level of educational expenditures induced by an additional FIU enrollment. They find an increase in total expenditures in the Florida State University System a decrease in total expenditures in the Florida Community College System. We calculate the cost paid by the government by taking the increase in total expenditures and netting out the private contributions made by enrolled students. We discount these expenditures over four years and find the total public cost per admitted student is \$2,617. Next, we account for the changes in taxes paid and transfers received based on the earnings gains calculated in the willingness to pay section. That results in a fiscal externality of \$27,062 and, when combined with the educational expenditures, a total net windfall to the government of \$24,445.

Combining these estimates, we get an MVPF of ∞. To obtain the confidence intervals, we bootstrap the enrollment, earning and tax revenue outcomes. We assume (conservatively) these estimates are perfectly correlated across bootstrap iterations. This leads to a confidence interval of $[\infty, \infty]$.

We also consider a specification where admission to FIU is valued at the cost of the transfer rather than based on the change in long-term earnings. This conservative willingness to pay method ignore any effect from increases in earnings. Instead, it applies the envelope theorem to those induced to get more schooling and assumes they are indifferent to the expenditure. All those do not change their behavior as a result of the scholarship value it as a dollar-for-dollar transfer. The resulting MVPF for this alternate specification is ∞ with a 95% confidence interval of $[\infty, \infty]$.

B.III.3 Texas Pell Grants

Our baseline specification for Pell Grant provision in Texas has an MVPF of ∞ with a 95% confidence interval of $\infty, \infty]$. This Appendix walks through the construction of this estimate.

The Pell Grant provides financial support to students from low-income families attending college. Work by Denning et al. (2019) provides the primary causal estimates for this analysis. They examine the impact of additional Pell Grant provision in the state of Texas using a discontinuity in Pell provision for those just above and just above the zero Expected Family Contribution threshold. Denning et al. (2019) use this discontinuity to calculate the impact of greater grant eligibility on years of school attended and earnings. We focus on their main specification – the impact of Pell Grant support on new college enrollee.

In our primary estimate we calculate willingness to pay for Pell Grant aid based on the increase in post-tax earnings amongst those who receive the aid. Denning et al. (2019) measure the impact of \$1,000 in aid provided. They observe earnings gains over the course of 7 years and find that post-tax earnings, discounted by 3% back to the time of initial expenditure, rise by \$5859. Next we use the income projection method discussed above and outlined in detail in Denning et al. (2019). We project the percentage earnings gains observed in year 7 and find a lifetime earnings impact of \$104,691. We estimate the fiscal externality associated with the first seven years of earnings by taking the tax rate calculated in Denning et al. (2019) and supplementing with an estimate of the effect on government transfers. That estimate of the transfer effect comes from our effective marginal tax rate calculation based on Congressional Budget Office research. (In the primary specification, we do not adjust for FICA taxes because we assume that those taxes flow back to the individual in the long run.) We then use the Congressional Budget Office approach to estimate the long-term tax rate. Details of our approach can be found in Appendix G. The estimated earnings gains provide the willingness to pay for all individuals induced to change their behavior and receive more education as a result of the grants. We calculate total willingness to pay by summing those earnings gains with the simple value of the transfer for the fraction of individuals not induced to change their behavior. This results in a willingness to pay of \$85,737.

We calculate net costs by starting with \$1,000 in initial program costs and accounting for any relevant fiscal externalities. First, we account for the additional costs due to increased educational attainment on the part of those eligible for Pell Aid. Denning et al. (2019) show that the provision of Pell Aid leads to greater educational persistence on the part of students. We sum the total number of years of additional enrollment up until seven years after first grant receipt. We calculate the costs based on total expenditures for a single year of four-year public college enrollment. (In particular, we follow the approach of Zimmerman (2014), and calculate costs based on data from the Delta Cost Project. Appendix B.VIII outlines the details of this method. This produces more conservative estimates than the approach taken in Denning et al. (2019). Denning et al. (2019) measure total government costs in terms of Pell Grants and other grant aid crowded in, rather than including government funding directly provided to schools.) The result is that \$1,000 in Pell Grant Aid costs an additional \$1,030 in fiscal costs during subsequent years. Next, we account for the changes in taxes paid and transfers received based on the earnings gains calculated in the willingness to pay section. That results in a fiscal externality of \$19,409 in additional revenue to the government and a total net windfall of \$17,379.

Combining these estimates, we get an MVPF of ∞. To obtain the confidence intervals, we bootstrap the enrollment, earning and tax revenue outcomes. We assume (conservatively) these estimates are perfectly correlated across bootstrap iterations. This leads to a confidence interval of $[\infty, \infty]$.

We also consider a specification where the Pell Grant is valued at the cost of the transfer rather than based on the change in long-term earnings. This conservative willingness to pay method ignore any effect from increases in earnings. Instead, it applies the envelope theorem to those induced to get more schooling and assumes they are indifferent to the expenditure. All those do not change their behavior as a result of the scholarship value it as a dollar-for-dollar transfer. The resulting MVPF for this alternate specification is ∞ with a 95% confidence interval of $[-\infty, \infty]$.

B.IV College Financial Support with Projected Earnings Changes

B.IV.1 Florida Student Access Grant

Our baseline specification for the provision of the Florida Student Access Grant (FSAG) has MVPF of 7.42 with a 95% confidence interval of $[1.00, \infty]$. This Appendix walks through the construction of this estimate.

The Florida Study Access Grant provided $1,300 of financial aid support for students below a given low of household income who enrolled in two-year or four-year public universities in Florida. Work by Castleman and Long (2016) provides the primary causal estimates for this analysis. They examine the FSAG using a regression discontinuity design that examines students just above and just below the Expected Family Contribution (EFC) cutoff that determines eligible for the FSAG. They calculate the impact of the FSAG on initial college enrollment, total credits received, and bachelors degree completion rates.

In our primary estimates we calculate willingness to pay for the FSAG using estimates from Castleman and Long (2016) on the total number of additional credits received as a result of the grant. From there, we translate those changes in credits in additional years of schooling and then use estimates from Zimmerman (2014) to estimate the impact on earnings. In particular, we use the results from Zimmerman to estimate a decline in earnings in years 1-7 after enrollment and then an increase in earnings over the rest of the lifecycle. (Zimmerman (2014) observes earnings gains in years 8-14 and we project those gains over the rest of the lifecycle using the approach in Appendix I.) We then calculate the fiscal externality at 20% over the bulk of the lifecycle. This figure comes from our calculation of the effective marginal tax rates based on estimates from the Congressional Budget Office. Details of our approach can be found in Appendix G. This results in a post-tax and post-transfer increase in earnings of $6,073. We also subtract out individual contributions to schooling costs, as approximated by the Expected Family Contribution at the regression discontinuity cutoff. After scaling by years of enrollment, that reduces willingness to pay by $289. The projected earnings gains provide the willingness to pay for all individuals induced to change their behavior and receive more education as a result of the grants. We calculate total willingness to pay by summing those earnings gains with the simple value of the transfer for the fraction of individuals not induced to change their behavior. We get a willingness to pay of $6,592.

The treatment in this case is the receipt of the FSAG, so the initial program costs are determined by cost of the grant scaled by the number of years of grant receipt. Using the estimates from Castleman and Long (2016) we calculate the total grant costs to be $891. Next, we account for the additional costs due to increased educational attainment on the part of those eligible for FSAG. We use estimates from Castleman and Long (2016) on the additional years of educational attainment. We calculate government costs per full time enrollee based on data from the Delta Cost Project. Appendix B.VIII outlines the details of this method. This increases costs by $1607. Next, we account for the changes in taxes paid and transfers received based on the earnings gains calculated in the willingness to pay section. That results in a fiscal externality of $1609 and a total net cost of $889.

Combining these estimates, we get an MVPF of 7.42. To obtain the confidence intervals, we bootstrap the enrollment, earning and tax revenue outcomes. We assume (conservatively) these estimates are perfectly correlated across bootstrap iterations. This leads to a confidence interval of $[1.00, \infty]$.

We also consider alternate approaches to calculating the earnings effect and determining willingness to pay. In the case of the earnings effect, we create a specification where our earnings projections are not based on credits completed, but rather initial enrollment and bachelors degrees received. We assume that these represent distinct groups of recipients and translate each outcome into an increase in years of schooling. In that case we assume that initial enrollment or bachelors degree receipt both result in two additional years of schooling. We then apply the same earnings projection method from Zimmerman (2014) using these schooling increases. The resulting MVPF for this alternate specification is 7.67 with a 95% confidence interval of $[0.61, \infty]$. We also consider a specification where the FSAG is valued at the cost of the transfer rather than based on the change in long-term earnings. This conservative willingness to pay method ignore any effect from increases in earnings. Instead, it applies the envelope theorem to those induced to get more schooling and assumes they are indifferent to the expenditure. All those do not change their behavior as a result of the scholarship value it as a dollar-for-dollar transfer. The resulting MVPF for this alternate specification is 0.91 with a 95% confidence interval of $[0.38, \infty]$.

B.IV.2 CUNY Pell Grant

Our baseline specification for the provision of Pell Grants at CUNY schools has MVPF of 1.39 with a 95% confidence interval of $[-3.61, 8.63]$. This Appendix walks through the construction of this estimate.

The Pell Grant provides financial support to students from low-income families attending college. Work by Marx and Turner (2018)provides the primary causal estimates for this analysis. They examine the impact of Pell Grants using a regression discontinuity design that examines students just above and just below the Pell Eligibility threshold. They calculate the impact of $1,000 in Pell Grant support on total credits earned, initial college enrollment and re-enrollment.

In our primary estimates we calculate willingness to pay for Pell Aid using estimates from Marx and Turner (2018) on the total number of additional credits received as a result of the grant. From there, we translate those changes in credits in additional years of schooling and then use estimates from Zimmerman (2014) to estimate the impact on earnings. In particular, we use the results from Zimmerman to estimate a decline in earnings in years 1-7 after enrollment and then an increase in earnings over the rest of the lifecycle. (Zimmerman (2014) observes earnings gains in years 8-14 and we project those gains over the rest of the lifecycle using the approach in Appendix I.) We calculate the fiscal externality at 20% over the bulk of the lifecycle. This figure comes from our calculation of the effective marginal tax rates based on estimates from the Congressional Budget Office. Details of our approach can be found in Appendix G. This results in a post-tax and post-transfer increase in earnings of $443. We also subtract out individual contributions to tuition, as approximated by the Expected Family Contribution at the regression discontinuity cutoff. After scaling by years of enrollment, that reduces willingness to pay by $21. The projected earnings gains provide the willingness to pay for all individuals induced to change their behavior and receive more education as a result of the grants. We calculate total willingness to pay by summing those earnings gains with the simple value of the transfer for the fraction of individuals not induced to change their behavior. We get a willingness to pay of $1,417.

The treatment in this case is eligibility for the Pell Grant, and the initial program costs are normalized to $1,000 for the sake of the analysis. Next, we account for the additional costs due to increased educational attainment on the part of those eligible for Pell aid. We use estimates from Marx and Turner (2018)on the additional years of educational attainment. We calculate government costs per full time enrollee based on data from the Delta Cost Project. Appendix B.VIII outlines the details of this method. This increases costs by $133. Next, we account for the changes in taxes paid and transfers received based on the earnings gains calculated in the willingness to pay section. That results in a fiscal externality of $117 and a total net cost of $1016.

Combining these estimates, we get an MVPF of 1.39. To obtain the confidence intervals, we bootstrap the enrollment, earning and tax revenue outcomes. We assume (conservatively) these estimates are perfectly correlated across bootstrap iterations. This leads to a confidence interval of $[-3.61, 8.63]$.

We also consider alternate approaches to calculating the earnings effect and determining willingness to pay. In the case of the earnings effect, we create a specification where our earnings projections are not based on credits completed, but rather initial enrollment and student re-enrollment. We assume that these represent distinct groups of recipients and translate each outcome into an increase in years of schooling. We assume that initial enrollment results in two additional years of schooling, while re-enrollment results in one additional year. We then apply the same earnings projection method from Zimmerman (2014) using these schooling increases. The resulting MVPF for this alternate specification is 2.60 with a 95% confidence interval of $[-2.52, 17.39]$. We also consider a specification where the Pell Grant is valued at the cost of the transfer rather than based on the change in long-term earnings. This conservative willingness to pay method ignore any effect from increases in earnings. Instead, it applies the envelope theorem to those induced to get more schooling and assumes they are indifferent to the expenditure. All those do not change their behavior as a result of the scholarship value it as a dollar-for-dollar transfer. The resulting MVPF for this alternate specification is 0.98 with a 95% confidence interval of $[0.61, 1.87]$.

B.IV.3 Social Security Benefits

Our baseline specification for the impact of the Social Security Student Benefit Program has MVPF of 7.26 with a 95% confidence interval of $[0.82, \infty]$. This Appendix walks through the construction of this estimate.

The Social Security Benefit Program provided $6,700 in yearly support for children aged 18-22 who had deceased parents and were attending college. Work by Dynarski (2003) provides the primary causal estimates for this analysis. They examine the Social Security Benefit Program using a difference-in-differences design that examines eligible and ineligible individuals before and after the elimination of the program. They calculate the impact on years of schooling and initial college enrollment.

In our primary estimates we calculate willingness to pay for the Social Security Benefit using estimates from Dynarski (2003) on the years of schooling completed. They estimate an average increase of 0.68 years of schooling per potential enrollee. From there, we use estimates from Zimmerman (2014) to estimate the impact on earnings. In particular, we use the results from Zimmerman to estimate a decline in earnings in years 1-7 after enrollment and then an increase in earnings over the rest of the lifecycle. (Zimmerman (2014) observes earnings gains in years 8-14 and we project those gains over the rest of the lifecycle using the approach in Appendix I.) We calculate the fiscal externality at 20.0% over the bulk of the lifecycle. This figure comes from our calculation of the effective marginal tax rates based on estimates from the Congressional Budget Office. Details of our approach can be found in Appendix G. The projected earnings gains increase willingness to pay by $25,391 for all individuals induced to change their behavior and receive more education as a result of the grants. We calculate total willingness to pay by summing those earnings gains with the value of the $6700 transfer that exceeds the cost of college for the fraction of individuals induced to change their behavior and with the simple value of the transfer for the fraction of individuals not induced to change their behavior. This produces a willingness to pay of $34,861.

The treatment in this case is the receipt of the Social Security benefit, so the initial program costs are $6,700 for each recipient. We scale that cost by the number of years of benefit take-up and the fraction of individuals eligible for the scholarship. That produces a program cost of $11,485. Next, we account for the additional costs due to increased educational attainment on the part of those eligible for the benefit. We use estimates from Dynarski (2003) on the additional years of educational attainment. We calculate government costs per full time enrollee based on data from the Delta Cost Project. Appendix B.VIII outlines the details of this method. Next, we account for the changes in taxes paid and transfers received based on the earnings gains calculated in the willingness to pay section. That results in a fiscal externality of $6,686 and, when combined with the educational expenditures, a total net cost of $4,800.

Combining these estimates, we get an MVPF of 7.26. To obtain the confidence intervals, we bootstrap the enrollment, earning and tax revenue outcomes. We assume (conservatively) these estimates are perfectly correlated across bootstrap iterations. This leads to a confidence interval of $[0.82, \infty]$.

We also consider alternate approaches to calculating the earnings effect, allowing for aid crowding out effects, specifying the amount of benefits, and determining willingness to pay. In the case of the earnings effect, we create a specification where our earnings projections are not based on years of school completed, but rather initial college enrollment. In this case we assume that initial enrollment results in two additional years of schooling. We then apply the same earnings projection method from Zimmerman (2014) using these schooling increases. That specification results in an MVPF of 7.22 with a 95% confidence interval of $[1.30, \infty]$. As an alterantive specification, we assume that the absence of the SS Benefit leads students to seek other forms of government aid. We make the conservative assumption that all students who would have been awarded Pell grants, so the baseline program cost is the benefit amount net of this other form of aid. This gives an MVPF of 12.01with a 95% confidence interval of $[0.74, \infty]$. We also consider a specification in which students receive only partial benefits in the amount of $4,700, giving an MVPF of 23.91with a 95% confidence interval of $[0.73, \infty]$. In the case of willingness to pay, we also considered a specification where the Social Security Student Benefit is valued at the cost of the transfer rather than based on the change in long term earnings. This conservative willingness to pay method ignore any effect from increases in earnings. Instead, it applies the envelope theorem to those induced to get more schooling and assumes they are indifferent to the expenditure. Individuals who do not change their behavior as a result of the scholarship value it as a dollar-for-dollar transfer. The resulting MVPF for this alternate specification is 1.45 with a 95% confidence interval of $[0.79, \infty]$.

B.IV.4 Adams Scholarship

Our baseline specification for the provision of the Massachusetts Adam's Scholarship has MVPF of 0.72 with a 95% confidence interval of $[-0.92, 3.13]$. This Appendix walks through the construction of this estimate.

The John and Abigail Adams Scholarship Program, administered by the Commonwealth of Massachusetts, is a merit aid program that provides tuition wavers to students, regardless of financial need. The scholarship is provided to all students who meet minimum test score criteria in their sophomore year of high school and who subsequently attend an in-state public college or university. Our analysis of the program is based on estimates from Cohodes and Goodman (2014), who analyze the program using a regression discontinuity design. They exploit the scholarship's eligibility criteria, comparing students just above and just below the test score threshold. Cohodes and Goodman (2014) measure the effect of the program on initial school enrollment and post-secondary graduation.

In our primary estimates, we calculate the first component of willingness to pay using Cohodes and Goodman's (2014) estimates of changing in schooling attainment. In particular, Cohodes and Goodman (2014) find no significant effect of the scholarship on overall four-year college enrollment at the threshold, but they do find that the scholarship induces students to shift from enrolling at ineligible four-year colleges towards enrolling Adams-eligible four year colleges. They also find that graduation rates for students at four-year schools decline on net because of the scholarship, and argue that this decline is due to the lower quality of Adams-eligible schools, which tend to exhibit lower completion rates and which have lower expenditures for student instruction than do ineligible colleges. Cohodes and Goodman (2014) also find that the scholarship leads to a statistically insignificant increase in community college enrollment within two years of high school.

Based on these effects, the impact of the scholarship on lifetime earnings is captured by (a) increases in years of schooling for those induced to attend a community college as a result of the scholarship, and (b) decreases in years of schooling for those who switch to an eligible four-year college but who subsequently fail to graduate. For simplicity, we assume that the returns to education remain identical across institutions. We translate these effects into years of schooling and the results imply that providing an additional Adam's scholarship results in 0.018 years of reduced schooling.

We translate these effects on years of schooling into effects on earnings using estimates from Zimmerman (2014). In particular, we use the results from Zimmerman to estimate an increase in earnings in years 1-7 after enrollment and then an decline in earnings over the rest of the lifecycle. (Zimmerman (2014) finds a decline in earnings in years 1-7 and then a subsequent earnings gain in years 8-14. This program is estimated to decrease attainment and so the effects operate in the opposite direction. Earnings effects beyond year 14 are estimated using the projection method outlined in Appendix I.) We calculate the fiscal externality at 20% over the bulk of the lifecycle. These figures comes from our calculation of the effective marginal tax rates based on estimates from the Congressional Budget Office. (Details of our approach can be found in Appendix G.) This procedure leads to an overall decline in lifetime post-tax earnings of $966 when discounted back to age 18 at a 3-percent rate.

In calculating changes in post-tax earnings, we also incorporate changes in private schooling costs as a result of the scholarship. We find that switching to Adam's four year schools causes private contributions to fall, while enrolling in a two-year college causes private contributions to rise. Together, we find that this net decline in school expenditures increases willingness to pay by $1201.

For those who receive the Adams scholarship but are not induced to change their behavior (i.e. for those whose enrollment/graduation outcomes are unchanged), willingness to pay is simply the value of the transfer. This yields an average WTP of $1129 for each scholarship recipient whose behavior is unchanged. Total willingness to pay is given by the sum of WTP for those induced to change their behavior and WTP for those who are not induced to do so; this yields a baseline WTP for the Adams scholarship of $1364 per recipient.

Initial program costs are given by the size of the Adams scholarship transfer. This yields an average direct cost of $1314 per recipient in present-discounted terms. Next, we account for additional changes in government costs caused by changes in enrollment – specifically, a) cost increases due to additional two-year enrollment, b)cost savings due to four-year students switching to less expensive Adams-eligible schools and c) the decline in graduation among four-year students. Following the approach of Zimmerman (2014), we calculate government costs per full time enrollee based on data from the Delta Cost Project. (Appendix B.VIII outlines the details of this method.) We assume that governments cover all educational expenses except net tuition and fees paid by the students. These calculations yield a decrease in government costs of $344 per student. We also account for the changes in taxes paid and transfers received based on the earnings changes calculated in the willingness to pay section (a decline of $238 on average for those induced to change their behavior). Together, when combined with direct program costs, this gives a total net cost of $1896.

Combining these estimates, we get an MVPF of 0.72. To obtain the confidence intervals, we bootstrap the enrollment, earnings, and tax revenue outcomes. We assume (conservatively) these estimates are perfectly correlated across bootstrap iterations. This leads to a confidence interval of $[-0.92, 3.13]$.

We also consider a specification where the scholarship is valued at the cost of the transfer rather than based on the change in long-term earnings. This conservative willingness to pay method ignores any effect from increases in earnings. Instead, it applies the envelope theorem to those induced to get more schooling and assumes they are indifferent to the expenditure. Individuals who do not change their behavior as a result of the scholarship value it as a dollar-for-dollar transfer. The resulting MVPF for this alternate specification is 0.60 with a 95% confidence interval of $[0.45, 0.79]$.

B.IV.5 Wisconsin Scholarships

Our baseline specification for the Wisconsin Scholar Grant has MVPF of 1.43 with a 95% confidence interval of $[1.01, 2.36]$. This Appendix walks through the construction of this estimate.

The Wisconsin Scholar Grant (WSG) is a privately funded grant program that provides low-income students $3,500 dollars in aid per year for a maximum of five years. Wisconsin residents are eligible for the WSG if they have qualified for a federal Pell grant (after filling out the FAFSA) and matriculated to a public state university within three years of graduating from a state's public high school. Eligible public Wisconsin institutions provide a limited set of offers through a randomized lottery, after which enrolled individuals receive funding in successive years by maintaining Pell eligibility and full-time enrollment in public universities or two-year colleges.

(Goldrick-Rab et al., 2016) use the randomization of the WSG offers to study its impact on degree completion and college persistence for 2008-2010 cohorts, the first three years of the program. They estimate the impact of being randomly assigned an offer on college credits separately for the first cohort (2008) and the latter two cohorts. They find an offer of WSG led to an average increase of 0.9 credits earned over the period of study for the first cohort and 2.1 credits earned amongst the second and third cohorts. Three years of college enrollment was observed after the WSG offer to the first two cohorts, but unfortunately the third cohort was only observed for two years, which means that the cumulative effects were calculated over different lengths of time.

To calculate the willingness to pay, we transform the total credits into additional years of schooling and then use estimates from Zimmerman (2014) to estimate the impact on lifetime earnings, using the fact that the average parental income in the sample is reported to be $29,918 for the first cohort.[10] We use the results from Zimmerman to estimate a decline in earnings in years 1-7 after enrollment and then an increase in earnings over the rest of the lifecycle. Zimmerman (2014) observes earnings gains in years 8-14 and we project those gains over the rest of the lifecycle using the approach in Appendix I. We calculate the fiscal externality at 20% over the bulk of the lifecycle. This figure comes from our calculation of the effective marginal tax rates based on estimates from the Congressional Budget Office. Details of our approach can be found in Appendix G. We subtract out the individual contribution to schooling, which is determined by the expected family contribution. Goldrick-Rab et al. (2016) report that the expected family contribution for the first cohort of students is $1,631. The projected earnings gains provide the willingness to pay for all individuals induced to change their behavior and receive more education as a result of the grants. We calculate total willingness to pay by summing those earnings gains with the simple value of the transfer for the fraction of individuals not induced to change their behavior. We estimate this induced fraction to be the ratio between the cohort-weighted average credit effect divided by the sum of the control mean of total credits and the credit effect. The resulting WTP is $9016.

(Goldrick-Rab et al., 2016) report the fraction of take-up of the grant through six semesters, which we use, along with the annual grant value ($3500), to find the present-discounted direct program cost. We also calculate the cost of increased educational attainment, net of the effective family contribution, under the assumption that grant recipients did not switch to attending two-year colleges over the course of the program. We calculate government costs per full time enrollee based on data from the Delta Cost Project. Appendix B.VIII outlines the details of this method. We also account for the changes in taxes paid and transfers received based on the earnings gain calculated in the willingness to pay section. Those effects reduce net costs by $780, and, when combined with the additional educational expenditures of $704 and the direct program cost of $6,366, a total net cost of $6289.

Combining these estimates, we get an MVPF of 1.43. To obtain the confidence intervals, we bootstrap the enrollment, earning and tax revenue outcomes. We assume (conservatively) these estimates are perfectly correlated across bootstrap iterations. This leads to a 95% confidence interval of $[1.01, 2.36]$.

We also consider a specification where the scholarship is valued at the cost of the transfer rather than based on the change in long-term earnings. This conservative willingness to pay method ignores any effect from increases in earnings. Instead, it applies the envelope theorem to those induced to get more schooling and assumes they are indifferent to the expenditure. All individuals who do not change their behavior as a result of the scholarship value it as a dollar-for-dollar transfer. The resulting MVPF for this alternate specification is 0.98 with a 95% confidence interval of $[0.90, 1.09]$.

B.IV.6 Georgia HOPE

Our baseline specification for the provision of the Georgia HOPE Scholarship has MVPF of 4.0 with a 95% confidence interval of $[0.73, 22.55]$. This Appendix walks through the construction of this estimate.

The Georgia HOPE program provides is a lottery-funded state program that provides higher educational funding through merit-based scholarships. The scholarships cover tuition, fees, and book-related expenses for public colleges and provides a fixed payment subsidy for private colleges (initially set at $500 in 1993, but increase to $1000 in 1994, $1500 in 1995, and $3000 in 1996). All Georgia high school students are eligible if they graduate with at least a B average.

Cornwell et al. (2006) look at the impact of the Georgia HOPE scholarship on first-time freshman enrollment by using a difference-in-difference design, comparing against control group states that do not implement the state program. They look at college enrollment from 1993, when the scholarship began, until 1997. They estimate a 8.6 percent increase in enrollment at four-year public colleges (with a reported t-statistic of 2.61), a 13.2 percent increase in enrollment for four-year private colleges (reported t-statistic: 5.62) and a 4.5 percent decrease in public community college enrollment (reported t-statistic: 0.6). That said, the paper estimates that some portion of these individuals were students who would have enrolled out of state in the absence of the program. We calculate the net number of new enrollees and use in our estimation.

To calculate the willingness to pay, we transform the enrollment effects into additional years of schooling and then use estimates from Zimmerman (2014) to estimate the impact on lifetime earnings. We use the results from Zimmerman to estimate a decline in earnings in years 1-7 after enrollment and then an increase in earnings over the rest of the lifecycle. (Zimmerman (2014) observes earnings gains in years 8-14 and we project those gains over the rest of the lifecycle using the approach in Appendix I.) We calculate the fiscal externality using a tax rate

[10]Note that no inflation adjustment was mentioned in the paper.

of 18.9% over the bulk of the lifecycle. This figure comes from our calculation of the effective marginal tax rates based on estimates from the Congressional Budget Office. Details of our approach can be found in Appendix G. The projected earnings gains provide the willingness to pay for all individuals induced to change their behavior and receive more education as a result of the grants. We calculate total willingness to pay by summing those earnings gains with the simple value of the transfer for the fraction of individuals not induced to change their behavior across the enrollment effects for the three types of postsecondary education. We calculate the effect on earnings per HOPE recipient. The resulting WTP is $5766.

We estimate the direct program costs to be $1849 by taking the annual average of aid disbursed per recipient for the 1993-1999 reported in Cornwell et al. (2006). We also calculate the cost of increased educational attainment, net of private tuition payments, under the assumption that grant recipients did not switch between different forms of postsecondary education over the course of the program. We calculate government costs per full time enrollee based on data from the Delta Cost Project. Appendix B.VIII outlines the details of this method. We also account for the changes in taxes paid and transfers received based on the earnings gain calculated in the willingness to pay section. This results in a fiscal externality of $1038, and, when combined with the educational expenditures of $632 from enrollment, a total net cost of $1443.

Combining these estimates, we get an MVPF of 4.0. To obtain the confidence intervals, we bootstrap the enrollment, earning and tax revenue outcomes. This leads to a 95% confidence interval of $[0.73, 22.55]$

We also consider a specification where the scholarship is valued at the cost of the transfer rather than based on the change in long-term earnings. This conservative willingness to pay method ignore any effect from increases in earnings. Instead, it applies the envelope theorem to those induced to get more schooling and assumes they are indifferent to the expenditure. All those do not change their behavior as a result of the scholarship value it as a dollar-for-dollar transfer. The resulting MVPF for this alternate specification is 1.11 with a 95% confidence interval of $[0.67, 3.30]$.

B.IV.7 Ohio Pell paper

Our baseline specification for the provision of Ohio Pell Grants has MVPF of 2.49 with a 95% confidence interval of $[1.00, 5.52]$. This Appendix walks through the construction of this estimate.

Bettinger (2004) studies the effect of the provision of Pell Grants on student retention in Ohio public colleges for the cohort of students who filed a FAFSA between 1999-2000 and attended college for the first time in that school year.[11]The identification strategy imputes the expected Pell Grant aid in 2000-2001 using family and financial information from 1999-2000, which limits variation in aid from Pell Grants to formula-based changes arising from increases in family size, tuition, or sibling college attendance between the two academic years. Bettinger (2004) estimate that a $1000 increase in Pell Grant aid leads to a 6.4 (s.e.: 0.3) percentage point decrease in the likelihood that the student withdraws from a four-year college. The estimates capture the effects of Pell Grants conditional on initial enrollment and therefore does consider the effects of the federal program on initial enrollment.

To calculate the willingness to pay, we transform the effect of Pell grants on withdrawal into additional years of schooling by assuming that individuals who did not withdraw complete one additional year of college education on average. We then use estimates from Zimmerman (2014) to estimate the impact on lifetime earnings, using the average family income of dependents who receive Pell grants in 1998-1999 as the parental income, $21,863, as approximating parental income for the sample. We use the results from Zimmerman to estimate a decline in earnings in years 2-7 from college enrollment and then an increase in earnings over the rest of the lifecycle. (Zimmerman (2014) observes earnings gains in years 8-14 and we project those gains over the rest of the lifecycle using the approach in Appendix I.) Relative to other earnings forecasts, the decline in earnings starts later because the population studied are students who have already entered college and have an average age of 18.8. We calculate the fiscal externality at 20.0% over the bulk of the lifecycle. This figure comes from our calculation of the effective marginal tax rates based on estimates from the Congressional Budget Office. Details of our approach can be found in Appendix G. We subtract out the individual cost contribution, which is determined by the effective family contribution that we take to be the the nationwide average family effective family contribution for all Pell grant dependents for 1998-1999. The projected earnings gains provide the willingness to pay for all individuals induced to change their behavior and receive more education as a result of the grants. We calculate total willingness to pay by summing those earnings gains with the simple value of the transfer for the fraction of individuals not induced to change their behavior. The resulting WTP is $2879.

Bettinger (2004) reports all effects in terms of a $1000 increase in Pell Grant aid, so we normalize the direct

[11]Marx and Turner (2018) also studies Pell Grants and Appendix B.IV.10 on that program provides more background on Federal Pell Grants.

cost of Pell Grants to \$1000. We then add the cost of increased educational attainment, net of the effective family contribution. We calculate government costs per full time enrollee in Ohio based on data from the Delta Cost Project. Appendix B.VIII outlines the details of this method. We also account for the changes in taxes paid and transfers received based on the earnings gain calculated in the willingness to pay section. This results in a fiscal externality of \$530, and, when combined with the educational expenditures from decreased withdrawals of \$686, a total net cost of \$1156.

Combining these estimates, we get an MVPF of 2.49. To obtain the confidence intervals, we bootstrap the enrollment, earning and tax revenue outcomes. This leads to a confidence interval of $[1.00, 5.52]$.

We also consider several alternate specifications for our MVPF calculation. We consider a specification where the scholarship is valued at the cost of the transfer rather than based on the change in long-term earnings. This conservative willingness to pay method ignore any effect from increases in earnings. Instead, it applies the envelope theorem to those induced to get more schooling and assumes they are indifferent to the expenditure. All those do not change their behavior as a result of the scholarship value it as a dollar-for-dollar transfer. The resulting MVPF for this alternate specification is 0.80 with a 95% confidence interval of $[0.62, 1.15]$. We also consider an alternate specification that is less conservative with regard to government educational costs. In particular, we assume that the government's contribution to educational expenditures at private colleges is limited to the Pell Grant. This leads to an MVPF of 0.83 [0.65,1.20] .

B.IV.8 DCTAG

Our baseline specification for the provision of the District of Columbia Tuition Assistance Grant (DCTAG) has MVPF of 22.97 with a 95% confidence interval of $[1.86, \infty]$. This Appendix walks through the construction of this estimate.

The District of Columbia Tuition Assistance Grant (DCTAG) program was established in 1999 to subsidize the tuition and fees of D.C. high school graduates that attend public institutions outside of the capital. It provided a subsidy to graduates equal to the difference between the in-state and out-of-state tuition rate, which de facto DC residents give in-state status at eligible public institutions. The maximum benefit a that students receive is \$10,000 per year enrolled (\$50,000 over their lifetime). The program initially had eligibility to public institutions in Virginia and Maryland in the first year, but later expanded to include all public postsecondary institutions nationwide. The program also allowed for subsidies for students to attend private colleges in DC or historically black colleges and universities, with a maximum benefit of \$2,500 per year (\$12,500 over their lifetime). To be eligible, for the program, individuals must have been DC residents for at least 12 months before college enrollment, graduated from HS (or obtained a GED) after 1997, and must begin undergraduates studies within three years of graduation.

Abraham and Clark (2006) study the impact of the DCTAG program on the enrollment decisions of DC residents. They provide a difference-in-difference estimate of the change in the net enrollment of DC residents (measured as a of 17-year-olds) after the rollout of the program (1998-2002) relative to comparison states[12] of 8.9%.[13] Enrollment effects were only estimated in even years. As a result, we assume this percentage increase in enrollment reflects the increased enrollment in the odd years following the establishment of DCTAG as well.

To calculate the willingness to pay, we transform these enrollment effects into additional years of schooling and then use estimates from Zimmerman (2014) to estimate the impact on lifetime earnings. In our baseline specification we assume that each enrollee leads to two additional years of schooling. We use the results from Zimmerman to estimate a decline in earnings in years 1-7 after enrollment and then an increase in earnings over the rest of the lifecycle. (Zimmerman (2014) observes earnings gains in years 8-14 and we project those gains over the rest of the lifecycle using the approach in Appendix I.) We calculate the fiscal externality at 18.9% over the bulk of the lifecycle, based on estimates from the Congressional Budget Office. Details of our approach can be found in Appendix G. For the fraction of individuals induced to enroll we subtract off their private contribution to schooling expenditures, determined by the cost in each state (discussed further below). The projected earnings gains provide the willingness to pay for all individuals induced to change their behavior and receive more education as a result of the grants. We calculate total willingness to pay by summing those earnings gains with the simple value of the transfer for the fraction of individuals not induced to change their behavior. The resulting WTP from this procedure is \$8959.

We estimate the direct program costs based on the average subsidy received and an assumed number of years of enrollment. This yields a value of \$1176.33. Increases in the years of education produce additional costs for

[12]The set of comparison states used is Maryland, Virginia, Connecticut, Delaware, New Jersey, North Carolina, and Pennsylvania

[13]Note that there were no standard errors provided for this estimate in the paper.

the government through the costs of college. However, the costs of college vary by state and the DCTAG-eligible residents attended postsecondary programs across various states. We produce a measure of the average costs of college and average tuition by using the tabulation of fall enrollment for DC high school graduates by school. Following the approach of Zimmerman (2014), we calculate the governments costs per full time enrollee for each state in which a DC resident enrolled in public university based on data from the Delta Cost Project. Appendix B.VIII outlines the details of this method. The supplemental costs of enrollment are $1297.79. After accounting for the increased tax revenue of $2084 from the additional years of education, we estimate the net cost of the program to be $390.

Combining these estimates, we get an MVPF of 22.97. To obtain the confidence intervals, we bootstrap the enrollment, earning and tax revenue outcomes. This leads to a 95% confidence interval of $[1.86, \infty]$.

We also consider a specification where the scholarship is valued at the cost of the transfer rather than based on the change in long-term earnings. This conservative willingness to pay method ignore any effect from increases in earnings. Instead, it applies the envelope theorem to those induced to get more schooling and assumes they are indifferent to the expenditure. All those do not change their behavior as a result of the scholarship value it as a dollar-for-dollar transfer. The resulting MVPF for this alternate specification is 3.02 with a 95% confidence interval of $[0.72, \infty]$.

B.IV.9 State Budget Cuts, Education Spending, and Tuition Reductions

Public postsecondary institutions are partially financed by state appropriations, which vary in their share of total institutional and may fluctuate due to statewide budget cuts (in response to, for example, economic downturns). Deming and Walters (2018) use the differential exposure of public postsecondary institutions to statewide budget cuts, measured as the share of total revenue coming from state appropriations, as an instrument to study the impact of education spending and tuition changes on college enrollment and degree attainment between 1990-2013 nationwide. We discuss how to use their estimates to determine the MVPF of a decrease in tuition or an increase in spending by postsecondary institutions.

National spending Our baseline specification for national spending increases has MVPF of 4.00 with a 95% confidence interval of $[1.51, 19.65]$. This Appendix walks through the construction of this estimate.

Using their exposure design, Deming and Walters (2018) estimate that a 1% increase in education spending by a public postsecondary institutions increases enrollment by 0.304% in the current year, 0.796% one year later, 0.845% two years later, and 0.830% three years later across all public postsecondary institutions in the sample. All of these effect sizes are significant at the 5% level at least. We sum these changes and take them to be the cumulative impact of increased spending on additional years of education (where an increase in enrollment counts as one additional year of education). The authors note in an earlier version of the draft (Deming and Walters, 2017), that budget shocks are serially correlated, with a AR(1) persistence coefficient estimated to be 0.55. Because we are looking at the cumulative impact 4 years from the shock, we scale the impact on additional years of school by $\sum_{i=0}^{3} 0.55^i = 2.02$ to account for the shock persistence.

We then use estimates from Zimmerman (2014) to estimate the impact on lifetime earnings. We use the results from Zimmerman to estimate a decline in earnings in years 1-7 after enrollment and then an increase in earnings over the rest of the lifecycle. (Zimmerman (2014) observes earnings gains in years 8-14 and we project those gains over the rest of the lifecycle using the approach in Appendix I.) We calculate the fiscal externality at 18.9% over the bulk of the lifecycle. This figure comes from our calculation of the effective marginal tax rates based on estimates from the Congressional Budget Office. Details of our approach can be found in Appendix G. For the fraction of individuals induced to enroll, we subtract off the private contribution, estimated to be the net tuition for public colleges in the US during 2001 (the midpoint between the period of study). The projected earnings gains provide the willingness to pay for all individuals induced to change their behavior and acquire additional years of educations as a result of the institutional spending increase. We calculate total willingness to pay by summing those earnings gains with the simple value of the transfer for the fraction of individuals not induced to change their behavior. The resulting WTP from this procedure is $3,173.

We normalize the change in spending to be $1000 per enrollee, so the direct program costs is the sum of the mechanical increase in per-pupil spending and the behavioral response due to change percentage increase in enrollment, yielding an estimate of $1046. Increasing the years of public postsecondary education results in additional fiscal impact through the costs of college. To incorporate these costs, we use the estimated cumulative enrollment effects for community colleges and four-year programs (calculated and scaled in the same way as the pooled estimate above) and calculate the governments costs per each type of enrollee on data from the Delta Cost

Project. Appendix B.VIII outlines the details of this method. We then take an average of these costs, weighted by the enrollment levels reported in Deming and Walters (2018). We subtract from this average college cost the private contribution in the form of net tuition for induced enrollees, where the net tuition is calculated in the same manner as the costs of college. This gives us a total enrollment cost of \$306. After accounting for the increased tax revenue of \$560 from the additional years of education, we estimate the net cost of the program to be \$793.

Combining these estimates, we get an MVPF of 4.00 To obtain the confidence intervals, we bootstrap the enrollment, earning and tax revenue outcomes. This leads to a 95% confidence interval of $[1.51, 19.65]$

We consider a specification where the spending is valued at the cost of the transfer rather than based on the change in long-term earnings. This conservative willingness to pay method ignore any effect from increases in earnings. Instead, it applies the envelope theorem to those induced to get more schooling and assumes they are indifferent to the expenditure. All those do not change their behavior as a result of the scholarship value it as a dollar-for-dollar transfer. The resulting MVPF for this alternate specification is 1.26 with a 95% confidence interval of $[0.90, 3.22]$. We also consider an alternative specification in which we use the college costs and tuition estimates that are provided byDeming and Walters (2018), which include non-school expenditures (e.g., scholarships). The resulting MVPF for this alternate specification is 2.11 with a 95% confidence interval of $[0.83, 5.21]$.

National Tuition Now we explain how we calculate the MVPF for a decrease in the tuition of postsecondary education using the estimates from the Deming and Walters (2018) exposure design. Our methodology is nearly identical to the MVPF for national spending above .Our baseline specification has MVPF of 1.02 with a 95% confidence interval of $[-1.02, 5.60]$.

Using their exposure design, Deming and Walters (2017) estimate that a 1% *decrease* in tuition for a public postsecondary institutions increases enrollment by -0.016% in the current year, 0.066% one year later, 0.031% two years later, and -0.073% three years later across all public postsecondary institutions in the sample. None of these estimated effect sizes are significant. We sum these changes and take them to be the cumulative impact of increased spending on additional years of education (where an increase in enrollment counts as one additional year of education). The authors note in an earlier version of the draft (Deming and Walters, 2017), that budget shocks are serially correlated, with a AR(1) persistence coefficient estimated to be 0.55. Because we are looking at the cumulative impact 4 years from the shock, we scale the impact on additional years of school by $\sum_{i=0}^{3} 0.55^i = 2.02$ to account for the shock persistence.

We then use estimates from Zimmerman (2014) to estimate the impact on lifetime earnings. We use the results from Zimmerman to estimate a decline in earnings in years 1-7 after enrollment and then an increase in earnings over the rest of the lifecycle. (Zimmerman (2014) observes earnings gains in years 8-14 and we project those gains over the rest of the lifecycle using the approach in Appendix I.) We calculate the fiscal externality at 18.9% over the bulk of the lifecycle. This figure comes from our calculation of the effective marginal tax rates based on estimates from the Congressional Budget Office. Details of our approach can be found in Appendix G. Because there is no information related to income background of the nationwide sample, we use the mean earnings of the forecasting sample for high school students in estimating the impact of earnings. For the fraction of individuals induced to enroll, we subtract off the private contribution, estimated to be the net tuition for public colleges in the US during 2001 (the midpoint between the period of study). The projected earnings gains provide the willingness to pay for all individuals induced to change their behavior and acquire additional years of educations as a result of the institutional spending increase. We calculate total willingness to pay by summing those earnings gains with the mechanical value of the transfer for the fraction of individuals not induced to change their behavior. The resulting WTP from this procedure is \$1,021.

We normalize the change in tuition to be \$1000 per enrollee, so the direct program costs is the mechanical decrease in tuition and the behavioral response due to change percentage increase in enrollment, yielding an estimate of \$1,001. Increasing the years of public postsecondary education results in additional fiscal impact through the costs of college. To incorporate these costs, we use the estimated cumulative enrollment effects for community colleges and four-year programs (calculated and scaled in the same way as the pooled estimate above) and calculate the governments costs per each type of enrollee on data from the Delta Cost Project. Appendix B.VIII outlines the details of this method. We then take an average of these costs, weighted by the enrollment levels reported in Deming and Walters (2018). We subtract from the average college cost the private contribution in the form of net tuition for induced enrollees, where the net tuition is calculated in the same manner as the costs of college. We also deduct the \$1000 per-enrollee tuition decrease for the newly enrolled. This gives us a total enrollment cost of \$4. After accounting for the increased tax revenue of \$6 from the additional years of education, we estimate the net cost of the program to be \$999.

Combining these estimates, we get an MVPF of 1.02. To obtain the confidence intervals, we bootstrap the enrollment, earning and tax revenue outcomes. This leads to a 95% confidence interval of $[-1.02, 5.60]$.

We consider a specification where the tuition deduction is valued at the cost of the transfer rather than based on the change in long-term earnings. This conservative willingness to pay method ignore any effect from increases in earnings. Instead, it applies the envelope theorem to those induced to get more schooling and assumes they are indifferent to the expenditure. All those do not change their behavior as a result of the scholarship value it as a dollar-for-dollar transfer. The resulting MVPF for this alternate specification is 1.0 with a 95% confidence interval of $[0.79, 1.63]$. We also consider an alternative specification in which we use the college costs and tuition estimates that are provided by Deming and Walters (2018), which include non-school expenditures (e.g., scholarships). This leads to an MVPF of 1.01 with a 95% confidence interval of $[-1.28, 3.10]$.

B.IV.10 Pell Grants for Adults

Our baseline specification for the provision of Pell Grants to adults (students age 22-35) has an MVPF of 2.18 with a 95% confidence interval of $[0.58, 7.55]$. This Appendix walks through the construction of this estimate.

Pell Grants provide financial support to low-income students attending college, including "independent students" (those not financially dependent on parents or guardians). Seftor and Turner (2002) provide the primary causal estimates for this analysis. They study the introduction of Pell Grants in 1972, implementing a difference-in-differences analysis using data from the Current Population Survey (CPS). They estimate the effect of Pell Grants on college enrollment among individuals ages 22 to 35 by comparing presumably eligible students (as determined by CPS data on income and family structure) before (1969-1972) and after (1974-1977) the program's creation, using presumably ineligible individuals as a control group.

Because Seftor and Turner (2002) only report estimates on college enrollment at a particular point in time, we must make assumptions regarding (a) the baseline share of Pell-eligible students who actually received the grants, and (b) how point-in-time enrollment translates into additional years of education. In our primary estimates, we assume, to be conservative, that all Pell-eligible enrollees received the grant at baseline, and that enrollment translates into two additional years of schooling.

For willingness to pay, we thus take the enrollment effects in Seftor and Turner (2002), translate them into additional years of schooling, and then use estimates from Zimmerman (2014) to estimate the impact on earnings. In particular, we use the results from Zimmerman to estimate a decline in earnings in years 1-7 after enrollment and then an increase in earnings over the rest of the lifecycle. (Zimmerman (2014) observes earnings gains in years 8-14 and we project those gains over the rest of the lifecycle using the approach in Appendix I.) We calculate the fiscal externality using a tax rate of 19.9% over the bulk of the lifecycle. This figure comes from our calculation of the effective marginal tax rates based on estimates from the Congressional Budget Office. (Details of our approach can be found in Appendix G.) Because of the time period and population considered by Seftor and Turner (2002), we assume conservatively that Pell recipients do not contribute out-of-pocket towards college costs. The projected earnings gains provide the willingness to pay of $168 for all individuals induced to change their behavior and receive more education as a result of the grants. We calculate total willingness to pay by summing those earnings gains with the simple value of the transfer for the fraction of individuals not induced to change their behavior. This produces a total willingness to pay of $238.

The treatment in this case is eligibility for the Pell Grant, and the initial program costs are $87. Next, we account for the additional costs due to increased educational attainment on the part of those induced to enroll by the creation of Pell Grants. Because the time period considered by Seftor and Turner (2002) falls before the earliest data from the Delta Cost Project, we use data from the National Center for Education Statistics to compute average education and related expenditures per full-time equivalent student for all institutions in the United States between 1974 and 1977. This adds $70 to net costs. Finally, we account for the changes in taxes paid and transfers received based on the earnings gains calculated in the willingness to pay section. On a per-capita (i.e. per-22-to-35-year-old) basis, this results in a fiscal externality of $48 and, when combined with the educational expenditures, a total net cost of $109.

Combining these estimates, we get an MVPF of 2.18. To obtain the confidence intervals, we bootstrap the enrollment, earning and tax revenue outcomes. We assume (conservatively) these estimates are perfectly correlated across bootstrap iterations. This leads to a 95% confidence interval of $[0.58, 7.55]$.

We also consider a specification where the scholarship is valued at the cost of the transfer rather than based on the change in long-term earnings. This conservative willingness to pay method ignore any effect from increases in earnings. Instead, it applies the envelope theorem to those induced to get more schooling and assumes they are indifferent to the expenditure. All those do not change their behavior as a result of the scholarship value it as a dollar-for-dollar transfer. This leads to an MVPF of 0.64 with a 95% confidence interval of $0.42, 0.96]$.

B.IV.11 Tennessee HOPE Scholarship

Our baseline specification for the Tennessee HOPE scholarship has an MVPF of 1.94 with a 95% confidence interval of $[1.04, 5.42]$. This Appendix walks through the construction of this estimate.

The Tennessee HOPE scholarship – which provides up to $6,000 per year for full-time enrollment at a four-year school and up to $3,000 per year for enrollment at a two-year school – is available to any Tennessee student, regardless of financial need, who (a) meets either a minimum grade point average threshold or a minimum ACT score threshold, and (b) attends an in-state non-profit school (whether public or private). Work by Bruce and Carruthers (2014) provides the primary causal estimates for this analysis. They examine the impact of the scholarship using a regression discontinuity design that examines students just above and just below the ACT score threshold for eligibility. Bruce and Carruthers (2014) document no significant change in overall enrollment at the ACT-score threshold for HOPE, but find evidence of shifts in enrollment from public two-year and private schools into public four-year schools among those at the eligibility threshold.

The full impact of the scholarship for those at the eligibly threshold is captured by the reduction in two-year public school enrollment and the resulting shift into four-year schooling, since these are the only individuals who see any earnings gains and who affect the government budget (via their additional schooling). (We assume in our baseline specification that shifting between public and private schools does not impact the government budget.) In their data, Bruce and Carruthers do not observe actual HOPE receipt, but observe an imperfect measure of HOPE eligibility. As such, we scale their first-stage result (for the marginal effect on HOPE eligibility of crossing the ACT threshold) by the share of students in their sample attending a HOPE-eligible institution (0.78) to get an implied take-up rate.

For willingness to pay, we thus take the enrollment-shift effects in Bruce and Carruthers (2014), translate them into additional years of schooling. We find that eligibility raises schooling by 0.022 years. We then use estimates from Zimmerman (2014) to estimate the impact on earnings. In particular, we use the results from Zimmerman to estimate a decline in earnings in years 1-7 after enrollment and then an increase in earnings over the rest of the lifecycle. (Zimmerman (2014) observes earnings gains in years 8-14 and we project those gains over the rest of the lifecycle using the approach in Appendix I.) We calculate the fiscal externality using a tax rate of 20.0% over the bulk of the lifecycle. This figure comes from our calculation of the effective marginal tax rates based on estimates from the Congressional Budget Office. (Details of our approach can be found in Appendix G.) The projected earnings gains increase willingness to pay by $1008 for all individuals induced to change their behavior and receive more education as a result of the scholarship. We also subtract out individual contributions to tuition for those induced to switch from two- to four-year schools as a result of the scholarship. We assume that this is equal to 24% of net tuition and fees for Tennessee schools based on an approximation regarding the generosity of the scholarship. When we scale this impact by the enrollment effect we calculate a $26 change in willingness to pay. We then calculate total willingness to pay by summing those earnings gains with the simple value of the transfer for the fraction of individuals not induced to change their behavior. Total willingness to pay equals $1832.

The treatment in this case is eligibility for the HOPE scholarship, and the initial program costs are set to the average total HOPE amount received scaled by the expected HOPE take-up rate for individuals crossing the RD threshold. That equals $970 in program costs. Next, we account for the additional costs due to increased educational attainment on the part of those induced to switch to four-year schools. Following the approach of Zimmerman (2014), we calculate government costs per full time enrollee based on data from the Delta Cost Project. (Appendix B.VIII outlines the details of this method.) This adds $243 to net costs. Next, we account for the changes in taxes paid and transfers received based on the earnings gains calculated in the willingness to pay section. This results in a fiscal externality of $267, and, when combined with the educational expenditures, a total net cost of $946 per student at the threshold.

Combining these estimates, we get an MVPF of 1.94. To obtain the confidence intervals, we bootstrap the enrollment, earnings, and tax revenue outcomes. We assume (conservatively) these estimates are perfectly correlated across bootstrap iterations. This leads to a 95% confidence interval of $[1.04, 5.42]$.

We also consider a specification where the scholarship is valued at the cost of the transfer rather than based on the change in long-term earnings. This conservative willingness to pay method ignore any effect from increases in earnings. Instead, it applies the envelope theorem to those induced to get more schooling and assumes they are indifferent to the expenditure. All those do not change their behavior as a result of the scholarship value it as a dollar-for-dollar transfer. The resulting MVPF for this alternate specification is 0.94 with a 95% confidence interval of $[0.69, 1.11]$.

B.IV.12 Kalamazoo Promise

Our baseline specification for the Kalamazoo Promise program has an MVPF of 1.93 with a 95% confidence interval of [0.88, 4.76]. This Appendix walks through the construction of this estimate.

The Kalamazoo Promise is a place-based scholarship program that is open to any student graduating from Kalamazoo (Michigan) Public Schools (KPS) who (a) resides in the Kalamazoo school district and (b) has been continuously enrolled in a KPS school since ninth grade. The Promise, a first-dollar scholarship, subsidizes up to 100 percent of tuition and fees at any public two- or four-year college in Michigan, with subsidy levels determined by the length of students' continuous enrollment in KPS. Work by Bartik et al. (2017) provides the primary causal estimates for this analysis. They examine the impact of the scholarship using a difference-in-differences design, comparing college enrollment and completion outcomes among Promise-eligible students before and after the program's introduction in 2005, using ineligible students as a control group. For our estimate, we use the result in Bartik et al. (2017), Table 3, that the Promise scholarship increased students' attempted college credits by 6.56 credits within four years.

For willingness to pay, we take these credit effects, translate them into additional years of schooling, and then use estimates from Zimmerman (2014) to estimate the impact on earnings. In particular, we use the results from Zimmerman to estimate a decline in earnings in years 1-7 after enrollment and then an increase in earnings over the rest of the lifecycle. (Zimmerman (2014) observes earnings gains in years 8-14 and we project those gains over the rest of the lifecycle using the approach in Appendix I.) We calculate the fiscal externality at 18.9% over the bulk of the lifecycle. This figure comes from our calculation of the effective marginal tax rates based on estimates from the Congressional Budget Office. (Details of our approach can be found in Appendix G.) The projected earnings gains increase willingness to pay by $15,566 for all individuals induced to change their behavior and receive more education as a result of the scholarship. We calculate total willingness to pay as equal to $31,012 by summing those earnings gains with the simple value of the transfer for the fraction of individuals not induced to change their behavior.

Initial program costs are set to the average present-valued Promise scholarship amount of $17,620 cited in Bartik et al. (2016), to which we add the administrative costs of the program (3.6 percent of $17,620, or $634). Next, we account for additional costs due to increased educational attainment on the part of those induced to enroll for additional credits as a result of the scholarship. Following the approach of Zimmerman (2014), we calculate government costs per full time enrollee based on data from the Delta Cost Project. (Appendix B.VIII outlines the details of this method.) This results in additional enrollment costs of $1,668 per eligible student. Finally, we account for the changes in taxes paid and transfers received based on the earnings gains calculated in the willingness to pay section ($3,873 per student). The resulting total net cost of the program is $16,049.

Combining these estimates, we get an MVPF of 1.93. To obtain the confidence intervals, we bootstrap the credits, earnings, and tax revenue outcomes. We assume (conservatively) these estimates are perfectly correlated across bootstrap iterations. This leads to a confidence interval of [0.88, 4.76].

We consider a specification where the scholarship is valued at the cost of the transfer rather than based on the change in long-term earnings. This conservative willingness to pay method ignore any effect from increases in earnings. Instead, it applies the envelope theorem to those induced to get more schooling and assumes they are indifferent to the expenditure. Individuals who do not change their behavior as a result of the scholarship value it as a dollar-for-dollar transfer. The resulting MVPF for this alternate specification is 0.96 with a 95% confidence interval of [0.81, 1.21]. We also consider an alternate specification where we assume there are additional returns to enrollment in a flagship university. We use results from Hoekstra (2009), suggesting that enrollment at a flagship university raises earnings by 20%. We add that impact to our earnings gains from Kalamazoo and estimate the MVPF to be 2.96 with a 95% confidence interval of [1.01, 12.76].

B.IV.13 Tennessee Pell Grants

Our baseline specification for Pell Grant provision in Tennessee has an MVPF of 0.77 with a 95% confidence interval of $[-1.21, 3.73]$. This Appendix walks through the construction of this estimate.

The Pell Grant provides financial support to students from low-income families attending college. Work by Carruthers and Welch (2019)provides the primary causal estimates for this analysis. They examine the impact of additional Pell Grant provision in the state of Tennessee using a discontinuity in Pell provision for those just above and just below the Expected Family Contribution threshold for Pell eligibility. Carruthers and Welch (2019) use this discontinuity to calculate the impact of Pell eligibility on the enrollment decisions of Tennessee high school graduates. We focus on their regression discontinuity results for enrollment in public and private two- and four-year colleges.

Carruthers and Welch (2019) find evidence of small decreases in enrollment among marginally Pell-eligible students in four-year in-state public universities. They also find small increases in enrollment in two-year schools (both in-state and out-of-state) and in out-of-state and private four-year colleges.

For willingness to pay, we take the total two- and four-year enrollment effects in Carruthers and Welch (2019) and translate them into additional years of schooling. In this case, the reduction in four-year schooling offsets the gains in two-year schooling and the total schooling change is -0.0072 years. We then use estimates from Zimmerman (2014) to estimate the impact on earnings. In particular, we use the results from Zimmerman to estimate a increase in earnings in years 1-7 after enrollment and then the decline in earnings over the rest of the lifecycle. (Zimmerman (2014) observes earnings gains in years 8-14 and we project those gains over the rest of the lifecycle using the approach in Appendix I.) We calculate the fiscal externality at 20% over the bulk of the lifecycle. This figure comes from our calculation of the effective marginal tax rates based on estimates from the Congressional Budget Office. (Details of our approach can be found in Appendix G.) Once taxes and transfers are netted out, the enrollment change reduces post-tax earnings by $335. Willingness to pay amongst the induced group is calculated by taking those earnings changes and adding changes in individual contributions to schooling costs ($27). We calculate total willingness to pay by summing those earnings gains with the simple value of the transfer at the eligibility threshold for the fraction of individuals not induced to change their behavior. Total willingness to pay equals $1111.

Initial program costs are set to the average total Pell amount received by marginally eligible students over the entirety of their time in school ($1,415). Next, we account for the additional changes in costs due to changes in enrollment as a result of the Pell grant. Following the approach of Zimmerman (2014), we calculate government costs per full time enrollee based on data from the Delta Cost Project. (Appendix B.VIII outlines the details of this method.) This decreases costs by $109. Next, we account for the changes in taxes paid and transfers received based on the earnings gains calculated in the willingness to pay section. This results in an increase in costs of $89 per student at the Pell eligibility threshold. Finally, we add the impact on government costs as the Pell Grant induces individuals are induced to attend more expensive schools. This raises costs by $54. Putting these values together results in a total net cost of $1449 per student at the threshold.

Combining these estimates, we get an MVPF of 0.77. To obtain the confidence intervals, we bootstrap the enrollment, earning and tax revenue outcomes. We assume (conservatively) these estimates are perfectly correlated across bootstrap iterations. This leads to a confidence interval of $[-1.21, 3.73]$.

We consider a specification where the scholarship is valued at the cost of the transfer rather than based on the change in long-term earnings. This conservative willingness to pay method ignore any effect from increases in earnings. Instead, it applies the envelope theorem to those induced to get more schooling and assumes they are indifferent to the expenditure. Individuals who do not change their behavior as a result of the scholarship value it as a dollar-for-dollar transfer. The resulting MVPF for this alternate specification is 0.98 with a 95% confidence interval of [0.78, 1.30].

We also consider alternate specifications that alter the cost of the Pell Grant. We include one alternate specification where it is assumed that the additional Pell Grant is only received in one year. That produces an MVPF of 0.34 with a 95% confidence interval of [-4.18, 18.95]). We include another specification where the Pell Grant is received for four years for those enrolled in 4-year schools and the Pell Grant is received two years for those enrolled in two-year schools. That produces an MVPF of 0.73 with a 95% confidence interval of [-1.45, 4.58].

B.V Community College

B.V.1 In-District Tuition in Texas

Our baseline specification for access to in-district tuition for community colleges in Texas has an MVPF of 349.5 with a 95% confidence interval of $[2.16, \infty]$. This appendix walks through the construction of this estimate.

Community colleges in Texas are partially financed through property taxes from the surrounding region. Individuals who live in a district whose property tax revenues support the school are eligible for discounted "in-district tuition". Denning (2017) uses the annexation of municipalities into districts that support community colleges to estimate the impact of reduced sticker price of tuition on enrollment. The effects of reduced tuition are estimated through a differences-in-differences in design in which individuals have access to in-district tuition rates when their municipality is annexed into the property tax base for the community college. They calculate the impact of reduced tuition on enrollment in both community and four-year colleges, degree completion, and credits attempted.

Our primary estimates calculates the willingness to pay for reduced tuition using estimates from Denning (2017) on the additional enrollment effects due to the tuition reduction, augmented with an estimate on degree completion over a horizon that extends beyond the span for which enrollment effects were estimated. We translate the increased enrollment and attainment into additional years of schooling by summing over the enrollment effects for both community and four-year colleges over individuals who have graduated high school within six years prior two enrollment. This provides the attainment effects for six years from high school, which we then add to the increase in four-year college degree attainment between 6 and 8 years from the district annexation. We estimate that providing the tuition deduction increases enrollment per eligible individual by 0.22 years. We then use estimates from Zimmerman (2014) to estimate the impact on lifetime earnings. We use the results from Zimmerman to estimate a decline in earnings in years 1-7 after enrollment and then an increase in earnings over the rest of the lifecycle. (Zimmerman (2014) observes earnings gains in years 8-14 and we project those gains over the rest of the lifecycle using the approach in Appendix I.) We calculate the fiscal externality using a tax rate of 19.9% over the bulk of the lifecycle. This figure comes from our calculation of the effective marginal tax rates based on estimates from the Congressional Budget Office. Details of our approach can be found in Appendix G. The willingness to pay associated with these earnings gains is $8,857. We subtract out the individual contribution to tuition ($407), which consists of the net-of-aid tuition multiplied by the additional years of enrollment at each type of institution. The projected earnings gains provide the willingness to pay for all individuals induced to change their behavior and receive more education as a result of the tuition reduction. We calculate total willingness to pay by summing those earnings gains with the simple value of the change in tuition for the fraction of individuals not induced to change their behavior. We get a value of $9,322.

The treatment in this context is eligibility of in-district tuition rates, and the initial program costs is $1000, the average change in community college tuition for municipalities annexed into community college taxing districts during the period of study scaled by the take-up rate. In addition to the direct program cost, we include the additional costs due to increased educational attainment, calculated separately for community college and four-year programs, for those living in districts with decreased tuition rates. We follow the approach of Zimmerman (2014) and calculate government costs per full time enrollee based on data from the Delta Cost Project. Appendix B.VIII outlines the details of this method. This raises costs by $1,339. Next, we account for the changes in taxes paid and transfers received based on the earnings gains calculated in the willingness to pay section. That results in a fiscal externality of -$2314 and, when combined with the educational expenditures, a total net cost of $26.

Combining these estimates, we get an MVPF of 349.5 . To obtain the confidence intervals, we bootstrap the enrollment, earning and tax revenue outcomes. We assume (conservatively) these estimates are perfectly correlated across bootstrap iterations. This leads to a confidence interval of $[2.16, \infty]$.

We consider alternate approaches to calculating the earnings effect and determining the willingness to pay. In the case of the earnings effect, we create a specification where our earnings projections are based on credits attempted at community college. We transform credits attempted into an increase in years of schooling and then apply the same earnings projection method from Zimmerman (2014) using these schooling increases. In that case we get an MVPF of 9.12 (95% CI: $[1.46, \infty]$). We also have an alternate specification that forecasts the earnings gain in education from community college and bachelor's programs separately. In this case, we use estimates from Mountjoy (2019)to estimate the returns to community college. This produces an MVPF of ∞ (95% CI: $[1.28, \infty]$). Finally, we consider a specification where the scholarship is valued at the cost of the transfer rather than based on the change in long-term earnings. This conservative willingness to pay method ignore any effect from increases in earnings. Instead, it applies the envelope theorem to those induced to get more schooling and assumes they are indifferent to the expenditure. All those that do not change their behavior as a result of the scholarship value it as a dollar-for-dollar transfer. The resulting MVPF for this alternate specification is 32.7 (95% CI: $[0.65, \infty]$).

B.V.2 In-District Tuition in Michigan

Our baseline specification for access to in-district tuition for community colleges in Michigan has an MVPF of29.46 with a 95% confidence interval of $[-2.70, \infty]$. This appendix walks through the construction of this estimate.

Community colleges in Michigan, as in Texas, are partially financed through property taxes that are assessed on properties from the surrounding community college taxing district. Individuals who live in a district whose property tax revenues support the school are eligible for discounted "in-district tuition". Acton (2018)uses the boundary of community college taxing districts to estimate the impact of reduced sticker price of tuition on enrollment. The effects of reduced tuition are estimated through boundary fixed effects regression of college outcomes of students who live just inside a community college district and face in-district tuition. They calculate the impact of reduced tuition on semesters of college completed, enrollment across various post-secondary institutions, and degree completion.

Our primary estimates calculates the willingness to pay for reduced tuition using estimates from Acton (2018) on enrollment and school completion. We translate those estimates into years of schooling (an increase of 0.029 years per in-district high school graduate) and then use estimates from Zimmerman (2014) to estimate the impact on lifetime earnings. We use the results from Zimmerman to estimate a decline in earnings in years 1-7 after enrollment and then an increase in earnings over the rest of the lifecycle. (Zimmerman (2014) observes earnings gains in years 8-14 and we project those gains over the rest of the lifecycle using the approach in Appendix I.) We calculate the fiscal externality using a tax rate of 20% over the bulk of the lifecycle. This figure comes from our calculation of the effective marginal tax rates based on estimates from the Congressional Budget Office. Details of our approach can be found in Appendix G. The willingness to pay associated with these earnings gains is $1683. We subtract out the individual contribution to tuition ($154), which consists of the net-of-aid tuition multiplied by the additional years of enrollment at each type of institution. (Note that the tuition price for community college comes from the average in-district tuition while the tuition for bachelor's programs comes from the Delta Cost Project.) The projected earnings gains provide the willingness to pay for all individuals induced to change their behavior and receive more education as a result of the tuition reduction. We calculate total willingness to pay by summing those earnings gains with the simple value of the change in tuition for the fraction of individuals not induced to change their behavior. We get a value of $1754.

The treatment in this context is eligibility of in-district tuition rates and the initial program costs is $261, the difference in the average tuition rate for in-district and out-of-district residents in Michigan scaled by schooling take-up. In addition to the direct program cost, we include the additional costs due to increased educational attainment, calculated separately for community college and four-year programs, for those living in districts with decreased tuition rates. We follow the approach of Zimmerman (2014) and calculate government costs per full time enrollee based on data from the Delta Cost Project. Appendix B.VIII outlines the details of this method. This increases costs by $243. Next, we account for the changes in taxes paid and transfers received based on the earnings gains calculated in the willingness to pay section. That results in a fiscal externality of -$444 and, when combined with the educational expenditures, a total net cost of $60.

Combining these estimates, we get an MVPF of 29.46. To obtain the confidence intervals, we bootstrap the enrollment, earning and tax revenue outcomes. We assume (conservatively) these estimates are perfectly correlated across bootstrap iterations. This leads to a confidence interval of $[-2.70, \infty]$.

We also consider alternate approaches to calculating the earnings effect and determining the willingness to pay. In the case of the earnings effect, we create a specification where our earnings projections are based on semesters of college completed. We then apply the same earnings projection method from Zimmerman (2014) using these schooling increases. This produces an MVPF of ∞ (95% CI: $[2.92, \infty]$). We also have an alternate specification that forecasts the earnings gain in education from community college and bachelor's programs separately. This uses returns to community college taken from Mountjoy (2019). This produces an MVPF of 54.84 (95% CI: $[-2.63, \infty]$). In the case of willingness to pay, we also consider a specification where the scholarship is valued at the cost of the transfer rather than based on the change in long-term earnings. This conservative willingness to pay method ignore any effect from increases in earnings. Instead, it applies the envelope theorem to those induced to get more schooling and assumes they are indifferent to the expenditure. All those do not change their behavior as a result of the scholarship value it as a dollar-for-dollar transfer. The resulting MVPF for this alternate specification is 3.78 (95% CI: $[0.54, \infty]$).

B.VI College Tax Credits

B.VI.1 American Opportunity Tax Credit

Enacted in 2009 as part of the American Recovery and Reinvestment Act, the American Opportunity Tax Credit (AOTC) expanded the eligibility for tax credits on higher education tuition and fees for both high- and low-income families. The legislation provides tax credits equal to 100% of the first $2,000, plus 25% of the next $2,000 (for a maximum of $2,500), for qualifying postsecondary tuitions and fees for each of the first four years of postsecondary education.

Work by Bulman and Hoxby (2015) provides the causal estimates for this analysis. They use kinks in the phase-out of the AOTC as well as a simulated instruments design to estimate the impact of tax credits on college enrollment. In explaining our MVPF calculation we begin with a detailed description of our approach for one regression kink in Bulman and Hoxby (2015) . We focus on the kink faced by joint filers at the kink at the beginning of the phase out region. We adopt the same approach to examine four other kinks in the tax schedule: the phase-out end for joint filers, the phase-out beginning for single filers, the phase-out end for single filers and the phase out beginning for independent singles filers. (Except for this last group, all other filers have dependents.) We also adopt this same approach to analyze Bulman and Hoxby's (2015) simulated instruments approach focused on the introduction of the AOTC.[14] We find substantial variation in the MVPF point estimates across these various

[14]It is worth noting that while this approach seeks to estimate the impact of credits in the absence of endogenous responses to the presence of credits, those may still be welfare relevant because they produce fiscal externalities. For example, individuals may reduce their income to become eligible for more education tax credits, which leads to a loss in government revenue on top of the tax credit

specifications, but the standard errors are very large. In all cases the top end of our confidence interval includes an infinite MVPF and the bottom end of our confidence interval includes an MVPF below 0.

AOTC for Joint Filers, Beginning of Phase Out Our baseline estimate of the MVPF of AOTC benefits is ∞ for married joint filers at the beginning of phase out region. This section walks through the details of how that estimate is produced.

For married joint filers, the American Opportunity Tax credits begins to phase out \$160,000 and ends at \$180,000. The phase out rate is linear over this interval, which allows Bulman and Hoxby (2015) , to exploit the discontinuous change in the slope of the benefit schedule at both the beginning and end of the phase period to study the impact of tax credits for households with income near these kinks for 2011 tax filers. We describe the calculation in detail here. For the 5 other AOTC MVPFs from Bulman and Hoxby (2015) making use of this design, we only highlight the differences relative to this program.

Bulman and Hoxby (2015) find that at the \$160,000 kink point, a dollar decrease in income leads to a \$0.107 increase in credits claimed by the household, which is statistically significant and close to the expected coefficient 0.125 if all households at the kink qualified for the maximum tax credits. The second stage regression finds that, at the kink point, college attendance decreases by 7×10^{-6} percentage points amongst 19- to 20-year-olds for each dollar of additional income[15].

To calculate the willingness to pay, we transform the effect of college attendance into additional years of schooling by assuming that individuals who enroll complete two additional years of college education on average. We then use estimates from Zimmerman (2014) to estimate the impact on lifetime earnings.We use the results from Zimmerman to estimate a decline in earnings in years 1-7 from college enrollment and then an increase in earnings over the rest of the lifecycle. (Zimmerman (2014) observes earnings gains in years 8-14 and we project those gains over the rest of the lifecycle using the approach in Appendix I.) We calculate post-tax and post-transfer earnings by assuming a tax and transfer rate of 9.6% during the period of earnings decline and 18.6% during the period of earnings gains. This figure comes from our calculation of the effective marginal tax rates based on estimates from the Congressional Budget Office. Details of our approach can be found in Appendix G. We subtract out the individual contribution to tuition, which is estimated by the average tuition net of aid using data from the Delta Cost Project for 2011. The projected earnings gains provide the willingness to pay for all individuals induced to change their behavior and receive more education as a result of the change in tax credits. We calculate total willingness to pay by summing those earnings gains with the simple value of the transfer for the fraction of individuals not induced to change their behavior[16]. The resulting WTP is 1.07.

We calculate net costs beginning with the direct cost of the tax credit, estimated at 0.107 the first stage regression. We then include three additional fiscal costs. First, we estimate the cost of increased educational attainment, net of private tuition payments. We follow the approach of Zimmerman (2014), in calculating government costs and net tuition per full time enrollee based on data from the Delta Cost Project in 2011. Appendix B.VIII outlines the details of this method. This yields an enrollment cost of 0.129. Second, we account for the changes in taxes paid and transfers received based on the earnings gain calculated in the willingness to pay section. This gives a 0.246 increase in government revenue. Finally, we incorporate changes in government educational spending as the tax credit induces individuals to attend more (or less) costly institutions. These are costs are due to changes in the composition of colleges attended by those receiving the credit.[17]Bulman and Hoxby (2015) provide estimates of the change in the change in core educational costs and the change tuition paid by the students at the kink point. The difference between these two measures the average change in government educational costs. We estimate that change at -0.139. Adding that value to our other costs components, we get a total net cost of -0.147.

Combining these estimates, we get an MVPF of ∞. To obtain the confidence intervals, we bootstrap the enrollment, earning and tax revenue outcomes.[18] This leads to a confidence interval of $[-5.95, \infty]$.

We also consider several alternate specifications. First, we incorporate the government costs of new college attendees but we omit the net changes in average costs of college conditional on enrollment. This leads to an MVPF of ∞ $[-5.92, \infty]$]. We also consider how the MVPF changes if we assume that the tax credit produces an

cost. In the absence of information on that behavioral response, we do not include those supplemental costs in this case. However, we do provide alternative specifications that attempt to bound these changes using MVPFs calculated from tax reforms.

[15]Bulman and Hoxby (2015)note that all second stage estimates fail the Ganong and Jager (2014) RKD placebo test at the 5% level. In calculating the MVPF standard errors, we approximate a p-values reported to be above 0.05 by taking a random draw of p-values from 0.05 to 0.9 and inferring the implied standard error.

[16]In instances where the presence of the credit reduces attendance we consider the fraction of induced individuals to be 0 for the purposes of determining the value of the transfer

[17]For the purposes of our analysis we assume that these costs, which are determined conditional on enrollment are distinct from the government cost of new college attendance.

[18]Note that the magnitude of the expected coefficient is the slope of the phase-out region.

additional fiscal externality as a result of an earnings response. We incorporate a lower bound fiscal externality based on our estimated MVPF of the 1993 EITC reform (1.11) and an upper bound fiscal externality based on our MVPF of the OBRA 93 change in top tax rates (1.85). (These estimates can be seen in Appendix F).This leads to MVPF estimates of ∞ (95% CI: $[-6.09, \infty]$) and ∞ , (95% CI: $[-7.08, \infty]$), respectively.

AOTC for Joint Filers, End of Phase Out Our baseline estimate of the MVPF of AOTC benefits is -1.77 for married joint filers at the end of phase out region. This section walks through the details of how that estimate is produced. Our methodology follows the same approach described above.

The kink point for these filers is at $180,000, and Bulman and Hoxby (2015) a dollar increase in income leads to 0.115 credits claimed by the household The second stage regression finds that, at the kink point, a dollar increase in income *decreases* college attendance by 7×10^{-6} percentage points for 19- to 20-year-olds.. In forecasting income, the tax and transfer rate is 9.6% during the period of earnings decline and 18.6% during the period of earnings gain. The WTP is -0.875, driven by -1.072 in post-tax earnings and -0.082 in private tuition contributions. The direct program costs are 0.115, the enrollment cost is -0.129, the additional tax revenue from higher earnings is -0.252, and the change in government educational costs amongst the enrolled is 0.257. Together, this yields a total net cost of 0.494. The calculated MVPF is -1.77 (95% CI: $[-29.91, \infty]$).

The alternative specification where we omit costs from changes in college composition results in an MVPF of -3.69 (95% CI: $[-12.85, \infty]$). The MVPF is -1.98 (95% CI: $[-489, \infty]$) when we incorporate an upper-bound fiscal externality in ours costs. The MVPF is -1.81 (95% CI: $[-437, \infty]$) when we incorporate a lower bound fiscal externality.

AOTC for Single Filers, Beginning of Phase Out Our baseline estimate of the MVPF of AOTC benefits is ∞ for single filers at the beginning of phase out region. This section walks through the details of how that estimate is produced. Our methodology follows the same approach described above for joint filers at the beginning of AOTC phase out.

The kink point for these filers is at $80,000, and Bulman and Hoxby (2015) estimate a dollar decrease in income leads to 0.202 credits claimed by the household. The second stage regression finds that, at the kink point, a dollar decrease in income increases college attendance by 4.3×10^{-5} percentage points for 19- to 20-year-olds. In forecasting income, the tax and transfer rate is 18.9% during the bulk of the lifecycle. The WTP is 4.72, driven by 5.03 in post-tax earnings and 0.506 in private tuition contributions. The direct program costs are 0.202, the enrollment cost is 0.795, the additional tax revenue from higher earnings is 1.24, and the change in government educational costs amongst the enrolled is -0.082. Together, this yields a total net cost of -0.33. The calculated MVPF is ∞ (95% CI: $[-8.87, \infty]$).

The alternative specification where we omit costs from changes in college composition results in an an MVPF of ∞ (95% CI: $[-14.86, \infty]$). The MVPF is ∞ (95% CI: $[-10.19, \infty]$) when we incorporate an upper-bound fiscal externality in ours costs. The MVPF is ∞ (95% CI: $[-10.72, \infty]$) when we incorporate a lower bound fiscal externality.

AOTC for Single Filers, End of Phase Out Our baseline estimate of the MVPF of AOTC benefits is -0.02 for married joint filers at the end of phase out region. This section walks through the details of how that estimate is produced. Our methodology follows the same approach described above for joint filers at the beginning of phase AOTC phase out.

The kink point for these filers is at $90,000, and Bulman and Hoxby (2015) a dollar increase in income leads to 0.216 credits claimed by the household. The second stage regression finds that, at the kink point, a dollar increase in income decreases college attendance by 2×10^{-6} percentage points for 19- to 20-year-olds. In forecasting income, the tax and transfer rate is now 0.03% during the period of earnings decline and 18.9% during the period of earnings gain. The WTP is -0.003, driven by -0.244 in post-tax earnings and -0.024 in private tuition contributions. The direct program costs are 0.216, the enrollment cost is -0.037, the tax revenue impact is -0.060, and the change in government educational costs amongst the enrolled is 0.003. Together, this yields a total net cost of 0.24. The calculated MVPF is -0.02 (95% CI: $[-2.32, \infty]$).

The alternative specification where we omit costs from changes in college composition results in an an MVPF of -0.02 (95% CI: $[-4.28, 10.10]$). The MVPF is -0.03 (95% CI: $[-2.73, \infty]$) when we incorporate an upper-bound fiscal externality in ours costs. The MVPF is -0.02 (95% CI: $[-2.39, \infty]$) when we incorporate a lower bound fiscal externality.

AOTC for Independent Single Filers, Beginning of Phase Out All previous estimates assumed that the college attendee is the dependent of the tax filer. Bulman and Hoxby (2015) also provide estimates of the effects at the kink for independent, single filers who both potentially attend college and receive the tax credits. Our baseline estimate of the MVPF of AOTC benefits is 6.75 for this group at the beginning of phase out region. This section walks through the details of how that estimate is produced. Our methodology follows the same approach described above.

The kink point for these filers is at $90,000, and Bulman and Hoxby (2015) a dollar decrease in income leads to 0.227 credits claimed by the household. The second stage regression finds that, at the kink point, a dollar increase in income increases college attendance by 1×10^{-6} percentage points for 19- to 20-year-olds. In forecasting income, the tax and transfer rate is now 19.6% during the period of earnings decline and 19.6% during the period of earnings gain.The WTP is 0.555, driven by 0.340 in post-tax earnings and 0.012 in private tuition contributions. The direct program costs are 0.227, the enrollment cost is 0.018, the additional tax revenue from higher earnings is 0.083, and the change in government educational costs amongst the enrolled is -0.080. Together, this yields a total net cost of 0.082. The calculated MVPF is 6.75 (95% CI: $[-1.51, \infty]$).

The alternative specification where we omit costs from changes in college composition results in an an MVPF of 3.42 (95% CI: $[-1.74, \infty]$). The MVPF is ∞ (95% CI: $[-1.96, \infty]$) when we incorporate an upper-bound fiscal externality in ours costs. The MVPF is 9.58 (95% CI: $[-1.56, \infty]$) when we incorporate a lower bound fiscal externality.

Simulated Instrument Design In addition to the cross-sectional variation used in the regression kink design, Bulman and Hoxby (2015) also use a simulated instrument design to study the impact of tax credit generosity before and after the tax credit expansion. Specifically, they instrument the use of tax credits with the eligibility for the credits before the enactment of the AOTC. Their simulated instrument predicts the households that would be eligible or ineligible for the AOTC before 2009, which allows for the implementation of differences-in-differences estimation. Unlike the previous regression kink designs, this approach incorporates responses across the household income distribution.

Our baseline estimate of the MVPF of AOTC benefits with the simulated instrument design is 10.05. This section walks through the details of how that estimate is produced. Our methodology follows the same approach described above for the MVPF derived from the regression kink design estimates for joint filers at the beginning of phase AOTC phase out. Our description below highlights the key differences.

In lieu of the income at the kink point, we produce an estimate of the parental earnings from descriptive statistics in Bulman and Hoxby (2015) by taking an average across income bins, weighted by the increase in credits attributed to the AOTC expansion. In contrast to the AOTC RKD, forecasting is set to being (2009), the year of the credit expansion. Bulman and Hoxby (2015) estimate the first stage of the simulated instruments to be 0.38, which we take to be the direct program cost. The second stage regression finds that the credit expansions resulting from the implementation of the AOTC program increased college attendance by 1.4×10^{-5} percentage points for 19-year-olds. In forecasting income, the tax and transfer rate is 18.9% during the bulk of the lifecycle. In contrast to the AOTC RKD, the forecast is set to begin in 2009, the year of the credit expansion. The WTP is 2.038, driven by 1.821 in post-tax earnings and 0.162 in private tuition contributions. The direct program costs are 0.38, the enrollment cost is 0.273, the additional tax revenue from higher earnings is 0.449, and the change in government educational costs amongst the enrolled is -0.001. Together, this yields a total net cost of 0.203. The calculated MVPF is 10.05 (95% CI: $[-12.60, \infty]$).

The alternative specification where we omit costs from changes in college composition results in an an MVPF of 9.98 (95% CI: $[-13.88, \infty]$). The MVPF is 72.30 (95% CI: $[-14.43, \infty]$) when we incorporate an upper-bound fiscal externality in ours costs. The MVPF is 12.57 (95% CI: $[-13.21, \infty]$) when we incorporate a lower bound fiscal externality.

B.VI.2 Tax Relief Act of 1997: Hope and Lifetime Learning Tax Credit

The Tax Relief Act of 1997 introduced two forms of federal tax credits for higher education – the Hope Learning Credit (HTC) and the Lifetime Learning Tax Credit (LLTC). Initially, the two types of credits were meant to target different types of students. The HTC was only available for the first two years of postsecondary education. The benefit applied to 100% of the first $1,000 of qualified educational expense and 50% of the next $1,000, resulting in a maximum benefit of $1,500 of credits per eligible student.[19] The LLTC, as the name suggests, was available for one's lifetime and is not restricted to the first two years of postsecondary education. The benefit applied to

[19] Qualified educational expenses are tuition and fees net of scholarships or grants received by the student.

20% of the first 5,000 of qualified educational expenses, resulting in a maximum of $1,000 credit per tax return.[20] In the year of implementation, both programs were phased based on the AGI (net of other deductions claimed by households) in the range $80,000-$100,000 for joint filers and $40,000-$50,000 for single filers. Students could not take both credits. Both programs were essentially replaced by the AOTC in 2009 (see Appendix B.VI.1), which provided weakly greater benefits at all income levels.

Long (2004) Using data from the 1990-2000 October Current Population Survey, Long (2004) estimates the impact of credit take-up from these two programs on college enrollment using a difference-in-difference around the enact of the 1997 Tax Relief Act. Our baseline estimate of the MVPF of the credit expansion from both the HTC and LLTC is -8.81. They estimate an odds ratio of 0.9342 on the likelihood that 18-24 years attend college following the enactment of the tax credit programs.[21] Using the mean enrollment rate of the control group, we determine that this implies a 0.030 percentage point decrease in attendance as a result of the program.

To calculate the willingness to pay, we transform the effect of college attendance into additional years of schooling by assuming that individuals who did not withdraw complete two additional years of college education on average. We then use estimates from Zimmerman (2014) to estimate the impact on lifetime earnings. We use the results from Zimmerman (2014) to estimate a decline in earnings in years 1-7 from college enrollment and then an increase in earnings over the rest of the lifecycle. (Zimmerman (2014) observes earnings gains in years 8-14 and we project those gains over the rest of the lifecycle using the approach in Appendix I.) We calculate post-tax and post-transfer earnings by assuming a tax and transfer rate of 20.0% during the bulk the lifecycle. This figure comes from our calculation of the effective marginal tax rates based on estimates from the Congressional Budget Office. Details of our approach can be found in Appendix G. We subtract out the individual contribution to tuition (including tax credits), which is estimated by the average tuition net of aid using data from the Delta Cost Project for 2011. The projected earnings gains provide the willingness to pay for all individuals induced to change their behavior and receive more education as a result of the change in tax credits (i.e., the estimated impact of attendance). We calculate total willingness to pay by summing those earnings gains with the simple value of the transfer for the fraction of individuals not induced to change their behavior. The resulting WTP is -2093.60.

We calculate net costs beginning with the direct cost of the tax credit. We calculate this to be 48.87, which is a composition of the mechanical effect for those attending college scaled by the credit claim rate estimated by Long (2004) and the behavioral response of credits claimed by those induced into attending college. We then include two additional fiscal costs. First, we estimate the cost of increased educational attainment net of private tuition by following the approach of Zimmerman (2014) in calculating government costs and net tuition per full-time enrollee based on data from the Delta Cost Project in 1998. Appendix B.VIII outlines the details of this method. This yields an enrollment cost of -404.71. Second, we account for the changes in taxes paid and transfers received based on the earnings gain calculated in the willingness to pay section. This gives a -593.36 increase in government revenue. Adding that value to our other costs components, we get a total net cost of 237.52.

Combining these estimates, we get an MVPF of -8.81. To obtain the confidence intervals, we bootstrap the enrollment, earning and tax revenue outcomes. This leads to a confidence interval of $[-\infty, \infty]$.

As an alternative specification, we consider how the MVPF changes if we assume that the tax credit produces an additional fiscal externality as a result of an earnings response through the tax code. We incorporate a lower bound fiscal externality based on our estimated MVPF of the 1993 EITC reform (1.12) and an upper bound fiscal externality based on our MVPF of the OBRA 93 change in top tax rates (1.85). (These estimates can be seen in Appendix F).This leads to MVPF estimates of -9.01 (95% CI: $[-\infty, \infty]$) and -9.73 (95% CI: $[-\infty, \infty]$), respectively.

Turner (2011): Hope Tax Credit Expansion Turner (2011) also uses difference-in-difference design to estimate the impact of education tax credit expansion due to HTC, but uses the Census Bureau's Survey of Income and Program Participation (SIPP). Our baseline estimate of the MVPF of the credit expansion from the HTC is 12.58. The approach is similar to that used for Long (2004). Turner (2011) estimates the marginal impact of tax credits on college attendance to be 0.00235 for first-year students and a 0.00210 increase in student persistence in the second year (that is, the effect of enrollment of a second-year of college conditional on having completed the first year).

To calculate the willingness to pay, we transform the effect of college attendance into additional years of schooling by assuming that individuals who enrolled in the second year of college (conditional on completing the first) complete an additional 1.5 years of college education on average, and add this to the single-year effect of

[20] In 2003, the program cap for qualified expenses was increased to $10,000, resulting in a maximum of $2,000 per credit return.

[21] Odds ratio are relative to the control group propensity, so that estimates of less than 1 should be interpreted as a negative effect of the treatment.

increased enrollment in the first year. We then use estimates from Zimmerman (2014) to estimate the impact on lifetime earnings. We use the results from Zimmerman (2014) to estimate a decline in earnings in years 1-7 from college enrollment and then an increase in earnings over the rest of the lifecycle. (Zimmerman (2014) observes earnings gains in years 8-14 and we project those gains over the rest of the lifecycle using the approach in Appendix I.) We calculate post-tax and post-transfer earnings by assuming a tax and transfer rate of 20.0% during the bulk of the lifecycle. This figure comes from our calculation of the effective marginal tax rates based on estimates from the Congressional Budget Office. Details of our approach can be found in Appendix G. We subtract out the individual contribution to tuition (including tax credits), which is estimated by the average tuition net of aid and tax credit subsidy using data from the Delta Cost Project for 2011. The projected earnings gains provide the willingness to pay for all individuals induced to change their behavior and receive more education as a result of the change in tax credits (i.e., the estimated impact of attendance). We calculate total willingness to pay by summing those earnings gains with the simple value of the transfer for the fraction of individuals not induced to change their behavior. The resulting WTP is $2,271.

We calculate net costs beginning with the direct cost of the tax credit. Turner (2011) estimates the average subsidy amongst the eligible population to be $1,104. From this, we calculate the direct program cost to be $434.44, which is a composition of the mechanical effect for those attending college in the either the first and second year and the behavioral response of credits claimed by those induced into attending college (first year estimate) or persisting (second year estimate). We then include two additional fiscal costs. First, we estimate the cost of increased educational attainment net of private tuition by following the approach of Zimmerman (2014) in calculating government costs and net tuition per full-time enrollee based on data from the Delta Cost Project in 1998. Appendix B.VIII outlines the details of this method. This yields an enrollment cost of $288.30. Second, we account for the changes in taxes paid and transfers received based on the earnings gain calculated in the willingness to pay section. This gives a $542.20 increase in government revenue. Adding that value to our other costs components, we get a total net cost of $180.53.

Combining these estimates, we get an MVPF of 12.58. To obtain the confidence intervals, we bootstrap the enrollment, earning and tax revenue outcomes. This leads to a confidence interval of (95% CI: $[-14.00, \infty]$). As an alternative specification, we consider how the MVPF changes if we assume that there is partial take-up of the tax credits amongst the eligible population, estimated at a lower bound of 63% by Maag and Rohaly (2007). This leads to an MVPF estimate of ∞ (95% CI: $[-23.13, \infty]$).

B.VI.3 Hope Tax Credit: Regression Kink Design

Bulman and Hoxby (2015) also provides causal estimates for the impact of Hope credits on college attendance analysis in 2007, before the implementation of the AOTC. (See Appendix B.VI.2 for a description of the HTC.) The use a regression kink design identical to the one used to estimate the impact of AOTC. They use kinks in the phase-out of the HTC. In particular, they use the same approach to study the impact of credits at the kink faced by joint filers at the kink at the beginning of the phase out region, the phase-out end for joint filers, the phase-out beginning for single filers, the phase-out end for single filers and the phase out beginning for independent singles filers. (Except for this last group, all other filers have dependents.) We describe the differences between the estimation of each MVPF relative to the detailed discussion of the approach in analyzing Joint filers at beginning of the phase out.[22]

HTC for Joint Filers, Beginning of Phase Out Our baseline estimate of the MVPF of HTC benefits is ∞ for married joint filers at the beginning of phase out region. This section walks through the details of how that estimate is produced. Our methodology follows the same approach described above.

The kink point for these filers is at $94,000 in 2007, and Bulman and Hoxby (2015) estimate that a dollar decrease in income leads to 0.073 credits claimed by the household The second stage regression finds that, at the kink point, a dollar decrease in income increases college attendance by 1.2×10^{-5} percentage points for 19- to 20-year-olds.. In forecasting income, the tax and transfer rate is 18.9% during the bulk of the lifecycle. The WTP is 1.35, driven by 1.39 in post-tax earnings and 0.120 in private tuition contributions. The direct program costs are 0.073, the enrollment cost is 0.206 (measured for 2007), the additional tax revenue from higher earnings is 0.344, and the change in government educational costs amongst the enrolled is -0.165. Together, this yields a total net cost of -0.230. The calculated MVPF is ∞ (95% CI: $[-3.66, \infty]$).

[22] Note that Bulman and Hoxby (2015) restrict estimation to 18- and 19-year-olds as the HTC can only applied to the first two years of postsecondary education.

The alternative specification where we omit costs from changes in college composition results in an an MVPF of ∞ (95% CI: $[-11.07, \infty]$). The MVPF is ∞ (95% CI: $[-3.75, \infty]$). when we incorporate an upper-bound fiscal externality in ours costs. The MVPF is ∞ (95% CI: $[-3.68, \infty]$). when we incorporate a lower bound fiscal externality.

HTC for Joint Filers, End of Phase Out Our baseline estimate of the MVPF of HTC benefits is 2.37 for married joint filers at the end of the phase out region. This section walks through the details of how that estimate is produced. Our methodology follows the same approach described above.

The kink point for these filers is at $114,000, and Bulman and Hoxby (2015) estimate that a dollar increase in income leads to 0.064 credits claimed by the household. The second stage regression finds that, at the kink point, a dollar increase in income increases college attendance by 4.0×10^{-6} percentage points for 19- to 20-year-olds. In forecasting income, the tax and transfer rate is now 9.6% during the period of earnings decline and 18.9% during the period of earnings gain. The WTP is 0.524, driven by 0.500 in post-tax earnings and 0.040 in private tuition contributions. The direct program costs are 0.064, the enrollment cost is 0.069 (measured for 2007), the additional tax revenue from higher earnings is 0.120, and the change in government educational costs amongst the enrolled is 0.209. Together, this yields a total net cost of 0.221. The calculated MVPF is 2.37 (95% CI: $[-2.18, \infty]$).

The alternative specification where we omit costs from changes in college composition results in an an MVPF of 42.01 (95% CI: $[-5.75, \infty]$). The MVPF is 2.73 (95% CI: $[-2.26, \infty]$) when we incorporate an upper-bound fiscal externality in ours costs. The MVPF is 2.44 (95% CI: $[-2.20, \infty]$) when we incorporate a lower bound fiscal externality.

HTC for Single Filers, Beginning of Phase Out Our baseline estimate of the MVPF of HTC benefits is -1.64 for single filers at the beginning of phase out region. This section walks through the details of how that estimate is produced. Our methodology follows the same approach described above.

The kink point for these filers is at $47,000, and Bulman and Hoxby (2015) estimate that a dollar decrease in income leads to 0.064 credits claimed by households. The second stage regression finds that, at the kink point, a dollar decrease in income increases college attendance by -5.0×10^{-6} percentage points for 19- to 20-year-olds. In forecasting income, the tax and transfer rate is 20.0% during the bulk of the lifecycle. The WTP is -0.265, driven by -0.453 in post-tax earnings and -0.050 in private tuition contributions. The direct program costs are 0.139, the enrollment cost is -0.086 (measured for 2007), the additional tax revenue from higher earnings is -0.120, and the change in government educational costs amongst the enrolled is -0.012. The calculated MVPF is -1.64 (95% CI: $[-28.32, \infty]$) .

The alternative specification where we omit costs from changes in college composition results in an an MVPF of -1.53 (95% CI: $[-11.31, \infty]$). The MVPF is -2.71 (95% CI: $[-\infty, \infty]$) when we incorporate an upper-bound fiscal externality in ours costs. The MVPF is -1.81 (95% CI: $[-19.73, \infty]$) when we incorporate a lower bound fiscal externality.

HTC for Single Filers, End of Phase Out Our baseline estimate of the MVPF of HTC benefits is 11.83 for single filers at the end of the phase out region. This section walks through the details of how that estimate is produced. Our methodology follows the same approach described above.

The kink point for these filers is at $57,000, and Bulman and Hoxby (2015) estimate that a dollar increase in income leads to 0.144 credits claimed by the household The second stage regression finds that, at the kink point, a dollar increase in income increases college attendance by 6.0×10^{-6} percentage points for 19- to 20-year-olds.. In forecasting income, the tax and transfer rate is 20.0% during the bulk of the lifecycle. The WTP is 0.661, driven by 0.577 in post-tax earnings and 0.060 in private tuition contributions. The enrollment cost is 0.103 (measured for 2007). The direct program costs are 0.144,, the additional tax revenue from higher earnings is 0.153, and the change in government educational costs amongst the enrolled is -0.038. Together, this yields a total net cost of 0.056. The calculated MVPF is 11.83 (95% CI: $[-4.67, \infty]$).

The alternative specification where we omit costs from changes in college composition results in an an MVPF of 7.00 (95% CI: $[-4.83, \infty]$). The MVPF is ∞ (95% CI: $[-6.22, \infty]$) when we incorporate an upper-bound fiscal externality in ours costs. The MVPF is 16.34 (95% CI: $[-5.04, \infty]$) when we incorporate a lower bound fiscal externality.

HTC for Independent Single Filers, Beginning of Phase Out All previous estimates assumed that the college attendee is the dependent of the tax filer. Bulman and Hoxby (2015) also provide estimates of the effects at the kink for independent, single filers who both potentially attend college and receive the tax credits. Our baseline estimate of the MVPF of HTC benefits is 18.86 for this group at the beginning of phase out region. This section walks through the details of how that estimate is produced. Our methodology follows the same approach described above.

The kink point for these filers is at \$47,000, and Bulman and Hoxby (2015) a dollar decrease in income leads to 0.116 credits claimed by the household. The second stage regression finds that, at the kink point, a dollar decrease in income increases college attendance 3.0×10^{-6} by percentage points for 19- to 20-year-olds. In forecasting income, the tax and transfer rate is 19.6% during the bulk of the lifecycle. The WTP is 0.683, driven by 0.598 in post-tax earnings and 0.030 in private tuition contributions. The direct program costs are 0.116, the enrollment cost is 0.052, the additional tax revenue from higher earnings is 0.148, and the change in government educational costs amongst the enrolled is 0.017. Together, this yields a total net cost of 0.036. The calculated MVPF is 18.86 (95% CI: $[-2.83, \infty]$).

The alternative specification where we omit costs from changes in college composition results in an an MVPF of 34.87 (95% CI: $[-3.81, \infty]$). The MVPF is ∞ (95% CI: $[-3.18, \infty]$) when we incorporate an upper-bound fiscal externality in ours costs. The MVPF is 28.66 (95% CI: $[-2.88, \infty]$) when we incorporate a lower bound fiscal externality.

B.VI.4 Above-the-line Deduction in Tuition and Fees

The above-the-line deduction in tuition and fees (DTF) was implemented in 2001 as part of the Economic Growth and Tax Relief Reconciliation Act. Under this program, households could deduct tuition and fees paid for undergraduate or graduate education from gross income without needing to itemize deductions. Households are eligible to use the DTF based on AGI net of all other above-the-line deductions. Beginning in 2004, the maximum deductions followed a tier system in which joint (single) filers with eligible income of less than \$130,000 (\$65,000) were eligible for a \$4,000 maximum deduction, while household with eligible incomes of \$130,000-\$160,000 (\$65,000-\$80,000) were eligible for a \$2,000 maximum deduction. Households above \$160,000 (\$80,000) were ineligible for the credit. The change in tax liability is based on the household claimed DTF scaled by the marginal tax rate at their income level.

Hoxby and Bulman (2016) provide the causal estimates for this analysis. They use the discontinuity in the maximum deductions at the income thresholds highlighted above to estimate the impact of the above-the-line deduction of tuition and fees on enrollment through a regression discontinuity design in which the area surrounding the discontinuity is eliminated (also known as a “doughnut hole” regression) to the possibility of manipulation in the running variable. In explaining our MVPF, we begin with a detailed descriptions of our approach for one regression discontinuity in Hoxby and Bulman (2016). We focus on the first discontinuity faced by joint filers at the \$130,000 eligible income level for 2004-2008, where the maximum deduction is reduced to \$2000). We adopt the same approach to examine the three other discontinuities in the deduction schedule: \$160,000 for joint filers (maximum deduction reduced to \$0), \$65,000 for single filers (maximum deduction reduced to \$2,000), and \$80,000 for joint filers (maximum deduction reduced to \$0).

Joint Filers, Max Deduction discontinuity at \$130,000 Our baseline estimate of the MVPF of the deduction in tuition and fees is -0.02 with a confidence interval of [-2.21, 7.59] for married joint filers at the first point of discontinuity in the deduction eligibility schedule. This section walks through the details of how that estimate is produced. For married joint filers, the maximum deduction for tuition and fees drops from \$4,000 to \$2,000 at \$130,000 of AGI, net of other deductions. The stepwise discontinuity of the phase out allows Hoxby and Bulman (2016) to estimate the impact of the tax deduction for households near the discontinuity in 2004-2008. We describe the calculation in detail here. For the three other DTF MVPFs from Hoxby and Bulman (2016) making use of this design, we only highlight the differences relative to this program.

Hoxby and Bulman (2016) find that at the \$130,000 discontinuity, households are estimated to claim \$1023 less in deductions, which is statistically significant. The second stage regression finds that, at the discontinuity, college attendance decreases by 0.003 percentage points amongst students that graduated high school in the previous year.[23]

To calculate the willingness to pay, we transform the effect of college attendance into additional years of schooling by assuming that individuals who enroll complete two additional years of college education on average. We then use estimates from Zimmerman (2014) to estimate the impact on lifetime earnings. We use the results from Zimmerman

[23] Hoxby and Bulman (2016) also represents effects for cohorts beyond the first year of high school graduation, but because this also includes persistence effects from those who were induced into going to college by the previous years tuition deductions, these estimates do not have a straightforward treatment effects interpretation in the context of our calculation.

to estimate a decline in earnings in years 1-7 from college enrollment and then an increase in earnings over the rest of the lifecycle. Zimmerman (2014) observes earnings gains in years 8-14 and we project those gains over the rest of the lifecycle using the approach in Appendix I. We calculate post-tax and post-transfer earnings by assuming a tax and transfer rate of 18.9%. This figure comes from our calculation of the effective marginal tax rates based on estimates from the Congressional Budget Office. Details of our approach can be found in Appendix G. We subtract out the individual contribution to tuition, which is estimated by the average tuition net of aid using data from the Delta Cost Project for 2006 (the midpoint between 2004-2008, the period studied). The projected earnings gains provide the willingness to pay for all individuals induced to change their behavior and receive more education as a result of the change in tax credits. We calculate total willingness to pay by summing those earnings gains with the simple value of the transfer for the fraction of individuals not induced to change their behavior.[24] The WTP is -$8, driven by a $385 decrease in post-tax earnings and $90 decrease in private tuition contributions, and a transfer value of $287.

We calculate net costs beginning with the direct cost of the tax credit, estimated at $286, which is the average deduction claimed of $1,023 multiplied by the marginal tax rate of 28% at $130,000. We then include three additional fiscal costs. First, we estimate the cost of increased educational attainment, net of private tuition payments. We follow the approach of Zimmerman (2014), in calculating government costs and net tuition per full time enrollee based on data from the Delta Cost Project in 2011. Appendix B.VIII outlines the details of this method. This yields an enrollment cost of $25. Second, we account for the changes in taxes paid and transfers received based on the earnings gain calculated in the willingness to pay section. This gives a $89 decrease in government revenue. Finally, we incorporate changes in government educational spending as the tax credit induces individuals to attend more (or less) costly institutions. These are costs are due to changes in the composition of colleges attended by those receiving the credit. Hoxby and Bulman (2016) provide estimates of the change in the change in core educational costs and the change tuition paid by the students at the kink point. The difference between these two measures the average change in government educational costs, which we calculate to be a $44 increase. Adding that value to our other costs components, we get a total net cost of $396.

Combining these estimates, we get an MVPF of -0.02. To obtain the confidence intervals, we bootstrap the enrollment, earning and tax revenue outcomes. This leads to a confidence interval of $[-2.21, 7.59]$.

We also consider several alternate specifications. First, we incorporate the government costs of new college attendees but we omit the net changes in average costs of college conditional on enrollment. This leads to an MVPF of -0.02 with a 95% confidence interval of $[-2.22, 16.64]$. We also consider how the MVPF changes if we assume that the tax credit produces an additional fiscal externality as a result of an earnings response. We incorporate a lower bound fiscal externality based on our estimated MVPF of the 1993 EITC reform (1.11) and an upper bound fiscal externality based on our MVPF of the OBRA 93 change in top tax rates (1.85). These estimates can be seen in Appendix F. This leads to MVPF estimates of -0.02 with a 95% confidence interval of $[-2.30, 9.20]$ and -0.03 with a 95% confidence interval of $[-2.72, 40.66]$, respectively.

Joint Filers, Max Deduction discontinuity at $160,000 Our baseline estimate of the MVPF of DTF benefits is 0.77 with a 95% confidence interval of [-1.83, 57.84] for married joint filers at the second discontinuity in the benefit schedule. This section walks through the details of how that estimate is produced. Our methodology follows the same approach described above.

Hoxby and Bulman (2016) find that at the $130,000 discontinuity, households are estimated to claim $1199 less in deductions, which is statistically significant. The second stage regression finds that, at the discontinuity, college attendance does not change amongst students that graduated high school in the previous year (i.e., the estimated effect is 0). In forecasting income, the tax and transfer rate is 18.9% during the short-run period of earnings decline and 18.6% during the period of long-run earnings gain. The WTP is $336, driven entirely by the change deductions claimed.. The direct program costs are $336 (derived from scaling the change in deductions by the marginal tax rate of 28%), the enrollment cost is 0, the additional tax revenue from higher earnings is 0, and the change in government educational costs amongst the enrolled is $98. Together, this yields a total net cost of $434. The calculated MVPF is 0.77 with a 95% confidence interval of $[-1.83, 57.84]$.

The alternative specification where we omit costs from changes in college composition results in an an MVPF of 1 with a 95% confidence interval of $[-1.96, \infty]$). The MVPF is 1.20 with a 95% confidence interval of $[-2.26, \infty]$) when we incorporate an upper-bound fiscal externality in ours costs. The MVPF is 0.84 with a 95% confidence interval of $[-1.91, 567.95]$) when we incorporate a lower bound fiscal externality.

[24]In instances where the presence of the credit reduces attendance we consider the fraction of induced individuals to be 0 for the purposes of determining the value of the transfer

Single Filers, Max Deduction discontinuity at $65,000 Our baseline estimate of the MVPF of DTF benefits is ∞ forsingle filers at the first discontinuity in the benefit schedule. This section walks through the details of how that estimate is produced. Our methodology follows the same approach described above.

Hoxby and Bulman (2016) find that at the $65,000 discontinuity, households are estimated to claim $361 less in deductions, which is statistically significant. The second stage regression finds that, at the discontinuity, college attendance decreases by 0.005 percentage points amongst students that graduated high school in the previous year. In forecasting income, the tax and transfer rate is 20.0% during the bulk of the lifecycle. The WTP is $485.23, driven by $491.0 in post-tax earnings and $95.6 in private tuition contributions. The direct program costs are $90.3 (derived from scaling the change in deductions by the marginal tax rate of 25%), the enrollment cost is $70.97, the additional tax revenue from higher earnings is $130.1, and thedecrease in government educational costs amongst the enrolled is $481. Together, this yields a total net windfall to the government of $449.9. The calculated MVPF is ∞ (95% CI: $[-\infty, \infty]$).

The alternative specification where we omit costs from changes in college composition results in an an MVPF of 15.58 (95% CI: $[0.08, \infty]$) . The MVPF is ∞ (95% CI: $[-\infty, \infty]$) when we incorporate an upper-bound fiscal externality in ours costs. The MVPF is ∞ (95% CI: $[-\infty, \infty]$) when we incorporate a lower bound fiscal externality.

Single Filers, Max Deduction discontinuity at $80,000 Our baseline estimate of the MVPF of DTF benefits is ∞ for single filers at the second discontinuity in the benefit schedule. This section walks through the details of how that estimate is produced. Our methodology follows the same approach described above.

Hoxby and Bulman (2016) find that at the $80,000 discontinuity, households are estimated to claim $524 less in deductions, which is statistically significant. The second stage regression finds that, at the discontinuity, college attendance does not change amongst students that graduated high school in the previous year (i.e., the point estimate of the effect is 0). In forecasting income, the tax and transfer rate is 20.0% during the period of earnings decline and 18.9% during the period of earnings gain. The WTP is $146.72, driven entirely by the change in deductions claimed. The direct program costs are equal to the WTP (derived from scaling the change in deductions by the marginal tax rate of 28%), the enrollment cost is 0, the additional tax revenue from higher earnings is 0, and the decrease in government educational costs amongst the enrolled is $245. Together, this yields a total net windfall of $98.3. The calculated MVPF is ∞ (95% CI: $[-\infty, \infty]$).

The alternative specification where we omit costs from changes in college composition results in an an MVPF of ∞ (95% CI: $[-\infty, \infty]$). The MVPF is ∞ (95% CI: $[-\infty, \infty]$) when we incorporate an upper-bound fiscal externality in ours costs. The MVPF is ∞ when we incorporate a lower bound fiscal externality (95% CI: $[-\infty, \infty]$).

B.VII Elimination of Barriers to College Entry

B.VII.1 FAFSA Assistance

Our baseline specification for FAFSA application assistance has two distinct MVPFs. We find an MVPF of 4.03 with a 95% confidence interval of [0.87, 10.76] for students who are dependents, and an MVPF of 2.12 with a 95% confidence interval of [-0.50, 11.11] for students who are independent tax filers. This Appendix walks through the construction of these estimates.

As analyzed by Bettinger et al. (2012), the FAFSA application assistance for which we compute the MVPF was provided by H&R Block in a randomized control trial of individuals who used H&R Block services to file their taxes. Treated individuals received information about college tuition, direct assistance filling out the FAFSA form, and assistance submitting the form. They were also contacted by a call center if they did not complete their form at the H&R Block offices. (Bettinger et al. (2012) also provided a more limited treatment to some individuals that provided information about aid eligibility but did not provide direct assistance on the FAFSA form. We do not calculate the MVPF of that treatment.) Bettinger et al. (2012) examine the impact of FAFSA assistance on Pell Grant receipt, initial school enrollment and total years of school attendance.

Dependent Filers Result First we explain how we calculate the MVPF for dependent individuals who receive FAFSA assistance. In our primary estimate we calculate willingness to pay from Bettinger et al. (2012), who estimate that dependent participants were enrolled in 0.191 additional years of college as a result of the program (Table 6). From there, we use estimates from Zimmerman (2014) to estimate the impact of this additional schooling on earnings. In particular, we use the results from Zimmerman (2014) to estimate a decline in earnings in years 1-7 after enrollment and then an increase in earnings over the rest of the lifecycle. (Zimmerman (2014) observes earnings gains in years 8-14 and we project those gains over the rest of the lifecycle using the approach in Appendix I.) We calculate post-tax and post-transfer earnings by assuming a tax and transfer rate of 9.6% during the bulk of the lifecycle. These figures comes from our calculation of the effective marginal tax rates based on estimates from the Congressional Budget Office. Details of our approach can be found in Appendix G. We also subtract out individual contributions to tuition, which we assume is given by the difference between the maximum Pell grant and the Expected Family Contribution for the treated individuals. That reduces willingness to pay by $905 times the number of years of additional enrollment. The projected earnings gains provide the willingness to pay for all individuals induced to change their behavior and receive more education as a result of the grants. We calculate total willingness to pay by summing those earnings gains with the simple value of the transfer for the fraction of individuals not induced to change their behavior, resulting in total WTP for the policy of $8,329 per treated individual.

We calculate net costs by starting with the direct program cost. This includes the costs of administering the program – including participation incentives, operating the call center and administrative materials., which sum to $88 per treated individual.– as well as the cost of Pell grants received as a result of the treatment, yielding a per-person direct program cost of $247. We next add the additional government costs for those induced to go to college as a result of the treatment (net of additional Pell aid included in the direct program cost). We estimate the cost of new college enrollment following the approach of Zimmerman (2014), calculating government costs per full time enrollee based on data from the Delta Cost Project. Appendix B.VIII outlines the details of this method. That produces an additional enrollment cost of $2,747 per treated individual. Finally, we account for the changes in taxes paid and transfers received based on the earnings gains calculated in the willingness to pay section. That results in a fiscal externality of $929 and a total net cost of $2,065.

Combining these estimates, we get an MVPF of 4.03. To obtain the confidence intervals, we bootstrap the enrollment, earning and tax revenue outcomes. We assume (conservatively) these estimates are perfectly correlated across bootstrap iterations. This leads to a confidence interval of [0.87, 10.76].

We also consider alternate approaches to calculating the earnings effect and determining willingness to pay. In the case of the earnings effect, we create a specification where our earnings projections are not based on years of school completed, but rather initial college enrollment. The years of schooling effects are restricted to a limited window and so they are not used as our baseline in this case. We then apply the same earnings projection method from Zimmerman (2014) using these schooling increases. This produces an MVPF of 4.11 with a 95% confidence interval of [0.91, 11.11]. In the case of willingness to pay, we considered a specification where the FAFSA assistance is valued at the cost of the transfer rather than based on the change in long term earnings. This conservative willingness to pay method ignores any effect from increases in earnings. Instead, it applies the envelope theorem to those induced to get more schooling and assumes they are indifferent to the expenditure. In this case, the only direct transfer is the small financial award given to those who participate in the study. The resulting MVPF for this alternate specification is 0.01 with a 95% confidence interval of [0.01, 0.04].

Independent Filers Result Now we explain how we calculate the MVPF for independent individuals who receive FAFSA assistance. Our methodology follows the same approach described above for dependent individuals. For independent filers, our primary estimate is based on Bettinger et al. (2012) estimate that independent participants with no prior college experience were enrolled in 0.027 additional years of college as a result of the program (Table 6). In the earnings forecast, the tax and transfer rate is now 9.6% during the period of earnings decline and 19.9% during the period of earnings gain, yielding a post-tax-and-transfer WTP of $658 per treated individual. This figure includes a reduction of WTP of $574 times the number of years of additional enrollment as the result of individual contributions to tuition. The direct program cost is $133, the enrollment cost is $388, the additional tax revenue from higher earnings is $187, and the resulting total cost is $334. This results in an MVPF of 2.12 with a 95% confidence interval of [-0.50, 11.11].

As with our estimate for dependent individuals, we also consider a number of specifications that vary our assumptions regarding both willingness to pay and cost. We once again considered the impact estimated based on initial college enrollment and find an MVPF of 2.07 with a 95% confidence interval of [-0.01, 8.88]). We also consider the conservative willingness to pay assumption based on the value of the transfer and find an MVPF of 0.06 with a 95% confidence interval of [0.02, 1.04]).

B.VII.2 Michigan HAIL Scholarship

Our baseline specification for the impact of the HAIL Scholarship has MVPF of 1.30 with a 95% confidence interval of [0.22, 3.32]. This Appendix walks through the construction of this estimate.

The HAIL Scholarship was a program designed to increase enrollment of high achieving low-income students at the University of Michigan. The program provided a guarantee to low income, high-achieving students that they would receive free tuition if they were accepted at the University of Michigan, Ann Arbor. Most of these students would have already been eligible for free tuition, but they might not have been aware of that fact. The HAIL program provided a letter to the students, to their parents, and to their principals that alerted them of the scholarship opportunity. Work by Dynarski et al. (2018) provides the primary causal estimates for this analysis. They examine a randomized control trail designed to test the impact of the HAIL Scholarship, and calculate the impact of the program on college enrollment.

In our primary estimates we calculate willingness to pay for the HAIL Scholarship using estimates from Dynarski et al. (2018) on college enrollment. In our baseline estimate, we assume that there are earnings gains from attending a four year school instead of no college or community college, but we do not assume that there are any additional earnings gains from attending a more selective four-year school. We alter that assumption in the specifications below. We assume that students who attend a four year school rather than attending no college increase their educational attainment by two years, while students who attend a four-year school rather than attending a community college increase their education attainment by one year. Under these assumptions, we translate the changes in enrollment estimated byDynarski et al. (2018) into additional years of schooling and then use estimates from Zimmerman (2014) to estimate the impact on earnings. In particular, we use the results from Zimmerman to estimate a decline in earnings in years 1-7 after enrollment and then an increase in earnings over the rest of the lifecycle. Zimmerman (2014) observes earnings gains in years 8-14 and we project those gains over the rest of the lifecycle using the approach in Appendix I. We calculate post-tax and post-transfer earnings by assuming a tax and transfer rate of 20% during the bulk of the lifecycle. These figures come from our calculation of the effective marginal tax rates based on estimates from the Congressional Budget Office. Details of our approach can be found in Appendix G. The projected earnings gains provide the willingness to pay for all individuals induced to change their behavior and receive more education as a result of the grants. We calculate total willingness to pay by summing those earnings gains with the simple value of the transfer for the fraction of individuals not induced to change their behavior, resulting in total WTP for the policy of $5,404.

The treatment in this case is promise of free tuition, so the initial program costs are the administrative costs of sending the letter ($10[25]) and the costs to the government due to increased enrollment. For the latter, we use estimates for additional years of educational attainment based on the enrollment changes documented in Dynarski et al. (2018) discussed above. We then follow the approach of Zimmerman (2014), and calculate government costs per full time enrollee based on data from the Delta Cost Project. Appendix B.VIII outlines the details of this method. This results in additional enrollment costs of $5,577 per treated individual. Next, we account for the changes in taxes paid and transfers received based on the earnings gains calculated in the willingness to pay section. This results in a fiscal externality of $1,424 and, when combined with the letter cost and additional enrollment expenditures, a total net cost of $\$10 + \$5,577 - \$1,424 = \$4,163$.

Combining these estimates, we get an MVPF of 1.30 with a 95% confidence interval of [0.22, 3.32]. To obtain the confidence intervals, we bootstrap the enrollment, earning and tax revenue outcomes. We assume (conservatively) these estimates are perfectly correlated across bootstrap iterations.

We also consider alternate approaches to calculating the earnings effect and determining willingness to pay. In the case of the earnings effect, we create a specification where individuals see an earnings gain if they attend the University of Michigan at Ann Arbor rather than a less selective four-year school. In particular, we use an estimate from Hoekstra (2009)to ground an assumption that enrollment at the University of Michigan at Ann Arbor is equivalent to two years of additional schooling. We make this assumption because Hoekstra (2009) argues that enrollment at a flagship university increases earnings by approximately 20%, or roughly twice the effect on earnings of an additional year of schooling in Zimmerman (2014). This approach produces an MVPF of 4.77 with a 95% confidence interval of $[0.92, \infty]$, and emphasizes the importance of long-term follow-up in understanding the impact of a policy like one one that encourages enrollment at flagship universities. We also considered a specification where individuals placed a de minimis valuation the HAIL Scholarship. We assume the value is one percent of the educational expenditures. This approach is designed to estimate a conservative on willingness to pay. It assumes that individuals place no value on the HAIL Scholarship letter and that even if they change enrollment behavior in response, they are essentially indifferent between the various alternatives. This specification results in an MVPF of 0. Finally, we considered a specification where willingness to pay for the HAIL Scholarship is valued based on the increase in school expenditures. This serves as a middle ground estimate between the case where WTP is determined by earnings and the case where WTP is near zero because it is the result of a behavioral response. In this context assume that individuals induced to change their behavior value each dollar of transfer at 50 cents on

[25]Dynarski et al. (2018) suggest that the letter costs less than $10 to produce and deliver. There may be additional costs associated with parent and school notification and the accompanying online portal, but those costs are not documented in the paper. As a fraction of total educational expenses they remain very small and are, therefore, very unlikley to impact the MVPF.

the dollar. We do not know if the individuals were induced to attend based on the first dollar of the scholarship or the last so we implicitly assume they change their schooling in response to the middle dollar. This results in an MVPF of 0.67 with a 95% confidence interval of [0.54, 0.92].

B.VIII College Cost Details – Delta Cost Project

Most college policies analyzed in this sample have an impact on years of schooling. Properly incorporating those effects into the MVPF requires an estimate of the government costs associated with those incremental schooling changes. While many programs analyzed in this Appendix provide financial aid to new enrollees, those expenditures do not fully capture the governments costs associated with student enrollment. If, for example, a Pell Grant induces an individual to attend a four-year public university, that individual is being induced to receive a heavily subsidized education. The government helps cover the cost of that schooling via state and local appropriations, amongst other ways.

In order to account for these costs, we need a measure educational expenditures per college student. For that purpose, we use data compiled by the Delta Cost Project based on underlying IPEDS data. The Delta Cost Project provides detailed information on college costs for postsecondary institutions from 1986 through 2014. More details can be found at: www.deltacostproject.org/delta-cost-data. We follow the approach of Zimmerman (2014), and determine college expenditures based on "direct educational expenditures." We use cost figures that are specific to the year of policy implementation and impacted schools. When analyzing policies that impact students at multiple schools, we measure costs by taking an enrollment-weighted average across schools. We often separate costs by four-year and two-year institutions, but restrict our focus to public universities.[26] In cases where the specific policy being analyzed does not provide a measure of student contributions to schooling, we also use data from the Delta Cost Project on net tuition.

Our default conservative assumption is that governments cover the full difference between educational costs and student contributions at both public and private universities. We also conduct robustness tests where we allow government contributions to costs to vary.

C Job Training

C.I Sample Frame and Baseline Assumptions

We begin our sample construction for job training policies using two prominent meta-analyses on the effects of job training – Card et al. (2010) and Card et al. (2017) – along with recent policies cited in Schochet et al. (2008). We restrict our sample to research based in the United States and to papers that use quasi-experimental variation to conduct their analysis. (This excludes any papers that rely on individual fixed effects or matching designs.) We require that all policies in our sample have estimated impacts on post-program earnings. We maintain a focus on interventions designed to increase human capital, rather than programs that provide temporary employment or job search assistance. We also search for and include any follow-up work that evaluates those same policy reforms cited in the meta-analyses. In cases where there are multiple papers evaluating the same job training program but measure different outcomes, we construct the MVPF of the policy reform. As such, we create one MVPF estimate for each policy, often combining results from multiple papers.

For all job training programs, we maintain a set of consistent assumptions for our baseline MVPF results. Willingness to pay for programs is measured by the increase in post-tax earnings for program participants net of any reductions in government transfers (such as AFDC). In cases where research has measured the impact of job training programs on transfers, we use those estimates directly. In all other cases, we use the estimated transfer rate from our estimates using Congressional Budget Office data discussed in Appendix G.[27] Using earnings net of transfers to measure WTP is implied by an envelope-theorem argument that one can use the impact on the individual's budget constraint to measure their willingness to pay. This argument requires an assumption that the earnings responses to the program are not the result of changes in labor effort. Because this assumption may not hold, we discuss below how MVPFs for each program change when we alternatively assume that participants value job training programs at their cost (and are indifferent to changes in earnings and transfers resulting from their behavioral responses to the program).

[26] We are aware of the Jaquette and Parra (2016) criticism of the Delta Cost Project. In cases where we draw data on individual schools in state university systems we confirm that the cost estimates are based on individual school and not "parent-child" school combinations.

[27] To maintain consistency across studies that measure a variety of different outcomes (such as health, drug use, and crime), we omit any additional willingness to pay from non-earnings impacts.

In our baseline specification for each program, we use only observed impacts on earnings and refrain from projecting earnings impacts forward in time. This is largely consistent with findings in these papers of long-run responses that are not significant. However, many of these programs were intended to increase human capital, which could in turn increase long-run earnings. To that aim, we conduct a robustness analysis to assess the potential implications of forecasting impacts beyond the observed time horizon.[28] In particular, Appendix Table C.I shows how MVPFs for the job training programs in our sample change as we vary the length of earnings projections between 2-4 years (the length of observed impacts for Job Training Partnership Act programs, WorkAdvance, Year Up, and JOBSTART), 8 years (the length of observed impacts for the National Supported Work Experiment), 21 years (the length of observed impacts for Job Corps), and until age 65.

One notable case where the estimates do change significantly is the case of the Year Up program. As noted below, research has documented large earnings increases as a result of the program in a short three-year follow-up window; if those gains persist, this suggests the program could have a much higher MVPF than is currently implied by its observed impacts. In other cases, using projected as well as observed earnings impacts tends to lead to extremely imprecise estimates for cases where we do not observe long-run outcomes. [29]

C.II Job Training Partnership Act

Designed to study the benefits and costs of employment and training programs for out-of-school youth and economically disadvantaged adults funded through the 1982 Job Training and Partnership Act (JTPA), the National JTPA Study was a randomized experiment that evaluated 16 local JTPA programs beginning in 1987. The study involved 20,600 participants (both youth and adult). Our estimates of the MVPF for JTPA-funded programs are derived from cost-benefit analysis resulting from the National JTPA Study as reported in Table 8 of Bloom et al. (1997). Because of the variety of programs included in the study, we compute separate MVPFs for adults and for youths. This pools Bloom et al.'s results for adult men and women and their results for female youths and male youth non-arrestees.[30]

For adult women, Bloom et al. (1997) (Table 8) report an increase in earnings of $1,683, which was supplemented by a wage subsidy of $154, as well as a reduction in welfare benefits of $235. Adult men, meanwhile, saw an increase in earnings of $1,355, a wage subsidy of $244, and an increase in welfare benefits of $334. The program cost was $1,381 for adult women and $1,175 for adult men (inclusive of the wage subsidy). Applying the same tax rate used in Bloom et al.'s cost-benefit analysis (12.8%) to the earnings impacts and pooling results for men and women (weighting by each group's relative sample size) yields a baseline MVPF for adults of 1.38 (95% CI: [-0.38, 2.09]). If we instead assume that participants value the program at cost, the resulting MVPF is 1.18 (95% CI: [0.57, 8.74]).

Meanwhile, Bloom et al. (1997) (Table 8) find that earnings for female youth increased by $136, with a wage subsidy of $74, a reduction in welfare benefits of $379, and a program cost (inclusive of the wage subsidy) of $1,466. Male youth non-arrestees, meanwhile, saw a decrease in earnings of $968, a wage subsidy of $100, an increase in welfare benefits of $119, and a program cost of $2,165. Applying a tax rate of 12.8% and pooling results for each of the two youth groups yield a baseline MVPF of -0.23 (95% CI: [-3.72, 1.24]); if participants valued JTPA programs at cost, the corresponding MVPF would be 1.10 (95% CI: [0.64, 12.22]).

Appendix Table C.I shows how these results would change if we forecast the observed earnings impacts into the future. Projecting from the observed 30 months to just 8 years leads to extraordinarily imprecise estimates in which one cannot reject ∞ or $-\infty$ for youth or adults under the assumption that participants value the program by their after-tax earnings gains.

C.III WorkAdvance

WorkAdvance is a workforce development program that provides unemployed, low-income adults with occupational skills training aimed at employment in targeted sectors such as information technology (IT), environmental remediation, transportation, health care, and manufacturing. Our estimate of the MVPF for WorkAdvance is based on Hendra et al. (2016) and Schaberg (2017), who report results from a randomized evaluation of WorkAdvance as implemented by four service providers in New York, Oklahoma, and Ohio from 2011 to 2013.

[28]For all job training programs discussed in this Appendix, earnings projections are conducted using the primary lifecycle earnings trajectory method described in Appendix I.

[29]For all projections, we use the final years of observed impacts (rather than the full set of observed impacts) as the basis for future earnings changes. We also assume that any observed reductions in transfers (such as AFDC) continue into the future as a constant share of changes in earnings, with the share equal to the ratio of observed transfer reductions to observed earnings impacts.

[30]Bloom et al. (1997) do not report benefit-cost findings for male youth arrestees because earnings impacts for this group derived from survey data (a large and significant decrease in earnings) contradict earnings impacts derived from UI administrative data (no impact on earnings).

Hendra et al. (2016) report average earnings gains of $1,945 per participant two years following random assignment, while Schaberg (2017) reports subsequent earnings gains of $1,865 for participants in the following (third) year. Discounting these impacts back to the program year at a 3 percent rate and applying a tax rate of 0.3% (calculated from CBO estimates) yields a total present-discounted after-tax earnings gain of $3,635 for participants and an increase in tax payments of $11. Pooling across the four providers included in the evaluation, the average program cost per participant was $5,641. However, as shown in Hendra et al. (2016), Table 4.3, providers also experienced average net cost reductions per participant of $940 for out-of-program training and support services because of WorkAdvance; we include these reductions as part of the program's fiscal externality. Together, these results yield a baseline MVPF of $\frac{3,635}{5,641-11-940} = 0.78$ (95% CI: [0.26, 1.34]) under the assumption that WorkAdvance participants value the program by their after-tax earnings gains. If participants valued WorkAdvance at cost, the MVPF would instead be $\frac{5,641}{5,641-11-940} = 1.203$ (95% CI: [1.201, 1.205]).[31]

C.IV Year Up

Year Up is a national job training program for youth between the ages of 18 and 24 who are disconnected from work and school. The program provides participants with six months of full-time training in the financial services and IT sectors followed by a six-month internship at a firm in those industries, along with support services such as counseling, instructional supports, and financial assistance in the form of a weekly stipend. Our MVPF estimate is based on recent initial results from a randomized evaluation of Year Up conducted by Abt Associates beginning in 2013 (Fein and Hamadyk (2018)), which cover three years of impacts following the program.[32]

Fein and Hamadyk (2018), Exhibit 6-1, document a significant decrease in treatment group earnings in the first year following random assignment (-$5,338), followed by large and significant earnings gains in the subsequent two years ($5,181 and $7,011, respectively). Discounting these impacts back to the program year at a 3 percent rate and applying a tax rate calculated from CBO estimates of 18.6% yields a total present-discounted after-tax earnings gain of $4,962 for participants and an increase in tax payments of $1,134. As a relatively intensive program, Year Up has a higher per-participant cost than other programs included in our sample: $28,290, including $6,614 for student stipends. The resulting baseline MVPF for Year Up is $\frac{4,962+6,614}{28,290-1,134} = 0.43$ (95% CI: [0.37, 0.48]). Using the alternative assumption that WTP for the program is equal to the program's cost, the MVPF is $\frac{28,290}{28,290-1,134} = 1.04$ (95% CI: [1.03, 1.05]).

However, the key question for the MVPF of this program is how long earnings gains persist. For consistency with other job training programs, we use the observed time period for the estimated length of gains. However, it is important to note that the short-run gains for Year Up are particularly large relative to other job training programs. Appendix Table C.I shows that if we project earnings gains for 5 additional years, the resulting MVPF is 2.78 (95% CI: [2.10, 3.59]), and if the gains hold for 18 additional years (21 total), the estimated MVPF would be infinite, that is, the program would pay for itself. In this sense, the key question for the efficiency and effectiveness of Year Up is how long the gains currently observed in the short run will persist into the future.

C.V JOBSTART

The JOBSTART Demonstration, which ran between 1985 and 1988, was patterned after the Job Corps program but implemented in a nonresidential setting. The program was developed by the Manpower Demonstration Research Corporation (MRDC) to target low-skill, young school dropouts, and its operating costs were primarily funded through the JTPA (Cave et al. (1993)). Our estimate of the MVPF for JOBSTART is based on the final report of the MRDC evaluation of the demonstration, Cave et al. (1993).

Cave et al. (1993) report that the operating costs of JOBSTART were $4,548 (in 1986 dollars) for each participant, and that individuals in the treatment and control groups incurred similar levels of costs from non-JOBSTART services. Cave et al. (1993) estimate that about 15 percent of the $4,548 program costs went to support services for JOBSTART participants such as child care and bus passes. The impacts of JOBSTART on AFDC ($74), food stamp (-$24), and General Assistance ($29) payments were reported for four years following the program, but none of these differences in transfer receipt are statistically significant in the aggregated sample. Cave et al. (1993) report statistically significant earnings losses in the first year ($499), but earnings gains in the last three years of observed earnings, summing to a $214 increase in earnings over the four years of observed earnings (this estimate

[31] As shown in Appendix Table C.I, these estimates would be larger if the earnings gains persist into the future. At 8 years, the MVPF would be 2.63 (95% CI: [0.87, 4.67]) and at 21 years (the length of time we observe JobCorps), the MVPF would be 6.81 (95% CI: [2.27, 12.18]).

[32] The Abt evaluation of Year Up is ongoing; according to Fein and Hamadyk (2018), future reports will provide up to six years of impacts.

is not statistically distinguishable from \$0). In our baseline estimates, we calculate tax rates from CBO estimates (0.7%) and assume that participants value JOBSTART as the sum of (a) the program's post-tax earnings and transfers impacts and (b) support services provided to participants, discounted back to the start of the program at a 3 percent discount rate. The resulting MVPF is 0.20 (95% CI: [0.04, 0.42]). The imprecision of the estimates for the observed four-year horizon is compounded when we attempt to forecast these gains into the future. At a 21-year projection length, we obtain a point estimate of 10.31 but we cannot reject an MVPF of either 0 or ∞ at a 95-percent confidence level.

If we instead assume that participants valued JOBSTART at cost, the resulting MVPF would be 0.98 (95% CI: [0.80,1.24]). Projecting earnings effects to 21 years under this alternative assumption yields an MVPF of 3.20 (though we cannot reject an MVPF of either 0.70 or ∞).

C.VI National Supported Work Demonstration

The National Supported Work Demonstration was a randomized evaluation, begun in 1975, of a subsidized work program that targeted four groups of unemployed individuals: adult women on AFDC, ex-addicts, ex-offenders, and youth who had dropped out of school. The program operated at 15 sites nationwide by local nonprofits. Participants were provided with temporary employment (for no longer than 18 months) with gradually increasing expectations for productivity and attendance and/or gradually decreased supervision. Because the precise parameters of interventions included in the evaluation varied from site to site, we provide separate MVPFs for each of the four groups targeted by the program. For ex-addicts and ex-offenders, we rely on the results in Hollister et al. (1984), who examine impacts up to 27 months following random assignment. For adult women and youth, we supplement these estimates with the longer-run results of Couch (1992), who estimates earnings impacts for NSW participants for eight years following the program using Social Security earnings record data.

Hollister et al. (1984) find a post-training earnings loss of \$153 due to program participation for the ex-addict target group, along with a post-training earnings gain of \$304 for the ex-offender target group. Hollister et al. (1984) report a program cost of \$4,413, including payments of \$2,558 to participants, for ex-addicts; the corresponding figures for ex-offenders are \$4,153 and \$2,489. At a tax rate of 12.9% (derived from CBO estimates), the sum of earnings changes and payments to participants correspond to an increase in tax payments of \$310 for ex-addicts and an increase in tax payments of \$369 for ex-offenders. Finally, Hollister et al. (1984) report additional savings to the government via reduced welfare utilization (\$530 for ex-addicts and \$219 for ex-offenders) and reduced administrative costs from non-NSW training programs (\$85 for ex-addicts and \$168 for ex-offenders), as well as reduced payments to participants from these substitute programs (\$13 for ex-addicts and \$32 for ex-offenders). Assuming that participants value the program as the sum of their post-tax earnings impacts, their post-tax direct payments from the program, and their change in total transfers, the MVPF for ex-addicts is $\frac{-153+2{,}558-310-530-13}{4{,}413-310-530-85} = 0.44$, and the MVPF for ex-offenders is $\frac{304+2{,}489-369-219-32}{4{,}153-369-219-168} = 0.64$. If participants instead value the program at cost, the MVPF for ex-addicts is $\frac{4{,}413}{4{,}413-310-530-85} = 1.27$ and the MVPF for ex-offenders is $\frac{4{,}153}{4{,}153-369-219-168} = 1.22$. However, in neither of these cases do we observe information on the sampling uncertainty of the estimates; as a result, these estimates are excluded from the category averages.

For the adult women and youth target groups, we use figures from Hollister et al. (1984) for costs and fiscal externalities, as well as their results for reduced welfare receipt (\$2,615 for adult women and \$474 for youth). Couch (1992), meanwhile, finds present-discounted (at a 3-percent rate) earnings impacts of \$7,768 for adult women and \$3,207 for youths. At CBO-derived tax rates of 12.9% and 0.7%, respectively, these correspond to changes in tax payments of \$1,002 and \$22 (including taxes on direct payments to participants). Under the post-tax WTP assumption, the resulting MVPFs for adult women and youth are 1.48 and 0.60, respectively. However, neither of these estimates are statistically distinct from $-\infty$ or ∞ (i.e. we can't formally reject either that the program was a Pareto improvement or made participants worse off). This is further compounded when we forecast these gains for additional years; unfortunately, sampling uncertainty ultimately prevents a precise conclusion for these individual programs. Under the program-cost WTP assumption, meanwhile, the MVPF for adult women is 2.59 (95% CI: [1.16, ∞]) and the MVPF for youth is 1.28 (95% CI: [0.21, ∞]).

C.VII Job Corps

The Job Corps program is the largest vocational education program in the United States. Established in 1964, Job Cops is administered by the U.S. Department of Labor and provides job training and other services to at-risk youth between the ages of 16 and 24 via a network of centers run by local public and private agencies (Schochet et al. (2008)). Our estimate of the MVPF for Job Corps is based on primarily on two papers resulting from the National

Job Corps study, a randomized control trial involving around 80,000 eligible applicants between 1994 and 1996. These are Schochet (2018), which reports impacts on participants' earnings for 21 years following the program using Social Security earnings records and IRS tax data, and the short-run cost-benefit analysis in Schochet et al. (2006).

Schochet et al. (2006) estimate the program costs to be $16,158.[33] We then use the earnings and tax series estimated in Schochet (2018) to form our primary source of fiscal externalities and willingness to pay. For the fiscal externalities, we sum the tax liability impacts that are directly observed for years 6-20 in Schochet (2018), discounting at 3%. This yields $52. For years 1-5, we do not observe tax and transfer revenue, so we instead use the CBO rate of 0.3% applied the the earnings decrease in the first 5 years, which decreases tax revenue by $0.18. We sum this with the value of the produced output from the participants of $220; combining with the programmatic cost it implies a net cost of $15,886. If we extrapolate to 65 years, it implies a net cost of $15,832. We also consider an alternative specification that includes cost reductions from crime reduction, AFDC receipt, and spending on other forms of public assistance that are estimated in Schochet et al. (2006). Including these cost reductions implies a net cost of $12,624.

For WTP, we construct the after-tax earnings using the same series from Schochet (2018) but subtracting the tax and transfer payments, which equals $69. To this, we add the value of transfers provided to recipients in the program to cover their food and clothing of $2,314. Summing, this implies a willingness to pay of $2,383, and an MVPF of 0.15 (95% CI: [-0.23, 0.58]). If one assumes the earnings impacts persist in percentage terms throughout the life cycle to age 65, it implies an MVPF of 0.18 (95% CI [-0.53, 1.06]).

An alternative assumption is that Job Corps is valued at its programmatic cost. This would imply an MVPF of 1.00 (95% CI [0.96, 1.06]). And, yet another alternative assumption would include the additional cost reductions from crime, AFDC, and other public spending. This implies an MVPF of 0.180 (95% CI [-0.27, 0.81).

[33]The programmatic costs occur over several years and the authors use a 4% discount rate to discount these back to the upfront cost. For simplicity givent he short horizon of the program, we retain this number throughout our assumptions even though we use a 3% discount rate in our baseline assumptions. This means that for our baseline assumptions, the programmatic costs are slightly low since they would be a bit higher under a 3% discount rate assumption.

Table C.I: Job Training MVPF Estimates

	Earnings Projection Length			
	2-4 Years[1]	8 Years	21 Years	Until Age 65
	(1)	(1)	(1)	(1)
WTP Valuation: Post-Tax-and-Transfer				
JTPA Adult	1.38 [-0.38, 2.09]	5.59 [1.02, +∞]	44.34 [1.03, +∞]	+∞ [1.03, +∞]
JTPA Youth	-0.23 [-3.72, 1.24]	-1.30 [-∞, +∞]	-8.62 [-∞, +∞]	. [., .]
WorkAdvance	0.78 [0.26, 1.34]	2.63 [0.87, 4.67]	6.81 [2.27, 12.18]	8.85 [2.94, 15.90]
Year Up	0.43 [0.37, 0.48]	2.78 [2.10, 3.59]	+∞ [+∞, +∞]	+∞ [+∞, +∞]
JOBSTART	0.20 [0.04, 0.42]	0.83 [-0.07, 2.82]	10.31 [-0.54, +∞]	+∞ [-1.02, +∞]
NSW Adult Women	-	1.48 [-∞, +∞]	3.37 [-∞, +∞]	4.21 [-∞, +∞]
NSW Youth	-	0.60 [-∞, +∞]	0.58 [-∞, +∞]	0.55 [-∞, +∞]
Job Corps	-	-	0.15 [-0.23, 0.58]	0.18 [-0.53, 1.06]
WTP Valuation: Program Cost				
JTPA Adult	1.18 [0.57, 8.74]	1.59 [0.29, +∞]	5.46 [0.16, +∞]	+∞ [0.13, +∞]
JTPA Youth	1.10 [0.64, 12.22]	1.24 [0.27, +∞]	2.24 [0.09, +∞]	+∞ [0.05, +∞]
WorkAdvance	1.20 [1.20, 1.20]	1.21 [1.20, 1.22]	1.22 [1.21, 1.24]	1.23 [1.21, 1.26]
Year Up	1.04 [1.03, 1.05]	1.55 [1.40, 1.73]	+∞ [+∞, +∞]	+∞ [+∞, +∞]
JOBSTART	0.98 [0.80, 1.24]	1.12 [0.78, 1.88]	3.20 [0.70, +∞]	+∞ [0.61, +∞]
NSW Adult Women	-	2.59 [1.16, +∞]	3.19 [1.38, +∞]	3.45 [1.48, +∞]
NSW Youth	-	1.28 [0.21, +∞]	1.28 [0.21, +∞]	1.27 [0.22, +∞]
Job Corps	-	-	1.00 [0.96, 1.06]	1.01 [0.92, 1.11]

Notes: "-": estimate not shown because observed impacts extend beyond given projection length; ".": estimate has negative willingness to pay (-$13,382) and negative cost ($-87)

[1]Column (1) shows MVPFs derived from observed earnings impacts for JTPA Adult and JTPA Youth (30 months of observed impacts), WorkAdvance (3 years of observed impacts), Year Up (3 years of observed impats), and JOBSTART (4 years of observed impacts).

D Social Insurance

D.I Sample Frames

We begin the construction of our sample selection of social insurance policies using the handbook chapter of Chetty and Finkelstein (2013). They identify a recent selection of papers estimating the costs and benefits of Medicaid, Medicare, unemployment, and disability insurance policies.[34] We supplement this with Krueger and Meyer (2002) who survey articles on the behavioral responses to unemployment insurance, Buchmueller et al. (2015)who survey the literature on Medicaid. We add recent work that exploits random assignment and quasi-experimental variation in these areas when those studies can be readily used to form estimates of the WTP and cost of the policy - most notably recent work exploiting judge or examiner assignment in disability insurance (Maestas et al., 2013; French and Song, 2014; Gelber et al., 2017) and discontinuities in health insurance subsidies (e.g. Finkelstein et al., 2019). In building our sample, we require in nearly all cases that existing research has analyzed the impact of these polices on individual earnings.[35] We incorporate the costs impacts of healthcare utilization changes where evidence is available, but we do not require those estimates for the inclusion of given policies in our sample. We incorporate mortality impacts into willingness to pay where evidence is available, but we do not require those for the inclusion of given policies in our sample. Along the way, we discuss the robustness of our results to the potential omission of these effects. We begin our discussion of these programs with health insurance expansions to children.

D.II Health Insurance Expansions to Children

Building upon the survey of Buchmueller et al. (2015) combined with more recent work, we focus our attention on four policy variations that expanded public health insurance to children and pregnant mothers. First, we consider state Medicaid expansions to pregnant women and infants, first studied in the paper by Currie and Gruber (1996b). Second, we consider the national expansion in the 1990 Omnibus and Reconciliation Act that extended Medicaid to all AFDC-eligible children born after September 30, 1983. Third, we consider state-level expansions of Medicaid to children as studied in Brown et al. (2015), which directly estimates the fiscal impact of these expansions on the tax revenue paid by the children as adults today. And finally, we include the recent work of Goodman-Bacon (2017), who studies the long-run impacts of the introduction of Medicaid to AFDC eligibles and uses these estimates to form an MVPF of the reform. Here, we utilize this MVPF for comparison but also conduct some harmonization of the variables included in order to facilitate comparison across policies.

D.II.1 Medicaid Expansion to Pregnant Women and Infants

After the initial introduction of Medicaid to AFDC eligibles, many states expanded their Medicaid programs to provide eligibility to pregnant women and infants during the 1980s-90s. Currie and Gruber (1996b) exploit this variation across states over time to form a "simulated instrument" that attempts to isolate the impact of Medicaid eligibility, which they use to look at the impact on infant mortality; Cutler and Gruber (1996) study the impact on the crowd out of private insurance coverage; Dave et al. (2015) study the impact on labor supply of eligible women, and lastly Miller and Wherry (Forthcoming) study the impact on future earnings and health of children whose parents obtained Medicaid eligibility. Here, we translate these estimates into their implied MVPF, beginning with costs and then turning to willingness to pay.

Initial cost increases: Currie and Gruber (1996b) Currie and Gruber (1996b) report the cost of expanding Medicaid eligibility through the Medicaid expansions yields a cost of $202 (1986 USD) per woman made eligible if she were to have a birth.[36] Translating into 2011 dollars using the CPI-U-RS corresponds to $396. And, scaling by the estimate in Currie and Gruber (1996b) that 11.4% of women in their sample are pregnant in any given year (footnote 17), it suggests a total cost of $3,473 in 2011 USD of providing Medicaid eligibility to a pregnant woman. Throughout, we follow Currie and Gruber and consider expanding eligibility, as opposed to take up, as the eligibility provides option value to those choosing not to take it up. But, one can inflate to the set of people who took up the policy by scaling by the take-up rate of 34%, which suggests a cost of providing a pregnant woman with Medicaid was $10,216 per birth.

[34] They also study workers compensation programs, which will be included in a later iteration of this draft.

[35] We include the 1983 Medicaid expansion without any evidence on earnings because the healthcare savings are sufficient to produce a nearly infinite MVPF. Our findings suggest child health insurance expenditures have very hgh MVPFs so we consider this to be the inclusion of a conservative estmate. We also do not require estimates on earnings for our analysis of adult health insurance expenditures. [FILL IN].

[36] Note this is not the cost for pregnant women and children in their first year, but is taken by Currie and Gruber as a cost estimate.

In addition to the initial cost of providing Medicare, Dave et al. (2015) provide evidence that the provision of Medicaid reduces female employment. They estimate Medicaid eligibility leads to a 21.9% reduction in employment, where we draw this number from the specification in Table 2 corresponding to the regression without linear trends to be consistent with the original Currie and Gruber (1996b) identification strategy. They note that 66% of their sample is employed with a mean earnings of $8,541, implying an average earnings of the employed of $8,541/.66=$12,941. Hence, the 21.9% impact on labor force participation implies roughly an earnings impact of $12,941*.219 = $2,834. Assuming a 19.9% tax rate (CBO rate corresponding to 100% of FPL), this implies a net cost to the government of $564 per eligible child. We assume here these effects operate for a single year, roughly corresponding to the length of Medicaid eligibility offered through the expansion to the parents.

Crowd Out A portion of those who obtain coverage through the government would have otherwise obtained private insurance coverage. Cutler and Gruber (1996) estimate that half of those who obtain insurance through these expansions are from a switch from private to public coverage, as opposed to uninsured to public coverage. From the perspective of the MVPF, these crowd out estimates do not affect costs – the government pays the cost of the insurance regardless of whether those individuals would have otherwise been privately insured. But, they will affect the benefits as those individuals receive a transfer for any benefits they obtain by not having to purchase private coverage. Indeed, this transfer will form our lower bound willingness to pay estimate.

Uncompensated Care While crowd out does not directly affect the cost to the government, the fact that the low-income uninsured do not pay all their medical bills affects the incidence of the insurance benefits. There is a large and growing literature suggesting that much of the uninsured end up not paying the full cost of their care when they go to the hospital. Finkelstein et al. (ming) suggest more than half of costs charged by low-income uninsured adults is uncompensated to the hospital. Moreover, the estimates in Gold et al. (1987) estimate that 50% of cost of births by low-income mothers occur in public clinics financed by the government.[37] This suggests that at least 25% of the upfront cost of Medicaid is recouped through reductions in other forms of uncompensated care spending. This implies an upfront cost to the government of $2,605 (before considering fiscal externalities).

Later Cost Decreases: Miller and Wherry (Forthcoming) Using the same empirical design as Currie and Gruber (1996b), Miller and Wherry (Forthcoming) document reductions in future hospitalizations of cohorts whose parents obtained Medicaid in states through these expansions. They document that a 1 percentage point increase in eligibility leads to a reduction in hospitalizations of 0.237% when children are 19 to 32 years old. The average resource cost of hospitalizations for these cohorts is $8,135 in 2011 USD per hospitalization. Using a 3% discount rate to account for the fact that these cost reductions occur roughly 26 years later, the costs if they were paid at birth would be $3,772. Given a hospitalization rate of 374 per 10,000 individuals (0.0374), the discounted average cost of hospitalizations for these cohorts would be $141 (unconditional on hospitalization). This suggests that expanding Medicaid eligibility to the child's mother reduces later life healthcare costs by $33 per year ($33 = 0.237% *100* $141). Of course, not all these costs accrue to the government; assuming as above that 50% of these costs would have been paid by the government, it suggests an impact of roughly $17 on the government budget per year. Accounting for the fact that the effects are found in the data for children spanning 14 years, this suggests a reduction in costs of $239 per child enrolled. Assuming these yearly cost savings remain constant throughout the lifecycle up to age 65 this discounted cost reduction increases to $530.

Future Tax Revenue In addition to cost savings from reduced medical expenses, Miller and Wherry (Forthcoming) also document impacts on high school graduation and future earnings in some specifications. A 1 percentage point increase in eligibility leads to an increase in high school graduation of 0.028 percentage points and an increase in personal income of 0.116%. The sample mean earnings is $31,331 (in 2011 USD), implying that the counterfactual mean earnings is $30,246 per year. Thus the increase in personal income from a one percentage point increase in eligibility is $35.09. This implies a yearly increase in earnings of $3,509 in 2009 for those eligible and a discounted sum of earnings gains over the observed period of $21,860. This observed earnings increase can then be extended by forecasting forward over the lifecycle through age 65, using the methodology detailed in appendix I. Combining the observed and forecasted earnings impacts with a 3% discount rate and an assumed effective tax rate of 18.9% from the CBO rate corresponding to 200% of the FPL, this implies providing eligibility to an additional parent increases future tax revenue by $10,023.

[37] Gold et al. (1987) show that there were roughly 600K births by uninsured mothers per year (17% of the 3.4 million births each year). Moreover, roughly 600K births occurred in public clinics financed by the government, and roughly 300K of these births occurred to uninsured mothers. Hence, roughly half (300K) of the uninsured births per year occurred in public clinics.

Cost of College attendance While the increased tax revenue reduces the effective cost to the government, some of these savings are offset by the fact that the government subsidizes college attendance. Miller and Wherry (Forthcoming) find an increase in college attendance of 3.5 percentage points (Miller and Wherry (Forthcoming), Table 5); using the national average public expenditure on college in 1997 of $6,448, and assuming those induced to enroll attend for two years this implies a cost increase of $371. Summing together all of these components suggests a net cost of $-7,014.

Willingness to Pay Because we estimate that the policy paid for itself, the MVPF will be infinite as long as the total willingnesses to pay for the policy is greater than zero. However, for completeness and for illustration in alternative specifications in which the costs are not fully repaid, we outline the construction of willingness to pay. Broadly, we include three components: the transfer to parents, the WTP for reductions in infant mortality, and children's WTP for increased earnings in adulthood.

First, those who previously paid for insurance who now obtain Medicaid coverage instead effectively receive a transfer from the government. Cutler and Gruber (1996) estimate that as much as 50% of women who become eligible for Medicaid choose to drop their private insurance policy for which they were either paying out of pocket or for which they may have accepted lower wages. Assuming 50% of the market is crowded out of private insurance and that the cost of the private insurance is roughly equal to the resource cost of the expansion ($3,473 per year) it reflects a $1,737 transfer to the children per eligible pregnant mother. This provides a lower bound estimate of the willingness to pay.

In addition, there is a willingness to pay from reductions in infant mortality. Currie and Gruber (1996b) estimate that a 1 percentage point increase in Medicaid eligibility results in .028 fewer infant deaths per thousand births. To translate these into a WTP statistic relevant for the MVPF, one needs to multiply this by the parents' WTP for a decrease in the odds of losing a child (alternatively the child's own hypothetical willingness to pay for their life). There is of course considerable debate about this parameter, we assume here that individuals have a VSL of $1M in 2012 dollars, which is roughly on the low end of estimates in the literature (although our results are similar with VSL assumption between $0 and $9M). This means they are willing to pay $10K for a 1% reduction in the infant mortality (e.g. $1M VSL). This suggests a willingness to pay of $1M * 2.8/1,000 = \$2,800$. In 2011 dollars, the willingness to pay is $2,763.

Third, we consider the WTP by the children for improved labor market prospects in adulthood. While 18.9% of the the 11.6% increase in gross earnings accrues to the government in terms of increased taxes, individuals should be willing to pay for the remaining after-tax increase in income assuming that it reflects an increase in wages, not an increase in the utility cost of effort. Over the observed 14 years of earnings this suggests a willingness to pay of roughly $17,728, and an additional $26,236 extrapolating to age 65. Summing the components, this suggest a net willingness to pay of $48,353 (44K in earnings, plus 2,763 from infant mortality, and the $1,737 transfer from crowding out of private insurance).

Summary The resulting MVPF is infinite because costs are negative. This is true both for our baseline specification, and for our lower bound WTP specification (which does not incorporate any infant mortality reduction benefits). It also is true if one were to not project costs beyond the observed time frame, as the costs are recouped by age 32.

D.II.2 Medicaid Expansion to Children Born After September 30, 1983

In the 1980s, congress passed a series of expansions to Medicaid that provided coverage to children born on Oct 1, 1983 or later. Card and Shore-Sheppard (2004), use this discontinuity in eligibility to estimate the causal effect of this Medicaid expansion to children on healthcare utilization during childhood. More recently, Wherry et al. (2018) use this variation to look at later-life healthcare utilization and prevalence of chronic health conditions, Wherry and Meyer (2016) look at later life mortality. However, to the best of our knowledge there is no work looking at the impact of these policies on incomes. We translate the available estimates into their implied MVPF, noting the potential omission on child earnings could understate the MVPF. We begin with costs, and then turn to WTP.

Costs The cost of providing Medicaid to non-disabled children in 1991 was roughly $902 per year per enrolled child in 1991 USD Wherry et al. (2018), which corresponds to $1,484 in 2012 USD using the CPI-U-RS. For those obtaining insurance eligibility through the expansion, 44.6% take up Medicaid. This implies a cost per child-year of Medicaid eligibility of $663. We assume this cost is spread uniformly over ages 8 through 14, and discount to age 8 using a 3% interest rate, which gives a discounted cost of $528. Part of this cost results from the causal effect of obtaining insurance coverage, which by the envelope theorem may not reflect the value of insurance. Indeed, using the birth discontinuity as an instrument for Medicaid coverage for the child, Card and Shore Shepard find an increase in the likelihood of a child having seen a doctor of 60% (s.e. 31%).[38]

Part of the cost paid by the government reflects compensation for care that otherwise would have gone uncompensated. Lo Sasso and Seamster (2007) use variation in childhood expansions of Medicaid to document that each additional child eligible for Medicaid reduces costs by $17 through both reductions in charity care and default on medical debt. The discounted value of these savings over the seven years of increased eligibility is $13.67. Assuming this falls to the government, it implies the net cost of eligibility to the government is $645 per year per child.

Wherry et al. (2018) revisit the birth discontinuity to look at health outcomes when children are 25 years old. They find significant reductions in chronic health conditions and healthcare utilization such as hospitalization and emergency room visits. Because they do not directly observe parental eligibility for Medicaid, they focus their analysis on the subset of Black families in their sample. And, we focus our estimates here on the CCT bandwidth specification, but we find similar conclusions if we were to use the IK bandwidth estimates. Those born just after Sept 30, 1983 are 7.7 percentage points more likely to be covered (Wherry et al. (2018) Table 2). Correspondingly, they find significant reductions in hospitalization and prevalence of chronic conditions. Using the CCT bandwidth selector, Appendix Table 24 shows that being born after the Sept 30 cutoff leads to a 0.164 reduction in log costs of publicly insured hospitalizations and 0.049 reduction in log costs of publicly insured emergency department visits.

Translating these reductions into the dollars recouped by the government requires the base levels of public spending on these populations. Using the summary statistics from the paper for the sample of Black children, the authors' calculations imply a total average cost of hospitalizations of $402 per year and $231 for emergency room expenses in 2012 USD[39]. While the authors do not have a breakdown of the total costs paid by the government, Wherry et al. (2018) estimate that between 41-69% of total costs for this sample are paid by the government – we take the midpoint of 55%. This suggests public expenditures of $221 on hospitalizations and $127 on emergency room costs per person per year. Multiplying by the 16.4% and 4.9% reductions, respectively, yields a total cost savings to the government of 36.25+6.24 = $42.49 (2012 USD) per person per year. Discounting these increased revenues back to the time when the initial outlays were made (when children were 8 at the earliest) yields a total cost savings of $26 using a 3% discount rate. These cost savings compare those born Oct 1 versus Sept 30, 1983 to those born just before. This discontinuity corresponds to a 0.87 increase in child-years of eligibility for Medicaid. This suggests that the discounted cost savings per year of Medicaid eligibility are $30 when individuals are 25 years old.

Because these reductions in expenditures are argued to be driven by reductions in chronic conditions that persist over time, our baseline specification assumes these effects persist as the individuals age. To account for this, we make the arguably conservative assumption that cost savings remain constant in levels, despite the growth in costs over the age distribution. We then discount these back to age 8 using the real interest rate assumption of 3% for our baseline specification. For consistency with the earnings forecasts, we assume the cost savings persist until age 65.[40] But, our results suggest that the cost savings to government pays for itself by the time children are age 54.

In addition to impacts on future health costs, there could also be impacts on future earnings and tax revenue. To that aim, Brown et al. (2015) use administrative tax data to compute the mean tax revenue for children in their 20s and early 30s separately by birthday around the Sept 30 1983 threshold. They argue that their results suggest an earnings impact of the 1983 discontinuity. However, their results in Appendix Figure OA.3 reveal apparently no significant changes in tax revenue around the discontinuity (but they do observe a discontinuity at the year end of Jan 1, 1984). However, data limitations prevent them from restricting their analysis to particular races or samples with higher Medicaid coverage rates. Nonetheless, we proceed in an arguably conservative manner by assuming no impact on earnings.

WTP We incorporate two components of willingness to pay. First, as noted above, cost of providing Medicaid to non-disabled children in 1991 was $1,484. Scaling by the take-up rate (44.6%), the cost of eligibility is $663. Spreading out the average gain in eligibility over ages 8 to 14 and discounting associated costs back to age 8 suggests that the discounted costs of eligibility gains and the uncompensated care costs sum to $528 and $14, respectively.[41] Scaling the costs of eligibility by the moral hazard rate (0.6) and subtracting off uncompensated care costs yields the valuation of the insurance at its mechanical program cost, which forms a lower bound estimate of the WTP. The corresponding discounted value is $317.

[38]They also find no evidence that the expansion of Medicaid coverage led to a crowd out of private coverage.

[39]See Wherry et al. (2015) p.27

[40]This is not because perhaps the health effects go away but rather because it is natural to assume that at some point there are naturally competing risks driving medical costs.

[41]Also, note that Card and Shore-Sheppard (2004) find no impact on private coverage or evidence of crowd out.

Next, Wherry and Meyer (2016) use the Oct 1, 1983 birth discontinuity to estimate the causal effect on childhood Medicaid eligibility on later-life mortality as children and teenagers. They find that the annual mortality rate for Black children born after September 30,1983 is lower than for those born after the cutoff by 0.443 per 10,000 children between the ages of 15 and 18. Assuming a VSL of $1M, the value of the mortality reductions in each of these years is $44. Discounting and aggregating the mortality reductions over ages 15-18 back to age 8 (the earliest age of when the Medicaid investments are made) suggests a willingness to pay of $138. Combining with the medical spending reductions yields a WTP of $454, which provides our point estimate for the willingness to pay in our baseline specification.

In the process, we ignore two additional likely benefits of the policy that could be incorporated in future work. First, the insurance likely provides an insurance value to the parents beyond the transfers value. Second, Wherry et al document a significant reduction in chronic health conditions for children as young adults. Children's willing to pay for these improvements would further increase their willingness to pay, and the MVPF, which we do not include.

Combining these estimates, our results suggest an infinite MVPF for both the baseline specification and lower bound specification, with a confidence interval of [0.26, ∞] for the baseline and [0.03, ∞] for the lower bound specification.

D.II.3 State Generosity Expansions for Children

In addition to the 1983 birth discontinuity that increased Medicaid access across all states, there is also state-level variation in expansions of Medicaid programs throughout the past several decades through programs such as the State Children's Health Insurance Program (SCHIP). Levine and Schanzenbach (2009) finds evidence that these expansions lead to an increase in test scores, and Cohodes et al. (2016) find evidence for an increase in educational attainment of the children who obtain childhood eligibility for Medicaid. Most recently, Brown et al. (2015) estimate the effect of these health insurance expansions on tax revenue and future government expenditures, which forms the basis of the MVPF construction here.

Brown et al. (2015) estimate that it costs Medicaid roughly $593 for every additional child-year eligible for the program. They also estimate that the program increased tax revenue by $533 through age 28. However, this estimate includes payroll taxes; for consistency with our other MVPF estimates, we subtract the payroll tax share from this estimate using the results in Table OA.9. This suggests that 7.5% of the tax revenue is from payroll taxation, and thus the non-payroll tax fiscal externality is $493.

As shown in Figure 2f of Brown et al. (2015), the authors find an increasing impact on tax revenue over time, implying the effects likely continue beyond age 28. For consistency with other programs, we forecast the individuals' earnings over the lifecycle. We assume the observed tax rate through age 28 and then use an effective tax rate of 18.9% from the CBO rate corresponding to 200% of the FPL in our projection through age 65. This suggests the policy increases tax revenue by $1,023 by age 65. Beyond income, it is important to note that the paper does not observe spending on healthcare (by the individuals or by the government). If the expansions improved health and reduced future medical spending, this would be a further cost reduction that is not included in our estimates.

In addition to taxes, Brown et al. (2015) find increases in future earnings and college enrollment and reductions in future mortality. The authors estimate that every additional child-year of Medicaid eligibility leads to an increase in after tax earnings from 19-28 of $644 un-discounted, and $304 discounted at 3%. Projecting the income gains at age 28 forward we estimate that a further $3,296 in post-tax earnings gains would be realized. The increase in college enrollment suggests the increase in earnings reflects increases in labor market opportunities, and thus we include the total post-tax earnings gain of $3,599 in the WTP of the beneficiaries. When considering costs we subtract the average public cost of enrolling in college of $8,014 per year, which increases government costs by $58. When considering WTP we also subtract the private cost of college enrollment, which reduces WTP by $16.

In addition to earnings, Medicaid eligibility reduces mortality rates. Every additional child-year of Medicaid eligibility reduces mortality through age 28 by 0.02%. Using a $1M VSL (2012 USD) and assuming the benefits accrue 23.5 years later (i.e. in the middle of the age range in the sample, discounted to age 0) it suggests a WTP for Medicaid eligibility from reduced mortality of $98. This implies a total WTP of $3,599+98-16 = $3,681. Our estimates of WTP also do not include any benefits to parents from the crowd out of private insurance policies and no valuation by the parents from reduced personal expenditures on children's healthcare. Combining, our resulting MVPF is infinite (95% CI [-0.36, ∞]).

D.II.4 Introduction of Medicaid to "Categorically Eligible" AFDC recipients

Goodman-Bacon (2018) and Goodman-Bacon (2017) study the impact of the introduction of Medicaid in the 1960s to "categorically eligibles", primarily consisting of families enrolled in AFDC. Using state variation in AFDC enrollment across states prior to the national Medicaid expansion, Goodman-Bacon conducts a difference in difference design for differential childhood exposure to Medicaid on adult outcomes for children born in the 1936-1976 cohorts. Goodman-Bacon then documents the impact on taxes and transfer spending on children in adulthood.

In contrast to the other childhood Medicaid papers, Section VII.D of Goodman-Bacon (2017) directly constructs the MVPF of the policy. They report an MVPF of 10.24. For our baseline specification, we replicate the estimates of Goodman-Bacon (2017). The approach for calculating the MVPF in Goodman-Bacon (2017) is similar to our approach with three exceptions. First, the willingness to pay component does not include a willingness to pay component arising from the impact of the policy on after-tax income. Figure 9 of Goodman-Bacon (2017) shows these effects are perhaps fairly small, with the main impact of Medicaid to replace transfer income with labor market income. Second, all effects are discounted using the realized nominal return on Treasury bills, instead of our main approach of using the CPI-U-RS combined with a 3% real interest rate assumption. Third, it includes a small change in net government costs due to changes in longevity. Fourth, the policy does not extrapolate to age 65, but rather uses the observed age distribution. We bootstrap the programs fiscal externality using the 5/95% CI reported for the Public Return in Table 9, which implies an standard error of $430. We then assume this is the only source of uncertainty in the MVPF, which leads to a slightly wider confidence interval for the MVPF that has a 5% boundary of 0.26, instead of the 1.3 reported in Table 9.

D.III Health Insurance Expansions to Adults

We form our sample frame of policies targeting adults by searching through work in Buchmueller et al. (2015) and Gruber and Madrian (2004). As noted in this work, most of the health insurance expansions have generally targeted children. Here, we focus on two recent papers that estimate both the willingness to pay and cost implications of recent health insurance expansions to non-elderly adults: variation in health insurance subsidies to low-income adults studied in Finkelstein et al. (2019), and the expansion of Medicaid via lottery to low-income categorically ineligible adults through the Oregon Health Insurance Experiment. For the latter, we rely on the quantification of willingness to pay and costs from Finkelstein et al. (Forthcoming).

In addition to non-elderly adults, we focus on two policies targeting the elderly. For the elderly, the primary health insurance program is Medicare. Because the program operates at a national level, there have historically only been two sources of exogenous variation used to identify its effects. First, Finkelstein (2007) uses the cross-region variation in pre-Medicare insurance rates to analyze the impact of Medicare; and Finkelstein and McKnight (2008) uses this same variation to conduct a cost-benefit analysis of the introduction of Medicare. Our analysis translates the estimates from this cost benefit analysis of Finkelstein and McKnight (2008) into their implications for individuals' willingnesses to pay for Medicare and the net cost to the government of the policy.

In addition to the introduction of Medicare, Card et al. (2009) use the discontinuity in Medicare eligibility at age 65 to study the impact of Medicare on mortality. However, while Card et al. (2009) find impacts on utilization and mortality, we do not measure an MVPF for the introduction of Medicare for two reasons. First, Card et al. (2009) suggest that their mortality effects must be operating through a channel that goes beyond just the provision of health insurance to those above 65. Second, they do not have direct measures of the costs associated with their increased utilization. For these reasons, we are unable to reliably construct an MVPF for the eligibility discontinuity at age 65.

Lastly, we also consider a welfare analysis of Medigap, using the recent work of Cabral and Mahoney (2019), who quantify the impact of Medigap on Medicare costs and discuss the implications for optimal taxation of Medigap. We translate this analysis into its implied MVPF.

We begin with our discussion of the policies targeting non-elderly adults, followed by Medicare and Medigap.

D.III.1 Oregon Health Insurance Experiment

Traditionally, Medicaid programs have been provided to children and families with children, or those on social security / disability income. However, recently there have been more expansions of Medicaid programs to low-income adults who are not categorically eligible (e.g. on AFDC). One such expansion occurred in 2008 when Oregon used a budgetary windfall to extend health insurance coverage to low-income single adults who were not categorically eligible for Medicaid (i.e. not on other forms of welfare like SSI). The program was over-subscribed and thus they used a lottery to restrict enrollment.

Finkelstein et al. (2012) use this lottery to generate random variation in Medicaid coverage. They find that Medicaid increased health care use across all categories of care, including outpatient care, preventive care, prescription drugs, hospital admissions, and emergency room visits. They also find that Medicaid improved self-reported health and reduced depression. However, they do not detect any statistically significant impact on mortality or physical health measures. In contrast, they find large reductions in out of pocket medical spending and reduced incidence of unpaid bills.

Finkelstein et al. (Forthcoming) translate the estimates of the causal effects of the program, combined with measures of the QALY for the health benefits, into a measure of recipients' willingness to pay for Medicaid. Using three different approaches to measure the insurance value of Medicaid, they find a WTP of $793, $1,421, and $1675 for the average complier enrollee. As discussed in Finkelstein et al. (2019), the analysis above does incorporate the valuation individuals have for any potential long-run impact of insurance on their health. But, it accomplishes this through its measured impact on self-reported health and reduced depression. Since the experiment occurred in 2008, it could of course be the case that future gains have realized. However, it is important to note that the estimates suggest that the program did not have statistically significant improvements in physical health in the short or medium run and seems to have had no statistically distinguishable impact on earnings of the enrollees, although the confidence intervals are quite large.

The cost to the government of covering the medical costs of those who took up the insurance is $3,600. As noted above, much of the impact of Medicaid is to reduce the prevalence of unpaid bills. Indeed, Finkelstein et al estimate that the net resource cost of covering these medical costs is $1,440, with the difference comprising transfers from the government to the providers of uncompensated care. These may be transfers to hospitals and other insureds, or other creditors of the uninsured who bear the incidence of unpaid medical debt. Moreover, some of these costs may ultimately be born by the government through reductions in hospital-level Medicaid payments through the DISH program or through reductions in tax deductions. We therefore consider a baseline case in which the cost savings from uncompensated care fall to the government and a robustness case in which there are other individuals paying the uncompensated care. In this latter case, we suppose they have the same social welfare weight as the beneficiaries, but alternative scenarios could discount this as discussed in Finkelstein et al. (2019).

Assuming the ultimate cost of uncompensated care is borne by the government suggests the cost to the government of providing Medicare is $1,440. This is higher than the mechanical increase in expenditure that would have been $1,147 in the absence of behavioral responses. The willingness to pay for the insurance varies depending on the method used to calculate. Their WTP estimates range from $1675 for the "complete information approach", $1421 for the "consumption-based optimization approach", and $793 for the "health-based optimization approach. Subsequently, these translate into an MVPF of 1.16, 0.99, and 0.55 respectively. These are shown in Appendix Table D.I.

Instead of assuming the government pays for the cost of uncompensated care, one can also assume that individuals themselves bear the cost of this care. In this case, the WTP rises to $3835, $3581, and $2953 in the three approaches. But, the cost to the government also increases to the full $3600. Hence, the MVPF in the three cases is given by 1.07, 0.99, and 0.82 respectively. These too are shown in Appendix Table D.I.

D.III.2 Health insurance subsidies to low-income families in Massachusetts

Medicaid provides insurance to qualified families and low-income adults generally below the poverty line or 133% of the poverty line. As part of their 2006 health reform, Massachusetts created health insurance subsidies to low-income adults and families not eligible for Medicaid, with incomes between 133% and 300% of the federal poverty line (FPL). The subsidies varied discontinuously as a function of FPL, with premiums increasing at 150%, 200%, and 250% FPL. Finkelstein et al. (2019) use these estimates to form measures of the willingness to pay and cost of these subsidies.

In contrast to the settings above in which beneficiaries' willingnesses to pay are not observed, the MA setting provides a direct measure of WTP. Assuming rational behavior, this circumvents issues about not observing long-run outcomes, as one can directly infer the valuation from the WTP. Finkelstein et al. (2019) find that willingness to pay is roughly 30% of the cost paid by the MA insurance program. However, prior work suggests that roughly 70% of care to these individuals goes uncompensated (Finkelstein et al. (2019)), so that individuals are roughly willing to pay their resource costs of coverage. As in the case of the Oregon example above, the primary baseline assumption is that the government pays the uncompensated care. This is conservative from the perspective of the main conclusions of the paper, as it leads to the highest MVPFs amongst our specifications.[42]

We form the MVPF using estimates of the willingness to pay and cost for insurance in MA. When prices are observed, willingness to pay for changes to insurance subsidies is straightforward. Lowering the cost of health

[42]One potential limitation of the analysis in Finkelstein, Hendren, and Shepard (2018) is that their cost measures only include healthcare expenditures, as opposed to any labor supply or labor earnings distortions in response to trying to enroll in the policy. Additionally, we do not incorporate any reductions from future government costs on medical spending; but given the low WTP, it would be consistent with not much significant future health improvements.

insurance by \$1 through a subsidy has a WTP of \$1 for the individuals choosing to purchase insurance.[43] The cost of the subsidy is the \$1 plus an additional cost from the new enrollees. Let f denote the number of new enrollees as a fraction of the current size of the market. These new enrollees both pay a premium, p, and impose a cost on the insurer, c. They therefore impose a positive (negative) cost on the insurer if the premium is below (above) their costs. The MVPF is given by

$$MVPF = \frac{1}{1 + \frac{p-c}{f}}$$

Finkelstein et al. (2019) estimate the subsidies, marginal costs, and fraction insured at different income thresholds, and they focus their analysis on those at 150% FPL, in which roughly 90% of the population is insured. For this specification, the MVPF is 0.8.[44] If we were to assume that the government did benefit from uncompensated care reductions, the MVPF would be lower.

We can also use the demand and cost curves in Finkelstein et al. (2019) to compute the MVPF at the 200% and 250% FPL thresholds. These yield MVPFs of 0.85 and 1.09 respectively. The MVPFs are higher at 250% FPL because the subsidies are lower so that the willingness to pay (p) exceeds the cost the marginal individual imposes on the government (c).

D.III.3 Introduction of Medicare

Finkelstein and McKnight (2008) study the introduction of Medicare in the late 1960s using variation in the pre-existing fraction of the population that was insured. They use this variation to estimate the causal effect of Medicare on healthcare utilization, health, and consumption smoothing. They find that Medicare reduces out of pocket spending by \$117. It also reduces expenditure on private health insurance by \$507; but results in an increase in public insurance spending by the government of \$766, of which \$142 is the result of increased healthcare utilization.

From a willingness to pay perspective, individuals should value value insurance through the reduction in expenditure on private health insurance of \$507 and the fact that they no longer have to pay the out of pocket spending of \$117, for a total of \$624. Thus, if Medicare were simply a transfer program it would have an MVPF of \$624 / \$766 = 0.82, as shown in Appendix Table D.I. But, in addition to the transfer, Finkelstein and McKnight (2008) calibrate a model of utility to incorporate the value of insurance in reducing consumption dispersion. Assuming a coefficient of relative risk aversion of 3, they find an additional willingness to pay for improved consumption smoothing of \$585.[45] Incorporating this consumption smoothing benefit suggests a WTP of \$1,209, which implies an MVPF of 1.58. In addition to the consumption smoothing benefits, they also estimate a small and statistically insignificant effect on mortality. Using a VSLY of \$100K per year, they find a WTP from improved health of \$36. This implies an MVPF of 1.63. This latter specification including all benefits forms our baseline estimate.

D.III.4 Medigap

Lastly, we consider the impact of a tax/subsidy for the Medigap program. Medigap policies help cover the remainder 20% of medical expenses that Medicare does not pay. While Medigap is conducted by private insurers, they only bear at most 20% of the cost from the behavioral response to the insurance policy. To that aim, Cabral and Mahoney (2019) study the impact of Medigap on healthcare utilization and use their results to consider about a policy that taxes Medigap. We translate these analyses into their implied MVPF of a Medigap tax. We consider the specification that analyzes the impact of going from a 5% to 0% tax on Medigap from Table 9 of Cabral and Mahoney (2019).

The willingness to pay for the tax is simply the mechanical impact. The premiums are \$1779, so the mechanical impact of removing the 5% tax is 5%*1779 = \$89 per Medigap enrollee, or \$39.14 per Medicare enrollee since

[43] One could have imagined asking individuals their WTP prior to when individuals knew their health condition. Hendren (2018) provides a method to translate the estimates in Finkelstein Hendren and Shepard into an ex-ante measure of WTP. For our baseline specification in which 90% of the market owns insurance, the distinction between ex-ante and observed WTP is not large, and thus we simply use observed WTP. But, in other cases one could consider an ex-ante notion of WTP. Indeed, we estimate that if 30% of the market owned insurance the value of the subsidies could be larger, rising from 1.29 to 1.78.

[44] Unfortunately, they do not provide a standard error, and thus we omit this specification from our domain averages.

[45] However, it is important to note that the consumption smoothing estimate is parameterized and, admittedly by the authors, sensitive to their specifications. For example, if individuals could smooth some of their expenditure shocks, the willingness to pay would be lower. Nonetheless, we include these benefits as part of our conservative approach to attempt to have any bias in our results not drive our key finding that MVPFs are higher for policies targeting children.

Medigap has 44% market share with the 5% tax. This $39.14 is the willingness to pay to avoid the tax.[46]

The cost of the tax change has two components, the lost tax revenue, $39 and the externality from increased Medicare expenses is $60. So, the government collects $99. Hence, the MVPF is 0.40 (=39.14/99).[47] Intuitively, to the extent to which Medigap coverage imposes an externality on the government's Medicare program, taxing Medigap is a comparatively efficient source of raising revenue.

[46]We do not incorporate statistical uncertainty into this willingness to pay estimate because we observe premiums and are considering a fixed tax.

[47]To calculate the standard error of the costs, we take the estimates from Cabral and Mahoney (2018) when they consider a 15% tax adjustment and note: "we calculate that the standard error of our baseline estimate of 4.3 percent total savings is 1.7 percentage points." We assume a t-stat of 4.3/1.7 = 2.53 for the case where cost = 0.

Table D.I: Adult Health MVPF Estimates

Policy	Specification		MVPF
	WTP method	Incidence of uncompensated care	
Oregon Health Insurance Experiment	Complete Information	Government	1.163 [1.086, 1.242]
	Complete Information	Individuals	1.065 [1.035, 1.097]
	Consumption Optimization	Government	0.987 [0.757, 1.224]
	Consumption Optimization	Individuals	0.995 [0.903, 1.090]
	Health Optimization	Government	0.551 [0.017, 1.099]
	Health Optimization	Individuals	0.820 [0.607, 1.040]
	Income as % of federal poverty line		
Health Insurance in Massachusetts	150		0.8
	200		0.847
	250		1.089
	Consumption smoothing	Value of a statistical life included	
Medicare Introduction 1965	Yes	Yes	1.626 [0.517, 3.829]
	Yes	No	1.579 [1.281, 3.094]
	No	No	0.815 [0.799, 0.818]
Taxing Medigap	Baseline		0.395 [0.222, 1.540]

D.IV Unemployment Insurance

There is a large literature studying optimal UI policies, often focused on estimating the "Baily-Chetty" condition (Baily (1978); Chetty (2006)). The Baily-Chetty condition asks whether individuals are willing to pay the net cost of additional unemployment insurance. The WTP for $1 of additional UI benefits is often measured as $1 + \gamma \frac{\Delta c}{c}$, where γ is a coefficient of risk aversion and $\frac{\Delta c}{c}$ is the impact of unemployment on consumption.[48] The cost of $1 of UI can be written as $1 + FE$, where the fiscal externality from providing $1 of additional unemployment insurance is measured from the impact of additional UI on unemployment duration.

If individuals were required to pay the cost of their additional unemployment insurance benefits, then additional UI would increase (decreases) welfare if $\gamma \frac{\Delta c}{c}$ is greater (less) than FE. But in practice, beneficiaries of additional UI benefits are not necessarily the ones who pay for the cost of those benefits. Therefore, we use these same components to estimate the MVPF of UI:

$$MVPF^{UI} = \frac{1 + \gamma \frac{\Delta c}{c}}{1 + FE} \tag{1}$$

While the Baily-Chetty condition generally thinks of UI as solely a social insurance policy; the MVPF places UI in the broader context of policies with distributional incidence. This means that additional spending on UI benefits may be desirable not solely because of its correction of market failures, but also because it could be a more efficient method of redistribution.

Sample Frame We build our sample of variation in unemployment insurance policies using the survey article of Schmieder and Von Wachter (2016). They survey the literature on two types of policy changes to unemployment insurance: (i) changes in the size of benefits and (ii) changes in the duration of benefit availability. Conveniently, Schmieder and Von Wachter (2016) provide estimates of both $\gamma \frac{\Delta c}{c}$ and FE from these policies, including both benefit changes and duration changes. We restrict this sample to policy changes in the United States and for which Schmieder and Von Wachter (2016) provide a fiscal externality estimate. These are:

- Card et al. (2015), which exploits a kink in the UI benefit schedule for Missouri to examine differential effects of UI on labor supply in expansionary and recessionary periods. (Benefits.)
- Chetty (2008), which exploits a difference-in-difference comparison of the maximum UI benefit level across states. (Benefits.)
- Johnston and Mas (2018), which uses a regression discontinuity design to study the effects of an unexpected 2011 cut to potential benefit duration in Missouri. (Extensions.)
- Katz and Meyer (1990), which uses time-varying cross-state variation in UI program parameters to identify effects on labor supply. (Benefits and extensions.)
- Kroft and Notowidigdo (2016), which uses variation in UI benefits within states across time, interacted with variation in state unemployment rates. (Benefits.)
- Landais (2015), which exploits kinks in the UI benefit schedules for five states in the late 1970s and early 1980s. (Benefits.)
- Meyer and Mok (2007), which uses a difference-in-differences design for an unexpected increase in benefits for a subset of UI claimants in New York. (Benefits.)
- Solon (1985), which uses a difference-in-differences design to analyze the effects of making UI benefits taxable for high-income households in Georgia. (Benefits.)

As noted below, in this Appendix we also include an additional set of MVPFs that incorporate impacts of UI policies on disability insurance take-up, using estimates from Lindner (2016) and Mueller et al. (2016).

[48] This is derived as a first order approximation from a Taylor expansion of the utility function assuming consumption is the only argument affecting the marginal utility of income.

Cost Estimates We use the estimates of the net fiscal externality per dollar of government spending from Schmieder and Von Wachter (2016), who directly calculate the externality resulting from behavioral responses in the form of increased durations of non-employment. For each paper, we use the fiscal externality calculated by Schmieder and Von Wachter (2016)under the assumption that the lost wages from these longer spells are taxed at the 2014 average U.S. labor tax wedge (31.5%). Fiscal externalities from benefit expansions are taken from their Table 2, while fiscal externalities from duration extensions are taken from their Table 1; we calculate MVPFs using all U.S.-focused studies for which Schmieder and Von Wachter (2016)provide an FE estimate.

Following Schmieder and Von Wachter (2016), our baseline analysis of the cost of UI focuses largely on unemployment duration, and it assumes that UI has no impact on other labor market outcomes such as job quality or total unemployment claims. However, Appendix Table D.II shows the effects of including spillovers onto DI using estimates from recent work by Lindner (2016) and Mueller et al. (2016) on the impact of UI on applications for disability insurance. To do so, we adjust Schmieder and Von Wachter (2016)'s fiscal externality estimates to include possible effects on government expenditures from changes in DI applications resulting from more generous UI.

In these alternative specifications, we adjust our benefit increase specifications using estimates from Linder (2016, Table 6, Column 4), who find that $100 of additional UI benefits each month leads to a reduction in the probability of applying for DI of 6 percent.[49]Using baseline DI application and approval rates of 1.68 percent and 60 percent, respectively, from Lindner (2016), and an assumption that UI benefits are claimed for four months on average, this implies a reduction in the probability of DI receipt of 0.00016 percentage points for each additional dollar of UI benefits.

[49]Lindner's logit-model estimates are semielasticities of the log odds ratio of DI application with respect to the UI monthly benefit amount (in hundreds of dollars). However, as the author notes in footnote 20, for small probabilities these can be interpreted as semielasticities of the conditional probability of application.

Table D.II: Unemployment Insurance MVPF Estimates

	FE without DI Spillovers	FE with DI Spillovers	WTP	MVPF without DI Spillovers	MVPF with DI Spillovers	Notes
	(1)	(2)	(3)	(4)	(5)	
Duration Extensions						
Johnston and Mas (2018)	0.57	0.58		0.83	0.82	Regression discontinuity design to study effects of unexpected 2011 cut to potential benefit duration in Missouri.
	[0.46, 0.69]	[0.47, 0.70]	1.30	[0.76, 0.90]	[0.75, 0.90]	
			[1.24, 1.36]			
Katz and Meyer (1990)	1.89	1.90		0.45	0.45	Uses time-varying cross-state variation in UI program parameters.
	[-0.39, 4.19]	[-0.39, 4.21]		[0.25, 2.12]	[0.25, 2.07]	
Benefit Increases						
Card et al. (2015), Expansion	0.59	0.26		0.74	0.93	Exploits a kink in Missouri UI benefit schedule to examine differential effects of UI on labor supply in expansionary and recessionary periods.
	[0.46, 0.73]	[-0.25, 0.79]		[0.67, 0.81]	[0.64, 1.51]	
Card et al. (2015), Recession	1.68	1.35		0.44	0.50	
	[1.36, 2.01]	[0.77, 1.95]		[0.39, 0.50]	[0.40, 0.65]	
Chetty (2008)	0.71	0.38		0.68	0.85	Exploits difference-in-difference comparison of the maximum UI benefit level across states.
	[-0.01, 1.41]	[-0.46, 1.28]		[0.48, 1.13]	[0.51, 2.10]	
Katz and Meyer (1990)	1.74	1.41		0.43	0.49	Uses time-varying cross-state variation in UI program parameters.
	[0.51, 3.17]	[0.11, 3.01]	1.17	[0.28, 0.78]	[0.30, 1.06]	
			[1.11, 1.22]			
Kroft and Notowidigdo (2016)	1.43	1.10		0.48	0.56	Uses variation in UI benefits within states across time, interacted with variation in state unemployment rates.
	[-0.21, 2.90]	[-0.67, 2.69]		[0.30, 1.69]	[0.32, 3.92]	
Landais (2015)	0.40	0.07		0.84	1.09	Exploits kinks in the UI benefit schedules for five states in late 1970s and early 1980s.
	[0.28, 0.52]	[-0.44, 0.58]		[0.76, 0.92]	[0.73, 2.03]	
Meyer and Mok (2007)	0.31	-0.02		0.89	1.20	Difference-in-differences design for unexpected increase in benefits for a subset of UI claimants in New York in late 1980s.
	[0.21, 0.41]	[-0.52, 0.51]		[0.82, 0.97]	[0.76, 2.34]	
Solon (1985)	0.14	-0.19		1.03	1.45	
	[0.09, 0.18]	[-0.68, 0.32]		[0.97, 1.09]	[0.88, 3.49]	Difference-in-differences design analyzing effects of making UI benefits taxable for high-income households in Georgia.

For duration extensions, we use estimates from Mueller et al. (2016, Table 2, Column 1) who find an elasticity of monthly SSDI applications with respect to monthly UI exhaustions of -0.0025. This implies that a 1-percent increase in exhaustions leads to 0.0025 percent fewer SSDI applications[50]. We translate this into the costs per dollar of UI spending as follows. Consider a hypothetical experiment where UI benefits are extended by one month. Mueller et al. (2016) assume that the marginal SSDI applicant has a job-finding rate of roughly 10 percent (p. 25), and so we assume that this hypothetical extension prevents 10 percent of UI-exhaustee-to-DI individuals from exhausting their benefits (since they are able to find a job during the four-week extension). Given Mueller et al. (2016)'s further assumptions (p. 25) that (i) weekly UI payments average around $300; (ii) there are roughly one-fifth as many SSDI applications as UI exhaustions in a given month; and (iii) about 60 percent of DI applications are approved, the resulting total change in the probability of DI receipt per dollar of extended benefits is thus:

$$(0.0025 * 0.2 * 0.1 * 0.6)/(4 * 300) = .000000025$$

Finally, we multiply each of these changes in the probability of DI receipt per dollar of UI[51] by the present-discounted fiscal cost of a DI award. For the cost of SSDI and Medicare benefits per award, we use figures for new SSDI beneficiaries in 1997 from Appendix Tables G3 and G4 of Von Wachter et al. (2011). To account for additional fiscal externalities resulting from DI receipt (e.g. from lost earnings), we further multiply the Von Wachter et al. (2011) by the per-dollar fiscal externality of a DI award from our estimate based on Maestas et al. (2013) described in detail below. In total, these calculations for spillovers to DI result in an additional fiscal externality of -$0.33 per dollar of UI for benefit expansions, and $0.008 per dollar for duration extensions.

Willingness to Pay As noted above, it is common in the literature on optimal unemployment insurance to approximate individuals' willingness to pay for $1 of additional unemployment insurance by $1 + \gamma\frac{\Delta c}{c}$ We use this approximation for our WTP measures , assuming a coefficient of relative risk aversion of 2 for consistency with Table 3 of Schmieder and Von Wachter (2016). For the impact of unemployment on consumption, we follow Hendren (2017b), who adjusts the analysis of Gruber (1997) to account for the fact that consumption diverges prior to the onset of unemployment. With this adjustment, Hendren (2017b) finds that unemployment leads to an 8-percent drop in consumption expenditure. We use this consumption drop for UI benefit increases, yielding a willingness to pay of $1.17 per dollar of additional benefits.

For benefit duration increases, we further adjust this number to account for the fact that these individuals are on the margin of exhausting their UI benefits. To do so, we use the estimates in Ganong and Noel (2019), who find that consumption declines by roughly 0.8 percent of pre-unemployment expenditure for each month in unemployment. Assuming that UI benefits last for 26 weeks, this implies (again, using Hendren (2017b)'s adjustment) a total drop in consumption of 15 percent for exhaustees and thus a willingness to pay for $1 of additional UI benefits via duration extensions of $1.30.

Summary For benefit increases, the MVPFs resulting from the baseline assumptions above range from 0.43 (for Katz and Meyer (1990), CI of [0.28, 0.78]) to 1.03 (for Solon [1985], CI of [0.97, 1.09]). For duration extensions, the MVPF for Katz and Meyer (1990) is 0.45 (95% CI: [0.25, 2.12]) and the MVPF for Johnston and Mas (2018) is 0.83 (95% CI: [0.76, 0.90]). These results, along with corresponding MVPFs that include additional fiscal externalities from spillovers to DI, are shown in Appendix Table D.II.

D.V Disability Insurance

We form our sample frame of disability insurance policies using the survey articles of Chetty and Finkelstein (2013) and Liebman, 2015. These surveys note that the literature using quasi-experimental variation to identify the impact of disability insurance on labor earnings begins with the work by Bound (1989), and more recently has exploited random assignment to judges and other aspects of the determination process of program eligibility.[52] We focus our attention on this more recent literature exploiting random assignment.

[50] However, the standard error on this elasticity estimate is 0.0034; Mueller et al. (2016) conclude there is no evidence that UI extensions reduce SSDI applications.

[51] Note that these represent the change in the probability of DI receipt conditional on unemployment at a particular point in time. For our baseline specification, we make the simplifying assumption that this is equal to the change in the unconditional probability of DI receipt at any point in time; in other words, we assume that those who are induced to not apply (to apply) for DI by more generous UI in a particular unemployment spell are not simply delaying (advancing) the timing of their application. The MVPFs would be slightly smaller if part of these estimates were the result of this time shifting.

[52] Chetty and Finkelstein, 2013 also note the analysis of Gruber, 2000 , which we omit from our sample frame because of its focus on Canada. However, we note that it also finds that disability insurance reduces labor earnings and thus government revenue. Similar to our findings here for US-based policies, this would imply an MVPF below 1.

To that aim, our resulting sample frame for studies of disability insurance (DI) consists of four recent empirical papers that estimate the labor earnings response to DI receipt. Three of these papers focus on the Social Security Disability Insurance (SSDI) program: Maestas et al. (2013) exploit the random assignment of SSDI applicants to program examiners and French and Song (2014) exploit the random assignment of appeals to administrative law judges. Gelber et al. (2017) exploit a kink in the SSDI benefit formula to examine the impact of an additional dollar of benefit receipt on labor earnings. (In the case of French and Song (2014), they present separate results for applicants for SSDI and for Supplemental Security Income (SSI). we focus here on their results for SSDI applicants.) Finally, Autor et al. (2016) examine the effect of the Department of Veterans Affairs Disability Compensation (DC) program by exploiting an administrative decision to unexpectedly expand DC eligibility automatically to certain Vietnam veterans.

Willingness to Pay Let $B^0(\circ)$ denote the status quo "untreated" benefit schedule. And let $B^1(\circ)$ denote an alternative "treatment" schedule with the introduction of a disability benefit. The envelope theorem implies that to first order, willingness to pay is determined by the mechanical cost of the benefit change holding taxable income fixed. An individual who would earn y_i^0 under the untreated benefit schedule would be willing to pay $B^0\left(y_i^0\right) - B^1\left(y_i^0\right)$ to be in the alternative world with the treatment benefits.

The key to capturing the mechanical cost is to hold earnings constant at the level they would have been without the reform. Aggregating across all individuals, the per-capita willingness to pay is

$$WTP = E\left[B^0\left(y_i^0\right) - B^1\left(y_i^0\right)\right] \tag{2}$$

The potential outcomes notation used in these formulas tells us that determining the willingness to pay requires knowing the counterfactual benefit that would have been provided if the untreated group had been subject to the treatment benefit schedule but held their earnings fixed, $E\left[B^1\left(y_i^0\right)\right]$. We seek in each case to identify this counterfactual level of benefits and to use that figure to measure our mechanical costs. We treat this set-up as parallel to the tax case, because we assume individuals are willing to pay \$1 for each additional dollar of DI benefits. This assumption is natural if we consider the beneficiaries of DI policies to be those who have already become disabled. Although these individuals, prior to becoming disabled, might have been be willing to pay a premium to shift consumption to a state of the world where disability limits their earnings capacity, they would have no additional willingness to pay from such consumption-shifting ex-post. Under the assumption that individuals are willing to pay \$1 for \$1 of disability insurance benefits, one should interpret the social marginal utilities of income as measures of society's willingness to pay for transferring to those who are disabled – and part of this social desire could be consistent with desires to provide insurance against future adverse outcomes.

Costs Our cost estimates capture the fiscal externalities from DI. This includes reduced tax revenue via the program's impacts on the labor supply of adults receiving DI benefits. When estimates are available, this also includes any changes in benefit receipt due to these labor supply distortions. This means we do not include any impacts from, for example, ex-ante responses to the existence of DI, or any spillover effects on children.[53]For each cost estimate, we calculate an effective marginal tax rate on labor income based on (i) data on earnings prior to DI receipt included in each paper, and (ii) estimates of the corresponding tax and transfer rate from the Congressional Budget Office.

Four Estimates on Disability Insurance Maestas et al. (2013) exploit the random assignment of SSDI applicants to program examiners. We calculate willingness to pay for DI receipt to be 16,179. This number is calculated based on the average benefit amount per SSDI recipient is \$20,024 and the finding in the paper that SSDI receipt causes 19.2% of individuals to reduce their earnings under the income eligibility threshold. Together this provides the mechanical cost of the transfer and means the counter-factual transfer per recipient is \$16,179. On the cost side we begin with the mechanical transfer of \$16,179 and the additional transfer cost of \$3,845 based on those who reduce their earnings to receive the SSDI benefit. Next, we use the finding in the paper that marginal SSDI recipients have \$3,781 less in labor earnings two years after initially receiving DI. At an effective marginal tax and transfer rate of 19.9 percent determined by our estimates using CBO data, this corresponds to an additional fiscal

[53]Although it falls outside of our U.S.-based sample frame, there is, evidence from Norway (Dahl et al., 2014) that adult DI receipt may further increase DI costs from externalities on children. (Dahl et al., 2014) find that children in families who are randomly assigned to more lenient judges are themselves more likely to file for and receive DI benefits in adulthood. If this were also true for DI programs in the United States, the additional spillover effect on children's future DI receipt would generate an additional fiscal externality, and would correspondingly reduce the MVPF for DI expenditures.

externality of $3,781*19.9% = $752. Putting these figures together we get an MVPF of 0.78 (95% CI [0.72, 0.85]). This is calculated based on: 16,179/ (16,179 + 3,781+ 752).

French and Song (2014)(Table 6, Panel G) exploit the random assignment of appeals to administrative law judges. We begin with an initial benefit level of $13,560[54] and the same assumption from Maestas et al. (2013) that SSDI receipt causes 19.2% of individuals to reduce their earnings under the income eligibility threshold. Together this provides the mechanical cost of the transfer and means the counter-factual transfer per recipient is $10,956. On the cost side we begin with the mechanical transfer of $10,956 and the additional transfer cost of $2,604 based on those who reduce their earnings to receive the SSDI benefit. Next, we use the finding in the paper that marginal SSDI recipients have $5,787 less in labor earnings. At a marginal tax and transfer rate of 20%, we get an additional fiscal externality of $1,157 and an MVPF of 0.74 (95% CI [0.71, 0.78]).[55]

Gelber et al. (2017) exploit a kink in the SSDI benefit formula to examine the impact of an additional dollar of benefit receipt on labor earnings. They find that meanwhile, a $1 increase in SSDI benefits leads to a reduction in mean annual earnings of $0.20 four years after initial UI receipt (Table 4). At an effective marginal tax and transfer rate of approximately 20%, that yields a fiscal externality of $0.04 and thus an MVPF of 0.96 (95% CI [0.95, 0.97]).[56]

Finally, Autor et al. (2016) find that an additional dollar of disability benefits leads to a reduction in mean earnings of 26 cents (Table 8, Panel D). This results in a fiscal externality of $0.05 at a tax+transfer rate of 18.9%. The final MVPF is 0.95 (95% CI [0.92, 0.98]).[57]

D.VI Supplemental Security Income

For SSI policies, we form our sample frame using the review of Duggan et al. (2015). As these authors note, SSI provides benefits for three distinct populations: children, working-age adults, and individuals above age 65. We focus our attention on cases where research has examined the the impact of SSI on tax-related behavior. This leaves us with two estimates in our sample focused on SSI for adults: French and Song (2014) who examine SSI receipt for adults using random assignment of SSI cases to judges and Deshpande (2016) who exploits variation in the stringency of age-18 continuing eligibility reviews.[58]

As is the case with our analysis of disability insurance, willingness to pay is determined by the size of the mechanical SSI transfer. Net costs include the mechanical transfer as well as any transfer change due to earnings decline and any other fiscal externality associated with those earnings changes.

Random Assignment to Administrative Law Judges As noted above in the section on DI, French and Song (2014), exploit the random assignment of appeals to judges, presenting results separately for SSDI and SSI applicants; in this section, we thus focus on their results for adult SSI applicants. As with our SSDI estimates, we assume individuals are willing to pay $1 for each additional dollar of mechanical SSI benefits.

We use 2013 data from the Social Security Administration showing that SSI recipients received an average of $546.38 from the federal government and $129.16 from state supplements each month. French and Song (2014) (Table 6, Panel G) find that three years after assignment to an administrative law judge, marginal SSI recipients receive $3,138 less in labor earnings. Given the 50% earnings disregard associated with additional earnings while receiving SSI, the willingness to pay for the transfer is 5,442 (7,011- 3,138*0.5).

[54]Our estimate of total DI benefits for each of these two papers is constructed as follows. For both Maestas et al. (2013) and French and Song (2014), we use the monthly value of cash DI benefits of $1,130 cited in French and Song (2014), p. 295. For Maestas et al. (2013), we also include $7,200 per year in Medicare costs (cited in Gelber et al. (2017), p. 230), yielding a total benefit value of 12*$1,130 + $7,200 =$20,760. However, French and Song (2014) explicitly exclude the value of health insurance in their labor supply elasticity calculations (p. 320), and so to remain consistent we include only the value of cash benefits of 12*$1,130 = $13,560 in calculating the associated MVPF. (Cost figures given in the text above are adjusted for inflation to match the causal estimates shown.)

[55]We have assumed in this case that those who reduce their earnings to receive SSDI benefits are indifferent before and after that change. This application of the envelope theorem represents a lower bound on the willingness to pay for that transfer. If the individuals valued the that additional transfer at 50 cents on the dollar, for example, the MVPF would be 0.87 for our estimate based on Maestas et al. (2013) and it would be 0.83 for our estimate based on French and Song (2014).

[56]There is no information provided in the paper that can be used to disentangle mechanical DI costs and behavioral DI costs as the result of earnings changes. Given that SSDI benefits are not reduced until individuals cross the earnings threshold, we assume that the behavioral responses observed here are for individuals already below the earnings threshold. If we were to apply a 20% reduction in WTP as is done in the previous two examples, the MVPF would be 0.77.

[57]The disability befits studied in Autor et al. (2016) are not determined by earnings levels. We assume that there are no behavioral responses that increase benefit receipt. If those behavioral responses were responsible for 20% of the transfer cost of the program, the MVPF would be 0.76.

[58]The program operates at a national level, and there is comparatively minimal analysis on its impacts on labor supply and other tax-relevant outcomes.

The costs of the program include the full cost of the transfer and additional fiscal externalities due to the earnings response. We estimate this group has an effective marginal tax and transfer rate of 9.6%. Applying that tax rate to the $3,138 earnings response and adding the transfer cost we get a total net cost of $7,312. This produces an MVPF of 0.74 (95% CI of [0.72, 0.77]).

Variation in Stringency of Age-18 Medical Review Deshpande (2016) uses a regression discontinuity design to estimate the effects of a policy change that increased the stringency of age-18 medical eligibility reviews for SSI. This in turn increased the likelihood that low-income youth with disabilities would be dropped from the program upon turning 18.[59] Deshpande (2016) uses these estimates to construct an MVPF of the policy, which ranges from 0.9 to 1.03. These estimates are quite similar to (although slightly larger than) our estimates below. As discussed below, we conduct a slightly different calculation than Deshpande (2016) that harmonizes the approach with the estimates for disability insurance above.[60]

Deshpande (2016, Table 4) finds that removal from SSI at age 18 leads to a drop in SSI income of $7,886, a drop in SSDI income of $551, and an increase in earnings of $3,001 per year in adulthood.[61] We assume that the willingness to pay for the SSI transfer is 6,386. This is given by the total value of the transfer ($7,786) net of the portion of the transfer provided due to the earnings decline ($1500.5 = $3,001 * 0.5).[62] As with our SSDI estimates, we assume in both cases that individuals are willing to pay $1 for each additional dollar of SSI. We note, however that Deshpande (2016) finds that removal from SSI leads to an increase in labor earnings volatility. Consequently, individuals may be willing to pay a premium to smooth consumption via SSI. Using a coefficient of relative risk aversion of 2, her results imply that this premium is roughly 15 cents for every dollar of SSI. While we leave out these consumption-smoothing benefits of SSI in our baseline specifications in order to compare them directly to our estimates for SSDI, .

The cost of the program is given by the total cost of the SSI transfer $7,786 plus the change in SSDI income $551, plus the additional fiscal externality due to the earnings change. We calculate the effective marginal tax and transfer rate is just 0.3% for this population. Adding these effects we get a total cost of $8,446 and an MVPF of 0.76. (95% CI of [0.58, 0.99]). If we were to further incorporate the ex-ante consumption-smoothing benefits of SSI, this would imply an MVPF of 0.87 (0.76 multiplied by 1.15).

E In-Kind Benefits and Subsidies

E.I Sample Frames

We focus our analysis of in-kind benefits and subsidies on housing and food policies. We begin by discussing our sample frames, then turn to the estimates.

Housing We build our sample of housing policies using the survey of Olsen and Zabel (2015) and Collinson et al. (2015).

Broadly, there are three sets of housing policies one could consider: public housing in which the government directly owns and operates housing, tenant-based vouchers that provide subsidies to families in private rental markets, and supply-side subsidized development such as low-income housing tax credits (LIHTC). However, the experimental and quasi-experimental empirical work that looks at the impact of these policies on taxable behavior has primarily focused on the provision of housing vouchers, as opposed to LIHTC and public housing. The one exception is work using sibling-design methods to estimate the impact of public housing on children. Since these methods are not well-suited to look at the impact on taxable behavior of the adult beneficiaries, we do not include these public housing policies.[63] We therefore focus our housing policy sample on those focused on housing vouchers

[59] To our knowledge, this is the only paper to date to study the impact of SSI on those enrolled as children.

[60] The most notable distinction is that we account for the 50% marginal tax on earnings, which suggests part of the average benefits that individuals receive as a result of their earnings declines are not valued at their full costs.

[61] We perform a static MVPF calculation using Deshpande's annual-average results since the relative magnitudes of the SSI and earnings effects are essentially constant sufficiently far into adulthood.

[62] As noted, this is the primary distinction of our MVPF calculation relative to that of Deshpande (2016).

[63] Collinson et al. (2015) write: "Little is currently known about the effects of the LIHTC on labor supply". On public housing, the classic work of Currie and Yelowitz (2000) explore the impact of public housing on grade retention, but not labor supply. Indeed, Olsen and Zabel (2015) write:

> We have no high-quality evidence on the cost-effectiveness and most other outcomes of the largest new low-income housing programs, and we have no recent evidence on the performance of older programs that still account for a substantial minority of assisted households. For example, no attempt has been made in several decades to determine how much better or worse housing public housing tenants occupy than they would have occupied in the absence of housing assistance. Almost all

and on services, like jobs plus, provided to those in public housing.

To begin the sample construction, Olsen and Zabel (2015) note that there were three major social experiments in housing: the Welfare to Work Voucher Evaluation, the Moving to Opportunity Experiment, and the Experimental Housing Allowance Program. The Welfare to Work program referenced in Olsen and Zabel began in 1999 and provided families on TANF (cash welfare) access to housing vouchers with the aim of improving their employment prospects. Here, we separately evaluate this program from the analyses of other welfare to work programs in the cash transfer section due to its focus on the provision of housing vouchers.[64] The Moving to Opportunity experiment provided housing vouchers to families living in high poverty public housing projects in the mid 1990s. The Experimental Housing Allowance Program conducted a series of experiments in the 1970s at the metropolitan area level to understand the impact of large scale tenant-based assistance relative to more traditional public housing. Here, we provide estimates for both the Welfare to Work and Moving to Opportunity experiment; Unfortunately, in contrast to the other two experiments, our review of the reports from the Experimental Housing Allowance Program did not render sufficient information on the impact of the policies on labor supply or taxable behavior. For this reason, we omit this program from our analysis.

The MTO and WtW experiments focus on providing housing vouchers to families in public housing. In addition, we also include results from the Jobs Plus program, which was a place-based initiative to provide employment services for families in public housing projects. While the RCTs mentioned above focus on modifications to the public housing program or the provision of vouchers to those in public housing, we also include quasi-experimental analysis of Jacob and Ludwig (2012) who exploit the lotteried allocation of housing vouchers in Chicago, along with Jacob et al. (2014) who study the impacts of those voucher receipts on children's test scores and graduation rates.

Food For food policies, we rely upon the survey of Hoynes and Schanzenbach (2018) to form our sample frame. We limit our attention to policies for which there exist estimates of the impact on taxable behavior, including taxable earnings of beneficiaries.

We begin with food stamps, or SNAP. Hoynes and Schanzenbach (2018) note that there are two analyses in the literature looking at the impact of SNAP on labor supply behavior. First, Hoynes and Schanzenbach (2012), Almond et al. (2011) and Hoynes et al. (2016) use the county-level rollout of the introduction of food stamps in the 1960s-70s. In a series of papers, they estimate the impact on adult earnings, child health, and child earnings in adulthood. We use the suite of these estimates to form the implied MVPF below.

In addition to the county-level rollout of the introduction of food stamps, the other quasi-experimental design used to study the impact of food stamps is recent work by East (2018a) and East (2018b). This work exploits the cross-state variation in food stamp eligibility for immigrants during the post welfare reform period (1996-2003). East (2018b) documents reductions in labor supply behavior of eligible adults, consistent with patterns found in the introduction of food stamps. East (2018a) studies the impact on measures of self-reported child health. We are not able to directly incorporate these latter impacts, but discuss their potential implications below.

As noted in Hoynes and Schanzenbach (2018), most work studying the impact of food stamps has focused on those with children; yet many beneficiaries of food stamps are over age 65. However, take-up amongst the elderly is low. To that aim, Finkelstein and Notowidigdo (2019) conduct an RCT to provide (a) information and (b) both information and enrollment assistance for likely-eligibles. They find positive effects on enrollment, with larger impacts from the relatively more intensive information and assistance intervention. They use their results to construct MVPFs under different assumptions about the rational cost of take-up. We directly import these estimates into the analysis below.

In addition to food stamps, the Women, Infants, and Children (WIC) program provides in-kind food transfers to pregnant women and families with children under age 5. There is a large body of work looking at the impact of these policies on maternal and child health, including outcomes such as birthweight. But to the best of our knowledge, there is no work estimating the impact of WIC on labor supply behavior, nor is there work analyzing the long-run impacts on child earnings in adulthood.

For these reasons, we do not include a WIC policy in our baseline domain averages for food policies. Nonetheless, we can use the estimates from existing literature to help think about the potential impacts of the the spillover onto children for the MVPF. To that aim, we conduct this illustration using estimates of the impact of WIC on

evidence is based on data from the 1960s and 1970s, and the aging of the public housing stock raises serious doubts about the applicability of these results to the current situation.

[64] As noted below, we label this program differently from other WtW estimates – we denote its label "Housing Vouchers AFDC", reflecting the fact that it provided vouchers to families on cash welfare (AFDC). Other welfare to work programs are considered in the cash transfers appendix.

birthweight from Hoynes et al. (2011), which we then translate into lifetime earnings of the children using the cross-sectional relationship between birthweight and earnings from Black et al. (2007). Although these estimates do not enter our baseline analysis, the results provide a stylized method for thinking about this recent growing literature through the lens of the MVPF.

Lastly, in addition to WIC, there are also the school lunch and school breakfast programs that provides free and reduced price lunch to students. Unfortunately, to the best of our knowledge there is not any work looking at the causal effect of these programs on adult or child earnings.[65] Since the spending on these programs exceeds that of WIC, it does suggest a high value to future work looking at these potential impacts and constructing their implied MVPFs.

E.II MVPF Details

E.II.1 Housing Vouchers

MTO We construct estimates of the impact of MTO by focusing our attention on the impact of the experimental vouchers relative to public housing. As noted in the final impacts evaluation, the cost of a voucher relative to public housing is roughly equivalent, so that the main additional cost is the cost of counseling and move services for families in the experimental group. We sum the impacts on parents, young, and old children. We use an estimate of 2.5 children per household (Goering et al. (1999) pg. 32), 33% between the ages of 13-18, and 67% between the ages of 0-12 (Chetty et al. (2016), Table 1); the HUD evaluations focused on one adult per household to measure impacts on earnings. We therefore assume one adult per household.

Chetty et al. (2016) find that MTO increases the earnings of children by 31% in their mid-20s for those children who recieved the experimental voucher between ages 0 and 12 (These children were average 8.2 years old at the time their parents obtained the voucher). Chetty et al. (2016) also find that MTO lowers earnings by 17% for those children who recieved the experimental voucher between ages 13 and 18 (Those children were on average 15.1 years old at the time their parents obtained the voucher). Extrapolating the observed earnings effects over the lifecycle and applying a 3% discount rate implies an increase in earnings of \$89,733 for the 0-12 year old children and a decrease in earnings of \$77,028 for the 13-18 year old children. Details on this projection method can be found in Appendix I. We then estimate the fiscal externality during the observed earnings period using the tax rate calculated in Chetty et al. (2016). We estimate the fiscal externality during the period of projected earnings using estimates from the Congressional Budget Office. Details of this approach can be found in Appendix G. When analyzing the combined sample of all treated children, we find that after-tax earnings rise by \$27,866 and that tax revenue rises by \$7,248 per child. We then combine these estimates with the estimated average of 2.5 children per family. Finally we also consider the public cost of changes in college enrollment for these children, which we estimate to be -\$342 in total due to the fall in college attendance amongst the older children.

For adults, Sanbonmatsu et al. (2011) finds a yearly increase in earnings over the 10 years after the program of -672.54 (s.e. 716.54)[66], to which we apply a tax rate of 30.7%.[67] They also find impacts of 120.29 (s.e. 245.44) and 664.54 (s.e. 335.54) on yearly TANF and SNAP receipt respectively.[68] Combining with the upfront cost of the counseling of \$3,783 and the fiscal externality from children gives a net cost to the government of -\$9,215.

To measure WTP, we combine the willingess to pay of adults and children. For adults, we conservatively assume that they place no value on the counseling benefits and set their WTP equal to 0.[69] For children, we determine willingness to pay based on changes in after-tax earnings. Earnings changes imply a WTP of \$33,846. Adjusting for a \$26 decrease in contributions to college expenditures and summing across 2.5 children per household implies a net WTP of \$69,601. This corresponds to an infinite MVPF (95% CI of $[-2.8, \infty]$). Assuming instead that the WTP is solely given by the program cost of \$3,783 implies an infinite MVPF with a 95% CI of $[0.15, \infty]$.

Chicago Housing Vouchers Jacob and Ludwig (2012) estimate the impact of receiving a housing voucher from the Chicago Public Housing Authority lotteries; they study the impact on earnings and transfer receipt of

[65] Bernstein et al. (2004) do evaluate an RCT providing universal free breakfast (beyond those who are just low income eligible); they find statistically insignificant effects on test scores.

[66] Estimates would be similar and more precise with just the first 2 year impact of -786.86 (s.e. 462.10).

[67] Congressional Budget Office (2016) data imply a tax rate of 0.7% for individuals in this income bracket, to which we add 30% as participants were housing voucher recipients. Here we use a tax only rate, rather than a tax and transfer rate as we do in many other scenarios, as we directly observe impacts on transfers via TANF and SNAP.

[68] Adjusted monthly income for housing vouchers is based on earned income net of TANF. As a result, we adjust the TANF amount to account for the 30% reduction in the fiscal externality associated with these benefits.

[69] Incorporating a negative WTP based on the reduction in earnings would reduce the aggregate WTP by \$5K, but leave the overall MVPF unchanged at infinity.

households. Using the same quasi-experimental variation, Jacob et al. (2014) study the impact on children's test scores.

To begin, the average mechanical cost of the housing voucher is $8,383 per year (Jacob and Ludwig (2012, Table 1)). Jacob and Ludwig (2012) document that receiving a housing voucher leads to a quarterly $328.95 reduction in earnings (s.e. 74.56). We estimate that the earnings are taxed at a federal rate of 12.9% (inclusive of EITC), to which we add 30% to correspond to the marginal tax rate on earnings imposed by the housing voucher program. This suggests a yearly tax revenue impact from the earnings reduction of $564. In addition to these earnings declines, Jacob and Ludwig (2012) document increases in receipt of public assistance, including TANF, Medicaid, and Food Stamps. We multiply these changes by average costs of enrollment in these programs. Summing these changes suggests an increased cost of the voucher of $1,712. Moreover, Combining, this suggests a yearly fiscal externality of $2,277. This implies that the net cost of the voucher is $10,660.[70]

For the willingness to pay for the voucher the key question is what is the cash-equivalent value of the $8,383 housing voucher. We consider two cases. First, we follow Jacob and Ludwig (2012) when considering willingness to pay; they take an estimate from Reeder (1985) who estimates suggests individuals are willing to pay 83% of the cost of the voucher, which implies a WTP of $6,958. We use this number as our baseline estimate. This implies an MVPF of 0.653 (95% CI [0.609, 0.704]). Results are qualitatively similar if we assume a different valuation, because the MVPF is simply scaled by the WTP relative to our 83% baseline. Even for 100% valuation, the qualitative result of an MVPF below 1 remains similar.

There is no work that directly observes the long-run impacts on children or on their college attendance; as a result our baseline specification does not include any impacts on children. But, Jacob et al. (2014) document impacts on test scores, finding small statistically insignificant effects. Specifically, they find that a housing voucher leads to an increase of 0.0029SD (s.e. 0.0316) on females aged 0-6, 0.0634SD (s.e. 0.0325) on males aged 0-6, 0.0126SD (s.e. 0.0273) on males aged 6-18, and 0.03 (s.e. 0.0273) on females aged 6-18. To translate this to an earnings impact we take the assumption that a 1SD increase in test scores translates to a 10% increase in earnings from Kline and Walters (2016), who survey the literature on the impacts of test scores on earnings in their table A.IV. This implies a present discounted value impact of a voucher on children of $3,274 over the lifecycle, which corresponds to an impact of $655 on tax revenue. We scale these effects by the average number of years on a housing voucher of 11[71], which implies 1 year of childhood housing vouchers correspond to a $60 increase in tax revenue. This implies a net cost of the voucher of $10,600 instead of the $10,660 measure above. For the willingness to pay, we consider two cases: one where the after-tax impacts on children are included in WTP and one where they are not. Without including the child WTP, this implies an MVPF of 0.657 (95% CI of [0.610, 0.707]). Including child WTP implies an MVPF of 0.683 (95% CI of [0.610, 0.769]).

We also consider an alternate specification where increases in transfers such as TANF and Food Stamp benefits are incorporated in willingness to pay. If receiving the housing voucher provided information about these other benefits and consequently induced take-up, then it might be appropriate to assume individuals value these new benefits at their resource cost. (By contrast, if these changes in benefits are the result of a household re-optimization in response to the voucher, then the application of the envelope theorem above is more appropriate.) This alternate specification along with the other baseline assumptions produces and MVPF of 0.816 with a 95% CI of [0.807, 0.823].

Housing Vouchers to AFDC Recipients (Welfare to Work) We use estimates of the impact of the Welfare to Work (WtW) program from Mills et al. (2006) and Wood et al. (2008). This program provided housing vouchers to families enrolled in AFDC with the aim of reducing welfare receipt. The program focused on a housing strategy to reduce welfare participation, and so we include this policy in the set of housing programs. (If the program were not classified as a housing expenditure it would have been classified as a job training program or analyzed alongside the other welfare to work programs discussed in the appendix focused on AFDC and cash transfers.) To distinguish it from other WtW estimates we denote its label "Housing Vouchers AFDC", reflecting the fact that it provided vouchers to families on cash welfare (AFDC)

The Welfare to Work voucher provided average benefits of $550 per month, implying a net present discounted mechanical cost of providing the voucher of $25,269 over the 4 years of the program. We incorporate effects estimated for the subsequent 3.5 years after the voucher provision. Over this time frame, the RCT yields no significant impact on earnings, with a reduction of $1,218 (s.e. 1,120). Using the tax rate of 0.7% (which corresponds to the tax rate faced by families earning $5,581), this implies a discounted fall in tax revenue of $9. In addition to earnings, they

[70] Our baseline estimates consider the program to operate in a static environment, so that if the voucher were removed labor earnings and transfer receipt would return to their counterfactual levels. If the negative impacts on earnings persist, then the MVPF of a given year of housing voucher would potentially be smaller.

[71] We estimate this quantity by taking the difference between the average year of voucher receipt and the end of the sample period, 2011.

also find a significant increase in food stamp receipt of $1,918 (s.e. 579). In addition to impacts on adults, Mills et al. (2006) study the impact on child outcomes, including college attendance. They find no impact on college attendance (point estimate of 0.000 and s.e. of 0.001). Despite the null point estimate, the bootstrap iterations include these impacts on tax revenue through differences in child earnings. Summing together these components, we find a net cost of the program of $27,376.

To measure willingness to pay, we assume for our baseline specification that individuals value the housing vouchers at 83% of their cost Reeder (1985).[72] This yields a WTP of $20,973 for the parents.[73] For WTP equal to this level, it implies an MVPF of 0.761. In addition to this component of WTP, we also consider the potential WTP of the children of voucher holders whose earnings change; here we assume individuals value these earnings changes at their after-tax value. This does not change the MVPF point estimate because the point estimate of the impact on college attendance is zero, but it does increase the standard error. We find an MVPF of 0.761 with a 95% CI of [0.708, 0.812]. This last estimate forms our baseline specification reported in the main text.

We also explore an alternate specification where increases in TANF and Food Stamp benefits are incorporated in willingness to pay. If receiving the housing voucher provided information about these other benefits and consequently induced take-up, then it might be appropriate to assume individuals value these new benefits at their resource cost. (By contrast, if these changes in benefits are the result of a household re-optimization in response to the voucher, then the application of the envelope theorem above is more appropriate.) This alternate specification produces and MVPF of 0.781 with a 95% CI of [0.739, 0.821].

Jobs Plus We use estimates of the impact of the jobs plus program from Bloom et al. (2005). This program provided a suite of services in public housing locations in 6 cities: Baltimore, Chattanooga, Dayton, Los Angeles, St. Paul, and Seattle. The services included job training, child care, and other employment services. The sites chosen for receiving these services were randomized at the level of the housing project within housing authority jurisdiction. The follow-up compares the average outcomes of residents in each site to the outcomes of residents of other sites within the same jurisdiction that were not selected. As noted in Bloom et al. (2005), the sites varied in their degree of implementation of the services. As a result, their follow-up analysis focuses on the "high impact" subsample of sites that implemented the full set of services envisioned by the policy: Dayton, Los Angeles, and St. Paul.

One concern with the analysis in Bloom et al. (2005) is that there are very few treatment and control groups; there are only 6 treatment sites that implemented the policies. While one can ask whether those 6 sites are significantly different than their control counterparts, one cannot adjust one's standard errors for shocks at the site level, which might be a reasonable concern if there is spatial sorting across neighborhoods within cities, for example. To that aim, we caution that the identification assumption in this analysis is potentially a bit concerning. Nonetheless, given the prominence of the analysis and difficulty analyzing place-based approaches, we illustrate the implications of these results for the policy's MVPF.

We present the MVPF for the full sample, the high impact sample, and for each of the locations separately. The mechanical cost of providing the services was estimated to be $2,500 over the four years of the program, $2,393 when discounted at a 3% rate (Bloom et al., 2005). We consider the MVPF under several scenarios about the length of the policy impacts. First, we consider only using 1 year impacts. For the full sample, this involves an increase in after-tax earnings of $425 and an increase in tax revenue of $73, where we form the latter by translating the impact on earnings into taxes and transfers using a CBO rate of 14.6%. Second, we use the observed 7 year estimates, which implies an impact on after-tax earnings of $2,729 and an impact on government revenue of $467. Third, we imagine the year impacts are continued for individuals lifetimes and assume that the 7th year impacts extend throughout the lifecycle to age 65. This implies an increase in after-tax income of $10,063 and an impact on government revenue of $1,720. In our baseline specifications we assume earnings are only impacted for the 7 years in sample.

We also consider the MVPF under two different WTP assumptions. First, we assume people value the program at its costs of $2,393 per person. This implies an MVPF for the full sample of 1.24 (95% CI of [1.08, 1.48]). Second, we assume individuals value the program at its effect on after-tax earnings. This implies an MVPF of 1.42 (95%

[72] Note that alternative scalings can be obtained by multiplying our MVPF by the ratio of one's belief about individuals WTP divided by 83%.

[73] This willingness to pay is determined by the mechanical value of the transfer. The voucher amount used in costs is the total voucher amount, inclusive of behavioral responses. We assume that increases the transfer as the result of behavioral responses are valued at 0 using the logic of the envelope theorem. The increase value of the transfer due to the behavioral response is equal to 30% times the change in income net of the change in TANF benefits. The 30% figure is used because that is the phase out rate on TANF benefits. The relevant income measure is changes in earnings net of changes in TANF benefits because housing voucher benefits are calculated based on adjusted monthly income which incorporates both earned income and welfare benefits.

CI of [0.45, 2.83]). We use the latter estimate as our baseline specification reported in the paper.

Table E.I: Jobs Plus MVPF Estimates

						MVPF (Observed Impacts)					MVPF (Forecasted Impacts)	
			Earnings Impact	After-Tax Earnings	Tax		WTP =After-Tax	Earnings Impact	After-Tax Earnings	Tax		WTP =After-Tax
	Program Cost	Earnings Impact (1 year)	(7 years)	Impact	Impact	WTP =Cost	Earnings	(30 years)	Impact	Impact	WTP =Cost	Earnings
Sample:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Full	2392.88	498	3195.76	2729.18	466.58	1.242	1.417 [0.446 ; 2.826]	11782.86	10062.56	1720.30	3.56	14.961
High Impact	2392.88	1141	7322.02	6253.00	1069.01	1.807	4.723 [0.987 ; 17.507]	26996.47	23054.98	3941.48	∞	∞
Dayton	2392.88	895	5743.39	4904.85	838.53	1.539	3.156 [0.694 ; 8.583]	21176.02	18084.32	3091.70	∞	∞
Los Angeles	2392.88	1120	7187.25	6137.92	1049.34	1.781	4.568 [0.689 ; 17.245]	26499.60	22630.66	3868.94	∞	∞
St Paul	2392.88	1492	9574.45	8176.58	1397.87	2.405	8.218 [1.185 ; 333.668]	35301.25	30147.27	5153.98	∞	∞
Baltimore	2392.88	-189	-1213.38	-1035.77	-177.15	0.931	-0.403 [-2.263 ; 2.421]	-4471.81	-3818.92	-652.88	0.79	-1.254
Chattanooga	2392.88	-224	-1440.32	-1227.58	-210.29	0.919	-0.472 [-2.084 ; 5.275]	-5299.92	-4526.13	-773.79	0.76	-1.429
Seattle	2392.88	394	2528.37	2159.23	369.14	1.182	1.067 [0.064 ; 2.52]	9322.18	7961.14	1361.04	2.32	7.715

E.II.2 Food Stamps

Introduction of Food Stamps We estimate the MVPF for the introduction of food stamps using estimates of its impact on labor supply of adult beneficiaries from Hoynes and Schanzenbach (2012), its impact on infant mortality from Almond et al. (2011), and its impact on adult earnings of children whose parents received food stamps from Bailey et al. (2019).

We begin with the impact on parents. We use the "high-impact" sample of non-elderly household heads with 12 or fewer years of education from Hoynes and Schanzenbach (2012) to estimate the fiscal externality from food stamps. 6% of this sample obtains food stamp benefits, which we assume to equal the average monthly benefits of $73.57 per household member (in 2005 USD). Multiplying by 12 months and scaling by the average household size of 3.29 implies a total benefit of $2,904. Hoynes and Schanzenbach (2012) then find that food stamp introduction leads to a decline in labor earnings of $219 (s.e. $966), which, scaling by the participation rate of 6%, implies that food stamp enrollment leads to a $3,650 decline in annual labor earnings. Using a tax and transfer rate of 12.9%, this implies a fiscal externality of $471. This implies a fiscal externality of $0.16 for every $1 of food stamp benefits.

Next, we incorporate the impacts on children from Bailey et al. (2019). Appendix Table 2 of Bailey et al. (2019) estimates the percentage impact on earnings using a sample of 25-54 year olds that corresponds to an ITT of 0.0114 (s.e. 0.0034). To translate this into the impact of food stamps, we divide by the first stage of 0.16 from Figure 2 which is the impact of the rollout on food stamp receipt. This suggests a 7.1% increase in earnings from childhood food stamp eligibility for 6 full years (ages 0-5), or 1.2% per year of childhood exposure for ages 0-5.

The paper does not report the mean incomes of children whose parents obtained benefits. We therefore set the level of child earnings using the level of parental earnings of $23,473 from the high impact sample in Hoynes et al. (2016). We estimate parent income rank within parent cohort using the profile of lifetime earnings in the 2015 ACS, a 0.5% wage growth assumption, and estimates of the distribution of parental earnings from Chetty et al. (2018). We then use Chetty et al. (2018)'s estimates of income mobility to estimate a child income rank, which we then convert back into an estimated dollar amount. The full details of this approach are outlined in Appendix I. The estimates in Bailey et al. (2019)'s Appendix Table 2 for child earnings correspond to the impact of exposure for age 0-5. For other ages, we follow the pattern in the paper and assume no impacts, as they also do not report such estimates. Combining, this suggests a fiscal externality of $0.24 for every $1 of SNAP spending, which we multiply by 35%, the proportion of SNAP benefits received by households with pre-school age children, obtained from the USDA, and we scale upwards by the average number of preschoolers in households with a preschooler of 1.32. This suggests that $1 of food stamp spending leads to an increase in government revenue from child responses of $0.11.

To construct the WTP, we sum the willingness to pay for both parents and children. For parents, the envelope theorem suggests that individuals are willing to pay for the mechanical cost of SNAP benefits, which we estimate to be $1,839. We arrive at this number by taking the $3650 increase in earnings and noting that SNAP benefits decline with earnings at a 30% phase out rate. This means that $1095 of the food stamp cost is the result of a cost increase from behavioral responses. Consequently, our point estimate suggests individuals value $0.62 for each $1 spent by the government on food stamps.[74] If we did not include any willingness to pay from child benefits, then our MVPF would be 0.59. For children, we incorporate the willingness to pay from their increase in after tax earnings and reductions in mortality. The after tax earnings impact leads to an $0.45 increase in WTP for every $1 of food stamps. Next, we incorporate reductions in childhood mortality using a $1M VSL and increases in lifetime longevity with a $20K QALY (in 2012 USD). Combined, that leads to an $.02 increase in WTP per $1 of food stamp spending. (That figure rises to $0.21 if we assume a $9M VSL and $180k QALY). Combining, this suggests an MVPF of 1.04 [-0.97, ∞].

We also examine an alternate specification where we include the impact of the policy on crime. In particular, we take the reduction in incarceration from Bailey et al. (2019), and the costs of incarceration from Heckman et al. (2010). If we assume that those observed as incarcerated are incarcerated for 20 years, we get an MVPF of 1.07. (Incarceration is only observed in small window from 2006-2013 so the long incarceration duration is implemented to account for limitations of that narrow time window.)

While our baseline analysis considers the food stamp policy that is not restricted to families with children aged 0-5, we can use the estimates to think about how the MVPF would be different if it did not also spend resources across the child age distribution. We assume the parental labor supply responses would be similar so that $1 of food stamps leads to an additional cost of $0.16. But, now we no longer down-weight the fiscal externality from child earnings by the fraction of food stamp benefits going to families with aged 0-5 children of 35%. This implies

[74] It is also worth noting that this willingness to pay is nearly identical to the value we would receive if we did not apply the envelope theorem in this context, but rather used estimates from Whitmore (2002) suggesting food stamps have a trade value of at least 65%. For our "conservative" willingness to pay specification, we make both the envelope theorem and trade value modifications and find that the MVPF falls to 0.39.

an MVPF of 2.28 under our other baseline assumptions.

We also consider a conservative specification in which SNAP recipients value food stamps at \$0.65 per \$1 of food stamps received, roughly the trade value estimated in Whitmore (2002). This leads to an MVPF of 0.39 (95% CI of [-0.68, ∞]).

SNAP to Immigrants East (2018b) studies the impact of immigrant SNAP eligibility on their labor supply. We focus our attention on the impact of food stamps on single women with 12 or less years of education. The paper also provides estimates on other sub-samples of the population, but the eligibility rates are smaller and estimates less precise. Indeed, the first stage estimates find no statistically significant impact of the food stamp eligibility expansion on these two subsamples. As a result, we proceed solely with the single women sample estimates. This provides a valid MVPF for the entire eligibility of immigrants under the assumption that the size of the behavioral response per dollar of benefits is similar for other subsamples.

On the sample of single women, East (2018b) documents hours reductions and changes in extensive margin labor supply responses. East (2018b) does not provide estimates on the impact on earnings or estimates of the earnings levels or tax rates faced by the immigrant subsample. As a result, a natural path forward to compute an MVPF for SNAP is to infer what fraction of the total increase in cost from SNAP eligibility is due to the behavioral response. This is analogous to the procedure we follow for the MVPF of EITC policies.

To that aim, East (2018b) compares the size of the labor supply response to the change in benefits received. They note that much of the increase in cost is actually accounted for by the behavioral response to the program. The point estimate for the impact on total SNAP benefits is \$275.30 (Table 4, column 6). This number includes both the mechanical cost increase and the increase from behavioral responses. The program leads to a reduction in employment by 4.8 percentage points (Table 5, column 9). The average benefits received for single women is \$3,317. If we assume, as is perhaps natural, that everyone who stops working does so in order to get the food stamp benefits of \$3,317, this suggests the behavioral response leads to a cost increase of \$159.22. This means that the mechanical willingness to pay for the policy is roughly \$116.08 for the single women subsample. This calculation assumes that those who work are not able to receive food stamps and that those receiving food stamps receive the average amount of benefits.

The total cost of food stamps includes not only the increased spending on SNAP, but also any externalities from reductions in tax revenue from the changes in labor supply. These impacts are potentially operating in different directions - on one hand, individuals might lose EITC benefits; on the other hand, they might obtain other transfers from the government when out of the labor force. Here, we are unable to incorporate these estimates into the MVPF but note they could be important for future work. Assuming these effects are small, the cost of the SNAP eligibility is \$275.30. This implies an MVPF of 0.42

$$MVPF = \frac{275.30 - 159.22}{275.30} \approx 0.42$$

with a 95% confidence interval of [-3.45,1.00].

We also consider a conservative specification in which SNAP recipients value food stamps at \$0.65 per \$1 of food stamps received, roughly the trade value estimated in Whitmore (2002). This leads to an MVPF of 0.004 (95% CI of [-0.03, 0.01]).

The calculations above doe not include any potential impacts on children. In addition to the impact on labor supply, East (2018b) uses the same variation to look at the impact on child health, finding that food stamps lead to increases in the fraction of children who report being in good health. However, the paper does not provide an estimate of the impact on behavior that could be used to infer the reduction in Medicaid or uncompensated care costs, nor is it yet possible to look at later life outcomes for these children. However, we note here that of course any potential long-run impacts of food stamps on child health and/or adult earnings could change the MVPF.

SNAP eligibility experiments to elderly Finkelstein and Notowidigdo (2019) conduct an RCT with individuals over age 65 to increase take-up. They conduct two treatment arms that provide (a) information and (b) information and enrollment assistance for likely-eligibles. They find positive effects on enrollment, with larger impacts from the relatively more intensive information and assistance intervention. Finkelstein and Notowidigdo (2019) provide an estimate of the MVPF under a couple of assumptions about the size of the rational limitations to take-up. Since the authors provide their own MVPF calculation, we refer readers to their paper (Section 6.3) for a more detailed discussion of their approach and results.[75] The authors estimate an MVPF of 0.89 for the

[75] In order to incorporate sampling uncertainty in the MVPF, we recreate their estimates using the numbers provided in their paper and bootstrap using the underlying parameters that are reported with sampling uncertainty. As a result of this process our estimates differ slightly from those reported in their paper.

information-only treatment and 0.92 for the information and assistance treatment.

Here, we outline the intuition of their MVPF construction and the key underlying assumptions. Most importantly, a welfare analysis of these policies requires taking a stand on why take-up was low. If individuals knew about the program and faced a hassle cost to take-up, then the marginal person who enrolls would not value the program. In contrast, if those who enroll did not previously know about their eligibility or were not making rational choices in choosing not to enroll, then the benefits they receive from the program can provide a first-order welfare gain.

Nonetheless, the fact that the program itself is aiming to increase take-up of a near-cash equivalent policy suggests that the MVPF of the policy is inherently bounded above by 1.[76] The key question is what fraction of the increased mechanical transfer is lost due to (a) behavioral responses and (b) costs of providing the take-up services.

Finkelstein and Notowidigdo (2019) provide an estimate of the MVPF under a couple of assumptions about the size of the rational limitations to take-up. Their baseline estimates of 0.89 for the information-only treatment and 0.92 for the information and assistance treatment are near the upper regions of the specifications they report.

Finally, it is worth noting that the marginal beneficiary of these policies is different from the average SNAP beneficiary. This incidence is an important component of the welfare analysis, and speaks to the possible value of of the social welfare weights of the beneficiaries. In their experiments, they find evidence that the marginal enrollee suffers inattention (e.g. there are impacts from postcard reminders) and that the marginal enrollees are eligible for less benefits than the average recipient.

Women, Infants, and Children As noted above, there is no work to our knowledge estimating the impact of WIC on earnings. However, we do have estimates of its impact on birth outcomes. To provide some guidance on this potential MVPF, we provide a translation the estimates of the impact of WIC on birthweight from Hoynes et al. (2011), who use the county-level rollout of WIC to estimate the impact on birthweight (because we do not observe earnings or college attendance impacts we do not include this MVPF in our baseline sample). We focus on the impact of WIC on the high impact sample of those with less than a high school education. This sample has an average birthweight of 3,205 grams and a first stage of the rollout on WIC participation of 30%. This implies WIC enrollment increases birthweight by 23.3 grams, or roughly 0.73%. We then multiply these effects by the elasticity of adult earnings with respect to birthweight of 0.12 from Black et al. (2007). This implies an earnings gain from WIC eligibility of 0.0876%.

We translate this into a lifetime impact on earnings and tax revenue using our usual projection methods that assume earnings follow the cross-sectional age profile over the lifecycle. This yields a lifetime (discounted) earnings impact of $80.56, to which we apply a 20% tax rate.

The average annual WIC expenditures from 1974-1979 was $230.51. Assuming individuals value these transfers at their face value cost, this suggests an MVPF of 1.38 (95% CI [1.10, 1.66]).

F Taxes and Transfers

We consider four sets of cash transfers: the Earned Income Tax Credit, the top marginal income tax rate, cash transfers such as EITC and Negative Income Tax experiment (NIT). Table I provides a list of all policies we consider. Here, we begin by outlining the sample frames for each group. We then outline the construction of the MVPF for each of the policies in these categories.

F.I Sample Frames

Top Tax Rates We form our sample of studies of changes to the top marginal income tax rate using the review of Saez et al. (2012), supplemented with more recent work. We exclude papers solely making time series comparisons and papers that do not report standard errors of the estimates.

This leads us to 21 estimates of the MVPF of top tax rate changes using estimates of the impact of taxes on taxable income from 16 different empirical studies. These provide MVPF estimates for five different federal policies changing the top marginal income tax rate: (1) the 1981 lowering of the top marginal income tax rate (ERTA81); (2) the 1986 lowering of the top marginal income tax rate (TRA86); (3) The 1993 increase in the top marginal income tax rate (OBRA93); (4) 2001 and 2003 decreases in the top marginal income tax rates (EGTRRA01

[76] One potential un-measured channel that could overturn this statement is if if improves health that subsequently reduces government Medicare and Medicaid costs. As Finkelstein and Notowidigdo (2019) note in footnote 30, Berkowitz et al. (2017) report an association between SNAP participation and lower Medicaid and Medicare costs for low-income adults. Future work could follow the SNAP recipients from this experiment and estimate their future medical costs.

and JGTRRA03); and (5) 2013 increase in top marginal income tax rate resulting from the ACA surcharge and expiration of EGTRRA01 cuts.

In many cases, the ETI is identified from changes in state tax policies in addition to variation at the federal level; however, for simplicity we focus solely on the application of these estimates to these federal policy changes.

EITC We form our sample of studies of the EITC using the review of Hotz and Scholz (2003) as a starting point. We exclude cross-sectional comparisons or structural papers that rotate the empirical variation to fit a structural model. We supplement these studies with papers from the more recent surveys of Nichols and Rothstein (2015) and Hoynes and Rothstein (2016). We then incorporate results from more recent literature, such as the Paycheck Plus experiment that occurred since publication of these reviews (Miller et al., 2017).

Our baseline estimates for EITC expansions focus on the 1986 and 1993 federal EITC expansions. For 1986, we use estimates from Eissa and Liebman (1996) for singles. And for the 1993 expansion, we use estimates from Hoynes and Patel (2018) for singles. For each, we combine these estimates with the results from Eissa and Hoynes (2004) for married couples to form an aggregated MVPF of the policy expansions.

In addition to analyses focusing on these two reforms in particular, there is also a large literature using variations in the EITC both at the federal and state level over the last several decades to estimate the implied intensive and extensive margin labor supply elasticities (e.g. Meyer and Rosenbaum (2001)). We illustrate how the behavioral responses in these papers imply MVPFs for the major reforms. Additionally, other papers have used such elasticities to simulate the impacts of welfare reforms and conduct a welfare analysis of past and future reforms (e.g. Eissa et al. (2008), Eissa et al. (2006), Eissa and Hoynes (2011)). For completeness, we show how one can map these analyses into their implied MVPFs.[77]

We focus our attention on the fiscal consequences of the labor earnings impact of the EITC. We do not include impacts on marriage; but we note existing work finds generally small effects that would not significantly alter our MVPF calculations (e.g. Eissa and Hoynes (2004); Ellwood (2000)).

In our baseline specifications, we do not include indirect effects on children. To the best of our knowledge, there are no papers looking at the direct effect of EITC on children's adult earnings. However, there is a literature estimating the impact on children's outcomes such as test scores and college attendance (Dahl and Lochner (2012); Maxfield (2013); Michelmore (2013); Bastian and Michelmore (2018); Manoli and Turner (2018)). We discuss in Section IV.B of the main text and in detail in Section 1.3.4 below how we can use these intermediate outcomes to understand how potential indirect effects on children impact the MVPF estimate.

Cash Welfare and Welfare Reform We form our sample of cash welfare and welfare reform using the recent survey articles of Ziliak (2015) and Moffitt (2008). While there is a large set of structural analyses of welfare reform, our sample frame focuses on studies that use reduced form policy variation for identification. This leads to 28 policies primarily consisting of the large set of randomized control trials conducted as part of reforms to state-level welfare programs in the mid-1990s. For these, we draw our sample frame from the cost-benefit analysis in Greenberg et al. (2010) of the 28 US-based programs; for each, we discuss how one can translate the RCT estimates and their cost-benefit calculations to form their implied MVPFs; yet for reasons we discuss below, the design of these RCTs renders conclusions about the MVPF often quite difficult because of an inability to discern willingness to pay, and in particular whether it is positive or negative. In addition to the RCTs of welfare reform, we also consider the generosity of AFDC as studied in Meyer and Rosenbaum (2001).[78] Lastly, we study the impact of changes to AFDC term limits as studied in Grogger (2003).

F.II Detailed MVPF Derivation

We begin by outlining the theory of how to construct the MVPF for variations in the tax and transfer schedule. This general setup will help illustrate our construction of the MVPF across a range of settings below.

Consider a group of individuals indexed by i that have pre-tax incomes y_i. They face a tax (minus transfers)

[77]We omit several papers discussed in previous literature reviews on the EITC. These include Dickert et al. (1995) who use cross-sectional variation across states and the structural analyses of Keane and Moffitt (1998), Keane et al. (1995), and Hoffman and Seidman (1990). We exclude Hotz et al. (2001) who study the impact of the 1993 reforms in California, instead opting for the national analysis in Hoynes and Patel (2018). But, we note that their estimates in Table 3 range from an elasticity of labor force participation of 0 if we use 1994 as the post year to in excess of 1 if we use 1998 or 2000 as the post year.

[78]We also discuss how our baseline estimates could differ if one incorporated indirect impacts on children using the estimates of the impact of AFDC on birthweight from Currie and Cole (1993).

schedule $T(\circ)$ so that their after-tax incomes are $y_i - T(y_i)$.[79] We seek to measure the MVPF of a change to the $T(y)$ schedule.

Willingness to Pay Let $T^0(\circ)$ denote the status quo "untreated" tax schedule. And let $T^1(\circ)$ denote an alternative "treatment" schedule. The envelope theorem implies that to first order, willingness to pay is determined by the mechanical cost of the tax change holding taxable income fixed. An individual who would earn y_i^0 under the untreated tax schedule would be willing to pay $T^0\left(y_i^0\right) - T^1\left(y_i^0\right)$ to be in the alternative world with the treatment tax rates. If taxes are higher in the alternative, then WTP is negative; if taxes are lower then WTP is positive.

The key to capturing the mechanical cost is to hold earnings constant at the level they would have been at under the untreated tax schedule. Aggregating across all individuals, the per-capita willingness to pay is

$$WTP = E\left[T^0\left(y_i^0\right) - T^1\left(y_i^0\right)\right] \tag{3}$$

The potential outcomes notation used in these formulas tells us that determining the willingness to pay requires knowing the counterfactual tax revenue that would have been collected if the untreated group had been subject to the treatment tax schedule but held their earnings fixed, $E\left[T^1\left(y_i^0\right)\right]$. In the case where a randomized control trial has been conducted, this means the ideal method of calculating WTP is to feed the distribution of control group earnings into the treatment group tax schedule. In the case where researchers have used other identification techniques such as difference-in-differences, the ideal method requires using the identification assumptions to calculate the counterfactual income for each unit y_i^0. Although these approaches are often feasible, most papers do not report mechanical costs calculated in this way. Consequently, in each program, we discuss how to approximate this statistic using commonly available information.

Cost The cost of the policy is the observed causal effect of the policy on the government budget

$$Cost = E\left[T^0\left(y_i^0\right)\right] - E\left[T^1\left(y_i^1\right)\right] \tag{4}$$

where $E\left[T^0\left(y_i^0\right)\right]$ is net government revenue in the untreated group and $E\left[T^1\left(y_i^1\right)\right]$ is net government revenue in the treatment group. Note that this difference includes the mechanical effect of the policy on taxes/transfers (changes in $T(\circ)$ holding y fixed) plus any impact of behavioral responses to the policy (from changes in y).

Generic MVPF The MVPF is given by

$$MVPF = \frac{WTP}{Cost} = \frac{E\left[T^0\left(y_i^0\right) - T^1\left(y_i^0\right)\right]}{E\left[T^0\left(y_i^0\right)\right] - E\left[T^1\left(y_i^1\right)\right]} \tag{5}$$

Equation (5) provides the general formula for the MVPF of tax changes. As noted above, most studies do not directly report these WTP and Cost statistics directly. Therefore, in each program, we discuss how we approximate this ratio using the available information from existing research.

Derivation of MVPF for changes in the top marginal income tax rate For changes in the top marginal income tax rate, we estimate the MVPF as described in equation (5) as follows. Let τ denote the marginal tax rate on incomes y_i above a threshold, $\bar{y}$. We derive the MVPF for changes to the top marginal income following assumptions embedded in the existing empirical literature that studies the causal effects of these reforms on taxable behavior. In particular, we assume no responses from individuals not subject to the reforms (e.g. no spillovers onto lower-income individuals). This implies that the MVPF can be written as:

$$MVPF = \frac{1}{1+FE} \tag{6}$$

where the fiscal externality is given by:

$$FE = -\frac{\tau}{1-\tau}\alpha\epsilon^{ETI} \tag{7}$$

[79] For now, we consider a static setup in which decisions about y_i are independent of past or future tax schedules, and in which transfers do not cause spillover effects on children. This setup is the most standard in existing literature on taxation. But, one can readily extend the framework to relax this assumption. In particular, if one observes the causal effect of a tax change today on earnings in future periods by either the individuals themselves or on future generations, then one can incorporate this into the net costs of the policy or, in cases in which there are externalities, into the willingness to pay as well.

where

$$\alpha = \frac{E\left[y_i\right]}{E\left[y_i - \bar{y} | y_i \geq \bar{y}\right]}$$

is the Pareto parameter of the income distribution and

$$\epsilon = \frac{1-\tau}{E\left[y_i\right]} \frac{dE\left[y_i\right]}{d\left(1-\tau\right)}$$

is the elasticity of taxable income.

To see this, we follow Saez et al. (2012) to consider a small change in the top marginal income tax rate, $d\tau = \tau^1 - \tau^0$, is given by:

$$\begin{aligned} WTP &= E\left[T^0\left(y_i^0\right) - T^1\left(y_i^0\right)\right] \\ &= \Pr\left\{y_i^0 \geq \bar{y}\right\} E\left[\tau^0\left(y_i^0 - \bar{y}\right) - \tau^1\left(y_i^0 - \bar{y}\right) | y_i^0 \geq \bar{y}\right] \\ &= -d\tau \Pr\left\{y_i \geq \bar{y}\right\} E\left[y_i - \bar{y} | y_i \geq \bar{y}\right] \end{aligned}$$

where the last line follows from the assumption that the tax change is small so that $\Pr\left\{y_i \geq \bar{y}\right\} \approx \Pr\left\{y_i^0 \geq \bar{y}\right\} = \Pr\left\{y_i^1 \geq \bar{y}\right\}$.

The cost of the tax change is given by:

$$\begin{aligned} Cost &= E\left[T^0\left(y_i^0\right)\right] - E\left[T^1\left(y_i^1\right)\right] = \\ &= \Pr\left\{y_i^0 \geq \bar{y}\right\} E\left[\tau^0\left(y_i^0 - \bar{y}\right) | y_i^0 \geq \bar{y}\right] - Pr\left\{y_i^1 \geq \bar{y}\right\} E\left[\tau^1\left(y_i^1 - \bar{y}\right) | y_i^1 \geq \bar{y}\right] \\ &= \Pr\left\{y_i \geq \bar{y}\right\} \left(E\left[\tau^0\left(y_i^0 - \bar{y}\right) | y_i \geq \bar{y}\right] - E\left[\tau^1\left(y_i^1 - \bar{y}\right) | y_i \geq \bar{y}\right]\right) \\ &= \Pr\left\{y_i \geq \bar{y}\right\} \left(E\left[\tau^0\left(y_i^1 - y_i^0\right) | y_i \geq \bar{y}\right] + E\left[\left(\tau^1 - \tau^0\right)\left(y_i^1 - \bar{y}\right) | y_i \geq \bar{y}\right]\right) \\ &= \Pr\left\{y_i \geq \bar{y}\right\} \left(\tau E\left[dy | y_i \geq \bar{y}\right] + E\left[y_i - \bar{y} | y_i \geq \bar{y}\right] d\tau\right) \end{aligned}$$

where $dy_i = y_i^1 - y_i^0$ is the causal impact of the tax change on taxable income. As in the empirical applications we study, we assume that $dy_i = 0$ for those with latent earnings below $\bar{y}$. The last line of the Cost equation follows from the assumption that the tax change is small.

Combining the WTP and Cost, we have

$$\begin{aligned} MVPF &= \frac{E\left[y_i - \bar{y} | y_i \geq \bar{y}\right]}{\tau E\left[\frac{dy}{d\tau} | y_i \geq \bar{y}\right] + E\left[y_i - \bar{y} | y_i \geq \bar{y}\right]} \\ &= \frac{1}{1 + FE} \end{aligned}$$

where

$$\begin{aligned} FE &= \tau \frac{E\left[\frac{dy_i}{d\tau} | y_i \geq \bar{y}\right]}{E\left[y_i - \bar{y} | y_i \geq \bar{y}\right]} \\ &= -\frac{\tau}{1-\tau} \frac{E\left[y_i\right]}{E\left[y_i - \bar{y} | y_i \geq \bar{y}\right]} \frac{1-\tau}{E\left[y_i\right]} \frac{dE\left[y_i\right]}{d\left(1-\tau\right)} \\ &= -\frac{\tau}{1-\tau} \alpha \epsilon^{ETI} \end{aligned}$$

Derivation of the MVPF for the EITC The earned income tax credit provides additional cash income to individuals through the tax schedule who have qualified earnings. In general, the EITC provides an incentive for individuals with children who are out of the labor force to enter the labor force. There is also an incentive for those above the phase-out region of the EITC to reduce their labor earnings, as documented for married households by Eissa and Hoynes (2004).

Consider expanding the EITC by \$$B$. This has a willingness to pay given by: \$$B$,

$$E\left[T^0\left(y_i^0\right) - T^1\left(y_i^0\right)\right] = B \Pr\left\{y_i^0 \in EITC\right\}$$

Expanding the generosity of EITC by $\$B$ has a willingness to pay of B times the number of people who are currently enrolled in EITC (absent the increase). The cost to the government is the sum of the mechanical cost plus the impact of the behavioral response to the policy. Ideally, one would estimate the fiscal externality by taking the government revenue to be the outcome variable in the regression specifications.

In practice, literature often focuses on two primary effects: the extensive margin elasticity for singles, and the impact on earnings for married households. We therefore separate the costs into two components

$$\begin{aligned} E\left[T^0\left(y_i^0\right)\right]-E\left[T^1\left(y_i^1\right)\right] &= E\left[T^0\left(y_i^0\right)\right]-E\left[T^1\left(y_i^0\right)\right]+E\left[T^1\left(y_i^0\right)\right]-E\left[T^1\left(y_i^1\right)\right] \\ &= E\left[T^0\left(y_i^0\right)\right]-E\left[T^1\left(y_i^0\right)\right]+f_{Single}E\left[T^1\left(y_i^0\right)-T^1\left(y_i^1\right)|i\in Single\right]+ \\ &\quad +f_{Married}E\left[T^1\left(y_i^0\right)-T^1\left(y_i^1\right)|i\in Married\right] \quad (8) \\ &= B\Pr\left\{y_i^0\in EITC\right\}+f_{Single}FE_{Single}+f_{Married}FE_{Married} \quad (9) \end{aligned}$$

where f_g is the fraction of the population in group $g\in\{Single, Married\}$ and FE_g is the fiscal externality for group g: $E\left[T^1\left(y_i^0\right)-T^1\left(y_i^1\right)|i\in g\right]$.

For single households, we model the fiscal externality assuming only extensive margin responses, consistent with patterns generally found in the literature. But we also conduct a robustness analysis using the fiscal externality from Chetty et al. (2013) that estimates a 4% fiscal externality from intensive margin responses to the EITC. Assuming the only fiscal externality is from the extensive margin, let $T(0)$ denote the taxes-transfers paid if out of the labor market and $T(y)$ denote the taxes-transfers paid if in the labor market (inclusive of the increased EITC payments). Then,

$$\begin{aligned} FE_{Single} &= E\left[T^1\left(y_i^0\right)-T^1\left(y_i^1\right)|i\in Single\right] \\ &= \left(\Pr\left\{y_i^1>0|i\in Single\right\}-\Pr\left\{y_i^0>0|i\in Single\right\}\right)\left[T(0)-T(y)\right] \quad (10) \end{aligned}$$

Intuitively, the fiscal externality depends on how many people enter the labor market, $\Pr\left\{y_i^1>0|i\in Single\right\}-\Pr\left\{y_i^0>0|i\in Single\right\}$, multiplied by the impact of the labor market entry on tax revenue, $T(0)-T(y)$. For Married households, we use estimates of the change in earnings at different points of the income distribution from Eissa and Hoynes (2004), which facilitate a construction of the fiscal externality.[80]

F.III MVPF Details

F.III.1 Top Tax Rates

We estimate the MVPF using equation 6 above. We draw upon estimates of α from Atkinson et al. (2011). Throughout, we measure τ as the sum of the federal income tax rate and a 5% state income tax rate assumption.[81] We draw upon estimates of ϵ^{ETI} from each of the studied discussed above. In practice, the reforms are discrete changes in τ. To account for this, we compute the fiscal externality in equation 7 separately for the pre- and post-reform tax rates, and then take an average of the two FEs. Table F.I presents the results for each of the reforms, along with the calculations implied by equations 6 and 7.

1981 ERTA The Economic Recovery Tax Act of 1981 lowered the top marginal federal income tax rate from 70% to 50%, which implies a reduction from 75% to 55% inclusive of a 5% state income tax. Atkinson et al. (2011) find a value of $\alpha=2.3$ (estimated as $\alpha=\frac{1.77}{1.77-1}=2.3$) in 1981. Table F.I presents estimates of the implied MVPF of the top tax rate reduction using four different estimates of the behavioral response to this policy.[82] Saez (2003) studies the impact of variation in the marginal tax rate induced by high rates of inflation combined with the fact that rates are set in nominal units. He estimates an ETI of 0.31, which implies an infinite MVPF (95% CI of [0.945, ∞]). Because this estimate is obtained most closely to the time of the 1981 reform, it forms our primary baseline estimate. In addition to this estimate, Table F.I presents results from other calculations of the ETI. To begin, Gruber and Saez (2002) use variation in state and federal top tax rates throughout the 1980s to estimate an ETI of 0.57, which also implies an infinite MVPF (95% CI of [0.926, ∞]).[83] Kopczuk (2005) estimates an ETI of 0.36,

[80] These responses estimated in Eissa and Hoynes (2004) for married households reflect both intensive and extensive margin responses by spouses.

[81] The ideal statistic would be to use the population-weighted average estimates from the samples used in each policy experiment; absent this information, we adopt a 5% assumption similar to the estimates used in Saez et al. (2012).

[82] We also exclude the unpublished thesis of Navratil (1995), but note that the ETI of 0.8 obtained in that paper suggests an infinite MVPF.

[83] We use the Gruber and Saez (2002) measure of the ETI for taxable income, not the broad based measure of income since the former is the relevant statistic for measuring the fiscal externality.

which again implies an infinite MVPF (95% CI of [0.297, ∞]). And, Burns and Ziliak (2016) use a state-by-cohort instrumental variables strategy to estimate an ETI of 0.43, which also implies an infinite MVPF (95% CI of [0.809, ∞]).

1986 TRA The Tax Reform Act of 1986 lowered the marginal rate from 50% down to 28%, implying a reduction from 55% to 33% inclusive of state taxes. Atkinson et al. (2011) find a value of $\alpha = 2$ (estimated as $\alpha = \frac{2}{2-1} = 2$) in 1986. Table F.I presents estimates of the implied MVPF of the top tax rate reduction from six different estimates of the behavioral response. Broadly, these estimates all vary depending on the method of controlling for the counterfactual income that would have been experienced by these income groups - and in particular in their method for dealing with mean reversion in income over time. See Saez et al. (2012) for a discussion of these empirical estimation issues.

First, Auten and Carroll (1999) estimate an ETI of 0.57, which implies an MVPF of 44.27 (95% CI of [2.370, ∞]). To our knowledge, this provides the most recent estimate of the implied ETI of the 1986 reform using variation exclusive to the 1986 reform. Therefore, it forms our baseline estimate. As with the 1981 reform, Table F.I reports the implied MVPFs using other ETI estimates that either were earlier analyses of the 1986 reform or relied on at least part of the 1986 reform for its identification. To begin, Moffitt and Wilhelm (1998) estimate an ETI of 0.66, which implies an infinite MVPF (95% CI of [1.037, ∞]). Next, using Gruber and Saez (2002)'s estimate an ETI of 0.57 implies an MVPF of 36.06 (95% CI of [0.980, ∞]). Kopczuk (2005)'s estimate of 0.36 implies an MVPF of 2.58 (95% CI of [0.548, ∞]). Weber (2014) provides estimate an ETI of 1.05, which implies an infinite MVPF (95% CI of [4.789, ∞]). Lastly, the estimate of 0.43 from Burns and Ziliak (2016) implies an MVPF of 3.83 (95% CI of [0.928, ∞]).

1993 OBRA The Omnibus Budget Reconciliation Act of 1993 generated an increase in the top marginal income tax from 31% to 39.6%, implying an increase from 36% to 44.6% inclusive of state taxes. Atkinson et al. (2011) find a value of $\alpha = 1.77$ (estimated as $\alpha = \frac{2.3}{2.3-1} = 1.77$) in 1993. We then present estimates of the MVPF using five sources for the ETI. Our baseline estimate comes from Carroll (1998) who uses the 1993 reform to estimate an ETI of 0.38, which implies an MVPF of 1.85 (95% CI of [1.212, 4.028]). Second, Goolsbee (2000) estimates an ETI of 0.34 for executives. Applying to the broader sample implies an MVPF of 1.69 (95% CI of [0.948, 8.476]). Third, Hall and Liebman (2000) also use data on executive compensation to estimate an ETI of -0.03, which implies an MVPF of 0.97 (95% CI of [0.419, ∞]). Fourth, Giertz (2007) estimates an ETI of 0.20 pooling across reforms, which implies an MVPF of 1.31 (95% CI of [1.108, 1.627]). And lastly, taking Burns and Ziliak (2016) estimate of 0.43 from variation in state reforms over this time period implies an MVPF of 2.09 (95% CI of [0.943, ∞]).

2001 EGTRRA The Economic Growth and Tax Relief Act of 2001 led to a reduction in the top marginal income tax rate from 39.6% to 35%, implying a reduction from 44.6% to 40% inclusive of state taxes. Atkinson et al. (2011) find a value of $\alpha = 1.66$ (estimated as $\alpha = \frac{2.51}{2.51-1}$) in 2001.

We then present estimates of the MVPF using four sources for the ETI. First, our baseline estimate comes from the recent estimate of Heim (2009), who estimates an ETI of 0.22. This implies an MVPF of 1.37 (95% CI of [0.918, 2.862]). In addition, we calculate MVPFs using the Auten et al. (2008) estimate an ETI of 0.39, which implies an MVPF of 1.89 (95% CI of [1.416, 2.930]). Kawano et al. (2016) estimates an ETI of 0.12 using the data covering 2001 but focused on the 2013 reform below. This implies an MVPF of 1.17 (95% CI of [0.836, 2.065]). Lastly, taking Burns and Ziliak (2016) estimate using state reforms of 0.43 implies an MVPF of 2.09 (95% CI of [0.947, ∞]).

2013 EGTRRA Expiration The 2001 tax cuts expired in 2013, leading to an increase in the top marginal income tax rate from 35% to 39.6%. This implies an increase from 40% to 44.6% inclusive of state taxes. Hendren (2017a) finds a value of $\alpha = 1.5$. We then present estimates of the MVPF using two sources for the ETI. First, our baseline estimate comes from Kawano et al. (2016) who use variation from the 2012 reform to estimate an ETI of 0.12, which implies an MVPF of 1.16 (95% CI of [0.873, 1.925]). Second, Saez (2016) directly estimate a fiscal externality of 0.19, which implies an MVPF of 1.23 (no standard errors reported).

Table F.I: Top Tax Rate MVPF Estimates

	MVPF	Pareto Parameter	τ-pre	τ-post	ETI	FE_{pre}	FE_{post}	FE	Notes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1981 ERTA Tax Cuts									
Saez (2003)	∞ [0.945 ; ∞]	2.30	75%	55%	0.31	-2.14	-0.87	-1.50	
Gruber and Saez (2002)	∞ [0.926 ; ∞]	2.30	75%	55%	0.57	-3.91	-1.59	-2.75	
Kopczuk (2005)	∞ [0.297 ; ∞]	2.30	75%	55%	0.36	-2.46	-1.00	-1.73	
Burns and Ziliak (2017)	∞ [0.809 ; ∞]	2.30	75%	55%	0.43	-2.97	-1.21	-2.09	
1986 TRA Tax Cuts									
Auten and Caroll (1999)	44.27 [2.370 ; ∞]	2.00	55%	33%	0.57	-1.39	-0.56	-0.98	
Moffitt and Wilhelm (1998)	∞ [1.037 ; ∞]	2.00	55%	33%	0.66	-1.61	-0.65	-1.13	
Gruber and Saez (2002)	36.06 [0.980 ; ∞]	2.00	55%	33%	0.57	-1.39	-0.56	-0.97	Gruber and Saez (2002) incorporate variation from both 1981 and 1986 reforms
Kopczuk (2005)	2.58 [0.548 ; ∞]	2.00	55%	33%	0.36	-0.87	-0.35	-0.61	
Weber (2011)	∞ [4.79 ; ∞]	2.00	55%	33%	1.05	-2.56	-1.03	-1.79	
Burns and Ziliak (2017)	3.83 [0.928 ; ∞]	2.00	55%	33%	0.43	-1.05	-0.42	-0.74	
1993 OBRA Tax Increases									
Carroll (1998)	1.85 [1.212 ; 4.028]	1.77	36%	45%	0.38	-0.38	-0.54	-0.46	
Goolsbee (2000)	1.69 [0.948 ; 8.476]	1.77	36%	45%	0.34	-0.34	-0.48	-0.41	
Hall and Liebman (2000)	0.97 [0.419 ; ∞]	1.77	36%	45%	-0.03	0.03	0.04	0.03	
Giertz (2007)	1.31 [1.108 ; 1.627]	1.77	36%	45%	0.20	-0.20	-0.28	-0.24	
Burns and Ziliak (2017)	2.09 [0.943 ; ∞]	1.77	36%	45%	0.43	-0.43	-0.61	-0.52	
2001 EGTRRA									
Heim (2009)	1.37 [0.918 ; 2.862]	1.65	45%	40%	0.22			-0.27	
Auten, Carroll and Gee (2008)	1.89 [1.416 ; 2.930]	1.65	45%	40%	0.39			-0.52	
Kawano et al (2016)	1.17 [0.836 ; 2.065]	1.65	45%	40%	0.12			-0.15	
Burns and Ziliak (2017)	2.09 [0.947 ; ∞]	1.65	45%	40%	0.43			-0.52	
2013 ACA Surcharge + EGTRRA Expiration									
Kawano et al (2016)	1.16 [0.873 ; 1.925]	1.50	40%	45%	0.12			0.14	
Saez (2016)	1.23							0.19	Saez (2017) provides a direct estimate of the FE of 0.19

F.III.2 Paycheck Plus

Our baseline specification for PayCheck Plus has an MVPF of 0.996 with a 95% confidence interval of $[0.870, 1.190]$. This Appendix walks through the construction of this estimate, and discusses several alternative specifications.

The Paycheck Plus Program provides an expanded earned income tax credit (a.k.a. "Bonus") to low-income singles who traditionally are not eligible for significant EITC benefits. The credit is worth up to $2000 per year and is available over three years (2014-2016 fiscal tax years with bonuses paying out in 2015-2017). Miller et al. (2017) calculate the impact of the bonus on income, employment, and after-tax income.

We approximate the values in equation (5) using the statistics provided in the tables of Miller et al. (2017) for the first two years of the experiment.[84] In 2014, the average bonus paid is $1,399 amongst those who take it, and 45.9% of people take it up. However, Miller et al. (2017) find a causal effect of the program on the extensive margin labor supply of 0.9%. This suggests that 0.9% of those people would not have taken it up had the bonus not been offered in 2014. Without specific information about the distribution of earnings amongst the control group, our best estimate is that 45% (45.9% - 0.9%) would have taken up the bonus and received an average bonus of $1,399. This approach implicitly assumes that all responses to the bonus occur on the extensive margin and that those entering the labor force obtain the average bonus size. Given that set-up, the envelope theorem implies a willingness to pay for the control group of $1399*45% = $630 (if we asked the control group how much they'd be willing to pay for Paycheck plus, we'd expect the average WTP to be $630). Repeating this calculation using the data from 2015 yields $1,364 * (34.8% - 2.5%) = $441.

To calculate costs, we need to know how much more money the government paid out in terms of the bonus, other EITC payments, changes in tax revenue, and how much less they paid out in other government programs (e.g. food stamps). Miller et al. (2017) provide an estimate of after tax income for the treatment and control group, along with an estimate of the earnings in the treatment and control group. The difference between the impact on earnings relative to the impact on after-tax income is the net cost to the government of the policy.

In 2014, the impact on earnings is $33 and the impact on after tax income is $654. Hence, the impact on government costs is $621. In 2015, the cost is $453 ($645-$192). Combining across years, we see that the WTP for the first two years of the program is $441+$630 = $1,071. And, the cost to the government of the program is $453+$621=$1,074. Combining, this suggests an MVPF of 0.996 ($= 1,071/1,074$) with a confidence interval of $[0.869, 1.169]$.[85] Table F.II, column (2) presents these results for the baseline specification.

We also consider two alternative methods for calculating the net cost to the government. First, we suppose that the only cost to the government is through increased bonus payments, and thus exclude any increased EITC payments or reductions in transfers. In this case, the MVPF is 0.958 (95% CI of [0.915, 1.005]), as shown in Column (1).

In contrast, Column (3) reports an alternative specification that attempts to incorporate any potential gains to the government from reductions in transfers, such as food stamps. To do so, we take the costs in the baseline specification (i.e. $1,074) and add in the impact on earnings multiplied by a 30% tax rate to conservatively adjust for any potential impacts on transfers. This leads to an MVPF of 1.063 (95% CI [0.718, 2.064]); the wider confidence interval reflects the comparatively uncertain impact on earnings.

Implications of Dynamic Effects The formulas above adopt the common assumption in the taxation literature that the impact of taxation can be modeled in a static (or repeated static) environment. This would be violated if taxation causes an increase or decrease in future earnings even after the tax change no longer applies. For example, if Paycheck Plus lasts for 3 years but has impacts on earnings after the credit is no longer available, the formulas in equation (5) would not accurately capture the MVPF. The correct formula requires summing across all future years. To capture this, let y_{it}^{g} denote the income of individual i in time t if assigned to group $g \in \{C, T\}$. And let $T_t^g(\circ)$ denote the tax schedule in period t for group g. The MVPF is now given by[86]

$$MVPF = \frac{\sum_t E\left[T_t^C\left(y_{it}^C\right) - T_t^T\left(y_{it}^C\right)\right]}{\sum_t E\left[T_t^C\left(y_{it}^C\right)\right] - E\left[T_t^T\left(y_{it}^T\right)\right]}$$

[84]The recent release of Miller et al. (2018) provides additional information on the third year of the program, but does not provide sufficient information to approximate the MVPF including this third year, as noted below. In particular, one needs to know the fraction of individuals in the treatment group that received the bonus in 2017. This information is provided in the 2016 report for years 2015 and 2016; and thus we provide the MVPF pooling over these two years. However, our read of the patterns in the paper suggest the MVPF would be nearly identical if we were to incorporate the third year results.

[85]To obtain the confidence intervals, we bootstrap the labor force participation, earnings, and tax revenue outcomes. We assume (conservatively) these estimates are perfectly correlated across bootstrap iterations.

[86]For simplicity, we omit the notation for time discounting in the formula below, but all revenues and WTP values are discounted to the time of the initial policy change.

The WTP continues to be the "mechanical" effect of the policy on transfers (summed across all periods) and the cost is now the sum across all periods of the impact of the policy on the government budget.

As noted, Paycheck Plus provided three years of bonus payment. We can imagine a case where earnings changes persists even after the bonus no longer provided. For example, Miller et al. (2017) find that behavioral responses lead federal taxes paid in the treatment group to exceed federal taxes paid in the control group by $24 in the second year. If those behavioral responses persist in subsequent years, that represents a positive externality and government costs are lower by $24 per year. Hence, in the long run, the MVPF would be slightly higher. For example, extrapolating over 10 years would reduce the cost by $240 from $1,074 to $834 (abstracting from discounting). This would imply an MVPF of 1.29. By contrast, if Paycheck Plus induced individuals to lower their hours worked and those hours reductions persisted for years to come, their subsequent earnings reductions could lead to reduced tax revenue and higher levels of transfers. That would decrease the the MVPF. For simplicity, our baseline specification assumes no long-run effects of the program, but future work could construct such estimates once these long-run impacts are observed.

Table F.II: Paycheck Plus MVPF Estimates

	Bonus Only **(1)**	**Bonus and Taxes** **(2)**	**Bonus, Taxes and Transfers** **(3)**
2014	0.980	1.014	1.030
	[0.941 ; 1.020]	[0.920 ; 1.136]	[0.774 ; 1.585]
2015	0.928	0.973	1.114
	[0.874 ; 0.984]	[0.807 ; 1.256]	[0.654 ; 4.949]
Pooled	0.958	**0.996**	1.063
	[0.911 ; 1.005]	**[0.870 ; 1.190]**	[0.721 ; 2.284]

Notes: This table presents estimates of the MVPF of Paycheck Plus using estimates from Miller et al. (2017). Each column presents estimates using a different method for calculating the fiscal externality from the program. Column (1) presents estimates assuming that the only cost to the government is through the bonus payments. Column (2) presents the baseline specification in the paper that assumes the cost to the government is the sum of the bonus payments plus any changes in tax liability. Column (3) presents a robustness specification that also assumes individuals face a 30% tax rate on income from reductions in transfers, such as food stamps. Each row presents estimates from different years of the program. The first row presents estimates using only data from 2014; the second row presents estimates using only data from 2015; and the final row presents the estimates pooling across both years. Bootstrapped 95% Efron (1982) bias-corrected confidence intervals are presented in brackets. The pooled specification in Column (2) is represented in bold and is the baseline specification in the main text.

F.III.3 Earned Income Tax Credit

We estimate the MVPF of the EITC using the approach outlined in equation (9) above. Table F.III provides the details of these calculations and outlines the various components.

TRA86 For the 1986 EITC expansion, Eissa and Liebman (1996) estimate that labor force participation for unmarried women increases by 2.8 percentage points[87] (Eissa and Liebman (1996), Table III, Column 4). We approximate the average credit paid, $B \Pr\left\{y_i^0 \in EITC\right\}$, using estimates from Meyer and Rosenbaum (2001). They estimate that the amount of net tax payments for single mothers who work in 1988 is $1,030, in contrast to $1,521 in 1984. This implies a difference in EITC payments of $491, which corresponds to $B \Pr\left\{y_i^0 \in EITC\right\}$ (see Meyer and Rosenbaum (2001), Appendix 2).

Next, we form our baseline estimate of $T(0)$ using administrative information on the average food stamp and AFDC payments to those out of the labor force, multiplied by the take-up rate of welfare programs. This yields an estimate of

$$\begin{aligned} T(0) &= f_{Take-up}\left(AFDC(0) + FOOD(0)\right) \\ &= -68.4\%\left(339 * 12 + 45.50 * 2.68 * 12\right) \\ &= -3,784 \end{aligned}$$

where $339 * 12 = 4,068$ is the average yearly AFDC payment[88], $45.50 * 2.68$ is the average monthly food stamp expenditure for a family (e.g. single parent with an average of 1.68 children)[89] and 68.4% is the take-up rate of welfare benefits fromBlank and Ruggles (1996), Table 1. This implies $T(0) = -3,903$ in 1986 USD. Translating to 1996 USD using a CPI ratio of 1.3719 yields $T(0) = 1.3719 * (-3,784) = -5,191$ in 1996 USD.[90]

For $T(y)$, we use estimates from Meyer and Rosenbaum (2001) Appendix 2 which show that amongst those who work, the average taxes paid in 1988 are $1,030 and the average welfare benefits are $1,478, which implies $T(y) = -448$ in 1996 USD. Combining, this suggests $T(0) - T(y) = -5,191 + 448 = -4,743$. This is the fiscal externality from entering the labor force inclusive of taxes and EITC, food stamps, and AFDC. We exclude any potential lost Medicaid benefits from entering the labor market. This is because of both an absence of data and a recognition that the average cost per enrollee may be a poor measure of the marginal cost for enrollees that choose to enter the labor market, who may not be representative of the general Medicaid population that includes the disabled. However, one can get a back-of-the-envelope sense of the impact of incorporating Medicaid into $T(y) - T(0)$ by noting that the per capita expenses were $41,300/19.8=$2,086[91]; assuming a 50% take-up rate amongst those eligible, it implies a cost of $1,043 per person eligible, which suggests a fiscal externality that is roughly 20% higher.

To form an estimate of FE_{Single}, we take the estimate of the fiscal externality, $T(0) - T(y)$, and multiply by the 2.8 percentage point increase in LFP. This yields $FE_{Single} = -132.8$. We normalize these costs by the average cost of the EITC per single mother. (This is because the labor supply response is measured in terms of percentage point increases in participation rather than percent increases.) We use an estimate from Eissa and Liebman (1996) that 0.729 of single women are in the labor force to get a per person cost of $358 = $491*0.729. This implies that every $1 of mechanical EITC benefits a fiscal externality of −$132.8/$358 = −0.37 for singles.

In practice, roughly half of EITC beneficiaries are married. In particular, Liebman (2002) uses the 1991 CPS to find that the fraction of EITC beneficiaries that are married is $f_{Married} = 0.523$ (Liebman (2002), Table 7.6). For married households, we use the estimates from Eissa and Hoynes (2004) who estimate the impact of EITC expansions on earnings of married households. They use their results to document changes in earnings for households in each region of the EITC schedule. For the 8.8% of the married population in the phase-in region, they find if individuals had not changed their behavior, the gross transfers obtained would have been $1,144 when combining both the 1986 and 1993 reforms.

[87] We use the estimate from column 4 instead of their baseline specification because it includes controls for unemployment and AFDC as well as state dummies.

[88] See https://aspe.hhs.gov/report/welfare-indicators-and-risk-factors-thirteenth-report-congress/afdctanf-program-data, Table TANF 6.

[89] See https://aspe.hhs.gov/report/welfare-indicators-and-risk-factors-thirteenth-report-congress/appendix-program-data, Table SNAP 2.

[90] This estimate is similar to what would be obtained using estimates from Table I of Meyer and Rosenbaum (2001) after adjusting for incomplete welfare take-up rates. In particular, if we had not multiplied by 68.4%, we would obtain a value for $T(0)$ of $7,589, which is similar to the estimate of $T(0)$ of 7583+169=7,752 from Meyer and Rosenbaum (2001) constructed for 1984 (excludes Medicaid costs).

[91] See Table 2 of https://www.cms.gov/Research-Statistics-Data-and-Systems/Research/ActuarialStudies/Downloads/MedicaidReport2017.pdf.

In response to the marginal subsidy, individuals increase their earnings to generate a change in after-tax income of $1,289, which implies an increase in after-tax income of $145, and a reduction of tax revenue of $\frac{0.01}{1-0.01}\$145 = \$1.46$, since the net tax rate is 1%. For the 6% in the plateau, they would have received $2,424 but after behavioral responses their after-tax income increases by $2,355. Since they face a tax rate of 15%, this implies a change in tax revenue of $\frac{.15}{1-.15} * (-\$69) = -\$12.18$. For the 42.9% in the phase-out region, they would have received $1,591, but they reduce their after-tax earnings so that on net they obtain $1,455. Given a 25% tax rate, this implies a change in tax revenue of $\frac{.25}{1-.25} * (-\$136) = -\$45.33$. Lastly, for those above the phase out region, they decrease their after-tax income by $41. Given a 15% tax rate, this implies a reduction in tax revenue of $\frac{.15}{1-.15} * (-\$41) = \$7.24$. Taking a weighted average of these tax impacts yields a net fiscal externality of 0.025 for every $1 of mechanical EITC expansion.

Normalized by a $1 expenditure on the program, we get a total net cost of

$$\begin{aligned} E\left[T^0\left(y_i^0\right)\right] - E\left[T^1\left(y_i^1\right)\right] &= 1 + (0.523 * 0.025) + (1 - 0.523)(-0.37) \\ &= 0.836 \end{aligned}$$

In order to get the MVPF we then divide each one dollar of mechanical EITC benefits by that net cost to get a MVPF of

$$MVPF^{EITC86} = \frac{1}{0.836} = 1.20$$

Our MVPF of 1.20 has a 95% confidence interval of [1.049, 1.381].

OBRA93 For the 1993 EITC expansion, we use estimates of the impact of the the EITC on labor supply and earnings of single women from Hoynes and Patel (2018). They estimate that roughly 71% of those with a high school degree are eligible for EITC, 60% with those less than HS degree and 68% for those with some college or less are eligible. Therefore, they choose a sample of those with some college or less. We assume for their experiment that roughly 68% of single women are eligible for EITC.

Hoynes and Patel (2018) do not directly measure the impact of EITC on the extensive margin labor supply response; but they do focus on its impact on incomes above the poverty line. We assume for simplicity that the behavioral response of moving above the poverty line is driven by a discrete change in labor earnings for which we approximate its implied fiscal externality below. They find that a $1000 increase in actual EITC take-up leads to an increase in the fraction that are above the poverty line by 8.4% (Table 1, column 2) for those with one or more children and 5.4% for those with two or more children. These samples rely on slighlty different sources of variation and so we take the average of the two, implying an increase of 6.9% increase in the fraction above the poverty line in response to a $1000 increase in EITC. If the estimates were smaller such as those found in Meyer and Rosenbaum (2001), it would imply an MVPF closer to 1 than what we obtain in our baseline estimate.

We next assume that this increase is driven by labor force entry so that the fiscal externality from these increases in incomes above the poverty line is given by $T(y) - T(0)$. We form our baseline estimate of $T(0)$ using administrative information on the average food stamp and AFDC payments to those out of the labor force, multiplied by the take-up rate of welfare programs. This yields an estimate for 1993 of

$$\begin{aligned} T(0) &= f_{Take-up}\left(AFDC(0) + FOOD(0)\right) \\ &= -68.4\%\,(373 * 12 + 2.681 * 67.95 * 12) \\ &= -4,557 \end{aligned} \tag{11}$$

where $373 * 12$ is the average yearly AFDC payment[92], $3 * 67.95$[93] is the average monthly food stamp expenditure for a family (e.g. single parent with an average of 1.68 children)[94], and 68.4% is again approximated using the take-up rate of welfare benefits from Blank and Ruggles (1996), Table 1. This implies $T(0) = -4,557$ in 1993 USD. Translating this into 1996 USD yields $T(0) = -4,891$.

[92]See https://aspe.hhs.gov/report/welfare-indicators-and-risk-factors-thirteenth-report-congress/afdctanf-program-data, Table TANF 6

[93]This estimate was originally retrieved at https://www.fns.usda.gov/sites/default/files/SNAPsummary.xls, this number has been rounded to $68 in a subsequent online table available at https://aspe.hhs.gov/report/welfare-indicators-and-risk-factors-thirteenth-report-congress/appendix-program-data.

[94]See https://aspe.hhs.gov/report/welfare-indicators-and-risk-factors-thirteenth-report-congress/appendix-program-data, Table SNAP 2.

For $T(y)$, we note that Meyer and Rosenbaum (2001) estimate that households with children on average pay \$79 of taxes in 1996 and receive \$1,488 in welfare benefits (Appendix 2), which implies $T(y) = -1,409$. Hence, $T(0) - T(y) = -3,482$. The government saves roughly \$3,482 for every person who enters the labor market. The estimates suggest that for the 1993 reform, every \$1000 in increase in EITC benefits leads to an increase in incomes above the poverty line of 6.9%. which implies a fiscal externality of \$241 ($= .069 * \$3,482$).

We know that roughly half of the EITC beneficiaries are married and so now we incorporate the effects on those individuals into the MVPF. Using the same calculation as in our analysis of TRA86 but substituting the EITC phase-in and phase-out rates which applied in 1993, we estimate that a \$1 increase in EITC benefits for married couples leads to an increase in EITC costs of \$0.042.

In order to calculate the MVPF it is important to distinguish between mechanical program costs and total costs. Estimates from Hoynes and Patel (2018) suggest 66% of single women would be in the labor force in the absence of the EITC. That means that the 6.9pp of women induced into the labor force as a result of the policy represent 10.5% of those receiving the EITC. We assume that EITC benefits are equal for inframarginal individuals and those induced to join the labor force. Consequently, we find that for each \$1,000 in EITC benefits, \$895 represent mechanical transfer and \$105 represents additional costs due to behavioral responses.

Hence, the total cost of the \$1000 increase in EITC benefits is $895 + (1 - .523) * (-240) + .523 * 38 = 800.23$. Hence, the MVPF is given by

$$MVPF^{EITC93} = \frac{895.0}{800.23} = 1.12$$

with a 95% confidence interval of [0.27, 1.20].

Additional Specifications: Meyer and Rosenbaum (2001) We also illustrate here how one can translate the estimates from Meyer and Rosenbaum (2001) into their implied MVPF of EITC. Meyer and Rosenbaum (2001) use variation in the EITC, welfare reform, and other variables over 1984-1996 to estimate their impacts on the labor supply of single women. They estimate that a \$1000 of additional tax credits leads to a 2.73 percentage point increase in labor force participation of single women. We can use our results above to show how this behavioral response estimate would lead to a different MVPF for either the 1986 or 1993 reforms.

For the 1986 reform, we can scale this response to a \$491 increase in credits, which would imply a 1.34 percentage point increase in labor force participation, in contrast to the 2.9 percentage point increase estimated in Eissa and Liebman (1996). This would imply that $FE_{Single} = T(0) - T(y) * 1.34\% = -\$4,743 * 1.34\% = -\$64$. We can calculate total costs based on the value of the mechancial EITC transfer and the fiscal externality amongst singles and married individuals. For a total transfer of \$491, we find there is a mechanical transfer of \$358 that leads to a total cost of $358 + 0.523 * 8.9 + (1 - 0.523)(-64) = 332$, which implies an MVPF of 1.08.

Similarly, for the 1993 reform, instead of a 6.9 percentage point increase in labor force participation we can imagine the results with a 2.73 percentage point increase in labor force participation of single women. This would imply a cost of \$1000 of additional EITC resulting \$960 in mechanical EITC transfers and a total cost of $960 - (1 - .523) * (95) + 0.523 * 42 = 936$, which implies an MVPF of 1.03.

Additional Specifications: Bastian and Jones (2019) In more recent work, Bastian and Jones (2019) use linked administrative data to measure the fiscal externality of EITC expansions directly. Their results focus on the combination of state- and federal-variation in EITC to directly measure the impact of a \$1000 increase in mechanical EITC payments on the net cost to the government. We do not include these results in our main MVPF estimates because their specifications each correspond to multiple combinations of different reforms. But, given the important findings in their paper it is important to discuss their MVPFs.

Their results suggest larger fiscal externalities associated with EITC expansions – to the point where on average the EITC nearly pays for itself. They construct an MVPF implied by their estimates, which they estimate to be between \$4.20 and \$5.30, depending on the specification. While their results align closely with existing literature on the extensive margin labor supply response to the EITC, their results suggest the parameterization of the fiscal externality implied by these labor supply responses may be incorrect in practice: to generate an MVPF of around 5 requires that the marginal people induced into the labor market are those who (a) were receiving large transfers when out of the labor market and (b) paid a higher amount of taxes than the average EITC recipient in the labor market. This suggests important future work to better understand the source of these differences and the fiscal externality.

Estimates from Previous Welfare Simulations: Eissa and Hoynes (2011) Previous literature has translated behavioral elasticities estimated from EITC variation into the implied MVPF of simulated tax reforms. Most commonly, these papers refer to the exercise as constructing the marginal excess burden (Eissa et al. (2006); Eissa et al. (2008)) of the tax policy, and lament the inability to measure the compensated elasticity. By working with the MVPF instead of the MEB, we are able to utilize the causal estimates directly. We conceptually close the budget constraint comparing the MVPF to the MVPF of other policies.

Eissa and Hoynes (2011) consider a hypothetical policy of expanding the EITC in 2011. They estimate the shape of the tax schedule and assume extensive and intensive margin elasticities from existing literature to estimate a marginal welfare gain per dollar of government revenue. They focus on expansions only to single women; since most expansions are generally provided to married adults as well, we augment their analyses to also incorporate behavioral responses amongst married individuals.

Eissa and Hoynes (2011) consider two policy variations: first, they consider reducing the phase-out slope, which generates a welfare gain per dollar of spending of 1.391 for singles with children (Eissa and Hoynes (2011), Table 7b), which implies a fiscal externality of -0.281 for singles. We combine this with the fiscal externality for responses of married couples above of 0.025 for every $1 of EITC to yield an average fiscal externality of $FE = -0.121$, which implies a cost of 0.888 and an MVPF of 1.12, as shown in Table F.III.

Eissa and Hoynes (2011) also consider a policy variation of expanding EITC generosity, which they estimate to generate a welfare gain per dollar of spending of 1.086, which implies a fiscal externality of -0.0792 for singles. Combined with the married estimate of 0.025, this implies an average fiscal externality of -0.025. This implies an MVPF of 1.02, as shown in Table F.III.

Estimates from Previous Welfare Simulations: Eissa, Kleven, and Kreiner (2008) Eissa et al. (2008) also provide analyses of the welfare impact per dollar of spending on EITC. They use linked data from the Survey of Income and Program Participation (SIPP) to calculate the tax schedule and incorporate both intensive and extensive margin responses. Table 2 of their paper provides the willingness to pay of single mother beneficiaries for for historical reforms to the EITC over the past several decades. Here, we use these estimates to measure the fiscal externality of the reform for singles, FE_{Single}, and then we combine them with our estimates above of the fiscal externality for married beneficiaries, $FE_{Married}$, to form the implied MVPF from these estimates.

For the 1986 reform, Eissa et al. (2008) estimate a willingness to pay of $3.84 for every $1 spent on single mothers. To provide the implied MVPF for the policies that actually expanded EITC for both married and single adults, we include the impact of behavioral responses from married adults as well. The estimates for singles implies a fiscal externality of -0.74 for singles. Hence the average fiscal externality for marrieds and singles is -0.331, which implies an MVPF of 1.49. Analogously, this procedure implies an MVPF of 1.28 for the 1990 reforms, 1.14 for the 1993 reforms, and 1.18 for the 2001 reforms, as reported in Table F.III.

Estimates from Previous Welfare Simulations: Eissa, Kleven, and Kreiner (2006) Lastly, Eissa et al. (2006) estimate the welfare impact of the 1986 expansion for single mothers using different elasticity assumptions. This provides estimates ranging from 2.07 for the low end of their estimates (which assumes an extensive margin elasticity of 0.3 and an intensive margin elasticity of 0.1) to estimates that suggest the EITC expansion generated Laffer effects for single women when the extensive margin elasticity equals 0.7. Combining with the estimates for marrieds would cut these estimates roughly in half. In particular, it suggests the fiscal externality for the low end is 0.517, which implies an average fiscal externality on singles and marrieds of 0.231. This implies an MVPF of 1.3. At the high end of their specification, the paper does not provide sufficient estimates to calculate the exact fiscal externality but notes the effect on single mothers is sufficient to cover the cost of the program, $FE_{Single} < -1$. This implies that the MVPF would be above 2, but we are unable to generate a precise point estimate for those simulations. We therefore report the range of potential estimates as $[1.3, \infty]$ in Table F.III (Note this range is not reflecting a confidence interval, but rather variation across specifications).

Table F.III: EITC MVPF Estimates

	MVPF	WTP	Cost	Singles					Married	Notes
	(1)			B	T(0)	T(y)	d[LFP]	FE_{Single}	$FE_{Married}$	
Baseline Estimates										
TRA86	1.196 [1.049 ; 1.381]	358	299	491	-5191	-448	0.028	-132.8	8.9	d[LFP] for singles from Eissa and Liebman (1996)
OBRA93	1.118 [0.274 ; 1.200]	895	800	1000	-4891	-1409	0.069	-240.3	37.8	d[LFP] for singles from Eissa and Patel (2014)
Alternate Estimates										
Meyer and Rosenbaum (2001)										
TRA86	1.08	358	332	491	-5553	-448	0.013	-129.5	8.9	d[LFP] for singles from Meyer and Rosenbaum (2001
OBRA93	1.03	960	936	1000	-4891	-1409	0.027	-95	40.6	d[LFP] for singles from Meyer and Rosenbaum (2001
Bastian and Jones (2019)	4.20-5.30									Bastian and Jones (2019) provide direct estimate of FE from linked CPS-Administrative data
Previous Welfare Simulations										
Eissa and Hoynes (2011; Table 6b)										
2011 phase-out	1.13	1	0.888	1				-0.281	0.042	Simulated impact of reducing the phase-out tax rate
2011 generosity	1.02	1	0.984	1				-0.079	0.042	Simulated impact of expanding generosity
Eissa, Kleven, and Kreiner (2008; Table 2)										
1986 Reform	1.494	1	0.669	1				-0.740	0.042	Simulated impact of 1986 EITC expansion
1990 Reform	1.280	1	0.781	1				-0.505	0.042	Simulated impact of 1990 EITC expansion
1993 Reform	1.135	1	0.881	1				-0.296	0.042	Simulated impact of 1993 EITC expansion
2001 Reform	1.178	1	0.849	1				-0.363	0.042	Simulated impact of 2001 EITC expansion
Eissa, Kleven, and Kreiner (2006; Table 4.4)										
1986 Reform	[1.3, ∞]									Simulated impact of 1986 EITC expansion

Notes: All dollar values in 1996 USD.

Table F.IV: Incorporating Indirect Impacts on Children

	MVPF	WTP	Cost	Singles					Married	Children				
	(1)			B	T(0)	T(y)	d[LFP]	FE_{Single}	$FE_{Married}$	WTP_{Child}	FE_{Child}	$d[\log(y_{Child})]$	d[Test]	d[College]
Baseline Estimates														
OBRA93	1.118	895	800	1000	-4891	-1409	0.069	-240.3	37.8					
	[1.059 ; 1.164]													
With Child Impacts added to OBRA93 Estimate														
Earnings Observed														
Bastian and Michelmore (2018)														
WTP = $1	∞	895	-2315	1000	-4891	-1409	0.069	-240.3	37.8	0	-3115	0.036		
	[0.071 ; ∞]													
WTP = $1 + y_{Child}	∞	14528	-2315	1000	-4891	-1409	0.069	-240.3	37.8	13633	-3115	0.036		
	[-4.042 ; ∞]													
College Attendance Observed														
Bastian and Michelmore (2018)														
WTP = $1 (College Forecast)	1.729	895	518	1000	-4891	-1409	0.069	-240.3	37.8	0	-283			0.005
	[0.632 ; ∞]													
WTP = $1 + y_{Child} (College Forecast)	3.966	2053	518	1000	-4891	-1409	0.069	-240.3	37.8	1158	-283			0.005
	[-0.961 ; ∞]													
Michelmore (2013)														
WTP = $1	1.152	895	777.0	1000	-4891	-1409	0.069	-240.3	37.8	0	-23			0.014
	11.000													
WTP = $1 + Child After Tax Earnings	1.202	934	777.0	1000	-4891	-1409	0.069	-240.3	37.8	39	-23			0.014
	[0.681 ; 1.473]													
Manoli and Turner (2018)														
WTP = $1	1.166	895	767.7	1000	-4891	-1409	0.069	-240.3	37.8	0	-33			0.020
	[0.309 ; ∞]													
WTP = $1 + Child After Tax Earnings	1.237	949	767.7	1000	-4891	-1409	0.069	-240.3	37.8	54	-33			0.020
	[-1.220 ; ∞]													
Test Scores Observed														
Dahl and Lochner (2012)														
WTP = $1	3.483	895	257	1000	-4891	-1409	0.069	-240.3	37.8	0	-543		0.045	
	[0.859 ; ∞]													
WTP = $1 + Child After Tax Earnings	11.941	3068.2	257	1000	-4891	-1409	0.069	-240.3	37.8	2173	-543		0.045	
	[1.793 ; ∞]													
Chetty, Friedman, and Rockoff (2011)														
WTP = $1	∞	895	-157	1000	-4891	-1409	0.069	-240.3	37.8	0	-958		0.077	
	[7.928 ; ∞]													
WTP = $1 + Child After Tax Earnings	∞	4726	-157	1000	-4891	-1409	0.069	-240.3	37.8	3831	-958		0.077	
	[40.279 ; ∞]													
Maxfield (2013)														
WTP = $1	∞	895	-301	1000	-4891	-1409	0.069	-240.3	37.8	0	-1101		0.098	
	[1.556 ; ∞]													
WTP = $1 + Child After Tax Earnings	∞	5300	-301	1000	-4891	-1409	0.069	-240.3	37.8	4405	-1101		0.098	
	[3.233 ; ∞]													

Notes: All dollar values in 1996 USD.

F.III.4 Incorporating Child Impacts into EITC

Our baseline estimates do not include indirect impacts of the EITC on children. If the EITC increases children's earnings, this generates a fiscal externality on the government. To that aim, there are a range of studies that have looked at the impacts of EITC on children's outcomes, such as test scores, college attendance, and earnings. The empirical approaches utilized in these papers tend to pool EITC estimates across different sources of variation (e.g. estimates exploiting the nonlinearity of the tax schedule, state EITC expansions, and federal EITC expansions). Because they do not correspond to one of our EITC historical policy experiments, we do not include them in our baseline estimates. But, we illustrate here how one can translate these estimates into their implied MVPF using the OBRA93 expansion as the baseline specification. Table F.IV reports these results.

Throughout, we construct estimates of the MVPF of the EITC under two assumptions about WTP. First, we consider a specification in which the child's after-tax earnings does not enter WTP, so that WTP is the same as in the baseline OBRA93 specification. This would be valid under an efficient household bargaining model, or if the increases in child earnings came as a result of increased effort so that children were not WTP for their increased earnings. Second, we consider the alternative specification in which child after-tax earnings are an additional component of WTP. In the presence of household optimization frictions or liquidity constraints, this latter specification may be more reasonable as an approximation to WTP.

Table F.IV reports the results. We discuss each of these estimates in turn.

Observed Impacts on Earnings We begin with the estimates from Bastian and Michelmore (2018). They find that an additional \$1000 of EITC in 2013 USD for children aged 13-18 increases earnings as adults by \$564 (s.e. \$245), which is an increase in earnings of 2.2%. They also find that an additional \$1000 of EITC for children aged 0-5 increases earnings by \$646, but not statistically distinct from zero (s.e. of \$818). Lastly, they find that an additional \$1000 of EITC for children aged 6-12 increases earnings by \$42, also not statistically distinct from zero (s.e. \$415).

We use our lifecycle model of the ACS cross-sectional earnings patterns by age to form an estimate on the lifetime after-tax income and tax revenue generated by the children, discussed further in Appendix G. We assume a uniform distribution of child ages in the EITC population and assume that families have 2 children. This suggests an aggregate tax revenue increase per \$1000 of initial EITC expenditure of \$3,115 in 1996 USD. This would suggest an infinite MVPF. An alternative WTP assumption is that the children would be willing to pay for their increase in after tax incomes. This would be true if parents were inefficiently investing in their child, so that children were willing to pay a markup for their parents obtaining the EITC funding. Assuming no change in labor effort by the children, an upper bound on this willingness to pay is given by the present discounted value of their after tax income, \$13,633. Since costs are negative, the MVPF remains infinite.

Forecasts using College Attendance In addition to earnings impacts, Bastian and Michelmore (2018) also report impacts on college attendance and graduation. We can use our forecast methods as we do for other programs to simulate the impacts on earnings implied by the college estimates. Table F.IV reports the resulting MVPF estimates. Here, we obtain a smaller MVPF ranging from 1.729-3.966. The fact that these estimates are lower than the infinite MVPF from the earnings impacts MVPF reflects the fact that the point estimates in Bastian and Michelmore (2018) imply the impact on child earnings is larger than what would be expected based on their impact on college.

We also report estimates of the impact from Michelmore (2013) on college attendance who finds significant impacts of EITC provision at age 18 on college enrollment. We simulate the impact on earnings in adulthood. To isolate the impact of this channel on the MVPF, we assume roughly 1/18 of children on EITC are age 18 and that families have 2 children. This implies an MVPF of 1.152-1.202, depending on whether the after-tax earnings impacts on the children are included in the WTP. The next rows report the impact of including the impacts from Manoli and Turner (2018) who estimate the impact of cash on hand induced by the EITC when children are 18 years old on going to college. Similar to Michelmore (2013), these estimates imply an MVPF of 1.166-1.237. The effects in Manoli and Turner (2018) and Michelmore (2013) both have a smaller impact on the MVPF because the estimated impacts are only found on those age 18. As noted in Bastian and Michelmore (2018), there may be impacts at additional ages which would drive up the MVPF.

Forecasts using Test Scores In addition to estimates using college attendance, there is also a large literature looking at the impact of the EITC on test scores. We translate each estimate of the impact on test scores into their adult earnings impacts using the assumption from Chetty et al. (2011) that a 1SD increase in test scores corresponds to an 9% increase in earnings. The remaining rows of Table F.IV report the implied MVPF using these estimates.

Dahl and Lochner (2012) estimate the impact of the EITC on test scores, finding an increase of .045 standard deviations for every $1000 of EITC payments in 1996 USD. Translating these into their implied earnings impacts on children throughout the lifecycle suggests an MVPF of 3.483 assuming no additional WTP from increased child earnings. The MVPF rises to MVPF of 11.941 if children are willing to pay for their after tax income increases. Chetty et al. (2011) estimate the impact of the EITC on test scores and find $1000 of credit leads to a 0.062 standard deviation increase in test scores. Translating into earnings, this implies an infinite MVPF. Lastly, Maxfield (2013) uses state and federal EITC changes to estimate that $1000 of EITC benefits (in 1996 USD) leads to a .098 SD increase in test scores. This also implies an infinite MVPF.

Summary In short, incorporating the potential impact on children of the EITC can significantly change the EITC estimates relative to the modest MVPFs near one that we find when only considering the impacts on adults. We note that estimates on earnings are generally precise; forecasts using college attendance suggest lower MVPF point estimates, but perhaps exclude other potential spillover channels. Estimates using impacts on test scores show slightly larger effects if one is willing to extrapolate from the cross-sectional relationship between earnings and test scores to form long-run effects. In the end, the results highlight the importance of better understanding the potential impacts of these policies on children.

F.III.5 Alaska Permanent Fund

The Alaska Permanent Fund provides cash transfers to individuals from Alaska. Jones and Marinescu (2018) study the impact of this policy on labor supply. Using data from 1982-2014, they find minimal impacts on the extensive margin, and some evidence of a shift from full- to part-time employment. We use these estimates to translate them into their implied fiscal externality of the program.

The average size of the credit was $1602 in 2014 USD that is provided every year. Because the credit is provided regardless of labor earnings, the willingness to pay is given by $1602.

The cost of the credit depends also on the impacts on taxable behavior. To that aim, Jones and Marinescu (2018) document an increase in labor force participation of 0.1pp (95% CI of [-0.031, 0.032]), or a 0.06pp increase per $1000 of spending. For those entering the labor market, the fiscal externality is given by $T(0) - T(y)$. We do not observe $T(0)$ or $T(y)$ in the sample. We approximate this using two methods. First, we assume that the fiscal externality from labor market entry is similar for singles. $T(0)$ is given by the same $-4,735$ in 1993 USD estimate we calculated for the 1993 OBRA expansions in equation (11) above; and, we assume those who enter the labor market obtain $T(y) = -1,409$ in 1996 USD, so that $T(0) - T(y) = \$5,567.88$ in 2014 USD. We also include intensive margin impacts in the baseline. Jones and Marinescu (2018) do not document any impact on earnings, but they do find increases in part time employment of 1.8pp, or 1.12pp per $1000 credit.If we assume this is a 50% reduction in earnings for those with a median income in 1996 of $80,830.57, it implies an $452.65 reduction in earnings (=.0112*0.5*80830.57). Assuming a tax rate of 20%, this implies a fiscal externality of 20%*50%*.0112*$80,830.57= $90.53.

This implies:

$$MVPF^{Alaska} = \frac{1000}{1000 - .0006 * 5,567.88 + 90.53} = 0.92$$

We also calculate the MVPF excluding the intensive margin impacts. Here, the MVPF would be $MVPF = \frac{1000}{1000 - .0006*5,567.88} = 1.003$. The equations above should make clear that the patterns are quite robust to alternative estimates of $T(y) - T(0)$. Even if the size of the fiscal externality were $T(y) - T(0) = \$10,000$, the MVPF would be 1.006. In sum, the finding of minimal impacts on labor earnings from the provision of the Alaska Permanent Fund payments implies the MVPF is close to 1.

F.III.6 AFDC Generosity (Meyer and Rosenbaum, 2001)

Meyer and Rosenbaum (2001) use variation in AFDC induced by welfare waivers provided to states over time during the 1990s. They model how this affects the nonlinear budget set for single mothers and in particular they study the impact of the generosity of $T(0)$ on labor earnings. They find that a $1000 increase in AFDC eligibility for those with zero earnings leads to an reduction in employment of -2.95 (s.e. 0.38) percentage points using their estimates from the CPS in Column (5) of Table IV (Meyer and Rosenbaum, 2001). We assume a similar fiscal externality for single mothers as was assumed for the 1986 EITC expansion: $T(0) - T(y) = -5,009$ in 1996 USD[95]. Hence, the cost of providing \$1000 of benefits to AFDC beneficiaries is increased by $0.0295 * 5,009 = 147.77$.

$$MVPF^{AFDC} = \frac{1000}{1000 + 147.77} = 0.87$$

Intuitively, because additional transfers to those out of the labor market reduce tax revenue and increase transfers, the net cost of the AFDC is above its mechanical cost.

Incorporating impacts on children In addition to our baselines estimate, Table F.V provides estimates which incorporate the impact of the AFDC on children. We use Currie and Cole (1993)'s estimates of the impact of AFDC participation on birth weights. Their results imply that AFDC participation during pregnancy increases birthweight by 4.6 ounces, or 4%, for Black children, and 32.0 ounces, or 27%, for poor white children on average. We scale this effect by the ratio of the program cost of our conceptual policy (\$1000) to the average yearly AFDC amount in the Curie and Cole sample, inflation adjusted to 1996. Thus we obtain that every \$1000 (1996 USD) in AFDC benefits to the mother in the year of birth increases birth weight by 1% for black children, and 7% for white children. We then multiply these effects by the elasticity of adult earnings with respect to birthweight of 0.1 from Black et al. (2007) and take a weighted average over black and white families to obtain an overall earnings increase of 0.4%. Applying our usual projection methods, this yields a lifetime (discounted) earnings impact of \$688.84, to which we apply a 20.0% tax rate. Finally, we use the fertility rate from Currie and Gruber (1996a) and assume that 6.5% of women on AFDC give birth every year. We add this impact to the simple WTP and costs described above, so that $FE = 147.77 - 0.065 * 0.2 * 688.84 = 138.81$ and $WTP = 1000 + 0.065 * (1 - 0.2) * 688.84 = 1035.82$, so that:

$$MVPF^{AFDC} = \frac{1035.82}{1000 + 138.81} = 0.91$$

[95] For the EITC expansion, we calculate the fiscal externality using the average SNAP and AFDC payments in 1986. Here we use the values from 1988 as Meyer and Rosenbaum (2001) use variation between 1984 and 1996.

Table F.V: AFDC MVPF Estimates

Panel A: AFDC Generosity

	MVPF	WTP	Cost	Parents					Children				Notes
	(1)			B	T(0)	T(y)	d[LFP]	FE_{Single}	WTP_{Child}	FE_{Child}	d[BW]/BW	d[Y]/Y	
Baseline Estimate	0.87 [0.846 ; 0.901]	1000	1148	1,000	-5457	-448	-0.030	147.8					d[LFP] for singles from Meyer & Rosenbaum (2001)
Including impacts on kids	0.91 [0.827 ; 0.997]	1036	1139	1,000	-5457	-448	-0.030	147.8	35.8	-8.95	0.039	0.004	BW (Birthweight) impact from Currie & Cole (1993)

Panel B: AFDC Term Limits

	MVPF	WTP	Cost	AFDC Use			Earnings		Notes
	(1)			AFDC Benefit	% on AFDC 60+ Months in 1993	Impact on utilization	d[Y]	FE	
Baseline Estimate	0.81 [0.727 ; 0.905]	1496	1839	5316	0.346	0.065			AFDC Utilization impact from Grogger (2003), baseline % on AFDC for 60+ months from Pavetti (1995)
Including Earnings Impacts	0.905 [0.749 ; 1.099]	1496	1654	5316	0.346	0.065	933	157	Earnings impacts from Grogger (2003)

Notes: AFDC generosity dollar values in 1996 USD, AFDC term limits in 1998 USD.

F.III.7 AFDC Term Limits

In addition to AFDC generosity, Grogger (2003) estimates the impact of term limits on AFDC on welfare use and labor income using variation in patterns for women with and without kids $<=$ 13 years old who are subject to the 5-year time limit (those without children under age 18 are not generally eligible for AFDC benefits). Section V summarizes the impact on welfare receipt, showing that providing time limits reduces take-up by people who are technically eligible but are worried about the time limit by 2.1%, or 6.5% of the welfare utilization rate.

To turn this into an MVPF, we consider a thought experiment of relaxing term limits. There's a mechanical cost savings for those who were on welfare longer than 5 years. In 1995, 34.6% of beneficiaries had been on welfare longer than 5 years (Pavetti (1995)). Absent any other changes, the 6.5% reduction in utilization which Grogger (2003) estimates implies a utilization rate of 28.1% without term limits. This 28.1%, multiplied by the average yearly welfare AFDC receipt, corresponds to the mechanical benefits and hence WTP for such a relaxation of term limits. The net cost to the government is then the total utilization incorporating the behavioral response, 34.6%, multiplied by the average yearly welfare AFDC receipt. Combining, this suggests an MVPF of term limits of $28.1/34.6 = 0.81$, where the AFDC yearly receipts in the numerator and denominator cancel each other out. This suggests the government imposes a welfare loss of \$0.81 for every \$1 of revenue it obtains from those on welfare beyond 5 years.

F.III.8 Negative Income Tax Experiment

A negative income tax (NIT) replaces all pre-existing benefits with a cash payment that is taxed back at a "clawback rate". Price and Song (2018) study the Seattle-Denver Income Maintenance Experiment which provided a lump-sum payment to families between \$17,600 and \$25,900 with clawback rates between 50% and 80%.[96] During the five-year period of the experiment, individuals face no other marginal tax rate (e.g. no federal income tax rate). The authors find that NIT led to long run earnings declines (largely driven by earlier retirement) and slight negative impacts on children's earnings. We use these short- and long-run estimates from Price and Song (2018) to form the MVPF[97]. The calculations involve many components, but at the end of the day the MVPF is well below 1 because the provision of the transfer reduced labor earnings by both adults and children.

Because of data limitations in matching the original RCT data to administrative records, Price and Song (2018) restrict their analysis to families with at least two children. Our unit of analysis throughout the construction of the MVPF is the family. We translate estimates of the policy's effect on individual parent variables, such as transfer receipt and tax revenue, into family estimates by multiplying by the ratio of parents to families, $5{,}185/3400 = 1.525$ (Price and Song (2018), Table A.1). Our long-run estimates then reflect a hypothetical family with 1.525 earner (aged 35 when the policy begins) and the average number children: 2.9 (Price and Song (2018), Table A.1).

We estimate the WTP and $Cost$ of the policy as the sum of short and long-run components. For costs, we have:

$$Cost = Cost_{p,short} + Cost_{p,long} + Cost_k$$

where $Cost_{p,short}$ is the short-run difference in net government payments between the treatment and control group during the five-year experiment. $Cost_{p,long}$ is the causal effect on net government taxes/transfers after the five years of the experiment, inclusive of effects on retirement for example. And, $Cost_k$ is the causal effect on net government/taxes/transfers from children's later life outcomes.

Willingness to pay can arise from two sets of beneficiaries:

$$WTP = WTP_p + WTP_k$$

where WTP_p is the WTP by the parents who received the tax change and WTP_k is any externality onto the children that is not captured by the WTP of the parents. For WTP_p, assuming the experiment is a small change in payments, the WTP is simply the mechanical change in tax liability, M. Conceptually, one can write the observed difference in tax/transfer payments by the government to parents during the experiment, $Cost_{p,short}$, as

$$Cost_{p,short} = M + B$$

where M is the mechanical component and B is the impact of the behavioral response on earnings on the payments. The mechanical component is given by the total cost minus the impact on costs from the behavioral response:

$$M = Cost_{p,short} + \tau dY_{short}$$

[96] All dollar figures in this section are in 2013 dollars.
[97] We also draw upon several estimates from Price and Song (2016).

where dY_{short} is the causal effect of the policy on earnings during the time of treatment and τ is the marginal tax rate faced by the families.

If the policy were a marginal change in parents' budget constraints, we would have $WTP_p = M$. However, the policy is not a marginal change. If the first dollar caused the behavioral response, dY_{short}, then the entirety of the impact of the behavioral response would reflect WTP. In contrast, if the last dollar of transfer caused the behavioral response, then none of the behavioral response would reflect a WTP by the beneficiaries. We allow for a fraction θ of the induced change in transfers from behavioral responses to be valued by the individuals, although our baseline specification sets $\theta = 0$. This means that WTP_p is given by M plus θ of $-\tau dY_{short}$ and θ of $Cost_{p,long}$.

$$WTP_p = Cost_{p,short} + (1-\theta)\tau dY_{short} + \theta Cost_{p,long}$$

Next, we consider any externalities of the policy that fall onto children. Here, we consider two cases. First, one may assume no externality on children. This would be valid if any impacts on children are the results of efficient household decision making (a la Becker). However, it may be the case that any changes to earnings of children are not captured by parental WTP. To that aim, in our baseline specification we allow for children to have WTP for an externality given by the size of after-tax earnings on children. Conceptually, in the NIT case children's earnings declined; so this suggests children would be willing to pay to prevent their parents from obtaining the transfer. Revealed preference makes it perhaps difficult to justify this argument, but for consistency with other policies where we incorporate WTP of children in this manner, we include this component.

Throughout, we incorporate uncertainty in our estimates of the MVPF using the reported standard errors and confidence intervals of each estimate, assuming positive correlation between components (signed appropriately to construct the most conservative confidence interval).

Estimating $Cost_{p,short}$ and $Cost_{p,long}$ Table F.VI reports the cost estimates from the parents. This is given by the causal effect of the policy on total tax and transfer payments from the government, inclusive of the lump-sum payment provided by the experiment. Discounting at a 3% interest rate, the change in tax/transfer payments equals $19,443 per person, as reported in row 1 of Table F.VI. Scaling to 1.525 household heads implies a total set of transfers of $Cost_{p,short} = \$29,651$.

Next, we compute $Cost_{p,long}$ as the sum of the tax revenue and SSDI costs from the adults in the long run. To begin, we note that Figure C.2b suggests the treatment leads to a present discounted value reduction in earnings from age 40 of $28,387. At each age we calculate the corresponding marginal tax rate given our baseline earnings estimate and our CBO tax+transfer rate (this is usually 19.9%). This implies a net present value loss of tax revenue of $4,307 per person, which corresponds to $6,568 per family, as reported in Table F.VI, column (3).

In addition, the policy leads individuals to apply more to SSDI. We translate these estimates into the cost of DI by assuming a PDV cost per SSDI recipient of $170,163. The policy leads to an increase in SSDI receipt of 3.35 percentage points per adult and 4.19 percentage points per family. We estimate this leads to an added fiscal cost of $8,242 per family.[98] This implies $Cost_{p,long} = 8242 + 6568 = \$14,810$.

Estimating WTP_p Next, we note that the policy had an impact on earnings in the first five years of $9,421 per person, as reported in Figure 1b and discounted at 3% per year. Imposing a representative clawback rate of 65%, this implies that the mechanical effect of the policy on after tax income was $19443 - 14367 \times 0.65 = 10105$. As we set $\theta = 0$ in our baseline specification this is the only non-zero component, implying $WTP_p = 1.53 \times 10105 = 15409$.

Estimating $Cost_k$ and Incorporating Child Impacts into WTP Next, we incorporate the child impacts. Calculation of the reduced tax income due to reduced child earning exercise is very similar to the corresponding exercise for adult earnings. For the representative child we calculate an average annual earnings reduction of $321. This results in a discounted decrease in earnings of $4847 and in tax income of $804. Over the 2.89 children this corresponds to a discounted decrease in earnings of $14056 and in tax income of $2325. Similarly, we incorporate the fiscal externalities from changes to SSDI claims, which implies a fiscal cost of $514 per child, or

[98] We arrive at this number for the cost per SSDI as follows. Following Von Wachter et al. (2011, Appendix Table G3 and G4), we assume that a person receiving SSDI receives it for 10 years if they are a man and 11.7 years if they are a woman. We transform Von Wachter et al. (2011)'s present discounted values into annual costs in 2013 dollars, resulting in an annual cost of $27,032 for women and $29,470 for men. We then sum and discount these annual costs using our own discount rate, given the assumed gender-specific duration that the person will receive SSDI. The expected cost for both parents and children is the simple average of these gender-specific discounted costs. The expected cost is $74,374 for the child and $170,163 for the parent (because they are discounted much less). These expected costs are then multiplied by the increased probability of claiming SSDI due to the policy (0.0335 for parents) and 0.0073 for children). Multiplying by the average number of adults results in a total expected increase in SSDI costs equal to $5,404 per adult.

\$1487 for the 2.89 children. Summing, this implies a net cost of $Cost_k = 3812$. Hence, the total cost is given by $Cost = 29651 + 14809 + 3812 = 48273$.

For the measure of WTP that includes child impacts we note that Figure C5.b implies a decrease in child after-tax earnings of 4044 in present discounted value per child. In addition, the policy increased SSDI receipt by \$542 per child. Summing across 2.89 children per family implies $WTP_k = -10212$.

MVPF The total cost of the policy is \$48,273 and parent and child WTP sum to \$5,198, which implies a baseline MVPF of 0.11 (95% CI of [-0.66, 3.81]. If we exclude the WTP for children, this implies a WTP of \$15,409, which implies an MVPF of 0.32 and 95% CI of [0.16, 1.08].

Table F.VI: Seattle and Denver Income Maintenance Experiment from Price and Song (2016)

	Individual Effects	N	Simulated Family	Price and Song (2018) Reference
Sample:	(1)	(2)	(3)	(4)
Cost			**48272.97**	
$Cost_{p,short}$	19443.40	1.525	29651.18	Figure 1a
$Cost_{p,long}$	9711.06	1.525	14809.37	
Tax Impact	4306.66		6567.66	Earnings from Figure C.2b + CBO tax/transfer rate
Disability Costs	5404.40		8241.71	Table C.1 + Wachter, Song & Manchester (2011, appendix tables G3 and G4)
$Cost_k$	1317.81	2.89	3812.42	
Tax Impact	803.78		2325.33	Earnings from Figure C.5b + CBO tax/transfer rate
Disability Costs	514.03		1487.09	Table C.4 + Wachter, Song & Manchester (2011, appendix tables G3 and G4)
WTP_p	10104.56	1.53	15409.46	
dY_p	-14367.44			Figure 1b
Behaviour valuation, θ	0			Valuation rate for parent behavioural changes
Clawback Rate, τ	0.65			Midpoint of 50-80% Clawback Rate
$(1-\theta)*\tau*dY_p$	-9338.83			
$Cost_{p,short} + (1-\theta)*\tau*dYp$	10104.56			
$\theta*Cost_{p,long}$	0.00			
WTP Including Child Externalities			**5197.92**	
WTP_k	-3529.74	2.89	-10211.54	
dY_k	-4847.55			
$dY_k * (1-\tau_k)$	-4043.77			
DI Income	514.03			
MVPF (without child WTP)			**0.32**	
95% CI			[.16,1.08]	
MVPF (Baseline with child WTP via earnings)			**0.11**	
95% CI			[-.66,3.81]	

Note: All values in 2013 USD from Price and Song (2016) who use a PCE deflator.

F.III.9 Welfare Reform

During the 1980s and 1990s, many states began transitioning their welfare programs from the traditional unrestricted cash welfare program of Aid to Families with Dependent Children (AFDC) to programs that impose limits on welfare receipt and attempt to help recipients enter or re-enter the labor force through education or job training programs, job search assistance, financial incentives for employment, and work experience / job placement programs. As part of the waiver process to implement these programs, states evaluated their programs through a randomized control trial (RCT). Each of the 27 US-based programs we study here were evaluated by the Manpower Development Research Corporation (MDRC).

Greenberg et al. (2010) (page 4) provides a description of the six primary categories of the 27 welfare to work program, summarized as:

> "**Mandatory work experience programs:** Often following a period of job search, individuals in these programs are assigned to unpaid jobs, which are usually located at government agencies or nonprofit institutions.
>
> **Mandatory job-search-first programs:** Individuals are assigned to job search activities upon program entry. Other types of assigned activities can follow for individuals who do not find jobs. All five of the programs analyzed in this category encouraged quick entry into work and strongly enforced a continuous participation mandate.
>
> **Mandatory education-first programs:** Individuals are assigned to education activities prior to job search. The most common of these activities were GED preparation classes or Adult Basic Education (ABE). In some programs, individuals could also participate in English as a Second Language (ESL), vocational training, or employment training classes. Typically, job search assignments follow the completion of courses of study.
>
> **Mandatory mixed-initial-activity programs:** Individuals are assigned to participate initially in either an education or training activity or in a job search activity, depending on an assessment of their needs. Other assigned activities follow these initial activities if individuals remain unemployed.
>
> **Earnings supplement programs:** Individuals are provided with financial incentives intended to encourage work. These incentives supplement their incomes while at work.
>
> **Time-limit-mix programs:** These programs require individuals to participate in employment-orientated activities, provide them with financial incentives, and limit the amount of time they remain eligible for welfare benefits or can receive benefits without working."
>
> *Source: Greenberg et al. (2010), Page 4*

The results were synthesized in the cost-benefit analysis provided in Greenberg et al. (2010). We follow Greenberg et al. (2010)'s perspective of using a 5-year time horizon, motivated by evidence they suggest that they do not find evidence of persistent effects.

Appendix Table 1 of Greenberg et al. (2010) provides the estimates of the impact of the program on participants income and transfers received. They also study the impact of each program on government tax and transfer payments along with operating costs of the program (e.g. the cost of running the education or job training program for participants). To that aim, we form estimates of the cost of each policy using estimates reported in Column E of Appendix Table 1 in Greenberg et al. (2010).

Difficulty Measuring WTP While the RCT facilitates a straightforward construction of the impact on costs, constructing the willingness to pay for these programs is vastly more difficult due to the design of the RCTs. As noted by MDRC[99], "all contained a core quid pro quo arrangement in which the government would offer education, training, job search assistance, and support services to people receiving cash welfare, while most recipients — the majority of them single parents — would be required to participate in such services in order to qualify for benefits." For example, individuals enrolled into the education programs were required to attend classes. Those who did not show up could lose 20-40% of their welfare benefits. As a result, some individuals might have a negative willingness to pay for the services provided. In contrast, others might have a positive willingness to pay for the education and job search assistance that was provided free of charge. For this reason, the design of the RCTs prevent a clear calculation of the willingness to pay for the program.

[99]See https://www.mdrc.org/project/evaluations-state-welfare-work-programs#design-site-data-sources, accessed on July 7, 2019.

As a result, we consider the welfare impact of these programs using three possible measures of willingness to pay to attempt to bound the MVPF of the RCTs. First, we assume individuals value the program at the program's operating costs. This would be valid if the program were traded in a private market and individuals were at the margin of purchasing the program. Second, we assume a willingness to pay equal to the size of the change in transfers. This would be an approximation to the willingness to pay in a world where some individuals had their benefits reduced from non-participation. Importantly, it could be an overstatement of the willingness to pay if some individuals participated only to avoid sanctions. Lastly, we consider valuing the program at its impact on net income after taxes and transfers. This would be a valid measure of willingness to pay if individuals had no change in effort in response to the program so that their monetary budget constraint captured their willingness to pay.

The results for each method are presented in Appendix Figure VII. We discuss each category in turn.

Earnings Supplements For programs providing earnings supplements, we find MVPFs ranging between 0.8 and 2. Note that this program is similar to a standard EITC experiment targeted towards individuals on AFDC. To that aim, it's perhaps not surprising that we find estimates loosely similar to the MVPFs for the EITC experiment above.

Job Search First For programs requiring participants participate in job search activities, we find that 4 out of 5 of these policies "pay for themselves". This means that if the willingness to pay is positive, then the MVPF would be infinite. To that aim, we do see individuals in the program obtain increased after tax income for 3 of these programs. But, individuals also experience a significant reduction in transfers. If individuals value the program at the size of their lost transfers, we find MVPFs that are finite and close to 1.5. This shows the value of better understanding the WTP of individuals to obtain these services alongside the cost to them of mandating participation. Nonetheless, the results suggest that providing complementary search benefits for those facing high tax rates may provide high returns.

Mandatory Education Programs For mandatory education programs, we find mixed results depending on the willingness to pay methods. Two out of six such programs had negative costs to the government; mandated education programs for those on welfare led to a reduction in government costs. As with job search programs, the key question is whether the participants are better off. On the one hand, they have higher earnings, which indicates they may be better off. On the other hand, they have reduced transfer income that exceeds their increase in income, so that their net income after taxes and transfers on average has declined. This suggests a component of the income decline is because some individuals were sanctioned for not showing up to class. To that aim, those individuals may have a negative willingness to pay for the program requirements, and thus it's unclear whether the program led to an increase or decrease in welfare. We find similar unclear patterns for programs mandating work experience.

Mixed programs The "Mixed" programs allowed case workers to funnel program participants into either education programs or job search programs, depending on assessed needs. For these programs, we find that 5 out of 8 of these programs pay for themselves. However, each of these also face reductions in transfer payments which could partially reflect the mandatory nature of the programs. However, even valuing the program at the reduced transfer cost suggests fairly high MVPFs because of the fiscal externalities induced by the increased earnings. This suggests a potential value in these programs for having caseworkers provide tailored services to help increase labor earnings for those receiving large transfers. On the other hand, it could be that even the realized reduction in transfers understates the costs imposed on participants to engage in the program; in which case the MVPFs would be lower.

Term Limits Imposing term limits leads to increased earnings and reduced transfer payments, as might be expected. However, individuals were also required to participate in employment search programs, which suggests that here again the measure of willingness to pay is complicated. And, the MVPFs span a wide range for these programs from being a Pareto increasing to Pareto decreasing policy.

Work Experience For work experience programs, we find estimates that again span a wide range and prevent clear conclusions without a more solid understanding of willingness to pay.

Summary Overall, the results for earnings supplements complement and are consistent with our estimates for the EITC policies. The remaining estimates provide a wide range of possible MVPFs that depend on the method used to measure willingness to pay. Indeed, within each category we find some combinations of programs and WTP assumptions that suggest perhaps the policies were Pareto improvements; others that suggest they were Pareto dominated. In this sense, while it was helpful for research to conduct the RCT of these major welfare to work policy changes, the results suggest a value to future work that focuses not only on measuring the impact of these policies on behavior but also measuring willingness to pay, especially for policies that both provide services and impose requirements on participants. Without such measures, it is not feasible to adequately infer the MVPF of these welfare reform RCTs.

G Tax Rates

This appendix outlines the tax rates we use to estimate the fiscal externality of each program for cases where tax and/or transfer payments are not directly reported in existing empirical literature. Since our primary conclusion of the paper is that policies targeting children have often "paid for themselves" through fiscal externalities in adulthood, we proceed in these cases by attempting to make conservative assumptions about the taxes faced by these populations. Our baseline approach uses tax rates and tax-and-transfer rates provided by the Congressional Budget Office (CBO) for 2016. This analysis could be performed used tax and transfer rates specific to the year of policy implementation, but we are not aware of any comprehensive historical source on the distribution of those rates. As a result, we use the 2016 data and assess the robustness of our results to alternate tax and transfer rates. In Appendix Figure V, we show that our primary conclusions continue to hold even if one were to conservatively use an assumption of a tax rate of 10% or a more aggressive 30% tax rate.

The tax-only rate includes federal and state individual income taxes, excluding payroll taxes. Federal tax rates are drawn from CBO estimates of the average marginal income tax rate in 2016 for each quintile of earnings (CBO, 2019 – see Exhibit 12). The state tax rate is taken from the CBO estimate (2.6ppt) of the contribution of state taxes to the total marginal tax rate for low and moderate-income taxpayers in 2016, on the basis of laws in place in 2013 (CBO, 2016 – see Table 1). We assume that the contribution of state taxes to the overall marginal tax rate is constant across earnings quintiles. To implement this tax rate, we crosswalk income estimates from each program to an income quintile using a historical series on household income from the Census (Census, 2017 – see Table H1).

The tax-and-transfer rate incorporates the combined effects of federal and state individual income taxes, SNAP benefits, and cost-sharing subsidies for health insurance[100] as of 2016 (CBO, 2016 – see Figure 4). The CBO releases estimates of this effective marginal tax rate for each 50% bin of the Federal Poverty Line (FPL) (e.g. 0-50% of FPL, 50-100% of FPL etc.). We use the median marginal tax rate within each bin as our estimate. In cases where incomes exceed 450% FPL, we assign the marginal rate for the top income group. In most cases, the policies we consider have information on the incomes of participants; but they often do not have information about the number of children. Therefore, to crosswalk income estimates from each program to the relevant bin of the FPL, we use the historical series on the FPL from the Census combined with a simplifying assumption that each family has two children.

To be conservative, our baseline estimates exclude payroll taxes because there is a reasonable view that at least some portion of these benefits will be returned to those individuals and therefore do not constitute net revenue to the government. To implement this, we subtract the payroll tax components from the marginal tax+transfer rates using the CBO estimate (13.9ppt) of the contribution of payroll taxes to the total marginal tax rate for low and moderate-income taxpayers in 2016 (CBO, 2016 – see Table 1). We further assume that the contribution of payroll taxes to the overall marginal tax rate is constant across the earnings distribution.

Lastly, in cases where we are forecasting earnings over the lifecycle, we assign the marginal tax rate to earnings at age 40 and apply this tax rate to all income over the lifecycle.

Table G.I reports the tax rates and tax+transfer rates used in our analysis for different income levels. And, as noted above, Appendix Figure V reports the robustness of our approach to the alternative approach of using a conservative 10% tax rate assumptions.

[100] Health insurance refers to Affordable Care Act (ACA) taxes. A comparison of the 2016 CBO tax rates to the 2012 CBO tax rates (which did not account for ACA taxes) suggests that the contribution of ACA taxes to the overall marginal tax rate is small (1-2ppt).

Table G.I

2016 Congressional Budget Office Tax Rate Estimates

	Raw Tax Rates	Including State Taxes and Excluding Payroll Taxes
	(1)	(2)
A. Tax-and-transfer rates, by bins of the Federal Poverty Line (FPL)		
0-50% FPL	0.142	0.003
50-99% FPL	0.235	0.096
100-149% FPL	0.338	0.199
150-199% FPL	0.339	0.200
200-249% FPL	0.328	0.189
250-299% FPL	0.325	0.186
300-349% FPL	0.326	0.187
350-399% FPL	0.327	0.188
>= 400% FPL	0.335	0.196
B. Tax-only rates, by earnings quintiles		
1st Quintile	-0.019	0.007
2nd Quintile	0.103	0.129
3rd Quintile	0.195	0.221
4th Quintile	0.201	0.227
5th Quintile	0.260	0.286

Notes: This table reports CBO estimates of marginal tax rates as of 2016. Panel A records tax-and-transfer rates which incorporate the combined effects of federal and state individual income taxes, SNAP benefits, and cost-sharing subsidies for health insurance. Panel B records tax-only rates which include federal individual income taxes, excluding payroll taxes. In both panels, Column 1 contains the raw CBO tax rate estimates. Column 2 additionally incorporates state individual income taxes (for the tax-only rates) and deducts payroll taxes. This is implemented by assuming a flat 2.6ppt contribution of state income taxes to the overall marginal tax rate and a flat 13.9ppt contribution of federal payroll taxes to the overall marginal tax rate.

H Confidence Intervals

This Appendix discusses our construction of the confidence intervals for the MVPF. We begin by outlining the approach, then we show through Monte Carlo simulations that our approach provides appropriate coverage. The coverage tends to be slightly conservative in cases where costs approach zero.

Let (w, c) denote the true WTP and cost for a given policy. Without loss of generality and for ease of comparison, we normalize the direction of policies so that we expect all policies to have positive willingness to pay (e.g. we consider the MVPF of a tax cut, not a tax increase). We observe a draw (W, C) from a sampling distribution in existing literature. We assume this distribution is normal, which is consistent with our procedure of forming these estimates as linear combinations of estimates from existing literature and using the reported standard errors as our measure of the sampling uncertainty.[101] It is well-known that bootstrap can provide a valid confidence set for (w, c) (e.g. Bickel et al. (1981)). However, our interest is in the MVPF; for $w > 0$, the MVPF equals $\frac{w}{c}$ if $c > 0$ and equals ∞ if $c < 0$.

We observe (W, C) not the true (w, c). We normalize our definition of the policies so that $W > 0$. But, in finite samples we might have some regions of the sampling distribution for which $W < 0$. For cases where $C > 0$, these do not pose a problem as $\frac{W}{C}$ is continuous as W crosses 0. However, a deep conceptual problem arises if in some regions of the sampling distribution we have both $W < 0$ and $C < 0$. These draws suggest that the researcher has uncertainty about the fundamental incidence of the policy. One can of course form an MVPF for policies with $W > 0$ and $C > 0$ just as easily as cases where $W < 0$ and $C < 0$. But, the intuitions behind these policies are the opposite: as one corresponds to expanding and the other corresponds to contracting the policy. Intuitively, there is an analogy to the weak instruments problem in IV regression when the sampling uncertainty suggests one is not sure whether the instrument increases or decreases the endogenous regressor.

To account for this source of uncertainty and ensure adequate coverage, we use the bootstrap to calculate the fraction of draws for which $W < 0$ and $C < 0$. Denote this fraction of the sampling uncertainty in this region by α^N. Then, for the remaining region of the sampling distribution, we can readily construct the MVPF: we assign $\frac{W}{C}$ to cases where $W < 0$ and $C > 0$ and we assign ∞ when $W > 0$ and $C < 0$. We then form a 95% confidence interval by taking the bootstrapped sampling distribution of the resulting MVPF estimates and forming the $95 + \alpha^N\%$ confidence interval of the resultant distribution[102]. By adding α^N, we account for the fact that in some draws we are unable to form a meaningful MVPF estimate. Lastly, we note that this distribution tends to be non-normal, and thus we use bias-corrected confidence intervals proposed in Efron (1982).

Monte Carlo Simulations Appendix Figure H.IA reports the coverage rates for our bias-corrected 95% confidence intervals of our MVPF estimator on a heatmap. As can be shown, we obtain appropriate coverage throughout the regions of the (w, c) parameter space, with slightly overly-conservative coverage as costs approach and go below zero.

We construct this figure as follows. Focusing on the region $w \in [0, 4]$, $c \in [-1, 3]$ we sample w, c uniformly from grid regions of length and width 0.25. The correlation between W, C is then sampled uniformly over $[-1, 1]$ and the standard errors of W, C are set to 0.5. Having drawn estimates W, C we bootstrap these quantities, forming bias-corrected 95% confidence intervals for the MVPF. We bootstrap W, C with 1000 iterations, consistent with our approach with our real MVPFs. We estimate our coverage rates over 3000 samples of w, c within each grid region.

As the figure shows, we attain appropriate 95% coverage (up to simulation noise) in every region. We over-cover significantly when w is positive and c is negative: in these cases the true MVPF is infinite, and the 95% CI will only fail to contain infinity if $C > 0$ for 97.5% of bootstrap draws. We also find that appropriate coverage is attained even without the Efron (1982) bias correction.

Relation to Weak Instruments The fact that a standard bootstrap procedure provides adequate coverage might be surprising in light of the literature on weak instruments. This literature documents that estimates of ratios, such as $\frac{w}{c}$ or a ratio of a "reduced form" to "first stage" in IV regression, have poor properties when the denominator is close to zero. The reason we do not obtain this problem in our case is that we are not interested

[101]The assumption of normality is not entirely without loss of generality. Meager (2016) conducts a meta-analysis of RCTs of micro-credit and finds evidence of fat tails in the underlying data that meaningfully affects inference. Because we do not obtain the underlying data of any of the studies, we abstract from these potential issues, but note the importance of future work reporting to the literature whether their data has a normal sampling distribution.

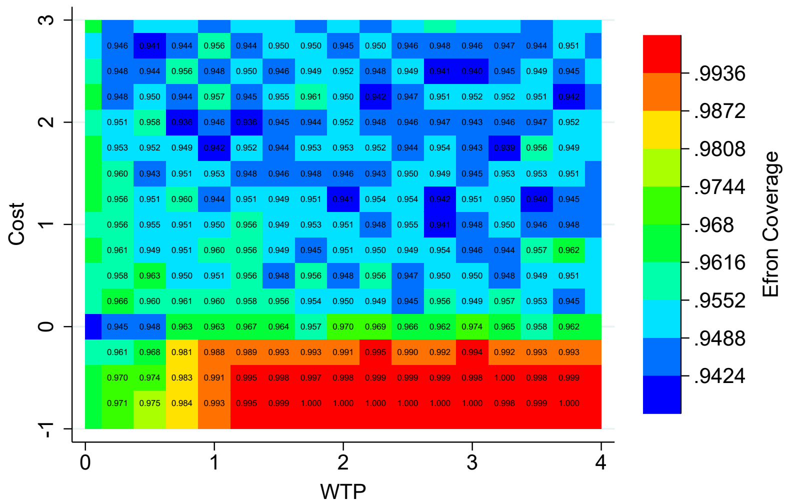
[102]In the case of $\alpha^N \geq 5\%$, we cannot reject the hypothesis that the MVPF is either $-\infty$ or ∞ and thus the confidence interval is $[-\infty, \infty]$.

in estimating the ratio, $\frac{w}{c}$. Rather, we wish to construct the MVPF which is capped at ∞ as c crosses below zero when $w > 0$. For this reason, the MVPF imposes a natural continuity in the parameter space as c crosses zero.

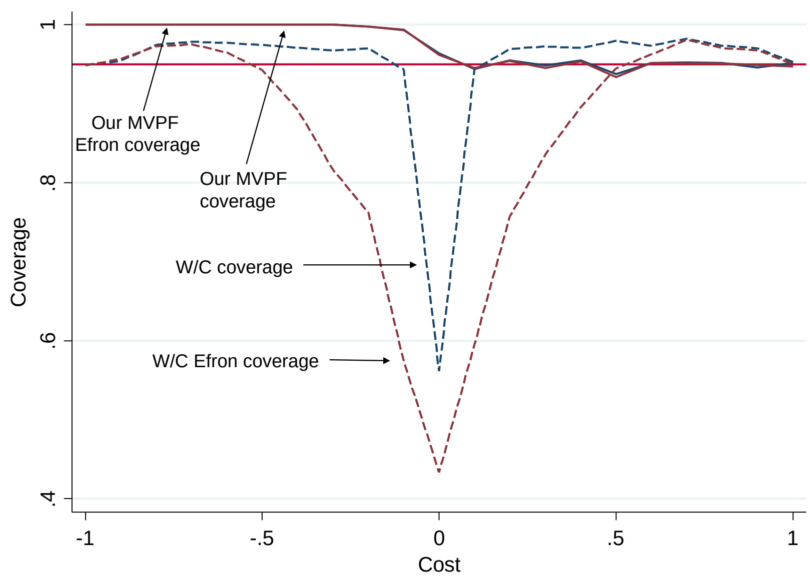
To illustrate this, Appendix Figure H.IB shows the coverage of regular and bias-corrected confidence intervals for our MVPF and for the estimator of the ratio $\frac{w}{c}$. We consider the illustrative case when w is fixed at 1 and c varies around 0. The dotted lines show what would happen to coverage rates if one were attempting to estimate the simple ratio $\frac{w}{c}$: coverage drops dramatically around $c = 0$. In contrast, we do not observe this drop in coverage for our estimator of the MVPF. This demonstrates that the cap on our MVPF ensures we attain appropriate coverage with our confidence intervals; our CIs attain at least 95% coverage as costs pass over zero.

APPENDIX FIGURE H.I: Confidence Intervals

A. Efron (1982) Bias-Corrected Confidence Interval Coverage



B. Coverage Around Zero Costs



Notes: This Figure reports Monte Carlo simulation results for our procedure for constructing appropriate confidence intervals for the MVPF. Panel A reports a heatmap of our simulated coverage for regions of the (w, c) parameter space. As illustrated, we obtain appropriate 95%+ coverage (up to simulation noise) throughout the parameter space. Panel B illustrates how the issues with constructing confidence intervals for the MVPF differ from the classic weak instruments problem. The dotted lines report confidence intervals for a canonical "IV" estimator that corresponds to the ratio w/c. This estimate "flips sign" when c crosses 0, which leads to a well-documented problem of poor coverage of traditional confidence intervals. In contrast, the MVPF reaches infinity when $w > 0$ and c crosses below 0. The coverage of our MVPF confidence intervals are illustrated in the solid lines (red for the baseline approach that uses the Efron bias correction; blue for the version without the Efron bias adjustment). As illustrated, we obtain appropriate coverage throughout the parameter space, even as c crosses 0.

I Projection Method

This section discusses our methods for projecting the lifecycle impacts on long-term earning gains based on short-term causal effects. We follow a straightforward projection approach similar to that of Chetty et al. (2016), and assess the robustness of our results to alternative forecasting methods. Details can be found in the online GitHub repository for this project.

Projecting Long-Term Earnings Gains For many policies, researchers have estimated earnings impacts over a limited number of years. In order to calculate a point estimate for the MVPF of those policies that uses harmonized time horizons across policies, we project out earnings gains over the lifecycle.

We apply our projection methods when we observe treatment and control incomes $Y^t_{\tilde{a}}$ and $Y^c_{\tilde{a}}$ at a given age $\tilde{a}$, implying an observed earnings impact \$$\Delta_{\tilde{a}} = Y^t_{\tilde{a}} - Y^c_{\tilde{a}}$. In that case, we want to project forward through the lifecycle to obtain earnings impacts for the rest of the lifecycle, $\{\Delta_a\}^{65}_{\tilde{a}+1}$. We assume a retirement age of 65 throughout.

We make a number of assumptions in order to predict these impacts. First we assume that the ratio of earnings between the treatment and control groups $Y^t_{\tilde{a}}/Y^c_{\tilde{a}}$ is constant throughout the lifecycle. Second, we assume that the lifecycle profile of earnings for those affected by the relevant policy follows the U.S. population average, $\bar{Y}_a$. Third, in order to estimate U.S. population average earnings at a given age in the relevant year, we estimate average earnings across ages in 2015 using data on individual incomes by age from the 2015 American Communities Survey (ACS).[103] We then assume that wages have grown and will grow at a constant 0.5% rate across the distribution. We account for inflation using CPI-U-RS.

Using our 0.5% wage growth assumption we can construct a hypothetical wage distribution for the years relevant to the policy, and then estimate the population average income for individuals of the appropriate age, $\bar{Y}_a$. We can then estimate earnings impacts Δ_a as

$$\Delta_a = \frac{Y^t_{\tilde{a}} - Y^c_{\tilde{a}}}{Y^c_{\tilde{a}}} \times \frac{Y^c_{\tilde{a}}}{\bar{Y}_{\tilde{a}}} \times \bar{Y}_a$$

Here, the first term reflects assumption that the percentage earnings impact remains constant over time. The second term reflects the assumption that earnings in the control group remain a constant fraction of the average population income at each age. The third term captures the population average income for individuals of the appropriate age. This methodology for conducting earnings forecasts is implemented by the Stata Ado file *est_ life_ impact*, which is included in the online GitHub repository for this project.

Forecasting Earnings Based on Other Causal Effects In some cases, researchers analyzing a given policy have not analyzed the impact of that policy on long-term earnings, but rather estimated its impact on a shorter-term outcome that is correlated with earnings. In order to translate these short-term outcome variables into an earnings effect, we must incorporate therefore an outside estimate of the outcome variable's relationship to earnings. In our primary specification, we only use short-term outcomes that are related to college enrollment or years college completed. In particular, we translate educational gains into earnings gains using estimates from Zimmerman (2014) on the return to schooling.

Combining a policy's effect on college enrollment with Zimmerman (2014)'s estimates of the return to schooling, allows us to measure $Y^t_{\tilde{a}}/Y^c_{\tilde{a}}$, the percentage impact of the policy on earnings. This methodology is implemented by the Stata Ado file *int_ outcome,* which is included in the online GitHub repository for this project. Once we've calculated that percentage earnings gain, we can then combine it with an estimate of counter-factual income and determine the long-run earnings impact using the methodology outlined in "Projecting Long-Term Earnings Gains" above. When bootstrapping our estimates we also bootstrap this percentage impact, assuming it has the same t-stat as the level effect on earnings estimated by Zimmerman (2014). We use the same set of draws of this effect across programs, ensuring that our confidence intervals remain valid when combining estimates across programs to construct averages.

In our robustness checks we incorporate estimates of the causal effect of expenditures on additional secondary outcomes such as child test scores or or low birth weight prevalence. We follow the same approach used to analyze college enrollment changes and translate those secondary outcomes into earnings gains.

[103] With only one exception, we always take mean incomes by age across all individuals in the ACS. The one exception is that when projecting the lifecycle earnings of those marginally attending community college we take this mean income across individuals with high school degrees but no college education.

Estimating Counterfactual Earnings The earnings projection method outlined in "Projecting Long-Term Earnings Gains" requires knowledge of a policy's yearly earnings impact, $\Delta_{\tilde{a}}$ and of the counterfactual level of income that individuals would have received in the absence of the policy, $Y^c_{\tilde{a}}$. In some cases, we do not have a direct estimate of the counter-factual earnings level. (This is particularly common when a policy benefits children and we use other causal effects such as educational attainment to project earnings outcomes.) In those instance, we estimate counterfactual incomes using information about parent incomes or parent income ranks. To do that, we use publicly available data on the relationship between child and parent income ranks from Chetty et al. (2018), as well as the income levels corresponding to these ranks.

In cases where we have direct information on parental income rank, we can then estimate a child income rank via the national rank-rank relationship estimated by Chetty et al. (2018). Given a child income rank we can then estimate the implied child income at age 34 in 2014, which we can then adjust to the required age and year of the policy at hand. In order to do so, we assume 0.5% wage growth and assume that an individuals' income in each year is constant as a fraction of the population average for individuals of the same age. This yields us an estimate of counterfactual child income and we then proceed with our projection method as in the simple case above.

In cases where we observe parental income, we must first translate that income level into a parental income rank before using the rank-rank estimates from Chetty et al. (2018). We do this by translating the observed parent income to the average age (42) and year (1997) of parent income measurement in Chetty et al. (2018). Once again we assume 0.5% wage growth and and assume that an individuals' income is constant as a fraction of the population average at the same age over the lifecycle. We assume throughout that trends in income over the lifecycle are constant across generations and hence are the same for parents and children. We then use the crosswalk Chetty et al. (2018) provide between parent household income and household income ranks to get an estimate of parent income rank. This parental income rank is then translated into an estimated child income level and incorporated in our projection method as the counter-factual income level.

Alternative Approach: In order to assess the robustness of our MVPF estimates to the assumptions made in our earnings projections, we estimate our MVPFs using a fixed forecast method. These results are presented in figure VI panel C. This fixed forecast method assumes that after the age at which an income effect is observed this effect is constant through to older ages, instead of following the lifecycle. In order to ensure this methodology is conservative the projection continues to follow mean wages backwards in time from the age at which incomes are observed; if an earnings impact of \$100 is observed at age 26, then we will project \$100 each year forwards, but for ages 18-25 we will estimate lower earnings impacts as mean earnings in the population are lower at these ages.

J Consistency with Social Welfare Function

This Appendix develops a standard intergenerational model to think about the implications of standard welfare theory for the pattern of MVPFs and welfare weights within and across generations. It illustrates how the broad patterns we observe for the MVPFs across generations and income levels are inconsistent with standard social welfare functions. The only rationale for declining MVPFs that are higher for poor children is that society implicitly wishes to redistribute to children who grew up in rich families but have low incomes as adults.

Setup For simplicity there are just two income types of equal size in the population: rich and poor. The probability of growing up to be a rich adult if one's family was rich as a child and the probability of becoming a poor adult if one grew up as a poor child is p which is assumed to be symmetric.

The expected utility of rich and poor children is given by

$$U_r = pu_r + (1-p)\,u_p$$

$$U_p = pu_p + (1-p)\,u_r$$

where we assume utilities have been normalized to be in dollar units of individual willingness to pay. This means that the value of \$1 to a rich adult is valued at $\$r$ to a rich child and $\$(1-r)$ to a poor child. Providing \$1 to a poor adult is valued at \$r to a poor child and $\$(1-r)$ to a rich child.

Social welfare from the perspective of children is given by

$$W^k = \eta^k_r U_r + \eta^k_p U_p$$

and ex-post social welfare from the perspective of adults is given by

$$W^a = \eta^a_{rr} p u_r + (1-p)\,\eta^a_{pr} u_r + \eta^a_{pp} p u_p + (1-p)\,\eta^a_{rp} u_p$$

where η^a_{rr} is the welfare weight on a child who grew up rich and is rich as an adult, η^a_{pr} is the welfare weight of a child who grew up poor and is now rich, η^a_{pp} is the welfare weight of a child who grew up poor and remains poor as an adult, and η^a_{rp} is the welfare weight of a child who grew up rich but is now poor. Note that in a standard welfarist model, your upbringing wouldn't affect your welfare weight conditional on your outcomes.

If individuals are on an unconstrained Euler equation, then the average marginal utility of income should be equated across time. Hence, the individuals are indifferent to whether they get $MVPF as a child versus the same $MVPF as adults. As a result, this would imply that the welfare weight of children equals the average welfare weight of adults from that background:

$$\eta^k_j = p\eta^a_j + (1-p)\,\eta^a_{-j} \text{ for } j = p, r$$

So, in particular, if we take an average across rich and poor children , we have

$$E\left[\eta^k\right] = E\left[\eta^a\right] \tag{12}$$

Where the expectation is taken across the population (in this case, it's just summing across η_j for $j = r, p$). Intuitively, if we respect individual's expected future utility and those expectations are right on average, then the average welfare weights are equated across time within a generation.

Moreover, suppose that the welfare weights are declining with income, so that $\eta^a_p > \eta^a_r$. Then, any weighted average of these weights will be declining in the amount of weight on the rich. In particular,

$$\eta^a_p > \eta^a_r \implies \eta^k_p > \eta^k_r \tag{13}$$

If society values rich more than poor adults and those weights do not depend on parental upbringing, then it must place more weight on rich as opposed to poor children. More generally, the statement is that if mobility is FOSD (the distribution of adult income is increasing in parental income), then the ordering of welfare weights of adults must be preserved for children.

In our case, the combination of equations 12 and 13 create a contradiction. As long as welfare weights for adults are declining in income, then welfare weights for children should be declining in parental income (equation 13). So, this means that the welfare weights of rich children must be higher than the welfare weight of poor children. But, we know that the welfare weight of rich children already exceeds the generally-agreed upon estimate of welfare weights for adults. So, the average welfare weight of children must exceed the average welfare weight of adults. This then contradicts equation 12.

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