

AN ANALYSIS OF VARIATIONS IN U.S. FERTILITY AND FEMALE LABOR FORCE PARTICIPATION TRENDS: A REPLY

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REPLY

It appears that the comment has two main points. First, my model and Easterlin's work provide a cohort interpretation of what are essentially period effects on fertility and, second, the support offered in my paper for both Easterlin's hypothesis and the conclusions of Butz and Ward is non-intuitive and contradictory. I think both of these points are exaggerated.

As far as I can understand, the evidence for my supposedly erroneous interpretation of fertility trends as cohort-based phenomena comes from two specific points. First, my conclusion that increases in relative income appear to lead to a less rapid decline in the age-fertility schedule until roughly age 32 and a more rapid decline at older ages is alleged to reflect only a strong period effect, the result of the behavior of my "most favored" cohort, which reached age 32 during the years of rapid fertility decline (presumably the 1928–1933 female birth cohorts). This allegation is not supported by strong empirical evidence and is, at best, speculative. My conclusion on the impact of relative income on the timing of fertility is derived from the estimated regression coefficients on age, age-squared, and the interactions of age and age-squared with relative income and wages. All cohorts in the sample contribute to the estimation of these regression coefficients, and while my favored cohorts undoubtedly influence the result obtained, it is by no means clear that the regression results and their implications are attributable solely to these cohorts.

The other argument used to support the claim that my cohort interpretations are misleading concerns the relative income variable. According to the comment, variations in my relative income variable reflect only strong period effects. Again, I think this statement is exaggerated, partly because of an apparent misunderstanding of the definition of my relative income variable. Unlike Easterlin's relative income variable, which focuses on the income of male cohorts 20–24 vis-à-vis their parents and is essentially constant over a cohort's life cycle, the relative income measure used in my paper allows the relative income of a cohort to vary over its working life cycle. There is no direct evidence in my paper of continuous slippage in relative income, because relative income varies by cohort and by age. The mean value of a cohort's relative income does decline from the 1931 cohort onward, but these means subsume a considerable amount of within cohort variation. Furthermore, the speculation about the pattern of relative income if the Eisenhower and Nixon-Carter economies were stronger or the Kennedy-Johnson economies weaker does not imply that my relative income measure is a period phenomenon. Indeed, an interesting implication of Easterlin's hypothesis is that these economies were themselves the result of the relative sizes of the various cohorts in the labor market at those times. It is possible that strong period effects themselves are only a reflection of underlying cohort effects.

The second point made in this comment—that my support for Easterlin and Butz

and Ward is non-intuitive and contradictory—is also not substantiated. First, my decomposition of the total fertility rate does not imply that the baby boom would have persisted during a period of zero growth in real income, as the comment suggests. Male income was allowed to vary; only female wages were held constant. In any case, this decomposition only highlighted the importance of increases in female wage rates (real wages) in explaining the decline in fertility.

Finally, my findings are neither “mid-course correction(s)” nor fundamentally contradictory. My results do not imply that the Easterlin hypothesis fails post-1965 or that female wage rates do not explain pre-1965 behavior. During the 1950s, in fact, real female wages changed very little while relative income increased, leading to the conclusion that the increase in fertility can be explained primarily by the changes in relative income during that time period. Further, my conclusion that the increases in real female wage rates were important predictors of the decline in fertility does not imply that Easterlin’s model fails. There is nothing inherent to the Easterlin hypothesis that precludes an effect of female earnings on fertility.

Despite my objections to the interpretation of my paper, I do believe that the author raises a valid point, which is the extent to which fertility behavior over the past century can be explained by cohort-specific versus period effects. While my paper may appear too wedded to cohort theories of fertility, I think the resolution of this issue must not rely solely on selective descriptive information on fertility and cyclical economic activity. Solid empirical research is needed to explain both the variations in U.S. fertility and their subsequent impacts on economic activity.