Critiquing Bastian (2022, 2023, 2024, and forthcoming): On Child Tax Credit Reform and the Sensitivity of Single Mothers to Work Incentives

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Abstract

In 2021, Congress passed and President Biden signed a major, but temporary, reform to the Child Tax Credit (CTC). Among other reforms to the credit, the American Rescue Plan Act (ARPA) made it available to non-workers on the same basis as workers. Attempts to make this reform permanent foundered, in part, due to opposition from policymakers who worried that the new credit would lead workers to withdraw from the labor force and otherwise discourage work.

These concerns drew strength from a working paper by a team of economists at the University of Chicago, published that fall, that predicted that making the ARPA CTC permanent would lead nearly 1.5 million parents to leave employment, most of them lower-income single mothers. (Corinth et al., 2021/22)

Critics of the paper have focused on the assumptions its authors made about the sensitivity of single mothers' employment decisions to changes in work incentives. Debate has revolved around the magnitude of a labor supply elasticity specifying the percentage change in employment for a given percentage change in the return to work. Supporters of the ARPA CTC have charged that the elasticity assumed by Corinth et al. for low-income single mothers, 0.75, is too large and stands in contrast to the available evidence.

No critic has been more dogged than Jacob Bastian, who, in a series of working papers has used multiple research strategies to arrive at the conclusion that the true elasticity for low-income single mothers is around 0.40. If true, the implication is that several hundred thousand fewer low-income single mothers than predicted by Corinth et al. would leave employment, and therefore child poverty would fall by more than Corinth et al. estimated.

This paper critiques four distinct Bastian critiques of Corinth et al. (Bastian 2022, 2023, 2024, and forthcoming) I find selective citation of the literature, unjustified assumptions, mismeasured concepts, and, most importantly, analytic errors. Strikingly, while Bastian concludes in each case that the Corinth et al. elasticity of 0.75 is much too high, after correcting his problems, all four of the papers reinforce the Corinth et al. elasticity.

At the time of this writing, the evidence we have is entirely consistent with Corinth et al.'s assumption about the sensitivity of single mothers' work decisions to work incentives. I estimate that their paper implies that about 650,000 low-income single mothers would leave employment if the ARPA CTC were made permanent. That does not include other single mothers, single fathers, and married parents that would leave employment. Policymakers must beware of the unintended consequences of reforms to the CTC.

In fall of 2021, near the climax of the debate over President Biden's Build Back Better agenda and its expanded child tax credit (CTC) proposal, Kevin Corinth, Bruce Meyer, Matthew Stadnicki, and Derek Wu released a working paper arguing that the expanded CTC would cause nearly 1.5 million parents to exit the workforce (Corinth et al., 2021, revised 2022). The paper proved highly influential and contributed to the defeat of Build Back Better.

As the debate over an expanded CTC has continued since that fall, economist Jacob Bastian has emerged as the biggest critic of the Corinth et al. paper. In no fewer than four distinct papers, Bastian has argued that the Corinth et al. study is flawed because it assumes that parents' employment decisions are too sensitive to changes in the pay-off to working. But a careful review of the methods in each paper reveals that after correcting a number of computational errors and faulty assumptions, *all four papers reinforce the conclusions of Corinth et al*.

The CTC expansion that inspired this empirical debate was a temporary policy change in 2021 that made the same CTC available for all but upper-income families, regardless of whether they incurred any income tax liability or had earnings. Since this reform removed features of the CTC that encouraged work—including an earnings requirement and a phase-in that, up to a point, increased the size of the credit as a parent received more earnings—the expected result would be that some parents would choose to work less or not to work at all.

There are two reasons for this prediction. First, as parents receive more income, they "spend" some of it on additional leisure. If you can earn the same amount by working fewer hours, you might decide to hold constant your income and work less. Second, the payoff to working more—or working at all—changes when the work-promoting features of the CTC are removed, and so some people will choose to work less or not at all. It is the increased incentive to not work at all due to the lower payoff to working that has been the focus of the debate—the so-called "substitution effect on the extensive margin."

At the heart of the matter is a deceptively simple equation. When a policy changes that affects work decisions, it alters the "return to work"—the payoff to working instead of not working. If workers are sensitive enough to changes in the return to work, they may choose to leave the workforce. Estimating the size of this group involves multiplying three numbers together: the average across workers of the percentage change in their returns to work, the percentage change in employment for a given percentage change in the return to work (the labor supply "elasticity"), and the number of workers in the group.

The first and third numbers may be estimated directly from data. The elasticity is also based on empirical analyses of data, but typically, researchers assume an elasticity based on their read of the labor economics literature. Bastian's work has suffered from issues with all three components of the equation.

In this paper, I focus on the estimates for EITC-eligible single mothers. I do so for three reasons. First, they constitute a large fraction of the population that might be affected by the expansion of the CTC in question. Second, comparing the methods in the Corinth et al. paper and Bastian's papers is more feasible for this group given what the authors provide for methodological detail

and evidence. Third, no one has yet determined a satisfactory way to model the effects of a CTC expansion on married parents, given that they make work decisions jointly.

Paper One: "How Would a Permanent 2021 Child Tax Credit Expansion Affect Poverty and Employment?" Forthcoming, *National Tax Journal*. Previous drafts include earlier versions with that title from 2022 and 2023, "Investigating the Effects of the 2021 Child Tax Credit Expansion on Poverty and Employment" (multiple drafts, 2022) and "Investigating How a Permanent Child Tax Credit Expansion Would Affect Poverty and Employment" (multiple drafts, 2022).

The first draft of this paper—which has evolved into a forthcoming article in *National Tax Journal*—was Bastian's initial attempt to refute Corinth et al. To do so, he used two different datasets (the American Community Survey, or ACS, and the Annual Social and Economic Supplement to the Current Population Survey, or CPS ASEC) to compute estimates for different groups of parents. These included the number of workers in each group and the average percent change in the return to work from expanding the CTC as occurred in 2021. Bastian used 2017 data (as did Corinth et al.), estimating everyone's taxes and CTC amounts under 2020 and 2021 tax law. He assumed a set of labor supply elasticities for each group. By multiplying the three numbers together, Bastian produced estimates of the number of each type of parent who would stop working due to the expanded CTC.

In the final version of the paper, Bastian concludes that 354,000-367,000 parents would stop working under such a policy, including 238,000-281,000 single mothers. Child poverty would fall by 28-30 percent, deep child poverty (having income under half the poverty line) by 43-49 percent.[1]

Looking specifically at low-income single mothers (those receiving the Earned Income Tax Credit, or EITC), Bastian estimated that 5.45-6.8 million workers would experience an average decline in the percent change in the return to work of 9.9 percent.[2] He assumed an elasticity of 0.4, meaning a 10 percent decline in the return to work would reduce the employment rate by roughly 4 percent. Multiplying the three parts of the employment equation, he estimated that 218,000-263,000 EITC-eligible single mothers would stop working under the reformed CTC. This was much lower than the number implied by Corinth et al., which I estimate as 645,000.[3] Bastian's estimates suggest that 62-72 percent of parents who stop working are EITC-eligible single parents, while the Corinth et al. figures indicate 83 percent.[4]

However, we can reconcile the 382,000-427,000 gap in these estimates by fixing problems with each of the three numbers Bastian uses to get his results.

Average percent change in the return to work. While relatively unimportant for his estimates, Bastian uses the wrong formula for the percent change in the return to work. The return to work is the difference between the income people receive if they work and the income they receive if they do not work. We can write this as:

$$(MI_i^W - GT_i^W + EITC_i^W + CTC_i^W + SB_i^W) - (MI_i^{NW} - GT_i^{NW} + EITC_i^{NW} + CTC_i^{NW} + SB_i^{NW}),$$

where *MI* is market income, *GT* is gross taxes (before credits), *EITC* and *CTC* are those tax credits, and *SB* is safety-net benefits. The superscripted *W* and *NW* mean that each of these amounts is either what one receives as a worker or as a non-worker. The subscripted *i* just means each individual *i* has their own return to work.

Note that a worker can receive safety-net benefits, such as food stamps (benefits from the Supplemental Nutrition Assistance Program, or SNAP). Non-workers can have market income and taxes, such as if they are married to someone who works or if they have investment income. For single parents, however, market income and gross taxes if they do not work are likely to be close to $0. Tax credits generally also require employment, but the CTC reform in question would have provided the same CTC regardless of work.

The *change* in the return to work due to an expanded CTC is just the difference between the new return to work after the reform and the old return to work. We can write it as:

$$\begin{aligned}\left(MI_{POST,i}^W - GT_{POST,i}^W + EITC_{POST,i}^W + CTC_{POST,i}^W + SB_{POST,i}^W\right) \\ - \left(MI_{POST,i}^{NW} - GT_{POST,i}^{NW} + EITC_{POST,i}^{NW} + CTC_{POST,i}^{NW} + SB_{POST,i}^{NW}\right) \\ - \left[\left(MI_{PRE,i}^W - GT_{PRE,i}^W + EITC_{PRE,i}^W + CTC_{PRE,i}^W + SB_{PRE,i}^W\right)\right. \\ \left. - \left(MI_{PRE,i}^{NW} - GT_{PRE,i}^{NW} + EITC_{PRE,i}^{NW} + CTC_{PRE,i}^{NW} + SB_{PRE,i}^{NW}\right)\right],\end{aligned}$$

where the terms are differentiated by the "*POST*" and "*PRE*" in the subscripts, indicating amounts from after or before the policy change. If no other policies change except for the CTC expansion to non-workers, this equation simplifies to:

$$CTC_{POST,i}^W - CTC_{POST,i}^{NW} - (CTC_{PRE,i}^W - CTC_{PRE,i}^{NW}) = 0 - \left(CTC_{PRE,i}^W - 0\right) = -CTC_{PRE,i}^W,$$

which is just the negative of the CTC that a worker would have received absent the policy reform.

Finally, the percent change in the return to work is the change divided by the initial (pre-reform) return to work, or:

$$\begin{aligned}-CTC_{PRE,i}^W \Big/ \left[\left(MI_{PRE,i}^W - GT_{PRE,i}^W + EITC_{PRE,i}^W + CTC_{PRE,i}^W + SB_{PRE,i}^W\right)\right. \\ \left. - \left(MI_{PRE,i}^{NW} - GT_{PRE,i}^{NW} + EITC_{PRE,i}^{NW} + CTC_{PRE,i}^{NW} + SB_{PRE,i}^{NW}\right)\right],\end{aligned}$$

which, if a non-worker has no market income, taxes, or tax credits is:

$$-CTC_{PRE,i}^W \Big/ \left[\left(MI_{PRE,i}^W - GT_{PRE,i}^W + EITC_{PRE,i}^W + CTC_{PRE,i}^W\right) - \left(SB_{PRE,i}^{NW} - SB_{PRE,i}^W\right)\right].$$

Bastian estimates what he calls the change in the return to work as the negative of the difference between the average tax rate after the reform and the average tax rate before the reform. In the case of the CTC expansion, this is:

$$-\{[(MI^{W}_{POST,i} - GT^{W}_{POST,i} + EITC^{W}_{POST,i} + CTC^{W}_{POST,i} + SB^{W}_{POST,i}) - (MI^{NW}_{POST,i} - GT^{NW}_{POST,i} + EITC^{NW}_{POST,i} + CTC^{NW}_{POST,i} + SB^{NW}_{POST,i})] / MI^{W}_{POST,i} - [(MI^{W}_{PRE,i} - GT^{W}_{PRE,i} + EITC^{W}_{PRE,i} + CTC^{W}_{PRE,i} + SB^{W}_{PRE,i}) - (MI^{NW}_{PRE,i} - GT^{NW}_{PRE,i} + EITC^{NW}_{PRE,i} + CTC^{NW}_{PRE,i} + SB^{NW}_{PRE,i})] / MI^{W}_{PRE,i}\}.$$

If nothing else changes, this simplifies to

$$-CTC^{W}_{PRE,i} \,/\, MI^{W}_{PRE,i}$$

This formula is incorrect, however, because rather than the denominator consisting of the pre-reform return to work, it is just pretax market income if one works (which is the same before and after the reform). My own analyses of this issue suggest that this is a relatively minor problem compared with the problems in the other two parts of the employment-change equation.[5]

Unfortunately, there is not enough information in the Corinth et al. paper to determine the average percent change in return to work values they used. But it appears that any difference between their estimates and Bastian's plays only a small role in their different results, given that the factors to which I now turn play such a large role.

Elasticity. Bastian uses an elasticity of 0.4 for EITC-eligible single mothers, while Corinth et al. use 0.75. All parties in the debate agree that a major explanation for the difference between Bastian's results and those of Corinth et al. involves the elasticities they use. Bastian notes that for "lower-income unmarried mothers" "[r]esearch on the EITC typically finds elasticities of around 0.6-0.7, with even higher estimates for younger and lower-education subgroups." (p. 10) But he then says that "there is debate about whether some of these higher elasticities are too large." He says that Chetty et al. (2013) "found smaller elasticities of 0.30-0.43" for single mothers. He cites estimates by Hoynes and Patel (2018) and by Bastian and Jones (2021) that are between 0.30 and 0.40. And he cites Bishop et al. (2005) as showing that elasticities for single women have fallen over time.

As I discussed in an earlier working paper (Winship, 2022), this evidence cannot justify using an elasticity of 0.40. For one, the high end of the range in Chetty et al. (0.43, from Meyer and Rosenbaum, 2001) has subsequently been corrected by Corinth et al. (which includes Meyer as a coauthor). They put the elasticity not at 0.43 but at 0.67—back in that "around 0.6-0.7" range with which Bastian begins and close to Corinth et al.

Corinth et al. also showed that Hoynes and Patel underestimated their own elasticity. Hoynes and Patel have since updated it (Hoynes and Patel, 2022), but Corinth and Meyer (2022) show that the revision also underestimates their elasticity. Corinth and Meyer put it at 1.0 to 1.4.

As for Bastian and Jones (2021), I showed in my working paper (Winship, 2022) that this estimate is for EITC-eligible women (including married mothers and non-mothers), not EITC-eligible single mothers. The authors provide the information needed to calculate the elasticity for EITC-eligible single mothers in the same way they calculate their 0.30-0.40 estimates, and it turns out to be around 1.5. Bastian has another paper in which he reports an elasticity of 0.58. As

we will see, his papers over the past two years have also tended to reinforce the Corinth et al. choice of 0.75.

My working paper also discussed why the Bishop et al. (2005) study is weak evidence that the elasticities of poor single mothers have declined. Among other reasons, I note that "while the most recent estimate in the study, from 2003, is significantly lower than those from the early 1990s, it is no lower than those from the second half of that decade….If elasticities are as high today as in the late 1990s, it would be difficult to argue that the 1990s EITC research is out of date." (p. 6)

In a recent note (Corinth and Winship, 2024), Corinth and I present the evidence on labor supply elasticities for single mothers. We show that whether one looks at reviews assessing the literature or at individual studies with original elasticity estimates, the average elasticity is somewhere around 0.80 to 0.85, and elasticities higher than 0.75 are very common. Only seven of 18 studies we reviewed were consistent with an elasticity as low as 0.4, while 10 of 18 were consistent with 0.75.

Perhaps for these reasons, Bastian has added a new rationale for using a lower elasticity in his most recent draft—the sensitivity of employment decisions to changes in the return to work might be asymmetric. Specifically, the sensitivity might be greater when a tax change incentivizes work than when it disincentivizes work.

He writes, "If the 2020 CTC was eliminated and not replaced with anything, this would lower the return to work and also lead to parents no longer being able to afford these fixed work costs, likely lowering employment…" analogously to the way an EITC expansion would increase employment. In contrast, Bastian says, "the 2020-to-2021 CTC change replaced the 2020 CTC with more benefits, which were not contingent on working. The 2021 CTC ensured that childcare and transportation would remain affordable for working parents."

It's worth calling this argument what it is: a giant red herring that should not be treated seriously. It's clearly a line of thinking that would not come up if Bastian's case were otherwise stronger, nor if it led to the opposite conclusion.

To be clear, fixed work costs are *part of* a person's calculation of the return to work (the pay-off to working rather than not working). If people would choose not to work in the event the CTC were eliminated, that would be because the return to work would have changed such that it would be no longer worth it to them to work. The fixed costs of working would be part of that calculation, but the fixed costs of working would have always been there. It was worth it for them to work before an elimination of the CTC despite those fixed costs; the elimination makes it no longer worth working. A reduction of the CTC reduces work, in part, because the fixed costs of working become "less affordable."

All Bastian is saying when he says that the 2021 CTC expansion ensured the fixed costs of working "remain affordable" for workers is that more money makes it easier for workers to keep working. But more money for non-workers makes it easier for workers to stop working too. The return to work declines if the CTC becomes available regardless of employment, because while

working becomes more “affordable,” not working becomes equally more “affordable.” That’s true regardless of whether one considers the fixed costs of working.

Finally, Bastian says that an expanded CTC can increase employment through an income effect. For instance, more income might lead people to work more because they can purchase more childcare. That’s certainly possible, but it’s an argument about the size of the income effect. It should not be smuggled into debates about the substitution effect. (And at any rate, as Bastian notes, income effects are generally regarded as being small.)

None of this has anything to do with elasticities, which simply give (1) the percent change in employment for (2) a given percent change in the return to work. The denominator in (2) is (2a) the change in the return to work divided by (2b) the pre-policy return to work. The percent change in the return to work in (2) will only be affected by fixed costs (so long as they don't change) insofar as a given change in the return to work caused by a policy (2a) will be a larger percentage of the pre-policy return to work (2b) if that pre-policy return to work incorporates the fixed costs of working than if it doesn't.

Now, as it happens, most of the research from which elasticities are computed do *not* incorporate the fixed costs of working. In principle, it would be better if they did. But it probably wouldn't change our conclusion about the effect of a child allowance.

As already noted, when one estimates the number of people who will stop working due to a policy change, one multiplies (1) the number of initially working people by (2) the average percent change in the return to work and then by (3) the elasticity. Say the elasticity has been estimated such that the percent change in the return to work in its denominator doesn't incorporate fixed costs. In that case, the average percent change in the return to work in (2) should also exclude fixed costs, else the corresponding terms will not cancel out correctly. The return to work in (2) has to be comparable to the return to work in the denominator of (3). For the same reason, if the return to work in the denominator of (3) incorporates fixed costs, then the return to work in (2) also has to do so. But whether fixed costs of working are or are not incorporated, the two terms in (2) and (3) would cancel out similarly, so long as they are computed consistently.

When Bastian uses 0.75 as an elasticity, he finds that 409,000-494,000 EITC-eligible single mothers would stop working after the CTC expansion.[6] That closes 45-60 percent of the gap with Corinth et al.

The upshot for Bastian’s results are not just that he underestimates how many parents would stop working in the presence of the 2021 CTC expansion. He also overestimates how much poverty would be reduced. Figure 8 of his paper shows the child poverty reduction for a given elasticity for EITC-eligible single mothers. The share of EITC-eligible single mothers who he predicts would stop working is low by 59 percent, compared with Corinth et al., implying that the product of the number of working EITC-eligible single mothers, their average change in the return to work, and the 0.75 elasticity should be multiplied by (1/.41=) 2.45. Correspondingly, we can just multiply the elasticity of 0.75 by 2.45 and see what kind of poverty reduction is implied by

Figure 8. The corresponding estimate is around 20-21 percent, rather than the 28 percent reduction he reports. Corinth et al. estimate a reduction of child poverty of 22 percent.[7]

Number of workers. Bastian estimates there are 545,000 EITC-eligible working single mothers.[8] In my earlier critique, I argued that an important difference between the Bastian and Corinth et al. results was that the latter use a dataset that links the ASEC to administrative tax and benefit data. By incorporating administrative data, Corinth et al. overcome large underreporting and undercount problems in the ASEC. The ASEC counts too few EITC-eligible single mothers relative to tax data.

In a paper utilizing Internal Revenue Service data, Maggie Jones and James Ziliak (2020, 2022) compare the number of EITC recipients in the tax data to the number identified by running the ASEC data through the same tax simulation tool that Bastian uses. They find that among head of household filers (single parents), the simulated ASEC EITC recipients in 2016 amount to just 56 percent of the recipients in the tax data.[9]

If the 5.45 million EITC-eligible working single mothers that Bastian finds in the ASEC are multiplied by 1/0.56, the result is 9.7 million workers. For comparison, if I back into the number of working EITC-eligible single mothers in the Corinth et al. study, by dividing the 645,000 mothers who stop working by the product of the 0.75 elasticity and Bastian's estimate of the average percent change in return to work, I find 8.7 million. (This may not be correct because Bastian's return to work estimates may not be correct, but the point is that the estimate of working EITC-eligible single mothers is much higher than Bastian's.) Using published IRS figures and the 2017 CPS ASEC, I estimate the number to be around 10.5 million for 2016.[10] It seems clear that Bastian's lack of access to administrative data means that he counts too few working EITC-eligible single mothers.

Using 9 million workers instead of the 5.45 million in the ASEC data, the number who would stop working increases from 218,000 to 356,000. The gap between Bastian's estimates and those of Corinth et al. closes by 32 percent. If we also use 0.75 for the elasticity, we get 668,000—more than Corinth et al.'s 645,000. Of note, using an elasticity of 1.1 puts the number at over one million.

The conclusion, then, is that the entire difference between Bastian's results and those of Corinth et al. can be attributed to two shortcomings of Bastian's analysis—his much-too-low elasticity, and the much-too-large undercount of working EITC-eligible single mothers in his survey data. Bastian significantly overstates (by about one-third) how much an expansion of the child tax credit would reduce child poverty. Though he sets his paper up as a refutation of Corinth et al. (2022), after correctly interpreting the literature and assessing the evidence, Bastian reinforces their conclusions. Were the 2021 child tax credit made permanent, a large number of single mothers would likely stop working, and while child poverty would decline in the short-term, it would be by much less than Bastian suggests, and the long-run effects of reduced parental employment might be considerable.

Paper Two: "Predicting the Employment Effects of a Permanent 2020-to-2021 CTC Change: Comparing Bastian (2022), Corinth, Meyer, Stadnicki, and Wu (2021), and Corinth and Meyer (2021)." Draft of November 23, 2022. Incorporated into "How Would a Permanent 2021 Child Tax Credit Expansion Affect Poverty and Employment?" (draft of December 22, 2022) as Appendix B.

In this paper, Bastian takes four distinct approaches in trying to discredit Corinth et al. I critiqued these in my 2022 working paper, and so I concisely summarize the problems with the paper here (Winship, 2022). In the first approach, Bastian tries to decompose the gap between his and Corinth et al.'s estimates of the number of parents who, due to the substitution effect described above, would stop working if the 2021 CTC reform were made permanent. Bastian put this number at 358,000 in his December 2022 draft using the CPS ASEC and the methods from his first paper. Corinth et al. put it at 1.32 million—a gap of over 960,000.

However, Bastian understated the share of this gap that was due to the different number of EITC-eligible single mothers who would stop working. He mistakenly attributed 200,000 of the gap to an error related to married parents that he accused Corinth et al. of making but which they did not make. That led him to understate the extent to which the gap was due to EITC-eligible single mothers. Instead of that single-mother gap being the 200,000-300,000 that he estimated, the real gap was about 427,000 (645,000 versus 218,000).

As we have seen above, this gap is entirely explained by the different elasticities used and the smaller number of EITC-eligible single mothers captured in the ASEC as compared with the ASEC linked to administrative data. According to Bastian, the 533,000-worker gap between the two studies in the number of parents *other than* EITC-eligible single mothers predicted to stop working is about equally due to elasticity differences and to Bastian's exclusion of parents with over $80,000 in earnings. In truth, a sizable share of the gap must be due to the significant differences in the way that the two studies model decisions by married parents to stop working, but there is no room in Bastian's accounting for this.

In my earlier critique, I defended Corinth et al.'s choice of elasticities (including those for married parents) and I defended their choice to include parents earning more than $80,000. (Meyer and Corinth have said publicly that even dropping all parents making $50,000 or more would still result in 1.1 million parents choosing not to work.) I suspect that Corinth et al.'s modeling of married parents' decisions overstates how many would stop working. But the impact can't be sizable, because Bastian's own accounting indicates that if his estimates for EITC-eligible single mothers were corrected, the other differences between their analyses would explain the two papers' different results without considering their very different ways of modeling married parents' decisions.

For Bastian's second critique of Corinth et al., he directs his attention to analyses conducted by Corinth et al. as a check on their main results. These analyses started in a separate research note but are now in an appendix of Corinth et al. In those analyses, the authors mimic the methods of another study looking at the EITC's effects on work, conducted by a National Academy of Sciences panel on child poverty (National Academies of Sciences, Engineering, and Medicine, 2019). Corinth et al. estimate that 1.16 million single mothers (including those not eligible for

the EITC) would stop working if the CTC were expanded. These are not estimates that Corinth et al. emphasize—they are only a check using the (related) methods of others.

Bastian tries to show that the 1.16 million estimate implies an implausibly large elasticity. He does so by estimating from his CPS ASEC data the number of single mothers (including those not eligible for the EITC) who would stop working, using an estimate of the average percent change in the return to work that applies to all single mothers and the Corinth et al. elasticity of 0.75. He gets an estimate of 520,000 single mothers who would stop working. If a 0.75 elasticity produces an estimate of 520,000, then the elasticity that would product an estimate of 1.16 million would be (1.16M/520K*0.75=) 1.7.

But as we have seen, Bastian's ASEC data understate the number of working EITC-eligible single mothers. The Corinth et al. results, when combined with Bastian's return-to-work estimates, suggest the true number is something like 8.7 million rather than the 5.45 million Bastian finds. If these additional workers constituted workers entirely missed in the ASEC (either not counted or counted as non-workers, as opposed to being counted as workers not eligible for the EITC) , then we could add 3.25 million workers to the 8.7 million working single mothers in the ASEC (not just those eligible for the EITC). Then we can re-do Bastian's analysis. The result is an elasticity of 1.21. This is higher than 0.75, but hardly implausible; studies have found higher elasticities for EITC-eligible single mothers (including at least one of Bastian's papers).

Bastian's third attempt to discredit Corinth et al. criticizes the methodology that Corinth et al. are mimicking in their check of their main results. The methodology, instead of assuming that a given percent change in the return to work will lead to the same percent change in employment, specifies that a $1,000 change in the return to work will lead to the same *percentage point* change in the employment *rate*. Bastian says that this methodology produces implausible elasticities for higher-income single mothers, because a change in the employment rate when the return to work changes by $1,000 will be lower when earnings are high. Of course this is true, but it's not relevant. If looking at higher-income single mothers, one should not use an absolute change in the return to work that applies to lower-income single mothers. But Corinth et al. are not doing this, and at any rate, they are not advocating the method, just mimicking it as a check against their own results.

Bastian levels one final critique against Corinth et al. He estimates the average percent change in the return to work among EITC-eligible single mothers caused by the 1990s EITC expansion. He finds that it is similar to the average percent change in the return to work that he estimates for the CTC expansion. This similarity leads him to argue that the CTC expansion would reduce the employment rate among EITC-eligible single mothers by the same number of percentage points as the EITC expansion increased it. This four-point decline translates into a disemployment effect of 400,000, which Bastian views as a vindication, since that is much closer to his preferred estimate than to the 1.16 million Corinth et al. obtain in the check of their main results.

In my earlier critique of Bastian, I showed that his calculations are flimsy. Estimating the average percent change in the return to work from the EITC expansion using slightly different years than Bastian, the result was about half the size. Because the average percent change in the

return to work from expanding the CTC would then be twice the average change from expanding the EITC, the effect of expanding the CTC on the employment rate would also be twice as large. Applied to his own figures, the result was a predicted employment loss of 1.13 million, which is very close to Corinth et al.'s 1.16 million.

Bastian, then, in this second paper provides four critiques intended to discredit the Corinth et al. estimates. One is irrelevant, and the other three, upon closer look, reinforce their analyses.

Paper Three: "What if the EITC Was Eliminated? Projecting the Labor Supply Effects of Ending the 2020 CTC and EITC Based on the Estimates in Corinth et al. (2021) and Corinth and Meyer (2021)." Draft of January 16, 2023.

Next, Bastian took a different tack. The 0.75 elasticity can't be reasonable, he argued in early 2023. If you assume EITC-eligible single mothers' work decisions are that sensitive to changes in the return to work, you get implausible results looking at real-world counterfactuals. The paper amounts to arguing that 30 years of research on the sensitivity of work decisions to incentives—a body of work to which Bastian has made key contributions—must be deeply flawed, so we cannot trust empirical analyses that cast CTC expansion in a poor light.

Bastian invites the reader to imagine a world in which the EITC and CTC were entirely eliminated. To simulate the effect on employment, he needs the three basic ingredients—an elasticity, an average percent change in the return to work, and the number of working EITC-eligible single mothers. He assumes a 0.75 elasticity (which he's trying to discredit). He has estimates of the average percent change in the return to work created by the CTC (9.9 percent) and the EITC (24.7 percent) from his CPS ASEC data. And he can also get the number of working EITC-eligible single mothers from that source. (Bastian also calculates estimates for single women not eligible for the EITC, using different return-to-work and elasticity values.)

However, rather than use the number of *working* EITC-eligible single mothers, he mistakenly plugs in the number of *all* EITC-eligible single mothers. So when he multiplies that by the average percent change in the return to work and the labor supply elasticity, the result overstates the number of single mothers who would stop working in his thought experiment. (Specifically, it overstates the number by the ratio of single mothers to employed single mothers, or the reciprocal of the single-mother employment rate.)

This is clearly a mistake, as it runs counter to his methods in the forthcoming *National Tax Journal* paper already discussed. In an earlier critique, I show what Bastian's analyses indicate when this mistake is corrected (Winship, 2024a). Rather than the 22 percent reduction in employed single mothers that Bastian reports, using the proper formula, the reduction from eliminating the EITC and CTC is 15 percent. Bastian overstates the impact by nearly 50 percent.

Bastian wants to argue that his results are implausible, which means the Corinth et al. elasticities are implausible. He says that the reduction in employment he estimates would put the single-mother employment rate at "a level not seen since 1950." But using the correct formula, I estimated that employment would drop to early 1980s levels—a period predating the first major expansion of the EITC and coming long before the CTC existed. In other words, eliminating the

EITC and CTC would put the employment of single mothers back where it was when the EITC was insubstantial and the CTC did not exist. That seems…consistent with the Corinth et al. elasticities.

Ironically, Bastian reinforces this conclusion later in the paper. He cites the research of Schanzenbach and Strain (2021) to argue that the "true" effect of eliminating just the EITC would be a 9.0-percentage-point reduction in the single mother employment rate. After correcting Bastian's error, his own analysis implies a 9.5-point reduction (rather than the 15.6-point reduction he estimates).

In this paper, then, Bastian manages to reinforce the findings of Corinth et al., to strengthen the case for the elasticities they used, to demonstrate that single mothers are highly sensitive to changes in the return to work, and that pro-work safety-net reforms have increased the employment of single mothers substantially.

Paper Four: "Research Note on Tax Credits, Poverty, and Elasticities." Draft of February 20, 2024. Previous draft: "Research Note on Elasticities, Work Incentives, and Recent Child Tax Credit Proposals," February 6, 2024.

Fast-forward a year, and Bastian made attempt number four to refute Corinth et al. Whereas in the previous paper, Bastian started with the Corinth et al. elasticities and then tried to show that using them produced unrealistic employment declines, in this paper, he looks at past changes in the return to work and employment declines and backs into the corresponding elasticities. His initial draft concluded that "unmarried mothers' employment elasticities are around 0.3-0.4, on average" and that "[e]lasticities near 0 and above 0.7 can occasionally be found, but these are outliers." (p. 10)

In a critique of that draft (Winship, 2024b), I showed that Bastian had more calculation errors, which I summarize here.

Recall once more that the key calculation of the number of single mothers who will stop working due to a policy change, (1), involves multiplying three figures together—(2) the number of working single mothers, (3) the average percent change in the return to work, and (4) an elasticity. To get the elasticity, (4), one can divide the number of single mothers who stop working, (1), by the number of working single mothers, (2), then divide again by the average percent change in the return to work caused by the policy change, (3). That is, one can divide the *share* of working single mothers who stop working by the average percent change in the return to work. This is what Bastian does, but he has problems in both the numerator and denominator.

To compute the numerator, the share of working single mothers who stop working, he uses estimates from the literature of the effect of specific tax policy changes on single-mother employment rates, as well as calculating the change in employment rates from his own data. But this effectively approximates the number of single mothers who stop working as a share of *all* single mothers. The employment change should be the number of single mothers who stop working as a share of *initially working* single mothers.

For the denominator, the average percent change in the return to work, Bastian first calculates the mean across working single mothers of their average tax rate, for every year from 1970 to 2019. He looks at several episodes of tax policy change and estimates the change in the mean average tax rate over the period after a policy change. He calls the negative of this change in average tax rates the change in the return to work.[11]

But Bastian's calculation here is wrong for the same reason discussed above in the critique of his *National Tax Journal* article—he is effectively expressing the average change in the return to work as a fraction of pre-tax earnings rather than as a fraction of the pre-policy-change return to work. Of the two problems, this one is less consequential than the numerator problem.

Fortunately, one can approximate the elasticities Bastian calculates after fixing his mistakes, as I discuss in my original critique. Essentially, his estimates can be adjusted by creating a bias factor reflecting his errors. I eyeballed his charts to obtain pre-policy-change estimates of the single mother employment rate and the average tax rate, and applied the bias factor. After doing so, the average across eight elasticity estimates was 0.77. After excluding the highest and lowest of the eight estimates, the range was from 0.58 to 1.10, and the mean was 0.80.[12]

Once again, Bastian's attempt to refute Corinth et al., upon correction, reinforced that an elasticity of 0.75 is entirely appropriate for EITC-eligible single mothers.

In response, Bastian released an updated draft two weeks later. This draft corrected the more important of his two errors, using the number of single mothers who stop working as a share of *initially working* single mothers (instead of as a share of all single mothers). He also modified some of the estimated employment effects he drew from the literature. Finally, he incorporated income available to non-workers in estimating the average percent change in the return to work.

Miraculously, Bastian arrived once again at results reinforcing his preferred elasticity estimate, concluding that, "the mean and median elasticity found here is 0.33, with the 25th to 75th percentile spanning 0.23 and 0.41. Elasticities near 0 and above 0.6 can occasionally be found, but these are outliers. I conclude that unmarried mothers' employment elasticities are 0.3-0.4, on average."

However, this attempt falls flat too. To reiterate, what Bastian is doing is dividing the number of single mothers who stop working as a share of initially working single mothers by the average percent change in the return to work in order to obtain elasticity estimates. He does this for four different policy episodes, and for each of those he provides one or more results, using employment change estimates from the literature. (He relies solely on these estimates that he interprets from the literature rather than also showing estimates based on his CPS-derived employment changes, as he did in the first draft.)

Once again, his denominator in these calculations is not actually the average percent change in the return to work. It is the negative of the change in average tax rates, which as we've discussed, is not the same thing.

Moreover, to estimate the change in average tax rates, he needs to define an interval during each policy-change episode over which to compute the difference. Bastian uses multiyear averages as his baselines: 1974-75, 1984-86, 1990-93, and 2005-09. He also uses multiyear averages as his endpoints: 1975-82, 1987-90, 1999-2000, and 2010-13.

These intervals are fairly arbitrary. But the bigger problem is that Bastian is dividing percent changes in employment (from estimates from the literature) due to *reforms of the EITC* by changes in the average tax rate due to *all tax policy and some transfers*.

To see how big a problem this is, I replicated his analyses, but dividing percent changes in employment due to the EITC (Bastian's numerators) by changes in the average tax rate *due to the change in the EITC*. I first confirmed I could replicate, over each of the four policy episodes he examines, the change in average tax rates due to all tax policy changes. He reports these amounts, and they can be obtained from his Figure 2.[13] Having concluded that I could replicate these reported amounts, I then proceeded to calculate the change in average tax rates due to changes in the EITC over these four periods.

Using the same numerators as Bastian but the correct denominators, I found that across seven elasticities I could compute with the data available, the mean was 1.00. Dropping the minimum and maximum, the range was 0.47 to 1.32, and the mean was 0.84.

For a fourth and (final?) time, Bastian has inadvertently reinforced the Corinth et al. results.

Conclusion

The coming debate over CTC reforms that will be revived by the expiration of the Tax Cuts and Jobs Act at the end of 2025 must be informed by the best evidence. Since the influential paper by Corinth et al. was released in fall of 2021, the leading critic of the paper—nearly the only critic offering empirical rejoinders to it—has produced a series of analyses that, when critiqued themselves, reinforce the conclusion that single mothers are as sensitive to changes in work incentives as Corinth et al. assumed.

Since that has been the focus of opponents' criticisms, overwhelmingly, the correct conclusion to draw, based on the state of knowledge today, is that a child allowance would reduce the employment of single mothers by many hundreds of thousands—much more than Bastian finds. My best estimate of the effect, having closely read the Corinth et al. paper, is that about 650,000 EITC-eligible single mothers would stop working if the CTC were permanently reformed as was temporarily done in 2021. Incorporating a higher elasticity and adding in single mothers not eligible for the EITC, it is entirely possible that number could surpass one million. Adding in single fathers and married parents who would stop working would raise that number further.

There may be other aspects to the Corinth et al. paper that are contestable. The one that has been criticized most—the key one in the debate—does not appear so.

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Notes

[1] All from Tables 2 and 3.

[2] The numbers of working EITC-eligible single mothers are shown in Table 2 and Table A1 of his draft of June 12, 2022. One can also back into the numbers from the final draft by dividing the number of poor working EITC-eligible single mothers assuming 2020 CTC policy (column 4 of Tables 2 and 3 in the final draft) by the poverty rate for these women (column 7). The average change in the return to work for the ACS is given on page 18 of the final draft, and it is also given as 9.9 percent in the CPS ASEC on page 1 of "What if the EITC Was Eliminated?" (2023). The CPS estimate can also be calculated by rounding 5.45 million working EITC-eligible single mothers to 5.5 million, then dividing the 218,000 predicted to stop working (in Table 3) by the product of 5.5 million workers and the assumed 0.4 elasticity.

[3] Table A8 in Corinth et al. indicates that 920,000 parents would stop working if the authors applied an elasticity of 0.05 to everyone besides EITC-eligible single mothers. Part of this is due to an income effect of 140,000, so the substitution effect is 780,000. Dropping the elasticity for "everyone else" to zero would lower the substitution effect further, so the number of EITC-eligible single mothers who would stop working with an elasticity of 0.75 would be less than that.

I used the estimates in Table A8 to back into a figure for the number of disemployed EITC-eligible single mothers. To do so, I first estimated the product of the number of EITC-eligible single mothers and their average change in the return to work (the first two parts of the three-part equation to get to the number of disemployed). Call this product *x*.

For each cell in Table A8, the total number of parents who would stop working due to the substitution effect equals *x*(elasticity for EITC-eligible single mothers) + (# other parents)(ave change in RTW among other parents)(elasticity for other parents). For each cell, I plug in the elasticities from the row and column margins of the table, the income effect given in the table note, and the total on the left-hand side of the equation from the estimate given in the table cell. Putting in bold the cell-specific quantities from the table, we get *x*= [**total number of parents who would stop working - change in emp due to inc effect** - (# other parents)(ave change in RTW among other parents)(**elasticity for other parents**)] / (**elasticity for EITC-eligible single mothers**). There are nine cells in the table. For each pair, I can set the right-hand side of this equation for each cell equal to one another, solving for the product, (# other parents)(ave change in RTW among other parents), which does not vary by cell. That product can then be plugged into the equation for *x* in each cell to get an estimate of *x*.

I averaged a number of the resulting estimates for *x*, which came to 0.86. Finally, I multiplied this number by Corinth et al.'s assumed elasticity of 0.75, which produces an estimate of 645,000 EITC-eligible single mothers who would stop working due to the substitution effect.

[4] If 645,000 EITC-eligible single mothers would stop working due to the substitution effect, that leaves 135,000 other parents who would also do so. (See the previous note.)

[5] To look at the potential impact on Bastian's estimates, I used the 2019 ASEC, which includes data on income and taxes for 2018. Tax law in 2018 was, for our purposes, the same as it was before the 2021 CTC reform. I computed the percent change in the return to work for EITC-eligible single mothers using earnings in place of market income. Like Bastian, I included in safety net benefits only those from SNAP. I assumed that if someone who worked did not work, they would receive SNAP benefits equal to the average among EITC-eligible single mothers who didn't work. I used the tax variables in the ASEC. I found that the average percent change in the return to work was -8.6 percent using Bastian's formula, versus -9.0 percent using the correct one. Using the incorrect formula would leave Bastian's estimates of reduced employment too low, but only by 4.5 percent.

[6] Tables 6 and A3.

[7] As for the deep poverty estimates, measurement error in the CPS ASEC is so bad that the survey cannot provide reliable estimates. See Corinth, Meyer, and Wu (2022).

[8] Shown in Table A1 of his draft of June 12, 2022. One can also back into it by dividing the number of poor working EITC-eligible single mothers assuming 2020 CTC policy (column 4 of Table 3 in the final draft) by the poverty rate for these women (column 7).

[9] See Table 4 of the 2020 working paper.

[10] For tax year 2016, 27 million tax returns were filed claiming an EITC. Of these, 48 percent had a head of household filing status (typically a single parent). (See https://web.archive.org/web/20190110065241/https://www.eitc.irs.gov/eitc-central/eitc-information-for-press/statistics/statistical-sample-wording/statistical-sample.) I estimate from the 2017 CPS ASEC that 81 percent of

single parents who worked and received an EITC were women. The IRS number will be somewhat too high if some claimants were ineligible to receive the EITC.

[11] Ideally, we would have the mean change in the average tax rate, rather than the change in the mean average tax rate. Also note that these estimates are for single mothers who are observed to be working in the data. The mean average tax rates might be different if they included non-working single mothers or were based solely on them.

[12] In my critique, I used what I took to be the most defensible baseline years for each policy episode in estimating the baseline single mother employment rate and the baseline average tax rate, two ingredients in my bias correction. I should have used the same baseline years as Bastian. However, having repeated the analyses using his years, the results are not meaningfully affected. The mean across the eight elasticity estimates is 0.78 instead of the 0.77 I reported. Dropping the minimum and maximum, the range is 0.58 to 1.13 instead of 0.58 to 1.10, and the mean is 0.81 instead of 0.80.

[13] I used WebPlotDigitizer 4.8, which extracts data points from images of charts after the user provides anchoring points for the x- and y-axes. See https://automeris.io/WebPlotDigitizer/. The application provides very precise estimates, under the assumption that the chart visually reflects the data points accurately.