Report on "The Earned Income Tax Credit and Infant Health Revisited"
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Thank you for the opportunity to respond to the paper by Dench and Joyce. In the interest of full disclosure, we served as reviewers on an earlier version of this paper submitted to AEJ Policy (where it was rejected). The earlier version of the paper was submitted as a comment on our paper published at AEJ Policy in 2015 ("Income, The Earned Income Tax Credit and Infant Health," American Economic Journal: Economic Policy. 7(1): 172–211, February 2015). The current paper is a modest revision of that earlier paper, it has dropped the "A Comment" from the title though basically still is a comment on our published paper (the title of the earlier version of the paper was "Income, the Earned Income Tax Credit and Infant Health: A Comment"). Thus, here we respond to their paper as we did to the earlier comment, with updates to reflect the current draft.

In our report we refer to the current manuscript as DJ and we refer to our 2015 AEJ Policy paper as HMS.

We summarize the paper under consideration as making 4 main points:

1. Comparing 1st births to 2nd births for OBRA 1993 difference-in-difference and event study analysis is problematic, because the earlier 1990 EITC expansion for 2nd births phased in over the "pre-period" for the 1993 expansion.
2. Comparing 2nd births to 3rd and higher births for OBRA 1993 difference-in-difference and event study analysis is problematic because a placebo test fails (4+parity vs. parity 3, for African American mothers).
3. There is sensitivity to adding quadratic trends by parity to panel fixed effects model using multiple expansions
4. The associations between the EITC, parity, and birthweight are confounded by the crack-cocaine epidemic

Below we address these three main comments followed by our response to other comments.

<u>Response to DJ Main Comment 1 (DJ Section II): Comparing 1st births to 2nd births for OBRA 1993 difference-in-difference and event study analysis is problematic.</u>

HMS use variation in the federal EITC to estimate the effects of the credit on infant health (low birth weight). They use two identification strategies. The main approach is to leverage the variation from the 1993 expansion of the federal credit, which varied by parity (of birth) and tax year. HMS use difference-in-difference and event study models – in both models forming treatment and control groups using parity of birth (e.g. compare parity 2+ to parity 1).

A central criticism in DJ is that HMS use a pre-period of 1991-1993 to estimate the OBRA 1993 difference-in-differences and event study models.

Why did HMS choose this time period: (1) Policy Changes -- Our choice of 1991-1993 for the pre-period was a deliberate, pre-determined choice, based on the timing of the tax change(s) and following the practice in the existing literature.

First, as HMS show in Figure 1, OBRA93 is the largest expansion of the EITC and also the only one where the policy expands differentially across three parity groups (1, 2, 3+ corresponding to childless, one child, and two or more children). Additionally, HMS Figure 1 shows that the OBRA93 expansion took place between 1993 and 1994 for parity 1 and parity 2. For parity 3 and higher, the tax change was phased in between 1994 to 1996. HMS Figure 1 also shows a previous tax change (OBRA90) that expanded the credit for parity 2 or more (childless filers, those of parity 1, have no EITC until 1994). It is because of this previous tax change that HMS use 1991-1993 as the pre-period for analyzing OBRA93 (rather than 1990-1993).

As HMS show, the vast majority of this increase from the OBRA90 change impacting parity 2 occurred by 1991. This is a key factor motivating HMS's decision to begin their pre-period in 1991, rather than earlier.

This is seen in the first panel of Response Table 1 which shows the difference (relative to 1993) in the *maximum credit* for each family size (parity) and the second panel of the table shows the difference-in-difference (relative to 1993, relative to parity 1). We also show the difference-in-difference in Response Figure 1.

Looking at the first panel (difference relative to 1993), the table shows that the gain in the maximum credit for those with 2 or more children rose by $199 between 1991 and 1993. However, between 1990 and 1991 it rose by $300. There is a similar pattern for those with 1 child. It is this large increase between 1990 and 1991 that led HMS to set the pre-period where they did.

Notably, Response Table 1 and Response Figure 1 also show that the OBRA90 expansions were quite similar for parity 2 and parity 3+ while the 1993 expansion was much larger for parity 3+. This illustrates why HMS present extensions of the "DD3" specification (the comparison of parity 3+ to parity 2) and use a pre-period back to 1987 (HMS Fig 3b).

What are the HMS treatments and how do they relate to the existing literature? Based on the tax expansions presented above, HMS present three different treatments. "DD1" compares births of parity 2 and higher to births of parity 1. "DD2" estimates a pooled model with simultaneous comparisons of parity 2 to parity 1 and of parity 3+ to parity 1. "DD3" drops first births and compares parity 3+ to parity 2. DJ's section II critique focuses on comparing *parity 2 to parity 1*, which is not the basis of any single HMS model. By focusing only on comparisons of 2 vs. 1 rather than 2+ vs 1 as in HMS DD1, the 1993 expansion does not appear to be as stark of an increase relative to the 1990 expansion. However, if DJ had started with the DD1 treatment, as was originally done in HMS, then the decision to start with 1990 is more obvious.

There is a large literature examining the effects of the EITC on employment of single mothers. At several points DJ characterizes that literature as focusing on the 1993 expansion for women with 2+ children relative to women with 1 child: "*The extensive literature on the employment effects of the EITC have used the large increases in the available tax credits to women of parity 3+ relative to women of parity 2*" (DJ p. 8) and "*Most research on the EITC has focused on this expansion and with good reason*" (DJ p. 2). This is incorrect. The existing literature almost always follows the pattern shown in HMS. Typically, one starts with the comparison of women with kids to women without kids (parity 2+ vs parity 1). Some papers then go on to limit the analysis to women with kids and compare those with 2+ children compared to 1 child (parity 3+ vs parity 2). HMS follow this approach.

Given DD1 is HMS's "base model" for this analysis (and is the base model used in the broader employment literature) it is clear why HMS use 1991-1993 as the pre period rather than 1990-1993. 60% of the 1990 expansion occurred in the first year (between 1990 and 1991) so dropping 1990 allows us to omit the biggest component of the prior policy expansion in the pre period. The remaining increase from 1991 to 1993 ($163) is small relative to the large increase for parity 2+ over 1994-1996 ($644 for parity 2 and $2061 for parity 3+).

DJ instead begin their critique of HMS by looking at parity 2 vs parity 1 (though according to the sample size in DJ Table 1 they use all observations; they should drop those of parity 3+ in this regression). DJ never present any DD1 estimates in their paper.

Why did HMS choose this time period: (2) EITC Literature -- Additionally, our choice to use OBRA93 -- in a difference-in-difference model and with a (short) pre-period – also follows other papers in this literature. For example, Evans and Garthwaite (2014) use a similar approach to examine impacts on maternal health.

As a minor point, we note that in DJ Footnote 4, the authors appear to misunderstand HMS's interest in the event study, given the phased-in implementation of OBRA93. HMS are examining the ramped up impacts in light of the fact that the EITC expansion for Parity 3+ phases in over several years. They are not motivated by delayed treatment impacts. As such, this does not speak to the issue of delayed treatment effects.

DJ's alternate DD analysis

DJ re-estimate the event study models using 1987-1990 as the pre-period (DJ Figures 2 and 3), in part justified by their view that "1987-1990 provide(s) a superior pre period with 1991-1998 as the more appropriate post period." (DJ p. 5). They then show that for this alternative analysis of their design, tests of the parallel trends assumption would fail.

We see this analysis as not very informative for HMS. While there are differential trends by parity over 1987-1990 for some subgroups, this is a period excluded by HMS from their analysis. Additionally, to reiterate the above, DJ's event study compares parity 2 to parity 1 which is never advanced by HMS as a standalone design. Furthermore, HMS discuss the longer term trends reflected in DJ Figure 3 (in particular see HMS Appendix Figure 1, and discussion on HMS p. 187-188).

DJ note that some of the expansion of OBRA90 for Parity 2+ occurs during years 1992 and 1993, the "pre period" years for HMS main analysis of OBRA93. This is not new: HMS figure 1 clearly shows this moderate expansion in these years, and also that the subsequent expansion 1993-1994 is much larger in magnitude. DJ claim that this invalidates a comparison of parity 2 vs. parity 1. This claim may reflect a misunderstanding of the research strategies in HMS. First, it is not uncommon to implement a DD analysis with "large growth" as the "treatment" of interest. In this way, parity 1 births experience small growth throughout, parity 2 births experience small growth in the (1991-1993) pre-period, and parity 2 births experience "large growth in EITC" in the post period. Second, in HMS parity 1 vs. parity 2 is a minor part of the overall analysis, and HMS analysis (Figure 2 Panel B) shows that this comparison provides both a small difference in EITC exposure, as well as a small change in the LBW rate. In the same figure, HMS show that the 3+ group receives both greater increase in EITC as well as greater reductions in LBW.

What is new and what is not new in DJ's critique? The magnitude of the EITC expansion across groups (parities) is presented in HMS. DJ's focus, in particular, on the relative size of the policy change for parity 2 vs parity 1 is presented in HMS Figure 2B (the dashed lines show the difference in difference of the maximum EITC benefit across groups to facilitate a comparison of the low birth weight treatment effects across groups and over time).

The issue of the longer term trends, the 1987-1990 pre-period examined in DJ, is also presented and discussed in HMS. HMS begin the paper by presenting the difference-in-difference results (HMS Table 2) along with the event study version of these results (HMS Figure 3). After presenting these main DD results, HMS discuss them within the context of longer term trends:

> "We provide additional context for these results in online Appendix Figure 1 panels A–D. In these figures we show raw trends in the rates of low birth weight by parity, over the period 1981–1999. In each figure we show low birth weight probabilities relative to the 1993 level. Online Appendix Figure 1, panel A has results for our high-impact group, and shows two main findings. First, the changes we observe in our experimental period occur within long-run decreases in rates of low birth weight for this population (although in the pre-1991 period there are many other policies changing, including earlier expansions of the EITC). Second, the raw trends for 1991–1998 show the same pattern as in our main event study results in Figure 3, panels A–C; thus, these results are not strongly impacted by the covariate controls in the model. In contrast, the trend for all births is one of increasing rates of low birth weight (online Appendix Figure 1, panel B). The long-run trend for white high-impact mothers (online Appendix Figure 1, panel C) is similar to that for all high-impact mothers, while for high-impact black mothers (online Appendix Figure 1, panel D), the trends are less monotonic. All the figures show that there are large changes in parity gaps in low birth weight that occur during the 1980s. It would be valuable, but outside of the scope of this study, to understand these changes better." [HMS p. 187-188]

In summary, in response to DJ section 2, we believe that the analysis either (1) reflects a recapitulation of results already reported in HMS, or (2) reflects a critique of a hypothetical analysis not performed in HMS. HMS's choice of the pre period is the direct result of the tax law changes and this approach is commonly used in the EITC literature. Additionally, HMS are clear in pointing out how this period fit into longer term trends. We remain of the view that explaining these longer run trends is outside the scope of HMS's paper.

<u>Response to DJ Main Comment 2 (*DJ section III*): Comparing 2nd births to 3rd and higher births for OBRA 1993 difference-in-difference and event study analysis is problematic because a placebo test fails (4+parity vs. parity 3, for African American mothers).</u>

In section III DJ observe that parity 4+ vs. parity 3 provides a placebo test of the 3+ vs 2 design in HMS. DJ show that this comparison produces statistically significant results. They further split the analysis by race, and show that both the main effects (3+ vs 2) and the falsified placebo tests (eg 4+ vs 3) are findings that hold for black mothers only.

What is new and what is not new in DJ: First, HMS provide this placebo test in a robustness and sensitivity test section. HMS describe it here:

“To examine this further, we examine a series of “pair-wise” comparisons of different parity births. Some of these comparisons (e.g., 2 versus 1, 3 versus 2) embed a treatment and some (e.g., 4 versus 3) form a “placebo test” for our estimation method.

We present results in online Appendix Table 6 for the high-impact sample. In the first row we compare second births (treated under the “one child” EITC schedule) to first births (untreated), and so on. The first two rows reinforce our main findings—there was a relative improvement in low birth weight for second births compared to first births, and also for third births compared to second births. The remaining rows of the table compare pairs of birth parities that are both “treated,” and we expect to find no estimated effect for these comparisons. This appears to be true for fifth versus fourth and sixth and higher versus fifth. However, we do find that low birth weight improved more for fourth births than for third births, which is not consistent with our expectation. To investigate this finding further, we estimated an event study model for this comparison. This analysis indicates that the 4 versus 3 difference begins in 1995 and grows after that.

The gap between fourth and third births does raise a cautionary note about potential parity-specific trends in birth weight, and our analysis should be interpreted in light of this caution. We believe that despite this, the preponderance of evidence indicates that the EITC does improve child health. First, the timing of these spurious trends does not correspond cleanly with the policy change. And second, in our “maxcredit” models, results are robust to inclusion of parity-specific trends.” (HMS p. 205)”

Second, HMS also investigate results by race. They begin in HMS Table 3 showing that the point estimates are largest for black women:

“Table 3 shows heterogeneity in effects by race and Hispanic origin within the high-impact sample. The EITC reduced the likelihood of having a low birth weight birth for black mothers of 0.73 percentage points’ (relative to a mean of 14.4 percent), more than four times higher than the effect on white mothers (0.13 percentage point decline relative to a mean of 8.1 percent). Interestingly, smaller treatment effects are experienced by Hispanic mothers than by non-Hispanic mothers (−0.13 versus −0.41 in the second+/first parity model). “ (HMS p. 188)

HMS go on to estimate a first stage effect of the EITC expansion on income to add interpretation to the results by racial subgroup.

“As shown in Table 3, the effects of the EITC expansion on LBW is larger for black, single, low-educated women, compared to white, single, low-educated women. Interestingly the CPS analysis, as shown in panels B and C of Appendix Table 1, shows a smaller first-stage effect on after-tax income for black women (compared to white). Thus, the estimated treatment on the treated results are larger for black compared to white women. For example, the comparison of second and higher births compared to first births shows a 5.3 percent reduction for blacks and a 1.1 percent reduction for whites (percent impact of a $1,000 TOT).” (HMS pp 191-192)

Given this, what is new is DJ’s presentation of the combination of the placebo analysis with the black subgroup analysis. While this is somewhat interesting, we are skeptical that it is a large enough contribution for a JHE audience.

Response to DJ Main Comment 3 (DJ section IV): Sensitivity to adding quadratic trends by parity to panel fixed effects model using multiple expansions

While the OBRA93 analysis is at the core of HMS, we also present estimates from a panel fixed effects model to allow for leveraging the variation across multiple EITC expansions. We estimate a parametric model where we control for the maximum credit (varying by parity and year) along with a full set of year and parity fixed effects (as well as demographic cell fixed effects, and state-year policy controls). This model is presented in HMS equation 2.

In section IV of their paper, DJ critique this approach by showing the sensitivity of the results to adding controls for parity x linear trend and parity x quadratic trends (DJ Table 3).

What is new and what is not new in DJ's critique? The issue of sensitivity to parity trends is presented and discussed in HMS. In HMS Table 5 we show the sensitivity of the results to adding controls for parity by linear time. In discussing these results, we say:

> "Due to the longer time span, with multiple EITC expansions, we can explore the sensitivity of the results to the inclusion of parity-specific linear trend (in year). The results (in columns 2, 4, and 6) show substantially larger estimates treatment effects for model with parity linear trends. While we may be "overfitting" the parity-time relationship, we view the robustness to including the parity trends as an important result." (HMS p. 195)

DJ expand on this approach and also control for parity by *quadratic* linear time. On page 11 DJ refer to the inclusion of quadratic trends by parity "*given the curvilinear relationship to low birth weight*." While it is true that this only involves three additional parameters, because the majority of the EITC occurs with the OBRA93 expansion, this represents a considerable burden on the data to estimate quadratic trend terms *separately from the effects of the policy*.

Presumably DJ's intention in adding quadratic controls is to control for "pre-existing trends"; trends that would have happened even if the policy had not changed. However, because the policy does change frequently over time, and in broadly non-linear fashion (see Response Figure 1 above), there is a nontrivial risk that the quadratic trends will be over-fit to capture the impact of the policy. This would not happen if the trends were fit only to time periods with no policy changes. But with a global polynomial fit there is a risk of the trends becoming "bad controls", in the sense of being fit to the causal impacts of the policy changes.

Finally, quadratic trends is not a standard specification in this literature. Given concerns about the short length of the time series, the many periods of change, and the potential for overfitting, it is unclear whether the quadratic should be preferred over linear or no trends.

DJ on page 11 present a new analysis limited to data from 1983-1993. They present this as a check against "over-fitting of trends". It is not clear how this analysis speaks to that concern. We believe that it does not address this concern.

Response to DJ main comment 4: Associations between the EITC and birthweight are confounded by the crack-cocaine epidemic.

In our view this is the most constructive part of the paper.

DJ frame their new analysis as "adding only 8 data points". We think it more accurate to characterize this as adding a new variable, and enriching the model to allow for parity-specific coefficients for this variable. (Note however in the text in the paper it is unclear if they are interacting the homicide rate with parity, and this should be clarified.) Given that HMS's design in some sense boils down to a 3-by-T design (with 3 parity groups), it is not surprising that adding another variable which has the same time pattern as the EITC expansions will confound the effects of the EITC expansions. Indeed, if we were to find another variable that peaked in 1990 and then declined, and interacted this variable with parity, it might serve a similar role. While there are important historical reasons to think that crack could play a role, we view the implementation in DJ to be asking a lot of the data.

Likewise, for this analysis to be more convincing we would like to know more about the timing of the initiation and ending of the crack cocaine epidemic across localities, as well as more about which different demographic groups were affected and how. There was large geographic variation in when the epidemic began and ended. Looking at state or county specific homicide rates and how those corresponded to changes in infant health by parity would add a lot to this analysis. Finally, it seems plausible that the EITC could interact directly with the spread of crack-cocaine epidemic. This should be considered as well.

As a minor note, it would be helpful to see the coefficients on the crack-proxy variable, for each parity. This could help to assess the "face validity" of this specification.

Below we provide responses to the other points raised by DJ (which we read as less central to their main argument):

(The numbering is ours.)

1) [DJ p. 13] "*Based on HMS's data and specifications, the EITC lowered the prevalence of smoking by 2.41 percentage points among black women of parity 3 relative to parity 2 over a mean prevalence of 19 percent. The finding is implausible for two reasons. First, it assumes smoking is an inferior good among low income, single women for which there is little evidence…*"

   HMS discuss this point in the paper (see HMS, pp. 177-178). They make the point that behaviors such as smoking and drinking could change due to increases in after tax income (e.g. smoking is a normal good). But they additionally point out that increases in employment (a robust finding for the EITC for single mothers) could independently lead to a decrease in smoking (HMS p.178). In the end, changes in income alone are not the only channel for the effect of the EITC on smoking. Thus, as is discussed in HMS, the approach does not presume or rely on smoking being an inferior good.

   There are three papers that focus on the effects of the EITC on smoking, and find results consistent with HMS. Averett and Wang (2013) use several difference-in-differences designs to show that smoking declines among low education mothers after the OBRA93 expansion. Cowan and Tefft (2012), similarly use difference-in-differences around the OBRA93 expansion to show that smoking declined among unmarried women with less than a college degree. In contrast Kenkel, Schmeiser, and Urban (2014) leverage changes to the maximum state and federal credit over time to show that smoking increases among low income adults. While no paper we know of has resolved the differences between these papers, the pattern of findings fits our hypothesis: work focusing on mothers or unmarried women find declines in smoking consistent with increases in employment among these groups. Kenkel, Schmeiser, and Urban find increases in smoking when they focus on the population of adults, potentially driven by men who do not change their labor supply in response to the EITC (Kenkel, Schmeiser, and Urban do not present separate results for men and women smokers).

It is also important to recognize that pregnant women have a unique relationship with smoking due to the role that information and prenatal care play. Elevating women out of poverty could change their information set, the quality of prenatal care they receive, or their decision to act on medical advice related to smoking. These changes could occur in a variety of ways that are not yet understood by the literature and that we are unable to measure, but suggest a more nuanced story than one solely reliant on the income elasticity of smoking. Ultimately, HMS report effects on smoking because that is the evidence provided by our research design. They acknowledge that it is not yet well understood how income, employment, and prenatal smoking interact.

2) [DJ Section V, p. 12] DJ argue that we wouldn't expect to see an effect [on low birth weight] anyways, given the medical literature.

   DJ cite a 2007 review by the Institute of Medicine (IOM) which states in the abstract: "The current methods for the diagnosis and treatment of preterm labor are currently based on an inadequate literature, and little is known about how preterm birth can be prevented" (DJ p. 12).

   We agree that there is a lack of understanding of biological mechanisms for individual cases of preterm birth, this is reflected in a low R^2 in many models of preterm birth. However, that does not mean that there are not plausible interventions that can change individual behavioral and environmental factors that influence the occurrence of pre-term birth. This is also recognized by the medical community. The same abstract cited by DJ from the IOM states directly before their quotation: "Preterm birth is a complex cluster of problems with a set of overlapping factors of influence. Its causes may include individual-level behavioral and psychosocial factors, neighborhood characteristics, environmental exposures…" (Institute of Medicine, 2007).

   Relatedly, stress is a documented predictor of pre-term birth (Berkowitz and Papiernik, 1993). There is a small but growing literature documenting that increases in income can lead to reductions in stress (Aizer et al 2016, Evans and Garthwaite 2014, Fernald and Gunnar 2009 and Haushofer and Shapiro 2016). Of particular note in this context is Evans and Garthwaite (2014) who find that increases in the EITC lead to reductions in maternal stress.

3) DJ state in their introduction as a framing for the paper "Despite an extensive set of analyses, the only comparison with a potentially credible design is between women of parity 2 to women of parity 3+"

This is a strange statement to make in the 4th paragraph of your paper.

4) In footnote 5, DJ critique HMS's use of the 3rd trimester as the time period to assign the EITC tax policy.

   HMS (pp190, fn 14) justify their assumption that the third trimester is the sensitive period for birthweight as follows "For example, the cohort exposed to the Dutch Famine in the third trimester had lower average birth weight than cohorts exposed earlier in pregnancy (Painter, Roseboom, and Bleker 2005). In addition, Almond, Hoynes, and Schanzenbach (2011) show that the impact of exposure to the food stamp program is greatest in the third trimester. Also see the review in Rush, Stein, and Susser (1980)."

   HMS also examine the sensitivity to this in Appendix Table 4, in an analysis of the effects on pregnancy behaviors such as prenatal care, smoking and so on. The results are qualitatively similar across alternative assumptions about timing (e.g. first trimester, second trimester).

<u>Additionally, we note some misstatements in DJ:</u>

- DJ state in the abstract that "Identifying small, casual effects of a national policy at a single point in time is exceedingly challenging." This statement misses the fact that there are tax law changes in 1986, 1990, 1993, 2001 and 2009) that create variations over time.

- DJ's description of their models (1) and (2) are incorrect. DJ state "Y_{pjst} is the rate of low birth by parity (p_i), demographic group (φ_j), state (λ_s), and year (δ_t)." This should instead say "Y_{pjst} is the rate of low birth weight for parity p, demographic group j, state s, and year t." The statement refers to the fixed effects instead of the subscripts of Y.

- On page 14 DJ imply that "*cash transfers from the EITC received mostly in February and spent largely on durables and transportation*" Patel (2012) finds that the EITC leads to increases in primarily non-durables, work related, and housing expenditures. Increases in durable and investment expenditures are concentrated in the first quarter, around the time of refund receipt. But taking account of the changes throughout the rest of the year, the effects are more concentrated in non-durables.

- At several places in the text, statements about "controls for trend" (fn9) or "trend terms interacted with parity" (p. 11) do not clearly state whether they are referring to linear trends or quadratic trends. This is an important distinction (as discussed above in our main point 3).

- DJ section V (The Etiology of Low Birth Weight and Plausible Mechanisms) focuses on "pre-term births". However, HMS is about low birthweight (LBW) status and not preterm birth. While the two are correlated, they are not the same. By moving between discussing HMS LBW results and citing medical literature that references preterm births, the distinction between the two is conflated and it leads to the impression that clinical research that applies to preterm births applies equally to low birth weight. This distinction warrants greater clarity in this section.

- On page 16 when describing the re-estimated DD with controls for homicide rates, DJ state “In this section we use the national homicide rate among black males 15-24 as a proxy for the spread of crack cocaine markets in the 1980s and 1990s. We add this *one variable* [emphasis added] to HMS’s DD specification of low birth weight between some parity 3+ and 2.” This is unclear. As stated this variable would not be identified given that it is only varying with time and there are already time fixed effects in the model. The notes to Table 4 state that the homicide rate is interacted with the treatment dummy. That needs to be corrected in the text.

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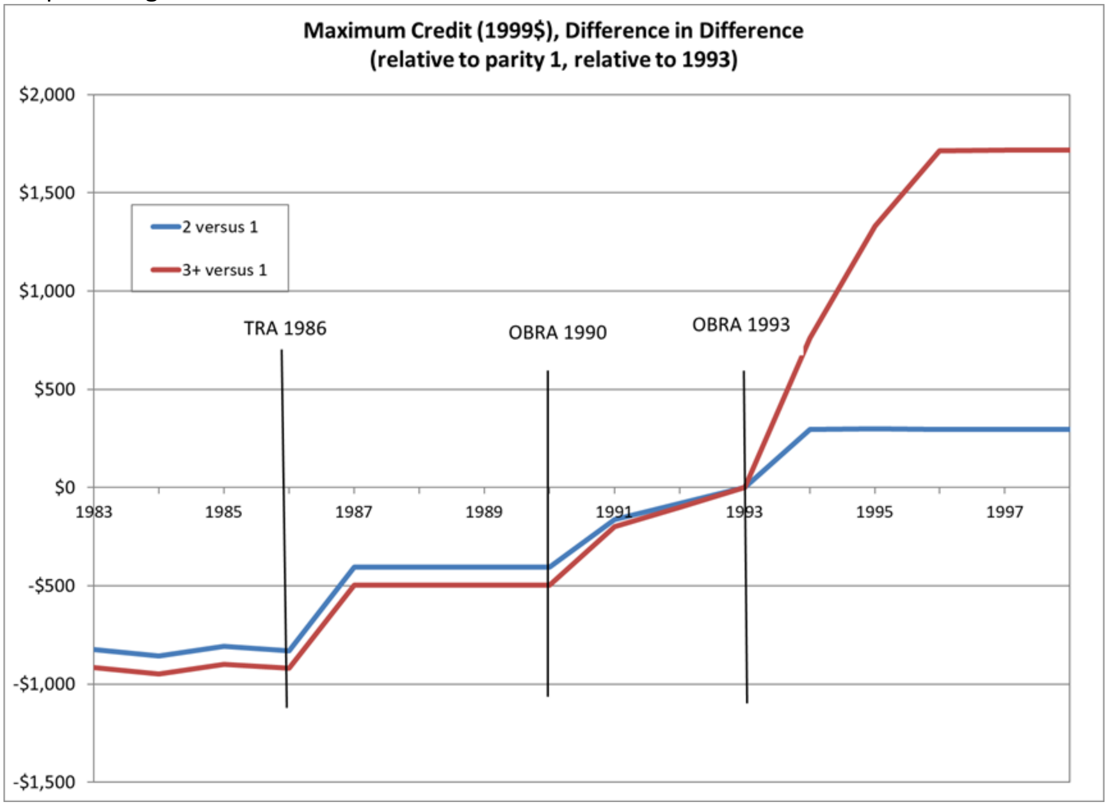
Response Figure 1



Response Table 1: Maximum Credit (1999$) by family size (parity of birth)

	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998
Difference (relative to 1993)												
childless (parity 1)	$0	$0	$0	$0	$0	$0	$0	$347	$346	$346	$347	$347
1 child (parity 2)	-$407	-$407	-$406	-$406	-$163	-$81	$0	$642	$646	$644	$643	$644
2+ children (parity 3+)	-$497	-$497	-$496	-$496	-$199	-$99	$0	$1,106	$1,677	$2,061	$2,063	$2,063
Difference in difference (across parity, relative to 1993)												
2 vs 1	-$407	-$407	-$406	-$406	-$163	-$81	$0	$296	$300	$297	$296	$297
3+ vs 1	-$497	-$497	-$496	-$496	-$199	-$99	$0	$760	$1,331	$1,714	$1,717	$1,716
3+ vs 2	-$90	-$90	-$90	-$90	-$36	-$18	$0	$464	$1,031	$1,417	$1,421	$1,420