Race Differences in Cohort Effects on Non-Marital Fertility in the United States: Reply to Martin

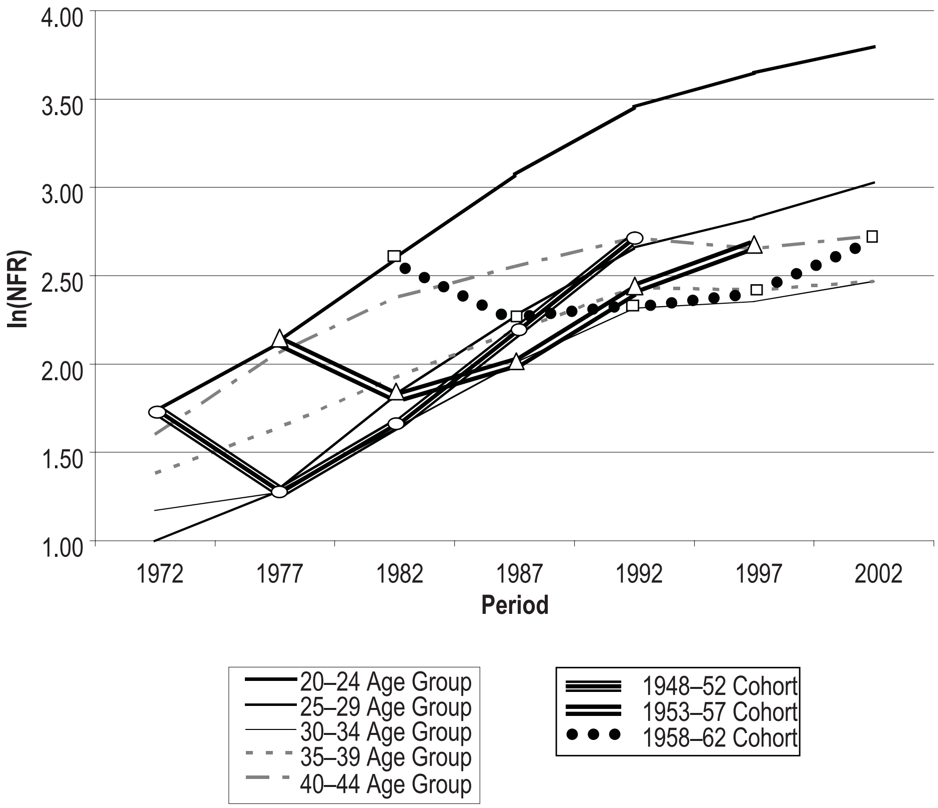
Jean Stockard, *University of Oregon*
Jo Anna Gray, *University of Oregon*
Robert O'Brien, *University of Oregon*
Joe Stone, *University of Oregon*

We appreciate the opportunity to clarify and provide additional tests of the key elements of our age-period-cohort analysis of non-marital birth rates in this March 2009 issue of *Social Forces*. Where Steve Martin, in his commentary, has suggested specific alternative specifications or interpretations of our findings, we have re-estimated and performed explicit tests of the alternatives. In the end we have demonstrated the robustness of our results and strengthened the case for our interpretations. The points Martin raises and our responses should help to clarify the nature of cohort effects and our interpretation of them. We thank Martin for his close attention to our paper.

As Martin notes, in the typical rectangular age-period table there are the same numbers of cells/observations for each age, the same number of cells/observations for each period, but different numbers of cells/observations for some of the cohorts. This is well known – yet cohort analysts persist in using such tables when studying cohort effects. These analysts know that if one does not take into account the age and period effects in such a table, then differences between cohorts could be produced by age and period effects. If one does control for the main effects of age and period, however, this will not create apparent cohort effects.

Martin suggests, however, that there are patterns of age*period interactions that are not related to cohort effects that could produce the patterns we observe along cohort diagonals. Certainly such patterns are possible: It is incumbent on the critic to point out these patterns and, given that our discipline considers theory important, to produce compelling reasons why such patterns might exist. Martin does both. He suggests that there is an interaction between age and period. He notes that "the non-marital birth ratio rose steeply for women in their 20s and slowly for women in their 40s." That is, increases in NFRs over time are different for different age groups. Martin then suggests that "[a] plausible explanation for this age*period pattern is that delays in marriage increased the unmarried population most rapidly among women in their 20s, so the rise in the non-marital birth ratio has been most pronounced among women in their twenties." His Figure 4 displays the differences in these slopes, and it is on this figure that much of his argument rests.

Figure 1. Trends in Non-marital Fertility by Age, Period and Cohort



We redrew Martin's Figure 4 as our Figure 1, but we added bolded lines for the three cohorts with complete age data. Note the distinct differences in the trajectories of these three cohorts over time, even in the context of the differences in the slopes of NFRs for the different age groups. The cohort born 1948-1952 (represented by the bolded triple line) shows a decline in NFRs from ages 20-24 and ages 25-29 and then rises steeply until the last observation for the cohort at ages 40-44 in 1982. In contrast, the cohort born from 1958 through 1962 has an NFR that drops between ages 20-24 and ages 25-29 and then rises at a very slow rate until the last observation for the cohort at ages 40-44 in 1997. The cohort born in the years 1953-1957 has yet a different and distinct trajectory. We might well interpret this graph as showing both the interactions of the relationship between age-groups and NFR over time and the effects of cohorts – *even when these interactions are taken into account*. We are wary, however, of trying to eyeball cohort effects from graphs without controls for age groups, periods and in this case age-groups by period interactions.

Testing Martin's Alternative Explanations

We first test whether these differences in steepness of the slopes for NFRs for women in different age categories can account for our finding of cohort effects. We created interaction terms between each of the age-groups (except for the oldest age-group that was used as a reference group) and time (coded 1972, 1977… 2002) to code the "age*period trend" interactions. We report the results of this analysis and others in Table 1. The first column of results reproduces the findings for the three cohort characteristics from Model 2 of Table 5 in our original article. We include the standard errors of these coefficients in parentheses. The next set of coefficients is from a model that has added the four interactions for age-groups by time to the other independent variables in the model from the original paper (age dummies, period dummies, the three cohort characteristics and the random effects for cohorts). This model is designed to assess whether the age*period trends are creating the appearance of cohort effects; if they are, the cohort effects should disappear when we control for these interactions. We find that the coefficient associated with the cohort characteristic measuring the birth cohort's NFR is only slightly decreased (but that its standard error is nearly doubled). The increased standard error is the result of the increased collinearity that occurs from adding the four interaction terms to the model. For the logged cohort sex ratio, there is essentially no change in the size of its coefficient. The logged cohort school enrollment coefficient shows the greatest change when we control for the age-group trend interactions,

Table 1: Cohort Effects With and Without the Controls Implied by Martin

	Model 2 Table 5			
	1	2	3	4
	Original Relationship	With Age by Time Interaction	With Proportion Unmarried Females	With Proportion and Proportion Squared Unmarried Females
Log NFR	.474***	.414**	.508***	.494***
	(.075)	(.133)	(.099)	(.097)
Log Sex Ratio	2.865	2.732	2.973[a]	2.889
	(1.734)	(1.949)	(1.7169)	(1.666)
Log School Enrollment	-.941**	-.692[a]	-1.023**	-.982**
	(.307	(.379)	(.3388)	(.332)

Note: [a]$p < .10$ *$p < .05$ **$p < .01$ ***$p < .001$. All tests are two-tailed. Standard errors are in parentheses.

but even in this case the change is less than one standard error – far from a statistically significant change.

Martin's explanation for these interactions, is "that delays in marriage increased the unmarried population most rapidly among women in their 20s, so the rise in the non-marital birth ratio has been most pronounced among women in their 20s." Fortunately, we can control for this effect by adding a variable that measures the proportion of unmarried women in each age group in each period to our model. The third column of Table 1 shows the results from a model in which we added this variable to Model 2 of Table 5 in our original paper. The effect of controlling for the proportion unmarried (which is Martin's suggested explanatory mechanism for the age*period trend interactions) is trivial on each of the coefficients for the three cohort characteristics. Each of the coefficients is slightly larger than reported in our original paper. The final set of results in Table 1 are from a model in which we added a variable for the proportion of unmarried women in each of the age-groups in each period and a variable for that proportion squared to the model. We used these two variables to code the unmarried population because of the work of Gray, Stockard and Stone (2006), which shows that both components are necessary to account for the effects of the proportion unmarried on the NFRs. Again the results are essentially the same as those in our original paper.[1] Our conclusion is that neither the age*period trend pattern suggested by Martin nor the proportion unmarried, his suggested mechanism for this pattern, can account for the cohort relationships reported in our paper.

Just a Coincidence?

Martin suggests that, although our cohort characteristics fit the patterns along the cohort diagonals, this is just a coincidence: "the cohort variables described by the authors have only a coincidental relationship with trends in the non-marital birth ratio." This is always a possibility: a general threat to statistical conclusion validity – the data may just fit the explanatory variable by chance. It is one of the reasons for significance tests (to test the hypothesis that the pattern observed is just a chance fit). One of the three cohort characteristics (non-marital births) that we use is related to the age-period-specific NFRs at the .0001 level after we have controlled for age and period using dummy variables. But, of course, such a pattern could be generated by some other factors, and the series we analyze is a short series of observations: only 11 different cohorts (albeit with multiple observations for most of the cohorts).

We have, however, used one of these characteristic (the percentage of the birth cohort members that were born to unmarried women) in diverse research settings and with different dependent variables. We have found that cohort NFR is related to homicide offending from 1960 through 1995 (O'Brien, Stockard and Isaacson 1999) and that this cohort characteristic is related to suicide in the United States with data from 1930-2000 and where the age-period data matrix is triangular rather than rectangular (Stockard and O'Brien 2002). We have found a similar positive relationship between cohort NFRs and suicides using data that cover birth cohorts from 1875 through 1985 and include 19 different nations (Stockard and O'Brien 2006). In each of these studies, the cohort NFRs are significantly related to age-period-specific suicide (or homicide) rates after we control for age and period effects. The cross-national study contained 22 different cohorts; each with a specific cohort NFR. Is the fit we observe a coincidence? We doubt it.

Improbably Large Coefficients and Weird Interactions

Martin cites two reasons why one should not accept our results. The first is the size of the coefficients associated with the cohort characteristics (supposedly they are "improbably large"). We are a bit baffled by this critique. For example the coefficient associated with the log of cohort NFR indicates that a one percent increase in cohort NFR is related to a .47 percent increase in age-period-specific non-marital birth rates. We do not find this to be an improbably large coefficient. The 95 percent confidence interval for this coefficient is .32–62.[2] The coefficient for the cohort characteristic associated with the sex ratio of the cohort is 2.86. This would seem excessive – but is not mentioned by Martin as a coefficient of concern. This may be because the 95 percent confidence interval for this coefficient includes zero, which is unlikely to be seen as an improbably large coefficient. The coefficient for school enrollment is -.94 which is large (but we would maintain not improbably large): a 1 percent shift in the school enrollment is associated with a .94 percent drop in the age-period-specific non-marital birth rate. We note, however, that the increases in non-marital births have been quite large over the period covered in our analysis, and that the 95 percent confidence interval for the schooling coefficient ranges from -.327 through -1.555. Certainly a coefficient falling in the lower portion of this range is not improbably large. The question of what is improbably large is, in part, a judgment call. Our judgment is that the probable size of these coefficients is not improbably large.

In the same paragraph Martin finds it "troubling" that the coefficient associated with school enrollment "is so powerfully *negative* for whites [but] is *positive* and statistically significant for blacks." As explained more fully in the article, we interpret these opposite signs as reflecting the different ways in which the context of race in the United States affects cohort effects. For white cohorts, increased investment in education in young adulthood is related to delaying marriage and motherhood.

For black cohorts this increased investment is related to an oft-cited "independence" effect resulting in motherhood outside of marriage. Such different patterns of results are precisely what one would expect to find as social contexts influence the cohort effects.

Martin's second concern is "a weird cohort*age interaction." He presents evidence of this weirdness in his Table 1, which presents the residuals for cohorts 5, 6 and 7. Martin apparently sees a compelling pattern in the residuals for cohorts 5, 6 and 7. "For the middle cohorts 5, 6 and 7, the fit was uniformly good at the middle ages and bad at the youngest and oldest ages." We do not find the displayed pattern of residuals nearly as unusual as Martin does nor do we find their pattern that easy to interpret. His statement above is true only for Cohort 6. For Cohort 5 the residual for the age group 25-29 (one of the middle age groups) is greater than the residual for the oldest age group. For cohort seven the residual for the age group 35-39 is larger than that for the youngest age group. The residuals for Cohort 5 and Cohort 7 are actually negatively correlated (r = -.21). This is hardly an indication that they follow a pattern of highest residuals at the youngest and oldest ages and small residuals in the middle age groups.

The residuals in Martin's Table 1 are consistent with our explanation based on cohort effects. This is not surprising because this table contains 15 of the 35 residuals on which the cohort effects in our analysis are based and, as we have shown above, these cohort effects are quite robust to the alternative explanations that Martin has suggested. Our analysis will only find cohort effects if the residuals within cohorts tend to have the same sign. That consistency in signs suggests that after controls for age and period effects the predicted age-period-specific NFRs are consistently lower (higher) than expected within different cohorts. This indicates that there may be something about the cohort that is associated with these rates that are lower or higher than expected. In Martin's Table 1, for each of the cohorts the residuals are consistent with cohort effects in that the residuals share the same sign within the cohorts – the only exception is the residual of .004 in Cohort 7. That is, 14 of the 15 residuals show the expected consistency. This result is consistent with Martin's Table 3.

Cohort effects only account for the pattern of residuals between cohorts and not for the pattern of residuals within cohorts. If a researcher were convinced that the residuals within cohorts 5, 6 and 7 had a non random pattern related to age – they could look for additional explanations for this phenomenon.

Martin notes the excellent fit of the cohort*age patterns produced by the age*period explanation in his Figure 4. We are not surprised by this excellent fit. His Figure 4 plots the age-period-specific NFRs for all 35 values of the dependent variable with lines that connect data points representing the same ages. Highlighting the observations for a cohort at different ages (as he does for Cohort 6) – reproduces the data for that cohort exactly. This is hardly an explanation for the data pattern. As we have shown in our Figure 1 (which reproduces in part his Figure 4), if one shows the trajectory for more than one cohort in such a figure, clear differences in cohort trajectories emerge.

Additional Analyses

When we were told that one of the reviewers of our original paper would publish a comment on our paper, we assumed that it would be on our use of mixed models to assess whether our not there were cohort effects in a set of data. Our standard procedure has been to control for age and period effects using dummy variables and then to assess if there is a statistically significant amount of variance associated with the cohort diagonals with these controls in the model (O'Brien et al. 2008). In our first additional analysis, we modify this procedure since Martin suggests that the cohort effects might be due to differences in the age*period trends of NFRs and differences in the proportions unmarried for different age groups in different periods. We ran a model with these variables and the age and period dummy variables included in the model (but not including the random effects for cohorts). This model resulted in a deviance of -116.5. When we add the random variance for cohorts term to the model the deviance was -150.6. Subtracting the deviance for the more inclusive model from that for the less inclusive model yields a chi-square of 34.1 [-116.5 – (-150.6)], with one degree of freedom. The fit of the model that posits differences in the cohort diagonals has a statistically significantly better fit ($p < .0001$).[3] Certainly the pattern of the data is highly consistent with cohort effects after controlling for the other explanations. We note that this addresses the first two sentences in Martin's conclusion that we make "a case for cohort-based effects on the non-marital fertility ratios of American women, but that case does not address the competing explanation of age*period effects. As this analysis shows, when we address the competing explanations by controlling for the age*period trend using four interaction terms and control for the age-period-specific proportion unmarried and the proportion unmarried squared there remains a very strong effect associated with cohorts in the NFRs of white women.

Martin then notes, in his short concluding paragraph, that "If all cohorts are represented at all ages, an age*period interaction should not be able to mimic a cohort effect." We have extended our cohort data to include six cohorts, from the cohort born 1933- 1937 to the cohort born 1958-1962. We have complete data for whites in all age groups for these six cohorts.[4] This is the data that according to Martin should not allow age*period interactions to mimic the effects of cohorts. Using the procedure described in the previous paragraph, we ran the analysis with just age and period dummy variables and found the deviance for the model to be -67.2. When we added the random variance term for cohorts we found the deviance was -93.5. Testing the significance of the improvement of fit in the models due to including the cohort diagonals, we find a chi-square of 26.3 [= - 67.2 – (-93.5)], with 1 degree of freedom. The fit of the model that posits differences in the cohort diagonals has a statistically significantly better fit ($p < .0001$), and this is consistent with cohort effects. In this case, the results are clearly not due to having incomplete data on some of the cohorts included in the analysis. It certainly appears that there is a pattern consistent with cohort effects, whether we control for the effects suggested by Martin or analyze data that includes only complete cohorts. We may well debate the specific causes of such effects, but sociologists and demographers should take such cohort effects very seriously,

Conclusion

Martin suggests two explanations for the cohort patterns found in our data: different trends in the relationships of age-groups to NFRs and differences in the proportion unmarried for age-groups in different periods. We enter variables representing both of these postulated relationships into our models and find that they do not significantly alter the coefficients associated with the cohort characteristics that we reported in our original paper. Thus, at least taking these possible explanations into consideration, we find that the cohort effects remain strong in our data. Martin claims that the coefficients for the cohort characteristics that we report are improbably large – we find no evidence of that in our reading of the results. Martin finds the pattern of residuals within cohorts to be "weird" given our model. In fact, the pattern of the residuals is supportive of cohort effects. Finally, we find cohort effects for age-period-specific NFRs when we analyze only cohorts with complete data, examining cohorts born in the years 1933-1937 and 1958-1962. According to Martin, such an analysis should not be subject to the biases that form the core of his critique. We want to thank Martin for raising these issues, for his professional manner in doing so, and allowing us to demonstrate the robustness of our analysis to other interpretations and under other specifications.

Notes

1. We also ran a model in which we included the proportion married and its square for each of the age-groups in each of the periods and the four age-by-period interactions. The log of the birth cohort's NFR and the log of school enrollment both remained statistically significant and had the same signs as in our original analysis.

2. We computed this as the point estimate plus and minus two standard errors.

3. We would add that we do not like to add so many variables to a model with so few degrees of freedom – but we have been obliged to do so in this response.

4. Because these additional data were only available for whites we did not include the information in the published paper. Our central aim in the paper was to compare results for the two race groups, and it was important to have comparable data sets for the two groups.

References

Gray, Jo Anna, Jean Stockard and Joe Stone. "The Rising Share of Nonmarital Births: Fertility Choice or Marriage Behavior?"*Demography* 43(2):241-53.

O'Brien, Robert M., Kenneth Hudson and Jean Stockard. 2008. "A Mixed Model Estimation of Age, Period, and Cohort Effects." *Sociological Methods and Research* 36(3):402-28.

O'Brien, Robert M., Jean Stockard and Lynne Isaacson. 1999. "The Enduring Effects of Cohort Characteristics on Age-Specific Homicide Rates: 1960-1995. *American Journal of Sociology* 104(4):1061-95.

Stockard, Jean, and Robert M. O'Brien. 2002a. "Cohort Variations and Changes in Age-Specific Suicide Rates over Time: Explaining Variations in Youth Suicide." *Social Forces* 81(2):605-42.

______. 2002b. "Cohort Effects on Suicide Rates: International Variations." *American Sociological Review* 67(6):854-72.

______. 2006. "Cohort Variation in Suicide Rates among Families of Nations: An Analysis of Cohorts Born from 1987 through 1985. *International Journal of Comparative Sociology* 47(1):5-33.

Ventura, S.J., and C.A. Bachrach. 2000. "Nonmarital Childbearing in the United States, 1940-99." *National Vital Statistics Reports* 48(16):1-40.