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# The EITC Disincentive: A Reply to Dr. Hilary Hoynes

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Abstract

**A rejoinder to:** Hilary Hoynes, "The EITC Disincentive: A Reply to Paul Trampe," *Econ Journal Watch* 4(3), September 2007: 321-325. Link.

**In the September issue Dr. Hilary Hoynes replies to my paper** "The EITC Disincentive: The Effect on Hours Worked of the Phase-out of the Earned Income Tax Credit." She criticizes my remarks about a figure in Eissa and Hoynes (2005), my downplaying the income effect, and my not including a control variable in my own investigation.

## The Income Effect

> Basic labor supply theory shows that an increase in income will lead to a reduction in labor force participation and hours work. This is known as the income effect. Theory also shows that a compensated increase in wages leads to an increase in labor force participation and hours worked. This is known as the wage or substitution effect….
> Trampe, by ignoring the income effect, incorrectly concludes that the EITC is work-promoting in the phase-in region. In the flat region, the EITC produces a negative income effect leading to an unambiguous reduction in hours worked. (Hoynes 2007, 321-322).

About a third of the way into my paper I note, "Of course it is also possible to observe what appears to be a disincentive throughout the income scale due to

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the simple fact that the subjective marginal benefit of each additional dollar of income declines as income rises" (311), Professor Hoynes is correct that I otherwise focus on the substitution effects. I figured it was reasonable to suppose that for families living in poverty the income effect of $4500 is not significant. If I'm wrong about the magnitude of the income effect in the plateau region, only a study specific to that income range could demonstrate that, not a study of the entire EITC population or of the entire population of single women with children regardless of income. What Dr. Hoynes' point argues for is separate studies of each income region of the EITC but she seems to be using the income effect point to defend papers which do just the opposite, lump every income level together.

> [S]ome taxpayers with incomes beyond the phase-out region may choose to reduce their hours of work and take advantage of the credit. (Hoynes 2007, 321-322)

I find it extremely unlikely that anyone with income just beyond the phase-out range would give up wages in order to receive the insignificant EITC benefit. In fact a study by John Scholz (Scholz 1996) suggests that many people near the end of the phase-out range do not even bother applying for the credit because the amount is not worth the paperwork. Furthermore, if someone did take some leisure time to get into the phase-out range, he or she would be in the population I am studying. Hoynes seems to be using the point to justify studies which include families with children above the EITC income range. Even if there were individuals who acted as she suggests I do not see how that justifies basing conclusions of the effects of EITC on those who did not act that way and remain above the EITC income range.

If the income effect for those whose incomes place them in the phase-in or plateau regions or for those whose benefit is mostly phased out is as minor as I think it is, using cases from income ranges other than the phase-out range grows the sample greatly but adds only a small number of the additional cases, if any at all, involving individuals who reduced their hours on account of the EITC, thus watering down the more substantial effects in the phase-out region to the point that statistically significant results are impossible. It may be argued that the goal of the papers I criticize was to find the effect of the EITC as a whole, whereas I am only looking for the effect of the phase-out rates. However, it would seem to me that the best way to measure the effects of the program as a whole is to study each income range separately. The income effect point would seem to support the expectation that EITC reduces hours worked, yet if researchers expand the population studied based on those theories, the result is that the effect is drowned in a sea of statistical noise.

## Comments on Earlier Studies

> Further, Meyer and Rosenbaum's NBER working paper version of their 2001 *QJE* paper (Meyer and Rosenbaum 1999) extends their method to examine impacts of the EITC on hours worked. This should be recognized. (Hoynes 2007, 322)

I wrote (312): "Bruce Meyer and Dan Rosenbaum (2001) found mixed results on hours worked for women with children—also using the entire income spectrum (Meyer and Rosenbaum 2001)." Further, I find it odd that Dr. Hoynes criticizes me for not discussing an earlier unpublished version of a paper I did discuss.

> Obviously, descriptive trends are not conclusive as to the impact of individual policies because there is much else changing over time. The second paragraph in the section "Previous studies: Labor Force participation" makes this mistake. (Hoynes 2007, 323)

I am commenting on papers which used graphs of descriptive trends as evidence of the impact of a particular policy. I'm suggesting the evidence in said graphs are not what the authors claim (or not limited to what they claim) but the basis of the analytical tool was their choice, not mine. I am quite aware that such trends are not conclusive as to the impact of individual policies and mentioned that such analysis is quite subjective.

> First, Trampe states that "... they do not comment on the dramatic increase in hours worked by single women without children starting in 1984 which was not accompanied by a similar increase for those with children." The EITC did not expand until the Tax Reform Act of 1986 so any change by single women without children prior to 1986 is not relevant. (Hoynes 2007, 323)

I merely noted that the divergence starting in 1984 should have been mentioned. The chart is presented as evidence that there is no significant difference in trends in hours worked between the EITC population and the non-EITC population. On the other hand, the divergence in the lines proceeds from the very beginning point on the chart, 1984. We do not know if the trends measure back before that, to the beginning of EITC in 1976. Or did the divergence in trends begin in 1983? Dr. Hoynes chastises me for not controlling for macroeconomic trends (see below) yet ignores that it is at least possible that the disincentive only has an effect during periods of economic growth such as the one which began in

1983 when there is more opportunity to increase one's hours.

> There was no policy change after 1993 so any fluctuation between 1997 and 2000 should not have anything to do with the program! (Hoynes 2007, 323)

The 1993 policy changes to which she refers were legislated in 1993 but implemented in stages from 1994-1996. The fluctuations began shortly after the policy change was fully implemented. It is reasonable to assume that it may take years for people to learn and adapt to policy changes, particularly complex policy changes.

## Response to Criticisms of the Method of My Own Statistical Investigation

> The fundamental problem with this approach is that it ignores the selected nature of the sample. As EITC expands, labor force participation increases which can lead to changes in the composition of the sample of those in the phase-out range. For example, what if women who enter the labor force work fewer hours than women already in the labor force? The hours will decrease with the expansion of the EITC yet (in this simple example) there was no reduction in hours worked! This is a very old problem in empirical labor supply and there are many approaches that are used to solve this basic endogeneity problem. (Hoynes 2007, 323)

I dealt with this problem by choosing a post-policy year ten years removed from the policy change (2006). The labor force expansion had long since taken place and there was no reason to believe that the percentage of those in the phase-out range who were new to the workforce in 2006 was higher than in my pre-policy year of 1993 (which itself was seven years removed from a smaller EITC expansion). The effect on hours, on the other hand was ongoing, as the phase-out rates have not changed since 1996. I used demographic control variables to account for other differences in the composition of the phase-out sample.

> The problem is that there is no control for year fixed effects in the model. Therefore, if there are any other factors that vary by year (labor market effects, other trends, other policies) the estimates will be biased unless there are perfect controls for these features (and in point of fact, there are NO controls of this sort in the model). This is a fundamental problem with the empirical model and in

> fact is the main reason that people use control groups; ideally they are selected such that they face the same environment except for not facing the policy change. (Hoynes 2007, 324)

Controlling for macroeconomic differences between the years was not possible if my model was to answer the question I was trying to answer. As Dr. Hoynes points out, controlling for such events when the value of the policy variable is determined primarily by year without a control group is pointless, as the value of the control variable will be the same for each case from a particular year. However introducing the control group would have upset the model in other ways.

When Eissa and Liebman (1996) conducted a similar study of the EITC expansion following the 1986 tax reform, they controlled for the national unemployment rate by using a control group of single women without children (and therefore not eligible for EITC) in the same income range as the rest of the sample. The policy change they were studying was one in which the maximum benefit of EITC was increased but the phase-out rate was actually reduced, creating a sizable income range which was outside the EITC before the policy change but in the phase-out range afterwards. In other words the entire sample from the pre-policy year was outside the program, without an EITC induced marginal tax rate. They were testing the effect of an EITC-induced marginal tax rate changing from 0% to 10-11% (depending on family size). Therefore adding a test group also outside of the program with a 0% marginal tax rate from EITC did nothing to change the basic composition of the population.

In my case, however, I was seeking to test the effects of moving from a 10-11% phase-out rate to 16-21% today. There was no one who remained at the 10-11% rate after 1993 so the only way to include a control group was to include cases of those outside the program. Unfortunately, adding a significant number of cases with an EITC phase-out rate of zero would have muddled the picture and left it impossible to isolate the effects of moving from 10-11% to 16-21%. As I mentioned in my conclusions, one possibility raised by Eissa-Liebman (1996) is that the effects of marginal tax rates are not linear. There may be little to no effect moving from a 0% rate to 10-11% but as the rates go higher there may come a point where the rates are high enough to trigger a response in enough cases to make a measurable difference in hours worked. Therefore it was impossible to isolate the effects of any macroeconomic control and isolate the effects of the 1993 expansion of the program at the same time, and I chose one over the other.

It is unfortunate that I could not isolate the macroeconomic effects because the effects, if any, would likely have served to advance the case that there is some discouragement of work. There are, of course, many such variables but the unemployment rate, which Eissa and Liebman used, has the most obvious

tie to the dependent variable of hours worked. In my pre-policy change year of 1993, when the phase-out rates were the lowest, the unemployment rate was 6.9%. In the year I used from the gradual implementation of the policy, 1994, the unemployment rate was 6.1%. In my post-policy change year, 2006, when the phase-out rates were at their highest, the unemployment rate was 4.6%. This means of course that the general labor market was working against the effects of the phase-out rate. If lower unemployment nationally is at all related to increasing hours for those in the EITC phase-out range (and Eissa and Liebman found that it is), then if I had been able to isolate such effects it could only have magnified the effects of the phase-out rates I found. Dr. Hoynes criticizes me for not using a control variable which, if there were any effects, could only have strengthened my finding.

> The determinants of labor supply of married couples differ from singles and this should be reflected in the empirical model. (Hoynes 2007, 324)

There is certainly nothing wrong with separating the sample by marital status in order to measure how the effects differ between the two groups, but that was not the question I was seeking to answer.

> Finally, why limit the analysis to a random sample of 200 households in the phase out region? The CPS has much larger samples than this and there is no reason to do this with modern computing opportunities. The larger samples will also allow for stratifying results by marital status. (Hoynes 2007, 324)

I reported the results of an empirical investigation undertaken for a project in my graduate studies. It is the only sample I have done. Redoing or enhancing the sample would have created indeterminacy and ambiguity about which data to include. Six hundred cases (200 each in 1993, 1994 and 2006) are more than adequate for statistically significant results.

## Concluding Remark

I am grateful to Dr. Hoynes for replying to my Comment. My understanding of the empirics of the EITC is being enriched by the exchange. I hope others reading the exchange feel likewise.

## REFERENCES

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