One problem I ran into is consistently having an N that is larger than that of Mandel and Semyonov. This could be due to differences in the sample size obtained through IPUMS. This could also be due to differences in cleaning procedures of the data. For example, I decided to clean for education using the more detailed EDUCD as opposed to EDUC. EDUCD has more categories that EDUC, for example “GED,” “associate’s degree,” and “master’s degree.” It is possible that the authors chose to clean their data using the EDUC variable and that these observations are included under “N/A”, which would lower their N compared to mine. I also decided to keep observations of “unpaid family worker” under CLASSWKRD because economically inactive people were already filtered out under LABFORCE. If the author’s decided to remove unpaid family workers, that would decrease their N relative to mine. However this likely wasn’t the main factor due to the low number of people who were categorized as “unpaid family worker” in the first place.

The means in my Table A1a are consistently lower than the authors’, though typically by a negligible amount. The means in Table A1b are also slightly different from the authors’ in some cases. These discrepancies are likely due to the larger N in my analysis. My indices of similarity in are almost identical to the authors’ Table A1a and are quite similar in Table A1b as well. Most of my regression coefficients in Table A2a are similar or identical to those in the authors’ table. However, there are two discrepancies for the regression coefficients for “has children under age 5”. The coefficients for 1980 White and 1990 Black are -0.002 and -0.0003 respectively, compared to the authors’ 0.004 and 0.008. However, both of my coefficients are not significant, whereas the authors’ 0.004 for 1980 White is significant (0.008 for 1980 Black is not significant). This implies that my answers should be taken with a grain of salt anyway. In fact, the only coefficient in my Table A2a that was significant for this variable is 2000 White, whereas the authors’ variables had a total of six significant coefficients. This discrepancy could be due to differences in IPUMS samples, or due to the larger N in my analysis (perhaps due to the education filtering mentioned earlier). Interestingly, the discrepancies for “has child under age 5” in Table A2a do not exist in Table A2b. The coefficients in my Table A2b for “Less than high school” are slightly larger than the authors’ coefficients, but only by about 0.1 to 0.2.

I understand the authors’ decision to compare only white and black non-Hispanic men and women, as including additional race categories would create complicated tables and figures. However, restricting the analysis to only these racial categories provides an oversimplified picture of the racial pay gap between men and women. I would extend the analysis by including two additional ethnoracial categories—Asian and Latino (of all races). The analysis for the Hispanic category may be particularly challenging given its census designation as an ethnicity rather than a race. It is likely that phenotypically black or *mestizo* Latino people receive lower wages than phenotypically white Latino people, and lumping all of these racial groups into one category will not capture the heterogeneity in wages within the Latino category. I also feel that it was a mistake to exclude the “other” race category in the authors’ original analysis. Approximately 40% of Latinos mark “other” in the race category (cite), and 97% of the “other” race category is comprised of Latinos (Compton et al. 2012:43). Therefore it would make sense to include “other” as “Latino” in this analysis.

Compton, Elizabeth, Michael Bentley, Sharon Ennis, and Sonya Rastogi. 2012. “2010 Census Race and Hispanic Origin Alternative Questionnaire Experiment.” Washington, DC: U.S. Census Bureau.